Do monetary incentives matter for identifying social preferences?

Registered Report (**Revised Proposal**)

submitted to the Special Issue of Experimental Economics on Incentivization *

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Abstract

This document is a proposal for a registered report for the special issue of Experimental Economics on Incentivization. Social preferences play an important role in a wide variety of domains. It is therefore important to measure them accurately. In this context, a particularly important question is whether monetary incentives matter for their identification. In this proposal, we provide the details of an experiment with a general population sample aimed at answering the following questions: i) Do incentives affect subjects' willingness to pay to *increase*, and their willingness to pay to *decrease* the payoff of others? ii) Do incentives affect the *distribution* of social preferences? iii) Do incentives affect the *precision* of estimated parameters of a model of inequality aversion? In addition to details on the proposed research design, we also outline our plan for the data analysis. In our view, our study will offer valuable insights on the role of incentives for the identification of social preferences, *no matter the outcome of the empirical analysis*. Thus, we believe that this proposed research would be a very timely contribution to this special issue on the role of incentivization in experimental economics.

Key Words: Social Preferences, Altruism, Inequality Aversion, Incentives

JEL Codes: C80, C90, D30, D63

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1 Introduction

Social preferences have been shown to play an important role in a wide variety of domains such as the labor market (e.g., Fehr et al., 1993; Charness, 2000; Bellemare and Shearer, 2007; Dur, 2009; Kube et al., 2012), bargaining decisions (e.g., Camerer and Thaler, 1995; Camerer and Loewenstein, 1993; Camerer, 2011), political economy (e.g., Tyran and Sausgruber, 2006; Kerschbamer and Müller, 2020; Fisman et al., 2017; Fehr ⓡ al., forthcoming), and contract design (e.g., Bierbrauer and Netzer, 2016; Bierbrauer et al., 2017; Schmidt and Ockenfels, 2021; Fehr et al., 2021), among others. Given their theoretical and empirical relevance, it is critical to measure them accurately. In this context, a particularly important question is whether monetary incentives matter for the elicitation of social preferences.

In contrast to other disciplines, the use of monetary incentives has been a pillar of experimental economics (Plott, 1986; Smith, 1982, 1991; Hertwig and Ortmann, 2001). Among others, behaviors measured using incentivized decisions are believed to more accurately capture subjects' "true preferences" than self-reported answers in hypothetical scenarios. In the context of social preferences, where concerns regarding social desirability are particularly high, there are good reasons to believe that monetary incentives might matter for the weight that people put on others' payoff. Consider a standard dicator game where subjects are asked to decide how to split USD 10 with an anonymous recipient. When the decision is hypothetical, i.e., when no money is at stake, sharing the money and behaving in an altruistic way is costless, which might lead to an overestimation of the extent to which individuals are other-regarding. Tying subjects' payment to their decisions mitigates this issue by forcing decision makers to carefully tradeoff their own material benefit with other-regarding concerns. However, incentivizing decisions also comes at a cost for researchers, who often need to spend thousands of dollars to incentivize the decisions of their participants. These costs can rapidly explode when measuring social preferences, since the decisions of subjects generally affect third parties that also need to be paid. For example, in an incentivized dictator game, the experimenter must not only pay out the decision maker, but also the recipient with whom the decision maker is paired.

This raises the question whether monetary incentives improve our measurement of social preferences, or whether it is possible to design a non-incentivized elicitation method that mimics the properties of an incentivized task. In this *registered report*, we propose to conduct an experiment that will allow us to answer this question in a sample that is broadly representative of the US population.

We plan to rely on a general population sample because such samples are being increasingly used by scientists to understand the role of preferences and beliefs for economic behavior. For example, broad population samples have been used to study questions such as the role of social preferences for political support for redistribution (Fisman et al., 2017; Kerschbamer and Müller, 2020; Fehr ⓡ al., forthcoming), to get a deeper understanding about how people reason about the economy (Andre et al., 2022; Stantcheva, 2021), or to study the role of beliefs for willingness to act against climate change (Dechezleprêtre et al., 2022; Falk et al., 2021), among others. The question of the role of monetary incentives for the identification of social preferences in broad population samples is particularly relevant given that several survey providers do *not* allow to incentivize decisions, i.e., they do *not* allow to pay participants on the basis of their decisions. In this context, understanding whether incentivization matters is an important empirical question.

Following Fehr ⓡ al. (forthcoming, 2023), we will elicit social preferences using a set of choice situations in which the decision maker has to decide on how to allocate "points" between herself and an anonymous partner. While the literature has often relied on simple dictator games (i.e., choice situations in which subjects can sacrifice resources to *increase* the payoff of others) to identify social preferences, our experimental paradigm also includes several decision situations where the decision maker can pay in order to *decrease* the payoff of others. This design allows for the identification of a broader range of social preferences. Indeed, while standard dictator games are well suited to identify altruism and the extent to which individuals are willing to trade-off equality and efficiency (see, e.g., Fisman et al., 2007, 2017), they do *not* allow to identify a broad range of other social preferences. For example, inequality aversion (Fehr and Schmidt, 1999; Charness and Rabin, 2002; Bolton and Ockenfels, 2000) implies that individuals might not only be willing to sacrifice some of their own payoff to increase the payoff of those worse off (aversion to advantageous inequality), but also to decrease the pay-off of those who are better off (aversion to disadvantageous inequality). Similarly, envious and spiteful individuals are willing to pay to destroy the payoff of others. Thus, by including choice situations where the decision maker can pay to decrease the payoff of others, our design solves this identification problem.

Our key treatment variation manipulates whether decisions are incentivized or not. In the *Hypothetical* treatment, we do *not* incentivize subjects' choices. In contrast, we incentivize subjects' decision in two treatments (*Low-Incentives* and *High-Incentives*) by paying them (and the recipients) on the basis of their choice in a randomly drawn choice situation. These treatments allow us to cleanly assess whether and how monetary incentives affect the measurement of social preferences. Moreover, the increase in stakes size between the *Low-Incentives* and the *High-Incentives* treatments, which are scaled by a factor of 5, allows us to assess whether stake size matters.

We outline our plan for the data analysis in the last section of this proposal. As discussed therein, we are interested in understanding the role of monetary incentives for social preferences at three levels.

First, we are interested in understanding whether monetary incentives affect subjects' choices at the descriptive level. In particular, we will analyze whether monetary incentives influence subjects' willingness to pay to *decrease* the other participants' payoff, and whether it affects their willingness to pay to *increase* the other participants' payoff.

Second, we will assess whether monetary incentives play a role for the *distribution* of social preferences. We will uncover the distribution of preferences in each treatment by applying the Dirichlet Process means (DP-means) algorithm, a Bayesian nonparametric clustering algorithm. This approach has several advantages. In particular, this approach enables the identification of preference types without committing to a pre-specified number of different preference types. Moreover, it does *not* requires an ex-ante specification or parameterization of types. It also does *not* presume a specific error structure. We discuss these, and other aspects of this procedure, in greater details in the results section.

Third, we will assess whether monetary incentives allow for a more *precise* estimation of social preferences parameters. To answer this question, we will compare the (distributions of) structural estimates of a model of inequality aversion (Fehr and Schmidt, 1999; Charness and Rabin, 2002) across the different treatments.

While our main analysis will rely on a large sample drawn from the general population, it is also interesting to understand the role of monetary incentives for the identification of social preferences in students. Indeed, students samples are still widely used in economic experiments, and insights from the general population might not extend to students. To shed light on this issue, we also plan to collect an additional sample of students carefully prescreened from Prolific (for details, see Section 3.4). The advantage of recruiting students directly from Prolific is that it keeps the entire experimental protocol constant, thereby enhancing the comparability between the results from the general population and the results from the student sample. To examine the effects of incentivization on the social preferences of students, we will largely reproduce the analysis conducted on the general population sample. This will allow us to assess whether the insights gathered from the general population also extend to students.[1]

In our view, this proposed study contributes to and improves on the existing literature (we discuss the literature in the next section) on the effects of monetary incentives for the identification of social preferences in several ways.

