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Subsidizing Quantity Donations: Matches, Rebates, and Discounts Compared*

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Abstract

We present online-experimental evidence that challenges the generalizability of established results on subsidizing giving by considering a ‘quantity donation’ scheme. We define this scheme as one in which donors choose how many units of a charitable good to fund, rather than the amount of money to give. We find that different subsidy types are equally effective in raising funds. This contrasts with the common result of matches being superior to rebates. The finding masks a higher likelihood of giving under rebates and larger donations under matches and discounts. Our results emphasize the role of small changes in the donation environment.

JEL Classifications: C93, D12, D64, H24, H41, L31

Keywords: Charitable giving, subsidies, online field experiment, framing

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Subsidies are often applied to incentivize charitable giving, with the two prevalent types being rebates and matches. In the case of a rebate subsidy, a third party refunds a fraction of the donation back to the donor, whereas in the case of a matching subsidy, the third party supplements the donation at a given rate. Both have been extensively studied in settings in which individuals decide how much money to give to a charity. Such a decision environment represents the most common form of solicitations and is captured by classic donation models, which typically assume a linear production technology for the charitable public good and normalize the per-unit prices of both private and public goods to one (Bergstrom et al., 1986; Andreoni, 1988, 1989). As a result, the prospective donor i 's choice is to divide her endowment w_i (in dollars) between private consumption x_i (in dollars) and giving g_i (in dollars) to the charitable good, G . In the following, we refer to the donor's choice of g_i in a setup in which the choice variable is money given as an "expenditure donation".

Despite the popularity of the expenditure donation scheme, important campaigns depart from its paradigm by framing the donor's choice variable in terms of physical units of a charitable good to fund. A prominent example that has attracted about one million donors from all over the world is *ShareTheMeal*, a smartphone app and initiative of the UN World Food Programme which is used to provide food to children in need. Donors for ShareTheMeal do not directly choose an amount of money to give. Instead, they are informed that feeding one child for a day costs \$0.50 and subsequently indicate the number of days with food they would like to fund.¹ This class of fundraising is also popular among development aid agencies who heavily rely on child sponsorships in their fundraising. Here, the monthly donation for the sponsorship is a

¹Similar food provision campaigns are the "100 Thousand Meals" appeal of the Salvation Army Australia or the "Help with €2" campaign of Misereor, the German Catholic Bishops Organisation for Development Cooperation.

fixed amount set by the charity—usually around \$35—and prospective donors choose the number of children to sponsor rather than the amount of money to donate. Other examples are fundraising drives for biodiversity conservation or reforestation programs, in which donors indicate the number of acres or trees to fund.² In these examples, the price of a unit of the charitable good G is no longer implicit. Instead, the fundraiser states an explicit price p and asks how many discrete units g_i the individual would like to fund. In this respect, the setting resembles early models of the private provision of public goods that include prices (e.g., Warr, 1983). In the following, we refer to the donor’s choice of g_i in a setup in which the choice variable is the quantity of the charitable good as a “quantity donation”.

Although under both schemes donors eventually provide money, quantity donations are not equivalent to expenditure donations. First, donors’ choice sets differ. For quantity donations, the units of the charitable good to be provided are typically indivisible, which introduces an element of discreteness that is largely absent in the virtually continuous expenditure donations. Second, the information provided to prospective donors differs. By stating the per-unit price of the charitable good, quantity donation schemes necessarily disclose the upper bound of the charity’s marginal cost of production, whereas expenditure donation schemes typically provide little information on the cost structure of producing the charitable good. While information on the share of fundraising and overhead costs is increasingly available to donors (Ribar and Wilhelm, 2002; Meer, 2014), the absence of information on the output impact of a contribution to a charity creates a source of considerable perception bias (Bekkers and Wiepking, 2011). Third, the framing of the choice differs. By asking for the number of physical units of the charitable good, quantity donations are likely to

²For instance, in the *Monarch Butterfly Habitat Exchange* program of the Environmental Defense Fund, donors sponsor acres of milkweed habitat for \$35 per acre. In the *Plant A Tree* program of the Jewish National Fund, donors choose the number of trees to be planted at \$18 a tree.

emphasize how a donation generates specific and concrete outcomes for recipients. The literature on charitable giving and other social dilemmas has provided numerous examples of how small changes in the decision environment can affect contributions (see e.g. Andreoni, 1995; Van Dijk and Wilke, 2000; Das et al., 2008; Chang and Lee, 2009; Dufwenberg et al., 2011; Cao, 2016).

Given those differences, it is an open question whether well-established results in the literature on expenditure donations hold for quantity donations. In this paper, we report results from an online field experiment designed to revisit the effectiveness of subsidy schemes for charitable donations in the case of quantity donations. Several key insights have emerged in the literature on rebates and matches applied to expenditure donations (see also Vesterlund, 2016, for a comprehensive review). While rebates and matches imply the same price of giving if the corresponding subsidy rates $1:m$ and r satisfy $r = \frac{1}{m+1}$, a robust finding is that overall donations received by the charity are higher and more price elastic under matches than under equivalent rebates (Eckel and Grossman, 2003, 2006a,b, 2008b, 2017; Davis et al., 2005; Lukas et al., 2010; Bekkers, 2015). Nevertheless, using a matching subsidy does not necessarily pay off. While Karlan and List (2007), Huck et al. (2015) and Eckel and Grossman (2017) find that a matching subsidy increases private contributions net of the subsidy compared to a no subsidy condition, others find no such crowding-in effect on the aggregate level (Karlan et al., 2011). At the same time, private contributions appear to be insensitive to higher subsidy rates (Karlan and List, 2007; Lukas et al., 2010; Bekkers, 2015).

The main reason why results on the relative effectiveness of subsidies may not generalize to quantity donations is that rebates and matches are no longer theoretically equivalent. Within a quantity donation scheme, rebates are refunded money while matches consist of added units. The smallest positive donation is to fund one unit of the charitable good. Given a unit price p , this

implies a minimum expense of p required under the match. In contrast, the rebate provides a refund on the donation given and, at subsidy rate of r , the costs of becoming a donor are $p(1-r) < p$. As a result, the rebate is potentially more effective in attracting donors. In addition, for $m > 1$, not every additional unit funded by the individual induces an additional matching payment since the matching payment is framed in discrete physical units. For example, at a matching rate of 1:2 only every second unit funded induces an additional matching payment of one unit. In contrast, under a rebate any increase in donations increases the subsidy payment, as is the case for both subsidy types under expenditure donations.

While we are not aware of any previous study that compares rebates and matches in a quantity donation setting, some important papers share design elements with a quantity donation scheme. Meier (2007) and Gneezy et al. (2014) for example feature a discrete choice set, but do not frame donations in physical quantities and focus on matches only. Results match those of the other expenditure donation literature. The paper closest to ours is Kesternich et al. (2016) who compare rebates and matches and who feature a discrete decision on a quantity to be provided. Their findings confirm the superiority of matches on provision and higher match price elasticities. However, the fact that their field experiment involves a binary decision on an impure public good for which the size of giving is tied to the private cobenefit makes it difficult to relate it to our setup. A few other studies implicitly employ an experimental design soliciting quantity donations to charitable or public goods (e.g. Diederich and Goeschl, 2017) but do not investigate rebate and matching subsidies.

Since for quantity donations the price per unit is explicit, price salience increases which in turn might make donations more price sensitive. However, donors still need to infer effective prices themselves when confronted with rebates or matches. Therefore, we test a third subsidy type that shares the the-

oretical features of the rebate but makes the effective price explicit: price discounts. In the case of private goods, price discounts have been investigated in the marketing literature (see e.g. Mishra and Mishra, 2011; Chen et al., 2012), in which they are framed in terms of the percentage price reduction with or without explicit information about the effective price, and have been compared to rebates and matches in an experimental study (Davis and Millner, 2005). A setting of quantity donations, in which each physical unit of the charitable good is associated with a well defined price, allows us to novelly apply this subsidy type to charitable giving. For a discount of rate d , subjects are explicitly confronted with the effective price $p(1 - d) < p$, complemented with information about how the price comes about.

The contribution of this paper is threefold. First, we define quantity donations as a separate class of charitable donations distinct from expenditure donations. Second, we investigate how rebates and matches perform in a setting of quantity donations and compare the results to the established literature on expenditure donations. Third, we check whether the discount subsidy offers an attractive alternative to these subsidy types in a setting of quantity donations.

In our online field experiment, subjects receive a monetary reward for answering an unrelated questionnaire and decide whether and how to use this money to donate to a predetermined charity. Donations are framed in quantities of nutritional packages provided for malnourished children, with an unsubsidized price of \$0.50 per package. Treatments differ by whether a subsidy is offered, the subsidy type (match, rebate, or discount) and the subsidy rate (1:2 and 1:1 for match and 33% and 50% for rebate and discount). We conduct both a between- and a within-subject design.

Our main results originate from the between-subject design and are as follows. All three subsidy types are equally effective in incentivizing private net

donations and generating total charity receipts. Hence, quantity donations do not replicate the superiority of the match observed for expenditure donations. However, the effectiveness of subsidies differs when disaggregating into the extensive and intensive margins of giving: Rebates are more effective in attracting donors whereas matches result in larger donations. Surprisingly, under the discount, the likelihood to give is significantly lower than under the rebate, and on both margins, behavior corresponds to that under the match.

1 Experimental design

1.1 Donation decision

We adapt the real-donation dictator game introduced by Eckel and Grossman (1996) and subsequently applied to compare subsidy types (Eckel and Grossman, 2003; Davis and Millner, 2005; Davis, 2006; Eckel and Grossman, 2006a,b). In the standard version of the game, subjects decide how much of their endowment to hold and how much to pass to a charity. This choice corresponds to an expenditure donation. In our variant of the game, subjects decide how many units of the charitable good to fund at a given price, using their endowment. This choice corresponds to a quantity donation.

Our variant of the game requires a charitable good or service that is easily quantifiable. We approached a relief organization, Sign of Hope, which makes extensive use of quantity donation calls in fundraising campaigns. Among their activities, we chose the treatment of malnourished children in a certain area of South Sudan as this service offered practical units and prices for our experiment. The children are treated in two “bush clinics” operated by the relief organization. Treating one child for one month using a special nutritional paste and high energy cookies requires a donation of \$15. We divided this number into practical units of nutritional packages per child and day which implies a price

(required donation) of \$0.50 per package. In the donation task of the online experiment, subjects were introduced to the charity, the charitable good, and its marginal provision cost to the charity.³

Table 1 shows the seven treatment conditions. In the control condition, no subsidy was applied. Subjects were endowed with \$2 and chose how many units of the charitable good to fund at a price of \$0.50. The remaining six treatment conditions were derived by applying the three subsidy types at two different levels each. We framed the rebate conditions as a 50% (33%) rebate, so that \$0.25 (\$0.17) per unit provided would be added to the final reward. Match conditions were framed as a match of each (every two) unit(s) the subject provides at no additional costs. Discount conditions were framed as a possibility of providing units for \$0.25 (\$0.33) apiece. For all subsidy types, subjects were informed that the subsidy is provided by “a third party”, a truthful but indefinite reference to the research budget involved. Subjects facing the discount subsidy learned that the reduced price results from a third party funding the remaining cost of \$0.25 (\$0.17) per package. The two subsidy levels imply effective prices of \$0.33 and \$0.25. Note however that for the 1:2 match, the effective price per package is not constant, since only every second unit provided by the subject induces an additional subsidy payment.

