Dimensions of donation preferences

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Dimensions of Donation Preferences: the Structure of Peer and Income Effects[*]

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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 subjects. Donations are increasing in peers' donations and past subjects' donations. These and other results suggest a model of heterogeneous beliefs about the social norm for giving. Estimation of such a model reveals substantial heterogeneity in subjects' beliefs about and adherence to the norm. A simple fundraising strategy increases donations by an estimated 30 percent by exploiting previously unstudied correlations between dimensions of donor preferences. *Keywords: charitable, donation, altruism, warm glow, social preferences, peer effects, experiment. JEL: D01, D64, A13.*

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1 Introduction

There are a variety of reasons that an individual might donate to charity. An understanding of donation preferences can inform the design of mechanisms that address the expected underprovision of donated goods. Much research in economics examines preferences over charitable giving through the framework of the impure altruism model of Andreoni (1989), in which donors may value the public benefits of their donations or the “warm glow” they feel from their own donation. Researchers have devised experiments that have identified motives for giving that include pure altruism (Ottoni-Wilhelm et al. 2017), income effects (Cherry et al. 2002; Cherry and Shogren 2008), and a variety of factors that can produce warm glow, such as reciprocity (Falk 2007; Croson 2008), signalling (Glazer and Konrad 1996; Andreoni and Bernheim 2009; Ariely et al. 2009; Bracha et al. 2011), social pressure (DellaVigna et al. 2012; Andreoni et al. 2017), and social norms (Krupka and Croson 2016), among others.

Given the variety of identified motivations for charitable giving, can we describe preferences with a single utility function? To which motivation(s) should charities appeal? When multiple techniques can appeal to a similar motivation, which should the fundraiser use? Are there diminishing returns to each technique? Are the individuals who respond to gifts from the charity the same individuals who respond to announcements about others’ giving? How should charities target and sequence their appeals?

In this study, we explore these questions with an approach that has not been commonly used in experimental studies to-date. Our goal was to estimate a flexible utility function for each subject, allowing each to exhibit different motives for giving, in order to evaluate a variety of potential fundraising mechanisms. Estimating such flexible utility functions requires multiple sources of variation. For example, DellaVigna et al. (2012) used three fundraising treatments and a variety of survey treatments to estimate a two-parameter distribution of utility function parameters and costs associated with giving. Ottoni-Wilhelm et al. (2017) estimated a two-parameter utility function for each subject using a within-subject design varying endowments and the contributions of others across six scenarios. Our study follows these examples and expands the design to incorporate a number of factors that have been found to affect charitable giving.

The design of our experiment varied multiple treatments both between and within subjects to provide a uniquely comprehensive characterization of donation preferences. Subjects were paid a piece rate for real-effort tasks, 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. 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 that informed subjects of either a high or low level of average donations in recent laboratory sessions.

We obtain a variety of results that paint a complex picture of motives for giving. Peer effects are positive, with subjects' donations increasing in those of labmates and past subjects. However, subjects did not respond to labmate donations from bonus income or to gifts by an anonymous donor. Income effects exhibit similarly strong dependence on the source: a £1 increase in earned income had no effect on donations (estimate of £0.008 with standard error 0.067), yet paying subjects a £1 bonus increased the average donation by £0.38 (standard error 0.04). These results were robust across subject nationality, subject responses to explanation of the design, and two randomly assigned orders of the treatment scenarios.

Though the average treatment effects were similar across groups of subjects, there was considerable heterogeneity in the responses. Comparing responses across what we label "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. There is also a strong correlation between a subject's giving from own bonus income and giving from others' bonus income, but these responses are not highly correlated with responses to social treatments. Thus, it appears that there are peer-effect and income-effect components to multi-dimensional donor types.

We argue that our results are most consistent with a model of pure warm glow driven by a preference to comply with an uncertain social norm.[1] In this model, subjects learn about the norm from donations by others. 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. We account for non-negativity of donations and allow subject perceptions and preferences to vary.

Our model allows us to estimate donations under counterfactual fundraising campaigns. We find that the charity would not benefit from simply 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 an estimated 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. However, even this simpler strategy may be difficult to implement in that it would require charities to gauge donor types prior to the fundraiser.

Our experiment follows several others that have estimated income effects and peer effects in charitable giving. Our estimates of the causal effects of income on donations employ a novel strategy of randomly assigning tasks, yet give results similar to those in the literature on "house money" or "windfall money" effects (Cherry et al. 2002; Cherry et al. 2005; Harrison 2007; Kroll et al. 2007; Erkal et al. 2011; Reinstein and Riener 2012; Carlsson et al. 2013). We generally find positive peer effects, consistent with past work finding positive effects of the perceived donations of others (Hermalin 1998; List and Lucking-Reiley 2002; Vesterlund 2003; Andreoni 2006a; Potters et al. 2007; Shang and Croson 2009, Huck and Rasul 2011; Karlan and List 2012; Smith et al. 2013, Huck et al. 2015; Kessler 2017).[2] [3] We build on these findings by examining heterogeneity across subjects, as in studies

[1] Social norms are commonly recognized rules for appropriate behavior in a 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 (Krupka and Croson 2016; Drouvelis et al. 2019).

[2] A related strand of literature has found that peer-to-peer solicitation can increase donations (Meer 2011; Meer and Rosen 2011; Castillo et al. 2014, 2017). These solicitations may work through social pressure, as in (DellaVigna et al. 2012; Andreoni et al. 2017), which our experiment avoids.

[3] Peer effects have been studied in a wide variety of settings. A few examples of these many papers

of conditional cooperation in public goods games (Fischbacher et al. 2001; Gächter 2007). We find heterogeneity in multiple dimensions of preferences, and we document correlations between them, which allows us to refine the model of donor preferences and provide guidance for increasing donations.

These studies make up part of a larger literature on donation preferences. The broader literature has been reviewed by Andreoni (2006b), Schokkaert (2006), List (2011), Andreoni and Payne (2013), and Vesterlund (2016). In a prominent example of this literature, Crumpler and Grossman (2008) provide evidence for warm glow by showing that subjects donate even when they know their donations will be offset so as not to affect the allocation to the recipient. Gangadharan et al. (2018) use this method to identify warm glow givers and then identify altruism by giving the same subjects the opportunity to transfer more money to the recipient. Both Gangadharan et al. (2018) and Ottoni-Wilhelm et al. (2017) identify altruism by pairing givers with individual recipients in order to control the level of giving by others. Studies tend to find stronger evidence for warm glow when subjects are simply one of many contributors to a charitable organization (e.g., volunteering in Brown et al. 2019) and when donation amounts are small (DellaVigna et al. 2012; Karlan and Wood 2017). Consistent with the latter types of studies, we only find evidence of warm glow in our setting of giving to a charity following a transaction, as in "point-of-sale" campaigns in retail stores and restaurants.[4] While such campaigns raise small amounts from each donor, these amounts can add up, as shown by 73 campaigns that raised over $440 million in 2016 (www.engageforgood.com, 2017).

