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Self-Nudging vs. Social Nudging in Social Dilemmas: An Experiment*

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Abstract

The exogenous manipulation of choice architectures to achieve social ends ('social nudges') can raise problems of effectiveness and ethicality because it favors group outcomes over individual outcomes. One answer is to give individuals control over their nudge ('self-nudge'), but the trade-offs involved are poorly understood. We examine how subjects self-nudge in a paradigmatic social dilemma setting and whether outcomes differ between the self-nudge and two exogenous nudges in line with perfect free-riding or full cooperation. Subjects recruited from the general population play a ten-round VCM online in fixed groups of four with one daily contribution decision. The nudge takes the shape of a non-participation default contribution, comparing zero, full, and self-determined levels. We find that the average self-nudge is 44% of the endowment and only 7% of subjects choose one of the two exogenous defaults. Yet, there is a hard trade-off between ethicality and effectiveness: Self-nudging groups do not better than groups under the perfect free-riding nudge. The reason is that non-defaulting subjects contribute less. Groups under the full cooperation default exhibit no reactance against the nudge and outperform both alternative choice architectures. (183 words)

Keywords: Nudging, choice architecture, defaults, public goods, behavioral economics, experiment.

JEL codes: H41, C92, D91

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1 Introduction

'Nudging'[1] has become popular among policy-makers over the last decade (Sunstein and Reisch, 2013; Halpern, 2015). The use of nudges in behavioral public policy, however, and the choice architects behind them have also attracted criticism. It has been argued, for example, that the choice architect uses, without consent, people's inertia or inattention against them (Sunstein, 2017). There has also been concern about unintended effects of nudges when the choice architect overlooks policy-relevant heterogeneity among the nudged population (Thunström et al., 2018). One of the most common objections has been against employing nudges in settings in which the interests of the choice architect and the decision maker are not necessarily aligned (Altmann et al., 2013; Sunstein, 2015; Hagman et al., 2015). Social dilemmas are the prime example for such settings. There, "social nudges" (Nagatsu, 2015) are deployed with the intention of delivering choice outcomes that generate higher benefits not necessarily to each individual, but to society (or a group) as a whole. An individual targeted by a social nudge may therefore have reason to be distrustful of choice architects who are likely to prioritize social objectives over the individual's own benefits (Sunstein, 2015). This is largely unproblematic when the individual's interests coincide with those of the group: People approve of social nudges that support societal outcomes they favor (Tannenbaum et al., 2017). When the interests do not align, however, people disapprove of a social nudge, particularly when it is "perceived as foolish, wrong, harmful, expensive, or as the imposition of some high-minded [...] elite" (Sunstein and Reisch, 2013). Such disapproval can lead to "psychological reactance" among the 'nudged' population and threaten or even reverse the effect of the nudge (Arad and Rubinstein, 2018).

When a lack of alignment between the choice architect and the targeted individuals threatens the effectiveness and ethicality of nudging, then one option is to increase alignment by personalizing the nudge on the basis of additional data about the individual (Sunstein, 2013).[2] A second option is to give individuals themselves control over the choice architecture. In Thaler and Sunstein's words, this could mean *"set[ting] the default by asking what reflective [individuals] would actually want."* (Thaler and Sunstein, 2008). Like nudging itself, the idea of such "self-nudging"[3] is not new: Users of fitness

[1] *Nudging* has been defined as a deliberate manipulation of the 'choice architecture', that is, the non-price elements of the economic environment in which people take decisions. The goal of the manipulation is to alter people's decisions while maintaining freedom of choice across options and their associated economic incentives (Thaler and Sunstein, 2008). Because of its potential to counteract well-known biases in human decision-making, nudging promises to deliver choice outcomes that generate – for 'the nudged' – higher own benefits than those that would arise in its absence.

[2] Personalized defaults acknowledge the heterogeneity among the nudged population and can reduce the problem of one-size-fits-all present in uniform nudges. Personalized nudges are likely to require gathering large amounts of personal data to improve the fit of the nudge (Thaler and Tucker, 2013; Yeung, 2017) such as past behavior (Briscese, 2019). The privacy dimensions of such data-intensive personalization raise ethics issues of their own. There are also concerns whether personalized nudges can be reconciled with notions of universality and equal treatment that apply to public policy (Mills, 2020).

[3] In the literature, the idea has also been discussed under the heading of "self-management" (Schelling, 1978) or "behavioral self-management"(Tontrup and Sprigman, 2019).

and health applications on smartphones set the frequency and timing of reminders, feedback, and other defaults (Caraban et al., 2019). People with weight management issues deliberately remove high-calorie items from their line of sight in the office and at home (Bucher et al., 2016). Members of mutual funds set their own default combination of dividend payout and reinvestment when they join an automatic dividend reinvestment plan (Feito-Ruiz et al., 2020).[4] The informational and ethical advantages of giving people control over the choice architecture that structures their environment have been highlighted in the philosophical literature (Reijula and Hertwig, 2020). Self-nudging could also have material advantages: In an experiment, subjects used the control opportunity to achieve higher own benefits by anticipating and avoiding behavioral biases (Tontrup and Sprigman, 2019). These observations point to the potential of self-nudging to help people make better decisions for their own benefit (Banerjee and John, 2021).

For social dilemmas, in which "better" decisions are intended to generate higher group benefits, self-nudging has so far received little attention. This is despite its potential to overcome the objections against social nudges imposed exogenously that were discussed above. The present paper contributes towards closing this research gap. Specifically, it reports on an online experiment that compares – in a paradigmatic social dilemma – the performance of exogenously chosen nudges with the performance of endogenous nudges along three dimensions: how individuals choose to set their own nudge (default choice), how self-nudges affect individual behavior (effectiveness), and how they affect group outcomes (efficiency).

The experimental design combines three familiar components. First, as the social dilemma, it employs the standard linear public goods game, or voluntary contribution mechanism (VCM). The strength of using the VCM lies in the clear and cardinal metric for measuring how individuals self-nudge and how effective the outcomes are for individuals and the group. Second, for the nudge, the design uses choice defaults, specifically default contribution levels in the VCM. Defaults are prototypical tools in the hands of the choice architect (Madrian and Shea, 2001; Thaler and Benartzi, 2004; Thaler and Sunstein, 2008) and have been implemented in economic experiments on social dilemmas before.[5] Our design considers both exogenous nudges set by a choice architect and, as a key innovation, self-nudges set individually by experimental subjects.[6] The third component is the intervention point for the nudge. Like earlier papers that look at exogenous

[4](Automatic) reinvestment plans determine the default action of a member of a mutual fund in the absence of an active reinvestment decision, but the member can deviate from the default at any time. Members can set the default to contributing all of their annual dividends back to the fund, receive all of their annual dividends as a cash pay-out, or some combination of the two.

[5]For example, Fosgaard and Piovesan (2015) manipulate the default entries on the contribution screen to either free-riding or perfect conditional cooperation. Bruns and Perino (2021) employ a similar manipulation. Barron and Nurminen (2020) introduce external approval and feedback at the contribution stage. Chen et al. (2021) consider social information.

[6]To our knowledge, the only other paper that gives subjects a role in nudging is by Engel and Kurschilgen (2020). They examine the effect of subjects reflecting on a contribution norm at the contribution stage in a laboratory study. Our design, by contrast, involves default setting at the participation stage in a multi-session experiment.

defaults in one-shot VCM settings (Altmann and Falk, 2009; Hokamp and Weimann, 2021), our nudges and self-nudges intervene at the participation stage of a finitely repeated VCM.[7] This is the stage after the group has been formed, but before members take their active contribution decisions. This means that we consider non-participation defaults that manipulate what contribution decision will be taken on behalf of those group members that fail to show up for the contribution stage.[8]

We implement the three components in a multi-session ten-round VCM (e.g. Isaac and Walker, 1988; Fehr and Gächter, 2000) with 264 members of the general public as subjects. The repeat VCM is played online in fixed groups of four over the course of ten days, with one contribution decision per day. The main treatment conditions are three different non-participation default contributions. These step in when a member fails to make his or her daily contribution decision. The natural baseline treatment is an exogenous default of perfect free-riding: Non-participating group members make a zero contribution in that round. This is the almost universal default in VCM experiments. We compare this, first, with the alternative treatment of an exogenous default of perfect cooperation: Non-participating members contribute their entire per-round endowment to the public good in that round. This default is used, for example, in the one-shot VCM experiments by Altmann and Falk (2009) and Hokamp and Weimann (2021). Against these two benchmarks, we allow for endogenous contribution defaults that faithfully implement Thaler and Sunstein's vision: Group members are asked to set individually, privately, and irrevocably their own, unique default level in the first of the ten-round interactions. Non-participating members contribute their personally chosen amount of the per-round endowment in that round. The information structure is uniform across treatments: Group members are never informed about the default that applies to other group members. After every round, they receive feedback that consists of average contributions of the other group members, including defaults, and the number of group members who participated in the contribution decision.

We have two main findings. First, compared to the two exogenous defaults, perfect free-riding (zero percent of endowment) and perfect cooperation (100 percent), subjects themselves chose a non-participation default contribution that averaged 45 percent of endowment. A default contribution of 50 percent of the per-round endowment was the modal choice. Fewer than ten percent of subjects chose either the perfect free-riding or a perfect cooperation default. The cardinal metric of contributions makes plain that

[7] In principle, the nudge could operate at one of three stages of the VCM: Group formation (Ahn et al., 2008), group participation (Cason et al., 2004), and group contributions (Fosgaard and Piovesan, 2015). The participation stage has attracted only limited attention in economic experiments: In laboratories subjects are already seated in front of their screens by the time that they have to take decisions. In online settings, however, experimentalists have been encountering the participation stage in the form of attrition problems and finding that subjects fail to reliably participate in group decisions in every round of interaction (Arechar, 2018; Horton, 2011; Shank, 2016).

[8] It has been suggested to us that endogenous nudges are analogous to "snudges". Kaiser et al. (2020) define them as "offering self-binding commitments". However, there is no commitment component implicit in subjects setting their own defaults.

in the social dilemma of the VCM, self-nudges lead to a choice architecture that differs substantially from both non-participation defaults typically employed in the literature. In particular, the self-nudge rarely coincides with the perfect cooperation default chosen by a choice architect who prioritizes group benefits. This discrepancy illustrates the problem of ethicality associated with the lack of alignment between choice architect and individual and underlines the potential that commentators detect in self-nudges.

Our second finding is that there is a trade-off between ethicality and effectiveness. This finding summarizes two observations in our data. One is that the *ethically problematic* perfect cooperation default raises total contributions by 15 percentage points relative to the baseline of a perfect free-riding default. This observation generalizes findings from one-shot interactions (Altmann and Falk, 2009; Hokamp and Weimann, 2021) to repeat interactions. The other observation is that the *ethically unproblematic* self-nudges fail to raise total contributions significantly relative to the baseline, despite the higher non-participation default. There are two possible mechanisms behind our finding: The extensive margin, i.e., the share of non-participants across treatments; and the intensive margin, i.e., the contributions from participants ('active' contributions). At 13 percent, the extensive margin is nearly identical across all three treatments. The extensive margin effect pushes up total contributions in the perfect cooperation and the self-nudge treatment relative to the baseline. This is because the same share of non-participants now contributes all or around half of the endowment, respectively, in the round in which they are absent, rather than contributing zero. The intensive margin, i.e., average active contributions, is statistically indistinguishable between the baseline and the perfect cooperation default at about 45 percent of endowment. We therefore find no evidence of "reactance" (Arad and Rubinstein, 2018) towards the perfect cooperation default. We also find no evidence that the exogenous default conveys a norm of higher contributions (Carlsson et al., 2015). The higher effectiveness of the full cooperation default is therefore primarily due to the extensive margin. When individuals self-nudge, active contributions are significantly lower (about 37 percent). As a result, the intensive margin effect counteracts the extensive margin effect and explains why total contributions under the self-nudge are not significantly higher than the baseline. The drivers of the intensive margin effect cannot be identified with the present design. We provide some guidance to future research in this area, however, by sharing data of belief evolution among our participants.

