Decision making on behalf of others

Xiaogeng Xu

To everyone who has or will read my papers
for her or his time and interests

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Contents

Introduction

People always make decisions without knowing the outcome for sure. An individual takes some action that leads to a probability distribution of possible outcomes. Decision making is considered as the approach to an action in face of uncertainty. Sometimes the outcome of a decision affects not only the decision maker but other people, to a far greater degree than affecting the decision maker. Societal decision makers, as considered by Lichtenstein et al. (1990), are those who make risky decisions on behalf of others such as decisions about drug approval, energy options, and genetic research. We may also find the decision making with influence on others in medical advice, parental decisions, consulting, mentoring and the like. Common in these contexts is that the decision maker is not the bearer of decision outcomes. The decision makers may not, aside from the psychological perspectives, experience utility from the outcomes nevertheless they are aware of their impacts on others' welfare. This inherent feature makes decision making on behalf of others distinct from private decision making that has been widely examined in decision science.

Existing studies in both psychology and economics have investigated how decision making on behalf of others differs from private decision making. Tunney and Ziegler (2015) proposed a framework in which they presented the factors that may lead to bias in surrogate decision making. The factors include intention, empathy, significance, accountability, and calibration. The framework provides a comprehensive understanding about what distinguish decision making for others from private decision making. First, the intention of a decision maker may alter the perception about the decision so that different decisions are made on behalf of others than for self. For example, Laran (2010) found out that people make more indulgent food choice on behalf of others than for themselves because of the intention of pleasure seeking on behalf of others. Other intentions such as impact seeking and warm glow (Andreoni, 1989; Hungerman and Ottoni-Wilhelm, 2018) can make decision makers perceive their decisions to be connected to the outcomes that matter in others' lives and choose differently from on behalf of themselves (Grant, 2007). Second, there can be an empathy gap such that the decision maker does not sufficiently consider needs or preferences of others (Boven et al., 2000). The empathy gap makes people feel less regretful and blamed for negative outcomes and less affected by visceral emotions (Kray, 2000; Loewenstein et al., 2001). Third, the significance of decision outcomes can lead to different decision making on behalf of others than for oneself. As found by Beisswanger et al. (2003), people show a large self-other discrepancy in making decisions with low-impact consequences but no self-other difference in high-impact decisions. Fourth, when holding accountable to the decision outcomes, decision makers may comply with certain required benchmarks. Existing studies have found out that social responsibility may reduce risky behavior compared to private decision making (Bolton et al., 2015; Charness and Jackson, 2009). Last, a decision maker can calibrate decision making based on the characteristics of a recipient and on social distance to a recipient. The results of Daruvala (2007) showed that when deciding on behalf of others, people combine their own risk preferences with gender-stereotype predictions about others' risk preferences and make a different decision from for themselves. Similary, Füllbrunn and Luhan (2015) found that people conform to their beliefs about others' risk preferences when deciding on behalf of others. Montinari and Rancan (2018) found that risk taking behavior increases with social distance. People take more risk when deciding for strangers than for friends.

Some other theories also suggest that decision making on behalf of others differ from private decision making. Social Values Theory, proposed by Stone and Allgaier (2008), suggests that social values and norms can make decision making on behalf of others different from private decision making. People are found to take more risk when risk taking is socially valued and vice versa when risk taking is considered inappropriate. With Construal-Level Theory, Liberman et al. (2007) argued that deciding on behalf of others leads to a psychological distance between the decision maker and others. The psychological distance creates a higher construal level such that the decision maker thinks abstractly and considers the overall situation instead of focusing on details. Besides, decision making on behalf of others can be different due to some cognitive biases in decision on behalf of others. The documented cognitive biases in decision making on behalf of others include omission bias (Zikmund-Fisher et al., 2006), confirmation bias (Jonas et al., 2005), and information distortion (Polman, 2010).

The question of how to explain the decision behavior with others' welfare at stake has drawn a growing attention in behavioral economic studies. A meta-analysis by Batteux et al. (2019) shows that in recent economic literature, there is no overall self-other difference in decisions involving risk. They further observed a frame dependent self-other difference: more risk averse for self in gain frame and more risk seeking for self in loss frame. The results of this meta-analysis show that risk preferences of decision makers are not sufficient to explain decision making on behalf of others. Factors other than private risk preferences need to be taken into account to understand decision making on behalf of others. The factors can be external decision environment and intrinsic characteristics of decision makers. The external factors include outcome domain, outcome impacts, accountability, incentives of reward and punishment. The intrinsic factors include private risk preferences, social preferences, beliefs about others and social values.

This thesis is devoted to expanding our understanding about decision making on behalf of others by investigating the factors, both external and intrinsic, that have not been investigated in existing studies.

The first paper, titled *"Ambiguity Attitudes in the Loss Domain: Decisions for Self versus Others" (Co-authored with Yilong Xu and Steven Tucker)*, studies whether and how people's ambiguity attitudes differ between decisions on behalf of others and for oneself in loss domain. Ambiguity, also known as *Knightan uncertainty*, arises when decision makers have limited information or incomplete confidence in the probability of occurrence of a stochastic event. As opposed to the familiar risk with objectively known probability distribution, ambiguity means that individuals make decisions under unquantifiable uncertainty. Similar to the four fold pattern of risk attitudes in prospect theory, there is evidence for a four fold pattern of ambiguity attitudes as summarized by Trautmann and van de Kuilen (2015). People are ambiguity averse for high-likelihood and ambiguity seeking for low-likelihood gain events, and the opposite for loss events. The ambiguity attitudes in individual decision making has been well documented but how people handle ambiguity in decisions on behalf of others is, up to date, lack of knowledge. In this study, we followed the design of Ellsberg game by Kocher et al. (2018) in a lab experiment to investigate decision making on behalf of others under ambiguity in loss domain. The study of König-Kersting and Trautmann (2016) has provided ambiguity attitudes in gain domain. We answered the question whether decision making on behalf of others also follows a fourfold pattern and whether ambiguity attitudes differ between decision for oneself and on behalf of others.

There are two main findings. First, in decision making on behalf of others, we find the ambiguity attitudes in line with the loss part of the four fold pattern in decision making for oneself. Second, ambiguity attitudes do not differ between deciding for oneself and deciding on behalf of others in loss domain. The four fold pattern of ambiguity attitudes pertains to the decisions on behalf of others. Together with previous studies, our study shows that ambiguity attitudes are not affected by agency situations in both gain and loss domains.

To summarize, this study contributes to the literature by filling in the missing part of ambiguity attitudes in decision making on behalf of others. We provide the first evidence of other regarding decision behavior under ambiguity in loss domain. Our results suggest that individual ambiguity attitudes may help explain decision behavior in agency or delegation when an objective probability distribution is unavailable.

The second paper, titled *"Giving When Responsible on behalf of others' Risk"*, studies the societal decision making in provision of social assistance. At the heart of social welfare system, social assistance allows citizens to take on opportunities of returns but also entail some risk borne by the recipients of social assistance. From the perspective of the providers of social assistance, the choice among different schemes of social assistance may lead to different risk profiles for the recipients. Such choices can be what treatment or therapy to be covered in medical care for poor patients, how to allocate limited resource between job training and life compensation for the unemployed, how to trade off quantity and quality of teachers for disadvantaged children. Due to incomplete information and uncontrollable factors, it is often difficult for the providers to foresee the outcomes of social assistance when making decisions. In some societies, everyone is entitled social assistance and also voice in policies of social assistance. In other societies, social assistance is organized, provided and decided by volunteers. This study investigates how the welfare institution influences decision making in provision of social assistance remains, which is still an open question in public behavioral economics.

In an online experiment, I assigned subjects to two treatments that represent stylized welfare institutions. In one treatment, *Non-voluntary* treatment, decision makers were asked to make risky decisions on behalf of recipients. In the other treatment, *Voluntary* treatment, decision makers first made a costly volunteering decision. If volunteering, the decision makers would make risk decisions on behalf of recipients. If not volunteering, the decision makers were later surprisingly asked to make risky decisions on behalf of different recipients.

The results of this study shows that there is difference of risk taking in social assistance between different institutions. Volunteer take more risk in decisions of social assistance with their own contributions than in an institution where decisions are made with provided resources. The overall difference is driven by both the institution and the intrinsic characteristic. On one hand, volunteers take more risk on behalf of others in the presence of the opportunity of volunteering than when they are asked to make decisions. On the other hand, the decision makers who are willing to volunteer take less risk on behalf of others than those unwilling to volunteer. The econometric analyses show that the intrinsic selection effect is reduced due to the fraction of non-volunteers and the institutional effect dominates so that the overall difference shows more risk taking in a voluntary institution. The study also shows that more risk seeking, female, and more altruistic decision makers are more likely to volunteer. The decision makers who are willing to volunteer are more risk averse in decisions on behalf of others than for themselves, and the opposite for the decision makers unwilling to volunteer.

To summarize, this study offers new insights to existing literature and provides implications of welfare institutions for social assistance. An institution with the opportunity of volunteering present leads to different risk taking behavior of volunteers in social assistance compared to an institution without such an opportunity. A institutional change makes people perceive the environment in a different way and leads to different decision behavior, similar as the findings of Bó et al. (2010) and Gneezy and Rustichini (2000). The less risk taking of volunteers than non-volunteers indicates that social preferences and responsibility may affect risk taking on behalf of others (Andersson et al., 2019; Pahlke et al., 2015). Furthermore, the other-self difference of risk taking is found to turn from negative for volunteers to positive for non-volunteers. The link between social preferences and the other-self difference in risk taking is statistically different, which is not found by Bolton et al. (2015). The results of this study imply that the inequality of ultimate benefits may be larger when social assistance relied more on voluntary contributions. This is not only due to incomplete coverage but the institutional impacts on decision behavior. Apart from the resources devoted to social assistance, the welfare institution can also influence the final outcomes of social assistance acts.

The last paper, titled *"Risk taking on behalf of others: Does the timing of uncertainty revelation matter?" (Co-authored with Alexander W. Cappelen, Erik Ø. Sørensen and Bertil Tungodden)*, we present a novel study of the effect of the timing of uncertainty revelation on risk taking on behalf of others. In particular, we study risk taking behavior in situations where the decision maker never learns about how uncertainty is resolved, a class of situations that can only happen in risk taking on behalf of others. A growing literature, both theoretically and empirically, have studied the interaction between time preferences and risk preferences, as reviewed by Epper and Fehr-Duda (2018). An interesting stylized fact that has drawn attention in economic studies is that risk tolerance increases with the delay before the revelation of decision outcomes. Some studies have documented such an effect of revelation delay on risk taking behavior on own behalf (Shelley, 1994; Noussair and Wu, 2006; Abdellaoui et al., 2011; van Winden et al., 2011; Onay et al., 2013). Whereas, it remains an open question whether the effect of revelation delay on risk taking that has been found in existing literature can generalize to risk taking on behalf of others. Moreover, the special situation where the decision maker never learn about decision outcomes makes it more interesting to examine how delay of uncertainty revelation, especially infinite delay, influences risk taking on behalf of others.

In this study, we examine whether how the timing of uncertainty revelation influences risk taking on behalf of others. In an online experiment with a large representative sample of Norway, we expose participants to four treatments with different delays of uncertainty revelation: now, short, long and never. In each treatment, participants chose how to determine the payoff for passive recipients, either a fixed payoff or a role of a fair die. In the *now* treatment, the participants were told that they would know their decision outcomes at the end of the study. In the *never* treatment, the participants were told that they would never know their decision outcomes. In the *short* and *long* treatment, the participants were told that they would know their decision outcomes one week and three months after the study, respectively.

To analyze the data of our experiment, we first look at reduced form results of the proportion that chose a lottery over a safe alternative. In order to look into the mechanism, we estimated a hierarchical Bayes model of rank dependent utility. Our main finding is that, in contrast to the documented effect of delay on risk taking, we find a precisely estimated null effect of revelation delay on risk taking on behalf of others (the average proportion that chose a lottery over a safe alternative). Estimating a hierarchical

Bayes model, we find some differences in how decisions are made, the median participant does become more risk seeking with long delays, but this effect is offset by differences in the role of heterogeneities within treatment. We also find that the socio-demographic variables we collected have little impact on risk taking. Interestingly, risk taking on behalf of others is more strongly related to own risk preferences than beliefs about the risk preferences of others and is also related to positive emotional states when making decisions.

To summarize, this study offers innovative findings about how delay of uncertainty revelation influences risk taking on behalf of others, a first step towards understanding risk taking behavior under delay of revelation of outcomes. Given the importance of risk taking on behalf of others, our study expands the understanding of other-regarding decision making and initiates the research questions of how decisions with others' welfare at stake are made under delayed revelation of outcomes. Our results provide implications in many real life decisions that are made on behalf of others, especially of those the outcomes will ever remain unknown for the decision makers.

Overall, the results presented in the three papers extend our understanding of other-regarding decision behavior. The first paper shows that decision making does not differ between deciding for oneself and others in face of ambiguous losses. The second paper demonstrates how institutions affect decision making on behalf of others under risk and suggest that social preferences can explain risk taking on behalf of others besides individual risk attitudes, gender and age. The third paper manifests the effect of the timing of outcome revelation on risk taking on behalf of others. The three papers broaden our understanding about other-regarding decision making, contribute to the growing literature with insightful findings, and provide implications for policy making that involve risk.

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I Ambiguity Attitudes in the Loss Domain: Decisions for Self versus Others

Ambiguity Attitudes in the Loss Domain: Decisions for Self versus Others*

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Abstract

We study whether people's ambiguity attitudes differ when deciding for themselves or for others in the loss domain. We find no systematic differences in ambiguity attitudes between self- and other-regarding decision-making. Our results are consistent with the loss part of the fourfold pattern of ambiguity attitudes.

Keywords: Ambiguity attitudes; Decision-making for others; Losses and uncertainty;
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1 Introduction

Ellsberg's paradox (Ellsberg, 1961) has inspired decades of research on attitudes towards ambiguity (situations where objective probabilities of potential outcomes are unknown), because of their potentials to explain important behavioral anomalies in economics and finance (e.g. Trautmann and van de Kuilen, 2015), and therefore inform policy choices (Farber, 2010; Weisbach, 2015). Although the vast majority of empirical studies in the literature concern decision-making for oneself under ambiguity, decisions in the field are often made on behalf of others and possibly in the loss domain.[1]

To date, the only study probing self- and other-regarding ambiguity attitudes is König-Kersting and Trautmann (2016), who find no difference between decision-making for oneself and for others in the realm of gains. This result was rather surprising given that, in the domain of known risks, a substantial number of studies show that people make different choices when making risky decisions for others, though the results are mixed (Reynolds et al., 2009; Sutter et al., 2009; Eriksen and Kvaløy, 2010; Chakravarty et al., 2011; Pollmann et al., 2014; Füllbrunn and Luhan, 2015). Additionally, when considering risky decisions in the loss domain, previous studies provide clear evidence that people are less loss-averse when making decisions for others (Polman, 2012; Andersson et al., 2014; Vieider et al., 2015; Füllbrunn and Luhan, 2017). Given these results, we conjecture that self-other differences might be more pronounced in decisions involving losses as opposed to gains.

Observing that the study by König-Kersting and Trautmann (2016) considers only prospects in the gain domain, we extend their study and compare people's ambiguity attitudes with pure loss prospects when making decision for oneself/others, and we are the first to probe this important open issue. In addition, we test if the loss part of the fourfold pattern also persists when making decisions for others (Viscusi and Chesson, 1999; Mauro and Maffioletti, 2004; Baillon and Bleichrodt, 2015; Trautmann and van de Kuilen, 2015; Bouchouicha et al., 2017; Kocher et al., 2018).[2]

We find that other-regarding ambiguity attitudes do not differ systematically from self-regarding ones. Attitudes toward ambiguity follow the loss part of the fourfold pattern in both self- and other-regarding decisions. The next section describes the experimental design and procedures. The results are presented in the third section. We conclude and discuss the potential implications of our results in the last section.

[1]An extreme example of such situation is when powers of attorney are drawn. In this case, agents typically make all financial/health decisions on behalf of their (possibly mentally incapable) principal without any her active involvements. Many decisions on family and financial matters share similar features, e.g., parental decisions for young children, fund managers choosing funds for clients during financial crisis.

[2]Previous research has suggested a four-fold pattern of ambiguity attitudes: people are ambiguity averse for low likelihood losses and ambiguity seeking for moderate likelihood losses, with the opposite pattern for gains.

2 Experiment Design and Procedures

The experiment consists of two stages. In the first stage, subjects participate in a real effort task of adding up five two-digit numbers (Niederle and Vesterlund, 2007). When five questions are solved correctly (with unlimited attempts), every subject earns a fixed payment of 20 New Zealand Dollars (NZD), which serves as an endowment for the potential losses in the second stage of the experiment. Subjects are not informed about the details of the second stage before finishing the first one. In the second stage, we extend the Ellsberg-urn task implemented by Kocher et al. (2018) to measure subjects' ambiguity attitudes in the loss domain when making decisions for themselves (SELF) or for others (OTHER). We employ both 2-color urns to capture moderate likelihood losses and the 10-color urns for low likelihood losses (see details below and Kocher et al. (2018)),[3] constituting a 2×2 between-subject design: SELF/Moderate, SELF/Low, OTHER/Moderate, and OTHER/Low.

Specifically, in treatment OTHER, subjects first make decisions for others as agents, and later serve as principals. Our one-way matching ensures that each agent will only serve a principal who is not at the same time making decisions for herself. That is, we rule out the possibility for direct reciprocity and this point has been clearly made in the instructions. Moreover, they get to see their results as principal only after making decisions as agents for their own principals.[4]

The Ellsberg tasks were administered as follows. In both 2-color and 10-color settings, subjects choose between opaque bags filled with 100 deflated balloons in either two and ten colors, respectively. In one bag, the distribution of the colors is known (risky prospect, see Table 1). In the other bag, the distribution of colors is unknown to the subjects (ambiguous prospect), but the potential colors in the bag are known to them. Subjects will lose 15 NZD if they choose the risky bag with known distribution and the color of the balloon drawn is blue, or if they choose the ambiguous bag and the color of the balloon drawn matches the color of their choice.[5] Otherwise, no money will be deducted from the initial earning of 20 NZD. In each setting, subjects make seven decisions between risky bags with varying number of blue balloons, and an ambiguous bag with unknown composition of colors. Following König-Kersting and Trautmann (2016), these decisions are shown sequentially in a fixed order, as demonstrated in Table 1.[6]

[3] See Online Appendix B for a picture of the bags used in the experiment.

[4] When severing as principals, every subject sees all choices made by her agent in treatment OTHER. A summary screen of lottery outcomes and earnings is also shown on their screen at the end.

[5] For the ambiguous prospect, allowing subjects choose their personal colors (out of 2 or 10 colors) prevents the experimenters from strategically filling the bags to the disadvantage of the subjects. This point has been clearly communicated to our subjects. Since the distribution is known to the subjects for the risky prospect, they are free from this problem and therefore we have predetermined the "losing" color as blue for the risky prospect. Subjects were encouraged to check the composition of the bags after the experiment, which was communicated when reading out the summary of the instructions.

[6] The motivation for this design feature was to reduce potential anchoring or range effects sometimes observed in

Table 1: Order of decisions and probabilities of loss in the risky prospects

Decision	Moderate-likelihood setting	Low-likelihood setting
1	0.50	0.10
2	0.35	0.01
3	0.65	0.19
4	0.40	0.04
5	0.60	0.16
6	0.45	0.07
7	0.55	0.13

Notes: In each decision set, subjects made seven decisions in a fixed order with various probabilities of losing 15 NZD. The probabilities are designed to reduce potential elicitation biases (Kocher et al., 2018). Entries read as follows. For Decision 1 of the Moderate-likelihood set, a subject will lose 15 NZD with the probability 50% if she chooses the risky prospect. If the ambiguous prospect is chosen, the chance of losing 15 NZD is unknown (2-color setting).

The first decision is a direct binary choice between an ambiguous prospect and its corresponding ambiguity-neutral risky prospect under expected utility. In the 2-color (10-color) task, this risky bag contains exactly 50 (90) red and 50 (10) blue balloons. Using the first choice of the sequence allows us to classify ambiguity attitudes, as typically done in single-choice designs, free from any potential choice list effects. We can classify subjects as ambiguity averse or ambiguity seeking based on their first decision (with ambiguity neutral subjects potentially included in both categories). For instance, an ambiguity seeking individual in the 10-color setting would prefer the ambiguous bag over the risky bag containing exactly 10 balloons in blue.

For the full set of seven decisions, we can calculate a *probability equivalent (PE henceforth)* for the ambiguous prospect, defined as the probability of a risky prospect such that an individual is indifferent between the risky and the ambiguous prospects. Operationally, we follow Kocher et al. (2018) and take the mid-point between the highest risky probability for which the decision maker chooses the risky prospect and the lowest risky probability for which she chooses the ambiguous one.[7] If someone is willing to accept a large known probability of loss, she is then considered ambiguity averse.[8]

single-screen choice lists.

[7]For instance, for the 2-color moderate likelihood task, suppose a decision maker prefers the risky bag when the chance of drawing a blue balloon is 0.55 but opts for the ambiguous option when the chance of a blue balloon increases to 0.6, then her PE is calculated as $0.55 + \frac{1}{2} * (0.6 - 0.55) = 0.575$. Additionally, we implement the following rule if one never changes her decision. Suppose that a subjects chooses the ambiguous prospect [the risky prospect] all the time in the moderate likelihood task, then her PE is set to 0.325 [0.675]. Similarly, if she chooses the ambiguous prospect [the risky prospect] for all decisions in the low likelihood task, then her PE is set to 0.005 [0.205].

[8]Indeed, for prospects in the loss domain, the larger the PE is, the more ambiguity averse a person is. Specifically, an individual is considered as ambiguity seeking (averse) if the elicited PE is smaller (larger) than 0.5 in the task with moderate likelihood losses, or 0.1 in case of low likelihood losses. The elicited PEs allow us to rank subjects by their degree of ambiguity aversion.

A total number of 236 subjects participated in our experiment with roughly 60 subjects in each of the four treatments. The experiment was conducted at the Waikato Experimental Economics Lab (WEEL) at University of Waikato, New Zealand. Participants were invited to participate via ORSEE (Greiner, 2015) and the experiment was computerized using z-Tree (Fischbacher, 2007). Each session lasted about 70 minutes with an average payment of 15 NZD.

Subjects read self-paced on-screen instructions,[9] followed by a summary by the experimenter. The bags for the Ellsberg tasks were placed on the table in front of the lab and subjects were encouraged to check the distribution of the risky bags after the experiment. We communicated to the subjects that once all seven decisions were made, a volunteer would draw a balloon from each bag to resolve the uncertainty and the drawing results would be summarized on their screen. This way, we aimed to ensure a high level of credibility perceived by the subjects regarding our procedure. The payoffs were calculated for each participant or each principal-agent pair by randomly selecting one choice task taken for real.

3 Results

3.1 Results from the whole sample

We first consider the results using the whole sample. The left panel of Table 2 shows the ambiguity attitudes of all subjects by treatment. We separately report the direction of ambiguity attitudes based on: *(i)* the proportion of ambiguous prospect chosen in the first decision task, and *(ii)* the probability equivalents derived from all seven decisions.[10]

For self-decisions, a minority of 38% chooses the ambiguous prospect in the first choice when facing moderate chance of losses, which is marginally lower than 50%, assuming that ambiguity neutral subjects choose randomly between the risky and the ambiguous prospects (binomial test, p=0.07; N=61). The corresponding average PE equals 0.499, which is indistinguishable from ambiguity neutrality (two-sided t-test, p=0.90; N=58). When the chance of losses is low, 42% subjects choose the ambiguous prospect in Decision 1, which is not significantly different from 50% (binomial test, p=0.30; N=59), but points in the direction of ambiguity aversion. The corresponding average PE is 0.112, indicating ambiguity aversion (two-sided t-test, p=0.01; N=56).

[9]The instructions for the experiment as well as the replication package can be accessed here: https://doi.org/10.11588/data/MHUGKP

[10]Although the seven decisions were made sequentially on separate screens, the consistency between the first choice and the elicited PEs is high for both tasks: 65.0% for the 2-color task and 71.8% for the 10-color task. However, there are 15 subjects for whom the probability equivalent cannot be calculated (SELF/Moderate: 3; SELF/Low: 3; OTHER/Moderate: 3; OTHER/Low: 6, accounting for 6% of the whole sample). This happens if a subject prefers the ambiguous prospect when the probability of losses in the ambiguous prospect is small and switches to the risky one when the probability gets larger.

Table 2: Ambiguity attitudes in loss domain

Treatment	Whole Sample: # obs.[a] First choice	Whole Sample: First choice:[b] ambiguous choices (%)	Whole Sample: All choices:[c] probability equivalent		Subsample: Distinct from Neutrality[d]: # obs.[a] First choice	Subsample: Distinct from Neutrality[d]: First choice: ambiguous choices (%)	Subsample: Distinct from Neutrality[d]: All choices: probability equivalent	
SELF/Moderate	61 (58)	37.71 AA^{*}	.499 (AS)		37 (34)	59.46 (AS)	.481 (AS)	
SELF/Low	59 (56)	42.37 (AA)	.112 AA^{**}	⌉[e]	41 (38)	29.27 AA^{**}	.124 AA^{***}	⌉[e]
OTHER/Moderate	58 (55)	41.38 (AA)	.495 (AS)	**	37 (34)	56.76 (AS)	.479 AS^{*}	*
OTHER/Low	58 (52)	29.31 AA^{***}	.132 AA^{***}	⌋	47 (41)	23.40 AA^{***}	.143 AA^{***}	⌋

Notes: a: The numbers in parentheses are the numbers of observations in each treatment where the probability equivalents can be defined. b: The entries of first choice report percentages of choosing the ambiguous prospect in the first decision in Table 1, two-sided binomial test against 0.5. c: Entries of all choices report means of probability equivalents, two-sided t-test against 0.5 (0.1) for moderate- (low-) likelihood task. d: The subsample consists of subjects who are not ambiguity neutral. e: Test if the PEs under OTHER/Low and SELF/Low are significantly different by Mann-Whitney U test. *, **, *** denote significance at the 10%, 5% and 1% level. No significant difference is found between decisions make for oneself and others with Moderate-likelihood of losses. AA = ambiguity aversion; AS=ambiguity seeking. Results that are insignificantly different from ambiguity neutrality indicated by parentheses.

As for the decisions made for others, in OTHER/Moderate, 41% of our subjects choose the ambiguous prospect in the first choice, indicating ambiguity aversion. Yet, the proportion is not significantly lower than 50% (binomial test, p=0.24; N=58). The corresponding average PE indicates that subjects are on average ambiguity seeking, though not significantly so (two sided t-test, p=0.52; N=55). In OTHER/Low, a minority of 29% of our subjects choose the ambiguous prospect in the first decision, which is significantly lower than 50% (binomial test, $p<0.01$; N=58), pointing in the direction of ambiguity aversion. This is confirmed by the corresponding PE that equals to 0.132, which is significantly larger than ambiguity-neutral probability of 0.1 (two sided t-test, $p<0.01$; N=52). The overall picture suggests that subjects are ambiguity neutral when facing moderate likelihood losses and ambiguity averse when facing low likelihood losses.[11]

We next consider if ambiguity attitudes differ across treatments when comparing decision-making for oneself and for others. For moderate likelihood losses, there is no difference between self/other decision-making. For low likelihood losses, we find some suggestive evidence that subjects are more ambiguity averse when making decisions for others than for oneself based on the PEs. However, no significant difference is found when considering only the first decision.

3.2 Results from the subsample distinct from ambiguity neutrality

Our whole sample contains a substantial proportion of people who potentially exhibit neutral attitudes towards ambiguity and cannot be properly identified by the tasks (see discussions in Kocher et al. (2018), Appendix A3). This proportion ranges from 19% to 39% in our treatments, sum-

[11] Similar to Kocher et al. (2018), the pattern we found based on the whole sample is not fully consistent with the loss part of the fourfold pattern because it predicts ambiguity seeking/neutrality in case of a moderate likelihood of losses.

marized in Table 3. We, therefore, present results based on a subsample that excludes ambiguity neutral subjects in the right panel of Table 2, following Kocher et al. (2018).

