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ABSTRACT

Altruism or Diminishing Marginal Utility?*

We challenge a commonly used assumption in the literature on social preferences and show that this assumption leads to significantly biased estimates of the social preference parameter. Using Monte Carlo simulations, we demonstrate that the literature's common restrictions on the curvature of the decision-makers utility function can dramatically bias the altruism parameter. We show that this is particularly problematic when comparing altruism between groups with well-documented differences in risk aversion or diminishing marginal utility, i.e., men versus women, giving motivated by pure versus warm glow motives, and wealthy versus poor.

JEL Classification: C91, D64

Keywords: altruism, marginal utility, biased inferences

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1 Introduction

Economics has long considered other-regarding preferences in shaping individuals' choices and behaviors (Becker, 1974). The relatively recent experimental evidence (e.g., from dictator, ultimatum, gift exchange and trust games) has spurred a rich body of research aimed at formally modeling social preferences to fit the behavioral phenomenon seen in the laboratory (Forsythe et al., 1994; Hoffman et al., 1994; Bolton and Zwick, 1995), including altruism and reciprocity (Andreoni, 1989; Levine, 1998; Andreoni and Miller, 2002), and fairness and inequity aversion (Rabin, 1993; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000).

The standard model of other-regarding preferences includes a decision-maker who receives utility from his own payoff or private consumption and utility from an *other's* (e.g., another person's or a charitable organization's) payoff, where a preference parameter governs the relative intensity between these two utility components. Previous literature has taken an interest in two aspects of these types of models: (1) the magnitude of the social preference parameter (see Table A1 for a limited set of examples) and (2) the differences in the shape of utility functions over payoffs to self versus payoffs to other that may result in different risk preferences and response to incentives for self versus other (see Table A2).

However, a measurement problem, similar to the problem identified in Andersen et al. (2008) with regards to estimating discount rates, arises when estimating the social preference parameter in models if we ignore or make simplifying assumptions about the shape of the utility function. Our paper stresses the importance of controlling for the curvature of the utility function when estimating social preferences. The necessity to control for the curvature of the utility function when estimating a parameter of interest has been shown to matter in a wide range of applications (Harrison, 2018), such as the estimation of subjective probabilities (Andersen et al., 2014), the estimation of correlation aversion that arises when intertemporal utility is non-separable and there is an interaction between risk and time preference (Andersen et al., 2018), and the estimation of bid functions in first price sealed bid auctions (Harrison and Rutström, 2008).

In this paper, we show that estimates of social preference parameters are significantly biased by incorrect or overly strict assumptions about the curvature of the decision-maker's utility that are ubiquitous throughout this literature. And while the majority of the paper focuses on the simplifying assumptions made about curvature, we also show that common and strict assumptions about the social preference parameter lead to biased estimates of the curvatures of the decision-maker's utility function.

We begin with a simple illustrative model. Consider a decision-maker with separable utility over self payoffs given by $u(s)$ and utility over other's payoffs given by $v(o)$, which represents the utility the decision-maker gets from providing money to the other individual or organization.[1] The

[1] In this paper, we are agnostic about the source of $v(o)$ as it is outside the scope of this paper. However, following

decision-maker is tasked with dividing income Y between himself, s, and an other, o; that is, he is playing a dictator game. In laboratory dictator games, other is almost always another subject or a charitable organization. Outside the lab, an individual who is deciding whether to give to a charitable organization is playing a dictator game with the organization. To choose the amount he wishes to give to the other, the dictator maximizes

$$U(s, o) = (1 - \alpha)u(s) + \alpha v(o), \quad \text{subject to } Y = s + p \times o \tag{1}$$

where α represents the weight the decision-maker puts on the utility from other's payoffs relative to the utility he gets from self payoffs, Y represents the total income the decision-maker must split between himself and the other, and p is the price of giving. The first order condition for the decision-maker's maximization problem is given by

$$\frac{\alpha}{1 - \alpha} = p\frac{u'(s)}{v'(o)} \tag{2}$$

The first order condition clearly shows that the curvature of $u(\cdot)$, $v(\cdot)$ and the social preference parameter, α, all affect the choice of how much to keep for oneself and to give to the other. However, one of two simplifying assumptions is often made: (1) the curvatures of $u(\cdot)$ and $v(\cdot)$ are equal and inferences are made about α or (2) a \$1 to self is the same as a \$1 to other (i.e., the decision-maker puts equal weight on payoffs to self and other) and inferences are made about the curvature of $u(\cdot)$ and $v(\cdot)$. While we found one exception to (1), DellaVigna et al. (2013) assume $u(\cdot)$ is linear and the curvature of $v(\cdot)$ takes on a specific value, Table A1, while not providing an exhaustive list, demonstrates the ubiquity of this simplifying assumption in the literature. Similarly, while we found a single exception to (2), Exley (2015) conducts a "normalization" task to avoid confounding altruism with her measures of risk aversion, Table A2 demonstrates the ubiquity of the second simplifying assumption in the literature. Most importantly, we found no paper that estimates the curvature of $u(\cdot)$, $v(\cdot)$ and α. Throughout the majority of our paper, we focus on the first case: the problem of making simplifying assumptions about the curvature and estimating α, but we return to the second case in an example in Section 4.4. In several papers listed in Table Appendix A, the authors do not explicitly model the preferences of the decision-maker. However, this does not mean the problem we identify does not exist in these cases. In fact, when preferences are not explicitly modeled and altruism is identified or measured, there is an *implicit* assumption that the curvatures of $u(\cdot)$ and $v(\cdot)$ are equal.

In this paper, we use Monte Carlo simulations to generate data allowing dictators to have different curvatures of utility over self and other's payoffs, and then estimate the dictators pref-

Harrison (2018), we acknowledge that it could represent the decision-maker's belief about the other's utility function, the other's true utility function or the decision-maker's paternalistic utility for the other. Alternatively, it may be entirely divorced from the utility the other actual receives and simply represents the utility the decision-maker gets from giving.

erences using the most common assumption in the literature, namely that the curvatures of $u(\cdot)$ and $v(\cdot)$ are equal. We show that this common assumption leads to significantly biased estimates of α.

Consistent with the simple illustrative model above, the Monte Carlo simulations show that the estimated bias in the degree of altruism, α, depends critically on $u(\cdot)$ and $v(\cdot)$. This result has important implications that go well beyond simply recognizing that the literature's estimates of altruism are biased. Most importantly, this result has direct implications when comparing altruism between groups, particularly when the two groups may significantly differ in the shape of their utility functions due to differences in risk aversion or differences in giving motives. For example, if two groups differ in their degree of diminishing marginal utility over their own payoffs, then, ceteris paribus, we would incorrectly estimate different degrees of altruism for the two groups. Similarly, if two groups of subjects differ over the curvature of the utility for the other (e.g., with one group having more pure motives while another has more warm glow motives), ceteris paribus, we would again incorrectly estimate different degrees of altruism. The most salient example is the literature that examines differences in altruism between men and women (Eckel and Grossman, 1998; Andreoni and Vesterlund, 2001; Cox and Deck, 2006).[2] In Section 4, we show that the standard assumption about the curvature of the utility functions, i.e., the curvatures of $u(\cdot)$ and $v(\cdot)$ are equal, is likely to lead to the over-estimation of altruism of women relative to men. Similarly, we examine how ignoring the motive for giving (i.e., pure versus warm glow) will lead to the over-estimation of altruism among individuals motivated by pure altruism relative to those motivated by warm glow. In a third example, we show that ignoring background wealth will also lead to the over-estimation of altruism among wealthier individuals relative to less wealthy individuals. This example echoes the results from Andreoni et al. (2017). In a field experiment Andreoni et al. (2017) found wealthier households make more pro-social choices than less wealthy households. Ignoring differences in wealth, their results suggest more altruism among the wealthier households. However, once they control for the marginal of money and the hardship that comes from being poor, they find no difference in social preferences between rich and poor households.

Our paper builds off of two seminal papers: Andersen et al. (2008) and Exley (2015). Andersen et al. (2008) demonstrates how a similar type of problem occurs in the estimation of discount rates if incorrect assumptions are made about the curvature of the utility function. In particular, using a similar CRRA utility framework, they show the estimated discount factors are twice as large if risk-neutrality is assumed rather than jointly estimating the parameter of risk aversion with the discount factor. Exley (2015) presents striking evidence of differences in the curvature of the utility over own payoffs versus utility over payoffs to charity when decision-makers are forced to make a trade-off between themselves and the charity. These results speak directly to our point–

[2]There is a large literature that looks at differences in altruism between groups. For example, older children versus younger children (Benenson et al., 2007) or individuals exposed to violent conflict or not (Voors et al., 2012).

the curvature of own and other's payoffs may differ significantly in models with other-regarding preferences and assuming otherwise leads to biased estimates of social preference parameters.

2 Modeling Altruism

The dictator game is widely used to elicit altruism (Camerer, 2010; Engel, 2011). In the earliest dictator game experiments, the dictator chooses to divide an endowment between himself (self) and another person (other). Building on Eckel and Grossman (1996b), Andreoni and Vesterlund (2001) and Andreoni and Miller (2002) extend this simple game by varying the price of giving to the other and decision-makers choose how much of an endowment to split between self and other at various prices.[3]

In this paper, we consider the modified dictator game, where the decision-maker's objective is to maximize

$$U(s,o) = (1-\alpha) * \frac{s^{(1-r_s)}}{(1-r_s)} + \alpha * \frac{o^{(1-r_o)}}{(1-r_o)}, \text{with } r_s,\ r_o < 1,^{4}\ \alpha \in [0,1] \quad (3)$$

subject to a budget constraint, $Y = s + p \times o$.

Our departure from the existing literature is to allow the decision-maker to have different curvatures over self payoffs and other's payoffs.[5] Allowing decision-makers to have different curvatures over own and other's payoffs means we cannot use the CES functional form, which is popular in this literature, and instead we opt for the CRRA functional form (Andreoni and Miller, 2002; Fisman et al., 2007).[6]

We allow for different curvatures over own and other's payoffs not simply because we can write down this less restrictive functional form, but rather, and critically, because there are substantial theoretical reasons and empirical evidence to suggest that the curvature of $u(\cdot)$ and $v(\cdot)$ are distinct. Here, we touch on some of the most important reasons why we should empirically allow r_s (in $u(\cdot)$) and r_o (in $v(\cdot)$) to have different potential values when we estimate altruism. First, note that there is ample evidence that laboratory subjects do not have linear utility over own payoffs, $u(\cdot)$, with most studies reporting a CRRA coefficient between .1 and .9 (Harrison and Rutström, 2008).[7] In contrast, there are many theoretical motives for giving that suggest a variety of functional forms

[3]This design has also been used to elicit distributional preferences (Jakiela (2013);Fisman et al. (2015);Fisman et al. (2015);Fisman et al. (2017)).

[4]We restrict, $r_s,\ r_o\ < 1$ so that the functional form is well-defined $\forall\ s,\ o \geq 0$.

[5]One exception is DellaVigna et al. (2012) and DellaVigna et al. (2013), which assume utility is linear over own payoffs (i.e., $r_s = 0$) and concave over the charity's payoff (i.e., $r_o \in [0,1]$). We consider this case in Appendix C and show the bias in estimated altruism that results from this assumption.

[6]However, there is a mapping from the set of optimal allocations given by the CRRA functional form into the set of optimal allocations given by the CES functional form, which we show in Appendix D.

[7]For instance, Harrison and Rutström (2008) re-investigate laboratory subjects' decisions from Hey and Orme (1994) and estimate a CRRA coefficient of .8 when assuming a logistic CDF (p71-72).

for $v(\cdot)$, including linear.[8] For example, Null (2011) represents pure altruism with a linear utility function and warm glow with a concave utility function. Conceptually, pure motives may be closer to linear reflecting that the additional benefit from giving may not diminish rapidly given there essentially always remains a "need" (e.g., providing food, educational and health services to people in less developed countries). On the other hand, utility stemming from impure motives or warm glow may have different implications for the shape of the utility function $v(\cdot)$. For instance, warm glow, including status-seeking and reputational concerns, may imply a rapidly diminishing marginal utility, as making a first donation may dramatically increase warm glow, while subsequent donations could add very little in terms of warm glow (e.g., donating blood once, and then seeing oneself as a "blood donor"). There is also no theoretical reason or evidence to suggest that the various potential shapes of $v(\cdot)$ are systematically correlated with the shape of $u(\cdot)$.[9]

There is also empirical evidence to suggest that the curvature over own and other's payoffs may differ. A model in which own payoffs and other's payoffs enter linearly is appealing in its simplicity (Levine, 1998), but it predicts a corner solution in dictator games, which only fits 25% of the data (Andreoni and Miller, 2002). More generally, Exley (2015) provides experimental evidence that suggests that when individuals face a trade-off between keeping money for themselves or giving money to other, there is a significant difference in the curvature over own payoffs versus the curvature over payoffs for other. In the absence of trade-off between self and other payoffs Chakravarty et al. (2011) found more risk aversion over self than other payoffs.

3 Simulation and Estimation

Fisman et al. (2007) (henceforth, FKM07) pushed the modified dictator game further by increasing the number of choices faced by each decision-maker and structurally estimating the parameters of a CES utility function. In the present paper, we simulate data using an experimental design similar to FKM07. Dictators successively face J randomly generated decisions in which they have to divide an endowment Y given the budget constraint $s + p * o = Y$, where s and o denote the amount allocated to self and other and p denotes the price of giving to other. In each decision, the decision maker must choose from one of 51 choices on the budget line. We refer to each choice on the budget line as i; ($i = 1$ to 51). The 51 choices on the budget line i that the decision maker must choose from are equally spaced out across the budget line from keeping nothing for himself to keeping everything for himself.[10] Figure E7a in the Appendix shows one randomly generated budget line and the 51 choices that the dictator has to choose among, and Figure E7b displays

[8] Andreoni et al. (1996), Null (2011), Lilley and Slonim (2014), and Brown et al. (2013) have all modeled pure and impure motives as separable with potentially different parametric forms.

[9] For example, there is no evidence suggesting that individuals who give for pure altruism (versus warm glow) are more or less risk-averse over own payoffs.

