Cheating in Mind Games: The Subtlety of Rules Matters

Ting Jiang
University of Pennsylvania; CentER, Tilburg University

Abstract

This paper employs two variants of the "mind game" to show how a subtle variation in the game's rules affects cheating. In both variants of the game, cheating is unobservable because subjects make their choices purely in their minds. The only difference stems from the ordering of steps that subjects are instructed to follow when playing the game. The order of play has a significant impact on cheating behavior, even though the rules cannot be enforced.

Keywords: cheating; mind game; rule design; moral image; decision time
JEL codes: C91, D63, H26, K42

Highlights

- Cheating is unobservable in the mind game.
- I compare cheating behavior using two variants of the "mind game".
- The only difference between the variants rests on the ordering of the instructed steps.
- The ordering not only affects the extent of cheating, but also leads to distinct behavioral patterns including decision time.

Author Contact Information:

Email: tingj@sas.upenn.edu
Phone: +1 215573428
Address: 249, S. 36th Street, PA19104, Philadelphia, the United States

1. Introduction

Society needs rules, and these rules need to be enforced since there is often an incentive to break rules for monetary gains. The enforcement of rules, however, does not always require costly punishment mechanisms. Though motivated by economic incentives to cheat, people often refrain from cheating out of social and moral considerations. Sometimes all it takes is a little tinkering with the rules to curb cheating. By exploiting two variants of a simple cheating game, which I call the "mind game", I show in this paper how cheating behavior is significantly affected by the ordering of the instructed steps.

The mind game is a die game in which subjects make a choice purely in their minds. The game consists of three steps: "choose a side", "throw a die" and "write the chosen side on paper". In both variants of the game subjects first choose which side of a six-sided die will count for their earnings: the side facing up "U" or the side facing down "D". Since subjects get paid based on their self-reported choice, they can cheat by lying about the choice made. The experimental manipulation rests on the prescribed order of the last two steps. In the "Throw-first" treatment, subjects are supposed to first throw the die and then write the chosen side, "U" or "D". In "Write-first", subjects are supposed to write the chosen side prior to throwing the die, which would prevent them from cheating if the order of the steps is followed. Because the order of the steps are only prescribed and not enforced, subjects can still cheat in "Write-first", but with greater physical or mental effort. In order to cheat, they have to either overwrite the side they wrote down, which can leave a trace of cheating, or disregard the prescribed order of play and play the game as if in "Throw-first". This study shows how such subtle variation in the rules of the game affects the extent of cheating.

Cheating in both variants refers to lying about the side chosen that results in a higher payoff than that of the honest side. I use the term "cheating" to distinguish it from pure lying since subjects can also lie for earning less. Thus, the type of cheating studied here is similar to what Erat and Gneezy (2012) classify as "selfish black lies", lies which benefit self at the cost of others, except that here cheating is at expense of the experimenter instead of other experimental subjects.

The mind games used in this paper add to the recent experimental paradigms that utilize the known distributions of die or coin tosses to infer cheating (e.g., Bucciol & Piovesan, 2011; Fischbacher & Heusi, 2008; Greene & Paxton, 2009; Hao & Houser, 2010). In Heusi & Fischbacher, subjects roll a six-sided die in a private room and get paid according to the self-reported outcome of one die throw, without the first step of the mind game in making a choice of the side in mind. Cheating can in theory still be detected if there is a hidden camera. Cheating cannot be statistically inferred at the individual level. In the mind game, subjects need not fear the exposure of their lying even under camera surveillance since the choice is made purely in the mind.[1] But subjects might fear the statistical inference of cheating since the game is repeated for twenty rounds. In this sense, the Throw-first variant resembles the coin-flip task by Greene and Paxton (2009). Their cheating game paradigm is less known to economists because the aim of the paper was to study the neural activity associated with honesty and dishonesty using fMRI and reaction time results. Nevertheless, the rules of the cheating task are similar to the Throw-first variant: subjects were asked to first make a guess about the coin flip outcome and self-report the guess only after they see the coin flip outcome. The priming of making a guess and making a choice can differ slightly, but to a negligible extent. Nevertheless, using a six-sided die instead of a coin has the advantage that the cheating gain varies with die outcomes. Since the gain equals the difference between the points of the opposite sides, which always add up to 7, it ranges from five points of difference with die outcomes "6" and "1" (high), three points with "5" and "2" (medium), to one point with "4" and "3" (low). The non-intrusive within-subject treatment of cheating gains enables further investigations on the more subtle and automatic cheating "strategies".

The results show that while no one cheats 100% in both variants, subjects in Write-first cheat significantly less. Moreover, subjects in Write-first only cheat for low gains whereas those in Throw-first cheat the most for medium gains. There are mainly two potential explanations for the distinct cheating patterns. First, subjects may dislike rule-breaking *per se*, and they cheat less in Write-first because cheating implies not only

[1] See also Shalvi, Handgraaf and De Dreu (2011) who try to eliminate fear of detection by letting subjects roll the die under a cup instead of in the open.

