



Nudging generosity: Choice architecture and cognitive factors in charitable giving[☆]



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ABSTRACT

In an experimental setup we investigate the effect of two different choice architectures on donation decisions. In the treatment group, subjects can either specify a charity of their choice, or select one from a list of five well-known charities; in the control group we do not provide a list. In a sample of 869 subjects we find a large treatment effect: Offering a list of default charities doubles the fraction of donors, as well as the revenue for charities. We find that the treatment intervention particularly affects subjects who tend to make intuitive choices.

1. Introduction

The act of donating money to strangers—while puzzling from a theoretical viewpoint—is a phenomenon of considerable economic importance. For example, in 2015 charitable giving exceeds two percent of the gross domestic product in the US, and the average household contributes almost \$3000.² The determinants of charitable giving are not only of interest to practitioners, but receive also increasing attention in the academic literature on pro-social behavior—often with a policy focus on mechanisms that boost donations (Andreoni and

Payne, 2013). Matching grants,³ raffles and auctions,⁴ and bundling⁵ can increase revenue. Furthermore, audience effects or social pressure influence the propensity to donate.⁶ Moreover, priming increases donation amounts among individuals with a high willingness to donate (Andersson et al., 2017).

This study shows that a simple change in the elicitation of donations can substantially increase revenue, virtually at no cost. Our results suggest that the choice architecture of the decision situation affects the heuristics that govern donation decisions. We ask experiment participants whether they would like to donate a percentage of their earnings

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¹ She was a Postdoctoral Research Associate at USC Dornsife INET while working on this paper.

² See <https://www.nprtrust.org/philanthropic-resources/charitable-giving-statistics/>.

³ Eckel and Grossman (2003) compare rebate subsidies to matching subsidies and find higher donations for the latter in the laboratory. Davis et al. (2005) find that the effect is smaller when subjects receive extra information regarding the consequences of their choices. The differences between rebate and matching subsidies have been confirmed in the field (Eckel and Grossman, 2008; 2017). Karlan and List (2007) study price effects in matching grants, and Meier (2007) studies long run effects of matching grants.

⁴ Davis et al. (2015) compare lotteries to English auctions and find higher revenues for the former. Carpenter et al. (2008) compare various auction formats.

⁵ See e.g. McManus and Bennet (2011) and Gneezy et al. (2010), see also Falk (2007) for the effect of gifts on donation.

⁶ See e.g. DellaVigna et al. (2012) and Li and Riyanto (2017).

from previous tasks to a charity of their choice.⁷ In the control group, we provide just a blank field to indicate a charity. In the treatment group, we provide participants with a list of five widely known charities and a blank field. Settings like this are commonly found on websites such as *AmazonSmile* or *betterplace.org*, which offer platforms where potential donors can choose from multiple charities.⁸ Indeed, just like in our design *AmazonSmile* adopted a setting where customers can choose among five default organizations or indicate their preferred organization in a blank field.

We observe a surprisingly strong treatment effect: Providing a list of five default charities doubles donations relative to the control condition.⁹ This highlights the importance of choice architecture: while in both conditions individuals face unrestricted choice sets in terms of who they want to advantage, people are nudged into donating when being confronted with the list.

The increase in the amount donated stems solely from an increase in the fraction of donors, while the donation amount conditional on donating is virtually unchanged. This pattern suggests that our treatment intervention affects the extensive margin (the decision to donate or not), but not the intensive margin (the amount conditional on donating). Consistent with this finding, individuals who rely to a greater extent on heuristics (as measured by a version of the cognitive reflection test, CRT) are significantly more likely to donate in the treatment condition, compared to individuals who do not rely on heuristics, while there is no such an effect on the donation amounts.

Our results are in line with *Soyer and Hogarth (2011)*, who observe donation decisions in treatments with lists of three to 16 charities to choose from, and find evidence for an increase of donations in the number of recipients. The authors vary the number of charities, whereas we focus on the difference between the presence and the absence of a list. In addition, while in their design the choice set is restricted by the treatments, the set of available charities is always unrestricted in our experiment (it is always possible to specify any charity as recipient).

