Social Context and Prosocial Behavior: Three Essays in Behavioral Economics

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Christoph Oslislo

aus

Köln

Referent: Jun. Professor Dr. Frederik Schwerter

Korreferent: Professor Dr. Bernd Irlenbusch

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Introduction

"Giving is inherently social." This quote by James Andreoni and A. Abigail Payne can be seen as the point of departure for this dissertation (Andreoni and Payne, 2013, p. 12). It points out that prosocial behavior is usually embedded in a social context. Studying the social context and the role that social aspects of the decision environment play in shaping giving decisions is crucial for a better understanding of social interactions and moral decision making.

This collection of essays explores the inherent sociality of giving. It addresses various social aspects of the decision environment, such as social information and social influence, group membership and social identity, and social norms. Methodologically, questions related to the decision context are well addressed empirically through economic experiments. A major advantage of controlled experiments is that they allow causal inference based on a comparison of behavior under different conditions to which subjects are ideally randomly assigned. From a behavioral economics perspective, studying issues related to the social context of prosocial behavior is critical to better understanding the determinants of giving and patterns in human decision making. For nonprofits and fundraising managers, clean empirical investigations of the decision environment in giving contexts are highly valuable for successful campaign design.[1]

The three papers presented here employ different empirical strategies to show that social aspects of a decision environment can significantly affect economic decisions. The first paper examines the effectiveness of a fundraising tool that provides information about others' donation decisions and sheds light on the underlying behavioral mechanisms. It is based on natural field experiments in which subjects are unaware of their participation in an experiment. The second study confirms the view that prosocial behavior can be highly context-dependent and asks whether social norms, often considered long-term and stable social constructs, can change as a result of elections. It empirically examines social change in a series of tightly controlled online laboratory experiments. The third paper shows that interventions designed to increase giving can backfire if they trigger associations that are detrimental to the targeted behavior. The empirical strategy of

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[1]For a German-language handbook article by the author on behaviorally informed fundraising strategies, see Oslislo (2024).

this study is based on a natural field experiment complemented by an online experiment, combining the advantages of both methods.

Chapter 1 presents the single-author study "Charitable Giving Underway: Two Field Experiments on Fundraising Thermometers." Fundraising thermometers, visual representations of the progress a nonprofit has already made toward a campaign goal, provide social information relative to an explicit fundraising goal. In collaboration with an international development organization, I conduct two natural field experiments with a total of 160,000 donors to examine the effectiveness of the tool in raising funds for a charitable cause. The use of fundraising thermometers significantly increases donation rates by 52% and funds raised by 42%. The effects of the tool on total donations are insensitive to a controlled exogenous variation in goal progress and in the number of donors needed to reach the goal, and are replicated with a different sample. Furthermore, long-term data suggest that engaging donors with realistic campaign goals enhances donor relationship management. Six months after the intervention, donors who were informed that the campaign goal had been met contribute significantly more in a subsequent solicitation compared to those who were told that the campaign was unsuccessful. Carefully designed control treatments that decompose the different informational elements of the tool allow for disentangling the underlying mechanisms. Neither the social information nor the explicit goal alone is sufficient to explain the observed effects. The results are consistent with the classic notion from psychology that unfinished processes, once started, generate a strong motivation in people to see them completed. The findings provide valuable guidance for fundraising practitioners on designing effective solicitation strategies and deepen our understanding of the mechanisms driving social influence in charitable giving.

Chapter 2 is about the study "Norms and Elections: How elected rules can make behavior (in)appropriate" and is based on joint work with Arno Apffelstaedt and Jana Freundt (Apffelstaedt et al., 2022). A number of recent observations suggest that social norms can change rapidly as a result of election outcomes. We explore this conjecture using a controlled online experiment. In our experiment, participants rate the social appropriateness of sharing income with poorer individuals. We compare the ratings for situations in which a rule has been elected that asks people to share with ratings when the elected rule asks people not to share. We also compare both situations with ratings in a decision environment in which there is no official rule at all. In the absence of an elected rule, sharing is widely considered socially appropriate, while not sharing is considered socially inappropriate. We show that elections can change this social norm: They shift the modal appropriateness perception of actions and, depending on the elected rule, increase their dispersion, i.e. erode previously existing consensus. As a result, actions previously judged socially inappropriate (not sharing) can become socially appropriate. This power prevails, albeit in a weakened form, even if the election process is flawed (introducing

a voting fee or "poll tax," bribing voters, disenfranchising poorer voters). Drawing on behavioral data from a related experiment, we demonstrate that election-induced norm shifts predict behavior change. An additional treatment suggests that both the social information contained in election results and the social appropriateness of following rules *per se* play a role in shifting social norms.

Chapter 3 presents the study "Identity Priming, Cognitive Spillovers and Charitable Giving" and is based on joint work with Luca Henkel and Frederik Schwerter. Behavioral interventions can change behavior by influencing the way people cognitively process information. We conduct a controlled natural field experiment with over 100,000 subjects to study the effect of a group appeal based on regional affiliation on donations to a charity fighting global poverty. The group appeal significantly reduces the number of donors by 33% and total donations by 43%. We relate this to prior research on identity priming in charitable giving, and argue that the negative treatment effect can be explained by a reallocation of cognitive resources induced by the intervention. An additional online experiment provides evidence for the mechanism and indicates that local community appeals shift donors' attention to local issues and away from the global issues targeted by the charity, affecting their giving decisions.

Chapter 1

Charitable Giving Underway: Two Field Experiments on Fundraising Thermometers

1.1   Introduction

Charitable giving plays an important role in addressing societal needs, and effective fundraising techniques are critical in motivating potential donors to give. This paper examines a fundraising tool with broad applicability and sheds light on the underlying behavioral mechanisms. The idea behind the tool is simple: charities frame fundraising appeals as part of an ongoing campaign with an explicit campaign goal defined by a numerical target, and provide graphical information about progress already made toward that goal. Fundraising practitioners seem confident in the effectiveness of such *fundraising thermometers* and feature the tool on nonprofit fundraising websites, social media pages, and marketing materials. Figure 1.1 shows four examples of nonprofit campaigns using a fundraising thermometer on a large crowdfunding platform.[1]

Despite their widespread use, fundraising thermometers have not been thoroughly analyzed conceptually, nor has their impact on charitable giving been rigorously tested. Theoretically, the effectiveness of the tool is not straightforward: On the one hand, if donors view contributions from different individuals as substitutes, providing information about funds already raised may crowd out individual gifts and thus harm overall fundraising efforts (Varian, 1994; Gächter et al., 2010). Additionally, revealing campaign progress could signal a higher level of public good provision, reduce marginal returns and thereby lower contributions (Landry et al., 2006). On the other hand, fundraising ther-

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[1]Screenshots for the four examples were taken on August 16, 2023, from https://www.gofundme.com.

Figure 1.1: Examples for fundraising thermometers on the fundraising platform GoFundMe
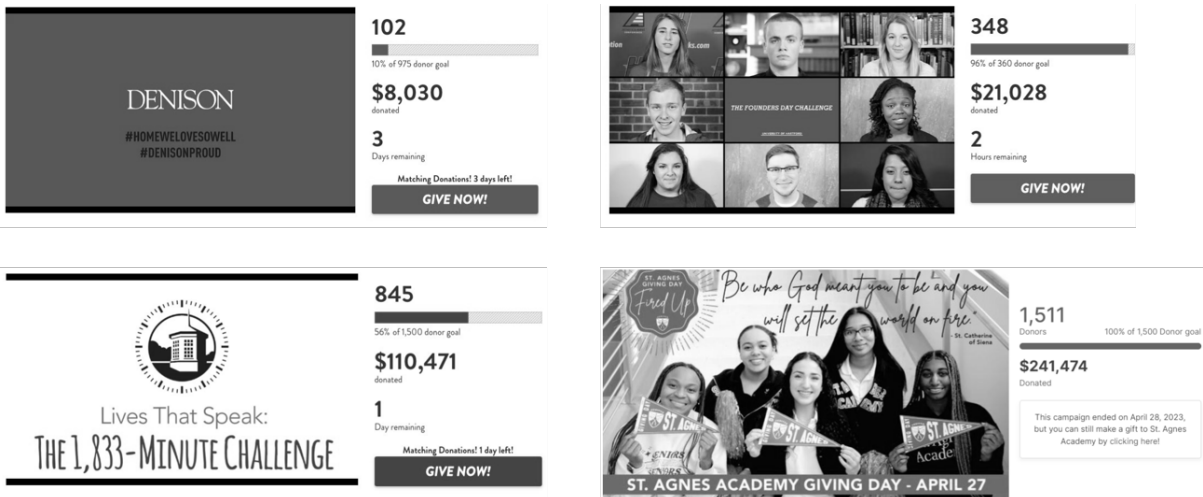
mometers offer information that could positively affect giving. By showing past donation behavior, the tool provides both normative and informational cues, which may promote conformity to social norms (Bernheim, 1994; Akerlof, 1997) or facilitate social learning about the charity's quality (Vesterlund, 2003). This study aims to provide the first clean empirical evaluation of the effectiveness of fundraising thermometers in real-world charitable campaigns.

I conduct two large-scale natural field experiments (Harrison and List, 2004). The design of the main experiment can be summarized as follows: in collaboration with a large nonprofit organization, I designed a direct mail solicitation campaign targeting 110,000 regular donors from the organization's donor base. All one-page solicitation letters are identical except for the last paragraph, which varies across treatment conditions and either includes a fundraising thermometer (*Therm*) or not (*Control*). Exogenous variations in the tool's specifications, namely the progress level and the number of donors needed to reach the goal, provide insights into the conditions under which fundraising thermometers influence giving behavior. To explore the underlying mechanisms, I decompose the tool into its two primary informational components and implement two additional treatments: one in which the last paragraph provides only the social information contained in the fundraising thermometers, but no explicit goal (*Social Info*), and one in which it presents only a campaign goal, but no social information (*Goal*). Donors are randomly assigned to one of the letter versions and giving patterns in response to the solicitation are compared between groups. A follow-up experiment with a similar design and a different sample of about 50,000 donors examines the effects of more extreme progress levels and of the presence of an infographic through an additional control treatment that closely resembles the layout of the *Therm* letters but does not include a fundraising thermometer (*Pseudo_Therm*).

Comparing giving behavior in response to letters with and without the tool yields large significant treatment effects: Relative to the control, fundraising thermometers increase the number of donations by 52% and total giving by 42%. This results is replicated with a different sample (52% and 40%, respectively). The effect of the tool on funds raised is insensitive to variations in the progress level and the number of donors needed to reach the campaign goal. Both providing social information alone and communicating a campaign goal alone have significant positive effects on giving relative to the control. However, both treatments perform significantly worse than the fundraising thermometer. Giving in response to the letter containing an infographic but no progress framing does not differ from giving in the control condition.

From a management perspective, another important question related to the effectiveness of fundraising thermometers is how donors respond to future information about whether a campaign was successful in meeting its goal. To shed light on this, I included a sentence in an information letter sent to all donors in the sample five months after the experimental intervention. The sentence provided aggregate data on the giving behavior of regular donors throughout the year. Depending on the campaign goal assigned to them in the main experiment, recipients could interpret this information as an indication of whether or not the campaign met its goal. To assess the effect of this information on donations, I analyzed long-term data from the partner organization, focusing on two solicitations - one before and one after the information letter. In the solicitation following the letter, donors assigned to a successful campaign give significantly more than those assigned to an unsuccessful campaign. Crucially, this difference in giving emerges only after donors are informed of the campaign's outcome, not before, revealing a causal effect of the information on subsequent generosity.

