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What Provides Justification for Cheating—Producing or Observing Counterfactuals?

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ABSTRACT

When people can profit financially by lying, they do so to the extent to which they can justify their lies. One type of justification is the observation and production of desirable counterfactual information. Here, we disentangle observing and producing of desired counterfactuals and test whether the mere observation is sufficient or whether one actually needs to produce the information in order to justify lying. By employing a modified version of the Die-Under-Cup task, we ask participants to privately roll a die three times and to report the outcome of the first die roll (with higher values corresponding to higher payoffs). In all three conditions, participants produce (roll the die) and observe the first die roll, which is relevant for pay. We manipulate whether participants produce and observe versus only observe the second and third die roll outcomes, which are both irrelevant for pay. Results reveal that people lie to the same extent—when producing and observing the counterfactuals, and when merely observing them. It seems that merely observing counterfactual information is sufficient to allow people to use this information to justify their lies. We further test whether creativity and moral disengagement are associated with dishonesty and replicate the finding showing that unethical behavior increases with creativity. Copyright © 2017 John Wiley & Sons, Ltd.

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KEY WORDS behavioral ethics; cheating; lying; observing counterfactuals; producing counterfactuals

INTRODUCTION

In everyday life, people encounter many situations in which they can profit financially by lying (Hofmann, Wisneski, Brandt, & Skitka, 2014). Whether people act honestly or lie in those situations depends, in part, on a cost–benefit analysis of the situation at hand (Becker, 1968). Beyond this, additional psychological factors affect cheating. For example, lying incurs psychological costs (Fischbacher & Föllmi-Heusi, 2013; Lundquist, Ellingsen, Gribbe, & Johannesson, 2009), which people seek to avoid, sometimes even by avoiding situations that enable them to lie (Fishbach & Woolley, 2015; Shalvi, Handgraaf, & De Dreu, 2011b).

Research suggests, however, that people tend to lie as long as they can justify their digression (Shalvi, Gino, Barkan, & Ayal, 2015; Bazerman & Gino, 2012; Shalvi, Dana, Handgraaf, & De Dreu, 2011a; Schweitzer & Hsee, 2002). Justifications can take several different forms (e.g., benefiting others while lying; Wiltermuth, 2011), and recent work suggests that counterfactual thinking is one of them (Briazu, Walsh, Deeprose, & Ganis, 2017). Specifically, when people produce and observe counterfactual information that displays a desirable outcome, they use this information as a justification for cheating (Shalvi et al., 2011a). Shalvi and colleagues conclude that observing counterfactual information provides justification and thus allows people to cheat in order to make a profit. Still, since people observed, and also produced the counterfactuals themselves in the reported experiment, it remains unclear whether the mere observation of the counterfactual is sufficient to increase cheating, or whether the production of the counterfactual plays a role as well.

Consider, for example, two colleagues at work who use an online tax calculator to determine if they are entitled to a tax refund. After entering all her or his information, one person is disappointed with the initial results and may observe that her or his colleague, who reported slightly different information, gets a more beneficial tax refund. While she or he indicated living 19.5 km away from work, her or his colleague rounded up the report and indicated living 20 km away, which crosses the threshold and warrants a higher refund. Here, we test whether observing such counterfactual information when it is produced by oneself (modifying the tax calculator yourself) as compared with others (observing a colleague modifying the tax calculator) has an effect on dishonest behavior such as reporting falsified information to gain a better tax refund. We do so by introducing a modified version of the Die-Under-Cup task (Shalvi et al., 2011a).

Observing versus producing desired counterfactuals

Shalvi et al. (2011a) employed a Die-Under-Cup task, asking participants to privately roll a die either once or three times,

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and to report the outcome of the first die roll only. Reporting higher outcomes meant earning more money. As participants rolled the die in private, they had the opportunity to lie about the actual observed die roll outcome, thus inflating their rewards. Results showed that, when participants rolled the die three times, the distribution of reports resembled the theoretical distribution of reporting the highest roll out of the three observed die rolls. When participants rolled the die only once, the distribution of reports resembled a pattern of an honest report (i.e., chance). The authors concluded that "having a justification to behave unethically, in the form of observing desired counterfactuals, allows people to maintain a feeling of honesty even when lying quite a bit" (p. 188). However, participants in the Shalvi et al. study not only did *observe* but also *produce* the counterfactuals. The experimental procedure does not allow concluding whether *producing* the high die roll led participants to feel it is justifiable to lie (e.g., "I *rolled* a 'five' on the third roll, thus it is ok to report it as the outcome of my first roll"), or whether merely *observing* a high die roll led participants to feel it is justifiable to lie (e.g., "I *saw* a 'five' on the third roll, thus it is ok to report it as the outcome of my first roll"). Hence, the question remains: Is it sufficient to *observe* a desired counterfactual or do people need to *produce* the counterfactuals themselves to feel that it is justified to lie? Here, we disentangle the acts of *observing* and *producing* counterfactuals by comparing participants who observed and produced counterfactuals to those who just observed counterfactuals that were produced by others. In doing so, we test two theoretical frameworks that lead to contrary predictions and consequently disentangle observing versus producing counterfactuals, which were entangled in the original design.

