Comparing rebate and matching subsidies controlling for donors' awareness: Evidence from the field

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Abstract: This paper compares the effectiveness of rebate and matching subsidies in the field and, to our knowledge, is the first to control for potential bias introduced by the failure to account for donors' awareness of the offered subsidies. Where previous field experiments have typically been limited to either rebate subsidies or matching subsidies, we study both types and determine whether donors are aware of any offered subsidy. We provide evidence that this methodological shortcoming (i.e., the loss of control) is not trivial. Our findings suggest that the assumption in earlier field studies, that the offered price is equal to the perceived or actual price, is likely incorrect and may result in underestimation of the price elasticities of giving. This set of results has strong implications for the design of effective subsidies in a variety of decision settings. In addition, our results serve to validate the lab studies' finding that matching subsidies are more powerful than rebate subsidies of equivalent cost at increasing total giving to charities.

Key words: Charitable giving, field experiment, subsidies

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1. Introduction

Standard economic theory would imply that rebate and matching subsidies with the same impact on the price of giving should be equally successful in increasing giving to nonprofit organizations; rebating a portion r of a donation is equivalent to matching a donation at the rate $m = r/(1-r)$. Several recent studies compared the two subsidy types in laboratory experiments and generally found that the two do not produce equivalent results (Eckel and Grossman, 2003, 2006a, 2006b; Davis, Millner and Reilly, 2005; Davis and Millner, 2005).¹ Field tests of the effectiveness of matching subsidies on charitable giving have been conducted by Huck and Rasul (2007, 2011); Rasul and Huck (2010), Karlan and List (2007) and Meier (2007).² Eckel and Grossman (2008), to our knowledge the only field experiment to examine the impact of rebate subsidies or to compare rebates to matches, also reports results qualitatively if not always quantitatively similar to laboratory results. Field experiments differ in important ways from lab experiments, and a choice between the two methodologies involves tradeoffs.³ The comparability

¹ An exception to this pattern is Davis (2006), who presents the two subsidies in a novel decision frame that eliminates observed differences between subjects' responses in the lab.

² Huck and Rasul (2007 2011) found that matches at the rate of either 50 or 100 percent by a "lead donor" significantly increased the total donation received by their nonprofit organization (the Bavarian State Opera), but partially crowd out own giving. Rasul and Huck (2010) explore the role of transactions costs in response to fundraising campaigns. Karlan and List (2007) examine the effect of matching subsidies (at rates of 100, 200 and 300 percent) on the response rate and amount of giving, and found that the fact of a subsidy, but not its level, positively affects contributions. Finally, Meier (2007) looked at the long-term impact of a one-time matching subsidy (at the rates of 25 or 50 percent) on giving and found that the matching offers increased giving to the public good in the short term, but in subsequent periods giving by those in the treatment group declined. There is a considerable literature that addresses the rebate aspect of the federal income tax code regarding charitable giving (see, for example, Auten et al. 2002, and Clotfelter, 1989).

³ Harrison and List (2004, p. 1012) identify six dimensions on which lab and field experiments can differ: the nature of the subject pool, information that the subjects bring to the task, the commodity, the task or trading rules applied, stakes levels, and the environment that the subject operates in. (For a discussion of these factors see also Carpenter, et al. 2005). The important differences for our study are the first two and last two factors. The subject pool consists of adults of all ages and income levels instead of the usual convenience sample of university students; donors probably know more about the activities of the charity than the descriptions provided to lab subjects; and decisions

between laboratory and field experiments is, however, open to question and is the focus of this study.

Lab experiments allow for a great deal of control over the decision environment, but at a cost, since lab decisions are necessarily artificial in some respects. It is this control issue that motivates this study. The superior level of control in the lab experiments, i.e., the fact that researchers know their subjects have read and understood the instructions, eliminates uncertainty about which price subjects considered when making their giving decisions. In the lab, instructions are typically read aloud, examples are given, and procedural questions answered. Field researchers, on the other hand, are unable to ascertain with any degree of certainty that subjects actually saw the subsidy offer, understood its impact on them, and took it into account when making their decisions.

In most field studies of subsidies to charitable giving, solicitations announcing the subsidies are mailed in either the solicitation letter or an included insert, leaving open the possibility that the offer goes unnoticed by the decision maker. It is also possible that the offer is noticed but not understood. It could then be ignored in the decision making process or if not ignored introduces noise into the data. As such, field researchers are unable to ascertain with any degree of certainty that subjects actually saw the subsidy offer, understood its impact on them, and took it into account when making their decisions. This limits the inferences that can be made about the effect of such subsidies, and raises questions about the comparability of lab and field studies, since those unaware of the subsidy obviously cannot respond to it. It is also possible, for reasons we discuss below, for the decision maker to reject the subsidy. Lacking any means to determine if an offer has been noticed and accepted, a researcher has little choice but to use the offered

are made using subjects' own money instead of ours, generally involving contributions that are larger than the lab-provided stakes.

price (the price of giving including the subsidy) to estimate the price elasticity of giving, assuming in effect that everyone in a treatment group is “treated,” and thereby estimating incorrectly the responsiveness of giving to the subsidy offers.

This study provides evidence that this methodological shortcoming (i.e., the loss of control) is not trivial. We require subjects to check a box on the pledge form in order to receive the offered subsidy. Checking the box indicates that they have read the enclosed materials and wish to receive the subsidy. Our procedure can only determine that the decision maker was both aware of the offer and chose to accept it. Given this, we assume that the offer price influenced the donation decision for these subjects. Thus we are able to distinguish between respondents who accept the subsidy and make their decision with the subsidy in mind, and respondents who do not, either because they are unaware of the offer or consciously reject it. We are unable to distinguish between donors who are aware of the offered subsidy and choose not to take it from those who are just unaware. Furthermore, we are unable to ascertain if our subjects who accepted the subsidies truly understood the enclosed materials. Because we are unable to separate those who are unaware from those who knowingly reject the subsidy, we treat them alike in our analysis below and refer to both groups as respondents who did not accept the subsidy. Importantly, our method allows us to gauge the impact of the subsidy on those who actually receive it.

Our findings suggest that the assumption in earlier field studies, that the offered price is equal to the perceived or actual price, is likely incorrect and may result in substantial underestimation of the price elasticities of giving. In addition, our results serve to validate the lab studies’ finding that matching subsidies are more powerful than rebate subsidies of equivalent cost at increasing total giving to charities. This result confirms similar findings on

savings decisions (Duflo, et al, 2006; Saez 2009), where matching subsidies are shown to have a stronger effect on take up rates and savings levels (including the subsidy) than rebate or credit subsidies. Taken together, this set of results has strong implications for the design of effective subsidies in a variety of decision settings.

