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MATCHING SUBSIDIES AND VOLUNTARY CONTRIBUTIONS: A REVIEW

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Abstract. This paper provides a synthesis of the experimental literature on matching subsidies in the context of charitable giving. We classify results according to four different outcome variables frequently considered in the literature and address (i) short-term effects of linear matching, (ii) the role of the matching rate, (iii) context-dependence of behavioural responses, (iv) the relevance of the price of giving, (v) long-term effects and (vi) nonlinear matching schemes. Based on this comprehensive review, we highlight several avenues for future research, such as putting stronger emphasis on competition in fundraising, long-term effects or heterogeneity in responses.

Keywords. Charitable giving; Experiments; Fundraising; Matching subsidy; Voluntary public good provision

1. Introduction

Fundraisers commonly apply matching subsidies to stimulate giving. The underlying idea is that a third party supplements the individual's contribution and thereby reduces the effective price of giving. Since large amounts of money are involved in such fundraising campaigns and nonprofit organizations are crucial to provide goods and services the private economy often fails to provide, it is of great importance to have a solid understanding of the effect of matching subsidies on fundraising success.

Therefore, we present a synthesis of the experimental literature on matching subsidies in the context of charitable giving. In a first step, we disentangle the different ways in which effectiveness is assessed and classify the empirical evidence accordingly. On this basis, we summarize the short-term effects of matching on the different outcome variables – namely charity receipts, checkbook giving, extensive and intensive margin – taking into account that a large donation by a third party might alternatively be used to announce a lead donor gift (Section 3.1). To allow the reader to easily access the available evidence as well as the key characteristics of the respective studies, we additionally provide detailed tables on the short-term effects found in the experimental literature (Table A1 for laboratory experiments and Table A2 for field experiments). Afterwards, we discuss the relevance of the matching rate (Section 3.2) as well as contextual factors (Section 3.3), and infer whether an effect of matching solely results from a change in the price of giving (Section 3.4). To complement the review, we also pay particular attention to long-term

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effects (Section 3.5) and recent alternative designs of matching schemes (Section 3.6). We provide a short summary on each of these aspects at the end of the respective sections.

Our review is related to that of Vesterlund (2016), who tackles the more general question of why people give to charities and provides an overview on different fundraising mechanisms. Although her broad review considers short- and long-term effects of matching subsidies, these are addressed rather briefly. Devoting a complete literature review to the effectiveness of matching in the charitable giving context allows us to extend the important work of Vesterlund (2016) in several dimensions. First, we offer a more comprehensive picture on the impact of matching, for example, by classifying and structuring results according to the different outcome variables considered, taking into account contextual factors and incorporating new evidence. Second, we elaborate whether the decrease in the price of giving is the single driver of the observed matching impact and tackle recent advances with respect to the design of matching subsidies. Finally, the detailed review allows us to identify research gaps and formulate avenues for future work on matching subsidies, while also serving as a source of information for practitioners.

In the following section, we offer a more detailed description of matching subsidies in the charitable giving context and introduce the four outcome variables commonly analysed in the literature. Based on these outcome variables, we discuss the experimental evidence on the effectiveness of matching in detail in Section 3. Finally, we conclude and highlight additional opportunities for future research.

2. Fundamentals of Matching Subsidies

We follow the definition of matching subsidies shaped by the empirical literature on charitable giving: a matching subsidy supplements the contributed money (e.g. at a linear rate) and both the individual's contribution as well as the matching payment are given to the charity. It is precisely this approach which is the focus of the literature review. We exclude rebates, which use subsidy payments to refund a fraction of the donor's contribution, while the charity still receives the entire amount contributed. Although rebates are commonly defined as being realized without any delay and are, therefore, strategically equivalent to matches, the charitable giving literature shows that subjects do not react the same way to rebates as they do to matches. For instance, a well-established result is that rebates raise considerably less money (see e.g. Eckel and Gossman, 2003; Davis *et al.*, 2005; Eckel and Gossman, 2006a, 2006b; Davis, 2006; Eckel and Gossman, 2008; Lukas *et al.*, 2010; Bekkers, 2015; Eckel and Gossman, 2017). Therefore, including both subsidy types would require reviewing the empirical evidence for each separately, exceeding the scope of the review.

The majority of the empirical literature considers linear matching schemes. In this case, a third party – often an anonymous donor – offers to match the individuals' contributions to the charity at a matching rate of $m > 0$.[1] Based on the matching rate, each individual i decides on her contribution d_i.[2] Thus, the charity receives $d_i * (1 + m)$ and the implied price of giving, which is the amount of money a donor needs to give so that the charity receives one dollar, can be expressed as $p_i = \frac{1}{1+m}$.

There are four outcome variables, each of which can be used to assess the effectiveness of a matching subsidy: charity receipts, checkbook giving, extensive and intensive margin. We use the following definitions and formally summarize them in Table 1. In line with Davis *et al.* (2005), we define *charity receipts* as the amount of money the charity receives including the matching subsidy. As control and treatment groups often differ in the exact group size, evidence is commonly based on the charity receipts per individual.

Although charity receipts constitute a highly relevant outcome variable, fundraisers or policy makers might be specifically interested in the effect on individuals' contributions, excluding the matching payment. Following Eckel and Grossman (2008), we refer to this as *checkbook giving*. As charity receipts and checkbook giving are closely linked, we can use the price elasticity of the former to conclude about the latter: a price elasticity of charity receipts above –1 indicates that decreasing the price decreases

Table 1. Outcome Variables.

Charity receipts	Checkbook giving	Extensive margin	Intensive margin
Amount the charity receives	Amount given excluding the match	Likelihood of giving a positive amount	Checkbook giving of contributors
$E[d_i(1+m)\|m, x_i]$	$E[d_i\|m, x_i]$	$\Pr(d_i > 0\|m, x_i)$	$E[d_i\|m, x_i, d_i)0]$

Note: d_i is the individual's contribution, m is the matching rate and x_i contains other factors which might be considered and do not need to be individual specific.

checkbook giving, referred to as crowding out, whereas one below –1 represents crowding in. A price elasticity of –1 correspondingly indicates that checkbook giving is not affected and the percentage increase in charity receipts mirrors the matching rate, for example, a matching rate of 0.5 increases charity receipts by 50%.

While charity receipts depend on checkbook giving, checkbook giving itself aggregates two behavioural responses: (i) the likelihood of giving, which we refer to as *extensive margin* and (ii) the checkbook giving conditional on contributing a positive amount denoted as *intensive margin* or *conditional checkbook giving*. Therefore, a lack of sensitivity on the level of checkbook giving might be explained by the absence of any effect on both margins, or for example, an increase in the likelihood of contributing paired with a counterbalanced decrease in conditional checkbook giving. The extensive margin is often of particular interest, as former donors are more likely to give in the future than those who have never donated to the charity (Landry *et al.*, 2010; Adena and Huck, 2019), even if the initial economic incentive, that is, a lottery, has been removed (Landry *et al.*, 2010). This aspect also emphasizes why focusing on short-term effects alone is not sufficient when assessing the effectiveness of matching subsidies.

The effect of linear matching subsidies on these four outcome variables depends on individuals' preferences, which are usually not known. As there are several motives for giving to charities (Bekkers and Wiepking, 2011; Vesterlund, 2016), various predictions can arise (Karlan and List, 2007; Huck and Rasul, 2011). In the following, we address three of them.

First, charitable giving can be seen as a public good, either if the population is altruistically inclined (Vesterlund, 2016) or if the potential donors directly benefit from the good or service provided by the charity. In this case, the total amount of money received by the charity enters the individuals' utility functions (e.g. Andreoni, 1989), as it translates into the respective goods or services provided by the charity. The matching subsidy decreases the price of giving, that is, it decreases the amount of money an individual needs to give so that the charity receives one dollar. Under the assumption of an ordinary good, this will increase charity receipts, whereas the impact on checkbook giving and both margins remains ambiguous.

Second, another commonly identified and discussed motive for giving is warm glow (Vesterlund, 2016). If people are purely motivated to give by warm glow, they only receive utility from what they give to the charity (and from private consumption), which implies that the level of checkbook giving and not the level of charity receipts directly contributes to the individual's utility (Andreoni, 1989, 1990). As matching does not affect the marginal costs of checkbook giving (Huck and Rasul, 2011), in case of pure warm glow preferences matching will neither affect checkbook giving nor any of the two margins, leading to an increase in charity receipts by exactly the matching subsidy. If individuals are additionally motivated by altruism, referred to as impure altruism (Andreoni, 1989), predictions become qualitatively similar to those under pure altruism.

