Subsidizing Charitable Contributions: A Natural Field Experiment Comparing Matching and Rebate Subsidies

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Abstract: We report the results of a field experiment conducted in conjunction with a mailed fundraising campaign of a nonprofit organization. The experiment is designed to compare the response of donors to subsidies in the form of matching amounts or rebated amounts. Matching subsidies are used by many corporations as an employee benefit; the US federal tax system encourages giving using a rebate subsidy by making donations tax deductible. The design includes a control group and two levels of subsidy of each type. Our main result is that matching subsidies result in larger total donations to charities than rebate subsidies, a result that is qualitatively similar to the lab findings. The estimated price elasticities for the matching subsidy are very similar to (and insignificantly different from) the lab experiments, while rebate subsidies lead to lower contributions in the field than in the lab. Since rebates in the field involve substantial lags and additional complications as compared with the "instant rebates" of the lab, this latter difference is not unexpected. The matching results are an important step in validating lab estimates of responsiveness to subsidies of charitable giving.

Keywords: Field experiment, Rebate subsidy, Matching subsidy, Charitable giving

JEL Codes: C93, D64, H41, L3

I. Introduction

Because voluntary contributions alone are unlikely to produce efficient levels of charitable activities, many governments around the world subsidize nonprofit or charitable organizations. Direct subsidies, consisting of government funds transferred directly to an organization, are often accompanied by policies that subsidize private gifts. In the United States, federal tax policy rebates to itemizing taxpayers a portion of their charitable contributions by making such expenditures deductible from taxable income.[1] Similar subsidies are seen in other Western countries. Charitable gifts also are encouraged by subsidies from other sources. Thousands of corporations match their employees' charitable gifts. In addition, fundraisers often provide gifts such as mugs or t-shirts as incentives for larger donations; these can be thought of as partial rebates for giving.

The pervasiveness of both rebate and matching forms of subsidy could mean that they are equally successful in increasing giving, or that one or the other is better suited to particular situations. Rebating a portion of a donation appears on the surface to be equivalent to matching a donation at an appropriate rate. Several recent studies compare the two subsidy types in laboratory experiments, and generally find that the two do not produce equivalent results (Eckel and Grossman, 2003, 2006a, 2006b; Davis, Millner and Reilly, 2005; Davis and Millner, 2005).[2] This paper reports results from a field study comparing the effects of rebates and matching subsidies for charitable contributions.

The field study design parallels a subset of the parameters used in the lab to facilitate comparison. Field experiments on charitable giving provide a bridge between laboratory research and studies that use data generated by choices made in non-experimental settings such as tax data. This point is made by List (2006), who argues that there is a risk of overdeveloping

ideas based on laboratory data, when these concepts have little parallel in the field; if a result holds up in the field, this legitimizes the lab results. Experimental data from the lab and the field are used most powerfully in conjunction to understand decision making, with the lab providing superior control but lacking realistic context, and the field providing rich context at the cost of some loss of control.

Exploring the effect of subsidies in the field, in partnership with a nonprofit organization, allows us to control key aspects of the choice situation, but in a more natural context than the lab, and with a broader variety of participants. Extending lab results to field contexts and to participants other than the convenience sample of university students can significantly enhance understanding of many types of decision making, including charitable donations. For example, List and Lucking-Reiley (2002) manipulate two aspects of a capital campaign for a university – seed money and a refund option – and find significant effects of both. Karlan and List (2007) examine the effect of subsidies on the response rate and amount of giving and find that the fact of a subsidy, but not its level, positively affects contributions. (See Harrison and List (2004) for a discussion of the value of field experiments.)

The field experiment was conducted in conjunction with Minnesota Public Radio's (MPR) annual fund drive, as described below. It has 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. Within each treatment there are two rates of subsidy that produce the same pairs of prices of giving $1 to charity: 20 and 25 percent rebates and 25 and 33 1/3 percent matches. As in the lab experiment, our main results are that, relative to functionally equivalent rebate subsidies, matching subsidies result in larger total contributions to the charity and price elasticities about one and one-half to three times as large as for the rebate subsidy. Thus the field

results are qualitatively similar to the lab results. In the case of matching, but not rebate, subsidies, the quantitative estimate of responsiveness from the field data is not statistically different from the lab estimate.

II. Rebate versus Matching Subsidies in the Lab

It is trivial to demonstrate that rebating part of a donation and matching a donation have equivalent effects on the price of donating. For example, consider a rebate subsidy of 20%; this would be equivalent to a marginal tax rate of 20 percent combined with tax deductibility of charitable contributions. Giving a deductible $1 to charity will result in the taxpayer reducing taxable income by $1 and reducing taxes owed by $0.20. In effect, it costs the giver only $0.80 out of pocket to give the charity $1. The equivalent match subsidy would be 25 percent. With no deductibility, but instead a 25 percent match, the donor could still put $1 into the hands of the charity at a cost of only $0.80 out of pocket. Giving $0.80 will trigger the 25 percent match (i.e. an additional $0.20 = 0.25 X $0.80) resulting in the charity receiving $1.00 in total.

It is easy to show that the effect of the subsidy on total giving should be nonnegative, but the impact on net individual giving (the donor's share of the total contribution , excluding the match but netting out the rebate) is less clear, and could be positive, zero, or negative. If the subsidy crowds out private giving, then net individual giving will fall; crowding in would mean an increase in net individual giving. Obviously, a charity hopes that subsidies will not only increase their total take, but also will increase net individual giving. However, in theory (and in practice), this may not be the case. Thus we have a clear theoretical prediction about the effect of the subsidies on the total amount received by the charities, but no clear prediction about the effect on net individual giving. Another clear prediction is that "checkbook" donations (the

amount initially transferred to the charity by the donor) should be higher under rebate than match subsidies. The recipient of a rebate subsidy should increase checkbook giving, while the recipient of a match subsidy may decrease, hold constant, or increase checkbook giving. In any case, it is easily verified that the difference in checkbook giving between the two subsidies should be nonnegative.

