DO MATCHES REALLY OUTPERFORM REBATES? NEW EVIDENCE FROM A NOVEL EXPERIMENT

ZEDEKIAH G. HIGGS[†] AND NESLIHAN ULER[‡]

Abstract.

This paper challenges the well-established result among existing experimental studies that donations are significantly more responsive to matches than to rebates and provides important implications for tax policy. 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 show that this bias cannot be eliminated by accounting for censored observations because the constraint under rebates affects the entire distribution of observed behavior, not only the behavior of individuals for whom the constraint is binding. We conduct 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, there is no longer a gap between the rebate- and match-price elasticities, demonstrating that individuals respond to price changes in the same way, regardless of the type of subsidy. We show that existing models are inconsistent with our findings and propose a simple extension to these models that accounts for our results.

Date: January 28, 2025.

*The authors gratefully acknowledge support from the National Science Foundation (Award Number: 2315706) and the University of Maryland (the Faculty-Student Research Award and the Dissertation Enhancement Award). We thank Steffen Huck, Stephanie Karol, John List, Ariel A. Listo Argul, Yusufcan Masatlioglu and Robert McClelland for their helpful comments. We also thank seminar participants at the Berlin Social Science Center (WZB), Groupe d'Analyse et de Théorie Economique Lyon St-Etienne (GATE), the Queen Mary University of London, the University of Birmingham, the Virginia Commonwealth University, Auburn University and the University of Maryland, and countless conference participants for their valuable feedback. A part of this project was completed while Uler was a Visiting Scholar at Università Ca' Foscari Venezia.

[†] University of Maryland. Email: zedhiggs@umd.edu.

[‡] University of Maryland and University of Michigan. Email: neslihan@umd.edu.

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), has consistently found that donors do not respond to rebates and matches equivalently.[1] 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 the framework used in those studies. Specifically, prior studies rely on what we term the "third-party framework." As we demonstrate in Section 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

[1] See Section 6 for a detailed overview of this literature.

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 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 $s_m = t/(1-t)$.[2]

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. 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

[2]These rates guarantee price equivalency.

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 the most they can donate is therefore \$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. Therefore, 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. Furthermore, other than removing the constraint under rebates, the budget sets under the tax and third-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 ran 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 third-party framework. The results of these experiments are not significantly different from our *third-party* experiment, suggesting that our findings are not driven by how taxes are framed or the source of funding. Instead, the rebate constraint triggers a behavioral response.

Our paper then demonstrates the inability of existing models of charitable giving to explain the empirical findings from our tax experiment, and proposes a simple extension of these models to reconcile our results. None of the prevailing models can explain why individuals exhibit greater generosity under matches compared to rebates while also exhibiting a uniform response to price changes under matches and rebates. Traditional theories grounded solely in pure altruism cannot account for the heightened impact of matches on donations. By contrast, warm-glow theories, such as 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 the subsidy types to have distinct price elasticities. We show that a straightforward extension to these models can accommodate all of our findings without abandoning the warm-glow motive. Thus, our paper not only provides insights into why the subsidy type matters, but it also advances the theoretical framework for modeling charitable giving.

Although 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 similar to estimates reported in previous match studies. And, although our rebate-price elasticity estimate contradicts

[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.

estimates reported in previous laboratory studies, it is consistent with estimates reported in many empirical studies using tax data.[4]

This study makes several important contributions. First, it helps to clarify whether individuals truly view rebates and matches differently, helping to improve the literature's understanding of why people give. To isolate the effect of changing the type of subsidy, 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. These studies are typically conducted using tax data, which usually only contain rebate subsidies and often lack sufficient price variation (Andreoni, 2006; List, 2011; Vesterlund, 2016). 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.

Second, this study provides new insights for discussions of tax policy.[5] Deriving policy implications based on previous laboratory or field experiments using the third-party framework is misleading. In many real-world donation settings, such as payroll giving, the disposable-income constraint is absent or less salient than in lab experiments with small endowments. Although field experiments could, in principle, offer more realistic insights, they often introduce other confounding factors less relevant to tax policy. For example,

[4]Empirical studies using tax data face difficult identification challenges, and their estimates are sensitive to the quality of the data and empirical methods used. Unsurprisingly, they have produced a wide range of elasticity estimates. Having said that, it is widely accepted that the true elasticity is around one in absolute value (Auten et al., 1992, 2002; Tiehen, 2001; Peloza and Steel, 2005; Andreoni, 2006; Bakija and Heim, 2011). Hence, our rebate-price elasticity estimate of –.848 is more consistent with a large body of empirical work than any of the estimates in the previous experimental studies.

[5]See also Scharf and Smith (2015), which conducts a hypothetical-survey experiment with a sample of taxpaying donors and has important implications for rebates and matches in the UK system of tax relief for charitable donations.

while payroll giving involves no time delay or uncertainty about receiving a tax rebate, field experiments typically do include these elements. Moreover, aside from a handful of studies (Turk et al., 2007; Blumenthal et al., 2012; Scharf and Smith, 2015), prior work has not examined rebates and matches in the context of taxation. By contrast, the rebate subsidy in our tax framework functions similarly to the current U.S. tax system (at least for taxpayers who itemize their deductions), thereby eliminating the constraint inherent to third-party designs while increasing policy relevance. Although previous studies have suggested that switching from rebates to matches in the U.S. tax system could significantly increase charitable giving, our findings suggest that such a restructuring may in fact have little effect on donations.

Finally, this study also contributes to our 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 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 3 outlines the experimental design and procedures for each of our experiments. Section 4 presents results. Section 5 shows that none of the extant theories of giving can consistently explain our findings and proposes a simple extension of existing models that can accommodate these results. Section 6 provides a brief overview of related literature, including attempts made to explain the disparity between rebates and

matches, and previous attempts to resolve the budget set issue present in the third-party framework. Section 7 concludes.

2. Theory

In this section 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. We then develop and analyze our novel tax framework. We show that the 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.

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.[6] 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 (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

$$\max_{g \in [0,w]} U(w - g,\ g,\ g + \lambda R_{-i}). \tag{2.1}$$

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

$$\max_{g_n \in [0,w]} U(w - g_n,\ g_n,\ (1 + s_m)g_n + \lambda R_{-i}), \tag{2.2}$$

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

[6] While use of the impure impact model simplifies the presentation, our main results can also be derived from the impure altruism model.

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

$$\max_{g_g \in [0,w]} U(w - (1 - s_r)g_g,\ g_g,\ g_g + \lambda R_{-i}), \tag{2.3}$$

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 (2.2) and (2.3) we must first express them in terms of the same choice variable. Rewriting the rebate problem (2.3) in terms of the donor's *net* donation, $g_n = (1 - s_r)g_g$, gives

$$\max_{g_n \in [0,\ (1-s_r)w]} U\left(w - g_n,\ \frac{g_n}{1 - s_r},\ \frac{g_n}{1 - s_r} + \lambda R_{-i}\right). \tag{2.4}$$

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 2.2) can be written as

$$\max_{g_n \in [0,w]} U\left(w - g_n,\ g_n,\ \frac{g_n}{1 - s_r} + \lambda R_{-i}\right). \tag{2.5}$$

Price-equivalent third-party rebates and matches can now be directly compared by comparing (2.4) and (2.5), 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.[7]

[7]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 5.

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 (2.4), 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 (2.5), 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 ($B_{1,r}$) are strict subsets of the budget sets they face under price-equivalent matches (B_1). This is shown graphically in Figure 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 the elasticities in an environment that keeps the budget sets identical.

2.2. **Tax framework.** 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 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

$$\max_{g_g \in \left[0, \frac{w}{1-t}\right]} U\left((1-t)\left[\frac{w}{1-t} - g_g\right], g_g, g_g + \lambda R_{-i}\right). \tag{2.6}$$

When there is a match subsidy provided in the tax framework, the individual's donations are no longer tax exempt. 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 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

$$\max_{g_n \in [0,w]} U\left(w - g_n, g_n, \frac{g_n}{1-t} + \lambda R_{-i}\right). \tag{2.7}$$

Comparing (2.7) to (2.5), 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 (2.6) in terms of the net donation, g_n, and simplifying, the individual's optimization problem for a rebate subsidy in the tax framework becomes

$$\max_{g_n \in [0,w]} U\left(w - g_n, \frac{g_n}{1-t}, \frac{g_n}{1-t} + \lambda R_{-i}\right). \tag{2.8}$$

Comparing (2.8) to (2.4), 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 (2.7) and (2.8), the budget sets for price-equivalent rebates and matches are now equal in the tax framework, suggesting the gap between price elasticities should decrease. That being said, since it is still being assumed the amount of warm glow received under rebates and matches differs, the gap between elasticities need not completely disappear.

2.3. **Comparison of frameworks.** A graphical comparison of rebates and matches in the third-party and tax frameworks is presented in Figure 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. It is also important to note that, as the price of giving is decreased, the discrepancy between the budget sets gets larger: $B_{1,r}$ becomes an increasingly smaller portion of B_1 as the price decreases.

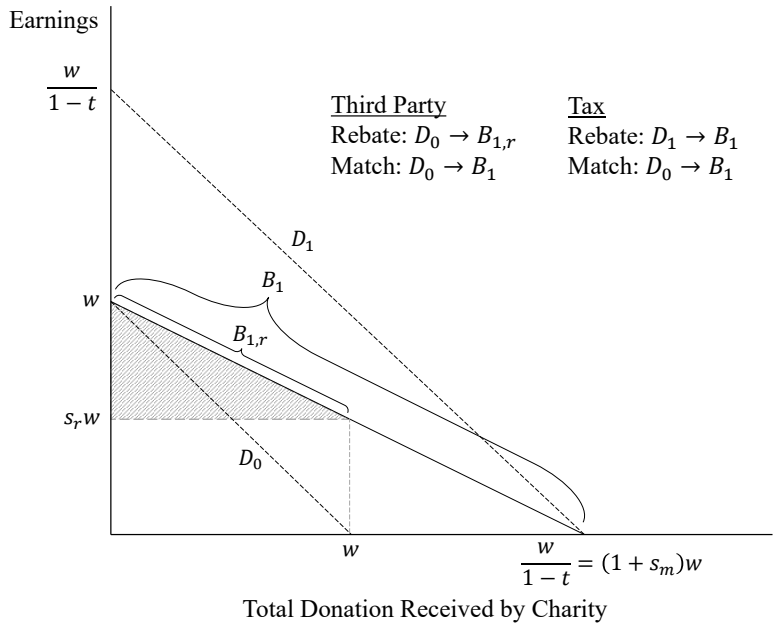

FIGURE 1. Budget sets for price-equivalent rebate and match subsidies in the third-party and tax frameworks.

Because of this, the bias induced by use of the third-party framework is likely worse at lower prices.

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 should generate identical outcomes. (To be clear, this is a theoretical

equivalence between *frameworks* (i.e., third-party vs. tax) and not between *subsidies*.) 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. Based on these papers, we conjecture that the entire distribution of donations will shift downward when the budget set gets smaller. While it might otherwise be possible to account for censored observations when estimating elasticities, this behavioral effect will introduce an unaccounted-for bias in the estimation of the rebate-price elasticity within the third-party framework.

2.4. **Main hypotheses.** The preceding analysis leads to two main hypotheses:

Hypothesis 1 *The gap between price elasticities will be smaller in the tax framework.*

Hypothesis 2 *The entire distribution of donations under rebates will shift between the third-party and tax frameworks.*

3. Experimental Design and Procedures

In total we run four separate real-donation experiments: two main experiments—which we refer to as the *third-party* experiment and the *tax* experiment—which form the basis of our main results, and two follow-up experiments—which we refer to as the *alt-tax* experiment and the *con-tax* experiment—designed to provide additional insights into the underlying mechanisms. No subject participated in more than one experiment.

In all four experiments, subjects are presented with a list of allocation decision problems in which they receive 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.

[8]The charity used is charity: water.

The instructions used in each experiment are nearly identical, with the exception of a few words. A copy of the instructions provided to subjects (including prepared statements read aloud by the experimenter) is provided in Online Appendix D.

In what follows, we first outline the experimental design. We then summarize the experimental procedures used.

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-subjects 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 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 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.

[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).

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

Figure D.1 in Online Appendix D 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 problem, which is enforced by the programming. 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

[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 is randomly selected to determine the subject's payment and donation to the charity.

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 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.[11] Thus, there are two additional baseline problems relative to the *third-party* experiment, resulting in a total of 12 decision problems rather than 10. Figure D.2 in Online Appendix D 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 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 gross endowment of y to the

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

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 2. List of budget sets used in the tax experiment.

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.

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 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 2. Figure D.3 in Online Appendix D provides an example decision sheet presented to subjects in the *alt-tax* experiment.

Since the only difference between this experiment and the *third-party* experiment is the use of taxation language, this design allows us to separate the effect of using a tax frame from the effect of changing the budget sets associated with rebates.

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. As in our *tax* experiment, when a rebate is provided in the *con-tax* experiment, individuals are asked to choose a donation from their pre-tax income and then taxed on their remaining income net of their donation. However, unlike the *tax* experiment, we now artificially constrain donors' choices under rebates so that they may not pass more than their net income w to the charity, replicating the constraints they face in the *third-party* experiment. This experiment allows us to cleanly observe the effects of introducing a constraint, holding all else constant, including the source of funding, which leads to a change in the decision set of individuals (i.e., D_1 versus D_0 in Figure 1).

The parameters used in the *con-tax* experiment are the same as those used in the *tax* experiment, listed in Table 2. Figure D.4 in Online Appendix D 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 the *con-tax* 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.

3.5. **Experimental Procedures.** All sessions were run using Zoom and Qualtrics. 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). 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* and *con-tax* experiments were conducted with a total of 144 and 146 subjects, respectively, after the main experiments were completed, between March of 2023 and February of 2024 using the same procedures

and subject pool.[12] All sessions for all four experiments were run by only one of the authors of the project, so that all subjects interacted with the same experimenter, guaranteeing there are no differences in the experimental procedures used across any of the experiments or any of the sessions. Each subject participated in only one experiment.

