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Substitution Between (and Motivations for) Charitable Contributions: An Experimental Study

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Abstract

I run a series of laboratory experiments to estimate and describe the extent to which an individual's charitable donation to one cause displaces his or her giving to another cause. The experiments also investigate motivations for giving, including the simple warm-glow and public-goods models, and the desire for influence, reputation, and normalizing behavior. In the first wave of experiments I allow 49 subjects to donate or keep any amount of their $10 "endowment" to any of three charities in each of six stages (with one stage randomly chosen for payments). Some of the stages include "shocks" (to certain charities), such as an expanded choice set, a higher "match" rate, and a promotional video. The second wave of experiments (48 subjects) has 13 stages, a larger set of treatments, and a $20 "endowment". All of the treatments have the hypothesized effect on giving, including the "price" shock; in contrast to previous experiments, the subjects exhibit price-elastic demand. Subjects also give significantly more when their decisions and identity are observed. The results demonstrate that "expenditure substitution" among charities can be seen in a laboratory setting. I find large own-price elasticities and very large cross-price elasticities – these charities are gross substitutes in the conventional sense. The substitution is stronger where the charities serve similar purposes, such as UNICEF and Care. In the "reduced form" model, using instrumental variables estimation, I estimate an expenditure substitution coefficient of 37% – when gifts to one charity are increased (or decreased) by a certain amount because of a shock, the sum of gifts to other charities decreases by 37% of this amount. The conditional-on-positive estimate of this substitution is 80%: where gifts to both charities remain positive, the "crowding out" is $0.80 for every $1.

1 Introduction and Motivation

In modeling philanthropic giving, economists have typically focused on two major issues: the "crowding out" of government grants and the impact of different tax regimes on overall giving. Little attention has been given to the extent to which an individual's contribution to one cause comes at the expense of her other philanthropy. This issue has come to the attention of policy-makers and journalists in the wake of the September 11, 2001 terrorist attacks,[1] and again after the 2004 Indian Ocean tsunami[2] and 2005's Hurricane Katrina[3] – in each case there was a concern that the flood of donations to the well-publicized cause would dampen giving to other charities. However, some have dismissed this concern, claiming that "donor fatigue" is a myth.[4].

The present paper reports experiments designed to elicit "homegrown values" surrounding charitable contributions. More specifically, I measure whether and to what extent an individual's contribution to one cause comes at the expense of his or her other philanthropy. I first present this substitution in terms of cross-derivatives and cross-elasticities, in response to price changes and other "specific" shocks, and discuss the ratios of these responses. I next use an instrumental variables approach to directly estimate "expenditure substitution" – the change in gift to one charity in response to a specific shock to another charity. The sign, magnitude, and nature of this effect is of great popular interest and is crucial to tax and social spending policy, nonprofit management, and individual ethical decision-making (see Reinstein, 2007). It is also important to the economic literature for at least two reasons. First, it complicates the estimation (and the normative implications) of the price-elasticity of giving and the crowding out of government spending. Second, knowing whether expenditure substitution occurs, whether it is "complete," and how it depends on the charities compared can offer evidence against or in support of several proposed economic models of giving, including the "public goods" (Becker, 1974), "warm-glow" (Andreoni, 1989), "tithing," and "Kantian" (Sugden, 1982) models[5].

Because it is difficult to observe exogenous shocks that alter giving to one charity without any independent effect on giving to other charities, observational ("happenstance") econometric evidence is vulnerable to the criticism that what is being observed is not the true ("expenditure") substitution effect. Most plausible shocks that cause more giving to one charity will cause more giving to the other charity, masking substitution effects. While this is less of a problem with an approach that exploits "specific" shocks (such as a college reunion year as a shifter of educational giving), such regressions have their own weaknesses, including high standard errors and the difficulty (or impossibility) of demonstrating that the required exclusion restrictions hold. In contrast, experiments can offer evidence for substitution patterns that are not vulnerable to endogeneity. In experiments we can also manipulate the price of giving to one charity independently of the price of other gifts. However,the laboratory setting raises issues of external validity.

While there has been some laboratory and field experimentation involving charitable giving in recent years (e.g., Shang and Croson, 2005; Falk, 2004; Frey and Meier, 2004), I found none that allowed gifts to multiple charities, and none that examined substitution

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1 <*http://www.sptimes.com/2002/09/04/911/Sept_11_donations_swa.shtml*>

2 <*http://www.cnn.com/2005/WORLD/africa/07/30/africa.hungry.ap/index.html*>

3 "Katrina Giving Cuts Donations To Other Groups; As Relief Contributions Pour In, Unrelated Charities Retool Plans To Get Back on Donors' Minds" – *The Wall Street Journal*, September 20, 2005.

In response, the government increased the maximum allowable tax deduction for charitable giving to 100 percent of income on donations made during the last part of 2005. – "Katrina Emergency Tax Relief Act...," by Candace Clark, UNC-Chapel Hill. <*http://www.johnbrownlimited.com/newsletter/1005/index.cfm*>

4 "Many Dismissing 'Donor Fatigue' as Myth"– *New York Times,* April 30, 2006

5 See Reinstein, 2007 Reinstein (2007a) for a further discussion of these models and their predictions.

patterns. As in Eckel and Grossman (2003) (henceforth EG), my experiment is essentially a "modified dictator game," in which "subjects make a series of allocation decisions dividing an endowment between themselves and their chosen charities." To estimate the conditional demand response to a "preallocation" (i.e., exogenously determined consumption of one good), the ideal experiment would exogenously shift the amount allocated to one good, and observe the resulting reallocation of the other choices. However, the consumption value of charitable giving, including benefits such as "warm glow" and self-signaling (e.g., Benabou and Tirole, 2003) may depend on the gift being voluntary. Thus, I cannot meaningfully implement such exogenous shifts. Instead, in a laboratory setting, I generate "shocks" that are characterized as specifically affecting one type of giving (e.g., a video promoting one charity) and then observe giving to this charity and to other charities. This allows estimation that is immune to the potential endogeneity and omitted variable bias of the observational analysis. On the other hand, experimental evidence is often criticized as lacking external validity; thus the two types of evidence are complementary. I use three types of treatment: a limited/expanded choice set, varying prices (match rates), and an informational/emotional appeal.[6] In using these indirect methods – generating a shock and observing the resulting substitution – I simultaneously test both the effects of these shocks and the substitution patterns, measuring both the direct impact on the shocked charity the indirect impact on the non-shocked charities (presumably via substitution)

I ran two distinct "waves" of experiments, each wave containing three sessions, and using a total of 97 subjects, mainly U.C. Berkeley undergraduates from a variety of departments, but also including some staff and alumni. The experiment lasted between (roughly) 30 minutes and one hour for each subject. In the first [second] wave there are six [13] decision-making stages for each subject, and the outcome of each stage (a payment to the subject and/or the charities) is "realized" with 1/6 [1/13] probability. In what I will refer to as the "baseline" stage a subject can donate up to $10 [$20] to one or more of three charities, and are informed that the experimenters will add an additional 20% "match" to each of their gifts. The subjects can keep what they do not donate. The charities, "CARE-USA" (henceforth "CARE" or "CR"), "Medical Research Charities," (henceforth "MRC") and "Scholarship America," (henceforth "SA") were chosen to mimic the categories "basic needs," "health", and "educational" (respectively) in the PSID data.

In addition to this "baseline stage" I offer treatments in various stages/waves including restricting the choice set to one charity, augmenting the choice set to include UNICEF or The Nature Conservancy (TNC), increasing the match rate to 50% for one charity, and showing a promotional video for CARE. Nearly all of these shocks had the hypothesized effect on gifts, including the "price" shock: in contrast to previous experiments, the subjects exhibit (locally) price-elastic demand.

While the experiment reveals much heterogeneity in subject's giving behavior, my results strongly suggest that these charities are substitutes. When a shock causes subjects

[6] The first two of these treatments in effect limit the budget or choice set, as in revealed preference analysis. Stages 9-13 of my 'second wave' experiments also involve 'social' treatments (observation of other subject's gifts); I analyze these in a separate paper.

to increase their giving to one charity they are far more likely to decrease their giving to the other (unshocked) charities than to increase this giving. In structural regressions I consistently find a large own-price elasticity, and large positive and significant cross-price effects. In direct 2sls regressions of giving to one charity on giving to another, where the latter is instrumented by the shocks mentioned above, I find "crowding out" at a 37% rate; in the Tobit version the conditional-on-positive effect is 80%. The crowding out (cross-price effect) is even stronger for the two charities that have the most similar goals: UNICEF and Care. This evidence complements the observational results of Reinstein (2007a) .