Table 3: Ambiguity neutral subjects by treatments

SELF/Moderate	SELF/Low	OTHER/Moderate	OTHER/Low
39.3%	30.5%	36.2%	19.0%

Notes: This table summarizes the percentage of subjects whose probability equivalents lie in the interval [0.475, 0.525] in the moderate-likelihood task, and in the interval [0.085, 0.115] in the low-likelihood task.

In this subsample, whether making decisions for oneself or for others, subjects show insignificant ambiguity seeking for moderate likelihood losses and strong ambiguity aversion for low likelihood losses. Specifically, in SELF/Moderate, 59% of our subjects choose the ambiguous prospect in the first decision (binomial test, p=0.32; N=37). In SELF/Low, this measure is 29% (binomial test, p=0.01; N=41). The corresponding average PE of decisions for oneself is 0.481 for moderate likelihood losses (two sides t-test, p=0.27; N=34) and 0.124 for low likelihood losses (two sided t-test, p<0.01; N=38). When making decisions for others, 57% choose the ambiguous prospect in the first decision in OTHER/Moderate (binomial test, p=0.51; N=37). This number is 23% in OTHER/Low (binomial test, p<0.01; N=47). The average PE of decisions for others is 0.479 for moderate likelihood losses (two sides t-test, p=0.08; N=34) and 0.143 for low likelihood losses (two sided t-test, p<0.01; N=41). This suggests that the behavior of subjects who are not ambiguity neutral is strongly in line with the predicted loss part of the fourfold pattern of ambiguity attitudes, replicating results of Kocher et al. (2018). This holds true both when making decisions for oneself and for others. Again, the only self/other difference is found when making decisions for low likelihood losses, weakly significant at p=0.052. That is, even for this subsample of subjects who exhibit the most pronounced ambiguity attitudes, there is no systematic difference between self- and other-regarding ambiguity attitudes.

4 Discussion and Conclusion

This paper investigates whether and how people's ambiguity attitudes differ when making decisions involving losses for others. We follow the design of Kocher et al. (2018) and replicate the result that when making decision for oneself, people are ambiguity neutral for moderate likelihood losses and ambiguity averse for low likelihood losses. These findings are in line with the loss part of the four-fold pattern commonly observed in the literature (Trautmann and van de Kuilen, 2015). Second, our results suggest that ambiguity attitudes do not differ when comparing decisions for oneself and for others in the loss domain. This is rather surprising given that studies of loss aversion (Polman, 2012; Andersson et al., 2014; Füllbrunn and Luhan, 2017) suggest that people do behave differently when making decisions for others with known risk of losses. Together with the results of König-Kersting and Trautmann (2016), it seems that ambiguity attitudes are not significantly affected by the agency situation in both gain and loss domains. This overall picture is reassuring as agents typically behave as if they were making decisions for themselves when acting on behalf of their principals, at least for the case where there is no asymmetric information and incentives problems that distort decisions as the case of credence goods (Dulleck and Kerschbamer, 2006).

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Appendices

Online Appendix A

This appendix presents the on-screen instructions of our experiment. A summary is provided by the experimenter after subjects finish these instructions at their own pace. The instructions provided here is based on treatment OTHER. The instructions for treatment SELF is identical, except that we administered game N (2-color setting) and game P (10-color setting) separately in different sessions. In treatment OTHER, subjects are randomly assigned to two groups. One group play game N (2-color) on behalf of their principals who are at the same time playing game P (10-color) for them as agents, following König-Kersting and Trautmann (2016).

A1. General instructions

Welcome to this experiment. At the end of the experiment your entire earnings from the experiment will be immediately paid to you in cash. It is prohibited to communicate with the other participants during the experiment. Should you have any questions please ask us. If you violate this rule, we shall have to exclude you from the experiment and from all payments. Today, you will play two games, game N and game P. In each game, your possible payoff is denoted in dollars.

Part 1: The real-effort task

In the first part of the experiment, you are asked to add up some numbers. You will earn $20 if you manage to solve 5 math problems correctly.

Part 2: Ambiguity measurements

This part of the experiment consists of two games, N and P. In each game there are two types of players, a decision-maker and a recipient. Each recipient is assigned to one decision-maker. The assignments of the roles as well as the pairings occur randomly by the computer.

In one of the games, you take the role of the decision-maker and in the other game you take the role of recipient.

Please note that your recipient will NOT play the role as your paired decision-maker. Rather, you will be paired with another participant who will make decisions on your behalf.

In each of the two games, the decision-maker completes 7 choice tasks. Only one of the choice tasks will be chosen randomly at the end of the experiment to determine the final earnings. Since you do not know which choice task is paid, you should behave in each choice task as if this were the one that determines your **recipient's** final payoffs.

For each choice task, the decision-maker is presented two opaque bags containing different distributions of colored balloons. The decision-maker chooses a bag from which a balloon will be drawn.

The decision-maker's choice of bag, together with the color of the drawn balloon, determines the final payoff of the recipient.

After the decision-makers complete their seven choice tasks, the unanswered choice tasks are presented to their paired recipient. After the recipient has viewed all the tasks, the computer randomly selects the choice task to be used to determine the recipient's final payoff.

The experimenters will then randomly select one of the experiment participants to come forward and draw a balloon from each of the bags.

Lastly, the decision-maker's decision is made known to the recipient and final payoffs calculated.

At the end of the experiment, all bags used in the experiment can be checked by the participants if they wish.

The next screen explains the game in more details. In treatment OTHER, subjects are randomly assigned to two groups. In one group, they see game N and in the other group, they see game P.

You are playing game N

You are the decision-maker in this game

In this game, each bag contains exactly 100 balloons. There are 2 types of balloons that may be in each of the bags: red and blue. In this game you decide between a bag labeled N-A and a bag labeled N-B. The bags N-B are numbered from N-B1 to N-B7 as there are 7 decisions in total and the distributions of coloured balloons in these bags are displayed on the following screen. The distribution of coloured balloons in bag N-A is unknown to you.

You are playing game P

You are the decision-maker in this game

In this game, each of the two bags contain exactly 100 balloons. There are 10 types of balloons that may be in each of the bags: blue, red, yellow, lime, dark green, pink, purple, orange, black and white. Your choice tasks are to decide between a bag labeled P-A and a bag labeled P-B. The bags P-B are numbered from P-B1 to P-B7, as there are 7 decisions in total and the distributions of coloured balloons in these bags are displayed on the following screen. The distribution of coloured balloons in bag P-A is unknown to you.

Choose the personal color

Before you make your selection of bags in the choice tasks, you must first select a color of balloon that will determine the outcome of the game. That is, if this color is drawn from the N-A bag [P-A bag, in case of low likelihood treatment] that you select, then the recipient loses [you lose, in case of SELF treatment] $15. However, if any other color is drawn, then the recipient loses [you lose] $0. Any losses will be deducted from the recipient's [your] earnings in Part 1 of the experiment. Please now select a color from the options. For all the other bags with known distributions of balloons, if a blue balloon is drawn, then the recipient loses [you lose] $15 if you chose the known distribution bag instead of the N-A bag in the choice task that is randomly selected for payment. If any other color is drawn, then the recipient loses [you lose] $0. Any losses will be deducted from your recipient's [your] earnings in Part 1 of the experiment. Please now select a color from the options.

The choice screen appears and the real experiment starts. After all decisions were made In treatment OTHER, subjects would get to see the choice problems faced by their agents when they later serviced as principals themselves. Afterwards, the experimenter asked a volunteer to resolve the uncertainty of the lotteries.

Online Appendix B: Picture of the bags in the experiment

II Giving When Responsible For Others' Risk

Giving When Responsible For Others' Risk*

Xiaogeng Xu†

Abstract

Social assistance is often thought of as an insurance scheme, allowing citizens to take more risk than they would in autarky. Provision of social assistance also involves uncertainty since providers have incomplete information and cannot fully predict social outcomes. Societies organize provision of social assistance in different ways, and we know little about how this influences the willingness of providers to take risk. In a stylized experiment, I investigate how two different institutions affect risk taking in provision. In one institution, everyone is entitled to social assistance and also to voice their opinions on how much risk to take. In the other institution, assistance is voluntarily given and not guaranteed. In the voluntary institution, providers of social assistance take 22% of a standard deviation more risk on behalf of others and only 48% receive assistance. The experimental design allows me to decompose this difference into a selection effect and an effect of the institution itself. The voluntary institution leads to greater risk taking, but this effect is counteracted by cautious behavior of volunteers.

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Keywords: welfare institution; uncertainy; risk preferences; other-regarding behavior

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1 Introduction

A system that provides social assistance can be seen as an insurance scheme, and many have argued that such an insurance scheme is at the heart of a welfare state (Sinn, 1995). The insurance scheme enables individuals to take on opportunities that bring high returns but might entail some risk. From the point of view of the providers of social assistance, the choice among different sorts of assistance is also a decision under uncertainty. Providers face uncertainties that arise from incomplete information and uncontrollable factors. These uncertainties make the ultimate benefits difficult to predict. Societies differ in who are responsible for making decisions about provision of services, and this might cause differences in the risk profiles for beneficiaries of social assistance. In some societies, everyone is entitled to social assistance and also to voice their opinions about policies regarding social assistance, as exemplified by the Nordic welfare states. In other societies, social assistance is organized, provided and decided by volunteers, as exemplified by some village economies studied by Townsend (1994). Most societies lie somewhere in between these extremes, but volunteerism is still important in developed economies (Romero, 1986). Whether and how these differences in institutions influence decision making in the provision of social assistance remains an open question in behavioral public economics.

In order to study the motivation at work in welfare institutions and welfare decisions, I designed an experiment with stylized welfare institutions in different treatments. Around 2000 subjects participated in an online experiment. First, participants were randomized into groups that were paid or not paid for taking part in a survey. Second, the lucky participants could provide support to the unlucky participants in two different treatments that are designed to represent different institutions for assistance. One group of the decision makers made risky decisions on behalf of recipients with money that the recipients were already entitled to. The other group of decision makers chose whether or not to voluntarily provide support to the recipients, and made risky decisions on behalf of the recipients with money that the decision makers contributed themselves.

The decision making can differ between welfare institutions by two main mechanisms. One mechanism is that the same people might behave differently in different institutions, and I refer to this as an *institutional effect*. Previous research has documented effects of institutions on behavior. In particular, people behave differently when regulations are exogenously imposed than when the same regulations are freely chosen (Bó et al., 2010). Similar impacts of endogenous rule choice on behavior are also found to result from self-crafted irrigation rules (Bardhan, 2000), voting on tax policy (Frey, 1998), and self-chosen reward and punishment in public good games (Sutter et al., 2010). These findings indicate that the decision making on social assistance may vary with different welfare institutions. The decision making on social assistance can be different when the social assistance depends on voluntary contribution and decisions are made by contributors, compared to when everyone is entitled to the social assistance and to voice their opinions on decision making.

The other mechanism for the different decision making between institutions is that those who volunteer are systematically different and they might make different decisions from others who do not volunteer regardless of the external institutions, and I refer to this as a *selection effect*. The attitude toward volunteering is heterogeneous and might result from different social preferences. The relation between social preferences and decision making on behalf of others has been studied by Andersson et al. (2019) and they showed that people who give more in a dictator game take less risk on behalf of others. Montinari and Rancan (2018) found that risk taking on behalf of others decreases with social distance. These findings suggest that heterogeneous social preferences can lead to different decision making on behalf of others.

To further understand the selection effect, one can ask whether risk taking on behalf of others is the same as risk taking on behalf of oneself. An extensive literature has examined this question, but the results are mixed. Some studies found that people take more risk on behalf of others than themselves (Andersson et al., 2016a; Chakravarty et al., 2011; Sutter, 2009), while others found that people take less risk on behalf of others than themselves (Bolton and Ockenfels, 2010; Bolton et al., 2015; Charness and Jackson, 2009; Eriksen and Kvaløy, 2010; Reynolds et al., 2009), and some found no difference between decisions on behalf of others and on behalf of oneself (Ertac and Gurdal, 2012). Comparing the risk taking among people with different strengths of social concern might show whether social preferences can explain the difference between decisions on behalf of others and oneself. The role of social preferences in decisions may provide insights about the mixed results in the previous literature that compared decisions on behalf of others to those on behalf of oneself.

A preview of results is that there is a significant difference in risk taking on behalf of others between different institutions. Decision makers who voluntarily contribute to social assistance are less risk averse in decision making than decision makers in the institution where everyone needs to make a decision. The difference between the institutions arises from both external institutions and intrinsic personal characteristics. On the one hand, volunteer decision makers take more risk in decision making when deciding with their own contributions in the presence of the opportunity of volunteering than when they have to make decisions with given resources. This provides evidence for influence of institutions on behavior. The more risk taking of volunteers may arise from warm glow (Andreoni, 1989), impact seeking (Duncan, 2004; Hungerman and Ottoni-Wilhelm, 2018), optimism from good intentions (Niehaus, 2014), and paternalism in social assistance (Reamer, 1983). On the other hand, when volunteer decision makers have to make decisions, they are more risk averse than those unwilling to volunteer. This shows evidence that social preferences are correlated with risk taking on behalf of others. In addition, volunteer decision makers are more risk averse when deciding for others than for themselves and the decision makers who have to make decisions show no other-self difference in risk taking. This paper sheds light upon how decision making differs in provision of social assistance between welfare institutions, and explains the difference with effects of institutions and of selection of decision makers.

The paper proceeds as follows: Section 2 presents the experimental design, Section 3 explains the empirical strategy, Section 4 demonstrates the results before Section 5 concludes.

2 Experiment

The experiment is designed to compare the risk taking decisions made in different welfare institutions, and to investigate the mechanisms behind risk taking on behalf of others. The experiment contains two treatments, *Non-voluntary* treatment and *Voluntary* treatment. *Non-voluntary* treatment represents the welfare institution in which everyone is entitled to social assistance and *Voluntary* treatment represents the welfare institution in which social assistance depends on voluntary contributions. In *Non-voluntary* treatment, the decision makers are aware of the availability of social assistance to everyone and they need to make decisions about risk taking on behalf of others. In *Voluntary* treatment, the decision makers first decide whether they want to voluntarily contribute to social assistance and volunteers go on to make decisions on behalf of others. Then the decision makers who did not want to contribute are asked to make decisions for different recipients who are not the recipients the decision makers chose not to help, and the social assistance is funded by the experimenter. The non-volunteers were not told about the decision making for different recipients when they made the volunteering choice. The risk taking decisions of the non-volunteers are the same as the risk taking decisions made by everyone in *Non-voluntary* treatment.

2.1 General procedure

The participants were recruited from Amazon Mechanical Turk. There were 1900 participants in this study.[1] All of the participants were residents in the USA. The experiment was held in two waves, with a small share of participants completing the experiment in a pilot study before the large share of participants were recruited. Both waves were finished in March 2017. The design follows the pre-analysis plan (Xu, 2017). The design is the same in the pilot study and the second wave, except that the price of the experimental currency unit is different: In the pilot part, each token is worth US$ 0.03 and in the second wave each token is worth US$ 0.02. This difference did not make the decisions made by participants different in the two waves (see footnote 2 in Section 2.2). Payoffs for the participants consist of a show-up fee and an earning which is based on the decision outcomes. The average payment was US$ 3.3. The experiment instructions can be found in Appendix B.1.

[1]The sample size is determined by a simulation based on a series of possible effect size (i.e., selection effects size). The assumptions are referred to previous experimental studies with the same investment game (Charness and Gneezy, 2012; Andersson et al., 2016a). The sample size is chosen so that the power is around 80%. Details of determining the sample size can be found in the pre-analysis plan (Xu, 2017), which was registered on the AEA RCT Registry before the experiment started.

2.2 Experiment design

At the beginning of the experiment, all participants finish a hypothetical dictator game. The dictator game was introduced by Kerschbamer (2015), and it provides a non-parametric approach to measure social preferences. The dictator game includes ten binary choices and requires the same workload of all participants. The ten choices are listed in Appendix A. This task not only creates an expectation of getting paid for finishing the task among participants, but also provides information about social preferences for the analysis of the selection effect. Kerschbamer and Muller (2017) have used the same dictator game as the task to elicit social preferences and found that the decisions in the game are valid predictors for distributional attitudes and voting behavior. The participants are informed that their final payoffs would not depend on their decisions in this task.

After finishing the dictator game, the experiment procedure is different for the recipients and the decision makers. The recipients are asked hypothetical questions about risk preferences and finish a short follow-up survey. The recipients are informed that their bonus payments depend on the decision outcomes of the decision makers. The decision makers follow the procedure illustrated in Figure 1. There is no feedback about any decision outcome or payoff throughout the whole experiment, and all participants receive the payoffs after the experiment.

Each decision maker is rewarded 100 tokens for finishing the dictator game and is asked to play an investment game. Each token is worth US$ 0.02.[2] In the investment game (Gneezy and Potters, 1997), a decision maker needs to choose how many tokens out of the 100 tokens, x, to invest in an asset that may return $2.5x$ with a chance of $1/3$, or lose all invested tokens with a chance of $2/3$. The expected payoff is $100+\frac{1}{6}\cdot x$. Risk averse people would invest some tokens between 0 and a hundred, and the investment decisions of risk-neutral and risk-seeking people should be 100 tokens. The decision makers are told that the final payments depend on the random process of the lottery. Let the decision on behalf of oneself be notated as Y_i^S, where the subscript i is a decision maker and the superscript S means that the decision is made on behalf of oneself.

After making the investment decisions, the decision makers are randomly allocated in two treatments; *Non-voluntary* treatment and *Voluntary* treatment. The proportion of *Non-voluntary* treatment and *Voluntary* treatment are one third (33%) and two thirds (67%), respectively. The unequal distribution between the two treatments is because the proportion of the decision makers who would choose to volunteer is assumed to be 50%. Since the number of decision makers in *Voluntary* treatment is around double the decision makers in *Non-voluntary* treatment, the number of decision makers in the *Non-voluntary* treatment and the number of decision makers

[2]Due to the limit of budget, each token in the pilot part of the experiment is worth US$ 0.03. In the rest part of the experiment, each token is worth US$ 0.02. Participants in the pilot part of the experiment count around 25% of the total participants. The decisions do not differ between the pilot part and the rest part of the experiment, $p = 0.26$ for t-test on the decisions on behalf of oneself and $p = 0.29$ for the t-test on the decisions on behalf of others.

in the *Voluntary* treatment who would choose to volunteer would be similar. Each decision maker in *Non-voluntary* treatment is asked to play the investment game on behalf of the recipient who is randomly matched with her. The decision maker is informed that the recipient is rewarded 100 tokens for finishing the hypothetical dictator game. The decision maker is also told that no other questions would be relevant for her payoff after this decision. Let the decision made by the decision maker be notated as Y_i^{NV}, where the subscript i is a decision maker and the superscript NV means that the decision is made in the institution where the decision maker is asked to decide for a recipient. Each decision maker in *Voluntary* treatment is asked to make a choice between the following two alternatives: The first alternative is to make a decision in the investment game on behalf of the recipient randomly matched with her. The recipient would get an earning from the decision outcome, and the final earning of the decision maker would not be affected. The second alternative is to double the outcome of her own decision and the recipient would get zero. A decision maker who chooses to make the decision on behalf of a recipient, a volunteer, is told that no other questions would be relevant for her payoff after the decision. There is no such message for a decision maker who chooses the second alternative. Let the decision made by a volunteer decision maker be notated as Y_i^V, where the subscript i is a decision maker and the superscript V means that the decision is made in the institution where the decision maker volunteers to contribute and decide on behalf of the recipient. The willingness to volunteer is notated as V, $V = 1$ if a decision maker is willing to volunteer, otherwise $V = 0$. The notation V allows me to term some counterfactual outcomes later.

After making the volunteering decision, each volunteer decision maker decides on behalf of the recipient. Each non-volunteer decision maker is then told that she is re-matched with a different recipient, and she needs to make a decision on behalf of the re-matched recipient. The previously matched recipients with the non-volunteers do not get anything above the show-up fee. Before making the decision, the non-volunteer decision maker is told that this is the last question that is relevant for anyone's payoff. Let the decision made by the non-volunteer be notated as Y_i^{NV}, where the subscript i is a decision maker and the superscript NV means that the decision is made in the institution where the non-volunteer decision maker is asked to make the decision. It is assumed that only the institution matters for the decisions, not the treatment.

At the end of the experiment, all decision makers finish a short survey. The survey is the same as the one for all the recipients. This survey asks background information such as age, gender, education and what party they would choose if there was an election tomorrow. The follow-up survey can be found in Appendix B.6.

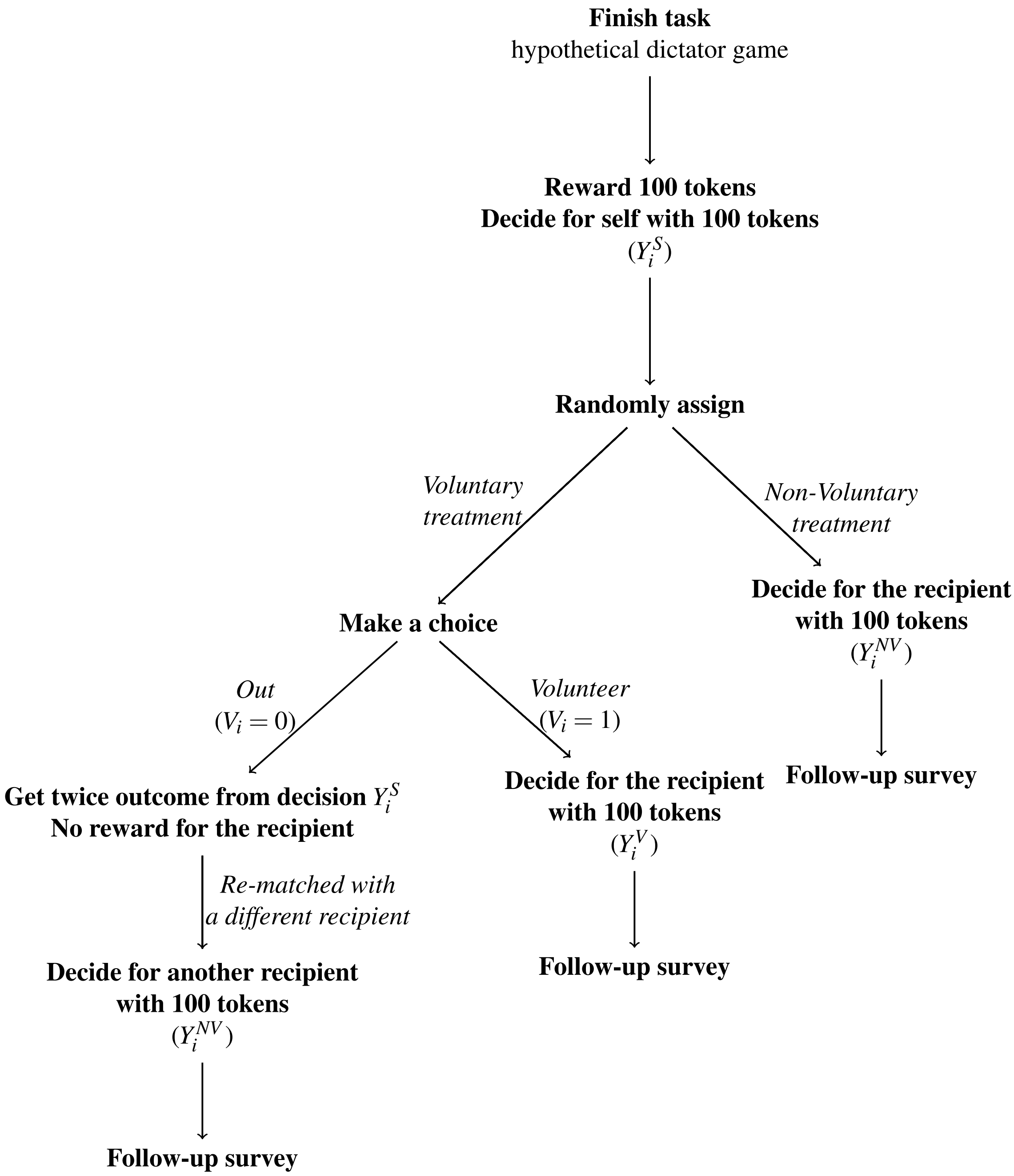


Figure 1: Experiment design

Notes: This figure describes the procedure for a decision maker. A decision maker can be assigned to the *Non-voluntary* treatment with a chance of 33% and the *Voluntary* treatment with a chance of 67%. When choosing between *Volunteer* and *Out*, a decision maker in the *Voluntary* treatment is unaware of the subsequent decision making for a re-matched recipient. All the recipients finish the first step of hypothetical dictator game, and answer a couple of hypothetical questions about risk taking. The follow-up survey is the same for all the participants. All decision outcomes are revealed after the experiment is over.

3 Empirical strategy

In this section, I test whether the decision making differs in welfare institutions and investigate mechanisms behind the different risk taking decisions. This section starts by proposing the hypotheses in Section 3.1. In Section 3.2, I explain how the effects hypothesized are identified by decisions in the experiment, and Section 3.3 details the methods of estimation.

3.1 Hypotheses

The experiment allows to answer the question whether different welfare institutions bring about different risk taking decisions on social assistance. Such a difference may arise from the different external institution and/or different decision makers. I begin with a hypothesis of the overall difference of risk taking decisions between the institutions. The next two hypotheses propose possible mechanisms. The last hypothesis will extend to the discussion about the difference between decisions on behalf of others and oneself.

Hypothesis 1. *Risk taking decisions on social assistance are different in different institutions (overall difference).*

In *Non-voluntary* treatment, decision makers are exogenously assigned and everyone is involved in decision making with given money. In *Voluntary* treatment, decision makers are endogenous and only the volunteers make decisions with their own contributions. By giving up twice of their own decision outcomes, the volunteer decision makers prevent passive recipients from no reward and decide on behalf of the recipients. Decision making of the two groups differs in two ways, the source of money for assistance and the decision makers. On one hand, the decision makers in *Voluntary* treatment choose to voluntarily provide assistance to the recipients and decide for the recipients. The decision makers in *Non-voluntary* treatment do not have the opportunity to contribute and are asked to decide with the money that the recipients are entitled to. The difference in the source of money arises from the external institution. On the other hand, volunteer decision makers are intrinsically different since they volunteer at the cost of losing the chance to double their own earnings. The willingness to volunteer might result from their intrinsic characteristics such as social preferences. These differences may reflect on decisions on behalf of others in different institutions, as stated in Hypothesis 1. The alternative hypothesis is that decisions in different institutions are the same. The *overall difference* is defined as

$$\textit{Overall difference} \equiv E[Y^V|V=1] - E[Y^{NV}].$$

$E[Y^V|V=1]$ is the expected decision on behalf of others of the decision makers who are willing to volunteer to help others in the voluntary institution, see also Figure 1. V is an indicator for the choice to volunteer or not, and it equals one if a decision maker is willing to volunteer and zero if unwilling to volunteer. Y^{NV} is the decision on behalf of others of the decision makers in the non-voluntary institution.