Our study complements the results reported by *Altmann et al. (2014)*, *Kraft-Todd et al. (2016)*, *Goswami and Urminsky (2016)*, and *Fiala and Noussair (2017)*, who investigate defaults in the suggested amount to donate. *Altmann et al. (2014)* find that changes in default amounts trigger changes in the distribution of donated amounts, but the revenue for charities remains unaffected. *Goswami and Urminsky (2016)* find an overall positive effect of low default amounts on both the probability to donate and on revenue. Similarly, *Kraft-Todd et al. (2016)* show that experimenters can increase the revenue for charities if they set the default levels for donations at two specific levels: (1) zero and (2) slightly above the median donation amount that individuals would choose in the absence of defaults. By contrast, *Fiala and Noussair (2017)* find no significant effects of default levels on donation behavior.

2. Design

Our experiment was part of a 20 min pen-and-paper study conducted at a Swiss university. The experiment was carried out in 38 tutorials of a mandatory undergraduate course, with on average 23 students per tutorial. All tutorials took place on the same day with the exception of two postponed tutorials. Participation in the experiment

⁷ Introducing donations in laboratory experiments was pioneered by *Eckel and Grossman (1996)*, who compare standard dictator games to dictator games with charities as recipients.

⁸ See *AmazonSmile* (<https://smile.amazon.com/>) and *betterplace.org* (<https://www.betterplace.org/>).

⁹ Our defaults are slightly different from the defaults discussed in the literature on retirement savings (*Madrian and Shea, 2001*), where the default is the option which is implemented when the individual remains inactive. Instead of providing a default option we provide a list of default charities, still requiring the participants to take an active decision.

was voluntary. Among the 1080 students in the tutorials 902 participated in our study, and 869 participants answered the donation question. The study consisted of financially incentivized experimental tasks (elicitation of risk, time, trust and confidence measures). Participants were informed that, once all participants handed in their sheets, we would draw one participant per tutorial to receive the experimental earnings of one randomly selected task. Selected participants earned on average CHF 82 (\approx USD 88). All other participants were not reimbursed.¹⁰

In the final question on the answer sheet (and before the recipient of the earnings was selected) we asked all participants whether they would like to donate a percentage of their (potential) experimental earnings to a charity of their choice. In case the answer was yes they had to indicate a percentage and a charity. In the control condition (*NoList*) the subjects had to fill in a blank field. In the treatment condition (*List*), the subjects could choose from a list of five charities (WWF, Red Cross, Doctors without Borders, Amnesty International, and UNICEF),¹¹ or they could use the blank field to indicate another charity (see *Appendix A.1*).

The randomization into the treatment and control conditions took place at the tutorial level and resulted in 19 tutorials per condition. The students were assigned to tutorial groups by university administrators, but could indicate a preferred time of day.¹² We stratified the randomization according to the time of the tutorial to avoid time of day as a confounding factor.

The subjects also took a version of the cognitive reflection test (CRT hereafter, *Frederick (2005)*; see *Appendix A.2*). The CRT assesses an individual's ability to suppress an intuitive and spontaneous wrong answer in favor of a reflective and deliberative right answer, and thus measures to what extent decisions are governed by heuristics. The CRT measure allows us to test whether individuals who tend to succumb to fast heuristics are particularly responsive to the treatment variation.

Furthermore, our data includes an alternative measure for pro-social preferences which is completely independent of our study. Upon enrollment to the university, students had the option to donate CHF 12 (\approx USD 13) to the university's fund for students in need ("social fund") by clicking a box online. We link their decisions to our experimental data to test for stability in social preferences. In our sample, 12% of students donated to the social fund.

3. Results

We find a surprisingly large treatment effect: Providing the list of five default charities doubles donations relative to the condition without the list. The average percentage donated is 8.2% in the *NoList* treatment and 16.3% in the *List* treatment ($p < 0.001$, Wilcoxon rank-sum test). The left panel of *Fig. 1* shows that the fraction of participants who are willing to donate a positive amount also doubles from 21.9% in *NoList* to 43.9% in *List* ($p < 0.001$, Fisher exact test).

Consequently, the percentage donated conditional on donating is almost identical in the two treatments (37.5% vs. 37.2%). Not only the average, but also the distribution of the percentages is very similar across treatments, as shown by the cumulative distributions in the right panel of *Fig. 1*. Thus, the defaults nudge a larger share of the population into donating, but do not systematically affect donors' choices. In other words, our treatment influences only the extensive margin of the

¹⁰ While the maximum possible earnings were considerable (CHF 200), the per capita financial incentives in this experiment of roughly CHF 3.50 for 20 min were about half of those paid in a typical laboratory experiment at this university (hourly rate of CHF 10 compared to roughly CHF 20).

