Do People Intervene to Make Others Behave Prosocially?

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Abstract

We experimentally investigate people's willingness to intervene in others' decision-making in order to promote a charitable donation. We find that only a minority of those subjects who would donate themselves enforce the donation by banning the selfish choice from the decision-maker's choice menu. Bans are more acceptable if they are implemented only after the decision-makers could choose between the selfish and the prosocial option themselves. Also, many subjects decide against offering decision-makers a monetary incentive to switch from the selfish to the prosocial choice. We discuss potential hypotheses about underlying motivations for the (non-) usage of interventions, with a special focus on the hypothesis that interventions to promote prosocial choice are more acceptable the more they respect the autonomy of others.

Keywords: Charity experiment, Prosocial behavior, Autonomy, Bans, Incentives

1. Introduction

The question which interventions increase prosocial behavior has gained much attention in empirical economic research (Andreoni (2015) and Lacetera et al. (2013) provide reviews on how to increase charitable fundraising and blood donations, respectively). However, the question which interventions are perceived as "acceptable" has gained much less attention. In this paper, we investigate which interventions otherwise unaffected third parties are willing to use in order to influence others' prosocial behavior. In particular, we consider two kinds of interventions: bans that directly enforce the prosocial action by removing the selfish option, and monetary payments that incentivize the prosocial action. One conjecture motivating our study is that the choice (not) to intervene may be driven by a desire to preserve others' autonomy.

Specifically, in our study, we let participants in an online experiment, called judges, decide about the rules that other participants in a subsequent lab experiment, called decision-makers, face. Without any own monetary stakes involved, judges decide whether to use bans and incentives to influence decision-makers' choice between a charitable donation of 10€, which yields a small payoff of 3€ for the decision-maker, and a large payoff of 10€ for the decision-maker that precludes, however, the donation. Each donation finances an eye surgery against blinding in Ethiopia.

In our main choice treatment, judges can either choose a ban to take away one of the two choice options from the decision-maker, or they can leave his choice set unaffected. We

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vary whether the decision-maker is informed about the judge's decision before (*Pre-choice*) or after (*Post-choice*) he made his own choice. While the former treatment variation leaves no room for the decision-maker to make a choice himself, the latter variation grants autonomy for doing good, but the judge may still alter the outcome in case the donation is not chosen. In a second choice treatment, judges can offer those decision-makers who initially chose to be selfish an additional private bonus of 2€ if they change their decision into a donation (*Opposing* incentive). As a control, the bonus can also be offered to those choosing the donation anyway (*Aligned* incentive). None of those interventions affects judges' payoffs; granted bonuses are paid by the experimenter.

A straightforward prediction in our settings would be that judges who would donate themselves choose to implement interventions that push decision-makers to also donate. To see why, suppose an altruistic judge values a dollar for the charity higher than a dollar for herself (and thus would donate herself), yet values a dollar for herself higher than a dollar for the decision-maker (a reasonable assumption if judges and decision-makers are randomly drawn from the same subject population as it is the case in our experiment). Then, assuming that procedural aspects of donations are irrelevant, this judge should be willing to use bans and monetary incentives to promote a donation. A judge with this motivation will in the following be described as "outcome-centered".

In contrast, an unwillingness to push others to behave prosocially, along with possible treatment effects, can occur if judges care about procedural aspects of interventions – *how* others are pushed to donate. One natural candidate is that judges wish to respect the decision-maker's autonomy. Indeed, according to scholars in philosophy, autonomy – freedom from external control or influence – possesses a non-instrumental, inherent value, which should be respected (Feinberg, 1978; Rawls, 1971, 1980; Young, 1982). Previous research in behavioral economics has shown that autonomy may affect economic outcomes (Bartling et al., 2014, 2012; Benz and Frey, 2008; Burdin et al., 2018; Cassar and Meier, 2018; Falk and Kosfeld, 2006; Fehr et al., 2013; Leider and Kessler, 2016). If judges respected others' autonomy, they might refrain from any interventions to push the decision-maker towards a donation, even if they donated themselves. And if they intervene, interventions may be more acceptable the more they respect the autonomy of others. For instance, while *Pre-choice* bans leave no room for the decision-maker to make a choice himself, *Post-choice* bans grant at least autonomy for doing good, and only alter the outcome in case the donation is not chosen. Monetary incentives allow choice, but may stand in conflict with some notion of autonomy – if trying to dissuade people from the choice they would have made without being incentivized, as has been forcefully argued by Grant (2006, 2011). This might suggest that *Opposing* incentives are less attractive than *Aligned* incentives.

Our main treatment data show that, surprisingly, more than half of the judges who donate themselves do not implement bans at all. Among those judges who intervene, we observe that bans are more acceptable if the decision-maker is informed about the intervention only after he made his choice and the intervention only comes into effect in case the choice is

inconsistent with the ban. Monetary incentives are more acceptable if the incentive is not used to dissuade a decision-maker who previously decided not to donate. This overall pattern of interventions is broadly consistent with the notion that many judges desire to respect the decision-makers' demand for autonomy. However, for instance the fact that we do not find a statistical relationship between subjects' willingness to use bans on the one hand, and incentives to influence others' donation behavior on the other hand, suggests that a full account of the underlying motivations that drive many judges' unwillingness to push decision-makers to act prosocially may extend beyond a simple concern for autonomy.

Our study is related to several strands of literature. Autonomy is a well-studied concept in philosophy. Besides its instrumental value of enabling people to decide for themselves, scholars also attribute a non-instrumental, inherent value to it (Feinberg, 1978; Rawls, 1971, 1980; Young, 1982). Additionally, autonomy together with relatedness and competence is a key component in self-determination theory (Deci and Ryan, 1985; Ryan and Deci, 2000) in psychology to motivate people, generating actions of superior quality than under extrinsic rewards like money (Lepper et al., 1973; Lepper and Greene, 1978; Titmuss, 1970). Recent economic research supports both empirically and theoretically that intrinsic motivation matters (Bénabou and Tirole, 2003, 2006; Bowles and Polania-Reyes, 2012; Frey and Oberholzer-Gee, 1997). In the context of prosocial activities, Ashraf et al. (2014) and Gneezy and Rustichini (2000) find that paying subjects for charitable fundraising activities decreases effort. When looking at autonomy in particular, there is laboratory evidence that people value to decide on their own (Bartling et al., 2014) and that they reduce their effort when experiencing more control and thereby less autonomy (Bartling et al., 2012; Falk and Kosfeld, 2006; Fehr et al., 2013). Leider and Kessler (2016) highlight that this negative reaction to control stems from procedural fairness concerns being violated, while Burdin et al. (2018) find no negative reactions to control but rather positive reactions if principals refrain from exerting control on agents. Using survey data, Benz and Frey (2008) show that self-employed people seem to gain procedural utility from being able to decide autonomously. While the conflict of bans with autonomy is rather obvious, incentives conflict with autonomy according to political scientist Grant (2006, 2011) insofar that they do not respect a choice an individual, capable of making moral choices, makes on his own. According to Grant, incentives may therefore be considered as a form of power trying to change one's own decision. This may lead to a decision "against own better judgment" which interferes with autonomy. We add to this literature by studying under which circumstances people refrain from intervening into others' prosocial decision-making.

With reference to research on philanthropy, there are various attempts like matching donations (Eckel and Grossman, 2003; Huck and Rasul, 2011; Huck et al., 2015; Meier, 2007), seed money and refunds (List and Lucking-Reiley, 2002) or peer comparison (Meer, 2011) to increase charitable giving. See Andreoni (2015) for a collection of influential papers. Similarly, several studies test monetary as well as non-monetary incentives to increase blood (Goette and Stutzer, 2008; Lacetera and Macis, 2010, 2013; Lacetera et al., 2014) as well as organ donations (Eyting et al., 2016; Kessler and Roth, 2012; Mellström and Johannesson, 2010). Our project

extends this research by looking at third-parties' willingness to intervene in others' prosocial decision-making. In this sense, it complements a study by Butera and Houser (2018), who find that charitable giving does not decrease if delegated. While Jacobsson et al. (2007) look at *what* kind of donations people prefer to give to others, in their case in-kind rather than monetary donations, our focus lies on the interventional tool people use to channel the donation behavior of others in light of its conflict with autonomy.

From a methodological perspective, our experimental design overlaps with studies deploying so-called spectator designs (Almas et al., 2020; Cappelen et al., 2013) to investigate third-parties' willingness to intervene in others' outcomes, often in redistribution settings. In a setting with time-delayed payments, Ambuehl et al. (2021) use a related design to study paternalistic interventions into others' choice set and find substantial intervention rates. Taking the perspective of third-parties is useful for investigating what interventions are considered as "acceptable" (see also the literature on repugnance in the domain of organ or blood donations (Roth, 2007, 2018)). Without such knowledge, barriers to the implementation of interventions may be overseen.

The rest of the paper is structured as follows. In section 2, we present our experimental design. Section 3 presents the results and Section 4 relates them to standard social utility models. The last section concludes.

2. Experimental Design

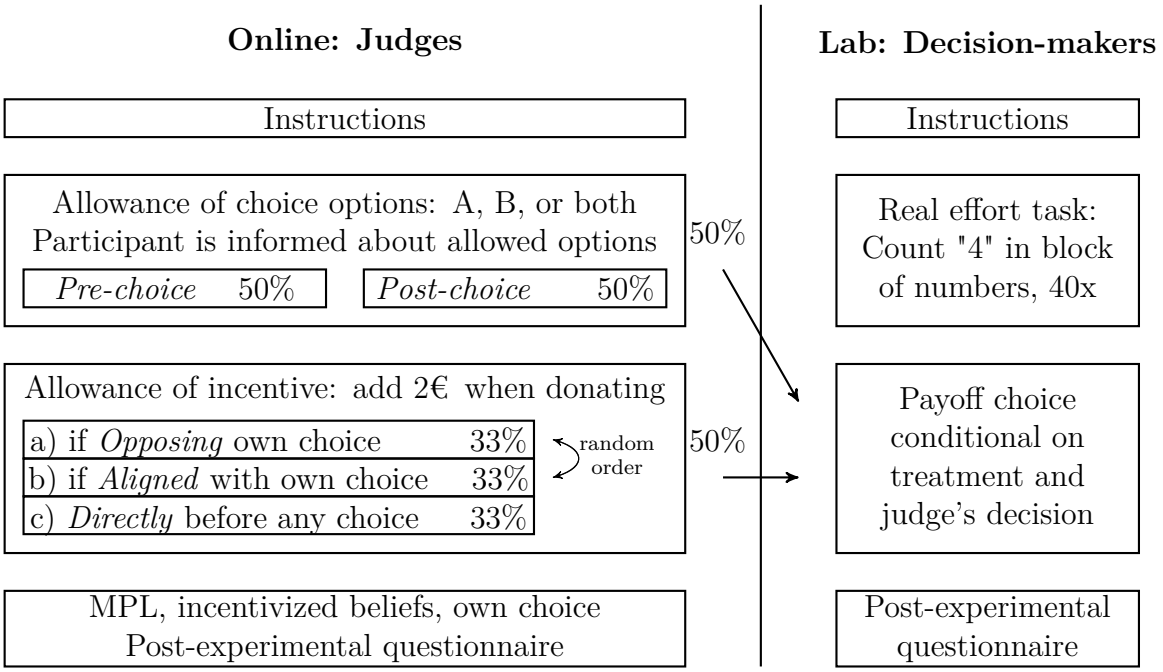
Our experiment consists of an online part and a laboratory part. Participants in the online part, called *judges*, can set the rules of the subsequent laboratory part, thereby determining the choice sets and payoffs of the participants in the laboratory, called *decision-makers*. In what follows, we refer to a judge as "she" and to a decision-maker as "he". Figure 1 depicts the structure of the experiment. Screenshots of how treatments are implemented as well as instructions for both parts of the experiment can be found in Appendix B.

2.1. Laboratory part of the experiment

Decision-makers in the laboratory part first have to work on a real-effort task and can then choose between two payoff alternatives. The real-effort task consists of correctly counting how often the number "4" is included in a block of numbers and is repeated 40 times. An example for such a counting task is provided in the instructions in Appendix B.2. The task is a version of that used in Abeler et al. (2011), generates no value for the experimenter nor pleasure for subjects completing it, but requires costly effort, which in turn might increase the subjects' perception of entitlement to a larger payoff.