Treatment conditions were administered in both a between-subjects (BS) and a within-subjects (WS) design to two different subject samples. In the BS design, subjects were introduced to a specific subsidy condition or the control and had to choose the desired number of units from a drop-down menu. In the WS design, all seven conditions were listed in random order and subjects

³Since U.S. subjects are likely to be unfamiliar with the German organization Sign of Hope, instructions stated that the charity had won the 2010 Transparency Award for German non-profit organizations. Additionally, subjects were provided with a link to the English web page of the charity’s section on relief efforts in Africa. In the interest of full disclosure, we also provided information on the charity’s share of overhead costs which is 20%. Hence, subjects might infer that actual provision cost per package amounts to \$0.40 while the remaining \$0.10 is used to run the organization.

entered, for each condition, an integer number indicating their desired number of units. Subjects were informed that one of the conditions would be randomly selected through a lottery and implemented.

Table 1: Treatments

Effective price	Subsidy type	Subsidy rate	Treatment name
\$0.50	No subsidy	–	NS
\$0.33	Rebate	33% (\$0.17)	R33
	Match	1:2	M33
	Discount	33% (\$0.17)	D33
\$0.25	Rebate	50% (\$0.25)	R25
	Match	1:1	M25
	Discount	50% (\$0.25)	D25

1.2 Subject recruitment

We recruited subjects from an online labor market, Amazon’s Mechanical Turk (AMT), restricted to US residents.⁴ In the posted task, interested workers were informed that they would earn \$2 for answering a 20-minutes academic survey on several topics. Donations were mentioned as one of the topics, but the real-donation dictator game was not particularly salient compared to other survey elements, so it is unlikely that subjects considered the donation task as the main subject of investigation. Interested workers followed a link which directed them to the survey containing the experiment on Qualtrics.

⁴AMT provides several benefits to the researcher, among them fast and easy access to subjects, a diverse subject pool, and low cost (Paolacci et al., 2010; Mason and Suri, 2012). Several papers have examined the suitability of AMT for experimental research and the quality of results obtained through AMT recruitment and have generally found encouraging results (Paolacci et al., 2010; Ross et al., 2010; Mason and Suri, 2012; Rand, 2012). Results in these papers highlight a high internal consistency of self-reported demographics, an incentive-compatibility of earnings, and a “spammer”-free workforce from the built-in reputation system. They also present and review results from successful replication of standard experimental games in AMT (e.g. Paolacci et al., 2010; Rand, 2012) In implementing our experiment, we followed the suggestions for researchers in that literature and the Guidelines for Academic Requesters on AMT (WeAreDynamo, 2014).

In total, we have 759 observations of participants starting the survey and 718 completed records.⁵ Incomplete records were subsequently dropped from the analysis. Among the incomplete observations, there were 27 assigned to the within-subject design that only suffered from missing entries in one or more of the treatment conditions. Taking a conservative approach, we also drop those. Interpreting these missing values as zeros and including them does not change the main results. The obvious concern that some subjects may fraudulently use multiple accounts to participate more than once is generally seen as a minor problem in online experiments (Horton et al., 2011; Paolacci et al., 2010). We follow the common approach to exclude subjects with identical IP addresses from the analysis.⁶ We also drop one subject in the BS sample who indicated an age below 18 in the questionnaire, despite having confirmed an age above 18 when agreeing to the informed consent statement. This leaves us with a sample of 671 subjects of which 558 took part in the between-subject design and 113 in the within-subject design.

1.3 Procedure

The experiment followed common procedures of online experimentation. Having followed the link to the survey platform, interested workers read and confirmed an informed consent page about the research study. Afterwards, they answered a survey which consisted of four parts: (1) the donation question, (2) a questionnaire on various topics, (3) a low-stake version of the Eckel-Grossman risk task (Eckel and Grossman, 2002, 2008a),⁷ and (4) a 5-item manipulation check

⁵Among the complete observations, three subjects had restarted the survey and hence created an incomplete duplicate record. We kept the complete observations of these three subjects after making sure that they had not encountered a treatment condition in their first attempt and gave the same answers in the survey.

⁶In the case of AMT, having multiple accounts is forbidden by Amazon’s Terms of Service (Mason and Suri, 2012) and creating an account requires a unique credit card number (Paolacci et al., 2010). Including subjects with duplicate IP addresses does not change the results.

⁷We opt for the Eckel-Grossman Risk Task because of its simplicity and quickness and because our samples of AMT workers were likely to exhibit larger heterogeneity in numeracy

questionnaire comparable to Eckel and Grossman (2003), Davis and Millner (2005), and Eckel and Grossman (2006b). Parts (1) and (2) were presented in random order. Hence, subjects encountered the donation question either before or after the questionnaire. The questionnaire consisted of questions on sociodemographics, employment, religious beliefs, as well as current ambient environmental conditions and the Ten Item Personality Inventory (TIPI), which is a standard one-minute version of more extensive multi-item instruments to assess the “Big Five” personality dimensions (Gosling et al., 2003; Ehrhart et al., 2009). After completion of the survey, a unique code was shown which had to be entered into the survey task window on AMT for subjects to receive their earnings. Average payouts were \$1.79 (net of donations and including an average of \$0.30 additional payment for the risk task). Subjects took on average 8.38 minutes to complete the experiment.

2 Results

Variables elicited in the questionnaire suggest a diverse sample of subjects: Slightly less than half of subjects are female, and slightly less than half graduated from college (see Table 5 in Appendix B). About one-third of subjects are married, and about the same share has children under age 16 living in the household. Both age and income are well spread, with median age in category 26–34 and median yearly income in category US\$40,000–49,999.

Answers to the manipulation check questions indicate that on average, subjects clearly understood instructions and procedures, felt that their anonymity was preserved, trusted the experimenters and the charity, and found the recipients of the donations worth supporting. Comparing the BS and WS sample by a multivariate analysis of variance (MANOVA) suggests that randomization into

than a standard laboratory sample of students. The Eckel-Grossman task has been shown to produce better results with people with low mathematical skill (Dave et al., 2010). Stakes start out at \$0.28 for the sure option and end up at \$0.02 and \$0.70 for the most risky gamble.

designs was successful ($p = 0.44$). This also applies to the randomization into treatment groups of the BS sample (see Table 6 in Appendix B for summary statistics, $p = 0.46$ for Pillai’s trace). In both tests, we exclude the manipulation check items from the list of dependent variables due to potential endogeneity. However, a separate MANOVA of the manipulation check questions does not indicate significant differences, and including them does not result in lower p -values than those reported here.

In the remainder of the section, we first compare the effectiveness of rebates and matches for the BS sample, taking into account different outcome dimensions. Afterwards, we present results on the discount subsidy. Finally, findings are related to the WS sample.

2.1 Rebates versus matches

In Table 2, we present descriptive results of the BS sample. Panel A contains the different outcome measures, with each row corresponding to a specific treatment. Column (1) shows the average number of nutritional packages selected by individuals, without accounting for any subsequent subsidy. In line with previous literature, we refer to this immediate choice in the experimental task as checkbook giving. Outcomes presented in the next three columns directly follow from this. Column (2) translates subjects’ choices into out-of-pocket expenses, which correspond to the notion of checkbook giving in a standard expenditure donation experiment. Hence, column (2) is column (1) evaluated at the nominal price at which the package was offered to subjects, that is, \$0.50 in all rebate and match treatments and \$0.33 (\$0.25) in the price discount treatment with low (high) subsidy rate. Column (3) reports individual net expenses after rebates have been accounted for and column (4) refers to the contributions of nutritional packages including the match. The latter represents the number of packages the charity “receives”, i.e. will be able to fund from the received do-

nations. If multiplied by \$0.50, column (4) corresponds to the charity receipts in dollars, a common focus in expenditure donation experiments. Since column (4) and charity receipts in dollars are perfectly collinear, we will focus on physical units in column (4) in the following analysis. Column (5) shows average charity receipts conditional on being a donor (intensive margin of giving) while column (6) reports on the fraction of donors, i.e. subjects who give at least one package (extensive margin of giving). For each outcome measure, Panels B and C report p -values of tests on the pairwise comparisons of treatments. In the case of comparing treatments with effective prices of \$0.33 and \$0.50, the price comparison also involves introducing a subsidy.

Focusing on the comparison of rebates and matches first, four main results follow from the aggregated outcomes reported in columns (1) to (4) in Table 2. First, charity receipts do not significantly differ between rebates and matches at both effective price levels ($p = 0.52$ and $p = 0.41$).

Result 1 (*Charity receipts*) *Charity receipts do not significantly differ between rebate and matching subsidies.*

Second, subjects seem to account for the subsidy type when making their decisions and choose significantly more units and higher out-of-pocket expenses under a rebate than under the equivalent match (\$0.85 vs. \$0.53 at the low subsidy rate and \$0.97 vs. \$0.56 at the high subsidy rate).

