Essays on Repugnance in Economic Transactions

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Introduction

The efficient exchange of goods and services is the core theme of economics and has been extensively studied for decades. However, with respect to some repugnant types of goods like organ and blood donations, personal data, or CO_2 emissions, there seems to exist an aversion to trade these goods at all or at least if money is involved. As Nobel Laureate Al Roth emphasizes, "distaste for certain kinds of transactions can be a real constraint [. . .], every bit as real as the constraints imposed by technology or by the requirements of incentives and efficiency" (Roth 2007, p. 38).

In this thesis, I study situations of economic exchange in terms of their ethical acceptability. Particularly, I investigate the scope and limits of social and monetary incentives by means of experiments in lab, online, and field settings. In the first two chapters, I look at what conditions make people refrain from fostering the implementation of their favored outcome for others. More precisely, I examine people's willingness to intervene in others' decision-making by monetary incentives along with choice-prescribing bans (Chapter 1) or along with information (Chapter 2). My results demonstrate that the perseverance of others' autonomy to decide for themselves (Chapter 1) as well as people's aversion to make others do something which the latter are not convinced of (Chapter 2) play an important role when judging the acceptability of economic interventions. In Chapters 3 and 4, I zoom in on one particular good, whose trade may be considered as repugnant, namely the sharing of personal data, and investigate issues with personal data sharing. In Chapter 3, I point out that personal data disclosure competition in modern online markets with both social comparison and high incentives for information sharing can increase information available in the economy, but may come along with a hidden cost of perceived pressure on disclosure-unwilling individuals with high privacy concerns. Focusing not on competition via personal information disclosure but on personal data as contributions to a public good, Chapter 4 highlights that making privacy protection salient together with public benefits may increase personal data contributions to a public good. Taken together, the four chapters of my thesis highlight important ethical but yet economically underesearched issues that have to be taken into account when designing new forms of economic exchange.

In what follows, I provide a short overview of each of the four chapters. In Chapter 1, titled "Protecting Autonomy versus Intervening to Promote Prosocial Behavior", which is joint work with Axel Ockenfels (University of Cologne), we investigate people's willingness to intervene in others' prosocial decision-making in light of a trade-off between promoting prosocial behavior and protecting the affected party's autonomy to decide for herself.[*] In an experiment, in which one group of participants determines the experimental rules for another group of participants within an charitable donation paradigm, we find that a majority of subjects who would donate to charity themselves refrains from interfering at all with others' autonomous charitable decision-making. People who intervene perceive interventions - which correspond to choice-prescribing bans and monetary incentives in the experiment - as more acceptable to promote prosocial behavior the more the interventions respect the autonomy of affected parties. More precisely, choice-prescribing bans are preferred if they first grant a feeling of perceived autonomy to let others decide for themselves, and monetary incentives prevail less acceptable if they are implemented against instead of aligned with one's own previous wants.

Chapter 2 studies an aversion to monetary incentives to make other people change their behavior and is accordingly called "The Aversion to Monetary Incentives for Changing Behavior". I provide evidence that monetary incentives are disliked because they are powerful in changing *what* people do but not *why* they do it besides for money. In an experiment, one group of participants decides about interventions which try to change others' behavior such that the latter donate to charity. I vary between treatments whether subjects can intervene by means of convincing information or monetary incentives. In additional treatments, the intervention corresponds to monetary incentives for subjects, who already received information, or of incentivizing information acquisition. While participants consider monetary incentives as more effective in changing behavior than informa-

[*]Both authors contributed equally to this project. They invented the research question and the experimental design together. Viola Ackfeld planned and conducted the experiment, analyzed the data, and prepared the first draft. Both authors wrote the final version of the paper together.

tive interventions, they are still less willing to intervene by monetary incentives compared to informative interventions to foster their preferred outcome, especially if people already made an informed decision. A comprehensive set of elicited beliefs supports the idea that this aversion to incentives stems from incentives' lack of changing one's reason to act.

Chapter 3 is named "Personal Information Disclosure under Competition for Benefits: Is Sharing Caring?" and is joint work with Werner Güth (LUISS Rome and Max Planck Institute for Research on Collective Goods).[†] In this paper, we investigate monetary incentives and social comparisons with other people's information sharing as motives for extensive personal information sharing. Moreover, we study consequences thereof. More precisely, in an experiment, we analyze the interaction of peer comparison and incentives to disclose potentially privacy-sensitive information. We find that information sharing is higher under incentives and further increases with additional peer comparison. Individuals, who initially disclose less information, react the most to the combination of incentives and social comparison, but also report to feel more compelled to disclose information. While increasing the availability of information, reliance on extensive information sharing does not prevail as a useful device to screen prosocial types. Our results provide an explanation for the current information-sharing trend, but also point to potentially neglected side effects of extensive personal information sharing.

The last chapter is called "Increasing personal Data Contributions: Field Experimental Evidence from an Online Education Platform" and is joint work with Tobias Rohloff (Hasso Plattner Institute and University of Potsdam) and Sylvi Rzepka (University of Potsdam).[‡] In this paper, we study personal data sharing as a contribution to a public good. In a field experiment on an online education platform, users are prompted to complete their user profiles. We examine whether public good contributions in form of personal data increase when varying the salience of public benefits and perceived privacy

[†]Both authors contributed equally to this project. They invented the research question and the experimental design together. Viola Ackfeld planned and conducted the experiment, analyzed the data, and prepared the draft. Werner Güth gave feedback on the draft.

[‡]All authors contributed equally to this project. Sylvi Rzepka and Viola Ackfeld developed the research question and the experimental design together. Tobias Rohloff implemented the experiment on the online platform and provided the necessary data. Viola Ackfeld analyzed the data. Sylvi Rzepka gave feedback on the statistical analysis. After Viola Ackfeld prepared the first draft of the paper, Viola Ackfeld and Sylvi Rzepka wrote the final version of the paper together. Tobias Rohloff gave feedback on the draft.

costs. Relative to a control message, we find that salience of the public benefits increases the number of contributed profile information. This effect further increases when adding a reference to privacy protection. However, we do not find proof that such treatments can also motivate users, who initially do not provide any personal data, to start contributing. These results highlight that even in a fast-paced environment like an online platform, reference to the social benefit may increase people's willingness to contribute personal data, but primarily for those who are somewhat willing to share such information.

Taken together, the results in all four chapters of this thesis contribute to a better understanding of ethical factors that influence economic decision-making and the scope and limits of social and monetary incentives. While insights from psychology and sociology have made their way into economic research via behavioral economics in the last decades, the inclusion of ethical aspects is still rare. This thesis highlights that including insights from the ethics literature can improve our understanding of decision-making further and prepares the ground for a new direction of interdisciplinary research.

Chapter 1

Protecting Autonomy versus Intervening to Promote Prosocial Behavior*

joint with Axel Ockenfels

Abstract

We experimentally investigate people's willingness to intervene in others' prosocial decision-making when facing a trade-off between promoting prosocial behavior and protecting the affected party's choice autonomy. We find that a majority of subjects who would give for charity themselves refrain from interfering at all with others' autonomous charitable decision-making. For those who intervene, the interventions – bans and monetary incentives – are more acceptable to promote prosocial behavior the more the interventions respect the autonomy of others.

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1.1 Introduction

The question which interventions increase prosocial behavior has gained much attention in empirical economic research (see for example Andreoni (2015) and Lacetera et al. (2013) for reviews on how to increase charitable fundraising and blood donations, respectively). However, the question which interventions are "acceptable" has gained much less attention. In this paper, we investigate which interventions third-parties are willing to use in order to influence others' prosocial behavior. In particular, we study a trade-off between promoting prosocial behavior and interfering with the affected party's autonomy to decide for himself. 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. 2012; Benz and Frey 2008; Cassar and Meier 2018; Fehr et al. 2013; Leider and Kessler 2016). Thus, a demand for autonomy may limit the acceptability of interventions to promote social behavior.

In this paper, we consider two kinds of interventions, for which this trade-off is particularly strong:[1] bans that remove the selfish option and thus directly enforce the prosocial action, and monetary payments incentivizing the prosocial action. While bans are effective, they leave no room for autonomous choice. In contrast, monetary incentives allow choice, but still may stand in conflict with some notion of autonomy – if trying to dissuade people from the choice they would have made without being incentivized (Grant 2006, 2011).

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. The former decide whether to use bans and incentives to channel the latter's choice between a charitable donation of 10€, which yields only 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. Judges

[1] A third interventional tool to consider would be nudges (Thaler and Sunstein 2008). However, nudges are a rather soft form of intervention, not affecting one's choice menu, so they do not create an equally strong trade-off between autonomy and outcomes.

can first either take away one of the two choice options from the decision-maker or leave his choice set unaffected. We vary whether the decision-maker is informed about the judge's decision before or after he made his own choice. While the former leaves no room for the decision-maker to make a choice himself, the latter grants autonomy for doing good, but may still subsequently alter the outcome in case the donation is not chosen. In a second decision, 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. In a control treatment, the bonus is offered to those choosing the donation anyway.

Suppose a 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). Then, if the autonomy of the decision-maker is irrelevant for the judge, this judge should be willing to use bans and incentives to promote a donation.

However, we find that more than half of the judges who donate themselves do not intervene 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. We interpret this overall pattern of interventions as evidence for many judges respecting the decision-makers' demand for autonomy. Moreover, our data suggest that while judges dislike to incentivize decision-makers to act "against their own will", subsequently banning preferred outcomes does not seem to raise much concerns. Indeed, we do not find a statistical relationship between subjects' willingness to use bans and incentives to influence others' donation behavior on the individual level, which reinforces our view that judges do not only care about outcomes, but also about the decision process by which this outcome is reached. Different interventions may therefore be perceived differently threatening to autonomy.

Our paper is related to several interdisciplinary strands of literature. Autonomy is an important 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 (2000b) find that paying subjects for charitable fundraising activities decreases effort. When looking at autonomy in particular, there is laboratory evidence that people reduce their effort when experiencing more control and thereby less autonomy (Bartling et al. 2012; Fehr et al. 2013). Leider and Kessler (2016) highlight that this negative reaction to control stems from procedural fairness concerns being violated. Similarly, 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 provide empirical support for the importance of autonomy in decision-making along those lines.

With reference to research on philantrophy, there are many different 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 into others' prosocial decision-making. 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.

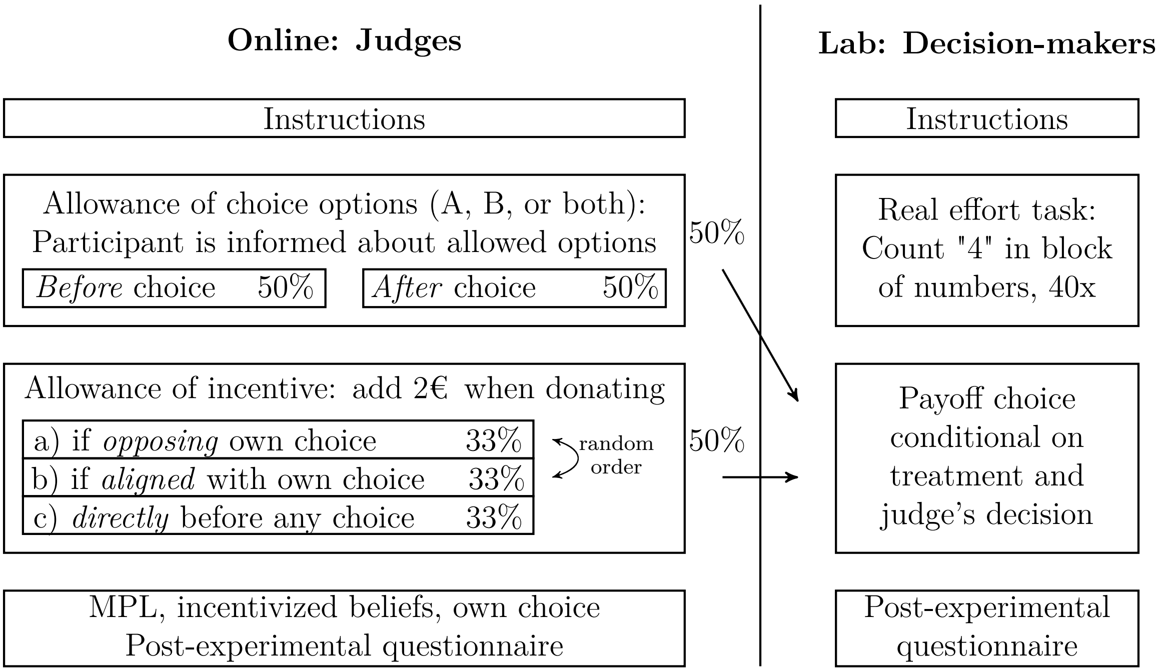
We investigate the trade-off between promoting prosocial behavior and granting others choice autonomy by looking at third-parties' willingness to use bans and incentives as two forms of interventions to influence the prosocial behavior of others. From a methodological perspective, our experimental design overlaps with studies deploying so-called spectator designs (Almas et al. Forthcoming; Cappelen et al. 2013) to investigate third-parties' willingness to intervene into others' outcomes, most often in redistribution settings. In a setting with time-delayed payments, Ambuehl et al. (2019) use a related design to study paternalistic interventions into others' choice set and find substantial intervention rates. Taking the perspective of third-parties into account is important because only investigating what works to increase prosocial behavior ignores that some tools may not be considered as acceptable (similar to repugnance in the domain of organ or blood donations (Roth 2007, 2018)). We provide evidence that many subjects are not willing to intervene at all to promote charitable giving, and that those who are willing seem to value and try to preserve the affected parties' choice autonomy. Moreover, we contribute new insights that the perceived appropriateness of incentives substantially diminishes if they are used as a means to dissuade people from their previous, autonomous choice. This supports the concerns raised by Grant (2006, 2011) that incentives can constitute a threat to autonomy.

The rest of the paper is structured as follows. In section 1.2, we present our experimental design. Section 1.3 presents and discusses the results, while the last section concludes.

1.2 Experimental Design

Our experiment consists of an online experiment and a laboratory experiment. Participants in the online experiment, henceforth called *judges*, can set the rules for the subsequent laboratory experiment, thereby determining the choice sets and payoffs of lab participants, called *decision-makers*. In what follows, we refer to a judge as "she" and to decision-maker as "he". Figure 1.1 depicts the structure of the experiment. Screenshots

Figure 1.1: Structure of the experiment



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

of how treatment differences are implemented as well as instructions for both parts of the experiment can be found in Appendix 1.B.

Laboratory experiment

Decision-makers in the laboratory experiment first have to fulfill a real-effort task, and can choose between two payoff alternatives afterwards. 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 can be found in the instructions in Appendix 1.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 can choose between receiving a payoff of 10€ without donating money (option A), and receiving a payoff of 3€ while donating 10€ to the German charity “Menschen für

Menschen" (option B). 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 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. After the payoff choice is made, the laboratory experiment ends with a brief post-experimental questionnaire.

Online experiment

Judges in the online experiment determine the rules for the laboratory experiment. Their own payoff for participation is 4€ plus some bonus options as explained below, and 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 which are implemented with 50% probability each. The first decision concerns the choice set of decision-makers. Judges can choose between allowing both options A and B, thereby letting the decision-maker choose the outcome himself, or limiting his choice set to the egoistic option only (option A), or the donation option only (option B), respectively.

Suppose a 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. Then, if the autonomy of the decision-maker is irrelevant for the judge, this judge would use her tools to promote a donation. Moreover, even if a judge would for herself selfishly decide against a charitable donation, she might value a donation to the charity higher than the decision-maker's payoff and thus enforce charitable giving by the decision-maker. On the other hand, if judges respect the decision-makers' autonomy, they may abstain from an intervention even if they donate themselves. This leads us to the following hypotheses.

Hypothesis 1a (Selfish intervention): Judges use bans to enforce charitable giving, in particular if they would donate themselves.

Hypothesis 1b (Autonomy): Judges do not use bans to enforce charitable giving, even if they donate themselves.

We randomly assign subjects into two treatments. In the *After* treatment, the decision-maker makes a choice and learns afterwards whether this choice is allowed by the judge matched to him.[2] If it is allowed, it is implemented. Otherwise, it is overridden by the other option, the only one which the judge allowed, which is then implemented. In the *Before* 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 *After* 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 matched judge allowed. In that sense, in the *After* treatment, judges can let decision-makers first decide on their own, preserving a feeling of autonomy for the donation-willing individuals, and at the same time enforce the prosocial outcome for those who are not willing to donate. Preserving a feeling of autonomy is not possible in the *Before* treatment. This leads us to our second set of hypotheses.

Hypothesis 2a (Outcome-based judgment): Judges use bans equally often in the *Before* and *After* treatment.
Hypothesis 2b (Preserving perceived autonomy): Judges use bans less often in the *Before* than in the *After* treatment.

An alternative hypothesis is that choice is 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, we might see the opposite relationship between treatments.

Hypothesis 2c (Unburdening from choice): Judges use bans more often in the *Before* than in the *After* treatment.

The second decision that judges make concerns whether or not the decision-maker should get a 2€ extra incentive which is added to the decision-maker's private payoff

[2]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.

if he donates. If used, the decision-maker's own payoff from option B 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 initially decided not to donate, i.e., after he chose option A. The *Aligned* incentive is added after the decision-maker initially donated, thereby only raising the decision-maker's payoff of the option he already chose. In both cases, decision-makers get the opportunity to subsequently revise their choice and we preselect the initial choice on screen as the default. 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 third. It does not yield a subsequent payoff increase like the other two incentives, but an increase in the payoff right from the beginning. This means that if the judge allows the *Direct* incentive, her matched decision-maker in the lab does not even see the initial version of option B (3€ for decision-maker, 10€ donation), but directly gets his own payoff in case of donation displayed as 5€, i.e., with the 2€ extra incentive added on top.

If judges are concerned about decision-makers' autonomy, we predict (following Grant (2006, 2011)) a less frequent use of the *Opposing* compared to the *Aligned* incentive since the former actively tries to change the decision-maker's own, initial choice and thereby openly disrespects his autonomy. To the contrary, an outcome-focused judge, in particular when she would donate herself, would use the *Opposing* incentive more often than the *Aligned* one since it can attract additional donors.

Hypothesis 3a: (Attracting more donors): Judges intervene more frequently by *Opposing* than by *Aligned* incentives.

Hypothesis 3b (No seduction to change autonomous choice): Judges intervene less frequently by *Opposing* than by *Aligned* incentives.

While our autonomy hypothesis provides no clear prediction regarding how the use of bans and incentives are related, purely outcome-based reasoning of judges would predict that whatever tools judges have at hand, they should use either all or none of the tools to increase the donation level. Therefore, we should expect a positive relationship between the willingness to enforce the donation by a ban and the willingness to use the *Opposing* incentive. Contrarily, if procedural factors play a role, the relationship may be ambiguous.

Hypothesis 4a: (Outcome-based interventions): Judges, who intervene by a ban, intervene more frequently by the *Opposing* incentive.
Hypothesis 4b (Procedural factors matter): There may be no relationship between judges' intervention behavior by bans and by the *Opposing* incentive.

After the main decisions are made, we collect several beliefs, attitudes, and additional measures related to our setting. 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. Additionally, we 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 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 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 he 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 toward the decision-maker.

[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 task on her own after the online experiment if her willingness to accept is low enough. With reference to the tasks' 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.

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 experiment and 61 in the laboratory experiment. Participants in the laboratory experiment 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 experiment 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.

1.3 Results

1.3.1 Descriptive statistics

Table 1.1 shows descriptive statistics of our sample in terms of demographics and factors important for the analysis. We have 216 judges, of which 128 are female, with an average age of 25.7 years. 37.5% study a major in economics, business or in a related field[5]. While the latter share is only 29.5% in the sample of decision-makers, both judges and decision-makers are similar in terms of age and gender. With reference to behavioral measures, 70.4% of judges choose to donate themselves and 79.2% select it as the delegated choice for the decision-maker. 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, and find that 74.5% and 57.9% of judges

[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.

[5] As a business or economics 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.

behave altruistically towards the charity and the decision-maker, respectively. Descriptive statistics regarding the behavior of decision-makers can be inferred from Table 1.A.1 in the Appendix.

Table 1.1: Descriptive statistics: Sample characteristics

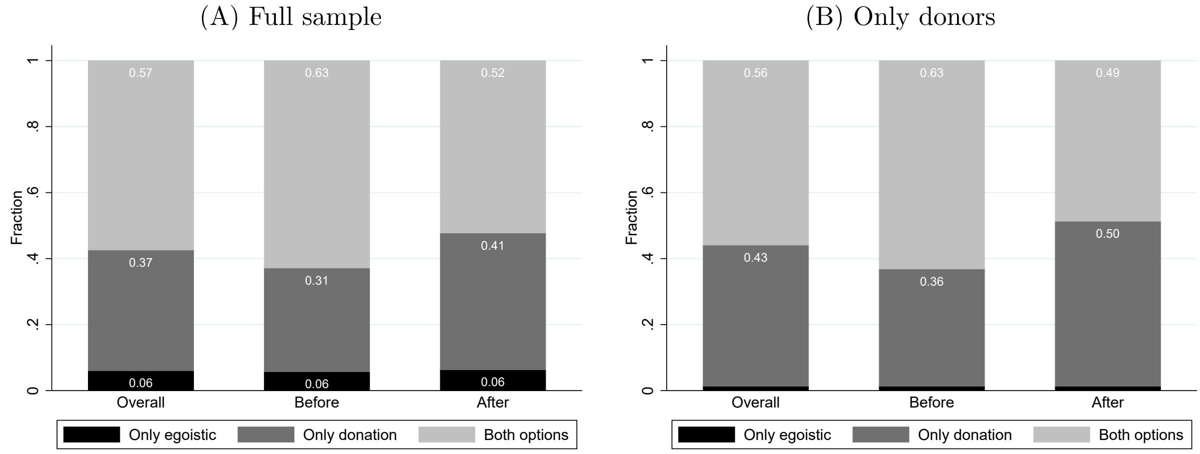
	Judges		**Decision-makers**	
	Mean	Std. dev.	Mean	Std. dev.
Female	0.593	0.492	0.590	0.496
Age	25.7	5.2	26.1	8.5
Business/econ major	0.375	0.484	0.295	0.444
Would donate herself	0.704	0.458		
Donation as delegated choice	0.792	0.407		
Altruistic towards decision-maker	0.579	0.495		
Altruistic towards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

Notes: Except age, all variables reported as fractions.

1.3.2 Use of bans

When pooling our data over treatments, as shown in the left bar of Figure 1.2, Panel A, the majority of judges leaves the choice set unrestricted, thereby granting decision-makers full 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. In X^2-goodness of fit tests, the fractions of judges not intervening, enforcing the donation, or enforcing the egoistic option, respectively, are highly statistically different from a distribution in which all judges intervene ($p < 0.001$). The distribution is also different from one in which judges randomize between the three options in case they do not care ($p < 0.001$). When restricting the sample to judges choosing the donation themselves, 56% leave the choice set unrestricted as displayed in the left bar of Figure 1.2, Panel B. Observing the majority of judges refraining from an intervention by a ban, although they would donate themselves, suggests that they take decision-makers' autonomy into account when considering to intervene. This confirms Hypothesis 1b.

Figure 1.2: Use of bans overall and by treatment



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

<u>Result 1</u>: The majority of judges does not intervene into the prosocial decision-making of others despite being willing to sacrifice their own payoff for the charitable outcome.

When also considering the point of time when the decision-maker is informed about the available choice set, either *Before* or *After* he makes his choice, the right and the middle bars of Figure 1.2, Panel A, show that the share of judges enforcing the donation is with 41% higher in the *After* treatment than with 31% in the *Before* treatment. While this difference is not statistically different in a ranksum test ($p = 0.128$), it indicates a trend towards a higher willingness of judges to intervene into 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 1.2, 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 disappears. 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 can be found in Figure 1.A.1 in the Appendix.

We investigate the treatment effect in more detail using a multinominal logit model, which allows us to predict changes in the probability which category an observation falls into based on our treatment dummy. Table 1.2 reports average marginal effects from the

Table 1.2: 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: *After*	-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: *After*	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 effect 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

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 no significant difference between *Before* and *After* treatments without controls. 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 *After* treatment are 13.6 percentage points less likely not to intervene into the choice of the decision-maker and 12.8 percentage points more likely to enforce the donation than in the *Before* 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. The treatment effect also remains unchanged when 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 *After* treatment. Table 1.A.2 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 favoring the idea that doing good autonomously is preferred.

<u>Result 2</u>: Judges use bans more often in the *After* than in the *Before* treatment to intervene into the choice of decision-makers.

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

[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.

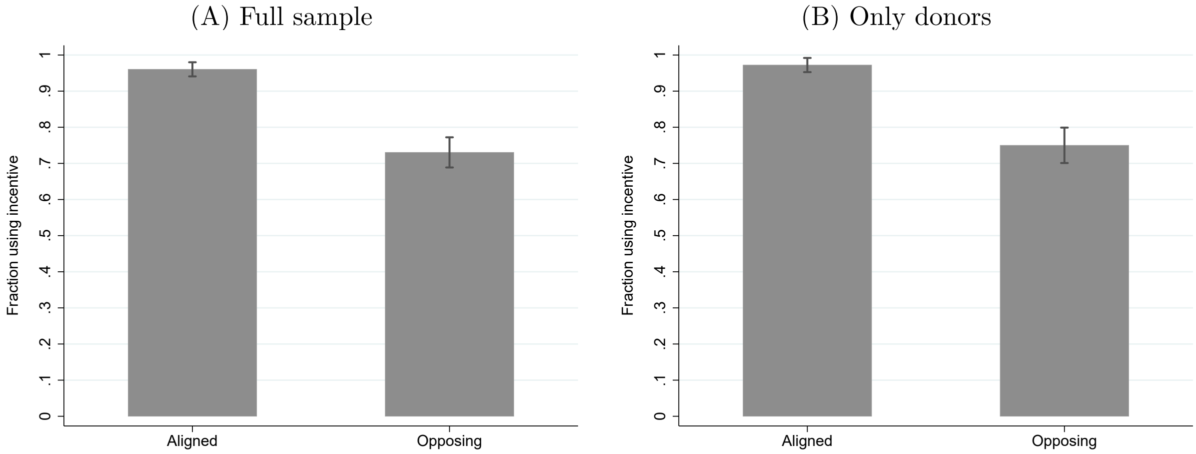
since the corresponding estimates show exact zero effects. Remarkably, judges studying a business or economics related major are 18-22 percentage points more likely to allow both choice options, which is significant at the 1% level. With a magnitude of 11-12 percentage points, the same holds for women at the 10% significance level. Furthermore, age turns out to affect the probability not to intervene positively. Figure 1.A.3 in the Appendix plots the regression coefficients and 95% confidence intervals of these results, of the altruism dummies, as well as of all other control variables used in column (4) of Table 1.2, which turn out to be significant at least at the 10% level. Additional insights, which emerge from the post-experimental survey questions, are discussed in the Appendix. 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. This provides additional support for our idea that judges who care for the decision-maker want to preserve as much of his autonomy as possible.

The additional survey evidence and the significance of the *After* treatment dummy speak against the argument that judges' restrictions of the decision-makers' choice set are mainly motivated by unburdening him from the choice between the two alternatives (Sunstein 2014, 2015). Rather, it favors the idea that judges prefer to preserve as much of decision-makers' autonomy as possible. Nonetheless, they trade off the autonomy they grant donation-willing decision-makers against an increased prosocial outcome level by enforcing the donation on donation-unwilling individuals afterwards. Therefore, while the act of doing good seems to be positively valued by judges, bans seem to be perceived as a valid means to change outcomes by many judges.

1.3.3 Use of incentives

We now focus on judges' willingness to use subsequent monetary incentives to change others' behavior into the direction of a donation. Figure 1.3 displays and Table 1.3 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 private payoff

Figure 1.3: Coefficient plot: Use of incentives



Notes: Coefficient plot from column (2) of Table 1.3. Vertical lines represent standard errors. Order controls set to value of first period.

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 1.3, is with 96.0% statistically not different from one ($p = 0.259$). 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 reveals a highly statistically different willingness to use the *Opposing* incentive compared to the *Aligned* one ($p < 0.001$).

We can exploit that we elicited the allowance of incentives in a 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* subsequent 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.[7]

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

Table 1.3: 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

We observe a strong treatment effect of the *Opposing* incentive in form of a 22-23 percentage points drop in incentive usage compared to the *Aligned* incentive across all full sample specifications in Table 1.3. 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 also robust to additionally including

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.

altruism controls from the multiple price list, various attitudes related to our setting from the post-experimental questionnaire, and contextual factors like judges' valuations 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, i.e., our controls for sufficient altruism towards the charity, results do not change.

While we cannot fully rule out that judges withhold the additional incentive to donate because of a desire to punish initially donation-unwilling decision-makers, this appears to be an unlikely motive in our context. Since the decision-maker's payoff only increases in case he donates, this punishment would only be effective if it prevented the decision-maker from being altruistic. Hence, successful punishment would result in egoistic choices, implying *higher* material payoffs for the decision-maker and *fewer* donations to charity, which would stand in contrast to the preferred outcome of most judges.

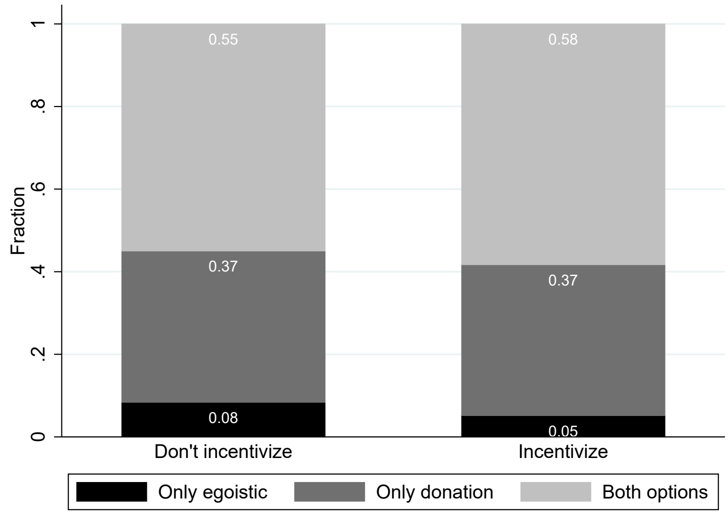
Overall, we conclude that incentives are perceived as a less appropriate means to channel behavior if they are implemented to dissuade somebody from his previous, autonomous choice. This confirms Hypothesis 3b.

<u>Result 3</u>: Subsequent incentives are less often allowed if they are implemented to dissuade the decision-maker from his previous choice in the *Opposing* treatment compared to incentives implemented *Aligned* with the decision-makers initial choice.

1.3.4 Relationship between the use of bans and incentives

How are the willingness to use bans and incentives related to each other? Purely outcome-based reasoning of judges, as stated in Hypothesis 4a, predicts that whatever tools judges have at hand, they should either always or never use these tools to increase the donation level. Therefore, we should expect a positive relationship between the willingness to enforce the donation by a ban and the willingness to use the Opposing incentive. When splitting our sample into judges allowing or forbidding the *Opposing* incentive, we do not

Figure 1.4: Use of bans by use of *Opposing* incentive



Notes: Sample split between judges using or not using the *Opposing* incentive as an interventional tool.

observe any differential pattern regarding the use of bans. The distribution of judges allowing only the donation, only the egoistic option, or both choice options looks highly similar in Figure 1.4 and is indistinguishable in Fisher's exact test ($p = 0.622$).

Moreover, the dummy for the allowance of the *Opposing* incentive does not result in any significance and possesses weak exploratory power ($Pseudo\, R^2 = 0.002$) when included as a single explanatory variable into a multinominal logit model in column (1) of Table 1.4. Additionally, it does not change previous results when added into the multinominal logit regressions from Table 1.2 as columns (2) to (4) of Table 1.4 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. For a graphical depiction of the relationship between bans and incentives by treatment, see Figure 1.A.2 in the Appendix.

We conclude that there is no statistical relationship between the willingness to use bans and incentives as a means to intervene into others' prosocial decision-making on the individual level. This supports our earlier finding that purely outcome-based reasoning cannot explain the patterns of judges' interventions and supports Hypothesis 4b.

<u>Result 4</u>: There is no statistical relationship between the use of bans and incentives.

Table 1.4: 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)	(0.111)	(0.106)
Treatment: *After*		-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: *After*		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 effect 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.

1.4 Conclusion

Many judges refrain from intervening to promote others' charitable giving, even if they would be willing to sacrifice for charity themselves. Many of those who do intervene appear to be affected by a desire to respect others' choice autonomy as much as possible. Interventions by bans are more attractive 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 choice if the incentive conflicts with the decision-makers' previous choice. That is, judges dislike dissuading somebody from his previous, autonomous choice by monetary incentives. We do not find evidence for unburdening others from having to make a choice as a contrary main intervention motive, as the idea of choice as a burden to the individual may suggest. Observing unaffected parties granting others as much autonomy as possible emphasizes that procedural factors and the resulting utility thereof matter and should be considered when deciding which policies to implement.

Remarkably, our results show an increased willingness to intervene by bans if the ban affects those who autonomously decide not to donate, but a decrease if monetary incentives are used to change an autonomous choice. There is one crucial difference between bans and incentives: While the judge implementing a ban determines the final outcome, it is still the decision-maker who has to make the final choice and change his behavior under incentives. While knowing that the donation does not correspond to the decision-maker's own choice seems not to hinder judges from intervening *per se*, knowing that the decision-maker has to actively act against his autonomous, initial choice himself may render an intervention less appealing. This provides empirical support for concerns raised by Grant (2006, 2011) that actions against "one's own wants" caused by incentives may be problematic. In summary, we infer from our results that the concept of autonomy, which both forms of intervention stand in conflict with, might have very different facets depending on how autonomy is affected. While people seem to value giving others' the opportunity to express their own preferences, but nonetheless are willing to trade off autonomy against outcomes by bans, they dislike monetary temptations which try to distract someone from his autonomous choice.

Since our investigation is in many aspects exploratory, the effects we detect and the perspective we take leave room for further investigation. Subsequent research may focus on whether being self-affected or unaffected by the rules one determines alters how people set rules. Moreover, the strong drop in the willingness to use monetary incentives to make people change their behavior deserves further attention as does the importance of procedural factors like autonomy in general when deciding for others. We see our project as a first attempt in the prosocial domain to better understand the delicate conflict between fostering a desired outcome and maintaining the autonomy of affected parties to decide for themselves, which any intervention comes along with. Given the trade-off at play, our results may also explain the appeal of nudges (Thaler and Sunstein 2008), whose conflict with reducing autonomy is likely not equally strong as it is for bans and incentives.

1.A Appendix: Additional results

Table 1.A.1: Descriptive statistics: Decision-makers

Treatment	**Before**	**After**	**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: "DM" corresponds to decision-maker. Donation chosen by decision-maker 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 *Before* treatment.

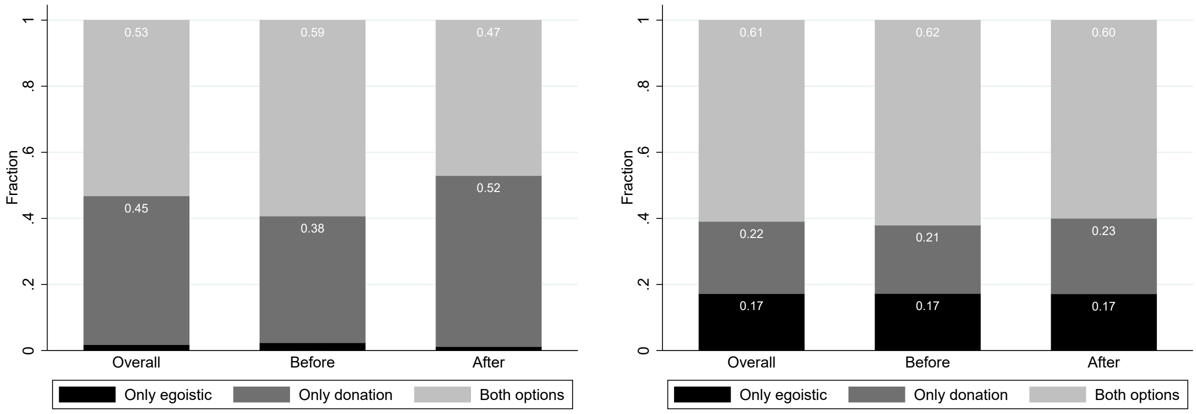
Table 1.A.2: Linear probability model of enforcing donation by treatment

	(1)	(2)	(3)	(4)	(5) Donor	(6) Delegate
Treatment: *After*	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.

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

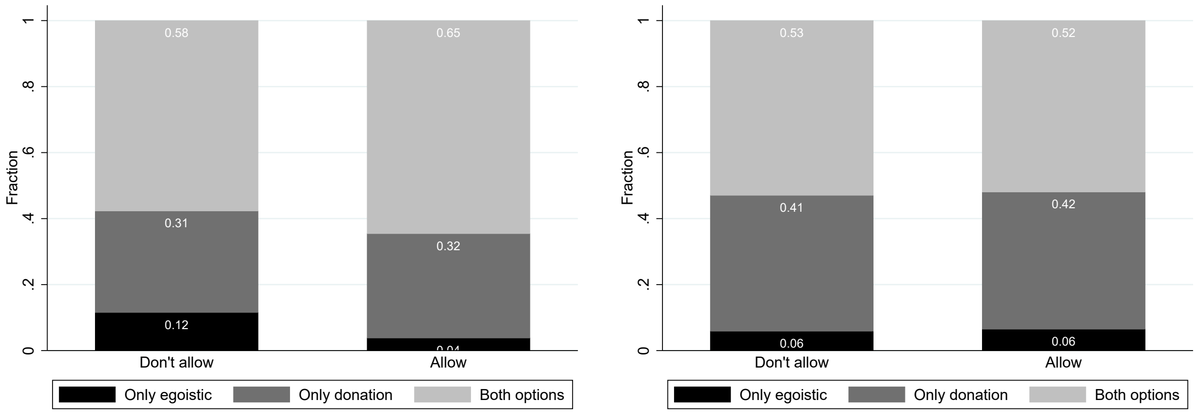
(A) Donation chosen as delegated choice　　　　(B) Only Non-donors



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

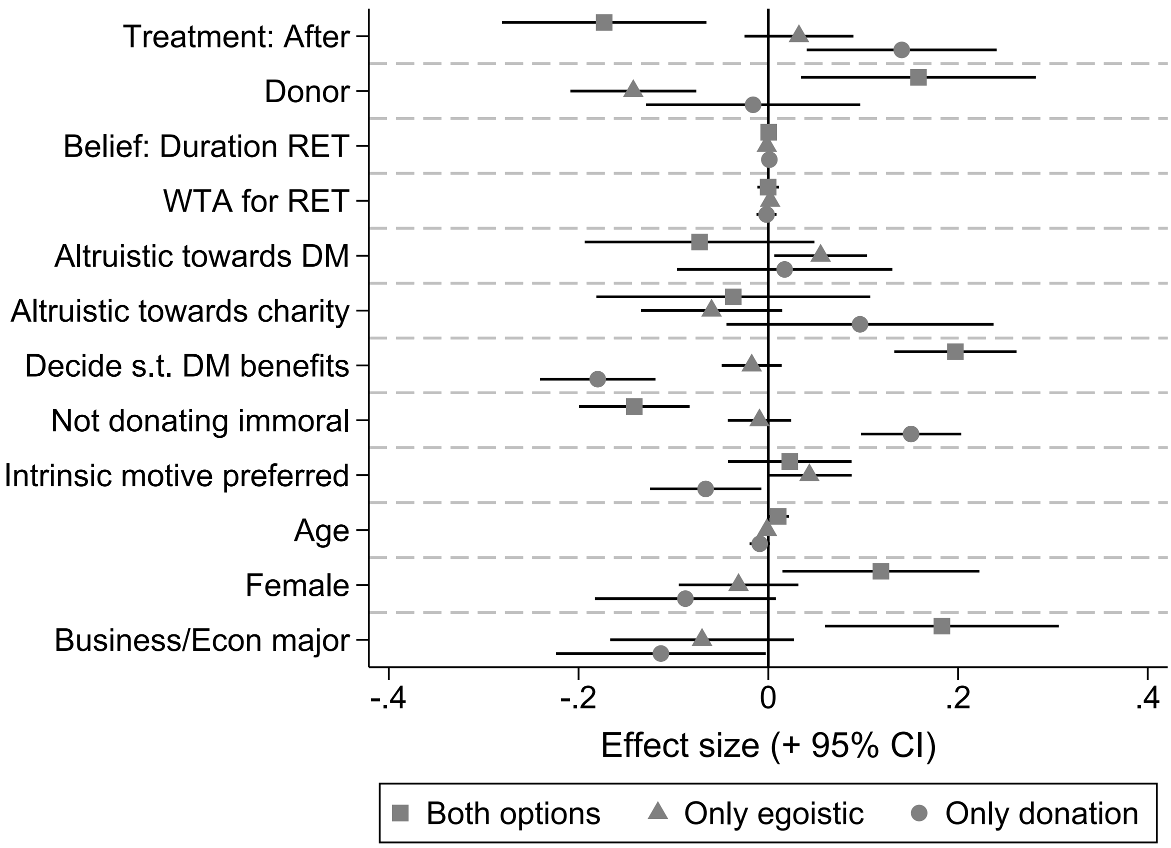
Figure 1.A.2: Relationship between bans and incentives by timing of ban implementation

(A) Before　　　　(B) After



As Figure 1.A.3 displays, apart from the factors discussed in the main text, several survey questions predict the use of bans. There is a clear pattern 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 into 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 allow only the egoistic option ($p = 0.057$) but 7 percentage points less likely to enforce the donation ($p = 0.027$), speaking in favor of the idea that judges may value if doing good is free of external influence. Moreover, for every standard deviation that subjects decide more in order 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 the interventions we observe may not be driven by a paternalistic motive. Rather, judges stating to care more about the decision-maker than about the charity try to preserve his freedom of choice, which supports our autonomy hypothesis. 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. Altruism towards the charity affects intervention behavior only insignificantly in the full model specification, but is depicted for the sake of completeness.

