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

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Abstract.

This paper challenges the well-established result among existing experimental studies that donations are significantly more responsive to matches than to rebates. In previous experimental studies the budget sets available to subjects under rebates are constrained relative to those available under matches, biasing estimates of the rebate-price elasticity. We 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, we find no statistical difference between our estimated rebate- and match-price elasticities. Furthermore, we show that the constraint under rebates affects the entire distribution of observed behavior, not only the behavior of individuals for whom the constraint is binding. Our paper contributes to theories of charitable giving and has important implications for tax policy.

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1. Introduction

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

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

In this paper, we challenge the assertion that donations are significantly more price elastic under matches than under rebates. We show that the discrepancy observed between rebate- and match-price elasticities in previous experimental studies is largely driven by features of the experimental designs used. Previous experiments use what we refer to as the *third-party framework*. As we demonstrate in Section 2, rebates and matches are not presented on equal footing in the third-party framework. Relative to matches, subjects' budget sets (available *consumption/total donation* bundles) are constrained when presented with rebates, making the range of possible total donations (as well as the range of possible private consumption levels) smaller under rebates than under matches. To see this, consider a subject who is endowed with \$10 and presented with two price-equivalent subsidies for giving: a 1:1 match and a 50% rebate. In both cases, the subject can pass a maximum of \$10 to the charity. In the case of the match, if the subject passes all \$10

to the charity, the charity receives a total donation of $20 and the subject leaves with nothing. However, in the case of the rebate, if the subject passes all $10 to the charity, the charity only receives $10 and the subject walks away with $5. The subject's budget set is significantly constrained under the rebate relative to the match, despite the two being price equivalent.

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

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

To see that this setup creates equal budget sets for price-equivalent rebates and matches, recall our earlier example of a subject who is endowed with $10 and presented with a 1:1

[1]These rates guarantee price equivalency.

match and a 50% rebate.[2] In the tax framework, the subject faces a 50% tax on their income. To remove the wealth effects of the tax and to create equivalence with our earlier example, the subject's pre-tax income is increased to \$20. In the case of the match, donations are not tax exempt. Because of this, the subject must pay \$10 in taxes regardless of how much they choose to pass to the charity and can donate up to \$10. With the match, if the subject donates all of their after-tax income, the charity will receive a total donation of \$20 and the subject will walk away with nothing (just like our previous example). In the case of the rebate, any donation provided by the subject is tax exempt. Thus, the subject can pass up to \$20 to the charity—this reduces their taxable income to 0, so they owe no taxes. The charity receives a total donation of \$20, and the subject leaves with nothing, exactly the same as the match. There are two important takeaways. First, unlike the third-party framework, the tax framework removes the constraint on subjects' choices under rebates, creating equality between the budget sets for price-equivalent rebates and matches. Second, other than removing the constraint under rebates, the budget sets under the tax and third-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 (–.714 and –1.304, respectively, p-value=0.022), 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 –1.143 and –1.108, respectively—and there is no longer any statistically significant difference (p-value=0.891).

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

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

constraint in the third-party framework. To help eliminate other possible explanations for the shift in behavior, we also run two additional experiments: an *alt-tax* experiment and a *con-tax* experiment. The *alt-tax* experiment combines the third-party framework with the taxation language used in the *tax* experiment to test the extent to which subjects' behavior is affected by the use of tax *language*. The *con-tax* experiment combines the tax framework with the constraint on donations present under rebates in the *third-party* experiment to test whether behavior is affected by the *source of funding* used to provide the subsidies for giving.[3] Importantly, both experiments contain the rebate constraint present in the third-party framework. The results of these experiments are not significantly different from our *third-party* experiment, providing further evidence that the rebate constraint creates a behavioral response.

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

This study makes several important contributions. First, it provides new insights for discussions of tax policy. To the best of our knowledge, our paper is the first experimental study to compare rebates and matches within a tax framework. Note that the rebate subsidy in our tax framework functions similarly to the current tax system in the U.S. (at least for taxpayers who itemize their deductions). Thus, this framework not only resolves the constraint issue present in the third-party framework, but it also has the additional benefit of being more directly relevant for discussions regarding tax policy.

[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.

[4]See Section 6 for a discussion of the rebate- and match-price elasticities reported in previous studies.

Previous experimental studies have found large gaps between rebate- and match-price elasticities, leading many to suggest that restructuring the U.S. tax system—to provide matches rather than rebates—could potentially result in a significant increase in charitable giving. The results of our study, however, suggest that restructuring the tax system may in fact have little effect on donations.

Second, this study helps to clarify whether individuals truly view rebates and matches differently, helping to improve the literature's understanding of why people give. To isolate the effect of changing the type of subsidy used, all other factors must be controlled. This is a feat for which laboratory experiments are uniquely positioned to accomplish. In contrast, observational studies struggle to make direct comparisons of rebates and matches. Observational studies are typically conducted using tax data, which usually only contains rebate subsidies—precluding any comparisons of rebates and matches—and often lacks sufficient price variation (see Section 6 for more details). And while field experiments allow researchers to compare both rebates and matches in a setting where they are able to introduce significant price variation, there remain various confounding factors which might cause donors to respond differently to rebates and matches in the field (e.g., time delays and uncertainty involved in receiving a rebate, beliefs about the probability of receiving a match, etc.). Our paper provides a better understanding of donors' underlying preferences by designing a laboratory experiment that (i) controls for all such confounding factors and (ii) removes the disparity between budget sets for price-equivalent rebates and matches.

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

maintains the warm-glow motive while achieving parity in price elasticities across subsidies, albeit at the expense of equalizing donation levels under both subsidies. This paper reignites the discussion on accurately modeling charitable giving.

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

The remainder of this paper is organized as follows. Section 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 discusses extant theories of giving and proposes a simple extension capable of explaining equal rebate- and match-price elasticities. Section 6 provides a brief overview of related literature, including attempts made to explain the disparity between rebates and matches, previous attempts to resolve the budget set issue present in the third-party framework, and a discussion of how our rebate- and match-price elasticities compare with previous estimates. Section 7 concludes.

2. Theory

In this section we theoretically analyze the decision setting faced by donors in the third-party framework used in previous experimental studies, and then we develop and analyze our novel tax framework. We show two main results. First, we formally show

that in the third-party framework an individual's budget set under a rebate is a strict subset of their budget set under the price-equivalent match. Second, we show that our novel tax framework eliminates the discrepancy between budget sets for price-equivalent rebates and matches, allowing us to provide an unbiased comparison of the rebate- and match-price elasticities of giving.

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

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

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

$$\max_{g_n \in [0,w]} U(w - g_n,\ g_n,\ (1 + s_m)g_n + \lambda R_{-i}), \tag{2}$$

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

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

$$\max_{g_g \in [0,w]} U(w - (1 - s_r)g_g,\ g_g,\ g_g + \lambda R_{-i}), \tag{3}$$

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

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

It is important to recognize that in order to directly compare (2) and (3) we must first express them in terms of the same choice variable. Rewriting the rebate problem (3) in terms of the donor's *net* donation, $g_n = (1 - s_r)g_g$, gives

$$\max_{g_n \in [0,\ (1-s_r)w]} U\left(w - g_n,\ \frac{g_n}{1-s_r},\ \frac{g_n}{1-s_r} + \lambda R_{-i}\right). \tag{4}$$

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) can be written as

$$\max_{g_n \in [0,w]} U\left(w - g_n,\ g_n,\ \frac{g_n}{1-s_r} + \lambda R_{-i}\right). \tag{5}$$

Price-equivalent third-party rebates and matches can now be directly compared by comparing (4) and (5), respectively.

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

The second reason for the discrepancy between third-party rebates and matches is that the available budget sets for the two subsidies are different. As shown in (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 (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

[6]It is important to recognize that this is an assumption of the model. One could instead assume that donors feel warm glow in the same way for rebates and matches, in which case the model would predict identical behavior for price-equivalent rebates and matches (ignoring any differences in budget sets). We discuss this in greater detail in Section 5.

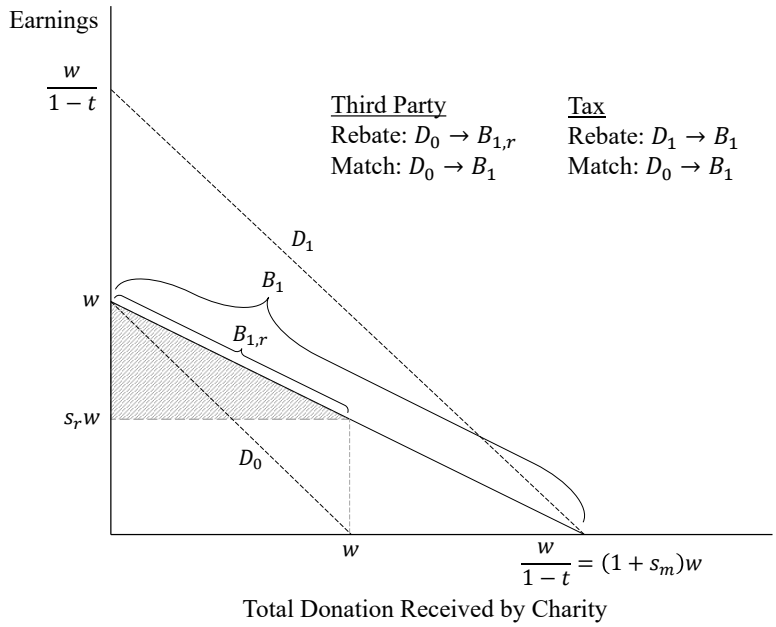

FIGURE 1. Budget sets for price-equivalent rebate and match subsidies in the third-party and tax frameworks.

the donor their entire income w. But when there is a rebate, the donor can never provide the charity with a total donation greater than w, and the donor cannot end up with less than $s_r w$ (that is, their donation can never cost them more than $g_n = (1 - s_r)w$). When subsidies are provided by a third party, the budget sets faced by donors under rebates are strict subsets of the budget sets they face under price-equivalent matches. This is shown graphically in Figure 1.

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

2.2. **Tax framework.** As we will now demonstrate, the disparity in budget sets—between price-equivalent third-party rebates and matches—is not present when the subsidies are instead provided via a tax system. Within a tax framework, individual i is endowed with

a gross income of y_i and faces an income tax of $0 \leq t < 1$. If there is a rebate subsidy provided for charitable donations, this is equivalent to donations being tax exempt. That is, $s_r = t$, and any donations that an individual passes to the charity will decrease their taxable income (decreasing their tax 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

$$\max_{g_g \in \left[0, \frac{w}{1-t}\right]} U\left((1-t)\left[\frac{w}{1-t} - g_g\right], g_g, g_g + \lambda R_{-i}\right). \tag{6}$$

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

$$\max_{g_n \in [0,w]} U\left(w - g_n, g_n, \frac{g_n}{1-t} + \lambda R_{-i}\right). \tag{7}$$

Comparing (7) to (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 (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

$$\max_{g_n \in [0,w]} U\left(w - g_n, \frac{g_n}{1-t}, \frac{g_n}{1-t} + \lambda R_{-i}\right). \tag{8}$$

Comparing (8) to (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 (7) and (8), the budget sets for price-equivalent rebates and matches are now equal in the tax framework, suggesting the gap between elasticities should decrease. That being said, if the difference in the amount of warm glow received under rebates and matches still exists, then the gap between elasticities need not completely disappear.