Our study offers several contributions to the body of research on charitable giving. We find that our subjects' behavior is consistent with past work on income and peer effects but does not replicate the finding of positive effects from anonymous lead donations. We also

and settings include criminal behavior (Bayer et al., 2009), energy use (Alcott 2011; 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; Herbst and Mas 2015), and especially in education (reviewed by Epple and Romano (2011), with recent contributions including Duflo et al. (2011), Imberman et al. (2012), Carrell et al. (2013), and Lim and Meer (forthcoming)). As in other experimental studies, we overcome the Manski (1993) reflection problem of inferring peer effects from observational data by exogenously varying information about baseline group behavior.

[4]Similarly, Andreoni et al. (2017) study donations solicited outside of a supermarket. 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.

find that subjects do not respond to our novel treatments that increase the giving of their labmates by providing them with bonuses. By varying giving by others in multiple ways, these treatments provide more robust evidence that subject choices are not motivated by the total amount donated, as they would be in the altruistic model. If subjects are indifferent to the amount received by the charity, then peer effects, which the literature has shown could arise through quality signaling or threshold effects in the level of the public good, instead appear to be best explained by more social factors such as norms. We show that subjects vary in their responsiveness to this social influence as well as their propensity to donate windfall income, dimensions of donation preferences that our design allows us to show are largely uncorrelated. We then estimate counterfactuals that demonstrate how identifying donor types could allow charities to improve the timing and targeting of their solicitations.

The paper proceeds as follows. Section 2 describes the design of the experiment. Results of the experiment appear in section 3. Section 4 proposes a model that synthesizes the results and presents estimates of the model and implied counterfactuals. Section 5 concludes.

2 Experimental Design

The experiment occurred in two steps. In the first part, subjects performed real-effort tasks that allowed them to generate income. In the second part, subjects were allowed to donate part of their earnings to a local charity in each of a series of scenarios. In this section, we describe each of these parts of the experiment in turn, followed by a description of the analytical methods that we apply to the resulting data.

The overall design of the experiment was intended to provide the most comprehensive description of donation preferences to date. We followed past research by varying subject income and information about donations of others, factors that can help to identify the altruism and warm glow components of the standard model of impure altruism, as we show in Appendix A. Most previous studies have varied one of these factors in a particular way, such as either announcing a donation by an anonymous individual or announcing the amount donated by peers. We varied each of these factors in multiple ways. Doing so allowed us to test whether past results replicated in our setting of small donations following a transaction.

Our design also allowed us to assess robustness by, for example, testing whether income effects depended on the source of income and whether peer effects depended on the degree to which other donors were peers. Moreover, varying these factors within subject made it possible to test whether a subject's response to one factor was correlated with her response to another factor, providing novel evidence on the connection between different motivations for giving.

2.1 Real-effort Tasks

Subjects performed two types of tasks: math and language tasks. Having all subjects complete both tasks before any randomization gave us a baseline measure of relative skills, which may vary with gender (Niederle and Vesterlund, 2010), and also increased the chances that a subject would earn enough to consider donating even if he or she was poor at one of the tasks. For both the math and language 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 language 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. Subjects earned 25 pence for each correct response in the language task and 50 pence for each correct response in the math task. These times and piece rates were chosen based on testing in two pilot experiments that we describe in Appendix B. We conducted these pilot experiments to test subjects' ability to perform tasks and answer questions, and most importantly, to obtain two different donation means that we could truthfully report to subjects in the main experiment, as we describe below.

We used a randomly assigned third task to study the effect of earned income on donations. For this third and final task, subjects were randomly assigned to again perform either the language or math task. The piece rate for each task remained the same, but the time available for this task was increased to five minutes in order to increase the variance of earned income. The induced variation was not only random but also predictable based on a subject's prior performance in the first two tasks.[5]

Distributions of earnings from the tasks are shown in Appendix Figure D.1. The mean of earnings was £9.99.

2.2 Donation Choices

Subjects were given the opportunity to donate part of their earnings upon completion of the tasks. The recipient charity was Acorns Children's Hospice, which is local and well known in the community, and for which subjects could click on a link for more information before choosing their donation. 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 C.

Scenario 2 simply repeated the opportunity to donate, changing nothing from Scenario 1 except that the donation scenarios had been explained. In this way, we tested whether

[5] Earnings can alternatively be varied by randomizing piece rates, which may result in a different set of "compliers" if subjects vary in terms of their preferences between tasks or strength of earnings motivation. The results of our approach turn out to match those of charitable giving studies that randomized piece rates (Tonin and Vlassopoulos 2017; Drouvelis et al. 2019).

explanation of the donation scenarios, which created an environment in which it was clearer that donation choices were part of the experiment, introduced experimenter demand effects (Zizzo, 2010).

The remaining scenarios were each designed to test for a category of income or peer effects. In total, there were 14 scenarios. Here, we describe each of these categories and examples of past research in which they have been studied. We provide these descriptions in an order intended to clarify the design of the experiment and then explain their order in the experiment and how we experimentally manipulated this order.

- *Information about past donations*: In contrast to the crowd-out predicted by the pure altruism model, research has found that donations are increasing in the amount that others have recently given (e.g., Shang and Croson 2009; Smith et al. 2013). We tested this in our in scenario #11 by informing subjects that the average donation in a previous experiment was either £1.225 or £2.135, the respective mean values from the two pilot experiments described in Appendix B. We will refer to these as the high and low signals because past donations may signal the amount given by others currently in the laboratory. To test this, we followed the donation choice by again asking subjects to estimate the average of their own labmates' first-opportunity donations (again providing a £1 incentive payment for a guess within £0.10 of the correct amount). Because this information treatment could affect a subject for the rest of the experiment, we employed between-subject variation, as we had for the earnings task treatment. However, as a test of subject consistency across scenarios, and as a way to test whether the result extended to other values of giving by others, we preceded the between-subject treatment with a scenario (#10) in which the subject could condition donations on the level of past donations. To facilitate comparison of responses between scenarios, we provided conditioning sets of past donations that included small ranges around the signal amounts of £1.225 and £2.135: 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.

- *Labmates' donations*: Much like the field research on past donations, studies in the laboratory have found that subjects respond positively to the amount donated by labmates (Vesterlund 2003; Potters et al. 2007). In scenario #3, we allowed each subject to condition the amount she would donate on the average donations of the other subjects in her session. We used the same ranges as in past-donations scenario #10, allowing us to compare the effects of the two treatments and test whether subjects are as responsive per pound donated by those in the lab that day as they are to donations by the more distant reference group of past participants. We then repeated this question in scenario #12, which followed the past donations signal.

- *Anonymous donations*: Several field experiments have found positive effects of announcing lead donations, an initial gift by a donor who is often anonymous (List and Lucking-Reiley 2002; Huck and Rasul 2011; Karlan and List 2012; Huck et al. 2015). In this scenario (#4) we again asked subjects to choose a donation for each of the ranges of labmates' first-opportunity donations, but subjects were informed that an anonymous donor ("Donor X") would augment each amount by donating an extra £0.45 per person. This was a lead donation, and not a matching gift, because if the scenario was selected, then the anonymous donation would be made regardless of any subject's donation choice. The researchers made these donations to the charity for all sessions in which this scenario was randomly selected for implementation.

