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Dimensions of Donation Preferences: The Structure of Peer and Income Effects

Abstract

Charitable donations provide positive externalities and can potentially be increased with an understanding of donor preferences. We obtain a uniquely comprehensive characterization of donation motives using an experiment that varies treatments between and within subject. Donations are increasing in peers' donations, past subjects' donations, and bonus income. These findings of peer and income effects do not extend to earned income, anonymous donations, or peers' donations of bonus income. A model of an uncertain social norm for giving can explain the patterns here and in several strands of past research. Estimation of the model reveals substantial heterogeneity in subjects' adherence to the norm and perceptions of its form. Correlations between these dimensions of preferences are such that charities with perfect information could increase net revenue using targeted give-aways to certain donors. A simpler fundraising strategy using only the social dimension of donor preferences increases donations by 30 percent.

JEL-Codes: D010, D640, A130.

Keywords: charitable, donation, altruism, warm glow, social preferences, peer effects, experiment.

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1 Introduction

There are a variety of potential reasons for an individual to donate to charity. An understanding of donation preferences can inform the design of mechanisms that address the expected underprovision of the public goods often provided by charities. Economists have developed an expansive literature on motives for charitable giving and methods for increasing donations (Andreoni and Payne, 2013). These studies typically use a small number of treatments to address a single question, such as whether announcing one donor's gift affects other potential donors. Such studies often provide evidence of social influence but rarely discern between theories predicting this result. Estimates of parameters including the stengths of altruism and income effects vary across studies.

To gain a broad understanding of individuals' donation preferences, we conducted a laboratory experiment varying multiple treatments both between and within subjects. Subjects performed tasks for piece-rate compensation, one of which was randomized to induce predictable variation in earnings. When subjects were informed of their earnings they were asked if they would like to donate to a local charity. Subjects were then informed that they would be shown several scenarios, that they could choose a different donation amount for each scenario, and that one of these scenarios would be selected at random for implementation. We constructed these scenarios so as to over-identify the canonical impure altruism model and to offer comparability with past research. Across scenarios, we allowed subjects to condition their donations on many different inputs, including the levels of bonus income, donations of concurrent subjects, and a donation by an anonymous donor. We also included a between-subjects treatment in which we used the results of earlier pilot experiments to inform subjects of either a higher or lower level of past average donations. Our design provides a broad range of results, and we describe these in sets that each inform a strand of the literature.

One set of our results speaks to the long-standing question of whether donations are motivated by altruism. Andreoni (1989) noted that the altruistic model of giving to a public good predicts that others' donations will crowd out one's own, whereas crowd-out may be limited if individuals obtain "warm glow" utility from their own donation. We find that subjects' donations are not crowded out by the gifts of an anonymous donor or by the gifts that other subjects make out of bonus income, both results providing evidence of pure warm glow. This aspect of donor motivation is likely to vary across settings. Brown et al. (forthcoming) vary returns to volunteering and find that it provides relatively strong warm glow. DellaVigna et al. (2012) and Karlan and Wood (2017) both provide evidence that altruism is a relatively important motivation for larger gifts, and Ottoni-Wilhelm et al. (2017) find that donations to a donor-specific cause are primarily driven by altruism. Our results are likely most relevant for charitable donations that immediately follow an economic transaction, such as those solicited outside of a supermarket, as studied by Andreoni and Rao (2011).[1] In our setting, the seller (of labor) is solicited, as in the donations of eBay sellers (Elfenbein et al., 2012) and in workplace giving campaigns like those conducted by United Way Worldwide.

Another set of our results help to distinguish between theories for why donations often increase, rather than decrease, with the amount of donations of others. When allowed to condition their donation on the amount donated by other subjects from either the same laboratory session or pilot experiments, three quarters of subjects chose donation amounts that were increasing in others' donations, resulting in a strong positive relationship overall. Our findings are therefore consistent with positive social influence (e.g., Shang and Croson, 2009; Meer, 2011; Smith et al., 2013). However, we find that gifts from an anonymous donor do not affect subjects' donations, and this is unusual in a literature that has found significant, positive effects (e.g., Huck and Rasul, 2011; Karlan and List, 2012; Huck et al., 2015). That the anonymous donation has no effect provides evidence that the mechanisms of increasing returns in donations (Andreoni, 1998; Marx and Matthews, 2000) and signaling of charity quality (Hermalin, 1998; Vesterlund, 2003; Andreoni, 2006; Potters et al., 2007) are less relevant for these donations. Unlike other studies, ours holds gifts by peers constant, and the results indicate that the giving of peers is paramount, suggesting that in other settings the anonymous donor provides a signal about the behavior of peers.

A third set of our results offers an explanation for the variability in past estimates of the effect of income on charitable giving. We find that random variation in earnings did not affect donations, yet paying subjects a bonus had a significant, positive effect. A review of studies in which income varies shows that the resulting change in charitable giving can range from zero to roughly forty percent of the change in income (Drouvelis and Marx, mimeo). The extremes of this range correspond closely to, respectively, our estimated effects of a £1 change in earned income (£0.04, with standard error 0.06) and bonus income (£0.38, with standard error 0.04). Thus, we show that the nature of income matters for generosity, similar to what Erkal et al. (2011) find for gifts to labmates.[2] Moreover, we find considerable heterogeneity across subjects in the response to financial bonuses, and this can help to explain findings on reciprocity and gift exchange. Many charities provide private benefits to potential donors, such as branded merchandise and invitations to special events, and these can

[1] Donations in our experiment are generally small, as in "point-of-sale" campaigns that solicit buyers in retail stores and restaurants. Although each donation is small, the amount of giving through such appeals is substantial, with a recent survey of 73 point-of-sale campaigns finding that they had raised over $440 million in 2016 (www.engageforgood.com, 2017).

[2] In separate experiments with purely between-subjects designs, we find very similar effects of the two types of income, and we elicit social norms and find that these depend on the amount of bonus income, consistent with the model we propose and estimate here (Drouvelis and Marx, mimeo).

increase donations (Falk, 2007; Sieg and Zhang, 2012). However, such give-aways to potential donors do not always increase donations by enough to cover their cost (Landry et al., 2006; Eckel et al., 2018). We find that achieving a positive financial return from such a strategy would require that a charity conduct sophisticated targeting based on multiple dimensions of donors' preferences.

Results of the experiment appear quite robust. We test for experimenter demand and order effects but find no evidence of either of these. The results was also not specific to local culture, as the same pattern held both for subjects from the UK or EU and for subjects from other (mostly Asian) countries. We do find heterogeneity across subjects, however, as our design allows us to identify responses at the individual level and also to make novel comparisons of a subjects' responses across multiple treatments. Comparing responses across social treatments provides evidence of a split between individuals whose preferences depend on others' choices and individuals whose preferences do not. The relative strength of responses to each social treatment suggests that individuals are most responsive to the choices of their nearest peers.

Given the reduced-form results of our experiment, we revisit and modify our model of preferences for giving. We argue that our results are most consistent with pure warm glow driven by a preference to comply with an uncertain social norm.[3] In this model, subjects learn about and conform to the norm when they learn what others have donated. We estimate this model for each individual subject using donation choices, beliefs about what others have donated (which we elicited with financial incentives), and the parameters of the scenarios they faced. Thus, we allow subject perceptions and preferences to vary. We allow desired (but not observed) donations to be negative, and we use our model estimates to consider counterfactual situations.

When estimating the model, we again find wide variation in the degree to which subjects are motivated by bonus income or by their peers' donations. We also show that these motivations are essentially uncorrelated across subjects. Our analysis of counterfactuals reveals that the charity would not benefit from providing individuals with either bonuses or information about the average donation of others. There are solicitation strategies using bonuses targeted to particular types of donors that can increase net donations, but these require a degree of information that would be rare for a charity. A simpler strategy can increase donations by 30 percent, however, by first soliciting those who are not motivated by their peers' donations and then announcing the average donation in this first round to the individuals who will respond positively to their peers' donations. These results further demonstrate the benefits of our multi-faceted design and show that information about

[3]Social norms are collectively recognized rules for appropriate behavior in a particular social environment (Elster, 1989; Ostrom, 2000). Norms appear to predict behavior in dictator games (e.g., Andreoni and Bernheim, 2009; Krupka and Weber, 2013) and donating to a charity (e.g., Krupka and Croson, 2016; Drouvelis and Marx, mimeo).

dimensions of donor type may or may not be worth acquiring in order to increase the funding of public goods.

Our paper provides a description of the nature of peer effects. Peer effects have been studied in a wide variety of settings, including charitable giving (Shang and Croson, 2009; Meer, 2011; and Smith et al., 2013), criminal behavior (Bayer et al., 2009), energy use (Allcott and Kessler, 2019), financial decisions (Bursztyn et al., 2014), business management (Cai and Szeidl, 2018), participation in public programs (Dahl et al., 2014), science (Waldinger, 2012), workplaces (Hjort, 2014), and especially in education (reviewed by Epple and Romano (2011), with recent contributions including Duflo et al. (2011), Imberman et al. (2012), and Carrell et al. (2013)). We show how one person's choice can affect the choices of that person's peers by establishing a social norm. Andreoni and Bernheim (2009) and Krupka and Weber (2013) provide evidence that social norms predict individuals' behavior in dictator games, and we find that this also applies to charitable giving. Moreover, we show how such norms and prosocial behavior can be shifted by alterring the staging of choices and information transmission across individuals with heterogeneous beliefs and preferences.

The paper proceeds as follows. Section 2 lays out a model of impure altruism that motivates the experimental design. Section 3 describes the design of the experiment. Because there are many facets to the experiment, Section 4 describes a set of empirical procedures for analyzing the data. Results appear in section 5. Section 6 discusses implications for the initial model, proposes and estimates an alternative model, and presents counterfactuals. Section 7 concludes.

2 Impure Altruism Model

The impure altruism model of Andreoni (1989) has become a workhorse for the field and has recently been validated by Ottoni-Wilhelm et al. (2017). We start from a version of this model that allows the "warm glow" component to depend on a variety of factors. The model motivates the variety of treatments that we incorporate in the experimental design.

Consider an individual i. Preferences may vary at the individual level, but for now we omit i subscripts for simplicity of notation. The individual receives income I, makes a charitable gift g, and consumes $c = I - g$. Charitable gifts to the same cause include those from immediate peers, γ_p, those from a wider reference group, γ_r, and those from others outside of this group, γ_o. Total gifts to the cause are $G = g + \gamma_p + \gamma_r + \gamma_o$.

Individuals maximize the utility function $U(g) = u(c) + a(G) + w(g)$. The functions $u(c)$, $a(G)$, and $w(g)$ are all strictly increasing and concave. In addition to the utility of consumption, $u(c)$, this form allows for impure altruism, namely the purely altruistic utility from the public good, $a(G)$, combined with the warm-glow utility obtained from one's own gift, $w(g)$. Warm glow may depend on the level of income or donations by others, and so could be written as $w(g|I, \gamma_p, \gamma_r, \gamma_o)$, but we leave this dependence implicit for notational simplicity.

The choice of g to maximize $U(g)$ gives the first-order condition $0 = \frac{dU}{dg} = -u'(c) + a'(G) + w'(g)$. We seek to understand how factors such as income and the donations of others affect one's own donation. The theoretical effect of changes in these variables on gifts can be captured by differentiating the first-order condition. For example, if income increases, then we have $0 = \frac{d}{dI}\frac{dU}{dg} = -\left(1 - \frac{\partial g}{\partial I}\right) u''(c) + \frac{\partial g}{\partial I} a''(G) + \frac{\partial g}{\partial I} w''(g) + \frac{\partial}{\partial I} w'(g)$, and therefore

$$\frac{\partial g}{\partial I} = \frac{\frac{\partial}{\partial I} w'(g) - u''(c)}{-\left(u''(c) + a''(G) + w''(g)\right)}.$$

Similarly, $\forall j \in \{p, r, o\}$,

$$\frac{\partial g}{\partial \gamma_j} = \frac{a''(G) + \frac{\partial}{\partial \gamma_j} w'(g)}{-\left(u''(c) + a''(G) + w''(g)\right)}.$$

These expressions motivate many of the treatments employed in the literature on charitable giving. In each expression, the denominator is strictly positive because all terms within the outer parentheses are negative. Hence, the sign of the derivative provides information about the terms in the numerator. When income increases, the resulting decrease in the marginal utility of consumption will have a positive effect on gifts. Income may also affect warm glow, and while the sign of $\frac{\partial}{\partial I} w'(g)$ is not theoretically determined, it is expected to be nonnegative, and unless it is sufficiently negative to overcome the effect on the marginal utility of consumption, income should increase giving. When gifts by others increase, the negative term $a''(G)$ in the numerator captures the negative effect of diminishing marginal utility derived from contributions to the public good. The second term captures the effect on warm glow, which could go in either direction, and if it is not positive and sufficiently large, then the entire expression will be negative. Absent (unmodeled) signaling, if donations by others increase one's own donation, then warm glow must be of greater marginal importance than altruism at the current values of all variables.

The baseline model motivates a variety of treatments meant to uncover the structure of the utility function. In particular, we experimentally vary income and donations of others in a variety of ways that are described in the next section. If we were to impose a parametric structure on the baseline model, then these treatments would provide over-identification for structural estimation of the model. Our findings, however, suggest a number of limitations of this model. We return to the theory in Section 6 to discuss these limitations and propose an alternative model of preferences.

3 Experimental Design

Our paper follows others that have used within-subject designs to study prosocial behavior. Andreoni and Miller (2002), Fisman et al. (2007), and Korenok et al. (2013) randomize budget sets in dictator games to study rationality. Deb et al. (2014) test how many subjects' choices of donation to a charity can be rationalized by various utility functions. Our study is most like those of Lilley and Slonim (2014) and Ottoni-Wilhelm et al. (2017) in that they study donations to charity and provide Tobit estimates of preference parameters. Our study varies the most inputs in order to study multiple motives for charitable donations and the correlations and interactions between these motives.

The experiment occurred in two steps. In the first step, subjects performed real-effort tasks that allowed them to generate income. In the second step, subjects were allowed to donate part of their earnings to a local charity. We describe each part in turn.

3.1 Real-effort tasks

Subjects performed two types of tasks: math and language tasks. All subjects completed at least one of each type of task so as to allow for heterogeneity in ability across tasks (Niederle and Vesterlund, 2010). For both tasks, items were presented to subjects on a computer screen. Subjects would type in an answer and click the "Submit" button. After each submission, a new item was immediately shown. For the math task, subjects were asked to multiply two two-digit numbers. For the word task, each subject had to arrange four pairs of letters to form a word. Subjects were told that they must use all pairs of letters to form the correct word and can re-arrange the order of the pairs but not the order of the letters within each pair. Two sheets of scratch paper and a pen were provided, but no other form of assistance was available. In each task, subjects were continuously shown the amount of time remaining.