First, our study goes beyond comparing choices in dictator games or ultimatum games across incentives conditions. Instead, we study whether incentives matter

- for subjects' willingness to pay to *increase* the payoff of others, and for their willingness to *decrease* the payoff of others
- for the *distribution* of social preferences, where we uncover social preferences using state-of-the-art Bayesian nonparametric methods.
- for the *precision* of structural estimates.

As such, the scope of this proposal is—to the best of our knowledge—much broader than the existing studies on the role of monetary incentives for social preferences.

Second, while previous research has investigated the effects of monetary incentives for behavior in dictator games (we review this literature in the next section), these studies often rely on student samples of relatively modest size. In contrast, our proposed study aims at answering this question in a much larger sample drawn from general population. We believe that addressing this question with a broad population sample is important given that such samples are being increasingly used by researchers. Moreover, previous research suggests that results from students samples might not always generalize to the general population (see, e.g., Falk et al., 2013; Anderson et al., 2013; Bellemare et al., 2011, 2008; Epper ⓡ al., 2023). It is therefore not clear whether the conclusions drawn from the existing evidence based on student samples would also generalize to the general population. Our proposed study will shed light on this issue.

Third, the fact that this proposal transparently outlines our research questions and our plan for the data analysis ex-ante lends further credibility to the empirical findings we will document. In this sense, this study will also contribute to the broader discussion on best

[1] Note, however, that it is *not* the aim of this paper to provide an extensive analysis of the differences in the social preferences of students and the general population. We address this specific research question in a separate paper (Epper ⓡ al., 2023).

practices in scientific research and the role that pre-registered studies and registered reports can play at increasing transparency in science (Nosek et al., 2018; Munafò et al., 2017; Miguel, 2021).

Finally, our experimental paradigm allows for the identification of a wide range of social preferences in a very portable way, and could easily be adopted by other researchers interested in measuring social preferences. In this context, it is important to establish whether relying on incentivized decisions is critical, whether stake size matters, or whether hypothetical questions suffice. If our results show that monetary incentives are *not* needed for the identification of social preferences, this would enhance the value of this elicitation method even further, as hypothetical questions can easily be incorporated into studies—including studies conducted with general population samples. If, in contrast, our results show that monetary incentives or stake sizes affect the identification of social preferences, we will be able to measure how severe this mismeasurement is. This could be useful for researchers willing to know how "off" their social preferences estimates might be when they rely on hypothetical or low-stakes decisions. Overall, we believe that our study will deliver valuable insights on the role of incentives for the identification of social preferences, *no matter the outcome of the empirical analysis*.

The remainder of the paper is organized as follows: In the next section, we review the literature on the effects of incentives for the identification of social preferences. In section 3, we provide details on the experimental design, the sample and the study implementation. We outline our plan for the data analysis in Sections 4. Finally, we outline our main hypotheses and the possible implications of our findings in section 5.

2 Literature Review

Our paper is connected to the literature interested in the effects of using monetary incentives in experimental research. In particular, our paper is linked to studies that have investigated the effects of monetary rewards for behavior in dictator games using student samples.[2] An

[2]We focus on studies that compare hypothetical versus incentivized decisions in dictator games because this is the main focus of our study. However, note that some papers have also looked the effects of stake sizes and alternative incentivization techniques (e.g., probabilistic payment) for giving in dictator games. We do not review these papers here for brevity. For interested readers, we recommend the excellent meta-analyses by Engel (2011) and by Larney et al. (2019). These reviews also cover a handful of other studies which we do not report here due to their very low sample sizes or their lack of randomization of treatment conditions.

early contribution to this literature is Forsythe et al. (1994), who conduct an experiment where they compare the effects of incentivized versus hypothetical decisions in a dictator game. They find that dictators donate significantly less when decisions are incentivized.[3] More recently, Bühren and Kundt (2015) report on an experiment where participants are asked to make decisions in three games (a prosocial game, an envy game, and a sharing game) which allows them to disentangle different forms of social preferences. Subjects are randomized into a treatment condition where decisions are incentivized, and a condition where decisions are hypothetical. They show that monetary incentives affect choices and the subsequent categorization of individuals into different social preference classes. In particular, they find that participants in the incentives treatment display more spite and less inequality aversion. Clot et al. (2018) conduct an experiment aimed at comparing the effects of different compensation mechanisms in a dictator game where participants have to decide how to allocate \$10. Participants are randomized into the following treatments: i) a treatment where decisions are implemented, ii) a treatment where decisions are hypothetical, iii) a treatment where the decisions of only 1 in 10 dictators are implemented, and iv) a treatment where the decisions of only 1 in 10 dictators are implemented, but the stake size is increased to \$100 (so as to maintain expected value constant). They find that hypothetical stakes lead to fewer egoistic and more egalitarian decisions. In addition, they find that probabilistic payment does not affect decisions, holding stakes constant. When stakes differ but expected stakes are constant, they report that dictators behave in a more egoistic way when their decision is implemented with certainty.

Engel (2011) reviews the literature on dictator games in his meta-analysis, covering over 100 papers published prior to 2010. On the role of incentives, he concludes that dictators' behavior when decisions are incentivized is *not* significantly different than when their decisions are hypothetical. He also finds that uncertainty regarding whether dictator's decisions will be implemented or not is *not* significantly related to giving. Finally, he only finds very weak evidence that stakes size significantly affects giving, despite covering studies with dramatic changes in stakes sizes ranging from \$0 to \$130.[4] These results are largely confirmed by a

[3] In addition, they also investigate whether stakes size (\$5 vs \$10) make a difference, and whether the conclusions drawn in dictator games carry over to ultimatum games. They can neither reject the hypothesis that monetary incentives do not affect proposals in the ultimatum game, nor the hypothesis that stakes size does not affect behavior. However, note that these experiments have low statistical power.

[4] The relation between stake size and giving is insignificant when he uses all the papers covered in his meta-analysis. However, when he focuses on studies that explicitly manipulated stakes sizes, he finds that higher stakes significantly reduce dictators' willingness to give. However, he qualifies this

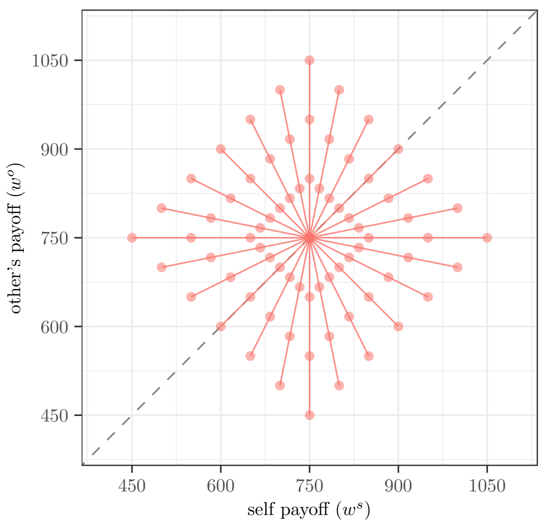
more recent meta-analysis by Larney et al. (2019), who find a significant but small effect of stake size on dictator games offers (and no effect of stakes on ultimatum game offers). Note, however, that this meta-analysis excluded studies with hypothetical stakes, and therefore cannot provide evidence on the effects of incentivization *per se*.

3 Experimental design

3.1 Measuring distributional preferences

We will elicit respondents' distributional preferences using a series of *twelve* incentivized money allocation tasks in which participants have to decide how to allocate experimental currency units (ECUs) between themselves and an anonymous other participant of the study. Figure 1 depicts these 12 budget lines, where the decision maker's own payoff is represented on the x-axis and the recipient's payoff is on the y-axis.[5]

Figure 1: Budget lines used to identify other-regarding preferences



These twelve choice situations systematically vary the cost and the efficiency consequences of redistribution, thereby allowing us to identify a wide range of other-regarding

effect as "*very small*", despite the wide variation in stake size.