Third, monetary incentives can crowd out intrinsic motivation (Frey and Jegen, 2001; Bénabou and Tirol, 2006). Thus, if matching is in place, giving of intrinsically motivated individuals might be crowed

out (Huck and Rasul, 2011), resulting in a negative effect on checkbook giving and the extensive margin. If sufficiently strong, it might even lead to a decrease in charity receipts.

This incomplete list of predictions on how matching might affect giving illustrates that empirical and especially experimental evidence is needed to assess matching subsidies. To the extent possible, we will relate the empirical evidence back to these predictions and highlight some other crucial aspects affecting the response to matching in Section 3.4. Note that an observed response might be consistent with various theoretical explanations (of which not all are necessarily presented here). For example, crowding out of checkbook giving can result from altruistic preferences as well as crowding out of intrinsic motivation.

3. Experimental Evidence

The methodological approaches used to study the effects of matching subsidies in the context of charitable giving comprise both laboratory and field experiments. The former are mainly based on a modified dictator game, in which subjects allocate money between themselves and a chosen or exogenously determined charity (e.g. Eckel and Grossman, 2003). Usually individuals decide how much money to give to the charity net of the subsidy, but Davis (2006) deviates from this standard procedure: the matching subsidy is added to the individual's endowment upfront and subsequently subtracted if the money is not allocated to the charity. In most laboratory experiments which we consider in this review, the comparison of a matching subsidy and a no subsidy control or different matching rates is based on a within-subjects design: Each individual faces several contribution decisions, differing in whether matching is offered and to which extent the experimenter matches the individual's contribution. This does not exclude the possibility that other aspects are examined in a between-subjects design, like the comparison of matches to rebates in Eckel and Grossman (2006b). In contrast to most laboratory experiments, the field experiments are based on a between-subjects design (each subject is assigned to a single treatment) and are commonly implemented together with a fundraiser via mail solicitations to a specific subject sample (see e.g. Karlan and List, 2007).

The respective studies mainly focus on linear matching covering different matching rates, which are mostly not greater than one. The smallest matching rate considered is 0.1, while the largest amounts to 5. Another important methodological aspect is that most studies offer estimates of treatment effects relative to a no subsidy condition for each matching treatment. However, for some studies we are only able to infer the effect of matching from an estimated price elasticity, which also takes into account the change over several matching treatments with different rates (Eckel and Grossman, 2003, 2006b). In those cases, we are therefore unable to disentangle the effect of introducing a matching subsidy and changing the rate offered.

Lack of information prevents researchers from determining the social optimum. Therefore, the four different outcome variables introduced in the previous section are used to judge the effectiveness of matching schemes. We classify the empirical evidence accordingly to increase comparability of results and infer general conclusions. In doing so, we first focus on the effect of introducing matching compared to not offering any subsidy (Section 3.1). As a charity might also be able to use the large donation by a third party as lead donor gift (i.e. announcing that a large donation has already been made), we also address the comparison of matching subsidies to this alternative fundraising strategy. Afterwards we discuss the impact of the matching rate (Section 3.2) and take a closer look at small differences in settings across studies as well as aspects analysed within experiments to infer the impact of contextual factors on the effectiveness of matching subsidies (Section 3.3). Since the main motivation for matching is the resulting decrease in the effective price of giving, we discuss to which extent the empirical evidence supports that matching simply changes the price of giving (Section 3.4). Subsequently, we shift our focus to long-term effects (Section 3.5), which are crucial to judge the effectiveness on a more aggregate level. For example, a

short-term increase in giving due to matching might be driven by intertemporal substitution and thus come at the expense of future contributions. Finally, we discuss alternatives to linear matching (Section 3.6).

3.1 *Does Linear Matching Affect Short-Term Giving?*

Our structured review on the effectiveness of matching subsidies in the short-term is supplemented by a detailed overview on the corresponding results in the matching literature, presented in Table A1 (laboratory experiments) and Table A2 (field experiments) in the Appendix. For each study which we consider, the tables provide information on the authors, the year of publication, the matching rates applied, the limit of the matching gift if it is presented to the subjects, the number of subjects in each matching treatment, and the percentage of treated subjects donating. As key feature of the tables, we report the quantitative effects of matching on the four outcome variables analysed in the respective study. The effects on charity receipts, checkbook giving and the intensive margin are presented as relative change (in percent) using the control condition without matching subsidy as baseline. For example, the first entry in the column of checkbook giving in Table A2 means that offering a match of 1, significantly increased the level of checkbook giving by 2.4%. The effects on the extensive margin are presented as absolute difference in the likelihood to give between the matching treatment and the control condition (in percentage points). The tables help the reader to easily and separately access the results of different studies featuring either laboratory or field experiments, to review their characteristics and draw comparisons. Furthermore, they allow to check which studies analyse a certain outcome variable or at least present data on it. The comment section is used to emphasize further important characteristics of a study and to report interesting results not fitting into the standard form of the tables. The second column differs across tables: In Table A1 it reports whether a within- or between-subjects design is used, whereas in Table A2 it provides information on whether a lead donor is announced in the control condition. The reason for this difference is that in all laboratory experiments which are presented in Table A1 the control condition does not include a lead donor announcement and all field experiments are based on a between-subjects design.

We start our review on short-term effects with the most aggregated of the four outcome variables introduced above – charity receipts – and afterwards gradually move to more disaggregated levels – checkbook giving, the extensive and finally the intensive margin. For now, we focus on the effect of introducing a matching subsidy compared to a baseline, in which neither a subsidy is offered nor a lead donor is announced.

In line with the law of demand, laboratory experiments suggest a positive effect of matching subsidies on charity receipts (Eckel and Gossman, 2003; Davis *et al.*, 2005; Davis, 2006; Eckel and Gossman, 2006b; Lukas *et al.*, 2010). The increase is roughly proportional to the matching rate or higher. For example, Davis *et al.* (2005) and Davis (2006) observe an increase of 36.1–63.3% for a matching rate of 0.5 and of 113.3–134.0% for a matching rate of 1. Consistently, the price elasticity estimated by Eckel and Grossman (2003) including matching rates of 0.25, 0.33 and 1 amounts to –0.9, while Eckel and Grossman (2006b) find a larger behavioural response resulting in a price elasticity of –2.6.

The price elasticities of charity receipts around or below –1 already point towards a nonnegative effect on checkbook giving. In most field experiments, linear matching significantly increases the level of checkbook giving (Karlan and List, 2007; Gneezy *et al.*, 2014; Huck *et al.*, 2015; Eckel and Grossman, 2017). For example, Karlan and List (2007) implement matching rates of 1, 2, and 3 in a mail-out to former contributors of a nonprofit organization. The authors identify a significant positive increase of 19% in the average checkbook giving across matching treatments relative to a baseline without subsidy. Some others find larger effects despite considering smaller matching rates, for example, a 50% increase for a matching rate of 0.5 (Huck *et al.*, 2015) or a 39% increase for a matching rate of 0.25 (Eckel and Grossman, 2017). Positive effects on checkbook giving are also found in laboratory experiments (Eckel *et al.*, 2007), at least for some matching rates and endowments (Davis, 2006). However, Lukas *et al.*

(2010) do not identify a significant effect for a matching rate of 0.25 and in the laboratory experiment by Charness and Holder (2019) the increase due to a linear match of 1 is merely significant in the case of a one-sided test. Similarly, Helms McCarty *et al.* (2018) do not find a significant impact on checkbook giving when considering matching rates of 0.1 and 1 in the field. Karlan *et al.* (2011) are the only ones who identify a significant negative effect for a subgroup of their sample, with the aggregate effect on the whole sample not being significantly different from zero.

With respect to the extensive margin, empirical evidence predominantly suggests a nonnegative effect. Huck and Rasul (2011), Karlan *et al.* (2011), Gee and Schreck (2018) and Helms McCarty *et al.* (2018) do not find a significant effect of linear matching on the aggregate, considering matching rates not greater than 1, while Karlan and List (2007) report a significant increase in the extensive margin by 0.4 percentage points, when pooling data on matching rates of 1, 2 and 3. This effect is substantial since the likelihood of contributing in the control group is 1.8%. In Huck and Rasul (2011), Gee and Schreck (2018) and Helms McCarty *et al.* (2018) the differences in response rates between the control group and the treatment group, which receives a linear match of 1, amount to 0.5, 0.8 and 0.6 percentage points with baseline response rates of 3.7%, 1.6% and 3.0%, respectively. Since all three papers have a substantially lower number of observations than Karlan and List (2007), insignificant effects might result from a lack of power. Eckel and Grossman (2017) send out solicitations where individuals are asked to fill out a survey and to donate. For each survey returned the researchers donate $5 to the charity. While the fraction of individuals completing the survey does not significantly differ across treatments (including rebate treatments), the effect on the likelihood of donating among respondents is significantly positive for a matching subsidy of 1/3 (8.4 percentage points) but insignificant for a matching subsidy of 0.25. Thus, it seems that a positive effect on the extensive margin might only arise if the matching rate is sufficiently large. Additional evidence for a positive impact is offered by Gneezy *et al.* (2014) for a matching rate of 1 and Knutsson *et al.* (2019) for matching rates of 1, 3 and 5. In both of these field experiments, subjects face a discrete choice set, either because they are asked to choose from a predetermined set of monetary values or because they make a binary choice on whether to donate the deposit received when returning bottles and cans. The result of the only laboratory experiment analysing the extensive margin generally fits well into those of the presented field experiments, but suggests a positive effect even for a matching rate as low as 0.25: Eckel *et al.* (2007) find that matching with a rate of 0.25, 0.5 or 1 significantly increases the likelihood to give by 2.0, 2.7 and 3.0 percentage points, respectively.