The laboratory experiment reported in Eckel and Grossman (2003) directly compared equivalent rebate and matching subsidies in a controlled setting. Factors that might make the two subsidies different in the field, such as the processing cost or delay associated with tax refunds or mail-in rebates, can be eliminated in the lab, allowing us to focus on the effect of changing the price of giving by the two different mechanisms. The experiment consisted of a series of allocation decisions, each involving a subject dividing specified experimenter-provided endowments between a charity and himself. The 168 participants first chose a target charity from a list of ten provided by the experimenter: the charities were selected to reflect a broad range of services and client groups. The charities varied in geographic scope (international, national, and local) and covered health, environmental, and social service charities. Each participant completed twelve allocation decisions that differed in three dimensions: endowment ($4.00, $6.00, $7.50, or $10.00); the cost to the participant of contributing $1 to the charity ($1, $0.80, $0.75, and $0.50); and whether the reduction in cost was a result of a rebate of a portion of any contribution or of a matching of any contribution. One decision was chosen at random for payment. Matches and rebates were 'instant': rebates were paid as part of the subjects' earnings. The protocol for these experiments builds on the standard double-anonymous dictator game (Hoffman, et al., 1996), ensuring, to the extent possible, the anonymity of the subjects' donation decisions.

The results of the experiment were surprising. In every pairwise comparison, the dollar value of the donation received by the charity (including any matching amounts) was significantly greater under the matching subsidy than under the rebate subsidy. For example, with an endowment of $7.50 (75 tokens) and a price of giving of $0.75, charities received approximately 21 percent more, on average, under the matching subsidy than under the rebate subsidy ($4.71 vs. $3.89). The pattern of giving in the results is otherwise consistent with standard predictions. Giving is price-sensitive, increasing as the cost of giving declines; and charitable services is a normal good, with donations to charity increasing with income. Estimated income elasticities fall between 0.77 and 1.03 (depending on the type of subsidy), and rebate and matching price elasticities are -0.34, and –1.07, respectively. Both price elasticities are significantly negative, and giving under a matching subsidy is approximately three times as responsive to changes in the price of giving as under a rebate subsidy. The significant difference between the price elasticity estimates for the two subsidy types indicates that the form of the subsidy has an important effect on the total amount received by the charities. Using the experimental data, we also can evaluate the effect of the subsidies on checkbook giving: the amount that the donor transfers to the charity, before adjusting for subsidies. As expected, average checkbook donations are greater for the rebate subsidies in 4 of the 5 relevant comparisons, but statistically significant only in two. The price elasticity for checkbook giving of course remains -0.34 for the rebate subsidy, but it not significantly different from zero for the match. Thus there appears to be some crowding out for the rebate subsidy, but none for the match, which explains the greater revenue flowing to the charity under the matching subsidy.

Eckel and Grossman (2003) proposed several alternative explanations for the nonequivalence result, including the possibility that participants did not fully understand the

equivalence of the rebate and matching subsidies. Since that study used a within-subjects design, with subjects making decisions under both rebate and match conditions, a failure to fully distinguish between them could product such results. Davis, Millner, and Reilly (2005) make the decisions more transparent by presenting in a detailed table the consequences of all possible alternatives. Compared to Eckel and Grossman (2003), the extra information increased checkbook giving for the rebate conditions and reduced checkbook giving for the match conditions, bringing them closer to equivalence; total contributions remained significantly greater under a match subsidy than under a rebate subsidy. The lack of equivalence in their data is driven primarily by 30-40% of their subjects whose checkbook giving is the same or nearly the same under both subsidy schemes – i.e. those who have little or no response to the subsidies, and therefore make decisions that are non-equivalent across the two. A real question remains whether this pattern of behavior – lack of full adjustment in some sense –holds up in the field, where (among other differences) donors would only see one type of subsidy. To investigate this, we first designed a new experiment, where subjects saw only one type.

In Eckel and Grossman (2006b), we implemented a between-subjects design, separating the decisions into two treatments, so that a subject encountered only rebate subsidies or matching subsidies. Each treatment consisted of a set of decisions that varied endowment and price under a single subsidy type. The results are consistent with the original study, suggesting that failure to adjust fully was not due to the presence of both subsidy types. Eckel and Grossman (2006a) further simplified the protocol to account for the possibility that making multiple decisions was difficult for participants. Participants first were given a choice between equivalent subsidies: a 50 percent rebate or a 100 percent match. Each then made only one decision from a single endowment of $20 with their chosen subsidy. While participants chose rebates and matches in

equal numbers, contributions were again significantly greater from those selecting the match subsidy than from those selecting the rebate subsidy.

Davis and Millner (2005) make the decision more familiar by placing the problem in an everyday context: the purchase of a private good (i.e. small, 1.25 oz. chocolate bars) subject to rebate and matching offers. Rebates were framed as refunds of a portion of the purchase price; matches were framed as, for example, "buy two get one free." Consistent with the other studies, they find participants purchase more chocolate bars under the match offers than under the strategically-equivalent rebate offers. They attribute the results in part to some subjects who did not respond to the subsidies, as with the charitable giving experiments. An important question is whether donors would exhibit the same non-response in decisions in natural field settings. If so, this suggests that, from the point of view of a charity, rebate subsidies are dominated by equivalent matching subsidies for increasing total contributions. In other words, if a charity is offered a pot of funding it can use for subsidies, it is better off devoting those funds to matching rather than rebating individual contributions.

To test this proposition, we implemented equivalent cash rebates and match offers in the familiar environment of a public radio fundraising event. Subjects made decisions in a natural setting, free of any possible experimenter demand or observer effects. Moving to the field means foregoing the control advantages of the lab, including the possibility of an instant rebate. However, the field protocol allows us to test the effect of different subsidy types on charitable giving in a more realistic setting, and with a more heterogeneous sample of participants. Each participant sees only one of the five treatments, and is unaware of the others.

Use of a field study also permits us to examine another possible explanation for the difference between rebate and matching subsidies: the participants' use of "house money" for

donation decisions. Since the lab participants were using the experimenter's money instead of their own for making the allocation decisions, the perceived salience of the opportunity cost of giving may have been lessened, causing them to be more likely to use a rule of thumb, or more likely to succumb to perceptions of experimenter demand, in making their allocation decisions. Field experiments allow us to test for this possibility. In the field experiment reported here, participants use their own income to make decisions about donations to public radio.

