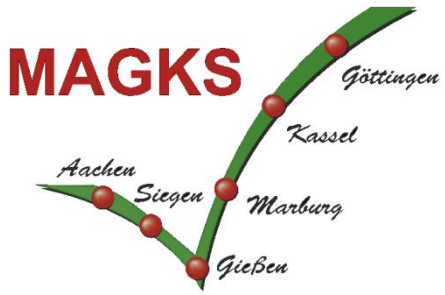

Joint Discussion Paper Series in Economics

by the Universities of

Aachen · Gießen · Göttingen
Kassel · Marburg · Siegen

ISSN 1867-3678

No. 26-2023

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Structuring Integrity: The Impact of Form Partitioning on Honesty in Self-Reports

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November 20, 2023

Abstract

This study investigates the impact of visual and content-wise partitioning on the truthfulness of self-report forms. In all treatments of this experiment, participants have to report how many correct predictions they have made for five coin tosses. The only difference between treatments is the design of the report form. Contrary to certain literature, findings reveal that less partitioned or bureaucratic forms correlate with increased honesty. With its concise yet insightful results, this study is relevant for policymakers and researchers in designing self-reporting structures when honest reporting is essential.

1 Introduction

It is said that good institutions that restrict cheating and encourage rule compliance are the pillars of a thriving economy [Besley and Persson, 2011]. Yet, there exist many situations where institutions must rely on individual honesty. Thus, in behavioral and experimental economics, honesty and trust have occupied central positions, influencing not only individual but also organizational decisions as well as the overall functioning of societies. Honesty and trust are shown to be correlated with increased collaboration, enhanced performance, proactivity and economic growth [Dirks and Ferrin, 2001, Gächter et al., 2004, Hugh-Jones, 2016, Gächter and Schulz, 2016].

However, giving too much trust without proper monitoring can lead to increased dishonest behaviors and frequently result in a gradual decline in ethical standards [Becker, 1968, Kirchler et al., 2008]. On the other hand, even with a no-trust policy signaled by sizable punishment accompanied

by close and often costly monitoring, it does not necessarily lead to more honesty [Sánchez-Pagés and Vorsatz, 2007, Peeters et al., 2013]. As a result, fostering both trust and honesty remains a challenging task.

[Rilke et al., 2016] proposed that self-reporting policy can be a useful tool for policymakers to convey their trust by taking self-reports at face value. This trust, in turn, can lead to more honest behaviors. Aside from ethical concerns, cost-effectiveness is also a matter of consideration, as pointed out by [Jiang, 2013], rule enforcement does not have to be costly and subtle rules can significantly affect dishonest behaviors. By implementing a self-reporting policy, organizations do not entirely forfeit its monitoring ability. On the contrary, policymakers can still convey their degree of trust by altering the frequency of self-reports (e.g. hourly self-report, daily self-report).

There are, however, only a few studies directly investigating the effect of self-report frequency on dishonesty. Namely, [Rilke et al., 2016] showed that a one-by-one reporting policy could lead to more dishonesty compared to a more trusting all-at-once policy. Participants in their experiment were asked to play a 20-question trivia game. Instead of answering the questions directly, the answer for each question was revealed first before participants had to report whether that was the answer they had in mind. Participants earned money every time they reported they made a correct answer. As a result, participants can cheat [1] to earn money, experimenters have no mean to detect dishonesty on an individual level. In treatment with a one-by-one reporting policy, participants were presented with each question separately. In contrast, all 20 questions are presented simultaneously on the same screen in the all-at-once policy. In both treatments, participants had to report the answer to each question. [Rilke et al., 2016] theorized that since each dishonest act in their study is small and easily forgotten, it is harder for participant to resist the temptation to cheat, especially in the one-by-one policy. Additionally, the step-by-step representation of the one-by-one report form makes it easier to slip into a habit of cheating, i.e., the slippery slope effect. Once the participants give in to cheating, they cheat a lot more. However, this study primarily examined the effect of visual representation of report form or procedure on ethicality as participants in all treatments must answer each question individually. Note that by design or unintentionally, when altering the frequency of the report form, it is often comprised of not only visual or temporal but also content-wise segregation. For exam-

[1] although there are differences between what is considered as cheating and lying in the literature since there is no need for such distinction in this paper, I use the term synonymously

ple, what will happen if the participants are asked to report the aggregate total number of correct answers instead of pointing out each correct answer individually?