After finishing the real-effort task, decision-makers can pick one of two payoff alternatives. They either receive a payoff of 10€ and do not donate any money, or they receive a payoff of 3€ while donating 10€ to the German charity "Menschen für Menschen". The donation finances an eye surgery in Ethiopia against blinding from the disease trachoma, the world's most common bacterial reason for blindness. We show pictures of the surgery and provide details regarding

Figure 1: Structure of the experiment



Notes: Probabilities reported as %. Treatment differences in italics. MPL refers to the multiple price lists for constructing altruism controls.

the causes and consequences of the disease in the instructions. We inform participants that we are going to upload donation receipts on our website after the experiment for verification purposes. After the payoff choice is made, the laboratory part of the experiment ends with a brief post-experimental questionnaire.

2.2. Online part of the experiment and hypotheses

Judges in the online part of the experiment determine the rules of the laboratory part. Their own payoff for this task is fixed at 4€. Judges can gain a bonus as explained below; the bonus does not relate to their decisions (not) to intervene. Since we recruit more judges than decision-makers, we randomly draw whose judges' decisions are implemented, and then match each of these judges to one decision-maker. Judges make two kinds of decisions, one of which is selected with 50% probability and implemented.

Main treatment: Bans. The first and main decision concerns judges' preference over the choice set of decision-makers. Judges can choose between allowing both payoff alternatives, the selfish alternative only, or the donation only. As explained in the introduction, by a straightforward choice model, an outcome-centered judge who would donate herself would ban the selfish option. Yet, procedural aspects of interventions may be relevant, too, such as a desire to respect decision-makers' autonomy. This motivation may lead to some judges abstaining from an intervention even if they donate themselves. We call this a procedure-centered motivation for interventions. This leads us to the following hypotheses regarding the use of bans.

Hypothesis 1a (Outcome-centered motivation): Judges use bans to enforce charitable giving if they donate themselves.

Hypothesis 1b (Procedure-centered motivation): Judges do not use bans even if they donate themselves.

We randomly assign subjects into two treatments. In the *Post-choice* treatment, the decision-maker makes a choice and learns afterwards whether this choice is allowed by the judge matched to him.[1] If it is allowed, it is implemented. Otherwise, it is overridden by the other option. In the *Pre-choice* treatment, options not allowed are already blanked out on the choice screen of the decision-maker, so he does not have to make a choice at all. In contrast, in the *Post-choice* treatment, any interference only emerges after the decision-maker had the opportunity to do good himself, and only if he decided in misalignment with what his judge allowed. Both options require judges to determine the set of possible outcomes, and they only differ by the procedure by which this set is determined. While the *Pre-choice* treatment directly implements the outcome, judges in the *Post-choice* treatment can let decision-makers first decide on their own, possibly preserving a feeling of autonomy for the donation-willing individuals, and afterwards enforce the implementation of the prosocial outcome for those who are not willing to donate. This leads us to our second set of hypotheses.

Hypothesis 2a (Outcome-centered motivation): Judges use bans equally often in the *Pre-choice* and *Post-choice* treatment.
Hypothesis 2b (Procedure-centered motivation): Judges use bans less often in the *Pre-choice* than in the *Post-choice* treatment.

An interesting alternative "procedural" hypothesis is that choice involving a trade-off between selfishness and prosociality may be regarded by judges as a burden rather than an opportunity (as pointed out by Heath and Tversky (1991), Loewenstein (1999), Sunstein (2014, 2015), and Tversky and Shafir (1992)), and judges may want to altruistically free decision-makers from such a burden. In this case, bans should be *more* attractive in the *Pre-choice* treatment:

Hypothesis 2c (Unburdening from choice): Judges use bans more often in the *Pre-choice* that in the *Post-choice* treatment.

Second treatment: Incentives. The second decision that judges make concerns whether or not the decision-maker should get a 2€ extra incentive which is added from the experimenter's account to the decision-maker's payoff if he donates. If used, the decision-maker's own payoff from donation increases from 3€ to 5€. Here, judges have to make three within-subject decisions out of which one is randomly chosen and implemented with equal probability. They have to decide about an *Opposing* incentive, an *Aligned* incentive, and a *Direct* incentive. We consider an incentive as *Opposing* if it is offered to the decision-maker for donating after he

[1] Whenever a decision-maker makes a choice, he knows that another participant has previously determined the rules for the lab experiment, but does not know what particular choice is affected and how. Importantly, he does not know that his choice may be overridden or that there may be the possibility to revise his choice.

initially decided against donation. The *Aligned* incentive is added after the decision-maker has initially donated, thereby raising the decision-maker's payoff of the option he already chose. In both cases, decision-makers get the opportunity to subsequently revise their initial choice (which is shown as the default on the later screen) after having seen the additional incentive. Moreover, we randomize the order by which the judge decides about the *Opposing* and the *Aligned* incentive. The *Direct* incentive serves as an additional control to rule out an aversion to adding 2€ per se and is always implemented only after judges have already decided about the *Opposing* and the *Aligned* incentive. Unlike the other incentives, a *Direct* incentive increases the decision-maker's payoff right from the beginning. This means that the respective decision-maker would not even see the initial payoff alternative (3€ for decision-maker, 10€ donation), but directly gets his own payoff in case of donation displayed as 5€.

A judge with an outcome-centered motivation, in particular if she donated herself, would try to attract more donations. New donations can only be attracted by *Opposing* incentives via making initially donation-unwilling individuals switch to the donation. *Aligned* incentives raise the payoff of donors but cannot attract more donations to charity. Hence, outcome-focused judges should use the *Opposing* incentive (weakly) more often than the *Aligned* incentive to implement the desired donation outcome.

In contrast, procedure-centered motivations would suggest that judges find the *Aligned* incentive more attractive. If judges are concerned about decision-makers' autonomy, following the reasoning of Grant (2006, 2011), the *Opposing* incentive is less attractive compared to the *Aligned* incentive since the former attempts to change the decision-maker's own, initial choice and in this sense disrespects the decision-maker's autonomous will. Thus, a judge concerned about this notion of autonomy should use *Opposing* incentives less often than *Aligned* incentives.[2]

Hypothesis 3a: (Outcome-centered motivation): Judges intervene (weakly) more frequently by *Opposing* than by *Aligned* incentives.

Hypothesis 3b (Procedure-centered motivations): Judges intervene less frequently by *Opposing* than by *Aligned* incentives.

Additional elicitations and experimental procedures. We collect several beliefs, attitudes, and other measures related to our setting after all decisions described above were made. Most importantly, judges have to make a choice between the two payoff alternatives for themselves, and we randomly pick one judge for whom this decision is implemented. Choosing the donation herself serves as our first measure of whether a judge cares about the charity. We also give 10% of decision-makers the chance to delegate the payoff choice to a judge, and let judges make a separate decision between the two payoff alternatives for such a situation. This serves as our second measure to identify to what extent the judge cares about the charity. Moreover, we

[2]Section 4 discusses another procedure-based hypothesis yielding the same prediction.

control for contextual factors that might influence judges' perception of decision-makers' choice situation, namely effort costs and duration of the previous real-effort task.[3]

On top of that, we measure judges' general valuation for allocating money (paid from the experimenter's account) to the decision-maker or the charity, respectively, via multiple price lists. One list item is randomly chosen and implemented in 10% of the cases independently from any previous choice. Judges can deduct up to 3€ from the payoff of the decision-maker or the charity, respectively, or add up to 5€ to it. Based on these decisions, we construct a measure of altruism, i.e., we call a judge "altruistic" if she always gives money to and never takes money from the decision-maker. We use an equal measure with reference to the charity. This way, we can control for potential confounds, e.g., spite or the potential role of inequality aversion towards the decision-maker. Furthermore, we elicit demographics and several attitudes related to our setting in a post-experimental questionnaire.

We recruited both judges and decision-makers from the subject pool of the Cologne Laboratory for Economic Research (CLER) using ORSEE (Greiner, 2015). Data collection for both parts of the experiment took place in August 2018, programmed in oTree (Chen et al., 2016). In total, 216 subjects participated in the online part and 61 in the laboratory part of the experiment. Descriptive statistics for the lab and online samples can be found in Tables A.1 and A.2 in the Appendix. Participants in the laboratory part earned on average 11.66€ in 45-minute sessions including a 4€ show-up fee. Online sessions took place within one week and lasted 13 minutes on average. Judges received 4€ lump-sum in cash for participation in the week after the experiment.[4] Except the bonus for the real-effort tasks, bonuses for judges were paid out separately after the laboratory part had taken place. We informed participants via email and via our homepage about who received a bonus. All cash payments were executed via anonymous participation codes.

3. Results

3.1. Use of bans

We first investigate judges' willingness to use bans with data pooled over the *Pre-choice* and *Post-choice* treatments, as shown in the left bar of Figure 2, Panel A. The majority of judges leaves the decision-maker's choice set unrestricted, thereby granting decision-makers full

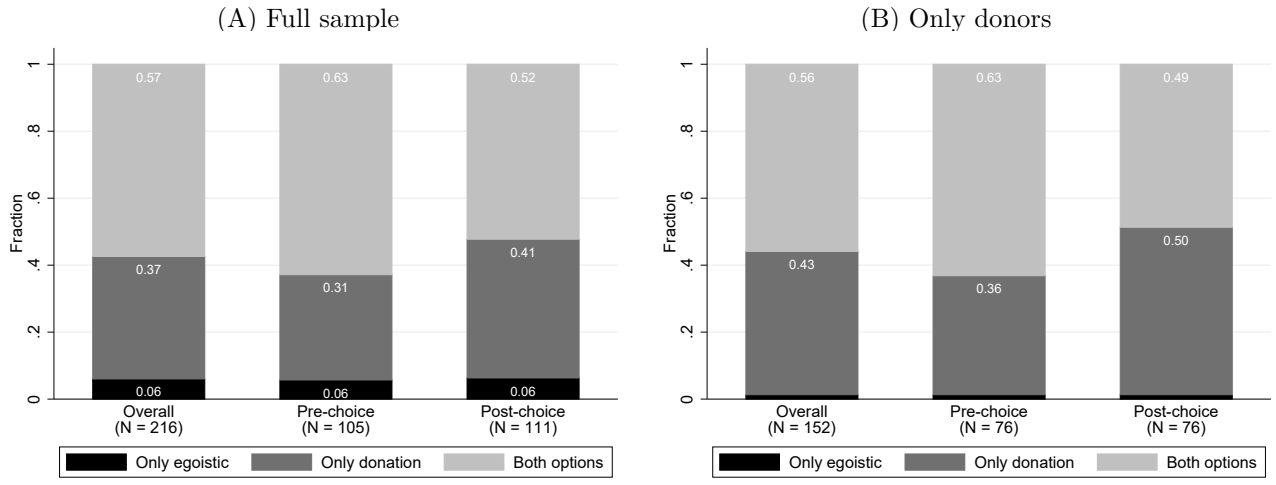
[3]We ask for judges' willingness to accept completing the real-effort task themselves using the BDM-mechanism (Becker et al., 1964). Judges can state any integer amount between 0 and 20€, and we randomly choose one judge who has to solve the 40 counting tasks on her own after the online experiment if her willingness to accept is low enough. With reference to the task's duration, judges guess how long it takes for decision-makers to finish the real-effort task. We elicit this belief in an incentive-compatible way by offering the judge with the guess closest to the true value a bonus of 5€, paid out after the laboratory experiment. In the same way, judges guess which fraction of decision-makers considers the choice between the two payoff alternatives as difficult, and which fraction would like to delegate it.

[4]Judges collected their payoff at our office on campus. Besides the one-week payout period, we added three more days for payoff: One day the week after the bonus was announced (i.e., two weeks after the laboratory experiment) and two days in the first week of the new lecture period. We decided to announce the two subsequent payoff days since we run our experiment during the term break, which resulted in a rather low pick-up rate of payoffs. In total, 47.2% of judges picked up their payoff.

autonomy to decide themselves. 43% of judges use bans. While a few judges, 6%, take away the option to do good and do not allow the donations, 37% enforce the donation. This distribution is highly statistically different from a distribution in which all judges enforce the donation ($p < 0.001$, X^2-test).[5] Moreover, it stands in stark contrast to judges' own prosociality: Overall, 70.4% of judges choose to donate themselves, and 79.2% select the donation on behalf of the decision-maker in case the decision-maker waives his right to decide himself.