Result 2 (*Checkbook giving*) *Checkbook giving is significantly higher under rebates than under matches and roughly proportionally accounts for the respective subsidy.*

Third, as a result of this responsiveness, not only charity receipts but also net donations do not show significant differences between rebates and matches ($p = 0.76$ and $p = 0.41$). Moreover, net donations exhibit a roughly constant share of around one quarter of the endowment across all treatment conditions and

Table 2: Between-subjects design

Treatment	N	Checkbook		Net	Charity receipts		%
		giving			All	By donors	
		(units)	(\$)	(\$)			(units)
		(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Summary statistics</i>							
NS	83	1.169 (1.413)	0.584 (0.706)	0.584 (0.706)	1.169 (1.413)	2.256 (1.177)	0.518 (0.503)
R33	71	1.690 (1.545)	0.845 (0.773)	0.558 (0.510)	1.690 (1.545)	2.400 (1.294)	0.704 (0.460)
M33	85	1.059 (1.339)	0.529 (0.670)	0.529 (0.670)	1.506 (2.021)	3.048 (1.886)	0.494 (0.503)
D33	90	1.478 (1.973)	0.488 (0.651)	0.488 (0.651)	1.478 (1.973)	2.771 (1.927)	0.533 (0.502)
R25	58	1.931 (1.705)	0.966 (0.853)	0.483 (0.426)	1.931 (1.705)	2.732 (1.379)	0.707 (0.459)
M25	80	1.113 (1.253)	0.556 (0.626)	0.556 (0.626)	2.225 (2.506)	3.787 (2.176)	0.588 (0.495)
D25	91	2.143 (2.831)	0.536 (0.708)	0.536 (0.708)	2.143 (2.831)	3.545 (2.879)	0.604 (0.492)
<i>Panel B: Subsidy type comparisons</i>							
R33 vs. M33		0.01	0.01	0.76	0.52	0.06	0.01
R33 vs. D33		0.44	0.00	0.44	0.44	0.27	0.03
M33 vs. D33		0.10	0.68	0.68	0.93	0.49	0.60
R25 vs. M25		0.00	0.00	0.41	0.41	0.01	0.15
R25 vs. D25		0.57	0.00	0.57	0.57	0.07	0.20
M25 vs. D25		0.00	0.84	0.84	0.84	0.63	0.82
<i>Panel C: Price comparisons</i>							
R33 vs. NS		0.03	0.03	0.79	0.03	0.58	0.02
M33 vs. NS		0.61	0.61	0.61	0.21	0.02	0.76
D33 vs. NS		0.24	0.35	0.35	0.24	0.12	0.84
R25 vs. R33		0.41	0.41	0.36	0.41	0.24	0.97
M25 vs. M33		0.79	0.79	0.79	0.05	0.09	0.23
D25 vs. D33		0.07	0.64	0.64	0.07	0.11	0.34

Shown in Panel A are mean values, with standard deviations in parentheses. Shown in Panel B and Panel C are p -values of two-tailed t -tests with unequal variances in columns (1) to (5) and of Pearson χ^2 tests for binary data in column (6).

neither the introduction of a subsidy nor increasing the subsidy rate produces significant changes. Even if we test the largest difference in numbers between the 50% rebate (R25) and the control (NS), it is not statistically significant ($p = 0.29$ for a two-tailed t -test).

Result 3 (*Net donations*) *There is no evidence for crowding-in or crowding-out of net donations by rebate or matching subsidies of any level.*

Finally, the demand for the provision of the charitable good follows the law of demand. Column (4) shows that charity receipts significantly increase in the subsidy level, either from introducing the subsidy (from 1.17 to 1.69 packages in case of the rebate) or from increasing the subsidy rate (from 1.51 to 2.23 packages in case of the match). Given result 3, this increase in charity receipts is entirely driven by subsidy payments.

Result 4 (*Law of demand*) *Charity receipts significantly decrease in the price.*

The indifference between rebates and matches regarding charity receipts contrasts with virtually all previous literature on expenditure donations. We therefore now examine whether results hold when controlling for the available covariates. Whereas a common approach in the literature is to use a Tobit Model with checkbook giving, charity receipts, or their logarithmized value as dependent variable, the discrete nature of our donation decision makes it an unsuitable choice to model our data.⁸ Instead, we estimate an Ordered Probit Model, based on a latent variable

$$y_i^* = x_i' \beta + \epsilon_i \quad (1)$$

where x_i is a vector of variables, including a constant, β is a vector of parameters to be estimated and ϵ_i is an i.i.d. standard normally distributed error term.

⁸This is supported by conditional moment tests significantly rejecting the assumption of normally distributed error terms for the Tobit Model with charity receipts or logarithmized charity receipts as dependent variable ($p < 0.01$). We nevertheless report the results of different Tobit specifications together with simple OLS results as robustness checks in Table 12 in Appendix B. Qualitative results do not substantially differ.

Given a set of possible outcomes $y_i \in \{y^1, \dots, y^J\}$ we observe

$$y_i = y^j \quad \text{if } \alpha_{j-1} < y_i^* \leq \alpha_j \quad \text{for } j = 1, \dots, J \quad (2)$$

where α_0 and α_J are set to $-\infty$ and ∞ , respectively, $\alpha_1 = 0$ and $\alpha_2, \dots, \alpha_{J-1}$ are threshold parameters to be estimated.

Although our primary interest lies in the effect on charity receipts, we use the number of nutritional packages directly provided by the individual (checkbook giving) as the dependent variable. Due to the deterministic relation between checkbook giving and charity receipts, we then use the model to calculate the average of the sample marginal effects on charity receipts. We do so for two reasons. First, the checkbook giving captures the immediate choice subjects make and is therefore the most intuitive concept to model the decision process. Second, it simplifies the estimation procedure, since for the choice sets of any two different treatments, one is a subset of the other and the smaller set is simply censored from above. In particular, subjects can give zero to four packages when facing a match, rebate or no subsidy, while in the discount treatments they can donate up to six or eight packages, depending on whether the discount rate is low or high. This can easily be accommodated by adding censoring to the model. The probability to observe an outcome $y_i = y^j$ can thus be written as

$$\begin{aligned} Pr(y_i = y^j | x_i) = & \mathbb{1} \{y^j < y_i^{max}\} \{\Phi(\alpha_j - x_i' \beta) - \Phi(\alpha_{j-1} - x_i' \beta)\} \\ & + \mathbb{1} \{y^j = y_i^{max}\} \{1 - \Phi(\alpha_{j-1} - x_i' \beta)\} \end{aligned} \quad (3)$$

where y_i^{max} represents the maximum possible checkbook giving for individual i which depends on the treatment the individual faces. Since in our data we never observe a checkbook giving of seven, we cannot include this outcome category in our model. Furthermore, only a single subject decides to donate five packages. We follow different strategies to deal with this issue. In our main analysis we

treat this subject as if it had donated six packages, while in the robustness checks we explicitly include the outcome category of five packages or omit the observation. The results across those strategies are similar (see columns (1) to (4) of Table 11 in Appendix B). Consequently, the set of possible outcomes for the following analysis is $y_i \in \{0, 1, 2, 3, 4, 6, 8\}$ for the 50% discount treatment, $y_i \in \{0, 1, 2, 3, 4, 6\}$ for the 33% discount treatment and $y_i \in \{0, 1, 2, 3, 4\}$ for all other treatments. The parameters $\theta = (\beta, \alpha_2, \alpha_3, \alpha_4, \alpha_5, \alpha_6)$ are estimated by maximum likelihood, without and with including all covariates except the manipulation check questions presented in Table 5 in x_i . In both cases, x_i includes dummies for subsidy types as well as subsidy type specific dummies for whether the offered subsidy rate is high and therefore the resulting effective price is low (\$0.25).

In Panel A of Table 3, columns (1) and (2), we present the results in the form of the average sample marginal effects on charity receipts. The formulas used to calculate these effects are derived in Appendix A. For example, results in column (1) of Panel A suggest that offering a 33% rebate increases average charity receipts per individual by about 0.5 packages compared to not offering any subsidy, whereas increasing the subsidy rate from 33% to 50% has no significant effect in the case of the rebate. Analogously to Table 2, the predicted levels of charity receipts are compared pairwise across subsidy types in Panel B, holding the effective price constant. The estimates confirm results 1 and 4. Furthermore, we calculate implied price elasticities and test whether they significantly differ from -1 in Panel C. Price elasticities below -1 can be interpreted as crowding-in, while price elasticities above -1 represent crowding-out. The estimated price elasticities are not significantly different from -1 which confirms result 3. To check for misspecification of the model we use the Lagrange Multiplier test derived by Glewwe (1997). In both model specifications, the Null of normally distributed error terms cannot be rejected ($p > 0.35$ and $p > 0.95$, without and

with covariates, respectively). We also check for heteroscedasticity (Columns (5)-(7) of Table 11 in Appendix B) and find that results are robust.⁹

A crucial question is whether the same level of charity receipts between matches and rebates arises because subjects perfectly account for the subsidy type by adjusting their checkbook giving. To answer this question, we take a look at the decision to donate, i.e. the extensive margin of giving, and the charity receipts conditional on being a donor, i.e. the intensive margin of giving. A histogram of the exact distribution of charity receipts by treatment is shown in Figure 1 in the Appendix B. If adapting the level of checkbook giving is the single driver, we should find similar levels for both the intensive and the extensive margin. However, as discussed in section 1, the rebate decreases the minimum net expense required to become a donor, making it potentially more effective on the extensive margin than the match. Results reported in column (6) of Table 2 confirm that rebates attract more donors than matches (12–20 percentage points), although the difference is only significant at the low subsidy rate. This suggests that the result on the level of charity receipts is at least partly driven by the fact that the rebate subsidy is more effective on the extensive margin. It would therefore be wrong to conclude that in a quantity donation setting matches and rebates do not differ in their effectiveness, although this might hold for the level of unconditional charity receipts in column (4).

Result 5 (*Extensive margin*) *The rebate subsidy is more effective in attracting donors than the match, although the difference is only significant at the low subsidy rate.*

⁹In two of three cases the main model is not rejected. Only if we use the whole set of covariates to explicitly model heteroscedasticity – including variables that are implausible to cause heteroscedasticity – the main model is rejected ($p < 0.01$). Still rebates and matches do not significantly differ in the level of charity receipts at the low subsidy rate, but matches are now estimated to raise significantly more packages than rebates at the high subsidy rate. However, one should be careful with taking these results at face value since this model specification produces some odd results. For example, a 33% discount is estimated to have a significantly negative impact on charity receipt of almost 0.4 packages, which is in strong contrast to what we observe in the data.

