UNIVERSITY OF GOTHENBURG
DEPARTMENT OF PSYCHOLOGY

Effects of a pro-social default manipulation on subsequent cheating behaviour

Elin Edén

Master Thesis in Psychology,
30 ECTS
PC2541
Spring 2018

Supervisor: Lars-Olof Johansson

Effects of a pro-social default manipulation on subsequent cheating behaviour

Elin Edén

Abstract. This study aimed to examine possible spillover effects of a pro-social default manipulation on subsequent cheating (pro-self) behaviour. 67 participants completed a laboratory experiment, where they first did a time estimation and construction task, then were subjected to either a pro-social or a pro-self default donation choice after which they were given the opportunity to cheat in a lottery. A significant difference on donation choices was found between default groups, such that more participants donated their bonus in the pro-social default condition. No significant spillover effect was however found between groups. This may be explained by small sample size and low cheating behaviour overall. Suggestions for further research are larger sample size and a more easily rationalizable cheating task.

The choices people make on a daily basis have great impact on society, although they may not always be well thought through or rational. Long-term positive consequences are not always guiding decisions, as can be seen in decisions about diet, smoking, drinking, savings, environmental behaviour etcetera. One way of helping people make better decisions is choice architecture interventions, also known as *nudges* (Thaler & Sunstein, 2009). The term "choice architecture" refers to the structure in how a certain choice is presented to the person choosing, an intervention which can greatly affect which choice he or she makes. There are different types of choice architecture interventions, such as incentives, feedback, structure of complex choices and default alternatives (Thaler, Sunstein & Balz, 2013). A default alternative is a choice option that doesn't require any active choice. Research shows that if an alternative is presented as a default, people tend to choose this option due to minimal effort, implied endorsement and reference dependence (Johnson & Goldstein, 2013; Thaler, Sunstein & Balz, 2013).

A default alternative is effortless to choose, especially if the active choice of another alternative requires filling out and posting a form or some other time-consuming action. Another type of effort is having to consider different alternatives and form an active opinion about which choice is preferable. By accepting the default this effort can then be avoided (Johnson & Goldstein, 2013). One everyday example is that many people keep the default options for screen saver on their computers (Thaler, Sunstein & Balz, 2013). The same effect can be seen in more substantial choices such as organ donation, where an opt-out system renders a much higher donation proportion than an opt-in system. The effort of making the active choice to opt in as an organ donor seems to be too large, even for those having a preference to donate (Johnson & Goldstein, 2003).

Furthermore, the implied endorsement of a default alternative means that people tend to see the default as a recommended alternative by someone more competent, the default gives information about what the right thing to do is. For someone not having a strong opinion this may ease the choice (Johnson & Goldstein, 2013). The

recommendation is often appreciated, especially if the choice is complicated and difficult (Thaler, Sunstein & Balz, 2013).

A default choice can also serve as a reference point which frames the options and changes the value of them. The person choosing then acts as if he or she has already chosen the default value, and compares the other choice/s to the default. Since the endowment effect makes people value the things they already have higher than the things they do not have, the perceived value of the default option tends to be higher (Dinner, Johnson, Goldstein, Liu & Rogers, 2011). Since every choice is a trade-off between different values there will be some loss and some gain. By changing the reference the choice will be interpreted differently, and loss aversion may influence what the person chooses. When, for example, people are to choose whether to save for retirement, the default may be either an active choice or auto-enrollment. If an active choice is required, the enrollment is framed as a present loss for a future gain. But if the default is auto-enrollment, opting out of this is perceived as a future loss and a present gain. Loss aversion will make the default seem better regardless of which option it is (Johnson & Goldstein, 2013; Kahneman & Tversky, 1983).

Although nudging or choice architecture is well known and has been tried in numerous settings, there are still some aspects that are not as well studied. Marchiori, Adriaanse and Ridder (2016) suggested that more research is needed when it comes to transparency, choice set, consequences and freedom of choice. Nudges are often seen as separate interventions, but it is unclear if there are any spillover effects on subsequent behaviours. Therefore more research should focus on such potential consequences, if nudges are to be recommended as a policy tool for long term positive effects. Such spillover effects may include negative spillover due to self-licensing behaviour or positive spillover through changes in self-perception or habits.