The empirical findings presented here are consistent with a classic notion in psychology that has recently received increasing attention from scholars in related fields. Psychologists have long suggested that the encounter with an ongoing, unfinished process creates emotional tension that people are motivated to resolve. As a result, individuals often feel compelled to invest in completing unfinished activities, even when doing so may not be in their best rational interest (Zeigarnik, 1938; Rickers-Ovsiankina, 1928). Building on this intuition, I propose that people tend to feel more committed to charitable campaigns that have already made progress than to those that are just beginning, which may explain the effectiveness of fundraising thermometers. This argument draws on research in marketing and consumer behavior, where studies show that consumers in rewards programs drink more coffee (Kivetz et al., 2006) and wash their cars more frequently (Nunes and Dreze, 2006) when they perceive progress toward a reward (e.g., pre-stamped spaces on a loyalty card).

This study contributes to a strand of the literature on the economics of charitable

giving that examines the demand side of charity, i.e., the role fundraisers play in the market for charitable giving (Rose-Ackerman, 1982; Andreoni, 1998; Romano and Yildirim, 2001; Vesterlund, 2003). Existing work shows that charitable giving is sensitive to various aspects of the decision architecture and can be influenced by behaviorally informed interventions.[2] The present paper provides the first clear evidence of the effectiveness of a specific low-cost intervention based on framing a solicitation as part of an ongoing fundraising campaign, rather than as a stand-alone solicitation, and illustrating some progress toward an explicit campaign goal.

Furthermore, by showing that providing social information (in the absence of a campaign goal) can increase giving, this study contributes to a vibrant literature on sequential fundraising and the effects of information about peer behavior on giving (Frey and Meier, 2004; Alpizar et al., 2008; Shang and Croson, 2009; Smith et al., 2015; Kessler, 2017). In particular, the experiments presented here provide some first field evidence for social influence via conformity in a prosocial domain, complementing recent evidence from online experiments (Goette and Tripodi, 2021). In the present setting, long-time regular donors are arguably well informed about the organization's work, leaving very limited scope for social learning about the quality of the charity (Vesterlund, 2003). Instead, it is argued that "conformists" in the subject pool interpret disclosed information about other donors' choices as a signal of appropriate behavior and feel compelled to conform their behavior to the social reference (Sliwka, 2007).

The results of this study also show that explicitly communicating a campaign goal (without any information about campaign progress) has significant positive effects on giving. Existing work in psychology (Ryan et al., 1996; Locke and Latham, 2006) and economics (Hsiaw, 2013) examines how goal setting can help overcome self-control problems in personal tasks. The present paper adds to this line of research and provides, to the best of my knowledge, the first evidence that the presence of an explicit group goal that can only be achieved through collective action can significantly increase donations in a charity campaign. For fundraising managers, this finding underscores the short- and long-term potential of setting and communicating realistic campaign goals and notifying donors when campaign goals are met.

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[2]Scholars have empirically studied a broad range of fundraising strategies aimed at increasing donations and learning about the determinants of giving, such as strategies based on social comparisons and prestige (Soetevent, 2005; Landry et al., 2010; Meer, 2011; DellaVigna et al., 2012; Andreoni et al., 2017), lottery incentives (Landry et al., 2006), donor gifts and thank-you calls (Falk, 2007; Samek and Longfield, 2023), rebate rules (Eckel and Grossman, 2003; Spencer et al., 2009), and warm-glow appeals (List et al., 2021). Another related strand of literature comprises field studies aimed at understanding when and why a generous leadership gift by a wealthy individual or institution raised during a quiet phase of a capital campaign affect giving and whether fundraisers should communicate a lead donation as an unconditional gift (List and Lucking-Reiley, 2002; Landry et al., 2006; Rondeau and List, 2008; Huck et al., 2015; Karlan and List, 2020) or introduce a matching scheme (Karlan and List, 2007; Eckel and Grossman, 2008; Huck and Rasul, 2011; Karlan et al., 2011; Adena and Huck, 2017).

The paper continues with a presentation of the central conceptual considerations related to fundraising thermometers that motivate the experimental design (section 1.2). In section 1.3, I present the experimental design and treatments in detail, followed by the empirical results (section 1.4). I then discuss possible mechanisms underlying the results as well as implications for practitioners (section 1.5). In section 1.6, I summarize the findings and suggest promising avenues for future research.

1.2 Conceptual considerations

Fundraising thermometers typically combine four pieces of information, each of which can influence giving behavior. First, the tool informs potential donors of a campaign goal defined by a numerical target. Researchers have argued that goals can have strong motivational effects across a wide range of applications (Locke and Latham, 2015). In a controlled fundraising experiment, Rondeau and List (2008) find that varying the goal of a capital campaign can significantly affect individual giving behavior and fundraising success.[3] In Figure 1.1, campaign goals are defined by the absolute number of donors and vary from 360 to 1,500 donors.

Second, fundraising thermometers provide social information, i.e. insights into the generosity of their peers. A number of previous studies based on observational data as well as field experiments find that donation decisions are influenced by previous decisions of others (Frey and Meier, 2004; Alpizar et al., 2008; Martin and Randal, 2008; Shang and Croson, 2009; Smith et al., 2015; Kessler, 2017). In the examples in Figure 1.1, fundraising thermometers provide information about the number of previous donations. For example, the donation request at the top left tells the potential donor that 102 people have donated so far, while the request at the bottom right tells the donor that 1,511 people have already made a donation.

Third, the tool provides a visual representation of the status of the campaign at the time of the solicitation by displaying the two pieces of information in relation to each other. From a theoretical point of view, the behavioral effects of different progress levels are not clear. On the one hand, freeriding incentives may become more pronounced the closer the contributions collected are to the campaign goal, because an individual may believe that the campaign is likely to succeed even without his or her contribution. On the other hand, the "goal gradient hypothesis" states that the closer someone gets to a goal,

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[3]In particular, the authors find that the size of contributions is positively correlated with the size of the campaign threshold. The authors argue that this finding is consistent with the notion that donors are concerned about the need for charitable contributions as expressed by the size of the campaign goal. Note that because the primary goal of their study is to examine the effects of different forms of leadership gifts, the authors do not implement a no-goal control condition. This study complements their findings by exogenously varying not only the size of the campaign goal, but also the presence of a goal.

the harder he or she will work to achieve it due to an increased perception of impact.[4] Perceived impact can be an important motivator for charitable giving if donors derive satisfaction from personally increasing the production of a public good and making a difference in solving a social problem (Duncan, 2004; Metzger and Günther, 2019). Thus, fundraising campaigns that are close to the campaign goal are expected to increase the motivation of individual donors to give (Cryder et al., 2013; Argo et al., 2020). Following this reasoning, the perceived impact of a donation in the top-left request in Figure 1.1, which indicates a campaign progress of 10%, may be lower than in the top-right request, which indicates that the campaign is very close to reaching its goal.

Fourth, fundraising thermometers provide information on how many donors the campaign is targeting. Note that the top left campaign needs 873 more donors, while the top right campaign needs only 12 more donors to reach its goal. This number affects the relative progress each additional donor makes toward the goal. In particular, in the top-left campaign, an additional donation reduces a much smaller proportion of the remaining distance to the goal than in the top-right campaign. From the perspective of an individual donor, the perceived responsibility for the success of the campaign may be more diffuse in an environment where a relatively large number of other donors are needed, i.e., where the individual feels that his or her decision contributes relatively little to the overall outcome, than in an environment with a smaller number of other donors. Intuitively, group size, and thus perceived responsibility, could have behavioral implications and alter individual giving decisions.[5]

In the following, I describe the design of controlled experiments in which the fundraising success of donation requests with fundraising thermometers in the style of the motivating examples in Figure 1.1 is systematically compared to requests without the tool. Additional control treatments allow conclusions to be drawn about the role of each of the four pieces of information described above for the effects of fundraising thermometers on donations.

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[4]The first systematic empirical investigations of goal gradient behavior were conducted in the fields of animal learning and behaviorism. A classic experiment with rats running in a maze showed that the closer the rats got to their food reward, the faster they ran (Hull, 1932). More recent studies have focused on goal-gradient behavior as a human tendency and have examined marketing tools designed to exploit the phenomenon (Nunes and Dreze, 2006; Kivetz et al., 2006).

[5]Existing empirical studies show that the perception of the likelihood that one's decision will determine an outcome can indeed have behavioral consequences. In a classic psychological experiment, Latane and Darley (1968) show that an individual's likelihood of acting prosocially can decrease as the size of the group of individuals facing the same decision problem increases. Relatedly, existing work in economics shows that the perception that one's decision is critical to an overall outcome can influence individual moral decision making (Falk et al., 2020).

1.3 Experimental design and procedures

I conduct two natural field experiments with donors of a large charitable organization. All subjects in the two samples know the charity and have been donating to the organization on a regular basis at the time of the experiments. This so-called "warm list" of donors allows me to use baseline data on individual characteristics for block random assignment of treatment conditions. Moreover, conducting the experiment with regular donors provides an opportunity to study not only the short-run but also the long-run effects of my interventions.

1.3.1 Basic setting

The partner organization is a well-known international nonprofit organization dedicated to providing humanitarian and developmental assistance to children worldwide. It currently operates in over 190 countries and territories. Much of the organization's fundraising is done through national committees, each set up as an independent local nongovernmental organization.

Working closely with fundraising experts at the headquarter of the German National Committee, I designed direct mail campaigns targeting regular donors. Regular donors have an ongoing commitment to donate a fixed amount of money to the organization on a regular basis. The purpose of the solicitation letter is to motivate donors to increase their regular giving.[6] The majority of the one-page solicitation letter was written and designed by fundraising professionals, following the organization's typical fundraising practices. The main text of the letter aims to draw donors' attention to a pressing development issue. All letters are identical except for the last paragraph, which varies depending on the treatment condition. At the end of the letter, donors who decide to upgrade select one of the suggested upgrade amounts (€5, €10, or €15 per month), or choose their own amount using the free-form field.

1.3.2 Treatment design

Overview of treatments   The experimental design of the main experiment comprises four different groups of treatments. To answer my main research question, I compare giving in response to the organization's standard solicitation letter (*Control*) to giving in response to letters with a fundraising thermometer (*Therm*). To examine mechanisms, two additional treatments are implemented: in the *Social Info* treatment, the last

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[6]In fundraising circles, this type of solicitation for regular donors is known as an "upgrade request". Similar to a one-time ask, the success of such a campaign is typically measured by response rates and the amount of money raised. For simplicity, I will refer to upgrade decisions as "donations."

paragraph provides information about the behavior of other donors - but no specific campaign goal - while in the *Goal* treatment, the last paragraph provides information about a campaign goal - but no information about progress toward the goal. In addition, the information provided is exogenously varied within the treatment groups, resulting in a total of nine letter versions (Table 1.1). The differences between the letter versions are described in more detail below.