When restricting the sample to judges choosing the donation themselves, a surprising share of 56% leaves the decision-makers' choice set untouched as displayed in the left bar of Figure 2, Panel B. Overall, observing the majority of prosocial judges refraining from an intervention by a ban strongly confirms Hypothesis 1b, and is consistent with the notion that judges take decision-makers' autonomy into account when considering to intervene.

Figure 2: Use of bans overall and by treatment



Notes: Full sample and restricted sample of judges donating themselves.

Result 1: The majority of judges does not ban the selfish option of others despite being willing to donate themselves.

[5]The distribution is also different from one in which judges randomize between the three options in case they do not care ($p < 0.001$, X^2-test).

Table 1: Average marginal effects of allowing full/restricted choice set by treatment

	Probability to be type allowing both options					
	(1)	(2)	(3)	(4)	(5) Donor	(6) Delegate
Treatment: Post-choice	-0.105	-0.136**	-0.135**	-0.173***	-0.163**	-0.157**
	(0.066)	(0.063)	(0.063)	(0.055)	(0.076)	(0.070)
Donor		-0.117	-0.098	0.158**		-0.060
		(0.074)	(0.074)	(0.063)		(0.093)
	Probability to be type allowing only donation					
	(1)	(2)	(3)	(4)	(5) Donor	(6) Delegate
Treatment: Post-choice	0.100	0.128**	0.135**	0.141***	0.163**	0.165**
	(0.064)	(0.062)	(0.061)	(0.051)	(0.076)	(0.070)
Donor		0.245***	0.215***	-0.016		0.071
		(0.066)	(0.065)	(0.058)		(0.094)
Demographics	No	Yes	Yes	Yes	Yes	Yes
Altruism controls	No	No	Yes	Yes	No	No
Survey Controls	No	No	No	Yes	No	No
N	216	216	216	216	152	171
Pseudo R2	0.007	0.109	0.132	0.403	0.054	0.074

Notes: Average marginal effects from multinominal logit regression to be the type allowing both choice options, allowing only donation, or allowing only the egoistic option as the dependent categorical variable, with the latter category being omitted. Robust standard errors in parentheses. "Donor" corresponds to restricted sample of judges who would donate themselves; "Delegate" to judges choosing the donation as the delegated choice for the decision-maker. Demographics include age, gender, and a dummy for business/economics students. Altruism controls include the unconditional giving/taking measures constructed from the multiple price lists. Survey controls capture the belief regarding duration and valuation of the real-effort task as well as the full set of attitudes elicited in the post-experimental survey. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

When also considering the point of time when the decision-maker is informed about the available choice set, either before (*Pre-choice*) or after (*Post-choice*) he makes his choice, the right and the middle bars of Figure 2, Panel A, show that the share of judges enforcing the donation is with 41% higher in the *Post-choice* treatment than with 31% in the *Pre-choice* treatment. While this difference is not statistically different in a ranksum test ($p = 0.128$), it seems to indicate a trend towards a higher willingness of judges to intervene in the choice of the decision-maker if they can grant a choice first. When restricting our sample to judges who donate themselves in Panel B of Figure 2, the effect becomes significant at the 10% level ($p = 0.072$). The general intervention pattern remains unchanged with the exception that the share of judges enforcing the egoistic option vanishes. Our second measure of charity valuation, selecting the donation as delegated choice, leads to the same conclusion ($p = 0.079$). The corresponding bar charts for this sample split as well as for judges choosing egoistically themselves are provided in Figure A.1 in the Appendix.

We further investigate the treatment effect using a multinominal logit model, which allows us to predict changes in the probability which intervention category an observation falls into based on our treatment dummy. Table 1 reports average marginal effects from the multinominal logit regressions of being the type allowing both choice options or being the type allowing only the donation, with allowing only the egoistic option as the baseline category.[6] Column (1) shows that without controls there are no significant difference between *Pre-choice* and *Post-choice* treatments. However, when controlling for whether the judge would donate herself in column (2), which is highly predictive for being the type allowing only the donation, as well as demographic factors like age, gender, and economics background, the treatment dummy becomes significant. Subjects in the *Post-choice* treatment are 13.6 percentage points less likely not to intervene in the choice of the decision-maker and 12.8 percentage points more likely to enforce the donation than in the *Pre-choice* treatment, with both marginal effects significant at the 5% level. Taking into account the judge's altruism towards the decision-maker and the charity, respectively, in column (3) does not change this result.[7] This shows that motivations related to spite or inequity do not drive judges' behavior (see Section 4). Moreover, the treatment effect is robust to controlling for the full set of attitudes elicited in the post-experimental survey as well as for contextual factors like the valuation of the real-effort task or beliefs about its duration in column (4).

Columns (5) and (6) show regression results for the restricted sample of judges donating themselves and judges choosing the donation as the delegated option, excluding judges who may not care about the charity. In these restricted samples, we find even 16 percentage points more enforcement of the donation in the *Post-choice* treatment. Table A.3 in the Appendix replicates all findings in a simple linear probability model, ignoring judges forbidding the donation. Overall, this provides evidence in line with Hypothesis 2b, consistent with the notion that many judges desire to respect others' decision-making autonomy – and inconsistent with the Hypothesis 2c that judges are mainly motivated by unburdening decision-makers from the choice between the two alternatives.[8]

[6]For each independent variable, the differences between the upper and the lower panel equals the probability change to be the type forbidding the donation. Judges donating themselves fall 13.9 percentage points less likely into the category of forbidding the donation ($p = 0.002$) in model (2). However, we draw no further inference regarding treatment effects here since only 13 observations fall into this category.

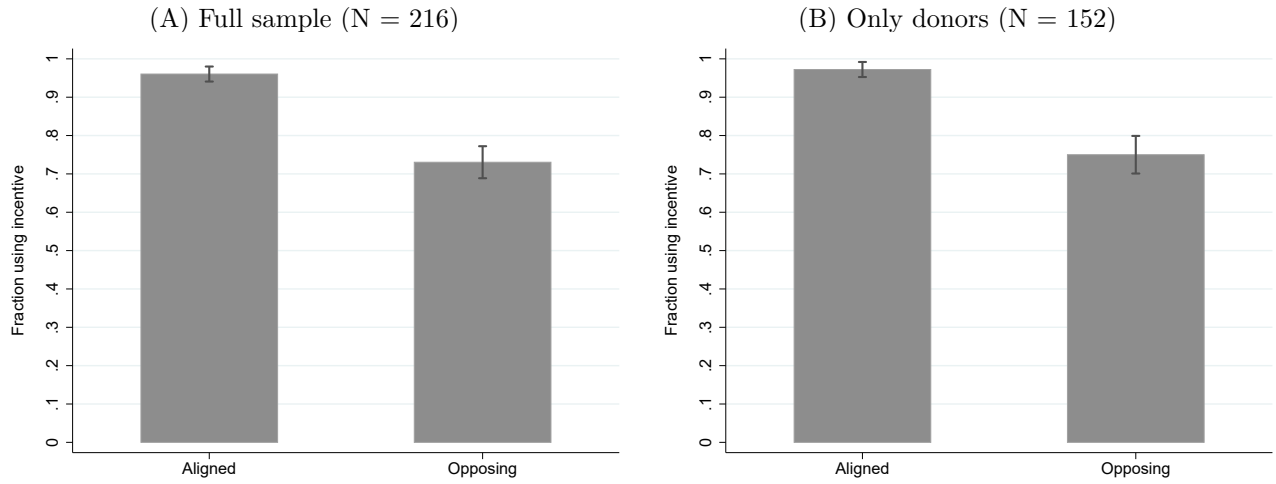
[7]Regarding altruism, we use dummy variables which equal one if the judge always adds and never takes away money in the independent distribution task via the multiple price lists. We construct such dummy variables both for altruism towards the charity and the decision-maker. 74.5% and 57.9% of judges behave altruistically towards the charity and the decision-maker, respectively.

[8]Survey evidence from the post-experimental questionnaire provides additional support for our interpretation that the perseverance of others' autonomy drives judges' willingness to use bans. Remarkably, judges stating a one standard deviation higher intention to benefit primarily the decision-maker with their choice are 19.7 percentage points more likely to allow both choice options and 18.0 percentage points less likely to enforce the donation, with both effects being significant at the 1% level and larger in magnitude than all other effects. A plot of the effects amongst others can be found in Figure A.2 in the Appendix.

Result 2: Judges use bans to enforce donations more often in the *Post-choice* than in the *Pre-choice* treatment.

3.2. Use of incentives

Figure 3: Coefficient plot: Use of incentives



Notes: Coefficient plot from column (2) of Table 2. Vertical lines represent standard errors. Order controls set to value of first period.

We now focus on judges' willingness to use subsequent monetary incentives to change others' behavior into the direction of a donation. Figure 3 displays and Table 2 reports the corresponding regression results from a linear probability model. We find that almost all judges are willing to allow the subsequent incentive if it is added to the payoff of the decision-maker in case he decided to donate anyway. The share of judges allowing this *Aligned* incentive, captured by the constant in column (1) of Table 2, is with 96.0% statistically not different from one ($p = 0.259$). This adds to our previous finding that outcome-based spite or inequity-aversion motives are unlikely to drive judges' behavior, as further discussed in Section 4. In contrast, the corresponding share of judges allowing the *Opposing* incentive is only 73.0%. This 23.0 percentage points drop is captured by the dummy variable for the *Opposing* incentive in the regression, which shows a highly statistically significant decrease ($p < 0.001$).

We can exploit our within-subject design by using a panel data structure to take into account both the decision regarding the *Aligned* and the *Opposing* incentive of each judge. From column (2) on, we estimate the effect of the *Opposing* incentive including judges who decided about this incentive after having already decided about the *Aligned* incentive first and vice versa. We cluster standard errors on the individual level to account for individual heterogeneity. We add a dummy variable capturing that an option is displayed second and interact it with our *Opposing* treatment dummy of interest to control for order effects, which exist but do not differ significantly between treatments.[9]

[9]Taking the chronological order into account may be important. For instance, Roth (2007) mentions that incentives can serve as a "slippery slope" into perceiving transactions as less repugnant over time. However,

We observe a strong treatment effect of the *Opposing* incentive in form of a 22 to 23 percentage points drop in incentive usage compared to the *Aligned* incentive across all full sample specifications in Table 2. When controlling in column (3) for demographics, whether judges would donate themselves, and whether they would allow a directly implemented incentive of the same value, effect size and significance of our treatment variable do not change. Thus, we can rule out that some kind of aversion to the 2€ incentive per se drives our results. The corresponding variable regarding the allowance of the *Direct* incentive is insignificant. Our results are robust to additionally including altruism controls from the multiple price list, various attitudes related to our setting from the post-experimental questionnaire, and contextual factors like judges' valuation of the real-effort task or beliefs about its duration in column (4). Here, the fact that including altruism controls leaves the treatment effect unchanged enables us to further rule out that judges generally do not like to give extra money to potentially donation-unwilling decision-makers. When restricting the sample to judges donating themselves or choosing the donation as the delegated option in columns (5) and (6), respectively, results do not change.

Overall, we conclude that here, too, procedural aspects of decision-making play an important role for judges, consistent with Hypothesis 3b.[10]

Result 3: Subsequent incentives are less acceptable when incentives are *Opposing* than when they are *Aligned*.

4. Relation of our findings to standard social utility models

So far, our hypotheses have been based on the rather coarse distinction between outcome- and procedure-centered motivations. This section explores other hypotheses that can be derived from standard social utility models, and to what extent they can plausibly organize our findings.

Outcome-based models of inequity aversion (Bolton and Ockenfels, 1998, 2000; Fehr and Schmidt, 1999) assume that people care about the equality of payoffs. Because judges earn less than decision-makers in our experiment, they might implement bans and choose bonuses in order to improve their relative payoff standing towards decision-makers. However, since models of inequity aversion postulate preferences over payoff *distributions*, they cannot contribute to explaining differences in the willingness to intervene across the experimental variations within our choice treatments. The sets of payoff distributions are identical for all choices within our

Elias et al. (2015) do not find empirical evidence for such an effect. We find that the incentive shown as second is always less likely allowed by roughly 10 percentage points. That said, the interaction affect between the *Opposing* treatment dummy and the order dummy is not statistically significant, which means that the order effect does not differ systematically between *Aligned* and *Opposing* incentives as the second choice.