However, effects might be heterogeneous, also for outcome variables other than the extensive margin. For example, Eckel and Grossman (2008) send out mails to raise funds for a nonprofit organization, which include matching rates of either 0.25 or 1/3. Compared to a baseline without a lead donor, the extensive margin of regular contributors with a membership of the organization (continuing members) is significantly lower when matching is in place. Such an effect cannot be identified for subjects who previously were members but are currently not and subjects who neither have been members nor have ever contributed.[3] Although not tested for significance, the descriptive statistics suggest that this negative impact might be strong enough to decrease the average checkbook giving of this particular subgroup. Karlan and List (2007) find that matching is more effective in increasing the extensive margin of prior donors that have donated a small amount than of those that have donated a large amount. The authors also show that the effect of matching on the level of checkbook giving and the extensive margin is slightly larger for prior donors who have not provided funds in the year of the field experiment yet than for those who have. Interestingly, the result that 'more involved' subjects react less positively to the match does not hold in general. Helms McCarty *et al.* (2018) categorize individuals into (i) those who are affiliated with the charity but have not donated so far, (ii) those that have donated regularly and (iii) those that have given their first donation in the year of the field experiment. The introduction of matching neither produced heterogeneous effects on the level of checkbook giving nor on the extensive margin. Furthermore, Karlan *et al.* (2011) detect a negative impact of a match on the extensive margin for prior donors who have not

donated in the year of the experiment, but a significantly more positive one for those who have, which also holds true with respect to checkbook giving. Their findings emphasize that a lack of impact on the aggregate might be the result of substantial heterogeneous effects cancelling each other out.

The donation history of individuals is not the only dimension at which heterogeneous treatment effects might occur. Karlan and List (2007) emphasize a spatial heterogeneity based on whether the majority of people in a state voted for George W. Bush in the 2004 presidential election. In the states that voted in favour of him, the match has a significant positive impact on the likelihood of contributing, while for the others no significant effect occurs. The importance of political aspects in this context might be particularly driven by the fact that the involved nonprofit organization is a political one. Based on the same data as Eckel and Grossman (2003), Eckel and Grossman (2004) investigate whether religious subjects are more or less responsive to matches than nonreligious subjects, but do not find a significant difference in implied price elasticities of charity receipts. A large pool of other individual characteristics is considered in Bekkers (2015). By including interaction effects in a multinomial logit model, he shows that the effect of matching on the likelihood of small donations (1–45% of the endowment) is more positive for females and first increases with the level of education but eventually decreases when individuals hold at least a bachelor degree. These effects are not present for medium donations (45–55% of the endowment), while for large donations (55–100% of the endowment) only holding at least a bachelor degree has a weakly significant negative influence on the impact of matching. A gender difference is also supported by Knutsson *et al.* (2019), who find that the positive effect of matching on the extensive margin is primarily driven by females.

The intensive margin exhibits a nonnegative response: Karlan and List (2007), Gee and Schreck (2018) and Helms McCarty *et al.* (2018) do not find a significant impact due to the introduction of linear matching, while Huck and Rasul (2011) find a significant increase of 35.9% due to a 0.5 match, but an insignificant one in case of a match of 1. Again, Eckel and Grossman (2008) emphasize the potential relevance of heterogeneity. They show that decreasing the effective price due to matching raises only the intensive margin of continuing members, with the corresponding price elasticity amounting to –0.1. However, it is important to note that (i) donors about whom the authors do not have complete information are excluded (about 1.1% of all donors), leading to the exclusion of some particularly high donations by continuing members in the control group[4] and (ii) separate mean comparisons of each matching treatment to the baseline do not reveal significant differences in the intensive margin for this subgroup.

So far, we have focused on comparing a matching subsidy to a no subsidy control without a lead donor. If a generous gift by a third party is available, the charity might as well consider using the money as unconditional gift (if possible). Therefore, the comparison of matching to a baseline in which a lead donor is announced is of particular interest to tackle the question how a generous gift should be used. In Gneezy *et al.* (2014) as well as Huck *et al.* (2015), the level of checkbook giving in the lead donor control treatment exceeds that in the matching treatments, but this difference is not statistically significant. Counterfactual simulations in Huck *et al.* (2015) suggest that a lead donor is preferable to linear matching, even when considering a larger range of linear matching rates. In contrast, Adena and Huck (2017) estimate a matching subsidy of 1 to increase checkbook giving by about 2.4% compared to a lead donor control. Similarly, Adena and Huck (2017) find a significant increase of 0.6 percentage points in the extensive margin whereas others do not find any significant difference (Huck and Rasul, 2011; Gneezy *et al.*, 2014). Concerning the intensive margin, matching has no effect (Gneezy *et al.*, 2014) or performs significantly worse (Huck and Rasul 2011; Adena and Huck 2017). The decrease due to a match of 1 ranges from 24% in Adena and Huck (2017) to 35% in Huck and Rasul (2011). The insignificant result in Gneezy *et al.* (2014) might be caused by the design as potential donors can only choose between four different donation amounts.

Overall, the comparison of linear matching to a baseline in which neither a matching subsidy is offered nor a lead donor is announced reveals a strictly positive impact on the amount the charity receives. Most studies observe a positive or at least nonnegative effect on checkbook giving, suggesting a low risk of a

failed matching campaign in terms of crowded-out contributions in the short-term. Exclusively focusing on the aggregate might, however, leave aside crucial response patterns as heterogeneous treatment effects potentially lead to negative outcomes for subgroups. Furthermore, the empirical evidence suggests that the impact of matching is not limited to a specific margin.

When evaluating the effectiveness of matching, it is important to consider the alternative of using the generous gift of a third party as lead donation. Although the empirical evidence on the comparison of a matching subsidy to a lead donor announcement is not distinct, a lead donor announcement can be an interesting alternative. An aspect in favour of the lead donor approach is that the gift is received by the charity for sure, whereas in case of a matching campaign the matching gift might not be exhausted, as happened in several field experiments (e.g. Eckel and Grossman, 2008, 2017; Huck and Rasul, 2011; Huck *et al.*, 2015; Adena and Huck, 2017). However, a charity might as well be able to receive the portion of the matching gift not exhausted during the matching campaign after the end of the campaign.

3.2 *Which Matching Rate to Choose?*

After discussing the effect of matching subsidies at different rates, we now focus on the relevance of the matching rate more explicitly. Since the matching rates considered in the literature range from 0.1 to 5, conclusions are generally limited to this interval. As in Section 3.1, we start with the most aggregate outcome variable and gradually move towards more disaggregated levels. We do not address the intensive margin as there is not sufficient evidence to draw conclusions.

The relative changes in charity receipts for different matching rates, which are presented in Tables A1 and A2, suggest a simple relationship: The level of charity receipts increases with the matching rate. This notion is supported by the negative price elasticity estimates for charity receipts by Eckel and Grossman (2003; 2006b) as well as the results on checkbook giving addressed in the following.

We have already presented evidence that a matching subsidy can increase checkbook giving, but the relevance of the matching rate on this outcome variable seems to be rather limited. In Karlan and List (2007), levels of checkbook giving under the considered matching rates of 1, 2 and 3 do not differ significantly.[5] Such lack of responsiveness with regard to the magnitude of matching is supported by Bekkers (2015) for matching rates of 0.5 and 1, as well as Helms McCarty *et al.* (2018) for matching rates of 0.1 and 1. However, it is in contrast to the significant positive impact of increases in the matching rate found in laboratory experiments (Eckel *et al.*, 2007, for matching rates of 0.25, 0.5, and 1; Adena and Huck, 2017, for matching rates of 0.5, 1, 1.5). Lukas *et al.* (2010) conduct a laboratory experiment with matching rates of 0.25, 0.33 and 1, and two different endowment levels, in which only a matching rate increase from 0.33 to 1 with an endowment of $10 is found to create a significant increase. In this study, the introduction of a 0.25 match does not affect checkbook giving either.