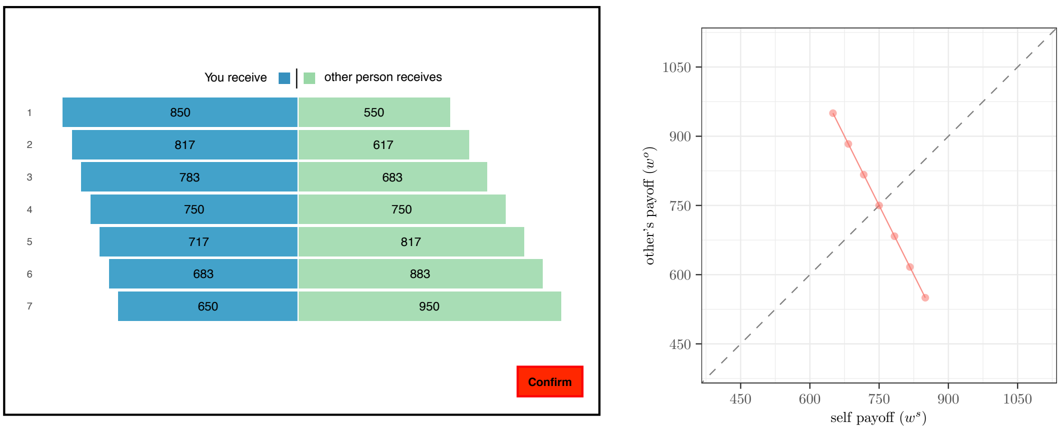
[5]The design is based on Fehr ⓡ al. (forthcoming, 2023). We provide further details on the various budget lines used for the identification of social preferences in Table 1 in the Appendix A.1.1. In addition, our design also comprises eight more choice situations that we can use to further validate the behavioral interpretation of the types identified. We provide further details on these additional budget lines in Appendix A.1.2.

behaviors. Negatively sloped budget lines (where subjects can sacrifice resources to *increase* the payoff of the other participant) allow us to identify behaviors such as altruism and aversion to advantageous inequality. Positively sloped budget lines (where subjects can pay to *decrease* the payoff of the other) allow us to identify behaviors such as envy, spite and aversion to disadvantageous inequality, among others.

Choice situations will be presented to subjects in random order directly on subjects' screens. They will be presented in a way that makes the distributional consequences of each allocation transparent. Figure 2a illustrates how a typical choice situation will be presented to our participants. In each choice situation, subjects will be able to choose between seven interpersonal allocations (labeled by 1 to 7)—all of them located on a budget line. Each available allocation consists of a specific distribution of ECUs between the participant (bars labeled by "You receive") and the other person (bars labeled by "other person receives"). We represent the available choices numerically and graphically in order to make the trade-offs and the associated payoff implications salient. Figure 2b plots the budget line corresponding to the example depicted in Figure 2a in the (ω^s, ω^o)-space ("own payoff", "other's payoff"). In this example, the slope of the budget line is -2, indicating that for every ECU the decision maker gives up, the other player receives 2 ECUs. Here, perfect equality in payoffs can be achieved by choosing allocation 4.

Figure 2: Example choice situation

(a) Decision screen (b) Budget line



3.2 Treatments

To assess the effects of monetary incentives for the identification of social preferences, we will implement the following three treatments in a *between* subjects design.

Hypothetical In this treatment, participants choices are hypothetical. Thus, their decisions neither affects their own payoff, nor the payoff of another participant. However, our instructions invite subjects to make decisions *as if* they were incentivized. We choose this approach because we are interested in the extent to which it is possible to elicit preferences in a non-incentivized survey such that they mimic as closely as possible the preferences elicited in an incentivized survey.

Low-Incentives In this treatment, the decisions of participants have real monetary consequences for them and for another, anonymous, participant of the study.[6] The exchange rate between points and USD is set to 500 points = USD 1.

High-Incentives In this treatment, the stake size is substantially larger than in the Low-Incentives treatment, as we set the exchange rate to 100 points = USD 1, i.e., stakes are *five times larger* than in the Low-Incentives treatment.

Importantly, we make the consequences of subjects' decisions salient by highlighting them in the relevant parts in the instructions (bold fonts). In addition, we add a control question that is specifically aimed at ensuring that participants understand whether their decisions will have real monetary consequences or not (see the control questions at the end of the instructions in Appendix C). This will help us validate our treatment effect.

In principle, it also would have been interesting to include further treatments in our study. For example, we could have added a treatment condition in which the subjects earn their endowment on the basis of a real-effort task. We felt, however, that our study is already quite complex and that further treatments would have diverted attention from our main questions – whether a purely hypothetical decision task undermines the reliable identification of social preferences and whether the size of the financial incentives matter. Moreover, previous research (e.g., Fehr ⓡ al., forthcoming; Epper et al., forthcoming) has shown that the social

[6]Importantly, our instructions make it clear that the other participant can *not* affect the decision maker's payoff.

preferences we elicit with our incentivized design have excellent out-of-sample predictive power for real world behaviors such as the demand for redistribution or charitable donations.

3.3 General population sample

Subject pool We will conduct this experiment in a general population sample that is broadly representative of the US population with respect to age and gender. Data collection will be completed in collaboration with Prolific.

Sample size In each treatment, we will collect data from 1000 subjects. Thus, our general population sample will comprise a total of 3000 participants.

Power We discuss the details of the statistical power of our design in Appendix D. Assuming that the standard deviation of individuals' parameter estimates is identical to the one from a previous study (conducted with a general population sample in Switzerland), a significance level of 0.01, and a sample of 1000 subjects per treatment, we achieve a power of 80% with a two-sided, two-sample t-test comparing differences in the inequality aversion parameters for effect sizes beyond 0.25 (i.e., an effect size of about 15 percent of a standard deviation of the structural parameters in the Swiss broad population sample).

3.4 Student sample

While our main analysis will rely on a general population sample, it is also interesting to understand the role of monetary incentives for the identification of social preferences in students. To shed light on this issue, we will collect responses from an additional 300 students carefully prescreened from Prolific. Specifically, we will recruit students from Prolific who jointly meet the following criteria:

- They are located in the USA.
- They are currently studying.
- They are enrolled in an undergraduate (BA/BSc) or graduate program (MA/MSc/MPhil).
- They are aged between 18 and 30.

In our view, subjects who meet these inclusion criteria share the main characteristics of students typically recruited in traditional subject pools for laboratory studies. These subjects will then be randomized into the different treatments, following exactly the same protocol as subjects from the general population sample. The advantage of recruiting students directly from Prolific is that it keeps the entire experimental protocol constant, thereby enhancing the comparability between the results from the general population and the results from the student sample.

3.5 Implementation

Procedure The experiment is fully computerized using Qualtrics and all the instructions are displayed directly on participants' screens. We provide a transcript of the instructions displayed on participants' screens in Appendix C. The study will also include questions about subjects' socio-demographics as well as a few additional survey items. We expect the study to last about 10 to 15 minutes.[7]

Exclusion restrictions The study will include control questions for the money allocation task (see instructions in Appendix C). These questions aim at identifying participants who do not understand the task, or do not pay attention (Berinsky et al., 2014). Participants who fail to pass these control questions will be excluded from the final sample.

Payment All participants are paid a show-up fee of USD 3, provided that they complete the study until the end. In addition, we incentivize respondents' choices in the *Low-Incentives* and the *High-Incentives* treatment by implementing one of their decisions at random.[8]

IRB Approval The study will obtain approval from the Human Subjects Committee of the Faculty of Economics, Business Administration and Information Technology at the University of Zurich.

[7] We piloted a related design (with incentives only), and the average time for reading the instructions of the money allocation task and making the decisions was about 8 minutes. Since the proposed study now also includes a few socio demographics and questionnaire measures, we expect the survey to be slightly longer.

[8] With the show-up fee alone, participants will earn more than the minimum pay of USD 8 per hour requested by Prolific.

Pre-registration The study will be pre-registered on the AEA RCT Registry upon approval of this proposal. The pre-registration will be made prior to the start of the data collection.

4 Plan for the data analysis

In this section, we describe the red line of *how we plan to analyze the data*. We begin each section by highlighting the specific research questions to be answered, followed by a short description of how we will adress these questions.