[10] Additional survey evidence from the post-experimental questionnaire provides only limited further insights regarding judges' underlying motivations to use incentives. However, when regressing the willingness to use the *Opposing* incentive on all control variables in a linear probability model, one survey question turns out to significantly influence the use of *Opposing* incentives: Judges agreeing by one standard deviation more to the statement "one should not try to dissuade somebody from a decision he made himself" are 7.3 percentage points less likely to use the *Opposing* incentive, as plotted in Figure A.3 in the Appendix. This suggests that an unwillingness to make people change their previous choice influences judges' decision to intervene, which is in line with our autonomy interpretation.

Table 2: Linear probability model: Use of incentives

	Probability to allow subsequent incentive					
	(1)	(2)	(3)	(4)	(5) Donor	(6) Delegate
Opposing incentive	-0.230***	-0.230***	-0.222***	-0.224***	-0.217***	-0.187***
	(0.046)	(0.046)	(0.046)	(0.047)	(0.051)	(0.048)
Displayed 2nd		-0.100***	-0.091**	-0.094**	-0.055	-0.077*
		(0.038)	(0.038)	(0.039)	(0.038)	(0.040)
Opposing inc. # Displayed 2nd		0.082	0.066	0.070	0.022	-0.023
		(0.074)	(0.075)	(0.075)	(0.080)	(0.080)
Direct incentive allowed			0.048	0.036	-0.014	0.049
			(0.049)	(0.051)	(0.048)	(0.054)
Donor			0.086*	0.066		
			(0.044)	(0.050)		
Constant	0.960***	0.960***	0.960***	0.942***	1.245***	1.010***
	(0.019)	(0.020)	(0.117)	(0.123)	(0.095)	(0.141)
Demographics	No	No	Yes	Yes	Yes	Yes
Altruism Controls	No	No	No	Yes	No	No
Survey Controls	No	No	No	Yes	No	No
N	216	432	432	432	304	342
R2	0.097	0.065	0.081	0.129	0.114	0.107

Notes: Standard errors in parentheses. Only judges' first decision considered in column (1) under use of robust standard errors. Panel structure exploited from column (2) on with standard errors clustered on participant level. "Donor" corresponds to restricted sample of judges who would donate themselves; "Delegate" to judges choosing the donation as the delegated choice for the decision-maker. Demographics include age, gender, and a dummy for business/economics students. Altruism controls include the unconditional giving/taking measures constructed from the multiple price lists. Survey controls capture the belief regarding duration and valuation of the real-effort task as well as the full set of attitudes elicited in the post-experimental survey. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

ban treatments and incentive treatments, respectively. For bonuses in particular, allowing the *Aligned* incentive would decrease one's relative payoff compared to that of the decision-maker, standing in stark contrast to inequality aversion. Moreover, our analysis controls for judges' willingness to allocate free money to a decision-maker via a separate decision, thereby ruling out inequality aversion as a motive to intervene. Hence, models of inequality aversion cannot explain our results.

Another behavioral phenomenon which may be applicable to our context is "warm-glow" of giving (Andreoni, 1989, 1990), which postulates that people engage in altruistic behavior because they derive utility from the act of giving. Applied to our setting, the utility generated for the judge from donating one dollar to charity out of own funds may be different than the utility generated for the judge from donating one dollar to charity from someone else's funds. One reason might be that the "warm glow" of giving may be larger when the money is given from one's own account than when the donation is pushed to be given from somebody else's

account.[11] If decision-makers experience only a small warm-glow of giving when they are forced or incentivized to donate, allowing decision-makers to experience a large warm-glow can explain why, for instance, altruistic judges would use bans more often *Post-choice* than *Pre-choice*. With *Post-choice* bans, decision-makers can first choose the donation on their own, allowing them to experience the warm glow. Accommodating decision-makers warm-glow giving preferences may be one reason, among others, why judges consider decision-makers' autonomy of choice as important. Indeed, such a motive may also more generally contribute to the importance of autonomy in decision-making, e.g., when employees decide to engage in corporate social responsibility measures as opposed to being requested by the employer to do so.[12]

Standard models of reciprocity (Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006; Rabin, 1993) assume that judges' benevolence towards decision-makers is influenced by the latters' benevolence towards them. Such models do not contribute to explaining judges' behavior in our setting because these models postulate that reciprocity is triggered by the (un)kindness of the opponent measured by the consequences of the opponent's behavior on *one's own* payoff. Yet, since the decision-maker cannot affect his judge's payoff, there is no scope for a reciprocal reaction by judges in these models. However, there are other notions of reciprocity that allow judges to respond to the decision-maker's initial behavior. Levine's (1998) model of reciprocity, for instance, suggests that players may want to punish others who have shown to be selfish in interactions *with third parties*. Applied to our setting, this model would allow a judge, who knows that the *Opposing* incentive is offered to decision-makers who have been selfish towards the charity, to punish the decision-maker for being a person with selfish motivations (see Bolton et al. (1998, 2005), and the literature cited therein, for a discussion and experimental tests).[13]

Indeed, our data might be interpreted to suggest that while bans are often viewed as tools that restrict choice autonomy, monetary incentives may (also) be viewed as reciprocal rewards or punishments. One piece of evidence for this comes from looking at the relationship between the willingness to enforce the donation by a ban and the willingness to use the *Opposing* incentive. Purely outcome-driven altruistic judges would use whatever tools they have at hand to promote donations. Thus, we would expect that judges, who intervene by a ban, also intervene more frequently by the *Opposing* incentive. If, however, respecting others' autonomy were the dominating concern, we might expect that judges who refrain from bans would more likely refrain from using *Opposing* incentives. Remarkably, we do not find a relationship between

[11] Evidence by Butera and Houser (2018) does not provide support that charitable giving is reduced if it is executed in delegation.

[12] See Briscese et al. (2021), Cassar (2019), and Cassar and Meier (2018) as well as the literature cited therein for related research.

[13] Punishment in this context implies withholding an incentive for donation and not altering the selfish choice. Therefore, punishment leads, in expectation, to a higher payoff for the decision-maker and less donations to the charity. That is, punishment in our context cannot be executed in financial terms as it is standard in the experimental economics literature, but only in terms of the decision-maker's utility.

the use of bans and Opposing incentives.[14] This may be because reciprocity might mitigate the relationship between the usage of the intervention tools: A ban implements the selfish decision-maker's less preferred choice, and so it might be used as a punishment for non-donating decision-makers, whereas the incentive provides the decision-maker with a potentially more attractive option, and hence might be used as a (weak) reward. That is, the availability of different intervention tools might trigger different motives and behavioral mechanisms - reciprocity being one of them - which are not necessarily strongly related to each other.[15]

5. Conclusion

People are surprisingly reluctant to intervene in order to make others behave prosocially, even if they themselves would act prosocially. In our experiment, less than half of those who would donate themselves enforce donations by others through banning the selfish option. And among those who implement a ban, many appear to be affected by a desire to respect others' choice autonomy as much as possible: Interventions by bans are more acceptable to judges if they leave room for choice first, thereby creating (an illusion of) autonomy. Also, there is a strong drop in judges' willingness to use financial incentives to promote prosocial behavior if the incentive conflicts with the decision-makers' previous choice that was independent and unswayed by the judge. This latter observation is consistent with the idea that judges dislike dissuading others from their previous, autonomous choice as predicted by Grant (2006, 2011). However, it is also consistent with the notion that judges refrain from incentivizing donations out of a desire to punish decision-makers who have previously behaved selfishly towards the charity as previously discussed.

In summary, we conclude that procedural aspects of interventions are important determinants for their acceptability. Our results indicate that a desire to respect autonomy organizes many of these procedural aspects, yet we also provide evidence that the underlying motivations for why judges do or do not push others may be more complex and may change with the nature of the intervention.

Since our investigation of which interventions are acceptable, and why, is in many respects exploratory, the behavioral phenomena we find raise new interesting questions. Importantly, there are several subtle differences between bans and incentives that deserve a closer look: A judge who implements a ban fully determines the final outcome, whereas it is still the decision-maker who is responsible for the final outcome under incentives. It would be interesting to study whether, in line with Grant (2006, 2011), it is this feature of the incentive – to act against "one's own wants" – that may render an intervention less appealing. Subsequent research may also focus on whether being self-affected or unaffected by the rules alters how judges set the rules.

[14] Fisher's exact test: $p = 0.622$. See Figure A.4 and Table A.4 in the Appendix for more details on the relationship between the use of bans and the use of *Opposing* incentives.

[15] By this interpretation, reciprocity would promote the use of bans and of incentives beyond what our outcome-centered hypotheses predict. However, our main finding is a surprisingly low level of interventions, much below what is predicted by outcome-centered motivations, so that reciprocity can only be part of the story.

Our project is a first attempt to understand the delicate conflict between fostering a desired social outcome and maintaining the autonomy of the affected parties to decide for themselves, which any intervention comes along with. Further research may also embed our experiment into a non-prosocial domain and could thus test whether autonomy plays a role for unaffected parties under a less pronounced trade-off.

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Appendix A. Further results

Descriptive statistics

Table A.1 shows descriptive statistics of our sample. 216 judges participate in the online part, of which 128 are female, with an average age of 25.7 years. Additionally, there are 61 decision-makers in the lab part, of which 36 are female, with an average age of 26.1 years. 37.5% of our judges study a major in economics, business, or in a related field. As a related field, we consider majors which include a substantial part of courses in business or economics, for example, business law, business informatics, or health economics. While the share of students in business, economics, or a related field is only 29.5% in the sample of decision-makers, both judges and decision-makers are similar in terms of age and gender. Moreover, 70.4% judges choose the donation themselves and 79.2% of them would choose it if their matched decision-maker decided to delegate his choice to them. On top of that, we use dummy variables to measure altruism, i.e., we set altruism to one if the judge always adds and never takes away money in the multiple price lists. When constructing such dummy variables both for altruism towards the charity and the decision-maker, we find that 74.5% and 57.9% of judges behave altruistically.

Table A.2 provides additional insights regarding how often which treatments are implemented in the laboratory part and which choices result. However, these results provide only limited insights due to the small number of observations in each condition and are not further interpreted.

Table A.1: Descriptive statistics: Sample characteristics

	Judges		**Decision-makers**	
	Mean	Std. dev.	Mean	Std. dev.
Female	0.593	0.492	0.590	0.496
Age	25.7	5.2	26.1	8.5
Business/econ major	0.375	0.484	0.295	0.444
Would donate herself	0.704	0.458		
Donation as delegated choice	0.792	0.407		
Altruistic towards decision-maker	0.579	0.495		
Altruistic towards charity	0.745	0.437		
N	216		61	

Notes: Except age, all variables reported as fractions.

Table A.2: Descriptive statistics: Decision-makers

Treatment	**Pre-choice**	**Post-choice**	**Opposing**	**Aligned**	**Direct**	Total
Fraction	0.262	0.262	0.148	0.164	0.164	1.000
Donation chosen by DM	0.100	0.500	0.222	0.300	0.500	0.346
Donation implemented	0.275	0.563	0.222	0.300	0.500	0.410
N	16	16	9	10	10	61

Notes: Donation chosen by decision-maker (DM) is restricted to the 10 decision-makers who are not restricted in their choice by judges and have the possibility to choose themselves in the *Pre-choice* treatment.