In section 2 I survey previous experiments examining charitable giving, both in a laboratory context and in natural settings. I also give a synopsis of the extensive literature on "public goods" and "dictator" experiments. Section 3 discusses a simple model of giving and the object of my estimation. In section 4 I describe my experiment and my procedures. Section 5 presents the main results, both on the substitution patterns and on "first-stage" effects, presenting results from a standard structural regression and from "reduced form" regressions. In section 6 I discuss internal and external validity, testing for robustness against alternative hypotheses. I conclude in section 7 by summarizing and interpreting my results and offering suggestions for future work.

2 Literature Review

EG (1996;1998;2003;2006) conduct several laboratory studies involving actual charitable giving. I will refer to such experiments (including my own) as "out-group" since the direct benefits of subjects' contributions will go to non-participants. Their 1996 paper extends the literature on double-anonymous dictator experiments to argue (in contrast to Hoffman, Elizabeth, McCabe, Kevin, and Smith, Vernon L., 1996) that altruism motivates behavior. They compare donations when the "counterpart" is the typical anonymous student subject to when the "counterpart" is an established charity (the American Red Cross), noting a higher rate of contribution in the latter case. They argue "fairness and altruism require context," and that the previous double-anonymous dictator experiments "removed virtually all motivation for donating..." as decision makers do not have "enough information to know if their partner is poor or otherwise deserving." In contrast, EG point out that in the real world "most donors, even those who wish to remain anonymous, know their recipient, or at least the general characteristic of their recipient, thus opening the door for altruism to play a role."[7] EG's 2004 work focuses on whether the "framing" of a charitable subsidy – as either a matched contribution or a rebate – determines its impact on giving. Their subjects make 12 allocation decisions (one of which is randomly chosen for payment), involving different endowments, different net prices, and with the (equivalent) subsidy expressed in two different ways. They estimate equation 1 (below) "using random effects, Tobit maximum likelihood to account for the panel nature of the data ... and for the censoring of the subjects choices from below and above (i.e. $ln(0.1) < ln$(CONTRIBUTIONS) $< ln$(maximum possible

[7] Such in-group experiments are further muddled (e.g., in Andreoni and Miller's (2002) search for "rational jealousy") by the fact that the subject may have no reason to prefer giving to another subject over leaving the money for the researchers.

CONTRIBUTIONS)."[8] In contrast to the predictions of classical economics, they find "in every comparable case... the dollar value of the donation is significantly greater under the matching subsidy than under the rebate subsidy." They also "report evidence consistent with earlier findings that women are more altruistic than men," and find income and price effects that have signs consistent with traditional economic theory.

$$ln(contributions)_{ij} = a_0 + a_1 ln(endowment)_{ij} + a_2 ln(price)_{ij} + a_3 X_i + \varepsilon_{ij} \quad (1)$$

One important finding that their paper overlooks (by focusing on the amounts the charity *receives* including the subsidy rather than the amounts donated) is that the amount donated, the "out of pocket expenditure" does not increase as the subsidy increases (and the price decreases). This suggests that the subject either chooses what he or she feels is an optimal amount of the public good (perhaps in a Kantian model; see Reinstein 2007) or completely internalizes the subsidy as part of the subject's own "warm glow." [9] EG estimate a price elasticity for match subsidies that is not significantly different from (but slightly below) one, when all controls are included. Thus, in the terminology of Andreoni (2006) , elasticity is below the "gold standard," and thus subsidizing giving is ineffective as a policy tool, as well ineffective as a tool for "shocking" giving (in terms of the amount an individual gives up) in a laboratory setting.

Several papers also use field experiments to examine charitable giving in a natural setting with quasi-experimental mechanisms that affect contributions. Frey and Meier (2004) test the effect of a matching mechanism on donations in a controlled field experiment. Analyzing the donations of students at the University of Zurich to two funds which benefit other students, they find "whereas a 25% increase of a donation does not increase the willingness to contribute, a 50% increase does have an effect." Carman, 2003 uses proprietary records from a large national bank with a workplace giving campaign and finds that social influences, particularly among salary and gender groups, play an important role in determining charitable contributions. Shang and Croson (2005) survey callers to a public radio station's fund drive, asking these potential donors various questions about their social network that listens to this station, and measures the impact of this question (and of the size of the network), on the resulting gift. Other experimental or quasi-experimental research examines the crowding out effects of public funding (Bolton and Katok, 1998), the prestige motivation for giving (Harbaugh, 1998), and fund-raising techniques and psychology/behavior (Weyant, 1996; List and Lucking-Reiley, 2002; Falk, 2004).[10]

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[8] They can assume random individual-fixed effects because their variables of interest (prices, endowments, and the framing of subsidies) are chosen randomly and thus should be orthogonal to any individual-specific components. In my estimation, this assumption is not appropriate, since the variables of interest (regressors) are a subject's gifts to the other charities in the experiment in a stage – thus I employ fixed-effects and IV techniques, discussed later in the paper. My technique also has the advantage that within variation generally is less than between variation, leading to more efficient estimation.

[9] See, for example, EG, 2003, table 4

[10] Richard Katzev (1995) surveys the experimental research into charitable giving across disciplines, finding 40 studies, mostly from social psychology but also from marketing, communication science, economics, and sociology. Katzev categorizes these into two groups: "Those that examined specific situational factors that occur in the fundraising setting ... and those that investigated the personal characteristics of the individuals involved in the exchange..."

3 Model

In this section I offer a framework for understanding the "expenditure substitution" that I estimate, and it's relationship to price effects, "specific" shock effects, and observables. In doing so, I address the critique that my estimates are incoherent regressions of one endogenous variable on another.

> One big worry, that the massive Sept. 11 donations would hamper other charities, didn't prove true ... Americans have donated more than $1 billion to the [Katrina/ flooding] relief .... But the largess is starting to come at the expense of charities with other missions.

> The challenge is people like Betty and Larry Sullivan ... They save all the charity solicitations they receive each year until December, then sort through them and make their contributions – generally about $30,000, or 10% of their income. ... But already this year, moved by the plight of the victims, the couple has given $4,000 for the tsunami relief effort and some $35,000 to help Hurricane Katrina survivors, says Mrs. Sullivan. They've also been volunteering ... The effort is taxing their financial resources .... As for donations to other charities this year, "That's history," says Mrs. Sullivan... (**?**)

Economists frame demand (including for "purchasing" charitable gifts) as a simultaneous decision to purchase a bundle of goods and services to maximize a utility function subject to a budget constraint. In this framework, parameters that affect the utility function (e.g., good weather) and the budget constraint (income and prices) are said to impact *all* of the consumption choices – these exogenous parameters are not seen as specific to any good – and we estimate (e.g.) own and cross-price derivatives and elasticities, as I do in section 5.3 (also estimating effects of other shocks). To ask "how does consumption of A affect consumption of B?" is not meaningful: we cannot assert causality for such simultaneous decisions, and the ratios of changes in these choices will depend on what is causing the changes.[11] However, as seen in the quotes above, non-economists often pose this question, frequently make their decisions sequentially, and conceive of one purchase coming at the expense of another. The above story of the Sullivans suggests that their enthusiastic response to disaster relief efforts will have a definite negative impact on their usual contributions to other causes, and that they might not have considered this impact when deciding to give and volunteer for Katrina and tsunami relief

It is not clear that any such event has a direct impact only on gifts to one cause, and not other causes – in the standard economic framework this statement is not meaningful. A shock "α" can be specific in that it only changes the *marginal utility* of gifts to one cause, as in the equation below, but if decisions are simultaneous, the shock will affect all choices.

[11] However, even our technical economic terms, although defined in terms of cross-price elasticities, suggest a more direct causation: coffee "substitutes for" tea, while cream "complements" both beverages.

$$
\begin{aligned}
U &= f(x, g_1, h(g_2, \alpha)) \\
&\quad \text{e.g., } f(x, g_1, g_2 - \alpha)
\end{aligned}
\tag{2}
$$

Still, if a policy-maker wants to predict the impact of the shock α on gifts to "unshocked" charities g_1, knowing (or having an estimate of) the effect on g_2 may be useful in assessing the magnitude of the shock. A policy-maker or charity may know there was a tsunami and also observe measures of disaster giving in response, and may want to predict or infer the likely impact on another set of charities. Furthermore, under some plausible conditions the "substitution" response to any shock will be a predictable function of the "direct" impact of the shock (above, on g_2).