Subjects performed three tasks. They completed the language task first and the math task second. Each of these tasks lasted two minutes and thirty seconds. For the third and final task, subjects were randomly assigned to perform either the language or math task, this time for a full five minutes. Subjects earned 25 pence for each correct response in the word task and 50 pence for each correct response in the math task. [4]

[4] Prior to conducting our experiment, we ran two pilot experiments in which we varied the number and types of tasks that subjects had to perform as well as the structure of donation choices. We describe both pilots in Appendix 7.

3.2 Donation choices

Subjects were given the opportunity to donate part of their earnings upon completion of the tasks. As with many naturally occurring solicitations, subjects were not made aware of the solicitation until it occurred. Donations were also kept private so as to minimize complications related to image motivation (Ariely et al., 2009; Filiz-Ozbay and Ozbay, 2014). We refer to the donation chosen at this point as the "first-opportunity donation" or the "Scenario-1 donation." Subjects were then asked to guess the average first-opportunity donation among other subjects in their session. Subjects' responses were incentivized in that estimates within £0.10 of the correct amount earned the subject an additional £1.

We then presented subjects with a series of incentivized scenarios designed to disentangle possible motivations for their donations. The instructions informed subjects that one of the scenarios would be selected at random and implemented after all choices had been made. The exact instructions for all donation scenarios, along with the rest of the experiment, appear in Appendix 7.[5] Scenario 2 simply repeated the offer to donate so as to test whether the knowledge that donation choices were being studied would induce experimenter demand effects (Zizzo, 2010). The remaining scenarios were eached designed to test for a category of income or peer effects:

- *Labmates' donations*: In these scenarios we allowed each subject to condition the amount she would donate on the average donations of the other subjects in her session. In particular, subjects were asked to indicate how much they wished to donate if others' average first opportunity donation lay in each of the following ranges: i) at least £0.75 but less than £0.80 per person; ii) at least £1.20 but less than £1.25 per person; iii) at least £1.65 but less than £1.70 per person; iv) at least £2.10 but less than £2.15 per person; v) any other amount.

- *Anonymous donations*: In these scenarios we again asked subjects to choose a donation for each of the ranges of labmates' first-opportunity donations, but subjects were also informed that an anonymous donor ("Donor X") would augment this amount by donating an extra £0.45 per person. The researchers made these donations to the charity for all sessions in which this scenario was randomly selected for implementation.

- *Bonus income:* These scenarios explored how subjects' donations depend upon their receipt of windfall bonus income. Subjects received a £1 bonus in one scenario and £2 in another. In separate scenarios, subjects were informed that half of the participants in the session would

[5] We also randomly assigned subjects to one of two orderings of the scenarios. Relative to the sequential ordering in Appendix 7, the second ordering was {1, 2, 3, 9, 4, 5, 6, 7, 8, 10, 11, 12, 14, 13}.

receive a £2 bonus and the other half would receive no bonus. Subjects were then informed to which half they were assigned. We elicited subjects' donation decisions for both cases (receiving the £2 bonus or receiving no bonus), randomly assigning the order in which these cases were presented.

- *Making donations for others:* In these scenarios we informed subjects that they would allocate a £2 bonus between another subject and the charity, i.e. the other participant received a £2 bonus minus the donation chosen by the subject. Subjects were also asked to again choose a donation amount for themselves.
- *Information about past donations*: In these scenarios we tested for the influence of the amount donated by subjects in "similar sessions" conducted earlier in the year (namely the pilot experiments described in Appendix 7). In one scenario we allowed each subject to condition on the amount of past donations using the same ranges as in the labmate-donations scenarios. In a subsequent scenario we revealed the actual average amount donated in a past experiment, which we truthfully randomized between £1.225 and £2.135 using the results from the respective pilot experiments. After subjects made their donations they were again asked to estimate the average of their own labmates' first-opportunity donations, again receiving a £1 incentive payment for a guess within £0.10 of the correct amount. We then repeated several scenarios to test for interactions between the effects of past donations and other motivations.

After all subjects had completed the donation scenarios, we randomly selected one scenario for implementation. Subjects were told which scenario had been selected, what donation amount they had chosen in this scenario, and their final take-home pay, including incentives and bonus payments. Finally, subjects responded to a post-experiment questionnaire that elicited demographic characteristics and administered the Cognitive Reflection Test (CRT), a measure of one's proclivity for reflection that is correlated with cognitive outcomes (see Frederick, 2005).

In total, the experiment included 169 subjects. We removed three outlier subjects whose donation amounts varied by more than £8 across scenarios. On average, subjects earned £12.49 for attending and performing tasks.[6] All experiments were conducted in the Birmingham Experimental Economics Laboratory (BEEL), and all treatments were computerized and programmed with the Multistage software from Caltech. Sessions lasted, on average, 65 minutes.

[6] At the time of the experiment £1 was equivalent to US$1.45.

4 Methods of Analysis

Our experimental design is considerably more involved than a basic design with one treatment group and one control. Multiple aspects were randomized across subjects, and because subjects make many decisions, we can also examine changes within subject. This complexity necessitates the use of a combination of analytical methods. In this section we describe our methods according to the type of regression so that the results in the following Section 5 can be organized according to the nature of their content.

Between-subject regressions

We randomly assigned several aspects of the experiment across subjects: whether the third task involves math or words, the order of scenarios, the order in which they receive bonuses of £0 vs. £2, and whether they are told the high or low amount of donations in a past session. For these treatments we estimate the following regression:

$$Y_i = \alpha_0 + \alpha_1 T_i + \alpha_2 X_i + \epsilon_i \tag{1}$$

In equation (1), the dependent variable is the donation of individual i in the relevant scenario. The key independent variable is an indicator for treatment, T_i. Random assignment of treatment mitigates correlation with unobserved subject characteristics, ϵ_i, that may affect donations. The coefficient α_1 therefore gives a consistent and unbiased estimate of the impact of the treatment on donations. As a robustness check (or to increase precision) we also sometimes include covariates, X_i, such as the baseline donation before the subject receives the signal about past donations. We also sometimes increase precision by defining the outcome, Y_i, as the change in a subject's donation from a scenario that preceded the treatment to a scenario that followed treatment. In all between-subject regressions, we use heteroskedasticity-robust standard errors.

Within-subject regressions

In a number of cases we are interested in how a subject's donation changes across scenarios or across conditioning sets within a scenario. For example, in a few scenarios we allowed subjects to choose a different donation for each range in which average donations of others might lie. For these scenarios we exploit the within-subject design by estimating regressions with subject fixed-effects, which absorb differences across subjects in the overall level of generosity.

$$Y_{il} = \beta_0 + \beta_1 D_{il} + \delta_i + \epsilon_{il} \tag{2}$$

The dependent variable in the regression, Y_{il}, is the donation of subject i when others' donations fall in range l, where $l = 1, 2, 3, 4$. For the independent variables, D_{il}, we use either dummy variables for each range or a single continuous variable that takes the value of the middle of range l. We also include an indicator variable for an anonymous donation, when relevant, to capture how the anonymous donation changes the subject's own donation. Controlling for individual fixed effects, δ_i, allows us to single out average within-subject changes, as captured by coefficient β_1. We cluster standard errors by subject when estimating these within-subject differences.

Slopes

The within-subject design also allows us to examine how a given subject's choice varies over related scenarios. We focus on two individual-specific responses: the response of individual donations to labmates' donations and the response to bonuses. Both of these inputs vary within-subject, allowing us to identify subject-specific slopes. To estimate individual-specific responses to labmates' donations, we augment equation (2) with the interaction of others' donation amount, D_{il}, with the individual fixed effects, δ_i. The coefficients on the interaction terms are individual-specific responses to labmates' donations. We examine the distribution of these responses across individuals and correlations between these and other subject characteristics. We estimate subjects' responses to bonus income similarly, pooling all bonus scenarios that precede the information about average past donations.

Earnings IV

To investigate the impact of earned income on donations we use an instrumental variables (IV) framework. This strategy is necessitated by the fact that earned income is endogenous by definition. It could be, for example, that an omitted factor (such as self-interest) causes some individuals to earn more and donate less, and this would cause a downward bias in OLS estimates if the causal effect of income is positive. We therefore use the random assignment of tasks to instrument for earned income. A simple way to do this would be to see how assignment to the math task affects earnings and donations, then scale the latter effect by the former. Doing so provides results similar to those that we will present, but if solving math problems directly affects donation behavior, then this simple approach will give a biased estimate of the earnings effect. Our IV strategy avoids this assumption by exploiting both the randomization of Task 3 and our knowledge of subjects' relative abilities in the two types of tasks.

Task 3 was randomly assigned to be either the word problem from Task 1 (worth £0.25 per correct answer) or the math problem from Task 2 (worth £0.50 per correct answer). Because subjects had already performed both types of task, we can control for their performance in each and use the randomization to predict their performance in Task 3. For a subject assigned to repeat Task $j \in \{1, 2\}$ in Task 3, we refer to Task j as the "relevant task." We use earnings in the relevant task as an instrument for total earnings. The 2SLS estimating equations are as follows.

$$Y_i = \theta_0 + \theta_1 Earnings_i + X_i\theta_2 + \epsilon_i \tag{3}$$

$$Earnings_i = \gamma_0 + \gamma_1 Instrument_i + X_i\gamma_2 + e_i \tag{4}$$

Equation (3) is the second stage, where the dependent variable, Y_i, is the donations of individual i. The instrumented variable, $Earnings_i$, is total earnings from all three tasks. In the regression, we control for Task 1 and Task 2 earnings (in the vector X_i), and therefore the variation in total earnings is driven by earnings in Task 3. Equation (4) is the first stage regression. We regress total task earnings on the instrument of earnings in the relevant task. This instrument exploits the fact that Task 3 is randomly assigned and that performance is predictable given performance in the relevant task completed earlier. The instrument is not collinear with the controls because it takes the value of Task 1 earnings for some subjects and the value of Task 2 earnings for other subjects. Because the time allotted for Task 3 was twice the amount of time allotted for each of Tasks 1 and 2 we expect a coefficient of roughly 2 in the first stage.

Lasso

We also test for links between individual responses to treatments and the background characteristics we obtained through the survey. To do so, we employ the Lasso method of Tibshirani (1996). Lasso is an empirical tool for selecting from among non-nested regression models. In our setting we have many subject characteristics that could predict each donor type. Lasso provides a systematic way of identifying which of these characteristics are of statistical importance.

5 Results

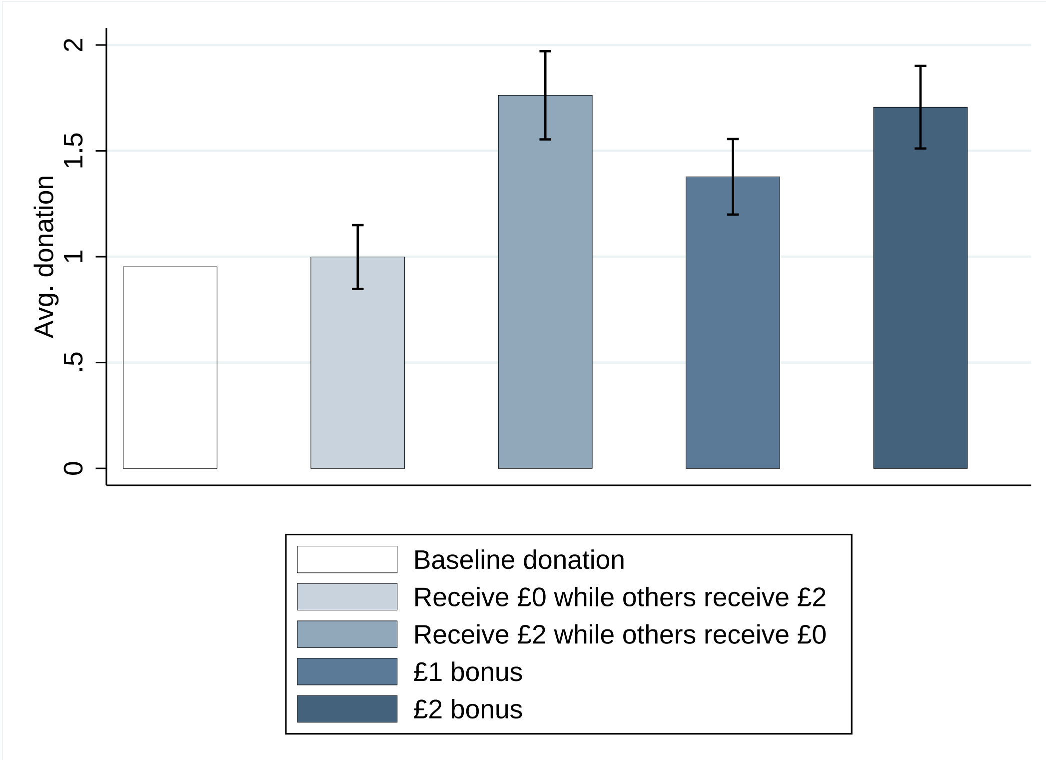
We first examine results for individual factors that could affect giving: income, the opportunity to donate for others, the amount of donations by others, and information about past donations. After showing which of these factors influence donations we address several potential concerns: specificity of results to a specific culture/nationality, experimenter demand effects, and ordering effects. None of these concerns appear to have any power to explain the behavior we observe. Lastly, we compare subjects' responses to multiple factors and find that information, giving of others, and giving for others are closely linked, but responsiveness to bonuses is unrelated to responsiveness to these other factors.

5.1 Factors of interest

5.1.1 Income

Figure 1 shows that receiving a bonus has a strong effect on donating. Donations are significantly higher than at baseline when the subject receives a bonus of either £1 or £2, and the larger of these bonuses has a greater effect on the donation. Pooling scenarios 5, 6, 7, 8 and the baseline scenario 2, we estimate that on average each £1 of bonus income increases the donation by £0.379 (w/ standard error 0.038). The last two bars of the figure, when compared to the first and third, show that varying the bonus paid to other subjects did not alter a subject's own donation. The same pooled regressions indicate an insignificant effect of less than £0.05 per £1 of bonus paid to other subjects.

Figure 1: Donation response to bonus income



Notes: The first bar represents average baseline (scenario 2) donation. Rest four bars correspond to average donations in four different scenarios, together with 95% Confidence Intervals (CI) indicating whether each average donation is statistically different from the baseline donation. The CIs are estimated from regressions of donations on a scenario dummy. For instance, the 95% CI of the second bar comes from a regression where we stack baseline donations and donations in the scenario when subjects receive £0 and others receive £2, and regress donations on a dummy variable indicating the latter scenario and individual fixed effects to single out changes. The line segment is the 95% CI of the coefficient of the dummy variable. Standard errors are clustered by individual in these regressions to adjust for serial correlations within each individual across scenarios. When the top line of the first bar doesn't overlap with the CI, this means that the difference between these two bars is statistically significant at 95% level.