Our findings merit attention: They show that the resolution of the informational and ethical disadvantages of one-size-fits-all social nudges (Reijula and Hertwig, 2020; Banerjee and John, 2021) likely comes at the cost of reducing effectiveness at the level of increasing individual contributions and of reducing efficiency at the group level. The presence of such a trade-off holds even before taking into account broader welfare-relevant considerations such as the potential psychological costs of having to set one own's default. Whether our findings generalize to social dilemmas other than the VCM, with all its advantages and disadvantages, is a matter for future research. Methodologically, the paper also shows how to use attrition productively to inject realism into the study of defaults, status-quo bias, and participation decisions in social dilemmas. The experimental setting of an online multi-day VCM (Isaac et al., 1994; Diederich et al., 2016) captures many features of real-world interactions. There, small but positive participation costs prevent participants in the interaction from making an active decision every time a decision is to be made (Pecorino and Temimi, 2007; Osborne et al., 2000). This realism is typically absent in laboratory experiments. In online experiments, it is typically regarded as a nuisance factor (Arechar et al., 2018). Our approach could be useful for addressing research questions in which increased realism is a step towards greater external validity.

2 Experimental Design and Procedure

We employ a standard VCM (Isaac and Walker, 1988; Fehr and Gächter, 2000) in groups of four and with an MPCR of 0.4. The game was repeated for ten rounds in partner-matching. In each round, subjects were endowed with 80 units of the experimental currency, which they could divide between a private account and a common group account. Each currency unit allocated to the private account would increase a subject's payoff by one unit, while each unit allocated to the group account would increase each group members' payoff by 0.4 units. Thus, the payoff, π, to an individual i in any given round is given by:

$$\pi_i = 80 - m_i + 0.4 \left(m_i + \sum_{j \neq i}^{4} m_j \right) \tag{1}$$

Equation 1 captures the social dilemma that (i) for a payoff-maximizing subject, there is a dominant strategy to allocate all her endowment to her private account, and (ii) the resulting outcome is Pareto-dominated by the case where all subjects allocate all their endowments to the group account.

To study the effect of (self-)nudging at the participation stage of the social dilemma, we use a "multiple session" variant of the VCM (Isaac et al., 1994; Diederich et al., 2016). In multiple session experiments, rounds typically last several days so that subjects depart from and return to the experiment for each single round. This design feature forces researchers to cope with attrition. For our purposes, attrition provides a natural way to introduce contribution default rules for the case of subjects not participating in a given round. Changing the default rule in a multiple session VCM does neither reduce or alter the choice set nor does it inherently change the economic incentives (Thaler and Sunstein, 2008). However, once introduced, making an active decision (and thus deviating from the default contribution decision) arguably incurs some small non-monetary costs of cognitive effort and time to overcome behavioral inertia. These small costs are commonly invoked to explain why defaults have the ability to "stick" (Blumenstock et al., 2018). Each invitation email to a new round reminded subjects that a non-participation default had been set.

Our design compares three treatments conditions. The first condition, X-FREE, exogenously sets the default contribution to perfect free-riding. That is, a subject would automatically contribute all of her experimental endowment to her private account when she did not participate in a given round. The second condition, X-COOP, exogenously set the default contribution to perfect cooperation. That is, a subject would contribute all endowment to the common account if she did not submit an active decision. In the third condition, SELF, we asked each subject to choose privately and irrevocably a default contribution amount for herself after her first round of the game. The chosen value would subsequently be applied to a round when absent. In all three treatment conditions, subjects only knew their own default contribution.

The design of the third condition ensures, in contrast to alternative designs, a faithful implementation of Thaler and Sunstein's thought experiment of asking subjects to deliberate on their preferred nudge. It also ensures clean comparability across treatments: In all three conditions, nudges are unchangeable across rounds, are private information, and are not influenced by prior experience.[9]

Table 1: Experimental treatments

Default contribution	Treatment abbr.	Number of groups	Number of subjects
Zero (perfect free-riding)	X-FREE	18	72
Full endowment (full cooperation)	X-COOP	20	80
Self-chosen	SELF	28	112
Total	.	66	264

2.1 Online recruitment

Subjects were recruited using an Internet polling company. Panel members who agreed to participate in an "interactive survey" lasting ten rounds over ten days entered basic demographic information during recruitment. In total, 416 panel members pre-registered for participation and were randomly assigned to one of the three treatments.[10] 289 panelists responded to our invitation email for round one by signing into the experimental website.[11] 271 subjects completed round one. In all three treatments, the number of complete records was not divisible by four, which is why we needed to dismiss seven more subjects. They received a fixed compensation of \$5. This left us with a sample of 264

[9] Alternative designs for a self-nudge could involve providing experience with the VCM mechanism (under some other default), allowing one or more opportunities to change the default, allowing an opt out of being able to change the default, different information structures and many other features. By sacrificing comparability with exogenous nudges, these alternatives are a natural next step in future research.

[10] We randomly assembled a higher number of experimental groups in the SELF treatment to optimize power given the expected higher variance in the endogenously chosen default values compared to the exogenously fixed default values in the two other treatments.

[11] There is little evidence for systematic selection of pre-registered panelists into the experiment based on the sociodemographic characteristics available to us from recruitment. An exception is an about nine percentage points higher show-up rate among females registrants that is significant at the ten percent level (see Table 5 in Appendix A).

subjects.[12] Table 6 in Appendix A reports summary statistics for the full sample and by treatment. Separate F-tests suggest that the characteristics are well-balanced across treatments.

2.2 Experimental procedure

Each experimental round commenced with an invitation email sent out early in the morning that contained login information and the link to the experimental website (Figure 1). On the login screen of the experimental website, subjects had to manually enter the login information, that is, user ID and password. Intentionally, login credentials could not be saved in the browser in order to maintain some effort cost of participation.

In the first round only, after logging in, subjects received an introduction with a succinct explanation of the VCM, including information about within-subjects random incentive scheme (one round was randomly drawn for payment) and the conversion rate of the experimental currency.[13]

The following main decision screen, presented in every round, displayed a history of play, provided access to a "payoff calculator" tool, reminded subjects of the default contribution of their treatment (from round two), and elicited contribution decisions. The history of play showed, for each previous round, own previous contribution decisions, the average contributions of the other three group members to the group account (including allocations from defaults), and the number of actively submitted decisions in the group (i.e., the number of participating subjects in a round). The "payoff calculator" allowed subjects to learn about the payoff consequences of different allocations. Subjects made their decisions how to allocate their per-round endowment between their private account and the group account using two fields, one for each account, that featured an auto-completion function to ensure that all of the endowment was used.

Depending on the experimental round and treatment, there were between two and four more screens. In the first round only, after making their contribution decisions, subjects were informed about their specific non-participation default rule. For the two exogenous default treatments, the screen simply explained the procedure. Subjects assigned to the SELF treatment were to choose their own default contribution. In order to ensure comparability, the description emphasized in all three treatments that the default rule would be private information (see Appendix B for the exact wording of each experimental screen).

[12] A common concern for online experiments is a loss of control about subjects identity and hence, multiple participation (REFERENCE). The polling company prevented double registration with the same panel IDs, which is confirmed by our data. What remains is the possibility of subjects using multiple accounts with the polling company. Of the 264 subjects, there was only one pair of observations with identical IP addresses. Those two observations had also stated the same region of residence, ZIP code, and gender upon registration. We conclude that the two observations of this pair highly likely represent the same subject. However, given the small share within the sample, our results are not affected by this issue. We therefore decided not to exclude the subject since we would then have to discard eight subjects forming two independent observations.

[13] The instructions could be reviewed later on any screen and in any round by clicking on a link available in the northeastern corner of the screen.

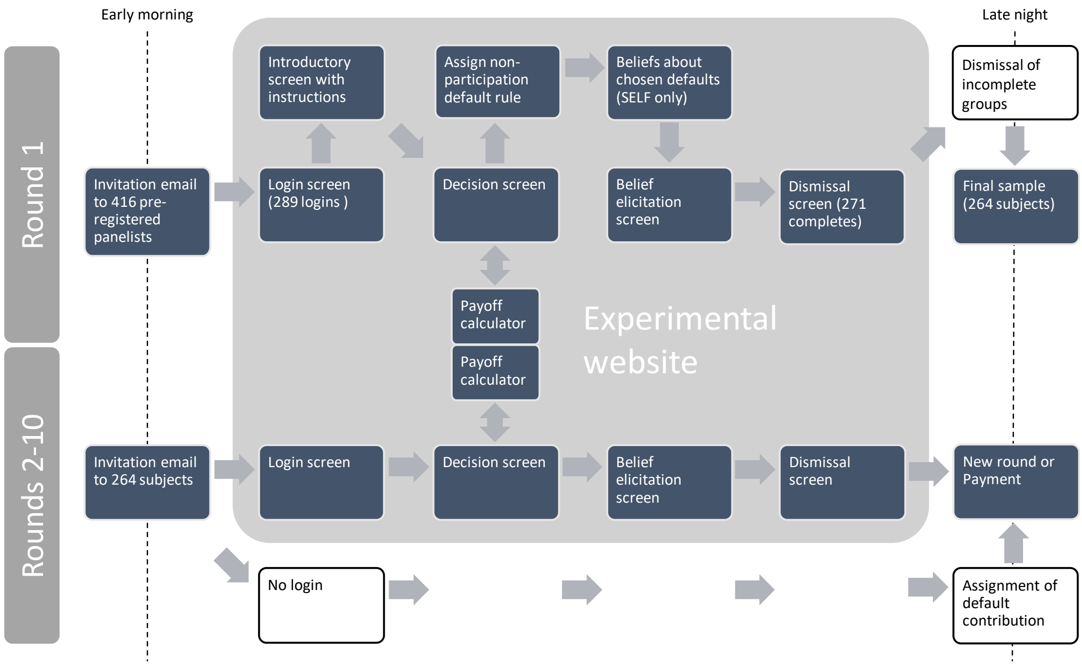


Figure 1: Flowchart of experimental procedure

In the course of the experiment, subjects were reminded of their default contribution, either exogenous or endogenously chosen, in each invitation email and on the decision screen of each following round. In the SELF treatment, the next screen elicited subjects' beliefs about the average default contribution of the other members in the group. In all treatments and rounds, the round concluded with a screen where subjects had to state their beliefs regarding their other group members' participation and contributions in that round. This belief elicitation was not incentivized (Charness et al., 2021).[14]

Each round was closed at about 2:00 AM the following day. After the final round, the experimenters randomly drew the payoff-relevant round, payoffs were computed, and payments initiated through the online polling company's payment infrastructure. The currency we used in the experiment was the same currency that the online polling company used to incentivize their surveys (1 unit = \$0.05). On average, subjects earned about 101 units of the experimental currency (i.e., \$5.05).

3 Results

We first present results on the self-chosen non-participation default. We then examine contributions in the two exogenous default treatments, X-FREE and X-COOP. This sets the stage for a comparison between exogenous defaults and the self-nudge treatment.

Figure 2 presents the distribution of default contribution levels set by subjects in the SELF treatment. The modal choice in the SELF treatment was exactly 40 experimental

[14]In the first round, where all subjects participated, this screen asked for contributions only.

currency units (50% of the endowment), chosen by 33.9 percent of subjects. Only 14.3 percent chose an amount that would match either one of the two exogenous defaults. In particular, about 7.1 percent chose a default of zero contribution and the same share a default of full contribution. On average, subjects chose to contribute 36.5 units (44.4% of the endowment) by default. We summarize this in our first result.

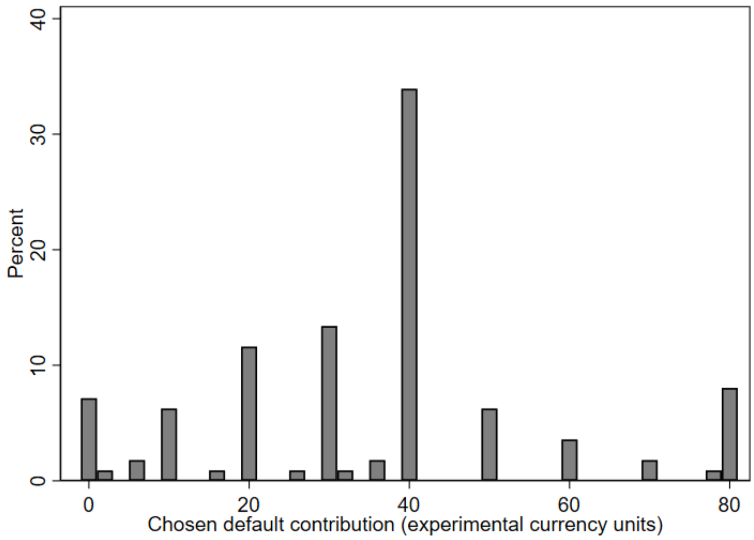


Figure 2: Histogram of chosen non-participation default contribution values in the SELF treatment

Result 1 *When asked to set their own non-participation default contribution, subjects' modal choice was to equally split the endowment. On average, subjects set a default contribution of 44 percent of their endowment. Only about 7 percent of subjects each set the default contribution at zero or 100 percent of their endowment.*

Result 1 is, to our understanding, the first reported evidence on how individuals set their own nudge in a social dilemma setting. It shows that for 93 percent of subjects, either the most common default in VCMs, X-FREE, or the default in line with maximizing group benefits, X-COOP, would differ from what they would choose themselves. Concerns about a possible misalignment between a choice architect and the targeted individuals therefore have an empirical basis.