4.1 Descriptive analysis

In this subsection, we will analyze subjects' choices at the descriptive level. In particular, we are interested in the following questions:

- *How do incentives affect subjects choices in the money allocation task at the descriptive level?*
- *Do incentives affect subjects' willingness to pay to decrease the other participant's payoff?*
- *Do incentives affect subjects' willingness to pay to increase other participant's payoff?*

To answer these questions, we will examine whether the treatments affect the distribution of subects' *modal* choice across negatively sloped budget lines (downward sloping budget lines in Figure 1) and across positively sloped budget lines (upward sloping budget lines in Figure 1) . Negatively sloped budget lines inform us about the amount of money individuals are willing to sacrifice in order to *increase* the payoff of the other individual. In contrast, positively sloped budget lines inform us about the amount of money individuals are willing to sacrifice in order to *decrease* the payoff of the other individual.[9]

For each budget line, we label the own-payoff maximizing allocation by $z = 6$, the own-payoff-minimizing allocation by $z = 0$, and the payoff-equalizing allocation by $z = 3$. The other four available allocations on each budget line are equidistantly placed between 0–3 and 3–6, respectively. We plan to depict the aggregate distribution of individual's modal choices using graphs similar to the example depicted in Figure 3. This allows for an easy comparison of the distribution of choices across treatments. We will then compare the mode distributions across treatments using Kolmogorov-Smirnov tests.

[9] We focus on the modal choice because it is less susceptible to random responses or to outliers.

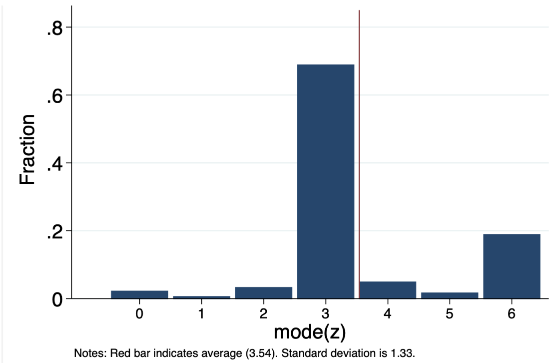
In addition, we also plan to depict subjects' modal choice on positively *and* on negatively sloped budget in each treatment, as depicted in Figure 4. Such a figure is useful for two reasons. First, it might alread hint at the existence of distinct preference types at the descriptive level.[10] Second, it allows to easily assess whether the same qualitative types emerge under both the different treatments. However, this descriptive approach has limitations. For example, it does not allow for a clear identification of the *distribution* of social preferences. We therefore turn to a more rigorous approach in the next section.

[10]In the example depicted in Figure 4, three separate behavioral agglomeration are clearly visible. The first behavioral agglomeration, located at point (6,6), comprises subjects who tend to maximize their own payoff both on positively and on negatively sloped budget lines. Thus, subjects in this behavioral agglomeration can be labelled as "*predominantly selfish*". The second behavioral agglomeration, located at point (3,3), comprises subjects who predominantly equalize payoffs, consistent with the label "*inequality averse*." The last behavioral agglomeration, located at point (6,3), comprises subjects who are willing to pay to increase the payoff of others, but who are not willing to pay to decrease the payoff of others. Such a behavior is consistent with *altruistic* concerns for the worse off. Note, however, that other behavioral agglomerations might appear (e.g., an envious type).

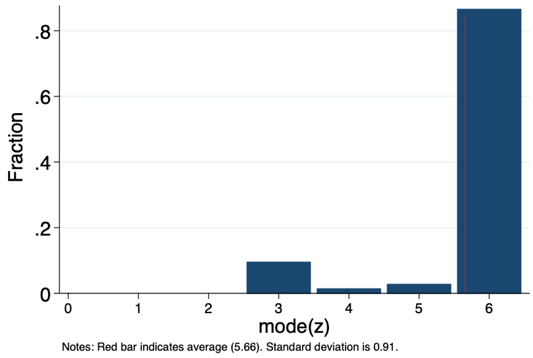
Figure 3: Distribution of modal choices **(EXAMPLE)**

Low-Incentives treatment

(a) Negatively sloped budget lines

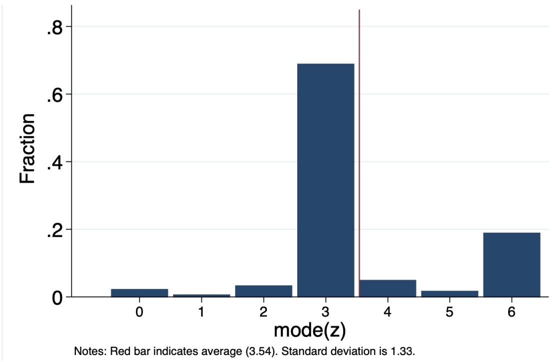


(b) Positively sloped budget lines

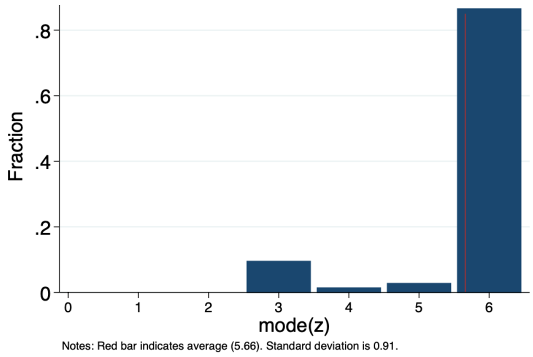


High-Incentives treatment

(c) Negatively sloped budget lines

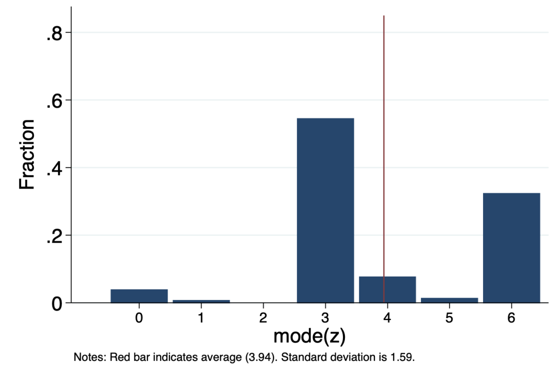


(d) Positively sloped budget lines

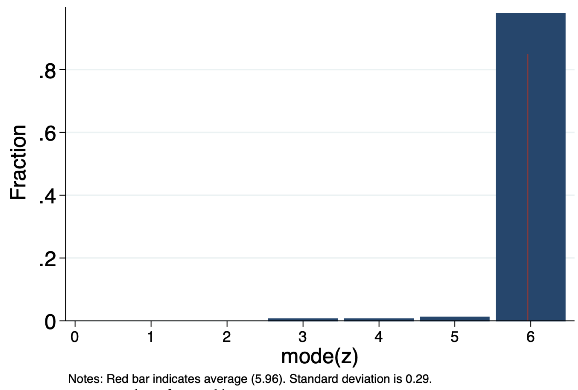


Hypothetical treatment

(e) Negatively sloped budget lines



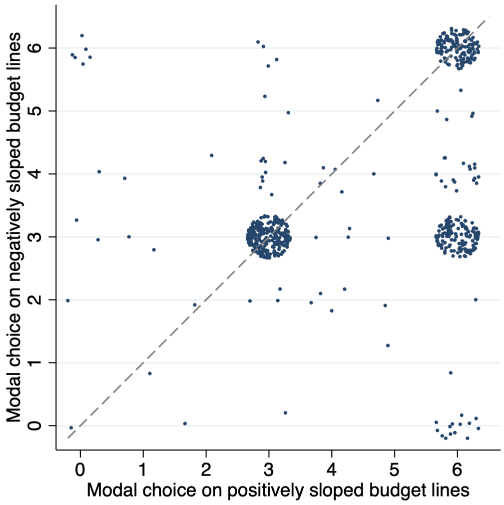
(f) Positively sloped budget lines



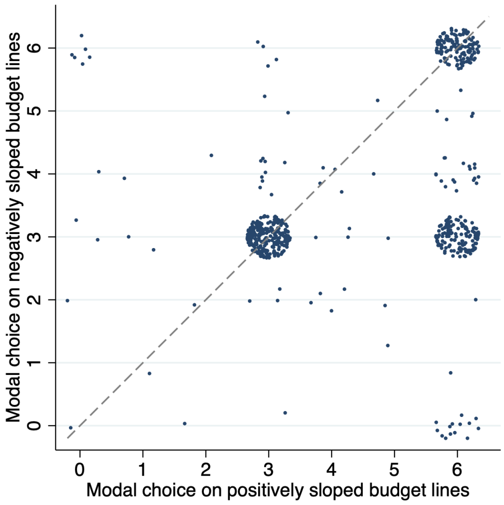
Notes: The distributions depicted here are examples that serve only for illustration purposes.