III. Field Study Procedures

The field study was conducted with the cooperation of Minnesota Public Radio (MPR) as part of regular MPR mailed fundraising drives conducted in December, 2001 and May, September and November, 2002. The experiment included four treatment groups and a control group. The treatments consist of 20 and 25% rebate subsidies and equivalent 25 and 33-1/3% matching subsidies. These levels are similar, but not identical to US federal marginal tax rates in effect in the period of the study. At that time, the marginal tax rate was 15% for family incomes below $45,200, 27.5% for incomes between $45,000 and $109,250; 30.5% for $109,250 - $166,500 incomes, and 35.5% for $166,500 - $297,350. The highest marginal tax rate was 39.1% for incomes over $297,000.[3] To give some idea of the fraction of donations covered by this subsidy level, giving by those with incomes below $100,000 (in the lower tax brackets) accounts for about 36% of all charitable giving in 2005, and this proportion would be slightly higher for 2001, the period of the study. Donations for this income range vary from 60% of all donations to religious organizations, to 4.3% of donations to arts organizations, with other categories of giving in between (Center on Philanthropy, 2007).

These treatments were implemented with three types of 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). Continuing and Lapsed member also received a table that detailed the benefits to joining at various gift levels (see Appendix).

Included with the usual mail solicitation for the treatment groups was a flyer announcing the rebate or match offer; flyers were not inserted in the control mailings (see Appendix for examples of the flyers). The flyers were about two-thirds of a normal page in size. On one side in large, bold letters, the reader was made aware of the potential to either receive a rebate on any contribution made or of the potential to increase the size of the contribution going to MPR. The opposite side provided details of the rebate (match) offer and a procedure for completing a survey on line, or requesting a hard copy. The survey was intended to collect socioeconomic information, information regarding a participant's pattern of charitable giving, and information designed to measure the participant's perceived benefits from the charity. However, despite the fact that we offered an additional $1 donation to MPR for every completed survey, response to the survey was disappointingly low and we were unable to make use of the responses. Note the control group did not receive the flyer, so was not invited to complete the survey. Caps on total subsidies were noted in fine print on the solicitation. [4]

IV. Results

The number of mailings, responses, checkbook and total revenue per solicitation, and mean checkbook and total donations for each of the five price/subsidy-type categories for each of

the three donor types are reported in Table 1. In total 372,495 solicitations were mailed: 19,690 to Continuing members of MPR; 75,000 to Lapsed members; and 277,805 to Prospects. Of these, 184,800 included subsidy offers. Note that the mailing was not balanced for Continuing and Prospect donors: we have data for considerably more donors in the control group than in the treatment categories. Responses were received from 7,278 persons, an overall response rate of 1.95 percent. The response rates varied by donor type as expected: the Continuing category had the highest (26.6 percent) and Prospects the lowest (0.5 percent), as shown in Table 1. These response rates are, on average, slightly lower than anticipated for Continuing members, but in the expected range for the other categories based on MPR's expectations from prior mailed solicitations.[5]

Table 1 about here

For Lapsed and Prospects donors, there is little variation in response rates across the different subsidy types. However, for the Continuing donors category, the no-subsidy control group has a significantly higher response rate (28.6 percent) than the treatment categories (around 20 percent). Our first thought was that the treatments for this group might not have been randomly assigned, through some error on the part of the charity. We had requested that a random sample of members be allocated to each subsidy type, and on the basis of conversations with the membership office, it seems clear that MPR did their best to deliver. We also did further testing to detect any failures of randomization. The information in our sample includes the donors' zip codes; we found no difference in the distribution of responses across zip codes.[6] In addition, we employed geographic information system (GIS) methods to test for differences in the distribution of responses across geographic locations, controlling for signal quality from the stations. While responses are clearly related to signal strength, we again did not find any

systematic differences between treatments.[7] Thus we have no support for the notion that randomization was improperly implemented for any of treatments or membership categories. We further investigated the possibility of response bias by testing for differences in zip code distribution and income between the control group and the treatment groups for Continuing Members, and found no statistical difference. Thus these Continuing donors in the control group are not different from the donors in the treatment groups in any systematic way that we can identify.

We cannot know for certain why the response rates are somewhat lower for the Continuing members receiving subsidy offers, but there are two factors that may have contributed. First, the treatment group received a mailing that was different from what they were accustomed to seeing. From conversations with Mr. Al Anderson, Director of Membership Marketing, an earlier change that was designed to make rejoining easier (merely adding a 1-800 number to the pledge form) resulted instead in a significant drop in their response rate of about the magnitude we observed for the treatment groups. Mr. Anderson's hypothesis about the impact of the 800 number is that potential donors set the pledge card aside intending to call, then did not. In our case, because of their loyal support of the organization, a substantial portion of the Continuing target group may have felt an obligation to consider the subsidy carefully as well as to complete the online survey, and may have set the mailing aside, fully intending to complete it at a later time. This is in contrast to the other two groups (Lapsed Members and Prospects), who exhibit no parallel difference in response rates for the treatment v. control groups, and who likely have no parallel 'loyalty' impulse.

A second potential factor is that the wording of the subsidy and request to complete a survey (as approved by our IRBs) may have made the donors aware that they were part of a

research project. Reluctance to participate might have impacted participation rates. However, this seems unlikely given that we do not observe a parallel difference in the two other donor categories, Lapsed and Prospects. We are skeptical that reluctance to participate is the cause of this large difference. If anything, we would expect Continuing members to be MORE willing to participate in a study than Lapsed or Prospect members, not less. These two categories should have been at least as averse to participating in a study, but there we see no statistical difference between the control and treatment group response rates. Therefore we infer that the former 'loyalty' argument is more likely to be the cause of the difference in response rates for Continuing Members in the no-subsidy group.[8]

Table 1 also contains revenue per solicitation mailed. Checkbook revenue per solicitation was $27.84 for the Continuing category, and substantially lower for Lapsed and Prospect members ($0.58 and $0.23). Total revenue per solicitation (including the matching subsidy) averaged $1.82 for the complete sample and follows a parallel pattern. (Keep in mind that these averages include disproportionate representation in the control group for Continuing and Propect mailings).

Of the 7,278 responses delivered to us by MPR, 7,195 had complete information and so were useable for subsequent analysis. Most critical were zip codes, needed to link the responses to neighborhood-level data in order to estimate household income, a small number of which were missing or inaccurate. The proportion of useable responses did not differ across donor types or subsidy types, and the average donation does not differ between the dropped observations and the useable ones.