Table 3: Estimation results (BS design)

	Ordered Probit (Charity receipts) [All]		Probit (Donated) [All]		Ordered Probit (Charity receipts) [Donors]	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Marginal effects</i>						
Rebate	0.546** (0.237)	0.558** (0.256)	0.186** (0.077)	0.229*** (0.081)	0.091 (0.256)	-0.025 (0.271)
Match	0.324 (0.261)	0.241 (0.287)	-0.024 (0.077)	-0.008 (0.085)	0.799** (0.359)	0.478 (0.391)
Discount	0.306 (0.247)	0.203 (0.271)	0.015 (0.076)	0.012 (0.084)	0.537 (0.332)	0.278 (0.366)
Rebate \times low price	0.218 (0.277)	0.197 (0.301)	0.003 (0.081)	-0.031 (0.088)	0.326 (0.266)	0.343 (0.279)
Match \times low price	0.888** (0.373)	1.072*** (0.406)	0.093 (0.077)	0.115 (0.083)	0.783* (0.448)	0.828* (0.469)
Discount \times low price	0.585* (0.326)	0.667* (0.365)	0.071 (0.073)	0.064 (0.084)	0.571 (0.448)	0.869* (0.509)
<i>Panel B: Subsidy type comparisons</i>						
R33 vs. M33	0.43	0.29	0.01	0.00	0.05	0.19
R33 vs. D33	0.37	0.22	0.02	0.01	0.17	0.40
M33 vs. D33	0.95	0.90	0.60	0.81	0.53	0.65
R25 vs. M25	0.23	0.17	0.14	0.30	0.00	0.01
R25 vs. D25	0.71	0.76	0.19	0.17	0.09	0.08
M25 vs. D25	0.43	0.32	0.82	0.70	0.33	0.76
<i>Panel C: Implied price elasticities</i>						
NS to R33	-0.93** (0.4)	-0.92** (0.41)			-0.1 (0.27)	0.03 (0.27)
NS to M33	-0.6 (0.47)	-0.44 (0.52)			-0.73** (0.31)	-0.44 (0.35)
NS to D33	-0.57 (0.45)	-0.38 (0.5)			-0.51* (0.31)	-0.27 (0.34)
R33 to R25	-0.44 (0.55)	-0.38 (0.58)			-0.46 (0.38)	-0.49 (0.39)
M33 to M25	-1.67** (0.66)	-1.96*** (0.7)			-0.82* (0.47)	-0.91* (0.52)
D33 to D25	-1.21* (0.65)	-1.39* (0.73)			-0.67 (0.51)	-1.01* (0.57)
Covariates ^a	No	Yes	No	Yes	No	Yes
N	558	428	558	428	326	256
Log likelihood	-801.28	-590.10	-372.07	-248.44	-427.37	-314.64

Standard errors reported in parenthesis, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The no subsidy control treatment (NS) is used as baseline. (1)–(2): Ordered Probit with the number of packages donated by the individual as dependent variable. (3)–(4): Probit for whether or not a donation was given. (5)–(6): Ordered Probit conditional on being a donor, with the number of packages donated by the individual as dependent variable. (1)–(2) and (5)–(6) treat a single observation with 5 selected packages as if it were 6.

Panel A presents average of the sample marginal effects. For (1)–(2) and (5)–(6) *marginal effects* refer to the average change in expected charity receipts over all individuals or donors only, respectively. For each individual considered, the change is calculated by taking the difference in expected charity receipts between receiving a particular subsidy at the low rate (rebate, match, discount) and not receiving any subsidy or between receiving a particular subsidy at the high rate (rebate \times low price, match \times low price, discount \times low price) and receiving the same subsidy at the low rate. Panel B presents p -values for the pairwise comparison of treatment effects (subsidy treatment vs. no subsidy) between subsidy types, based on the average of the sample marginal effects.

Panel C presents implied price elasticities, which are calculated as $\frac{\Delta Q / (Q_1 + Q_2)}{\Delta P / (P_1 + P_2)}$. For the 1:2 match, a constant effective price of \$0.33 is assumed.

^a Covariates comprise all variables reported in Table 5 except for the manipulation check items. Likelihood ratio tests reject that their coefficients in model (2), (4) and (6) are jointly zero ($p < 0.01$, $p < 0.01$ and $p < 0.05$, respectively).

The fact that the difference in the extensive margin is more pronounced for the low subsidy rate is not surprising, since for the 1:2 match the first unit donated does not result in a matching payment. Consequently, the minimum expense required to become a donor is larger than for the equivalent rebate while the impact of the action is the same: a single nutritional package received by the charity. As a result, not only the costs but the effective prices at the margin of becoming a donor differ, further decreasing the relative attractiveness of the match.

Charity receipts at the intensive margin (column 5) under both match conditions significantly exceed the corresponding values in the rebate conditions. Comparing the match and rebate treatments to the control suggest that the rebate raises unconditional charity receipts primarily through the extensive margin, while the match unfolds its impact through the intensive margin.

Result 6 (*Intensive margin*) *Charity receipts obtained from donors are higher under a match than under the corresponding rebate subsidy.*

Again, we supplement the summary statistics by estimating appropriate parametric models. Columns (3) and (4) of Table 3 refer to a Probit without and with covariates, respectively, while columns (5) and (6) capture the intensive margin by estimating an Ordered Probit Model for donors only. The latter is set up analogously to the Ordered Probit Model described above. If we assumed that after controlling for observable characteristics, the error terms between the decisions to donate and how much to donate are uncorrelated, we could interpret these two models jointly as a Two-Part model. The parametric estimation confirms results 5 and 6, but for the intensive margin only the difference between matches and rebates at the high subsidy rate remains significant when covariates are included.

One concern regarding the comparison of rebate and match subsidies in our experiment is that differences in the budget constraints for charity receipts may

drive some of the results. While for rebates, the highest possible number of packages received is always four, match treatments allow donors to provide up to eight packages to the charity, if the subsidy rate is high. Similar differences apply to all experiments comparing rebates and matches in the expenditure donation literature, as they also endow subjects with a limited amount of money. However, in the expenditure donation literature, it is shown that the higher effectiveness of the match observed in laboratory studies also holds in field experiments where subjects use their own income (Eckel and Grossman, 2008b, 2017). If the budget constraint matters in our design, the results may understate the effectiveness of the rebate compared to the match. This implies that the rebate may actually be even more effective than the match in the case of quantity donations.

We revisit Result 1 and Result 6 by recoding subjects' choices in order to equalize budget constraints. In our data, a total of 29.5% of subjects give the maximum amount under the rebate, compared to 10.9% in the match conditions. For each condition, we set all charity receipts above four packages to four packages (the maximum level of charity receipts under rebates). Table 9 (see Appendix B) presents the results. Although the rebate treatments now provide the highest number of packages on average in column (1), the difference to the corresponding match treatments is at best only marginally significant. Hence, Result 1 does not seem to be entirely driven by differences in budget constraints. In contrast, column (2) on the intensive margin of giving does not replicate Result 6. Hence, we cannot exclude the possibility that the match only creates larger donations in settings where the budget constraint is binding for a sufficiently large share of individuals.

2.2 Discount subsidies

The responses to the discounts are presented in Table 2. Discounts turn out to be as effective as rebates and matches, since charity receipts in column (4) do not significantly differ from the other two subsidy types ($p \geq 0.44$ for each comparison). Hence, the increased salience of the effective price does not affect price sensitivity. Checkbook giving in units (column 1) under the discount is statistically indistinguishable from the corresponding rebate, while checkbook giving in dollars (column 2) is statistically indistinguishable from the corresponding match. This observation fits a rational agent who wants to equalize net donations and charity receipts under the different subsidies. In consequence, net donations in column (3) are not statistically different from those observed for rebates and matches, with no evidence for crowding-in or -out. In line with the law of demand, charity receipts increase in the subsidy level, although only the increase from 1.48 to 2.14 packages induced by the price change from \$0.33 to \$0.25 results in a significant difference ($p = 0.07$). These descriptive findings are fully confirmed by the Ordered Probit estimates in Table 3, columns (1)–(2). Implied price elasticities are mostly estimated to lie in between those of rebates and matches, but much closer to those of matches than to those of rebates.

Result 7 (*Discounts*) *The discount subsidy produces the same level of charity receipts and net donations as rebates and matches. Increasing the subsidy rate increases charity receipts, without crowding-in or crowding-out net expenses.*

Interestingly, columns (5) and (6) of Table 2 and columns (3) and (6) of Table 3 show that discounts behave much like matches when disaggregating behavior into the two margins: At both effective prices, they do not significantly differ at the extensive or intensive margin. This is particularly surprising with respect to the extensive margin, as similar to rebates, a discount reduces the minimum net expense required to donate. Yet, at the effective price of \$0.33 the discount

is about 20 percentage points less effective in attracting donors than the rebate frame.

Result 8 (*Discounts at the extensive and intensive margins*) *Despite similar properties at the extensive margin, discounts are significantly less effective in attracting donors than rebates. Instead, they elicit a response equivalent to matches at both margins.*

2.3 Within-subject design

If the results of the WS design mirrored results 1 to 8, the within-subject variation could be used to learn more about how those results come about. However, as we will show, the results of the WS design substantially differ and can therefore not be used in the proposed manner. Although under these circumstances we rank the external validity of the BS design higher, as outlined below, the WS data can provide important insights into subjects' decision process when they are forced to compare different conditions.

Table 4 presents summary statistics and tests of subjects' choices in the WS design analogously to Table 2. Beginning with the level of charity receipts in column (4), we observe that matches and discounts are clearly more effective in providing the charitable good than rebates. This finding is most pronounced for the low price of \$0.25 while for \$0.33, the discontinuities in the match—the first and third unit funded do not result in an additional match payment—may discourage giving. This might explain why the match is significantly more effective than the discount at the high but not the low subsidy rate. Turning to checkbook giving in columns (1) and (2), we find that subjects do not follow a “constant contribution rule”. Checkbook giving in units significantly differs between subsidy types, with discounts producing the highest and matches the lowest levels at a given effective price. For net donations in column (3), we observe that matches and discounts significantly crowd-in net expenses when

introducing the subsidy while rebates do not. For the rebate, an increase in the subsidy rate does lead to significant crowding-out. Nevertheless, in column (4), the law of demand holds for all subsidy types. A histogram of the distribution of charity receipts by treatment is shown in Figure 2 in Appendix B.

At first glance the discrepancy in results compared to the BS sample might be surprising, but a closer look to the extensive margin offers a simple explanation for most differences. Unlike in the between-subject design, we find that for a given effective price, subsidies are equally successful in attracting donors. The reason is likely to be that subjects do not decide whether to donate for each subsidy separately, but rather make a single choice across all subsidies with a similar rate. Under these circumstances, the results from the between-subjects design are more likely to be transferable to the field.

3 Discussion

The equivalence of matches and rebates observed in our quantity donation experiment is without parallel in a literature that has, so far, employed an expenditure donation context.¹⁰ There are three candidate explanations for this divergence: Behavior between the two donation classes could differ only for rebates, it could differ only for matches, or it could differ for both. To explore this further, we compare our observed price sensitivities for each subsidy type with those in the literature. For the match, the behavior observed in our data corresponds to the behavior observed in most studies. In line with Karlan and List (2007), Lukas et al. (2010), and Bekkers (2015), we find net expenses under the match to be insensitive to changes in the subsidy rate. Furthermore, the introduction of a matching subsidy does not affect net expenses either, which

¹⁰We are aware of only one other paper finding the same level of charity receipts between rebates and matches. Davis (2006) uses a highly artificial decision frame in which subjects decide on the amount of charity receipts instead of checkbook giving. The objective of Davis (2006) is to investigate why the standard result is observed rather than to employ a decision frame of practical relevance.