Table 1.1: Main Experiment: Overview of the nine letter versions

			Variable letter components			
	Treatment	**Letter version**	**Social Information**	**Goal**	**Progress**	**n**
1	*Control*	*Control*	No	No	No	12,214
2	*Therm*	*Therm_HI_88*	14,000	16,000	88%	12,206
3		*Therm_LO_88*	140	160	88%	12,219
4		*Therm_LO_28*	140	500	28%	12,226
5		*Therm_HI_28*	14,000	50,000	28%	12,216
6	*Social Info*	*Info_LO*	140	No	No	12,223
7		*Info_HI*	14,000	No	No	12,210
8	*Goal*	*Goal_HI*	No	16,000	No	12,216
9		*Goal_LO*	No	500	No	12,215
TOTAL						**109,945**

Fundraising thermometers   Fundraising thermometers graphically illustrate the progress a campaign has already made toward its goal. The progress level shown by the fundraising thermometer in the *Therm* letters is exogenously varied between 28% and 88%. In addition, the number of donors needed to reach the goal is varied between 20/360 ("LO") and 2,000/36,000 ("HI") while keeping the level of progress constant. Slight variations in the wording ensure that all statements made are true (see subsection 1.3.4 for more details). To learn about potential interactions between progress and group size, a 2x2 factorial design is implemented where actual progress is varied along one dimension and the numerical information about the campaign goal and other donors is varied along the other dimension (Figure 1.2).

Social information   Letters in the *Social Info* treatments provide information about how many other donors have increased their regular donations in the past, but do not specify a campaign goal. The wording is identical to the corresponding wording in the *Therm* letters. To capture potential differences in the effects of different versions of the fundraising thermometer, I implement two different versions of *Social Info*: one version with a relatively low number of positive responses in the past (140) and one with a relatively high number (14,000) - *Info_LO* and *Info_HI*, respectively.

Figure 1.2: Main Experiment: Four different fundraising thermometer specifications

	LO Setting	HI Setting
Small Progress (28%)	Goal: 500 Social Info: 140	Goal: 50,000 Social Info: 14,000
Large Progress (88%)	Goal: 160 Social Info: 140	Goal: 16,000 Social Info: 14,000
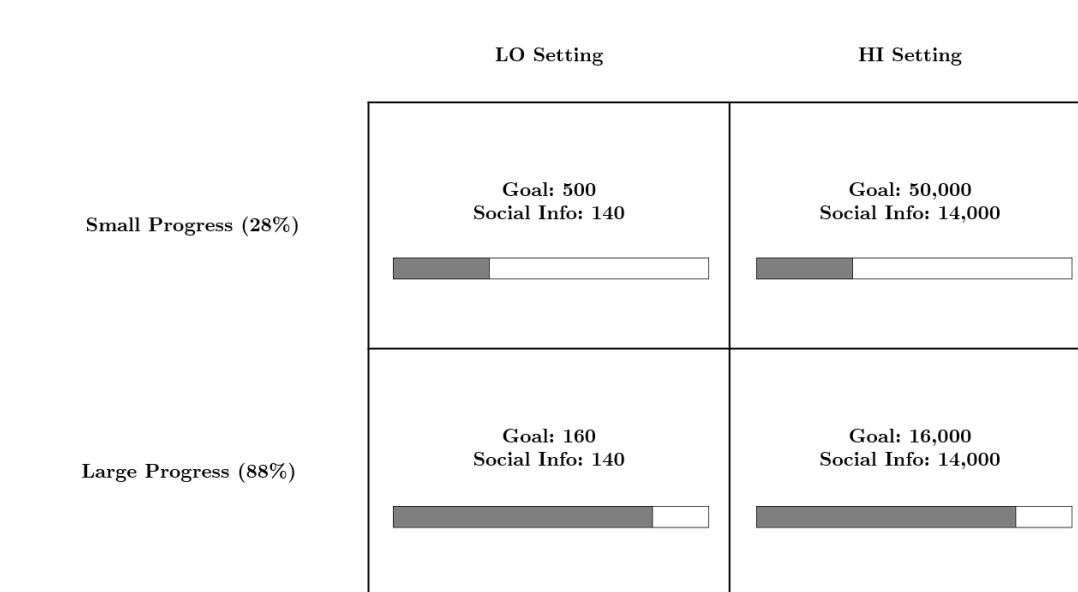
Campaign goal Letters in the *Goal* treatment communicate an explicit goal but no information about progress toward the goal. Again, the wording is identical to the wording in the corresponding *Therm* letters. To account for potential differences in the effects that differently ambitious campaign goals may have on behavior, I implement two letter versions within the *Goal* treatments: A letter version with a relatively ambitious goal (16,000) and a version with a relatively unambitious goal (500) - *Goal_HI* and *Goal_LO*, respectively.

1.3.3 Sample and randomization method

There are 109,945 participants in the main experiment (see appendix subsection 1.7.1 for more details on data preparation). On average, donors in the sample are 50% male, donate €166 per year to the partner organization, and have been a regular donor for 16 years. 43% of the individuals in my sample also received the request as an email two weeks after the solicitation letters were sent.[7] 22% of the donors were originally acquired through a face-to-face fundraising campaign, with the remainder acquired through other acquisition channels. Potentially relevant donor baseline characteristics were balanced ex ante by treatment assignment.[8] Balancing was done by first dividing the population into small strata according to five baseline variables based on demographics, prior donations, and contact history that were found to significantly affect the outcome variables in previous

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[7]The email was a digitized version of the solicitation letter, including all variable parts, and was sent to those donors who had provided an email address to the organization in the past.

[8]Appendix Table 1.9 and Table 1.10 present summaries of the available baseline data and show the effectiveness of the randomization method in generating treatment and control assignments orthogonal to the baseline information.

studies with the partner organization, and then randomizing treatment assignment within each stratum (Athey and Imbens, 2017).

1.3.4 Implementation

Figure 1.3 shows the layout of the letter and the wording of the last paragraph for each treatment. In consultation with the fundraising experts and based on data from the organization's database, I use slightly different wording between letter versions to allow for exogenous variation in the information provided while ensuring that all social information is truthful and all campaign goals are sufficiently realistic. In particular, within the *Therm* and *Social Info* conditions, I vary the wording with respect to the time period ("since the beginning of the year" vs. "to date") as well as with respect to the reference group ("recipients of this letter" vs. "helpers").

Figure 1.3: Treatment Implementation: Illustration of the letter design (left) and the variable component in the four different treatments (right).
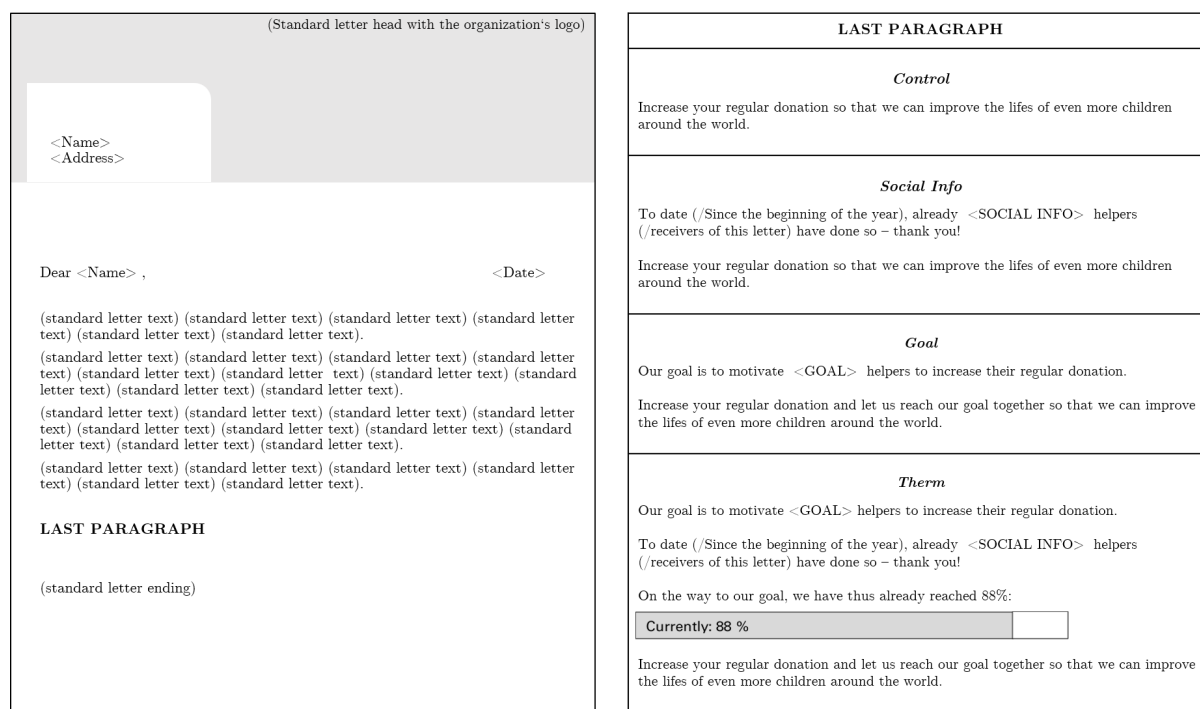
All letters were mailed on June 13, 2022. To ensure that donors receive the correct version of the letter according to their treatment assignment, the organization placed an individual order with the mailing service for each version of the letter. Each order was thus assigned to a specific mailing list, so I can confidently rule out the possibility that donors received versions of the letter to which they were not assigned. The cut-off date for the analysis was July 11, exactly four weeks after the mailing date.

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1.3.5 Long-term effects

To examine potential long-term effects of my interventions, the organization provided me with detailed individual-level data on donor responses to two subsequent direct mail solicitations, as well as upgrade and churn rates within six months of the intervention. Unlike the upgrade request, the two subsequent requests were one-time donation requests, typically sent in response to a specific recent event, such as a natural disaster or escalation of a war situation. The letters were sent three and six months after the treatment intervention (see Figure 1.4 for a timeline). The letters were designed by the organization's fundraising professionals and did not explicitly mention the upgrade request from the main experiment. Five months after the intervention, i.e., between the two subsequent solicitations, the donors in my sample received an information letter. This letter did not contain an explicit request for a donation. Instead, the letter provided information about the organization's activities, including the fact that 16,000 regular donors had decided to increase their regular giving in the past. Note that depending on the letter version assigned in the upgrade request of the main experiment, this information can be interpreted either as pure social information (by donors in the control group and donors in the *Social Info* treatments), as information that the campaign goal was met (by donors in the *Therm* and *Goal* treatments with goals less than or equal to 16,000), or as information that the campaign goal was *not* met (for donors in the *Therm* and *Goal* treatments with goals greater than 16,000).

Figure 1.4: Timeline of direct mail communication with donors in the main experiment in 2022.
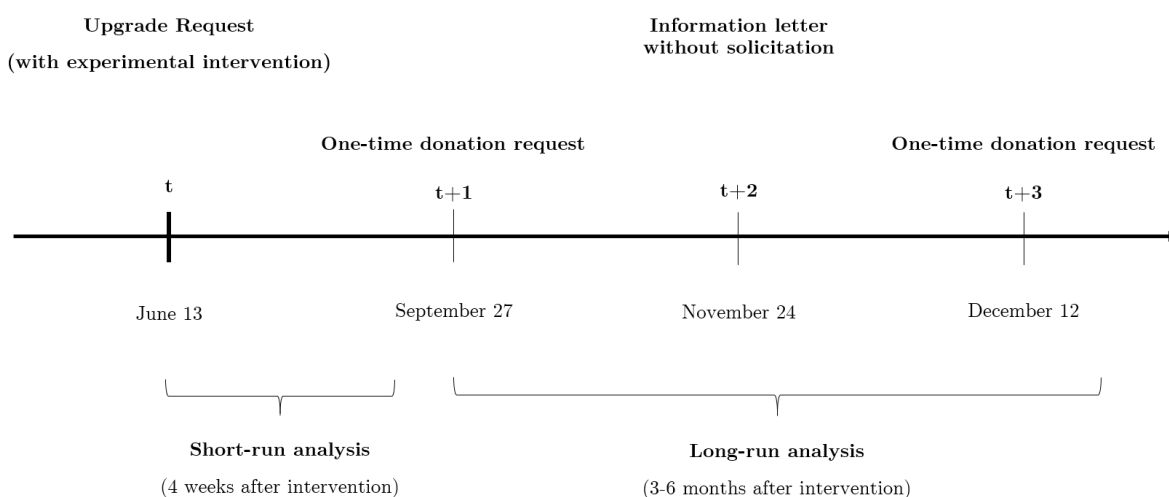
1.3.6 Follow-up Experiment

The purpose of the follow-up experiment with another group of regular donors is threefold: First, I attempt to conceptually replicate the effects of fundraising thermometers on giving with a different sample. The basic setting, randomization method, and implementation procedures of the follow-up experiment closely mirror those of the main experiment. The sample consists of 47,864 individuals who have been donating to the charity on a regular basis and have not yet received an upgrade solicitation in the current year. The main text of the solicitation letter is identical to the letters in the main experiment. Again, only the last paragraph varies across treatment conditions. The letters are mailed on August 10, 2022, and four weeks after the mailing date, the partner organization provided data for analysis.

