Behavioral Economic Phenomena in Decision-Making for Others

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Abstract: We examine whether biases identified in the behavioral-economics literature apply in decision-making for others (DMfO). We conduct a laboratory experiment in which subjects make decision on behalf of themselves and others in eighteen tasks that measure the following biases: present-bias in time preferences, reflection effect in risk preferences, compound risk aversion, decoy effect, anchoring bias, endowment effect, and identifiable-victim bias. In our experiment, DMfO is DMfO simpliciter: unincentivized decisions made by one individual on behalf of another--the individual making decisions faces no direct costs or benefits when engaging in DMfO (as they would in a principal-agent framework or with bequest motives), and DMfO is not framed as giving advice or guessing others' behavior. Although we find that DMfO is by and large statistically indistinguishable from decisions for oneself, we identify the following self-other discrepancies: (i) willingness to pay (i.e., bids to procure goods and donations to charity) is higher in DMfO than in decisions for oneself in tasks associated with the anchoring bias, endowment effect, and identifiable-victim bias; and (ii) the propensity to give uninterpretable responses is higher in DMfO than in decisions for oneself. We also find order effects, with DMfO more similar to decisions for oneself when DMfO follows decision making for oneself. Lastly, in response to open-ended items soliciting self-reports of subjects' approach to DMfO, most subjects report having followed some version of the "Golden Rule" (e.g., deciding for others as they would for themselves) or having tried to maximize the other subject's payment or utility; very few subjects report motivations that can be construed as rivalrous.

Keywords: Decisions making for others, laboratory experiments, social preferences, anchoring bias, endowment effect, identifiable-victim bias

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1. Introduction

Many decisions are made on behalf of others. Elected officials, non-profit trustees, doctors, health-care proxies, financial advisors, and investment-fund managers routinely make decisions with the interests of constituents, clients, or stakeholders in mind. While it is hard to estimate the proportion of resources allocated by decisions made on behalf of others, it is likely substantial, with a profound impact on individual and social welfare. Also important to note is that contexts and incentives vary widely in such decisions. The performance of investment-fund managers is usually financially incentivized, while that of non-profit trustees is not; the decision-making of doctors is usually on behalf of an individual patient, while that of elected officials is on behalf of a whole community.

A recent literature is taking shape to better understand decision-making for others (DMfO), particularly in the realm of risk preferences. Here, we use DMfO to mean DMfO *simpliciter*: unincentivized decisions made by one individual on behalf of another—DMfO that is not framed as giving advice or trying to guess behavior or preferences, and in which the decision makers' material outcomes are unaffected by her decisions.[2] An aim of this paper is to better understand DMfO simpliciter; only then can its common role be identified in the varied set of complex decisions in which it is relevant. As such, we conduct a laboratory experiment to test for "self-other discrepancies"—that is, differences in DMfO and decisions individuals make for themselves—in a broad array of tasks, some related to risk preferences and others not. Our main research question is whether established decision-making biases (present bias, reflection effect, compound risk aversion, decoy effect, anchoring bias, endowment effect, and identifiable-victim bias) persist in DMfO. We also consider whether individuals' decisions for themselves align with how they want others to decide for them, and we analyze self-reports of how individuals approach DMfO.

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[2] We consider the examples noted in the first paragraph to involve DMfO, but to be confounded by other concerns. For instance, some of the examples can be characterized as principal-agent scenarios, whereby one individual empowers another to make decisions on her behalf, with the former only able to observe the outcomes of the latter's decisions, not the decisions themselves. Other important economic decisions related to DMfO include social preferences, whereby an individual's decisions are influenced by others' outcomes; and externalities, whereby market participants' decisions impact non-market participants' outcomes. Though social preferences and externalities do not directly include DMfO, they may be impacted by DMfO. For example, it may be that altruism depends on the alignment of others' decisions for themselves and decisions one would make on their behalf; or that externalities can be framed as an indirect form of DMfO.

This research is important for four reasons. First, many of these decision-making biases have not been previously studied in the DMfO literature. Second, self-other discrepancies may suggest a role for DMfO in improving the decisions we make for ourselves, and vice versa. Third, identifying self-other discrepancies may improve our understanding of the nature of biases; for example, a large self-other discrepancy may suggest a bias is more emotionally based, whereas a small discrepancy may suggest it is more cognitively based. Fourth, our results may help answer questions critical to any future modeling of DMfO; for example, in the absence of incentives, do individuals tend to approach DMfO as surrogates or rivals?

We replicate four of the seven biases (present bias, compound risk aversion, anchoring bias, and endowment effect) in decisions for oneself, and for these four biases we identify the bias in DMfO; the other three biases (reflection effect, decoy effect, and identifiable-victim bias) are identified in neither decisions for oneself nor DMfO. Our results indicate that DMfO simpliciter is by and large statistically indistinguishable from decisions for oneself for most decisions in most tasks. That said, we find two main self-other discrepancies. First, willingness-to-pay is higher in DMfO in the tasks associated with the anchoring bias, endowment effect, and identifiable-victim bias; in other words, subjects make higher donations to charity and higher bids to procure goods on behalf of others than for themselves. Second, subjects are more likely to exhibit uninterpretable preferences in DMfO than in decisions for themselves. We also find evidence of order effects, with DMfO more similar to decisions for oneself when it follows them, and that individuals' decisions for themselves align with how they want others to decide for them. Lastly, in response to open-ended items soliciting self-reports of subjects' decision-making process when making decisions on behalf of others, approximately 60 percent of subjects report having followed some version of the "Golden Rule" (e.g., deciding for others as they would for themselves), and approximately 40 percent report having tried to maximize the other subject's payment or utility; very few subjects report motivations that can be construed as rivalrous. One way to interpret our findings is that individuals approach DMfO similarly to decisions for themselves except with a greater willingness to spend money and with less attention; reduced attention is exhibited by the greater extent of uninterpretable preferences exhibited in DMfO and the tendency for decisions for oneself to anchor subsequent DMfO but not vice versa.

2. Literature review

Below, we motivate our choice of behavioral biases and provide a brief review of each bias. We then provide a brief review of the DMfO literature.

2.1. The behavioral biases

The standard microeconomic theory of decision-making can be broadly divided into the following domains: choice under certainty, choice under uncertainty, intertemporal choice, and choice in social interactions. Numerous behavioral biases have been established within each of these domains, and we include in our experiment at least one such bias from each domain.[3] Below, we briefly review the literature for each bias we include in our experiment.

In standard choice under certainty, preferences are assumed to be complete and stable vis-à-vis changes in framing or elicitation procedures. In behavioral economics, the anchoring bias is the phenomenon whereby estimates and valuations are biased toward an (irrelevant) initial value (Furnham & Boo, 2011; Tversky & Kahneman, 1974). For example, Ariely et al. (2003) ask subjects to write down the last two digits of their social security numbers and then indicate their willingness to pay for a variety of products; social security numbers and willingness to pay are strongly positively correlated. The endowment effect is the phenomenon whereby an individual's ownership of a good increases her valuation of it (Thaler, 1980). For example, when subjects randomly are given a mug in an experiment, the minimum price at which they are willing to sell it exceeds the maximum price at which those subjects not given a mug are willing to pay to buy one (Thaler, 1980). The decoy effect is the phenomenon whereby preferences are biased toward one good over another if the former strictly dominates a third good on the menu (Huber et al., 1982). For example, Huber et al. (1982) present subjects with the hypothetical choice of purchasing a six-pack of beer with an average quality rating of 50 for $1.80 or one with an average quality rating of 70 for $2.60; the choice of the latter option significantly increases when a third option with an average quality rating of 70 and price of $3.00 is also presented.[4]

[3] Our other criteria for inclusion are that the biases be feasible to effectively test for in DMfO, and that the tasks used to identify the biases be, in toto, practicable in a single lab session to avoid attrition and subject fatigue.

[4] Herne (1999) extends the decoy effect to choices under uncertainty. She finds that, on average, lottery x is preferred to lottery y when the two are presented with a third lottery z that is strictly dominated by x, but that y is preferred to x when z is strictly dominated by y. In our experiment, we test for the decoy effect in choices under uncertainty, as much of the DMfO literature is concerned with risk.

In standard choice under uncertainty, individuals are typically categorized as being: risk neutral if they are indifferent between a lottery and its expected value, risk averse if they prefer the expected value of a lottery to the lottery itself, and risk-seeking if they prefer a lottery to its expected value. In behavioral economics, the reflection effect is a feature of prospect theory and refers to the phenomenon of being risk-averse in gains and risk-seeking in losses (Kahneman & Tversky, 1979). Compound risk aversion is the phenomenon of having a higher certainty equivalent for a simple lottery than a compound lottery—i.e., a lottery with outcomes that are themselves lotteries—with the same expected value (Abdellaoui et al., 2015; Halevy, 2007).

In the standard model of intertemporal choice, discounting refers to the preference for sooner rather than later utility. In behavioral economics, present bias is the (additional) preference for utility received now (versus any future period)—i.e., the phenomenon of intensified discounting of later relative to current utility (Ainslie, 1975). Present bias is identified in experiments when an individual prefers a current reward of *x* to a larger reward of *x'* in *t* periods, but prefers a reward of *x'* in *t*+*n* periods to a reward of x in *n* periods (Benhabib et al., 2010).

Lastly, in the standard model of social interactions, individuals are assumed to behave strategically so as to maximize their own payoffs. In behavioral economics, social preferences or altruism describe the phenomenon whereby individuals' behavior depends on both their own and others' payoffs. For example, in the absence of material incentives to do so, subjects are on average willing to give a substantial portion of their endowments to an anonymous other subject in dictator games (Forsythe et al., 1994). The identifiable-victim bias is the phenomenon of a greater willingness to help identified rather than unidentified others (Jenni & Loewenstein, 1997). For example, subjects give more in anonymous dictator games when they know the receiver's subject number than when they do not (Small & Loewenstein, 2003).