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. The tax framework resolves this issue by allowing individuals to choose a point on the *decision set* D_1, representing their pre-tax income. After choosing a point along D_1, the tax then moves their final consumption point vertically downward, making the entire budget line B_1 obtainable. Therefore, while the comparison of rebate- and match-price elasticities is confounded by differences in budget sets when using the third-party framework, this issue is not present when using our tax framework.

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

2.4. **Main Hypotheses.** The preceding analysis leads to two testable hypotheses.

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

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

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

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

3. Experimental Design and Procedures

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

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

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

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 Appendix D.

In what follows, we first outline the designs of our experiments. 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-subject design with three sources of variation in the problems: (1) the endowment amount ($w \in \{80, 120\}$), (2) the price of giving ($p \in \{1, 0.67, 0.5\}$), and (3) the type of subsidy used (either a match, a rebate, or no subsidy). Table 1 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

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

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

TABLE 1. List of budget sets used in the third-party experiment.

any confusion subjects may have regarding the effects of the subsidies. Depending on the treatment the subject is randomly assigned to, either the rebate is always shown before the equivalent match, or vice versa.

Figure D.1 in 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.[11] Once acceptable pass amounts have been entered for each problem, subjects submit their decisions for all 10 problems simultaneously. At the conclusion of the experiment, one problem is randomly selected to determine the subject's payment and donation to the charity.

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

[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), $(1 + s_m)g_i$.

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

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.

sets (i.e., those without any subsidies for giving) are implemented using both tax rates.[12] Thus, there are two additional baseline problems relative to the *third-party* experiment, resulting in a total of 12 decision problems rather than 10.

Figure D.2 in 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 that they would like to pass to the charity. When no subsidy is provided or a matching subsidy is provided, any amount passed by subjects is *not* tax exempt. Thus, subjects owe a tax bill of $ty = \frac{tw}{1-t}$ regardless of any amount they choose to pass, effectively leaving them with only w to be allocated between themselves and the charity. However, when a rebate subsidy is provided, any amount subjects pass to the charity *is* tax exempt and lowers their tax bill by an amount equal to tg. Thus, subjects are now able to pass their entire initial

[12]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.

endowment of y to the charity, since they will only need to pay taxes on any amount they choose to hold. The *tax* experiment is otherwise identical to the *third-party* experiment.

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

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

3.4. **Con-tax experiment.** The *con-tax* experiment is identical to our *tax* experiment, with one exception: there is now an artificial constraint placed on the amount subjects are able to pass under rebates. The parameters used in the *con-tax* experiment are the same as those used in the *tax* experiment, listed in Table 2. Figure **??** in 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 this experiment should generate donation behavior similar to the *tax* experiment—at least for unconstrained subjects—since the only difference between these two experiments is the presence of a constraint under rebates. However, if being constrained under rebates has a behavioral effect on donation decisions, then donations under rebates in the *con-tax* experiment will be very similar to the donations under rebates in the *third-party* experiment.

3.5. **Experimental Procedures.** The two main experiments—*third-party* and *tax*—were run synchronously with a total of 147 and 151 subjects, respectively, between June

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

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

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

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

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

microphones before transferring all of the subjects to the main Zoom room to begin the experiment. The experimenter was able to track the survey progress of each participant in real-time using the Qualtrics software. At all times, subjects were able to use the Zoom chat to communicate with the experimenter, but communication between subjects was disabled.

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

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

4. Results

We first report the results of the baseline *third-party* experiment and demonstrate that they are qualitatively and quantitatively in line with previous studies. We then report the results of the *tax* experiment and demonstrate that behavior in the tax framework is significantly different from that in the third-party baseline. Finally, we report the results of our two follow-up experiments: the *alt-tax* experiment and the *con-tax* experiment.

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

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.15 in which we plot the average decisions made in the Davis et al. (2005) study.[17] We can see that our results are qualitatively in line with their findings.

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

We next estimate rebate- and match-price elasticities. Following Eckel and Grossman (2003), we estimate the demand for charitable giving using a log-linear specification with subject-level random effects:

$$(1)\quad Y_{ij} = \beta_0 + \beta_1 \cdot E_{ij} + \beta_2 \cdot R_{ij} \times E_{ij} + \beta_3 \cdot M_{ij} \times E_{ij} + \beta_4 \cdot R_{ij} \times P_{ij} + \beta_5 \cdot M_{ij} \times P_{ij} + \beta_6 \cdot R_{ij} + \beta_7 \cdot M_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij},$$

where $i = 1, \ldots, 147$ indexes subjects, $j = 1, \ldots, 10$ indexes the allocation decision problems, $Y_{ij} = \ln(\text{Total Donation})_{ij}$, $E_{ij} = \ln(\text{Endowment})_{ij}$, $P_{ij} = \ln(\text{Price})_{ij}$, 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. Total Donation is total *gross* donation (in Tokens) received by the charity + 1 Token. Note that we add 1 Token to total donations because the logarithm of zero is not defined. Endowment is defined as total Tokens provided to the subject (80 Tokens or 120 Tokens). Price is defined as price of giving \$1 to the charity (\$0.50, \$0.67, or \$1.00).

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

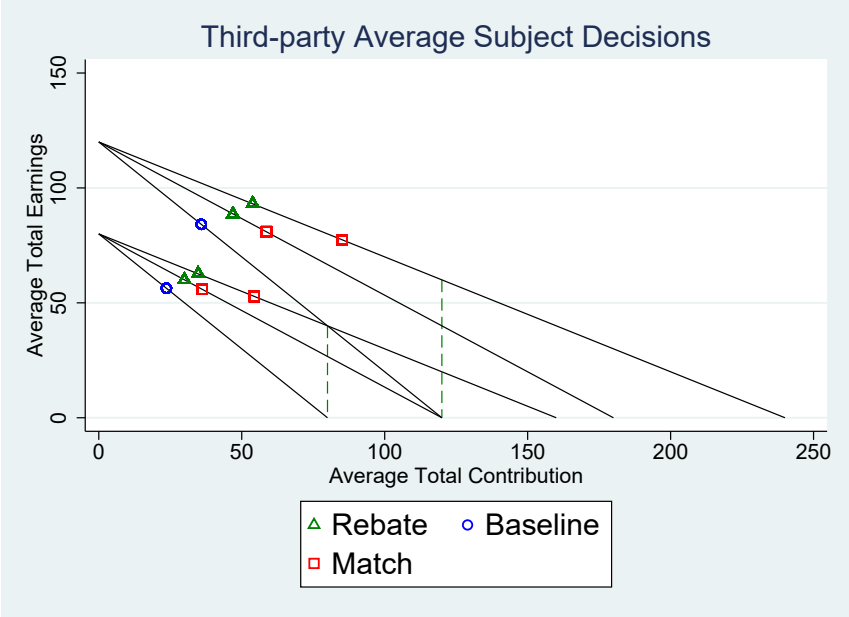

FIGURE 2. Average decisions in our third-party experiment.

Equation (1) is estimated using a random-effects tobit maximum likelihood model grouped by subject i, accounting for the censoring of subjects' Total Donations from both below and above. The random effects, ν_i, are assumed i.i.d. $N(0, \sigma_\nu^2)$, and the ε_{ij} are assumed i.i.d. $N(0, \sigma_\varepsilon^2)$ independently of ν_i.

Column 1 of Table 3 presents the results of our *third-party* experiment. In addition, Table A.4 presents the results of our *third-party* experiment alongside the results of the original Eckel and Grossman (2003) experiment. Our rebate- and match-price elasticity estimates are somewhat higher (in absolute terms) than those estimated by Eckel and Grossman (–0.714 and –1.304 vs. –0.340 and –1.067, respectively).[18] However, like Eckel and Grossman (2003), we find that our estimated rebate- and match-price elasticities are significantly different at the .05 level of confidence (p-value=0.022). That is, consistent with previous studies, we find that subjects are significantly more responsive to matches than rebates.

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

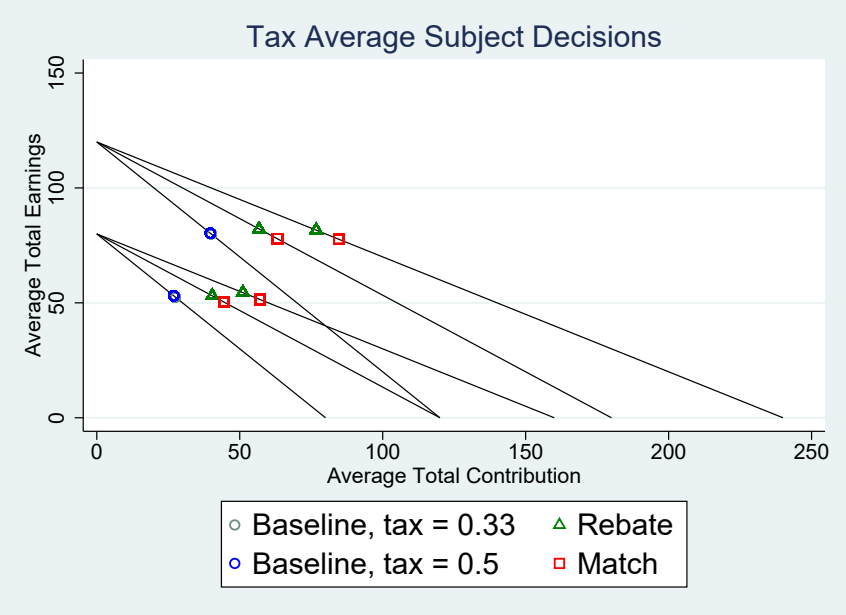

FIGURE 3. Average decisions in the tax experiment.

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

Figure 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, and both subsidies appear to move outward from the baselines (circles) along the same paths (unlike the *third-party* experiment, in which rebates appear to move diagonally upward while matches appear to move horizontally outward). When rebates and matches are presented within the tax framework, they appear to produce similar responses to price changes, contrary to the behavior observed in the third-party framework.

Table A.5 reports the average donation amounts by subsidy type for each budget in the *tax* experiment. Relative to the *third-party* experiment, the gap between average donation amounts for price-equivalent rebates and matches is now substantially smaller. That being said, rebates and matches are still found to be significantly different at the 1%

significance level, reflecting the fact that subjects still tend to donate more under matches than rebates. However, the gap between rebates and matches now appears to be more or less fixed across prices, holding endowment constant.