¹¹ We conducted a similar study one year before ($n = 1081$, *NoList* only). For the present study we selected the five most popular charities from the year before as default charities. The fraction of donors as well as the distribution of donations in the year before was virtually the same as in *NoList* of the present study (20.6% vs. 21.9%, $p = 0.576$, Fisher exact test).

¹² All tutorials took place on the same weekday.

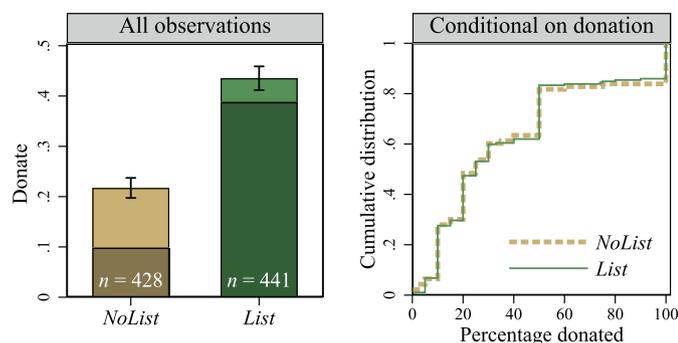


Fig. 1. Left panel: Fraction of the respondents who donated a positive amount. The darker parts indicates donations to the five charities in the list, the lighter parts indicate donations to other charities; spikes indicate standard errors. Right panel: Cumulative distribution of percentage donated conditional on donating.

donation decision, while we do not observe any behavioral changes at the intensive margin.

In the left panel of Fig. 1 we also differentiate between donations made to one of the five charities on the list (darker part) and charities that are not on the list (lighter part of the bars). The *List* treatment creates a pronounced crowding out effect on charities that are not on the list: In the *NoList* condition, 11% of all subjects donate to an organization that is not among the five major charities; in the *List* condition, 5% of all subjects donate to an organization that is not among the five major charities. Conditional on donating, this difference is even larger. In the *NoList* condition, 52% of the donors choose to benefit a charity which is not among the five major charities; in the *List* condition, only 11% of the donors choose a charity which is not on the list.

Our data allows us to investigate the heterogeneity of the treatment effect with respect to gender, pro-social preferences, and tendency to use heuristics. To analyze both the extensive and the intensive margin, we implement a hurdle model with two tiers (Cragg, 1971). The first tier consists of a binary choice model of the donation decision; the second tier consists of a truncated normal model of the donation amount.¹³

The left column of Model (1) in Table 1 confirms our overall treatment effect of 22 percentage points on average, controlling for gender and the social fund indicator.¹⁴ The fraction of students who donated to the social fund prior to the experiment is 12%. Among these, 43.6% donate in the experiment. This fraction drops to 31.1% among the students who did not contribute to the social fund. In the model the estimated effect is twelve percentage points and significant. This is tentative evidence for stability in pro-social preferences across different domains of altruistic behavior (cf. Volk et al., 2012).

We also observe a significant gender effect. Female subjects, who make up 37% of the sample, donate significantly more than male subjects. With seven percentage points the effect is a bit more than half the size of the social fund effect.¹⁵ The right column of Model (1) shows

¹³ Unlike Cragg (1971), who uses a probit specification for the binary choice model, we will use a linear probability model, because it allows for a straightforward interpretation of the interaction effects. Results from probit specifications are very similar.

¹⁴ We also ran specifications which control for wealth effects (available upon request). Given subjects' answers in the previous tasks we can calculate expected earnings for each individual. Controlling for expected earnings neither affects our results, nor adds to the explanatory power of our models.

¹⁵ This is in line with the literature on observational data (see e.g. Willer et al., 2015). The experimental literature on gender effects in pro-sociality remains inconclusive, see Croson and Gneezy (2009) for a survey, or Houser and Schunk (2009) and Capraro and Marcelletti (2014) for recent studies on dictator game giving. Research studying donating to charities (Li et al., 2011; Dreber et al., 2014) find that female participants donate significantly more than male participants. Using a very similar elicitation procedure as in the present study in an online experiment conducted with participants of the sixth wave of the World Values Survey in Germany, Kistler et al. (2017) find an insignificantly

Table 1
Hurdle models of the donation decision.

