



Economics Department Discussion Papers Series

ISSN 1473 – 3307

Conditional generosity and uncertain income: Evidence from five experiments

Christian Kellner, David Reinstein and Gerhard Riener

Paper number 17/07

Conditional generosity and uncertain income: Evidence from five experiments*

Christian Kellner^a

David Reinstein^b

Gerhard Riener^{**c}

^a *University of Southampton*

^b *University of Exeter*

^c *Heinrich Heine University Düsseldorf*

November 10, 2017

Abstract

We study how other-regarding behavior extends to environments with income uncertainty and conditional commitments. Should fundraisers ask a banker to donate “if he earns a bonus” or wait and ask after the bonus is known? Standard EU theory predicts these are equivalent; loss-aversion and signaling models predict a larger commitment *before* the bonus is known; theories of affect predict the reverse. In five experiments incorporating lab and field elements (N=1363), we solicited charitable donations from lottery winnings worth between \$10 and \$30, varying the conditionality of donations between participants. While the results suggest some heterogeneity across experimental contexts and demographic groups, in each experiment conditional donations (“if you win”) were higher than ex-post donations. Pooling across experiments, this is strongly statistically significant; we find a 23% greater likelihood of donating and a 25% larger average donation commitment in the *Before treatment*. Our findings add to our understanding of pro-social behavior and have implications for charitable fundraising, for effective altruism giving pledges, and for experimental methodology.

Keywords: Social preferences, contingent decision-making, signaling,
field experiments, charitable giving.

JEL codes: D64, C91, C93, L30, D01, D03, D84.

* Acknowledgments: We would like to thank the Universities of Essex, Düsseldorf, Mannheim and Bonn for providing financial support for our research. We would also like to thank the many colleagues who have offered us valuable advice and comments, including Johannes Abeler, Michelle Brock, Gary Charness, Jean Hindriks, Brit Grosskopf, Zack Grossman, Sandra Ludwig, Friederike Mengel, Graeme Pearce, Timothy Rakow, and Jeroen van de Ven, and conference and seminar participants at the Advances in Field Experiments, the British Academy, CAGE/CMPO, Einaudi, ESA, Essex, Exeter, FUR, J-LAGV, London Experimental Workshop, M-Bees, Sheffield, the Science of Philanthropy Initiative, SUNY Albany, and UCSB. We are also grateful to Samuel Dexter, Edward Dickerson, Saga Eriksson, Jonathan Homola, Louis Philipp Lukas, Kajetonas Orlauskas, Stavros Poupakis, and Agata Siuchninska for excellent research assistance.

** Corresponding author (riener@uni-duesseldorf.de).

1 Introduction

Most research on other-regarding behavior considers choices under certainty. However, decisions in this domain often involve risk, uncertainty, and contingent plans. We provide unique evidence on how other-regarding behavior extends to income uncertainty and to contingent commitments. This is motivated by a particular practical question: what is the best way to ask for a charitable donation from an individual who may get an uncertain bonus income? Should you ask her *before*—to make a contingent commitment to donate if she wins the bonus or ask her *after*—to donate after her bonus has been revealed?

There are important differences between these two modes of asking which may impact behavior: (i) *Before* commitments are from uncertain income. (ii) *Before* commitments to donate are not realized with certainty. (iii) *After* commitments follow an experience of winning. If she is a standard expected utility maximizer only caring about outcomes, this will not matter. In contrast, certain theories of *affect* predict that she will donate more *after* winning. However, we show that under particular specifications, loss-aversion and signaling models predict a larger commitment for giving *conditionally*, in advance of learning the outcome. This latter prediction is largely substantiated by our evidence from a series of lab and web-based experiments, discussed below.

This is an important issue for policymakers and fundraisers. Many employees receive windfall payments in supplement to their regular income. In the 2011/12 tax year, bonuses to UK workers totaled £37 billion, of which £13 billion was in the financial sector, at an average rate of £12,000 per worker (ONS, 2012). In the USA, Wall Street banks distributed \$26.7 billion in bonuses in 2013 (NY Comptroller, 2014). Anecdotal evidence suggests that a significant share of this bonus income was not fully anticipated.¹ In the wake of recession and scandals in the financial markets, bankers have been encouraged to give back their bonuses, or donate them to charity.² Our evidence suggests it may be more effective to ask bankers to commit to donate from *future* bonuses. Moreover, this question is relevant to situations in which individuals are asked or volunteer to donate from actual or potential financial gains. Lottery organizers may include a tick-box to make a conditional pledge. Ethical investment accounts could automatically donate gains that exceed expectations.³ This is not merely hypothetical: several prominent movements and organizations ask students, workers, and entrepreneurs to publicly pledge a share of their future income and profits.⁴

This also has implications for experimental methodology, in particular, the random lottery incentive scheme, where only one stage is chosen randomly for payment (see Cubitt et al., 1998). Particularly where

1. From our personal correspondence: “Most people at the top or the bottom of the performance level will know they’re (not) getting a bonus—people in the middle will be unsure until they’re announced. Among the people who know they’re going to get a bonus, the size of the bonus is uncertain until announced.”, Raj C: Hedge Fund Manager, London (2015). See also forum posts <<http://www.quora.com/Bonuses/How-accurately-can-an-employee-predict-his-or-her-annual-bonus-in-advance-e-g-in-the-banking-industry>>, accessed 7 Feb, 2015.

2. “Johnson: Bankers should assuage guilt by giving bonuses to homeless scheme.” – *The Guardian*, 13 Feb 2009.

3. E.g., Triodos Bank offers a “Save and Donate” account <<http://www.triodos.co.uk/en/personal/savings-overview/charity-saver/>>, accessed 29 Sep. 2017; however this currently involves *fixed* interest rates and a fixed donation share.

4. “Giving What We Can,” founded by Tony Ord, asks people to make a *giving pledge* to donate roughly 10% of their future income. According to their website <<https://www.givingwhatwecan.org/about-us/history/>> (accessed 25 Sep 2017) they have over 3000 members who have donated over \$24 million (and pledged to donate far more); a large share of whom are students, who presumably face great uncertainty over their lifetime earnings. *TheLifeyouCansave.org*, promotes a smaller-percentage pledge with rates adjusted by income and by country. The Founders pledge (<https://founderspledge.com/about-us>, accessed 12 Sept. 2016) asks tech entrepreneurs who have yet to ‘cash out’ to make a legally-binding commitment to donate 2% or more of their potential proceeds to a social cause of their choice. Motivated by our research, the London-based City Philanthropy recently held a “Bonus pledge think tank” to explore this idea (See: <http://www.cityphilanthropy.org.uk/events/1-bonus-pledge-think-tank> and <http://www.cityphilanthropy.org.uk/news/call-city-firms-help-cabinet-office-research-%E2%80%99windfall%E2%80%99giving>, accessed 12 Sept. 2016).

signaling is relevant, subjects may not treat each stage independently, and may integrate their choices into a global decision frame.

To the best of our knowledge, there is no direct economic evidence on the effect of the resolution of income uncertainty on other-regarding behavior. Tonin et al. (2014) and Reinstein (2010) each ran experiments involving charitable donation in uncertain environments, where subjects knew that only one of a series of decisions would be implemented; both found that donations declined over time. According to Reinstein “if individuals are not [expected utility] maximizers over outcomes but gain warm glow utility from unrealized commitments, this decline could be attributed to satiation of warm glow”. In laboratory dictator games Brock et al. (2013), Sandroni et al. (2013) and Höchtl et al. (2015) all found that social preferences and fairness concerns appear to depend on a combination of *ex post* and *ex ante* concerns. Smith (2011) found that giving (to other subjects who had incurred an income loss) was higher when giving decisions were made using the strategy method than when subjects were asked *ex-post*. These results argue against a model where an individual maximizes expected utility with a consistent utility function that considers only outcomes.

Grossman (2015) focused on measuring and differentiating self and social signaling. His laboratory experiments (with a standard student sample) involved binary-choice dictator games where an individual’s choice may be randomly overruled with a given probability. The treatments varied this probability, and whether the “observer” (another subject or the experimenter) saw the outcome, the choice, and the probability of overrule. In general, he found that the probability that the choice was overruled had neither a large nor a significant effect on choices in *any* of the observability conditions. Our approaches differ substantially; in particular, our focal treatment involves a conditional commitments from uncertain *income*.⁵

Generosity involving *unconditional future* commitments is a distinct but related issue. Breman (2011) ran a field experiment asking donors to commit to increase their regular donation either immediately, in one months time, or in two months time. She found the longest delay led to the greatest increase in contributions. Her explanation is that the cost of giving occurs at the time of payment, while “the warm-glow ... will be experienced at the time of committing to giving.”⁶ Commitments in Breman’s experiment could be reversed but they rarely were. While Andreoni, Serra-Garcia (2016) also find greater “Give-Later” commitments (in longitudinal laboratory experiments), they find frequent renegeing on previous pledges, as well as heterogeneous dynamic inconsistency.

Our evidence abstracts from this issue: in our experiments the uncertainty is resolved almost immediately and there is no difference in when the donations are realized. However, in real-world fundraising applications (e.g., in the bankers’ bonuses context), asking for conditional donations of uncertain income is likely to also mean asking for a *delayed* donation.

5. Our experiments differ from his along several other dimensions (binary vs. continuous choices, different sets of probabilities, with/without a real-effort task, session-level versus within-session variation, oral versus computer instructions), and we also provide evidence from several field contexts. For our context that most resembles his—the laboratory Uncertain Collection treatment—we also find a null result. In general, for our experiments involving *students*, we find small effects, which a standard laboratory sample size would have limited power to detect.

6. Breman draws on Thaler et al. (2004), whose “Save More Tomorrow” experiment found that individuals save more when asked to pre-commit a portion of future pay raises towards retirement savings. She extends the “pre-commitment for the future” part of their treatment to the charitable domain; our experiments extend the *commit uncertain salary increases* effect (which the authors argue is driven by loss aversion). These results largely support Andreoni, Payne (2003), who write that “a commitment to a charity may yield a warm-glow [benefit] to the givers before ... the costs are paid”. This raises the question “when does the benefit of giving occur and how long does it last?” By this logic we might expect to see charitable giving exclusively through end-of-life bequests, which would yield warm glow that could be savored over one’s entire life. However, bequest giving is rare (Cabinet Office, 2011, Giving White Paper, HM Stationary Office.)

We present the results of a series of five experiments in distinct contexts, with complementary strengths, each combining lab and field characteristics in a different way. Our experiments offer the first systematic insight into contingent giving from known or uncertain income. To avoid contrast and experimenter-demand effects, all experiments involve only between-subject treatment variation and only a single charitable ask; thus we use large samples to detect moderate-sized effects. Table 1 in section 3 offers a brief summary of the differences between our five experiments.

Our laboratory experiments offer a more controlled environment, where we can be sure subjects are making decisions on their own, they cannot communicate, and they can tangibly verify that outcomes are randomly determined (see Maniadis et al., 2014). Our web-based evidence offers environmental validity and is less prone to experimenter demand. Our lab experiments varied the presentation of the earnings as random (the “bonus” being awarded with 50% probability) and the timing of the contribution request. Depending on the treatment, we observed conditional pre-commitments for (losing and) winning states or decisions after winning (or losing) a lottery. While the lab experiment involved two levels of earnings and five treatments, we focus on the two treatments that most parallel those in our web-based experiments.⁷

In all of the experiments the results are in the same direction: people committed to donate more when asked immediately *before* they knew if they had won. Although these differences are statistically significant in only some of these experimental contexts (two at $p < 0.01$ and one at $p < 0.10$), they are strongly significant when we pool across all experiments. Overall, conditional donations (“if you win”) were 25% higher (3.8 percentage points higher) in the *Before* treatments (Table 5). The effect was particularly strong and significant in our web-based experiment using non-student participants: giving nearly doubled, and its incidence increased by 50%. Further modelling suggests a non-linear relationship: the *Before* treatment has a larger effect for those predicted (by pre-determined observables like age and gender) to donate less.

2 Basic Setup and Predictions

In this section we offer a theoretical perspective on giving when income is uncertain. Consider two income levels, w and ℓ , where $w > \ell \geq 0$. The decision maker knows she is facing a lottery with a non-degenerate probability p of winning w and a probability $1 - p$ of losing and earning ℓ . Consider the following settings.

After setting (A): Income is unknown until a lottery is resolved, and she learns whether she has won or lost the lottery. If she has won, she learns her income and then is immediately asked to donate to a specific charity. She donates $g_w^a \geq 0$. If she loses, she may also be asked to give, and she chooses $g_\ell^a \geq 0$.⁸

Before setting (B): Income is unknown until a lottery is resolved. Before she learns the outcome, she is asked to make a binding commitment to donate to a specific charity *if she wins* w . She commits to give $g_w^b \geq 0$ if she wins (if she loses nothing is collected).

7. Given the large variance in donation choices we had limited power to detect moderate-sized differences among all of these. Thus, we only report them in the appendix for completeness. The other three treatments were the following. *Before Both*: a separate ex-ante decision for losing and winning states. *Uncertain Collection*: the income was known and certain, but there was only a 50% chance that a committed donation was to be collected. *Benchmark*: either low or high income, but with no income uncertainty. Our results are very similar if we pool this *Benchmark* treatment with the *After* treatment reported below, and also similar (but stronger) if we pool the *Before* and *Before Both (if win)* treatments.

8. However, in three of five of our experiments we did not ask losers to donate.

Our main question is: “how does her commitment or pledge in the *Before* setting, to donate if she wins, compare to her donation in the *After* setting when she has already won?”, i.e. “what is the relationship between g_w^b and g_w^a ?”

To isolate the drivers of potential differences in donations in the above settings, we consider the following additional environments. Individuals in setting (D) face no uncertainty, as they know from the beginning whether they have a high or low income; we label their donations g_w^d and g_ℓ^d , indexed by income. Those in setting *U* also have a definite income (w or ℓ), but their chosen donation (labelled g_w^u or g_ℓ^u) will only be collected with probability p , and otherwise it is returned to them. Finally, we consider a variation of the *Before* setting, where the individual makes *two* donation choices before the uncertainty is resolved; one for low income (g_ℓ^{bb}) and one for high income (g_w^{bb}).

2.1 Expected utility maximization over outcomes

In most previous models of charitable giving, only an individual’s *realized* contribution (and consumption) affects her utility. She may care about the total amount of the public good provided (Becker, 1974), she may gain warm glow from the amount of her own income she has actually given away (one interpretation of Andreoni, 1990), or she may care about her impact on outcomes (Duncan, 2004) or on an individual she identifies with (Atkinson, 2009). Here, intentions and commitments to contribute that are unrealized do not affect utility. Although these theoretical papers generally do not consider uncertain environments, they have been applied to such contexts (e.g., Vesterlund 2003; DellaVigna et al. 2012, as well as in numerous laboratory experiments). While other models emphasize the reputation and signaling benefits of giving (Harbaugh, 1998), self-signaling (Benabou et al., 2006), moral concerns of reciprocity (Sugden, 1984), or a *Kantian* motive (Sugden, 1982; Roemer, 2010), these have been modeled solely in terms of *actual* donations.⁹ Because of this, the timing and uncertainty of the decision (i.e., whether it is a sure thing or a prospect) is irrelevant to the individual’s choice. This will hold for any model that can be expressed in terms of expected utilities over outcomes; this is stated in prediction 1 and is trivially proven in Appendix A.1.