While bonus income strongly increased donations, earnings from tasks did not. The two panels of Table 1 show the two stages of our Two-Stage-Least-Squares regressions as well as the Ordinary-Least-Squares counterpart. In Panel (a), we see that the earnings instrument, earnings from the task that was randomly chosen to be repeated, has a highly significant effect on total earnings. Subjects were given twice as much time to work on the third task, and thus the coefficient on the instrument is close to two. The F statistic exceeds 200, indicating that the instrument is quite strong. In Panel (b), we see the effects of earnings on donations in each of the first two scenarios. All of the estimates are small, and estimated effects on the donation in Scenario 2, just after the experiment is explained, are negative in sign. The OLS estimates are close to those of the IV, suggesting that their bias is limited.[7] The IV estimates have larger standard errors but are sufficiently precise that

[7]If we control for earnings in each of the first two tasks, as we do in the IV regressions, reduces both OLS estimates. One potential interpretation explanation of this pattern is that generosity is positively correlated with ability but negatively correlated with effort.

the confidence intervals on each would rule out an effect larger than a £0.17 change in donations for each £1 of additional earnings. Figure C.1 in Appendix 7 provides a scatter plot of donations against predicted earnings, providing visual evidence that there is no relationship between the two.

Table 1: Donation response to earned income

(a) IV first stage

	(1) Earnings from Tasks
Earnings instrument	1.8400***
	(0.1235)
Task 1 earnings	0.8555***
	(0.1341)
Task 2 earning	0.9340***
	(0.0870)
First stage F-stats	222.08
N	166
Adj. R-squared	0.86

(b) OLS and IV second stage

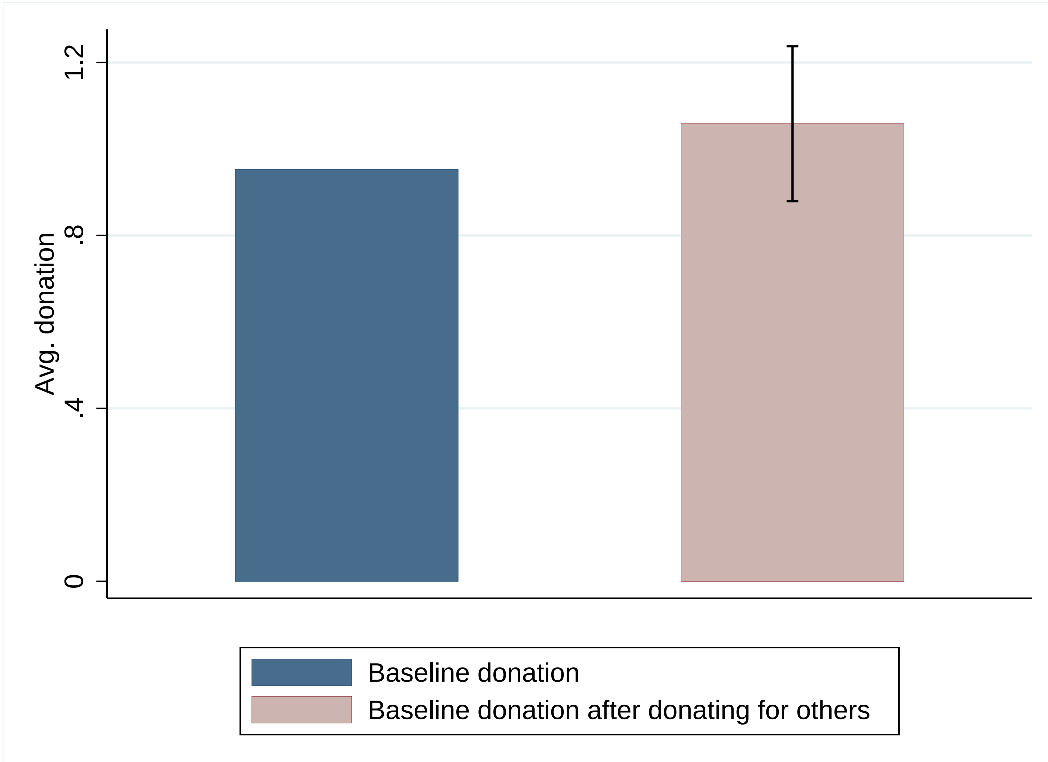
	(1) Scenario 1 donation OLS	(2) Scenario 1 donation IV	(3) Scenario 2 donation OLS	(4) Scenario 2 donation IV
Earnings from Tasks	0.0264	0.0411	-0.0113	-0.0300
	(0.0235)	(0.0618)	(0.0203)	(0.0568)
N	166	166	166	166
Adj. R-squared	0.00	-0.01	-0.00	0.03

Notes: The table shows 2SLS results of the impact of earnings on donations.The instrument for earnings from tasks is the earning from task 1 (2) if 'words' ('math') is randomized to task 3.*** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Robust standard errors in parentheses.

5.1.2 Donations for others

In one scenario, each subject chooses what portion of a bonus for another subject will be donated. On average, subjects choose a donation of £1.01 (standard error 0.053), or almost exactly half of the bonus. The modal donation is £1, similar to the 50-50 norm frequently observed in games between subjects (Andreoni and Bernheim, 2009). Figure 2 shows that subjects themselves then give slightly more than they do at baseline, perhaps correctly inferring an expected increase in their own income, but the increase is not statistically significant (p-value=0.244).

Figure 2: Response of own donation to donating for another subject



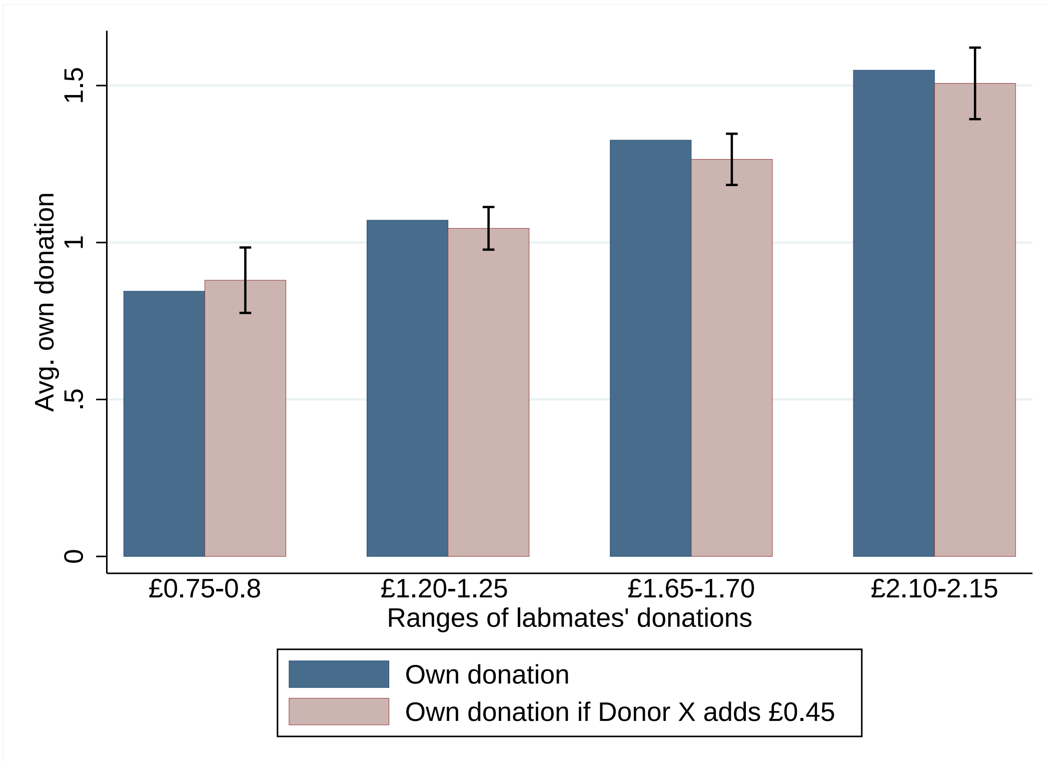
Notes: Bars represent average donations in each scenario. The line segment on the second bar gives 95% Confidence Intervals (CI) indicating equality of two bars. See footnote of Figure 1 for more details.

5.1.3 Donations by others

Figure 3 shows that donations are strongly increasing in those of labmates but not in those of an anonymous donor. The first bar in each pair indicates that the higher the range of donations by labmates, the more a subject is willing to donate. We estimate this slope, controlling for subject fixed effects, and find that on average a subject gives an additional £0.53 for each additional £1 given by the average labmate, an effect size comparable to that of receiving a £1 bonus. We find that this within-subject difference is statistically significant at the 1 percent level ($p < 0.001$). This positive slope is reminiscent of the conditional cooperation that is often found in public good games (for an overview, see Gächter (2007)), but in our experiment the subjects are contributing to an outside charity rather than to each other, making social norms a more likely explanation for this pattern than considerations of fairness, justice, or reciprocity.

In contrast, an additional donation by an anonymous donor does not increase giving, as can be seen from the fact that the second bar in each pair is similar in size to the first. Table C.1 in Appendix 7 presents our slope estimates and confirms that the anonymous donor has a negative and statistically insignificant effect. This finding is in contrast to much of the literature, which finds positive effects from the announcement of an anonymous donation. A unique feature of this study is that the anonymous donor's gift occurs in conjunction with another reference point for giving, as will be discussed further in Section 6.

Figure 3: Donation response to donations of labmates and anonymous donor

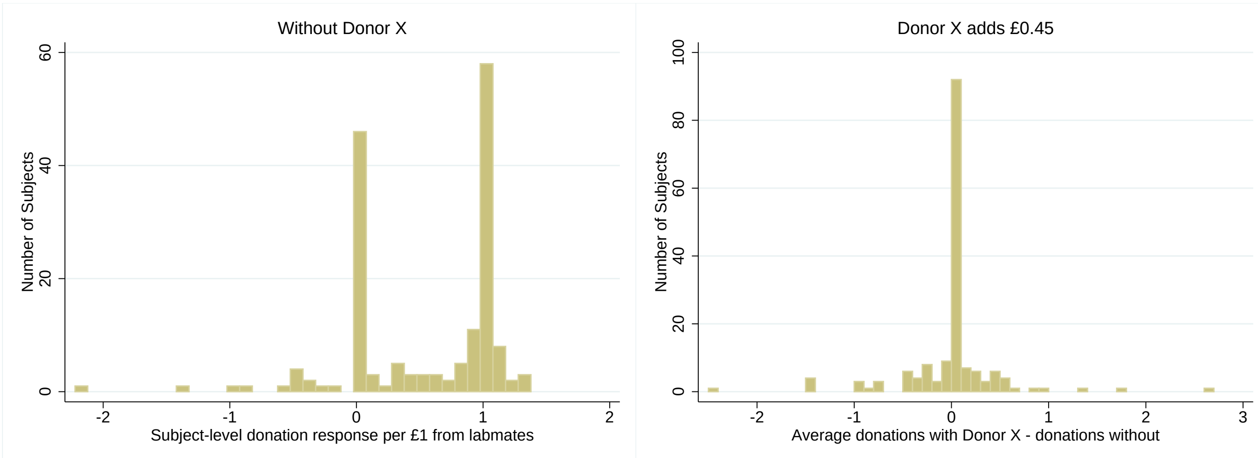


Notes: Bars represent average donations in each scenario given ranges of labmates' donations. The line segments on the second bar of each bar pair are 95% Confidence Intervals (CIs) indicating equality of these two bars. These CIs are estimated from regressions of donations on a dummy that equals 1 if it is in the scenario when Donor X also contributes and 0 otherwise. See footnote of Figure 1 for more details about the regression.

The within-subject design also allows us to examine heterogeneity in the donation patterns. In particular, we can estimate the regressions separately for each individual to obtain an individual-specific slope in labmates' donations and an individual-specific anonymous donor effect. The distributions of these coefficients are plotted, respectively, in the two panels of Figure 4. Some subjects are quite responsive to labmates, while others not. One quarter of all subjects have a coefficient of exactly zero, meaning they donate the same amount regardless of others' donations. A few subjects have negative coefficients, as would be predicted if the sessions' donations produced a decreasing-returns public good and one subject's donations crowd out another's. The vast majority of those who responded to others, though, had positive coefficients. In subsequent analysis we will distinguish between "labmate responders," or those whose coefficient is nonzero, and "labmate nonresponders."

The second panel of Figure 4 shows that most subjects do not adjust giving in either direction in response to the increment from the anonymous donor.

Figure 4: Heterogeneous responses to others' donations

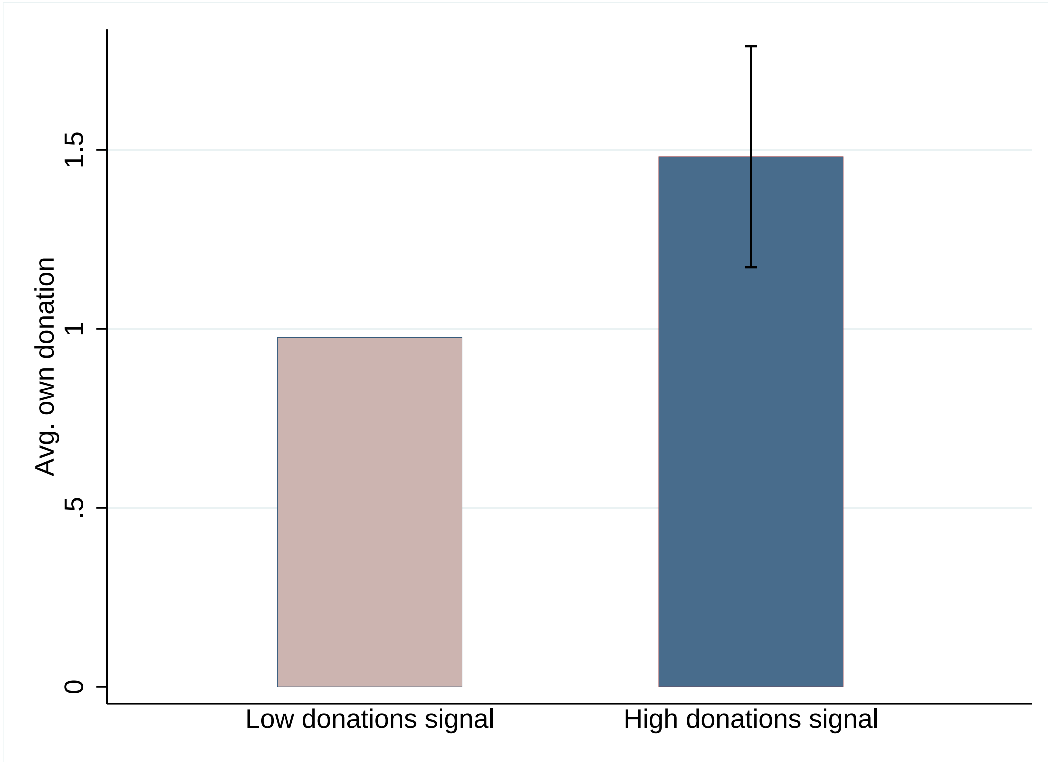


Notes: Distributions of coefficients from subject-specific regressions of conditional donations on minimum value of range of donations by labmates (Panel A) or an anonymous donor (Panel B).