Hypothesis 2. *Same people make different risk taking decisions on social assistance in different institutions (institutional effect).*

Given that one is willing to contribute to social assistance, she may behave differently in risk taking decisions when the opportunity of volunteering is present. The behavior change with the decision making institution has been found in previous studies. Bó et al. (2010) found that people behave differently in a cooperation game when a fine is endogenously chosen by the subjects than when it is exogenously imposed. People might behave differently when they self-select to make risk taking decisions, than when they are asked to make such decisions. In a field experiment, Gneezy and Rustichini (2000) found that a fine for being late to pick up children at kindergartens increased late-arriving, and they argued that a simple external change may influence people's perceptions regarding the environment and their decision making. When social assistance depends on voluntary contribution, decision makers may perceive more social impacts they make by providing assistance and deciding for others. The perceived social impacts can be different when decision makers decide for others with given money in the other institution. We may expect an effect of the external institution on risk taking decisions on social assistance. The institutional effect is defined as

$$\textit{Institutional effect} \equiv E[Y^V|V=1] - E[Y^{NV}|V=1].$$

$E[Y^V|V=1]$ is the expected decision of volunteer decision makers, and $E[Y^{NV}|V=1]$ is the expected decision of decision makers willing to volunteer in the non-voluntary institution.

Hypothesis 3. *People with different attitudes toward voluntary contribution to social assistance behave differently in risk taking decisions on social assistance (selection effect).*

In an institution where social assistance does not rely on voluntary contributions, people who would voluntarily help others may behave differently in risky welfare decisions than those who would not help others. Since to volunteer means losing the chance of doubling own earning, decision makers who are willing to volunteer can be intrinsically different from those unwilling to volunteer. The intrinsic difference may lie in social preferences, social minding, social responsibility, etc. As social preferences and social responsibility are found to affect risk taking on behalf of others (Andersson et al., 2019; Bolton et al., 2015; Freundt and Lange, 2017; Pahlke et al., 2015), we may hypothesize about the impacts of attitudes toward volunteering on risk taking decisions on social assistance. The selection effect is defined as

$$\textit{Selection effect} \equiv E[Y^{NV}|V=1] - E[Y^{NV}|V=0].$$

$E[Y^{NV}|V=1]$ is the expected decision made by decision makers who are willing to volunteer when they are asked to decide for others, and $E[Y^{NV}|V=0]$ is the expected decision made by decision makers who are unwilling to volunteer when they are asked to decide for others.

Hypothesis 4. *People with different attitudes toward voluntary contribution to social assistance behave differently in risk taking decisions on social assistance, compared to decisions on their own behalf (selection effect on other-self difference).*

The last hypothesis extends from the third hypothesis and looks at the difference in risk taking on behalf of others and on behalf of oneself. The issue of other-self difference in risk taking has been discussed in existing studies, and the results are mixed with more risk taking on behalf of others than oneself, less risk taking, and no difference between decisions on behalf of others and oneself. To forward this hypothesis is to see if the intrinsic social minding behind volunteering can provide insights in the so far mixed findings about the difference between risk taking on behalf of others and oneself (Chakravarty et al., 2011; Eriksen and Kvaløy, 2010; Reynolds et al., 2009; Sutter, 2009). The selection effect on the difference between decisions for others and oneself is defined as

$$\textit{Selection effect on other-self difference} \equiv E[(Y^{NV}-Y^{S})|V=1] - E[(Y^{NV}-Y^{S})|V=0]$$

The first component is the expected other-self difference in risky decisions made by decision makers who are willing to volunteer. The second component is the expected other-self difference in risky decisions made by decision makers who are unwilling to volunteer.

3.2 Identification

In this section, I explain how the effects are identified by the decisions in the experiment. Although most of the components in the effects can be observed directly in the experiment, the challenge is that volunteering to contribute to social assistance is always an endogenous choice, and it cannot be observed in *Non-voluntary* treatment. Decision makers cannot be randomly assigned to the treatments based on their preferences for volunteering, it is only the opportunity of volunteering that can be randomly assigned. Hypotheses 2, 3, and 4 all concern the heterogeneity in the preference for volunteering.

Hypothesis 1 states that the risk taking decisions differ between institutions. As shown in Equation 1, the overall difference can be identified by comparing the decisions of the volunteers in *Voluntary* treatment and the decisions in *Non-voluntary* treatment. Both of the two groups of decisions can be directly observed in the experiment

$$\textit{Overall difference} = E[Y^{V}|V=1] - E[Y^{NV}]. \tag{1}$$

Furthermore, to manifest the mechanisms behind the difference in decisions between the institutions, I decompose the overall difference as seen in Equation 1.1. On the right hand side of Equation 1.1, the first component represents the difference driven by external institutions; whether the opportunity to volunteer and decide on behalf of others is present or not. The second component is the product of the difference of risk taking driven by the intrinsic willingness to volunteer and the share of decision makers unwilling to volunteer,

$$\begin{aligned} \textit{Overall difference} \;=\; & \underbrace{\Big(E[Y^V|V=1] \;-\; E[Y^{NV}|V=1]\Big)}_{\textit{Institutional effect}} + \\ & \underbrace{\Big(E[Y^{NV}|V=1] \;-\; E[Y^{NV}|V=0]\Big)}_{\textit{Selection effect}} \cdot \Big(1-P(V)\Big). \end{aligned} \tag{1.1}$$

$E[Y^V|V=1]$ is the expected decision of the volunteer decision makers in voluntary institution. $E[Y^{NV}|V=1]$ is the expected decision of the decision makers who are willing to volunteer in non-voluntary institution. $E[Y^{NV}|V=0]$ is the expected decision of the decision makers who are unwilling to volunteer in non-voluntary institution. $P(V)$ is the share of decision makers who are willing to volunteer.[3]

Hypothesis 2 states that risk taking decisions on behalf of others may change with the external institution. The decision makers behave differently in risk taking when it is possible to volunteer and make decisions with their own contributions than when they are asked to make the decisions with the public resource. The institutional effect can be seen by comparing the decisions of volunteer decision makers to those of the decision makers in *Non-voluntary* treatment who are willing to volunteer but not given the opportunity to volunteer,

$$\textit{Institutional effect} \;=\; E[Y^V|V=1] \;-\; E[Y^{NV}|V=1]. \tag{2}$$

Because the preference for volunteering is unobserved in *Non-voluntary* treatment, the decisions of the decision makers who are willing to volunteer in *Non-voluntary* treatment cannot be directly estimated. However, if we consider the decisions in *Non-voluntary* treatment and the pooled decisions of both the volunteers and non-volunteers in *Voluntary* treatment, the institutional effect can be identified by comparing the decisions between the two treatments. Since the decision makers who are unwilling to volunteer never make decisions in voluntary institution, the institutional effect only influences risk taking of the decision makers who are willing to volunteer. If the proportion of the decision makers who are willing to volunteer is not zero, then a difference in risk taking between the two treatments should be expected if there is institutional effect. To see how the institutional effect is identified by comparing decisions between

[3]The decomposition is derived by expanding Equation 1 with the law of total probability and rearranging. Details can be found in Appendix C.

the treatments, we first decompose the decisions in *Non-voluntary* treatment as

$$E[Y^{NV}] \;=\; E[Y^{NV}|V=1]\cdot P(V) \;+\; E[Y^{NV}|V=0]\cdot(1-P(V)).$$

Then, subtract the pooled decisions in *Voluntary* treatment with the decomposed $E[Y^{NV}]$,

$$\underbrace{E[Y^{V}|V=1]\cdot P(V) \;+\; E[Y^{NV}|V=0]\cdot(1-P(V))}_{\textit{Pooled decisions in Vol treat}} \;-\; E[Y^{NV}]$$
$$=\; (E[Y^{V}|V=1] \;-E[Y^{NV}|V=1])\cdot P(V).$$

If $P(V)\neq 0$, then

$$E[Y^{V}|V=1]-E[Y^{NV}|V=1] \;=\; \frac{\textit{Pooled decisions in Vol treat} \;-\; E[Y^{NV}]}{P(V)}.$$

The left hand side is the institutional effect. Thus, the institutional effect is identified as

$$\begin{aligned}\textit{Institutional effect} \;&=\; E[Y^{V}|V=1]-E[Y^{NV}|V=1]\\ &=\;\Big\{\underbrace{E[Y^{V}|V=1]\cdot P(V) \;+\; E[Y^{NV}|V=0]\cdot(1-P(V))}_{\textit{Vol treat}} \;-\; \underbrace{E[Y^{NV}]}_{\textit{Non-vol treat}}\Big\}\Big/P(V).\end{aligned} \tag{2.1}$$

The institutional effect is identified by the fraction of difference in decisions between the treatments and the share of decision makers who are willing to volunteer. Both the difference in decisions and the share of volunteering can be observed in the experiment.

Hypothesis 3 focuses on whether decision makers with different attitudes toward volunteering make different decisions on behalf of others. The selection effect can be seen by comparing the decision makers who are willing and unwilling to volunteer. Since the decision makers who are unwilling to volunteer would never make decisions in voluntary institution, the comparison takes place only for the decisions made when decision makers are asked to decide. The decisions are shown in the definition of the selection effect in Equation 3,

$$\textit{Selection effect} \;=\; E[Y^{NV}|V=1] \;-\; E[Y^{NV}|V=0]. \tag{3}$$

In the selection effect, the decisions of the assigned decision makers who are unwilling to volunteer can be observed from the decisions made the non-volunteers in *Voluntary* treatment. As the institutional effect, the selection effect also contains the unobserved component, the decisions of the decision makers in *Non-voluntary* treatment who are willing to volunteer. But the selection effect can be identified by comparing the decisions in *Non-voluntary* treatment and the decisions of the non-volunteers in *Voluntary* treatment. If there is a difference between the decisions in *Non-voluntary* treatment and the decisions of the non-volunteers in *Voluntary* treatment and if the proportion of volunteering is not zero, then we may expect a selection effect. The arguments for how the selection effect can be identified by comparing the decisions of the non-volunteers and the decisions in *Non-voluntary* treatment are similar to the arguments for identifying the institutional effect.[4] The selection effect is

$$\begin{aligned} \textit{Selection effect} &= E[Y^{NV}|V=1] - E[Y^{NV}|V=0] \\ &= \Big\{ E[Y^{NV}] \;-\; \underbrace{E[Y^{NV}|V=0]}_{\textit{Decisions of non-volunteers}} \Big\} \Big/ P(V). \end{aligned} \tag{3.1}$$

The selection effect can be identified by the fraction of the difference between the decisions in *Non-voluntary* treatment and the decisions of the non-volunteers, and the share of the decision makers who are willing to volunteer. All the components can be observed in the experiment.

Hypothesis 4 focuses on whether people with different attitudes toward volunteering show different patterns in the other-self difference of risk taking decisions. This hypothesis takes the individual risk taking decisions as a benchmark and asks if there is a selection effect in the difference between decisions on behalf of others and on behalf of oneself. The selection effect on the difference between decisions on behalf of others and on behalf of oneself is defined as,

$$\textit{Selection effect on other-self difference} = E[(Y^{NV} - Y^{S})|V=1] - E[(Y^{NV} - Y^{S})|V=0]. \tag{4}$$

The selection effect on other-self difference extends the selection effect to the difference between decisions for others and for oneself. Likewise, the selection effect on other-self difference contains an unobserved component, the difference between decisions for others and oneself by the decision makers in *Non-voluntary* treatment who are willing to volunteer. However, the effect can be identified by comparing the decision makers in *Non-voluntary* treatment and the non-volunteers in *Voluntary* treatment. This is because if decision makers willing and unwilling to volunteer do not have different patterns in other-self difference and the proportion of volunteering is not zero, then the other-self difference between the decision makers in *Non-voluntary* treatment and the non-volunteers in *Voluntary* treatment should not be different. Otherwise, we may expect a selection effect on other-self difference of risk taking. The arguments for identifying the selection effect on other-self difference are detailed in Appendix C. The selection effect on other-self difference of risk taking decisions is

$$\begin{aligned} \textit{Selection effect on other-self difference} &= E[(Y^{NV} - Y^{S})|V=1] - E[(Y^{NV} - Y^{S})|V=0] \\ &= \Big\{ E[(Y^{NV} - Y^{S})] - \underbrace{E[(Y^{NV} - Y^{S})|V=0]}_{\substack{\textit{Other-self diff} \\ \textit{of non-volunteers}}} \Big\} \Big/ P(V). \end{aligned} \tag{4.1}$$

[4]Details of the arguments can be found in Appendix C.

3.3 Estimation

To illustrate the overall difference in risk taking between the two decision making institutions, I compare decisions between the volunteers in *Voluntary* treatment and the assigned decision makers in *Non-voluntary* treatment. Since the decisions of the two groups of decision makers can be directly observed in the experiment, the estimate of the overall difference is shown as

$$\widehat{Overall\ difference} = \overline{Y}_{Vol,\ V=1} - \overline{Y}_{Non\text{-}vol}.$$

$\overline{Y}_{Vol,\ V=1}$ is the mean of the decisions made by the volunteer decision makers in *Voluntary* treatment. $\overline{Y}_{Non\text{-}vol}$ is the mean of the decisions made by the decision makers in *Non-voluntary* treatment.

The estimations of the effects in Hypotheses 2, 3 and 4 are built on the expressions of the effects identified in Section 3.2. The institutional effect in Hypothesis 2 is identified by comparing the decisions between *Voluntary* treatment and *Non-voluntary* treatment. The estimate of the institutional effect equals the difference between the two treatments divided by the expected share of the decision makers who are willing to volunteer,

$$\widehat{Institutional\ effect} = \frac{\overline{Y}_{Vol} - \overline{Y}_{Non\text{-}vol}}{\overline{V}}.$$

$\overline{Y}_{Vol}$ is the mean of the decisions in *Voluntary* treatment. $\overline{Y}_{Non\text{-}vol}$ is the mean of the decisions in *Non-voluntary* treatment. $\overline{V}$ is the mean share of volunteers. The estimate of the institutional effect is based on the expression in Equation 2.1. When estimating the effect, due to Jensen's inequality, the expected inverse of the estimated probability, $E[1/\overline{V}]$, is not equal to the inverse of the probability, $1/E[\overline{V}]$, this leads to a bias in the estimate of the effect sizes.[5] The same bias is also in the selection effect and the selection effect on other-self difference, as the two estimates of the two effects include the mean share of volunteers ($\overline{V}$) as the denominator. Given the sample size and the observed proportion of volunteering in the current experiment, this bias is very small.[6]

The estimate of the selection effect equals the difference in decisions between the non-volunteers and the decision makers in *Non-voluntary* treatment divided by the expected share of the decision makers who are willing to volunteer,

$$\widehat{Selection\ effect} = \frac{\overline{Y}_{Vol,\ V=0} - \overline{Y}_{Non\text{-}vol}}{\overline{V}}.^{7}$$

$\overline{Y}_{Vol,\ V=0}$ is the mean of the decisions made by the non-volunteer in *Voluntary* treatment, and $\overline{Y}_{Non\text{-}vol}$ is the mean of the decisions made by the decision makers in *Non-voluntary* treatment.

[5] Jensen's inequality states that if Z is a random variable and φ is a convex function, then $\varphi(E[Z]) \leq E[\varphi(Z)]$.
[6] See Appendix D for details.
[7] See Equation 3.1 and Appendix C for how the expression is derived.

The estimate of the selection effect on other-self difference equals the difference between the non-volunteers in *Voluntary* treatment and the decision makers in *Non-voluntary* treatment, divided by the expected share of the decision makers who are willing to volunteer,

$$\widehat{\textit{Selection effect on other-self difference}} = \frac{\overline{(Y - Y^S)}_{\textit{Vol, V=0}} - \overline{(Y - Y^S)}_{\textit{Non-vol}}}{\overline{V}}.^{8}$$

$\overline{(Y - Y^S)}_{\textit{Vol, V=0}}$ is the mean of other-self difference in decisions of the non-volunteer decision makers in *Voluntary* treatment. $\overline{(Y - Y^S)}_{\textit{Non-vol}}$ is the mean of other-self difference in the decisions in *Non-voluntary* treatment.

4 Results

There are 1990 participants in this experiment. 1143 participants are passive recipients, and 847 participants are decision makers. 564 decision makers are assigned to *Voluntary* treatment, and 283 decision makers are assigned to *Non-voluntary* treatment. The average payment is US$ 3.32.

Descriptive statistics of all decision makers are summarized in Table 1. The x-score and y-score are computed from choices in the hypothetical dictator game and the score values range from -2.5 to 2.5, see Appendix A. X-score and y-score indicate social preferences in unfavorable and favorable inequality, respectively. Higher values of both scores indicate more weight a decision maker puts on the payoff of someone else relative to her own payoff. A positive score means that a decision maker's utility increases with the payoff of the other person, and a negative score means that a decision maker's utility decreases with the payoff of the other person. So greater inequality aversion implies a higher score when a decision maker is ahead (y-score) and a lower score when she is behind (x-score). Self-interest implies that both scores are close to zero because self-interested decision makers only care about their own payoffs. X-score and y-score can be defined for 808 decision makers. The other 39 (4.6%) decision makers displayed inconsistency by switching between unequal and equal distributions more than once when the payoff for themselves increases along the choice lists. The share of inconsistent decision makers is similar to 4.2% inconsistent subjects in the study by Kerschbamer (2015). Instead of dropping the inconsistent decision makers, a dummy of being consistent in the dictator game is included in subsequent analyses and mean values of the scores are imposed for the inconsistent decision makers. The joint distribution of the scores is summarized in Appendix A. Similar to previous results (Kerschbamer, 2015; Kerschbamer and Muller, 2017), many decision makers (46%) have a positive y-score and a negative x-score. This means that many decision makers are inequality averse. The two scores are negatively correlated with each other ($p < 0.01$). This can be explained by the fact that y-score increases with inequality aversion and x-score

[8] See Equation 4.1 and Appendix C for how the expression is derived.

decreases with inequality aversion. Besides the distributional preferences, I collected other background information about age (in years), gender, education and political tendency. 45% of the decision makers are female and 63% have obtained college or higher degrees. 26% of the decision makers prefer the Republican party to other parties.

Table 2 reports regression results of the risk taking decisions, x-score, and y-score on background characteristics. In the first column, decisions on behalf of oneself are regressed on age, gender, education, and political preferences for all decision makers. Only the dummy for preferring the Republican party has a statistically significant effect on the risk taking for oneself. The decision makers who prefer the Republican party invest 4.7 tokens more than other decision makers. The null gender effect on risk seeking is different from the finding in previous literature that men are more risk prone than women (Croson and Gneezy, 2009), but similar to the study of Holt and Laury (2002) that used real and high incentives and did not find any gender effect on risk taking. In the second column, decisions for others are regressed on the same background variables for the observations in *Non-voluntary* treatment. Only decision makers in *Non-voluntary* treatment are included because this treatment is a baseline without selection. It shows that no background variables have a statistically significant effect on decisions for others. The demographic variables do not explain the variance in decisions on behalf of others. So decisions for others are not explained by age, gender, education, and political preferences in this sample. In the third column, x-score is regressed on the background variables among the decision makers who are consistent in the dictator game. All the variables have statistically significant effects on x-score. The negative effect of age implies that older decision makers put less weight on payoffs of others when they are behind others. Similarly, the negative effect of the dummy for female implies that female decision makers care less about others' payoffs when they are behind others. The positive effects of the dummy for high education and the dummy for preferring the Republican party imply that decision makers who are highly educated and who prefer the Republican party care more about others' payoffs when they are behind others. In the last column, y-score is regressed on the background variables also among the consistent decision makers in dictator game. Age and the dummy for preferring the Republican party have statistically significant effects on y-score. The positive effect of age implies that older decision makers put more weight on others' payoffs when they are ahead of others. This is consistent with the results of meta analysis by Engel (2011), but in contrast with the findings of Falk et al. (2018). The negative effect of the dummy for preferring the Republican party implies that decision makers who prefer the Republican party put less weight on others' payoffs when they are ahead. The effect of political preferences on distributional preferences confirms the finding by Kerschbamer and Muller (2017). They used the same dictator game introduced by Kerschbamer (2015) and found that distributional preferences elicited from the dictator game are predictive for political attitudes.

The overview of decisions is shown in Figure 2. The decisions are measured as tokens invested in the investment game on behalf of oneself or others. Both distributions in Panel A and Panel B display a three-hump shape, and many investment decisions are 0, 50, and 100 tokens. On average, decision makers invest more tokens and take more risk on behalf of others, than on behalf of themselves. Panel C of Figure 2 plots the joint distribution of the decisions on behalf of others and on behalf of oneself, together with the regression line and the weighting markers. Not surprisingly, the decisions on behalf of others are positively correlated with the decisions on behalf of oneself. The weighted markers show that many decision makers invest the same number of tokens for themselves and for others. These numbers are zero token, fifty tokens, and a hundred tokens.

Table 1: Descriptive statistics of the decision makers

Variable	Mean	S.D.	Min	Max	Obs
x-score	-0.57	1.34	-2.5	2.5	808
y-score	1.21	1.29	-2.5	2.5	808
age (years old)	35.88	11.33	19	73	847
female	0.45	0.50	0	1	847
High education	0.63	0.48	0	1	847
Prefer Republican party	0.26	0.44	0	1	847

Notes: X-score and y-score range from -2.5 to 2.5. X-score being 2.5 means a decision maker always prefers unequal distributions in favor of others to an equal distribution. X-score being -2.5 means the reversed preference. Y-score being 2.5 means a decision maker always prefers an equal distribution to unequal distributions in favor of herself. Y-score being -2.5 means the reversed preference. The dummy variable, ”High education”, equals one if a subject has college degrees, Master degree, Doctoral degree, or professional degrees such as JD (Juris Doctor), MD (Doctor of Medicine). Otherwise, the dummy equals zero. The dummy variable, ”Prefer Republican party”, equals one if a subject would vote for the Republican party in a hypothetical election. Otherwise if a decision maker would vote for the Democratic party or other options, the dummy equals zero.

Table 2: Regressions on background characteristics

Covariate	Risk taking decision		Hypothetical dictator game	
	Decision for self	Decision for others	X-score	Y-score
age (in years)	-0.14	-0.07	-0.01**	0.01**
	(0.10)	(0.16)	(0.00)	(0.00)
female	1.98	4.55	-0.33***	0.03
	(2.27)	(3.55)	(0.09)	(0.09)
high education	0.54	-1.08	0.22**	-0.06
	(2.31)	(3.67)	(0.10)	(0.09)
Republican	4.73*	4.42	0.19*	-0.36***
	(2.56)	(4.05)	(0.10)	(0.10)
Constant	40.72***	34.69***	-0.24	1.01***
	(4.05)	(6.55)	(0.17)	(0.17)
r2	0.01	0.01	0.04	0.02
N	847	283	808	808

Notes: In columns 1-4, the dependent variables are decision for self (in tokens, 0 to 100), decision for others (in tokens, 0 to 100), x-score (-2.5 to 2.5), and y-score (-2.5 to 2.5). The independent variables include age, the dummy for female, the dummy for having high education, and the dummy for preferring the Republican party. The regression in the second column only includes decision makers in *Non-voluntary* treatment. The regressions in the last two columns only include decision makers who are consistent in the hypothetical dictator game. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses.

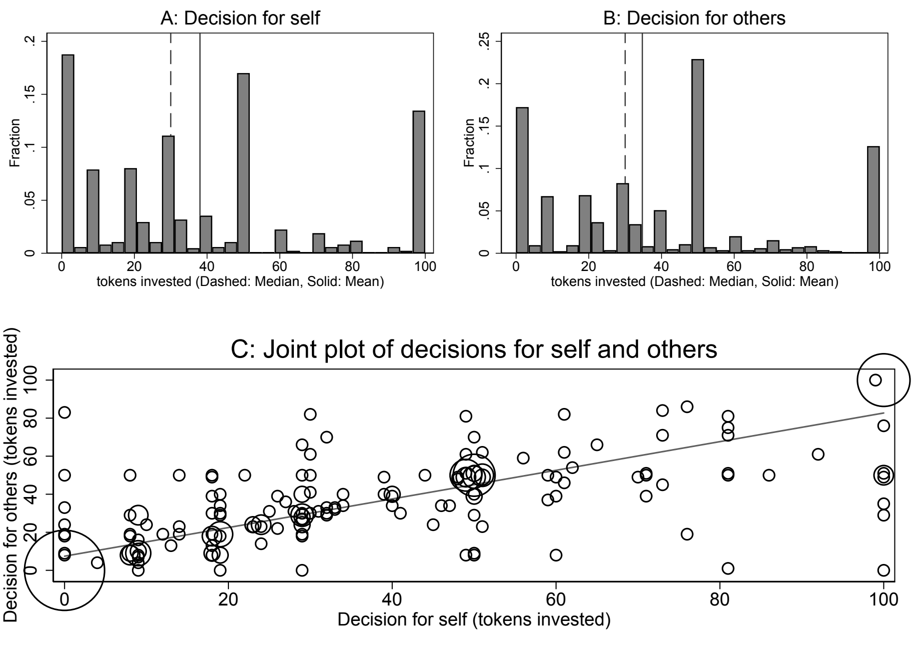


Figure 2: Distributions of decisions

Notes: Panel A shows the distribution of decisions for self in both treatments. Panel B shows the distribution of decisions for others in *Non-voluntary* treatment. Panel C shows the joint distribution of decisions for self and others in *Non-voluntary* treatment. The decisions are the number of tokens invested out of the total 100 tokens, and the decisions range from 0 to 100.

4.1 Who are the volunteers?

In *Voluntary* treatment, decision makers made a choice between opting out and volunteering (see Figure 1). By choosing to opt out, a decision maker doubled her own decision outcome and the recipient received no reward. By choosing to volunteer, a decision maker kept her own decision outcome as it was and decided on behalf of the recipient. When making this choice, decision makers face a trade-off between costs and motives in both pecuniary and moral concerns. Costs of volunteering include the expected payoff from the investment game, responsibility of taking risk for others, and possibly guilt about undesirable outcomes for others. To see how observed variables influence the volunteering decision, the volunteering decisions in *Voluntary* treatment are regressed on the covariates in a Logit model and the results are shown in Table 3. The first column only contains the decisions on behalf of oneself as the explanatory variable. The second column includes the decisions in the dictator game and the consistency of the decisions in the dictator game. The third column includes the demographic variables such as age, gender, education, and political preferences. The last column includes all the covariates.

The first column shows that an increase in the decision for oneself has a large positive effect on the probability of choosing to volunteer. When the private investment of a decision maker increases from zero to a hundred percent (all the hundred tokens), the probability of volunteering increase 23.2% as seen in the first column of Table 3. This effect is slightly smaller when we control all other covariates, as seen in the fourth column. When the subjects self-selected to contribute and decide with their contributions, more risk averse subjects are less likely to contribute. The role of private risk preferences in generosity was previously found by Cettolin et al. (2017). Their study showed that more risk averse givers allocate more to others when earnings of others are exposed to risk and allocate less to others when their own earnings are exposed to risk. Results of this study provide complementary evidence on the role of private risk preferences on generosity. When earnings of both the decision makers and recipients are exposed to risk, more risk averse decision makers are less likely to allocate to the recipients. Freundt and Lange (2017) found out that risk averse people give more to others in the absence of risk. By contrast, this study shows that in the presence of risk in others' payoffs, risk averse subjects are less likely to give and help others from no rewards.

The second column shows that the higher x-score is, the less likely a decision maker would choose to volunteer. A decision maker is is 3.4% less likely to volunteer when her x-score is one point larger. The more a decision maker weighs the payoffs of others under unfavorable inequality (less inequality averse), the less likely she is to volunteer. The effect of x-score is smaller than the effect of y-score. A decision maker is 13.9% more likely to volunteer when her y-score is one point larger. The more a decision maker weighs the payoffs of others under favorable inequality (more inequality averse), the more likely she is to volunteer. The effects of both scores are statistically significant and do not change much when the other covariates are controlled in the fourth column. The two effects are in line with the finding of **?** that the more generous a decision maker is in a dictator game, the more likely she is to equalize opportunities under risk. The volunteering decision is between an unequal distribution in favor of a decision maker (double earning for a decision maker and no earning for a recipient) and an equal distribution (both have chance to earn). The y-score indicates how benevolent a decision maker is under a favorable inequality. The positive effect of y-score confirms that the more a decision maker cares about others' welfare when they are ahead of others, the more likely she is to volunteer. As x-score captures how much a decision maker weighs others' payoffs under an unfavorable inequality, it is difficult to directly interpret the effect of x-score on the volunteering decision. However, the negative correlation between x-score and y-score explains the negative effect of x-score on the volunteering decision.