Figure 4: Descriptive evidence on subjects' modal choices **(EXAMLPE)**

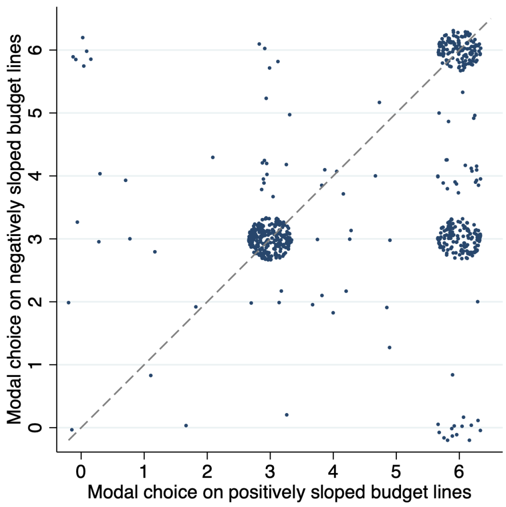
(a) *Low-Incentives* treatment



(b) *High-Incentives* treatment



(c) *Hypothetical* treatment



Notes: The distributions depicted here are examples that serve only for illustration purposes.

4.2 Cluster analysis: Do monetary incentives affect the distribution of social preferences?

In this subsection, we are interested in the following question:

- *Do incentives affect the distribution of social preferences in the population ?*

To identify behavioral heterogeneity in our population more rigorously, we will apply a Bayesian nonparametric approach—the Dirichlet Process (DP) means clustering algorithm

(Kulis and Jordan, 2012). This algorithm groups individuals into clusters according to their *behavioral similarities*. In our context, clusters are based on subjects' 12 distributional choices in the money allocation task, and similarity is measured by "how close" an individual's allocation profile is to the average allocation of a cluster. Ultimately, individuals' are assigned to the cluster whose centroid—i.e. the mean allocation in the 12 distributional choices—is the closest to their own allocation profile in the 12-dimensional space of interest. We describe the formalism of the DP-means algorithm in Appendix B.

An important aspect of the DP-means approach is that it enables the identification of preference types without committing to a pre-specified number of different preference types. Moreover, this approach does neither require an ex-ante specification or parameterization of types, nor does it presume a specific error structure. This means that it remains ex-ante agnostic about key distributional assumptions, and it does not constrain heterogeneity to lie within a predetermined set of models or parameter space.[11] The DP-means algorithm allows for all possible type partitions of the data spanning from a representative agent (i.e. a single data-generating process) up to as many types as there are individuals in the population (i.e. n data-generating processes), i.e., it determines the number of preferences types endogenously. Thus, (i) the actual number of types, (ii) the assignment of each individual to one of the types and (iii) the behavioral (preference) properties of the types emerge endogenously.[12]

We will run the DP-means algorithm separately on each treatment. We will display the distribution of types identified in the different treatments in the Table 1 below.[13] We will then use a χ^2 test to assess whether the distribution of preference types depends on the treatment assignment.

[11] In this regard, our approach differs from previous work (e.g. Bellemare et al., 2008; Fisman et al., 2015, 2017; Bruhin et al., 2018) that characterized preference heterogeneity on the basis of structural assumptions on preferences and error terms.

[12] The fact that the number of types adapts to the data has important benefits (see Kulis and Jordan, 2012). Most notably, as previous work has shown (see Comiter et al., 2016), this feature of the algorithm yields higher quality type-separation than methods that specify the number of types prior to clustering (such as k-means).

[13] Importantly, the DP-means algorithm does *not* assign labels to clusters. In order to do that, one needs to carefully examine subjects' decisions in each cluster. We will do so in a dedicated Appendix. In Table 1, we give examples of possible types that could emerge, but other types such as, e.g. spiteful or efficiency-seeking, could emerge.

Table 1: Type distributions identified using clustering analysis

	Low-Incentives	**High-Incentives**	**Hypothetical**
Type 1 (e.g. Inequality averse)	XXX%	XXX%	XXX%
Type 2 (e.g. Altruistic)	XXX%	XXX%	XXX%
Type 3 (e.g. Selfish)	XXX%	XXX%	XXX%

4.3 Structural Analysis: Do monetary incentives increase the precision of structural estimates?

In this subsection, we are interested in the following question:

- *Do monetary incentives allow for a more precise estimation of social preferences parameters?*

To answer this question, we will estimate the parameters of a model of inequality aversion (Fehr and Schmidt, 1999) *separately* for subjects in different treatments. More specifically, for each treatment we will structurally estimate the model

$$V_i\left(w_{ij}\right) = w_{ij}^s - \alpha_i \max\left\{w_{ij}^o - w_{ij}^s, 0\right\} - \beta_i \max\left\{w_{ij}^s - w_{ij}^o, 0\right\}$$

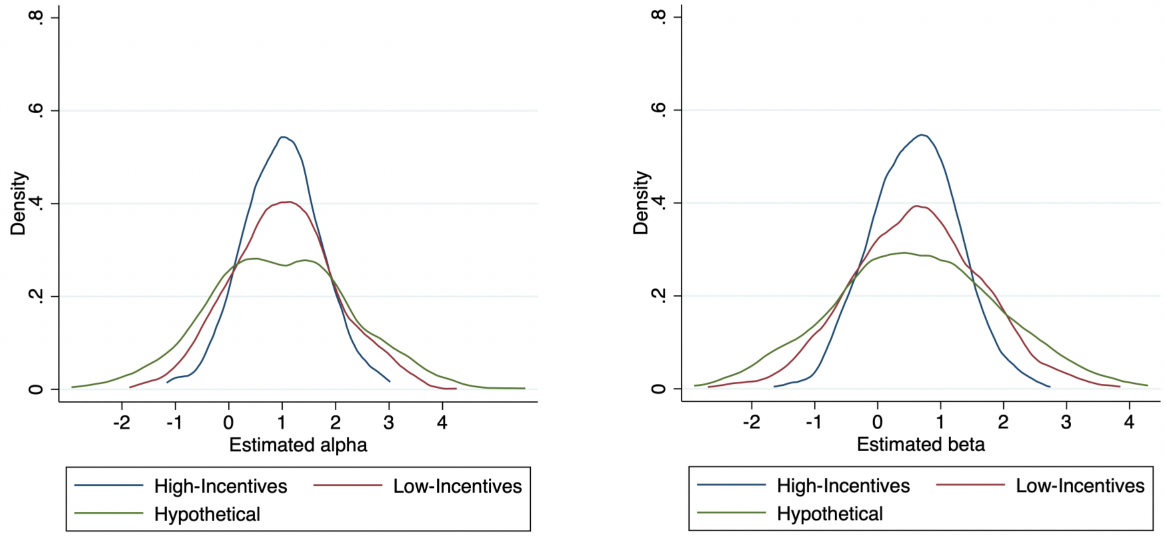
where $w_{ij} = \left(w_{ij}^s, w_{ij}^o\right)$ corresponds to individual i's decision on budget line j on how to allocate money between herself (superscript s for self) and the other person (superscript o for other), α_i denotes aversion towards disadvantageous inequality (behindness aversion) and β_i denotes aversion towards advantageous inequality (aheadness aversion).[14]

Of course, different preference types are expected to be related to different values of α and β. For example, inequality averse individuals are both averse to disadvantageous and to advantageous inequality and are therefore expected to have $\alpha > 0$ and $\beta > 0$. Our main interest in to assess whether incentives allow for a more precise estimation of α and β. We will represent the distribution of these structural parameters graphically using density plots similar to those presented in Figure 5. We will use Kolmogorov-Smirnov tests to assess whether the distributions of structurally estimated parameters depend on the treatment assignment.

[14]Note that the inequality aversion models of Fehr and Schmidt (1999) is equivalent to Charness and Rabin (2002) in the two person case and in absence of restrictions on the α and β parameters.

Figure 5: Distribution of structurally estimated parameters **(EXAMLPE)**

(a) Alpha (b) Beta



Notes: The distributions depicted here are examples that serve only an illustration purposes.