- *Bonus income:* As described above, we randomly assigned one of the earnings tasks in order to estimate income effects on donations. Studies have found that the income effect may be larger for windfall income than for earnings (Cherry et al. 2002; Cherry et al. 2005; Harrison 2007; Kroll et al. 2007; Erkal et al. 2011; Reinstein and Riener 2012; Carlsson et al. 2013). We therefore explored how subjects' donations depend on their receipt of windfall bonus income. Subjects received a £1 bonus in one scenario (#5) and £2 in another (#6), allowing us to estimate nonlinear responses. In separate scenarios (#7-8), subjects were informed that half of the participants in the session would 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. Scenario #13 repeated the £2 bonus, the largest and most comparable to the next treatment category, following the past-donation signal.

- *Making donations for others:* In these scenarios, we tested our most novel treatment. If subjects could choose the amount donated by others, then this would influence the amount donated to the charity but not a subject's perceptions about others' beliefs or behavior. Allowing subjects to choose donations for others therefore offers a test between altruistic models, which depend on the amount donated, and warm glow models that depend on the amount of the subject's own donation. Thus, in scenario #9, we informed subjects that they would allocate a £2 bonus between another subject (who was selected at random) and the charity. We then asked subjects to choose a donation amount for themselves, providing a test for whether the increase in donations by labmates that was not of these labmates' choosing had an effect on the subject's own donation. Scenario #14 repeated this scenario following the past-donation signal.

A few constraints determined our ordering of the tasks. First, we wanted to give subjects an opportunity to donate before explaining that there would be many donation scenarios, and we wanted to do the same after this explanation without changing other information, which pinned down scenarios #1 and #2. Because information about past donations would alter the subject's beliefs for the rest of the experiment, we placed this scenario after all others, then repeated some of the main scenarios to allow for interactions between the information and other motivations. We placed the anonymous donor scenario (#4) after the labmates' donations scenario (#3) upon which it built.

The rest of the order of the scenarios was not uniquely pinned down, which lead us to develop a main order that we thought most intuitive and then to randomly assign variants of this order. In the main order, we placed the donate-for-labmate scenario (#9) last before the information treatment because it involved the potential complicating factor of thinking about others' preferences. We placed the bonus scenarios (#5 through #8) in order of bonus amount and complexity, with the scenarios paying all subjects a bonus before the scenarios paying bonuses to only half of all subjects. As noted above, we randomly assigned the order of the two scenarios in which only half of the subjects received bonuses, which we thought were the scenarios most likely to exhibit order effects, say through emotional responses to being lucky or unlucky in the earlier scenario. We also randomly assigned an alternative order in which the donate-for-labmate scenarios appeared earlier in the sequence.[6] Appendix Table D.1 shows that randomizing the order of the scenarios had no effect on donations in any scenario.

Following the donation scenarios, we randomly selected the scenario to be implemented. 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 (Frederick 2005) and predicts some forms of prosociality (Corgnet et al. 2015). The questionnaire provided subject characteristics that allow us to test for heterogeneity in the effects of our treatments.

While our design allowed for a wide variety of utility functions, we hypothesized that our study would replicate past research on income effects and that there would be two subject types in terms of peer effects:

1. Subjects will donate a smaller percentage of marginal earned income (task 3) than unearned income (scenarios 5, 6, 8, 13).

2. Altruistic subjects will donate less when the amount received by the charity increases (scenarios 4, 7, 9, 14).

3. Social norm adherents will donate more when donations of other subjects increase (scenarios 3, 10, 11, 12).

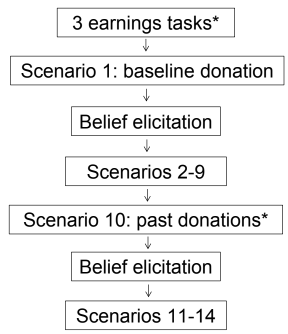
The design is summarized in Figure 1, with individual treatments and results listed in Table 1 below. In total, the experiment included 169 subjects.[7] On average, subjects

[6] Relative to the sequential ordering in Table 1, the second ordering was {1, 2, 3, 9, 4, 5, 6, 7, 8, 10, 11, 12, 14, 13}.

[7] We removed three subjects whose donated amounts varied greatly across scenarios, with respective ranges of £0-14, £0-16, and £2-20. The variance in these choices suggests that these subjects either did not understand the instructions or did not take their choices seriously. Inclusion of these subjects reduces statistical precision but has little effect on the pattern of results.

earned £12.49 for attending and performing tasks. 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.

Figure 1: Experiment design



* Subjects per Treatment		Past donations	
		£1.225	£2.135
Task 3	Math	43	39
	Language	44	40

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 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 is unusual in that our treatments vary not only budget sets but also the nature of income, the information set, and the conditioning set. This omnibus design provides rich data capturing subject-level responses to a variety of stimuli, allowing us to test for multiple motives for charitable donations, flexible functional forms, and correlations or interactions between motives.

2.3 Methods of Analysis

The complexity of our design necessitates the use of a combination of regression methods. We describe these methods here according to the type of treatment analyzed.

Between-subject regressions

We randomly assigned several aspects of the experiment across subjects: whether the third task involves math or language, 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 (3, 4, 10, and 12) 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, 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 language problem from Task 1 (worth £0.25 per correct answer) or the math problem from Task 2 (worth £0.50 per correct answer). For a subject who earned more in Task $j \in \{1, 2\}$, our instrument for total earnings is an indicator for whether the subject was randomly assigned to repeat Task j.[8]

[8] Seven subjects earned identical amounts in the first two tasks. We code them as being better at math,

The 2SLS estimating equations are as follows.

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

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

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. Equation (4) is the first stage regression. We regress total task earnings on $Instrument_i$, an indicator for assignment to the subject's better task. This instrument exploits the fact that performance is predictable given performance in the relevant task completed earlier, but causal identification is provided by the random assignment of Task 3.

The fact that donations must be greater than or equal to zero can potentially cause complications. This truncation issue typically arises in observational studies with continuous covariates, whereas in randomized trials, dummy variables for treatment arms fully saturate a regression model and thus provide the mean values of donations in each arm. In our experiment, truncation can arise when we estimate effect of earnings, a continuous variable. As in past research (e.g., Huck and Rasul 2011), we estimate a Tobit model and hurdle model. The hurdle model consists of separate regressions, one capturing the extensive-margin response with a dummy outcome for positive donations, while the other captures the intensive-margin response with log donations as the outcome and the sample restricted to those with positive donations. We use an IV Probit model when the outcome is an indicator variable.

3 Experiment Results

Table 1 displays results for each scenario and sub-scenario. For each, we show the share donating, median donation, and mean donation. We also estimate the difference in means between the scenario or sub-scenario and the relevant baseline, which is Scenario 2 for all scenarios preceding the provision of information about past donations and is Scenario 11 for all scenarios after the information is provided. For scenarios in which choices were elicited for several possible values of giving by others, the table displays the estimated slope of own

which will reduce the power of the instrument but allow us to maintain the same sample as in other analyses.

donations with respect to those of others.