	(1)		(2)	
	Donate	Amount	Donate	Amount
List (D)	0.224*** (0.032)	1.055 (18.229)	0.189*** (0.046)	-5.553 (21.514)
Social fund (D)	0.108** (0.050)	-22.132 (23.043)	0.044 (0.079)	-30.529 (54.576)
Female (D)	0.068* (0.035)	-7.290 (18.540)	0.040 (0.040)	-16.441 (27.587)
List × social fund			0.122 (0.101)	11.976 (59.414)
List × female			0.058 (0.067)	13.454 (36.757)
Constant	0.174*** (0.028)	-39.391 (26.611)	0.191*** (0.032)	-34.823 (25.273)
σ		63.998		63.905
F, χ^2	18.6	1.6	14.5	1.8
p	0.000	0.658	0.000	0.870
N	827	270	827	270

Notes: OLS estimates. Independent variables are the treatment dummy (*List*), contribution to the social fund, interactions, and a gender dummy. Baseline case is the *NoList* condition with a donation probability of 21.0%. Not included are observations where information on covariates or the donation amount are missing. Robust standard errors, clustered on tutorial, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the results of the truncated regression explaining the amount donated conditional on donation. All covariates as well as the whole model are far from significance.

Apart from affecting the level of donations, our control variables, in particular gender, might also interact with the treatment. Croson and Gneezy (2009) review the literature on gender effects in social preferences and conclude that, while there is no clear gender effect in average pro-sociality, it seems that women are more responsive to changes in the donation context. In a similar vein, DellaVigna et al. (2013) find no gender effect in a baseline treatment where households are asked to donate in a door-to-door campaign. However, when given the chance to avoid the contact with the solicitor, women donate less than men.¹⁶

Model (2) shows the results of the model with interaction effects.¹⁷ The results for the extensive margin suggest a stronger treatment effect for the social fund donors, but this result should be interpreted with caution, because it is insignificant. Likewise, the point estimate for gender suggest that women react more strongly to the introduction of the list than male subjects, yet this result is also insignificant in our data. Similar to Model (1), Model (2) has no explanatory power for the decision at the intensive margin.

The models in Table 2 investigate the differences between intuitive and deliberate decision makers. We classify a subject as deliberate if he or she answers at least two out of three CRT questions correctly, and subsequently also test other definitions and functional forms. We assume that deliberate subjects reflect more upon the answers to the CRT questions, and thus may also reflect more upon the donation decision. Based on this definition, 32% of subjects in the sample are classified as deliberate. Since the experiment was run by pen-and-paper we could not strictly enforce answers, and we have to take missings in the CRT

(footnote continued)

negative coefficient for female participants.

¹⁶ Mellström and Johannesson (2008) investigate crowding-out effects in blood donations. They find that men do not react to the introduction of financial incentives, while there is strong crowding out of the intrinsic motivation to donate blood for female donors.

¹⁷ Coefficients and significance levels are very similar if we estimate the two interaction effects separately.

Table 2
Hurdle models of the donation decision.

	(3)		(4)	
	Donate	Amount	Donate	Amount
List (D)	0.213*** (0.030)	−4.849 (14.715)	0.308*** (0.051)	−18.064 (22.780)
Deliberate (D)	−0.003 (0.039)	−35.959** (18.182)	0.076 (0.053)	−52.275** (24.370)
List × deliberate			−0.151** (0.066)	23.941 (30.960)
Missing CRT (D)	0.099* (0.051)	5.030 (21.139)	0.100* (0.051)	3.822 (21.492)
Constant	0.213*** (0.047)	16.452 (33.176)	0.162*** (0.052)	28.880 (35.474)
Controls	Yes	Yes	Yes	Yes
σ		59.061		58.759
F, χ^2	10.2	13.8	10.7	16.2
p	0.000	0.128	0.000	0.094
N	864	285	864	285

Notes: OLS estimates. Independent variables are the treatment dummy (List), an indicator variable for being deliberate (at least 2 out of 3 CRT questions answered correctly), interactions, and an indicator variable for having at least one unanswered CRT question (missing CRT). Control variables include gender and the set of CRT questions asked. Baseline case is the *NoList* condition with a donation probability of 21.9%. Not included are observations where information on covariates or the donation amount are missing. Robust standard errors, clustered on tutorial, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

questions into account. To control for missings in the CRT questions we include an indicator variable for whether the subject misses at least one CRT question. In our sample 12% of the subjects miss at least one CRT question, and 81% of these subjects are classified as intuitive.

