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Stochastic Income and Conditional Generosity

Christian Kellner, David Reinstein, and Gerhard Riener*

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Abstract

We study how other-regarding behavior extends to environments with uncertain income 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 both predict a larger commitment *before* the bonus is known; theories of affect predict the reverse. In field and lab experiments, we allow people to donate from lottery winnings, varying whether they decide before or after learning the lottery's outcome. Males are more generous when making conditional donations before knowing the outcome, while females' donations are unaffected. Males also commit more in treatments where income is certain but the donation's *collection* is uncertain. This supports a signaling explanation: it is cheaper to commit to donate before the uncertainty is unresolved, thus a larger donation is required to maintain a positive image. This has implications for experimental methodology, for fundraisers, and for our understanding of pro-social behavior.

Keywords: Social preferences, contingent decision-making, signaling, uncertainty, prospect theory, affective state, gender, charitable giving, public goods, experiments, field experiments, bonuses.

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

1 Introduction

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

*Kellner, Bonn; Reinstein (corresponding author), University of Essex, Wivenhoe Park, Colchester CO4 3SQ, United Kingdom; drein@essex.ac.uk; +44 1206 87 3518; Riener, Mannheim and DICE, Düsseldorf. Acknowledgments: We would like to thank the the Universities of Essex, Düsseldorf, Mannheim and Bonn for providing financial support for our research. We would also like to thank the many colleagues and seminar and conference participants who have offered us valuable advice and comments, including Johannes Abeler, Michele Brock, Gary Charness, Zack Grossman, Sandra Ludwig, Friederike Mengel, Timothy Rakow, and Jeroen van de Ven. We are also grateful to Edward Dickerson, Saga Eriksson, Jonathan Homola, Kajetonas Orlauskas, and Stavros Poupakis for excellent research assistance.

expected utility maximizer with an unchanging utility function who only cares about outcomes, this should not matter.

However, there are important differences between these two modes—asking for a "*before commitment" versus asking for an "after commitment"*—which may have an impact on 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 a random draw.

Thus, several alternative models predict a difference in behavior depending on when a person is asked to commit. We report on a series of field and laboratory experiments involving uncertain prizes and appeals for charitable giving, offering the first economic evidence on this question, and differentiating among alternative models. In each of our experiments male (but not female) subjects committed significantly more when asked *before* than when asked *after*, and the gender interaction is also significant. The laboratory evidence suggests the male response was driven by the uncertain *collection* of the donation rather than the uncertainty of the income.[1]

This is an important issue for policymakers and fundraisers. Many workers receive windfall payments, such as bonuses, 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). Over a similar period, individual charitable giving was £9.3 billion. In the United States, Wall Street banks distributed $26.7 billion in bonuses in 2013 (Office of the NY State Comptroller, 2014). Anecdotal evidence (from our personal correspondence) suggests that a significant share of this bonus income was not fully anticipated.[2]In the wake of recent recession and scandals in the financial markets, bankers have been encouraged to give back their bonuses, or donate them to charity.[3] Our evidence suggests it may be more effective to ask bankers to commit to donate from *future* bonuses *if* they exceed a particular expected value.

This question is also relevant to a variety of other situations in which individuals may be asked, or volunteer, to commit or donate from actual or potential financial gains. Lottery and raffle organizers may ask for pledges from participants, in the event that they win. Philanthropists such as Warren Buffet and Marc Benioff might ask up-and-coming entrepreneurs to commit to donate a share of their *future* payoffs if their start-ups make it big; philanthropic venture capitalists may make this a condition of funding. "Ethical" investment accounts could be set up to automatically donate dividends and capital gains that exceed expectations.[4] In several prominent cases university students have been asked to publicly pledge a share of their future income.[5]

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[1] We complemented our original field experiment to gain more confidence in the robustness of our results (see Maniadis et al., 2014) and to further explore the gender differences in a more controlled environment.

[2] "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.

[3] "Johnson: Bankers should assuage guilt by giving bonuses to homeless scheme." – *The Guardian*, 13 Feb 2009.

[4] Triodos Bank, for example, offers a "Save and Donate" account <http://www.triodos.co.uk/en/personal/savings-overview/charity-saver/>, promoted as an ethical way to save. However, these accounts currently involve *fixed* interest rates and a fixed share that is donated.

[5] The Gates-Buffet "Giving Pledge" asks billionaires to commit to give away at least half their wealth to charity in

This also has an important implication for experimental methodology. If individuals' conditional donation choices for a state of the world respond to the probability that this state will be realized, this violates simple expected utility maximization over outcomes. This casts doubt on the standard experimental claim that when only one stage is chosen randomly for payment (the *random lottery incentive scheme*), subjects treat each stage independently (see Cubitt et al., 1998).

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 and Vlassopoulos (2014) and Reinstein (2010) have run experiments involving charitable donation in uncertain environments, where only one of a series of decisions will be implemented; both found that donations decline 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) and Kircher et al. (2009) both 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 the giving decisions were made using the strategy method than when subjects were asked ex-post. These results argue against relying on a model where an individual maximizes expected utility with a consistent utility function that considers only *outcomes*.

Grossman (2015) focuses on measuring and differentiating self and social signaling. His experiments involved binary-choice dictator games where an individual's choice may be randomly overruled with a given probability. His treatments varied this probability, and varied whether the "observer" (another subject or the experimenter) saw the outcome, the choice, and the probability of overrule. In general, he finds that the probability the choice is overruled had no large nor significant effect on choices in *any* of the observability conditions.[6] His results are broadly consistent with our evidence presented below; we find insignificant overall effects but significant gender-differentiated effects; he does not observe gender.[7]

their lifetime or in their will (*http://en.wikipedia.org/wiki/The_Giving_Pledge*). Marc Benioff's SF Gives recently asked 20 tech companies to give at least $500,000 a piece to fight poverty *<http://valleywag.gawker.com/the-marc-benioff-public-shaming-school-of-philanthropy-1564843302>*. "Giving What We Can," founded by Tony Ord, has asked students to make a *giving pledge* to donate roughly 10% of their future income. According to their website *<http://www.givingwhatwecan.org/about-us>* they have 269 members and over $100 million has been pledged. The Chronicle of Higher Education (13 Feb., 2011) reported on a similar pledge by students at Rutgers University.

6 He argues that his results are somewhat supportive of a model of social signaling (particularly when he excludes those who were "money-maximizers" in an earlier game) but do not support self-signaling.

7 Our approaches differ substantially. Grossman focuses on distinguishing social and self-signalling; we focus on the effect of making conditional commitments from uncertain *income*, and separating this from the impact of uncertain *collection*. Our lab experiments differ (binary vs. continuous choices, different sets of probabilities, with/without a real-effort task, session-level versus within-session variation), and we also provide *field* experiment evidence. The similarity is strongest between several treatments in our lab experiment and in Grossman's *followup* experiment. In his followup, each of his subjects decides between allocating {5,1} or {1,10} to herself and the American Cancer Society, respectively. He varies the probability that the subject's choice will be overruled in favor of the less generous outcome; either 1/6 or 5/6 of choices will be overruled. He also varies whether the *experimenter* observes the subject's choice or only the outcome (the experimenter, who oversees the die roll, always knows the probabilities; Grossman's treatments are at the session level by oral instruction). His comparison between *high* and *low* probability treatments, particularly within his *Outcome* treatments, is broadly similar to our comparison between *Baseline* and *Uncertain* lab treatments. In both cases, only the subject knows her own choice, so any difference is most easily attributed to self-signaling. Again, he finds that the probability of overrule does not significantly affect the choices in *any* observability treatment, but he can