Subjects were recruited from a large pool of potential participants representing different majors and 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,[13] change their Zoom name to an anonymous five-digit code, and then return them 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

[12]Overall, subject characteristics among different experiments are similar (see Table A.1).

[13]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. Second, the use of unique survey links prevented subjects from being able to restart the survey. 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 subjects started the survey before being instructed to do so.

provided by the experimenter that allowed subjects to continue to the experiment, which they were then able to complete at their own pace. The experiments took approximately one hour to complete.[14]

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 (Online Appendix D.5) and completed a payment form to document their earnings.

4. Results

4.1. **Third-party experiment.** Figure 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 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 2 with Figure B.16 (presented in Online Appendix B), in which we plot the average decisions made in the Davis et al. (2005) study.[15] We can see that our results are qualitatively in line with their findings.

Table A.2 reports the average donation amounts (in Tokens) 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).

[14]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.

[15]Due to space constraints, several tables and figures are included in our Online Appendix. The letters preceding the numbers indicate the section where each table or figure can be found.

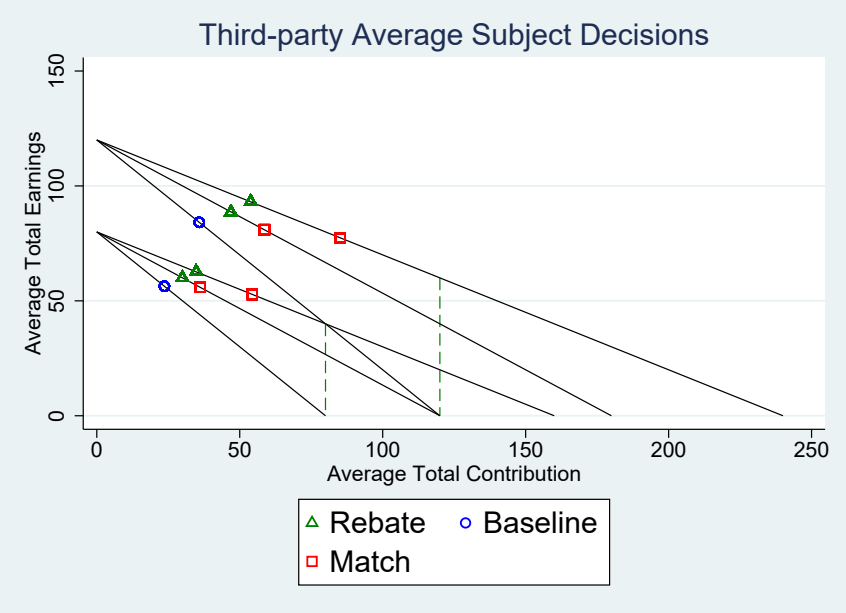

FIGURE 2. Average decisions in our third-party experiment.

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:

$$Y_{ij} = \beta_0 + \beta_1 \cdot E_{ij} + \beta_2 \cdot R_{ij} \times E_{ij} + \beta_3 \cdot M_{ij} \times E_{ij} + \beta_4 \cdot R_{ij} \times P_{ij} + \beta_5 \cdot M_{ij} \times P_{ij} + \beta_6 \cdot R_{ij} + \beta_7 \cdot M_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij}, \tag{4.1}$$

where $i = 1, \ldots, 147$ indexes subjects, $j = 1, \ldots, 10$ indexes the allocation decision problems, $Y_{ij} = \text{Total Donation}_{ij}$, $E_{ij} = \ln(\text{Endowment})_{ij}$, $P_{ij} = \ln(\text{Price})_{ij}$, 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.[16] Endowment is defined as the total Tokens provided to the subject (80 Tokens or 120 Tokens). Price is defined as the price of giving \$1 to the charity (\$0.50, \$0.67, or \$1.00). Total Donation is total *gross* donation (in Tokens) received by the charity. Note that we do not take the log of total donations because the logarithm of zero is not defined. To account for censoring in Total Donation—from below and from

[16]The subject-level covariates are age, sex, income, an indicator for those who do not want to report their income, political view, level of religiosity, previous donation amount, how well they know the charity used in the experiment, and how well they understood the instructions. All of our qualitative results are robust to the inclusion or exclusion of these variables. See Table A.12 for a comparison of the elasticity estimates derived with and without the inclusion of the control variables.

above—we employ a Tobit model, avoiding the use of any log-like transformations (Chen and Roth, 2024).[17]

Column 1 of Table 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. For example, Eckel and Grossman (2003) report rebate- and match-price elasticities of -0.340 and –1.067, respectively. 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 99% level of confidence (p-value=0.000), with donations significantly more responsive to matches than rebates.

4.2. **Tax experiment.** Having demonstrated that we can replicate the large gap in price elasticities between rebates and matches in our *third-party* experiment, we now turn to analyzing the results of our *tax* experiment.

Figure 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 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. More importantly, 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).

Table A.3 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 5%

[17]A log-like transformation of the dependent variable (such as adding a small fixed amount to Total Donation and then taking its log value, or using an IHS transformation of Total Donation) is problematic. The elasticity estimates produced by a log-like transformation are dependent on the scale used (Chen and Roth, 2024). In fact, we can easily confirm in our data that, when using a log-like transformation of the dependent variable, estimates change dramatically when total donations are in Tokens or are scaled to be in dollars. Therefore, we choose to use level-log specifications throughout the paper to maintain scale-independence in our estimations.

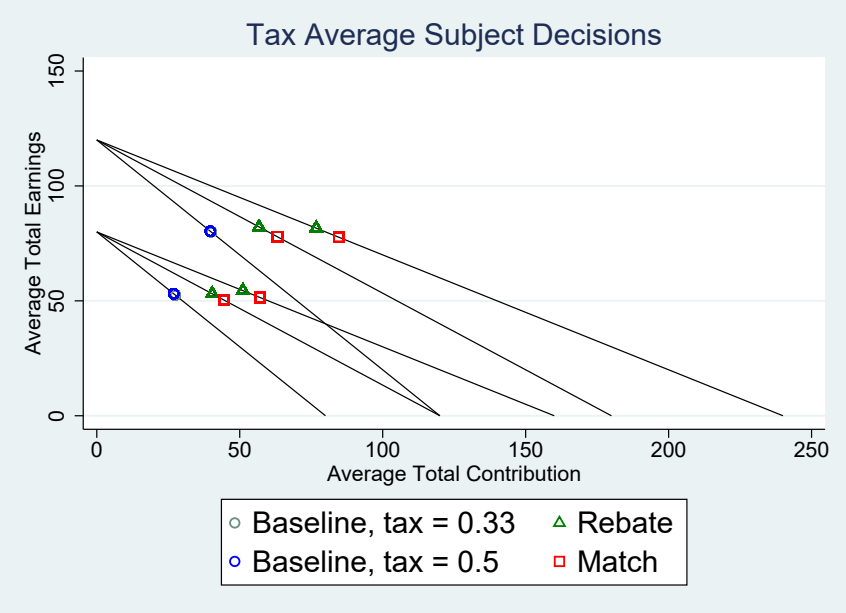

FIGURE 3. Average decisions in the tax experiment.

significance level, reflecting the fact that subjects still tend to donate more under matches than rebates.

Next, we analyze the price elasticities. Figure 3 suggests that although donations remain higher under matches than under rebates, they may no longer be more responsive to price changes under matches. This is suggested by the fact that, for both subsidy types, donations now move horizontally in response to price changes. We use the same estimation strategy as before, with the exception that we now include an additional variable (Tax Rate) to control for the tax rate (0.33 or 0.5) applied to gross endowments.

Column 2 of Table 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), and there is no longer any significant difference between elasticities (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.[18]

[18]The absence of a gap between rebate- and match-price elasticities in the *tax* experiment is a surprising finding since, as we demonstrated in Section 2, it is inconsistent with extant models of charitable giving. We provide a more detailed discussion of this issue in Section 5.

Regression Results: random effects tobit maximum likelihood				
Dependent variable=total donation received by charity				
	(1) Third-party 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 (β_0)	-128.995 (124.269)	-34.050 (103.318)	-220.705** (73.693)	-263.432** (77.159)
Constant–rebate subs. ($\beta_0 + \beta_6$)	-201.996 (121.793)	-138.979 (103.259)	-215.318** (73.645)	-284.051** (77.114)
Constant–match subs. ($\beta_0 + \beta_7$)	-316.066** (121.702)	-151.966 (103.194)	-373.445** (73.482)	-386.281** (76.991)
Endowment–no subs. (β_1)	31.480** (7.699) [.434]	30.150** (5.586) [.435]	36.674** (4.712) [.657]	29.189** (5.300) [.490]
Endowment–rebate subs. ($\beta_1 + \beta_2$)	47.185** (5.434) [.650]	51.010** (5.510) [.735]	34.648** (4.713) [.620]	34.631** (5.325) [.582]
Endowment–match subs. ($\beta_1 + \beta_3$)	69.043** (5.336) [.952]	54.977** (5.474) [.792]	67.892** (4.615) [1.216]	55.423** (5.206) [.931]
Rebate price (β_4)	**-32.571**** (7.667) [-.449]	**-58.853**** (11.056) [-.848]	**-27.060**** (9.391) [-.485]	**-22.781*** (10.577) [-.383]
Match price (β_5)	**-84.874**** (7.505) [-1.170]	**-62.359**** (11.018) [-.899]	**-73.883**** (9.292) [-1.323]	**-57.537**** (10.455) [-.966]
Tax rate (β_8)		-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

* $p < 0.05$, ** $p < 0.01$. Standard errors in parentheses. Elasticity estimates in brackets.

Note: The models for each experiment are run separately. For each experiment, we use a level-log specification with subject-level random effects: $Y_{ij} = \beta_0 + \beta_1 \cdot E_{ij} + \beta_2 \cdot R_{ij} \times E_{ij} + \beta_3 \cdot M_{ij} \times E_{ij} + \beta_4 \cdot R_{ij} \times P_{ij} + \beta_5 \cdot M_{ij} \times P_{ij} + \beta_6 \cdot R_{ij} + \beta_7 \cdot M_{ij} + \beta_8 \cdot T_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij}$, where i indexes subjects, j 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, T_{ij} = Tax Rate$_{ij}$, and X_i is a row vector of subject-level covariates. Total Donation is total *gross* donation (in Tokens) received by the charity. Price is defined as price of giving \$1 to the charity (\$0.50, \$0.67, or \$1.00). Tax Rate equals the tax rate applied to endowments (0.33 or 0.5). Tax Rate is dropped in the Third-party estimation. Endowment represents $w \in \{80, 120\}$, *not* $y = \frac{w}{1-t} \in \{120, 160, 180, 240\}$. This allows for direct comparisons of the estimates from the *third-party* and other experiments.

TABLE 3. Regression results for all experiments

Results from the first two columns of Table 3 are consistent with our Hypothesis 1. A more direct test of this hypothesis is provided in Online Appendix C.1. Table C.1 presents results from a combined model in which data from the *third-party* and *tax* experiments are pooled. We find that donations are more responsive to rebate prices in the *tax* experiment than in the *third-party* experiment (p-value=0.053). Although the gap in price elasticities in the *third-party* experiment is statistically different from zero (p-value=0.000), the gap in price elasticities in the *tax* experiment is not statistically different from zero (p-value=0.738). More importantly, the price elasticity gap is significantly smaller in the *tax* experiment compared to the *third-party* experiment (p-value=0.002), consistent with Hypothesis 1.

While not directly relevant to our main hypotheses, it is also interesting to test whether starting with a different tax rate affects donations independently of any subsidy effects. Both the impure altruism and impure impact models predict that, all else equal, the tax rate alone should have no effect on giving behavior. Both Figure 3 and our regression analysis show support for this prediction. In Figure 3, 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 3. The estimated coefficient on Tax Rate captures the effect of facing a higher tax rate, holding all else equal (including subjects' budgets), and is statistically insignificant (p-value=0.719).

4.3. **Is there a shift in donation behavior between experiments?** 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. In this section, we investigate whether the censored observations in the *third-party* experiment explain the differences between experiments, or whether the donation behavior differs between experiments even for those subjects who are unconstrained.

In the *third-party* experiment, donations are constrained under rebates to the level of the initial endowment, w_{ij}. This might mechanically induce a smaller rebate-price elasticity, relative to the match-price elasticity, and therefore might explain the difference we observe between our main experiments.[19] However, as we will show in this section, this is not the case. We provide several pieces of supporting evidence below.

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.5 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 Online Appendix C.2, indicate that censoring partly explains—but cannot fully account for—the differences we observe between these two experiments.[20]

[19] Ideally, the Tobit model would be able to account for any effect mechanically induced by the constraint under rebates, if one exists, and provide an unbiased estimate of the rebate-price elasticity. However, this would require that donors' rebate-price elasticities be uncorrelated with the probability of being constrained. If, for example, the donors who are constrained tend to have more elastic demands for giving, the Tobit model will underestimate the true average rebate-price elasticity.

[20] In other words, censoring leads to biased estimations in the *third-party* experiment, but the entire bias cannot be explained by censoring alone.

Finally, we show that under rebates the entire donation distribution shifts in the *third-party* experiment. Figures 4 and 5 show how dramatically the distributions for rebates (blue lines) and matches (red lines) diverge in the *third-party* experiment (left panel) relative to the *tax* experiment (right panel) when the price of giving is 0.5.[21] Kolmogorov–Smirnov tests formally confirm our observations. The distributions for rebates and matches diverge well before the constraint occurs in the *third-party* experiment (p-values are 0.007 and 0.000 for the left panels of Figures 4 and 5, respectively).[22] In contrast, the distributions for rebates and matches are not statistically different from each other when the constraint is removed in the *tax* experiment (p-values are 0.183 and 0.234 for the right panels of Figures 4 and 5, respectively). The constraint in the third-party framework appears to shift the entire distribution of behavior under a rebate.