Feelings of ownership of the die roll outcome may affect the extent to which people feel it is legitimate to use the outcome as counterfactual information to justify lying. Arguably, one is more likely to feel ownership of an outcome that he or she has produced compared with an outcome that he or she only observed. Cognitive dissonance theory (Aronson & Mills, 1959; Festinger, 1957) suggests that the more effort people put into a task, the more they value its outcome (Aronson & Mills, 1959). Accordingly, self-produced objects are judged as more valuable than objects produced by others (Norton et al., 2011). However, even when limited physical effort is involved in generating the outcome (e.g., rolling a die), people still develop a sense of ownership. The endowment effect demonstrates that people develop a sense of ownership just by being arbitrarily endowed with a good (e.g., coffee mug), even though they did not put any physical effort into obtaining the good (Kahneman, Knetsch, & Thaler, 1990; Schurr & Ritov, 2013). A related line of research shows that people judge the person who created an object to be its legitimate owner, even when the owner puts little effort into creating it (Levene, Starmans, & Friedman, 2015) or when others slightly alter the object (Kanngiesser, Gjersoe, & Hood, 2010). Taken together, it seems that even when the creation of a counterfactual requires little psychical effort, those who produce the counterfactual (i.e., rolled the die) will have a higher sense of ownership than those who just observed it.

If ownership of the outcome plays a role in shaping people's lies, we should find that participants who merely observe counterfactuals lie less than participants who produce them. Additionally, we would expect that even when merely observing the counterfactuals, the ownership of the die that was used to produce the counterfactuals will affect dishonesty. That is, people that observed outcomes created with their own die should feel more comfortable to use it to justify their lies than people who observed outcomes created with another person's die. We thus varied the ownership of the die (self vs. other) in the conditions in which participants merely observed counterfactual information.

Alternatively, it might be that ownership of a counterfactual is not important to determine people's lies. From a cognitive construction point of view, knowledge is built by processing and interpreting experiences and previous knowledge (Resnick, 1991). Along this line of thought, it might be that merely observing a counterfactual is sufficient for people to use it as a justification. Indeed, people are rather flexible when they judge truthfulness of events or need to find justifications. Findings from an autobiographical Implicit Association Task revealed that imagined events were associated with truth rather than with falsity. That is, imagining an event increased its implicit truth value, even when the event was explicitly acknowledged as false (Shidlovski, Schul, & Mayo, 2014). Similarly, participants who read a scenario in which they are late for work, while seeing a traffic jam on the other side of the road, used the traffic jam as a justification for being late (Shidlovski, Mayo, Ariely, & Schul, n.d.). Altogether, it seems that sometimes people perceive a situation that did not actually happen to them (but was imagined or observed) as though it was real. This suggests that in the case of ethical judgment and use of justifications, *observing* counterfactuals might be sufficient to allow cheating and the act of *producing* might have a minor (if any) importance in the process. If this is the case, observing counterfactual information should give participants enough justification to cheat, regardless of who produced them.

Individual differences—creativity and moral disengagement

While justifications allow people to benefit from dishonesty without updating their moral self-concept (Mazar, Amir, & Ariely, 2008), the ability to create these self-serving justifications depends on the situation (Pittarello, Leib, Gordon-Hecker, & Shalvi, 2015; Shalvi et al., 2011a) but also on personality traits. Creativity, as a mind-set or a personality trait, allows people to justify their lies even without observing (and/or producing) desired counterfactuals (Gino & Ariely, 2012). An additional strategy, that allows maintaining a moral self-concept while behaving unethically, is to disconnect the respective situation from its ethical relevance. Moral disengagement is a personality trait that enables people to detach the moral aspects from a given situation (Moore, 2015). When one does not perceive a situation as pertaining to the moral sphere, one does not need any justification for his or her behavior. Indeed, studies have found that people high (vs. low) on moral disengagement need less justification in

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order to lie (Bandura, 1990, 2002; Gino & Ariely, 2012). Consequently, we further test whether creativity and moral disengagement affect people's dishonesty in general, and when they produce versus observe counterfactual information.

Current research

We disentangle *observing* and *producing* counterfactual information by employing a modified version of the Die-Under-Cup task (Shalvi et al., 2011a). We asked participants to privately roll a die three times and to report the outcome of the first die roll (with higher values corresponding to higher payoffs: 1 = *€1*, 2 = *€2*, 3 = *€3*, 4 = *€4*, 5 = *€5*, and 6 = *€6*). Our combined condition (*Producing & Observing*) was a direct replication of the multiple rolls set-up used in Shalvi et al. (2011a). In this condition, participants were asked to roll a die three times, thus *producing* and *observing* both the information that is relevant to determine their payment (i.e., first roll) and the irrelevant counterfactual information (i.e., second and third die rolls). In two experimental conditions, participants *observed* but did not *produce* the second and third die roll outcomes. In these experimental conditions, participants rolled and observed the first die roll (which was relevant to their payment) and then observed two additional die roll outcomes, which were rolled by another participant. Thus, in these conditions, the counterfactual information (i.e., irrelevant for payment) was produced by another person. If a higher level of dishonesty is observed in the *Observing & Producing* condition than in the experimental conditions, it will suggest that it is not merely *observing* counterfactual information that liberates people to lie but *producing* those counterfactual pieces of information is necessary as well. In contrast, if a similar level of dishonesty is observed in the combined and experimental conditions, it will suggest that merely *observing* counterfactual information liberates people to lie, even without *producing* the counterfactual information themselves.

We implemented two versions of the *observing* set-up to test whether ownership of the die matters. In one of the experimental conditions, the two additional die rolls were conducted using the participant's own die (*Observing, own die*); in the other experimental condition, the two additional die rolls were conducted with another person's die (*Observing, other's die*). This variation allowed testing die ownership as a potential moderator of the observed effects. We further measured participants' creativity and moral disengagement separately, and before the Die-Under-Cup task.