The question of generosity over *uncertain* income is distinct but related to generosity over *future* income. Breman (2011) ran a field experiment asking donors to pre-commit to increase their regular donation either immediately, in one months time, or in two months time. Commitments could be reversed but they rarely were. 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."[8] These results largely support Andreoni and Payne (2003), who write that "a commitment to a charity may yield a warm-glow to the givers before they actually mail the check. Hence, the benefits can flow before the costs are paid".[9]

Much previous work has found gender differences in the levels and determinants of other-regarding behavior. In laboratory dictator experiments involving "donations" to other subjects Andreoni and Vesterlund (2001) found that men give more than women when the price is low, while women give more than men when the price is high (the price is defined as the amount a subject sacrifices of her own payoff per unit given to another subject). If we consider our uncertain collection treatments as involving as a lower price for sending a positive signal, this would predict that men respond more than women to these treatments (as we observe). However, the results of Cox and Deck (2006) suggest that women are *more* sensitive to the cost-benefit ratio in such settings: they are more generous than men particularly when social distance is low or when the cost of generosity is low. Furthermore, in a charitable giving field experiment, Meier (2007) finds women respond (insignificantly) *more* to the price of giving.

Relative to women, men's donations appear to be more driven by prestige and signaling concerns, at least insofar as they are signaling "above average donations". Evidence for this comes largely from experiments involving actual charitable giving. Jones and Linardi (2014) found that (in lab and field experiments) women tended to "conform to the modal contribution when ... names and contributions are revealed" while men reacted to visibility "by increasingly choosing a contribution equal to the maximum of his group members' contributions". They hypothesize that "unlike males, females are 'wallflowers' who suffer disutility from both negative and positive reputation." In another laboratory experiment involving real charitable giving, Jingping (2013) found that "men's giving was sensitive to category setting and public reporting, while women's [was] not", arguing that "compared to women, men value a generous image and conform more to the profile of 'image-seeker' in pro-social behaviors." This is consistent with Pan and Houser (2011), who found that males but not females responded positively to a competition to be the best contributor in a public goods game, and with Rigdon et al. (2009) who found that men but not women, transferred more in response to a minimal social cue (three "watching eyes" dots).

not distinguish by gender.

[8] Breman draws on Thaler and Benartzi (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).

[9] This motivates the question "when does the benefit of giving occur and how long does it last?" By the logic of these papers we might expect to see charitable giving exclusively through end-of-life bequests (at least in countries where bequest giving is not penalized). These commitments would yield warm glow that could be savored over one's entire life, leaving the remaining wealth for personal consumption of durable goods that could be enjoyed sooner, and for longer. However, only 7% of people leave money to charity in their will. Bequest giving raises £2 billion a year, which is approximately 20% of UK giving by individuals (Cabinet Office, 2011, Giving White Paper, HM Stationary Office.)

At the extensive margin, Meier (2007) also found in a field experiment that males were more sensitive to social information.[10] Böhm and Regner (2013) and Van Vugt and Iredale (2013) offer further evidence that public goods provision responds more strongly to being observed for men than for women, the latter find this holds mainly when the observer is a women deemed attractive. These authors explicitly invoke an account from evolutionary biology, arguing that for men, providing public goods signals their generosity, making them more attractive to women.

We present the results of a laboratory and a field experiment—with complimentary strengths—that offer the first systematic insight into contingent giving from known or uncertain income. Our first piece of evidence comes from a web-based field experiment involving students at the University of Essex.[11] Participants completed a survey or online task in return for a 25% chance of winning a prize worth £20 (a restaurant or an Amazon.co.uk voucher). When they entered the website they were assigned either to the *Before* treatment, which asked "Before we reveal if you have won ... how much you would like to donate, if you win the prize ...?" or to the *After* treatment, which asked winners "Now that you have won ... how much you would like to donate ...?". Committed amounts were automatically deducted from the prize (voucher amounts were reduced). While we found no strongly significant differences in amounts committed overall, in light of previous work, we differentiated our results by gender. For *male* (but not female) subjects we found greater giving in the *Before* (relative to the *After*) treatment. The gender difference in response to this treatment was statistically significant.

Our laboratory evidence, discussed below, serves to confirm and replicate this result, and allows us to provide more nuanced findings. Our field evidence can be seen as reduced form. It suggests that a *before* ask might be successful in some field settings if pledges were made binding, but it does not isolate the mechanisms responsible for this. Several behavioral economic models could broadly explain the differences. As noted above, the *Before* and *After* treatments differ in at least three respects. There are both theoretical and practical implications of identifying the effects of each element; policymakers and fundraisers need to know which elements are (more) important in tailoring their policies. For example, if it is merely the *uncertainty* of the commitment that increases (expected) contributions it might be effective to ask for conditional commitments, e.g., "in the event of a large Ebola outbreak in West Africa in 2016", or to ask for commitments to donate bone marrow "in the unlikely event that you are a match."

To get at this, our lab experiments varied the presentation of the earnings as random or fixed, the timing of the contribution request, and the probability that the committed donation was collected. Depending on the treatment, we observed conditional pre-commitments for (losing and) winning states, decisions after winning (or losing) a lottery, or decisions over certain income. We found small treatment effects on average, and insignificant overall differences in the amounts

[10] Students at Zurich were asked to donate a specific amount to one or both of two social funds, and were presented with one of two pieces of information about the rate of contribution. Male than (but not female) students at Zurich were significantly more likely to donate to at least one fund when they were told that a higher percentage of students had recently donated, and gender differences were significant.

[11] We also ran an earlier "*Commitment*" field experiment following up a Valentine's day card site at three UK universities. In this earlier experiment, we found greater commitments were made in the *Before* treatment, with similar results for both genders. However, these commitments often went unfulfilled and may have been based on a very different calculus. The *actual* donations were not significantly different across treatments (but given the sample size there was little statistical power to detect a difference here). This suggests that the default option of automatic deduction may have been critical, but because of design differences, more evidence is needed on this point. Details of the *Commitment* experiment are available by request.

committed. However, as in our field experiment, we found significant treatment effects for males and significant gender differences. Furthermore, males committed more in treatments where the *donation* was uncertain, whether or not the income was uncertain. This suggests a (self-)signaling motivation may be driving the male response, which is consistent with the aforementioned prior evidence on gender, reputation, and signaling.

Our paper proceeds as follows. In Section 2 we formalize the experimental environment and the notation. We introduce basic models of expected utility over outcomes, signaling, loss aversion, and affective state. We discuss the predictions of these models for the "before versus after" comparisons and for the variations of these used in our laboratory experiment, allowing us to empirically differentiate between models. In Section 3.1 we present the details of our field experiment, while Section 3.2 explains the key elements and implementation of our laboratory experiment. In section 4 we first present our main results across both experiments, and then present more detailed results for our laboratory experiment. Section 5 concludes.

2 Basic Setup and Predictions

In this section we offer a theoretical perspective on giving when income and/or a donation's collection is uncertain. We present several simple models, explaining how these yield distinct predictions, and thus how our experimental findings can be differentiated from alternative hypotheses.

2.1 Canonical setup

We first depict several variants of "normal giving," "conditionally asking before," and "asking after," essentially describing each of the treatments used in our laboratory and field experiment.[12] We use these to explain and distinguish the predictions of several models of "giving in uncertain environments"; these differential predictions are summarized in Table 1 at the end of this section.