Further supporting this claim, Figures 6 and 7 show that the match distributions are similar between the *third-party* and *tax* experiments (p-values are 0.774 and 0.929 for the right panels of Figures 6 and 7, respectively), but the rebate distributions are dramatically different (p-values are 0.013 and 0.008 for the left panels of Figures 6 and 7, respectively).[23] That is, it is the rebate distribution that shifts, not the match distribution.

[21] In the main text, we focus on cases where the price of giving is 0.5. At a price of 0.67, the disparity between budget sets for rebates and matches is smaller, and because of this we do not have sufficient statistical power to statistically distinguish the two distributions. Nevertheless, the same qualitative findings hold at this higher price. See Online Appendix B for all related figures.

[22] Our results remain unchanged when we use censored donation amounts for the match subsidy in the *third-party* experiment to make the match and rebate conditions more comparable.

[23] We also conducted a more stringent analysis in which we censored total donations under the rebate subsidy in the *tax* experiment to mirror the conditions of the *third-party* experiment. Under this approach, the p-values are 0.013 and 0.090 for the left panels of Figures 6 and 7, respectively.

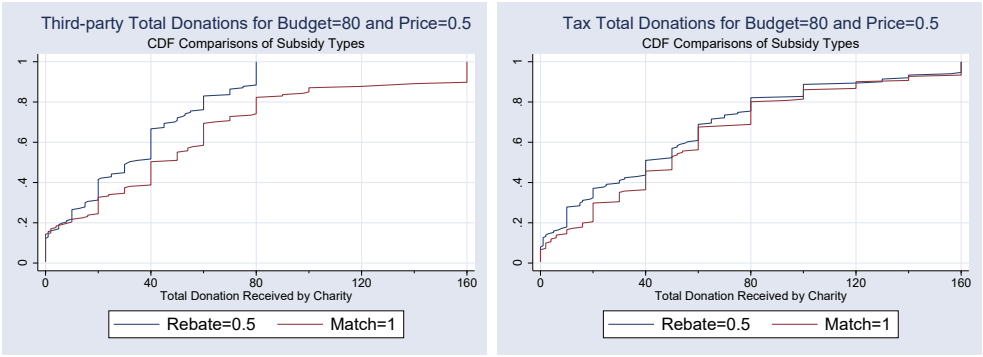

FIGURE 4. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. Third-party on left, Tax on right.

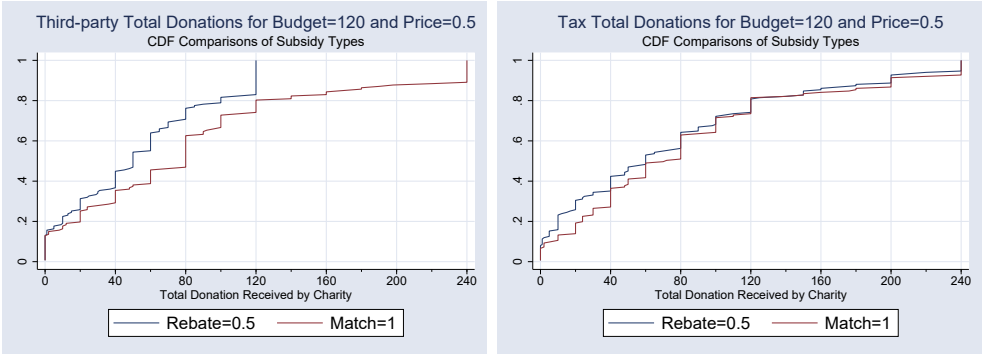

FIGURE 5. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. Third-party on left, Tax on right.

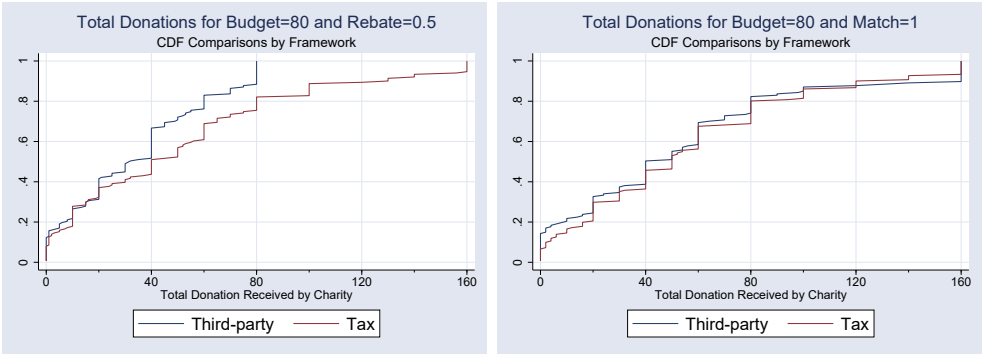

FIGURE 6. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=80. Third-party in blue, Tax in red.

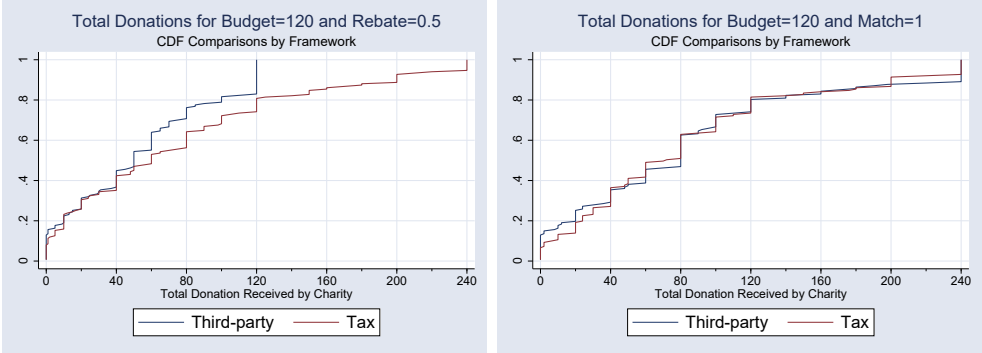

FIGURE 7. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=120. Third-party in blue, Tax in red.

To summarize, although censored observations partly explain why previous work has identified large gaps between price elasticities for rebates and matches, we show that it is not the only cause of the disparity. We show that the entire distribution of donations shifts under rebates relative to matches in the *third-party* experiment. Moreover, in our *tax* experiment, we see that by expanding the budget sets faced by subjects under rebates—to be equal to those under matches—the distribution of donations under a rebate converges toward the distribution under a match. Finally, when comparing our two main experiments, donation distributions are similar under matches but are dramatically different under rebates. These findings provide support for our Hypothesis 2.

4.4. **Does tax framing explain our results?** So far, our analysis supports both of our hypotheses and shows that individuals do not necessarily respond differently to price changes under different subsidy types. It also underscores that results from experiments using a third-party framework should not be generalized to tax policy. However, it remains unclear whether the convergence in price elasticities arises because the *tax* experiment eliminates the constraint—placing rebates and matches on equal footing—or because the tax framing itself alters donation behavior. Both mechanisms are of interest, but they have distinct implications for the charitable sector and for theories of charitable giving, making it essential to distinguish between them.

In addition to removing the constraint under rebates, the *tax* experiment differs from the *third-party* experiment in two ways: the mention of taxation and the decision set under rebates (i.e., D_1 versus D_0 in Figure 1). Therefore, to understand whether the framing

we use in the *tax* experiment plays a role in our main findings, we conduct two follow-up experiments—the *alt-tax* and *con-tax* experiments. The *alt-tax* experiment mirrors the *third-party* experiment—including the use of the decision set D_0 under rebates—but employs the tax language used in the *tax* experiment.[24] The *con-tax* experiment mirrors the *tax* experiment—including the use of the decision set D_1 under rebates—but enforces the same constraint on donations under rebates that is present in the *third-party* experiment.[25] If the shift in donation behavior under rebates in the *tax* experiment is driven by either the use of tax language or the decision set D_1, it should also appear in at least one of the follow-up experiments. However, if the shift in behavior is primarily due to the presence of the constraint, then behavior in both follow-up experiments should resemble that of the *third-party* experiment.

The results of both follow-up experiments are very similar to those of the *third-party* experiment. Figure 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 8 with Figures 2 and 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.6 and A.9 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, using tax language and changing the decision set appear to have little effect on behavior.

To provide further evidence, we also compare the donation behavior in these experiments to that of our main experiments. Average donations in the *alt-tax* and *con-tax* experiments are quantitatively very similar to those in the *third-party* experiment (see Figure B.15). Table A.7 (Table A.10) compares the average donations in the *third-party*

[24]Since the only difference between the *alt-tax* and *third-party* experiments is the mention of taxation, directly comparing these experiments isolates the behavioral effect of introducing tax-related language, while holding all other factors constant.

[25]Since the only difference between the *con-tax* and *tax* experiments is the constraint, the direct comparison of these experiments gives us the behavioral effect of the introduction of a constraint, holding all else constant.

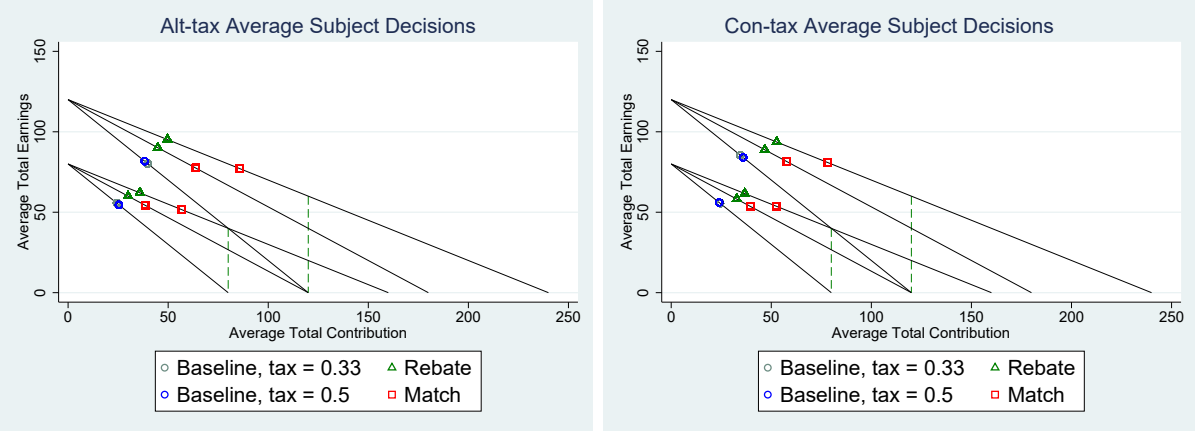

FIGURE 8. Average decisions in the alt-tax (left) and con-tax (right) experiments.

and *alt-tax* (*con-tax*) experiments for each budget set. There are no statistically significant differences between the two follow-up experiments and the *third-party* experiment 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 experiments. Table A.8 (Table A.11) compares the average donations in each experiment by budget. While average donations under match subsidies appear to be very similar in the follow-up experiments and the *tax* experiment, 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 effects created by the use of a tax frame.

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 3 (as well as in columns (3) and (4) of Table A.12). For both experiments, we find large gaps between the rebate- and match-price elasticity estimates. The gaps are 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 the framing we used in the *tax* experiment is responsible for shifting donation decisions under rebates.

We conclude that neither the change in decision sets nor the use of tax language in the *tax* experiment significantly affect subjects' behavior, and therefore neither can explain the observed 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. Furthermore, our results also show that subjects are not responding to changes in their decision sets. Instead, subjects appear to only respond to changes in their budget sets.

4.5. **Discussion.** Having eliminated the possibility that our results are driven by the use of tax language or changes in decision sets, we argue that the gap between price elasticities observed in the *third-party* experiment (as well as in previous studies using the same framework) is driven by a behavioral bias stemming from the presence of constraints on the budget sets. Hence, the differences we observe between our two main experiments—the *third-party* and *tax* experiments—can be explained by the ability of our *tax* experiment to remove those constraints.

The behavioral bias we identify is not unique to our setting. In fact, as we explained before, our findings are consistent with previous research showing that expanding the budget set available to subjects (into the negative domain) affects the *entire* distribution of observed behavior (List, 2007; Bardsley, 2008).

One might wonder what causes this behavioral bias when individuals' budget sets are constrained. One possible explanation for such a behavioral bias is reference dependence (Tversky and Kahneman, 1991). For example, an agent might adopt the mid-point of all possible actions as a reference point. Since the mid-points of the budget sets differ between our main experiments under rebates—and also between the different subsidy types within the *third-party* experiment—it is unsurprising that reference-dependent agents would make different decisions. Nevertheless, we prefer to remain agnostic about the precise drivers of this behavioral effect. Instead, our main aim here is to document that it exists in the third-party framework and to highlight the importance of using the tax framework to obtain unbiased price elasticity estimates.

5. A Simple Extension of Existing Models

In this section, we discuss how existing theories of charitable giving fail to align with our findings from our *tax* experiment and provide a simple extension of these models that is consistent with our data. Recall that in our *tax* experiment matches continue to perform significantly better than rebates, even if only by a small margin (see Table A.3). At the same time, Table 3 reveals no significant differences in the price elasticities for rebates and matches, either in magnitude or statistical significance.

None of the existing theories can explain these two results simultaneously. Traditional theories based on pure altruism cannot account for the greater effect of matches on donations, instead predicting identical donations and price elasticities for rebates and matches. 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, as we demonstrate in Section 2. Consequently, these models are also inconsistent with our findings.

Below, we present a model that does not require distinct price elasticities and still allows matches to outperform rebates. We proceed in several steps to clarify which modeling choices drive each result. First, we discuss how subsidies might affect warm glow, a consideration that informs both our current model and future modeling initiatives. Second, building on that discussion, we offer a simple extension of impure altruism and impure impact models that can explain the equivalence of price elasticities, with the caveat that it also requires equivalence in donations. Third, we show that introducing a straightforward shift in preference parameters within our model can then account for all of our findings from the *tax* experiment.