Table 1: Donations in each scenario

	Share donating	Median	Mean	Difference (from baseline)	SE	Within -scenario slope	SE
1. First opportunity	0.68	0.5	1.1	0.15*	(0.08)		
2. Baseline	0.65	0.5	0.95				
3a. Labmates giving £0.75 - £0.8	0.86	0.75	0.84	-0.11	(0.11)	0.53***	(0.05)
3b. Labmates giving £1.20 - £1.25	0.84	1.2	1.07	0.12	(0.1)		
3c. Labmates giving £1.65 - £1.70	0.84	1.65	1.33	0.38***	(0.1)		
3d. Labmates giving £2.10 - £2.15	0.83	2	1.55	0.6***	(0.1)		
3e. Labmates giving other amount	0.74	1	1.29	0.34***	(0.08)		
4a. Anonymous donor, labmates giving £0.75 - £0.8	0.84	0.75	0.88	-0.07	(0.11)	0.47***	(0.06)
4b. Anonymous donor, labmates giving £1.20 - £1.25	0.82	1.2	1.05	0.1	(0.1)		
4c. Anonymous donor, labmates giving £1.65 - £1.70	0.81	1.5	1.26	0.31***	(0.1)		
4d. Anonymous donor, labmates giving £2.10 - £2.15	0.83	2	1.51	0.56***	(0.11)		
4e. Anonymous donor, labmates giving other amount	0.76	0.93	1.24	0.29***	(0.08)		
5. £1 Bonus	0.83	1	1.38	0.43***	(0.09)		
6. £2 Bonus	0.87	1	1.71	0.76***	(0.1)		
7. Receive £0 while others receive £2	0.65	0.5	1	0.05	(0.08)		
8. Receive £2 while others receive £0	0.89	1	1.76	0.81***	(0.11)		
9a. Donation chosen for a labmate from £2 bonus	0.84	1	1.01	0.06	(0.12)		
9b. Own donation after choosing for labmate	0.77	1	1.06	0.11	(0.09)		
10a. Past session giving £0.75 - £0.8	0.83	0.75	0.81	-0.14	(0.1)	0.47***	(0.05)
10b. Past session giving £1.20 - £1.25	0.81	1.2	1.03	0.08	(0.1)		
10c. Past session giving £1.65 - £1.70	0.81	1.54	1.24	0.29***	(0.1)		
10d. Past session giving £2.10 - £2.15	0.81	2	1.45	0.5***	(0.1)		
10e. Past session giving other amount	0.73	0.82	1.15	0.2**	(0.08)		
11. New baseline: Information about past giving	0.83	1	1.22				
12a. Post-information, labmates giving £0.75 - £0.8	0.81	0.75	0.77	-0.45***	(0.05)	0.50***	(0.05)
12b. Post-information, labmates giving £1.20 - £1.25	0.81	1.2	1.02	-0.2***	(0.04)		
12c. Post-information, labmates giving £1.65 - £1.70	0.81	1.61	1.23	0.01	(0.04)		
12d. Post-information, labmates giving £2.10 - £2.15	0.81	2	1.45	0.23***	(0.05)		
12e. Post-information, labmates giving other amount	0.73	1	1.15	-0.07	(0.08)		
13. Post-information, £2 Bonus	0.87	1	1.6	0.38***	(0.09)		
14a. Post-information donation chosen for a labmate from £2 bonus	0.86	1	1.06	-0.16**	(0.08)		
14b. Post-information own donation after choosing for labmate £2 bonus	0.78	1	1.12	-0.1	(0.07)		

Notes: The table shows donation results for all scenarios. *** denotes significance at the 1-percent level, ** denotes significance at the 5-percent level, and * at the 10-percent level. Standard errors are in parentheses, and the standard errors for within-scenario slope are clustered by individual.

Within-subjects effects can be read directly from Table 1. First, we see from Scenarios 1 and 2 that after explanation of the experiment, subjects reduced their donations by a marginally significant £0.15. In Appendix Table D.2, we show that choices in the rest of the experiment are similar between subjects who changed their donation at this point and subjects who did not. Next, we see that in all scenarios that vary giving by labmates, subjects increase their donations by roughly £0.50 for each additional £1 given by others, and these slopes are all significant at the 1-percent level. However, the similarity between donations in Scenarios 3 and 4 reveals that subjects were not responsive to giving by the anonymous donor. Results for Scenarios 5, 6, 8, and 13 all show that bonus income significantly increases donations. In contrast, Scenarios 7, 9, and 14 show that subjects do not give more when bonuses are provided to others.

Result 1. *Donations increase with the amount donated by others in the lab.*

Result 2. *Donations by subjects do not respond to the anonymous donation.*

Result 3. *Donations increase with the amount of a subject's bonus income.*

Result 4. *Donations do not respond to the amount of other subject's bonus income.*

The effects of our between-subjects treatments cannot be read from Table 1. The first of these treatments randomized the third earnings task to estimate effects of earned income on donations. Results appear in Table 2. The first-stage regressions in the first sub-table show that assignment to a subject's better task of the two increases earnings by an average of £3.69 overall and by £4.82 among subjects who donate at the first opportunity. The second sub-table shows that this has no significant effect on average donations or on either the extensive or intensive margins of donating. Appendix Table D.3 shows that earned income also has no effect on donations made in Scenario 2, after the experiment has been explained.

Result 5. *Donations do not respond to earned income.*

Table 2: Initial donation response to earned income

(a) IV first stage

	(1) Earnings of all	(2) Earnings of donors
Assignment to Subject's Better Task	3.6935***	4.8163***
	(0.7322)	(0.8481)
Constant	8.1220***	7.9864***
	(0.4230)	(0.5025)
N	166	112
First stage F	25.45	32.25

(b) OLS and IV second stage

	(1) Donation OLS	(2) Donation IV	(3) Any donation IV Probit	(4) Log donation IV	(5) Donation IV Tobit
Earnings from Tasks	0.0264	0.0083	0.0032	0.0205	0.0087
	(0.0235)	(0.0668)	(.0198)	(0.0405)	(0.0949)
Constant	0.8321***	1.0121	0.6535***	-0.2105	0.5185
	(0.2698)	(0.6937)	(0.2000)	(0.4379)	(0.9582)
N	166	166	166	112	166

Notes: The table shows 2SLS results of the impact of earnings on donations. *** 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 other between-subject treatment varied information about past donations. Subjects gave significantly more if they were assigned the high signal about past donations (£2.135) than if they were assigned the low signal (£1.225). The effect size per dollar of past donations is estimated in Table 3, 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.