5.1.4 Information about past donations

Figure 5 shows that subjects are quite responsive to the amount of past donations. On average, subjects donate about £1 after receiving the low signal (£1.225) vs. £1.5 after the high signal (£2.135). The effect size per dollar of past donations is estimated in Table 2, where we present regressions both without and with a control for the subject's original donation. In both cases we find that the subject donates about £0.50 more per £1 of additional donations by past subjects. The effect is significant at the 1 percent level.

Figure 5: Donation response to signal about past donations



Notes: Bars represent average own donations when different signals are revealed. The line segment on the second bar is 95% Confidence Interval (CI) indicating equality of two bars. See footnote of Figure 1 for more details.

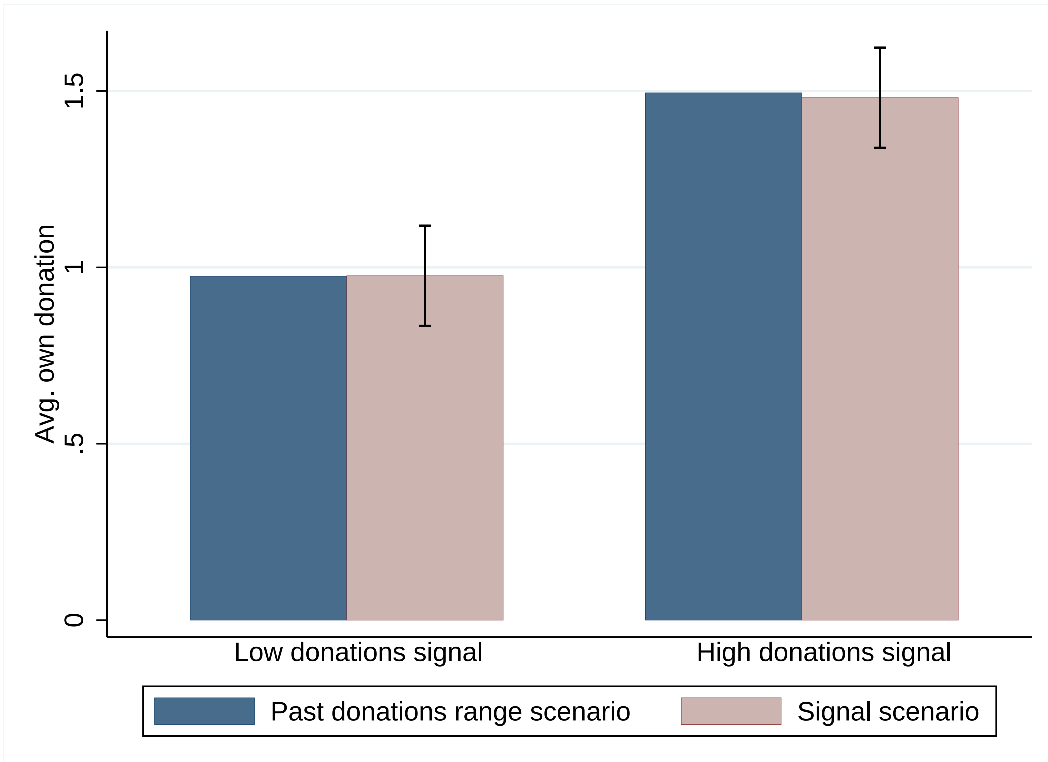
Table 2: Donation response to amount of past donations

	(1) Donation after signal	(2) Labmate Responders
Amount signaled	0.5547***	0.5191***
	(0.1718)	(0.1456)
Original donation		0.3565***
		(0.0965)
N	166	166
Adj. R-squared	0.06	0.31

Notes: *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Robust standard errors in parentheses.

We also find that subjects accurately predict what they will donate for a given level of past donations. Again, we chose the ranges for the conditioning scenarios so that the signaled amount would fall within one of these ranges. In Figure 6 we compare what subjects gave for each level of the signal with what they had said they would give if past donations were in the range into which their random signal would subsequently fall. The close proximity of the two numbers suggests that subjects understood these scenarios and had stable preferences within the realm of donations related to past donations.

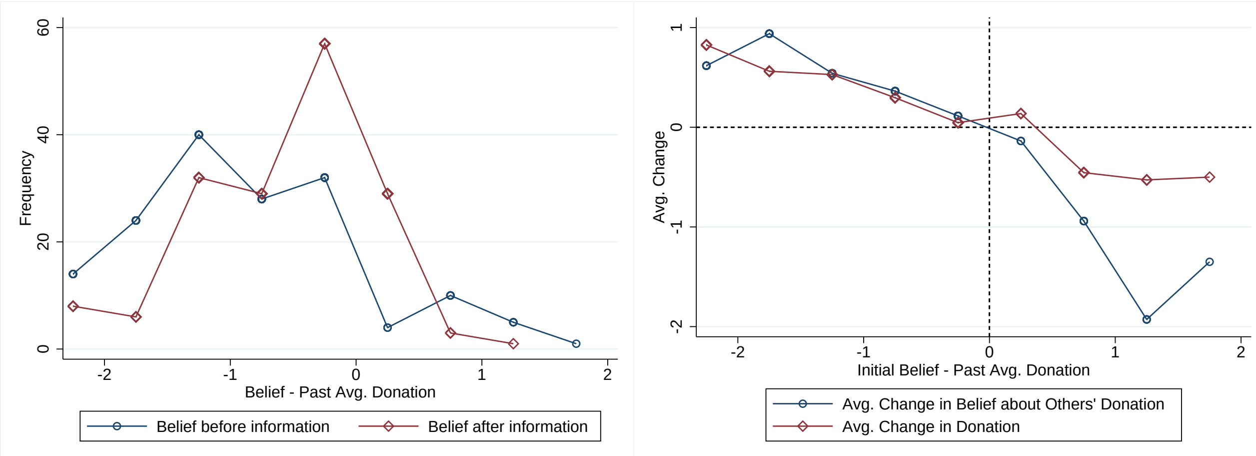
Figure 6: Equivalence of post-signal donations with relevant pre-signal conditional choices



Notes: Donations compared between scenarios. In past donations range scenario, subjects chose a donation for each of several ranges in which average past donations might lie. In signal scenario the actual average was given. For each level of the signal, the figure compares what donors gave after the signal with what they gave in the past donations range scenario for the range that matched the signal that they later received. The line segment on the second bar gives 95% Confidence Interval (CI) indicating equality of two bars. See footnote of Figure 1 for more details.

The response in donations is closely related to changes in beliefs about labmates' giving. Having elicited beliefs about labmates' donations following the first-opportunity donation and following the signal we are able to plot responses of both beliefs and donations as a function of initial beliefs. Figure 7 shows heterogeneity by initial beliefs, which we plot relative to the amount of the past donation that was later signaled to the subject. The left panel shows the distribution of beliefs before and after the signal, and this distribution narrowed towards the signaled amount. The right panel of the figure shows how both beliefs and donations changed as a function of initial beliefs. Subjects whose initial belief was below (above) the signaled amount adjusted beliefs upward (downward) after receiving the signal. Donations shifted in the same direction as beliefs, though by a smaller amount when beliefs shifted downwards.

Figure 7: Updating of beliefs and corresponding updating of donations



Notes: Panel A shows that the distribution of expected donations by labmates tightening around the value of past average donations provided to the subject. Panel B shows that subjects with initial expectations less than (greater than) the provided value adjust this belief upwards (downwards), and donations shift in the same direction as that beliefs do.

5.2 Irrelevant factors

5.2.1 Subject nationality

Table 3 demonstrates that the results we have obtained are not only relevant for British students. In each row we estimate the regression for one of our basic findings. In the first column we show results for the full sample. The second column restricts the sample to students who are from either the UK (N=65) or the EU (N=13) and the third column restricts the sample to students from other nations (N=88), most of whom are from East Asia. The forth column shows the estimated difference between the column (2) and column (3). In four of the five regressions this difference is not statistically significant, indicating that these giving patterns are statistically indistinguishable across subject nationalities. The one case of a significant difference is that while subjects from Europe respond positively to the donations of labmates, this response is even larger among the non-Europeans. Thus, the patterns we have observed all hold among subjects of either European or non-European origin.

Table 3: Similarity of main results across subject nationalities

	(1) All	(2) UK&EU	(3) Other	(4) Differences
Effect of bonus	.3794***	.3534***	.4024***	.049
	(.0378)	(.0576)	(.05)	(.076)
N	[830]	[390]	[440]	[830]
Effect of earnings	.0411	.0585	.0439	-.0146
	(.0618)	(.1363)	(.0676)	(.1521)
N	[166]	[78]	[88]	[166]
Effect of anonymous donor	-.0235	-.0281	-.0195	.0086
	(.0378)	(.0692)	(.0371)	(.0783)
N	[332]	[156]	[176]	[332]
Slope of labmate response	.5261***	.3091***	.7183***	.4092***
	(.0459)	(.0711)	(.0516)	(.0876)
N	[664]	[312]	[352]	[664]
Effect of high signal	.5191***	.4086**	.6873***	.2787
	(.1456)	(.2037)	(.181)	(.2724)
N	[166]	[78]	[88]	[166]

Notes: *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level.

5.2.2 Experimenter demand effects

Our within-subjects design carries some potential concerns for interpretation. First, once subjects know that their donation choices are being scrutinized they may behave differently than they would otherwise. Second, the order in which scenarios are presented could influence subjects' choices. We present evidence to minimize both concerns.

After subjects make their first donation choice they learn that there will be several more donation scenarios. Knowing that their donations are being studied could turn subjects off and decrease donations or could increase them if they wish to appear prosocial. For this reason we included another scenario that came after the instructions for the donation scenarios but before any further information was provided. Subjects who change their donation amount upon learning that the donations are part of the experiment are in the minority, and the number who increase their donations (22 percent) is nearly the same as the number who decrease them (26 percent). To formally check for experimenter demand effects we estimate our main results by these categories of "experiment responders." Results appear in Table 4. For four of the five estimated effects, results are either insignificant for all three groups or are significantly positive for all three groups. For the effect of signaling high past donations we only achieve statistical significance among non-responders, but the coefficients are positive for all three groups and are nonmonotonic in the direction of change, suggesting that this effect is simply less precisely estimated, particularly among the less-populated groups. None of these results are consistent with experimenter demand effects.

Table 4: Similarity of main results across subject responses to learning of experiment

	(1) All	(2) No change	(3) Increased	(4) Decreased
Effect of bonus	.3668***	.3606***	.3576***	.3872***
	(.0325)	(.043)	(.0571)	(.0785)
N	[664]	[348]	[144]	[172]
Effect of earnings	.0411	-.0453	-.008	.2203
	(.0618)	(.0926)	(.0274)	(.1279)
N	[166]	[87]	[36]	[43]
Effect of anonymous donor	-.0235	.0123	-.0026	-.1135
	(.0378)	(.0492)	(.0606)	(.0947)
N	[332]	[174]	[72]	[86]
Slope of labmate response	.5261***	.4428***	.727***	.5263***
	(.0459)	(.0617)	(.0867)	(.0992)
N	[664]	[348]	[144]	[172]
Effect of high signal	.5547***	.7054**	.298	.4143
	(.1718)	(.2761)	(.2526)	(.2946)
N	[166]	[87]	[36]	[43]

Notes: *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Categories in columns indicate how a subject's donation changed from Scenario 1 to Scenario 2, when the subject learned that multiple donation scenarios would be assessed.

5.2.3 Order effects

To check whether the order of the tasks affected behavior we randomly assigned two aspects of scenario ordering. One aspect was the order of the scenarios in which a subject received £2 and others received £0 or vice versa. Table 5 shows that this had no significant effect on the donation in either scenario. The second aspect was that subjects were also assigned to one of two overall scenario orderings.[8] These orderings differ in whether the scenario in which subjects donate for others appear last before the information on past donations and last overall, or instead appear

[8]If we number the tasks sequentially in the ordering that appears first in 7 then the other ordering is {1,2,3,9,4,5,6,7,8,10,11,12,14,13}.

before the anonymous-donor scenario and the post-information bonus scenario. As Table 5 shows, the order of the scenarios had no significant effects on donations in any scenario.

Table 5: Similarity of donations across randomly assigned order of scenarios

	Scenario...									
	5	6	7	8	9 (self)	9 (other)	10a	10b	10c	10d
Scenario Order	-0.3166	-0.2425	-0.3099	-0.1201	-0.2102	-0.0422	0.0023	-0.0954	-0.2214	-0.2849
	(0.2528)	(0.2661)	(0.2556)	(0.2762)	(0.1773)	(0.1065)	(0.1396)	(0.1430)	(0.1536)	(0.1735)
N	166	166	166	166	166	166	166	166	166	166
Adj. R-squared	0.00	-0.00	0.00	-0.00	0.00	-0.01	-0.01	-0.00	0.01	0.01

	Scenario...									
	10e	11	12a	12b	12c	12d	12e	13	14 (self)	14 (other)
Scenario Order	-0.1640	-0.1957	0.0669	0.0145	-0.1368	-0.1822	-0.0744	-0.2747	-0.1688	-0.0643
	(0.2252)	(0.1575)	(0.1272)	(0.1308)	(0.1442)	(0.1675)	(0.2115)	(0.2608)	(0.2006)	(0.1073)
N	166	166	166	166	166	166	166	166	166	166
Adj. R-squared	-0.00	0.00	-0.00	-0.01	-0.00	0.00	-0.01	0.00	-0.00	-0.00

Notes: *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Robust standard errors in parentheses.

Finally, we perform simple placebo tests of our information treatment. Because this information was not provided until several scenarios had been completed it could not have affected donations in the initial scenarios. Table 6 confirms that there were no significant differences in early-scenario donations across subjects who would later receive different information about past donations.