[10] Note that FKM07 used a continuous budget line. We depart from them by dividing the budget constraint in 51 choices along the budget line for computational reasons.

a graphical example of 50 randomly generated budget lines that a dictator could face.[11,12] In our simulation we use J=$3,000$ decisions, which corresponds to 60 dictators making 50 decisions and similar to the sample size used in FKM07.

In this Section, we describe the Monte Carlo simulations and the estimation that we perform to demonstrate that altruism and the curvature of utility function are confounded, leading to erroneous conclusions regarding altruism.

3.1 Simulation

We denote the true preference values (i.e., the values from the Data Generating Process, DGP) of altruism and the curvature over self payoffs and the curvature of other's payoffs as α_{DGP}, and $r_{s,DGP}$, $r_{o,DGP}$, respectively. We model the decision-makers' choices over the 51 i choices on each of the J budget lines (i.e., decisions) as a multinomial logit. The DM's probability to pick the i^{th} choices on the j^{th} budget line (i.e., for decision j) is given by:

$$P_j(i|\alpha_{DGP}, r_{s,DGP}, r_{o,DGP}, \mu_{DGP}) = \frac{e^{\frac{u_i}{\mu_{DGP}}}}{\sum_{l=1}^{51} e^{\frac{u_l}{\mu_{DGP}}}} \tag{4}$$

where μ_{DGP} is the decision error (or noise) associated with each choice. When $\mu \to \infty$ each choices i become equally likely to get selected and when $\mu \to 0$ the choice with the highest utility is chosen with certainty (see Harrison and Rutström (2008); Wilcox (2008) for reviews of stochastic model of choices).

The step-by-step process for each simulation with α_{DPG}, $r_{s,DGP}$, $r_{o,DGP}$ and μ_{DGP} is as follows[13]

1. Set $\alpha_{DGP} = .5$, $r_{s,DGP}$ and $r_{o,DGP}$ to some given value in $r_{s,DGP} \in \{0, 0.01, ...0.9\} \times r_{o,DGP} \in \{0, 0.01, ...0.9\}$.[14,15]

2. We then run $K = 1,000$ trials. Each trial will be designated as trial k, $k = 1$ to K, and each trial k consists of the following steps 2a to 2d:

   (a) Set $\mu_{k,DGP} \sim U[(0.8, 1.2)]$.

[11] Figure E8 shows choices made by representative subjects in FKM07 experiment on those budget lines.

[12] We generate the budget lines using a procedure similar to FKM07's. First, we randomly pick one of the axis (self or other's payoffs) with equal probability. Second, we randomly pick the intersect of this axis with the budget line from $U(50, 85)$. And finally, we randomly pick the intersect of the remaining axis and the budget line from $U(5, 85)$.

[13] The Stata code used to perform the simulation and estimation is available upon request.

[14] Our results are robust to the choice of α_{DGP}. In Appendix F, we perform the same exercise with $\alpha_{DGP} = .25$ and $\alpha_{DGP} = .75$.

[15] We choose to simulate data for $r_{s,DGP}$ and $r_{o,DGP}$ over the range $[0, 0.9]$ as it is the values commonly found in the literature. For instance, Harrison and Rutström (2008) estimate curvature over self payoff and Chakravarty et al. (2011) estimate curvature over other's payoffs.

(b) Randomly generate $J = 3,000$ budget lines.

(c) For each decision j:

i. Calculate $P_j(i|\alpha_{DGP}, r_{s,DGP}, r_{o,DGP}, \mu_{k,DGP})\ \forall i$.

ii. Randomly choose one choice on the budget line i, $A_j(\alpha_{DGP}, r_{s,DGP}, r_{o,DGP}, \mu_{k,DGP})$ where the probability that i is chosen is equal to $P_j(i|\alpha_{DGP}, r_{s,DGP}, r_{o,DGP}, \mu_{k,DGP})$.

(d) Using these $A_j(\cdot)$s, we estimate the parameters of the utility function from equation 3, denote estimated values as $(\widetilde{\alpha}_k, \widetilde{r}_{s,k}, \widetilde{r}_{o,k}, \widetilde{\mu}_k)$. We estimate (see Section 3.2 below) the utility function under two main cases by maximizing the log-likelihood of those $J = 3,000$ decisions using the Stata's modified Newton-Raphson (NR) algorithm.

3. From the K estimates of α, compute the percentage of over/under estimation and statistical power of the estimates.

4. Repeat steps 1 to 3 for $(r_{s,DGP}, r_{o,DGP}) \in \{0, 0.01, ...0.9\} \times \{0, 0.01, ...0.9\}$.

3.2 Estimation

We begin by estimating the model without making any restrictions on the values of r_s, r_o, α and μ. The detailed results from the unrestricted model are presented in Appendix B. The estimates from the unrestricted model show that we are always able to recover the DGP parameters; that is, at the 5 percent significance level we reject that $\widetilde{r}_s = r_{s,DGP}$, $\widetilde{r}_o = r_{o,DGP}$, and $\widetilde{\alpha} = \alpha_{DGP}$ in 5% of the trials. In other words, any bias in the estimates that arises when we estimate models with restrictions on the parameters cannot be attributed to sample size or our estimation method, and will thus be due to the model assumptions that are (in)correctly imposed.

Next, we turn to the main exercise, where we estimate the model given in (3) assuming that $r_s = r_o$, as is commonly done in the literature.[16] We show that this commonly used assumption results in biased estimates. We also show that when this restriction imposed on the parameters coincidentally matches the data generating process (i.e., when $r_{s,DGP} = r_{o,DGP}$) then the estimates on α are unbiased. After performing the estimation, we calculate the bias in the estimated α, investigate confidence intervals and issues surrounding hypothesis testing, including Type I and Type II errors.

3.2.1 Assuming identical curvature over self and other's payoffs.

Following the bulk of the literature, we consider the case when the curvatures of utility over self and other's payoffs are assumed to be identical. Therefore, we estimate the parameters of the

[16] In Appendix C we also consider the case examined in DellaVigna et al. (2012) and DellaVigna et al. (2013), where they assume linear utility over self payoffs, $r_s = 0$, and concavity over other's payoff o, $r_o \in [0, 1]$. Again, we obtain biased estimates.

following utility function:

$$u(s,o) = (1-\alpha) * \frac{s^{(1-r)}}{(1-r)} + \alpha * \frac{o^{(1-r)}}{(1-r)}, \text{with } r < 1,\ \alpha \in [0,1] \tag{5}$$

by maximizing the log-likelihood of the generated choices.

3.2.2 Bias

We first examine the percentage by which the estimated level of altruism, $\widetilde{\alpha}$, is under and over estimated. Recall, we defined $\widetilde{\alpha}_k$ as the value at which the parameter α is estimated in trial k and α_{DGP} the true value of α used to generate the data. This bias is defined as:

$$\%_{bias}(\alpha) = \frac{1}{K}\sum_{k=1}^{K} \frac{\widetilde{\alpha}_k - \alpha_{DGP}}{\alpha_{DGP}} * 100 \tag{6}$$

where K is the number of trials. Figure 1 reports the percentage of over/under estimation in $\widetilde{\alpha}$ depending on the true value of coefficient of risk aversion. Figure 1a shows that there is 0 bias from assuming $r_s = r_o$ in our estimation when, in fact, $r_{s,DGP} = r_{o,DGP}$.

In Figure 1b, we hold the value of $r_{o,DGP}$ constant (at $r_{o,DGP}$=.3) and report the percentage of bias depending on the true value of $r_{s,DGP}$. For example, when $r_{s,DGP} = .7$ and $r_{o,DGP} = .3$, but the estimated model incorrectly restricts that $r_s = r_o$, we will over-estimate α by 47.5%.[17,18] On the other hand, when $r_{s,dgp} = .1$, we under-estimate α by 29.2%.

Figure 1c provides a more general perspective and allows both $r_{o,DGP}$ and $r_{s,DGP}$ to vary simultaneously. The x-axis represents the coefficient of risk aversion over self payoffs $r_{s,DGP}$ and the y-axis represents the coefficient of risk aversion over other's payoffs $r_{o,DGP}$. The z-axis represents the percentage of over- or under-estimation of $\widetilde{\alpha}$. The blue plane represents the area on the z-axis where the bias is equal to 0, while the area above and below this plane represents an upward and downward estimated bias on $\widetilde{\alpha}$, respectively. When $r_{s,DGP} = r_{o,DGP}$, along the diagonal, note that we estimate no bias and the shaded estimates exactly cross this diagonal line. However, when $r_{s,DGP}$ does not equal $r_{o,DGP}$, there is bias.

More specifically, assuming identical curvature over self and other's payoffs, we overestimate altruism when $r_{s,DGP} > r_{o,DGP}$. The intuition is as follows. When the decision-maker's utility is more concave over self payoffs than over other's payoffs, but we do not account for this additional concavity in our estimation, we attribute higher levels of giving to a higher level of altruism rather than the greater concavity over self payoffs than other's payoffs. Similarly, we underestimate

[17] The value chosen for $r_{s,DGP}$ and $r_{o,DGP}$ in this example are realistic. Indeed, Chakravarty et al. (2011) eliciting risk preferences from multiple price list, estimated a CRRA coefficient of .689 over self payoffs and .248 over other's payoffs.

[18] In that case, $\widetilde{r}$ is on average estimated to .48.

altruism when $r_{o,DGP} < r_{s,DGP}$ because we attribute the lower level of giving to differences in altruism rather than differences in concavity over self payoffs and other's payoffs.

Result 1. *When we assume $r_s = r_o$, but $r_{s,DGP} \neq r_{o,DGP}$, then estimates of altruism, $\widetilde{\alpha}$, are biased.*

(i) When $r_{s,DGP} > r_{o,DGP}$, $\widetilde{\alpha} > \alpha_{DGP}$.

(ii) When $r_{s,DGP} < r_{o,DGP}$, $\widetilde{\alpha} < \alpha_{DGP}$.

3.2.3 Hypothesis Testing: Significance Levels and Statistical Power

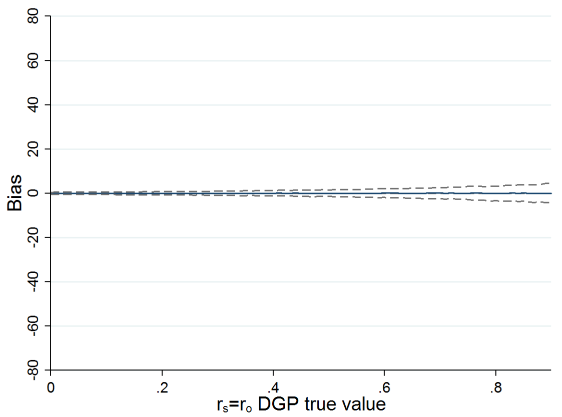
Next, we investigate the confidence intervals obtained on the biased estimates and examine the likelihood of rejecting the null hypothesis when true (Type I error) and the likelihood of failing to reject a null when untrue (Type II error).

First, we test the null hypothesis that the level of altruism is equal to its true value, $H_0 : \widetilde{\alpha} = \alpha_{DGP} = .5$.[19] Figure 2 shows the probability of a Type I error. Figure 2c allows $r_{s,DGP}$ and $r_{o,DGP}$ to vary simultaneously while Figures 2a and 2b show two-dimensional slices of Figure 2c. We should reject the null in 5% of the trials, but when the curvatures over self and other's payoffs are distinct from each other, we reject the null in a majority of trials even though the null is true. When risk aversion over self and other's payoffs are approximately equal, the statistical test is of correct size and we reject the null in 5% of the trials. For instance, in Figure 2a $r_{o,DGP}$ is fixed to .3, when $r_{s,DGP}$ is also equal to .3 the null is rejected in 5.9% of the trials, but when $r_{s,DGP} = .31$ it is rejected in 91.4% of the trials and when $r_{s,DGP} = .32$ it is rejected in 100% of the trials. Figure 2c shows that when $r_{s,DGP}$ and $r_{o,DGP}$ differ by more than a few percents, then the null that estimated $\widetilde{\alpha} = \alpha_{DGP}$ is rejected in 100% of the trials.

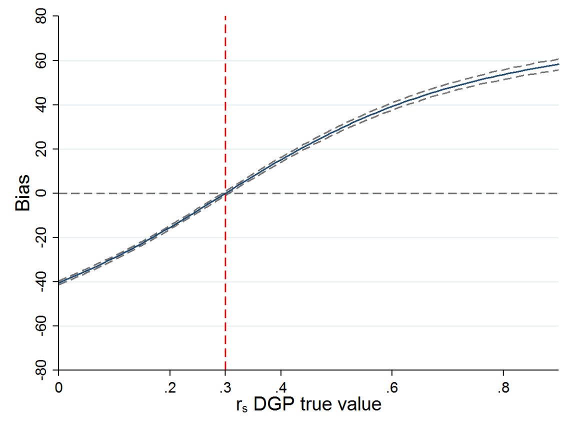
Second, we examine Type II error by testing the probability of rejecting a null hypothesis that $\widetilde{\alpha}$ is equal to a value distinct from its true value. In particular, we test $H_0 : \widetilde{\alpha} = .4$ and $H_0 : \widetilde{\alpha} = .6$, when the true value of $\alpha_{DGP} = .5$. Figure 3 reports the results. Figure 3c shows the probability to reject $H_0 : \widetilde{\alpha} = .4$ and Figure 3d the probability to reject $H_0 : \widetilde{\alpha} = .6$. Figure 3a and 3b represents 2-dimensional slices of Figure 3c and 3d. In these figures the blue line is the probability to reject $H_0 : \widetilde{\alpha} = .4$ and the green line the probability to reject $H_0 : \widetilde{\alpha} = .6$. By construction, we should reject the null hypotheses in the majority of trials but we fail to reject the null in 95% of the trials for several set of values ($r_{s,DGP}$, $r_{o,DGP}$). For instance, as shown by the green line in Figure 3a, if we assume $r_s = r_o$, but $r_{s,DGP} = .169$ and $r_{o,DGP} = .3$, we reject the null that $\widetilde{\alpha}$ equals .4 in only 5.5% of the trials despite the fact that α_{DGP} is in fact equal to .5. In Figure 3c, we fail to reject the null $H_0 : \widetilde{\alpha} = .4$ when $r_{s,DGP} \approx r_{o,DGP} - 0.13$ and in Figure 3d we fail to reject the null that

[19] Standard errors are clustered by subjects (i.e., by group of 50 decisions.). Recall that we simulate 60 dictators which each make 50 decisions.