Table A.3: Linear probability model of enforcing donation by treatment

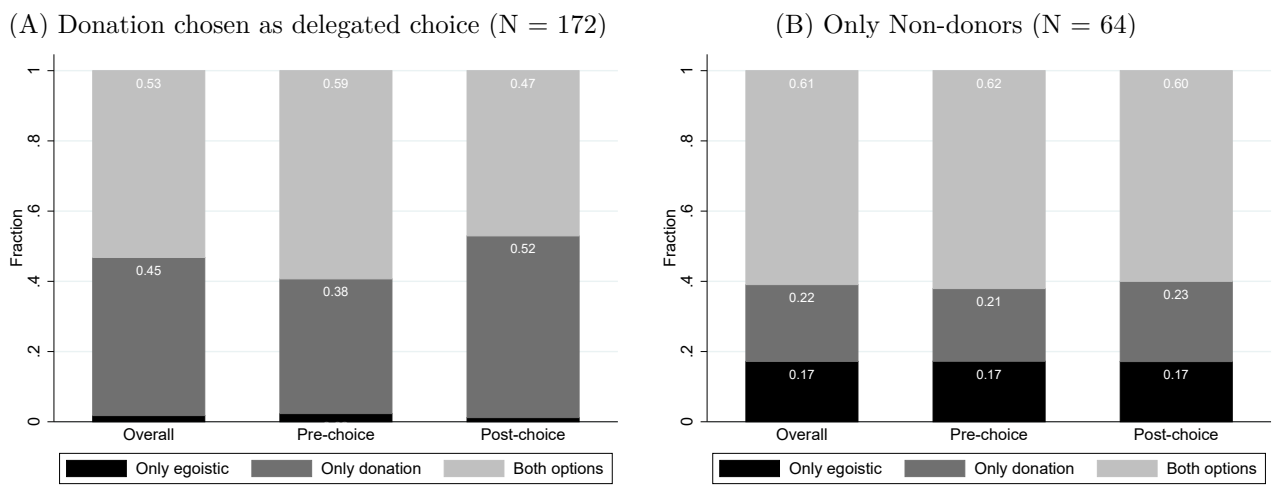
	(1)	(2)	(3)	(4)	(5) Donor	(6) Delegate
Treatment: Post-choice	0.109	0.137**	0.136**	0.154***	0.166**	0.161**
	(0.068)	(0.067)	(0.066)	(0.057)	(0.081)	(0.074)
Donor		0.212***	0.181**	-0.081		
		(0.070)	(0.071)	(0.061)		
Altruism controls	No	Yes	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	No	No
Survey Controls	No	No	No	Yes	No	No
N	203	203	203	203	150	168
R2	0.012	0.088	0.110	0.412	0.064	0.081

Notes: Analysis ignores judges forbidding the donation. Baseline category is allowing both choice options. Standard errors in parentheses. "Donor" corresponds to restricted sample of judges who would donate themselves; "Delegate" to judges choosing the donation as the delegated choice for the decision-maker. Demographics include age, gender, and a dummy for business/economics students. Altruism controls include the unconditional giving/taking measures constructed from the multiple price lists. Survey controls capture the belief regarding duration and valuation of the real-effort task as well as the full set of attitudes elicited in the post-experimental survey. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Survey evidence regarding factors affecting the use of bans and incentives

Several factors influence the use of bans. Figure A.2 plots the regression coefficients and 95% confidence intervals of all main factors, of the altruism dummies, as well as of all other control variables used in column (4) of Table 1, which turn out to be significant at least at the 10% level. Judges studying a business or economics related major are 18 to 22 percentage points more likely to allow both choice options, which is significant at the 1% level. With a magnitude of 11 to 12 percentage points, the same holds for women at the 10% significance level. Furthermore, age turns out to affect the probability not to intervene positively. Intervention behavior seems not to be driven by different assumptions about decision-makers' valuation of the real-effort task or diverging beliefs about the latter's duration since the corresponding estimates show exact zero effects. Altruism towards the decision-maker results more often in allowing only the

Figure A.1: Use of bans overall and by treatment



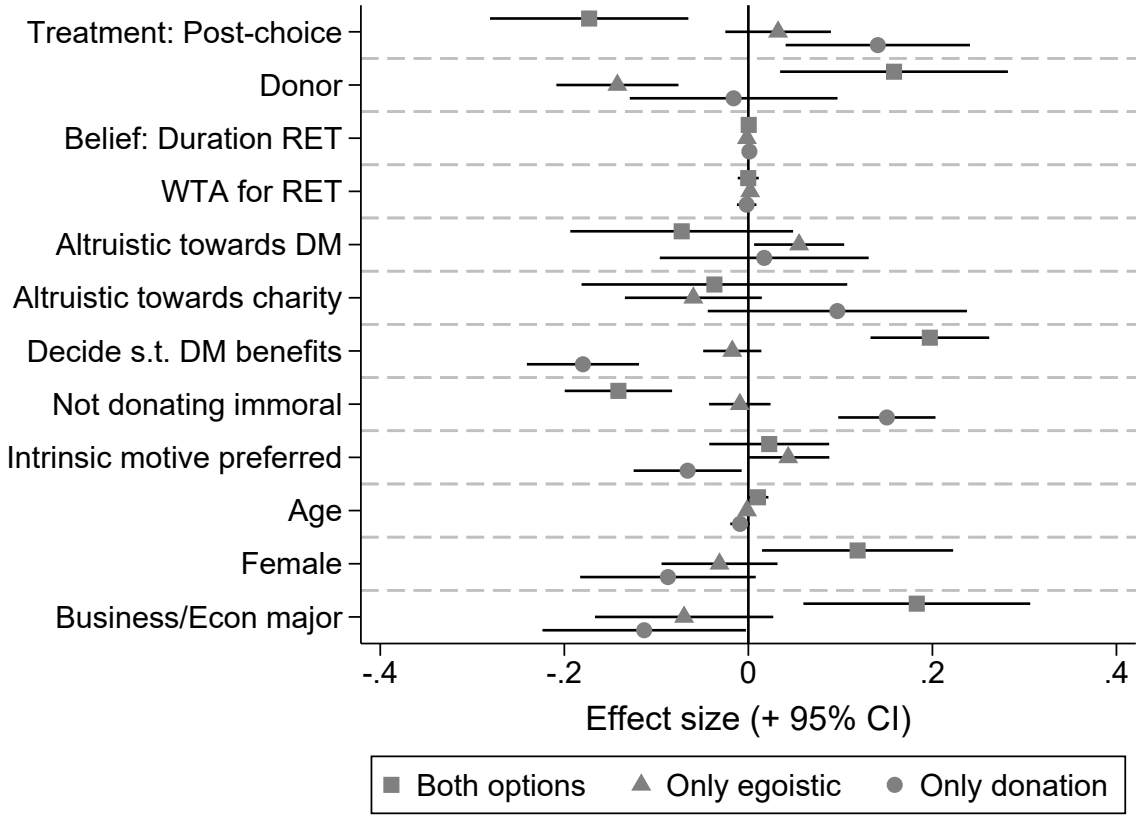
Notes: Restricted samples of judges choosing the donation as the delegated option and judges not donating themselves.

egoistic option. Altruism towards the charity affects intervention behavior only insignificantly in the full model specification, but is depicted for the sake of completeness.

Additional insights, which emerge from the post-experimental survey questions, show that subjects, who judge not donating in the experiment one standard deviation more morally reprehensible, are 15 percentage points more likely to enforce the donation and 14 percentage points less likely not to intervene in the decision (both $p < 0.001$). If judges state a one standard deviation higher preference for actions based on intrinsic motives, they are 4 percentage points more likely to forbid the donation ($p = 0.057$) and 7 percentage points less likely to enforce it ($p = 0.027$), speaking in favor of the idea that judges may value if doing good is free of external influence. Moreover and largest in magnitude, for every standard deviation that subjects decide more to benefit the decision-maker, they are 20 percentage points more likely not the restrict the choice set and 18 percentage points less likely to enforce the donation (both $p < 0.001$). This suggests that judges may try to preserve the autonomy of decision-makers with their choices. Note that the high explanatory power of the items just discussed causes a reversal of the effect of the judge's own willingness to donate on her intervention behavior.

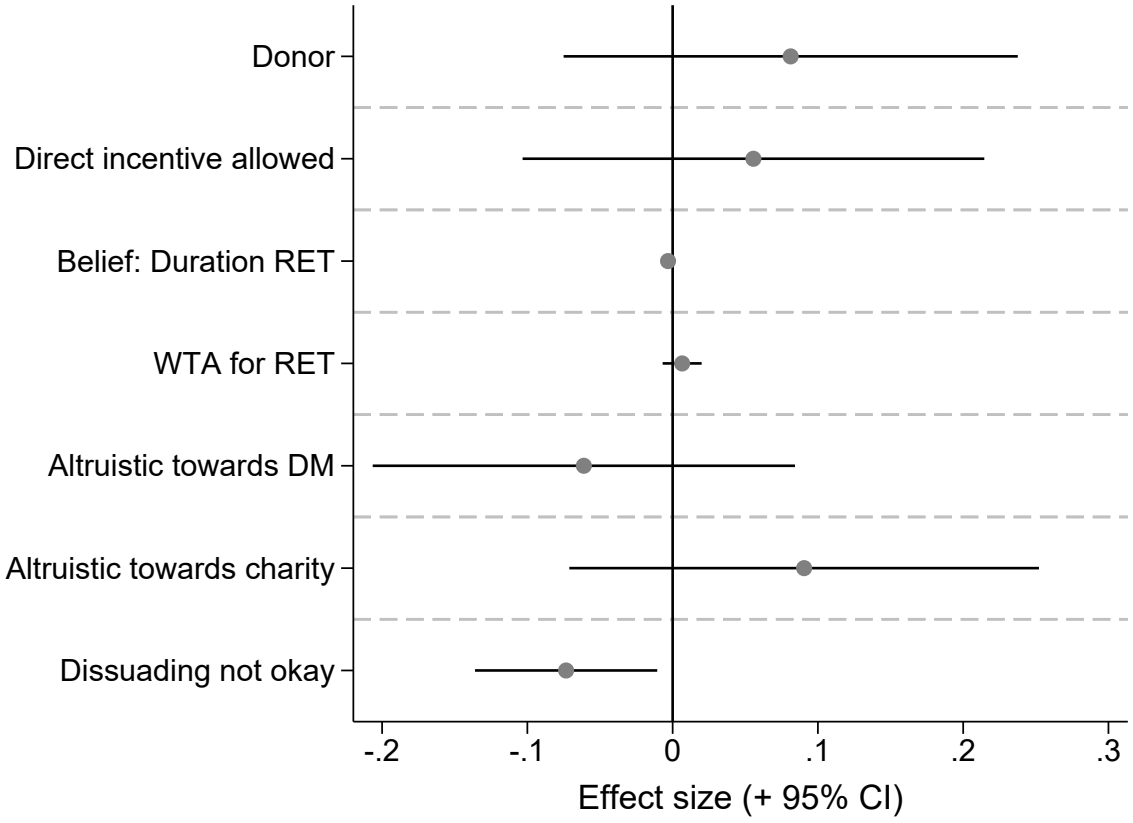
Regarding factors influencing the differential use of incentives, which only applies to *Opposing* incentives, Figure A.3 displays regression coefficients along with 95% confidence intervals from a linear probability model with the use of the *Opposing* incentives as the dependent variable and the full set of control variables as independent variables. Only judges' view of the statement "one should not try to dissuade somebody from a decision he made himself" significantly effects the use of *Opposing* incentives on a significance level lower than 10% ($p = 0.022$). In particular, for every standard deviation a judge agrees more to that statement, she uses the *Opposing* incentive 7.3 percentage points less often. Not wanting others to change a previously made decision due to incentives is in line with the idea that incentives may constitute a threat to autonomy as Grant (2006) proposes. Thus, this further survey evidence supports our autonomy hypothesis.

Figure A.2: Coefficient plot of factors significantly affecting the use of bans



Notes: The figure plots average marginal effects and corresponding 95% confidence intervals with robust standard errors from the multinominal logit regression as specified in column (4) of Table 1 not to intervene by a ban (squares), to forbid the donation (triangles), or to enforce the donation (circles). While all post-experimental survey items are considered in the regression, only those significant at least at the 10% level are depicted.

Figure A.3: Coefficient plot of factors significantly affecting the use of incentives



Notes: The figure plots average marginal effects and corresponding 95% confidence intervals with robust standard errors from regressing allowance of the *Opposing* incentive on the full set of independent variables. While all post-experimental survey items are considered in the regression, only those significant at least at the 10% level are depicted.

Relationship between the use of bans and the use of Opposing *incentives*

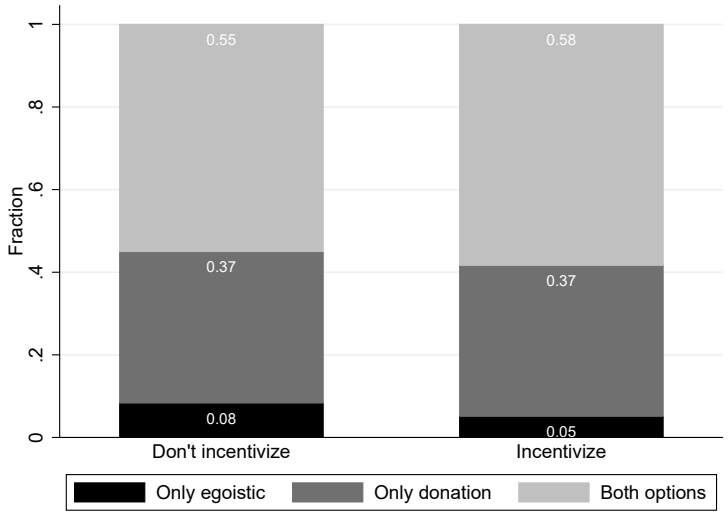
In order to investigate the relationship between the use of bans and the use of *Opposing* incentives, we include the dummy variable for allowing the use of *Opposing* incentives as a single explanatory variable into a multinominal logit model as in Table 1, i.e., in the main analysis regarding the use of bans. In column (1) of Table A.4, the dummy for allowing the Opposing incentive is not significant and possesses only weak exploratory power (Pseudo-$R^2 = 0.002$). Additionally, the dummy does not change previous results when added to the multinominal logit regressions from Table 1 as columns (2) to (4) of Table A.4 show. The previous results and the treatment effect remain unaffected. This also holds when only considering the restricted samples of judges donating themselves or choosing the donation as the delegated option in column (5) and (6), respectively.