2.2. DMfO

The majority of the DMfO literature considers risk-taking on behalf of others. These (and other DMfO) studies vary widely in their methodologies, with some using only hypothetical questions, some framing DMfO as advice, some framing DMfO as predictions of others' behavior, some providing financial incentives for decision-makers, some involving decisions for or by groups, etc. In the DMfO-risk literature, the reported self-other discrepancies are similarly varied: some find lower risk-aversion in DMfO (Agranov et al., 2014; Chakravarty et al., 2011; Lee, 2018; Polman, 2012; Pollmann et al., 2014), and some higher (Bolton & Ockenfels, 2010; Charness & Jackson, 2009; Eriksen & Kvaløy, 2010; Eriksen et al., 2017; Füllbrunn & Luhan, 2015; Pahlke et al., 2015; Reynolds et al., 2009). Perhaps unsurprisingly, given such mixed results, Ericksen et al. (2017) find that the variance in measured risk aversion is greater in DMfo than decisions for oneself. For a thorough review of this literature see Polman (2018).[5]

A few DMfO studies consider other types of decisions. Reported self-other discrepancies in time preferences are again mixed: some find lower rates of discounting in DMfO (Shapiro, 2010), and some greater (de Oliveira & Jacobson, 2016). Present bias has not been considered in DMfO experiments, but beliefs elicited about one's own and others' behavior indicates that individuals correctly predict others' present bias but underestimate their own (Fedyk, 2018). Unlike in the cases of risk aversion and discounting, reported self-other discrepancies in loss aversion are consistent across studies, with loss aversion lower in DMfO than in decisions for oneself (Andersson et al., 2014; Füllbrunn & Luhan, 2017; Pahlke et al., 2012; Polman, 2012). Other biases for which self-other discrepancies have been examined are ambiguity aversion and the identifiable-victim bias; for neither bias is a self-other discrepancy identified (König-Kersting & Trautmann, 2016; Kogut & Beyth-Marom, 2008).[6]

3. Experimental Design

To identify self-other discrepancies in decision-making biases, we conducted a laboratory experiment at Santa Clara University (SCU) in the spring quarter of 2017. The 190 subjects were SCU students, recruited by email. The experiment was administered using Qualtrics in a classroom equipped with down-view computer monitors. The experiment lasted 60 minutes,

[5] Self-other discrepancies in risk are identified using modified DMfO environments. In Charness & Jackson (2009), Pahlke et al. (2012), and Pahlke et al. (2015), subjects make decisions on behalf of a group of two, of which they are members. In Füllbrunn & Luhan (2015) and Reynolds et al. (2009), subjects make decisions on behalf of a group (4-6 members), of which they are not members. In Eriksen & Kvaloy (2010), there is no DMfO at all, rather professional financial advisors (who often engage in DMfO as part of their work) make decisions on behalf of themselves and those decisions are compared to those of students.

[6] In Kogut & Beyth-Marom (2008), subjects report how much they would expect the "average student" to donate in hypothetical scenarios.

with average payments of $28, and minimum (maximum) payments of $20 ($36).[7] There were 7 sessions, the smallest of which had 17 subjects and the largest of which had 39.

In brief, our experimental procedure was as follows (additional details provided below). Each subject:

- Completed an informed-consent form and read general instructions
- Completed 18 incentivized tasks in each of three environments in the following order (randomly assigned by subject):
  - (Control) Environment A (on subject's own behalf) then B (on behalf of another subject) then C (as the subject would want another subject to decide on the subject's behalf)[8]
  - (Treatment) Environment B then A then C
- Completed a questionnaire that included demographic items and items soliciting an explanation of the subject's behavior in and approach to the different environments
- Received payment for one randomly selected task from Environment A or B (or payment instructions if the task involved a future payment) and exited the session

Having each subject complete both Environments A and B and randomizing the order enabled identification of order effects. Appendix A presents the full Qualtrics instructions of the version in which Environment B was encountered first. The 3 decision-making environments, 18 tasks, questionnaire items, and payment details are described below.

3.1. Decision-making environments

3.1.1. Environment A: Decision-making for oneself

In the instructions for Environment A, subjects were informed that they would complete 18 tasks in which they would make decisions for themselves. They were informed that payments would be based on one randomly selected task—the "payment task"—from Environment A or B; and if the payment task was from Environment A, then their payment would be $20 plus (or minus)

[7] If subjects showed up but could not be seated because they were late or the session was full, they were given a $5 show-up fee and rescheduled for a later session.
[8] In the instructions for each environment, environments were referred to as "activities," with no reference to "A," "B," or "C." Also, tasks were not explicitly numbered when subjects were completing them.

their cash earnings (or losses) from the payment task, in addition to any non-cash items earned in the task.

3.1.2. Environment B: DMfO

In the instructions for Environment B, subjects were informed that they would complete 18 tasks on behalf of a randomly-assigned, anonymous subject from the same session (Participant X); and a different randomly-assigned, anonymous subject (Participant Z) from the same session would complete the 18 tasks on the subject's behalf. Subjects were ensured that they and Participants X and Z would not be informed of each other's identities. Subjects were informed that if the payment task was from Environment B, then their payment would be $20 plus (or minus) the cash earnings (or losses) from the payment task, in addition to any non-cash items earned in the task completed on their behalf by Participant Z; and Participant X's payment would be $20 plus (or minus) the cash earnings (or losses) from the payment task, in addition to any non-cash items earned in the task that the subject completed on Participant X's behalf.

3.1.3. Environment C: Self-as-other

In the instructions for Environment C, subjects were asked to complete the 18 tasks as they would want someone else to complete the tasks on their behalf; specifically, to indicate how they would want Participant Z to have completed the tasks on their behalf. Subjects were informed that their decisions in Environment C would not impact their or any other participant's payments.

3.2. The 18 tasks

In sections 3.2.1-3.2.7 below, we list the tasks that correspond to each of the seven biases, and how each bias is identified from the corresponding task(s). Because we were attempting to test for a broad swath of biases, our measures for individual biases are coarse in some cases, which may bias against identification of the biases and of self-other discrepancies in the biases. The 18 tasks appeared in the order below in all three environments and for all subjects. We opted, for the sake of simplicity, not to randomize the tasks' order.

For ease of presentation, the descriptions below correspond to Environment A, in which subjects made decisions for themselves. Extrapolation to Environment B is generally achieved by adding the phrase “on behalf of Participant X” to the Environment-A instructions. For example, if an item reads “please choose Option A or Option B” in Environment A, then it reads “please choose Option A or Option B on behalf of Participant X” in Environment B. In Environment C, it reads “please indicate whether you would want Participant Z to choose Option A or Option B on your behalf;” any exceptions to such extrapolations are noted. To minimize any confusion about the environment being faced in a given task, subjects were reminded on each task’s screen of whom the decision was being made for and how payments would be determined.

3.2.1. Tasks 1-4: Present bias and time preferences

Each of Tasks 1-4 consisted of a multiple-price list (MPL) in which subjects chose between a smaller, sooner reward (SSR) and a larger, later reward (LLR). In Task 1, the LLR was $10 in 16 days; the SSR was payable on the day of the experiment and ranged in $1 increments from $0 to $10. Subjects indicated whether they prefer $10 in 16 days or $0 today, $10 in 16 days or $1 today, …, and whether they prefer $10 in 16 days or $10 today. Task 3 was similar but with an LLR of $25 in 10 days and thus an SSR ranging in $1 increments from $0 to $25. Tasks 2 and 4 both had front-end delays. In Task 2, the LLR was $10 in 30 days, and the SSR was in 16 days. In Task 4, the LLR was $25 in 24 days, and the SSR was in 10 days.

The MPL method used in Tasks 1-4 allows, for each task, identification of the switchpoint-SSR (SSSR) such that subjects prefer SSSR to the LLR, but prefer the LLR to (SSR - $1). For time preferences to be interpretable through the MPL, subjects must choose the LLR in the first question (when the SSR is $0), continue choosing it until they switch to the SSR, and then choose the SSR thereafter. In our analyses of time preference, we drop subjects whose responses do not follow this pattern.

For each Task i, the corresponding discount rate (DR_i) can be calculated by 1 - ($SSSR_i/LLR_i$).[9] A subject’s overall time preferences are measured by the subject’s average

[9] Given the MPLs’ $1-increments, the true dollar value of the switchpoint could be any value between SSSR and (SSSR-0.99). We approximate the DR by assuming the SSSR as the true dollar value of the switchpoint.

discount rate over the four tasks. Ideally, Task 2 (4) should be identical to Task 1 (3), except with a front-end delay. Unfortunately, scheduling conflicts did not allow us to time payments perfectly. We therefore present two alternative methods for identifying present-bias. The potentially confounded method identifies present-bias when $DR_1 > DR_2$ and $DR_3 > DR_4$, with a subject's overall present-bias as the average of the two binary variables corresponding to whether each of these inequalities holds. This method is potentially confounded because if $DR_1 > DR_2$, it may simply be because the delay between the SSR and LLR is longer in Task 1 than Task 2. The more conservative method identifies present-bias only when $DR_3 > DR_4$.

3.2.2. Tasks 5-8: Reflection effect and risk preferences

In Tasks 5-8, subjects faced MPLs in which they chose between a lottery and a series of fixed payments. In Task 5 (6), the lotteries were a 25% (50%) chance of a $20 gain and a 75% (50%) chance of $0 gain.[10] The fixed payments ranged in $1 increments from $0 to $20 in both tasks. In Task 7 (8), the lotteries were a 25% (50%) chance of a $20 loss. The fixed losses ranged in $1 increments from $0 to $20 in both tasks. Because subjects were endowed with $20 at the beginning of the task, any losses in Tasks 7 and 8 would be deducted from the $20-endowment, rendering impossible a net loss.