A content-wise manipulation of the self-report form can be found in one of the studies by [Desai and Kouchaki, 2015]. In this study, a researcher called 94 randomly selected garages near the Boston (USA) area for a price quote on changing a car's brake pads. In one treatment, the researcher asked for the total cost of the job, while in another, a comprehensive cost report with a separate cost of labor and replacement parts. The finding showed garages are less likely to overbill clients when asked to provide a more comprehensive cost report. The authors conjecture that when asked to provide an estimated cost for the labor and the parts separately, mechanics feel more accountable as part of their work can be checked.

From a macro point of view, visual cues and content of self-report form can substantially affect specific types of dishonest participants, driving them to behave more honestly or dishonestly. [Pascual-Ezama et al., 2020] found different profiles of dishonest participants typically found in dishonesty experiments. Namely, there are participants who (a) skip experimental procedure, (b) do not skip but lie about the outcome or (c) other deliberate rule violations include throwing the coin multiple times instead of once. In a more visually unpartitioned form, participants with profile (a) might skip the long list of answers and report the aggregated outcomes. Thus, the erosion or slippery slope effect is likely negated for participants with profile (a), making the report look more honest than the report of a partitioned form.

The frequency of self-report forms is also relevant from the academic perspective when measuring dishonesty. The majority of non-strategic paradigms in honesty studies asked participant to self-report their outcomes, such as reporting the result of a coin-flip [Bucciol and Piovesan, 2011], a die roll [Fischbacher and Föllmi-Heusi, 2013] (see [Gerlach et al., 2019] for a more comprehensive overview of strategic and non-strategic honesty research paradigms). However, how the report questions are presented to the participants is often considered trivial. One example of changes could be an innocent shift from an on-paper to an on-screen experimental session. Despite the same experimental setup, the on-screen session often has a more extended page (as participants can scroll down the screen). On-paper experiments often have more segregated content due to the physical limitation of the sheet of paper. Additionally, experimenters can inadvertently decide to use a less comprehensive reporting form to fit into the sheet of paper or to shorten the time it takes to complete the report form in subsequent studies or replications. As shown in this research, these slight modifications

can affect the results of experiments employing multiple die-roll or coin-flip paradigms.

As a result, there is a need to investigate the visual aspects and the comprehensive aspects of report forms. In order to achieve this goal, this study compares four different treatments that vary the partitioning level of a self-report form in two main factors with which outcomes of 5 coin tosses are reported: visual and comprehensiveness.

The first factor "visual" is binary in that partitioned visual means participants see report questions sequentially in different screens, also referred to as *"unbundled"*. On the opposite, unpartitioned visual means participants see report questions all at once on a single screen (*"bundled"*). The result suggests that participants in treatments with more visually segmented self-report forms cheat more. This result aligns with the abovementioned evidence by [Rilke et al., 2016].

The second factor "comprehensiveness" is also binary in that a high level of comprehensiveness requires participants to report their outcomes one by one, *"with detail"*, whereas, low level of comprehensiveness means only the aggregated outcome is reported, "*without detailed*". Similar to the result of visual partitioning, outcomes in self-reported form that require more report details (more segmented) are more likely to be dishonestly inflated. This result contradicts the abovementioned result from [Desai and Kouchaki, 2015]. However, it is problematic to directly compare offering price quotes to other self-report tasks of employees in a firm or participants in laboratory settings. As also pointed out by [Desai and Kouchaki, 2015], their study was conducted in a naturally occurring field setting, additional factors, such as the publicity of the price estimate, can influence the result.

Overall, the results of this paper provide evidence for what I call *"partitioning effect"* of self-report form on honesty. The results of this paper also contribute to a broader literature investigating subtle and non-invasive changes in reporting protocol that can influence honesty. Papers that investigate the influence of subtle interventions on dishonesty include [Jiang, 2013], which show that subtle rule changes have a significant impact on cheating behaviors; [Conrads and Lotz, 2015] on communication channels; [Rahwan et al., 2019] on the dishonesty of bankers when thinking about their jobs.