Result 6. *Donations increase with the amount donated by past subjects.*

Table 3: Donation response to amount of past donations

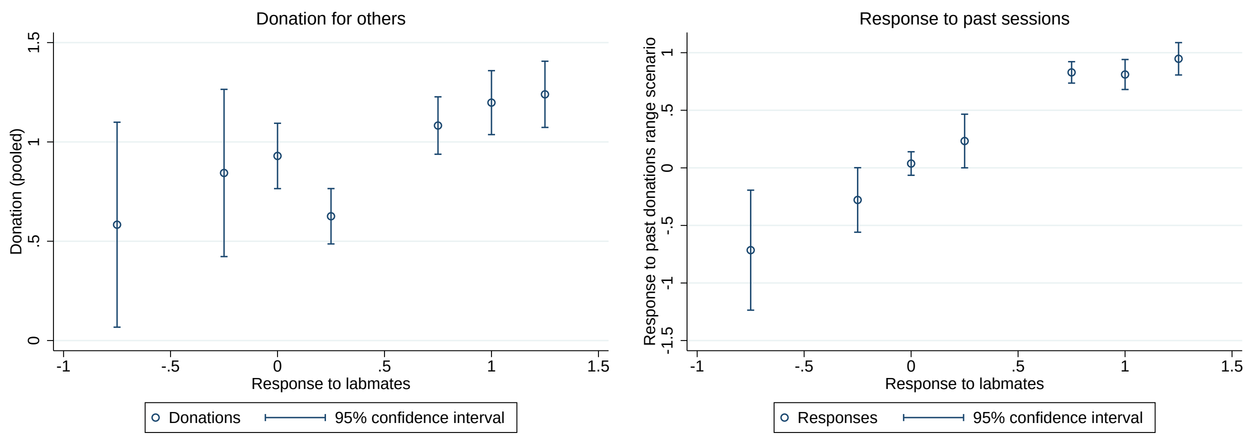
	(1) Donation	(2) Donation
Amount signaled	0.5547***	0.5191***
	(0.1718)	(0.1456)
Original donation		0.3565***
		(0.0965)
N	166	166
Adj. R-squared	0.06	0.31

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.

Many of our results for individual treatments are consistent with past research. As found in other studies, giving is more responsive to bonus income than to earned income. We also obtain the common result that subjects give more when they believe or know that others give more. Appendix Table D.4 shows that these patterns are consistent across subjects, with the Lasso method (Tibshirani, 1996) finding little evidence of heterogeneity across observed subject characteristics. We do not know of another study that varies the level of contributions to the charity by paying bonuses to other subjects, which we find, across multiple scenarios, has no effect on the giving of subjects not receiving the bonus. Our finding that appears most unlike corresponding estimates in the literature is that an anonymous donation does not affect our subjects' giving. Our within-subject design allows us to estimate not only the average effect of the anonymous donor but also the subject-level effects, and Appendix Figure D.2 shows that the effect is zero for the vast majority of subjects. Because this finding contrasts with positive effects found in the literature, we subsequently embedded a between-subject version of this treatment in another experiment, as we describe in Appendix E. We again found no effect of the anonymous donation, indicating that this was not due to our within-subjects design and that future research could explore the mechanisms determining whether announcements of anonymous donations affect giving.

Our experimental design is most novel in its ability to describe heterogeneity in subject types and how these relate across treatments. This allows us to describe dimensions of subject type. One apparent dimension is responsiveness to social scenarios involving others, correlations between which can be seen in Figure 2. 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 are increasing with the labmate response. The responses are not perfectly correlated, but they appear to indicate that socially-oriented motivation is a principal component, or dimension, of donor preference types.

Figure 2: 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.

Income effects appear to be another important dimension along which donor types vary. Appendix Figure D.3 shows that subjects' own charitable response to bonus income is highly correlated with the amount that they donate from others' bonus income. That the responses are similar despite the fact that only one is costly to the subject herself suggests that both choices are driven by beliefs about the amount of windfall income that is most appropriate to donate. While these choices are correlated with each other, they are not correlated with social motivation. For example, Appendix Table D.5 shows that donations from bonus income are similar across subjects who respond to their labmates' choices and those who do not. It appears, therefore, that income effects and peer effects represent distinct components of heterogeneity in multi-dimensional donor types.

Result 7. *Subjects' responses are positively correlated between any two social treatments and* *between any two bonus treatments but not between social treatments and bonus treatments.*

In summary, we find that income and peer effects vary across subjects and stimuli. We hypothesized that unearned income would have a greater effect than earned income, and this was supported by results 3 and 5. We find no evidence (results 2 and 4) that subjects donate less when the amount received by the charity increases, as in the altruistic model. Result 1, however, is that subjects donate more when donations of other subjects increase. This response is consistent with a social norm, and result 7 suggests that adherence to such a norm is not correlated with donation preferences related to income.

4 Model and Implications

To describe donation preferences in our setting and consider counterfactual scenarios, we refine the basic impure altruism model and then estimate its parameters. Our first refinement emphasizes warm glow based on our results related to giving by others. Our second refinement is to specify a form of warm glow based on our other reduced-form results.

The impure altruism model allows for altruistic and warm glow motivations. Pure altruism can be identified by responses to the amount contributed to the charity. Several results indicate that subjects are not motivated by total contributions to the charity. First, lead donations by an anonymous donor do not crowd out subjects' donations. Second, although subjects' donations are generally increasing in those of their peers, they do not donate more when their peers receive bonuses. Third, when subjects choose another subject's donation from bonus income, they generally choose a positive amount and therefore know that this additional amount is being donated, yet they also do not significantly change their own donations in this scenario.[9] These effects could all be zero on average if some subjects respond positively rather than negatively, but our heterogeneity analysis allows us to rule this out: responses to both the anonymous donor and to others' donations from bonus income are zero for most subjects, rather than positive or negative.

Economists define altruism as a preference over the value of the public good. Subjects'

[9]Strategic subjects could form beliefs about labmates' choices over bonus income and lower their own donations to offset those choices. We consider this unlikely because it would reduce donations, compounding the more standard crowding-out effect, and we do not find any reduction in own donations.

indifference to the amount given to the charity indicates no altruistic motivation at the margin. This is perhaps to be expected because preferences of impure altruists approach those of pure warm-glow givers as the amount of donations by others becomes large (Ottoni-Wilhelm et al., 2017). The extent of pure altruism cannot be identified for such givers. This does not imply that giving is never altruistic; indeed, Ottoni-Wilhelm et al. (2017) find that most individuals behave altruistically when giving by others is limited. Rather, it implies that a model of altruism does not describe behavior in our setting. Thus, we make the simplifying assumption of removing it from the model.

Given these results, we focus hereafter on the nature of the warm glow function. Our estimates of income effects also help to simplify the model. The finding that earned income has no discernible effect suggests that utility over consumption is approximately linear over the range of values made possible by the stakes of the experiment. 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.

4.1 Model

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

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

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

We now propose structure for the warm glow function. As Andreoni and Payne (2013) note of the "warm glow" term:

> The pejorative nature of the term is deliberate, and meant to remind the reader that the notion of a warm-glow is something of a black box. That is, there are many notions— sympathy, guilt, norm adherence, social approval—that could fall under the umbrella...

Among the potential components of warm glow listed by Andreoni and Payne (2013), we focus on norm adherence. Consider a social norm $n\left(I_u, \gamma_p\right)$ 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 with a parameter α giving the relative importance of warm glow. If $Ew\left(g, I_u, \gamma_p\right) = -E\left[\frac{\alpha}{2}\left(g - n\left(I_u, \gamma_p\right)\right)^2\right]$, then the first-order condition (along with a non-negative donation constraint[11]) implies that

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

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. As the strength of warm glow (α) increases, the optimal donation approaches the level of the norm, and as it decreases, the donation falls to zero. 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

[10]Drouvelis et al. (2019) focus on the effect of bonus income on donations and test whether this can be explained by social norms. Using the incentivized norm elicitation method of Krupka and Weber (2013), Drouvelis et al. (2019) find that subjects believe most people would consider a donation that was between £0 and £1 to be more appropriate if it was given from a bonus of £1 than if it was given from a bonus of £3. Work such as that of Shang and Croson (2009) suggests that giving by others could similarly affect norms, or more generally it may establish a reference point or rule of thumb.