Prediction 1. *Expected utility maximizers*

$$\begin{aligned} g_w^d &= g_w^a = g_w^b = g_w^{bb} = g_w^u & \text{and} \\ g_\ell^d &= g_\ell^a = g_\ell^{bb} = g_\ell^u \end{aligned}$$

2.2 Signaling and self-signaling

The idea that people are able to positively signal to themselves or others by committing to give with positive probability, even if the gift is not realized, is supported by Sandroni et al. (2013). In their experiment, subjects were presented with three options: to get more money; to get less money and more of some other good; and to flip a coin between these two alternatives. When the “other” good was a consumption good, randomization was negligible. When it was a social good that yielded payoffs to another subject, nearly a third of their subjects randomized. While they highlight fairness as the key issue, other interpretations are possible. Suppose that the *commitment* to donate with some probability *itself* yields a benefit (e.g., self-signaling, impact, or warm glow), but there are locally diminishing returns to both private consumption

9. To be precise, the reciprocity and Kantian models mentioned are procedural and not based on utility-maximization; still, these do not have an explicit role for unrealized commitments. We also note that more recent work has argued, in an experimental context, that intentions and commitments may yield direct utility and signaling value; we return to this later.

and donation. Here, the choice of a coin flip over the social good can be seen as convexifying over private consumption and other-regarding choices to find an optimum.

Donating with certainty or with some probability may allow the individual to differentiate herself from “worse types”, and this may benefit her reputation or self-esteem (Benabou et al., 2006), yielding a utility gain. On the other hand, she is sacrificing expected income, hence sacrificing utility from consumption. Both the signaling benefit and the utility of additional income may not only be a function of the expected values, but may depend on the probability distribution. As noted above, Tonin et al. (2014) allow benefits via self-signaling to accumulate even for unrealized uncertain donations. However, they do not consider that the signaling value itself may be lower when the probability of realizing the commitment decreases.

We offer a simple signaling model—for derivations and proofs see Appendix A.2—with two types of agents (or two types of self): one who gets an inherent benefit from donating to the charity (a “good type”), and one who does not (a “bad type”).¹⁰ We demonstrate that uncertain collection of committed donations, as in our *Before* and *Uncertain Collection* settings, can lead to higher (conditional) donations. We focus on parameter values where, at the good types’ preferred donation (ignoring signaling), the bad types have an incentive to pool to gain reputation. Here, as the probability of collection decreases below one, the level of conditional-on-collection donations that can be sustained as an equilibrium satisfying the intuitive criterion increases. Essentially, as the intent to donate can still be demonstrated, while the cost of actually donating will only be paid with a probability less than one, the (minimum) conditional-on-collection donation must increase in order to separate types.

If an individual only recalls his or her own true type with error, the signaling model can be considered as an equivalent *self*-signaling model, as noted in Benabou et al. (2006; 2011). Note the arguments above do not automatically carry over to the *before both* setting: if the reputation takes into account both types of conditional donations, then a bad type who donates only conditional on winning would fully reveal his type.

Prediction 2. *Signaling generosity, where the separation constraint binds*

$$g_w^u = g_w^b > g_w^d = g_w^a = g_w^{bb}$$

*for good types, while bad types are unaffected by the treatment. A similar relationship will hold for donations from the lower level of income if condition 4 also fails at ℓ , which need not be the case.*¹¹

10. This model is distinct from Grossman (2015). He models a continuum of types with binary choices, where the outcome is not entirely deterministic: either choice may be overruled by nature with a known probability. He further solved for cases varying the observer’s information about the choice and environment. As in our model, both the signaling value and the material cost of a particular donation increase in the probability the donation is realized. In his model, where the observer sees only the choice and not the probability, a donation commitment (of a specified size) is more likely where the donation is collected with a lower probability, because signaling is “cheaper”. However, in considering a varying probability of realization, Grossman only compares environments where the externally-observed probability of realization—and thus the external signaling value—is constant. He does not model a case where the observer (or future self) sees both the choice and the probability, as in our model, and these vary together. This case is particularly relevant to donations from uncertain income. For the “after” ask, the probability of realization is one, and this is common knowledge to the fundraiser and potential donor. For the “before ask” we consider, it is common knowledge that this probability is less than one. However, in the real world, the exact probability of a bonus may not be common-knowledge; the impact of this may be considered in later work.

11. Under the standard assumption that $u(\cdot)$ is concave, the parameter space where this holds at income ℓ is a proper subset of the parameter space where this holds at income w .

2.3 Loss Aversion and Reference Points

When making—even riskless—choices, it is often argued that decisions are influenced by anticipated gains and losses relative to a reference point (see Tversky et al., 1991). Thaler et al. (2004) claim “... once households get used to a particular level of disposable income, they tend to view reductions in that level as a loss.” In considering this model, we suppose the individual has a reference point over her own consumption, not including charitable giving, and her utility function sums a standard reference-independent term and a *gain-loss* component. Her donation decision, whether stochastic or certain, anticipates how the donation will reduce the remaining wealth available for her own consumption. If this will fall below her reference point, she will incur a psychological loss. We assume there is no gain-loss utility over the donation itself (i.e., a single target, as in Camerer et al., 1997).¹² While the reference point may change over time, we assume here that she is *myopic* in the sense that when making a decision she does not anticipate these changes. For simplicity, we consider a utility function embodying a linear loss function, i.e.,

$$v(x, g, \pi) = \begin{cases} u(x) + \omega(Dg) & \text{if } x \geq \pi \\ u(x) - \delta[u(\pi) - u(x)] + \omega(Dg) & \text{if } x < \pi; \end{cases}$$

subject to the budget constraint $x + g \leq E$.

As before, x represents own consumption, g is the committed donation expenditure and D indicates whether it is realized, π is a reference point, specified below, and δ is a (strictly) positive constant. Here $u(\cdot)$, the sub-utility of own-consumption, is assumed to be strictly increasing and concave, as is the “warm glow” function $\omega(\cdot)$. We consider two different ways this reference point may update to the realization of uncertainty. First, we consider immediate adjustment, second, we consider a very sticky adjustment process.¹³ To save space, all of these derivations are in the online appendix (“Loss aversion models”).

Suppose the reference point always corresponds to the expected future income at the point of the decision, the maximum own-consumption one could achieve if one’s investments paid their expected value. This implies that contributions incur a *loss* in all cases except for conditional contributions from an expected win, implying greater contributions in the *Before* than in the other treatments, i.e.:

Prediction 3. *Loss Aversion, expected income, immediate adjustment*

$$g_w^{bb} = g_w^b > g_w^d = g_w^a = g_w^u,$$

$$\text{provided } g_w^b < w - (pw + (1-p)l), \text{ and } g_\ell^d = g_\ell^a = g_\ell^u = g_{ell}^{bb}.$$

12. This may hold if donating nothing and using all income for own-consumption is typically seen as the default, thus the basis for a reference point. Note that this model’s predictions would be qualitatively the same if there were two targets, but the gain-loss utility were far more salient for consumption targets than for giving targets.

13. We can consider the “ask,” even a fairly neutral ask, as a special shock motivating giving by changing the environment/context or temporarily adjusting the utility function to make the utility slope of giving particularly steep (via alleviating guilt or providing special warm glow); see Andreoni, Rao (2011) and models in Reinstein (2011) and Kotzebue et al. (2009). Hence we may predict individuals will give a larger share of their “winnings” when asked in our experiments than the share of their income they might normally donate. The reference consumption basket might be based on her expectations before being asked to donate, thus deducting no donation; alternately, it may have anticipated a small probability of an ask, or it might immediately subtract the expected value of the conditional donation after the ask. For any of these the reference consumption is still less than the higher earnings w .

This analysis generalizes to any intermediate reference point. If we assume that an individual's reference point corresponds to the original expected-value income throughout the relevant decision period, we have a slightly different prediction:

Prediction 4. *Loss Aversion, expected income, no adjustment*

$$g_w^{bb} = g_w^b = g_w^a > g_w^d = g_w^u \text{ and} \\ g_\ell^{bb} = g_\ell^a < g_\ell^d = g_\ell^u.$$

If the reference point does not adjust rapidly, then donations from an anticipated or actual *win* (loss) will be higher (lower) than donations from income that was not subject to uncertainty.

2.4 Affective state (unanticipated) and generosity

A favorable realization of a lottery may put people in a good mood, an unfavorable resolution may do the opposite. Theories and evidence on the interaction of affective state and generosity point to more giving when an individual is in a positive mood (Levin et al., 1975; Weyant, 1978; Underwood et al., 1976; Kidd et al., 2013; Drouvelis et al., 2016). On the other hand Fishbach et al. (2007) offer mixed evidence, and Kuhn et al. (2011), find “greater lottery winnings do not raise the likelihood that a household will donate its fee for completing our survey to charity”. Putting this together we might predict greater generosity *after* a prize has been won, relative to *before* the prize outcome is known, and relative to a certain income. We might also predict *lower* generosity after *failing* to win the prize relative to after a certain income (although the “negative state relief” model of Cialdini et al., 1973, predicts the reverse). If individuals in the *Before* setting do not anticipate their change in mood from winning or losing, and if neither non-random earnings nor facing a lottery directly affects mood, then (ignoring other effects) the conditional commitments in the *Before* setting will equal the *Benchmark* donations for the corresponding income levels.¹⁴

Prediction 5. *Affective state*

$$g_w^a > g_w^d = g_w^b = g_w^{bb}, g_\ell^a < g_\ell^d = g_\ell^b = g_\ell^{bb}$$

3 Designs and Implementation

We ran five experiments over different contexts, demographics, framing and rules. We describe each of these in this section before presenting our results.

Our lab experiment (involving a field commodity: the charitable donation) permits greater control and design flexibility. The four web-based experiments have many of the usually cited advantages of field experiments (see Harrison et al., 2004). In particular, they offer less risk of experimenter demand effects and reflect more naturalistic, less self-conscious behavior: participants were unlikely to know they were participating in an experiment, and still less likely to consider that they were in an experiment focused on charitable giving. There is also greater environmental validity. These experiments resemble real-world contexts that participants may be accustomed to: universities often run employability promotions, researchers

14. Similar predictions could arise out of an (indirect) reciprocity model (see Simpson et al., 2008), e.g., if the lottery's sponsor were the charity itself, or were believed to be sympathetic to the charity; the reciprocity motive would also have to depend on the realization of the “gift” and not only its probabilistic implementation.

run broad surveys and often give participants the option to donate their earnings, many promotions involve a prize lottery, and web sites often ask for donations.

Table 1 presents a summary of the experiments highlighting the most important differences between our experiments. Some of these variations were part of our initially conceived design, e.g., exploring the sensitivity to a non-student sample, and to varying the probability of realization. Others were introduced dynamically and in response to referee’s suggestions, e.g., the opportunity to reverse a *Before* commitment in the Prolific design. Still other differences reflect feasibility concerns, and our desire to leverage a range of naturalistic environments.

These variations impart a benefit. Gathering evidence over domains varying in the “distribution of the characteristics of the units” (participant demographics) and the “specific nature of the treatments [and] treatment rate” helps us to explore the “credibility of generalizing to other settings”, i.e., the sensitivity of the results to the context, the framing, and the subject pool (Athey et al., 2017).

Table 1. Summary of Experiments

Label	Context	Date	Participants		Population	Location	Donation Fulfillment	Base pay	Don. stake	Prob(Win)	Charity(s)	Match %
			total	usable								
Prolific	Employment choices	2017	320	240	General, non-student population	UK	Automatic (reversible)	£1	£10 Money	50% or 10%	Oxfam, British Heart Fdn	25%
Omnibus	Internet survey, ESSEXLab recruitment	2017	734	460	Students & staff	Essex	Automatic	0	£10 Amazon voucher	50%	Oxfam, British Heart Fdn	25%
Employability	Employability survey	2013–14	592	375	Students	Essex	Automatic	0	£20 Restaurant or Amazon	25%	Oxfam, WWF	10%
Laboratory	Fluid intelligence measure	2013–16	433	131	Students	Mannheim, Dusseldorf	Automatic	€7	€14 Money	50%	Bread for the World, WWF	-
Valentine’s	Valentine’s cards	2012	205	159	Students & staff	Essex, Bristol, Warwick	Active follow-up	0	£20 (£30) Restaurant	Ambiguous	Right to Play	-

Notes: This table summarizes the most important features of the experiments reported. The Prolific study also enabled us to use a rich set of previously collected background variables. “Usable” refers to participants in the treatments described; namely, they were asked a single time to make a donation from the listed “Donation stake” in either the *Before* or the *After* context.

In addition to the web links to our experimental instruments (below), we provide further details on each experiment, including several screen shots, and information on other treatment arms in the appendix.

3.1 Valentine’s

In 2012 we ran the first field experiment, tied to a St. Valentine’s Day E-card web site accessible at three UK universities (Bristol, Essex, and Warwick). This was also advertised as a fundraiser for Right-to-Play, a popular international charity which had been endorsed by the Essex Student Union. Students and staff who completed a survey were randomly selected to win restaurant vouchers and were given the opportunity to donate from this voucher. The site was promoted through extensive flyering, posterage, email lists to members of university organizations, and online media, including Facebook and Twitter. The exact chances of winning were not known in advance; participants only knew the total number of restaurant vouchers

to be given away.¹⁵ We implemented two treatments, which we will refer to as *Before* and *After*, which were assigned orthogonally to other subtle design variations in earlier parts of the Valentine's promotion.¹⁶ Individuals in both treatments were directed to a website informing them that the draw had taken place (so they had already won or lost, though they did not know which).

Before In the *Before* treatment, participants were provided with information about the charity Right-to-Play and asked whether they were willing to donate £1 or more. After making their decision, they proceeded to a page letting them know if they had won. For winners this read "Congratulations you have WON the free dinner for two at [Restaurant name] (value [£30/£20/£20 at Warwick/Bristol/Essex respectively]). Please continue to learn how to claim your prize. For losers this read "Sorry, you have not won". Proceeding to the next page, those in the *Before* treatment received further instructions about how to claim their prize and how to fulfill their pledge.

After In the *After* treatment, participants were first directed to the page where they were informed whether they won. They were then asked to pledge to donate before learning *how* to claim their prize.

In both treatments (as in all subsequent field experiments), the 'ask' required participants make an active decision, with choice architecture that weakly suggested donating. Participants had to either choose a donation amount or *uncheck* a tick-box in order to proceed. Here pledges to donate were *not* binding; a donor had to follow through on her pledge by donating online, at the Student Union office, or at the restaurant itself. However, many did not fulfill their pledges; while 20 students owed a donation, our most inclusive measure suggests that only 12 of these made any sort of donation. In light of this, there are reasonable interpretations of these results, including:

- (I.1) Students in both treatments may have pledged sincerely, and forgotten (and not seen our reminder emails) or found it too effortful to fulfill their small contributions.
- (I.2) The *Before* treatment led to additional sincere pledges. However, at the point they were asked to *fulfill* their contribution, their income was certain and tangible, resembling the *After* treatment. This may have discouraged students from donating in spite of the disutility of cognitive inconsistency.
- (I.3) Many *Before* pledgers never intended to fulfill their commitments; pledges may have been driven by magical thinking, a desire to please the experimenters, or simply carelessness.