Additional evidence is offered by Kesternich *et al.* (2016) who study a special public good setting in which subjects make binary choices. Customers of a German long-distance bus operator have the opportunity to offset their carbon emissions at a predetermined price when purchasing tickets. As the authors point out, the contribution decision is directly linked to a private consumption choice, which rather makes it an impure public good. If a match is in place, the company itself offsets an additional amount of carbon emissions determined by the matching rate offered. A matching rate of 1/3 creates no effect, but a matching rate of 1 is found to significantly increase the likelihood of offsetting compared to the control group, which results in a significant increase in checkbook giving. Interestingly, a further increase in the matching rate from 1 to 3 does not generate any additional impact, confirming the finding of Karlan and List (2007) that an increase in the matching rate above 1 neither significantly affects checkbook giving nor the extensive margin.

The evidence on how the matching rate affects checkbook giving is rather mixed, but suggests that increasing the matching rate does no harm. If a matching subsidy at a given rate was sufficient to increase

checkbook giving in the field, increasing the matching rate never resulted in a significant further increase in checkbook giving. How to ensure that the chosen matching rate is sufficient to create an effect? A matching rate of 1 has proven to be effective in crowding in donations in several settings (Davis, 2006; Gneezy *et al.*, 2014, Huck *et al.*, 2015; Kesternich *et al.*, 2016). However, there are cases in which it did not work out (Davis, 2006; Karlan *et al.*, 2011; Helms McCarty *et al.*, 2018) and sometimes smaller matching rates have been sufficient (Davis, 2006; Huck *et al.*, 2015; Eckel and Grossman, 2017).

Findings on the extensive margin suggest that matching rates need to be large enough to create a positive impact. Eckel and Grossman (2017) do not find an effect for a matching rate of 0.25 but for 1/3, whereas Kesternich *et al.* (2016) estimate a significant impact for a matching rate of 1 but not for 1/3. Helms McCarty *et al.* (2018) neither find a significant effect for a low matching rate of 0.1 nor for a rate of 1. Meier (2007) investigates the donation behaviour of students in the field, focusing on a slightly different version of the extensive margin. At the time of paying the tuition fees, each student has to decide whether to donate to no fund, a single fund, or two funds. Each decision is associated with a fixed amount of money. Offering a matching rate of 0.25 or 0.5 for contributing to both funds, pooling the data for these into a single treatment and taking into account pretreatment differences reveals a significant increase in the likelihood of contributing to both funds. However, if the two matching treatments are considered separately in a logit regression, the lower matching rate is not found to have a significant coefficient while the higher matching rate does. Nevertheless, the two coefficients are not statistically different. There is additional laboratory evidence that increasing the matching rate up to 1 can increase the likelihood to give (Eckel *et al.*, 2007). Despite this positive relation between the matching rate and the likelihood to give, increases above 1 have not been found to create an additional effect in the field (Karlan and List, 2007; Kesternich *et al.*, 2016). Yet, subgroups like females might remain responsive to increases of the matching rate above 1 (Knutsson *et al.*, 2019).

To summarize, the choice of the matching rate depends on the objective. If a large gift can only be used as a matching subsidy and charity receipts in the short-term are to be maximized, a larger matching rate is beneficial. If instead the focus is on checkbook giving or the likelihood to give, the level of the matching rate needs to be sufficiently large to create an effect, whereas further increases will do no harm but might as well not offer any additional advantage. There is some evidence that increasing the matching rate up to 1 can be beneficial, while increases above 1 have not been found to be effective on the aggregate, at least in the field. Furthermore, matching rates of 1 turned out to be effective across several studies, yet there are cases in which they were not.

3.3 *Does Context Matter?*

The context in which a matching offer is made might alter its effectiveness, either by changing how the offer is perceived or by directly manipulating crucial aspects of decision making. An example for the former is the presentation of the matching subsidy. In most studies, matching is explained as a matching payment of a specific level *for every dollar (euro) donated* by the individual (see e.g. Davis *et al.*, 2005; Eckel and Grossman, 2006b; Karlan and List, 2007; Lukas *et al.*, 2010; Huck and Rasul, 2011). Karlan and List (2007) supplement the standard presentation by an illustrative example, which is based on one of three sample donations suggested to the individual. It turns out to be irrelevant for the effect of matching whether the highest, middle or lowest sample amount is used to illustrate the match. Nevertheless, the presentation of the match can be relevant. Karlan *et al.* (2011) find that framing the matching rate of 1/3 as $*25 for each $75 donated* instead of $*1 for each $3 donated* negatively affects checkbook giving as well as the likelihood of donating. A possible reason put forward by the authors is that subjects might misunderstand the match, thinking they are required to donate $75 before any matching payment is made. A similar alternative representation of the match of 1 as $*25 for each $25 donated* does not create any significant change in the impact of the match.

Sometimes fundraising campaigns not only make use of suggested donation levels, but restrict the donor's choice to certain amounts of money. In contrast to the previously discussed framing of the match, this has a direct impact on the nature of the decision problem faced by the individual. Five different studies analyse the effectiveness of matching under a discrete donation decision (Meier, 2007; Gneezy *et al.*, 2014; Kesternich *et al.*, 2016; Knutsson *et al.*, 2019; Charness and Holder, 2019). Contrasting their results to other experiments suggests, however, that implementing a discrete choice set does not systematically alter the effectiveness of matching.

In contrast to the experimental studies discussed so far, a fundraising campaign might aim at raising a particular amount of money while the money is returned if this threshold is not reached. In such a setting, the introduction of matching reduces the level of checkbook giving required to reach the threshold. As a result the impact might be less positive than in settings without a threshold. Rondeau and List (2008) offer evidence on this, using a laboratory and a field experiment, which are both based on a between-subjects design. Importantly, if the money raised exceeds the threshold, it is invested in additional provision. In the field experiment, Sierra Club supporters are asked via mail to donate for the expansion of an environmental education program delivered to students and in each treatment subjects are informed about the threshold as well as the number of other supporters solicited. Holding the threshold constant at $5000, a matching subsidy of 1 has no significant impact on the intensive margin compared to a no subsidy control. Furthermore, the level of checkbook giving is about 10% lower under the matching subsidy, though this difference is not tested for significance. The laboratory experiment consists of a public good game, in which subjects form groups of 6 and allocate their endowment of $12 between a private and a group account. The money allocated to the group account only results in a payment if a certain threshold is reached. Keeping the threshold constant, matching is found to crowd out checkbook giving relative to a no subsidy condition without lead donor announcement. Hence, only the laboratory experiment offers evidence that in the presence of a threshold the matching impact might turn negative. Yet there is no direct comparison to a setting without a threshold and the fact that a public good game is used limits the comparability to other laboratory studies that rely on a modified dictator game with actual donations to a charity.

In practice, the matching payment is often limited to a certain amount of money to eliminate risk for the party funding the subsidy. Subjects need to be informed about this, which might be a crucial aspect, since it offers information on the likelihood of being matched. In general, a smaller matching limit is exhausted more easily, which could decrease the perceived likelihood that the individual's donation will be matched, and in turn increase the expected price of giving. An explicit test of different matching limits ($25,000; $50,000; $100,000; no limit) stated in a donation campaign did not impact the effectiveness of matches (Karlan and List, 2007). Yet, it is an open question whether these results apply to different fundraising campaigns. For example, a certain matching limit might appear to be more binding in case of a large international organization since a large pool of potential donors is involved.

Finally, in Section 3.1, we discussed heterogeneous treatment effects. There is some evidence that females react more positively to matching subsidies. Furthermore, the donation history of individuals seems to influence the impact of matching, though the direction is ambiguous. Future research is needed to draw general conclusions and shed more light on heterogeneous treatment effects. At this point, it is also important to note that the experimental studies consider very different subject pools, like students (e.g. Eckel and Grossman, 2003; Davis *et al.*, 2005; Eckel and Grossman, 2006b; Lukas *et al.*, 2010), former contributors (Karlan and List, 2007; Karlan *et al.*, 2011; Eckel and Grossman, 2017; Helms McCarty *et al.*, 2018) or opera attendees (Huck and Rasul, 2011; Huck *et al.*, 2015; Adena and Huck, 2017). While this might explain small differences, on the aggregate, empirical findings are relatively robust across different samples.

There are several reasons why the effect of matching subsidies might be context-dependent. Nevertheless, most of the contextual factors on which empirical evidence is available do not exert a

substantial influence. While the literature offers some interesting insights into heterogeneous effects, more research is needed to analyse group specific responses.