Moral licensing is a phenomenon where a person first acts in a moral way, but then acts immorally in subsequent situations (Blanken, van de Ven & Zeelenberg, 2015). This seemingly inconsequent behaviour is understood as a feeling of having done your part, where your previous good behaviour entitles you to act more selfishly later (Mullen & Monin, 2016). An individual's moral behaviour is controlled by self-perception of one's own moral standards. A good deed strengthens the feeling of morality while an immoral act weakens this feeling. When the self-image is threatened, such as after an immoral act, the tendency then is to act morally. When the individual has already acted morally however, the self-image is not threatened but strengthened and he or she can then justify acting less moral (Truelove, Carrico, Weber, Raimi & Vandenbergh, 2014). In a meta-review of moral licensing, Blanken et al. (2015) estimated the effect size to be a Cohen's d of 0.31.

Normally, people have a tendency for consistency, which means that they act in a similar way in different situations. This would render positive spillover in a nudged choice where the individual follows the nudge. These opposite phenomena, licensing and consistency, present a contradiction that makes predicting spillover effects quite complex. According to a review of recent research, licensing is common when there are multiple competing goals, whereas consistency is more common when individuals have abstract instead of concrete thinking and when they are connecting their behaviour to their underlying values (Mullin & Monin, 2016). Gneezy, Imas, Brown, Nelson and Norton (2012) argued that costly prosocial behaviours lead to more consistency while cost-free (e.g. hypothetical) behaviours are more likely to render licensing effects. According to Cornelissen, Bashshur, Rode and Menestrel (2013) an ethical mindset may

predict consistency or licensing behaviour, where an outcome-based ethical mindset leads to licensing and a rule-based mindset leads to consistency. However, Mullin and Monin (2016) could not definitely confirm these explanations in their review, more research is still needed.

One example of immoral behaviour that may be affected by spillover effects is cheating. The opportunity to cheat creates a moral dilemma. While most people want to see themselves as honest persons, it's also a fact that dishonesty often pays off. They therefore tend to find a balance between honesty and cheating, where they can receive some benefits but still uphold their self-image. Through socialization individuals internalize norms and values in society, and form a view of what is acceptable or unacceptable behaviour. If they value honesty, dishonest behaviour may lead to a need for updating their own moral self-concept, which is unpleasant. This is especially true if the behaviour is highly immoral, while smaller immoral acts may be possible without affecting self-image (Mazar, Amir & Ariely, 2008). Brown et al. (2011) argued that cheating which is easy to rationalize will happen more often than cheating which is harder to rationalize. Rationalizing is a form of reinterpretation of immoral behaviour, enacted by creating a psychological barrier between behaviour and self-concept. According to Ariely (2012) this often leads to self-deception, where people start to believe that they would have reached the same result even without cheating.

There are different ways to explore cheating behaviour. In this study, a procedure adapted from Fischbacher and Föllmi-Heusi (2013) was used. The procedure uses die rolling, where participants self-report which number is shown on the die. Different numbers give different economic payoff, and there is no incitement to be honest. The authors found a high level of cheating in their experiment, where 20% of participants cheated fully, 39% were fully honest and a large portion cheated partly. They explain these results with lying aversion (making some people fully honest), and maintenance of a positive self-image (making some people only cheat partly). Their study did not include any spillover but was a separate cheating study only.

Aims and hypotheses

In this master thesis, I aimed to find out if there were any differences in cheating behaviour depending on which default alternative participants had received in an earlier task. This is in line with suggestions made by Marchiori et al. (2016) that more research is needed when it comes to spillover effects of choice architecture interventions.

The default manipulation in this master thesis was a choice between keeping or donating a bonus, where participants were randomized to either a pro-social default (donation) or a pro-self default (keeping the bonus). The procedure to test cheating behaviour was as described by Fischbacher and Föllmi-Heusi (2013), but with the modification that the payoff was not a small payment directly after the study but instead a chance to win a larger sum in a lottery later.

The chosen method aimed to find differences that could possibly have an impact on actual choice architecture decisions. The choices were concrete rather than hypothetical and the prosocial behaviours were costly rather than cost-free. If negative spillover effects were found, this would imply that long term consequences of default manipulations may not be as beneficial as previously thought.

The master thesis is part of a project, Downstream behavioral effects of choice architecture manipulations, led by Martin Hedesström and Lars-Olof Johansson at the University of Gothenburg and financed by the Marcus and Amalia Wallenberg Foundation. In the same project there were other studies exploring closely related research questions. This was the first lab experiment in the project, and the dataset of this study has not been used in any other studies.