2. Previous Work

It is straightforward to show that the effect on total charitable giving (i.e., the amount actually received by the charity) of a rebate subsidy of rate r should be equivalent to that of a match subsidy of rate $r/(1-r)$. Both laboratory and field experiments have, however, found that total giving is greater with match subsidies than their functionally equivalent rebate subsidies. Furthermore, the price elasticity of giving is significantly larger for match subsidies than rebate subsidies. Eckel and Grossman (2003 and 2006b) compared equivalent rebate and matching subsidies in controlled laboratory settings. In every pairwise comparison, the dollar value of the total donation received by the charity was significantly greater under the matching subsidy than under the rebate subsidy. Estimated match subsidy price elasticities were two to three times as large as the estimated rebate subsidy elasticities. For example, Eckel and Grossman (2003) report rebate and match price elasticities of -0.34, and -1.07, respectively.

Eckel and Grossman (2008) conduct a field study to determine if the laboratory results are replicated in the field. The field experiment was conducted in conjunction with an annual fund drive by Minnesota Public Radio (MPR). Solicitations were mailed to three different categories of (potential) donors: continuing members (persons who make regular contributions and maintain membership), lapsed members (persons who have in the recent past been members but have let their memberships lapse), and prospects (persons who have never been members and have no history of contributing to MPR). It had three main treatments: a baseline with no

subsidy, a rebate to donors of a portion of their contributions to the charity, and equivalent matching subsidies. Eckel and Grossman assumed that all respondents were aware of any offered subsidy and that all donations were conditioned on the after subsidy price of giving. The rebate and match subsidy rates were the same rates used in the laboratory experiments (rebate rates: 20 and 25 percent; match rates: 25 and 33 1/3 percent). They report results that are both qualitatively and quantitatively similar to lab results; the reported rebate and match price elasticities are -0.11, and -1.05, respectively. For comparison with the LSS results, we re-estimated the MPR regressions for continuing members only. The reasons and results are discussed below.

Other field experiments (Huck and Rasul, 2007, 2011; Rasul and Huck, 2010; Karlan and List, 2007; Meier, 2007; and Meier and Frey, 2003) examining subsidization of charitable contributions focused on match subsidies only, and of these only Huck and Rasul (2011) and Karlan and List (2007) estimated price elasticities of giving. Huck and Rasul (2011) report price elasticities between -0.53 and -1.12; Karlan and List report price elasticities between -0.225 and -0.668. In both cases, the authors assume that all respondents were aware of any offered subsidy and that all donations were conditioned on the after-subsidy price of giving.

The studies by Huck and Rasul (2011) and Karlan and List (2007) are most similar to our own, though neither study addresses the comparison between rebate and matching subsidies. Huck and Rasul (2011) address an important potential confound in assessing the impact of price subsidies relative to a baseline with no subsidies. The presence of a “lead donor” or “concerned member” – or, in our case, “researcher,” implies an endorsement of the quality of the charity. Such an endorsement may be interpreted by prospective donors as a signal of the quality of the charity, and may increase both the probability of making a donation and the size of the donation,

independent of any matching offer. Prospective donors may assume that the endorser has vetted the organization and deemed it worthy of support; they would be unlikely to generously support a bad charity. Huck and Rasul (2011) disentangle these two effects by including a no-subsidy treatment with a “lead donor” who has already provided a substantial portion of the required budget. Their results suggest that the quality signal implicit in the endorsement by a major donor has a substantial effect on subsequent donors, almost doubling average donations received relative to the baseline. Important for our study is the fact that any endorsement effect in our design is common to both treatments, and does not affect the comparison between the two.

3. Field Study Procedures

The field study was conducted with the cooperation of Lutheran Social Service (LSS) as part of regular LSS mailed fundraising drives. Mailings to past contributors occurred in November-December 2003 and May-July 2004. Targeted past contributors were randomly allocated to one of five treatments: No Subsidy, 20% rebate, 25% match, 25% rebate, and 33 1/3% match. Included in the solicitation was a flyer announcing either the rebate or matching offer and providing relevant details (see Appendix for examples of the flyers); no flyers were included in solicitations mailed to the no subsidy donors. Similar to Karlan and List (2007), the flyers explain that any subsidy comes from the researchers, not the charity, and that any unclaimed subsidies will not accrue to the charity.⁴ It is stated clearly in the flyers that the subsidy is funded by researchers. Of course, as noted above, the presence of a subsidy may send a signal to prospective donors that Lutheran Social Services is a worthy recipient of their charitable dollars.

⁴ In a lab experiment, Chavanne et al. 2011 show that rebates increase giving only when the subsidy comes from outside the group. We take this to mean that subsidies that explicitly come from outside the organization are likely to be more effective.

As such, we are unable to separate the subsidy incentive effect from the signaling effect.⁵ It should not, however, affect our comparison of rebate and matching subsidies.

A cover letter, common to all treatments, contained a request to complete and return an enclosed survey, which collected socio-economic data, a subject's pattern of charitable giving, and a measure of the subject's perceived benefits from the charity. To encourage recipients to complete the survey, the survey form indicated that \$5 would be donated to LSS for every completed survey returned. Budget limitations required the overall maximum paid out in rebates, matches, and survey incentives to be capped and donors were informed of these limits.⁶

The unique feature of our design is a check box amended to the usual LSS pledge forms, on which donors indicated their contributions. The check box was added along the top of the standard pledge form used by LSS. Donors were required to mark the box if they wished to receive the offered subsidy and were referred to the included materials that explained the subsidy offer. The check box was included to ensure that donors were aware of the subsidy possibility, and, if not, to draw their attention to the enclosed flyer. Previous field studies did not contain this feature, making it impossible to know if donors were aware of the subsidy when making their decisions.⁷ Thus we are able to examine donations contingent on the decision to accept the offered subsidy, as well as the characteristics of donors who accept the subsidy.

⁵ Lab experiments would suffer from this same problem. The choice of a charity (or charities) to include may be perceived by the subjects as a signal that the researchers deemed this charity (these charities) to be worthy of supporting.

⁶ The subsidy-specific caps were stated on the respective flyers -- 20% rebate: \$5,000; 25% match: \$10,000; 25% rebate: \$8,000; and 33 1/3% match: \$15,000. These limits were informed by the researchers' experience from a prior field experiment (Eckel and Grossman, 2008). In practice, the limits were nonbinding.

⁷ In the studies of savings subsidies, subjects were informed face-to-face about the availability and nature of the subsidy, so there was no possibility that they could fail to be aware of it (Duflo, et al. 2006; Saez, 2009). Including the checkbox makes comparison of the charitable giving and savings studies possible.

4. Results

4.1 Summary Statistics

Table 1 reports solicitations, mean revenue per solicitation, number of respondents, number of survey only respondents, number of donor respondents, and number of donors who checked the box, all by price/subsidy type. A total of 24,116 solicitations were mailed with 1207 complete responses received, an overall response rate of 5.0 percent.⁸ This includes people who returned the survey only (338), as well as people who made positive donations (869).⁹ Information provided by LSS for eighteen appeals between December 2002 and December 2006 suggests that our 5 percent response rate is within their normal range.¹⁰ For the eighteen appeals, the response rates varied from as little as 1.3 percent to as high as 16.4 percent with an overall average of 4 percent.