3.4 *Is It All about the Price of Giving?*

As discussed in Section 2, there are several predictions on how matching might affect charitable giving, each associated with a set of assumptions about the underlying preferences. Overall, crowding out of the intrinsic motivation to give seems not to be a major aspect as matching mostly creates a nonnegative effect on checkbook giving. Furthermore, the number of cases in which checkbook giving is positively affected stands in contrast to the predictions based on pure warm glow. Instead, findings are in line with individuals being motivated by pure or impure altruism. Thus, one is tempted to conclude that all that matching does is changing the price of giving. In the following, we address three crucial aspects of why this might not be the case.

First, several experimental studies comparing matching and rebate subsidies reveal higher charity receipts and a larger price elasticity under matching, although equivalent rebates imply the same price of giving (see e.g. Eckel and Grossman 2003; Davis *et al.*, 2005; Eckel and Grossman, 2006a, 2006b; Davis, 2006; Eckel and Grossman, 2008; Lukas *et al.*, 2010; Bekkers, 2015; Eckel and Grossman, 2017; or for an overview Vesterlund, 2016). Thus, it seems that either the matching subsidy, the rebate subsidy or both do not solely change the price of giving. It is not the goal of this paper to review the empirical evidence on how the difference between rebates and matches comes about, but matching might additionally encourage giving due to the cooperative element that somebody else is also contributing to the charity (Eckel and Grossman, 2003). However, Hungerman and Ottoni-Wilhelm (2018) offer an appealing explanation, which is in line with matching subsidies only changing the price of giving. In their model of charitable giving, referred to as impure impact giving model, subjects receive utility from the overall level of charity receipts (allowing that more weight is given to the charity receipts produced by the subject's donation than to the charity receipts resulting from the donations of other subjects) as well as warm glow utility. As the warm glow utility is assumed to come from the level of checkbook giving (which for the rebate is the amount passed to the charity and not the amount actually sacrificed after accounting for the refund), a matching subsidy only changes the price of giving whereas a rebate subsidy additionally changes the price of the warm glow utility. The authors find support for their theoretical model when exploring charitable giving data in the context of a real-world tax rebate policy and a matching subsidy offer which naturally occurred in the field. Estimates of the price elasticity of charity receipts amount to 1.2 and 0.2 for the match and the rebate, respectively.

Second, a change in the price of giving can only trigger a behavioural response if it is recognized in the first place. While in a laboratory experiment it is unlikely that subjects are not aware of the subsidy offer, Eckel and Grossman (2017) show that this might not hold in the field. In their field experiment, donors are required to check a box to receive the matching payment offered in the solicitation letter. Slightly more than 25% of those that face a matching offer and donate fail to do so. When only those checking the box are treated as receiving the match and facing the reduced effective price, the price elasticity of charity receipts based on all subjects that answer an attached survey increases in absolute terms from 1.3 to 5.4. This suggests that not recognizing the offer might be a relevant factor. However, as the authors point out, they cannot verify why the box has not been checked, and thus it is not clear whether it is a lack of awareness or a conscious rejection of the matching offer which is driving the results. Furthermore, the design does not allow to draw conclusion about the extent to which missing the offer affects behaviour on the extensive margin.

Third, matching possibly creates a signal for potential contributors, for example, that the nonprofit organization is worth supporting (Karlan and List, 2007; Huck and Rasul, 2011). If this is the case and subjects in the control group are simply asked for donations, the estimated effect on checkbook giving

includes both a price and a signalling effect, at least in a between-subjects design. In a within-subjects design a signal rather affects all treatments equally as, if at all, it alters the belief about the true state of the world which is independent from the treatment condition. Still, most evidence on checkbook giving as well as the extensive and intensive margin is obtained in field experiments with a between-subjects design. To isolate the part of the behavioural response caused by the decrease in the price of giving, it would be necessary to send a similar signal in the baseline treatment. One attempt to achieve this is by mentioning a lead donor who has already provided some funding for the project. If positive signalling matters, we should expect the pure price effect to be lower than the estimates presented so far. The discussion in Section 3.1 already suggests that the difference between a matching subsidy and a lead donor treatment is less positive compared to the difference of a matching subsidy and a no subsidy control treatment without lead donor announcement. Some studies offer direct evidence on this by considering both possible baselines (Rondeau and List, 2008; Huck and Rasul, 2011; Gneezy *et al.*, 2014; Huck *et al.*, 2015). Significant positive effects on checkbook giving turn insignificant if a lead donor baseline is used (Gneezy *et al.*, 2014; Huck *et al.* 2015). The same is found for the effect on the extensive margin (Huck and Rasul, 2011; Gneezy *et al.*, 2014). On the intensive margin insignificant or significant positive differences turn significantly negative (Huck and Rasul, 2011), become insignificant (Gneezy *et al.*, 2014) or remain insignificant (Rondeau and List, 2008). The empirical evidence shows that, in line with expectations, removing the signalling effect by using a lead donor baseline shifts estimates downwards. This is an important step in establishing the potential relevance of signalling embedded in matching subsidies. However, the approach hinges on the assumption that the signal sent by a lead donor baseline perfectly mirrors that of a matching subsidy. An intuitive example why this might not be the case is that a lead donor donation is unconditional on what others give, whereas the match is not.

Despite the fact that most experimental evidence is consistent with the idea that the impact of matching solely results from a change in the price of giving, fully attributing the impact to the price change might be wrong. Yet the precise decision process and factors underlying the behavioural response to matches are not fully identified, which offers an interesting avenue for future research.

3.5 *Do Matches Work in the Long-Term?*

So far, we have focused on the effect of matching on immediate contribution decisions. What about long-term consequences, especially if the matching offer is subsequently removed? Few studies take the long-term perspective into account and it is still unclear what channels are driving long-term effects if they arise. A negative long-term effect might arise from intertemporal substitution or a persistent reduction of intrinsic motivation, whereas habit formation potentially keeps contributions at a high level if they have been increased by matching in the first place (Meier, 2007). Furthermore, a matching campaign which successfully increases the extensive margin might create a positive long-term impact as previous donors are more likely to give in the future than nondonors (Landry *et al.*, 2010; Adena and Huck, 2019).

Studying the donation decision students regularly face when paying their tuition fees, Meier (2007) finds a negative effect of matching on checkbook giving after the matching offer has been removed. This impact is strong enough to outweigh any positive effect observed in the matching period, leading to an overall negative but insignificant point estimate for the net effect of matching on checkbook giving. In contrast, Karlan *et al.* (2011) find no effect of matching on checkbook giving during the treatment period but an increase in checkbook giving in the six subsequent months. As pointed out by the authors, this result should be treated with caution, since significance diminishes as soon as a logarithmic specification is used and the timing of the study might be relevant. Kesternich *et al.* (2016) identify a persistent effect of matching compared to the control group. For repeated bookings with the match still in place as well as for subsequent bookings in a period where the match is removed, the treatment group with a match of 1 has a significantly higher extensive margin and checkbook giving. However, this neither holds for a

match of 1/3 nor 3. While these studies focus on contributions which were previously matched, Bekkers (2015) investigates long-term consequences for contributions which are only related to those previously matched: Individuals complete an online survey and allocate their reward among themselves and one of three health charities. Looking at their giving behaviour in the context of a tsunami relief campaign nine months later reveals no significant difference in contributions between individuals whose donations were matched and those whose donations were not. Nevertheless, there is weak evidence for motivational crowding out as altruistic values elicited in the survey seem to promote a negative long-term effect of matching subsidies on checkbook giving. In contrast, the interaction of joy of giving and having received a matching subsidy is significantly positive.

Overall, these findings show very mixed results of matching in the long-term, which might be driven by case specific characteristics. Yet, the limited empirical evidence available emphasizes that long-term consequences can occur and are a very relevant aspect to judge the effectiveness of matching subsidies as well as the risk associated with running matching campaigns. More research is needed to draw general conclusion, in particular about the donation behaviour after a match has been removed (see Meier, 2007; Karlan *et al.*, 2011; Bekkers, 2015; Kesternich *et al.*, 2016) as well as the dynamics of repeatedly applying a matching subsidy (see Kesternich *et al.*, 2016). Future research should also try to identify under which conditions and through which channels long-term effects are likely to occur. Can matches form habits and keep contributions on a higher level or does matching cause harm due to intertemporal substitution or motivational crowding out? There is some evidence of motivational crowding out (Bekkers, 2015) as well as evidence that matching subsidies, which are successful in increasing the extensive margin, can create a long-term effect on the extensive margin and checkbook giving (Kesternich *et al.*, 2016).