This study was designed to investigate downstream behavioural effects of a pro-social default manipulation. The research question was whether there were differences in subsequent cheating (pro-self) behaviour between participants who had been subjected to either a pro-social or a pro-self default manipulation.

The hypotheses were:
H1: We expect more participants to donate their bonus in the pro-social default condition than in the pro-self default condition
H2: We expect a pro-social default (default donation of bonus to WWF) in choice 1 to lead to more cheating in choice 2
H3: We expect that this effect will be stronger among participants who follow the pro-social default (as compared to those not following the pro-social default or those receiving a pro-self default)
H4: We expect more cheating among participants who choose to donate in a pro-social default condition than among participants who choose to donate in a pro-self default condition

The first hypothesis is the expected default effect. The second and third hypothesis build on the expectation of a negative spillover effect, primarily driven by the group following a pro-social default. The fourth hypothesis is based on the assumption that there will be more licensing and less consistency after a passive choice to donate than after an active choice.

Method

Preregistration

This is a preregistered study. The full preregistration can be found at osf.io/bp9rw. This means that the hypotheses, procedure and data analysis were all planned and registered before the study took place.

Participants

Participants were recruited through the participant pool at the Department of Psychology at the University of Gothenburg. The pool consists of people who have voluntarily chosen to register through an online recruitment system to participate in psychological experiments. All participants are 18 years or older. An invitation was distributed through this online system. 82 participants signed up, 15 were no-shows and 67 participated in the experiment. The participants were randomized to either a pro-

social default condition or a pro-self default condition. Participants were not informed about this randomization.

44 of the participants (67.5%) were women, 20 of them (29.9%) were men and 3 had another gender identity (4.5%). The age of the participants was 20-70 years, M = 34.2, s = 13.2, median age was 31 years old.

In the preregistration for this study, the planned sample size was 120 participants. Due to difficulties recruiting enough participants, combined with the almost non-existing cheating in choice 2, the study was discontinued before the maximum number of participants was met.

Procedure

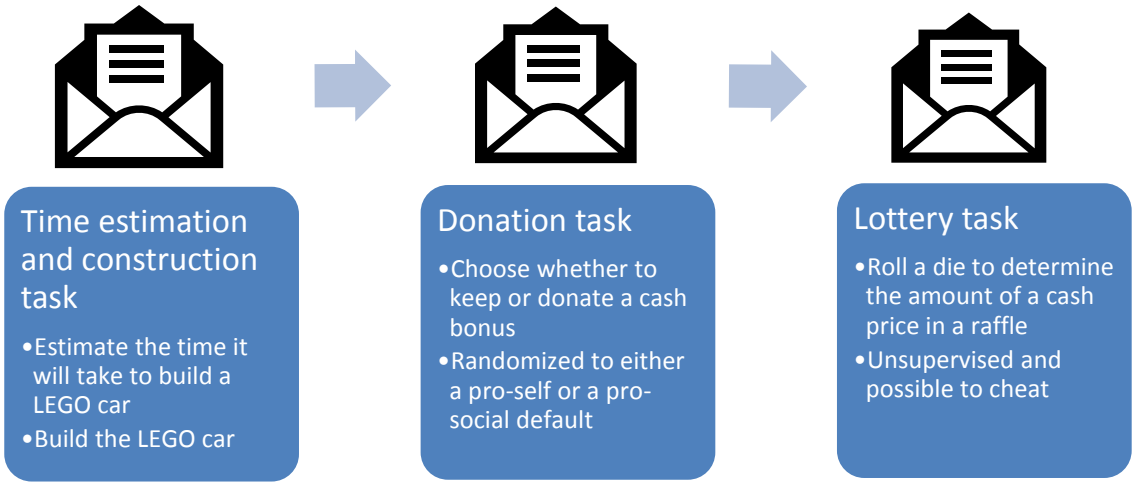

Figure 1. Experiment procedure with three different tasks.

The experiment took place on campus, in a lab room. Participants received a scratch ticket ("Trisslott" which is a scratch ticket from the Swedish state lottery) as a payment for their participation. The scratch ticket is worth 30 SEK (approximately USD 3.5). Depending on their choices in the experiment, they also had the chance to receive a cash bonus of 50 SEK (approx USD 6) and a chance to win a raffle with a maximum amount of 1500 SEK (USD 180). More details can be seen below.

When the participants arrived at the lab, they were asked to sit down at a table where they received three numbered envelopes with instructions (see Appendix 1). The first was a time estimation and construction task, this was ostensibly the main purpose of the study as presented to participants. The second was choice 1 (donating or keeping) and the third was choice 2 (cheating or not cheating). The procedure with three different tasks is presented in Figure 1. Each participant was left alone in the room after a brief introduction, with the test leader waiting outside the room during the experiment.