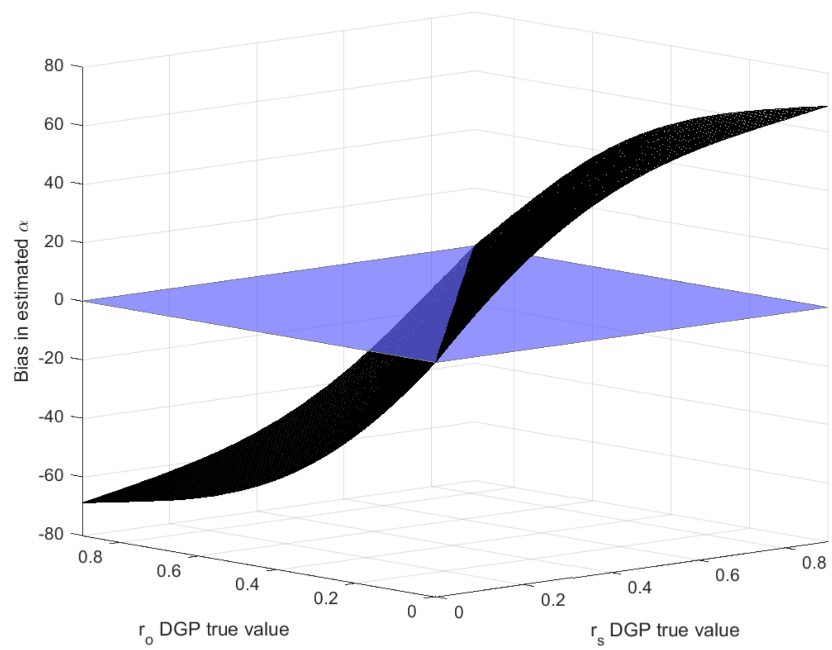
Figure 1: Bias in Estimated Altruism, $\widetilde{\alpha}$



(a) $r_{o,DGP} = r_{s,DGP}$



(b) $r_{o,DGP} = 0.3$



(c) 3-Dimensional

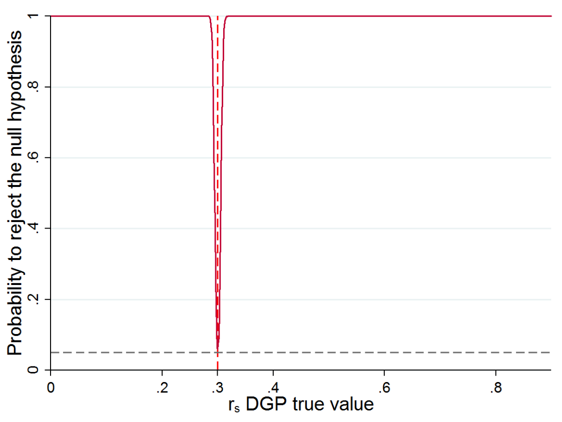
Percentage of under/over estimation of altruism, $\widetilde{\alpha}$. In Figures 1b and 1a the dash line is the corresponding 95% Monte Carlo confidence interval.

$H_0 : \widetilde{\alpha} = .6$ when $r_{s,DGP} \approx r_{o,DGP} + 0.13$. Hence, we do not only reject the null hypothesis when true but, in some cases, we also fail to reject it when false.

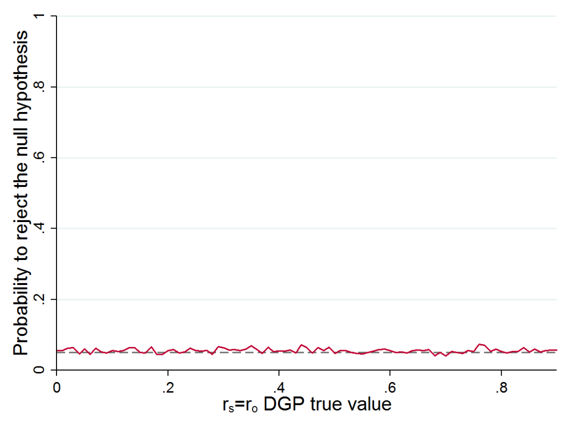
Result 2. *When we assume $r_s = r_o$, but $r_{s,DGP} \neq r_{o,DGP}$, then statistical tests reach erroneous conclusions:*

(i) Type I error: We reject the null, $H_0 : \widetilde{\alpha} = \alpha_{DGP}$, when true in too many trials.

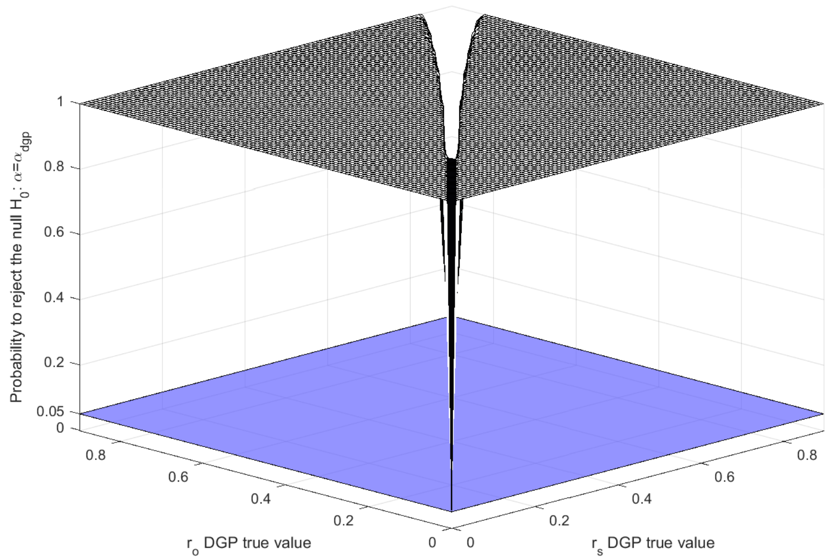
Figure 2: Power Calculations, Type I Error



(A) $r_{o,DGP}$ is fixed to .3



(B) $r_{s,DGP} = r_{o,DGP}$

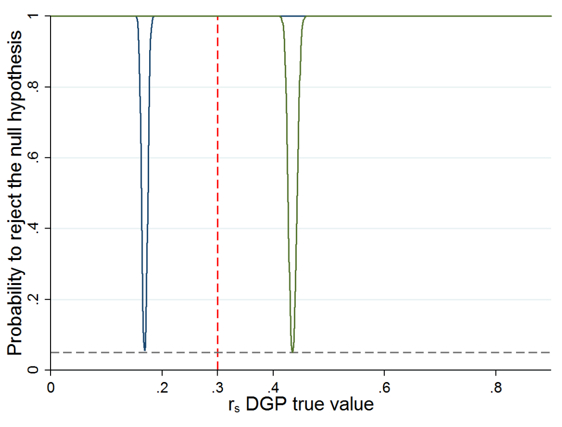


(C) 3-Dimensional

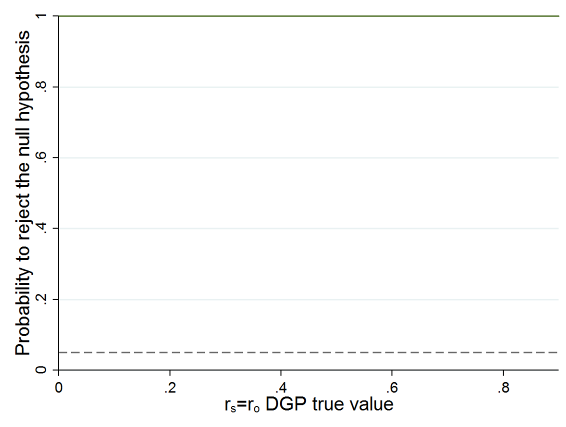
Probability to reject the null hypothesis $H_0 : \widetilde{\alpha} = \alpha_{DGP}$ at the 5% level. 1,000 trials per set of parameters ($r_{s,DGP},\ r_{o,DGP}$). In panel (A), $r_{s,DGP} \in \{0, 0.001, .., 0.9\}$.

(ii) Type II error: In some cases, we fail to reject the null, $H_0 : \widetilde{\alpha} = \alpha$, when it is not true in too many trials.

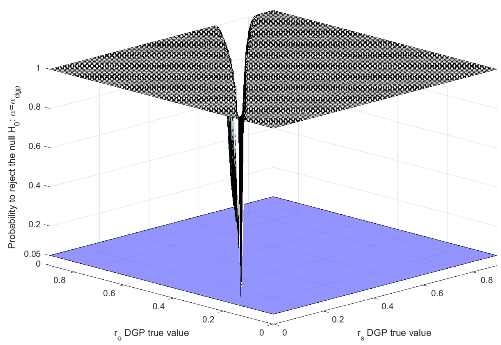
Figure 3: Power Calculations, Type II Error



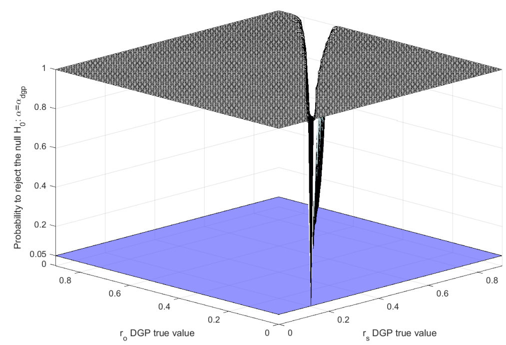
(A) $r_{o,DGP}$ is fixed to .3



(B) $r_{s,DGP} = r_{o,DGP}$



(C) $H_0 : \widetilde{\alpha} = .4$



(D) $H_0 : \widetilde{\alpha} = .6$

Probability to reject the null hypotheses at the 5% level. In Figures 3a and 3b, $H_0 : \widetilde{\alpha} = .4$ (blue) and $H_0 : \widetilde{\alpha} = .6$ (green). Estimated assuming $r_s = r_o$. 1,000 trials per set of parameters $(r_{s,DGP},\ r_{o,DGP})$. In panel (A), $r_{s,DGP} \in \{0, 0.001, .., 0.9\}$.

4 Implications

In this section, we consider three examples that highlight how the common assumption that $r_s = r_o$ can lead to erroneous conclusions. First, we examine gender differences in altruism. Second, we look at motives for giving. Third, we look at wealth effects and altruism. In each of our examples, we set $\alpha_{DGP} = .5$ for each considered group (e.g., men versus women) and impose assumptions on either the curvature of $r_{s,DGP}$ or on the curvature of $r_{o,DGP}$. That is, we generate data where there is no difference in altruism between the two groups and show how the empirically incorrect assumption that $r_s = r_o$ leads to differences in estimates of altruism between the two groups. We then test the null hypothesis that the level of altruism between each of the two considered groups are equal. In each example, we incorrectly reject the null in 100% of trials.

We then consider a fourth example in which we show that assuming individuals put equal weight on self and other payoffs, i.e., $\alpha = .5$, when α_{DGP} leads to incorrect inferences about the differences between r_s and r_o. This incorrect assumption can lead researchers to conclude that individuals are more risk-averse towards others and provide a more inelastic labor supply when working for charity payoffs than working for self payoffs.

4.1 Gender

Suppose we have a sample of 30 men and 30 women with the same level of altruism, $\alpha_{male,DGP} = \alpha_{female,DGP} = .5$. The women in our sample are more risk averse than men over self payoffs, $r_{s,female,DGP} = .9$, $r_{s,male,DGP} = .1$, but men and women have the same level of risk aversion over other's payoffs, $r_{o,female,DGP} = r_{o,male,DGP} = .6$.[20] In other words, the only difference between men and women in our simulation is their degree of risk aversion. In our simulation, we assume women are more risk averse to reflect a number of experimental results (see Croson and Gneezy (2009)) though other experimental evidence does not find this difference (e.g., see Harrison et al. (2007)). If we analyze this sample restricting risk aversion to be identical over self and other's payoffs, but allowing altruism to differ by gender, we estimate the coefficient of altruism to be .22 for men and .69 for women.[21] We reject the null hypothesis of absence of gender difference in altruism ($H_0 : \alpha_{male} = \alpha_{female}$) in 100% of the trials. To illustrate Figure 4 represents the values at which $\widetilde{\alpha}$ are estimated for men and women on a 3-dimensional figure.

4.2 Motives for Giving

Next, we examine the estimated level of altruism based on two distinct motives for giving: pure altruism and warm glow. Suppose our sample consists of two group of decision-makers with equal levels of altruism, $\alpha_{PA,DGP} = \alpha_{WG,DGP} = .5$, but who differ in their motive for giving. Group PA is driven by pure altruism, $r_{o,PA,DGP} = .1$ and group WG is driven by warm glow, $r_{o,WG,DGP} = .9$.[22] Further, assume that the two groups have similar curvatures over self payoffs, $r_{s,PA,DGP} = r_{s,WG,DGP} = .6$. In this case, we estimate the coefficient of altruism to be .77 for the individuals motivated by pure altruism and .31 for the individuals motivated by warm glow.[23] We therefore over-estimate the altruism of the pure altruist and under-estimate the altruism of the individual motivated by warm glow. Again, we incorrectly reject the null hypothesis ($H_0 : \widetilde{\alpha}_{PA} =$

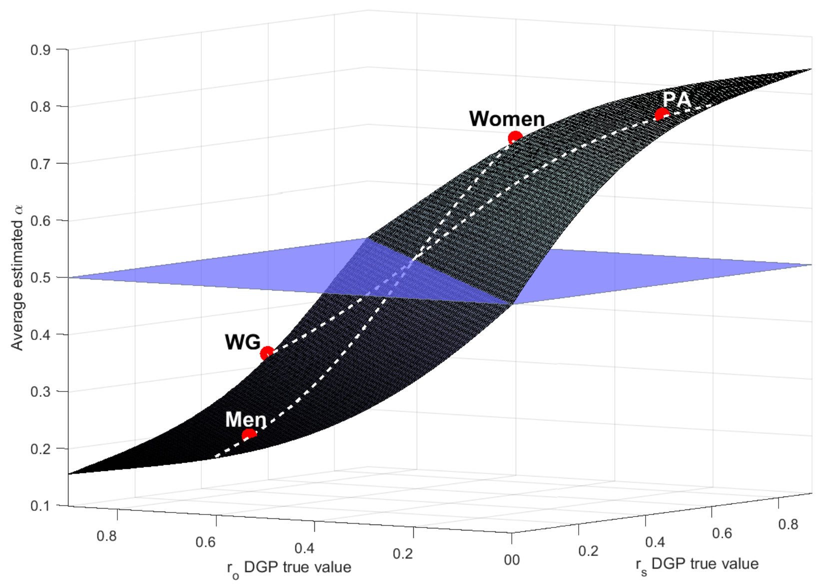
[20] We chose values for $r_{s,female,DGP}$ and $r_{s,male,DGP}$ that are found in the literature. For instance, using the data from Harrison and Rutström (2009) and performing the same estimation, but using the CRRA utility function $u(x) = \frac{x^{1-r}}{1-r}$ instead the power utility function $u(x) = x^r$, we estimated $r_{s,female} = .91$ and $r_{s,male} = .06$.