2 Experiment

This lab-in-field experiment was conducted in the top 129 infrastructure high schools in Ecuador, in conjuction with the "Showing Life Opportunities" Project[2] during the initial baseline survey in 2019. A total of 13748 participants (M_{age} = 16.19, SD_{age} = 2.63, 51.29 % females) participated in the experiment. The study was realized through an online learning platform called SmartSparrow in computer classrooms during school hours. Before starting the experiment, participants took part in an extensive general survey ranging from demographic variables to socioeconomic background and personality traits. They were then faced with a block of economic experimental tasks, the first task of which was the present experiment. Before commencing this block, participants were asked not to communicate with anyone except their instructors. The order of the experimental tasks in this block is the same for all participants.

In this study, each participant had to follow a simple decision task. S/he could earn money by flipping a virtual coin 5 times sequentially. Before flipping each virtual coin, the participants were asked to predict the outcome of the coin, whether it would be a head or a tail. The participants were requested to make the predictions in their minds without communicating with anyone. They can click on the virtual coin to simulate flipping it. A short animation is played after clicking on the coin on the screen. This virtual coin is provided by an unaffiliated third-party external website that does not collect data on the coin flip. As a result, participants can flip a coin as many times as they wish in all treatments, however, they were informed that the first coin flip starts after the first click on the coin (excluding the transition animation). Each correct prediction rewards the participant 0.2 US dollar[3]. Accordingly, participants could earn an amount between 0 and 1 US dollar. Since this method does not allow us to observe cheating individually directly, the distribution of the reported outcomes is compared across different treatments and with the expected (fair) distribution.

Each participant participated in only one treatment group. Treatments were randomized on an individual level instead of a classroom level in order to maximize statistical power. I specifically employed the coin-flip paradigm due to its simplicity in comparison to the die-rolling paradigm [Fischbacher

[2]See https://doi.org/10.48529/e27k-br04

[3]Ecuador's minimum monthly wage in 2019 was 394 US dollars. The actual payment for each participant is randomly selected from one of the experiments in the economic experiment block. Incentives are kept similar between experiments based on the time required to complete the tasks.

and Föllmi-Heusi, 2013], see [Abeler et al., 2014, Conrads and Lotz, 2015] for similar coin-flip constructs. After reading the instructions, participants are asked to answer 2 control questions to ensure they understand the instructions correctly. If answered incorrectly, an explanation prompt is displayed on the screen. Participants can only proceed if they answer the control questions correctly. The experimental manipulation is implemented only on the report of correct predictions. I follow a basic 2 by 2 factorial design with 2 main effects: *detailed* (with or without) and *bundled* (with or without), also refer to as *comprehensive partitioning* and *visual partitioning* respectively.

In the first level of the *detailed* main effect, *"with detailed"*, participants must report whether their prediction is correct for each coin flip. Treatment *"without detailed"* means participants must only report how many correct predictions they had made out of the 5 coin flips.

In the first level of the *bundled* main effect, *"with bundled"*, report questions are displayed on one screen. There is no clear separation between the tosses. Treatment *"without bundled"* means each toss is visually displayed on its own, having its own screen. This separation is made salient for the participants by requiring them to click the *Next* button in order to proceed to the next screen. To ensure participants understand the instruction (i.e., flipping a coin 1 time per screen), the instructions for each screen display a round counter starting from 1. The instructions on each screen also mention that only the first coin flip counts for each screen/round.

The experimental design and number of observations are summarized in Table 1, the experiment constitutes 4 treatment groups: detailed-bundled (*D-B*), detailed-without bundled (*D-woB*), without detailed-bundled (*woD-B*) and without detailed-without bundled (*woD-woB*). The baseline treatment of this experiment is the *woD-B* group. Participants must choose from a pre-made list of radio buttons instead of typing in a number indicating how many correct predictions in *"without detailed"* treatment groups.

Additionally, since the experiment is performed with a relatively large sample size, statistical differences not pertaining to the main treatment effect might be amplified [Lenth, 2001]. One of such trivial effects could be how strikingly different the appearance of the self-report forms is or how long of the text the participants must read between each form. As a result, treatments are kept with a relatively similar number of words and clicks to other treatments. See Figure 1 for an illustration of the experimental design.