Table A.4: Average marginal effects of allowing full/restricted choice set by allowance of *Opposing* incentive

	Probability to be type allowing both options					
	(1)	(2)	(3)	(4)	(5) Donor	(6) Delegate
Opposing incentive	0.032	0.045	0.057	0.014	-0.101	-0.088
	(0.075)	(0.073)	(0.072)	(0.066)	(0.110)	(0.106)
Treatment: Post-choice		-0.132**	-0.131**	-0.173***	-0.163**	-0.157**
		(0.064)	(0.063)	(0.055)	(0.076)	(0.070)
Donor		-0.120	-0.102	0.156**		-0.061
		(0.074)	(0.074)	(0.064)		(0.093)
	Probability to be type allowing only donation					
	(1)	(2)	(3)	(4)	(5) Donor	(6) Delegate
Opposing incentive	-0.002	-0.030	-0.041	0.004	-0.085	-0.161
	(0.073)	(0.071)	(0.068)	(0.063)	(0.111)	(0.109)
Treatment: Post-choice		0.127**	0.134**	0.140***	0.163**	0.162**
		(0.062)	(0.061)	(0.051)	(0.076)	(0.070)
Donor		0.247***	0.217***	-0.016		0.075
		(0.066)	(0.065)	(0.058)		(0.095)
Altruism controls	No	Yes	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	No	No
Survey Controls	No	No	No	Yes	No	No
N	216	216	216	216	152	171
Pseudo R2	0.002	0.111	0.134	0.403	0.058	0.080

Notes: Average marginal effects from multinominal regression to be the type allowing both choice options, allowing only donation, or allowing only egoistic option as the dependent categorical variable, with the latter category being omitted. Robust standard errors in parentheses. "Donor" corresponds to restricted sample of judges who would donate themselves; "Delegate" to judges choosing the donation as the delegated choice for the decision-maker. Demographics include age, gender, and a dummy for business/economics students. Altruism controls include the unconditional giving/taking measures constructed from the multiple price lists. Survey controls capture the belief regarding duration and valuation of the real-effort task as well as the full set of attitudes elicited in the post-experimental survey. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure A.4: Use of bans by use of *Opposing* incentive



Notes: Sample split between judges using (N = 156) or not using (N = 60) the *Opposing* incentive as an interventional tool.

Appendix B. Experimental material

Appendix B.1. Screenshots of treatment differences

Figure B.1: Decision screen judges: Bans *Pre-choice* treatment

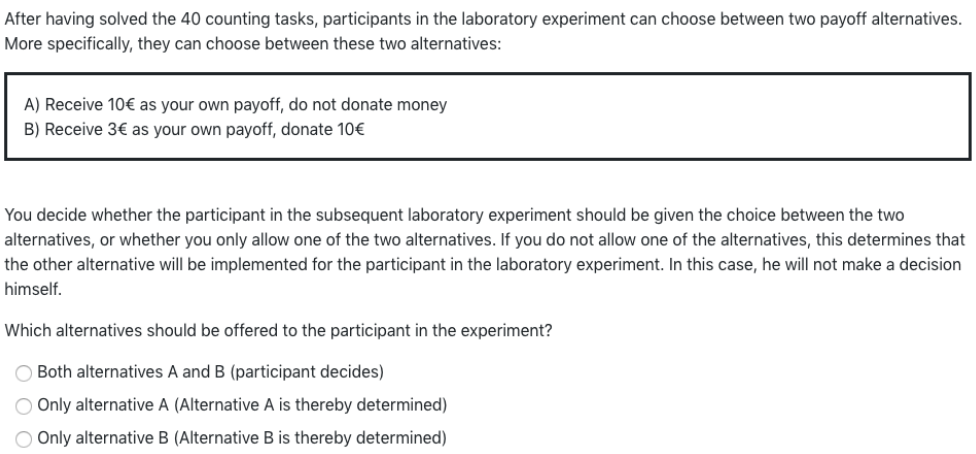


Figure B.2: Decision screen judges: Bans *Post-choice* treatment

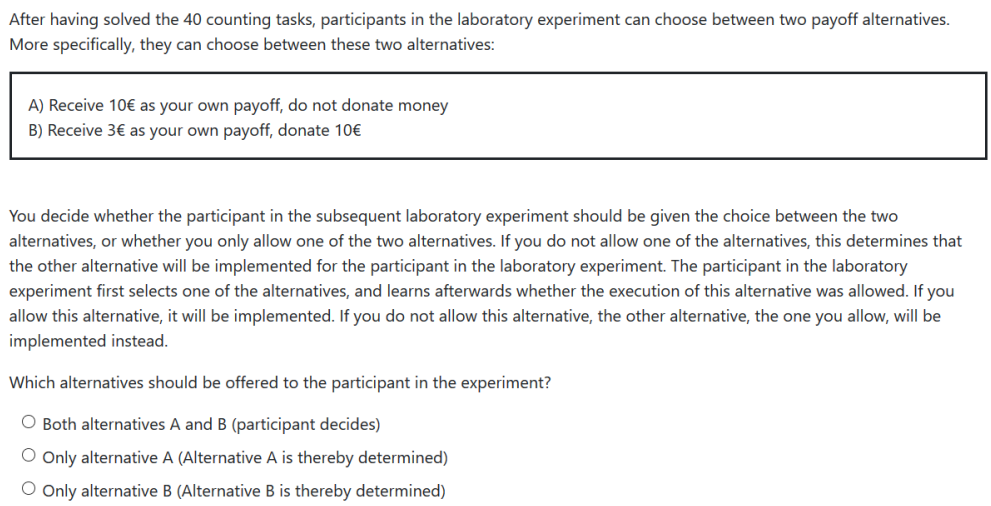


Figure B.3: Decision screen judges: *Opposing* incentives treatment

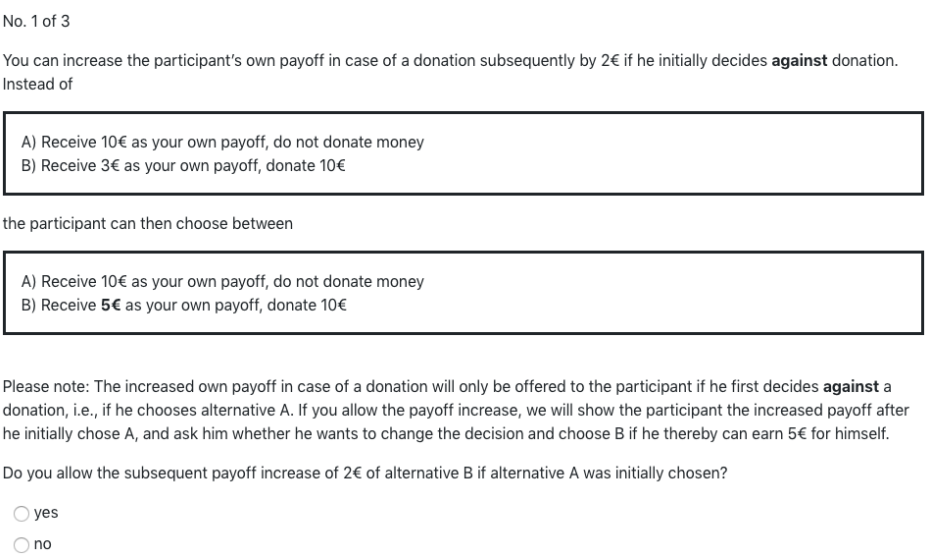


Figure B.4: Decision screen judges: *Aligned* incentives treatment

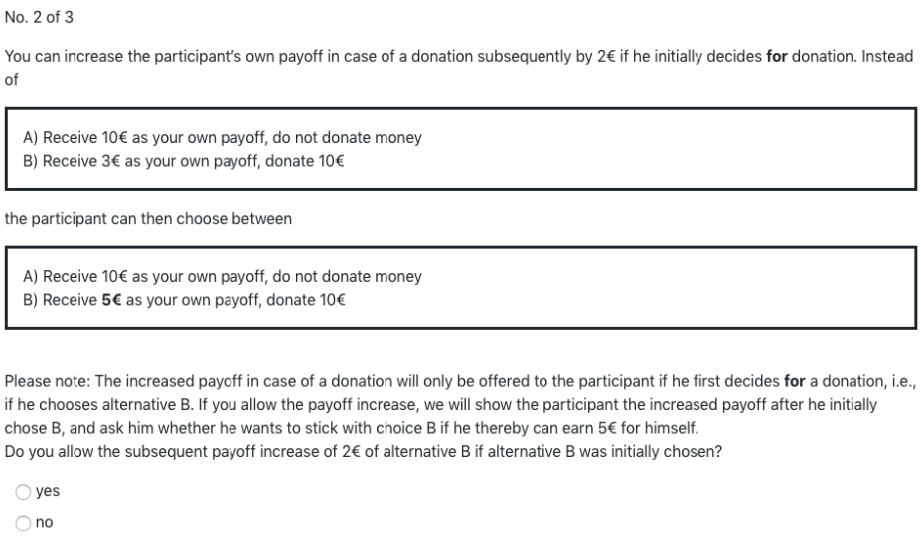


Figure B.5: Decision screen judges: *Direct* incentives treatment

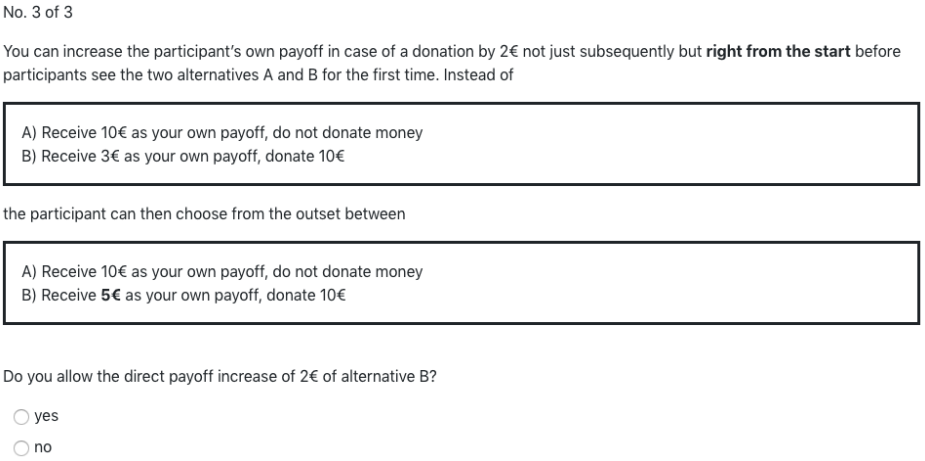


Figure B.6: Decision screen decision-makers: Bans *Pre-choice* treatment

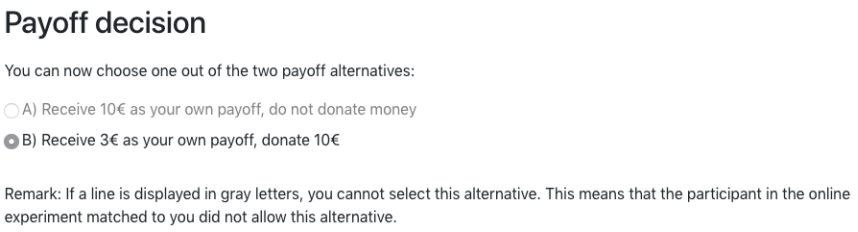


Figure B.7: Decision screen decision-makers: Bans *Post-choice* treatment

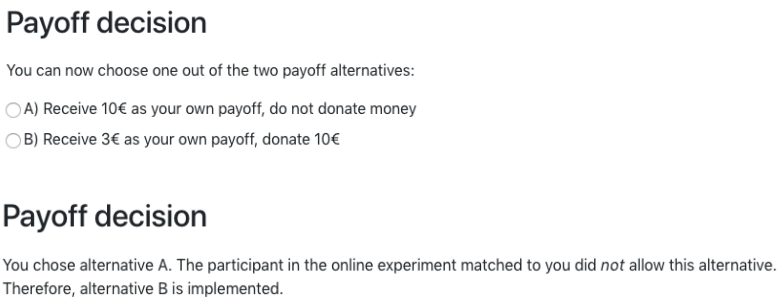


Figure B.8: 2^{nd} decision screen decision-makers: *Opposing* incentives treatment