[21] $\widetilde{r}$ is on average estimated to .35 for men and 0.74 for women.

[22] Null (2011) models pure motives with linear utility and warm glow with concave utility. The results are not sensitive to the exact values chosen for $r_{o,DGP}$.

[23] $\widetilde{r}$ is on average estimated to .35 for individuals motivated by pure altruism and 0.74 for individuals motivated by warm glow.

Figure 4: Mean of the estimated level of altruism $\widetilde{\alpha}$



The red dots represents the values at which $\widetilde{\alpha}$ is estimated in the gender and motives for giving illustrative examples.

$\widetilde{\alpha}_{WG}$) in 100% of the trials.[24] Figure 4 represents the values at which $\widetilde{\alpha}$ are estimated for both groups on a 3-dimensional figure.

Result 3. *Assuming $r_{s,group1} = r_{o,group1}$ and $r_{s,group2} = r_{o,group2}$, when $r_{s,DGP,group1} \neq r_{o,DGP,group1}$ or $r_{s,DGP,group2} \neq r_{o,DGP,group2}$, can lead to incorrect inferences about the relative levels of altruism between groups.*

4.3 Wealth Effects

In our third example, we examine how unobserved wealth may affect the estimated level of altruism. The CRRA utility function displays decreasing absolute risk aversion, meaning that, ceteris paribus, wealthier individuals are willing to take on more risk. Thus, suppose we are comparing two group of subjects. Group W (Wealthy) is given \$20 before playing the dictator game and group P (Poor) is not given anything. Both groups are otherwise identical, with the same level of altruism, $\alpha_{W,DGP} = \alpha_{P,DGP} = .5$, the same curvature of utility over other's payoffs, $r_{o,W,DGP} = r_{o,P,DGP} = .6$, and the same curvature of utility over self payoffs, $r_{s,W,DGP} = r_{s,P,DGP} = .6$. In this case, ignoring the initial wealth of both groups we estimate $\widetilde{\alpha}$ to .62 for the group which received the \$20 and $\widetilde{\alpha}_P = .5$ for the other group.[25] We reject the null hypothesis ($H_0 : \widetilde{\alpha}_W = \widetilde{\alpha}_P$) in 100% of the trials.

[24] In this test statistic we again assume 30 dictators which each make 50 decisions in each groups. The standard errors are clustered by dictator.

[25] $\widetilde{r}$ is on average estimated to .46 in group W and 0.6 in group P.

If we do take into account the initial wealth of both groups in the estimation we reject the null hypothesis that the level of altruism is different in both groups in 5% of the trials.

Result 4. *Ignoring difference in wealth, can lead to incorrectly inferring that wealthier individuals are more altruistic than less wealthy individuals.*

4.4 Risk and Elasticity

Our fourth example addresses the problem of confounding α with the curvature of the utility functions from a different perspective. Here, we show how incorrectly assuming that a \$1 in self payoffs is equivalent to a \$1 in other's payoffs will lead to the conclusion that the curvature between $u(\cdot)$ and $v(\cdot)$ differ, when in truth, they do not. Using the CRRA utility framework, this problem manifests as concluding that individuals have different risk preferences for payoffs to self than payoffs to other (Chakravarty et al., 2011) or that individuals respond differently to incentives when working for self than working for charity (Imas, 2014). Suppose, that $\alpha_{DGP} = \frac{1}{3}$, $r_{s,DGP} = r_{o,DGP} = .6$ and in the estimation we wrongly assume that $\alpha = 0.5$ while allowing r_s and r_o to differ. In that case, $\widetilde{r}_s$ is, on average, estimated to .46 and $\widetilde{r}_o$ to .69. The null $H_0 : \widetilde{r}_s = \widetilde{r}_o$ is rejected in 100% of the trials. We thus underestimate risk aversion over self payoffs and overestimate it over other's payoffs.

Result 5. *Assuming $\alpha = .5$, when $\alpha \neq .5$, can lead to incorrect inferences about r_s and r_o.*

5 Two Approaches to Address Bias

Our simulations demonstrate that estimates of social preferences can be biased if we do not take into consideration the shape of the utility function over both own payoffs and other's payoffs. We suggest two potential solutions to address this. A first approach follows the structural estimation put forth in Andersen et al. (2008), but adapting it to social preferences. An alternative approach follows the calibration exercise in Exley (2015) and allows the researcher to assess whether the assumption of $r_s = r_o$ is innocuous and suggests an upper or lower bound on the social preference parameter. The appropriateness of each of the two approaches we propose below greatly depends on the aims of the researcher.

5.1 A Structural Approach

One method to estimate altruism that accounts for curvature is to experimentally generate data data to identify curvature over own payoffs, curvature over other payoffs and altruism and then estimate all three parameters in a structural model. This follows closely from the time preference literature (Andersen et al., 2008). In fact, Harrison (2018) discuss the numerous applications and advantages of joint estimation of parameters of the utility function and mention the advantages of jointly estimating utility over self and other payoffs in the estimation of social preferences.

In Appendix B, we show that when one allows the curvature over self and other payoffs to differ the coefficient of altruism is estimated without bias. We estimated all parameters of our model on simulated data using the modified dictator game used in Fisman et al. (2007). While we used values of the parameters as found in the literature, and a sample size to simulate the data that is common in many laboratory experiments, we expect that the structural estimation would be more demanding on actual experimental data.

To address the increased demand we propose that an additional direction for research is to develop an experiment that can identify all three parameters, which mimics the design proposed by Andersen et al. (2008). For example, the design would include three distinct tasks: (1) a task to elicit curvature over self-payoffs; (2) an analogous task to elicit curvature over others payoffs; and (3) a task to elicit the coefficient of altruism, such as a modified dictator game.

While jointly estimating the parameters of the utility function has many advantages (see Harrison (2018)), it requires experimental subjects to make many decisions which might not be possible if the researcher has time or budget constraints. Next, we turn to an alternative approach.

5.2 A Calibration Approach

In this section, we propose a simple calibration exercise to help researchers test for the validity of the assumption $r_s = r_o$ and provides either an upper or lower bound on α. The goal of the calibration exercise is to find points of indifference between amounts of money for self and money to the other person or charity for each subject. Exley (2015) provides an easy-to-implement approach to do this. For instance, Exley (2015) finds the amount a subject would give to a charity, \$X, that is indifferent to receiving \$1 for themselves, i.e., where $u(\$1) = v(\$X)$, by presenting subjects with a multiple price list in which the amount to self is held constant (\$1) and the amount given to charity increases incrementally. Subjects are then asked, on each line, whether they prefer \$1 to self (and \$0 to charity) or \$0 for self and some amount \$X for charity, and uses the line where they switch to determine the point of indifference.

Suppose an individual has preferences over payoffs to self (s) and payoffs to another person or charity (o), where we will denote o_n as the o such that the individual is indifferent between s_n and o_n. The idea is to use a multiple price list calibration approach to find the o_n for each s_n contained in the relevant payoff space, S. For example, suppose our decision-maker has CRRA utility and let $S \in s_1 = 1, s_2 = 2, s_3 = 3$ and denote $O \in o_1, o_2, o_3$. We now consider the three relevant assumptions about curvature: (1) $r_s = r_o$; (2) $r_s > r_o$; and (3) $r_s < r_o$. If case 1 is true, then $o_2 = 2 \times o_1$ and $o_3 = 3 \times o_1$. If case 2 is true, then $o_2 < 2 \times o_1$ and $o_3 < 3 \times o_1$. Finally if case 3 is true, then $o_2 > 2 \times o_1$ and $o_3 > 3 \times o_1$. Let the corresponding α for each case be $\bar{\alpha}$, α_{lo}

and α_{hi}, respectively. Thus, when o_i is increasing at a slower (faster) rate than s_i, then it must be that $r_s < (>) r_o$ and $\bar{\alpha}$ is an upper (lower) bound on the true α.

6 Conclusion

This paper has demonstrated that imposing an incorrect restriction of the equality on the curvature of the utility function for self and other, which is ubiquitous in the economics literature, leads to systematically biased estimates of the relative intensity of social preferences. While point estimates are usually taken with "a grain of salt" due to many factors associated with laboratory data, the current paper also demonstrates that extensive comparative static inferences on social preferences, such as based on gender differences, should also be broadly questioned. More generally, the current paper provides a blunt reminder of the critical importance of combining theory, experimental evidence, and econometric analysis to avoid generating seemingly robust yet potentially incorrect inferences across substantive research agendas (such as gender differences in social preferences). The current results stress the critical need for future empirical research on social preferences to relax assumptions on the curvature of preferences over self and other.

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Appendix A Past Literature

Table A1: Example of past Experimental Literature estimating altruism from dictator game

Paper	Setting	Recipient	Assumption	Finding
Measuring Altruism				
Andreoni and Miller (2002)	Laboratory	Other subject	$r_s = r_o$	Choices are consistent with generalized axiom of revealed preferences.
Harrison and Johnson (2006)	Laboratory	Other subject & Charity	$r_s = r_o$	Revealed altruism depends upon the identity of the residual claimant.
Fisman et al. (2007)	Laboratory	Other subject	$r_s = r_o$	Choices are consistent with generalized axiom of revealed preferences.
Comparing Altruism Between Decision Makers				
Andreoni and Vesterlund (2001)	Laboratory	Other subject	$r_s = r_o$	Men are more sensitive to the price of giving.
Eckel and Grossman (1998)	Laboratory	Other subject	$r_s = r_o$	Women are more altruistic.
Cox and Deck (2006)	Laboratory	Other subject	$r_s = r_o$	Women are more sensitive to the price of giving.
Benenson et al. (2007)	Laboratory	Other subject	$r_s = r_o$	Older children and children from higher SES environments are more altruistic.
Carpenter et al. (2008)	Laboratory	Charity	$r_s = r_o$	Students are less altruistic than non student subjects.
Ahmed (2009)	Laboratory	Other subject	$r_s = r_o$	Religious students are more altruistic.
Jacobsen et al. (2011)	Laboratory	Charity	$r_s = r_o$	Nurses are more altruistic than real estate brokers.
Voors et al. (2012)	Laboratory in the Field	Other subject	$r_s = r_o$	Victims of conflict are more altruistic toward their neighbors.
DellaVigna et al. (2012)	Field	Charity	$r_s = 0, r_o > 0$	Social pressure is a determinant of giving.
DellaVigna et al. (2013)	Field	Charity	$r_s = 0$ $r_o \in [0, 1]$	Women are more altruistic.
Fisman et al. (2015)	Laboratory	Other subject	$r_s = r_o$	Subjects exposed to economic recession are less altruistic and more sensitive to the price of giving.
Fisman et al. (2015)	Laboratory & Online Laboratory	Other subject	$r_s = r_o$	Elite students are less altruistic and more sensitive to the price of giving than the average American.
Fisman et al. (2017)	Online Laboratory	Other subject	$r_s = r_o$	Republicans are more sensitive to price of giving than democrats. No significant relationship between voting behavior and altruism.
Comparing Altruism Between Recipients				
Eckel and Grossman (1996a)	Laboratory	Other subject & Charity	$r_s = r_o$	Recipients' perceived worthiness increases giving.
Slonim and Garbarino (2008)	Laboratory	Other subject	$r_s = r_o$	Choosing the recipient increases altruism.
Fong and Luttmer (2009)	Online Laboratory	Charity	$r_s = r_o$	On average, recipient's race does not influence giving.
Fong and Luttmer (2011)	Online Laboratory	Charity	$r_s = r_o$	Recipients' perceived worthiness increases giving.
Comparing Altruism Across contexts				
Benz and Meier (2008)	Laboratory & Field	Charity	$r_s = r_o$	Altruism is more pronounced in the laboratory. Altruism in the laboratory and in the field correlate.

Table A2: Example of past Experimental Literature comparing curvature over self and other's payoffs.

Paper	Setting	Recipient	Experimental task	Assumption	Finding
Comparing Risk Aversion over self and other's payoffs					
Eriksen and Kvaløy (2010)	Laboratory	Other subject	Investment Task	$\alpha = 0.5$	More risk averse over other's than self payoffs.
Chakravarty et al. (2011)	Laboratory	Other subject	Multiple Price List	$\alpha = 0.5$	Less risk averse over other's than self payoffs.
Andersson et al. (2014)	Online Laboratory	Other subject	Multiple Price List	$\alpha = 0.5$	No difference in utility's curvature over self and other's payoffs. More loss averse over other's than self payoffs.
Exley (2015)	Laboratory	Other subject & Charity	Multiple Price List / Dictator Game	-	More risk averse over other's than self payoffs when decision forces trade-off between self and other's payoffs. But no difference in the absence of trade-off.
Rogers (2017)	Laboratory	Charity	Multiple Price List & Bomb Risk Elicitation Task	$\alpha = 0.5$	No difference in risk aversion over self and other's payoffs.
Comparing response to incentives toward self and other's payoffs					
Imas (2014)	Laboratory	Charity	Real Effort Task	$\alpha = 0.5$	When incentives increase, effort toward self payoffs increases but effort toward other's payoffs remain constant.
Tonin and Vlassopoulos (2014)	Online Laboratory	Charity	Real Effort Task	$\alpha = 0.5$	Incentives toward other's payoffs are less effective than incentive toward self payoffs to increase effort but the difference is not large.
Charness et al. (2016)	Laboratory	Charity	Real Effort Task	$\alpha = 0.5$	With high incentives more effort is exerted for self than other's payoffs. With low incentives less effort for self than other's payoffs.
Imas and Loewnstein (2018)	Laboratory	Charity	Real Effort Task	$\alpha = 0.5$	Sensitivity to incentives' scope on effort expenditure toward other's payoffs depends on the tangibility of the outcomes.