Table 6: Similarity of pre-signal donations across randomly assigned signals

	Donation in Scenario ...							
	1	2	3	4	5	6	7	8
Signal	0.1780	0.0998	-0.0016	0.0963	0.2849	0.4915*	0.3184	0.5655*
	(0.2762)	(0.2537)	(0.1540)	(0.1662)	(0.2824)	(0.2968)	(0.2856)	(0.3047)
N	166	166	166	166	166	166	166	166
Adj. R-squared	-0.00	-0.01	-0.01	-0.00	0.00	0.01	0.00	0.02

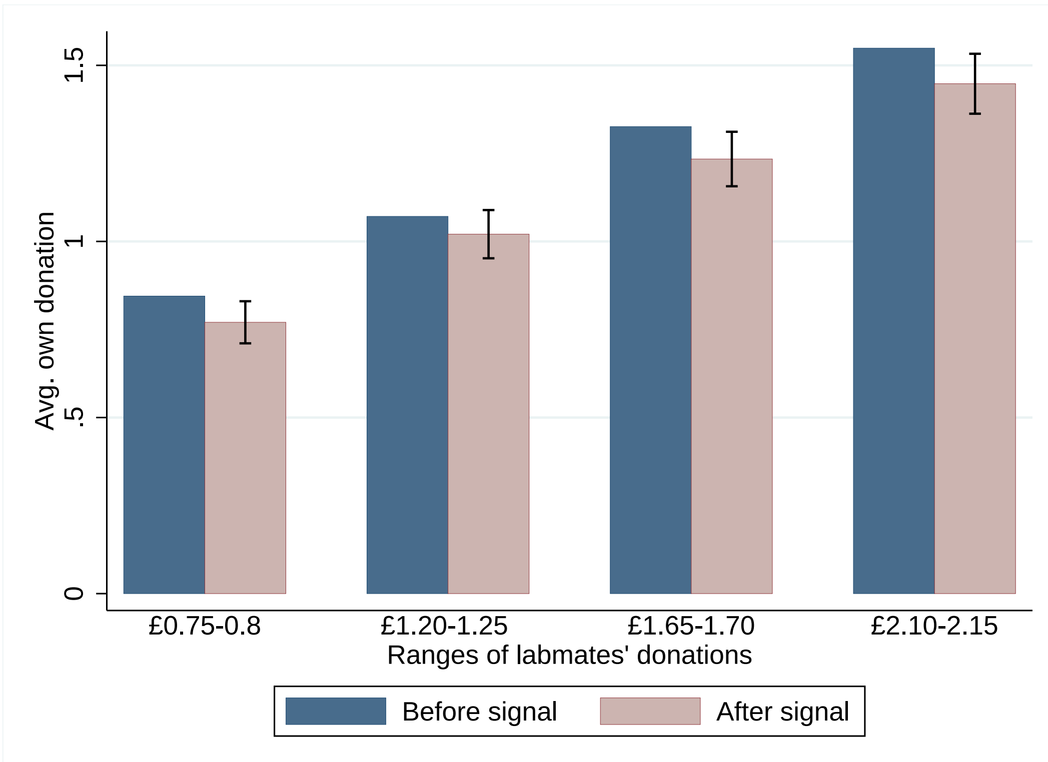
Notes: *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Robust standard errors in parentheses. The dependent variables are donations in each scenario and the independent variable is the amount signaled.

5.3 Interactions between factors

5.3.1 Social factors

Past donations did not dramatically change how subjects responded to the donations of their labmates. This can be seen in Figure 8, where the first in each pair of bars shows the donation before the signal and the second the donation after the signal. If subjects had used labmates' donations as a proxy for what most people would do then learning what most people had done in the past would render subjects unresponsive to labmates, i.e. decreasing the slope across bars to zero. Instead, the slope only decreases (from 0.526) by 0.0271 (standard error 0.0298). Thus, labmates' donations somewhat override past donations.

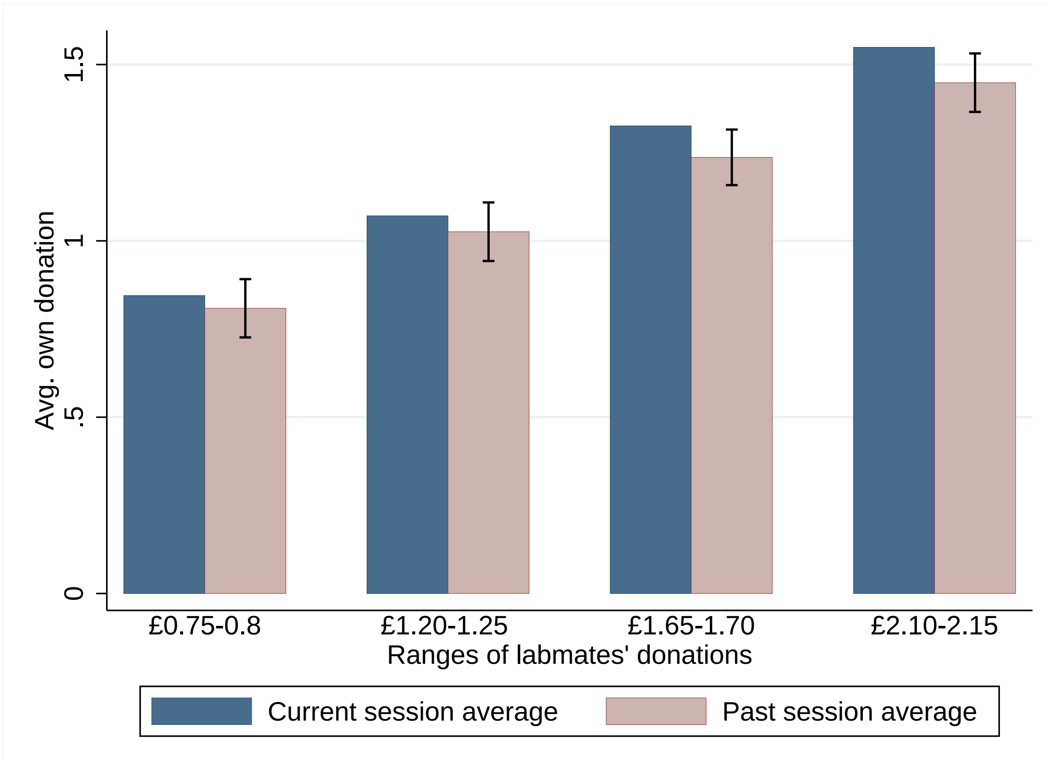
Figure 8: Donation response to labmates' donation before and after signal



Notes: Bars represent average donations before and after signal given ranges of labmates' donations. The line segments on the second bar of each bar pair are 95% Confidence Intervals (CIs) indicating equality of these two bars. These CIs are estimated from regressions of donations on a dummy that equals 1 if it is after signal and 0 otherwise. See footnote of Figure 1 for more details about the regression.

Figure 9 compares giving in the scenario where subjects could condition on labmates' donations with the scenario in which they could condition on past donations. The slope is upwards in both cases but is steeper for labmates' donations than for past donations. In other words, subjects' preferred gift amount is influenced more by current labmates than by a larger number of past subjects. These responses are quantified in Table 7. On average, the amount that subjects believe their labmates have donated increases by £0.46 when reported past donations increase by roughly £1. As reported above, we found that the average subject gives an additional £0.53 for each additional £1 given by the average labmate. The effect of the high-past-donations signal on beliefs about labmates should therefore increase donations by roughly £0.25 (or 0.46*0.53). As Table 7 shows, the actual effect was to increase donations by £0.52. Thus, only about half of the effect of the high-past-donations signal is explained by its effect on beliefs about labmates' donations, suggesting that past donations are of independent importance. This pattern of results could arise if subjects use both past and concurrent subjects' donation amounts as signals of the amount that one normatively "should" donate.

Figure 9: Donation response to past and current average donations



Notes: Bars represent average donations in each scenario given ranges of labmates' donations. The line segments on the second bar of each bar pair are 95% Confidence Intervals (CIs) indicating equality of these two bars. These CIs are estimated from regressions of donations on a dummy that equals 1 if it is from the scenario when past sessions' average donation are given and 0 otherwise. See footnote of Figure 1 for more details about the regression.

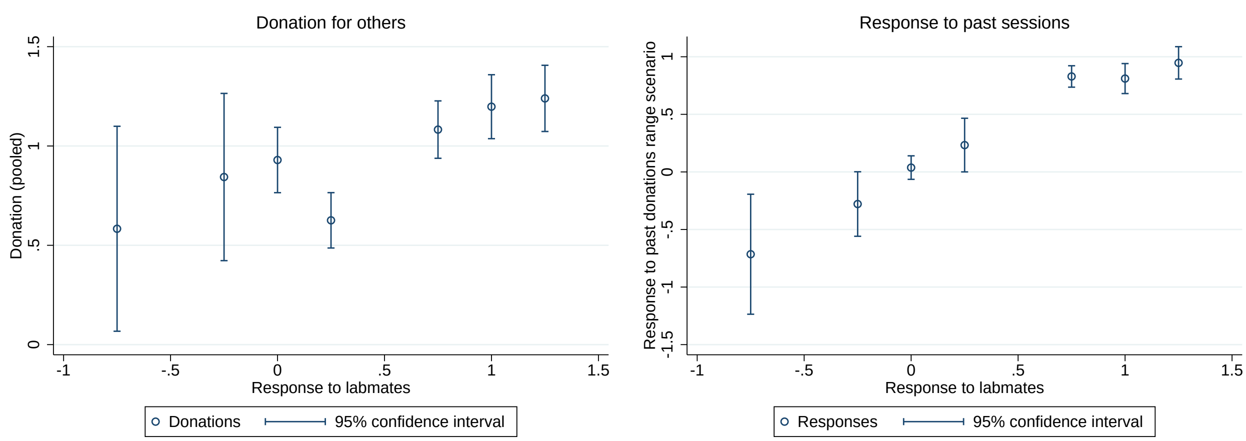
Table 7: Belief and donation responses to amount of past donations

	(1)	(2)	(3)	(4)	(5)	(6)
	All subjects		Labmate Responders		Non-Responders	
	Belief	Donation	Belief	Donation	Belief	Donation
Amount of Past Donations signaled	0.4551***	0.5191***	0.4343***	0.5887***	0.5141**	0.3226
	(0.1167)	(0.1456)	(0.1409)	(0.1444)	(0.1940)	(0.2450)
N	166	166	122	122	44	44
Adj. R-squared	0.08	0.31	0.07	0.22	0.13	0.75
Controls for Scenario 2 donations		X		X		X

Notes: *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Robust standard errors in parentheses.

Table 7 and Figure 10 show that the responses to the various social motivations are related. As shown in Table 7, when we split subjects by whether or not they respond to their labmates' donations we find that labmate responders' donations are nearly twice as responsive to the amount donated in the past. This difference is not driven by differences in beliefs, which are affected as much for non-responders as for responders. We also see the correlations between the responses across social scenarios in Figure 10. In both panels of the figure we plot estimates as a function of the labmate response, i.e. the slope of own donations in those of labmates. The two panels show, respectively, that both the amount donated for another subject and the response to past donations (as was shown in Figure 9) are increasing with the labmate response. These correlations are far from perfect but do point to variation across individuals in the degree of socially-oriented motivation.

Figure 10: Correlation of donation responses to labmates with donation for others and response to past donations

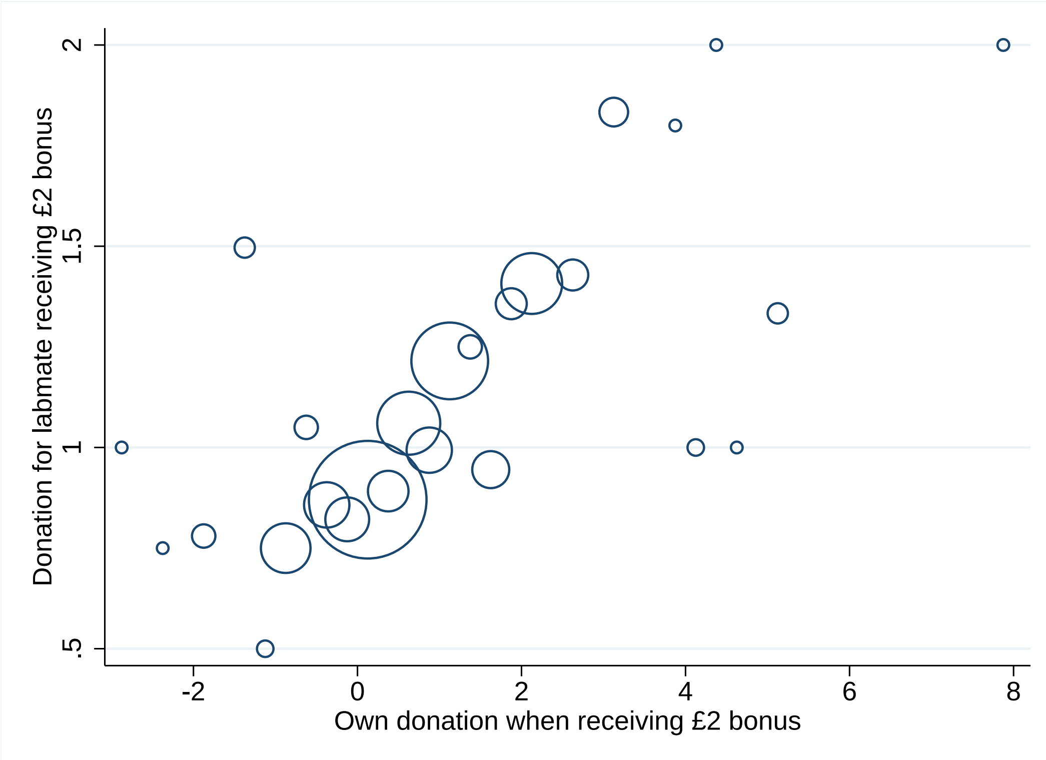


Notes: Regressions of one subject-level measure on another. Estimates for subjects whose response to labmate donations (x axis in both panels) is exactly 0, exactly 1-for-1, or in each range of width 0.5. Dependent variable in first panel is donation chosen for another labmate. Dependent variable in second panel is response to past sessions in scenario allowing conditioning on ranges of past donations.

5.3.2 Bonus income & other factors

Figure 11 plots a subjects' donation from another subject's bonus against the donation from her own bonus. As was just seen within the social scenarios, responses within the financial scenarios are highly correlated with each other.

Figure 11: Correlation of donation with response to bonus income with donation for labmate



Notes: This figure shows the correlation between own donations and donations for labmates when receiving £2 bonus. Similar values of own donation are pooled in the same £0.25 bin. Larger marker means more observations in the bin.

However, when we compare a subject's response in a social scenario to her response in a financial scenario we do not see much correlation. For example, Table 8 shows the response to bonus income when we separate the sample by whether or not the subject was responsive to labmates' donations. We see that labmate responders and non-responders give amounts that are similar, and statistically indistinguishable, when they receive a bonus. Responses to bonuses are even less correlated with other responses to social cues.