Under I.1, *Before* would raise more than *After* if fulfillment were made easier. Similarly, *Before* would raise more under I.1 or I.2 with automatic deduction from prizes. However, under I.3, *Before* pledges (hence donations) under automatic deduction would fall to the level of *After* pledges.

Our evidence from the Prolific experiment—0/30 participants who could have lowered their contributions did so—suggests that I.3 is unlikely. However, as these contexts differed, the choices might have

15. We advertised 75, 25, and 10 restaurant vouchers worth £20, £20, or £30 each at Essex, Bristol, and Warwick, respectively. The actual probabilities (ignoring the few who did not log in to check their prizes) were roughly 82% (75/92) at Essex, 32% (25/77) at Bristol, and 28% (10/36) at Warwick. The precise language (at Essex): "Here you have the opportunity to send Valentine's day E-cards to anyone with an Essex email. Just by logging in and completing the survey, you will get a coupon for 20% off at Naka Thai, and be entered into the draw to win one of 75 vouchers worth £20 for dinner at Naka Thai (on East Hill in Colchester). You do not have to send any E-cards." Again, a set of relevant screenshots is available in the online appendix.

16. In the earlier part, we varied whether a student's identity and her donation (from a prior donation request) would be revealed to the Valentine's e-card recipient(s).

involved a different calculus; thus we will present our pooled results both including and excluding the Valentine's experiment.¹⁷

3.2 Employability

Our "Employability" field experiment was run in 2013/14 in the context of a promotion funded and announced by the University of Essex Faculty of Social Sciences.¹⁸ Participants could win either a £20 Amazon or a £20 dinner voucher. The design resembled the *Valentine's* experiment, with two relevant differences. First, participants *knew* the (25%) probability of winning. Second, committed donations were *automatically* deducted from the value of the voucher (i.e., we would reduce the voucher/certificate amounts by the amount donated). Participants were informed "[...] your donation will be automatically deducted from your prize and passed on to the charity of your choice, plus an additional 10% from our own funds [...]."

Eligible undergraduate students were sent a series of emails, mainly from the departmental administrators, encouraging them to participate, with text such as the following.

Subject: Employability promotion—a 1 in 4 chance of winning a £20 prize for doing a short survey.
Text: Please go to [SITE]—we have 80 free dinners for two in Colchester to give away, worth £20 each and at least 40 £20 Amazon vouchers!! If you log on, you will have a one in four overall chance of winning one of these prizes!

This was also promoted through extensive flyering, posterling, university web sites, and social media. We obtained 352 valid responses that involved a donation choice. No students were allowed to participate more than once. Participants first signed in with their email, department, and study year. Half were then asked to sign up for a job site (JobsOnline) and enter two jobs of interest to them.¹⁹ Next, they were informed of which prize they had a 25% chance of winning (the prize selection was orthogonal to other treatments). After this they were presented our *Before* or *After* donation treatment (screenshots in appendix Figure B.0).

We offered a 10% matching contribution for all donations, and donations were publicly made and recorded on JustGiving, either anonymously or with a message, as the participant wished. We took these steps to offer an incentive to donate *within* the experiment and increase the baseline level of donations, and to reflect typical fundraising campaigns, which often involve matches and social incentives.²⁰

3.3 Omnibus

We ran our "Omnibus" field experiment in June 2017.²¹ Existing ESSEXLab participants were awarded £10 Amazon prizes with 50% probability in exchange for completing a wide-ranging omnibus survey. On

17. Additional features specific to the Valentine's experiment: 1. All were asked whether they were willing to donate £1 or more. This aimed to increase the baseline rate of positive donation and enhance the experiment's power by legitimizing low-value contributions (see Weyant, 1978). 2. Losers were also asked if they wanted to donate (but 0/47 did so). 3. Losers at Essex were still given a small "prize", a 20% discount at Naka Thai. 4. We did not offer a matching contribution. 5. We provided a link for participants to verify the *total* contribution made. 6. There was only a single charity involved.

18. This faculty included Economics, Government, Sociology, Language and Linguistics departments, the Center for Psychoanalytic Studies, and later the Essex Business School. The first run was 4 June 2013 – 21 January 2014. Eligible students were in their first and second years through October 2013, and in their second and third (final) years in following academic year. The next run was 14 May–25 July 2014. We began with this same set of departments (excluding participants in the previous run), and ultimately expanded eligibility to all undergraduate students at the University of Essex, in order to use all of the prizes before our institutional deadline. The online appendix provides further practical details and a timeline.

19. This was one of two additional treatments administered orthogonally to the donation treatments, each for half the subjects. i. This "employability" treatment required half of participants to sign up for a jobs site and enter two jobs of interest. ii. A question and answer treatment asked about rates of employment and salary. The latter "information" treatment occurred *after* the donation treatment. We do not expect that the former treatment would have any effect on donation behavior, and our donation results do not differ substantially by this treatment. More details on these treatments and their assignment ordering are in the online appendix.

20. A copy of the experimental instrument can be tested at <https://goo.gl/qSvhi1>; this will cycle through each of the treatments.

21. A copy of the experimental instrument can be tested at <https://goo.gl/vmHEK6>; this will cycle through each of the treatments.

June 1, nearly the entire pool of 2736 ESSEXLab participants were emailed a direct personalized link with an invitation to take the Omnibus; roughly half of these were randomly assigned to our study, of which we included the first 600 to respond. Non-responders were reminded one and two weeks later. We gave them a deadline of June 18, and provided further information about the Omnibus.²²

Roughly 89% of those who began the survey completed it; median total response time was 12.8 minutes among completers.

After clicking the link, the first screen noted “[...] you have a 50% chance of winning a £10 Amazon voucher. After you complete this survey, we will reveal whether you have won this prize and explain how to claim it.” Aside from the smaller prize and greater chance of winning, this experiment was very similar to the Employability experiment. Chosen donations were automatically deducted from prizes, and participants could choose between two charities, here Oxfam or the British Heart Foundation. The *Before* and *After* treatments were block-randomized (stratified) by gender.

3.4 Prolific

We ran one final web-based experiment, to garner evidence from non-students, to test the importance of the *binding* commitment implied by the automatic deduction, and to consider the sensitivity of the donation to the probability of winning/donation-realization in the *Before* treatment.²³ From July 29 through August 1 2017, we recruited 320 participants through Prolific Academic (prolific.ac), a widely-used recruitment platform for researchers and startups (an additional five participants began but did not complete the survey). We advertised our study as “Employment choices (basic payment plus bonus opportunities)”, promising a base pay of £1 and a duration of about ten minutes (mean actual response time was 5.9 minutes, median 4.3 minutes). We screened for only non-student UK residents, native-English speakers, aged 18 and older, and who had not done any of our previous studies. This left 4212 eligible participants.

The study began with our non-deception rules and a consent form. We next announced their probability of winning the £10 prize (corresponding to the treatments shown below). Next, mainly as a “non-deceptive obfuscation” (Zizzo, 2010) we presented a vignette involving a job interview for an “Assurance Trainee” position, and a series of hypothetical questions about requested and anticipated salary. We next elicited self-reported happiness, followed by a series of unincentivized verbal crystallized intelligence questions. The *Before* and *After* donation questions and prize realizations followed this. Finally they were asked non-incentivized risk-preference, trust elicitation, and (again) happiness questions.

We randomly allocated the following treatments.

Before-50% [80 participants assigned] This *Before* ask, with a 50% chance of winning £10, was similar to the ones in the other web-based experiments. However, unlike in the previous experiments, *Before*-winners (after learning they had won) were reminded of their donation (or non-donation) choice, and told “Would you like to revise your donation decision?” If they chose to revise, they were presented the donation choice once again (with the language from the *After* treatment).²⁴

22. This Omnibus was funded as an ESSEXLab innovation. Survey questions were all unincentivised, and included demographics, psychometrics, political attitudes, and economic beliefs and preferences. These will be connectable to participants’ later laboratory behavior using an anonymized code. The text of the invitation email sent to participants is given in our online appendix.

23. A copy of the full experimental instrument can be viewed and tested at <https://goo.gl/xZWDqg>; this will cycle through each of the treatments. You can type any characters in the box requesting a “Prolific ID”; typing “skip” will skip the vignettes and verbal intelligence questions.

24. As the opportunity to revise was a surprise, and the geographically-dispersed Prolific participants were unlikely to have communicated (even if they had been in the same treatment), we consider the initial donation decision in the *Before*-50% and *After*-

Before-10% [80 participants assigned] Identical to *Before-50%*, but with a 10% chance of winning £10.

After-50% [160 participants assigned, 80 winners] This *After* ask, with a 50% chance of winning £10, was nearly identical to the *After* treatments in the Omnibus and Employability experiments.

While participants in Prolific are aware that they are being paid to participate in *research*, it seems unlikely that our participants realized that our study concerned charitable giving. Participants spent the large majority of their time on the employment and intelligence questions, and essentially only saw a single screen involving charitable giving. Note that Prolific *already* gives their participants the opportunity to donate their base pay to one of two charities after each experiment. In a separate survey of 190 participants (details available by request) we found that 97% of them new about Prolific’s donation option (although only 17% could correctly identify either of the charities Prolific works with). This should further reduce the extent to which our charitable treatments were perceived as experimental. None of our participants donated the base payments from our study, and only 1 of 240 had ever previously donated via Prolific.

AEA registration of design, hypotheses, and analysis plans. We registered our Omnibus and Prolific experiments with the AEA registry in advance of conducting these studies. Our registered plans, both initial and revised, can be found at <https://www.socialscienceregistry.org/trials/2180>.²⁵ We registered a pre-analysis plan, including power calculations, and including our plans to bifurcate our estimates by gender, indicated religiosity, and stated risk-aversion (for the Omnibus) and also by initial stated-happiness (for the Prolific study). As shown below, some of these interactions proved significant (as well as some not pre-registered), but the heterogeneity appears to be entangled with a nonlinear treatment response.

3.5 Laboratory Experiment

As noted above, the laboratory environment permits more control and a wider variety of treatments. The design is shown in Table 2.²⁶ Subjects were seated at computer terminals and given a code number. They next performed a Raven’s matrix task—a language-free multiple choice intelligence test—lasting about half an hour (Raven, 1936). Subjects were told they would be rewarded €7 for this (or €14 in more than half of the *Benchmark* and *Uncertain Collection* treatments) independently of their performance. Next, the *Benchmark* and *Uncertain* subjects were reminded of their earnings, and the rest were told “with a probability of 50 percent you will be rewarded a bonus of €7 on top of your already acquired income of €7.”²⁷

We took steps to demonstrate to the subjects that neither their performance nor their donation choices could affect their chances of winning. Each was given a printed code and pointed to a sealed envelope pinned to the inside of the laboratory door. They were told that the code would determine the “random” outcomes, and that they could check this against the sheet at the end of the experiment. This measure aimed both to rule out a direct material incentive to donate and to reduce the influence of magical thinking for subjects who believe that “karma” can influence future but not predetermined events.

50% to to be comparable to the *Before* and *After* treatments in our other experiments. All results presented below are based on the initial (rather than the revised) donation choices; however, results are not sensitive to this.

25. Some details were revised and registered after the initial registry but before the experiment began. The history of changes to the registration can be seen at <https://www.socialscienceregistry.org/trials/2180/history/19934>. Additional common sense small changes were made after the experiment began for feasibility reasons, as noted.

26. In the first wave of lab experiments we also included a “*Before Both*” treatment. Results are not sensitive to this: Table C.12 shows this treatment had similar effects as *Before*; further results are available upon request.

27. For those whose income was deterministic (*Benchmark* and *Uncertain*) and never expressed as a probability, we assigned more than half to the higher income. This allowed us greater power to distinguish between treatments from donation commitments from €14.

Table 2. Laboratory - Experimental design

		<i>Certain treatments</i>		<i>Probabilistic treatments</i>
Treatment		<i>Benchmark</i>	<i>After</i>	<i>Before</i>
<i>Income</i>		Known	Bonus lottery	Bonus lottery
<i>Donation</i>		Certain	Certain	Probabilistic
<i>Stage</i>				
0	<i>Real-effort</i>	Task	Task	Task
1	<i>Learn income</i>	€7 or €14	€7	€7
2	<i>Bonus info</i>	No info	Possible €7 bonus	Possible €7 bonus
3	<i>Message 1</i>	Reminded flat-rate income	Learn bonus outcome (<i>w</i> or <i>l</i>)	<i>None</i>
4	<i>Giving decision</i>	Giving decision	Giving decision	Giving decision
5	<i>Message 2</i>	<i>None</i>	<i>None</i>	Learn bonus outcome (<i>w</i> or <i>l</i>)
6	<i>Belief elicitation and questionnaire</i>			

Before Those in the *Before* treatment were given a chance to conditionally donate, before learning if they won the bonus, with the text (translated from German):

In case of you winning the bonus of € 7, we now want to give you the opportunity to donate a part of the income you have earned in this experiment to a charitable organization. In doing so, you can choose between “Brot fur die Welt” (Bread for the World) and the “World Wildlife Fund (WWF)”. [...] Please enter the amount of your donation in case of you winning the bonus (amount can be between € 0 and € 14). In case of you not winning the bonus, nothing will be deducted from your income and the organization will not receive a donation [...]

After *After* treatment subjects were first informed whether they received the bonus and then were given the opportunity to donate to the above organizations, with similar language as above.

Benchmark *Benchmark* subjects were also asked to donate from their (known) income, with virtually identical language as for the *After* subjects.

Uncertain Collection *Uncertain* subjects were told that with a probability of 50% they would have the opportunity to donate, and asked to enter a donation in case of you being able to donate.

All subjects were told “we will increase your donation by an additional 25 percent taken out of our own budget.” Following the donation decision, we asked the subjects to make a series of incentivized and hypothetical predictions, followed by survey questions.²⁸ Finally, we revealed net earnings, and revealed to the *Uncertain Collection* subjects whether their donations would be collected. We opened the sealed envelope to demonstrate that the random draws had indeed been pre-determined. Payments were made and donations passed to the charities, with a subject monitor, as promised.

These experiments were run in Düsseldorf and Mannheim on a standard experimental subject pool (recruitment was conducted via ORSEE; Greiner, 2004), using virtually identical protocols and zTree code

28. We first asked them to predict for what others donated; subjects were informed that they would be given €0.50 per answer that was within €1 of the correct answer. First, they were asked guess the average overall donation. We next told them the two possible earnings asked them to guess the average contribution from each level of earnings. Finally, we asked them a hypothetical question: what would their own donations have been had they earned the other income/bonus amount? Details of this part of the design, the incentives, and the results, are available by request. [Note to referee: included in supplemental materials submitted] These results are largely of independent interest, and not strongly related to the main question of this paper.

at each lab (Fischbacher, 2007). We ran nine sessions over five days in January–February 2013 and November 2014, and 24 sessions over 7 days in March–April 2016. A complete set of relevant screen shots and translations are available in our online appendix.