Consider two income levels, w and ℓ, where $w > \ell > 0$. For our *Benchmark* setting, a person with a certain income of w or ℓ may donate g_w or g_ℓ, respectively. On the other hand, she may know she is facing a lottery with a non-degenerate probability p of winning a prize $w > 0$ and a probability $1 - p$ of losing and getting a prize $\ell \geq 0$. Consider the following settings.

***After* setting (A):** She learns whether she has won or lost the lottery. If she has won, she is then asked to donate to a specific charity. She donates $g_w^A \geq 0$. If she loses, she is also asked to give, and she donates $g_\ell^A \geq 0$.

***Before* setting (B):** 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.[13]

[12] See Table 2 for a concrete depiction of the lab procedure. Although the prize in the our field experiment was either a dinner or an Amazon voucher, a restricted form of consumption, this is inconsequential to our argument. See Pollak (1969) for a discussion of conditional demand; a "gift in kind" that cannot be traded can be considered in that context.

[13] In the first wave of laboratory experiments we also included a *"Before Both"* treatment (BB). This was as in *Before,* but the subject was also asked to commit to give $g_\ell^{bb} \geq 0$ if she loses. As our self-signaling model does not make a clear prediction for this treatment, we dropped it in our second wave to more efficiently test this model, and for brevity. Our results are not substantially affected by the inclusion or exclusion of this treatment

Our main question is: how does her commitment, in the *Before* setting, to donate "if she wins", compare to her donation in the *After* setting when she has already won, i.e., $g_w^b \lesseqgtr g_w^a$.

For all of the donations above, after any uncertain earnings have been resolved, the donation chosen for that state is passed to a charity with certainty. However, ex-ante, the individual in the *Before* setting who commits to donate "if she wins" will only have to pass this on with probability p. To isolate the "uncertain donation" aspect of the *Before* setting we consider:

Uncertain setting (U): The individual's income is certain (at w or ℓ) but she commits to donate knowing the donation g_w^u or g_ℓ^u (from income w or ℓ, respectively) will only be collected with probability $P = p$, and otherwise she will keep this money.

Expected utility maximization over outcomes

In the most widely cited 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 the original theoretical papers generally do not consider uncertain environments, they have been applied in 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 (Bénabou and Tirole, 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.[14] 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 (trivially proved in the appendix).

Prediction 1. *Expected utility maximizers*

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

2.2 (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 Kircher et al. (2009). In

(available by request). In general, male (but not female) subjects committed more from the winning income in the BB treatment, relative to the baseline. For both genders the commitment from the winning income in the BB treatment is similar to the commitment in the *Before* and *Uncertain* treatments.

[14] 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.

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 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 (Bénabou and Tirole, 2003), yielding a utility gain. On the other hand, she is sacrificing some expected income, hence sacrificing utility from consumption. Both the signaling benefit and the utility benefit of additional income may not only be a function of the expected values, but may depend on the probability distribution. As noted above, Tonin and Vlassopoulos (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.

Below, we offer a simple signaling model 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").[15] 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, 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.

We demonstrate, using a simple signaling model, that the uncertain collection of donations can lead to higher (conditional) donations. We then argue that this extends to the setting where income is uncertain and individuals commit an amount to donate, if anything, in the event of their receiving a bonus. In our example and our lab experiment, the probability that a subject's committed donation is realized is the same value ($p < 1$) for *Before* and *Uncertain* treatments; for the *Benchmark* and *After* treatments this probability is 1. Note that if one only recalls his or her own true type with some error, the signaling model can be considered as an equivalent *self*-signaling model, as noted in Bénabou and Tirole (2006) and formalized in Bénabou and Tirole (2011). Cueva and Dessı (2010) offer experimental evidence for this model and for self-image concerns in a charitable giving context.

[15] Grossman (2015) presents a related model (independently developed). 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 solves for cases that vary 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, he does not explicitly model the case where the observer (or future self) sees both the choice and the probability, as in our model.

Signaling Model of Reputation with uncertain collection

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

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

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, "good" or "bad," respectively, drawn by nature with $pr(\theta = 1) := \mu \in (0, 1)$.[16] $u(\cdot)$ represents the sub-utility of own-consumption. The function $\omega(Dg)$ 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 ϕ may depend on $\mathbf{g_{-i}}$ and g in equilibrium, where $\mathbf{g_{-i}}$ is the vector of others' committed contributions.[17]

As in Bénabou and Tirole (2006), we consider a direct payoff from reputation (in a social or self-signaling context, "which may be instrumental ... or purely hedonic"). By standard assumptions, she will maximize the expected value of this Bernoulli utility function subject to the budget constraint

$$x + g \leq E,$$

where E denotes wealth. We consider only one income level initially and hence omit any superscripts indicating income. The expected value of the utility can be restated as

$$U^{\theta}(E, g) = u(E) + p[u(E - g) - u(E)] + p\theta\omega(g) + R(\phi),$$

where $p = pr(D = 1)$, i.e., the probability (at the time the donation decision is made) that the donation will be collected. We consider equilibria where someone is assumed to be a potential good type only if he donates 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.[18]

[16] Our key insights will 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.

[17] 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 Bénabou and Tirole (2011). These 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 ..."

[18] 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.

Separating equilibrium: constraints

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

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

The relevant incentive compatibility condition of the bad type requires

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

Let g^* represent a good type's preferred donation *net of reputation,* i.e.[19]

$$g^* = argmax_g\ \{u(E-g) + \omega(g)\}.$$

Solutions

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

$$-p[u(E-g^*) - u(E)] \geq r. \tag{4}$$

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 by the intuitive criterion 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, $-p[u(E-g^*) - u(E)] < r$.

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:

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

In this case, if the collection probability p decreases, the minimum level of conditional donations that separates types ($g_1^{\min}$) increases.[20]

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

[20] We may also have pooling equilibria where both types contribute g^{pool} satisfying $g* \leq g^{pool} < g_1^{\min}$. These are

Summarizing Cases 1 and 2 Thus, beginning at a value of p where the separation constraint binds, 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.[21]

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* setting are all $p = 1/2$. Suppose that the reputational benefit is such that case 2 applies for $p = 1/2$ while case 1 obtains if $p = 1$. This would imply that in the *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 *Uncertain* donation commitment.

We can extend this to situations where income is also uncertain ($E = E_h$ with probability p, $E = E_\ell$ otherwise), and donations are collected only if income is high. The objective function will become:

$$U^\theta(E, g) = pu(E_h) + (1-p)u(E_\ell) + p[u(E_h - g) - u(E_h) + \theta\omega(g)] + R(\phi).$$

This leaves the above analysis unchanged. Where the "winning and collection probability" p is low enough relative to reputational benefits r, the intuitive equilibrium *Before* commitment of good types, $g_1^{\min}$, increases above g^*. However, for this same probability p, an ask *after* a win will only elicit a donation of $g^* < g_1^{\min}$, provided r is not too high, or else a donation between $g_1^{\min}$ and g^*.[22]

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

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

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

for good types, while bad types are unaffected by the treatment. A similar relationship will

possible where bad types are willing to contribute g^* even to gain the lower reputation $\tilde{R}(g^{pool}|pooling)$. I.e.,

$$-p[u(E - g^*) - u(E)] < \lambda r < r, \tag{6}$$

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.

[21] The net effect on expected contributions $pg_1^{\min}$ depends on the concavity of the material sub-utility function $u(\cdot)$ We have $-p\frac{dg_1^{min}}{dp} = \frac{u(E)-u(E-g_1^{min})}{u'(E-g_1^{min})} \geq g_1^{min}$ (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.