Assuming that utility is separable in own consumption and charitable gifts, and assume an additive specific shock (α):

$$U = f(x) + V(g_1, g_2 - \alpha) \tag{3}$$

And we have the budget constraint:

$$x + p_1 g_1 + p_2 g_2 \leq Y \tag{4}$$

...normalizing the price of own consumption to one.

Under this model, the marginal "indirect effect" of a shock ($\frac{\partial g_1}{\partial \alpha}$) can be expressed simply as a function of the marginal "direct effect" of the shock ($\frac{\partial g_2}{\partial \alpha}$). Standard comparative statics of the optimal choices (assuming an interior solution and other standard regularity conditions) yields the total derivatives:

$$\frac{dx}{d\alpha} = \frac{\lambda_\alpha}{U_{xx}} \tag{5}$$

$$\frac{dg_1}{d\alpha} = \frac{\lambda_\alpha (p_2 U_{12} - p_1 U_{22})}{U_{12}^2 - U_{11} U_{22}} \tag{6}$$

$$\frac{dg_2}{d\alpha} = 1 + \frac{\lambda_\alpha (p_2 U_{11} - p_1 U_{12})}{-U_{12}^2 + U_{11} U_{22}} \tag{7}$$

Where: $\lambda(\alpha, p_1, p_2)$ is the shadow value of the budget constraint, $U_{IJ} \equiv \frac{\partial^2 U}{\partial I \partial J}$, $\lambda_\alpha \equiv \frac{\partial \lambda(\alpha, p_1, p_2)}{\partial \alpha}$.

Hence:

$$\frac{dg_1}{d\alpha} = (\frac{dg_2}{d\alpha} - 1) \frac{p_2 U_{12} - p_1 U_{22}}{p_1 U_{12} - p_2 U_{11}} \tag{8}$$

The sign of the marginal effect on g_1 (relative to the marginal effect on g_1) can be either positive or negative, and will depend on the partial second derivatives of utility and the relative prices.

Looking at the discrete effect:

$$g_1(\alpha_1) - g_1(\alpha_0) = \int_{\alpha_0}^{\alpha_1} [(\frac{dg_2}{d\alpha}(g_1,g_2) - 1)\frac{p_2 U_{12}(g_1,g_2) - p_1 U_{22}(g_1,g_2)}{p_1 U_{12}(g_1,g_2) - p_2 U_{11}(g_1,g_2)}]d\alpha \quad (9)$$

With quadratic utility[12], the partial derivatives will be constants, and the *discrete* indirect effect, as well as the marginal effect, will be a simple linear function of the direct effect.

$$g_1(\alpha) = A + Bg_2(\alpha) \quad (10)$$

Quadratic utility is often justified as a second-order approximation to any other utility function. With other utility functions the partial derivatives may vary at different consumption bundles, so the indirect effect may be a nonlinear function of the direct effect, but these should be solvable (equation 9) for a predictable functional form, which for estimation purposes, can be approximated to any desired accuracy by a polynomial function. In section 5.4 I estimate the response of the "unshocked" charities to the shocks as a function of the response of the shocked charities.

4 Experimental Design and Procedures

4.1 Design: Wave One

Subjects make decisions in six stages[13], and the outcome of each stage (a payment to the subject and/or the charities) is realized with equal (1/6) probability. This is explained to subjects in detail, although the precise rules for each stage are not given until the beginning of that stage. This randomized choice of one stage for payment is standard in economic experiments, and was used in EG's (2003) charitable giving experiments, among many others. This setup permits controlled within-subject comparisons, without "wealth effects" of early-round earnings affecting late-round behavior (Friedman and Sunder, 1994). Subjects whose preferences meet standard economic assumptions will treat their decision in one stage as independent of their decision in another stage: they will simply select the optimal allocation for each stage, since only one will be realized. At the end of the experiment the subjects complete a survey (on their demographics, charitable giving behavior, and perceptions of the experiment), are "debriefed" on the purpose of the experiment, and are paid the amount determined by their choice in the randomly chosen stage, in addition to the initially promised "base payment" ($5 in the first two sessions of the experiment and $8 in the third session).

[12] $U(x_0, x_1, ..., x_n) = x_0 + \Sigma_{i=1}^n \alpha_i x_i - (\Sigma_{i=1}^n \beta_i x_i^2 + 2\Sigma_{i \neq j}^n \gamma_{ij} x_i x_j)/2$
With α_i , $\beta_i, \gamma_{ij} > 0 \; \forall \; i, j$
i.e.,$U(Q) = \alpha' Q - \frac{1}{2} Q' \boldsymbol{\beta} Q$

See, e.g., Andreoni and Gale, 1996

[13] A formal detailed description of both waves of experiments is given in the appendix.

1. **Table 1: Experiment Design** – Wave 1: Endowment $10

Stage	1	2	3	4	5	6
Choice set	1 charity per stage, stratified			all 3 charities		
Match Rate	Base 20%			Base 20%	50% for CARE-USA 20% for others	Base 20 %
Other Treatments						Promotional Video CARE-USA

In each of the stages, subjects are given an allocation of $10 and the opportunity to donate (out of their $10 "allocation") to one or more of three charities. I attempted to mimic the charity categories in the PSID data, so that the results can be compared. I chose the charity "CARE-USA" (international development and relief) to represent "basic needs," "Medical Research Charities" (a large pool of research institutions) to represent "health" and "Scholarship America" (college scholarships to underprivileged youths) to represent "education." These donations, not to exceed the "pay," are matched at (at least) a baseline (20%) rate in all cases, to induce subjects to give within the lab rather than wait to decide later. In addition, in later stages (4-6) I introduce various treatments designed to spur one of the three causes. To measure underlying individual variation between stages (e.g., fatigue or learning), I leave some "control" observations – repeating certain stages for certain subjects. The treatments are administered in random order where practical (noting, e.g., that to be isolated "news" must be in the last stage).

The "baseline" choice is given in Stage 4 – a subject can donate up to $10 to one or more of three charities, and are informed that the experimenters will add an additional 20% "match" to each of their gifts. There are three basic kinds of treatments: 1. A limited/expanded choice set: in stages 1-3 participants could only give to one charity per stage, while in stages 4-6 they could allocate among all three charities (and their own consumption) in each stage 2. Price variation: In Stage 5, most subjects were given an increased (50%) [14] matching rate for gifts to the charity CARE, and the usual 20% matching rate for gifts to the other charities 3. Information/Propaganda ("video shock"): In Stage 6 most subjects were required to watch a CARE promotional video. The video is both informative and persuasive. In the terminology of Friedman and Casar (2004) I employ a "within-subjects" design, but with some between-subject variation. While the within subjects design reduces variance and "controls for subjects personal idiosyncrasies," the between-subject variation (control subjects) and random ordering (for stages 1-3) allow me to control for time and learning effects.

[14] The use of a 50% match is motivated by the findings of Meier and Frey (2004), as I discuss in section 2.

2. **Table 2: Experiment Design** – Wave 2: Endowment \$20 – First 10 treatments

Stage	1	2	3	4
Choice set	3 charities			
Match Rate	Base 20%	Base 20% for 2, 50% for 1 charity stratified		

Stage	5	6	7	8
Choice set		4 charities TNC and UNICEF stratified		4 charities UNICEF
Match Rate	Base 20%			

3. **Table 3: Wave 2, Social Treatments***

Stage	9	10	11	12	13**
Social Treatment	Decision anonymously reported to randomly-selected participant.	Standard	Decision reported to subject on your left	Standard	Decision reported to matched subject 2 to left/right

**Standard choice set and match rate:* 3 charities (CARE, SA, MRC) with base match rate of 20%. Endowment \$20.

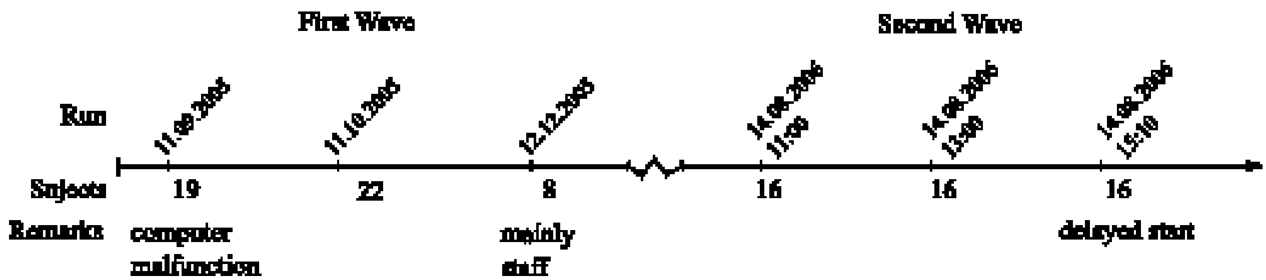
**Known to be the final stage

4.2 Design: Wave two

Wave two resembles the first wave in many ways. Subjects make donation decisions in a series of stages, one of which is randomly chosen to be realized. As in session one, subjects receive an automatic base payment (\$5) and are given the same endowment in each stage to divide between themselves, and a (varying) set of charities. Other than involving larger endowment, the "baseline" choice, offered in stages one and five, is the same as in wave one, involving the same three "main" charities.