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).[26] The impure altruism model—which combines warm-glow giving with

[26]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).

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.

So, how should subsidies for charitable giving affect warm glow? The answer depends on how we interpret warm glow. Under one interpretation, donors experience warm glow only for the portion of the donation they fund themselves—i.e., net of any subsidized contribution. In that case, with a match at rate s_m, the donor feels warm glow only for the amount g. With a rebate at rate s_r, the donor experiences warm glow for $(1-r)g$. Alternatively, donors may derive warm glow from the total impact of their donation on the charity. Under a match at rate s_m, this means feeling warm glow for the entire amount $(1+m)g$, where g is the donor's out-of-pocket contribution. Under a rebate at rate s_r, the donor would feel warm glow for the amount g.

However, prior theoretical analyses of rebates and matches have taken a third approach. Rather than assume donors categorically do or do not receive warm glow from subsidized funds, they have implicitly assumed that donors feel warm glow for their *out-of-pocket* donations. At first glance, this assumption seems logical: because donors actively choose the amount they pay, it makes sense that they would feel warm glow for that portion. Yet, it implies that donors receive warm glow from subsidized funds under a rebate but not under a match. Specifically, under a rebate, the donor's out-of-pocket contribution is the same as the *gross* donation g_g (i.e., the total amount going to the charity, including the subsidy). In contrast, under a match, the donor's out-of-pocket contribution equals the *net* donation g_n (i.e., only the donor's personal cost, excluding the subsidy). Consequently, this assumption treats price-equivalent rebates and matches as inherently different, and therefore predicts that their price elasticities will generally diverge—contradicting our empirical findings.

We now propose a straightforward extension of the impure altruism and impure impact models that preserves the warm-glow motive while achieving parity in price elasticities across subsidies. Specifically, we relax the assumption that donors feel warm glow for their out-of-pocket donation regardless of any subsidies. This change allows for the possibility that donors may not derive full warm glow from the subsidized portion of their

donation (e.g., when receiving tax rebates).[27] To capture this idea, we introduce two parameters, $\delta_m, \delta_r \in [0,1]$, which govern how much warm glow donors receive from matched and rebated funds, respectively. When $\delta_m, \delta_r = 0$, donors experience no warm glow from subsidized funds. When $\delta_m, \delta_r = 1$, donors treat subsidized funds and personal contributions as identical sources of warm glow. Of course, δ_m and δ_r need not be 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 in the tax framework under rebates and matches are now written, respectively, as

$$\max_{g_g \in \left[0, \frac{w}{1-t}\right]} U\left((1-t)\left[\frac{w}{1-t} - g_g\right],\ [1-(1-\delta_r)t]\, g_g,\ g_g + \lambda R_{-i}\right),\ \text{and} \tag{5.1}$$

$$\max_{g_n \in [0,w]} U\Big(w - g_n,\ (1+\delta_m s_m)g_n,\ (1+s_m)g_n + \lambda R_{-i}\Big). \tag{5.2}$$

Rewriting (5.2) in terms of t (using $s_m = \frac{t}{1-t}$) gives

$$\max_{g_n \in [0,w]} U\left(w - g_n,\ \frac{[1-(1-\delta_m)t]g_n}{1-t},\ \frac{g_n}{1-t} + \lambda R_{-i}\right), \tag{5.3}$$

and rewriting the donor's problem under a rebate (eq. 5.1) in terms of the donor's *net* donation, $g_n = (1-t)g_g$, gives

$$\max_{g_n \in [0,w]} U\left(w - g_n,\ \frac{[1-(1-\delta_r)t]g_n}{1-t},\ \frac{g_n}{1-t} + \lambda R_{-i}\right). \tag{5.4}$$

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.

[27] See Bénabou and Tirole (2006), which explains the superiority of matching mechanisms by suggesting that donors may feel greedy when accepting rebates, potentially diminishing the warm-glow utility derived from donating. Chan et al. (2022) formally tests this hypothesis and finds empirical support for it.

Although this simple model can explain the equivalence of the price elasticities for rebates and matches, it is still not entirely consistent with our data—when the price elasticities are equal, the model requires that the donation levels under rebates and matches also be equal. Nevertheless, our data can be easily reconciled with the model by introducing a small shift in preferences between private and public good consumption. This modification aligns with Eckel and Grossman's (2003, p. 698) conjecture that matches may generate more donations than rebates because they elicit more cooperative behavior.

A natural question, then, is whether incorporating a preference shift between matches and rebates without altering price elasticities is possible. We show that this is indeed possible by considering a simple quasi-linear utility function that is linear in private consumption. We simplify the model by assuming $\delta_r = \delta_m = 0$ and $R_{-i} = 0$. Individuals' utility function for a given subsidy type $j \in \{m, r\}$ is given as:

$$U(c,\ g_n,\ R) = c + \frac{\theta_j}{1 + 1/e}\left(\frac{g_n^{\gamma}\ R^{(1-\gamma)}}{\theta_j}\right)^{1+1/e}, \tag{5.5}$$

in which $c = w - g_n$, $R = g_n/(1-t)$, e is the conventional quasi-linear price elasticity parameter, $\gamma \in [0, 1]$ is the strength of warm-glow, and θ_j is a taste-shifting parameter with $j \in \{m, r\}$.[28] Finally, we assume $\theta_m > \theta_r$.

It can easily be shown that the optimal level of gross donation (R) equals:

$$g_g = \theta_j(1-t)^{-\gamma+e-\gamma e}. \tag{5.6}$$

Since $\theta_m > \theta_r$, the gross donation under matches is larger than the gross donation under rebates. It is also easy to show that the price elasticity is $\epsilon_j = (1-\gamma)(1+e) - 1$ and, therefore, is independent of the type of the subsidy. In other words, while gross donations are higher under a match, price elasticities are identical.

In summary, we have shown that our experimental findings can be reconciled within a coherent theoretical framework. While we acknowledge that other explanations are possible, our discussion and model elucidate key mechanisms that shape charitable giving decisions. In doing so, they also offer a useful foundation for future research seeking to understand and model charitable giving decisions.

[28]This functional form is adapted from the example in Hungerman and Ottoni-Wilhelm (2021) (p. 1564), with two key modifications: (i) the taste-shifting parameter varies by subsidy type, and (ii) rebated funds, like matched funds, do not generate warm glow.

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.[29]

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 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 field studies (Eckel and Grossman, 2008, 2017), online experiments (Bekkers, 2015; Gandullia and Lezzi, 2018; Gandullia, 2019; Sasaki et al., 2022) and additional laboratory studies (Davis et al., 2005; Davis and Millner, 2005; Eckel and Grossman, 2006a,b, 2008). 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 2, because rebates and matches affect warm glow differently, the impure impact model can explain the observed gap between rebate- and match-price elasticities.[30] However, while it is able to explain the disparity between rebates and matches in the context of charitable giving, it is unable to account for the similar gaps in price elasticities observed in other contexts, including in studies of consumption goods and investment decisions (Davis et al., 2005; Davis and Millner, 2005). The replication of the gap between rebates and matches in other settings suggests that it is not driven by a behavioral effect unique to charitable giving, calling into question the

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

[30]To be clear, the gap can also be explained by other models—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 2 demonstrates this result extends to general utility forms.

adequacy of warm-glow-based explanations. Notably, however, all experimental studies documenting this gap share the same fundamental design.

This paper makes several novel contributions to the literature. First, we show that the gap between rebates and matches observed in previous studies largely stems from the experimental design. Second, we document a systematic shift in the entire distribution of donations under the third-party framework. Third, we demonstrate that once the budget set issue is resolved, the gap in price elasticities between rebates and matches disappears, suggesting that the longstanding assumption implicitly accepted in previous research—namely, that rebates and matches affect warm glow differently—likely does not hold once all confounding factors are controlled for. Fourth, we propose a new model of charitable giving that consistently accounts for these findings. Finally, we conduct the first experimental study to compare rebates and matches fully embedded in a tax framework.[31]

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 it the first to attempt to remove the disparity. 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).[32] However, each of these studies uses an alternative approach from ours that does not necessarily work to remove the disparity, as we explain in detail below. More importantly, we are the first to document how the

[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.

[32]Sasaki et al. (2022) also addresses the budget set issue, but without removing the disparity between budget sets. The paper tackles the issue using two alternative approaches. The first approach is to use hypothetical questions with large endowments. The second approach is to demonstrate that the substantial gap in donations between rebates and matches persists even when the analysis is restricted to participants for whom the constraint is not binding. They interpret their findings as evidence that the imposition of a constraint under rebates does not play a large role in the gap in donations. However, this interpretation relies on the assumption that individuals who are unconstrained remain unaffected by the introduction of the constraint—an assumption we have demonstrated to be incorrect in this paper.

presence of a constraint shifts the entire distribution of donations. Finally, we are also able to eliminate the confound using a natural framework.

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.[33] 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.[34] While the findings of their

[33]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.

[34]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,

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. 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.

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 than via rebates. We show theoretically that the third-party framework used in previous experiments creates a discrepancy between budget sets for price-equivalent 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

---

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.

novel experiment which equates the budget sets for price-equivalent rebates and matches, thereby enabling an unbiased comparison of their estimated price elasticities.

Our principal findings confirm our core hypothesis: once the budget sets are aligned, there is no statistical difference between the price elasticities of rebates and matches. To ensure these results are not specific to our experimental setting or subject pool, we replicate the third-party framework of earlier research, and again observe the conventional gap in elasticities. Two additional experiments further test the mechanisms behind our main results and show that the observed parity in elasticities indeed stems from eliminating the budget-set discrepancy, rather than from behavioral changes induced by our taxation frame.

Closer inspection reveals two important insights. First, when the experimental design constrains donations under rebates, the most generous donors become censored, mechanically biasing estimates of price elasticities. Second, and more importantly, the constraint not only affects individuals for whom it is binding, but shifts the entire distribution of donations. Our findings extend those of List (2007) and Bardsley (2008), showing that expanding or contracting budget sets can substantially alter behavior—even without introducing an option to take or changing individuals' wealth. 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.

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 suggest a simple extension of previous warm-glow models of giving which relaxes this assumption. As we show, relaxing this assumption within a warm-glow model can explain the equivalence of price elasticities and—by introducing a small preference shift under matches—can also account for the slight superiority of matches over rebates.

Finally, as the first experimental study to compare rebates and matches within a tax framework, our work carries important implications for tax policy. Extrapolating results from experiments using third-party frameworks (whether in the lab or the field) may be misleading in this context. The disposable-income constraint present in lab experiments is highly salient, and it is unrealistic to expect this constraint to be as pronounced in real-world settings. Meanwhile, field experiments with third-party rebates can involve significant delays or uncertainty about receiving the rebate, further complicating donors' decisions. Although rebates in a tax system may also involve time delays, donors are typically more confident that they will eventually receive them. Moreover, in certain settings such as payroll giving, individuals are neither constrained by their disposable income nor subject to a delay in receiving rebates, just like in our tax-based design.

By resolving the budget-set discrepancies present in earlier studies, we find that donors respond equally to price changes brought about by rebates and matches. While prior research has suggested that charitable giving could rise significantly if the tax system were restructured to offer matches rather than rebates, our findings suggest that such a policy shift may actually have little impact on giving.

References

Andreoni, James (1988) "Privately provided public goods in a large economy: the limits of altruism," *Journal of Public Economics*, 35 (1), 57–73.

——— (1989) "Giving with impure altruism: Applications to charity and Ricardian equivalence," *Journal of Political Economy*, 97 (6), 1447–1458.

——— (1990) "Impure altruism and donations to public goods: A theory of warm-glow giving," *Economic Journal*, 100 (401), 464–477.

——— (2006) "Philanthropy," in Kolm, Serge-Christophe and Jean Mercier Ythier eds. *Handbook of the Economics of Giving, Altruism and Reciprocity*, 2, 1201–1269: Elsevier.

Andreoni, James and A. Abigail Payne (2013) "Charitable giving," in Auerbach, Alan J., Raj Chetty, Martin Feldstein, and Emmanuel Saez eds. *Handbook of Public Economics*, 5, 1–50: Elsevier.

Auten, Gerald E., James M. Cilke, and William C. Randolph (1992) "The effects of tax reform on charitable contributions," *National Tax Journal*, 45 (3), 267–290.

Auten, Gerald E., Holger Sieg, and Charles T. Clotfelter (2002) "Charitable giving, income, and taxes: An analysis of panel data," *American Economic Review*, 92 (1), 371–382.

Bakija, Jon and Bradley T. Heim (2011) "How does charitable giving respond to incentives and income? New estimates from panel data," *National Tax Journal*, 64 (2), 615–650.

Bardsley, Nicholas (2008) "Dictator game giving: altruism or artifact?" *Experimental Economics*, 11 (2), 122–133.

Becker, Gary S. (1974) "A theory of social interactions," *Journal of Political Economy*, 82 (6), 1063–1093.

Bekkers, René (2015) "When and why matches are more effective subsidies than rebates," in Deck, Cary A., Enrique Fatas, and Tanya Rosenblat eds. *Replication in Experimental Economics*, 18 of Research in Experimental Economics, 183–211: Emerald Group Publishing Limited.

Blumenthal, Marsha, Laura Kalambokidis, and Alex Turk (2012) "Subsidizing charitable contributions with a match instead of a deduction: What happens to donations and compliance?" *National Tax Journal*, 65 (1), 91–116.

Bénabou, Roland and Jean Tirole (2006) "Incentives and Prosocial Behavior," *American Economic Review*, 96 (5), 1652–1678, 10.1257/aer.96.5.1652.

Chan, Nathan W., Stephen Knowles, Ronald Peeters, and Leonard Wolk (2022) "Perception of generosity under matching and rebate subsidies," *Judgment and Decision Making*, 17 (5), 1058–1071, 10.1017/S1930297500009323.