The offer of a subsidy does not have a significant effect on the decision to respond. The response rates range from a high of 5.4 percent for the no subsidy and 20 percent rebate categories to 4.3 percent for the 25 percent rebate, but a χ^2 contingency table test finds these differences to be statistically insignificant.¹¹ Seventy-two percent of all respondents made a donation. No subsidy respondents are the least likely to make a donation; 33 1/3 percent match respondents are the most likely to give. Respondents from the treatment group that saw the 33 1/3 percent match are, according to Fisher's exact tests, significantly more likely to make

⁸ An additional 247 responses (17 percent of total responses) were received but excluded because of incomplete information. Mean giving for included and excluded observations did not differ significantly. Including these extra observations (where possible) when calculating summary statistics does not alter the reported results.

⁹ Subject characteristics by treatment are reported in Appendix Table 1, and by responses in Appendix Table 2.

¹⁰ Correspondence with Steve Griffith of Lutheran Social Services.

¹¹ Donation rates (number of donations divided by solicitations) are 3.6 percent overall, and range between 3.1 percent for the 25 percent rebate to 4.0 percent for the 33-1/3 percent match, but the differences are not significant. Our response rates are similar to Huck and Rasul's (2007) response rates, which varied from 3.5 to 4.7 percent, depending upon the treatment.

Table 1: Solicitations, Respondents, Donations, and Checked Boxes

	Solicitations	Mean Revenue per Solicitation^a	Respondents (% of solicitations)^b	Survey Only (% of respondents)^c	Donation > \$0 (% of respondents)	Donors who check box (% of donors)^d
No-subsidy	5501	\$2.28	297 (5.4%)	92 (31.0%)	205 (69.0%)	N/A
20% Rebate	4463	\$2.30	242 (5.4%)	70 (28.9%)	172 (71.1%)	67 (39.0%)
25% Match	4462	\$3.16	216 (4.8%)	65 (30.1%)	151 (69.9%)	112 (74.3%)
25% rebate	4856	\$2.03	208 (4.3%)	59 (28.4%)	149 (71.6%)	57 (38.3%)
33 1/3% Match	4834	\$3.36	244 (5.0%)	52 (21.3%)	192 (78.7%)	140 (72.9%)
Total	24116	\$2.61	1,207 (5.0%)	338 (27.9%)	869 (72.1%)	N/A
Total Subsidy Treatments Only	18615	2.71	910 (4.9%)	246 (27.0%)	664 (73.0%)	376 (56.6%)

^a For mean revenue per solicitation figures we used all responses with contributions that could be positively allocated to a treatment group even though they might have been excluded from subsequent analysis for varying reasons (n = 247). The figures do not include the \$5 per survey completion payment, or any matching contributions.

^b Percentage respondents is not significantly different across treatments using a chi-square contingency table test ($\chi^2(4) = 9, p > .05$).

^c Subjects who returned the survey but whose own donation = \$0.

^d Fisher's exact test indicates a significant difference in the acceptance rate between the match and rebate subsidies, $p < .0001$

donations than subjects in any other treatment.¹²

Mean revenue per solicitation (RPS hereafter) varied from \$2.03 (25 percent rebate) to \$3.36 (33 1/3 percent match). These figures represent only out-of-pocket donations. They exclude the \$5 per survey completion payment as well as any matching contributions. Mean RPS for the No Subsidy category did not differ significantly from the mean RPS of those offered a rebate subsidy, but was significantly less than the mean RPS of those offered a match subsidy. The mean RPS of those offered a rebate subsidy was also significantly less than the mean RPS of those offered a match subsidy.¹³ This result shows that the presence of a match increases own initial giving by potential donors. Since neither the response rate nor the overall donation rate per solicitation differs significantly across categories, this difference can be attributed to higher contributions by those who make the decision to donate.

However, of the respondents who donated to LSS, not all accepted the offered subsidy. Of the 664 donors in the four subsidy treatments, 376 (56.6 percent) checked the box on their pledge forms indicating that they wanted to receive the offered subsidy. For both rebates and matches the failure to check the accept-the-subsidy box indicates an unawareness of the offered subsidy, a conscience decision not to accept the offered subsidy, misperceptions concerning the offer, or a reluctance to deal with other costs (for the rebate subsidies) associated with the offers.¹⁴ Donors in the match treatments are almost twice as likely to accept the subsidy as their counterparts in

¹² All pairings of the proportion of donors in the 33 1/3 percent match with the other price/subsidy categories had Fisher's Exact Test p-values < 0.02. Meier and Frey (2003) report a similar result (a higher willingness to give by subjects offered a 33 1/3 percent match than subjects offered a 25 percent match).

¹³ Results available on request.

¹⁴ Subjects may reject the offered subsidy due to a reluctance to deal with something new or different. Consistent with this hypothesis, Eckel and Grossman (2008) report that MPR's Director of Membership Marketing observed that a seemingly innocuous change in MPR's solicitation appeal -- merely adding a 1-800 number to the pledge form -- resulted in a significant drop in their response rate. Compounding this reluctance is that approximately two-thirds of our sample is age 60 or older (see Appendix Table 1) and evidence suggests that people become less open to novelty and change as they age (See, for example, Srivastava et al. 2003, Roberts et al. 2006, and Westerhoff, 2008). Alternatively subjects may not have understood how the subsidy would affect them and the charity. Finally, a rebate subsidy entails extra costs associated with keeping track of and cashing a rebate check.

the rebate treatments (73 percent versus 38 percent).¹⁵ A Fisher's exact test indicates that this difference is significant and that the decision to check the box is not independent of the subsidy type offered.

4.2 Nonparametric Tests

We next consider nonparametric analysis of the donations. Table 2 reports median total donations by those who did and did not accept the subsidies, by subsidy category. For all

Table 2: Median Donations for Donors Who Failed to Accept and Who Accepted Offered Subsidy by Category *

	Did Not Accept Subsidy		Accepted Subsidy
	Median Donation ^a		Median Donation ^{ab}
	All Respondents N	Donors Only N	N
20% Rebate	\$10 175	\$25 105	\$50 67
25% Match	\$0 104	\$25 39	\$62.50 112
25% Rebate	\$10 151	\$25 39	\$50 57
33 1/3% Match	\$2.50 104	\$25 52	\$66.67 140
No Subsidy	\$20 297	\$25 205	

* For all subsidy categories, the difference in median donations for those who accepted the the subsidy and those who did not (both all and donors only), Median and Wilcoxon test results were significant with p-values < 0.05 or better.

^a – Excludes \$5 for returning the survey.

