ABSTRACT

Title of Dissertation: INCREASING CHARITABLE GIVING USING SUBSIDIES: THEORY AND EXPERIMENTS

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This dissertation combines theoretical analysis with economic experiments to advance our understanding of why people give. In particular, this dissertation focuses on the use of subsidies for giving—e.g., rebates and matches—as a tool for increasing charitable giving. The research included in this dissertation provides important guidance to charitable organizations seeking to design fundraisers optimally to maximize charitable receipts. Furthermore, this research also provides important guidance to policy-makers seeking to better understand the interplay between tax policy and charitable giving. The results of this dissertation can contribute to more effective fundraising campaigns and more efficient tax policy.

Chapter 1 challenges the well-established result among existing experimental studies that donations are significantly more responsive to matches than to rebates. In previous experimental studies the budget sets available to subjects under rebates are constrained relative to those available under matches, biasing estimates of the rebate-price elasticity. We con-

duct a novel experiment that removes the constraint under rebates, producing equal budget sets for price-equivalent rebates and matches. Contrary to previous studies, we find dramatically smaller differences in donations under price-equivalent matches and rebates. More importantly, we find no statistical difference between our estimated rebate- and match-price elasticities. Furthermore, we show that the constraint under rebates affects the entire distribution of observed behavior, not only the behavior of individuals for whom the constraint is binding. This chapter contributes to theories of charitable giving and has important implications for tax policy.

Chapter 2 studies how donor uncertainty affects their response to match subsidies in the context of charitable giving. It explores whether donors are responsive to exogenous changes in the probability of receiving a match. I develop a theoretical model of giving that incorporates uncertainty around matches. I demonstrate the model is capable of explaining the discrepancies in match-price elasticities of giving observed across previous field experiments and observational studies. I then derive testable hypotheses from the model, and design and run an economic experiment to test these hypotheses. The results of my experiment provide clear evidence that donors are responsive to changes in the probability of receiving a match. As a result, the same donor may respond differently to match subsidies depending on the setting. This work identifies an important aspect of donor decision making, contributing to a better understanding of why people give. It has important implications for theories of giving, the optimal design of fundraisers, and tax policy.

Chapter 3 builds on Chapter 2 to continue studying how donor uncertainty affects their response to match subsidies in the context of charitable giving. It explores whether donors are responsive to endogenous changes in the probability of receiving a match resulting from changes in fundraiser characteristics. The results provide strong evidence supporting the notion that changes in fundraiser characteristics can affect donors' beliefs about the probability of receiving a match, in turn affecting their donation decisions and the observed response to match subsidies. The effectiveness of a match subsidy varies depending on the characteristics of the fundraiser, so that the optimal fundraising strategy varies across fundraisers. This chapter provides new guidance for fundraisers interested in increasing charitable donations through the use of match subsidies.

INCREASING CHARITABLE DONATIONS USING SUBSIDIES: THEORY AND EXPERIMENTS

by

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Dedication

For Sydney.

Declaration

The first chapter of this dissertation is jointly authored with Neslihan Uler. The Dissertation Committee acknowledges that Zedekiah Higgs made substantial contributions to relevant aspects of this chapter.

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Chapter 1: Rebates and Matches

1.1 Introduction

To increase giving, charitable organizations often subsidize donations by designing fundraisers in which donors' contributions receive a match, typically provided by a wealthy lead donor. In this case, for every \$1 a donor passes to the charity, the charity receives $1 + s_m$ dollars, where s_m is the match rate. The price to the donor of providing the charity with a total of \$1 then becomes $1/(1 + s_m)$. Alternatively, donations can also be subsidized using rebates. At a rebate rate of s_r, the price of providing \$1 to the charity is $1 - s_r$. Both matches and rebates can be used to attract donations by lowering the price of giving. When $s_r = s_m/(1 + s_m)$, a rebate at rate s_r and a match at rate s_m are price equivalent, meaning both subsidies produce the same price of giving.

A large body of research, including both laboratory experiments (Eckel and Grossman, 2003, 2006a,b; Davis et al., 2005) and field experiments (Eckel and Grossman, 2008, 2017; Sasaki et al., 2022), has consistently found that donors do not respond to rebates and matches equivalently. Instead, total donations received by the charity are significantly higher when matches are offered versus when price-equivalent rebates are offered. Across studies, match-price elasticities are repeatedly estimated to be much larger than rebate-price elasticities in absolute value. This finding has had important implications for fundraising, as well as for tax policy (List, 2011; Andreoni and Payne, 2013; Vesterlund, 2016).

In this paper, we challenge the assertion that donations are significantly more price elastic under matches than under rebates. We show that the discrepancy observed between rebate-and match-price elasticities in previous experimental studies is largely driven by features of the experimental designs used. Previous experiments use what we refer to as the *third-party framework*. As we demonstrate in Section 1.2, rebates and matches are not presented on equal footing in the third-party framework. Relative to matches, subjects' budget sets (available *consumption/total donation* bundles) are constrained when presented with rebates, making the range of possible total donations (as well as the range of possible private consumption levels) smaller under rebates than under matches. To see this, consider a subject who is endowed with $10 and presented with two price-equivalent subsidies for giving: a 1:1 match and a 50% rebate. In both cases, the subject can pass a maximum of $10 to the charity. In the case of the match, if the subject passes all $10 to the charity, the charity receives a total donation of $20 and the subject leaves with nothing. However, in the case of the rebate, if the subject passes all $10 to the charity, the charity only receives $10 and the subject walks away with $5. The subject's budget set is significantly constrained under the rebate relative to the match, despite the two being price equivalent.

One might assume that the discrepancy between budget sets for rebates and matches in the third-party framework should be more or less benign. After all, the constraint under rebates should only affect the most generous donors, and, to the extent the estimated rebate-price elasticity is biased by the constraint, the bias can be reduced by accounting for the censored observations during estimation. However, this assumes that the effect of the constraint is only mechanical in nature, disregarding any potential *behavioral* effects. We argue this assumption does not hold. We show that the constraint under rebates in the third-party framework in fact has a significant behavioral effect. Not only does it mechanically restrict the decisions of the most generous donors, but it shifts the *entire* distribution of observed donations, significantly influencing the behavior of donors for whom the constraint is nonbinding. Within the third-party framework it is not possible to separate the behavioral effects of the constraint from any effects resulting from the type of subsidy used and, as a result, the comparison of estimated rebate- and match-price elasticities is significantly biased in studies using the third-party framework.

To prove the third-party framework produces biased elasticity estimates, and to produce an unbiased comparison of rebates and matches, we design a novel experiment which removes the disparities between budget sets for price-equivalent rebates and matches. We accomplish this task by using what we refer to as the *tax framework*, in which rebate and match subsidies are funded by tax revenues taken from subjects. In this framework, subjects' incomes are taxed at rate t, and any donations they choose to pass to the charity are either (i) tax exempt, in which case the subject receives an effective rebate at rate $s_r = t$, or (ii) not tax exempt, but are matched by the government (i.e., the experimenter) at a match rate of $s_m = t/(1-t)$.[1]

To see that this setup creates equal budget sets for price-equivalent rebates and matches, recall our earlier example of a subject who is endowed with \$10 and presented with a 1:1 match and a 50% rebate.[2] In the tax framework, the subject faces a 50% tax on their income. To remove the wealth effects of the tax and to create equivalence with our earlier example, the subject's pre-tax income is increased to \$20. In the case of the match, donations are not tax exempt. Because of this, the subject must pay \$10 in taxes regardless of how much they choose to pass to the charity and can donate up to \$10. With the match, if the subject donates all of their after-tax income, the charity will receive a total donation of \$20 and the subject will walk away with nothing (just like our previous example). In the case of the rebate, any donation provided by the subject is tax exempt. Thus, the subject can pass up to \$20 to the charity—this reduces their taxable income to 0, so they owe no taxes. The charity receives a total donation of \$20, and the subject leaves with nothing, exactly the same as the match. There are two important takeaways. First, unlike the third-party framework, the tax framework removes the constraint on subjects' choices under rebates, creating equality between the budget sets for price-equivalent rebates and matches. Second, other than removing the constraint under rebates, the budget sets under the tax and third-

[1]These rates guarantee price equivalency.

[2]See Section 1.2 for an in-depth comparison of the third-party and tax frameworks.

party frameworks are identical.

We find substantial evidence to support our claim that the constraint under rebates in the third-party framework significantly biases elasticity estimates. The bulk of this evidence comes from our two main experiments: our *third-party* experiment and our *tax* experiment. The *third-party* experiment replicates the third-party framework used in previous experimental studies, demonstrating that we are able to reproduce previous results. Importantly, even when adjusting for censoring, we find a large and statistically significant gap between the rebate- and match-price elasticities of giving (–.449 and –1.170, respectively, p-value=0.000), with donations being substantially more responsive to match subsidies. The *tax* experiment uses the tax framework and, hence, eliminates the constraint issue that is present in the *third-party* experiment. As expected, the gap in donations between rebates and matches is greatly reduced. The estimated rebate- and match-price elasticities of giving converge—to –.848 and –.899, respectively—and there is no longer any statistically significant difference (p-value=0.748).

Upon closer inspection of our results, it is clear the entire distribution of behavior shifts under rebates when moving between frameworks, suggestive of a behavioral response to the constraint in the third-party framework. To help eliminate other possible explanations for the shift in behavior, we also run two additional experiments: an *alt-tax* experiment and a *con-tax* experiment. The *alt-tax* experiment combines the third-party framework with the taxation language used in the *tax* experiment to test the extent to which subjects' behavior is affected by the use of tax *language*. The *con-tax* experiment combines the tax framework with the constraint on donations present under rebates in the *third-party* experiment to test whether behavior is affected by the *source of funding* used to provide the subsidies for giving.[3] Importantly, both experiments contain the rebate constraint present in the

[3] An example of this would be if subjects are more motivated to take advantage of rebates in the tax framework because they particularly like reducing their tax bill. If this is the case, subjects will give more under rebates in the tax framework relative to the third-party framework not because the constraint under rebates is relaxed, but because they do not view subsidies funded by taxes as being equivalent to subsidies funded by a third-party donor. We thank Steffen Huck for providing us with this example.

third-party framework. The results of these experiments are not significantly different from our *third-party* experiment, providing further evidence that the rebate constraint creates a behavioral response.

Furthermore, while our focus is on producing an unbiased *comparison* of rebate- and match-price elasticities and not on producing estimates that are independently externally valid, we do note that our estimates are consistent with those of previous field experiments and observational studies. Our estimated match-price elasticity is qualitatively similar to estimates reported in previous match studies. And, although our rebate-price elasticity estimate contradicts estimates reported in previous laboratory studies, it is consistent with estimates reported in many empirical studies using tax data.[4] This strengthens our argument that rebate-price elasticity estimates from experiments using the third-party framework are biased.

This study makes several important contributions. First, it provides new insights for discussions of tax policy. To the best of our knowledge, our paper is the first experimental study to compare rebates and matches within a tax framework. Note that the rebate subsidy in our tax framework functions similarly to the current tax system in the U.S. (at least for taxpayers who itemize their deductions). Thus, this framework not only resolves the constraint issue present in the third-party framework, but it also has the additional benefit of being more directly relevant for discussions regarding tax policy. Previous experimental studies have found large gaps between rebate- and match-price elasticities, leading many to suggest that restructuring the U.S. tax system—to provide matches rather than rebates—could potentially result in a significant increase in charitable giving. The results of our study, however, suggest that restructuring the tax system may in fact have little effect on donations.

Second, this study helps to clarify whether individuals truly view rebates and matches differently, helping to improve the literature's understanding of why people give. To iso-

[4]See Section 1.6 for a discussion of the rebate- and match-price elasticities reported in previous studies.

late the effect of changing the type of subsidy used, all other factors must be controlled. This is a feat for which laboratory experiments are uniquely positioned to accomplish. In contrast, observational studies struggle to make direct comparisons of rebates and matches. Observational studies are typically conducted using tax data, which usually only contains rebate subsidies—precluding any comparisons of rebates and matches—and often lacks sufficient price variation (see Section 1.6 for more details). And while field experiments allow researchers to compare both rebates and matches in a setting where they are able to introduce significant price variation, there remain various confounding factors which might cause donors to respond differently to rebates and matches in the field (e.g., time delays and uncertainty involved in receiving a rebate, beliefs about the probability of receiving a match, etc.). Our paper provides a better understanding of donors' underlying preferences by designing a laboratory experiment that (i) controls for all such confounding factors and (ii) removes the disparity between budget sets for price-equivalent rebates and matches.

Third, this study highlights a discrepancy between existing models of charitable giving and our empirical findings. We observe that, while individuals exhibit greater generosity under matches compared to rebates, donations respond uniformly to price changes across different subsidies. None of the prevailing models can reconcile both observations simultaneously. Traditional theories solely based on pure altruism fail to account for the differing impact of matches on donations, whereas warm glow theories, including Andreoni (1989)'s impure altruism model and Hungerman and Ottoni-Wilhelm (2021)'s impure impact model, can explain the superiority of matches over rebates but require distinct price elasticities. In our study, we propose a straightforward extension to these models that maintains the warm-glow motive while achieving parity in price elasticities across subsidies, albeit at the expense of equalizing donation levels under both subsidies. This paper reignites the discussion on accurately modeling charitable giving.

Finally, this study also contributes to the understanding of the behavioral effect first identified by List (2007) and Bardsley (2008), which find that expanding the budget sets available to subjects can influence the entire distribution of behavior. Unlike their settings, here we are able to manipulate subjects' budget sets without introducing any option to take. By moving from the third-party framework to our tax framework, we are able to expand subjects' budget sets (under rebates) while holding the income, price, and initial allocation constant. Even in this setting, we continue to find that expanding subjects' budget sets affects the entire distribution of behavior. This suggests that *any* manipulations of budget sets may have important effects on subjects' behavior, regardless of how such manipulations are implemented and irrespective of any potential differences between giving and not taking (Korenok et al., 2014; Grossman and Eckel, 2015; Dreber et al., 2013; Smith, 2015). This result is an important reminder of the need to carefully consider the context in which decisions are made in the laboratory before generalizing the results (Levitt and List, 2007).

The remainder of this paper is organized as follows. Section 1.2 develops the theoretical model, formally demonstrates the disparity in budget sets present in previous experimental studies, demonstrates how our novel taxation framework resolves the issue, and presents theoretical predictions. Section 1.3 outlines the experimental design and procedures for each of our experiments. Section 1.4 presents results. Section 1.5 discusses extant theories of giving and proposes a simple extension capable of explaining equal rebate- and match-price elasticities. Section 1.6 provides a brief overview of related literature, including attempts made to explain the disparity between rebates and matches, previous attempts to resolve the budget set issue present in the third-party framework, and a discussion of how our rebate- and match-price elasticities compare with previous estimates. Section 1.7 concludes.

1.2 Theory

In this section we theoretically analyze the decision setting faced by donors in the third-party framework used in previous experimental studies, and then we develop and analyze our novel tax framework. We show two main results. First, we formally show that in the third-party framework an individual's budget set under a rebate is a strict subset of their budget set under the price-equivalent match. Second, we show that our novel tax framework eliminates the discrepancy between budget sets for price-equivalent rebates and matches, allowing us to provide an unbiased comparison of the rebate- and match-price elasticities of giving.

1.2.1 Third-party framework

Consider an individual i with income $w_i > 0$. Let i's utility be represented by the impure impact model developed by Hungerman and Ottoni-Wilhelm (2021), so that i's utility is given by $U_i(x_i, g_i, R)$, where $x_i = w_i - g_i$ is i's consumption of the private good and $g_i \in [0, w_i]$ is their donation to the charity.[5] The last term $R \equiv R_i + \lambda R_{-i}$, where R_i is the donor's impact (that is, the *total amount received* by the charity as a result of their donation, g_i), R_{-i} is exogenous charity output contributed by others, and λ is a weight. The second argument of U_i captures the 'warm-glow' that i derives from the act of giving, independent of any effect their donation has on the output of the charity (Andreoni, 1989, 1990). In the following analysis we drop the i subscripts for brevity. In the absence of any subsidies for giving, i's optimization problem is given by

$$\max_{g \in [0,w]} U(w - g,\ g,\ g + \lambda R_{-i}). \tag{1.1}$$

If a third-party provides a match subsidy, $s_m \geq 0$, i's optimization problem becomes

$$\max_{g_n \in [0,w]} U(w - g_n,\ g_n,\ (1 + s_m)g_n + \lambda R_{-i}), \tag{1.2}$$

where g_n denotes i's *net* donation, which is the total cost to the individual of making their donation.

Now suppose that a third-party provides a rebate subsidy, $0 \leq s_r < 1$. In this case, i's

[5] While use of the impure impact model simplifies the presentation, the testable hypotheses we derive in Section 2.4 can also be derived from the impure altruism model.

optimization problem is given by

$$\max_{g_g \in [0,w]} U(w - (1 - s_r)g_g,\ g_g,\ g_g + \lambda R_{-i}), \tag{1.3}$$

where g_g denotes i's *gross* donation, which is the total amount received by the charity (i.e., the donor's *impact*).

It is important to recognize that in order to directly compare (1.2) and (1.3) we must first express them in terms of the same choice variable. Rewriting the rebate problem (1.3) in terms of the donor's *net* donation, $g_n = (1 - s_r)g_g$, gives

$$\max_{g_n \in [0,\ (1-s_r)w]} U\left(w - g_n,\ \frac{g_n}{1 - s_r},\ \frac{g_n}{1 - s_r} + \lambda R_{-i}\right). \tag{1.4}$$

For a given match rate, s_m, and rebate rate, s_r, to be *price equivalent*, it must be the case that $s_m = \frac{s_r}{1-s_r}$. Using this relation to substitute for s_m, the donor's optimization problem when there is a match subsidy (equation 1.2) can be written as

$$\max_{g_n \in [0,w]} U\left(w - g_n,\ g_n,\ \frac{g_n}{1 - s_r} + \lambda R_{-i}\right). \tag{1.5}$$

Price-equivalent third-party rebates and matches can now be directly compared by comparing (1.4) and (1.5), respectively.

It is clear from this comparison that donor behavior will not in general be the same for third-party rebates and matches, even when they are price-equivalent. There are two reasons for this discrepancy. First, the type of subsidy matters: donors do not receive warm glow in the same way for rebates and matches. While donors feel warm glow for their *gross* donation (g_g) when there is a rebate, when there is a match they only feel warm glow for their *net* donation (g_n). Thus, as demonstrated by Hungerman and Ottoni-Wilhelm (2021), price-equivalent rebates and matches are not equivalent to donors. Therefore, one can expect a gap between rebate- and match-price elasticities, assuming the model's assumption that donors receive different levels of warm glow for rebated funds and matched funds holds.[6]

The second reason for the discrepancy between third-party rebates and matches is that the available budget sets for the two subsidies are different. As shown in (1.4), when there is a third-party rebate the donor chooses $g_n \in [0, (1 - s_r)w]$. For the price-equivalent third-party match shown in (1.5), the donor instead chooses $g_n \in [0, w]$. This can be intuitively understood by considering a donor who always wants to donate as much as possible to the charity. When there is a match, the charity will receive $(1 + s_m)w > w$, and it will cost the donor their entire income w. But when there is a rebate, the donor can never provide the charity with a total donation greater than w, and the donor cannot end up with less than $s_r w$ (that is, their donation can never cost them more than $g_n = (1 - s_r)w$). When subsidies are provided by a third party, the budget sets faced by donors under rebates are strict subsets of the budget sets they face under price-equivalent matches. This is shown graphically in Figure 1.1.

Since the third-party framework creates a disparity in budget sets, one cannot attribute the previously reported large differences in rebate- and match-price elasticities entirely to the type of subsidy. To isolate the true effect of how donors respond to the type of subsidy, one needs to elicit those elasticities in an environment that keeps the budget sets identical.

1.2.2 Tax framework

As we will now demonstrate, the disparity in budget sets—between price-equivalent third-party rebates and matches—is not present when the subsidies are instead provided via a tax system. Within a tax framework, individual i is endowed with a gross income of y_i and faces an income tax of $0 \leq t < 1$. If there is a rebate subsidy provided for charitable donations, this is equivalent to donations being tax exempt. That is, $s_r = t$, and any donations that an individual passes to the charity will decrease their taxable income (decreasing their tax

[6]It is important to recognize that this is an assumption of the model. One could instead assume that donors feel warm glow in the same way for rebates and matches, in which case the model would predict identical behavior for price-equivalent rebates and matches (ignoring any differences in budget sets). We discuss this in greater detail in Section 1.5.

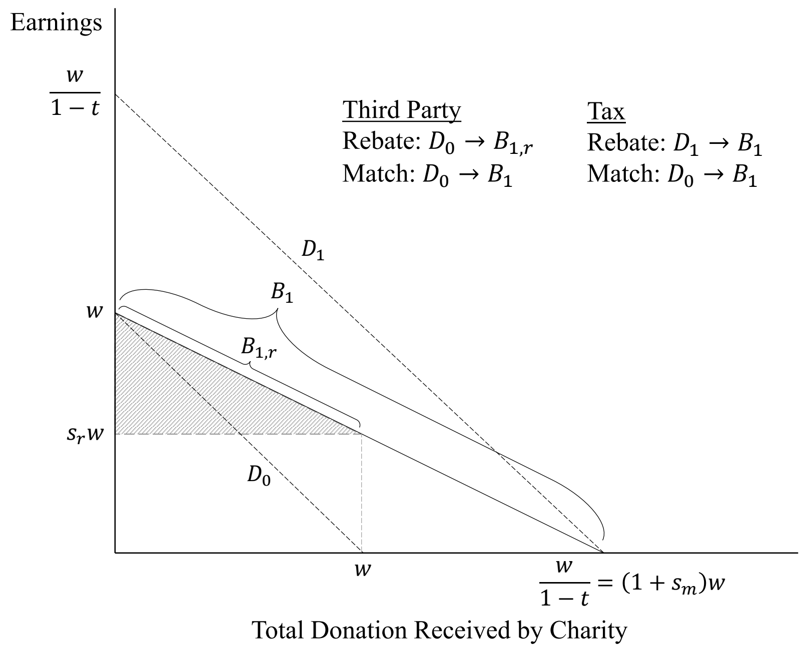


Figure 1.1: Budget sets for price-equivalent rebate and match subsidies in the third-party and tax frameworks.

liability). Letting $w_i = (1-t)y_i$ and dropping i subscripts, the individual's optimization problem for a rebate subsidy provided in a tax framework is given by

$$\max_{g_g \in \left[0, \frac{w}{1-t}\right]} U\left((1-t)\left[\frac{w}{1-t} - g_g\right], g_g, g_g + \lambda R_{-i}\right). \tag{1.6}$$

When there is a match subsidy provided in the tax framework, the individual's donations are no longer tax exempt. That is, the donor faces a tax bill of ty_i, regardless of any donations they choose to pass to the charity. Therefore, the maximum amount the donor can pass is w_i. However, any amount that they choose to pass to the charity will be matched at the match rate s_m using tax revenues. By setting $s_m = \frac{t}{1-t}$, we establish price-equivalency between the match and the rebate. The donor's optimization problem for a match subsidy provided via the tax system is given by

$$\max_{g_n \in [0,w]} U\left(w - g_n, g_n, \frac{g_n}{1-t} + \lambda R_{-i}\right). \tag{1.7}$$

Comparing (1.7) to (1.5), we can see that a match subsidy provided via the tax system is equivalent to a match subsidy provided by a third-party. That is, the theory predicts that, with respect to match subsidies, donor behavior should be unaffected by the framework used. However, this is not be the case for rebate subsidies. Writing (1.6) in terms of the net donation, g_n, and simplifying, the individual's optimization problem for a rebate subsidy in the tax framework becomes

$$\max_{g_n \in [0,w]} U\left(w - g_n, \frac{g_n}{1-t}, \frac{g_n}{1-t} + \lambda R_{-i}\right). \tag{1.8}$$

Comparing (1.8) to (1.4), we can see that a rebate provided within a tax framework is not theoretically equivalent to a rebate provided by a third party, because the donor's choice set (and budget set) is no longer constrained. As seen by comparing (1.7) and (1.8), the budget sets for price-equivalent rebates and matches are now equal in the tax framework, suggesting the gap between elasticities should decrease. That being said, if the difference in the amount of warm glow received under rebates and matches still exists, then the gap between elasticities need not completely disappear.

1.2.3 Comparison of frameworks

A graphical comparison of rebates and matches in the third-party and tax frameworks is presented in Figure 1.1. For both frameworks, line D_0 shows the *decision set* faced by an individual when a match subsidy is provided. After choosing a point on D_0, the match subsidy moves their final consumption point horizontally outward (by the amount $s_m g_n$) to the budget line B_1. We can see that the entire line B_1 is obtainable. However, when a rebate is provided by a third party, only part of B_1 is obtainable. In this case, the individual's *decision set* is still given by line D_0, but after the individual chooses how much to pass to the charity, the rebate subsidy moves their final consumption point vertically upward (by the amount $s_r g_g$) to the budget line $B_{1,r}$. The section of B_1 to the right of w is no longer obtainable. The tax framework resolves this issue by allowing individuals to choose a point on the *decision set* D_1, representing their pre-tax income. After choosing a point along D_1, the tax then moves their final consumption point vertically downward, making the entire budget line B_1 obtainable. Therefore, while the comparison of rebate- and match-price elasticities is confounded by differences in budget sets when using the third-party framework, this issue is not present when using our tax framework.

Other than the issue of budget sets not being identical under rebates and matches when provided by a third-party donor, our tax framework and the standard third-party framework are theoretically equivalent under both the impure altruism model and the impure impact model. In other words, according to the existing models of giving, for individuals whose donation decisions are not constrained by the upper bound in the third-party rebate scenario (i.e., for individuals who choose $g_g < w$), the tax and third-party frameworks generate identical outcomes. (To be clear, this is a theoretical equivalence between *frameworks* (i.e., third-party vs. tax), not between *subsidies* (i.e., rebates vs. matches).) However, the results of Bardsley (2008) and List (2007) suggest that expanding/restricting the budget set available to donors may affect the decisions of *all* donors, not only those for whom the third-party constraint is binding.[7] Therefore, the frameworks might not be behaviorally equivalent under rebates.

1.2.4 Main Hypotheses

The preceding analysis leads to two testable hypotheses.

In the third-party framework, censored observations under rebates could imply a less elastic response to a change in prices and bias the estimated rebate elasticity towards zero. Moreover, motivated by the results of List (2007) and Bardsley (2008), we argue that, by restricting the budget set under rebates, the third-party framework may even distort the behavior of donors who are not directly affected by the constraint. Based on these papers,

[7] Unlike the settings presented in Bardsley (2008) and List (2007), though, here we are expanding individuals' budget sets within the positive domain, rather than expanding into the negative domain.

we conjecture that the entire distribution of donations will shift downwards when the budget set gets smaller. This will imply an even larger bias in the estimation of the rebate-price elasticity.

Hypothesis 1 *Estimates of the rebate-price elasticity of demand for donations in the tax framework will be larger (in absolute value) than those in the third-party framework. Hence, the gap between elasticities will be smaller in the tax framework.*

Hypothesis 2 *Donation behavior of individuals will change as the budget set is expanded, even when their decisions are unconstrained.*

1.3 Experimental Design and Procedures

In total we run four separate experiments: two main experiments, which we refer to as the *third-party* experiment and the *tax* experiment, upon which our main results are based, and two follow-up experiments, which we refer to as the *alt-tax* experiment and the *con-tax* experiment, intended to provide further insights into the mechanisms at play.

In all four experiments, subjects are presented with a list of allocation decision problems where they are provided an endowment and must decide how much of it to donate to a charity. The charity is real, and a description of it is presented to subjects during the instructions.[8] Across decision problems, the amount endowed to subjects is varied, and different prices of giving are created through the use of rebate and match subsidies. The same combinations of endowments, prices of giving, and subsidy types are used in each of the experiments. However, subsidies in the *third-party* experiment are provided using a third-party framework, while subsidies in the *tax* experiment are provided using a tax framework. The *alt-tax* and *con-tax* experiments use a combination of the third-party and tax frameworks.

The instructions used in each experiment are nearly identical, with the exception of a

[8] The charity used is charity: water. The description provided to subjects is the same in all four experiments. The full instructions for each experiment are provided in Appendix A.4.

few words. A copy of the instructions provided to subjects (including prepared statements read aloud by the experimenter) is provided in Appendix A.4.

In what follows, we first outline the designs of our experiments. We then summarize the experimental procedures used.

1.3.1 Third-party experiment

The *third-party* experiment follows the experimental design used in both the original Eckel and Grossman (2003) study and the Davis et al. (2005) replication.[9] After going through the instructions and introducing the charity, subjects are presented with a total of 10 decision problems. We follow a within-subject design with three sources of variation in the problems: (1) the endowment amount ($w \in \{80, 120\}$), (2) the price of giving ($p \in \{1, 0.67, 0.5\}$), and (3) the type of subsidy used (either a match, a rebate, or no subsidy). Table 1.1 lists the parameters used for each decision problem. All prices are presented for each endowment amount, and both subsidy types (i.e., rebates and matches) are presented for each price (except $p = 1$, where no subsidy is used). To produce a price-of-giving of $p = 0.5$, a $1 : 1$ match (i.e., $s_m = 1$) and a 50% rebate (i.e., $s_r = 0.5$) are provided. To produce a price-of-giving of $p = 0.67$, a $0.5 : 1$ match (i.e., $s_m = 0.5$) and a 33% rebate (i.e., $s_r = 0.33$) are provided.

The problems are ordered first by endowment (low to high) and then by price-of-giving (high to low). This ordering groups the problems together by budget set (i.e, price-equivalent

[9] In an attempt to shrink the gap between rebates and matches, Davis et al. (2005) run an added information treatment in which subjects are provided with tables that show them what their total earnings and the total donation received by the charity would be for different example pass amounts. Note that this is in contrast to the original design of Eckel and Grossman (2003) which does not provide such information. Davis et al. (2005) show that this added information—which helps to eliminate calculation errors made by subjects—shrinks the observed gap between rebates and matches. However, the remaining gap is still statistically significant. As we explain below, our *third-party* experiment provides subjects with the exact amount of their total earnings and the total donation received by the charity, calculated based on the subject's specific decision of how much to pass. Therefore, our experiment provides more detailed information in a continuous form of the information tables provided in Davis et al. (2005). That said, in our setting subjects only see calculations based on values they choose to enter, whereas the information tables provided in Davis et al. effectively serve as examples, showing subjects calculations based on decisions they may have otherwise never considered.

Third-party Experiment Budget Sets				
Problem	Endowment	Price	Rebate Rate	Match Rate
1	80	1		
2	80	0.67	0.33	
3	80	0.67		0.5
4	80	0.5	0.5	
5	80	0.5		1
6	120	1		
7	120	0.67	0.33	
8	120	0.67		0.5
9	120	0.5	0.5	
10	120	0.5		1

Table 1.1: List of budget sets used in the third-party experiment.

rebate and match questions are always presented next to each other) to reduce any confusion subjects may have regarding the effects of the subsidies. Depending on the treatment the subject is randomly assigned to, either the rebate is always shown before the equivalent match, or vice versa.

Figure A.16 in Appendix A.4 provides an example decision sheet faced by subjects in the *third-party* experiment. Each problem informs subjects of the amount of their endowment and, if applicable, the type and rate of subsidy provided for charitable donations. For each problem, subjects must choose an amount, g_i, to pass to the charity. This is done by entering the desired amount into a text-entry box. Subjects are presented with all 10 decision problems simultaneously, and they are free to enter their choices in any order. Upon entering the desired pass amount for a given problem, the entered value is automatically rounded to the nearest integer and the remaining columns of the problem automatically fill with the correct values based on the parameters of the problem and the subject's pass decision.[10] Subjects may edit their decisions at any time before submitting them. The amount passed in each problem cannot be negative and cannot be more than the allotted endowment for the

[10]The first column of each problem reports the given parameters (i.e., the endowment and subsidy type and rate). The second column provides a text-entry box for the subject to enter their desired *pass* amount, g_i. The remaining columns report, respectively, the amount the subject *holds* for themselves, $w - g_i$; the subject's *total earnings* for the problem (assuming the problem is selected for payment), $w - (1 - s_r)g_i$; and the *total donation received* by the charity (assuming the problem is selected for payment), $(1 + s_m)g_i$.

problem, which is enforced by the programming.[11] Once acceptable pass amounts have been entered for each problem, subjects submit their decisions for all 10 problems simultaneously. At the conclusion of the experiment, one problem is randomly selected to determine the subject's payment and donation to the charity.

1.3.2 Tax experiment

The *tax* experiment is similar to the *third-party* experiment, and it follows the theoretical model closely. The defining difference from the *third-party* experiment is that subjects are now taxed on their gross (initial) endowment, and subsidies for giving are funded by tax revenue rather than a third-party. The initial endowments and subsidy rates are set such that the budget sets faced by subjects are identical to those used in the *third-party* experiment (with the exception that there is now no constraint imposed on subjects' decisions when there is a rebate subsidy).

Table 1.2 lists the parameters used for each decision problem in the *tax* experiment. Because the tax affects subjects' wealth levels, the initial (pre-tax) endowment provided to them must be adjusted to account for the tax. That is, to provide a subject who faces a tax at rate t with a *net* endowment of w, the subject must initially be provided with a *gross* endowment of $y = \frac{w}{1-t}$. The gross endowments for each problem are set to provide the same net endowments as those used in the *third-party* experiment. In order to identify any effects of the tax rate that are independent of the subsidy rate, the baseline budget sets (i.e., those without any subsidies for giving) are implemented using both tax rates.[12] Thus, there are two additional baseline problems relative to the *third-party* experiment, resulting in a total of 12 decision problems rather than 10.

[11]If a subject attempts to pass a negative amount or an amount that exceeds their endowment for a given problem, the remaining columns will still calculate and report the corresponding values based on their decision. However, if the subject attempts to submit their decisions, they will receive an error message informing them that their stated decision is not acceptable. The subject cannot submit their decisions until their responses for all questions are acceptable.

[12]In Table 1.2, Problems 1 and 4 both provide a net endowment of 80 and a price of giving of 1, but Problem 1 implements a tax rate of 0.33 while Problem 4 uses a tax rate of 0.5. Likewise, Problems 7 and

Tax Experiment Budget Sets						
Problem	Gross Endowment	Tax Rate	Net Endowment	Price	Rebate Rate	Match Rate
1	120	0.33	80	1		
2	120	0.33	80	0.67	0.33	
3	120	0.33	80	0.67		0.5
4	160	0.5	80	1		
5	160	0.5	80	0.5	0.5	
6	160	0.5	80	0.5		1
7	180	0.33	120	1		
8	180	0.33	120	0.67	0.33	
9	180	0.33	120	0.67		0.5
10	240	0.5	120	1		
11	240	0.5	120	0.5	0.5	
12	240	0.5	120	0.5		1

Table 1.2: List of budget sets used in the tax experiment.

Figure A.17 in Appendix A.4 gives an example decision sheet faced by subjects in the *tax* experiment. For each decision problem, subjects are informed of the parameter values defining their budget, and they are asked to enter the amount that they would like to pass to the charity. When no subsidy is provided or a matching subsidy is provided, any amount passed by subjects is *not* tax exempt. Thus, subjects owe a tax bill of $ty = \frac{tw}{1-t}$ regardless of any amount they choose to pass, effectively leaving them with only w to be allocated between themselves and the charity. However, when a rebate subsidy is provided, any amount subjects pass to the charity *is* tax exempt and lowers their tax bill by an amount equal to tg. Thus, subjects are now able to pass their entire initial endowment of y to the charity, since they will only need to pay taxes on any amount they choose to hold. The *tax* experiment is otherwise identical to the *third-party* experiment.

1.3.3 Alt-tax experiment

The *alt-tax* experiment builds on the third-party framework, with the only difference being that it uses taxation language like that used in our *tax* experiment. Subjects' endowments

10 both provide a net endowment of 120 and a price of giving of 1, differing only in the tax rate used.

are taxed just as they are in the *tax* experiment, but subsidies for giving are provided exactly as they are in the *third-party* experiment. Importantly, rebate subsidies are not provided by making donations tax-exempt, but instead are provided by a third party. Thus, while the problems are presented using tax language equivalent to that used in the tax framework, the budget sets faced by subjects in the *alt-tax* experiment are exactly the same as those in the *third-party* experiment, including the disparity between price-equivalent rebates and matches.

The parameters used in the *alt-tax* experiment are the same as those used in the *tax* experiment, listed in Table 1.2. Figure A.18 in Appendix A.4 provides an example decision sheet presented to subjects in the *alt-tax* experiment.

This design allows us to separate the effect of using the language of taxation from the effect of changing the budget sets associated with rebates.

1.3.4 Con-tax experiment

The *con-tax* experiment is identical to our *tax* experiment, with one exception: there is now an artificial constraint placed on the amount subjects are able to pass under rebates. The parameters used in the *con-tax* experiment are the same as those used in the *tax* experiment, listed in Table 1.2. Figure A.19 in Appendix A.4 provides an example decision sheet presented to subjects in the *con-tax* experiment.

If the shift in donation behavior under rebates in our *tax* experiment is driven by reasons not related to the constraint issue, then this experiment should generate donation behavior similar to the *tax* experiment—at least for unconstrained subjects—since the only difference between these two experiments is the presence of a constraint under rebates. However, if being constrained under rebates has a behavioral effect on donation decisions, then donations under rebates in the *con-tax* experiment will be very similar to the donations under rebates in the *third-party* experiment.

1.3.5 Experimental Procedures

The two main experiments—*third-party* and *tax*—were run synchronously with a total of 147 and 151 subjects, respectively, between June of 2021 and February of 2022. Each experimental session was randomly assigned to one of the two main experiments. The *alt-tax* experiment was conducted with a total of 144 subjects after the main experiments were completed, between March and June of 2023 using the same procedures and subject pool.[13] Moreover, all sessions were run by only one of the authors of the project, so that all subjects across all sessions interacted with the same experimenter, guaranteeing there are no differences in the experimental procedures used across any of the sessions. Each subject participated in only one experiment.

Due to the COVID-19 outbreak, all sessions were run using Zoom and Qualtrics.[14] A total of 588 subjects were recruited from the University of Maryland on a first-come-first-serve basis using the ORSEE software (Greiner, 2015). Subjects were recruited from a large pool of potential participants representing different majors and different grade levels. After registering for an experimental session, subjects were sent a link to a Zoom meeting room where, upon entering, they were placed in a waiting room while the experimenter checked them in one at a time. For each subject, the experimenter would transfer them to the main Zoom room, verify their student ID, provide them with a unique link to the Qualtrics survey,[15] change their Zoom name to an anonymous five-digit code, and then return them

[13] Subject characteristics among different experiments are similar (see Tables A.1 and A.2).

[14] Even though COVID-19 restrictions were lifted in the middle of our experiments, in order to keep the procedures identical across different sessions and experiments, we ran all of our sessions online.

[15] Providing a unique survey link to each subject was important for several reasons. First, it prevented distribution of the survey to users other than the intended subject. Because subjects were sent their survey link and asked to open it while in the Zoom chat, the IP address of the device they were using was logged by Qualtrics. If a subject were to send the link and open it on a different device, this would be registered by Qualtrics. However, this never occurred. Second, the use of unique survey links prevented subjects from being able to restart the survey. Because the data and progress for each link is tracked and stored by Qualtrics, any attempt to close and reopen the link—with a different browser, an incognito window, or after clearing any cookies, etc.—would simply result in the survey returning to the same point at which it was closed. This also prevented any issues arising from a subject accidentally closing the survey before completing it; the subject could simply reopen the link and return to where they left off. Finally, the use of unique survey links also allowed the experimenter to track the progress of each subject. This was important for verifying that all subjects were present while the instructions were read, as well as verifying that no

to the waiting room before repeating the process with the next subject. After all subjects were checked-in, the experimenter would turn off all cameras and mute all microphones before transferring all of the subjects to the main Zoom room to begin the experiment. The experimenter was able to track the survey progress of each participant in real-time using the Qualtrics software. At all times, subjects were able to use the Zoom chat to communicate with the experimenter, but communication between subjects was disabled.

To continue to the instructions page, subjects were required to enter a password that was provided by the experimenter. This prevented subjects from starting the survey early, and it allowed the experimenter to verify that all subjects were present without the need to turn their cameras on for visual confirmation. After verifying that all subjects were on the instructions page, the experimenter then read the instructions aloud. At the end of the instructions, subjects were presented with an opportunity to ask the experimenter any questions through the Zoom chat. After answering any questions, another password was provided by the experimenter that allowed subjects to continue to the experiment, which they were then able to complete at their own pace.

During the experiment, all decisions were made in Tokens, where 10 Tokens = 1 US dollar. At the end of the experiment, one problem was randomly selected for each subject, to determine their payment and the donation to be made to the charity on their behalf. Subjects then answered standard demographic questions and completed a payment form to document their earnings. The experiments took approximately one hour to complete.[16]

1.4 Results

We first report the results of the baseline *third-party* experiment and demonstrate that they are qualitatively and quantitatively in line with previous studies. We then report the results

subjects started the survey before being instructed to do so.

[16]Many subjects were able to complete the experiment in less than an hour. However, to prevent other subjects from being distracted or feeling rushed, all subjects were asked to remain in the Zoom meeting until being dismissed by the experimenter. Once the experimenter was able to verify that all surveys had been successfully submitted, they would dismiss all subjects simultaneously.

of the *tax* experiment and demonstrate that behavior in the tax framework is significantly different from that in the third-party baseline. Finally, we report the results of our two follow-up experiments: the *alt-tax* experiment and the *con-tax* experiment.

1.4.1 Third-party experiment

Figure 1.2 plots the average decisions made in our *third-party* experiment. The gap between price-equivalent rebates and matches is represented by the gap between triangles (rebates) and squares (matches) along the same budget line. If price-equivalent rebates and matches produced the same donations, the triangles and squares would overlap. It is clear from Figure 1.2 that total donations are much more responsive to matches than to rebates. Controlling for endowment, as the budget lines get flatter (i.e., as the price of giving decreases), total donations increase only slightly under rebates, whereas total donations increase dramatically under matches. This is in line with previous work (Eckel and Grossman, 2003, 2006a,b; Davis et al., 2005). For example, compare Figure 1.2 with Figure A.15 in which we plot the average decisions made in the Davis et al. (2005) study.[17] We can see that our results are qualitatively in line with their findings.

Table A.3 reports the average donation amounts by subsidy type for each budget in our *third-party* experiment. Donations are substantially higher under matches than under the price-equivalent rebates in all comparisons, and the differences are highly statistically significant for all of the comparisons (with all p-values equal to 0.000).

We next estimate rebate- and match-price elasticities. We estimate the demand for charitable giving using a level-log specification with subject-level random effects:

$$Y_{ij} = \beta_0 + \beta_1 \cdot E_{ij} + \beta_2 \cdot R_{ij} \times E_{ij} + \beta_3 \cdot M_{ij} \times E_{ij} + \beta_4 \cdot R_{ij} \times P_{ij} + \beta_5 \cdot M_{ij} \times P_{ij} + \beta_6 \cdot R_{ij} + \beta_7 \cdot M_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij}, \quad (1.9)$$

[17]The data used to create Figure A.15 can be found in Table 6 of Davis et al. (2005) and comes from their added information treatment.

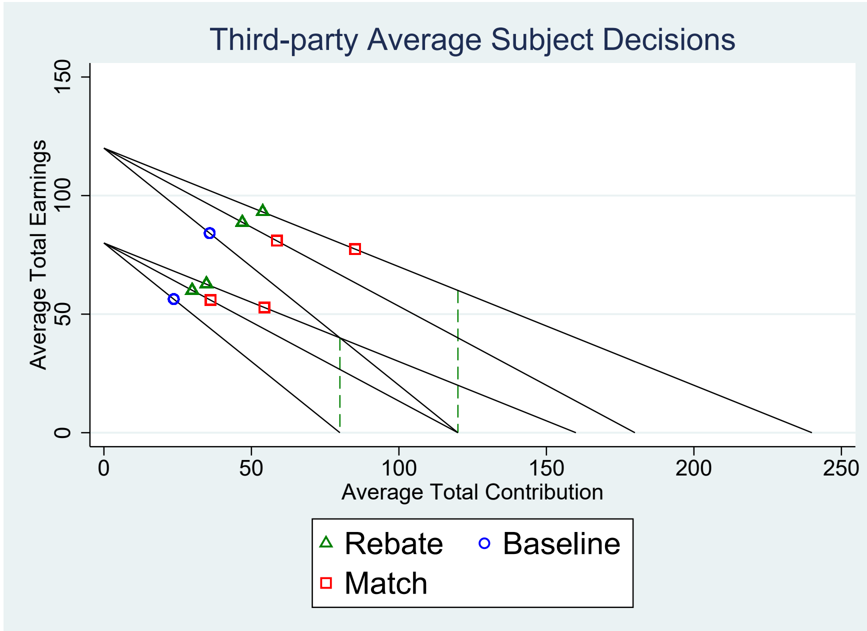


Figure 1.2: Average decisions in our third-party experiment.

where $i = 1, \ldots, 147$ indexes subjects, $j = 1, \ldots, 10$ indexes the allocation decision problems, Y_{ij} = Total Donation$_{ij}$, E_{ij} = ln(Endowment)$_{ij}$, P_{ij} = ln(Price)$_{ij}$, R_{ij} is an indicator for rebate subsidies, M_{ij} is an indicator for match subsidies, and X_i is a row vector of subject-level covariates. Total Donation is total *gross* donation (in Tokens) received by the charity. Note that we do not take the log of Total Donation because the log of zero is undefined. To account for censoring in Total Donation—from below (at 0) and from above—we employ a Tobit model, avoiding the use of any log-like transformations (Chen and Roth, 2024). Endowment is defined as total Tokens provided to the subject (80 Tokens or 120 Tokens). Price is defined as price of giving $1 to the charity ($0.50, $0.67, or $1.00).

Equation (1.9) is estimated using a random-effects tobit maximum likelihood model grouped by subject i, accounting for the censoring of subjects' Total Donations from both below and above. The random effects, ν_i, are assumed i.i.d. $N(0, \sigma_\nu^2)$, and the ε_{ij} are assumed i.i.d. $N(0, \sigma_\varepsilon^2)$ independently of ν_i.

Column 1 of Table 1.3 presents the results of our *third-party* experiment. Our rebate- and match-price elasticity estimates (–0.449 and –1.170, respectively) are in line with previous experimental results. Importantly, consistent with previous studies using the third-party framework, we find that our estimated rebate- and match-price elasticities are significantly different at the .05 level of confidence (p-value=0.000), with donations significantly more responsive to matches than rebates. As an example of the extent to which our *third-party* results align with previous studies, in Table A.4 we present our results alongside the results of the original Eckel and Grossman (2003) experiment.[18] To provide a direct comparison, Table A.4 reports the results of a log-log specification that uses the same scale used by Eckel and Grossman.[19] When using the same scale and specification, our rebate- and match-price elasticity estimates are somewhat higher (in absolute terms) than those estimated by Eckel and Grossman (–0.658 and –1.120 vs. –0.340 and –1.067, respectively), but donations are still observed to be more responsive to matches than rebates. However, unlike Eckel and Grossman (2003), we find that our estimated rebate- and match-price elasticities are no longer significantly different at the .05 level of confidence (p-value=0.105).[20]

1.4.2 Tax experiment

Having demonstrated that the results of our *third-party* experiment are consistent with previous studies, we now turn to analyzing the results of our *tax* experiment.

Figure 1.3 graphs the average consumption bundles of subjects in the *tax* experiment. Note that the vertical dashed lines are no longer present, because the budget sets under

[18]The Eckel and Grossman (2003) results are reported in Table 5 of their paper.

[19]Eckel and Grossman estimate a log-log specification. Because the logarithm of 0 is undefined, they use a log-like transformation of Total Donation, so that Y_{ij} = ln(Total Donation (in \$'s) + .01). Because the elasticity estimates produced by a log-like transformation are dependent on the scale used (Chen and Roth, 2024), in Table A.4 we rescale our Total Donation variable from Tokens to \$'s and use the same constant term used by Eckel and Grossman for the log-like transformation (i.e., we add .01). By using the same scale as Eckel and Grossman, our elasticity estimates can be directly compared, despite the fact that the estimates are sensitive to the choice of scale. Note, however, that our general results (presented in Table 1.3) are not scale-dependent, since we do not use a log-like transformation for Total Donation.

[20]It is important to note, though, that this result depends on the scale chosen. When measuring Total Donation in Tokens and using the log-like transformation Y_{ij} = ln(Total Donation (in Tokens) + 1), we continue to find a significant difference between the estimated rebate- and match-price elasticities of giving (p-value=0.022). This scale-dependency is why we opt to avoid using a log-like transformation in our main specification. Importantly, our (scale-independent) main results find a significant difference between rebate- and match-price elasticities in the *third-party* experiment.

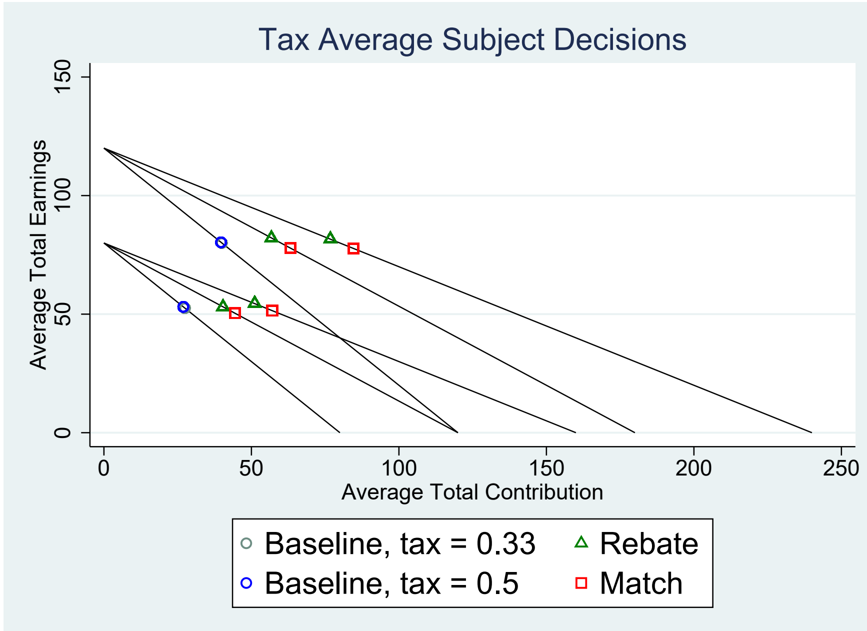


Figure 1.3: Average decisions in the tax experiment.

rebates are no longer constrained. It is immediately apparent that the results of the *tax* experiment are qualitatively different from the *third-party* experiment. The gap between rebates and matches is nearly gone, and both subsidies appear to move outward from the baselines (circles) along the same paths (unlike the *third-party* experiment, in which rebates appear to move diagonally upward while matches appear to move horizontally outward). When rebates and matches are presented within the tax framework, they appear to produce similar responses to price changes, contrary to the behavior observed in the third-party framework.

Table A.5 reports the average donation amounts by subsidy type for each budget in the *tax* experiment. Relative to the *third-party* experiment, the gap between average donation amounts for price-equivalent rebates and matches is now substantially smaller. That being said, rebates and matches are still found to be significantly different at the 1% significance level, reflecting the fact that subjects still tend to donate more under matches than rebates. However, the gap between rebates and matches now appears to be more or less fixed across prices, holding endowment constant.

Mirroring the analysis of the *third-party* experiment, demand for charitable giving in the *tax* experiment is estimated using a level-log specification with subject-level random effects:

$$Y_{ij} = \beta_0 + \beta_1 \cdot E_{ij} + \beta_2 \cdot R_{ij} \times E_{ij} + \beta_3 \cdot M_{ij} \times E_{ij} + \beta_4 \cdot R_{ij} \times P_{ij} + \beta_5 \cdot M_{ij} \times P_{ij} + \beta_6 \cdot R_{ij} + \beta_7 \cdot M_{ij} + \beta_8 \cdot T_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij}, \quad (1.10)$$

where $i = 1, \ldots, 151$ indexes subjects, $j = 1, \ldots, 12$ indexes the allocation decision problems, Y_{ij} = Total Donation$_{ij}$, E_{ij} = ln(Net Endowment)$_{ij}$, P_{ij} = ln(Price)$_{ij}$, R_{ij} is an indicator for rebate subsidies, M_{ij} is an indicator for match subsidies, T_{ij} = Tax Rate$_{ij}$, and X_i is a row vector of subject-level covariates. Total Donation and Price are defined as before. Net Endowment equals total endowment net of any taxes (80 Tokens or 120 Tokens). Tax Rate equals the tax rate applied to endowments (0.33 or 0.5).

Note that the variable Endowment has been renamed Net Endowment to clarify that, in the *tax* experiment, the relevant endowment for a subject accounts for any taxes they must pay. That is, Net Endowment represents $w \in \{80, 120\}$, *not* $y = \frac{w}{1-t} \in \{120, 160, 180, 240\}$. This allows us to control for the budget set that subjects are constrained to, and it allows for direct comparisons of the estimates from the *third-party* and *tax* experiments.

Column 2 of Table 1.3 presents the estimation results for the *tax* experiment. Rebate- and match-price elasticities are now estimated to be nearly identical (–0.848 and –0.899, respectively). While there is a significant difference between the rebate- and match-price elasticities in the *third-party* experiment (p-value=0.000), there is no longer any significant difference between elasticities in the *tax* experiment (p-value=0.748). That is, when the subsidies for giving are presented in the framework of taxation, subjects respond to rebate- and match-price changes equally.

Our design also allows us to test, separate from any subsidy effects, whether starting with a different tax rate matters for donations. Both the impure altruism and impure impact models predict that, all else equal, the tax rate should have no effect on giving behavior.