However, there are several important differences. The wave two sessions are longer, involving 13 stages (plus the survey), and the endowment is doubled (now \$20) to make each decision roughly as salient as in wave one. Stages 2-4 offer a "price shock," a higher (again 50%) match rate, but now offered alternately (with the ordering varied) to each of the three main charities. Stages 6, and 7 expand the choice set to a total of four charities, alternately (ordering varied) adding UNICEF and The Nature Conservancy. Stage 8 again involves the three 'main' charities and UNICEF, but offers a higher (50%) match rate for gifts to UNICEF. Stages 9-13 involve various 'social' treatments (observation of other subject's gifts)[15]

[15] To keep the present paper coherent and concise, I focus on the first eight stages, mentioning the results from the latter stages only briefly, and saving the in-depth analysis of social effects for future work.



1. **Figure 1: Timeline of experiments**

4.3 Procedures

My experiment was run using the Experimental Social Science Laboratory (X-Lab) at the University of California, Berkeley under the X-Lab Master Human Subjects Protocol,[16] and approved by the Committee for Protection of Human Subjects. Subjects were recruited by the X-Lab (those interested are put on an email list and informed about opportunities to participate) from a wide pool of students and staff at the University of California, Berkeley. None of the subjects were allowed to participate in my experiment more than once, although many had taken part in X-Lab experiments run by other researchers. The experiment involved 97 subjects in total, recruited by the X-Lab through emails to students and staff at U.C. Berkeley. The subjects were mainly U.C. Berkeley undergraduates from a variety of departments, but included some staff and alumni. Emails were sent out to subjects who voluntarily signed up into a "permanent" database of potential subjects. In the emails, subjects were promised an average payment of $15.[17] In the third session we made an effort to recruit staff members, offering a larger ($8) base payment, while in the second wave (sessions 4-6) the base payments were again $5.

I have completed six sessions of the experiment[18], three runs of the first wave and three runs of the second. Each session took roughly one hour, plus about 15 minutes to record payments and print and distribute checks. I show "screenshots" of selected stages in the appendix. A complete set of such images, as well as the software code, and the complete integrated data set of experimental results (including decisions, survey responses, and response times) are available by request. The video used for the wave one, stage 6 treatment can be viewed at: *<http://www.care.org/videos/picture_a_world.mov.>* The first wave of experiments used the software Medialab-v4[19], and the second wave was programmed and

16 *<http://xlab.berkeley.edu/cphs/master_protocol.pdf.>*

17 The text of the email reads: ..."If you are a U.C. Berkeley student or staff member, the X-Lab is conducting a research experiment and would like your participation. For approximately one hour, with an additional half hour of administrative time to prepare payments, you will earn an average of $15."

18 Because of a computer failure, the first session had to be conducted via oral instruction and answers written with paper and pen, no post-experiment survey data were collected, and assignment of a stage for payment was done by generating 20 random numbers, one for each subject. The experiment was otherwise identical to that of the other sessions. The first run, although conducted under a slightly different apparent conditions ("frames"), does not have significantly different overall levels of giving in the first 3 stages than the second or third run.

19 Empirisoft, www.empirisoft.com.

conducted with the software Z-Tree (Fischbacher, 2007). After making the choices the subjects answered a series of survey questions, and were debriefed on the purpose of the experiment. One of the stages was randomly selected by the computer[20] for each subject, and the amounts the subject chose to contribute and keep in that stage were recorded. After all subjects were complete, the checks to subjects were printed out and distributed with roughly a 10-15 minute delay. The subjects were paid the amount they chose to keep in the randomly chosen stage, plus the initially promised "base payment" – $5 in sessions 1,2,4,5, and 6 and $8 in the third session. Within one week of each session, the X-Lab wrote a single check to each of the charities equal to the amounts promised.

5 Experimental Results

5.1 Aggregate giving patterns[21]

On average (averaged over subjects as well as stages; see table 4) subjects chose to give away roughly $3 out of the $10 in the 1st wave and $4 out of the $20 in the second wave, with 20–35% (depending on wave and run) keeping the entire endowment in a typical stage. These responses resemble those of other studies. For example, EG report a 31% rate of giving in their 1996 study, with 27.1% of the subjects keeping the full $10, and a (roughly) 50% rate of contribution in their 2003 study.

The six sessions all show evidence of substitution (see figures 2 and 3). The strongest effect is seen in the fourth stage of the first wave – when the subjects can allocate up to $10 (matched at the 20% rate) between the three charities, average giving to each charity is roughly half as high as in stages where subjects can give to only one charity. This difference is strongly statistically significant in all cases, as table 27 shows. This suggests a large crowding-out: total giving increases by only $0.88 on average when all three charities are in the choice set (total giving would need to increase by over $4 for no crowding-out). In contrast, for the second wave, we see little or no crowding out when a fourth charity (UN or TNC) is introduced to the chance set in the fifth and sixth treatment stages. However, Simonson, Itamar and Tversky, Amos (1992) claim that "the attractiveness of an option is enhanced if it is an intermediate option in the choice set." This suggests that subjects may have bias towards making an equal division between the options available, and thus apparent substitution from the choice shock in the first wave may be an artifice.

In the final three stages of the first wave both types of shocks to CARE– the higher match rate (50%) and the four minute promotional video – had similar positive effects, increasing average giving to the "shocked" charity from $1.29 to $2.23 and $2.33, respectively. Both the (wave one) price and video treatments are strongly statistically significant in tests for differences in the true population mean (table 27 in the appendix). These positive effects are in spite of an apparent downward trend in giving over time. For the eight control

[20] In the first run, because of computer problems, I used a list of randomly generated numbers, one for each subject's ID.

[21] Descriptions of these and all other variables are given in table 13 in the appendix.

4. **Table 4: Summary statistics of gifts**

First Wave					**Second Wave**	
Variable	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**
sa_	0.81	1.17	195	1.02	1.77	624
Gsa_	0.49	0.5	195	0.4	0.49	624
mrc_	0.69	1.02	195	1.16	1.7	624
Gmrc_	0.46	0.5	195	0.47	0.5	624
cr_	1.8	2.18	195	1.4	2.19	624
Gcr_	0.65	0.48	195	0.46	0.5	624
un_				2.19	2.23	96
Gun_				0.72	0.45	96
tnc_				1.31	2.5	48
Gtnc_				0.52	0.5	48
tot_	2.9	2.71	294	4.02	4.41	624
Gtot_	0.74	0.44	294	0.65	0.48	624
gave_ever	0.88	0.33	49	0.9	0.31	48
gave_ever_cr	0.86	0.35	49	0.75	0.44	48
gave_ever_mrc	0.65	0.48	49	0.77	0.42	48
gave_ever_sa	0.69	0.47	49	0.75	0.44	48

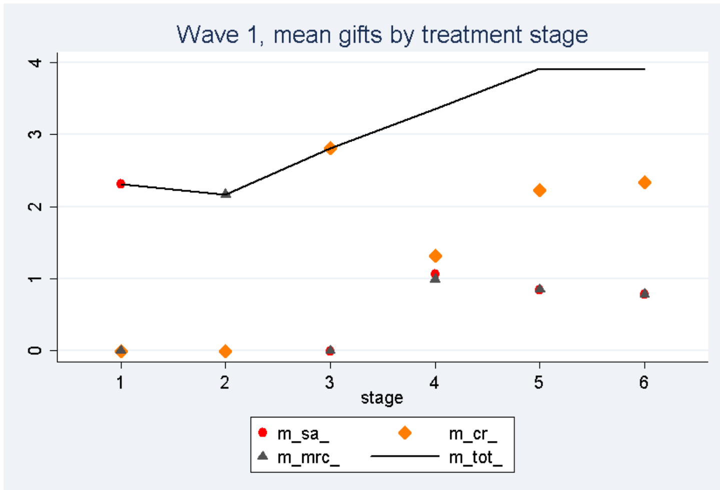
VARIABLE NAME	DESCRIPTION
X_	Gift to charity X[1] in stage[2]
GX_	Dummy: Gave to X[1] in stage[2]
tot_	Sum of gifts in stage
Gtot_	Gave to some charity in stage
gave_ever	Dummy: Subject gave in some stage
Gave_ever_X	Dummy: Subject gave to X[1] in some stage