Moving on to contributions in the VCM, we first focus on the two exogenous default treatments. Figure 3 shows mean total contributions over the course of the experiment for each of the three treatments. For the baseline of a zero contribution default (X-FREE), total contributions start out at 41.8 units (52.3%) in round 1. We observe a decline over time, ending up at 29.6 units (37%). Hence, behavior in X-FREE is similar to the typical pattern of play in VCM experiments (e.g., Fehr and Gächter, 2000). By contrast, total contributions in X-COOP, while starting at about the same value as X-FREE before the introduction of defaults (36.1 units or 45%), first increase and then remain stable over time, ending at 40.1 unit (50%). Panel (a) of Table 2 reports an average difference between X-FREE and X-COOP of 11.7 units (14.6 percentage points of endowment).

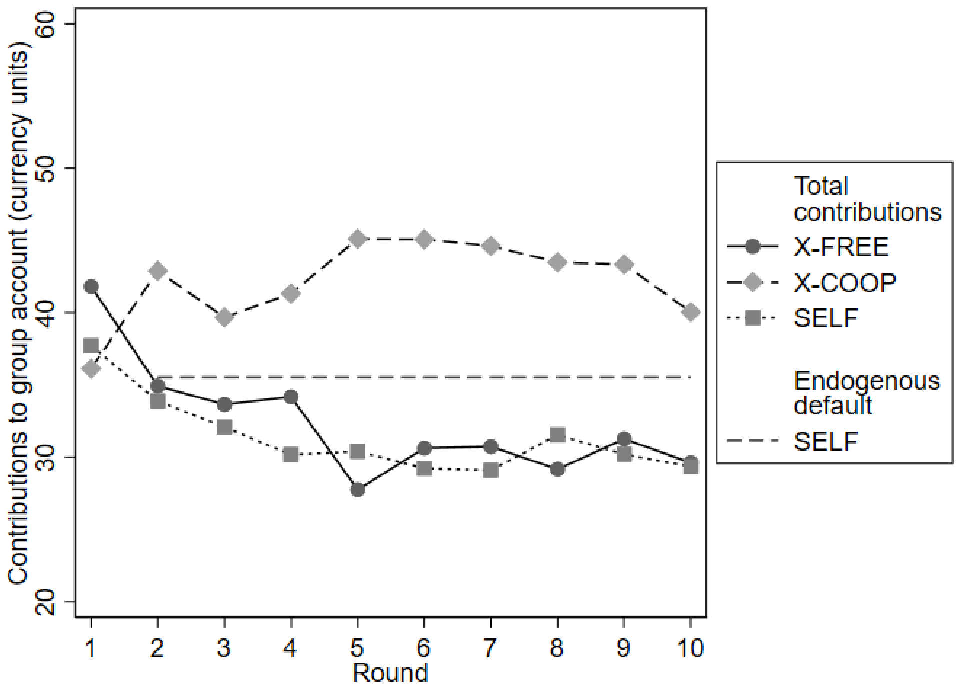


Figure 3: Mean total contributions by treatment and round and mean self-determined default contribution

Panel (b) of Table 2 reports a series of two-sample Fligner-Policello Robust Rank Order tests that examine statistical significance taking the group mean across rounds as one observation (e.g., Stoop et al., 2012).[15] The averages in Table 2 exclude the first and last round in order to reflect that treatment differences only started in round 2 and to account for potential endgame effects.[16] The null hypothesis that the group means in X-FREE are sampled from the same population as those in X-COOP is significantly rejected ($p = 0.003$). Further support comes from a random-effects GLS regression reported in column (1) of Table 3.[17] We regress total contributions on treatment indicators, a continuous round variable (assuming a linear trend), and the interaction of both, controlling for several sociodemographic characteristics and the weekday of the round. The X-FREE

[15]The alternative—taking the group means in each round as one independent observation and testing each experimental round separately—is reported in Table 8 in Appendix A and delivers similar results.

[16]Results are not sensitive to these exclusions. See the final column in Table 8 in Appendix A for the exact same tests as in Table 2 but averaging over rounds 1 to 10.

[17]A Breusch-Pagan LM test for random effects rejects the Null that there are no significant differences across subjects, hence the RE GLS is to be preferred over a simple pooled OLS for the panel data. To be conservative, we cluster standard errors on the group rather on the subject level. Another alternative to the RE GLS would be a Tobit with limits at zero and the full endowment. Estimates are generally more significant than the RE GLS with clustering at the group level (results available upon request). We therefore stick with the RE GLS as the more conservative specification. For column 3, a random-effects Probit regression as alternative to the LPM yields similar results.

treatment serves as the baseline. The estimates confirm a significant difference of 9.3 units (11.6 pp) in the level of total contributions to X-COOP as well as a positive trend across rounds of 1.4 units (1.8 pp) per round that more than offsets the linear decline in the baseline. All of this evidence constitutes our second result.

Result 2 *Under an exogenous non-participation default of full cooperation, total contributions are significantly higher than under an exogenous non-participation default of zero contribution by about 15 percentage points.*

There are two candidate mechanisms behind Result 2. At the extensive margin of contributing, default contributions by non-participating subjects mechanically drive total contributions apart because every non-participating subject under X-FREE contributes zero while every non-participating subject under X-COOP contributes the entire endowment. Non-participation is infrequent, but not negligible: The average share of non-participating subjects is 11.5 percent in the X-FREE and 14.8 percent in the X-COOP treatment (see column (3) in Table 2). This implies that the mechanical effect at the extensive margin is responsible for most of the difference in total contributions (11.5 out of the 14.6 pp). At the same time, the mechanical effect cannot explain differences in dynamic play. Beyond the mechanical effect, behavioral feedback at the extensive margin could further affect total contributions. The experimental evidence, however, fails to support the conjecture that treatments cause differences in subjects' propensity to participate: Test results in column (3) of Table 2 do not reject the hypothesis that non-participation rates in X-FREE and X-COOP are the same ($p = 0.39$). Figure 4 shows the development of the mean non-participation rate over time. In general, between about 5 and 20 percent of subjects default in a given round, but there is no obvious pattern of systematic differences. Regression results in column (3) of Table 3 underline that participation does not differ significantly, either in levels or in the trend, between the two exogenous treatments.

The second candidate mechanism is behavioral feedback at the intensive margin of contributions. For levels, the data are not supportive of this possible explanation for Result 2. Figure 5 shows active contributions across rounds: On average, contribution levels by those subjects who actively participate look very similar across the two exogenous default treatments. Column (2) of Table 2 bears this out: Treatment averages in X-FREE and X-COOP are similar and not significantly different ($p = 0.69$). Behavioral feedback at the intensive margin could, however, have dynamic effects: At the mean experimental round, we estimate a significant trend effect (see column (2) of Table 3). Contributions under X-COOP decrease by an estimated 1.1 units per round less than those under X-FREE, explaining the differences in the trend observed in *total* contributions. This dynamic effect therefore contributes to explaining Result 2.

The comparison of the X-FREE and X-COOP treatments shows that variations in exogenous defaults can cause variations in contribution levels and trends through me-

Table 2: Round 2-9 average group means and pairwise treatment comparisons

Treatment	Total contributions (1)	Active contributions (2)	Non-participation rate (3)
(a) Average group mean for rounds 2-9 (S.D.) in experimental currency units			
X-FREE	31.54 (11.22)	35.41 (11.95)	0.115 (0.107)
X-COOP	43.20 (12.06)	37.22 (10.59)	0.148 (0.131)
SELF	30.83 (9.72)	29.94 (10.02)	0.127 (0.127)
(b) Pairwise FP-tests, p-values:			
X-FREE vs. X-COOP	0.003	0.689	0.392
SELF vs. X-FREE	0.751	0.133	0.703
SELF vs. X-COOP	0.000	0.010	0.624

Note: Two-Sample Fligner-Policello (FP) Robust Rank Order Tests of the round 2-9 average of experimental group means.

Table 3: Random-effects GLS regressions of total contributions, active contributions, and participation

	Total contributions (1)	Active contributions (2)	Participation (3)
X-COOP=1	9.272**	1.694	0.027
	(3.601)	(3.524)	(0.032)
SELF=1	-0.946	-3.462	0.021
	(3.309)	(3.352)	(0.035)
Round	-0.988**	-0.954**	-0.004
	(0.436)	(0.407)	(0.004)
X-COOP=1 × Round	1.428***	1.092**	0.002
	(0.538)	(0.488)	(0.006)
SELF=1 × Round	0.299	0.227	0.006
	(0.487)	(0.465)	(0.005)
Constant	37.053***	39.645***	0.130*
	(5.298)	(4.722)	(0.069)
Additional controls	Yes	Yes	Yes
N	2,470	2,209	2,223
R^2 overall	0.075	0.057	0.053

Note: Baseline is the X-FREE treatment. Additional controls include gender, age, region of residence, education, income, weekday of the experiment. The Round variable is mean-centered, hence, coefficient estimates correspond to marginal effects estimated at mean experimental round. Column (3) is based on a linear probability model.