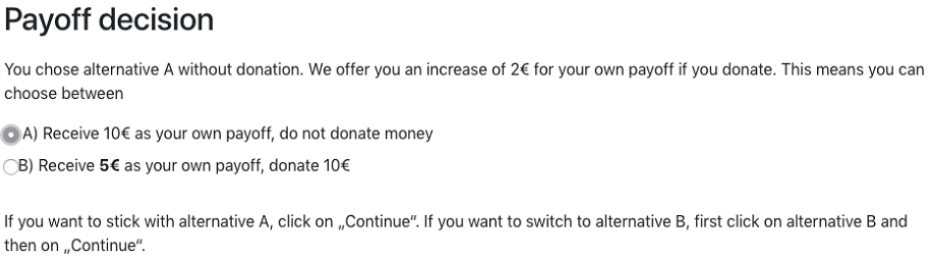


Figure B.9: 2^{nd} decision screen decision-makers: *Aligned* incentives treatment

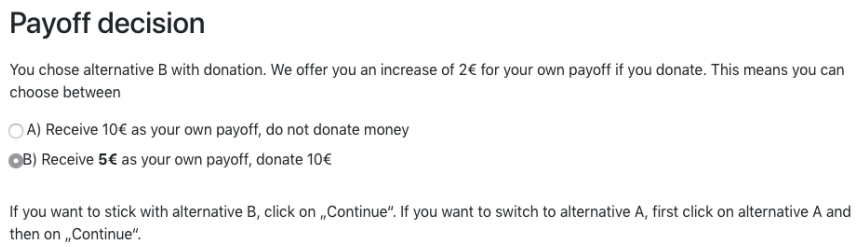
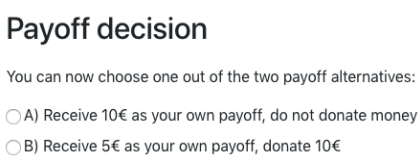


Figure B.10: Decision screen decision-makers: *Direct* incentives treatment



[Translated from German. Treatment differences marked in different colors, randomizations in square brackets and italic]

Instructions online experiment

Information prior to participation

Study title: Online experiment UCDG

Supervisors: Viola Ackfeld, Axel Ockenfels

Description: You participate in a research study on individual decision-making. You will be asked to read instructions on screen, answer questions, and make several choices which determine the amount you are going to be paid.

Participant rights: If you have read this form and have decided to participate in this project, please understand that your participation is voluntary and that you have the right to discontinue participation at any time without penalty or loss of benefits to which you are otherwise entitled.

Payoff: You must answer questions and make decisions in full to receive your payoff. You are going to receive your payoff the week after the experiment in cash in the „Studierenden Service Center“ building of the University of Cologne.

Data protection: All statements are made anonymously, and your individual privacy is maintained in all published data resulting from the study. Data generated will be analyzed by researchers of the Cologne Laboratory for Economic Research (CLER) as well as further authorized researchers, and are stored on a secure server to which only authorized people have access.

I understand that I may contact the supervisor of this experiment if I require further information about the research, and that I may contact the supervisor of this experiment or the Ethics Commission in charge if I wish to make a complaint related to my involvement in the research. I agree to these conditions.

[Yes, participate. / No, don’t participate]

Contact information

Viola Ackfeld: ackfeld@wiso.uni-koeln.de
Ethics Commission: otten@wiso.uni-koeln.de

Instructions

Information regarding the experiment

This experiment consists of two parts. You were randomly chosen from the pool of registered participants of the Cologne Laboratory for Economic Research (CLER) to participate in the first part of the experiment, the online experiment. The second part of the experiment consists of a laboratory experiment, in which further randomly chosen participants, who are registered for experiments at the Cologne Laboratory for Economic Research (CLER) as you are, will participate. It is not possible to participate in both parts of the experiment. The second part of the experiment is going to be executed within the next weeks.

Your task in this online experiment is to specify the rules of the subsequent laboratory experiment. Thus, the choice options and the payoff of a participant in the laboratory experiment depend on your decision. If you allow that participants in the laboratory can choose a particular action, they will be able to do so. If you do not allow it, then participants will not be able to select it. Please take these consequences into account when making your decisions. If there will be more participants in the online experiment than in the laboratory experiment, we will choose randomly with equal chance among all participants of the online experiment whose decisions are going to be implemented.

The online experiment will take about 15 minutes, in a few cases longer. Your payoff in case of full participation in this experiment is **4€**. You receive your payment in cash in the week of August 20 to 24, 2018, at the "Studierenden Service Center". Additionally, you have the possibility to earn a bonus. The bonus will be paid out in cash after the laboratory experiment will have taken place.

Working task

Participants in our laboratory experiment first have to complete a working task: They have to count how many times the number "4" is contained in a block of numbers. The working task is considered to be fulfilled if the participant has successfully completed 40 of such counting tasks. Counting incorrectly does not affect his payoff but extends his time in the laboratory. You find an example of such a counting task below:

1888578030	9476200488
3083111909	1623934883
3734931406	3355722138
4884145680	8044565736
4437896673	9882873762
3294471411	7116717925
9426119243	1599996879
5194173629	9984350367
3903249037	7467204619

Payoff decision

After finishing the 40 counting tasks, participants can choose between two payoff alternatives. In case of choosing the first alternative, they earn a higher payoff for themselves; in case of choosing the other alternative, they earn a smaller payoff for themselves, but donate money to the charity “Menschen für Menschen”. The donation finances an eye surgery in Ethiopia worth 10€ which saves one patient with the disease trachoma in an advanced stage from blinding. After the experiment, we are going to upload the donation receipts on our website ockenfels.uni-koeln.de under the category „Aktuelles”.

Information about the disease trachoma

Trachoma is an eye disease which is caused by infection with Chlamydia trachomatis bacteria, and which is the most common infectious cause of blinding. It is considered as one of the „most neglected tropical diseases” by the World Health Organization (WHO). It most prominently occurs in Africa. Due to trachoma, 1.9 million people suffer from visual impairment or blindness. Blindness from trachoma is irreversible. With your donation, you finance a surgery (called trichiaris surgery) in Ethiopia, which prevents the blinding of one patient due to trachoma.

References: WHO Trachoma fact sheet (2018), Quarcoo and Bundschuh (2015), www.menschenfuermenschen.de, Rainer Kwiotek/Zeitenspiegel

Your decision in this online experiment

In this experiment, you are going to make two consecutive decisions. We are going to randomly draw one out of these two decisions with 50% chance each, and implement it in the laboratory experiment. This means that *either* your first *or* your second decision is going to be implemented in the laboratory experiment, but not both. Since it is uncertain at this point of time which of your decisions will be implemented, please make each of your decision as if it is the one that counts.

First decision

After having solved the 40 counting tasks, participants in the laboratory experiment can choose between two payoff alternatives. More specifically, they can choose between these two alternatives:

A) Receive 10€ as your own payoff, do not donate money
B) Receive 3€ as your own payoff, donate 10€

You decide whether the participant in the subsequent laboratory experiment should be given the choice between the two alternatives, or whether you only allow one of the two alternatives. If you do not allow one of the alternatives, this determines that the other alternative will be implemented for the participant in the laboratory experiment. In this case, he will not make a decision himself. The participant in the laboratory experiment first selects one of the alternatives, and learns afterwards whether the execution of this alternative was allowed. If you allow this alternative, it will be implemented. If you do not allow this alternative, the other alternative, the one you allow, will be implemented instead.

Which alternatives should be offered to the participant in the experiment?

- Both alternatives A and B (participant decides)
- Only alternative A (Alternative A is thereby determined)
- Only alternative B (Alternative B is thereby determined)

Second decision

With a probability of 50% your second decision is going to be implemented in the laboratory experiment.

In this decision, you can alter the payoff of the participant in the laboratory experiment. We are going to show you three versions of payoff alterations of which one is randomly chosen with equal probability and implemented.

No. 1 of 3 [*order of 1 and 2 randomized*]

You can increase the participant's own payoff in case of a donation subsequently by 2€ if he initially decides **against** donation. Instead of

A) Receive 10€ as your own payoff, do not donate money
B) Receive 3€ as your own payoff, donate 10€

the participant can then choose between

A) Receive 10€ as your own payoff, do not donate money
B) Receive **5€** as your own payoff, donate 10€

Please note: The increased own payoff in case of a donation will only be offered to the participant if he first decides **against** a donation, i.e., if he chooses alternative A. If you allow the payoff increase, we will show the participant the increased payoff after he initially chose A, and ask him whether he wants to change the decision and choose B if he thereby can earn 5€ for himself.

Do you allow the subsequent payoff increase of 2€ of alternative B if alternative A was initially chosen? yes/no

---

No. 2 of 3 [*order of 1 and 2 randomized*]

You can increase the participant's own payoff in case of a donation subsequently by 2€ if he initially decides **for** donation. Instead of

A) Receive 10€ as your own payoff, do not donate money
B) Receive 3€ as your own payoff, donate 10€

the participant can then choose between

A) Receive 10€ as your own payoff, do not donate money
B) Receive **5€** as your own payoff, donate 10€

Please note: The increased payoff in case of a donation will only be offered to the participant if he first decides **for** a donation, i.e., if he chooses alternative B. If you allow the payoff increase, we will show the participant the increased payoff after he initially chose B, and ask him whether he wants to stick with choice B if he thereby can earn 5€ for himself.

Do you allow the subsequent payoff increase of 2€ of alternative B if alternative B was initially chosen? yes/no

---

No. 3 of 3

You can increase the participant's own payoff in case of a donation by 2€ not just subsequently but **right from the start** before participants see the two alternatives A and B for the first time. Instead of

A) Receive 10€ as your own payoff, do not donate money B) Receive 3€ as your own payoff, donate 10€

the participant can then choose from the outset between

A) Receive 10€ as your own payoff, do not donate money B) Receive **5€** as your own payoff, donate 10€

Do you allow the direct payoff increase of 2€ of alternative B? yes/no

---

Additonal payments [*order of blocks randomized*]

Independent of your previous decisions, there is a 10% chance that there will be a bonus or deduction payment for the participant or the charity at the end of the laboratory experiment. In that case, one of the ten options stated below is randomly selected with equal probability. You decide whether we implement the corresponding bonus or deduction.

Subtract 3€ lump-sum from donation	yes/no
Subtract 1€ lump-sum from donation	yes/no
Add 1€ lump-sum to donation	yes/no
Add 3€ lump-sum to donation	yes/no
Add 5€ lump-sum to donation	yes/no

Subtract 3€ lump-sum from participant's payoff	yes/no
Subtract 1€ lump-sum from participant's payoff	yes/no
Add 1€ lump-sum to participant's payoff	yes/no
Add 3€ lump-sum to participant's payoff	yes/no
Add 5€ lump-sum to participant's payoff	yes/no

Bonus decisions

At this point, you have the possibility to earn a bonus which will be added to your fixed payoff of 4€. The bonus will be paid out in cash after the laboratory experiment will have taken place. We are going to announce on our homepage who receives a bonus.

Below, you see the two payoff alternatives again. Which alternative would you choose for yourself? For you or another randomly drawn participant in the online experiment, we are going

to execute this choice. Thus, you could receive the corresponding monetary amount as a bonus for yourself, and, if applicable, we would donate 10€ for eye surgery.

A) Receive 10€ as your own payoff, do not donate money
B) Receive 3€ as your own payoff, donate 10€

Bonus decisions

How strongly are you convinced that one should choose alternative [A/B] in this choice situation? You can adjust your answer on a 7-point scale from “fully disagree” to “fully agree”.

Bonus decisions

What do you think, which fraction of the participants in the laboratory experiment, having the choice between both alternatives, will choose the donation? The participant in the online experiment with the closest guess is going to receive a bonus of 5€.