If ownership is an important factor for using die roll outcomes as a justification for cheating, participants will lie more when they produce (compared with merely observe) counterfactuals (H1a). On the other hand, if ownership is not important and the mere observation of an outcome is sufficient to craft a justification, participants will lie to the same extent when they observe as well as produce counterfactuals (H1b). Furthermore, on the basis of previous findings, we expect to replicate the effects of creativity and moral disengagement on cheating. Namely, higher levels of creativity (H2) and moral disengagement (H3) will increase cheating behavior.

METHOD

Participants and design

A total of 181 participants were recruited via the psychology department's database ORSEE to take part in the study (Greiner, 2004). There were missing values on several variables due to the fact that data were collected using a paper-pencil questionnaire.[1]

We aimed at a sample size similar to that of Shalvi et al. (2011a), who recruited about 60 participants per condition.[2] Participants were German students with different majors from the University of Göttingen. Each participant received a show-up fee of €2 and an additional payment on the basis of their performance in the task (total payment ranged between €3 and €8). Participants were assigned to one of three conditions of a one-factorial design, manipulating whether the counterfactual information was produced and observed (*Producing & Observing*), merely observed, using the participant's own die (*Observing, own die*), or merely observed, using a die of another participant (*Observing, other's die*). The condition was chosen randomly.

Procedure

Participants showed up at the lab—four to nine at a time—and were seated in private cubicles (Figure 1B). Every cubicle contained two plastic cups: a cup with a hole on the bottom of it, and a cover-cup to ensure confidentiality (Figure 1 C, right). First, all participants read and signed an informed consent form. Second, the experimenter rolled a couple of dice a few times in order to show that the dice are fair. Then, each participant picked a die out of a box with multiple dice and learned that he or she will be asked to roll a die three times and report the outcome of the first die roll. They also learned that this report would determine their payment, with higher reports corresponding to higher payoff (i.e., 1 = *€1*, 2 = *€2*, 3 = *€3*, 4 = *€4*, 5 = *€5*, and 6 = *€6*).

In the *Producing & Observing* condition ($N = 60$), participants were asked to roll the die by shaking the cup, peep through the hole in the cup, and remember the outcome. Afterwards, they were asked to move one cubicle to their right, taking their cup and their die with them. There, they were asked to roll the die again and peep through the hole to observe the outcome. Participants did this twice, each time moving to a different cubicle and rolling their die. After the three rolls were completed, participants were asked to report the outcome of the first die roll. We asked participants to

[1]Only analyses of the main dependent variable "reported die roll" include all 181 subjects. Additional analysis with regard to manipulation checks, similarity among conditions and scales of moral disengagement and creativity include a maximum of 165 participants, which is how many participants actually completed both parts of the study.

[2]A post-hoc power analysis revealed that a one-way ANOVA would detect small to medium effects of $f = .20$ with a power of .66 for a total sample size of 180.

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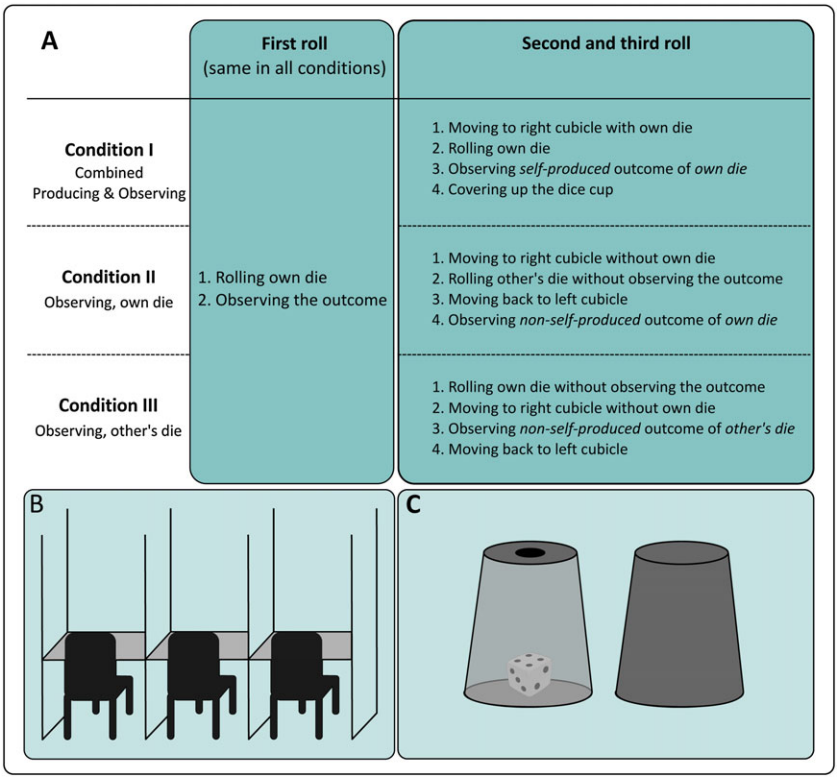

Figure 1. Overview of experimental procedure. (A) Description of all conditions. (B) Sketch of the lab; participants moved from one cubicle to another between the die rolls. (C) Illustration of the cups; the left cup had a hole in the bottom, so that participants were able to peep through it; the right cup was used to cover the left cup. Both cups were non-transparent. [Colour figure can be viewed at wileyonlinelibrary.com]

move from one cubicle to another between each of the rolls in order to keep all three conditions comparable (Figure 1 A, condition I).