[22] Note also that as the ex-ante probability that the donation is realized is the same in either setting (*Before* or "after a win") the expected value of the donation will be higher for the *Before* ask than it would be if the individual was only asked to donate after winning.

hold for donations from the lower level of income if condition 4 also fails at $E = E_\ell$, which need not be the case.[23]

Heterogeneity This model can be extended to reflect signaling *among* individuals in a "group" with identical observable characteristics, e.g., gender. 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) will hold for another group and the donation commitments will respond to the uncertain collection for the latter group only.[24]

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 and Kahneman, 1991). Thaler and Benartzi (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).[25] While the reference point may change over time, we assume here that she is *myopic* in the sense that when making a (commitment) decision she does not anticipate these changes. For simplicity, we consider a utility function embodying a linear loss function, i.e.,

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

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, r is a reference point, specified below, and δ is a 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 $\omega(\cdot)$.

[23] Under the standard assumption that $u(\cdot)$ is concave, the parameter space where this holds at income E_ℓ is a proper subset of the parameter space where this holds at income E_w.

[24] 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.

[25] 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.

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. 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.[26] 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:

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

$$g_w^b > g_w = g_w^a = g_w^u \, (where \; g_w < w - z) \, and$$
$$g_\ell = g_\ell^a.$$

The analysis above generalizes to any intermediate reference point (derivation in appendix.) 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*

$$g_w^b = g_w^a > g_w = g_w^u \, and$$
$$g_\ell^a < g_\ell.$$

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 and Isen, 1975; Weyant, 1978; Underwood et al., 1976; Bekkers and Wiepking, 2011; Kidd et al., 2013; **?**). On the other hand Fishbach and Labroo (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".[27]

[26] 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 and Rao (2011) and models in Reinstein (2011) and Kotzebue and Wigger (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.

[27] In "Tournament Outcomes and Prosocial Behaviour" Kidd et al. (2013) participants take part in a real effort tournament followed by the opportunity to contribute to a set of well-known charities. The authors found that those who are higher ranked contribute more (after controlling for earnings) and argue that this is driven by the "positive affect experienced by the winners [...] determined by the difference between their realized and expected ranks."

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, 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.[28]

Prediction 5. *Affective state*

$$g_w^a > g_w = g_w^b,\ g_\ell^a < g_\ell$$

2.5 Magical thinking and other theories

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 individual who believes in Karma (cf. Levy and Razin, 2006) may believe she will be rewarded for good acts (or good commitments) and punished for bad ones.[29] 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). Details are in the online appendix ("Additional results").

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 ("Alternate and empirically equivalent models"), arguing that these are less relevant than the models presented above, and are not supported by our evidence.

[28] Similar predictions could arise out of an (indirect) reciprocity model (see Simpson and Willer, 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.

[29] 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.)

2.6 Summary of theories and empirical implications

	Benchmark	*After*	*Before*	*Uncertain*
Wealth probabilities, at time of donation	Certain (w, ℓ)	$pr(w) = p$; resolved	$pr(w) = p$; unresolved	Certain (w, ℓ)
Probability commitment is collected	*Certain*	*Certain*	P=p	P=p
Observations	g_w, g_ℓ	g_w^a or g_ℓ^a	g_w^b	g_w^u,g_ℓ^u
NB: $0 < P = p = pr(w) < 1$; wealth $\ell < w$				
Models' predictions (vs. benchmark)				
1. Expected utility max (over outcomes)		(=,=)	=	=
2. Signaling generosity		(=,=)	+	(+,?)
Where Case 2 holds for w, $P = p$				
3. Loss averse, linear loss function,				
Expected wealth (or intermed.) refc. pt.				
a. Immediate adjustment		(=,=)	+	=
b. No (or slow) adjustment		(+,=)	+	=
4. Affective state (unanticipated)		(+,-)	=	=

Table 1: Potential comparisons, predictions

The *Benchmark* column in Table 1 indicates the standard allocation of certain wealth in the *Benchmark* setting (here, as in the *Uncertain* column, w and ℓ should not be thought of as a "win" or "loss" incomes, but just as two levels of income). In the columns *After, Before*, and *Uncertain*, we depict the predicted differences from column the benchmark, for these potential variations in the setting. Where two elements are presented in parenthesis, this represents the donation from w and from ℓ respectively. Rows 1–4 represent the distinct models described above. None of the rows in this table are identical across all columns; thus, they can be differentiated empirically. In particular, note that the signaling model is the only one that predicts that the committed donation will respond to the certainty of the donation's *collection*.[30]

3 The Experiments

We ran both a field and a lab experiment; each have complementary strengths. The field experiment environment has some generalizability to real-world fundraising: universities often run employability promotions, and web sites often ask for donations.

[30] However, the theory allows for *potential* overlap in contributions between the models, depending on the size of the effects, on how reference points move, and on other parameters. We may also expect heterogeneous behavior and some or all of these effects may be present for any individual, and the relative importance of these effects may vary by individual and with the environmental context. Thus, in between-subject experiments, we are not able to exactly identify an individual's motivation (however, within-subject designs are vulnerable to reference point and apparent-contrast effects). We see our results as plausible measures of the most quantitatively important net effects. Note also that for 3b in the *After* column the latter equality depends on our assumption of a linear loss function; relaxing this, the difference will depend on the curvature of the loss function. For loss aversion models the *Uncertain* prediction relies on the assumption that the reference point does not depend on the anticipated contribution (see Footnote 26).

3.1 Field experiment: Design

Our field experiment was launched in the context of an employability promotion funded by the University of Essex Faculty of Social Sciences; all promotional information was explicitly noted as coming from this faculty.[31] Eligible undergraduate students were sent a series of emails 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!

The site was also promoted through extensive flyering, postering, university web sites, and online social media. We obtained 352 valid responses that involved a donation choice.[32] No students were allowed to participate more than once.

Participants first signed in with their email, department, and study year. Half of the participants were then asked to sign up for a job site (JobsOnline) and enter two jobs of interest to them.[33] Next, they were informed of the prize which they had a 25% chance of winning (the prizes selection was orthogonal to other treatments). After this they were presented our donation treatments, seen in the screenshots in Figure 1,which closely resemble the Before and After treatments described above. We gave away two prizes—the dinner and the Amazon voucher—and asked for contribution commitments, which were to be automatically directly deducted from these prizes (i.e., we would reduce the voucher/certificate amounts by the amount donated).[34]

As the screenshots show, we made several choices aimed at increasing the baseline rate of contributions. Participants were required to either enter a donation amount and a charity, or uncheck a box if they preferred not to donate. 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. All donation commitments were automatic deducted from prizes won, and we informed participants of this, stating "…your donation will

[31] This faculty included Economics, Government, Sociology, Language and Linguistics departments, the Center for Psychoanalytic Studies, and later the Essex Business School. The eligible students were in their first and second years through October 2013, and then were in their second and third (final) years in following academic year. In the later run, we began with this same set of departments, and ultimately expanded eligibility to all undergraduate students in all departments at the University of Essex, in order to be able to use all of the prizes before our institutional deadline. In the online appendix we give further practical details and a timeline.

[32] Recall that those in the *After* treatment who did not win were not asked to donate. Only one entry was included per participant—repeat participants' later entries were removed. We could look up or infer gender for 339 observations; details on response rates, attrition, and invalid responses are given in the appendix.

[33] 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 on their assignment ordering are given in the online appendix.

[34] Randomization checks on the treatment balance (and summary statistics) are given in Table 3. As noted, the experiment was run over two academic years, with a virtually identical web site and two separate but very similar promotions. In the later run we only offered the Amazon voucher prize. Results (by request) were similar for both years. In the online appendix we give further practical details and a timeline.