Mean checkbook donations are highest for the Continuing category, as are total donations. In the Continuing category, mean checkbook giving is higher for the 20% rebate than

for the 25% match (p-value = 0.02). For all other rebate/match pairings and for all donor types, the difference in mean checkbook giving is not significant (p-value > 0.15 in every case). However, in all cases, the mean total giving is higher for the match than the corresponding rebate subsidy. In no case does the checkbook contribution under the rebate subsidy increase sufficiently to offset the amount of the subsidy: the closest is the 20% rebate for Continuing members, which is 14.9 percent higher than the baseline no-subsidy contribution. In all cases, checkbook giving under matching subsidies is not significantly different from the baseline case, indicating zero crowding out (p –value $\geq$ 0.20 in every case). On average, this is consistent with partial crowding out for rebate subsidies, and zero crowding out for matching subsidies.

a. Gift Donors

Fifty-seven percent of all donors made contributions at the basic membership level or one of the higher gift levels. The cost of an annual "membership" for Prospects was $42, for Continuing and Lapsed donors it was $84.[9] Membership conveys upon the donor certain benefits not available to those contributing less. In particular, members are provided early purchase opportunities for *A Prairie Home Companion* tickets, subscriptions to *Minnesota Monthly*, and discount cards good at selected restaurants and shops in Minnesota. The higher gift levels of $120, $180, $240, $360, $500 and $1,000 provide membership along with additional benefits (see Appendix for details). These donors receive an additional direct benefit and may view their donations as the purchase price of this private good. For this reason we control for gift levels in the analysis that follows.

For those offered a match subsidy, membership requires an out-of-pocket donation of $42 ($84). In theory, assuming the 25 percent match subsidy, the donor could contribute only $33.4 ($67.2) and the membership price would be reached when the match is added. However, this

option was not made available to potential donors. Thus, qualification for all benefit levels was determined by checkbook giving.

Figure 1 about here

Figure 1 shows the distribution of donations for the three donor types. The impact of the fee structure is very clear in the figure. Continuing and Lapsed members show a mode at the membership level of $84; Prospects show a (stronger) mode at $42. All groups show substantial contributions above and below membership levels.

b. Regression Results

Table 2 about here

Table 2 contains definitions for variables used in the regressions, and Table 3 reports OLS regression analysis of the checkbook and total donations to MPR. The dependent variables are the natural log of dollar donations, Checkbook and Total, respectively. We estimate two specifications for each dependent variable. The first controls for the price of giving (separately for rebate and match subsidies), income, membership status (Prospect, Lapsed), and whether the contribution exceeds thresholds for basic membership and gift levels. The second interacts the price and income variables with membership status. The base group is Continuing donors, with dummy variables for the other two types of contributors. As a proxy for family income, we use median family income for 1999 by zip code as reported by the U.S. Bureau of the Census.[10] Rather than a precise measure of family income, Income is instead an indicator of the range of income in which the donor is likely to fall. For both equations, the dependent variable as well as the price and income variables enter as natural logs; coefficients can be interpreted as elasticities.

Table 3 about here

Consider first the two models for Total Donations, including the subsidy. In the first model, we see that the elasticity of giving for the Rebate Price is -0.112, less than one in absolute value, indicating considerable crowding out: about 89 percent of the subsidy is crowded out, and the subsidy falls far short of paying for itself. The Match Price elasticity is -1.045, which is not significantly different from 1, indicating zero crowding out: the subsidy just pays for itself in increased total contributions. Income elasticity is positive, and significantly different from zero. The coefficients on member type indicate that Lapsed and Prospect giving are significantly lower on average than the baseline (Continuing) category, and Prospect giving is significantly lower than Lapsed (p-value < 0.001). Coefficients on attaining membership are positive, and similar between membership categories. Gift thresholds are positively related to donations at all gift levels.

The second model allows for interactions between membership types and the price and income variables. The Rebate Price elasticity is significantly different from zero only for Continuing members. For Match Price, the elasticity indicates the largest response to the subsidy for Continuing members; only for this category is the elasticity large enough to indicate some crowding in. Income elasticities are now insignificantly different from zero, though all remain positive. All other coefficients are similar to the first model.

For completeness, we also include parallel regressions for checkbook giving. Since under the no-subsidy and rebate treatments, checkbook giving equals total giving, these coefficients are unchanged in comparison with the Total Donations models. However, the Match Price elasticity is now insignificantly different from zero for all except Continuing members. This confirms the result that the only subsidy that pays for itself is a match subsidy, and only with Continuing members. Interestingly, the estimated checkbook-giving elasticities for rebate

and match subsidies are very similar: pairwise tests indicate that the rebate and match price elasticities are not significantly different from each other (p-value = 0.79). The subsidy only significantly affects checkbook giving for Continuing members, and the effect is about the same for both types of subsidy. Similar to the finding by Karlan and List (2007), the mere presence of a subsidy has an impact on checkbook donations by Continuing members, regardless of the type of subsidy.

Could the result be caused by the large number of members who give exactly the $42/$84 membership fee? Membership thresholds were based on checkbook giving, and members who desire only to reach this level might be more likely to ignore the subsidy. In the data analysis we control for the threshold effects on checkbook giving (in all models); this does not eliminate the observed difference between the two subsidies. An alternative approach is to eliminate the $42 contributors from the lapsed donors and the $84 contributors from the other categories. When we do this, the results are qualitatively unchanged (results available on request). We find no evidence that the results are driven by membership thresholds.

Could the results be due to differences in income tax treatment between the two subsidiy types? The equivalence of a rebate subsidy and the appropriately designed match subsidy may be complicated by donors' perceptions of the tax consequences of a rebate subsidy should the donor itemize. For both types of subsidy, only the net donation is deductible, and if responses to the subsidies are equivalent, then the tax deductible component will be exactly the same across subsidy types. For the rebate subsidy, only the net amount given is deductible (MPR's receipts were for the net amount given); equivalently, for the match, the net donation is the out-of-pocket donation, which is fully deductible. This tax equivalence may not be fully taken into account by donors when their decisions are made. Indeed, if they perceive that the "checkbook" giving is the

deductible amount, this could exaggerate the impact of the rebate on giving relative to the matching subsidy, producing a result the opposite of the one we obtain. Thus we find no evidence that perceived asymmetric tax treatment produces the observed results.