Appendix B Unrestricted model: Allowing Different Curvatures of Utility over self and other's payoffs

In this Section, we show that we are able to accurately retrieve the true parameters when estimating the model used in the DGP. We perform the same simulation as in Section 3 and re-estimate the parameter of the utility function used to generated dictators' choices.[26] We therefore estimate the utility function:

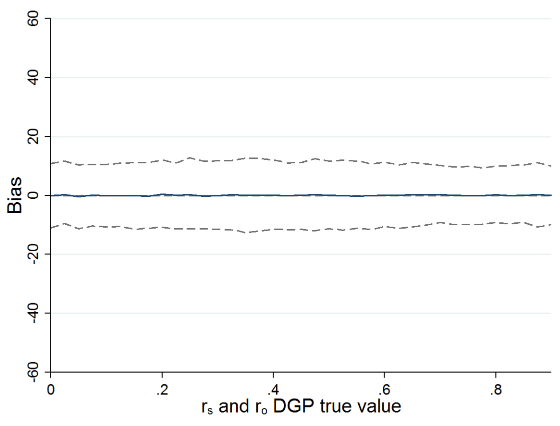
$$u(s,o) = (1-\alpha) * \frac{s^{(1-r_s)}}{(1-r_s)} + \alpha * \frac{o^{(1-r_o)}}{(1-r_o)}, \text{with } r_s,\ r_o < 1, \alpha \in [0,1] \tag{7}$$

Appendix B.1 Bias

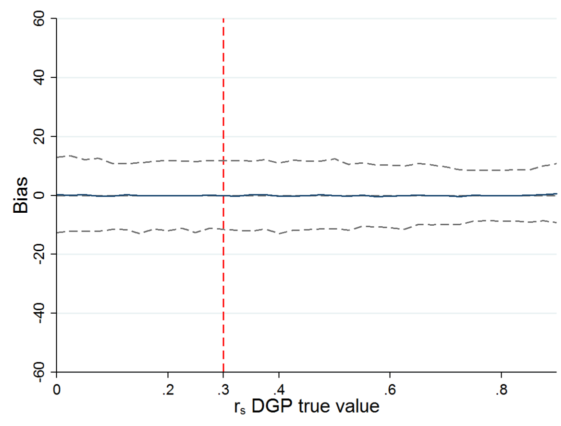
Figure B1 displays the percentage of bias in the estimated $\widetilde{\alpha}$ depending on the true value of $r_{s,DGP}$ and $r_{o,DGP}$. Figure B1c allows $r_{s,DGP}$ and $r_{o,DGP}$ to vary simultaneously while Figure B1a and B1b show two-dimensional slices of Figure B1c. In Figure B1a, $r_{s,DGP} = r_{o,DGP}$ and in Figure B1b, $r_{o,DGP}$ is fixed to 0.3. In all cases, there is no bias; we accurately recover the parameter α.

[26] In this section we perform our simulation $K = 1,000$ times for each case $(r_{s,DGP}, r_{o,DGP}) \in \{0, 0.025, ...0.9\} \times \{0, 0.025, ...0.9\}$ instead of $K = 1,000$ times for each case $(r_{s,DGP}, r_{o,DGP}) \in \{0, 0.01, ...0.9\} \times \{0, 0.01, ...0.9\}$ as in Section 3. We reduce the number of cases to save computing time.

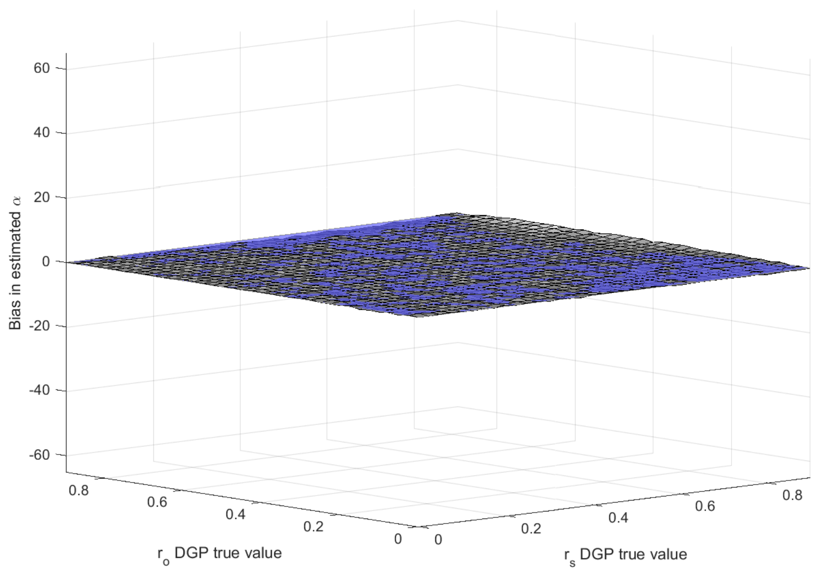
Figure B1: Bias in Estimated Altruism, $\widetilde{\alpha}$



(A) $r_{o,DGP} = r_{s,DGP}$



(B) $r_{o,DGP} = .3$



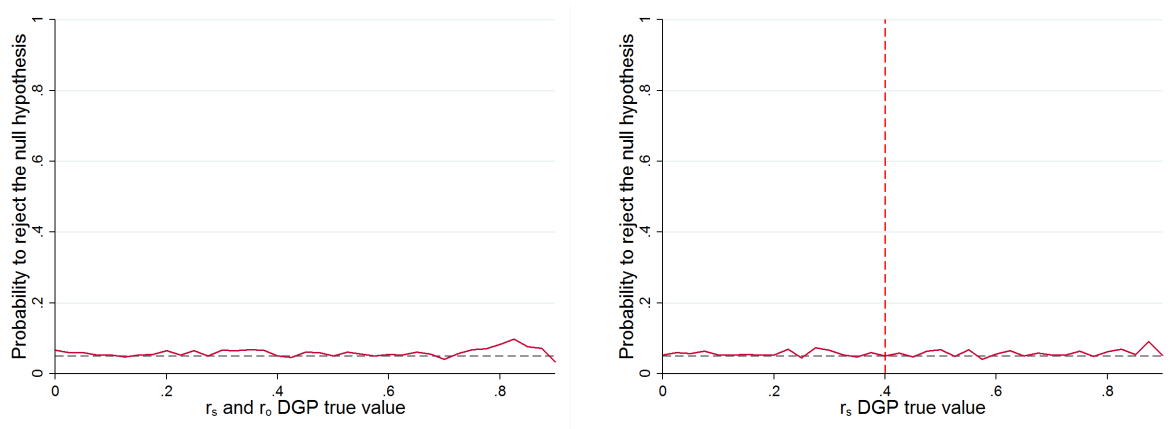
(C) 3-Dimensional

Percentage of under/over estimation of altruism $\widetilde{\alpha}$ for the unrestricted model. In Figures B1b and B1a the dash line is the corresponding 95% Monte Carlo confidence interval.

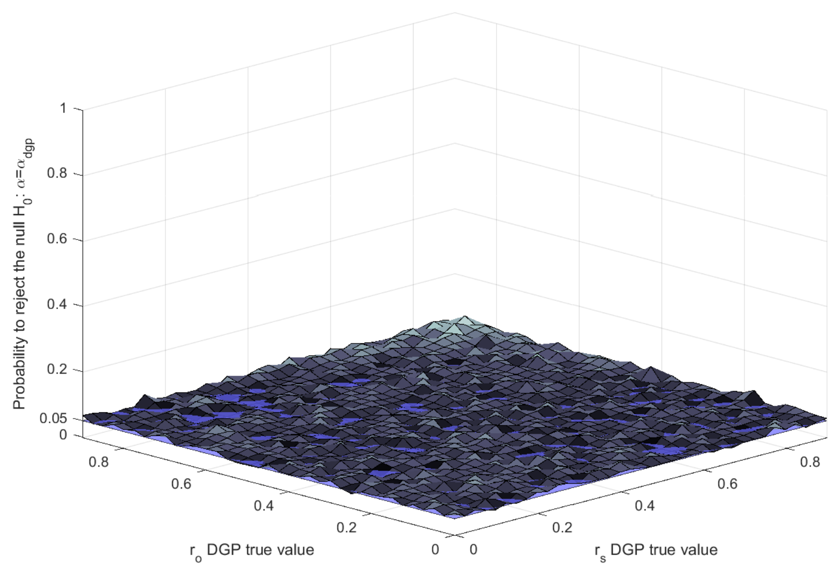
Appendix B.2 Statistical Power

Figure B2 reports the probability of Type I error. We test the null hypothesis that $\widetilde{\alpha}$ equals its true value ($H_0 : \widetilde{\alpha} = \alpha_{DGP}$). In all cases, the test statistics is of correct size; we reject the null that $\widetilde{\alpha}$ is equal to its true value in 5% of the trials at the 5% level.

FIGURE B2: POWER CALCULATIONS, TYPE I ERROR



(A) $r_{s,DGP} = r_{o,DGP}$ (B) $r_{o,DGP}$ IS FIXED TO .3

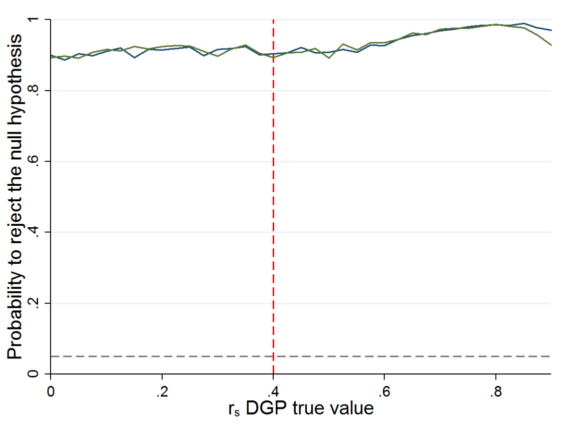


(C) 3-DIMENSIONAL

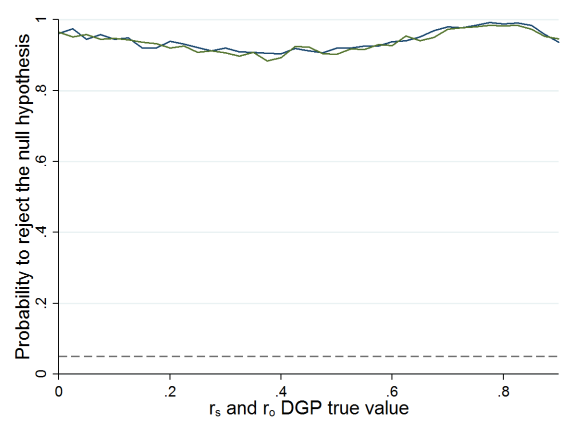
Probability to reject the null hypothesis $H_0 : \widetilde{\alpha} = \alpha_{DGP}$ at the 5% level. 1,000 trials per set of parameters ($r_{s,DGP},\ r_{o,DGP}$).

Figure B3 reports the probability of Type II error. We test two null hypotheses that $\widetilde{\alpha}$ is equal to values distinct from its true value. We report the probability to reject the null $H_0 : \widetilde{\alpha} = .4$ and $H_0 : \widetilde{\alpha} = .6$ at the 5% level. We reject these null hypotheses in at least 85% of the trials in all cases. We therefore have enough power to reject the null when false.

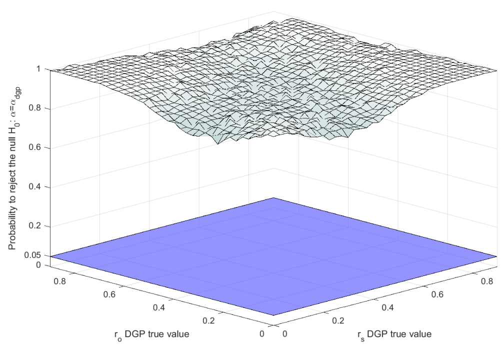
Figure B3: Power Calculations, Type II Error



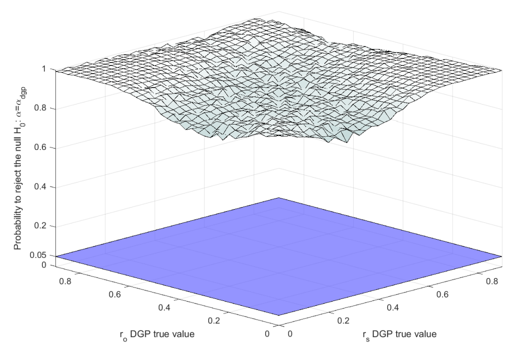
(A) $r_{o,DGP}$ is fixed to .3



(B) $r_{s,DGP} = r_{o,DGP}$



(C) $H_0 : \widetilde{\alpha} = .4$



(D) $H_0 : \widetilde{\alpha} = .6$

Probability to reject the null hypotheses at the 5% level. In Figures B3a and B3b, $H_0 : \widetilde{\alpha} = .4$ (blue) and $H_0 : \widetilde{\alpha} = .6$ (green). Estimated assuming $r_s = r_o$. 1,000 trials per set of parameters $(r_{s,DGP},\ r_{o,DGP})$.

Appendix C Assuming linear utility over self and concave utility over other's payoffs.

DellaVigna, List, and Malmendier (2012) assumed linear utility over self payoffs and concave utility over other's payoffs. In particular, they estimated the utility function:

$$u(s, o) = s + \alpha * \log(\Gamma + o), \text{with } \Gamma \geq 0 \tag{8}$$

We investigate whether this assumption could lead to biased estimates by performing the same simulation as in Section 3[27] but estimating the utility function:

$$u(s, o) = s + \alpha * \frac{o^{(1-r_o)}}{(1-r_o)}, \text{with } r_o < 1 \tag{9}$$

instead of (5).