Second, I use the follow-up experiment to further examine the role of the progress level for the effectiveness of fundraising thermometers. In particular, I compare the effects on giving of two extreme progress levels (9% and 91%) and one intermediate level (51%).

Third, an additional control condition sheds light on the role of the presence of an information graphic on giving. Donors assigned to the *Pseudo_Therm* treatment receive a version of the letter that at first glance looks identical to the *Therm* letters, as it also contains an information graphic in the form of a bar graph. However, in this version of the letter, the bar graph does not represent any kind of progress. Instead, the last paragraph informs donors of the WHO definition of when children are considered acutely malnourished. Accordingly, the bar graph illustrates "80 % of the appropriate weight for their age." Table 1.2 gives an overview of the five different letter versions.

Table 1.2: Follow-up Experiment: Overview of the five letter versions

			EXPERIMENTAL DESIGN OF THE FOLLOW-UP EXPERIMENT			
			Variable letter components			
	Treatment	Letter version	Social Information	Goal	Progress	n
1	*Control*	*Control*	No	No	No	9,555
2	*Infographic*	*Pseudo_Therm*	No	No	No (80%)	9,566
3	*Therm*	*Therm_HI_9*	8,200	90,000	9%	9,582
4		*Therm_HI_51*	8,200	16,000	51%	9,588
5		*Therm_HI_91*	8,200	9,000	91%	9,573
TOTAL						**47,864**

1.4   Results

The main analysis focuses on three outcome variables: First, the effect on total donations per individual is presented. Second, the effect on donation incidence is presented as

a variable that equals one if a positive donation decision is made and zero otherwise, allowing me to examine changes in donors' dichotomous decision between not donating and donating in response to the solicitation letter. Third, treatment effect estimates on conditional donation amounts are estimated by restricting the sample to individuals who make a donation and comparing donation amounts across groups.[9]

1.4.1 Descriptive statistics

Table 1.3 presents descriptive results of the main experiment. In total, the campaign raised more than €0.5 million per year in pledges. 3,963 donors (3.7 %) were persuaded to make an average donation of €129. There are large differences in response rates between groups: The average donation probability in the most successful treatments is 56% higher than in the least successful treatment (4.2% vs. 2.7%).[10] Figure 1.5 shows the average donation per individual by treatment condition. All four *Therm* treatments outperform the *Goal* treatments, while both *Goal* treatments outperform the two *Social Info* treatments. The average donations in the control group are the lowest. Within the *Therm* conditions, the small progress (28%) performs better than the large progress (88%).

Table 1.3: Main Experiment: Descriptive summary

	Treatments									TOTAL
	Control	Info_LO	Info_HI	Goal_HI	Goal_LO	Therm_HI_88	Therm_LO_88	Therm_LO_28	Therm_HI_28	
Solicitation letters	12,214	12,223	12,210	12,216	12,215	12,206	12,219	12,226	12,216	109,945
Number of donations	326	375	425	440	424	474	469	519	511	3,963
Response rate (%)	2.7	3.1	3.5	3.6	3.5	3.9	3.8	4.2	4.2	3.6
Mean cond. amount (€/year)	135	138	124	125	134	126	132	124	128	129
Donations raised (€/year)	44,010	51,750	52,700	55,000	56,816	59,724	61,908	64,356	65,408	511,672

1.4.2 Treatment Effects

In the following, econometric measures and tests of the statistical significance of the patterns described above are presented.[11] Figure 1.6 compares total donations per donor

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[9]Examining effects on average conditional donation amounts necessarily confounds two effects: my treatment manipulation may change the type of person who responds (attracting donors with higher and lower typical amounts differently), and it may change the conditional donation amount for individuals who would have decided to donate regardless of the letter version. The experimental design does not allow me to distinguish between these two potential effects.

[10]The numbers only include donors who *increased* their regular donation in response to the solicitation letter. However, there were also 21 donors who *decreased* their regular donation amount. Downgrades are spread across all treatment conditions and none of the documented results change when they are included in the analysis.

[11]I present results from Ordinary Least Squares (OLS) models with treatment dummies as independent variables in the main part of the paper. In the appendix, it is shown that the main results on total donations are robust to analyses based on two-part models and Randomization Inference, and that reported effects on the probability of giving are robust to Probit models (Table 1.11).

Figure 1.5: Main experiment: Total donations in response to the nine different letter versions
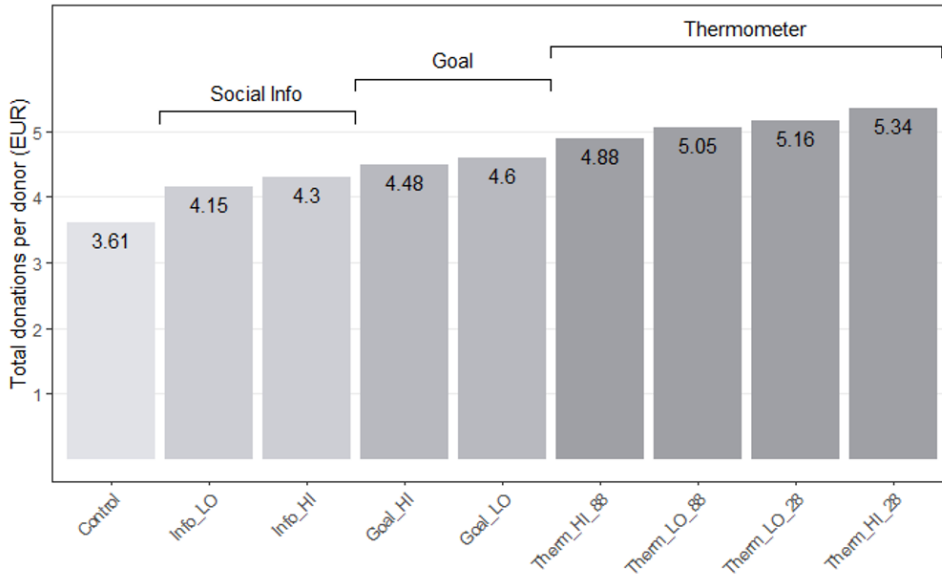
across the four treatment groups. The numbers in this figure are computed by pooling across letter versions with different specifications within each of the three treatment groups. Subjects in a *Therm* treatment donate significantly more than subjects in each of the other experimental conditions. The *Goal* treatment performs significantly better than *Control*, and the difference between *Social Info* and *Control* is weakly significant. Table 1.4 presents the results of linear regressions examining marginal effects of the three treatment interventions on giving. Compared to the control condition, fundraising thermometers increase total donations per letter by €1.50, which corresponds to a percentage increase in total revenue of 42%. The effect is driven by a highly significant 1.4 percentage point increase in the average probability of donating, corresponding to a 52% increase in the participation rate. The treatment effect on the average conditional donation amount of €135 tends to be negative, but is not significant. All treatment effects are robust to adding controls.

Result 1. *Compared to standard solicitations, fundraising thermometers have significant positive effects on giving rates and total donations raised.*

Providing information about the number of previous donors (without an explicit campaign goal) leads to a highly significant 0.6 percentage point increase in the probability of giving (+22%) and a weakly significant increase in total donations (+17%). Communicating a campaign goal leads to a 0.9 percentage point increase in the the probability of giving (+33%) and a 26% increase in total revenue per letter. Both are highly significant. The effect of both treatments on conditional donation amounts tends to be negative, but is not significant. The estimated differences between the *Therm* and *Social Info* treatment, as well as between the *Therm* and *Goal* treatment, in total donations and giving

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rate are significant. The effect of communicating a campaign goal on giving tends to be stronger than the effect of providing social information, but the difference is insignificant.

Result 2. *Merely providing social information and merely communicating an explicit campaign goal each have significant positive effects on donation rates. However, both strategies perform significantly weaker than the fundraising thermometer.*

Figure 1.6: Main experiment: Total donations by treatment group. Significance levels for pairwise t-tests: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$
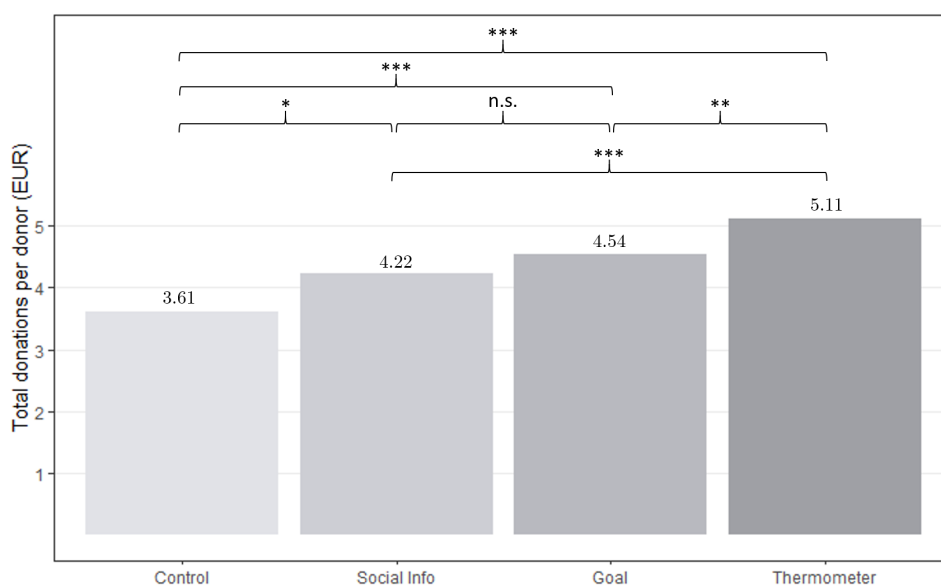
1.4.3 Comparison of different versions of fundraising thermometers

Within the set of *Therm* treatment conditions, the level of progress displayed and the number of donors needed to reach the goal are exogenously varied. Communicating a progress level of 88% instead of 28% results in a significant 0.4 percentage point decrease in the response rate (Table 1.6). However, due to a simultaneous insignificant increase in conditional donation amounts, the effects on total donations tend to be negative, but is not significant. Switching from the LO to the HI setting has no significant effects on donation decisions.[12]

Result 3. *Among the fundraising thermometer letters, a small progress level (28%) significantly increases response rates compared to a large progress level (88%). Neither the*

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[12]Among the *Info* and *Goal* letter versions, the data show no significant effects of changing the numbers (i.e., the social information provided and the campaign goal) on total donations. In the appendix, corresponding regression analyses for the subtreatment groups are provided (Table 1.12).