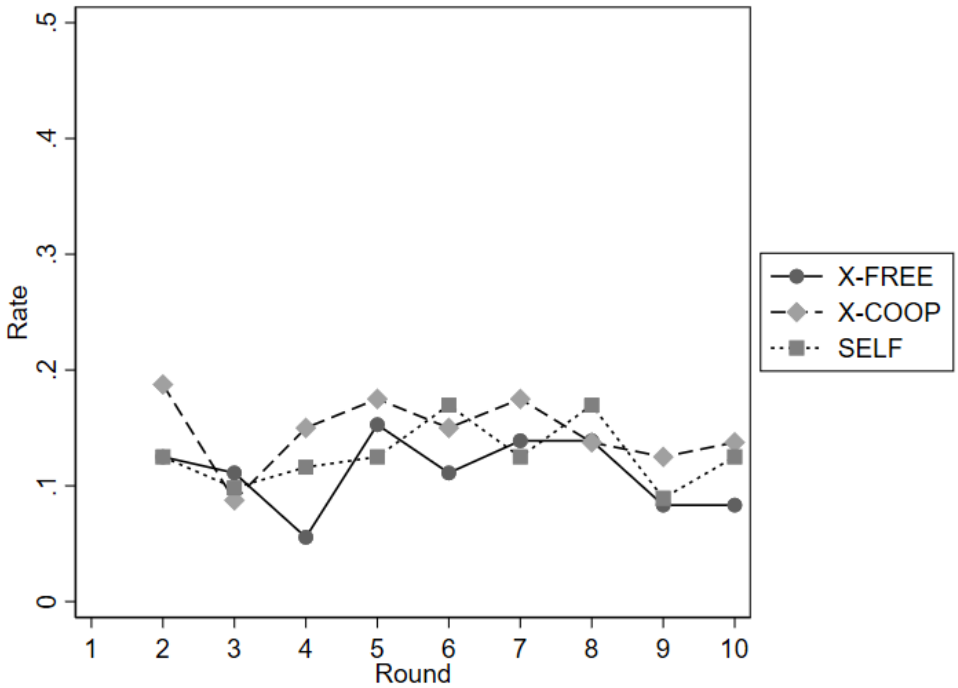


Figure 4: Mean non-participation rate, by treatment and round

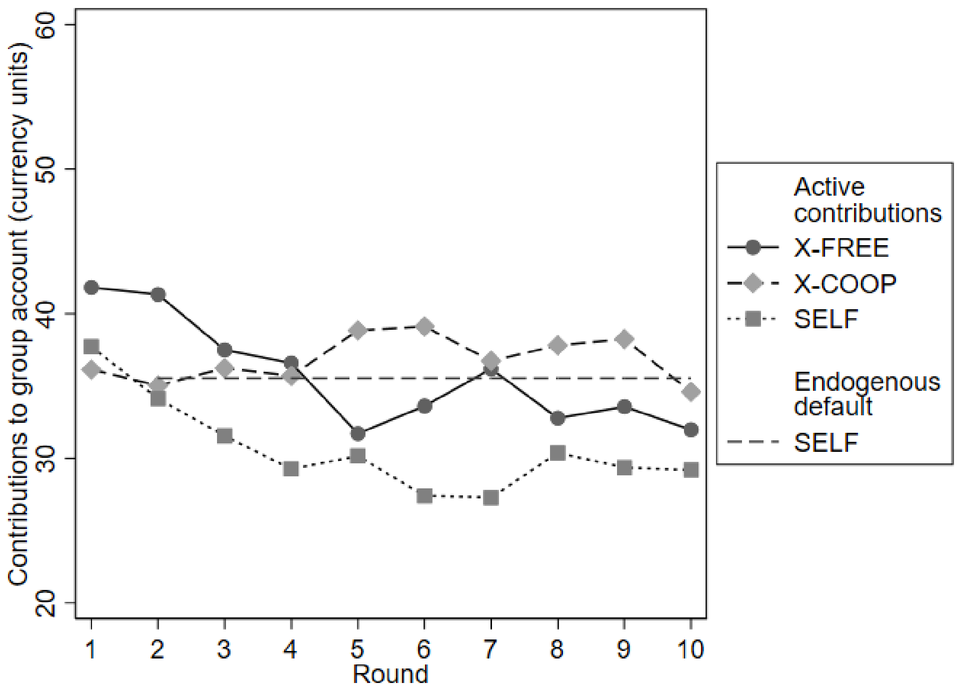


Figure 5: Mean active contributions, by treatment and round, and mean endogenous default value

chanical and behavioral effects at the extensive and intensive margin. We now turn to the endogenous default treatment in which participants make participation and contributions choices under self-determined nudges (SELF). Revisiting Figure 3, the pattern of total contributions in the SELF treatment is very similar to that of the baseline, the exogenous default of zero contributions. Column (1) in panel (a) of Table 2 reports an average contribution of about 30.8 units (38.5%). The null hypothesis of no pair-wise differences in the contribution levels (panel b) is highly significantly rejected when comparing SELF to X-COOP ($p < 0.001$), but not when comparing SELF to X-FREE ($p = 0.751$). In rounds 2 to 9, contributions under SELF and X-COOP differ by 12.4 units (15.5 pp). The random-effects GLS regression in column (1) of Table 3 confirms this: Under self-determined nudges (SELF), contributions do not differ significantly from those under a zero-contribution default (X-FREE), both in level and in trend. A full-contribution default (X-COOP), on the other hand, induces statistically significant differences in levels ($p = 0.001$) and trends ($p = 0.004$).

Result 3 *Total contributions under self-nudging are not higher or lower than under an exogenous default of perfect free-riding. As a consequence, they are significantly lower by about 16 percentage points than total contributions under an exogenous default of full cooperation.*

An examination of the underlying effects shows that at the extensive margin, non-participation in the SELF treatment does not differ statistically from that in the exogenous treatments (SELF vs. X-FREE: $p = 0.703$ and SELF vs. X-COOP: $p = 0.624$). The mechanism of default contributions implies that similar shares of non-participants across treatments should place total contributions in the SELF treatment between those in the exogenous treatments. Mean default contribution in SELF are, after all, about 36 units (44%) compared to 0 units in X-FREE and 80 units in X-COOP. Yet, result 3 showed that this is not the case. The reason is that an intensive margin effect of self-nudging exerts a downward pressure on active contributions, which Figure 5 captures. Active contributors in the SELF treatment contribute 7.3 units (9.1 pp) less than those in the X-COOP treatment, a significant difference ($p = 0.010$, column (2), Table 2). The difference of the two treatment indicators is marginally significant in the corresponding regression ($p = 0.087$, column (2), Table 3). Active contributors in the SELF treatment also contribute 5.5 units (6.9 pp) less than those in the X-FREE treatment, but the difference is not significant ($p = 0.133$, Table 2). The intensive margin effect explains why total contributions under self-nudges did not outperform those under a free-riding default, let alone approach those under a full cooperation default, even though the mechanism of default contributions favored such an outcome.

The differences in the contributions patterns across treatments naturally translate into group payoffs. Table 4 reports mean group payoffs in rounds 2-9 by treatment. The mean of average group payoffs in the X-COOP treatment (106 units) exceeds those in the X-

FREE (99 units) and in the SELF (99 units) treatment, a statistically significant difference ($p = 0.001$ and $p < 0.001$, respectively). The impact of defaults on the variance in group payoffs deliver a different pattern: Variance does not differ statistically ($p = 0.400$) between the X-COOP and the X-FREE treatment while the dispersion of payoffs in SELF is significantly lower than under the two exogenous defaults ($p = 0.038$ and $p = 0.002$). This reflects the fact that the default values under SELF are less extreme than under X-FREE and X-COOP.

Table 4: Payoffs

Treatment	Average group payoffs	
	Mean	Std. dev.
	(1)	(2)
a. Summary statistics		
X-FREE	98.93	7.91
X-COOP	105.93	8.47
SELF	98.50	6.77
b. Pairwise tests (p-values):		
X-FREE vs. X-COOP	0.001	0.400
SELF vs. X-FREE	0.384	0.038
SELF vs. X-COOP	0.000	0.002

Note: Pairwise tests of mean values report two-Sample Fligner-Policello (FP) Robust Rank Order Tests of the group mean payoffs (in units) over rounds 2-9. Pairwise tests of standard deviations are based on two-sided F-tests on group means of rounds 2-9.

Payoffs in experiments are not the same as welfare in experiments. In the present experiment, there are two additional factors that a welfare assessment would need to take into account. If setting a default imposes non-negligible psychological cost on subjects, then the payoffs in the SELF treatment will need to be scaled down in welfare terms. If being a subject in a VCM with non-participation defaults that differ from the preferred default imposes a psychological cost, on the other hand, then a similar scaling down would need to apply to most payoffs in the X-FREE and the X-COOP treatment.

4 Additional evidence

The purpose of the experimental design was to examine how subjects set their own nudges in a paradigmatic social dilemma setting when given the opportunity and whether outcomes in social dilemmas differ as a result of the different choice architectures of exogenous and endogenous nudges. With the results in hand, we report here on additional correlational evidence in order to provide guidance for future research on the underlying causes.

We first report on the behavioral correlates of default choice in the SELF treatment before reporting on the evolution of beliefs in all three treatments.

In the SELF treatment, subjects choose their own default contribution. This choice is strongly correlated with the contribution decision in round 1 (correlation coefficient 0.344) that directly precedes it. Over half of the subjects (62.5%) chose exactly their first-round contribution to the common account as default contribution. For 29 percent of subjects in SELF, this corresponded to the modal choice of half of the endowment. About 45 percent of the variation in the default is explained by variation in first-round contributions. Among the 37.5 percent of subjects who chose a different amount than their first-round contribution, 9.8 percent of subjects chose a value that was larger than their first-round choice (on average by 28.1 currency units) while 27.7 percent chose a value that was smaller (on average by 17.8 units). These patterns in setting a personalized non-participation default in a social dilemma merit future investigation to see whether they follow the accumulated evidence on the cognitive and affective drivers of contribution behavior in social dilemmas (Bouwmeester et al., 2017; Goeschl and Lohse, 2018).

Such investigation will also need to explain the patterns that connect default setting and behavior in later rounds of the social dilemma. At the subject level, active contributions are significantly positively correlated with the chosen default contribution (marginal effect 0.27 units, $p < 0.01$, in RE GLS regression, see Table 7 in Appendix A). It would be interesting to understand whether this can be fully explained by individual social preferences or whether additional mechanism need to be invoked. There is no evidence that those choosing a higher default value contribute less in later rounds. Such behavior could arise as a result of strategic signaling if subjects' default contributions were public information. The private nature of the default in our experiment excludes this possibility by design, but future research may well find different patterns under different information structures. The probability of non-participation is uncorrelated with the default contribution: If there are cognitive or affective drivers that link the choice of default with the choice to participate, then our experiment did not uncover their presence and direction.

The experiment also elicited subjects' beliefs about others' behavior. The evidence that emerges from these (unincentivized) beliefs can be summarized as follows. Overall, the general population subjects were rather pessimistic about the cooperative prosocial behavior of their fellow participants. First and foremost, in the SELF treatment, they believed other members of their group would choose a lower default amount for themselves on average than they actually did (29.61 vs. 35.86 currency units, $p < 0.01$, Wilcoxon matched-pairs signed-rank test). Asked about how many of the other group members they believed to not participate in the current round, they over-estimated the non-participation rate by 7.7 percentage points across treatments ($p < 0.001$, Wilcoxon matched-pairs signed-rank test). Likewise, subjects underestimated the share of subjects who participated and give more than zero, by 5.4 percentage points ($p < 0.001$, Wilcoxon matched-pairs signed-rank test). In the same manner, subjects underestimated the average amount those active, non-zero contributors give, by 7.9 currency units ($p < 0.001$, Wilcoxon matched-pairs signed-rank test).[18]

Against these generally pessimistic expectations about their peers in the SELF treatment, subjects in the X-COOP treatment had a relatively optimistic view about the size and development of active contributions. To show this, we turn to treatment differences in the beliefs. Figure 6, panels (a) to (c), depict the development of the answers to the three belief questions over the course of the experiment. For panel (d) in Figure 6, we combine answers to the second and third question to construct the belief about active contributions. Hence, panel (d) is the belief counterpart to Figure 5 whereas panel (a) is the belief counterpart to Figure 4. Also shown in all panels by a dashed line is the average belief for the average default amount subject chose in SELF. We observe no clear differences between treatments in panels (a) or (b), which is confirmed by test results ($p \geq 0.432$ for pairwise comparisons using FP Robust Rank Order Tests).[19] In contrast, panels (c) and (d) show that subjects expect others who participate to provide higher active contributions in X-COOP compared to the other two treatments. Tests and regression results support this observation. In non-parametric testing, the difference between X-COOP and SELF is significant for both variables ($p = 0.056$ and $p = 0.073$) but not the difference between X-FREE and X-COOP ($p = 0.374$ for both variables). In regression results analogously to Table 3 and reported in Table 10 in Appendix A, the level difference between X-COOP and SELF is not clearly significant ($p = 0.108$ and $p = 0.130$) but the slope effect is. We estimate a time trend not significantly different from zero for X-FREE and SELF. Compared to that, the time trend of X-COOP is significantly more positive, by roughly 1.1 currency units per round for both variables ($p < 0.01$). The same result holds for directly comparing X-COOP and SELF. Taken together, this is evidence that subjects update their beliefs in X-COOP more positively than in the other treatments in the course of the experiment, leading to (almost) significant overall differences between X-COOP and the other two treatments.

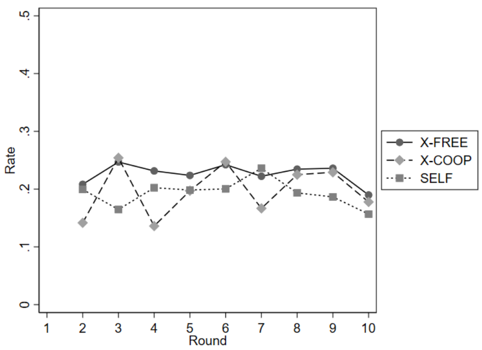
Future attempts to explain why self-nudging performed relatively poorly and an exogenous full cooperation default performed relatively well will need to also explain these patterns of belief evolution in the experiment.

5 Conclusions

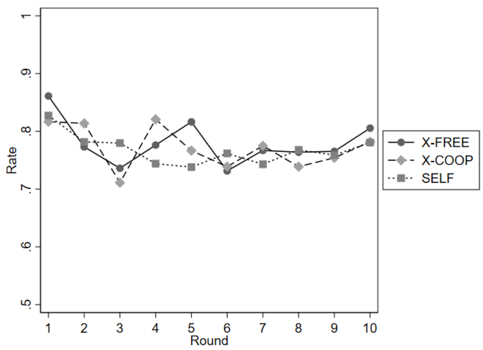
In this study, we investigated how individuals self-nudge in a social dilemma setting and how self-nudging affects group outcomes. We argued that these questions merit investigation as social nudges become increasingly popular and therefore spread beyond contexts for which the nudge concept was originally intended. Giving individuals control over how they will be nudged for social ends is one candidate answer to resolving a

[18] See panel a. in Table 9 of Appendix A for exact mean values of the belief variables.