Tasks 5-8 allow for the identification of a switchpoint, hereafter denoted the certainty equivalent (CE_i), for each lottery such that subjects prefer the CE to the lottery, but prefer the lottery to a fixed payment of (CE – $1).[11] Reponses had to follow a pattern similar to that described in section 3.2.1 to be interpretable (i.e., in Tasks 5 & 6, choosing the lottery in the first question, continuing to choose the lottery until switching to the fixed gain, and then choosing the fixed gain thereafter; and in Tasks 7 & 8, choosing the fixed loss in the first question, continuing to choose the fixed loss until switching to the lottery, and then choosing the lottery thereafter). In our analyses of risk preferences, we drop subjects whose responses do not follow this pattern.

For each Task i, the corresponding proxy of risk preferences (RP_i) can be calculated by CE_i/EV_i, where EV_i is the expected value of the lottery. A subject's overall risk preferences in gains

[10] Hereafter, we will only present the probability of non-zero gains and losses in a given lottery. For example, the Task 5 lottery would be described as a 25% chance of a $20 gain.
[11] Given the MPLs' $1-increments, the true dollar value of the switchpoint could be any value between CE and (CE-0.99). We assume CE to be the true dollar value of the switchpoint.

(losses) are calculated by taking the average of RP_5 and RP_6 (RP_7 and RP_8). The reflection effect can be identified twice: in Tasks 5 & 7 if $CE_5 < 5$ (risk averse in gains) and $|CE_7| < 5$ (risk seeking in losses), and again in Tasks 6 & 8 if $CE_6 < 10$ (risk averse in gains) and $|CE_8| < 10$ (risk seeking in losses). A subject's overall reflection effect is coded as one if the reflection effect is identified twice, one-half if it is identified once, and zero otherwise.

3.2.3. Tasks 9-10: Compound risk aversion

In Tasks 9 & 10, subjects were informed of a bucket at the front of the room containing 90 balls colored blue, pink, and yellow. Subjects were informed that 30 of the balls were pink, and that the remaining 60 balls were some combination of blue and yellow, with all combinations equally likely. Subjects were informed that a ball would be drawn at the end of the session. In Task 9, subjects were asked to choose either (Option A) a payment of $10 if a pink ball is drawn or (Option B) a payment of $10 if a blue ball is drawn. In Task 10, subjects were asked to choose either (Option A) a payment of $10 if a pink or yellow ball is drawn or (Option B) a payment of $10 if a blue or yellow ball is drawn.

In Task 9, Option A represents a simple lottery: a 1/3 probability of gaining $10. Option B represents an actuarially equivalent compound lottery: a 1/3 probability of gaining $10, generated from one lottery over the combination of blue and yellow balls (i.e., {0 blue, 60 yellow}, {1 blue, 59 yellow}, … ,{60 blue, 0 yellow}) and a second lottery over drawing a blue ball. In Task 10, Option B represents a simple lottery and Option A the actuarially equivalent compound lottery. Compound risk aversion is identified if subjects choose Option A in Task 9 and Option B in Task 10.

3.2.4. Tasks 11-14: Decoy effect

In Tasks 11 & 12, subjects had to choose between three options: A, B, and C, each of which was a lottery. Options A and B were the same in Tasks 11 & 12: a 40% chance of an $8 gain and an 80% chance of a $4 gain, respectively. But Option C varied in Tasks 11 & 12: a 35% chance of a $7 gain and a 75% chance of a $3 gain, respectively. Option C is the decoy, and is strictly dominated by a different option in each task: Option A in Task 11 and B in Task 12. Tasks 13 & 14 were structured similarly: Option A offered a 70% chance of a $5 gain, Option B a 30% chance of a $10 gain, and Option C a 65% chance of a $4 gain in Task 13 and a 25%

chance of a $9 gain in Task 14. The decoy effect is identified twice: once if Option A is chosen in Task 11 and B in Task 12, and again if Option A is chosen in Task 13 and B in Task 14. A subject's overall decoy effect is coded as one if the decoy effect is identified twice, one-half if it is identified once, and zero otherwise. We also code a subject's overall decoy effect using only the Task-13 and -14 choices as a more conservative alternative because a risk neutral subject would be indifferent between Options A and B in Tasks 11 and 12.

3.2.5. Task 15: Anchoring bias

In Task 15, subjects' attention was directed to a "Uniware 16 Oz Stainless Steel Travel Mug" that was at the front of the room; the mug was displayed so it was visible to all subjects. Subjects were then asked if they thought the retail price of the mug was more or less than a specified value (the anchor) and to indicate their best guess of the mug's retail price. Next, subjects were informed that the mug would be auctioned and were asked to enter a bid (the most they would be willing to pay) for the mug in the auction.[12] Subjects indicated their estimates of the mug's price and their bids on a slider that ranged in $0.10 increments from $0 to $20. Because subjects were endowed with $20 at the beginning of the task, their bids would be deducted from the $20-endowment, rendering impossible a net loss. Subjects were randomized into receiving a $5- or $15-anchor. The anchoring bias is identified when subjects who receive the $5-anchor report significantly lower bids than subjects who receive the $15-anchor.

3.2.6. Tasks 16-17: Endowment effect

In Task 16, subjects were shown a picture of SCU stickers and a chocolate bar on their computer screens. Subjects were randomized into having a checkmark to the left of the stickers or the chocolate bar and told that the checkmark indicated which item they would receive at the end of the session. Subjects were then given the opportunity to sell the good they were endowed with to a randomly-assigned, anonymous subject in the session who was not endowed

[12] In Environment B, each subject was asked to enter a bid for the mug on behalf of Participant X; specifically, to enter the most the subject thought Participant X was willing to pay for the mug. However, in Environment B as in A, subjects indicated whether they thought the mug's retail price was more or less than the anchor and gave their own estimates of the retail price of the mug; they did not answer these two items on behalf of Participant X. Thus, estimates of the retail price of the mug were not used to identify the anchoring bias.

with that good. Specifically, they had to enter the minimum amount (ask) they would be willing to accept to sell the good. They were told that the transaction would occur at the average of the ask and the maximum willingness to pay (bid) entered by the potential buyer if the bid > the ask. Subjects were instructed to enter an ask of $20 if they did not want to sell the good. In Task 17, subjects were given the opportunity to buy the good they were not endowed with from another randomly-assigned, anonymous subject who was endowed with the good. Subjects had to enter a bid; and if the bid > the ask entered by the potential seller, the transaction occurred at the average of the bid and ask. Subjects were instructed to enter a bid of $0 if they did not want to buy the good. Subjects indicated their bids and asks on a slider that ranged in $0.10 increments from $0 to $20. Because subjects were endowed with $20 at the beginning of the task, their bids would be deducted from the $20-endowment, rendering impossible a net loss. The endowment effect is identified when the difference between asks and bids is positive on average.

3.2.7. Task 18: Identifiable victim bias and charitability

In Task 18, subjects were given information about Save the Children India. All subjects were informed that "Below is some information from Save the Children's website about their work in India." The first and third paragraphs were identical for all subjects: the first included basic information about the charity and the problems it addresses and the third discussed the accomplishments of the charity. The second varied depending on whether the subject was randomized into the identifiable-victim condition (and shown a picture of a young Indian girl and given her name and specific information about her plight) or the statistical-victim condition (and given statistical information about children's plight in India). Subjects were given the opportunity to donate any portion of their $20-endowment to the charity. We use two alternatives for measuring charitability: the donation size and a dummy variable indicating a positive donation. Using either measure of charitability, the identifiable-victim bias is identified when the average charitability of identifiable-victim-condition subjects is greater than that of statistical-victim-condition subjects.[13]

[13] This design is a strong test of the identifiable-victim bias, as the donations in both conditions are to the same entity (Save the Children India). The design closely follows that of Small & Loewenstein (2007), a psychology study. In that paper's identifiable-victim condition, though, it is claimed that all donations will go to the girl in the photo and her family. We could not make such a claim, as deception is not permitted in economics experiments. As such, we do not expect to observe as large an identifiable-victim bias as observed in Small & Loewenstein (2007).

3.3. Questionnaire

Finally, subjects completed a questionnaire. In addition to basic demographic items, the questionnaire contained open-ended items asking subjects to explain their decision-making approach in the different environments. These items are used to classify subjects and to test whether behavior varies by self-reported approach.

3.4. Payments

At the beginning of the instructions, subjects were informed that: (i) they would receive a cash payment at the end of the session or on a future date based upon the decisions they and others made during the session; (ii) any future payment would be made in cash on campus on and after a specified date, and that detailed instructions for payment pick-up would be distributed at the end of the session; (iii), they may receive non-cash items at the end of the session; and (iv) all cash payments and any non-cash items would be distributed in a manner ensuring their anonymity.

Recall that in the instructions for Environments A and B, subjects were informed that one payment task would be randomly chosen from the 36 tasks in those environments. At the end of the session, the experimenter displayed a document on a screen at the front of the room that informed the subjects of the methodology for choosing the payment task. It worked as follows: balls numbered 1 to 18 were placed in a bingo spinner and one ball was chosen. If the number on the chosen ball was odd (even), then the payment task would be from Environment A (B). The chosen ball was then put back in the spinner, and another ball was chosen to determine the payment task (the numbering scheme of the tasks was on the projected document). Any additional necessary randomizations, for example, lottery implementation, were achieved using the spinner and an appropriate number of balls.