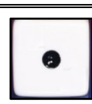
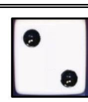
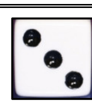
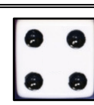
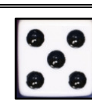
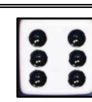
lying, as in Throw-first, but also breaking the rule of following the prescribed order of play. This explanation however cannot easily explain the result that subjects in Write-first only cheat for low gains and forego the opportunities to cheat in the face of other gains after the rule of the prescribed order is already broken. Second, recent theories and findings suggest that people care about keeping a moral (self) image (e.g., Benabou & Tirole, 2002; Fischbacher & Heusi, 2008), and they only cheat to the extent that their moral image is maintained (Ariely, 2012; Mazar, Amir & Ariely, 2008). For moral judgement, intentions matter (Charness & Dufwenberg, 2006; Falk, Fehr & Fischbacher, 2008). In Write-first, it is harder to keep a moral image because one's bad intent to cheat is harder to deny or ignore given the deliberate act of postponing writing in order to cheat. In Throw-first, one can more easily deceive oneself about the nature of cheating as it only requires an internal twist of the mind. One can more easily get away with excuses that the cheating outcomes are due to errors or "luck". Process can matter more than the mere outcome as it helps to discern the actor's good or bad moral disposition.

2. Experimental Design

2.1 Mind game

The mind game consists of three steps: "choose a side", "throw a die online" and "write the chosen side on paper". Subjects choose purely in mind which side of the die counts for their final earnings, the side facing up (U) or the side facing down (D), and get paid based on the actual die outcomes and their self-reported choices of sides (see Table 1).

Table 1: Earning points corresponding to "Up" or "Down"

						
U	1	2	3	4	5	6
D	6	5	4	3	2	1

Note*:* Every point is equivalent to five Euro-cents.

Subjects can cheat by writing down a different side than the one chosen on the paper outcome forms. Moreover, they can only cheat by misreporting the side, since they do not self-report the die outcomes that are randomly generated and recorded online. Cheating gains vary from 5 points (1 vs. 6), 3 points (2 vs. 5) to 1 point (3 vs. 4). By repeating the game twenty times, cheating can be inferred both at the aggregate and at the individual level if the proportion of "lucky" choices reported is statistically improbable. To examine how the order of steps affects cheating behavior, the two treatments differ only in the prescribed order of the last two steps (see Table 2):

Table 2: Steps of the two mind game variants

	The order of the steps
Throw-first:	Choose → Throw → Write
Write-first:	Choose → Write → Throw

In order to cheat, subjects in Write-first have to, in addition to lying, either overwrite the side they wrote down or mentally disregard the prescribed order of steps and throw before writing. It turns out that no one made any correction on the outcome form. Thus, the order of steps that cheaters follow is identical in the two variants: choose a side, throw the die and write the side.

2.2 Experimental procedure

The experiment was conducted in CentER lab at Tilburg University in September 2010. There were three sessions in each treatment, and 43 subjects in total participated.[2] A session lasted 30 minutes on average including 20 rounds of the mind game as well as a short questionnaire. On average, subjects earned 6 Euro. Instructions were read aloud to subjects in the waiting hall followed by a test of understanding (see Appendix A1 for the

[2] Four out of forty-three subjects cannot be included in the analysis: two self-reported confusion about the game and expressed disagreement with the answer to the understanding test in the post-experimental survey; another two had a same password which made it impossible to match their choices of sides to the die outcomes since the password was the only source of information for the matching.

instructions). Afterwards, they sat in front of separated computers with a paper outcome form and played the game at their own speed (see Appendix A2 for the outcome form and Appendix B for the screen shots). On the welcoming page online, they were asked to come up and fill in a personal password for picking up their payment in the secretaries' office one week later. In this way, anonymity from the experimenter was assured.

It is reasonable to expect that subjects hold the belief that the experimenter has no access to their minds in both treatments. The experimenter also has no possibility to make any statistical inference about either subjects' honesty or their success/luck during the experiment. This is partly achieved by the method of combining computer with paper and pen. To make any inference, the experimenter needs to know both the sides chosen and the die outcomes, which is only possible after subjects submit their outcome forms. Even when subjects submitted the outcome form at the end of the experiment, the experimenter also cannot make any statistical inference without going through the lengthy process of matching each outcome form to the die outcomes in the online database, since only "U" or "D" were written on the paper outcome form. In case subjects might fear statistical inference by the experimenter after the experiment, such fear can be assumed to be the same in both treatments. Nevertheless, the act of switching the steps, not the lying of the side, can in theory be caught but with great difficulty. Since subjects go through the entire experiment at their own speed for every step and every round, it is very difficult for the experimenter to eyeball if subjects follow these steps or not, unless the experimenter pauses the whole experiment for investigation. Even if the experimenter could have all of a sudden walked towards a certain subject during the experiment, all three steps would have been finished within a few seconds before the experimenter arrived. Still, in Write-first, subjects might have bigger fear of being caught in Write-first even without any supervision as the perceived risk of being caught matters more for behavior than the actual risk. This experiment does not rule out the possibility that the subtle rule manipulation of the order of steps leads to more effective governance by affecting the perceived risk of being caught without incurring actual cost of supervision.