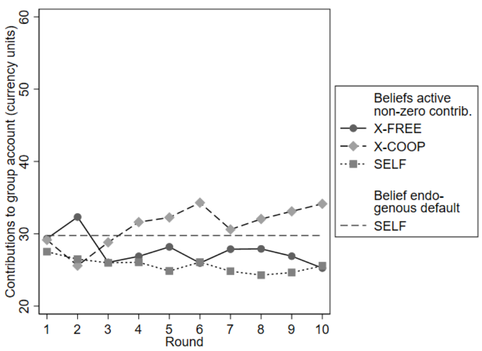
[19] Here and for the following references to tests, see Table 9 in Appendix A for the full set of tests.



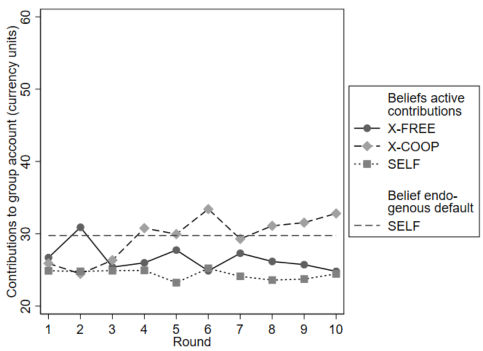
(a) Belief how many other group members did not participate in round, depicted as rate



(b) Belief how many other group members participated and gave more than zero, depicted as rate



(c) Belief of average contribution by others who actively participated and gave more than zero



(d) (Constructed) belief of average contribution by others who actively participated

Figure 6: Belief questions

possible conflict between choice architect and targeted individual. Three dimensions were relevant in our assessment of how well self-nudging performs. One was the choice of defaults people choose for themselves. This allowed us to compare their choices with the baseline of perfect free-riding in most VCM experiments and with perfect cooperation, the natural choice of a social nudge by a choice architect seeking to maximize group benefits. The second dimension was to measure voluntary contributions in the VCM. This allowed us to measure both the effectiveness and the efficiency of self-nudging using a theoretically intuitive and empirically validated metric. The third dimension was to benchmark the effectiveness and efficiency of self-nudging against those of two exogenous social nudges, one of perfect free-riding and another of full cooperation. To give the idea of nudging meaning and purpose, the experimental procedures relied on a multi-session online environment in which subjects from the general population have to overcome a minimum of inertia to take a daily contribution decision repeatedly.

The first insight that our experiment delivered was that the conflict between the choice architect and the targeted individual is real. When subjects chose their default contributions themselves, heterogeneity in self-nudging was high, ranging from zero to full contribution. The average default was just under half of the endowment, with an equal split the modal choice. Only a small minority of subjects themselves chose one of the two benchmarks of either zero or full contribution. Full contribution maximizes group benefits and is the obvious choice for the choice architect of the social nudge in the voluntary contribution mechanism. Our evidence shows that when she was actually "asked", Thaler and Sunstein's "reflective individual" set her default at a very different level. This discrepancy provides empirical support to arguments that social nudges violate the philosophical premises and libertarian ethics of nudging. Libertarian paternalists favoring the social nudge would need to justify why that nudge is allowed to override the nudges that people would choose for themselves. Incidentally, the fact that our subjects rarely opted for the zero contribution default provides empirical support for arguments that most public goods experiments, including our own, have the wrong choice architecture. Among experimenters, the zero contribution default has been the obvious choice in the voluntary contribution mechanism: Subjects that do not participate in a round are treated as having contributed zero. While the opposite of a social nudge, the zero contribution default does also not represent the choice architecture that our subjects chose for themselves.

The second insight that our experiment delivered was that exogenous social nudges, while ethically problematic, work: Groups in which full contribution was the non-participation default contributed more in the VCM, both through their defaults, but more importantly also through active contributions. As a result, efficiency was higher than in groups in which zero contribution was the default, as intended by an exogenous choice architect. The reason why social nudges worked in our experiment, relative to a zero contribution default, can be satisfactorily explained by the mechanical effects of a full contribution default that ensures that for each non-participating group member the entire per-round endowment ends up in the group, rather than the private, account. The evidence gives little reason for invoking additional behavioral feedback between the choice of default and subjects' participation and active contribution decisions. Neither were perceptibly affected by the exogenous default, zero or full. This lack of a detectable link between social nudging and behavior in our experiment is worth stressing. For one, it would imply that finding evidence for "psychological reactance" is likely to require other designs. The absence of a link also contrasts with previous evidence that exogenous defaults can affect or alter the social norm. This link may therefore merit another look in future research.

The third, and perhaps most important, insight is that despite its favorable ethics and moderately cooperative contribution defaults, self-nudging fails. The endogenous default resulted in considerably and significantly lower overall efficiency than the full contribution default. In fact, group efficiency under self-nudging did not exceed that under a zero contribution default. These relative performances can no longer be exclusively attributed to mechanical effects: If participation and active contribution behavior had been unaffected by the default, then groups that self-nudge should have reached an efficiency level at around mid-point between the zero contribution default and the social nudge. Instead, active contributions under self-nudging were the lowest among the three regimes. The reasons for the negative link between self-nudging and active contribution behavior are an interesting area for future research. One guiding piece of evidence is that subjects that self-nudge are pessimistic about group performance in the social dilemma. This pessimism may be an unintended side effect of initiating reflection among players when they are asked to set their default. This is a question that future research will want to address, as well as the role of information structures in which the default setting is embedded.

In sum, choice architecture in social dilemmas remains particularly interesting and relevant in the light of current societal challenges. Discussions about how to reconcile divergent alignments between policy-makers and those targeted by their policies will continue and are vital in liberal democracies. On ethical grounds, self-nudging offers an attractive solution. As our experimental evidence highlights, however, self-nudging can be outperformed on efficiency grounds by an ethically more problematic social nudge. Unless self-nudging can be reliably engineered to approximate the performance of social nudges, a consensus on how to strike the right trade-off between ethics and effectiveness will be required to guide instrument choice in policy-making.

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Appendix

A Online Supplementary Material

A.1 Selection

When signing up for the experiment with the Internet polling company, subjects had to enter a selection of sociodemographic data. This allows us to test for possible selection effects at the point at which subjects transfer from the registration stage to round 1 of the experiment. In other words, we can test whether there are indication that registered subjects with certain characteristics were significantly more likely not to 'show up' for round 1. Table 5 reports the results of a probit regression that the propensity to show up for round 1 is slightly elevated for females and somewhat reduced for the second lowest income decile, a category that contains relatively few subjects, though.

Table 5: Selection analysis

	(1)
Female	0.086*
	(0.046)
25-34	0.067
	(0.103)
35-44	-0.075
	(0.098)
45-54	-0.049
	(0.097)
55 and older	0.034
	(0.102)
East Germany	-0.066
	(0.053)
Berlin	0.022
	(0.109)
Has academic education	0.016
	(0.051)
900-1200 EUR	-0.243**
	(0.108)
1300-1500 EUR	0.019
	(0.112)
1500-2000 EUR	-0.106
	(0.109)
2000-2600 EUR	-0.103
	(0.098)
2600-3600 EUR	-0.037
	(0.099)
3600-5000 EUR	-0.085
	(0.098)
above 5000 EUR	-0.146
	(0.129)
Observations	385
Pseudo-R^2	0.036
Log-likelihood	-226.20

Notes: Probit regression reporting marginal effects. Dependent variable 1 if invited panelist showed up to first round. Baseline is male, age category 18-24, lives in West Germany, has no academic education, income below $900.

A.2 Summary statistics of the sample

Table 6: Summary statistics by treatment

Variable	Full sample			X-COOP			X-FREE			SELF			F-test
	μ	σ	N	μ	σ	N	μ	σ	N	μ	σ	N	p-value
Female	0.51	0.50	264	0.44	0.50	72	0.49	0.50	80	0.56	0.50	112	0.27
Age 18-24	0.10	0.30	264	0.08	0.28	72	0.11	0.32	80	0.10	0.30	112	0.84
Age 25-34	0.19	0.39	264	0.17	0.38	72	0.13	0.33	80	0.24	0.43	112	0.11
Age 35-44	0.24	0.43	264	0.24	0.43	72	0.29	0.46	80	0.21	0.41	112	0.42
Age 45-54	0.28	0.45	264	0.24	0.43	72	0.29	0.46	80	0.30	0.46	112	0.60
Age above 55	0.20	0.40	264	0.28	0.45	72	0.19	0.39	80	0.15	0.36	112	0.11
Academic education[a]	0.31	0.46	257	0.30	0.46	70	0.32	0.47	78	0.30	0.46	109	0.96
Residence													
West Germany	0.71	0.45	264	0.64	0.48	72	0.75	0.44	80	0.73	0.44	112	0.27
East Germany	0.23	0.42	264	0.25	0.44	72	0.21	0.41	80	0.22	0.42	112	0.85
Berlin	0.06	0.24	264	0.11	0.32	72	0.04	0.19	80	0.04	0.21	112	0.11
Income													
Below 900EUR	0.08	0.28	254	0.09	0.28	69	0.10	0.31	77	0.06	0.25	108	0.63
900-1200EUR	0.07	0.26	254	0.13	0.34	69	0.04	0.19	77	0.06	0.25	108	0.10
1300-1500EUR	0.11	0.31	254	0.12	0.32	69	0.10	0.31	77	0.10	0.30	108	0.95
1500-2000EUR	0.09	0.29	254	0.14	0.35	69	0.09	0.29	77	0.06	0.23	108	0.13
2000-2600EUR	0.20	0.40	254	0.22	0.42	69	0.17	0.38	77	0.21	0.41	108	0.70
2600-3600EUR	0.19	0.39	254	0.13	0.34	69	0.18	0.39	77	0.22	0.42	108	0.31
3600-5000EUR	0.20	0.40	254	0.13	0.34	69	0.25	0.43	77	0.22	0.42	108	0.19
Above 5000EUR	0.06	0.23	254	0.04	0.21	69	0.06	0.25	77	0.06	0.23	108	0.85

Notes: The final column reports p-values from regressing the variable on treatment dummies and conducting an F-test for the joint significance of the regressors. [a] 1 if subject has at least some college education.

A.3 Additional tables

Table 7: Random-effects GLS regressions of active contributions and defaulting behavior on the chosen default value

	Active contribution (1)	1 if defaulting (2)
Default value	0.266***	-0.001
	(0.099)	(0.001)
Round	-0.541**	-0.003
	(0.265)	(0.004)
Female	0.054	-0.027
	(2.581)	(0.046)
Age:		
18-24	-1.825	0.068
	(5.693)	(0.078)
35-44	5.087	0.060
	(4.736)	(0.082)
45-54	-4.527	-0.064
	(3.804)	(0.061)
55 and older	0.165	0.062
	(4.150)	(0.087)
Residence:		
East Germany	1.849	0.141**
	(3.501)	(0.062)
Berlin	0.147	0.078
	(6.495)	(0.093)
Academic education	2.014	0.033
	(3.367)	(0.062)
Income:		
below 900 EUR	-8.376	0.157
	(9.817)	(0.122)
900-1200 EUR	5.400	-0.128*
	(10.055)	(0.073)
1300-1500 EUR	-2.862	0.074
	(9.529)	(0.111)
2000-2600 EUR	3.979	-0.056
	(7.243)	(0.063)
2600-3600 EUR	-4.057	-0.058
	(7.616)	(0.048)
3600-5000 EUR	3.410	0.056
	(7.296)	(0.061)
above 5000 EUR	-0.238	-0.163**
	(8.695)	(0.075)
Round weekday:		
Monday	0.288	-0.064*
	(1.324)	(0.036)
Tuesday	-1.535	-0.090**
	(2.179)	(0.036)
Wednesday	-2.420	-0.068*
	(2.199)	(0.040)
Thursday	-2.048	-0.056
	(1.506)	(0.045)
Friday	-2.875*	0.004
	(1.591)	(0.053)
Saturday	-3.539***	-0.031
	(1.230)	(0.035)
Constant	22.131**	0.180
	(9.033)	(0.114)
N	737	840
R-squared overall	0.148	0.126

Notes: SELF treatment only. Baseline is male, age category 25-34, lives in West Germany, has no academic education, income \$1.500-\$2.000, round took place on Sunday.