^b – Includes any matching contribution. Without matching contributions, the median donation would be \$50 for both match subsidies. Test results would not change.

respondents (donors only respondents) who did not accept the offered subsidies, median giving was \$10 (\$25) for both of the Rebate categories, \$0 (\$25) for the 25% Match, and \$2.50 (\$25)

¹⁵ A matching contribution in response to one's own contribution may be easier to accept than a rebate of one's own contribution, especially from a charitable organization.

for the 33 1/3% Match. For those respondents who accepted the offered subsidies, median giving for both the 20% and 25% Rebate subsidies was \$50 and for the 25% and 33 1/3% Match subsidies it was \$62.50 and \$66.67, respectively. Both median tests indicate that median total donations for respondents who accepted the offered subsidies are significantly different from median total donations for respondents who rejected the subsidies - for both all respondents and donor only respondents (p-values < 0.05).¹⁶ Even if the matching contributions are excluded, the median donation by those who accepted the subsidies significantly exceeded the median donation for donors who did not accept the subsidies (p-values < 0.05). These findings are reaffirmed by the Wilcoxon two-sample test results. For each subsidy category, the null hypothesis that the two distributions come from the same population is rejected.¹⁷

Another interesting finding is that the No Subsidy respondents are consistently more generous than all respondents who did not accept the offered subsidies, regardless of type or subsidy level. The median level of giving was \$20 for the No Subsidy respondents. The differences are significant for the two match subsidies (p-values at least < 0.02 for both the Wilcoxon and median tests) but insignificant for the rebate subsidies.¹⁸ This implies that there are some differences between the donors and nondonors within the subsidy treatments. We return to this question below in an analysis of the decision to accept the subsidy.

4.3 Comparing Elasticity Estimates for LSS, Minnesota Public Radio and Lab Studies

In the following analysis we evaluate the results of this study relative to our earlier field (Eckel and Grossman, 2008) and lab experiments (Eckel and Grossman, 2003 and 2006b)

¹⁶ Results available upon request.

¹⁷ The same conclusions hold even if the nondonors are dropped from the sample.

¹⁸ Results available upon request.

comparing rebate and match subsidies.¹⁹ For purposes of comparison, we estimate three models (1-3) for the LSS data. For the first two LSS models, we assume that if a donation were made the offered price was the perceived or accepted price – that is, that all subjects in the treatment group were treated. The dependent variable in both models is DONATE1, defined as the log of \$5 plus own giving plus any matching amount (see Table 3 for a definition of all variables used in this and subsequent LSS regressions).²⁰ The first LSS model is estimated for the full sample including respondents with out-of-pocket donations of \$0 (i.e., all respondents); the second LSS model is estimated for a sample that excludes \$0 donation respondents, recognizing

Table 3: Regression Variables and Their Definitions

Variable Name	Definition
ACCEPT	= 1 if box was checked
REBATE	= 1 if rebate subsidy was offered
COLLEGEGRAD	= 1 if respondent had a college degree or higher
DONATE	= 1 if out-of-pocket donation was > 0
DONATE1	= log of the total LSS would have received if it was assumed that all donors accepted the offered subsidy (includes any match) + \$5
DONATE2	= log of the total received by LSS (includes any match for donors who checked box) + \$5
R20	= 1 if offered the 20% rebate subsidy
R25	= 1 if offered the 25% rebate subsidy
M25	= 1 if offered the 25% match subsidy
M33	= 1 if offered the 33 1/3% match subsidy
INCOME	= log of respondent's income in thousands (defined as the midpoint of the subject's income range and = \$155,000 for those with income > \$140,000);
ITEMIZE	= 1 if respondent itemizes on his/her federal income tax
ATTEND	= 1 if subject attended religious services

¹⁹ Our lab data and our LSS field data are not perfectly comparable. In the lab experiments subjects are not offered the option of accepting/rejecting the offered subsidies. They are fully informed about any subsidies and know that the subsidies will be applied to their decisions.

²⁰ The \$5 is added because by returning the completed survey, the respondent causes \$5 to be given to LSS. Therefore the Tobit regressions are censored on the left at the log of \$5. Note that we add in the subsidy for all contributions by those in the matching treatment, regardless of whether they accepted the subsidy to make this parallel to the MPR data.

that the MPR sample only included respondents with non-zero donations (i.e., donors only). Model 3 estimates the corrected price elasticities and is discussed in detail in section 3.4 below. The MPR model re-estimates Eckel and Grossman's original model for the subsample of continuing members (i.e., donors who contribute annually to MPR) only (recognizing that the LSS sample is comprised entirely of prior donors).

The LSS regressions control for whether the individual itemizes deductions when filing his federal income tax return. The results reported in Table 4 are marginal effects from censored Tobit regressions. As our focus is on the elasticity estimates, we suppress the ITEMIZE coefficients; this variable is always positive and statistically significant and excluding it does not alter our results.

Comparing the results for LSS models 1 and 2 to results from the Eckel and Grossman's (2008) MPR study (model 4) and their (2003 and 2006b) laboratory studies (models 5-6), we find qualitatively and quantitatively similar results. In each case, the matching subsidies result in larger total contributions to the charity than rebate subsidies, with the match price elasticity significantly and, in most cases, substantially larger than the rebate price elasticity. Both the match price elasticity and rebate price elasticity estimates are similar in magnitude across the LSS and the MPR models and the 2003 laboratory study. Leaving aside LSS Model 3 for the moment, match price elasticity estimates vary from -1.05 to -3.17.; rebate price elasticity estimates vary from -0.19 to -1.49.²¹ The elasticity estimates in the 2006b study are each approximately two to three times larger than those from any of the other studies.²²

²¹ In a field study similar to Eckel and Grossman 2008, Karlan and List (2007) estimate match price elasticities for much higher subsidy levels (100 percent, 200 percent and 300 percent, yielding prices of giving equal to \$.5, \$.33 and \$.25.). Their estimate, excluding the match, is -.225, with a range of between 0 and -.668 across states. Our comparable match price elasticity estimate, excluding the match, is -0.712 with a standard error of 0.23.

²² This reflects the fact that subjects in the 2006b study (conducted in Minnesota), relative to subjects in the 2003 study (conducted in Texas), were less generous in the no subsidy and low subsidy cases and approximately equal in generosity at the higher subsidy rates. The difference in responsiveness may be due to subject pool differences.

Table 4: Comparison of Lab and Field Elasticity Estimates (std. errors): Total Giving

	Field Studies	N	Elasticity (std. error)		
			Rebate	Match	Income
1	LSS Study^{be} (MPR Comparable – All Respondents)	1207	-0.518 (0.37)	-1.249* (0.36)	0.227* (0.06)
2	LSS Study^{ce} (MPR Comparable – Donor Subsample)	869	-0.379 (0.27)	-1.055* (0.26)	0.366* (0.04)
3	LSS Study^{de} (Corrected)	1207	-5.122* (0.43)	-5.434* (0.32)	0.192* (0.05)
4	MPR Study Eckel and Grossman (2008)^a (Continuing Members)	5183	-0.193* (0.05)	-1.099* (0.05)	0.014 (0.01)
	Laboratory Studies				
5	Eckel and Grossman (2003)	168	-0.340* (0.19)	-1.067* (0.18)	0.821* (0.07)
6	Eckel and Grossman (2006b)	90	-1.491* (0.24)	-3.174* (0.24)	0.987* (0.17)

* - significant at the 5% level or better, two-tailed test.