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

$$(2)\quad Y_{ij} = \beta_0 + \beta_1 \cdot E_{ij} + \beta_2 \cdot R_{ij} \times E_{ij} + \beta_3 \cdot M_{ij} \times E_{ij} + \beta_4 \cdot R_{ij} \times P_{ij} + \beta_5 \cdot M_{ij} \times P_{ij} + \beta_6 \cdot R_{ij} + \beta_7 \cdot M_{ij} + \beta_8 \cdot T_{ij} + X_i\gamma + \nu_i + \varepsilon_{ij},$$

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

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

Column 2 of Table 3 presents the estimation results for the *tax* experiment. Rebate- and match-price elasticities are now estimated to be nearly identical (–1.143 and –1.108, respectively). While there is a significant difference between the rebate- and match-price elasticities in the *third-party* experiment (p-value=0.022), there is no longer any significant difference between elasticities in the *tax* experiment (p-value=0.891). That is, when the subsidies for giving are presented in the framework of taxation, subjects respond to rebate- and match-price changes equally.

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

behavior. Both Figure 3 and our regression analysis show support for this prediction. Looking at Figure 3, we see that subjects' decisions appear to be unaffected by the tax rate. The blue circles depict average baseline choices when the tax rate is 0.5, and the teal circles depict average baseline choices when the tax rate is 0.33. It is very difficult to see both circles since they are nearly perfectly overlapping. That is, for a given budget line, absent of any subsidy for giving, on average decisions are not affected by the tax rate. The same conclusion holds when we look at the estimated coefficient on Tax Rate in column 2 of Table 3. Given that Net Endowment accounts for the impact of the tax rate on subjects' budgets, the estimated coefficient on Tax Rate captures the effect of facing a higher tax rate, holding all else equal (including subjects' budgets). Based on the coefficient estimate reported in Table 3 (–0.264), it appears that subjects may have a small negative behavioral response to being taxed at a higher rate. However, this effect is statistically insignificant (p-value=0.42).

We conclude this section by noting that the absence of a gap between rebate- and match-price elasticities in the *tax* experiment is unexpected and is inconsistent with extant models of charitable giving, as we demonstrated in Section 2 (assuming individuals derive warm-glow from donations). We provide a more detailed discussion of this issue in Section 5.

Regression Results: random effects tobit maximum likelihood				
Dependent variable=ln(total donation received by charity)				
	(1) Baseline Coefficient (*s.e.*) [elasticity]	(2) Tax Coefficient (*s.e.*) [elasticity]	(3) Alt-Tax Coefficient (*s.e.*) [elasticity]	(4) Con-Tax Coefficient (*s.e.*) [elasticity]
Constant (no subsidy)	-3.260 (4.288)	1.397 (3.084)	-4.534 (2.517)	-5.644* (2.801)
Constant (rebate subsidy)	-3.549 (4.240)	.746 (3.083)	-4.111 (2.516)	-4.396 (2.802)
Constant (match subsidy)	-4.437 (4.239)	.932 (3.082)	-5.203* (2.514)	-6.076* (2.799)
(Net) Endowment (no subsidy)	.934** (.201) [.850]	.685** (.136) [.663]	.856** (.130) [.839]	.808** (.157) [.772]
(Net) Endowment (rebate subsidy)	1.027** (.143) [.934]	.772** (.135) [.747]	.751** (.131) [.736]	.565** (.159) [.540]
(Net) Endowment (match subsidy)	1.191** (.142) [1.083]	.798** (.135) [.772]	1.006** (.129) [.987]	.931** (.156) [.890]
Rebate price	-.785** (.202) [-.714]	-1.181** (.271) [-1.143]	-.795** (.260) [-.780]	-.622* (.314) [-.594]
Match price	-1.434** (.200) [-1.304]	-1.144** (.270) [-1.108]	-1.318** (.259) [-1.293]	-1.038** (.311) [-.993]
Tax rate		-.264 (.331) [-.256]	-.087 (.317) [-.086]	-.082 (.381) [-.079]
Controls	Y	Y	Y	Y
Subjects	147	151	144	146
Observations	1470	1812	1728	1752

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

Note: The models for each experiment are run separately.

TABLE 3. Regression results for all experiments

	(1) third-party Coefficient (*s.e.*) [elasticity]	(2) tax Coefficient (*s.e.*) [elasticity]	(3) third-party Coefficient (*s.e.*) [elasticity]	(4) tax Coefficient (*s.e.*) [elasticity]
ln_net_income	0.934**	0.685**	0.839**	0.723**
	(0.201)	(0.136)	(0.175)	(0.150)
rebate_x_net_income	0.092	0.087	-0.024	0.112
	(0.247)	(0.192)	(0.214)	(0.212)
match_x_net_income	0.256	0.113	0.165	0.070
	(0.246)	(0.192)	(0.214)	(0.211)
tax_rate		-0.264		-0.060
		(0.331)		(0.365)
rebate_x_price	-0.785**	-1.181**	-0.478**	-0.803**
	(0.202)	(0.271)	(0.173)	(0.299)
match_x_price	-1.434**	-1.144**	-1.059**	-1.052**
	(0.200)	(0.270)	(0.173)	(0.297)
rebate_dummy	-0.289	-0.651	0.234	-0.759
	(1.140)	(0.894)	(0.988)	(0.986)
match_dummy	-1.177	-0.465	-0.711	-0.310
	(1.136)	(0.892)	(0.987)	(0.982)
Constant	-3.260	1.397	-4.402	0.667
	(4.288)	(3.084)	(4.261)	(3.378)
Subjects Dropped	N	N	Y	Y
Controls	Y	Y	Y	Y
Subjects	147	151	119	103
Observations	1470	1812	1190	1236

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

TABLE 4. A comparison of the third-party and tax experiments, dropping subjects who are constrained under rebates (or would be in the third-party framework).

4.3. **The constraint under rebates has a behavioral effect.** The results of our *tax* experiment suggest that, contrary to the conclusions drawn from previous studies, rebates and matches may in fact produce equal price elasticities of giving. The ability of our *third-party* experiment to replicate the large gap between rebate- and match-price elasticities

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

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

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

To further demonstrate the extent to which behavior changes between experiments, we censor any total donation amounts exceeding the subject's net endowment under rebates

in the *tax* experiment and re-estimate a combined model for our *third-party* and *tax* experiments. The findings, detailed in Appendix C.1, decisively indicate that censoring alone cannot account for our main results. In fact, this analysis suggests an even greater disparity in behavior between the two frameworks.

Expanding subjects' budget sets has previously been shown to significantly affect behavior, even for unconstrained subjects for whom expansions of the budget set should theoretically have no impact (List, 2007; Bardsley, 2008). That is, expanding the budget set available to subjects affects the *entire* distribution of observed behavior. Thus, given that the *third-party* experiment provides budget sets under rebates that are strict subsets of the budget sets under the price-equivalent matches, it is to be expected that the distribution of donations under a rebate will be significantly lower than the distribution of donations under an equivalent match.[19] The greater the subsidy provided (and the lower the price of giving) the more significant the shift in the distribution will be, since the disparity between the budgets sets for rebates and matches increases with the size of subsidy. Figures 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 .5.

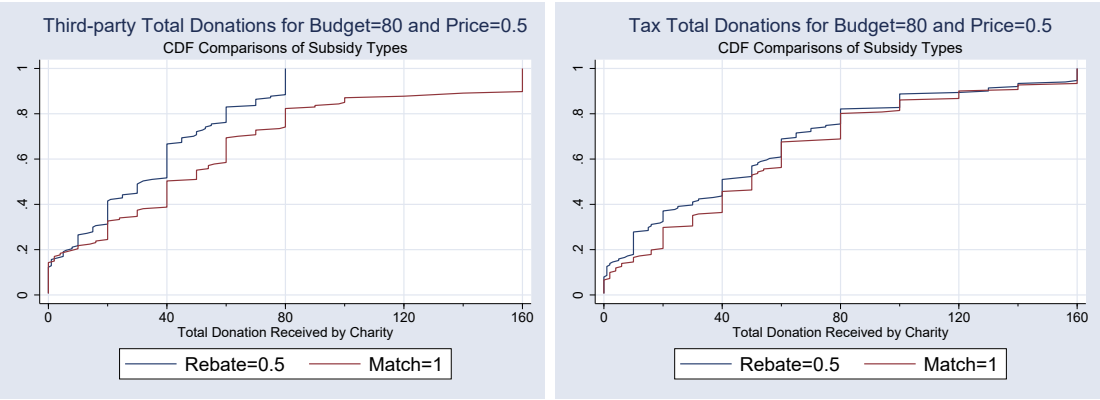

FIGURE 4. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. Third-party on left, Tax on right.

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

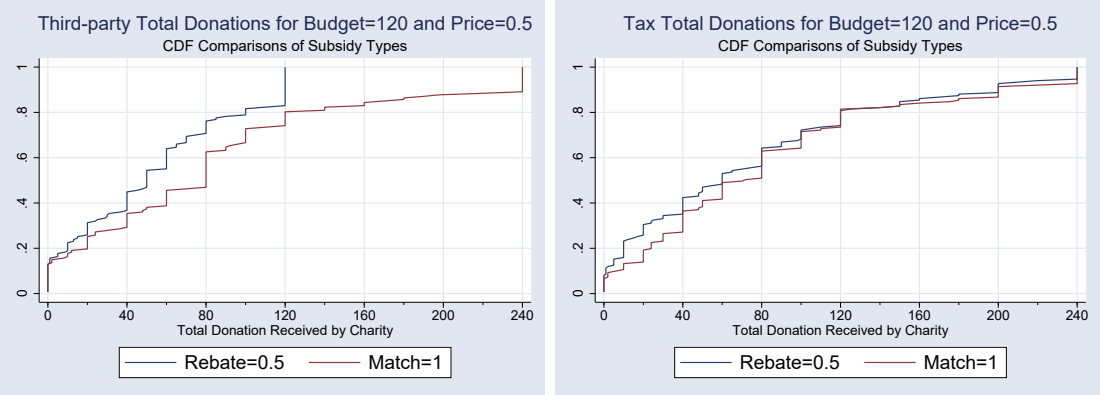

FIGURE 5. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. Third-party on left, Tax on right.

While the distributions for rebates and matches diverge well before the constraint occurs in the *third-party* experiment, they overlap in the *tax* experiment when the constraint is removed. The constraint in the third-party framework appears to shift the entire distribution of behavior under the rebate. Further supporting this claim, Figures 6 and 7 show that the match distributions are the same in the *third-party* and *tax* experiments. That is, it is the rebate distribution that shifts, not the match distribution. This is consistent with our claim that the constraint under rebates in the third-party framework induces a behavioral response.