Figure 1.A.3: 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 1.2 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.

1.B Appendix: Experimental material

1.B.1 Screenshots of treatment differences

Figure 1.B.1: Decision screen judges: Bans *Before* treatment

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.

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)

Figure 1.B.2: Decision screen judges: Bans *After* treatment

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. 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)

Figure 1.B.3: Decision screen judges: *Opposing* incentives treatment

No. 1 of 3

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

Figure 1.B.4: Decision screen judges: *Aligned* incentives treatment

No. 2 of 3

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€

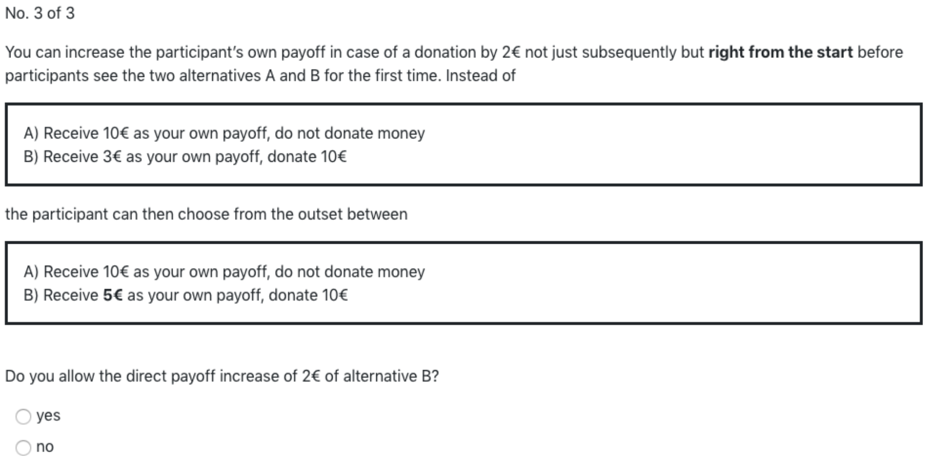
the participant can then choose between

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

Figure 1.B.5: Decision screen judges: *Direct* incentives treatment


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€

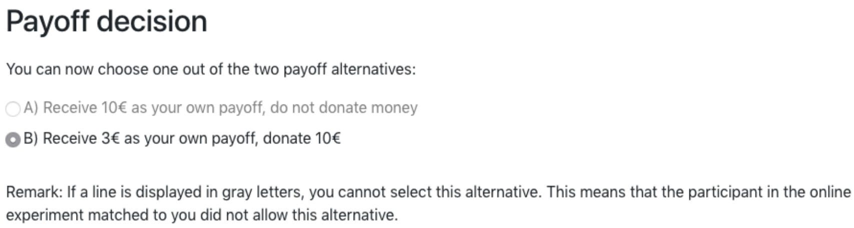
the participant can then choose from the outset between

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

Figure 1.B.6: Decision screen decision-makers: Bans *Before* treatment

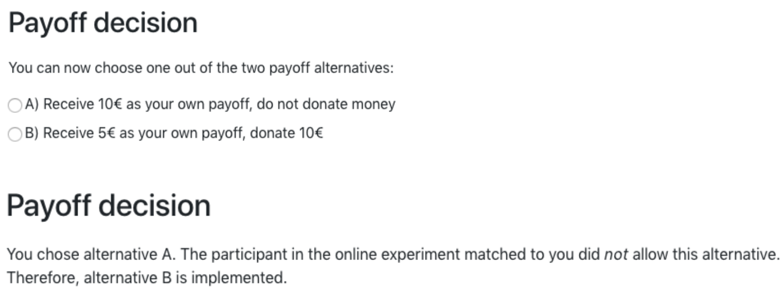

Payoff decision

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€ as your own payoff, donate 10€

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.

Figure 1.B.7: Decision screen decision-makers: Bans *After* treatment


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 5€ as your own payoff, donate 10€

Payoff decision

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

Figure 1.B.8: 2^{nd} decision screen decision-makers: *Opposing* incentives treatment

Payoff decision

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".

Figure 1.B.9: 2^{nd} decision screen decision-makers: *Aligned* incentives treatment

Payoff decision

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".

Figure 1.B.10: Decision screen decision-makers: *Direct* incentives treatment

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 5€ as your own payoff, donate 10€

1.B.2 Instructions

[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]

<u>Contact information</u>

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 before 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 after 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!

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.

Chapter 2

The Aversion to Monetary Incentives for Changing Behavior*

Abstract

In this paper, I study an aversion to monetary incentives to make other people change their behavior. Particularly, I provide evidence that monetary incentives are disliked because they are powerful in changing what people do but not why they do it besides for money. In an experiment, one group of participants decides about interventions which try to change others' behavior. Between treatments, I vary whether the intervention consists of convincing information or monetary incentives. I find that participants consider monetary incentives as more effective in changing behavior than informative interventions. Nonetheless, they are less willing to intervene by monetary incentives compared to informative interventions to foster their preferred outcome. A comprehensive set of elicited beliefs supports the idea that this aversion to incentives stems from incentives' lack of changing one's reason to act.

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2.1 Introduction

Monetary incentives are a common economic tool to steer human behavior (Angrist and Lavy 2009; Bettinger 2012; Charness and Gneezy 2009; Finkelstein et al. 2007; John et al. 2011; Just and Price 2013; Volpp et al. 2009), and in many situations in life, people consider such monetary incentives as a legitimate form of influence. However, in some morally loaded or repugnant contexts like organ or blood donation (Roth 2007), using money may raise concerns. In other fields of social science, authors articulate an aversion to using extrinsic rewards like monetary incentives to achieve an outcome (Ryan and Deci 2000; Sandel 2012; Satz 2010; Titmuss 1970). One potential reason causing this aversion is that monetary incentives may lead to a deviation of what people genuinely want to do and what they end up doing because of money.

Political scientist Grant (2006, p. 30) argues that this results from incentives circumventing the need to convince people: *"Incentives attempt to circumvent the need for persuasion [...]. When incentives are employed, there is no need to convince people that collective goals are good or to motivate them to pursue those goals by appeals to rational argument or personal convictions."* Put differently, incentives change actions while leaving the underlying reason to act[1] unaffected, and people prefer that others act on the basis of what the latter are convinced of. Thus, people do not only care about *what* others do but also about *why* they do it.[2]

In this paper, I provide evidence for an aversion to monetary incentives for changing behavior, and examine whether this aversion stems from the feature that incentives are powerful in changing actions, but do not change the underlying reason to act. I test this

[1]"Reason to act" corresponds to the underlying motivational foundation of the intervention-free decision an individual makes. Hence, it describes an action not executed based on monetary considerations. In induced value theory (Smith 1976), this part of preferences is to be neutralized. While the literature on cognitive dissonance in psychology, dating back to Festinger (1957), claims that one may change what one believes if the dissonance between actions and values becomes too strong, and Ambuehl (2017) finds economic evidence for people's attempts to persuade themselves under high incentives, my project focuses on the aversion to making other people experience such a dissonance.

[2]In a standard economic framework, there is no such distinction. I assume that external parties, who evaluate monetary incentives, have a preference for consistency of others' decisions made in the presence or absence of monetary incentives. While such a preference for consistency might be negligible, for example, in the context of payment for work, which affected parties would not engage in without being paid for, it may have profound effects in repugnant settings. Which transactions societies consider as repugnant and thus the associated importance on the overlap of incentivized and non-incentivized decisions may differ between societies and may change over time (Roth 2007).

idea by comparing interventions which are either of informative or monetary nature, and use these interventions to make people deviate from their previous choice.

While comprehensive information provision constitutes a form of convincing influence according to Martinson (1996), information may have limited power to change outcomes. In contrast, while monetary incentives are powerful in changing outcomes, they may have limited power to convince. In light of this trade-off, I hypothesize that unaffected parties are less willing to use monetary incentives to change others' behavior compared to informative interventions although they expect money to be more effective in altering outcomes. Always implementing interventions in opposition to one's own previous choice allows me to study the aversion to monetary incentives in absence of concerns to crowd out any intrinsic motivation.

In order to test the aversion to incentives, I let participants in an online experiment decide about the rules of a subsequent laboratory experiment. Participants in the lab can choose between taking money for themselves or donating it to UNICEF for vaccinations against the disease tetanus. Online participants decide whether those laboratory participants, who initially decide against the donation, should be targeted by an intervention[3] that tries to change their decision into a donation. Initially generous individuals always receive the intervention. I vary between treatments whether the intervention consists of a private monetary incentive in the *Incentives* treatment or an informative video about tetanus vaccinations in the *Information* treatment. More precisely, this means that, for example, in the *Incentives* treatment, all subjects first choose between donating or not. Those who donate always receive additional money at the end of the experiment. Those who do not donate receive additional money if their matched online participant agrees to intervene and if they switch to the donation in a subsequent choice, otherwise not. At every decision stage, I closely track online participants' beliefs regarding the donation rate in the laboratory experiment as well as their beliefs regarding how informed and convinced laboratory participants feel.

[3] In this paper, I refer to "intervention" as any attempt to change initially donation-unwilling individuals' behavior, while "treatments" refer to the different types of intervention in the different experimental conditions. The term "incentives" always corresponds to monetary incentives.

A difference in intervention behavior between treatments could be either driven by a pure information level effect, i.e., that people do not want to intervene in uninformed participants' decision-making in general, or by an aversion to intervening against somebody's conviction. In order to distinguish these explanations, I conduct an additional *Informed Incentives* treatment, in which lab participants see the informative video already before they make their initial choice, and online participants can only subsequently intervene by a monetary incentive. If unaffected parties refrain from using incentives also for more informed individuals, it must be more than a pure information effect which drives a higher willingness to intervene by the video in the *Information* treatment. In particular, those individuals, who have seen the video in the *Informed Incentives* treatment but are still unwilling to donate, are informed but not convinced to donate. Hence, an intervention by money disrespects their previous wants and may therefore be similarly or even more disliked in the *Informed Incentives* treatment than in the *Incentives* treatment at a lower information level.

In a further treatment, the *Paid Information* treatment, I distinguish the willingness to incentivize the donation outcome in the *Incentives* treatment from the willingness to incentivize information acquisition. If money is accepted as long as it leaves room to change more than just behavior, i.e., one's reason to act, incentivizing information acquisition may be a second best approach to make monetary interventions feasible.

I find that participants consider monetary incentives as the most effective tool to change behavior. In contrast, they believe that incentives are less able to convince people that the donation is the preferable option to choose. In light of this trade-off between changing behavior and changing convictions, I observe a lower willingness of participants, who favor the donation, to intervene by monetary incentives, particularly by *Informed Incentives*, compared to information. Hence, unaffected parties refrain from using the most effective tool to promote their favored outcome because of its lacking ability to change people's reason to act. This finding stands in stark contrast to the standard economic idea of outcome-maximizing behavior. Consequently, my results show that not only *what* others do matters for people but also *why* they do it.

The paper primarily contributes to three strands of economic literature.[4] First, it adds to recent studies investigating people's willingness to intervene in others' decision-making. Ambuehl et al. (2019) look at interventions in others' time-inconsistent decision-making via choice set restrictions and provide evidence for paternalism. Jacobsson et al. (2007) find that subjects prefer to give others in-kind rather than cash transfers to promote healthy behavior. In a vignette study regarding egg-cell donation, Ambuehl and Ockenfels (2017) show that unaffected parties prefer not to incentivize uninformed people to become donors. My project goes one step further by uncovering that even when being informed, incentivizing others to act against their own intervention-free decision may be depreciated. This extends the recent finding in Ackfeld and Ockenfels (2019) that third-parties do not like to give money to others to make them act in opposition to their autonomous choice.[5] Ackfeld and Ockenfels (2019) find that participants are generally willing to increase others' own, private payoff, which is paired with a charitable donation, if these other people want to choose the donation anyway. However, acceptance decreases if money is offered to those who initially decided against the donation. In the present paper, I focus on the latter case, i.e., interventions that stand in explicit conflict with one's own wants, attempting to *change* behavior. Particularly, I provide a new explanation why people may be reluctant to use monetary incentives under such conditions: Monetary incentives only change behavior but not the underlying reason to act. Since informative interventions leave room to also change the reason to act and thereby the intervention-free choice, such intervention might be preferred by unaffected parties.

Second, by studying incentives' inability to convince, I provide empirical evidence for concerns raised by political scientist Grant (2006) that incentives are used to circumvent the need to convince. Similar concerns exists in other fields of social science (Deci 1971; Deci and Ryan 1985; Deci et al. 1999; Lepper et al. 1973; Lepper and Greene 1978; Ryan and Deci 2000; Sandel 2012; Satz 2010), in which actions based on extrinsic rather

[4]Using a paradigm which involves donations for medical interventions in developing countries to study moral decision-making, the project also overlaps with research by Bartling and Özdemir (2017), Kirchler et al. (2016), and Sutter et al. (2020). By letting unaffected parties decide about others' outcomes, the project is methodologically similar to experiments using so-called spectator-designs in redistribution settings (Almas et al. Forthcoming; Cappelen et al. 2013).

[5]Unrelated to incentives but in a similar context, Kessler and Roth (2014) find that people think next of kin prefer not to donate their deceased's organs if the latter decided against being donors in an active choice frame compared to an opt-in setting in which the latter may not made a previous choice.

than intrinsic motives are considered as inferior. While there exists economic research on crowding-out of intrinsic motivation as postulated by Titmuss (1970) and reviewed by Bowles and Polania-Reyes (2012) and Frey and Jegen (2001)[6], this research differs from my approach since my interventions only target individuals who are intrinsically *un*willing to behave prosocially. Hence, by design, my interventions cannot crowd out any intrinsic motivation. Finding an aversion to monetary incentives even when crowding-out of intrinsic motivation is no concern establishes the aversion to monetary incentives as an even more fundamental principle than previously assumed.

Third, my interpretation of incentives as a means to change behavior but not the underlying reason to act may help to better understand the aversion to trade some goods based on moral grounds, so-called repugnance (Roth 2007). For example, in a vignette study by Ambuehl et al. (2015), up to about one quarter of participants disapprove very high payments for participation in a medical trial. Similarly, Elias et al. (2019) show that people are reluctant towards direct monetary payments to organ donors, while not disapproving monetary compensation in general.

Besides repugnant transactions, incentives' inability to change the reason to act may serve more generally as a new explanation why incentives may not work in certain contexts like promoting effort in prosocial fundraising (Ashraf et al. 2014; Gneezy and Rustichini 2000b) and why the effectiveness of incentives vanishes substantially once they are removed (Charness and Gneezy 2009; Kesternich et al. 2016; Meier 2007). Investigating how people judge interventions by monetary incentives may help to better understand when and for what processes incentives are a powerful tool and under which conditions they may not work as intended (Bowles and Polania-Reyes 2012; Gneezy and Rustichini 2000a; Gneezy et al. 2011; Kamenica 2012). In this attempt, I focus not only the perception of incentives per se, but also test whether a policy, that incentivizes information acquisition, can reduce the aversion to incentives. Leaving room for also changing the reason to act, the aversion to incentives may attenuate if not the outcome but the information acquisition process is incentivized. While I do not find that the aversion to incentives is reduced when incentivinzing information acquisition, results show that it at least reduces the controversy of this issue between opponents and supporters of an outcome.

[6] See Bénabou and Tirole (2003, 2006) for economic models incorporating intrinsic motivation.

The remainder of this paper is structured as follows. Section 2.2 describes the experimental design and states corresponding hypotheses. Section 2.3 presents the experimental results before Section 2.4 concludes.

2.2 Experimental Design

General setup

The experimental design builds on Ackfeld and Ockenfels (2019) and consists of two parts, namely a laboratory and an online experiment. Instructions for both parts can be found in Appendix 2.B.2, and the structure of the laboratory experiment is displayed in Figure 2.1. Participants in the online experiment, so-called *judges*, determine the rules of the subsequent laboratory experiment. This means that they can influence the outcomes and payoffs of lab participants, henceforth called *decision-makers*.[7] Decision-makers choose between receiving a payoff of 5€ without donating money and a payoff of 1€ including a 6€ donation to UNICEF.[8] Each donation finances tetanus vaccinations for 20 people in developing countries. All decision-makers read a brief text introducing them to the symptoms and the transmission channel of tetanus in the instructions.

The tasks of judges in the online experiment is to decide about the implementation of a subsequent intervention for those decision-makers who initially decide against the donation. This means that decision-makers choose one of the two payoff options first. If they decide not to donate, they may be subsequently treated by an intervention to change their decision into a donation.[9] A judge randomly matched to them decides about implementing this intervention or not. If a decision-maker donates already in the first period, the intervention is always implemented at the end of the experiment. The allowance of the subsequent interventions for those decision-makers, who do not donate at the beginning, is my main outcome of interest in the experiment. Hence, the focus lies on the decision of judges.[10]

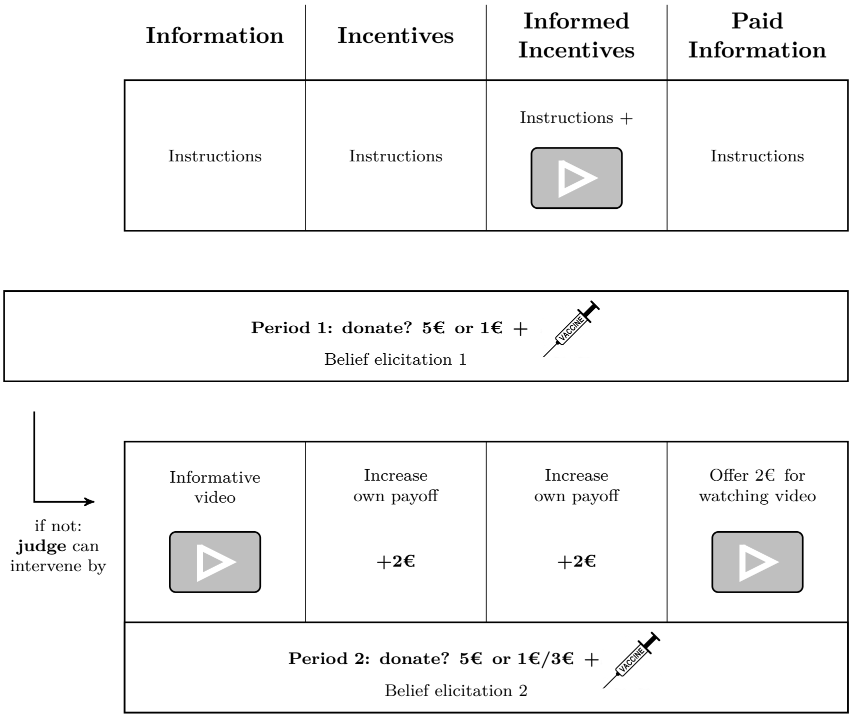
[7]For the sake of comprehensibility, I refer to a judge as "she" and to a decision-maker as "he" in what follows.

[8]Donation receipts are uploaded on the research homepage after the experiment, and I inform both judges and decision-makers about the upload in the experimental instructions.

[9]Whenever a subsequent intervention is implemented, the decision-maker's previous choice is preselected as the default on the second choice screen.

[10]Except from descriptive statistics, I only report results regarding judges' behavior in this paper.

Figure 2.1: Structure of the laboratory experiment



Notes: Treatment differences separated by vertical lines. Judges decide between-subjects about executing the intervention after decision-maker decided against the donation. Decision-makers, who donate in the first period, always receive the intervention at the end of the experiment. Icons symbolize the informative video and the donation for vaccination (© Mykola Lytvynenko, Dreamstime.com), respectively.

Using a charitable donation paradigm allows me to study monetary incentives, which are implemented in conflict with subjects' initial wants and try to change their decision, while maintaining feasibility of the experiment from a research ethics perspective. Finding an aversion to monetary incentives for changing behavior in such an ethically rather modest paradigm speaks in favor of finding the same or an even stronger effect in the field. A paradigm instead focusing on consumption goods (Ambuehl 2017; Ambuehl et al. 2019) would rather evoke paternalistic choices than a trade-off between the ability to achieve a desired outcome and the inability to convince. Hence, it would not suit the context I am studying. Moreover, in a donation paradigm, in which people may value and want to foster the charitable outcome, the circumstances speak against detecting an aversion to monetary incentives. This is because people, who value the donation, should use whatever tool they have at hand to promote the donation. Hence, finding an aversion to money in a setting, in which people care for the charity, strengthens my findings.

Treatments

Judges decide about interventions in a between-subject design, i.e., each judge decides about only one particular intervention. Section 2.B.1 in the Appendix displays how those interventions are implemented in the different treatments. In the *Information* treatment, the intervention consists of a video which informs participants about the process and advantages of tetanus vaccination.[11] In the *Incentives* treatment, the intervention increases the private payoff of the decision-maker if he donates by 2€. This means that he receives a payoff of 3€ instead of only 1€ for himself when choosing the 6€-donation. Importantly, judges in all treatments watch the video themselves. Hence, information asymmetries cannot drive judges' intervention behavior.

If the aversion to monetary incentives for behavioral change stems from their lack of changing someone's reason to act, and information leave room for changing more than actions, interventions by *Information* should be more frequently allowed by judges than *Incentives*. In order to distinguish the effect of acting better informed (Ambuehl and Ockenfels 2017; Ambuehl et al. 2018) from acting against one's own conviction when being incentivized, I add an additional treatment. In this *Informed Incentives* treatment, decision-makers already watch the informative video before making their first choice and can afterwards be targeted by the 2€ incentive if they do not donate. Individuals not donating in this treatment are informed about the donation – as informed as they can be in the *Information* treatment – but they are not convinced to donate. Thus, an intervention by incentives still disrespects their own choice. If the aversion to incentives stems from their lack of changing someone's reason to act, I expect judges to allow not only *Incentives* but also *Informed Incentives* less frequently than *Information*.

[11] The video is published by UNICEF, 2:10 minutes long, and available via YouTube under https://www.youtube.com/watch?v=zDcFHHwaQ1E in German. An English translation of the text can be found in Appendix 2.B.3. Whenever a participant watches the video, the participant has to answer two comprehension questions correctly before being able to proceed with the experiment. This guarantees that participants pay attention to the video.

Hypothesis 1: Judges allow subsequent interventions by *Information* more often than interventions by *Incentives* and *Informed Incentives*.

In addition, a natural extension is to distinguish whether people generally refuse monetary interventions, or whether acceptance rises if money leaves room to change not only someone's action but also the reason to act. In order to shed light on this question, I give judges in a fourth treatment the opportunity to offer decision-makers a 2€ extra payoff for watching the informative video about tetanus vaccinations. Again, judges decide about subsequently implementing this intervention for initially donation-unwilling decision-makers. This *Paid Information* treatment allows to investigate how money can still be beneficially used to increase a prosocial outcome level if directly incentivizing the outcome raises opposition. On the one hand, if money for behavioral change is generally disliked, one should observe a similarly low willingness to intervene by *Paid Information* as by *Incentives* and *Informed Incentives*. On the other hand, in the *Paid Information* treatment, money is only used to incentivize the information acquisition process and thereby leaves room for the decision-maker to convince himself of the benefits of donation on the way. Both factors may apply in light of an aversion to incentives for behavioral change. Since in Ackfeld and Ockenfels (2019) we find that procedural concerns play an important role when deciding to intervene, I state my hypothesis in line with the latter proposition, while the former constitutes a reasonable alternative hypothesis.

Hypothesis 2: Judges allow subsequent interventions by *Paid Information* more often than interventions by *Incentives* and *Informed Incentives*.

Decision-makers, who select the donation already as their first choice, always receive the intervention at the end of the experiment. Judges are informed about this. Hence, in fact, judges decide about *not* implementing the intervention for initially donation-unwilling decision-makers rather than implementing it at all. By always implementing the intervention for initially generous decision-makers, I can rule out that a lower willingness to intervene by money than by information is driven by judges not wanting initially egoistic decision-makers in the end to be better off than initially generous ones. As a consequence, refusing to intervene by monetary incentives in order to avoid that initially egoistic decision-makers are better off than initially generous decision-makers cannot drive my results.

Table 2.1: Beliefs elicited regarding donation rate, information level, and feeling convinced

Fraction donating	before intervention		after intervention	
Information level: tetanus vaccinations	before intervention		after intervention	
	for donors	for non-donors	for new donors	for non-donors
Convinced to choose donation	before intervention		after intervention	
	for donors	for non-donors	for new donors	for non-donors

Notes: All beliefs elicited for judges on treatment level between-subjects. Judges in the *Paid Information* treatment additionally guess the share of decision-makers agreeing to watch the video. Beliefs "after intervention" (rightmost columns) are restricted to decision-makers who initially do not donate, and are elicited with pre-intervention beliefs regarding the fraction donating and regarding non-donors informedness and conviction displayed on screen.

At every stage decision-makers choose between donating or not, I closely track judges' beliefs about decision-makers' behavior and attitudes.[12] First, judges state their belief regarding the share of decision-makers who choose the donation. Second, conditional on decision-makers donating or not, I separately elicit judges' beliefs about how informed the corresponding decision-makers feel about tetanus vaccinations and how convinced decision-makers are that the donation is the payoff alternative one should choose in this experiment. Decision-makers report these measures on a scale from 0 for feeling not at all informed (convinced) to 100 for feeling fully informed (convinced). Judges report their corresponding beliefs on the same scale for all situations which the decision-maker can encounter. The full list of beliefs elicited is summarized in Table 2.1. In the *Paid Information* treatment, I additionally ask for judges' belief regarding the number of participants watching the video. For each belief elicited, I pay the five best-guessing judges a bonus

[12]When eliciting beliefs with respect to post-intervention outcomes, I display the judge's own pre-intervention beliefs as reference points on screen, i.e, which fraction of decision-makers she thinks donates as well as her beliefs regarding how informed and how convinced initially donation-unwilling decision-makers feel.

of 2€ to guarantee incentive compatible, truthful reporting.[13] This generates the main bonus opportunity for judges in the online experiment.

In line with the reasoning above that incentives can change behavior but not the reason to act, I hypothesize that judges expect *Incentives* and *Informed incentives* to be more effective, i.e., to attract more additional donors, than *Information* and *Paid Information.* Nonetheless, they expect *Incentives* and *Informed incentives* to be less able than *Information* and *Paid Information* to also affect the reason to act.

Hypothesis 3a: Beliefs about the effectiveness to change the behavior of non-donors are higher for *Incentives* and *Informed Incentives* than for *Information* and *Paid Information.*

Hypothesis 3b: Beliefs about the ability to convince non-donors are lower for *Incentives* and *Informed Incentives* than for *Information* and *Paid Information.*

Additional measures

I collect several additional measures related to the experiment after the main decisions are made. First, judges state their valuation for allocating money to another decision-maker or the charity, respectively, by adding or subtracting 1€ from their payoff. Thus, I can control for judges' general attitude towards giving money to the decision-maker or the charity, respectively. Moreover, judges decide about showing another decision-maker another video about tetanus vaccinations.[14] Hereby, I can control for judges' general attitude towards confronting decision-makers with visuals about tetanus vaccinations. Except for the decision to add or subtract money to or from the charity's payoff, judges make all other decisions conditional on the decision-maker's choice in the experiment, i.e., whether the decision-maker donates right from the beginning, after the intervention, or never. All decisions are implemented as binary yes/no decisions, and one of them is randomly chosen and implemented in the lab experiment in 10% of the cases independently

[13]Ties are broken randomly. In order to rule out endowment effects and since beliefs can only be validated after the laboratory experiment took place, judges learn about their bonus from belief elicitation only when they pick up their payoff the week after the laboratory experiment.

[14]As the first one, the second video is also published by UNICEF and is available via YouTube under https://www.youtube.com/watch?v=zZJTnZvman0 in German. An English translation of the text can be found in Appendix 2.B.3. Only the sequence from 0:05-0:41 and the sequence from 1:52-2:07 of the original video are used to avoid potentially unpleasant scenes. Judges can but do not need to watch the second video themselves.

from any previous choice. Based on these decisions, I can not only control for judges' general willingness to intervene by money and an informative video, respectively, but can also do so conditionally on decision-makers acting generously in the experiment or not. Thus, when included in regression analyses, I can rule out that feelings like, for example, spite towards an initially egoistic decision-maker make judges refrain from intervening by money.

Second, judges choose between the two payoff alternatives themselves, and I randomly pick five judges for whom this decision is implemented. Judges state their own information level and their conviction that the donation is the payoff alternative one should choose. Letting judges make a donation decision for themselves allows to distinguish which judges indeed prefer and hence likely want to foster the donation. Excluding judges, who do not choose the donation and may not value the donation sufficiently, reduces noise in intervention behavior.[15] Judges' own choice between the two payoff alternatives as well as an allocation decision to divide 6€ between oneself and another decision-maker in the lab, which controls for social preferences and which is also implemented for five randomly selected judges, generate bonus options for judges beyond bonuses for beliefs. As a second measure for whether judges care about the charity, judges make a decision between the two payoff alternatives in case the decision-maker wants to delegate it. I give 10% of decision-makers in the lab experiment the opportunity to delegate their choice between the two payoff alternatives to the judge and implement the corresponding choice.[16] Demographics and several attitudes related to the setting are elicited in a post-experimental questionnaire.

[15] Note that this might exclude judges who generally value the charitable donation, but not sufficiently to sacrifice their own money for it.

[16] As a further indicator in the *Incentives* and the *Information* treatment, I give judges in 10% of the cases the opportunity to switch to the other treatment. Particularly, I ask judges in the *Incentives* treatment whether they want to replace their initial intervention decision (intervene by the 2€ incentive or not) by showing the decision-maker the video about tetanus vaccinations and vice versa. Note that switching to an intervention by showing the video makes no sense in the *Informed Incentives* treatment since decision-makers already watched it. Since the *Paid Information* treatment does not incentivize an outcome but information acquisition, switching may be possible but complicates the setting disproportionately.

Procedural details

Both judges and decision-makers stem from the same subject pool of the Cologne Laboratory for Economic Research (CLER) at the University of Cologne and are randomly invited to one of the two parts. Since the experiment focuses on the behavior of judges, I sample more judges than decision-makers. I randomly draw some of these judges and implement their decisions in the laboratory experiment, which takes place the week after the online experiment. Judges receive a fixed 5€ compensation for participation plus additional bonuses from the incentivized belief elicitation tasks, their own choice between the two payoff alternatives, and the independent distribution task to measure social preferences. They collect their payoff in cash the week after the laboratory experiment, i.e., two weeks after the online experiment. In addition to the variable payoff from the decision to donate or not, decision-makers in the laboratory experiment receive a show-up fee of 4€.

Data collection for both parts of the experiment took place in May 2019. The experiment was programmed in oTree (Chen et al. 2016) with participants recruited via ORSEE (Greiner 2015). 380 and 88 subjects participated in the online and laboratory experiment, respectively. The latter earned on average 8.08€ in 25-minute sessions including a 4€ show-up fee. Online sessions took place within one week, and participants received 5€ lump-sum plus an average bonus of 0.36€, which were paid out in cash two weeks after the experiment.[17] For collecting their payment, online participants received an anonymous participant code at the end of the experiment.

2.3 Results

2.3.1 Descriptive statistics

Table 2.2 reports descriptive statistics of both the online and the laboratory sample. Judges and decision-makers are on average 25.6 and 25.3 years old and in 60.3% and 52.3% of the cases female, respectively. Judges chose the donation as their own choice in 60.0% of the cases and implemented it in 75.5% of cases as the delegated choice for

[17]The pick-up rate was 58%.

the decision-maker. 34.1% of decision-makers choose the donation on their own.[18] This fraction increases to 44.3% after the intervention stage. Pooling all treatments, 83.9% of judges decide to intervene. 70.3% of judges consider the video as informative and 71.7% as convincing.

Table 2.2: Descriptive statistics: Sample characteristics

	Judges		**Decision-makers**	
	Mean	Std. dev.	Mean	Std. dev.
Female	0.603	0.495	0.523	0.525
Age	25.6	6.9	25.3	8.1
Intervention implemented	0.839	0.368		
Donor	0.600	0.491		
Donation chosen for DM as delegated choice	0.755	0.430		
Social preference / Amount kept out of 6€	4.537	1.453		
Video considered as informative	0.703	0.250		
Video considered as convincing	0.717	0.251		
Donor before intervention			0.341	0.477
Donor after intervention			0.443	0.500
N	380		88	

Notes: Except age and social preference, all variables reported as fractions. "Donor after intervention" includes both donors donating after the intervention and right from the beginning.

2.3.2 Beliefs

Pre-intervention beliefs

In this section, I first of all verify that the video in the experiment affects judges' beliefs regarding donation rates and how informed and convinced decision-makers feel, respectively. I do so by comparing judges' beliefs regarding these outcomes in the pre-intervention period in the *Informed Incentives* treatment with all other treatments, which do not exhibit a video stage before the first donation. Table 2.3 reports the corresponding OLS regres-

[18]The difference between judges' and decision-makers' donation share may stem from the fact that the payoff choice is decision-makers' main decision in the experiment and is implemented in 100% of the cases. For judges, the main decision is to set the rules for the subsequent laboratory experiment, which is emphasized in the instructions. On top of that, the donation is only implemented for five randomly drawn judges and constitutes one bonus opportunity amongst others.

sion results with different beliefs as the dependent variable and whether the video was shown before the first donation decision as the independent variable.

As column (1) shows, judges' beliefs about the donation rate are with 7.9 percentage points significantly higher if decision-makers see the video before they make their initial donation decision in the *Informed Incentives* treatment. This is a large effect in comparison to the baseline belief of 39.8%. The increase can be attributed to the informative nature of the video. Judges state significantly higher beliefs regarding how informed decision-makers feel about tetanus vaccinations if the latter saw the video before. This applies both to decision-makers who decide for the donation (column 2) and for those who decide against it (column 3).

Table 2.3: OLS regression results regarding judges' pre-intervention beliefs

	Donations	Feeling informed		Feeling convinced	
		Donors	Non-Donors	Donors	Non-Donors
	(1)	(2)	(3)	(4)	(5)
Video before 1st decision	7.933***	7.848***	4.390*	0.736	2.385
	(2.902)	(2.179)	(2.581)	(2.022)	(2.772)
Constant	39.807***	70.532***	45.350***	83.864***	45.125***
	(1.539)	(1.214)	(1.392)	(1.129)	(1.402)
N	380	380	380	380	380
R2	0.02	0.03	0.01	0.00	0.00

Notes: Robust standard errors in parentheses. "Video before 1st decision" corresponds to first stage of *Informed Incentives* treatment. All other treatments serve as baseline. "Donors" corresponds to decision-makers choosing the donation, "Non-Donors" to decision-makers choosing the egoistic option. * p<0.10, ** p<0.05, *** p<0.01.

In contrast, there is no difference in judges' beliefs in terms of how convinced decision-makers feel if decision-makers see or do not see the video. Judges belief that those who donate feel similarly convinced that the donation is the option one should choose in the experiment in all treatments (column 4); those who do not donate also feel similarly convinced (column 5). In other words, independent of the treatment, judges expect decision-makers to donate if they are convinced to do so, and do not expect them to donate if they are not convinced. This is important for the subsequent analysis since interventions in the *Informed Incentives* treatment target decision-makers believed to be similarly unconvinced as in all other treatments. Hence, decision-makers only differ in the information level about tetanus vaccinations from judges' perspective.

When adding controls for judges' age and gender, social preferences, the option chosen as delegated choice, adding or subtracting money in the independent distribution task or showing another video, respectively, and several controls from the post-experimental questionnaire in Table 2.A.1, or restricting the sample only to judges who donate themselves in Table 2.A.2 in the Appendix, results do not change.

Post-intervention beliefs

If there is a trade-off between incentives' ability to change an action and to change the reason to act, the corresponding beliefs of judges about post-intervention behavior and attitudes are expected to differ by treatment. Figure 2.2 plots these beliefs and their corresponding 95% confidence intervals. Panel (A) focuses on judges' beliefs about which share of initial non-donors alters the initial decision and decides to donate after the intervention. Panel (A) reveals that judges expect with roughly 53% a higher increase in donations in the *Incentives* and *Informed Incentives* treatments than in the *Information* and *Paid Information* treatments with only 40% to 43% (all individual differences between the former and the latter treatments: $p < 0.008$, ranksum test[19]). Put differently, judges expect monetary incentives to be more effective in changing behavior than information or the incentivized acquisition thereof. This provides support for Hypothesis 3a.

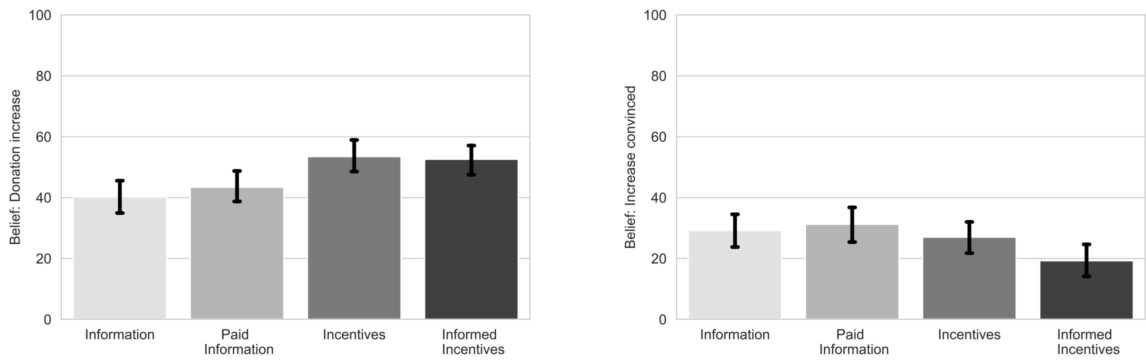
Result 1: Judges consider *Incentives* and *Informed Incentives* as more effective in increasing donation outcomes than *Information* and *Paid Information.*

The opposite pictures prevails when looking at judges' beliefs regarding the increase in how convinced decision-makers, who switch from not-donating to donating, feel that one should donate. Panel (B) of Figure 2.2 shows the highest increase in feeling convinced compared to the individual conviction level after the first donation if the intervention is

[19] All p-values from ranksum tests in this paper refer to two-sided ranksum tests.

Figure 2.2: Bar plots of judges' beliefs regarding increases in donations and in feeling convinced by treatment

(A) Increase in donations (B) Increase in feeling convinced



Notes: Increases in the number of initial non-donors, who donate after the intervention, and how convinced these new donors feel after their post-intervention donation compared to their individual pre-intervention, non-donor level of conviction. Vertical lines represent 95% confidence intervals.

by *Information* or *Paid Information*.[20] While there is a drop in this value in the *Incentives* compared to the *Information* treatment, it is not statistically significant ($p = 0.357$, ranksum test). However, when looking at the *Informed Incentives* treatment, in which it it is clear that decision-makers are similarly informed about tetanus vaccinations as they can be in the *Information* and *Paid Information* treatments, but nonetheless are not convinced to donate, the drop in judges' belief regarding feeling convinced is significant ($p < 0.001$, *Information* versus *Informed Incentives*, ranksum test).[21] Moreover, besides looking at individual differences in feeling convinced to donate before and after the intervention, I examine the absolute level of feeling convinced of initial non-donors, who switch to the donation after the intervention. As depicted in Figure 2.A.1 in the Appendix, the absolute levels of feeling convinced to donate are significantly lower in both the *Incentives* and the *Informed Incentives* treatments than in *Information* and *Paid Information* (all $p < 0.001$, ranksum test). Overall, these results provide evidence for the lacking ability of incentives to convince people. Table 2.4 substantiates findings by OLS regression results with and without control variables for age and gender, social

[20] See Figure 2.A.1 in the Appendix for the corresponding bar graph of judges' beliefs about how informed initial non-donors feel when switching to the donation.

[21] The drop in judges' belief regarding conviction is also marginally significant in the *Informed Incentives* relative to the *Incentives* treatment ($p = 0.051$, ranksum test).

Table 2.4: OLS regression results regarding judges' post-intervention beliefs

	Increase donations		Increase informed		Increase convinced	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentives	13.226***	15.957***	-14.037***	-14.232***	-2.215	-3.780
	(3.767)	(3.709)	(3.040)	(3.349)	(3.949)	(4.182)
Informed Incentives	12.352***	13.143***	-15.526***	-15.805***	-9.956**	-10.141**
	(3.679)	(3.812)	(3.120)	(3.504)	(3.906)	(4.153)
Paid Info	3.167	6.863*	0.919	1.832	2.041	1.031
	(3.784)	(3.951)	(3.561)	(3.711)	(4.016)	(4.385)
Constant	40.258***	8.464	28.326***	43.782*	29.236***	58.270**
	(2.764)	(20.373)	(2.273)	(23.749)	(2.826)	(27.239)
Controls	No	Yes	No	Yes	No	Yes
N	380	380	380	380	380	380
R2	0.05	0.21	0.11	0.18	0.03	0.13

Notes: Table reports judges' beliefs regarding increases in the number of initial non-donors, who donate after the intervention, and how convinced (informed) these new donors feel after their post-intervention donation compared to their individual pre-intervention, non-donor level of conviction (information). Robust standard errors in parentheses. *Information* treatment used as baseline category. Controls include age, gender, social preferences, survey controls, whether the donation is chosen as delegated choice and by the judge herself, and dummies for showing the video and giving/taking to/from the charity/decision-maker. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

preferences, the option chosen as delegated choice, adding or subtracting money in the independent distribution task or showing another video, respectively, and several controls from the post-experimental questionnaire. Columns 3 and 4 of Table 2.4 additionally confirm the informativeness of the video. Results remain unchanged when only considering beliefs of those judges who donate themselves in Table 2.A.2 in the Appendix. Hence, for *Informed Incentives* and directionally also for *Incentives*, I find support for Hypothesis 3b.