Since the experiment is rather short, subjects might easily figure out the experimental purpose and behave in an unexpected manner. The unusual combination of computer and paper and pen might also lead subjects to over-contemplate on what the experimenter is after. To hide the focus on cheating, I included a truthful yet uninformative introduction of the experimental purpose (see Appendix A1). In short, subjects are informed that it is about "…individuals' successive responses towards outcomes that are randomly generated in an incentivized lottery setting…" The emphasis was put on the sequence of "Up" or "Down" chosen, rather than their earnings in the experiment. This way, it might be more natural that subjects were required to hand in a paper outcome form containing only the sides chosen in each round, UDUD… And it might alleviate the potential suspicion towards the mix and match method of computer and paper and pen. However, future experiments using a control treatment without the introduction can be run to find out the exact effect of such an introduction on cheating behavior. In the survey after the experiment, a few students expressed concern about the possibility to cheat in this experiment and warned the experimenter to upgrade the procedure to prevent fraud. For instance, one subject wrote, "there is one problem, I just think people can change their decisions after they see the result." Two students orally communicated a similar message to the experimenter after the experiment was over.

3. Results

Result 1a: *Subjects in Throw-first have significantly higher foresights* than those in Write-first.

There are a number of ways to infer the probability of cheating in the mind game. A straightforward approach is to test if subjects earn improbably more "4", "5", "6" than "1", "2", "3", as if they had "foresight" of the die outcomes when they chose the side. First, let f_{ij} be the indicator of lucky earnings which are higher than three for individual i in round j; let individual "foresight" $\mathrm{F_i}$ denote the average of lucky earnings f_{ij} over the 20 rounds of individual i:

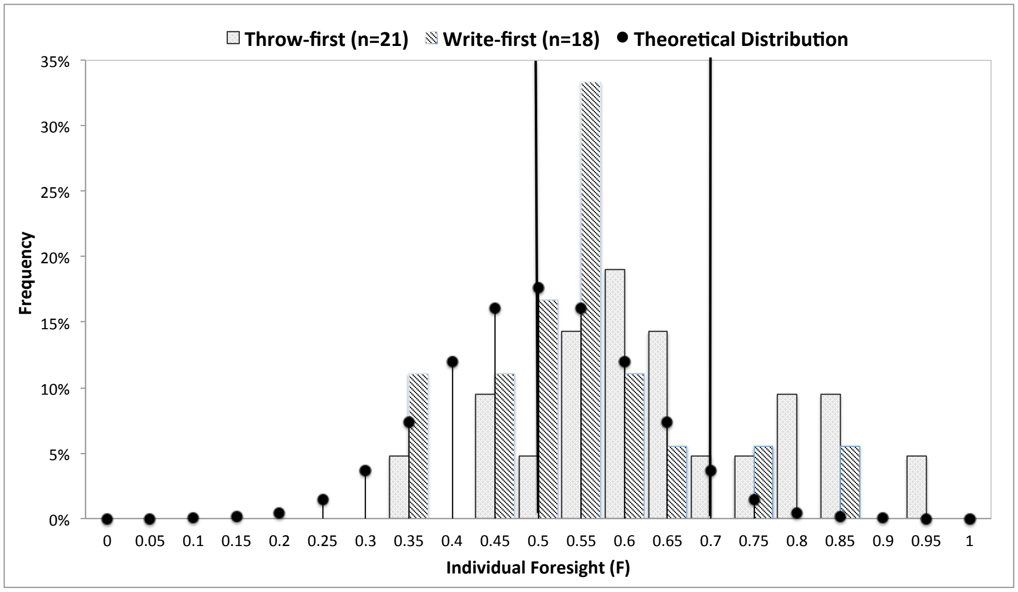
$$\mathrm{F_i} = \frac{\sum_{j=1}^{20} f_{\mathrm{ij}}}{20}, where\ f_{\mathrm{ij}} = 1 \text{ if earning} > 3;\ 0\ otherwise.$$