Subjects classified as deliberate donate on average 10.3%, while subjects classified as intuitive are considerably more generous with average donations of 15.7%.¹⁸ The results from Model (3) show that these differences are caused by differences at the intensive margin, because deliberate and intuitive subjects are equally likely to donate. Conditional on donating, deliberate subjects donate smaller amounts. In Model (4) we investigate whether the effect of the list of charities affects deliberate and intuitive subjects differently. Our estimate for the treatment effect on the extensive margin is twice as large for the intuitive subjects than for deliberate subjects, and the interaction term is significant. Consequently, intuitive subjects react much more strongly to the treatment than deliberate subjects. With respect to the amount donated we do not find significant interaction effects along the deliberate-intuitive dimension.¹⁹

4. Discussion

We study the impact of a default list of charities on donation behavior and find three main results: First, the default list strongly affects the decision to donate at the extensive margin: the number of donors doubles in the *List* condition, compared to the *NoList* condition. Second, we find no effect of the default list on the donation amount conditional on donating. Third, presenting a list of default charities crowds out donations for organizations that are not on the list.

We observe an increase of the donation probability from 22% to 44% in reaction to the list. Aside from being highly significant, our treatment effect is also sizeable in comparison to other, more costly manipulations of the decision environment. For example, Falk (2007)

¹⁸ This is in line with the results of Ben-Ner et al. (2004), who report that cognitive abilities negatively relate to generosity in dictator games.

¹⁹ The results reported in Table 2 are largely insensitive to alternative specifications for the CRT score; see Table A1 in the appendix for details.

reports that including postcards as gifts in solicitation letters increases the probability of a donation from 12% (no gift) to 14% (small gift) to 21% (large gift). Karlan and List (2007) find that matching donations at various rates increases the probability of a donation from 1.8% (no match) to 2.2%.

Different mechanisms could drive our findings. As a first mechanism, the presence of the list could trigger a person’s willingness to donate by activating altruistic motives. The list of organizations might remind participants of the good work these charities do, or the mere mentioning of an organization might trigger an emotional response.²⁰ As a second mechanism, the list could simply lower transaction costs of donating. Ticking a box requires arguably less effort than naming a charity. With the data at hand we cannot separate these two explanations. A first step in distinguishing between the two mechanisms could be an experimental variation of the number of organizations on the list. Reducing the number of organizations from five to one, for example, might reduce respondents’ emotional reactions, but not the transaction costs involved in donating.

In addition, a third mechanism might contribute to the effect of the list: The list may trigger fast decision heuristics. We find suggestive evidence for this claim in two respects. On the one hand, the list has a smaller effect on deliberate people, who are less likely to use fast decision heuristics. On the other hand, we find a large crowding-out effect. People substitute the charities in the *NoList* condition for the readily available charities in the *List* condition.

This third explanation is in line with recent literature on deliberation in decision making. Söllner et al. (2013) provide evidence on the relation between decision time and deliberation. In an experimental setup, the individuals are faced with one out of two representations of a choice environment, one that is easy to understand or one that is more complicated. The authors find that the complicated representation slows down the decision time and therefore induces deliberate decision making. Our finding is also consistent with a literature showing that fast decision heuristics can lead to emotional or affective responses, which then trigger non-selfish behavior. Schulz et al. (2014) find that cognitive load, intended to increase affective decision making, leads to more altruistic choices in mini-dictator games. Rand et al. (2012) show that intuition increases contributions in a public goods game, whereas reflection decreases contributions. In our setup, however, we cannot directly test whether decision time or emotional responses account for the effect that we find. Further research is needed to establish this link.

Lastly, the large crowding-out of organizations that are not on the list is consistent with research on “contextual inference”. Subjects in the *List* condition may conclude that the experimenters would like to endorse or recommend the organizations on the list, or may assume that the organizations on the default list are of higher quality than organizations that are not on the list (McKenzie et al., 2006). This can increase the reputation of the charities on the list in the eyes of the participants. The subjects may also conclude that the default list includes the most popular charities, which would correspond to an information effect. Individuals who prefer to adhere to social norms or standards may thus choose the default organizations over other organizations (Cappelletti et al., 2014; Huh et al., 2014). Again, further research is needed to support the importance of contextual inference in our setting.