Time estimation and construction task. In the first envelope, participants were instructed to build a LEGO car. They were told to first estimate the time it would take for them to build the car, and then to ring a bell when they started and when they finished building. The time was measured by the test leader outside of the room.

Donation task. In the second envelope, participants had the choice to either keep or donate a cash bonus. They were randomized to either of two conditions – either a pro-social default or a pro-self default condition, with two different sets of instructions.
In the pro-social default condition, the participant is informed that there is a bonus (50 SEK, approx. USD 6) for their participation that will be donated to WWF. If they do not want the bonus to be donated, they can actively choose to receive it themselves by removing it from a donation envelope. Donation to WWF is thus the default in this condition.

In the pro-self default condition, the participant is informed that they will receive a cash bonus (50 SEK, approx. USD 6) for their participation. If they instead want to donate this bonus to WWF, they can actively choose to do so by putting it in a donation envelope. Keeping the bonus is thus the default in this condition.
Participants were then asked to put the donation envelope in a box and proceed to the third envelope.

Lottery task. In the third envelope, participants were instructed that there will be a cash price awarded (in a raffle) to one participant after the study is completed. The amount of the cash price is determined by dice rolling. The participant was asked to roll the die (unsupervised) and write down what the die shows. If they win the raffle, the amount they will receive is:

If 6 is reported, the amount is 0 SEK.
If 1 is reported, the amount is 300 SEK (approx. USD 36)
If 2 is reported, the amount is 600 SEK (approx. USD 72)
If 3 is reported, the amount is 900 SEK (approx. USD 108)
If 4 is reported, the amount is 1200 SEK (approx. USD 144)
If 5 is reported, the amount is 1500 SEK (approx. USD 180)

Since we used a normal (fair) die, each number should be reported by 16.7 percent of the participants if there are no cheaters. To make cheating possible, the rolling of the dice was unsupervised. This means that cheating cannot be established at an individual level, but on group level only. This is a procedure adapted from Fischbacher and Föllmi-Heusi (2013).

When they had reported the number of the die and their contact information in the event that they should win, they were instructed to ring the bell again to finish the experiment. They were then given their Trisslott lottery ticket and received a short debriefing about the study.

Randomization. The randomization was carried out by creating three-digit codes (in column A) for all participants in a spreadsheet. The RAND function in Excel was used to generate a random number (in column B) in each row. The rows were then sorted in ascending order based on column B. After this, the first half of the rows (participants) was assigned to the pro-social default condition, and the second half to the pro-self default condition (specified in column C).

The three-digit codes were used to identify each participant during the experiment. The instructions and envelopes were prepared beforehand by a collaborator so that the test leader was blinded to which condition each participant was assigned to.

Coding and analysis

All data from the experiments was entered into SPSS for statistical analysis. The pro-self default condition was dummy coded 0 and pro-social default was dummy coded 1. Keeping the bonus was dummy coded 0 and donating the bonus was dummy coded 1.

During analysis, the participants were handled as belonging to four different groups depending on default condition and donation choices:
A: pro-self default, not following the default (donating)
B: pro-self default, following the default (keeping)
C: pro-social default, following the default (donating)
D: pro-social default, not following the default (keeping)

Group (default, bonus)
Group A: (0,1)
Group B: (0,0)
Group C: (1,1)
Group D: (1,0)

In the cheating test (choice 2), the number 6 on the dice was recoded to 0, all other numbers were used as is. This means that the expected mean of this variable, without any cheating, is 2.5.

Pearson's chi-square test was used to test for hypothesis 1, and t-tests for hypotheses 2, 3 and 4. For exploratory analysis, Pearson correlation and binary logistic regression were used. α-level used for significance was $p<0.05$. SPSS was used for all statistical analyses.

Ethical considerations

Subjecting participants to a default manipulation may cause some psychological discomfort. However, the ethical implications are limited since choice is not restricted. All participants were given a short debriefing after the experiment, and were offered to receive additional information after the study was completed. Participants were not instructed nor encouraged to cheat in the lottery task, any such cheating was the result of their own choices. All in all, the study was considered to be ethically acceptable with minimal risk of long-term consequences for participants.