V. The Lab and the Field

Comparing lab and field data, we find qualitatively similar results: in the field, matching subsidies result in larger total contributions to the charity than rebate subsidies, echoing our lab findings. Furthermore, the quantitative estimates of the impact of the matching subsidy (but not the rebate subsidy) are remarkably close. For comparison we report estimated income and price elasticities from our laboratory study (Eckel and Grossman, 2003) side-by-side with our estimated elasticities from this filed study (see Table 4).

Table 4 about here

The estimated elasticity for the Rebate Price in the lab (-0.340) is about three times as large in magnitude as the aggregate field estimate (-0.112), and nearly twice the size of the largest estimated effect for Continuing members (-0.193). However, the Match Price estimate from the lab data is similar in magnitude to our field estimate (-1.067 and -1.045, respectively), and the difference is not statistically significant. Thus the difference between the rebate and match subsidies in their impact on giving is smaller in the lab. In particular, moving from the lab to the field does not eliminate the difference in the two subsidies, but rather increases the difference, making them even less equivalent.

The most dramatic difference is between the lab and field income elasticity estimates (0.821 versus 0.026). This is likely explained by the different concepts of income in the two studies. Neither study uses actual income. In the lab study, income is defined as endowment

which varied between $4 and $10; the field study uses median family income for 1999 by zip code as reported by the U.S. Bureau of the Census as a proxy for family income. With such different measures of income, differences in estimated income elasticities are not unexpected.

To our knowledge there is no known Chow test that permits us to test whether elasticities estimated using censored Tobit (the lab paper) and OLS are statistically different. We test whether the field estimates are different from the point estimates from the lab study by estimating the OLS model restricting the appropriate elasticities to be equal to those from the lab model. (This is of course a strong test since it effectively assumes a zero standard error for the lab estimate.) The p-values are reported in Table 4. The rebate and income elasticity estimates are significantly different across the two studies ($p < 0.001$), the match elasticity estimates are not ($p = 0.61$).

An important difference between the lab and the field is in the verifiability of the subjects' knowledge of the treatments – the match and rebate subsidy levels. The superior control inherent in laboratory experiments means that we are able to verify that subjects were aware of, and acted on the basis of, the subsidies. Moving to the field means an increase in realism, but this is at the expense of control. In the data from the MPR study, this means that we cannot verify explicitly whether subjects considered the subsidy when making their decision, although prominent notices of the available rebate and match subsidies were included in the mailings to potential donors. In addition to the notice, a mailing included a solicitation letter, a donor card, and a return envelope, so it is not impossible that some donors "missed" the subsidy notice. To our knowledge, this lack of verification is a feature of every other field study of charitable giving that has been conducted using a mail-in campaign.

Even if all donors are aware of the subsidy, donations may be "sticky", and, like the subjects of Davis, Millner and Reilly (2005), some donors may fail to fully adjust their behavior to the subsidy. If donors fail to notice a subsidy, or fail to adjust their behavior, this alone would make a matching subsidy more attractive to a charity. It is clear that cash rebates are not effective in charitable campaigns, explaining why they are never used.[11]

VI. Conclusions

We present results of a field experiment designed to test the impact of alternative ways to subsidize charitable giving. A rebate subsidy of s_r and a matching subsidy of rate $s_m = s_r/(1 - s_r)$ present a donor with the same net cost of giving to the charity and therefore should result in the same level of giving. Laboratory experimental results contradict this prediction: contributions are significantly higher with matching subsidies than with rebate subsidies, and the estimated match price elasticity of giving is between two and three times greater than the rebate price elasticity (Eckel and Grossman, 2003).

Our main result confirms the lab finding that matching subsidies result in larger total donations to charities than rebate subsidies. In the field, the estimated price elasticity for the matching subsidy is similar to that in the lab, but the rebate elasticity is smaller in magnitude: the matching elasticity is more than five times the rebate elasticity. On average, while the rebate subsidy increases out of pocket giving, especially for Continuing members, the increase is not enough to offset the cost of the subsidy. Matching a donor's contribution appears to leave out of pocket contributions largely unchanged; the match crowds out a portion of giving only for Lapsed members, and crowds in a small portion of giving for Continuing members. Controlling

for other factors (income, prior giving status, gift levels), our analysis provides evidence that a program to match contributions is superior to one that rebates contributions.

This larger disparity in the impacts of rebate and matching subsides in the field is not surprising given the differences between the lab and the field. The superior control of the lab allowed us to eliminate some of the more obvious differences between rebates and matches. In the lab subjects did not have to wait for a rebate: we are able to give "instant" rebates: rebate payments are given at the end of the lab session, as is typical for lab payments. In addition, there was no uncertainty about whether the rebate would be paid, or the source of the funds to pay for it. Although we included this information in the MPR mailing, not all donors may have noticed it. Taking the lab results at face value -- assuming the lab results constitute a reasonable estimate of differences in donor response to matching and rebate subsidies -- one might anticipate a larger difference in the field. This finding validates the lab estimates of the difference in responsiveness of donations to match and rebate subsidies.

If donors indeed noticed the subsidies and took them into account, why might the subsidies have such different impacts on total giving? Bénabou and Tirole (2006) suggest a reason why rebate subsidies may be less effective than matching subsidies. They argue that 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. Investigating this hypotheses must wait for a different experimental design.

Subsidizing donations affects the total donations received by the charity. Our results show that, for a given budget allocated to subsidizing charities, matches are much more effective

than rebates at increasing the flow of funds to the charities. This result relies on the observation that when faced with a rebate opportunity, donors do not fully increase their donations to offset the rebate. In addition, a matching subsidy produces a small amount of "crowding in" for Continuing donors, the most responsive category of donors. Although we would not want to make such a recommendation without further study and extensive piloting, if this nonequivalence result were to continue to hold in a longer-term policy context, the implication would be that replacing the current tax deductibility of contributions with an equivalently-costly matching subsidy system could lead to an increase in the flow of funds to charitable organizations.