In none of the sessions did the randomization result in the payment task being Task 15, 16, or 17, so all payments were in cash. Cash payments for the day of the experiment were distributed by subject-number as subjects exited the session. Envelopes containing cash were prepared for each subject number, and subjects lined up and gave their subject number to an administrator to receive their envelope. If the payment task was one of Tasks 1-4 and the subject had chosen the future payment, the envelope distributed at the end of the session contained a receipt indicating the on-campus location for picking up the future payment and the date and time after which the future payment would be available. To receive the future payment, the subject had to exchange the receipt for an envelope marked with her subject number containing the payment.

4. Results

To identify self-other discrepancies, we compare the Environment-A choices of the 97 subjects who completed Environment A first to the Environment-B choices of the 93 subjects who completed Environment B first in section 4.1. Because we use multiple outcomes to identify self-other discrepancies in the same bias, we use Bonferroni adjustments—considered the most conservative approach (List et al., 2016)—to control the familywise error rate at a level not exceeding 0.05. Specifically, we apply the Bonferroni adjustments to the alphas, not the *p*-values; i.e., throughout the paper we report raw *p*-values, but significance is not indicated by the raw *p*-value being less than the standard Type-I error rate alpha of 0.05. Instead, we derive the Bonferroni-adjusted alphas by dividing 0.05 by the number of tests in a family, defining families as all outcomes used to identify a particular bias.[14] In the anchoring-bias tasks, we measure self-other discrepancies for three outcomes (bid with a $5-anchor, bid with a $15-anchor, and anchoring bias in bids); as such, we consider self-other discrepancies significant if their unadjusted *p*-value is less than 0.017 (= 0.05/3). In the endowment-effect tasks, we measure self-other discrepancies for three outcomes (bids, asks, and endowment effect); as such, we consider self-other discrepancies significant if their unadjusted *p*-value is less than 0.017 (= 0.05/3). In the identifiable-victim-bias tasks, we measure self-other discrepancies for two outcomes (mean donations and the proportion of positive donations); as such, we consider self-other discrepancies significant if their unadjusted *p*-value is less than 0.025 (= 0.05/2). For time discount rates, present bias, risk preferences, the reflection effect, compound risk aversion, and the decoy effect, only one measure is used to identify self-other discrepancies; as such, no adjustment is necessary.

[14] A more conservative approach would be to define the family as including all outcomes for which we attempted to identify self-other discrepancies. This would imply that all tests of self-other discrepancies (including uninterpretable preferences as defined in sections 3.2.1 and 3.2.2) should be evaluated with a significance level of 0.05/15 = 0.003. We do not use this approach because our ultimate goal is not to identify whether self-other discrepancies exist at all, but whether specific biases exhibit them. The only self-other discrepancy that is rendered insignificant using the more conservative definition of family is that in 4.1.4.

It should be noted that choices made in the second environment encountered are potentially confounded by order effects and thus are inappropriate for across-subject identification of self-other discrepancies. As such, in the main text, we do not present the analogous comparisons to those in section 4.1 with choices made in the second environment encountered; for the interested reader, these comparisons and the comparisons pooling the first and second environments are included along with first-environment results in Tables 1 and 3-6. In section 4.2, we consider order effects. Specifically, we compare the Environment-A choices of the 97 subjects who completed Environment A first to the Environment-A choices of the 93 subjects who completed Environment A second; and we compare the Environment-B choices of the 93 subjects who completed Environment B first to the Environment-B choices of the 97 subjects who completed Environment B second. Lastly, in section 4.3, we analyze the self-reported approaches to DMfO and conduct subgroup analyses based on them.[15] In sections 4.2 and 4.3, Bonferroni adjustments are made where appropriate and described therein.

4.1. Self-other discrepancies in the first environment

In none of the seven biases do we find evidence of self-other discrepancies. This does not mean that we find no significant differences between DMfO and decisions for oneself. Significant self-other discrepancies are identified in the tasks associated with the anchoring bias, endowment effect, and identifiable-victim bias (Tasks 15-18), with a common theme: an increased willingness to pay in Environment B relative to A. Significant self-other discrepancies are not identified in the tasks associated with other biases (i.e., present bias and discount rates, reflection effect and risk preferences, compound risk aversion, and decoy effect); these results are presented in Table 1. The only exception is that the propensity to exhibit uninterpretable preferences in the MPLs in Tasks 1-8 is higher in Environment B than A. It should be noted that when we do not identify significant self-other discrepancies, it is not necessarily the case that no self-other discrepancy exists. It may be that our instruments are not sufficiently sensitive, or our sample not sufficiently large to identify these discrepancies. To summarize, Table 2 lists the biases, whether they are identified in Environment A and/or

[15] Environment-C results (not presented) are not statistically distinguishable from Environment-A. This may because there is no difference between how subjects choose for themselves and how they want others to choose for them, or because of order effects. Environment-C results are available upon request from the authors.

Environment B (restricting the data to the first environment encountered), and whether self-other discrepancies are identified in the biases and/or the tasks associated with the biases. Significant self-other discrepancies are presented below.

4.1.1. Anchoring bias in the first environment

Of the 97 subjects who completed Environment A first, 49 were randomized to the \$5-anchor and 48 to the \$15-anchor. As confirmation of anchoring bias in decisions for oneself, these subjects' bids differ significantly (\$4.70 versus \$6.81, $p = 0.045$). Our first novel result is that the anchoring bias persists in DMfO. Of the 93 subjects who completed Environment B first, 41 were randomized to the \$5-anchor and 52 to the \$15-anchor; the former's bids are significantly lower than the latter's (\$7.50 versus \$10.72, $p = 0.001$).

Further, we find that bids are significantly greater in Environment B than A (\$5-anchor: \$7.50 versus \$4.70, $p = 0.000$; \$15-anchor: \$10.72 versus \$6.81, $p = 0.000$). The self-other discrepancy in the magnitude of the anchoring bias, as measured by the difference in bids with a \$15- versus \$5-anchor in Environment A versus B, is insignificant (\$2.11 versus \$3.21, $p = 0.430$); this is also the case if we compare the Environment-A difference between \$15- and \$5-anchor bids divided by the Environment-A mean bid to the Environment-B difference between \$15- and \$5-anchor bids divided by the Environment-B mean bid (0.367 versus 0.349, $p = 0.917$). In sum, the lack of a self-other discrepancy in the magnitude of the anchoring bias masks that bids with both anchors are greater in DMfO than in decisions for oneself. See Table 3.

4.1.2. Endowment effect in the first environment

Of the 97 subjects who completed Environment A first, 50 were endowed with a chocolate bar and 47 with stickers. Pooling these subjects together, we find evidence of the endowment effect in decisions for oneself: asks are significantly greater than bids (\$5.42 versus \$1.44, $p = 0.000$).[16] We also find that the endowment effect persists in DMfO. Of the 93 subjects who

[16] Average asks and bids are presented separately for chocolate bars and stickers in Table 4. We test the equality of asks for chocolate bars and stickers for those in the same environment and order, resulting in four separate tests. The Bonferroni adjusted alpha is 0.0125 (=0.05/4). In none of the tests can we reject the equality of asks for chocolate and asks for stickers (see Column 3 of Table 4). In the

completed Environment B first, 46 were endowed with a chocolate bar and 47 with stickers. Pooling these subjects together, we find that asks are significantly greater than bids ($5.23 versus $3.33, p = 0.006).

Further, while asks are statistically indistinguishable in Environments A and B ($5.42 versus $5.23, p = 0.814), bids are significantly lower in Environment A than B ($1.44 versus $3.33, p = 0.000). The self-other discrepancy in the magnitude of the endowment effect is statistically insignificant with the Bonferroni-adjusted alpha ($1.91 versus $3.98, p = 0.026). It is interesting to note that we do not find evidence that the endowment effect (a bias predicated on ownership) is weaker—or that subjects' asks for the endowed item are less—in DMfO than in decisions for themselves; similarly, it is interesting to note that subjects are willing to pay more for the non-endowed item in DMfO than in decisions for themselves. See Table 4.

4.1.3. Identifiable-victim bias in the first environment

Of the 97 (93) subjects who completed Environment A (B) first, 43 (41) were randomly assigned to the identifiable-victim condition and 54 (52) to the statistical-victim condition. No evidence of identifiable-victim bias emerges in decisions for oneself, nor DMfO. In both environments, charitability (measured by the propensity to donate and mean donations) is statistically indistinguishable for the identifiable- and statistical-victim conditions (Environment A: 0.674 versus 0.741, p = 0.479 and $5.49 versus $5.74, p = 0.851; and Environment B: 0.902 versus 0.904, p = 0.982 and $6.96 versus $7.53, p = 0.679).[17]

While the identifiable-victim bias is not observed, there is evidence of self-other discrepancies in charitability. Pooling subjects in both conditions, the propensity to donate is significantly greater in Environment B than A (0.903 versus 0.711, p = 0.001); no such pattern exists for mean donations ($7.28 versus $5.62, p = 0.081). See Table 5.

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corresponding tests of the equality of bids for chocolate bars and stickers, we cannot reject equality in three of the four tests; for subjects who encounter Environment A first, Environment A bids for chocolate bars are statistically significantly higher for than for stickers (see Column 6 of Table 4). The same results obtain if the Bonferroni adjustments consider data from the first and second environments separately, using an adjusted alpha of 0.025 (= 0.05/2).

[17] As noted in footnote 12, this result is not a surprise, as our identifiable-victim-bias measure is a strong test of the bias that was made more so because deception is not used in economics experiments.