Results

This study was designed to investigate downstream behavioural effects of a pro-social default manipulation. The research question was whether there were differences in subsequent cheating (pro-self) behaviour between participants who had been subjected to either a pro-social or a pro-self default manipulation.

In this section the confirmatory analysis of each hypothesis and some additional exploratory analysis will be presented.

Hypothesis testing

H1. *We expect more participants to donate their bonus in the pro-social default condition than in the pro-self default condition.*

Pearson's chi-square test was used to compare donation choices in the pro-social default condition and the pro-self default condition. The results of the analysis confirmed the hypothesis. The participants in the pro-social default condition were significantly more inclined to donate their bonus to WWF than participants in the pro-self default condition ($\chi^2(1, N = 67) = 3.35$, $p<.05$) (one-tailed). The number of participants donating or keeping in each group is presented in Figure 2.

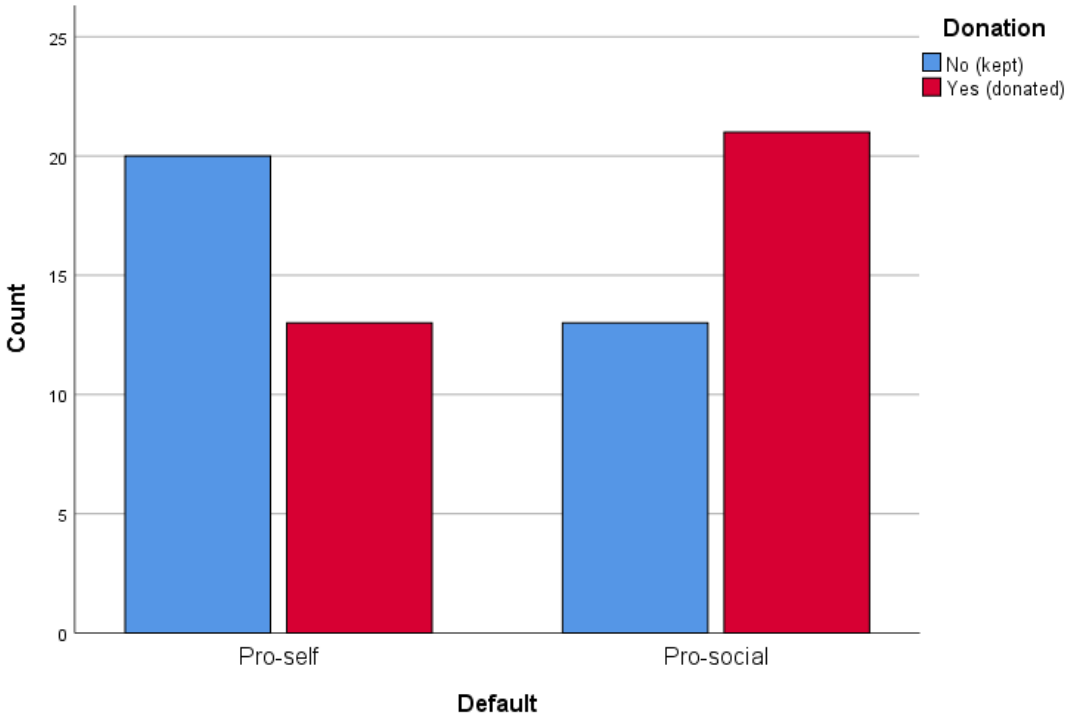

Figure 2. Number of participants donating or keeping the bonus in the pro-social and pro-self default conditions.

H2. *We expect a pro-social default (default donation of bonus to WWF) in choice 1 to lead to more cheating in choice 2.*

A one-sided *t*-test was used to test if there was more cheating in the pro-social default group compared to the pro-self default group. The results showed that there was a somewhat higher mean reported number on the die in the pro-social default group ($M = 2.8$, $s = 1.8$) than in the pro-self default group ($M = 2.3$, $s = 1.8$), but this difference was not statistically significant; $t(65) = 0.97$, $p = 0.17$ one-tailed.

H3. *We expect that this effect will be stronger among participants who follow the pro-social default (as compared to those not following the pro-social default or those receiving a pro-self default).*

The results of a *t*-test between participants following the pro-social default (M = 2.6, s = 2.0) and the rest of the participants (M = 2.5, s = 1.8) showed no differences in cheating; $t(65) = 0.06$, $p = 0.95$.