1. Where $X \in \{CR, SA, MRC, UN, TNC\}$
2. Universe: Stages where charity X in choice set

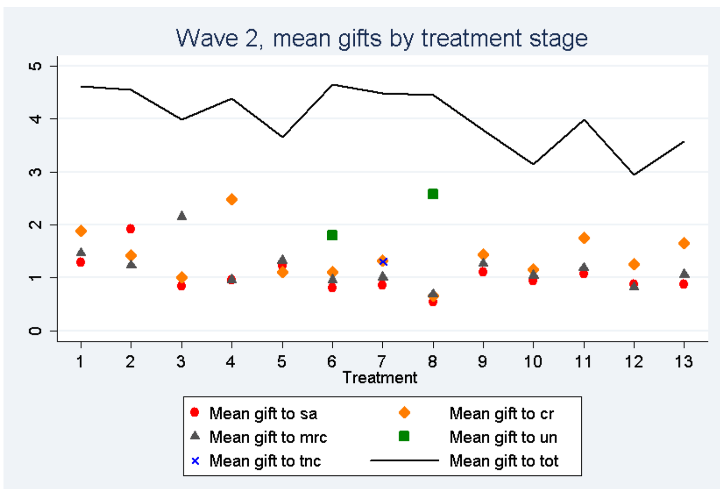
observations in the first wave, (repeated "baseline" stages) there was, on average, a *decrease* in giving to CARE of $0.44. Although not as dramatic as in the fourth stage, there is a statistically significant decrease in mean giving to MRC plus SA in the treated fifth and sixth stages: a drop of roughly 40 cents after either the price or movie shock.

Similar price effects are seen in the second wave. When the match is increased for a charity (treatment stages 2,3,4, and 8), the average gift to this charity increases, and gifts to the other charities decline. There is again an overall negative trend in giving over time – this can be seen by comparing total giving in the baseline stages 1, 5, 9, 10, and 12.[22]

[22] The 10th and 12th stages are arguably not "baseline" as their may be an influence of partners' previous gifts, although I do not find any such influence effects.



2. **Figure 2: Aggregate giving patterns, Wave 1**



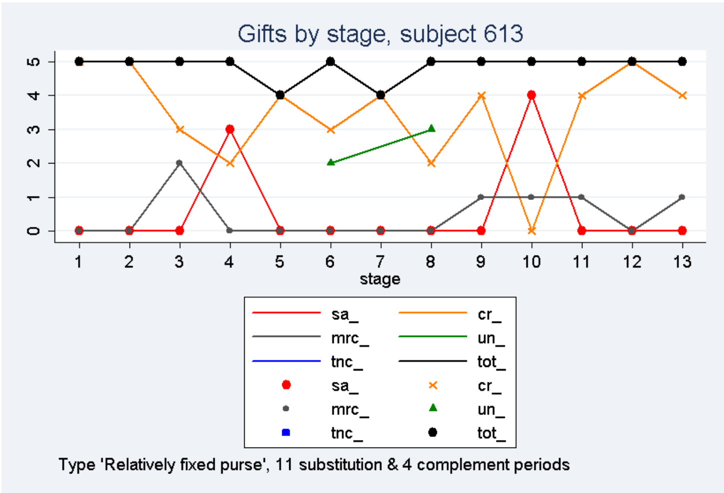
3. **Figure 3: Aggregate giving patterns, Wave 2**

5.2 Individual-level Analysis: Substitution

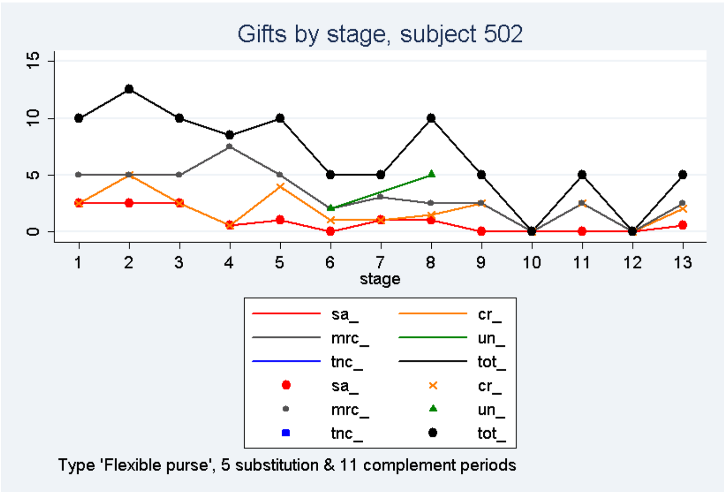
Subjects vary in their response to these shocks, and in their substitution. Table 5 broadly outlines several important patterns of behavior, and a significant fraction of subjects fall into each category (the categories are not necessarily exclusive). 27% of subjects give infrequently (less than one quarter of the time). Among those who give more than 25% of the time, I divide those who show a relatively "fixed purse" from those who show a "flexible purse," the latter exhibiting more than a 25% standard deviation from period to period in their giving. I define a "substitution period" as one in which the subject gave more to one charity and less to another than in the previous period. A period is a "complement period" if the subject changed their gift to multiple charities in the same direction relative to the previous stage, but did not change any gifts in the opposite direction. 8% of my sample gave frequently, to multiple charities, but had no substitution periods. Figures 4 - 7 display the decisions of four such caricatured "types."

5. **Table 5: Types of behavior, 2nd wave**

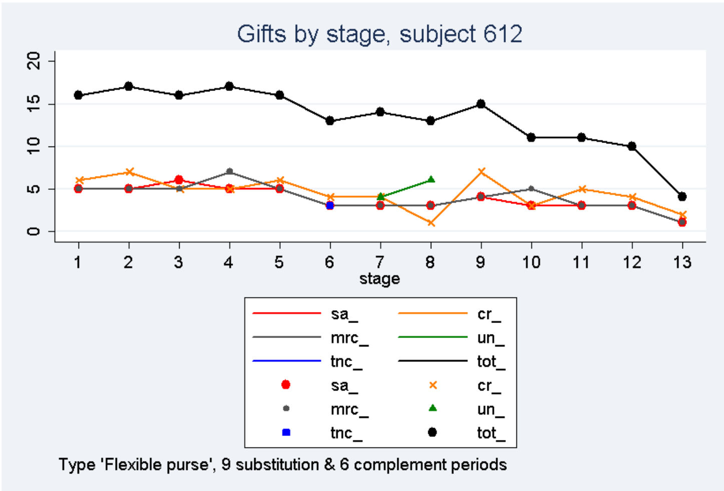
Variable	Mean	Std. Dev.
Relatively fixed purse (s.d.$<$ 25%), frequent giver	0.33	0.48
Flexible purse, multiple charities, frequent giver	0.31	0.47
Gave $<$ 25% of the time	0.27	0.45
No substitutions, multiple charities, frequent giver	0.13	0.33
Only gave to 1 cause	0.06	0.24
Number of substitution periods	3.5	3.48
Number of complement periods	2.71	2.05
N	48	



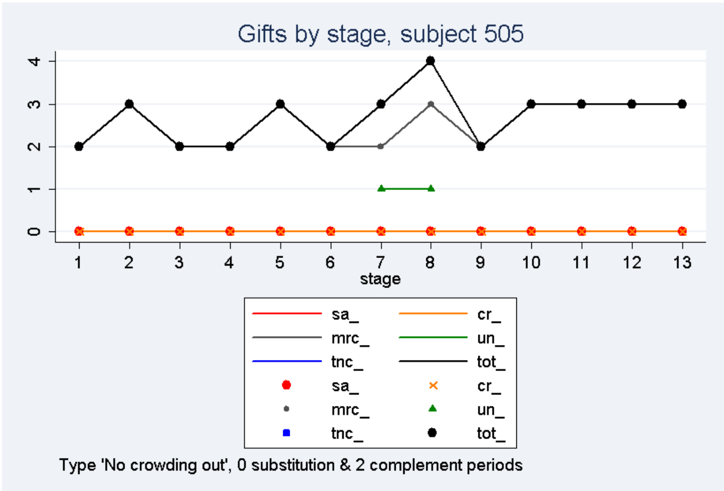
4. **Figure 4: Giving by subject 613 over "stage"**



5. **Figure 5: Giving by subject 502 over "stage"**



6. **Figure 5: Giving by subject 612 over "stage"**



7. **Figure 7: Giving by subject 505 over "stage"**

6. **Table 6: Directional changes in giving**

	Decrease	No change	Increase	Total
Decrease	7	22	126	155
No change	13	117	50	180
Increase	9	12	21	42
Total	29	151	197	377

The qualitative (directional) evidence at the individual level yields a strong result. I define a "positive treatment" to a charity as a higher match rate for that charity (for SA, MRC,CARE, or UN), a movie having been shown promoting that charity (for CARE), or simply the charity becoming part of the choice set (for UN and TNC). When my experimental subjects increase their giving to one charity in periods where this charity (and only this charity) is given to a positive treatment they generally decrease their total giving to the other charities and rarely increase it, as seen in table 6.