4.1.4. Uninterpretable preferences in the first environment

Recall that discount rates (Tasks 1-4) and risk preferences (Tasks 5-8) are measured using MPLs. For example, in Task 1, subjects choose Option A or B in 11 distinct questions. In all 11 questions, Option A is to receive $10 in 16 days, while Option B varies in $1-increments from receiving $0 to $10 today. To deduce a subject's discount rate, she must have chosen Option A in the first question, Option A in subsequent questions until switching to Option B, and then Option B thereafter (the later a subject switches, the lower her deduced discount rate); any other behavior would render her preferences uninterpretable. Overall uninterpretability is measured as the proportion of all MPLs completed by a subject that exhibit uninterpretable preferences.

We find significant self-other discrepancies in uninterpretability: it is significantly lower for those in Environment A than B (0.028 versus 0.090, p = 0.008). This might suggest that subjects pay less attention in DMfO than in decisions for oneself. See Table 6.

4.2. Order effects

Below, we present evidence that the order in which subjects encounter the environments impacts their choices. Specifically, DMfO more closely aligns with decisions for oneself when subjects encounter Environment A before B.

4.2.1. Anchoring-bias order effects

Figure 1 shows the distributions of bids for the mug by anchor, environment, and order of environment. An order effect is there visualized. Specifically, the proportion of zero bids is distinctively low in DMfO for subjects who encounter Environment B first. Pooling those with a $5- and $15-anchor, the proportion of subjects who make positive bids for the mug in Environment B is significantly higher for subjects who encounter Environment B first rather than second (0.989 versus 0.887, p = 0.004); a similar pattern holds for mean bids ($9.30 versus $7.17, p = 0.005); the Bonferroni-adjusted alpha for these tests is 0.025 (= 0.05/2), and means are presented in Table 3. A Kolmogorov-Smirnov test rejects the equality of the distribution of Environment-B bids when Environment B is encountered first versus second (p = 0.017). No such pattern holds for Environment-A bids when comparing those who encounter Environment A first rather than second (proportion of positive-bids: 0.784 versus 0.828, p = 0.442; mean bids: \$5.74 versus \$5.34, p = 0.559; Kolmogorov-Smirnov p = 0.604). In other words, subjects are less likely to make positive bids, and average bids are smaller, in DMfO when DMfO follows decisions for oneself; it is important to note that smaller bids are characteristic of decisions for oneself.

4.2.2. Endowment-effect order effects

Figure 2 shows the distributions of bids by good, environment, and order of environment. Another order effect is there visualized. Specifically, as above, the proportion of zero bids is distinctively low in DMfO for subjects who encounter Environment B first. Pooling those who were endowed with a chocolate bar and stickers, the proportion of subjects who make positive bids in Environment B is higher among those who encounter Environment B first rather than second (0.839 versus 0.619, p = 0.001); there is no such pattern for mean bids (\$3.33 versus \$2.59, p = 0.185); the Bonferroni-adjusted alpha for these tests is 0.025 (= 0.05/2), and means are presented in Table 4. A Kolmogorov-Smirnov test rejects the equality of the distribution of Environment-B bids when Environment B is encountered first versus second (p = 0.003). No such pattern holds for Environment-A bids when comparing those who encounter Environment A first rather than second (proportion of positive-bids: 0.536 versus 0.441, p = 0.191; mean bids: \$1.44 versus \$1.58, p = 0.710; Kolmogorov-Smirnov p = 0.535). In other words, subjects are less likely to make positive bids in DMfO when DMfO follows decisions for oneself; it is important to note that a lower propensity to bid is a characteristic of decisions for oneself.

4.2.3. Identifiable-victim-bias order effects

Figure 3 shows the distributions of donations by environment and order of environment. Because we do not find a significant difference in charitability in the identifiable- and statistical-victim conditions using either charitability measure, we pool subjects together. So doing, we identify order effects in the propensity to donate on behalf of others: it is greater for those who encounter Environment B first rather than second (0.903 versus 0.742, p = 0.004); no such pattern holds for mean donations (\$7.28 versus \$5.80, p = 0.111); the Bonferroni-adjusted alpha for these tests is 0.025 (= 0.05/2), and means are presented in Table 5. A Kolmogorov-Smirnov test does not reject the equality of the distribution of Environment-B donations when Environment B is encountered first versus second (p = 0.147). The difference between Environment-A donations for those who encounter Environment A first rather than second is statistically insignificant (proportion of positive donations: 0.817 versus 0.711, p = 0.087; mean donations: \$5.62 versus \$5.48; Kolmogorov-Smirnov p = 0.498). In other words, subjects are less likely to donate in DMfO when DMfO follows decisions for oneself; it is important to note that a lower propensity to donate is a characteristic of decisions for oneself.

4.2.4. Uninterpretability order effects

We identify order effects in the uninterpretability of MPL responses. The uninterpretability of Environment-B responses is significantly higher when Environment B is encountered first rather than second (0.090 versus 0.037, p = 0.040). No such pattern holds for Environment A (0.028 versus 0.031, p = 0.863). Means are presented in Table 6. A possible interpretation of this result is that subjects are less likely to pay close attention in DMfO than decisions for themselves; but that after close attention in decisions for themselves, they either continue to pay close attention in DMfO or anchor DMfO responses to decisions for themselves.

4.3. Self-reported approach to decision-making in Environment B

In the questionnaire, subjects were asked: "When completing the tasks on behalf of Participant X, what factors did you consider in completing the tasks? Please explain." Responses were independently coded by the two experimenters; the codes were then merged to produce a limited set of factors that subjects reported to have considered in DMfO. The coding allowed each subject to consider multiple factors. In cases in which the independent codes did not match, the experimenters discussed the mismatch until agreement regarding the appropriate coding was achieved. Table 7 provides some examples of subjects' responses, and Table 8 lists the most frequently considered factors along with the proportion of subjects who reported each factor.

We find that approximately 60% of subjects reported that they made decisions on behalf of Participant X either as if for themselves, as if they were Participant X, as someone else would decide for them, or as they would want someone else to decide for them. We pool these subjects together and refer to them as Golden-Rule followers (GRFs). Further, we find that approximately 40% of subjects reported that they made decisions on behalf of Participant X either to maximize Participant X's earnings or utility, or by using cost-benefit analysis. We pool these subjects together and refer to them as Maximizers. Note, a subject can be both a GRF and Maximizer. Finally, there is no order effect: the difference in the propensity to be a GRF or Maximizer for those who encountered Environment A versus B first is insignificant (GRF: 0.588 versus 0.591, p = 0.958; Maximizers: 0.340 versus 0.484, p = 0.044); the Bonferroni-adjusted alpha for these tests is 0.025 (= 0.05/2).

We also test whether behavior is statistically distinguishable for those who report a particular approach to DMfO and those who do not. Specifically, for each of Tasks 15-18 in each of Environments A and B, we test the equality of GRFs' and non-GRFs' responses and the equality of Maximizers' and non-Maximizers' responses. Environment A (B) responses pool data from subjects who encountered Environment A (B) first and second. Only significant differences are discussed. We find that the endowment effect in Environment B is larger for GRFs than non-GRFs ($3.50 versus $0.97, p = 0.012); the Bonferroni-adjusted alpha for this test is 0.017 (= 0.05/3), which accounts for having tested bids, asks, and the endowment effect. We also find that Maximizers have significantly less uninterpretability than non-Maximizers in Environment B (0.019 versus 0.094, p = 0.004). Lastly, the difference between Maximizers' Environment-B and -A uninterpretability is significantly closer to zero than that of non-Maximizers (-0.016 versus 0.068, p = 0.002).[18]

5. Discussion

In a laboratory experiment designed to identify self-other discrepancies in seven decision-making biases, we identify present bias, compound risk aversion, anchoring bias, and endowment effect in both decisions for oneself and in DMfO; the magnitudes of these biases are statistically indistinguishable in decisions for oneself and DMfO. The reflection effect, decoy

[18] Similar tests for Tasks 1-14 in most cases do not identify significant differences between GRFs and non-GRFs or between Maximizers and non-Maximizers. The exceptions are: (i) the discount rate and present bias of GRFs are significantly higher than those of non-GRFs when restricting to Environment A decisions of those who encountered Environment A second (discount rate: 0.190 versus 0.083, p = 0.005; present bias: 0.437 versus 0.177, p = 0.001), and (ii) the proportion of Maximizers exhibiting the decoy effect (as measured with the alternative approach) is significantly lower than the proportion of non-Maximizers exhibiting the decoy effect (0 versus 0.141, p = 0.024). The Bonferroni-adjusted alphas for the results in both (i) and (ii) are 0.025 (=0.05/2) to account for tests of both discount rate and present bias in (i) and to account for tests using both approaches to measure of the decoy effect in (ii).

effect, and identifiable-victim bias are not identified, neither in decisions for oneself nor DMfO. However, we do find some self-other discrepancies: willingness to pay is higher in DMfO than decisions for oneself in tasks associated with the anchoring bias, endowment effect, and identifiable-victim bias. The final self-other discrepancy we find is that uninterpretable preferences are more common in DMfO. We also find evidence of order effects, with DMfO more similar to decisions for oneself when it follows them. Lastly, we find evidence that individuals tend to approach DMfO as surrogates, not rivals; most subjects report that they follow some version of the "Golden Rule" and/or try to maximize the surrogates' payment or utility.

That we find no evidence of self-other discrepancies in the tasks associated with risk and time preferences is perhaps not surprising, as prior DMfO research regarding these two preferences is mixed. That we find no evidence of a self-other discrepancy in the tasks associated with the decoy effect may be because, like the anchoring bias, it is rooted in unconscious perception, which may be less likely to vary between DMfO and decisions for oneself.