Figure 1: Field experiment screenshots

(a) Before

(b) After (win)

be automatically deducted from your prize and passed on to the charity of your choice, plus an additional 10% from our own funds…" Extensive further details, including additional screen shots, a timeline, information on other treatment arms, and implementation details, are given in the online appendix.

3.2 Laboratory Experiment: Design and implementation

As discussed above, the laboratory environment permits more control and a wider variety of treatments; we use this to differentiate between alternative theoretical models. Our laboratory design followed the description in Section 2.1; with a *Benchmark* of certain income, and *After, Before*, and *Uncertain* donation treatments; this is depicted in Table 2.[35]

Subjects were seated at computer terminals and given a code number. They were told that they were to perform a Raven's matrix task lasting about half an hour (Raven, 1936).[36] Subjects were told they would be rewarded €7 for this (or €14 in more than half of the *Benchmark* and *Uncertain* treatments) independently of their performance.[37] After the task, in screen III, the *Benchmark* and *Uncertain* subjects were reminded of their earnings, and the rest were told:[38]

> With a probability of 50 percent you will be rewarded a bonus of €7 on top of your already acquired income of €7. Whether you will be rewarded this bonus depends on the code that you have been given at the beginning.

We used this tangible pre-assigned code, in conjunction with a sealed envelope pinned to the laboratory door, to demonstrate to the subjects that neither their performance nor their donation choices could affect their chances of winning (details are in the online appendix).[39]

The donation decisions followed the design described in Section 2.1. Those in the *Before* treatment were next 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)". We will increase your donation by an additional 25 percent taken out of our own budget. At the end of a session, one person will be randomly chosen to monitor that the conductor of the experiment will transfer all collected donations to the respective organizations. Please enter the amount of your donation in case of you winning the bonus (amount can be between

[35] In our first wave we also included a *"Before Both"* treatment; results are not sensitive to this, as discussed in an earlier footnote. These results are available by request.

[36] This task is a language-free multiple choice intelligence test where subjects have to recognize patterns and choose the correct pattern to complete a sequence of patterns. The version we use consists of five blocks with twelve sequences of patterns each.

[37] 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.

[38] All instructions are translated from the original German. The complete set of screenshots and translations are given in an online appendix.

[39] This may also reduce the influence of magical thinking on donation behavior; some subjects may believe that "karma" can influence future but not predetermined events.

	Treatment			
Stage	Benchmark	After	Before	Uncertain
1	Info on pay for task: [€7 or €14]	. . . (always) [€7]	. . . (always) [€7]	. . . either [€7 or €1
2		Real effort task		
3	Reminded: Income [€7/€14]	—— Info: possible bonus [€7/€0] ——		Rem: [€7/ €14]
4	☐	☐	*Donation (if win bonus)*	☐
5	☐	——— Bonus revealed ———-		☐
6	*Donation (unconditional)*	*Don. (unconditional)*	☐	*Don. (if collected*
7-10	Incentivized questions about others' donations, hypothetical questions about own donations, survey			
11	Reveal/confirm outcomes, make payments			

Table 2: Experimental Design
Note: Boxes (☐) imply that this stage was not present for the treatment in this column. *Donation decisions are in italics.*

> €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. Please choose the charity you want to donate to (if you have entered 0 above, no selection is necessary).

Next (screen V), *Before* and *After* subjects were told whether they won the bonus. *After* subjects were then asked if they would like to donate, with similar language as above, i.e.:

> 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. . . . Please enter the amount of your donation (amount can be between 0 and [total earnings: €7 or €14]). . . . Please choose the charity . . . [ellipses indicate text in common across treatments]

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

The *Uncertain* subjects were instructed:

> With a probability of 50% you will now have the opportunity to donate a part of the income you have earned in this experiment to a charitable organization. Whether you will have the opportunity to donate depends on the code that you have been given at the beginning—it is independent of your performance regarding the tasks . . . Please enter the amount of your donation in case of you being able to donate (amount can be between 0 and [total earnings, €7 or €14]). In case of you not being able to donate nothing will be deducted from your income and the charity will receive no donation.

Following the donation decision, we asked the subjects to make a series of incentivized and hypothetical predictions, followed by survey questions.[40]Finally, we revealed net earnings including

[40] 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. These results are largely of independent interest, and not strongly related to the main question of this paper.

earnings from predictions, and revealed to those in the *Uncertain* treatment whether their donations would be collected. We opened the sealed envelope to verify to subjects that the random draws (bonuses and collection) had indeed been pre-determined. Payments were made and donations were then passed to the charities, with a subject monitoring this, as promised.

The laboratory experiments were run in Dusseldorf and Mannheim on a standard experimental subject pool, using virtually identical protocols and Ztree code at each lab (Fischbacher, 2007). We ran nine sessions over five days in January and February of 2013 and November 2014. A complete set of relevant screenshots and translations are available in our online appendix.

4 Results

4.1 All experiments

We first present evidence on whether our randomization successfully balanced the treatments. Table 3 reports the results of comparisons between two treatment arms of the field experiment and among the four treatments in the laboratory experiment. The mean values of observable variables are similar across treatments, and we do not detect significant differences in either the field or the lab experiment. All donations are reported in Euros.[41] 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 field experiment were not asked to donate).

[41] Donations from the field experiment in the UK are evaluated at an exchange rate of 1.1971 EUR/GBP (October 1, 2013 rate).

Table 3: Summary statistics and randomization checks

Panel A: Field Experiment

	(1) Before	(2) After	(3) p-value	(4) Obs
Jobs Treatment	0.25	0.21	0.50	352
	(0.03)	(0.05)		
Female	0.51	0.58	0.30	348
	(0.03)	(0.06)		
E. Bus. School	0.24	0.28	0.50	352
	(0.03)	(0.05)		
Observations	280	72		

Panel B: Laboratory Experiment

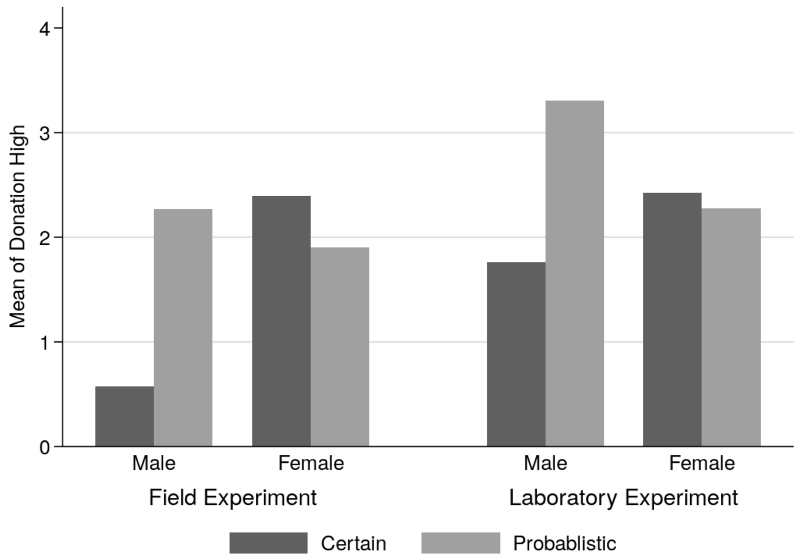
	(1) Income Certain	(2) Before	(3) After	(4) Uncertain	(5) p-value	(6) Obs.
High Income	0.54	0.53	0.49	0.64	0.52	195
	(0.08)	(0.07)	(0.07)	(0.07)		
Female	0.51	0.55	0.45	0.51	0.77	195
	(0.08)	(0.07)	(0.07)	(0.07)		
Age	24.41	23.69	24.18	25.15	0.54	195
	(0.88)	(0.45)	(0.60)	(1.02)		
No religion	0.59	0.50	0.57	0.49	0.72	195
	(0.08)	(0.07)	(0.07)	(0.07)		
Previous donor	0.41	0.38	0.29	0.32	0.57	195
	(0.08)	(0.06)	(0.07)	(0.07)		
Observations	41	58	49	47		

Note: This table reports the results of comparisons of the treatments in the Lab and the Commitment experiment. For the field experiment we test for differences between the treatments using a t-test. In the laboratory experiment we test for differences among all treatments with a joint F-test. For the field experiment the number of observations for the Female variable is lower than for the other characteristics; due to a technical error on our web site we could not verify the gender for four of the participants.