Table 8: Donation response to own and others' bonus income

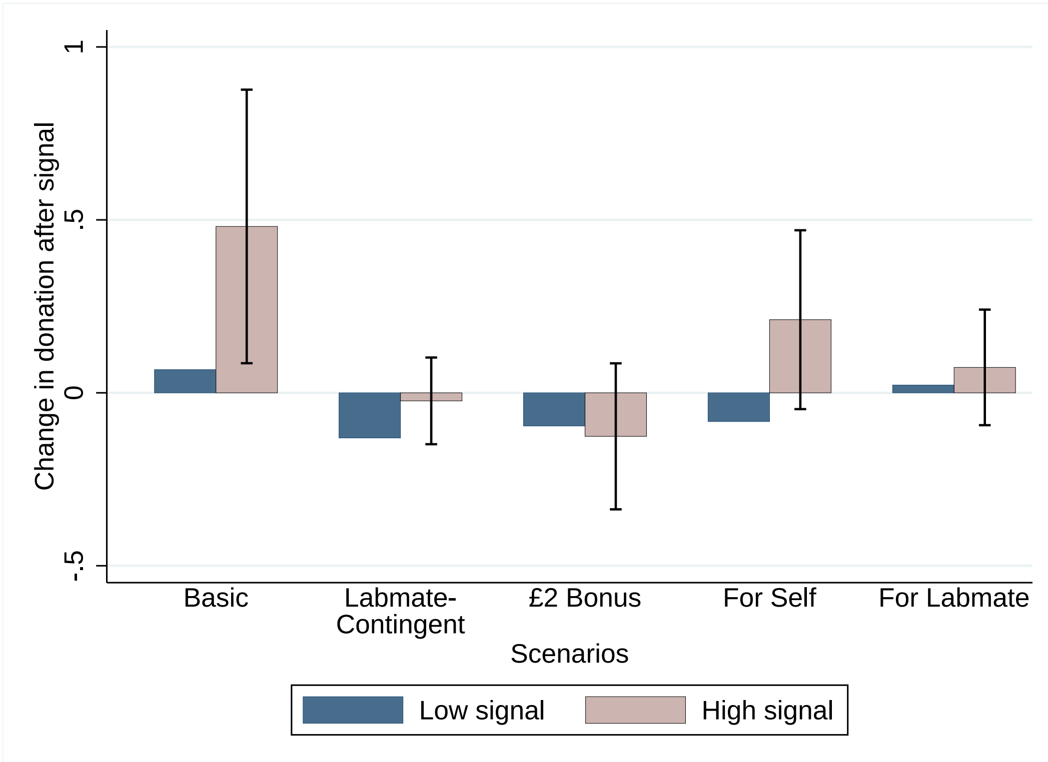
	(1) All	(2) Responders	(3) Non-responders
Own bonus	0.3794***	0.4021***	0.3162***
	(0.0378)	(0.0436)	(0.0758)
Bonus for others	0.0353	0.0243	0.0655
	(0.0380)	(0.0467)	(0.0620)
N	830	610	220
Adj. R-squared	0.23	0.24	0.19

Notes: *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Individual FE is included in all regresssions and standard errors are clustered by individual.

Moreover, the initial effect of the signal about past donations does not carry over into financial scenarios. In Figure 12 we plot the change in donations for each of the scenarios that we posed both before and after the past-donations signal. For each scenario we plot this within-subject change for the subjects that received the low signal vs. the change for those that received the high signal. The basic scenario shows, as seen above, that the signal affected donations that were made with no further information. This effect became marginal in the scenario in which subjects could condition on the amount donated by labmates, suggesting related motives. The amount of past donations had no effect, however, on the subject's donation when a bonus was offered. The distinction is perhaps most clear in the final scenario, where past donations also had no effect on the amount donated from another subject's bonus, but it continued to significantly influence a subject's own donation.[9]

[9]Figures C.2 and C.3 in Appendix 7 show the donations before and after information provision, which are differenced to obtain the results shown here.

Figure 12: Responses to past donations by scenario type



Notes: Bars represent average change in donations before and after signal in each scenario given different signal. The line segments on the second bar of each bar pair are 95% Confidence Intervals (CIs) indicating equality of these two bars. These CIs are estimated from regressions of change in donations on a dummy that equals 1 if high donation amount is signaled and 0 otherwise. See footnote of Figure 1 for more details about the regressions.

Overall, we do not find that many subject characteristics have strong power to predict their donation responses. Table 9 displays the predictors selected by the Lasso method and their effects on the within-subject responses to labmates' donations, within-subject responses to bonus income, and across-subject donations chosen for others. The full list of available subject characteristics and their definitions appear in Table C.2 of Appendix 7. As noted before, subjects who are not from the UK or EU are more responsive to their labmates' donations, and we see here that on average they donate an extra £0.13 of their labmate's £2 bonus. Most other determinants of behavior have small effects and are only predictive of one type of outcome. The interesting exception is the Cognitive-Reflective Test of Frederick (2005). One additional point on this test, i.e. one additional correct answer to one of a handful of questions with seemingly-intuitive-but-incorrect answers, is associated with giving £0.0586 less per £1 of labmates' donations and £0.0325 more per £1 of bonus income. This result suggests that individuals who are more reflective may be more motivated by extra income and less motivated by the actions of others, though the differences are again small. We read these results as indicating that the strengths of an individual's motivations are different dimensions are not strongly related, i.e. that giving types should be thought of as multi-dimensional.

Table 9: Donor types and individual characteristics

	(1)	(2)	(3)	(4)
	Labmates responses		Effect of bonus	Donate for others
	Dummy	Slope		
Has a Polit. Party	-0.0229			
Times Partic. in Past Exp.	-0.0240	-0.0245		
Cognitive-Reflective Test Score	-0.0766	-0.0586	0.0325	
Not from UK or EU	0.0240	0.3741		0.1336
Feels Most People Fair		-0.0371		
Would Avoid Paying for Transit		0.0275		
Has Donated to Charity		0.1927		
Involvement in Organizations		-0.0123		
No. of Known Labmates			0.0264	
Male			-0.0677	
Age				-0.0098
Married				-0.1437
Mother's Educational Attainment				0.0939
Log of Past Donations				0.0168

Notes: Appendix Table C.2 provides description of these variables.

5.4 Implications for the impure altruism model

In the baseline model of Section 2, altruism implies crowd-out of donations when others' donations increase. In our experiment, several results indicate that subjects are not altruistic, i.e. are not motivated by total contributions to the charity. Most directly, donations by an anonymous donor do not crowd out subjects' donations. This is true not only on average but also generally within-subject, with only a small percentage of subjects reducing their donations when the anonymous donor makes a supplementary contribution. This result contrasts with several experiments that have found that announcing a gift by an anonymous donor increased subjects' donations. What is unique about our scenario, to our knowledge, is that the anonymous donors' donation is presented in addition to the level of donations by labmates. While this anonymous donation should still have an effect if it signals quality, it will be redundant if it merely signals what individuals like the subject should give. Our subjects' lack of response to the anonymous donor suggests the latter.

Two other results provide evidence against altruistic motivation in our setting. For one, although subjects' donations are generally increasing in those of their peers, they do not donate more when their peers receive bonuses. Most subjects donate more when they themselves receive a bonus, which means that either subjects incorrectly infer that others will not do the same or that subjects view other subjects' donations out of bonus income as irrelevant. Another telling result is that when subjects choose another subject's donation from bonus income, they generally choose a positive amount, yet they also do not significantly change their own donations in this scenario. The fact that the responses to both the anonymous donor and to others' donations from bonus income are zero for most subjects, rather than positive or negative, also goes against altruism-based mechanisms that can explain positive effects of others' donations on one's own, including increasing returns to scale in the provision of the public good or inter-donor signaling of the quality of that good. Given these results, we focus hereafter on the nature of the warm glow function.

6 Social Norm Model

6.1 Model

Our results suggest a major refinement of the basic model. Altruistic motivation appears to be minimal for the type of donation we study, and hence we focus on a model with pure warm glow. Nonetheless, subjects' donations respond to bonus income and the amount of donations by others, results that can be obtained from a model of an uncertain social norm.

If we remove altruism from the model, assume linearity in consumption over the relevant range of payoffs, and allow for uncertainty in the warm glow that will be obtained from a gift, then expected utility has the form

$$U(g) = I - g - Ew(g, I_u, \gamma_p),$$

again for income I , gift g, unearned income I_u, and average giving by peers γ_p. The first-order condition is $1 = Ew'(g^*, I_u, \gamma_p)$.

We now propose structure for the warm glow function. Consider a social norm $n(I_u, \gamma_p)$ quantifying what a person "should" give.[10] Given the experiment's results, we expect that many subjects perceive this function as increasing in the argument of unearned income I_u. Subjects may be uncertain, however, about what the norm is. A desire to adhere to the norm can be represented by a simple quadratic loss function. If $Ew(g, I_u, \gamma_p) = -E\left[(g - n(I_u, \gamma_p))^2\right]$, then the first-order condition (along with a non-negative donation constraint[11]) implies that

[10] More generally, giving by others may establish a reference point, or rule of thumb, with no normative content. However, it is not clear why unearned income would affect this reference point, and in Drouvelis and Marx (mimeo) we elicit social norms for donations and show that these vary with the level of unearned income.

[11] Subjects in the experiment are also potentially constrained by the amount of their income from the experiment. In practice, only two subjects donated all of their income from the experiment.

$$g^* = \max\left\{0, E\left[n\left(I_u, \gamma_p\right)\right] - \frac{1}{2}\right\}.$$

The optimal charitable gift reflects a desire to match the social norm but a recognition that donating is costly in terms of one's own private resources. This very simple model can fit the wide variety of results from our experiment. The expectation $E\left[n\left(I_u, \gamma_p\right)\right]$ will be affected by the donations of peers so long as the individual is not certain of the norm. By construction, it can also capture that gifts increase with unearned income. Giving need not respond to earned income, of which the individual may feel more deserving. The expected value of the norm also need not be affected by donations of non-peers, for whom a different norm may apply, when the donations of closer peers are known. This would explain why past studies have found significant effects of anonymous donors while we find no effect; anonymous donations may offer a noisy signal of the relevant norm, while we control for the giving of closer peers, a more precise signal. In addition, such a model can explain why subjects respond to labmates' baseline donation amounts but not to the occurrence of labmates receiving bonuses and hence donating more. As a simple example, if $n\left(I_u, \gamma_p\right) = \alpha + \beta I_u$, then labmates' donations out of bonus income provide information about β rather than α and so are not relevant to the subject's own (no-bonus) donation. Similarly, no information is gained when subjects choose each other's donations from bonus income. We do not see donations respond to either of these scenarios.

A model in which subjects learn about a norm can also explain the pattern of responses in our "social" treatments. In our experiment we provide reference points from labmates in one scenario and reference points from past lab attendees in another scenario. Subjects respond to both of these scenarios, suggesting a role for both past information about a larger number of subjects and current information from a smaller number of subjects. By comparing behavior in the donations-from-labmates scenarios that occur before versus after the revelation of the amount of past donations, we learn about the relative importance of these two reference points. If the past donations dominate, then we should see the slope of donations with respect to labmates' donations drop to zero, whereas if labmates' donations dominate then we should see no change in the slope. What we find is closer to the latter, with the slope falling by an amount that is small and not statistically significant (0.027, with standard error 0.0298). This suggests that individuals place a lot of importance on reference points provided by especially recent or proximate peers, which may offer an explanation for why experimental researchers have been successful in manipulating individuals' donations. Thus, our model can capture the effect of the announcement of past donations without including these directly in the utility function by allowing them to alter beliefs about the level of giving by peers.

6.2 Estimation and counterfactuals

Our preferred model describes giving as driven by a social norm that depends on the potential donor's financial circumstances. It appears that subjects feel an obligation to donate and that this obligation grows with unearned income and the giving of peers. The perceived importance of these factors may vary across subjects. We therefore estimate the utility function separately for each subject. We then use these estimated functions to consider counterfactual situations that vary the timing of information revelation and the amount and distribution of bonus income.

Parameterizing $En\left(I_u, \gamma_p\right)$, we estimate the giving of each individual subject i in scenario s as

$$g_{is}^{*} = \max\left\{0, \beta_0^i + \beta_1^i I_{u,is} + \beta_2^i E\gamma_{p,is} + \beta_3^i \left(E\gamma_p\right)_{is}^2 + \beta_4^i I_{u,is} E\gamma_{p,is} + u_{is}\right\}.$$

This flexible functional form allows for heterogeneity across subjects in beliefs about what others donate, the effect of these beliefs, the effect of unearned income, and any interaction between these. We use Tobit estimation to account for the fact that donations cannot be negative. The Tobit estimator assumes that the subject's error term across scenarios, u_{is}, is normally distributed.[12] We obtain similar results from Ordinary-Least-Squares estimation despite instances of censoring at zero among roughly half of subjects.[13]

Determination of the values of the explanatory variables is straightforward. We set $I_{u,is}$ equal to the amount of the bonus a subject receives, which is zero in non-bonus scenarios. We populate $E\gamma_p$ using the beliefs that we captured by incentivized elicitation at the beginning of donations and after we provided information about past donations. We assume these beliefs hold constant until new information is provided. In scenarios that allow subjects to condition on narrow ranges of donations by labmates, we set $E\gamma_p$ equal to the middle value of the range, e.g., $E\gamma_p = 0.775$ when labmate donations are known to lie between 0.75 and 0.80. We exclude the initial donation (made before the within-subjects design was explained), scenarios conditioning on ranges of past donations or on the anonymous donor, and scenarios in which subjects choose donations for other subjects.

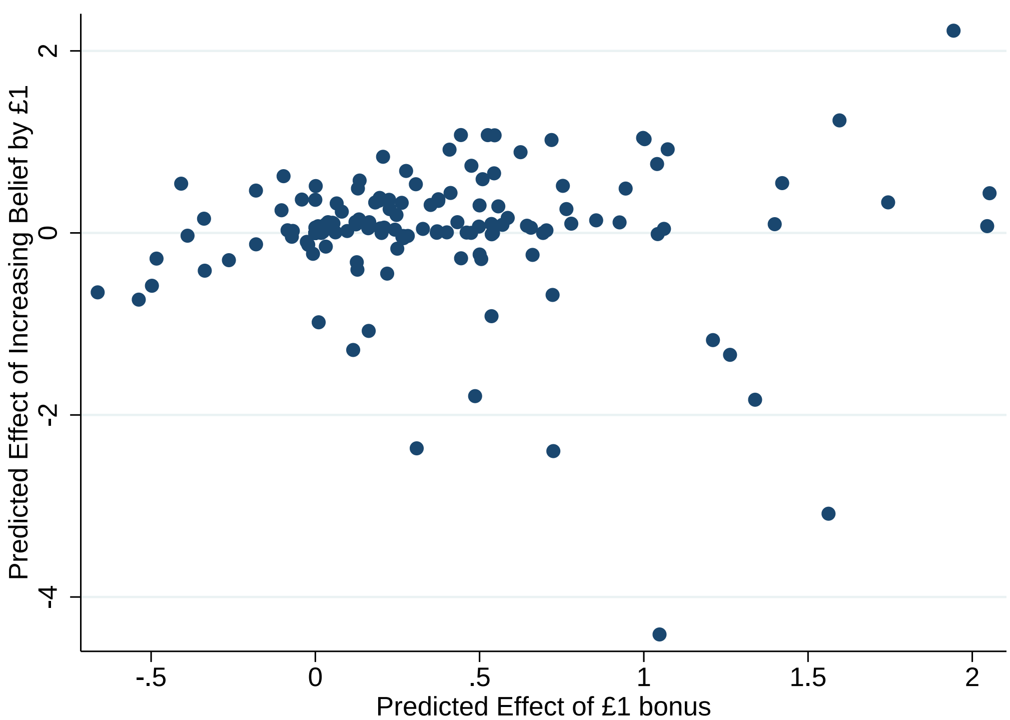
Estimated parameters vary considerably across subjects. As a comprehensive summary of the estimation results, we present estimated effects of increasing either beliefs or bonus income by £1.