^aThe subsample of continuing members is used rather than the full sample since the LSS sample is comprised of prior donors to LSS.

^bThese elasticity estimates are derived by assuming all respondents accepted their offered subsidy (i.e., dependent variable is DONATE1).

^cThese elasticity estimates are derived by limiting the sample to respondents with out-of-pocket donations > \$0 and assuming all respondents accepted their offered subsidy.

^dThese elasticity estimates are derived by assuming the subsidy prices only applied to those respondents who checked the “accept subsidy” box on their donor card. The dependent variable is DONATE2).

^eFor all three regressions, a Wald test rejects the null hypothesis of no difference in rebate and match elasticities. These regression models include a dummy variable for donors who itemized on the federal income tax returns. Including or excluding this variable does not appreciably alter the results.

The most dramatic difference between the field studies and the lab studies is in the income elasticity estimates (varying between 0.014 and 0.366). However, it is important to note that income is measured differently across the studies. The LSS measure of income is an individual measure consisting of the midpoint of the self-reported income range from the survey. The MPR study used median family income for 1999 by zip code as reported by the U.S. Bureau of the Census and thus is only an indicator of the range of income in which the donor is likely to fall based on the location of the donor’s residence. The laboratory studies define income as the

experimenter-provided endowment. The two lab studies provide similar estimates, and the differences in the two field studies are not surprising given the varying measures.

4.4 Corrected Price Elasticity Estimates

Assuming that all respondents in the field studies accepted the offered subsidies is obviously incorrect. As noted in Table 1, a sizable number of respondents did not accept the offered subsidy, either because they failed to notice it or rejected it. (Since we are unable to distinguish these two behaviors, we refer to all those who failed to check the box, regardless of the reason, as having “not accepted” the subsidy.) Treating these respondents as though they had accepted the subsidy distorts the estimated elasticities. In the analysis below, we compare those who did not accept the subsidy with those who did, as well as the no subsidy control group. If those who failed to accept the subsidy behaved in a manner consistent with our no subsidy control group, we can conclude that the offered subsidy did not affect their decisions. This is an improvement over previous field studies that were unable to distinguish the treated from the untreated within a treatment group, and focus analysis on those who took the subsidy into account when making their decisions to determine the treatment effects.

To correctly estimate the elasticities of giving, the measure of total donation for model 3 is now defined as the log of \$5 plus own giving plus any subsidy for those who accepted the offered subsidy (DONATE2). Model 3 distinguishes between those who accepted the subsidy offered and those who did not. Elasticities of giving for those who accepted the offered subsidies (-5.12 and -5.43 for rebate and match respectively) still differ significantly from each other [Wald test $\chi^2(1) = 166$ p-value < 0.05]. The higher estimated elasticity of giving under the match subsidy is consistent with the previous lab and field results reported in Eckel and Grossman (2003, 2006b, and 2008). Again, the other results are unchanged.

Next we compare our elasticity estimates to those from the MPR estimates and the MPR comparable LSS estimates. We find that assuming that, in the absence of more specific information, all respondents accepted the offered subsidies results in a significant underestimation of the elasticities of giving. Treating all respondents as having accepted the offered subsidy biases elasticity estimates for both subsidy types downward.

Neither our uncorrected nor corrected estimated elasticities are likely to be the true elasticities. Our uncorrected estimates are likely to represent a lower bound; if all donors were aware of the offered subsidies we might have expected greater giving as compared to at least some of the unaware donors. Similarly, our corrected estimates are likely to represent an upper bound; the more price sensitive donors are more likely to have been the donors opting to accept the offered subsidies. The true elasticities likely lie somewhere in between.

4.5 Accepting the Offered Subsidy

In Table 5 we report Probit regression results for the decision to accept the subsidy. The sample excludes the No Subsidy respondents since they did not receive a subsidy offer. The model estimated is:

$$\text{ACCEPT} = f1 + f2\text{REBATE} + f3\text{COLLEGEGRAD} + f4\text{ITEMIZE} + f5\text{ATTEND} + \varepsilon \quad (1)$$

The results provide strong evidence of rebate aversion as indicated by the negative effect of REBATE; donors are significantly less likely to accept the rebate offer (as noted above). The positive and significant coefficient for COLLEGEGRAD suggests that more educated people are more willing to make the effort to take into account the details of the subsidy offer. This may make them more likely to notice an offered subsidy, more likely to understand its conditions, and more trusting that accepting the offer is good for the organization. Respondents who attended

religious services (ATTEND) were also more likely to accept the subsidy.²³ Itemizing on one's federal tax return (ITEMIZE) did not influence the decision to accept or reject a subsidy offer.

Table 5: Probability of Accepting the Offered Subsidy: Probit Regression
(Dependent variable = ACCEPT)

Variable	Marginal Effect (Std. Error)
REBATE	-0.268* (0.03)
COLLEGEGRAD	0.108* (0.04)
ITEMIZE	0.041 (0.04)
ATTEND	0.123* (0.04)
CONSTANT	-0.175 (0.06)
LLF	-572
N ⁺	910

* - 95 percent confidence level, two-tailed test.

+ - Respondents in subsidy treatments only.

In lab experiments (Eckel and Grossman, 2006a) we gave subjects the option of choosing a 1 for 1 matching subsidy or a 50 percent instant rebate. Subjects chose the two in equal proportions. This suggests that the higher rejection rate for the rebate offer is not because, other things equal, subjects dislike rebate subsidies. Rather it could perhaps reflect the extra costs associated with keeping track of and cashing a rebate check.²⁴

²³ In additional robustness tests we distinguished the frequency of attendance at religious services. A log likelihood ratio test did not reject the null hypothesis that, jointly, the coefficients of the four attendance variables were equal ($\chi^2(5) = 5.4$, p-value > 0.05).

²⁴ Saez (2009) reports similar patterns for matching and credit (rebate) subsidies for opening an X-IRA, a product offered by H&R Block. In his study, the take-up rate for matching is around 50% higher than for the rebate subsidy. This suggests that “frictions” – credit constraints or the inconvenience of keeping track of a rebate check – are an important reason. Bénabou and Tirole (2006) suggest another explanation for the differential in acceptance rates for rebate and matching surveys. If giving is (partially) motivated by warm glow, then accepting a rebate offer may make the donor feel “greedy,” reducing the warm glow benefit and making the donor feel less good about himself. Rejecting the rebate offer maintains the warm glow feelings. Warm glow would be unaffected (or possibly enhanced) by a match subsidy.