In the *Observing, own die* condition ($N = 61$), participants were asked to roll the die by shaking the cup, peep through the hole in the cup, and remember the outcome. After that, they were asked to cover the cup with the cover-cup and move one cubicle to their right. In this cubicle, they were asked to roll the die but to avoid looking at the outcome. As the cup was covered by another cup, this was easy to do. Participants then moved back to their initial cubicle, removed the cover-cup, and peeped through the hole of the cup underneath to see the outcome of the die roll. In other words, they observed an outcome on their own die, which was rolled by another participant. Each participant did this twice, resulting in observing three die roll outcomes, one of which the participant had produced and that was relevant to their payment, and two of which another participant in the session had produced using the participants' die. After the three rolls were completed, participants were asked to report the outcome of the first die roll to determine their payment (Figure 1A, condition II).

In the *Observing, other's die* condition ($N = 60$), participants were asked to roll the die by shaking the cup, peep through the hole in the cup, and remember the outcome. Then, they were asked to cover the cup with the hole with the cover-cup, roll their own die again, without looking at the outcome, and move to the cubicle to their right. In the new cubicle, they were asked to remove the cover-cup and check the outcome of the die through the hole of the bottom cup. In other words, they observed an outcome of another participant's die, which had also been rolled by that other participant. Each participant did this twice, resulting in observing three die roll outcomes, one of which the participant had produced and was relevant to their payment, and two of which another participant had produced using a different die. After the three rolls were completed, participants were asked to report the outcome of the first die roll to determine their payment (Figure 1A, condition III).

Materials

Personality measures

At least 1 day before the lab session, participants completed two personality measures. Items were answered on a scale from 1 to 6 (1 = *not at all*, 6 = *totally agree*), with 7 = *prefer not to answer*; the latter was not used by any of the participants. Blank answers were treated as missing values.

Creativity. Participants completed a creativity scale used by Gino and Ariely (2012). This five-item questionnaire ($\alpha = .83$; 95% CI [.74; .92]) included questions such as "I have a lot of creative ideas" and "I like to do things in an original way" and allowed us to evaluate the extent to which participants are creative.

Moral disengagement. Participants completed a moral disengagement scale (Moore, Detert, Klebe Treviño, Baker, & Mayer, 2012). This eight-item questionnaire ($\alpha = .65$; 95% CI [.55; .76]) included questions such as "people shouldn't

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be held accountable for doing questionable things when they were just doing what an authority figure told them to do" and "taking something without the owner's permission is okay as long as you're just borrowing it" and allowed us to assess the extent to which participants can detach moral aspects from a given situation.

Other measures

Manipulation check. After the die roll task, we asked participants to (i) evaluate the extent to which they felt they "owned the second and third die roll outcome to the same extent that they owned the first die roll outcome" and (ii) the extent to which they "produced the outcome of the second and third die rolls."

Similarity among conditions. Participants evaluated (i) the extent to which they believed that the die was fair; (ii) whether or not "the second and third die roll outcomes could have as well been produced as the first die roll" and vice versa (outcome possibility, two items, $\alpha = .90$); (iii) the extent to which participants felt that they could be caught cheating (detection probability, two items, $\alpha = .62$); (iv) the extent to which participants felt that they were able to influence the first, second, and third die roll outcomes (ability to influence die roll outcomes, two items, $\alpha = .86$); and (v) the ability to recall the first, second, and third die roll outcomes (ability to recall die roll outcomes, three items, $\alpha = .80$). We included these items for two reasons—first, in order to confirm that the conditions were as comparable as possible and second, in order to rule out alternative explanations in case we find differences between conditions.

Justification to cheat. In order to test whether participants felt that their ability to justify cheating differed between conditions and influenced their reports, we asked to what extent participants felt that it is ok to cheat (justification to cheat, four items, $\alpha = .84$).

Additional items. We asked participants to evaluate the extent to which the second and/or third die roll influenced their reporting of the first die roll outcome. Furthermore, we asked how bad participants would feel if they would have got caught reporting a higher outcome than they actually rolled. Finally, in order to confirm that participants (mis)report only when they have a financial incentive to do so, we further asked participants to report the second and third die roll outcomes, which were both irrelevant for pay; see Supporting Information.

All items are listed in the Appendix. The original items of the study in German and the data are available at open science framework at osf.io/cuz5j.

RESULTS

Manipulation check

A one-way analysis of variance (ANOVA) of the item "I feel that I owned the second and third die roll outcome to the same extent as I owned the first die roll outcome" revealed a main effect of condition, $F(2, 163) = 11.66$, $p < .001$, $\eta^2 = .13$. Planned comparisons, equal variances not assumed, showed that the extent to which participants felt that they "owned the second and third die roll to the same extent as they owned the first die roll" was, indeed, greater in the *Producing & Observing* condition ($M = 3.67$, $SE = .27$) than in both of the experimental conditions, the *Observing, own die* condition ($M = 2.13$, $SE = .20$) and the *Observing, other's die* condition ($M = 2.60$, $SE = .21$), $t(86.57) = -4.28$, $p < .001$. However, there was no significant difference between the *Observing, own die* and *Observing, other's die* conditions, $t(109.93) = -1.59$, $p = .115$, suggesting that people feel differently towards outcomes that they produced as compared with those that they observed, but indifferently to whether they owned the die or not.