Table 4: Within-subject design

Treatment	N	Checkbook		Net donations	Charity receipts		% donating
		giving			All	By donors	
		(units)	(\$)	(\$)			(units)
		(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Summary statistics</i>							
NS	113	0.558 (1.026)	0.279 (0.513)	0.279 (0.513)	0.558 (1.026)	1.750 (1.105)	0.319 (0.468)
R33	113	0.867 (1.278)	0.434 (0.639)	0.286 (0.422)	0.867 (1.278)	2.130 (1.147)	0.407 (0.493)
M33	113	0.699 (1.085)	0.350 (0.542)	0.350 (0.542)	0.965 (1.614)	2.535 (1.695)	0.381 (0.488)
D33	113	0.982 (1.547)	0.324 (0.510)	0.324 (0.510)	0.982 (1.547)	2.362 (1.580)	0.416 (0.495)
R25	113	0.991 (1.373)	0.496 (0.686)	0.248 (0.343)	0.991 (1.373)	2.196 (1.233)	0.451 (0.500)
M25	113	0.805 (1.109)	0.403 (0.554)	0.403 (0.554)	1.611 (2.218)	3.434 (2.052)	0.469 (0.501)
D25	113	1.363 (1.996)	0.341 (0.499)	0.341 (0.499)	1.363 (1.996)	2.906 (2.003)	0.469 (0.501)
<i>Panel B: Subsidy type comparisons</i>							
R33 vs. M33		0.01	0.01	0.03	0.27	0.19	0.32
R33 vs. D33		0.08	0.00	0.08	0.08	0.42	0.56
M33 vs. D33		0.00	0.25	0.25	0.80	0.62	0.16
R25 vs. M25		0.04	0.04	0.00	0.00	0.00	0.56
R25 vs. D25		0.00	0.00	0.00	0.00	0.03	0.59
M25 vs. D25		0.00	0.05	0.05	0.05	0.18	1.00
<i>Panel C: Price comparisons</i>							
R33 vs. NS		0.00	0.00	0.78	0.00	0.13	0.00
M33 vs. NS		0.01	0.01	0.01	0.00	0.02	0.02
D33 vs. NS		0.00	0.02	0.02	0.00	0.04	0.00
R25 vs. R33		0.06	0.06	0.05	0.06	0.79	0.10
M25 vs. M33		0.13	0.13	0.13	0.00	0.02	0.01
D25 vs. D33		0.00	0.24	0.24	0.00	0.13	0.06

Shown in Panel A are mean values, with standard deviations in parentheses. Shown in Panel B and Panel C are p -values of two-tailed paired t -tests in columns (1) to (4), of two-tailed unpaired t -tests with unequal variances in column (5), and of McNemar's χ^2 test for paired binary data in column (6).

is similar to results found by Karlan et al. (2011) but different from the significant increase documented by Karlan and List (2007), Huck et al. (2015), and Eckel and Grossman (2017). Comparable evidence for rebates is scarcer: Eckel and Grossman (2017) find no significant effect of rebate subsidies on charity receipts, in contrast to our results, but they study lower rebate rates than we do. No other study explicitly tests for significant changes in charity receipts or net expenses caused by the introduction of a rebate or changes in the rebate rate. However, most of the descriptive results on checkbook giving and estimated price elasticities suggest a slight increase in charity receipts paired with crowding-out of net expenses (Eckel and Grossman, 2003; Davis et al., 2005; Bekkers, 2015). We find instead that rebates significantly increase charity receipts and that the minuscule decrease in net expenses observed in our data is highly insignificant. In our view, this favors the hypothesis that it is different behavior under the rebate subsidy that explains why we diverge from results derived in the expenditure donation context.

The extensive margin results in our setting also diverge from the literature. While we find that rebates attract more donors than matches, Bekkers (2015) finds the opposite using similar subsidy rates in an expenditure donation context. Theoretical considerations as discussed in the introduction suggest that this could be driven by a threshold effect: Under discrete quantity donations, the minimum net expense required to become a donor differs. It is lower under a rebate than under a match of the same subsidy rate, leading to a greater number of donors.

An interesting yet open question in this context is how the effectiveness of different subsidy types differs with the level of the unsubsidized unit price. At a given subsidy rate, a larger unsubsidized price increases the absolute difference in the minimum expense required to become a donor between rebates and matches. As a result, the differences on the extensive margin might become

more pronounced, which in turn might be sufficient to make the rebate raise more money than the match.

4 Conclusions

In this paper, we define a class of donations in which donors are asked to choose the number of discrete units of the charitable good to fund instead of the amount of money to give. We call the former a quantity donation and the latter an expenditure donation. We present empirical evidence from an online field experiment designed to analyze how different subsidy types affect quantity donations. By doing so we focus on the two prevalent subsidy types, rebates and matches, as well as a novel type framed as a simple price discount. The latter can be applied since for quantity donations, each physical unit has a well-defined price that can be explicitly reduced.

The results remarkably differ from the well-established findings for expenditure donations. Matching subsidies do not outperform rebates but are equally effective in raising funds. Yet matching and rebate subsidies create different responses at the extensive and intensive margin of giving. While rebates significantly increase the fraction of donors, matches produce larger donations. The significantly higher likelihood to give under the rebate compared to the match is in contrast to the expenditure donation literature and makes the rebate catch up with the match in the quantity donation setting of our experiment. Although on the extensive margin the price discount subsidy is theoretically equivalent to the rebate, no comparable effect is observed. Instead the discount behaves like the match on both margins. On the aggregate it is equally effective as matches and rebates. All three subsidy types do not significantly affect net expenses, which implies that the price sensitivity under the match mirrors that of expenditure donations while rebates appear to make subjects respond more positively

to price changes under quantity donations than under expenditure donations.

Our results underline the relevance of the decision environment when soliciting donations and, thus, have important implications for practitioners. First, charities that employ quantity donations in their fundraising efforts cannot rely on the insights from the existing literature on expenditure donations. Second, in cases where funds are not tied to being used as a subsidy, subsidizing quantity donations is not necessarily beneficial as on the aggregate it may not crowd-in private giving. Third, whether it is useful to apply a certain type of subsidy to quantity donations depends on a charity's objectives. If for some reason the charity desires to maximize the set of donors, our evidence suggests that a rebate is preferable over a match. If the charity instead seeks to maximize charity receipts, the choice of the subsidy type is irrelevant.

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Appendix A Derivation of marginal effects

To explain the calculation of the average of the sample marginal effects we first explicitly write x_i in equation (1) as

$$x_i = \begin{pmatrix} rebate_i \\ match_i \\ discount_i \\ rebate_i \times low\ price_i \\ match_i \times low\ price_i \\ discount_i \times lowprice_i \\ w_i \end{pmatrix},$$

where $rebate_i$, $match_i$, and $discount_i$ are dummies for whether individual i faces a particular subsidy type, $rebate_i \times low\ price_i$, $match_i \times low\ price_i$, and $discount_i \times lowprice_i$ are subsidy type specific dummies indicating whether the subsidy rate is high and the effective price is low (\$0.25), and w_i is a vector of covariates including a constant.

We use the estimated parameters $\hat{\theta}$ and the deterministic relationship between checkbook giving, y_i , and charity receipts, cr_i , to calculate the expected level of charity receipts predicted by the model, \hat{E}_i , for each individual under each treatment condition. For example, to receive individual i 's expected level of charity receipts under the 50% rebate we set $x_i^{R25} = (1, 0, 0, 1, 0, 0, w_i)'$ and calculate

$$\begin{aligned} \hat{E}_i^{R25} &= \sum_{k=0}^8 kPr(\widehat{cr_i = k} | x_i^{R25}) = \sum_{k=0}^4 kPr(\widehat{y_i = k} | x_i^{R25}) \\ &= 4 - \Phi(\hat{a}_4 - x_i^{R25'} \hat{\beta}) - \Phi(\hat{a}_3 - x_i^{R25'} \hat{\beta}) - \Phi(\hat{a}_2 - x_i^{R25'} \hat{\beta}) - \Phi(-x_i^{R25'} \hat{\beta}) \end{aligned}$$

where $Pr(\widehat{cr_i = k} | x_i^{R25}) = 0$ for $k > 4$ since the maximum level of charity

receipts under the rebate is four. The second equality then follows from the fact that for all treatment conditions except the match the level of checkbook giving (in physical units) is equal to the level of charity receipts (in physical units). Finally, we use equation (3) to calculate $Pr(\widehat{y_i = k} | x_i^{R25})$. The expected levels of charity receipts for the other six conditions are accordingly calculated as

$$\begin{aligned}
\hat{E}_i^{NS} &= 4 - \Phi(\hat{a}_4 - x_i^{NS'} \hat{\beta}) - \Phi(\hat{a}_3 - x_i^{NS'} \hat{\beta}) - \Phi(\hat{a}_2 - x_i^{NS'} \hat{\beta}) - \Phi(-x_i^{NS'} \hat{\beta}) \\
\hat{E}_i^{R33} &= 4 - \Phi(\hat{a}_4 - x_i^{R33'} \hat{\beta}) - \Phi(\hat{a}_3 - x_i^{R33'} \hat{\beta}) - \Phi(\hat{a}_2 - x_i^{R33'} \hat{\beta}) - \Phi(-x_i^{R33'} \hat{\beta}) \\
\hat{E}_i^{M33} &= 6 - 2\Phi(\hat{a}_4 - x_i^{M33'} \hat{\beta}) - \Phi(\hat{a}_3 - x_i^{M33'} \hat{\beta}) - 2\Phi(\hat{a}_2 - x_i^{M33'} \hat{\beta}) \\
&\quad - \Phi(-x_i^{M33'} \hat{\beta}) \\
\hat{E}_i^{M25} &= 8 - 2\Phi(\hat{a}_4 - x_i^{M25'} \hat{\beta}) - 2\Phi(\hat{a}_3 - x_i^{M25'} \hat{\beta}) - 2\Phi(\hat{a}_2 - x_i^{M25'} \hat{\beta}) \\
&\quad - 2\Phi(-x_i^{M25'} \hat{\beta}) \\
\hat{E}_i^{D33} &= 6 - 2\Phi(\hat{a}_5 - x_i^{D33'} \hat{\beta}) - \Phi(\hat{a}_4 - x_i^{D33'} \hat{\beta}) - \Phi(\hat{a}_3 - x_i^{D33'} \hat{\beta}) \\
&\quad - \Phi(\hat{a}_2 - x_i^{D33'} \hat{\beta}) - \Phi(-x_i^{D33'} \hat{\beta}) \\
\hat{E}_i^{D25} &= 8 - 2\Phi(\hat{a}_6 - x_i^{D25'} \hat{\beta}) - 2\Phi(\hat{a}_5 - x_i^{D25'} \hat{\beta}) - \Phi(\hat{a}_4 - x_i^{D25'} \hat{\beta}) \\
&\quad - \Phi(\hat{a}_3 - x_i^{D25'} \hat{\beta}) - \Phi(\hat{a}_2 - x_i^{D25'} \hat{\beta}) - \Phi(-x_i^{D25'} \hat{\beta})
\end{aligned}$$

We use the expected level of charity receipts to calculate average of the sample marginal effects for introducing a subsidy type at the low rate (rebate, match, discount) and for changing the subsidy rate for a specific subsidy type from low to high (rebate \times low price, match \times low price, discount \times low price). The average of the sample marginal effects (AMEs) in column (1) and (2) of Table 3 are calculated according to

$$AME^{rebate} = \frac{1}{N} \sum_{i=1}^N E_i^{R33} - E_i^{NS}$$

$$\begin{aligned}
AME^{match} &= \frac{1}{N} \sum_{i=1}^N E_i^{M33} - E_i^{NS} \\
AME^{discount} &= \frac{1}{N} \sum_{i=1}^N E_i^{D33} - E_i^{NS} \\
AME^{rebate \times low \ price} &= \frac{1}{N} \sum_{i=1}^N E_i^{R25} - E_i^{R33} \\
AME^{match \times low \ price} &= \frac{1}{N} \sum_{i=1}^N E_i^{M25} - E_i^{M33} \\
AME^{discount \times low \ price} &= \frac{1}{N} \sum_{i=1}^N E_i^{D25} - E_i^{D33}
\end{aligned}$$

Standard errors are calculated using the delta method.