F_T is defined as the average F_i in Throw-first and F_w as the average F_i in Write-first. As the die is fair, under the null-hypothesis of no cheating at all, f_{ij} should be binomially distributed with 0.5 incidence of high earnings "4", "5", "6". Thus, inference of cheating can be drawn when the proportion of high earnings significantly deviates from the theoretical level of 0.5. It turns out that the hypothesis that there is no cheating at all at the treatment level can be rejected based on the one-tailed binomial test (Throw-first: 269/420, $p = 0.001$; Write-first: 197/360, $p = 0.041$). An alternative way to infer cheating is to test if subjects' average earnings are significantly higher than 3.5. Such an earning-based test also takes into account the size of the gain instead of purely the frequency. It turns out that the null-hypothesis that the average earnings are 3.5 can be rejected only in Throw-first (Wilcoxon Signed-rank test, $p < 0.000$), but not in Write-first ($p = 0.27$). Cheating in Write-first is not robust based on earnings. One possibility of not finding significant cheating based on earnings is that subjects might have tried to cheat marginally in gain to avoid being suspected by experimenters' eyeballing. In other words subjects might have restricted cheating only to rounds with low gains. Moreover, subjects in Write-first also seem to restrict cheating frequency in order to avoid a suspicious high foresight at the individual level. Based on independent observations of individual foresight F_i, it is only in Throw-first that the mean individual foresight is significantly higher than 0.5 based on the Wilcoxon Signed-rank test (Throw-first: $p = 0.001$, $n = 21$; Write-first: $p = 0.11$, $n = 18$).

Above all, subjects in Write-first cheat much less, if at all, as their foresights are significantly lower than those in Throw-first ($F_w = 0.55$ vs. $F_T = 0.64$) [two-tailed Mann-Whitney-Wilcoxon test (MWW) based on independent observations F_i, $p = 0.032$]. The treatment difference in cheating is robust using the earning-based measure (see Appendix C1).

An interesting question remains in what way subjects cheat more in Throw-first. Is the difference due to more subjects cheating in Throw-first or subjects in Throw-first cheating to a bigger extent? As partly depicted in Figure 1, although no one has the foresight level of 100%, Throw-first features a higher proportion of statistically identifiable "cheaters" (33% vs. 11%, Fisher's Exact test, $p = 0.1$), defined as those who exhibit improbably high individual foresight ($F_i \geq 0.7$) (one-tailed binomial test, 14/20, p = 0.058).

Figure 1**:** Distributions of Individual Foresights

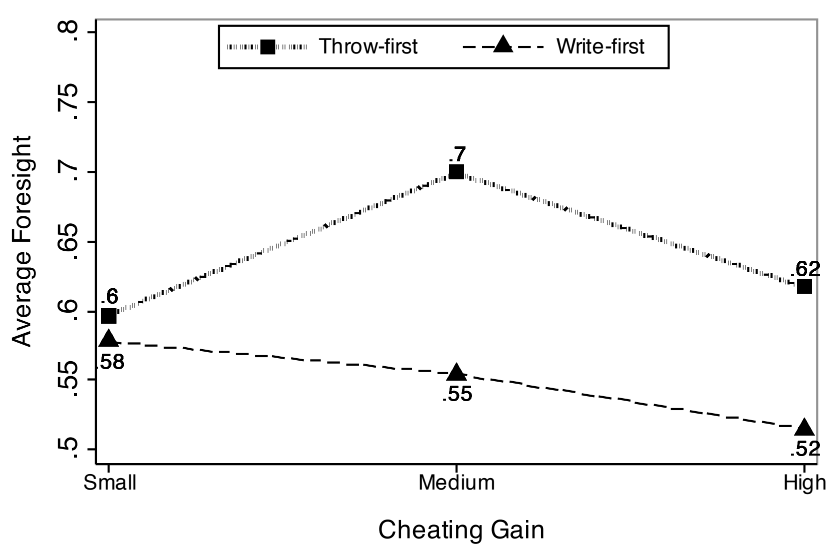


However, in the light of the "small cheating" found in Mazar et al. (2008), if subjects sometimes cheat so little that cheating is not statistically inferable at the individual level, it is then still possible that there are as many subjects who cheated in Write-first except that they only cheated to the extent that the foresight remained below the 0.7 cheater threshold and hence could not be detected by statistical inference as cheaters as if in disguise. One piece of evidence that points to the potential existence of cheaters in disguise is that the proportion of subjects whose foresights are at least 0.5 or above in Throw-first is not statistically different from that in Write-first (85% vs. 78%, FE, p = 0.4). This is mainly due to the spike at the level of 0.55 in Write-first, which might even suggest a small cheating threshold around 0.6 although more data is needed to confirm this. Overall, it points to the intricacy in the cheating patterns and the possibility that the difference in the order of play might have caused subjects to cheat more cautiously to avoid being suspected by statistical inference. As follows, *Result 1b* suggests that subjects in Write-first seem to have cheated more for small gain, if at all.