4.4 Do monetary incentives matter for the identification of students' social preferences?

In this subsection, we are interested in the following question:

- *Do monetary incentives matter for the identification of social preferences in students?*

To answer this question, we will rely on the data collected from students. Specifically, we will pool the student sample collected separately (Section 3.4) with the students that populate our general population sample.[15] We will then replicate the analysis conducted with the genreal population sample. Note, however, that it is *not* the aim of this paper to provide an extensive analysis of the differences in the social preferences of students and the general population. We address this research question in a separate paper (Epper ⓡ al., 2023).

5 Hypotheses

In the previous section, we have outlined the precise questions that our experimental design allows us to tackle, and how we plan to address them. Here, we would like to formulate some

[15]By construction, some respondents in the general population sample will be students. We will be able to identify these students using the same questions as the ones used for screening students in the student sample (see Section 3.4 for details).

concrete hypotheses, and highlight what would be the possible implications of treatment differences (or absence thereof).

Broadly speaking, our experimental design allows to shed light on the effects of using monetary incentives for the identification of social preferences. A key feature of our proposal is that we investigate this question at different levels of analysis. In particular, we will examine the *distribution* of qualitatively distinct preference types in a population (clustering analysis), and we will assess both the *strength* and the *precision* of these preferences (structural analysis). Thus, our hypotheses consider both of these dimensions.

Our first hypothesis relates to whether relying on incentivized decisions is critical to identify social preferences, or whether hypothetical questions suffice. In the context of social preferences, where concerns regarding social desirability are particularly high, individuals might not reveal there true preferences in absence of real monetary stakes. For example, it is costless for a subject to behave in an altruistic way if decisions are hypothetical. If that is the case, hypothetical stakes might lead to an overestimation of the extent to which individuals are other-regarding. This can be particularly problematic if, e.g., such estimates are then used to make behavioral predictions. Tying subjects' payment to their decisions might mitigate this issue by forcing decision makers to more carefully tradeoff their own material benefit with other-regarding concerns.

Hypothesis 1. *Monetary incentives do not matter for the identification of social preferences.*

If our results show that monetary incentives do *not* affect social preferences, researchers could more broadly adopt our elicitation procedure with hypothetical stakes. This would be particularly appealing in contexts where incentivization is logistically difficult to organize (e.g., some online studies) or very costly. If, in contrast, our results show that monetary incentives do affect social preferences, then we will be able to assess how large the mismeasurement related to hypothetical stakes is. This could be particularly useful for researchers interested in knowing how imprecise their social preferences estimates are when elicited with hypothetical decisions.

Our second hypothesis relates to the effects of stake size. While the social preferences elicited under hypothetical stakes might differ from those elicited with real monetary stakes (Hypothesis 1), it is also possible that the size of the monetary stakes matters for the identification of social preferences. In particular, larger stakes might affect the distribution of social preferences in a population. This might be the case if, for example, larger stakes induce decision makers to make more selfish decisions. It is also possible that higher stakes increase the precision with which social preferences are estimated, e.g., if higher stakes lead participants to think more carefully about their decisions.

HYPOTHESIS 2. *The strength of monetary incentives does not matter for the identification of social preferences.*

If our results show that the stake size does *not* matter, then researchers could largely rely on low-powered incentives to reliably elicit social preferences. It is, however, also possible that stake size will affect social preferences. In particular, as discussed above, stakes might affect the precision with which social preferences are estimated. If this is the case, then researchers interested in *precisely* estimating the strength of social preferences may want to rely on using high-powered incentives to elicit preferences, whereas researchers only interested in the qualitative nature of preferences may rely on low-powered incentives.

Finally, it is important to note that answers to the two hypotheses above might vary depending on whether one considers results from the clustering analysis or the structural analysis. For example, it is possible that there are no treatment differences in *all* the dimensions in which we assess social preferences, e.g., that the same behavioral types emerge in the same proportions across treatments *and* that the structurally estimated parameters are identical. However, it is also possible that treatment differences exist in only some dimensions. For example, it is plausible that the distribution of preferences types as identified by the clustering remains stable across treatments, but that there are treatment differences in the structurally estimated parameters. Such a result would imply that the nature of the research questions should determine whether monetary incentives should be used or not: Researchers only interested in assessing the qualitative nature of behavioral types and their prevalence in the population could rely on either hypothetical or real monetary stakes, while researcher interested in the quantitative distribution of types and the strength of these preferences should carefully consider using the appropriate incentivization method.

Overall, we believe that a big advantage of our proposed study is that it will provide researchers with extensive information on the different dimensions in which social preferences might differ, and how incentives affect each of these dimensions, so that they can make an informed decision about whether and how to incentivize their measurement of social preferences.

6 Statements and Declarations

See next page.

DISCLOSURE STATEMENT

With reference to the submission:

“*Do monetary incentives matter for identifying social preferences?*”

Registered Report (Proposal) submitted to the Special Issue of Experimental Economics on Incentivization

co-authored with Aljosha Henkel, Thomas Epper, and Julien Senn.

I declare that:

(1) None of the authors have any relevant, material or financial interests that relate to the research described in this paper.

(2) None of the authors have held any position in organizations that relate to this research.

(3) No party outside of the authors has had the right to review the manuscript prior to submission.

I accept that disclosure statements will be made available upon publication.

Zurich, 10.04.2024

Ernst Fehr

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A Background information on the experimental task

A.1 Choice situations in the money allocation task

A.1.1 Center bundle

Table A.1 provides further details on these choice situations. The meaning of the list of variables displayed in the Table is as follows:

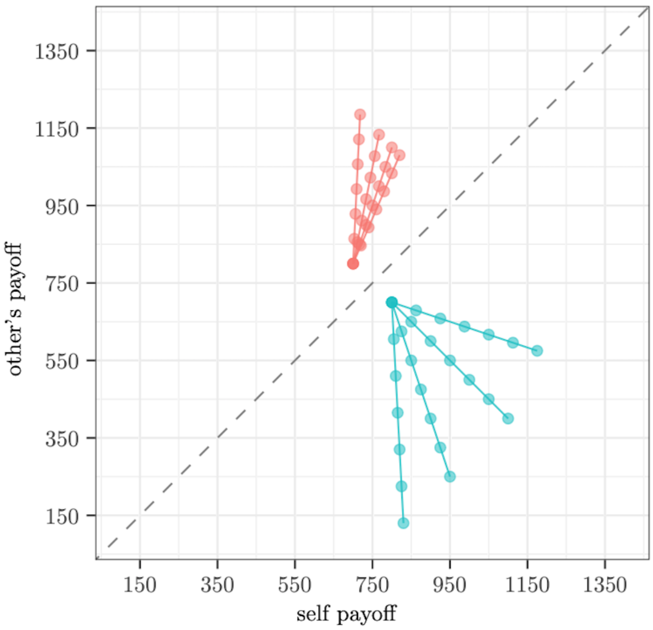
- 'choiceId': the unique identifier for each choice situation.
- $(own1, other1)$: represents the payoff combination at the lower end of the budget line (in points).
- $(own2, other2)$: represents the payoff combination at the upper end of the budget line (in points).
- 'bundle': indicates to which bundle the respective choice situation belongs to.
- 'slope': the slope of the budget line in the "own payoff – other payoff" space.

Table A.1: Choice situations in the money allocation task

choiceId	own1	own2	other1	other2	slope
1	450	1050	750	750	0.0
2	500	1000	800	700	-0.2
3	550	950	850	650	-0.5
4	600	900	900	600	-1.0
5	650	850	950	550	-2.0
6	700	800	1000	500	-5.0
7	750	750	1050	450	-Inf
8	700	800	500	1000	5.0
9	650	850	550	950	2.0
10	600	900	600	900	1.0
11	550	950	650	850	0.5
12	500	1000	700	800	0.2

A.1.2 Displaced bundles

Figure A.1: Additional budget lines



B Identifying preference types using Dirichlet Process Means

B.1 The method

This appendix provides an overview of the clustering algorithm used to identify the preference types and their distribution in the population. For a more detailed description of the DP-means algorithm and for a discussion of its key differences with other clustering methods such a k-means, see Fehr ⓡ al. (forthcoming, 2023).