Table 1.4: Main experiment: Main treatment effects

	Dependent variable:					
	Total donations		Donation probability		Conditional amounts	
	(1)	(2)	(3)	(4)	(5)	(6)
Therm	1.502***	1.513***	0.014***	0.014***	−7.695	−9.577
	(0.306)	(0.305)	(0.002)	(0.002)	(7.158)	(6.668)
Social Info	0.615*	0.629*	0.006***	0.006***	−4.313	−3.045
	(0.327)	(0.326)	(0.002)	(0.002)	(7.552)	(7.022)
Goal	0.930***	0.937***	0.009***	0.009***	−5.534	−6.481
	(0.337)	(0.336)	(0.002)	(0.002)	(7.748)	(7.252)
Constant	3.608***		0.027***		135.196***	
	(0.267)		(0.001)		(6.776)	
Observations	109,945	109,945	109,945	109,945	3,963	3,963
Controls	no	yes	no	yes	no	yes
Test of differences between coefficients						
Therm vs. Social Info	0.887***	0.883***	0.008***	0.008***	−3.382	−6.803
Therm vs. Goal	0.572**	0.576**	0.005***	0.005***	−2.162	−3.107
Goal vs. Social Info	0.316	0.308	0.003	0.003	−1.221	−3.330

Coefficients represent marginal effects of the treatment interventions on giving (OLS). Robust standard errors in parentheses. Significance levels: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. The dependent variable in columns (1) and (2) is the amount donated in euros, in columns (3) and (4) a binary variable equal to one if a positive donation decision is made and zero otherwise, and in columns (5) and (6) the amount donated in euros conditional on a donation. The constant represents giving in the control group; the three reported independent variables are treatment dummies. Added controls in columns (2), (4), and (6) are the randomization strata variables: a gender dummy, a dummy for having received an email, previous gift size, years since first gift, and a dummy for a face-to-face acquisition channel. See Appendix Table 1.14 for regression results with all coefficients reported. The lower part of the table shows estimates and significance markers of the difference between reported regression coefficients. Robustness Tests: Appendix Table 1.11 presents regression results for total donations based on a two-part model, results for donation probability based on a probit model as well as standard, multiple hypotheses adjusted, and randomization inference based p-values for the six hypotheses.

Table 1.5: Main experiment: Effects of group size and progress level within the *Thermometer* treatments

	Total donations		Donation probability		Conditional amounts	
	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
HI setting	0.003	0.177	−0.000	−0.001	−0.333	4.801
	(0.295)	(0.422)	(0.002)	(0.003)	(4.619)	(6.279)
Large progress (88%)	−0.283	−0.109	−0.004**	−0.004	3.136	8.502
	(0.295)	(0.411)	(0.002)	(0.003)	(4.630)	(6.232)
HI setting		−0.348		0.001		−10.742
× Large progress (88%)		(0.590)		(0.004)		(9.260)
Constant	5.250***	5.164***	0.042***	0.042***	126.169***	123.621***
	(0.252)	(0.288)	(0.002)	(0.002)	(3.666)	(4.047)
Observations	48,867	48,867	48,867	48,867	1,973	1,973

Coefficients represent marginal effects of a variation in the number of donors needed to reach the goal and in the progress level on giving (OLS). Robust standard errors in parentheses. Significance levels: $*p < 0.1$; $**p < 0.05$; $***p < 0.01$. The dependent variable in columns (1) and (2) is the amount donated in euros, in columns (3) and (4) a binary variable equal to one if a positive donation decision is made and zero otherwise, and in columns (5) and (6) the amount donated in euros conditional on a donation. The constant represents giving in the treatment condition *Therm_LO_28*. "HI setting" is a dummy variable for changing the campaign goal from 500 (140) to 50,000 (14,000) and the number of previous donors from 140 to 14,000. "Large progress" is a dummy variable for changing the progress level from 28% to 88%.

progress level nor the number of donors needed to reach the goal has a significant effect on total donations raised.

1.4.4 Long-term effects

Long-term panel data provided by the organization for analysis eight months after the treatment interventions in the main experiment allow for the analysis of long-term treatment effects. A simple comparison of the control group and the three treatment groups shows no significant effects on long-term giving patterns (see Appendix Table 1.13). However, the data also allows for a more nuanced analysis and sheds light on how donors respond to information about whether or not the communicated campaign goal was met. Recall the timeline of direct mailings that donors in my sample receive over the course of 2022: Three months after the intervention (in t+1), all donors in my sample receive a one-time solicitation. Five months after the intervention (in t+2), all donors receive an information letter, followed by another one-time donation request (in t+3). While all donors received *the same* information letter stating that 16,000 donors increased their regular giving amount, different goals during the treatment intervention now correspond to an exogenous variation in campaign success.

Figure 1.7 shows that six months after the interventions in the main experiment,

Table 1.6: Main experiment: Effects of group size and progress level within the *Thermometer* treatments

	Dependent variable:	
	Total donations	
	(1)	(2)
HI setting	0.003	0.177
	(0.295)	(0.422)
Large progress (88%)	−0.283	−0.109
	(0.295)	(0.411)
HI setting		−0.348
x Large progress (88%)		(0.590)
Constant	5.250***	5.164***
	(0.252)	(0.288)
Observations	48,867	48,867

Figure 1.7: Main experiment: Long-run treatment effects. Donors received a one-time solicitation in period t+1. In t+2, donors were informed of the success or failure of the campaign from period t. After receiving this information, donors were sent another one-time donation request (t+3). Letter versions are clustered by whether the letter included a campaign goal and, if so, whether the campaign goal was met. Error bars represent standard errors of the mean.
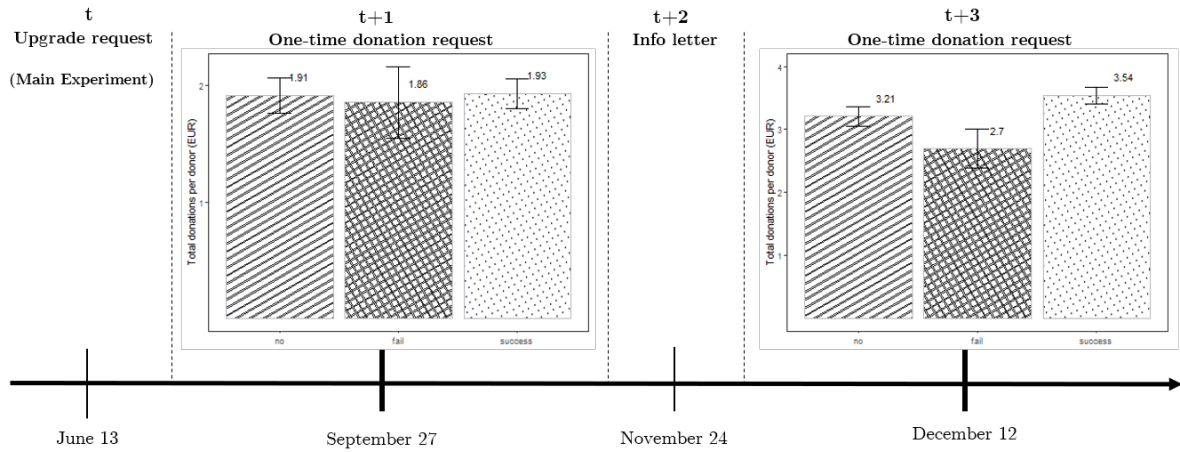
Table 1.7: Main experiment: Long-run treatment effects

	Dependent variable:	
	Total donations	
	Solicitation (t+1)	Solicitation (t+3)
	(1)	(2)
Goal	0.016	0.334
	(0.219)	(0.311)
Goal × Goal failed	−0.074	−0.841***
	(0.305)	(0.318)
Constant	1.913***	3.206***
	(0.113)	(0.219)
Observations	109,945	109,945

 Coefficients represent marginal effects of communicating a campaign goal that is met ("Goal") on total donations and the difference in the effect when the goal is not met ("Goal × Goal failed") (OLS). Information is provided in t+2. Robust standard errors in parentheses. Significance levels: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. The constant represents giving in treatments without a goal. "Goal failed" is a dummy variable for Goal > 16,000.

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donors assigned to a successful campaign give significantly more in a one-time solicitation than donors assigned to an unsuccessful campaign. This difference exists only after donors are informed of the success or failure of the campaign (in t+3), but not before (in t+1). Regression results confirm this finding: The interaction effect in column 2 of Table 1.7 shows that donors assigned to a successful campaign give €0.84 more than donors assigned to an unsuccessful campaign, an increase of 28%. However, before learning of the campaign's success, this highly significant difference does not exist (column 1).

Result 4. *Six months after the intervention in the main experiment, informing donors that the campaign was successful leads to significantly more donations in a one-time solicitation than informing donors that the campaign goal was not met.*

1.4.5 Follow-up Experiment

Figure 1.8 illustrates total donations by treatment group in the follow-up experiment. The descriptive patterns are the same as in the main experiment: total donations in all of the three *Therm* treatments are larger than in the control group. Among the *Therm* letter versions, it appears that the medium progress condition (51%) performs better than the two more extreme progress levels (9% and 91%). Total giving in the *Pseudo_Therm* treatment is not different from the control.

Table 1.8 confirms the first impression regarding the positive effects of the fundraising thermometer. Columns (1), (3), and (5) show that the tool has highly significant positive effects on donations that are very similar in magnitude to those in the main experiment: Compared to the control condition, total donations per letter increase by €1.54, which corresponds to a percentage increase in total revenue of 40%. The average probability of giving increases significantly by 1.4 percentage points, corresponding to a 52% increase in participation rates. The effects on average conditional giving are again negative, but small and not statistically significant. The *Pseudo_Therm* treatment has no significant effect on giving compared to giving in the control group. The models in columns (2), (4), and (6) include interaction terms with the extreme progress levels and show that changing the progress level again does not significantly change the effects of the tool on total giving. If anything, more extreme levels of progress seem to reduce the treatment effect relative to the intermediate progress level - but the reported effects are not statistically significant.

Figure 1.8: Follow-up experiment: Total donations in response to the five different letter versions
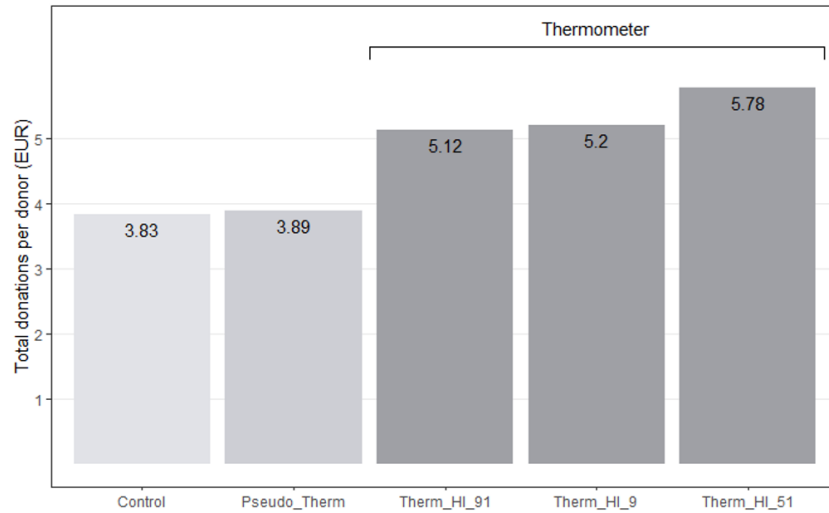
Table 1.8: Follow-up experiment: Main Treatment effects and effects of the progress level on giving.