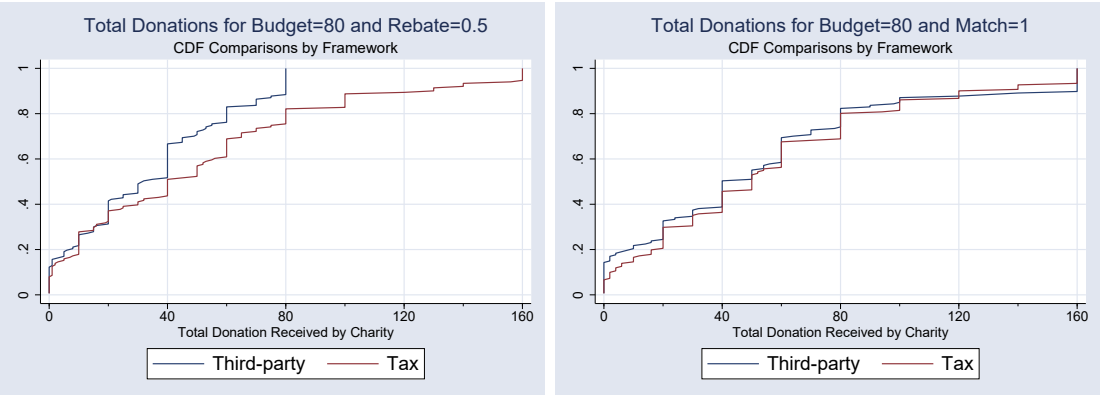

FIGURE 6. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=80. Third-party in blue, Tax in red.

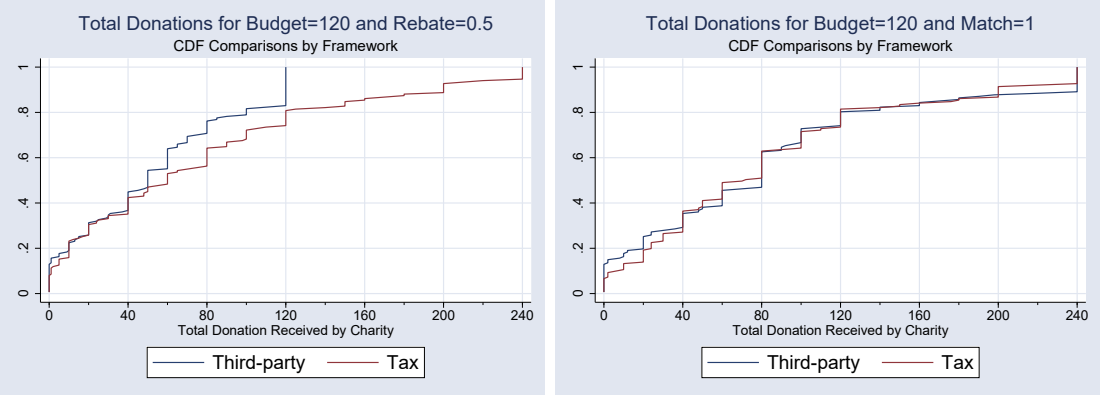

FIGURE 7. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=120. Third-party in blue, Tax in red.

4.4. **Follow-up experiments.** Section 4.3 illustrates that the distribution of donations under rebates shifts significantly between the *third-party* and *tax* experiments. In this section, we present evidence that this shift is predominantly driven by the presence of the constraint in the *third-party* experiment, and is not driven by the tax language used in the *tax* experiment. Recall that both of our follow-up experiments—the *alt-tax* experiment and the *con-tax* experiment—use the exact same budget sets as the *third-party* experiment: the *alt-tax* experiment mirrors the *third-party* experiment but employs the tax language used in the *tax* experiment, and the *con-tax* experiment mirrors the *tax* experiment but enforces the same constraint on donations under rebates that is present in the *third-party* experiment. Thus, if the use of tax language is responsible for the shift in donation behaviors under rebates, this shift will also be evident in the follow-up experiments. However, if the shift in behavior is mostly driven by the presence of the constraint, then behavior in the follow-up experiments should resemble that of the *third-party* experiment.

The results in both of the follow-up experiments are very similar to those of the *third-party* experiment. Figure 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,

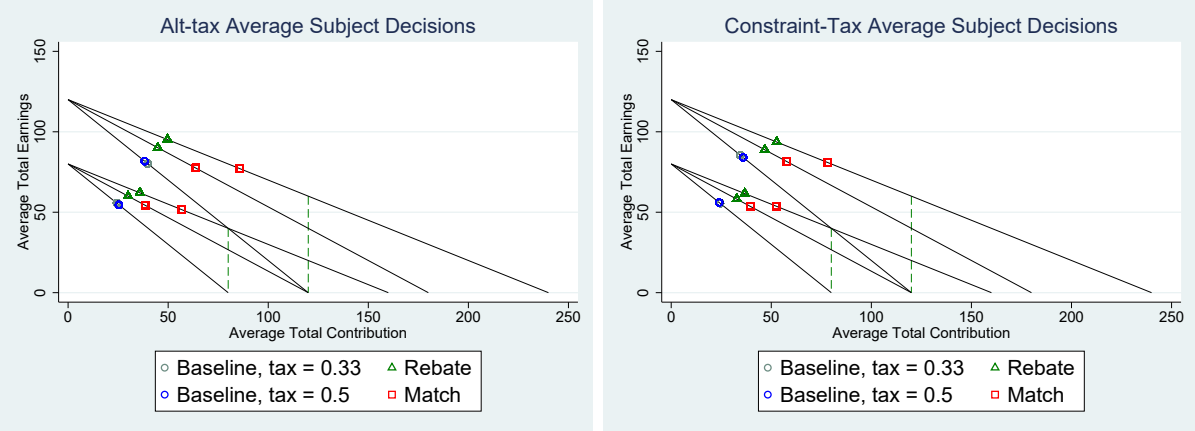

FIGURE 8. Average decisions in the alt-tax (left) and con-tax (right) experiments.

there appear to be large gaps between the average total donations for price-equivalent rebates and matches in both follow-up experiments, mirroring the gaps observed in the *third-party* experiment. Tables A.8 and A.11 confirm that the gaps between total donations for rebates and matches are large and highly statistically significant for each budget set in the *alt-tax* and *con-tax* experiments. Thus, the taxation language appears to have little effect on behavior. Instead, changes in behavior appear to be mostly driven by the change in budget sets across experiments.

To provide further evidence, we also compare donation behavior in these experiments to our main experiments. Average donations in the *alt-tax* and *con-tax* experiments are quantitatively very similar to those in the *third-party* experiment. Table A.9 (Table A.12) compares the average donations in the *third-party* and *alt-tax* (*con-tax*) experiments for each budget set. There are no statistically significant differences between the two experiments for any of the budget sets. A comparison of the average donations in the *alt-tax* (*con-tax*) and *tax* experiments, on the other hand, shows significant differences between the two experiments. Table A.10 (Table A.13) compares the average donations in each experiment by budget. While average donations under match subsidies appear to be very similar for the two experiments, each of the rebate subsidies result in significantly different average donation amounts. This suggests our main results are indeed driven by removing the budget constraint present under rebates in the *third-party* experiment, rather than by any behavioral effect created by the use of tax language.

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

We conclude that the tax language used in the *tax* experiment does not significantly affect subjects' behavior, and therefore cannot explain the disparities between the *third-party* and *tax* experiments. Although previous research has found that framing decisions in the context of taxation can significantly alter subjects' behavior (see, e.g., Eckel et al. (2005)), the results of our follow-up experiments demonstrate that subjects in our setting are not simply responding to the mention of taxation.

5. Discussion

Recall that in our *tax* experiment matches continue to perform significantly better than rebates, even if only by a small margin (see Table A.5). At the same time, Table 3 reveals no significant differences in the price elasticities for rebates and matches, either in magnitude or statistical significance. Combined, these two results are inconsistent with existing models of charitable giving. Traditional theories based solely on pure altruism fail to account for the differing impact of matches on donations, whereas warm glow theories, including Andreoni (1989)'s impure altruism model and Hungerman and Ottoni-Wilhelm (2021)'s impure impact model, can explain the superiority of matches over rebates but require distinct price elasticities. In other words, none of the existing theories can explain these findings simultaneously. In this section, we propose a straightforward extension to the impure altruism and impure impact models that maintains the warm-glow motive while also achieving parity in price elasticities across subsidies, albeit at the expense of equalizing donation levels under both subsidies.

In the charitable giving literature, warm glow is an accepted feature of individuals' decision-making process. The existence of warm glow has been repeatedly confirmed, and

there are strong theoretical justifications for its inclusion in models of giving (Andreoni and Payne, 2013).[20] The impure altruism model—which combines warm-glow giving with altruistic giving—was first introduced by Andreoni (1989, 1990). However, warm glow was not originally defined within the context of subsidies for giving, and because of this, the impure altruism model is silent with respect to how warm glow should be affected by such subsidies.

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

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

[20] 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).

The reason why assuming individuals feel warm glow for their out-of-pocket donation creates a disparity between rebates and matches is that their out-of-pocket donation actually represents a different choice variable depending on the type of subsidy being provided (see Section 2). Under a rebate, a donor's out-of-pocket donation is the same as their *gross* donation, g_g (i.e., the donor's *impact*/the total amount received by the charity, *including* any subsidized funds). Under a match, a donor's out-of-pocket donation is the same as their *net* donation, g_n (i.e., the donor's personal cost/the amount received by the charity *excluding* any subsidized funds).

To assume that donors receive warm glow based on their out-of-pocket donation is therefore equivalent to assuming that they fail to account for the effects of the subsidies—instead, they simply focus on their out-of-pocket donation. This assumption may or may not be true, but it seems inconsistent to make this assumption for the warm-glow portion of utility while simultaneously not making this assumption for other portions of utility, e.g., the private consumption portion of utility.[21] Furthermore, this assumption implies donors consider price-equivalent rebates and matches to be different—and therefore predicts that rebate- and match-price elasticities will in general not be equal—which directly contradicts our results.

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

[21] That is, if we are going to assume that donors fail to account for the effects of subsidies for warm glow, then to be consistent we should also assume they fail to account for the effects that subsidies have on the cost of giving. For example, using the impure impact model of giving outlined in Section 2, a donor's utility given a rebate at rate s_r should be given by $U(w - g_g,\ g_g,\ g_g + \lambda R_{-i})$ rather than $U(w - (1 - s_r)g_g,\ g_g,\ g_g + \lambda R_{-i})$.

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

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

$$(2) \qquad \max_{g_n \in [0,w]} U\Big(w - g_n,\ (1+\delta_m s_m)g_n,\ (1+s_m)g_n + \lambda R_{-i}\Big).$$

Rewriting (2) in terms of t (using $s_m = \frac{t}{1-t}$) gives

$$(3) \qquad \max_{g_n \in [0,w]} U\left(w - g_n,\ \frac{[1-(1-\delta_m)t]g_n}{1-t},\ \frac{g_n}{1-t} + \lambda R_{-i}\right),$$

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

$$(4) \qquad \max_{g_n \in [0,w]} U\left(w - g_n,\ \frac{[1-(1-\delta_r)t]g_n}{1-t},\ \frac{g_n}{1-t} + \lambda R_{-i}\right).$$

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

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.[22]

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

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 both field studies (Eckel and Grossman, 2008, 2017; Sasaki et al., 2022) and additional laboratory studies (Davis et al., 2005; Davis and Millner, 2005; Eckel and Grossman, 2006a,b, 2008).[23] 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.[24] However, while this model is able to explain the disparity between rebates and matches in the context of charitable giving, it is unable to explain why some papers also find similar gaps in price elasticities when consumption goods or investment decisions are studied instead of charitable goods (Davis et al., 2005; Davis and Millner, 2005). The fact that the gap between rebates and matches has been replicated in other settings suggests it is not the result of some behavioral effect unique to charitable giving, implying that theoretical explanations based on warm glow may be misguided. One factor that *is* consistent across all of the experimental studies finding a gap between rebates and matches, however, is the experimental design used.