To protect anonymity, we were careful to ensure that lab subjects never learned *each other's* earnings or contributions, and we never connected an individual's identity to her treatment or her choices. Still, as noted in the introduction, we cannot rule out a signalling motive. In making payments, we (the experimenters) could infer how much each subject earned, which would have allowed us to make a probabilistic inference about her likely contribution. Subjects may have anticipated this, implying a possible “signaling to the experimenter” motivation. Furthermore, subjects may want to discuss their lab experience with others afterward. If it is common-knowledge that lying brings a strong internal moral cost, *reported* choices may hold a similar signaling power as actual verifiable choices. Finally, previous work suggests that subjects often bring real-world norms and heuristics into the laboratory (e.g. Burns, 1985; Hoffman et al., 1996).

Summary statistics and randomization tests. In the appendix we present summary statistics along with evidence that our randomization successfully balanced the treatments. Appendix Tables C.1 through C.5 compare the two treatments for the web-based experiments and the four treatments from the laboratory experiment. The means values of observable variables are similar across treatments, and we do not detect significant differences for the great majority of tests. In the Prolific experiment, there is some imbalance by age: those in the Before trial are significant older. However, our results are barely affected by including controls for age (cf. Table 7). The difference is likely due to an unlucky draw and multiple hypothesis testing. As reported above, attrition was tiny: only 5 of 325 participants who began the Prolific survey quit (typically before reaching the prize/donation stage) or timed out.

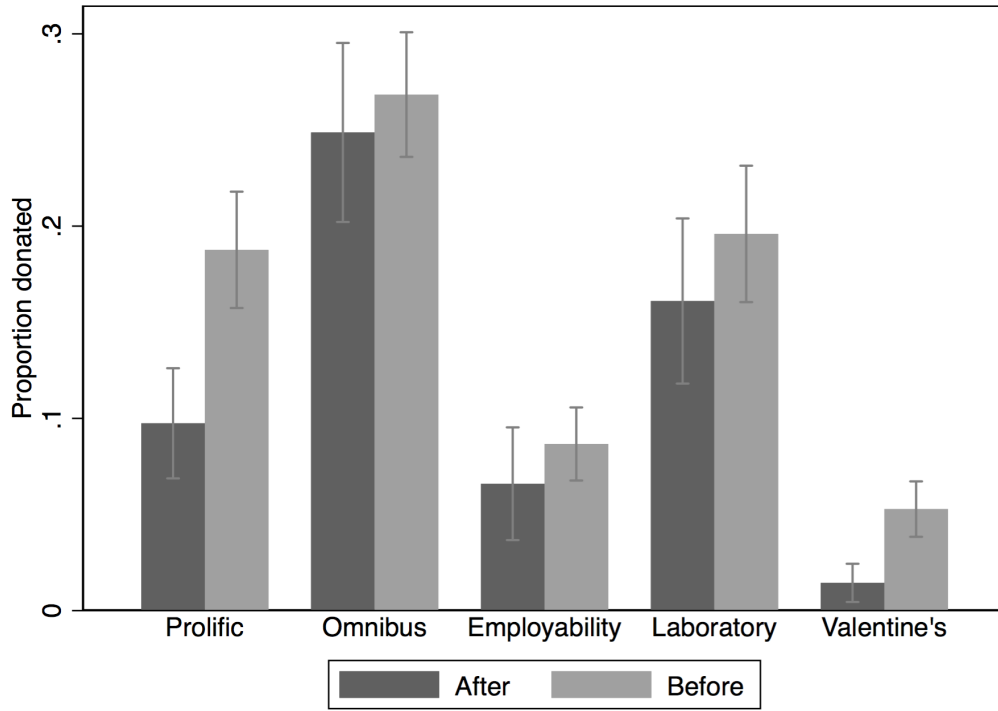
4 Results

We first compare the donations in the Before and After treatments in each experiment. As seen in Figure 1, in each experiment average donations were higher in the Before treatment relative to the After treatment (although the differences are sometimes within the conventional standard-error margin). Table 3 reports the incidence of donating a positive amount, as well as the shares of the endowments donated by treatment, for each experiment, and pooling across experiments.²⁹

Table 4 reports ordinary least squares regressions on donation levels (above) and incidence (below) for the Before treatment versus the After treatment in each experiment, excluding participants who earned the lower level of income (or who failed to win the prize).

Each of our experiments suggest an effect in the same direction—a positive effect of “asking before” on donation behavior. The impact is particularly large and highly-significant in the *Prolific* experiment, suggesting that the *Before* treatment might be most effective for non-students. However, in both regression analysis (Table 4) and in simpler tests (Table 3), in about half of our experiments this fails to reach conventional levels of statistical significance. This may stem from a lack of power (noted in AEA-registered power tests). As shown in Table C.15, the between-subject variance in donation behavior is large, both here and in previous work. For all regression tables we present 95% confidence intervals to convey the precision of

29. All donations are reported in Euro. Donations from the field experiment in the UK are evaluated at an exchange rate of 1.1971 EUR/GBP (October 1, 2013 rate). For comparability across experiments, in this subsection we do not report on donations from the lower income in the lab (recall that losers in the most of the field experiments were not asked to donate).



Bars: estimated standard deviations.

Figure 1. Mean share committed by experiment, by Before vs. After

Table 3. Summary Statistics: Shares of endowments donated by treatment

	Pooled	Prolific	Omnibus	Employability	Laboratory	Valentine's
After						
Incidence	0.42	0.40	0.50	0.31	0.64	0.13
Mean	0.15	0.10	0.25	0.07	0.16	0.01
Median	0.00	0.00	0.05	0.00	0.14	0.00
75 th pctl	0.20	0.20	0.50	0.05	0.29	0.00
Std. dev.	(0.26)	(0.15)	(0.35)	(0.15)	(0.19)	(0.04)
Before						
Incidence	0.48	0.60	0.56	0.31	0.74	0.35
Mean	0.17	0.19	0.27	0.09	0.20	0.05
Median	0.00	0.10	0.10	0.00	0.14	0.00
75 th pctl.	0.25	0.30	0.50	0.05	0.29	0.05
Std. dev.	(0.27)	(0.23)	(0.34)	(0.20)	(0.19)	(0.09)
Total						
Incidence	0.46	0.53	0.54	0.31	0.70	0.28
Mean	0.16	0.16	0.26	0.08	0.18	0.04
Median	0.00	0.10	0.10	0.00	0.14	0.00
75 th pctl.	0.20	0.20	0.50	0.05	0.29	0.05
Std. dev.	(0.26)	(0.22)	(0.35)	(0.19)	(0.19)	(0.08)
Diff. in incidence	-0.06	-0.20	-0.06	-0.00	-0.10	-0.22
p-value (Fisher)	0.05	0.00	0.28	1.00	0.25	0.00
Diff. in means	-0.02	-0.09	-0.02	-0.02	-0.03	-0.04
p-value (rank sum)	0.05	0.00	0.32	0.81	0.14	0.00
p-value (t-test)	0.18	0.00	0.57	0.33	0.30	0.00
Observations	1363	240	460	375	129	159

Notes: Average proportion of earnings donated for the Before treatment versus the After treatment in each experiment. For the Lab experiment, this excludes data from the *Certain*, *Before-Both*, and *Uncertain* treatments. We exclude all participants with the lower earnings (lab) and all those in the *After* treatments who did not win the prizes (and thus were not asked to donate). "Incidence": share donating a positive amount. At the bottom we report p-values for tests of differences between outcomes in Before and After treatments, from exact-tests (for incidence) and for rank-sum and t-tests (for proportion donated).

Table 4. Ordinary Least Squares Regressions: Donations levels and incidence by experiment

Panel A: Levels	Prolific	Lab	Employability	Omnibus	Valentines
Before	0.90*** [0.40,1.40]	0.85* [-0.16,1.86]	0.49 [-0.50,1.49]	0.24 [-0.57,1.05]	0.92*** [0.42,1.42]
Constant	0.97*** [0.63,1.32]	1.88*** [1.29,2.47]	1.58*** [0.75,2.42]	2.98*** [2.31,3.64]	0.35** [0.061,0.63]
Observations	240	129	375	460	159
Panel B: Incidence	Prolific	Lab	Employability	Omnibus	Valentines
Before	0.20*** [0.068,0.33]	0.097 [-0.099,0.29]	0.0033 [-0.11,0.12]	0.056 [-0.041,0.15]	0.21*** [0.080,0.34]
Constant	0.40*** [0.29,0.51]	0.65*** [0.50,0.80]	0.31*** [0.20,0.41]	0.50*** [0.42,0.58]	0.13*** [0.041,0.23]
Observations	240	129	375	460	159

Notes: This table reports coefficients and 95% confidence intervals from ordinary least squares regressions on donation levels (top) and incidence (bottom) for the Before treatment versus the After treatment in each experiment. These results exclude participants with the lower level of income. The Lab column also includes a hidden dummy for the laboratory location. Logistic regression results for incidence models yield similar results (see appendix).

our estimates, and to allow inference about the bounds of our effect. In many cases these bounds are wide; e.g., for the Omnibus trial the effect is bounded above at over $1/3$ of the mean donation.

We pool our data across all of our experiments to perform a meta-analysis, allowing greater statistical power. For statistical inference, we consider this as a draw from a population composed of likely participants in each of our experiments, with shares corresponding to our relative sample sizes of UK students, German students, and UK nonstudents. In Figure 2 we present a histogram of the shares of endowment donated, pooled over all experiments. This reveals a small shift away from zero donations towards moderate donations. In order to take into account potential session-specific correlated errors (for the lab experiments) and date-specific correlated errors (for the field experiments) we use cluster-robust standard errors at these levels. All regressions (except where noted) include de-meaned dummies for each experiment, and the interactions of these with the *Before* treatment. This estimator recovers the *average* treatment effect (ATE) for our source population in the presence of heterogeneity. In contrast, OLS estimators are more efficient if effects are homogenous, but they achieve this efficiency by (over)weighting observations (relative to shares of the source population) with higher conditional variance in the treatment and less residual variance in the outcome variable. With heterogeneous treatment effects this yields an arbitrarily weighted estimate of treatment effects (Angrist J. D. and J. S. Pischke, 2008, p. 58), while the "fully interacted" estimator recovers the ATE (see Athey et al., 2017, equation 5.4). However, our results are similar with or without these interactions, as well as with additional interactions by specific pre-determined variables (table C.8).³⁰

As shown in Table 5, the difference for the Before and After treatments is strongly statistically significant in the pooled data and the pooled 95% confidence interval is between 1% and 6% percent of the endowment, implying a proportional increase of 6%-38% of the average donation rate.

30. We provide robustness checks with specifications of all models in the appendix. Variable-selection models (elastic-net and lasso; details upon request) with penalties for non-treatment pre-determined control variables also yielded similar results.

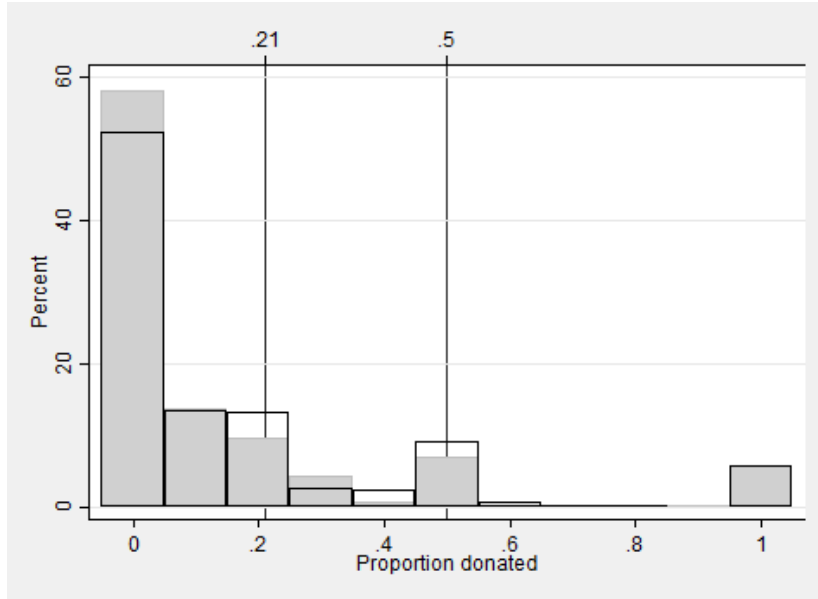


Figure 2. Histograms of shares donated, pooled over all experiments

Notes: Average proportion of earnings donated for the Before treatment versus the After treatment, pooling across all experiments. For the Lab experiment, this excludes data from the *Certain*, *Before-Both*, and *Uncertain* treatments. We exclude all participants with the lower earnings (lab) and all those who did not win the prizes. The vertical lines indicate the 75th and 90th percentile of the pooled donation share.

Table 5. Ordinary least squares regressions on Donations: Pooled over experiments

	Pooled			Pooled w/o Valentines		
	(1) Proportion	(2) Level	(3) Incidence	(4) Proportion	(5) Level	(6) Incidence
Before	0.035*** [0.010,0.061]	0.53*** [0.181,0.872]	0.097*** [0.053,0.140]	0.042*** [0.016,0.067]	0.52*** [0.178,0.870]	0.095*** [0.047,0.143]
Constant	0.14*** [0.120,0.155]	1.90*** [1.636,2.159]	0.43*** [0.393,0.474]	0.15*** [0.130,0.165]	1.97*** [1.711,2.235]	0.47*** [0.424,0.506]
Observations	1363	1363	1363	1204	1204	1204

Notes: This table reports coefficients and 95% confidence intervals from ordinary least squares regressions. Dependent variables are donations as shares of endowment, donation levels, donation incidence for the Before treatment versus the After treatment. We run pooled analysis comparing all experiments (Columns 1 to 3) and excluding the Valentine's experiment (Columns 4 to 6). All regressions include de-meaned dummy controls, which subsume a control for differing endowments, as well as the interactions of these with the Before treatment. We account for potential session-specific correlated errors (for the lab experiments) and date-specific correlated errors (for the web-based experiments) by cluster-robust standard errors at these levels. Results of t-tests indicated at following significance levels * $p < 0.1$, ** $p < .05$, *** $p < .01$. Logistic regression results for incidence models yield similar results (see appendix). For the continuous outcomes, negative binomial models yielded similar results (see appendix, Table C.11).

Result 1. Overall, the Before treatment increased the average amount donated relative to the After treatment.

Result 2. Overall, participants were more likely to give in the Before relative to the After treatment.

Columns 3 and 6 of Table 5 report the coefficients from a linear probability model for the incidence of giving (logistic and probit specifications lead to similar significance levels and estimated marginal effects.) Pooling across all experiments, the *Before* treatment has a significant impact on the extensive margin response, whether or not we limit the sample to experiments with automatic deduction only, i.e. excluding the Valentine's experiment.

Table 6. Quantile regressions on Donation shares: Pooled over experiments

	Quantile				
	50	60	70	80	90
Before	0.32** [0.04,0.60]	0.38* [-0.03,0.79]	0.84** [0.18,1.50]	0.40 [-1.92,0.99]	0.47* [-0.05,0.98]
Constant	0.53*** [0.29,0.75]	1.01** [0.72,0.14]	1.30*** [0.77,1.83]	2.64*** [2.16,3.12]	4.67*** [4.26,5.10]
Observations	1363	1363	1363	1363	1363

Notes: This table reports coefficients and 95% confidence intervals from quantile regressions of donations as shares of the endowment; quantiles indicated at top. Data is pooled over all experiments, excluding donations from the lower income level. All regressions include de-meaned dummy controls, as well as the interactions of these with the Before treatment. Results of t-tests indicated at following significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

For the continuous outcomes the true data generating process must be nonlinear, as giving can never be negative. For robustness to functional form mis-specification, we estimated a negative binomial model paralleling Table 5, presented in Table C.11 in the appendix. This method is more robust to heterogeneity than the Tobit specification (Gourieroux C. et al., 1984; Greene, 1994). In each case the results are consistent with our linear regressions.