[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.

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.

4.2 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

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

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. Subject-level results are displayed in Appendix Figure D.4. The figure is consistent with reduced-form results in that there is little correlation between the predicted effects of increasing bonus income and of increasing subjects' beliefs about others' donations. 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).

[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.

Table 4 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. Not surprisingly, while donations are increasing in the amount of bonus income, they increase by considerably less than the cost of providing the bonuses.

Table 4: 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 4 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 to the limited effects of providing basic information, 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 4 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. The average cost of these bonuses is £0.38, the share of the subjects receiving the bonus. Relative to the baseline solicitation, this tactic increases donations by more than the cost of the bonuses, even though individual subjects respond to bonuses by less than one-for-one. However, relative to the 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. These findings are consistent with a literature showing that give-aways to potential donors can increase donations but do not always cover their costs (Landry et al., 2006; Falk, 2007; Sieg and Zhang, 2012; Eckel et al., 2018).

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.

Our results 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. Whether this is true in settings beyond our experiment is an empirical question. Some individuals may be especially responsive to give-backs, particularly when their donations are not limited to their earnings in the laboratory. Moreover, while we find that few observable characteristics predict subject type in our sample (Appendix Table D.4), charities may have data related to potential donors that is more predictive. Indeed, recent work by Cagala et al. (mimeo) finds that fundraising returns can be increased by targeting based on the frequency of an individual's past giving. Our general conclusion is that donors have multi-dimensional types that imply opportunities to increase the effectiveness of charitable solicitations by learning about donor types and structuring appeals accordingly.

5 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 to 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. Estimation of this model reveals informative heterogeneity in donor types along multiple dimensions. In particular, we show that subjects' responses to different types of information about donations by others are more highly correlated with each other than with subjects' responses to bonus income. Counterfactuals suggest that charities would need considerable information about donor types to improve fundraising.

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, in our subject pool, was not strongly correlated with demographics.

It is our hope that this study will also be viewed as an example of the potential benefits of a multi-faceted within-subjects research design. While many studies vary budget sets within subject, it is less common to test a variety of disparate treatments in this way. Some detractions of such a design are that subjects experiencing multiple treatments may take each less seriously or may become more prone to experimenter demand effects as they gain in understanding of what is being tested. Our tests for order effects or differential behavior according to a subject's donation response to explanation of the scenarios should reduce these concerns, but the concerns cannot be eliminated completely. Another issue is that it may be difficult to find the type of parsimonious explanation for the full pattern of results that we believe the social norm model provides here. These concerns should be weighed against potential benefits of the design, such as the savings in time and subject payments relative to the number of between-subject experiments that would be needed to test all of the treatments. While a between-subject design provides an average treatment effect, a within-subject design reveals changes at the subject level. This allows for greater analysis of heterogeneity, which we find to be considerable for donor types, and allows for distinguishing whether an insignificant average response is due to offsetting responses in opposing directions, as we have done for the effects of an anonymous donation. We view such a design as a complement to between-subject designs, and we suspect that it may be particularly useful in studying behaviors about which there are many competing theories or for which researchers wish to better describe heterogeneity or estimate treatments among particular behavioral types.

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: 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 4 to discuss these limitations and propose an alternative model of preferences.

Appendix B: Pilot Experiments

Prior to conducting our main experiment, we ran two pilot experiments, which we label Pilot 1 and Pilot 2. The pilots were intended to test subjects' ability to perform tasks and answer questions over a series of donation scenarios. In our first pilot, we attempted to involve at least half as many subjects as the 282 who had participated in the experiment of Drouvelis and Marx (2018). Ultimately, 223 subjects participated in Pilot 1, and we found that the new within-subject design provided greater statistical power than our previous work and thus required fewer subjects. As such, we employed a smaller sample of 91 subjects in Pilot 2. The second pilot allowed us to test alternative tasks and revisions to scenarios that appeared to confuse subjects in Pilot 1. As in the main experiment, both pilots consisted of two parts, which we discuss in turn.

Part 1: Real-effort tasks: The nature of the tasks performed during Part 1 is the same as described in Section 2 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 language 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 language, the hard language, the easy math and the easy language task. The piece rate payment for correct answers was 3 pence for the easy version of either the language or the math task and 21 pence for the hard version of either the language 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 the experiment of Drouvelis and Marx (2018). 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).

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 C: 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 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.

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 £X [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

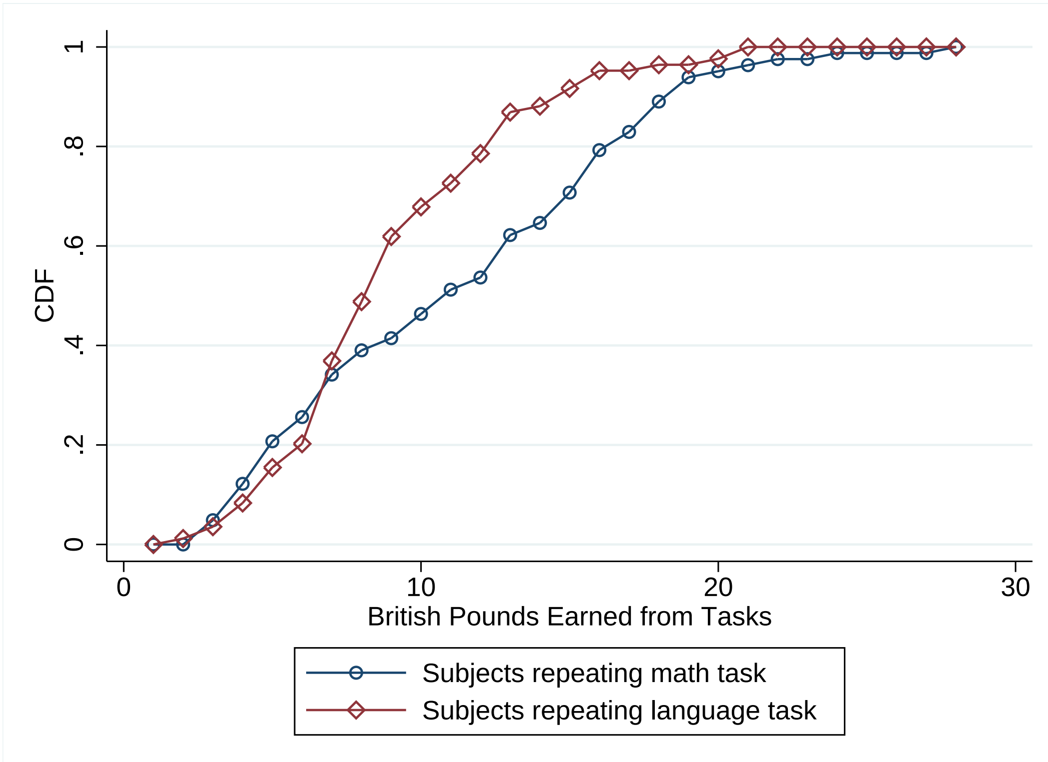
In this scenario you can again choose a donation for another participant. You will be randomly assigned to one other person in the laboratory. This person will receive a bonus of £2 minus any portion of the £2 that you choose to have donated to Acorns.