Both Figure 1.3 and our regression analysis show support for this prediction. Looking at Figure 1.3, we see that subjects' decisions appear to be unaffected by the tax rate. The blue circles depict average baseline choices when the tax rate is 0.5, and the teal circles depict average baseline choices when the tax rate is 0.33. It is very difficult to see both circles since they are nearly perfectly overlapping. That is, for a given budget line, absent of any subsidy for giving, on average decisions are not affected by the tax rate. The same conclusion holds when we look at the estimated coefficient on Tax Rate in column 2 of Table 1.3. Given that Net Endowment accounts for the impact of the tax rate on subjects' budgets, the estimated coefficient on Tax Rate captures the effect of facing a higher tax rate, holding all else equal (including subjects' budgets). Based on the elasticity estimate reported in Table 1.3 (–0.070), it appears that subjects may have a small negative behavioral response to being taxed at a higher rate. However, this effect is statistically insignificant (p-value=0.719).

We conclude this section by noting that the absence of a gap between rebate- and match-price elasticities in the *tax* experiment is unexpected and is inconsistent with extant models of charitable giving, as we demonstrated in Section 1.2 (assuming individuals derive warm-glow from donations). We provide a more detailed discussion of this issue in Section 1.5.

Regression Results: random effects tobit maximum likelihood				
Dependent variable=total donation received by charity				
	(1) Baseline Coefficient (*s.e.*) [elasticity]	(2) Tax Coefficient (*s.e.*) [elasticity]	(3) Alt-Tax Coefficient (*s.e.*) [elasticity]	(4) Con-Tax Coefficient (*s.e.*) [elasticity]
Constant (no subsidy)	-128.995 (124.269)	-34.050 (103.318)	-220.705** (73.693)	-263.432** (77.159)
Constant (rebate subsidy)	-201.996 (121.793)	-138.979 (103.259)	-215.318** (73.645)	-284.051** (77.114)
Constant (match subsidy)	-316.066** (121.702)	-151.966 (103.194)	-373.445** (73.482)	-386.281** (76.991)
(Net) Endowment (no subsidy)	31.480** (7.699) [.434]	30.150** (5.586) [.435]	36.674** (4.712) [.657]	29.189** (5.300) [.490]
(Net) Endowment (rebate subsidy)	47.185** (5.434) [.650]	51.010** (5.510) [.735]	34.648** (4.713) [.620]	34.631** (5.325) [.582]
(Net) Endowment (match subsidy)	69.043** (5.336) [.952]	54.977** (5.474) [.792]	67.892** (4.615) [1.216]	55.423** (5.206) [.931]
Rebate price	-32.571** (7.667) [-.449]	-58.853** (11.056) [-.848]	-27.060** (9.391) [-.485]	-22.781* (10.577) [-.383]
Match price	-84.874** (7.505) [-1.170]	-62.359** (11.018) [-.899]	-73.883** (9.292) [-1.323]	-57.537** (10.455) [-.966]
Tax rate		-4.886 (13.584) [-.070]	-2.621 (11.459) [-.047]	3.616 (12.873) [.061]
Controls	Y	Y	Y	Y
Subjects	147	151	144	146
Observations	1470	1812	1728	1752

Notes: The models for each experiment are run separately. * $p < 0.05$, ** $p < 0.01$

Table 1.3: Regression results for all experiments

1.4.3 The constraint under rebates has a behavioral effect

The results of our *tax* experiment suggest that, contrary to the conclusions drawn from previous studies, rebates and matches may in fact produce equal price elasticities of giving. The ability of our *third-party* experiment to replicate the large gap between rebate- and match-price elasticities observed in previous studies suggests that our results are not unique to our experimental setting or subject pool. However, while it is clear that behavior is significantly different in the *third-party* and *tax* experiments, it is less clear *why* this is the case. We argue that the main driver of these results is the *behavioral response* of subjects to the constraint placed on their budget sets under rebates in the *third-party* experiment: because the *tax* experiment removes this constraint and presents rebates and matches on equal footing, the disparity between donations under rebates and matches greatly diminishes. To support this claim, we make several observations.

There are two potential ways that the constraint in the *third-party* experiment could influence the observed donation decisions. The first, more direct way is by mechanically limiting donations under rebates to the level of the initial endowment, w_{ij}. The second—and, as we will argue, significantly more impactful—way is through a *behavioral* effect that influences the donation decisions of *all* participants, not just those constrained by the endowment limit. We contend that the mechanical effect alone cannot account for our findings; rather, the constraint also induces a substantial behavioral effect that biases price elasticity estimates.

If the constraint under rebates in the third-party framework only serves to mechanically reduce the donations of the most generous donors, then behavior in both the *third-party* and *tax* experiments should be identical for all subjects who donate less than their endowment under rebates. Moreover, if we were to retroactively impose the same constraints on choices under rebates in the *tax* experiment—censoring any observations that exceed the third-party constraint—we should observe identical behavior across both experiments. As a direct result of this fact, it follows that the percentage of subjects observed to be constrained should be equal across experiments. However, as Table A.7 shows, this is not the case. The percentage of subjects in the *tax* experiment whose decisions under rebates would be constrained if they faced the same constraint as subjects in the *third-party* experiment (32%) is significantly greater (p-value=0.011) than the percentage of subjects observed to be constrained in the *third-party* experiment (19%). That is, subjects are observed to be significantly more generous under rebates in the *tax* experiment relative to the *third-party* experiment, indicating that behavior is not the same across experiments.

To further demonstrate the extent to which behavior changes between experiments, we censor any total donation amounts exceeding the subject's net endowment under rebates in the *tax* experiment and re-estimate a combined model for our *third-party* and *tax* experiments. The findings, detailed in Appendix A.3.1, decisively indicate that censoring alone cannot account for our main results. In fact, this analysis suggests an even greater disparity in behavior between the two frameworks.

Expanding subjects' budget sets has previously been shown to significantly affect behavior, even for unconstrained subjects for whom expansions of the budget set should theoretically have no impact (List, 2007; Bardsley, 2008). That is, expanding the budget set available to subjects affects the *entire* distribution of observed behavior. Thus, given that the *third-party* experiment provides budget sets under rebates that are strict subsets of the budget sets under the price-equivalent matches, it is to be expected that the distribution of donations under a rebate will be significantly lower than the distribution of donations under an equivalent match.[21] The greater the subsidy provided (and the lower the price of giving) the more significant the shift in the distribution will be, since the disparity between the budgets sets for rebates and matches increases with the size of subsidy. Figures 1.4 and 1.5 show how dramatically the distributions for rebates (blue lines) and matches (red lines)

[21] One possible explanation for such a behavioral effect is reference dependence (Tversky and Kahneman, 1991). For example, an agent might consider the mid-point of all possible actions as a reference point. However, we prefer to be agnostic regarding what explains this behavioral effect. Instead, our main aim here is to document that it exists in this setting.

diverge in the *third-party* experiment (left panel) relative to the *tax* experiment (right panel) when the price of giving is .5.

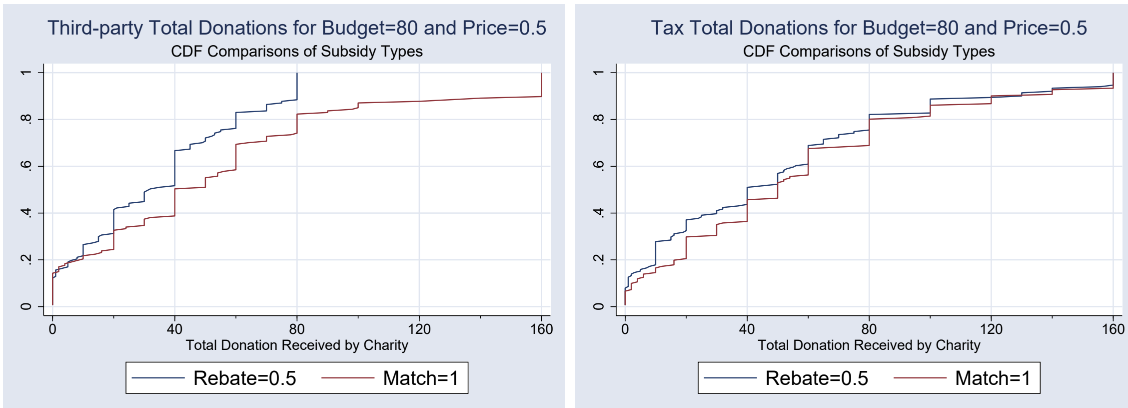


Figure 1.4: CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. Third-party on left, Tax on right.

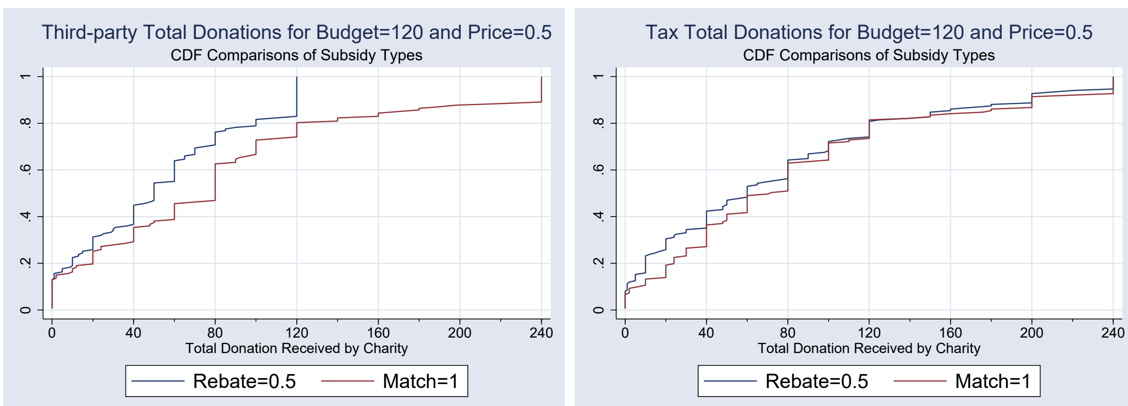


Figure 1.5: CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. Third-party on left, Tax on right.

While the distributions for rebates and matches diverge well before the constraint occurs in the *third-party* experiment, they overlap in the *tax* experiment when the constraint is removed. The constraint in the third-party framework appears to shift the entire distribution of behavior under the rebate. Further supporting this claim, Figures 1.6 and 1.7 show that the match distributions are the same in the *third-party* and *tax* experiments. That is, it is the rebate distribution that shifts, not the match distribution. This is consistent with our claim that the constraint under rebates in the third-party framework induces a behavioral response.

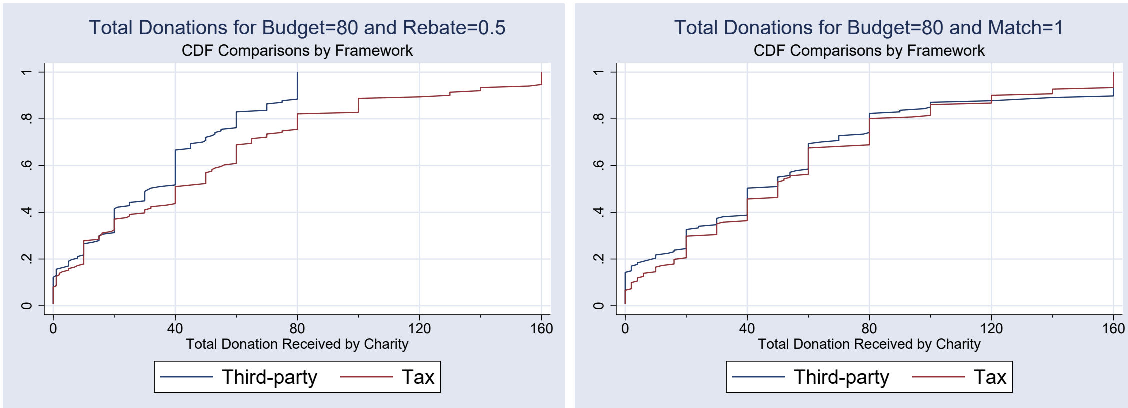


Figure 1.6: CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=80. Third-party in blue, Tax in red.

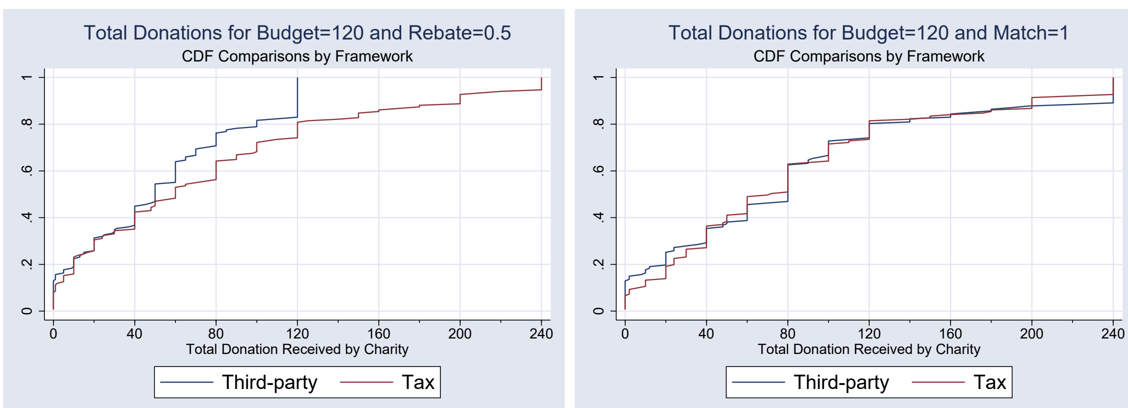


Figure 1.7: CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=120. Third-party in blue, Tax in red.

1.4.4 Follow-up experiments

Section 1.4.3 illustrates that the distribution of donations under rebates shifts significantly between the *third-party* and *tax* experiments. In this section, we present evidence that this shift is predominantly driven by the presence of the constraint in the *third-party* experiment, and is not driven by the tax language used in the *tax* experiment. Recall that both of our follow-up experiments—the *alt-tax* experiment and the *con-tax* experiment—use the exact same budget sets as the *third-party* experiment: the *alt-tax* experiment mirrors the *third-*

party experiment but employs the tax language used in the *tax* experiment, and the *con-tax* experiment mirrors the *tax* experiment but enforces the same constraint on donations under rebates that is present in the *third-party* experiment. Thus, if the use of tax language is responsible for the shift in donation behaviors under rebates, this shift will also be evident in the follow-up experiments. However, if the shift in behavior is mostly driven by the presence of the constraint, then behavior in the follow-up experiments should resemble that of the *third-party* experiment.

The results in both of the follow-up experiments are very similar to those of the *third-party* experiment. Figure 1.8 graphs the average consumption bundles of subjects in the *alt-tax* and *con-tax* experiments. Note the presence of the vertical dashed lines, which reflect the budget constraints faced under rebates, just like in the *third-party* experiment. Comparing Figure 1.8 with Figures 1.2 and 1.3, the behaviors in the *alt-tax* and *con-tax* experiments are qualitatively in line with behavior in the *third-party* experiment. Importantly, there appear to be large gaps between the average total donations for price-equivalent rebates and matches in both follow-up experiments, mirroring the gaps observed in the *third-party* experiment. Tables A.8 and A.11 confirm that the gaps between total donations for rebates and matches are large and highly statistically significant for each budget set in the *alt-tax* and *con-tax* experiments. Thus, the taxation language appears to have little effect on behavior. Instead, changes in behavior appear to be mostly driven by the change in budget sets across experiments.

To provide further evidence, we also compare donation behavior in these experiments to our main experiments. Average donations in the *alt-tax* and *con-tax* experiments are quantitatively very similar to those in the *third-party* experiment. Table A.9 (Table A.12) compares the average donations in the *third-party* and *alt-tax* (*con-tax*) experiments for each budget set. There are no statistically significant differences between the two experiments for any of the budget sets. A comparison of the average donations in the *alt-tax* (*con-tax*) and *tax* experiments, on the other hand, shows significant differences between the two

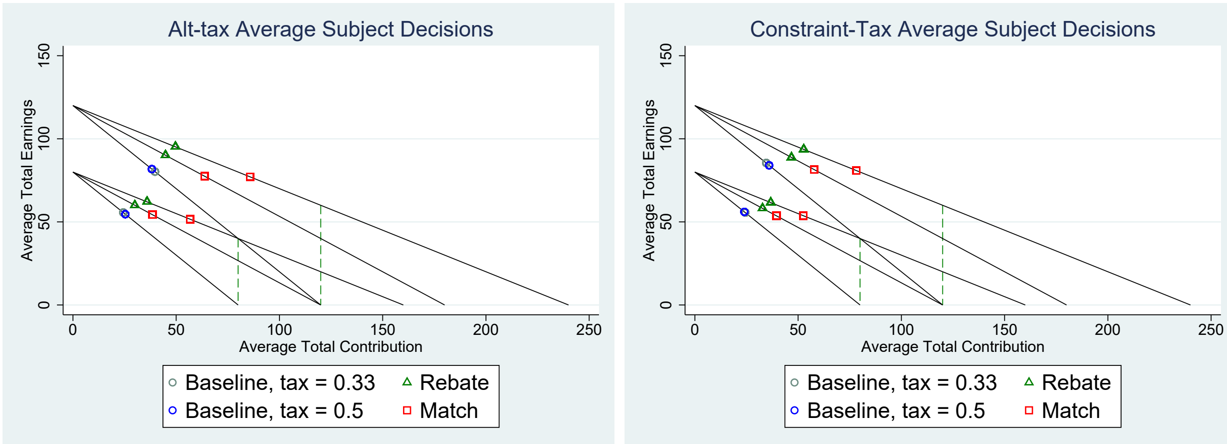


Figure 1.8: Average decisions in the alt-tax (left) and con-tax (right) experiments.

experiments. Table A.10 (Table A.13) compares the average donations in each experiment by budget. While average donations under match subsidies appear to be very similar for the two experiments, each of the rebate subsidies result in significantly different average donation amounts. This suggests our main results are indeed driven by removing the budget constraint present under rebates in the *third-party* experiment, rather than by any behavioral effect created by the use of tax language.

To derive estimates of the rebate- and match-price elasticities of giving in the *alt-tax* and *con-tax* experiments, we estimate demand using the same specification used for the *tax* experiment. Results are reported in columns (3) and (4) of Table A.14. Once again we find large gaps in the rebate- and match-price elasticity estimates. The gap is highly significant in both the *alt-tax* experiment (p-value=0.000) and the *con-tax* experiment (p-value=0.001). Together with the results reported above, the findings from these follow-up experiments provide no evidence to support the notion that tax framing is responsible for shifting donation decisions under rebates.

We conclude that the tax language used in the *tax* experiment does not significantly affect subjects' behavior, and therefore cannot explain the disparities between the *third-party* and *tax* experiments. Although previous research has found that framing decisions in the context of taxation can significantly alter subjects' behavior (see, e.g., Eckel et al.

(2005)), the results of our follow-up experiments demonstrate that subjects in our setting are not simply responding to the mention of taxation.

1.5 Discussion

Recall that in our *tax* experiment matches continue to perform significantly better than rebates, even if only by a small margin (see Table A.5). At the same time, Table 1.3 reveals no significant differences in the price elasticities for rebates and matches, either in magnitude or statistical significance. Combined, these two results are inconsistent with existing models of charitable giving. Traditional theories based solely on pure altruism fail to account for the differing impact of matches on donations, whereas warm glow theories, including Andreoni (1989)'s impure altruism model and Hungerman and Ottoni-Wilhelm (2021)'s impure impact model, can explain the superiority of matches over rebates but require distinct price elasticities. In other words, none of the existing theories can explain these findings simultaneously. In this section, we propose a straightforward extension to the impure altruism and impure impact models that maintains the warm-glow motive while also achieving parity in price elasticities across subsidies, albeit at the expense of equalizing donation levels under both subsidies.

In the charitable giving literature, warm glow is an accepted feature of individuals' decision-making process. The existence of warm glow has been repeatedly confirmed, and there are strong theoretical justifications for its inclusion in models of giving (Andreoni and Payne, 2013).[22] The impure altruism model—which combines warm-glow giving with altruistic giving—was first introduced by Andreoni (1989, 1990). However, warm glow was not originally defined within the context of subsidies for giving, and because of this, the impure altruism model is silent with respect to how warm glow should be affected by such subsidies.

[22] Warm glow has been convincingly documented in many experimental studies (Crumpler and Grossman, 2008; Konow, 2010; Tonin and Vlassopoulos, 2010; Ottoni-Wilhelm et al., 2017). And, as Andreoni and Payne (2013) point out, evidence of warm glow has even been documented beyond the field of economics (Harbaugh et al., 2007). There has also long been theoretical support for warm glow (Becker, 1974; Cornes and Sandler, 1984; Steinberg, 1987; McClelland, 1989; Andreoni, 1989, 1988).

So how should subsidies for giving affect warm glow? The answer may depend on how we interpret warm glow. For example, we may believe donors should feel warm glow from the *impact* they have on the charity—i.e., they should receive warm glow for the total amount received by the charity as a result of their donation. In the case of a match at rate s_m, the donor would receive warm glow for the amount $(1+m)g$, where g is the donor's 'out-of-pocket' donation. In the case of a rebate at rate s_r, the donor would receive warm glow for the amount g. However, it is also conceivable that donors would only feel warm glow for the actual cost of their donation, net of any subsidized funds (i.e., funds not provided by the donor). In this case, given a match at rate s_m, the donor would only receive warm glow for the amount g. And, for a rebate at rate s_r, the donor would only receive warm glow for the amount $(1-r)g$.

The impure impact model of Hungerman and Ottoni-Wilhelm (2021), however, takes a third approach. Rather than assume donors either do or do not feel warm glow for subsidized funds, the model implicitly assumes that donors receive warm glow for their *out-of-pocket* donations. In some sense, this assumption is logical: a donor's out-of-pocket donation is the amount the donor chooses to pass to the charity, so it would make sense they would feel warm glow for this amount. However, this assumes the donor receives warm glow for subsidized funds under a rebate but doesn't receive warm glow for subsidized funds under a match. That is, it *assumes* donors treat rebates and matches differently. Thus, the disparity between rebates and matches is an *assumption*, not a *prediction*, of the model.

The reason why assuming individuals feel warm glow for their out-of-pocket donation creates a disparity between rebates and matches is that their out-of-pocket donation actually represents a different choice variable depending on the type of subsidy being provided (see Section 1.2). Under a rebate, a donor's out-of-pocket donation is the same as their *gross* donation, g_g (i.e., the donor's *impact*/the total amount received by the charity, *including* any subsidized funds). Under a match, a donor's out-of-pocket donation is the same as their *net* donation, g_n (i.e., the donor's personal cost/the amount received by the charity *excluding* any subsidized funds).

To assume that donors receive warm glow based on their out-of-pocket donation is therefore equivalent to assuming that they fail to account for the effects of the subsidies—instead, they simply focus on their out-of-pocket donation. This assumption may or may not be true, but it seems inconsistent to make this assumption for the warm-glow portion of utility while simultaneously not making this assumption for other portions of utility, e.g., the private consumption portion of utility.[23] Furthermore, this assumption implies donors consider price-equivalent rebates and matches to be different—and therefore predicts that rebate- and match-price elasticities will in general not be equal—which directly contradicts our results.

We propose a simple extension which relaxes the assumption that donors feel warm glow for their out-of-pocket donation regardless of any subsidies. We simply introduce two new parameters, $\delta_m, \delta_r \in [0,1]$, which allow donors to vary in how much warm glow they receive from subsidized funds for matches and rebates, respectively. When $\delta_m, \delta_r = 0$, donors receive no warm glow for matched and rebated funds, respectively. When $\delta_m, \delta_r = 1$, donors fully receive warm glow from matched and rebated funds, respectively. That is, they feel the same amount of warm glow from subsidized funds as they do for their own funds. Of course, δ_m and δ_r are not necessarily equal. Note that the impure impact model of Hungerman and Ottoni-Wilhelm (2021) is a special case of this model when $\delta_m = 0$ and $\delta_r = 1$.

A donor's optimization problems under rebates and matches (in the tax framework) are now written, respectively, as

$$\max_{g_g \in \left[0, \frac{w}{1-t}\right]} U\left((1-t)\left[\frac{w}{1-t} - g_g\right],\ [1-(1-\delta_r)t]\, g_g,\ g_g + \lambda R_{-i}\right), \text{ and} \tag{1.11}$$

[23]That is, if we are going to assume that donors fail to account for the effects of subsidies for warm glow, then to be consistent we should also assume they fail to account for the effects that subsidies have on the cost of giving. For example, using the impure impact model of giving outlined in Section 1.2, a donor's utility given a rebate at rate s_r should be given by $U(w-g_g,\ g_g,\ g_g+\lambda R_{-i})$ rather than $U(w-(1-s_r)g_g,\ g_g,\ g_g+\lambda R_{-i})$.

$$\max_{g_n \in [0,w]} U\Bigg(w - g_n,\ (1 + \delta_m s_m)g_n,\ (1 + s_m)g_n + \lambda R_{-i}\Bigg). \tag{1.12}$$

Rewriting (1.12) in terms of t (using $s_m = \frac{t}{1-t}$) gives

$$\max_{g_n \in [0,w]} U\left(w - g_n,\ \frac{[1 - (1 - \delta_m)t]g_n}{1 - t},\ \frac{g_n}{1 - t} + \lambda R_{-i}\right), \tag{1.13}$$

and rewriting the donor's problem under a rebate (eq. 1.11) in terms of the donor's *net* donation, $g_n = (1 - t)g_g$, gives

$$\max_{g_n \in [0,w]} U\left(w - g_n,\ \frac{[1 - (1 - \delta_r)t]g_n}{1 - t},\ \frac{g_n}{1 - t} + \lambda R_{-i}\right). \tag{1.14}$$

It is now clear that the donor will view price-equivalent rebates and matches as equivalent anytime $\delta_r = \delta_m$ and will therefore have equal rebate- and match-price elasticities of demand. Likewise, they will not view price-equivalent rebates and matches as equivalent—and in general will have different rebate- and match-price elasticities of demand—whenever $\delta_r \neq \delta_m$. Therefore, this model allows for the possibility of either equal or unequal price elasticities. However, it requires the same level of donations under rebates and matches when the price elasticities are equal.

1.6 Related Literature

In this section we summarize the related literature (with a greater focus on previous laboratory experiments) and explain our unique contributions to this literature.[24]

1.6.1 Attempts at explaining the disparity

The first comparison of rebates and matches in the context of charitable giving is due to the seminal laboratory experiment conducted by Eckel and Grossman (2003). In response to the

[24]More detailed overviews of the literature comparing rebates and matches can be found in Vesterlund (2016) and Epperson and Reif (2019).

Eckel and Grossman study, numerous follow-up studies have been conducted in an attempt to verify and explain the disparate effects of matches and rebates. These studies include both field studies (Eckel and Grossman, 2008, 2017; Sasaki et al., 2022) and additional laboratory studies (Davis et al., 2005; Davis and Millner, 2005; Eckel and Grossman, 2006a,b, 2008).[25] And while each of these studies has replicated the discrepancy between rebates and matches, thus convincing the field that it is not simply an aberration, there has been little consensus regarding the cause of the difference. In general, though, the literature has interpreted these results as evidence that donor behavior contradicts the standard theoretical model of giving.

To resolve this issue, Hungerman and Ottoni-Wilhelm (2021) introduce a new model of giving, which they call the impure impact model. As demonstrated in Hungerman and Ottoni-Wilhelm (2021) and summarized in Section 1.2, because rebates and matches affect warm glow differently, the impure impact model can explain the observed gap between rebate- and match-price elasticities.[26] However, while this model is able to explain the disparity between rebates and matches in the context of charitable giving, it is unable to explain why some papers also find similar gaps in price elasticities when consumption goods or investment decisions are studied instead of charitable goods (Davis et al., 2005; Davis and Millner, 2005). The fact that the gap between rebates and matches has been replicated in other settings suggests it is not the result of some behavioral effect unique to charitable giving, implying that theoretical explanations based on warm glow may be misguided. One factor that *is* consistent across all of the experimental studies finding a gap between rebates and matches, however, is the experimental design used.

Our main contribution to the literature comparing rebates and matches is to show the extent to which the gap between rebates and matches observed in previous studies may be driven by the experimental design used. We show theoretically in Section 1.2 that the

[25] For more recent examples, see Bekkers (2015), Gandullia and Lezzi (2018) and Gandullia (2019).

[26] To clarify, this model is not necessary to explain the gap—if an individual receives *any* amount of warm glow utility (e.g., if they have *pure warm-glow* utility or *impure altruism* utility), theory predicts they will respond differently to rebates and matches. This was demonstrated by Turk et al. (2007) using an additively separable utility function. Our discussion in Section 1.2 demonstrates this result extends to general utility forms.

third-party framework used in previous studies imposes a constraint when subsidies are implemented via rebates. By designing a novel experiment using a taxation framework, we are able to produce equivalent budget sets for price-equivalent rebates and matches. We then use a controlled virtual laboratory setting—which provides enhanced control beyond what is feasible in field settings—to test the equivalence of rebates and matches. We find that donations are higher for matches than for rebates, but the gap between subsidies is now substantially smaller than in previous studies. More importantly, there is no longer any gap between the price elasticities for rebates and matches, suggesting that the assumption implicitly accepted in previous research—that rebates and matches affect warm glow differently—is likely not correct (when all confounding factors are controlled for).

1.6.2 Previous attempts to equate budget sets

Our paper is not the first to identify the disparity between budget sets for rebates and matches present in previous experimental studies, nor is our paper the first to attempt to remove the disparity. However, our paper is the first to identify the seriousness of this issue.

Prior efforts to remove the disparity between budget sets for rebates and matches are made in Davis (2006), Lukas et al. (2010) and Blumenthal et al. (2012). However, each of these studies uses an alternative approach from ours.

Lukas et al. (2010) remove the constraint on subjects' decisions under rebates by allowing subjects to borrow against their future earnings (i.e., their future rebate) when deciding how much to donate to the charity. While this design removes the disparity between budget sets for price-equivalent rebates and matches, Lukas et al. continue to find a statistically significant gap between the price elasticities for rebates and matches. This might be because borrowing from future rebates—and donating more than their income—may be confusing to subjects, given its unnatural and complex setting. It is also possible subjects are simply averse to the notion of 'borrowing' money or spending more than their income. Our tax framework has the advantage of being more intuitive and familiar to subjects, given that it mirrors how tax policy works in the real world.

Rather than expanding subjects' choices under rebates, Blumenthal et al. (2012) take the opposite approach, choosing to instead *restrict* subjects' choices under matches to be equal to their (constrained) choices under rebates, and continue to find large gaps in donation behavior. While this approach equalizes budget sets, constraining subjects' choices under both rebates *and* matches leads to biased estimates of *both* elasticities.[27] Unless the bias happens to be exactly the same under both rebates and matches, one cannot theoretically make a clean comparison of the price elasticities.

Yet another approach to equalizing budget sets is taken by Davis (2006), though the focus of their paper is not on the disparity in budget sets. Subjects are asked to choose Maximum Possible Contribution levels (i.e., total donations including subsidies, if any) under different endowment and subsidy levels instead of being asked to choose contribution levels. Noticing that previous designs introduce a constraint under rebates, Davis allows subjects to borrow from their future rebates to eliminate the constraint. Davis (2006) finds no difference between different subsidy formats.[28] While the findings of their paper are entirely consistent with ours, it is not clear what drives their results. In their setting, since subjects are choosing total donations including subsidies (if any), there is no perceivable difference between rebates and matches, which might artificially send a signal to subjects to behave the same between the two different subsidy types. In addition, Davis attributes their results to the *isolation effect*, and does not discuss the removal of the rebate constraint as a possible explanation. The isolation effect posits that individuals isolate their attention on the amount they are tasked to allocate (i.e., their endowment), ignoring the effects of any subsidies on the final allocation.

[27]Recall that the constraint becomes more impactful as the price of giving decreases, confounding the effects of price changes and resulting in biased price elasticity estimates.

[28]The only other paper that we are aware of that does not find differences between subsidy types is Diederich et al. (2022), but in the context of 'unit donation' schemes, where donors purchase units of charitable output rather than selecting a dollar amount to donate. While our results may seem comparable to theirs, their paper is not directly relevant to ours. First, as discussed in Diederich et al., there are many differences between unit donation schemes and money donation schemes. Second, and more importantly, the budget sets for price-equivalent rebates and matches are still not equivalent under a unit donation scheme (Diederich et al., 2021, 2022). Since the aim of our paper is to isolate the effect of the type of subsidy keeping everything else constant, Diederich et al.'s result is not informative for our analysis.

While the results of our paper are not consistent with an isolation effect hypothesis (which would require subjects to pass donations at a constant level under different subsidies and price levels), the results of Davis (2006) are entirely consistent with a rebate constraint issue. This increases our confidence in our results.

While there have been some attempts at removing the disparity between budget sets for price-equivalent rebates and matches, our paper shows that *how* we correct the constraint issue matters. Moreover, the general consensus among the existing literature has been that the issue is more or less negligible—the number of subjects observed to be constrained under rebates is typically low, and any bias introduced by the constraint can be addressed by simply accounting for censoring. Ours is the first paper in this literature to demonstrate the importance of this issue by showing that the constraint under rebates causes the entire distribution of donations to shift, introducing significant bias.

1.6.3 Previous estimates of rebate- and match-price elasticities

The main focus of our paper is to provide an unbiased *comparison* of rebate- and match-price elasticities. As we have previously mentioned, there are many additional factors present in real-world settings which will influence how individuals respond to rebates and matches. (Indeed, the absence of such confounding factors is one of the advantages of using a laboratory experiment to compare subsidies.) However, we believe that the harmony between our elasticity estimates and those previously estimated using empirical (i.e., non-experimental) methods further supports our assertion that the comparisons of rebates and matches in previous experimental studies are significantly biased. Therefore, we briefly discuss how our rebate- and match-price elasticity estimates fit within the extant literature.

We begin by discussing our match-price elasticity estimate (of −.899). While there are many studies estimating the match-price elasticity of giving in laboratory and field settings, we are not aware of any studies estimating the match-price elasticity of giving within a tax framework.[29] Instead, previous studies consider matches that are provided by a third-party donor. That being said, we have only argued that estimation of the *rebate*-price elasticity is biased by the use of the third-party framework. Thus, previous studies should still provide unbiased estimates of the *match*-price elasticity. And, indeed, our estimate is consonant with previous estimates produced in both lab and field settings, which are almost always around 1 in absolute value (Eckel and Grossman, 2003, 2008; Karlan and List, 2007; Karlan et al., 2011; Huck and Rasul, 2011).

Placing our rebate-price elasticity estimate (of –.848) within the literature is more difficult. Rebate-price elasticity estimates based on laboratory experiments using the third-party framework are biased, and no previous experiments have used a tax framework. The bias induced by the constraint under rebates in the third-party framework may be much less prevalent in field settings where donors are much less likely to provide donations anywhere near the constraint, but third-party rebates in the field are typically associated with significant time-delays (and, possibly, with uncertainty) that are not present in our laboratory setting. Because of this, empirical studies based on tax data may be a better comparison. While there is also a time-delay in receiving a rebate provided via the tax system, donors may be more confident that they will actually receive the rebate. Furthermore, empirical studies based on the tax system closely match the framework used in our experiment.

Empirical studies using tax data face difficult identification challenges, and their estimates are sensitive to the quality of data and empirical methods used. Unsurprisingly, they have produced a wide range of rebate-price elasticity estimates. That being said, our estimate of –.848 is consistent with a large body of empirical work. Andreoni (2006) notes that studies using cross sectional survey data from tax returns typically estimate the rebate-price elasticity to be between –1.1 and –1.3. Auten et al. (1992) and Auten et al. (2002) use panel

[29] One possible exception is the study by Scharf and Smith (2015), though the taxation framework they use is different than ours. Taking advantage of the unique tax structure in the UK—where some individuals receive a tax-funded matching donation provided by the government—they do in fact estimate a match-price elasticity for a *hypothetical* match subsidy implemented via taxes. That is, they estimate the match-price elasticity using a hypothetical survey-based approach.

data covering multiple tax reforms—which might produce more believable exogenous variation in prices—and estimate the rebate-price elasticity to be –1.11 and –1.26, respectively. In a similar study, Tiehen (2001) estimates the rebate-price elasticity to be between –0.9 and –1.1. In a meta-analysis of empirical studies based on tax data, Peloza and Steel (2005) find an average rebate-price elasticity of –1.1 after excluding outliers. And, in a more recent overview of the research, Bakija and Heim (2011) conclude that the tax-price (rebate-price) elasticity of giving is likely less than –1 (i.e., elastic).

Both our rebate- and match-price elasticity estimates are consistent with previous research, providing support for our experimental design. Our experiment is also the first incentive-compatible study to compare rebate and match subsidies that are both provided via a tax system.[30] As discussed, all previous experimental studies consider rebates and matches that are provided by a third party.[31] In the case of empirical studies, data limitations typically prevent the comparison of rebates and matches. The only empirical study we are aware of which compares rebates and matches is Hungerman and Ottoni-Wilhelm (2021), but in their setting matches are provided by a third-party donor rather than the tax system. As such, our elasticity estimates may also be more informative for tax policy than previous studies.

1.7 Conclusion

This paper challenges the well-established result among existing experimental studies that donations are significantly more responsive to price changes implemented via matches versus price changes implemented via rebates. We show theoretically that the third-party framework used in previous experiments creates a discrepancy between budget sets for price-equivalent

[30] Scharf and Smith (2015) also compare rebate and match subsidies using a taxation framework, but they use hypothetical scenarios.

[31] To the best of our knowledge, there are only two experimental studies that consider rebates and matches in the context of tax policy (Turk et al., 2007; Blumenthal et al., 2012), but in both of these papers rebates and matches are funded by the experimenter and not through taxes. Moreover, their focus is entirely different than ours. Both of these studies are mainly concerned with whether the type of subsidy affects tax compliance.

rebates and matches, and we argue that this discrepancy may significantly bias the comparison of price elasticity estimates reported in previous studies. To resolve this issue, we design a novel experiment which equates the budget sets for price-equivalent rebates and matches. We then use this novel experimental design to provide an unbiased comparison of price elasticity estimates.

The results of our main experiment confirm our suspicions—we find no statistical difference between the estimated rebate- and match-price elasticities of giving, suggesting previous studies' results are indeed significantly biased. To verify our results are not an artefact of our experimental setting (including any potential characteristics of our subject pool that are unique to our study), we also run an experiment replicating the third-party design used in previous studies. The results of our replication experiment are consistent with previous studies. Importantly, consistent with previous studies, we find a large and statistically significant difference between the estimated rebate- and match-price elasticities. Thus, we are able to conclude that our results are not an artefact of our experimental setting. Moreover, we run two additional experiments to investigate the mechanisms behind our main finding. Our results provide further evidence that the parity in price elasticities is driven by our experimental design's ability to remove the disparity in budget sets, rather than possible behavioral differences created by the use of a taxation frame.

Upon closer inspection of our results, we are able to confirm that the budget set issue present in previous experimental studies does in fact alter the entire distribution of subject behavior. That is, the constraint present under rebates in previous studies not only affects the decisions of subjects for whom the constraint is binding, but actually affects the behavior of all subjects. This extends the findings of List (2007) and Bardsley (2008) to a setting in which budget sets are expanded/contracted *without* introducing an option to take (and without affecting wealth levels). These results serve as an important reminder of the need to carefully consider the designs of experiments before interpreting their results, specifically with respect to the budget sets available to subjects. As we demonstrate in this study, subjects' behavior is highly sensitive to manipulations of the available budget sets.

We reiterate that we are not arguing that our rebate- and match-price elasticities are more externally valid than previous experimental estimates. Rather, we are simply arguing that our *comparison* of the rebate- and match-price elasticities is more valid. Extrapolating results from third-party experiments might be misleading for tax policies since it is unrealistic to expect the disposable-income constraint to be as salient as in these experiments. Moreover, in certain scenarios such as payroll giving, individuals are not constraint by their disposable income, just like in our tax framework. By resolving the budget set issue present in previous studies, we find that donors are equally responsive to price changes resulting from rebates and matches. Our results have important implications for charitable organizations interested in maximizing donations, and they also have important implications for tax policy. While previous studies have suggested that charitable giving could be significantly increased by restructuring the tax system to provide matches instead of rebates, our study suggests that restructuring the tax system in this way may actually have little impact on giving. Furthermore, as the first study to compare rebates and matches which are both provided within a tax framework, our study makes an important contribution to the literature on tax policy.

Finally, our study also contributes to theoretical models of giving. While previous studies have suggested the need for a theoretical model of giving capable of explaining why rebates and matches would produce *different* price elasticities, the results of our study instead suggest the need for a model capable of explaining why rebates and matches would produce *equal* price elasticities. We clarify that previous studies have implicitly assumed that donors feel warm glow differently for rebates and matches, and that it is this assumption which drives their theoretical nonequivalence. We also suggest a simple extension of previous warm-glow models of giving, which relaxes this assumption and allows existing models to explain the equivalence of price elasticities, albeit at the expense of equalizing donation levels under both subsidies. Our paper re-opens the discussion of how we should model charitable giving.

Chapter 2: Donors Care About the Probability of Receiving a Match

2.1 Introduction

Match schemes are commonly used by charitable organizations to boost donations. Putting aside the question of whether a match is the most effective *form* of subsidy the charity can use to increase donations (see the previous chapter), if they want the subsidy to be effective it is important they carefully select the subsidy *rate*. In the case of a match, this means deciding what match rate to offer. While charities aim to maximize donations by choosing the optimal match rate, existing studies show mixed results on the effectiveness of different match rates. Depending on the study, match subsidies either have no effect on donors, partial positive effects but only for some donors, or significantly positive effects for all donors (see, e.g., Rondeau and List (2008), Karlan and List (2007), and Huck and Rasul (2011), respectively).

In this chapter, I aim to develop a better understanding of how match rates influence donor behavior, enabling charities to design more effective fundraisers and advancing our understanding of why people give. To do this, I take a novel approach in modeling matches, framing them in the context of uncertainty. The underlying mechanism is simple and intuitive: because match subsidies typically always place a cap on the total amount of donations that can be matched, donors perceive matches as uncertain. When deciding how much to give, the donor forms beliefs about the likely total giving of all other donors. If they believe the total donations received by the charity are likely to exceed the match limit, they will consider it unlikely that they will receive a match, making the match less effective.

This framework provides an appealing explanation for the apparently conflicting results reported in existing studies: depending on the characteristics of the fundraiser—e.g., its size and scope, the average wealth of the donor pool, etc.—donors will hold different beliefs about the expected total giving of others, and as a result they will also hold different beliefs about the probability of receiving a match. For example, if a lead donor were to offer to match donations to a donor's local church up to a maximum amount of $25k, depending on the size of the church the donor may believe they have a high probability of receiving a match. However, if the same match offer were provided for a large organization like the Red Cross, donors would likely perceive the probability of receiving a match to be negligible. If donors care about the impact of their donation, they will respond to matches differently depending on the characteristics of the fundraiser and their expectations about the total giving of others.[1]

I formalize this mechanism by developing a theoretical model in which donors are assumed to be impure-impact givers (Hungerman and Ottoni-Wilhelm, 2021). I assume donors take the total giving of others to be a random variable, forming a probability distribution representing their beliefs about its likely value. These beliefs are assumed to be functions of the characteristics of the fundraiser, including the number of potential donors, their wealth levels, etc., as well as the match rate and match limit.

[1]There are two objections that I suspect some readers will be raising at this point. The first is that donors cannot reasonably be assumed to know the characteristics of the fundraiser. While I agree with this and believe that any realistic representation of donor behavior would incorporate this fact, for the current analysis it is inconsequential. Although donors may not know the true characteristics of the fundraiser, they should in most cases still be able to form *beliefs* about the characteristics. For example, in the example provided with the local church and the Red Cross, while it would be unrealistic to expect donors to know the exact number of potential donors for the Red Cross (or even their local church), it is not at all unreasonable to expect that they should *believe* that there are many more potential donors for the Red Cross. The end result of this belief is the same—they will expect the Red Cross to receive more donations and therefore perceive a lower probability of receiving a match.

The second objection I anticipate some readers making is that donors have no way of differentiating between receiving a match and not receiving a match. For example, if a donor provides a gift early in a match campaign before the match limit has been met, and then at some later time the match limit is exceeded, does their donation retroactively become unmatched? I agree that this is an interesting philosophical question—one that truly tickles the mind—but if the donor is concerned with the *impact* of their gift, then the answer is yes. To calculate the impact of their gift, the donor must consider how much the charity would forgo in its absence. From this perspective, it is clear then that their donation did not receive a match.

Analysis of the model highlights a key feature of match subsidies that has received little discussion in the literature: one cannot increase the match rate without either decreasing the match limit or increasing the size of the lead gift. Previous studies have highlighted the importance of the lead-gift effect and argued that one must control for it when estimating the match-price elasticity. However, as the match rate (μ) is increased, if the lead gift (ϕ) is held constant, the *match limit* ($\frac{\phi}{\mu}$) mechanically decreases. For example, if the match rate is doubled—so that every dollar donated receives twice as many matching dollars—it takes half the amount of donations to fully exhaust the lead gift. Thus, holding the lead gift constant, an increase in the match rate creates two counteracting effects: on the one hand, it increases the *potential* impact of the donor's gift *if* a match is received, but at the same time, it also decreases the probability of receiving a match. The magnitude of the effect on the donor's perceived probability of receiving a match will depend on their beliefs about the likely giving of others. Because these beliefs are a function of the characteristics of the fundraiser, the donor's response to a change in the match rate can vary substantially across settings. As I show in Section 2.3, the same donor can sometimes respond positively to an increase in the match rate (i.e., increase their out-of-pocket donation in response) and other times respond negatively, even when the lead gift amount and the match rate (and the change in the match rate) are the same in both cases. That is, if one fails to account for the difference in the donor's perceived probabilities of receiving a match in each setting, it would appear that the subject is acting inconsistently.

To test the performance of the model, I derive theoretical predictions from it and design a real-incentive (online) laboratory experiment to test the predictions. While the overall aim is to eventually demonstrate that fundraiser characteristics can affect how donors respond to match subsidies by shifting their beliefs about the probability of receiving a match, in this chapter I focus on first establishing that donors are in fact responsive to changes in the probability of receiving a match. To do this, I exogenously vary the probability with which matches are received by simply stating that the individual's donation will be matched with a given probability (e.g., .25, .5, etc.).

The results of the experiment provide strong evidence that donors are responsive to changes in the probability of receiving a match, and the observed behavior of subjects in the experiment is largely consistent with the predictions of the theoretical model. While larger match rates induce larger average donations, this effect diminishes as the match probability decreases. This finding provides support for the theoretical model, and it also documents the mechanism by which match rate increases can potentially result in a decrease in donations: if the match rate increase induces a large enough reduction in donors' perceived probabilities of receiving a match, the effectiveness of the match will be significantly reduced. These findings document the existence of a new mechanism important to donors' decision-making process and advance our understanding of why people give. The insights developed help to reconcile previous results, and they provide charitable organizations with meaningful guidance for designing more effective fundraisers.

The remainder of this paper is organized as follows. Section 2.2 discusses related literature. Section 2.3 develops the theoretical model, presents the assumptions needed, and derives theoretical results. Section 2.4 presents the experimental design and outlines the experimental procedures. Section 2.5 presents results, and Section 2.6 concludes. Proofs of theoretical results are provided in Appendix B.1. Appendices B.2, B.3, and B.4 present additional tables, figures, and materials from the experiment, respectively.

2.2 Literature Review

There is a large body of literature studying the use of match subsidies in charitable giving. As I will discuss in detail, evidence on the effectiveness of match subsidies is mixed, but there is strong evidence suggesting that, in general, donors prefer matches to the absence of any subsidy (Fang et al., 2021), and that overall matching grants increase donations for the fundraisers using them without crowding out donations to other fundraisers (Meer, 2017). A significant portion of the research on matches involves laboratory and field studies that seek to compare rebates and matches, to identify whether they produce disparate effects on giving (e.g., Eckel and Grossman (2003, 2008); Higgs and Uler (2023)). Importantly, laboratory experiments using match subsidies provide matches that are always certain. Because of this, their results are not particularly germane to a discussion of uncertain matches.

Outside of the laboratory, there have also been several field studies looking singularly at match subsidies. As mentioned in the introduction, this literature has reported inconsistent match-price effects. Rondeau and List (2008) find that individuals are unresponsive to matching gifts but are responsive to challenge gifts,[2] implying that the best match rate is a rate of 0:1 (i.e., no matching). In a natural field experiment, Karlan and List (2007) find that matching offers lead to both higher response rates (i.e., a greater likelihood of giving) and larger donations, but that increasing the match ratio above 1:1 has no additional effect. Similarly, in another natural field experiment, Karlan et al. (2011) find no difference between offering match rates of 1:1 and 1:3, and find only weak evidence that matches have any effect on donations. In fact, they find that the matching only increases giving *after* the match deadline has expired. From the perspective of uncertain matches, if individuals consider the probability of their donations being matched when deciding what to give, these findings would suggest that matching may decrease giving, since the perceived probability of their donations being matched should drop to zero after the deadline has passed.

However, while the studies mentioned provide evidence that larger match rates are not always beneficial, it is not clear that their findings apply more generally. In each of the studies, the experiments were designed for charities that routinely solicit donations, ranging from 3-4 times a year (Rondeau and List, 2008) to monthly (Karlan and List, 2007). Because of this, individuals may form a habit of donating a set amount at each time interval, making them less responsive to one-time changes in the price of giving. Moreover, other studies have found strong evidence of increased giving resulting from larger match rates (Meier, 2007; Huck and Rasul, 2011).

[2] A challenge gift is where a lead gift is simply announced, but is not used to match the donations of others.

Huck and Rasul (2011) are careful to keep the size of the lead gift constant while varying the match rate, in order to prevent differences in the lead gift from confounding their results. Even after controlling for the effect of the lead gift, they continue to find that larger match rates result in greater charitable receipts. In spite of this, though, they also find that simply announcing the presence of the lead gift generates higher donations. Similar results are also reported in Huck et al. (2015). While counterintuitive, these results are actually consistent with an uncertain-matches framework: if donors respond negatively to a match, they can end up increasing their donations as the match rate is increased, as long as the increase in the match rate sufficiently decreases their perceived probability of receiving a match.

Though it remains an open question whether simply announcing a lead gift is superior to offering a match, there is significant evidence suggesting the lead gift has an important impact on donations, separate from any price effects created by the match (Andreoni, 1998; Chen et al., 2005). The mere presence of a matching grant can serve as a signal of the charity's quality, increasing donations (Vesterlund, 2003). In a recent example of this, Karlan and List (2020) find that matching grants provided by the Bill and Melinda Gates Foundation are more effective when they are given publicly versus when they are given anonymously, suggesting donors interpret the matching grant as a sort of endorsement. Building on the model in Landry et al. (2006), Landry et al. (2010) develop a model that incorporates the quality signal associated with matching grants. Similar to the model I develop, their model incorporates uncertainty in the donor's decision-making process. However, in their model it is the quality of the charity that is uncertain, and the presence of a matching grant acts to increase the probability that the charity is perceived as high-quality.

Beyond any quality signals, however, there is also a great deal of evidence that donors particularly care about their *impact*. Meer (2014) finds that higher overhead costs reduce the amount donated to charities. Similarly, Gneezy et al. (2014) find that donors give more when they're informed that another party is covering the charity's overhead costs. This is true regardless of the amount of overhead costs, implying that it's not that donors are concerned with the efficiency of the charity, but rather that they simply want to attribute their own donation to a larger impact. However, in Green et al. (2015), the authors find that reminding donors of the multiplicative effects of matching grants has no impact on their donations, potentially suggesting they are unmoved by considerations of their impact (or that they had already accounted for the effect of the match on their impact, making the reminder moot). In the model I develop, it is essential that donors consider their impact—if not, changes in the match probability will not enter their utility functions.

That said, irrespective of impact considerations, changes in donors' beliefs about the giving of others may still affect their behavior (Croson, 2007; Shang and Croson, 2009; Croson and Shang, 2013; Smith et al., 2015). Even if donors are completely unconcerned with their impact, they may respond to changes in the donations of others due to reciprocity concerns (Croson, 2007; Fischbacher and Gächter, 2010). In this case, a change in the match rate may cause donors to adjust their donation amounts by shifting their beliefs about the likely total giving of others. Examples of this type of behavior have previously been observed in charitable giving research, and other-regarding preferences have also been documented in other settings (Fehr and Schmidt, 1999; Fehr and Leibbrandt, 2011; Fischbacher et al., 2001).

While the charitable giving literature has identified a long list of possible motivations that may influence donor behavior in the context of matches, little work has been done to study the effect of donors' perceptions about the probability of receiving a match. Yet, this notion is consistent with many aspects of previous findings. Previous results suggest that donors not only care what others give, but they also form beliefs about the behavior of others. Previous results also suggest that donors are concerned with the impact of their donations, seeking to maximize their impact, all else equal. These insights are incorporated in a model with uncertain matches: donors are concerned with their impact (i.e., whether or not they will receive a match), and they form beliefs about the behavior of others (i.e., to derive their perceived probability of receiving a match).

Furthermore, the uncertain-matches framework is also able to provide an intuitive explanation for previous, seemingly inconsistent estimates of the response to match subsidies. Moreover, as I will now discuss, there are several other results for which the uncertain-matches framework provides appealing potential explanations.

One such result is that of Charness and Holder (2019). Using a laboratory experiment, they study how giving decisions are affected by *competition* for matches, both between individuals and between groups. In contrast with Duffy and Kornienko (2010), they find that donations are lower when individuals must compete with one another for their donations to be matched, compared to when their donations are matched with certainty. However, they find that donations increase when individuals are part of a group that competes with another group for their donations to be matched. Charness and Holder are unable to provide a definitive explanation for their findings, but they suggest that individuals may be responding to team effects (i.e., individuals feel as though they are part of a team, and they don't want to be the reason the team loses).

Interestingly, a model where individuals consider the probability of their donations being matched could also explain the findings presented in Charness and Holder (2019). While it is not the focus of their paper, Charness and Holder actually vary the probability of individuals' donations being matched across treatments. When individuals compete with one another for their donations to be matched, their perceived probabilities of being matched should be lower than when their donations are matched with certainty. This suggests that they should give less, which is what Charness and Holder find. When the competition is instead among teams, the perceived probability of being matched should still be less than certainty, which all else equal should decrease donations. However, the potential payoff to an individual of increasing their donation would now be much larger, since their donation could potentially result in the donations of their entire team being matched.[3] For an expected utility maximizer, this

[3]This assumes that the individual either receives utility from their impact, or that they receive warm glow from giving and the increase in their team's total donations increases the amount of warm glow they receive. If an individual is purely altruistic (or they receive no warm glow for the team's matched funds), causing their team's donations to be matched will still increase their utility, but the effect will be much smaller, since the total amount received by the charity will only be marginally increased. Note, however, that if an individual believes that their team will win and have its donations matched, they will perceive

large increase in the payoff could more than offset the decreased likelihood of being matched, resulting in larger donations.