In Table 6 we present quantile regression results, allowing us to infer the effects of the *Before* treatment on the donation outcome *distribution* (but, as widely noted, this does not necessarily identify the "distribution of the treatment effect"). We find an increase for each of the 5th to 9th deciles, statistically significant for all but the 8th), and the strongest effect on the 7th decile. (The 7th decile donation share is 14.2% of the endowment in the After treatment and 20% in the before treatment; see above histogram.)

4.1 Heterogeneity of effects

Pooled data: Gender, age, religiosity. In Table C.8 in the appendix we report regressions with the Before treatment interacted with key pre-determined observables.³¹ As we de-mean each of the binary interacted terms, the base coefficient remains a consistent estimator of the average treatment effect, while the interaction terms represent treatment effect differences between groups (see Athey et al., 2017).

Much previous work has found gender differences in the levels and determinants of other-regarding behavior, in their sensitivity to “price” and cost/benefit ratio (Andreoni, Vesterlund, 2001; Cox et al., 2006), in their response to the time delay (Bremner, 2011), and in their sensitivity to reporting, prestige, competition, and previously-reported donations (Meier, 2007; Pan et al., 2011; Jingping, 2013; Jones et al., 2014). Women in our experiments donate more than men in the After treatment, and the effect of the Before treatment is somewhat smaller for women.

Overall, Table C.8 suggests substantial heterogeneity in treatment effects by age and gender, and these interactions are sometimes significant.³² However, most categories with a higher baseline dummy (rep-

31. Where missing, these variables are linearly imputed using other variables in this regression. The results (available by request) are not sensitive to this imputation. The results are also similar when we introduce each interaction in isolation.

32. Note that the first three columns of these models include treatment-experiment interactions to avoid confounding heterogeneity with differences in the range of demographics by experiment. These interactions are hidden to save space; several of these experiment-treatment interactions are significant even after the demographic controls; details available by request. Note that the *religiosity* interaction is not significant when we include experiment interactions and dummies. This weakly suggests that "magical thinking" is not driving our result; however, the confidence intervals for this interaction are large, implying limited power to detect a difference.

resenting a higher mean in the After treatment) have a negative interaction term, and vice-versa. When baseline outcome levels vary between groups, it is difficult to distinguish heterogeneity from nonlinear treatment effects. Our evidence is also consistent with a smaller impact of the *Before* treatment for more generous individuals. This may be explained by a steeply diminishing marginal utility of donations in this context. In the appendix Table C.9 we provide results from a power model offering evidence of this nonlinear relationship. Table C.10 reveals that the demographic interaction coefficients lose their significance in this nonlinear specification.³³ This is also consistent with the histogram (Figure 2), which suggests shifts from the smallest donations to medium donations, but no shift towards the largest donations.

4.2 Further treatment comparisons and experiment-specific results

Happiness/affect, and donations. In the Prolific and Omnibus experiments, after (but not immediately after) the prize determination and donation questions we elicited a standard measure of happiness; we report relevant results in table C.7. Unsurprisingly, those who failed to win stated a significantly lower level of happiness (about $\frac{3}{4}$ of a standard deviation). However, it does not appear that this increased happiness is driving those (winners) in the After treatment to donate more. In the Prolific experiment we also asked the same happiness question near the beginning of the survey. We find a tightly bounded near-zero relationship between this earlier happiness measure and the chosen donation; the 95% confidence interval is less than $\frac{1}{4}$ of a standard deviation.

Other treatments. Our laboratory design allows a richer set of treatment comparisons. However, other than the result discussed above (Before versus After) we find insignificant differences in giving from the higher level of earnings between individual treatments, and the large variance in responses and the wide and overlapping confidence intervals suggests a lack of statistical power to distinguish among these. We report on these results in the appendix (Figure C.11 and Table C.12).³⁴

Probabilities of winning (Prolific). The Prolific experiment also included a *Before* treatment with a 10% chance of winning. We find (Table 7) very strong effects of both *Before* treatments relative to the *After* treatment (£0.96 in the 50% treatment and £0.60 in the 10% treatment, significant at the 1-percent and 5 percent levels, respectively). The difference between these two *Before* treatments is not statistically significant, and the confidence intervals reveal limited power to distinguish these.³⁵

We control for two background variables—these come from Prolific screener" questions, which participants are asked to answer when they first sign up for Prolific, and throughout the months and years they are registered. Thus, these were likely answered well in advance of our study, minimizing any possible contamination. We see a near-zero relationship to de-meaned age. However, we find a strong correlation

33. We also ran a two-step procedure (i) regressing giving on pre-determined observables and generating a prediction using After-treatment data, and (ii) regressing giving in the Before treatment on this predicted value and demographics. Again the results (available by request) are consistent with a diminishing-returns treatment effect, and show little evidence of direct heterogeneity by demographics.

34. For the Before-both treatment, where the subjects are asked to make pair of conditional choices, one for the each income state, we also find a lower level and incidence of donation from the *lower* level of income relative to the benchmark (as well as relative to the Before-both choice for the winning state). As noted above, there are multiple interpretations of Before-both choices, so we do not highlight this result. Finally, we find that subjects with the lower income are less likely to donate in the *Uncertain-collection* treatment relative to the benchmark. This loosely suggests that the signaling model is not driving responses; however alternative explanations are possible, and this is significant only at $p < 0.10$, and we did not try to replicate this in other experimental environments.

35. Recall that the signaling model predicts that if a 50% realization of a donation choice yields a larger (good-type) commitment than does an ex-post (100%) choice, a 10% probability of realization must lead to a donation that is still larger. Thus, to the extent that we can rule out a substantially *larger* average donation in the Before-10 treatment relative to Before-50, this speaks against the signaling model.

between giving in our experiment and the participant's response to the question "how much, if anything, did you donate to charity in the last 12 months?"; this supports the relevance and generalizability of our results to external environments.

Table 7. Prolific: Linear models of donation levels and incidence

	Levels	Incidence
	(1)	(2)
– Before	0.96*** [0.30,1.62]	0.17** [0.014,0.32]
– Before 10%	-0.36 [-1.09,0.37]	0.043 [-0.11,0.19]
Age (centered)	0.039 [-0.020,0.097]	0.001 [-0.010,0.012]
Not previous donor (de-meaned)	-1.04*** [-1.57,-0.50]	-0.34*** [-0.48,-0.19]
Before-10% summed	0.60** [0.02,1.18]	0.21*** [0.06,0.37]
Constant	1.01*** [0.66,1.36]	0.39*** [0.28,0.50]
Observations	230	230

Notes: This table reports coefficients and 95% confidence intervals for regressions on donations by Treatment for the Prolific experiment. The first row coefficient is the impact of the Before-50% treatment, and the second row the adjustment to this impact for the Before-10% treatment. Regression controls for Prolific background variables: age (de-meaned and imputed if missing) and self-reported non-giver; results are similar without controls. Dependent variables are (1) the levels of donations in Euros (2) donation incidence. We report robust standard errors. Results of t-test indicated at the following significance levels * $p < 0.1$, ** $p < .05$, *** $p < .01$.

Dynamic consistency and fulfillment of pledges. As noted earlier, in the Valentine's experiment, only 12 of 20 winners who pledged a donation followed this up by fulfilling it. However, in the Prolific experiment, which allowed Before-winners to revise their donation choice, of the 30 who had pledged a positive amount, none chose to reduce this after winning, and four chose to increase it. Furthermore, 2/19 of the Before-winners who had *not* pledged chose to revise their decision to a positive amount. Considering these as a random draw from a larger population, we can infer that there is less than a 5% probability that 9.5% or more of the source population would cancel or reduce their donation (exact binomial test, $n = 30$, $K = 0$).

4.3 Synthesis

Revisiting Table 4, the greater giving in the *Before* versus *After* treatments in the lab and field is consistent with both loss-aversion (under an expected-wealth or intermediate reference point, which immediately adjusts) and with the signaling model. It is inconsistent with expected utility over outcomes, with the affective mood argument, or with loss-aversion with a slowly-changing (or unchanging) reference point.

Our evidence from the Prolific experiment suggests that a *legally-binding* pledge may not be necessary for *give-if-you-win* to be a successful fundraising strategy. This finding is consistent with Breman's (2011) field experiment, in which very few donors deviated from their previously committed (increases in) contributions, even though deviating only required a small effort.

4.4 Further alternative explanations

Participants may have non-standard beliefs about probabilities and randomness. In particular, they may believe that their commitment to contribute will increase their likelihood of winning. This may stem from

“magical thinking”, an illusion of control (see seminal article by Langer, 1975, and the literature following it) or exhibiting “just world beliefs” (Rubin et al., 1975). An individual who believes in Karma (cf. Levy et al., 2006) may believe she will be rewarded for good acts (or good commitments) and punished for bad ones.³⁶ While we can not rule this out, we emphasize in each experiment that stochastic outcomes have been determined by random draw *prior* to their donation choices. We also differentiate our results by measures of stated religious affiliation, finding no significant differences (however, our sample yielded limited power to detect an effect). Several other behavioral models and concerns could also predict donation behavior distinct from the standard expected utility model, including adaptation, tangibility, present-bias, a status-quo reference point, and uncertainty aversion. We discuss these in the online appendix, arguing that these are less relevant than the models presented above, and are not supported by our evidence.

5 Conclusion

Our experiments are the first to document the effect of the resolution of income uncertainty on other-regarding behavior, augmenting existing evidence that such behavior may not be well-explained by outcome-based expected utility theory. As noted, this also has an important implication for experimental methods: many experiments used a “random problem selection mechanism” (Azrieli et al., 2012), selecting only a single decision stage for payment, arguing that this ensures no feedback between stages. This may be violated: e.g., in a dictator game, if an earlier stage’s incentives prompted a generous commitment, this might satiate the desire for signaling and lead to lower commitments in later stages (as seen in Tonin et al., 2014 and Reinstein, 2010). The implications for the *strategy method* (Selten, 1967) are similar; in making such decisions, subjects may trade off the costs and benefits of signaling between contingencies.

We find higher donations and a greater propensity to commit to donate when individuals are asked to conditionally commit before learning if they have won a prize or bonus, relative to those asked after they have won. This result is statistically significant ($p < .01$) in pooled data and in three of five distinct contexts ($p < .01$ for two contexts and $p < .10$ for one context) across several different populations (Essex students, UK nonstudents, German students). The magnitude of this effect is within the range of effects estimated in other charitable giving experiments (see appendix C.7). The effect is stronger for those groups predicted to donate less in the *After* treatment. However, it may not carry over into every situation; in other environments asking after a bonus may be more effective; perhaps affect/mood may dominate. Allowing for heterogeneous motivations, the theory presented is ambiguous, suggesting that results may vary according to the environment. Still, our evidence strongly suggests that in relevant environments contributions involving uncertain realization and/or uncertain income do not follow the predictions of standard expected utility models.

Although our experiments are of a limited scale, they may be relevant to other forms of prizes, be they from gambling, state lotteries, or workplace bonuses. Some sectors, most famously the financial sector, offer substantial bonuses, the exact magnitude of which are often unclear ahead of time. Our findings suggest that asking workers to commit to give a share of their bonus (or their “bonus in excess of a specified

36. Participants may donate more before if they believe that a spiritual force affects their winning probabilities; but it is not clear whether in the *Before* treatment she will give conditional or unconditionally. She may want to appease the gods by saying “I will donate anyway,” or she may want to give them an incentive to make her a winner by making her donation conditional on a win. Similarly, she may donate more *After* out of a sense of gratitude towards this spiritual force. (As this is difficult to pin down, we did not include it in the table.)

expectation”) could be an effective revenue generator for charities. In many countries, including the USA, tax rebates at the end of a fiscal year are both common and uncertain in magnitude, offering another potential application. In general, policymakers could promote “windfall giving”, and help clarify the legal environment surrounding commitments of uncertain income.

References

- Andreoni, J., A. Payne (2003), Do government grants to private charities crowd out giving or fund-raising? *American Economic Review* 792–812.
- Andreoni, J., J. Rao (2011), The power of asking: How communication affects selfishness, empathy, and altruism, *Journal of Public Economics* 95 (7) 513–520.
- Andreoni, J. (1990), Impure Altruism and Donations to Public Goods: A Theory of Warm-Glow Giving, *The Economic Journal* 100 464–477.
- Andreoni, J., M. Serra-Garcia (2016), Time-Inconsistent Charitable Giving, technical report, National Bureau of Economic Research.
- Andreoni, J., L. Vesterlund (2001), Which is the fair sex? Gender differences in altruism, *Quarterly Journal of Economics* 116 (1) 293–312.
- Angrist J. D. and J. S. Pischke (2008), *Mostly Harmless Econometrics : An Empiricist ’ s Companion*, March 290.
- Athey, S., G. W. Imbens (2017), Chapter 3 - The Econometrics of Randomized Experimentsa, in: *Handbook of Economic Field Experiments*, edited by A. V. Banerjee, E. Duflo, volume 1, Supplement C volumes, *Handbook of Field Experiments*, DOI: 10.1016/bs.hefe.2016.10.003, North-Holland 73–140, URL: <http://www.sciencedirect.com/science/article/pii/S2214658X16300174> (visited on 09/25/2017).
- Atkinson, A. B. (2009), Giving overseas and public policy, *Journal of Public Economics* 93 (5-6) 647–653.
- Azrieli, Y., C. P. Chambers, P. J. Healy (2012), Incentives in experiments: A theoretical analysis, technical report, Working Paper, Ohio State University.
- Becker, G. (1974), *A Theory of Marriage: Part II*.
- Benabou, R., J. Tirole (2006), Incentives and Prosocial Behavior, *American Economic Review* 96 (5) 1652–1678.
- Benabou, R., J. Tirole (2011), Identity, morals, and taboos: Beliefs as assets, *The Quarterly Journal of Economics* 126 (2) 805–855.
- Breman, A. (2011), Give more tomorrow: Two field experiments on altruism and intertemporal choice, *Journal of Public Economics* 95 (11) 1349–1357.
- Brock, J. M., A. Lange, E. Y. Ozbay (2013), Dictating the risk: Experimental evidence on giving in risky environments, *The American Economic Review* 103 (1) 415–437.
- Burns, P. (1985), Experience and Decision Making, *Research in Experimental Economics*, (ed. V. Smith) 3.
- Camerer, C., L. Babcock, G. Loewenstein, R. Thaler (1997), Labor supply of New York City cabdrivers: One day at a time, *The Quarterly Journal of Economics* 112 (2) 407–441.
- Cialdini, R. B., B. L. Darby, J. E. Vincent (1973), Transgression and altruism: A case for hedonism, *Journal of Experimental Social Psychology* 9 (6) 502–516.
- Comptroller, N. S. (2014), Wall Street Bonuses Went Up In 2013, Press release. online, URL: <http://www.osc.state.ny.us/press/releases/mar14/031214.htm>.
- Cox, J. C., C. A. Deck (2006), When are women more generous than men? *Economic Inquiry* 44 (4) 587–598.
- Cubitt, R. P., C. Starmer, R. Sugden (1998), On the validity of the random lottery incentive system, *Experimental Economics* 1 (2) 115–131.
- DellaVigna, S., J. List, U. Malmendier (2012), Testing for Altruism and Social Pressure in Charitable Giving, *The Quarterly Journal of Economics* 127 (1) 1–56.
- Drouvelis, M., B. Grosskopf (2016), The effects of induced emotions on pro-social behaviour, *Journal of Public Economics* 134 1–8.