The *magnitude* of this substitution is a thornier issue: it may differ across individuals[23] and may be nonlinear, i.e., larger or smaller at different levels of initial contribution and in response to changes of different magnitudes. Furthermore, individuals may not even have consistent preferences – they may change their substitution patterns over time and with "noneconomic" influences such as EG's (2003) framing of a subsidy. Although the directional pattern mentioned above is quite strong, there is naturally some heterogeneity, both in overall giving (as seen in section 5.1) and in substitution patterns.

[23] For example,there is evidence (see Reinstein, 2007a), that large givers substitute more than small givers – which could imply heterogeneity, nonlinearity, or both.

5.3 Pooled Analysis: 'Structural form'

Psuedo-Structural form – specification:

I estimate subject i's gift to charity j in period t as an exponential model, allowing a charity-specific effect, a time (period) slope, estimation of constant (own and cross) price elasticities, and proportional effects for each of the "shocks":

$$g_{jit} = e^{\alpha_j} \times t^{\beta_t} \times \prod_{k=1..K} p_{kt}^{\beta_{jk}} \times \prod_{m=1..M} \exp(x_{jtm}\pi_{jm}) \times U_{jt} \tag{11}$$

This is more simply expressed in the similar form:

$$\ln(g_{jit} + 0.1) = \alpha_j + \beta_t t + \boldsymbol{\beta}_j^{(p)\prime} \mathbf{p}_{jt}^L + \mathbf{\Pi}_j^{\prime} \mathbf{X}_{jt} + \mu_{jt} \tag{12}$$

Where:

$\mathbf{p}_{jt}^L = (\ln(p_{1t}), ..., \ln(p_{Kt}))$: Vector of logs of 'prices' of gifts to each charity

$\mathbf{X}_{jt} = (x_{jt1,}..., x_{jtM})$.: Vector of M other variables potentially affecting gifts to charity j (and other charities)

...Including choice set dummies, 'movie' shock, social treatments,

Coefficients of interest: $\boldsymbol{\beta}_j^{(p)} = (\beta_{j1}...\beta_{jk})$– own and cross-price effects ('elasticities')

Elements of $\mathbf{\Pi}_j = (\pi_{j1}...\pi_{jM})$: "Own and cross" effects of choice and movie shocks; overall effects of social shock

The specification (12) resembles that of several classic papers that focus on the price and income elasticity of giving, including Taussig (1967), Feldstein and Clotfelter (1976), and Feldstein, Martin and Taylor, Amy (1976) . However, equation 11, (motivated by Silva and Tenreyro, 2006)[24], estimated using the Poisson pseudo-maximum-likelihood estimator, does not require a transformation of the left-hand side variable, which is often at a corner (zero giving). Given the correct functional form specification, standard identification conditions should hold here: the regressors vary exogenously by design.

[24] The exponential model also more robust provides a more robust estimation of elasticities, as the log-log model only yields useful elasticity estimates under very specific assumptions on the error term.

7. **Table 7: Pseudo-structural regressions**

Psuedo-structural, charity level, W2 and W1 stages 4-6

	(1)	(2)	(3)	(4)	(5)
Technique:	RE, linear	RE, log-linear	Xt-Poisson-RE, exponential	RE, log-linear	Xt-Poisson-RE, exp
Dependent variable	gift_	lgift_	gift_	lgift_	gift_
shocked	0.87***				
	(0.1)				
othershocked	-0.31***	-0.27***	-0.30***		
	(0.07)	(0.10)	(0.05)		
stage	-0.047***	-0.051***	-0.040***	-0.060***	-0.040***
	(0.01)	(0.02)	(0.007)	(0.02)	(0.008)
Dobsdknown	0.30***	0.41***	0.24***	0.50***	0.25***
	(0.1)	(0.2)	(0.07)	(0.2)	(0.08)
lprc		-4.03***	-2.02***	-3.76***	-1.92***
		(0.7)	(0.3)	(0.7)	(0.3)
lprccr_other				1.56**	1.24***
lprcmrc_other				1.64*	1.99***
lprcsa_other				0.87	0.56
un_prcshock				0.0015	-0.069
un_prcshock_onCR				0.50***	0.25***
un_ch				-0.28	-0.32***
un_ch_onCR				-0.26	-0.28*
tnc_ch				0.0055	-0.14
Dchar3	-0.29***	-0.18*	-0.27***	-0.055	-0.36***
Dchar4	-0.40***	-0.49***	-0.37***	-0.21	-0.19
Constant	1.64***	-2.14***	0.23	-1.76***	0.66***
Observations	2313	2313	2313	2313	2313
Number of newid	97	97	97	97	97
rho	0.31	0.47		0.47	

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

5.4 Pooled Analysis: Nonstructural Form

As discussed in the introduction, it is useful to estimate the effect of "specific" shocks to a charity on gifts to other charities, as a function of the shocks' "direct" effect. Put another way, we want to know what change in gifts to B to expect when we see a "shocked" change in gifts to A. This estimation could be attempted by regressing one charitable gift on another, controlling for relevant observables, as implied by the following model:

$$g_{jit} = [\beta_{ji}] + \beta_{ji}t + \boldsymbol{\alpha}'_{ji}\mathbf{g}_{\tilde{}j,it} + \mathbf{\Pi}'_{ji}\mathbf{x}_{ijt} + \epsilon_{jit} \tag{13}$$

$$E(\epsilon_{jit}) = 0 \tag{14}$$

If the gifts to other charities were exogenously manipulated[25], and not endogenously chosen, the coefficients $\boldsymbol{\alpha}$ would represent a conditional demand response (see: Pollak, 1969). In the present case, although the decisions are endogenous, I argue that what we estimate can be interpreted in as a quadratic approximation of (a multi-charity version of) the "reduced form" equation 10.

In addition to the issue of interpretation, there remain econometric identification problems with estimating an equation such as 5.4 using, say, OLS with a fixed effect. in particular, it is not clear that the identification condition $E(\epsilon_{jit} \times \epsilon_{\tilde{}jit}) = 0 \; \forall \; j, \tilde{}j$ will hold. In the experimental context, we might argue that period-to-period movements in a subject's gifts to a particular charity are largely driven by the designed exogenous shocks, at least after controlling for a time trend. I.e., the controlled treatments $\frac{p_j}{p_k}$, ..., Θ_k will shift $\mathbf{g}_{\tilde{}j}$ independently of g_{jit}. However,we cannot rule out the possibility that a subject's overall generosity may vary in specific periods for other reasons. Furthermore, there is the possibility of reverse causality, which could bias the estimates.

[25] For example, in the case of a typical product, this could be achieved if the government or an experimenter compelled the purchase and consumption of additional units of this product. For charitable giving, I argue that enforce donations do not yield the same utility as voluntary ones, and thus such a manipulation is impossible.

8. **Table 8: Instrumented regressions**

Instrumented regressions, nonstructural

COEFFICIENT	(1) IV-FE, wave 2	(2) Tobit IV, wave 2
not_	-0.37***	-0.80***
	(0.07)	(0.3)
stage	-0.087***	-0.17***
	(0.01)	(0.05)
Dobsdknown	0.46***	1.02**
	(0.1)	(0.4)
Dchar3	-0.15*	-0.12
	(0.09)	(0.3)
Dchar4	-0.24***	-0.47
	(0.09)	(0.3)
Constant	2.89***	3.04***
Observations	1872	1872

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Instruments – 1. Price of charities, UN and TNC choice

These instrumental variable results do not differ significantly from the "conditional correlation" fixed-effects regressions reported in appendix table 26, suggesting the bias from stage to stage variations in latent generosity and other unobservables is minimal. The Tobit results are much larger in magnitude, because they estimate "conditional-on-positive" effects.