H4. *We expect more cheating among participants who choose to donate in a pro-social default condition than among participants who choose to donate in a pro-self default condition.*

The results of a *t*-test between participants following the pro-social default (M = 2.6, s = 2.0) and the participants following the pro-self default (M = 2.5, s = 2.0) showed no differences in cheating; $t(32) = 0.16$, $p = 0.88$.

Exploratory analysis

The participants made a time estimation of a construction task, and both the estimation and the actual building time were registered. The exploratory analysis aims to examine if overestimation or underestimation has any correlation with donation choices or cheating behaviour.

The time difference between estimated time and actual building time (in seconds) was computed into the variable Time_diff. The Pearson correlation between Time_diff (M = 180.1, s = 619.3) and reported number of the die (M = 2.6, s = 1.8) was calculated. There was no significant correlation between time estimation accuracy and cheating behaviour, Pearson's $r(65) = -0.16$, $p = 0.21$ (two-tailed).

To examine if there was any correlation between time estimation accuracy (over-/underestimation) and donation choices, a binary logistic regression was performed. The results showed no significant correlation, χ^2 (1, N = 67) = 2.14, $p = 0.11$ (two-tailed).

Discussion

The aim of this master thesis was to explore if spillover effects can be found in a subsequent choice after a default manipulation. No significant spillover effects were found, but there are some explanations that may guide further research.

The first hypothesis was that the default manipulation would have a significant effect on donation choices. This hypothesis was confirmed by data, which shows that the procedure in choice 1 was a default manipulation that had the potential to induce spillover effects on choice 2. The default effect is in line with previous research on choice architecture interventions (Dinner et al., 2011; Johnson & Goldstein, 2003; Johnson & Goldstein, 2013; Thaler & Sunstein, 2009; Thaler, Sunstein & Balz, 2013). The number of participants donating their bonus was significantly higher among those receiving the pro-social default compared to those receiving the pro-self default. Overall, half of all participants chose to donate their bonus. A possible improvement of this experiment could be to include a third group with forced choice, where neither donation nor keeping the money is a default alternative but where participants would have to actively choose. Mullen and Monin (2016) recommended in their review article that studies should include a baseline condition, which could be achieved by having a group with forced choice or a group not being subjected to choice 1 at all.

However, contrary to the findings of Fischbacher and Föllmi-Heusi (2013), the cheating manipulation in this study did not work. The majority of all participants were honest and did not report a higher number to gain extra money. One possible explanation is cultural differences, there is a strong societal norm in Sweden against cheating which could decrease levels of cheating overall (although Sweden and Switzerland are usually considered to be quite similar). Another explanation is sampling bias, the participants were recruited from a pool which consists of people who have voluntarily chosen to register to participate in psychological experiments. This self-selection could potentially lead to a pool of more pro-socially oriented people than the rest of the population, especially since the economic incentives for participating are quite humble. These participants might thus be more honest and/or compliant than is normally expected. A third potential factor is that the economic gain from cheating was neither guaranteed nor immediate. A temporally distant outcome is generally valued less than a temporally proximal outcome (Soman et al., 2005). When deciding whether to cheat or not, the balance between self-serving value and moral (honest) behaviour could hence be shifted towards honesty when the potential gain is distant.

The second, third and fourth hypotheses were not confirmed, although there was some tendency towards a negative spillover between default conditions. The inability to confirm these hypotheses is to be expected when cheating levels are very low and the sample size is small. Considering these limitations, the tendency (although not statistically significant) may suggest that a spillover effect possibly exists even if this study cannot determine whether this is the case. This tendency could only be seen in testing the second hypothesis, while there were no differences between groups in testing the third and fourth hypotheses. This means that while there may be some potential spillover from the default manipulation, this does not seem to be explained by differences in the actual choices (donating or keeping), but rather by the default condition itself. Some caution is needed when discussing differences between these groups though, since cell sizes are rather small. Blanken et al. (2015) suggested that a minimum number of participants per cell should be 165 to be able to detect moral licensing. Such level of statistical power was impossible to reach within this study due to time constraints, costs and recruitment difficulties.

Brown et al. (2011) argued that rationalizability of cheating behaviour is an important aspect, where easily rationalizable cheating will happen more often. The cheating in this experiment is probably not rationalizable enough, while other potential cheating behaviours might be. Mazar, Amir and Ariely (2008) found that cheating was less rationalizable when the reward was monetary than when tokens were given, and that introducing such a medium offers participants more room to interpret their dishonest behavior as acceptable. The monetary prize in this experiment may therefore lead to lower rationalizability than if some other type of reward had been given.