Field studies can provide valuable information that is unavailable in lab environments, but that information comes at a cost. The degree of control that is available in the lab is foregone in the field, and in our experience there are always unanticipated elements in field designs. For example, it is possible that including a survey request in the subsidy treatments may have decreased response rates for the charity's most loyal donors, the Continuing members. Several other aspects of the design could also be improved upon in a follow up study. However, this experiment cost nearly $40,000 in subsidies, plus a substantial administrative cost billed by the charity. Given the lower level of control, the possibility of mistakes on the part of the charity, limitations imposed by Internal Review Boards for human subjects research and restrictions placed by the organizations themselves, it is important to weigh the costs of field studies against what can be gained. Special care must be taken to pretest and refine designs before going in the field; unintended consequences are nevertheless likely, and much more difficult to recover from than lab experiments.

Our opinion is that the best studies of charitable giving will be the result of long term partnerships between researchers and charitable organizations (e.g. the ongoing work of Rachel Croson and Jen Shang with National Public Radio, funded by the organization). Longitudinal studies are needed to fully understand the impact of a subsidy. In our own case, the effect of the subsidies may well have adjusted more fully if they were used in several sequential fundraising campaigns. However, questions also arise about the effect of removing a subsidy once a donor has responded to one. Will long term giving remain higher, or will a donor lapse to previous levels, or worse, reduce giving when subsidies are not available? These questions are beyond the scope of this study, but we hope to answer questions like this in future research.

Endnotes

[1] Thus subsidy rates vary depending on whether a taxpayer itemizes deductions (the subsidy is zero for non-itemizers) and the taxpayer's marginal tax rate, which at the time of the study ranged from 15 percent for family incomes below $45,200 to 35 percent for incomes over $297,450.

[2] 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.

[3] http://www.taxfoundation.org/taxdata. In the later part of the study, the lowest marginal tax rate dropped to 10% for incomes below $12,000.

[4] Budget limitations required the overall maximum paid out in rebates, matches, and survey incentives to be capped. Subsidy-specific caps were included on the respective flyers in fine print at the bottom (see Appendix).Caps were as follows: 20% rebate: $4,500; 25% match: $7,500; 25% rebate: $8,000; and 33 1/3% match: $15,000.The caps distributed our limited funds in a way that reflected the authors' best guesses of how people would respond to the subsidies based on information provided by MPR on average contributions and response rates, and results from the laboratory experiment in Eckel and Grossman (2003). In the end, we did not need to impose the caps, but rather were able to shift our budgeted amounts marginally between categories so as to complete the subsidies for all treatment-group contributors. In a similar study, Karlan and List (2007) show that caps do not impact giving.

[5] MPR projects approximately a 30 percent response rate for Continuing members, 0.8 – 1.0 percent for Lapsed members, and 0.4 – 0.5 percent for Prospects (from conversations with Al Anderson, Director, Membership Marketing, MPR, May 5, 2000).

[6] Note MPR released data only on responses, not mailings.

[7] This analysis, including maps, is available on request from the authors.

[8] It is widely accepted that higher-education individuals are more willing to participate in surveys, as are targets with stronger ties to the relevant topic or organization (Dillman, 2000; Groves and Couper, 1998; Tourangeau, Rips and Rasinski, 2000). If some Continuing members were averse to participating, this effect should be concentrated among lower-education, and therefore lower-income targets. However, we see no income differences between the no-subsidy and subsidy categories of respondents.

[9] The regular annual membership fee is $84; however, MPR offers Prospects the opportunity to purchase membership at half price.

[10] U.S. Bureau of the Census, Census 2000, Summary File 3.

[11] In future studies we plan to add an additional hurdle for receiving the subsidy that requires the subject to indicate, by checking a box, their intention to receive the subsidy. In addition, we plan to collect basic demographic and income data from the subjects in order to test and control for inadvertent failures of random sampling.

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Figure 1. Distribution of Contributions by Donor Type.