6 Robustness Checks

6.1 Internal validity issues

Each stage in the experiment is realized with equal probability, and only one stage is realized. Thus, the classical expected utility maximizer should treat each stage independently. However, from a psychological perspective, previous stages may cast a shadow. In particular,one might worry that offering a higher match rate and then taking it away, as in wave two stage five, would discourage later for giving. However, there is no evidence of a particular discouragement effect in this stage:

9. **Table 9: Test of wave 2 stage 5 difference**

Test for discouragement effect, Poisson, Wave 2

	(1)
Dependent variable:	gift_
Dchar3	-0.29**
	(0.1)
Dchar4	-0.15
	(0.1)
lprc	-2.34***
	(0.3)
lprcmrc_other	1.55***
	(0.5)
lprcsa_other	0.17
	(0.5)
lprccr_other	1.00*
	(0.5)
stage	-0.044***
	(0.009)
stage5	0.030
	(0.08)
Dobsdknown	0.37***
	(0.07)
Constant	0.43
	(0.3)
Observations	1872
Number of newid	48

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

10. **Table 10: Results by size of endowment/wave**

Test size of endowment X substitution,

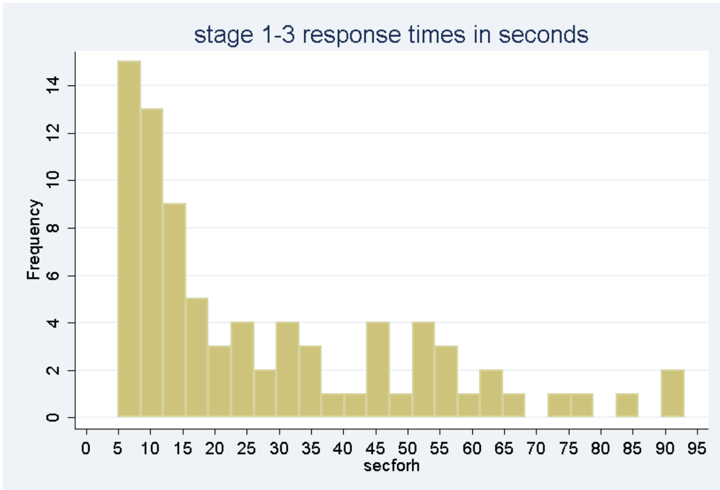
	(1)	(2)
Technique:	Poisson, FE	OLS-FE
Dependent variable:	gift_	gift_
lprc	-2.44***	
	(0.3)	
lprc X Wave 1	-0.68	
	(0.5)	
not_		-0.19***
		(0.02)
not_ X Wave 1		-0.34***
		(0.03)
Dchar3	-0.45***	
Dchar4	-0.26**	
lprccr_other	0.69*	
lprcmrc_other	1.64***	
lprcsa_other	0.18	
un_ch	-0.39***	
un_prcshock	-0.030	
tnc_ch	-0.13	
stage	-0.017**	-0.033***
Dobsdknown	0.13*	
Constant	0.20	1.99***
Observations	2754	2754
Number of newid	97	97

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A common criticism of laboratory experiments is that the monetary incentives and experimental treatments do not dominate the "noise", and that the amounts of money at stake are too small to be taken seriously. In such a case, if we increase the stake enough, the incentives to maximize should increase, and the subjects decisions should become more responsive to the treatments. I offer some evidence on this by differentiating the results in the first and second waves, noting that the latter had twice the endowment.[26]

The own price elasticity is (insignificantly) *larger* in magnitude than in the first wave, with the smaller endowment. This suggests that if the (arguably) larger incentives in the

[26] The second wave has a larger *overall* incentive, although admittedly the incentive per stage is roughly the same. Still, it can be argued that the cost of paying attention to the rules and incentives is largely a fixed cost rather than a per-stage cost, and incurring this cost will yield a greater benefit in the second wave.



8. **Figure 8: Response Times**

latter wave are salient, the smaller incentives in the former wave are also. As further evidence that the incentives are salient, I note that most subjects take more than the minimum time to enter their choices (figure 8)

Tables 22 through 24 in the appendix show that the vast majority of the subjects claim that they understood the experiment and were confident in it's truthfulness. A smaller majority had confidence in the charities themselves.

6.2 External Validity

The issue of external validity is particularly important when we are trying to estimate homegrown values, such as the nature of the preferences over charitable donations. Friedman and Sunder (1994) invoke the concept of "parallelism" to presume that results will carry over to the real world, and claim that the skeptic "has the burden of stating what is different about the outside world that might change the result observed in the laboratory." Anticipating this, I address several possible differences.

Harrison (1992) notes that: "Lab responses may be censored by field opportunities." A subject may under-report her valuation if a good is cheaper in the field. I address this with the baseline 20% match rate, which (particularly for those who do not itemize deductions on their taxes) should make gifts in the laboratory a bargain. If a subject can re-sell a good, she may over-report her true valuation. In the present experiment, a subject might donate in the laboratory and in turn reduce her giving in the field. I differentiate the results in table 11 by including an interaction term (column 1) and then running a separate regression (column 2), for those who claim to have donated previously, to detect whether such intertemporal substitution may be occurring.

In the first column, the interaction term suggests greater responsiveness to price, and perhaps intertemporal substitution, it is not significant. The second column's regression shows similar price effects for the "non-outside-givers" as for the "outside givers" suggesting that intertemporal substitution is not driving my main results.

11. **Table 11: "Outside givers" versus rest**

Test outside giving X price, Poisson-RE

	(1)	(2)
Subset	Wave 2, CR,MRC,SA	... and did not givers outside
	gift_	gift_
Dchar3	-0.29**	-0.074
Dchar4	-0.15	0.47**
lprc	-1.49***	-1.55***
	(0.4)	
go_lprc	-0.72	
	(0.5)	
lprccr_other	1.46***	1.16*
lprcmrc_other	2.01***	2.27***
lprcsa_other	0.62	0.29
un_ch	-0.45***	-0.23**
un_prcshock	0.057	0.18
tnc_ch	-0.14	0.22*
stage	-0.043***	-0.074***
Dobsdknown	0.26***	0.42***
Constant	0.78***	0.35
Observations	1872	1131
Number of newid	48	29

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

As the demographics (tables 14 - 21 in the appendix) show, my sample is not representative of the American population at large, nor of the population of charitable donors. I employ the typical "convenience sample" of students (and some university staff), paid volunteers for such an experiment, young, educated, and living in a liberal academic setting. While this may not be a problem for testing the theory of universal behavior, it is well-known that there are wide differences in charitable giving behavior among socioeconomic groups. While I observe some heterogeneity in substitution patterns, as reported in section 5.2, there are no clear patterns along observable socioeconomic lines. Table 12 shows (mean) coefficients that are remarkably similar across the various groups in my study.[27]

[27] While I test for differences across sex, ethnicity, income, and religion, I do not observe meaningful variation in categories such as education, region, or age. Future work should expand the sample along these dimensions.

12. **Table 12: Differentiated results by demographics**

Poisson, W2 and W1 (run 2-3) stage 4-6, by demography

COEFFICIENT	(1) All	(2) females only	(3) Asians only	(4) Income below 63, 000	(5) Identified as a religion
Dchar3	-0.083*	-0.24***	0.11*	0.013	0.024
	(0.04)	(0.06)	(0.06)	(0.07)	(0.06)
Dchar4	-0.19***	-0.60***	-0.17***	-0.30***	-0.18***
	(0.05)	(0.06)	(0.07)	(0.08)	(0.07)
lprc	-2.76***	-2.59***	-2.68***	-3.24***	-2.86***
	(0.3)	(0.3)	(0.4)	(0.4)	(0.4)
othershocked	-0.34***	-0.37***	-0.22***	-0.17**	-0.35***
	(0.05)	(0.06)	(0.07)	(0.08)	(0.07)
stage	-0.042***	-0.050***	-0.035***	-0.025**	-0.038***
	(0.007)	(0.009)	(0.01)	(0.01)	(0.01)
movie	0.12	0.13	0.016	0.19	0.048
	(0.10)	(0.1)	(0.2)	(0.2)	(0.2)
Dobsd	-0.12	-0.12	-0.20	-0.24	-0.11
	(0.09)	(0.1)	(0.1)	(0.2)	(0.1)
Dobsdknown	0.44***	0.57***	0.53***	0.60***	0.42***
	(0.09)	(0.1)	(0.1)	(0.2)	(0.1)
Dobsdopsex	-0.036	-0.071	-0.11	-0.096	0.049
	(0.09)	(0.1)	(0.1)	(0.1)	(0.1)
Constant	-0.17	0.18	-0.34	-0.78***	-0.15
Observations	2652	1425	1494	1353	1272
Number of newid	97	49	47	43	44

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

7 Conclusion

7.1 Experimental Results and Implications

This series of experiments offer some of the first evidence on how people give to multiple charities in the lab. The direct effects of treatments were strong and in the predicted direction. Most shocks are effective: people give more to a charity when its match rate was higher, when the decision was observed, when they were motivated by a video, and when there were fewer other charities in the choice set. Several of these direct effects are interesting in themselves; as mentioned before, the gifts are own-price elastic with regard to match rates, contrasting with some previous literature. There is also clear evidence of a reputation motive: subjects give more when their decision and their identity is observed.