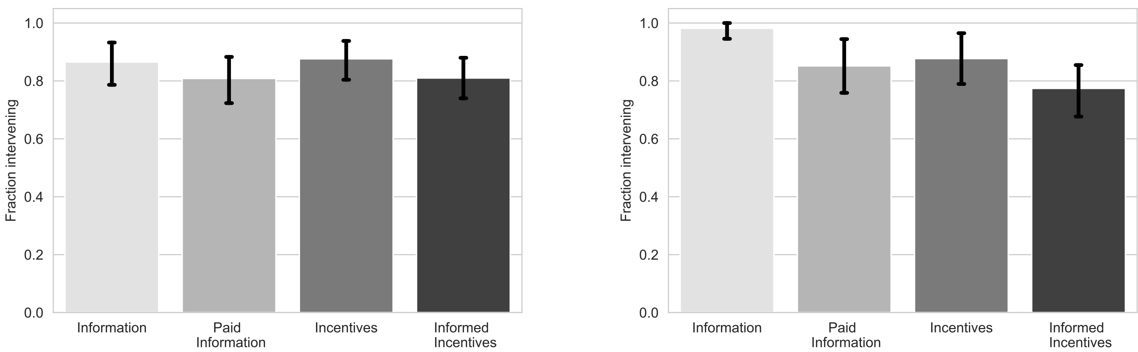
Result 2: Judges consider *Informed Incentives* as less able to convince non-donors than *Information* and *Paid Information*.

2.3.3 Intervention behavior

How does the perceived trade-off between incentives' ability to change behavior but their relative inability to change the reason to act transfer into judges' intervention behavior? Figure 2.3 plots the frequencies by which judges intervene for each treatment.

Figure 2.3: Bar plots of intervention behavior by treatment

(A) Full sample (B) Only donors



Notes: Vertical lines represent 95% confidence intervals. "Donors" corresponds to restricted sample of judges who would donate themselves.

In the full sample in Panel (A), there is no statistically significant difference between any of the treatments (all individual treatment differences $p \geq 0.199$ in ranksum tests).[22] However, analyzing the full sample of judges ignores that some judges may not even value the donation to charity and see no objective in enhancing it. In order to rule out such a confound, Panel (B) studies only those judges, who choose the donation themselves. In this sample, intervention behavior differs by treatment. Except one judge, all out of 55 judges allow the subsequent intervention in decision-makers behavior by *Information*, which is not statistically different from 100% in a t-test ($p = 0.322$). In contrast, intervention rates are significantly lower in all other treatments. Compared to the *Information* treatment, participants are 10.5 percentage points less likely to intervene by *Incentives*, which is a statistically significant decrease ($p = 0.033$, ranksum test). This decrease even doubles if looking at the *Informed Incentives* treatment, in which judges know that they target behavioral change of informed but still not convinced decision-makers. Under such

[22]Intervention rates are generally rather high and in particular higher than in Ackfeld and Ockenfels (2019). This may be due to the fact that the donation in this experiment is financed by windfall money and does not require a previous real-effort task as in Ackfeld and Ockenfels (2019).

conditions, money is disliked the most. Note that finding a more pronounced drop in the willingness to intervene by *Informed Incentives* compared to *Incentives* ($p = 0.144$, ranksum test) reassures that not a pure aversion to give extra money to initially egoistic types drives the aversion to use monetary incentives. In that case, intervention rates in the *Incentives* and *Informed Incentives* treatment should be equal. Rather, setting monetary incentives to evoke behavior in clear opposition to what one is convinced to do appears to be the driving force behind the aversion to monetary incentives. Additional results reported in Figure 2.A.2 in the Appendix and the discussion thereof further support this idea by showing that judges more likely intervene the larger they perceive the convincing effect of the intervention on decision-makers to be.

When looking at judges' willingness to intervene in the *Paid Information* treatment, which incentivizes the information acquisition process, I find a similar intervention rate as that in the *Incentives* treatment ($p = 0.701$, ranksum test), i.e., a significantly lower one than in the *Information* treatment ($p = 0.014$, ranksum test). This means that incentivizing the information acquisition process instead of the donation outcome and thereby leaving room to become convinced of the donation does not eliminate the aversion to using money.

Regression results from a linear probability model in columns 1 and 3 of Table 2.5 confirm the effects just discussed for the full sample and for the restricted sample of donors, respectively. All effects remain unchanged if various control variables are included in the regressions in columns 2 and 4: demographic control variables for age and gender, social preferences, the option chosen as delegated choice, adding or subtracting money in the independent distribution task or showing another video, respectively, and several controls from the post-experimental questionnaire. In all specifications, which exclude noise in form of judges not donating and thus not sufficiently valuing the prosocial outcome, I find an aversion to monetary incentives. Using probit and logit regression models in Table 2.A.3 in the Appendix confirms these results and reveals even larger marginal effects regarding donors' aversion to monetary incentives for changing others' behavior. In summary, I find evidence for Hypothesis 1, claiming that interventions by *Information* are more often allowed than interventions by *Incentives* and *Informed Incentives*. However, I

Table 2.5: Linear probability model of likelihood to intervene by treatment

	Full sample		Donors		Non-Donors		Full sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentives	0.011	0.021	-0.105**	-0.117*	0.199**	0.253*	0.199**	0.222**
	(0.050)	(0.059)	(0.048)	(0.063)	(0.099)	(0.130)	(0.099)	(0.110)
Informed Incentives	-0.055	-0.025	-0.208***	-0.197***	0.192*	0.342***	0.192*	0.276**
	(0.054)	(0.060)	(0.057)	(0.062)	(0.100)	(0.125)	(0.100)	(0.109)
Paid Information	-0.057	-0.037	-0.130**	-0.123*	0.074	0.087	0.074	0.090
	(0.055)	(0.060)	(0.053)	(0.063)	(0.108)	(0.128)	(0.108)	(0.112)
Donor		0.071					0.305***	0.304***
		(0.061)					(0.085)	(0.099)
Incentives							-0.303***	-0.324***
# Donor							(0.110)	(0.122)
Informed Incentives							-0.400***	-0.471***
# Donor							(0.115)	(0.123)
Paid Information							-0.203*	-0.194
# Donor							(0.121)	(0.127)
Intercept	0.865***	0.901***	0.982***	0.675	0.676***	1.172*	0.676***	0.741**
	(0.037)	(0.335)	(0.018)	(0.427)	(0.083)	(0.631)	(0.083)	(0.327)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	380	380	228	228	152	152	380	380
R2	0.01	0.13	0.05	0.25	0.04	0.25	0.05	0.18

Notes: Robust standard errors in parentheses. *Information* treatment used as baseline category. "Donors" corresponds to restricted sample of judges who would donate themselves, "Non-Donors" to judges not donating. Controls include age, gender, social preferences, survey controls, whether the donation is chosen as delegated choice, and dummies for showing the video and giving/taking to/from the charity/decision-maker. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

do not find statistical support for Hypothesis 2, which suggests a less pronounced aversion to incentivinzing the information acquisition process in the *Paid Information* treatment compared to *Incentives* and *Informed Incentives*.

Result 3: Judges are less willing to intervene by *Incentives*, *Informed Incentives*, and *Paid Information* than by *Information*.

Columns 5 and 6 of Table 2.5 additionally report results regarding the intervention behavior of judges not willing to donate themselves, as depicted in Figure 2.A.3 in the Appendix. Compared to the *Information* treatment, judges unwilling to donate intervene significantly *more* often by *Incentives* and *Informed Incentives*. Hence, I find the opposite pattern than for judges who donate themselves. This differential effect can be decomposed in columns 7 and 8 of Table 2.5 when looking at interaction effects between judges' own donation behavior and the treatment dummies. Judges, who value the donation and donate themselves, generally intervene more often since they want to foster the prosocial outcome. This amounts to a 30.5 percentage points increase in interventions with and without control variables. Relative to this generally higher willingness of donors to intervene, there is a strong differential effect for donors in *Incentives* and *Informed Incentives* treatments compared to those in *Information.* In the former two treatments, donors strongly shy away from intervening by money, which amounts to a drop in interventions of 30.3 to 47.1 percentage points. Thus, the aversion to intervene by monetary incentives counteracts their general willingness to intervene. In total, this results in a drop in donors' willingness to intervene in *Incentives* and *Informed Incentives* treatments. Contrarily, since non-donors do not want to foster the donation, but prefer money, they are willing to transfer their favorism for money on decision-makers and implement the payoff increase in *Incentives* and *Informed Incentives* treatments. As a result, the overall intervention rate in the *Incentives* treatment, for example, appears to be similar for donors and non-donors with 87.7% and 87.5%, respectively, but is driven by very different motives.

Note that the differential effect regrading donors and non-donors intervention behavior is much less pronounced in the *Paid Information* treatment. This means that incentivizing the information acquisition process seems not to create similarly strong opposite responses between those who like or dislike the outcome as *Incentives* and *Informed Incentives* do. Therefore, while incentivinzing the information acquisition process cannot solve the aversion to incentives per se in this experiment, it may be able to at least resolve very strong opposing opinions regarding the monetary, interventional tool.

2.4 Conclusion

In this paper, I investigate the aversion to monetary incentives for changing behavior. In an experiment, in which one group of participants can attempt to change the donation behavior of others, I find that participants' willingness to intervene is lower if the intervention consists of money instead of information. This finding is surprising at first glance given that people believe that money is more effective than information to change people's behavior. A comprehensive set of elicited beliefs allows me to track this aversion back to money's inability to change more than just actions, i.e., one's reason to act. Incentivinzing the information acquisition process instead of the outcome does not resolve the aversion to incentives.

The paper's investigation of incentives' inability to change the reason to act can help to understand why monetary incentives sometimes do not work as intended (Ashraf et al. 2014; Bowles and Polania-Reyes 2012; Gneezy and Rustichini 2000a,b; Gneezy et al. 2011; Kamenica 2012) or prevail to be ineffective in the long-run (Charness and Gneezy 2009; Kesternich et al. 2016; Meier 2007). Similarly, it provides an explanation for why people shy away from monetarily incentivizing others when the incentivized action conflicts with rather than corresponds to an individual's autonomous choice (Ackfeld and Ockenfels 2019). My results also help to better evaluate which interventional tools may be feasible to be implemented by policy makers in the field. While Ambuehl and Ockenfels's (2017) vignette study finds support for the use of incentives in ethically loaded contexts as long as participants are cognitively aware of the consequences of the incentivized action, this project's results show that incentives may still not be a feasible tool if people have already made a decision for themselves. Moreover, results do not confirm that avoiding to directly incentivize outcomes as in Elias et al. (2019) significantly resolves concerns with monetary incentives.

My results may apply to so-called repugnant contexts like organ or blood donation (Roth 2007), but also more generally. In many domains in life like eating less meat, studying for long-run goals, or flying less, acting based on what one is convinced to do instead of what one is incentivized to do may be preferred by uninvolved parties. While similar results have been found in the economic literature on motivation crowding (Bowles and Polania-Reyes 2012; Frey and Jegen 2001; Frey and Oberholzer-Gee 1997; Titmuss 1970), this paper documents an aversion to monetary incentives also in a context in which motivation crowding-out does not apply. This establishes the aversion to monetary incentives for changing behavior as a more fundamental principle than previously assumed.

Money's inability to convince may explain why one often finds information campaigns rather than monetary incentives in the real world. For example, instead of increasing fines, policy makers often use safety campaigns on highways to foster considerate driving.

Instead of pricing roads or plastics, they appeal to climate friendly behavior via awareness campaigns. How to change people's behavior is a main political issue. Money's inability to convince people may explain why the use of monetary incentives in real life is limited and why it constitutes a steady topic of discourse between political parties.

Further research is needed to distinguish in detail under which conditions the aversion to monetary incentives applies and how it is affected by different institutional settings. Moreover, since this paper exclusively focuses on beliefs and behavior of the unaffected, intervening party, the responses of the parties affected by the intervention additionally have to be taken into account. Feedback about these responses may affect the intervening party's beliefs and intervention behavior, and may reshape how the trade-off between money's ability to change actions but its inability to change the underlying reason to act is perceived.

2.A Appendix: Additional results

Table 2.A.1: OLS regression results regarding judges' pre-intervention beliefs with controls

	Donations	Feeling informed		Feeling convinced	
		Donors	Non-Donors	Donors	Non-Donors
	(1)	(2)	(3)	(4)	(5)
Video before 1st decision	6.768**	8.660***	4.843*	1.712	2.855
	(2.940)	(2.443)	(2.845)	(1.889)	(2.930)
Constant	15.636	43.998**	9.950	54.345*	15.016
	(21.575)	(18.654)	(24.841)	(31.760)	(21.111)
N	380	380	380	380	380
R2	0.22	0.12	0.10	0.14	0.07

Notes: Robust standard errors in parentheses. "Video before 1st decision" corresponds to first stage of *Informed Incentives* treatment. All other treatments serve as baseline. "Donors" corresponds to decision-makers choosing the donation, "Non-Donors" to decision-makers choosing the egoistic option. Controls include age, gender, social preferences, survey controls, whether the donation is chosen as delegated choice and by the judge herself, and dummies for showing the video and giving/taking to/from the charity/decision-maker. * p<0.10, ** p<0.05, *** p<0.01.

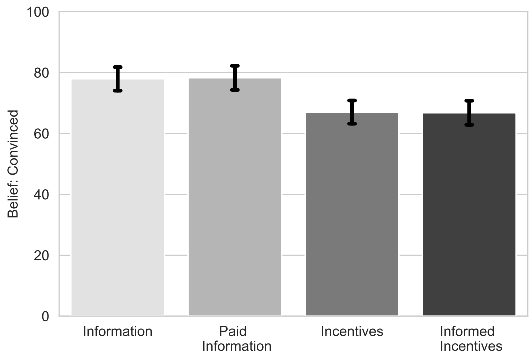
Table 2.A.2: Regression results regarding judges' pre- and post-intervention beliefs - Only donors

	Donations before	Informed before D	Informed before ND	Convinced before D	Convinced before ND	Increase donations	Increase informed	Increase convinced
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Video before 1st decision	8.770**	8.973***	8.772***	3.252	4.634			
	(3.564)	(2.388)	(3.044)	(1.984)	(3.387)			
Incentives						14.667***	-18.188***	-6.374
						(4.921)	(3.806)	(4.905)
Informed Incentives						11.647**	-22.806***	-12.871***
						(4.632)	(3.659)	(4.389)
Paid Info						2.722	2.905	0.780
						(4.984)	(4.319)	(4.664)
Intercept	46.892***	71.753***	42.922***	85.054***	43.108***	45.982***	32.855***	35.145***
	(2.027)	(1.500)	(1.772)	(1.468)	(1.756)	(3.579)	(2.755)	(3.167)
N	228	228	228	228	228	228	228	228
R2	0.02	0.05	0.03	0.01	0.01	0.06	0.23	0.05

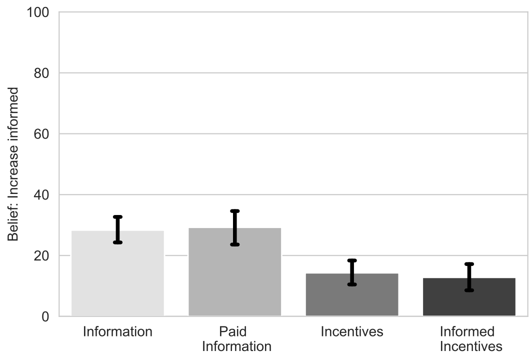
Notes: Robust standard errors in parentheses. "Video before 1st decision" corresponds to first stage of *Informed Incentives* treatment. All other treatments serve as baseline in columns 1 - 5. *Information* treatment used as baseline category in columns 6 - 8. "D" corresponds to decision-makers choosing the donation, "ND" to decision-makers choosing no donation. Only restricted sample of judges donating themselves considered. * p<0.10, ** p<0.05, *** p<0.01.

Figure 2.A.1: Bar plots of judges' beliefs of feeling convinced (absolute) and increase in feeling informed after intervention by treatment

(A) Absolute belief of feeling convinced



(B) Increase in feeling informed



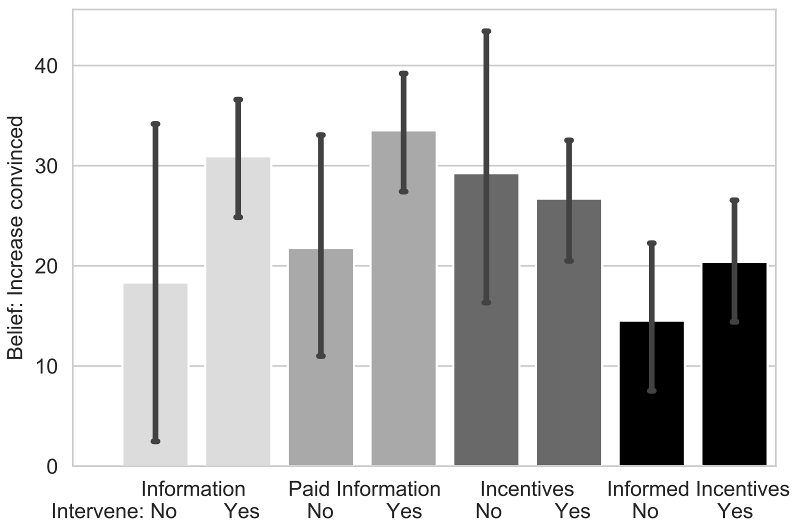
Notes: Vertical lines represent 95% confidence intervals. Panel (A) displays absolute values of judges' beliefs regarding how convinced initial non-donors feel after switching to the donation. Panel (B) displays judges' beliefs regarding how much more informed initial non-donors feel after the intervention.

Intervention behavior by perceived power of intervention to convince

If the ability to convince matters for the willingness to use an intervention, judges may intervene more often when considering an intervention as more convincing. On the individual level, this effect should hold independent of the treatment. For example, a judge in the *Informed Incentives* treatment, who holds a higher belief than another judge in the same treatment that an intervention by money is convincing, should more likely intervene by money than the other judge even if the ability of monetary incentives to convince is generally perceived to be lower than that of, e.g., *Information.* As Figure 2.A.2 displays, this pattern holds in all treatments except the *Incentives* treatment. In those treatments, the judge's belief that the intervention is convincing is higher if she decides to intervene than if she decides not to intervene. Thus, the ability to convince appears to guide intervention behavior.

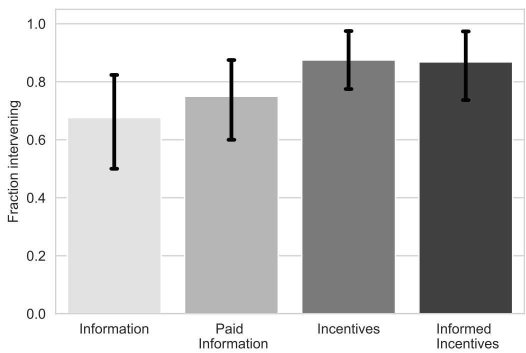
From a statistical perspective, note that the 95% confidence intervals in Figure 2.A.2 are rather large when looking at judges who do not intervene. This is due to the small number of observations in this subsample, which results in limited statistical power to detect a difference between intervening and non-intervening judges. Yet, an independent samples t-test confirms that judges who intervene expect the intervention to convince decision-makers significantly more ($p = 0.051$). This supports the idea that intervention behavior is driven by the intervention's ability to convince.

Figure 2.A.2: Bar plots of judges' beliefs regarding increase in feeling convinced by treatment and intervention decision



Notes: Increase in how convinced new donors feel after their post-intervention donation compared to their individual pre-intervention, non-donor level of conviction. Vertical lines represent 95% confidence intervals.

Figure 2.A.3: Bar plots of intervention behavior of non-donors by treatment



Notes: Vertical lines represent 95% confidence intervals. "Non-Donors" corresponds to restricted sample of judges who would not donate themselves

Table 2.A.3: Marginal effects from probit and logit models of likelihood to intervene by treatment

	Full sample		Donors	
	Logit	Probit	Logit	Probit
	(1)	(2)	(3)	(4)
Incentives	0.013	0.013	-0.181**	-0.171**
	(0.058)	(0.057)	(0.077)	(0.075)
Informed Incentives	-0.054	-0.055	-0.247***	-0.246***
	(0.053)	(0.053)	(0.066)	(0.068)
Paid Info	-0.056	-0.056	-0.201***	-0.192***
	(0.054)	(0.054)	(0.074)	(0.073)
N	380	380	228	228
Pseudo R2	0.01	0.01	0.08	0.08

Notes: Marginal effects at mean of logit and probit models, respectively. Robust standard errors in parentheses. *Information* treatment used as baseline category. "Donors" corresponds to restricted sample of judges who would donate themselves. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

2.B Appendix: Experimental material

2.B.1 Screenshots of treatment differences

Figure 2.B.1: Decision screen judge: *Incentives* and *Informed Incentives* treatments

Your task in this online experiment

Your task in this experiment consists of defining the rules for a participant in the subsequent laboratory experiment. This participant will be randomly assigned to you. If this participant has chosen 5€ for himself and has thereby decided **against** the donation, you can decide whether he should subsequently receive an additional payoff of 2€ for himself, so that he changes his decision and donates. Participants, who decide for the donation already at the beginning, always receive the an additional payoff of 2€ for themselves at the end of the experiment.

You can increase the private payoff of the participant in the laboratory experiment subsequently by 2€ if he chooses the donation. Instead of

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

he can then choose between

A) Receive 5€ as your own payoff, do not donate money
B) Receive **3€** as your own payoff, donate 6€

Do you want to offer the participant matched to you a 2€ higher payoff if he initially decided **against** the donation? Afterwards, the participant has the opportunity to choose between the two payoff alternatives again.

- Yes
- No

Figure 2.B.2: Decision screen judge: *Information* treatment

Your task in this online experiment

Your task in this experiment consists of defining the rules for a participant in the subsequent laboratory experiment. This participant will be randomly assigned to you. If this participant has chosen 5€ for himself and has thereby decided **against** the donation, you can decide whether he should subsequently receive additional information regarding tetanus vaccinations by video, so that he changes his decision and donates. Participants, who decide for the donation already at the beginning, always receive the additional information regarding tetanus vaccinations by video.

You can show the participant in the laboratory experiment the video about tetanus vaccinations, which you have watched yourself before.

Do you want to show the participant matched to you the video if he initially decided **against** the donation? Afterwards, the participant has the opportunity to choose between the two payoff alternatives again.

- Yes
- No

Figure 2.B.3: Decision screen judge: *Paid Information* treatment

Your task in this online experiment

Your task in this experiment consists of defining the rules for a participant in the subsequent laboratory experiment. This participant will be randomly assigned to you. If this participant has chosen 5€ for himself and has thereby decided **against** the donation, you can decide whether he should subsequently receive an additional payoff of 2€ for himself for watching the video about tetanus vaccinations, so that he changes his decision and donates. Participants, who decide for the donation already at the beginning, always receive the opportunity to inform themselves about tetanus vaccinations and receive a compensation of 2€ for this at the end of the experiment.

You can offer the participant in the laboratory experiment an additional payoff of 2€, which he receives if he watches the video about tetanus vaccinations, which you have watched yourself before.

Do you want offer the participant matched to you a 2€ higher payoff for watching the video if he initially decided **against** the donation? Afterwards, the participant has the opportunity to choose between the two payoff alternatives again.

- Yes
- No

2.B.2 Instructions

Instructions online experiment

Information regarding participation

Study titel: CvsB Online

Supervisor: Viola Ackfeld

Description: You participate in a scientific decision-making experiment. You will be asked to read instructions, answer questions, and make several choices which can determine the amount you and other participants are going to be paid. On top of that, this experiment contains video sequences. You will receive your payment <u>in cash in the week from May 27 to May 31</u> at the “Studierenden Service Center (SSC)“ of the University of Cologne.

Participant rights: Your participation in this experiment is voluntary. In order to use your data for research, you need to complete the entire experiment. You may withdraw from participation at any time during the experiment without giving any reasons.

Data protection: All statements in this experiment are made anonymously and do not allow to draw conclusions regarding individual participants. There is no relationship between your anonymous statements in the experiment and the personal data, which are stored in the participant portal of the Cologne Laboratory for Economics Research (CLER) for the sake of invitation to experiments. Data will be used for scientific purposes only and will be saved only for scientific data analysis. In order to guarantee scientific transparency and within scientific cooperation projects, the collected data may be made available for subsequent use by third parties.

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, do not participate

<u>Contact information</u>

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

Ethics Commission: Michael Otten, otten@wiso.uni-koeln.de

Information regarding the experiment

Welcome to this experiment and thank you for your participation. Since this experiment includes video sequences, please ensure that you can listen to them when you proceed with the experiment. If you do not load the experiment from a computer, please also make sure that you are connected to a WiFi network or have enough data at your disposal, respectively.

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 will participate, who are registered for experiments at the Cologne Laboratory for Economic Research (CLER) as you are. It is not possible to participate in both parts of the experiment. The second part of the experiment is going to be executed next week.

Your task in this online experiment is to determine 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 set a rule, then the corresponding rule is implemented in the laboratory experiment. 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.

Today’s online experiment will take 15 to 20 minutes. Your payoff in case of full participation in this experiment is **5€**. You receive your payment in cash in the week from May 27 to May 31 at the “Studierenden Service Center (SSC)“. Additionally, you have the opportunity to earn a bonus.

You can receive a bonus either by a decision at the end of this experiment or by a correct guess regarding participants‘ behavior in the laboratory experiment. During the course of the experiment, there will be several of these guesses. For each of these guesses, those five participants in this online experiment, whose single guesses are closest to the true average value in the laboratory experiment, receive a bonus of 2€ each.

* If the word participants is mentioned in this experiment, it refers to male, female, and diverse participants.

Payoff decision

In the laboratory experiment participants can choose between two payoff alternatives. In case of choosing the one alternative, they earn a higher payoff for themselves; in case of choosing the other, they earn a smaller payoff for themselves, but donate money to the charity UNICEF for vaccinations against the disease tetanus in developing countries. In particular, participants can choose between the following payoff alternatives:

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

B) Receive 1€ as your own payoff, donate 6€

When we refer to „the donation“ in this experiment, alternative B is meant. After the experiment, we are going to upload the donation receipts on our website ockenfels.uni-koeln.de under the category „Aktuelles”.

Info box: Tetanus vaccination

With a donation to UNICEF within the framework of this experiment, 20 people in developing countries can be vaccinated against the disease tetanus. These 20 people receive three vaccinations each to be protected against tetanus for several years. In case of giving birth, the immunization is also temporarily transferred to the newborn child.
Tetanus, also known as lockjaw, is a disease, in which bacteria get into the body via contaminated wounds. There, the poison of the bacteria initiates muscle strength, which can be fatal. Especially when giving birth, there is an increased risk that mother or child fall sick of tetanus.

Sources: Federal Center for Health Education, World Health Organization

At this point, we show you a video about tetanus vaccinations. Please watch the video. Afterwards, we will ask you some brief questions regarding the content of the video before you can proceed with the experiment. Participants in the laboratory experiment see the info box but not the video [informed incentives: both the info box and the video].

Video

[video]

Control questions:

1. For how long after birth is a child protected against tetanus if the mother is vaccinated against tetanus? [not at all, 2 month, 3 weeks, 1 year]
2. How are the fridges operated, in which the tetanus vaccine is stored? [with solar energy, with electricity, with gasoline, there is no need to cool the vaccine]

[proceed only if answered correctly, otherwise watch again]

Video

On a scale from 0 = „not at all informative“ to 100 = „fully informative“: How informative do you think is the video?

On a scale from 0 = „not at all convincing“ to 100 = „fully convincing“: How convincing do you think is the video?

Guesses I

At this point, please guess which decisions and statements participants in the laboratory experiment will make. For each of these guesses, those five participants in this online experiment, whose single guesses are closest to the true average value in the laboratory experiment, receive a bonus of 2€ each.

As already explained, participants in the laboratory experiment decide between the following payoff alternatives *without* having watched the video about tetanus vaccinations [informed incentives: after having watched the video about tetanus vaccinations]:

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

What do you think, which fraction of participants in the laboratory experiment will **choose** the **donation** (Alternative B)?

____%

On a scale from 0 = „not at all well informed“ to 100 = „fully informed“: What do you think, how well **informed** do participants in the laboratory experiment feel on average about tetanus vaccinations…

… if they decide **for** the donation? ____

… if they decide **against** the donation? ____

On a scale from 0 = „not at all convinced“ to 100 = „fully convinced“: What do you think, how **convinced** do participants in the laboratory experiment feel on average that the donation is the payoff alternative one should choose in such a situation…

… if they decide **for** the donation? ____

… if they decide **against** the donation? ____

Your task in this online experiment

Your task in this experiment consists of defining the rules for a participant in the subsequent laboratory experiment. This participant will be randomly assigned to you. If this participant has chosen 5€ for himself and has thereby decided **against** the donation, you can decide whether he should subsequently receive additional information regarding tetanus vaccinations by video / an additional payoff of 2€ for himself [for watching the video about tetanus vaccinations], so that he changes his decision and donates. Participants, who decide for the donation already at the beginning, always receive the additional information regarding tetanus vaccinations by video / an additional payoff of 2€ for themselves / the opportunity to inform themselves about tetanus vaccinations and receive a compensation of 2€ for this at the end of the experiment.

[Information]: You can show the participant in the laboratory experiment the video about tetanus vaccinations, which you have watched yourself before.

[Paid information]: You can offer the participant in the laboratory experiment an additional payoff of 2€, which he receives if he watches the video about tetanus vaccinations, which you have watched yourself before.

[Incentives, informed incentives]: You can increase the private payoff of the participant in the laboratory experiment subsequently by 2€ if he chooses the donation. Instead of

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

he can then choose between

A) Receive 5€ as your own payoff, do not donate money
B) Receive **3€** as your own payoff, donate 6€

Do you want to show the participant matched to you the video / offer the participant matched to you a 2€ higher payoff [for watching the video] if he initially decided **against** the donation? Afterwards, the participant has the opportunity to choose between the two payoff alternatives again. Yes/No

---

<u>Guesses II</u>

At this point, please guess again which decisions and statements participants in the laboratory experiment will make. For each of these guesses, those five participants in this online experiment, whose single guesses are closest to the true average value in the laboratory experiment, receive a bonus of 2€ each.

<u>Values of your previous guesses</u>	
Proportion of participants who donate in %	x
Informedness of participants who decide against the donation	y
Convincedness of participants who decide against the donation	z

[paid information] What do you think, which fraction of participants in the laboratory experiment, who initially decided **against** the donation, will **watch the video** if they receive an additional payoff for this?

____%

What do you think, which fraction of participants in the laboratory experiment, who initially decided **against** the donation, decide to **donate after all**, if they receive additional information by video / an additional payoff [for watching the video]?

____%

On a scale from 0 = „not at all well informed“ to 100 = „fully informed“: What do you think, how well **informed** do participants in the laboratory experiment, who initially decided against the donation and then receive additional information by video / an additional payoff [for watching the video], feel on average about tetanus vaccinations,…

… if they subsequently decide **for** the donation instead? ____

… if they continue to decide **against** the donation? ____

On a scale from 0 = „not at all convinced“ to 100 = „fully convinced“: What do you think, how **convinced** do participants in the laboratory experiment, who initially decided against the donation and then receive additional information by video / an additional payoff [for watching the video], feel on average that the donation is the payoff alternative one should choose in such a situation,…

… if they subsequently decide **for** the donation instead? ____

… if they continue to decide **against** the donation? ____

Further decisions

Besides your opportunity to receive a bonus for correct guesses, at this point you have a further opportunity to increase your fixed payoff of 5€.

Below, you see the two payoff alternatives again. Which alternative would you choose for yourself? For five randomly drawn participants 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 6€ for tetanus vaccinations to UNICEF. There is no subsequent possibility to change your decision.

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

On a scale from 0 = „not at all well informed“ to 100 = „fully informed“: How well informed do you feel about tetanus vaccinations?

____

On a scale from 0 = „not at all convinced“ to 100 = „fully convinced“: How convinced do you feel that the donation is the payoff alternative one should choose in such a situation?

____

Further decisions

Independent of your previous decisions, in 10% of the cases you can show another participant in the laboratory experiment a [informed incentives, information, paid information: another] video about tetanus vaccinations or initiate an increase or reduction of his payoff. Moreover, you can initiate an increase or reduction of the total amount donated to UNICEF for tetanus vaccinations. You make eleven decisions of this kind. One of these eleven decisions will then be randomly chosen with equal probability and implemented at the end of the laboratory experiment.

Do you want to deduct 1€ from the donation to UNICEF?

- Yes
- No

Do you want to add 1€ to the donation to UNICEF?

- Yes
- No

Do you want to deduct 1€ from the payoff of another participant? [each time yes/no]

- If he donates right at the beginning
- If he donates if he has received additional information by video / an additional payoff [for watching the video]
- If he never donates

Do you want to add 1€ to the payoff of another participant? [each time yes/no]

- If he donates right at the beginning
- If he donates if he has received additional information by video / an additional payoff [for watching the video]
- If he never donates

Do you want to show another participant another video about tetanus vaccinations? If you want, you can watch the video beforehand. [show video] [each time yes/no]

- If he donates right at the beginning

- If he donates if he has received additional information by video / an additional payoff [for watching the video]
- If he never donates

Further decisions

Besides your opportunity to receive a bonus for correct guesses, at this point you have a further opportunity to increase your fixed payoff of 5€.

If you can divide an amount of 6€ between yourself and another participant in the laboratory experiment, independent of your already made decisions, which distribution do you choose? For five randomly drawn participants in the online experiment, we are going to execute this choice. Thus, you and a participant in the laboratory experiment would both receive the corresponding amount of money.

Self	Participant in the laboratory experiment	
0€	6€	O
1€	5€	O
2€	4€	O
3€	3€	O
4€	2€	O
5€	1€	O
6€	0€	O

[only in information, incentives]

At this point, you can choose another measure to change the mind of the participant matched to you so that he changes his decision and donates if he initially decided *against* the donation. In 10% of the cases, we implement this measure instead of the previous measure.

[information] Instead of showing the participant in the laboratory experiment the video about tetanus vaccinations, you can increase his payoff by 2€ if he chooses the donation. Instead of

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

he can then choose between

A) Receive 5€ as your own payoff, do not donate money
B) Receive **3€** as your own payoff, donate 6€

[incentives] Instead of offering the participant in the laboratory experiment an additional payoff of 2€ for himself, you can show him the video about tetanus vaccinations, which you have watched yourself before.

In your previous decision, you have decided that the participant in the laboratory experiment matched to you should subsequently receive [no] additional informationen by video / [no] additional payoff for himself if he initially decided against the donation.

[set previous intervention decision (first button) as a default for radio button]

O [not allowed] Do not intervene / [information + allowed] Show video / [incentives + allowed] Offer a 2€ higher payoff

O [information] Offer a 2€ higher payoff / [incentives] Show video

If you want to stick with [not allowed] not intervening / [information + allowed] showing the participant the video about tetanus vaccinations / [incentives + allowed] offering the participant a 2€ higher payoff for himself, click on „continue“. If you prefer to [information] offer the participant a 2€ higher payoff for himself / [incentives] show the participant the video about tetanus vaccinations, first click on this option and then on „continue“.

<u>Questionnaire</u>

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

1. Tetanus is a serious disease.
2. Vaccination is useful.
3. UNICEF is a reliable charity.
4. In this experiment, it is morally reprehensible not to donate.
5. I decided such that the participant benefits first and foremost.
6. I wish that the participant in the laboratory experiment receives a fair payoff.

<u>Questionnaire</u>

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

7. One should not try to dissuade somebody from a decision he made himself.
8. It is better if people act by intrinsic motives (e.g., interest, conviction, …) than by extrinsic motives (e.g., duty, payment, …).
9. Money can change convictions.

Questionnaire

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

10. There should be more information campaigns regarding blood donation.
11. There should be more information campaigns regarding organ donation.
12. People should receive a payoff for donating blood.
13. People should receive a payoff for donating an organ.

Questionnaire

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

14. I do the experimenter a favor if I state a high value regarding the question: “On a scale from 0 = „not at all well informed“ to 100 = „fully informed“: What do you think, how well **informed** do participants in the laboratory experiment feel on average about tetanus vaccinations?“

15. I do the experimenter a favor if I state a high value regarding the question: “On a scale from 0 = „not at all convinced“ to 100 = „fully convinced“: What do you think, how **convinced** do participants in the laboratory experiment feel on average that the donation is the payoff alternative one should choose in such a situation?”

Personal details

Please provide the following information about yourself:

Age: ____ years

Gender: male, female, diverse

Semester: ___

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

Major: ______________________

Nationality: _________________

How did you access the experiment? [Computer/Laptop, Mobile phone, Tablet, other device]

Do you want to tell us something regarding this experiment? _________________________

Payoff

The experiment is finished now.

[if randomly selected for donation] You belong to the five randomly selected participant in the online experiment whose decision between the two payoff alternatives is implemented. You chose alternative [A/B] with/without donation. You receive a payoff of 1/5€, and 0/6€ are donated to UNICEF for tetanus vaccinations.

[if randomly selected for social preferences] You belong to the five randomly selected participant in the online experiment whose distribution decision between you and a participant in the laboratory experiment is implemented. You chose a distribution of __€ for yourself and __€ for the participant in the laboratory experiment.

You receive your payoff of 5[+y]€ in the week from May 27 to May 31 (not on the holiday on May 30) at the "Studierenden Service Center (SSC)" building, room 4.226, daily between 10am and 12pm and between 1pm and 4pm. You will learn whether you receive an additional bonus for your guesses when collecting your payoff since we can only calculate the bonus payments after the laboratory experiment has taken place next week.

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. Without this code, we cannot match your payoff. Please also bring a photo ID to the payout.

[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 regarding participation

Study titel: CvsB

Supervisor: Viola Ackfeld

Description: You participate in a scientific decision-making experiment. You will be asked to read instructions, answer questions, and make several choices which can determine the amount you and other participants are going to be paid. On top of that, this experiment can contain video sequences.

Participant rights: Your participation in this experiment is voluntary. In order to use your data for research, you need to complete the entire experiment. You may withdraw from participation at any time during the experiment without giving any reasons. If you choose to withdraw from the experiment, you will be paid the 4 Euro show-up fee but not the additional amount that you would have earned during the experiment.

Data protection: All statements in this experiment are made anonymously and do not allow to draw conclusions regarding individual participants. There is no relationship between your anonymous statements in the experiment and the personal data, which are stored in the participant portal of the Cologne Laboratory for Economics Research (CLER) for the sake of invitation to experiments. Data will be used for scientific purposes only and will be saved only for scientific data analysis. In order to guarantee scientific transparency and within scientific cooperation projects, the collected data may be made available for subsequent use by third parties.

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, do not participate

Contact information

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

Ethics Commission: Michael Otten, otten@wiso.uni-koeln.de

Information regarding the experiment

Welcome to this experiment and thank you for your participation. If you have questions during the experiment, you can always raise your hand. One of the experiments will come to you to answer your questions. From now on, please do no longer ask questions loudly and do not communicate with other participants*. Since this experiment can contain video sequences, please put on the headphones provided. Independent of your decisions, you are guaranteed to receive a show-up fee of 4 €.

In this experiment, you can choose between two payoff alternatives. In case of choosing the one alternative, you earn a higher payoff for yourself; in case of choosing the other, you earn a smaller payoff for yourself, but donate money to the charity UNICEF for vaccinations against the disease tetanus in developing countries. In particular, you can choose between the following payoff alternatives:

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

B) Receive 1€ as your own payoff, donate 6€

When we refer to „the donation“ in this experiment, alternative B is meant. After the experiment, we are going to upload the donation receipts on our website ockenfels.uni-koeln.de under the category „Aktuelles”.

Info box: Tetanus vaccination

With a donation to UNICEF within the framework of this experiment, 20 people in developing countries can be vaccinated against the disease tetanus. These 20 people receive three vaccinations each to be protected against tetanus for several years. In case of giving birth, the immunization is also temporarily transferred to the newborn child.
Tetanus, also known as lockjaw, is a disease, in which bacteria get into the body via contaminated wounds. There, the poison of the bacteria initiates muscle strength, which can be fatal. Especially when giving birth, there is an increased risk that mother or child fall sick of tetanus.

Sources: Federal Center for Health Education, World Health Organization

* If the word participants is mentioned in this experiment, male, female and diverse participants are meant.

[if informed incentives]

Video

At this point, we show you a video about tetanus vaccinations. Please watch the video. Afterwards, we will ask you some brief questions regarding the content of the video before you can proceed with the experiment.

.

[video]

Control questions:

1. For how long after birth is a child protected against tetanus if the mother is vaccinated against tetanus? [not at all, 2 month, 3 weeks, 1 year]
2. How are the fridges operated, in which the tetanus vaccine is stored? [with solar energy, with electricity, with gasoline, there is no need to cool the vaccine]

[proceed only if answered correctly, otherwise watch again]

[if 10% delegation]

Delegate

We offer 10% of the participants in this experiment the opportunity to delegate the choice between the two alternatives. This means that the decision of another participant from a previous experiment is executed, whom we explained the rules of today's experiment, and who made a corresponding decision for you.