4.6 Who Gives

We now consider the effect of the subsidy offers on the likelihood of giving ($DONATE = 1$ if a donation was made). Using probit models we estimate two specifications: one controlling for subsidy type alone (R20, R25, M25, and M33) and one controlling for subsidy type, attendance at religious services (ATTEND), and itemizing on the previous year's federal tax return (ITEMIZE). The equations estimated are:

$$DONATE = g_1 + g_2R20 + g_3R25 + g_4M25 + g_5M33 + \varepsilon \quad (2)$$

$$DONATE = h_1 + h_2R20 + h_3R25 + h_4M25 + h_5M33 + h_6ITEMIZE + h_7ATTEND + \varepsilon \quad (3)$$

Table 6 presents the Probit results, using the sample of 1207 respondents. Relative to the No Subsidy treatment, the offer of a subsidy significantly increases the probability of donating only for those receiving the 33 1/3 percent match subsidy offer. The probability a recipient of the 33 1/3 percent match subsidy will donate is 8 -10 percentage points higher than that for a No Subsidy recipient. Not surprisingly, itemizers are more likely (approximately 10 percent more likely) to donate than are non-itemizers. Attendance at religious services also increases the likelihood of donating (approximately 8 percent) to this religious-based charitable organization.

5. Conclusions

In this paper, we offer the first attempt to control for the bias introduced by the failure in field experiments to account for donors' awareness of offered subsidies. In the laboratory, the experimenter can be sure that the subjects are, at least, informed of any inducements to donate. In the field, extra steps must be taken to ascertain that respondents knew of any subsidy offered, and to distinguish those who were treated among those who were merely assigned to the treatment group. In previous field experiments, the experimenters failed to take these steps and therefore cannot determine if respondents were aware of any offered subsidy. In the studies by

Table 6: Probability of a Donation: Probit Regression
(Dependent variable = DONATE)

Variable	Marginal Effect (Std. Error)	
	(1)	(2)
R20	0.020 (0.04)	0.014 (0.04)
R25	0.025 (0.04)	0.022 (0.04)
M25	0.008 (0.04)	0.001 (0.04)
M33	0.095* (0.03)	0.084* (0.04)
ITEMIZE	...	0.103* (0.03)
ATTEND	...	0.083 (0.04)
CONSTANT	0.185* (0.03)	0.030 (0.05)
Log LF	-712	-703
N	1207	1207

* - significant at the 5% level, two-tailed test.

Eckel and Grossman (2008); Huck and Rasul (2007, 2011); Karlan and List (2007); Meier (2007); and Rasul and Huck (2010), the experimenters implicitly assumed that all respondents observed the offered subsidy and made their giving decisions with the subsidies in mind. Assuming that the offered price (the price of giving after the subsidy) equals the actual price (the price the subject actually considers when making his giving decision), these studies incorrectly estimate the responsiveness of giving to the subsidy offers. We present results of a field experiment designed to correct that shortcoming. We also offer a comparison of the effectiveness of rebate and matching subsidies in the field. With the exception of Eckel and Grossman (2008), previous field experiments have been limited to matching subsidies. In this paper we study both approaches. The study was conducted in conjunction with a religiously affiliated social services charity, Lutheran Social Service of Minnesota.

Our respondents were required to check a box on their pledge forms in order to receive any offered subsidy; checking the box indicated that they had read the enclosed materials and wished to receive the subsidy. The design feature enables us to distinguish between respondents who accepted the subsidy, and made their decisions with the subsidy in mind, and respondents who either consciously rejected the subsidy or were unaware of the subsidy offered. That is, we can distinguish between giving by those who were randomly allocated to a treatment from those who accepted the treatment and whose decisions responded to it.

Several conclusions can be drawn from our results. Analyzing the data under the erroneous assumption that all respondents knew of, accepted, and conditioned their giving on the offered subsidies results in an underestimation of the price elasticities of giving for both match and rebate subsidies. We show that giving by those who check the pledge card box accepting the offered subsidies is more responsive to the price of giving than those who failed to check the box. Furthermore, even after correcting for acceptance of the offered subsidy, we find that, consistent with earlier studies (Eckel and Grossman, 2003, 2006b, and 2008), giving is more responsive to match subsidies than rebate subsidies, though the difference is less in magnitude.

We also report evidence that some givers are averse to the offered subsidies and in particular to rebate subsidies. Only 39 percent of all donors who received a rebate subsidy offer (regardless of the rate) accepted the subsidy compared with 73 percent of all match donors. For those donors who were aware of the offered subsidy but refused it, this difference is likely due in part to the perceived costs entailed in the rebate process (keeping track of and cashing a check). Accepting a match subsidy does not have this cost. Educated donors (college degree or more) are more likely to accept the offered subsidy, suggesting that paying attention to the details of the offer may have a cognitive cost for lower-educated respondents. Though we cannot know why

donors failed to accept the offered subsidy, we find that the pattern of giving by these donors, regardless of the subsidy type and amount offered, is more similar to that of our No Subsidy control group, suggesting strongly that the offered subsidy did not materially affect their decisions.

While overall response rates (including donors and those who only completed the survey) were statistically the same across treatment groups, the higher match subsidy increased the probability that a responder donated money. Overall, 72 percent of all responders made a donation; for the 33 1/3 percent match group, the rate was 79 percent. Total donations (own giving plus any match) by donors who accepted the offered subsidies were significantly greater than for donors who rejected the offered subsidies. As indicated by the median tests, the presence of a subsidy appears to have triggered a movement by donors from one focal point to a higher focal point, regardless of the subsidy type.

For those donors who did not accept the offered subsidies, we found no significant differences in giving levels across subsidy treatments. However, donors who failed to accept the offered match subsidy gave significantly less than the No Subsidy control group.

Assuming our results are confirmed, the findings have important implications for fundraisers, and for government policies toward subsidization of nonprofit and charitable organizations. As a manager of a fundraising effort, it would be very important to know the response to a matching or rebate campaign of both those who accept a subsidy and those that do not. While the response of consumers to rebates and matching in the context of marketing of consumer goods is well known (e.g., Buckeley 1998, Bruce et al. 2006, Chen et al. 2005, Soman 1998) responses of the targets of fundraising campaigns are less well known. We are the first to provide estimates of the responses of both groups to two different methods of subsidizing giving.

From a practical standpoint, the study has two main implications for fundraisers. First, it is clear that matching subsidies are the way to go. The matching subsidy is more effective than the rebate subsidy in raising revenue, even when the matches themselves are excluded from revenue (Table 1). Furthermore, a 33^{1/3}% match is more effective than a 25% match. Prior studies find some evidence of crowding out at rates as low as 50% (Huck and Rasul 2011), or show no effect of higher subsidy rates above 100% (Karlan and List 2007), so establishing that the 33/3% rate generates more revenue (exclusive of subsidies themselves) than the 25% rate is important. A second implication comes from the result that assessing the impact of a fundraising campaign is not as simple as calculating an ITT effect on everyone assigned to the treated group. Not everyone notices a matching offer, and among those that do, there is some evidence that they explicitly reject it. The fundraisers will notice the importance of making sure their potential donors see the offer, and understand its conditions. Care should be taken to draw the donor's attention to the offer, and explain clearly its terms.