The effect of the dummy for being inconsistent in the dictator game shows that a consistent decision maker is 18.4% less likely to volunteer than an inconsistent decision maker. This large marginal effect of being consistent in the dictator game is because inconsistent decision makers are more likely to volunteer than the consistent decision makers (Fisher's test on the share of volunteering, $p = 0.06$). 65.5% of the inconsistent decision makers in *Voluntary* treatment (19 out of 29) chose to volunteer. This proportion of volunteering is higher than the proportion among the consistent decision makers; 46.5%. Female decision makers are 12% more likely to volunteer as shown in the third column and this effect holds when other covariates are included in the fourth column. The other variables including age, the dummy for having high education, and the dummy for being in the pilot study have small and insignificant effects on volunteering. The small effect of being in the pilot study shows that the decision makers in the two waves do not differ in the probability of volunteering.

The positive effect of pro-sociality on the tendency to volunteer is consistent with the findings by Carpenter and Myers (2010) that altruism has a positive effect on self-selecting as volunteer firefighters. Female decision makers are more likely to volunteer than male decision makers. This is consistent with previous findings that women are more altruistic and inequality averse (Andreoni and Vesterlund, 2001; Croson and Gneezy, 2009; Kamas and Preston, 2015) and that women are more likely to volunteer (Babcock et al., 2017).

47.5% (268) of the decision makers in *Voluntary* treatment chose to volunteer and decide on behalf of the recipients. The remaining 52.5% (296) decision makers chose to opt out. Randomized assignment to treatment implies that a similar proportion of decision makers in *Non-voluntary* treatment would have opted out if they had been asked to make the volunteering decision. The observed proportion of volunteering and the balanced covariates between the treatments imply that there should be some decision makers in *Non-voluntary* treatment who would volunteer if they were in *Voluntary* treatment.[9] Comparing the decisions of the volunteer decision makers in *Voluntary* treatment and their counterparts in *Non-voluntary* treatment reveals whether volunteers decide in different ways in different institutions. When the oppor-

[9] All the covariates in the Logit model are balanced between *Voluntary* treatment and *Non-voluntary* treatment, $p > 0.25$ in the t-tests of all the covariates.

tunity to volunteer is absent, comparing decision makers who are willing to volunteer to those unwilling to volunteer shows whether the attitude toward volunteering influences risk taking on behalf of others. The external and intrinsic impacts may lead to different risk taking decisions in different institutions. I first look at the overall difference of risk taking between the volunteers in *Voluntary* treatment and decision makers in *Non-voluntary* treatment. Then I investigate how the external and intrinsic differences drive the overall difference of risk taking between institutions.

Table 3: Marginal effects on the probability of volunteering

	(1)	(2)	(3)	(4)
decision for self (investment percent)	0.232***			0.191***
	(0.061)			(0.056)
x-score		-0.034**		-0.030**
		(0.015)		(0.015)
y-score		0.139***		0.132***
		(0.014)		(0.014)
consistency in dictator game		-0.184**		-0.197**
		(0.083)		(0.083)
age			0.003	0.002
			(0.002)	(0.002)
female			0.120***	0.105***
			(0.041)	(0.038)
high education			0.003	0.040
			(0.043)	(0.040)
Republican			-0.031	0.005
			(0.048)	(0.044)
pilot			-0.071	-0.037
			(0.049)	(0.045)
N	564	564	564	564

Notes: The dependent variable is equal to one if a decision maker chooses to volunteer and make the decision on behalf of a recipient, otherwise it equals zero if a decision maker chooses to double her own decision outcome and leave the recipient with no reward. The observations are the decision makers in *Voluntary* treatment. The independent variables include decisions on behalf of oneself (scaled as percent of investment out of the total 100 tokens), x-score and y-score from the hypothetical dictator game (see Appendix A), dummy for consistency in the dictator game, age, dummy for female, dummy for high education, dummy for preferring Republican party, and dummy for being in the pilot study. The higher the x-score is, the more a decision maker cares about others' payoffs in an unfavorable inequality. The higher the y-score is, the more a decision maker cares about others' payoffs in a favorable inequality. The x-score and y-score are available only for the decision makers who were consistent in the dictator game. For the inconsistent decision makers, the two scores are set as the averages of the scores of the consistent decision makers. The dummy for consistency is one if a decision maker made consistent choices in the ten decisions of the dictator game. The dummy for high education is one if a decision maker has college or higher degree, otherwise it is zero. The dummy for preferring Republican is one if a decision maker prefers to vote for the Republican party in an assumed election, otherwise it is zero. The dummy for being in the pilot study is one if a decision maker participated in the pilot study, otherwise it is zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses.

4.2 Overall difference

The overall difference is defined as the difference of risk taking between decision makers who were exposed to different institutions of decision making, as seen in Equation 1. One institution is that the decision makers choose whether to contribute to social assistance and volunteers decide on behalf of the recipients with their contributions. The other institution is that decision makers are asked to decide on behalf of the recipients with money that the recipients are entitled to. Volunteer decision makers in *Voluntary* treatment decide in the former institution and decision makers in *Non-voluntary* treatment decide in the latter institution. Decision making in the two institutions differs in both the presence of the opportunity of volunteering and the selection of decision makers. The difference of risk taking between volunteers in *Voluntary* treatment and decision makers in *Non-voluntary* treatment shows the total effect of institutions and selection on risk taking for others.

Figure 3 plots the decisions of the volunteer decision makers in *Voluntary* treatment and the decision makers in *Non-voluntary* treatment. On average, volunteer decision makers in *Voluntary* treatment invest 6.93 (0.22 of standard deviation) more tokens on behalf of the recipients than the decision makers in *Non-voluntary* treatment. This overall difference is statistically significant (two-sided t-test, $p = 0.01$; $N = 549$). Volunteer decision makers are less risk averse than assigned decision makers on behalf of the recipients. The overall difference is descriptive and no control variables are included in the regression of the overall difference. This is because the overall difference may arise from the different institutions or different decision makers or both. A regression with control variables would rule out the effect of selection of decision makers, since the effects of background variables on self-selecting to be volunteers are considerable, see Table 3. In the next subsection, I decompose the overall difference into the effect of the institution and the effect of intrinsic characteristics as demonstrated in Section 3.2.

Result 1. *Decision makers take more risk in decisions on social assistance in an institution where they decide with their own resources than in an institution where all make decisions with provided resources.*

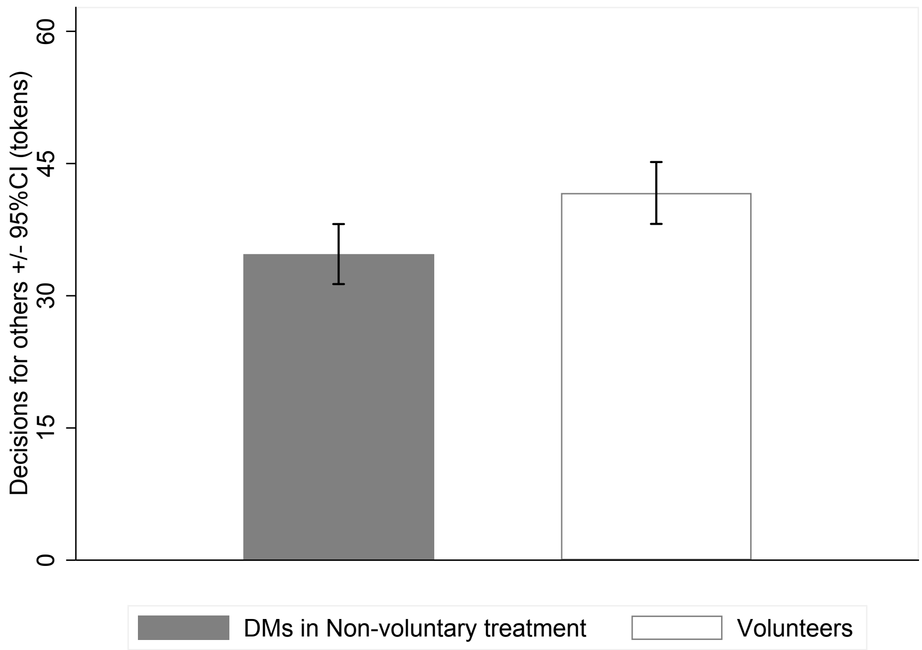


Figure 3: Overall difference

Notes: The left bar shows the average number of tokens invested by the decision makers in *Non-voluntary* treatment and the right bar shows the average number of tokens invested by the volunteer decision makers in *Voluntary* treatment. The number of invested tokens ranges from 0 to 100. The error bars depict 95 percent confidence intervals of the mean decisions.

4.3 Decompose the overall difference

In the decomposition exercise of Equation 1.1, the overall difference is attributed to two components. One component represents the effect of the external institution and the other represents the effect of intrinsic characteristics of decision makers. Recall the decomposed overall difference as below,

$$\begin{aligned} \textit{Overall difference} \;=\; & \underbrace{\Big(E[Y^V|V=1] \;-\; E[Y^{NV}|V=1]\Big)}_{\textit{Institutional effect}} + \\ & \underbrace{\Big(E[Y^{NV}|V=1] \;-\; E[Y^{NV}|V=0]\Big)}_{\textit{Selection effect}} \cdot \Big(1-P(V)\Big). \end{aligned}$$

If at least one decision maker decides not to volunteer, then $P(V) \neq 1$, and both the external institution and the selection of decision makers influence the overall difference in risk taking. The institutional effect and the selection effect is discussed in the following subsections.[10]

4.3.1 Institutional effect

Figure 4 shows the comparison between decisions in *Non-voluntary* treatment and in *Voluntary* treatment. On average, decision makers in *Voluntary* treatment invest more tokens than decision makers in *Non-voluntary* treatment on behalf of the recipients. The first column of Table 4 shows that decision makers on average invest 7.09 more tokens (0.23 of standard deviation) for the recipients in *Voluntary* treatment than in *Non-voluntary* treatment. The second column of Table 4 shows that the difference in decisions between the two treatments is smaller but still statistically significant when the background variables are controlled. The background variables have impacts on decisions on behalf of recipients. Risk taking on own behalf positively affects risk taking on behalf of others. The coefficient of the decision for self is statistically significant and less than one. This implies a link between attitude toward private risk and attitude toward others' risk. Decision makers refer to private risk taking when deciding for others. I return to the other-self comparison in more detail in Section 4.4. The negative effect of age on risk taking means that older decision makers take less risk for others. This is consistent with previous results of increasing risk aversion with age (von Gaudecker et al., 2011; Dohmen et al., 2011; Tymula et al., 2013). The pro-sociality in a favorable inequality (y-score) has negative effects on risk taking for others. The negative effect of the pro-sociality index (y-score) conforms to the findings that social preferences may influence the risk taking on behalf of others (Andersson

[10] In the pre-analysis plan (Xu, 2017), I registered the method of propensity score matching to estimate the effects. A propensity score of each decision maker is computed from an estimated logistic model. Based on the score, each volunteer is matched with a decision maker in *Non-voluntary* treatment so to identify the willingness to volunteer in *Non-voluntary* treatment. If there is some unobserved variable that can explain the volunteering decision and is not included in computing the propensity score, then the matching quality deteriorates and the estimation can be biased. The results of propensity score matching are statistically significant and qualitatively similar to the results in this section but quantitatively smaller, see details in Appendix F.

et al., 2016b; Montinari and Rancan, 2018).

The coefficients of dummy for *Voluntary* treatment in the first two columns of Table 4 are both statistically significant. The difference in decisions for the recipients between the two treatments provides evidence for the institutional effect. The institutional effect is not equal to the conditional difference shown by the coefficient, unless the expected share of volunteering is equal to one. To estimate the institutional effect, we follow the expression of estimate in Equation 3.3. The institutional effect is estimated by dividing the observed difference between the two treatments by the share of volunteering.

The estimate of the institutional effect is shown in Table 5. Decision makers who are willing to volunteer take more risk when they volunteer to provide assistance and decide for the recipients (*Voluntary* treatment), than they do when they are asked to decide for the recipients (*Non-voluntary* treatment). The difference is around 14.9 tokens with a standard error of 4.7. The institutional effect is statistically different from zero ($p < 0.01$). It is evident that the institution of decision making has impacts on the risk taking on behalf of the recipients, and the opportunity of volunteering leads to greater risk taking for the recipients.

Result 2. *Volunteer decision makers take more risk on behalf of others in the institution where they have the opportunity to contribute to social assistance and decide than in the institution where they are asked to make decisions.*

4.3.2 Selection effect

Figure 5 compares the decisions of the decision makers in *Non-voluntary* treatment to the decisions of opt-out decision makers in *Voluntary* treatment. The decision makers in *Non-voluntary* treatment invest less tokens on behalf of the recipients than the opt-out decision makers in *Voluntary* treatment. The opt-out decision makers in *Voluntary* treatment on average invest 7.23 more tokens (0.23 of the standard deviation) for the recipients than decision makers in *Non-voluntary* treatment, as seen in the third column of Table 4. The fourth column of Table 4 shows that the difference of decisions between the non-volunteers in *Voluntary* treatment and decision makers in *Non-voluntary* treatment is slightly larger and statistically significant when the background variables are controlled. The effects of the background variables on risk taking for the recipients among the opt-out decision makers in *Voluntary* treatment and the decision makers in *Non-voluntary* treatment are shown in the fourth column of Table 4. These effects of the background variables are similar to the effects shown in the second column with all the decision makers included. The coefficient of decision for oneself is statistically significant. The coefficient of age is statistically significant and negative. One notable difference between the results in the fourth column and the second column is that the coefficient of y-score turns not statistically significant when the volunteer decision makers are not included.

If decision makers unwilling to volunteer take same risk on behalf the recipients as do the decision makers willing to volunteer, then there is no selection effect and the decisions of

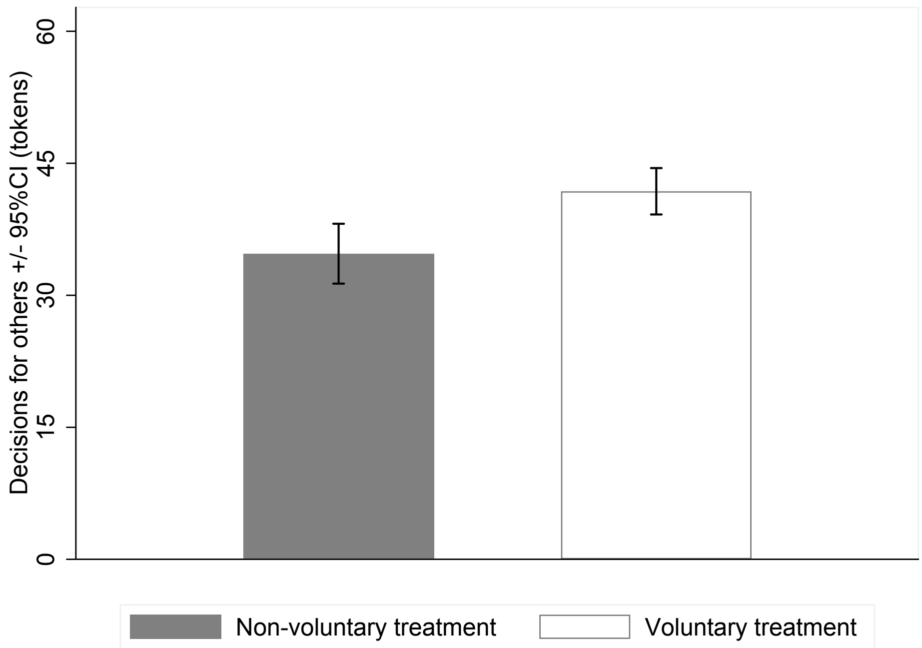


Figure 4: DMs in Non-voluntary treatment vs. DMs in Voluntary treatment

Notes: The left bar shows the average number of tokens invested by the decision makers in *Non-voluntary* treatment and the right bar shows the average number of tokens invested by the decision makers in *Voluntary* treatment. The number of invested tokens ranges from 0 to 100. The error bars depict 95 percent confidence intervals of the mean decisions.

Table 4: Components of observed differences in Hypotheses 2-4

Covariate	Decisions for others *Vol vs. Non-vol*		Decisions for others *Opt-out vs. Non-vol*		Other-self difference *Opt-out vs. Non-vol*	
	(1)	(2)	(3)	(4)	(5)	(6)
Vol treatment	7.09***	5.30***	7.23***	8.25***	9.33***	8.95***
	(2.19)	(1.39)	(2.64)	(1.81)	(1.99)	(1.99)
decision for self (tokens)		0.72***		0.70***		
		(0.03)		(0.03)		
x-score		-0.29		-0.57		-0.75
		(0.51)		(0.69)		(0.74)
y-score		-1.42**		-1.18		-1.17
		(0.59)		(0.74)		(0.83)
age (years old)		-0.12**		-0.14**		-0.07
		(0.06)		(0.07)		(0.08)
female		-1.64		0.18		-0.06
		(1.40)		(1.81)		(1.94)
high education		0.83		1.98		1.88
		(1.37)		(1.79)		(1.96)
consistency		2.41		3.26		5.44
		(2.35)		(3.27)		(3.42)
Republican		1.06		2.19		1.63
		(1.67)		(2.14)		(2.31)
pilot		0.30		-0.02		-0.60
		(1.72)		(2.16)		(2.39)
Constant	34.73***	11.91***	34.73***	10.49**	-1.53	-4.67
	(1.73)	(3.25)	(1.73)	(4.23)	(1.09)	(4.33)
r2	0.01	0.58	0.01	0.53	0.04	0.04
N	847	847	579	579	579	579

Notes: The dependent variable in the first four columns is the decision made on behalf of others. The decisions are in the number of invested tokens, ranging from 0 to 100. The dependent variable in the last two columns is the difference in decisions made on behalf of others and on behalf of oneself. The difference in decisions ranges from -100 to 100. The independent variables are decision for oneself (excluded in the last two columns), x-score and y-score from the hypothetical dictator game (see Appendix A), age, dummy for female, dummy for high education, dummy for consistency in dictator game, dummy for preferring Republican party, and dummy for being in the pilot study. The x-score and y-score are computed only for the decision makers who were consistent in the hypothetical dictator game. For the inconsistent decision makers, the two scores are set as the respective average values among the consistent decision makers. The first and second columns include the decisions for the recipients made by all the decision makers in both treatments. From the third column onward, each column includes the opt-out decision makers in *Voluntary* treatment and the decision makers in *Non-voluntary* treatment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses.

Table 5: Estimates of the effects (Bootstrap s.e.)

	Estimates
Institutional effect	14.92***
	(4.72)
Selection effect	-15.23***
	(5.51)
Selection effect on other-self difference	-19.63***
	(4.25)

Notes: All the effects are measured as tokens. The positive institutional effect means that the volunteers invest more for others in *Voluntary* treatment than in *Non-voluntary* treatment. The negative selection effect means that the decision makers who are willing to volunteer invest less for others than those unwilling to volunteer. The negative selection effect on other-self difference means that the other-self difference of the decision makers willing to volunteer is smaller than those unwilling to volunteer. The coefficients used to compute the estimates correspond to the coefficients of the dummy for *Voluntary* treatment in the first, third, and fifth columns in Table 4. The estimate for the expected share of volunteering is the observed share of volunteering in *Voluntary* treatment. The estimates are computed by dividing the coefficients with the share of volunteering. For the institutional effect, all the decision makers are included in estimation. For the selection effect and selection effect on other-self difference, the non-volunteer in *Voluntary* treatment and decision makers in *Non-voluntary* treatment are included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses are bootstrapped based on 800 replications with replacements.

opt-out decision makers should not be different from the decisions of the decision makers in *Non-voluntary* treatment. The observed difference instead shows evidence for the selection effect. Recall the estimate of the selection effect,

$$\underbrace{\left(E[Y^{NV}|V=1] - E[Y^{NV}|V=0]\right)}_{\textit{Selection effect}} = \frac{\overline{Y}_{\textit{Non-vol}} - \overline{Y}_{\textit{Non-volunteers in Vol}}}{\overline{V}}.$$

The selection effect is not equal to the coefficient of the dummy for *Voluntary* treatment, unless the share of volunteering is equal to one. To estimate the selection effect, the average difference is divided by the mean share of volunteering. The selection effect is around 15.2 tokens with a standard error of 5.5. The selection effect is significantly different from zero ($p < 0.01$). Decision makers with different attitudes toward volunteering take different amounts of risk on behalf of the recipients, although the opportunity of volunteering is absent and they are asked to make the decisions. Decision makers unwilling to volunteer take more risk on behalf of the recipients than those willing to volunteer.

The selection effect shows that the non-volunteers take more risk on behalf of others than the volunteers do. In contrast, the finding in Section 4.1 shows that the non-volunteers take less risk on behalf of themselves than the volunteers do. This implies that volunteers and non-volunteers may exhibit different risk taking for others compared to risk taking for themselves. The other-self difference in risk taking is discussed in Section 4.4.

Result 3. *When being asked to make decisions, decision makers who are willing to volunteer take less risk in decisions of social assistance than the decision makers who are unwilling to volunteer.*

4.4 Selection effect on other-self difference of risk taking

Figure 6 depicts the comparison of decisions for others and oneself among the opt-out decision makers in *Voluntary* treatment and the decision makers in *Non-voluntary* treatment. We first look at the pattern of other-self difference in each of the two groups. Decision makers in *Non-voluntary* treatment on average invest 1.53 fewer tokens for others than for themselves. This difference is not statistically different from zero (two-sided t-test, $p = 0.16$; $N = 283$), similar with previous findings (Andersson et al., 2016a; Eriksen et al., 2017). Non-volunteers in *Voluntary* treatment averagely invest 7.80 tokens more for others than for themselves. The other-self difference is statistically different from zero (two-sided t-test, $p < 0.01$; $N = 296$). The pattern of other-self difference between the two groups differs from each other ($p < 0.01$) with the background variables controlled, as seen in the last column of Table 4. The large difference of other-self comparison shows evidence for a selection effect on other-self difference of risk taking. Since the non-volunteers in *Voluntary* treatment and the decision makers in *Non-voluntary* treatment do not differ in decision on their own behalf (two-sided t-test, $p = 0.44$;

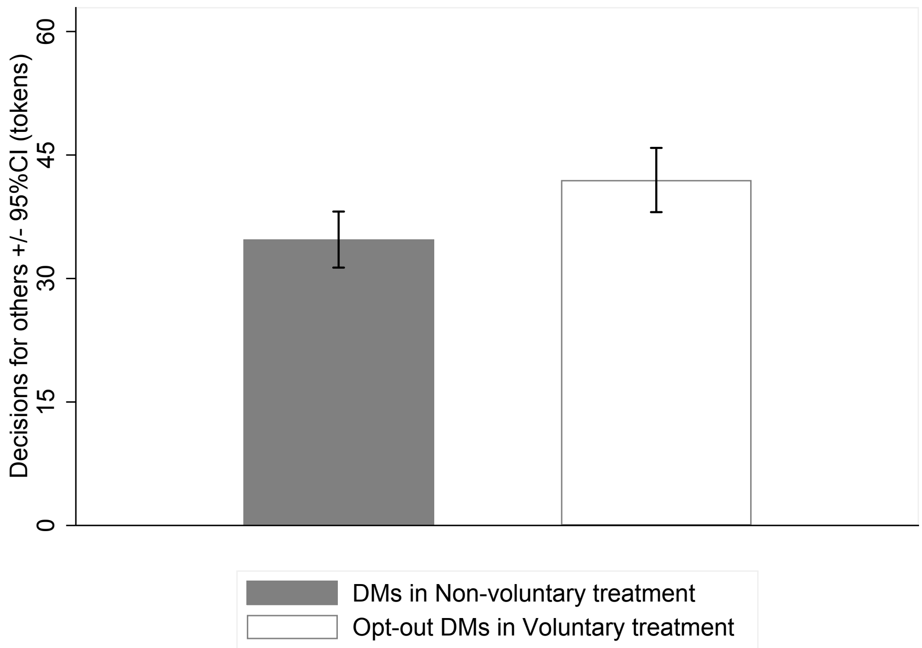


Figure 5: DMs in Non-vol treatment and Opt-out DMs in Voluntary treatment

Notes: The left bar shows the average number of tokens invested by the decision makers in *Non-voluntary* treatment and the right bar shows the average number of tokens invested by the non-volunteers in *Voluntary* treatment. The number of invested tokens ranges from 0 to 100. The error bars depict 95 percent confidence intervals of the mean decisions.

$N = 579$), the different other-self comparisons mainly arise from the difference of decisions for others. In addition, volunteer decision makers in *Voluntary* treatment are found to invest 2.55 fewer tokens for others than for themselves (two-sided t-test, $p < 0.01$; $N = 268$). This less risk taking for others than for self is consistent with previous findings (Füllbrunn and Luhan, 2015; Reynolds et al., 2009). In particular, the less risk taking of volunteers is consistent with the finding by Charness and Jackson (2009) that the sense of responsibility leads to less risk taking for others than for self. Moreover, the other-self difference in risk taking decreases with pro-sociality in favorable inequality (y-score), see Table E1 in Appendix E. The link between social risk taking and distributional preferences is found statistically significant, in contrast to Bolton et al. (2015).

The selection effect on other-self difference of risk taking is not equal to the coefficient of the dummy for *Voluntary* treatment in the fifth column of Table 4, unless the expected share of volunteering is equal to one. As shown in the expression for the effect below, the estimate of this effect is achieved by dividing the observed difference of other-self difference of risk taking between the decision makers in *Non-voluntary* treatment and the non-volunteers in *Voluntary* treatment by the share of volunteering,

$$\underbrace{(E[(Y^{NV} - Y^S)|V = 1] - E[(Y^{NV} - Y^S)|V = 0])}_{\textit{Selection effect on O-S diff}} = \frac{\overline{(Y - Y^S)}_{\textit{Vol, V=0}} - \overline{(Y - Y^S)}_{\textit{Non-vol}}}{\overline{V}}.$$

The estimate of the selection effect on other-self difference is around 19.6 tokens with a standard error of 4.3. The selection effect on other-self difference is statistically different from zero ($p < 0.01$). This implies that the gap between risk taking for others and risk taking for oneself can be explained by the attitudes toward volunteering. As the attitudes toward volunteering are endogenous from from intrinsic characteristics, the other-self difference in risk taking can be explained better taking social preferences into account. This provides insights into the mixed results about the other-self difference in risk taking in existing literature.