Result 1b: *Treatment difference of individual foresight is statistically significant only for medium gain, weakly significant for high gain and insignificant for small gain.*

Previous studies have shown slightly different results on the exact cheating patterns corresponding to different cheating gains. Gneezy (2005) uses a sender-receiver communication game and shows that lying increases with the gain from the lie. Similar findings are found in follow-up studies in which lying is explicitly hurting other experimental participants. As for studies in which lying is at the cost of the experimenter, Fischbacher and Heusi (2008) found that not all subjects cheat for the highest gain, although the highest gain is still more often reported than the second highest gain. In contrast, Mazar et al. (2008) showed that subjects cheat for the two lower gains, but not for the two higher gains. Shalvi et al. (2011) shows that subjects avoid both major and minor lies. In this experiment, the cheating levels seem to differ both across and within treatments, corresponding to cheating gains of 1, 3 and 5, as depicted in Figure 2. The treatment difference in foresight is mainly driven by the behavioral differences in cheating for medium gain as the individual foresight difference is insignificant for small gain (MWW, $p = 0.39$), significant for medium gain ($p = 0.02$) and weakly significant for high gain ($p = 0.1$).

Figure 2: Average Foresight and Cheating Gains



While significant cheating can only be found for small gains in Write-first, it is found for all levels of gains in Throw-first with its peak for medium gains. In Throw-first, the null hypothesis of no cheating is rejected for all levels of gains at the group level (one-tailed binomial test, small gain: 83/139, p = 0.014; medium gain: 107/153, p < 0.001; high gain: 79/128, p = 0.005). Moreover, the average foresight level is at its peak at the medium gain and higher than that at the small gain with weak significance [Logistic regression with f_{ij} as dependent variable, Wald chi2(1) = 3.3, p = 0.07, z =-1.82, Standard Error adjusted for 21 clusters], and statistically insignificant when compared with high gains (p = 0.11). This indicates that some subjects might have avoided minor lies, partly confirming the pattern shown in Shalvi et al. (2011). In Write-first, the null hypothesis of no cheating at the group level is rejected in and only in the case of small gain when die outcomes are 3 and 4 (one-tailed binomial test, 63/109, p = 0.06), consistent with the "small cheating" pattern found in Mazar et al. (2008). However, the probability of f_{ij} = 1 for small gain is not significantly higher than for either of the other two gains in within-treatment bilateral comparisons (logistic regressions with f_{ij} as dependent variable). Robustness check with more data is necessary to confirm that this pattern was not due to chance, though unlikely.

Since subjects play the game repeatedly twenty times, the question remains whether the difference in cheating patterns is robust over time. Do they cheat differently in the latter rounds after gaining more experience with the game? It turns out that while the foresight levels remain the same in the first half and in the second half in Throw-first (Wilcoxon Signed-rank test, 0.63 vs. 0.65, p = 0.58), foresight level drops in the second half in Write-first but not statistically significant (0.58 vs. 0.51, p = 0.21). Treatment difference in foresight is however significant in the second half (MWW, p = 0.038), and not in the first half (p = 0.245). Moreover, the distinct cheating patterns regarding different gains are also more pronounced in the latter rounds (see Appendix C2 for details on the time trend of cheating patterns). All in all, the result seems to confirm a stronger treatment difference in cheating behavior over time.

Overall, the extent of cheating seems to differ depending the size of gain. In Treatment Throw-first, subjects cheat the most for the medium gain. In Write-first, if it was not due to chance that cheating only occurred for small gain, then it is possible that some subjects intended to cheat but changed their mind and refrained from cheating when the die outcomes yielded a medium or high gain. However, given the few cases of cheating in Write-first, future experiments with a bigger sample size should be run to check the robustness of this pattern. If the result turns out to be robust, an important implication would be that some subjects forego cheating in some rounds, even though they would have already broken the prescribed rule of the order of play and postponed writing down the side. This implication would cast doubt on the "rule-breaking aversion *per se*" explanation as the main drive behind the treatment difference. If subjects cheat less in Write-first because they want to break fewer rules, then they should simply switch the order of the steps less often and always cheat regardless of the level of gain. Rather, this result would then be more in line with the explanation that subjects cheat less in Write-first because their moral self-image is hurt by the additional deliberate act of switching the order of step. Foregoing cheating in the face of high cheating gain (or only cheat for small gain) can help reconcile subjects' moral image and the desire to cheat (Mazar et al., 2008). Meanwhile, it raises the interesting question of whether a more deliberate cheating process would potentially require more or less cognitive efforts in resisting the cheating temptation.