What might explain the self-other discrepancy in willingness-to-pay? One possibility is that individuals care less about spending others' money, as there is no cost of doing so; of course, this is difficult to reconcile with the fact there is no direct benefit of doing so either. Another possibility suggested by Polman et al. (2017) is that individuals believe that others' money has less purchasing power than their own money. A final possibility is that given that individuals do not know others' preferences, they are reluctant to indicate an extreme value, such as $0, for the others' willingness-to-pay. We do not have data that can speak to the first two possibilities, but the reluctance to indicate $0-bids in DMfO (that we report in section 4.2) is consistent with the third possibility. Conversely, it is interesting to consider why subjects did not more often indicate a willingness-to-pay of $0 in DMfO, as such a decision would afford Participant X the maximum flexibility and require subjects to make the fewest possible assumptions about Participant X's preferences. For instance, a willingness-to-pay of $0 in DMfO would be the equivalent of gifting cash instead of a gift item, and Participant X could always use the cash to later buy the item or make a donation. One possible explanation is that individuals enjoy exercising power over others (Pikulina & Tergiman, 2018) or attempting to guess others' preferences.

Our study raises three additional questions that we leave to future research to examine. First, would the benevolent surrogacy we observe survive in alternate DMfO environments? For example, when the surrogate has conflicting incentives, does self-interest fully dominate, or does actual behavior reflect a convex combination of self-interested behavior and DMfO simpliciter. Second, is there a correlation between the benevolent surrogacy we observe and social preferences? Third, are surrogates more charitable with others' money because they are freeriding, or because they think it is "right" to be charitable?

A last note warrants mention. Our order-effect result may help explain a common practice when seeking guidance or advice from professionals or friends: asking what they would do if they were in our shoes. Our results suggest that re-framing a question in such a manner may change the guidance or advice that is given to be more aligned with what individuals would decide for themselves. That this simple exercise changes responses confirms the importance of framing in decision-making in general and in DMfO in particular.

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Table 1. Tasks 1-14: Compound risk aversion, discount rate, present bias, risk preferences, reflection effect, and decoy effect

		Proportion Compound Risk Averse	Discount Rate	Proportion Present Biased	Proportion Present Biased (Conservative)	CE/EV Gains	CE/EV Losses	Proportion Exhibiting Reflection Effect	Proportion Exhibiting Decoy Effect	Proportion Exhibiting Decoy Effect (Conservative)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1st Environment	A	0.495**	0.118	0.210**	0.273**	1.404	0.908	0.040	0.108	0.124
		(0.051)	(0.018)	(0.032)	(0.040)	(0.046)	(0.049)	(0.015)	(0.022)	(0.034)
		n = 97	n = 86	n = 93	n = 94	n = 91	n = 92	n = 87	n = 97	n = 97
	B	0.462**	0.164	0.302**	0.312**	1.323	1.023	0.093	0.129	0.129
		(0.052)	(0.022)	(0.042)	(0.044)	(0.056)	(0.060)	(0.025)	(0.027)	(0.035)
		n = 93	n = 77	n = 78	n = 80	n = 82	n = 80	n = 75	n = 93	n = 93
	A-B	0.032	-0.046	-0.092	-0.038	0.082	-0.115	-0.053	-0.021	-0.005
		(0.073)	(0.028)	(0.051)	(0.059)	(0.071)	(0.074)	(0.028)	(0.035)	(0.048)
		{0.662}	{0.101}	{0.076}	{0.520}	{0.254}	{0.122}	{0.057}	{0.556}	{0.913}
2nd Environment	A	0.592**	0.149	0.338**	0.336**	1.459	0.966	0.054	0.129	0.118
		(0.051)	(0.019)	(0.038)	(0.046)	(0.052)	(0.050)	(0.021)	(0.027)	(0.034)
		n = 93	n = 79	n = 89	n = 89	n = 88	n = 85	n = 84	n = 93	n = 93
	B	0.505**	0.150	0.298**	0.367**	1.329	0.985	0.071	0.072	0.093
		(0.051)	(0.022)	(0.036)	(0.046)	(0.048)	(0.039)	(0.022)	(0.021)	(0.030)
		n = 97	n = 80	n = 88	n = 91	n = 93	n = 91	n = 91	n = 97	n = 97
	A-B	0.086	0.000	0.039	-0.031	0.130	-0.018	-0.018	-0.057	0.025
		(0.072)	(0.029)	(0.053)	(0.065)	(0.071)	(0.062)	(0.030)	(0.033)	(0.045)
		{0.235}	{0.991}	{0.455}	{0.637}	{0.069}	{0.772}	{0.552}	{0.090}	{0.569}
Pooled 1st & 2nd Env	A	0.542**	0.133	0.273**	0.304**	1.431	0.936	0.047	0.118	0.121
		(0.036)	(0.013)	(0.025)	(0.030)	(0.035)	(0.033)	(0.013)	(0.017)	(0.024)
		n = 190	n = 165	n = 182	n = 183	n = 179	n = 177	n = 171	n = 190	n = 190
	B	0.484**	0.157	0.300**	0.341**	1.326	1.326	0.081	0.100	0.111
		(0.036)	(0.015)	(0.027)	(0.032)	(0.037)	(0.037)	(0.016)	(0.017)	(0.023)
		n = 190	n = 152	n = 166	n = 171	n = 175	n = 175	n = 166	n = 190	n = 190
	A-B	0.058	-0.024	-0.028	-0.037	0.105	-0.390	-0.035	0.018	0.011
		(0.051)	(0.020)	(0.037)	(0.044)	(0.050)	(0.049)	(0.020)	(0.024)	(0.033)
		{0.260}	{0.242}	{0.458}	{0.400}	{0.037}	{0.000}	{0.092}	{0.449}	{0.749}

Notes: Standard errors in parentheses. A-B is the difference between Environment A and Environment B values. Raw *p*-values of tests of equality in Environment A and Environment B are reported in curly brackets. See sections 3.2.1-3.2.4 for measures of compound risk aversion, discount rate, present bias, CE/EV, reflection effect, and decoy effect. In columns (1), (3), (4), (7), (8), and (9), stars refer to the significance of tests for the biases, with * indicating $p < 0.05$ and ** indicating $p < 0.01$. In column (1), the proportion of subjects who are compound-risk-averse is tested against 0.25, the proportion that would exhibit compound-risk-aversion by chance alone. In columns (3) and (4), the proportion of subjects exhibiting present bias is tested against the proportion that could exhibit it by chance alone given the average discount rate in the tasks with no front-end delay: in column (3), 1st Envrionment A value 0.1223, 1st Environment B value 0.1762, 2nd Environment A value 0.1703, and 2nd Environment B value 0.1574, pooled Environment A value 0.1453, and pooled Environment B value 0.1663; in column (4), 1st Environment A value 0.1376, 1st Environment B value 0.1790, 2nd Environment A value 0.1636, 2nd Environment B value 0.1834, pooled Environment A value 0.1503, and pooled Environment B value 0.1813. In column (7), the proportion of subjects exhibiting the reflection effect is tested against 0.12125, the average of exhibiting the reflection effect in Tasks 5 and 7 ($0.04 = (4/20)^2$) and in Tasks 6 and 8 ($0.2025 = (9/20)^2$). In columns (8) and (9), the proportion of subjects exhibiting the decoy effect is tested against 0.1111 the proportion that could exhibit It by chance alone.

Table 2. Summary of results for all biases in first environment encountered

Bias	Replicated in Environment A	Replicated in Environment B	Self-Other Discrepancy
Present bias	Yes	Yes	No
Reflection effect	No	No	No
Compound risk aversion	Yes	Yes	No
Decoy effect	No	No	No
Anchoring bias	Yes	Yes	Bias: No Subtask: Yes ($Bids_{DMfO}$ > $Bids_{self}$)
Endowment effect	Yes	Yes	Bias: No Subtask: Yes ($Bids_{DMfO}$ > $Bids_{self}$)
Identifiable victim bias	No	No	Bias: No Subtask: Yes ($Donation_{DMfO}$ > $Donation_{self}$)

Table 3. Tasks 15: Bids for mug and anchoring bias

		Bids			Proportion of Bids > 0		Pooling Anchors	
		$5-Anchor	$15-Anchor	Anchoring Bias	$5 Anchor	$15 Anchor	Proportion Bids > 0	Bids
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st Environment	A	$4.70	$6.81	$2.11	0.755	0.813	0.784	$5.74
		(0.593)	(0.855)	(1.037)	(0.062)	(0.057)	(0.042)	(0.527)
		n = 49	n = 48	{0.045}	n = 49	n = 48	n = 97	n = 97
	B	$7.50	$10.72	$3.22	1.000	0.981	0.989	$9.30
		(0.562)	(0.705)	(0.938)	(0.000)	(0.019)	(0.011)	(0.492)
		n = 41	n = 52	{0.001}	n = 41	n = 52	n = 93	n = 93
	A-B	-$2.80	-$3.91	-$1.11	-0.245	-0.168	-0.205	-$3.56
		(0.827)	(1.102)	(1.377)	(0.068)	(0.058)	(0.044)	(0.722)
		{0.001}	{0.001}	{0.420}	{0.001}	{0.005}	{0.000}	{0.000}
2nd Environment	A	$4.88	$5.71	$0.83	0.878	0.788	0.828	$5.35
		(0.551)	(0.627)	(0.856)	(0.052)	(0.057)	(0.039)	(0.427)
		n = 41	n = 52	{0.336}	n = 41	n = 52	n = 93	n = 93
	B	$6.27	$8.09	$1.82	0.898	0.875	0.887	$7.17
		(0.690)	(0.843)	(1.087)	(0.044)	(0.048)	(0.032)	(0.549)
		n = 49	n = 48	{0.097}	n = 49	n = 48	n = 97	n = 97
	A-B	-$1.40	-$2.38	-$0.99	-0.020	-0.087	-0.059	-$1.83
		(0.908)	(1.040)	(1.373)	(0.067)	(0.075)	(0.051)	(0.699)
		{0.128}	{0.024}	{0.471}	{0.768}	{0.254}	{0.250}	{0.010}
Pooled 1st & 2nd Env	A	$4.78	$6.24	$1.46	0.811	0.800	0.805	$5.55
		(0.407)	(0.524)	(0.674)	(0.042)	(0.040)	(0.029)	(0.340)
		n = 90	n = 100	{0.032}	n = 90	n = 100	n = 190	n = 190
	B	$6.83	$9.46	$2.63	0.944	0.930	0.937	$8.21
		(0.457)	(0.559)	(0.732)	(0.024)	(0.026)	(0.018)	(0.377)
		n = 90	n = 100	{0.000}	n = 90	n = 100	n = 190	n = 190
	A-B	-$2.05	-$3.22	-$1.17	-0.133	-0.130	-0.132	-$2.67
		(0.612)	(0.766)	(0.981)	(0.048)	(0.048)	(0.033)	(0.500)
		{0.001}	{0.000}	{0.233}	{0.006}	{0.007}	{0.000}	{0.000}