An additional one-way ANOVA with "I produced the outcome of the second and third die roll" as dependent variable revealed a main effect of condition, $F(2, 170) = 7.66$, $p = .001$, $\eta^2 = .08$. Participants felt to a greater extent that they produced the second and third outcomes in the *Producing & Observing* condition ($M = 2.84$, $SE = .27$) than in both of the experimental conditions, the *Observing, own die* ($M = 1.73$, $SE = .19$), and *Observing, other's die* conditions ($M = 1.75$, $SE = .22$), $t(87.497) = -3.55$, $p < .001$. There was no significant difference between the *Observing, own die* and *Observing, other's* die condition, $t(110.14) = -.089$, $p = .929$. Contrasts were calculated correcting for unequal variances.

Similarity among conditions

Five one-way ANOVAs with condition as independent variable, predicting (i) the extent to which participants believed that the die was fair; (ii) outcome possibility; (iii) detection probability; (iv) ability to influence die roll outcomes; and (v) ability to recall die roll outcomes revealed no differences between conditions (all F's < 1.69, all p's $> .19$). For an overview of descriptive statistics, see Table 1. Interestingly, even though in some settings people's involvement in a situation increases their perception of success (although the outcome is determined by chance—i.e., illusion of control; Langer, 1975), participants in our design did not feel that they had higher influence on the die roll outcomes when they produced versus observed the die roll outcomes.

Cheating

To assess whether participants were lying, we compared the distribution of reports in each condition to the distribution that is expected if participants are honest (i.e., a uniform distribution of 16.66% for each die roll outcome). Indeed, in all three conditions, the distribution of reports differed from the uniform distribution expected from an honest report (*Producing & Observing:* Kolmogorov–Smirnov $Z = 2.06$, $p < .01$; *Observing, own die*: Kolmogorov–Smirnov $Z = 1.49$, $p < .05$; *Observing, other's die*: Kolmogorov–Smirnov $Z = 1.93$, $p < .01$). This is in line with past work (Shalvi et al., 2011a; Fischbacher & Föllmi-Heusi, 2013), that shows that people use the privacy of the task in order to lie and boost their profit.

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Table 1. Means and standard errors of conditions (item scales range from 1 to 6)

	Fairness of die	Outcome possibility	Detection probability	Ability to influence die roll outcomes	Ability to recall die roll outcomes
Observing & producing	5.27	5.64	2.84	1.69	5.72
	(.18)	(.08)	(.19)	(.16)	(.10)
Observing, own die	5.38	5.56	2.36	1.44	5.68
	(.17)	(.13)	(.17)	(.13)	(.10)
Observing, other's die	5.49	5.50	2.64	1.65	5.62
	(.14)	(.14)	(.20)	(.18)	(.12)
Overall mean	5.38	5.57	2.62	1.59	5.67
	(.09)	(.07)	(.11)	(.09)	(.06)

To test our competing hypotheses H1a and (the null hypothesis) H1b, we conducted an ANOVA with the reported first die roll outcome as dependent variable and condition as independent variable (*Producing & Observing* vs. *Observing, own die* vs. *Observing, other's die*). Results revealed no differences between the means of reported outcome between conditions, $F(2, 178) = .237$, $p = .789$. Participants in the *Producing & Observing* condition reported rolling an average of $M = 4.35$ ($SE = .22$); participants in the *Observing, own die* reported rolling an average of $M = 4.23$ ($SE = .20$); and participants in the *Observing, other's die* reported rolling an average of $M = 4.43$ ($SE = .21$). Additionally, we compared the distributions between conditions. Analyses revealed that distributions did not differ significantly from each other; all Kolmogorov–Smirnov $Z < .72$, all $p > .1$. Thus, it seems that in all three conditions participants behaved similarly. Hence, there is no support for H1a that producing increases cheating compared with observing counterfactuals, and we have to tentatively retain the null hypothesis H1b.

Next, we assessed whether participants showed behavior that is consistent with reporting the highest value of the three rolls, a process that Shalvi et al. (2011a) suggested and found. In order to do so, we compared the distributions of reports in each condition to the theoretical distribution of reporting the highest value of three die rolls; see Figure 2 for the distributions in all three conditions. In the theoretical distribution of reporting the highest value of three die rolls, the value 1 is very unlikely to occur (only if a participant saw 1–1–1, which happens in 1 out of $6^3 = 216$ times), but higher values are more likely to happen; see Figure 2, left panel. Analysis revealed that the distributions of the *Producing & Observing* and *Observing, other's die* conditions did not differ from the theoretical distribution of choosing the highest of three rolls (Kolmogorov–Smirnov $Z = 1.26$, $p > .1$, and Kolmogorov–Smirnov $Z = 1.35$, $p > .05$, respectively). Hence, in these two conditions, we could not reject the hypothesis that people who produced and observed the counterfactual information and those who merely observed the counterfactual information (rolled with someone else's die) reported the highest of three rolls they have observed. Unexpectedly, and although the means and distributions did not differ between conditions (see preceding texts), in the *Observing, own die* condition, the distribution of reports statistically differed from the theoretical distribution of reporting the highest of three rolls, Kolmogorov–Smirnov $Z = 1.84$, $p < .01$. Including a Bonferroni correction for our post-hoc tests ($m = 3$; $\alpha = .05/3 = .0167$) still yielded the same results.