Result 2: *The honest in Throw-first spend more decision time than those in Write-first.*

Result 2 is based on reaction time data, assuming that more reaction time indirectly indicates more cognitive efforts exerted (e.g., Rubinstein, 2007). Since subjects go through the exact same steps, it allows us to draw inferences based on the decision time about the cognitive efforts required for subjects to stay honest. Would the exerted cognitive efforts differ in the two variants? According to Greene and Paxton (2009), the cognitive efforts exerted should not differ since their findings suggest that honesty results from the absence of temptation rather than the active resistance of temptation. Notwithstanding the response time's general tendency to be noisy and the limited sample size in this experiment, I find that non-cheaters (as defined earlier) in Throw-first spent on average 15 seconds per round, which is significantly more than the average of 11 seconds spent by the non-cheaters in Write-first based on both non-parametric test (MWW, $p = 0.06$) and a linear regression with standard error adjusted for 31 clusters [F(1, 30) = 6.08, $p = 0.02$]. The result seems to suggest that subjects struggle more to be honest in Throw-first, potentially because the harm on the moral image, if one cheats, is not sufficiently big to induce more clear and active cheating resistance.

So far, all the results are in line with the moral image explanation. Nevertheless, the somewhat puzzling and intricate cheating patterns triggered by the small variation in the rules of the game call for further investigations on the underlying psychological processes. The subtlety of the manipulation also calls for caution in the potential fragility of the result. As mentioned above, future experiments can be run to systematically investigate the effect of the additional introduction for hiding the focus on cheating.

It also remains an interesting topic to disentangle the effects of subjects caring about self-image or the image perceived by others. Moreover, it could be the case that people simply care about their moral image rather than having an intrinsic cheating-averse preference and thus might be even willing to lie about their lying. When subjects were asked how they decide about the side in the survey, deciding randomly or deciding ahead for a few rounds or all rounds, about two-third in Write-first and half in Throw-first reported deciding ahead, appearing to have played the game honestly. As it turns out, cheating was found among those who reported "deciding ahead" in both variants (F_W= 0.554, F_T= 0.63), but not among those "deciding randomly" in Write-first. Economic major and gender also seem to play a role on the overall cheating propensity. Male participants (n = 19) have higher individual foresights on average than those of female participants (n = 19) (0.64 vs. 0.56, MWW, $p = 0.07$). Economic and econometrics students (n = 14) have higher individual foresights than those of the non-economics students[3] (n = 24) (0.66 vs. 0.56, MWW, $p = 0.08$). Moreover, the gender effect is only robust among non-economics students ($p = 0.03$) and the Economics major effect is only robust among female participants ($p = 0.08$). But given the small sample size, more data are required to make more general claims.

[3] It includes 17 students majoring in finance, accounting and other business majors as well as 6 in information management and 1 in sociology.

2.4. Conclusion

By exploiting two novel variants of the mind game, I show in this paper how the variation in the mere order of play, which makes the cheating process more or less deliberate, significantly affects cheating behavior. While subjects in Throw-first only need to lie about the choice made purely in the mind, those in Write-first need to play the game in a different order than the one prescribed in order to cheat. The overall results suggest that cheating in Write-first is curbed by the required act of changing the order of play. Subjects not only cheated less frequently, they also most likely have foregone cheating in the face of medium and high cheating gain. The most plausible explanation of the results is that one's moral image is more hurt in Write-first because the deliberate choice of changing the order of play implies that subjects are taking a more active role in their cheating and thus more responsible for the cheating outcome. An alternative explanation is that subjects in Write-first fear the detection of switching the order of steps and as a result reveal their intent to cheat. The reaction time results further suggests that the additional deliberate act required to cheat in fact helps subjects in reducing the cognitive efforts to stay honest and resist the cheating temptation.

As a policy implication, my results suggest that sometimes even small changes in the rules, like the order of the instructed steps, are enough to render cheating more effortful and the intent to cheat more clear, and in turn prevent people from cheating. For instance, imagine a small reform in the tax filing procedure. Instead of letting citizens file their taxes only once a year all together, make it obligatory that they report a taxable item within a month after the event occurs. To prevent any cumbersome procedure, these periodically self-reported paper or electronic files only need to be dated and signed, and at most submitted online in a personal taxation account, without the need of any immediate attention in filing it for calculating tax return. This way, for each item one evades, one would have to purposefully break the rules of filing it on time, which would reveal their intent to evade taxes and even in a repeated manner if they evade multiple items. Just the mere rule of requiring the act of noting down a tax relevant item when it occurs is sufficient to make any error or omission less likely, and if occurred, the intent to cheat more transparent. This could potentially eliminate the type of tax evasion that relies on the self-deceptive excuses of errors resulting from memory fuzziness or multiple items handling.

Lastly, as the mind game is unobtrusive, easily implementable and yet versatile, it enables a wide range of investigations on intricate cheating patterns and can serve as a useful tool for future research on cheating.

References

Ariely, D., 2012. *The Honest Truth about Dishonesty.* New York: Harper Collins.