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?

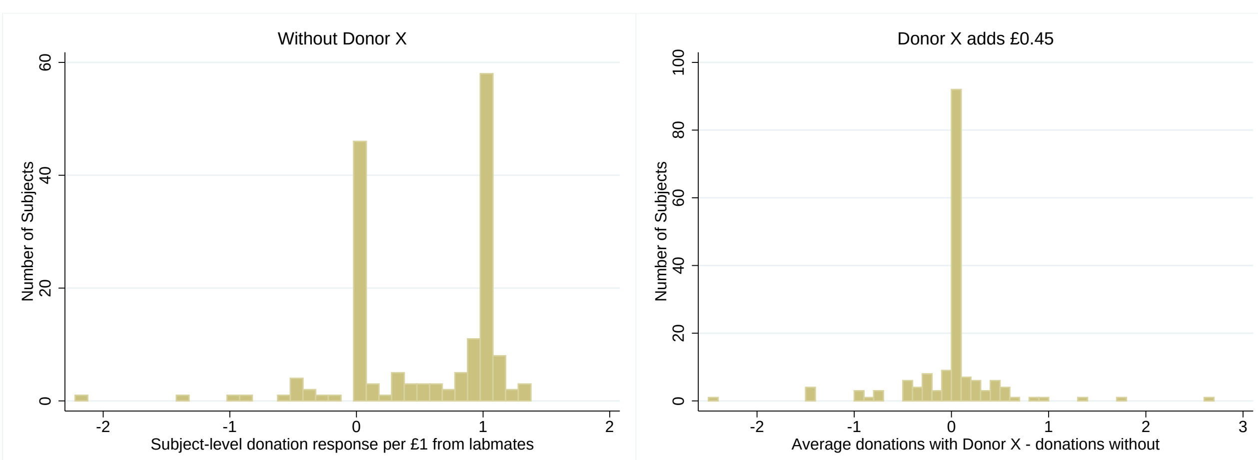
Appendix D: Additional Figures and Tables

Figure D.1: Cumulative distribution of earnings from tasks



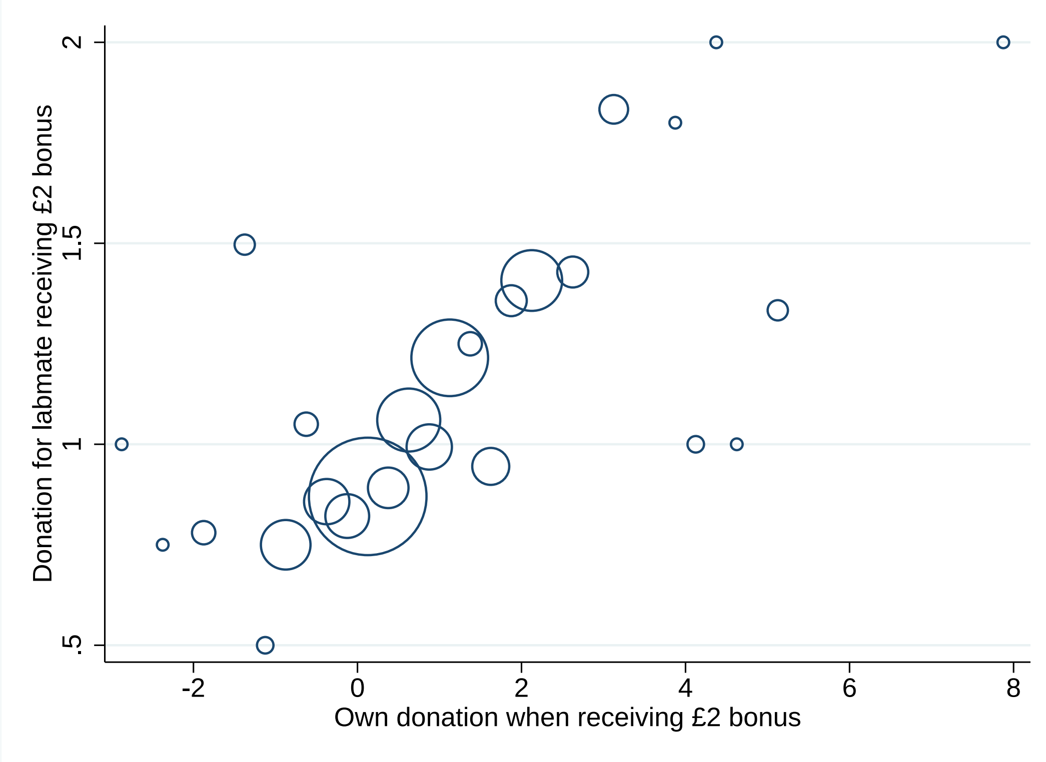
Notes: The figure describes earnings in experimental tasks (excluding show-up fee and any incentive payments). A CDF is shown for subjects assigned to repeat the math task (N=82) and those assigned to repeat the language task (N=84). A Kruskal-Wallis rank test rejects equality of the distributions (p=0.0291).

Figure D.2: Heterogeneous responses to others' donations



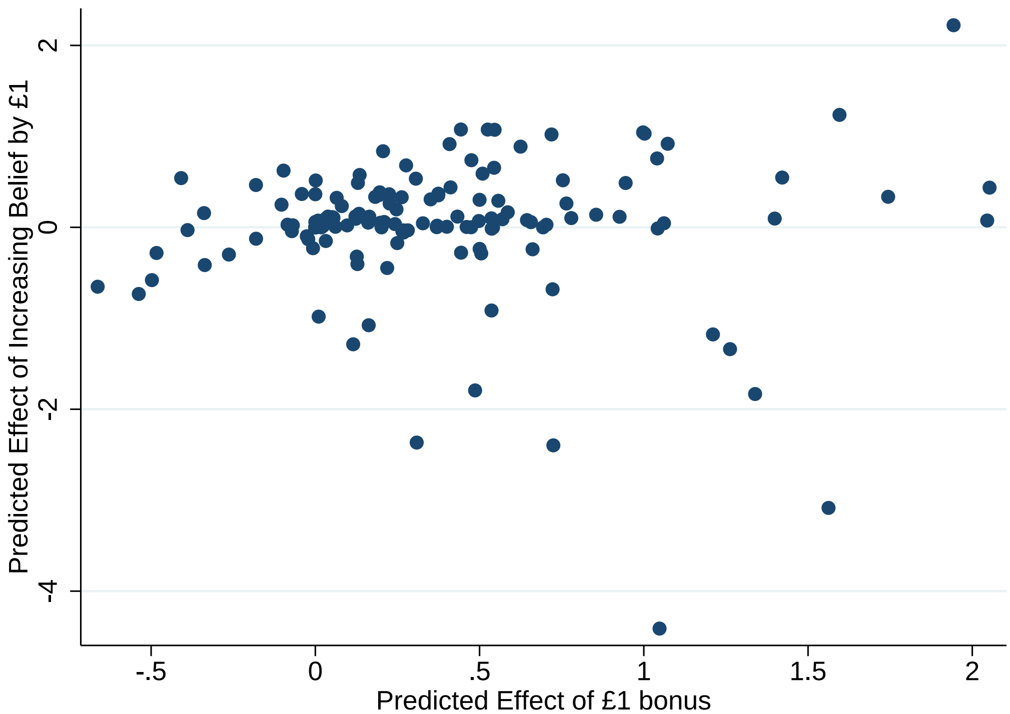
Notes: Distributions of coefficients from subject-specific regressions of conditional donations on minimum value of range of donations by labmates (Panel A) or an anonymous donor (Panel B).