Justification to cheat

In order to further explore whether observing versus producing counterfactuals has an effect on how justifiable participants perceive misreporting the truth, we tested whether

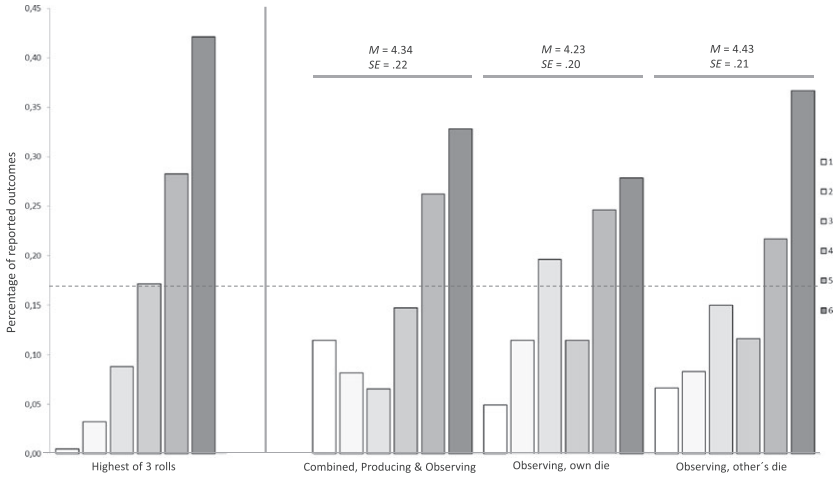

Figure 2. Comparison with theoretical distribution. The theoretical distribution of choosing the highest of three die rolls (left), the percentage of reported outcomes in the *Observing & Producing* (second to the left), *Observing, own die* (second to the right), and *Observing, other's die* (right) conditions. The dashed line represents the honest distribution predicted by chance (16.67% per die outcome)

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Table 2. Bivariate correlations of condition, covariates and reports of first die roll

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Reports of first die roll	1.00													
2. Condition (1 = *producing & observing*, 2 = *observing, own die*, 3 = *observing, other's die*)	.02 182	1.00												
3. Justification to cheat	.15* 175	.17* 175	1.00											
4. Fairness of die	.10 172	.07 172	.08 166	1.00										
5. Outcome possibility	−.06 176	−.07 176	−.11 170	.26*** 168	1.00									
6. Influence of other rolls	.11 182	−.08 182	.20** 175	−.23** 172	−.27*** 176	1.00								
7. Detection probability	.04 172	−.06 172	−.26*** 167	−.28*** 164	−.08 168	.14† 172	1.00							
8. Ability to influence outcomes	.13† 180	−.01 180	.13† 173	−.14† 171	−.21** 174	.09 180	.10 171	1.00						
9. Ability to remember outcomes	−.03 181	−.05 181	−.18* 174	.12 171	.24** 175	−.06 181	−.02 171	−.03 179	1.00					
10. Negative utility being detected	−.15* 178	−.12 178	−.22** 172	−.04 168	.13† 173	−.09 178	.13† 171	−.07 176	.08 177	1.00				
11. Number of people in session	.10 182	−.09 182	−.03 175	−.02 172	.05 176	−.03 182	−.01 172	−.03 180	−.10 181	−.01 178	1.00			
12. Correct guess of study goal	−.04 168	−.19* 168	−.11 161	.10 160	.22** 162	−.15† 168	−.17* 159	−.08 166	.07 167	−.03 164	.04 168	1.00		
13. Moral disengagement	.05 165	−.01 165	.22** 158	.02 155	−.17* 159	−.02 165	.01 155	.09 163	−.27*** 164	−.04 161	.04 165	−.05 153	1.00	
14. Creativity	.21** 165	−.00 165	.07 158	.14† 155	−.12 159	.06 165	.09 155	.09 163	−.05 164	−.20* 161	.00 165	−.18* 153	.15† 165	1.00

Below the correlations, we report a number of observations. Correct guess of study goal was a free-form field and transformed into a binary variable, coding whether or not participants correctly guessed the study goal. Thus, we report point-biserial correlations for this variable.

†$p < .1$;
*$p < .05$;
**$p < .01$;
***$p < .001$ (two-sided).

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justification to cheat differed among conditions. A one-way ANOVA with condition as independent variable, predicting the extent to which participants report it is justifiable to cheat, revealed no significant effect for condition, $F(2, 172) = 2.481$, $p = .087$.

Creativity

The five creativity items from the creativity measure were averaged into a mean creativity score per participant. An analysis of covariance predicting the reported first die rolls, with condition as independent variable and creativity as a continuous measure, revealed a main effect for creativity, $F(1, 159) = 7.498$, $p = .007$, $\eta_p^2 = .045$. Specifically, in line with H2 and replicating Gino and Ariely (2012), we found that the more creative a person is, the higher die roll outcomes that he or she reports, $r = .209$, $p = .007$. We found neither a main effect of condition, $F(2, 159) = .399$, $p = .672$, nor an interaction between condition and creativity, $F(2, 159) = .067$, $p = .935$.

We further assessed whether creativity and the maximum die roll outcome (among the second and third die rolls) affected the reported first die roll outcome, which was relevant for the payment. The results revealed a significant positive effect of creativity ($\beta = .21$, $p = .007$), indicating that more creative people report higher die roll outcomes. There was neither a main effect of the maximum of the reported second and third rolls ($\beta = .01$, $p = .85$) nor an interaction ($\beta = .04$, $p = .64$).