Benabou, R., Tirole, J., 2002. Self-Confidence and Personal Motivation. *The Quarterly Journal of Economics, 117*(3), 871-915.

Bucciol, A., Piovesan, M., 2011. Luck or Cheating? A Field Experiment on Honesty with Children. *Journal of Economic Psychology 32*(1).

Charness, G., Dufwenberg, M., 2006. Promises and Partnership. *Econometrica,* 74(6): 1579-601.

Erat, S., Gneezy, U., 2012. White Lies. *Management Science,* 58(4) 723-733.

Fischbacher, U., Heusi, F., 2008. Lies in Disguise: An Experimental Study on Cheating. *TWI Research Paper Series (40).*

Falk, A., Fehr, E., Fischbacher, U., 2008. Testing theories of fairness - Intentions matter. *Games and Economic Behavior,* 62(1), 287–303.

Gneezy, U., 2005. Deception: The Role of Consequences. *American Economic Review,* 95, 384-394

Greene, J. D., Paxton, J. M., 2009. Patterns of Neural Activity Associated with Honest and Dishonest Moral Decisions. *PNAS,* 106(30), 12506-11.

Hao, L., Houser, D., 2010. Honest Lies. *GMU Working Paper in Economics No. 11-16.*

Mazar, N., Amir, O., Ariely, D., 2008. The Dishonesty of Honest People: A Theory of Self-Concept Maintenance. *Journal of Marketing Research,* 45(6), 633-644.

Shalvi, S., Handgraaf, M. J. J., De Dreu, C. K., 2011. Ethical Maneuvering: Why People Avoid Both Major and Minor Lies. *British Journal of Management,* 22: S16–S27.

Rubinstein, A., 2007. Instinctive And Cognitive Reasoning: A Study Of Response Times. *The Economic Journal,* 117, 1243-1259.

Appendix A1. Experimental Instructions

Introduction:

It is known that human beings seem to expect dependencies between successive events in spite of the fact that they know that the events occur independently of each other. Individuals' successive responses tend to be mutually dependent towards outcomes that are randomly generated. This experiment studies individuals' successive responses towards outcomes that are randomly generated in an incentivized lottery setting.

Payment：

At the beginning of the experiment, you will be asked to come up with a personal password for the sake of the payment. As of 20th September, you can pick up your payment by presenting your password to the secretaries of Department of Economics in K412. At the end of the experiment, you will be also asked to fill in a short online questionnaire in which you have to fill in the same password as the one for the experiment in order to guarantee an additional reward for the questionnaire.

Die-throwing game: Instructions

You are about to play a die-throwing game. In this game, you can throw a virtual online die 20 times for earning points. All resulting points will be exchanged to Euros. Every point you earn is equivalent to 5 Eurocents.

The die has six sides and each side has a different number of dots. The pairs of numbers add up to 7 on the opposite sides: 1 vs. 6, 2 vs. 5 and 3 vs. 4, and vice versa. We call the visible side facing up of any dice you throw the up side "U" and the opposite invisible side which faces down the down side "D". For instance, if the up side is 1, the down side of the die is 6 (See the table below for all the sides.)

U	1	2	3	4	5	6
D	6	5	4	3	2	1

In each round, the number of points that you score, depends on the throw of the die as well as on the side that you have chosen in that round. Each round consists of one throw. Before throwing, you have to choose the relevant side for that round. Note that the die outcomes are random and the outcome you see on the screen corresponds to the upside. We will explain you the detailed procedure of each round in steps:

[Treatment Throw-first]

Step 1: before every throw, make a choice of side in your mind between "U" (Up) and "D" (Down).

Step 2: throw the virtual die by clicking on the "Throw Die" button on the screen.

Step 3: fill in the side ("U" or "D") you have chosen in your mind in Step 1 in the outcome form.

[Treatment Write-first]

Step 1: before every throw, make a choice of side in your mind between "U" (Up) and "D" (Down).

Step 2: fill in the side ("U" or "D") you have chosen in your mind in Step 1 in the outcome form.

Step 3: throw the virtual die by clicking on the "Throw Die" button on the screen.

On the basis of the outcomes in step 2 stored on our web server and the corresponding side filled in the outcome form, your final earning points will be calculated and converted to Euros as your payment. For instance, if you have chosen "D" in your mind and the die outcome turns up to be "4", your earn 3 points for that throw, whereas if you have chosen "U" in your mind, you earn 4 points.

Test questions:

1. If you chose side "D" in your mind and the die outcome turns up to be "5", how many points do you earn?_____________
2. If you chose side "U" in your mind and the die outcome turns up to be "6", how many points do you earn?_____________

Appendix A2. Outcome Forms

Please fill in your personal password for picking up your payment: ___________.

Please repeat the following three steps for each of the 20 Throws:

[Treatment Throw-first]

Step 1: before every throw, make a choice of side in your mind between "U" (Up) and "D" (Down).