A framework in which matches are uncertain can potentially explain observed behavior in other settings as well. In a framed field experiment, Null (2011) finds that donors spread their donations among multiple charities rather than focusing their funds on the charities with the highest match rates, failing to take advantage of the lower prices created by the differential match rates. Null argues this behavior suggests that individuals receive warm glow not only from the amount that they donate, but also from the act of giving itself. That is, there appears to be a fixed benefit to giving, motivating individuals to spread donations across multiple charities. However, an uncertain-matches framework can potentially provide an alternative explanation. If individuals are concerned about the probabilities of their donations being matched, similar behavior might be observed, as spreading donations across multiple charities may hedge their risk and maximize the expected amount of donations receiving a match.

In closing, I now turn to discussing the only other study I am aware of that considers donors' beliefs about the probability of receiving a match, that of Gee and Schreck (2018). Through a combination of theoretical, field, and lab results, Gee and Schreck (2018) find that individuals are more likely to donate when they believe they are more pivotal to securing matching funds.[4] This is an important contribution to the literature because it provides evidence that individuals not only form beliefs about the giving behavior of others but also use these beliefs to form further beliefs about the probability of receiving matching funds. However, there are several ways in which the research I present in this chapter differs from the work done by Gee and Schreck (2018). First, their study considers 'threshold matches,' where subjects are placed in groups and a match is received if the *number of group*

their price of giving as being lower.

[4]Adena and Huck (2022) combine this insight with the use of customized threshold levels to effectively incentivize donors to contribute more. In their setting, donors aren't grouped, so each donor knows with certainty that they are pivotal to securing the matching funds. Their results provide additional evidence that donors care about their impact.

members providing a donation reaches some threshold number, similar to the designs used in Offerman et al. (1996) and Anik et al. (2014). While this design allows them to vary subjects' beliefs about the probability of receiving a match, it is a very different design from those used in previous laboratory and field experiments studying the response to match subsidies, and it is not clear what price of giving the subjects face in this setting. Second, in their theoretical model and experimental design there are no match limits, so it is not possible to study the relationship between the match rate and the match limit, and how they may simultaneously influence donors' beliefs about the probability of being matched. Finally, in both their theoretical model and their experimental design subjects are unable to determine the amount they would like to donate. Rather, subjects face a binary decision to either pass an exogenously determined amount to the charity or provide no donation. Because of this, it is not possible to estimate how donation *amounts* respond to changes in the probability of being matched.

A model of giving that incorporates donor uncertainty regarding the receipt of matches is intuitive, consistent with previous results, and provides plausible explanations for a variety of observed behavior. The development of such a model, and its validation in an experimental setting, advances our understanding of why people give and makes an important contribution to the literature.

2.3 Theory

In this section, I develop a theoretical model of giving in which donors consider matches to be uncertain. Whether or not a donor receives a match depends on the match limit and the total giving of others. Receiving a match is uncertain because donors do not know the total giving of others (at least at the time they are making their donation decision). Instead, each donor views the total giving of others to be a random variable, constructing a probability distribution for its realized value. Each donor's PDF of the total giving of others is a function of the characteristics of the fundraiser, such as the size of the fundraiser (i.e., the number of potential donors) and the average wealth of potential donors. Because fundraiser characteristics vary, the model is able to explain why donors might respond to match subsidies differently depending on the setting.

2.3.1 Basic Setup

For a given charity, there are a total of N potential donors, where N is exogenously determined. Each potential donor $i = 1, ..., N$ has utility given by the impure impact model of Hungerman and Ottoni-Wilhelm (2021). That is, each potential donor's utility is given by $U_i(w_i - g_i, g_i, R_i + \lambda R_{-i})$. I assume that U_i is strictly increasing and concave in each of its arguments for all i.

Each donor i is endowed with exogenous wealth, $w_i \in \mathbf{R}_+$, from which they choose an amount, $g_i \in [0, w_i]$, to donate to the charity. Let $G = \sum_{i=1}^{N} g_i$ be the total amount passed to the charity by all individuals $i = 1, ..., N$, and let $G_{-i} = \sum_{j \neq i} g_j$ be the total amount of donations passed to the charity by all individuals other than i. Donations to the charity are matched by a third party donor at some match rate, $\mu \in \mathbf{R}_+$, up to the point that the exogenously set lead gift, $\phi \in \mathbf{R}_+$, has been fully depleted. That is, as long as $\mu\,(g_i + G_{-i}) \leq \phi$ (that is, total donations do not exceed the match limit), individual i's donation, g_i, is matched at rate μ, so that the total donation received by the charity (i.e., donor i's *impact*) is $R_i = (1 + \mu)g_i$, consisting of i's donation of g_i and the third party's matching donation of μg_i. If $\mu G_{-i} < \phi < \mu(g_i + G_{-i})$, then i's donation is *partially* matched. In this case, donor i's impact is given by $R_i = g_i + \phi - \mu G_{-i}$. Finally, if $\mu G_{-i} \geq \phi$ (other's total donations exceed the match limit), i's donation, g_i, is not matched, and donor i's impact is given by $R_i = g_i$.

Note that the lead gift amount, ϕ, is different from the match limit, $\frac{\phi}{\mu}$. The lead gift is the total amount of funds available for matching donations, while the match limit is the total amount of donations that can be matched before the lead gift is fully exhausted. Therefore, holding the size of the lead gift, ϕ, constant, increasing the match rate, μ, will mechanically decrease the match limit, $\frac{\phi}{\mu}$. As the match rate is increased, fewer donations are required to fully exhaust the lead gift.[5]

To simplify the analysis, I ignore the possibility of donor i receiving a partial match. Instead, I will assume that donor i considers a partial match to be equivalent to a full match. This is formalized by the following assumption.

Assumption 1 *Donors consider their impact when receiving a partial match to be equivalent to their impact when receiving a full match. For all $i = 1, ..., N$, we have the following:*

$$R_i = \begin{cases} (1+\mu)g_i & \text{for } 0 \leq G_{-i} < \frac{\phi}{\mu} \\ g_i & \text{for } \frac{\phi}{\mu} \leq G_{-i} \leq W_{-i} \end{cases} \tag{2.1}$$

Under this assumption, donors view the match as binary: they either receive a full match or no match, depending only on whether or not the total giving of others fully exhausts the match limit. This is equivalent to donors simply ignoring the possibility of receiving a partial match. In practice, for large fundraisers with many donors, the probability of receiving a partial match will be negligible and likely would be ignored by the vast majority of donors.[6]

As long as the total giving of others is less than the match limit (i.e., $G_{-i} < \frac{\phi}{\mu}$), donor i will consider their impact to be $R_i = (1+\mu)g_i$. Therefore, donor i's utility is given by:

$$U_i\big(x_i, g_i, R\big) = \begin{cases} U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big) & \text{for } 0 \leq G_{-i} < \frac{\phi}{\mu} \\ U_i\big(w_i - g_i,\ g_i,\ g_i + \lambda R_{-i}\big) & \text{for } \frac{\phi}{\mu} \leq G_{-i} \leq W_{-i} \end{cases} \tag{2.2}$$

[5]For example, consider a lead gift of \$1,000. If a 1:1 match is provided ($\mu = 1$), it will require \$1,000 in donations to fully exhaust the lead gift. However, if the match rate is increased to a 2:1 match ($\mu = 2$), then it will only take \$500 in donations to fully exhaust the lead gift.

[6]In order for a donor to perceive a significant probability of receiving a partial match, they would either need to have a high level of confidence that G_{-i} is very close to (but just below) the match limit, $\frac{\phi}{\mu}$, or they would need to be making a very large donation, g_i, relative to their beliefs about the total giving of others, G_{-i}.

where $W_{-i} = \sum_{j \neq i} w_j$. The parameters μ, ϕ, N, $\mathbf{w}$, λ, and R_{-i} are all exogenous. The total giving of others, G_{-i}, is assumed to be exogenous to g_i, but may be a function of the parameters. Together, the above two expressions give donor i's utility for any choice of $g_i \in [0, w_i]$ and any realization of the total giving of others, G_{-i}.

While (2.2) gives donor i's utility for any choice of $g_i \in [0, w_i]$ and total giving of others, G_{-i}, they are unable to optimize over (2.2) because the total giving of others, G_{-i}, is not known with certainty to them. Instead, I assume donors take G_{-i} to be a realization of a random variable $\tilde{G}_{-i}$, and they form beliefs about the distribution of $\tilde{G}_{-i}$ based on observable characteristics of the fundraiser. This is formalized by the following definition.

Definition 1 $F_i\big(G_{-i}|N, \mu, \phi, \mathbf{w}\big) = P_i\left(\tilde{G}_{-i} \leq G_{-i}|N, \mu, \phi, \mathbf{w}\right)$ is the CDF of donor i's perceived distribution of G_{-i}, given N, μ, ϕ, and $\mathbf{w}$, where $\mathbf{w}$ is the average wealth level among the fundraiser's population of potential donors, and $\tilde{G}_{-i}$ is a real-valued random variable.

That is, based on the values of N, μ, ϕ, and $\mathbf{w}$, each donor forms a distribution of the likely values of G_{-i}.[7] Assuming donors are expected utility maximizers and $F_i\big(G_{-i}|N, \mu, \phi, \mathbf{w}\big)$ is twice continuously differentiable for all i, donor i will choose $g_i \in [0, w_i]$ to maximize

$$EU_i(g_i|\cdot) = F_i\left(\frac{\phi}{\mu}\middle|N, \mu, \phi, \mathbf{w}\right) U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big) + \left[1 - F_i\left(\frac{\phi}{\mu}\middle|N, \mu, \phi, \mathbf{w}\right)\right] U_i\big(w_i - g_i,\ g_i,\ g_i + \lambda R_{-i}\big), \quad (2.3)$$

where $F_i(\frac{\phi}{\mu}|\cdot)$ is the donor's perceived probability of being matched, and $[1 - F_i(\frac{\phi}{\mu}|\cdot)]$ is their perceived probability of not being matched. Also note that $U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big)$ is donor i's utility given a certain match (i.e., $F_i(\frac{\phi}{\mu}|\cdot) = 1$ and $\mu > 0$), and $U_i\big(w_i - g_i,\ g_i,\ g_i +$

[7] I assume the parameter values are known by all donors, choosing to instead introduce uncertainty through each donor's construction of their perceived distribution of G_{-i}. However, since each donor forms their own beliefs about the distribution of G_{-i}, the model would be equivalent if donors were to instead each hold unique beliefs about the values of the parameters.

$\lambda R_{-i}\big)$ is their utility when there is no match (i.e., $F_i(\frac{\phi}{\mu}|\cdot) = 0$ or $\mu = 0$). Thus, EU_i is simply the weighted average of the potential outcomes, match and no match.

The following definitions will be used to simplify the notation in the proceeding analysis:

Definition 2 Donor i's utility when *not* receiving a match is given by

$$U_i^0 \equiv U_i\big(w_i - g_i,\ g_i,\ g_i + \lambda R_{-i}\big). \tag{2.4}$$

Definition 3 Donor i's utility when receiving a match at rate μ is given by

$$U_i^1 \equiv U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big), \tag{2.5}$$

Definition 4 Donor i's *perceived probability of receiving a match* is given by

$$p_i \equiv F_i\left(\frac{\phi}{\mu}\,\middle|\, N, \mu, \phi, \mathbf{w}\right). \tag{2.6}$$

Using these definitions, donor i's optimization problem (2.3) can be rewritten as choosing $g_i \in [0, w_i]$ to maximize

$$EU_i(g_i|\cdot) = p_i U_i^1 + (1 - p_i) U_i^0 \tag{2.7}$$

To guarantee that any local maximum is also a global maximum, I also make the following assumption.

Assumption 2 *Let donor i's optimization problem in the absence of a match (i.e., $F(\cdot) = 0$) be given by $\max_{g_i\in[0,w_i]} U_i^0(g_i|\cdot)$, and let their optimization problem in the presence of a certain match (i.e., $F(\cdot) = 1$) be given by $\max_{g_i\in[0,w_i]} U_i^1(g_i|\cdot)$. Then for all $i = 1, ..., N$, we* *have the following:*

$$(i)\ \ U^0_{gg} < 0\ \forall g \in [0, w_i] \tag{2.8}$$

$$(ii)\ \ U^1_{gg} < 0\ \forall g \in [0, w_i] \tag{2.9}$$

2.3.2 Model Predictions

The Effect of a Change in the Match Probability

In this section I demonstrate that changes in the perceived probability of being matched (henceforth referred to as p) affect donors' optimal donation amounts. That is, donors should in general be responsive to changes in p. In what follows, I assume interior solutions and drop the i subscripts for brevity. Taking the derivative of (2.3) with respect to g, the first order condition is given by

$$F\left(\frac{\phi}{\mu}\middle|\cdot\right)\cdot\left(-\frac{\partial U^1}{\partial x}+\frac{\partial U^1}{\partial g}+(1+\mu)\frac{\partial U^1}{\partial R}\right) + \left[1-F\left(\frac{\phi}{\mu}\middle|\cdot\right)\right]\cdot\left(-\frac{\partial U^0}{\partial x}+\frac{\partial U^0}{\partial g}+\frac{\partial U^0}{\partial R}\right)=0 \tag{2.10}$$

where $U^1 = U\big(w-g,\ g,\ (1+\mu)g+\lambda R_{-i}\big)$ is the donor's utility when there is a match, and $U^0 = U\big(w-g,\ g,\ g+\lambda R_{-i}\big)$ is the donor's utility in the absence of a match. Letting $FOC^1(g)$ denote the donor's first order condition given a certain match (i.e. $p = 1$), and letting $FOC^0(g)$ denote the donor's first order condition in the absence of a match, the above first order condition can be rewritten as

$$pFOC^1(g) + (1-p)FOC^0(g) = 0. \tag{2.11}$$

That is, the donor's first order condition given an uncertain match is simply the weighted average of their FOC's for the two possible outcomes (match and no match). This leads to the following result:

Lemma 1: *For each donor i, let $g^0(\mu) \in (0, w)$ be the optimal choice of g in the absence of a match, so that $g^0(\mu)$ maximizes $U^0 = U(w - g,\ g,\ g + \lambda R_{-i})$.*[8] *Likewise, let $g^1(\mu) \in (0, w)$ be the optimal choice of g given a certain match at rate μ, so that $g^1(\mu)$ maximizes $U^1 = U(w-g,\ g,\ (1+\mu)g+\lambda R_{-i})$. Finally, let $g^*(\mu)$ be the optimal choice of g given an uncertain match at rate μ, so that $g^*(\mu)$ maximizes $EU = pU^1 + (1-p)U^0$, where $p \in (0, 1)$. Then $g^*(\mu) \in (g^0(\mu),\ g^1(\mu))$ for all μ such that $g^0(\mu) \neq g^1(\mu)$, and $g^*(\mu) = g^0(\mu) = g^1(\mu)$ for all μ such that $g^0(\mu) = g^1(\mu)$.*

Proof of Lemma 1 is provided in appendix B.1. Lemma 1 states that, for any match rate μ, each donor's optimal choice of g when the match is uncertain (g^*) must fall between their optimal choice of g when no match is received (g^0) and their optimal choice of g when the match is received with certainty (g^1).

Letting g^* denote the optimal choice of g as defined by (2.11), we can use the implicit function theorem to determine the donor's optimal response to a change in p:

$$\frac{\partial g^*}{\partial p} = \frac{FOC^0(g^*) - FOC^1(g^*)}{|H(g^*)|}, \tag{2.12}$$

where $|H(g^*)| < 0$ is the determinant of the Hessian for (2.3) evaluated at g^*.[9] The sign of $\frac{\partial g^*}{\partial p}$ depends on the sign of the numerator in (2.12), which can vary across donors. However, based on how a donor responds to receiving a certain match at rate μ relative to receiving no match, the sign of $\frac{\partial g^*}{\partial p}$ can be determined. This is formalized in the following lemma:

Lemma 2: *For each donor i and all μ, if $g^1(\mu) > (=) < g^0(\mu)$, then $FOC^0\left(g^*(\mu)\right) -$*

[8] Note that since μ does not enter U^0, $g^0(\mu)$ is constant in μ. That is, $g^0(\mu) = g^0$ for all $\mu \in \mathbf{R}_+$.

[9] Since the donor's optimization problem (2.3) is a function of only g and we have assumed an interior solution, the Hessian is just the term EU_{gg}. That is, $|H(g^*)|$ is just the second order condition evaluated at g^*. Since U^0_{gg}, $U^1_{gg} < 0\ \forall g \in [0, w]$ and EU is just a weighted sum of U^0 and U^1, it follows that $EU_{gg} < 0\ \forall g \in [0, w]$.

$FOC^1\left(g^*(\mu)\right) < (=) > 0$ *for all* p.

Proof of Lemma 2 is provided in appendix B.1. This result leads to Proposition 1.

Proposition 1: *For any donor whose optimal donation increases (decreases) in response to a certain match, their optimal donation when the match is uncertain will be monotonically increasing (decreasing) in their perceived probability of being matched,* p. *That is, for each donor* i*, if* $g^1(\mu) > (=) < g^0(\mu)$*, then* $\frac{\partial g^*(\mu)}{\partial p} > (=) < 0$*, where* $\frac{\partial g^*(\mu)}{\partial p} = \frac{\partial g^*}{\partial p}|_\mu$ *is the partial evaluated at the match rate* μ.

Proof. Proposition 1 follows directly from applying Lemma 2 to the expression for $\frac{\partial g^*}{\partial p}$ given in equation (2.12), noting that $|H(g^*)| < 0$.

This result leads to the following testable hypothesis:

Hypothesis 1: *Holding all else equal, each donor's donations will respond monotonically to exogenous changes in the probability of being matched.*

Note that Hypothesis 1 uses *exogenous* variation in the perceived probability of being matched. That is, the change in p cannot be the result of a change in any of the characteristics of the fundraiser (e.g., μ, ϕ, N, etc.). Such changes in p are possible in a laboratory setting—where the experimenter can directly vary p while holding all else constant—allowing for Hypothesis 1 to be directly tested. Experimental results consistent with Hypothesis 1 will demonstrate that donors' decisions are influenced by the probability of being matched.

2.4 Experiment Design and Procedures

From January to March of 2024, I used the Prolific platform to recruit 150 subjects to participate in an online experiment. After choosing to participate in my study on Prolific, subjects are provided with a link to a Qualtrics survey. After completing the Qualtrics survey and verifying their submission in Prolific, participants are paid a $4 base payment through Prolific. In addition to the base payment, subjects are also paid bonus payments ranging from $0-$9, depending on their decisions during the experiment.

Upon opening the Qualtrics survey, subjects are first taken to a consent page. After consenting to participate in the study, they are taken to the instructions page. After reading through the instructions, subjects are then taken to the main task of the experiment. Following the main task, subjects are then asked to complete follow-up tasks to measure their risk preferences, and then are asked to complete some basic survey questions before submitting their survey.

Section 2.4.1 discusses the main task of the experiment. Section 2.4.2 discusses the follow-up tasks. Section 2.4.3 discusses the survey questions included in the experiment. Section 2.4.4 outlines the procedures used in the experiment. A copy of the full text of the experiment is provided in Appendix B.4.

2.4.1 Main Task

In the main task of the experiment, subjects are presented with a menu of 13 decision problems. In each problem, they are provided with an endowment of 80 Tokens (10 Tokens = $1) and asked how much of their endowment they would like to pass to a real charity.[10] Among the 13 decision problems, four different match rates are offered: $\mu \in \{0, 0.5, 1, 2\}$. For each nonzero match rate, the probability of being matched is exogenously varied using four different match probabilities: $p \in \{.25, .5, .75, 1\}$. Table 2.1 lists the parameters used for each problem in the experiment.

All 13 decision problems are presented simultaneously within a single table. Each row presents a question, and there are five separate columns. The first column contains the

[10] The charity used is charity:water. Subjects are provided with a description of the charity in the experiment instructions. A copy of the full instructions presented to subjects can be found in the appendix to this chapter.

Table 2.1: Experiment match rates and match probabilities used.

Question	endowment (Tokens)	match rate (μ)	match probability (p)
1	80	0	0
2	80	$1/2$	.25
3	80	$1/2$	.5
4	80	$1/2$	.75
5	80	$1/2$	1
6	80	1	.25
7	80	1	.5
8	80	1	.75
9	80	1	1
10	80	2	.25
11	80	2	.5
12	80	2	.75
13	80	2	1

description for each problem, which informs the subject of their endowment, the match rate, and the probability of receiving a match. With the exception of the one problem that provides no match, the description for each problem has the following format: "*You are endowed with 80 Tokens. There is a* {25%, 50%, 75%, 100%} *chance that your donation will be matched at a* {0.5:1, 1:1, 2:1} *rate by the experimenter.*" The description for the problem with no match reads "*You are endowed with 80 Tokens. There is a* 0% *chance that your donation will be matched by the experimenter.*

The second column of each problem provides a text-entry box in which subjects are prompted to enter the number of Tokens they would like to pass to the charity. After a subject enters a value in column 2 of a given problem, the remaining columns (3-5) automatically populate to provide the subject with the following information: column 3 reports the number of Tokens the subject will hold for themselves (their endowment minus the amount they pass), column 4 reports the total donation that will be received by the charity if no match is received, and column 5 reports the total donation that will be received by the charity if a match is received.[11] These values are reported to subjects to reduce confusion and prevent

[11]For questions that provide a 100% chance of receiving a match, column 4 displays "N/A" regardless of

calculation errors, and subjects are free to edit their responses as many times as they like before submitting them.

Once a subject has provided acceptable responses to all 13 questions, they are able to submit their decisions. If they fail to answer any of the questions, or if they provide an unacceptable response to any of the questions (e.g., they attempt to pass a negative amount or an amount greater than their endowment), they receive an error message informing them of the specific issue and are unable to continue until providing an acceptable response. To maintain incentive compatibility, only one problem is randomly selected for payment (Azrieli et al., 2018).

To account for any order effects induced by the order in which the 13 questions are presented, subjects are randomly assigned one of four different orders. The orders used are shown in table 2.2. The first order exactly matches what is presented in table 2.1, with questions ordered first by ascending match rates and then by ascending match probabilities. The remaining orders vary by presenting match rates in ascending order and/or match probabilities in descending order, as well as placing the no-match question at either the beginning or end of the list.

2.4.2 Follow-up Tasks

After completing the main task, subjects are then asked to complete two follow-up tasks to measure their risk preferences: a *Payment Follow-up Task* and a *Donation Follow-up Task*. The order in which the tasks are presented to subjects is randomized across subjects.

Both tasks present subjects with a list of 11 choices, where they must choose either Option A or Option B. In the *Payment Follow-up Task*, Option A always provides the subject with "*A 50% chance of receiving $1, and $0 otherwise*". Option B provides a guaranteed fixed payment, increasing in $.10 increments, from $0 in the first question to $1 in the

the amount the subject chooses to pass, helping to make it clear to the subject that the match is certain. Likewise, for the question that provides a 0% chance of receiving a match, column 5 displays "N/A" regardless of the amount the subject chooses to pass, helping to make it clear that there is no chance of receiving a match.

Table 2.2: Parameter values used for each question order.

	Order 1		Order 2		Order 3		Order 4	
Question	μ	p	μ	p	μ	p	μ	p
1	0	0	0	0	1/2	1	2	1
2	1/2	.25	2	.25	1/2	.75	2	.75
3	1/2	.5	2	.5	1/2	.5	2	.5
4	1/2	.75	2	.75	1/2	.25	2	.25
5	1/2	1	2	1	1	1	1	1
6	1	.25	1	.25	1	.75	1	.75
7	1	.5	1	.5	1	.5	1	.5
8	1	.75	1	.75	1	.25	1	.25
9	1	1	1	1	2	1	1/2	1
10	2	.25	1/2	.25	2	.75	1/2	.75
11	2	.5	1/2	.5	2	.5	1/2	.5
12	2	.75	1/2	.75	2	.25	1/2	.25
13	2	1	1/2	1	0	0	0	0

final question. The *Donation Follow-up Task* is identical, except that the options provide donations to the charity rather than payments to the subject. The presentation of the tasks is modeled after the presentation used by Exley (2016) for similar tasks.[12]

The first and last questions of each follow-up task provide an attention/understanding check. In the first question, subjects are choosing between a lottery that provides a \$1 payment (or donation) with 50% probability and a guaranteed payment (or donation) of \$0, so it is expected that all subjects should choose Option A (the lottery) in the first question. In the last question, Option B provides a guaranteed payment (or donation) of \$1, so it is expected that all subjects should prefer Option B in the final question.[13]

The *Payment Follow-up Task* and *Donation Follow-up Task* allow me to estimate subjects' risk preferences for payments and donations, respectively, based on when they choose to switch from Option A to Option B (assuming they switch exactly once). Subjects who are more risk averse will switch from Option A to Option B earlier, while those who are

[12]See figures A.3 and A.4 in Appendix A of Exley (2016). The lotteries and guaranteed payments used are different, but I have borrowed Exley's instructions.

[13]If a subject were for some reason to prefer that the charity receive nothing, it would be rational for them to begin by choosing Option B and then switch to choosing Option A.

more risk loving will switch later.

2.4.3 Survey Questions

After completing the follow-up tasks, subjects answer some basic demographic questions, including age, gender, estimated family income, political views (e.g., conservative, liberal, etc.), importance of religion in their life, and estimated charitable contributions within the last year. Subjects are also asked to rate—on a scale of 0 to 10—how familiar they are with charity:water, how well they understood the procedure used to determine their payment, how well they understood the procedure used to determine their total donation, and how confident they were during the experiment that their donation (and any matching donation provided by the experimenter, if applicable) would actually be donated to the charity on their behalf. Summary statistics for subjects' responses to the survey questions are provided in Table B.1.

2.4.4 Procedures

Once subjects finish answering the survey questions, their payment and total donation are reported to them, including whether their donation received a match. To determine each subject's payment, one question from the main task is randomly selected. Subjects receive the number of Tokens they chose to hold for themselves in the randomly selected problem. These Tokens are converted to US dollars at the rate 10 Tokens = \$1. One problem is then also randomly selected from the *Payment Follow-up Task*. If the subject chose the guaranteed payment (Option B) in the randomly selected problem, then that amount is added to their total payment. If they instead chose the lottery (Option A), the computer 'flips a coin' to determine the outcome of the lottery.[14]

Each subject's total donation is calculated through a similar process to that of their

[14] This is done by drawing a random number x from a uniform distribution over $(0, 1)$. If $x < 0.5$, then the subject receives a payment of \$1. Otherwise, the subject receives \$0.

payment. The same randomly selected decision problem that was selected to determine their payment in the main task is also used to determine their total donation. If problem j is the randomly selected problem and it provides a match at rate μ_j with probability p_j, the computer first 'flips a coin' to determine whether the match will be received.[15] If a match is determined to be received, then the subject receives a match at rate μ_j and their total donation from the main task is given by $(1+\mu_j)g_j$, where g_j is the amount they chose to pass in question j. If a match is not received, the subject's total donation from the main task is given by g_j. A random question is then also drawn from the *Donation Follow-up Task*, and the resulting additional donation (if any) is added to the subject's total donation.

Subjects' earnings are paid to them through Prolific using bonus payments within two business days after they complete the experiment.[16] This is in addition to the \$4 base payment they receive, which is automatically paid to them after they verify their submission in Prolific. The average total payment received by participants (including both the base payment and the bonus payment) is \$10.16. The median completion time among participants is 11 minutes and 32 seconds, and the average reward-per-hour received is \$20.81.

Subjects' total donations, including any matched funds received, were donated to the charity after the completion of the experiment, in a single lump-sum donation. On average, each subject donated \$4.93 to the charity. The total donation received by the charity as a result of the experiment is \$738.80.

2.5 Results

Table B.1 presents summary statistics for subjects. Section 2.5.1 presents evidence that the probability of receiving a match affects donors' decisions. Section 2.5.2 presents evidence that donations respond monotonically to changes in the match probability, consistent

[15]This is done by drawing a random number x from a uniform distribution over $(0, 1)$. If $x < p_j$, then the subject receives a match. Otherwise, if $x \geq p_j$, then a match is not received.

[16]In practice, subjects received their bonus payments within 1-2 hours of verifying their submission in Prolific. However, in the description posted to Prolific, and in the experiment instructions, subjects were provided with a guarantee that their bonus payment would be received within two business days.

with the theoretical model and Hypothesis 1. Section 2.5.3 compares the behavior of three different types of donors: those who increase their out-of-pocket donation in response to a match (*match lovers*), those who decrease their out-of-pocket donation in response to a match (*match haters*), and those who are unresponsive to match subsidies (*match ignorers*). This analysis provides further support for the theoretical model, finding that the data are largely consistent with Hypothesis 1 across donor types. Section 2.5.4 tests whether subjects' behavior is affected by the order in which the questions are presented to them.

2.5.1 Evidence that match probabilities matter

Figure 2.1 shows the average amount passed (i.e., the out-of-pocket donation) by subjects at each match rate and match probability.[17] Figure 2.1 demonstrates two key findings. First, on average out-of-pocket donations are increasing in the match rate. Second, holding the match rate constant, on average donations increase as the match probability increases. These results can also be seen in tables B.3 and B.5.

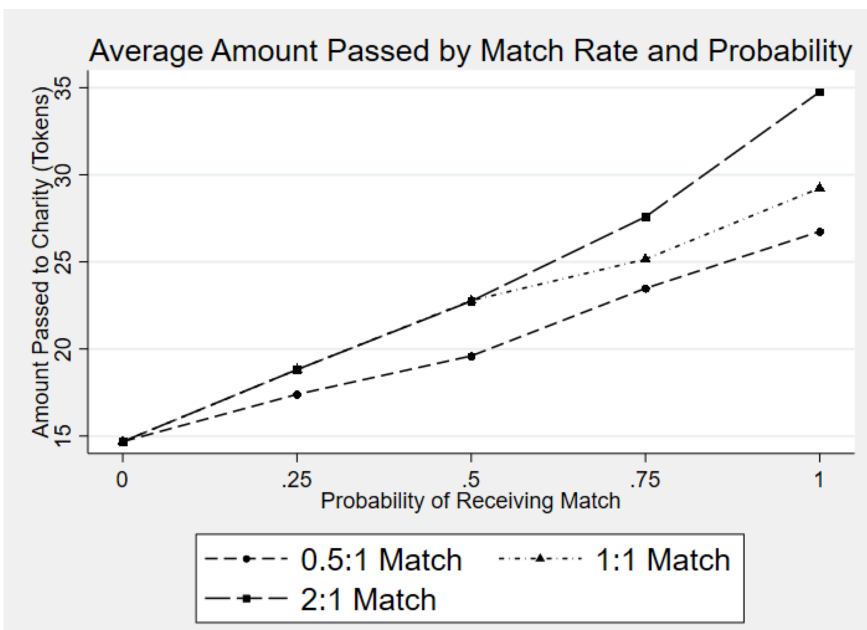


Figure 2.1: A comparison of the average amount passed (in Tokens) across all subjects.

[17] Figure B.1 shows the average total impact at each match rate and match probability (assuming a match is received).

Table 2.3 reports regression results for out-of-pocket donations, using a random-effects model and a Tobit model accounting for censoring. When excluding the interaction term between price and match probability, both models estimate statistically significant coefficients on both price and match probability. Subjects increase their out-of-pocket donations as the match rate increases (i.e., as the price decreases), and they also increase their out-of-pocket donations as the probability of receiving a match increases.

When the interaction term between price and match probability is included, the coefficient on price goes to 0. This is consistent with our theoretical predictions. The fact that the coefficient on price goes to 0 implies that subjects are unresponsive to price changes that occur when the probability of receiving a match is zero (i.e., subjects do not respond to matches that occur with probability zero). Instead, the coefficient on the interaction term is significant and negative, implying that subjects become increasingly more responsive to price decreases (increases in the match rate) as the probability of receiving the match increases.

Conversely, the negative coefficient on the interaction term also implies that subjects become increasingly more responsive to increases in the match probability as the match rate increases. In both models, the coefficient on the interaction term is the same magnitude as the coefficient on the match probability. This implies that, when no match subsidy is provided (i.e., price = 1), changes in the match probability have no effect on how much subjects choose to pass. This is again consistent with our theoretical model.

2.5.2 Evidence that donations respond monotonically to match probability

On average, subjects increase their out-of-pocket donations in response to an increase in the match rate. And, on average, subjects monotonically increase their donations as the match probability increases. Thus, on average, Hypothesis 1 holds.

However, Hypothesis 1 also predicts that, for any subjects who *decrease* their out-of-pocket donations in response to an increase in the match rate, their donations must monotonically *decrease* as the match probability increases. To check whether this holds in the

Table 2.3: Regressions for Amount Passed

logPass	(1)	(2)	(3)	(4)
logPrice	-0.418***	-0.060	-0.639***	-0.037
	(0.091)	(0.117)	(0.097)	(0.240)
Match probability	0.986***	0.566***	1.599***	0.898***
	(0.118)	(0.161)	(0.101)	(0.274)
Match(=1)	-0.084	0.179	-0.130	0.313
	(0.120)	(0.122)	(0.143)	(0.215)
PRICExPROB		-0.574***		-0.952***
		(0.181)		(0.347)
Constant	1.454***	1.454***	0.476**	0.475**
	(0.139)	(0.139)	(0.239)	(0.238)
Model	RE	RE	Tobit	Tobit
Observations	1950	1950	1950	1950

Standard errors in parentheses
Robust standard errors reported for (1) and (2)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

data, I segment subjects into one of three groups: (1) *match lovers*, who increase their donation in response to a certain match; (2) *match haters*, who decrease their donation in response to a certain match; and (3) *match ignorers*, whose behavior is unaffected by the certain match.

There are two important points regarding the classification of match lovers and haters. First, the classifications are made based on how much a subject passes when no match is received and how much they pass when a match is received *with certainty.* Second, consistent with the theoretical model, this classification is made at the subject-match rate level. For example, the same subject may be classified as a *match lover* for a 0.5:1 match and as a *match hater* for a 2:1 match. The theory allows for subjects to respond positively to some match rates and negatively to others. All that is required is that donations respond monotonically as the match probability varies.

To test the extent to which subjects' behavior is consistent with Hypothesis 1, I classify all subject-match rate pairs and check for monotonicity. For example, if a subject is a *match*

Table 2.4: Number of subjects whose decisions are consistent with the predictions of Hypothesis 1.

		0.5:1 Match		1:1 Match		2:1 Match		All Match Rates	
		Count	Percent	Count	Percent	Count	Percent	Count	Percent
Lovers	Non-mon	15	19.23	10	11.36	9	9.38	34	12.98
	Monotonic	63	80.77	78	88.64	87	90.63	228	87.02
	Total	78		88		96		262	
Ignorers	Non-mon	12	18.46	8	14.81	10	21.74	30	18.18
	Monotonic	53	81.54	46	85.19	36	78.26	135	81.82
	Total	65		54		46		165	
Haters	Non-mon	1	14.29	2	25.00	1	12.50	4	17.39
	Monotonic	6	85.71	6	75.00	7	87.50	19	82.61
	Total	7		8		8		23	
All Types	Non-mon	28	18.67	20	13.33	20	13.33	68	15.11
	Monotonic	122	81.33	130	86.67	130	86.67	382	84.89
	Total	150		150		150		450	

lover for a 2:1 match ($\mu = 2$), then it must be the case that $g_i(\mu = 2, p = 0) \leq g_i(\mu = 2, p = .25) \leq g_i(\mu = 2, p = .5) \leq g_i(\mu = 2, p = .75) \leq g_i(\mu = 2, p = 1)$. For a *match hater*, all of the relations are reversed. For *match ignorers*, all of the relations hold with equality (i.e., a *match ignorer* should pass the same amount at all match probabilities).

Table 2.4 shows the number of subjects whose out-of-pocket donations respond monotonically to changes in the match probability, for each donor type (match *lovers*, *ignorers*, and *haters*) and match rate. Overall, subjects' behavior is consistent with Hypothesis 1 nearly 85% of the time. The majority of subjects (58%) are *match lovers* across all match rates, and the proportion of *match lovers* increases as the match rate increases (52%, 58.7%, and 64% of subjects are *match lovers* at the 0.5:1, 1:1, and 2:1 match rates, respectively). Furthermore, subjects' behavior also becomes increasingly consistent with Hypothesis 1 as the match rate increases: about 81% of *match lovers* display monotonicity at the 0.5:1 match rate, close to 89% at the 1:1 match rate, and nearly 91% at the 2:1 match rate.

The second most common type is *match ignorers*, who account for 37% of all subject-match rate pairs. In contrast with the number of *match lovers*, the number of *match ignorers* decreases as the match rate increases: from 43% for the 0.5:1 match, to 36% for the 1:1 match, and 31% for the 2:1 match. As the match rate increases, subjects appear to shift from being *match ignorers* to *match lovers*, perhaps because the match becomes more salient. Overall, the behavior of *match ignorers* is consistent with Hypothesis 1 about 82% of the time.

Few subjects are categorized as *match haters*. Across all match rates, *match haters* only account for 5% of subjects. That being said, among those observed to be *match haters*, nearly 83% display monotonicity in their decisions. Thus, the behavior of *match haters* is largely consistent with Hypothesis 1.

Overall, behavior is largely consistent with Hypothesis 1, with 85% of decisions displaying monotonicity in the match probability. Furthermore, behavior is consistently monotonic across all three types of subjects, with monotonicity observed more than 80% of the time for all three types: *match lovers*, *match ignorers*, and *match haters*.

2.5.3 Comparing match lovers, haters, and ignorers

Figure 2.2 shows the average out-of-pocket donations made by each of the different types of donors. Among subjects who behave consistently with monotonicity (top row), *match haters* are significantly more generous than the other types of donors when no match is provided. As the match probability increases, they provide smaller donations. Given their initial generosity, it appears they are very concerned with how much the charity receives: as the match probability increases, the expected amount received by the charity increases. This behavior suggests that altruistic motives may weigh more heavily in the utility functions of *match haters*.

Table 2.5 displays the average amount passed by donors of each type, making no distinction between those who behave consistently with Hypothesis 1 and those who behave inconsistently with it. In contrast with *match haters*, *match lovers* give the least amount among all types when no match is provided. However, this group is highly responsive to changes in the match probability, giving about the same amount as *match haters* when the match is received with certainty. (*Match lovers* give less than *match haters* when there is a

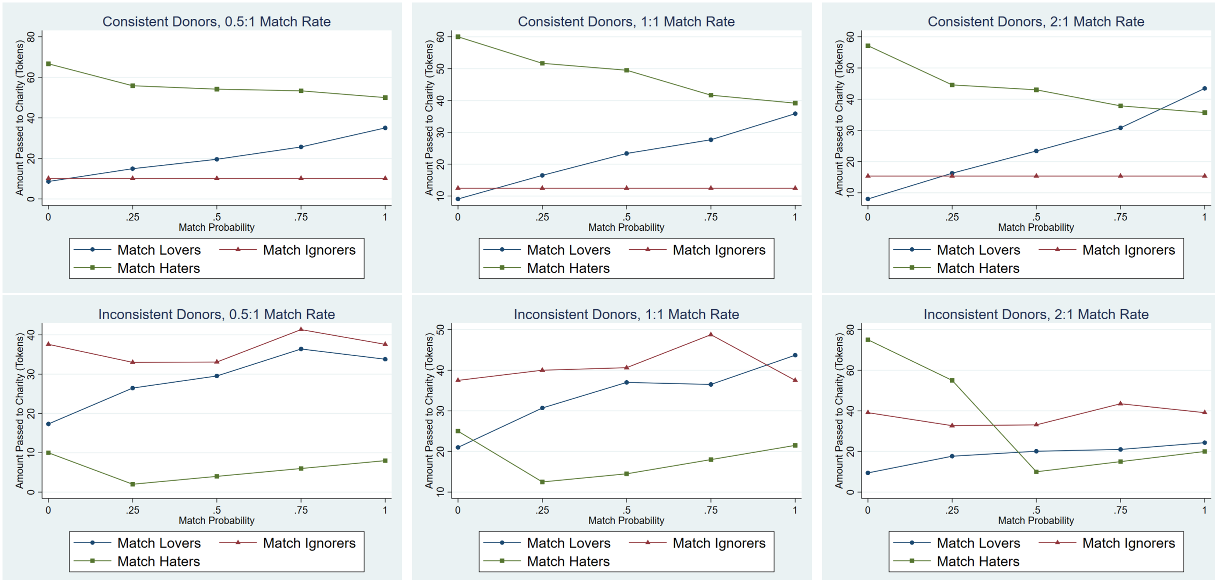


Figure 2.2: A comparison of the average amount passed (in Tokens) by type and match rate. Top row shows the averages among subjects who behave consistently with Hypothesis 1 (monotonicity); bottom row shows the averages among subjects who violate monotonicity.

0.5:1 certain match, about the same when there is a 1:1 certain match, and more when there is a 2:1 certain match. This reflects the fact that *match lovers* give more as the match rate increases, while *match haters* give less as the match rate increases.)

Match ignorers give more than *match lovers* when no match is provided, but because they are unresponsive to changes in the match rate, they give substantially less than *match lovers* as the match rate increases. On average, *match ignorers* give the least amount of the three types. Their behavior is consistent with pure warm-glow preferences where no warm glow is felt from matched funds (Higgs and Uler, 2023).

2.5.4 Checking whether question order matters

Figure 2.3 shows the average out-of-pocket donations of subjects assigned to each of the four different question orders, plotted at each match rate. A gap appears to grow between Orders 1 and 2 and Orders 3 and 4 as the match rate increases. Orders 1 and 2 display the questions in order of increasing match probabilities, whereas Orders 3 and 4 display them in order of decreasing match probabilities (see Table 2.2). Orders 1 and 2 also display the

Table 2.5: Average amount passed (in Tokens) by each donor type.

		Match Probability				
		$p=0$	$p=.25$	$p=.5$	$p=.75$	$p=1$
0.5:1 Match	*Lovers* ($n=78$)	10.19	17.15	21.49	27.72	34.81
	Ignorers ($n=65$)	15.22	14.37	14.38	15.91	15.22
	Haters ($n=7$)	58.57	48.14	47.00	46.57	44.00
1:1 Match	*Lovers* ($n=88$)	10.44	18.11	24.92	28.68	36.75
	Ignorers ($n=54$)	16.17	16.54	16.63	17.83	16.17
	Haters ($n=8$)	51.25	41.88	40.75	35.75	34.75
2:1 Match	*Lovers* ($n=96$)	8.16	16.41	23.09	29.90	41.67
	Ignorers ($n=46$)	20.52	19.13	19.22	21.48	20.52
	Haters ($n=8$)	59.38	45.88	38.88	35.00	33.75

no-match question first, while it is displayed last in Orders 3 and 4. A preliminary look at Figure 2.3 suggests these differences may affect subjects' behavior, at least at higher match rates. Figure B.2, which shows average out-of-pocket donations broken down by match rate and match probabilities, shows that the gap between Orders 1 and 2 and Orders 3 and 4 is most prevalent among the 2:1 match-rate questions, growing in size as the match probability increases. This further suggests that subjects' behavior may be influenced by the order in which match probabilities are presented. However, upon closer analysis, there are no statistically significant differences between question orders.

Table B.12 shows the average out-of-pocket donations among subjects assigned to each question order, broken down by each match rate and match probability.[18] The last column reports p-values from one-way ANOVA tests of equality of means. The null hypothesis of equal means across all question orders cannot be rejected for any of the questions, including the 2:1 match questions that appear to diverge in Figure 2.3.[19]

Table B.11 reports the results of regressions including interaction terms for each of the

[18]A more detailed analysis of the average out-of-pocket donations in each question for each of the question orders is provided in Tables B.7, B.8, B.9, and B.10.

[19]Because sample sizes are not equal across question orders, it is possible the reported p-values from one-way ANOVA tests are smaller than their true values, increasing the likelihood of rejecting the null hypothesis (Miller Jr, 1997). Given that we still fail to reject the null hypothesis for each question, this strengthens support for our claim that subjects' behavior is not significantly affected by question order.

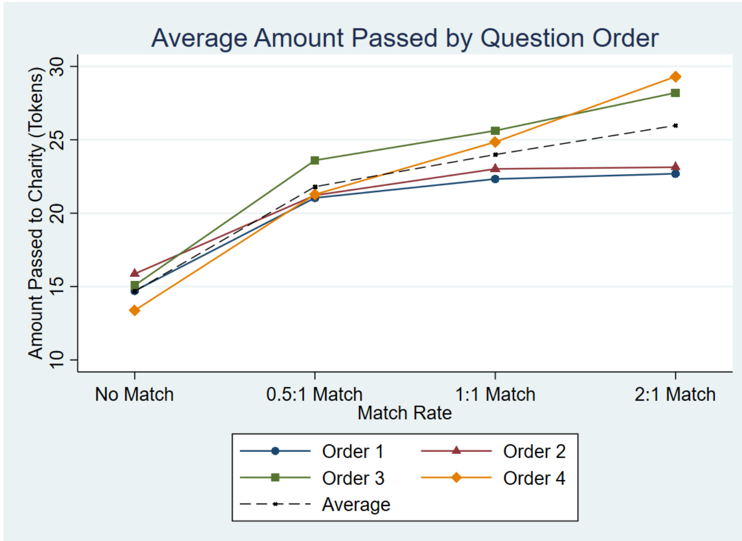


Figure 2.3: A comparison of the average amount passed (in Tokens) across all subjects for each question order.

question orders. With the exception of model 3, Wald tests of the equality of coefficients—for *price*, *match probability*, and the interaction term between *price* and *match probability* (for models 2 and 4)—across question orders fail to reject the null hypothesis of equality. In model 3, the coefficients on *price* are found to be statistically different across question orders (p=.000). This difference is driven by question order 4, for which the estimated coefficient on price is significantly more negative. However, when interaction terms between *price* and *match probability* are included (model 4), the difference between question orders disappears. No statistically significant differences are observed between question orders for the coefficient estimates on *Price*, *Match Probability*, or the interaction term *PRICExPROB*.

Although there is some qualitative evidence that the order in which match probabilities are presented may influence subjects' donation decisions, these differences are not statistically significant. Furthermore, regardless of any potential question-order effects, Figures 2.3 and B.2 show that the main features of subjects' behavior are consistent across all question orders: first, on average, subjects increase their out-of-pocket donations as the match rate increases; and second, on average, subjects increase their out-of-pocket donations as the probability of receiving a match increases. Across all question orders, subjects are responsive to both the match rate and the match probability.

2.6 Conclusion

Match subsidies are a common strategy used by charitable organizations to boost donations. However, despite their popularity, there is no consensus on what the optimal match rate is or, if it differs across settings, how a fundraiser should go about determining it. Existing studies show mixed results on the effectiveness of different match rates, providing contradictory estimates that suggest that match subsidies can have varying impacts on donor behavior.

This research proposes an intuitive model of charitable giving in which donors consider matches to be uncertain, forming beliefs about the expected donations of others and using these beliefs, together with the match limit, to derive their perception of the probability of receiving a match. This model is consistent with previous research, is capable of providing an explanation for the seemingly contradictory match-price elasticity estimates found in previous work, and produces meaningful insights into the interactions between the match rate and the match limit.

The model is based on a simple mechanism: if the total donations of the other donors exceed the match rate, the donor does not consider their donation matched. To formalize this mechanism, a theoretical model was developed, assuming donors are impure-impact givers. Donors form probability distributions about the total giving of others, influenced by fundraiser characteristics, the match rate, and the match limit. The model reveals that increasing the match rate, while holding the lead gift size constant, creates counteracting effects: it increases the potential impact of a gift if a match is received but decreases the probability of receiving a match. Because of this, the effect of changing the match rate is ambiguous, depending on the donor's perceptions about the total giving of others.

Formal theoretical results were derived from the model, and the theoretical predictions were tested through a real-incentive online laboratory experiment. The experiment introduced exogenous variation in the probability of receiving a match in an effort to establish that donors are responsive to changes in the match probability. The results of the experiment confidently showed that donors are indeed responsive to changes in the probability of receiving a match. Larger match rates induce larger average donations, but this effect diminishes as the match probability decreases. Furthermore, donor behavior is observed to be consistent with the predictions of Hypothesis 1 more than 85% of the time. And this consistency holds among all three types of donors: *match lovers*, *match haters*, and *match ignorers*. These findings provide strong support for the theoretical model and demonstrate the importance of considering donors' perceived match probabilities.

Overall, this research advances our understanding of donor behavior and provides charitable organizations with valuable guidance for designing more effective fundraisers. By considering donors' perceived probabilities of receiving a match, charities can optimize their use of lead gifts to maximize donations, thereby enhancing the efficiency and impact of their fundraising efforts.

Chapter 3: How Fundraiser Characteristics Affect the Response to Match Subsidies

3.1 Introduction

In the previous chapter, we developed a model of charitable giving in which donors perceive match subsidies as uncertain, forming beliefs about the total giving of others in order to derive their perceived probability of receiving a match. The model allows for the characteristics of the fundraiser to affect donors' beliefs about the likely distribution of total donations by others. However, in the previous chapter we simplified the analysis by considering exogenous changes in the match probability. This allowed us to derive testable predictions and, based on these predictions, we designed an online laboratory experiment to validate the model.

The results of the experiment closely aligned with our theoretical predictions, providing strong support for the model. In particular, we convincingly demonstrated that donors are responsive to changes in the match probability. Furthermore, they respond to changes in the match probability in predictable ways, consistent with our theoretical model.

In this chapter, we seek to extend our results. We return to the development of the theoretical model, focusing now on endogenous changes in the match probability resulting from changes in fundraiser characteristics. We specifically consider the effects of changes in the number of potential donors and the wealth levels of potential donors, along with thoroughly exploring the predicted effects associated with changes in the match rate and the match limit. With our theoretical predictions in hand, we turn to designing an online laboratory experiment to validate the model.

The experiment is similar to the one used in the previous chapter, except that rather than providing exogenous match probabilities, each problem now provides a match up to a given match limit. Donors are placed in groups, and their donation in a given problem receives a match only if the total donations of their group members do not exceed the stated match limit. In this way, the matches are made uncertain, reflecting the design of real-world fundraisers. Similar to the experiment in the previous chapter, in this experiment subjects are presented with various prices (i.e., match rates) along with various match limits meant to create meaningful variation in their perceived match probabilities.

Unlike the previous experiment, in this experiment subjects complete a total of four stages. Each stage presents subjects with the same list of questions, with the same match rates and match limits. However, as stage provides a different combination of group size (i.e., the number of other participants each subject is grouped with) and endowments. The variation across stages is intended to produce meaningful variation in subjects' perceived match probabilities by affecting their beliefs about the likely total giving of their group. For example, as the group size increases (from N=5 to N=10), the match rates in each question remain the same, so subjects' perceived match probabilities should likely decrease, since it would be reasonable to expect a group of 10 donors to be more likely to exceed the match limit than a group of only 5 donors. Similar arguments hold for changes in the endowments.

The results of the experiment strongly support the hypothesis that donors are responsive to endogenous changes in the match probability. Furthermore, observed behavior is fairly consistent with theoretical predictions. Estimated match-price elasticities are in line with previous estimates reported in the literature. However, the match price also shows strong correlations with the match limit and group size. That is, matches are found to be significantly more effective at increasing donations when the match limit is larger and the group size is smaller. This finding is consistent with the theoretical model. As the match limit increase and the group size decreases, the (perceived) probability of receiving a match should increase.

These findings extend our results from the previous chapter to the case of endogenous changes in match probabilities. The theoretical model developed provides valuable new insights that advance our understanding of why people give, and the results of the experiment provide support for the model's validity.

The remainder of this paper is organized as follows. Section 3.2 develops the theoretical model, presents the assumptions needed, and derives theoretical results. Section 3.3 presents the experimental design and outlines the experimental procedures. Section 3.4 presents results, and Section 3.5 concludes. Proofs of theoretical results are provided in Appendix C.1. Appendices C.2, C.3, and C.4 present additional tables, figures, and materials from the experiment, respectively.

3.2 Theory

In the previous chapter, I developed a theoretical model of giving where donors perceive matches as uncertain. A donor's eligibility for a match hinges on the match limit and the aggregate donations from others. The uncertainty arises because donors are unaware of the total contributions from others when making their donation decisions. Consequently, each donor treats the total giving of others as a random variable and constructs a probability distribution for its value. The probability density function (PDF) of the total giving of others depends on fundraiser characteristics, such as the size of the fundraiser (i.e., the number of potential donors) and the average wealth of potential donors. In this way, the characteristics of a fundraiser can vary a donor's perceived probability of receiving a match, thereby affecting the donor's response to an advertised match offer.

In this chapter, I continue to use the model developed in the previous chapter. However, whereas in the previous chapter I looked only at exogenous changes in a donor's perceived probability of receiving a match, here I extend the analysis to encompass endogenous changes resulting from changes in the characteristics of a fundraiser. Specifically, I consider how changes in the number of potential donors and the average wealth of potential donors affect the response to changes in the match rate.

In what follows, I reintroduce the model and restate all definitions, assumptions, etc. that are essential to the analysis in this chapter. I then turn to deriving theoretical predictions regarding the response to changes in fundraiser characteristics. Based on the theoretical predictions, testable hypotheses are developed.

3.2.1 Basic Setup

For a given charity, there are a total of N potential donors, where N is exogenously determined. Each potential donor $i = 1, ..., N$ has utility given by the impure impact model of Hungerman and Ottoni-Wilhelm (2021). That is, each potential donor's utility is given by $U_i(w_i - g_i, g_i, R_i + \lambda R_{-i})$. I assume that U_i is strictly increasing and concave in each of its arguments for all i.