- Duncan, B. (2004), A Theory of Impact Philanthropy, *Journal of Public Economics* 88 (9-10) 2159–2180.
- Eckel, C. C., P. J. Grossman (1996), Altruism in Anonymous Dictator Games, *Games and Economic Behavior* 16 (2) 181–191, URL: <http://www.sciencedirect.com/science/article/pii/S0899825696900810> (visited on 08/21/2017).
- Fischbacher, U. (2007), z-Tree: Zurich toolbox for ready-made economic experiments, *Experimental Economics* 10 (2) 171–178.
- Fishbach, A., A. A. Labroo (2007), Be better or be merry: How mood affects self-control, *Journal of Personality and Social Psychology* 93 (2) 158.
- Gourieroux C., A. M., A. Trognon (1984), Pseudo Maximum Likelihood Methods: Applications to Poisson Models, *Econometrica* 52 701–720.
- Greene, W. H. (1994), Accounting for excess zeros and sample selection in Poisson and negative binomial regression models.
- Greiner, B. (2004), An online recruitment system for economic experiments, GWDG Bericht 63, edited by K. K. Macho 79–93.
- Grossman, Z. (2015), Self-signaling versus social-signaling in giving, *Journal of Economic Behavior & Organization* (Forthcoming).
- Harbaugh, W. T. (1998), The Prestige Motive for Making Charitable Transfers, *The American Economic Review* 88 (2) 277–282, URL: [http://links.jstor.org/sici?sici=0002-8282\(199805\)88:2%3C277:TPMFMC%3E2.0.CO;2-9](http://links.jstor.org/sici?sici=0002-8282(199805)88:2%3C277:TPMFMC%3E2.0.CO;2-9).
- Harrison, G., J. List (2004), Field Experiments, *Journal of Economic Literature* 42 1009–1055.
- Höchtel, W., R. Kerschbamer, P. Kircher, S. Ludwig, A. Sandroni (2015), The Perils of Advocating Transfers.
- Hoffman, E., K. McCabe, V. Smith (1996), Social Distance and Other-Regarding Behavior in Dictator Games, *American Economic Review* 86 653–660.
- Huck, S., I. Rasul (2011), Matched fundraising: Evidence from a natural field experiment, *Journal of Public Economics* 95 (5) 351–362.
- Jingping, L. (2013), Four Essays on the Economics of Pro-Social Behaviors.
- Jones, D., S. Linardi (2014), Wallflowers: Experimental Evidence of an Aversion to Standing Out, *Management Science* 60 (7) 1757–1771.
- Karlan, D., J. A. List (2007), Does Price Matter in Charitable Giving? Evidence from a Large-Scale Natural Field Experiment, *American Economic Review* 97 (5) 1774–1793, URL: <https://www.aeaweb.org/articles?id=10.1257/aer.97.5.1774> (visited on 08/21/2017).
- Kidd, M., A. Nicholas, B. Rai (2013), Tournament outcomes and prosocial behaviour, *Journal of Economic Psychology* 39 387–401.
- Kotzebue, A. v., B. U. Wigger (2009), Charitable Giving and Fundraising: When Beneficiaries Bother Benefactors, in: *XVI Encuentro de Economía Pública: 5 y 6 de febrero de 2009: Palacio de Congresos de Granada* 48.
- Kuhn, P., P. Kooreman, A. Soetevent, A. Kapteyn (2011), The effects of lottery prizes on winners and their neighbors: Evidence from the Dutch Postcode Lottery, *American Economic Review* 101 (5) 2226–2247.
- Langer, E. J. (1975), The illusion of control. *Journal of personality and social psychology* 32 (2) 311.
- Levin, P. F., A. M. Isen (1975), Further studies on the effect of feeling good on helping, *Sociometry* 141–147.
- Levy, G., R. Razin (2006), A Theory of Religion: Linking Individual Beliefs, Rituals, and Social Cohesion, Department of Economics WP, LSE.
- Maniadis, Z., F. Tufano, J. A. List (2014), One Swallow Doesn't Make a Summer: New Evidence on Anchoring Effects, *American Economic Review* 104 (1) 277–90, URL: <http://www.aeaweb.org/articles.php?doi=10.1257/aer.104.1.277>.
- Meier, S. (2007), Do women behave less or more prosocially than men? Evidence from two field experiments, *Public Finance Review* 35 (2) 215–232.
- Pan, X. S., D. Houser (2011), Competition for trophies triggers male generosity, *PloS one* 6 (4) e18050.
- Raven, J. (1936), Mental tests used in genetic studies: The performance of related individuals on tests mainly educative and mainly reproductive, Unpublished master's thesis, University of London.

- Reinstein, D. (2010), Substitution Among Charitable Contributions: An Experimental Study.
- Reinstein, D. A. (2011), Does One Charitable Contribution Come at the Expense of Another? *The BE Journal of Economic Analysis and Policy* 11 (1).
- Reinstein, D., G. Riener (2012a), Decomposing desert and tangibility effects in a charitable giving experiment, *Experimental Economics* 15 (1) 229–240, (visited on 08/17/2017).
- Reinstein, D., G. Riener (2012b), Reputation and influence in charitable giving: an experiment, *Theory and Decision*; Dordrecht 72 (2) 221–243, URL: <https://search.proquest.com/docview/916001281/abstract/507947BE86B14E51PQ/1> (visited on 08/17/2017).
- Roemer, J. E. (2010), Kantian equilibrium, *The Scandinavian Journal of Economics* 112 (1) 1–24.
- Rubin, Z., L. A. Peplau (1975), Who Believes in a Just World? *Journal of Social Issues* 31 (3) 65–89.
- Sandroni, A., S. Ludwig, P. Kircher (2013), On the difference between social and private goods, *The BE Journal of Theoretical Economics* 13 (1) 151–177.
- Selten, R. (1967), Die Strategiemethode zur Erforschung des eingeschränkt rationalen Verhaltens im Rahmen eines Oligopol-experiments, in: *Beiträge zur Experimentellen Wirtschaftsforschung*, edited by H. Sauerman, JCB Mohr (Paul Siebeck), Tübingen.
- Simpson, B., R. Willer (2008), Altruism and Indirect Reciprocity: The Interaction of Person and Situation in Prosocial Behavior, *Social Psychology Quarterly* 71 (1) 37–52.
- Smith, A. (2011), Group composition and conditional cooperation, *The Journal of Socio-Economics* 40 (5) 616–622.
- Sugden, R. (1982), On the Economics of Philanthropy, *Economic Journal* 92 (366) 341–350.
- Sugden, R. (1984), Reciprocity: The Supply of Public Goods Through Voluntary Contributions, *The Economic Journal* 94 (376) 772–787, URL: [http://links.jstor.org/sici?sici=0013-0133\(198412\)94:376%3C772:RTSOPG%3E2.0.CO;2-H](http://links.jstor.org/sici?sici=0013-0133(198412)94:376%3C772:RTSOPG%3E2.0.CO;2-H).
- Thaler, R. H., S. Benartzi (2004), Save More Tomorrow: Using behavioral economics to increase employee saving, *Journal of political Economy* 112 (S1) S164–S187.
- Tonin, M., M. Vlassopoulos (2014), An experimental investigation of intrinsic motivations for giving, *Theory and Decision* 76 (1) 47–67.
- Tversky, A., D. Kahneman (1991), Loss aversion in riskless choice: A reference-dependent model, *The Quarterly Journal of Economics* 106 (4) 1039–1061.
- Underwood, B., J. F. Berenson, R. J. Berenson, K. K. Cheng, D. Wilson, J. Kulik, B. S. Moore, G. Wenzel (1976), Attention, negative affect, and altruism: An ecological validation, *Personality and social psychology bulletin* 3 (1) 54–58.
- Vesterlund, L. D. (2003), The Informational Value of Sequential Fund-raising, *Journal of Public Economics* 87 1278–1293.
- Weyant, J. M. (1978), Effects of mood states, costs, and benefits on helping. *Journal of Personality and Social Psychology* 36 (10) 1169.
- Zizzo, D. J. (2010), Experimenter demand effects in economic experiments, *Experimental Economics* 13 (1) 75–98.

Appendix

A Theoretical Predictions

A.1 Expected Utility over outcomes

Consider an individual maximizing a Bernoulli utility function $v(x, g)$, where x represents consumption and g the charitable contribution, subject to non-negativity constraints and to the budget constraint $x + g \leq z$; Wealth or purchasing power (in a given state of the world) is here denoted by $z \in \{w, \ell\}$.

Let us assume her utility satisfies the standard expected-utility properties, so that the utility of a prospect is the probability-weighted sum of the utility of each element. Suppose she is asked to make a conditional decision, choosing g_w and g_ℓ before learning the realization of z . Assuming non-satiation, we can substitute in the budget constraints and express her problem as

$$g_w^b, g_\ell^b := \operatorname{argmax}_{g_w, g_\ell} (1-p)v(l - g_\ell, g_\ell) + pv(w - g_w, g_w),$$

where p is her probability of winning the prize. As explained in the main text, this characterizes the most widely cited models of giving, including a warm glow model where, as we assume throughout, the warm glow derives only from the amount *actually* donated. It is trivial to see that the same choices obtain when the donation decision is made after any uncertainty about income has resolved, and for the *Uncertain Collection* case. Thus, a standard model will predict $g_z^a = g_z^b = g_z^d = g_z^u$ for $z \in \{w, \ell\}$ or for any level of income. This remains true for g_w^b , if, as in our field experiment and in setting B , we constrain $g_\ell^b = 0$. In other words, the timing of the decision (i.e., whether it is a sure thing or a prospect), is irrelevant to the individual's choice.

A.2 Signaling Model of Reputation with uncertain collection

We define an individual's Bernoulli utility as an additively separable function:

$$v(x, g) = u(x) + \theta\omega(Dg) + R(\phi), \quad (1)$$

where x is an individual's own consumption, g is the amount committed to donate, and D is an indicator variable taking the value one if the committed donation is collected, and zero otherwise. $\theta\omega(\cdot)$ is his intrinsic utility from donating, and $\theta \in \{0, 1\}$ reflects his type, "bad" or "good", respectively, drawn by nature with $pr(\theta = 1) := \mu \in (0, 1)$.³⁷ The function $u(\cdot)$ represents the sub-utility of own-consumption, and $\omega(Dg)$ represents his private benefit from actually giving Dg (akin to a warm-glow function, but equally representing the private benefit from augmenting a public good). $R(\phi)$ is his utility from his reputation, a function of ϕ , which represents the posterior probability he and others put on him being of type $\theta = 1$, where $R(0) = 0$, $R(\mu) = \lambda r$, $R(1) = r$; $r > 0$, $0 \leq \lambda \leq 1$. Note that ϕ may depend on \mathbf{g}_{-i} and g in equilibrium, where \mathbf{g}_{-i} is the vector of others' committed contributions.³⁸

37. Our key insights generalize to a model in which types have continuous support, and the probability distribution may condition on a set of observable variables including gender and previous actions, as long as some uncertainty remains.

38. Note that we are assuming he knows his own type θ at the point he makes his decision. To make this a model where *self-signaling* is important, he must have limited memory of θ but better memory of past actions, as in Benabou et al. (2011). These

As in Benabou et al. (2006), we consider a direct payoff from reputation (in a social or self-signaling context, “which may be instrumental ... or purely hedonic”). We focus now on the setting where the individual faces income uncertainty and is asked about a contingent donation only for the state with high income, w . By standard assumptions, she will maximize the expected value of this Bernoulli utility function subject to the budget constraint

$$x + g \leq z,$$

where z denotes wealth. As donation commitments are only made for one income level, we omit the income superscript for g . The expected value of the utility can be restated as

$$U^\theta(g) = u(l) + p[u(w - g) - u(l)] + p\theta\omega(g) + R(\phi),$$

where p is the probability (at the time the donation decision is made) that the income is w and the donation will be collected. We consider equilibria where someone is assumed to be a potential good type only if he donates some amount which we will define as g_1 . Note that in a separating equilibrium reputation benefits are 0 for the bad types and r for good types. As we are only allowing positive donations ($g \geq 0$ is an implied constraint), it is trivial to show that in a separating equilibrium bad types donate nothing, i.e., $g_0 = 0$, which we assume henceforth. In a pooling equilibrium, everyone will get reputation benefit $R(\mu) = \lambda r$, i.e., some share of the reputation benefit of being known to be a good type.³⁹

Separating equilibrium: constraints. We next state the constraints for a separating equilibrium. The relevant constraint of the good type is that

$$U^1(g_1) \geq U^1(g) \quad \forall g. \tag{2}$$

The relevant incentive compatibility condition of the bad type requires:

$$U^0(0) \geq U^0(g_1). \tag{3}$$

Let g^* represent a good type's preferred donation *net of reputation*, i.e.⁴⁰

$$g^* = \underset{g}{\operatorname{argmax}} \{u(w - g) + \omega(g)\}.$$

Solutions .

authors write: “This self-assessment or signal, however, may not be perfectly recalled or ‘accessible’ later on —in fact, there will be strong incentives to remember it in a self-serving way. Actions, by contrast, are much easier to quantify, record and remember than their underlying motivation, making it rational for an agent to define himself partly through his past choices ...”

39. λ may depend on the *actual* share of good types in the population, but this will not affect our results unless we are comparing across distinct populations.

40. Note that, excluding reputation, the probability of collection does not matter for the optimal decision here.

Case 1 . Suppose at g^* the bad type will not deviate even if that brings him reputation benefit r , i.e., suppose

$$-p[u(w - g^*) - u(w)] \geq r. \quad (4)$$

Then, in the separating equilibrium with the lowest level of contributions (which is also the one that maximizes welfare for the good type, and the only one satisfying the intuitive criterion), $g_1 = g^*$, independent of p . The bad type's incentive constraint does not bind in this case, while the good type chooses her warm-glow maximizing donation level, satisfying condition 2. Note that there cannot be a pooling equilibrium here. Summing up, for the intuitive equilibrium in this parameter space, *conditional* donations do not change in the probability that they are collected; hence the intuitive criterion predicts that the *expected* donation will increase in p . Conversely, the expected contribution will decrease as p decreases up until the point at which Condition 4 no longer holds, i.e., up to the point where the collection probability is low enough to tempt bad types to imitate the good types.

Case 2 . Suppose condition 4 fails, i.e., $-p[u(w - g^*) - u(w)] < r$.