The results conclusively demonstrate that "expenditure substitution" among charities can be seen in a laboratory setting. I find large own-price elasticities and very large cross-price elasticities – these charities are gross substitutes in the conventional sense. The substitution is stronger where the charities serve similar purposes, such as UNICEF and Care. In the "reduced form" model, using instrumental variables estimation, I estimate an expenditure substitution coefficient of 37% – when gifts to one charity are increased (or decreased) by a certain amount because of a shock, the sum of gifts to other charities decreases by 37% of this amount. The conditional-on-positive estimate of this substitution is 80%: where gifts to both charities remain positive, the "crowding out" is $0.80 for every $1. Both the structural and reduced form estimates obscure a great deal of heterogeneity– subjects vary greatly in their substitution patterns, as seen in section 5.2 and in table[26] in the appendix.

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8 Appendix

13. **Table 13: Variable definitions**

Variable name	Description
Summary Statistics	
X_	Gift to charity X[1] in stage[2]
GX_	Dummy: Gave to X[1] in stage[2]
tot_	Sum of gifts in stage
Gtot_	Gave to some charity in stage
gave_ever	Dummy: Subject gave in some stage
Gave_ever_X	Dummy: Subject gave to X[1] in some stage
Charity Level	
gift_	Subject's gift to lhs charity
D_X	Dummy: lhs charity is X[1]
stage	Actual stage of experiment (1..13)
not_	Sum of gifts to charities other than lhs charity
shocked	lhs charity has "shock"[3]
othershocked	some rhs charity has shock[4]
Dobsdknown	Decision and subject's identity observed
Dobsdopsex	... by member of opposite sex
lprc	log(price of $1 recieved by lhs charity)
lprcX_other	log(price of $1 recieved by X[1] where lhs charity is not X)
un_prcshock	UN 50% match
un_prcshock_onCR	UN 50% match and lhs charity is CR
un_ch	UN in choice set
un_ch_onCR	UN in choice set and lhs charity is CR
tnc_ch	TNC in choice set
go_lprc	[gave outside of lab] $\times$ lprc

1. Where $X \in \{CR, SA, MRC, UN, TNC\}$
2. Universe: Stages where charity X in choice set
3."Shock" defined as 50% match
4. ... also UN or TNC in choice set for 'othershocked'

14. **Table 14: Sex**

Item	Number	Per cent
Lost Data	5	5
Male	43	44
Female	49	51
Total	97	100

15. **Table 15: Race/Ethnicity**

Item	Number	Per cent
African American/Black	3	4
Asian or Pacific Islander	47	60
Don't want to answer	6	8
Hispanic/Latin	8	10
Other/Mixed	6	8
White/Caucasian	8	10
Total	78	100

16. **Table 16: Religion**

Item	Number	Per cent
Not collected	19	20
Catholic	15	15
Protestant	7	7
Other Christian	8	8
Other Religion	14	14
No preference/ No religion	28	29
Don't want to answer	6	6
Total	97	100

17. **Table 17: Political affiliation**

Item	Number	Per cent
Democrat	34	44
Republican	4	5
Green Party	1	1
Other Party	1	1
No political affiliation	34	44
Don't want to answer	4	5
Total	78	100

18. **Table 18: Marital status**

Item	Number	Per cent
Single, Never Married	46	82
Married	5	9
Divorced/Separated	4	7
Don't Want to Answer	1	2
Total	56	100

19. **Table 19: Number of children (3rd run only)**

Item	Number	Per cent
0	5	63
1	2	25
2	1	13
Total	8	100

20. **Table 20: Income or parent's income if dependent**

Item	Number	Per cent
0-10, 000	7	9
10, 000−24,000	12	15
24, 000−41,000	14	18
41, 000−63,000	10	13
63, 000−94,000	8	10
94, 000−130,000	9	12
130, 000−200,000	4	5
More than 200, 000	6	8
Don't want to answer	8	10
Total	78	100

21. **Table 21: College major**

Item	Number	Per cent
Arts and humanities, psychology, legal studies	8	17
business/ economics/ political science/ public policy	19	40
Physical and life sciences / engineering / math	18	38
Other	1	2
Non-student	2	4
Total	48	100

22. **Table 22: Understood rules of experiment**

Item	Number	Per cent
Disagree	5	6.41
Neutral/No opinion/Don't want to answer	4	5.13
Agree	69	88.46
Total	78	100.00

Source: run1_6_data_wide.dta

23. **Table 23: Confident we pay the charities**

Item	Number	Per cent
Disagree	2	2.56
Neutral/No opinion/Don't want to answer	6	7.69
Agree	70	89.74
Total	78	100.00

24. **Table 24: Confident in random choice of stage**

Item	Number	Per cent
Disagree	4.00	5.13
Neutral/No opinion/Don't want to answer	9.00	11.54
Agree	65.00	83.33
Total	78.00	100.00

25. **Table 25: Confident charities use money as stated**

Item	Number	Per cent
Disagree	23	29.49
Neutral/No opinion/Don't want to answer	5	6.41
Agree	50	64.10
Total	78	100.00

26. **Table 26: Descriptive regressions –** Overall and by Wave/Run

XTRC, charity level, non-structural

COEFFICIENT	(1) W1 period 4-5, wave 2	(2) Wave 1	(3) W2	(4) Run 4	(5) R5	(6) R6
not_	-0.32***	-0.47***	-0.21***	-0.25**	-0.18	-0.19**
	(0.05)	(0.05)	(0.06)	(0.1)	(0.1)	(0.07)
Dchar3	-0.18*	-0.21**	-0.021	0.059	0.13	-0.16
	(0.1)	(0.09)	(0.2)	(0.4)	(0.2)	(0.1)
Dchar4	-0.22*	-0.17*	-0.11	0.098	-0.090	-0.24*
	(0.1)	(0.09)	(0.2)	(0.5)	(0.2)	(0.1)
stage	0.023	0.27***	-0.052***	-0.022	-0.060**	-0.078***
	(0.05)	(0.06)	(0.02)	(0.03)	(0.03)	(0.03)
Dobsdknown	0.13**		0.32**	0.30	0.39*	0.29
	(0.07)		(0.1)	(0.2)	(0.2)	(0.3)
Constant	2.41***	1.41***	2.56***	2.88***	2.26***	2.51***
	(0.4)	(0.2)	(0.5)	(0.8)	(0.7)	(0.9)
Observations	2313	882	1872	624	624	624
Number of newid	97	49	48	16	16	16
Sigma of Beta_1	0.18	0.12	0.16	0.19	0.23	0.079

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

27. **Table 27: Mean Differences in Gifts**

Stages:	**4 - 1,2,3**	**5 - 4, treated##**	**6 - 4, treated##**	**Controls###**
CARE	-1.39***	0.94***	1.22***	-0.44
	(.25)	(.29)	(0.36)	.47
SA	-1.15***	-0.29***	-0.38***	0.11
	(.21)	(.14)	(.16)	.11
MRC	-1.16***	-0.19***	-0.28***	-0.22
	(.21)	(.10)	(.12)	(0.28)
MRC+SA	N/A	-0.48***	-.66**	-0.11
	N/A	(0.21)	(0.24)	(.31)
Total	#0.89***	0.46***	0.57**	-0.56
	(.21)	(.23)	(.34)	(.71)
Obs.	49	29	29	9

Source: X-Lab runs 1,2,3

\# Comparing mean giving in stages 1-3 to total giving in stage 4

\## *For subset: gave to SA or MRC in Stage 4;* …results for all 45 treated observations are similar but smaller in magnitude

\### Difference is for controlled stage - 4th stage