It is also worth noting that for the treatment group *woD-woB*, which requires participants to go through each coin-flip screen-by-screen without giving details, participants must click on a tick-box indicating that they have made their prediction and flipped the coin before moving to the next

Table 1: Summary of treatments and number of observations

		Detailed	
		with	without
Bundled	with	D-B (2290, 51.4% female)	woD-B (4612, 51.82% female)
	without	D-woB (4582, 50.85% female)	woD-woB (2264, 50.97% female)

Note: Randomization into treatment groups are drawn from a normal distribution instead of a uniform distribution due to the built-in randomization behavior of the online learning platform.

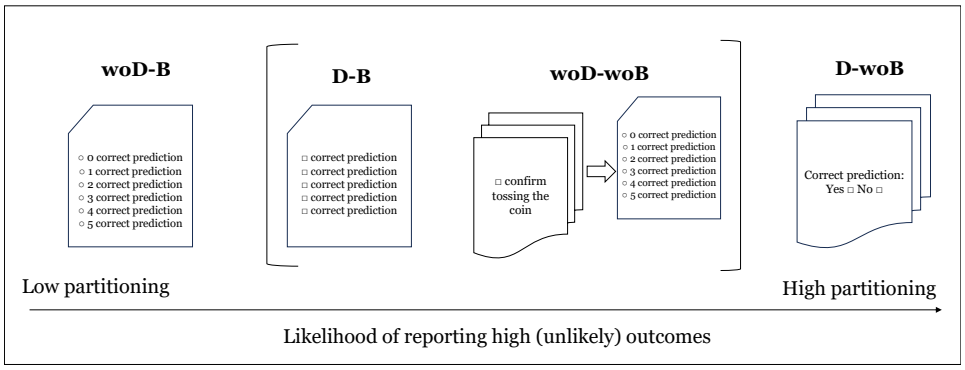

Figure 1: Illustration of the experimental design

coin flip. This is to confirm that participants actually landed on the page and supposedly performed their tasks before moving on since the experiment was delivered online.

3 Result

Over-reporting was prevalent in all experimental treatments. As seen in Figure 2, the distributions of the reported outcomes in 4 treatment groups are skewed to the right, indicating a tendency to report high outcomes. Both binomial tests and Kolmogorov-Smirnov tests show statistically significant differences (p-values < 0.01) between the truthful (fair) distribution and the distributions of reported correct predictions across most reported outcomes in all treatment groups. Despite this overall tendency of misreporting, the treatment effects can also be observed.

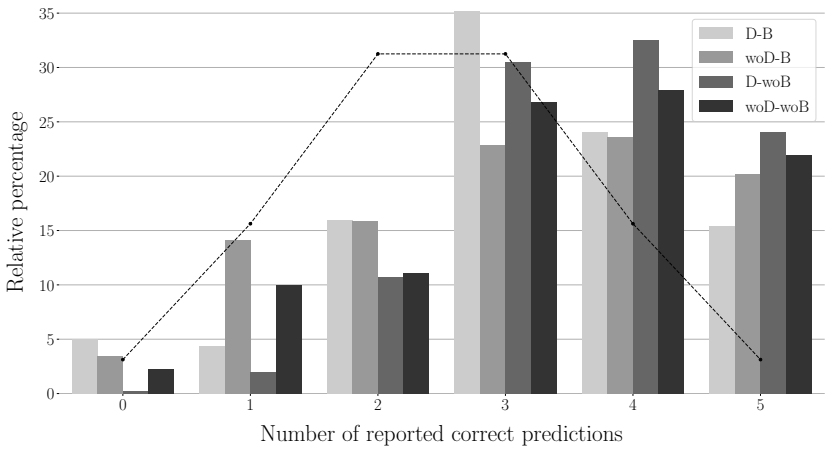

Figure 2: Percentage of reported outcomes by treatments.

Participants tend to report higher outcomes when the report is more partitioned (more detailed and/or more unbundled). Pairwise comparison between detailed (*D-B* and *D-woB*) and without-detailed groups (*woD-B* and *woD-woB*) differed significantly ($p < 0.001$, one-sided Mann-Whitney-U test). Similarly, reported outcomes in bundled (*D-B* and *woD-B*) and in without-bundled groups (*D-woB* and *woD-woB*) differed significantly from each other ($p < 0.001$, one-sided Mann-Whitney-U test). Interestingly, when testing for an order effect in the four treatment groups, I found partitioning effects on the number of claimed correct predictions ($p < 0.001$, one-sided Jonckheere-Terpstra). In other words, as the design of the report becomes more detailed and visually partitioned (less bundled), the more the number of high reported outcomes and the less the number of low reported outcomes (0, 1, or 2) (for summary, see Table 2).