Our main contribution to the literature comparing rebates and matches is to show the extent to which the gap between rebates and matches observed in previous studies may

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

[24]To clarify, this model is not necessary to explain the gap—if an individual receives *any* amount of warm glow utility (e.g., if they have *pure warm-glow* utility or *impure altruism* utility), theory predicts they will respond differently to rebates and matches. This was demonstrated by Turk et al. (2007) using an additively separable utility function. Our discussion in Section 2 demonstrates this result extends to general utility forms.

be driven by the experimental design used. We show theoretically in Section 2 that the third-party framework used in previous studies imposes a constraint when subsidies are implemented via rebates. By designing a novel experiment using a taxation framework, we are able to produce equivalent budget sets for price-equivalent rebates and matches. We then use a controlled virtual laboratory setting—which provides enhanced control beyond what is feasible in field settings—to test the equivalence of rebates and matches. We find that donations are higher for matches than for rebates, but the gap between subsidies is now substantially smaller than in previous studies. More importantly, there is no longer any gap between the price elasticities for rebates and matches, suggesting that the assumption implicitly accepted in previous research—that rebates and matches affect warm glow differently—is likely not correct (when all confounding factors are controlled for).

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

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

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

Rather than expanding subjects' choices under rebates, Blumenthal et al. (2012) take the opposite approach, choosing to instead *restrict* subjects' choices under matches to

be equal to their (constrained) choices under rebates, and continue to find large gaps in donation behavior. While this approach equalizes budget sets, constraining subjects' choices under both rebates *and* matches leads to biased estimates of *both* elasticities.[25] 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.[26] While the findings of their paper are entirely consistent with ours, it is not clear what drives their results. In their setting, since subjects are choosing total donations including subsidies (if any), there is no perceivable difference between rebates and matches, which might artificially send a signal to subjects to behave the same between the two different subsidy types. In addition, Davis attributes their results to the *isolation effect*, and does not discuss the removal of the rebate constraint as a possible explanation. The isolation effect posits that individuals isolate their attention on the amount they are tasked to allocate (i.e., their endowment), ignoring the effects of any subsidies on the final allocation. 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.

[25]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.

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

While 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.

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

We begin by discussing our match-price elasticity estimate (of –1.108). While there are many studies estimating the match-price elasticity of giving in laboratory and field settings, we are not aware of any studies estimating the match-price elasticity of giving within a tax framework.[27] Instead, previous studies consider matches that are provided by a third-party donor. That being said, we have only argued that estimation of the *rebate*-price elasticity is biased by the use of the third-party framework. Thus, previous studies should still provide unbiased estimates of the *match*-price elasticity. And, indeed, our estimate is consonant with previous estimates produced in both lab and field settings,

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

which are almost always a little larger than 1 in absolute value (Eckel and Grossman, 2003, 2008; Karlan and List, 2007; Karlan et al., 2011; Huck and Rasul, 2011).

Placing our rebate-price elasticity estimate (of –1.145) within the literature is more difficult. Rebate-price elasticity estimates based on laboratory experiments using the third-party framework are biased, and no previous experiments have used a tax framework. The bias induced by the constraint under rebates in the third-party framework may be much less prevalent in field settings where donors are much less likely to provide donations anywhere near the constraint, but third-party rebates in the field are typically associated with significant time-delays (and, possibly, with uncertainty) that are not present in our laboratory setting. Because of this, empirical studies based on tax data may be a better comparison. While there is also a time-delay in receiving a rebate provided via the tax system, donors may be more confident that they will actually receive the rebate. Furthermore, empirical studies based on the tax system closely match the framework used in our experiment.

Empirical studies using tax data face difficult identification challenges, and their estimates are sensitive to the quality of data and empirical methods used. Unsurprisingly, they have produced a wide range of rebate-price elasticity estimates. That being said, our estimate of –1.145 is consistent with a large body of empirical work. Andreoni (2006) notes that studies using cross sectional survey data from tax returns typically estimate the rebate-price elasticity to be between –1.1 and –1.3. Auten et al. (1992) and Auten et al. (2002) use panel data covering multiple tax reforms—which might produce more believable exogenous variation in prices—and estimate the rebate-price elasticity to be –1.11 and –1.26, respectively. In a similar study, Tiehen (2001) estimates the rebate-price elasticity to be between –0.9 and –1.1. In a meta-analysis of empirical studies based on tax data, Peloza and Steel (2005) find an average rebate-price elasticity of –1.1 after excluding outliers. And, in a more recent overview of the research, Bakija and Heim (2011) conclude that the tax-price (rebate-price) elasticity of giving is likely less than –1 (i.e., elastic).

Both our rebate- and match-price elasticity estimates are consistent with previous research, providing support for our experimental design. Our experiment is also the first incentive-compatible study to compare rebate and match subsidies that are both provided

via a tax system.[28] As discussed, all previous experimental studies consider rebates and matches that are provided by a third party.[29] In the case of empirical studies, data limitations typically prevent the comparison of rebates and matches. The only empirical study we are aware of which compares rebates and matches is Hungerman and Ottoni-Wilhelm (2021), but in their setting matches are provided by a third-party donor rather than the tax system. As such, our elasticity estimates may also be more informative for tax policy than previous studies.

7. Conclusion

This paper challenges the well-established result among existing experimental studies that donations are significantly more responsive to price changes implemented via matches versus price changes implemented via rebates. We show theoretically that the third-party framework used in previous experiments creates a discrepancy between budget sets for price-equivalent rebates and matches, and we argue that this discrepancy may significantly bias the comparison of price elasticity estimates reported in previous studies. To resolve this issue, we design a novel experiment which equates the budget sets for price-equivalent rebates and matches. We then use this novel experimental design to provide an unbiased comparison of price elasticity estimates.

The results of our main experiment confirm our suspicions—we find no statistical difference between the estimated rebate- and match-price elasticities of giving, suggesting previous studies' results are indeed significantly biased. To verify our results are not an artefact of our experimental setting (including any potential characteristics of our subject pool that are unique to our study), we also run an experiment replicating the third-party design used in previous studies. The results of our replication experiment are consistent with previous studies. Importantly, consistent with previous studies, we find

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

[29]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.

a large and statistically significant difference between the estimated rebate- and match-price elasticities. Thus, we are able to conclude that our results are not an artefact of our experimental setting. Moreover, we run two additional experiments to investigate the mechanisms behind our main finding. Our results provide further evidence that the parity in price elasticities is driven by our experimental design's ability to remove the disparity in budget sets, rather than possible behavioral differences created by the use of a taxation frame.

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

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

rebates and matches which are both provided within a tax framework, our study makes an important contribution to the literature on tax policy.

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

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Appendix A. Additional Tables

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

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

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

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

TABLE A.2. Summary demographic data for each experiment

	Net Income	Price	Total Donation rebate	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	34.8	54.38	.000
Std. err.			2.19	3.96	
N			147	147	
Mean	120	1	35.86		
Std. err.			2.69		
N			147		
Mean	120	.67	46.91	58.62	.000
Std. err.			3.08	4.10	
N			147	147	
Mean	120	.5	53.83	85.16	.000
Std. err.			3.42	6.09	
N			147	147	

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

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

Regression Results: random effects tobit maximum likelihood

Dependent variable=ln(total donation received by charity)			
Variable	(1) EG results Coefficient (standard error) [Elasticity]	(2) Third-party Coefficient (standard error) [Elasticity]	$H_0 : (1) = (2)$ (p-value) two-tailed†
Constant (β_0) (no subsidy)	-1.557** (.458)	-1.824 (.945)	NO (.799)
Constant ($\beta_0 + \beta_6$) (rebate subsidy)	-1.101** (.232)	-2.112** (.693)	NO (.170)
Constant ($\beta_0 + \beta_7$) (match subsidy)	-.987** (.273)	-3.000** (.687)	YES (.006)
Endowment (β_1) (no subsidy)	1.100** (.223) [1.030]	.934** (.202) [.851]	NO (.582)
Endowment ($\beta_1 + \beta_2$) (rebate subsidy)	.895** (.128) [.838]	1.026** (.143) [.935]	NO (.495)
Endowment ($\beta_1 + \beta_3$) (match subsidy)	.820** (.138) [.767]	1.190** (.142) [1.085]	NO (.062)
Rebate price (β_4)	-.364 (.187) [-.340]	-.786** (.202) [-.716]	NO (.134)
Match price (β_5)	-1.140** (.185) [-1.067]	-1.434** (.200) [-1.307]	NO (.280)
Subjects Observations	168 2016	147 1470	

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

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

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

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

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

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

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

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

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

† Two-tailed Welch's t-test.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Elasticity Estimates								
Dependent variable=ln(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)	.851 (.185)	.849 (.184)	.664 (.132)	.663 (.132)	.839 (.128)	.839 (.128)	.772 (.150)	.772 (.150)
Endowment (rebate subsidy)	.935 (.132)	.934 (.131)	.748 (.131)	.748 (.131)	.736 (.129)	.736 (.128)	.540 (.151)	.540 (.152)
Endowment (match subsidy)	1.085 (.131)	1.083 (.130)	.773 (.131)	.772 (.131)	.987 (.127)	.986 (.127)	.890 (.149)	.890 (.149)
Rebate price	-.716 (.185)	-.714 (.184)	-1.145 (.262)	-1.144 (.262)	-.781 (.255)	-.780 (.255)	-.594 (.300)	-.594 (.300)
Match price	-1.307 (.184)	-1.304 (.183)	-1.108 (.262)	-1.108 (.262)	-1.294 (.254)	-1.293 (.254)	-.992 (.297)	-.993 (.298)
Tax rate			-.256 (.321)	-.256 (.321)	-.086 (.311)	-.086 (.311)	-.079 (.363)	-.079 (.364)
Controls	N	Y	N	Y	N	Y	N	Y
Subjects	147		151		144		146	
Observations	1470		1812		1728		1752	