Table 8: Mean values and pairwise Fligner-Policello robust rank order tests between treatments

Treatment	$\grave{U}(n,m)$ Test	Round 1	2	3	4	5	6	7	8	9	10	All rounds pooled
a. Total contributions (incl. contributions from defaults):												
X-FREE		41.819	34.931	33.653	34.181	27.75	30.625	30.736	29.167	31.264	29.611	32.374
X-COOP		36.138	42.9	39.675	41.338	45.113	45.075	44.625	43.5	43.35	40.05	42.176
SELF		37.714	33.884	32.080	30.170	30.411	29.223	29.089	31.554	30.196	29.348	31.367
X-FREE vs. X-COOP	$\grave{U}(18,20)$	1.474	-1.923	-1.308	-1.422	-4.153	-3.326	-4.025	-3.183	-2.744	-2.056	-2.565
	One-tailed	*	**	*	*	***	***	***	***	***	**	***
SELF vs. X-FREE	$\grave{U}(28,18)$	-1.029	-0.087	-0.437	-1.156	0.742	-0.461	-0.673	0.598	-0.374	0.086	-0.414
	Two-tailed	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
SELF vs. X-COOP	$\grave{U}(28,20)$	0.761	-2.505	-2.054	-2.726	-3.241	-4.121	-5.410	-2.919	-3.992	-2.606	-3.595
	Two-tailed	n.s.	**	**	**	***	***	***	***	***	**	***
b. Actively submitted contributions:												
X-FREE		41.819	41.333	37.495	36.588	31.708	33.611	36.185	32.778	33.565	31.972	35.706
X-COOP		36.138	35.029	36.24	35.704	38.838	39.117	36.738	37.813	38.246	34.596	36.845
SELF		37.714	34.122	31.554	29.256	30.155	27.408	27.280	30.378	29.360	29.185	30.641
X-FREE vs. X-COOP	$\grave{U}(18,20)$	1.474	1.029	0.097	0.341	-1.387	-1.306	0.127	-1.042	-1.050	-0.583	-0.252
	One-tailed	*	n.s.	n.s.	n.s.	*	*	n.s.	n.s.	n.s.	n.s.	n.s.
SELF vs. X-FREE	$\grave{U}(28,18)$	-1.029	-1.485	-1.487	-1.864	-0.482	-1.493	-2.413	-0.712	-1.312	-0.539	-1.509
	Two-tailed	n.s.	n.s.	n.s.	*	n.s.	n.s.	**	n.s.	n.s.	n.s.	n.s.
SELF vs. X-COOP	$\grave{U}(28,20)$	0.761	-0.488	-1.369	-1.682	-2.166	-3.213	-2.734	-1.869	-2.868	-1.409	-2.249
	Two-tailed	n.s.	n.s.	n.s.	*	**	***	**	*	***	n.s.	**
c. Default rates:												
X-FREE		–	0.125	0.111	0.056	0.153	0.111	0.139	0.139	0.083	0.083	0.111
X-COOP		–	0.188	0.088	0.15	0.175	0.15	0.175	0.138	0.125	0.138	0.147
SELF		–	0.125	0.098	0.116	0.125	0.170	0.125	0.170	0.089	0.125	0.127
X-FREE vs. X-COOP	$\grave{U}(18,20)$ Two-tailed	–	-1.047	0.256	-1.600	-0.315	-0.862	-0.603	0.242	-0.663	-1.118	-1.007
SELF vs. X-FREE	$\grave{U}(28,18)$ Two-tailed	–	0.107	-0.021	1.067	-0.424	1.200	-0.409	-0.097	0.193	0.463	0.479
SELF vs. X-COOP	$\grave{U}(28,20)$ Two-tailed	–	-1.095	0.284	-0.624	-0.799	0.307	-1.028	0.071	-0.537	-0.664	-0.541

Note: We compare group means as independent observations

Table 9: Beliefs: Round 2-9 mean values and pairwise tests of treatments

Treatment	Beliefs about defaults rates (1)	Beliefs about active non-zero rate (2)	Beliefs about active nonzero contributions (3)	Beliefs about active contributions (4)
a. Average group means for rounds 2-9 (S.D.)				
X-FREE	0.231 (0.125)	0.766 (0.143)	27.76 (10.37)	26.763
X-COOP	0.200 (0.122)	0.765 (0.144)	31.04 (10.79)	29.613
SELF	0.198 (0.111)	0.759 (0.125)	25.40 (8.21)	24.316
b. Pairwise FP-tests (p-values):				
X-FREE vs. X-COOP	0.482	0.944	0.374	0.374
SELF vs. X-FREE	0.432	0.932	0.456	0.483
SELF vs. X-COOP	0.921	0.822	0.056	0.073

Notes: Two-tailed Two-Sample Fligner-Policello (FP) Robust Rank Order Tests of the round 2-9 mean values for each experimental group.

Table 10: Random-effects GLS regressions of beliefs variables

	Beliefs about nonpart. rate (1)	Beliefs about rate pos.ctr. (2)	Beliefs about pos. contrib. (3)	Beliefs about active contrib. (4)
X_COOP=1	-0.025	-0.007	4.236	3.649
	(0.035)	(0.041)	(2.742)	(2.727)
SELF=1	-0.047	0.016	-0.258	-0.571
	(0.032)	(0.035)	(2.381)	(2.463)
Round	-0.004	-0.002	-0.279	-0.148
	(0.004)	(0.005)	(0.294)	(0.319)
X_COOP=1 × Round	0.006	-0.001	1.079***	1.106***
	(0.006)	(0.007)	(0.405)	(0.406)
SELF=1 × Round	0.001	0.000	0.113	0.082
	(0.006)	(0.006)	(0.355)	(0.383)
Constant	0.327***	0.750***	30.899***	29.385***
	(0.051)	(0.054)	(3.348)	(3.399)
Additional controls	Yes	Yes	Yes	
N	1,943	2,115	2,073	2,073
R-squared overall	0.052	0.044	0.059	0.058

Standard errors clustered for experimental groups. Baseline is the X-FREE treatment. Additional controls include gender, age, region of residence, education, income, and the weekday of the experiment. The Round variable is mean-centered, hence, coefficient estimates correspond to marginal effects estimated at mean experimental round.

B Experimental screens, instructions, and round invitation email (not for publication)

Screen: Login

Viewed in rounds: all

You must log in or register before you can continue.

Interactive Survey

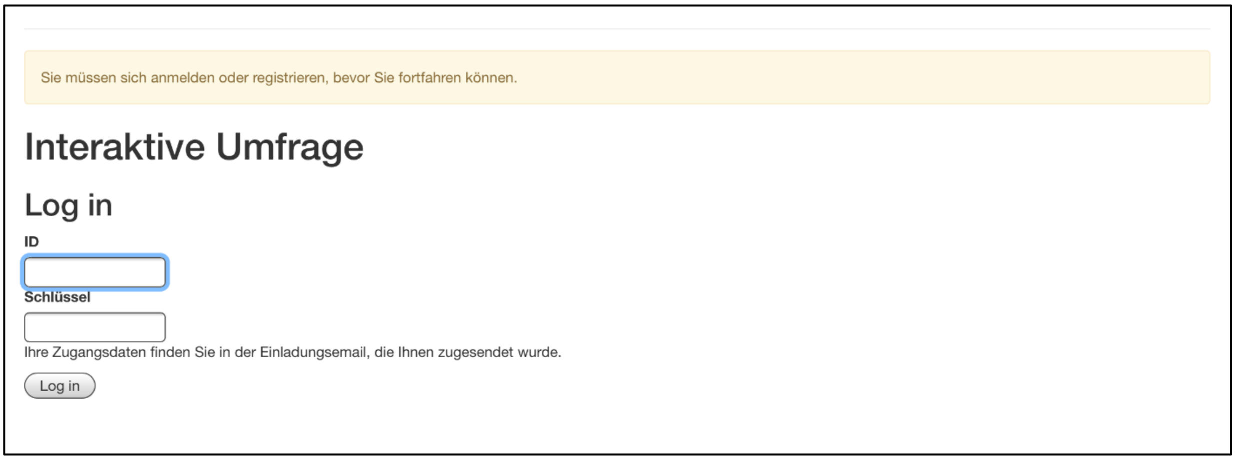
Log in

ID

Password

Your login details can be found in the invitation email which has been sent to you.

Log in



Sie müssen sich anmelden oder registrieren, bevor Sie fortfahren können.

Interaktive Umfrage

Log in

ID

Schlüssel

Ihre Zugangsdaten finden Sie in der Einladungsemail, die Ihnen zugesendet wurde.

Log in

Screen: Introduction

Viewed in rounds: 1

Signed-in user: **z8046b** Instructions Logout

Login successful.

Interactive Survey

Welcome

Dear participant,

Thank you very much for your willingness to participate in this interactive survey on decision making!

Before the first round starts today, please familiarize yourself with the rules of the survey. To do so, please click now on the link below and download the instructions.

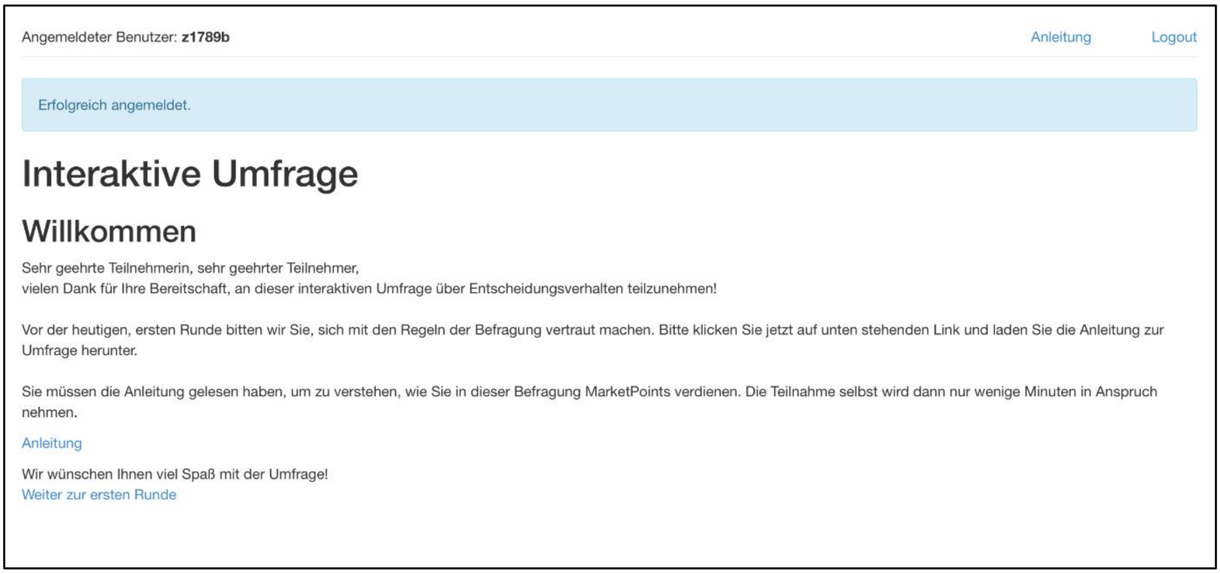
You need to have read the instructions in order to understand how to earn MarketPoints in this survey.

Participating in the survey itself will then only take a few minutes of your time.

Instructions

We hope you enjoy the survey!

Proceed to the first round


Angemeldeter Benutzer: **z1789b** Anleitung Logout

Erfolgreich angemeldet.

Interaktive Umfrage

Willkommen

Sehr geehrte Teilnehmerin, sehr geehrter Teilnehmer,
vielen Dank für Ihre Bereitschaft, an dieser interaktiven Umfrage über Entscheidungsverhalten teilzunehmen!

Vor der heutigen, ersten Runde bitten wir Sie, sich mit den Regeln der Befragung vertraut machen. Bitte klicken Sie jetzt auf unten stehenden Link und laden Sie die Anleitung zur Umfrage herunter.

Sie müssen die Anleitung gelesen haben, um zu verstehen, wie Sie in dieser Befragung MarketPoints verdienen. Die Teilnahme selbst wird dann nur wenige Minuten in Anspruch nehmen.