5. Conclusion

A subtle change in the choice environment that does not alter the choice set leads to a large change in behavior: Providing participants with a list of default charities doubles both the fraction of donors and the revenue for charities. Since the choice set is unrestricted in both

²⁰ This could go both ways. For example, if some of the charities in the list are involved in a scandal, then negative emotions might reverse the treatment effect. To the best of our knowledge, at the time of the experiment none of the charities in the list was involved in a scandal.

conditions, our results cannot be explained within standard theories that abstract from informational costs and psychological factors. Our findings thus highlight the importance of “choice architecture” (Thaler and Sunstein, 2008) in donation decisions.

Our findings have important implications for marketing efforts among charities as well as among firms that promote charitable giving. Websites such as *AmazonSmile* or *betterplace.org*, which provide a central platform for potential donors to choose from many different cha-

rities, become increasingly common. By providing lists of default beneficiaries these organizations can increase revenue. However, charitable organizations that are not on the list will lose revenue as they are crowded out. Furthermore, given that the extensive margin of the donation decision seems to be more clearly affected, compared to the intensive margin, the organizations may want to focus their marketing resources on inducing more people to donate, rather than increasing the donation amounts among the donors.

Appendix A

A.1. Treatment intervention

Fig. A1 displays the elicitation of the donation choices in the *List* condition, while Fig. A2 displays the elicitation in the *NoList* condition (translated from German). While both choice sets do not restrict the potential recipients of the donation, the *List* condition displays five widely-known charitable organizations.

17. If your student ID is randomly drawn: Would you like to donate part of your earnings for a charitable organisation of your choice? (This input is binding – a bank transfer will be directly carried out in the pay-out room.)

No

Yes, percent directed to:

- WWF
- Red Cross
- Mediciens Sans Frontières (Doctors without borders)
- Amnesty International
- UNICEF
- other _____

Fig. A1. *List* condition. Participants are provided with defaults in addition to the blank field.

17. If your student ID is randomly drawn: Would you like to donate part of your earnings for a charitable organisation of your choice? (This input is binding – a bank transfer will be directly carried out in the pay-out room.)

No

Yes, percent directed to _____.

Fig. A2. *NoList* condition. Participants are only provided with a blank field.

A.2. Cognitive reflection test

The cognitive reflection test is a widely used measure to assess individuals’ ability to suppress an intuitive and spontaneous wrong answer in favor of a reflective and deliberative right answer. It consists of three questions. To minimize the probability that subjects can copy their answer from their neighbor (or obtain the correct answer from participants of earlier sessions), we created six question sets consisting of questions that are very similar or identical to the three questions by Frederick (2005). Each subject answered one question set (three questions). The CRT question sets are balanced across the *List* and *NoList* condition.

We have the following distributions of CRT: 12% of individuals answered no question correctly, 19% answered one question correctly, 33% answered two questions correctly, and 36% answered three questions correctly. 10% out of 869 subjects have at least one CRT question that is unanswered.

1. Bat-and-ball-type questions

- A bat and a ball cost \$1.10 in total. The bat costs \$1.00 more than the ball. How much does the ball cost? (Set 1)
- A stock and a stock-option cost \$110 in total. The stock costs \$100 more than the stock-option. How much does the stock-option cost? (Set 2)
- A motorist and his car weigh 1100 kg in total. The motorist weighs 1000 kg less than the car. How much does the motorist weigh? (Set 3)
- A cyclist and his cycle weigh together 120 kg. The cycle weighs 100 kg less than the cyclist. How much does the cycle weigh? (Set 4)
- A broom and a dustpan weigh 1.1 kg in total. The broom weighs 1 kg more than the dustpan. How much does the dustpan weigh? (Set 5)
- A bottle of wine and a corkscrew cost together 60 CHF. The bottle of wine costs 50 CHF more than the corkscrew. How much does the bottle of wine cost? (Set 6)

2. Machine-type questions

Table A1
Hurdle models of the donation decision.