TABLE A.14. Comparison of Elasticity Estimates

	third-party	tax	alt-tax	con-tax
ln_charity_total				
ln_net_income	0.934***	0.685***	0.856***	0.808***
	(0.201)	(0.136)	(0.130)	(0.157)
rebate_x_net_income	0.092	0.087	-0.105	-0.243
	(0.247)	(0.192)	(0.185)	(0.223)
match_x_net_income	0.256	0.113	0.150	0.124
	(0.246)	(0.192)	(0.183)	(0.221)
tax_rate	0.000	-0.264	-0.087	-0.082
	(.)	(0.331)	(0.317)	(0.381)
rebate_x_price	-0.785***	-1.181***	-0.795***	-0.622**
	(0.202)	(0.271)	(0.260)	(0.314)
match_x_price	-1.434***	-1.144***	-1.318***	-1.038***
	(0.200)	(0.270)	(0.259)	(0.311)
rebate_dummy	-0.289	-0.651	0.423	1.248
	(1.140)	(0.894)	(0.860)	(1.037)
match_dummy	-1.177	-0.465	-0.670	-0.432
	(1.136)	(0.892)	(0.854)	(1.028)
age	-0.104	-0.032	0.043	0.023
	(0.093)	(0.069)	(0.038)	(0.045)
Female	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Male	-0.329	-0.177	0.515**	-0.116
	(0.392)	(0.284)	(0.239)	(0.283)
Other	0.506	1.881	0.518	10.883
	(1.406)	(1.150)	(0.937)	(129.265)
income_refuse	0.453	-1.849	1.935	2.713
	(3.154)	(2.376)	(1.970)	(2.304)
logINCOME	0.166	-0.103	0.185	0.269
	(0.272)	(0.204)	(0.175)	(0.206)
conservative	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
liberal	1.764*	0.141	0.282	0.589
	(1.001)	(0.446)	(0.604)	(0.755)
moderate	1.137	0.204	-0.002	0.364
	(1.037)	(0.478)	(0.610)	(0.769)
prefer not to say	1.960	-1.176*	0.576	-0.151
	(1.303)	(0.673)	(0.700)	(0.870)
unsure/undecided	1.377	-0.091	0.008	0.067
	(1.101)	(0.626)	(0.641)	(0.763)
important	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
not important	-0.220	0.670*	0.935***	-0.122
	(0.518)	(0.406)	(0.322)	(0.404)
somewhat important	-0.616	-0.172	0.614*	-0.327

Appendix B. Additional Figures

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

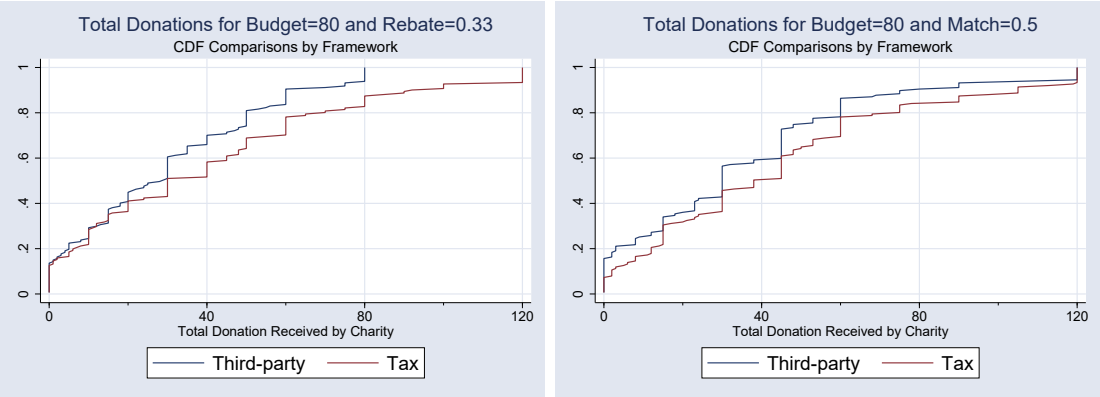

Figure B.1. CDFs of Donations for Rebate (left) and Match (right) when Price=.67 and Budget=80. Third-party in blue, Tax in red.

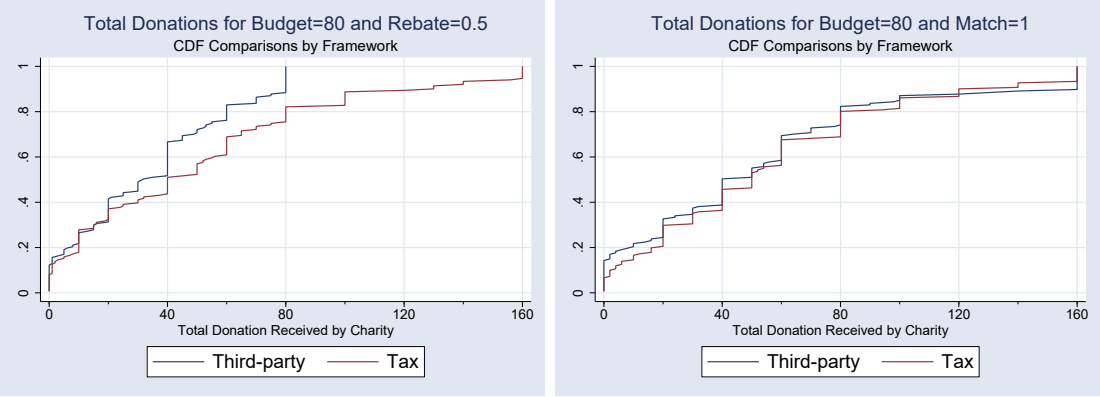

Figure B.2. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=80. Third-party in blue, Tax in red.

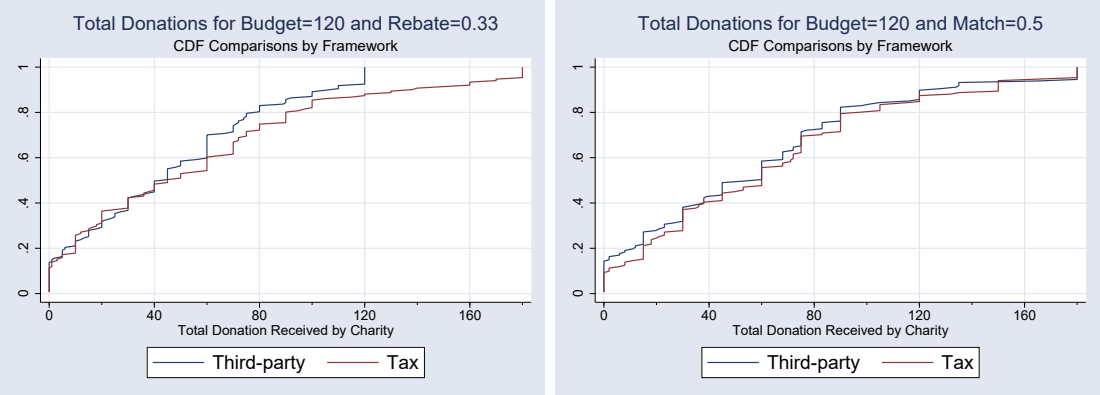

FIGURE B.3. CDFs of Donations for Rebate (left) and Match (right) when Price=.67 and Budget=120. Third-party in blue, Tax in red.

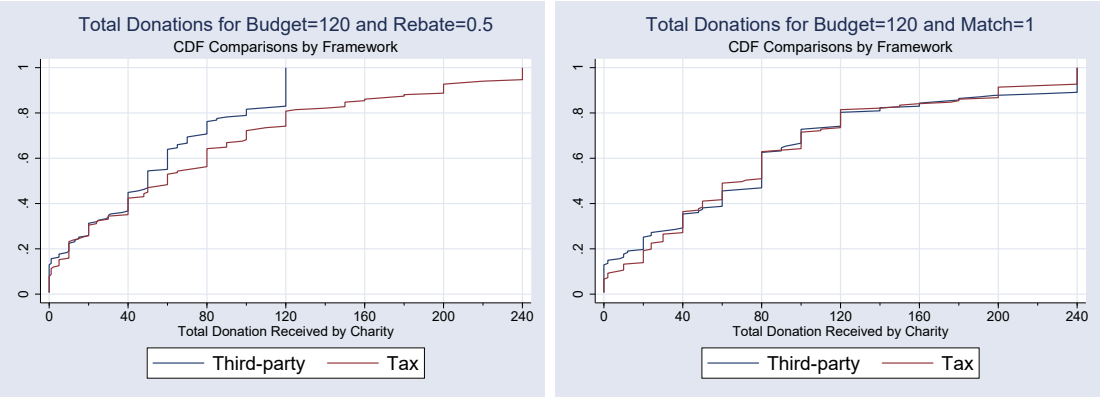

FIGURE B.4. CDFs of Donations for Rebate (left) and Match (right) when Price=.5 and Budget=120. Third-party in blue, Tax in red.

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

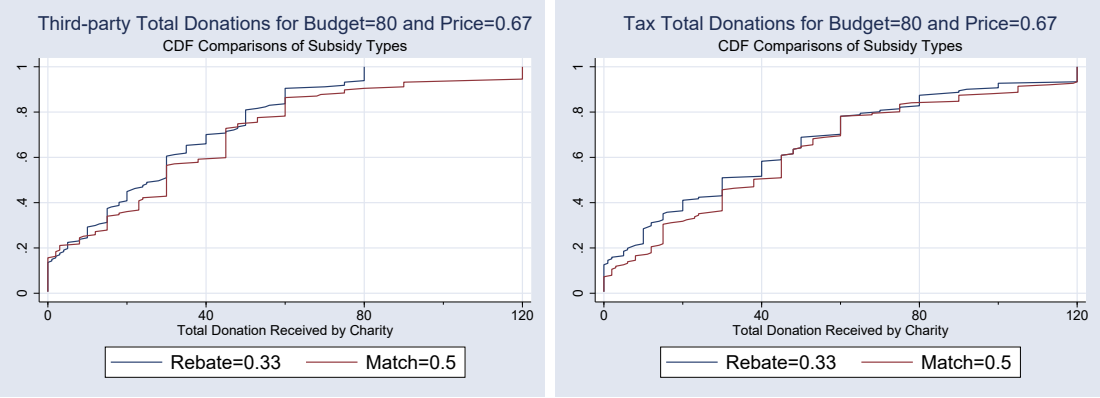

FIGURE B.5. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=80. Third-party on left, Tax on right.

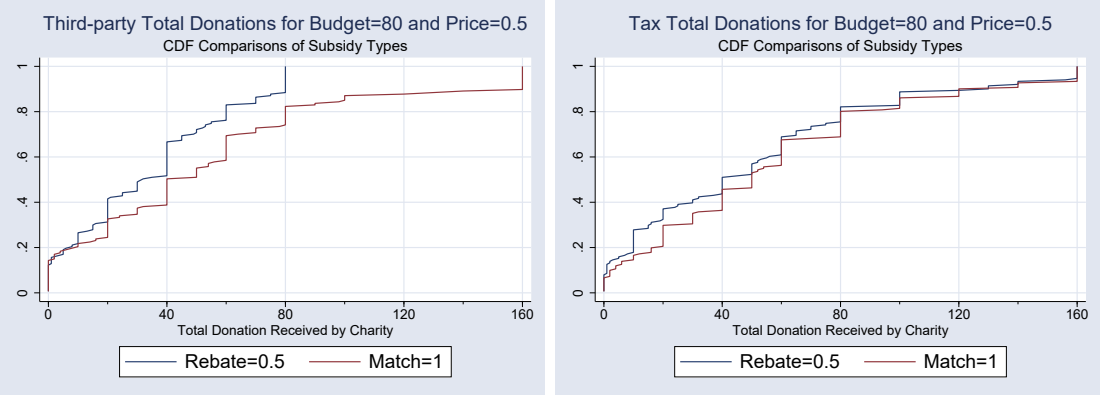

FIGURE B.6. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. Third-party on left, Tax on right.