Variables: "E. Bus. School" indicates participants who were students in the Essex Business School. "High Income" represents those with treatments where they earned 14 Euros. "Previous donor" indicates participants who indicated in the survey that they had recently donated to charity.

Next, we compared the donations for our treatments with probabilistic collection (pooling *Before* and *Uncertain* for the lab experiment*)* to the donations for treatments with certain collection (pooling *Benchmark* and *After* for the lab). This comparison is depicted in Figure 2, and summary statistics and parametric and non-parametric tests are in Table 4.

Figure 2: Mean donations/commitments (from higher income) by experiment and by certain versus probabilistic collection



Note: This reports the average donations for all treatments with certain collection versus all treatments with probabilistic collection. For the Lab experiment, we pool *After* and *Income Certain* to create the group *Certain.* We pool the treatments *Before* and *Uncertain* into the group *Probabilistic.*

Table 4: Donations/commitments by experiment and by certain versus probabilistic collection

		Field			Lab		
Treatment		Total	Male	Female	Total	Male	Female
Certain	Mean	1.63	0.56	2.39	2.07	1.76	2.43
	Median	0.00	0.00	0.00	2.00	1.00	2.00
	75th percentile	1.20	0.00	2.39	4.00	2.00	4.00
	s.d.	(3.76)	(1.56)	(4.62)	(2.07)	(2.26)	(1.80)
	Positive Donation	0.31	0.17	0.40	0.65	0.56	0.76
Probabilistic	Mean	2.02	2.24	1.86	2.81	3.31	2.28
	Median	0.00	0.00	0.00	2.00	4.00	2.00
	75th percentile	1.20	1.20	2.39	4.00	5.00	4.00
	s.d.	(4.71)	(5.43)	(3.99)	(2.80)	(3.26)	(2.14)
	Pos. Donation	0.31	0.30	0.32	0.73	0.71	0.74
Total	Mean	1.94	1.93	1.98	2.55	2.76	2.33
	Median	0.00	0.00	0.00	2.00	2.00	2.00
	75th percentile	1.20	1.20	2.39	4.00	4.00	4.00
	s.d.	(4.53)	(5.00)	(4.13)	(2.59)	(3.02)	(2.02)
	Pos. Donation	0.31	0.28	0.34	0.70	0.66	0.75
Difference (certain vs. probabilistic)		-0.39	-1.68	0.53	-0.74	-1.55	0.15
p-value (t-test)		0.51	0.10	0.47	0.12	0.04	0.78
p-value (Wilcoxon rank sum)		0.89	0.10	0.30	0.16	0.03	0.59
Observations		352	164	184	134	70	64

Note: This table reports on donations for all treatments with certain collection versus all treatments with probabilistic collection, excluding the donations from the lower income level. For the Lab experiment, we pool *After* and *Income Certain* to create the group *Certain.* We pool *Before,* and *Uncertain* to set the group *Probabilistic.* The sum of observations in columns 2 and 3 is less than column 1; due to a web site error, we could not verify the gender for four participants.

Next we consider this in a linear regression context. Table 5 reports the effect of a donation's collection being probabilistic (rather than certain) on the amount donated. Again, in columns 3 and 4, we pool *Before* and *Uncertain* treatments to create the dummy Probabilistic, indicating that subjects knew that their committed donation was collected with a probability below one. In columns (5) and (6) we exclude the *Uncertain* treatment from the analysis so that Probabilistic reflects only the *Before* treatment. The qualitative results are the same in all columns: males gave more in response to probabilistic treatments, females did not, gender interactions are significant, and the average effect of treatments (pooling across genders) is insignificant or marginally significant. Summarizing these findings:

Result 0: In aggregate, we find no robustly significant effects in the field experiment, while in the lab experiment we observe an overall positive effect of the aggregated probabilistic treatments.

Result 1: In both lab and field experiments, males donated more in the probabilistic treatment(s) than in the certain treatment(s), as reported in Figure 2 and Table 4. For both experiments, the relevant coefficients are significant in a linear regression, as shown in Table 5. The difference is also significant in rank sum tests, as shown in Table 4. In contrast, for females there is no significant difference between these treatments in either the field or the laboratory experiment; our sample yielded limited power to detect an effect.[42]

Result 2: In both lab and field, the amount donated responded significantly more positively to the probabilistic treatments for males than for females, as seen in Table 5.

Table 5: Ordinary Least Squares regressions of donation amount

	Field		Lab			
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.069	1.84**	-0.52	1.08	-0.31	1.18*
	(0.49)	(0.76)	(0.44)	(0.67)	(0.47)	(0.69)
Probabilistic	0.42	1.68***	0.85*	1.56**	0.71	1.41**
	(0.51)	(0.55)	(0.43)	(0.68)	(0.45)	(0.70)
Probabilistic × female		-2.21**		-1.61*		-1.50*
		(0.96)		(0.84)		(0.86)
Constant	1.59***	0.56**	2.48***	1.77***	2.42***	1.73***
	(0.46)	(0.28)	(0.42)	(0.52)	(0.42)	(0.54)
Observations	348	348	134	134	104	104

Notes: Hidden controls—Regressions in columns 3–6 include a laboratory location dummy [Mannheim] and an interacted dummy [Mannheim×Female]. For the lab, we exclude donations from the lower income, and we pool *Before,* and *Uncertain* to set the group *Probabilistic* (in columns 5 and 6 *Uncertain* is excluded so this represents *Before* only). Robust standard errors are reported in parentheses. Results of t-test indicated at following significance levels * p<0.1, ** p<.05, *** p<.01.

We next examine the impact of our treatments on *whether* participants donated. Table 6 reports the effect of a donation's collection being probabilistic on whether an individual committed

[42] Given our sample size and estimated standard error, if the treatment effect for women was 10% of the base treatment, we had only a 6% chance of detecting such an effect in the field experiment, and an 8% chance in the lab experiment. The test's power only attains 80% or better if the treatment effect was over 96% of the female mean for the base treatment in the field experiment, or if it was over 58% of this in the lab experiment. (Estimated power from a two-sample means test Satterthwaite's t-test allowing unequal variances).

to make a donation. Again, we pool the lab treatments *Before and Uncertain* to create the dummy Probabilistic, and again in columns (5) and (6) Probabilistic represents the *Before* treatment only. In the field experiment, the probability that a male in the *Before* treatment committed to donate something is significantly larger (at the 10% level) than the probability that a male in the *After* treatment donated. The gender interaction is also significant: females also reacted significantly less positively to probabilistic treatments at the *extensive* margin. The results are qualitatively similar in the laboratory experiments (see columns 4 and 6), but are not statistically significant.

Result 3: Male subjects in the field experiment were more likely to commit to donate in the *probabilistic* treatment relative to when their earnings are known (significant at the 10% level), and the gender difference is significant, as shown in Table 6.