Our implementation of the algorithm is based on an iterative refinement. We first span an m-dimensional space, with m denoting the number of budget lines used for the clustering algorithm (in our case, $m = 12$, the twelve budget lines presented in Table 1 in the main paper). Consequently, each individual's choices are represented by a single point in the 12-dimensional space. We then ask how subjects populate this space. Specifically, we are interested in the number of clusters (i.e. types) that emerge and individuals' assignment to clusters. A cluster is characterized by the set of the individuals assigned to the cluster and the associated mean vector of observations (the "centroid"), which – in our case – represents the mean (cluster- representative) behavior of all individuals in m-dimensional space that belong to the cluster.

We initialize the algorithm with a single centroid specified as the global mean vector. At this stage, all data points are assigned to this single centroid. We then refine by iterating over the following two steps: First, we sequentially go through the list of data points in m-dimensional space (i.e. subjects), and check for each subject whether any of the squared Euclidean distances to the centroid exceeds the cluster penalty parameter λ. If this is the case, we open up a new cluster with the actual data point's location vector as the centroid. Otherwise, we assign the data point to its nearest cluster. Second, we collect the subjects assigned to the same clusters and update the centroids by computing the mean vector for each cluster. These two steps are repeated until convergence is reached, i.e. until there is no change in subjects' assignments.

As Kulis and Jordan (2012) demonstrate, this iterative procedure is equivalent to minimizing the objective

$$\min_{\{g_c\}_{c=1}^{k}} \sum_{c=1}^{k} \sum_{x \in g_c} \|\boldsymbol{x} - \boldsymbol{\mu}_c\|^2 + \lambda k ,$$

where $\boldsymbol{x}$ denotes the vector of observations, $\boldsymbol{\mu}$ the vector of centroids, and g the cluster partitioning of $\boldsymbol{x}$. It is straightforward to see that this objective is equivalent to the k-means objective except for the additional penalty term λk.

An important aspect of the DP-means approach is that it enables the identification of preference types without committing to a pre-specified number of different preference types. Moreover, this approach does neither require an ex-ante specification or parameterization of types, nor does it presume a specific error structure. This means that it remains ex-ante agnostic about key distributional assumptions, and it does not constrain heterogeneity to lie within a predetermined set of models or parameter space.[16] The DP-means algorithm allows for all possible type partitions of the data spanning from a representative agent (i.e. a single data-generating process) up to as many types as there are individuals in the population (i.e. n data-generating processes), i.e., it determines the number of preferences types endogenously. Thus, (i) the actual number of types, (ii) the assignment of each individual to one of the types and (iii) the behavioral (preference) properties of the types emerge endogenously.[17]

[16] In this regard, our approach differs from previous work (e.g. Bellemare et al., 2008; Fisman et al., 2015, 2017; Bruhin et al., 2018) that characterized preference heterogeneity on the basis of structural assumptions on preferences and error terms.

[17] The fact that the number of types adapts to the data has important benefits (see Kulis and Jordan, 2012). Most notably, as previous work has shown (see Comiter et al., 2016), this feature of the algorithm yields higher quality type-separation than methods that specify the number of types prior to clustering (such as k-means).

C Instructions

In the following, we reproduce the instructions of the money allocation task for the *Low-Incentives* and the *Hypothetical* treatments. In addition, the study will also include some questions aimed at measuring participants' key socio-demographics. Note that all the instructions will be displayed directly on participants' computer screens.

[Social preference task – Low Incentives]

[Instructions]

We now proceed with a task in which you have to take decisions on how to allocate points between yourself and another participant of the study.

In what follows, we describe the instructions for this task. Please read them carefully.

What will you have to do in the following task?

You will be asked to take decisions in different choice situations. In each of these choice situations, you will have to decide how to allocate points between yourself and another participant.

Who is the other participant?

The other participant will take part in another part of the study. Anonymity between yourself and the other participant is guaranteed, i.e. that neither you nor the other person will ever learn about each other's identity.

Moreover, the other participant will not take decisions that affect you, i.e. you will not be affected in any way by the decisions of the other participant.

What will be the consequences of your decisions?

The points gathered during this study will be **converted into US dollars** at the following exchange rate

500 points = $ 1

At the end of this study, the computer will randomly select one of the choice situations **and pay you according to your decision in that choice situation.** This decision-dependent payment will be added to your fixed payment of $3. The other participant will also be paid according to **your** decision in that choice situation.

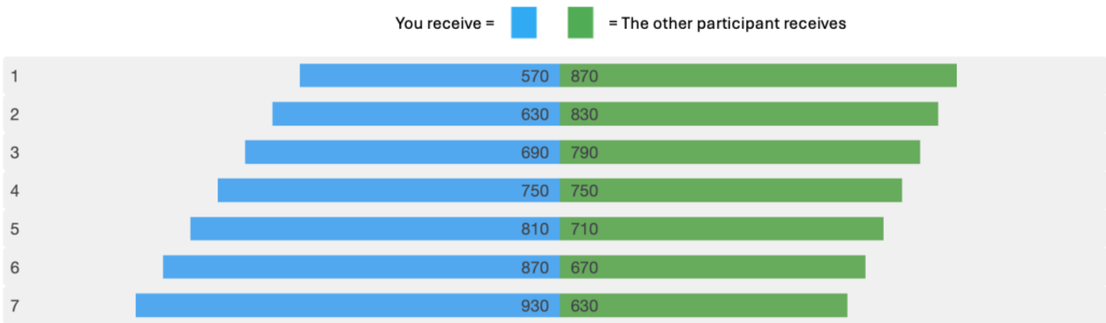
Since every choice situation has an equal chance of being drawn for payment, it is important that you think carefully about each decision.

What kind of decisions will you have to take?

In each choice situation you will be asked to allocate points between yourself and another participant of the study. You will always have the choice between seven different alternatives, numbered from 1 to 7. Each alternative consists of a distribution of points between you and the other participant.

Example

The figure below illustrates a typical choice situation as it will appear on your screen.



In this example,

- choosing alternative 1 yields you 570 points and the other participant 870 points.
- choosing alternative 7 yields you 930 points and the other participant 630 points.
- the total amount of points to be distributed varies from one alternative to another.

How do you make a choice?

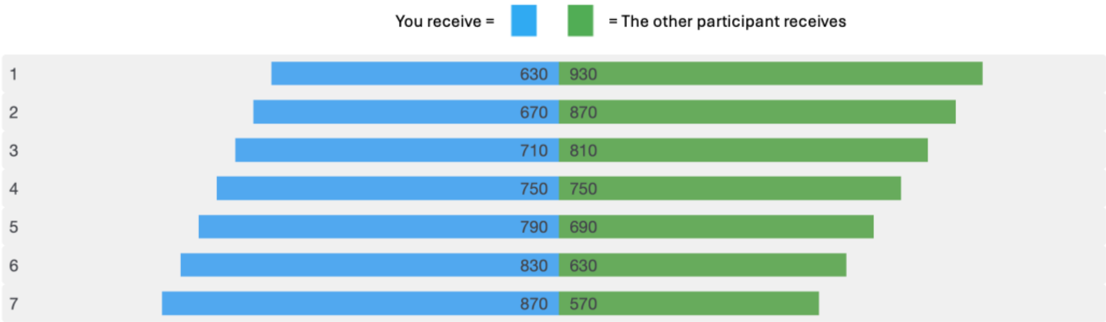
You make your choice by clicking on your preferred alternative. You can change your choice as many times as you want. Once you press the 'Next' button at the bottom right of the screen, your choice is validated and can no longer be reverted. Directly after you press "Next", the next choice situation will appear on the screen. This will be repeated until all the choices have been made.

[Control questions]

Before you start with the task, we would like to make sure that you understand what is asked from you in this task, and what the consequences from your choices are.

To show us that you understand the task, please answer the comprehension questions below. Participants who do not correctly respond to these questions will not be allowed to proceed with the study.

Consider the following example.



1. How many points do you get if you chose alternative 5? [790]

2. How many points does the other participant obtain if you chose alternative 6? [630]

3. What is the total number of points that you and the other participant receive together if you chose alternative 3? Is it 710, 810, or 1520 points? [1520]

4. Do your choices have real monetary consequences for you and the other participant? [yes, no]

[Success control questions]

You have successfully answered all the control questions. You will now start with the decision task. As of now, your decisions matter for your payment, and for the payment of another participant.