Appendix B Additional figures and tables

Table 5: Summary statistics

Variable	Full sample			BS sample			WS sample		
	Mean	SD	<i>N</i>	Mean	SD	<i>N</i>	Mean	SD	<i>N</i>
Female	0.48	0.50	671	0.48	0.50	558	0.46	0.50	113
Age (years):									
18–25	0.25	0.43	671	0.25	0.44	558	0.21	0.41	113
26–34	0.38	0.49	671	0.37	0.48	558	0.43	0.50	113
35–54	0.29	0.45	671	0.29	0.46	558	0.27	0.45	113
55–64	0.07	0.25	671	0.06	0.25	558	0.07	0.26	113
>65	0.01	0.12	671	0.01	0.12	558	0.01	0.09	113
Married	0.33	0.47	667	0.34	0.47	554	0.27	0.45	113
Children ^a	0.30	0.46	671	0.30	0.46	558	0.26	0.44	113
College degree	0.48	0.50	670	0.47	0.50	558	0.54	0.50	112
Income ^b (US\$):									
<10,000	0.09	0.28	649	0.09	0.28	540	0.07	0.26	109
10,000–19,999	0.11	0.31	649	0.11	0.31	540	0.11	0.31	109
20,000–29,999	0.12	0.33	649	0.12	0.33	540	0.14	0.35	109
30,000–39,999	0.13	0.34	649	0.11	0.32	540	0.20	0.40	109
40,000–49,999	0.15	0.35	649	0.15	0.35	540	0.15	0.36	109
50,000–74,999	0.20	0.40	649	0.21	0.41	540	0.12	0.33	109
75,000–99,999	0.09	0.29	649	0.09	0.28	540	0.09	0.29	109
100,000–150,000	0.10	0.30	649	0.10	0.30	540	0.10	0.30	109
>150,000	0.02	0.15	649	0.02	0.15	540	0.02	0.13	109
Residential environment:									
Rural	0.20	0.40	671	0.20	0.40	558	0.19	0.39	113
Suburban	0.52	0.50	671	0.51	0.50	558	0.54	0.50	113
Urban	0.29	0.45	671	0.29	0.45	558	0.27	0.45	113
Registered voter	0.87	0.34	663	0.86	0.34	552	0.88	0.32	111
Not-for-profit ^c	0.05	0.22	671	0.06	0.23	558	0.03	0.16	113
Religious ^d	0.13	0.34	659	0.13	0.34	548	0.14	0.34	111
Religion:									
Atheist	0.37	0.48	643	0.38	0.49	533	0.36	0.48	110
Agostic	0.09	0.28	643	0.08	0.28	533	0.10	0.30	110
Roman-Catholic	0.12	0.32	643	0.12	0.32	533	0.14	0.34	110
Protestant	0.18	0.38	643	0.18	0.38	533	0.17	0.38	110
Other Christian	0.12	0.33	643	0.12	0.33	533	0.12	0.32	110
Other Religion	0.12	0.33	643	0.13	0.33	533	0.11	0.31	110
Task order ^e	0.51	0.50	671	0.52	0.50	558	0.46	0.50	113
Big Five (scale 1–7):									
Extraversion	3.21	1.62	626	3.18	1.60	520	3.32	1.73	106
Agreeableness	5.02	1.23	628	5.04	1.24	523	4.91	1.20	105
Conscientiousness	5.11	1.29	630	5.13	1.30	525	4.97	1.24	105
Emotional stability	4.65	1.52	638	4.62	1.53	531	4.79	1.49	107
Openness	4.70	1.29	640	4.67	1.29	532	4.83	1.32	108
Risk pref. (scale 1–6)	4.00	1.79	667	4.06	1.78	554	3.65	1.79	113
Manipulation check questions (scale 1-5):									
Clarity ^f	4.56	0.68	663	4.58	0.67	551	4.46	0.70	112
Anonymity ^g	4.48	0.73	663	4.49	0.72	551	4.43	0.78	112
Trust experiment ^h	4.04	0.93	660	4.04	0.93	549	4.01	0.93	111
Trust charity ⁱ	4.13	0.90	662	4.13	0.90	550	4.13	0.92	112
Deserving recipients ^j	4.47	0.81	661	4.48	0.82	549	4.42	0.79	112

^a Has children under age 16 living in household. ^b Household income. ^c Works for a not-for-profit organization. ^d Frequently attends religious services. ^e 1 if the subject encountered the donation task after the questionnaire, 0 if before. ^f “The instructions, questions, and tasks in this survey were clear and easy to understand”. ^g “The procedures followed in this experiment preserved your anonymity”. ^h “The money you donated to the charity will be given to the charity”. ⁱ “The charity will use the money to provide the chosen number of nutrition packages”. ^j “The recipients of the donations are deserving of support”.

Table 7: Between-subjects design: Nonparametric tests

Treatment	N	Checkbook		Net donations	Charity receipts		% donating
		giving			All	By donors	
		(units)	(\$)	(\$)	(units)	(units)	(binary)
		(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Summary statistics</i>							
NS	83	1.169 (1.413)	0.584 (0.706)	0.584 (0.706)	1.169 (1.413)	2.256 (1.177)	0.518 (0.503)
R33	71	1.690 (1.545)	0.845 (0.773)	0.558 (0.510)	1.690 (1.545)	2.400 (1.294)	0.704 (0.460)
M33	85	1.059 (1.339)	0.529 (0.670)	0.529 (0.670)	1.506 (2.021)	3.048 (1.886)	0.494 (0.503)
D33	90	1.478 (1.973)	0.488 (0.651)	0.488 (0.651)	1.478 (1.973)	2.771 (1.927)	0.533 (0.502)
R25	58	1.931 (1.705)	0.966 (0.853)	0.483 (0.426)	1.931 (1.705)	2.732 (1.379)	0.707 (0.459)
M25	80	1.113 (1.253)	0.556 (0.626)	0.556 (0.626)	2.225 (2.506)	3.787 (2.176)	0.588 (0.495)
D25	91	2.143 (2.831)	0.536 (0.708)	0.536 (0.708)	2.143 (2.831)	3.545 (2.879)	0.604 (0.492)
<i>Panel B: Mann-Whitney U tests</i>							
R33 vs. M33		0.01	0.01	0.21	0.06	0.28	0.01
R33 vs. D33		0.08	0.00	0.08	0.08	0.72	0.03
M33 vs. D33		0.38	0.47	0.47	0.90	0.34	0.61
R25 vs. M25		0.01	0.01	0.89	0.89	0.03	0.15
R25 vs. D25		0.39	0.00	0.39	0.39	0.65	0.20
M25 vs. D25		0.20	0.50	0.50	0.50	0.06	0.82
R33 vs. NS		0.02	0.02	0.51	0.02	0.70	0.02
M33 vs. NS		0.66	0.66	0.66	0.76	0.11	0.76
D33 vs. NS		0.66	0.27	0.27	0.66	0.51	0.84
R25 vs. R33		0.49	0.49	0.36	0.49	0.25	0.97
M25 vs. M33		0.53	0.53	0.53	0.07	0.08	0.23
D25 vs. D33		0.22!	0.61	0.61	0.22	0.41	0.34
<i>Panel C: Robust rank order tests</i>							
R33 vs. M33		0.01	0.01	0.24	0.08	0.32	0.02
R33 vs. D33		0.09	0.00	0.09	0.09	0.74	0.06
M33 vs. D33		0.42	0.51	0.51	0.91	0.38	0.66
R25 vs. M25		0.01	0.01	0.89	0.89	0.04	0.24
R25 vs. D25		0.41	0.00	0.41	0.41	0.68	0.30
M25 vs. D25		0.22	0.53	0.53	0.53	0.08	0.85
R33 vs. NS		0.03	0.03	0.54	0.03	0.72	0.05
M33 vs. NS		0.69	0.69	0.69	0.78	0.15	0.79
D33 vs. NS		0.68	0.31	0.31	0.68	0.53	0.86
R25 vs. R33		0.51	0.51	0.39	0.51	0.29	0.98
M25 vs. M33		0.56	0.56	0.56	0.09	0.12	0.30
D25 vs. D33		0.24	0.63	0.63	0.24	0.43	0.41

Shown in Panel A are mean values, with standard deviations in parentheses. Shown in Panel B and C are p -values of two-tailed Mann-Whitney U and robust rank order tests.