You belong to the 10% of participants who can delegate their choice between the two payoff alternatives. What do you want to do?

Delegate decision / Choose yourself [jump to payoff if delegate chosen]

[if participant chose "delegate decision"]

Delegate

The participant matched to you chose Alternative [A/B]. You receive a payment of 5/1€ and 0/6€ are donated to UNICEF for tetanus vaccinations.

Payoff choice

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

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

Payoff choice

You have chosen Alternative A/B. You receive a payoff of 5/1€ and 0/6€ are donated to UNICEF for tetanus vaccinations.

On a scale from 0 = „not at all well informed“ to 100 = „fully informed“: How well **informed** do you feel about tetanus vaccinations?

____

On a scale from 0 = „not at all convinced“ to 100 = „fully convinced“: How **convinced** do you feel that the donation is the payoff alternative one should choose in such a situation?

____

[If A + **incentives/informed incentives** + allowed]

Payoff choice

You chose alternative A without donation.

We offer you to increase your own payoff in case of donation by 2€. This means that you can now choose between

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

B) Receive **3€** as your own payoff, donate 6€

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“.

Note: There will be no further possibility to subsequently change your decision.

[If A + **information** + allowed]

Video

You chose alternative A without donation.

At this point, we show you a video about tetanus vaccinations. Please watch the video. Afterwards, we will ask you some brief questions regarding the content of the video before you can proceed with the experiment.

[video]

Control questions:

1. For how long after birth is a child protected against tetanus if the mother is vaccinated against tetanus? [not at all, 2 month, 3 weeks, 1 year]
2. How are the fridges operated, in which the tetanus vaccine is stored? [with solar energy, with electricity, with gasoline, there is no need to cool the vaccine]

[proceed only if answered correctly, otherwise watch again]

Payoff choice

Here, you see the two payoff alternatives again. 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“.

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

Note: There will be no further possibility to subsequently change your decision.

[If A + **paid information** + allowed]

You chose alternative A without donation.

We offer you to increase your payoff in this experiment by 2€ if you watch a video, which informs you about tetanus vaccinations. If you watch the video, we will ask you some brief questions regarding the content of the video afterwards before you can proceed with the experiment.

Do you want to watch the video? You receive 2€ in exchange. Yes/No

---

[if yes:]

Video

[video]

Control questions:

1. For how long after birth is a child protected against tetanus if the mother is vaccinated against tetanus? [not at all, 2 month, 3 weeks, 1 year]

2. How are the fridges operated, in which the tetanus vaccine is stored? [with solar energy, with electricity, with gasoline, there is no need to cool the vaccine]

[proceed only if answered correctly, otherwise watch again]

Payoff choice

Here, you see the two payoff alternatives again. 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“.

A) Receive 5€ as your own payoff, do not donate money
B) Receive 1€ as your own payoff, donate 6€

Note: There will be no further possibility to subsequently change your decision.

[If A + allowed (in **paid information**: + watched video)]

Payoff choice

You chose alternative [A/B]. You receive a payoff of 1/3/5€, and 0/6€ are donated to UNICEF for tetanus vaccinations.

On a scale from 0 = „not at all well informed“ to 100 = „fully informed“: How well **informed** do you feel about tetanus vaccinations?

____

On a scale from 0 = „not at all convinced“ to 100 = „fully convinced“: How **convinced** do you feel that the donation is the payoff alternative one should choose in such a situation?

____

Questionnaire

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

1. Tetanus is a serious disease.
2. Vaccination is useful.
3. UNICEF is a reliable charity.
4. In this experiment, it is immorally reprehensible not to donate.

Questionnaire

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

5. One should not try to dissuade somebody from a decision he made himself.
6. It is better if people act by intrinsic motives (e.g., interest, conviction, …) than by extrinsic motives (e.g., duty, payment, …).
7. Money can change convictions.

Questionnaire

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

8. There should be more information campaigns regarding blood donation.
9. There should be more information campaigns regarding organ donation.
10. People should receive a payoff for donating blood.
11. People should receive a payoff for donating an organ.

Questionnaire

Please answer the following questions. You can refine your answer on a 7-point-scale from 1=”fully agree”, 7=”fully disagree”.

12. I do the experimenter a favor if I state a high value regarding the question: “On a scale from 0 = „not at all well informed“ to 100 = „fully informed“: How well **informed** do you feel about tetanus vaccinations?”
13. I do the experimenter a favor if I state a high value regarding the question: “On a scale from 0 = „not at all convinced“ to 100 = „fully convinced“: How **convinced** do you feel that the donation is the payoff alternative one should choose in such a situation?”

Personal details

Please provide the following information about yourself. [if B directly chosen in **incentives**, **informed incentives**: You receive 2€ for filling in the questionnaire.]

Age: ____ years

Gender: male, female, divers

Semester: ___

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

Major: ______________________

Nationality: _________________

[if B directly chosen in **information**]

Video

At this point, we show you a video about tetanus vaccinations. Please watch the video. Afterwards, we will ask you some brief questions regarding the content of the video before you can proceed with the experiment.

[video]

Control questions:

1. For how long after birth is a child protected against tetanus if the mother is vaccinated against tetanus? [not at all, 2 month, 3 weeks, 1 year]
2. How are the fridges operated, in which the tetanus vaccine is stored? [with solar energy, with electricity, with gasoline, there is no need to cool the vaccine]

[proceed only if answered correctly, otherwise watch again]

[if B directly chosen in **paid information**]

We offer you to increase your payoff in this experiment by 2€ if you watch a video, which informs you about tetanus vaccinations. If you watch the video, we will ask you some brief questions regarding the content of the video afterwards before you can proceed with the experiment.

Do you want to watch the video. You receive 2€ in exchange. Yes / No

---

[if yes]

<u>Video</u>

[video]

Control questions:

1. For how long after birth is a child protected against tetanus if the mother is vaccinated against tetanus? [not at all, 2 month, 3 weeks, 1 year]
2. How are the fridges operated, in which the tetanus vaccine is stored? [with solar energy, with electricity, with gasoline, there is no need to cool the vaccine]

[proceed only if answered correctly, otherwise watch again]

<u>Your decision will be implemented.</u>

If you are not forwarded automatically, click “Continue”.

[if additional video allowed]

<u>Video</u>

Here, you see a [if already watched a video: another] video about tetanus vaccinations. Please watch the video.

[video]

<u>Payoff</u>

The experiment is finished now.

[if drawn for bonus/deduction] Recently, we explained participants in another experiment the course of this experiment, and let them inter alia decide whether money should be added to / subtracted from your payoff as lump-sum. The participant matched to you decided that a lump-sum of 1€ should be added/subtracted to/from your payoff.

[if drawn for social preferences] Recently, we explained participants in another experiment the course of this experiment, and let them inter alia decide how to distribute an amount of 6€ between himself and you. The participant matched to you allocated [z]€ to you.

Including your show-up fee of 4€ [if bonus/deduction: , the bonus/deduction of 1€,] [if social preferences: , the share allocated to you in the distribution decision] [if B directly chosen in incentives, informed incentives: , the 2€ for filling in the questionnaire] [if paid information + watched video: , the 2€ for watching the video] and your payoff of [1/3/5€] from the donation decision, you receive a final payoff of **[x]€.**

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

The donation receipts with the total amount donated in this experiment will soon be published on the website ockenfels.uni-koeln.de under the category „Aktuelles".

Thank you very much for your participation in this 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.

2.B.3 Translated video texts

Video 1

I traveled to Madagascar with UNICEF because we want to defeat tetanus amongst new-borns here. And we had a look at how exactly this works.

This is the national storage for these vaccines in Madagascar. You can already feel it's a little cooler here. They must be kept between four and eight degrees. These are the tetanus vaccines – you can see them here. They are brought from the storage up to the smallest village in Madagascar.

A dose of vaccine takes quite a long journey to arrive where we need it. What matters is that it is cooled during the entire time, and this is easy to figure out because there is a small dot on every vial containing the vaccine. If this dot changes color, you can see immediately: This dose of vaccine is no longer durable. Those fridges, which they have there, are very special and are not connected to the power supply because there is no electricity. The doctor also told us that they unfortunately can't give births at night because they don't have light. But, the refrigerator works because it runs on petrol. Great!

Tetanus is a bacterium. That's important to know. It is transmitted naturally in extremely unhygienic conditions, at birth. Once you have it, you can't heal it anymore. That means one has to take preventive action. It's just a small shot – if you think about it – containing 0.5 milliliters you have to inject and you know you'll be safe afterwards. The great thing is: Mothers only need three doses of vaccine to be protected - five years for themselves and they pass the immunization on to the child for two months after birth, and this is of course a great thing. Then, both are safe in any case.

The dream of every family is to have a child that is healthy. That is the most important thing. We're not satisfied until tetanus is completely defeated. Every support counts! We have already achieved a lot, but we need your contribution. Therefore, please donate so that we can continue working in the fight against tetanus in new-borns.

Video 2

Hello and welcome to UNICEF TV. I traveled to the Central African Republic, one of the poorest countries in the world, to learn about tetanus vaccinations. The Central African Republic is located right in the middle of Africa. 4.5 million people live here under most challenging conditions. Only every 4th person has access to clean drinking water, and only every 4th woman can give birth to her child in a health station or a hospital, often only after walking for kilometers. UNICEF trains midwives because hygienic conditions at birth and skilled care significantly reduce the risk of infection. Tetanus is fatal, but so easy to defeat. You just have to know how!

Chapter 3

Personal Information Disclosure under Competition for Benefits: Is Sharing Caring?*

joint with Werner Güth

Abstract

Personal information is shared extensively every day, partly in exchange for benefits or as a reaction to other people's information sharing. In this paper, we experimentally investigate these two motives by analyzing the interaction of peer comparison and incentives to disclose potentially privacy-sensitive information. We find that information sharing is higher under incentives, and further increases under peer comparison. This effect is driven by those initially disclosing less, who additionally report feeling more compelled to reveal information. Our results provide an explanation for the current information-sharing trend while pointing to a potentially neglected side effect.

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"Most hiring requires a LinkedIn profile now, so although we use this narrative of choice [...] they substantively don't really have a choice because in the modern workforce you have to use social media, and you have to use the internet. [...] When people have to use these platforms [...] to get a job, they will still use it, and so we are sort of coercing and compelling people to hand over a lot of information [...]."

– Christopher Wylie, Cambridge Analytica - May 16, 2018

3.1 Introduction

Extensive sharing of personal information has become a stylized fact and one of the major societal changes of the 21st century. Getting access to many new services or exchange platforms nowadays often implicitly requires sharing one's personal information. Hence, in many situations in life, extensive personal information sharing may be driven by motives which go beyond a direct preference for information sharing.

First, there may be *strategic competition* in personal information disclosure if people compete for monetary rewards or the beneficial attention of others. For example, people compete via extensive personal information disclosure for the attention of overnight guests or recruiters on Airbnb and LinkedIn, respectively, or for that of followers on Instagram or Youtube. The mircrofinance platform Kickstarter even recommends borrowers to include soft, personal information in their requests. Second, one may react to the information disclosure behavior of one's *peers*, i.e., the more others share, the more likely one adapts to their behavior (Acquisti et al. 2012; Böhme and Pötzsch 2011; Chang et al. 2016). This effect might be especially pronounced in competitive settings. Abstaining for the information-sharing economy may become more and more impossible the more competing peers engage in disclosure. This may lead to situations in which some people, who have higher privacy valuations than others, feel pressured to reveal information about themselves, but incur high privacy costs. Hence, under information disclosure competition against peers, they may be worse off compared to a world without such information-sharing dynamics.

This paper analyzes different motives for extensive personal information sharing and sheds light on the potentially hidden costs thereof. First, we study the interaction of competition for benefits and observing peers' sharing as a channel explaining extensive personal information disclosure. In particular, we investigate whether incentives to reveal personal information lead to more information sharing, and how one adapts one's initial choice in reaction to peer comparison. Second, we explore whether and how the interplay of these two factors contributes to subjectively perceived pressure to reveal more, or more unpleasant, information. Thereby, we point to a potential side effect of extensive information disclosure competition, which has not gained attention so far.

We investigate these questions in a laboratory experiment, which enables us to provide causal evidence on competition via personal information disclosure, and to disentangle via a two-by-two design how peer comparison and disclosure competition interact. Two participants compete for distribution power in an allocation game. In the main treatments, a third participant selects who determines the allocation. In order to be selected, candidates striving for distribution power can endogenously reveal answers from a potentially privacy-sensitive questionnaire, thus making information sharing strategic. In the control treatments, distribution power is randomly assigned, so information sharing has no strategic aspect. As a second dimension, we inform participants in half of the treatments (without prior announcement) about their competitor's disclosure choice, and give them the opportunity to adapt their own. Thereby, we can test for the effect of peer comparison on disclosure behavior with and without competition involved. Afterwards, we measure participants' perceived pressure to disclose information, as well as game-related outcomes.

We find that information disclosure doubles under strategic incentives compared to the control condition with random assignment of allocation power. Moreover, subsequent peer comparison boosts information disclosure in the strategic, but not in the random setting. This effect is driven by subjects who are initially relatively unwilling to disclose much, but reveal more information when learning to lag behind. In line with the idea of reluctant adaptations of the less disclosure-willing candidates, these participants report feeling more compelled to disclose information afterwards. We do not find such an effect for strategic incentives in general. Additional results on distributor choice and distribution outcomes show a positive effect of revealing more information on being selected, but ambiguous results regarding generosity in payoff distribution.

Investigating motives and hidden costs of personal information sharing, the contributions of our paper are threefold. Firstly, we allow for endogenous sharing of personal information, and thereby show how such information can be strategically employed to compete. Experimental research regarding the value of personal information has mainly been based on exogenous provision of personal information so far (Bohnet and Frey 1999; Brandts et al. 2006; Charness and Gneezy 2008; Eckel and Petrie 2011), and therefore does not take into account how such information can be strategically used to attract attention.[1] While Benndorf et al. (2015) motivate their study with strategic privacy-sensitive information disclosure, they only use exogenously assigned, impersonal information without an intrinsic privacy value for participants. Our study goes one step further in understanding privacy concerns by using information with an intrinsic private value for participants.[2]

Secondly, we shed light on motives driving the personal information-sharing trend. In particular, we provide novel evidence on the dynamics created by the interaction of strategic incentives and peer comparison. Acquisti et al. (2012), Böhme and Pötzsch (2011), and Chang et al. (2016) show that peer comparison spurs one's personal information-disclosure behavior. We provide causal evidence that such peer effects may be especially pronounced under competition for benefits and also affect those who are initially unwilling to disclose information. The combination of these two motives may explain the recent boom in extensive personal information sharing, a stylized fact of the digital age, whose underlying dynamics have mainly been neglected so far. In this way, we may detect a new form of competition in society.

Thirdly, we uncover that extensive information sharing might not necessarily generate improvements for all involved parties. Lee (2014) and Wang et al. (2011) find correla-

[1]See Hermstrüwer and Dickert (2017) and Holm and Samahita (2018) for experimental research on how the presence of personal information affects prosocial behavior in light of maintaining a social image, and Gaudeul and Giannetti (2017) on group formation and contribution behavior based on the endogenous provision of personal information. See Bartoš et al. (2016) for research on how limited attention can influence the selection of candidates.

[2]Several papers try to measure the economic value of privacy, but find ambiguous results (Benndorf and Normann 2018; Beresford et al. 2012; Jentzsch et al. 2012; Tsai et al. 2011). See Farrell (2012) for a discussion regarding the economic properties of privacy and Acquisti et al. (2016) and Tucker (2015) for comprehensive surveys on this topic.

tional evidence for hidden costs of information sharing. To the best of our knowledge, our paper is the first to provide causal, empirical evidence for side effects of personal information sharing. If disclosure-unwilling individuals feel pressured to disclose more, or more unpleasant, and costly information than they intrinsically would like to, they may be worse off compared to a world without such information-sharing dynamics. While previous economic research has investigated side effects of social pressure (DellaVigna et al. 2012, 2017; Reyniers and Bhalla 2013) and competition (Brandts et al. 2009) in isolation, we investigate potential side effects based on a combination of these motives in the highly relevant context of personal information sharing.[3]

The remainder of this paper is structured as follows. In Section 3.2, we review related literature. We present our experimental design and corresponding hypotheses in Section 3.3. Section 3.4 reports and Section 3.5 discusses the results. The last section concludes.

3.2 Literature

Our paper primarily builds on two strands of literature: the value of personal information provision and the impact of peer comparison.[4] We contribute to the first literature by endogenizing the information-sharing decision, and to the second by providing evidence on the existence and consequences of peer comparison in the new and highly relevant context of personal information sharing with heterogeneous privacy types (Plesch and Wolff 2018). We combine both literatures by investigating the interaction of peer comparison and strategic incentives for information sharing, and discover potentially hidden costs. Examining the interplay of these two motives and its consequences in the context of personal data sharing, our paper dynamically adds to research on the economics of privacy

[3]Note that this paper focuses on potentially overseen cost at the disclosing market side, but refrains from a welfare analysis. Since non-anonymous personal information in real-world markets may be more predictive for behavior than the anonymous data used in this experiment, we would otherwise risk underestimating welfare gains. Rather, our approach attempts to reveal a subjective and usually unobservable type of information-disclosure costs. Finding such hidden costs already in a setting with anonymous information likely implies more pronounced effects in the field with identifiable data. Hence, the effects we find may constitute a lower bound for costs, independently of gains.

[4]We also touch on several other strands of literature. Our experimental design consists of elements from the partner selection and proposer competition literature. Regarding partner selection, a couple of studies show that partner selection can help to overcome coordination failures (Coricelli et al. 2004; Page et al. 2005; Riedl et al. 2016; Wang et al. 2012). Proposer competition prevails to affect the distribution of money in favor of the responder (Roth et al. 1991).

(Acquisti et al. 2016; Benndorf and Normann 2018; Beresford et al. 2012; Jentzsch et al. 2012; Tucker 2015). By endogenizing the sharing of personal information, our experiment substantially extends a design by Brandts et al. (2006).[5]

Several studies show a positive value of personal information sharing in line with our results. For example, subjects in distribution games give more if personal information like name, major, hobbies, and home city of the recipient are revealed (Bohnet and Frey 1999; Charness and Gneezy 2008)[6]. Hermstrüwer and Dickert (2017) report higher contributions when participants previously consented to reveal their name together with their contribution afterwards.[7] Remarkably, participants even seem willing to pay for seeing the partner's photo in trust games (Eckel and Petrie 2011). With regard to the endogenous provision of personal information, Gaudeul and Giannetti (2017) find higher contributions in public good games when group formation is based on endogenously provided names. Observational data from online microfinance platforms mostly support the idea that personal information sharing is valuable. Michels (2012) reports lower interest rates for loan requests containing a photo, and inversely, Pope and Sydnor (2011) show that chances to get a loan decrease without one. The latter's analysis reveals that even given observable financial indicators, the provision of a picture matters for receiving funding. Böhme and Pötzsch (2010) find evidence for such a relationship for commercial, but not for private borrowers.

Theoretically, the positive value of information sharing is predicted by unraveling theory (Milgrom 1981). Under market competition, good types share their private information, while non-sharing correctly evokes suspicion about quality. However, laboratory tests confirm unraveling only partially (Benndorf 2018; Jin et al. 2017), especially when adding a more privacy-sensitive framing (Benndorf et al. 2015). We go one step further by using not only exogenously assigned information, but real personal information. This renders full unraveling even less likely in our experiment.

[5]Brandts et al. (2006) use a personality questionnaire to determine allocation power in a distribution task, either randomly or based on this questionnaire. Since information is exogenously provided in Brandts et al. (2006), their focus lies on how being actively selected affects distributional behavior, while we are interested in the amount of information endogenously provided.

[6]However, Charness and Gneezy (2008) cannot confirm this result in the ultimatum game.

[7]In the opposite setting in Holm and Samahita (2018), participants are more likely to hide their picture subsequently if they behaved less generously, but Hermstrüwer and Dickert (2017) do not find such an effect for names.

Evidence for how well voluntarily provided personal information can predict types is mixed. Duarte et al. (2012) observe a positive relationship between the appearance of trustworthiness in pictures and actual trustworthiness in microfinance. Ge et al. (2017) show that connected social media information can serve as a deterrence mechanism for credit default. While creditors in Pope and Sydnor's (2011) study seem to make use of voluntarily provided personal information, they fail to infer all relevant hints on creditworthiness. Relatedly, Iyer et al. (2016) only find a significant effect of insightful inference from voluntarily provided personal information for low-credit categories. Our results suggest that information sharing under competition for benefits can indeed enhance the voluntary provided amount of relevant information, but inference on quality may be obfuscated by sharing too much non-insightful information if competition is present.

As a second dimension, our project is related to several aspects of the literature on peer effects, predominantly peer pressure driven by conformity seeking (Asch 1951; Bernheim 1994) and social comparison (Clark and Oswald 1998; Festinger 1954; Frey and Meier 2004). A variety of empirical papers documents that peers have a strong impact on how we behave.[8] Given the diverse range of settings in which peer effects seem to be at work, peer comparison likely also affects personal information disclosure. However, evidence analyzing peer effects regarding endogenous information revelation is rare. First related results point in the direction that the amount of information others reveal influences one's own disclosure behavior. Findings by Acquisti et al. (2012) and Chang et al. (2016) indicate that people are more willing to answer sensitive questions or disclose sensitive pictures, respectively, when knowing that others have done so. On online microfinance platforms, Böhme and Pötzsch (2011) find that borrowers adapt their loan request to the most recent requests listed on the top of the starting page regarding how much to write, whether to add a photo, what personal information to disclose, and how identifiable to present oneself. Results regarding adaptations within the same loan category further suggest positive peer effects, but are less conclusive. We provide experimental evidence

[8] For example, people show more effort in the workplace (Falk and Ichino 2006; Mas and Moretti 2009), vote in elections (Bond et al. 2012; DellaVigna et al. 2017; Funk 2010), adapt their investment behavior (Bursztyn et al. 2014), or donate more (Alpizar et al. 2008; DellaVigna et al. 2012; Frey and Meier 2004; Meer 2011) due to peers. See Bursztyn and Jensen (2017) for a review.

on the effect of peer comparison in the domain of personal data sharing and its interaction with strategic incentives to disclose information.

Both the influence of peers and the competitive aspect in personal information sharing may, however, cause unintended and non-negligible side effects. Research by DellaVigna et al. (2012, 2017) shows that actions meant to increase welfare can even have negative welfare effects if social pressure is accounted for.[9] Moreover, there is evidence that peer comparison harms happiness (Reyniers and Bhalla 2013), and that peer pressure in form of competition decreases well-being without creating any gains (Brandts et al. 2009). Exploratory studies surveying or interviewing Facebook users confirm peer pressure in the online world. Wang et al. (2011) report that the desire to appear favorable to one's peers induces people to post something they regret afterwards. With regard to social comparison, Lee (2014) finds a positive correlation between comparison-seeking frequency on Facebook and negative feelings from comparison. We contribute to this literature by providing causal evidence regarding the hidden costs of personal information sharing competition.[10]

3.3 Experimental design

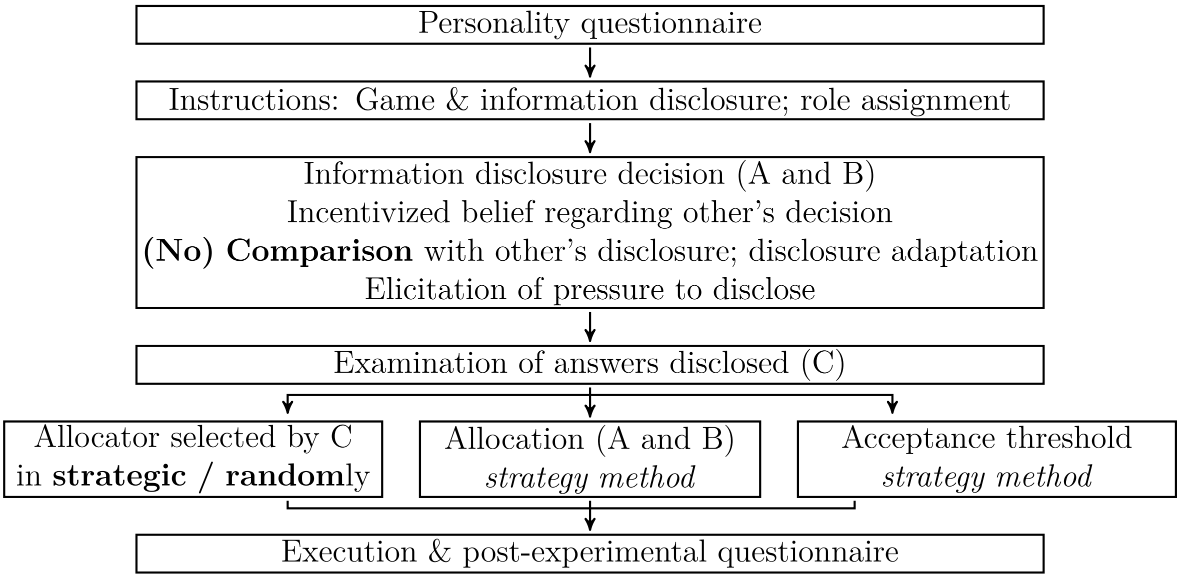
Our experimental design, depicted in Figure 3.1, consists of the following parts: First of all, information is generated. Second, participants endogenously decide which pieces of information to reveal. Third, they can revise their decision after peer comparison. Fourth, information is taken into account for role selection. Fifth, the allocation and the resulting payoffs are determined. Steps one, four, and five are adapted from Brandts et al. (2006). In order to guarantee understanding, participants have to answer several comprehension questions correctly before being allowed to make decisions regarding our

[9]Similarly, Funk (2010) observes a policy intervention aimed at increasing voter turnout which has the opposite consequence to the expected one, due to the role of social pressure not being taken into account.

[10]Recent theoretical models in economics try to combine peer effects with information disclosure and privacy. Daughety and Reinganum (2010) build a model with different privacy scenarios in which marginal types in a regime in which it is possible to waive privacy are, in equilibrium, pressured to reveal their type because they care about how they are perceived by others. Ali and Bénabou's (Forthcoming) model emphasizes that in fast-changing societies with variability in norms, extensive personal information sharing based on image concerns hinders the correct aggregation of information by a policy maker to infer society's true aggregated preferences.

outcomes of interest. In what follows, the different parts, procedures, and treatments are described in detail. We first focus on the personal information we elicit, continue with the game, the endogenous information revelation process, and additional measures we use, and finish by describing the treatments and as well as corresponding hypotheses. The experimental instructions can be found in Appendix 3.B.

Figure 3.1: Structure of the experiment



Notes: Overview of the experimental steps. A unit of observation in the experiment consists of three participants A, B, and C. Treatment differences are marked in bold letters.

Personal information

While a first-best approach to study personal information disclosure might be to access real-world data, for example from social media, such data come with shortcomings. First, they are complex and what information people have already accessed or what they infer from it is out of experimental control, which is likely to impair causal inference. Secondly, studying the dynamic interaction of strategic incentives and peer comparison, our channel of interest, and at the same time eliciting measures for potential side effects seems hardly possible with field data on a experimentally sound level. Instead, we follow a second-best approach and generate relevant and potentially sensitive, but anonymous and controllable personal information similar to Frik and Gaudeul (2016).

Table 3.1: Questionnaire

Question 1	Do you make decision mainly in such a way that you benefit yourself?
Question 2	Do you consider inequality in society, which is based on different performances, as something negative?
Question 3	Are there reasons which justify reading emails or messages of friends?
Question 4	Would you accept a well-paid job if you knew it hurts others?
Question 5	Do you only participate in laboratory experiments because of money?
Question 6	Is it acceptable to lie in some situations?
Question 7	Should people who voluntarily donate an organ receive payment for it?
Question 8	Is winning important to you?
Question 9	Is it okay to read one's text messages on the cellphone while driving?
Question 10	Does it affect you a lot if you fail an exam, or failed one in the future?
Question 11	Is it okay to drive a car after drinking one glass of beer (0.5 liters) or one glass of wine (0.2 liters)?
Question 12	Is it important to you what others think about you?

Notes: Scale: 1 = not at all, 7 = definitely. Order randomized.

We use a 12-item questionnaire to elicit opinions and personality traits measured on a 7-point scale, shown in Table 3.1. Some questions refer to characteristics likely related to experimental game behavior, while others ask for rather unrelated, subjective opinions or attitudes regarding controversial or sensitive issues. For example, we elicit how participants perceive inequality, whether money is their only reason to participate in experiments, how they assess payment for organ donation, or whether they feel impairment when failing an exam.[11] Participants receive 3 Euro for answering the questionnaire, being aware that all information they provide can affect their payments in the experiment, but without knowing yet what will follow in the second part.[12] We let participants explicitly

[11]The information we elicit is mainly subjective statements and can, by the nature of this kind of information, hardly be verified. Although some authors argue that the use of information which cannot be verified might be problematic in contexts related to pricing privacy (Benndorf and Normann 2018; Schudy and Utikal 2017), alternatives like pictures or names used in previous studies (Benndorf and Normann 2018; Bohnet and Frey 1999; Charness and Gneezy 2008; Eckel and Petrie 2011; Gaudeul and Giannetti 2017; Hermstrüwer and Dickert 2017; Holm and Samahita 2018) create problems of identifiability instead. Using information which cannot be verified, but contains no inherent right or wrong can overcome this issue (Frik and Gaudeul 2016) and is adapted in this work. We are interested in endogenous information revelation as a reaction to different treatment manipulations, and there is no reason to assume that answering the questionnaire initially varies between our treatments.

[12]Eliciting information in the first part for the second part, in which it might be payoff-relevant, without prior knowledge of this connection might be considered as problematic since we only inform participants gradually about the course of the experiment. However, such an approach becomes necessary

consent to this non-standard approach by stating that "additional payment depends on the statements [...] you and your interaction partners make" before the information revelation stage. Moreover, we emphasize voluntariness of participation and the right to leave the experiment at any time.

The questions are designed such that there is no general right or wrong. Consequently, the relevance of a question must be subjectively assessed, which leaves room for interpretation. We use this kind of questions for three reasons. Firstly, in everyday life, one often has to decide which information to disclose to others without knowing how that information will be perceived and interpreted. Secondly, such questions preserve anonymity, thereby guaranteeing high experimental standards. If we find side effects of information disclosure even in case of our comparably less privacy-sensitive information, perceived pressure in the field with identifiable information is likely even stronger. Thirdly, having no clear right or wrong renders lying unreasonable. We randomize the order of questions to avoid any order effects.[13]

Distribution game

After answering the questionnaire, participants receive the second part of the instructions explaining the experimental game and the preceding possibility to reveal information. The game is the impunity game (Bolton and Zwick 1995), played one-shot in randomly assigned groups of three players. One player, the proposer, distributes a pie of 17 Euro between herself and the other two group members. The other two players are responders who can only accept or reject their own share. They only learn their own proposed shares and decide independently of each other. Unlike in the ultimatum game, a rejection in

in experimental economics if more elaborate research questions require more flexible designs. See, for example, Brandts et al. (2006) and Khalmetski et al. (2015) for other research which requires non-standard techniques. Since the purpose of our experiment is to investigate how economic and social pressure affect the willingness to disclose potentially sensitive information, telling participants in advance what will follow would distort their initial reports. In fact, asking participants for their acceptance of subsequently using their answers does not indicate any resentment. On a scale from 1 to 7, with 7 being the full approval of subsequent information usage, the lowest treatment average is 5.47. Some participants even commented positively on the fact that personal information could influence the distribution phase.

[13]In particular, we display the questions on two separate screens with six questions each, and randomize the screens order, as well as the position of questions within the screens, to avoid order effects. Acquisti et al. (2012) find order effects in the willingness to answer intrusive questions. We use ten different random orderings of questions, and control for these orderings in the regression analyses.

the impunity game does not imply that all players earn zero. Instead, only the rejecting player receives zero, while the proposer's payoff remains unaffected, as does that of the other responder. However, the proposer is informed about the responder's rejection as a form of *voice*.[14]

We utilize the strategy method when eliciting proposer and responder choices, i.e., participants make decisions for all situations they could face. This allows us to compare the allocation behavior of those who become proposer and those who do not. For the role of the responder, we elicit acceptance thresholds which are then implemented conditionally on the first stage offer. A special feature of our game is that not all three group members can become the proposer. In particular, at the beginning of the experiment, we randomly match three players into a group and assign them to one of the three roles A, B, and C,[15] which remain constant during the whole interaction. Participants in role A and B compete to become the proposer of the impunity game. We refer to this role as the *allocator* from now on. The participant in role C cannot become *allocator* and always takes the role of a responder, but may select the *allocator*. Before doing so, she can access the information revealed by players A and B. The next section explains this endogenous disclosure procedure.

Endogenous information disclosure

After reading the instructions of the second part of the experiment, participants are aware of the allocation task and the opportunity to reveal information in this setting. Participants in role A and B can decide which answers from the questionnaire they want to reveal to player C. For each information revealed, subjects have to pay a small fee of 10 Cents which is subtracted from their lump-sum payoff of 3 Euro from the first part.[16] Keeping information secret is possible at no cost. The small fee mimics transaction costs of personal information disclosure. For example, extending one's online profile requires a small

[14]In order to render voice meaningful, the proposer has to offer at least 1 Euro to every participant including herself. As a consequence, a rejection inevitably causes a loss for the rejecting responder. We refrain from payoff-relevant punishment to avoid that varying beliefs regarding responder behavior between the different treatments drive proposers' choices.

[15]In order to avoid ordinal ordering inherent in the letters A, B, and C, we use the colors red, blue, and green during the experiment.

[16]Revelation costs are adopted from Benndorf et al. (2015).

amount of time and effort, which increases the more features you fill in. Methodologically, it limits experimenter demand concerns of asking for information provision in such a setting. Finding different information revelation patterns under disclosure costs would therefore strengthen our results. The information disclosure decision is our main variable of interest in this paper. In particular, we focus on the total number of disclosures, stating corresponding hypotheses at the end of this section.

Additional measures

Due to our interest in potential side effects of information disclosure, we elicit the participants' perceived pressure to disclose information right after they have made their final disclosure choice. Particularly, we ask them "Did you feel compelled to reveal more information than you initially wanted to?", measured on a 7-point scale.[17] Moreover, considerable heterogeneity in disclosure behavior may exist in such a setting and may impact game behavior. This heterogeneity is likely to stem from differences in privacy concerns, which we measure post-experimentally based on Westin's privacy index, as in Harris Interactive (2001),[18] and based on social media activity measures, the latter taken from Frik and Gaudeul (2016).

Since the decision to disclose might also hinge on the perceived relevance of a question to predict behavior in the subsequent allocation task and the associated discomfort, we elicit these factors for each question on a 7-point scale in the post-experimental questionnaire. We consider an information as relevant or unpleasant, respectively, if the participant selects at least a value of 4 on the 7-point scale. The sum of answers, whose

[17]While such survey measures rely on self-reported perceptions that may be different from behavioral decision data, psychologists suggest that self-reports are the best way to measure subjective emotions (Robinson and Clore 2002). This approach has also been adopted by economists. See, for example, Alesina et al. (2004), Blanchflower and Oswald (2004), Brandts et al. (2009), Charness and Grosskopf (2001), and Reyniers and Bhalla (2013).

[18]In line with the 7-point scales we use for all other ordinal ratings, we also use a 7-point instead of a 4-point scale for the three questions determining the Westin privacy index. These questions stem from the 2001 version of Westin's privacy classification, as published in Harris Interactive (2001). See Kumaraguru and Cranor (2005) for a review of Westin's privacy indexes. We classify scores from 1 to 3 as "disagree" and 5 to 7 as "agree", and follow Westin's definition of the three privacy types *Unconcerned, Fundamentalist*, and *Pragmatists* based on those definitions: *Fundamentalists* agree that consumers have lost control over personal information, do not believe that companies handle their data in an appropriate way, and question existing privacy laws. The *Unconcerned* make the opposite statements, while *Pragmatists* hold mixed opinions.

disclosure is perceived as unpleasant, serves as a second indicator of potential side effects of information disclose. Moreover, since our experiment involves peer comparison, we use a 7-item version of the INCOM social comparison index (Schneider and Schupp 2011) in order to control for heterogeneity in the habit of comparing oneself with others. On top of that, we elicit beliefs regarding the competitor's answer score and disclosure decisions in an incentive-compatible way. In particular, subjects receive a bonus of 3.50€ and 0.50€, respectively, at the end of the experiment if they correctly guessed the other candidate's answer and disclosure decision.[19]

Treatments

Table 3.2: Treatments: Two-by-two factorial design

		Allocator choice	
		random	strategic (by C)
Peer comparison	No	RA	SA
	Yes	RAC	SAC

The experimental design consists of four treatments based on a two-by-two factorial design, which vary in how information is revealed and how the *allocator* is selected. The first dimension distinguishes how the *allocator* is determined and is adapted from Brandts et al. (2006). In *random* treatments, one of the subjects in role A or B is randomly chosen with equal probability to become *allocator*. In *strategic* treatments, C decides whether A or B becomes *allocator*. Obviously, the two conditions differ in their incentives to provide information to C. In *random*, there should be no reason to disclose any information beyond one's genuine preference for information sharing. In contrast, information sharing can serve a strategic purpose in *strategic* because it may raise one's chance to become the payoff-determining *allocator*, creating a situation of proposer competition (Roth et al. 1991).[20] Consequently, varying the selection procedure allows us to distinguish non-

[19]Given the different chances of a correct answer guess on a 7-point scale and a correct disclosure guess on a 2-point scale, i.e., disclose or non-disclose, we determine bonuses to be equal in expectation, setting them to 3.50 Euro for a correct answer guess and 1 Euro for a correct disclosure guess. One guess is randomly chosen and evaluated for payoff at the end.

[20]If there is an intrinsic value of decision rights, as in Bartling et al. (2014), this effect may also be captured in the *strategic* term.

strategic information disclosure, i.e., one's baseline sharing preference, from strategic information sharing which is triggered by the monetary incentive.

The second dimension of our two-by-two factorial design varies whether there is a social *comparison* stage or not before information is revealed to C. This allows us to investigate how peer pressure affects the willingness to disclose information. In the comparison stage, participants learn which answers the other player competing for *allocator* power disclosed, but not the exact score of the answers. Players A and B can adjust their revelation choice, or simply reconfirm their previous one. The previous choice is preselected as the default on the screen, so that for maintaining the previous choice participants just have to click on "proceed".[21] If a subject wants to adjust her previous choice, she can do so by changing the preselected disclosure decisions from "no" to "yes" or vice versa. As in the initial disclosure stage, the change in revelation can be made for each question separately and costs 10 Cents per disclosure.[22]

We denote the four treatments resulting from our two-by-two design by *random* (RA), *random-comparison* (RAC), *strategic* (SA), and *strategic-comparison* (SAC). In what follows, we discuss how the different levels of strategic and social impact inherent in these treatments may affect information disclosure and perceived pressure to disclose. We refer to the initial disclosure choice before the peer comparison stage as "ex ante" disclosure and to the subsequent one as "ex post" disclosure, respectively.

Data collection

Data were collected in the Cologne Laboratory for Economic Research in November and December 2017 using zTree (Fischbacher 2007) for programming and ORSEE (Greiner 2015) for participant recruitment. The experiment lasted approximately 50 minutes and participants earned on average 13€, including a show-up fee of 4€. In total, 294 people

[21]In order to ensure comparability between treatments, participants in the treatments without comparison also see another screen, but with only their own choices displayed. Here, they just have to click on "proceed" to continue. In principle, they can also adjust their choices, but there should be no straightforward reason to do so, except that the belief elicitation tasks in between resulted in some deeper thoughts about how much to disclose.

[22]Note that the 10-Cent transaction costs are not reimbursed if a subject decides to hide an answer she disclosed before.

participated in 10 experimental sessions. We oversampled the *strategic* treatments due to our interest in active *allocator* selection by C-participants.

Hypotheses

Allocator selection by C in the *strategic* treatments SA and SAC likely incentivizes individuals to disclose ex ante more information than in the *random* treatments RA and RAC. While *random* selection of the *allocator* elicits one's intrinsic preference for information revelation without additional incentives, the prospect of gaining *allocator* power might seem worth to sacrifice some privacy. This corresponds to incurring a cost, likely in form of privacy costs, worth to be paid in exchange for the strategically beneficial position.

Hypothesis 1 (Strategic disclosure): The amount of information revealed ex ante is higher in *strategic* treatments than in *random* treatments.

Subsequent peer *comparison* likely initiates adaptation to the disclosure behavior of the competitor. Changes in RAC can be fully attributed to a classical peer effect, while changes in SAC are further triggered by competition in gaining the attention of player C via revealing more. Therefore, we expect more disclosure changes in SAC, and in particular more upward changes due to its *strategic* aspect.[23]

Hypothesis 2 (Social comparison): Peer *comparison* leads to more ex post disclosure changes under *strategic* incentives than without.

Reactions to peer *comparison* under *strategic* incentives are likely driven by one's own ex ante disclosure choices relative to that of the competitor, and may thus be heterogeneous. In particular, we expect that those who learn that they revealed fewer answers than their competitor under *strategic* benefits adapt their initial disclosure choice and disclose more. Reyniers and Bhalla (2013) find such an effect in the context of charitable donations, i.e., under peer comparison those who initially attempted to donate less revise their choice upwards. Such a reaction is even more likely to occur in our setting since

[23]The fact that *comparison* in SAC inherently provides information regarding how much disclosure may be necessary to capture distributional benefits may even emphasize this reaction. Although downward corrections are also possible, e.g., after initially overestimating the other's disclosure, we do not expect that peer *comparison* initiates much hiding of information in SAC.

the incentive to adapt is not only driven by soft factors like image concerns, but also by expected monetary benefits in SAC.