3.6 *What about Alternative Matching Subsidies?*

Although the major part of the literature focuses on linear matching, alternative forms are considered as well. The mechanism in Meier (2007) can be interpreted as threshold matching since individuals' checkbook giving is only matched if they donate the fixed amount to both funds. Huck *et al.* (2015) introduce a different kind of threshold matching, in which individuals' contributions are matched at a rate of 1 if they give at least €50. The performance of this matching scheme does not substantially differ from a simple linear match of 1, mainly because the threshold is below the amount potential donors give anyways. Another matching type investigated by Huck *et al.* (2015) consists of a fixed gift of €20 for any positive donation. Unsurprisingly, this significantly increases the extensive margin, but has no significant effect on checkbook giving or the intensive margin when compared to a baseline without lead donor. As the impact of both nonlinear matching schemes is likely to heavily depend on the threshold at which the linear or fixed gift match is offered, the authors estimate a structural model. Counterfactual simulations make them conclude that a charity is best off if it simply announces a lead donor. If the charity is forced to use the lead donor's money in a matching scheme, they suggest using a fixed matching gift with a strictly positive threshold. The comparison of the different matching schemes is based on the predicted level of checkbook giving. However, it is worth noting that the structural model used for the counterfactual simulations particularly overpredicts the intensive margin for fixed gift matching.

Gee and Schreck (2018) analyse a threshold matching gift for which the threshold is based on the number of donors. Potential donors are randomly and anonymously assigned to groups of 10 and a fixed gift of $50 is additionally given to the charity if a certain number of people within the group donate. In their field experiment, the potential donors are former attendees of an education program for exceptionally good students financed by donations. Subjects receive a fundraising letter from the charity operating this program and either face a no subsidy condition, a linear matching subsidy at rate 1 or a threshold match gift varying in the number of donors within a group needed to reach the threshold (1, 2 or 3 donors). Subjects are aware that their group consists of program attendees of the same year. Only the matching gift

with a threshold of three donors is sufficient to significantly increase the extensive margin compared to the no subsidy condition by about 2 percentage points. Yet, this estimated effect is not significantly different from those of the other matching treatments. No treatment effects are found on the intensive margin. In a within-subject design laboratory experiment with binary donation decisions of giving \$5 or not, the authors investigate a wider range of thresholds (1, 3, 5, 7 or 10) but do not offer a comparison to linear matching. Again, the threshold of 3 performs best with respect to the extensive margin, outperforming the lower threshold and a no subsidy control while being significantly indistinguishable from higher thresholds. Furthermore, the authors show that the belief of being pivotal to secure the matching gift is one important channel to explain differences between the different threshold treatments.

Another matching alternative is presented in Meer (2017), who investigates daily data from an online fundraising platform used by teachers to request funding for school materials. Projects that satisfy certain observable criteria receive a match of 1 from partners of the website or an 'Almost Home' match, in which all of the needed funds are provided by a partner organization as long as the last \$100 are raised by private donors. This represents a cumulative matching threshold for all potential donors. If a project's goal is not reached, donors can decide to get back their donations or to have it reallocated to a different project. In a pooled analysis of both schemes, matching is found to significantly increase the likelihood of receiving a donation on a given day (by 0.76 percentage points, with a baseline of 3 percentage points). However, conditional on receiving a positive amount, the pooled analysis shows that matching has a slightly negative effect on the checkbook giving received per day. As a result, average checkbook giving is raised by 2.8%.

Charness and Holder (2019) investigate two alternative matching schemes which contain an element of competition. In the first scheme, a subject's donation is matched at a rate of 1 if it is at least as large as the median donation. In the second scheme, subjects are anonymously assigned to groups of three and the total amount donated by the group is matched at rate 1 if it does not fall below the median donation of all groups. Both schemes are compared to a no subsidy baseline as well as a classic linear match of 1 in a laboratory experiment, which is based on a between-subjects design. The group competition scheme raises a significantly higher level of checkbook giving than any other treatment, while the level of checkbook giving does not significantly differ between the individual competition scheme and the classic linear match.

As has been shown in Section 3.1, a matching subsidy has a rather negative impact on the intensive margin when compared to a lead donor baseline.[6] Adena and Huck (2017) investigate a matching scheme designed to eliminate such a negative effect: In contrast to previously discussed matching schemes, the matching payment is not given to the same project the contributor supports, but to a different one. In particular, opera visitors are asked to donate for a project aimed to offer children at the age of 2–4 their first experience with classical music. In case of the alternative matching scheme, the matching payment supports the visits of an opera bus at certain locations like schools. Whereas the classical linear match is found to significantly decrease the intensive margin compared to a lead donor baseline, the alternative matching scheme does not. However, both matching treatments increase the level of checkbook giving compared to the lead donor control and their effects do not significantly differ. The theoretical model in the paper suggests that the alternative matching scheme should become more effective when complementary projects are used. This is supported by the results of the within-subjects design laboratory experiment: If the two projects are complements, the alternative matching scheme outperforms the classic linear matching subsidy with respect to the level of checkbook giving as well as the intensive margin. No such difference is found if the projects are substitutes.

Although linear matching schemes are simple to explain to potential contributors and seem to be a natural choice, the empirical evidence shows that small deviations from these linear matching schemes can offer attractive alternatives, especially if they are designed to target particular shortcomings of linear schemes. It will be interesting to observe whether such alternative schemes find their way into fundraising campaigns and support the findings obtained so far.

4. Conclusion

Matching schemes are applied frequently in fundraising campaigns, which makes their effectiveness an object of particular interest. To offer a comprehensive review and allow a more diversified assessment of matching subsidies, we classified results according to four different outcome variables and discussed (i) short-term effects of linear matching, (ii) the choice of the matching rate, (iii) context-dependence, (iv) the relevance of the price of giving, (v) long-term effects and (vi) nonlinear matching schemes. Furthermore, we supplemented our review with detailed tables on the short-term effects of linear matching in laboratory and field experiments (Tables A1 and A2).

A crucial remark is that although matching subsidies manage to increase charity receipts and can even increase checkbook giving, they are not necessarily the best strategy at hand. If possible, the money of the third party could be used in a different way, for example, as an unconditional and announced lead donor gift. As pointed out in Sections 3.1 and 3.4, comparing the match to a lead donor treatment generally shifts estimated effects downwards. Checkbook giving in the matching condition is once estimated to significantly exceed (Adena and Huck, 2017) and never falls significantly below that of the lead donor treatment (Gneezy *et al.*, 2014; Huck *et al.*, 2015), but point estimates speak in favour of a lead donor in two of three studies (Gneezy *et al.*, 2014; Huck *et al.*, 2015). Another promising alternative strategy might be to use the money of the third party to cover a charity's overhead costs, which reduces the price of giving with respect to the output produced by the charity. There is evidence that the level of overhead costs negatively affects giving (e.g. Bowman, 2006; Meer, 2014). Furthermore, Gneezy *et al.* (2014) find that using the money of a third party to cover overhead costs results in higher checkbook giving and a higher extensive margin than using it as a matching subsidy of 1. It might even be possible to incorporate this finding in the design of matching by framing the matching payment as being used to cover overhead costs.

We have already emphasized that focusing on short-term effects can be problematic as important long-term consequences are neglected. When assessing matching subsidies from a social planner's perspective, there is another crucial aspect we need to take into account: potential business stealing due to reallocation of donations towards charities that implement matching. For example, a charity might increase checkbook giving by attracting donors which otherwise would have given the same amount of money to a different charity. Meer (2017) does not find any support for business stealing but shows that the number of matched competitors on an online fundraising platform positively affects checkbook giving to projects, although the magnitude is very small. Even an intertemporal cannibalization is rejected since the average daily number of matched competitors over the previous sixty days has a small but significantly positive effect on checkbook giving. Null (2011) shows in a within-subjects design experiment that the matching rate affects the allocation of a limited amount of money between three similar charities. However, different matching rates across these charities rarely result in subjects shifting the entire money to the charity with the highest matching rate (considering only the charities that receive a positive donation when matching rates across charities are equal). Although further recent studies analyse inter- and intracharity spillover effects of fundraising campaigns (e.g. Krieg and Samek, 2017), more evidence on how particularly matching affects giving to competing charities at the same point in time or over time is needed.

We have learned a lot about matching from the experimental literature, but some crucial questions remain open. Closing these research gaps will not only elevate our level of understanding but will also enable practitioners to improve the design of fundraising schemes and subsidies to foster voluntary giving.

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Data availability Statement

Data sharing is not applicable to this article as no new data were created or analysed in this study.

Notes

1. While we refer to matching as a situation in which some party matches the individuals' contributions, it is also possible to allow each individual to match the contributions of others. However, such mutual matching has not been investigated in the charitable giving literature so far and is difficult to apply in practice if the number of players is large. In the context of public goods, there is a large amount of theoretical literature on mutual matching (e.g. Guttman, 1978; Danziger and Schnytzer, 1991; Varian, 1994; Boadway *et al.*, 2007; Buchholz *et al.*, 2011, 2012; Guttman, 2013) but empirical evidence is rare, even when we consider settings in which the matching payment is directed to the other agent and not to the public good (Andreoni and Varian, 1999; Charness *et al.*, 2007; Bracht *et al.*, 2008; Kass *et al.*, 2015).
2. In general, matching does not need to be explicit. For example, Guttman (1978) subsumes all forms of voluntary cost sharing of public good provision as implicit matching, including conditional cooperation behavior. In such a case, an individual can theoretically identify the implicit matching rate offered by another party by observing that party's reaction to changes in the own contribution level. However, this is a highly complex task and experiments are by and large designed to make matching explicit.
3. The response rates for the different subject types are presented in the paper but it is unclear whether the reported test results are based on pairwise comparisons or comparing the control to the pooled subsidy treatments including rebates. Therefore, we conduct Pearson's χ^2 tests, comparing the response rate of each matching treatment to that of the control for each subject type ($p < 0.01$ for each matching treatment considering regular contributors with a membership, referred to as continuing members in the paper, and $p > 0.2$ in all other cases considered).
4. The information on the number of mailed solicitations, the revenue per solicitation and the mean checkbook donation (intensive margin of donors on which the authors have complete information), presented in Table 1 of Eckel and Grossman (2008), allows to calculate that the average donation of those 55 continuing members excluded from the control group amounts to about \$1871 (compared to \$85.15 of those considered).
5. If the treatments with different matching rates are not pooled in the regression, the match of 1 is not estimated to significantly affect checkbook giving in the first place. If instead they are pooled, matching is estimated to have a significant effect on checkbook giving. The same holds for the results on the extensive margin.
6. Some authors refer to this as crowding out (Huck and Rasul, 2011; Huck *et al.*, 2015; Adena and Huck, 2017). To avoid confusion we exclusively use this term to refer to a decrease in checkbook giving compared to a baseline without lead donor.

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Appendix

Table A1. Short-Term Effects of Matching in Laboratory Experiments (Relative to Baseline without Lead Donor).

Paper	Betw. subjects design	Matching rate	Stated matching limit	Number of subjects in treatment	Donors in treatment (%)	Charity receipts (%)	Checkbook giving (%)	Extensive margin (pp)	Intensive margin (%)	Comments
Charness and Holder (2019)	Yes	1		48	29.2	186.6[d]	Not sig.	–4.2[d]		Subjects can donate nothing, $4, $8, $12, or $16. Subjects' donations are either not matched, matched at a linear rate of 1, matched at a linear rate of 1 if they are not below the median donation, or matched at a rate of 1 if the amount donated by their group of 3 (to which they are anonymously allocated) is not below the median group donation.
		1 if not below median		40	32.5	174.0[d]	Not sig.	–0.8[d]		
		1 if group not below median		42	28.6	240.3[***d]	102.1[***d]	–4.8[d]		
Davis *et al.* (2005)	No	0.5		43		62.2[d]/ 36.1[d]	8.0[d]/ −8.3[d]			Endowment $8/$12. Tested for significance in Davis (2006). Treatments in which subjects make their decision by circling a row in an outcome table are not reported.
		1		43		134.0[d]/ 113.3[d]	17.0[d]/ 6.7[d]			
Davis (2006)	No	0.5		61		63.3[***d]/ 57.1[***d] [62.2[***d]/ 36.1[***d]]	8.2[*a]/ Not sig. [8.0[*a]/ Not sig.]			Endowment $8/$12. Values in squared brackets are based on data from Davis *et al.* (2005). Rephrases decision problem to shift focus towards amount the charity receives. Using one-sided *t*-tests testing whether the effect is positive.
		1		61		132.7[***d]/ 124.5[***d] [134.0[***d]/ 113.3[***d]]	17.0[**a]/ 12.2[**a] [17.2[**a]/ Not sig.]			
Eckel and Grossman (2003)	No	0.25		168						Primary focus on comparing rebates and matches. Reported result is for endowment level $6. Significant[b] (one-sided test) negative price elasticity of charity receipts in random effects Tobit model (−0.94).
		0.33		168		32.4[d]				
		1		168						
Eckel and Grossman (2006b)	No	0.25		46		107.9[d]/ 123.7[d]/ 87.2[d]				Endowment $4/$6/$7.5. Significant[b] negative price elasticity of charity receipts in Tobit model (−2.59).
		0.33		46		115.8[d]/ 142.7[d]/ 86.3[d]				
		1		46		347.5[d]/ 366.4[d]/ 287.7[d]				

(*Continued*)

Table A1. *Continued.*

Paper	Betw. subjects design	Matching rate	Stated matching limit	Number of subjects in treatment	Donors in treatment (%)	Charity receipts (%)	Checkbook giving (%)	Extensive margin (pp)	Intensive margin (%)	Comments
Eckel *et al.* (2007)	No	0.25		256		34.9[d]/ 38.0[d]/ 37.5[d]	7.9[d]/ 10.4[d]/ 10.0[d]			Endowment \$10/\$20/\$50. Significant positive effect of each matching treatment on extensive margin in random effects logit model (2.0pp,[***] 2.7pp[***] and 3.0pp[***] for matching rate 0.25, 0.5 and 1, respectively). Significant positive effect of matching on checkbook giving in random effects Tobit model.
		0.5		256		68.1[d]/ 73.7[d]/ 75.6[d]	11.9[d]/ 15.9[d]/ 17.0[d]			
		1		256		146.8[d]/ 155.7[d]/ 157.4[d]	23.3[d]/ 27.9[d]/ 28.7[d]			
Gee and Schreck (2018)	No	\$50 if 1 of 10 give		100	39			15.0[***]		Endowment \$16. Binary decision to give \$5 or nothing.
		\$50 if 3 of 10 give		100	50			26.0[***]		
		\$50 if 5 of 10 give		100	43			19.0[***]		
		\$50 if 7 of 10 give		100	45			21.0[***]		
		\$50 if 10 of 10 give		100	42			18.0[***]		
Lukas *et al.* (2010)	No	0.25		55		37.2[d]/ 33.1[d]	Not sig./ Not sig.			Endowment \$10/\$20. Effect of each matching treatment on charity receipts is significantly positive in Tobit regression in which the matching effect is not allowed to differ by endowment.
		0.33		55		40.0[d]/ 54.2[d]	5.2[d]/ 15.9[d]			
		1		55		157.1[d]/ 163.0[d]	28.5[d]/ 31.5[d]			

The effects on charity receipts, checkbook giving and the intensive margin are presented as relative change (in percent) using the control condition without matching subsidy as baseline. The effects on the extensive margin are presented as absolute difference in the likelihood to give between the matching treatment and the control condition (in percentage points).
All experimental results which are presented in this table are based on a control condition without lead donor announcement.
Matching rates of 0.33 and 1/3 are sometimes used interchangeably. If instructions are provided, we use the corresponding matching rate, otherwise, we rely on the rate stated in the paper.
[***]$p < 0.01$, [**]$p < 0.05$, and [*]$p < 0.10$ in conducted test, two-sided if not otherwise stated in the comment section. Values without an asterisk are not tested for significance. Using regression results (including covariates) whenever applicable.
[a]Since the tables in Davis *et al.* (2005) and Davis (2006) showed inconsistencies, values are based on own calculations using the data retrieved October 10, 2017, from http://www.people.vcu.edu/~dddavis/vita.htm.
[b]Refers to the coefficient and not directly to the reported marginal effect. The coefficient is significant at the 5% level or better.
[c]We thank the author(s) for providing this information upon request.
[d]Value is calculated by using descriptive statistics, e.g. reported means or response rates.
[e]Not considering undelivered solicitations.

Table A2. Short-Term Effects of Matching in Field Experiments (Between-Subjects Design).