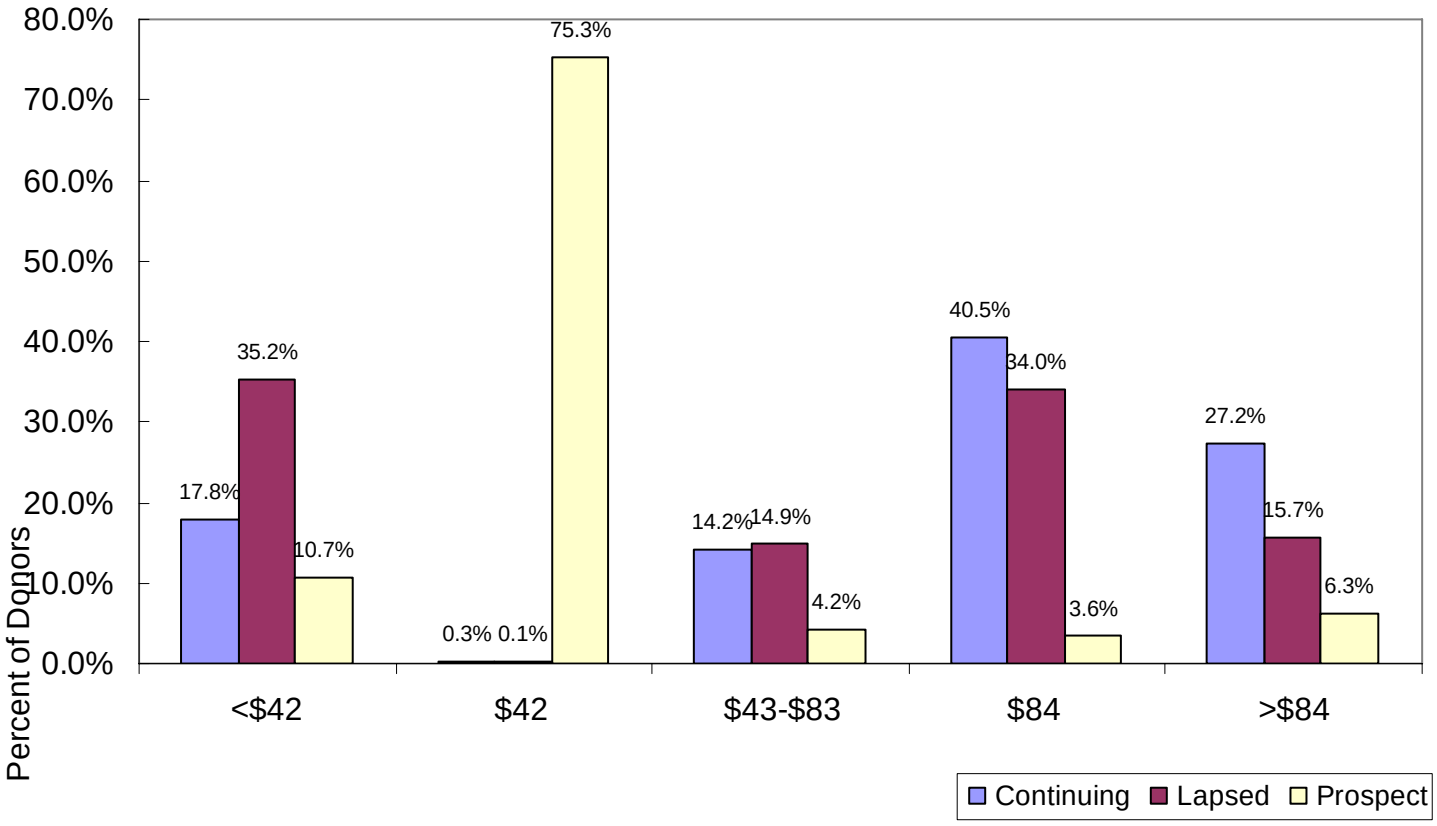


Table 1. Summary Statistics by Donor and Subsidy Type.

Subsidy	Mailed	Responses (Rate)	Checkbook Revenue per Solicitation	Total Revenue per Solicitation	Useable Responses*	Mean Checkbook Donation (Std. Dev.)	Mean Total Donation (Std. Dev.)
Continuing							
No subsidy	14,890	4,262 (28.6%)	$30.97	$30.97	4,207	$85.15 (70.11)	$85.15 (70.11)
20% Rebate	1,200	266 (22.1%)	$21.62	$21.62	266	$97.83 (61.11)	$97.83 (61.11)
25% Match	1,200	230 (19.2%)	$16.50	$20.62	227	$86.18 (52.14)	$107.72 (65.17)
25% Rebate	1,200	248 (20.7%)	$17.73	$17.73	248	$85.79 (43.74)	$85.79 (43.74)
33 1/3% Match	1,200	237 (19.8%)	$16.60	$22.13	235	$84.38 (73.65)	$112.50 (98.20)
Total	19,690	5,243 (26.6%)	$27.84	$28.43	5,183	$85.84 (68.14)	$88.06 (70.34)
Lapsed							
No subsidy	15,000	130 (0.9%)	$0.55	$0.55	127	$64.37 (38.60)	$64.37 (38.60)
20% Rebate	15,000	116 (0.8%)	$0.54	$0.54	115	$70.26 (42.30)	$70.26 (42.30)
25% Match	15,000	140 (0.9%)	$0.58	$0.73	139	$63.19 (34.39)	$78.98 (42.98)
25% Rebate	15,000	144 (1.0%)	$0.60	$0.60	141	$62.36 (39.26)	$62.36 (39.26)
33 1/3% Match	15,000	150 (1.0%)	$0.62	$0.83	148	$62.93 (46.63)	$83.91 (62.17)
Total	75,000	680 (0.9%)	$0.58	$0.65	670	$64.39 (40.46)	$72.31 (47.02)
Prospect							
No subsidy	157,805	778 (0.5%)	$0.23	$0.23	769	$47.22 (27.56)	$47.22 (27.56)
20% Rebate	30,000	154 (0.5%)	$0.25	$0.25	151	$49.29 (45.11)	$49.29 (45.11)
25% Match	30,000	174 (0.6%)	$0.29	$0.36	171	$50.00 (35.04)	$62.50 (43.80)
25% Rebate	30,000	119 (0.4%)	$0.19	$0.19	119	$47.02 (25.30)	$47.02 (25.30)
33 1/3% Match	30,000	133 (0.4%)	$0.20	$0.26	132	$44.80 (18.22)	$59.74 (24.30)
Total	277,805	1,358 (0.5%)	$0.23	$0.25	1,342	$47.55 (30.15)	$50.61 (32.46)

[a] * - Accurate zipcodes were provided.

Table 2. Variable Definitions.

Variable	**Definition**
Contributions	Dollar contributions to MPR
Rebate Price	Price of giving $1 to charity (Rebate subsidy)
Match Price	Price of giving $1 to charity (Match subsidy)
Income	Average income for zip code
Prospect	=1 if the donation is from a Prospect member
Lapsed	=1 if the donation is from a Lapsed member
Membership	=1 if checkbook Contribution reaches the membership threshold ($42 for Prospect, $84 otherwise)
G120 – G1000	Dummy variable = 1 checkbook Contributions reach the relevant gift threshold (e.g., G120 = 1 if checkbook contributions are at least 120)

Table 3. Amount Donated Regression.
(Dependent variable: ln(Checkbook Contributions), ln (Total Contributions)

	Coefficient (Std. Error)			
Variable	**Checkbook+**		**Total++**	
ln(Rebate Price)	-0.112* (0.04)	…	-0.112* (0.04)	…
ln(Rebate Price) x Prospect	…	-0.016 (0.08)	…	-0.016 (0.08)
ln(Rebate Price) x Lapsed	…	0.245 (0.20)	…	0.245 (0.20)
ln(Rebate Price) x Continuing	…	-0.193* (0.05)	…	-0.193* (0.05)
ln(Match Price)	-0.045 (0.04)	…	-1.045* (0.04)	…
ln(Match Price) x Prospect	…	-0.069 (0.07)	…	-1.069* (0.07)
ln(Match Price) x Lapsed	…	0.357 (0.20)	…	-0.642* (0.20)
ln(Match Price) x Continuing	…	-0.099* (0.05)	…	-1.099* (0.05)
Income	0.026* (0.01)	…	0.026* (0.01)	…
Income x Prospect	…	0.037 (0.03)	…	0.037 (0.03)
Income x Lapsed	…	0.072 (0.06)	…	0.072 (0.06)
Income x Continuing	…	0.014 (0.01)	…	0.014 (0.01)
Prospect	-0.686* (0.05)	-0.773* (0.14)	-0.686* (0.05)	-0.773* (0.14)
Lapsed	-0.146* (0.02)	-0.301 (0.25)	-0.146* (0.02)	-0.301 (0.25)
Membership x Prospect	0.962* (0.05)	0.961* (0.05)	0.962* (0.05)	0.961* (0.05)
Membership x Lapsed/Continuing	0.914* (0.01)	0.914* (0.01)	0.914* (0.01)	0.914* (0.01)
G120	0.415* (0.01)	0.415* (0.01)	0.415* (0.01)	0.415* (0.01)
G180	0.431* (0.02)	0.430* (0.02)	0.431* (0.02)	0.430* (0.02)
G240	0.239* (0.02)	0.238* (0.02)	0.239* (0.02)	0.238* (0.02)
G360	0.348* (0.01)	0.353* (0.01)	0.348* (0.01)	0.353* (0.01)
G500	0.363* (0.04)	0.361* (0.04)	0.363* (0.04)	0.361* (0.04)
G1000	0.841* (0.14)	0.838* (0.14)	0.841* (0.14)	0.838* (0.14)
CONSTANT	3.437* (0.05)	3.483* (0.06)	3.437* (0.05)	3.483* (0.06)
R^2	77.4	77.5	77.4	77.4
Log Likelihood Function	-1741	-1732	-1889	-1733
N	7195	7195	7195	7195