Chen, Jiafeng and Jonathan Roth (2024) "Logs with zeros? Some problems and solutions," *The Quarterly Journal of Economics*, 139 (2), 891–936.

Cornes, Richard and Todd Sandler (1984) "Easy riders, joint production, and public goods," *Economic Journal*, 94 (375), 580–598.

Crumpler, Heidi and Philip J. Grossman (2008) "An experimental test of warm glow giving," *Journal of Public Economics*, 92 (5-6), 1011–1021.

Davis, Douglas D. (2006) "Rebate subsidies, matching subsidies and isolation effects," *Judgment and Decision Making*, 1 (1), 13–22.

Davis, Douglas D. and Edward L. Millner (2005) "Rebates, matches, and consumer behavior," *Southern Economic Journal*, 72 (2), 410–421.

Davis, Douglas D., Edward L. Millner, and Robert J. Reilly (2005) "Subsidy schemes and charitable contributions: a closer look," *Experimental Economics*, 8 (2), 85–106.

Diederich, Johannes, Catherine C. Eckel, Raphael Epperson, Timo Goeschl, and Philip J. Grossman (2022) "Subsidizing unit donations: Matches, rebates, and discounts compared," *Experimental Economics*, 25 (2), 734–758.

Diederich, Johannes, Raphael Epperson, and Timo Goeschl (2021) "How to design the ask? Funding units vs. giving money," AWI Discussion Paper 698, Heidelberg University.

Dreber, Anna, Tore Ellingsen, Magnus Johannesson, and David G. Rand (2013) "Do people care about social context? Framing effects in dictator games," *Experimental Economics*, 16, 349–371.

Eckel, Catherine C. and Philip J. Grossman (2003) "Rebate versus matching: Does how we subsidize charitable contributions matter?," *Journal of Public Economics*, 87 (3-4), 681–701.

_____ (2006a) "Do donors care about subsidy type? An experimental study," in Isaac, R. Mark and Douglas D. Davis eds. *Experiments Investigating Fundraising and Charitable Contributors*: Emerald Group Publishing Limited.

_____ (2006b) "Subsidizing charitable giving with rebates or matching: Further laboratory evidence," *Southern Economic Journal*, 72 (4), 794–807.

_____ (2008) "Subsidizing charitable contributions: A natural field experiment comparing matching and rebate subsidies," *Experimental Economics*, 11 (3), 234–252.

_____ (2017) "Comparing rebate and matching subsidies controlling for donors' awareness: Evidence from the field," *Journal of Behavioral and Experimental Economics*, 66, 88–95.

Eckel, Catherine C., Philip J. Grossman, and Rachel M. Johnston (2005) "An experimental test of the crowding out hypothesis," *Journal of Public Economics*, 89 (8), 1543–1560.

Epperson, Raphael and Christiane Reif (2019) "Matching subsidies and voluntary contributions: A review," *Journal of Economic Surveys*, 33 (5), 1578–1601.

Gandullia, Luca (2019) "The price elasticity of warm-glow giving," *Economics Letters*, 182, 30–32.

Gandullia, Luca and Emanuela Lezzi (2018) "The price elasticity of charitable giving: New experimental evidence," *Economics Letters*, 173, 88–91.

Greiner, Ben (2015) "Subject pool recruitment procedures: organizing experiments with ORSEE," *Journal of the Economic Science Association*, 1 (1), 114–125.

Grossman, Philip J. and Catherine C. Eckel (2015) "Giving versus taking for a cause," *Economics Letters*, 132, 28–30.

Harbaugh, William T., Ulrich Mayr, and Daniel R. Burghart (2007) "Neural responses to taxation and voluntary giving reveal motives for charitable donations," *Science*, 316 (5831), 1622–1625.

Hungerman, Daniel M. and Mark Ottoni-Wilhelm (2021) "Impure impact giving: Theory and evidence," *Journal of Political Economy*, 129 (5), 1553–1614.

Konow, James (2010) "Mixed feelings: Theories of and evidence on giving," *Journal of Public Economics*, 94 (3-4), 279–297.

Korenok, Oleg, Edward L. Millner, and Laura Razzolini (2014) "Taking, giving, and impure altruism in dictator games," *Experimental Economics*, 17, 488–500.

Levitt, Steven D. and John A. List (2007) "What do laboratory experiments measuring social preferences reveal about the real world?" *Journal of Economic Perspectives*, 21 (2), 153–174.

List, John A. (2007) "On the interpretation of giving in dictator games," *Journal of Political Economy*, 115 (3), 482–493.

_____ (2011) "The market for charitable giving," *Journal of Economic Perspectives*, 25 (2), 157–180.

Lukas, Indra, Philip J. Grossman, and Catherine C. Eckel (2010) "Preference or confusion: Understanding the differential impact of rebate and matching subsidies," Saint Cloud State University Working Paper .

McClelland, Robert (1989) "Voluntary donations and public expenditures in a federalist system: Comment and extension," *American Economic Review*, 79 (5), 1291–1296.

Ottoni-Wilhelm, Mark, Lise Vesterlund, and Huan Xie (2017) "Why do people give? Testing pure and impure altruism," *American Economic Review*, 107 (11), 3617–33.

Peloza, John and Piers Steel (2005) "The price elasticities of charitable contributions: A meta-analysis," *Journal of Public Policy & Marketing*, 24 (2), 260–272.

Sasaki, Shusaku, Hirofumi Kurokawa, and Fumio Ohtake (2022) "An experimental comparison of rebate and matching in charitable giving: The case of Japan," *Japanese Economic Review*, 73 (1), 147–177.

Scharf, Kimberley and Sarah Smith (2015) "The price elasticity of charitable giving: does the form of tax relief matter?" *International Tax and Public Finance*, 22 (2), 330–352.

Smith, Alexander (2015) "On the nature of pessimism in taking and giving games," *Journal of Behavioral and Experimental Economics*, 54, 50–57.

Steinberg, Richard (1987) "Voluntary donations and public expenditures in a federalist system," *American Economic Review*, 77 (1), 24–36.

Tiehen, Laura (2001) "Tax policy and charitable contributions of money," *National Tax Journal*, 54 (4), 707–723.

Tonin, Mirco and Michael Vlassopoulos (2010) "Disentangling the sources of pro-socially motivated effort: A field experiment," *Journal of Public Economics*, 94 (11-12), 1086–1092.

Turk, Alex, Maryamm Muzikir, Marsha Blumenthal, and Laura Kalambokidis (2007) "Charitable contributions in a voluntary compliance income tax system: Itemized deductions versus matching subsidies," in *The IRS research bulletin: Proceedings of the 2007 IRS research conference*, 51–74.

Tversky, Amos and Daniel Kahneman (1991) "Loss Aversion in Riskless Choice: A Reference-Dependent Model," *Quarterly Journal of Economics*, 106 (4), 1039–1061.

Vesterlund, Lise (2016) "Using experimental methods to understand why and how we give to charity," in Kagel, John H. and Alvin E. Roth eds. *Handbook of Experimental Economics*, 2, 91–151: Princeton University Press.

Supplemental Online Material

Appendix A. Additional Tables

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.1. Summary demographic data for each experiment

	Endowment	Price	Total Donation rebate	Total Donation 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	34.76	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 paired t-tests.

TABLE A.2. Total donations (in Tokens) in the third-party experiment

	Endowment	Price	Tax Rate	Total Donation rebate	Total Donation match	rebate=match p-value†
Mean	80	1	.33	27.44		
Std. err.				1.71		
N				151		
Mean	80	1	.5	26.91		
Std. err.				1.73		
N				151		
Mean	80	.67	.33	40.38	44.45	.012
Std. err.				2.89	2.88	
N				151	151	
Mean	80	.5	.5	51.12	57.05	.013
Std. err.				3.72	3.68	
N				151	151	
Mean	120	1	.33	39.95		
Std. err.				2.66		
N				151		
Mean	120	1	.5	39.71		
Std. err.				2.60		
N				151		
Mean	120	.67	.33	56.77	63.24	.005
Std. err.				4.20	4.07	
N				151	151	
Mean	120	.5	.5	76.76	84.62	.033
Std. err.				5.73	5.69	
N				151	151	

†Reported p-values are for paired t-tests.

TABLE A.3. Total donations (in Tokens) in the tax experiment.

	Endowment	Price	Subsidy	Total Donation Third-party	Total Donation Tax	Third-party=Tax p-value†
Mean	80	1		23.66	27.18	.102
Std. err.				1.77	1.21	
N				147	302	
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	.003
Std. err.				2.01	2.89	
N				147	151	
Mean	80	.5	match	54.38	57.05	.623
Std. err.				3.96	3.68	
N				147	151	
Mean	80	.5	rebate	34.76	51.12	.000
Std. err.				2.19	43.72	
N				147	151	
Mean	120	1		35.86	39.83	.225
Std. err.				2.69	1.86	
N				147	302	
Mean	120	.67	match	58.62	63.24	.425
Std. err.				4.10	4.07	
N				147	151	
Mean	120	.67	rebate	46.91	56.77	.059
Std. err.				3.08	4.20	
N				147	151	
Mean	120	.5	match	85.16	84.62	.949
Std. err.				6.09	5.69	
N				147	151	
Mean	120	.5	rebate	53.83	76.76	.001
Std. err.				3.42	5.73	
N				147	151	

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

TABLE A.4. 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.5. Number of subjects who are (or would be) constrained by a third-party rebate

				Total Donation		rebate=match
	Endowment	Price	Tax Rate	rebate	match	p-value†
Mean	80	1	.33	24.35		
Std. err.				1.67		
N				144		
Mean	80	1	.5	25.38		
Std. err.				1.77		
N				144		
Mean	80	.67	.33	29.97	38.56	.000
Std. err.				1.90	2.48	
N				144	144	
Mean	80	.5	.5	35.90	56.75	.000
Std. err.				2.18	3.72	
N				144	144	
Mean	120	1	.33	39.76		
Std. err.				2.77		
N				144		
Mean	120	1	.5	38.24		
Std. err.				2.72		
N				144		
Mean	120	.67	.33	44.83	63.81	.000
Std. err.				2.94	4.14	
N				144	144	
Mean	120	.5	.5	49.66	85.82	.000
Std. err.				3.21	5.79	
N				144	144	

†Reported p-values are for paired t-tests.

TABLE A.6. Total donations (in Tokens) in the alt-tax experiment.

	Endowment	Price	Subsidy	Total Donation Third-party	Total Donation Alt-Tax	Third-party=Alt-tax p-value[†]
Mean	80	1		23.66	24.86	.575
Std. err.				1.77	1.21	
N				147	288	
Mean	80	.67	match	36.18	38.56	.514
Std. err.				2.66	2.48	
N				147	144	
Mean	80	.67	rebate	29.92	29.97	.985
Std. err.				2.01	1.90	
N				147	144	
Mean	80	.5	match	54.38	56.75	.663
Std. err.				3.96	3.72	
N				147	144	
Mean	80	.5	rebate	34.76	35.90	.712
Std. err.				2.19	2.18	
N				147	144	
Mean	120	1		35.86	39.00	.344
Std. err.				2.69	1.94	
N				147	288	
Mean	120	.67	match	58.62	63.81	.374
Std. err.				4.10	4.14	
N				147	144	
Mean	120	.67	rebate	46.91	44.83	.625
Std. err.				3.08	2.94	
N				147	144	
Mean	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.7. Comparison of total donations (in Tokens) in the third-party and alt-tax experiments

	Endowment	Price	Subsidy	Total Donation Alt-tax	Total Donation Tax	Alt-tax=Tax p-value[†]
Mean	80	1		24.86	27.18	.179
Std. err.				1.21	1.21	
N				288	302	
Mean	80	.67	match	38.56	44.45	.122
Std. err.				2.48	2.88	
N				144	151	
Mean	80	.67	rebate	29.97	40.38	.003
Std. err.				1.90	2.89	
N				144	151	
Mean	80	.5	match	56.75	57.05	.955
Std. err.				3.72	3.68	
N				144	151	
Mean	80	.5	rebate	35.90	51.12	.000
Std. err.				2.18	3.72	
N				144	151	
Mean	120	1		39.00	39.83	.759
Std. err.				1.94	1.86	
N				288	302	
Mean	120	.67	match	63.81	63.24	.922
Std. err.				4.14	4.07	
N				144	151	
Mean	120	.67	rebate	44.83	56.77	.021
Std. err.				2.94	4.20	
N				144	151	
Mean	120	.5	match	85.82	84.62	.883
Std. err.				5.79	5.69	
N				144	151	
Mean	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.8. Comparison of total donations (in Tokens) in the alt-tax and tax experiments

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

†Reported p-values are for paired t-tests.

TABLE A.9. Total donations (in Tokens) in the con-tax experiment.