Anleitung

Wir wünschen Ihnen viel Spaß mit der Umfrage!
Weiter zur ersten Runde

Instructions
for the interactive survey
in cooperation with GlobalTestMarket

Please read these instructions carefully before starting the survey! This will ensure that you know how to influence your earnings according to your answers.

How many Market Points (MP) you earn through this interactive survey depends both on your own decisions and on the decisions of other participants!

All information provided during the survey will be treated anonymously. Your email address will be deleted upon completion of the survey.

YOUR DECISION SITUATION

You have been randomly assigned to a group of **four** GlobalTestMarket members. The other three members of your group will take this survey like you and read the same instructions as you.

The key content of this survey is the following question: You are given **80** MP - **how would you like to distribute it between two alternatives**? One alternative is a private "account" for you personally, the other is a joint group "account". The three other members of your group are asked the same question.

The two accounts describe different alternatives: Each MP you allocate to your **private account** increases your **own** earnings of the round by **one** MP (i.e., you simply keep the MP). Each MP you allocate to the joint group account increases the earnings of **all** members of the group (i.e., including you) by **0.4** MP each. Likewise, each MP that **another** member assigns to the group account increases the earnings of **all** members (i.e., including your earnings) by 0.4 MP each. Therefore: Whoever pays one MP into the group account increases the earnings of all (including herself/himself) by 0.4 points. For example, if there are 4 MP on the group account, **each** member receives 1.6 MP as earnings through the group account (plus the MP on their own private account).

Summary: Your **personal earnings of the round = allocation to the private account + 0.4 x sum of all group members' contributions to the group account.**

ROUNDS PROCEDURE

In total, the survey consists of **ten rounds**. You have **one day** per round. At the beginning of each round, you will receive an email inviting you to log in and submit your response.

On the survey website, you will be asked **in each single round** how you would like to distribute the 80 MP. You will be able to reconsider your decision and review the desired distribution, while viewing information about previous rounds on the decision screen.

If you do **not** participate in a round and do **not** submit a decision, your MPs are not lost. Instead, certain **preset default values** automatically apply to the distribution of your points in that round. You will learn more about the preset distribution during the first round. However, **full participation in the first round** is mandatory.

After all ten rounds have been completed, **one** round of the survey will be drawn at random. **Only** the result of this one round will count as earnings from the survey for you and the other members.

THE DECISION SCREEN

The decision screen is the main screen of the survey. At the top of the screen you will see an **overview of the previous rounds**. For each round, you will see how many MP have been allocated to your private account and to the group account (either by yourself or automatically according to the default setting if you have not submitted a decision).

You will also see how many MP on average the **three other group members** have allocated to the group account and how many members in your group have submitted a decision.

With this information, you can calculate your earnings in previous rounds using the "payoff calculator" in the middle of the screen (see description below).

In the lower part of the screen, you can submit your decision regarding the allocation of the 80 MP for the current round.

THE "PAYOFF CALCULATOR" BUTTON

You have here the opportunity to try out different distributions and calculate round earnings on a trial basis. To do this, enter a point distribution in the first two lines.

In order for your earnings to be calculated and displayed according to the rule described in the section "YOUR DECISION SITUATION", you still have to **estimate the behavior of the other members (or you can take it from the overview of past rounds)**. To do this, enter the average contribution of the other group members to the group account in the third line.

Then click on the button "Calculate" and your personal earnings will be calculated and entered in the table in the lower part of the screen. You can **try out several combinations and compare them with one other**.

FINAL QUESTIONS

After submitting your decision, you will be asked in each round **a few more questions** on the following screens, e.g. about your estimate. After that, the round is over.

QUESTIONS AND CONTACT

By **clicking on the "Instructions" link at the top right of the survey screens**, you can view, download, or print this document again.

If you still have **questions**, you can reach the survey team by clicking on the "Contact" link at the top of the survey screens. You are also welcome to send any comments or suggestions regarding this survey.

Screen: Decision

Viewed in rounds: all

Signed-in user:: **z2683b** Instructions Logout

Login successful.

Interactive Survey

Previous Rounds

Round	Your allocation to your private account	Your allocation to the group account	Average allocation of the three other group members to the group account	Number of decisions submitted in your group (*)
1	40	40	36.67	4
2	45	35	38.33	4

[X-FREE:] (*) For whoever does **not** submit a decision, the **default rule** for the allocation of points applies (as from round 2). This is **for you personally**:
80 MP for your private account, **0 MP** for the group account.
[X-COOP:] (*) For whoever does **not** submit a decision, the **default rule** for the allocation of points applies (as from round 2). This is **for you personally**:
0 MP for your private account, 8**0 MP** for the group account.
[SELF:] (*) For whoever does **not** submit a decision, the **self-defined default rule** for the allocation of points applies (as from round 2). You have chosen:
C MP for the private account, **D MP** for the group account.

Calculate earnings on a trial basis here:
Payoff calculator

Today’s decision

How would you like to distribute your 80 MarketPoints in this round?

Allocation to your private account:

Allocation to the group account:

Absenden (Submit)

[X-FREE:]

Angemeldeter Benutzer: **z9343b** Anleitung Logout

Erfolgreich angemeldet.

Interaktive Umfrage

Frühere Runden

Runde	Ihre Zuteilung zu Ihrem privaten Konto	Ihre Zuteilung zum Gruppenkonto	Durchschnittliche Zuteilung der drei anderen Gruppenmitglieder zum Gruppenkonto	Anzahl eingegangener Entscheidungen in Ihrer Gruppe (*)
1	10	70	27.67	4
2	50	30	28.33	3

(*) Wer **keine** Entscheidung übermittelt, für den gilt die **Voreinstellung** für die Verteilung der Punkte (ab Runde 2). Diese ist **für Sie persönlich:**
80 MP für das private Konto, **0 MP** für das Gruppenkonto.

Berechnen Sie Verdienste probeweise hier:
Verdienstrechner

Heutige Entscheidung

Wie möchten Sie Ihre 80 MarketPoints in dieser Runde verteilen?

Zuteilung zu Ihrem privaten Konto:

60

Zuteilung zum Gruppenkonto:

20

Absenden

[SELF:]

Angemeldeter Benutzer: **z0285b** Anleitung Logout

Interaktive Umfrage

Frühere Runden

Runde	Ihre Zuteilung zu Ihrem privaten Konto	Ihre Zuteilung zum Gruppenkonto	Durchschnittliche Zuteilung der drei anderen Gruppenmitglieder zum Gruppenkonto	Anzahl eingegangener Entscheidungen in Ihrer Gruppe (*)
1	40	40	21.33	4
2	10	70	18.00	3

(*) Wer **keine** Entscheidung übermittelt, für den gilt die **selbst festgelegte Voreinstellung** für die Verteilung der Punkte (ab Runde 2). Sie haben festgelegt:
10 MP für das private Konto, **70 MP** für das Gruppenkonto.

Berechnen Sie Verdienste probeweise hier:
Verdienstrechner

Heutige Entscheidung

Wie möchten Sie Ihre 80 MarketPoints in dieser Runde verteilen?

Zuteilung zu Ihrem privaten Konto:

60

Zuteilung zum Gruppenkonto:

20

Absenden

Screen: Payoff calculator

Viewed in rounds: 1-10 (if accessed)

Payoff calculator

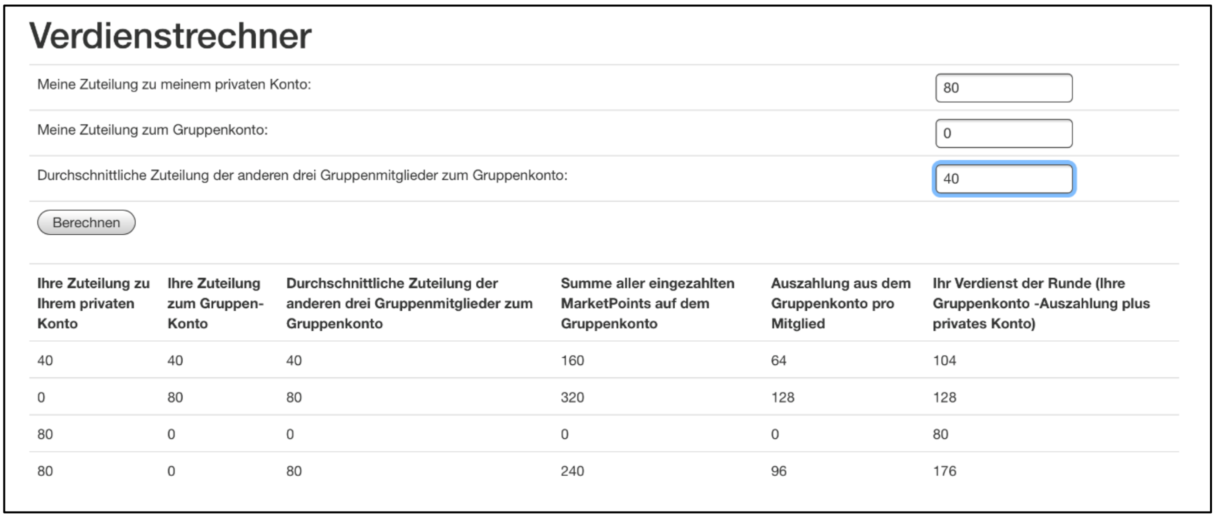
My allocation to my private account:

My allocation to the group account:

Average allocation of the three other group members to the group account:

Calculate

Your allocation to your private account	Your allocation to the group account	Average allocation of the three other group members to the group account	Sum of all deposited MarketPoints on the group account	Payout per member from the group account	Your payoff of the round (your payout from group account plus private account)
40	40	20	100	40	80
0	80	80	320	128	128
80	0	80	240	96	176



Verdienstrechner

Meine Zuteilung zu meinem privaten Konto: 80

Meine Zuteilung zum Gruppenkonto: 0

Durchschnittliche Zuteilung der anderen drei Gruppenmitglieder zum Gruppenkonto: 40

Berechnen

Ihre Zuteilung zu Ihrem privaten Konto	Ihre Zuteilung zum Gruppen-Konto	Durchschnittliche Zuteilung der anderen drei Gruppenmitglieder zum Gruppenkonto	Summe aller eingezahlten MarketPoints auf dem Gruppenkonto	Auszahlung aus dem Gruppenkonto pro Mitglied	Ihr Verdienst der Runde (Ihre Gruppenkonto -Auszahlung plus privates Konto)
40	40	40	160	64	104
0	80	80	320	128	128
80	0	0	0	0	80
80	0	80	240	96	176

Screen: Non-participation default rule

Viewed in rounds: 1

Signed-in user: **z8046b** Instructions Logout

Interactive Survey

Default rule

If you do not actively submit a decision on the allocation of MarketPoints in one of the remaining nine rounds of the survey, it is not a problem. Your MPs are not lost! The following applies:

If you do not submit **an active decision** on the allocation of MPs in the following rounds, then a **default** allocation is used instead. This applies to all group members for whom an active decision is missing in a round.

[X treatments:] **Your personal** default rule is displayed below. It applies **only to you personally** and to all further rounds of the survey, whenever you do **not** submit a decision. **The other group members'** default rules **may be different or the same.**

Default Setting:

Allocation to your private account if **no** decision has been made: 80 [X-COOP: 0]

Allocation to the group account, if **no** decision has been made: 0 [X-COOP: 80]

Continue

[SELF:] Please set **your own** default rule now and enter it in the answer fields below. It will apply to all further rounds of the survey, whenever you do **not** submit a decision. The other members of your group will also set a personal default setting. However, it will not be disclosed. Likewise, **your** default setting will **not** be communicated to the other group members.

Set the default rule:

Allocation to your private account if **no** decision has been made:

Allocation to the group account, if **no** decision has been made:

Continue

[X-FREE:]

Angemeldeter Benutzer: **z1789b** Anleitung Logout

Interaktive Umfrage

Voreinstellung

Wenn Sie in einer der restlichen neun Runden der Umfrage keine aktive Entscheidung über die Verteilung der MarketPoints übermitteln, ist das kein Problem. Ihre MP sind nicht verloren! Es gilt:

> Wenn Sie in den folgenden Runden **keine aktive Entscheidung** über die Verteilung der MPs übermitteln, dann wird stattdessen eine **voreingestellte** Verteilung herangezogen. Das gilt für alle Gruppenmitglieder, für die eine aktive Entscheidung in einer Runde fehlt.