In the policy realm, the results also suggest that replacing the current system of tax rebates with an equivalent-cost matching subsidy system could increase overall contributions to charitable organizations. The UK has a system (Gift Aid) introduced in 1990 that combines both a rebate for the donor and a match for the charity (Chennells et al. 2000). Under the Gift Aid scheme, donors are effectively rebated the taxes owed on the “grossed-up” amount of the donation and the charity receives a match at the basic-rate tax.²⁵

If LSS contributors are typical, and matching subsidies increase giving nearly as much as rebate subsidies, then substituting matches could increase total giving. This implication relies on

²⁵ For example, assume the basic-rate tax is 20% and a donor contributes \$100. The donor can claim a donation of \$125, receiving tax relief of \$25 (20% of the grossed-up amount of \$125). The charity receives a matching contribution from the government equal to the \$25. For a discussion of the impact of the Gift Aid program on charitable giving, see: www.dsc.org.uk/policyandresearch/research/main_content/giftaid.pdf.

the inference from these results that the presence of a subsidy is the critical factor in stimulating contributions. Considerably more work would be necessary to confirm this possibility before changes in the tax system were considered.

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The following appendices are not for publication

Appendix Table 1: Comparison of Subject Characteristics by Treatment

	No Subsidy N (%)	20% Rebate N (%)	25% Match N (%)	25% Rebate N (%)	33 1/3% Match N (%)	χ^2 Contingency Table Test Statistic (d.f.) p-value
Income (\$000)						
< \$20	25 (0.08)	19 (0.08)	17 (0.08)	15 (0.07)	16 (0.07)	19.2 (20) 0.511
\$20-\$50	132 (0.44)	109 (0.45)	80 (0.37)	92 (0.44)	91 (0.37)	
\$50-\$80	76 (0.26)	67 (0.28)	61 (0.28)	56 (0.27)	69 (0.28)	
\$80-\$110	36 (0.12)	29 (0.12)	34 (0.16)	20 (0.10)	44 (0.18)	
\$110-\$140	10 (0.03)	10 (0.04)	13 (0.06)	9 (0.04)	9 (0.04)	
> \$140	18 (0.06)	8 (0.03)	11 (0.05)	16 (0.08)	15 (0.06)	
Itemize Deductions^a	217 (0.73)	189 (0.78)	170 (0.79)	153 (0.74)	206 (0.84)	
Annual Giving						
< \$100	6 (0.02)	3 (0.01)	5 (0.02)	3 (0.01)	2 (0.01)	11.9 (12) 0.456
\$100-\$500	35 (0.12)	30 (0.12)	23 (0.11)	22 (0.11)	37 (0.15)	
\$500-\$1,000	43 (0.14)	31 (0.13)	33 (0.15)	23 (0.11)	20 (0.08)	
> \$1,000	213 (0.72)	178 (0.74)	155 (0.72)	160 (0.77)	185 (0.76)	
Attendance at Religious Services						
Never	11 (0.04)	5 (0.02)	5 (0.02)	3 (0.01)	5 (0.02)	15.0 (16) 0.527
< Once per Month	23 (0.08)	15 (0.06)	18 (0.08)	18 (0.09)	23 (0.09)	
At least once a month	31 (0.10)	19 (0.08)	16 (0.07)	30 (0.14)	23 (0.09)	
Once a week	182 (0.61)	164 (0.68)	147 (0.69)	130 (0.63)	159 (0.65)	
> Once a week	50 (0.17)	39 (0.16)	30 (0.13)	27 (0.13)	34 (0.14)	

Education						
< High School	4 (0.01)	5 (0.02)	6 (0.03)	4 (0.02)	3 (0.01)	
High School Diploma	48 (0.16)	35 (0.14)	35 (0.16)	33 (0.16)	27 (0.11)	17.1 (16)
Some College	65 (0.22)	52 (0.21)	33 (0.15)	41 (0.20)	45 (0.18)	0.378
College Degree	84 (0.28)	79 (0.33)	73 (0.34)	80 (0.38)	88 (0.36)	
Post College Degree	96 (0.32)	71 (0.29)	69 (0.32)	50 (0.24)	81 (0.33)	
Age						
26-35	1 (0.00)	3 (0.01)	4 (0.02)	5 (0.02)	5 (0.02)	18.8 (12)
36-45	19 (0.06)	7 (0.03)	14 (0.06)	7 (0.03)	19 (0.08)	0.094
46-60	57 (0.19)	48 (0.20)	51 (0.24)	55 (0.26)	57 (0.23)	
> 60	220 (0.74)	184 (0.76)	147 (0.68)	141 (0.68)	163 (0.67)	
Marital Status						
Married	187 (0.63)	148 (0.61)	147 (0.68)	135 (0.65)	167 (0.68)	
Widowed	67 (0.23)	49 (0.20)	36 (0.17)	38 (0.18)	43 (0.18)	11.5 (12)
Divorced/Separated	11 (0.04)	19 (0.08)	10 (0.05)	16 (0.08)	12 (0.05)	0.486
Single	32 (0.11)	26 (0.11)	23 (0.11)	19 (0.09)	22 (0.09)	
N	297	242	216	208	244	

a - Binomial Proportions test p-values

	No Subsidy	20% Rebate	25% Match	25% Rebate
20% Rebate	0.177			
25% Match	0.143	0.875		
25% Rebate	0.902	0.261	0.214	
33 1/3% Match	0.001	0.074	0.113	0.004

Appendix Table 2: Subject Characteristics by Response

	Non Donors N (%)	No Subsidy Offered N (%)	Did Not Check Box N (%)	Checked Box N (%)	χ^2 Contingency Table Test Statistic (d.f.) p-value
Income (\$000)					
< \$20	27 (2.1)	18 (8.8)	26 (9.08)	21 (5.6)	16.9 (15) 0.323
\$20-\$50	148 (43.8)	91 (44.4)	124 (43.1)	141 (37.5)	
\$50-\$80	92 (27.2)	53 (25.9)	70 (24.3)	114 (30.3)	
\$80-\$110	42 (12.4)	23 (11.2)	41 (14.2)	57 (15.2)	
\$110-\$140	15 (4.4)	6 (2.4)	8 (2.8)	22 (5.4)	
> \$140	14 (4.1)	14 (6.8)	19 (6.6)	21 (5.6)	
Itemize Deductions^a	239 (70.7)	155 (75.6)	231 (80.2)	310 (82.4)	
Annual Giving					
< \$100	9 (2.7)	5 (2.4)	4 (1.4)	1 (0.3)	15.3 (9) 0.083
\$100-\$500	40 (11.8)	25 (12.2)	39 (13.5)	43 (11.4)	
\$500-\$1,000	43 (12.7)	31 (15.1)	41 (14.2)	35 (9.3)	
> \$1,000	248 (72.8)	144 (70.2)	204 (70.8)	297 (79.0)	
Attendance at Religious Services					
Never	4 (1.2)	9 (4.4)	6 (2.1)	10 (2.7)	24.5 (12) 0.018
< Once per Month	23 (6.8)	18 (8.8)	31 (10.8)	25 (6.6)	
At least once a month	28 (8.3)	23 (11.2)	27 (9.4)	41 (10.9)	
Once a week	218 (64.5)	119 (58.0)	185 (64.2)	260 (69.1)	
> Once a week	65 (19.2)	36 (17.6)	39 (13.5)	40 (10.6)	