	Endowment	Price	Subsidy	Total Donation Third-party	Total Donation Con-tax	Third-party=Con-tax p-value[†]
Mean	80	1		23.66	24.18	.809
Std. err.				1.77	1.23	
N				147	292	
Mean	80	.67	match	36.18	39.55	.387
Std. err.				2.66	2.83	
N				147	146	
Mean	80	.67	rebate	29.92	32.77	.334
Std. err.				2.01	2.16	
N				147	146	
Mean	80	.5	match	54.38	52.52	.736
Std. err.				3.96	3.82	
N				147	146	
Mean	80	.5	rebate	34.76	36.75	.538
Std. err.				2.19	2.37	
N				147	146	
Mean	120	1		35.86	35.24	.850
Std. err.				2.69	1.86	
N				147	292	
Mean	120	.67	match	58.62	57.80	.888
Std. err.				4.10	4.12	
N				147	146	
Mean	120	.67	rebate	46.91	46.77	.976
Std. err.				3.08	3.26	
N				147	146	
Mean	120	.5	match	85.16	78.23	.403
Std. err.				6.09	5.58	
N				147	146	
Mean	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.10. Comparison of total donations (in Tokens) in the third-party and con-tax experiments

	Endowment	Price	Subsidy	Total Donation Con-tax	Total Donation Tax	Con-tax=Tax p-value[†]
Budget	80	1		24.18	27.18	.084
Std. err.				1.23	1.21	
N				292	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	.081
Std. err.				1.86	1.86	
N				292	302	
Budget	120	.67	match	57.80	63.24	.349
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.11. 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 (no subsidy)	.410 (.105)	.434 (.110)	.408 (.079)	.435 (.084)	.619 (.087)	.657 (.091)	.459 (.088)	.490 (.093)
Endowment (rebate subsidy)	.615 (.083)	.650 (.087)	.690 (.085)	.735 (.090)	.584 (.086)	.620 (.091)	.545 (.090)	.582 (.095)
Endowment (match subsidy)	.899 (.095)	.952 (.100)	.744 (.086)	.792 (.091)	1.146 (.101)	1.216 (.105)	.876 (.097)	.931 (.102)
Rebate price	-.425 (.104)	-.449 (.110)	-.797 (.157)	-.848 (.167)	-.457 (.160)	-.485 (.170)	-.358 (.168)	-.383 (.179)
Match price	-1.105 (.126)	-1.170 (.132)	-.843 (.157)	-.899 (.167)	-1.247 (.171)	-1.323 (.181)	-.909 (.173)	-.966 (.184)
Tax rate			-.066 (.184)	-.070 (.196)	-.045 (.193)	-.047 (.205)	-.057 (.203)	-.061 (.216)
Controls	N	Y	N	Y	N	Y	N	Y
Subjects	147		151		144		146	
Observations	1470		1812		1728		1752	

TABLE A.12. Comparison of Elasticity Estimates

Appendix B. Additional Figures

B.1. **CDFs of Donations, comparing third-party and tax experiments by Budget, Subsidy Type and Price.**

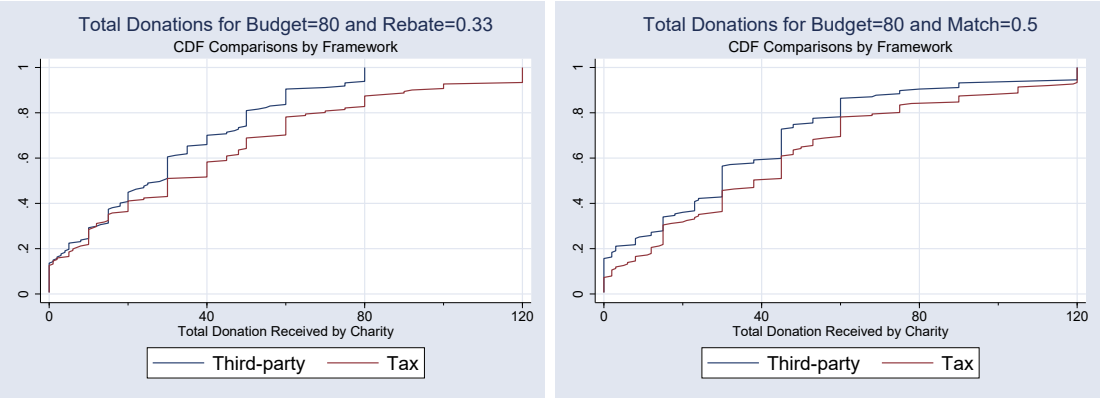

Figure B.1. CDFs of Donations for Rebate (left) and Match (right) when Price=.67 and Budget=80. Third-party in blue, Tax in red.

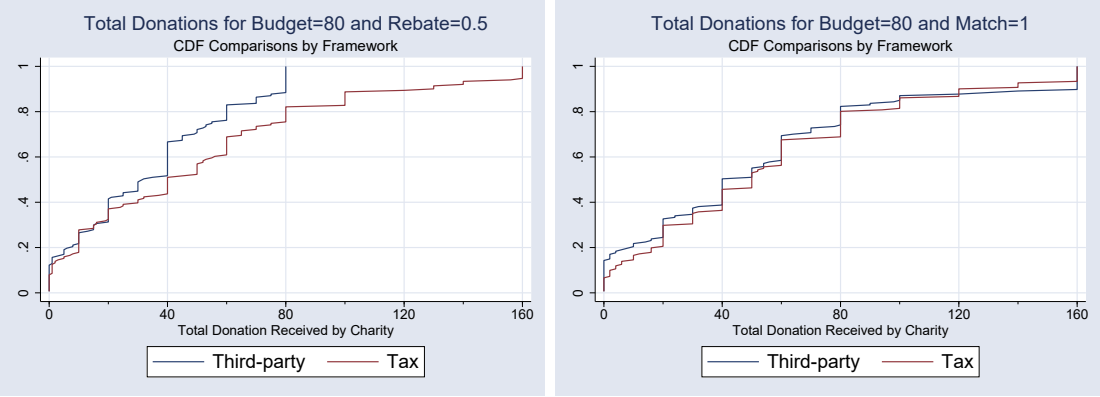

Figure B.2. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=80. Third-party in blue, Tax in red.

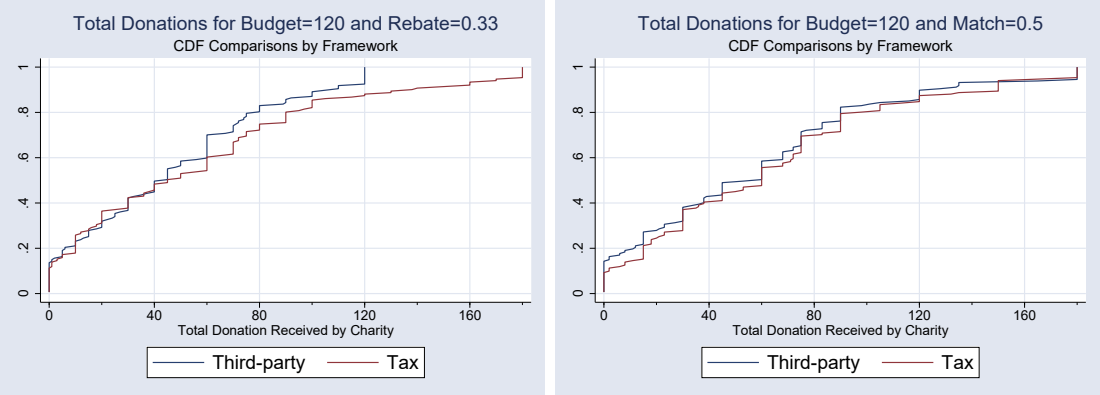

FIGURE B.3. CDFs of Donations for Rebate (left) and Match (right) when Price=.67 and Budget=120. Third-party in blue, Tax in red.

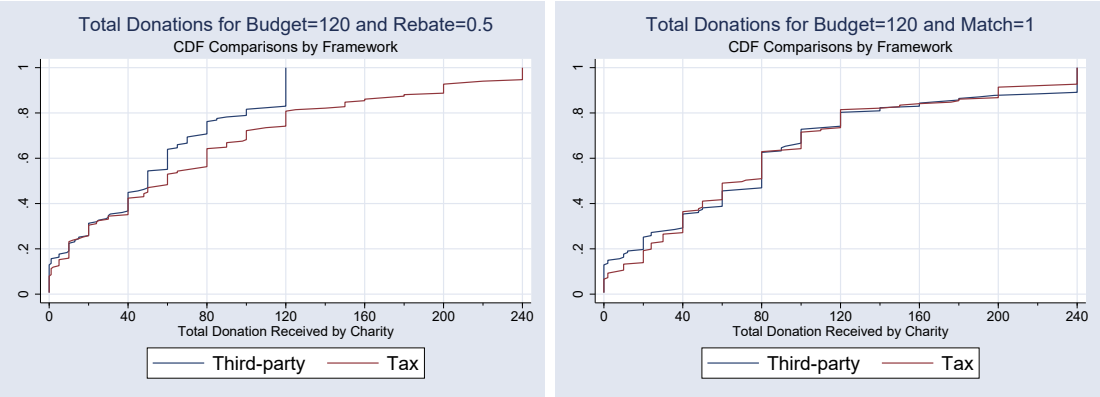

FIGURE B.4. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=120. Third-party in blue, Tax in red.

B.2. **CDFs of Donations, comparing rebates and matches by Budget, Subsidy Type and Price.**

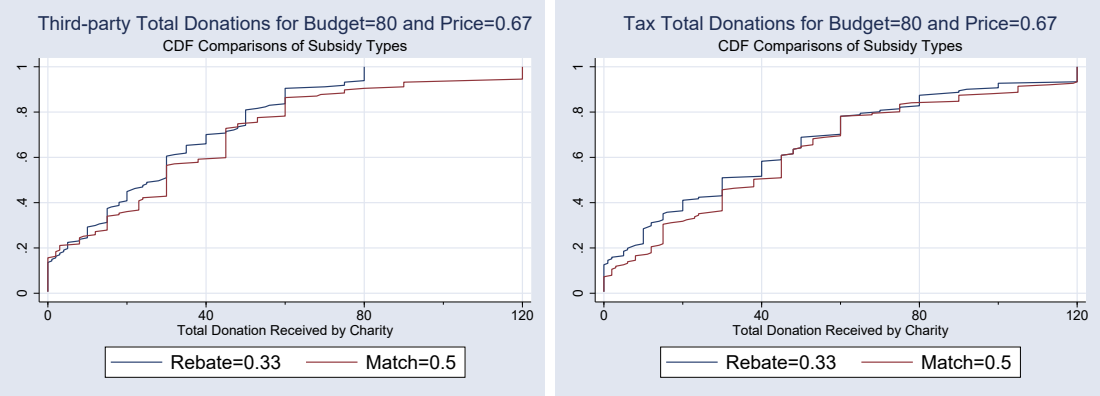

FIGURE B.5. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=80. Third-party on left, Tax on right.

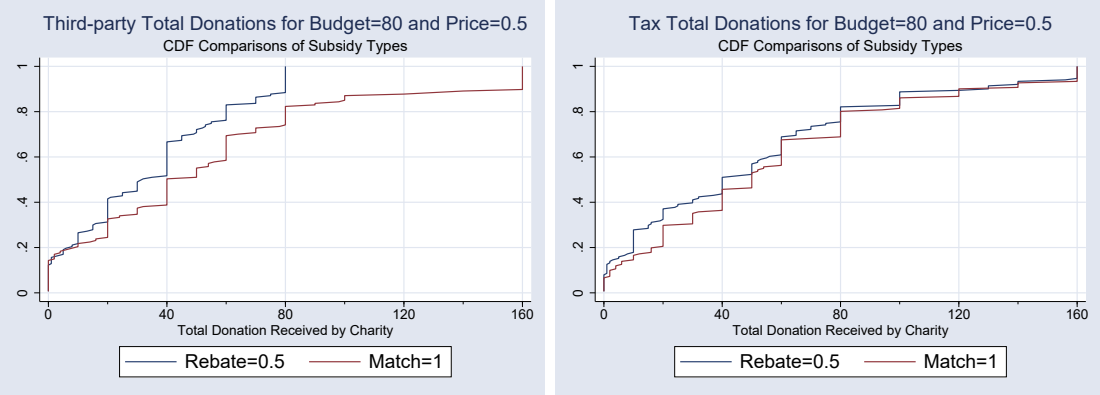

FIGURE B.6. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. Third-party on left, Tax on right.

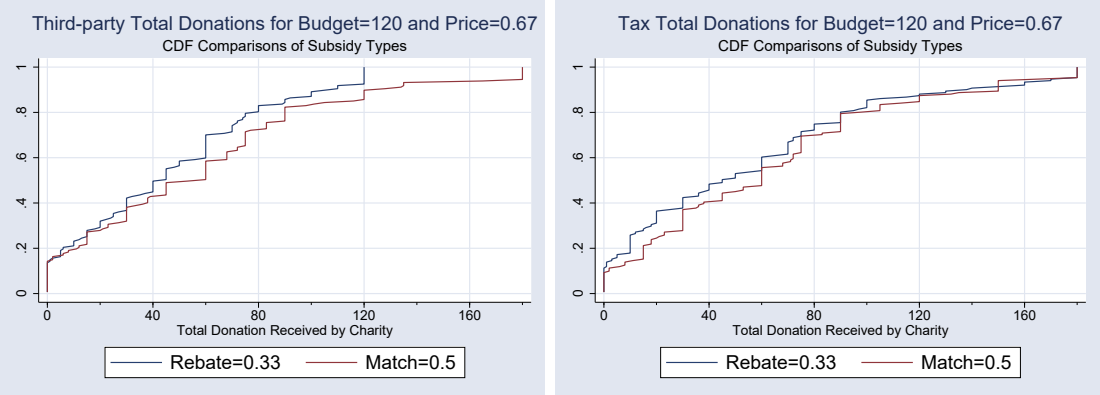

FIGURE B.7. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=120. Third-party on left, Tax on right.

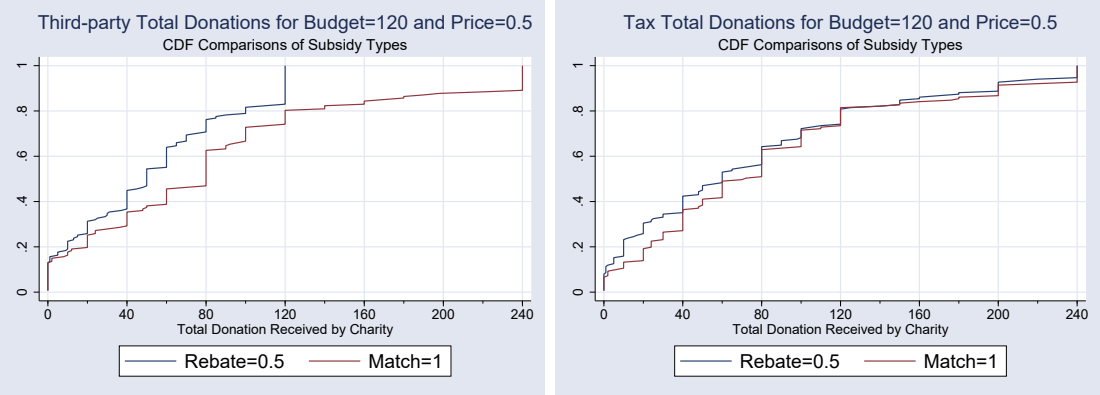

FIGURE B.8. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. Third-party on left, Tax on right.