Moral disengagement

The eight moral disengagement items were averaged into a mean moral disengagement score per participant. Generally, moral disengagement did not influence the report of the first die roll ($r = .05$, $p = .52$). An analysis of covariance with condition (*Producing & Observing* vs. *Observing, own die* vs. *Observing, other's die*) and moral disengagement as a continuous measure predicting the value of the reported first die roll, did not reveal any significant effects, all F's $< .44$, all p's $> .68$. Hence, H3 was not supported, showing that in our setting, one's ability to disconnect the moral aspects of the situation did not affect cheating behavior.

Covariates and full model

Table 2 provides a full correlation matrix of cheating behavior (report of first die roll) and further measures for exploratory purposes. As one would expect, there was a significant correlation between justification to cheat and the values participants reported as the first die roll outcome ($r = .15$, $p = .04$). The more participants thought it is ok to cheat, the higher the values they reported. Moreover, in line with utility theory, there was a significant negative correlation between the extent to which participants would feel bad being caught cheating and the reported first die roll outcome, $r = -.15$, $p = .04$. That is, the worse one would feel getting caught cheating, the lower the outcome that he or she reported. Furthermore, there was a tendency of participants to report higher outcomes when they believed to be able to influence the die roll outcomes ($r = .13$, $p = .08$).

Table 3 displays the full regression model predicting the reported first die roll outcome with condition and all control factors, including creativity and moral disengagement. As becomes evident, partial correlation coefficients predicting the reported first die roll outcomes are only significant for the number of people in the session and creativity levels. The more people took part in a session, the higher their reported outcome; the more creative people were, the higher their reports.

DISCUSSION

Shalvi et al. (2011a) found that observing self-produced counterfactual information leads people to lie for self-profit. Still, it remained unclear whether observing counterfactuals is sufficient to shape people's lies or whether generating the counterfactuals themselves is necessary in order to allow people to lie. Research demonstrating the effect of imagination of events on implicit truth value would predict that the perception of what is true and what is false is rather flexible, and that observing counterfactuals is sufficient to use them as justifications for lying. This would lead to no difference between people who merely observe and those who observe and produce counterfactual information.

In line with this account, we found that merely observing counterfactuals is sufficient to lead people to lie for self-profit. In all three conditions, participants lied to a similar degree and inflated their profit. Response distributions in all three conditions differed from a uniform distribution of honest reports and did not differ from one another. Additionally,

Table 3. Ordinary least squares regression reporting standardized beta coefficients of condition and covariates on reports of first die roll

Reports of first die roll	Partial correlations	*t*-value
Condition (1 = *producing & observing*, 2 = *observing, own die*, 3 = *observing, other's die*)	.10	1.13
Justification to cheat	.09	.98
Fairness of die	.02	.18
Outcome possibility	−.02	−.14
Influence of other rolls	.03	.32
Detection probability	.08	.87
Ability to influence outcomes	.04	.46
Ability to remember outcomes	−.05	−.51
Negative utility being detected	−.05	−.52
Number of people	.18*	2.03
Correct guess of study goal	.02	.27
Moral disengagement	−.03	−.36
Creativity	.25*	2.59
Constant	—	.35
Observations	130	
Adjusted R^2	.044	

See Appendix for the items of covariates.
*$p < .05$.

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the mean of the die roll outcome relevant for participants' payment did not differ between the three conditions. These results indicate that participants behaved similarly in all three conditions. In two out of three conditions, the distribution of reports did not differ from the distribution of "highest of three rolls." Hence, in these conditions, the findings were in line with the proposal by Shalvi et al. (2011a) that people use the second and third rolls as a justification. Taken together, it seems that it is sufficient to merely *observe* counterfactuals, and that this, in turn, provides enough justification for cheating. Our findings replicate the results presented by Shalvi et al. and corroborate the conclusion that merely *observing* desired counterfactuals enhances cheating. The finding further adds to the literature showing that truth perception can be rather flexible and particularly that imagining or merely observing an event increase its truth value, and consequently allow people to use it as justification (Shidlovski et al., 2014; Shidlovski et al., n.d.).

Observed patterns in all three conditions did not significantly differ from each other, in terms of both the mean and the distribution of reported outcomes. Hence, participants might have used similar strategies across conditions. That said, we note that analyzing each distribution separately revealed no significant difference between the observed distribution and the expected distribution of "choosing the highest of three rolls" in two out of the three conditions. Recent research shows that people use the "highest of three" strategy across different countries (Gächter & Schulz, 2016), and that being instructed to test the die (and thus observe additional die roll outcomes) increases cheating behavior compared with participants who are not instructed to test the die (Abeler, Nosenzo, & Raymond, 2016). We encourage future research to use meta-analytical approaches to assess the extent to which this strategy is robust to different settings.

Additionally, we add further evidence to the literature on the "dark side of creativity" (Gino & Ariely, 2012; Mai, Ellis, & Welsh, 2015; Niepel, Mustafić, Greiff, & Roberts, 2015). Replicating the study of Gino and Ariely (2012), we show a positive correlation between creativity and unethical behavior — the more creative a participant was, the higher die roll outcomes he or she reported. We further find that people are somewhat honest about their dishonesty (Halevy, Shalvi, & Verschuere, 2014). The less people felt negative feelings associated with getting caught cheating and the more they stated it is ok to lie, the higher were their reported outcomes.