Additionally, a point of interest in this study is the fraction of participants who report a maximizing outcome of "5". As hypothesized above, this willingness to report such an unlikely outcome (3.12 %) is partly driven by the design of the report form. The report of payoff maximizing outcome significantly increases as the report appears to be more partitioned (more detailed and more de-bundled) ($p < 0.001$, one-sided Jonckheere-Terpstra). Further pairwise comparison between the least partitioned group and the most partitioned group qualified this result, showing that *woD-B* and *D-woB* significantly differ from each other (20.16 % vs. 24.07%, $p < 0.001$, χ^2 test). Statistical differences are also observed between partially partitioned

Table 2: Overview of results.

Treatment	n	M	Relative percentage of reported correct predictions					
			0	1	2	3	4	5
D-B	2290	3.15	4.98	4.41***	15.98***	35.2	24.02***	15.41***
woD-B	4612	3.1	3.4	14.14***	15.83***	22.9***	23.57***	20.16***
D-woB	4582	3.65	0.26***	1.96***	10.69***	30.49	32.52***	24.07***
woD-woB	2264	3.34	2.25***	9.94***	11.13***	26.86***	27.92***	21.91***
JT Test			0.001***	0.001***	0.001***	0.923	0.001***	0.001***
Honest distribution			3.12	15.62	31.25	31.25	15.62	3.12

Note: n is the number of observations in each treatment group, M denotes the mean reported correct predictions by treatment and reported outcomes are represented as a share of total outcomes by treatment. *** indicates statistical significant at 1% level, ** at 5% level, * at 10% level, based on one-sided binomial testing in comparison to the honest distribution. Jonckheere-Terpstra Test refers to the one-sided p-values of the Jonckheere-Terpstra test for ordered alternatives by reported number with treatment as the independent variable. JT tests are done in descending order for outcome 0, 1, 2 and ascending order for outcome 3, 4, 5.

treatment groups, *D-B* and *woD-woB*, (15.41 % vs. 21.91%, p = 0.02, χ^2 test).

A closer investigation into these results using regressions also corroborates this result. Using ordered logistic regressions (Table 3, model 1-3), I find the statistically significant treatment effects on the likelihood of reporting high outcomes compared to the baseline treatment *woD-B*. More specifically, results for the *woD-woB* and *D-woB* groups, which are the 2 most distant groups in terms of partitioning level from the baseline *woD-B* (see Figure 1), remain significant and robust (Table 3, model 1-3). The closest group to the baseline, the *D-B* group, shows no significant results. The treatment effect for the *D-B* group seems to only concentrate of the maximum level of reporting"5" instead of the wider levels such "3","4" or "5".

Table 3, model 4-7 shows the probit regression results for the maximum reported outcome of "5" correct predictions. Interestingly, when controlling for gender, the effect of *woD-woB* treatment disappears. However, when put in comparison with the reported outcome of "4", which can be conceptualized as "partial lying" [Fischbacher and Föllmi-Heusi, 2013], the effect of *woD-woB* treatment is significant again. This suggests that compared to the baseline treatment, participants in *woD-woB* group are likely to report a more partially high outcome instead of the apparent maximum outcome. The same conclusion cannot be said for the *D-B* group from the baseline, as most participants tend to report a maximum outcome and not a partial one. In other words, *woD-woB* and *D-B* fall into one category between the two most distant treatment groups *woD-B* and *D-woB*. Their relative distance

between each other on the partitioning scale still needs to be discovered. For the most distant group *D-woB* comparing to the baseline, over-reporting is shown to be significant in all high levels, especially for both "4" or "5".