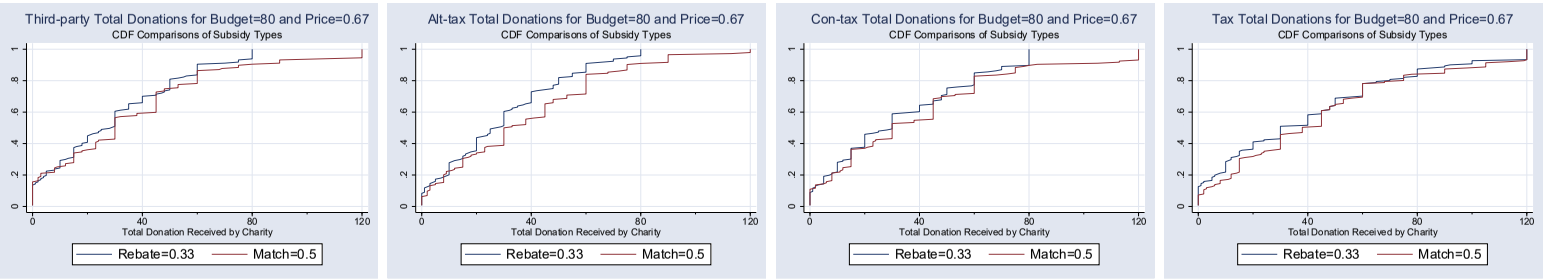

FIGURE B.9. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=80. From left to right: Third-party, Alt-tax, Con-tax, Tax.

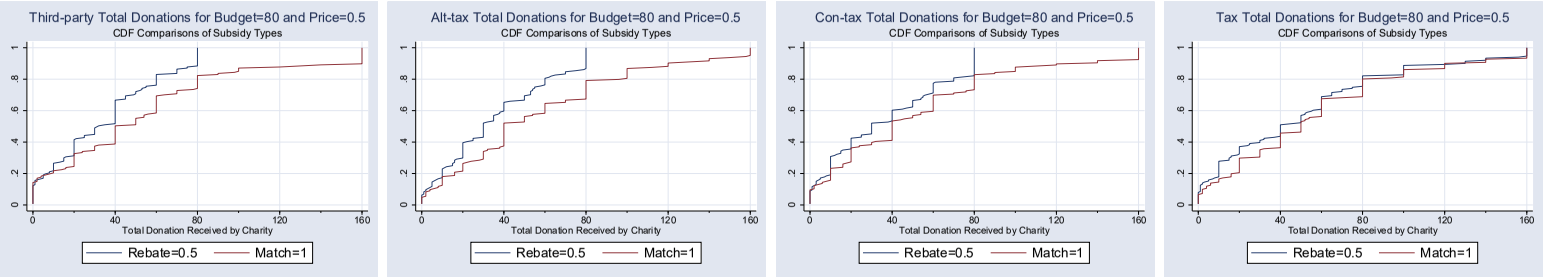

FIGURE B.10. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. From left to right: Third-party, Alt-tax, Con-tax, Tax.

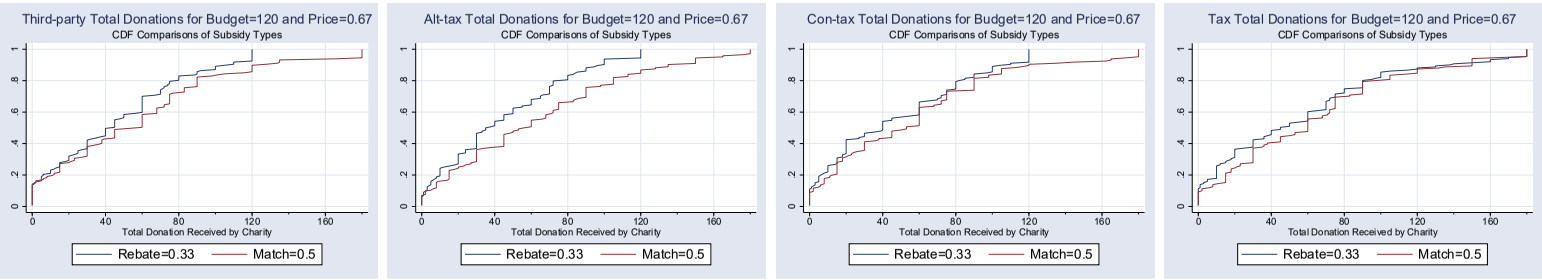

FIGURE B.11. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=120. From left to right: Third-party, Alt-tax, Con-tax, Tax.

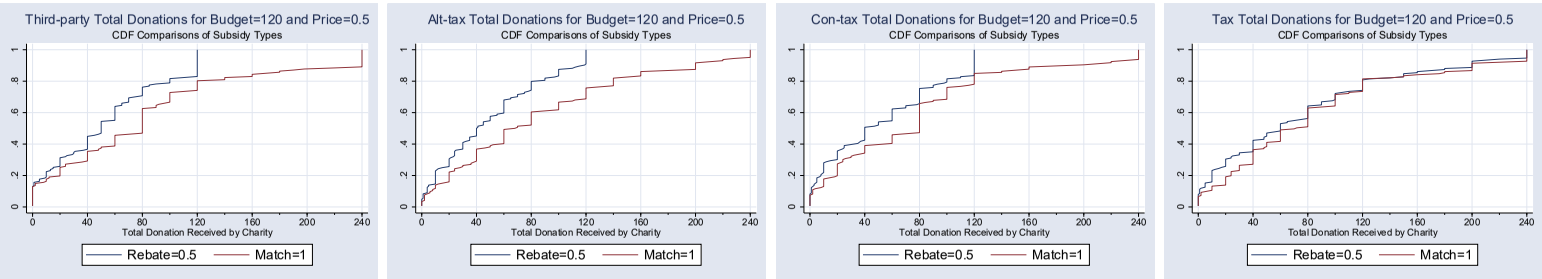

FIGURE B.12. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. From left to right: Third-party, Alt-tax, Con-tax, Tax.

B.3. **CDFs of Donations, comparing all experiments by Budget, Subsidy Type and Price.**

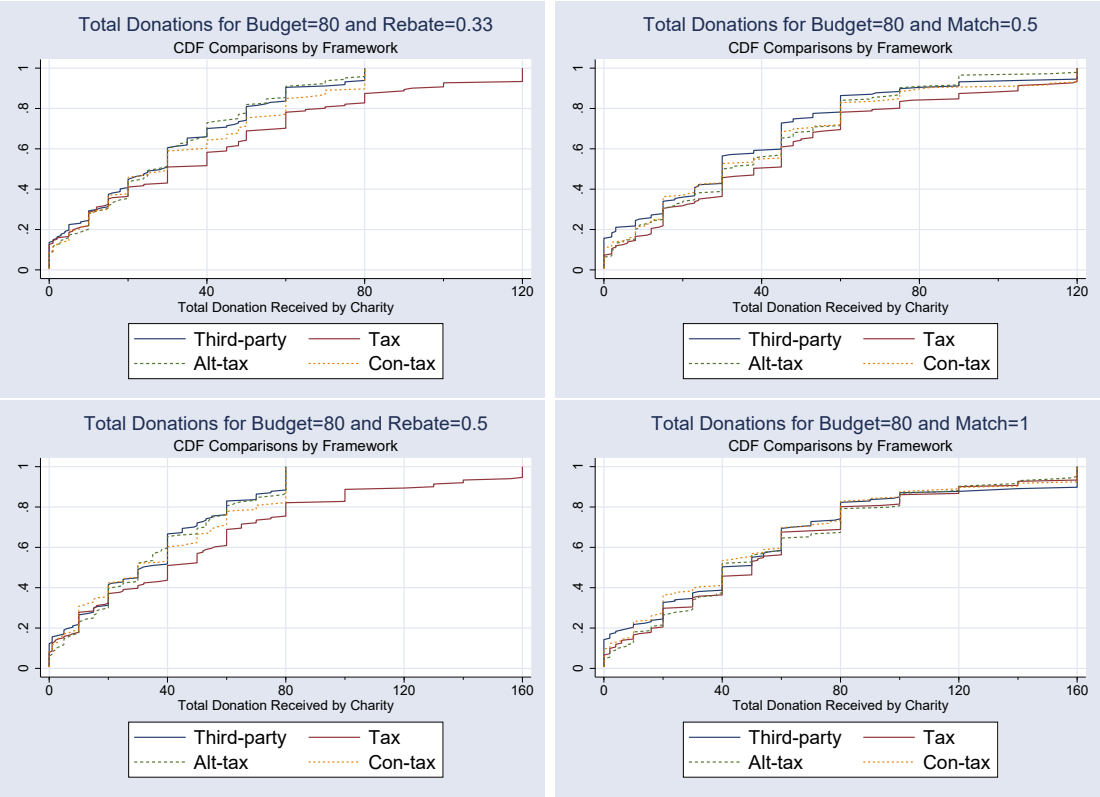

FIGURE B.13. CDFs of donations in all experiments for questions where Budget=80. Left-side graphs show rebate questions, right-side graphs show match questions. Top graphs show Price=0.67, bottom graphs show Price=0.5.

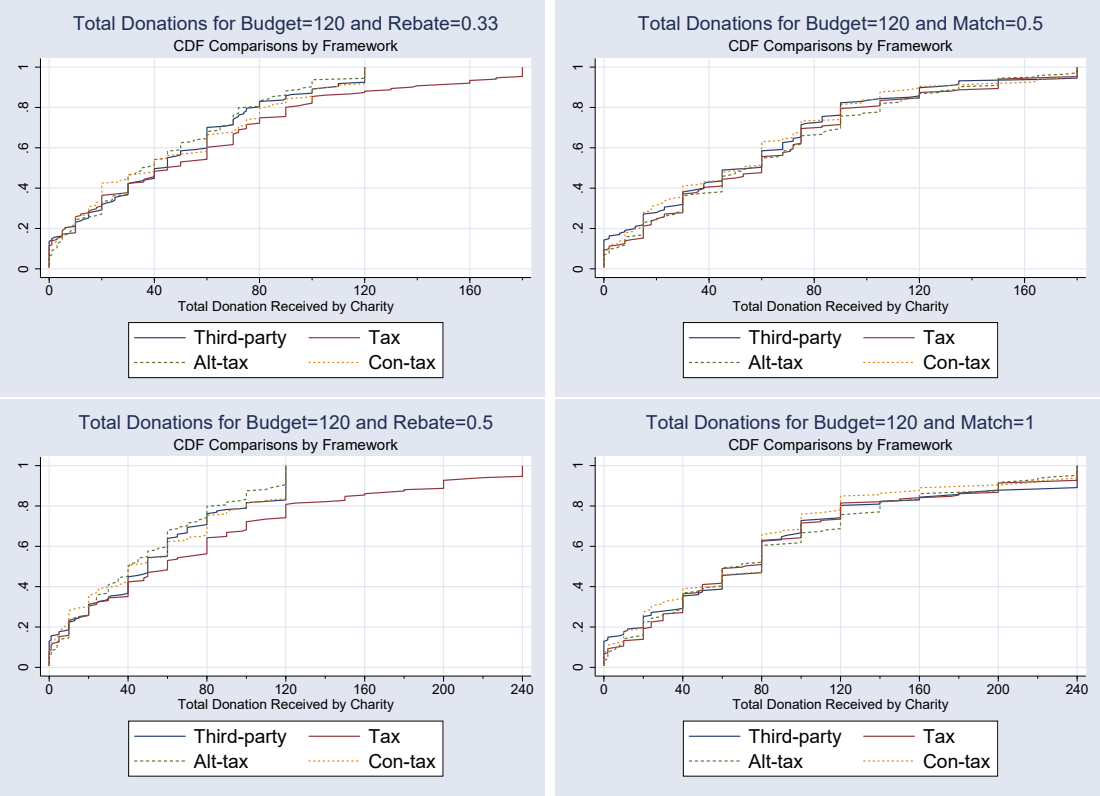

FIGURE B.14. CDFs of donations in all experiments for questions where Budget=120. Left-side graphs show rebate questions, right-side graphs show match questions. Top graphs show Price=0.67, bottom graphs show Price=0.5.