Interestingly, in our design, we obtained no evidence for an association between moral disengagement and cheating. So far, moral disengagement has shown to be positively related to unethical behavior in various contexts, such as in business decision making (e.g., Moore et al., 2012; Ogunfowora, Bourdage, & Nguyen, 2013), academic context (e.g., Farnese, Tramontano, Fida, & Paciello, 2011), everyday decision making (e.g., Detert, Treviño, & Sweitzer, 2008), and in classic experimental cheating paradigms (e.g., Gabbiadini, Riva, Andrighetto, Volpato, & Bushman, 2013). However, some studies do not show this pattern. For example, Panasiti, Pavone, Merla, Aglioti, and Perc (2011) did not find a direct relation between moral disengagement and lying. In the context of our experiment, it might be the case that by providing justification via counterfactual information, people did not need to rely on their ability to disengage the moral aspects from the situation in order to cheat, resulting in a null effect of moral disengagement on cheating.

We further found a positive association between the number of participants in a session and the reported die roll outcomes. Although we had no ex ante prediction about this factor, we suggest two possible explanations for this effect. One possibility is that with more people in a session, participants have an increased feeling of anonymity, which is known to increase unethical and illegal behavior (Becker, 1968). Rendering this possibility less likely, however, we did not find a meaningful correlation between the number of participants in a session and detection probability (i.e. participants subjective estimates for cheating being detected) (Table 3). The second possibility is that more participants in a session increased the saliency of social comparison, which in turn pushed participants to lie. Indeed, information that imposes threat to self and enhances social comparison increases people's motivation to lie (Argo, White, & Dahl, 2006). It might be the case that the more people participants saw in a session, the more they compared themselves to others, and, in turn, the more they lied.

Although sometimes produced outcomes are judged as more valuable than observed ones (e.g., Norton, Mochon, & Ariely, 2012; Festinger, 1957; Aronson & Mills, 1959), we found no difference in the extent to which people use those outcomes in order to justify their lies. One possible reason for this is that in our study, participant's physical effort was identical in all conditions. An interesting avenue for future research could be to assess the extent to which the physical effort people put into a task (for example rolling a heavy vs. a light die) affect people's use of counterfactuals. An additional avenue could be to test whether other ways of learning about counterfactual information provide enough justification for cheating. For example, whether imagining or even simply hearing about counterfactual information is enough to increase cheating. Previous research shows that imagining an event increases its truth value (Shidlovski et al., 2014), suggesting that even being familiar with counterfactuals by imagination may lead to cheating.

Furthermore, the social identity of the person who produces the counterfactual information is an intriguing direction to be further explored. More specifically, whether that person is an in-group versus out-group member or is perceived positively versus negatively by others might be relevant when people decide whether to use the counterfactuals created by that person as a justification to lie. Previous research showed that people use cues that encourage unethical behavior from their in-group members, but not from their out-group members (Gino, et al., 2009). In line with that finding, it might be that counterfactuals produced by out-group members are less likely to be used as a justification than counterfactuals that are produced by in-group members.

Finally, incentives to use counterfactual information for personal advantage can be not only financial but also social or ideological. The fact that merely observing counterfactuals is sufficient to unethically use it for profit has important social and political implications. For instance, it has become

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more and more popular to read news articles on the internet and share them via social media (Lee & Ma, 2012; Purcell, Rainie, Mitchell, Rosenstiel, & Olmstead, 2010). Reading unreliable and sometimes false information on various websites might lead people to share it in order to gain social advantages or unethically skew others' views on important issues. Future research should explore the use and consequences of using counterfactual information when the motivation is not purely or immediately financial.

CONCLUSION

When people encounter situations in which they can profit financially by lying, they tend to lie as long as they can justify their acts. One type of justification is producing and observing beneficial counterfactual information. Here, we disentangle the *observation* and *production* of counterfactual information in order to investigate which of the two mechanisms actually provides the justification to lie. We show that merely observing counterfactuals is enough to use them for justifying cheating. We further provide additional evidence for a positive relationship between creativity and cheating, namely, more creative people tend to exhibit more dishonest behavior. Our results corroborate and refine previous research findings and provide additional insights for a better understanding of the mechanisms underlying unethical behavior.

APPENDIX

Manipulation check

(1) I own the outcomes of the second and third die roll to the same extent as I own the outcome of the first die roll.
(2) I produced the outcomes of the second and third die roll.

Justification to cheat

(1) It is ok to report an outcome that is different from the first die roll outcome.
(2) It is ok to report the outcome of the second or third die roll.
(3) It is my prerogative to report the second or third die roll outcome.
(4) It is my prerogative to report any number, instead of the first, second, or third roll.

Fairness of die

(1) My die was fair, that is, all outcomes had the same likelihood.

Outcome possibility

(1) I could have as well produced the second and third die roll outcomes in the first die roll.
(2) I could have as well produced the first die roll outcome in the second or third die roll.

Influence of other rolls

(1) The second and/or the third die roll influenced my reporting of the first die roll outcome.

Detection probability

(1) I think it is not possible to track the true outcome of my first die roll.
(2) It is possible to find out whether or not I truly reported the first die roll outcome (r).

Ability to influence outcomes

(1) I was able to influence the outcome of the first die roll.
(2) I was able to influence the outcomes of the second and third die rolls.

Ability to remember outcomes

(1) I fully recall the outcome of the first die roll.
(2) I fully recall the outcome of the second die roll.
(3) I fully recall the outcome of the third die roll.

Negative utility being detected

(1) How bad would you feel if you were caught reporting a higher outcome than you actually produced? (*not bad at all* to *very bad*).

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