Thus if $g_1 \leq g^*$ the bad type would have an incentive to deviate and donate, i.e., the IC constraint is binding for bad types. Thus $g_1 = g^*$ cannot be part of equilibrium play. There are multiple separating equilibria. Consider the separating equilibrium with the lowest level of contributions, which is the only equilibrium that will survive the intuitive criterion. Here, a good type's contribution g_1^{\min} solves:

$$-p[u(w - g_1^{\min}) - u(w)] = r. \quad (5)$$

In this case, if the collection probability p decreases, the minimum level of conditional donations that separates types (g_1^{\min}) increases.⁴¹

Summarizing Cases 1 and 2. Thus, beginning at a value of p where the separation constraint does not bind, i.e., (4) holds with inequality, reducing p a small amount has no effect on conditional donations ($g_1 = g^*$) but lowers expected donations (pg^*). Reducing it further causes (4) to no longer hold, but permits only an intuitive separating equilibrium where h 's donate $g_1^{\min} > g^*$. Further reducing p increases g_1^{\min} but lowers the probability the contribution is realized.⁴²

The analysis extends to situations where income is not uncertain but the collection of donations is (by interpreting the collection probability as p and setting both w and ℓ to the realised income).

We can now compare across settings. For illustration—and resembling our lab experiment—assume that the probability of winning in the *Before* and *After* settings, and the probability the donation is collected in the *Uncertain Collection* setting are all $p = 1/2$. Suppose that the reputational benefit is such that

41. We may also have pooling equilibria where both types contribute g^{pool} satisfying $g^* \leq g^{pool} < g_1^{\min}$. These are possible where bad types are willing to contribute g^* even to gain the lower reputation $\bar{R}(g^{pool}|pooling)$. I.e.,

$$-p[u(w - g^*) - u(w)] < \lambda r < r, \quad (6)$$

where the latter inequality is given to highlight that a pooling equilibrium could be ruled out under a weaker condition than condition 4. For lower values of p this equation holds for a wider range of preferences. However, the pooling equilibrium also does not survive the intuitive criterion. There is always a deviation that is only profitable for the good type, as he enjoys not only the reputational gain $(1 - \lambda)r$ but also, unlike the bad type, a warm glow.

42. The net effect on expected contributions pg_1^{\min} depends on the concavity of the material sub-utility function $u(\cdot)$. We have $-p \frac{dg_1^{\min}}{dp} = \frac{u(w) - u(w - g_1^{\min})}{u'(w - g_1^{\min})} \geq g_1^{\min}$ (where the latter inequality follows iff u is convex), implying $\frac{d(pg_1^{\min}(p))}{dp} \leq 0$ if and only if u is convex. Thus, under a standard assumption of diminishing returns to own-consumption (concave $u(\cdot)$), lowering p will reduce *expected* contributions.

case 2 applies for $p = 1/2$ while case 1 obtains if $p = 1$. This would imply that in the *Before* and *Uncertain Collection* setting good types will commit to donate $g^u = g_1^{\min} > g^*$. In the *After* setting (with the same income) corresponding to $p = 1$, good types will donate $g^a = g^* < g_1^{\min}$. Alternately, suppose case 2 held for both $p = 1$ and $p = 1/2$. Here donations in the *After* setting would be above g^* , but still below g_1^{\min} , the commitment in the settings *Before* and *Uncertain Collection*.

Summarizing the above, where parameters are consistent with case 2 (under the *Before* or *Uncertain Collection* settings) this model yields Prediction 2 (already stated in the main text).

Prediction 6. *Signaling generosity, where the separation constraint binds*

$$g_w^u = g_w^b > g_w^d = g_w^a$$

for good types, while bad types are unaffected by the treatment. A similar relationship will hold for donations from the lower level of income if condition 4 also fails at income l , which need not be the case.⁴³

Heterogeneity. This model can be extended to reflect signaling where individuals can be publicly identified by a certain characteristic, e.g., gender, and the groups are known to have different type distributions and utility parameters. If we allow all the individual parameters in Equation 1 to differ by the group's observable characteristic, case (1) is more "likely" to hold for groups with a smaller reputation motive (smaller r) relative to the warm glow term (of good types in that group). I.e., as r declines the parameters move towards case 1 above, and if $R(\cdot)$ is not present the results are as in the expected utility model. Thus, under some background environments case (1) may hold for one group, e.g., women, while case (2) may hold for another group and the donation commitments will respond to the uncertain collection for the latter group only.⁴⁴

A.3 Loss Aversion and Reference Points

When making—even riskless—choices, it is often argued that decisions are influenced by anticipated gains and losses relative to a reference point (see Tversky et al., 1991). Thaler et al. (2004) claim "... once households get used to a particular level of disposable income, they tend to view reductions in that level as a loss." In considering this model, we suppose the individual has a reference point over her own consumption, not including charitable giving, and her utility function sums a standard reference-independent term and a *gain-loss* component. Her donation decision, whether stochastic or certain, anticipates how the donation will reduce the remaining wealth available for her own consumption. If this will fall below her reference point, she will incur a psychological loss. We assume there is no gain-loss utility over the donation itself (i.e., a single target, as in Camerer et al., 1997).⁴⁵ While the reference point may change over time, we assume here that she is *myopic* in the sense that when making a decision she does not anticipate these changes. For simplicity, we consider a utility function embodying a linear loss function, i.e.,

43. Under the standard assumption that $u(\cdot)$ is concave, the parameter space where this holds at income w is a proper subset of the parameter space where this holds at income l .

44. Note that if a greater *share* of one group are good types, perhaps implying a larger λ , this will only affect the conditions for a pooling equilibrium but will not affect our conditions for cases 1 and 2.

45. This may hold if donating nothing and using all of one's income for own-consumption is typically seen as the default, thus the basis for a reference point. Note that this model's predictions would be qualitatively the same if there were two targets, but the gain-loss utility were far more salient for consumption targets than for giving targets.

$$v(x, g, \pi) = \begin{cases} u(x) + \omega(Dg) & \text{if } x \geq \pi \\ u(x) - \delta[u(\pi) - u(x)] + \omega(Dg) & \text{if } x < \pi; \end{cases}$$

subject to the budget constraint $x + g \leq E$.

As before x represents own consumption, g is the committed donation expenditure and D indicates whether it is realized, π is a reference point, specified below, and δ is a (strictly) positive constant. Here $u(\cdot)$, the sub-utility of own-consumption, is assumed to be strictly increasing and concave, as is the “warm glow” function $\omega(\cdot)$. We consider two different ways this reference point may update to the realization of uncertainty. First, we consider immediate adjustment, second, we consider a very sticky adjustment process.⁴⁶ To save space, all of these derivations are in the online appendix (“Loss aversion models”).

Suppose the reference point always corresponds to the expected future income at the point of the decision, the maximum own-consumption one could achieve if one’s investments paid their expected value. This implies that contributions incur a *loss* in all cases except for conditional contributions from an expected win, implying greater contributions in the *Before* than in the other treatments, i.e.:

Prediction 7. *Loss Aversion, expected income, immediate adjustment*

$$g_w^b > g_w = g_w^a = g_w^u \text{ (provided } g_w^b < w - (pw + (1 - p)l) \text{) and} \\ g_\ell = g_\ell^a.$$

This analysis generalizes to any intermediate reference point if we assume that an individual’s reference point corresponds to the original expected-value income throughout the relevant decision period, we have a slightly different prediction:

Prediction 8. *Loss Aversion, expected income, no adjustment*

$$g_w^b = g_w^a > g_w = g_w^u \text{ and} \\ g_\ell^a < g_\ell.$$

If the reference point does not adjust rapidly, then donations from an anticipated or actual *win* (loss) will be higher (lower) than donations from income that was not subject to uncertainty.

46. We can consider the “ask,” even a fairly neutral ask, as a special shock motivating giving by changing the environment/context or temporarily adjusting the utility function to make the utility slope of giving particularly steep (via alleviating guilt or providing special warm glow); see Andreoni, Rao (2011) and models in Reinstein (2011) and Kotzebue et al. (2009). Hence we may predict individuals will give a larger share of their “winnings” when asked in our experiments than the share of their income they might normally donate. The reference consumption basket might be based on her expectations before being asked to donate, thus deducting no donation; alternately, it may have anticipated a small probability of an ask, or it might immediately subtract the expected value of the conditional donation after the ask. For any of these the reference consumption is still less than the higher earnings w .

B Screen shots

Further screen shots for all experiments available by request; viewable copy of Qualtrics survey instruments hosted online (see footnotes in main text)



C Supplementary results

C.1 Summary Statistics and randomization

Table C.1. Valentine's

	(1) Before	(2) After	(3) p-value	(4) N
Anonymity treatment in Val stage	2.009 (0.068)	1.959 (0.065)	0.596	205
Donor treatment	2.467 (0.052)	2.459 (0.051)	0.911	205
Cards sent in Val St.	0.626 (0.093)	0.704 (0.141)	0.639	205
Year of study	3.292 (0.195)	3.235 (0.187)	0.831	204
Female	0.764 (0.041)	0.694 (0.047)	0.261	204
No religion	0.561 (0.048)	0.429 (0.050)	0.059	205
University	1.850 (0.072)	1.745 (0.069)	0.293	205
Previous volunteer	0.336 (0.046)	0.357 (0.049)	0.757	205
Previous donor	0.654 (0.046)	0.714 (0.046)	0.358	205
<i>N</i>	107	98		

Table C.2. Employability

	(1) Before	(2) After	(3) p-value	(4) N
Jobs Treatment	0.25 (0.03)	0.20 (0.05)	0.34	375
Female	0.53 (0.03)	0.59 (0.06)	0.37	323
Business	0.19 (0.02)	0.21 (0.05)	0.60	375
<i>N</i>	300	75		

Table C.3. Essex Omnibus

	(1) Before	(2) After	(3) p-value	(4) N
Female	0.63 (0.03)	0.62 (0.04)	0.84	460
Age	28.52 (0.72)	27.74 (0.92)	0.52	460
FirstLangEng	0.62 (0.03)	0.61 (0.04)	0.84	460
BirthNationUK	0.54 (0.03)	0.56 (0.04)	0.66	460
Siblings	1.84 (0.08)	1.80 (0.13)	0.80	460
DecisionModeImp	1.78 (0.02)	1.77 (0.03)	0.94	460
<i>N</i>	304	156		

Table C.4. Prolific

	(1) Before	(2) After	(3) p-value	(4) N
Female	0.73 (0.04)	0.65 (0.06)	0.24	230
Age	28.03 (0.53)	25.93 (0.39)	0.01	230
Non-giver (self reported)	0.22 (0.03)	0.24 (0.05)	0.64	227
Gave up to 50 (self reported)	0.54 (0.04)	0.51 (0.06)	0.75	227
Gave over 50 (self reported)	0.25 (0.04)	0.24 (0.05)	0.93	227
<i>N</i>	162	78		

Table C.5. Laboratory

	(1) Income Certain	(2) Before	(3) Before both	(4) After	(5) Uncertain	(6) p-value	(7) N
High Income	0.56 (0.06)	0.54 (0.06)	0.49 (0.06)	0.48 (0.05)	0.63 (0.05)	0.29	430
Female	0.52 (0.06)	0.51 (0.06)	0.52 (0.06)	0.48 (0.05)	0.42 (0.06)	0.68	419
Age	23.90 (0.65)	24.07 (0.55)	23.47 (0.51)	23.21 (0.47)	24.58 (0.82)	0.50	419
No religion	0.54 (0.06)	0.46 (0.06)	0.43 (0.06)	0.51 (0.05)	0.51 (0.06)	0.62	430
Previous donor	0.33 (0.05)	0.35 (0.06)	0.37 (0.05)	0.30 (0.04)	0.27 (0.05)	0.66	430
<i>N</i>	79	74	79	115	83		

Table C.6. Further summary statistics: Prolific sample

Variable	Mean	(Std. Dev.)	N
Personal Income (GBP)	19,880	(8498)	209
Female	0.70		230
Age	27.3	(5.81)	230
Non-giver (self reported)	0.23		227
Caucasian	0.83		240
Max degree: Secondary school	0.35		240
Max: Undergrad degree	0.27		240
Max (Post)-Grad degree	0.10		240
Labour party affil.	0.45		240
Has children	0.47		240
Identifies as monocultural	0.45		240

Notes: Key summary statistics from Prolific Academic sample, from previously collected survey 'screener' questions. Income imputed as mean of range-coding.

Table C.8. OLS on Donations: Age, gender and religiosity, pooled over experiments

	(1) Proportion	(2) Level	(3) Incidence	(4) Proportion	(5) Level	(6) Incidence
Before	0.033** [0.008,0.059]	0.49*** [0.138,0.837]	0.089*** [0.048,0.130]	0.031* [-0.006,0.068]	0.48** [0.015,0.951]	0.074** [0.003,0.145]
Female (centered)	0.065** [0.004,0.126]	0.97** [0.164,1.767]	0.13** [0.029,0.231]	0.053* [-0.007,0.113]	0.79** [0.012,1.562]	0.095* [-0.007,0.197]
20 to <30 years	-0.011 [-0.079,0.057]	-0.22 [-1.091,0.653]	-0.15** [-0.275,-0.029]	0.090** [0.022,0.157]	0.65 [-0.379,1.685]	0.14* [-0.018,0.305]
30 to <40 years	0.10* [-0.006,0.211]	1.15* [-0.168,2.468]	-0.12* [-0.247,0.012]	0.23*** [0.103,0.350]	2.30*** [0.734,3.861]	0.18 [-0.084,0.447]
40-50 years	-0.075 [-0.240,0.091]	-0.97 [-3.024,1.077]	-0.18 [-0.536,0.169]	0.088 [-0.034,0.210]	0.63 [-0.976,2.242]	0.11 [-0.204,0.427]
Non-religious	0.019 [-0.020,0.058]	0.18 [-0.281,0.634]	0.0090 [-0.114,0.132]	0.020 [-0.019,0.060]	0.23 [-0.267,0.732]	0.018 [-0.111,0.147]
Before × Female (centered)	-0.045 [-0.101,0.011]	-0.73* [-1.585,0.117]	-0.059 [-0.166,0.048]	-0.036 [-0.088,0.015]	-0.68 [-1.497,0.135]	-0.026 [-0.131,0.079]
Before × 20 to <30 years	-0.017 [-0.183,0.150]	-0.12 [-2.187,1.939]	0.084 [-0.128,0.297]	0.028 [-0.051,0.107]	-0.041 [-1.054,0.972]	0.087 [-0.070,0.243]
Before × 30 to <40 years	-0.15** [-0.271,-0.023]	-1.69** [-3.224,-0.160]	0.014 [-0.198,0.226]	-0.12** [-0.225,-0.010]	-1.77*** [-3.061,-0.474]	-0.0024 [-0.237,0.232]
Before × 40-50 years	0.12 [-0.130,0.367]	1.40 [-1.573,4.379]	0.15 [-0.251,0.558]	0.12 [-0.039,0.272]	0.90 [-0.999,2.797]	0.12 [-0.209,0.458]
Before × Non-religious	-0.032 [-0.091,0.028]	-0.21 [-0.920,0.496]	0.016 [-0.140,0.172]	-0.044* [-0.098,0.009]	-0.42 [-1.037,0.198]	-0.014 [-0.149,0.121]
Constant	0.14*** [0.120,0.157]	1.91*** [1.636,2.178]	0.43*** [0.392,0.474]	0.14*** [0.099,0.189]	1.90*** [1.307,2.490]	0.41*** [0.336,0.484]
Centered experiment dummies, interactions	Yes	Yes	Yes	No	No	No
Observations	1296	1296	1296	1296	1296	1296

Notes: OLS regressions on donation shares of endowment, levels and incidence for Before versus After treatments; interacted with de-meaned gender, age, non-religious, and risk attitude categorical variables, excluding donations from the lower income level. Missing values of these variables are linearly imputed from other variables in this regression. All regressions include dummies for each experiment (hidden). Columns 4-6 also include interactions of the Before treatment with de-meaned experiment dummies (not shown). Dependent variables: (a) shares donated from endowment, (b) actual donation levels in Euros, (c) donation incidence. Cluster-robust standard at the session (date) levels for the lab (web-based) experiments account for potential correlated errors at these levels. T-tests at * $p < 0.1$, ** $p < .05$, *** $p < .01$.