Table 6: Linear probability model regressions of donation incidence

	Field		Laboratory			
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.051	0.24**	0.091	0.23	0.079	0.22
	(0.049)	(0.10)	(0.080)	(0.15)	(0.092)	(0.15)
Probabilistic	0.0059	0.14*	0.071	0.14	0.074	0.18
	(0.060)	(0.079)	(0.086)	(0.13)	(0.093)	(0.14)
Probabilistic × female		-0.23**		-0.16		-0.21
		(0.12)		(0.17)		(0.19)
Constant	0.28***	0.17**	0.61***	0.55***	0.63***	0.57***
	(0.058)	(0.068)	(0.087)	(0.11)	(0.091)	(0.11)
Observations	348	348	134	134	104	104

Note: Hidden controls–Regressions in columns 3–6 include a laboratory location dummy [Mannheim] and an interacted dummy [Mannheim×Female]. For the lab, we exclude donations from the lower income, and we pool *Before,* and *Uncertain* to set the group *Probabilistic* (in columns 5 and 6 *Uncertain* is excluded so this represents *Before* only). Robust standard errors are reported in parentheses. Results of t-test indicated at following significance levels * p<0.1, ** p<.05, *** p<.01.

For robustness to functional form mis-specification we estimated a negative binomial specification, regressing donation amounts on the same variables as in Table 5. This is one method to address the nonlinear response which must characterize the true data generating process, as giving can never be negative. For donation incidence we estimated a Probit regression paralleling Table 6. In each case the results, reported in the online appendix ("Alternative specifications"), are consistent with our linear regressions; however, the (male) coefficient on "Probabilistic" is no longer significant in the Probit regression.

4.2 Specific Results: Laboratory Experiment

We next present a more detailed analysis focusing on our laboratory data, which offers a richer set of treatments; average responses are depicted in Figure 3.

Figure 3: Mean donation by treatment, by gender

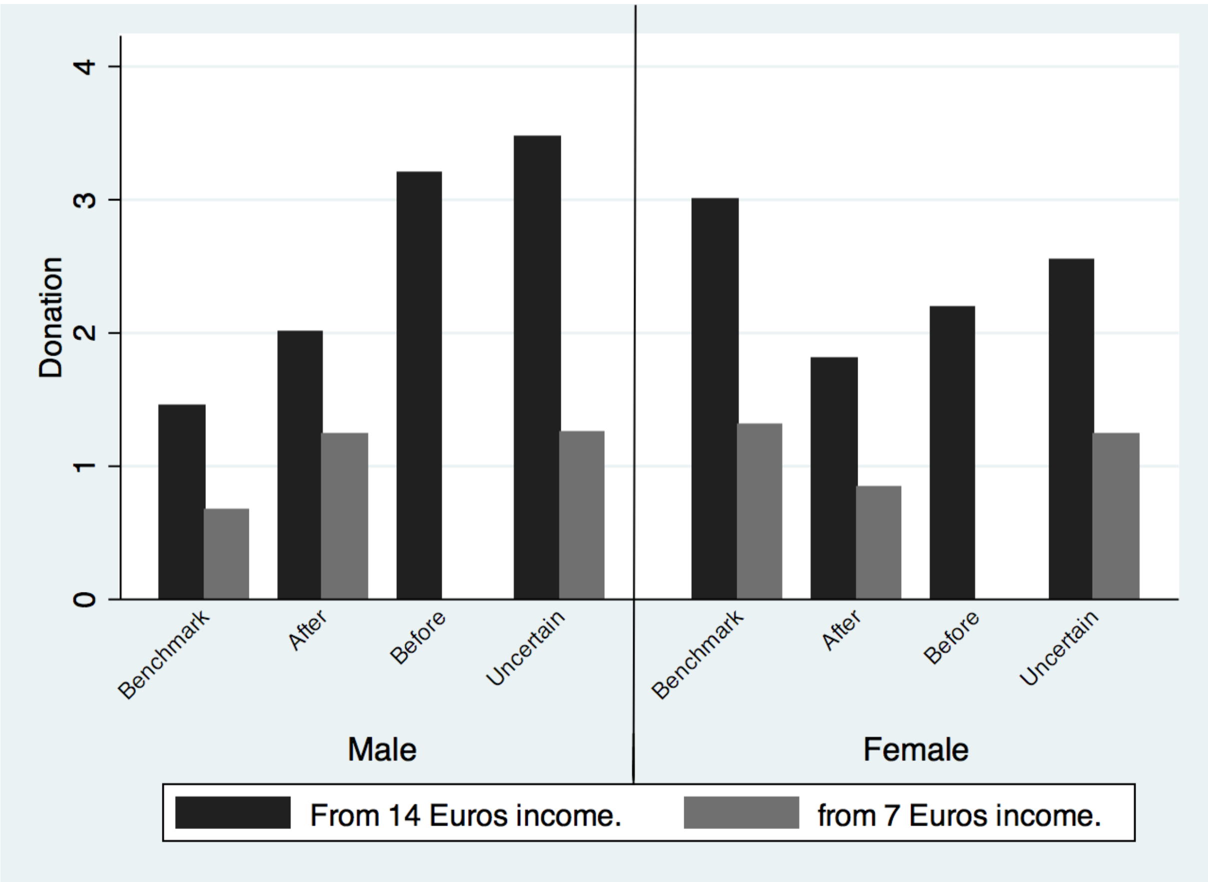


Table 7 analyzes the impact of each treatment on the amount committed to be donated. Considering the donations/commitments from 14 Euros, we see strong treatment effects for males, and some gender differences. Relative to benchmark donations, men committed significantly more in the *Before* treatment, as well as in the *Uncertain* collection treatment; there is no statistically significant difference in males' giving among the treatments with uncertain collection. Women reacted less positively than men to all treatments compared to the baseline, and this difference is significant for *Before* and *Uncertain* treatments. We find no significant differences for donations or commitments from 7 Euros; although the differences mainly had the same same signs as for the "Earn 14" columns, our sample yielded limited power to detect an effect.[43]

[43] Given our sample size and estimated standard error, we had only a 6% chance of detecting an effect if the treatment effect was 10% of the base treatment. The test's power only attains 80% or better if the treatment effect was over 121% of the mean for the base treatment. (Estimated power from a two-sample means test Satterthwaite's t test allowing unequal variances). Confidence intervals are wide. In column 8 of Table 7, the Uncertain (male) coefficient has a 95% confidence interval of (-1.919, 2.91), and for "Uncertain × female" the CI is (-3.40, 2.11). Further details by request.

Table 7: Ordinary Least Squares Regressions on Donations by Earnings

	Earn 14				Earn 7			
	(1) Pooled	(2) Male	(3) Female	(4) Interact	(5) Pooled	(6) Male	(7) Female	(8) Interact
Certain								
– After	-0.34	0.54	-0.90	0.54	0.031	0.56	-0.49	0.56
	(0.61)	(0.85)	(0.74)	(0.86)	(0.45)	(0.73)	(0.51)	(0.70)
Probabilistic								
– Before	0.53	1.74**	-0.54	1.74**				
	(0.52)	(0.74)	(0.62)	(0.74)				
– Uncertain	0.94	2.02**	-0.33	2.02**	0.16	0.59	-0.23	0.50
	(0.72)	(0.99)	(0.91)	(1.00)	(0.60)	(1.27)	(0.73)	(1.20)
Interaction Female								
– After × Female				-1.44				-1.04
				(1.13)				(0.86)
– Before × Female				-2.28**				
				(0.96)				
– Uncertain × Female				-2.35*				-0.64
				(1.35)				(1.38)
Female	-0.48			1.80**				0.62
	(0.44)			(0.73)				(0.49)
Constant	2.64***	1.46***	3.25***	1.46***	1.08***	0.66	1.47***	0.75**
	(0.47)	(0.51)	(0.52)	(0.51)	(0.33)	(0.47)	(0.46)	(0.34)
Observations	134	70	64	134	61	26	35	61

Note: Hidden controls – Laboratory location dummy [Mannheim] and an interacted dummy [Mannheim ×Female]. Robust standard errors are reported in parentheses. Results of t-test indicated at following significance levels * p<0.1, ** p<.05, *** p<.01.