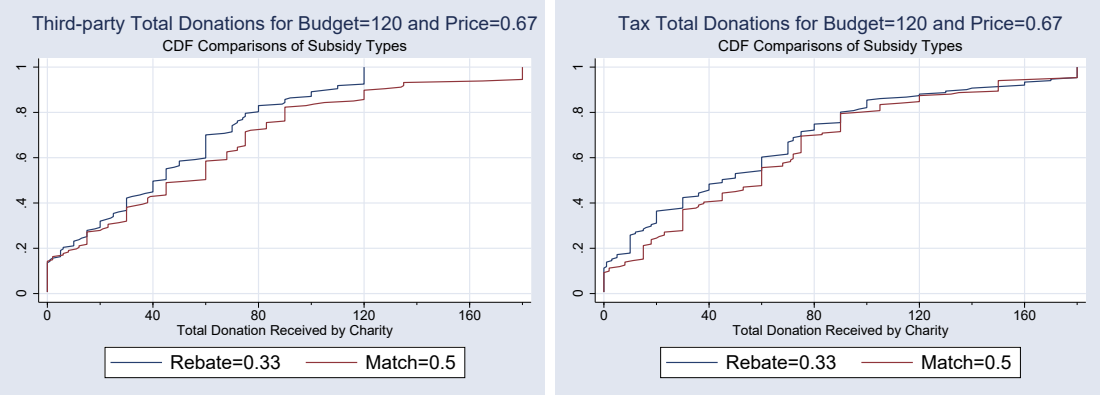

FIGURE B.7. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=120. Third-party on left, Tax on right.

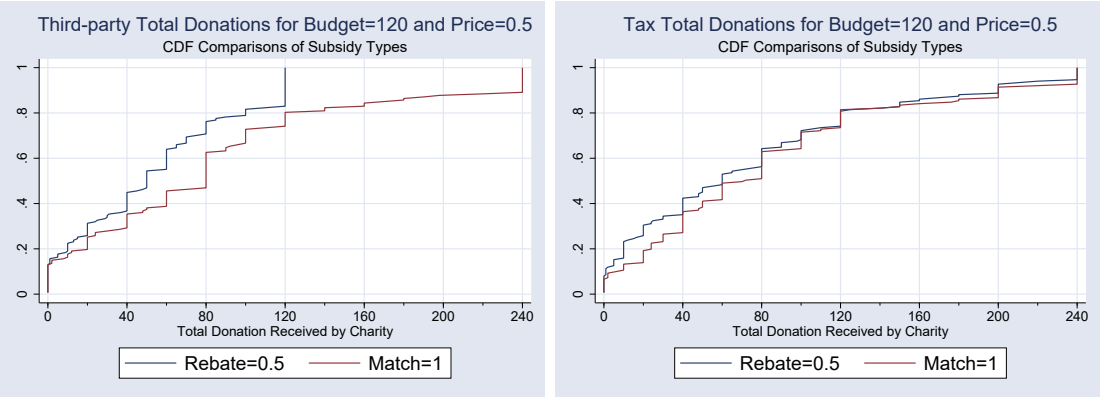

FIGURE B.8. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. Third-party on left, Tax on right.

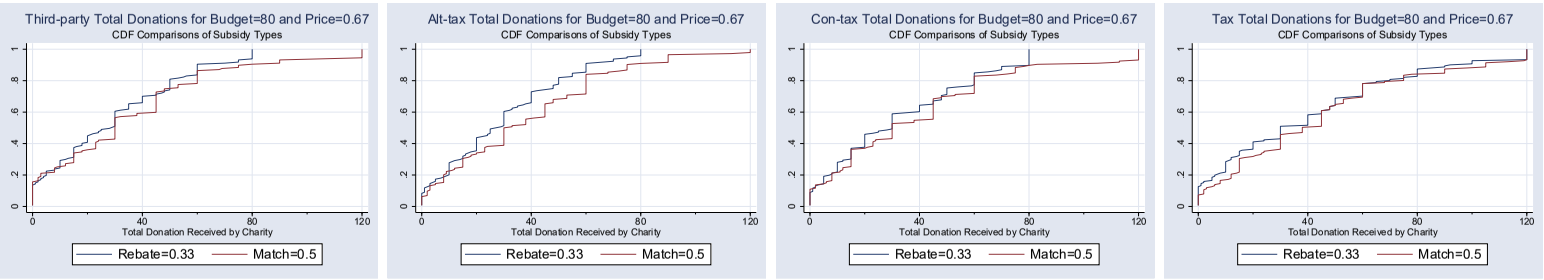

FIGURE B.9. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=80. From left to right: Third-party, Alt-tax, Con-tax, Tax.

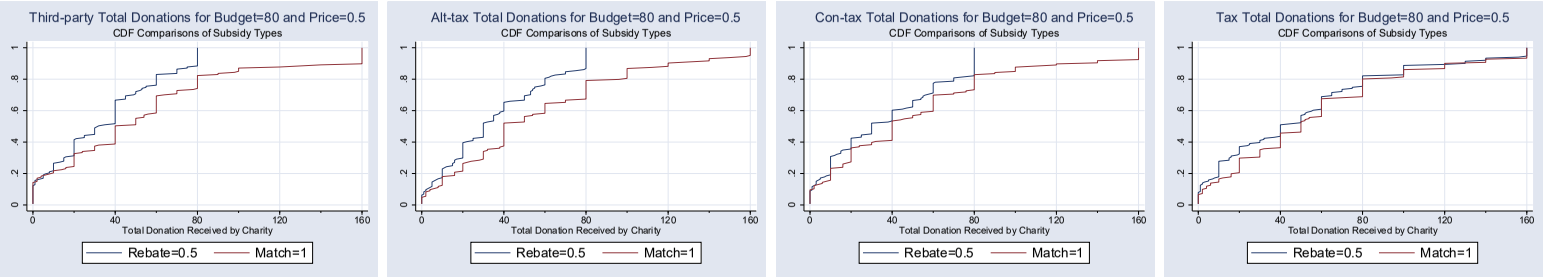

FIGURE B.10. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=80. From left to right: Third-party, Alt-tax, Con-tax, Tax.

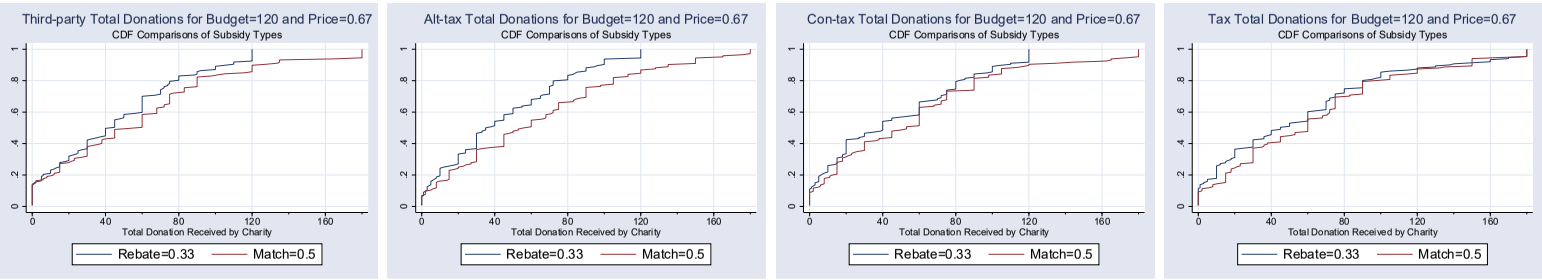

FIGURE B.11. CDFs of Donations for Rebate (blue) and Match (red) when Price=.67 and Budget=120. From left to right: Third-party, Alt-tax, Con-tax, Tax.

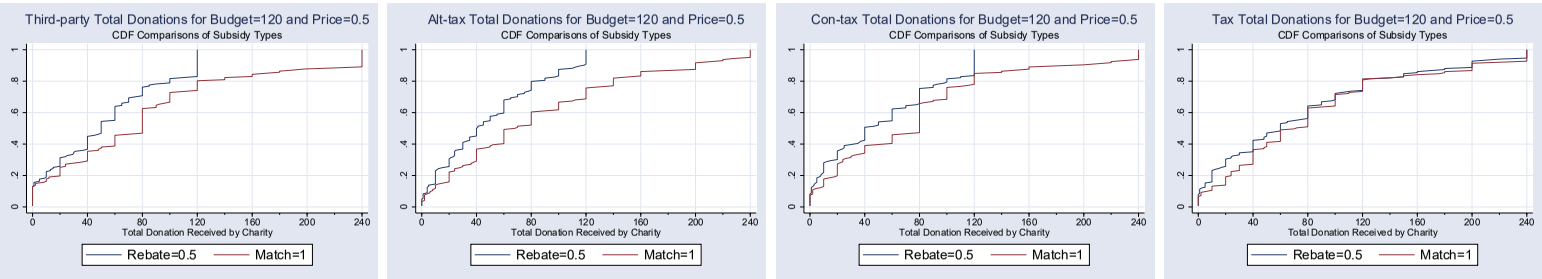

FIGURE B.12. CDFs of Donations for Rebate (blue) and Match (red) when Price=.5 and Budget=120. From left to right: Third-party, Alt-tax, Con-tax, Tax.