Step 2: throw the virtual die by clicking on the "Throw Die" button on the screen.

Step 3: fill in the side ("U" or "D") you have chosen in your mind in Step 1 in the outcome form.

[Treatment Write-first]

Step 1: before each throw, make a choice of side in your mind between "U" (Up) and "D" (Down).

Step 2: fill in the side ("U" or "D") you have chosen in your mind in Step 1 in the outcome table.

Step 3: throw the virtual die by clicking on the "throw die" button on the screen.

	T_1	T_2	T_3	T_4	T_5	T_6	T_7	T_8	T_9	T_{10}	T_{11}	T_{12}	T_{13}	T_{14}	T_{15}	T_{16}	T_{17}	T_{18}	T_{19}	T_{20}
The Side "U" or "D"																				

When you are finished with all the steps, please put the outcome form in the envelope and open the website under Internet Explorer's favorite menu - "a short questionnaire". When you are finished with the questionnaire, please turn off all IE windows and inform the experimenter.

Appendix B. Screen Shots

Treatment Throw-first

Screen Start:

If you are ready, please start the game by filling in your personal password: ________

Screen 1:

Round X

If you have chosen the side ("U" or "D") in your mind, please click on the button below:

Throw the Die

Screen 2:

The die outcome for this round is:

Please fill in the side on the outcome form and click on the button below:

Next Round

Screen Final:

You have finished all the 20 rounds. You can proceed by clicking on "a short questionnaire" under the IE "Favorites" menu.

Treatment Write-first

Screen Start:

If you are ready, please start the game by filling in your personal password: ________

Screen 1:

Round X

If you have chosen the side (“U” or “D”) in your mind, please fill in the side on the outcome form and click on the button below:

Throw the Die

Screen 2:

The die outcome for this round is:

Next Round

Screen Final:

You have finished all the 20 rounds. You can proceed by clicking on "a short questionnaire" under the IE “Favorites” menu.

Appendix C. Additional Data Analysis

C1: Earning Measure

One potential concern of the foresight measure is that it only takes into account the cheating frequency, but not the cheating gain. For instance, though subjects cheat more frequently in Throw-first, it is still possible that subjects cheat for the same amount of points in both treatments if subjects in Throw-first would only cheat for the low gain and those in Write-first would only cheat for the high gain. Thus, I use a second measurement related to total earnings. Since subjects are randomly exposed to cheating opportunities of earning five, three or one points corresponding to different die outcomes, the actual realization of die throws over the 20 rounds is not the same across subjects. To capture the proportion of the actual earning relative to the maximal possible earning with cheating given the individual's draw of die throws, the earning measurement is defined as the aggregate of the actual earnings per round r_{ij} normalized over the difference of the aggregate maximum earnings $\overline{r}_{\mathrm{ij}}$ (when $\mathrm{F_i} = 1$) and minimal earnings $\underline{r}_{\mathrm{ij}}$ (when $\mathrm{F_i} = 0$):

$$\mathrm{R_i} = \frac{\sum_{j=1}^{20} r_{\mathrm{ij}} - \sum_{j=1}^{20} \underline{r}_{\mathrm{ij}}}{\sum_{j=1}^{20} \overline{r}_{\mathrm{ij}} - \sum_{j=1}^{20} \underline{r}_{\mathrm{ij}}}, where\ \underline{r}_{\mathrm{ij}} = \left(r_{\mathrm{ij}} \middle| f_{\mathrm{ij}} = 0\right);\ \overline{r}_{\mathrm{ij}} = \left(r_{\mathrm{ij}} \middle| f_{\mathrm{ij}} = 1\right).$$

$\mathrm{R_i}$ is between 0 and 1. Without cheating, $\mathrm{R_i}$ is also expected to be 0.5. The results show that Throw-first again exhibits improbably high earning levels ($\mathrm{R_T} = 0.64$) based on the individual earnings (Wilcoxon signed-ranked test, $p = 0.001$, $n = 21$). Subjects in Throw-first also cheat for more points than those in Write-first based on $\mathrm{R_i}$ [Mann-Whitney-Wilcoxon (MWW) test, $p = 0.055$). Write-first, however, does not exhibit significant cheating ($\mathrm{R_W} = 0.54$, Wilcoxon signed-ranked test, $p = 0.139$, $n = 18$). This is consistent with the results based on foresight measure at the individual level.

C2: Time Trend of Foresight

As depicted in Figure C1, the treatment difference in foresights is most pronounced in the last five rounds (15-20) regardless of the level of the cheating gain. While Write-first displays persistently lower foresight when the gain is medium throughout all rounds and more so in the last ten rounds, it displays a much lower foresight when the gain is high only in the last five rounds.

Figure C1: Time Trend of Foresight by Cheating Gain