Result 4. *Decision makers who are unwilling to volunteer take more risk on behalf of others than themselves. This other-self difference reverses for decision makers who are willing to volunteer.*

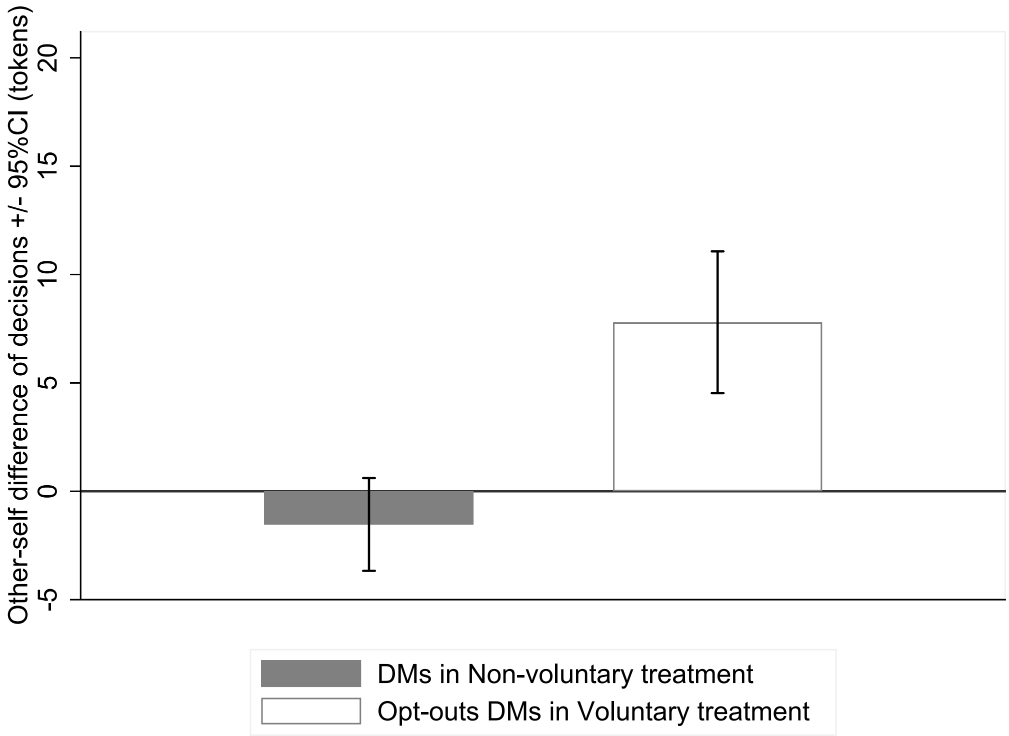


Figure 6: Other-self difference in decisions

Notes: The left bar shows the mean other-self difference of invested tokens of the decision makers in *Non-voluntary* treatment and the right bar shows the mean other-self difference of the invested tokens of the non-volunteers in *Voluntary* treatment. The other-self difference of invested tokens ranges from -100 to 100. The error bars depict 95 percent confidence intervals of the mean other-self difference.

5 Conclusion

Social assistance helps build stronger communities and promote equality and opportunity. Providers of social assistance often face tough decisions that involve uncertainties and unpredictable outcomes. In social assistance for the unemployed, the unemployment agencies need to allocate limited resources between different schemes such as income replacement and job training support. In health care, medical professionals choose what treatments and therapies to cover for poor patients and treatments help cure or prolong life expectancy with different probabilities. In child care for disadvantaged children, the child services may recruit enough low-qualified teachers to ensure all children to be taken care of, or fewer high-qualified teachers to promote children's development but possibly leave some children with insufficient care. Common in these examples is that during provision and implementation of social assistance, decisions of providers influence the final benefits received by the recipients.

Societies organize provision of social assistance in different ways. In some societies, social assistance is available to everyone and everyone is expected to voice their opinions on welfare decisions through democratic elections. In other societies, social assistance relies on voluntary contributions and the contributors to social assistance also make decisions about the provision of assistance. This study is devoted to revealing whether and how decision making differs in these two institutional settings that characterize different welfare institutions.

In a novel experiment, subjects in one treatment could choose whether to voluntarily contribute to social assistance and make risk taking decisions with their own contributions on behalf of others, and subjects in the other treatment were asked to make risk taking decisions on behalf of others with given money. I investigated whether the decision behavior is different between the two institutions and the mechanisms underlying the difference in decision making. The results show strong evidence for an overall difference in risk taking decisions on social assistance between different institutions. Volunteer decision makers take more risk on behalf of assistance recipients than assigned decision makers do. Furthermore, I decomposed the overall difference into the two components, the external institutions and the intrinsic attitudes toward volunteering. On the one hand, decision makers who are willing to volunteer take more risk on behalf of the recipients when it is possible to volunteer than when the opportunity of volunteering is absent. This confirms the institutional impacts on risk taking behavior in social assistance and indicates that individuals may behave differently in different social welfare institutions. On the other hand, decision makers who are willing to volunteer take less risk for the recipients than those unwilling to volunteer. Although the opportunity to contribute to social assistance is absent, the attitude toward volunteering still plays a strong role in risk taking for others. When everyone is involved in decision making on social assistance, the share of subjects who are willing to help others can influence social decisions in an institution. In addition, volunteer decision makers take less risk for the recipients than for themselves. The decision makers unwilling to volunteer take more risk for the recipients than for themselves. This provides evidence for the relationship between social preferences and risk taking on behalf of others, which has been investigated in recent studies. From this experiment, I found that more pro-social subjects are more cautious when deciding on behalf of others, compared to deciding on behalf of themselves.

The results of this study imply that inequality of delivered social benefits may be larger when the social assistance relies more on voluntary private contributions. This is not only because of the incomplete coverage, but also because volunteers take more risk on behalf of others. Notably, decision making in the public sector is usually under external review and evaluation, and such an accountability is found to reduce risk taking on behalf of others in previous studies (Bolton et al., 2015; Charness and Jackson, 2009; Füllbrunn and Luhan, 2015; Pahlke et al., 2015; Pollmann et al., 2014; **?**). On the other side, the outcome-based earning incentives for workers in voluntary private sectors may encourage risk taking on behalf of others as competition and bonuses may increase risk taking on behalf of others (Andersson et al., 2019) compared to decision making under no incentives. The difference shown in the experiment might be further reinforced by other institutional differences in the field.

Appendices

A Dictator game

The dictator game consists of ten binary choices (Kerschbamer, 2015), participants need to choose between the binary choices that contain different hypothetical payoffs for themselves and someone else.[11]

The decisions of the dictator game are transformed into a score vector, (x, y). The generated x-score and y-score are indicators of social preferences, and will be used as control variables in following analysis.

The ten choices are shown in Table A1. The choices in the upper part describe one's distributional preferences in a disadvantageous situation. The earlier one switches to the option LEFT, the more altruistic she is, and the higher x-score will be. The decisions will be transformed into a x-score, as seen in Table A2. Likewise, the choices of the lower part in Table A1 describe one's distributional preferences in an advantageous situation. The decisions of the lower part will be transformed into a y-score, as in Table A2. The earlier one switches to the option LEFT, the less selfless she is, and the lower the y-score will be. The participants who exhibit inconsistent choices in the dictator game will be assigned x-score and y-score with zero.[12]

Table A3 shows the joint distribution of x-score and y-score in density among the consistent decision makers (95.4%). It can be seen that most decision makers have negative x-scores and positive y-scores, or both scores close to zero. Figure A1 shows the joint distribution of the two scores with a regression line and weighting markers. According to Kerschbamer (2015), such a geographical display of the scores shows that most decision makers show social preferences of inequality aversion and selfishness. As seen from the downward regression line in Figure A1, the two scores are negatively correlated with each other (pairwise correlation coefficient -0.15, $p < 0.01$). This joint distribution is also similar to what was observed by Kerschbamer (2015) and Kerschbamer and Muller (2017).

[11]As a method of eliciting social preferences, this dictator game does not discriminate between arbitrary set of preference types. It also does not impose any assumptions of structural formulation of preferences, or require participants of much time and efforts to elicit social preferences.

[12]The aim of assigning a value to x- and y-scores is to keep the inconsistent participants in the sample. How much the values are does not matter in later analysis as the consistency will be controlled by a dummy.

Table A1: Dictator game

X-list:

Dec. Nr.	LEFT		Your Choice		RIGHT	
	you receive	other person receive			you receive	other person receive
1	8 dollars	13 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
2	9 dollars	13 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
3	10 dollars	13 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
4	11 dollars	13 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
5	12 dollars	13 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars

Y-list:

Dec. Nr.	LEFT		Your Choice		RIGHT	
	you receive	other person receive			you receive	other person receive
6	8 dollars	7 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
7	9 dollars	7 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
8	10 dollars	7 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
9	11 dollars	7 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars
10	12 dollars	7 dollars	LEFT ◯	RIGHT ◯	10 dollars	10 dollars

Table A2: Determination of (x, y)-score

subject chooses LEFT for the 1st time in row	in X-list (x-score)	in Y-list (y-score)
1	2.5	-2.5
2	1.5	-1.5
3	0.5	-0.5
4	-0.5	0.5
5	-1.5	1.5
never	-2.5	2.5

Table A3: Joint distribution of x-score and y-score (%)

y-score	x-score					
	-2.5	-1.5	-0.5	0.5	1.5	2.5
2.5	20.67	1.11	3.84	13.74	2.10	0.87
1.5	0.99	0.50	0.74	2.72	0.37	0.00
0.5	2.48	0.50	14.73	23.14	0.37	0.00
-0.5	1.24	0.50	2.60	2.85	0.00	0.00
-1.5	0.25	0.00	0.00	0.00	0.00	0.00
-2.5	2.35	0.37	0.25	0.50	0.00	0.25

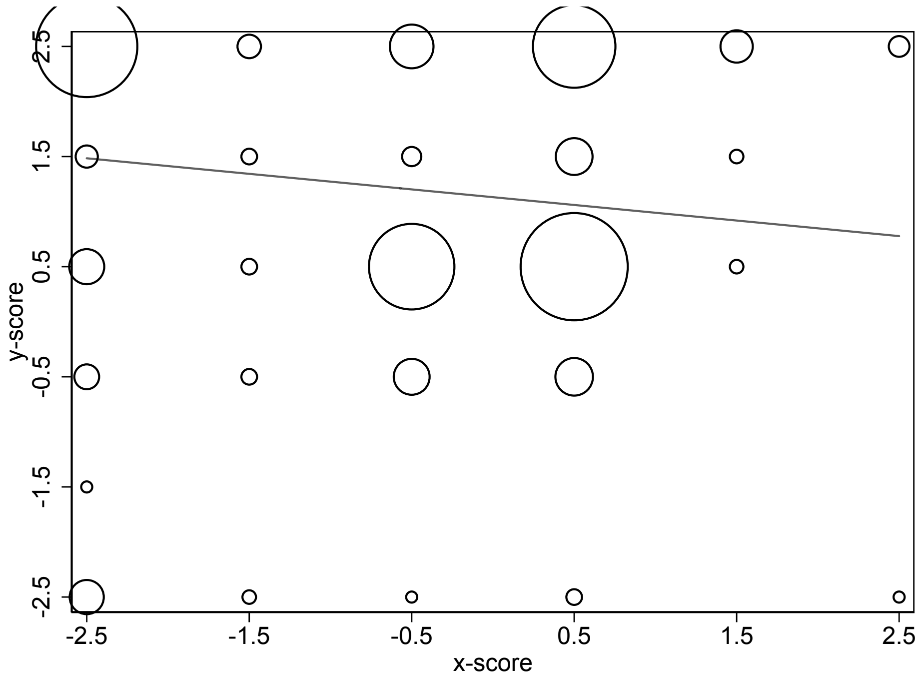


Figure A1: Distributions of x-score and y-score

Notes: This figure shows the joint distribution of x-score and y-score with the regression line and the weighting markers. Both scores range from -2.5 to 2.5.

B Instructions

B.1 Consent form

Introduction

Welcome to this research project! We appreciate your participation very much.

This is a study about decision-making. Norwegian School of Economics has provided funds for this project.

Procedures

This survey consists of several parts. You will be given instructions on screen before every single part of the survey. Please note that there is no way back to revise your answers, so always make sure to read the instructions and consider your decisions carefully before you continue.

Benefits

Your payment will consist of the participation fee and possibly a bonus. Every participant will get a participation fee of $1 for completing the HIT. The bonus $0-$7 [The bonus $0-$10.5 in the pilot study] depends on the tokens you accumulate throughout the HIT. The exact amount of tokens that you receive will depend on your and/or others' decisions. In the end, each token is converted into US dollars, with 2 cents per token [3 cents per token in pilot study].

Payment

At the end of this survey, you will be given a participation code. You will need to copy this code to the survey code field on the AMT web page that directed you here at the beginning. If you earn a bonus, it will be paid to you using the bonus system within a few days after the completion of this HIT. Your payment for taking the HIT will be sent to you shortly after the completion of this HIT.

Confidentiality

All data collected from this survey will be used only for research purpose. The worker IDs will never be published. Only the primary investigator, who is in charge of payment, will have access to the worker IDs.

Questions about the Research

Should you have questions regarding this survey, please contact thechoicelab@nhh.no.

I have read, understood and agreed with the above consent form. I want to participate this study.

◯ Yes

◯ No

B.2 Assign worker ID

Welcome!

Please note that your participation will be registered on the following Amazon Mechanical Turk worker ID:

The worker ID was retrieved automatically when you clicked on the link that brought you here. This step is necessary for assigning payments to the right account and to ensure that you participate in this study only once.

B.3 Decision for self

Now you are rewarded 100 tokens for finishing the questionnaire. You are asked to make a decision with the rewarded 100 tokens.

You need to decide how many tokens, from 0 up to 100 tokens, you would like to invest in the following game: With a chance of 1/3 (33%) you will make two and a half times the amount you invest; with a chance of 2/3 (67%) you will lose the amount you invest.

Your bonus will be total amount of tokens earned from the game and tokens that are not invested.

Please click the continue button and make your decision.

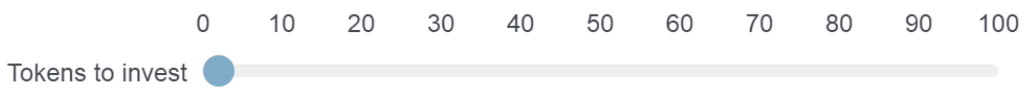
Recall the game:

You are asked to make a decision with the rewarded 100 tokens.

You need to decide how many tokens, from 0 up to 100 tokens, you would like to invest in the following game: With a chance of 1/3 (33%) you will make two and a half times the amount you invested; with a chance of 2/3 (67%) you will lose the amount you invested.

Your bonus will be total amount of tokens earned from the game and tokens that are not invested.

Please move the slider to the amount you would like to invest.



B.4 Voluntary treatment

Now you are anonymously matched with another participant in this study. You need to make a choice between two alternatives. Your decision would influence the bonus of the participant matched with you, and s/he knows that his/her bonus would depend on another participant's decision.

Please click the continue button and make your choice.

Recall the game:

You need to decide how many tokens, from 0 up to 100 tokens, you would like to invest in the following game: With a chance of 1/3 (33%) two and a half times the amount invested; with a chance of 2/3 (67%) lose the amount invested.

The bonus will be total amount of tokens earned from the game and tokens that are not invested.

Please consider carefully and choose between alternatives A and B:

A: The participant matched with you will be rewarded 100 tokens for finishing the questionnaire. On behalf of this participant, you need to decide how many tokens to invest in the previous game. The bonus of this participant would depend on your decision for him/her. Your own bonus will still depend on your decision for yourself.

B: The participant matched with you will not be rewarded anything for finishing the questionnaire. The bonus of this participant will be zero. Instead, your own bonus will be doubled (i.e. the total amount of tokens you earn from the game and tokens not invested will be doubled).

◯ A

◯ B

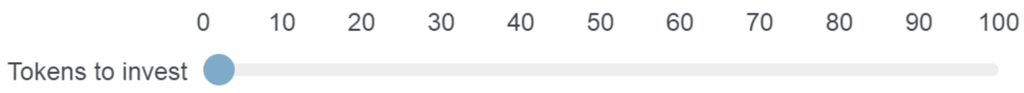
(The page if the subject chooses A)

Recall the game:

You need to decide how many tokens, from 0 up to 100 tokens, you would like to invest in the following game: With a chance of 1/3 (33%) two and a half times the amount invested; with a chance of 2/3 (67%) lose the amount invested.

The bonus will be total amount of tokens earned from the game and tokens that are not invested.

Please move the slider to the amount of tokens you would like to invest on behalf of the participant matched with you.



(The page if the subject chooses B)

This is the last question that may affect payments for you and others. Now you are **matched with another participant**, different from the one in the previous question. This new participant is rewarded 100 tokens for finishing the questionnaire and knows that his/her bonus would depend on another participants' decision.

On behalf of this participant, you need to decide how many tokens to invest in the previous game. The bonus of this participant would depend on your decision for him/her. Your own bonus will not be affected.

Please click the continue button and make the decision.

Recall the game:

You need to decide how many tokens, from 0 up to 100 tokens, you would like to invest in the following game: With a chance of 1/3 (33%) two and a half times the amount invested; with a chance of 2/3 (67%) lose the amount invested.

The bonus will be total amount of tokens earned from the game and tokens that are not invested.

Please move the slider to the amount of tokens you would like to invest on behalf of **the participant newly matched with you**.

B.5 Non-voluntary treatment

This is the last question that may affect payments. Now you are anonymously matched with another participant in this study. S/he is rewarded 100 tokens for finishing the questionnaire and knows that his/her bonus would depend on another participant's decision. On behalf of this participant, you need to decide how many tokens to invest in the previous game.

The bonus of this participant would depend on your decision for him/her. Your own bonus will not be affected.

Please click the continue button and make the decision.

Recall the game:

You need to decide how many tokens, from 0 up to 100 tokens, you would like to invest in the following game: With a chance of 1/3 (33%) two and a half times the amount invested; with a chance of 2/3 (67%) lose the amount invested.

The bonus will be total amount of tokens earned from the game and tokens that are not invested.

Please move the slider to the amount of tokens you would like to invest on behalf of the participant matched with you.

B.6 Questionnaire

Thank you for finishing the above questions. Now we would like you to fill in the form before the end of this HIT.

How old are you?

What is your gender?

◯ Male

◯ Female

What is the highest level of education you have completed?

◯ Less than High School

◯ High School / GED

◯ Some College

◯ 2-year College Degree

◯ 4-year College Degree

◯ Masters Degree

◯ Doctoral Degree

◯ Professional Degree (JD, MD)

Which political party would you vote for if there was an election tomorrow?

◯ Republican

◯ Democratic

◯ Other

C Decompose the overall difference and identify the selection effect and selection effect on other-self difference

For the overall difference, we apply the law of total probability on $E[Y^{NV}]$ and re-arrange $E[Y^V|V=1]$, we get

$$\begin{aligned}
\textit{Overall difference} &= E[Y^V|V=1] - E[Y^{NV}] \\
&= \underbrace{\Big(E[Y^V|V=1]\cdot P(V) + E[Y^V|V=1]\cdot(1-P(V))\Big)}_{E[Y^V|V=1]} - \\
&\quad \underbrace{\Big(E[Y^{NV}|V=1]\cdot P(V) + E[Y^{NV}|V=0]\cdot(1-P(V))\Big)}_{E[Y^{NV}]}.
\end{aligned}$$

Combine and re-arrange, get

$$\begin{aligned}
\textit{Overall difference} &= \Big(E[Y^V|V=1] - E[Y^{NV}|V=1]\Big)\cdot P(V) + \\
&\quad \Big(E[Y^V|V=1] - E[Y^{NV}|V=0]\Big)\cdot\Big(1-P(V)\Big) \\
&= \Big(E[Y^V|V=1] - E[Y^{NV}|V=1]\Big)\cdot P(V) + \\
&\quad \Big(E[Y^V|V=1] - E[Y^{NV}|V=1] + E[Y^{NV}|V=1] - \\
&\quad E[Y^{NV}|V=0]\Big)\cdot\Big(1-P(V)\Big) \\
&= \Big(E[Y^V|V=1] - E[Y^{NV}|V=1]\Big) + \\
&\quad \Big(E[Y^{NV}|V=1] - E[Y^{NV}|V=0]\Big)\cdot\Big(1-P(V)\Big).
\end{aligned}$$

Thus, the overall difference is decomposed to two components. One component represents the institutional effect $\Big(E[Y^{NV}|V=1] - E[Y^{NV}|V=0]\Big)$ and the other component represents the selection effect $\Big(E[Y^{NV}|V=1] - E[Y^{NV}|V=0]\Big)$ multiplied by the share of decision makers who are unwilling to volunteer.

For the selection effect, we start by decomposing the decisions in *Non-voluntary* treatment,

$$E[Y^{NV}] = E[Y^{NV}|V=1]\cdot P(V) + E[Y^{NV}|V=0]\cdot(1-P(V)).$$

Then, subtract the decomposed $E[Y^{NV}]$ from the decisions of the non-volunteer decision makers

$$E[Y^{NV}] - E[Y^{NV}|V=0] = (E[Y^{NV}|V=1] - E[Y^{NV}|V=0])\cdot P(V).$$

If $P(V) \neq 0$, then

$$E[Y^{NV}|V=1] - E[Y^{NV}|V=0] \; = \; \frac{E[Y^{NV}] \; - \; E[Y^{NV}|V=0]}{P(V)}.$$

For the selection effect on other-self difference in decisions, we start by decomposing the other-self difference in *Non-voluntary* treatment,

$$E[(Y^{NV} - Y^{S})] \; = \; E[(Y^{NV} - Y^{S})|V=1] \cdot P(V) \; + \; E[(Y^{NV} - Y^{S})|V=0] \cdot (1 - P(V)),$$

then, subtract the decomposed expression from the other-self difference of the non-volunteers,

$$E[(Y^{NV} - Y^{S})] \; - \; E[(Y^{NV} - Y^{S})|V=0] \; = \; (E[(Y^{NV} - Y^{S})|V=1] - E[(Y^{NV} - Y^{S})|V=0]) \cdot P(V).$$

If $P(V) \neq 0$, then

$$E[(Y^{NV} - Y^{S})|V=1] - E[(Y^{NV} - Y^{S})|V=0] \; = \; \frac{E[(Y^{NV} - Y^{S})] \; - \; E[(Y^{NV} - Y^{S})|V=0]}{P(V)}.$$

D The difference between $\frac{1}{E[V]}$ in the estimates and $E[\frac{1}{V}]$ in the effects

The expected share of decision makers who are willing to volunteer is the number of volunteers divided by the total number of the decision makers. So $E[V]$ can be written as

$$
\begin{aligned}
E[V] &= E[\frac{\overline{X}}{N}] \\
&= \frac{E[\overline{X}]}{N}.
\end{aligned}
$$

X is the number of volunteers and N is the number of the decision makers in *Voluntary* treatment. Suppose that in a population of N decision makers, each would choose to volunteer with probability p, X follows a Binomial distribution $B(N;p)$. Since we assume that the proportion of volunteering is not zero, the number of volunteers, X, would be at least one. So comparing $\frac{1}{E[V]}$ to $E[\frac{1}{V}]$ is equivalent to comparing $\frac{1}{E[\overline{X}|X\geq 1]}$ to $E[\frac{1}{\overline{X}}|X \geq 1]$, since the number of the decision makers in *Voluntary* treatment is a constant, N.

First, we consider $\frac{1}{E[\overline{X}|X\geq 1]}$ and compute the denominator $E[\overline{X}|X \geq 1]$. We expand $E[\overline{X}]$ as,

$$
E[\overline{X}] \;=\; 0\cdot(1-p)^N \;+\; E[\overline{X}|X \geq 1]\cdot\underbrace{(1-(1-p)^N)}_{Prob(X\geq 1)}.
$$

Since $X \sim B(N;p)$ and $(1-p)^N \simeq 0$ when N is large, $E[\overline{X}] = Np$ and the right hand side equals to $E[\overline{X}|X \geq 1]$. We get

$$
\frac{1}{E[\overline{X}|X \geq 1]} \;=\; \frac{1}{Np}. \tag{I.1}
$$

Second, we consider $E[\frac{1}{X}|X \geq 1]$. The probability of each value of $\frac{1}{X}$ should be same as the probability of the given value of X, since $X \sim B(N;p)$, we have

$$
E[\frac{1}{\overline{X}}|X \geq 1] \;=\; \sum_{k=1}^{N}\frac{1}{k}\binom{N}{k}p^k(1-p)^{N-k}\Big/(1-(1-p)^N). \tag{I.2}
$$

Given the number of the decision makers in *Voluntary* treatment, $N = 564$, and the observed proportion of volunteering, $p = 0.475$, we plug the values into Equations I.1 and I.2. The value of $\frac{1}{E[\overline{X}|X\geq 1]}$ is around 0.0037327 and the value of $E[\frac{1}{\overline{X}}|X \geq 1]$ is around 0.0037397. The difference between the two is very small; only 0.19% of the component in the effect, $E[\frac{1}{\overline{X}}|X \geq 1]$. Thus, we conclude that in the present experiment, this difference is negligible. Moreover, the difference would be smaller when the sample size gets larger and the estimates of the effects are consistent.

E Regression of other-self differences of decisions

Table E1: Other-self difference of risk taking

Covariate	Dependent variable: Other-self difference
Vol treatment	4.59***
	(1.51)
x-score	-0.37
	(0.56)
y-score	-1.92***
	(0.67)
age	-0.08
	(0.06)
female	-2.22
	(1.52)
high education	0.50
	(1.51)
consistency	5.63**
	(2.64)
Republican	-0.34
	(1.83)
pilot	-0.69
	(1.90)
Constant	-1.19
	(3.41)
r2	0.03
N	847

Standard errors are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F Propensity score matching

The propensity score matching method, as the estimation method in 3.3, was registered in the pre-analysis plan (Xu, 2017). When considering the propensity score matching in the pre-analysis plan, I assumed that the volunteering decision relies on the decisions in the Kerschbamer dictator game and computed the power accordingly. Specifically, decision makers were matched based on propensity scores so to identify the decision makers in *Non-voluntary* treatment who are willing to volunteer. In fact, the predictive power of the explanatory variables is quite weak. The estimation results show that the effects are qualitatively similar between the two estimation methods and both results are significant. But the results from propensity score matching are quantitatively smaller.

F.1 Estimation using propensity score matching

An alternative method to estimate the effects is based on the propensity score matching (Angrist and Pischke, 2009; Morgan and Winship, 2007). In the method of propensity score matching, I identify the decision makers in *Non-voluntary* treatment who are willing to volunteer. Thereby, a comparison group of decision makers is selected from *Non-voluntary* treatment. The effects can be estimated by comparing the selected comparison group to the volunteers and to the opt-out decision makers. The difference between the two estimation methods is how to approach the decisions made by the decision makers willing to volunteer in *Non-voluntary* treatment. In the method of Section 3.3, the expected probability of volunteering is estimated and used as a component in estimating the effects. In the propensity score matching method, the expected probability of volunteering is individually estimated, and such estimates of probability of volunteering are used to select a comparison group, instead of being a component of the effect estimates.

Based on the methods introduced by Imbens and Rubin (2015)[13], I conduct the propensity score matching as follows. First, I regress the self-selecting decision in *Voluntary* treatment on risk taking decisions on behalf of oneself, decisions in the dictator game (see Appendix A), age, gender, education, political tendency, etc. Second, a propensity score is estimated for each of all the decision makers based on the model. The higher the propensity score is, the more likely the subject is to volunteer to contribute and be a decision maker. Last, each of the volunteers in *Voluntary* treatment is matched with one decision maker in *Non-voluntary* treatment based on the principle of nearest neighbor with replacement.[14] A voluntary decision maker in *Voluntary* treatment is matched with one decision maker in *Non-voluntary* treatment whose propensity score is closest to hers.

Decisions of decision makers who are identified as willing to self-select, may provide an

[13] In particular, I focus on Chapters 12, 13, and 14 to apply the propensity score matching method.

[14] The matching allows replacement so that several decision makers in *Non-voluntary* treatment can be matched with the same volunteer in *Voluntary* treatment.

approach to the expected decision of decision makers in *Non-voluntary* treatment who prefer self-selecting to opting-out ($E[Y^{NV}|V=1]$). I compare decisions of these identified decision makers to those of volunteers and non-volunteers. Details of the propensity score matching can be found in Appendix F.2.

The estimates in Table F1 confirm the robustness of the results. The estimates of effects all hold the same directions and similar magnitudes with the propensity matching method. The estimate of selection effect is smaller and less pronounced than the identified estimate. This, in fact, bolsters the large positive overall difference of risk taking, with the selection effect as a negative component and the institutional a positive component (see Eq.1.1). Because the selection effect is smaller and less significant than identified, we observe a relatively stronger role of the institutional effect and a large positive overall difference between the two institutions.

Table F1: Estimates from propensity matching

	Estimates (Bootstrap s.e.)
Institutional effect	6.03**
	(3.03)
Selection effect	-6.34*
	(3.46)
Selection effect on other-self difference	-11.07***
	(2.33)

Notes: All the effects are in tokens. Institutional effect indicates the difference between the tokens invested by volunteer decision makers and by matched decision makers. Selection effect indicates the difference between the tokens invested by the decision makers in *Non-voluntary* treatment who are willing to volunteer and the non-volunteers in *Voluntary* treatment. Selection effect on other-self difference indicates the difference of other-self difference between the decision makers in *Non-voluntary* treatment who are willing to volunteer and the non-volunteers in *Voluntary* treatment. The bootstrap includes 800 replications and the standard errors are estimated from the replications without dropping observations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses.