C.2 Results: Happiness, donations

Table C.7. Happiness, winning, and donations

	(1) Happiness at end, normalized	(2) Happiness at end, normalized	(3) Donation share (Prolific)
Won prize	0.75*** [0.38,1.13]	0.31 [-0.085,0.70]	
Omnibus expt	-0.39*** [-0.50,-0.28]	-0.63*** [-0.84,-0.42]	
Won prize, Omnibus expt		0.64*** [0.24,1.05]	
Happiness at start, normalized			-0.0021 [-0.029,0.025]
Observations	460	460	240

Notes: This table reports coefficients and 95% confidence intervals from OLS regressions. Data from Omnibus and Prolific experiments (columns 1-2), and Prolific only (column 3). Happiness variables de-meaned and divided by standard deviation, derived from self reported rating scales. Results of t-tests indicated at following significance levels * $p < 0.1$, ** $p < .05$, *** $p < .01$.

C.3 Heterogeneity and nonlinearity

Below, we report maximum likelihood estimates of models of the form

$$Y_i^\alpha = \beta_0 + \beta_1 X_{1i} + \beta_2 X_{2i} + \dots \beta_j X_{ji} + \epsilon_i,$$

where $\epsilon_i \sim N(0, \sigma^2)$.

Table C.9. Power model (nonlinear, ML) of Donation shares (from higher income), pooled over experiments

Number of obs = 1353
Wald chi2(8) = 231.47
Log likelihood = 85.77, Prob >chi2 = 0.000

Donation Proportion	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]
Before	0.056	0.009	6.450	0.000	0.039 0.073
Dummy: Age<20	-0.137	0.034	-4.060	0.000	-0.203 -0.071
Dummy: Age 20-30	-0.053	0.020	-2.630	0.008	-0.092 -0.013
α	3.832	0.498	7.690	0.000	2.856 4.808
σ	0.227	0.004	52.020	0.000	0.219 0.236

Notes: Maximum likelihood estimates of power models (dependent variable raised to the power α) of donation shares as a function of treatment, and demeaned experiment and imputed age categories. Experiment/lab dummy coefficients hidden.

Table C.10. Power model (nonlinear, ML) of Donation shares (from higher income), pooled over experiments

Number of obs = 1296
Wald chi2(17) = 184.69
Log likelihood = 67.41, Prob >chi2 = 0.000

Donation Proportion	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]
Before	0.060	0.011	5.57	0.000	0.039 0.081
Before \times Dummy: Age<20	0.151	0.072	0.21	0.833	-0.125 0.155
Before \times Dummy: Age 20-30	0.009	0.042	0.20	0.840	-0.074 0.091
Age<20	-0.147	0.056	-2.63	0.009	-0.256 -0.037
Age 20-30	-0.062	0.031	-1.97	0.049	-0.123 -0.000
Before \times Female	-0.032	0.028	-1.13	0.258	-0.088 0.024
Female	0.041	0.022	1.82	0.068	-0.003 0.084
α	3.722	0.503	7.39	0.000	2.735 4.709
σ	0.230	0.005	50.91	0.000	0.221 0.239

Notes: Maximum likelihood estimates of power models (dependent variable raised to the power α) of donation shares as a function of treatment, and demeaned experiment, age category, and gender baseline, all interacted with treatment. Experiment/lab dummy coefficients and interactions hidden.

C.4 Pooled results: Robustness

Table C.11. Negative binomial and Probit regressions: Donation shares, incidence; pooled

	(1) Neg-Binom-Pooled	(2) Neg-Binom w/o Val.	(3) Probit-pooled	(4) Probit w/o Val.
Before	0.38*** [0.25,0.52]	0.31*** [0.17,0.44]	0.27*** [0.16,0.39]	0.25*** [0.12,0.38]
Constant	2.33*** [2.21,2.46]	2.56*** [2.44,2.69]	-0.19*** [-0.30,-0.087]	-0.091* [-0.20,0.016]
lnalpha	1.80*** [1.62,1.98]	1.76*** [1.58,1.93]		
Observations	1363	1204	1363	1204

Note: This table reports coefficients and 95% confidence intervals from regressions for the Before treatment versus the After treatment, excluding donations from the lower income level. Columns 1-2 report Negative Binomial models, pooling across all experiments (Col. 1) and excluding the Valentine's experiment (Col. 2). Dependent variables are the shares (multiplied by 100) donated from the endowment. Columns 3-4 report Probit regressions of donation incidence, again pooling for all experiments (Col. 3) and excluding Valentines (Col. 4). All regressions include centered dummies for each experiment, as well as the interaction of these with the Before treatment (not shown). We account for potential session-specific correlated errors (for the lab experiments) and date-specific correlated errors (for the web-based experiments) by cluster-robust standard errors at these levels. Results of t-test indicated at following significance levels * $p < 0.1$, ** $p < .05$, *** $p < .01$.

C.5 Additional results by experiment

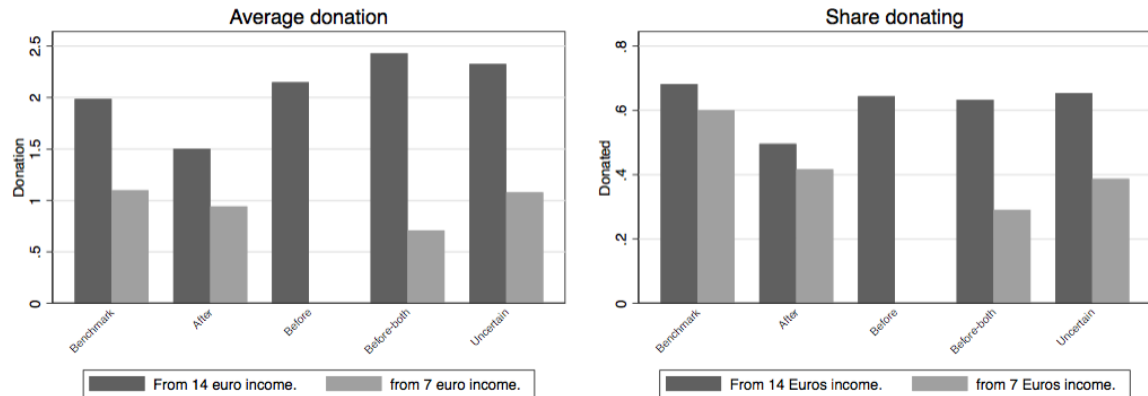


Figure C.11. Mean share committed by experiment, by Before vs. After

Table C.12. Laboratory: Donation amounts and incidence (OLS)

	Levels		Incidence	
	(1) High income	(2) Low income	(3) High income	(4) Low income
Treatment	ref.		ref.	
– Before	0.76* [-0.077,1.59]		0.097 [-0.066,0.26]	
– Before-both	0.44 [-0.61,1.50]	-0.56** [-1.07,-0.058]	0.040 [-0.16,0.24]	-0.17 [-0.39,0.046]
– After	0.27 [-0.78,1.31]	-0.27 [-1.05,0.50]		
– Uncertain	0.34 [-0.58,1.26]	-0.24 [-1.11,0.62]	0.0050 [-0.18,0.19]	-0.065 [-0.38,0.25]
– Income Certain			-0.0047 [-0.23,0.22]	0.22 [-0.082,0.53]
Constant	1.99*** [1.40,2.58]	1.17*** [0.66,1.68]	0.65*** [0.50,0.80]	0.43*** [0.28,0.58]
Observations	304	205	304	205

Notes: This table reports coefficients and 95% confidence intervals from ordinary least squares regressions on donations by Treatment for the lab experiment. As dependent variables we use (a) the levels of donations in Euros (Columns 1-4) and (b) donation incidence (Columns 5-8). In the Before-both treatment each subject made two choices – donation commitments from high income (if you win) and from low income are reported in the corresponding columns. We account for potential session-specific correlated errors by cluster-robust standard errors. Results of t-tests indicated at following significance levels * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.6 Robustness checks, by experiment

Table C.13. Negative Binomial Regressions: Donations by experiment

Panel A: Levels	Prolific	Lab	Employability	Omnibus	Valentines
Donation from hi income					
Before	0.66*** [0.26,1.05]	0.39* [-0.048,0.82]	0.27 [-0.31,0.86]	0.076 [-0.19,0.34]	1.30*** [0.42,2.18]
Constant	-0.026 [-0.37,0.32]	0.62*** [0.30,0.94]	0.46* [-0.069,0.98]	1.09*** [0.87,1.31]	-1.06** [-1.88,-0.25]
lnalpha	0.35** [0.039,0.66]	-0.25 [-0.74,0.23]	1.95*** [1.73,2.18]	0.96*** [0.78,1.14]	1.54*** [1.13,1.95]
Observations	240	129	375	460	159

Notes: This table reports coefficients and 95% confidence intervals from on donations for the Before treatment versus the After treatment in each experiment, excluding donations from the lower income level.

Table C.14. Laboratory: Donation amounts (Neg. Bin.) and incidence (Logit)

	Levels				Incidence			
	(1) High income	(2) High income	(3) Low income	(4) Low income	(5) High income	(6) High income	(7) Low income	(8) Low income
main								
Treatment	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
– Income Certain	-0.13 [-0.58,0.33]	-0.077 [-0.83,0.67]	0.15 [-0.50,0.81]	-0.18 [-0.74,0.38]	-0.019 [-0.94,0.90]	1.89*** [0.67,3.10]	0.92 [-0.33,2.17]	0.54 [-0.96,2.04]
– Before	0.20 [-0.15,0.55]	0.48* [-0.067,1.02]			0.47 [-0.25,1.18]	0.52 [-0.42,1.45]		
– Before-both	0.075 [-0.33,0.48]	0.37 [-0.17,0.92]	-0.29 [-0.85,0.28]	-2.13*** [-2.85,-1.41]	0.19 [-0.65,1.03]	-0.67 [-1.93,0.58]	-0.78 [-1.74,0.18]	-2.68*** [-3.65,-1.70]
– Uncertain	0.032 [-0.42,0.48]	-0.086 [-0.69,0.51]	0.14 [-0.56,0.84]	-1.67*** [-2.56,-0.78]	0.021 [-0.73,0.78]	-0.96* [-2.03,0.12]	-0.27 [-1.52,0.97]	-1.87*** [-3.10,-0.64]
Female (centered)		0.48 [-0.31,1.26]		0.46 [-0.48,1.40]		1.08 [-0.24,2.41]		0.76 [-0.46,1.97]
– Income Certain × Female (centered)		-0.44 [-1.41,0.53]		-0.38 [-1.65,0.89]		-0.48 [-2.11,1.14]		-0.92 [-3.20,1.37]
– Before × Female (centered)		-0.27 [-1.23,0.68]				-0.61 [-2.38,1.16]		
– Before-both × Female (centered)		-0.53 [-1.66,0.60]		0.040 [-1.28,1.37]		-0.55 [-2.39,1.29]		-0.50 [-1.99,0.99]
– Uncertain × Female (centered)		-0.54 [-1.39,0.31]		-0.36 [-1.98,1.26]		0.18 [-1.39,1.75]		-0.28 [-2.72,2.15]
Constant	0.81*** [0.46,1.17]	0.49* [-0.0075,0.98]	-0.058 [-0.53,0.42]	-0.12 [-0.49,0.24]	0.62* [-0.0078,1.26]	1.78*** [0.90,2.65]	-0.29 [-0.87,0.29]	-0.12 [-0.73,0.49]
/								
lnalpha	-0.022 [-0.32,0.28]	-0.18 [-0.52,0.15]	0.69*** [0.17,1.21]	0.53* [-0.061,1.12]				
Observations	304	293	205	198	304	293	205	198

Notes: This table reports coefficients and 95% confidence intervals from on donations by Treatment for the lab experiment. As dependent variables we use (a) the levels of donations in Euro (Columns 1-4) and (b) donation incidence (Columns 5-8). We account for potential session-specific correlated errors by cluster-robust standard errors. Results of t-test indicated at following significance levels * p<0.1, ** p<.05, *** p<.01.

C.7 Related experiments

Table C.15. Comparing charitable giving experiments: Summary statistics and power

Experiment	N	Endowment	Share donating	Share donated	Mean donation	SD	SD/Mean	Effect Size %
This study								
- Valentine's	159	£20	28%	4.1%	£0.83	1.63	197%	103%
- Employability	375	£20	31%	8.3%	£1.97	4.59	233%	25%
- Lab (all treatments)	430	€7,€14	61%	16.3%	£1.99	2.5395	128%	24%
- Omnibus	460	£20	54%	26.2%	£3.13	4.15	133%	8%
- Prolific	240	£10	53%	15.8%	£1.58	2.1515	136%	57%
Authors' working papers								
Reinstein: Berkeley Pilot (Wave 1)	49	\$10	74%	21.0%	\$2.10	2.06	98%	
Reinstein (2010) ... Wave 2	48	\$20	65%	23.0%	\$4.60	4.94	107%	18%
Published studies								
Eckel et al. (1996)	48	\$10	73%	30.1%	\$3.01	3.19	106%	
Karlan et al. (2007)	50,083		2.0%		\$0.90	0.05	6%	19%
Huck et al. (2011)	25,000	N/A	4.1%			10		44%
Reinstein, Riener (2012a)	190	€5,7.5,10	57%	18.3%	€1.23	1.75	142%	52%
Reinstein, Riener, 2012b	192	8 €	77%	25.0%	€1.80	1.81	101%	72%
Jones et al. (2014)	150	\$10	73%	37.3%	\$3.73	3.49	94%	21%
Tonin et al. (2014)	196	£10	81%	47.9%	£4.79	3.31	69%	21%

Notes: £: UK pounds, \$: US dollars, €: Euros, no inflation adjustments. In experiments with multiple donations, results reported for first Ask only. **Endowment:** Amount(s) paid to participants which could be donated; vouchers for Valentines, Employability, Omnibus. **N:** Observations with a giving decision (Valentines: excludes non-winners). **Effect size %:** divides first reported (regression) result by mean donation Kellner et al - Lab: Donation from higher income reported for "Before Both" treatment

Online appendix

See link (<https://goo.gl/RKRL81>) or attached file)

Includes:

1. Derivation of result for Loss Aversion models
2. Alternate and empirically equivalent models
 - Adaptation, habituation, relativity with complementarity
 - Tangibility
 - Uncertainty aversion (ambiguity)
 - Present bias
3. Web-based experiment details and key screens
 - Employability promotion (University of Essex)
 - Valentine's experiment
 - Omnibus
 - Recruitment screen from Prolific
4. Lab: A complete set of relevant screenshots and translations