Table 8: Within-subject design: Nonparametric tests

Treatment	N	Checkbook		Net	Charity receipts		% donating
		giving			All	By donors ^a	
		(units)	(\$)	(\$)			(units)
(1)	(2)	(3)	(4)	(5)	(6)		
<i>Panel A: Summary statistics</i>							
NS	113	0.558 (1.026)	0.279 (0.513)	0.279 (0.513)	0.558 (1.026)	1.750 (1.105)	0.319 (0.468)
R33	113	0.867 (1.278)	0.434 (0.639)	0.286 (0.422)	0.867 (1.278)	2.130 (1.147)	0.407 (0.493)
M33	113	0.699 (1.085)	0.350 (0.542)	0.350 (0.542)	0.965 (1.614)	2.535 (1.695)	0.381 (0.488)
D33	113	0.982 (1.547)	0.324 (0.510)	0.324 (0.510)	0.982 (1.547)	2.362 (1.580)	0.416 (0.495)
R25	113	0.991 (1.373)	0.496 (0.686)	0.248 (0.343)	0.991 (1.373)	2.196 (1.233)	0.451 (0.500)
M25	113	0.805 (1.109)	0.403 (0.554)	0.403 (0.554)	1.611 (2.218)	3.434 (2.052)	0.469 (0.501)
D25	113	1.363 (1.996)	0.341 (0.499)	0.341 (0.499)	1.363 (1.996)	2.906 (2.003)	0.469 (0.501)
<i>Panel B: Subsidy type comparisons</i>							
R33 vs. M33		0.01	0.01	0.01	0.51	0.55	0.32
R33 vs. D33		0.02	0.00	0.02	0.02	0.79	0.56
M33 vs. D33		0.00	0.04	0.04	0.32	0.62	0.16
R25 vs. M25		0.00	0.00	0.00	0.00	0.00	0.56
R25 vs. D25		0.00	0.00	0.00	0.00	0.08	0.59
M25 vs. D25		0.00	0.09	0.09	0.09	0.06	1.00
<i>Panel C: Price comparisons</i>							
R33 vs. NS		0.00	0.00	0.22	0.00	0.09	0.00
M33 vs. NS		0.01	0.01	0.01	0.00	0.05	0.02
D33 vs. NS		0.00	0.62	0.62	0.00	0.05	0.00
R25 vs. R33		0.00	0.00	0.31	0.00	0.86	0.10
M25 vs. M33		0.08	0.08	0.08	0.00	0.01	0.01
D25 vs. D33		0.00	0.01	0.01	0.00	0.13	0.06

Shown in Panel A are mean values, with standard deviations in parentheses. Shown in Panel B and C are p -values of two-tailed Wilcoxon signed-rank tests. ^a p -values in this column reflect two-tailed Mann-Whitney U tests.

Table 9: Robustness check for charity receipts censored at 4 packages (BS design)

Treatment	Charity receipts	
	All (units)	By donors (units)
	(1)	(2)
<i>Panel A: Summary statistics</i>		
NS	1.169 (1.413)	2.256 (1.177)
R33	1.690 (1.545)	2.400 (1.294)
M33	1.271 (1.538)	2.571 (1.192)
D33	1.233 (1.446)	2.313 (1.188)
R25	1.931 (1.705)	2.732 (1.379)
M25	1.800 (1.702)	3.064 (1.009)
D25	1.495 (1.615)	2.473 (1.372)
<i>Panel B: Subsidy type comparisons</i>		
R33 vs. M33	0.09	0.51
R33 vs. D33	0.06	0.73
M33 vs. D33	0.87	0.31
R25 vs. M25	0.65	0.21
R25 vs. D25	0.12	0.36
M25 vs. D25	0.23	0.01
<i>Panel C: Price comparisons</i>		
R33 vs. NS	0.03	0.58
M33 vs. NS	0.66	0.22
D33 vs. NS	0.77	0.82
R25 vs. R33	0.41	0.24
M25 vs. M33	0.04	0.04
D25 vs. D33	0.25	0.53
R25 vs. NS	0.01	0.09
M25 vs. NS	0.01	0.00
D25 vs. NS	0.16	0.40

Shown in Panel A are mean values, with standard deviations in parentheses. Shown in Panel B and C are p -values of two-tailed t -tests with unequal variances

Table 10: Complete estimation results (BS design)

	Ordered Probit (Charity receipts) [All]		Probit (Donated) [All]		Ordered Probit (Charity receipts) [Donors]	
	(1)	(2)	(3)	(4)	(5)	(6)
Rebate	0.408*	0.455*	0.491*	0.710**	0.083	-0.025
	(0.178)	(0.210)	(0.209)	(0.263)	(0.231)	(0.276)
Match	-0.075	-0.147	-0.060	-0.023	-0.071	-0.335
	(0.173)	(0.211)	(0.193)	(0.247)	(0.239)	(0.299)
Discount	0.053	-0.015	0.038	0.035	0.040	-0.158
	(0.170)	(0.206)	(0.191)	(0.243)	(0.231)	(0.291)
Rebate × low price	0.152	0.151	0.008	-0.106	0.292	0.350
	(0.194)	(0.231)	(0.234)	(0.302)	(0.240)	(0.287)
Match × low price	0.097	0.193	0.236	0.338	-0.265	-0.265
	(0.173)	(0.207)	(0.196)	(0.246)	(0.237)	(0.293)
Discount × low price	0.175	0.254	0.181	0.188	0.060	0.255
	(0.164)	(0.205)	(0.188)	(0.246)	(0.218)	(0.282)
Female		0.377**		0.350*		0.362*
		(0.123)		(0.147)		(0.167)
Age:						
26–34		0.128		0.105		0.259
		(0.159)		(0.190)		(0.223)
35–54		0.245		0.186		0.373
		(0.170)		(0.205)		(0.238)
55–64		0.350		0.358		0.449
		(0.250)		(0.307)		(0.333)
≥ 65		1.211**		1.565*		0.843
		(0.422)		(0.651)		(0.489)
Married		-0.109		-0.179		0.059
		(0.147)		(0.178)		(0.198)
Children		0.051		0.206		-0.257
		(0.135)		(0.163)		(0.189)
College degree		0.062		0.198		-0.183
		(0.123)		(0.150)		(0.162)
Income (\$):						
10,000–19,999		-0.110		0.166		-0.563
		(0.271)		(0.317)		(0.382)
20,000–29,999		-0.295		-0.136		-0.326
		(0.256)		(0.299)		(0.361)
30,000–39,999		-0.160		0.065		-0.418
		(0.261)		(0.309)		(0.352)
40,000–49,999		0.318		0.655*		-0.232
		(0.245)		(0.300)		(0.315)
50,000–74,999		0.057		0.178		-0.071
		(0.239)		(0.281)		(0.324)
75,000–99,999		0.310		0.763*		-0.262
		(0.281)		(0.347)		(0.370)
100,000–150,00		0.385		0.506		0.066
		(0.273)		(0.331)		(0.356)
> 150,000		-0.048		-0.034		-0.051
		(0.419)		(0.478)		(0.592)
Residential environment:						
Suburban		-0.036		-0.041		0.014
		(0.156)		(0.192)		(0.204)
Urban		-0.068		-0.101		-0.017
		(0.174)		(0.212)		(0.230)
Registered voter		-0.342*		-0.594**		0.098
		(0.169)		(0.221)		(0.214)
Not-for-profit		0.404		0.116		0.955**
		(0.228)		(0.275)		(0.311)
Religious		0.157		0.223		0.020
		(0.195)		(0.241)		(0.257)
Religion:						
Agostic		0.247		0.368		-0.258
		(0.217)		(0.273)		(0.288)

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Roman-Catholic	0.245 (0.191)	0.546* (0.244)	-0.228 (0.242)			
Protestant	0.478* (0.203)	0.455 (0.247)	0.429 (0.268)			
Other Christian	0.103 (0.193)	-0.017 (0.230)	0.240 (0.273)			
Other Religion	0.068 (0.201)	0.083 (0.238)	0.060 (0.287)			
Task order	-0.234* (0.115)	-0.222 (0.138)	-0.149 (0.159)			
Risk pref.	0.018 (0.033)	-0.020 (0.039)	0.078 (0.047)			
Big Five:						
Extraversion	-0.054 (0.039)	-0.057 (0.048)	-0.088 (0.055)			
Agreeableness	-0.017 (0.051)	0.029 (0.061)	-0.121 (0.073)			
Conscientiousness	-0.039 (0.050)	0.028 (0.060)	-0.157* (0.073)			
Emotional stability	-0.033 (0.045)	-0.131* (0.055)	0.184** (0.062)			
Openness	0.100* (0.050)	0.102 (0.061)	0.149* (0.071)			
Constant	0.070 (0.125)	0.075 (0.486)	0.045 (0.138)	-0.007 (0.580)	0.331 (0.172)	0.143 (0.679)
α_1	0.550*** (0.046)	0.607*** (0.057)				
α_2	1.012*** (0.061)	1.129*** (0.076)		0.686*** (0.066)	0.790*** (0.084)	
α_3	1.146*** (0.065)	1.299*** (0.082)		0.859*** (0.073)	1.017*** (0.094)	
α_4	1.298*** (0.083)	1.466*** (0.106)		1.055*** (0.099)	1.256*** (0.135)	
α_5	1.384*** (0.100)	1.594*** (0.135)		1.160*** (0.120)	1.428*** (0.174)	
N	558	428	558	428	326	256
Log likelihood	-801.28	-590.10	-372.07	-248.44	-427.37	-314.64

Standard errors reported in parenthesis, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The no subsidy control treatment (NS) is used as baseline.

Table 11: Robustness Checks – Ordered Probit (BS design)

	Omit		Include		Heteroscedasticity		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Marginal effects</i>							
Rebate	0.546** (0.237)	0.558** (0.254)	0.546** (0.237)	0.561** (0.255)	0.605** (0.254)	0.616** (0.244)	0.343* (0.209)
Match	0.323 (0.261)	0.242 (0.285)	0.324 (0.261)	0.242 (0.285)	0.28 (0.303)	0.286 (0.316)	0.154 (0.212)
Discount	0.304 (0.247)	0.191 (0.269)	0.306 (0.247)	0.199 (0.27)	0.248 (0.313)	0.178 (0.3)	-0.39** (0.184)
Rebate × low price	0.218 (0.277)	0.174 (0.298)	0.218 (0.277)	0.183 (0.298)	0.003 (0.312)	0.046 (0.293)	-0.117 (0.236)
Match × low price	0.887** (0.372)	1.062*** (0.404)	0.888** (0.373)	1.067*** (0.405)	0.983** (0.41)	0.849** (0.404)	0.709** (0.3)
Discount × low price	0.556* (0.326)	0.619* (0.363)	0.581* (0.326)	0.667* (0.364)	0.55 (0.393)	0.443 (0.377)	0.883*** (0.229)
<i>Subsidy type comparisons</i>							
R33 vs. M33	0.43	0.29	0.43	0.29	0.28	0.29	0.4
R33 vs. D33	0.37	0.2	0.37	0.21	0.27	0.14	0
M33 vs. D33	0.95	0.87	0.95	0.89	0.92	0.74	0
R25 vs. M25	0.23	0.15	0.23	0.16	0.11	0.21	0.02
R25 vs. D25	0.78	0.83	0.72	0.74	0.65	0.92	0.3
M25 vs. D25	0.38	0.26	0.42	0.32	0.3	0.22	0.17
Covariates ^a	No	Yes	No	Yes	Yes	Yes	Yes
Heteroscedasticity	No	No	No	No	Yes	Yes	Yes
N	557	427	558	428	428	428	428
Log likelihood	-797.07	-585.76	-803.24	-592.06	-584.09	-575.30	-554.95

In all models the dependent variable is the number of packages selected by the individual. The no subsidy control treatment (NS) is used as baseline. (1)–(2) omit the observation with 5 selected packages. (3)–(4) explicitly include the possibility to give 5 packages in the model. (5)–(7) expand the main model by heteroscedasticity. The variance is modeled as $\exp(z_i p)$ where z_i does not include a constant. In (5) z_i includes age, income, sex, religious and task order, in (6) it includes all covariates but the Big Five and risk preferences, and in (7) it includes all covariates. Using a likelihood ratio test the main model with homoscedasticity is not rejected when compared to (5) or (6), but is rejected when compared to (7), $p < 0.01$.