	Dependent variable:					
	Total Donations		Donation Probability		Conditional Amounts	
	(1)	(2)	(3)	(4)	(5)	(6)
Thermometer	1.542***	1.955***	0.014***	0.017***	−8.541	−8.630
	(0.387)	(0.483)	(0.002)	(0.003)	(8.878)	(9.685)
Thermometer x Small Progress (9%)		−0.583		−0.004		−1.002
		(0.513)		(0.003)		(8.179)
Thermometer x Large Progress (91%)		−0.659		−0.005*		1.328
		(0.522)		(0.003)		(8.782)
Pseudo Thermometer	0.059	0.059	0.003	0.003	−10.838	−10.838
	(0.427)	(0.427)	(0.002)	(0.002)	(9.605)	(9.610)
Constant	3.827***	3.827***	0.027***	0.027***	139.851***	139.851***
	(0.323)	(0.323)	(0.002)	(0.002)	(8.135)	(8.140)
Observations	47,864	47,864	47,864	47,864	1,738	1,738

Coefficients represent marginal effects of the treatment interventions on giving (OLS). Robust standard errors in parentheses. Significance levels: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. The dependent variable in columns (1) and (2) is the amount donated in euros, in columns (3) and (4) a binary variable equal to one if a positive donation decision is made and zero otherwise, and in columns (5) and (6) the amount donated in euros conditional on a donation. The constant represents giving in the control group. "Small Progress" ("Large Progress") is a dummy variable for changing from an intermediate level of progress (51%) to a small (large) progress level. "Pseudo Thermometer" is a treatment dummy for the *"Pseudo_Therm"* condition. See Appendix Table 1.15 for regression results with controls.

1.5 Discussion

The results of this study show that fundraising thermometers can have large positive effects on campaign success. The effects are insensitive to variations in actual progress and the number of donors needed to reach the campaign goal and cannot be explained by the mere presence of a graphic illustration. The analyses also show that neither the social information nor the explicit campaign goal alone are sufficient to explain the large treatment effects. Instead, the combination of the two creates a progress framing that may explain the effects of fundraising thermometers on giving.

1.5.1 Progress framing increases giving

All fundraising thermometers in this study display some level of campaign progress. As a consequence, potential donors make contributions they otherwise would not have made. This finding aligns with work in psychology and consumer research showing that people tend to invest more effort in unfinished tasks than in those that have not yet begun. Empirical studies on this phenomenon have focused on marketing tools like coffee or car wash rewards cards that come pre-stamped with some progress (Nunes and Dreze, 2006; Kivetz et al., 2006). The key takeaway from this research is that showing individuals some level of progress toward a goal significantly boosts their motivation to complete the task, even when the progress framing doesn't alter the remaining effort needed to reach the goal.

A common explanation for this behavior is the Zeigarnik effect, which refers to the tendency to remember interrupted or unfinished tasks better than others (Zeigarnik, 1938). Psychologists suggest that leaving tasks unfinished creates psychological tension, driving people to resume and complete them, even when doing so may not seem rational (Rickers-Ovsiankina, 1928; Lewin, 1928). This power of progress framing can extend beyond personal goals and stamp cards to charitable giving: donors are more inclined to contribute to a campaign that is underway than to one that hasn't started yet. Framing a donation request as part of an ongoing campaign, rather than a stand-alone appeal, can leverage this effect. Fundraising thermometers may trigger an "unfinishedness" cue, encouraging donors to help move the campaign closer to its finish line.

1.5.2 Progress level has no effect on giving

In light of previous work on unconditional leadership gifts in capital campaigns, it may seem puzzling why the presented analyses reveal no positive effects of goal proximity on giving. In a seminal study, List and Lucking-Reiley (2002) found that increasing seed money from 10 to 67 of a campaign goal significantly boosted subsequent contributions.

However, the experimental data here indicate that a high progress level (88%) actually led to a significantly *lower* donation rate compared to a smaller progress level (28%). While this effect did not appear in the follow-up experiment, none of the analyses showed a positive relationship between increasing campaign progress and giving. Two explanations may account for this discrepancy.

First, in List and Lucking-Reiley (2002), the charitable project was new to potential donors, and the size of the lead gift may have served as a quality signal for the project's prospects-especially since the seed money came from the project's host institution. This likely influenced donors' perceptions of the project's success and thus their willingness to contribute. In contrast, the current study involves donors who have been regularly supporting the organization for several years, making it likely they are already very familiar with the organization and its projects. As a result, there is little scope for learning from campaign progress about charity quality, which may explain why progress levels did not affect donations in this experiment. Second, the fundraising thermometers in this study provide social information and define the goal in terms of the total number of donors, creating a situation where an individual's marginal contribution to the campaign goal is censored to either 1 (contribute) or 0 (not contribute). In contrast, List and Lucking-Reiley (2002) focused on a capital campaign where goals and social information were framed in terms of money raised. This difference in design changes the decision problem. The ability to decide not only whether but also how much to contribute in a campaign already close to its goal might encourage donors to give in ways they wouldn't in an early-stage campaign.

1.5.3 Social information and conformity

The analysis presented in subsection 1.4.2 shows that solicitation letters providing social information (without an explicit campaign goal) result in significantly more donations than standard letters. Several mechanisms may explain how information about others' behavior influences prosocial decisions, with two of the most discussed in charitable giving being social learning based on quality signals (Vesterlund, 2003) and social conformity (Bernheim, 1994; Akerlof, 1997).

In the context of charitable giving, social learning occurs when potential donors infer information about a charity's trustworthiness and quality from the behavior of their peers, especially when acquiring all decision-relevant information is too costly. This is particularly plausible when the charity is new to donors or when an established charity is launching a new project (Vesterlund, 2003). However, as discussed in subsection 1.5.2, the environment of this study leaves little room for quality signaling. The fact that participants have been regular donors for years and have received numerous solicitations

and materials makes it unlikely that they gain new insights about the organization's quality by observing the upgrade behavior of other regular donors.[13]

Conformity is another potential mechanism explaining the impact of social information. In charitable giving, Smith et al. (2015) find substantial peer effects on online fundraising platforms, with little evidence that donations signal charity quality. Similarly, Goette and Tripodi (2021) show that social influence plays a role when beliefs about charity quality are held constant and social learning is unlikely to be a driving factor. Instead, donors may use information about others' past decisions to determine what is socially appropriate for them and align their behavior with perceived social norms (Akerlof, 1980; Bernheim, 1994; Ostrom, 2000a). In the current study, participants may be uncertain about their moral obligations and use social information to assess whether they should increase their regular giving or determine what the appropriate amount might be (Shang and Croson, 2009; Sasaki, 2019).

1.5.4 Campaign goal motivates giving

Data from the main experiment show that communicating a campaign goal defined by a specific number of gifts-without mentioning progress—has an immediate, significant positive effect on donations. Interestingly, the size of the goal does not affect this short-term treatment effect. In psychology, research suggests that setting specific goals increases motivation for costly actions by providing a benchmark for satisfaction (Locke and Latham, 1990; 2006). Most existing studies focus on personal goals, where individuals decide how much effort to invest, typically receiving rewards only upon success. However, the context in this study differs since the goal is collective, and each person's contribution is fixed. The results here indicate that even in such a group setting, goals can motivate behavior.

Effectively managing regular donors requires looking beyond short-term responses to solicitations. This study also finds that donors assigned to campaigns that met their goals contributed significantly more in a future solicitation than those assigned to campaigns that fell short. Crucially, this difference in generosity only emerges after donors are informed of the campaign's success or failure, not before, confirming that this information drives the change. For fundraising campaign designers, two key implications arise: first, setting realistic campaign goals that are likely to be achieved fosters long-term donor engagement. Second, informing donors when a campaign has reached its goal is a valuable strategy for maintaining donor relationships throughout the donor lifecycle.

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[13]AIn addition, a social learning mechanism would have led us to expect that the treatment effect should be stronger for "younger" donors - those who have recently become regular donors and are still learning about the quality of the charity. However, a heterogeneity analysis of the social information effect, based on subsamples binned by years since first gift, shows the opposite pattern (see Appendix subsection 1.7.7 for details).

1.6 Conclusion

This paper presents the findings of two direct mail fundraising experiments conducted with donors from a large charitable organization. Solicitations that included a fundraising thermometer raised significantly more money than standard solicitations. Neither the level of progress displayed nor the number of donors needed to reach the goal significantly influenced the effect size. Providing only social information or communicating only a fundraising goal also led to notable increases in giving, but both were less effective than using a fundraising thermometer. Taken together, this study offers strong evidence of the effectiveness of fundraising thermometers and provides insights into how information about peer behavior can be leveraged to boost individual giving.

The treatment effects observed are both substantial and robust, and they were replicated with a different sample. However, the precise behavioral mechanisms behind these effects remain less straightforward. The explanation proposed here suggests that fundraising thermometers help frame a campaign as already underway, which may motivate donors to give. Future research with more controlled experimental designs could clarify the psychological mechanisms underlying this effect. In addition, future studies might explore the role of how campaign goals are defined and the type of social information-whether number of gifts, gift sizes, or total funds raised-for their effects on giving behavior.

Beyond the immediate effects, this study highlights the long-term impact of behavioral interventions on future solicitations. The results suggest that donors remember the information from a solicitation letter for several months, opening the door to long-term fundraising strategies. However, the long-term effects of interventions and intertemporal spillovers in giving are not well understood. A systematic investigation of the dynamics of charitable giving, as well as the circumstances under which fundraisers should consider trade-offs between short-term campaign successes and long-term benefits from donor relationship management, will be of great value to both scholars and practitioners.

1.7 Appendix

1.7.1 Data preparation

During the planning phase of this project, the partner organization provided me with data on 157,977 regular donors. Anticipating that a follow-up experiment would be valuable for further insights, I randomly divided the group into a large sample for a well-powered main experiment (110,000) and a smaller sample for the follow-up experiment (47,977). In order to be able to make truthful statements about the exact number of donors who had already increased their regular donation amount in the current year (for the treatment variants *Info_LO*, *Therm_LO_88*, and *Therm_LO_28*), I manually added 125 donors to the sample, randomly selected from a list of 2,685 donors who had already increased their regular donation amount in 2022 at that time. I then conducted the treatment assignment according to the randomization procedure described above. For various organizational reasons, the final mailing list was reduced by 180 donors who were removed from the list between randomization and mailing, either by the agency (e.g., because the address was no longer valid) or by the organization's fundraising professionals (e.g., because they had asked the organization to stop sending them solicitations in the meantime). The data preparation and treatment assignment for the follow-up experiment followed the same procedure.

1.7.2 Summary statistics with coviariance balance check

Table 1.9 and Table 1.10 present summaries of the available baseline data. On average, donors in the sample are 50% male, donate €166 (€165 in the follow-up experiment) per year to the partner organization, and have been a regular donor for 16 years. 43% (44% in the follow-up experiment) of the individuals in my sample (those donors who had provided an email address to the organization in the past) received an additional email two weeks after the solicitation letters were sent. The email was a digitized version of the solicitation letter, including all variable parts. 22% of the donors were originally acquired through face-to-face fundraising, with the remainder acquired through other acquisition channels. Column 11 in Table 1.9 (column 7 in Table 1.10) shows the effectiveness of the randomization method in generating treatment and control assignments orthogonal to the baseline information. F-tests for each of the baseline variables show that all potentially important covariates are balanced across treatment variations.