B.4. **Average Donations in All Treatments.**

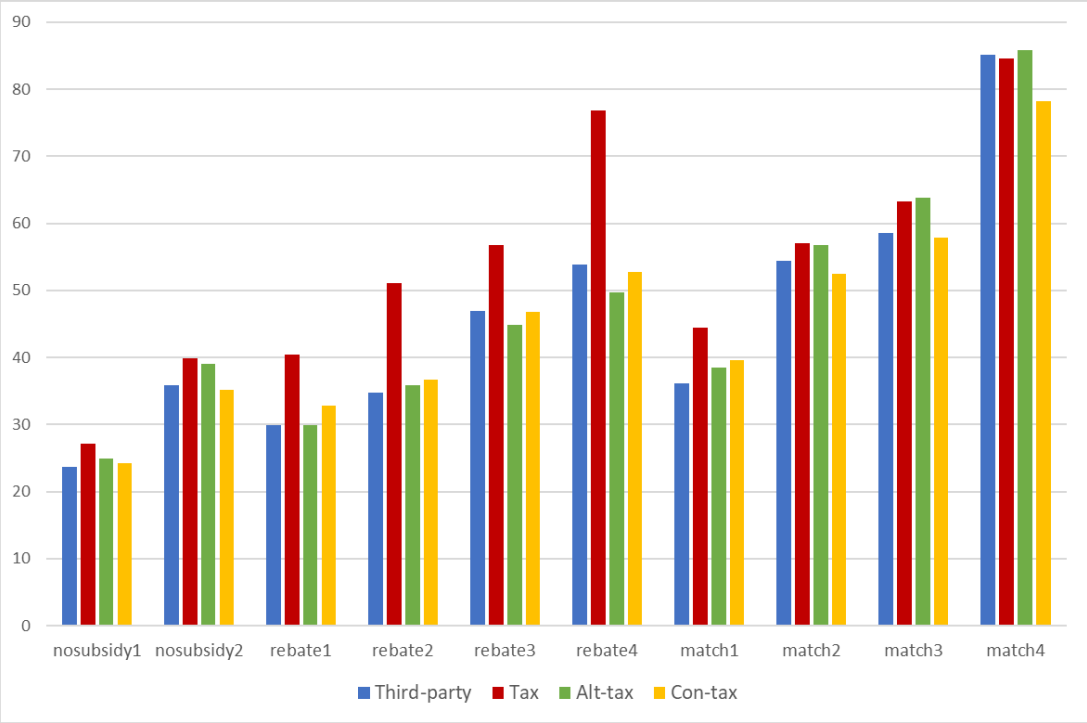

FIGURE B.15. Average donations for each question in all treatments.

B.5. **Added Information Treatment of Davis et al. (2005).**

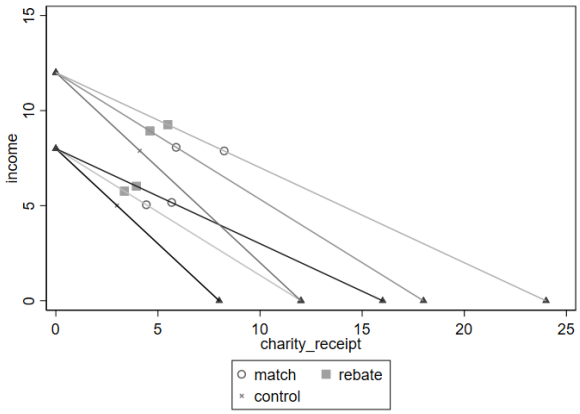

FIGURE B.16. Plot of Table 6 from Davis et al. (2005) – "added information" treatment.

Appendix C. Additional Analysis

This section pools data from the *third-party* and *tax* experiments.

C.1. **Combined Model.** We run the following combined model (equation C.1), where D_{ij} is an indicator for the tax framework and all the other variables are defined as in the main text. The full results of the combined model are presented in Table C.1.

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

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 (see the *third-party* and *tax* results in Table 3). 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.

We see that donations are more responsive to rebate prices in the tax experiment (p-value =0.053). Next, we examine whether the gap in price elasticities is smaller in the tax experiment. To do that, it is enough to test whether $(\beta_4 - \beta_5)$ is statistically significantly different than zero. We find that the mean (standard error) of $(\beta_4 - \beta_5)$ is -48.67 (15.44) and is significantly different than zero at the 1% significance level (p-value=0.002). This provides strong support for our Hypothesis 1.

C.2. **Censoring alone cannot explain our findings.** To test whether censoring can explain our results, we run the same combined model as in equation C.1, with one exception. We now replace Y_{ij} with $\tilde{Y}_{ij}$, where $\tilde{Y}_{ij}$ is defined as:

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

Regression Results: random effects tobit maximum likelihood

Dependent variable=Y=total donation received by charity			
Variable	(1) Third-party Coefficient α (standard error)	(2) Tax Coefficient $\alpha+\beta$ (standard error)	(3) Third-party = Tax p-value
Constant–no subsidy $(\alpha_0; \alpha_0+\beta_0)$	-85.10 (85.13)	-67.55 (80.82)	0.70
Constant–rebate subsidy $(\alpha_0+\alpha_6; \alpha_0+\alpha_6+\beta_0+\beta_6)$	-157.97 (81.09)	-171.77* (80.71)	0.71
Constant–match subsidy $(\alpha_0+\alpha_7; \alpha_0+\alpha_7+\beta_0+\beta_7)$	-272.30** (80.93)	-185.13* (80.64)	0.02
Endowment–no subsidy $(\alpha_1; \alpha_1+\beta_1)$	31.51** (8.07)	30.20** (5.42)	0.89
Endowment–rebate subsidy $(\alpha_1+\alpha_2; \alpha_1+\alpha_2+\beta_1+\beta_2)$	47.19** (5.69)	50.93** (5.34)	0.63
Endowment–match subsidy $(\alpha_1+\alpha_3; \alpha_1+\alpha_3+\beta_1+\beta_3)$	69.11** (5.58)	54.97** (5.31)	0.07
Rebate price $(\alpha_4; \alpha_4+\beta_4)$	**-32.78**** (8.03)	**-58.64**** (10.72)	**0.05**
Match price $(\alpha_5; \alpha_5+\beta_5)$	**-84.99**** (7.85)	**-62.19**** (10.68)	**0.09**
Tax rate (β_8)		-4.77 (13.17)	
Rebate price - Match price $(\alpha_4-\alpha_5; \alpha_4+\beta_4-\alpha_5-\beta_5)$	**52.21**** (11.22)	**3.54** (10.60)	**0.00**
Subjects			298
Observations			3,282

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

Note: The coefficients and p-values are derived from the following combined model: $Y_{ij} = \alpha_0 + \beta_0 \cdot D_{ij} + \alpha_1 \cdot E_{ij} + \beta_1 \cdot E_{ij} \times D_{ij} + \alpha_2 \cdot E_{ij} \times R_{ij} + \beta_2 \cdot E_{ij} \times R_{ij} \times D_{ij} + \alpha_3 \cdot E_{ij} \times M_{ij} + \beta_3 \cdot E_{ij} \times M_{ij} \times D_{ij} + \alpha_4 \cdot P_{ij} \times R_{ij} + \beta_4 \cdot P_{ij} \times R_{ij} \times D_{ij} + \alpha_5 \cdot P_{ij} \times M_{ij} + \beta_5 \cdot P_{ij} \times M_{ij} \times D_{ij} + \alpha_6 \cdot R_{ij} + \beta_6 \cdot R_{ij} \times D_{ij} + \alpha_7 \cdot M_{ij} + \beta_7 \cdot M_{ij} \times D_{ij} + \beta_8 \cdot T_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij}$, where D_{ij} is an indicator for the tax framework and all the other variables are defined as in the main text.

TABLE C.1. Combined (third-party and tax experiment) results

where R_{ij} is an indicator equal to 1 when a rebate subsidy is provided and 0 otherwise.

Note that censoring Y_{ij} in this way only has the potential to affect the total donation amounts in the rebate questions of the *tax* experiment. If censoring in the *third-party* experiment only causes the results of the *third-party* and *tax* experiments to differ through its mechanical effect on observed donations, then when we also censor the total donations in the *tax* experiment, the results from both experiments should be identical. In particular, the gap in price elasticities in the *third-party* experiment and the gap in price elasticities in the *tax* experiment should be identical. However, this is not what we find.

The full results of the combined model with censored observations in the tax framework are presented in Table C.2. 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 between experiments. While the coefficients on the rebate price and match price in the third-party framework (α_4 and α_5, respectively) are statistically different ($p = 0.000$), in the tax framework they are only marginally statistically different ($p = 0.079$). More importantly, the gaps in price elasticities are statistically significantly different. The gap between the rebate- and match-price estimates in the third-party framework is given by $\alpha_4 - \alpha_5 = 52.389$. The corresponding gap in the tax framework is given by $(\alpha_4 + \beta_4) - (\alpha_5 + \beta_5) = 18.181$. Thus, when using the censored observations $\tilde{Y}_{ij}$, the gap *between gaps* (for the *third-party* and *tax* experiments), γ, is equal to $\gamma = (\alpha_4 - \alpha_5) - [(\alpha_4 + \beta_4) - (\alpha_5 + \beta_5)] = \beta_5 - \beta_4 = -34.207$. This gap is significantly different from zero at the 5% significance level (p-value=0.022).

Our results show that behavior is significantly different between the *third-party* and *tax* experiments even when we intentionally censor the donations under rebates in the *tax* experiment just like the *third-party* experiment. Although censoring partly explains why previous work identifies large gaps in price elasticities, the significant drop in the gap in elasticities indicates that the mechanical effect of the constraint under rebates cannot fully explain the observed differences between the *third-party* and *tax* experiments. Therefore, we reject the null hypothesis that donations are only mechanically affected by the constraint under rebates. 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=$\tilde{Y}$=censored total donation amounts received by charity			
Variable	(1) Third-party Coefficient α (standard error)	(2) Tax Coefficient $\alpha+\beta$ (standard error)	(3) Third-party = Tax p-value
Constant–no subsidy $(\alpha_0; \alpha_0+\beta_0)$	-89.14 (81.65)	-73.09 (77.52)	0.71
Constant–rebate subsidy $(\alpha_0+\alpha_6; \alpha_0+\alpha_6+\beta_0+\beta_6)$	-161.75** (77.79)	-119.23 (77.67)	0.24
Constant–match subsidy $(\alpha_0+\alpha_7; \alpha_0+\alpha_7+\beta_0+\beta_7)$	-276.37** (77.63)	-193.67* (77.35)	0.02
Endowment–no subsidy $(\alpha_1; \alpha_1+\beta_1)$	31.49** (7.73)	30.27** (5.18)	0.90
Endowment–rebate subsidy $(\alpha_1+\alpha_2; \alpha_1+\alpha_2+\beta_1+\beta_2)$	47.12** (5.46)	39.43** (5.31)	0.31
Endowment–match subsidy $(\alpha_1+\alpha_3; \alpha_1+\alpha_3+\beta_1+\beta_3)$	69.09** (5.35)	55.65** (5.08)	0.07
Rebate price $(\alpha_4; \alpha_4+\beta_4)$	**-32.46**** (7.70)	**-44.34**** (10.47)	**0.36**
Match price $(\alpha_5; \alpha_5+\beta_5)$	**-84.85**** (7.52)	**-62.52**** (10.22)	**0.08**
Tax rate (β_8)		-4.77 (13.17)	
Rebate price - Match price $(\alpha_4-\alpha_5; \alpha_4+\beta_4-\alpha_5-\beta_5)$	**52.39**** (11.22)	**18.18** (10.60)	**0.02**
Subjects			298
Observations			3,282

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

Note: The coefficients and p-values are derived from the following combined model with censored donation amounts: $\tilde{Y}_{ij} = \alpha_0 + \beta_0 \cdot D_{ij} + \alpha_1 \cdot E_{ij} + \beta_1 \cdot E_{ij} \times D_{ij} + \alpha_2 \cdot E_{ij} \times R_{ij} + \beta_2 \cdot E_{ij} \times R_{ij} \times D_{ij} + \alpha_3 \cdot E_{ij} \times M_{ij} + \beta_3 \cdot E_{ij} \times M_{ij} \times D_{ij} + \alpha_4 \cdot P_{ij} \times R_{ij} + \beta_4 \cdot P_{ij} \times R_{ij} \times D_{ij} + \alpha_5 \cdot P_{ij} \times M_{ij} + \beta_5 \cdot P_{ij} \times M_{ij} \times D_{ij} + \alpha_6 \cdot R_{ij} + \beta_6 \cdot R_{ij} \times D_{ij} + \alpha_7 \cdot M_{ij} + \beta_7 \cdot M_{ij} \times D_{ij} + \beta_8 \cdot T_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij}$, where $\tilde{Y}_{ij}$ is censored total donations, D_{ij} is an indicator for the tax framework and all the other variables are defined as in the main text.

TABLE C.2. Combined (third-party and tax experiment) results using censored donation amounts

Appendix D. Experimental Materials

D.1. **Third-party Experiment Materials.**

D.1.1. *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. 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.

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.

D.1.2. *Third-party Decision Sheet.* Figure D.1 shows an example of the decision sheets presented to subjects in the *third-party* experiment.

FIGURE D.1. Screenshot of an Example Decision Sheet for the *Third-party* Experiment in Qualtrics

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.				

D.2. **Tax Experiment Materials.**

D.2.1. *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.

D.2.2. *Tax Experiment Decision Sheet.* Figure D.2 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.	The total amount of Tokens held for yourself.	Your total earnings, accounting for any applicable taxes.	Total donation received by the Charity, including any applicable matched funds.
	Pass	Hold	Your earnings	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 D.2. Screenshot of an Example Decision Sheet for the Tax Experiment in Qualtrics

D.3. **Alt-tax Experiment Materials.**

D.3.1. *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.

D.3.2. *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 D.3. Screenshot of an Example Decision Sheet for the Alt-tax Experiment in Qualtrics

D.4. **Con-tax Experiment Materials.**

D.4.1. *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.

D.4.2. *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 D.4. Screenshot of an Example Decision Sheet for the Con-tax Experiment in Qualtrics

D.5. **Questionnaire.** [These survey questions were common to all experiments and were presented to subjects after they finished the experiment.]

1. What is your age in years?
2. What is your gender?
   - male
   - female
   - other
3. What is your Major (or intended Major if undeclared)?
4. What is your 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.
   - Less than $50,000
   - Between $50,000 and $75,000
   - Between $75,000 and $100,000
   - Between $100,000 and $150,000
   - Between $150,000 and $200,000
   - More than $200,000
   - Don't know/Prefer not to answer
5. How would you describe your political views?
   - moderate
   - liberal
   - unsure/undecided
   - prefer not to say
6. How important is religion in your life?
   - very important
   - important
   - somewhat important
   - not important
7. During the past 12 months, how much money have you donated to charitable causes?

- Less than $5
- Between $5 and $10
- Between $10 and $20
- More than $20
- Don't know/Prefer not to answer

8. 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:

9. How well did you understand how your earnings and total donations 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: