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Transactions Costs in Charitable Giving: Evidence from Two Field Experiments*

Imran Rasul and Steffen Huck

Abstract

In large-scale fundraising campaigns based on direct mailings, typically less than 5% of individuals donate to the charitable cause. We present evidence from two field experiments designed to measure the existence of transaction costs that inhibit charitable giving in such fundraising campaigns, and shed light on the nature of such transaction costs. The experiments are designed in conjunction with the Bavarian State Opera House. The first mail-out experiment was implemented over two stages using a within-subject design. We develop a theoretical framework that makes precise the identifying assumptions under which we can exploit this two-stage design to measure the following structural parameters among potential donors: (i) the share of donors who would make a strictly positive donation in the complete absence of transaction costs and (ii) the probability that a potential donor has sufficiently low transactions costs to make a strictly positive donation. Our results imply response rates to mail-out solicitations would almost double in the complete absence of transaction costs. The second field experiment provides more evidence on the nature of transaction costs. We distinguish between *ex ante* transaction costs, which prevent the choice problem from being considered and *ex post* transaction costs, which prevent choices being implemented. We find that the likelihood of a donation being made increases by 26% in response to even a small reduction in *ex post* transaction costs.

KEYWORDS: charitable giving, field experiment, reminders, transaction costs

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1 Introduction

This paper presents evidence from two field experiments on the effect of individual transactions costs on charitable giving. Our study is motivated by the observation that charitable fundraising campaigns based on mailing solicitations typically elicit positive donations from less than 5% of solicitees. This finding has been confirmed by a series of large-scale field experiments in a variety of empirical settings [Eckel and Grossman 2003, Falk 2007, Karlan and List 2007, Huck and Rasul 2008]. Moreover, such low response rates are prevalent despite: (i) charitable organizations targeting individuals that typically have some affinity to the fundraising organization, and so might place higher value on the goods they provide than other randomly selected individuals; (ii) charitable organizations often engaging in repeat solicitations [Bekkers and Weipking 2007].¹

If individuals choose not to give this could be due to them either not valuing the charitable good sufficiently highly, or because of prohibitive transaction costs. These transaction costs might for example be related to the time costs of decision making. It is important to distinguish between these explanations for why individuals do not give because each has different implications for the design of fundraising schemes. For example, if non-response is because individuals do not value or feel warm glow towards the good, then such individuals should not be targeted in the first place. Alternatively, if non-response is due to transaction costs, attempts to change default options or to reduce the transactions costs of making and implementing decisions can have large effects on outcomes.²

Much of the economics literature on charitable giving falls into one of three strands, and our study relates to all three. The first strand of the literature

¹Response rates are higher with more personal forms of solicitation, such as door to door solicitations [Landry *et al.* 2006, DellaVigna *et al.* 2009]

²For example, the effect of changing default choices in organ donation has for example been documented to have dramatic effects on behavior [Johnson and Goldstein 2003]. It is now also well recognized that consumers tend to over use default or first presented choices in a variety of settings such as savings and voting behavior, despite low switching and search costs [Madrian and Shea 2001, Rubinstein and Salant 2006].

studies the determinants of individual giving such as altruism or warm glow [Andreoni 1989, 1990, Fehr and Gächter 2000]. Recent evidence also suggests individuals might give because of social concerns or social pressure [Akerlof and Kranton 2000, DellaVigna *et al.* 2009]. The second strand studies the related question of why fundraisers exist in the first place even if individuals feel some warm glow towards the fundraising project [Andreoni 2006, Romano and Yildirim 2001, Vesterlund 2003, Potters *et al.* 2005, 2007]. Our study emphasizes the role that transactions costs might have in inhibiting individual giving behavior even if potential donors feel warm glow towards the project.

The third strand of the literature focuses on the extensive and intensive margins of giving, namely understanding the determinants of why individuals give at all, and understanding how much is given conditional on some positive donation being made. This includes studies on matching and rebate schemes [Eckel and Grossman 2003, Karlan and List 2007, Huck and Rasul 2008], lead gifts [List and Lucking-Reiley 2002], gift exchange [Falk 2007], door-to-door solicitations [Landry *et al.* 2006], social recognition [Andreoni and Petrie 2004], and social comparisons [Frey and Meier 2004, Croson and Zhang 2009]. Our study also builds on the smaller literature examining the specific role of repeat solicitations or reminders in charitable giving [Diamond and Noble 2001, Van Diepen *et al.* 2006, Bekkers and Weipking 2007, Meer and Rosen 2009].³

We present evidence from two field experiments to identify whether non-response is due to transactions costs, and to estimate the sensitivity of choices to small changes in transactions costs holding constant other causes for non-response. The first experiment also sheds light on the profitability of using repeat solicitations, and the second explores the precise nature of transaction costs. Both experiments are conducted in conjunction with the Bavarian State Opera in Munich. Taken together, these field experiments shed light on the

³Despite the prevalence of reminders to take actions, the effects of reminders has not been much studied in the context of charitable giving. The closest literature remains that on recall bias where agents incorrectly recall their priors when presented with new information [Fischhoff and Beyth 1975]. The marketing and survey design literatures study the use of reminders using field experiments to compare alternative means of eliciting survey responses or inducing actions [Linsky 1975, Church *et al.* 2004]. In general, less attention is given to understand why and how individuals respond to reminders [Koo and Rohan 1996].

existence, magnitude, and nature of transactions costs in charitable giving.

The first field experiment, referred to as the ‘reminder experiment’, is implemented using a within-subject design over two phases. The first phase involved a mail-out of letters to 5,000 individuals designed to elicit donations for a social youth project the opera was engaged in. Donations were received to this original mail-out over the next four weeks. Six weeks after the original mail-out, when there were no longer any donations being given in response to the original solicitation, we implemented a second phase in which non-donors to the original mail-out were sent a reminder to donate. We develop a theoretical framework that makes precise the identifying assumptions under which we can exploit this two-stage design to measure the following structural parameters among potential donors: (i) the share of recipients who would donate some positive amount to the cause in the absence of transaction costs; (ii) the probability that any given recipient draws sufficiently low transactions costs to make a strictly positive donation.

Our results imply that in the complete absence of transaction costs, between 6 and 7% of solicitees would prefer to make some strictly positive donation, which compare to actual response rates of between 3 and 4%. In other words, around 46% of the recipient population experience sufficiently high transaction costs that prevent them from actually responding positively to the fundraising call. Hence in this context response rates could almost double in the complete absence of transaction costs. Overall, we find that the behavior of recipients in response to reminders is such that it is profitable for the fundraiser to remind recipients to donate.

Our second field experiment, which we refer to as the ‘transaction costs’ experiment, relates to exactly the same fundraising project but two years later. This field experiment estimates how sensitive individuals are to changes in transaction costs by soliciting donations from 25,000 individuals that are randomly allocated to four treatments that exogenously vary transaction costs. We explore whether such costs are predominantly *ex ante*, so that non-response occurs because recipients do not consider the choice problem in the first place, or whether they are *ex post* in the sense that individuals do not implement

their choice. While it is virtually impossible to reduce transaction costs to zero, our experiment provides an indication of how reasonable the estimates from the first field experiment are, by reducing transactions costs incrementally. We find that a small reduction in *ex post* transaction costs increased response rates by 26% relative to the baseline treatment. We take this as evidence that the estimate from the first field experiment of a potential doubling of response rates is not that far off the mark.

On the economics of charitable giving, our analysis implies that devoting more effort to changing behavior on the extensive margin of charitable giving might be more cost effective than fundraising schemes aimed at changing recipient behavior on the intensive margin. In more general settings, our analysis highlights the possibility of designing ways to reduce transaction costs so market outcomes better reflect individual preferences, as well as thinking through default choices in cases where transactions costs cannot be easily reduced.

The paper is organized as follows. Section two describes the design, conceptual framework, and results from the reminder field experiment. Section three discusses the transaction costs field experiment. Section four discusses the implications and external validity of our results using data from a similarly scaled field experiment conducted with the Royal Opera House in London.

2 The Reminder Experiment

2.1 Design

The reminder field experiment is implemented in two phases, six weeks apart. The first phase took place in June 2006 when the Bavarian State Opera organized a mail-out of letters to around 5,000 individuals designed to elicit donations for a social youth project the opera was engaged in, called ‘Stück für Stück’.⁴ The project’s beneficiaries are children from disadvantaged fami-

⁴A number of other treatments were also implemented in the first and second phases of the field experiment, as described in more detail in Huck and Rasul [2008]. In this paper we focus on the subset of treatments that shed light on the nature of transactions costs in charitable giving.

lies whose parents are almost surely not among the recipients of the mail-out. Hence the fundraising campaign relates to a public goods project that conveys no immediate benefits to potential donors, where the public good is continuously expandable.⁵

The recipients were randomly selected from the opera's database of customers who had purchased at least one ticket to attend either the opera or ballet, in the twelve months prior to the mail-out. Recipients were randomly assigned to one of two treatments. These varied in terms of whether information was conveyed about the existence of an anonymous lead donor. Respondents were truthfully told that the lead donor had provided an unconditional lead gift of €60,000. The mail-out letters were therefore identical in both treatments with the exception of one paragraph relating to the existence of the lead donor. The wording of this key paragraph read as follows,

Control: *This is why I would be glad if you were to support the project with your donation.*

Lead Donor: *A generous donor who prefers not to be named has already been enlisted. He will support "Stück für Stück" with €60,000. Unfortunately, this is not enough to fund the project completely which is why I would be glad if you were to support the project with your donation.*

Six weeks later we implemented the second phase of the reminder experiment. We sent non-donors to the original mail-out a reminder to donate. These non-donors were randomly assigned into two reminder treatments: a control group in which no reminder was sent, and a simple reminder letter in which recipients were reminded about the fundraising project. The reminder letter simply offered recipients another "opportunity to make a donation". For those previously in the lead donor treatment, the reminder letter did not again mention the existence of the lead donor. The precise format and wording of each mail-out letter is in the Appendix.⁶

⁵The project finances small workshops and events for schoolchildren with disabilities or from disadvantaged areas. These serve as a playful introduction to the world of music and opera. It is part of the Bavarian State Opera's mission to preserve the operatic art form for future generations and the project is therefore a key activity to fulfill this mission.

⁶A number of other treatments were also implemented in the first and second phases of

Table 1: The Design of the Reminder Experiment

		Phase One Treatment	
		Control	Lead Donor
Phase Two Treatment	Reminder	Control-Reminder (1430)	Lead donor-Reminder (1431)
	No Reminder	Control-No Reminder (715)	Lead donor-No Reminder (720)

Notes: The numbers in parentheses are the number of recipients in each treatment combination. Only non-donors to the original mail-out treatment in phase one are treated in phase two. There are around 250 individuals that donated in the original phase one treatments.

The reminder experiment generates the 2×2 design shown in Table 1. The number of recipients in each treatment is shown in parentheses. Only individuals that did not respond to the original solicitation and therefore were sent a reminder letter are included in the figures. *A priori*, we did not expect many recipients of the original mail-out to respond after six weeks, when the reminder letters began to be distributed. Hence individuals were twice as likely to be assigned to the reminder treatment in the second phase, than to the second phase control group of receiving no reminder. As discussed below, in actuality there were indeed zero donations made after six weeks, with 95% of donations to the original mail-out being made within a month.

Recipients in each treatment face an identical budget set. A one Euro donation always leads to exactly one Euro being received by the fundraiser, and no additional information on total amounts raised for example, becomes available to recipients between the two phases. As the original mail-out and reminder letters are sent six weeks apart, it is also plausible that there are no significant changes in incomes or in relative prices between the original and reminder solicitations. Of course if there are large changes in the budget constraint over time, then we could observe individuals responding even in the absence of a reminder letter, which is not the case.

At both stages of the experiment, recipients are randomly assigned into treatments as shown in Table 2. Recipient characteristics are available in the opera house's database, which records details on individuals that have purchased an opera ticket in the recent past. Given randomization, recipients are not significantly different to each other across the treatments in either phase. In common with many fundraising drives, mail-out recipients are likely to have higher affinity towards projects organized by the fundraiser than the average individuals. Annually, they purchase on average 6.3 opera tickets and expenditures on opera tickets are over €400. Hence for any given realization of transaction costs, these targeted recipients should be more likely to give

the field experiment. In this paper we aim to shed light on the nature of transactions costs in charitable giving. Hence to keep clear the exposition, we focus on the subset of treatments in which the information available to recipients and the choice sets remain constant over the two phases.

Table 2: Random Assignment of Recipients into Treatments, by Experimental Phase

Mean, standard error in parentheses

P-value on test of equality of means with control group in box brackets

Treatment Description	Number of Recipients	Female [Yes=1]	Number of Tickets Bought in Last 12 Months	Number of Ticket Orders in Last 12 Months	Average Value of Tickets Bought in Last 12 Months	Total Value of All Tickets Bought in Last 12 Months	Munich Resident [Yes=1]	Year of Last Ticket Purchase [2006=1]
Phase One: Control	3787	.466 (.008)	6.30 (.178)	2.23 (.047)	86.6 (.666)	416 (7.88)	.416 (.008)	.565 (.008)
Phase One: Lead donor	3770	.478 (.008) [.269]	6.27 (.153) [.906]	2.22 (.047) [.838]	86.3 (.650) [.687]	423 (7.73) [.541]	.416 (.008) [.980]	.574 (.008) [.420]
Phase Two: Control	4253	.479 (.008)	6.11 (.145)	2.19 (.045)	86.1 (.615)	406 (6.80)	.418 (.008)	.555 (.008)
Phase Two: Reminder	8470	.470 (.005) [.341]	6.19 (.094) [.623]	2.20 (.032) [.847]	85.7 (.432) [.589]	415 (5.62) [.313]	.426 (.005) [.394]	.568 (.005) [.160]

Notes: The tests of equality are based on an OLS regression allowing for robust standard errors. The "year of first entry" is the year in which the respondent is first recorded to have bought an opera ticket. All monetary amounts are in Euros.

to a fundraising project organized by the opera house than individuals not recorded on the opera’s database.⁷

Finally, we note that recipients are told the truth—the lead gift was actually provided. The opera had no explicit fundraising target in mind, nor was any such target discussed in the mail-out. The money raised for the project is not used to finance one large event but rather a series of several smaller events, as made clear in the mail-out letter. Hence the project is of a linearly expandable nature such that recipients know that marginal contributions will make a difference.⁸

2.2 Conceptual Framework

In Huck and Rasul [2009] we show the data from the first phase of the experiment can be organized within a familiar framework of individual utility maximization where individuals are assumed to have complete, transitive, continuous, monotone, and convex preferences over two arguments—their private consumption, c , and the donation given for the project, g . Hence in the absence of transaction costs each individual’s utility maximization problem is,

$$\max_{c, g} u(c, g) \text{ subject to } c + g \leq y, \quad c, g \geq 0, \quad (1)$$

where we normalize the price of consumption to one. Hence in this framework individuals potentially feel warm glow towards the project and would find it optimal to provide some strictly positive donation in the complete absence of transaction costs.

When non-donors to the original mail-out are reminded to donate six weeks

⁷Treatment assignment is also independent across the two design stages, as shown in Table A1. Here we estimate an ordered probit regression of the second phase treatments against the first phase treatment, using all the treatments actually implemented in the two phases, we find no significant correlation between the treatment assignments in each phase, as expected under random assignment.

⁸The effects of such seed money are in general ambiguous and depend on whether the project has an explicit target, and on whether individuals believe the project is far from, or close to, its designated target, and whether these beliefs encourage or discourage donations [List and Lucking-Reiley 2002, Vesterlund 2003, Andreoni 2006, Rondeau and List 2008].

later in the second phase of the experiment, the null hypothesis is that such individuals should still not donate, assuming there have been no changes in preferences, relative prices, or incomes over the six week period. All else equal, standard theory implies if individuals found it optimal not to donate to the original mail-out, then they should still find it optimal not to donate when reminded to do so.

The alternative hypothesis is that the mere receipt of a reminder to donate triggers or cues a new draw from the same distribution of transaction costs [Laibson 2001]. Hence an individual may be observed to donate once reminded even if they had not donated when faced with a nearly identical choice problem six weeks earlier. The distribution of transaction costs might of course vary across donors because some face higher time costs than others. Our analysis focuses on the existence of transaction costs. Given the very nature of low response rates to fundraising drives, it is difficult to generate sufficiently large numbers of responses to make meaningful inference about such transactions costs might vary across donors, and we can at best offer only suggestive evidence on this.

With a single fundraising mail-out, transaction costs would not be identified because non-response could be due to high transaction costs or underlying preferences. However, our two-stage experimental design identifies the share s of recipients who would donate some positive amount in the absence of transaction costs, and the probability t that an individual draws sufficiently low transactions costs that enables her to implement her optimal strictly positive donation. To see this note that the response rates for the two phases of the reminder experiment, p and q respectively, can be expressed in terms of s and t as follows,

$$p = st, \tag{2}$$

$$q = s(1 - t)t,$$

where recall that only non-donors to the original mail-out are reminded to donate in the second phase. Hence the response rate to the original mail-out p is the number of donors divided by the number of mail-out recipients, and

the response rate to reminders q is defined as the number of individuals that donate in response to a reminder in the second phase, divided by the number of non-donors to the original mail-out. By observing response rates across both phases we can then identify the following underlying structural parameters: (i) the share of recipients who would donate some positive amount in the absence of transaction costs s ; and, (ii) the probability that a recipient draws sufficiently low transactions costs to make a strictly positive donation, t , as,

$$s = \frac{p^2}{p - q}, \quad (3)$$

$$t = \frac{p - q}{p}. \quad (4)$$

We are able to recover these structural parameters in our field experiment under two identifying assumptions. First, the reminder letter contains no information about the charitable project that changes recipient preferences. For example, recipients do not update their beliefs about the project quality from the mere receipt of a reminder letter. If so this would alter the marginal rate of substitution between private consumption c and donations given g and could affect the likelihood that they would give even in the absence of any transaction costs, s .

There are a number of reasons to believe the reminder in our experiment does not serve as a signal of the project quality or otherwise change recipient preferences. First, as documented in Bekkers and Weipking [2007] it is common practice among fundraisers to send reminders or engage in repeated solicitations and so individuals would not be unusually surprised to receive such a reminder. Second, the reminder letter is rather sparse in content, as shown in the Appendix, and provides no information on total amounts raised for example. Third, we exploit the fact that we can compare responses to the reminder treatment between individuals that were exposed to the control treatment in the first phase or to the lead donor treatment. The latter group are likely to have already received a signal of the project quality from the presence of the lead donor [Vesterlund 2003, Andreoni 2006]. Hence if the mere

receipt of the reminder affects preferences, it should do so differentially across these histories of first phase treatments. Finally, the model makes precise that if the reminder letter conveys any information about the project, then conceivably $q > p$ and the estimated parameters would be negative.

The second identifying assumption is that there are no systematic changes in preferences, relative prices, or incomes over the six weeks between the experimental phases. Hence changes in behavior across the stages of the experiment do not represent changes in the budget set available to recipients. Of course if there are large changes in budget constraints over time, then standard theory suggests we should observe individuals responding even in the absence of a reminder letter. This is controlled for in the Control-No Reminder treatment combination.

2.3 Results

Columns 1 and 2 of Table 3 show response rates and donations given for the first phase of the reminder experiment. The rows split these outcomes for the control and lead donor treatments. Response rates are 3.7% and 3.5% in the two treatments. These magnitudes are not significantly different from each other, as reported at the foot of Table 3, and are in line with the low response rates found in other comparable large-scale field experiments on charitable giving. In contrast, on the intensive margin of giving there is a large effect of the mere presence of a lead gift. Conditional on giving, donations almost double in size from €74.3 in the control treatment to €132 in the lead donor treatment. In Huck and Rasul [2008] we discuss this difference in much more detail. Our emphasis in this paper is on the extensive margin of whether recipients give at all, and how this relates to the transaction costs of giving.⁹

⁹As documented in Huck and Rasul [2008] this result on the intensive margin is not driven by outliers. Quantile regression estimates confirm the effect of the lead donor is to stretch rightward the entire distribution of donations given conditional on other observable characteristics of donors. In Huck and Rasul [2008] we explore how behavior on the intensive margin of giving responds both to the lead donor, and other treatments, that are not exploited in this analysis, in which donations in phase one are matched in a variety of ways.

Table 3: Outcomes for the Reminder Experiment

Mean, standard error in parentheses

P-values on tests of equalities with comparison group in box brackets

Phase One Treatment	Row	Comparison Group	Phase Two Treatments					
			<u>Phase One Treatments</u>		<u>Control (No Reminder)</u>		<u>Reminder</u>	
			Response Rate	Mean Donation	Response Rate	Mean Donation	Response Rate	Mean Donation
	Column:	(1)	(2)	(3)	(4)	(5)	(6)	
Control Group	(1)		.037 (.003)	74.3 (6.19)	0	-	.017 (.003)	63.8 (10.4)
Lead Donor	(2)		.035 (.003)	132 (14.3)	0	-	.015 (.003)	96.6 (12.5)
		Row (1) = Row (2)	[.564]	[.000]			[.765]	[.049]

Notes: All monetary amounts are measured in Euros. The tests of equality in box brackets are based on a mean comparison t-test against a two sided alternative hypothesis. The final row shows the p-value on a Mann Whitney test of the hypothesis that the two samples are from populations with the same distribution. Mean donations are conditional on giving.

The remaining Columns present outcomes in each of the treatment combinations in our 2×2 design. Columns 3 and 4 show that if no reminder is sent, response rates are zero in the second phase so that there is not a single donation made after six weeks from the time of the original mail-out. In contrast, Columns 5 and 6 show that there are responses to the reminder letter. The response rate in Control-Reminder is 1.7% and it is 1.5% in the Lead donor-Reminder combination. The average number of days to respond to the reminder is 13 if the recipient was previously assigned to the Control matching treatment, and the mean response time to the reminder is 17 days for those previously assigned to the Lead donor treatment. The median response times are closer at 11 and 13 days respectively.

Four points are of note. First, the fact that there are significant responses to the reminder letter contradicts the hypothesis that if individuals found it optimal not to donate to the original mail-out, then they should find it optimal to not donate when reminded to do so. The results also contradict the hypothesis of individual behavior being characterized by a random utility model. If so, there should also be no differential responses to the two reminder treatments. This is because if recipients are too busy, say, on the day they receive the initial mail-out, to make a donation even though their $g^* > 0$, they should with perfect recall, be able to wait and donate on a later date when such transactions costs are lower. Hence with such time-varying transactions costs, receiving a reminder letter *per se* should have no impact on responses. We therefore assume that the mere receipt of a reminder triggers recipients to receive another draw from their distribution of transaction costs. This is observationally equivalent to the reminder refreshing recipient memories about the original mail-out.

Second, response rates in the Control-Reminder and Lead donor-Reminder treatments are not significantly different. If the announcement of a significant lead donor alters recipients' perceptions about the quality of the project [Vesterlund 2003, Andreoni 2006], this result suggest the reminder letter has no inherent informational content about the project quality because it does not differentially affect recipients with different priors about project quality.

Third, donations in the second phase remain significantly higher for those who were exposed to the lead donor in the first phase even though the reminder letter does not explicitly mention the lead gift again. The design does not allow us to disentangle whether—(i) recipients are able to recall this detail from the phase one mail-out letter, or, (ii) the original mail-out letter is kept and referred to when they receive the reminder letter. Although donations fall in the reminder treatment relative to the original mail-out, they fall by 14% for those originally in the control group, from €74.3 to €63.8, and by 27% for those originally in the lead donor treatment, from €132 to €96.6. Hence if the mere receipt of a reminder itself serves as a signal of the project quality, say, or conveys some other information about the project, the effect on the intensive margin is slightly more detrimental among those who were previously informed of a large lead donor for the project. However, there is no corresponding effect on the extensive margin, and this is our focus in this paper.

Fourth, the total amounts raised by reminders are substantial. For those in the Control-Reminder (Lead donor-Reminder) treatment, €2747 (€5175) is raised. Hence a total of €7922 is raised from sending 8470 reminders, corresponding to a gross return of €0.935 per reminder. This is between two and three times the postage costs. Accounting for the cost of paper, an envelope, and the opportunity cost of funds that might have been raised had the employees of the Opera house been engaged in alternative activities, there is likely to be a very high rate of return of sending reminders. Hence in this setting it remains profitable for the charitable organization to engage in repeat solicitations.

We now exploit the two-phase design of our field experiment to provide unconditional estimates of: (i) the share of recipients who would donate some positive amount in the absence of transaction costs, s ; and, (ii) the probability that a recipient draws sufficiently low transactions costs, t . Doing so we obtain the following estimates for those previously in the control or lead donor

treatments in phase one,

$$\begin{aligned} s^{control} &= .069 \text{ and } t^{control} = .541, \\ s^{lead\ donor} &= .061 \text{ and } t^{lead\ donor} = .571. \end{aligned} \tag{5}$$

The estimates are similar across both histories of phase one treatments suggesting that recipient preferences and the transactions costs of making and implementing decisions are orthogonal to these treatment assignments. More precisely the estimates imply, first, there are around 6 to 7% of the recipient population that have preferences such that, in the absence of transaction costs, they would find it optimal to donate some positive amount to this particular project. Second, almost half the recipient population—around 43 to 46%—experience transactions costs sufficiently large to prevent them making their preferred donation. Hence if transactions costs could be eliminated altogether, response rates would nearly double.

More generally, it might be that s and t are functions of the donation given, g^* . For example, wealthier individuals might prefer to give more conditional on making a donation, but also face higher time costs of giving.¹⁰ To explore whether such effects are first order, we examined three bins of donations of $[0,50)$, $\{50\}$, and $(50,\infty)$ and repeated the above exercise for the Control-Reminder and Lead donor-Reminder treatment combinations.¹¹ Doing so, we find estimates for $t(g^*)$ ranging from .32 to .53, but with no clear sign of $t'(g^*)$ emerging. For instance, in Control-Reminder $t(g^*)$ slightly increases with donation size, but in Lead donor-Reminder it appears to fall slightly. In both cases, we cannot reject the hypothesis that $t(g^*)$ is independent of the donation given g^* so the transaction costs of making and implementing decisions, are orthogonal to the preferred donation.

¹⁰For example, consider a model in which each recipient faces a transactions cost τ drawn from some distribution $F(\tau)$, where τ is i.i.d. across individuals and time. If (c^*, g^*) maximizes utility conditional on giving, the recipient gives if $u(c^*, g^*) \geq \tau$, and the likelihood of giving is $t(g^*) = F(u(c^*, g^*))$. If the only source of heterogeneity is income then $t(g^*)$ is identical for recipients with the same g^* .

¹¹These bins were chosen so that, approximately, an equal number of donations fell into each bin.

An obvious caveat is that our sample sizes are small and so we do not have much power to detect whether transactions costs vary with donations given. However, we note that on observables, those who donate in phase one of the experiment are not much different to those that donate with the reminder treatment. In both phases, recipients that have placed more ticket orders in the year prior to the first mail-out, or that have greater expenditures on opera tickets, are significantly more likely to donate in both phase one and phase two of the experiment. To the extent that such observables are correlated to donated amounts, this is in line with the hypothesis that donated amounts are uncorrelated to the transaction costs of giving. However, there remains scope for future research to try and better understand whether and how transaction costs might relate to amounts given.

3 The Transaction Cost Experiment

3.1 Design

Given the low response rates found in real world voluntary contribution settings, the possibility to double the number of donors in this setting of charitable giving by eliminating transaction costs inspired us and the opera house to design and implement a second field experiment. Specifically, in the summer of 2008 we again solicited donations from 25,000 regular attendees to the opera, to contribute towards the same children and youth programme organized by the opera house. Given our earlier findings on the effectiveness of lead donors, all treatments in this study had an anonymous lead donor present who committed €15,000 to the project.¹²

The experiment was designed to shed more light on the precise nature of transaction costs. To do so we define two types of transaction costs. *Ex ante*

¹²Using data from another field experiment with the opera house, carried out in 2007, we find the announcement of a lead gift of €15,000 has similar effects to the announcement of a lead gift of €60,000. A similar result is reported by Karlan and List [2007] that the presence of a lead donor matters for donations, not the precise magnitude of the lead gift. As modelled in Andreoni [2006], we view such lead gifts as being sufficiently large to influence subsequent giving behavior.

transaction costs inhibit recipients from even considering the choice problem. This might be because they do not open the mail-out letter in the first place. Alternatively, *ex post* transactions costs inhibit recipients from implementing their decision. In our context this might be due to binding time constraints on filling out and returning reply slips with the desired donation amount.

The baseline treatment was designed in the same way as all the earlier treatments in the first field experiment. Specifically, recipients were simply informed about the opera's bank details and there was no further aid for making the donation. Nor was there any option to pay through means other than a bank transfer. To shed light on *ex ante* transaction costs, our second treatment changed the external appearance of the envelope in which the mail-out letter arrived, so that the envelope clearly indicates "Bring Opera to the Children". Any behavioral response to this lowering of *ex ante* transaction costs would indicate non-response is in part driven by individuals not even considering the choice problem to begin with.

The third treatment kept the appearance of the letter the same as in the control treatment but varied *ex post* transaction costs so that it became slightly easier for recipients to implement their choice. We did this in two ways. First, we attached a pre-filled bank transfer form to the mail-out that already contained details for the opera house as well as the appropriate references. This form is relevant for those who conduct their bank transfers either in person or by mail. Both methods are commonly used in Germany. To this partially completed form, recipients still needed to add their own name and banking details and the amount they want to donate.¹³ Second, we additionally offered recipients the option to make a donation by phone using a credit card.

The final treatment combined both methods to reduce *ex ante* and *ex post* transaction costs. Recipients were randomly assigned to each treatment so that around 6,200 individuals were in each. The Appendix shows the mail-out envelopes and bank transfer forms.

¹³For most individuals not using online banking, the alternative to using the pre-filled out form from the opera would be to use one of their own forms, typically, pre-filled out with own details. So the actual effect of the opera form is that one has to enter more familiar own details rather than the unfamiliar details of the opera.

3.2 Results

Table 4 presents outcomes by treatment. Three points are of note. First, comparing the control treatment to the *ex ante* transaction cost treatment, we see there is little effect on either the extensive or intensive margin of charitable giving. In both treatments the response rate is around 2.7% and the average donation, conditional on giving, is just over €90.¹⁴ There are three interpretations of this. First, non-response does not primarily stem from recipients being too busy to open the mail-out letter in the first place. Second, this treatment has little impact on *ex ante* transaction costs. Indeed, given that *ex ante* transactions costs likely stem from recipients' time costs, it might be very difficult for fundraisers to reduce such costs generally. A third interpretation is that there are two types of recipient that are affected in different ways by the clear labelling of the mail-out letter. The first type experience a reduction in *ex ante* transactions and are more likely to open the letter as a result. The second recipient type become less likely to open the letter and so no effect is found overall. Evidence that individuals take advantage of avoiding solicitations when they are able to do is presented in DellaVigna *et al.* [2009].

Second, comparing the control treatment to the *ex post* transaction cost treatment, there is a significant increase in response rates from 2.7% to 3.4% as such transactions costs are marginally reduced. This places in perspective the claim from the first field experiment that, in this setting, if transactions costs could be reduced to zero, response rates could almost double. Here we find a marginal reduction in *ex post* transaction costs leads to a 26% increase in response rates relative to the control treatment, even if the bulk of transactions costs are likely to remain. There is no corresponding effect on the intensive margin of how much is donated, suggesting that *ex post* transaction costs are not much related to the amounts given, g^* . We take this as evidence that the

¹⁴These figures differ slightly from those in the control treatment in the first field experiment. There are two reasons for this. First, presumably there are changes in income and relative prices in the two years between the field experiments that drive charitable giving. Second, there are some recipients that are part of both studies and their behavior might differ from recipients contacted for the first time. Such repeat subjects are of course randomly assigned over the four treatments in this experiment.

Table 4: Outcomes for the Transaction Costs Field Experiment

Mean, standard error in parentheses

P-values on tests of equalities with comparison group in box brackets

Treatment	Row	Comparison Group	Response Rate	Mean Donation
		Column:	(1)	(2)
Control Group	(1)		.027 (.002)	90.8 (8.21)
Marked Letter (<i>Ex ante</i> TC)	(2)		.026 (.002)	96.7 (12.1)
		Row (1) = Row (2)	[.593]	[.683]
Filled form (<i>Ex post</i> TC)	(3)		.034 (.002)	87.1 (7.08)
		Row (1) = Row (3)	[.034]	[.733]
Marked Letter (<i>Ex ante</i> TC) and Filled form (<i>Ex post</i> TC)	(4)		.031 (.002)	76.9 (4.80)
		Row (3) = Row (4)	[.489]	[.234]

Notes: All monetary amounts are measured in Euros. The tests of equality in box brackets are based on a mean comparison t-test against a two sided alternative hypothesis. Mean donations are conditional on giving.

estimate from the first field experiment of a potential doubling of response rates is not that far off the mark, and that it is likely to be profitable for fundraisers to reduce such *ex post* transaction costs [Warwick 2003].

Third, comparing the *ex post* transaction costs treatment to the final treatment in which both *ex ante* and *ex post* transaction costs are incrementally reduced, confirms the earlier result that non-response in this setting does not stem from the existence of *ex ante* transaction costs. The results also confirm there is no interaction between these two types of transaction cost, on neither the extensive nor intensive margins of giving.

4 Discussion

This paper is among the first to identify and measure transaction costs in charitable giving. We find that transaction costs are of first order importance to understand why even targeted recipients do not respond to fundraising drives based on mailed out solicitations. The evidence from our first field experiment suggests around half the recipients of a fundraising letter who would make a donation in the absence of transaction costs, do not do so given the transactions costs they face. This experiment also confirms that is profitable for fundraisers to use reminders to re-solicit donations rather than not sending any reminders at all. The evidence suggests such reminders trigger a new draw from the transaction cost distribution of recipients. Individuals would not otherwise respond to the fundraising campaign. Our second field experiment sheds light on the nature of transaction costs and suggests behavior on the extensive margin of giving is responsive to even incremental reductions in transaction costs, particularly to small reductions in *ex post* rather than *ex ante* transaction costs. The fact that such small differences to transactions costs can have large effects on behavior mimics some recent findings on automatic enrolments and defaults in savings behavior [Madrian and Shea 2001, Choi *et al.* 2003].¹⁵

¹⁵These results suggest other comparative statics related to reminders—such as varying the time at which reminders are sent, or varying the information sent with the reminder

To address concerns over the external validity of our first field experiment, we report findings from a similarly scaled field experiment on charitable giving we ran in conjunction with the Royal Opera House (ROH) in Covent Garden, London. In this experiment, the fundraising project was to raise money to repair the sets and costumes of traditional operatic productions. A mail-out was sent to 5,000 selected members of the ROH database. Recipients were selected to be those whose ticket purchase history had revealed a preference for such traditional productions.

The field experiment was again implemented in two phases. First, a mail-out letter was sent explaining the fundraising project. This is analogous to the control treatment in phase-one of the reminder field experiment described in Section 2. The second phase was implemented six weeks later when non-donors to the original mail-out were reminded of the opportunity to donate.

In London, the response rates in phases one and two, p and q , were 1.1% and .3% respectively. This implies the share of recipients who would donate some positive amount in the absence of transaction costs is $s^{ROH} = .015$, and the probability that a recipient draws sufficiently low transactions costs to implement their donation is $t^{ROH} = .724$. Hence the data suggests a smaller share of recipients in London would like to contribute to the project than in the Munich based fundraising project, but that recipients in London face lower transactions costs of making and implementing these choices. In both the London and Munich settings, it remains true that it is profitable for the charitable organization to engage in repeat solicitations.¹⁶

Our analysis has important implications for the economics of charitable giving. As suggested by our first field experiment, a significant proportion of even a well-targeted recipient population experience sufficiently high transaction costs that prevent them from donating when they would prefer to do so. As highlighted by our second field experiment, a significant fraction of

about previous donations—should be investigated in future research.

¹⁶We can only speculate why transactions costs appear higher in Munich than London. Two explanations are that in London it is easier to make donations through credit cards, or that opera goers in London with a preference for traditional productions, are older and more likely to be retired.

recipients can be induced to donate with even incremental reductions in the transactions costs of giving. Hence, devoting more effort to changing behavior on the extensive margin of charitable giving might be more cost effective and simple to implement, than fundraising designs aimed at changing recipient behavior on the intensive margin on how much they give.¹⁷

This conclusion is reinforced by two facts. First, the most commonly observed schemes to raise the amount given—matched gifts—are both costly and not especially effective from the fundraisers’ perspective. This has been shown in this specific empirical setting and for the same fundraising project [Huck and Rasul 2008]. The ineffectiveness of matched gifts has also been documented in other settings [Karlan and List 2007]. Second, as highlighted at various parts of our analysis, the new donors that are induced to contribute once transactions costs are lowered do not give significantly less than those who otherwise contribute. This reinforces the notion that transactions costs—that might relate to the time costs of decision making—appear not much correlated to the optimal amounts given.

Finally, our analysis sheds light on transactions costs in fundraising drives based on mail-out solicitations. While households might be often approached through mail-out solicitations by fundraisers, other approaches are also in use such as door to door solicitations, telemarketing and so on. Understanding the transaction costs in those settings remains for future research.

¹⁷In each case we derive implications for the effects on giving when one charitable organization unilaterally alters the transaction costs of giving. A separate set of issues arise if all charitable organizations were to simultaneously reduce the transaction costs of giving. Depending on how individuals allocate expenditures across charities, such effects might be larger or smaller than those we document. One recent study of individual giving to multiple charitable organizations is Sieg and Zhang [2009].

Table A1: Random Assignment to Treatments Across Phases

Robust standard errors in parentheses

	Ordered Probit	Bivariate Probit: Reminder Treatment Assignment	
	Reminder Treatment Assignment	R1	R2
Phase One Treatment: Lead Donor	-0.003 (.027)	.001 (.009)	-0.000 (.012)
Observations	21225	21225	21225

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Robust standard errors estimated throughout, and marginal effects are reported for the bivariate probit regressions. The reminder treatments are as follows -- R1: No Reminder; R2: Reminder. The reference category is the individual being originally assigned to the Control group for the phase one treatment.

Appendix: The First Field Experiment: Mail-Out Letter (Translated)

Bayerische Staatsoper
Staatsintendant
Max-Joseph-Platz 2, D-80539 München
www.staatsoper.de

[ADDRESS OF RECIPIENT]

Dear [RECIPIENT],

The Bavarian State Opera House has been investing in the musical education of children and youths for several years now as the operatic art form is in increasing danger of disappearing from the cultural memory of future generations.

Enthusiasm for music and opera is awakened in many different ways in our children and youth programme, “Erlebnis Oper” [*Experience Opera*]. In the forthcoming season 2006/7 we will enlarge the scope of this programme through a new project “Stück für Stück” that specifically invites children from schools in socially disadvantaged areas to a playful introduction into the world of opera. Since we have extremely limited own funds for this project, the school children will only be able to experience the value of opera with the help of private donations.

[This paragraph describes each matching scheme and is experimentally varied as described in the main text of the paper].

As a thank you we will give away a pair of opera tickets for Engelbert Humperdinck’s “Königskinder” on Wednesday, 12 July 2006 in the music director’s box as well as fifty CDs signed by Maestro Zubin Mehta among all donors.

You can find all further information in the enclosed material. In case of any questions please give our Development team a ring on [*phone number*]. I would be very pleased if we could enable the project “Stück für Stück” through this appeal and, thus, make sure that the operatic experience is preserved for younger generations.

With many thanks for your support and best wishes,

Sir Peter Jonas, Staatsintendant

Appendix: The First Field Experiment: Mail-Out Letter (Translated)

“Stück für Stück”

The project “Stück für Stück” has been developed specifically for school children from socially disadvantaged areas. Musical education serves many different functions in particular for children and youths with difficult backgrounds -- it strengthens social competence and own personality, improves children’s willingness to perform, and reduces social inequality. Since music education plays a lesser and lesser role in home and school education, the Bavarian State Opera has taken it on to contribute to it ourselves. The world of opera as a place of fascination is made attainable and accessible for young people.

In drama and music workshops, “Stück für Stück” will give insights into the world of opera for groups of around 30 children. They will be intensively and creatively prepared for a subsequent visit of an opera performance. These workshops encourage sensual perception – through ear and eye but also through scenic and physical play and intellectual comprehension – all of these are important elements for the workshops. How does Orpheus in “Orphee and Eurydice” manage to persuade the gods to let him save his wife from the realm of dead? Why does he fail? Why poses the opera “Cosi fan tutte” that girls can never be faithful? It is questions like these that are investigated on the workshops.

The workshops are also made special through the large number and variety of people who are involved in them: musicians, singers, directors, and people from many other departments, ranging from costumes and makeup to marketing. The participants in each workshop work through an opera’s storyline, and are introduced to the production and will meet singers in their costumes as well as musicians. This makes the workshops authentic. After the workshops the participants are invited to see the actual opera production.

Through your donation the project “Stück für Stück” will be made financially viable so that we can charge only a small symbolic fee to the participants. This makes it possible to offer our children and youth programme also to children from socially disadvantaged backgrounds that can, thus, learn about the fascination of opera.

Note: In German, Stück für Stück is a wordplay --- “Stück” meaning “play” as in drama and “Stück für Stück” being an expression for doing something bit by bit.

Appendix: The Second Field Experiment: Mail-Out Letter (Translated)

Bayerische Staatsoper
Staatsintendant
Max-Joseph-Platz 2, D-80539 München
www.staatsoper.de

[ADDRESS OF RECIPIENT]

Dear [RECIPIENT],

Some weeks ago you heard from us about our fundraising call for the project *Stück für Stück* that forms part of the children and youth programme of the Bavarian State Opera. As this call has expired now I am happy to inform you that we received many generous donations, that supported the project [*in the reminder with information treatment (which we do not use for our analysis) it was also stated that over 150,000 euros had been raised*].

Next to our artistic endeavour, cultural heritage, education, and the fostering of creativity are a key part of the Bavarian State Opera's social responsibilities. Given this background and the success of our fundraising call so far, we would like to give you another opportunity to donate and invite you to support *Stück für Stück* also in the coming weeks.

Please transfer your donation (for which you will receive a tax receipt) to our account:

Recipient: STOK Bayern, BuSt. München
Bank: Bayerische Landesbank Girozentrale
Sort code: 700 500 00
Account number.: 24592
Reference: „Kapitel 1581, Titel 28201 Kinder-/Jugendprogramm“
For donations from abroad:
BIC: BYLADEMM
IBAN-Nr.: DE3670050000000024592
USt-ID-Nr.: DE 814173346

If you have any queries please call our Development team at 089-2185-1106 any time.

I would be glad if you could help to support *Stück für Stück* for school children from deprived background to introduce them to the world of opera.

Many thanks for your help and best wishes,

Dr. Ulrike Hessler

The Second Field Experiment: Reducing *Ex Ante* Transaction Costs



The Second Field Experiment: Reducing *Ex Post* Transaction Costs

Überweisung/Zahlschein		Beleg/Quittung für den Kontoinhaber	
Name und Sitz des überweisenden Kreditinstituts		Konto-Nr. des Kontoinhabers	
Bankleitzahl		Empfänger	
Begünstigter: Name, Vorname/Firma (max. 27 Stellen)		Konto-Nr. des Empfängers	
STOK BAYERN, BuSt, MUENCHEN		bei (Kreditinstitut)	
Konto-Nr. des Begünstigten	Bankleitzahl	Betrag	
2 4 5 9 2	7 0 0 5 0 0 0 0	EUR	
Kreditinstitut des Begünstigten		Kunden-Referenznummer	
BAYERISCHE LANDESBANK		- noch Verwendungszweck (nur für Empfänger)	
Betrag: Euro, Cent		Kontoinhaber/Einzahler: Name	
EUR		18	
Kunden-Referenznummer - Verwendungszweck, ggf. Name und Anschrift des Überweisenden - (nur für Begünstigten)		Datum, Unterschrift	
KIAPIITIEL 1151811, TITTEL 28201 UT			
noch Verwendungszweck (insgesamt max. 3 Ziffern & 27 Stellen)			
02 KINDER- / JUGENDPROGRAMM			
Kontoinhaber/Einzahler: Name, Vorname/Firma, Ort (max. 27 Stellen, keine Straßen- oder Postfachangaben)			
Konto-Nr. des Kontoinhabers			

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