Hypothesis 3 (Heterogeneous effects): Those who disclose less ex ante in SAC react to peer *comparison* and adapt their disclosure decision.

So far, we have focused on the effect of strategic incentives and social comparison on information disclosure. If subjects change their initial level of disclosure in SAC after peer *comparison*, this can be driven both by an updated belief about the right amount of information to disclose or by social pressure.[24] In order to investigate the aspect of social pressure, we asked participants: "Did you feel compelled to reveal more information than you initially wanted to?" right after they made their ex post revelation decision. Perceived pressure should play a role in *strategic* treatments due to their competitive nature (Brandts et al. 2009), and should be especially strong when paired with peer *comparison* in SAC (DellaVigna et al. 2012, 2017). Regarding heterogeneity, we expect the increase in pressure in SAC to be driven by the initially disclosure-unwilling, who learn that they lag behind in revelation competition.

Hypothesis 4 (Pressure to disclose): Perceived pressure to disclose information increases a) in *strategic* compared to *random* treatments, b) even more so in combination with social *comparison* in SAC, and in this case c) driven by those learning to be the one disclosing less.

Besides this potential cost, personal information disclosure might also create benefits. Previous research both in experimental settings (Bohnet and Frey 1999; Brandts et al. 2006; Charness and Gneezy 2008; Eckel and Petrie 2011; Gaudeul and Giannetti 2017; Hermstrüwer and Dickert 2017; Holm and Samahita 2018), as well as on microfinance platforms (Böhme and Pötzsch 2010; Ge et al. 2017; Michels 2012; Pope and Sydnor 2011), provides evidence that adding soft, personal information can beneficially influence outcomes. If information overbidding is a way to compete for attention, the extent of personal information sharing likely affects *allocator* selection in the *strategic* treatments

[24] See Section 3.5 for a discussion.

of our experiment. Particularly, those individuals who disclose more information should be more likely selected as *allocators*.

Hypothesis 5 (Beneficial information overbidding): Participants who reveal more information in *strategic* treatments are more likely selected as *allocators*.

We refrain from stating explicit hypotheses regarding the influence of sharing on caring, i.e., from information disclosure on generosity in impunity play since field evidence for such a relationship is mixed (Duarte et al. 2012; Iyer et al. 2016; Pope and Sydnor 2011), and it is not the focus of our project.

3.4 Results

3.4.1 Descriptive statistics

Table 3.3 shows descriptive statistics of our sample. Participants are 55.8% female and on average 24.3 years old. With reference to Westin's privacy index, our sample is roughly split into two halves, privacy "pragmatists" and "fundamentalists". Hardly anyone is classified as "unconcerned".[25] Except for age, statistical tests do not reveal any differences between treatment groups in terms of demographics, privacy preferences, and social media behavior. Descriptive statistics regarding outcome variables for the restricted sample of *allocator* candidates (role A and B) are summarized in Table 3.A.1 in the Appendix.

3.4.2 Answers ex ante disclosed

First, we analyze the aggregated amount of information disclosed ex ante, i.e., before social *comparison*. Hypothesis 1 predicts more disclosure in *strategic* treatments. Indeed, participants react to the *strategic* setting with more information revelation. Compared to *random*, information revelation doubles from 1.9 to 3.8 answers on average in the *strategic* context. Figure 3.2 depicts the distribution of the number of answers disclosed. In *random*, more than half of the participants disclose nothing, while only 12.9% do so in *strategic*. Instead, the majority of 46.6% of observations in the latter treatment falls in the

[25] Therefore, we pool pragmatists and unconcerned subjects in the subsequent analyses and only use a dummy for fundamentalists.

Table 3.3: Descriptive statistics: Sample characteristics

	Total	RA	RAC	SA	SAC	p-value
Female	**55.8%**	48.3%	65.0%	51.7%	58.6%	0.232
Age	**24.3**	25.9	23.5	24.3	23.8	0.045
Westin Fundamentalist	**51.0%**	43.3%	53.3%	54.0%	51.7%	0.606
Westin Pragmatist	**47.6%**	56.7%	45.0%	43.7%	47.1%	0.456
Westin Unconcerned	**1.4%**	0.0%	1.7%	2.3%	1.1%	0.911
Profile public	**15.0%**	15.0%	16.7%	16.1%	12.6%	0.905
Profile identifiable	**69.4%**	61.7%	73.3%	67.8%	73.6%	0.415
Ability compare	**4.4**	4.3	4.5	4.5	4.3	0.394
Opinion compare	**4.8**	4.6	5.0	5.0	4.7	0.209
N	**294**	60	60	87	87	

Notes: p-values in last column show accuracy of randomization into treatments based on individual characteristics, and stem from Kruskal-Wallis-tests for age, ability compare, and opinion compare, and from Fisher's exact tests otherwise.

range between two and four revelations. A Wilcoxon ranksum-test confirms that the two distributions are statistically different form each other ($p < 0.001$). While the answers that participants give are themselves obviously meaningful for disclosure, the focus of our analysis is not which particular information participants are willing to disclose, but how incentives and social comparison affect information disclosure in general. Therefore, the analysis on the answer level is left to the interested reader in the Appendix.[26]

As a general empirical strategy in this paper, we estimate the effect of our treatment dimension, *strategic* incentives, social *comparison*, and their interaction, on different outcomes y_i, i.e.,

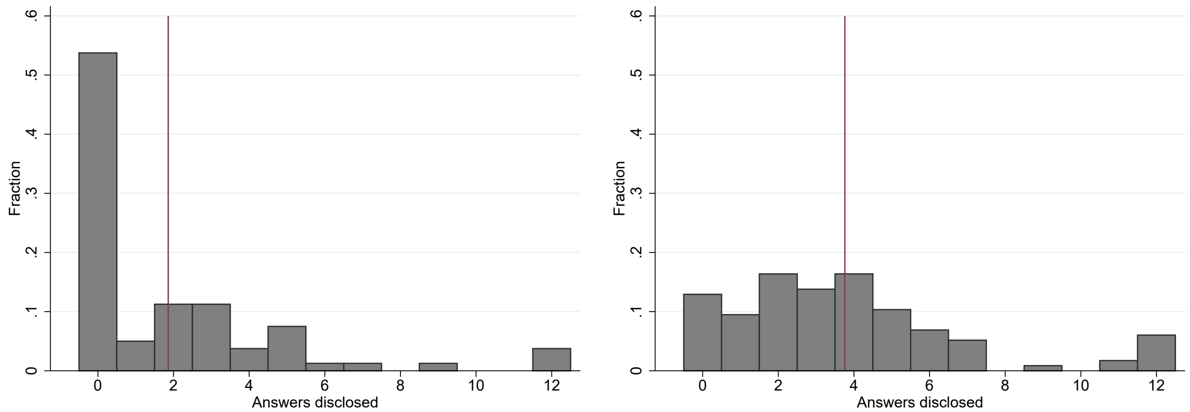
$$y_i = \beta_0 + \beta_1 strategic_i + \beta_2 comparison_i + \beta_3 strategic_i * comparison_i + \beta' X_i + \epsilon_i \quad (1)$$

in which X_i is a vector of individual characteristics of individual i, and ϵ_i denotes an error term clustered at group level. We are interested in β_1, β_1, and β_3 capturing the effect of *strategic* incentives, social *comparison*, and the differential effect of social *comparison* in *strategic* settings, respectively.

[26]In the Appendix, we provide histograms of the content of answers in Figure 3.A.1, and Table 3.A.2 reports probit regression results on factors affecting disclosure on question level.

Figure 3.2: Histograms of answers ex ante disclosed

(A) Random (B) Strategic



Notes: Vertical lines represent means.

Table 3.4 reports corresponding OLS regression results with reference to the number of answers ex ante disclosed by *allocator* candidates. The effect of the *strategic* incentive to reveal more information is statistically significant at the 1% level, as already suggested by the descriptive analysis. Participants in *strategic* disclose on average 1.9 answers more. At this stage, peer *comparison* has not yet taken place, so insignificant effects of the *comparison* coefficient and its interaction with *strategic* in column (2) confirm that there are no initial differences between groups with and without subsequent feedback.

As controls, age and gender, as well as nine dummy variables for the ten random orders of questions, are added in column (3). In general, women disclose significantly less than men, quantitatively about one answer less on average. Moreover, to capture attitudes that are relevant for our setting, column (4) adds control variables for privacy concerns via a dummy for Westin's privacy fundamentalists, the two dimensions ability and opinion compare of the INCOM social comparison index, and two dummy variables capturing identifiability of the participant's social media profile and strangers' access to it. All specifications confirm that strategic incentives enhance information disclosure and thus Hypothesis 1. In the Appendix, we show that controlling for the many zero disclosures, which occur particularly in *random* treatments, by a tobit model even strengthens our results. Moreover, results are robust to a 90% winsorization on treatment level.

<u>Result 1</u>: More information is revealed ex ante in *strategic* than in *random* treatments.

Table 3.4: Effect of strategic incentives on ex ante disclosure

	Answers ex ante disclosed			
	(1)	(2)	(3)	(4)
strategic	1.913***	1.927***	1.974***	2.106***
	(0.432)	(0.663)	(0.652)	(0.670)
comparison		0.475	0.669	0.700
		(0.633)	(0.672)	(0.669)
strategic # comparison		-0.027	-0.203	-0.244
		(0.864)	(0.843)	(0.858)
constant	1.862***	1.625***	2.071	0.971
	(0.317)	(0.501)	(1.457)	(1.740)
basic controls	No	No	Yes	Yes
preference controls	No	No	No	Yes
N	196	196	196	196
R2	0.091	0.096	0.171	0.185

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

3.4.3 Ex post disclosure changes

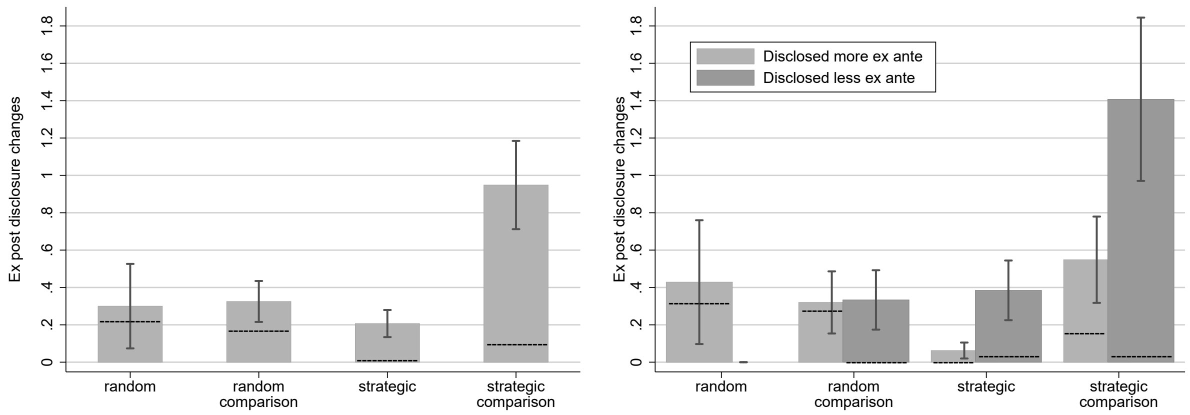
We now investigate how social comparison affects disclosure behavior. After the initial disclosure stage and a belief elicitation task, subjects can revise their disclosure choice. Without prior announcement, participants in *comparison* treatments learn the disclosure choice of the other *allocator* candidate. Particularly, they learn which answers their competitor disclosed, but not the content of answers, and can revise their choices. In order to maintain comparability between treatments with and without *comparison*, subjects can also revise their disclosure choice when not receiving feedback on their competitor's behavior. As the dependent variable, we focus on the absolute amount of disclosure changes independent of their direction.[27] A disclosure change is measured as a different disclosure choice ex post than ex ante, i.e., $x^{ex\,ante} \neq x^{ex\,post}$. We sum up these sin-

[27]One would miss important changes when only measuring the amount of information disclosed ex ante and ex post: It would overlook inverse changes like "subsequently disclose answer x, but hide answer y", which would be reported as zero, but might indicate adaptation to the other's disclosure.

gle disclosure changes for all twelve answers to derive our outcome variable of interest, $\sum_{n=1}^{12} |x_n^{ex\,ante} - x_n^{ex\,post}|_i$, which can range from 0 to 12. Hypothesis 2 predicts that social *comparison* has a stronger effect under *strategic* incentives in SAC than in RAC without.

Figure 3.3: Coefficient plots of ex post disclosure changes by treatment

(A) Disclosure changes overall (B) Disclosure changes by ex ante disclosure



Notes: Vertical lines represent standard errors clustered at group level. Horizontal lines divide ex post disclosure changes into upward and downward changes depicted above and below the line, respectively.

Panel A of Figure 3.3 depicts ex post disclosure changes by treatment as a coefficient plot based on OLS regression. The horizontal line separating the bars in an upper and a lower part distinguishes the direction of the changes. The fractions below the line are disclosure reductions, while extensions are depicted above. We observe a small number of ex post disclosure changes in treatments without peer comparisons, probably as a reaction to intermediate belief elicitation. Compared to the baseline level of changes in RA, there are not more ex post changes in RAC after social *comparison*. However, substantial changes occur when combining social *comparison* with *strategic* incentives to disclose.

Table 3.5 reports the corresponding regression results, as specified in Equation (1), with ex post disclosure changes as the dependent variable. The *strategic-comparison* interaction effect in column (1) is statistically significantly positive at the 5% level. This means that participants who face strategic benefits react to peer comparison. The interaction effect of *strategic-comparison* equals at least 0.72 disclosure changes, and remains significant independent of the control variables included in columns (2)-(4). By adding up the three coefficients of interest, a stable effect size of 0.65 disclosure changes emerges

Table 3.5: Ex post disclosure changes by treatment

	Ex post disclosure changes					
	(1)	(2)	(3)	(4)	(5) high	(6) low
strategic	-0.093	-0.105	-0.132	-0.284	-0.351	0.143
	(0.236)	(0.219)	(0.220)	(0.278)	(0.294)	(0.361)
comparison	0.025	0.019	-0.045	-0.086	-0.089	-0.004
	(0.250)	(0.230)	(0.237)	(0.247)	(0.340)	(0.414)
strategic # comparison	0.716**	0.718**	0.824**	0.824**	0.610	1.258*
	(0.350)	(0.344)	(0.362)	(0.358)	(0.431)	(0.728)
own ex ante disclosure				0.082		
				(0.062)		
own - other's ex ante disclosure				-0.061		
				(0.047)		
constant	0.300	0.676	-0.106	-0.332	-0.348	-0.526
	(0.225)	(0.530)	(0.477)	(0.475)	(0.573)	(0.938)
basic controls	No	Yes	Yes	Yes	Yes	Yes
preference controls	No	No	Yes	Yes	Yes	Yes
N	196	196	196	196	116	80
R2	0.058	0.081	0.131	0.150	0.205	0.262

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. The lower candidate is the one who disclosed strictly fewer answers ex ante than her competitor.

for treatment SAC in addition to the 0.3 baseline level of changes in RA. In total, this equals nearly one absolute disclosure change in SAC on average. In contrast, the *comparison* variable is weak and insignificant, and implies that social *comparison* per se does not overcome one's intrinsic preference for privacy, including a potential reluctance to disclose personal details.

Controlling for privacy- and social comparison-related factors in column (3) increases the size of the *strategic-comparison* interaction effect. Interestingly, participants who score higher on the ability dimension of the INCOM social comparison index, i.e., those who often compare their own ability with others, make significantly more disclosure changes (p = 0.028). In column (4), we additionally control for one's own ex ante disclosure, i.e., the absolute disclosure level, and for the disclosure difference to the competitor, i.e., the relative disclosure. Both factors do not significantly affect adaptation behavior, and leave our results unchanged. The same holds when performing a 90% winsorization on treatment level as a robustness check, which can be found in Table 3.A.4 of the Appendix. Consequently, the interplay between incentives and social comparison seems crucial for adapting one's personal information disclosure. This confirms Hypothesis 2.

Result 2: Peer *comparison* induces significantly more ex post disclosure changes under *strategic* incentives than without.

In order to better understand ex post disclosure changes, we also investigate the relevance and direction of ex post disclosure changes. First, is the information used in our experiment relevant, i.e., perceived as meaningful for *allocator* selection and allocation behavior? In columns (1) and (2) of Table 3.A.5 in the Appendix, we only consider ex post disclosure changes of answers which player C considers as relevant indicators for game behavior. Results with and without additional controls reveal a similar picture as in the main analysis. The *strategic-comparison* interaction term is significant, suggesting that peer comparison indeed fosters the disclosure of more relevant answers.

Second, we examine the direction of disclosure changes, which can be inferred from Figure 3.3 by looking at the horizontal division lines of the bars. Moreover, we analyze whether a change mimics the disclosure decision of the competitor. The *strategic-comparison* interaction effect is significant for disclosure extensions and for adaptations to the disclosure choice of the other. In SAC in particular, 89.1% of all changes are upward changes, and 85.5% are adaptations. For a detailed analysis and corresponding regression results, see Table 3.A.6 in the Appendix. Two important aspects prevail: First, we follow peers in what we disclose, which can be regarded as an intensive margin and fits to conformity seeking (Asch 1951; Bernheim 1994). One wants to avoid deviating from the disclosure choice of the other and therefore adapts to her revelation behavior. Second, the primary direction of change with both peer *comparison* and *strategic* incentives is upwards, which resembles an extensive margin.

Regarding heterogeneity in ex post disclosure changes, Hypothesis 3 predicts that the observed changes in SAC are driven by those learning to lag behind. This turns out to be true when splitting our sample into two subgroups based on the criterion whether an individual is the one who discloses more or strictly less information ex ante than the other.[28] Looking at the corresponding coefficient plots for these subsamples depicted in Panel B of Figure 3.3 shows that the bar for the lower candidate in the SAC treatment is by far the highest. In this condition, subjects make on average 1.4 ex post disclosure changes, of which 97.4% are extensions and 84.2% adaptation mimicking the disclosure behavior of the competitor. In columns (5) and (6) of Table 3.5, we run the previous disclosure change regressions separately for the two subgroups.[29] The *strategic-comparison* interaction effect turns out to be significant for lower candidates. Those who learn that they disclose more information ex ante do not see the need to react to peer comparison, while those who realize that they lag behind revise their ex ante disclosure choice. This supports the idea that, with peer comparison and competition in information disclosure, those generally unwilling to disclose adapt their behavior to their environment. However, by splitting the sample, one loses statistical power in the regression analysis, resulting in significance only at the 10% level to substantiate Hypothesis 3.

<u>Result 3</u>: Subjects who disclose less ex ante under *strategic* incentives and peer *comparison* alter their disclosure choice.

One interesting additional observation when looking at Panel B in Figure 3.3 is that there are some changes going on under peer *comparison* even without *strategic* incentives to disclose. These changes are bi-directional. Some subjects in RAC, who learn that they disclosed more, reveal less information, which can be inferred from most ex post disclosure changes of higher candidates lying below the horizontal line in Panel B in Figure 3.3. In contrast, lower candidates expand their disclosure, so both groups converge to each other.

[28]Since we call a group member the "lower" candidate only if she reveals strictly less information ex ante, and therefore assign a value of zero to the dummy if both candidates in a pair disclose the same amount of information ex ante, we have more "high" than "low" candidates.

[29]Since our two-by-two design already requires an interaction regression when analyzing the full sample, a further interaction for heterogeneous groups would result in triple interaction. In order to maintain interpretability of results, we use sample splits instead.

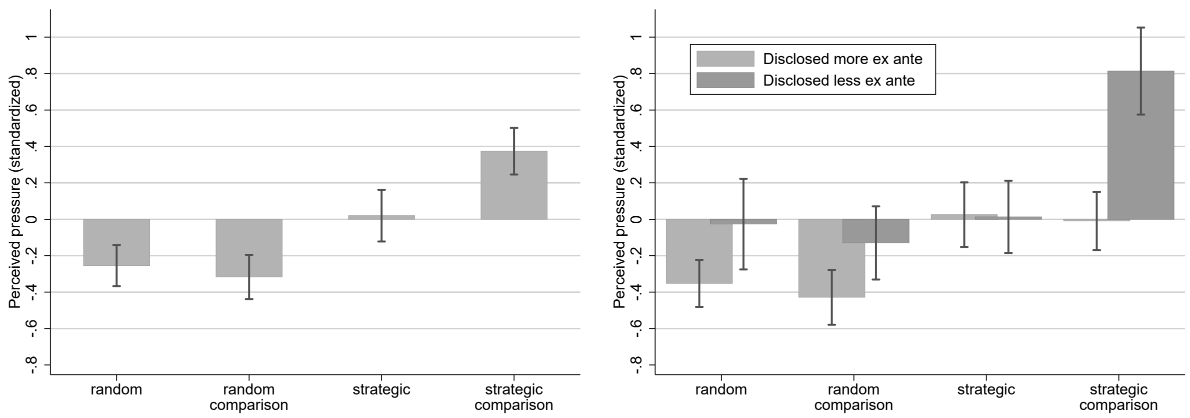
On the contrary, in SAC, even if one is already ahead, one more likely reacts by disclosing more rather than less. This supports the idea that incentives for personal information sharing push the extensive margin of disclosure up.

3.4.4 Hidden cost of information disclosure

Are there potential side effects of extensive personal information disclosure, in particular pressure to disclose? In SAC, particularly those who reveal less ex ante widen their disclosure due to peer comparison. Therefore, we explore whether peer comparison results from peer pressure by analyzing answers to the question "Did you feel compelled to reveal more information than you initially wanted to?", elicited right after participants' ex post disclosure choice. Panel A in Figure 3.4 shows the coefficient plot of the level of perceived pressure measured in standard deviations for the four treatments. In *random* treatments, the level of pressure with and without *comparison* is similarly low and lies between -0.32 and -0.25 standard deviations. The corresponding *comparison* regression coefficient, distinguishing the pure effect of social comparison, is insignificant and small in magnitude in all regression specifications displayed in columns (1)-(3) of Table 3.6. Thus, social comparison per se does not seem to trigger pressure to share information.

Figure 3.4: Coefficient plot of perceived pressure to disclose by treatment

(A) Perceived pressure overall (B) Perceived pressure by ex ante disclosure



Notes: Vertical lines represent standard errors clustered at group level.

However, the combination of peer comparison and incentives seems to render information sharing compelling. We find that the interaction of *strategic* incentives and peer *comparison* increases perceived pressure by 0.42 to 0.48 standard deviations, depending on the specification. Although it is statistically significant only if controlling for other factors, it is large in magnitude. If we winsorize the data by 90% on treatment level, shown in Table 3.A.14 of the Appendix, this finding is robust and becomes significant at the 10% level already without any controls. This provides directional evidence in line with Hypothesis 4b. However, we do not observe a statistically significant increase in pressure from *strategic* incentives alone. The level of pressure in the SA treatment equals the average level in our sample, and is not significantly higher than in the RA treatment on conventional levels ($p = 0.131$). Unlike Brandts et al. (2009), we cannot confirm that competition per se has detrimental effects, and cannot confirm Hypothesis 4a.

Table 3.6: Perceived pressure to disclose information by treatment

	Perceived pressure				
	(1)	(2)	(3)	(4) high	(5) low
strategic	0.274	0.250	0.254	0.379**	-0.142
	(0.180)	(0.183)	(0.175)	(0.191)	(0.345)
comparison	-0.062	-0.078	-0.089	-0.066	-0.201
	(0.165)	(0.171)	(0.169)	(0.205)	(0.342)
strategic # comparison	0.416	0.433*	0.475*	0.132	1.117**
	(0.252)	(0.252)	(0.246)	(0.292)	(0.468)
constant	-0.254**	0.105	-0.373	-0.696	0.355
	(0.112)	(0.330)	(0.472)	(0.714)	(0.797)
basic controls	No	Yes	Yes	Yes	Yes
preference controls	No	No	Yes	Yes	Yes
N	196	196	196	116	80
R2	0.076	0.081	0.113	0.202	0.202

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Basic controls include gender and age. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. The lower candidate is the one who disclosed strictly fewer answers ex ante than her competitor.

We further look at heterogeneity in perceived pressure when being the one disclosing less ex ante. Hypothesis 4c postulates that perceived pressure is comparably high for those individuals. Panel B in Figure 3.4 shows the corresponding coefficient plot split by who in the pair discloses less ex ante. In *random* treatments, those who disclose more and do so without incentives feel the least compelled. Their information disclosure decision seems to be intrinsically motivated and free from pressure. Similarly to the effect for ex post disclosure changes in Panel B of Figure 3.3, participants realizing in SAC to have disclosed less ex ante feel most pressured. The effect size is with 0.81 standard deviations large in magnitude compared to the standardized average of zero. Running separate regressions for candidates with ex ante higher or lower disclosure in a pair, the *strategic-comparison* interaction effect in the low candidate subsample in column (5) of Table 3.6 is significant at the 5% level. Therefore, we infer from our results that observing to lag behind in personal information revelation under competition is perceived as more compelling. This supports Hypothesis 4c.[30]

Result 4: Perceived pressure to disclose information increases under peer *comparison* in combination with *strategic* incentives, especially when learning to have disclosed less ex ante, but not under *strategic* incentives in general.

As a second indicator for hidden cost of information disclosure, we additionally focus on the disclosure of information, whose revelation participants consider as unpleasant. Their correlation with all ex post disclosure changes, as well as with relevant disclosure changes, is quite high, at 0.663 and 0.560, respectively. In accordance, regression results in columns (3) and (4) of Table 3.A.5 in the Appendix uncover that the combination of strategic incentives and peer comparison causes unpleasant disclosure changes. The *strategic-comparison* interaction effect is significant at the 10% and 5% level when not including or including controls, respectively. This means that peer comparison and strategic incentives come along with another side effect: They trigger the revelation of information which participants do not like to disclose. Hence, while disclosure changes may have a beneficial effect on outcomes via making more relevant information available, they also come along at the cost of causing discomfort.

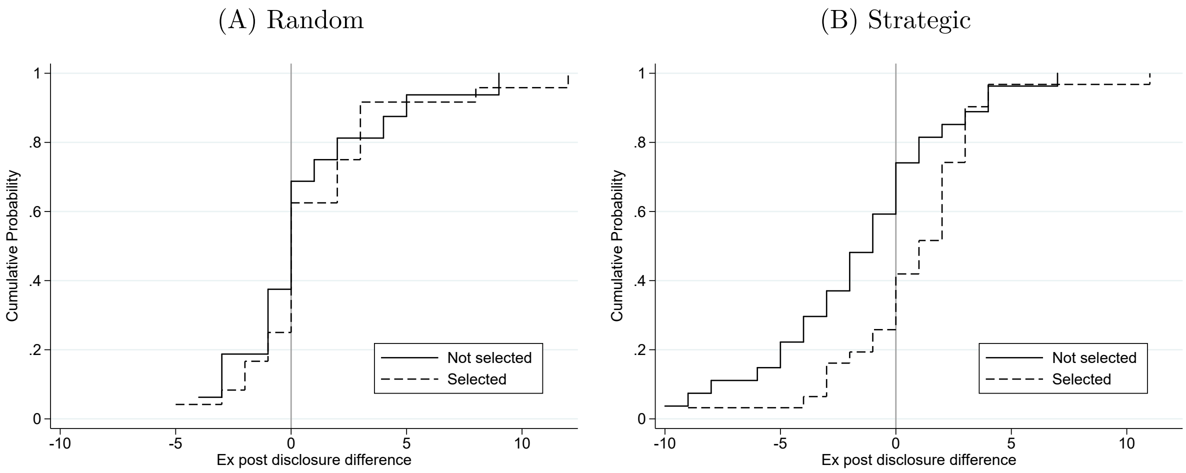
[30] Note that the *strategic* coefficient turns significant in the restricted sample of candidates being ahead in column (4), indicating that pressure may also be caused by *strategic* incentives per se. However, we did not have any hypothesis regarding heterogeneous reactions for initially more disclosure-willing subjects, and we therefore refrain from further interpreting this result.

3.4.5 Effects of information disclosure on outcomes

<u>*Allocator* selection based on information disclosure</u>

In this section, we briefly analyze how personal information disclosure affects the probability to become the *allocator*. *Allocator* candidates seem to assume that C takes personal information into account for *allocator* selection since they disclose more information in *strategic* treatments. Indeed, participants in role C look at the information provided. On average, they investigate 10.5 out of 12 answers disclosed ex post, and in 79.6% of the cases all answers.[31]

Figure 3.5: Probability of becoming *allocator* by difference in information disclosure



Notes: Cumulative probability functions of becoming *allocator*.

Panel B of Figure 3.5 illustrates that disclosing more information than the other candidate indeed increases the likelihood to become *allocator* in *strategic* treatments.[32] The curves present the cumulative probability distribution of being selected, conditional on the ex post difference in answers disclosed relative to one's competitor. In Panel B, the line for non-selected candidates is shifted to the left, meaning that their probability of *not* being selected is higher the more they lag behind. Over a large range of the abscissa, there is first-order stochastic dominance between the two lines. While 25.5% of *allocators* stem from the group of participants disclosing less, suggesting that what has

[31]In a ranksum-test, the number of answers inspected by C is with 9.9 clicks insignificantly smaller in *random* than in *strategic* treatments with 10.8 clicks ($p = 0.274$).

[32]Since C is not informed about the comparison stage, we can ignore the *comparison* dimension and pool observations.

been disclosed also matters for selection, disclosing more seems highly decisive to become *allocator*. In fact, a two-sample Kolmogorov-Smirnov test rejects the hypothesis that the difference in ex post disclosures between selected and non-selected *allocators* is the same ($p = 0.004$). There is no such difference in treatments with *random allocator* selection displayed in Panel A of Figure 3.5 ($p = 0.988$). In sum, this suggests that the pure amount of personal information sharing can impact how much attention one receives from others, so engaging in competition via information overbidding seems to pay off for disclosure-willing individuals. When restricting our analysis to information which C considers as relevant indicators for allocation behavior, the effect that more sharing raises the likelihood of being selected increases.[33] The corresponding probit regression analysis in Table 3.A.9 of the Appendix confirms these findings and substantiates Hypothesis 5.

Result 5: Disclosing more information significantly increases the probability of being selected as *allocator* in *strategic* treatments.

As a corollary to this finding, it is worth pointing out that participants who disclose less ex ante also most often disclose less ex post and are therefore less likely to become *allocator* in SAC. In spite of the opportunity to catch up, they fail to become *allocator* in 74.1% of the cases, which is statistically different from a 50% chance in a two-sided binomial test ($p = 0.019$) and indistinguishable from the corresponding chance without *comparison* in SA in a ranksum-test ($p = 0.698$). Thus, for this group, ex post disclosure changes do not pay off.

Allocation behavior

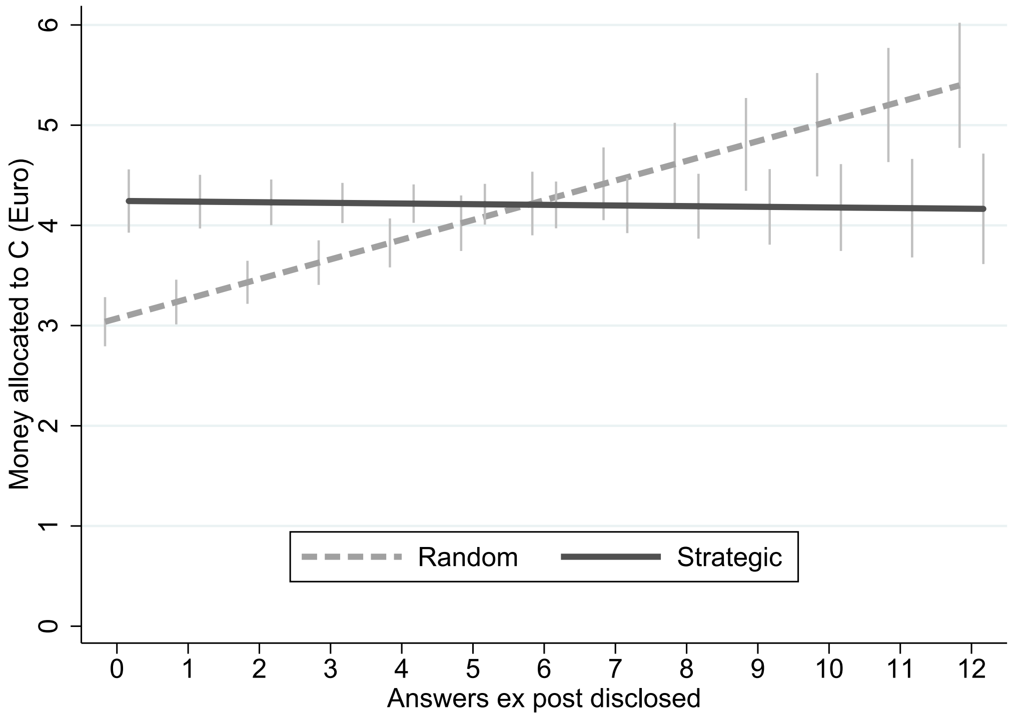
While revealing more information is beneficial for becoming the *allocator*, is this role also beneficial in terms of payoff? OLS regressions in Table 3.A.10 in the Appendix confirm that this is true. When dividing the 17€ pie themselves, subjects earn almost three times as much as they would receive from their matched competitor. Instead of the

[33] This additionally confirms that the anonymous information in our experiment is indeed perceived as relevant personal information in this context.

average payoff of 3.40€ from their competitor, they keep 9.73€ for themselves. Hence, becoming an *allocator* is highly beneficial in terms of payoff.

Is *allocator* selection based on information disclosure competition also beneficial for the other market side, i.e., players C selecting the *allocator*? In order to explore this, we look at allocation behavior conditional on personal information sharing. We measure prosocial behavior by the amount one gives to C. We summarize the main effects here and refer to Table 3.A.11 in the Appendix for a more detailed analysis.

Figure 3.6: Coefficient plot of money allocated to C by information disclosure and selection



Notes: Vertical lines represent standard errors clustered at group level.

First of all, and in line with Brandts et al. (2006),[34] participants in *strategic* treatments give significantly more to C and thereby reciprocate the favor of being selected. This represents the level effect in the coefficient plot in Figure 3.6. Second, there seems to be a positive relationship between intrinsic disclosure-willingness and generosity in *random* treatments.[35] This can be inferred from the slope in Figure 3.6. However, this

[34] Brandts et al. (2006) call the effect that selected participants give more to the selecting party than randomly chosen participants the "I-want-you" effect. We confirm its existence in a modified setting.

[35] However, we do not want to emphasize this result too much since the number of observations with many disclosures in *random* treatments is limited.

positive relationship is distorted by revelation competition in *strategic* treatments. Subjects disclosing more personal information in *strategic* treatments do not offer more to player C. Hence, there is no *direct* effect of disclosure behavior on prosocial behavior in treatments with *allocator* selection. In the corresponding OLS regression specification in Table 3.A.11 in the Appendix, this means that the significant effects of the amount disclosed ex post and its interaction with the *strategic* coefficient cancel out. Third, there is no *indirect* effect on prosocial behavior from being chosen as an *allocator* as a result of one's disclosure. Those who become *allocators* in *strategic* treatments do not offer significantly more to player C than non-selected candidates. Thus, participants in role C do not suffice in picking the more generous candidates based on endogenously disclosed personal information. Statistical support based on OLS regression results for all the findings discussed above can be found in Table 3.A.11 in the Appendix.

While there is no positive effect of the total amount of information disclosed ex post, the sum of disclosures becomes meaningful if we focus only on those answers which C rates as relevant for allocation behavior. Regressions in Table 3.A.12 in the Appendix repeat the previous analysis only with the disclosure of relevant answers. Here, more disclosure indeed implies generosity, i.e., *allocators* disclosing more relevant answers distribute more money to player C and less money to themselves. Taking together the findings on overall and relevant ex post disclosure, there appears to be a positive relationship between disclosing more and being more generous, but sharing irrelevant information under *strategic* incentives obfuscates this relationship. Thus, competition for benefits might lead to more information sharing, and hence to more meaningful information diffusion, but also to less straightforward signaling due to information abundance.

The Appendix also reports acceptance thresholds from the impunity game as a form of "choice and voice". While social *comparison* might decrease acceptance, we do not find robust treatment effects regarding acceptance thresholds. However, acceptance thresholds are significantly higher than predicted by game theory, so subjects are willing to forgo some money in our experiment when being offered too little. Even though altruistic sanctioning in monetary terms is excluded, respondents often engage in non-monetary altruistic accusation via choosing a positive acceptance threshold.

3.5 Discussion: Robustness of results based on beliefs

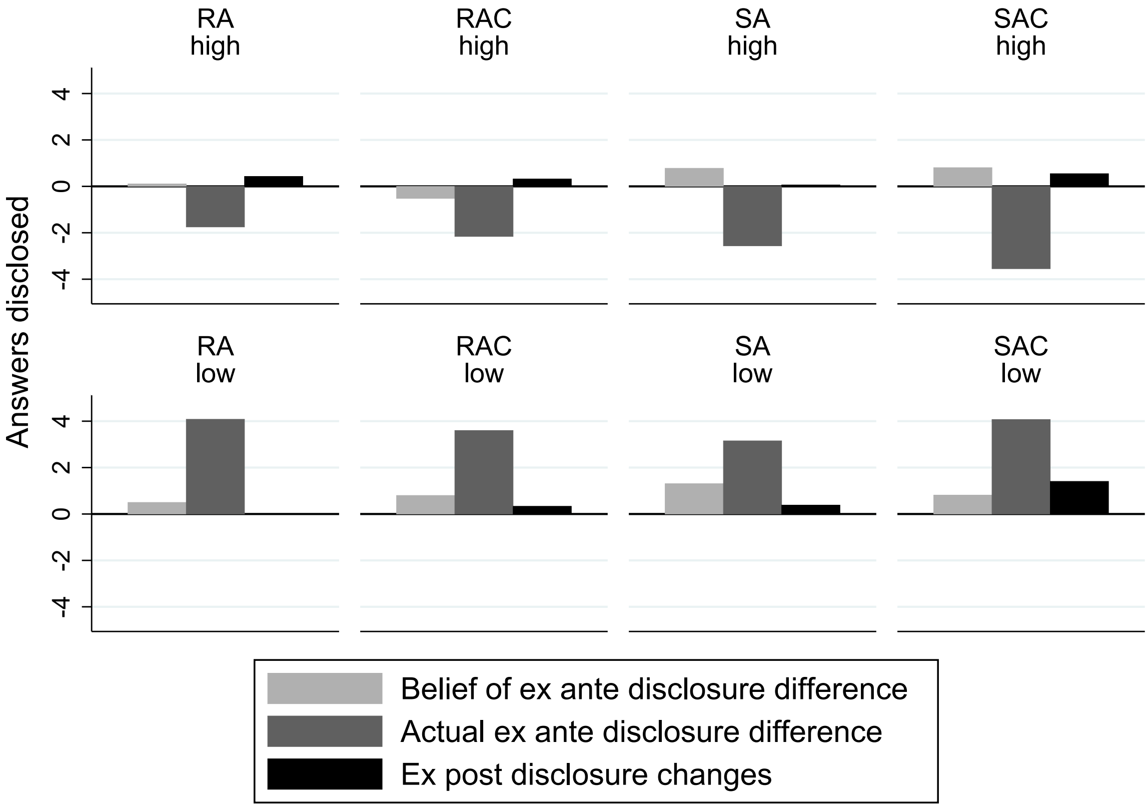
Our analysis of disclosure behavior highlights that the combination of benefits for information sharing and observing peers' information sharing increases disclosure. In this section, we try to explore more deeply why this is the case in the SAC treatment. One explanation already discussed and supported by our results is peer pressure stemming from peer comparison. Another conflicting explanation is the effect of information provision per se. Providing important information about the other candidate's information-sharing behavior in SAC could trigger changes in disclosure behavior due to less uncertainty about how much and which information one has to reveal to increase one's chances of becoming the *allocator*. However, we argue in the following why a pure information-provision argument cannot fully explain our results.[36] Moreover, we provide evidence that there are no systematic differences with respect to content of the given answers between those who disclose more or less information, respectively.

Ex post disclosure changes could be driven by wrong beliefs about the competitor's disclosure and by social pressure. Subjects, likely updating their incorrect beliefs about how much more information disclosure is needed for becoming *allocator*, seem to account for 22.2% of those candidates in our SAC sample, who lag behind ex ante. In these cases, subjects fully catch up or even overbid their competitor in terms of ex post information disclosure after peer *comparison*. Such behavior may resemble imitation learning (Huck et al. 1999; Vega-Redondo 1997). In contrast, 29.6% adapt partly by one to three disclosure extensions, but still disclose less ex post. This group seems to trade off privacy concerns by reducing the distance to the competitor, likely due to conformity seeking (Asch 1951; Bernheim 1994). 44.4% of participants do not react at all when learning about the disclosure choice of their peer. This type likely does not want to trade off privacy against potential benefits of disclosing more.

Since our detailed dataset contains beliefs about how much a subject expects her competitor to disclose, we can examine the information provision explanation in more

[36] While we cannot completely rule out that the high perceived pressure in SAC stems from receiving information in a competitive setting per se and is not related to the privacy component of our data, significant pressure seems to exist under such conditions, at least if personal information is at work. If anything, general applicability of our results to competitive settings with peer comparison would increase the relevance of our results even further.