Figure D.3: 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. Own donation when receiving £2 is the difference between baseline donations and the donations when receiving £2 bonus. Corresponding scenarios both before and after signals of past giving are used. Similar values of own donation are pooled in the same £0.25 bin. Larger marker means more observations in the bin.

Figure D.4: Heterogeneity of estimated choices



Notes: Subject-level predictions from estimates of the model of preferences in Section 4. N=144.

Table D.1: 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.

Table D.2: Similarity of main results across subject responses to learning of experiment

	(1) All	(2) Non-responders	(3) Positive-responders	(4) Negative-responders	(5) P-value of equality (columns 2-4)
Effect of bonus	.3668***	.3606***	.3576***	.3872***	.947
	(.0325)	(.043)	(.0571)	(.0785)	
N	[664]	[348]	[144]	[172]	
Effect of earnings	.0411	-.0453	-.008	.2203	.1933
	(.0618)	(.0926)	(.0274)	(.1279)	
N	[166]	[87]	[36]	[43]	
Effect of anonymous donor	-.0235	.0123	-.0026	-.1135	.4836
	(.0378)	(.0492)	(.0606)	(.0947)	
N	[332]	[174]	[72]	[86]	
Slope of labmate response	.5261***	.4428***	.727***	.5263***	.0259
	(.0459)	(.0617)	(.0867)	(.0992)	
N	[664]	[348]	[144]	[172]	
Effect of high signal	.5547***	.7054**	.298	.4143	.532
	(.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.

Table D.3: Scenario-2 donation response to earned income

	(1) Donation OLS	(2) Donation IV	(3) Any donation IV Probit	(4) Log donation IV	(5) Donation IV Tobit
Earnings from Tasks	-0.0113	-0.0382	0.0145	-0.0291	-0.0176
	(0.0203)	(0.0613)	(0.0181)	(0.0428)	(0.0875)
Constant	1.0649***	1.3339**	0.4974**	0.2438	0.6271
	(0.2503)	(0.6650)	(0.2103)	(0.4280)	(0.8989)
N	166	166	166	108	166
First stage F				26.36	

Notes: The table shows 2SLS results of the impact of earnings on donations in Scenario 2. The first stage of the IV regressions is capture in Panel (a) of Table 2, with the exception of the particular sample with positive donations in column (4), for which the relevant first-stage F statistic is included. *** 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 D.4: Donor types and individual characteristics

Results

	(1) Labmates responses	(2)	(3) Effect of bonus	(4) 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

Variable Definitions

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)

Notes: The table shows the correlation between donation responses and individual characteristics. The top panel reveals the relevant characteristics predicted by lasso method, and the bottom panel gives the list of individual characteristics that are included in the estimation.

Table D.5: 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: This table shows the donations from bonus income for the full sample (column 1), and separately for subjects who respond to their labmates' choices (column 2) and those who do not (column 3). *** 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.

Appendix E: Between-subjects Anonymous-donor Experiment

In this section, we present the results from additional sessions we conducted in a separate series of experiments in order to test why the anonymous donor was found to have an insignificant effect on donations, as described in section 5.3.1. In these additional sessions, we first had subjects perform a real-effort task (Part 1) allowing them to earn some income from the experiment. Following this (in Part 2), subjects are provided with an unannounced opportunity to make a donation, identical to the design of the main experiment we presented in the paper (section 3.2). For the donation opportunity, subjects could be randomly allocated to one out of two donation scenarios, to which we refer as "Without Donor X" and "With Donor X". In the former scenario, subjects simply had to indicate how much money (out of their total earnings from Part 1 of the experiment) they would like to donate to a local charity (which was the same as in the main experiment); while in the latter scenario, subjects were informed that an anonymous donor will add £0.45 before they were asked to make a donation decision.

Subjects were assigned one of only two scenarios, allowing us to test for between-subjects differences in donation behavior in the presence of an anonymous donor. We thereby rule out confounding factors that could have influenced donations in the main experiment, such as subjects being tired or having misunderstood the scenarios due to the presence of multiple scenarios or the fact that only one of the scenarios was actually paid in each session.

In addition, we test whether the lack of significant effects in the anonymous donor scenarios is an artefact of the earnings context in that subjects generated income as in Niederle and Vesterlund (2010) rather than the tasks explained in section 3.1. In sum, subjects participated in three different rounds that differed with regards to how they are paid in each round. In all three rounds, subjects were randomly paired with another participant and had to perform a math task (solving additions of four two-digit numbers) in a fixed period of three minutes. Specifically, subjects in Round 1 of the experiment are paid piece rate, while in Round 2, they participate in a tournament, where the subject with the highest number of correct additions from the pair receives everything and the other subjects receives nothing. In Round 3, subjects are asked to select which of the two compensation schemes (piece rate vs. tournament) they prefer and subsequently, they perform the addition task based on their preferred payment scheme.

In various sessions of this experiment, we randomly allocate subjects to three separate between-subjects conditions in which subjects (before the beginning of Round 3) are asked to solve a word puzzle containing neutral or competitive word primes. In some of the sessions, we also included a control treatment whereby subjects did not have to solve any word puzzle (no priming). In the following analysis, the dependent variable is the amount donated to the charity, and the independent variable consists of a dummy which equals 1 for the "With Donor X" scenario and 0 otherwise. We present four OLS regression models: in Model (1) we look at differences in donation behavior in the no priming (control) treatment, whereas in Models (2) and (3) we look at donation differences in the treatments with neutral and competitive word primes, respectively.

Our regression results from these models confirm our earlier findings that the anonymous donor has a statistically insignificant effect, regardless of whether we analyze behavior in treatments with or without primes. The same conclusion is drawn if we pool all data considering donation behavior in all three treatments as shown in the last column of the regression table (Model 4).

Table E.1: Individual characteristics included in the analysis and variable description

	(1) Full sample	(2) No priming	(3) Neutral priming	(4) Competitive priming
A. Control group mean	0.51	0.62	0.52	0.37
B. OLS estimates				
Pooled reference point treatments	-0.083	-0.118	-0.088	-0.029
	(0.082)	(0.153)	(0.153)	(0.102)
Observations	498	184	165	149