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

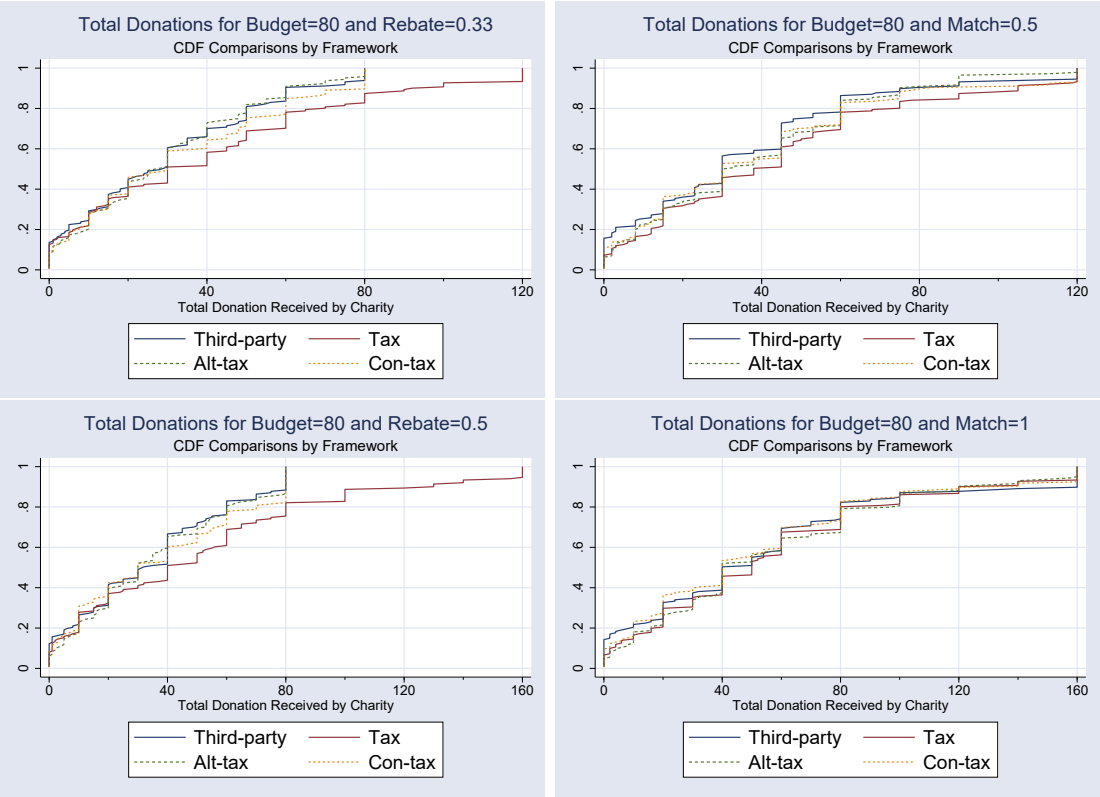

FIGURE B.13. CDFs of donations in all experiments for questions where Budget=80. Left-side graphs show rebate questions, right-side graphs show match questions. Top graphs show Price=0.67, bottom graphs show Price=0.5.

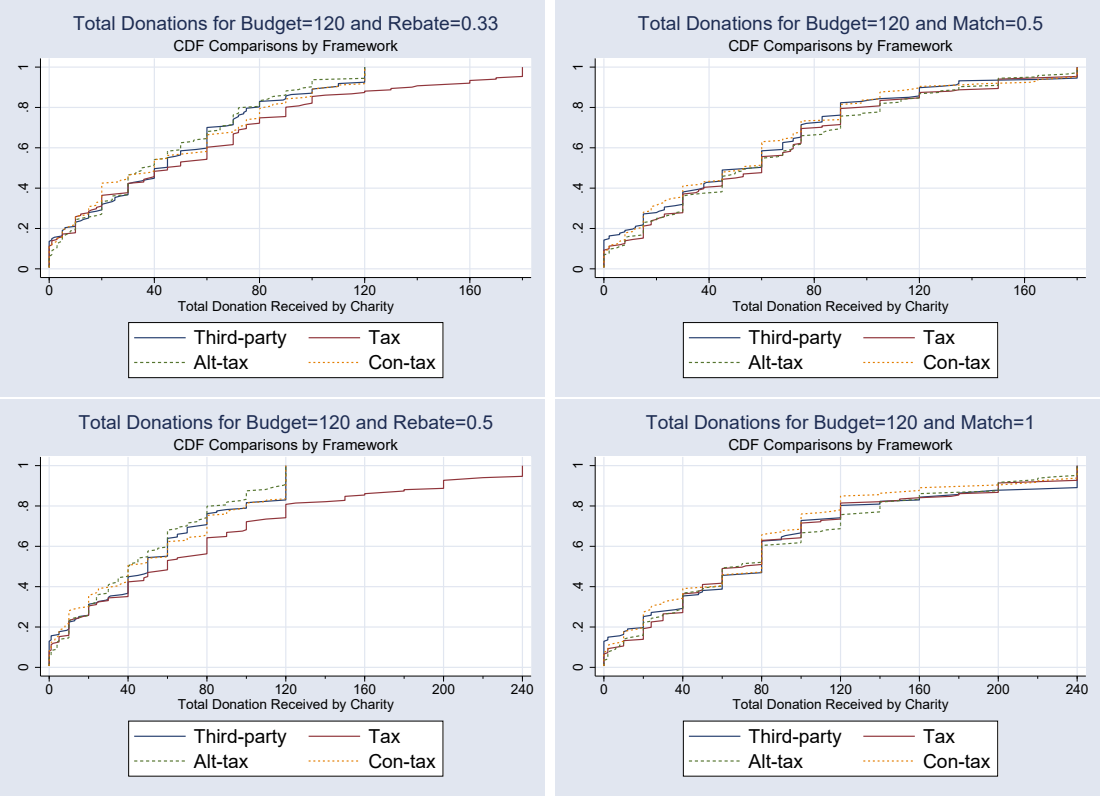

FIGURE B.14. CDFs of donations in all experiments for questions where Budget=120. Left-side graphs show rebate questions, right-side graphs show match questions. Top graphs show Price=0.67, bottom graphs show Price=0.5.

B.4. **Added Information Treatment of Davis et al. (2005).**

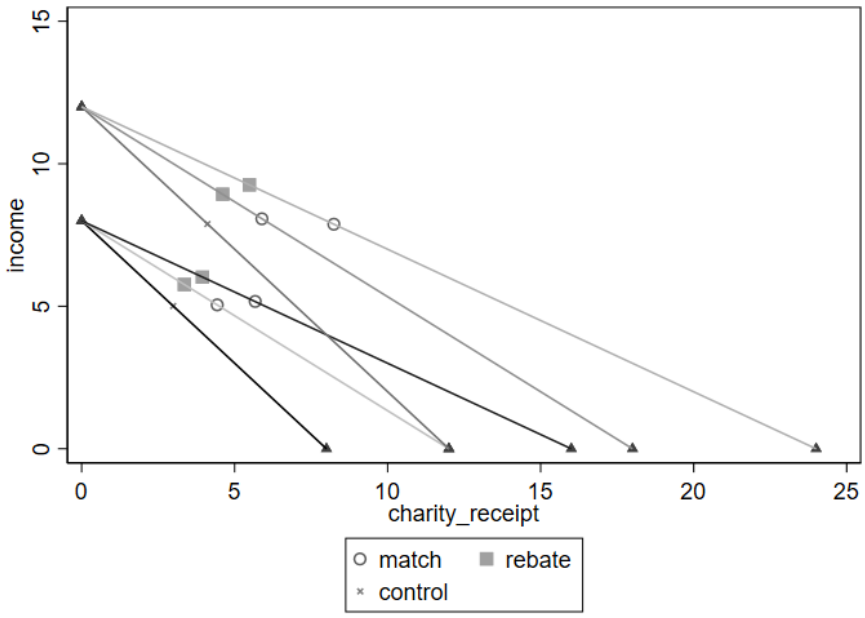

FIGURE B.15. Plot of Table 6 from Davis et al. (2005) – "added information" treatment.

Appendix C. Additional Analysis

C.1. **Censoring cannot explain our findings.** To test whether censoring can explain our results, we run the following combined model.

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

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

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

The results of the combined model are essentially identical to our previous results, with the exception that the rebate-price coefficient for the tax framework (β_4) is now estimated to be -1.269 ($p = 0.000$), whereas before it was estimated to be -1.181 ($p = 0.000$). Note that this change is a result of using the censored total donation amounts $\tilde{Y}_{ij}$ and has nothing to do with using a combined model.[30] The full results of the combined model are presented in Table C.1.

We see that censoring observations in the tax experiment in the same way that they are censored in the third-party experiment does not remove the disparity in responses to rebates and matches. While the coefficients on the rebate price and match price in the third-party framework (α_4 and α_5, respectively) are statistically different ($p = 0.026$), in the tax framework they are still not statistically different ($p = 0.658$).[31] In fact, using

[30]Given that the tax framework indicator D_{ij} is interacted with every variable, the combined model produces essentially identical results as estimating separate models for each framework (third-party and tax). The small differences occur because the combined model considers the third-party observations to have a tax rate of 0, providing the model with slightly more information about subjects' response to a change in the tax rate. The full results of the combined model using the unconstrained total donations are provided in Table C.2.

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

the censored observations, $\tilde{Y}_{ij}$, actually *increases* the disparity in behavior between the two frameworks, causing the rebate-price estimate to be significantly *smaller* (i.e., more negative) than the match-price estimate in the tax experiment, whereas in the third-party experiment the rebate-price estimate is statistically significantly larger than the match-price estimate.

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

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

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

To further demonstrate the significance of the difference in observed behavior between the third-party and tax experiments, we construct a Fisher Exact P-value for the probability of observing a gap between gaps as large as the one observed in our data. Under the sharp null hypothesis that assignment to the tax experiment has no effect on individuals' responses to rebates and matches (other than the mechanical effect of relaxing the constraint under rebates), we can estimate a Fisher Exact P-value for the probability of observing a γ as large as 0.766. We do this in the following way: (1) we censor any donation observations in the tax experiment that *would have been* censored in the third-party experiment (i.e., we use $\tilde{Y}_{ij}$); (2) we randomly assign individuals to either the third-party or the tax experiment; (3) based on the random assignment, we estimate the combined model given in Equation 1; (4) using the coefficient estimates from the combined model, we then calculate γ_n for each iteration n.

Using a total of 100,000 random assignments, we estimate a Fisher Exact P-value for the probability of observing a $\gamma > 0.766$ of $p = 0.0163$. (The two-sided p-value for the

probability of observing a γ *as extreme* as 0.766 is equal to $p = 0.0316$.) Based on these results, we reject the null hypothesis that donations are only mechanically affected by the constraint under rebates. Behavior is significantly different between the third-party and tax experiments. This supports that individuals have a significant *behavioral* response to the constraint under rebates in the third-party experiment.

Regression Results: random effects tobit maximum likelihood

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

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

TABLE C.1. Combined (third-party and tax experiment) results using censored donation amounts

Regression Results: random effects tobit maximum likelihood

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

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

TABLE C.2. Combined (third-party and tax experiment) results using uncensored 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. Subjects were required to enter a number in the 'Pass' column (i.e., Column 4) for each problem. After entering a value into Column 4, the remaining columns (i.e., Columns 5-7) automatically fill with the correct values based on the subject's choice of how much to pass. Subjects are unable to edit the information in Columns 5-7 (except by editing the value in Column 4). Subjects may answer the decision problems in any order they like, and they are able to edit their choices up until the time they submit their responses. While subjects can enter any number they wish into Column 4 (including negative numbers and values in excess of their endowment for the problem), and Columns 5-7 will be calculated and presented to them, they will be unable to submit their responses if any of them violate their budget constraints. The subject is informed of any unacceptable responses and asked to edit them. Non-numerical responses are not possible (they are immediately edited to be blank), and subjects are required to provide a response for each problem (i.e., they may not leave any problems blank).

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

ALLOCATION DECISION PROBLEMS

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

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

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

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.

FIGURE D.2. Screenshot of an Example Decision Sheet for the Tax Experiment in Qualtrics

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%.				

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.*

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

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.				