Panel A presents average of the sample marginal effects, standard errors are reported in parenthesis, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Marginal effects* refer to the average change in expected charity receipts over all individuals. For each individual the change is calculated by taking the difference in expected charity receipts between receiving a particular subsidy at the low rate (rebate, match, discount) and not receiving any subsidy or between receiving a particular subsidy at the high rate (rebate × low price, match × low price, discount × low price) and receiving the same subsidy at the low rate.

Panel B presents p -values for the pairwise comparison of treatment effects (subsidy treatment vs. no subsidy) between subsidy types, based on the average of the sample marginal effects.

^a Covariates comprise all variables reported in Table 5 except for the manipulation check items.

Table 12: Robustness Checks – OLS and Tobit (BS design)

	OLS		Tobit			
	(1)	(2)	(3)	(4)	(5)	(6)
Rebate	0.513*	0.64**	0.568**	0.61**	0.585**	0.648**
	(0.29)	(0.267)	(0.259)	(0.277)	(0.241)	(0.257)
Match	0.207	0.001	0.322	0.252	0.038	0.006
	(0.332)	(0.286)	(0.276)	(0.3)	(0.234)	(0.257)
Discount	0.151	0.033	0.382	0.273	0.126	0.053
	(0.317)	(0.28)	(0.273)	(0.297)	(0.232)	(0.254)
Rebate \times low price	0.219	0.021	0.25	0.206	0.184	0.117
	(0.33)	(0.29)	(0.298)	(0.32)	(0.269)	(0.288)
Match \times low price	0.916**	0.6**	0.695*	0.868**	0.391	0.512*
	(0.391)	(0.3)	(0.36)	(0.389)	(0.257)	(0.278)
Discount \times low price	0.846**	0.35	0.68**	0.787**	0.322	0.37
	(0.42)	(0.294)	(0.346)	(0.385)	(0.247)	(0.277)
Covariates	Yes	Yes	No	Yes	No	Yes
N	428	428	558	428	558	428

Standard errors are reported in parenthesis, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. For (1) robust standard errors are reported. The no subsidy control treatment (NS) is used as baseline. (1),(3) and (4) use charity receipts as dependent variable. All other models use the logarithm of charity receipts and charity receipts of zero are recoded as $\log(0.1)$. For (3)–(6) values refer to the average of the sample marginal effects, which are based on the average change in the dependent variable over all individuals. For each individual the change is calculated by taking the difference in the dependent variable between receiving a particular subsidy at the low rate (rebate, match, discount) and not receiving any subsidy or between receiving a particular subsidy at the high rate (rebate \times low price, match \times low price, discount \times low price) and receiving the same subsidy at the low rate.

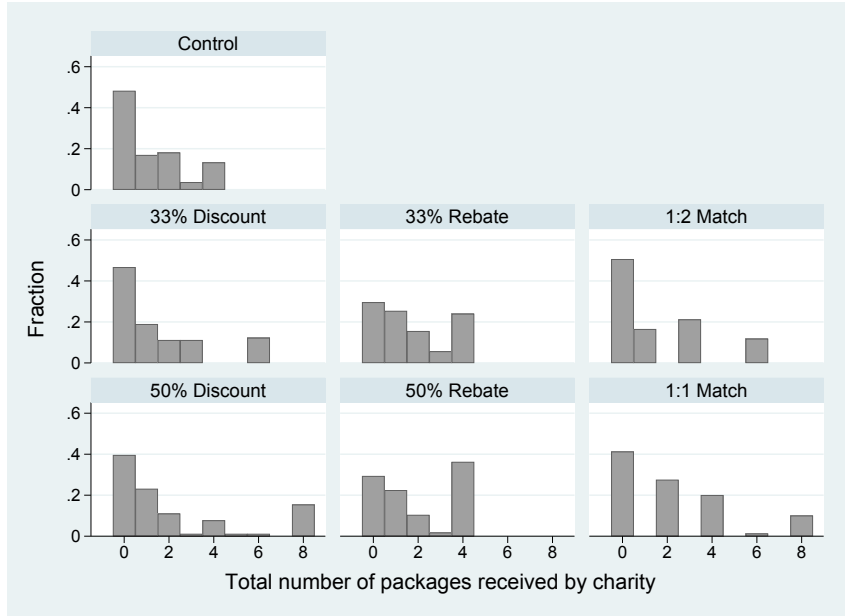


Figure 1: Distribution of charity receipts (BS design)

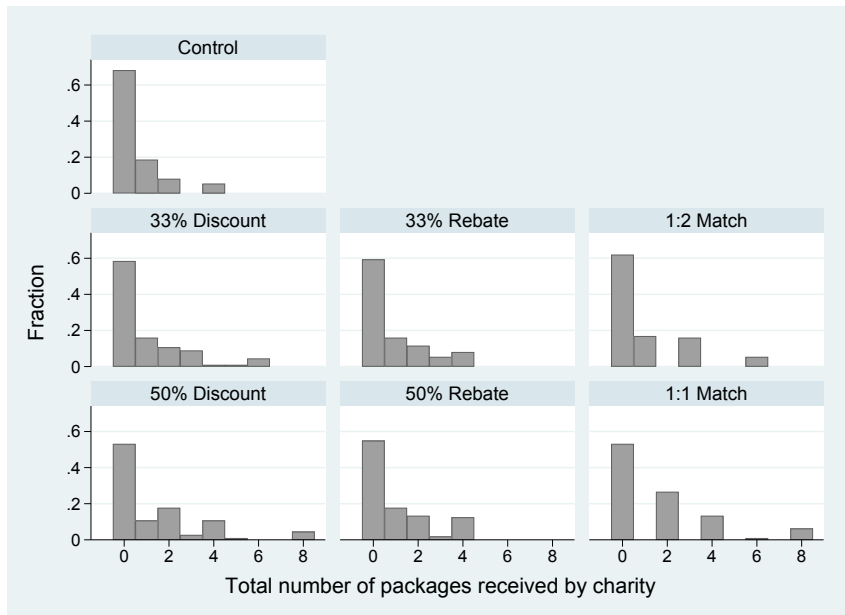


Figure 2: Distribution of charity receipts (WS design)

Appendix C Instructions (screen shots)

In this survey, each participant will have the opportunity to provide nutritional packages for malnourished children in the African country of South Sudan.



Decades of civil war have ravaged South Sudan, and many children are severely malnourished. The nutritional packages will be delivered by Sign of Hope, an accredited relief organization that operates two hospitals in South Sudan.

In 2010, Sign of Hope won the Transparency Award for German non-profit organizations. Eighty cents out of every dollar they receive go directly to relief efforts, while the remaining twenty cents cover their overhead costs. You can read more about the organization on <http://www.hoffnungszeichen.de/sign-of-hope-africa.html>.



One nutritional package, which feeds one malnourished child for one day, can be provided by the charity for a donation of \$0.50. The package consists of a specially designed paste and high-energy cookies to help the children gain weight.

In this survey, you will be able to provide these nutritional packages for **\$0.25** apiece (a third party will fund the remaining \$0.25). You may use all, part, or none of your reward of \$2.00 for this HIT to provide packages. Thus, you may choose any number between 0 and 8 packages. \$0.25 per package will be subtracted from your reward.

Please indicate your choice below:

Figure 3: Donation question in the BS design, treatment D25. The final paragraph differed between treatments; corresponding phrases can be found in Figure 4.

In this survey, each participant will have the opportunity to provide nutritional packages for malnourished children in the African country of South Sudan.



Decades of civil war have ravaged South Sudan, and many children are severely malnourished. The nutritional packages will be delivered by Sign of Hope, an accredited relief organization that operates two hospitals in South Sudan.

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One nutritional package, which feeds one malnourished child for one day, can be provided by the charity for a donation of \$0.50. The package consists of a specially designed paste and high-energy cookies to help the children gain weight.

In this survey, you may use all, part, or none of your reward of \$2.00 for this HIT to provide these nutrition packages.

Below, you find seven different price conditions. For each condition, please indicate the number of packages you choose to provide under this condition.

One of the seven price conditions will be implemented in real. This condition will be randomly selected through a lottery after this survey. So, since each case may become real with equal probability, you will want to answer each condition as if it is the condition that will be implemented .

Your choice:

Condition C:

In this condition, you will be able to provide these nutritional packages for **\$0.50 apiece**. Thus, you may choose any number between 0 and 4 packages. \$0.50 per package will be subtracted from your reward.

Condition E:

In this condition, you will be able to provide these nutritional packages for **\$0.33 apiece** (a third party will fund the remaining \$0.17). Thus, you may choose any number between 0 and 6 packages. \$0.33 per package will be subtracted from your reward.

Condition D:

In this condition, you will be able to provide these nutritional packages for **\$0.25 apiece** (a third party will fund the remaining \$0.25). Thus, you may choose any number between 0 and 8 packages. \$0.25 per package will be subtracted from your reward.

Condition R:

In this condition, you will be able to provide these nutritional packages for \$0.50 apiece. Thus, you may choose any number between 0 and 4 packages. \$0.50 per package will be subtracted from your reward. A third party has agreed to fund a **33% rebate** for each package you provide. Upon completion of the survey, the rebate (\$0.17 per package provided) will be added to your reward.

Condition S:

In this condition, you will be able to provide these nutritional packages for \$0.50 apiece. Thus, you may choose any number between 0 and 4 packages. \$0.50 per package will be subtracted from your reward. A third party has agreed to fund a **50% rebate** for each package you provide. Upon completion of the survey, the rebate (\$0.25 per package provided) will be added to your reward.

Condition M:

In this condition, you will be able to provide these nutritional packages for \$0.50 apiece. Thus, you may choose any number between 0 and 4 packages. \$0.50 per package will be subtracted from your reward. A third party has agreed to **match every two packages you provide**, at no additional cost to you. So, for example, if you choose to provide 2 packages, Sign of Hope will receive 3.

Condition N:

In this condition, you will be able to provide these nutritional packages for \$0.50 apiece. Thus, you may choose any number between 0 and 4 packages. \$0.50 per package will be subtracted from your reward. A third party has agreed to **match each package you provide**, at no additional cost to you. So, for example, if you choose to provide 2 packages, Sign of Hope will receive 4.

Figure 4: Donation question in the WS design