Figure 3.7: Disclosure behavior and beliefs by treatment and ex ante disclosure



Notes: The lower candidate is the one who disclosed strictly fewer answers ex ante than her competitor.

detail. If the belief about the competitor's amount of disclosure was systematically too low, the additional information provided in *comparison* treatments should initiate more disclosures in order to outbid one's competitor. However, the beliefs of those disclosing less, depicted in Figure 3.7, are correct: They expect their competitor to disclose more information than they do themselves, as the first bars of "SA low" and "SAC low" show. If participants had expected to disclose less, they should have behaved according to their correct belief by already disclosing more ex ante. Recall here that participants are unaware that they will be able to revise their choice when making their ex ante disclosure decision, and should consequently act as if it is the final choice. Thus, wrong beliefs about how much disclosure is necessary to become *allocator* seem an unlikely explanation for discrepancies in disclosure. Rather, privacy costs may hinder those who lag behind from catching up with those having lower privacy costs. In effect, additional information under social comparison rather confirms that one lags behind, rather than providing new insights.

Moreover, if wrong beliefs were decisive for disclosing less, one should see a strong reaction when learning how much more revelation is needed to outbid the competitor. Rather than observing this, there is only a minor increase in disclosures after the *comparison* stage in SAC (compare the third bar in category "SAC low" of Figure 3.7 to the second bar). Thus, the majority of disclosure changes are unlikely to occur in order to outbid the competitor. The candidate lagging behind reveals somewhat more, but more often than not refrains from jumping ahead in disclosure. This highlights peer pressure in information disclosure competition as a more likely driver of the personal information disclosure dynamics we observe.

Overall, our results indicate that there seem to be substantial restrictions that make subjects refuse to share their personal information despite being aware of lagging behind in information disclosure competition. On the one hand, for some subjects, this might be the content of the information. Subjects not only need to consider the number of questions that they reveal, but also their answers to these questions. If they expect their personal data to send a bad signal, they may prefer to refrain from disclosing their information. On the other hand, some subjects may experience a general unwillingness to share their data, independently of how good or bad their information is, as a form of privacy cost. In order to distinguish these two explanations, we check for systematic differences between the content of answers given by those *allocator* candidates who disclose ex ante more or less information, respectively. Table 3.A.7 in the Appendix reports results from corresponding OLS regressions at question level. Neither for the full sample nor for the restricted sample of subjects in *strategic* treatments are there any significant differences in answer content between subjects who disclose more or less ex ante, except for question 7.[37] Hence, for 11 out of 12 answers, there are no systematic differences in terms of content of the answers

[37]Question 7 reads "Should people who voluntarily donate an organ receive payment for it?" and has no right or wrong answer. While this answer has a marginally significant impact on whether a subject becomes the *allocator* in the regression with both disclosure and answer content included in Table 3.A.15 in the Appendix, it is not significant without controlling for disclosure and is not predictive for allocating more money to player C in the distribution stage.

one could reveal. We infer from these results that different content and thus different signals are not driving disclosure behavior.

On top of finding no systematic differences in actual content between those disclosing more or less, there are no differences either with regard to beliefs regarding content. For each answer, we compare a subject's own answer with her guess regarding which answer her opponent has given. Regressing each of these differences again on an indicator for being the one disclosing fewer answers in Table 3.A.8 returns no statistically significant effects. Hence, subjects who disclose fewer answers do not belief that the answers they have given are systematically different from the answers of their competitor. This conclusion always holds, regardless of whether we look at the full sample or only at subjects in *strategic* treatments. Consequently, differences in beliefs regarding content cannot drive our results. Rather, some people seem to feel a general reluctance to share personal details, independently of whether the content is good or bad.

In summary, we conclude that wrong beliefs about the benefits of information sharing, as well as different content of the information to share, cannot explain our findings. On the contrary, heterogeneous privacy costs in a world with information disclosure competition and conformity seeking can explain the observed patterns in disclosure behavior. In combination with the perceived pressure that disclosure-unwilling individuals report, our results therefore highlight important new patterns in personal information sharing, which have so far been overseen.

3.6 Conclusion

In this paper, we investigate personal information sharing under competition for benefits. Particularly, we examine the interaction of strategic incentives and peer comparison to disclose personal information as a channel leading to more and more information sharing as observed in the field. Moreover, we provide evidence on a potentially neglected side effect in such a context, i.e., an increase in perceived pressure to disclose for those who are intrinsically less willing to share information about themselves. Our setting best fits modern markets, for example social media platforms like Instagram and Youtube, Airbnb, LinkedIn, or microfinance or crowdfunding platforms like Kickstarter, in which one market side strives for another's beneficial attention by providing personal information. It also applies to offline markets, for example the housing market in which prospective tenants try to stand out from the crowd of applicants by bringing a well-designed folder with abundant documents, but is exacerbated by online markets.

In our lab experiment, participants can endogenously reveal potentially sensitive answers from a personality and opinion questionnaire in order to be selected to determine the allocation decision in an impunity game. We vary in how far information sharing can serve a strategic purpose, and analyze how it is influenced by peer comparison. Results show that strategic incentives double disclosure, and that this effect is fostered by subsequent peer comparison. This dynamic response is primarily driven by those participants who learn from social comparison that they revealed less, a priori, than their competitors. It goes along with an increase in perceived pressure to have to disclose information and with the disclosure of more unpleasant information. We find that more disclosure-willing individuals are more frequently picked, but that the abundance of information shared under incentives obfuscates the positive relationship between sharing more relevant information and desirable behavior.

Which implications can be drawn from our results? First, it is unlikely that all information sharing we observe online nowadays is based on a pure preference for revelation. Rather, modern markets of the 21st century trade personal information as a medium of exchange for benefits, and people respond to this incentive by revealing more. Second, peer pressure exists in personal information disclosure. Observing others who freely share personal information for benefits triggers intrinsically reluctant individuals to adapt their behavior. This adaptation process, driven by the interplay of benefits and observing peers sharing, sheds light on the channel underlying the present, seemingly unstoppable trend of more and more voluntary information disclosure. Third, the high level of pressure, which participants in our experiment report after being influenced by a more disclosure-willing peer, and the sharing of more information, which is perceived as unpleasant, provide indicative evidence of a potential and so far neglected side effect of markets with information revelation competition. Those who freely share information in exchange for benefits and incur low privacy costs exert social pressure on the more disclosure-unwilling to adapt.

The more others share, the harder it becomes to abstain. In effect, disclosure-unwilling individuals may partly adapt, incurring high privacy cost without meaningfully affecting outcomes. They would have been better off in a state with less overall disclosure driven by strategic incentives and peer comparison.

Our results are in line with evidence by Brandts et al. (2009), DellaVigna et al. (2012, 2017), and Reyniers and Bhalla (2013), illustrating that competition or social pressure can reduce well-being or welfare. However, we refrain from a welfare analysis since the personal data we use may be less predictive for real-world behavior than personal data exchanged in the field. Thus, we would underestimate potential welfare gains for the selecting market side. Rather, we focus on understanding the disclosure side, emphasize the power of peer dynamics in markets with gains from personal information sharing, and point out that a reluctant group might be hurt. The effects we find in our setup with anonymous personal information are likely even stronger in the field with non-anonymous and more privacy-sensitive personal data.

Moreover, our finding that competition via extensive personal information sharing is beneficial for the disclosure-willing market side, but provides only limited insights for the selecting one, is in line with evidence from the field (Iyer et al. 2016; Michels 2012; Pope and Sydnor 2011). In our setting, incentives to share more personal information obfuscate the positive relationship between the number of relevant disclosures and generosity towards others. Since a lot of personal information sharing occurs in settings which incentivize people to reveal personal details, for example on Airbnb to attract guests, on LinkedIn to attract recruiters, or on microfinance and crowdfunding platforms to attract investors, competition via personal information revelation might lead to extensive information sharing in order to catch attention, rather than highlighting the qualitatively most suitable options. The recent introduction of "superhosts"[38] on Airbnb might be a result of such information overbidding and questions the usefulness of extensive endogenous disclosure. As a consequence, personal information sharing may not be caring.

Although our study provides helpful insights into the channels underlying recent extensive (online) information sharing, it also has shortcomings. It relies on rather subjective

[38] "Superhost" is a rating of excellence on Airbnb which might have become necessary because with the mass of information already provided by hosts, screening based on this information is no longer useful.

opinions and attitudes as a source of personal information in a laboratory environment which might be less sensitive than identifiable information like names or photos in the real world. Further research might narrow the gap to field settings to show how peer comparison and strategic benefits, in part jointly and partly in isolation, affect endogenous personal information disclosure, but under less experimental control. A more detailed analysis of adaptation patterns of initially disclosure-unwilling individuals and their perceived pressure seems to be another promising perspective for further research.

3.A Appendix: Additional results

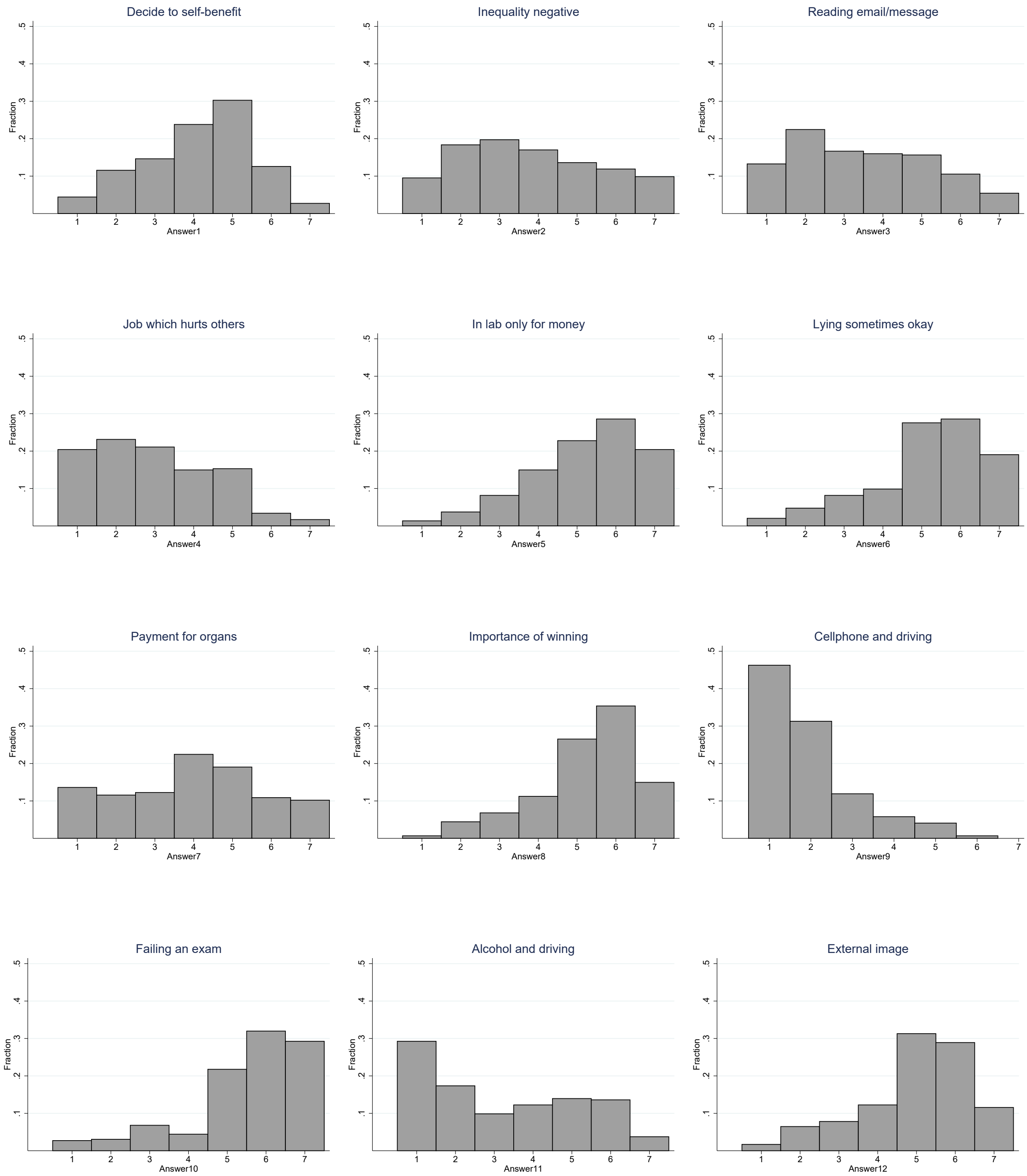
3.A.1 Descriptive statistics of answers and outcomes

Table 3.A.1: Descriptive statistics: Outcome variables

	random	random comparison	strategic	strategic comparison
Answers ex ante disclosed	1.63	2.10	3.55	4.00
	(3.078)	(2.610)	(2.957)	(3.217)
Ex post disclosure changes	0.30	0.33	0.21	0.95
	(1.454)	(0.730)	(0.585)	(1.820)
Relevant ex post disclosure changes	0.20	0.05	0.12	0.53
	(0.853)	(0.221)	(0.422)	(1.168)
Unpleasant ex post disclosure changes	0.10	0.30	0.26	0.52
	(0.632)	(0.158)	(0.256)	(0.522)
Perceived pressure (standardized)	-0.25	-0.32	0.02	0.37
	(0.786)	(0.748)	(1.024)	(1.144)
Own payoff (€)	10.70	9.95	9.64	8.98
	(3.818)	(3.493)	(3.764)	(3.706)
C's payoff (€)	3.15	3.63	4.03	4.40
	(1.902)	(1.835)	(2.060)	(2.094)
Acceptance threshold	1.98	2.45	1.83	2.50
	(1.510)	(1.853)	(1.488)	(1.719)
N	40	40	58	58

Notes: Standard deviations in parentheses. Only role A and B considered.

Figure 3.A.1: Histograms of answers



Notes: Answers on 7-point scale (1 worst, 7 best).

3.A.2 Additional results: Ex post and ex ante disclosure behavior

Table 3.A.2: Probit regressions - Disclosure-affecting factors at question level

	Answer (x) disclosed ex ante					
	(1)	(2)	(3)	(4)	(5)	(6)
unpleasant	-0.039***	-0.012	-0.023	-0.022*	-0.040***	-0.015
	(0.014)	(0.014)	(0.014)	(0.012)	(0.015)	(0.013)
relevant	0.034**	0.044***	0.015	0.032***	-0.002	-0.004
	(0.016)	(0.012)	(0.015)	(0.012)	(0.013)	(0.013)
answer	-0.121***	0.054***	-0.039**	-0.132***	-0.052***	-0.025
	(0.019)	(0.014)	(0.017)	(0.015)	(0.017)	(0.017)
strategic	0.207***	0.179***	0.091	0.228***	0.062	0.032
	(0.058)	(0.057)	(0.056)	(0.048)	(0.057)	(0.054)
baseline probability	0.348	0.251	0.199	0.381	0.215	0.169
N	196	196	196	196	196	196
Pseudo R2	0.255	0.219	0.098	0.344	0.139	0.081
	(7)	(8)	(9)	(10)	(11)	(12)
unpleasant	-0.000	-0.025	0.017	-0.035*	-0.025	0.008
	(0.017)	(0.016)	(0.017)	(0.019)	(0.018)	(0.016)
relevant	-0.009	0.015	0.049***	0.046***	0.036**	0.023
	(0.014)	(0.016)	(0.014)	(0.015)	(0.015)	(0.017)
answer	0.033**	-0.028	-0.113***	0.015	-0.068***	0.053**
	(0.013)	(0.021)	(0.030)	(0.016)	(0.015)	(0.023)
strategic	0.201***	0.113**	0.177***	0.092	0.135**	0.244***
	(0.062)	(0.052)	(0.053)	(0.057)	(0.055)	(0.052)
baseline probability	0.220	0.214	0.302	0.204	0.252	0.267
N	196	196	196	180	196	196
Pseudo R2	0.146	0.077	0.172	0.109	0.190	0.184

Notes: * p<0.10, ** p<0.05, *** p<0.01. Marginal effects displayed, representing changes in the probability to disclose a certain answer, with disclosure decision of answer(x) as 0-1 (no/yes) outcome variable. Standard errors in parentheses clustered at group level. Control dummies for the ten different randomizations of questions used included.

Factors affecting disclosure at answer level

As reported in probit regressions in Table 3.A.2, several factors seem to affect the probability to reveal a particular answer from the questionnaire. Of course, the answer one gave significantly affects disclosure for most questions. Moreover, the perceived relevance for predicting subsequent allocation behavior increases the probability of disclosing the answer. In contrast, a feeling of discomfort to reveal a particular answer decreases it. The *strategic* coefficient reports by disclosing which particular answers participants respond to the disclosure incentive. All answers are disclosed significantly more often in *strategic* treatments, except answers three, five, six, and ten.

Table 3.A.3: Tobit regressions - Effect of strategic incentives on ex ante disclosure

	Answers ex ante disclosed			
	(1)	(2)	(3)	(4)
strategic	3.264***	3.581***	3.509***	3.666***
	(0.658)	(1.024)	(0.989)	(1.000)
comparison		1.240	1.429	1.466
		(1.093)	(1.097)	(1.086)
strategic # comparison		-0.592	-0.748	-0.826
		(1.289)	(1.231)	(1.239)
constant	0.236	-0.407	0.765	-0.087
	(0.544)	(0.856)	(1.929)	(2.309)
sigma	3.898***	3.883***	3.714***	3.686***
	(0.314)	(0.320)	(0.300)	(0.285)
basic controls	No	No	Yes	Yes
preference controls	No	No	No	Yes
N	196	196	196	196
Pseudo R2	0.032	0.035	0.051	0.053

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses clustered at group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

Relevance and discomfort of ex post disclosure changes

Table 3.A.5 reports OLS regression results regarding the perceived relevance and discomfort of ex post disclosure changes. Patterns are similar to the main results. The *strategic-comparison* interaction effect is significant in all specifications with and without controls both with respect to relevant and unpleasant disclosure changes. Hence, while subsequent peer comparison under strategic incentives leads to the disclosure of more relevant information, it also makes participants reveal information that they perceive as

Table 3.A.4: Robustness to winsorization - Ex ante disclosures and ex post disclosure changes

	Answers ex ante disclosed				Ex post disclosure changes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
strategic	2.026***	2.002***	2.015***	2.160***	0.090	0.060	0.016
	(0.409)	(0.633)	(0.620)	(0.638)	(0.083)	(0.096)	(0.115)
comparison		0.400	0.573	0.614	0.200*	0.185	0.120
		(0.567)	(0.595)	(0.593)	(0.112)	(0.119)	(0.125)
strategic # comparison		0.048	-0.122	-0.175	0.507**	0.514**	0.620**
		(0.817)	(0.790)	(0.805)	(0.250)	(0.252)	(0.271)
constant	1.750***	1.550***	2.250	1.305	0.100*	0.469	-0.270
	(0.284)	(0.461)	(1.370)	(1.572)	(0.055)	(0.426)	(0.423)
basic controls	No	No	Yes	Yes	No	Yes	Yes
preference controls	No	No	No	Yes	No	No	Yes
N	196	196	196	196	196	196	196
R2	0.109	0.114	0.192	0.207	0.099	0.119	0.170

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. Results winsorized by 10% on treatment level.

particularly unpleasant and costly.

Directions of ex post disclosure changes and adaptation behavior

We look at three other outcome variables of ex post disclosure behavior, namely the direction of changes, i.e., the number of ex post upward and downward changes, respectively, and whether the change mimics the competitors' revelation. The direction of changes can be inferred from Figure 3.3 in the main analysis, and Table 3.A.6 shows the corresponding regression results. The *strategic-comparison* interaction effect is significant for the number of adaptations to the other's ex ante disclosure and for the number of upward changes, i.e., ex post disclosure of answers not disclosed ex ante. None of our explanatory variables prevails significant for downward changes, i.e., answers disclosed ex ante, but hidden ex post.

Two main messages follow from this analysis. First, looking at columns (1) and (2) of Table 3.A.6, ex post disclosure changes are adaptations to disclosures of one's competitor. This alludes to conformity seeking (Asch 1951; Bernheim 1994). In fact, the correlation

Table 3.A.5: Relevance and discomfort of ex post disclosure changes by treatment

	Relevant Changes		Unpleasant Changes	
	(1)	(2)	(3)	(4)
strategic	-0.079	-0.067	-0.031	-0.075
	(0.142)	(0.136)	(0.104)	(0.093)
comparison	-0.150	-0.171	-0.075	-0.109
	(0.135)	(0.136)	(0.102)	(0.096)
strategic # comparison	0.564***	0.601***	0.213*	0.261**
	(0.211)	(0.224)	(0.126)	(0.131)
constant	0.200	-0.098	0.100	0.004
	(0.131)	(0.341)	(0.099)	(0.186)
controls	No	Yes	No	Yes
N	196	196	196	196
R2	0.059	0.133	0.025	0.103

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Controls include gender and age, as well as dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

between the number of adaptations and the number of ex post disclosure changes is very high at 0.84, and the interaction effect in column (1) of Table 3.A.6 for adaptations is similar in magnitude to that in column (1) of Table 3.5 for ex post disclosure changes. Testing for similarity of the interaction effects across the two regressions with adaptations and ex post disclosure changes as outcome variables yields a p-value of 0.924, so there is no indication to reject similarity of the two coefficients. When competing with peers, one seems to disclose that kind of information that peers also disclose, which can be regarded as an *intensive margin*.

Secondly, reported in columns (3)-(6) of Table 3.A.6, the combination of social and economic incentives to disclose captured by the *strategic-comparison* interaction effect explains disclosure extensions, but not disclosure reductions. This alludes to an *extensive margin* because one reveals more information if others do so, given that one can benefit from revelation. The finding that the ex ante disclosure changes of interest are mainly

Table 3.A.6: Adaptations to competitor and directions of ex post disclosure changes

	Adaptations		Upward changes		Downward changes	
	(1)	(2)	(3)	(4)	(5)	(6)
strategic	-0.122	-0.151	0.115	0.041	0.208	0.173
	(0.227)	(0.209)	(0.086)	(0.112)	(0.223)	(0.178)
comparison	-0.000	-0.050	0.075	0.011	0.050	0.056
	(0.240)	(0.221)	(0.090)	(0.111)	(0.242)	(0.207)
strategic # comparison	0.707**	0.773**	0.580**	0.681**	-0.136	-0.143
	(0.321)	(0.332)	(0.255)	(0.275)	(0.248)	(0.234)
constant	0.225	0.170	0.075	0.006	-0.225	0.112
	(0.222)	(0.415)	(0.054)	(0.384)	(0.222)	(0.247)
controls	No	Yes	No	Yes	No	Yes
N	196	196	196	196	196	196
R2	0.061	0.122	0.091	0.146	0.011	0.111

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Basic controls include gender, age, and dummies for the ten different randomizations of questions used. Preference controls include dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

disclosure extensions shows that peer comparison in a world with benefits seems to affect disclosure behavior in only one direction, namely to reveal more.

Table 3.A.7: Answer scores by more / less initial disclosure on question level

	Answer to question (x)					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.180	-0.112	0.167	0.087	-0.339	0.003
	(0.196)	(0.245)	(0.282)	(0.236)	(0.233)	(0.222)
constant	4.618***	4.056***	3.541***	2.956***	4.878***	5.207***
	(0.296)	(0.273)	(0.417)	(0.340)	(0.390)	(0.313)
N	196	196	196	196	196	196
R2	0.057	0.053	0.083	0.080	0.055	0.045

	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	-0.869***	-0.040	0.151	0.057	0.082	0.111
	(0.293)	(0.212)	(0.156)	(0.254)	(0.307)	(0.202)
constant	4.268***	5.145***	1.966***	5.097***	3.459***	4.986***
	(0.469)	(0.282)	(0.261)	(0.311)	(0.475)	(0.285)
N	196	196	196	196	196	196
R2	0.080	0.036	0.036	0.028	0.030	0.019

	Answer to question (x) - Strategic treatments only					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.302	0.079	0.366	0.111	-0.078	0.076
	(0.254)	(0.332)	(0.378)	(0.319)	(0.315)	(0.282)
constant	4.661***	4.023***	3.380***	2.632***	4.289***	4.900***
	(0.359)	(0.302)	(0.544)	(0.260)	(0.426)	(0.433)
N	116	116	116	116	116	116
R2	0.077	0.077	0.086	0.051	0.124	0.102

	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	-1.116***	0.103	0.263	-0.064	0.400	-0.140
	(0.386)	(0.267)	(0.192)	(0.345)	(0.406)	(0.270)
constant	4.183***	4.949***	1.869***	5.157***	3.112***	5.132***
	(0.588)	(0.330)	(0.313)	(0.443)	(0.682)	(0.336)
N	116	116	116	116	116	116
R2	0.168	0.047	0.087	0.054	0.077	0.026

Notes: * p<0.10, ** p<0.05, *** p<0.01. Content of answer(x) on 7-point scale as outcome variable. The lower candidate is the one who disclosed strictly fewer answers ex ante than her competitor, with the latter serving as a baseline. Standard errors in parentheses clustered at group level. Control dummies for the ten different randomizations of questions used included.

Table 3.A.8: Perceived answer score differences by more / less initial disclosure on question level

	Difference in answer (x)					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.023	-0.030	0.134	0.077	0.003	0.113
	(0.201)	(0.222)	(0.211)	(0.269)	(0.231)	(0.292)
constant	0.447*	0.265	-0.025	0.170	0.623*	-0.348
	(0.227)	(0.351)	(0.269)	(0.400)	(0.355)	(0.211)
N	196	196	196	196	196	196
R2	0.067	0.116	0.138	0.026	0.141	0.065

	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	0.344	-0.350	-0.107	-0.069	0.347	-0.048
	(0.285)	(0.298)	(0.265)	(0.261)	(0.242)	(0.322)
constant	0.203	0.550*	0.512	0.243	-0.424	-0.018
	(0.485)	(0.291)	(0.366)	(0.274)	(0.466)	(0.313)
N	196	196	196	196	196	196
R2	0.079	0.099	0.045	0.066	0.105	0.050

	Difference in answer (x) - Strategic treatments only					
	(1)	(2)	(3)	(4)	(5)	(6)
lower candidate	0.202	0.074	0.089	0.122	-0.354	0.282
	(0.288)	(0.291)	(0.312)	(0.350)	(0.255)	(0.380)
constant	0.274	0.026	-0.545**	0.252	0.927**	-0.516*
	(0.290)	(0.503)	(0.228)	(0.569)	(0.428)	(0.291)
N	116	116	116	116	116	116
R2	0.066	0.094	0.170	0.046	0.172	0.105

	(7)	(8)	(9)	(10)	(11)	(12)
lower candidate	0.459	-0.513	-0.258	-0.018	0.142	0.175
	(0.382)	(0.408)	(0.368)	(0.328)	(0.314)	(0.428)
constant	0.395	0.694*	0.379	0.071	-0.446	-0.275
	(0.604)	(0.383)	(0.460)	(0.389)	(0.679)	(0.421)
N	116	116	116	116	116	116
R2	0.101	0.161	0.079	0.069	0.103	0.063

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Difference between guess of the other's answer and one's own answer as outcome variable for answer(x). The lower candidate is the one who disclosed strictly fewer answers ex ante than her competitor, with the latter serving as a baseline. Standard errors in parentheses clustered at group level. Control dummies for the ten different randomizations of questions used included.

3.A.3 Additional results: *Allocator* selection and distribution outcomes

Allocator selection

Table 3.A.9: Effect of difference in information disclosure on probability to become *allocator*

	Allocator			
	(1)	(2)	(3)	(4)
own - other's ex post disclosures	0.042***	0.030*		
	(0.016)	(0.018)		
own - other's relevant ex post disclosures			0.077***	0.074**
			(0.026)	(0.030)
blue displayed first		-0.060		-0.101
		(0.111)		(0.104)
red		0.033		0.012
		(0.139)		(0.125)
baseline probability	0.534	0.537	0.534	0.539
randomization controls	No	Yes	No	Yes
N	58	58	58	58
Pseudo R2	0.077	0.202	0.091	0.249

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports marginal effects from probit regressions with robust standard errors in parentheses. One *allocator* candidate is randomly chosen per group to calculate disclosure difference. Relevant disclosures only take disclosures into account which player C marks as relevant predictor of game behavior. "Red" and "blue displayed first" are dummies if the candidate's color is red and whether player blue is displayed above red on the screen for *allocator* selection. "Red" corresponds to role A and "blue" to role B in instructions. Randomization controls include dummies for the ten different randomizations of questions used. Only *strategic* treatments considered.

Table 3.A.9 shows the results of probit regressions with the probability of being selected as the *allocator* as the dependent variable. Since active *allocator* selection only takes place in *strategic* treatments, we only consider this subsample in our analysis. Moreover, we randomly draw one of the two *allocator* candidates in each group since otherwise each difference would be counted twice in the analysis. Columns (1) and (2) investigate how the difference in answers disclosed ex post affects the probability of being selected. Each additionally disclosed answer increases the chance to become *allocator* by 4.2 percentage points in column (1). Since we assigned participants the colors red (A) and blue (B) in our experiment for better identification, we add dummies equal to one if the color assigned is red, and if the blue player is randomly chosen to be displayed first on the choice

screen of player C, respectively. Moreover, we add dummies for the order in which the questions are displayed. Doing so decreases effect size and significance in column (2), but still confirms the relevance of disclosing relatively more than the competitor, i.e., a 3.0 percentage points higher probability to be selected for each additional answer disclosed.[39] The same analysis is repeated in columns (3) and (4) for a slightly modified outcome variable, which only considers those answers for the calculation of the disclosure difference which player C marks as relevant indicators for impunity game behavior. Results reveal a qualitatively similar pattern, but a bigger effect size, namely a 7.4% to 7.7% higher probability of being selected for each relevant answer one discloses more than the competitor. Consequently, disclosing more answers is indeed beneficial for being selected according to our experimental data, supporting Hypothesis 5, especially if the question is considered as relevant.

Allocation behavior

In order to check whether becoming *allocator* is beneficial in terms of payoff, Table 3.A.10 compares payoffs if subjects become *allocator* or not. Since we elicit payoff allocations before revealing who becomes *allocator* via the strategy method, for each subject, we can calculate a payoff for the case of becoming *allocator*, and one if not. The latter is the payoff allocated to her by her matched competitor if the competitor becomes *allocator* instead. We compare these two possible payoffs based on whether a subject decides herself about the payoff distribution. Column (1) shows a large and highly significant effect from determining the allocation oneself. Compared to a 3.40€ payoff from the competitor, subjects receive 6.33€ more if they determine the allocation themselves. This result is robust if controls are included in column (2). If we additionally include a dummy variable for *strategic* treatments and interact it with whether the payoff is self-determined or not in columns (3) and (4), the self-determination effect even increases in magnitude. This means that becoming *allocator* is highly beneficial in terms of payoff.

Table 3.A.11 presents results on how information disclosure in different treatments carries over to prosocial behavior, measured by the amount one keeps for oneself as the

[39]We limit the set of control variables to features visible to C when choosing the *allocator* since she does not know other characteristics about the participant.

Table 3.A.10: Payoff if determined by oneself or allocated by competitor

	Own payoff			
	(1)	(2)	(3	(4)
self-determined	6.327***	6.327***	7.038***	7.038***
	(0.407)	(0.411)	(0.642)	(0.648)
strategic			0.187	0.217
			(0.278)	(0.284)
self-determined # strategic			-1.201	-1.201
			(0.825)	(0.833)
constant	3.398***	3.190***	3.288***	3.157***
	(0.136)	(0.839)	(0.215)	(0.846)
controls	No	Yes	No	Yes
N	392	392	392	392
R2	0.537	0.548	0.544	0.554

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Each subject is included once as if determining the allocation decision herself and once as if receiving the payoff from her matched competitor. Controls include gender, age, dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

allocator in Panel A, and by the amount one gives to C in Panel B.[40] A lower coefficient in Panel A represents less egoistic behavior, while a higher coefficient in Panel B represents more generosity.

Pooling the data with and without peer comparison in column (1) confirms the "I-want-you" effect (Brandts et al. 2006): Selected *allocators* give more to the selector compared to a situation with random *allocator* assignment, i.e., *allocators* reciprocate the favor of their selection by offering more to C. When investigating all four treatments separately in column (2), the point estimate of the *strategic* coefficient does not change much, staying in the range of 80 to 90 cents which C earns more on average. This means that previous social *comparison* does not affect subsequent distribution behavior, and seems not to be detrimental for prosociality in our setting. Therefore, we stick with data pooled over *comparison* when investigating allocation behavior in more detail.

[40]Since the pie of 17€ is fixed, it is redundant also to report the amount given to the competitor.

Table 3.A.11: Payoff allocations by treatment and disclosure behavior

	Own payoff				
	(1)	(2)	(3)	(4)	(5)
strategic	-1.015*	-1.062	-0.329	-1.967**	-1.718**
	(0.551)	(0.823)	(0.772)	(0.756)	(0.817)
comparison		-0.750			
		(0.851)			
strategic # comparison		0.095			
		(1.098)			
allocator			-0.250		
			(0.784)		
strategic # allocator			-1.371		
			(1.027)		
answers ex post disclosed				-0.379***	-0.398***
				(0.118)	(0.129)
strategic # answers ex post disclosed				0.445***	0.421**
				(0.164)	(0.173)
constant	10.325***	10.700***	10.450***	10.997***	11.332***
	(0.427)	(0.676)	(0.562)	(0.467)	(1.799)
controls	No	No	No	No	Yes
N	196	196	196	196	196
R2	0.018	0.027	0.047	0.048	0.078

	C's payoff				
	(1)	(2)	(3)	(4)	(5)
strategic	0.828***	0.884**	0.460	1.205***	1.058**
	(0.288)	(0.413)	(0.421)	(0.394)	(0.430)
comparison		0.475			
		(0.430)			
strategic # comparison		-0.113			
		(0.573)			
allocator			0.075		
			(0.406)		
strategic # allocator			0.735		
			(0.556)		
answers ex post disclosed				0.197***	0.209***
				(0.058)	(0.063)
strategic # answers ex post disclosed				-0.203**	-0.192**
				(0.086)	(0.091)
constant	3.387***	3.150***	3.350***	3.039***	2.766***
	(0.217)	(0.336)	(0.292)	(0.240)	(0.946)
controls	No	No	No	No	Yes
N	196	196	196	196	196
R2	0.040	0.051	0.064	0.066	0.095

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Controls include gender, age, dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

Are players selected as *allocators* actually those who act more generously? We can answer this question with our strategy data. Since participants who reveal more information are more likely selected as *allocators*, we investigate whether C benefits from this selection strategy. Although the interaction effect of the *strategic* and the *allocator* variable in columns (3) of Table 3.A.11 point in the direction of more prosociality, it is not significant ($p = 0.189$), i.e., selected *allocators* do not systematically behave more generously. In Table 3.A.15, we report further results on question level regarding which answers predict allocation behavior, and to which extent these answers are taken into account for *allocator* selection.

Column (4) of Table 3.A.11 shows the direct effect of information disclosure on allocation behavior. There is a significant positive effect of the *strategic* coefficient on the amount allocated to C, as in the initial specifications, which can be attributed to 1.97€ of forgone own earnings of the *allocator*. Moreover, we observe highly significant effects on prosociality in opposite directions with respect to the number of answer disclosed ex post alone and with respect to its interaction with the *strategic* coefficient. The former effect captures the influence of more information sharing on prosocial behavior in *random* treatments. Interestingly, people intrinsically motivated to share personal information seem to keep less for themselves and give more to others. Quantitatively, for each answer they disclose, they give approximately 18 and 20 cents more to the other candidate and player C, respectively.

The positive relationship between more personal information disclosure and generosity vanishes with incentives. The ex post disclosure coefficient and its interaction with the *strategic* coefficient almost entirely cancel out. This means that more information sharing does not correspond to more prosociality in case of *strategic* incentives for information disclosure. While the level effect of more prosociality in *strategic* treatments remains strong in magnitude, revelation competition seems to destroy the predictive power of endogenous information disclosure for prosocial behavior. Figure 3.6 in the main text shows the corresponding coefficient plot. While without incentives to share information only the intrinsically motivated types disclose information, with incentives the non-intrinsic, opportunistic types also start to disclose, thereby diluting the original relationship. The

Table 3.A.12: Payoff allocations by treatment and relevant disclosure behavior

	Own payoff		C's payoff	
	(1)	(2)	(3)	(4)
strategic	-0.566	-0.338	0.466	0.325
	(0.578)	(0.622)	(0.297)	(0.319)
relevant answers ex post disclosed	-0.294*	-0.367**	0.197**	0.235***
	(0.165)	(0.166)	(0.084)	(0.085)
strategic # relevant answers ex post disclosed	-0.076	-0.074	0.239	0.243
	(0.310)	(0.305)	(0.182)	(0.193)
constant	10.601***	10.459***	3.203***	3.257***
	(0.454)	(1.764)	(0.231)	(0.909)
controls	No	Yes	No	Yes
N	196	196	196	196
R2	0.038	0.077	0.083	0.120

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Controls include gender, age, dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM.

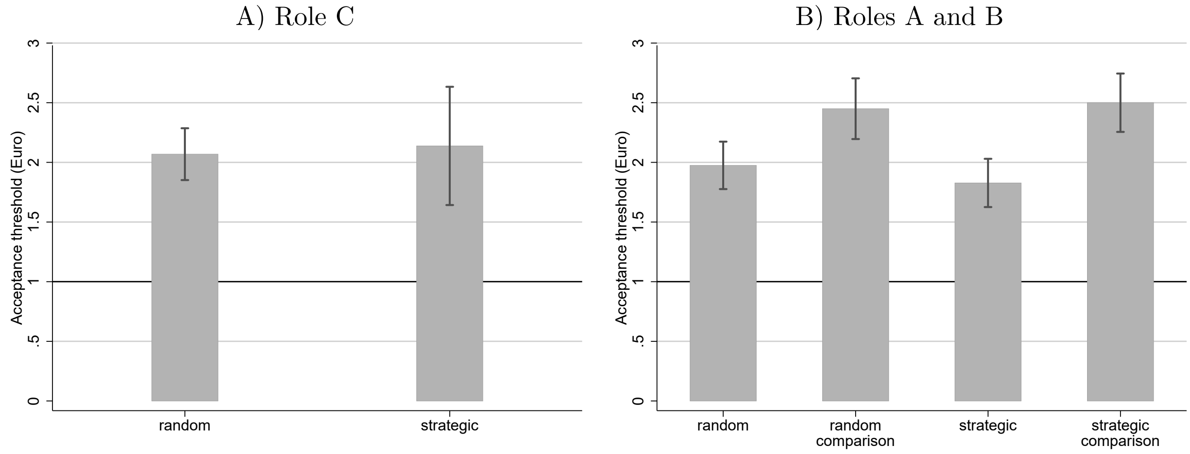
positive relationship between intrinsic information sharing and generosity remains when winsorizing ex post disclosures at the 90%-level in columns (3)-(6) of Table 3.A.14.

When instead looking at the relationship between generosity and those disclosed answers, which C considers as relevant indicators of game behavior in Table 3.A.12, the amount of disclosure becomes meaningful. As in the previous analysis, there is a significantly positive (negative) effect on the amount the *allocator* gives to C (herself). In contrast, there is no longer a significantly negative interaction between the amount of disclosure and the *strategic* coefficient. This means that *strategic* incentives do not distort the positive relationship between disclosing more and being more generous when it comes to relevant information disclosure. Taken together, this implies that information has to be screened more carefully regarding its relevance if incentives for disclosure are involved.

Acceptance thresholds

This section presents acceptance thresholds in the impunity game elicited for all three players of a group by using the strategy method. Figure 3.A.2 plots the acceptance thresholds in all four treatments of subjects in roles A and B in Panel B. We depict those

Figure 3.A.2: Coefficient plot of acceptance thresholds by role and treatment



Notes: Vertical lines represent standard errors clustered at group level. The line at level one depicts the minimum payoff when not rejecting.

of C separately in Panel A because C is in another info set when stating her acceptance threshold as she already knows who becomes *allocator* at that point in time. Moreover, Panel A consists of only two bars since C is not informed about the different social *comparison* levels. Since each player receives a payoff of 1€ for sure if she accepts the *allocator's* offer, setting an acceptance threshold higher than 1€ might cause a payoff loss, and is weakly dominated for subjects interested only in their own payoff. Nonetheless, all bars display significantly higher acceptance thresholds (all $p < 0.001$), ranging from 1.83€ to 2.50€. This means that subjects are willing to forgo some money in our experiment when being offered too little.

We try to disentangle what drives the high acceptance thresholds in simple OLS regressions displayed in Table 3.A.13, but find no significant differences between treatments and roles except for *comparison*. The *strategic* coefficient is neither significant in column (1) for role C nor in column (2) for roles A and B, and provides zero explanatory power ($R^2 = 0.000$). In contrast, social *comparison* turns out to push the acceptance threshold upwards. While this effect turns out to be significant in columns (3) and (4) when investigated pooled over the *strategic* dimension, it is not if this dimension is additionally taken into account. As a consequence, we refrain from conclusions regarding acceptance behavior in the impunity game.