Table 3: Overview of regression results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
age	-0.018***	-0.021***	-0.023***		-0.055***	-0.049***	-0.048***	-0.040***
	(0.006)	(0.006)	(0.006)		(0.003)	(0.001)	(0.004)	(0.004)
isFemale	0.027	0.028	0.009		0.004	0.006	0.006	-0.049**
	(0.030)	(0.031)	(0.032)		(0.024)	(0.024)	(0.025)	(0.024)
D-B	0.041	0.045	0.046	-1.019***	-0.231***	-0.227***	-0.218***	-0.006
	(0.046)	(0.046)	(0.046)	(0.032)	(0.038)	(0.038)	(0.038)	(0.035)
D-woB	0.673***	0.684***	0.698***	-0.704***	0.083***	0.087***	0.095***	0.248***
	(0.038)	(0.038)	(0.038)	(0.020)	(0.029)	(0.029)	(0.029)	(0.028)
woD-woB	0.334***	0.336***	0.330***	-0.775***	0.010	0.015	0.020	0.110***
	(0.047)	(0.047)	(0.047)	(0.029)	(0.036)	(0.036)	(0.036)	(0.035)
preferenceRisk		0.089***	0.052***		0.005		0.006	0.026***
		(0.007)	(0.008)		(0.005)		(0.006)	(0.006)
preferenceTime		0.006	0.002		-0.002		-0.001	-0.004
		(0.006)	(0.006)		(0.005)		(0.005)	(0.004)
preferenceTrust		0.008	0.007		0.012**		0.013**	-0.009*
		(0.007)	(0.007)		(0.005)		(0.005)	(0.005)
sumAssets			0.009**				-0.016***	0.006*
			(0.004)				(0.003)	(0.003)
More controls	No	No	Yes	No	No	No	Yes	Yes
Observations	13,748	13,748	13,707	13,748	13,748	13,748	13,707	13,707

Note: Reference group is treatment "$woD - B$". Robust standard errors are shown in parentheses. More controls are answers to 28 personality-trait pre-survey including big 5. See Appendix for the full regression table. Models 1-3: Ordered logit estimates with reported outcome as dependent variable. Models 4-7: Probit regression with reported "5" as dichotomous dependent variable. Models 8: Probit regression with reported "4" as dichotomous dependent variable. *** indicates statistical significant at 1% level, ** at 5% level, * at 10% level

Lastly, I analyzed further potential socio-demographic and psychological traits that might affect higher-outcome reporting. Age showed a consistent negative and significant correlation on the likelihood of reporting higher outcomes, compared to the baseline level (see Table 3, model 1-3). This is in line with previous research on cheating behavior with young children [Bucciol, 2008], which shows that children cheat when cheating is profitable and they are not observed. However, the interpretation of this age effect must be taken into the context of this research project since the sample does not vary significantly in age ($M_{age} = 16.19$, $SD_{age} = 2.63$). Additionally, I found a significant positive effect of self-reported risk preference on the likelihood of reporting higher outcomes and the self-reported sum of household assets (see Table 3, model 2-3). Several other controls for personality traits also showed

significant results (see Appendix for more details). Despite these significant controls, the main treatment effects remained robust with a high statistically significant level. Overall, this result corroborates the hypothesis that (unlikely) high-outcome reporting is more prevalent as the self-reporting form becomes more (visually and comprehensively) partitioned.

4 Discussion and conclusion

The current study explored how various self-reporting policies can influence intrinsic honesty by employing a coin-flip task as a research paradigm. Although dishonesty manifested across all self-reporting policies, the degree of dishonesty varied based on the level of partitioning required by the report form on both channels: detail-wise and visual-wise. Specifically, as the report form becomes visually and comprehensively partitioned, the prevalence of extreme payoff-maximizing responses increases. In contrast, in laxer forms, there is a higher incidence of partial dishonesty (characterized by just slight overstatements of one's outcomes) recorded. These findings suggest that the perceived "instrinsic cost of dishonesty" can be influenced by the report policies, aligning with previous research on this topic ([Jiang, 2013, Desai and Kouchaki, 2015, Rilke et al., 2016]). Interestingly, age emerges as a significant variable in moderating the effect of over-reporting. Older participants are less inclined to report improbable high outcomes as reporting policy becomes more partitioned. This result also aligns with other meta-analytical findings by ([Gerlach et al., 2019]). However, contrary to this meta-analytical study, I find no significant correlation between gender and honesty, especially compared to other studies using the coin-toss paradigm.

Additionally, examining other socio-demographic and psychometric variables indicates that certain individual traits, even household wealth level, may correlate with the likelihood to report higher outcomes. A few particular variable of interest are the self-report level of trust and risk preference. Participants who reported that they are "unwilling to take risk" or "do not trust that people have only the best intentions" tend to refrain from over-reporting.