Each donor i is endowed with exogenous wealth, $w_i \in \mathbf{R}_+$, from which they choose an amount, $g_i \in [0, w_i]$, to donate to the charity. Let $G = \sum_{i=1}^{N} g_i$ be the total amount passed to the charity by all individuals $i = 1, ..., N$, and let $G_{-i} = \sum_{j \neq i} g_j$ be the total amount of donations passed to the charity by all individuals other than i. Donations to the charity are matched by a third party donor at some match rate, $\mu \in \mathbf{R}_+$, up to the point that the exogenously set lead gift, $\phi \in \mathbf{R}_+$, has been fully depleted. That is, as long as $\mu\,(g_i + G_{-i}) \leq \phi$ (that is, total donations do not exceed the match limit), individual i's donation, g_i, is matched at rate μ, so that the total donation received by the charity (i.e., donor i's *impact*) is $R_i = (1 + \mu)g_i$, consisting of i's donation of g_i and the third party's matching donation of μg_i. If $\mu G_{-i} < \phi < \mu(g_i + G_{-i})$, then i's donation is *partially* matched. In this case, donor i's impact is given by $R_i = g_i + \phi - \mu G_{-i}$. Finally, if $\mu G_{-i} \geq \phi$ (other's total donations exceed the match limit), i's donation, g_i, is not matched, and donor i's impact is given by $R_i = g_i$.

Note that the lead gift amount, ϕ, is different from the match limit, $\frac{\phi}{\mu}$. The lead gift is the total amount of funds available for matching donations, while the match limit is the total amount of donations that can be matched before the lead gift is fully exhausted. Therefore, holding the size of the lead gift, ϕ, constant, increasing the match rate, μ, will mechanically decrease the match limit, $\frac{\phi}{\mu}$. As the match rate is increased, fewer donations are required to fully exhaust the lead gift.[1]

To simplify the analysis, I ignore the possibility of donor i receiving a partial match. Instead, I will assume that donor i considers a partial match to be equivalent to a full match. This is formalized by the following assumption.

Assumption 1 *Donors consider their impact when receiving a partial match to be equivalent to their impact when receiving a full match. For all $i = 1, ..., N$, we have the following:*

$$R_i = \begin{cases} (1+\mu)g_i & \text{for } 0 \leq G_{-i} < \frac{\phi}{\mu} \\ g_i & \text{for } \frac{\phi}{\mu} \leq G_{-i} \leq W_{-i} \end{cases} \tag{3.1}$$

where $W_{-i} = \sum_{j \neq i} w_j$.

Under this assumption, donors view the match as binary: they either receive a full match or no match, depending only on whether or not the total giving of others fully exhausts the match limit. This is equivalent to donors simply ignoring the possibility of receiving a partial match. In practice, for large fundraisers with many donors, the probability of receiving a partial match will be negligible and likely would be ignored by the vast majority of donors.[2]

As long as the total giving of others is less than the match limit (i.e., $G_{-i} < \frac{\phi}{\mu}$), donor i

[1] For example, consider a lead gift of \$1,000. If a 1:1 match is provided ($\mu = 1$), it will require \$1,000 in donations to fully exhaust the lead gift. However, if the match rate is increased to a 2:1 match ($\mu = 2$), then it will only take \$500 in donations to fully exhaust the lead gift.

[2] In order for a donor to perceive a significant probability of receiving a partial match, they would either need to have a high level of confidence that G_{-i} is very close to (but just below) the match limit, $\frac{\phi}{\mu}$, or they would need to be making a very large donation, g_i, relative to their beliefs about the total giving of others, G_{-i}.

will consider their impact to be $R_i = (1+\mu)g_i$. Therefore, donor i's utility is given by:

$$U_i\big(x_i, g_i, R\big) = \begin{cases} U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big) & \text{for } 0 \le G_{-i} < \frac{\phi}{\mu} \\ U_i\big(w_i - g_i,\ g_i,\ g_i + \lambda R_{-i}\big) & \text{for } \frac{\phi}{\mu} \le G_{-i} \le W_{-i} \end{cases} \tag{3.2}$$

The parameters μ, ϕ, N, $\mathbf{w}$, λ, and R_{-i} are all exogenous. The total giving of others, G_{-i}, is assumed to be exogenous to g_i, but may be a function of the parameters. Together, the above two expressions give donor i's utility for any choice of $g_i \in [0, w_i]$ and any realization of the total giving of others, G_{-i}.

While (3.2) gives donor i's utility for any choice of $g_i \in [0, w_i]$ and total giving of others, G_{-i}, they are unable to optimize over (3.2) because the total giving of others, G_{-i}, is not known with certainty to them. Instead, I assume donors take G_{-i} to be a realization of a random variable $\tilde{G}_{-i}$, and they form beliefs about the distribution of $\tilde{G}_{-i}$ based on observable characteristics of the fundraiser. This is formalized by the following definition.

Definition 1 $F_i\big(G_{-i}|N, \mu, \phi, \mathbf{w}_{-i}\big) = P_i\left(\tilde{G}_{-i} \le G_{-i}|N, \mu, \phi, \mathbf{w}_{-i}\right)$ is the CDF of donor i's perceived distribution of G_{-i}, given N, μ, ϕ, and $\mathbf{w}_{-i}$, where $\mathbf{w}_{-i}$ is a vector consisting of the wealth level of every donor among the fundraiser's population of potential donors other than donor i, and $\tilde{G}_{-i}$ is a real-valued random variable.

That is, based on the values of N, μ, ϕ, and $\mathbf{w}_{-i}$, each donor forms a distribution of the likely values of G_{-i}.[3] Assuming donors are expected utility maximizers and $F_i\big(G_{-i}|N, \mu, \phi, \mathbf{w}_{-i}\big)$

[3] I assume the parameter values are known by all donors, choosing to instead introduce uncertainty through each donor's construction of their perceived distribution of G_{-i}. However, since each donor forms their own beliefs about the distribution of G_{-i}, the model would be equivalent if donors were to instead each hold unique beliefs about the values of the parameters.

is twice continuously differentiable for all i, donor i will choose $g_i \in [0, w_i]$ to maximize

$$EU_i(g_i|\cdot) = F_i\left(\frac{\phi}{\mu}\middle|N, \mu, \phi, \mathbf{w}_{-i}\right) U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big)$$
$$+ \left[1 - F_i\left(\frac{\phi}{\mu}\middle|N, \mu, \phi, \mathbf{w}_{-i}\right)\right] U_i\big(w_i - g_i,\ g_i,\ g_i + \lambda R_{-i}\big), \quad (3.3)$$

where $F_i(\frac{\phi}{\mu}|\cdot)$ is the donor's perceived probability of being matched, and $[1 - F_i(\frac{\phi}{\mu}|\cdot)]$ is their perceived probability of not being matched. Also note that $U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big)$ is donor i's utility given a certain match (i.e., $F_i(\frac{\phi}{\mu}|\cdot) = 1$ and $\mu > 0$), and $U_i\big(w_i - g_i,\ g_i,\ g_i + \lambda R_{-i}\big)$ is their utility when there is no match (i.e., $F_i(\frac{\phi}{\mu}|\cdot) = 0$ or $\mu = 0$). Thus, EU_i is simply the weighted average of the potential outcomes, match and no match.

The following definitions will be used to simplify the notation in the proceeding analysis:

Definition 2 Donor i's utility when *not* receiving a match is given by

$$U_i^0 \equiv U_i\big(w_i - g_i,\ g_i,\ g_i + \lambda R_{-i}\big). \quad (3.4)$$

Definition 3 Donor i's utility when receiving a match at rate μ is given by

$$U_i^1 \equiv U_i\big(w_i - g_i,\ g_i,\ (1+\mu)g_i + \lambda R_{-i}\big), \quad (3.5)$$

Definition 4 Donor i's *perceived probability of receiving a match* is given by

$$p_i \equiv F_i\left(\frac{\phi}{\mu}\middle|N, \mu, \phi, \mathbf{w}_{-i}\right). \quad (3.6)$$

Using these definitions, donor i's optimization problem (3.3) can be rewritten as choosing

$g_i \in [0, w_i]$ to maximize

$$EU_i(g_i|\cdot) = p_i U_i^1 + (1 - p_i) U_i^0 \tag{3.7}$$

To guarantee that any local maximum is also a global maximum, I also make the following assumption.

Assumption 2 *Let donor i's optimization problem in the absence of a match (i.e., $F(\cdot) = 0$) be given by $\max_{g_i \in [0, w_i]} U_i^0(g_i|\cdot)$, and let their optimization problem in the presence of a certain match (i.e., $F(\cdot) = 1$) be given by $\max_{g_i \in [0, w_i]} U_i^1(g_i|\cdot)$. Then for all $i = 1, ..., N$, we have the following:*

$$(i)\ \ U_{gg}^0 < 0\ \forall g \in [0, w_i] \tag{3.8}$$

$$(ii)\ \ U_{gg}^1 < 0\ \forall g \in [0, w_i] \tag{3.9}$$

3.2.2 Model Predictions

The Effect of a Change in the Perceived Match Probability

In this section I demonstrate that changes in the perceived probability of being matched (henceforth referred to as p) affect donors' optimal donation amounts. That is, donors should in general be responsive to changes in p. In what follows, I assume interior solutions and drop the i subscripts for brevity. Taking the derivative of (3.3) with respect to g, the first order condition is given by

$$F\left(\frac{\phi}{\mu}\middle|\cdot\right) \cdot \left(-\frac{\partial U^1}{\partial x} + \frac{\partial U^1}{\partial g} + (1+\mu)\frac{\partial U^1}{\partial R}\right) + \left[1 - F\left(\frac{\phi}{\mu}\middle|\cdot\right)\right] \cdot \left(-\frac{\partial U^0}{\partial x} + \frac{\partial U^0}{\partial g} + \frac{\partial U^0}{\partial R}\right) = 0 \tag{3.10}$$

where $U^1 = U\big(w-g,\ g,\ (1+\mu)g+\lambda R_{-i}\big)$ is the donor's utility when there is a match, and $U^0 = U\big(w-g,\ g,\ g+\lambda R_{-i}\big)$ is the donor's utility in the absence of a match. Letting $FOC^1(g)$ denote the donor's first order condition given a certain match (i.e. $p=1$), and letting $FOC^0(g)$ denote the donor's first order condition in the absence of a match, the above first order condition can be rewritten as

$$pFOC^1(g) + (1-p)FOC^0(g) = 0. \tag{3.11}$$

That is, the donor's first order condition given an uncertain match is simply the weighted average of their FOC's for the two possible outcomes (match and no match). This leads to the following result:

Lemma 1: *For each donor i, let $g^0(\mu) \in (0,w)$ be the optimal choice of g in the absence of a match, so that $g^0(\mu)$ maximizes $U^0 = U(w-g,\ g,\ g+\lambda R_{-i})$.*[4] *Likewise, let $g^1(\mu) \in (0,w)$ be the optimal choice of g given a certain match at rate μ, so that $g^1(\mu)$ maximizes $U^1 = U(w-g,\ g,\ (1+\mu)g+\lambda R_{-i})$. Finally, let $g^*(\mu)$ be the optimal choice of g given an uncertain match at rate μ, so that $g^*(\mu)$ maximizes $EU = pU^1 + (1-p)U^0$, where $p \in (0,1)$. Then $g^*(\mu) \in (g^0(\mu),\ g^1(\mu))$ for all μ such that $g^0(\mu) \neq g^1(\mu)$, and $g^*(\mu) = g^0(\mu) = g^1(\mu)$ for all μ such that $g^0(\mu) = g^1(\mu)$.*

Proof of Lemma 1 is provided in appendix C.1. Lemma 1 states that, for any match rate μ, each donor's optimal choice of g when the match is uncertain (g^*) must fall between their optimal choice of g when no match is received (g^0) and their optimal choice of g when the match is received with certainty (g^1). This provides us with our first testable hypothesis:

Hypothesis 1: *For a given match rate μ, the amount a donor chooses to donate will always fall between the amount they pass when there is no match and the amount they pass*

[4] Note that since μ does not enter U^0, $g^0(\mu)$ is constant in μ. That is, $g^0(\mu) = g^0$ for all $\mu \in \mathbf{R}_+$.

when the match is certain. That is, it must always be the case that $g^*(\mu) \in [g^0(\mu),\ g^1(\mu)]$.

Note that Hypothesis 1 holds irrespective of the characteristics of the fundraiser (with the exception of μ, which must remain constant). This is because the other parameters only enter the donor's utility function through their effect on the donor's perceived probability of receiving a match.[5] Thus, without knowing or making any assumptions regarding how a donor's perceived probability of receiving a match varies in response to a change in the characteristics of the fundraiser, because the donor's perceived probability of receiving a match must always be between 0 and 1, we know that their optimal donation g^* must also always be between g^0 and g^1.

Letting g^* denote the optimal choice of g as defined by (3.11), we can use the implicit function theorem to determine the donor's optimal response to a change in p:

$$\frac{\partial g^*}{\partial p} = \frac{FOC^0(g^*) - FOC^1(g^*)}{|H(g^*)|}, \tag{3.12}$$

where $|H(g^*)| < 0$ is the determinant of the Hessian for (3.3) evaluated at g^*.[6] The sign of $\frac{\partial g^*}{\partial p}$ depends on the sign of the numerator in (3.12), which can vary across donors. However, based on how a donor responds to receiving a certain match at rate μ relative to receiving no match, the sign of $\frac{\partial g^*}{\partial p}$ can be determined. This is formalized in the following lemma:

Lemma 2: *For each donor i and all μ, if $g^1(\mu) > (=) < g^0(\mu)$, then $FOC^0\left(g^*(\mu)\right) - FOC^1\left(g^*(\mu)\right) < (=) > 0$ for all p.*

[5]This is a result of using the impure impact model of giving, which takes the total giving of others to be exogenous. For models in which the total giving of others is endogenous (e.g., Andreoni (1989, 1990); Becker (1974); Bergstrom et al. (1986); Cornes and Sandler (1984)), changes in a donor's beliefs about the total giving of others would directly affect their utility and alter their optimal donation amount, independent of any match effects.

[6]Since the donor's optimization problem (3.3) is a function of only g and we have assumed an interior solution, the Hessian is just the term EU_{gg}. That is, $|H(g^*)|$ is just the second order condition evaluated at g^*. Since $U^0_{gg},\ U^1_{gg} < 0\ \forall g \in [0, w]$ and EU is just a weighted sum of U^0 and U^1, it follows that $EU_{gg} < 0\ \forall g \in [0, w]$.

Proof of Lemma 2 is provided in appendix C.1.

The Response to a Change in the Match Rate

A key feature of this model is its ability to provide insights into why match-price elasticities can vary across fundraisers, which I will now demonstrate. To begin, it is important to keep in mind that each donor's perceived distribution of G_{-i} is a function of the parameters $(N, \mu, \phi, \mathbf{w}_{-i})$, which in general will vary across fundraisers. While the exact relationships between the parameters and donors' beliefs about G_{-i} are free to vary across donors, I enforce the following assumptions:[7]

Assumption 3: *For all donors $i = 1, ..., N$,*

$$(a)\quad \frac{\partial F_i(\frac{\phi}{\mu}|\cdot)}{\partial N} = \frac{\partial F_i(G_{-i}|\cdot)}{\partial N}\Big|_{G_{-i}=\frac{\phi}{\mu}} < 0$$

$$(b)\quad \frac{\partial F_i(\frac{\phi}{\mu}|\cdot)}{\partial \phi} = \frac{1}{\mu} f(\frac{\phi}{\mu}|\cdot) + \frac{\partial F_i(G_{-i}|\cdot)}{\partial \phi}\Big|_{G_{-i}=\frac{\phi}{\mu}} > 0$$

$$(c)\quad \frac{\partial F_i(\frac{\phi}{\mu}|\cdot)}{\partial \mu} = -\frac{\phi}{\mu^2} f(\frac{\phi}{\mu}|\cdot) + \frac{\partial F_i(G_{-i}|\cdot)}{\partial \mu}\Big|_{G_{-i}=\frac{\phi}{\mu}} < 0$$

$$(d)\quad \frac{\partial F_i(\frac{\phi}{\mu}|\cdot)}{\partial \mathbf{w}_{-i}} = \frac{\partial F_i(G_{-i}|\cdot)}{\partial \mathbf{w}_{-i}}\Big|_{G_{-i}=\frac{\phi}{\mu}} < 0$$

Part (a) says that each donor's perceived probability of being matched decreases as the number of potential donors increases, all else equal. Essentially, donors believe that the larger the donor base is, the more the charity will on average receive in donations, all else equal.

Part (b) says the perceived probability of being matched *increases* as the match amount,

[7]Although I believe Assumption 3 to be reasonable—and I expect it should hold for most donors—it is mostly presented to simplify the discussion that follows. Technically, Assumption 3 can be tested by eliciting donors' perceived probabilities (to the extent that donors' reported beliefs are accurate reflections of their behavior). If the signs of any of the conditions presented in Assumption 3 are flipped, the predictions presented below will likewise flip signs in a straightforward way.

ϕ, increases, all else equal. Note that an increase in ϕ has two effects on the probability of being matched: a direct effect (the first term in (b)), resulting from the increase in the match limit, and an indirect effect (the second term in (b)), resulting from the shift in the donor's perceived distribution of the total giving of others. Assumption (b) does not restrict the second term to be positive (i.e., they may believe donations will increase or decrease in response to an increase in ϕ). Instead, assumption (b) simply states that the direct effect must outweigh the indirect effect, so that the overall effect will be to increase the probability of being matched.

Part (c) says the perceived probability of being matched decreases as the match rate, μ, increases, all else equal. Note that, like the effect of an increase in ϕ, an increase in μ results in both a direct and an indirect effect. In this case, the direct effect is negative, since an increase in the match rate will mechanically decrease the match limit ($\frac{\phi}{\mu}$ is decreasing in μ). However, the indirect effect may once again be positive or negative. That is, donors may believe that total donations will either increase or decrease in response to an increase in the match rate. All that part (c) requires is that the direct effect outweigh the indirect effect so that the overall effect is negative.

Finally, part (d) says the perceived probability of being matched decreases as the wealth of the other potential donors increases, all else equal. That is, donors believe that donations will on average increase as the population of potential donors becomes more wealthy.

For each of these assumptions, I have assumed that the parameters of the fundraiser are known by all donors. This helps to make the presentation clearer, but it is not necessary for the analysis that will follow. It can instead be assumed that each donor forms their own beliefs about the parameters of the fundraiser. In that case, assumptions (a)-(d) would dictate how each donor's perceived probability of being matched would change as their *beliefs* about each parameter value change, holding all else equal.[8]

[8]For example, a given donor is most likely unable to quantify the number of potential donors for any particular fundraiser, and they may not even be able to form a reasonably accurate estimate. However, they may still be able to identify that the Red Cross has significantly more potential donors than their local church. And, if so, it would be reasonable to assume that given a $10,000 match limit, the donor would

Proposition 1 states that, for a given match rate μ, each donor's optimal donation is monotonic in their perceived probability, p, of being matched. Therefore, any donors who are responsive to matches should also be responsive to changes in p. However, when the changes in p are the result of changes in the characteristics of the fundraiser, the overall impact on donor behavior is much less straightforward.

As an example, consider the effect of an increase in the match rate, μ, for a donor who responds positively to a certain match (i.e., a donor for whom $g^1 > g^0$). Assume for now that $\frac{\partial g^1}{\partial \mu} > 0$, i.e, the donor's optimal donation under a certain match is increasing in the match rate. Because the donor responds positively to certain matches, it may seem reasonable to expect that they will also increase their donation in response to an increase in the *uncertain* match rate. However, this ignores the effect that increasing the match rate, μ, has on the perceived probability, p, of being matched. Accounting for this effect, it is possible for the same donor to *decrease* their donation in response to an increase in the match rate when the match is uncertain. Thus, a given donor may display different match-price elasticities depending on the characteristics of the fundraiser, and in general responses to matches may vary significantly across different fundraisers. This is formalized in the following result:

For a donor with EU given by equation (3.3), we have

$$\frac{\partial g^*}{\partial \mu} = \left\{ \left[-\frac{\phi}{\mu^2} \cdot f\left(\frac{\phi}{\mu}\Big|\cdot\right) + \frac{\partial F\left(\frac{\phi}{\mu}\big|\cdot\right)}{\partial \mu} \right] \cdot \left[FOC^0(g^*) - FOC^1(g^*)\right] \right.$$
$$\left. + F\left(\frac{\phi}{\mu}\Big|\cdot\right) \cdot \left[g^*\left(U^1_{xR} - U^1_{gR} - (1+\mu)U^1_{RR}\right) - U^1_R\right] \right\} \Big/ |H(g^*)| \quad (3.13)$$

Since $|H(g^*)| < 0$, the sign of 3.13 will be the opposite of the sign of the numerator. The first term in brackets captures the effect of the change in the match rate on the donor's perceived probability of being matched. By Assumption 3, this effect is negative. However,

perceive a much greater chance of being matched if the fundraiser in question was their local church rather than the Red Cross (*assumption (a)*). Thus, the model is able to provide insight into why donor behavior might vary across fundraisers, even if donors lack perfect knowledge of the parameters.

as highlighted in the discussion of Assumption 3, the magnitude of this effect will depend on the characteristics of the fundraiser (particularly the value of $\frac{\phi}{\mu}$), and in general it will vary across donors. From Lemma 2, the sign of the second term in brackets depends on the donor's response to a certain match. Thus, the first addend in the numerator can be either positive or negative, depending on the donor. Likewise, the sign of the final term in brackets also depends on the donor's response to a certain match. This is summarized in the following proposition:

Proposition 2: *For each donor, if* $\frac{\partial g^1}{\partial \mu} > (=) < 0$*, then the first addend of the numerator of* $\frac{\partial g^*}{\partial \mu}$ *will be* $> (=) < 0$*, and the second addend of the numerator will be* $< (=) > 0$*. Thus, the first and second addends will always have opposite signs, and the overall sign of* $\frac{\partial g^*}{\partial \mu}$ *will be ambiguous.*

Proof of Proposition 2 is provided in Appendix C.1. While Proposition 2 does not lend itself to any easily testable hypotheses, it does provide insight into why donors might exhibit ostensibly contradictory reactions to changes in the match rate across different fundraisers. Donors may have negative match-price elasticities when they perceive a high probability of being matched and positive match-price elasticities when they perceive a low probability of being matched (and *vice versa*). As the perceived distribution of G_{-i} becomes more concentrated around the cutoff of $\frac{\phi}{\mu}$, such "reversals" will become more pronounced.[9]

[9] As the distribution of G_{-i} becomes more concentrated around $\frac{\phi}{\mu}$, the first term in the numerator of $\frac{\partial g^*}{\partial \mu}$ becomes more negative, increasing the magnitude of the first addend in the numerator. By Proposition 2, the first addend is positive for donors for whom $\frac{\partial g^1}{\partial \mu} > 0$. Thus, as the first addend increases in magnitude (i.e. becomes more positive), it becomes increasingly likely that $\frac{\partial g^*}{\partial \mu} < 0$. The opposite argument holds for donors for whom $\frac{\partial g^1}{\partial \mu} < 0$, so that it becomes increasingly likely that $\frac{\partial g^*}{\partial \mu} > 0$ as their perceived distribution of G_{-i} becomes more concentrated around $\frac{\phi}{\mu}$.

The Response to Changes in Other Fundraiser Characteristics

The responses of donors to changes in each of the remaining fundraiser parameters (ϕ, N, and $\mathbf{w}_{-i}$) are less ambiguous. For these parameters, we can derive the following effects:

$$\frac{\partial g^*}{\partial \phi} = \frac{\left[\frac{1}{\mu} f(\frac{\phi}{\mu}|\cdot) + \frac{\partial F_i(\frac{\phi}{\mu}|\cdot)}{\partial \phi}\right] \cdot [FOC^0(g^*) - FOC^1(g^*)]}{|H(g^*)|} = \frac{\partial g^*}{\partial p} \cdot \frac{\partial F}{\partial \phi}, \tag{3.14}$$

$$\frac{\partial g^*}{\partial N} = \frac{\frac{\partial F_i(\frac{\phi}{\mu}|\cdot)}{\partial N} \cdot [FOC^0(g^*) - FOC^1(g^*)]}{|H(g^*)|} = \frac{\partial g^*}{\partial p} \cdot \frac{\partial F}{\partial N}, \text{ and} \tag{3.15}$$

$$\frac{\partial g^*}{\partial \mathbf{w}_{-i}} = \frac{\frac{\partial F_i(\frac{\phi}{\mu}|\cdot)}{\partial \mathbf{w}} \cdot [FOC^0(g^*) - FOC^1(g^*)]}{|H(g^*)|} = \frac{\partial g^*}{\partial p} \cdot \frac{\partial F}{\partial \mathbf{w}_{-i}}. \tag{3.16}$$

Since $|H(g^*)| < 0$, the sign of each effect is given by the opposite sign of the numerator. In general, the signs of the effects will vary across donors and fundraisers. However, Assumption 3 can be combined with Lemma 2 to give us Proposition 3:

Proposition 3: *For each donor, if $g^1 > (=) < g^0$, then each of the following must hold:*

$$(i)\ \ \frac{\partial g^*}{\partial \phi} > (=) < 0$$

$$(ii)\ \ \frac{\partial g^*}{\partial N} < (=) > 0$$

$$(iii)\ \ \frac{\partial g^*}{\partial \mathbf{w}_{-i}} < (=) > 0$$

The proof of Proposition 3 follows directly from Assumption 3 and Lemma 2. Proposition 3 leads to the following testable hypotheses:

Hypothesis 2: *Donors who increase (decrease) their donation in response to a certain match will decrease (increase) their donation in response to an increase in the number of potential donors.*

Hypothesis 3: *Donors who increase (decrease) their donation in response to a certain match will decrease (increase) their donation in response to an increase in the wealth of the population of potential donors, holding their own wealth constant.*

3.3 Experiment Design and Procedures

From January to March of 2024, I used the Prolific platform to recruit 150 subjects to participate in an online experiment. After choosing to participate in my study on Prolific, subjects are provided with a link to a Qualtrics survey. After completing the Qualtrics survey and verifying their submission in Prolific, participants are paid a $9 base payment through Prolific. In addition to the base payment, subjects are also paid bonus payments ranging from $0-$13, depending on their decisions during the experiment.

Upon opening the Qualtrics survey, subjects are first taken to a consent page. After consenting to participate in the study, they are taken to the instructions page. During the instructions, subjects are required to answer a series of questions to test their understanding.[10] After reading through the instructions and correctly answering all test questions, subjects are then taken to the main task of the experiment. Following the main task, subjects are then asked to complete follow-up tasks to measure their risk preferences, and then are asked to complete some basic survey questions before submitting their survey.

Section 3.3.1 discusses the main task of the experiment. Section 3.3.2 discusses the

[10] There are a total of three test questions. Each question presents subjects with an example scenario and asks them to select the correct answer from a drop-down list. If they select an incorrect answer, they are provided with an explanation for why their answer is wrong and asked to try again. There are no limits on the number of attempts a subject can make, but they cannot proceed until they answer each question correctly. While it is technically possible for a subject to get through the questions without actually understanding the task—for example, by indiscriminately trying each answer until they happen upon the correct one—the Prolific platform has implemented safeguards to prevent this sort of behavior. Attention-check questions are commonly included in surveys, and Prolific allows researchers to withhold payment from any subject who incorrectly answers at least two attention-check questions within a survey. This holds even if the subject takes the time to complete the entire survey (which may be a sizable commitment, depending on the study). Because of this, subjects have a very real incentive to carefully answer all questions. In fact, a handful of subjects actually returned their survey before completing the instructions, likely out of fear that their payment would be withheld as a result of them incorrectly answering the test questions.

follow-up tasks. Section 3.3.3 discusses the survey questions included in the experiment. Section 3.3.4 outlines the procedures used in the experiment. A copy of the full text of the experiment is provided in Appendix C.4.

3.3.1 Main Task

The main task consists of 4 stages. In each stage, subjects are randomly grouped with other subjects and each member of the group is provided with the same endowment (and the endowment of each group member is common knowledge). The number of other group members and the endowment provided to each member of the group varies across stages, with $N \in \{5, 10\}$ and $\mathbf{w} \in \{80, 120\}$, where the endowment is provided in Tokens (10 Tokens = \$1). Note that N denotes the number of *other* members in a subject's group, so that the full group size, including the subject, is given by $N + 1$.

Within each stage, subjects are presented with a menu of 13 problems (similar to the menu of problems used in the experiment from the previous chapter) in which they must decide how much of their endowment to pass to a real charity.[11] Among the 13 decision problems within each stage, four different match rates are offered: $\mu \in \{0, 0.5, 1, 2\}$. Furthermore, for each nonzero match rate, three different lead gifts/match limits are provided.[12] Table 3.1 shows the parameter values used in each question, which remain the same across all four stages.

In each question, any amount a subject chooses to pass to the charity will receive a match at the stated match rate as long as the total donations of the N other group members does not exceed the stated match limit for the problem. Consistent with the theoretical model developed in Section 3.2, whether or not a subject's donation receives a match depends only

[11] As in the last chapter, the charity used is *charity: water*. The description of the charity provided in the instructions to the experiment is identical to that used in the previous chapter.

[12] The match limit ($\frac{\phi}{\mu}$) is determined by the amount of the lead gift (ϕ) and the match rate (μ), so that, for a given match rate, the lead gift and match limit convey the same information. However, in the experiment, subjects are informed of the match limit in each problem—not the lead gift amount—since this is typically the procedure used in real-world fundraisers and likely reduces confusion on the part of donors.

Table 3.1: Match rates and lead gifts/match limits used in all stages.

Question	match rate (μ)	lead gift (ϕ)	match limit ($\frac{\phi}{\mu}$)
1	0	NA	NA
2	$1/2$	80	160
3	$1/2$	120	240
4	$1/2$	160	320
5	$1/2$	∞	∞
6	1	120	120
7	1	160	160
8	1	240	240
9	1	∞	∞
10	2	160	80
11	2	240	120
12	2	320	160
13	2	∞	∞

on the decisions of the *other* members of their group, and there are no partial matches.

Subjects are placed into new random groups for each stage and no information about the decisions of other subjects is ever provided. However, subjects are always informed of the group size (N) and the endowments of all group members ($\mathbf{w}$). The only role of the groups is to determine whether a subject's donations are matched (i.e., whether or not their group's total donations, G_{-i}, exceed the match limit, $\frac{\phi}{\mu}$), but subjects never receive any information about the giving of others or whether the match limit was exceeded. Instead, subjects must construct their own beliefs about the probability of being matched based on the exogenously determined parameters, N, μ, ϕ, and $\mathbf{w}$.

Within each stage, all 13 decision problems are presented simultaneously within a single table. Each row presents a question, and there are five separate columns. The first column contains the description for each problem, which informs the subject of the number of other subjects in their group (N), the amount that they and each of their group members have been endowed with ($\mathbf{w}$), the match rate (μ), and the match limit ($\frac{\phi}{\mu}$). The descriptions for each problem all begin with the sentence, "*You, and each of the* {5, 10} *other members of your group, are endowed with* {80, 120} *Tokens.*" With the exception of the problems that provide certain matches and the one problem that provides no match, the remainder of the description for each problem has the following format: "*The experimenter will match donations at a* {0.5:1, 1:1, 2:1} *rate up to a match limit of* {80, 120, 160, 240, 320} *Tokens.*" The problems that provide certain matches instead read "*The experimenter will match all donations at a* {0.5:1, 1:1, 2:1} *rate.*" The description for the problem with no match says nothing more than the first sentence, informing subjects of their group size and endowments.

The second column of each problem provides a text-entry box in which subjects are prompted to enter the number of Tokens they would like to pass to the charity. After a subject enters a value in column 2 of a given problem, the remaining columns (3-5) automatically populate to provide the subject with the following information: column 3 reports the number of Tokens the subject will hold for themselves (their endowment minus the amount they pass), column 4 reports the total donation that will be received by the charity if no match is received, and column 5 reports the total donation that will be received by the charity if a match is received.[13] These values are reported to subjects to reduce confusion and prevent calculation errors, and subjects are free to edit their responses as many times as they like before submitting them.

Once a subject has provided acceptable responses to all 13 questions within a stage, they are able to submit their decisions for the stage. If they fail to answer any of the questions, or if they provide an unacceptable response to any of the questions (e.g., they attempt to pass a negative amount or an amount greater than their endowment), they receive an error message informing them of the specific issue and are unable to continue until providing an acceptable response. After successfully submitting their responses, they move on to the next stage, where they are presented with the same 13 questions but are assigned a new combination of group size (N) and endowments ($\mathbf{w}$). After completing a stage, subjects are unable to

[13]For questions that provide a 100% chance of receiving a match (i.e., those with no match limit), column 4 displays "N/A" regardless of the amount the subject chooses to pass, helping to make it clear to the subject that the match is certain. Likewise, for the question that provides a 0% chance of receiving a match, column 5 displays "N/A" regardless of the amount the subject chooses to pass, helping to make it clear that there is no chance of receiving a match.

Table 3.2: Parameter values used for each question order.

	Order 1		Order 2		Order 3		Order 4	
Question	μ	$\frac{\phi}{\mu}$	μ	$\frac{\phi}{\mu}$	μ	$\frac{\phi}{\mu}$	μ	$\frac{\phi}{\mu}$
1	0	NA	0	NA	$1/2$	∞	2	∞
2	$1/2$	160	2	80	$1/2$	320	2	160
3	$1/2$	240	2	120	$1/2$	240	2	120
4	$1/2$	320	2	160	$1/2$	160	2	80
5	$1/2$	∞	2	∞	1	∞	1	∞
6	1	120	1	120	1	240	1	240
7	1	160	1	160	1	160	1	160
8	1	240	1	240	1	120	1	120
9	1	∞	1	∞	2	∞	$1/2$	∞
10	2	80	$1/2$	160	2	160	$1/2$	320
11	2	120	$1/2$	240	2	120	$1/2$	240
12	2	160	$1/2$	320	2	80	$1/2$	160
13	2	∞	$1/2$	∞	0	NA	0	NA

return to it. To maintain incentive compatibility, only one problem from all four stages is randomly selected for payment (Azrieli et al., 2018).[14]

To account for any order effects induced by the order in which the 13 questions are presented, subjects are randomly assigned one of four different orders. Subjects are assigned the same question order in all four stages. The orders used are shown in Table 3.2. The first order exactly matches what is presented in Table 3.1, with questions ordered first by ascending match rates and then by ascending lead gifts/match limits. The remaining orders vary by presenting match rates in ascending order and/or match limits in descending order, as well as placing the no-match question at either the beginning or end of the list.

Similarly, to account for any order effects associated with the parameter values presented in each stage (N and $\mathbf{w}$), subjects are presented with the stages in random order. Given that there are four distinct stages, there are $4! = 24$ possible orders. By limiting the analysis to only the first stage presented to each subject, I am able to make between-subject comparisons

[14]To help subjects understand the procedure, they are told that one of the four stages will be randomly selected, and that one problem from the selected stage will then be randomly selected to determine payment. In practice, however, the computer drew a random number between 1 and 52 to determine the problem selected for payment, rather than conducting a two-stage lottery.

that are free of any potential stage-order effects. Looking at all stages presented to subjects, I am also able to make within-subject comparisons of behavior across stages. Because the order of stages is completely randomized for each subject, I am also able to test for stage-order effects.

Below I discuss each stage in greater detail. Although the order of stages is randomized for each subject, I number them 1 through 4 to make it easier to refer to each stage.

Stage 1: $N = 5$**,** $\mathbf{w} = 80$

In Stage 1, subjects are grouped with 5 other subjects (i.e., $N = 5$), and all subjects are endowed with 80 Tokens (i.e., $\mathbf{w} = \mathbf{80}$). The total wealth of the other members of each subject's group in Stage 1 is 400 Tokens (5 other subjects, each endowed with 80 Tokens). Therefore, for any match limit less than 400 Tokens, it is possible for the total donations of the group to be large enough to exceed the match limit. That is, the match becomes uncertain.

Stage 2: $N = 10$**,** $\mathbf{w} = 80$

In Stage 2, subjects are again endowed with 80 Tokens each, but they are now grouped with 10 other subjects ($N = 10$). The total wealth of the other members of each subject's group is now 800 Tokens (10 other subjects with 80 Tokens each). Relative to Stage 1, the other members of each subject's group now have twice as many Tokens available to them. According to Proposition 3 in Section 3.2, for questions that provide uncertain matches, the increase in group size should cause subjects to adjust their donation amounts relative to Stage 1, due to a decrease in their perceived probabilities of receiving a match. Whether they should be expected to increase or decrease their donations relative to Stage 1 will depend on their response to certain matches, which will also be observed.[15] Thus, the comparison

[15]Note that the model developed in Section 3.2 predicts that a change in the number of other potential donors (N) will only affect a donor's choice of donation through its effect on the donor's perceived probability of receiving a match. Because of this, the model also predicts that changes in N should have no impact on how much a donor gives when no match is offered or when a match is received with certainty. That is, going

of Stages 1 and 2 provides a test of Hypothesis 2.

Stage 3: $N = 5$**,** $\mathbf{w} = 120$

In Stage 3, subjects are grouped with 5 other subjects ($N = 5$), and they are now each endowed with 120 Tokens ($\mathbf{w} = \mathbf{120}$). The total wealth of the other members of each subject's group is 600 Tokens (5 other subjects with 120 Tokens each). Relative to Stage 1, the other members of each subject's group now have 50% more Tokens available to them. According to Proposition 3, the increase in the endowments of the *other* members of a subject's group should affect their giving decisions in the uncertain-match questions, as a result of a decrease in their perceived probability of receiving a match.[16] However, this result is confounded by the wealth effect that results from the increase in the donor's own endowment. As a result, Hypothesis 3 cannot be directly tested by comparing Stages 1 and 3. That said, the model also predicts that an increase in N should have no effect on a donor's behavior in the no-match and certain-match questions. Therefore, the wealth effect that results from increasing the donor's own endowment can be estimated by comparing their behavior in the no-match and certain-match questions in Stages 1 and 3.

Stage 4: $N = 10$**,** $\mathbf{w} = 120$

In Stage 4, subjects are grouped with 10 other subjects ($N = 10$), and they are each endowed with 120 Tokens ($\mathbf{w} = \mathbf{120}$). The total wealth of the other members of each subject's group is 1200 Tokens (10 other subjects with 120 Tokens each). Relative to Stage 1, Proposition 3 now predicts the largest effect on behavior in uncertain-match questions, since there are now two factors contributing to a decrease in donors' perceived probabilities of receiving a

---

from Stage 1 to Stage 2 should have no effect on the amount donors give in the no-match and certain-match questions. Therefore, the comparison of Stages 1 and 2 allows for a test of the theoretical model. If subjects are observed to adjust their donation amounts in the no-match and certain-match questions when moving between Stages 1 and 2, this would suggest that the giving decisions of others do in fact enter into donor utility.

[16] Note that Proposition 3 and Hypothesis 3 are both written in terms of the wealth levels of the *other* potential donors, $\mathbf{w}_{-i}$.

match. Like Stage 3, there is a wealth effect resulting from the increase in subjects' own endowments (relative to Stage 1).

3.3.2 Follow-up Tasks

After completing all four stages of the main task, subjects are then asked to complete two follow-up tasks to measure their risk preferences: a *Payment Follow-up Task* and a *Donation Follow-up Task*. The order in which the tasks are presented to subjects is randomized across subjects.

Both tasks present subjects with a list of 11 choices, where they must choose either Option A or Option B. In the *Payment Follow-up Task*, Option A always provides the subject with "*A 50% chance of receiving $1, and $0 otherwise*". Option B provides a guaranteed fixed payment, increasing in $.10 increments, from $0 in the first question to $1 in the final question. The *Donation Follow-up Task* is identical, except that the options provide donations to the charity rather than payments to the subject. The presentation of the tasks is modeled after the presentation used by Exley (2016) for similar tasks.[17]

The first and last questions of each follow-up task provide an attention/understanding check. In the first question, subjects are choosing between a lottery that provides a $1 payment (or donation) with 50% probability and a guaranteed payment (or donation) of $0, so it is expected that all subjects should choose Option A (the lottery) in the first question. In the last question, Option B provides a guaranteed payment (or donation) of $1, so it is expected that all subjects should prefer Option B in the final question.[18]

The *Payment Follow-up Task* and *Donation Follow-up Task* allow me to estimate subjects' risk preferences for payments and donations, respectively, based on when they choose to switch from Option A to Option B (assuming they switch exactly once). Subjects who are more risk averse will switch from Option A to Option B earlier, while those who are

[17]See figures A.3 and A.4 in Appendix A of Exley (2016). The lotteries and guaranteed payments used are different, but I have borrowed Exley's instructions.

[18]If a subject were for some reason to prefer that the charity receive nothing, it would be rational for them to begin by choosing Option B and then switch to choosing Option A.

more risk loving will switch later.

3.3.3 Survey Questions

After completing the follow-up tasks, subjects answer some basic demographic questions, including age, gender, estimated family income, political views (e.g., conservative, liberal, etc.), importance of religion in their life, and estimated charitable contributions within the last year. Subjects are also asked to rate—on a scale of 0 to 10—how familiar they are with charity:water, how well they understood the procedure used to determine their payment, how well they understood the procedure used to determine their total donation, and how confident they were during the experiment that their donation (and any matching donation provided by the experimenter, if applicable) would actually be donated to the charity on their behalf. Copies of the questions included in the survey are provided in Appendix C.4. Summary statistics for subjects' responses to the survey questions are provided in Table C.2.

3.3.4 Procedures

Once subjects finish answering the survey questions, their payment is reported to them. To determine each subject's payment, one question from the main task is randomly selected. Subjects receive the number of Tokens they chose to hold for themselves in the randomly selected problem. These Tokens are converted to US dollars at the rate 10 Tokens = \$1. One problem is then also randomly selected from the *Payment Follow-up Task*. If the subject chose the guaranteed payment (Option B) in the randomly selected problem, then that amount is added to their total payment. If they instead chose the lottery (Option A), the computer 'flips a coin' to determine the outcome of the lottery.[19]

Subjects are also provided with information regarding their total donation. Each subject's total donation is calculated through a similar process to that of their payment. The

[19] This is done by drawing a random number x from a uniform distribution over $(0, 1)$. If $x < 0.5$, then the subject receives a payment of \$1. Otherwise, the subject receives \$0.

same randomly selected decision problem that was selected to determine their payment in the main task is also used to determine their total donation. In addition to this amount, a random question is also drawn from the *Donation Follow-up Task*, and the resulting additional donation (if any) is added to the subject's total donation.

The message the subject receives regarding their total donation varies depending on the nature of the problem randomly selected to determine their payment. If their randomly selected problem provided no match, then the subject is informed of this and told that their final donation is equal to the amount they chose to pass. If their randomly selected problem provided a match with certainty, then they are informed of this, and the applicable matching amount, based on the match rate and the amount they chose to pass, is added to their total donation. In this case, their total donation (not including the *Donation Follow-up Task*) is given by $(1 + \mu_k)g_k$, where k denotes the randomly selected problem, μ_k is the match rate in the selected problem, and g_k is the amount they chose to pass in the selected problem.

If a subject's randomly selected problem provided an uncertain match—that is, a match that depends on the total donations of the other members of the subject's group—then the subject is not informed whether they will receive a match, since all match determinations are made at the end of the study, after all survey responses have been received. Instead, the subject is reminded of the parameters in their selected problem (including the match rate and match limit) and told that they will be randomly grouped with the applicable number of other subjects from the experiment, and the decisions of those random group members (in the same problem) will be used to determine whether they will receive a match. The subject is also informed of the possible amounts of their total donation, based on the outcome of the matching process. That is, they are told what their total donation will be if they do not receive a match, as well as what their total donation will be if they do receive a match.[20]

Subjects' earnings are paid to them through Prolific using bonus payments within two

[20]The details of the process for determining match outcomes are presented to subjects in the instructions, before they begin the main task. The full instructions, including the description of the matching process, are provided in Appendix C.4.

business days after they complete the experiment.[21] This is in addition to the $9 base payment they receive, which is automatically paid to them after they verify their submission in Prolific. The average total payment received by participants (including both the base payment and the bonus payment) is $17.33. The median completion time among participants is 27 minutes and 18 seconds, and the average reward-per-hour received is $19.79.

At the time of this writing, match determinations have not yet been made for each subject, and subjects' total donations have not yet been donated to the charity. Once match determinations have been made for each subject so that each subject's total donation can be calculated, the sum of all subjects' total donations will be donated to the charity in a single lump-sum donation. On average, subjects chose to donate $2.95 out-of-pocket to the charity. The maximum average total donation, assuming all subjects end up receiving a match, is $5.65. The total donation received by the charity as a result of the experiment will be between $442.90 (assuming no subjects receive a match) and $847.40 (assuming all subjects receive a match).

3.4 Results

Table C.2 presents summary demographic statistics for all subjects. Section 3.4.1 presents evidence that fundraiser characteristics affect how donors respond to match subsidies. Section 3.4.2 looks more closely at the response to changes in the match limit, and considers the extent to which observed behavior is consistent with the theoretical model. Section 3.4.3 classifies subjects as *match lovers*, *haters*, and *ignorers* to further explore the heterogeneity in behavior and test the predictions of Hypotheses 1 and 2. Section 3.4.4 tests for order effects, based on the order in which the stages are presented.

[21] In practice, subjects received their bonus payments within 1-2 hours of verifying their submission in Prolific. However, in the description posted to Prolific, and in the experiment instructions, subjects were provided with a guarantee that their bonus payment would be received within two business days.

3.4.1 Evidence that fundraiser characteristics affect the response to matches

Table 3.3 reports the results of estimating various log-log Tobit regression models for subjects' total gross donations (assuming any offered matches are received), accounting for censoring from above and below.[22] Column (1) presents the results of regressing only on endowment, price, and a dummy variable equal to 1 whenever a match is offered. The estimated coefficient on price (–1.252) is in line with previous research, particularly previous experimental work.

In column (2), two additional controls are included, for the match limit and group size. Both controls are highly statistically significant, demonstrating that, on average, donations vary as fundraiser characteristics vary. The signs of both coefficients are in line with our expectations: on average donations are increasing in the match limit and decreasing in group size.

Column (3) introduces an interaction term between price and the match limit. This term is statistically significant, but the coefficients on price and match limit are no longer statistically significant. However, this is in line with our expectations, since this implies that donors will be unresponsive to price changes when the match limit is 0 (i.e., when there is no chance of receiving the match). The negative sign on the interaction term is also consistent with our expectations, since it implies that decreases in price (i.e., increases in the match rate) become more effective at increasing donations as the match limit increases.

Column (4) adds another interaction term between price and group size. The addition of the interaction term removes the significance of the coefficient on group size. Both of the coefficients on match limit and group size are estimated to be 0, while both of the interaction terms are statistically significant. This suggests that group size and match limit mainly impact donations through their interactions with price, while having little direct impact on behavior. Again, this is consistent with the theoretical model and predictions. Furthermore, the positive sign on the interaction term between price and group size is also consistent with our expectations, since this implies that donors become less responsive to

[22]Table C.1 reports the results using the amount passed (i.e., subjects' out-of-pocket donations).

Table 3.3: Comparison of Tobit regression models

Charity receipts	(1)	(2)	(3)	(4)	(5)	(6)
Endowment	0.634***	0.615***	0.619***	0.619***	0.618***	0.617***
	(0.054)	(0.054)	(0.054)	(0.054)	(0.054)	(0.054)
Price	-1.252***	-1.313***	-0.321	-0.735**	-0.666*	-0.043
	(0.040)	(0.042)	(0.309)	(0.346)	(0.350)	(0.729)
Match (=1)	0.041	-0.452***	0.375	0.435	0.382	0.390
	(0.051)	(0.109)	(0.278)	(0.279)	(0.282)	(0.282)
Match limit		0.082***	-0.066	-0.077	-0.014	-0.043
		(0.016)	(0.049)	(0.049)	(0.070)	(0.076)
Group size		-0.135***	-0.131***	0.040	0.141	0.105
		(0.032)	(0.032)	(0.071)	(0.108)	(0.114)
Price*Match limit			-0.180***	-0.194***	-0.188***	-0.323**
			(0.056)	(0.056)	(0.056)	(0.149)
PxN				0.251***	0.196*	-0.102
				(0.094)	(0.104)	(0.323)
MLxN					-0.028	-0.014
					(0.022)	(0.026)
PxMLxN						0.065
						(0.066)
Constant	0.161	0.512	0.497	0.163	-0.031	0.045
	(4.544)	(4.544)	(4.577)	(4.581)	(4.581)	(4.585)
Controls	Y	Y	Y	Y	Y	Y
Observations	7722	7722	7722	7722	7722	7722

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

decreases in price as the number of group members increases.

Columns (5) and (6) include additional interactions between the match limit and group size and between match limit, group size, and price. The inclusion of these interaction terms reduces the precision of the coefficient estimates on the other interaction terms, with the coefficient on the price-group size interaction losing all statistical significance in the final model. However, while less precise, the interaction term between price and match limit remains statistically significant in every model, suggesting that the match limit is an important factor in determining how donors respond to match subsidies.

Taken together, these results provide strong evidence that the response to match subsidies varies as fundraiser characteristics vary. In particular, there is especially strong evidence that the response to match subsidies is influenced by the match limit. While we have not ruled out other possible explanations, these results are consistent with the theoretical model developed in Section 3.2, in which donors consider the probability of receiving a match.

3.4.2 The effect of changes in the match limit

Figure 3.1 plots the average amount passed at each match limit for each match rate.[23] For each match rate, out-of-pocket donations appear to increase as the match limit increases, consistent with our prediction that the response to matches is influenced by fundraiser characteristics. However, contrary to our prediction in Hypothesis 1, Figure 3.1 shows that on average uncertain matches do not always fall between the amount passed in the absence of a match and the amount passed given a certain match. Instead, out-of-pocket donations appear to respond negatively to the low match limits (relative to the no-match baseline) and peak at the high match limits, before falling again when matches are provided with certainty. This pattern holds even when controlling for group size and endowment (see Figure C.1, which produces a separate graph for each group size/endowment combination).

Hypothesis 1 places no restrictions on the slopes of the lines between the endpoints. For example, out-of-pocket donations may increase between *low* and *medium* match limits and then decrease between *medium* and *high* match limits for a given match rate. However, it requires that the out-of-pocket donations at each uncertain match level (low, medium, and high) fall between the no-match and certain-match (no limit) levels. Figure 3.1 shows that this does not hold for any of the match rates. That said, Figure 3.1 plots the *average* donation amounts, and thus it is still possible that individual donors behave consistently with Hypothesis 1.

It is not clear why donors would behave in this way. One possibility is that subjects were simply confused about how the matching subsidies work. To test whether confusion might

[23]The exact values plotted in Figure 3.1 can be found in Table C.3.

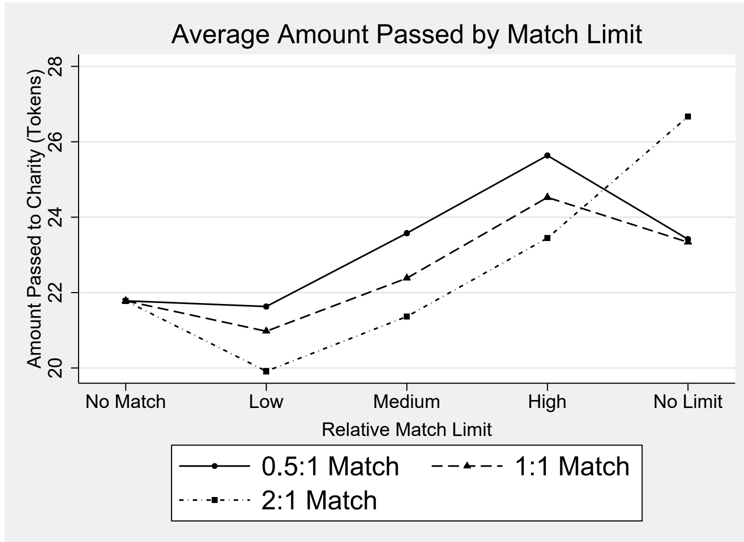


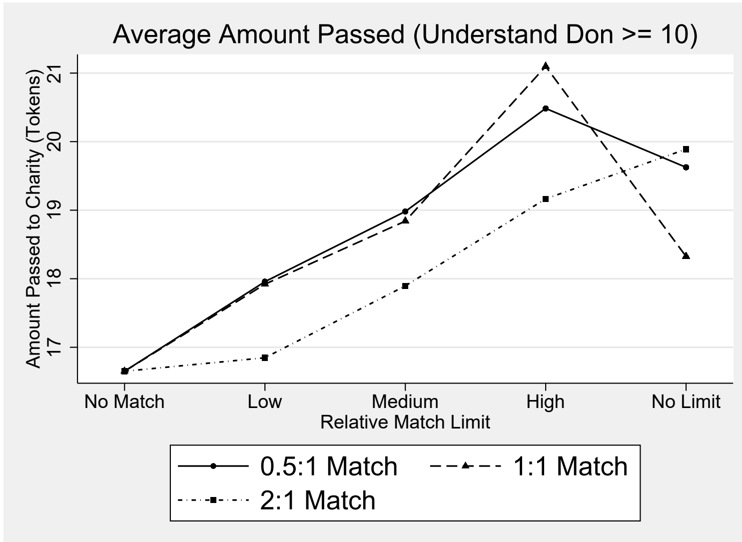
Figure 3.1: Average amount passed (in Tokens) at each match limit

explain the observed behavior, I remake Figure 3.1 including only subjects who report a 10 out of 10 when asked in the follow-up survey to rate their understanding of how their total donation is determined. The results are shown in Figure 3.2.

Figure 3.2 shows that the behavior of subjects who report fully understanding how the match subsidies work is more consistent with Hypothesis 1. However, for the 0.5:1 and 1:1 match rates, subjects continue to reduce their out-of-pocket donations under certain matches relative to the high match limits. While not consistent with Hypothesis 1, this behavior is consistent with a lead gift effect. It appears that mentioning a (high) match limit may lead donors to give more. Although the certain-match questions implicitly imply the largest match limit, no match limit is explicitly mentioned in these questions.

3.4.3 Match lovers, haters, and ignorers

To better understand the extent to which subject behavior is consistent with the predictions of the theoretical model, I classify subjects as *match lovers*, *haters*, and *ignorers* based on their response to certain-matches at each match rate. A subject is classified as a *match lover* at a given match rate if they choose to pass more to the charity when receiving the match



Notes: Averages are calculated using observations from 41 subjects who report a 10 out of 10 when asked to rate their understanding of how their total donation is determined. Reported averages are for out-of-pocket donations, calculated in Tokens (10 Tokens = $1).