Notes: Standard errors in parentheses. Anchoring bias is the difference between $15-anchor bids and $5-anchor bids. A-B is the difference between Environment A and Environment B bids. Raw *p*-values of tests of equality in Environment A and Environment B, and of tests of equality of $5- and $15-anchor bids, are reported in curly brackets.

Table 4. Tasks 16-17: Asks and bids for chocolate and stickers and endowment effect

		Asks			Bids			Endowment Effect		Pooling Chocolate and Stickers			
		Chocolate	Stickers	Chocolate - Stickers	Chocolate	Stickers	Chocolate - Stickers	Chocolate	Stickers	Asks	Bids	Endowment Effect	Proportion Bids >0
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1st Environment	A	$5.69	$5.13	$0.56	$2.34	$0.59	$1.75	$3.35	$4.53	$5.42	$1.44	$3.98	0.464
		(0.804)	(0.773)	(1.118)	(0.526)	(0.174)	(0.541)	(0.973)	(0.770)	(0.556)	(0.283)	(0.624)	(0.051)
		n = 50	n = 47	{0.615}	n = 47	n = 50	{0.002}	{0.001}	{0.000}	n = 97	n = 97	{0.000}	n = 97
	B	$5.72	$4.75	$0.97	$4.08	$2.56	$1.52	$1.65	$2.19	$5.23	$3.33	$1.91	0.839
		(0.823)	(0.734)	(1.101)	(0.528)	(0.541)	(0.755)	(0.973)	(0.915)	(0.550)	(0.384)	(0.671)	(0.038)
		n = 46	n = 47	{0.379}	n = 47	n = 46	{0.047}	{0.094}	{0.019}	n = 93	n = 93	{0.005}	n = 93
	A-B	-$0.03	$0.37	-$0.41	-$1.74	-$1.96	$0.23	$1.70	$2.34	$0.18	-$1.86	-$2.07	-0.375
		(1.151)	(1.066)	(1.568)	(0.745)	(0.549)	(0.937)	(1.371)	(1.208)	(0.783)	(0.474)	(0.921)	(0.064)
		{0.977}	{0.726}	{0.794}	{0.022}	{0.001}	{0.806}	{0.215}	{0.053}	{0.814}	{0.000}	{0.026}	{0.000}
2nd Environment	A	$4.78	$3.86	$0.92	$1.74	$1.41	$0.33	$3.04	$2.65	$4.32	$1.58	$2.74	0.559
		(0.686)	(0.588)	(0.902)	(0.283)	(0.410)	(0.496)	(0.737)	(0.380)	(0.451)	(0.247)	(0.514)	(0.052)
		n = 46	n = 47	{0.311}	n = 47	n = 46	{0.505}	{0.000}	{0.001}	n = 93	n = 93	(0.000)	n = 93
	B	$4.68	$6.54	-$1.87	$3.39	$1.83	$1.56	$1.28	$4.71	$5.58	$2.59	$2.99	0.619
		(0.618)	(0.950)	(1.120)	(0.642)	(0.466)	(0.787)	(0.891)	(1.039)	(0.565)	(0.399)	(0.692)	(0.050)
		n = 50	n = 47	{0.099}	n = 47	n = 50	{0.050}	{0.153}	{0.000}	n = 97	n = 97	(0.000)	n = 97
	A-B	$0.10	-$2.68	$2.79	-$2.57	-$0.42	-$1.23	$1.76	-$2.06	-$1.27	-$1.01	-$0.26	-0.06
		(0.921)	(1.117)	(1.449)	(0.359)	(0.625)	(0.937)	(1.160)	(1.278)	(0.727)	(0.474)	(0.895)	(0.072)
		{0.910}	{0.018}	{0.054}	{0.021}	{0.507}	{0.189}	{0.129}	{0.107}	{0.083}	{0.035}	{0.773}	{0.408}
Pooled 1st & 2nd Env	A	$5.25	$4.49	$0.76	$2.04	$0.99	$1.06	$3.21	$3.51	$4.88	$1.51	$3.37	0.511
		(0.532)	(0.487)	(0.722)	(0.299)	(0.219)	(0.368)	(0.613)	(0.531)	(0.361)	(0.188)	(0.406)	(0.037)
		n = 96	n = 94	{0.294}	n = 94	n = 96	{0.005}	{0.000}	{0.000}	n = 190	n = 190	{0.000}	n = 190
	B	$5.18	$5.65	-$0.47	$3.74	$2.18	$1.56	$1.44	$5.65	$5.41	$2.95	$2.46	0.726
		(0.509)	(0.604)	(0.789)	(0.415)	(0.355)	(0.545)	(0.658)	(0.604)	(0.394)	(0.278)	(0.498)	(0.032)
		n = 96	n = 94	{0.552}	n = 94	n = 96	{0.005}	{0.030}	{0.000}	n = 190	n = 190	{0.000}	n = 190
	A-B	$0.08	-$1.15	$1.23	-$1.69	-$1.19	-$0.50	$1.77	-$2.14	-$0.53	-$1.44	$0.91	-0.215
		(0.736)	(0.776)	(1.070)	(0.511)	(0.417)	(0.660)	(0.897)	(0.881)	(0.534)	(0.335)	(0.643)	(0.049)
		{0.918}	{0.139}	{0.250}	{0.001}	{0.005}	{0.449}	{0.049}	{0.015}	{0.319}	{0.000}	{0.159}	{0.000}

Notes: Standard errors in parentheses. Endowment effect is the difference between asks and bids. A-B is the difference between Environment A and Environment B values. Raw *p*-values of tests of equality in Environment A and Environment B, of tests of equality of chocolate and sticker valuations, and of tests of equality of asks and bids, are reported in curly brackets.

Table 5. Task 18: Contributions to charity and identifiable-victim bias

		Donations			Proportion of Donations > 0			Pooling Identifiable and Statistical Victims	
		Identifiable Victim	Statistical Victim	Identifiable Victim Bias	Identifiable Victim	Statistical Victim	Identifiable Victim Bias	Donations	Proportion Don. > 0
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st Environment	A	$5.49	$5.74	-$0.25	0.674	0.741	-0.066	$5.62	0.711
		(0.988)	(0.878)	(1.321)	(0.072)	(0.060)	(0.093)	(0.653)	(0.046)
		n = 43	n = 54	{0.851}	n = 43	n = 54	{0.479}	n = 97	n = 97
	B	$6.96	$7.53	-$0.57	0.902	0.904	-0.001	$7.28	0.903
		(0.990)	(0.932)	(1.369)	(0.040)	(0.041)	(0.062)	(0.677)	(0.031)
		n = 41	n = 52	{0.679}	n = 41	n = 52	{0.982}	n = 93	n = 93
	A-B	-$1.47	-$1.79	$0.32	-0.228	-0.163	-0.065	-$1.65	-0.192
		(1.399)	(1.280)	(1.896)	(0.087)	(0.074)	(0.110)	(0.940)	(0.056)
		{0.296}	{0.165}	{0.866}	{0.011}	{0.029}	{0.555}	{0.081}	{0.008}
2nd Environment	A	$6.23	$4.89	$1.34	0.878	0.769	0.109	$5.48	0.817
		(0.891)	(0.696)	(1.114)	(0.052)	(0.059)	(0.081)	(0.554)	(0.040)
		n = 41	n = 52	{0.232}	n = 41	n = 52	{0.181}	n = 93	n = 93
	B	$6.30	$5.39	$0.91	0.674	0.796	-0.122	$5.80	0.742
		(1.099)	(0.723)	(1.272)	(0.072)	(0.055)	(0.089)	(0.630)	(0.045)
		n = 43	n = 54	{0.478}	n = 43	n = 54	{0.176}	n = 97	n = 97
	A-B	-$0.07	-$0.51	$0.43	0.204	-0.027	0.231	-$0.32	0.075
		(1.420)	(1.005)	(1.735)	(0.090)	(0.081)	(0.120)	(0.842)	(0.060)
		{0.961}	{0.616}	{0.804}	{0.026}	{0.738}	{0.054}	{0.708}	{0.216}
Pooled 1st & 2nd Env	A	$5.85	$5.32	$0.53	0.774	0.755	0.019	$5.55	0.763
		(0.664)	(0.562)	(0.865)	(0.046)	(0.042)	(0.062)	(0.429)	(0.031)
		n = 84	n = 106	{0.542}	n = 84	n = 106	{0.760}	n = 190	n = 190
	B	$6.62	$6.44	$0.18	0.786	0.849	-0.063	$6.52	0.821
		(0.738)	(0.594)	(0.936)	(0.045)	(0.035)	(0.056)	(0.464)	(0.028)
		n = 84	n = 106	{0.847}	n = 86	n = 106	{0.260}	n = 190	n = 190
	A-B	-$0.77	-$1.12	$0.35	-0.012	-0.094	0.082	-$0.97	-0.058
		(0.993)	(0.817)	(1.286)	(0.064)	(0.055)	(0.084)	(0.632)	0.042
		{0.438}	{0.172}	{0.786}	{0.853}	{0.086}	{0.329}	{0.127}	{0.165}

Notes: Standard errors in parentheses. Identifiable victim bias is the difference between identifiable-victim and statistical-victim charitability. A-B is the difference between Environment A and Environment B values. Raw *p*-values of tests of equality in Environment A and Environment B, and of tests of equality of identifiable-victim- and statistical-victim-charitability, are reported in curly brackets.