F.2 Procedure for propensity score matching

In order to identify subjects in *Non-voluntary* treatment who would self-select as decision makers, the choice between volunteer and opt-out in *Voluntary* treatment are regressed on decisions on own behalf, x-score, y-score, and other controls in a Logit model. The selection of covariates into the specification follow the method proposed by Imbens and Rubin (2015). Details of how to select covariates can be found in Appendix F.3. The rationale of selecting the covariates into the specification is to obtain balance of important covariates between treatments. This ensures that important covariates for explaining the probability of self-selecting are included in the model. As shown in Table F2, decisions for oneself, y-score, age, gender, consistency in dictator game [15], squared y-score, interaction of gender and x-score, and interaction of consistency and age, and squared decision for self, have significant effects in explaining the probability of volunteering. Y-score has very large positive effects on choosing to volunteer. Propensity scores are computed from the estimated model. Last, each subject in *Non-voluntary* treatment is matched with a subject in *Voluntary* treatment. The matching procedure follows the principle of nearest neighbor. The results of matching can be seen in Table F3. 137 subjects in *Non-voluntary* treatment are identified as willing to volunteer. The remaining 146 subjects in *Non-voluntary* treatment do not show large enough propensity scores, and are not identified as willing to volunteer. Decisions of these matched subjects in *Non-voluntary* treatment who prefer volunteering to opting out are compared to those of self-selected and opt-out subjects to test the hypotheses. This procedure is replicated in Bootstrap to derive standard errors of the estimate.

[15] Consistency in the dictator game has large effects on volunteering decision, because 19 out of 29 inconsistent decision makers choose to give to others.

Table F2: Marginal effects on choosing to volunteer

	Logit
Decision for self	0.006***
	(0.002)
x-score	0.016
	(0.019)
y-score	-0.128**
	(0.055)
age	-0.016**
	(0.008)
female	0.217***
	(0.067)
high education	0.046
	(0.038)
consistency in dictator game	-0.968***
	(0.271)
y score × y score	0.038***
	(0.008)
female × x score	-0.059**
	(0.030)
consistency × age	0.019**
	(0.009)
deci_self^2	-0.000**
	(0.000)
N	564

Notes: The dependent variable is the binary variable for volunteering decision. It equals one if a decision maker chooses to make the decision for the recipient, and equals zero if the decision maker chooses to double his own payoff. The independent variables are decision for the decision maker herself, x score and y score from the dictator game (see Appendix A), age, dummy for female, dummy for high education, dummy for consistency in dictator game, interaction between female and x score, interaction between consistency and age, and squared decisions on behalf of oneself. The selection procedure of the covariates in the specification of the model can found in Appendix F.3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses.

Table F3: Descriptives after matching (s.d. in parentheses)

	Volunteers	Non-volunteers	Comparison group in Non-vol	Remaining in Non-vol
# Obs	268	296	137	146
Decisions for self	44.21 (31.62)	34.17 (33.05)	38.90 (31.21)	33.78 (32.47)
Decisions for others	41.66 (29.25)	41.96 (34.18)	35.63 (27.44)	33.88 (30.76)
x score	-0.75 (1.40)	-0.38 (1.19)	-0.65 (1.42)	-0.42 (1.26)
y score	1.64 (1.24)	0.73 (1.22)	1.52 (1.26)	0.88 (1.12)
Age	36.99 (11.95)	35.05 (11.00)	37.19 (11.94)	34.30 (9.93)
Female	0.52 (0.50)	0.39 (0.49)	0.47 (0.50)	0.41 (0.49)
High education	0.61 (0.49)	0.61 (0.49)	0.67 (0.47)	0.63 (0.48)
Prefer Republican	0.25 (0.43)	0.27 (0.44)	0.24 (0.43)	0.27 (0.45)

F.3 Selecting the covariates into the Logit model

In this study, nine covariates are candidates of the linear predictors for the volunteering decision. They are decision for self, x-score, y-score, age, dummy for female, dummy for high education, dummy for support for Republican, dummy for consistency in dictator game, and dummy for whether a decision maker is in the pilot study or not, see Table F4. In addition, four quadratic covariates and thirteen interaction covariates, are among the candidates of the predictors of the volunteering decision. The quadratic covariates include squared decision for self, squared age, squared x-score, and squared y-score. The squared terms of other covariates do not enter the quadratic covariates, because those are all binary covariates and squared terms are the same as the original values. The other thirteen interaction covariates are either combinations of a dummy covariate and a continuous covariate, or two dummy covariates, see Table F5.

The procedure of selecting the covariates follows the method introduced by Imbens and Rubin (2015). The point of the method is to find a suitable specification for the propensity score, that would obtain balance on the important covariates. The procedure has three parts. First, basic covariates are chosen on substantive grounds. These basic covariate are *a priori* viewed as important for explaining the volunteering decision and plausibly related to the measures for other-regarding decisions. Second, some of the remaining covariates are selected into the specification of the Logit model. One of the remaining covariates, each at a time, is added into the model, together with the basic covariates. A likelihood ratio statistic is calculated to assess the null hypothesis that the newly included covariate has a zero coefficient. The pre-set threshold for the test statistic is one (Imbens and Rubin, 2015). If all the likelihood ratio test statistics are less than one, the selection procedure ends. If at least one of the likelihood ratio test statistics is larger than one, the covariate with the largest likelihood ratio statistic is added into the model. So the covariate that increases the likelihood function sufficiently largely, will be selected into the model. The process continues until all likelihood ratio statistics are less than one. Third, some of the quadratic and interaction covariates are selected into the specification of the Logit model. Essentially the same as the process in the second part, one of the high-order covariates, each at a time, is added into the model, together with the basic covariates and the selected linear covariates that are selected in the second step. A likelihood statistic is calculated to assess the null hypothesis that the newly included covariate has a zero coefficient. The pre-set threshold for the test statistic is 2.71 (Imbens and Rubin, 2015). If all the likelihood ratio test statistics are less than 2.71, the selection procedure ends. If at least one of the likelihood ratio test statistics is larger than 2.71, the covariate with the largest likelihood ratio statistic is added into the Logit model. The process continues until all likelihood ratio statistics are less than 2.71.

In the first part, three basic covariates are selected because they are believed to have large impacts on the volunteering decision. The three covariates are decision for self, x-score denoting pro-sociality in unfavorable inequality, and y-score denoting pro-sociality in favorable inequality. In the second and third parts, at each step of the process, the covariate with the largest t-test statistic (also larger than threshold values) is selected into the Logit model, as shown in Table F4 and Table F5. Final specification of the Logit model includes decision for self, x-score, y-score, age, female, dummy for high education, dummy for consistency in the dictator game, squared y-score, interaction between consistency and age, interaction between female and x-score, and squared decision for self.

Table F4: Likelihood ratio statistics for sequential selection of covariates to enter linearly

Covariate	Step				
	1	2	3	4	5
age	2.06	3.23	**2.22**		
female	7.42	**8.20**			
high education	0.07	0.83	1.21	**1.09**	
Republican	0.05	0.02	0.06	0.02	0.02
consistency	**16.72**				
pilot	1.35	0.78	0.82	0.76	0.69

Notes: Boldface numbers in each column are the largest likelihood ratio test statistic at each step. In the Logit model for volunteering decision, the base covariates include decisions for self, x-score, and y-score. The remaining covariates include age, dummy for female, dummy for high education, dummy for support for Republican, dummy for consistency in the dictator game, and dummy for whether in the pilot study. At each step, one of the remaining covariates is added into the Logit model and a likelihood ratio test is conducted to check whether the coefficient of the newly added covariate is zero or not. The pre-set threshold for the test statistic is one. If all likelihood ratio statistics are less than one, the selection procedure ends, and no covariate is added. If at least one of the likelihood ratio statistics is greater than one, the covariates with the largest likelihood ratio test statistic is added into the model.

Table F5: Likelihood ratio statistics for sequential selection of quadratic and interaction covariates

Covariate	Step				
	1	2	3	4	5
decision for self × decision for self	4.11	3.91	4.09	**4.18**	
age × age	0.00	0.01	0.17	0.11	0.13
x score × x score	2.09	0.01	0.03	0.06	0.08
y score × y score	**19.19**				
female × decision for self	1.60	1.91	2.47	2.80	2.33
female × x score	4.93	4.59	**4.10**		
female × y score	0.00	0.01	0.01	0.06	0.13
female × age	0.15	0.08	0.02	0.12	0.09
female × high education	0.23	0.33	0.32	0.15	0.14
female × consistency	0.48	0.51	0.07	0.21	0.36
high education × decision for self	0.18	0.60	0.72	0.67	0.59
high education × xscore	0.06	0.15	0.24	0.28	0.17
high education × y score	1.84	0.95	0.65	0.55	0.50
high education × age	0.07	0.45	0.85	0.70	0.37
high education × consistency	0.05	0.05	1.02	0.88	0.70
consistency × decision for self	1.08	1.13	1.90	1.78	1.62
consistency × age	6.21	**6.00**			

Notes: Boldface numbers in each column are the largest likelihood ratio test statistic at each step. In the Logit model for volunteering decision, the linear covariates include decisions for self, x-score, y-score, age, dummy for female, dummy for high education, and dummy for consistency in the dictator game. The candidate quadratic and interaction covariates include squared decisions for self, squared age, squared x score, squared y score, and the interaction terms combined with a dummy covariate and a continuous covariate, and two dummy covariates. At each step, one of the remaining quadratic and interaction covariates, is added into the Logit model with all the linear covariates that are selected. A likelihood ratio test is conducted to check whether the coefficient of the newly added covariate is zero or not. The pre-set threshold for the test statistic is 2.71. If all likelihood ratio statistics are less than 2.71, the selection procedure ends. If at least one of the likelihood ratio statistics is greater than 2.71, the covariate with the largest likelihood ratio test statistic is added into the model.

F.4 Assessing the balance conditional on the estimated propensity score

The adequacy of the Logit model specification is checked by exploiting a property of the propensity score. That is, the treatment indicator and the covariates are independent of each other, given the estimated propensity score.[16] Ideally, the sample is first stratified into subsamples or blocks within each of which all observations have the exact same value of estimated propensity score. Then, check whether covariates are independent of treatment indicator. But, the first step is feasible only if the estimated propensity score takes on a relatively small number of values. In practice, the sample is split into blocks within each of which the estimated propensity score varies very little. Within the resulting blocks, the independence of the treatment indicator and the covariates is assessed. The iterative process of constructing blocks starts from the whole sample, and a t-test is conducted to check if the the linearized propensity score[17] is independent of the treatment indicator. Within each block, if the t-statistic is in absolute value less than one, then the linearized propensity score is uncorrelated with the treatment indicator. Otherwise, if the t-statistic is absolutely larger than one, then the linearized propensity score is not independent of treatment and the block will be split to two new blocks with equal subsample size. [18] The iterative process continues until all t-statistics of all blocks are in absolute values below one. As shown in Table F6, the whole sample is split into two blocks, within each of which the linearized propensity score is uncorrelated with the treatment indicator.[19]

To assess the balance of covariates, there are three sets of tests (Imbens and Rubin, 2015). First, a test for each covariate in the model specification based on the average of the within-block average differences by treatment indicator. Specifically, for each covariate and each block, the within-block difference and the within-block sampling variance are computed. Then, the average difference is the weighted within-block difference, and the sampling variance is the weighted within-block variances. The weights are the block sizes relative to the total sample size. Covert the average difference and the sampling variance into a z-value. If the z-values are not substantially larger in absolute values than one, then there is satisfactory balance in the covariates. This suggests that the specification of the propensity score is adequate. The z-values are shown in the third column of Table F7. None of the z-values is absolutely much larger than one, indicating excellent balance. Second, for each covariate and each block, the

[16] The estimated propensity score substitutes the super-population propensity score, to investigate the property.

[17] The linearized propensity score is $\hat{l}(x) = ln(\frac{\hat{e}(x)}{1-\hat{e}(x)})$ where $\hat{e}(x)$ is estimated propensity score. The reason to use linearized propensity score is that, compared to the propensity score, the linearized propensity score is more likely to have a distribution that is well approximated by a normal distribution Imbens and Rubin (2015).

[18] The block is split into two new blocks based on the median of the linearized propensity score. The observations with a linearized propensity score less than the median will become a new block, and the other half observations with a linearized propensity score larger than median will be another new block.

[19] There are three observations that are trimmed from the whole sample (847), to ensure some overlap between the treatment groups. The observations in *Non-voluntary treatment* with propensity scores that are less than the smallest propensity score among *Voluntary treatment*, are dropped. The observations in *Voluntary treatment* with propensity scores that are greater than the largest propensity score among *Non-voluntary treatment*, are dropped.

covariate is regressed on the block dummy and the interaction of block dummy and treatment indicator. An F-test is conducted on the coefficients on the interaction terms. Large positive test statistics suggest that the covariates are not balanced within the block, so the specification of the propensity score is inadequate. The test statistics are shown in the fourth column of Table F7. The small values suggest that the difference in average covariate values is zero in each block. Third, for each covariate and each block, the difference between treatments is checked by a t-test. A t-statistic with smaller absolute value than conventional critical value, suggests that the covariate is well balanced within the block. In the first and second columns of Table F7, the t-statistic with the largest absolute value is 1.91, which is smaller than conventional critical value of t-test (1.96). This, again, suggests that the covariates are well balanced within each block. Therefore, the assessments show that the overall balance of covariates for the specification of the Logit model is satisfactory, and the model specification is adequate.

Table F6: Determination of the number of blocks and their boundaries

Step	Block	Lower bound	Upper bound	Width	# Non-vol treatment	# Vol treatment	t-Statistic
1	1	0.08	0.90	0.82	283	561	1.05
2	1	0.08	0.42	0.34	150	272	0.121
	2	0.43	0.90	0.47	133	289	-0.015

Notes: At each step, within each block the t-test is conducted to check whether the linearized propensity score varies between the two treatments or not. The t-statistic is shown in the last column in the table. The lower and upper bounds, and the interval between the lower and upper bound are listed in the second, third, and fourth column, respectively. The numbers of observations in *Non-voluntary* treatment and *Voluntary* treatment are in the sixth and seventh columns. At the first step, the whole sample is taken as one single block. The t-statistic of first step is in absolute value larger than one. Continue to the second step, this block is split into two new blocks with equal size. At the second step, the t-statistic is in absolute value less than one in both blocks. The block construction ends here. There are two blocks, within each of which the linearized propensity score is independent of the treatment indicator.

Table F7: z-Values for balancing tests: Specification of propensity score

	Within blocks		Overall		1-Block
	1	2	t-Test	F-test (z-Value)	t-Test
Covariate					
decision for self	0.48	0.79	0.89	0.24	−1.07
x score	−0.81	0.54	0.02	0.40	0.35
y score	−0.89	−0.49	−0.93	0.61	−0.18
age	−0.61	0.56	0.04	0.31	−0.17
female	−0.36	0.38	0.03	0.12	−0.16
high education	0.07	−1.50	−1.04	0.00	1.05
consistency	−1.29	−0.50	−0.84	0.28	1.07
y score × y score	0.74	−0.06	0.36	0.34	−1.19
consistency × age	−1.17	0.27	−0.41	0.84	0.40
female × x score	−1.02	1.62	0.96	0.47	−0.58
decision for self × decision for self	0.50	0.75	0.89	0.25	−1.00

Notes: The rows correspond to the eleven covariates. The first two columns show the t-statistics for each block and each covariate. The third and fourth columns are for the overall tests, and the third one for the z-value of the test of equality of (unadjusted) average covariate values for the two treatments, and the fourth one for the test of the block-adjusted average covariate values for the two treatments. The last column, for comparison purpose, presents the t-statistics for the null hypothesis that the overall covariate values are equal in the two treatments, not adjusted for the blocks.

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III Risk taking on behalf of others: Does the timing of uncertainty revelation matter?

Risk taking on behalf of others: Does the timing of uncertainty revelation matter?

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Abstract

In a large online probability based panel of the general population of Norway, we examine the effect that varying delays in uncertainty revelation have on risk taking on behalf of others. We find a precisely estimated null effect of revelation delay on the average proportion that chose a lottery over a safe alternative. Estimating a hierarchical Bayes model of rank dependent utility, we find some differences in how decisions are made, the median participant does become more risk seeking with long delays, but this effect is offset by differences in the role of heterogeneities within treatment.

JEL Classification Numbers: C91, D63.

1 Introduction

How do people behave when making risky decisions on behalf of others? Examples of risk-taking for others can be found in many contexts: politicians take risky decisions on behalf of their voters, managers on behalf of their employees and shareholders, parents on behalf of their children, and physicians on behalf of their patients. Due to its practical relevance and the extensive literature on individual risk preferences, risk-taking on behalf of others is receiving increasing attention in economics.[1] One commonality of most experimental studies is that the outcomes of decisions made on behalf of others are instantaneously resolved and revealed to decision makers (Andersson, Holm, Tyran, and Wengström, 2016; Bolton, Ockenfels, and Stauf, 2015; Füllbrunn and Luhan, 2015; Reynolds, Joseph, and Sherwood, 2009). However, instantaneous revelation often does not apply to real-life decisions like those mentioned above, and it excludes a class of decision situations characterized by a delay before the decision outcomes are resolved and the outcomes are revealed to decision makers. Moreover, some decision makers never learn the outcomes of their decision making for others, either because of a long temporal distance to future generations (such as with climate policies) or because it is hard to sort out the causal effects (such as with charitable donations).

We report from a novel study of the effect of the timing of uncertainty revelation on risk taking on behalf of others. To our knowledge, we provide the first study of risk taking behavior in situations where the decision maker never learns about how uncertainty is resolved. This

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[1]See, for example, Eriksen, Kvaløy, and Luzuriaga (2017) and Füllbrunn and Luhan (2015) for overviews.

can only be studied when people take risk on behalf of others. In a large online experiment, we implement four treatments in a between-subjects setting. We vary the delay before the decision-makers, who have no monetary stakes in the decision, learn about the consequences of their decisions for recipients who have worked on a real-effort task. Because we look at decision making for others, we can also look at the effect of decision makers never learning about the outcomes for recipients, and we can compare such decision making to that with immediate and different finite delays of information revelation. The effect of delayed revelation of uncertainty informs us whether situations without ex-post revelation of the outcome constitute a distinct class of decision situations, or whether decisions in these situations are similar to decisions made in situations involving a delay.

For economists thinking about decision making for others, a natural benchmark is to assume that decision makers have preferences for outcomes for others just as they have preferences for outcomes for themselves. However, what such preferences should look like is an open question. Much of mainstream economics is based on the idea that decision makers are fully or incompletely altruistic, and that it is the decision maker's beliefs about the preferences of others that matter (e.g. Becker, 1974). Another possibility is that decision makers apply their own preferences paternalistically to the outcomes of others (Ambuehl, Bernheim, and Ockenfels, 2019). A large meta-analysis of 49 studies about risk taking on behalf of others (Batteux, Ferguson, and Tunney, 2019) found some differences by domain and frame, but small overall differences between decision making for self and decision making for others. However, this does not speak directly to the question of whether own or altruistic preferences are used, since in most experiments with random assignment into the roles of decision maker and recipient, average preferences of decision makers coincide with rational beliefs about the preferences of others. We ask participants directly about what they believe about the preferences of others, this allows us to evaluate if actual risky decisions on behalf of others are more strongly correlated with own risk preferences or with beliefs about the preferences of others.

In decision making for self, experimental studies have documented broad heterogeneity in how decisions are made when time discounting and risk taking is allowed to interact. Epper and Fehr-Duda (2018) review this literature, and document a number of stylized facts which they claim can be accounted for by rank dependent utility (Quiggin, 1982); among these are the finding that risk tolerance seems to increase with delay and that there seems to be intrinsic preference for resolution timing, in particular that many prefer uncertainty to resolve at a later date. Chew and Ho (1994) explain this by "hope," which they say "...is experienced when there is enjoyment in delaying the resolution of uncertainty often involving a potential gain" (p. 268 Chew and Ho, 1994). Similarly, there can be a negative utility of "anxiousness" related to potential losses. There now exists a number of works that build on and extend these insights into psychologically rich models of decision making under risk, both within psychological (Loewenstein, Weber, Hsee, and Welch, 2001) and economic traditions (Caplin and Leahy, 2001; Ely, Frankel, and Kamenica, 2015).

It is an open question whether the findings summarized by Epper and Fehr-Duda (2018) generalize to risk taking for others, and we provide a first step towards filling this gap in knowledge in the present study. Our main finding is that we find a precisely estimated null effect of revelation delay on the average proportion that chose a lottery over a safe alternative. Estimating a hierarchical Bayes model of rank dependent utility, we find some differences in how decisions are made, the median participant does become more risk seeking with long delays, but this effect is offset by differences in the role of heterogeneities within treatment. We also find that the socio-demographic variables we collected have little impact on risk taking. Interestingly, risk taking on behalf of others is more strongly related to own risk preferences than beliefs about the risk preferences of others, indicating a paternalistic tendency among our participants.

In the following Section 2, we proceed by outlining a theoretical framework for interpreting the data we aim to collect, and in Section 3 we summarize our experimental design. In Section 4 we show statistics on the implementation and the sample we collected, before analyzing the choices made and relating our results to our pre-specified hypotheses in Section 5. We make some concluding remarks in Section 6.

2 Theoretical framework

Our aim is to shed light on risk taking on behalf of others with uncertainty revelation at different points in time. The most direct way of doing so is to randomize revelation delay and look at average risk taking using direct behavioral measures such as the proportion of times decision makers chose a risky option. We do so, but we also want to learn more about *how* the decisions are made.

While there have been models proposed that could in principle account for differential timing of uncertainty revelation for decisions made on own behalf (e.g. Caplin and Leahy, 2001), there is not much prior work that can be used to formulate fully empirical models of such theories, and hardly any for our setting in which we also want to allow for decision makers that never learn about the outcomes for recipients. Instead of trying to formulate such an all-encompassing model, which in all likelihood would require both exogenous and endogenous within individual variation in delay, we take an intermediate approach. Our empirical strategy is to exogenously vary the timing of uncertainty revelation and model decision making at these different horizons with a rich but standard model of decision making under uncertainty. Estimating such a model separately for each horizon allows us to consider treatment effects on average choice behavior, but also on estimated model parameters.

Before going into details about the design, we outline the model that we aim to quantify at each horizon. A lottery can be represented by list

$$L = (x_1, p_1; x_2, p_2; \cdots ; x_n, p_n), \quad x_1 < x_2 < \cdots < x_n,$$

in which x_i is a monetary outcome and p_i is the corresponding probability. Assume that a participant has a utility function over money $u(x)$. Earlier results have shown that probability distortion is important both in decision making for others (Vieider, Villegas-Palacio, Martinsson, and Mejía, 2016) and in private decisions over different horizons (Abdellaoui, Diecidue, and Önçüler, 2011), so we want to allow for rank dependent utility (Quiggin, 1982). For the case of a simple lottery with only two outcomes, the rank dependent utility can be formulated as

$$\text{RDU}(L) = (1 - w(p_2))u(x_1) + w(p_2)u(x_2), \tag{1}$$

with w being a non-decreasing weighting function that maps $[0, 1]$ to itself, with $w(0) = 0$ and $w(1) = 1$.

For our implementation, we consider the conventional power utility functions over money,

$$u(x) = x^{\rho}, \quad \rho > 0, \tag{2}$$

and the Prelec (1998) class of probability weighting functions,

$$w(p) = e^{-\beta(-\log p)^{\alpha}}, \quad w(0) = 1, \quad \alpha, \beta > 0. \tag{3}$$

Our aim for the experimental design is now to identify and estimate the vector of parameters (α,β,ρ), separately for each treatment, ideally while allowing for considerable heterogeneity in these parameters also within treatments.

3 Experimental design and implementation

In this section we explain our choice of risk-taking task, and account for how some features of decision making on behalf of others that do not fit straightforwardly into the framework of earlier papers are taken into account in the design of our treatments: We need to distinguish between risk resolution and risk revelation, since in our setting it is possible that risk is resolved and recipients paid without any information being revealed to the decision maker. We want to highlight the role of this information revelation so we suppress the issue of time valuation of money by fixing the date of payment. Finally, we account for the details of our implementation.

3.1 The risk-taking task

Binswanger (1981) introduced the concept of having participants make discrete choices between alternatives presented to them, and this is a flexible principle that has been applied in many important studies (such as Tversky and Kahneman, 1992; Holt and Laury, 2002; von Gaudecker, van Soest, and Wengström, 2011). Abdellaoui et al. (2011) specialized this design for a context that is similar to ours, by providing ten different lotteries, which, when compared to varying safe amounts, allow identification of the three parameters we are interested in. We adopted the lotteries of Abdellaoui et al. (2011) for our study, but made some differences to the procedures and presentation.

All the lotteries considered by Abdellaoui et al. (2011) are binary lotteries with probabilities that vary in steps of $1/6$. They presented the lotteries as six different coupons in a box, and made participants choose between two boxes of coupons (in one box, all the coupons were the same). The experiment interface then presented the same box of lotteries but iterated the value of the coupons in the "safe" box to elicit the certainty equivalent of each lottery. Each participant repeated this for each of ten pre-determined lotteries. We adopted this task for decision making on behalf of others, but modified it in two ways, both in order to limit the number of choices that participants need to deal with. First, we did not aim to identify all parameters at the individual level, but limited our aim to study the distribution of parameters at the population level. With this restriction in mind, for each participant we sampled four out of the ten lotteries of Abdellaoui et al. (2011). Second, instead of an iterated procedure to determine the certainty equivalent, we let participants choose between each of the lotteries sampled for them and seven different safe outcomes in a list presented to them, providing 28 discrete decisions from each participant.[2] Mapping the probabilities to standard dice with outcomes $\{1,\ldots,6\}$, the pool of ten lotteries we sampled from and presented to participants are reported in Table 1.

Abdellaoui et al. (2011) found that these 10 lotteries were sufficient to identify the full vector (α,β,ρ), both risk aversion and the probability weighting function of Prelec (1998). However, there are limits to such identification. For people with extreme behavior, these parameters are not likely to be separately identified. Consider a person who always chose the

[2] A pilot study was conducted to test whether the context matters for a single choice, whether the spread of the safe outcomes (small or large) influences choices between lotteries and safe outcomes. Results of the pilot study found no such contextual effects.

Table 1: Pool of lotteries shown to participants

	Die outcome (X)							
Lottery	1	2	3	4	5	6	$E[X]$	Safe outcome alternatives
1	0	0	0	0	0	240	40	(10, 20, 30, 40, 50, 60, 70)
2	0	0	0	0	240	240	80	(20, 40, 60, 80, 100, 120, 140)
3	0	0	0	0	120	120	40	(10, 20, 30, 40, 50, 60, 70)
4	120	120	120	120	240	240	160	(130, 140, 150, 160, 170, 180, 190)
5	60	60	60	60	120	120	80	(70, 75, 80, 85, 90, 95, 100)
6	80	80	80	80	200	200	120	(90, 100, 110, 120, 130, 140, 150)
7	180	180	180	180	240	240	200	(190, 195, 200, 205, 210, 215, 220)
8	0	0	0	240	240	240	120	(30, 70, 100, 130, 170, 200, 230)
9	0	0	240	240	240	240	160	(30, 70, 100, 130, 170, 200, 230)
10	0	240	240	240	240	240	200	(30, 70, 100, 130, 170, 200, 230)

Note: The pool of 10 lotteries; participants are randomly presented 4 lotteries out of the ten lotteries and decide between these lotteries and the 7 safe outcome alternatives. 10 NOK is worth about €1.

safe alternative. No model can help us distinguish whether this was a risk neutral person with extreme under-weighting of probabilities ($\alpha < 1$ and $\beta \to \infty$) or an extremely risk averse person without any probability distortions. Distinguishing between probability weighting and risk aversion requires that people make choices that are not all corner solutions.