Table 3.A.13: Acceptance thresholds by role and treatment

	Acceptance threshold					
	(1)	(2)	(3)	(4)	(5)	(6)
	C	A and B	A and B	A and B	A and B	A and B
strategic	0.069	-0.049			-0.147	-0.136
	(0.321)	(0.231)			(0.282)	(0.300)
comparison			0.592**	0.600**	0.475	0.454
			(0.227)	(0.242)	(0.321)	(0.341)
strategic # comparison					0.197	0.246
					(0.450)	(0.473)
controls	No	No	No	Yes	No	Yes
N	98	196	196	196	196	196
R2	0.000	0.000	0.032	0.049	0.033	0.051

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses, in columns (2)-(6) clustered at group level.

Table 3.A.14: Robustness to winsorization - Perceived pressure and allocation behavior

	Pressure		Own payoff		C's payoff	
	(1)	(2)	(3)	(4)	(5)	(6)
strategic	0.264	0.242	-2.153***	-1.933**	1.293***	1.161***
	(0.178)	(0.171)	(0.770)	(0.831)	(0.402)	(0.438)
comparison	-0.062	-0.089				
	(0.165)	(0.169)				
strategic # comparison	0.416*	0.474*				
	(0.249)	(0.243)				
answers ex post disclosed			-0.540***	-0.572***	0.274***	0.294***
			(0.147)	(0.149)	(0.075)	(0.076)
strategic # answers ex post disclosed			0.603***	0.594***	-0.280***	-0.277***
			(0.186)	(0.190)	(0.099)	(0.101)
constant	-0.254**	-0.328	11.195***	11.775***	2.945***	2.546***
	(0.112)	(0.454)	(0.481)	(1.788)	(0.248)	(0.943)
controls	No	Yes	No	Yes	No	Yes
N	196	196	196	196	196	196
R2	0.076	0.113	0.057	0.088	0.073	0.103

Notes: * p<0.10, ** p<0.05, *** p<0.01. Table reports OLS regression coefficients with standard errors in parentheses clustered at group level. Controls include gender, age, dummies for "Westin fundamentalist", publicly accessible and identifiable social media profiles, respectively, and the ability and opinion comparison seeking indexes from INCOM. Results winsorized by 10% at treatment level. Pressure reported in standard deviations, payoffs in €.

Selection and allocation outcomes by answers

Table 3.A.15 reports how answers translate into outcomes. Regarding allocation behavior in *strategic* treatments, answers 1, 9, 10, and indicatively also answer 2, turn out to be predictive for the amount one allocates to player C, but these answers are only insufficiently taken into account for *allocator* selection in column (2). A probit model in columns (2) finds significant effects of answers 1 and 2 on the probability of being selected as *allocator* only at the 10% level, and no effect for the other answers predictive for behavior. Note that answers taken into account for *allocator* selection are limited to answers which are actually disclosed since player C can only take these answers into account for selection. When considering content of the disclosed answers and disclosure per se separately in column (3), the pattern just described fades. With reference to question 1, its pure disclosure seems to matter more than its content. In addition, at the 5% significance level, disclosure of questions 5, 8, and 12 appears to be important for C's *allocator* selection decision, as well as the content of questions 3, 8, and 12.

Table 3.A.15: Effects of answers and disclosures on selection and allocation behavior

	Allocation to C	Allocator	
	(1)	(2)	(3)
Answer1	-0.360**	0.052*	-0.062
Answer2	0.184*	0.030*	0.052
Answer3	0.002	-0.047	-0.074**
Answer4	-0.121	0.000	-0.055
Answer5	0.002	-0.001	0.063*
Answer6	-0.063	0.002	0.050
Answer7	-0.009	0.027	0.067*
Answer8	-0.198	0.024	-0.242***
Answer9	-0.394**	-0.009	-0.030
Answer10	0.243**	-0.003	-0.080
Answer11	-0.059	-0.049	0.043
Answer12	-0.061	0.013	0.167***
Ex post disclosure question1			0.515***
Ex post disclosure question2			-0.316
Ex post disclosure question3			0.168
Ex post disclosure question4			0.203
Ex post disclosure question5			-0.353**
Ex post disclosure question6			0.001
Ex post disclosure question7			-0.002
Ex post disclosure question8			1.253***
Ex post disclosure question9			0.005
Ex post disclosure question10			0.567
Ex post disclosure question11			-0.228*
Ex post disclosure question12			-0.868***
constant	6.736***		
baseline probability		0.498	0.504
randomization controls	Yes	Yes	Yes
N	116	116	116
R2 / Pseudo R2	0.395	0.085	0.343

Notes: * p<0.10, ** p<0.05, *** p<0.01. Marginal effects of probit model displayed in columns (2) and (3), representing changes in probability of becoming *allocator* based on question-level disclosures and answers. Answers are on a 7-item scale (1 worst, 7 best). Answers in columns (2) and (3) are restricted to those which are disclosed. Column (1) represents OLS regression results with the monetary amount allocated to player C as the outcome variable. Only strategic treatments considered. Standard errors clustered at group level not displayed for the sake of readability. Randomization controls include dummies for the ten different randomizations of questions used. R^2 reported in column (1), Pseudo R^2 in columns (2) and (3).

3.B Appendix: Instructions

Translated from German. Instructions taken from *strategic* treatments; variations in *random* treatments displayed in [square brackets].

Instructions: Part 1

Welcome, and thank you very much for your participation in this experiment. Please read the following instructions carefully. If you have any questions, feel free to raise your hand at any time. One of the experimenters will approach you to answer your questions. Please do not ask any more questions loudly, and do not communicate with other participants in the experiment. If you break this rule, we will have to dismiss you from the experiment and the associated payoff. No participant receives any information about the identity and payoffs of other participants during or after the experiment.

The experiment consists of two parts. You receive the instructions for the second part at the beginning of the second part.

Each participant receives 4 Euros for participating in this experiment. Moreover, your additional payment depends on the statements and on the decisions you and your interaction partners make, i.e., your decisions impact your own payoff as well as that of the other participants.

The first part of the experiment begins with a brief questionnaire. Please answer all questions carefully. For filling in the questionnaire, you receive 3 Euros. The questionnaire has to be filled in completely. If you do not agree to this practice, you have now or at any time during the experiment the possibility to leave the experiment without further consequences and without losing your guaranteed show-up fee of 4 Euros.

Instructions: Part 2

In this experiment, you interact in a group with two other players. For better distinction, the colors Red, Blue, and Green are assigned to the three participants, and represent their roles within the group. Groups and the roles Red, Blue, and Green are randomly assigned during the experiment, and then remain fixed for the whole experiment. The allocation decision, which will be explained in what follows, takes place exactly once.

Allocation decision

In this experiment, one participant should decide on the distribution of 17 Euro between all three group members. We call the player, who makes this decision, the *allocator* in what follows. Only Red and Blue can take the role of the *allocator*. [With a probability of 50% each, chance] Green decides whether Red or Blue can determine the allocation of the 17 Euro in the role of the *allocator*. Green cannot be the *allocator*.

Before the *allocator* is determined [randomly] by Green and the allocation decision is made, group members in the role Red and Blue can disclose information about themselves to the green participant. Whether you provide information about yourself to Green, and if yes, which, is completely optional for you. Particularly, you decide for each answer to the questionnaire whether the green participant is allowed to learn this information. For each answer disclosed, we subtract 10 Cents from your budget of 3 Euros from the first part of the experiment. Green can look at the disclosed information about the other two group members from the questionnaire before [chance] Green decides whether Red or Blue takes the role of the *allocator*.

The *allocator* can distribute the 17 Euro as integer, positive amounts between himself and the other two group members. The amount has to be distributed in full, and each member has to receive at least 1 Euro. Hence, the allocator can give each group member including himself 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, or 15 Euro, but the total amount must not exceed 17 Euro.

Each of the other two group members can decide which minimal amount of money he requires to receive from the *allocator* to accept his offer, or reject it otherwise. In case the *allocator's* offer is smaller than the minimum acceptable amount, one rejects his offer and receives 0 Euro. In case the offered amount is higher, one accepts the offer and receives the offered amount, i.e., at least 1 Euro. The two participants make this decision independently of each other. This means that your decision whether to accept or reject the offer affects only your own payoff, but does not affect the payoffs of the other two group members. In particular, the payoff of the *allocator* remains unaffected, independently of whether the other two group members accept or reject his offer, and always equals the amount the *allocator* kept for himself. However, the *allocator* learns whether his chosen monetary amounts are accepted or not.

In role Red or Blue, you will be asked to make one decision in case you become *allocator* and one in case you do not become *allocator*. Afterwards, [chance] Green decides who becomes *allocator*. At the end of the experiment, all group members will be informed about the decisions relevant for them, and their resulting payoffs.

Guesses of answers and information disclosed

During the experiment, we will ask you to guess how the other candidate for the role of the *allocator* (Red or Blue) answered the questionnaire, i.e., which answer (with seven response options) he chose for each of the questions. In addition, for each answer you will be asked to guess the other candidate's decision to disclose his response (yes or no). More precisely, this means that Red guesses the answers and corresponding disclosure decisions of Blue, and Blue guesses the answers and corresponding disclosure decisions of Red. Whether Green guesses the answers and disclosure decision of Red or Blue is determined by chance. At the end of the experiment, one of your guesses will be randomly selected for bonus payment. In case an answer guess is selected, you receive a bonus of 3.50 Euro if your guess is correct. In case a disclosure guess is selected, you receive a bonus of 1 Euro if your guess is correct. If your guess is not correct, you do not receive a bonus. Please note that *only one* of your guesses will be paid, i.e., *either* an answer guess *or* a disclosure guess, but not both. For this payoff mechanism, you fare best if you always state the value which equals your true guess.

Chapter 4

Increasing Personal Data Contributions: Field Experimental Evidence from an Online Education Platform[*]

joint with Tobias Rohloff and Sylvi Rzepka

Abstract

We study personal data sharing as a contribution to a public good. In a field experiment on an online platform, in which users are prompted to complete their profiles, we investigate whether the salience of public benefits and reduced privacy costs increases personal data contributions. Compared to a control message, we find that emphasis on public benefits increases the number of contributed profile information. Further reference to privacy protection enhances this effect. However, we do not find clear evidence that such treatments can also motivate users, who initially do not share any personal data, to start contributing. Our results highlight that even in a fast-paced online environment, reference to the social benefit can increase people's willingness to contribute, but mainly for those who are somewhat willing to share personal data.

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4.1 Introduction

Personal data may be the new currency of the 21st century and can create large benefits for society, for example, by allowing to route traffic efficiently (Cramton et al. 2019; Lv et al. 2014) or to predict diseases or their outbreak (Ginsberg et al. 2009; Obermeyer and Emanuel 2016). In such contexts, sharing personal information equals contributing to a public good since the benefits of data-driven services are to a large extent non-rivalry and their consumption non-excludable.[1] As a consequence, contributions to these new services in form of personal data are likely underprovided. In attempts to overcome this underprovision, Mozilla, for example, solicits users to "donate" their voice to their open-source voice database to help machines learn to speak naturally.[2] Similarly, the online platform openhumans.org explicitly asks for donations in form of personal data to conduct research.

Contributions to public goods in form of personal data differ from well-studied contributions in form of money or effort (Chaudhuri 2011) because they are individual-specific and potentially privacy-sensitive. A 10-Euro contribution is the same independent of whether it is entirely provided by one person or partly by several people. This is not the case for personal data because one extensive contribution from only one person leads to a less diverse database than an equal amount of contributions from many different people. Hence, public good provision, which is meant to fit the needs of various people based on the aggregation of personal data, requires a wide, diverse, and representative contribution base.[3] Otherwise, the most comprehensive public good cannot be provided. While other individual-specific contributions to public goods have already been studied, for example, knowledge (Chen et al. 2019; Zhang and Zhu 2011) and feedback (Bolton et al. Forthcoming; Bolton et al. 2004; Cabral and Li 2015; Chen et al. 2010; Ling et al. 2005), personal data as contributions have so far been neglected. However, personal data contributions may work differently than other individual-specific contributions to public

[1]Note the difference to services for which users "pay with their data", for example, if discounts for products or the use of an app require the opening of a user profile with mandatory entries, which renders usage excludable. Our focus lies on contexts in which people share personal data on a voluntary basis.

[2]See https://voice.mozilla.org/en.

[3]As aggregation technology, think of a benevolent social planer who produces the best possible public goods or services for society based on the personal data contributions she receives.

goods since they are additionally privacy-sensitive.[4] Compared to the cost of providing feedback or knowledge, this adds further costs - privacy costs - to contributing personal data and may therefore aggravate underprovision.

In this paper, we study personal data as contributions to a public good in light of the benefits and costs of sharing such information. More precisely, we randomize users of a non-profit online education platform into treatments with or without salient reference to the public benefit of sharing personal data on the platform, and with or without additional reference to privacy protection. By investigating whether salience of public benefits and reduced perceived privacy costs can mitigate underprovision of personal data, we address the individual-specific as well as privacy-sensitive nature of this new contribution type. Studying such personal data contributions is important since with scarce information on users, the public good that fits society's needs best cannot be provided.

We test the importance of emphasizing public benefits of personal data sharing and privacy protection in a natural field experiment (Harrison and List 2004) on one of Germany's largest massive open online course (MOOC) platforms. Our intervention aims at increasing the stock of personal data available to the non-profit platform, our public good provider, such that the platform can target the particular needs of its users with the best fitting services to facilitate learning on the platform, for example, via planning prompts (Andor et al. 2018).[5] At baseline, only 48% of users have any entry in their profile with the mean user having 2.6 out of 11 profile categories filled. To increase personal data sharing, we implement pop-up messages in four courses on the platform. Besides a control pop-up message (*Control*), in one treatment, we increase the salience of the public benefit for the whole user community when contributing personal data (*Benefit*). In a further treatment, we additionally highlight data protection standards, thereby reducing potentially overestimated privacy costs (*Benefit + Cost*). Our experimental design allows us to investigate whether the privacy-sensitivity of personal data attenuates the effectiveness of attempts to increase personal data contributions.

[4]See Acquisti et al. (2016) and Tucker (2015) for comprehensive reviews on the economics of privacy.

[5]Planning prompts prove effective only for a subgroup of learners in Andor et al. (2018), and the platform can only target this subgroup adequately when having sufficient information to identify this user subgroup.

Overall, our treatments of interest increase both the quantity, i.e, the amount, and the quality, i.e, the diversity in disclosed content, of contributed personal data. Salient public benefits significantly boost profile completeness by 5.3% compared to the control group.[6] If combined with emphasis on limited privacy costs, this effect increases to 6.4%. Both effects are sizeable given that more users in the control group react to the pop-up by following the link to their profile compared with users that receive the longer messages highlighting the public benefit of data contribution and privacy protection. While we find no clear evidence for treatment effects on the extensive margin, i.e., whether users have any filled profile entry, on average, we do observe an increase in having filled any profile entry which is rated as privacy sensitive. These results imply that internalizing public benefits matters for personal data contributions and that neglecting the privacy-sensitivity nature of personal data may attenuate the effectiveness of attempts to mitigate underprovision. Furthermore, the type of users who contribute their personal data changes significantly, especially in *Benefit + Cost*, the combined treatment. For instance, more senior employees but also younger users and more females disclose personal data, which makes the contribution basis more diverse and may thus allow to target educational services more precisely.

Our paper relates and contributes to the literature on mitigating underprovision of public goods in two ways. First, we gauge whether using insights on how to enhance individual-specific contributions to public goods carries over to personal data as contributions. Previous research studies feedback giving and knowledge sharing as forms of individual-specific contributions to a public good, for example, on online platforms such as eBay or MovieLens, a movie recommendation platform. Results by Cabral and Li (2015) suggest that the underprovision of feedback on eBay cannot successfully be tackled with monetary incentives. In contrast, behaviorally motivated interventions appear more successful in mitigating underprovision. For instance, reputation systems in Bolton et al. (2004) enhance feedback provision but do not internalize all external effects in full.[7]

[6]Whenever we refer to an effect size (in %) in this paper, we mean the additional treatment effect relative to the average (post-intervention) level in *Control*. This means that we divide the size of the treatment dummies by the size of the regression constant.

[7]With larger social distance, which may be particularly relevant on online platforms, underprovision of feedback worsens (Bolton et al. Forthcoming).

Moreover, social comparisons such as knowing that others provide feedback prevail effective in increasing feedback contributions (Chen et al. 2010). In contrast, according to Ling et al. (2005), only emphasizing the uniqueness of one's individual-specific feedback increases contributions, while highlighting public and personal benefits has the opposite effect. With respect to knowledge as an individual-specific contribution to a public good as on Wikipedia, results show that a combination of private and public benefits (Chen et al. 2019) as well as a large number of beneficiaries (Zhang and Zhu 2011) are drivers for contributions to the public information good.[8] Building on these insights, we implement behaviorally-informed interventions in a field experiment, which aim at increasing a new form of individual-specific contribution to a public good, namely personal data. In particular, we increase the salience (Bordalo et al. 2013; Chetty et al. 2009) of the public benefit when contributing personal information to a public education good.

Second, we contribute to research in the domain of privacy by investigating the effect that privacy sensitivity of personal data has on data provision. Research in this domain has so far focused on pricing or sharing personal data under varying data protection standards in other than public-benefit-enhancing settings.[9] While we study the role of privacy in a setting in which personal data are not sold for profit but serve the common good, the literature on privacy provides important indications for how people react to privacy salience. For one thing, it suggests that contextual cues affect the sharing of personal information (John et al. 2011; Tsai et al. 2011). For another, it shows that salience of privacy standards rather than the actual comprehensiveness of privacy protection appears important when individuals decide about sharing personal information (Marreiros et al. 2017; Tucker 2014).[10] Since not taking privacy concerns into account may result in over-

[8]With respect to laboratory evidence on the relationship between group size and public good provision, early results, as reviewed by Ledyard (1995), finds ambiguous results. In contrast, Andreoni (2007) reports that doubling the number of beneficiaries increases contributions but not by the same amount. Diederich et al. (2016) find a positive effect of group size in a linear public good game with a large, heterogeneous subject pool. Goeree et al. (2002) also estimate a positive relationship. Wang and Zudenkova (2016) claim that there is a discontinuous relationship between contributions to public goods and group size with the relationship being positive for small groups.

[9]Regrading pricing privacy, Benndorf and Normann (2018), Beresford et al. (2012), Jentzsch et al. (2012), and Tsai et al. (2011) try to elicit a monetary value of privacy. Feri et al. (2016) show that some subjects react to the risk of privacy breaches.

[10]When confronted with information about online companies' privacy policies, subjects in Marreiros et al. (2017) are less willing to share personal information independent of whether the information regarding companies' privacy protection standards are positive or negative. With respect to privacy in advertising,

seeing side effects of data sharing (Ackfeld and Güth 2019), our experiment targets the salience of data protection and hence privacy costs, which may otherwise attenuate the effect of our public benefit salience intervention. Our results highlight that additionally taking privacy into account when trying to increase contributions to a public good in form of privacy-sensitive personal data may increase the efficacy of such attempts.

The remainder of this paper is structured as follows. Section 4.2 describes the data and the experimental design. Our empirical strategy is outlined in Section 4.3. Section 4.4 presents the experimental results and Section 4.5 concludes.

4.2 Experimental set-up

4.2.1 Online platform environment

We conduct our field experiment on one of the biggest German massive open online course (MOOC) platforms with more than 200,000 users, openHPI, which offers free online courses on computer science as well as information and communication technology for beginners and experts either in English or German. Particularly, we implement our experiment in four courses offered between September 2019 and February 2020, namely "Network virtualization - from simple to cloud", "Introduction to successful remote teamwork", "The technology that has changed the world - 50 years of internet", and "Data engineering and data science".[11] While slightly different in structure, all courses consist of video lectures and individual or group assignments. Moreover, all courses use the same interface and have the same requirements to earn certificates for validating one's participation.[12]

Tucker (2014) shows that shifting the perception of but not the actual control over personal profile information on Facebook raises the willingness to click on personalized ads. While the data Tucker (2014) uses stem from an awareness campaign of a non-profit organization, the intervention is still used to generate higher revenues for an external party.

[11]We pre-registered to conduct our experiment also in the course "Human-Centered Design: Building and Testing Prototypes". However, since another experiment took place in this course, we refrained from additionally implementing our experiment there.

[12]In most courses, participants can earn a "Confirmation of Participation" if accessing 50% of the material. When achieving 50% of points in the assignments and the final exam, participants receive a "Record of Achievement". While the course material can also be accessed after the scheduled course dates, graded exercises and tests are no longer available afterwards.

A user profile is automatically created when a new user registers on the platform. Registration is necessary for enrolling in courses. It requires the real name[13] of the user to be printed on course certificates and an email address for verification and to receive course alerts. Besides these required fields, users can voluntarily provide the following information in their profiles: date of birth, company affiliation, career status, highest educational degree, professional experience, professional position, city, gender, and country.[14] Shortly prior to our intervention, two more profile categories were introduced, namely main motivation for taking courses on the platform and regular computer usage, which replaced one old category.[15] We use these new profile categories on the one hand to rationalize the appearance of our interventional pop-up message in courses, and on the other hand to provide a reason for a profile review also for those users who have already filled their profile completely before the intervention. The new categories were published eight days before the start of the first treated course. Hence, it is unlikely that participants have encountered the new categories before being directed to their user profiles via our interventions.[16]

4.2.2 Experimental design

We implement simple pop-up messages in courses, which enrolled users see once when accessing the material of the second or a later course week.[17] By implementing our interventions in courses, we can make sure that users likely access the online education platform during our intervention period and receive the intervention at a similar point of time in terms of platform activity. Two workdays before the intervention (days 5-

[13]While users could in principle use fake names, they would thereby eliminate their prospect to receive a personalized certificate, which they can use, for example, in job applications. This makes the use a fake names a minor concern.

[14]Additionally, users can define a display name as a pseudonym in their profile, which is used in the course forum. However, this does not contain any relevant, real-world information about users and is therefore disregarded in our analysis.

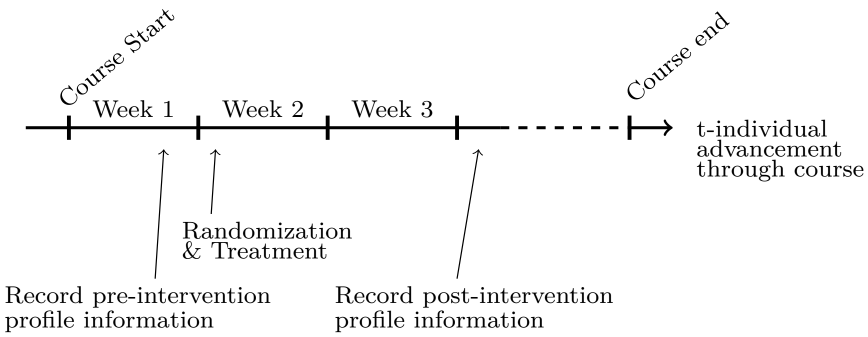
[15]The replaced category was also related to computer usage but contained less distinguishable inputs in terms of content.

[16]Only 5.8% of users in our sample updated their profiles independently before seeing the intervention pop-up. These updates are captured by the pre-intervention profile entries. The different time spans between the publishing date and a course's starting date are captured by course dummies in regressions.

[17]New course material is published every week. In principle, in the third week of the course, users could directly open the material of the third week without having accessed the second week's material. Nonetheless, they would see the pop-up.

6 of the course), we record user profile information of all enrolled course participants. More precisely, we measure which user filled which profile entries, and if entries are filled, by which content. This serves as our baseline, pre-intervention profile filling level. We compare this level with the grade of profile filling 21-22 days after course start, i.e., 14-15 days after our intervention. Hence, course participants have two weeks to edit their profiles in response to the intervention. Figure 4.1 graphical represents the timeline of the experiment. Since participants do not necessarily have to access the course material in a specific week and may start the courses one week later and catch up until the course ends, collecting post-intervention data after two weeks allows us to also have those users in our sample, who lack slightly but not too far behind in course progress.[18]

Figure 4.1: Timeline of the experiment



By design, we exclude users who do not make it to the material of the second course week.[19] First, this is because the first week's material is already full of information which would either risk our intervention to be overseen or risk to cause the overseeing of important course-related information.[20] Second, when aiming at improving the public good of online education in line with the needs of its users based on extended user information, excluding only marginally interested users is reasonable since the information and wants of rather uninterested users do not necessarily need to be represented in the improved services of the platform. Platform improvement based on extended information availability is meant to benefit those users with a genuine interest in courses but difficulties

[18]11.0% of users in our treated sample access the course the first time later than seven days after course start.

[19]Approximately one third of enrolled course participants reaches the required material.

[20]For example, in one of the courses, there is a planing prompt pop-up when participants access the course material the first time, which would stand in conflict with our treatment pop-up.

with course completion, for example, due to time-inconsistency or a lack of course-related knowledge.

We randomly assign users, active in the second course week, into three treatment groups. Randomization takes place via platform-wide user IDs. Due to technical reasons, we have to exclude users who access the course material exclusively via the mobile app.[21] If a user is enrolled in more than one course, she also receives the intervention more than once, but we count her only in the chronologically first course.

Table 4.1: Wording of treatments

Control	**Benefit**	**Benefit + Cost**
Dear Learner, We have updated our profile categories. Please take a moment to complete your profile.	Dear Learner, We have updated our profile categories. Please take a moment to complete your profile. By providing your information, you support openHPI in improving its free online education services and the learning experience for the whole openHPI community.	Dear Learner, We have updated our profile categories. Please take a moment to complete your profile. By providing your information, you support openHPI in improving its free online education services and the learning experience for the whole openHPI community. Your profile will only be visible to you and the openHPI team but not to other openHPI users. Your data will only be used for research and platform improvement in accordance with our data protection standards.

Notes: The words "data protection" in *Benefit + Cost* contain a link to the privacy protection guidelines of the platform. All treatments include a link to the user profile at the end.

Our treatments are implemented on the online learning platform as simple pop-up messages. Table 4.1 shows the different treatment texts and Figure 4.A.1 in the Appendix displays a screenshot of the most comprehensive pop-up. Pop-up messages in all

[21]By excluding users, who access the material exclusively via the mobile app, we lose 509 potential observations compared to 6155 treated users in the final sample.

treatments, including a *Control* treatment, contain the following text together with a link to the individual user profile: "*Dear Learner, We have updated our profile categories. Please take a moment to complete your profile.*" The *Control* group ensures that we can distinguish a pure reminder effect of the pop-up message from effects due to salience of public benefits and overestimated privacy costs. In the *Benefit* treatment, the standard text is extended by a hint to the public benefit that providing personal information can have for the whole user community. It reads: "*By providing your information, you support openHPI in improving its free online education services and the learning experience for the whole openHPI community.*" In the *Benefit + Cost* treatment, a remark is added to this statement emphasizing privacy protection standards, particularly who has access to the shared information: "*Your profile will only be visible to you and the openHPI team but not to other openHPI users. Your data will only be used for research and platform improvement in accordance with our data protection standards.*" The reference to data protection includes a link to the data protection webpage.[22]

By extending the *Control* text rather than using a similarly long but irrelevant text, we give the *Control* condition an overproportionally high chance that users read its text until the end and click on the link to their profile. In light of convex effort cost of text reading time (Augenblick et al. 2015), fewer users may finish reading the longer treatment texts in *Benefit* and especially *Benefit + Cost*. Hence, our design speaks against finding corresponding treatment effects and therefore even strengthens any findings.

4.2.3 Hypotheses

Following results by Andreoni (2007) and Zhang and Zhu (2011) in the domains of monetary and knowledge contributions to public goods, we hypothesize that highlighting the public benefit of personal data contributions in the *Benefit* treatment increases personal data contributions since the perception of the number of beneficiaries of the contribution rises. In the *Control* treatment, such public benefits may not be known or may not be sufficiently salient (Bordalo et al. 2013; Chetty et al. 2009) so they are not taken into

[22]The link opens a new browser tab with the data protection guidelines. Opening another tab implies that we distract users in the *Benefit + Cost* treatment from editing their entries on the profile page. This diminishes the chance to find a treatment effect in *Benefit + Cost* and thus strengthens any findings.

account by users when deciding about sharing personal data with the platform.

Hypothesis 1: Emphasizing the public *Benefit* of contribution increases personal data contributions relative to *Control*.

Since we study a potentially privacy-sensitive contribution to a public good, the positive effect of highlighting the number of beneficiaries on information contribution may be attenuated by privacy concerns. If this is true, we expect personal data contributions to rise more strongly if the public benefit is paired with a statement clarifying how data is used in the *Benefit + Cost* treatment, thereby reducing potential misperceptions of data sharing standards and increasing the salience thereof. Note that the wording of the privacy protection statement is the same as in the platform's data protection guidelines, to which users have to consent during registration. Hence, the *Benefit + Cost* treatment only adds information which should at least implicitly be known by users.

Hypothesis 2: Additionally emphasizing data protection standards in the *Benefit + Cost* treatment further increases personal data contributions relative to the *Benefit* treatment.

4.3 Empirical strategy

We estimate effects of our treatment dummies on post-intervention information disclosure, controlling for the initial disclosure level:

$$y_{it} = \beta_0 + T'\beta_1 + \beta_2 y_{it-1} + X'\beta_3 + \epsilon_{it} \tag{1}$$

for individual i with T being a vector of treatment dummies $T_1 = Benefit$ and $T2 = Benefit + Cost$ and with X being a matrix of several control variables. y_{it-1} are the pre-treatment outcomes.[23]

[23] The pre-analysis plan included a typo in the equation, i.e., multiplication between the betas. We are not interested in heterogeneous effects based on pre-intervention entries in the main analysis as the pre-analysis plan clearly indicates.

As main dependent variables y_{it}, we focus on 1) the extensive margin, i.e., whether any profile category is filled after the treatment intervention, 2) the intensive margin, i.e., how many profile categories are filled, and 3) whether users click on the link to their profile. Clicking on the profile link in the pop-up corresponds to an intention to provide personal data in our experiment. Because there is no baseline for clicking on the link, Equation (1) simplifies to

$$y_i = \beta_0 + T'\beta_1 + X'\beta_2 + \epsilon_i. \tag{2}$$

As secondary outcomes, we look at those categories, which treatment-blind students assistants rate as more or less privacy-sensitive.[24] Furthermore, we investigate the number of deleted and added entries separately, and look at previously filled but updated categories. Reviewing categories may be of importance if, for example, IT proficiency or work experience has increased since the last revision of the profile.

As controls X, we include several context-related variables into our regressions.[25] First, we add course fixed-effects. These dummies do not only capture differences between courses but also different durations between the respective course start date and the publishing of the new profiles categories. Second, we use the enrollment date and the first show-up in the course after course start to control for self-organization skills and the level of commitment to the course, the latter included as a dummy for course access no later than that of the median user. Third, we include a dummy variable for whether the course is the first course the user takes on the platform in order to account for experience with and potential trust towards the platform. Fourth, we control for different reactions between different nationalities, for example, with respect to privacy concerns (Bellman

[24]Four student assistants rate each profile category on a scale from 1 = "not at all privacy-sensitive" to 7 = "totally privacy-sensitive". They are informed that they are rating user profile categories from an online education platform and that these data are only shared with the platform but not with other users, exactly as it is the case on the platform. We sum up the student assistants' ratings for each category and calculate the average privacy-sensitivity. We call a category privacy sensitive if its mean rating is higher than the mean rating over all categories. The sensitive categories are one's company affiliation, the highest educational degree, professional experience, and the current job position.

[25]We pre-registered two more control variables: a dummy for whether a user allows web-tracking and a dummy for whether the user clicks on the link to privacy protection guidelines in the pop-up. However, web-tracking was not recorded correctly in all courses and only three users clicked on the profile link so we refrain from including these controls.

et al. 2004; IBM 2018) by including a dummy for course access from Germany, measured by the browser location from which logged in users main access the platform.

4.4 Results

4.4.1 Descriptive statistics

This section reports descriptive statistics of our baseline pre-intervention sample and additionally allows to check whether randomization into treatments was successful. First, we document the pre-intervention outcomes for all treatment groups in Panel (A) of Table 4.2 and with more content detail in Table 4.A.4 in the Appendix. 48.0% of users have at least one entry in their profile before the intervention, and the average profile includes 2.6 filled categories out of 11. On the entry level, the share of missing information before the intervention is at least 61.2%. For the two newly introduced categories, "main motivation" and "regular computer use", this share is much higher, i.e., 94.9% and 94.7%, respectively. χ^2-tests on the entry level do not detect any statistically significant differences across the treatment groups in terms of the share of missing values pre-intervention (all $p > 0.128$).[26]

Second, we report the pre-intervention sample composition in Panel (B) of Table 4.2. For 18.9% of users in our sample, the course is the first course they take on the platform. 87.0% access the course from a browser located in Germany. This high share is not surprising given that three out of four courses in our sample are taught in German. 57.3% of users participate in the course "Data Engineering & Data Science", 18.8% in "50 Years of Internet", and 19.2% in "Network Virtualization". Only 5.2% of users participate in the English-speaking course "International Teams".

Third, Panel (C) of Table 4.2 describes users' pre-intervention course behavior and related course information, and confirms that users across treatments are similar in these domains. On average, users enroll 53.7 days prior to course start and begin working on the material 2.7 days after the course start. Since our sample only includes users who are still active in the second course week, we observe a high level of first-week activity: Users access 92.3% of the material and complete 82.1% of all self-tests in the first course week.

[26] The entry with the largest difference between treatments is company affiliation ($p = 0.128$). However, this is not surprising given that users in our sample report 563 different affiliations. All other differences are insignificant with a p-value of at least 0.240.

Table 4.2: Pre-intervention course activity and characteristics overall and by treatment

	Pooled	Control	Benefit	Benefit + Cost	p-value
Panel A: Pre-intervention profile status					
Share of users with any entries	**0.480**	0.480	0.478	0.481	0.974
(extensive margin)	**(0.500)**	(0.500)	(0.500)	(0.500)	
Number of profile entries per users	**2.560**	2.604	2.525	2.542	0.924
(intensive margin)	**(3.284)**	(3.304)	(3.282)	(3.265)	
Panel B: Pre-intervention sample composition					
Course is 1st course	**0.189**	0.184	0.189	0.1958	0.686
	(0.392)	(0.388)	(0.391)	(0.396)	
Course accessed from Germany	**0.870**	0.875	0.870	0.865	0.672
	(0.336)	(0.331)	(0.336)	(0.341)	
Course “International Teams”	**0.052**	0.051	0.050	0.053	0.886
	(0.221)	(0.220)	(0.218)	(0.225)	
Course “50 Years of Internet”	**0.188**	0.184	0.188	0.192	0.778
	(0.391)	(0.387)	(0.391)	(0.394)	
Course “Data Science & Engineering”	**0.573**	0.576	0.573	0.569	0.894
	(0.495)	(0.494)	(0.495)	(0.495)	
Panel C: Pre-intervention course behavior					
Days enrolled after start	**-53.7**	-53.2	-56.3	-51.7	0.111
	(83.9)	(84.5)	(86.0)	(81.0)	
Days until first action in course	**2.7**	2.7	2.6	2.7	0.701
	(3.9)	(3.9)	(3.9)	(4.0)	
% material accessed in first week	**0.923**	0.922	0.926	0.921	0.460
	(0.207)	(0.207)	(0.202)	(0.211)	
% self-test solved in first week	**0.821**	0.823	0.8263	0.815	0.666
	(0.308)	(0.304)	(0.302)	(0.319)	
N	6155	2052	2060	2043	

Notes: Mean values reported with standard deviations in parentheses. Participants enrolled in more than one course are only included in the earlier course. p-values stem from χ^2-tests of independence of frequencies between treatments.

In sum, for all pre-intervention characteristics, we find no statistically or economically significant differences between treatments. All p-values from $\chi 2$-tests for equal distribution over all treatments exceed the 10% significance level. Thus, randomization into treatment was successful.

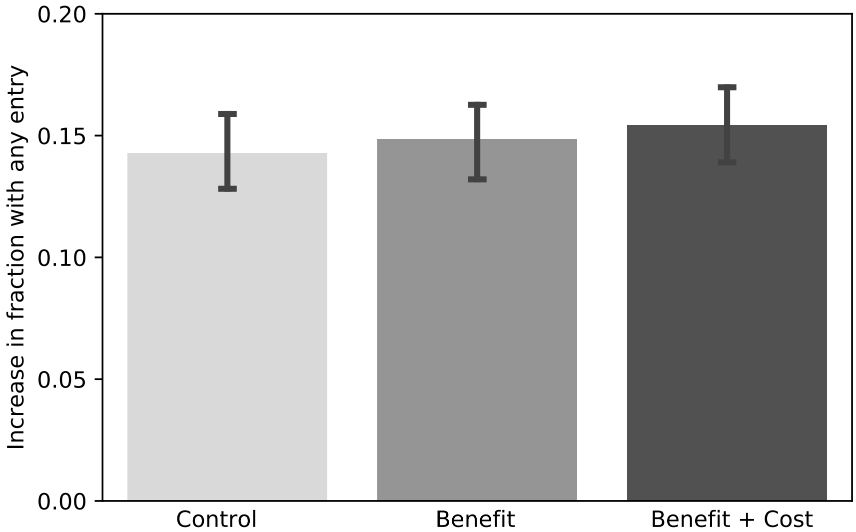
4.4.2 Main results

In this section, we investigate treatment effects on our three main outcomes of interest: 1) the extensive margin, i.e., whether any profile fields are filled after the intervention, 2) the intensive margin, i.e., how many profile fields are filled after the intervention, and 3) whether users click on the link to their profile in the pop-up.

For the first outcome, the extensive margin, i.e., whether a user has any filled profile entry, we do not find differences between *Control*, *Benefit*, and *Benefit + Cost*. As displayed in Figure 4.2, the number of users having any profile entry increases similarly strongly in all conditions (all $p > 0.305$, ranksum test). OLS regressions controlling for the pre-intervention profile status in columns (1) and (2) of Table 4.3, as specified in Equation (1), confirm this finding independent of whether course fixed effects and other covariates are included. Nonetheless, the 95% confidence intervals can only rule out effect sizes beyond [-5.2%; 8.8%] for the *Benefit* treatment and beyond [-2.7; 11.3%] for the *Benefit + Cost* treatment relative to the share of users with any profile entry post-intervention in *Control*. This means that our treatment effects are rather imprecisely estimated than clear zero effects. Thus, at the extensive margin, we conclude that treatment effects point into the hypothesized directions, but we do not find statistical support for our hypotheses that emphasizing the public benefit of contribution, especially if paired with salient privacy protection, increases personal data contributions.

For our second main outcome, the intensive margin, we detect a substantial and statistically significant increase in the amount of profile entries as depicted in Figure 4.3. While the average user has 2.6 profile entries before the intervention, we observe significantly more entries after the intervention with 3.9 entries pooled over all treatments ($p < 0.001$, paired t-test). This increase differs significantly between treatments. In line with our hypotheses, the increases in profile entries in *Benefit* and *Benefit + Cost* are statisti-

Figure 4.2: Extensive Margin: Increase in fraction of users with any profile entry by treatment



Notes: Figure displays the increase in the fraction of users with any profile entry filled from pre to post and corresponding 95% confidence intervals.

cally significantly larger than in the *Control* group ($p = 0.017$ and $p = 0.005$, t-test).[27] The largest increase appears in the *Benefit + Cost* treatment, in which the number of filled profile fields rises from 2.5 entries before the intervention to 4.0 entries afterwards. Controlling for pre-intervention profile completion in an OLS regression, as specified in Equation (1), in column (3) of Table 4.3, we obtain positive point estimates for both the *Benefit* and *Benefit + Cost* treatment dummies significant at the 5% and 1% level, respectively. Precisely, users in *Benefit* contribute on average 0.18 additional profile entries independent of their pre-intervention profile status compared to *Control*. In *Benefit + Cost*, users even disclose 0.22 additional entries. In other words, every fifth treated participant in *Benefit* and *Benefit + Cost* fills out one more empty profile category than the *Control* group participants do ex post. While the two treatment coefficients are not statistically different from each other in column (4) with controls included ($p = 0.653$), the larger increase in *Benefit + Cost* indicates that emphasizing privacy protection along

[27]The same conclusion holds if only inspecting profile entries which were already part of the profile in the past, i.e., all entries except motivation to take courses on the platform and computer usage, as Figure 4.A.2 in the Appendix shows ($p = 0.030$ and $p = 0.014$, t-test).