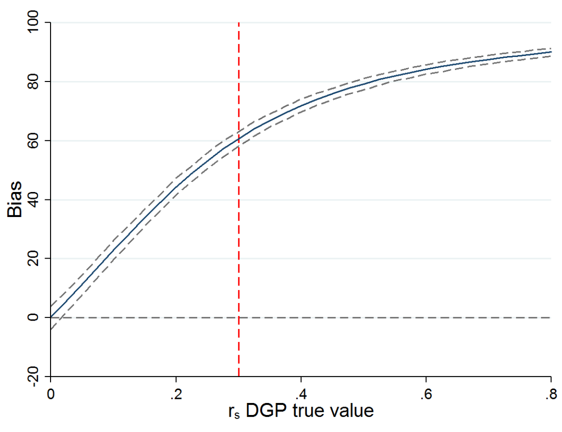
Appendix C.1 Bias

Figure C4 reports the percentage of under/over estimation in the level of altruism given $r_{s,DGP}$ and $r_{o,DGP}$. Figure C4a and C4b show two-dimensional slices of Figure C4c. In Figure C4a, $r_{o,DGP}$ is fixed to .3 and in Figure C4b, $r_{s,DGP}$ is fixed to 0. When the utility over self payoffs is not linear (i.e., when $r_{s,DGP} > 0$), the estimated level of altruism exhibit a substantial upward bias. For instance, when $r_{s,DGP} = .7$ and $r_{o,DGP} = .3$ the altruism level is, on average, estimated to 0.94 which represents an overestimation of 88%.[28] However when $r_{s,DGP} = 0$ (e.g., as in Figure C4b) there is no bias.

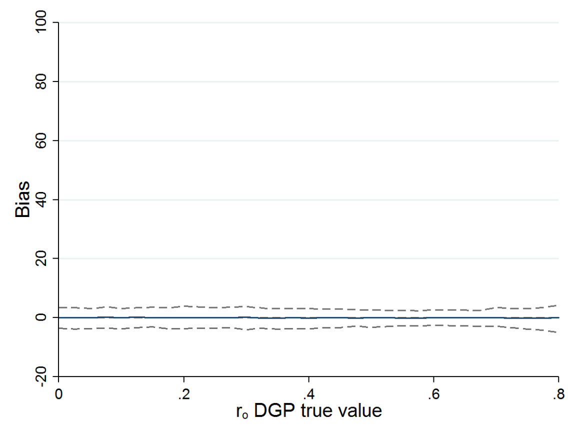
[27] In this section we perform our simulation $K = 500$ times for each case $(r_{s,DGP}, r_{o,DGP}) \in \{0, 0.025, ...0.8\} \times \{0, 0.025, ...0.8\}$ instead of $K = 1,000$ times for each case $(r_{s,DGP}, r_{o,DGP}) \in \{0, 0.01, ...0.9\} \times \{0, 0.01, ...0.9\}$ as in Section 3. We reduce the number of cases and trials per cases to save computing time. We restrained our simulation to the cases where $r_{s,DGP} \leq 0.8$ and $r_{o,DGP} \leq 0.8$ because the Maximum Likelihood is difficult to maximize when we assume $r_s = 0$ and $r_{s,DGP} > 0.8$ or $r_{o,DGP} > 0.8$

[28] In that case r_o is, on average, estimated to 0.54.

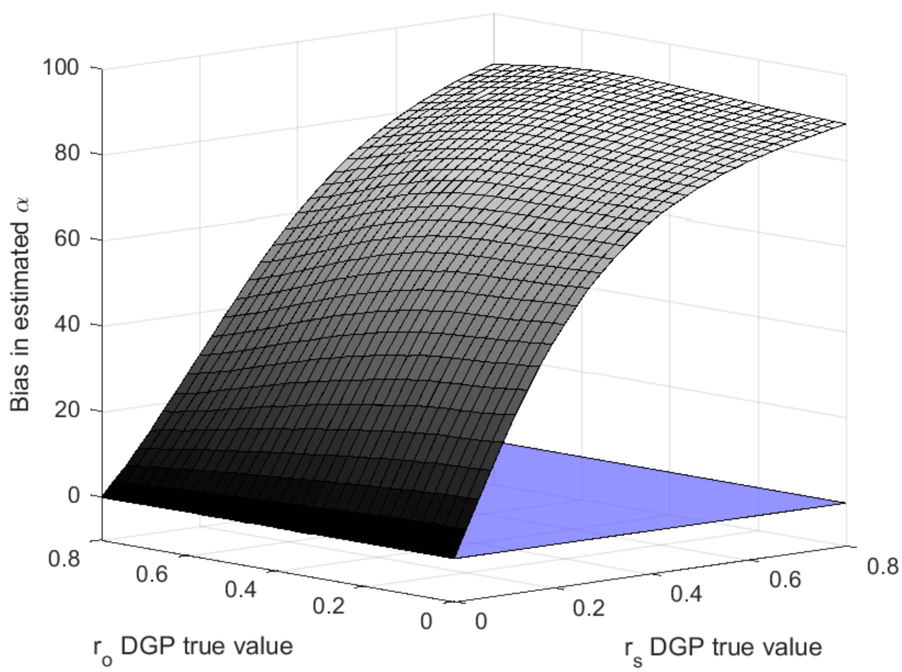
Figure C4: Bias in Estimated Altruism, $\widetilde{\alpha}$



(a) $r_{o,DGP} = .3$



(b) $r_{s,DGP} = 0$



(c) 3-Dimensional

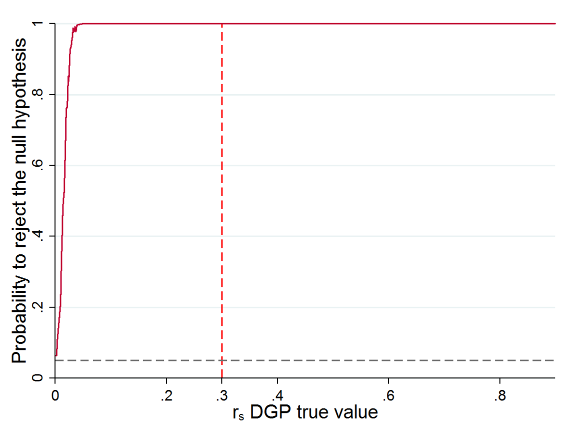
Percentage of under/over estimation of altruism $\widetilde{\alpha}$. In Figures 1b and 1a the dash line is the corresponding 95% Monte Carlo confidence interval.

Appendix C.2 Statistical Power

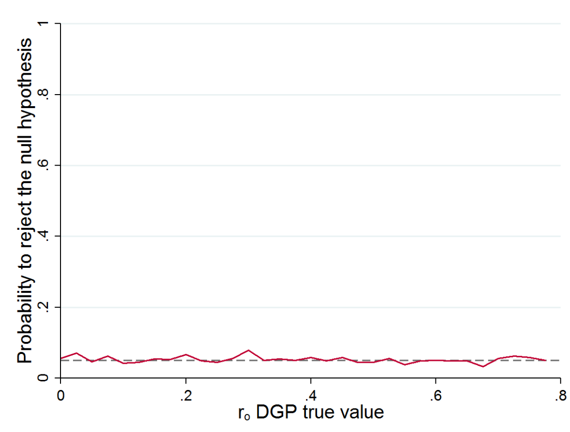
We now turn to the probability of Type I error. Figure C5 shows the probability to reject the null hypothesis that the estimated altruism level is equal to its true value ($H_0 : \widetilde{\alpha} = \alpha_{DGP}$) given $r_{s,DGP}$ and $r_{o,DGP}$. Figure C5a and C5b show two-dimensional slices of Figure C5c. When $r_{s,DGP}$ is equal to 0 (e.g., in Figure C5b) the statistical test is of correct size; we do reject the null in 5% of the trials. But when the utility over self payoffs is concave ($r_{s,DGP} > 0$) we reject the null in too many trials. For instance, when $r_{s,DGP} = .7$ and $r_{o,DGP} = .3$ we reject the null in 100% of the trials. We therefore reject the null even when it's true.

We have seen that $\widetilde{\alpha}$ exhibit a large upward bias. We now explore how likely we are to fail to reject the null that $\widetilde{\alpha}$ is equal to values above its true value. We consider the null hypothesis $H_0 : \widetilde{\alpha} = .6$ and $H_0 : \widetilde{\alpha} = .8$. That is, we test whether the estimated level of altruism is estimated 20% and 60% above its true value. Figure C6 reports the results. For a large set of parameters $(r_{s,DGP}, r_{o,DGP})$ we do not reject the null that $\widetilde{\alpha} = .6$ or that $\widetilde{\alpha} = .8$ in a sufficient number of trials. For instance, when $r_{s,DGP} = .293$ and $r_{o,DGP} = .3$ we reject $H_0 : \widetilde{\alpha} = .8$ in only 5% of the trials. Therefore, assuming linear utility over self payoffs comes at a price; if the utility over self payoffs is instead concave the altruism level will be substantially over-estimated.

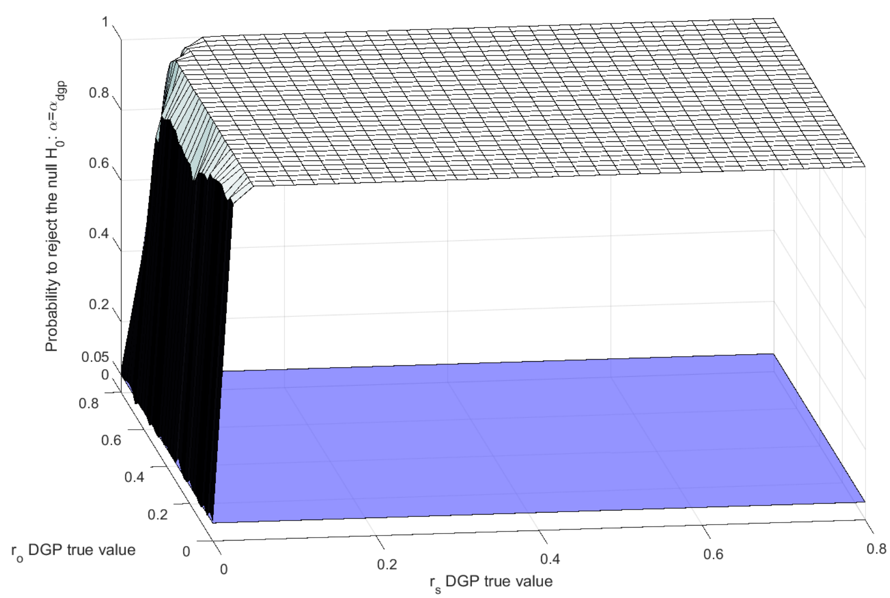
Figure C5: Power Calculations, Type I Error



(A) $r_{o,DGP}$ is fixed to .3



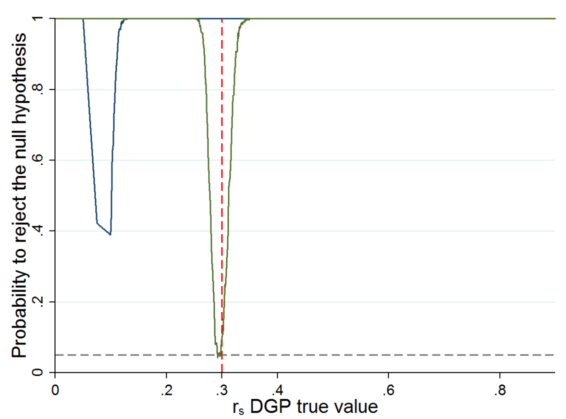
(B) $r_{s,DGP}$ is fixed to 0



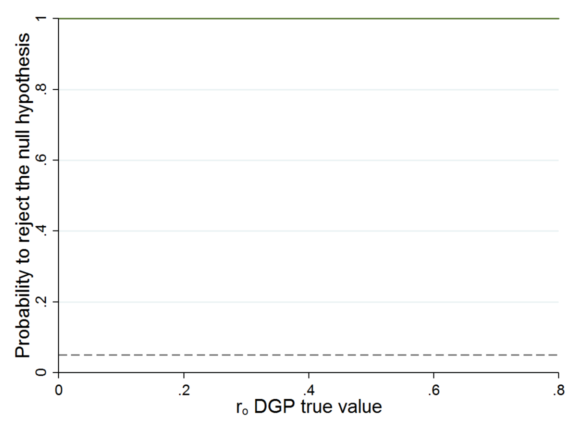
(C) 3-Dimensional

Probability to reject the null hypothesis $H_0 : \widetilde{\alpha} = \alpha_{DGP}$ at the 5% level. 500 trials per set of parameters $(r_{s,DGP},\ r_{o,DGP})$. In panel (A), $r_{s,DGP} \in \{0, 0.001, .., 0.9\}$.

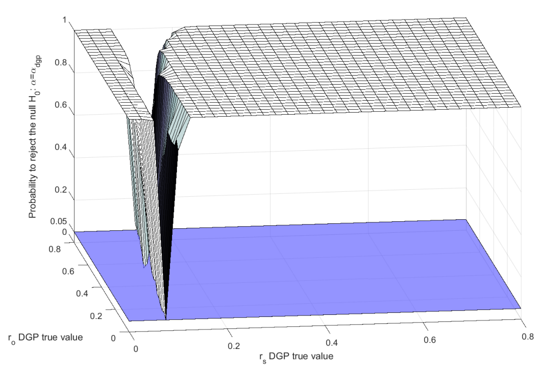
Figure C6: Power Calculations, Type II Error



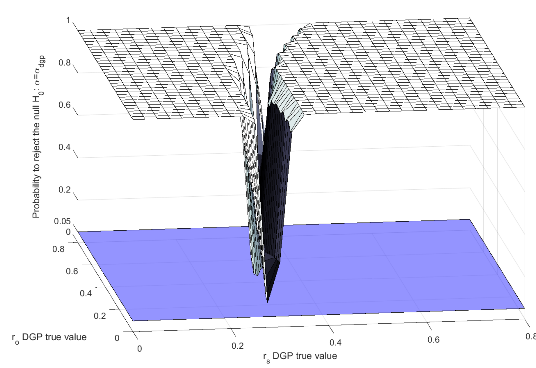
(A) $r_{o,DGP}$ is fixed to 0.3



(B) $r_{s,DGP}$ is fixed to 0



(C) $H_0 : \widetilde{\alpha} = .6$



(D) $H_0 : \widetilde{\alpha} = .8$

Probability to reject the null hypotheses at the 5% level. In Figures C6a and C6b, $H_0 : \widetilde{\alpha} = .4$ (blue) and $H_0 : \widetilde{\alpha} = .6$ (green). 500 trials per set of parameters ($r_{s,DGP},\ r_{o,DGP}$). In panel (A), $r_{s,DGP} \in \{0, 0.001, .., 0.9\}$.