Figure 3.2: Average amount passed by match limit—full understanding

with certainty versus the baseline (no match) question. *Match haters* are defined conversely. A subject is classified as a *match ignorer* at a given match rate if they pass the same amount in the certain-match question as in the no-match question. It is important to note that, consistent with the theoretical model, the same subject may be a *match lover* at one match rate (e.g., a 1:1 match) while simultaneously being a *match hater* (or *match ignorer*) at some other match rate (e.g., a 2:1 match).

Because subjects face the no-match question and the same certain-match questions four different times (once in each stage), the classifications are based on their *average* decisions across all four stages. For example, if a subject passes an average of 10 Tokens in the no-match questions across all four stages, and they pass an average of 20 Tokens in the questions providing a 2:1 match *with certainty* across all four stages, they are classified as a 2:1-*match lover*. It is possible, however, that in one (or more) of the stages the subject may have chosen to pass more in the no-match question than they did in the 2:1 certain-match question.

Table 3.4 lists the number of each type of donor at each match rate, as well as the number

Table 3.4: Donor types and their consistency with Hypothesis 1.

		0.5:1 Match		1:1 Match		2:1 Match		All Match Rates	
		Count	Percent	Count	Percent	Count	Percent	Count	Percent
Lovers	Inconsistent	40	74.07	37	60.66	36	50.70	113	60.75
	Consistent	14	25.93	24	39.34	35	49.30	73	39.25
	Total	54		61		71		186	
Ignorers	Inconsistent	13	20.63	15	27.78	12	26.67	40	24.69
	Consistent	50	79.37	39	72.22	33	73.33	122	75.31
	Total	63		54		45		162	
Haters	Inconsistent	19	57.58	26	74.29	22	64.71	67	65.69
	Consistent	14	42.42	9	25.71	12	35.29	35	34.31
	Total	33		35		34		102	
All Types	Inconsistent	72	48.00	78	52.00	70	46.67	220	48.89
	Consistent	78	52.00	72	48.00	80	53.33	230	51.11
	Total	150		150		150		450	

Notes: Table lists the number of subjects at each match rate, broken down by donor type, whose behavior is consistent with Hypothesis 1, which predicts that all donations in uncertain-match questions should fall between the amount passed in the no-match question and the amount passed in the certain-match question. Each question type is averaged across all four stages for each subject. For example, a subject's decisions in each stage for the questions with a 0.5:1 match rate and a LOW match limit are averaged, to derive the subject's average pass amount in those questions. This is repeated for all match rate/match limit combinations. For a subject's behavior to be classified as being consistent with Hypothesis 1, their average pass amounts for each uncertain-match question at a given match rate must be between their average pass amount in the no-match questions and their average pass amount in the certain-match questions (at the same match rate). Subjects are classified as *match lovers* for a given match rate if they pass more in the certain-match questions at that match rate, averaged across all four stages, than they do in the no-match questions, averaged across all four stages. *Match haters* are classified conversely. *Match ignorers* for a given match rate pass the same amount, averaged across all four stages, in the no-match and certain-match questions.

of donors of each type who behave consistently with Hypothesis 1. While the number of *match haters* remains more or less constant across match rates, the number of *match lovers* increases substantially as the match rate increases (from 54 at the 0.5:1 match rate to 71 at the 2:1 match rate), with subjects shifting from being *match ignorers.* Overall, *match lovers* are the most represented donor type. While not shown in Table 3.4, 20 subjects pass the same amount in every question throughout the experiment. Of those 20 subjects, 19 of them passed 0 Tokens in every question.

Table 3.4 shows that, overall, subject behavior is not generally consistent with Hypothesis 1. Across all match rates, subjects behave consistently with Hypothesis 1 51% of the time.

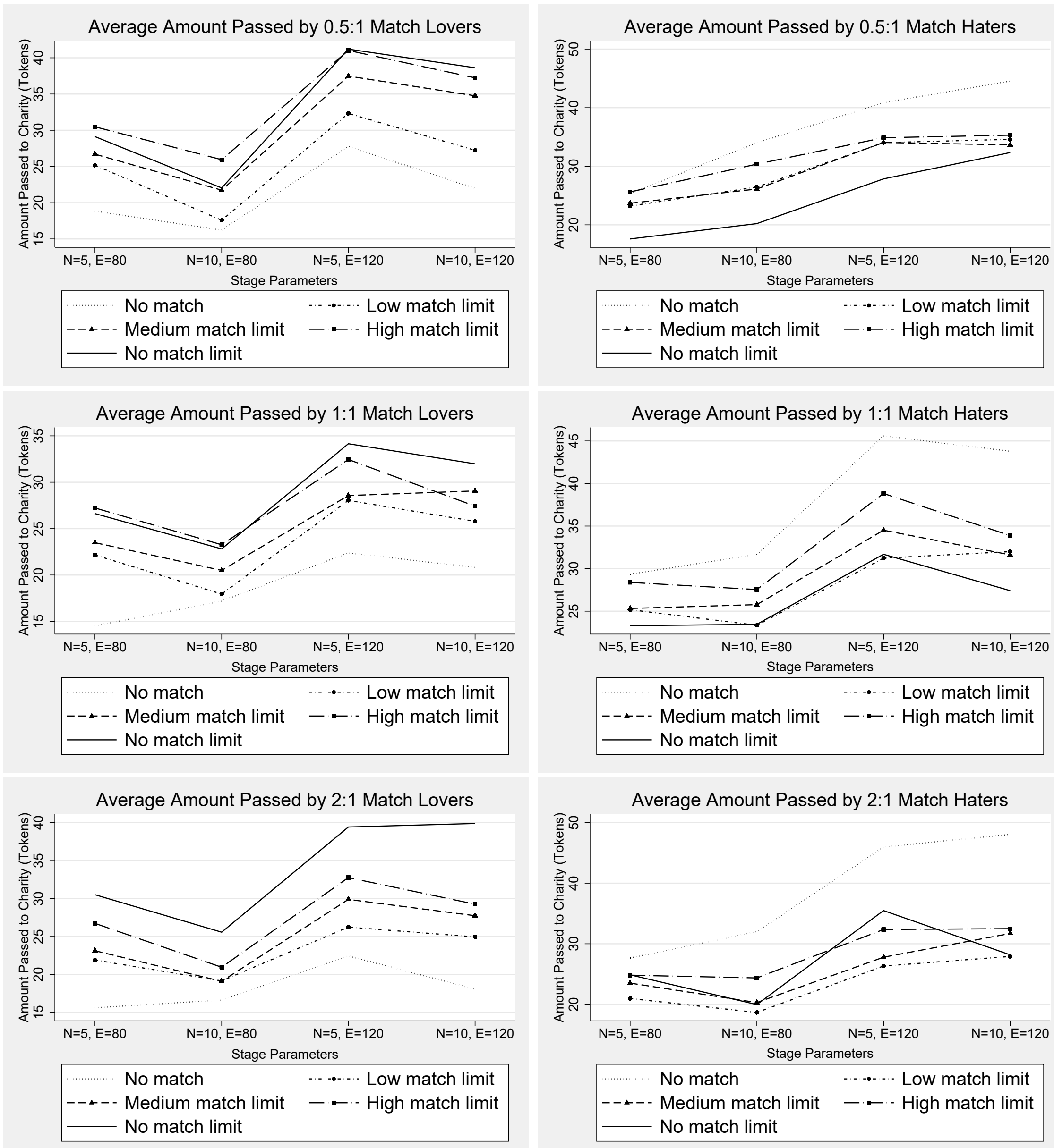
Match ignorers are the most consistent donor type, but this is mostly driven by the 20 subjects who simply choose to pass the same amount in every question. Interestingly, it appears that the same donors remain consistent throughout all match rates: as the number of *match lovers* who behave consistently with Hypothesis 1 increases as the match rate increases, the number of *match ignorers* who behave consistently drops, while the number of subjects behaving inconsistently with Hypothesis 1 remains relatively constant in both groups. Thus, it appears that the consistent *match ignorers* shift to being *match lovers* as the match rate increases, increasing the number of consistent *match lovers* as they do so.

Although subjects are only observed to behave consistently with Hypothesis 1 51% of the time, in general behavior does not stray too far from the predicted behavior. This can be seen in Figure 3.3. Figure 3.3 plots the average out-of-pocket donations of *match lovers* (left column) and *match haters* (right column) at the 0.5:1 match rate (top row), 1:1 match rate (middle row), and 2:1 match rate (bottom row). Within each panel, the average pass amounts are plotted for each match limit and each unique stage (as defined by the group size and endowment). Overall, the behavior of *match lovers* is fairly consistent with Hypothesis 1—with the exception of the 0.5:1 match, *match lovers* tend to consistently donate amounts that fall between their donations in the no-match questions (dotted light-gray line) and their donations in the certain-match questions (solid black line). Likewise for *match haters*, with the exception of the 2:1 match rate, their average donations under uncertain matches tend to fall between their no-match and certain-match donations, consistent with Hypothesis 1. However, somewhat counterintuitively, *match haters* pass more to the charity as the match limit increases, just as *match lovers* do.

Figure 3.3 also provides evidence that donors are responsive to changes in the number of group members, N. Consistent with Hypothesis 2, all else equal, *match lovers* tend to decrease their donations when the number of group members increases.[24] The behavior of

[24] This can be seen by the fact that the first and last sections of each line tend to be downward sloping. The first section shows the change in the average amount passed when going from ($N = 5$, $w = 80$) to ($N = 10$, $w = 80$), and the last section shows the change when going from ($N = 5$, $w = 120$) to ($N = 10$, $w = 120$). The fact that the last two points tend to be higher than the first two points, resulting in the

Figure 3.3: A comparison of match lovers and haters



Notes: Left column: *match lovers*. Right column: *match haters*. Top row: 0.5:1 match. Middle row: 1:1 match. Bottom row: 2:1 match. *Match lovers* are subjects whose average out-of-pocket donations are higher for certain-match questions than no-match questions, and vice versa for *match haters*. *Match lovers* and *match haters* are defined at the match-rate level—that is, the same subject may be a *match lover* at one match rate (e.g., a 1:1 match) and a *match hater* at a different match rate (e.g., a 2:1 match). At the 0.5:1 match rate, there are 54 *match lovers* and 33 *match haters*. At the 1:1 match rate, there are 61 *match lovers* and 35 *match haters*. At the 2:1 match rate, there are 71 *match lovers* and 34 *match haters*.

match haters, however, is less consistent. At the 0.5:1 match rate, they increase their average donations under uncertain matches as the group size increases (consistent with Hypothesis 2), but at the 1:1 match rate they decrease their donations as the group size increases. Furthermore, both *match lovers* and *match haters* appear to adjust their donations in the no-match and certain-match questions in response to changes in group size, which is inconsistent with the predictions of the theoretical model.[25]

Although *match haters* appear to decrease their donations under 1:1 matches as the group size increases, the regression results provided in Table 3.5 show that in general this behavior is not significant. Table 3.5 reports the results of regressions run separately for *match lovers* and *match haters*. However, unlike in the preceding analysis in which *match lovers* and *haters* were classified at the subject-match rate level, in Table 3.5 they are classified at the subject level. This is done by averaging each subject's donations in *all* certain-match questions (i.e., including the 0.5:1 certain-match questions, the 1:1 certain-match questions, etc.) and comparing the average to their average donation in the no-match questions. From Table 3.4 we know that donors move between donor types as the match rate varies, so averaging donations across all match rates in this way likely introduces additional noise.[26] Thus, Table 3.5 includes additional results based on stricter classifications—*super match lovers* and *haters*—which only include subjects who are classified as *match lovers* at every match rate or, respectively, as *match haters* at every match rate. This of course significantly reduces the number of observations, though. There are only 39 subjects classified as *super*

middle section being upward sloping, reflects the wealth effect that results from the higher endowment in the last two points relative to the first two points ($w = 120$ versus $w = 80$, respectively).

[25] Recall that the total donations of others are taken to be exogenous, and therefore only affect the donor's utility through their impact on the donor's perceived probability of being matched. Therefore, for the no-match and certain-match questions—where the probability of receiving a match cannot be affected—changes in group size shouldn't enter the donor's utility function.

[26] That being said, it appears that most of the movement is from the *match ignorer* group into the *match lover* group, since the number of *match haters* remains essentially constant. Though the predictions derived from the theoretical model are based on classifying *match lovers* and *haters* at the donor-match rate level, if a subject sometimes acts as a *match ignorer* and other times acts as a *match lover*, it does not seem unreasonable to consider them to be a *match lover* in general. Of course, if they sometimes act as a *match hater*, then labeling them a *match lover* would be troubling, and I cannot with certainty rule this possibility out based on the data provided in Table 3.4 alone.

match lovers, and a measly 13 subjects who are classified as *super match haters*.[27]

In general, *match haters* are not responsive to changes in group size.[28] However, consistent with what we observe in Figure 3.3, the estimated coefficients on the interaction terms between price and match limit are negative for *match haters* and *super match haters*, reflecting the fact that *match haters* increase their donations as the match limit increases. However, the coefficients on match limit are also negative and statistically significant across models, reflecting the fact that they still donate less when a match is offered, relative to the no-match questions.

Furthermore, consistent with our analysis thus far, Table 3.5 shows that *match lovers* increase their donations in response to increases in the match limit, and decrease their donations in response to increases in group size. Moreover, these effects work predominantly through price, affecting the response to matches. Thus, it appears the behavior of *match lovers* is largely consistent with Hypotheses 1 and 2.

3.4.4 Checking for stage-order effects

Table 3.6 reports regression results for regressions estimated separately for each stage of the experiment. The Stage 1 models, reported in columns (1) and (5), are estimated using data only from the first stage that each subject completed, precluding any order effects. Column (1) shows that subjects are responsive to price changes, and on average they increase their gross donations as the match limit increases. However, the coefficient estimates for endowment and group size are not statistically significant, due to the lack of within-subject variation.

Comparing columns (1) through (4), there don't appear to be significant changes in

[27]There are also 38 subjects who would be classified as *super match ignorers*, since they (on average) give the same amount in the no-match questions as in the certain-match questions at each match rate. These 38 subjects include the 20 who always pass the same amount in every question, but they also include 18 subjects who pass different amounts but on average are unresponsive to all certain matches. Combined, the *super match lovers*, *haters*, and *ignorers* account for 60% of all subjects (90 out of 150).

[28]Subjects who are classified as *super haters* do appear to pass more on average as group size increases. However, when an interaction term between price and group size is included, this effect disappears, suggesting any response to group size does not influence the effectiveness of price changes.

Table 3.5: Comparing Match Lovers and Haters

Pass	Lovers	Haters	Lovers	Haters	Super Lovers	Super Haters	Super Lovers	Super Haters
Endowment	0.653***	0.600***	0.658***	0.604***	0.700***	0.579***	0.704***	0.584***
	(0.071)	(0.067)	(0.071)	(0.066)	(0.077)	(0.129)	(0.077)	(0.129)
Price	-0.840***	0.157***	-0.170	0.752*	-0.339***	0.596***	0.291	2.152***
	(0.055)	(0.051)	(0.454)	(0.427)	(0.059)	(0.100)	(0.495)	(0.828)
Match limit	0.171***	-0.000	-0.016	-0.143**	0.162***	-0.231***	0.003	-0.443***
	(0.021)	(0.020)	(0.064)	(0.060)	(0.023)	(0.039)	(0.069)	(0.116)
Group size	-0.244***	-0.007	-0.035	0.123	-0.309***	0.156**	-0.151	0.117
	(0.042)	(0.039)	(0.094)	(0.088)	(0.045)	(0.076)	(0.101)	(0.171)
Match (=1)	-0.826***	-0.164	0.217	0.631*	-0.297*	1.323***	0.590	2.507***
	(0.144)	(0.135)	(0.366)	(0.342)	(0.156)	(0.262)	(0.396)	(0.663)
PxML			-0.229***	-0.175**			-0.196**	-0.260*
			(0.074)	(0.069)			(0.080)	(0.134)
PxN			0.300**	0.188			0.228*	-0.065
			(0.123)	(0.116)			(0.134)	(0.225)
Constant	-0.675*	0.560	-1.104***	0.290	-0.407	0.462	-0.733*	0.514
	(0.374)	(0.343)	(0.408)	(0.376)	(0.391)	(0.689)	(0.429)	(0.749)
Observations	3458	2106	3458	2106	2028	663	2028	663

Notes: Standard errors in parentheses. All columns report results of Tobit log-log regressions of the out-of-pocket donation, accounting for censoring from below and above. *Lover* and *Hater* columns report results from regressions restricted to subjects who are classified as *match lovers* and *match haters*, respectively. Subjects are classified as *lovers* if their average pass amount across all certain-match questions (including all match rates and stages) is greater than their average pass amount in all no-match questions (including all stages). *Haters* are defined conversely. Subjects are classified as *Super Lovers* if they are classified as *match lovers* at all three match rates (0.5:1, 1:1, and 2:1). Whereas the *Lovers* and *Haters* columns require that subjects be *match lovers* or *match haters* on *average*, the *Super Lovers* and *Super Haters* columns require that they display *match loving* or *match hating* behavior within each match rate. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

behavior between stages. The coefficients on price and match limit are stable across stages, and in every stage the lack of within-subject variation in endowment and group size results in statistically insignificant estimates. However, when comparing the models in columns (5) through (8), the story is different. The match limit affects how subjects respond to price changes in columns (6), (7), and (8), and group size also affects how subjects respond to price changes in columns (6) and (8). However, neither the match limit nor group size are estimated to have a statistically significant effect on the response to price changes in column (5). That is, these effects appear to develop only after subjects have completed the first stage.

Table 3.6: Regressions by Stage

Gross don.	Stage 1 (1)	Stage 2 (2)	Stage 3 (3)	Stage 4 (4)	Stage 1 (5)	Stage 2 (6)	Stage 3 (7)	Stage 4 (8)
Endowment	1.619	0.100	2.093*	-1.286	1.621	0.102	2.182*	-1.283
	(0.998)	(1.068)	(1.170)	(1.114)	(0.997)	(1.067)	(1.271)	(1.113)
Price	-1.343***	-1.278***	-1.307***	-1.295***	-1.579**	-0.849	0.362	-0.918
	(0.076)	(0.075)	(0.069)	(0.075)	(0.640)	(0.614)	(0.567)	(0.632)
Match limit	0.098***	0.065**	0.089***	0.073**	0.064	-0.141	-0.107	-0.118
	(0.030)	(0.028)	(0.026)	(0.030)	(0.091)	(0.086)	(0.079)	(0.090)
Group size	-0.514	-0.013	-0.115	0.127	-0.344	0.330	-0.210	0.453
	(0.599)	(0.639)	(0.691)	(0.664)	(0.611)	(0.647)	(0.735)	(0.674)
Match (=1)	-0.481**	-0.425**	-0.509***	-0.365*	-0.291	0.721	0.586	0.695
	(0.202)	(0.194)	(0.178)	(0.200)	(0.518)	(0.493)	(0.451)	(0.514)
PxML					-0.042	-0.252**	-0.238***	-0.233**
					(0.104)	(0.099)	(0.090)	(0.103)
PxN					0.243	0.485***	-0.181	0.464***
					(0.173)	(0.169)	(0.157)	(0.171)
Constant	-2.583	3.159	-5.968	6.987	-2.927	2.474	-6.293	6.338
	(5.689)	(6.365)	(7.130)	(6.791)	(5.688)	(6.383)	(7.976)	(6.778)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1950	1950	1950	1872[a]	1950	1950	1950	1872[a]

Notes: Standard errors in parentheses. All columns report the results of log-log Tobit regressions for the total donation received by the charity (assuming a match is received), accounting for censoring from below and above. Each stage of the experiment is estimated separately. In this table, the *stage* refers to the order in which the subject completed the experiment, not to a particular group size/endowment combination. For example, the Stage 1 model restricts observations to only those obtained in the first stage completed by each subject.

[a] Due to a small error in the experiment code when the experiment was first published, 6 participants were presented with the same group size and endowments in the third and fourth stages of the experiment. Because of this, the observations from the fourth stage were dropped for these subjects, reducing the total number of observations in the fourth stage by 78 (13 fourth-stage questions times 6 participants).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Tables C.4 and C.5, I repeat the analysis using a combined regression that includes a full set of stage-dummy interaction terms. This allows the model to use within-subject variation to produce more precise estimates of the coefficients on endowment and group size, while still estimating separate coefficients for each stage. With the added power, the coefficients on endowment are now highly statistically significant and consistent with the baseline results reported in Table 3.3. Furthermore, the same pattern of finding statistically significant coefficient estimates for group size, and the interaction between price and group size, still exists. Wald tests of joint equality across stages show statistically significant differences in the coefficients on group size in both Table C.4 (p-value = .000) and Table C.5 (p-value = .000). For the model including interaction terms between price and group size, reported in Table C.5, a Wald test of joint equality across stages produces a p-value of .056. The comparisons across stages suggest there may be a stage-order effect with respect to the effect of group size. While it also appears there may be an effect on the response to the match limit as well, this difference is not found to be statistically significant.

It seems plausible that the effect of group size on the response to matches might change as subjects are exposed to additional stages. Subjects may be unable to form clear expectations regarding the probability of receiving a match, and they may have little sense of how the number of group members might affect the match probability. As the number of group members changes between stages, though, the original stage may serve as an anchor, with subjects adjusting their behavior in recognition of the impact that the change in group size has on the probability of receiving a match.

However, upon closer inspection of the estimates for each stage, it can be seen that the statistical difference is driven by Stage 3. A Wald test of the joint equality of the coefficients on group size in Table C.5, excluding Stage 3, produces a p-value of .228. Importantly, the coefficient estimates for Stages 2 and 4 are very similar, challenging the notion that subjects are learning across stages. Furthermore, going from Stage 1 to Stage 2, the change in coefficient estimates on group size and PxN move in the opposite direction as the change going from Stage 2 to Stage 3. Again, this challenges the notion that subjects are learning. Instead, it appears that the behavior in Stage 3 is anomalous and does not follow a trend consistent with a continuous shift in subject behavior.

Table C.6, which lists the average pass amounts for each match rate in each stage, finds no statistical difference in the average donations in each stage. However, when the analysis controls for the group size and endowments faced by subjects in each stage, it becomes clear that behavior in Stage 3 is significantly different from the other stages. Table C.10 provides the average pass amounts across stages at each match rate, with separate tables for each group size/endowment combination. Looking at this table, it becomes clear that the differences between stages are driven by subjects who are presented with a group size/endowment combination of (N=5, Endow=120) or (N=10, Endow=80) in Stage 3. Subjects who are presented with the (N=5, Endow=120) combination in Stage 3 give significantly more than subjects who encounter the combination in other stages. Conversely, subjects who encounter the (N=10, Endow=80) combination in Stage 3 give significantly less than subjects who encounter the combination in other stages. This behavior can also be seen in Figure C.3, which plots the values listed in Table C.10. The (N=10, Endow=80) graph, shown in the top-right panel, shows a significant drop in average donations in Stage 3 relative to the other stages. The (N=5, Endow=120) graph, shown in the bottom-left panel, shows a significant peak in average donations in Stage 3.

Figure C.2 plots the average amount passed by subjects at each match rate and match limit, graphed separately for each stage. Average behavior in Stage 3 does not appear qualitatively different from the behavior in Stages 2 and 4. Interestingly, it is Stage 1's behavior that appears different. Whereas the average amounts passed given a low match limit are lower than the no-match average for every match rate in Stages 2 through 4, this pattern does not hold in Stage 1. Instead, in Stage 1 average donations increase monotonically in the match limit for all match rates, until decreasing in response to certain matches at the 0.5:1 and 1:1 rates.

Figure C.1 plots the average pass amounts in separate graphs for each of the different stages, as defined by the number of group members and endowments. It is strange the extent to which average behavior in the (N=5, endow=80) stage (displayed in the top-left panel of Figure C.1) is similar to the average behavior of subjects in Stage 1 (displayed in the top-left panel of Figure C.2). Because the order of stages is randomized for each subject, subjects were presented with all four different stages (i.e., group size and endowment combinations) in Stage 1. Thus, it is unclear why average behavior appears so similar between the (N=5, endow=80) stage and Stage 1.

3.5 Conclusion

This study builds upon our previous model of charitable giving, where donors perceive match subsidies as uncertain and form beliefs about the total giving of others to estimate their probability of receiving a match. Our earlier findings demonstrated that donors respond predictably to exogenous changes in match probabilities, aligning closely with our theoretical predictions.

In extending this work, we focused on endogenous changes in match probabilities resulting from variations in fundraiser characteristics, such as the number of potential donors and their wealth levels. By developing a theoretical model and designing an online laboratory experiment, we tested how changes in match rate and match limit, along with group size and endowment variations, influence donor behavior.

The experiment revealed that donors are responsive to endogenous changes in match probabilities. Consistent with our theoretical model, donations increased with higher match limits and smaller group sizes, confirming that donors' perceived probabilities of receiving a match play a critical role in their decision-making process.

Overall, this research provides significant insights into why people give and offers practical guidance for designing effective fundraisers. By understanding how donors form beliefs about match probabilities based on fundraiser characteristics, charities can optimize match schemes to maximize donations, thereby enhancing the efficiency and impact of their fundraising efforts.

Appendix A: Appendix for Chapter 1

A.1 Additional Tables

Summary Statistics	Third party		Tax		$H_0 : \mu_1 = \mu_2$
	μ_1	σ_1	μ_2	σ_2	p-value†
Age	21.06	1.904	21.21	1.928	.498
Knowledge of charity	1.01	1.996	1.38	2.325	.134
Understanding of task	8.24	2.175	7.70	2.428	.043
SEX					
Female	.61	.489	.66	.473	.309
Male	.38	.486	.32	.468	.308
Other	.01	.116	.01	.114	.978
INCOME					
Don't know/Prefer not to answer	.14	.343	.10	.299	.326
Less than $50,000	.14	.350	.14	.346	.925
Between $50,000 and $75,000	.13	.336	.08	.271	.160
Between $75,000 and $100,000	.08	.274	.14	.346	.113
Between $100,000 and $150,000	.18	.382	.24	.426	.190
Between $150,000 and $200,000	.17	.376	.17	.372	.917
More than $200,000	.16	.370	.14	.346	.560
POLITICS					
Prefer not to say	.05	.227	.05	.224	.956
Unsure/Undecided	.13	.336	.07	.260	.107
Liberal	.52	.500	.56	.497	.497
Moderate	.24	.426	.21	.409	.589
Conservative	.06	.240	.11	.308	.162
RELIGION					
Not important	.47	.499	.42	.493	.365
Somewhat important	.22	.413	.29	.455	.144
Important	.16	.370	.16	.366	.919
Very important	.15	.357	.13	.339	.670
RECENT DONATIONS					
Less than $5	.37	.482	.31	.463	.307
Between $5 and $10	.13	.336	.15	.353	.680
Between $10 and $20	.10	.303	.17	.372	.107
More than $20	.40	.490	.38	.485	.673
Observations	147		151		

† Reported p-values are based on two-tailed Welch's t-tests.

Table A.1: Summary demographic data for third-party and tax experiments

Summary Statistics	Third-party		Tax		Alt-tax		Con-tax	
	μ_1	σ_1	μ_2	σ_2	μ_3	σ_3	μ_4	σ_4
Age	21.06	1.904	21.21	1.928	22.09	3.093	22.71	3.193
Knowledge of charity	1.01	1.996	1.38	2.325	1.90	2.747	1.49	2.416
Understanding of task	8.24	2.175	7.70	2.428	7.56	2.071	7.77	1.964
SEX								
Female	.61	.489	.66	.473	.58	.496	.47	.501
Male	.38	.486	.32	.468	.41	.493	.53	.501
Other	.01	.116	.01	.114	.01	.117	.01	.083
INCOME								
Don't know/Prefer not to answer	.14	.343	.10	.299	.15	.361	.21	.410
Less than $50,000	.14	.350	.14	.346	.30	.459	.31	.463
Between $50,000 and $75,000	.13	.336	.08	.271	.14	.347	.14	.345
Between $75,000 and $100,000	.08	.274	.14	.346	.15	.361	.11	.313
Between $100,000 and $150,000	.18	.382	.24	.426	.12	.324	.12	.330
Between $150,000 and $200,000	.17	.376	.17	.372	.08	.267	.05	.228
More than $200,000	.16	.370	.14	.346	.06	.243	.05	.228
POLITICS								
Prefer not to say	.05	.227	.05	.224	.08	.277	.08	.276
Unsure/Undecided	.13	.336	.07	.260	.19	.392	.26	.440
Liberal	.52	.500	.56	.497	.37	.484	.35	.478
Moderate	.24	.426	.21	.409	.33	.471	.27	.448
Conservative	.06	.240	.11	.308	.03	.184	.03	.182
RELIGION								
Not important	.47	.499	.42	.493	.33	.473	.28	.451
Somewhat important	.22	.413	.29	.455	.30	.459	.35	.478
Important	.16	.370	.16	.366	.19	.392	.24	.428
Very important	.15	.357	.13	.339	.18	.386	.13	.338
RECENT DONATIONS								
Less than $5	.37	.482	.31	.463	.21	.408	.28	.451
Between $5 and $10	.13	.336	.15	.353	.24	.430	.27	.444
Between $10 and $20	.10	.303	.17	.372	.18	.386	.20	.400
More than $20	.40	.490	.38	.485	.37	.484	.25	.436
Observations	147		151		144		146	

Table A.2: Summary demographic data for each experiment

	Net Income	Price	Total Donation rebate	match	rebate=match p-value†
Mean	80	1	23.66		
Std. err.			1.77		
N			147		
Mean	80	.67	29.92	36.18	.000
Std. err.			2.01	2.66	
N			147	147	
Mean	80	.5	34.8	54.38	.000
Std. err.			2.19	3.96	
N			147	147	
Mean	120	1	35.86		
Std. err.			2.69		
N			147		
Mean	120	.67	46.91	58.62	.000
Std. err.			3.08	4.10	
N			147	147	
Mean	120	.5	53.83	85.16	.000
Std. err.			3.42	6.09	
N			147	147	

†Reported p-values are for Wilcoxon matched-pairs signed-rank tests of equality.

Table A.3: Total donations (in Tokens) in the third-party experiment

Regression Results: random effects tobit maximum likelihood

Dependent variable=ln(total donation received by charity (in $'s) + .01)

Variable	(1) EG results Coefficient (standard error) [Elasticity]	(2) Third-party Coefficient (standard error) [Elasticity]	$H_0 : (1) = (2)$ (p-value) two-tailed†
Constant (β_0) (no subsidy)	-1.557** (.458)	-2.740** (.731)	 (.170)
Constant ($\beta_0 + \beta_6$) (rebate subsidy)	-1.101** (.232)	-2.588** (.581)	 (.017)
Constant ($\beta_0 + \beta_7$) (match subsidy)	-.987** (.273)	-3.338** (.577)	 (.000)
Endowment (β_1) (no subsidy)	1.100** (.223) [1.030]	1.025** (.294) [.704]	 (.839)
Endowment ($\beta_1 + \beta_2$) (rebate subsidy)	.895** (.128) [.838]	1.065** (.209) [.732]	 (.488)
Endowment ($\beta_1 + \beta_3$) (match subsidy)	.820** (.138) [.767]	1.317** (.207) [.904]	 (.046)
Rebate price (β_4)	-.364 (.187) [-.340]	-.958** (.295) [-.658]	 (.089)
Match price (β_5)	-1.140** (.185) [-1.067]	-1.630** (.292) [-1.120]	 (.156)
Subjects	168	147	
Observations	2016	1470	

*, ** Significant at the 5% and 1% level, two-tailed test.
† Two-tailed Welch's t-test of equality.

Table A.4: Eckel and Grossman (2003) and baseline (third-party) experiment results

	Net Income	Price	Total Donation rebate	match	rebate=match p-value†
Mean	80	1	27.44		
Std. err.			1.71		
N			151		
Mean	80	.67	40.38	44.45	.002
Std. err.			2.89	2.88	
N			151	151	
Mean	80	.5	51.1	57.05	.002
Std. err.			3.72	3.68	
N			151	151	
Mean	120	1	39.95		
Std. err.			2.66		
N			151		
Mean	120	.67	56.77	63.24	.000
Std. err.			4.20	4.07	
N			151	151	
Mean	120	.5	76.76	84.62	.003
Std. err.			5.73	5.69	
N			151	151	

†Reported p-values are for Wilcoxon matched-pairs signed-rank tests of equality.

Table A.5: Total donations (in Tokens) in the tax experiment

	Net Income	Price	Subsidy	Total Donation Third Party	Total Donation Tax	Third Party=Tax p-value†
Mean	80	1		23.66	27.18	.055
Std. err.				1.77	1.68	
N				147	151	
Mean	80	.67	match	36.18	44.45	.036
Std. err.				2.66	2.88	
N				147	151	
Mean	80	.67	rebate	29.92	40.38	.049
Std. err.				2.01	2.89	
N				147	151	
Mean	80	.5	match	54.38	57.05	.399
Std. err.				3.96	3.68	
N				147	151	
Mean	80	.5	rebate	34.8	51.1	.014
Std. err.				2.19	43.72	
N				147	151	
Mean	120	1		35.86	39.83	.125
Std. err.				2.69	2.57	
N				147	151	
Mean	120	.67	match	58.62	63.24	.374
Std. err.				4.10	4.07	
N				147	151	
Mean	120	.67	rebate	46.91	56.77	.311
Std. err.				3.08	4.20	
N				147	151	
Mean	120	.5	match	85.16	84.62	.876
Std. err.				6.09	5.69	
N				147	151	
Mean	120	.5	rebate	53.83	76.76	.042
Std. err.				3.42	5.73	
N				147	151	

† Wilcoxon rank-sum test with exact p-value.

Table A.6: Comparison of total donations (in Tokens) in the third-party and tax experiments

	Third Party		Tax		Total		$\mu_1 = \mu_2$
	count	pct. (μ_1)	count	pct. (μ_2)	count	pct.	p-value†
Not constrained	119	.81	103	.68	222	.74	.011
Constrained	28	.19	48	.32	76	.26	
Total	147		151		298		

† Two-tailed Welch's t-test.

Table A.7: Number of subjects who are (or would be) constrained by a third-party rebate

	Net Income	Price	Tax Rate	Total Donation rebate	Total Donation match	rebate=match p-value†
Budget	80	1	.33	24.35		.355
Std. err.				1.67		
N				144		
Budget	80	1	.5	25.38		
Std. err.				1.77		
N				144		
Budget	80	.67	.33	29.97	38.56	.000
Std. err.				1.90	2.48	
N				144	144	
Budget	80	.5	.5	35.9	56.75	.000
Std. err.				2.18	3.72	
N				144	144	
Budget	120	1	.33	39.76		.116
Std. err.				2.77		
N				144		
Budget	120	1	.5	38.24		
Std. err.				2.72		
N				144		
Budget	120	.67	.33	44.83	63.81	.000
Std. err.				2.94	4.14	
N				144	144	
Budget	120	.5	.5	49.66	85.82	.000
Std. err.				3.21	5.79	
N				144	144	

†Reported p-values are for Wilcoxon matched-pairs signed-rank tests of equality.

Table A.8: Total donations (in Tokens) in the alt-tax experiment.

	Net Income	Price	Subsidy	Total Donation Third-party	Alt-Tax	Third-party=Alt-tax p-value†
Budget	80	1		23.66	24.35	.777
Std. err.				1.77	1.67	
N				147	144	
Budget	80	.67	match	36.18	38.56	.514
Std. err.				2.66	2.48	
N				147	144	
Budget	80	.67	rebate	29.92	29.97	.985
Std. err.				2.01	1.90	
N				147	144	
Budget	80	.5	match	54.38	56.75	.663
Std. err.				3.96	3.72	
N				147	144	
Budget	80	.5	rebate	34.8	35.9	.713
Std. err.				2.19	2.18	
N				147	144	
Budget	120	1		35.86	39.76	.312
Std. err.				2.69	2.77	
N				147	144	
Budget	120	.67	match	58.62	63.81	.373
Std. err.				4.10	4.14	
N				147	144	
Budget	120	.67	rebate	46.91	44.83	.625
Std. err.				3.08	2.94	
N				147	144	
Budget	120	.5	match	85.16	85.82	.937
Std. err.				6.09	5.79	
N				147	144	
Budget	120	.5	rebate	53.83	49.66	.374
Std. err.				3.42	3.21	
N				147	144	

†Reported p-values are for two-sided Welch's t-tests.

Table A.9: Comparison of total donations (in Tokens) in the third-party and alt-tax experiments

	Net Income	Price	Subsidy	Total Donation Alt-tax	Tax	Alt-tax=Tax p-value†
Budget	80	1		24.35	27.18	.179
Std. err.				1.67	1.68	
N				144	302	
Budget	80	.67	match	38.56	44.45	.123
Std. err.				2.48	2.88	
N				144	151	
Budget	80	.67	rebate	29.97	40.38	.003
Std. err.				1.90	2.89	
N				144	151	
Budget	80	.5	match	56.75	57.05	.955
Std. err.				3.72	3.68	
N				144	151	
Budget	80	.5	rebate	35.9	51.1	.000
Std. err.				2.18	3.72	
N				144	151	
Budget	120	1		39.76	39.83	.984
Std. err.				2.77	2.57	
N				144	302	
Budget	120	.67	match	63.81	63.24	.922
Std. err.				4.14	4.07	
N				144	151	
Budget	120	.67	rebate	44.83	56.77	.021
Std. err.				2.94	4.20	
N				144	151	
Budget	120	.5	match	85.82	84.62	.883
Std. err.				5.79	5.69	
N				144	151	
Budget	120	.5	rebate	49.66	76.76	.000
Std. err.				3.21	5.73	
N				144	151	

†Reported p-values are for two-sided Welch's t-tests.

Table A.10: Comparison of total donations (in Tokens) in the alt-tax and tax experiments

	Net Income	Price	Tax Rate	Total Donation rebate	Total Donation match	rebate=match p-value†
Budget	80	1	.33	24.42		
Std. err.				1.73		
N				146		
						.162
Budget	80	1	.5	23.95		
Std. err.				1.76		
N				146		
Budget	80	0.67	.33	32.77	39.55	.000
Std. err.				2.16	2.83	
N				146	146	
Budget	80	0.5	.5	36.75	52.52	.000
Std. err.				2.37	3.82	
N				146	146	
Budget	120	1	.33	34.53		
Std. err.				2.60		
N				146		
						.262
Budget	120	1	.5	35.95		
Std. err.				2.66		
N				146		
Budget	120	0.67	.33	46.77	57.80	.000
Std. err.				3.26	4.12	
N				146	146	
Budget	120	0.5	.5	52.72	78.23	.000
Std. err.				3.55	5.58	
N				146	146	

†Reported p-values are for Wilcoxon matched-pairs signed-rank tests of equality.

Table A.11: Total donations (in Tokens) in the con-tax experiment.

	Net Income	Price	Subsidy	Total Donation Third-party	Con-Tax	Third-party=Con-tax p-value†
Budget	80	1		23.66	24.18	.834
Std. err.				1.77	1.75	
N				147	146	
Budget	80	.67	match	36.18	39.55	.386
Std. err.				2.66	2.83	
N				147	146	
Budget	80	.67	rebate	29.92	32.77	.334
Std. err.				2.01	2.16	
N				147	146	
Budget	80	.5	match	54.38	52.52	.735
Std. err.				3.96	3.82	
N				147	146	
Budget	80	.5	rebate	34.76	36.75	.538
Std. err.				2.19	2.37	
N				147	146	
Budget	120	1		35.86	35.24	.870
Std. err.				2.69	2.63	
N				147	146	
Budget	120	.67	match	58.62	57.80	.888
Std. err.				4.10	4.12	
N				147	146	
Budget	120	.67	rebate	46.91	46.77	.975
Std. err.				3.08	3.26	
N				147	146	
Budget	120	.5	match	85.16	78.23	.402
Std. err.				6.09	5.58	
N				147	146	
Budget	120	.5	rebate	53.83	52.72	.822
Std. err.				3.42	3.55	
N				147	146	

†Reported p-values are for two-sided Welch's t-tests.

Table A.12: Comparison of total donations (in Tokens) in the third-party and con-tax experiments

	Net Income	Price	Subsidy	Total Donation Con-tax	Total Donation Tax	Con-tax=Tax p-value†
Budget	80	1		24.18	27.18	.215
Std. err.				1.75	1.68	
N				146	302	
Budget	80	.67	match	39.55	44.45	.225
Std. err.				2.83	2.88	
N				146	151	
Budget	80	.67	rebate	32.77	40.38	.036
Std. err.				2.16	2.89	
N				146	151	
Budget	80	.5	match	52.52	57.05	.394
Std. err.				3.82	3.68	
N				146	151	
Budget	80	.5	rebate	36.75	51.12	.001
Std. err.				2.37	3.72	
N				146	151	
Budget	120	1		35.24	39.83	.217
Std. err.				2.63	2.57	
N				146	302	
Budget	120	.67	match	57.80	63.24	.348
Std. err.				4.12	4.07	
N				146	151	
Budget	120	.67	rebate	46.77	56.77	.061
Std. err.				3.26	4.20	
N				146	151	
Budget	120	.5	match	78.23	84.62	.423
Std. err.				5.58	5.69	
N				146	151	
Budget	120	.5	rebate	52.72	76.76	.000
Std. err.				3.55	5.73	
N				146	151	

†Reported p-values are for two-sided Welch's t-tests.

Table A.13: Comparison of total donations (in Tokens) in the con-tax and tax experiments

Elasticity Estimates								
Dependent variable=total donation received by charity								
Variable	(1) Third-party Elasticity (standard error)		(2) Tax Elasticity (standard error)		(3) Alt-tax Elasticity (standard error)		(4) Con-tax Elasticity (standard error)	
Endowment	.410	.434	.408	.435	.619	.657	.459	.490
(no subsidy)	(.105)	(.110)	(.079)	(.084)	(.087)	(.091)	(.088)	(.093)
Endowment	.615	.650	.690	.735	.584	.620	.545	.582
(rebate subsidy)	(.083)	(.087)	(.085)	(.090)	(.086)	(.091)	(.090)	(.095)
Endowment	.899	.952	.744	.792	1.146	1.216	.876	.931
(match subsidy)	(.095)	(.100)	(.086)	(.091)	(.101)	(.105)	(.097)	(.102)
Rebate price	-.425	-.449	-.797	-.848	-.457	-.485	-.358	-.383
	(.104)	(.110)	(.157)	(.167)	(.160)	(.170)	(.168)	(.179)
Match price	-1.105	-1.170	-.843	-.899	-1.247	-1.323	-.909	-.966
	(.126)	(.132)	(.157)	(.167)	(.171)	(.181)	(.173)	(.184)
Tax rate			-.066	-.070	-.045	-.047	-.057	-.061
			(.184)	(.196)	(.193)	(.205)	(.203)	(.216)
Controls	N	Y	N	Y	N	Y	N	Y
Subjects	147		151		144		146	
Observations	1470		1812		1728		1752	

Table A.14: Comparison of Elasticity Estimates

A.2 Additional Figures

A.2.1 CDFs of Donations, comparing third-party and tax experiments by Budget, Subsidy Type and Price

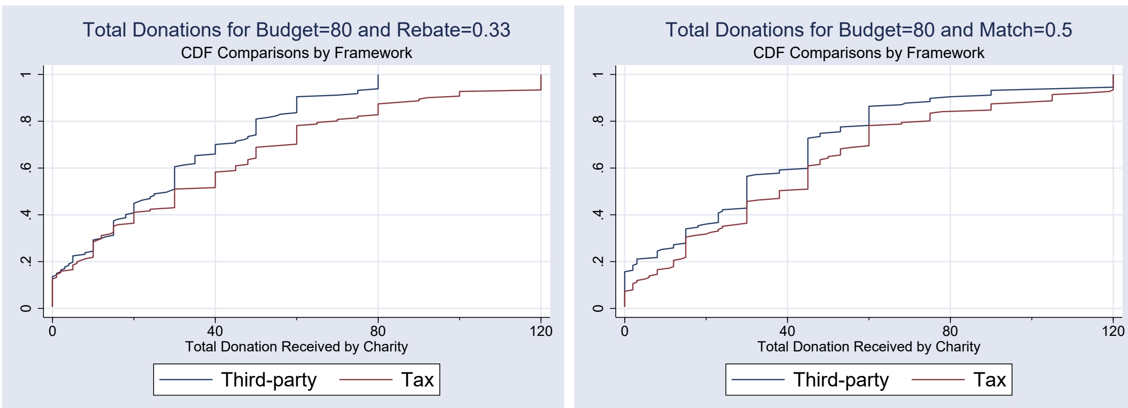


Figure A.1: CDFs of Donations for Rebate (left) and Match (right) when Price=.67 and Budget=80. Third-party in blue, Tax in red.

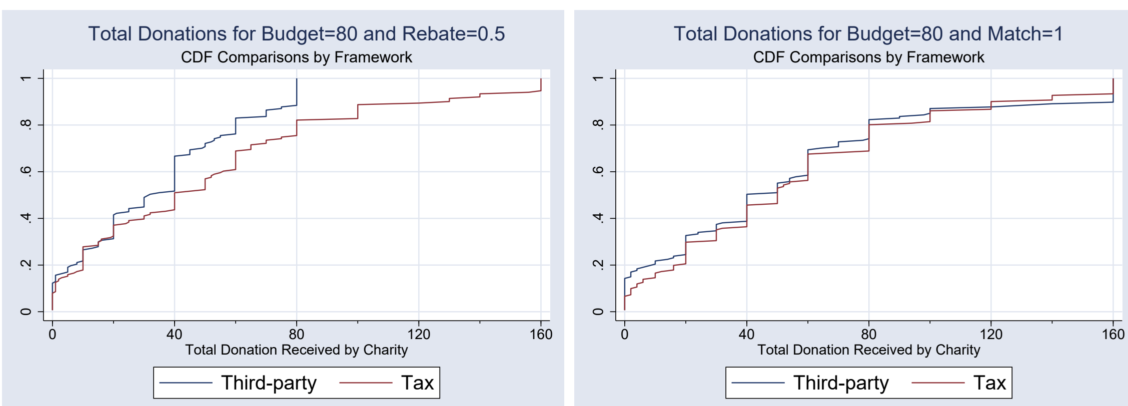


Figure A.2: CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=80. Third-party in blue, Tax in red.

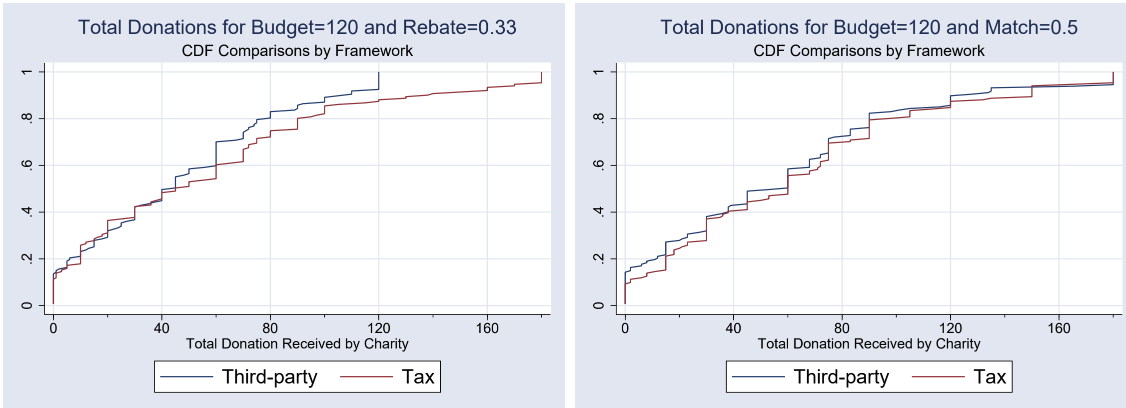


Figure A.3: CDFs of Donations for Rebate (left) and Match (right) when Price=.67 and Budget=120. Third-party in blue, Tax in red.

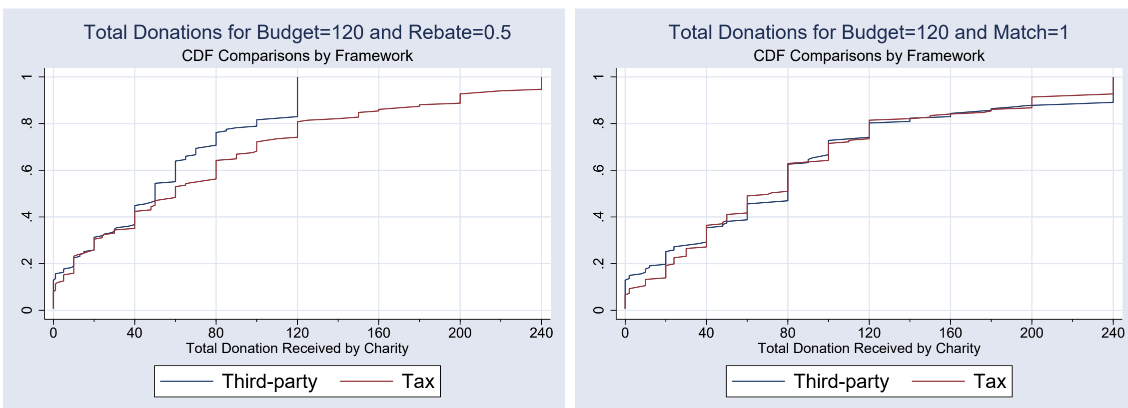


Figure A.4: CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=120. Third-party in blue, Tax in red.

A.2.2 CDFs of Donations, comparing rebates and matches by Budget, Subsidy Type and Price

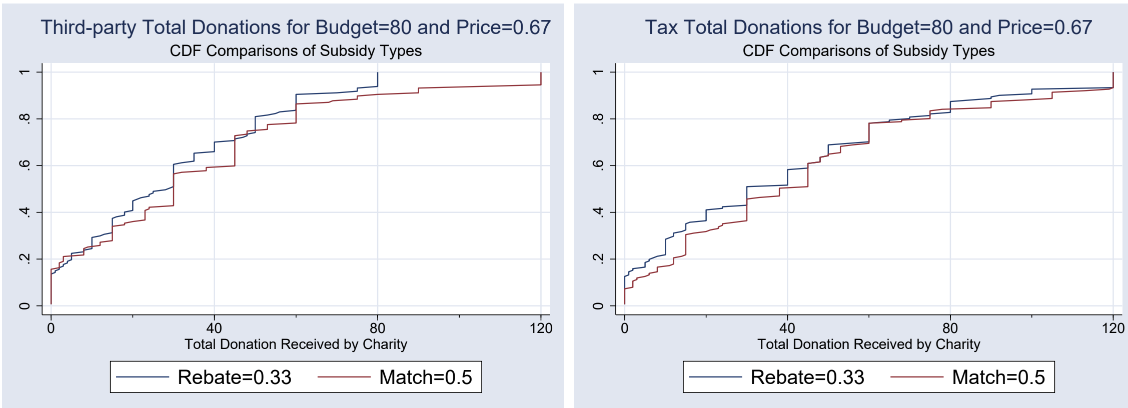


Figure A.5: CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=80. Third-party on left, Tax on right.

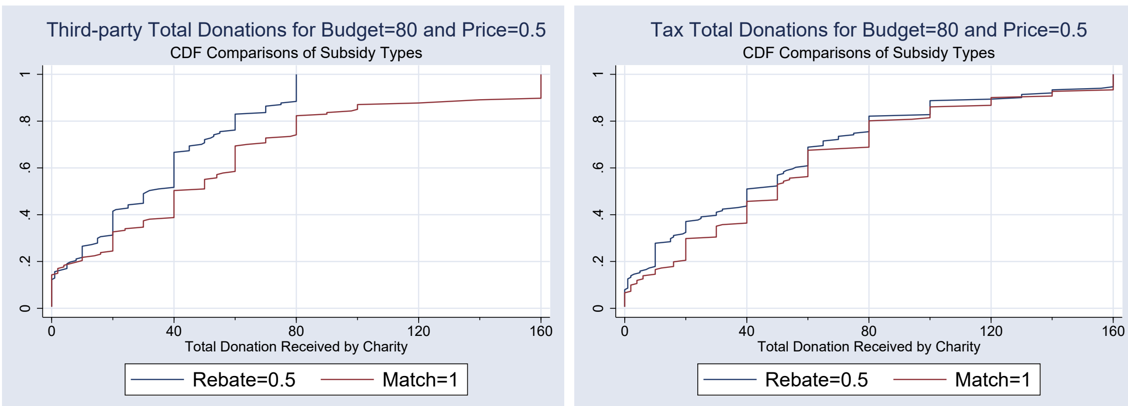


Figure A.6: CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. Third-party on left, Tax on right.

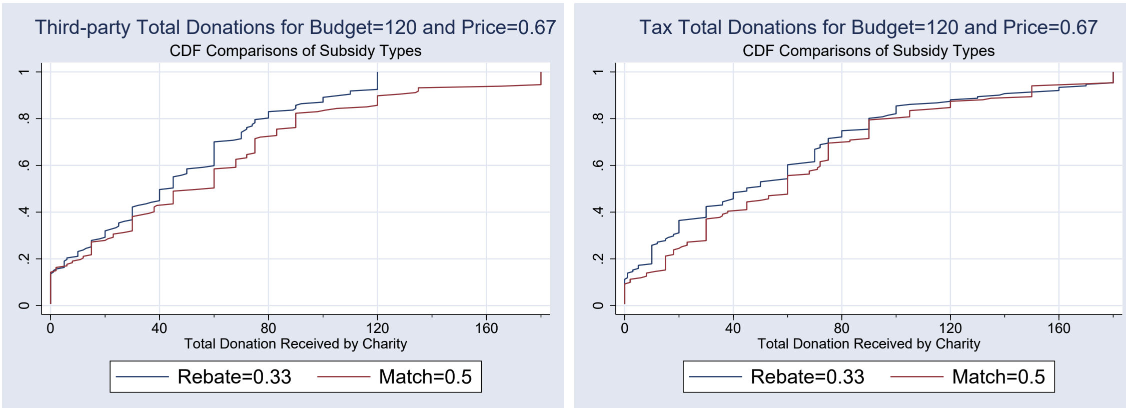


Figure A.7: CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=120. Third-party on left, Tax on right.