In addition, the effect of time estimation on donation choices and level of cheating was explored. The time estimation task was completed by all participants, but was mainly intended to constitute a plausible reason to receive a payment and bonus without disclosing the true nature of the experiment. A possible correlation between time estimation and pro-social/pro-self behaviour would constitute new knowledge not previously observed. The analysis did not show any statistically significant differences, though it is possible that there is an effect size smaller than can be detected with this number of participants.

Limitations

The two primary limitations to this study were the small sample size and the very limited amount of cheating recorded in choice 2. Carrying out an experiment where participants partake individually one by one is time consuming, which limits the possibility to achieve a sample size large enough to find smaller effect sizes within the context of a master thesis. Recruiting problems also emerged, as a smaller part of the participant pool than expected chose to sign up for this study. In addition to this, about one fifth of participants who had signed up for a time slot didn't show up at their pre-booked time, even though they could choose their own time slot through an online booking system.

Before the study started, 120 participants were planned to be included in the experiment. However, the combination of recruitment difficulties and the low level of cheating led to a decision to discontinue the experiment after 67 participants. When cheating levels are low, a much larger sample size is needed to be able to detect potential effects. This decision was made after consultation with my supervisors.

Recommendations for further research

A suggestion for further research is to measure a more common and less controversial cheating behaviour, where cheating is perceived as more acceptable and a higher level of cheating can be expected. Researchers should aim to find cheating tasks where cheating is more easily rationalizable. This will make it easier to examine potential differences between groups. To avoid intertemporal discounting, any potential economic gain from cheating should be immediate instead of distant. A group with forced choice and/or no donation task could also be included, to create a baseline condition. With these changes, there is a better chance of finding spillover effects if there are any.

The sampling of participants should also be given some consideration. The sample size should be large enough to detect smaller effect sizes. To avoid sampling bias, self-selection of participants should be avoided if possible.

All in all, this master thesis could serve as a guide for further laboratory experiments in the research project on downstream behavioural effects of choice architecture manipulations, or for related research in other settings.

References

Ariely, D. (2012). *The (honest) truth about dishonesty*. New York: HarperCollins.

Blanken, I., Van de Ven, N., & Zeelenberg, M. (2015). A meta-analytic review of moral licensing. *Personality and Social Psychology Bulletin, 41(4),* 540-558. doi: 10.1177/0146167215572134

Brown, R., Tamborski, M., Wang, X., Barnes, C., Mumford, M., Connelly, S., & Devenport, L. (2011). Moral credentialing and the rationalization of misconduct. *Ethics & Behavior, 21(1),* 1-12. doi: 10.1080/10508422.2011.537566

Cornelissen, G., Bashshur, M., Rode, J., & Le Menestrel, M. (2013). Rules or consequences? The role of ethical mind-sets in moral dynamics. *Psychological Science, 24(4),* 482-488. doi: 10.1177/0956797612457376

Dinner, I., Johnson, E., Goldstein, D., Liu, K., & Rogers, W. A. (2011). Partitioning default effects: Why people choose not to choose. *Journal of Experimental Psychology: Applied, 17(4),* 332-341. doi: 10.1037/a0024354

Fischbacher, U., & Föllmi-Heusi, F. (2013). Lies in disguise – an experimental study on cheating. *Journal of the European economic association, 11(3),* 525-547. doi: 10.1111/jeea.12014

Gneezy, A., Imas, A., Brown, A., Nelson, L., & Norton, M. (2012). Paying to be nice: Consistency and costly prosocial behavior. *Management Science, 58(1),* 179-187.

Johnson, E. J., & Goldstein, D. (2003). Do defaults save lives? *Science, 302(5649),* 1338-1339.

Johnson, E. J., & Goldstein, D. (2013). Decisions by default. In E. Shafir (Ed.), *The behavioral foundations of public policy* (pp. 417-427). Princeton: Princeton University Press.

Kahneman, D., & Tversky, A. (1983) Choices, values and frames. *American Psychologist, 39(4),* 341-350.

Marchiori, D. R., Adriaanse, M. A., & De Ridder, D. T. D. (2017). Unresolved questions in nudging research: Putting the psychology back in nudging. *Social and Personality Psychology Compass, 11(1),* N/a. doi: 10.1111/spc3.12297

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.