Result 4: Relative to the *Benchmark,* men (but not women) committed significantly more in *each* of the treatments (*Before, Uncertain)* where the donation's *collection* was uncertain; their response was similar whether or not income was uncertain.

Table 8 analyzes the *likelihood* of committing or contributing something. Here few treatments are individually significant, and there are no significant gender differences.

Table 8: Linear Probability Model on Donation Incidence by Earnings

	Earn 14				Earn 7			
	(1) Pooled	(2) Male	(3) Female	(4) Interact	(5) Pooled	(6) Male	(7) Female	(8) Interact
Certain								
– After	-0.14	-0.13	-0.11	-0.13	-0.095	-0.070	-0.10	-0.070
	(-0.98)	(-0.63)	(-0.56)	(-0.63)	(-0.63)	(-0.33)	(-0.46)	(-0.34)
Probabilistic								
– Before	-0.0090	0.089	-0.092	0.089				
	(-0.08)	(0.50)	(-0.64)	(0.50)				
– Uncertain	0.017	0.042	0.0034	0.042	-0.19	-0.33	-0.16	-0.33
	(0.13)	(0.22)	(0.02)	(0.22)	(-1.10)	(-1.09)	(-0.70)	(-1.11)
Interaction Female								
– After × Female				0.021				-0.034
				(0.07)				(-0.11)
– Before × Female				-0.18				
				(-0.79)				
– Uncertain × Female				-0.038				0.17
				(-0.15)				(0.44)
Female	0.090			0.20				0.045
	(1.12)			(1.01)				(0.18)
Constant	0.68***	0.62***	0.83***	0.62***	0.60***	0.58***	0.62***	0.58***
	(6.15)	(3.99)	(6.53)	(3.99)	(4.92)	(3.14)	(3.57)	(3.18)
Observations	134	70	64	134	61	26	35	61

Note: Hidden controls – Laboratory location dummy [Mannheim] and an interacted dummy [Mannheim ×Female]. Robust standard errors are reported in parentheses. Results of t-test indicated at following significance levels * p<0.1, ** p<.05, *** p<.01.

As we did for the regressions in Section 4.1, we estimated a negative binomial specification parallel to Table 7 and a Probit regression paralleling Table 8. Again, the results (reported in the online appendix "Alternative specifications") are consistent with our linear regressions.

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. 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.

For men in our experiments, donations were higher in treatments where the *donation's collection* was uncertain, whether or not the income was known. This supports a signaling explanation: commitments realized with a lower probability must involve larger amounts to have the same signaling power. Returning to the table of predictions (Table 1), we see that our results for males are consistent only with row 2.[44]

While our field experiment participants may have been making choices in front of peers, in our laboratory experiment a direct *external* signaling explanation may be less plausible; we took strong steps to ensure that subjects identities could not be tracked to their decisions or treatments. Thus, the most plausible explanation may be signaling to oneself and a self-image concern. Still, we might not rule out an *indirect* external signaling explanation. Firstly, subjects may want to discuss their lab experience with others afterward. If it is common-knowledge that lying brings a strong internal moral cost, the *reported* choices may hold a similar signaling power as actual verifiable choices. Secondly, several authors offer evidence suggesting that subjects often bring real-world norms into the laboratory, perhaps as a simplifying heuristic (e.g. Hoffman et al., 1996; Burns, 1985). Thus the male subjects' *lab* behavior may reflect their normal image-seeking *real-world* motivations as well as, perhaps, their self-signaling.

In both experiments we found that women give roughly the same amount whether or not the collection is certain. The significant gender difference in this response could be explained by a smaller reputation motive relative to women's warm glow term; previous evidence for this was presented in the introduction. We interpret this in the context of our signaling model under heterogeneity (as modeled in Section 7). For our lab experiment, if parameter values for the male subgroup resembled case 2 (under $p = 1/2$), this could explain why their *Before* donations exceeded their *After* donations (g^*). This also describes our field experiment (letting $p = 1/4$), and would describe a situation where fundraisers only felt it appropriate to ask for donations from bonuses (or lottery wins, etc).

Our results, found in a particular context for a particular population 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 suggests that contributions involving uncertain realization and/or uncertain income do not follow the predictions of standard expected utility models. Furthermore, at least some subsets of our participants donate more when asked to donate from uncertain gains than when asked ex-post.

Although our experiments are of a limited scale, they may be relevant to other forms of prizes, be they from gambling, from the national lottery, 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 in male-dominated industries like banking to commit to give a share of their bonus (or their "bonus in excess of a specified 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,

[44] I.e., the signaling model where, with uncertain collection, good types can only distinguish themselves from bad types by committing a larger amount than they would otherwise prefer. This signaling model also made a potentially similar prediction for contributions from the *lower* level of income. However, as noted: (i) the prediction need not be the same, as condition 4 depends on income, and (ii) our results for donations/commitments from €7 are not inconsistent with this, but merely *noisy*.

offering another potential application. In general, policymakers could promote "windfall giving", encouraging people to pledge to donate a proportion of their unanticipated gains. When it is unclear whether and how much an individual will win or earn, the legality of committing them to pledge a percentage of that windfall is ambiguous; governments can help to clarify this legal environment. Still, previous evidence suggests that a legally-binding pledge may not be necessary for this strategy to be effective; it may be enough to make pledges automatically deducted by default.[45]

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[45] In particular, in Breman's(2011) field experiment on regular donors, very few deviated from their committed (increases in) contributions, even though doing so would only require a small effort.

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Appendix

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 $x + g \leq E_z$; E_z represents wealth or purchasing power in state of the world z, where E_z is strictly increasing in its subscript. Under standard assumptions, this implies unique indirect demand functions, optimal contributions $g^*(E_z)\forall z$, or, more simply g_0^* and g_1^* for $z = 0$ and $z = 1$ respectively. In the context of our experiments, we can consider E_1=w, the wealth after winning the prize (or bonus) and $E_0 = \ell$, the wealth after not winning.

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_0 and g_1 before learning the realization of z. Assuming non-satiation, we can substitute in the budget constraints and express her problem as

$$g_{0b}^*, g_{1b}^* := argmax_{g_0,g_1}(1-p)v(y_0 - g_0, g_0) + pv(y_1 - g_1, g_1),$$

where p is her probability of winning the prize, and $v(x, g)$ is her indirect utility with $x$available for personal consumption, and where she actually gives g. 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 will maximize this conditional problem, i.e., $g^*_{0b} = g^*_0$ and $g^*_{1b} = g^*_1$. (If, as in our field experiment and setting B, we constrain $g^*_{0b} = 0$, $g^*_{1b} = g^*_1$ will still hold.) 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 similar argument follows for the *Uncertain* case. Thus, a standard model will predict $g_z = g^a_z = g^b_z = g^u_z$ for $z = w$ or for any level of income.

Online appendix

See link (http://goo.gl/RwcoSS) or attached file.

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