Paper	Lead donor control	Matching rate	Stated matching limit	Number of subjects in treatment	Donors in treatment (%)	Charity receipts (%)	Checkbook giving (%)	Extensive margin (pp)	Intensive margin (%)	Comments
Adena and Huck (2017)	Yes	1	€30,000	6143	2.1		2.4**	0.6**	−23.7**	
		1 to diff. project	€30,000	6144	1.9		1.8*	0.4*	Not sig.	
Bekkers (2015)	No	0.5		184	51.1		57.5[d]	15.9[d]	8.4[d]	Survey based field experiment. Checkbook giving and intensive margin are based on the fraction of the endowment passed, since endowment varies across subjects (depends on the time spend to answer the survey).
		1		173	57.2		89.7[d]	22.0[d]	16.5[d]	
Eckel and Grossman (2008)	No	0.25	$7500	1200/ 15,000/ 30,000	19.2/ 0.9/ 0.6	−33.4[d]/ 32.7[d]/ 56.5[d]	−46.7[d]/ 5.5[d]/ 26.1[d]	−9.4[d]/ 0.0[d]/ 0.1[d]	Not sig./ Not sig./ Not sig.	Continuing/lapsed/ prospect members. Intensive margin based on donors for which authors receive complete information on a set of covariates (about 1.1% of donors excluded). OLS regression results show a significant negative price elasticity of the intensive margin for continuing members (−0.099**) but not for others.
		1/3	$15,000	1200/ 15,000/ 30,000	19.8/ 1.0/ 0.4	−28.5[d]/ 50.9[d]/ 13.0[d]	−46.4[d]/ 12.7[d]/ −13.0[d]	−8.8[d]/ 0.1[d]/ −0.1[d]	Not sig./ Not sig./ Not sig.	
Eckel and Grossman (2017)	No	0.25	$10,000	4462	3.4		38.6[d]	Not sig.		Additional $5 donated by the researchers if the survey attached to the solicitation was completed. Only individuals who returned the complete survey are considered (completion rate does not differ across all treatments). Significant positive effect on checkbook giving, but significance level not stated. Significant negative price elasticity of charity receipts in Tobit model (−1.25** and −5.43** if corrected for donors' awareness).
		1/3	$15,000	4834	4.0		47.4[d]	8.4**		

(*Continued*)

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Table A2. *Continued.*

Paper	Lead donor control	Matching rate	Stated matching limit	Number of subjects in treatment	Donors in treatment (%)	Charity receipts (%)	Checkbook giving (%)	Extensive margin (pp)	Intensive margin (%)	Comments
Gneezy *et al.* (2014)	Yes	1	$10,000	10,000	4.4		Not sig.	Not sig.	Not sig.	Individuals can donate nothing, $20, $50 or $100.
	No	1	$10,000	10,000	4.4		51.9^{***d}	1.05^{***d}	15.7^{***d}	Individuals can donate nothing, $20, $50 or $100.
Gee and Schreck (2018)	No	1	$3000	599	2.3	83.0^{d}	−8.6^{d}	Not sig.	Not sig.	Subjects attended a program operated by the organization in the past. In all nonlinear treatments, subjects are allocated to groups of 10 and additional $50 are given to the charity if at least 1, 2 or 3 subjects of the group donate. All group members attended the program in the same year, and groups are anonymous and randomly assigned.
		$50 if 1 of 10 give		300	2.0	17.0^{d}	−3.4^{d}	Not sig.	Not sig.	
		$50 if 2 of 10 give		298	2.4	−45.1^{d}	−45.1^{d}	Not sig.	Not sig.	
		$50 if 3 of 10 give		299	3.7	190.4^{d}	190.4^{d}	1.9*	Not sig.	
Helms McCarty *et al.* (2018)	No	0.1		2119	2.8		Not sig.	Not sig.	Not sig.	
		1	€100 per donor	2122	3.6		Not sig.	Not sig.	Not sig.	
Huck and Rasul (2011)	Yes	0.5	€60,000	3745	4.2	Not sig.		Not sig.	−30.1**	Relative effects for charity receipts are based on absolute effects in OLS regression divided by average charity receipts in lead donor control obtained from the descriptive statistics. In a corresponding Tobit model both estimated treatment coefficients are significant at the 10% level.
		1	€60,000	3718	4.2	66.5**		Not sig.	−35.4***	
	No	0.5	€60,000	3745	4.2	125.8^{d}		Not sig.	35.9^{*d}	
		1	€60,000	3718	4.2	176.3^{d}		Not sig.	Not sig.	

(*Continued*)

Table A2. *Continued.*

Paper	Lead donor control	Matching rate	Stated matching limit	Number of subjects in treatment	Donors in treatment (%)	Charity receipts (%)	Checkbook giving (%)	Extensive margin (pp)	Intensive margin (%)	Comments
Huck *et al.* (2015)	Yes	0.5	€60,000	3745	4.2		Not sig.			Results for matching rates 0.5 and 1 are based on same data as Huck and Rasul (2011). Only new insights reported here. €20 threshold gift means that additional €20 are given to the charity if the subject donates.
		1	€60,000	3718	4.2		Not sig.			
		1 above €50	€60,000	3746	4.3		Not sig.	0.8^{*d}	-25.8^{**d}	
		€20 threshold gift	€60,000	3746	4.7		-29.2^{*d}	1.2^{***d}	-47.6^{***d}	
	No	0.5	€60,000	3745	4.2		50.2^{**d}			Results for matching rates 0.5 and 1 are based on the same data as Huck and Rasul (2011). Only new insights are reported here. €20 threshold gift means that additional €20 are given to the charity if the subject donates.
		1	€60,000	3718	4.2		37.6^{*d}			
		1 above €50	€60,000	3746	4.3		49.8^{**d}	Not sig.	31.8^{**d}	
		€20 threshold gift	€60,000	3746	4.7		Not sig.	1.0^{**d}	Not sig.	
Karlan and List (2007)	No	1	Varying	11,133	2.1	130.9^{d}	Not sig.	Not sig.	Not sig.	Pooled analysis of matching vs. control: $18.9\%^{*}$ increase of checkbook giving and $0.4pp^{***}$ increase of the extensive margin. No significant effect on the intensive margin. Maximum matching amount is either not stated, $25,000, $50,000 or $100,000.
		2	Varying	11,134	2.3	280.2^{d}	Not sig.	Not sig.	Not sig.	
		3	Varying	11,129	2.3	363.9^{d}	Not sig.	Not sig.	Not sig.	
Karlan *et al.* (2011)	No	1/3		6714			Not sig.	Not sig.		Significant positive effect on checkbook giving in post-experiment period.
		1		6438			Not sig.	Not sig.		
Kesternich *et al.* (2016)	No	1/3		1342	26.4	Not sig.	Not sig.	Not sig.		Binary decision of whether or not to offset carbon emissions from bus travel (in first booking).
		1		1282	31.0	126.2^{***d}	13.1^{**d}	4.9^{***}		
		3		1364	29.3	327.9^{***d}	Not sig.	3.1^{*}		
Knutsson *et al.* (2019)	No	1		541^{c}	14.4			4.6^{**c}		Individuals can decide whether to donate the deposit received when returning cans and bottles.
		3		544	16.5			6.7^{***c}		
		5		541	18.9			9.1^{***c}		

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Table A2. *Continued.*

Paper	Lead donor control	Matching rate	Stated matching limit	Number of subjects in treatment	Donors in treatment (%)	Charity receipts (%)	Checkbook giving (%)	Extensive margin (pp)	Intensive margin (%)	Comments
Meer (2017)		1 or threshold gift	Project specific				2.8***	0.8***	−1.5*	Units of analysis are fundraising results of different projects on different days. Results are based on pooled analysis of a match at rate 1 and a threshold gift match, where the full project is funded if the last $100 are provided by donors. 20.8% of the more than 27 million observations include one of the two matches. The intensive margin is thus the average effect on checkbook giving, considering only those day-project observations with a positive amount. Contributions returned or reallocated to another project if project's goal not reached.
Meier (2007)	No	0.25 0.5		265 267				Not sig.[c] 13.5**[c]		Students face decision whether to contribute fixed amounts to no, one or two funds. Only in the latter case a match is received if offered. The extensive margin refers to the likelihood of contributing to both funds. 67.9% of those receiving a match donate to both funds.
Rondeau and List (2008)	Yes No	1 1	$2500 $2500	750 750	4.8 4.8		−23.9[d,e] −10.4[d,e]	−0.1[d,e] 0.8[d,e]	Not sig. Not sig.	Contributions returned if threshold is not reached. Contributions returned if threshold is not reached. Include additional control with different threshold than matching treatment, but results are not reported here.

The effects on charity receipts, checkbook giving and the intensive margin are presented as relative change (in percent) using the control condition without matching subsidy as baseline. In a baseline with a lead donor, charity receipts do not include the lead donation. The effects on the extensive margin are presented as absolute difference in the likelihood to give between the matching treatment and the control condition (in percentage points).
All experimental results, which are presented in this table, are based on a between-subjects design.
Matching rates of 0.33 and 1/3 are sometimes used interchangeably. If instructions are provided, we use the corresponding matching rate; otherwise, we rely on the rate stated in the paper.
$^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.10$ in conducted test, two-sided if not otherwise stated in the comment section. Values without an asterisk are not tested for significance. Using regression results (including covariates) whenever applicable.
[a]Since the tables in Davis *et al.* (2005) and Davis (2006) showed inconsistencies, values are based on own calculations using the data retrieved October 10, 2017, from http://www.people.vcu.edu/~dddavis/vita.htm.
[b]Refers to the coefficient and not directly to the reported marginal effect. The coefficient is significant at the 5% level or better.
[c]We thank the author(s) for providing this information upon request.
[d]Value is calculated by using descriptive statistics, e.g. reported means or response rates.
[e]Not considering undelivered solicitations.

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