Mullen, E., & Monin, B. (2016). Consistency versus licensing effects of past moral behavior. *Annual Review of Psychology, 67,* 363-385. doi: 10.1146/annurev-psych-010213-115120

Soman, D., Ainslie, G., Frederick, S., Li, X., Lynch, J., Moreau, P., Mitchell, A., Read, D., Sawyer, A., Trope, Y., Wertenbroch, K., & Zauberman, G. (2005). The psychology of intertemporal discounting: why are distant events valued differently from proximal ones? *Marketing Letters, 16(3/4),* 347-360. doi: 10.1007/s11002-005-5897-x

Thaler, R. H., & Sunstein, C. R. (2009). *Nudge – improving decisions about health, wealth and happiness. Revised and expanded edition.* New York: Penguin Books.

Thaler, R. H., Sunstein, C. R. & Balz, J. P. (2013). Choice architecture. In E. Shafir (Ed.), *The behavioral foundations of public policy* (pp. 428-439). Princeton: Princeton University Press.

Truelove, H. B., Carrico, A. R., Weber, E. U., Raimi, K. T., & Vandenbergh, M. P. (2014). Positive and negative spillover of pro-environmental behavior: an integrative review and theoretical framework. *Global Environmental Change, 29*, 127-138. doi: 10.1016/j.gloenvcha.2014.09.004

Appendix 1

First instruction, all participants:

Tack för att du deltar i vår undersökning. På bordet framför dig finns en konstruktionsuppgift. Tänk på att **läsa igenom alla instruktioner noga**.

Du ska börja med att **uppskatta hur lång tid** du tror att det kommer att ta för dig att bygga ihop konstruktionen.

Det är **inte viktigt att göra det snabbt**, bedöm bara hur lång tid det tar om du bygger i **normal takt**.

Ange tiden du tror att det tar här:

____min ____sek.

När du har skrivit din tidsuppskattning och är redo att börja bygga, plinga en gång i klockan för att starta tidtagningen.

När du har byggt klart plingar du två gånger i klockan för att stoppa tidtagningen. Du kan därefter öppna kuvert 2.

Second instruction, pro-self default condition:

Som ett extra tack för att du deltar i vår undersökning ger vi dig även en bonus på 50 kr. Dina pengar finns i det mindre kuvertet märkt ”Till försöksdeltagaren”.

Om du hellre vill donera pengarna till Världsnatur-fonden WWF kan du flytta över sedeln till det mindre kuvertet märkt ”Till WWF”.

Lägg sedan kuvertet märkt WWF i lådan. Ditt eget kuvert behåller du och tar med dig härifrån. Valet är anonymt.

När du lagt i kuvertet i lådan kan du öppna kuvert 3.

Second instruction, pro-social default condition:

Som ett extra tack för att du deltar i vår undersökning ger vi även en bonus på 50 kr, som kommer att doneras till Världsnaturfonden WWF för din räkning. Pengarna finns i det mindre kuvertet märkt ”Till WWF”.

Om du inte vill att bonusen doneras till WWF utan hellre tar pengarna själv kan du flytta över sedeln till ditt eget mindre kuvert som är märkt ”Till försöksdeltagaren”.

Lägg sedan kuvertet märkt WWF i lådan. Ditt eget kuvert behåller du och tar med dig härifrån. Valet är anonymt.

När du lagt i kuvertet i lådan kan du öppna kuvert 3.

Third instruction, all participants:

Efter undersökningens slut kommer ett pengapris att lottas ut till en av deltagarna. Du ska nu **slå en tärning** som avgör hur stor din vinst blir om du vinner lotteriet.

Det första kastet avgör din ersättning. Slå därefter tärningen flera gånger till för att kontrollera att den ger ett rättvist resultat.

Slår du en sexa får du 0 kronor om du vinner lotteriet

Slår du en etta får du 300 kronor om du vinner lotteriet

Slår du en tvåa får du 600 kronor om du vinner lotteriet

Slår du en trea får du 900 kronor om du vinner lotteriet

Slår du en fyra får du 1200 kronor om du vinner lotteriet

Slår du en femma får du 1500 kronor om du vinner lotteriet

När du känner dig säker på att tärningen är rättvis skriver du vad ditt första tärningskast visade här:

_____

Ange email-adress eller telefonnummer som vi kan kontakta dig på om du vinner lotteriet:_____

Om du inte vill delta i lotteriet behöver du inte ange kontaktuppgifter, men vi ber dig att ändå ange vad tärningen visade.

Du är nu klar med undersökningen. Plinga tre gånger i klockan så kommer försöksledaren. Låt allt ligga kvar på bordet, utom ditt eget kuvert som du ska ta med dig.