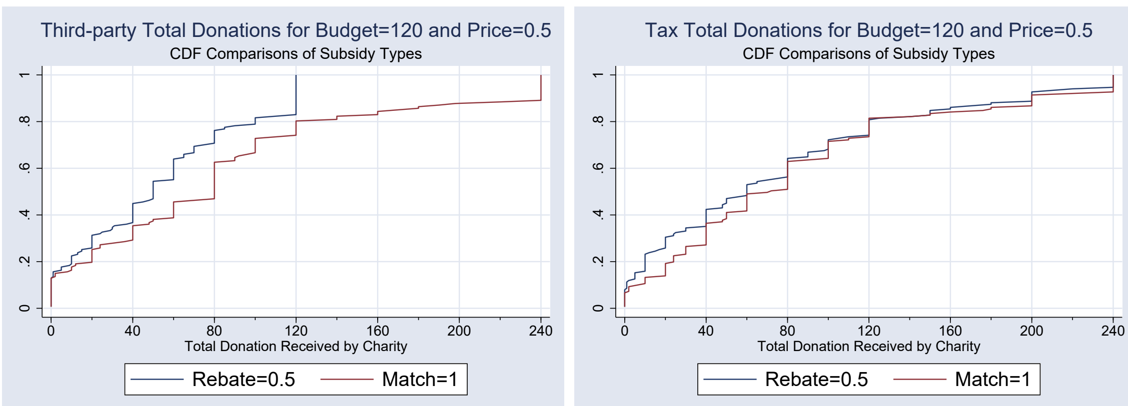


Figure A.8: CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. Third-party on left, Tax on right.

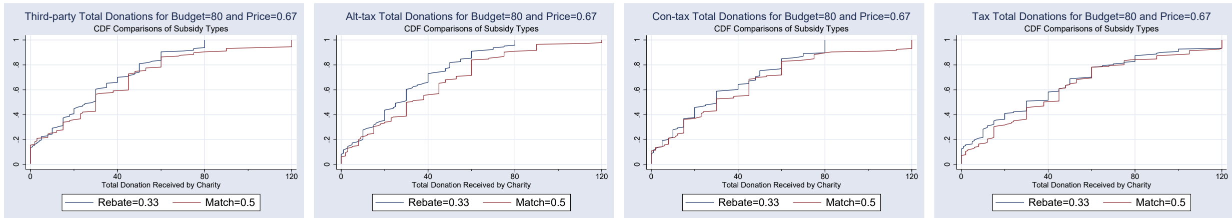


Figure A.9: CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=80. From left to right: Third-party, Alt-tax, Con-tax, Tax.

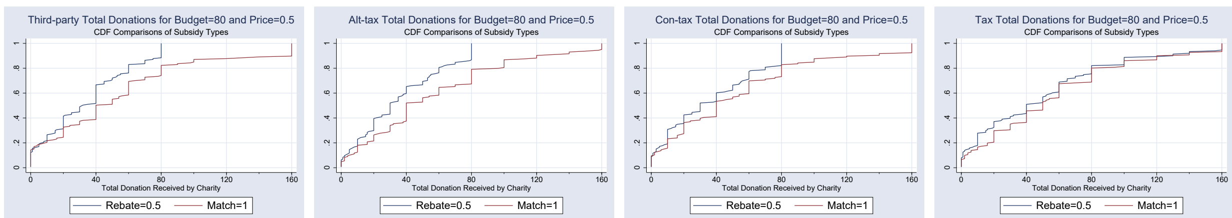


Figure A.10: CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. From left to right: Third-party, Alt-tax, Con-tax, Tax.

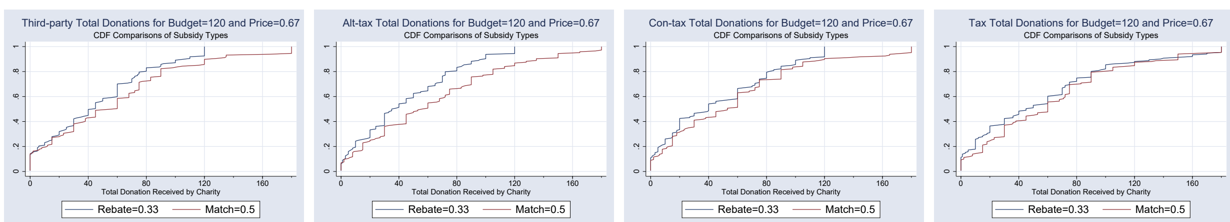


Figure A.11: CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=120. From left to right: Third-party, Alt-tax, Con-tax, Tax.

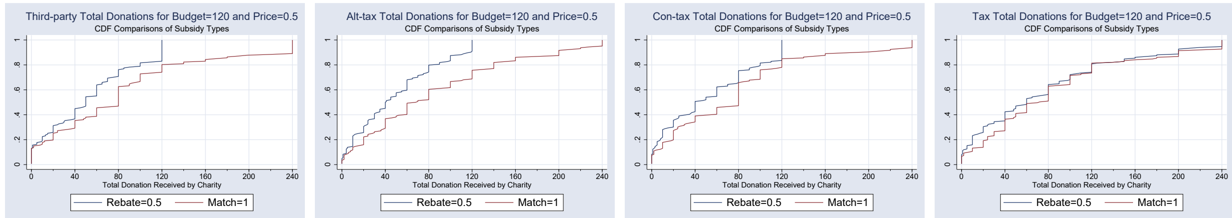


Figure A.12: CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. From left to right: Third-party, Alt-tax, Con-tax, Tax.

A.2.3 CDFs of Donations, comparing all experiments by Budget, Subsidy Type and Price

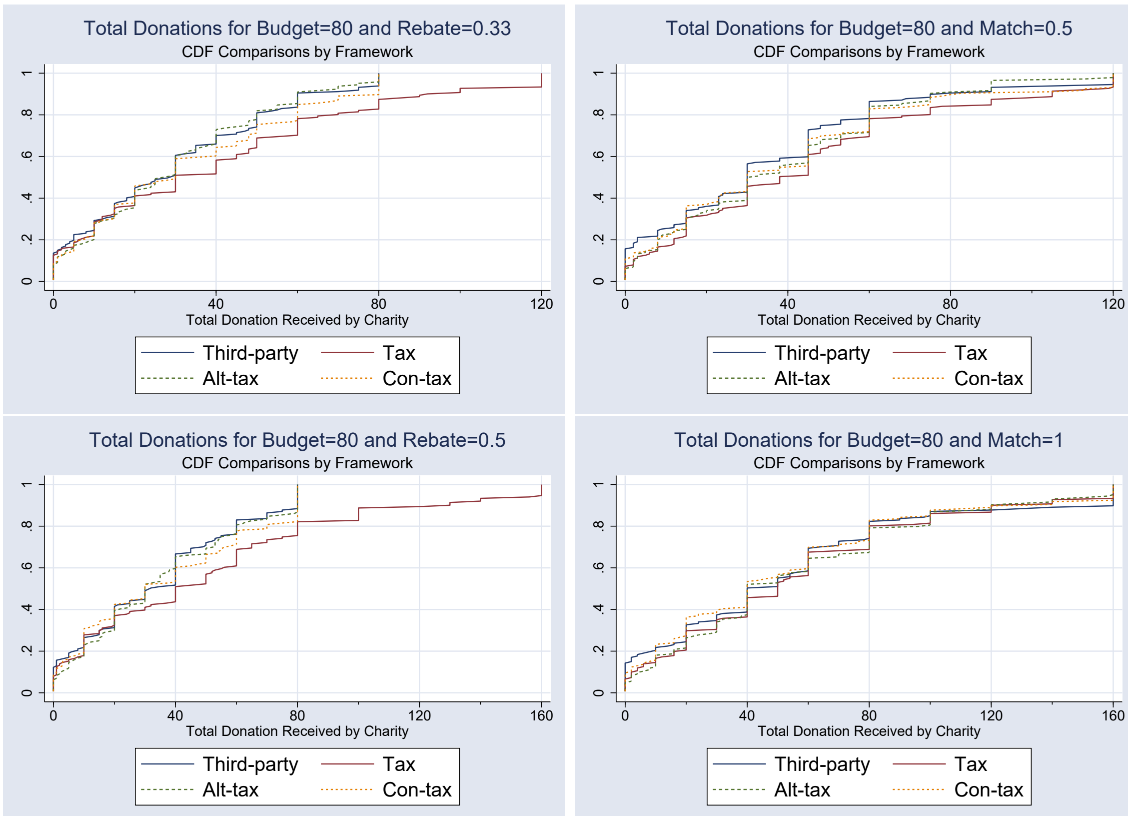


Figure A.13: CDFs of donations in all experiments for questions where Budget=80. Left-side graphs show rebate questions, right-side graphs show match questions. Top graphs show Price=0.67, bottom graphs show Price=0.5.

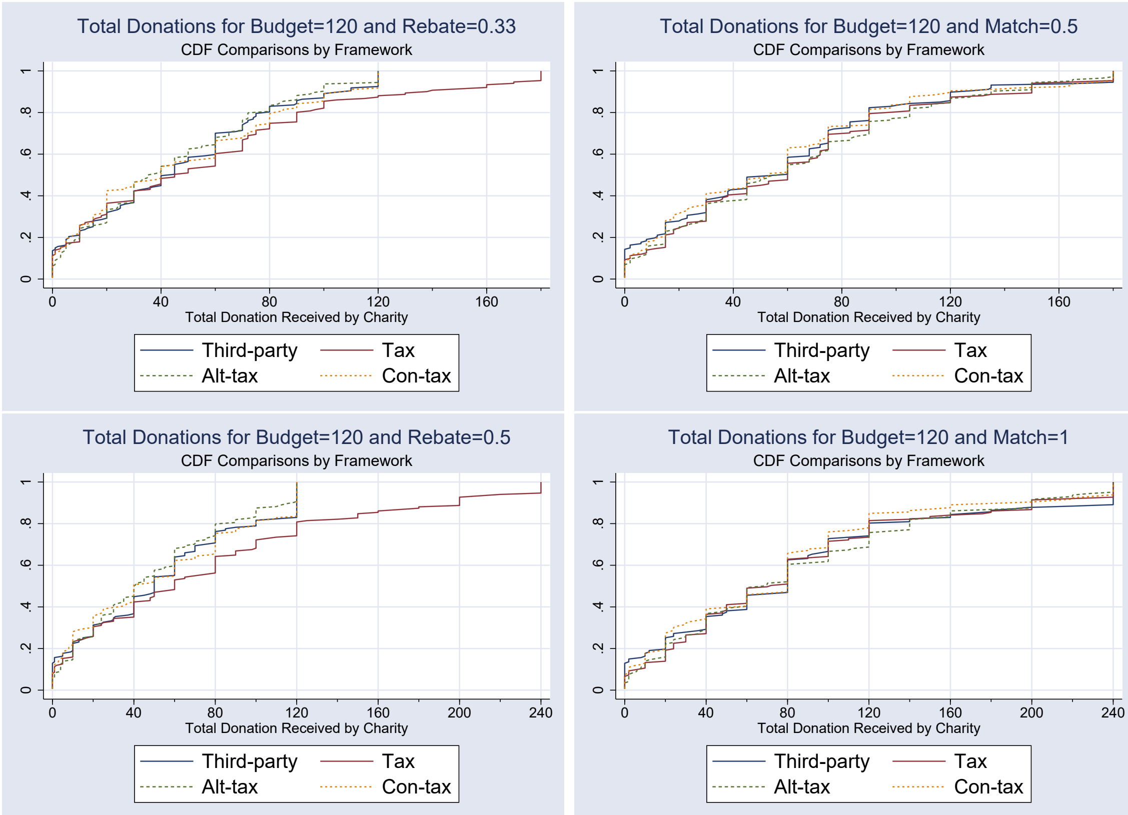


Figure A.14: CDFs of donations in all experiments for questions where Budget=120. Left-side graphs show rebate questions, right-side graphs show match questions. Top graphs show Price=0.67, bottom graphs show Price=0.5.

A.2.4 Added Information Treatment of Davis et al. (2005)

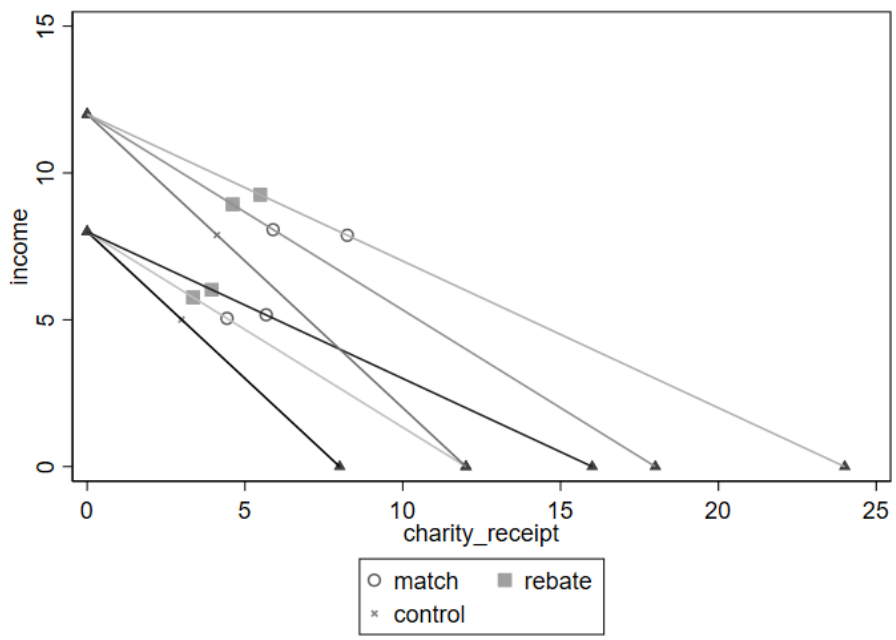


Figure A.15: Plot of Table 6 from Davis et al. (2005) – "added information" treatment.

A.3 Additional Analysis

A.3.1 Censoring cannot explain our findings

To test whether censoring can explain our results, we run the following combined model.

$$\begin{aligned}\tilde{Y}_{ij} = \alpha_0 + \beta_0 \cdot D_{ij} + \alpha_1 \cdot E_{ij} + \beta_1 \cdot E_{ij} \times D_{ij} + \alpha_2 \cdot E_{ij} \times R_{ij} + \beta_2 \cdot E_{ij} \times R_{ij} \times D_{ij} \\ + \alpha_3 \cdot E_{ij} \times M_{ij} + \beta_3 \cdot E_{ij} \times M_{ij} \times D_{ij} + \alpha_4 \cdot P_{ij} \times R_{ij} + \beta_4 \cdot P_{ij} \times R_{ij} \times D_{ij} \\ + \alpha_5 \cdot P_{ij} \times M_{ij} + \beta_5 \cdot P_{ij} \times M_{ij} \times D_{ij} + \alpha_6 \cdot R_{ij} + \beta_6 \cdot R_{ij} \times D_{ij} \\ + \alpha_7 \cdot M_{ij} + \beta_7 \cdot M_{ij} \times D_{ij} + \beta_8 \cdot T_{ij} + \nu_i + \varepsilon_{ij}, \quad \text{(A.1)}\end{aligned}$$

where D_{ij} is an indicator for the tax framework and

$$\tilde{Y}_{ij} = \begin{cases} w_{ij} & \text{if } R_{ij} = 1 \text{ and } Y_{ij} > w_{ij} \\ Y_{ij} & \text{otherwise} \end{cases}$$

The results of the combined model are essentially identical to our previous results, with the exception that the rebate-price coefficient for the tax framework (β_4) is now estimated to be -1.269 ($p = 0.000$), whereas before it was estimated to be -1.181 ($p = 0.000$). Note that this change is a result of using the censored total donation amounts $\tilde{Y}_{ij}$ and has nothing to do with using a combined model.[1] The full results of the combined model are presented in Table A.15.

We see that censoring observations in the tax experiment in the same way that they are censored in the third-party experiment does not remove the disparity in responses to

[1] Given that the tax framework indicator D_{ij} is interacted with every variable, the combined model produces essentially identical results as estimating separate models for each framework (third-party and tax). The small differences occur because the combined model considers the third-party observations to have a tax rate of 0, providing the model with slightly more information about subjects' response to a change in the tax rate. The full results of the combined model using the unconstrained total donations are provided in Table A.16.

rebates and matches. While the coefficients on the rebate price and match price in the third-party framework (α_4 and α_5, respectively) are statistically different ($p = 0.026$), in the tax framework they are still not statistically different ($p = 0.658$).[2] In fact, using the censored observations, $\tilde{Y}_{ij}$, actually *increases* the disparity in behavior between the two frameworks, causing the rebate-price estimate to be significantly *smaller* (i.e., more negative) than the match-price estimate in the tax experiment, whereas in the third-party experiment the rebate-price estimate is statistically significantly larger than the match-price estimate.

The gap between the rebate- and match-price estimates in the third-party framework is given by $\alpha_4 - \alpha_5 = 0.644$. The corresponding gap in the tax framework is given by $(\alpha_4 + \beta_4) - (\alpha_5 + \beta_5) = -0.123$. Thus, when using the censored observations $\tilde{Y}_{ij}$, the gap *between gaps* (for the third-party and tax experiments), γ, is equal to

$$\begin{aligned}\gamma &= (\alpha_4 - \alpha_5) - [(\alpha_4 + \beta_4) - (\alpha_5 + \beta_5)] \\ &= \beta_5 - \beta_4 = 0.766\end{aligned}$$

This gap is larger than the corresponding gap when using the uncensored observations (=0.683). If the constraint under rebates were simply mechanically affecting donations (i.e., if there were no behavioral effect), then censoring observations in the tax experiment should reduce the gap between gaps. The fact that it instead increases the gap between gaps indicates that the mechanical effect of the constraint under rebates cannot explain the observed differences between the third-party and tax experiments.

To further demonstrate the significance of the difference in observed behavior between the third-party and tax experiments, we construct a Fisher Exact P-value for the probability of observing a gap between gaps as large as the one observed in our data. Under the sharp null hypothesis that assignment to the tax experiment has no effect on individuals' responses to rebates and matches (other than the mechanical effect of relaxing the constraint under

[2]For the tax framework, the rebate- and match-price coefficients are given by $\alpha_4 + \beta_4$ and $\alpha_5 + \beta_5$, respectively.

rebates), we can estimate a Fisher Exact P-value for the probability of observing a γ as large as 0.766. We do this in the following way: (1) we censor any donation observations in the tax experiment that *would have been* censored in the third-party experiment (i.e., we use $\tilde{Y}_{ij}$); (2) we randomly assign individuals to either the third-party or the tax experiment; (3) based on the random assignment, we estimate the combined model given in Equation A.1; (4) using the coefficient estimates from the combined model, we then calculate γ_n for each iteration n.

Using a total of 100,000 random assignments, we estimate a Fisher Exact P-value for the probability of observing a $\gamma > 0.766$ of $p = 0.0163$. (The two-sided p-value for the probability of observing a γ *as extreme* as 0.766 is equal to $p = 0.0316$.) Based on these results, we reject the null hypothesis that donations are only mechanically affected by the constraint under rebates. Behavior is significantly different between the third-party and tax experiments. This supports that individuals have a significant *behavioral* response to the constraint under rebates in the third-party experiment.

Regression Results: random effects tobit maximum likelihood		
Dependent variable=$\tilde{Y}$		
Variable	(1) Third-party Coefficient (standard error)	(2) Tax Coefficient (standard error)
Constant (β_0)	-1.808	-.119
(no subsidy)	(.96)	(.66)
Constant ($\beta_0 + \beta_6$)	-2.089*	-.652
(rebate subsidy)	(.70)	(.68)
Constant ($\beta_0 + \beta_7$)	-2.964*	-.591
(match subsidy)	(.69)	(.65)
Endowment (β_1)	.933*	.685*
(no subsidy)	(.21)	(.14)
Endowment ($\beta_1 + \beta_2$)	1.024*	.745*
(rebate subsidy)	(.15)	(.14)
Endowment ($\beta_1 + \beta_3$)	1.185*	.800*
(match subsidy)	(.14)	(.14)
Rebate price (β_4)	-.785*	-1.269*
	(.21)	(.28)
Match price (β_5)	-1.429*	-1.147*
	(.20)	(.27)
Tax rate (β_8)		-.263
		(.33)
Subjects	147	151
Observations	1470	1812

*Significant at the 1% level, two-tailed test.

Table A.15: Combined (third-party and tax experiment) results using censored donation amounts

Regression Results: random effects tobit maximum likelihood

Dependent variable=Y

Variable	(1) Third-party Coefficient (standard error)	(2) Tax Coefficient (standard error)
Constant (β_0)	-1.806	-.112
(no subsidy)	(.94)	(.66)
Constant ($\beta_0 + \beta_6$)	-2.085*	-.765
(rebate subsidy)	(.69)	(.65)
Constant ($\beta_0 + \beta_7$)	-2.963*	-.582
(match subsidy)	(.68)	(.65)
Endowment (β_1)	.933*	.685*
(no subsidy)	(.20)	(.14)
Endowment ($\beta_1 + \beta_2$)	1.024*	.773*
(rebate subsidy)	(.14)	(.13)
Endowment ($\beta_1 + \beta_3$)	1.185*	.799*
(match subsidy)	(.14)	(.13)
Rebate price (β_4)	-.783*	-1.184*
	(.20)	(.27)
Match price (β_5)	-1.428*	-1.146*
	(.20)	(.27)
Tax rate (β_8)		-.264
		(.33)
Subjects	147	151
Observations	1470	1812

*Significant at the 1% level, two-tailed test.

Table A.16: Combined (third-party and tax experiment) results using uncensored donation amounts

A.4 Experimental Materials

A.4.1 Third-party Experiment Materials

Third-party Instructions

INSTRUCTIONS

Introduction. Thank you for participating in this online experiment. This experiment is interested in studying how individuals make decisions. You will be making decisions individually. Your decisions and earnings during the experiment will be confidential and will only be associated with an ID number.

You will be compensated for your participation. At the end of the experiment, you will receive a show-up reward of $5. This show-up reward is not contingent on the decisions that you make during the experiment, and it will be yours to keep just for participating. In addition to the show-up reward, you will also have an opportunity to earn additional money. The amount you are paid will depend on the decisions you make in the experiment and luck, as will be explained in detail below. During the experiment, your earnings will be calculated in Tokens. At the end of the experiment the total amount of Tokens you have earned will be converted to US Dollars at the following rate:

10 Tokens = 1.00 US Dollar

Your $ earnings (plus the $5 show-up reward) will be paid to you in private in the form of an **electronic Amazon gift card** within 48 hours after the completion of the experiment.

At any time, you can use the chat box in the Zoom room to ask the experimenter a question. No other participants will see your questions. The experimenter has muted everyone's microphones and turned off videos to avoid any interruptions during the experiment.

During the experiment you will be provided with opportunities to make donations to **charity: water**, a nonprofit organization that works to bring safe and clean drinking water to the nearly 800 million people in the world living without access to clean water. The majority of people without access to clean water live in isolated rural areas, and they must spend hours every day walking many miles to collect water for their families. This water often carries diseases that lead to sickness. **charity: water** works with local experts and community members to install sustainable water solutions, including wells, piped water systems, BioSand Filters, and systems for harvesting rainwater.

Allocation Decisions. In this experiment, you will be presented with 10 allocation decision problems. In each problem, you will be endowed with a certain amount of money, and you will be asked to allocate this money between yourself and *charity: water* ("the Charity"). You will do this by deciding the amount that you would like to pass to the Charity. For each decision problem, the computer will then calculate the amount that you will hold for yourself (your endowment minus the amount you pass to the Charity), your total earnings, and the total donation that will be received by the Charity. After you have made decisions for all 10 problems, only one problem will be randomly selected to be carried out, and your decision in this problem will determine your payment and the amount received by the Charity. An example of the type of allocation decisions you will be presented with is given below.

[To be read only; does not appear in subject instructions]: Please take a moment to look at the example allocation decisions. Note that for each problem you are asked to enter the number of Tokens you would like to pass to the Charity. In the first problem, you are endowed with a total of 80 Tokens. For every 1 Token you pass to the Charity, the Charity will receive 2 Tokens: your 1 Token and a matching 1 Token provided by the experimenter. After you enter the number of Tokens you would like to pass to the Charity, the remaining columns will automatically fill with the correct values. Note that the Total Donation received by the Charity (Column 5) is twice the amount that you have chosen to pass. This is because, in this question, your donation is matched 1:1 by the experimenter. In the second question, your endowment is 120 Tokens. However, your donation is not matched, and therefore the Total Donation received by the Charity is the same as the amount you choose to pass. Finally, in the third example question your endowment is 80 Tokens, and for every Token you choose to pass to the Charity, the experimenter refunds to you 0.5 Tokens. Note that the Total Donation received by the Charity in this question is equal to the number of Tokens you choose to pass. However, Your Earnings are larger than the amount you hold for yourself, since you will also be receiving a refund. [**End**]

Example Allocation Decision Problems:

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, including any applicable rebate. Your Earnings	Total donation received by the Charity, including any applicable matched funds. Total Donation
1.) You are endowed with 80 Tokens. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	25	55	55	50
2.) You are endowed with 120 Tokens. For every 1 Token you pass, the Charity will receive 1 Token.	40	80	80	40
3.) You are endowed with 80 Tokens. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.5 Tokens.				

As you enter an amount to Pass in each question, the remaining columns (Hold, Your Earnings, and Total Donation) will automatically display the corresponding values based on your decision of how much to Pass and the specific conditions listed in column (1). Note that your endowment may differ between problems. Also note that, depending on the condition listed in column (1), the amount the Charity receives (i.e., Total Donation) may not be equal to the amount you choose to Pass. Likewise, the amount you Hold may not be equal to the amount you earn (i.e., Your Earnings). It is important that you carefully make note of the endowment and conditions given for each problem when making your allocation decisions.

Once you have entered appropriate decisions for each problem, you will be able to submit your decisions. After submitting your decisions, **one problem will be randomly selected to determine your payment.** You will be obligated to pass to the Charity the amount you have entered in column (2) of the selected problem. <u>This amount, plus any matched funds provided by the experimenter (for applicable problems), will actually be donated to the Charity.</u> The Total Donation received by the Charity is given in column (5). Your choice of how much to pass to the Charity in the randomly selected decision problem, combined with the condition provided in column (1) of the selected problem, will determine Your Earnings in this experiment. This amount is given in column (4). Within 48 hours after the experimental session ends, you will be paid your earnings (plus the $5 show-up reward) in the form of an electronic Amazon gift card.

If you have any questions, please message the experimenter using the Zoom chat. If you do not have any questions, you are free to continue to the experiment.

Third-party Decision Sheet

Figure A.16 shows an example of the decision sheets presented to subjects in the *third-party* experiment.

ALLOCATION DECISION PROBLEMS

Below are 10 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how to allocate the endowment listed in column (1) between yourself and *charity: water* ("the Charity"). Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instructions.

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, including any applicable rebate. Your earnings	Total donation received by the Charity, including any applicable matched funds. Total donation
1.) You are endowed with 80 Tokens. For every 1 Token you pass, the Charity will receive 1 Token.	5	75	75	5
2.) You are endowed with 80 Tokens. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.33 Tokens.	12	68	72	12
3.) You are endowed with 80 Tokens. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.	22	58	58	33
4.) You are endowed with 80 Tokens. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.5 Tokens.	35	45	63	35
5.) You are endowed with 80 Tokens. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	45	35	35	90
6.) You are endowed with 120 Tokens. For every 1 Token you pass, the Charity will receive 1 Token.				
7.) You are endowed with 120 Tokens. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.33 Tokens.				
8.) You are endowed with 120 Tokens. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.				
9.) You are endowed with 120 Tokens. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.5 Tokens.				
10.) You are endowed with 120 Tokens. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.				

Figure A.16: Screenshot of an Example Decision Sheet for the *Third-party* Experiment in Qualtrics

Subjects were required to enter a number in the 'Pass' column (i.e., Column 4) for each problem. After entering a value into Column 4, the remaining columns (i.e., Columns 5-7) automatically fill with the correct values based on the subject's choice of how much to pass. Subjects are unable to edit the information in Columns 5-7 (except by editing the value in Column 4). Subjects may answer the decision problems in any order they like, and they are able to edit their choices up until the time they submit their responses. While subjects can enter any number they wish into Column 4 (including negative numbers and values in excess of their endowment for the problem), and Columns 5-7 will be calculated and presented to them, they will be unable to submit their responses if any of them violate their budget constraints. The subject is informed of any unacceptable responses and asked to edit them. Non-numerical responses are not possible (they are immediately edited to be blank), and subjects are required to provide a response for each problem (i.e., they may not leave any problems blank).

There are two different orders used for the 10 decision problems. Which order a subject is presented with is randomly determined for each subject. In the alternate ordering the only difference is that the rebate subsidies are shown before the match subsidies. That is, Problems 2 and 3 are swapped, Problems 4 and 5 are swapped, Problems 7 and 8 are swapped, and Problems 9 and 10 are swapped. Both orderings organize the problems first by the amount of the endowment (either 80 or 120 Tokens), and then by the price of giving (either 1, 0.67, or 0.5).

A.4.2 Tax Experiment Materials

Tax Experiment Instructions

INSTRUCTIONS

Introduction. Thank you for participating in this online experiment. This experiment is interested in studying how individuals make decisions. You will be making decisions individually. Your decisions and earnings during the experiment will be confidential and will only be associated with an ID number.

You will be compensated for your participation. At the end of the experiment, you will receive a show-up reward of $5. This show-up reward is not contingent on the decisions that you make during the experiment, and it will be yours to keep just for participating. In addition to the show-up reward, you will also have an opportunity to earn additional money. The amount you are paid will depend on the decisions you make in the experiment and luck, as will be explained in detail below. During the experiment, your earnings will be calculated in Tokens. At the end of the experiment the total amount of Tokens you have earned will be converted to US Dollars at the following rate:

10 Tokens = 1.00 US Dollar

Your $ earnings (plus the $5 show-up reward) will be paid to you in private in the form of an **electronic Amazon gift card** within 48 hours after the completion of the experiment.

At any time, you can use the chat box in the Zoom room to ask the experimenter a question. No other participants will see your questions. The experimenter has muted everyone's microphones and turned off videos to avoid any interruptions during the experiment.

During the experiment you will be provided with opportunities to make donations to **charity: water**, a nonprofit organization that works to bring safe and clean drinking water to the nearly 800 million people in the world living without access to clean water. The majority of people without access to clean water live in isolated rural areas, and they must spend hours every day walking many miles to collect water for their families. This water often carries diseases that lead to sickness. **charity: water** works with local experts and community members to install sustainable water solutions, including wells, piped water systems, BioSand Filters, and systems for harvesting rainwater.

Allocation Decisions. In this experiment, you will be presented with 12 allocation decision problems. In each problem, you will be endowed with a certain amount of money, and different conditions will be placed on this money depending on the problem. You will then be asked to decide how much money to allocate to *charity: water* ("the Charity"), accounting for the amount of your endowment and the specific conditions provided. You will do this by deciding the amount that you would like to pass to the Charity. For each decision problem, the computer will then calculate the amount that you will hold for yourself (the total amount available to you to allocate, given your endowment and the conditions provided, minus the amount you pass to the Charity), your total earnings, and the total donation that will be received by the Charity. After you have made a decision for all 12 problems, only one problem will be randomly selected to be carried out, and your decision in this problem will determine your payment and the amount received by the Charity. An example of the type of allocation decisions you will be presented with is given below.

[**To be read only; does not appear in subject instructions**]**:** Please take a moment to look at the example allocation decisions. Note that for each problem you are asked to enter the number of Tokens you would like to pass to the Charity. In the first problem, you are endowed with 160 Tokens. However, your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate between yourself and the Charity. For every 1 Token you pass to the Charity, the Charity will receive 2 Tokens: your 1 Token and a matching 1 Token provided by the experimenter. After you enter the number of Tokens you would like to pass to the Charity, the remaining columns will automatically fill with the correct values. Note that the Total Donation received by the Charity (Column 5) is twice the amount that you have chosen to pass. This is because, in this question, your donation is matched 1:1 by the experimenter. In the second question, your endowment is 240 Tokens, which is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate between yourself and the Charity. However, your donation is not matched, and therefore the Total Donation received by the Charity is the same as the amount you choose to pass. Finally, in the third example question your endowment is 160 Tokens, and for every Token you choose to pass to the Charity, the Charity will receive 1 Token. Note that the Total Donation received by the Charity in this question is equal to the number of Tokens you choose to pass. However, Your Earnings are less than the amount you hold for yourself, since any Tokens you hold for yourself will be taxed by the experimenter at a rate of 50%. **[End]**

Example Allocation Decision Problems:

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, accounting for any applicable taxes. Your Earnings	Total donation received by the Charity, including any applicable matched funds. Total Donation
1.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	20	60	60	40
2.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	35	85	85	35
3.) You are endowed with 160 Tokens. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 50%.				

As you enter an amount to Pass in each question, the remaining columns (Hold, Your Earnings, and Total Donation) will automatically display the corresponding values based on your decision of how much to Pass and the specific conditions listed in column (1). Note that your endowment may differ between problems. Also note that, depending on the condition listed in column (1), the amount the Charity receives (i.e., Total Donation) may not be equal to the amount you choose to Pass. Likewise, the amount you Hold may not be equal to the amount you earn (i.e., Your Earnings). It is important that you carefully make note of the endowment and conditions given for each problem when making your allocation decisions.

Once you have entered appropriate decisions for each problem, you will be able to submit your decisions. After submitting your decisions, **one problem will be randomly selected to determine your payment.** You will be obligated to pass to the Charity the amount you have entered in column (2) of the selected problem. This amount, plus any matched funds provided by the experimenter (for applicable problems), will actually be donated to the Charity. The Total Donation received by the Charity is given in column (5). Your choice of how much to pass to the Charity in the randomly selected decision problem, combined with the condition provided in column (1) of the selected problem, will determine Your Earnings in this experiment. This amount is given in column (4). Within 48 hours after the experimental session ends, you will be paid your earnings (plus the $5 show-up reward) in the form of an electronic Amazon gift card.

Tax Experiment Decision Sheet

Figure A.17 shows an example of the decision sheets presented to subjects in the *tax* experiment.

ALLOCATION DECISION PROBLEMS

Below are 12 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how much to allocate to *charity: water* ("the Charity"), accounting for the endowment listed in column (1) and the condition listed in column (1). Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instructions.

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, accounting for any applicable taxes. Your earnings	Total donation received by the Charity, including any applicable matched funds. Total donation
1.) You are endowed with 120 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	5	75	75	5
2.) You are endowed with 120 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.	12	68	68	18
3.) You are endowed with 120 Tokens. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 33%.	15	105	70	15
4.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	21	59	59	21
5.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	27	53	53	54
6.) You are endowed with 160 Tokens. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 50%.	33	127	64	33
7.) You are endowed with 180 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.				
8.) You are endowed with 180 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.				
9.) You are endowed with 180 Tokens. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 33%.				
10.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.				
11.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.				
12.) You are endowed with 240 Tokens. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 50%.				

Figure A.17: Screenshot of an Example Decision Sheet for the Tax Experiment in Qualtrics

A.4.3 Alt-tax Experiment Materials

Alt-tax Experiment Instructions

INSTRUCTIONS

Introduction. Thank you for participating in this online experiment. This experiment is interested in studying how individuals make decisions. You will be making decisions individually. Your decisions and earnings during the experiment will be confidential and will only be associated with an ID number.

You will be compensated for your participation. At the end of the experiment, you will receive a show-up reward of $5. This show-up reward is not contingent on the decisions that you make during the experiment, and it will be yours to keep just for participating. In addition to the show-up reward, you will also have an opportunity to earn additional money. The amount you are paid will depend on the decisions you make in the experiment and luck, as will be explained in detail below. During the experiment, your earnings will be calculated in Tokens. At the end of the experiment the total amount of Tokens you have earned will be converted to US Dollars at the following rate:

10 Tokens = 1.00 US Dollar

Your $ earnings (plus the $5 show-up reward) will be paid to you in private in the form of an **electronic Amazon gift card** within 48 hours after the completion of the experiment.

At any time, you can use the chat box in the Zoom room to ask the experimenter a question. No other participants will see your questions. The experimenter has muted everyone's microphones and turned off videos to avoid any interruptions during the experiment.

During the experiment you will be provided with opportunities to make donations to **charity: water**, a nonprofit organization that works to bring safe and clean drinking water to the nearly 800 million people in the world living without access to clean water. The majority of people without access to clean water live in isolated rural areas, and they must spend hours every day walking many miles to collect water for their families. This water often carries diseases that lead to sickness. **charity: water** works with local experts and community members to install sustainable water solutions, including wells, piped water systems, BioSand Filters, and systems for harvesting rainwater.

Allocation Decisions. In this experiment, you will be presented with 12 allocation decision problems. In each problem, you will be endowed with a certain amount of money, and different conditions will be placed on this money depending on the problem. You will then be asked to decide how much money to allocate to *charity: water* ("the Charity"), accounting for the amount of your endowment and the specific conditions provided. You will do this by deciding the amount that you would like to pass to the Charity. For each decision problem, the computer will then calculate the amount that you will hold for yourself (the total amount available to you to allocate, given your endowment and the conditions provided, minus the amount you pass to the Charity), your total earnings, and the total donation that will be received by the Charity. After you have made a decision for all 12 problems, only one problem will be randomly selected to be carried out, and your decision in this problem will determine your payment and the amount received by the Charity. An example of the type of allocation decisions you will be presented with is given below.

[To be read only; does not appear in subject instructions]: Please take a moment to look at the example allocation decisions. Note that for each problem you are asked to enter the number of Tokens you would like to pass to the Charity. In the first problem, you are endowed with 160 Tokens. However, your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate between yourself and the Charity. For every 1 Token you pass to the Charity, the Charity will receive 2 Tokens: your 1 Token and a matching 1 Token provided by the experimenter. After you enter the number of Tokens you would like to pass to the Charity, the remaining columns will automatically fill with the correct values. Note that the Total Donation received by the Charity (Column 5) is twice the amount that you have chosen to pass. This is because, in this question, your donation is matched 1:1 by the experimenter. In the second question, your endowment is 240 Tokens, which is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate between yourself and the Charity. However, your donation is not matched, and therefore the Total Donation received by the Charity is the same as the amount you choose to pass. Finally, in the third example question your endowment is 160 Tokens, which is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate between yourself and the charity. Note that the Total Donation received by the Charity in this question is equal to the number of Tokens you choose to pass. However, Your Earnings may be greater than the amount you hold for yourself, since the experimenter provides you with a refund of 0.5 Tokens for every Token you pass to the Charity. [**End**]

Example Allocation Decision Problems:

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, accounting for any applicable taxes. Your Earnings	Total donation received by the Charity, including any applicable matched funds. Total Donation
1.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	23	57	57	46
2.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	31	89	89	31
3.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.5 Tokens.				

As you enter an amount to Pass in each question, the remaining columns (Hold, Your

Earnings, and Total Donation) will automatically display the corresponding values based on your decision of how much to Pass and the specific conditions listed in column (1). Note that your endowment may differ between problems. Also note that, depending on the condition listed in column (1), the amount the Charity receives (i.e., Total Donation) may not be equal to the amount you choose to Pass. Likewise, the amount you Hold may not be equal to the amount you earn (i.e., Your Earnings). It is important that you carefully make note of the endowment and conditions given for each problem when making your allocation decisions.

Once you have entered appropriate decisions for each problem, you will be able to submit your decisions. After submitting your decisions, **one problem will be randomly selected to determine your payment.** You will be obligated to pass to the Charity the amount you have entered in column (2) of the selected problem. This amount, plus any matched funds provided by the experimenter (for applicable problems), will actually be donated to the Charity. The Total Donation received by the Charity is given in column (5). Your choice of how much to pass to the Charity in the randomly selected decision problem, combined with the condition provided in column (1) of the selected problem, will determine Your Earnings in this experiment. This amount is given in column (4). Within 48 hours after the experimental session ends, you will be paid your earnings (plus the $5 show-up reward) in the form of an electronic Amazon gift card.

Alt-tax Experiment Decision Sheet

ALLOCATION DECISION PROBLEMS

Below are 12 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how much to allocate to *charity: water* ("the Charity"), accounting for the endowment listed in column (1) and the condition listed in column (1). Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instructions.

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, accounting for any applicable taxes. Your earnings	Total donation received by the Charity, including any applicable matched funds. Total donation
1.) You are endowed with 120 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	11	69	69	11
2.) You are endowed with 120 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.33 Tokens.	13	67	71	13
3.) You are endowed with 120 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.	24	56	56	36
4.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	39	41	41	39
5.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.5 Tokens.	25	55	68	25
6.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	43	37	37	86
7.) You are endowed with 180 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.				
8.) You are endowed with 180 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.33 Tokens.				
9.) You are endowed with 180 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.				
10.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.				
11.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token, and the experimenter will refund to you 0.5 Tokens.				
12.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.				

Figure A.18: Screenshot of an Example Decision Sheet for the Alt-tax Experiment in Qualtrics

A.4.4 Con-tax Experiment Materials

Con-tax Experiment Instructions

INSTRUCTIONS

Introduction. Thank you for participating in this online experiment. This experiment is interested in studying how individuals make decisions. You will be making decisions individually. Your decisions and earnings during the experiment will be confidential and will only be associated with an ID number.

You will be compensated for your participation. At the end of the experiment, you will receive a show-up reward of $5. This show-up reward is not contingent on the decisions that you make during the experiment, and it will be yours to keep just for participating. In addition to the show-up reward, you will also have an opportunity to earn additional money. The amount you are paid will depend on the decisions you make in the experiment and luck, as will be explained in detail below. During the experiment, your earnings will be calculated in Tokens. At the end of the experiment the total amount of Tokens you have earned will be converted to US Dollars at the following rate:

10 Tokens = 1.00 US Dollar

Your $ earnings (plus the $5 show-up reward) will be paid to you in private in the form of an **electronic Amazon gift card** within 48 hours after the completion of the experiment.

At any time, you can use the chat box in the Zoom room to ask the experimenter a question. No other participants will see your questions. The experimenter has muted everyone's microphones and turned off videos to avoid any interruptions during the experiment.

During the experiment you will be provided with opportunities to make donations to **charity: water**, a nonprofit organization that works to bring safe and clean drinking water to the nearly 800 million people in the world living without access to clean water. The majority of people without access to clean water live in isolated rural areas, and they must spend hours every day walking many miles to collect water for their families. This water often carries diseases that lead to sickness. **charity: water** works with local experts and community members to install sustainable water solutions, including wells, piped water systems, BioSand Filters, and systems for harvesting rainwater.

Allocation Decisions. In this experiment, you will be presented with 12 allocation decision problems. In each problem, you will be endowed with a certain amount of money, and different conditions will be placed on this money depending on the problem. You will then be asked to decide how much money to allocate to *charity: water* ("the Charity"), accounting for the amount of your endowment and the specific conditions provided. You will do this by deciding the amount that you would like to pass to the Charity. For each decision problem, the computer will then calculate the amount that you will hold for yourself (the total amount available to you to allocate, given your endowment and the conditions provided, minus the amount you pass to the Charity), your total earnings, and the total donation that will be received by the Charity. After you have made a decision for all 12 problems, only one problem will be randomly selected to be carried out, and your decision in this problem will determine your payment and the amount received by the Charity. An example of the type of allocation decisions you will be presented with is given below.

[**To be read only; does not appear in subject instructions**]**:** Please take a moment to look at the example allocation decisions. Note that for each problem you are asked to enter the number of Tokens you would like to pass to the Charity. In the first problem, you are endowed with 160 Tokens. However, your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate between yourself and the Charity. For every 1 Token you pass to the Charity, the Charity will receive 2 Tokens: your 1 Token and a matching 1 Token provided by the experimenter. After you enter the number of Tokens you would like to pass to the Charity, the remaining columns will automatically fill with the correct values. Note that the Total Donation received by the Charity (Column 5) is twice the amount that you have chosen to pass. This is because, in this question, your donation is matched 1:1 by the experimenter. In the second question, your endowment is 240 Tokens, which is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate between yourself and the Charity. However, your donation is not matched, and therefore the Total Donation received by the Charity is the same as the amount you choose to pass. Finally, in the third example question your endowment is 160 Tokens, but the total amount you can pass to the Charity is capped at 80 Tokens. For every Token you choose to pass to the Charity, the Charity will receive 1 Token. Note that the Total Donation received by the Charity in this question is equal to the number of Tokens you choose to pass. However, Your Earnings are less than the amount you hold for yourself, since any Tokens you hold for yourself will be taxed by the experimenter at a rate of 50%. [**End**]

Example Allocation Decision Problems:

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, accounting for any applicable taxes. Your Earnings	Total donation received by the Charity, including any applicable matched funds. Total Donation
1.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	23	57	57	46
2.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	37	83	83	37
3.) You are endowed with 160 Tokens. Of your 160 Token endowment, you may pass up to 80 Tokens to the Charity. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 50%.				

As you enter an amount to Pass in each question, the remaining columns (Hold, Your

Earnings, and Total Donation) will automatically display the corresponding values based on your decision of how much to Pass and the specific conditions listed in column (1). Note that your endowment may differ between problems. Also note that, depending on the condition listed in column (1), the amount the Charity receives (i.e., Total Donation) may not be equal to the amount you choose to Pass. Likewise, the amount you Hold may not be equal to the amount you earn (i.e., Your Earnings). It is important that you carefully make note of the endowment and conditions given for each problem when making your allocation decisions.

Once you have entered appropriate decisions for each problem, you will be able to submit your decisions. After submitting your decisions, **one problem will be randomly selected to determine your payment.** You will be obligated to pass to the Charity the amount you have entered in column (2) of the selected problem. This amount, plus any matched funds provided by the experimenter (for applicable problems), will actually be donated to the Charity. The Total Donation received by the Charity is given in column (5). Your choice of how much to pass to the Charity in the randomly selected decision problem, combined with the condition provided in column (1) of the selected problem, will determine Your Earnings in this experiment. This amount is given in column (4). Within 48 hours after the experimental session ends, you will be paid your earnings (plus the $5 show-up reward) in the form of an electronic Amazon gift card.

Con-tax Experiment Decision Sheet

ALLOCATION DECISION PROBLEMS

Below are 12 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how much to allocate to *charity: water* ("the Charity"), accounting for the endowment listed in column (1) and the condition listed in column (1). Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instructions.

	Select the amount you would like to pass to the Charity. Pass	The total amount of Tokens held for yourself. Hold	Your total earnings, accounting for any applicable taxes. Your earnings	Total donation received by the Charity, including any applicable matched funds. Total donation
1.) You are endowed with 120 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	7	73	73	7
2.) You are endowed with 120 Tokens. Of your 120 Token endowment, you may pass up to 80 Tokens to the Charity. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 33%.	22	98	65	22
3.) You are endowed with 120 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.	61	19	19	92
4.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.	29	51	51	29
5.) You are endowed with 160 Tokens. Of your 160 Token endowment, you may pass up to 80 Tokens to the Charity. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 50%.	37	123	62	37
6.) You are endowed with 160 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 80 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.	53	27	27	106
7.) You are endowed with 180 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.				
8.) You are endowed with 180 Tokens. Of your 180 Token endowment, you may pass up to 120 Tokens to the Charity. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 33%.				
9.) You are endowed with 180 Tokens. Your endowment is taxed by the experimenter at a rate of 33%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1.5 Tokens; your 1 Token and a matching 0.5 Tokens provided by the experimenter.				
10.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 1 Token.				
11.) You are endowed with 240 Tokens. Of your 240 Token endowment, you may pass up to 120 Tokens to the Charity. For every 1 Token you pass, the Charity will receive 1 Token. Any money you choose to hold for yourself will be taxed by the experimenter at a rate of 50%.				
12.) You are endowed with 240 Tokens. Your endowment is taxed by the experimenter at a rate of 50%, leaving you with 120 Tokens to allocate. For every 1 Token you pass, the Charity will receive 2 Tokens; your 1 Token and a matching 1 Token provided by the experimenter.				

Figure A.19: Screenshot of an Example Decision Sheet for the Con-tax Experiment in Qualtrics

Appendix B: Appendix for Chapter 2

B.1 Proofs

Proof of Lemma 1: I will first prove that $g^0 < g^1 \implies g^0 < g^* < g^1$. The proof consists of two parts. First I will show that $g^0 < g^*$, and then I will show that $g^* < g^1$, thus completing the proof that $g^0 < g^1 \implies g^0 < g^* < g^1$.

Recall that g^* is defined by the first order condition $FOC^*(g^*) = pFOC^1(g^*) + (1 - p)FOC^0(g^*) \equiv 0$. Likewise, g^0 and g^1 are defined by the first order conditions $FOC^0(g^0) \equiv 0$ and $FOC^1(g^1) \equiv 0$, respectively. By the second order conditions for U^0 and U^1 (Assumption 1), $FOC^0(g)$ and $FOC^1(g)$ are decreasing in g. Furthermore, since EU is a weighted sum of U^0 and U^1, $FOC^*(g)$ must also be decreasing in g. Thus, it follows that $FOC^*(g^0) = pFOC^1(g^0) + (1 - p)FOC^0(g^0) > 0$, since $g^0 < g^1$ implies $FOC^1(g^0) > 0$. Finally, since $FOC^*(g^0) > 0$, it follows that $g^* > g^0$.

To see that $g^* < g^1$, note that $FOC^*(g^1) = pFOC^1(g^1) + (1 - p)FOC^0(g^1) < 0$, since $g^0 < g^1$ implies $FOC^0(g^1) < 0$. Thus, because $FOC^*(g)$ is decreasing in g, it follows that $g^* < g^1$, thereby demonstrating that $g^0 < g^1 \implies g^0 < g^* < g^1$.

The proof that $g^1 < g^0 \implies g^1 < g^* < g^0$ follows similarly. If $g^1 < g^0$, then $FOC^*(g^0) = pFOC^1(g^0) + (1 - p)FOC^0(g^0) < 0$, since $FOC^1(g)$ is decreasing in g. Therefore, $g^1 < g^0$ implies $g^* < g^0$. Furthermore, $g^1 < g^0$ implies $FOC^*(g^1) = pFOC^1(g^1)+(1-p)FOC^0(g^1) > 0$, since $FOC^0(g)$ is decreasing in g. This implies $g^* > g^1$, thereby demonstrating that $g^1 < g^0 \implies g^1 < g^* < g^0$.

Finally, if $g^0 = g^1$, then both g^0 and g^1 solve $FOC^*(g) = 0$. Thus, $g^* = g^0 = g^1$, completing the proof of Lemma 1.

Proof of Lemma 2: First consider $g^1 > g^0$. From Lemma 1, we have that $g^0 < g^* < g^1$. Therefore, $FOC^0(g^*) < 0$ and $FOC^1(g^*) > 0$, implying $FOC^0(g^*) - FOC^1(g^*) < 0$. That is, $g^1 > g^0 \implies FOC^0(g^*) - FOC^1(g^*) < 0$.

Now consider $g^0 > g^1$. From Lemma 1, we have that $g^1 < g^* < g^0$. Therefore, $FOC^0(g^*) > 0$ and $FOC^1(g^*) < 0$, implying $FOC^0(g^*) - FOC^1(g^*) > 0$. That is, $g^0 > g^1 \implies FOC^0(g^*) - FOC^1(g^*) > 0$.

Finally, consider $g^0 = g^1$. From Lemma 1, we have that $g^* = g^0 = g^1$. Therefore, $FOC^0(g^*) = FOC^1(g^*) = 0$, implying $FOC^0(g^*) - FOC^1(g^*) = 0$. That is, $g^0 = g^1 \implies FOC^0(g^*) - FOC^1(g^*) = 0$, completing the proof of Lemma 2.