Please think carefully before taking a decision in each choice situation.

[Social preference task: decision screens]

Please choose your preferred alternative.

[DISPLAY CHOICE SITUATIONS]

[Social preference task – Hypothetical]

[Instructions]

We now proceed with a task in which you have to take decisions on how to allocate points between yourself and another participant.

In what follows, we describe the instructions for this task. Please read them carefully.

What will you have to do in the following task?

You will be asked to take decisions in different **hypothetical** choice situations. In each of these choice situations, you will have to decide how to allocate points between yourself and another hypothetical participant.

Who is the other participant?

Imagine that you are paired with a hypothetical participant that participates in another part of the study, and that anonymity between yourself and the other participant is guaranteed, i.e. that neither you nor the other person will ever learn about each other's identity.

Moreover, imagine that the other (hypothetical) participant will not take decisions that affect you, i.e. that you will not be affected in any way by the decisions of the other participant.

What will be the consequences of your decisions?

Your choices will have **no real monetary consequences** for you nor the other participant, but please imagine that you are allocating points that have monetary value between yourself and the other participant.

Imagine, in particular, that the points gathered during this study are converted into US dollars at the following exchange rate:

500 points = $ 1.

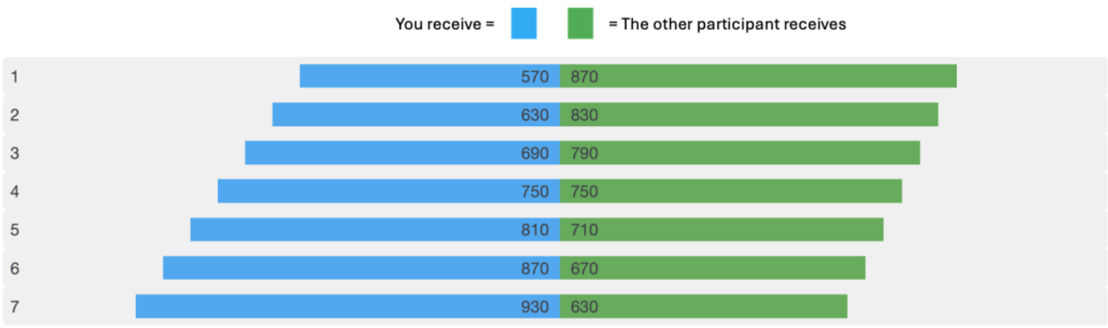
Thus, although your choices will have no real monetary consequences for you nor the other participant, please make your choices as if you and the other participant were paid accordingly.

What kind of decisions will you have to take?

In each choice situation you will be asked to allocate points between yourself and another participant of the study. You will always have the choice between seven different alternatives, numbered from 1 to 7. Each alternative consists of a distribution of points between you and the other participant.

Example

The figure below illustrates a typical choice situation as it will appear on your screen.



In this example,

- choosing alternative 1 yields you 570 points and the other participant 870 points.
- choosing alternative 7 yields you 930 points and the other participant 630 points.
- the total amount of points to be distributed varies from one alternative to another.

How do you make a choice?

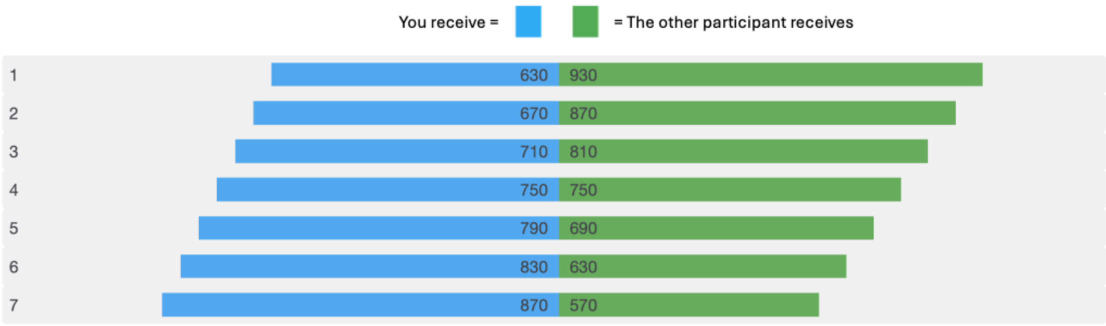
You make your choice by clicking on your preferred alternative. You can change your choice as many times as you want. Once you press the ‘Next’ button at the bottom right of the screen, your choice is validated and can no longer be reverted. Directly after you press “Next”, the next choice situation will appear on the screen. This will be repeated until all the choices have been made.

[Control questions]

Before you start with the task, we would like to make sure that you understand what is asked from you in this task, and what the consequences from your choices are.

To show us that you understand the task, please answer the comprehension questions below. Participants who do not correctly respond to these questions will not be allowed to proceed with the study.

Consider the following example.



1. How many points do you get if you chose alternative 5? [790]

2. How many points does the other participant obtain if you chose alternative 6? [630]

3. What is the <u>total</u> number of points that you and the other participant receive together if you chose alternative 3? Is it 710, 810, or 1520 points? [1520]

4. Do your choices have real monetary consequences for you and the other participant? [yes, no]

[Success control questions]

You have successfully answered all the control questions. You will now start with the decision task.

Please think carefully before taking a decision in each choice situation.

[Social preference task: decision screens]

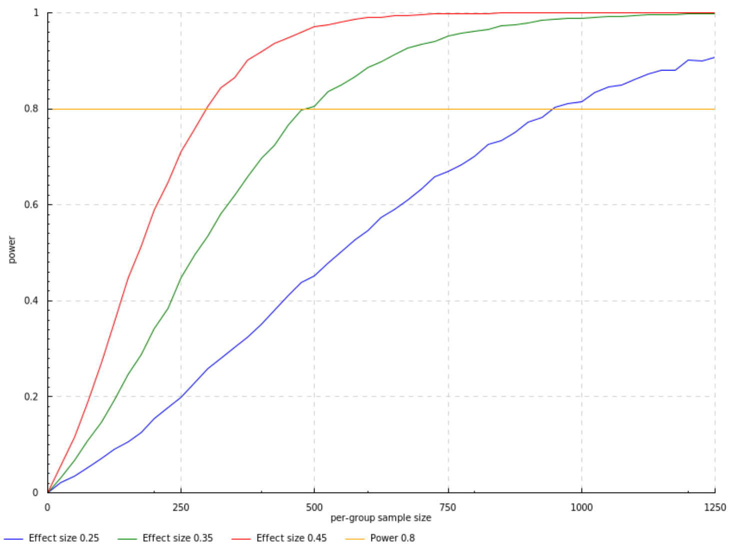
Please choose your preferred alternative.

[DISPLAY CHOICE SITUATIONS]

D Power analysis

To assess sample size requirements for our main analysis with the general population sample, we consider the statistical power of our main hypotheses tests. Specifically, we look at a two-sided Welch t-test on the difference between inequality aversion parameters α or β between two treatments (e.g., Hypothetical and Low-Incentives). We set the probability of a Type I error to 1%, and the standard deviations of the parameters in the two samples to the values we obtained from a previous (incentivized) study conducted in Switzerland using a similar experimental design. The sample size depicted on the x-axis refers to the per-group sample size (e.g. the hypothetical treatment). We computed the power of the test under the assumption that the number of participants is identical in both treatments. Thus, a specific number X on the x-axis means that both the hypothetical and the incentivized (e.g. Low-Incentives) treatments contain X participants. The blue curve shows, for different per-group sample sizes, the power we have to detect a difference of 0.25 (using the above assumptions) between the parameters of the two treatments. With a sample size of 1000 participants per group, we obtain a power which is slightly higher than 80 percent to detect a difference in the parameters α or β of 0.25, i.e., an effect size of about 15 percent of a standard deviation of the structural parameters in the Swiss broad population sample. The green curve depicts the power curve for a larger effect size of 0.35, and the red curve depicts the power curve for an effect size of 0.45.

Figure C.1: Power vs. per-group sample size for various effect sizes



Note: The figure depicts power curves for various effect sizes. The horizontal line indicates a power of 80%.