[12] We drop 22 of the 166 subjects because the Tobit estimation did not converge. Ottoni-Wilhelm et al. (2017) are similarly unable to estimate their model for 7 out of 85 subjects. The share of dropped subjects is slightly greater here because we estimate more parameters per scenario.

[13] The donation is zero for 348 of the 2736 observations. 61 of the 144 subjects give a strictly positive amount in at least one scenario and give zero in at least one other.

Each point in Figure 13 displays the predicted effect of both potential changes relative to the predicted baseline donation. For most subjects, either change causes them to increase their gift by some amount between £0 and £1. Notably, there is little correlation between the two responses. This lack of correlation is consistent with the direct comparisons between donations under different scenarios in the experiment, such as in Table 8, which showed that a subjects response to one stimulus do not necessarily predict that subject's response to another stimulus. Moreover, for one third of subjects, we estimate a significant negative interaction between bonus effects and peer effects, meaning that beliefs about others' donations have less influence on a subject when that subject has received bonus income. This is consistent with evidence from other settings that subjects' social preferences change when a financial concern is introduced (Gneezy and Rustichini, 2000b;Gneezy and Rustichini, 2000a).

Figure 13: Heterogeneity of estimated choices



Notes: Predictions for each subject from estimated model of preferences. N=144.

Table 10 displays the results of estimating the model. We consider several situations, and for each we calculate subjects' average donations and what it would cost per subject for the charity to provide any bonuses employed in the situation. The first three situations demonstrate the model fit by comparing predictions to the actual donations in the baseline scenario and the initial scenarios with bonuses of £1 and £2, respectively. In each case, we see that the model prediction is close to the observed average donation. Moreover, while donations are increasing in the amount of bonus income, they increase by considerably less than the cost of providing the bonuses.

Table 10: Model fit and counterfactual situations

Situation	Truth	Predicted	Avg. Cost
1. Baseline	0.89	0.98	0
2. £1 bonus	1.38	1.35	1.00
3. £2 bonus	1.72	1.73	2.00
4. Correct all beliefs		0.89	0
5. Announce avg. of socially unresponsive		1.29	0
6. Announce avg. of socially unresponsive after giving £1 to each		1.48	0.38
7. Announce avg. of socially unresponsive after giving £1 to the bonus-responsive		1.53	0.22

Notes: "Truth" equal to average donations in Scenarios 2, 5, and 6, respectively. "Predicted" equal to average donations predicted by the model. "Avg. Cost" equal to value of any bonuses paid, divided by the total number of subjects. N=144.

Rows 4 through 7 of Table 10 contemplate counterfactual situations involving techniques that the charity might employ to increase donations. In row 4, we correct subjects' beliefs by setting them equal to the observed average baseline donation. Subjects may prefer such certainty; plugging the optimal donation into the expected utility function, one can show that for a given expected value of the norm, expected indirect utility is decreasing in the variance of beliefs about the norm. The charity has little incentive to provide this information, however, because doing so has a small negative effect on donations. In contrast, we find an increase of more than £0.30 in the situation considered in row 5. In this situation, the charity first solicits "socially unresponsive" subjects whose donations are not increasing in others' donations, then announces the average donation from this first round to the remaining subjects who are socially responsive. It turns out that baseline donations for unresponsive subjects are relatively large, and therefore announcing these increases the donations of subjects who respond to their peers. This sequential solicitation costs the charity nothing, and the result suggests that the nature of heterogeneity in the preferences of potential donors offers a new mechanism for the literature comparing sequential and simultaneous solicitations.

The final two rows of Table 10 consider a sequential solicitation in which the charity expends resources to increase donations made in the first round. In the situation in row 6, the charity gives £1 to each potential donor in the initial solicitation. This situation is similar to the annual fundraiser in which the March of Dimes organization includes dimes in solicitation letters to potential donors. The average cost of these bonuses is £0.38, the share of the subjects receiving the bonus. Relative to the basic sequential solicitation in row 5, providing these bonuses increases average revenue by less than £0.20, and therefore fails to cover costs. However, it is possible to further subdivide the socially unresponsive subjects into those who do or do not increase their donation when receiving a bonus. Row 7 shows that if the charity further targets bonuses to individuals who are socially unresponsive but positively bonus-responsive, then average cost falls to £0.22, and average donations rise by £0.24 over those in the basic sequential solicitation. Thus, with sufficient targeting, it is possible for the charity to increase its resources by giving money to potential donors.

While give-backs to donors can potentially benefit a charity, our results provide a cautionary tale. First, it is readily apparent that the cost exceeds the benefit if bonuses are not targeted. While this result may be overturned if receiving the bonus from the charity itself induces reciprocity, it is consistent with evidence from the field of donor give-backs not covering their cost. Second, targeting must be precise to obtain even a small positive return, incorporating both the strength of preference for matching one's own donation to that of others and the strength of donative response to the windfall. Further targeting, or refinements to the staging of solicitation and information sharing, could further increase the charity's return. However, implementing such procedures would require charities to obtain detailed information about individuals' preference types with regard to both bonus income and giving by others. Charities would need to run within-subject experiments to obtain such precise information if, as seen in Table 9 for this population, there are few observable characteristics that predict subject type, including past donations. Our results therefore suggest that charities may benefit more from costless strategies such as staggering solicitations than from sending resources to those from whom they are seeking support.

7 Conclusion

Our experiment provided multiple pieces of evidence on the form of peer and income effects in charitable donation preferences. A large majority of subjects increased their donations when others donated more or when they received bonus income. In contrast, subjects did not respond earned income, anonymous donations, or bonuses paid to others. A model of uncertain social norms can explain these donation patterns and others in the literature, and estimation of this model reveals informative heterogeneity in donor types along multiple dimensions.

Our findings on motivations for charitable donations have relevance for workplace charity campaigns, retail-transaction solicitations, and the design of experiments on prosocial behavior. Workplace charity campaigns, such as those run by the United Way, could potentially increase donations by holding their campaigns when companies make bonus payments to employees. Solicitations that follow purchases may wish to highlight any savings that a customer received on the purchase. More generally, charities could potentially benefit from identifying donors who are responsive to their peers' donations and approaching them after using common techniques to increase the donations of the other donors. However, such targeted strategies require information about donor type that does not strongly correlate with demographics.

While our experiment has provided a rich set of results related to donations after transactions, there are numerous questions raised for future research. Individuals appear to give according to social reference points, and future research could explore how individuals form beliefs about the relevant reference point and why they adhere to these apparent norms. It would also be of value to practitioners to identify individual characteristics that have greater power to predict how individuals' giving behavior responds to different stimuli. Alternatively, research might develop mechanisms through which donors would reveal their types or examine the properties of fundraising markets when some charities invest in learning donor types.

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Appendix A: Pilot Experiments

Prior to conducting our experiment, we ran two pilots experiments in which we varied the number and types of tasks that subjects had to perform in Part 1 as well as the structure of Part 2 of our main experiment as reported in the paper. In this Appendix, we refer to the details of the structure for both pilot experiments, called Pilot 1 and Pilot 2. Both experiments consist of two parts, Part 1 and Part 2. We discuss the structure of each Part in turn.

Part 1: Real-effort tasks: The nature of the tasks performed during Part 1 is the same as described in Section 3 of the paper. For the pilot experiments, we varied the number of tasks performed, the piece-rate payments for correct answers, and the time that subjects were given to perform the tasks. Specifically, in Pilot 1, subjects were asked to perform two tasks (in the following sequence): the hard word and the hard math task. The piece rate payment was 25 pence and 50 pence, respectively. Subjects were given a 5-minute time limit for each task. In Pilot 2, subjects were asked to perform six tasks (in the following sequence): the easy math, the hard math, the easy word, the hard word, the easy math and the easy word task. The piece rate payment for correct answers was 3 pence for the easy version of either the word or the math task and 21 pence for the hard version of either the word or the math task. Subjects were given a 3-minute time limit for each task.

Part 2: Donation choices: After subjects had completed Part 1, they were given the opportunity to donate some of their earnings to the local charity. Following their donation decisions, subjects were then asked to make donation choices with respect to a number of scenarios which assess the relative strength of various mechanisms that may be important in explaining donation patterns. The instructions informed subjects that one of the scenarios would be selected at random and implemented after all choices had been made. These scenarios focused on the following mechanisms:

- *Beliefs about the average of others' first-opportunity donations:* After subjects had decided about their own first-opportunity donations, they were asked to report what they think others (excluding themselves) in their session had given as their first-opportunity donation. Subjects' responses were incentivized in that estimates within £0.10 of the correct amount earned the subject an additional £1.

- *Labmates' actual donations:* In this scenario we allowed each subject to condition the amount they would donate on the average donations of the other subjects in her session. In particular, subjects were asked to indicate how much they wished to donate for possible ranges of labmates' first-opportunity donations. In Pilot 1 we asked subjects how much they wish to donate if the average of others' first opportunity donation was : i) less than £0.50 per person; ii) at least £0.50 but less than £1.00 per person; iii) at least £1.00 but less than £1.50 per person; iv) at least £1.50 but less than £2.00 per person; and v) at least £2.00 per person. In Pilot 2 we asked subjects the same question but we used more and smaller ranges of others' first opportunity donation. These were: i) at least £0 but less than £0.66 per person; ii) at least £0.66 but less than £0.67 per person; iii) at least £0.67 but less than £1.04 per person; iv) at least £1.04 but less than £1.05 per person; v) at least £1.05 but less than £1.42 per person; vi) at least £1.42 but less than £1.43 per person; vii) at least £1.43 but less than £1.80 per person; viii) at least £1.80 but less than £1.81 per person; ix) at least £1.80 per person. We further asked subjects to decide for the same ranges as above in a condition in which the first-opportunity donation was implemented for all but one randomly-selected subject, while for this subject the conditional choice was implemented.

- *Minimum amount donated by an anonymous donor:* In these scenarios, subjects were told that an anonymous donor ("Donor X") will donate as necessary to ensure that donations for a given session will be at least some amount plus the subject's own donation. More specifically, in Experiment 1, subjects had to indicate how much they would like to donate if the anonymous donor guarantees that the average donations of others in their session will be: i) at least £0.01 per person?; ii) £0.50 per person?; iii) £1.00 per person?; iv) £1.50 per person?; v) £2.00 per person? Responses to open-ended survey questions indicated that subjects did not understand these instructions and believed their own donation would affect the amount donated by Donor X. Pilot 2 was more like the final experiment in that subjects had to indicate, for each of the nine ranges of labmates' donations, how much they would like to donate if the anonymous donor adds £0.38 per person.

- *Information about past donations:* In Experiments 1 and 2, subjects were informed of the average amount donated in a separate experiment described by Drouvelis and Marx (2018), which had "similar sessions" to those reported in the current paper. Here we exploited differences in gifts across treatments to randomly vary the signaled amount without deceiving subjects. The relevant average donations were £0.665 and £1.047. Within sessions we evenly divided subjects into those who received a low signal amount and a high signal amount. After the information signal we allowed subjects to choose a new donation amount and then asked them to again estimate the average of their labmates' first-opportunity donations. Subjects' responses were again incentivized in that correct estimates within £0.10 were compensated with an additional £1.

After subjects had completed each of the above scenarios, we randomly selected which scenario to implement and informed subjects of the scenario, their donation decision under the scenario, and any extra payments for correct beliefs about others. Finally, subjects responded to a post-experimental questionnaire in which we collected data on their demographic characteristics and on the Cognitive Reflection Test (CRT) of Frederick (2005).

In total, 223 subjects participated in Pilot 1, and 91 subjects participated in Pilot 2. All experiments were conducted in the Birmingham Experimental Economics Laboratory (BEEL) and all treatments were computerized and programmed with the Multistage software from Caltech. Subjects on average earned £9.89 in Pilot 1 and £14.93 in Pilot 2. [14] Sessions lasted, on average, 55 minutes.

[14] At the time of Pilot 1 (Pilot 1) £1 was equivalent to US$1.25 (US$1.24).

Appendix B: Experiment Instructions

Welcome! You are about to take part in an experiment. This experiment is run by the "Birmingham Experimental Economics Laboratory" and has been financed by various research foundations. Just for showing up you have already earned £2.50. You can earn additional money depending on the decisions made by you and other participants. It is therefore very important that you read these instructions with care.

It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. You may use the provided scrap paper but no phones, calculators, or other devices. If you use a device, talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your following of these rules.

We will first jointly go over the instructions. After we have read the instructions, you will have time to ask clarifying questions. Please do not touch the computer or its mouse until you are instructed to do so. Thank you.

This experiment consists of three different timed tasks. You will be paid a fixed amount of money for each correct answer you provide in each task. The total amount of money you will earn from this experiment will be £2.50 for showing up plus the sum of your earnings from each task of the experiment.

After Task 3 you will be told how many correct responses you gave in each of the tasks. After this you will collect your earnings.

Following these instructions you will find the instructions for Task 1 of the experiment. You will receive new instructions for the other tasks once everyone in the room has completed Task 1.

Task 1

Task 1 consists of arranging pairs of letters to form words like the following examples:

TR, EA, TS, RE = RETREATS.   CU, FF, LI, NK = CUFFLINK.

You must use all the letters. You can change the order of the pairs but you cannot change the order of the two letters within each pair. You will have 2.5 minutes to provide answers.

You will be paid 25 pence for each correct answer provided during the 2.5 minute time limit.

To answer a problem, you will simply type the word on the keyboard, then press OK and another problem will appear. You can choose not to answer a question by pressing the OK button. The answer will then be recorded as being incorrect and you will be moved to the next problem. To help with time management, there will be a clock counting down the seconds for the 2.5 minute duration.

Task 2

Task 2 consists of solving 2-number multiplication problems like the following example:

10 x 97 = 970.   20 x 30 = 600.

You will have 2.5 minutes to provide answers.

You will be paid 50 pence for each correct answer provided during the 2.5 minute time limit.