A number of risk-taking tasks are known from the experimental literature. One of the most elaborate, but also most powerful in terms of learning about risk aversion is that of Choi, Fisman, Gale, and Kariv (2007). In its standard formulation, this task relies on participants choosing on a budget line, allocating an endowment between two equi-probable assets. If variation in probabilities could be added to this task, it would be a powerful tool for our purposes, but we worried that within-individual variation in probabilities would make this task too complicated for an online experiment with general population participants that have limited experience with economic experiments.

At another other extreme are simple tasks such as the investment game of Gneezy and Potters (1997). In this task, participants choose how much of an endowment to allocate to a risky asset. This has the potential of providing a continuous measure of risk taking, but in practice, participants often choose corner solutions and it is not clear how one would extend this task to allow collecting sufficiently rich information to identify the full model we are interested in.

3.2 Overview of treatments

We had our participants (decision makers) make decisions on risk for others, determining the payment that these others (recipients) would get for doing a small task, with variation in when decision makers would learn about the effect of their decisions on the recipients. When making over risk on own behalf, it is convenient to have payment and revelation of uncertainty coincide. Depending on one's perspective, this has the possible disadvantage that risk preferences and the time-valuation of money are conflated. When we are interested in risk taking on behalf of others, it has the further disadvantage that we would have to find a date at which to pay

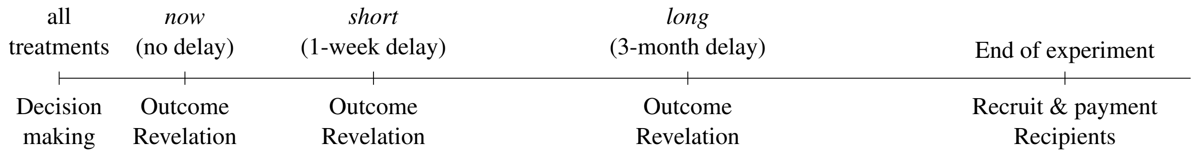

Figure 1: Timeline of the experiment

recipients even if we do not want to ever inform decision makers about the outcome drawn. We resolved this by fixing all payments to recipients to a fixed date, regardless of treatment. This allows us to focus only on the timing of when decision makers are told about what the payments are.

Our main interest is in how risk taking changes if the delay in uncertainty resolution is immediate, short or long – or if decision makers possibly never learn about the outcome. This gives us four treatments that we call *now*, *short*, *long*, and *never*. The short delay we set to one week, the long delay to three months. The date of payments we set at five months after the start of the experiment (see Figure 1 for an illustration of the timeline of the experiment).[3]

3.3 Implementation

We contracted with the survey provider Norstat to implement our experiment with 2000 participants drawn from their Norwegian probability-based general population panel. All participants have actively been recruited by Norstat, most of them by being contacted by phone, and Norstat works to maintain a balanced panel with respect to basic demographics. Norstat also aims to certify the quality of their panel in several ways: They have procedures to identify duplicate panel membership; they restrict the amounts of studies panelists are invited to take part in (most panelists take part in 1–2 studies per month); they weed out "speeders" that consistently use much less time on the studies they participate in than other panelists.

The experimental participants recruited by our survey provider were sent to a web interface implemented in Python/Django at a web server run at NHH Norwegian School of Economics, and all their decisions and survey answers were collected at this server. Before being allocated to a treatment and given experimental instructions, participants had to register their consent to standard procedures, but also to being contacted by the survey provider by SMS text message after the conclusion of the experiment – even if they by that time had decided to leave the Norstat panel. If they decided not to consent to this, they returned to the Norstat site. If they consented, they were randomized into a treatment, shown instructions and sent on to make decisions and answer some survey questions. At the conclusion of the survey questions, the experiment web server drew outcomes for recipients. If participants were in the *now* treatment, they were then immediately informed about the consequences of their decisions. For the participants in the *short* or *long* treatments, the experiment team once a week downloaded the drawn results and provided Norstat with a spreadsheet that contained experiment specific id codes, the messages to be sent, and the date at which the messages were to be sent; Norstat then prepared the text messages for sending. The experiment team did not at any point access identifying information of the participants. The experimental design was approved by the Institutional Review Board at NHH Norwegian School of Economics.[4]

[3] All treatments except *now* necessarily involves re-contacting the participants in the experiment after the conclusion of their session, so the *now* vs *short* comparison might also reflect any effect of the re-contacting itself (such as maybe some less trust in the delayed messages arriving).

[4] Reference NHH-IRB 06/19, March 4th, 2019.

A consenting participant was shown instructions in two parts, in separate web pages. The first part explained that they would be asked to make decisions about money and risk on behalf of another person, "the recipient." The recipient had done a small task, and their payment for this task would be decided by the decision of the participant, and recipient is informed about this. Participants were informed that they would make a number of decisions and that the research team would draw one fifth of all participants, and each of these participants would have one of their decisions implemented for a recipient in five months time. They were then given the treatment specific information about how they would learn the outcome of the experiment:

Treatment "now": You will be informed about if and how your decisions affect the recipient at the end of the study.

Treatment "short": You will be informed by Norstat about if and how your decisions affect the recipient 7 days after the study.

Treatment "short": You will be informed by Norstat about if and how your decisions affect the recipient 3 months after the study.

Treatment "never": You will not be informed about if and how your decisions affect the recipient.

The second part of the instructions explained the details of the decisions they were to make: The participant could decide if the outcome for the recipient should be a fixed amount or if it should be determined by the roll of a (fair) die. A table showed what the decision interface would look like, and they were told that they would be asked to decide on four different dice, each of them with a different color and a different mapping between eyes and monetary outcomes for the recipient. They were then reminded about how they would be informed about the outcome of the experiment.

After confirming that they had read the instructions, participants proceeded to the decision making interface. Figure 2 shows what this looked like for one of the dice they faced. The four dice were randomly sampled from those in Table 1 and shown in a random order. The different colors were chosen to make sure that the interfaces gave visual confirmation that they had proceeded to make decisions about a different lottery, and the colors (red, green, blue, and yellow) were randomly assigned to dice.

After making choices, participants answered questions about their own risk preferences, as well as their beliefs about the risk preferences of others. We use hypothetical questions similar to those in the study by Falk, Becker, Dohmen, Enke, Huffman, and Sunde (2018). We also asked a question about social concern, also modeled after Falk et al. (2018), and one question about their emotional state while making decisions (van Winden, Krawczyk, and Hopfensitz, 2011). Following these questions, we also asked about some basic socio-demographics (age, gender, education, and whether they are parents).

In the last part of the experiment, we recruit recipients from the online labor platform Amazon Mechanical Turk. Recipients receive a fixed payment of one US dollar, and perform a minor task to justify payment. They are informed that additional earnings will be determined by a decision maker randomly matched with them, and given a short description of the decision maker's problem.

En grønn terning

Nå skal du bestemme mellom å rulle den grønne terningen og 7 forskjellige sikre beløp. Dersom du ruller terningen vil utbetalingene bli:

Terningen viser:						
Betaling ved den grønne terningen:	180 kr	180 kr	180 kr	180 kr	240 kr	240 kr

På vegne av mottakeren ber vi deg å velge mellom

- A: å rulle den grønne terningen,
- B: sikker betaling,

i hver av disse situasjonene (hvor den sikre betalingen varierer):

		Alternativ A:	Alternativ B:	
		Rulle grønn terning	**Sikker betaling**	
Situasjon 1		○	○	190 kr
Situasjon 2		○	○	195 kr
Situasjon 3		○	○	200 kr
Situasjon 4		○	○	205 kr
Situasjon 5		○	○	210 kr
Situasjon 6		○	○	215 kr
Situasjon 7		○	○	220 kr

Jeg har tatt beslutning for alle situasjonen og vil gå videre

Husk at om du blir trukket ut til å bestemme betaling, så vil en av dine beslutninger faktisk bestemme betalingen for en virkelig mottaker. **Du vil bli informert av Norstat om hvordan din beslutning evt. påvirker mottakerens betaling 7 dager etter undersøkelsen.**

Figure 2: Screenshot of a decision screen

Table 2: Attrition

	Treatment			
	Now	Short	Long	Never
Reading instructions	0.148	0.144	0.129	0.139
Making decisions	0.074	0.078	0.080	0.068
Answering background questions	0.002	0.001	0.001	0.000
Revoking consent	0.027	0.030	0.029	0.029
Total attrition:	0.252	0.254	0.240	0.236

Note: The table shows at what stage of the experiment the participant left the study, and the total amount of attrition by treatment. Only participants that gave initial consent (and hence was assigned a treatment) are counted.

4 Descriptive statistics

In this section we first provide some statistics on the implementation, before we give an overview of the choices made by decision makers and how these relate to characteristics of the decision makers.

4.1 Sample and attrition

In total, 2904 Norstat panelists logged onto the experiment web server with valid participation ids. Of these 230 were never randomized into treatment, either because they clicked the "no consent" button (169), or because they abandoned the study without signing out (61), and a total of 2674 participants were randomized into one of our four treatments. 2019 completed, giving us a post-randomization attrition rate of 24.5 percent, perhaps indicating that the experiment was more tedious or complicated than the Norstat panelists are used to. Fortunately, the attrition was not correlated with treatment assignment. Table 2 documents that more than half of the attrition was participants abandoning the experiment while reading instructions, about a third of the attrition was participants abandoning the experiment while in the process of making decisions about risk taking. Some participants kept a copy of the consent form open in their browser and revoked consent at some point after randomization. All these rates are very similar across treatments.

Even if attrition is not correlated with treatment assignment, attrition might cause the sample of participants to become less representative. We do not have any information on the participants that left before answering the background questions, but we can compare the final sample with the Norwegian population. Table 3 compares the distribution of participants across age and gender with official numbers from Statistics Norway; these are two characteristics in which there are unlikely to be much measurement error in, and for which official statistics of high quality are readily available. We see that our experimental sample is older than the population, particularly males 18–30 are underrepresented in our sample. Among women, the problem is not quite as severe as among the male in the younger age groups. Among the 60–80 year olds, we have oversampled both men and women to about the same extent. Overall, our sample has a slightly higher proportion of males than the Norwegian population, but not by much more than what could be expected due to random sampling.

Another concern that has been made about participants in online surveys and experiments is

Table 3: Representativeness of sample

	Male		Female	
	Sample	Population	Sample	Population
A. Share of age group – within gender				
$[18,20)$	0.003	0.033	0.016	0.030
$[20,30)$	0.079	0.174	0.138	0.165
$[30,40)$	0.135	0.174	0.143	0.167
$[40,50)$	0.179	0.177	0.165	0.169
$[50,60)$	0.214	0.168	0.180	0.161
$[60,70)$	0.206	0.137	0.201	0.137
$[70,80)$	0.168	0.096	0.143	0.104
$[80,\cdot)$	0.017	0.041	0.014	0.066
B. Share of gender group				
Total	0.543	0.501	0.457	0.499

Note: The shares from the sample are coded from those that provided valid ages in the short survey after the decisions. The population numbers are taken from Statistics Norway's 2019 numbers (Table 10211), `https://www.ssb.no/statbank/table/10211/`.

that they "speed" through the survey instrument without paying sufficient attention to the information presented to them (see, e.g., Greszki, Meyer, and Schoen (2014) and Zhang and Conrad (2014)). As mentioned in Section 3.3, this is a criterion that Norstat, our survey provider, attempts to screen out of the panel, but it is also something we can examine with the data we have collected. From entering the website to the first decisions, the median participant used 4 minutes and 5 seconds (with the 25th percentile spending 2 minutes and 57 seconds, the 75th percentile at 5 minutes and 47 seconds). Less than 1 percent of participants used less than a minute from start to the first decisions.[5] The median time for the subsequent three decisions was 31 seconds (with the 25th percentile at 21 seconds and the 75th percentile at 46 seconds), with only 1.5 percent spending less than 10 seconds. We conclude that speeding is not a major concern in our data.

Table 4 provides descriptive statistics on the non-incentivized information we collected toward the end of the study. In addition to the age and gender variables we discussed above, comparing our sample to the Norwegian population, we see that most of our participants are parents, which means that they must have some experience making decisions on behalf of others. We see also that about a third of our participants have high school or less as their highest completed education, and fully 60 percent report to have higher education. Even if we might have some measurement error with the simplified categorization we used in the survey, it seems fair to conclude that our sample is better educated than the Norwegian population – of which 34 percent have attained higher education, and about 63 percent have high school or less.[6]

In Table 4 we can also see that on average our participants evaluate themselves as slightly

[5]The mean time to completion is not a useful statistic for detecting speeding, since the mean time to completion is dominated by a thin tail of participants who are likely to have taken long breaks before completing the survey.

[6]The education statistics for the full population are taken from Statistics Norway's 2018 numbers (Table 11293), `https://www.ssb.no/statbank/table/11293/`, for the population 16 and above.

Table 4: Descriptive statistics

Variable	Mean	Std. Dev.
Age (years)[1]	52.1	16.2
	(0.36)	(0.19)
Female	0.457	
	(0.011)	
Is a parent	0.729	
	(0.010)	
Education:		
Middle school	0.043	
	(0.005)	
High school	0.267	
	(0.010)	
Tertiary vocational	0.090	
	(0.006)	
Higher education	0.600	
	(0.011)	
Self evaluation of risk willingness (1–7 scale)	3.593	1.357
	(0.030)	(0.021)
Belief about others' risk willingness (1–7 scale)	4.111	0.907
	(0.020)	(0.022)
Own willingness for good works (1–7 scale)	4.889	1.274
	(0.028)	(0.021)
Affect during experiment:		
anxious	0.030	
	(0.004)	
excited	0.345	
	(0.011)	
hopeful	0.187	
	(0.009)	
worried	0.118	
	(0.007)	
none of the above	0.320	
	(0.010)	
Observations	2019	

[1] There were 17 participants that did not provide an age.

Note: Descriptive statistics on the self-reported and non-incentivized data collected in the experiment. Standard errors in parentheses. Standard deviations are provided for the numerical variables only; the standard errors of the standard deviations are calculated with the jackknife.

cautious when it comes to risk taking, averaging 3.6 on a 1–7 scale. On average, they also believe other participants to be more risk taking than themselves, with an average of 4.1. It is also interesting that the standard deviation in what they believe about others is as much as 2/3 of the self-reported willingness to take risk, which might indicate that the population is not particularly well informed about the preferences of others. The correlation between own risk willingness and the belief about others risk willingness is 0.32. It is perhaps not surprising that this correlation is positive, but it is also not so large that it should be impossible to distinguish between them in trying to account for decision making. When it comes to evaluating their own willingness to support good works without expecting anything in return, the participants average almost a whole point above neutral on the 1–7 scale, this might reflect that those willing to take part in online research are positively selected on pro-social attitudes, but perhaps also a self serving bias.

The final group of variables in Table 4 concern the self reported affect during the experiment. Participants could check one out of five alternatives, and a bit more than half have indicated that they were either "excited" or "hopeful," both of which can be classified as positive anticipatory emotions. Only a small minority indicated the negative anticipatory emotions ("anxious" or "worried"), while about a third did not find any of these categories to be a fit description of their emotional state.

4.2 Decisions

Figure 3 summarizes the choices in the experiment for each of the treatments in each of the ten different lotteries. A benchmark to keep in mind is that a risk neutral agent without any probability distortion would always choose the lottery for the first safe alternative, and never take the lottery for the last safe alternative. First, we can note that all lotteries provide evidence for both risk averse and risk seeking behavior, since the proportion that chose the lottery over the first (and smallest) safe alternative is well below unity, and the proportion that chose the lottery over the last (and largest) safe alternative is well above zero. Choice probabilities do not react as strongly to the value of the safe alternative as one would have believed. Consider the first safe amount in the first lottery. Participants evaluate a $1/6$ probability of 240 NOK vs a safe amount of 10 NOK, and about 25 percent of participants chose the safe amount of 10 NOK. This seems risk averse in the extreme. Second, comparing each treatment (row) for a given lottery (column), it seems that choices do not differ much by treatment, at least not in any striking way.

Figure 4 provides a complementary view of the same data, focusing instead on the proportion of times each individual participant chose the lottery in the 28 decisions that they faced. First, we can note that about 10 percent of participants never chose the lottery – slightly more in the *now* and *short* treatment, slightly less in the *long* and *never* treatments. In all treatments there are also a few percent of the participants who always chose the lottery over the safe alternative. Given the large range of safe outcome alternatives available (cf. the rightmost column of Table 1), this is a bit surprising. Second, only in the *now* treatment do the remaining participants center on a symmetric bell-shaped distribution. The three other treatments seem markedly non-symmetric, even if there are no differences in central tendency (the averages of the proportion of lottery choice are 0.440, 0.449,0.448, and 0.434, the differences are not significant). An Epps and Singleton (1986) test for equality of distribution, with the natural extension to 4 groups, reject that the four treatments generate the same distribution of average lottery choices at the 10% level ($p < 0.095$).

The pictures of Figure 3 and Figure 4 hide some of the behavior by individuals with respect

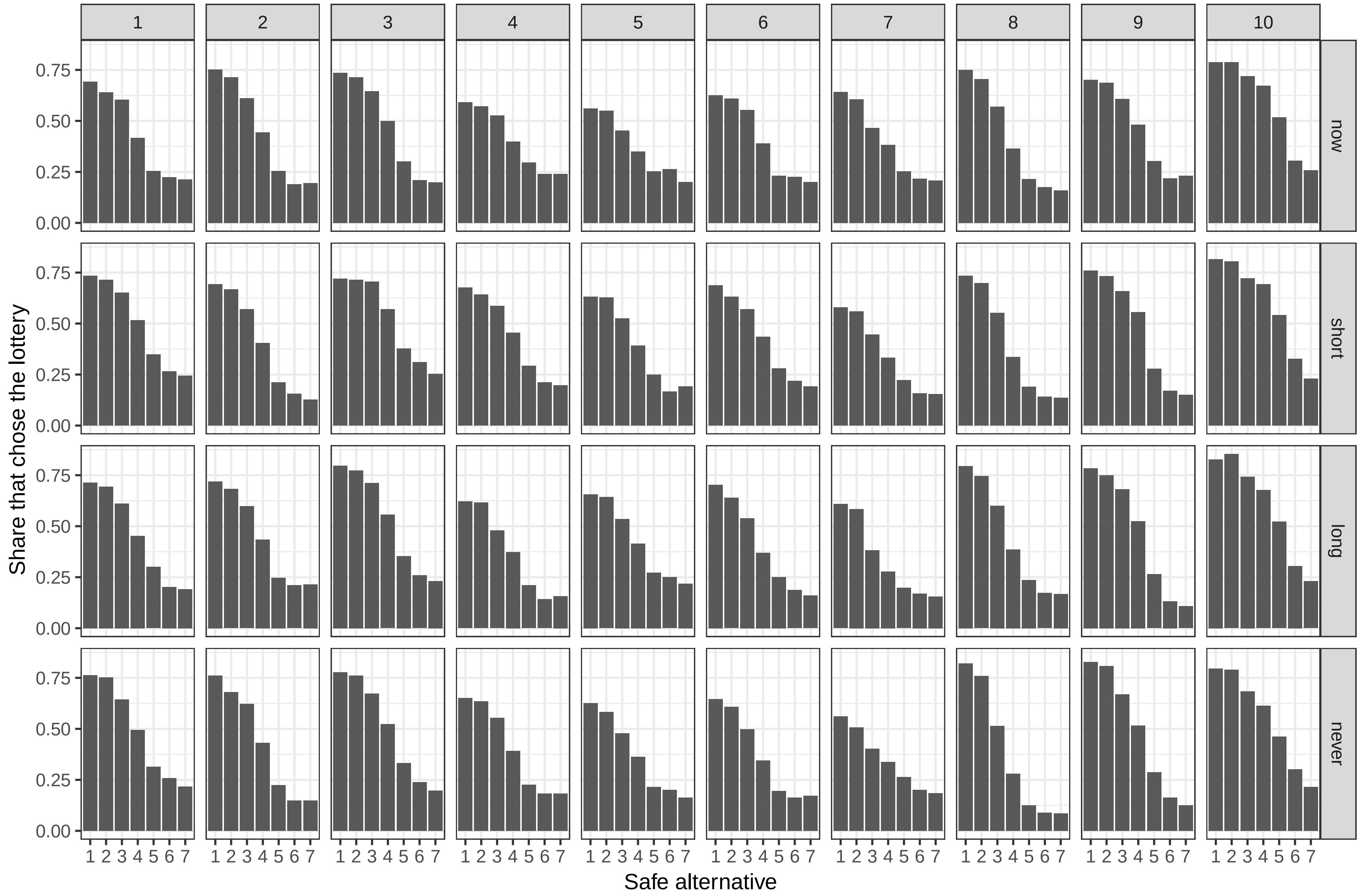

Figure 3: Distribution of choices

Note: For each of combination of lottery and treatment, the graph shows the proportion that chose the lottery for each of the seven safe outcome alternatives. The die numbers and the safe outcome alternatives refer to the dice in Table 1.

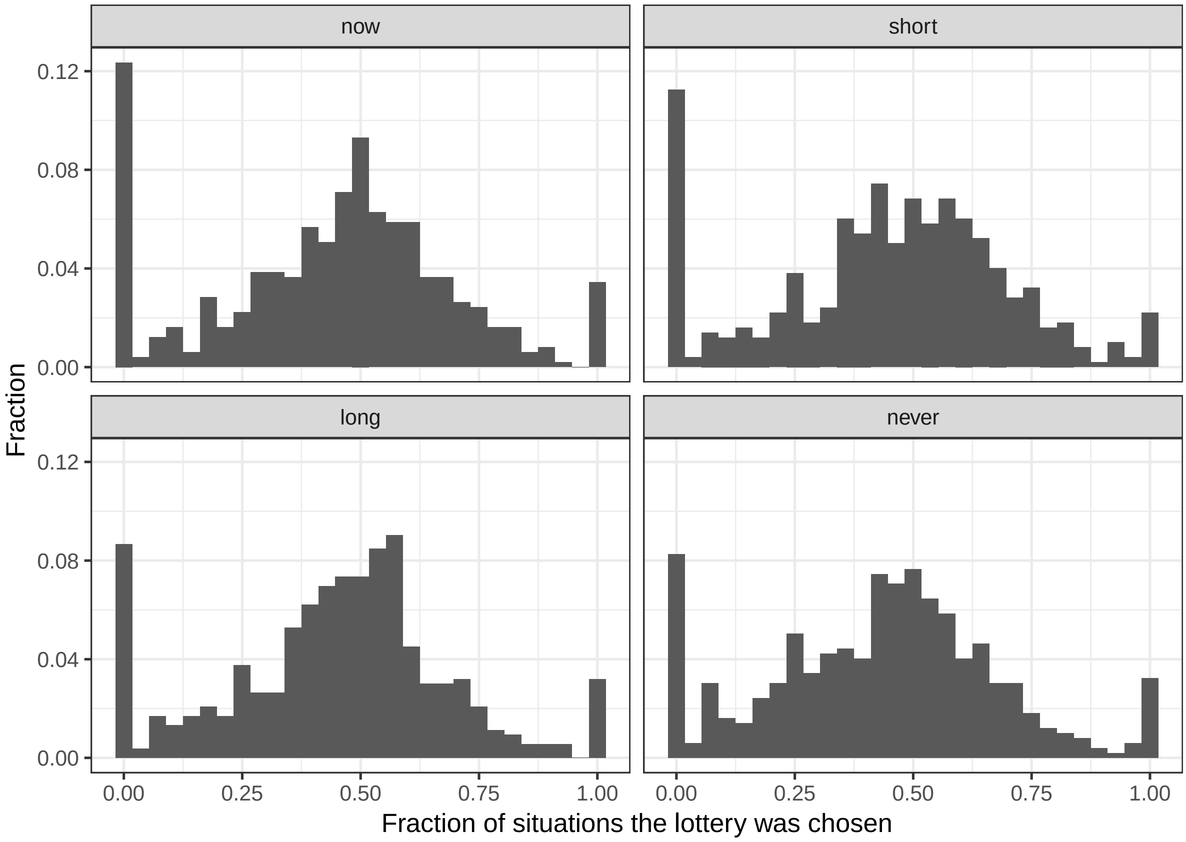

Figure 4: Distribution of the proportion of lottery choices

Note: Each participant is represented by the proportion of situations in which they chose the lottery over the safe option.

to the dice they make decisions over. Examining the 8076 lotteries that our participants considered, we find that in about 10 percent of the lotteries, participants chose the lottery regardless of the safe alternative, and in about 21 percent of the lotteries participants always chose the safe amount. In about 11 percent of the lotteries, participants made a switch from taking the safe amount to taking the lottery as the value of the safe alternative increased, this is inconsistent with reasonable interpretations of what altruistic behavior on behalf of the recipient would imply.

5 Results

In this section we first look at direct behavioral evidence for treatment effects on risk taking on behalf of others, before formulating an empirical counterpart of the choice model in Section 2. The empirical choice model is estimated with a hierarchical Bayes procedure before the evidence is summarized with respect to our registered hypotheses.

5.1 Reduced form evidence

In Table 5, we put the proportion of lottery choices from Figure 4 on the left hand side of a regression on treatment dummies and the non-incentivized variables collected in the survey at the end of the experiment on the right hand side. In the first column, we include only dummies for each of the treatment, and from this we can confirm that there are no substantial differences in average risk taking between treatments. The dummies are small and precisely estimated, and a preliminary conclusion is that of a null finding: There are no significant differences in the proportion of lottery choices by the horizon of risk revelation.

In the second column, we introduce the survey variables, the treatment dummy parameters do not change in any substantial way. The socio-demographic variables, age, gender, being a parent and education level do not seem to matter much at all. It is, however, interesting to see that self reported willingness to take risk on own behalf has a substantial and precisely estimated effect on risk taking, but beliefs about the risk willingness of others are not important. This is in line with the concept of "projective paternalism" discussed by Ambuehl et al. (2019). It could potentially also be due to reverse causality from taking a lot of risk to reporting risk willingness, but one could easily think that this reverse causality would be just as strong – or even stronger – on beliefs about the risk willingness of others, as that would seem to provide a perfect rationalization for having taken risk on the behalf of others. It is also striking to see how large the impact of the indicators for self reporting positive anticipatory emotions is. The estimated coefficient on "hopeful" is more than 20 percent of the mean and more than 40 percent of a standard deviation of the outcome.

Allowing the parameters to differ by treatment (columns 3-6) does not reveal any substantial heterogeneities in the effect of the survey variables.

5.2 An empirical choice model

While the reduced form results on the proportion of risk choices in Section 4.2 suggest precise null effects of our treatments, that is not informative about the mechanisms behind lottery choice. The pictures in Figure 4 seem to allow for effects on choices that are smoothed out when one focuses on the average proportions alone. There might be effects on risk aversion and probability distortion that average out. This sub section describes an empirical version of the choice model in Section 2. This model allows inference with rich heterogeneity in the important parameters within a hierarchical Bayesian model, estimated using the probabilistic programming language Stan (Carpenter, Gelman, Hoffman, Lee, Goodrich, Betancourt, Brubaker, Guo, Li, and Riddell, 2017; Stan Development Team, 2018).