Table 1.9: Main Experiment: Summary statistics with covariates balance check

	Summary Statistics										
Treatment	*Control*	*Social Info*		*Goal*		*Thermometer*					
Letter Version	*Control*	*Info_LO*	*Info_HI*	*Goal_HI*	*Goal_LO*	*Therm_HI_88*	*Therm_LO_88*	*Therm_LO_28*	*Therm_HI_28*	Full Sample	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Baseline variables											
Female (*binary*)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	1
Email (*binary*)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	0.43 (0.50)	1
Previous gift size (€)	166 (145)	165 (134)	166 (144)	165 (138)	166 (144)	164 (136)	166 (142)	167 (146)	165 (140)	166 (141)	0.903
Years since first gift (*years*)	16.39 (9.15)	16.33 (9.10)	16.33 (9.13)	16.37 (9.12)	16.40 (9.14)	16.35 (9.16)	16.38 (9.13)	16.33 (9.13)	16.35 (9.14)	16.36 (9.13)	0.999
Acquired via F2F (*binary*)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	1
N	12,214	12,223	12,210	12,216	12,215	12,206	12,219	12,226	12,216	109,945	

The numbers represent baseline values of strata variables (means) and standard deviations in parentheses for each treatment cell (columns 1-9). Column 10 reports baseline data for the whole sample. Column 11 reports the p-values of joint tests that the mean levels of each baseline variable are the same for all treatment cells. "Female": gender dummy; "Email": dummy for having received an email after direct mail; "Previous gift size": previous regular annual donation amount; "Age": years since first donation to the organization; "Acquired via F2F": acquisition channel dummy.

Table 1.10: Follow-up Experiment: Summary statistics with covariates balance check

	Summary Statistics						
Treatment	*Control*		*Thermometer*				
Letter Version	*Control*	*Pseudo_Therm*	*Therm_HI_91*	*Therm_HI_9*	*Therm_HI_51*	Full Sample	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline variables							
Female (*binary*)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	1
Email (*binary*)	0.44 (0.50)	0.44 (0.50)	0.44 (0.50)	0.43 (0.50)	0.44 (0.50)	0.44 (0.50)	1
Previous gift size (€)	166 (140)	165 (132)	164 (129)	164 (135)	164 (135)	165 (134)	0.876
Years since first gift (*years*)	16 (9.2)	16 (9.1)	16 (9.1)	16 (9.1)	16 (9.2)	16 (9.1)	0.987
Acquired via F2F	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	0.22 (0.41)	1
N	9,555	9,566	9,573	9,582	9,588	47,864	

The numbers represent baseline values of strata variables (means) and standard deviations in parentheses for each treatment cell (columns 1-5). Column 6 reports baseline data for the whole sample. Column 7 reports the p-values of joint tests that the mean levels of each baseline variable are the same for all treatment cells. "Female": gender dummy; "Email": dummy for having received an email after direct mail; "Previous gift size": previous regular annual donation amount; "Years since first gift": years since first donation to the organization; "Acquired via F2F": acquisition channel dummy.

1.7.3 Robustness of main result

Table 1.11 shows the robustness of the main results presented in Table 1.4 by presenting treatment effects on total donations using a two-part model (Belotti et al. (2015)) and on donation rates using a probit regression. The lower part of the table presents p-values from the effects presented in Table 1.4, as well as multiple hypothesis adjusted p-values and p-values from randomization inference.

Table 1.11: Main experiment: Robustness checks

	Dependent variable:			
	Total donations		Donation probability	
	(1)	(2)	(3)	(4)
Thermometer	1.566***	1.462***	1.185***	1.187***
	(0.350)	(0.341)	(0.026)	(0.026)
Social Info	0.684*	0.705*	0.090***	0.093***
	(0.385)	(0.375)	(0.028)	(0.029)
Goal	1.013***	0.943**	1.125***	1.125***
	(0.381)	(0.371)	(0.028)	(0.028)
Model	Two-PM	Two-PM	Probit	Probit
Controls	no	yes	no	yes
Observations	109,945	109,945	109,945	109,945

P-Values (Standard/Multiple hypotheses adj./Randomization Inference)

H_0 (Thermometer = Control)	$<.001$ / $<.001^M$ / $<.001^R$
H_0 (Social Info = Control)	$.059$ / $.119^M$ / $.038^R$
H_0 (Goal = Control)	$.007$ / $.029^M$ / $.004^R$
H_0 (Thermometer = Social Info)	$<.001$ / $.002^M$ / $<.001^R$
H_0 (Thermometer = Goal)	$.024$ / $.073^M$ / $.020^R$
H_0 (Goal = Social Info)	$.256$ / $.256^M$ / $.300^R$

Two-part regression models for zero-inflated data in columns 1 and 2 construct marginal effects by combining two separate estimations: The first model is a probit model for indicators about any non-zero responses. The second model is a OLS regression model for non-zero response values. P-values in lower part of the table stem from pairwise comparisons of mean total donations. Multiple hypotheses adjusted p-values, denoted by ^{M}, are constructed using the Bonferroni-Holm method for six hypotheses (Holm, 1979). Randomization inference p-values, denoted by ^{R}, stem from 500 repetitions of hypothetical assignment in which the number of donors assigned to each treatment remains identical to the number in the actual experiment.

1.7.4 Subtreatment analysis among *Social Info* and among *Goal* letter versions

Table 1.12 presents the results of a comparison between the two letter variations within the set of *Social Info* (panel A) and within the set of *Goal* (panel B) conditions. The data show positive effects of providing information that a relatively large number of other individuals have already donated compared to providing information about a relatively small number of other donors. However, this effect is only weakly significant. Again, the effects on conditional donation amounts tend in the opposite direction, leading to insignificant effects on total donations. Panel B shows that there are no significant effects of the ambitiousness of the campaign goal.

Table 1.12: Main experiment: Effects of the information given within the control treatments

	Dependent variable:		
	Total Donations	Donation Probability	Conditional Amounts
Panel (A): *Social Info* **Treatments**	(1)	(2)	(3)
Large info (14,000)	0.155 (0.375)	0.004* (0.002)	−13.709** (6.801)
Constant (140)	4.146*** (0.282)	0.031*** (0.002)	138.166*** (5.624)
Observations	24,433	24,433	800
Panel (B): *Goal* **Treatments**	(4)	(5)	(6)
Large goal (16,000)	−0.115 (0.410)	0.001 (0.002)	−9.310 (7.577)
Constant(500)	4.597*** (0.320)	0.035*** (0.002)	134.404*** (6.467)
Observations	24,431	24,431	864

Coefficients represent marginal effects of a variation in the specific information on giving within *Social Info* treatments (Panel A) and within *Goal* treatments (Panel B) (OLS). Robust standard errors in parentheses. Significance levels: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. The dependent variable in column (1) is the amount donated in euros, in column (2) a binary variable equal to one if a positive donation decision is made and zero otherwise, and in column (3) the amount donated in euros conditional on a donation. In panel A, the constant represents the treatment condition *Info_LO*. "Large info" is a dummy variable for changing the number of previous donors from 140 to 14,000. In panel B, the constant represents the treatment condition *Goal_LO*. "Large goal" is a dummy variable for changing the campaign goal from 500 to 16,000.

1.7.5 Longterm effects of the different letter versions

Table 1.13 shows the long-run effects of the treatment interventions. The data show that none of the three treatments have significant long-term effects on giving in either of the next two solicitations, on upgrade behavior beyond 4 weeks after the intervention, and on cancellation rates.

Table 1.13: Main Experiment: Long-run treatment effects in 2022

| | Dependent variable: | | | |
| | Solicitation (t+1) | Solicitation (t+3) | Longrun Upgrade | Longrun Cancel |
	(1)	(2)	(3)	(4)
Thermometer	0.220	0.500	0.207	0.000
	(0.287)	(0.379)	(0.353)	(0.002)
Social Info	0.218	0.481	0.051	0.000
	(0.226)	(0.405)	(0.433)	(0.002)
Goal	0.001	0.536	−0.281	−0.000
	(0.237)	(0.347)	(0.341)	(0.002)
Constant	1.766***	3.097***	4.190***	0.025***
	(0.191)	(0.330)	(0.352)	(0.002)
Observations	109,945	109,945	109,945	109,945

Coefficients represent marginal effects of the treatment interventions on long-term giving (OLS). Robust standard errors in parentheses. Significance levels: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. The three independent variables are treatment dummies from the intervention in the main experiment in June. Outcome variables in columns 1 and 2 are total donations (in €) in response to a one-time donation request in September and December, respectively. Outcome variables in columns 3 and 4 are the long-run total upgrade amounts and cancellation probabilities, respectively, within 5 months after data provision for the main analysis (July 11 - December 30). All models include the following strata variables as unreported controls: "Female" (gender dummy), "Email" (dummy for having received an email), "Annual donation" (previous gift size), "Age" (years since first gift), and "F2F" (dummy for acquisition channel). The variables "Annual donation" and "Age" are standardized to receive a standard normal random variable.

1.7.6 Exploratory covariate-effect analysis

Adding the randomization strata variables as covariates to the regression reveals some significant correlations that I did not hypothesize, but which may be of interest to future work and fundraising managers (Table 1.14 and Table 1.15): In both experiments, women are more likely to give than men, and higher prior giving predicts both a higher likelihood of giving and larger gifts. In addition, donors who have been giving regularly for a long time seem to be more likely to give than *younger* donors. Donors who were originally acquired through a face-to-face campaign are much less likely to donate than donors who were acquired through another channel or who decided to become regular donors on their own. In the follow-up experiment, donors who provided an email address to the organization and who received an additional digital version of the donation request are more likely to donate. However, this last effect is not found in the main experiment.

Table 1.14: Main experiment: Main treatment effects with reported controls.

	Total donations		Donation probability		Conditional amounts	
	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
Thermometer	1.502***	1.513***	0.014***	0.014***	−7.695	−9.577
	(0.306)	(0.305)	(0.002)	(0.002)	(7.158)	(6.668)
Social Info	0.615*	0.629*	0.006***	0.006***	−4.313	−3.045
	(0.327)	(0.326)	(0.002)	(0.002)	(7.552)	(7.022)
Goal	0.930***	0.937***	0.009***	0.009***	−5.534	−6.481
	(0.337)	(0.336)	(0.002)	(0.002)	(7.748)	(7.252)
Female		0.277		0.002**		0.168
		(0.189)		(0.001)		(3.115)
Previous gift size		2.146***		0.006***		2.320***
		(0.286)		(0.001)		(0.191)
Email		0.179		0.002		0.844
		(0.214)		(0.001)		(3.277)
Years since first gift		0.789***		0.007***		−3.279
		(0.136)		(0.001)		(2.240)
F2F		−2.698***		−0.021***		−10.564*
		(0.240)		(0.001)		(5.703)
Constant	3.608***	3.965***	0.027***	0.029***	135.196***	131.706***
	(0.267)	(0.302)	(0.001)	(0.002)	(6.776)	(6.607)
Observations	109,945	109,945	109,945	109,945	3,963	3,963
Controls	no	yes	no	yes	no	yes

Coefficients represent marginal effects of the treatment interventions on giving (OLS). Robust standard errors in parentheses. Significance levels: $*p < 0.1$; $**p < 0.05$; $***p < 0.01$. The dependent variable in columns (1) and (2) is the amount donated in euros, in columns (3) and (4) a binary variable equal to one if a positive donation decision is made and zero otherwise, and in columns (5) and (6) the amount donated in euros conditional on a donation. The constant represents giving in the control group. Added controls in columns (2), (4), and (6) are the randomization strata variables: a gender dummy, a dummy for having received an email, previous gift size, years since first gift, and a dummy for acquisition channel. The variables "previous gift size" and "years since first gift" are standardized to receive a standard normal random variable.