Table 6. Proportion of subjects with at least one uninterpretable response in Tasks 1-8

	A	B	A-B
1st Environment	0.028	0.09	-0.062
	(0.010)	(0.021)	(0.023)
	n = 97	n = 93	{0.008}
2nd Environment	0.031	0.037	-0.006
	(0.011)	(0.015)	(0.019)
	n = 93	n = 97	{0.730}
Pooled 1st & 2nd Env	0.030	0.063	-0.034
	(0.007)	(0.013)	(0.015)
	n = 190	n = 190	{0.0243}

Notes: Standard errors in parentheses. A-B is the difference between Environment A and Environment B proportions. Raw *p*-values of tests of equality in Environment A and Environment B are reported in curly brackets.

Table 7. Sample responses to the item: "When completing the tasks on behalf of Participant X, what factors did you consider in completing the tasks? Please explain."

I pretty much just did for them what I would have wanted
I chose pretty conservative answers, keeping in mind how I would want the questions answered.
I chose what I would have chosen for myself.
I completed the task as if I was participant X.
How I felt that another participant would complete the survey on my behalf. I didn't feel that someone completing on my behalf would have my best interest in mind.
I tried to answer most questions in the middle. I did not make any risky moves.
Definitely immediacy. I know some students live off campus and have rent and other things to buy and often do not have the time to waste to wait for money to come in. Even though it was little amounts (that definitely could not pay for rent) I know every little bit counts.
I considered how I figured the average person would complete the task.
I was considerate to the participant.
I both considered how I might want to make certain decisions (particularly in regards to the tasks in which we were supposed to value items) and what would most optimize the other participant's chance of payment and amount of payment. I assumed in my decisions that the participant wanted the most money possible, and valued cash over material possessions.
I tried to maximize the amount of money they would get in the same way I did for me.
I choose based off of what I would have wanted, but added slightly more risk. I did that for 2 reasons, one, maybe the other person liked the risk, and two, it's not my money.
Thought it would be more interesting/exciting to have things based on luck.

Table 8. The factors subjects most frequently report that they considered in DMfO and proportion of subjects that report having taken each factor into consideration

	Proportion of Subjects	St. Dev.
Golden Rule	0.589	(0.493)
As if they were me	0.384	(0.488)
As if I were them	0.005	(0.073)
As someone else would answer for me	0.005	(0.073)
As I want other to answer for me	0.153	(0.361)
Maximize earnings	0.411	(0.493)
More conservative for others	0.116	(0.321)
Less conservative for others	0.058	(0.234)
Consider average/others' preferences	0.100	(0.301)
Didn't know their preferences	0.053	(0.224)
Did what's fair	0.074	(0.262)

Figure 1. Histogram of bids for mug in Task 15

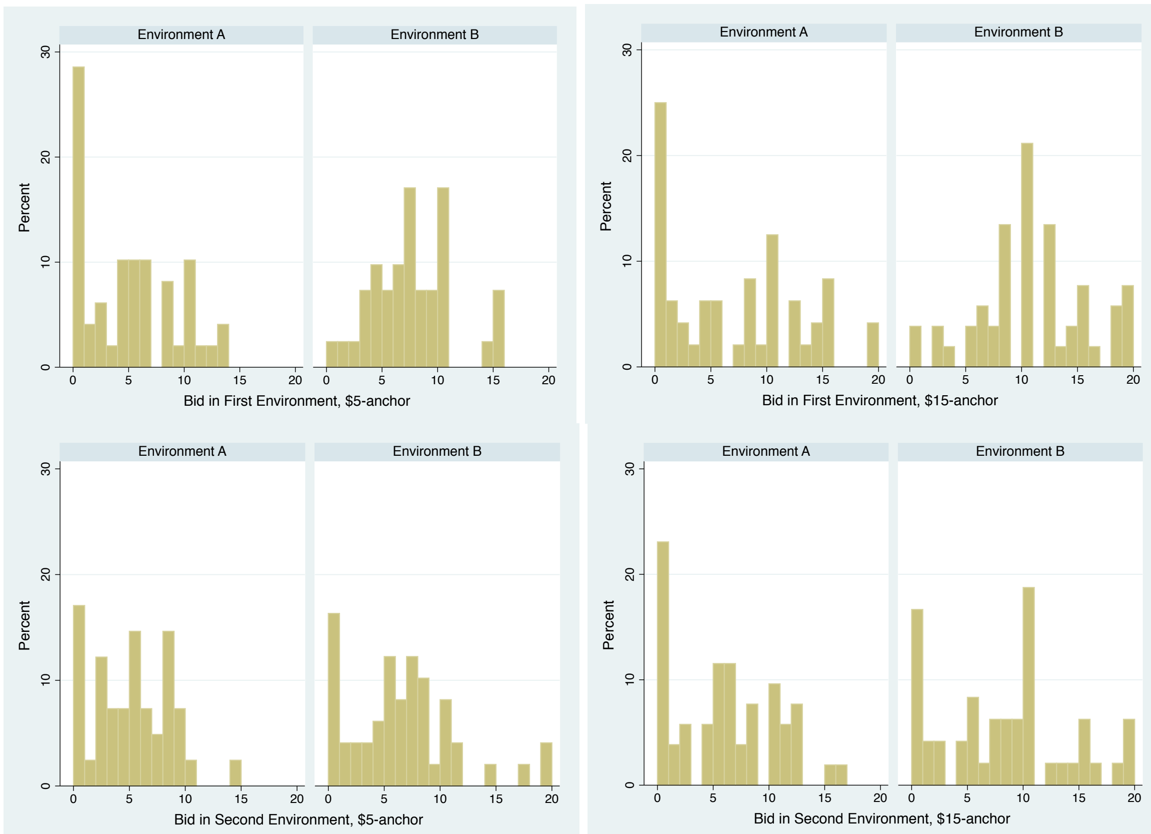

Figure 2. Histogram of bids for stickers and chocolate bars in Tasks 16-17

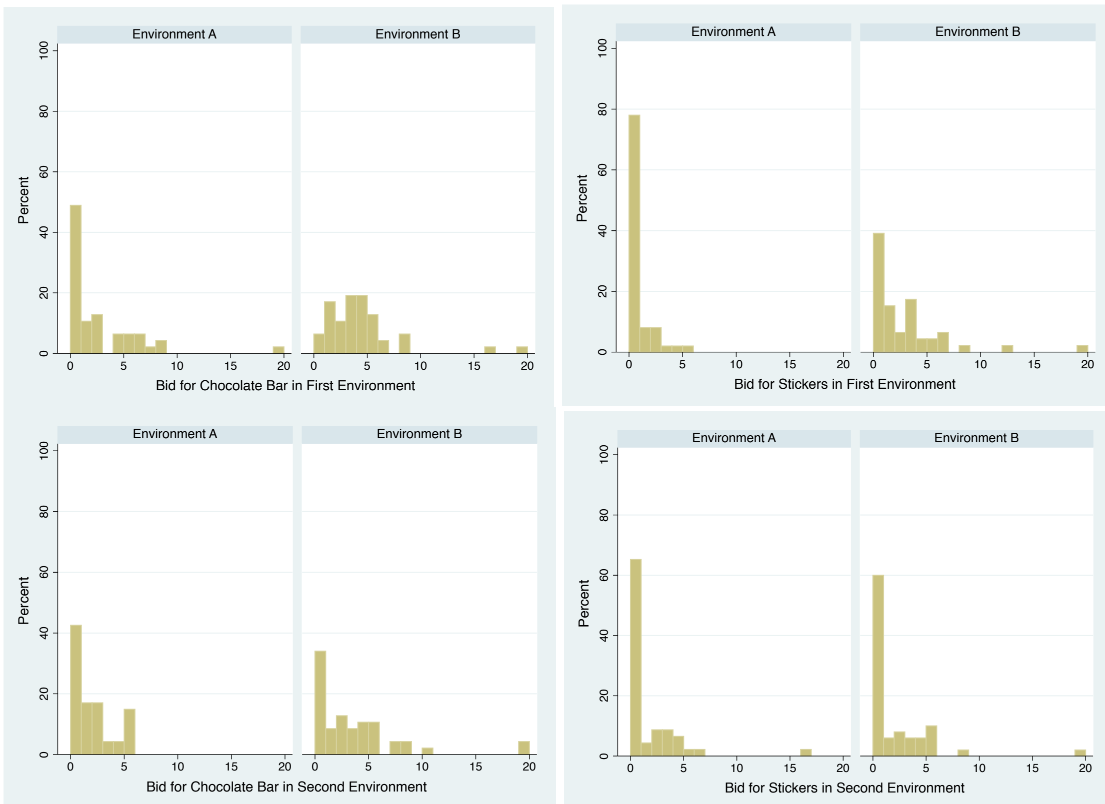

Figure 3. Histogram of donations in Task 18