Table 4.3: OLS regression results of main outcomes on treatment

	Any entry		Number of entries		Link clicked	
	(1)	(2)	(3)	(4)	(5)	(6)
Benefit	0.005	0.005	0.184**	0.186**	-0.052***	-0.052***
	(0.010)	(0.010)	(0.080)	(0.080)	(0.014)	(0.014)
Benefit + Cost	0.012	0.012	0.223***	0.224***	-0.044***	-0.044***
	(0.010)	(0.010)	(0.081)	(0.081)	(0.014)	(0.014)
Any Entry pre	0.715***	0.715***				
	(0.008)	(0.009)				
Entries pre			0.878***	0.879***		
			(0.008)	(0.008)		
Constant	0.280***	0.258***	3.469***	3.210***	0.722***	0.565***
	(0.010)	(0.018)	(0.056)	(0.134)	(0.010)	(0.025)
Controls	No	Yes	No	Yes	No	Yes
N	6155	6155	6155	6155	6155	6155
R2	0.55	0.55	0.55	0.55	0.00	0.02

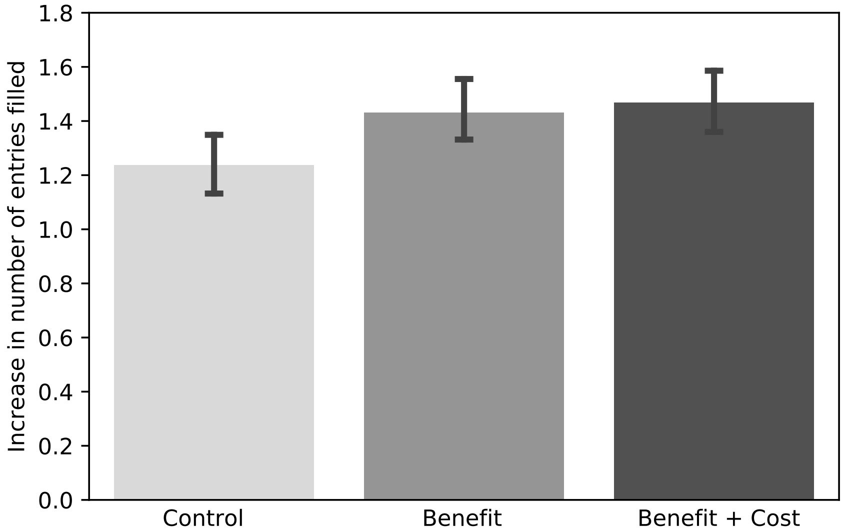
Notes: Robust standard errors in parentheses. Controls include dummies for the courses "International Teams", "50 Years of Internet", and "Data Science & Engineering", whether the course is the first course on the platform, whether the course is accessed from Germany, and whether it is accessed earlier than the median access, as well as the day of enrollment relative to the course start. "Entries pre" in columns (3) and (4) are transformed to a mean of zero. * p<0.10, ** p<0.05, *** p<0.01.

with the public benefit may generate somewhat more personal data contributions. In short, for people with prior profile entries, we find evidence for Hypothesis 1 that salience of the public benefit can encourage users to disclose more personal data. Albeit lacking statistical significance, the point estimates also provide suggestive directional evidence for Hypothesis 2, stating that such effects may be attenuated if privacy concerns are not taken into account.

In Table 4.A.3 in the Appendix and the discussion thereof, we report further results on the intensive margin with respect to heterogeneous reactions to treatments by different user subgroups. We do not find any statistically significant heterogeneous effects and therefore conclude that any version of the pop-up prompting users to complete their profile attracts information from all subgroups alike.

Our third main outcome, clicking on the profile link, may indicate an intention to contribute personal data. Surprisingly, significantly more users click on the link in the

Figure 4.3: Intensive margin: Increase in number of profile entries by treatment

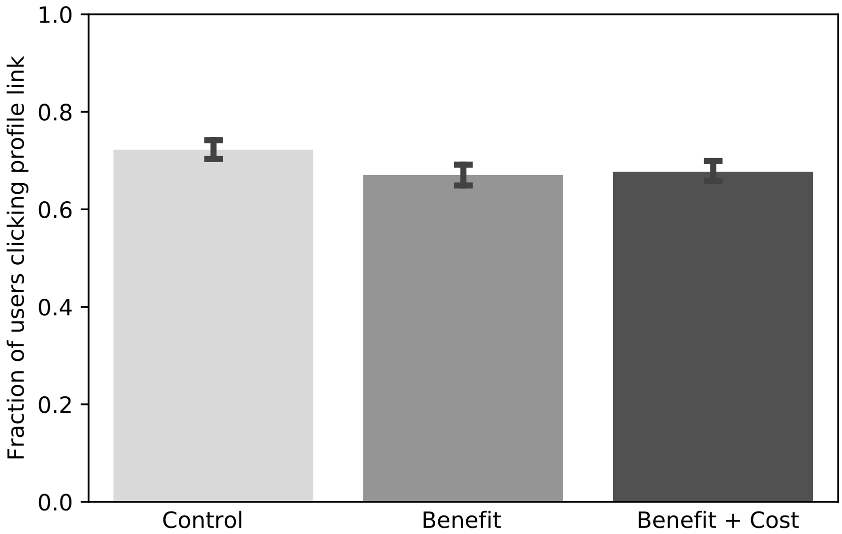


Notes: Figure displays the increase in the number of profile entries filled from pre to post and corresponding 95% confidence intervals.

Control group than in the *Benefit* and *Benefit + Cost* treatments as Figure 4.4 displays ($p < 0.001$ and $p = 0.002$, ranksum test). Concretely, 72.2 % of *Control* group users click on the link while 67.0% and 67.7% do so in *Benefit* and *Benefit + Cost*, respectively. The same picture prevails if investigating treatment effects in an OLS regression framework without and with control variables in columns (5) and (6) of Table 4.3, with treatment effects corresponding to decreases of 7.2% and 6.1%, respectively, in *Benefit* and *Benefit + Cost* relative to the baseline.

While the higher share of users clicking on the link in the *Control* group is surprising at first glance, it is well in line with convex effort cost of text reading time (Augenblick et al. 2015). The *Control* group text is the shortest so users may be more likely to read it to the end. The *Benefit* and *Benefit + Cost* texts are longer by one or three sentences, respectively. Therefore, users may not read it to the end and are thus less likely to reach the button with the profile link. In light of convex effort cost, the fraction of users clicking on the link should be the lowest in the *Benefit + Cost* treatment which has the longest pop-up text. However, this is not what we observe. Instead, users in the *Benefit + Cost* exhibit an equally high intention to contribute their data than users in *Benefit* ($p = 0.607$,

Figure 4.4: Participants who click on profile link by treatment



Notes: Figure displays fraction clicking on profile link and corresponding 95% confidence intervals.

ranksum test). This may indicate that higher effort costs of reading the longer text in *Benefit + Cost* seem to be counteracted by reduced privacy cost of information sharing due to the privacy protection statement in the *Benefit + Cost* treatment.

Overall, our main results suggest that at the intensive margin, pop-up wording matters. Relative to the number of post-intervention entries in the *Control* group, the *Benefit* treatment and the *Benefit + Cost* treatment increase available user information by 5.3% and 6.4%, respectively, for those who had some entries prior to the intervention.[28] At the extensive margin, the prompt wording does not affect users' willingness to share any information significantly, but also suggests effects in the hypothesized directions. While more users in the *Control* group show an intention to edit their profile by clicking on the link more often, they do not eventually provide more information. In contrast, users in the *Benefit* and *Benefit + Cost* treatments are less likely to click on the pop-up link, but if they do, they seem to do so with a higher intention to actually provide data. This makes up for the lower clicking rate and indicates that reference to public benefits and

[28]Remarkably, given that we observe a large increase from pre to post overall, a simple reminder message by itself seems very effective to make users provide any personal details. However, this result is only correlational and reminders are not the focus of our investigation.

potentially overestimated privacy costs impacts people's actual willingness to contribute personal data.

4.4.3 Further outcomes: Types of changes

There may be different types of profile changes masked by the main outcomes. First, users may react differently to treatments depending on how privacy-sensitive they perceive the profile categories. Therefore, we study treatment effects both on the intensive and the extensive margin regarding the sharing of sensitive and insensitive personal information, respectively. Second, the intervention may induce changes in different directions. Hence we analyze the effect on profile extensions, profile reductions, and updates of profile entries separately.

First, not all personal data are equally sensitive. Therefore, we let student assistants rate the profile categories by their privacy-sensitivity and count an entry as sensitive if the mean rating of a category is higher than the mean of the individual mean ratings.[29] We find significant increases both in the number of insensitive and sensitive profile fields in the *Benefit* and *Benefit + Cost* treatments compared to *Control* as columns (1) and (2) of Table 4.4 reveal. While point estimates for *Benefit* and *Benefit + Cost* look similar for both entry types ($p = 0.682$ and $p = 0.937$, respectively), the underlying effect sizes for sensitive and insensitive entries differ in magnitude. In particular, there are 5.1% and 6.1% sensitive entries more in *Benefit* and *Benefit + Cost* than ex post for the mean user in *Control*, while the additional treatments effects are only 3.5% and 3.7% for insensitive categories.[30] This suggests that the emphasis on the public benefit, especially if paired with reference to privacy protection, increases the willingness to share a larger amount not only of rather insensitive but also of sensitive personal information. Table 4.A.1 in the Appendix confirms this result including control variables. Consequently, we find strong

[29]The sensitive categories are company affiliation, highest educational degree, professional experience, and job position. The insensitive categories are city and country, age, gender, current career status, motivation for joining the platform, and computer usage. In fact, the categories rated as the most sensitive are also those with the highest number of missing values in the pre-intervention sample when ignoring the fields with the most potential outcomes to choose from besides affiliation, namely country and city.

[30]Note that less categories are rated as sensitive than insensitive, namely four relative to seven. Hence, even given a higher constant in column (2) than in column (1), there is more scope for improvement for insensitive categories.

evidence in favor of Hypothesis 1 in the domain of both sensitive and insensitive profile categories.

The inspection on the extensive margin, i.e., of having any sensitive or insensitive entry in the profile, in Table 4.A.2 in the Appendix moreover reveals that there are significantly more platform users having any sensitive entry in their profile if reference to the public benefit or public benefit and privacy protection is added in *Benefit* and *Benefit + Cost* compared to *Control*. For insensitive entries, there is no such difference. This means that the *Control* message performs similarly well in motivating new users to fill in any insensitive profile field as the *Benefit* and *Benefit + Cost* messages. In contrast, *Benefit* and *Benefit + Cost* outperform the *Control* message to make new users contribute any sensitive information. Hence, enhancing the salience of the public benefit of contributing data with or without reference to data protection unveils sensitive information from new people, which enhances the availability of potentially rare information on the platform.

Table 4.4: OLS regression results of further outcomes by treatment

	Sensitive entries		Type of changes		
	Yes	No	Extensions	Deletions	Updates
	(1)	(2)	(3)	(4)	(5)
Benefit	0.060**	0.071*	0.185**	0.000	0.008
	(0.027)	(0.038)	(0.080)	(0.003)	(0.009)
Benefit + Cost	0.071**	0.074**	0.225***	0.002	0.011
	(0.028)	(0.038)	(0.081)	(0.003)	(0.009)
Entries pre	0.852***	1.011***	-0.119***	0.003***	0.019***
	(0.006)	(0.004)	(0.008)	(0.001)	(0.001)
Constant	1.167***	2.013***	1.293***	0.004**	0.035***
	(0.019)	(0.026)	(0.056)	(0.002)	(0.006)
N	6155	6155	6155	6155	6155
R2	0.64	0.77	0.02	0.01	0.05

Notes: The Table reports OLS regression results on the intensive margin. Robust standard errors in parentheses. "Entries pre" for sensitive and insensitive categories correspond to only those ex ante filled entries classified as sensitive or insensitive, respectively. All "Entries pre" are transformed to a mean of zero. * p<0.10, ** p<0.05, *** p<0.01.

Second, our results are driven by profile extensions. As Table 4.4 shows, the intervention triggered mostly profile extensions (column (3)) but nearly no deletions (column (4)) or content updates (column (5)). The treatment indicators in the regression on deletions in column (4) are close to zero and insignificant, and the constant is small in magnitude. This means that our intervention does not come along with an unintended side-effect of destroying information content. In contrast, effect sizes for extensions in column (3) look very similar to those of the intensive margin in the main analysis in column (4) of Table 4.3. We find that users extend their profile on average by 0.19 more profile entries in *Benefit* relative to *Control* and by 0.23 in *Benefit + Cost*. While the difference between the two treatment dummies is insignificant ($p = 0.636$), it provides further support that adding an emphasize on privacy protection if anything has a small positive effect on users' willingness to share more profile information. Hence, we again find evidence for Hypothesis 1 and directional support for Hypothesis 2 when studying profile extensions.

4.4.4 Shifts in the distribution of personal characteristics

In this section, we evaluate whether our intervention on the online education platform generates more diverse voluntarily provided information about platform users. Particularly, we investigate whether we do not only get more data from similar people, but also whether the characteristics that users share are different from those previously available.[31] Hence, we look at the quality of contributed personal data in contrast to the analysis regarding quantity in the previous sections.

For all profile categories, the distributions of pre- and post-intervention personal data content differ.[32] We quantify these distributional shifts via marginal effects from multinominal logit regressions. In the multinominal logit models, we exploit the panel structure of our data and introduce a dummy variable indicator for the post-intervention period. This indicator captures the shift in the distribution of personal characteristics before and after the intervention. The results are reported in Table 4.5. Marginal effects for the pooled sample can be found in Table 4.A.5 in the Appendix along with graphical depictions of the shifts in reported profile content from pre to post in Figure 4.A.3.

[31] We pre-registered to run analyses based on profile entry content, in particular to predict learning behavior in previous courses. However, since we do not have enough users in our treated sample for whom we also have data from previous courses, we cannot conduct this analysis. Therefore, we restrict our attention to the distribution of profile entry content.

[32] We refrain from studying the profile categories country, city, and organizational affiliation because this entries contain too many different realizations as outcomes.

Table 4.5: Marginal effects of multinominal logit regressions regarding pre-post shifts in profile content distributions by treatment

		Control		Benefit		Benefit + Cost	
Outcome	Category	Marg ef	Std er	Marg ef	Std er	Marg ef	Std er
position	department head	0.0080	0.009	0.0173	0.011	0.0288**	0.011
	intern	0.0043	0.004	-0.0027	0.007	-0.0131**	0.005
	project manager	0.0043	0.011	0.0131	0.013	0.0006	0.015
	team leader	-0.0032	0.009	-0.0031	0.010	-0.0002	0.010
	technician	-0.0134	0.012	-0.0247*	0.013	-0.0160	0.014
career status	academic researcher	0.0027	0.004	0.0004	0.005	0.0066	0.004
	other	-0.0008	0.007	0.0122	0.008	-0.0020	0.009
	professional	-0.0006	0.010	-0.0061	0.011	0.0092	0.011
	student	0.0012	0.006	-0.0018	0.006	-0.0044	0.005
	teacher	-0.0024	0.004	-0.0048	0.004	-0.0093**	0.004
professional life	more than 10 years	0.0114	0.010	0.0005	0.010	0.0227**	0.010
	up to 10 years	-0.0184**	0.008	-0.0085	0.008	-0.0099	0.009
	up to 5 years	0.0070	0.008	0.0079	0.008	-0.0128	0.008
highest degree	bachelor	0.0033	0.007	-0.0021	0.007	0.0023	0.007
	diplom	0.0381***	0.010	0.0397***	0.011	0.0672***	0.012
	high student	-0.0112	0.008	-0.0035	0.008	-0.0174**	0.008
	magister	0.0023	0.003	0.0095**	0.005	0.0037	0.004
	master	-0.0053	0.008	-0.0138	0.008	-0.0221***	0.008
	other	-0.0266***	0.007	-0.0383***	0.008	-0.0409***	0.009
	phd	-0.0007	0.006	0.0086	0.006	0.0072	0.006
gender	female	0.0197***	0.007	0.0087	0.008	0.0237***	0.008
	male	-0.0197***	0.007	-0.0087	0.008	-0.0237***	0.008
age group	<20	0.0034	0.002	0.0010	0.003	0.0060*	0.003
	20-29	0.0090**	0.004	0.0158***	0.006	0.0120**	0.005
	30-39	0.0070	0.006	0.0035	0.006	0.0032	0.006
	40-49	0.0055	0.006	-0.0152**	0.006	-0.0121**	0.006
	50-59	-0.0081	0.006	-0.0088	0.007	-0.0084	0.006
	60-69	-0.0098***	0.004	0.0003	0.005	-0.0033	0.005
	70+	-0.0070***	0.003	0.0035	0.004	0.0027	0.003

Notes: Table reports average marginal effects from multinominal logit regressions for each outcome in the leftmost column with standard errors clustered on user level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

For work-related characteristics, we see more senior users disclosing information, particularly in the *Benefit + Cost* treatment. For example, for job position, we see a 2.9 percentage points increase in users indicating they are department heads. This increase is driven by users in the *Benefit + Cost* treatment, and shifts the distribution away from interns and technicians in this treatment. In the same vain, fewer users report to be teachers as a profession. This effect is again driven by significant distributional shifts in the *Benefit + Cost* treatment. Moreover, the post-intervention distribution includes more users indicating more than 10 years of work experience rather than experience between 5 and 10 years compared to the pre-intervention distribution. While such effects may result from updating the profile information content, the treatment-level analysis indicates that the increase stems from users in the *Benefit + Cost* treatment while the decrease stems from *Control*.

Focusing on demographics, we observe shifts that point to a more diverse user group than the pre-intervention data suggest. We find a disproportionately strong increase in users reporting a diploma as their highest educational degree. This pre to post increase is significant in all treatments but particularly pronounced in *Benefit + Cost* with 6.7 percentage points. The shift goes along with a significant decrease in users reporting a non-standard educational degree in all treatments, and additionally with significant decreases in users indicating a Master degree or still being in high school in *Benefit + Cost*. In *Benefit*, more users report a Magister degree. Moreover, after the intervention, a higher share of users indicates being female. With an increase of 2.4 percentage points, the shift is most pronounced in the *Benefit + Cost* treatment. Furthermore, we observe more younger users in our sample. Both the shares of users younger than 20 years and that of users in their twenties increase significantly after the intervention at the expense of users between 40 and 69. While the increase in users in their twenties is significant in all treatments, it differs by treatment which age groups are represented less in the post-intervention sample. In *Control*, we observe fewer people in their sixties or older, while in *Benefit* and *Benefit + Cost*, the main decrease occurs in the age group between 40 and 49.

For the new profile categories, which elicit motivation for taking courses on the platform and computer proficiency, we see large overall increases of the available information because very few participants had provided this information prior to the intervention.[33] While we find significant shifts in the content for both new categories, we refrain from interpreting these shifts due to the limited number of filled entries before the intervention.

[33]These categories are new, but were visible upon course start prior to the intervention. This means the pre-intervention distribution shows the motivation and computer expertise of participants who had filled these categories prior to the announcement.

Rather, it is worth noting that most users report a professional motivation (66%) and high or intermediate level of expertise in computer usage (50% and 45%).

Overall, we observe shifts in the distribution of voluntarily shared personal information between the pre- and post-intervention datasets, most notably in the *Benefit + Cost* treatment. This means that the intervention does not only increase the quantity of personal data contributions but also the quality since we observe not only more information of the same kind but different content distributions. The more diverse database after the intervention may help the public good provider to better tailor its services to fit the needs of all users. Finding the most pronounced shift in the *Benefit + Cost* treatment indicates that emphasizing privacy protection besides making the public benefit salient is most effective in generating diverse personal information donations.

4.5 Conclusion

In this paper, we study how to increase the provision of personal data contributions to a public good and address the individual-specific as well as privacy-sensitive nature of this new contribution type. In a field experiment on one of Germany's largest online education platform, we test whether emphasizing the public benefit - a behavioral intervention that has proven effective for other types of contributions (Andreoni 2007; Zhang and Zhu 2011) - also significantly increases users' willingness to contribute personal data. Furthermore, we investigate whether overestimated privacy costs attenuate potentially positive effects of a pure public benefit intervention.

We find that emphasizing the public benefit of sharing personal details significantly increases users' willingness to contribute more personal information. This effect is especially pronounced if privacy protection is made salient in addition to the pure public benefit, mitigating potentially overestimated privacy costs. The effects we find are substantial given that users click more often on the link to their profile when they see a brief control message rather than the longer treatment messages highlighting the public benefit of data contribution and privacy protection. While we do not find clear evidence that salience of public benefit and privacy protection can motivate users without any prior profile entry to disclose personal data in general, it induces them to start sharing personal details rated as privacy-sensitive. Furthermore, we find significant increases in the quality of provided information in terms of diversity of content, especially in the most comprehensive treatment which highlights public benefits and privacy protection jointly.

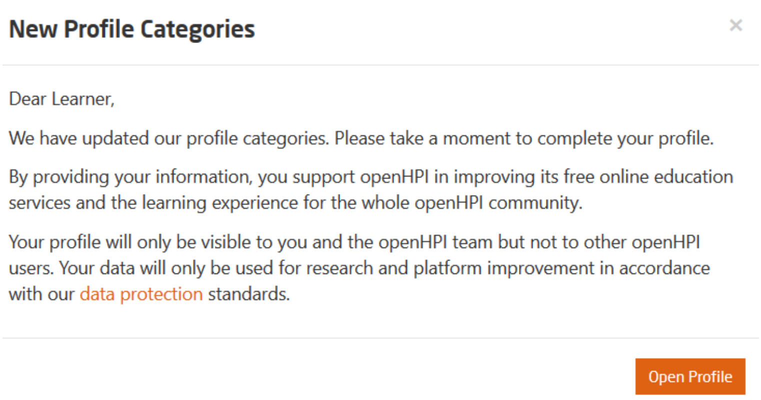
Our results provide confirming evidence regarding the debate on whether the size of beneficiaries indeed positively influences public good provision (Andreoni 2007; Chen et al. 2019; Diederich et al. 2016; Goeree et al. 2002; Ledyard 1995; Ling et al. 2005; Wang and Zudenkova 2016; Zhang and Zhu 2011). We find such an effect even on a fast-paced online platform using simple and inexpensive pop-up messages. Besides field evidence from Wikipedia in form of a massive disruption like its nationwide shut-down (Zhang and Zhu 2011) and, in combination with private benefit, via a sophisticated targeting mechanism by Chen et al. (2019), our results show that emphasis of public benefits works for a different individual-specific contribution type to a public good, namely personal data. Via simple and easy-to-implement pop-up messages highlighting public benefits, we generate sizable effects in the domain of user information provision.

Furthermore, our results imply that the privacy-sensitivity of the type of contribution needs to be taken into account when tackling underprovision of public goods most effectively. In the treatment, in which we do not only make the public benefit more salient but also the personal data protection standards, we consistently find stronger effects compare to the control group than when only the public benefit is made salient. Hence, in contrast to laboratory findings by Marreiros et al. (2017) but in line with evidence from illusory privacy protection on Facebook (Tucker 2014), emphasizing privacy protection seems to indeed positively influence personal data sharing in the field.

Overall, we conclude that reference to public benefits, especially if paired with reference to privacy protection, can help increase available information about online platform users and thereby may help the online education platform to improve their educational services for the social good. Making costs and benefits salient prevails to have sizeable effects both on the quantity and the quality of provided information. With more diverse and representative user information, the platform can target their public services more precisely. On a more general stance, our findings suggests that taking the privacy-sensitive nature of personal data as contributions into account can help digital public goods to overcome their particularly pronounced risk of underprovision.

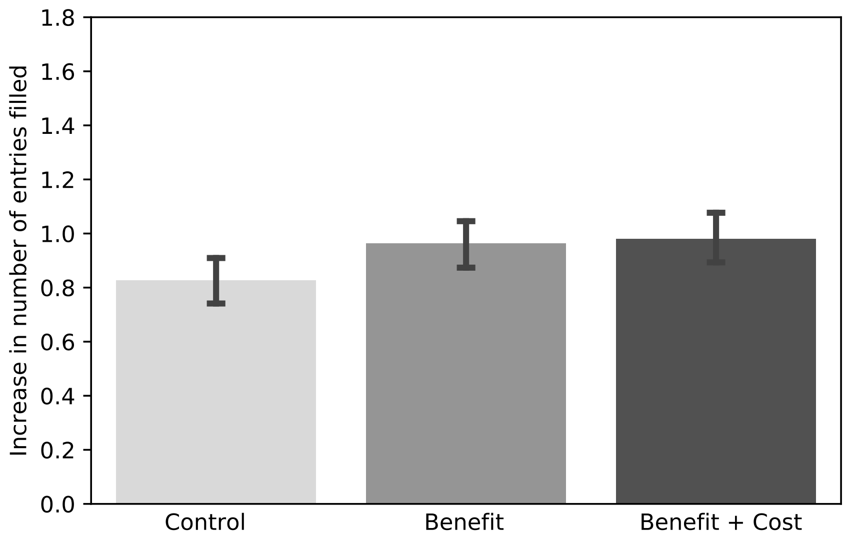
4.A Appendix: Additional results

Figure 4.A.1: Screenshot of interventional pop-up message



Notes: Benefit + Cost treatment displayed.

Figure 4.A.2: Increase in number of old profile entries by treatment



Notes: Figure displays the increase in the number of profile entries filled from pre to post and corresponding 95% confidence intervals. Old entries exclude most recently added profile fields "main motivation" and "regular computer use".

Heterogeneity on the intensive margin

Besides our main hypotheses, we check for heterogeneity between different groups of users defined by always available platform process data. First, treatment effects may be stronger for first-time users, i.e., user for whom the treated course is the first course they take on the MOOC platform. These users have no experience with the platform so they

Table 4.A.1: OLS regression results of further outcomes with controls by treatment

	Sensitive entries		Type of changes		
	Yes	No	Extensions	Deletions	Updates
	(1)	(2)	(3)	(4)	(5)
Benefit	0.060**	0.073*	0.186**	0.000	0.008
	(0.027)	(0.038)	(0.080)	(0.003)	(0.009)
Benefit + Cost	0.072***	0.070*	0.226***	0.002	0.012
	(0.028)	(0.038)	(0.081)	(0.003)	(0.009)
Entries pre	0.853***	1.026***	-0.118***	0.003***	0.019***
	(0.006)	(0.004)	(0.008)	(0.001)	(0.002)
Constant	1.103***	1.804***	1.030***	-0.000	0.001
	(0.047)	(0.060)	(0.134)	(0.005)	(0.012)
N	6155	6155	6155	6155	6155
R2	0.64	0.78	0.03	0.01	0.05

The Table reports OLS regression results on the intensive margin. Robust standard errors in parentheses. All specifications estimated with controls including dummies for the courses "International Teams", "50 Years of Internet", and "Data Science & Engineering", whether the course is the first course on the platform, whether the course is accessed from Germany, and whether it is accessed earlier than the median access, as well as the day of enrollment relative to the course start. "Entries pre" for sensitive and insensitive categories corresponds to only those ex ante filled entries classified as sensitive or insensitive, respectively. All "Entries pre" are transformed to a mean of zero. * p<0.10, ** p<0.05, *** p<0.01.

more likely underestimate the size of the user community and hence the public benefits of personal data contribution compared to users with previous course experience. Hence, first-time users may show a stronger reaction to the *Benefit* treatment. However, we find no support for such an effect. In an OLS regression reported in column (1) of Table 4.A.3, the interaction effect between first-time course taking and *Benefit* is insignificant and small in magnitude.

Besides the potentially underestimated public benefit, less experience with the platform may also mean that first-time users have less trust in the platform so they may be less willing to share sensitive personal data with the platform than experienced users. Mitigating this obfuscation may therefore additionally lead to stronger responses to the *Benefit + Cost* treatment for first-time compared to experienced users. While there is a sizeable interaction term of *Benefit + Cost* with first-time course taking, it is not distinguishable from the interaction with *Benefit* at conventional significance levels ($p = 0.193$).

Table 4.A.2: OLS regression results of disclosing any (in)sensitive entry on treatment

	Sensitive		Insensitive	
	(1)	(2)	(3)	(4)
Benefit	0.023**	0.023**	-0.007	-0.007
	(0.010)	(0.010)	(0.006)	(0.006)
Benefit + Cost	0.023**	0.023**	-0.007	-0.007
	(0.010)	(0.010)	(0.006)	(0.006)
Any Entry pre	0.795***	0.794***	0.918***	0.919***
	(0.006)	(0.007)	(0.005)	(0.006)
Constant	0.190***	0.157***	0.087***	0.079***
	(0.008)	(0.017)	(0.007)	(0.010)
Controls	No	Yes	No	Yes
N	6155	6155	6155	6155
R2	0.58	0.58	0.87	0.87

Notes: The Table reports OLS regression results on the extensive margin. Robust standard errors in parentheses. Controls include dummies for the courses "International Teams", "50 Years of Internet", and "Data Science & Engineering", whether the course is the first course on the platform, whether the course is accessed from Germany, and whether it is accessed earlier than the median access, as well as the day of enrollment relative to the course start. "Any entry pre" for sensitive and insensitive categories corresponds to only those ex ante filled entries classified as sensitive or insensitive, respectively. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Consequently, we find no clear support for first time users reacting differently to any of our treatments than experienced users.

Second, we divide our sample based on pre-intervention commitment to the course, i.e., we call a user committed if she shows up in the course earlier than the median user to work on the first week's course material. We expect that committed users are more willing to reciprocate the platform's course offer by contribution data. Yet, we do not find support for such an effect. As column (2) of Table 4.A.3 reports, the interactions of early first course action with *Benefit* and *Benefit + Cost* are negative, small in magnitude, and not statistically significant at any conventional level. Hence, commitment to the platform in form of early course action does not lead to more reciprocal behavior via contributing more personal data.

Third, heterogeneous responses to treatments may exists between Germans and non-Germans. Since previous research indicates that Germans hold comparably strong privacy

Table 4.A.3: OLS regression results: Heterogeneity by treatment

	Number of entries			
	First course	First action	Germany	Entries pre
	(1)	(2)	(3)	(4)
Benefit	0.184**	0.195	0.005	0.064
	(0.085)	(0.124)	(0.237)	(0.164)
Benefit + Cost	0.161*	0.249**	0.085	0.069
	(0.085)	(0.127)	(0.233)	(0.167)
Entries pre	0.879***	0.879***	0.879***	
	(0.008)	(0.008)	(0.008)	
First Course	-0.191			
	(0.164)			
Benefit # First Course	0.014			
	(0.232)			
Benefit + Cost # First Course	0.328			
	(0.238)			
Early First Action	0.123			
		(0.111)		
Benefit # Early First Action		-0.015		
		(0.162)		
Benefit + Cost # Early First Action		-0.041		
		(0.165)		
Germany		-0.147		
		(0.176)		
Benefit # Germany			0.208	
			(0.252)	
Benefit + Cost # Germany			0.159	
			(0.248)	
Zero Entries Pre				-3.246***
				(0.149)
Many Entries Pre				3.842***
				(0.133)
Benefit # Zero Entries Pre				0.270
				(0.309)
Benefit # Many Entries Pre				-0.082
				(0.267)
Benefit + Cost # Zero Entries Pre				0.164
				(0.212)
Benefit + Cost # Many Entries Pre				0.122
				(0.190)
Constant	1.302***	1.271***	1.392***	4.434***
	(0.130)	(0.136)	(0.180)	(0.168)
N	6155	6155	6155	6155
R2	0.55	0.55	0.55	0.48
p: Benefit + Benefit # Subgroup =	0.193	0.799	0.720	0.719
Benefit+Cost + Benefit+Cost # Subgroup				0.860

Notes: Table reports OLS regression results on the intensive margin. Robust standard errors in parentheses. All specifications estimated with controls including dummies for the courses "International Teams", "50 Years of Internet", and "Data Science & Engineering". Additional controls for first course, early first course action, and access from Germany are always included even if not listed in the Table. The last row reports p-values of testing for treatment differences between *Benefit* and *Benefit + Cost* for the respective user subgroups in the column. $p<0.10$, ** $p<0.05$, *** $p<0.01$.

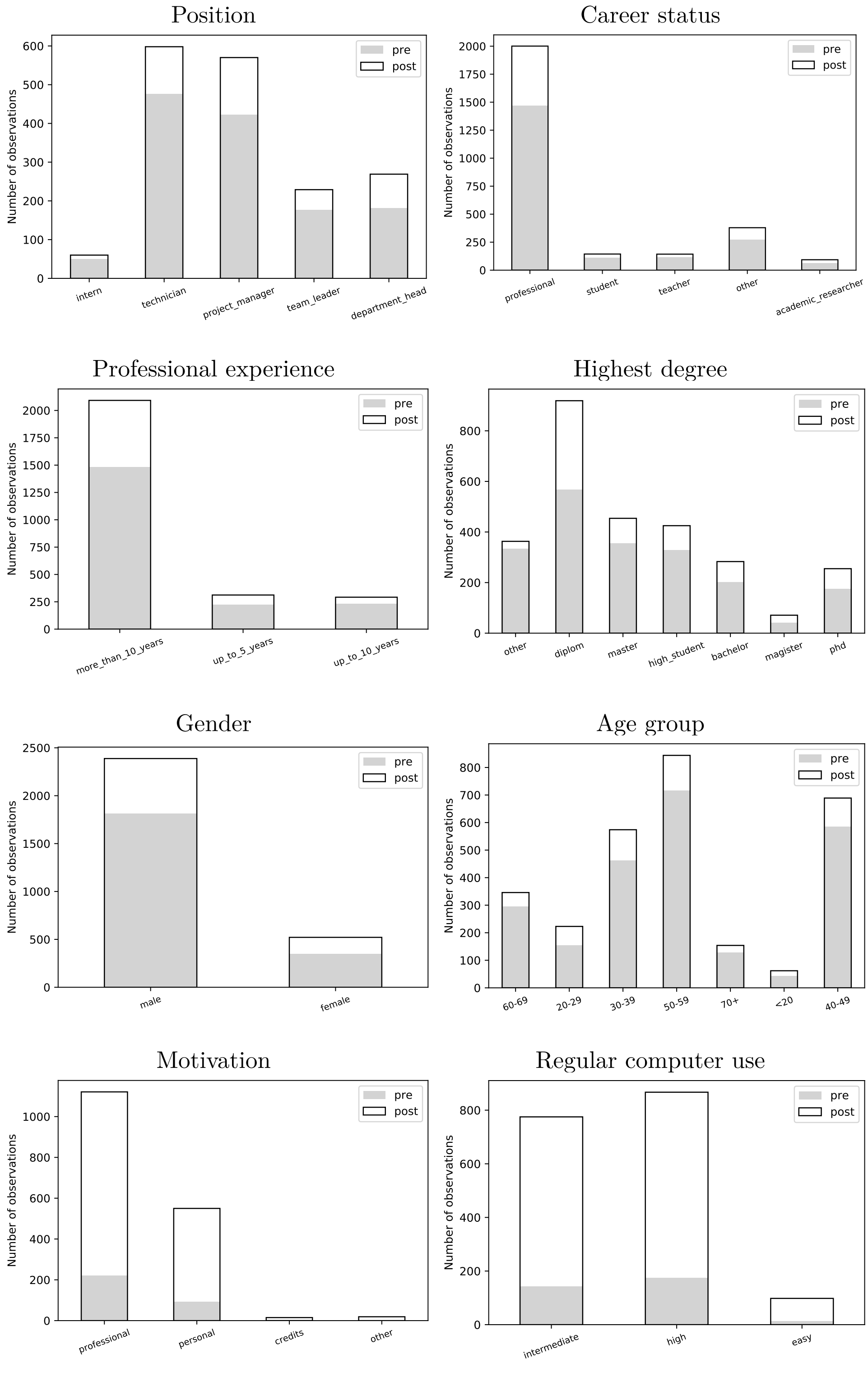
concerns (Bellman et al. 2004; IBM 2018), the *Benefit + Cost* treatment may generate a stronger positive effect on personal information disclosure for Germans than non-Germans. We measure this variable based on whether the most frequently used platform access location when logged in lies in Germany or not. However, we do not find that the privacy protection emphasis in *Benefit + Cost* makes German users more willing to contribute their data. While the interaction effect of the *Benefit + Cost* dummy with the Germany dummy in column (3) of Table 4.A.3 is larger than zero, it is not significant. Moreover, it is smaller in magnitude than the interaction effect with *Benefit* and statistically indistinguishable in a joint test for equal reactions across treatments as specified in the last row ($p = 0.720$). Thus, in our context, there is no statistical support for Germans reacting more to privacy protection than non-Germans.

Fourth, users with few profile fields filled before the intervention may be more prone to privacy concerns regarding data sharing. In order to study this, we split our sample into three categories: user with no initially filled profile entries, users with at least one but not more than median entries, and users with more than median entries. If privacy concerns limit initial disclosure, we may see more additional entries generated in the *Benefit + Cost* treatment for users with relatively few initial entries due to salient privacy protection standards in this treatment. Nonetheless, we do not find evidence that users with different many ex ante entries react differently to the intervention. There are no significant interaction effects of the pre-intervention profile completion status and the *Benefit* and *Benefit + Cost* treatment dummies in column (4) of Table 4.A.3, and the reactions of users with zero or many initial entries, respectively, to *Benefit* and *Benefit + Cost* are indistinguishable ($p = 0.719$ and $p = 0.860$, respectively). Consequently, emphasizing privacy protection in the *Benefit + Cost* treatment does not motivate user with fewer initial entries to disclose more.

In sum, we find no evidence for heterogeneous responses between treatments for subgroups defined by process data. In other words, any version of the pop-up prompting users to complete their profile will attract information from all subgroups alike. This suggests choosing the most effective version in terms of intensive margin changes, the *Benefit*

+ *Cost* wording, to provide the platform with a solid non-selective boost in available information.

Figure 4.A.3: Histograms of profile entry content before and after the intervention



Notes: Figure displays histograms of which data content is contributed before and after the intervention for different profile fields.

Table 4.A.4: Content of pre-intervention profile entries

Outcome	Category	Control		Benefit		Benefit + Cost	
		Mean	Std dev	Mean	Std dev	Mean	Std dev
position	*missing*	0.777	0.416	0.789	0.408	0.798	0.401
	department head	0.035	0.184	0.030	0.170	0.023	0.152
	intern	0.005	0.070	0.011	0.105	0.008	0.091
	project manager	0.068	0.251	0.068	0.253	0.070	0.254
	team leader	0.032	0.176	0.025	0.155	0.029	0.168
	technician	0.083	0.276	0.077	0.267	0.071	0.258
career status	*missing*	0.667	0.472	0.669	0.471	0.676	0.468
	academic researcher	0.011	0.105	0.010	0.100	0.009	0.093
	other	0.048	0.214	0.038	0.192	0.046	0.210
	professional	0.243	0.429	0.244	0.429	0.229	0.420
	student	0.015	0.120	0.021	0.143	0.018	0.132
	teacher	0.017	0.128	0.018	0.133	0.022	0.147
professional life	*missing*	0.680	0.467	0.684	0.465	0.691	0.462
	more than 10 years	0.243	0.429	0.245	0.430	0.234	0.423
	up to 10 years	0.041	0.198	0.036	0.187	0.037	0.188
	up to 5 years	0.036	0.186	0.034	0.181	0.039	0.193
highest degree	*missing*	0.671	0.470	0.673	0.469	0.678	0.467
	bachelor	0.031	0.174	0.033	0.179	0.034	0.182
	diplom	0.092	0.289	0.097	0.296	0.088	0.283
	high student	0.057	0.231	0.053	0.225	0.050	0.219
	magister	0.006	0.076	0.006	0.079	0.008	0.088
	master	0.058	0.233	0.057	0.232	0.059	0.236
	other	0.049	0.216	0.054	0.227	0.059	0.236
	phd	0.037	0.188	0.026	0.158	0.023	0.150
gender	*missing*	0.642	0.479	0.659	0.474	0.646	0.478
	female	0.051	0.220	0.055	0.229	0.064	0.244
	male	0.307	0.461	0.285	0.452	0.291	0.454
age group	*missing*	0.604	0.489	0.620	0.485	0.611	0.488
	20-29	0.023	0.150	0.026	0.158	0.027	0.162
	30-39	0.079	0.270	0.074	0.262	0.072	0.258
	40-49	0.093	0.291	0.096	0.294	0.097	0.296
	50-59	0.121	0.326	0.115	0.319	0.114	0.318
	60-69	0.048	0.214	0.042	0.201	0.054	0.226
	70+	0.027	0.162	0.018	0.135	0.017	0.130
	<20	0.004	0.066	0.009	0.093	0.008	0.088
motivation	*missing*	0.947	0.224	0.954	0.210	0.945	0.228
	credits	0.000	0.022	0.001	0.031	0.000	0.000
	other	0.000	0.000	0.000	0.022	0.000	0.000
	personal	0.016	0.126	0.016	0.124	0.013	0.114
	professional	0.037	0.188	0.029	0.168	0.042	0.200
computer use	*missing*	0.947	0.224	0.950	0.219	0.943	0.231
	easy	0.001	0.031	0.003	0.058	0.001	0.038
	high	0.028	0.164	0.026	0.160	0.031	0.173
	intermediate	0.024	0.154	0.021	0.143	0.024	0.155

Notes: Table reports mean shares of entry content pre-intervention and corresponding standard deviations.

Table 4.A.5: Marginal effects of multinominal logit regressions regarding pre-post shifts in profile content distributions

Outcome	Category	Marginal effect	Standard error
position	department head	0.0173***	0.006
	intern	-0.0035	0.003
	project manager	0.0068	0.008
	team leader	-0.0022	0.006
	technician	-0.0183**	0.007
career status	academic researcher	0.0031	0.003
	other	0.0031	0.005
	professional	0.0007	0.006
	student	-0.0016	0.003
	teacher	-0.0054**	0.002
professional life	more than 10 years	0.0116*	0.006
	up to 10 years	-0.0123**	0.005
	up to 5 years	0.0007	0.005
highest degree	bachelor	0.0013	0.004
	diplom	0.0485***	0.006
	high student	-0.0108**	0.005
	magister	0.0052**	0.002
	master	-0.0138***	0.005
	other	-0.0351***	0.005
	phd	0.0046	0.004
gender	female	0.0176***	0.005
	male	-0.0176***	0.005
age group	20-29	0.0123***	0.003
	30-39	0.0046	0.004
	40-49	-0.0072**	0.004
	50-59	-0.0085**	0.004
	60-69	-0.0043*	0.003
	70+	-0.0004	0.002
	<20	0.0035**	0.002
motivation	credits	-0.0007	0.005
	other	0.0124	0.010
	personal	0.0305	0.024
	professional	-0.0421*	0.025
computer use	easy	0.0229*	0.014
	high	-0.0319	0.025
	intermediate	0.0091	0.025

Notes: Table reports average marginal effects from multinominal logit regressions for each outcome in the leftmost column. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

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Eidesstattliche Erklärung

nach § 8 Abs. 3 der Promotionsordnung vom 17.02.2015

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Köln, 10.03.2020