Appendix C.3 An Illustrative Example

DellaVigna, List, Malmendier, and Rao (2013) found women to be more altruistic than men by allowing the distribution of altruism to differ by gender in DellaVigna et al.'s (2012) estimation. To investigate whether this gender difference could be due to the specific assumption made on the utility's curvature we re-investigate the sample examined in Section 4.1.[29] Estimating the level of altruism assuming utility to be linear over self payoffs, we estimate the coefficient of altruism at .59 for men and .95 for women. We reject the null hypothesis of absence of gender difference in 100% of the trials. The gender difference observed in DellaVigna et al. (2013), may therefore be due to the specific assumption they made over self and other's curvature of utility.

[29]In this sample, there is 30 men and 30 women with $\alpha_{male,DGP} = \alpha_{female,DGP} = .5$, $r_{s,female,DGP} = .9$, $r_{s,male,DGP} = .1$ and $r_{o,female,DGP} = r_{o,male,DGP} = .6$.

Appendix D Proof: Relationship between CES and CRRA functional form

In this Section, we show that the CES functional form and the CRRA functional form, when $r_s = r_o$, give the same optimal allocation.

Optimal allocation with the CES utility function

We define the CES utility function as:

$$u_{ces}(s,o) = [(1-\gamma)s^{\rho} + \gamma o^{\rho}]^{\frac{1}{\rho}}, \text{with } \rho < 1,\ \gamma \in [0,1] \tag{10}$$

The DM maximizes (10) subject to $Y = s + p \times o$

The Lagrangian is

$$\mathcal{L}_{ces}(s,o,\lambda) = u_{ces}(s,o) - \lambda(s + p * o - Y) \tag{11}$$

The first order conditions are given by,

$$\frac{\partial \mathcal{L}_{ces}(s,o,\lambda)}{\partial s} = \frac{1}{\rho}(1-\gamma)\rho * s^{\rho-1}[(1-\gamma)s^{\rho} + o^{\rho}]^{\frac{1-\rho}{\rho}} - \lambda = 0 \tag{12}$$

$$\frac{\partial \mathcal{L}_{ces}(s,o,\lambda)}{\partial o} = \frac{1}{\rho}\gamma * \rho * o^{\rho-1}[(1-\gamma) * s^{\rho} + o^{\rho}]^{\frac{1-\rho}{\rho}} - \lambda * p = 0 \tag{13}$$

$$\frac{\partial \mathcal{L}_{ces}(s,o,\lambda)}{\partial \lambda} = s + p * o - Y = 0 \tag{14}$$

Which gives

$$s^*_{ces} = \frac{Y}{1 + p^{\frac{\rho}{\rho-1}}(\frac{\gamma}{1-\gamma})^{\frac{1}{1-\rho}}} \tag{15}$$

$$o^*_{ces} = \frac{Y}{p + (\frac{1-\gamma}{\gamma})^{\frac{1}{1-\rho}} p^{\frac{1}{1-\rho}}} \tag{16}$$

Optimal allocation with the CRRA functional form

The DM maximizes the utility function

$$u_{crra}(s,o) = (1-\alpha) * \frac{s^{(1-r)}}{(1-r)} + \alpha * \frac{o^{(1-r)}}{(1-r)}, \text{with } r < 1,\ \alpha \in [0,1] \tag{17}$$

subject to $Y = s + p \times o$

The Lagrangian is

$$\mathcal{L}_{crra}(s,o,\lambda) = u_{crra}(s,o) - \lambda(s + p * o - Y) \tag{18}$$

The first order conditions are given by,

$$\frac{\partial \mathcal{L}_{crra}(s,o,\lambda)}{\partial s} = (1-\alpha)s^{-r} - \lambda = 0 \tag{19}$$

$$\frac{\partial \mathcal{L}_{crra}(s,o,\lambda)}{\partial o} = \alpha * o^{-r} - \lambda * p = 0 \tag{20}$$

$$\frac{\partial \mathcal{L}_{crra}(s,o,\lambda)}{\partial \lambda} = s + p * o - Y = 0 \tag{21}$$

Which gives

$$s^*_{crra} = \frac{Y}{1 + p^{\frac{r-1}{r}}(\frac{\alpha}{1-\alpha})^{\frac{1}{r}}} \tag{22}$$

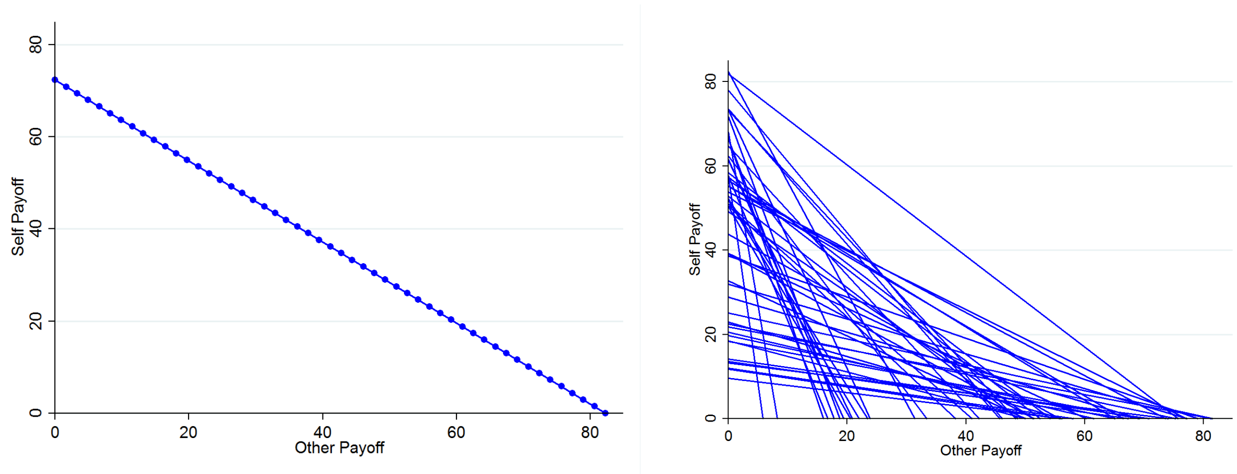
$$o^*_{crra} = \frac{Y}{p + (\frac{1-\alpha}{\alpha})^{\frac{1}{r}} p^{\frac{1}{r}}} \tag{23}$$

Mapping between the two sets of optimal allocation

When $r = 1 - \rho$, and $\alpha = \gamma$ then $(s^*_{ces}, o^*_{ces}) = (s^*_{crra}, o^*_{crra})$

Appendix E Budget lines

Figure E7: Graphical example of randomly generated budget lines

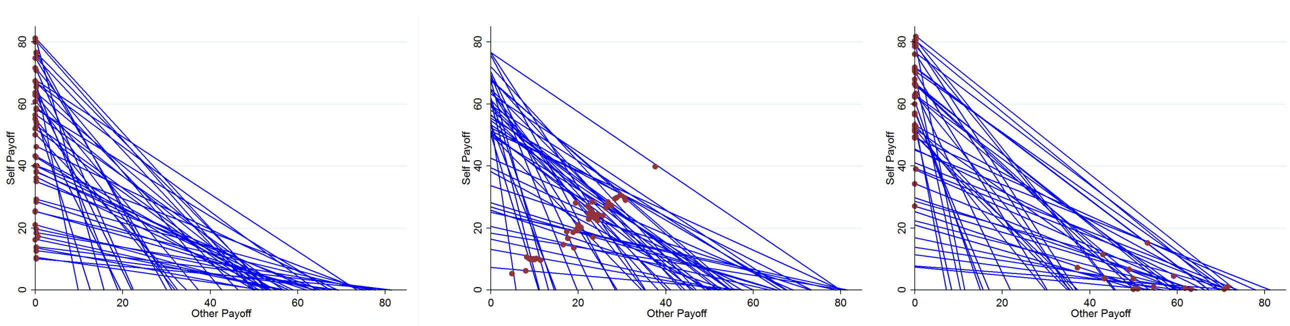


(A) (B)

One randomly generated budget line and the 51 choices that the dictator has to choose among (Panel A). Graphical example of 50 randomly generated budget lines that a dictator could face (Panel B). On the x-axis payoffs for other and on the y-axis payoffs for self.

Figure E8 shows the choices made by representative subjects in FKM07 experiment. On panel A the subject exhibit selfish preference, on panel B preference for decreasing difference in payoffs, and on panel C preference for maximizing total payoff.

Figure E8: Graphical example of choices made by subjects in the FKM07 experiment



(A) (B) (C)

Selfish preference (Panel A), preference for decreasing difference in payoffs (Panel B), preference for maximizing total payoffs (Panel C). On the x-axis payoffs for other and on the y-axis payoffs for self.

Appendix F Robustness to the value of α_{DGP}

In the body of the paper, we used the value $\alpha_{DGP} = .5$ to simulate the data. Here we perform the same exercise with $\alpha_{DGP} = .25$ and $\alpha_{DGP} = .75$. Our results are robust to the choice of α_{DGP}. In this Section, we perform our simulation $K = 500$ times for each case in $(r_{s,DGP}, r_{o,DGP}) \in \{0, 0.025, ...0.9\} \times \{0, 0.025, ...0.9\}$.

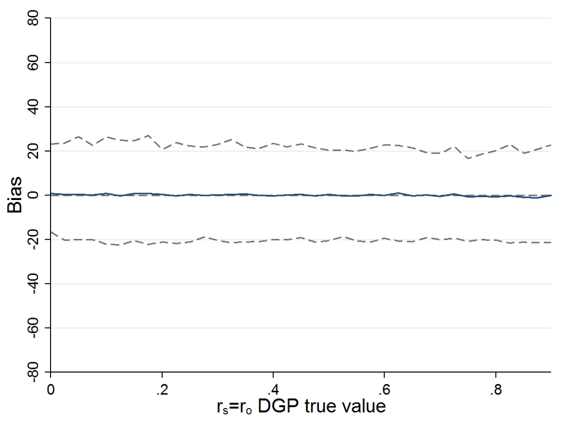
Appendix F.0.1 Unrestricted model: Allowing Different Curvatures of Utility over self and other's payoffs.

First, we reproduce the estimation made in Appendix B. We estimate the utility function:

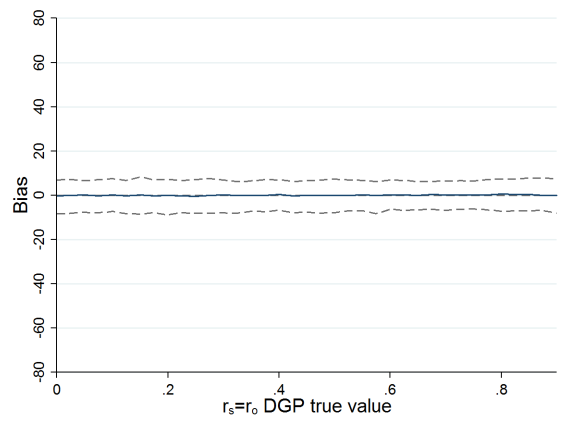
$$u(s,o) = (1-\alpha) * \frac{s^{(1-r_s)}}{(1-r_s)} + \alpha * \frac{o^{(1-r_o)}}{(1-r_o)}, \text{with } r_s,\ r_o < 1, \alpha \in [0,1] \tag{24}$$

Figure F9 shows the percentage of under/over estimation of altruism $\widetilde{\alpha}$ depending on the true value of $r_{s,DGP}$ and $r_{o,DGP}$. On the Left panel, the figures represents the results for $\alpha_{DGP} = .25$ and on the Right the results for $\alpha_{DGP} = .75$. In all cases, there is no bias in the estimation; we accurately retrieved the true parameters when estimating the unrestricted model.

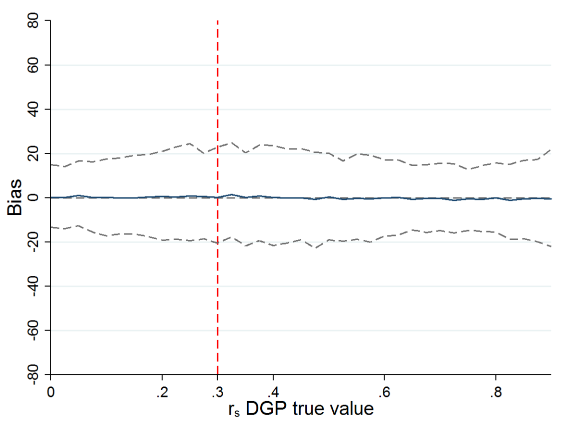
Figure F9: Bias in Estimated Altruism, $\widetilde{\alpha}$ for $\alpha_{DGP} = .25$ (Left) and $\alpha_{DGP} = .75$ (Right)



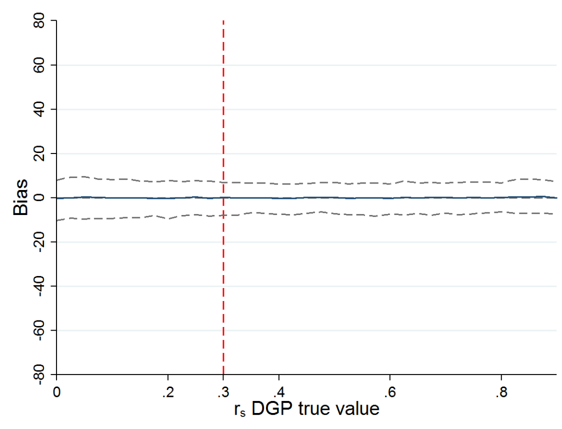
(a) $r_{o,DGP} = r_{s,DGP}$



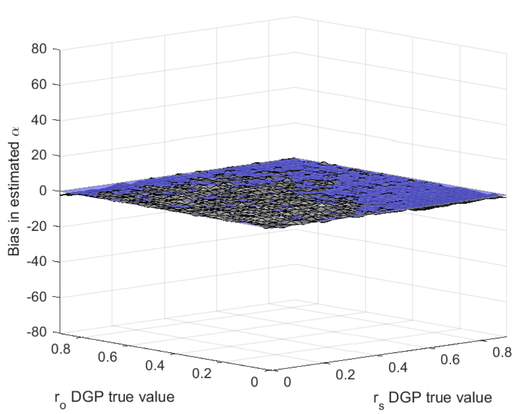
(b) $r_{o,DGP} = r_{s,DGP}$



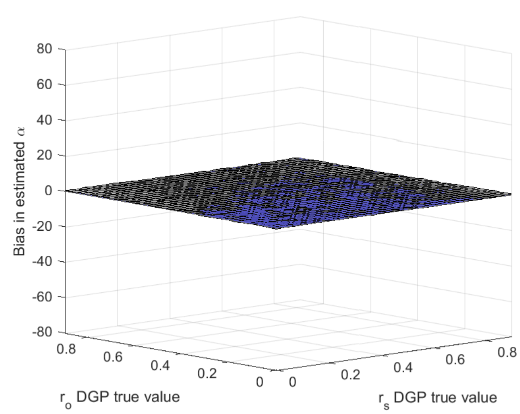
(c) $r_{o,DGP} = .3$



(d) $r_{o,DGP} = .3$



(e) 3-Dimensional



(f) 3-Dimensional

Percentage of under/over estimation of altruism $\widetilde{\alpha}$ for the unrestricted model with $\alpha_{DGP} = .25$ (Left) and $\alpha_{DGP} = .75$ (Right). In Figures F9a, F9b, F9c and F9d the dash line is the corresponding 95% Monte Carlo confidence interval.