[a] + Dependent variable = checkbook contribution
[b] ++ Dependent variable = total received by MPR
[c] * significant at the 5% level, two-tailed test.

Table 4: Comparison of Lab and Field Elasticity Estimates

	Estimates (Std. Error)		*p*-value[a]
Elasticity	Field	Laboratory	
Income	0.026 (0.01)	0.821 (0.07)	0.00
Price – Rebate	-0.112 (0.04)	-0.340 (0.19)	0.00
Price– Match	-1.045 (0.04)	-1.067 (0.18)	0.61

a The reported p-values are tests of the restriction that the relevant elasticity to be equal to the reported point-estimate from the lab studies, using the third model in Table 3,.

Appendix

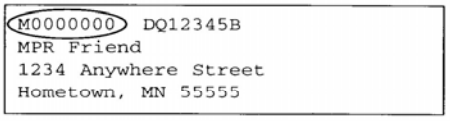
Rebate Mailing

Here is how you can get a 20% Rebate on your membership contribution to Minnesota Public Radio

Get a rebate on your membership contribution

MPR is participating in a study examining contributions to nonprofit institutions. The study is being conducted by researchers at Saint Cloud State University and Virginia Polytechnic Institute and State University in Blacksburg, Virginia (Study Group). As part of this study, you will receive a 20% rebate* of your contribution to MPR. (For every $1 you contribute the researchers will rebate you $0.20.)

Regardless of your donation choice, the Study Group would appreciate your completing an online, internet questionnaire developed by the researchers. To complete the survey, please go to www.MPRsurvey.vt.edu** and enter the code number above your name on the contribution form (see circle at right). If you do not have internet access, please call 800-228-7123 to receive a copy of the survey in the mail. Your responses to the survey will be held in the strictest confidence and your anonymity will be protected. The researchers will not be provided any identifying information. For every completed survey, $1 will be donated by the researchers to MPR (up to a maximum of $1,000).



Philip Grossman
Study Group Head

* The study will refund contributions in the order in which they are received until an overall maximum of $4,500 is reached.
** At the completion of this project a description of the study and a brief summary of the results will be posted at this Internet address.

Match Mailing

Here is how you can increase your membership contribution to Minnesota Public Radio by 25%

Have your membership contribution matched

MPR is participating in a study examining contributions to nonprofit institutions. The study is being conducted by researchers at Saint Cloud State University and Virginia Polytechnic Institute and State University in Blacksburg, Virginia (Study Group). As part of this study, your contribution to MPR will be matched at the rate of 25%*. (For every $1 you contribute the researchers will contribute an additional $0.25.)

Regardless of your donation choice, the Study Group would appreciate your completing an online, internet questionnaire developed by the researchers. To complete the survey, please go to www.MPRsurvey.vt.edu** and enter the code number above your name on the contribution form (see circle at right) If you do not have internet access, please call 800-228-7123 to receive a copy of the survey in the mail. Your responses to the survey will be held in the strictest confidence and your anonymity will be protected. The researchers will not be provided any identifying information. For every completed survey, $1 will be donated by the researchers to MPR (up to a maximum of $1,000).

M0000000 DQ12345B
MPR Friend
1234 Anywhere Street
Hometown, MN 55555

Philip Grossman
Study Group Head

* The study will match individual contributions in the order in which they are received until an overall maximum of $7,500 is reached.
** At the completion of this project a description of the study and a brief summary of the results will be posted at this Internet address.

Donor gift thresholds:

Member Benefits

Benefits	Annual Membership			Contributors (Annual membership included)			
	$84/YR	$120/YR	$180/YR	$240/YR	$360/YR	$500/YR	$1000/YR+
Early purchase opportunities for A Prairie Home Companion® tickets at the Fitzgerald Theater St Paul, MN	■	■	■	■	■	■	■
Twelve issues of Minnesota Monthly® with MPR's listening guide	■	■	■	■	■	■	■
MemberCard benefits with 2-for-1 and other discounts at restaurants, events and shops throughout the MPR listening region	■	■	■	□	■	■	□
Personal tour of MPR studios		■	■	■	■	■	■
Public Radio MusicSource™ discount			□	■	□	■	■
@MPR, a special newsletter for contributors				□	■	■	■
Personal on-air message(s)					1	2	3
Recognition in MPR's annual report to funders					■	■	■
Invitations to breakfasts with nationally-known public radio journalists						■	■
Invitations to recording sessions, house concerts and other unique events							■

Minnesota Public Radio invites all levels of support. Membership benefits, including advance purchase opportunities for *A Prairie Home Companion*® tickets, are provided at the $84 plus levels. All Friends of Minnesota Public Radio will receive an MPR Friends Card, current program schedule and have access to special one time 2-for-1 ticket offers as benefits.

TAX INFORMATION

CONTRIBUTIONS TO MPR are tax deductible to the extent that tangible personal benefits are not received in return. If you receive a premium as a thank-you for your contribution, MPR will tell you the value of that premium and whether its value is substantial enough that it must be deducted from your contribution. If you receive Minnesota Monthly®, you must deduct the fair market value of the magazine ($14.95) from your contribution, plus the value of any other premium or tangible benefit you receive.

EMPLOYER MATCHING GIFTS

YOU MAY BE ELIGIBLE to have your contribution matched by your employer. Please check with your business or personnel office to see if your company has a matching gift program.

7/01