To answer a problem, you will simply type the numbers on the keyboard, then press OK and another problem will appear. You can choose not to answer a question by pressing the OK button. The answer will then be recorded as being incorrect and you will be moved to the next problem. To help with time management, there will be a clock counting down the seconds for the 2.5 minute duration.

Task 3

Subjects receive instructions only for the task they have been randomly assigned to perform on their screens.

Experimenter's announcement: You will now have an additional 5 minutes to perform one of the tasks. The rules and payment rate will be the same as when you performed the task before.

At the end of Task 3, subjects will get the following instructions:

Experimenter's announcement: You can now see the number of correct answers you gave in each of the tasks. Please give me a moment to print the results.

You will now be given an opportunity to donate some of your income from the experiment to a charity, and last, you will be asked to complete a survey.

Written onscreen: Thank you, you have completed the tasks. Your total earnings from today's experiment (including your £2.50 show-up fee) sum to £[***Autofill***].

Thank you, you have completed the tasks. Your total earnings from today's experiment (including your £2.50 show-up fee) sum to £[Autofill].

Would you like to donate some of your earnings to Acorns Children's Hospice of Birmingham? If so, please enter the amount (between £0 and £[Autofill]) in the box provided.

Thank you for considering donating to Acorns. We'd like to ask you a few questions about this. We will call the amount that you just entered on the previous screen your "*first-opportunity donation*." What do you think was the *average* first-opportunity donation among participants besides yourself in your laboratory session?

If your guess is within £0.10, you will receive an additional £1. When we refer to the average across people we include those who give zero.

Now we're going to give you some opportunities to let your donation depend on some information. We'll ask you to make a series of choices under different scenarios. After all students have responded to all scenarios we will select one of these scenarios at random and implement your choice in that scenario. We'll use the first-opportunity donation as Scenario 1. We will only implement the randomly-selected scenario, so you should make your choice in each scenario as if that is the scenario that will be implemented. Each scenario is equally likely to be implemented.

If you have any questions, please raise your hand. Otherwise, click to proceed. If you finish responding to all scenarios before other participants you will need to wait until others finish.

Scenario 2

This is a simple scenario that does not involve any additional information.

How much would you like to donate if this scenario is selected?

Scenario 3

In this scenario you can donate based on the first-opportunity donations of other participants in your laboratory session. If this scenario is selected we will calculate the average among others in your session (excluding you), determine the interval in which this average lies, and implement your desired donation for that outcome.

How much would you like to donate if the average of other participants' first-opportunity donation was…

a. at least £0.75 but less than £0.80 per person?

b. at least £1.20 but less than £1.25 per person?

c. at least £1.65 but less than £1.70 per person?

d. at least £2.10 but less than £2.15 per person?

e. any other amount?

Scenario 4

In this scenario you can donate based on the first-opportunity donations of other participants in your laboratory session and an anonymous donor (who we'll call "Donor X").

How much would you like to donate if the average of other participants' first-opportunity donation was…

a. at least £0.75 but less than £0.80, and Donor X adds £0.45 per person?

b. at least £1.20 but less than £1.25, and Donor X adds £0.45 per person?

c. at least £1.65 but less than £1.70, and Donor X adds £0.45 per person?

d. at least £2.10 but less than £2.15, and Donor X adds £0.45 per person?

e. any other amount, and Donor X adds £0.45 per person?

Scenario 5

In this Scenario, all the participants in this session will receive an extra £1 as a bonus.

How much would you like to donate to Acorns if this scenario is selected?

Scenario 6

In this Scenario, all the participants in this session will receive an extra £2 as a bonus.

How much would you like to donate to Acorns if this scenario is selected?

Scenario 7

In this Scenario, half the participants in this session will receive an extra £2 as a bonus, and the other half will receive no bonus. You have been randomly assigned to the half that will receive [£2 / no bonus].

How much would you like to donate to Acorns if this scenario is selected?

Scenario 8

In this Scenario, half the participants in this session will receive an extra £2 as a bonus, and the other half will receive no bonus. You have been randomly assigned to the half that will receive [£2 / no bonus].

How much would you like to donate to Acorns if this scenario is selected?

Scenario 9

In this scenario you can choose a donation for another participant. You will be randomly assigned to one other person in the laboratory. This person will receive a bonus of £2 minus any portion of the £2 that you choose to have donated to Acorns.

How much of the £2 would you like to have donated to Acorns if this scenario is selected?

How much of your own earnings would you like to donate to Acorns if this scenario is selected?

Scenario 10

Earlier this semester BEEL ran an experiment like the one you've participated in today, and we gave participants an opportunity to donate a portion of their earnings to Acorns.

In this scenario you can donate based on the average first-opportunity donations across laboratory sessions of this earlier experiment.

How much would you like to donate if this average was…

a. at least £0.75 but less than £0.80 per person?
b. at least £1.20 but less than £1.25 per person?
c. at least £1.65 but less than £1.70 per person?
d. at least £2.10 but less than £2.15 per person?
e. any other amount?

Scenario 11

Earlier this semester BEEL an experiment like the one you've participated in today, and we gave participants an opportunity to donate a portion of their earnings to Acorns. The average donation across sessions in this experiment was £<u>X</u> [1.225 / 2.135] per person.

How much would you like to donate to Acorns if this scenario is selected?

Now you can guess again: What do you think was the average first-opportunity donation among participants besides yourself in your laboratory session?

If your guess is within £0.10, you will receive an additional £1.

Scenario 12

In this scenario you can again donate based on the first-opportunity donations of other participants in your laboratory session.

How much would you like to donate if the average of other participants' first-opportunity donation was…

a. at least £0.75 but less than £0.80 per person?

b. at least £1.20 but less than £1.25 per person?

c. at least £1.65 but less than £1.70 per person?

d. at least £2.10 but less than £2.15 per person?

e. any other amount?

Scenario 13

In this Scenario, all the participants in this session will receive an extra £2 as a bonus.

How much would you like to donate to Acorns if this scenario is selected?

Scenario 14

In this scenario you can again choose a donation for another participant. You will be randomly assigned to one other person in the laboratory. This person will receive a bonus of £2 minus any portion of the £2 that you choose to have donated to Acorns.

How much of the £2 would you like to have donated to Acorns if this scenario is selected?

How much of your own earnings would you like to donate to Acorns if this scenario is selected?

Appendix C: Additional Figures and Tables

Figure C.1: Does earned income affect donations?

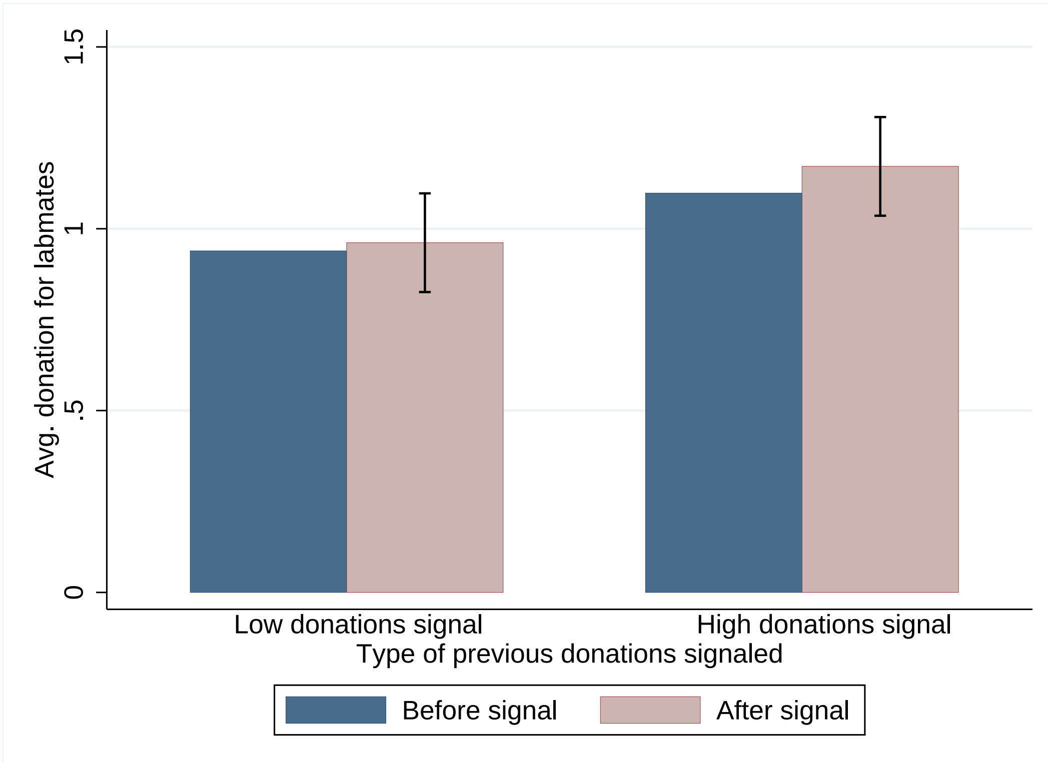
Notes: The figure shows the correlation between residualized (against task 1 and task 2 earning) toatal earning and donations right after tasks.

Table C.1: Within-subject responses to donations by others

	No outside donor		Donor X adds £0.45		Stacked
	(1)	(2)	(3)	(4)	(5)
Others' Avg. Donation	0.5261***	0.5384***	0.4668***	0.1923	0.5261***
	(0.0459)	(0.1118)	(0.0561)	(0.2204)	(0.0459)
Others' Avg. Donation Squared		-0.0042		0.0947	
		(0.0371)		(0.0670)	
Anonymous Donor * Others' Avg.					-0.0593
					(0.0448)
Anonymous Donor Scenario Dummy					0.0624
					(0.0676)
N	664	664	664	664	1328
Adj. R-squared	0.40	0.40	0.25	0.25	0.24

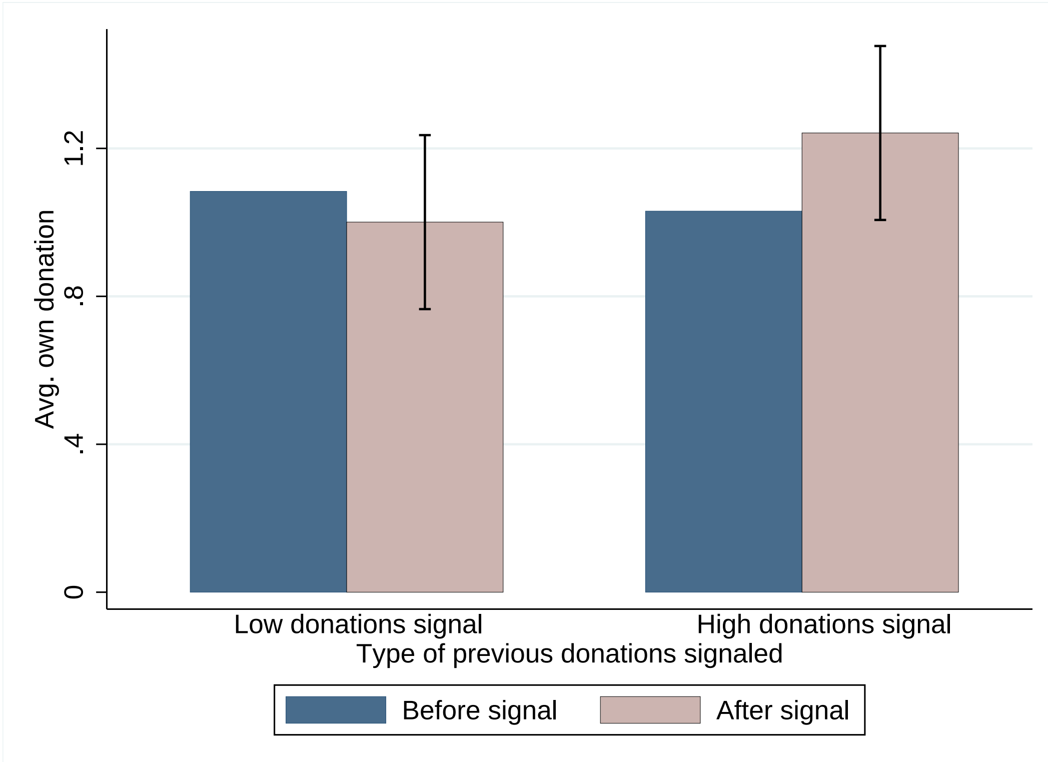
Notes: Each observation is a donation decision in scenarios conditioning on the level of donations by labmates (1 & 2), by the anonymous donor (3 & 4), or both. "Others' Average Donation" is the minimum of the range of possible donations by others for each scenario choice. All regressions include subject fixed effects, and standard errors clustered by subject. *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level.

Figure C.2: Donation for labmates with £2 bonus, by signal



Notes: Bars represent average donations for labmates when labmates are given £2 bonus before and after signal by signal. The line segments on the second bar of each bar pair are 95% Confidence Intervals (CIs) indicating equality of these two bars. These CIs are estimated from regressions of donations on a dummy that equals 1 if the scenario is after signal is revealed and 0 otherwise. See footnote of Figure 1 for more details about the regression.

Figure C.3: Own donation after donating for others, by signal



Notes: Bars represent average donations before and after signal by signal. The line segments on the second bar of each bar pair are 95% Confidence Intervals (CIs) indicating equality of these two bars. These CIs are estimated from regressions of donations on a dummy that equals 1 if it is a post-signal scenario and 0 otherwise. See footnote of Figure 1 for more details about the regression.

Table C.2: Individual characteristics included in the analysis and variable description

Individual characteristics	Variable description
Age	Age of the subject
Male	Dummy equals 1 if the subject is male
Married	Dummy equals 1 if the subject is married
Father's Education Level	Linear ranking of (Primary, 2ndry, some U, U degree, Post grad)
Mother's Education Level	Linear ranking of (Primary, 2ndry, some U, U degree, Post grad)
Attend Services	Religious svcs: 0: never, 1: 1-2/year, 2: 1/mo., 3: 1/week.”
Has a Polit. Party	If the subject belongs to a political party
Feels Most People Fair	Subject thinks most people fair (vs. take advantage if they can)
Would Avoid Paying for Transit	Justified on public transport? 0: never, 1: sometimes, 2: often.
Ever Partic. in Past Exp.	Has the subject ever participated in an economics experiment
Times Partic. in Past Exp.	How many times the subjects participated in experiments
No. of Known Labmates	How many labmates the subject knows
Cognitive-Reflective Test Score	Frederick (2005)
Not from UK or EU	Nationality = Other
Has Donated to Charity	If the subject has donated before.
Log of Past Donations	log(1+amount)
Has Religion	Does the subject have religion
Knows Any Labmates	If the subject knows any labmates by name
Involvement in Organizations	Sum across types. 0: None, 1: Mbr, 2: Active Mbr, 3: Mgr, 4: Board Mbr. (Types: Sport clubs, Music group, Political party, Lobby group, Non-profit institution, Other kind of voluntary organisation)