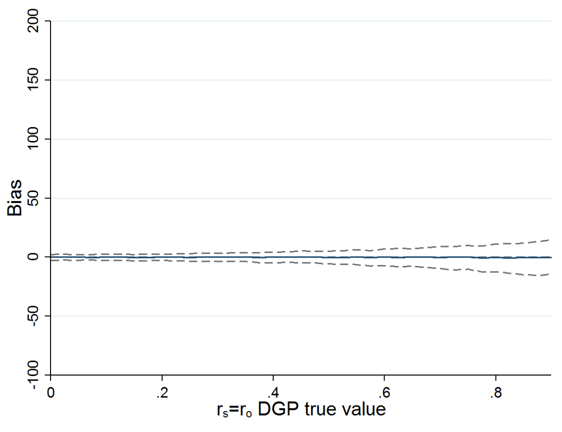
Appendix F.0.2 Assuming identical curvature over self and other's payoffs.

We now impose the restriction $r_s = r_o$ in the estimation to reproduce the estimation made in Section 3.2.1 where we used $\alpha_{DGP} = .5$. We estimate the utility function:

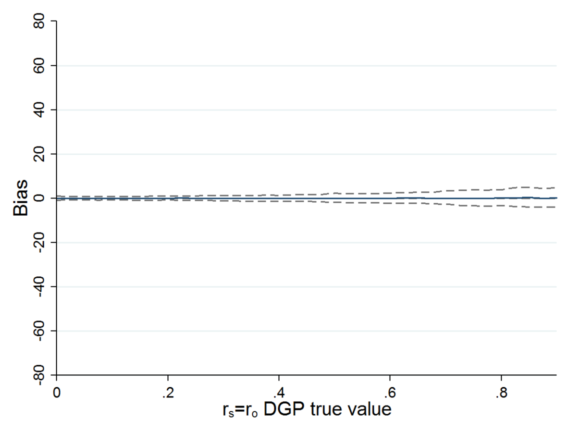
$$u(s,o) = (1-\alpha) * \frac{s^{(1-r)}}{(1-r)} + \alpha * \frac{o^{(1-r)}}{(1-r)}, \text{with } r < 1, \alpha \in [0,1] \quad (25)$$

Figure F10 shows the results. On the Left panel $\alpha_{DGP} = .25$ and on the Right panel $\alpha_{DGP} = .75$. Our conclusions are robust to the choice of α_{DGP}; we overestimate altruism when $r_{s,DGP} > r_{o,DGP}$, underestimate it when $r_{s,DGP} < r_{o,DGP}$ and there is no bias when $r_{s,DGP} = r_{o,DGP}$.

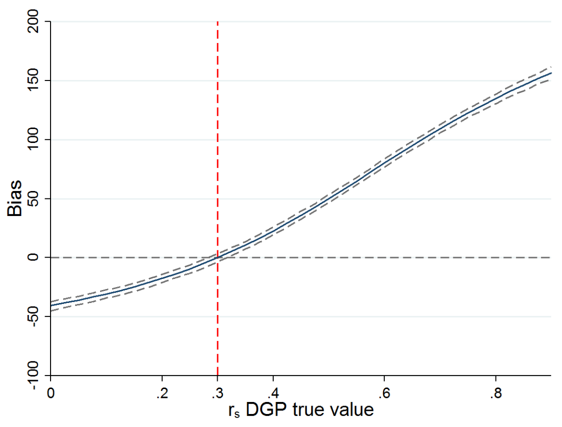
Figure F10: Bias in Estimated Altruism, $\widetilde{\alpha}$ for $\alpha_{DGP} = .25$ (Left) and $\alpha_{DGP} = .75$ (Right)



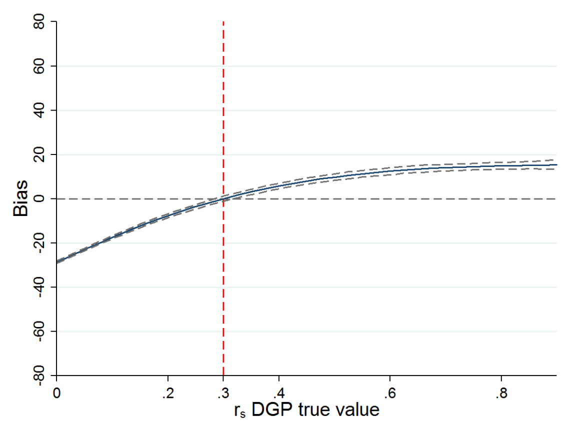
(a) $r_{o,DGP} = r_{s,DGP}$



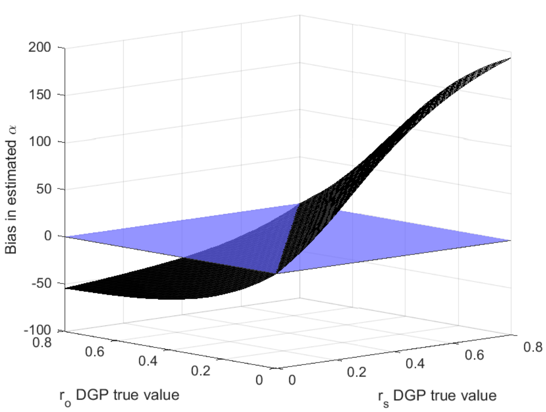
(b) $r_{o,DGP} = r_{s,DGP}$



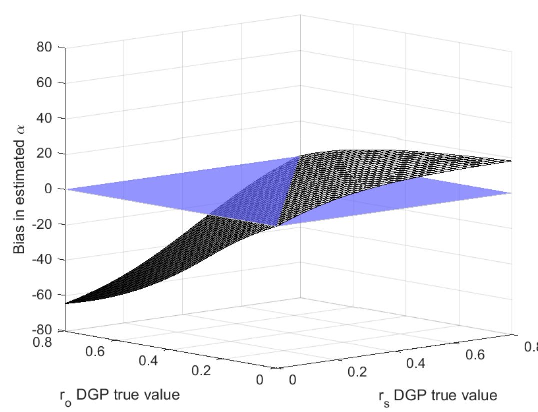
(c) $r_{o,DGP} = .3$



(d) $r_{o,DGP} = .3$



(e) 3-Dimensional



(f) 3-Dimensional

Percentage of under/over estimation of altruism $\widetilde{\alpha}$ when imposing the restriction $r_s = r_o$ in the estimation with $\alpha_{DGP} = .25$ (Left) and $\alpha_{DGP} = .75$ (Right). Note that the scale on the z-axis is not the same on the Left and Right panel. In Figures F10a, F10b, F10c and F10d the dash line is the corresponding 95% Monte Carlo confidence interval.

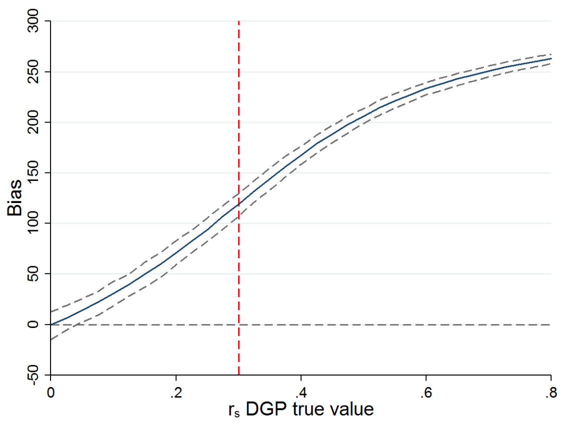
Appendix F.0.3 Assuming linear utility over self and concave utility over other's payoffs.

Finally, we test the robustness of the results presented in Appendix C. We assume linear utility over self payoffs and concave utility over other's payoffs in the estimation. In particular, we estimate the utility function:

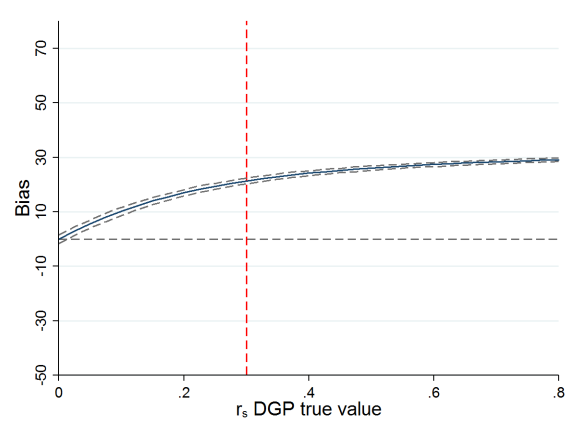
$$u(s,o) = s + \alpha * \frac{o^{(1-r_o)}}{(1-r_o)}, \text{with } r_o < 1, \alpha \in [0,1] \tag{26}$$

Figure: F11 shows the results. On the Left panel $\alpha_{DGP} = .25$ and on the Right panel $\alpha_{DGP} = .75$. Our conclusions are robust to the choice of α_{DGP}; if $r_{o,DGP} > 0$ we overestimate altruism.

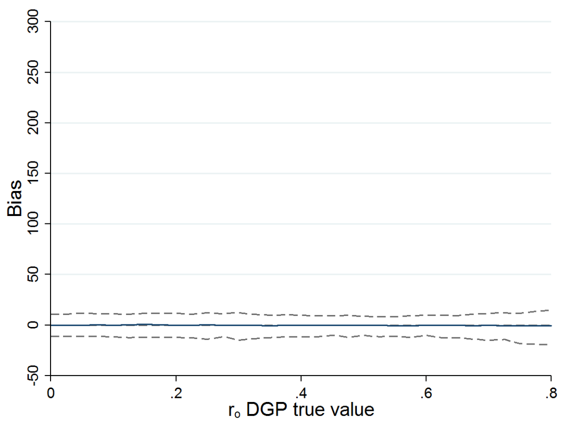
Figure F11: Bias in Estimated Altruism, $\widetilde{\alpha}$ for $\alpha_{DGP} = .25$ (Left) and $\alpha_{DGP} = .75$ (Right)



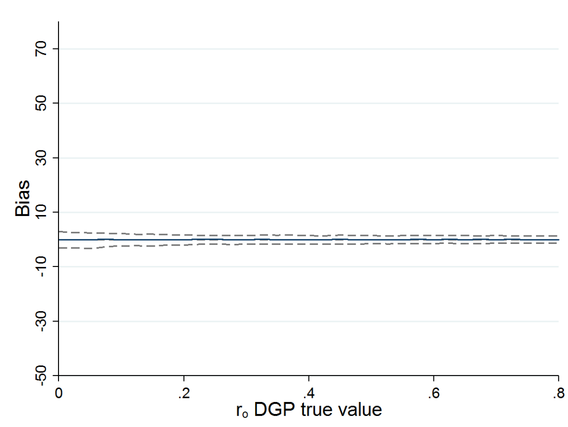
(a) $r_{o,DGP} = .3$



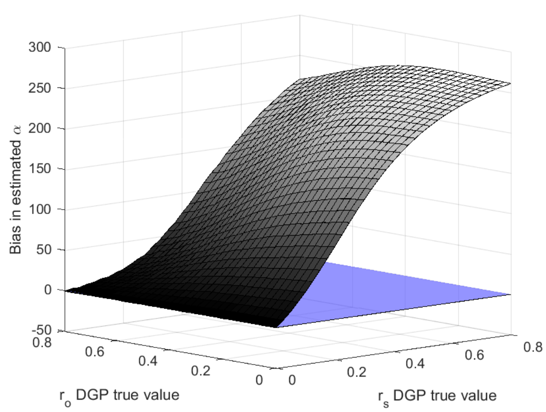
(b) $r_{o,DGP} = .3$



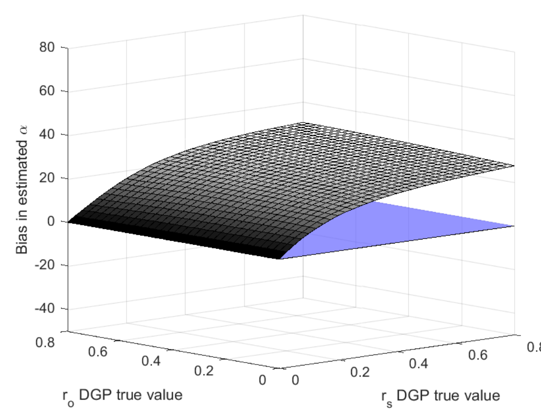
(c) $r_{s,DGP} = 0$



(d) $r_{s,DGP} = 0$



(e) 3-Dimensional



(f) 3-Dimensional

Percentage of under/over estimation of altruism $\widetilde{\alpha}$ when imposing the restriction $r_s = 0$ in the estimation with $\alpha_{DGP} = .25$ (Left) and $\alpha_{DGP} = .75$ (Right). Note that the scale on the z-axis is not the same on the Left and Right panel. In Figures F11a, F11b, F11c and F11d the dash line is the corresponding 95% Monte Carlo confidence interval.