_____% donate

What do you think, which fraction of the participants in the laboratory experiment considers the choice between the two payoff alternatives as difficult (this means they answer the statement “I consider the decision between the two payoff alternatives as difficult.” with 5, 6, or 7 on a scale from 1 (fully disagree) to 7 (fully agree))? The participant in the online experiment with the closest guess is going to receive a bonus of 5€.

___% consider the decision as difficult

Below, you see the two payoff alternatives again. We are going to offer some of the participants in the laboratory experiment the opportunity not to have to make the decision between the two payoff alternatives themselves, but to follow the choice of a participant in the online experiment. If the participant in the laboratory experiment decided to delegate the choice to you, which alternative would you select for him in this case?

A) Receive 10€ as your own payoff, do not donate money
B) Receive 3€ as your own payoff, donate 10€

What do you think, which fraction of the participants in the laboratory experiment would like to delegate the choice? The participant in the online experiment with the closest guess is going to receive a bonus of 5€.

____% would like to delegate the choice

What do you think, how long do participants in the laboratory experiment need on average to solve the 40 counting tasks? The participant in the online experiment with the closest guess is going to receive a bonus of 5€.

___ minutes

For which monetary amount would you be willing to solve the 40 counting tasks yourself? For you or another randomly drawn participant in the online experiment, we are going to execute this choice. If the number you stated is smaller than a number randomly generated by the computer, you have to solve the counting tasks, and receive the higher amount as a bonus. If the number is bigger, you will not receive the bonus. Under these conditions, the best you can do is to state the true amount for which you are willing to solve 40 counting tasks. You can state [1,2,3,4,5,6,7,8,9,10,11,12,13,14,15,16,17,18,19,20] € (no decimal numbers) as the amount you want to receive, and you have to solve the 40 counting tasks at the end of the experiment if you are randomly chosen among all participants in the online experiment. You receive the bonus for the counting tasks together with your fixed payment next week.

I request ____€ to solve 40 counting tasks.

Questionnaire

Please answer the following questions. You can adjust your answer on a 7-point scale from “fully disagree” to “fully agree”.

I consider the decision between the two payoff alternatives as difficult.

The participant in the laboratory experiment holds it against me if I take the possibility to decide between the two payoff alternatives himself away from him.

I decided such that the participant benefits first and foremost.

I wish that the participant in the laboratory experiment receives a fair payoff for solving the counting tasks.

In this experiment, it is immorally reprehensible not to donate.

Questionnaire

Please answer the following questions. You can adjust your answer on a 7-point scale from “fully disagree” to “fully agree”.

One should not try to dissuade somebody from a decision he made himself.

People are glad if they are not confronted with a difficult decision.

It is okay to override the decisions of others if it is in their own interest.

It is better if people act by intrinsic motives (e.g., interest, conviction, …) than by extrinsic motives (e.g., duty, payment, …).

If somebody is willing to donate an organ, nobody should hinder him from doing so.

People should get money for donating an organ.

Personal information

Please provide the following information about yourself:

Age: ____ years

Gender: male, female, other

Department: WiSo Fakultät, Rechtswissenschaftliche Fakultät, Medizinische Fakultät, Philosophische Fakultät, Mat-Nat Fakultät, Humanwissenschaftliche Fakultät, other, I'm not a student

Major: _____

Semester: ___

Nationality: _____

How much disposable money do you have per month? [0 – 200 €], [200 – 400 €], [400 – 600 €], [600 – 800 €], [800 – 1000 €], [1000 – 1200 €], [more than 1200 €], [I prefer not to answer]

Comments: _____

[if selected for counting task & WTA small enough] **Counting task**

You were selected to solve the 40 counting tasks yourself, and stated an amount of [*WTA*]€ for doing so. You now have to solve the tasks, and you are going to receive a bonus of **y€** in exchange.

[if selected for counting task & WTA small enough] **Counting task**

Task solved so far: [z]

1888578030	9476200488
3083111909	1623934883
3734931406	3355722138
4884145680	8044565736
4437896673	9882873762
3294471411	7116717925
9426119243	1599996879
5194173629	9984350367
3903249037	7467204619

How often do you count the number “4”? _____

The experiment is finished now!

Below, you see a code. Please write down the code or take a picture of it, and show us the code when collecting your payoff. You receive your payoff of 4[+y]€ in the week of August 20 to 24, 2018, in the “Studierenden Service Center” building (SSC), room 4.226, daily from 10am to 12pm, and from 1am to 4pm. We are going to inform you about bonus payments subsequently via our homepage ockenfels.uni-koeln.de.

Your code is ***[Code]***.

Thank you very much for your participation in this online experiment and your support of our research!

Address questions regarding this experiment to ackfeld@wiso.uni-koeln.de, and complaints to ackfeld@wiso.uni-koeln.de or to otten@wiso.uni-koeln.de as the representative of the ethics commission in charge.

Instructions laboratory experiment

Information prior to participation

Study title: Laboratory experiment UCDG

Supervisors: Viola Ackfeld, Axel Ockenfels

Description: You participate in a research study on individual decision-making. You will be asked to read instructions on screen, answer questions, and make several choices which determine the amount you are going to be paid.

Participant rights: If you have read this form and have decided to participate in this project, please understand that your participation is voluntary and that you have the right to discontinue participation at any time without penalty or loss of benefits to which you are otherwise entitled. Your right to receive a show-up fee of 4€ is preserved even when discontinuing the experiment.

Payoff: You must answer questions and make decisions in full to receive your payoff.

Data protection: All statements are made anonymously, and your individual privacy is maintained in all published data resulting from the study. Data generated will be analyzed by researchers of the Cologne Laboratory for Economic Research (CLER) as well as further authorized researchers, and are stored on a secure server to which only authorized people have access.

I understand that I may contact the supervisor of this experiment if I require further information about the research, and that I may contact the supervisor of this experiment or the Ethics Commission in charge if I wish to make a complaint related to my involvement in the research. I agree to these conditions.

[Yes, participate. / No, don't participate]

Contact information

Viola Ackfeld: ackfeld@wiso.uni-koeln.de

Ethics Commission: otten@wiso.uni-koeln.de

Instructions

Information regarding the experiment

This experiment consists of two parts. You participate in the second part of the experiment, the laboratory experiment. Other participants, who are registered for experiments at the Cologne Laboratory for Economic Research (CLER) as you are, and who were also randomly chosen, have already participated in the first part of the experiment, the online experiment. It is not possible to participate in both parts of the experiment.

Participants in the online experiment determined the rules for this laboratory experiment. These rules are now implemented. Independent of the decisions of the participant matched to you and your own decisions you are guaranteed to receive a show-up fee of 4 €.

Working task

As a participant in our laboratory experiment, you first have to complete a working task: You have to count how many times the number “4” is contained in a block of numbers. The working task is considered to be fulfilled if you have successfully completed 40 of such counting tasks. Counting incorrectly does not affect your payoff but extends your time in the laboratory.

Payoff decision

After finishing the 40 counting tasks, you can choose between two payoff alternatives. In case of choosing the first alternative, you earn a higher payoff for yourself; in case of choosing the other alternative, you earn a smaller payoff for yourself, but you donate money to the charity “Menschen für Menschen”. The donation finances an eye surgery in Ethiopia worth 10€ which saves one patient with the disease trachoma in an advanced stage from blinding. After the experiment, we are going to upload the donation receipts on our website ockenfels.uni-koeln.de under the category „Aktuelles”.

Information about the disease trachoma

Trachoma is an eye disease which is caused by infection with Chlamydia trachomatis bacteria, and which is the most common infectious cause of blinding. It is considered as one of the „most neglected tropical diseases” by the World Health Organization (WHO). It most prominently occurs in Africa. Due to trachoma, 1.9 million people suffer from visual impairment or blindness. Blindness from trachoma is irreversible. With your donation, you finance a surgery (called trichiaris surgery) in Ethiopia, which prevents the blinding of one patient due to trachoma.


References: WHO Trachoma fact sheet (2018), Quarcoo and Bundschuh (2015), www.menschenfuermenschen.de, Rainer Kwiotek/Zeitenspiegel

You can choose between the following two payoff alternatives:

A) Receive 10€ as your own payoff, do not donate money

B) Receive 3/5€ as your own payoff, donate 10€

We offer 10% of the participants in the laboratory experiment the possibility to delegate the choice between the two alternatives. In that case, the decision of one of the participants in the online experiment will be executed, who already made a decision for such a case. We will inform you whether you belong to that 10% before you may have to decide.

First, please work on the working task consisting of 40 counting tasks.

Counting tasks

Task solved so far: [z]

1888578030	9476200488
3083111909	1623934883
3734931406	3355722138
4884145680	8044565736
4437896673	9882873762
3294471411	7116717925
9426119243	1599996879
5194173629	9984350367
3903249037	7467204619

How often do you count the number “4”? _____

Counting tasks

You solved 40 counting tasks. This means that the working tasks is finished.

Delegation

[*if 10% + strategy method treatment*] You belong to the 10% of participants who can delegate their choice between the two payoff alternatives. What do you want to do? delegate/choose yourself [jump to payoff if delegate chosen]

Payoff decision

You can now choose one out of the two payoff alternatives:

A) Receive 10€ as your own payoff, do not donate money
B) Receive 3/5€ as your own payoff, donate 10€

Which alternative do you want to choose? Please click on the corresponding line.

[*if pre treatment*] Remark: If a line is displayed in gray letters, you *cannot* select this alternative. This means that the participant in the online experiment matched to you did not allow this alternative.

Payoff decision

[if A + treatment 2a]

You chose alternative A without donation. We offer you an increase of 2€ for your own payoff if you donate. This means you can choose between

A) Receive 10€ as your own payoff, do not donate money
B) Receive **5€** as your own payoff, donate 10€

If you want to stick with alternative A, click on „Continue“. If you want to switch to alternative B, first click on alternative B and then on „Continue“.

[if B + treatment 2b]

You chose alternative B with donation. We offer you an increase of 2€ for your own payoff if you donate. This means you can choose between

A) Receive 10€ as your own payoff, do not donate money

B) Receive **5€** as your own payoff, donate 10€

If you want to stick with alternative B, click on „Continue“. If you want to switch to alternative A, first click on alternative A and then on „Continue“.

[*if post treatment*] **Payoff decision**

You chose alternative A/B. The participant in the online experiment matched to you did *[not]* allow this alternative. Therefore, alternative A/B is implemented.

Payoff

Alternative A/B is implemented. You receive a payoff of 3/5/10€, and 0/10€ are donated for eye surgery in Ethiopia.

Questionnaire

Please answer the following questions. You can adjust your answer on a 7-point scale from “fully disagree” to “fully agree”.

I consider the decision between the two payoff alternatives as difficult.

I would hold it against the participant in the online experiment if he took the possibility to decide between the two payoff alternatives myself away from me.

In this experiment, it is morally reprehensible not to donate.

Questionnaire

Please answer the following questions. You can adjust your answer on a 7-point scale from “fully disagree” to “fully agree”.

One should not try to dissuade somebody from a decision he made himself.

People are glad if they are not confronted with a difficult decision.

It is okay to override the decisions of others if it is in their own interest.

It is better if people act by intrinsic motives (e.g., interest, conviction, …) than by extrinsic motives (e.g., duty, payment, …).

If somebody is willing to donate an organ, nobody should hinder him from doing so.

People should get money for donating an organ.

Personal information

Please provide the following information about yourself.

[if B + treatment 2a or A + treatment 2b] For filling in this questionnaire, you receive an additional payoff of 2€.

Age: ____ years

Gender: male, female, other

Department: WiSo Fakultät, Rechtswissenschaftliche Fakultät, Medizinische Fakultät, Philosophische Fakultät, Mat-Nat Fakultät, Humanwissenschaftliche Fakultät, other, I'm not a student

Major: _____

Semester: ___

Nationality: _____

How much disposable money do you have per month? [0 – 200 €], [200 – 400 €], [400 – 600 €], [600 – 800 €], [800 – 1000 €], [1000 – 1200 €], [more than 1200 €], [I prefer not to answer]

Comments: _____

The experiment is finished now!

[if bonus/deduction] In a further decision, the participant in the online experiment matched to you decided that a lump-sum of [x]€ is added/subtracted to/from your payoff.

Including your show-up fee of 4€ and your payoff from the donation decision, you receive a final payoff of [4+3/5/10€+bonus/deduction]€.

Please fill in this amount in the receipt provided, and wait until the experimenter calls you for payout.

Thank you very much for your participation in this experiment and your support of our research!

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