	(5)		(6)	
	Donate	Amount	Donate	Amount
List (D)	0.318*** (0.065)	-16.457 (29.173)	0.260*** (0.071)	19.442 (29.185)
CRT(3)	0.020 (0.023)	-19.388* (11.243)		
CRT = 1 (D)			-0.008 (0.087)	53.759 (43.738)
CRT = 2 (D)			0.114 (0.075)	-11.699 (39.693)
CRT = 3 (D)			0.069 (0.072)	-20.068 (35.628)
List × CRT(3) score	-0.059* (0.032)	8.010 (14.719)		
List × (CRT = 1)			0.094 (0.107)	-69.809* (40.861)
List × (CRT = 2)			-0.109 (0.088)	-47.005 (44.236)
List × (CRT = 3)			-0.099 (0.095)	3.127 (36.385)
Missing CRT (D)	0.088* (0.051)	5.447 (21.105)	0.135** (0.064)	3.066 (25.723)
Constant	0.083 (0.058)	26.441 (37.793)	0.063 (0.071)	0.875 (40.754)
Controls	Yes	Yes	Yes	Yes
σ		59.636		57.139
F, χ^2	9.9	13.0	10.3	22.5
p	0.000	0.223	0.000	0.070
N	864	285	864	285

Notes: Hurdle model: linear probability model for *donate*, truncated regression for *amount*. Independent variables are the treatment dummy (List), the number of correctly answered CRT questions (linear and as dummy variables with 0 being the reference category), interactions, and an indicator variable for having at least one unanswered CRT question (missing CRT). Control variables include gender and the set of CRT questions asked. Not included are observations where information on covariates or the donation amount are missing. Robust standard errors, clustered on tutorial, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

- If it takes 10 concrete mixers 10 min to mix 10 tons of concrete, how long would it take 100 concrete mixers to mix 100 tons of concrete? (Set 1)
 - If it takes 5 bulldozers 5 min to level 5 m², how long would it take 10 bulldozers to level 10 m²? (Set 2)
 - If it takes 10 workers 10 min to make 10 widgets, how long would it take 50 workers to make 50 widgets? (Set 3)
 - If it takes 5 printers 5 min to print 5 posters, how long would it take 100 printers to make 100 posters? (Set 4)
 - If it takes 10 people 10 min to make 10 widgets, how long would it take 100 people to make 100 widgets? (Set 5)
 - If it takes 5 machines 5 min to make 5 widgets, how long would it take 100 machines to make 100 widgets? (Set 6)
3. Lily-pad-type questions
- On a corn field vermin are spreading. Every day the affected area doubles in size. If it takes 32 days until the whole field is affected, how long would it take until half of the field is affected? (Set 1, see [Frederick, 2005](#))
 - In a lake, there are algae. Every day, the affected area doubles in size. If it takes 100 days until the whole lake is affected by algae, how long would it take until half the lake is affected? (Set 2)
 - On a wheat field vermin are spreading. Every day the affected area doubles in size. If it takes 60 days until the whole wheat field is affected, how long would it take until half of the field is affected? (Set 3)
 - On a lake an oil film is spreading. Every day, the area doubles in size. If it takes 24 days for the oil film to cover the entire lake, how long would it take for the oil film to cover half of the lake? (Set 4)
 - In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? (Set 5)
 - On a meadow there are primrose. Every year the area where primrose are growing doubles in size. If it takes 10 years for the primrose to cover the entire meadow, how long would it take for the primrose to cover half of the meadow? (Set 6)

A.3. Additional analyses

Models (5) and (6) show the results of alternative specifications for the CRT score, in order to assess the robustness of the findings from Model (4) in the main text. All specifications include interactions between the treatment variable and the CRT score. Model (5) uses a linear CRT score instead of a dummy variable for being deliberate. The results are similar to the results from Model (4), i.e., students with higher CRT scores react less strongly to the treatment. Compared to Model (4) this interaction effect is smaller and only significant at the 10%-level. Model (6) further investigates the functional form of the treatment effect conditional on CRT by including dummy variables for each value of the CRT score, interacted with the treatment. The effect heterogeneity occurs primarily at the cutoff between a CRT score of 1 and 2, where the coefficients on the interaction term switch their sign. This finding suggests that the treatment effects are non-linear in CRT. In this specification, however, the interaction terms are no longer significant.

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