A problem with the model in equation (1)–(3) (on page) is that given a lottery and the parameters (α,β,ρ), the choice is deterministic. In order to fit actual data, we must allow choices to be stochastic. Since our choices are discrete between binary alternatives, a natural way to complete the model is by adding random utility terms to each alternative (McFadden, 1974). In the context of binary choice, this is equivalent to a logit model,

$$P[L] = \Lambda\big(\lambda \cdot \Delta U(L,S)\big), \tag{4}$$

in which $\lambda > 0$ is a parameter that reflects the variance of the random utility term, Λ is the distribution function of the logistic distribution, and $\Delta U(L,S)$ is the utility difference between the lottery and the safe alternative. The vector of parameters that fully describes a decision maker's behavior, θ, is

$$\theta = (\alpha,\beta,\rho,\lambda).$$

The model of equations (1)–(3) is not cardinal, and embedding it into a random utility model makes it necessary to decide on a cardinalization of the systematic utility, $\Delta U(L,S)$. We follow von Gaudecker et al. (2011) in cardinalizing the rank dependent utility by its certainty equivalent, such that the systematic part of utility can be written $\Delta U(L,S) = CE(RDU(L)) - S$. This

Table 5: Regressions of average risk taking on background characteristics

			Treatment			
			Now	Short	Long	Never
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment short	0.009	0.009				
	(0.016)	(0.015)				
Treatment long	0.008	0.007				
	(0.015)	(0.014)				
Treatment never	−0.006	−0.001				
	(0.016)	(0.015)				
Age		−0.001	0.000	−0.003	−0.002	−0.001
		(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Female		−0.008	−0.039	0.017	−0.019	0.012
		(0.011)	(0.022)	(0.021)	(0.021)	(0.022)
Parent		0.005	−0.004	0.013	0.040	−0.023
		(0.013)	(0.027)	(0.026)	(0.025)	(0.026)
High School		0.020	−0.090	0.083	0.012	0.085
		(0.026)	(0.054)	(0.051)	(0.055)	(0.052)
Tertiary vocational		0.010	−0.076	0.046	−0.001	0.067
		(0.030)	(0.061)	(0.058)	(0.060)	(0.060)
Higher education		0.021	−0.086	0.101	0.022	0.062
		(0.025)	(0.052)	(0.049)	(0.053)	(0.050)
Own risk willingness		0.051	0.052	0.062	0.038	0.049
		(0.004)	(0.008)	(0.008)	(0.009)	(0.008)
Belief about other's risk willingness		0.003	−0.007	0.013	−0.011	0.017
		(0.006)	(0.012)	(0.012)	(0.012)	(0.012)
Own willingness for good works		−0.019	−0.036	−0.020	−0.010	−0.009
		(0.004)	(0.008)	(0.008)	(0.008)	(0.008)
Hopeful		0.106	0.157	0.063	0.117	0.088
		(0.015)	(0.031)	(0.030)	(0.029)	(0.033)
Excited		0.088	0.126	0.060	0.113	0.050
		(0.013)	(0.026)	(0.026)	(0.025)	(0.025)
Worried		0.007	0.021	0.001	0.060	−0.037
		(0.018)	(0.036)	(0.034)	(0.036)	(0.035)
Anxious		0.000	−0.055	0.016	0.049	−0.022
		(0.031)	(0.067)	(0.063)	(0.064)	(0.056)
Constant	0.440	0.351	0.519	0.247	0.406	0.214
	(0.011)	(0.048)	(0.096)	(0.094)	(0.095)	(0.092)
Observations	2,019	2,002	488	497	527	490
R^2	0.001	0.151	0.213	0.206	0.116	0.165

Note: Ordinary least squares regressions of the proportion of situations in which participants chose the lottery on treatment dummies and background variables. The left out categories are the "now" treatment, males, those who are not parents, those with less than high school education, and those that do not report any of the listed affect categories. Standard errors in parentheses.

money metric utility scale has the further advantage that the scale of random utility can be put in money terms and interpreted independently of the utility function parameters.

In Section 3 we briefly discussed how identification of all parameters relied on people not making choices in one corner. With the stochastic formulation of the choice model, this condition now changes to a requirement that all choice probabilities are in the interior of $(0,1)$ (the model assumptions ensure this). But considering the number of participants who go to one of the extremes in Figure 4, we can tell that our model will struggle to account fully for the heterogeneity in the tails of the parameter distributions.

If we also add the notational convention that individual $n = 1,\dots,N$ make choices in situations indexed by $j = 1,\dots,J$, we can reformulate our full model in terms of the indicator function for choosing a lottery L as

$$I^L_{nj} \sim \text{Bernoulli}(p_{nj}), \tag{5}$$

$$\text{logit}(p_{nj}) = \lambda_n(\text{CE}_{nj} - s_{nj}), \tag{6}$$

$$\text{CE}_{nj} = \left((1-\pi_{nj})x^{\rho_n}_{1nj} + \pi_{nj}x^{\rho_n}_{2nj}\right)^{1/\rho_n}, \tag{7}$$

$$\pi_{nj} = \exp\left(-\beta_n(-\log p_{2nj})^{\alpha_n}\right). \tag{8}$$

The first of these equations says that the outcome is Bernoulli distributed with probability p_{nj}; the second that the choice probabilities are determined by a logit transformation (alternatively, that the random utility terms are extreme value distributed); the third formulates the certainty equivalent of rank dependent utility as functions of the probability weights and the outcomes, and the final how the probability weights are determined. Equations (5)–(8) now encode a likelihood function for the data generated by an individual (these data are conventionally notated as y), and we refer to this likelihood function as $p(y|\theta)$.

Allowing for maximum heterogeneity in parameters such that θ_n varies from individual to individual, we follow the hierarchical Bayes methodology and formulate a set of distributions of the parameters that characterize an individual,[7]

$$\log \alpha_n \sim N(\mu_\alpha, \sigma^2_\alpha), \tag{9}$$

$$\log \beta_n \sim N(\mu_\beta, \sigma^2_\beta), \tag{10}$$

$$\log \rho_n \sim N(\mu_\rho, \sigma^2_\rho), \tag{11}$$

$$\log \lambda_n \sim N(\mu_\lambda, \sigma^2_\lambda). \tag{12}$$

With a conventional abuse of notation, it is now possible to express the distribution of θ as $p(\theta|\phi)$, with $\phi = (\mu_\alpha, \mu_\beta, \mu_\rho, \mu_\lambda, \sigma_\alpha, \sigma_\beta, \sigma_\rho, \sigma_\lambda)$. The prior distribution can now be written $p(\theta,\phi) = p(\theta|\phi)p(\phi)$. From Bayes' theorem, it is now possible to write $p(\phi,\theta|y) \propto p(y|\theta)p(\theta|\phi)p(\phi)$, and brute force integration can provide the distribution of hyper parameters, $p(\phi|y)$. Algorithmic innovations have made the necessary multivariate integration possible by way of simulation. Stan provides the No U-Turn Sampler (NUTS), which relies on Hamiltonian Monte Carlo (HMC) for the integration (Carpenter et al., 2017). The posterior of the hyper parameters, $p(\phi|y)$, can be calculated by specifying $p(\phi)$, a prior on the hyper parameters. Stan also provides individual level posterior distributions for θ_i which we will not emphasize in this paper.

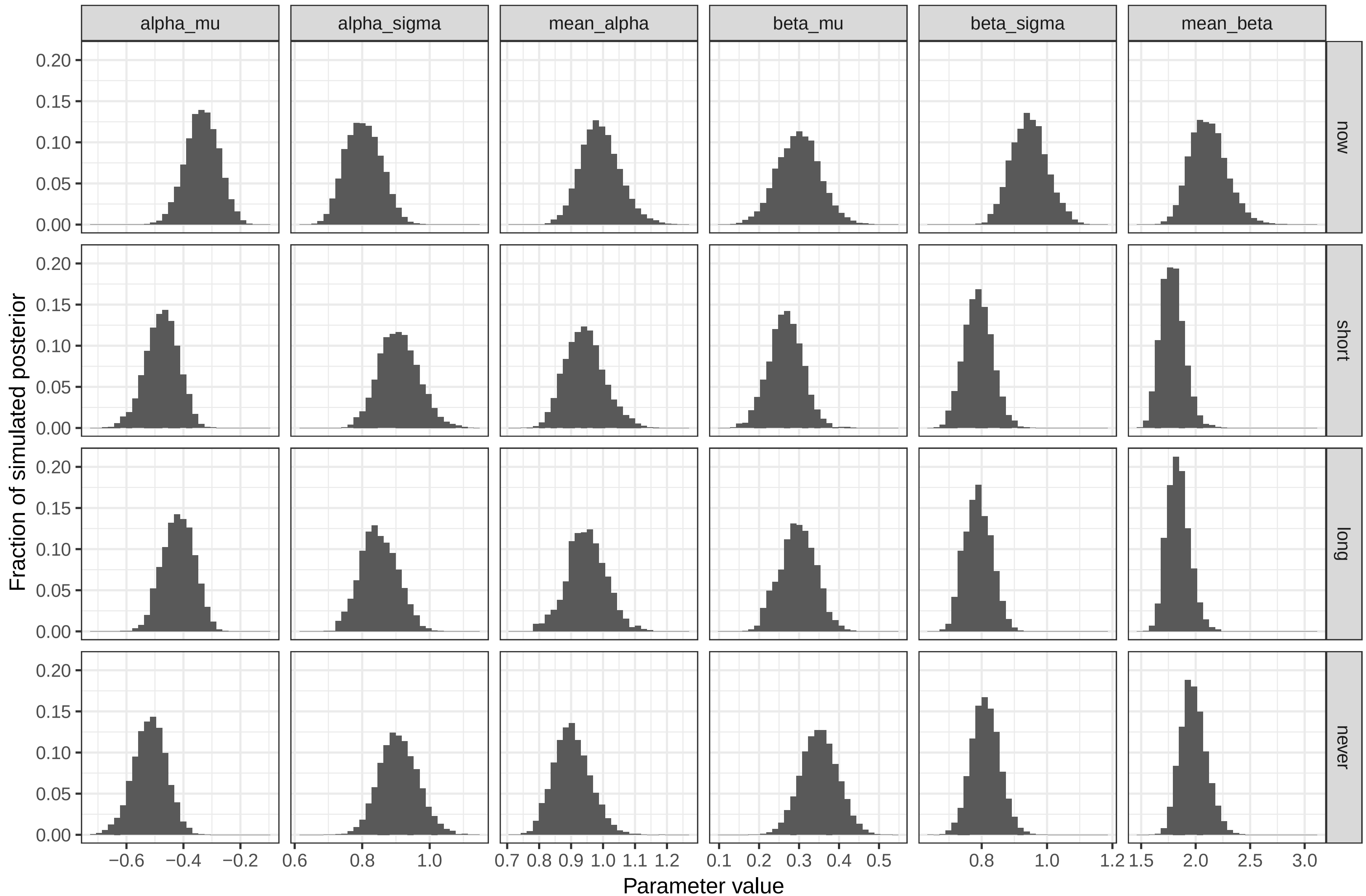

Figure 5: Posterior distribution of hyper-parameters for the probability weighting function

Note: Marginal distributions of the $(\mu_\alpha, \mu_\beta, \sigma_\alpha, \sigma_\beta)$ parameters for each of the four treatments. Column 3 and 6 present the implied posterior for $E[\alpha]$ and $E[\beta]$.

5.3 Choice model estimates

We estimated the model separately for the four different treatments, with the same priors on the hyper parameters $p(\phi)$, ensuring that any difference in the posteriors derive from the data alone. Our model is highly non-linear in parameters and this makes it necessary to formulate weakly informative and regularizing priors to ensure that NUTS will converge. We use symmetric normal priors on the μ_k's, and positive half-Cauchy for the standard deviations:

$$\mu_k \sim N(0,1), \quad \text{for } k \in \{\alpha, \beta, \rho\}, \tag{13}$$

$$\mu_\lambda \sim N(0.01,1), \tag{14}$$

$$\sigma_k \sim \text{Cauchy}_+(0,1), \quad \text{for } k \in \{\alpha, \beta, \rho, \lambda\}. \tag{15}$$

With 3 chains of 3000 warmup iterations and 3000 sampling iterations, we evaluated convergence with visual inspection of the sampling trace, the number of effective samples and the $\widehat{R}$-statistic of Gelman and Rubin (1992). For one treatment (*now*), the chains did not seem to converge properly, and for this treatment we re-ran estimation with 6 chains, 6000 warmup iterations and 6000 sampling iterations and then the chains passed our tests. In Figure 5 we present the marginal distribution of the samples of the hyper parameter posteriors for the probability weighting functions and the implied expectations for α and β.[8]

Looking across the columns of Figure 5, there are no striking treatment differences, and most parameters seem reasonably precisely estimated. One exception is that the σ_β values in the *now* treatment are higher than those in the other treatments (and $P(\sigma_\beta^{\text{now}} > \sigma_\beta^{\text{short}}) = 0.99$). More disturbing is the high level of all the σ_β's. While it is not straightforward to interpret what the implications are of the estimated parameters, we can sample (α, β) using the median estimates of $(\mu_\alpha, \mu_\beta, \sigma_\alpha, \sigma_\beta)$ and then examine the implied probability weighting functions. Figure 6 shows 500 samples of the weighting functions (and the weighting function of a participant with median parameter values). There is a strikingly large heterogeneity in weighting functions. A substantial proportion seems to put almost no positive weight on probabilities at all, which implies that they evaluate lotteries by their worst possible outcome. This is one way that the model can account for the proportion of people that never chose the lottery in any of their 28 situations. The median parameter values imply a moderate under-weighting of all probabilities large enough to be a feature of the lotteries we exposed our participants to (in our lotteries, probabilities started at 1/6).

Turning to Figure 7 and the marginal posterior distributions of $(\mu_\rho, \mu_\lambda, \sigma_\rho, \sigma_\lambda)$, one thing we can note about the distribution of α is that the median participant in treatments *now* and *short* seem to be approximately risk neutral (with posteriors μ_α centered around 0), with point estimates of median ρ at 1.03 in both the *now* and the *short* treatment. However, the median participant in the *long* and *never* treatments seem to be somewhat risk seeking, with median ρ's of 1.15 and 1.16 in the *long* and *never* treatments respectively. Considering the narrowness of the posteriors, we have some confidence in this conclusion, the posteriors imply that $P(\mu_\rho^{\text{never}} > \mu_\rho^{\text{now}}) = 0.95$, $P(\mu_\rho^{\text{never}} > \mu_\rho^{\text{short}}) = 0.97$, $P(\mu_\rho^{\text{long}} > \mu_\rho^{\text{now}}) = 0.94$, and $P(\mu_\rho^{\text{long}} > \mu_\rho^{\text{now}}) = 0.96$. The differences are not quite as stark in the mean ρ's, reflecting the role of heterogeneities.

The estimates of μ_λ, reflecting the impact of random utility on the median participant are for the most part centered around -1.75 (somewhat higher in the *short* treatment). This estimate implies that if two alternatives have monetary worth 10 NOK apart (about €1), the most

[7] See e.g Gelman, Carlin, Stern, Dunson, Vehtari, and Rubin (2013) for a text book treatment or Nilsson, Rieskamp, and Wagenmakers (2011) for a more complete discussion in the context of choices under risk.

[8] With $\log z \sim N(\mu, \sigma^2)$, the expectation is calculated as $E[z] = \exp(\mu + \sigma^2/2)$.

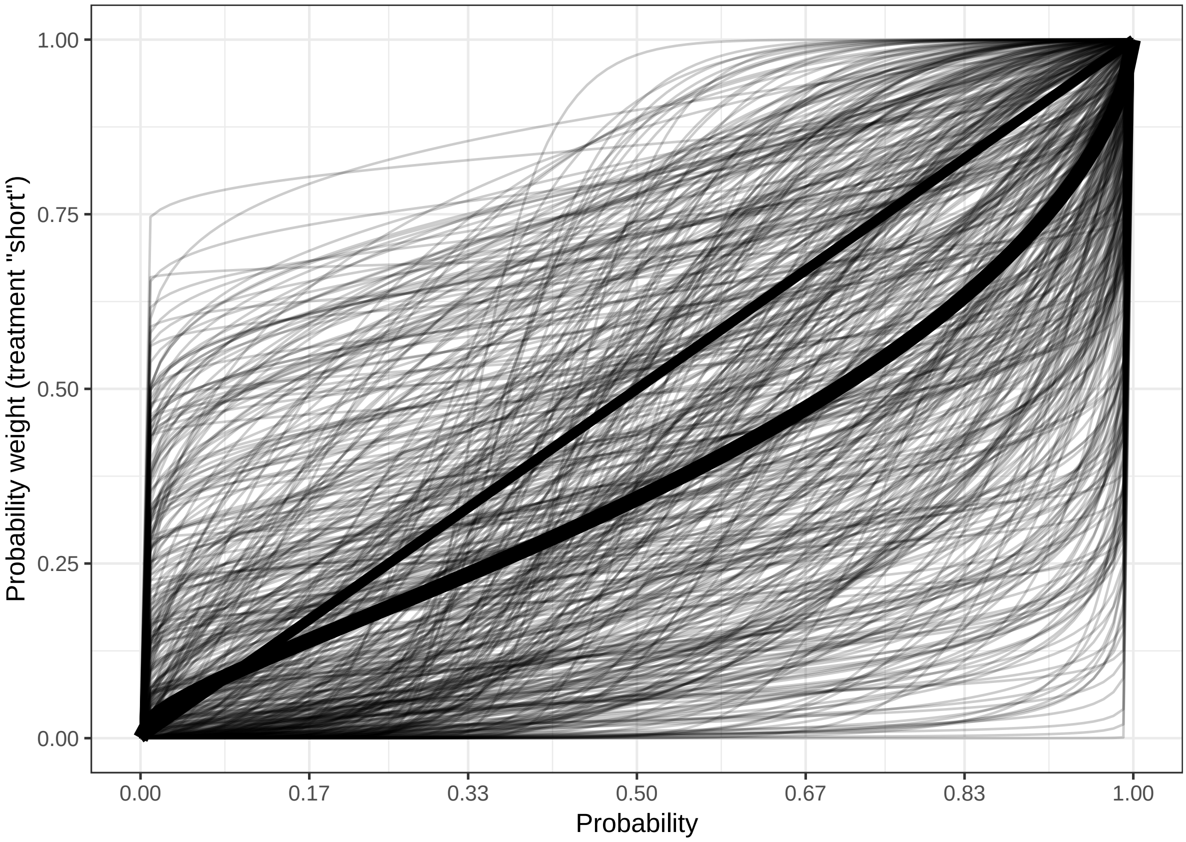

Figure 6: Probability weighting functions

Note: The graph shows 500 different probability weighting functions. For illustration, parameters are drawn from the *short* prior of (α, β) given hyper parameters that are medians from the posterior of $(\mu_\alpha, \mu_\beta, \sigma_\alpha, \sigma_\beta)$. Also shown is the 45-degree line and the weighting function implied by the median parameter values (in bold). The probabilities of the lotteries used in the experiment are indicated on the first axis.

valuable alternative will be chosen with probability $\Lambda(\exp(-1.75) \cdot 10) = 0.85$, which seems like a reasonable amount of random utility. If we instead look at the posterior for *mean* λ, we see some disturbingly large values. This reflects the influence of a long tail of the log normal distribution, which drives the choice probabilities to 1 for a segment of the population. Working together with how some participants underweight probabilities in the extreme (as shown in Figure 6), this allows the model to predict the fairly large proportion of the participants who never chose to take the lottery (as shown in Figure 4).

5.4 Discussion

In our pre-analysis plan we registered five main hypotheses.[9] In this section we relate our pre-specified hypotheses to the evidence from Section 5.1 and Section 5.2. There are few papers that provide relevant information about risk taking on behalf of others, so our hypothesis were to a large degree based on what others have found studying risk taking on behalf of self.

Our first hypothesis was that decision makers do not distinguish between no delay (the *now* treatment) and a short delay (the *short* treatment). We intended this hypothesis to mainly concern the mode informing participants about the outcome. A one-week delay is about as

[9]Our study was registered at the American Economic Association's registry for randomized controlled trials (RCT ID: AEARCTR-0004403).

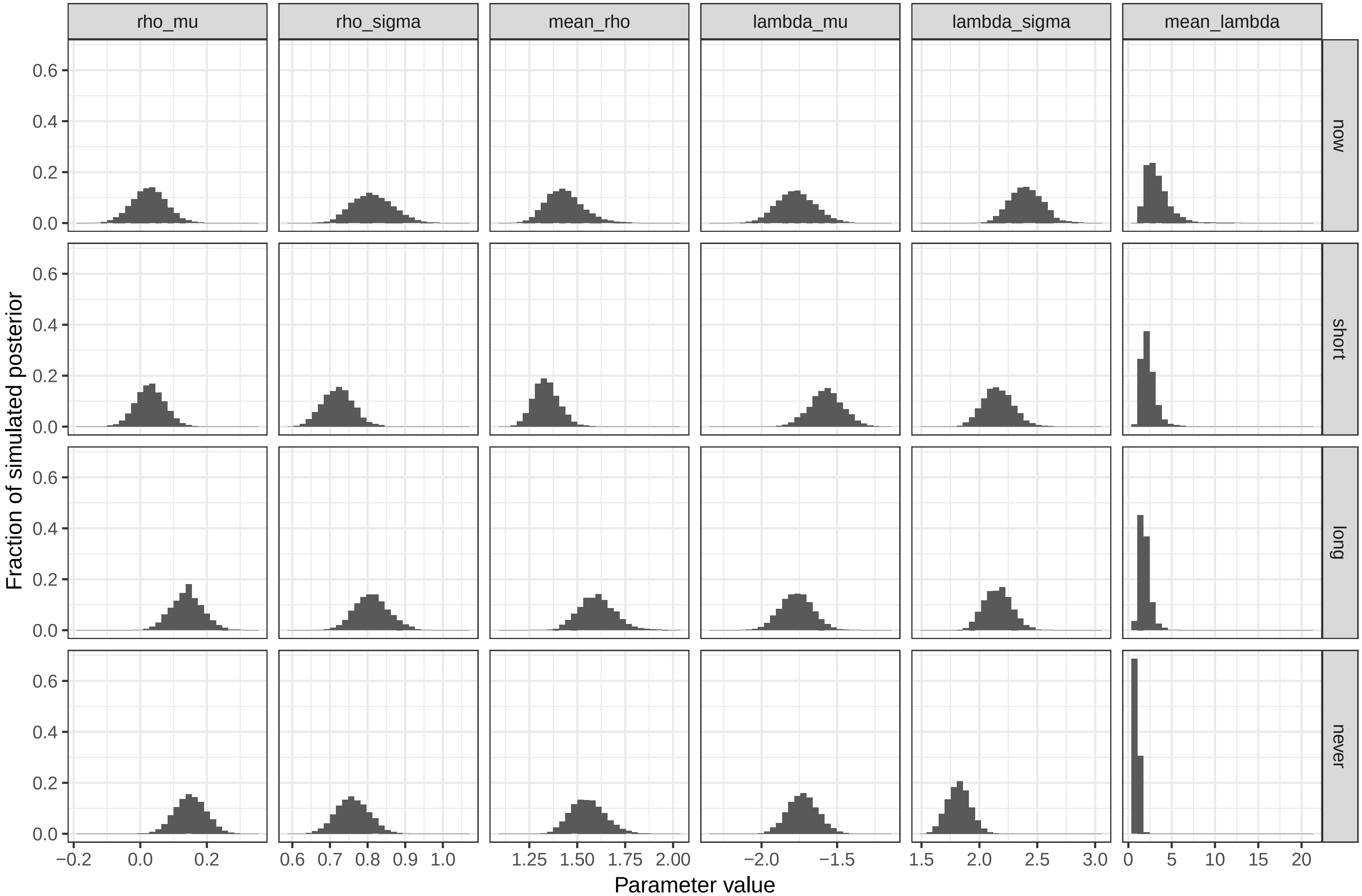

Figure 7: Posterior distribution of hyper-parameters for risk aversion and random utility

Note: Marginal distributions of the $(\mu_\rho, \mu_\lambda, \sigma_\rho, \sigma_\lambda)$ parameters for each of the four treatments (from the same estimation run as the posteriors in Figure 5) Column 3 and 6 present the implied posterior for $E[\rho]$ and $E[\lambda]$.

short as practically possible while working with a survey provider. It is, however, contrary to Zimmermann (2015) who found that even a two-day delay is sufficient to encourage more risk taking for self. The reduced form evidence of Section 4 certainly do not provide any evidence against this hypothesis. The choice model also do not provide evidence of any difference in risk aversion, but we do find that in the *now* treatment there is more heterogeneity in β, one of the weighting function parameters.

Our second hypothesis, based on previous evidence on risk-taking on behalf of self, was that there is more risk taking with a long than with a short delay. This would be in line with evidence on risk taking for self (Shelley, 1994; Önçüler, 2000; Noussair and Wu, 2006; Abdellaoui et al., 2011; Onay, La-Ornual, and Önçüler, 2013; Savadori and Mittone, 2015), but most of these also conflate the timing of information with the timing of payment. Our reduced form evidence provides evidence against this, with a precisely estimated null effect. The choice model estimates do suggest that the median participant in the *long* treatment is somewhat risk seeking, in contrast to the risk neutrality of the median participant we find in the *now* and *short* treatments, but this difference is not sufficiently strong to have effects on average risk taking (it is counteracted by a number of smaller differences in other parameters). We also formulated an hypothesis to follow up, that differences in risk taking between short and long delays are due primarily to differences in probability weighting rather than differences in risk aversion (as found for decision taking on own behalf by Abdellaoui et al. (2011)). The choice model estimates clearly contradict this hypothesis.

Our final two hypotheses were competing predictions about how decisions with no revelation of uncertainty would be closer to either no delay at all (hypothesis four), or to an imaginary extension of delay into eternity which would be closer to a long delay (hypothesis five). These hypotheses concern specific features of decision taking on behalf of others; previous studies examining decision taking for self obviously cannot hide the information completely since experimenters eventually have to pay participants. Our reduced form evidence does not speak to this hypothesis, since we did not find any differences in average risk taking between treatments. To the extent that we find any systematic differences in the choice model estimates, with respect to the ρ parameter for risk aversion we find that the *never* treatment is closer to the *long* treatment, providing some evidence for hypothesis five and against hypothesis four.

6 Concluding remarks

Using a probability based general population sample, we studied risk taking on behalf of others with different delays in the revelation of how decision making behavior affected the outcomes of recipients. Our sample is substantially larger than any of those in the meta-analysis of Batteux et al. (2019). Even so, we find very little evidence of any effect of socio-demographic variables on risk taking. Even gender, which many have found to be important for risk taking for self (e.g. the meta-analyses and reviews of Byrnes and MillerBernou, 1999; Croson and Gneezy, 2009; Charness and Gneezy, 2012) does not seem to impact risk taking on behalf of others in our study.

Examining the correlates of risk taking for others, we find that choices are much stronger correlated with self reported own preferences for risk taking than with beliefs about how willing others are to take risk. This contradicts standard altruist models of motivation and is evidence in favor of participants being motivated by more paternalistic concerns.

We do not find any average treatment effects of the randomly assigned revelation delays on risk taking for others. The reduced form evidence is a precisely estimated null effect of treatment on the number of lotteries chosen. Our experimental design ensures that this null effect results from information revelation alone, without any interaction with the time valuation of money.

A hierarchical Bayes model of rank dependent utility, allowing for heterogeneity in parameters both within and across treatment, points to a lot of heterogeneity in both probability weighting and pure risk aversion. Comparing the estimates of hyper parameters across treatments reveal that there are also some differences between treatments in *how* decisions are made. The median participant in the treatments with long delay and in the treatment where outcomes are never revealed are estimated to be more risk seeking than those facing a short delay or an immediate uncertainty revelation. This effect is not seen in average choice behavior because there are offsetting differences in the role of heterogeneity within treatment.

We believe our study open up several avenues for further research: First, our analysis document a large amount of heterogeneity. Is it possible to account for this richness? Second, we have restricted our analysis to differential delays in information revelation, keeping the date of payment constant. Much of the prior research on decision making for self also varies the delays in payment. A natural extension of our work would be to examine whether risk taking on behalf of others is affected by the delay profile of payments. Given the importance of decision making on behalf of others for many of the major challenges both for public policy, in medicine, and in raising children, we believe it is of great importance to better understand how such decisions are made.

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