Education					
< High School	9 (2.7)	3 (1.5)	2 (0.7)	8 (2.1)	
High School Diploma	54 (16.0)	35 (17.1)	44 (15.3)	45 (12.0)	27.1 (12)
Some College	67 (19.8)	44 (21.5)	72 (25.0)	53 (14.1)	0.008
College Degree	111 (32.8)	57 (27.8)	99 (34.4)	137 (36.4)	
Post College Degree	97 (28.7)	66 (32.2)	71 (24.7)	133 (35.4)	
Age					
< 35	6 (1.8)	0 (0.0)	4 (1.4)	8 (2.1)	12.2 (9)
36-45	19 (5.6)	14 (6.8)	13 (4.5)	20 (5.3)	0.202
46-60	81 (24.0)	33 (16.1)	62 (21.5)	92 (24.5)	
> 60	232 (68.6)	158 (77.1)	209 (72.6)	256 (68.1)	
Marital Status					
Married	230 (68.0)	123 (60.0)	175 (60.8)	256 (68.1)	
Widowed	58 (17.2)	51 (24.9)	61 (21.2)	63 (16.8)	14.9 (9)
Divorced/Separated	18 (5.3)	6 (2.9)	19 (6.6)	25 (6.6)	0.094
Single	32 (9.51)	25 (12.2)	33 (11.5)	32 (8.5)	
N	388	205	288	376	

a - Binomial Proportions test p-values

	Non Donors	No Subsidy	Did Not Check Box
No Subsidy	< 0.001		
Did Not Check Box	< 0.001	0.222	
Checked Box	< 0.001	0.049	0.462

APPENDIX

Safe Homes, Hopeful Futures

An opportunity for you to increase the size of your gift to LSS by 20% at no cost to you or LSS

Lutheran Social Service of Minnesota is participating in a study * with a donor who will refund part of your contribution to you. **If you checked the box on the bottom of your contribution card, the donor will refund 20% of your contribution.** The table below illustrates the refund amount and net cost to you of giving various amounts to LSS. **Note that the refund will be paid for by the donor doing the research, NOT by LSS.**

If you give:	Your 20% refund will be:	Net cost of your gift to you:	LSS receives in total:
\$35	\$7.00	\$28.00	\$35.00
\$50	\$10.00	\$40.00	\$50.00
\$100	\$20.00	\$80.00	\$100.00
\$250	\$50.00	\$200.00	\$250.00
\$500	\$100.00	\$400.00	\$500.00

Remember, in order for you to receive the refund, you need to check the box on the contribution form on the bottom of the enclosed letter indicating you have read this enclosure and are willing to participate.

* The study provides funding for refunds up to a total of \$5,000. Refunds will be provided on a first-come, first-serve basis. If you have questions about the donor, the study, or the procedures for receiving your refund, please contact Professor Philip Grossman at 1-320-308-4232.

Safe Homes, Hopeful Future

An opportunity for you to increase the size of your gift to LSS by 25% at no cost to you or LSS

Lutheran Social Service of Minnesota is participating in a study * with a donor who will refund

part of your contribution to you. **If you checked the box on the bottom of your contribution card, the donor will refund 25% of your contribution.** The table below illustrates the refund amount and net cost to you of giving various amounts to LSS. **Note that the refund will be paid for by the donor doing the research, NOT by LSS.**

If you give:	Your 25% refund will be:	Net cost of your gift to you:	LSS receives in total:
\$35	\$8.75	\$26.75	\$35.00
\$50	\$12.50	\$37.50	\$50.00
\$100	\$25.00	\$75.00	\$100.00
\$250	\$62.50	\$187.50	\$250.00
\$500	\$125.00	\$375.00	\$500.00

Remember, in order for you to receive the refund, you need to check the box on the contribution form on the bottom of the enclosed letter indicating you have read this enclosure and are willing to participate.

* The study provides funding for refunds up to a total of \$8,000. Refunds will be provided on a first-come, first-serve basis. If you have questions about the donor, the study, or the procedures for receiving your refund, please contact Professor Philip Grossman at 1-320-308-4232.

Safe Homes, Hopeful Futures

An opportunity for you to increase the size of your gift to LSS by 25% at no cost to you or LSS

Lutheran Social Service of Minnesota is participating in a study * with a donor who will match part of your contribution. **If you checked the box on the bottom of your contribution card, your contribution will be matched at a rate of 25% by the donor.** The table below illustrates details of the matching amounts possible for different levels of contribution. **These matching amounts are paid by the donor doing the research, NOT by LSS**

If you give:	The donor gives 1/4 (25%) more:	LSS receives in total:
\$35	\$8.75	\$43.75
\$50	\$12.50	\$62.50
\$100	\$25.00	\$125.00
\$250	\$62.50	\$312.50
\$500	\$125.00	\$625.00

Remember, for LSS to receive the matching amount, you need to check the box on the contribution form on the bottom of the enclosed letter indicating you have read this enclosure and are willing to participate.

Thank you for your time, consideration and support of LSS.

* The study provides funding for matching funds up to a total of \$8,000. Matching will be provided on a first-come, first-serve basis. If you have questions about the donor, the study, or the procedures for matching your contribution, please contact Professor Philip Grossman at 1-320-308-4232.

Safe Homes, Hopeful Futures

An opportunity for you to increase the size of your gift to LSS by 33 1/3% at no cost to you or LSS

Lutheran Social Service of Minnesota is participating in a study * with a donor who will match part of your contribution. **If you checked the box on the bottom of your contribution card, your contribution will be matched at a rate of 33 1/3% by the donor.** The table below illustrates details of the matching amounts possible for different levels of contribution. **These matching amounts are paid by the donor doing the research, NOT by LSS**

If you give:	The donor gives 1/3 (33 1/3%) more:	LSS receives in total:
\$35	\$11.66	\$46.66
\$50	\$16.65	\$66.65
\$100	\$33.30	\$133.30
\$250	\$83.25	\$333.25
\$500	\$166.50	\$666.50

Remember, for LSS to receive the matching amount, you need to check the box on the contribution form on the bottom of the enclosed letter indicating you have read this enclosure and are willing to participate.

Thank you for your time, consideration and support of LSS.

* The study provides funding for matching funds up to a total of \$15,000. Matching will be provided on a first-come, first-serve basis. If you have questions about the donor, the study, or the procedures for matching your contribution, please contact Professor Philip Grossman at 1-320-308-4232.