4.1 Shortcomings and directions for future research

As always, it is essential to exercise caution when extrapolating experimental findings. The "mind game" paradigm in this study is a rather particular boundary case where participants can be absolutely sure that there is no concern of exposure or punishment if they act dishonestly. There are arguably

few situations in real-world settings that this situation holds. However, it is a valuable tool for investigators to look into the pure intrinsic cost of dishonesty. Furthermore, compared to the "trivia game" paradigm [Schurr et al., 2012, Rilke et al., 2016] where participants have to claim that they know the answers to difficult trivia questions, the coin toss "mind game" used in this study has the advantage that the outcome is entirely determined by probability. Therefore, participants do not have to justify for their ability (or lack thereof) to complete the task. Thus, cheating behaviors, as observed in this study, might be slightly inflated (see [Kajackaite and Gneezy, 2017, Gerlach et al., 2019] for similar concerns). This raises the question of the validity of this result in wider settings in which detection chance and punishment for dishonest act is implemented. For example, consider the scenario where a reporter must fill out a highly partitioned form consisting of 5 distinct elements. In this case, each falsely reported element incurs an additive punishment. Now, let us compare this situation to an alternative scenario where the form is unsegmented but instead carries a fixed and severe punishment if a false element in the report is detected. Which of these two forms would deter dishonest reporting more?

While this research tried to vary treatments based on the level of form partition systematically, it has potential flaws. Combining with the relatively large sample size of the study, these flaws can manifest and influence the experiment results. One particular concern is the form with segmented visual and unsegmented report details (woD-woB), which, compared to other forms, requires participants to recall their results at the end once they have tossed the coin 5 times. Therefore, participants can use "poor memory" as a reason for choosing a high outcome, similar to "moral licensing". To mitigate this potential confound, participants are asked in all treatments to keep track of their correct prediction using fingers on their other hands (non-dominated) or other means (e.g., pen and paper), if they feel the need to do so. Of course, it cannot be ruled out that some participants still insist on using memory and make a recall mistake by doing so. This leads to an interesting future research question on whether or not participants intentionally strategize their reporting modes to preserve their self-image. For example, they could choose a highly segmented report form or insist on using their own abilities without external aid to make cheating easier for their self-image.

Lastly, it is important to acknowledge that the coin toss paradigm employed in this study does not offer explicit measures of cheating. We can only observe an implausible aggregate level of high outcomes reporters. Some participants indeed correctly predicted 5 coin tosses, for example. Never-

theless, it maintains a high level of external validity since individuals often lack direct knowledge of whether someone is engaging in dishonest behavior. One must instead rely on indirect cues. Furthermore, there is evidence that cheating in the mind game in the lab is statistically significantly correlated with cheating in the field [Potters and Stoop, 2016]. Future research may benefit from experimental tasks that allow to assess individual's dishonesty more directly, for example, the sender-receiver game [Gneezy, 2005]. This subsequent investigation could also explore whether individual differences in critical factors such as gender, psychological traits or other socio-economic variables that might moderate any observed effects, thus deepening our comprehension of the identified patterns.

4.2 Conclusion

Together, this research suggests that visual representation of the self-report form and the required report content play a role in promoting honesty. As the self-report form becomes more partitioned (both visually and comprehensively), cheating becomes more prevalent. Aside from the "slippery slope" effect, I conjecture that certain visual representations in the report form might also enhance small aspects of how the report content is generated, which in turn drives the report results. For example, in this study, isolating each coin toss into distinct sections emphasizes their independence, a statistically correct understanding. Conversely, grouping all coin tosses together in a single section may lead participants to perceive interrelated outcomes, potentially discouraging them from reporting excessively high numbers of correct predictions. Thus, decision-makers in organizations need to not only focus on the report's content but also on how it is delivered. Furthermore, careful consideration is necessary in designing self-report forms, as there may be an interaction between the report content generation process and how the report is actually formulated. From a theoretical perspective, this study reveals how an experimental paradigm is administered to subjects within the context of studying honesty, which significantly impacts the experimental outcomes. It is crucial to acknowledge that such an effect should not be dismissed as trivial. Such an effect could arise by shifting the experiment from paper to the computer screen, where there is generally more space to display content. On a more applied note, a trusting and less bureaucratic self-reporting process for content that is very difficult or costly to validate might lead to more honest behavior and, thus, can be cost-effective.

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