B.2 Additional Tables

Table B.1: Summary demographic data for subjects

Summary Statistics	μ	σ	Min	Median	Max
Age	40.05	11.618	20	37	72
Knowledge of charity	1.17	2.342	0	0	9
Understanding of pay	8.23	2.196	0	9	10
Understanding of donation	8.36	1.977	0	9	10
Confidence in donation	7.45	3.095	0	9	10
SEX					
Female	.46	.500			
Male	.51	.501			
Other	.03	.162			
INCOME					
Don't know/Prefer not to answer	.01	.115			
Less than $50,000	.30	.460			
Between $50,000 and $75,000	.32	.468			
Between $75,000 and $100,000	.11	.310			
Between $100,000 and $150,000	.15	.355			
Between $150,000 and $200,000	.05	.212			
More than $200,000	.07	.250			
POLITICS					
Prefer not to say	.00	.000			
Unsure/Undecided	.01	.082			
Liberal	.60	.492			
Moderate	.27	.444			
Conservative	.13	.334			
RELIGION					
Not important	.49	.502			
Somewhat important	.21	.406			
Important	.13	.341			
Very important	.17	.374			
RECENT DONATIONS					
Less than $20	.41	.493			
Between $20 and $50	.24	.429			
Between $50 and $100	.11	.318			
More than $100	.24	.429			
Observations	150				

Table B.2: Regressions including all controls

		(1)	(2)	(3)	(4)
	logPass				
	logPrice	-0.418***	-0.060	-0.640***	-0.037
		(0.091)	(0.117)	(0.097)	(0.240)
	Match probability	0.986***	0.566***	1.599***	0.898***
		(0.118)	(0.162)	(0.100)	(0.274)
	Match(=1)	-0.084	0.179	-0.131	0.313
		(0.121)	(0.122)	(0.143)	(0.215)
	PRICExPROB		-0.574***		-0.952***
			(0.182)		(0.347)
	Age	0.007	0.007	0.016	0.016
		(0.009)	(0.009)	(0.018)	(0.018)
Sex (Female omitted)	Male	-0.307	-0.307	-0.589	-0.589
		(0.242)	(0.243)	(0.409)	(0.409)
	Other	1.355**	1.355**	2.614**	2.616**
		(0.533)	(0.533)	(1.188)	(1.190)
	Income refuse (=1)	2.408	2.408	2.194	2.192
		(2.000)	(2.000)	(3.680)	(3.681)
	logIncome	0.330*	0.330*	0.498	0.498
		(0.178)	(0.178)	(0.304)	(0.304)
Politics (conservative omitted)	liberal	0.804**	0.804**	1.327**	1.328**
		(0.396)	(0.396)	(0.651)	(0.652)
	moderate	0.415	0.415	0.788	0.789
		(0.411)	(0.411)	(0.679)	(0.679)
	unsure/undecided	-0.917*	-0.917*	-1.035	-1.041
		(0.550)	(0.551)	(2.389)	(2.391)
Religion (important omitted)	not important	0.108	0.108	0.299	0.298
		(0.308)	(0.308)	(0.590)	(0.590)
	somewhat important	0.203	0.203	0.424	0.424
		(0.342)	(0.342)	(0.653)	(0.653)
	very important	-0.100	-0.100	-0.195	-0.197
		(0.399)	(0.399)	(0.703)	(0.703)
	logDonation	0.153	0.153	0.232	0.232
		(0.129)	(0.129)	(0.222)	(0.222)
	Knowledge of charity	0.078*	0.078*	0.158**	0.159**
		(0.042)	(0.042)	(0.080)	(0.080)
	Understand payment	0.022	0.022	0.159	0.160
		(0.079)	(0.079)	(0.180)	(0.180)
	Understand donation	-0.187**	-0.187**	-0.479**	-0.480**
		(0.089)	(0.089)	(0.197)	(0.198)
	Donation confidence	0.100**	0.100**	0.206***	0.206***
		(0.039)	(0.039)	(0.068)	(0.068)
	Constant	-2.981	-2.981	-6.401*	-6.403*
		(2.122)	(2.122)	(3.608)	(3.610)
	Model	RE	RE	Tobit	Tobit
	Observations	1950	1950	1950	1950

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Average amount passed by match rate

	Baseline	0.5:1 Match	1:1 Match	2:1 Match
mean	14.68	21.81	24.00	25.98
semean	1.87	.93	.93	.91
min	0	0	0	0
max	80	80	80	80
p50	0	18.13	20	25
count	150	600	600	600

Table B.4: Average amount received by charity (assuming a match is received) by match rate

	Baseline	0.5:1 Match	1:1 Match	2:1 Match
mean	14.68	32.79	47.99	77.93
semean	1.87	1.40	1.85	2.72
min	0	0	0	0
max	80	120	160	240
p50	0	27.25	40	75
count	150	600	600	600

Table B.5: Average amount passed by match rate and match probability

		0.5:1 Match				1:1 Match				2:1 Match			
	Baseline	p25	p50	p75	p100	p25	p50	p75	p100	p25	p50	p75	p100
mean	14.68	17.39	19.6	23.48	26.75	18.81	22.78	25.15	29.23	18.81	22.75	27.59	34.76
semean	1.87	1.85	1.86	2.03	2.13	1.85	1.89	2.00	2.12	1.77	1.87	2.05	2.28
min	0	0	0	0	0	0	0	0	0	0	0	0	0
median	0	10	10	20	20	10	20	20	25	10	20	25	30
max	80	80	80	80	80	80	80	80	80	80	80	80	80
count	150	150	150	150	150	150	150	150	150	150	150	150	150

Table B.6: Average impact (assuming match is received) by match rate and match probability

		0.5:1 Match				1:1 Match				2:1 Match			
	Baseline	p25	p50	p75	p100	p25	p50	p75	p100	p25	p50	p75	p100
mean	14.68	26.17	29.46	35.33	40.19	37.63	45.56	50.31	58.47	56.44	68.24	82.76	104.28
semean	1.87	2.78	2.78	3.04	3.20	3.71	3.77	4.00	4.24	5.31	5.60	6.16	6.84
min	0	0	0	0	0	0	0	0	0	0	0	0	0
median	0	15	15	30	30	20	40	40	50	30	60	75	90
max	80	120	120	120	120	160	160	160	160	240	240	240	240
count	150	150	150	150	150	150	150	150	150	150	150	150	150

B.2.1 Comparing results across question orders

Table B.7: Question order 1: Average amount passed by match rate and match probability

		0.5:1 Match				1:1 Match				2:1 Match			
	Baseline	p25	p50	p75	p100	p25	p50	p75	p100	p25	p50	p75	p100
mean	14.71	17.29	19.39	23.44	24.05	17.37	22.51	24.29	25.15	18.00	21.12	23.00	28.63
semean	3.58	3.39	3.52	3.86	3.90	3.26	3.59	3.70	3.94	3.39	3.68	3.71	3.95
min	0	0	0	0	0	0	0	0	0	0	0	0	0
median	0	9	10	20	12	12	18	20	15	11	20	20	25
max	80	80	80	80	80	80	80	80	80	80	80	80	80
count	41	41	41	41	41	41	41	41	41	41	41	41	41

Table B.8: Question order 2: Average amount passed by match rate and match probability

		0.5:1 Match				1:1 Match				2:1 Match			
	Baseline	p25	p50	p75	p100	p25	p50	p75	p100	p25	p50	p75	p100
mean	15.87	16.70	19.23	22.73	26.20	18.40	19.90	24.13	29.63	18.63	20.20	23.50	30.20
semean	4.07	4.04	4.39	4.88	4.91	4.19	3.90	4.62	4.81	3.53	3.94	4.28	4.76
min	0	0	0	0	0	0	0	0	0	0	0	0	0
median	7.5	10	10	20	20	10	20	20	22.5	15	15	20	22.5
max	80	80	80	80	80	80	80	80	80	60	80	80	80
count	30	30	30	30	30	30	30	30	30	30	30	30	30

Table B.9: Question order 3: Average amount passed by match rate and match probability

		0.5:1 Match				1:1 Match				2:1 Match			
	Baseline	p25	p50	p75	p100	p25	p50	p75	p100	p25	p50	p75	p100
mean	15.08	18.33	20.03	25.59	30.46	19.95	24.38	27.03	31.13	19.13	24.15	31.36	38.18
semean	3.80	3.70	3.15	3.66	3.97	3.84	3.81	3.84	4.00	3.49	3.50	4.15	4.57
min	0	0	0	0	0	0	0	0	0	0	0	0	0
median	0	10	20	20	30	10	20	25	30	10	20	30	40
max	80	80	80	80	80	80	80	80	80	80	80	80	80
count	39	39	39	39	39	39	39	39	39	39	39	39	39

Table B.10: Question order 4: Average amount passed by match rate and match probability

		0.5:1 Match				1:1 Match				2:1 Match			
	Baseline	p25	p50	p75	p100	p25	p50	p75	p100	p25	p50	p75	p100
mean	13.38	17.10	19.68	22.03	26.30	19.50	23.65	24.98	31.28	19.48	24.95	31.68	41.13
semean	3.64	3.86	4.02	4.13	4.50	3.77	3.87	4.10	4.40	3.78	3.88	4.22	4.77
min	0	0	0	0	0	0	0	0	0	0	0	0	0
median	0	1	3.5	15	17.5	10	22.5	22.5	37.5	10	25	30	40
max	80	80	80	80	80	80	80	80	80	80	80	80	80
count	40	40	40	40	40	40	40	40	40	40	40	40	40

Table B.11: Regressions for Amount Passed by Question Order

	(1)	(2)	(3)	(4)
logPrice	-0.149	0.271	-0.220	0.561
	(0.270)	(0.164)	(0.231)	(0.220)
PRICExOrder2	-0.243	-0.534*	-0.343	-0.922
	(0.333)	(0.081)	(0.222)	(0.183)
PRICExOrder3	-0.138	-0.272	-0.248	-0.440
	(0.406)	(0.331)	(0.341)	(0.495)
PRICExOrder4	-0.693**	-0.576*	-1.100***	-1.124*
	(0.011)	(0.091)	(0.000)	(0.086)
Match probability	0.815***	0.322	1.382***	0.487
	(0.001)	(0.288)	(0.000)	(0.345)
MATCH PROBxOrder2	0.047	0.388	0.039	0.697
	(0.887)	(0.353)	(0.891)	(0.378)
MATCH PROBxOrder3	0.318	0.476	0.354	0.563
	(0.341)	(0.276)	(0.187)	(0.442)
MATCH PROBxOrder4	0.297	0.160	0.432	0.446
	(0.373)	(0.732)	(0.111)	(0.549)
Match(=1)	0.072	0.380*	0.082	0.655
	(0.753)	(0.075)	(0.762)	(0.109)
Match(=1)xOrder2	-0.343	-0.557*	-0.515	-0.940
	(0.290)	(0.066)	(0.208)	(0.129)
Match(=1)xOrder3	0.071	-0.028	0.115	-0.028
	(0.823)	(0.935)	(0.765)	(0.962)
Match(=1)xOrder4	-0.393	-0.308	-0.529	-0.542
	(0.260)	(0.354)	(0.177)	(0.358)
Order 2 (=1)	0.225	0.225	0.295	0.296
	(0.577)	(0.578)	(0.671)	(0.670)
Order 3 (=1)	-0.038	-0.038	-0.070	-0.069
	(0.921)	(0.921)	(0.914)	(0.914)
Order 4 (=1)	-0.250	-0.250	-0.570	-0.572
	(0.507)	(0.508)	(0.378)	(0.376)
PRICExPROB		-0.673**		-1.218*
		(0.048)		(0.063)
PRICExPROBxOrder2		0.466		0.897
		(0.338)		(0.370)
PRICExPROBxOrder3		0.215		0.280
		(0.629)		(0.764)
PRICExPROBxOrder4		-0.187		0.027
		(0.736)		(0.977)
Constant	1.486***	1.486***	0.587	0.586
	(0.000)	(0.000)	(0.196)	(0.197)
Model	RE	RE	Tobit	Tobit
Observations	1950	1950	1950	1950

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.12: Average amount passed in each question order.

		Order 1	Order 2	Order 3	Order 4	p-value†
No Match	$p = 0$	14.71	15.87	15.08	13.38	.974
		(22.95)	(22.30)	(23.76)	(23.05)	(.988)
0.5:1 Match	$p = .25$	17.29	16.70	18.33	17.10	.992
		(21.69)	(22.15)	(23.10)	(24.40)	(.893)
	$p = .5$	19.39	19.23	20.03	19.68	.999
		(22.53)	(24.02)	(19.66)	(25.44)	(.448)
	$p = .75$	23.44	22.73	25.59	22.02	.932
		(24.70)	(26.72)	(22.87)	(26.09)	(.806)
	$p = 1$	24.05	26.20	30.46	26.30	.745
		(24.99)	(26.87)	(24.80)	(28.46)	(.806)
1:1 Match	$p = .25$	17.37	18.40	19.95	19.50	.959
		(20.84)	(22.93)	(23.99)	(23.81)	(.814)
	$p = .5$	22.51	19.90	24.38	23.65	.872
		(22.98)	(21.37)	(23.78)	(24.48)	(.887)
	$p = .75$	24.29	24.13	27.03	24.98	.955
		(23.71)	(25.28)	(23.97)	(25.94)	(.936)
	$p = 1$	25.15	29.63	31.13	31.27	.692
		(25.24)	(26.36)	(24.98)	(27.80)	(.907)
2:1 Match	$p = .25$	18.00	18.63	19.13	19.48	.991
		(21.71)	(19.31)	(21.77)	(23.91)	(.689)
	$p = .5$	21.12	20.20	24.15	24.95	.781
		(23.58)	(21.60)	(21.88)	(24.52)	(.855)
	$p = .75$	23.00	23.50	31.36	31.68	.254
		(23.73)	(23.42)	(25.93)	(26.70)	(.828)
	$p = 1$	28.63	30.20	38.18	41.13	.142
		(25.31)	(26.07)	(28.53)	(30.18)	(.688)
Count		41	30	39	40	

Note: Out-of-pocket donations are reported. Standard deviations are shown in parentheses.

†One-way ANOVA tests of equality of means across all four question orders. Bartlett's equal-variances tests are reported in parentheses.

B.3 Additional Figures

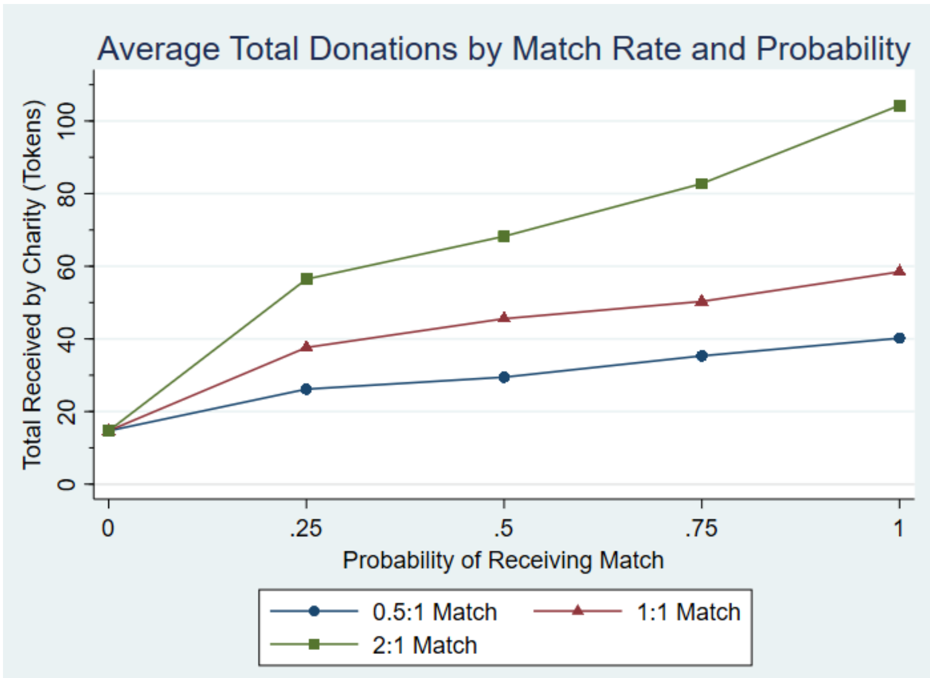


Figure B.1: A comparison of the average total impact (in Tokens, assuming a match is received) across all subjects.

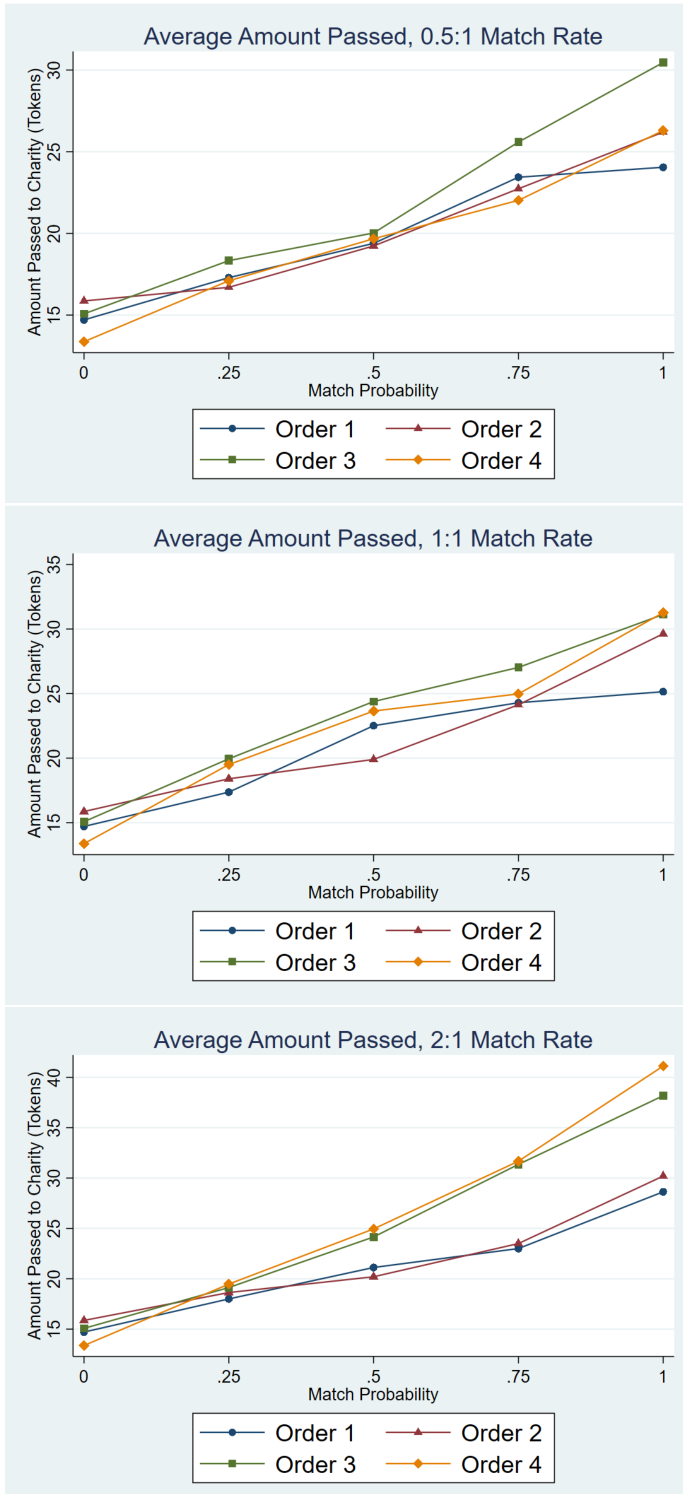


Figure B.2: Comparison of the average amounts passed across question orders for each match rate and match probability.

B.4 Experiment Materials

B.4.1 Donation follow-up task

Donation Follow-up Task

Please answer the following questions. For each question, select whether you prefer Option A or Option B.

Option A will always be *the Charity receives a $1 donation with probability 50%, and a $0 donation otherwise.*

Option B will be *the Charity receives a donation of some dollar amount*. As you proceed down the rows of the list, the amount of the donation received by the Charity will increase from $0 to $1.

For each row, all you have to do is decide whether you prefer Option A or Option B. Indicate your preference by selecting the corresponding button. **Most people begin by preferring Option A and then switch to Option B, so one way to complete this list is to determine at which row you prefer to switch from Option A to Option B.**

At the conclusion of the experiment, one row will be randomly selected, and a donation will be made to the Charity on your behalf based on the decision you have made in the selected row. If you have selected the lottery option (Option A) in the randomly selected row, the computer will flip a coin to determine the outcome of the lottery. All donations will actually be donated to the Charity, and you will be informed of the outcome of this process at the conclusion of the experiment. **(Note that any donations provided to the Charity in this task are provided by the experimenter and do not affect your earnings.)**

	Select your preferred option.		
	Option A	Option B	
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.10 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.20 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.30 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.40 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.50 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.60 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.70 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.80 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.90 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$1 donation

Figure B.3: Donation follow-up task example screen

B.4.2 Payment follow-up task

Payment Follow-up Task

Please answer the following questions. For each question, select whether you prefer Option A or Option B.

Option A will always be *you receive a $1 payment with probability 50%, and a $0 payment otherwise.*

Option B will be *you receive a payment of some dollar amount*. As you proceed down the rows of the list, the amount of the payment you receive will increase from $0 to $1.

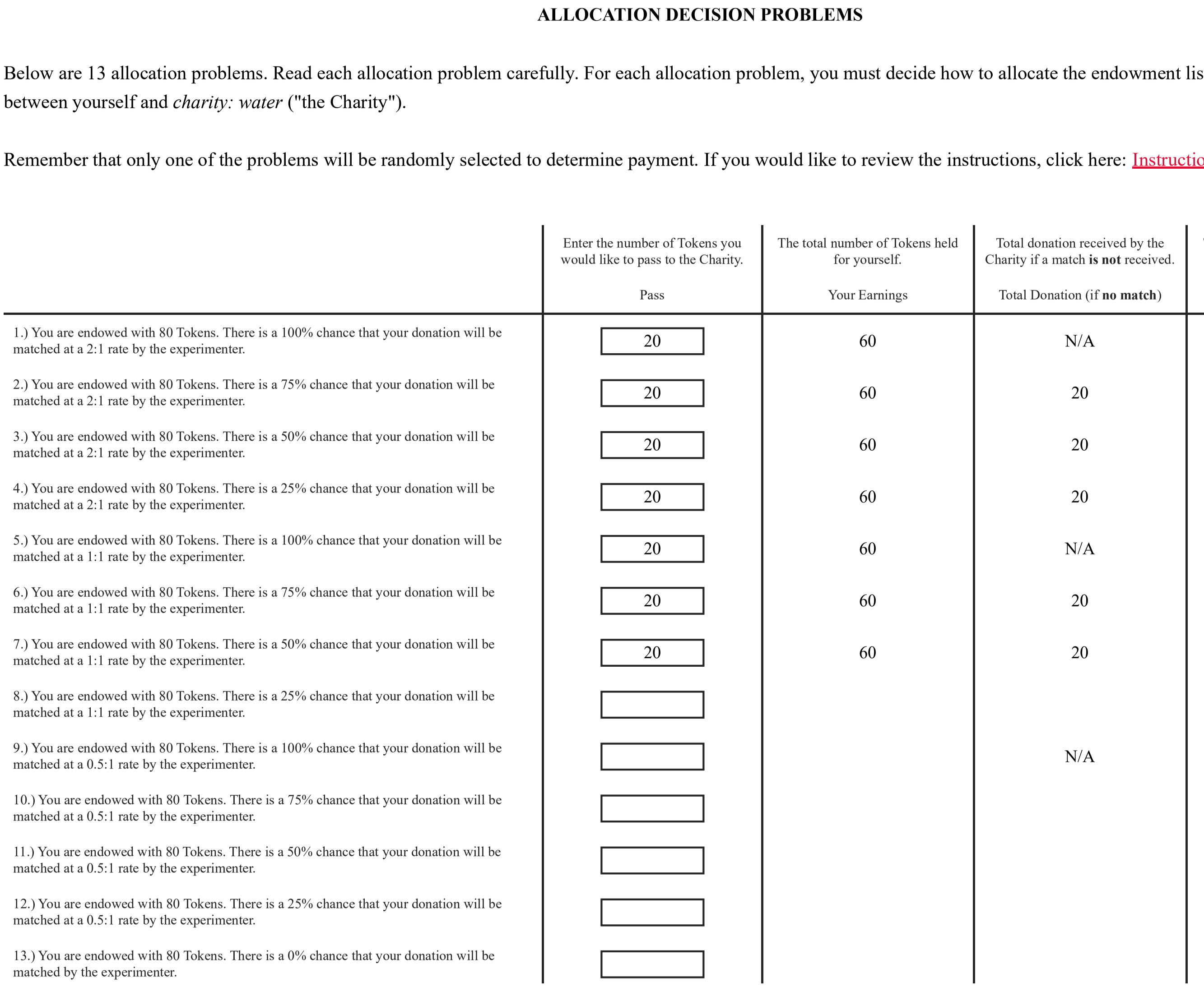
For each row, all you have to do is decide whether you prefer Option A or Option B. Indicate your preference by selecting the corresponding button. **Most people begin by preferring Option A and then switch to Option B, so one way to complete this list is to determine at which row you prefer to switch from Option A to Option B.**

At the conclusion of the experiment, one row will be randomly selected, and you will be paid based on the decision you have made in the selected row. If you have selected the lottery option (Option A) in the randomly selected row, the computer will flip a coin to determine the outcome of the lottery. You will be informed of the outcome of this process at the conclusion of the experiment.

	Select your preferred option. Option A	Option B	
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.10 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.20 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.30 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.40 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.50 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.60 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.70 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.80 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.90 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$1 payment

Figure B.4: Payment follow-up task example screen

B.4.3 Main task decision sheet



ALLOCATION DECISION PROBLEMS

Below are 13 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how to allocate the endowment liste
between yourself and *charity: water* ("the Charity").

Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instruction

	Enter the number of Tokens you would like to pass to the Charity. Pass	The total number of Tokens held for yourself. Your Earnings	Total donation received by the Charity if a match **is not** received. Total Donation (if **no match**)
1.) You are endowed with 80 Tokens. There is a 100% chance that your donation will be matched at a 2:1 rate by the experimenter.	20	60	N/A
2.) You are endowed with 80 Tokens. There is a 75% chance that your donation will be matched at a 2:1 rate by the experimenter.	20	60	20
3.) You are endowed with 80 Tokens. There is a 50% chance that your donation will be matched at a 2:1 rate by the experimenter.	20	60	20
4.) You are endowed with 80 Tokens. There is a 25% chance that your donation will be matched at a 2:1 rate by the experimenter.	20	60	20
5.) You are endowed with 80 Tokens. There is a 100% chance that your donation will be matched at a 1:1 rate by the experimenter.	20	60	N/A
6.) You are endowed with 80 Tokens. There is a 75% chance that your donation will be matched at a 1:1 rate by the experimenter.	20	60	20
7.) You are endowed with 80 Tokens. There is a 50% chance that your donation will be matched at a 1:1 rate by the experimenter.	20	60	20
8.) You are endowed with 80 Tokens. There is a 25% chance that your donation will be matched at a 1:1 rate by the experimenter.			
9.) You are endowed with 80 Tokens. There is a 100% chance that your donation will be matched at a 0.5:1 rate by the experimenter.			N/A
10.) You are endowed with 80 Tokens. There is a 75% chance that your donation will be matched at a 0.5:1 rate by the experimenter.			
11.) You are endowed with 80 Tokens. There is a 50% chance that your donation will be matched at a 0.5:1 rate by the experimenter.			
12.) You are endowed with 80 Tokens. There is a 25% chance that your donation will be matched at a 0.5:1 rate by the experimenter.			
13.) You are endowed with 80 Tokens. There is a 0% chance that your donation will be matched by the experimenter.			

Figure B.5: Main task example screen

B.4.4 Survey questions

The following questions were included as part of a survey subjects were asked to complete after completing the main task and follow-up tasks of the experiment, but before being informed of their bonus payment amount. A response was required for each question.

1. What is your age in years? [**subjects enter an integer value into a text-entry box**]

2. What is your gender?

   a) Male

   b) Female

   c) Other

3. What is you best estimate of your family's annual Income? In addition to your own personal earnings, include income earned by your parents and/or guardians if they give you financial support in whole or in part.

   a) Less than \$50,000

   b) Between \$50,000 and \$75,000

   c) Between \$75,000 and \$100,000

   d) Between \$100,000 and \$150,000

   e) Between \$150,000 and \$200,000

   f) More than \$200,000

   g) Don't know/Prefer not to answer

4. How would you describe your political views?

   a) conservative

b) moderate

c) liberal

d) unsure/undecided

e) prefer not to say

5. How important is religion in your life?

a) very important

b) important

c) somewhat important

d) not important

6. During the past 12 months, how much money have you donated to charitable causes?

a) Less than $20

b) Between $20 and $50

c) Between $50 and $100

d) More than $100

7. How well do you know **charity: water**? Please rate your prior knowledge on a 0 to 10 scale, where 0 indicates no prior information at all and 10 indicates perfect knowledge: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

8. How well did you understand how your earnings are calculated in this experiment? Please rate your understanding on a 0 to 10 scale, where 0 indicates no understanding at all and 10 indicates a perfect understanding: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

9. How well did you understand how your total donation is calculated in this experiment? Please rate your understanding on a 0 to 10 scale, where 0 indicates no understanding

at all and 10 indicates a perfect understanding: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

10. While making your donation decisions in this experiment, how confident were you that your donation (and any applicable matching donation) will actually be donated to **charity: water** on your behalf? Please rate your level of confidence on a 0 to 10 scale, where 0 indicates no confidence at all and 10 indicates complete confidence: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

B.4.5 Instructions

Introduction. Thank you for participating in this online experiment. This experiment is interested in studying how individuals make decisions. You will be making decisions individually. Your decisions and earnings during the experiment will be confidential and will only be associated with an ID number.

Compensation. You will be compensated for your participation. At the end of the experiment, you will receive a show-up reward of $4. This show-up reward is not contingent on the decisions that you make during the experiment, and it will be yours to keep just for participating. You will automatically receive this payment through Prolific upon completion of the experiment.

In addition to the show-up reward, you will also have an opportunity to earn additional money, which will be paid to you through Prolific in the form of a bonus payment. The amount of your bonus payment will depend on the decisions you make in the experiment and luck, as will be explained in detail below.

During the experiment, your earnings will be calculated in Tokens. At the end of the experiment the total amount of Tokens you have earned will be converted to US Dollars at the following rate:

10 Tokens = 1.00 US Dollar

Your $ earnings (including the $4 show-up reward and any bonus payments you earn) will be paid to you in private through Prolific. The show-up reward will be paid automatically once you have registered your submission in Prolific. Your bonus payment will be paid through Prolific within 1-2 business days after your submission has been received. **Bonus payments are backed by the University of Maryland and are guaranteed to be paid to you within 1-2 business days after you register your survey response in Prolific.** If you do not receive your bonus payment with 2 business days, or if you believe an error has been made regarding your bonus payment, please reach out to the research team using the contact information provided in the Consent Form.

The Charity. During the experiment you will be provided with opportunities to make donations to **charity: water**, a nonprofit organization that works to bring safe and clean drinking water to the more than 700 million people in the world living without access to clean water. The majority of people without access to clean water live in isolated rural areas, and they must spend hours every day walking many miles to collect water for their families. This water often carries diseases that lead to sickness. **charity: water** works with local experts and community members to install sustainable water solutions, including wells, piped water systems, BioSand Filters, and systems for harvesting rainwater. 100% of any donations you provide to **charity: water** will go toward funding water projects.

Allocation Decisions. In this experiment, you will be presented with 13 allocation decision problems. In each problem, you will be endowed with a certain number of Tokens, and you will be asked to allocate these Tokens between yourself and *charity: water* ("the

Charity"). You will do this by deciding the number of Tokens that you would like to pass to the Charity. You cannot pass a negative number of Tokens, and you cannot pass more Tokens than what you are endowed with in any given problem. Any Tokens you choose to pass will be donated to the Charity. Any Tokens you do not pass will be paid to you after the completion of the experiment.

Any Tokens you choose to pass to the Charity may also be matched by the experimenter, increasing the total amount received by the Charity as a result of your donation. The total amount received by the Charity will depend on the *match rate* offered by the experimenter.

> *Match rate*: the match rate is the rate at which your donation will be matched. There are three different match rates you will be presented with in this experiment:
>
> > **0.5:1.** A match rate of 0.5:1 means that for each \$1 you donate, an additional \$0.50 will be provided by the experimenter. Thus, for each \$1 you donate, the charity will receive a total of \$1.50 (your \$1 plus a matching \$0.50 provided by the experimenter).
> >
> > **1:1.** A match rate of 1:1 means that for each \$1 you donate, an additional \$1 will be provided by the experimenter. Thus, for each \$1 you donate, the charity will receive a total of \$2 (your \$1 plus a matching \$1 provided by the experimenter).
> >
> > **2:1.** A match rate of 2:1 means that for each \$1 you donate, an additional \$2 will be provided by the experimenter. Thus, for each \$1 you donate, the charity will receive a total of \$3 (your \$1 plus a matching \$2 provided by the experimenter).

The probability of receiving a match varies across decision problems. After you have submitted decisions for all problems, the computer will randomly determine whether your donation will receive a match, based on the match probability listed in each decision problem. For example, if a problem provides a 25% chance of receiving a match, then the computer will randomly provide a match 25% of the time, and 75% of the time it will not provide a match.

An example of the type of allocation decision problems you will be presented with is given on the next page.

——————————————— Page Break ———————————————

Example Allocation Decision Problems

An example of the type of allocation decision problems you will be presented with is given below. For each decision problem, you are asked to enter the number of Tokens you would like to pass to the Charity. You do this by typing your desired number of Tokens into the box provided in the Pass column. Once you enter the number of Tokens you would like to pass to the Charity, the computer will automatically calculate and display the corresponding values for the remaining columns.

Example Allocation Decision Problems:

	Enter the number of Tokens you would like to pass to the Charity. Pass	The total number of Tokens held for yourself. Your Earnings	Total donation received by the Charity if a match **is not** received. Total Donation (if **no match**)	Total donation received by the Charity if a match **is** received. Total Donation (**with match**)
1.) You are endowed with 80 Tokens. There is a 0% chance that your donation will be matched by the experimenter.	20	60	20	N/A
2.) You are endowed with 80 Tokens. There is a 50% chance that your donation will be matched at a 0.5:1 rate by the experimenter.	30	50	30	45
3.) You are endowed with 80 Tokens. There is a 100% chance that your donation will be matched at a 2:1 rate by the experimenter.			N/A	

Feel free to enter Pass values in the example problems above, and note how the remaining columns automatically fill after you do so. Each of the remaining columns is summarized below:

> *Your Earnings*: this column reports your earnings (in Tokens) for each allocation decision problem, based on your decision of how many Tokens to pass to the Charity.

It is the number of Tokens you hold for yourself, which is equal to your Endowment minus the amount you choose to Pass.

*Total Donation (if **no match**)*: this column reports the total donation (in Tokens) that will be received by the Charity if your donation **does not** end up being matched. This column is equal to the number of Tokens you choose to Pass.

*Total Donation (**with match**)*: this column reports the total donation (in Tokens) that will be received by the Charity if your donation **does** end up being matched. This column is always larger than the number of Tokens you choose to Pass, since it also includes the matching donation provided by the experimenter. The exact amount in this column will depend on the *match rate* offered by the experimenter.

Note that in Example Problem 1 the *Total Donation (**with match**)* column displays "N/A" regardless of the number of Tokens you choose to Pass. This is because in this problem there is a 0% chance that your donation will be matched. Therefore, you know with certainty that your donation will not be matched, and the total donation received by the Charity will be equal to the value listed in the *Total Donation (if **no match**)* column.

Similarly, in Example Problem 3 the *Total Donation (if **no match**)* column displays "N/A" regardless of the number of Tokens you choose to Pass. This is because in this problem there is a 100% chance that your donation will be matched. Therefore, you know with certainty that your donation will be matched, and the total donation received by the Charity will be equal to the value listed in the *Total Donation (**with match**)* column.

For Example Problem 2, there is a 50% chance that your donation will be matched. Therefore, there is a 50% chance that the total donation received by the Charity will be equal to the value listed in the *Total Donation (**with match**)* column, and a corresponding 50% chance that the total donation received by the Charity will be equal to the value listed in the *Total Donation (if **no match**)* column.

On the next page, we'll review how your bonus payment and total donation will be determined. After that, you'll be ready to complete the experiment.

——————————————— Page Break ———————————————

Determining Your Payment and Total Donation. Once you have entered appropriate decisions for each problem, you will be able to submit your decisions. After submitting your decisions, **one problem will be randomly selected to determine your payment and the total donation received by the Charity.** Your decision of how much to pass to the Charity in the randomly selected problem will determine the amount of your bonus earnings in this experiment. Your bonus earnings will be equal to the amount listed in the *Your Earnings* column of the decision problem randomly selected for payment.

You will be obligated to pass to the Charity the amount you have entered in the *Pass* column of the randomly selected problem. <u>This amount, plus any matched funds provided by the experimenter (if applicable), will actually be donated to the Charity.</u>

Informing You of Your Payment and Total Donation. You will be informed of which problem was randomly selected for payment at the end of the experiment. At that time, you will also be informed of your total bonus earnings based on your decision in the randomly selected problem. Your bonus earnings will be paid to you through Prolific within 1-2 business days after you complete the experiment and register your submission through Prolific. The show-up reward, which is in no way affected by your decisions in this experiment, will be automatically paid to you once you register your submission in Prolific.

You will also be informed as to whether or not your donation was randomly determined to receive a match, along with the total donation that will be provided to the Charity on your behalf. The procedure used to determine whether you receive a match is detailed below.

Determining Whether Your Donation Will Receive a Match. The total donation received by the Charity will depend on whether or not your donation ends up receiving a match. The computer will randomly determine whether or not your donation will receive a match, based on the match probability listed in the randomly selected decision problem. For example, if the decision problem randomly selected to determine your payment provides a 25% chance of receiving a match, then the computer will randomly match your donation 25% of the time (and *not* match your donation the remaining 75% of the time).

All donations provided by participants of this study, including any applicable matching contributions (as determined by the procedure detailed above), will be donated to *charity: water* in a single, lump-sum donation at the conclusion of the study. **This donation is backed by the University of Maryland and will actually be provided to the Charity.**

If you have finished reading through these instructions, you are free to continue to the experiment.

Appendix C: Appendix for Chapter 3

C.1 Proofs

Proof of Lemma 1: I will first prove that $g^0 < g^1 \implies g^0 < g^* < g^1$. The proof consists of two parts. First I will show that $g^0 < g^*$, and then I will show that $g^* < g^1$, thus completing the proof that $g^0 < g^1 \implies g^0 < g^* < g^1$.

Recall that g^* is defined by the first order condition $FOC^*(g^*) = pFOC^1(g^*) + (1 - p)FOC^0(g^*) \equiv 0$. Likewise, g^0 and g^1 are defined by the first order conditions $FOC^0(g^0) \equiv 0$ and $FOC^1(g^1) \equiv 0$, respectively. By the second order conditions for U^0 and U^1 (Assumption 1), $FOC^0(g)$ and $FOC^1(g)$ are decreasing in g. Furthermore, since EU is a weighted sum of U^0 and U^1, $FOC^*(g)$ must also be decreasing in g. Thus, it follows that $FOC^*(g^0) = pFOC^1(g^0) + (1 - p)FOC^0(g^0) > 0$, since $g^0 < g^1$ implies $FOC^1(g^0) > 0$. Finally, since $FOC^*(g^0) > 0$, it follows that $g^* > g^0$.

To see that $g^* < g^1$, note that $FOC^*(g^1) = pFOC^1(g^1) + (1 - p)FOC^0(g^1) < 0$, since $g^0 < g^1$ implies $FOC^0(g^1) < 0$. Thus, because $FOC^*(g)$ is decreasing in g, it follows that $g^* < g^1$, thereby demonstrating that $g^0 < g^1 \implies g^0 < g^* < g^1$.

The proof that $g^1 < g^0 \implies g^1 < g^* < g^0$ follows similarly. If $g^1 < g^0$, then $FOC^*(g^0) = pFOC^1(g^0) + (1 - p)FOC^0(g^0) < 0$, since $FOC^1(g)$ is decreasing in g. Therefore, $g^1 < g^0$ implies $g^* < g^0$. Furthermore, $g^1 < g^0$ implies $FOC^*(g^1) = pFOC^1(g^1) + (1-p)FOC^0(g^1) > 0$, since $FOC^0(g)$ is decreasing in g. This implies $g^* > g^1$, thereby demonstrating that $g^1 < g^0 \implies g^1 < g^* < g^0$.

Finally, if $g^0 = g^1$, then both g^0 and g^1 solve $FOC^*(g) = 0$. Thus, $g^* = g^0 = g^1$, completing the proof of Lemma 1.

Proof of Lemma 2: First consider $g^1 > g^0$. From Lemma 1, we have that $g^0 < g^* < g^1$. Therefore, $FOC^0(g^*) < 0$ and $FOC^1(g^*) > 0$, implying $FOC^0(g^*) - FOC^1(g^*) < 0$. That is, $g^1 > g^0 \implies FOC^0(g^*) - FOC^1(g^*) < 0$.

Now consider $g^0 > g^1$. From Lemma 1, we have that $g^1 < g^* < g^0$. Therefore, $FOC^0(g^*) > 0$ and $FOC^1(g^*) < 0$, implying $FOC^0(g^*) - FOC^1(g^*) > 0$. That is, $g^0 > g^1 \implies FOC^0(g^*) - FOC^1(g^*) > 0$.

Finally, consider $g^0 = g^1$. From Lemma 1, we have that $g^* = g^0 = g^1$. Therefore, $FOC^0(g^*) = FOC^1(g^*) = 0$, implying $FOC^0(g^*) - FOC^1(g^*) = 0$. That is, $g^0 = g^1 \implies FOC^0(g^*) - FOC^1(g^*) = 0$, completing the proof of Lemma 2.

Proof of Proposition 2: If $\frac{\partial g^1}{\partial \mu} > (=) < 0$, then $g^1(\mu_1) > (=) < g^1(\mu_0)$ whenever $\mu_1 > \mu_0$. Letting $\mu_0 = 0$ and noting that $g^1(0) = g^0(0) = g^0(\mu)$, it follows that $\frac{\partial g^1}{\partial \mu} > (=) < 0 \implies g^1(\mu) > (=) < g^0(\mu)$. It is now a direct result of Assumption 3 and Lemma 2 that the first addend of the numerator of $\frac{\partial g^*}{\partial \mu}$ will be $> (=) < 0$. Thus, it only remains to show that the second addend of the numerator will be $< (=) > 0$.

Recall that g^1 is the choice of g that maximizes $U^1(g) = U(w - g,\ g,\ (1 + \mu)g + \lambda R_{-i})$. Totally differentiating the associated first order condition with respect to μ gives

$$\frac{\partial g^1}{\partial \mu} = \frac{g^1 \left[U^1_{xR} - U^1_{gR} - (1 + \mu)U^1_{RR}\right] - U^1_R}{|H^1(g^1)|},$$

where $|H^1(g^1)| < 0$ is the second order condition derived from $U^1(g)$, evaluated at g^1. Thus, $\frac{\partial g^1}{\partial \mu} > (=) < 0$ if and only if $g^1 \left[U^1_{xR} - U^1_{gR} - (1 + \mu)U^1_{RR}\right] - U^1_R < (=) > 0$. Since $F(\cdot) \geq 0$, it follows that the second addend of the numerator of $\frac{\partial g^*}{\partial \mu}$ will be $< (=) > 0$, completing the proof.

C.2 Additional Tables

Table C.1: Comparison of Tobit regression models

Pass	(1)	(2)	(3)	(4)	(5)	(6)
Endowment	0.611***	0.593***	0.596***	0.596***	0.595***	0.594***
	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)
Price	-0.296***	-0.356***	0.623**	0.231	0.296	0.895
	(0.036)	(0.037)	(0.278)	(0.310)	(0.314)	(0.656)
Match (=1)	0.072	-0.412***	0.402	0.458*	0.409	0.417*
	(0.045)	(0.098)	(0.249)	(0.250)	(0.253)	(0.253)
Match limit		0.081***	-0.066	-0.076*	-0.017	-0.044
		(0.014)	(0.044)	(0.044)	(0.063)	(0.068)
Group size		-0.126***	-0.122***	0.039	0.132	0.098
		(0.028)	(0.028)	(0.064)	(0.096)	(0.101)
Price*Match limit			-0.178***	-0.191***	-0.185***	-0.315**
			(0.050)	(0.050)	(0.050)	(0.134)
PxN				0.237***	0.186**	-0.101
				(0.084)	(0.093)	(0.291)
MLxN					-0.026	-0.013
					(0.020)	(0.023)
PxMLxN						0.062
						(0.060)
Constant	0.299	0.632	0.620	0.312	0.129	0.207
	(4.219)	(4.154)	(3.983)	(3.885)	(3.934)	(3.856)
Controls	Y	Y	Y	Y	Y	Y
Observations	7722	7722	7722	7722	7722	7722

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Summary demographic data for all subjects

Summary Statistics	μ	σ	Min	Median	Max
Age	40.72	12.057	20	38	76
Knowledge of charity	.93	1.968	0	0	10
Understanding of pay	7.57	2.344	1	8	10
Understanding of donation	7.53	2.339	0	8	10
Confidence in donation	7.51	2.896	0	9	10
SEX					
Female	.41	.494			
Male	.58	.495			
Other	.01	.082			
INCOME					
Don't know/Prefer not to answer	.05	.212			
Less than $50,000	.31	.465			
Between $50,000 and $75,000	.21	.411			
Between $75,000 and $100,000	.17	.374			
Between $100,000 and $150,000	.17	.380			
Between $150,000 and $200,000	.07	.250			
More than $200,000	.02	.140			
POLITICS					
Prefer not to say	.01	.082			
Unsure/Undecided	.01	.115			
Liberal	.54	.500			
Moderate	.23	.420			
Conservative	.21	.411			
RELIGION					
Not important	.48	.501			
Somewhat important	.22	.416			
Important	.15	.362			
Very important	.15	.355			
RECENT DONATIONS					
Less than $20	.43	.497			
Between $20 and $50	.16	.368			
Between $50 and $100	.13	.334			
More than $100	.28	.451			
Observations	150				

Table C.3: Average amount passed (in Tokens) for each match rate and match limit

		0.5:1 Match				1:1 Match				2:1 Match			
	Baseline	LOW	MED	HIGH	INF	LOW	MED	HIGH	INF	LOW	MED	HIGH	INF
mean	21.78	21.63	23.58	25.64	23.42	20.97	22.38	24.53	23.34	19.91	21.37	23.45	26.67
semean	1.10	1.06	1.13	1.17	1.17	1.01	1.04	1.08	1.08	1.00	1.01	1.03	1.16
min	0	0	0	0	0	0	0	0	0	0	0	0	0
p50	10	10	14	15	10	12	15	17.5	15	10	15	16	20
max	120	120	120	120	120	120	120	120	120	120	120	120	120
count	594	594	594	594	594	594	594	594	594	594	594	594	594

Table C.4: Comparing stages in a combined model, no interaction terms

Charity receipts	Stage 1 (1)	Stage 2 (2)	Stage 3 (3)	Stage 4 (4)	p-value[a]
Endowment	0.655***	0.627***	0.523***	0.651***	.880
	(0.126)	(0.127)	(0.127)	(0.128)	
Price	-1.367***	-1.284***	-1.307***	-1.291***	.888
	(0.082)	(0.082)	(0.082)	(0.084)	
Match limit	0.102***	0.065**	0.089***	0.073**	.853
	(0.032)	(0.032)	(0.031)	(0.033)	
Group size	-0.132*	0.037	-0.441***	0.018	.000
	(0.074)	(0.074)	(0.074)	(0.075)	
Match (=1)	-0.510**	-0.425**	-0.508**	-0.365	.959
	(0.219)	(0.215)	(0.213)	(0.224)	
Constant	0.301	0.232	1.572	0.083	
	(4.608)	(4.607)	(4.612)	(4.612)	
Controls	Y	Y	Y	Y	
Observations	7722	7722	7722	7722	

Notes: Standard errors in parentheses. Reported coefficient values come from a combined log-log Tobit regression model for total charity receipts (assuming a match is received), accounting for censoring from below and above. The combined model includes dummy variables for each stage, along with a full set of interactions between each stage dummy and all covariates. The coefficient estimates reported for each stage are calculated by combining the appropriate terms from the full model, using the delta method to calculate standard errors. The full set of controls is included in the combined model, but the controls are not interacted with stage dummies.

[a] Reported p-values are for Wald tests of joint equality between the coefficients for all stages.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Comparing stages in a combined model, with interaction terms

Charity receipts	Stage 1 (1)	Stage 2 (2)	Stage 3 (3)	Stage 4 (4)	p-value[a]
Endowment	0.656***	0.631***	0.527***	0.655***	.885
	(0.125)	(0.126)	(0.126)	(0.128)	
Price	-1.574**	-0.773	0.323	-0.901	.266
	(0.693)	(0.681)	(0.677)	(0.710)	
Match limit	0.064	-0.144	-0.105	-0.120	.419
	(0.098)	(0.096)	(0.094)	(0.101)	
Group size	0.032	0.352**	-0.553***	0.342**	.000
	(0.147)	(0.147)	(0.147)	(0.150)	
Match (=1)	-0.301	0.739	0.570	0.709	.509
	(0.561)	(0.547)	(0.539)	(0.576)	
Price*Match limit	-0.046	-0.256**	-0.234**	-0.236**	.512
	(0.113)	(0.110)	(0.108)	(0.116)	
PxN	0.239	0.455**	-0.171	0.467**	.056
	(0.186)	(0.187)	(0.187)	(0.191)	
Constant	-0.017	-0.403	1.777	-0.556	
	(4.613)	(4.612)	(4.618)	(4.616)	
Controls	Y	Y	Y	Y	
Observations	7722	7722	7722	7722	

Notes: Standard errors in parentheses. Reported coefficient values come from a combined log-log Tobit regression model for total charity receipts (assuming a match is received), accounting for censoring from below and above. The combined model includes dummy variables for each stage, along with a full set of interactions between each stage dummy and all covariates. The coefficient estimates reported for each stage are calculated by combining the appropriate terms from the full model, using the delta method to calculate standard errors. The full set of controls is included in the combined model, but the controls are not interacted with stage dummies.

[a] Reported p-values are for Wald tests of joint equality between the coefficients for all stages.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Average amount passed (in Tokens) in each stage, by match rate

	Stage 1	Stage 2	Stage 3	Stage 4	p-value[a]
Baseline	19.15	22.75	23.54	21.69	.518
	(1.94)	(2.23)	(2.35)	(2.30)	
0.5:1 Match	23.84	23.16	24.48	22.75	.717
	(1.14)	(1.11)	(1.17)	(1.11)	
1:1 Match	22.53	22.55	23.46	22.68	.910
	(1.03)	(1.02)	(1.08)	(1.08)	
2:1 Match	23.15	21.84	24.09	22.30	.447
	(1.04)	(1.00)	(1.13)	(1.05)	

Notes: Standard errors in parentheses. Average amount passed (in Tokens) in each stage, averaged by all questions of the same match rate. The stage numbers refer to the order in which subjects completed the experiment. For example Stage 1 is the first stage of the experiment, and Stage 4 is the last stage of the experiment. This table provides insights into whether or not average behavior varies systematically as subjects progress through the experiment.

[a] Reported p-values are exact p-values from Wilks' lambda tests of equality across all stages, assuming homogeneity.

Table C.7: Average amount passed at each match rate

	Baseline	0.5:1 Match	1:1 Match	2:1 Match
mean	21.78114	23.56481	22.80471	22.84891
semean	1.103122	.5663019	.5263568	.5275196
min	0	0	0	0
p50	10	12	15	15
max	120	120	120	120
count	594	2376	2376	2376

Table C.8: Average amounts passed in no-match and 0.5:1 match-rate questions by stage

	Baseline S1	Baseline S2	Baseline S3	Baseline S4	Match 05 S1	Match 05 S2	Match 05 S3	Match 05 S4
mean	19.14667	22.74667	23.54	21.6875	23.84333	23.15667	24.47833	22.74826
semean	1.935385	2.230763	2.349785	2.296992	1.143581	1.107865	1.168888	1.107845
min	0	0	0	0	0	0	0	0
p50	10	11	10	10	12	12	14	10
max	120	120	115	120	120	120	120	120
count	150	150	150	144	600	600	600	576

Table C.9: Average amounts passed in 1:1 and 2:1 match-rate questions by stage

	Match 1 S1	Match 1 S2	Match 1 S3	Match 1 S4	Match 2 S1	Match 2 S2	Match 2 S3	Match 2 S4
mean	22.52667	22.54667	23.46333	22.67708	23.15333	21.83833	24.085	22.29688
semean	1.031806	1.020241	1.080555	1.080526	1.040934	.9966071	1.128061	1.049529
min	0	0	0	0	0	0	0	0
p50	15	15	15	15	15	15	15	15
max	120	120	120	120	120	120	120	120
count	600	600	600	576	600	600	600	576

Table C.10: Average amount passed by match rate and stage, for each group size/endowment combo

		N=5					N=10				
		Stage 1	Stage 2	Stage 3	Stage 4	p-value[a]	Stage 1	Stage 2	Stage 3	Stage 4	p-value[a]
Endow=80	Baseline	16.41	19.58	17.94	15.49	.862	17.84	19.83	13.50	23.83	.275
		(3.41)	(3.58)	(4.53)	(3.18)		(3.21)	(3.93)	(3.44)	(4.39)	
	0.5:1 Match	19.12	22.9	20.49	18.59	.352	19.80	21.29	13.19	22.06	.002
		(1.70)	(1.91)	(2.21)	(1.73)		(1.71)	(1.95)	(1.52)	(1.88)	
	1:1 Match	19.80	22.96	18.90	20.96	.415	20.33	20.95	12.61	21.28	.001
		(1.64)	(1.67)	(2.11)	(1.82)		(1.67)	(1.88)	(1.34)	(1.83)	
	2:1 Match	20.85	22.91	18.45	21.03	.383	20.07	19.01	13.29	20.91	.005
		(1.61)	(1.65)	(2.03)	(1.87)		(1.73)	(1.64)	(1.42)	(1.79)	
	count	39	40	33	37		37	35	36	40	
Endow=120	Baseline	26.24	23.69	35.58	18.56	.122	14.69	27.37	25.60	29.97	.155
		(4.56)	(5.93)	(5.47)	(4.49)		(3.75)	(4.48)	(4.36)	(6.20)	
	0.5:1 Match	28.22	25.05	38.46	20.44	.000	28.53	23.51	24.63	31.27	.126
		(2.35)	(3.04)	(2.79)	(2.18)		(3.22)	(2.04)	(2.11)	(3.05)	
	1:1 Match	26.11	24.99	34.78	20.48	.000	23.69	21.65	26.05	29.08	.145
		(2.14)	(2.88)	(2.54)	(2.00)		(2.76)	(1.83)	(2.00)	(3.04)	
	2:1 Match	26.54	24.38	35.61	19.00	.000	25.09	21.25	27.26	29.42	.091
		(2.16)	(2.81)	(2.63)	(1.78)		(2.79)	(1.88)	(2.21)	(2.96)	
	count	42	32	38	36		32	43	43	31	

Notes: Standard errors in parentheses. Each panel of table displays data for a different group size/endowment combination. Each panel shows the average amount passed (in Tokens) in each stage, averaged by all questions of the same match rate (and same group size/endowment combination). The stage numbers refer to the order in which subjects completed the experiment. For example Stage 1 is the first stage of the experiment, and Stage 4 is the last stage of the experiment. Count lists the number of subjects who face the given group size/endowment combination in each stage of the experiment. Note that for each match rate there are four questions (with varying match limits), so the number of observations for the match rate averages is four times the number of subjects. This table provides insights into whether or not average behavior varies systematically as subjects progress through the experiment, based on when they encounter each group size/endowment combination.

[a] Reported p-values are exact p-values from Wilks' lambda tests of equality across all stages, assuming homogeneity.