Ihre persönliche Voreinstellung wird Ihnen unten angezeigt. Sie gilt **nur für Sie persönlich** und für alle weiteren Runden der Umfrage, immer wenn Sie **keine** Entscheidung abgeben. Die Voreinstellungen **für andere Gruppenmitglieder können anders oder gleich sein**.

Voreinstellung:

Zuteilung zum privaten Konto, wenn keine Entscheidung vorliegt:

80

Zuteilung zum Gruppenkonto, wenn keine Entscheidung vorliegt:

0

Weiter

[SELF:]

Angemeldeter Benutzer: **z0285b** Anleitung Logout

Interaktive Umfrage

Voreinstellung

Wenn Sie in einer der restlichen neun Runden der Umfrage keine aktive Entscheidung über die Verteilung der MarketPoints übermitteln, ist das kein Problem. Ihre MP sind nicht verloren! Es gilt:

> Wenn Sie in den folgenden Runden **keine aktive Entscheidung** über die Verteilung der MPs übermitteln, dann wird stattdessen eine **voreingestellte** Verteilung herangezogen. Das gilt für alle Gruppenmitglieder, für die eine aktive Entscheidung in einer Runde fehlt.

Bitte legen Sie **Ihre eigene** Voreinstellung jetzt **selbst fest** und tragen Sie sie in die Antwortfelder unten ein. Sie gilt für alle weiteren Runden der Umfrage, immer wenn Sie **keine** Entscheidung abgeben. Die anderen Mitglieder Ihrer Gruppe legen ebenfalls eine persönliche Voreinstellung fest. Sie erfahren diese jedoch nicht. Ebenso wird **Ihre** Voreinstellung **nicht** den anderen Gruppenmitgliedern mitgeteilt.

Festlegen der Voreinstellung:

Für die Zuteilung zum private Konto:

Für die Zuteilung zum Gruppenkonto:

Weiter

Screen: Beliefs about chosen defaults

Viewed in rounds: 1 (SELF only)

Signed-in user: **z0686h** Instructions Logout

Interactive Survey

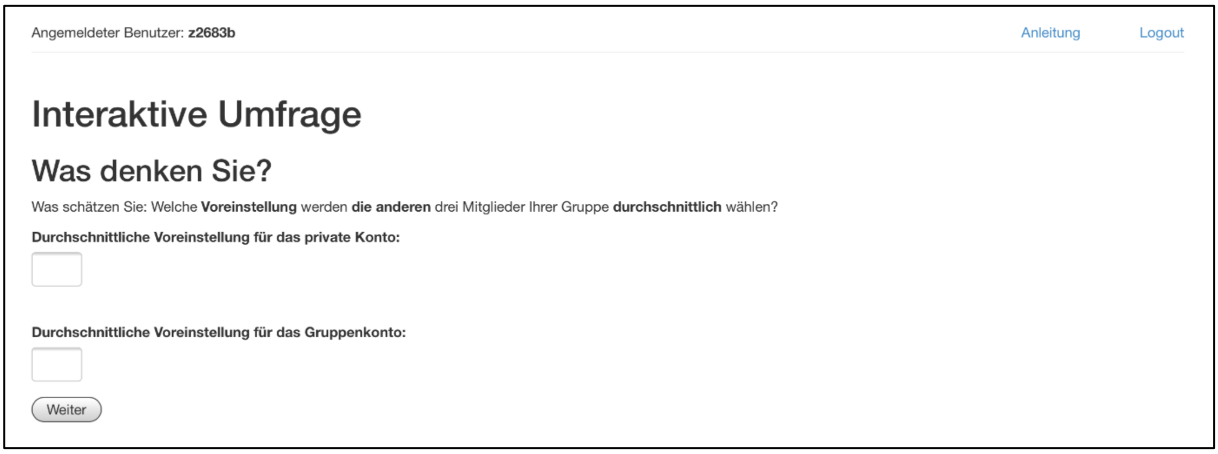
What do you think?

What is your estimate? **On average**, which **default rule** will the three **other** members of your group choose?

Average default rule for the private account:

Average default rule for the group account:

Weiter (Continue)



Angemeldeter Benutzer: **z2683b** Anleitung Logout

Interaktive Umfrage

Was denken Sie?

Was schätzen Sie: Welche **Voreinstellung** werden **die anderen** drei Mitglieder Ihrer Gruppe **durchschnittlich** wählen?

Durchschnittliche Voreinstellung für das private Konto:

Durchschnittliche Voreinstellung für das Gruppenkonto:

Weiter

Screen: Belief elicitation

Viewed in rounds: 1

Signed-in user: **z8046b** Instructions Logout

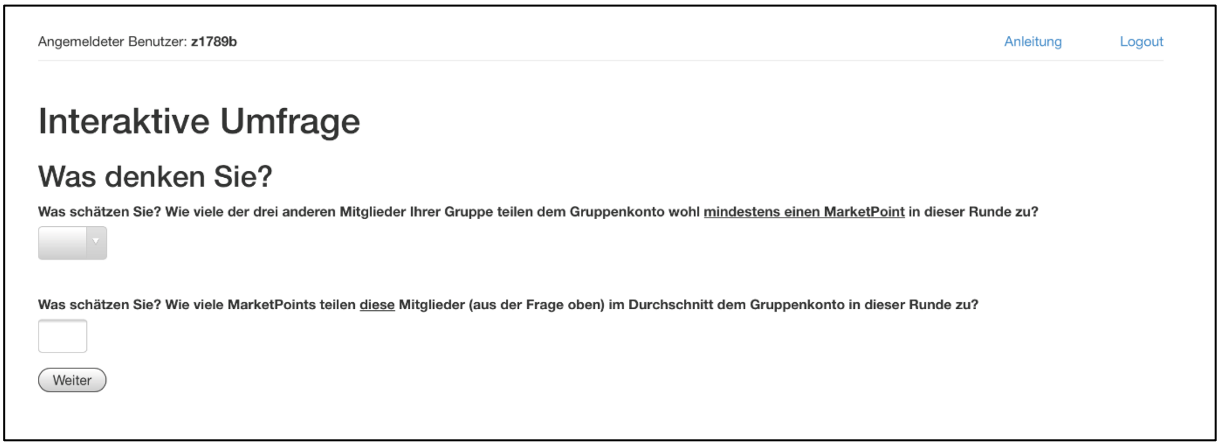
Interactive Survey

What do you think?

What is your estimate? How many of the three other members of your group will allocate <u>at least one MarketPoint</u> to the group account in this round?

What is your estimate? How many MarketPoints will <u>those</u> members (from the question above) allocate to the group account on average in this round?

Weiter (Continue)


Angemeldeter Benutzer: **z1789b** Anleitung Logout

Interaktive Umfrage

Was denken Sie?

Was schätzen Sie? Wie viele der drei anderen Mitglieder Ihrer Gruppe teilen dem Gruppenkonto wohl <u>mindestens einen MarketPoint</u> in dieser Runde zu?

Was schätzen Sie? Wie viele MarketPoints teilen <u>diese</u> Mitglieder (aus der Frage oben) im Durchschnitt dem Gruppenkonto in dieser Runde zu?

Weiter

Screen: Belief elicitation

Viewed in rounds: 2-10

Signed-in user: **z0686h** Instructions Logout

Interactive Survey

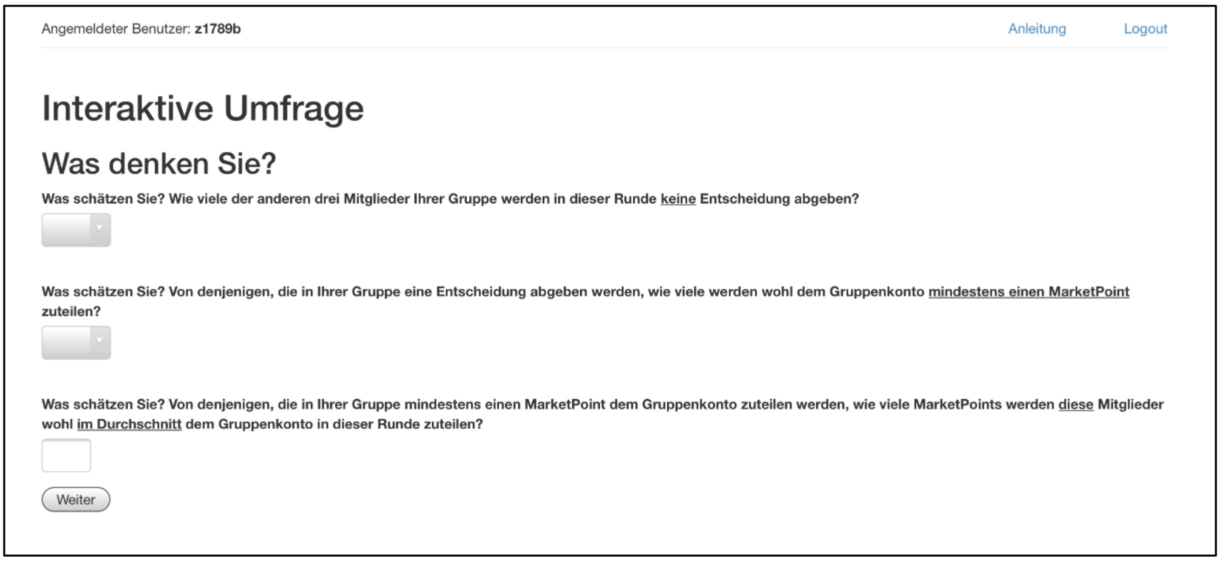
What do you think?

What is your estimate? How many of the three other group members will <u>not</u> submit a decision in this round?

What is your estimate? Among those who will submit a decision in your group, how many do you think will allocate <u>at least one</u> MarketPoint to the group account?

What is your estimate? Among those in your group who will allocate at least one MarketPoint to the group account, how many MarketPoints <u>on average</u> do you think <u>these</u> members will allocate to the group account in this round?

Weiter (Continue)



Angemeldeter Benutzer: **z1789b** Anleitung Logout

Interaktive Umfrage

Was denken Sie?

Was schätzen Sie? Wie viele der anderen drei Mitglieder Ihrer Gruppe werden in dieser Runde <u>keine</u> Entscheidung abgeben?

Was schätzen Sie? Von denjenigen, die in Ihrer Gruppe eine Entscheidung abgeben werden, wie viele werden wohl dem Gruppenkonto <u>mindestens einen MarketPoint</u> zuteilen?

Was schätzen Sie? Von denjenigen, die in Ihrer Gruppe mindestens einen MarketPoint dem Gruppenkonto zuteilen werden, wie viele MarketPoints werden <u>diese</u> Mitglieder wohl <u>im Durchschnitt</u> dem Gruppenkonto in dieser Runde zuteilen?

Weiter

Screen: Dismissal

Viewed in rounds: 2-10

Signed-in user: **z0686h** Instructions Logout

Interactive Survey

Thank you for your answers in this round. We will notify you by email when the survey continues.


Angemeldeter Benutzer: **z1789b** Anleitung Logout

Interaktive Umfrage

Vielen Dank für Ihre Antworten in dieser Runde. Wir werden Sie per E-Mail benachrichtigen, wenn es weitergeht.

Invitation email rounds 2-10: Sent in rounds: 2-10 Adressees: all subjects Subject line: Interactive Survey: Invitation to the <2nd/3rd/…/final> round

Dear participant,

Today the **<2nd/3rd/…/final>** round of the interactive survey starts. You have **one day** to log in and submit your answers. There are ten rounds in total.

Just click on the following link (or copy and paste the text into the address line of your browser):

http://www.interaktive-umfrage.de

Too busy at the moment? No Problem! Your MarketPoints are not lost in this round, even if you do not take action. Whoever does not participate in a round, for that person

[X-FREE / X-COOP:] a **default rule** for the allocation of points applies. This is **in your case**: **A MP** for the private account, **B MP** for the group account. The other participants can have the same or a different default rule.

[SELF:] the **self-defined default rule** for the allocation of points applies. You have chosen: **C MP** for the private account, **D MP** for the group account.

Please find below your personal login data again for your convenience ("0" and "1" are numbers):

```
Participant ID:   z1234h
Password:         1a2b3
```

Kind regards,

Your interactive survey team
in cooperation with GlobalTestMarket