Table C.11: Estimated probability of receiving a match in each question of experiment, based on observed behavior

			Estimated Probability of Receiving a Match			
Question	Match Rate	Match Limit	Stage 1 (N=5, E=80)	Stage 2 (N=10, E=80)	Stage 3 (N=5, E=120)	Stage 4 (N=10, E=120)
1	0	0	.0023[a]	0	.0015[a]	0
2	0.5	160	.909	.4295	.6982	.1891
3	0.5	240	.9921	.7892	.9056	.3975
4	0.5	320	1	.9046	.9846	.655
5	0.5	∞	1	1	1	1
6	1	120	.7281	.2146	.5719	.0709
7	1	160	.8931	.353	.7348	.1392
8	1	240	.9912	.7305	.9035	.4379
9	1	∞	1	1	1	1
10	2	80	.4586	.0617	.3427	.0216
11	2	120	.7167	.1851	.5479	.0601
12	2	160	.8487	.3095	.6813	.1246
13	2	∞	1	1	1	1

Notes: Table reports the estimated true match probabilities in each question, based on observed behavior in the experiment. Reported match probabilities for each question are calculated by forming 10,000 random groups (of either size $N = 5$ or $N = 10$, depending on the question), summing the total donations of the group, and determining whether the total donations of the group are less than or equal to the match limit for the question (in which case a match is received) or whether the total donations exceed the match limit (in which case no match is received). The estimated match probabilities reported are the proportion of random groups for which total donations are less than or equal to the match limit for the question. Note that, because the match determinations for each subject are based entirely on the donation decisions of their randomly selected group members, their own decision of how much to donate has no impact on their probability of receiving a match. Therefore, match probabilities for each subject are exogenous to their own decisions, and the reported values in this table are a good approximation for the true match probabilities faced by each subject. (Each subject technically faces slightly different match probabilities, since the calculation of their probabilities will exclude their own decisions from the sample when drawing random groups. However, in practice this effect is negligible.)

[a] Estimated match probabilities in the no-match questions can be nonzero because of the process used to determine whether a match is received. A match is received if the total donations of the other members of a subject's group are less than *or equal* to the match limit. Thus, a match can be 'received' in the no-match questions as long as every member of the group passes nothing. This rarely occurs when the group size is 5 ($N = 5$), and is never observed to occur when the group size is 10 ($N = 10$), reflecting the lower likelihood of being matched with group members who all choose to pass nothing as the group size increases. Note that 'receiving' the match is meaningless in these questions, since the match rate is equal to 0.

Table C.12: Regressions using observed match probabilities

Pass	(1)	(2)	(3)	(4)	(5)	(6)
Endowment	0.610***	0.685***	0.673***	0.762***	0.751***	0.739***
	(0.048)	(0.049)	(0.049)	(0.059)	(0.059)	(0.064)
Price	-0.295***	-0.399***	-0.187**	-0.420***	-0.205**	0.260
	(0.036)	(0.038)	(0.085)	(0.039)	(0.087)	(0.326)
Match (=1)	0.070	-0.176***	0.006	0.118	0.315*	0.715**
	(0.045)	(0.055)	(0.085)	(0.146)	(0.162)	(0.307)
Match probability[a]		0.257***	0.004	0.461***	0.204	0.351**
		(0.032)	(0.097)	(0.095)	(0.132)	(0.169)
Price*Match prob.[a]			-0.310***		-0.314***	-0.054
			(0.112)		(0.112)	(0.252)
Match limit				-0.081**	-0.084**	-0.173**
				(0.036)	(0.036)	(0.069)
Group size				0.118**	0.111**	0.132
				(0.056)	(0.056)	(0.082)
Price*Match limit						-0.140
						(0.103)
PxN						0.069
						(0.146)
Constant	-1.097***	-1.441***	-1.388***	-2.027***	-1.962***	-1.947***
	(0.325)	(0.327)	(0.328)	(0.418)	(0.418)	(0.418)
Observations	7800	7800	7800	7800	7800	7800

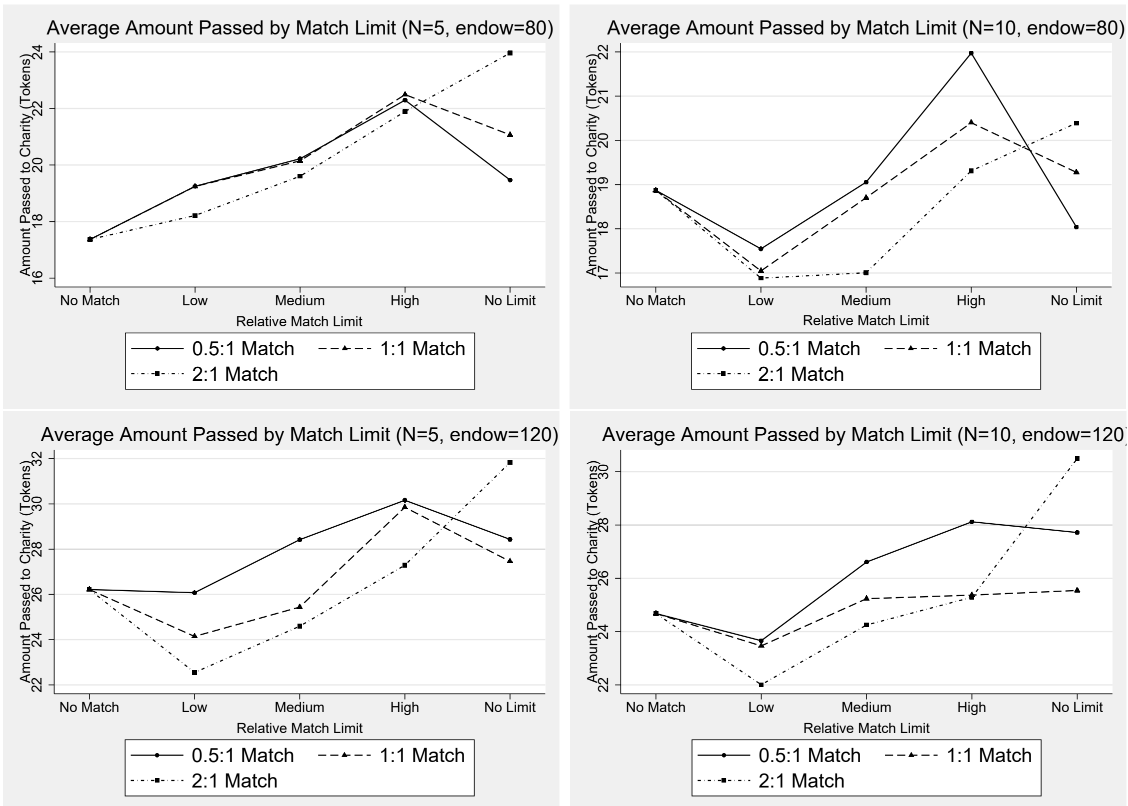
Notes: Standard errors in parentheses. All columns show the results of Tobit regressions of the log of the number of Tokens passed on the log of endowment (in Tokens), log price, and a match dummy. All models also account for censoring from above (at the endowment level) and below (at 0). The match probability covariate is constructed from observed behavior in the experiment; see Table C.11 for more details. Price*Match prob. is an interaction between the log-price and the constructed match probabilities. The remaining covariates are all logged, and the interaction terms interact the log variables.

[a] Match probabilities are estimated using subjects' observed donation decisions in each question. See Table C.11 for a discussion of the procedure used to calculate the match probability for each question, as well as a tabulation of the estimated match probabilities.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

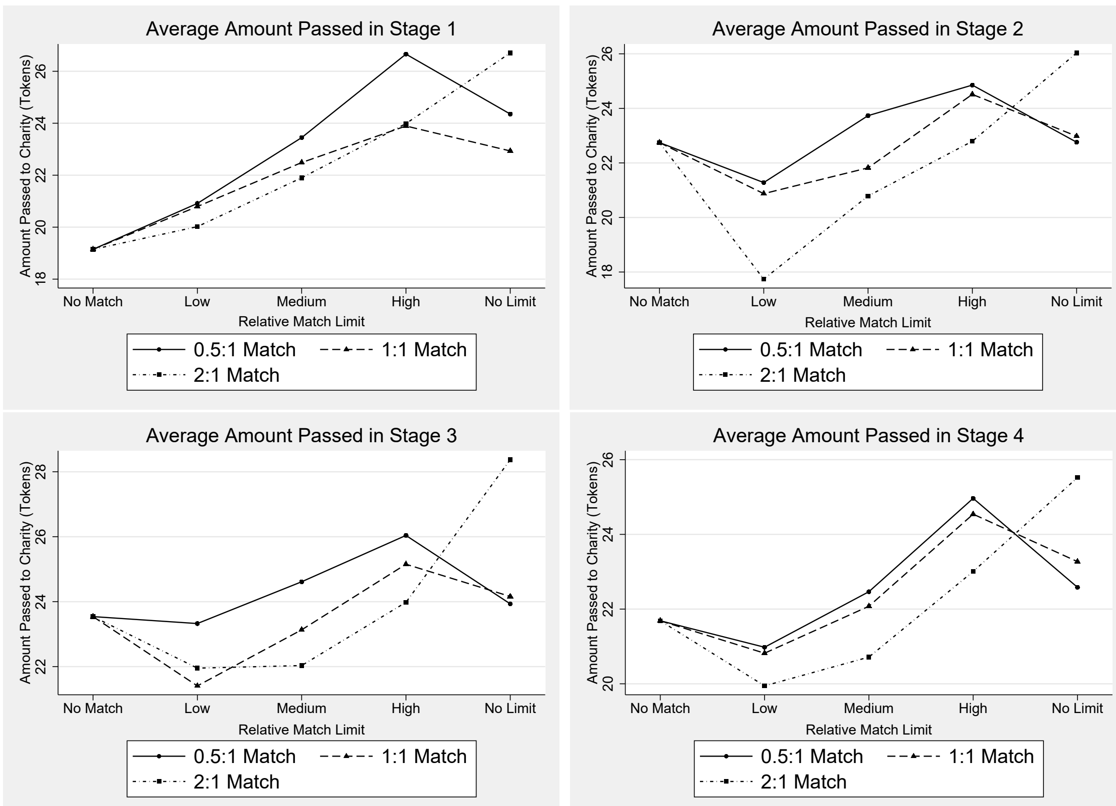
C.3 Additional Figures

Figure C.1: Average amount passed at each match limit for each match rate, graphed separately for each group size and endowment combination.



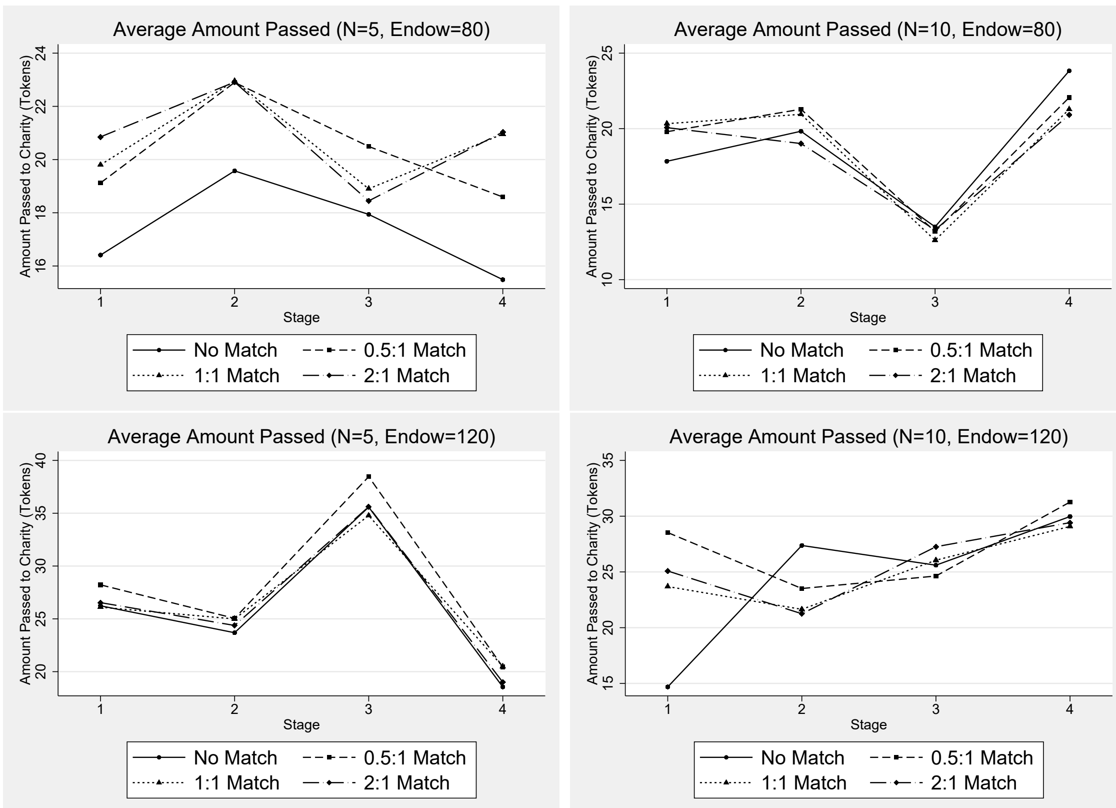
Notes: Figure plots the average amount passed (i.e., out-of-pocket donation) by all subjects at each match rate and match limit, graphed separately for each unique stage, as defined by the combination of the group size and endowment. Here, 'stage' does not refer to the order in which subjects complete the main task of the experiment. To make this distinction clear, each graph is labeled by its parameters (e.g., $N = 5$, endow $= 80$). The purpose of plotting each stage separately is to provide insight into the extent to which subject behavior changes in response to changes in group size and endowment. Note that the match limits are different for each match rate. Therefore, to make the match rates comparable and to plot them all within the same graph, I redefine each match rate's match limits as LOW, MEDIUM, and HIGH.

Figure C.2: Average amount passed at each match limit for each match rate, graphed separately for each stage.



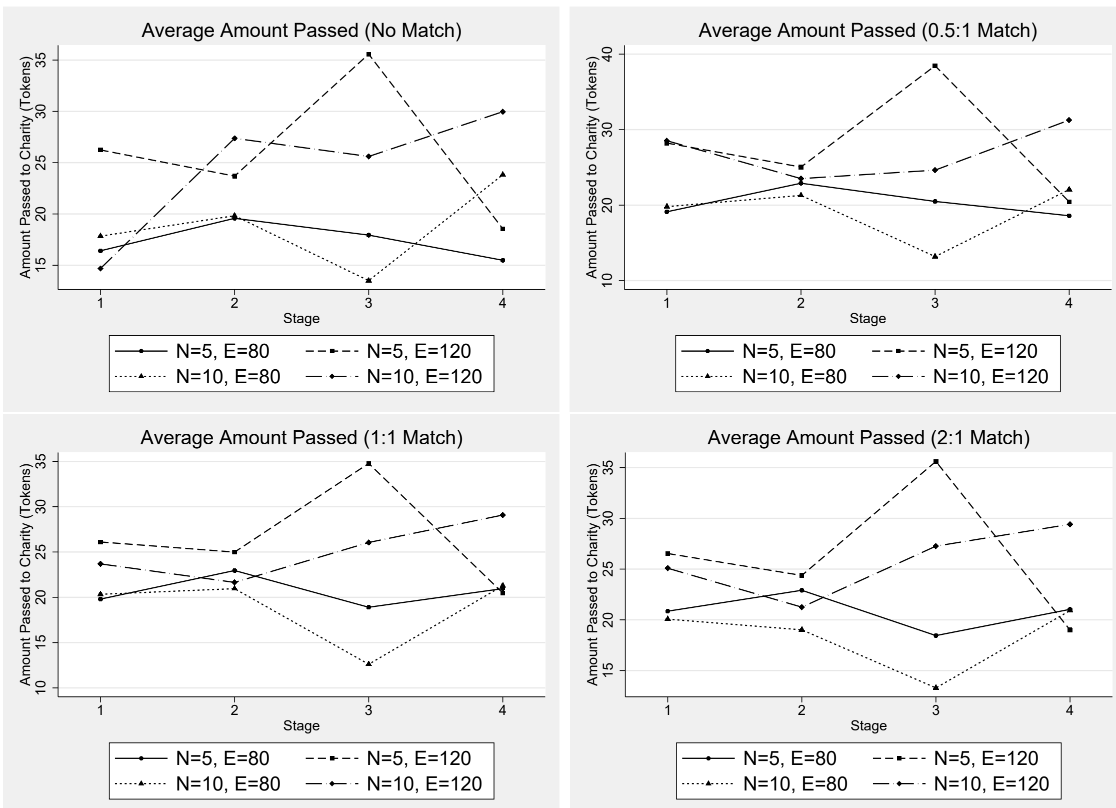
Notes: Figure plots the average amount passed (i.e., out-of-pocket donation) by all subjects at each match rate and match limit, graphed separately for each stage. Here, 'stage' refers to the stage order (e.g., the first stage, the second stage, etc.), not the unique group size and endowment combination that defines the different stages. The purpose of plotting each stage separately is to provide insight into the extent to which subject behavior changes over time, as they are exposed to additional stages. Note that the match limits are different for each match rate. Therefore, to make the match rates comparable and to plot them all within the same graph, I redefine each match rate's match limits as LOW, MEDIUM, and HIGH.

Figure C.3: Average amount passed at each match rate in each stage, graphed separately for each group size/endowment combo.



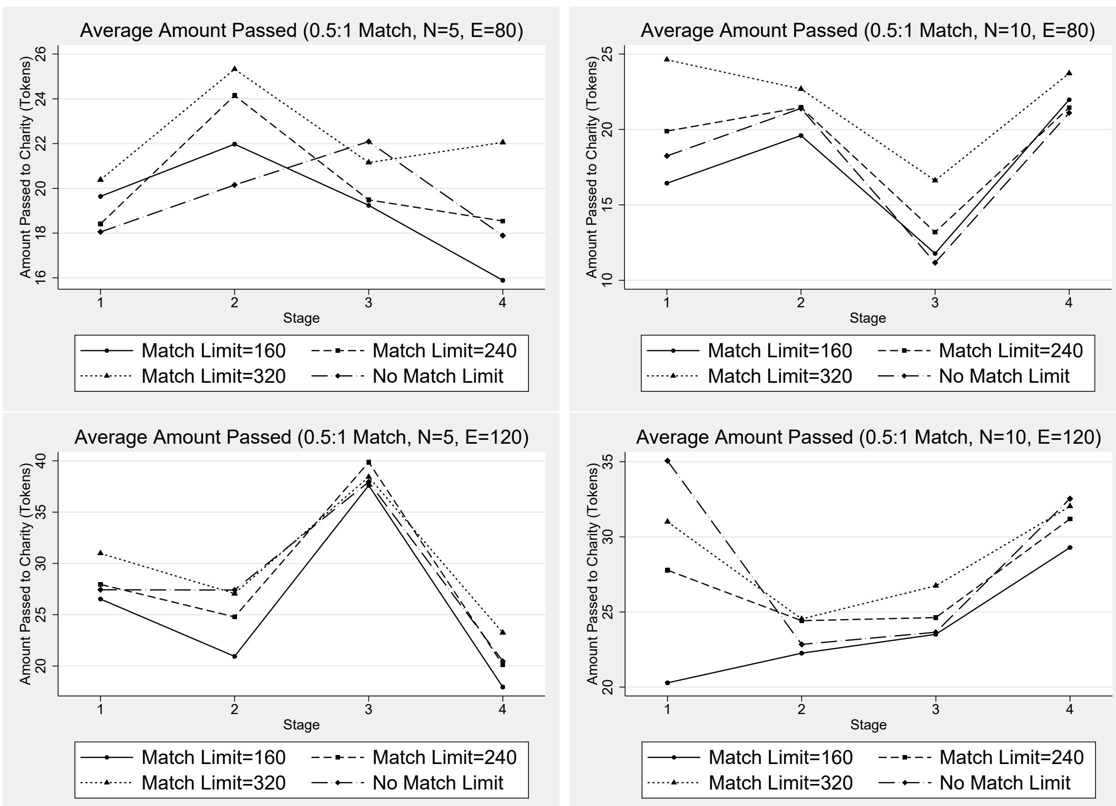
Notes: Top panels: Endow=80. Bottom panels: Endow=120. Left-side panels: N=5. Right-side panels: N=10. Figure plots the average amount passed (i.e., out-of-pocket donation) by all subjects in each stage for each match rate, graphed separately for each group size/endowment combo. Here, 'stage' refers to the stage order (e.g., the first stage, the second stage, etc.) that subjects complete the main stage of the experiment in, not the unique group size and endowment combination that defines the different stages. The purpose of plotting each group size/endowment combination separately is to provide insight into the extent to which subject behavior changes over time (i.e., across stages of the experiment), while holding the group size and endowment constant. Without controlling for group size and endowment, the averages across stages may vary simply as a result of variation in the number of subjects facing each group size and endowment in each stage.

Figure C.4: Average amount passed for each group size/endowment combo in each stage, graphed separately for each match rate.



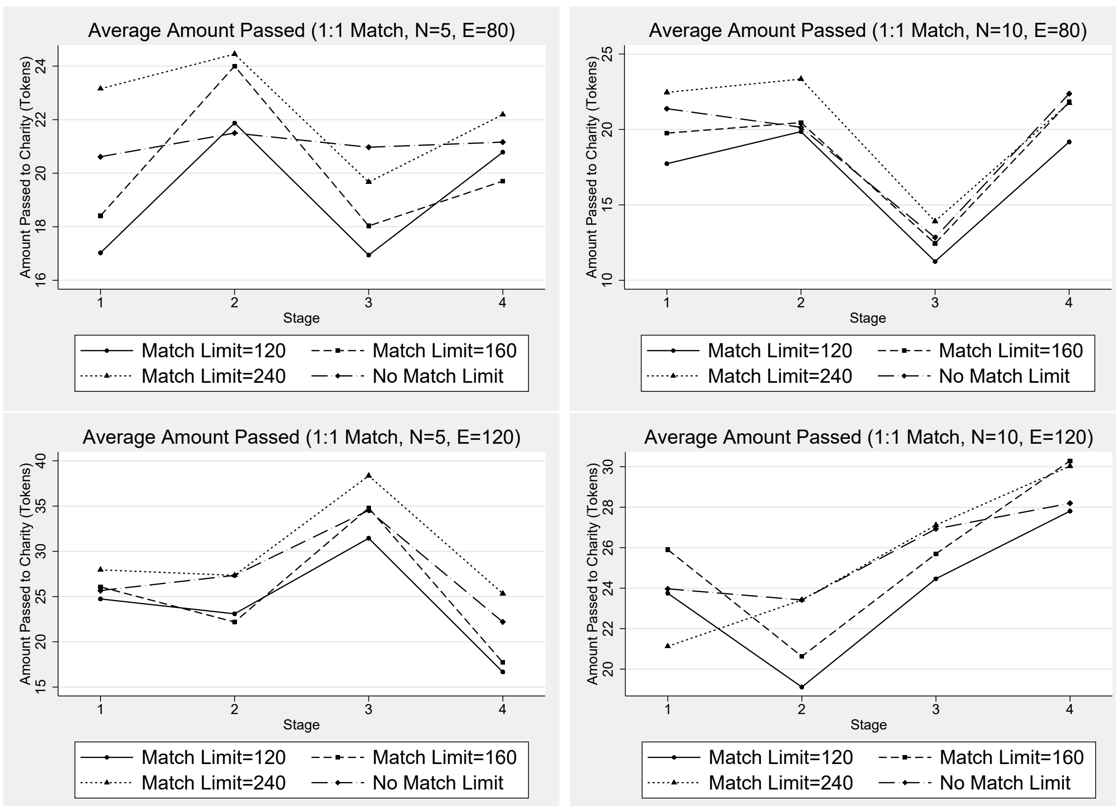
Notes:

Figure C.5: Average amount passed in 0.5:1 match-rate questions in each stage, graphed separately for each group size/endowment combo.



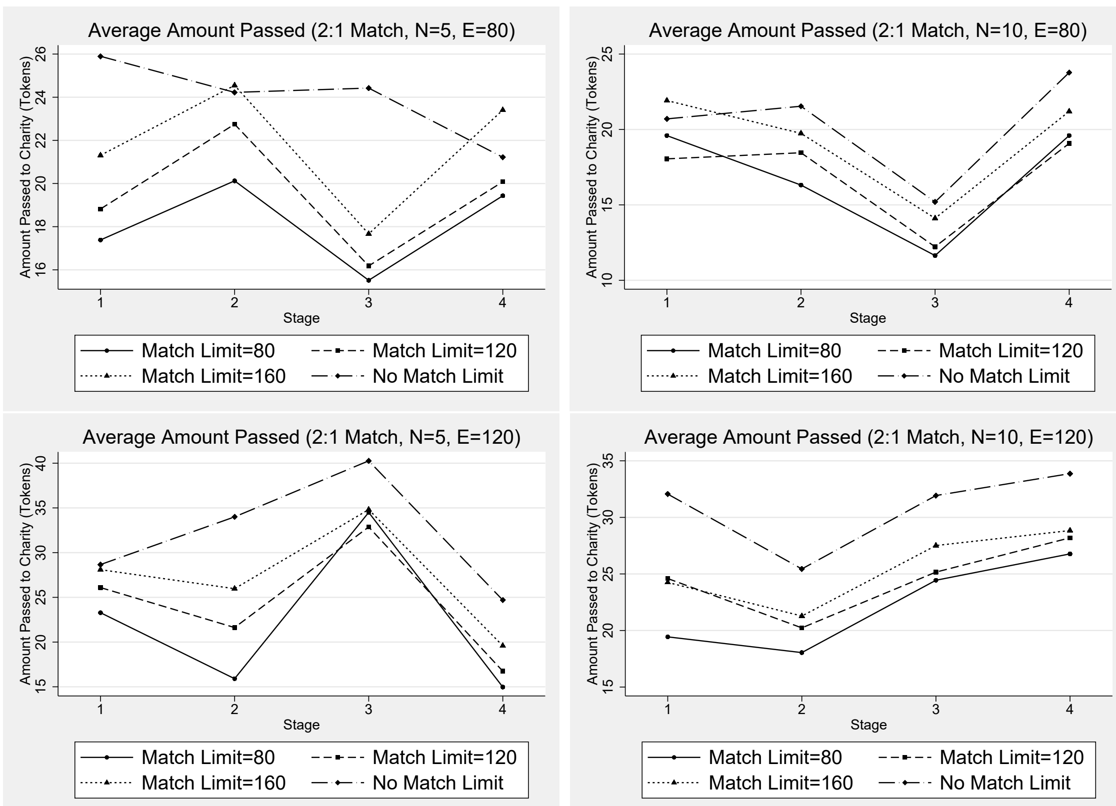
Notes:

Figure C.6: Average amount passed in 1:1 match-rate questions in each stage, graphed separately for each group size/endowment combo.



Notes:

Figure C.7: Average amount passed in 2:1 match-rate questions in each stage, graphed separately for each group size/endowment combo.



Notes:

C.4 Experiment Materials

C.4.1 Survey questions

The following questions were included as part of a survey subjects were asked to complete after completing the main task and follow-up tasks of the experiment, but before being informed of their bonus payment amount. A response was required for each question.

1. What is your age in years? [**subjects enter an integer value into a text-entry box**]

2. What is your gender?

   a) Male

   b) Female

   c) Other

3. What is you best estimate of your family's annual Income? In addition to your own personal earnings, include income earned by your parents and/or guardians if they give you financial support in whole or in part.

   a) Less than $50,000

   b) Between $50,000 and $75,000

   c) Between $75,000 and $100,000

   d) Between $100,000 and $150,000

   e) Between $150,000 and $200,000

   f) More than $200,000

   g) Don't know/Prefer not to answer

4. How would you describe your political views?

a) conservative

b) moderate

c) liberal

d) unsure/undecided

e) prefer not to say

5. How important is religion in your life?

a) very important

b) important

c) somewhat important

d) not important

6. During the past 12 months, how much money have you donated to charitable causes?

a) Less than $20

b) Between $20 and $50

c) Between $50 and $100

d) More than $100

7. How well do you know **charity: water**? Please rate your prior knowledge on a 0 to 10 scale, where 0 indicates no prior information at all and 10 indicates perfect knowledge: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

8. How well did you understand how your earnings are calculated in this experiment? Please rate your understanding on a 0 to 10 scale, where 0 indicates no understanding at all and 10 indicates a perfect understanding: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

9. How well did you understand how your total donation is calculated in this experiment? Please rate your understanding on a 0 to 10 scale, where 0 indicates no understanding at all and 10 indicates a perfect understanding: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

10. While making your donation decisions in this experiment, how confident were you that your donation (and any applicable matching donation) will actually be donated to **charity: water** on your behalf? Please rate your level of confidence on a 0 to 10 scale, where 0 indicates no confidence at all and 10 indicates complete confidence: [**subjects enter an integer value (between 0 and 10) into a text-entry box**]

C.4.2 Donation follow-up task

Donation Follow-up Task

Please answer the following questions. For each question, select whether you prefer Option A or Option B.

Option A will always be *the Charity receives a $1 donation with probability 50%, and a $0 donation otherwise.*

Option B will be *the Charity receives a donation of some dollar amount*. As you proceed down the rows of the list, the amount of the donation received by the Charity will increase from $0 to $1.

For each row, all you have to do is decide whether you prefer Option A or Option B. Indicate your preference by selecting the corresponding button. **Most people begin by preferring Option A and then switch to Option B, so one way to complete this list is to determine at which row you prefer to switch from Option A to Option B.**

At the conclusion of the experiment, one row will be randomly selected, and a donation will be made to the Charity on your behalf based on the decision you have made in the selected row. If you have selected the lottery option (Option A) in the randomly selected row, the computer will flip a coin to determine the outcome of the lottery. All donations will actually be donated to the Charity, and you will be informed of the outcome of this process at the conclusion of the experiment. **(Note that any donations provided to the Charity in this task are provided by the experimenter and do not affect your earnings.)**

	Select your preferred option.		
	Option A	Option B	
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.10 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.20 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.30 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.40 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.50 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.60 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.70 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.80 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$0.90 donation
$1 donation with probability 50%, and $0 donation otherwise	○	○	$1 donation

Figure C.8: Donation follow-up task example screen

C.4.3 Payment follow-up task

Payment Follow-up Task

Please answer the following questions. For each question, select whether you prefer Option A or Option B.

Option A will always be *you receive a $1 payment with probability 50%, and a $0 payment otherwise.*

Option B will be *you receive a payment of some dollar amount*. As you proceed down the rows of the list, the amount of the payment you receive will increase from $0 to $1.

For each row, all you have to do is decide whether you prefer Option A or Option B. Indicate your preference by selecting the corresponding button. **Most people begin by preferring Option A and then switch to Option B, so one way to complete this list is to determine at which row you prefer to switch from Option A to Option B.**

At the conclusion of the experiment, one row will be randomly selected, and you will be paid based on the decision you have made in the selected row. If you have selected the lottery option (Option A) in the randomly selected row, the computer will flip a coin to determine the outcome of the lottery. You will be informed of the outcome of this process at the conclusion of the experiment.

	Select your preferred option.		
	Option A	Option B	
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.10 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.20 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.30 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.40 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.50 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.60 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.70 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.80 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$0.90 payment
$1 payment with probability 50%, and $0 payment otherwise	○	○	$1 payment

Figure C.9: Payment follow-up task example screen

C.4.4 Main task example decision sheet: N=5, Endow=80

Below are 13 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how to allocate the endowment liste
between yourself and *charity: water* ("the Charity").

Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instruction

In this stage, you are grouped with 5 other participants, and you are each endowed with 80 Tokens.

	Enter the number of Tokens you would like to pass to the Charity. Pass	The total number of Tokens held for yourself. Your Earnings	Total donation received by the Charity if a match **is not** received. Total Donation (if **no match**)	To C T
1.) You, and each of the 5 other members of your group, are endowed with 80 Tokens.	7	73	7	
2.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 80 Tokens.	0	80	0	
3.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 120 Tokens.	15	65	15	
4.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 160 Tokens.	11	69	11	
5.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match all donations at a 2:1 rate.	13	67	N/A	
6.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 120 Tokens.	28	52	28	
7.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 160 Tokens.	44	36	44	
8.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 240 Tokens.	26	54	26	
9.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match all donations at a 1:1 rate.			N/A	
10.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 160 Tokens.				
11.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 240 Tokens.				
12.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 320 Tokens.				
13.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match all donations at a 0.5:1 rate.			N/A	

Figure C.10: Main task example screen: N=5, Endow=80

C.4.5 Main task example decision sheet: N=10, Endow=80

Below are 13 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how to allocate the endowment list
between yourself and *charity: water* ("the Charity").

Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instructio

In this stage, you are grouped with 10 other participants, and you are each endowed with 80 Tokens.

	Enter the number of Tokens you would like to pass to the Charity. Pass	The total number of Tokens held for yourself. Your Earnings	Total donation received by the Charity if a match **is not** received. Total Donation (if **no match**)
1.) You, and each of the 10 other members of your group, are endowed with 80 Tokens.	6	74	6
2.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 80 Tokens.	12	68	12
3.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 120 Tokens.	14	66	14
4.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 160 Tokens.	18	62	18
5.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match all donations at a 2:1 rate.	10	70	N/A
6.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 120 Tokens.	5	75	5
7.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 160 Tokens.	33	47	33
8.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 240 Tokens.	17	63	17
9.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match all donations at a 1:1 rate.			N/A
10.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 160 Tokens.			
11.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 240 Tokens.			
12.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 320 Tokens.			
13.) You, and each of the 10 other members of your group, are endowed with 80 Tokens. The experimenter will match all donations at a 0.5:1 rate.			N/A

Figure C.11: Main task example screen: N=10, Endow=80

C.4.6 Main task example decision sheet: N=5, Endow=120

Below are 13 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how to allocate the endowment list
between yourself and *charity: water* ("the Charity").

Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instructio

In this stage, you are grouped with 5 other participants, and you are each endowed with 120 Tokens.

	Enter the number of Tokens you would like to pass to the Charity. Pass	The total number of Tokens held for yourself. Your Earnings	Total donation received by the Charity if a match **is not** received. Total Donation (if **no match**)
1.) You, and each of the 5 other members of your group, are endowed with 120 Tokens.	10	110	10
2.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 80 Tokens.	15	105	15
3.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 120 Tokens.	20	100	20
4.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 160 Tokens.	20	100	20
5.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match all donations at a 2:1 rate.	21	99	N/A
6.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 120 Tokens.	31	89	31
7.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 160 Tokens.	25	95	25
8.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 240 Tokens.	37	83	37
9.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match all donations at a 1:1 rate.			N/A
10.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 160 Tokens.			
11.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 240 Tokens.			
12.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 320 Tokens.			
13.) You, and each of the 5 other members of your group, are endowed with 120 Tokens. The experimenter will match all donations at a 0.5:1 rate.			N/A

Figure C.12: Main task example screen: N=5, Endow=120

C.4.7 Main task example decision sheet: N=10, Endow=120

Below are 13 allocation problems. Read each allocation problem carefully. For each allocation problem, you must decide how to allocate the endowment liste
between yourself and *charity: water* ("the Charity").

Remember that only one of the problems will be randomly selected to determine payment. If you would like to review the instructions, click here: Instructio

In this stage, you are grouped with 10 other participants, and you are each endowed with 120 Tokens.

	Enter the number of Tokens you would like to pass to the Charity. Pass	The total number of Tokens held for yourself. Your Earnings	Total donation received by the Charity if a match **is not** received. Total Donation (if **no match**)	T C T
1.) You, and each of the 10 other members of your group, are endowed with 120 Tokens.	11	109	11	
2.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 80 Tokens.	15	105	15	
3.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 120 Tokens.	10	110	10	
4.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 2:1 rate up to a match limit of 160 Tokens.	20	100	20	
5.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match all donations at a 2:1 rate.	31	89	N/A	
6.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 120 Tokens.	33	87	33	
7.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 160 Tokens.	25	95	25	
8.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 240 Tokens.	17	103	17	
9.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match all donations at a 1:1 rate.			N/A	
10.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 160 Tokens.				
11.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 240 Tokens.				
12.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match donations at a 0.5:1 rate up to a match limit of 320 Tokens.				
13.) You, and each of the 10 other members of your group, are endowed with 120 Tokens. The experimenter will match all donations at a 0.5:1 rate.			N/A	

Figure C.13: Main task example screen: N=10, Endow=120

C.4.8 Instructions

Introduction. Thank you for participating in this online experiment. This experiment is interested in studying how individuals make decisions. You will be making decisions individually. Your decisions and earnings during the experiment will be confidential and will only be associated with an ID number.

Compensation. You will be compensated for your participation. At the end of the experiment, you will receive a show-up reward of $9. This show-up reward is not contingent on the decisions that you make during the experiment, and it will be yours to keep just for participating. You will automatically receive this payment through Prolific upon completion of the experiment.

In addition to the show-up reward, you will also have an opportunity to earn additional money, which will be paid to you through Prolific in the form of a bonus payment. The amount of your bonus payment will depend on the decisions you make in the experiment and luck, as will be explained in detail below.

During the experiment, your earnings will be calculated in Tokens. At the end of the experiment the total amount of Tokens you have earned will be converted to US Dollars at the following rate:

10 Tokens = 1.00 US Dollar

Your $ earnings (including the $4 show-up reward and any bonus payments you earn) will be paid to you in private through Prolific. The show-up reward will be paid automatically once you have registered your submission in Prolific. Your bonus payment will be paid through

Prolific within 1-2 business days after your submission has been received. **Bonus payments are backed by the University of Maryland and are guaranteed to be paid to you within 1-2 business days after you register your survey response in Prolific.** If you do not receive your bonus payment with 2 business days, or if you believe an error has been made regarding your bonus payment, please reach out to the research team using the contact information provided in the Consent Form.

The Charity. During the experiment you will be provided with opportunities to make donations to **charity: water**, a nonprofit organization that works to bring safe and clean drinking water to the more than 700 million people in the world living without access to clean water. The majority of people without access to clean water live in isolated rural areas, and they must spend hours every day walking many miles to collect water for their families. This water often carries diseases that lead to sickness. **charity: water** works with local experts and community members to install sustainable water solutions, including wells, piped water systems, BioSand Filters, and systems for harvesting rainwater. 100% of any donations you provide to **charity: water** will go toward funding water projects.

Allocation Decisions. In this experiment, you will be presented with various allocation decision problems. In each problem, you will be endowed with a certain number of Tokens, and you will be asked to allocate these Tokens between yourself and *charity: water* ("the Charity"). You will do this by deciding the number of Tokens that you would like to pass to the Charity. You cannot pass a negative number of Tokens, and you cannot pass more Tokens than what you are endowed with in any given problem. Any Tokens you choose to pass will be donated to the Charity. Any Tokens you do not pass will be paid to you after the completion of the experiment.

In some problems a *matching grant* will be provided by the experimenter. *Matching grants* are common among fundraisers, and you have likely encountered them before. Fundraisers using a *matching grant* may solicit donations with a message along the lines of, "Any donations you provide will be matched by a generous donor up to $100,000." To ensure that you understand how the *matching grants* provided throughout this experiment affect your donations, we provide a brief overview of their basic features.

Matching Grants. A matching grant is a commitment by a *lead donor* to match donations at an agreed upon rate (i.e., the *match rate*) up to some agreed upon limit (i.e., the *match limit*). In the example given above, the *match rate* is 1:1 (for each $1 you give, the charity receives a total of $2: your $1 and a matching $1 provided by the *lead donor*) and the *match limit* is $100,000 (the maximum amount of donations the *lead donor* is willing to match). When your donation receives a match, this increases the amount received by the charity as a result of your donation.

There are three defining features of any matching grant that are important to understand:

> *Lead donor*: this is the donor or entity responsible for providing the matching donations. Throughout this experiment, the lead donor will always be the experimenter. That is, all matching grants are provided by the experimenter.
>
> *Match rate*: the match rate is the rate at which your donation will be matched. There are three different match rates you will be presented with in this experiment:
>
> > **0.5:1.** A match rate of 0.5:1 means that for each $1 you donate, an additional $0.50 will be provided by the experimenter. Thus, for each $1 you donate, the charity will receive a total of $1.50 (your $1 plus a matching $0.50 provided by the experimenter).
> >
> > **1:1.** A match rate of 1:1 means that for each $1 you donate, an additional $1 will be provided by the experimenter. Thus, for each $1 you donate, the charity will

receive a total of $2 (your $1 plus a matching $1 provided by the experimenter).

2:1. A match rate of 2:1 means that for each $1 you donate, an additional $2 will be provided by the experimenter. Thus, for each $1 you donate, the charity will receive a total of $3 (your $1 plus a matching $2 provided by the experimenter).

Match limit: the match limit is the maximum amount of donations the lead donor is willing to match. Once the match limit is met, the lead donor will no longer match any more donations. Whether or not the match limit is met depends on the total amount of donations provided by donors in the fundraiser.

Determining Whether Your Donation Will Receive a Match. Throughout the experiment, you will be randomly grouped with other participants. The size of your group may vary, but you will always be informed of the number of participants in your group. Whether or not your donation is matched will depend on the donation decisions of the participants in your randomly assigned group.

There are two possible cases:

Case 1: the total donations provided by the *other members* of your group are such that the match limit *is not* met. In this case, any donation you provide *will* receive a match.

Case 2: the total donations provided by the *other members* of your group are such that the match limit *is* met. In this case, any donation you provide *will not* receive a match.

Note that in both cases, whether or not your donation is matched depends entirely on the decisions of the *other members* of your group. That is, your choice of how much to donate has no impact on whether or not you receive a match.

To help illustrate this, consider the following examples:

Example 1: You are grouped with 3 *other* participants (N=3). The experimenter will match donations at a 1:1 rate up to a match limit of $6. Each of the other members of your group choose to donate $2 each. You choose to donate $1.

In this example, each of the other members of your group choose to donate $2. Since there are 3 other group members, this means their total donations are $6 (3 x $2 = $6). Therefore, the match limit of $6 has been met by the other members of your group, and your donation will *not* be matched.

Thus, as a result of your $1 donation, the total donation received by the charity will be $1.

Example 2: You are grouped with 2 *other* participants (N=2). The experimenter will match donations at a 2:1 rate up to a match limit of $5. Each of the other members of your group choose to donate $2 each. You choose to donate $2.

In this example, each of the other members of your group choose to donate $2. Since there are 2 other group members, this means their total donations are $4 (2 x $2 = $4). Therefore, the match limit of $5 has not been met by the other members of your group, and your donation *will* be matched.

Thus, as a result of your $2 donation, the total donation received by the charity will be $6 (your $2 plus a matching $4 provided by the experimenter).

Testing Your Understanding. To make sure you understand the process for determining whether or not your donation will be matched, please answer the following questions:

Question 1. You are grouped with 2 *other* participants (N=2). The experimenter will match donations at a 1:1 rate up to a match limit of \$5. The other members of your group each donate \$2. You choose to donate \$3.

1a) Will your donation of \$3 receive a match?

1b) What will be the total donation received by the charity as a result of your \$3 donation?

Question 2. You are grouped with 4 *other* participants (N=4). The experimenter will match donations at a 0.5:1 rate up to a match limit of \$4. The other members of your group each donate \$1. You also choose to donate \$1.

2a) Will your donation of \$1 receive a match?

2b) What will be the total donation received by the charity as a result of your \$1 donation?

Question 3. You are grouped with 3 *other* participants (N=3). The experimenter will match donations at a 2:1 rate up to a match limit of \$5. The other members of your group each donate \$1. You choose to donate \$2.

3a) Will your donation of \$2 receive a match?

3b) What will be the total donation received by the charity as a result of your \$2 donation?

Great job! You've passed the test!

Now that you know how the matching subsidies work in this experiment, you are almost ready to begin. On the next page, we will go over the experiment procedures and present some example allocation decision problems.

——————————————— Page Break ———————————————

Experiment Procedures. We now briefly outline how this experiment will be conducted. This experiment consists of four (4) stages, each of which can be completed at your own pace. In each stage of the experiment, you will be grouped with other participants and each of you will be endowed with a certain number of Tokens. The size of your endowment and your group size will both remain constant within each stage of the experiment, but they may vary across different stages. At the beginning of each stage, you will be informed of your group size and the amount of your endowment.

Within each stage of the experiment, you will be presented with a list of allocation decision problems. For each problem, you must enter the number of Tokens you would like to pass to the charity. You will do this by typing the number of Tokens you would like to pass into the provided box for each problem. An example of the format used to present the allocation decision problems is shown below.

Example Allocation Decision Problems:

For each of the following Allocation Decision Problems, you are grouped with 5 other participants, and each of you are endowed with 80 Tokens.

	Enter the number of Tokens you would like to pass to the Charity. Pass	The total number of Tokens held for yourself. Your Earnings	Total donation received by the Charity if a match **is not** received. Total Donation (if **no match**)	Total donation received by the Charity if a match **is** received. Total Donation (**with match**)
1.) You, and each of the 5 other members of your group, are endowed with 80 Tokens.	[ ]			N/A
2.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match donations at a 1:1 rate up to a match limit of 160 Tokens.	[ ]			
3.) You, and each of the 5 other members of your group, are endowed with 80 Tokens. The experimenter will match all donations at a 2:1 rate.	[ ]		N/A	

Each allocation decision problem you will be presented with will be different, so it is important that you carefully read each problem before making your decision. Once you enter the number of Tokens you would like to pass to the Charity for a given problem, the computer will automatically calculate and display the corresponding values for the remaining columns.

Feel free to enter Pass values in the example problems above, and note how the remaining columns automatically fill after you do so. Each of the remaining columns is summarized below:

Your Earnings: this column reports your earnings (in Tokens) for each allocation decision problem, based on your decision of how many Tokens to pass to the Charity. It is the number of Tokens you hold for yourself, which is equal to your Endowment minus the amount you choose to Pass.

*Total Donation (if **no match**)*: this column reports the total donation (in Tokens) that will be received by the Charity if your donation **does not** end up being matched. This column is equal to the number of Tokens you choose to Pass.

*Total Donation (**with match**)*: this column reports the total donation (in Tokens) that will be received by the Charity if your donation **does** end up being matched. This column is equal to the number of Tokens you choose to pass **plus** the corresponding matching contribution made by the experimenter.

Note that in Example Problem 1 the *Total Donation (**with match**)* column displays "N/A" regardless of the number of Tokens you choose to Pass. This is because in this problem there is no match offered. Therefore, you know with certainty that your donation will not be matched, and the total donation received by the Charity will be equal to the value listed in the *Total Donation (if **no match**)* column.

Similarly, in Example Problem 3 the *Total Donation (if **no match**)* column displays "N/A" regardless of the number of Tokens you choose to Pass. This is because in this problem there is no match limit. Therefore, you know with certainty that your donation will be matched, and the total donation received by the Charity will be equal to the value listed in the *Total Donation (**with match**)* column.

For Example Problem 2, your donation may or may not be matched, depending on the decisions of your randomly selected group members. If the total donations of your group members do not exhaust the match limit of 160 Tokens, then your donation will be matched and the total donation received by the Charity will be equal to the value listed in the *Total Donation (**with match**)* column. If instead the total donations of your group members do exhaust the match limit, then your donation will not be matched and the total donation received by the Charity will be equal to the value listed in the *Total Donation (if **no match**)* column.

After you have entered decisions for all problems within a stage, you can submit your decisions. You will then advance to the next stage. In each stage, you will be assigned to a new random group. The size of your group may change between stages. Your endowment may also change between stages. It is important that you note these changes as you move from stage to stage. Other than changes in group size and endowment, all four stages are identical.

On the next page, we'll review how your bonus payment and total donation will be determined. After that, you'll be ready to complete the experiment.

——— Page Break ———

Determining Your Payment and Total Donation. Once you have entered appropriate decisions for each problem in all four stages, you will be able to submit your decisions. After submitting your decisions, **one stage will be randomly selected. From this randomly selected stage, one problem will then be randomly selected to determine your payment and the total donation received by the Charity.** That is, only one allocation decision problem from the entire experiment will be randomly selected to determine your bonus payment in this experiment. Your decision of how much to pass to the Charity in the randomly selected problem will determine the amount of your bonus earnings in this experiment. Your bonus earnings will be equal to the amount listed in the *Your Earnings* column of the decision problem randomly selected for payment.

You will be obligated to pass to the Charity the amount you have entered in the *Pass* column of the randomly selected problem. This amount, plus any matched funds provided by the experimenter (if applicable), will actually be donated to the Charity.

Informing You of Your Payment and Total Donation. You will be informed of which problem was randomly selected for payment at the end of the experiment. At that time, you will also be informed of your total bonus earnings based on your decision in the randomly selected problem. Your bonus earnings will be paid to you through Prolific within 1-2 business days after you complete the experiment and register your submission through Prolific. The show-up reward, which is in no way affected by your decisions in this experiment, will be automatically paid to you once you register your submission in Prolific.

All determinations regarding whether or not a participant's donation will receive a match will be made *at the conclusion of the study*, after *all* participants have submitted their responses. For this reason, we are unable to inform you at the conclusion of this experiment whether you will receive a match. Once all responses have been received, the experimenter will then randomly form groups and use these groups to determine whether or not a match will be provided to each participant. The exact procedure that will be used is detailed below.

Determining Whether Your Donation Will Receive a Match. The total donation received by the Charity will depend on whether or not your donation ends up receiving a match. Whether or not your donation receives a match will be determined in the following way:

(i) One allocation decision problem will be randomly selected to determine your payment. For example, let's assume *Stage 3: Problem 4* is randomly selected.

(ii) After the study is completed and responses have been received from all participants, we then randomly group you with other participants, where the number of group members is based on the group size listed in your randomly selected decision problem. For example, if *Stage 3: Problem 4* has a group size of 10, then you will be randomly grouped with 10 other participants from the experiment.

(iii) We then sum the total donation decisions of your random group members *in your randomly selected decision problem*. That is, if your randomly selected decision problem is *Stage 3: Problem 4*, then we sum the donation decisions of your randomly selected group members in the same decision problem (*Stage 3: Problem 4*).

(iv) Based on the *match rate* and *match limit* provided in your randomly selected decision problem, we then determine if the total donations of your group members exhausts the match limit.

i. If the total donations of your group members does not exhaust the match limit, then your donation will be matched at the applicable match rate provided in your randomly selected decision problem. In this case, the total donation provided to *charity: water* on your behalf will be equal to the value listed in the *Total Donation (**with match**)* column of your randomly selected decision problem.

ii. If the total donations of your group members exhausts the match limit, then your donation will not be matched. In this case, the total donation provided to *charity: water* on your behalf will be equal to the value listed in the *Total Donation (if **no match**)* column of your randomly selected decision problem.

All donations provided by participants of this study, including any applicable matching contributions (as determined by the procedure detailed above), will be donated to *charity: water* in a single, lump-sum donation at the conclusion of the study. **This donation is backed by the University of Maryland and will actually be provided to the Charity.**

If you have finished reading through these instructions, you are free to continue to the experiment.

C.4.9 Quiz questions used in instructions

Testing Your Understanding. To make sure you understand the process for determining whether or not your donation will be matched, please answer the following questions:

Question 1. You are grouped with 2 *other* participants (*N*=2). The experimenter will match donations at a 1:1 rate up to a match limit of $5. The other members of your group each donate $2. You choose to donate $3.

1a) Will your donation of $3 receive a match?

Figure C.14: Quiz question 1 from experiment instructions before subject attempts to answer

Testing Your Understanding. To make sure you understand the process for determining whether or not your donation will be matched, please answer the following questions:

Question 1. You are grouped with 2 *other* participants (*N*=2). The experimenter will match donations at a 1:1 rate up to a match limit of $5. The other members of your group each donate $2. You choose to donate $3.

1a) Will your donation of $3 receive a match?

No

Not quite. Since the 2 other participants in your group both choose to donate $2, they provide a total donation of $4 (2 x $2 = $4). Since $4 is less than the match limit of $5, your donation will be matched.

Figure C.15: Example of screen presented to subjects after answering quiz question 1 incorrectly

Testing Your Understanding. To make sure you understand the process for determining whether or not your donation will be matched, please answer the following questions:

Question 1. You are grouped with 2 *other* participants (*N*=2). The experimenter will match donations at a 1:1 rate up to a match limit of \$5. The other members of your group each donate \$2. You choose to donate \$3.

1a) Will your donation of \$3 receive a match?

[Yes ⌄]

That's right! Since the 2 other participants in your group both choose to donate \$2, they provide a total donation of \$4 (2 x \$2 = \$4). Since \$4 is less than the match limit of \$5, your donation will be matched.

1b) What will be the total donation received by the charity as a result of your \$3 donation?

[\$3 ⌄]

Not quite. Your donation will receive a 1:1 match. This means that for each \$1 you donate, the experimenter will provide a matching \$1. Thus, for each \$1 you donate, the charity will receive \$2 (your \$1 plus the matching \$1 provided by the experimenter).

Figure C.16: Example of screen presented to subjects after answering part a of quiz question 1 correctly and part b incorrectly

Question 2. You are grouped with 4 *other* participants (*N*=4). The experimenter will match donations at a 0.5:1 rate up to a match limit of \$4. The other members of your group each donate \$1. You also choose to donate \$1.

2a) Will your donation of \$1 receive a match?

No

That's right! Since the 4 other participants in your group all choose to donate \$1, they provide a total donation of \$4 (4 x \$1 = \$4). Since \$4 is *greater than or equal to* the match limit of \$4, the match limit has been met, and your donation will *not* be matched.

2b) What will be the total donation received by the charity as a result of your \$1 donation?

\$1.00

Correct! Because the match limit has been met by the other members of your group, you will *not* receive a match.

Only one more question to go!

Figure C.17: Quiz question 2 from experiment instructions, including correct answers and corresponding messages

Question 3. You are grouped with 3 *other* participants (*N*=3). The experimenter will match donations at a 2:1 rate up to a match limit of $5. The other members of your group each donate $1. You choose to donate $2.

3a) Will your donation of $2 receive a match?

Yes

That's right! Since the 3 other participants in your group all choose to donate $1, they provide a total donation of $3 (3 x $1 = $3). Since $3 is less than the match limit of $5, the match limit has not been met, and your donation will receive a match.

3b) What will be the total donation received by the charity as a result of your $2 donation?

$6

Correct! Your donation will receive a 2:1 match. This means that for each $1 you donate, the experimenter will provide a matching $2. Thus, if you donate $2, the charity will receive a total of $6 (your $2 plus a matching $4 provided by the experimenter).

Great job! You've passed the test!

Now that you know how the matching subsidies work in this experiment, you are almost ready to begin. On the next page, we will go over the experiment procedures and present some example allocation decision problems.

Figure C.18: Quiz question 3 from experiment instructions, including correct answers and corresponding messages

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