Table 1.15: Follow-up Experiment: Main treatment effects with reported controls

	Total donations		Donation probability		Conditional amounts	
	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
Thermometer	1.542***	1.572***	0.014***	0.014***	−8.541	−7.405
	(0.387)	(0.386)	(0.002)	(0.002)	(8.878)	(8.778)
Pseudo Thermometer	0.059	0.084	0.003	0.003	−10.838	−9.688
	(0.427)	(0.426)	(0.002)	(0.002)	(9.605)	(9.310)
Female		0.521*		0.006***		−8.893
		(0.311)		(0.002)		(5.700)
Previous gift size		2.425***		0.007***		29.575***
		(0.319)		(0.001)		(4.602)
Email		0.939***		0.006***		3.132
		(0.350)		(0.002)		(5.902)
Years since first gift		1.394***		0.010***		−0.864
		(0.215)		(0.001)		(3.432)
F2F		−2.135***		−0.018***		−6.712
		(0.364)		(0.002)		(9.094)
Constant	3.827***	3.598***	0.027***	0.026***	139.851***	137.078***
	(0.323)	(0.426)	(0.002)	(0.002)	(8.135)	(10.451)
Observations	47,864	47,864	47,864	47,864	1,738	1,738

Coefficients represent marginal effects of the treatment interventions on giving (OLS). Robust standard errors in parentheses. Significance levels: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. The dependent variable in columns (1) and (2) is the amount donated in euros, in columns (3) and (4) a binary variable equal to one if a positive donation decision is made and zero otherwise, and in columns (5) and (6) the amount donated in euros conditional on a donation. The constant represents giving in the control group. Added controls in columns (2), (4), and (6) are the randomization strata variables: a gender dummy, a dummy for having received an email, previous gift size, years since first gift, and a dummy for acquisition channel. The variables "previous gift size" and "years since first gift" are standardized to receive a standard normal random variable.

1.7.7 Heterogeneous treatment effects of social information by years since first gift

Table 1.16 shows a heterogeneity analysis of the marginal effects of the *Social Info* treatment on the probability of giving compared to the *Control* treatment. Subjects are binned according to how long they have been regular donors to the charity. A comparison of the coefficients between the three models shows that the treatment effect is significant only for those donors who have been regular donors for more than 21 years.

Table 1.16: Main Experiment: Treatment effects of social information on donation probability by years since first gift ("Age"), binned at 33 and 66 percentiles

	Dependent variable:		
	Donation Probabilty		
	(1)	(2)	(3)
Social Info	0.0003	0.005	0.013***
	(0.003)	(0.004)	(0.004)
Constant	0.018***	0.032***	0.031***
	(0.002)	(0.003)	(0.003)
Observations	12,790	11,721	12,136
Age bin	young	medium	old
	(< 12 years)	(12-21)	(21 $<$)

Coefficients represent marginal effects of the treatment intervention *Social Info* on donation probability (OLS). Robust standard errors in parentheses. Significance levels: $*p < 0.1$; $**p < 0.05$; $***p < 0.01$. The constant represents giving in the control group. "Age" denotes years since first gift.

Chapter 2

Norms and Elections: How elected rules can make behavior (in)appropriate

Note: This chapter was written jointly with Arno Apffelstaedt and Jana Freundt. A version was published in the Journal of Economic Behavior & Organization (Volume 196, April 2022, pages 148-177).[1] All three authors contributed to the design and implementation of the research, analysis of the results, and writing of the manuscript.

2.1 Introduction

Can elections shift people's ideas of what is ethically right and what is wrong? A number of recent observations suggest that social norms, often considered persistent long-standing social constructs, can change rapidly as a result of election outcomes. In 2016, shortly after the United Kingdom voted for Brexit, the country experienced a sharp rise in hate crime, which many observers attribute to a Brexit-induced increase in the social acceptability of xenophobic views and actions. As a result of the referendum, "anti-immigrant and anti-foreigner rhetoric had become 'normalised' ", making Britain effectively "a more racist country", the United Nations claim.[2] Similar claims were made after the election of Donald Trump as president of the United States that same year.[3] There are also

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[1] `https://doi.org/10.1016/j.jebo.2022.01.031`; `https://creativecommons.org/licenses/by/4.0/`

[2] See Brown, D. and Coates, S., "UK 'more racist after Brexit''', *The Times*, May 12, 2018. Available at: `https://www.thetimes.co.uk/article/uk-more-racist-after-brexit-qb7hd7xl7` (accessed February 18, 2021).

[3] Many popular press articles reported an increase in racially motivated violence and sexism after Trump's election, the cause of which was attributed to a change in social norms. The election of a person who is "openly hostile to women [...] normalizes abusive behavior and gives implicit permission

examples where elections appear to have lead to more tolerant norms: Baskaran and Hessami (2018) and Kedia and Pareek (2020) observe that electoral successes of female candidates in Germany and the US, respectively, seem to have improved norms regarding the treatment of women and reduced bias against them in elections as well as in the workplace. Jung and Tavits (2021) document an increase in the social acceptability of abortion following a pro-abortion vote in the Irish referendum of 2018. If the general conjecture that election outcomes can influence norms is true, it most likely holds for other domains as well, be it norms regarding same-sex relationships (Aksoy et al., 2020), compliance with Covid-19 regulations (Galbiati et al., 2021), or everyday behavior such as alcohol consumption (Lane et al., 2021).

A fundamental challenge in interpreting the relationship between norms and election outcomes is that elections are generally not exogenous to the society in which social norms develop. Observed differences in behavior can therefore also be attributed to other (possibly unobservable) factors that correlate with election outcomes, making it difficult to prove causality. Moreover, the fact that in the field, norms can typically only be observed indirectly from behavior obstructs the separation of presumed norm changes from changes in personal moral views or behavioral preferences. In this paper, we address these challenges and provide clean causal evidence on the effect of elections on social norms using a simple experiment.

Our experimental approach is based on the Krupka-Weber method (Krupka and Weber, 2013), which elicits social norms by asking subjects to estimate the degree of social approval of different actions in a hypothetical choice situation. Subjects are incentivized to provide a rating that is identical with the most common rating in their session, making the elicited social approval a direct elicitation of social norms or "normative expectations". We apply this method to investigate how social norms concerning prosocial vs. selfish behavior are affected by the outcome of majority elections. We speculate that focal points in the coordination game might be created via "cultural values" such as prosociality, obedience to societal rules or the importance of democratic principles. A major benefit of using the Krupka-Weber method is that we elicit social norms directly using social approval ratings instead of indirectly using behavior. We thereby complement existing studies that also target election-induced norm changes but due to their indirect elicitation method cannot disentangle them from, say, changes in personal moral views or behavioral preferences.

The specific setting for our norms experiment derives from a behavior experiment which we introduce in detail in another paper (Apffelstaedt and Freundt, 2022). In

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for others to perpetuate it", the Huffington Post (Nov 16, 2016), for instance, writes. See Jeltsen, M., "Trump's Election Raises Fears Of Increased Violence Against Women", *The Huffington Post*, Nov 16, 2016. Available at: `https://www.huffpost.com/entry/trump-women-rights-violence-fears_n_582a0f63e4b02d21bbc9f186` (accessed February 18, 2021).

the behavior experiment, participants must choose whether to act prosocially and give away a fixed part of their experimental endowment (3 out of 10 lottery tickets) to a lesser endowed individual ("Give"), or to act selfishly and keep the entire endowment to themselves ("Don't Give"). Participants make this decision twice, once in a "rule-free" environment (stage 1) and once again after a "code of conduct" has been elected by a referendum among the participants (stage 2). Participants in our norms experiment rate the social appropriateness of (not) giving away income in this setting. Depending on the treatment, they are either asked to make ratings in a situation in which there is no rule (stage 1) or to condition their ratings on a rule having been elected and put into force (stage 2). This setting allows us to exogenously manipulate the election outcome as well as whether there has been an election at all. Holding everything else constant, we can thus directly measure changes in social norms as a result of election outcomes by comparing norm ratings across the different choice environments. In this way, our lab design provides what is, as far as we know, the first clean test of the effects of elected rules on social norms and complements existing field research on this topic.

In our main treatment (StdMajority), we elicit social approval ratings conditional on participants having elected a code of conduct by a simple majority vote. The elected code tells subjects either that "everybody should choose *Give*" (Rule:Give) or that "everybody should choose *Don't Give*" (Rule:Don't). To identify the effect of this election on social norms, we compare the ratings elicited in StdMajority to each other as well as to the ratings in a benchmark treatment NoRule in which subjects rate actions Give and Don't Give in the absence of an election. We find that the election indeed has a strong impact on social norms. Most impressively, and much in line with the anecdotal evidence reported above, we show that majority-elected rules can cause actions previously judged socially inappropriate (Don't Give) to become socially appropriate. This is the case, specifically, if the elected rule asks subjects to not give (Rule:Don't). We also find a statistically significant effect on social norms of Rule:Give being elected into power, but the size of the effect is much smaller.

Having established this main result, we answer two additional questions. First, we ask whether norm shifts require "free and fair" majority votes, where by by "free" we relate to the notion that all subjects are able to vote for the rule of their choice and by "fair" to the notion that all votes have equal power and are counted accurately (see the *Encyclopedia of American Civil Rights and Liberties*, Stooksbury et al., 2017). To answer this question, we run three additional treatments describing a situation in which the electoral process is subject to salient deficiencies, namely a voting fee or "poll tax," a bribe that induces subjects to change their vote, or the disenfranchisement of poor voters. We find that in the case of such deficiencies, elected rules still considerably shift social norms about prosocial actions, but their power to do so is weakened. Second, we ask the

question of whether the norm shifts we observe in our experiment can predict changes in behavior. Drawing on data from the behavior experiment in Apffelstaedt and Freundt (2022), we show that social norms indeed predict giving decisions under each elected rule in a different sample of participants.

We discuss and analyze the role of two possible mechanisms that might underlie the election-induced shifts in normative expectations we observe. As one mechanism, we discuss the possibility that elections carry informative value about the underlying preferences and values in the society. As a second mechanism, we discuss the possibility that showing compliance with rules per se (irrespective of their specific content) may be a source of social approval. To shed light on these two mechanisms, we present the results of an additional treatment variation in which we aim to disentangle the role of the informational value of election outcomes and that of mere rule compliance. Our results suggest that both mechanisms play a role and are important for understanding the impact of elections on social norms.

We conclude by discussing our main findings as well as other interesting patterns in our data in light of the above mechanisms. In our discussion, we point to an important open question: Do elections necessarily lead to more agreement on social norms? In the main body of the paper, we focus on modal, mean, and median ratings to analyze the impact of elections on social norms. However, our data also show that elections can lead to a greater dispersion of ratings. In other words, instead of leading to more agreement on what is ethically right and what is wrong, elections may also undermine pre-existing norm consensus. In principle, we think it is possible that changes to the perceived norm consensus may play at least as large a role in shaping observed behavior after elections as changes to the norm itself.

Related Literature. Social norms—defined as "shared understandings about actions that are obligatory, permitted, or forbidden" (Ostrom, 2000b, p.144)—govern many parts of our everyday lives, ranging from economic and political decisions to cultural practices and are thus an important element of any social group. In this paper, we focus on so-called *injunctive norms* or normative expectations in a population.[4] Building on work by Cialdini and Trost (1998) and Ostrom (2000b), Krupka and Weber (2013) define injunctive social norms as collective perceptions or judgments regarding the appropriateness of actions. This requires that norms are "jointly recognized, or collectively perceived, by members of a population" (Krupka and Weber, 2013, pp.498-499).

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[4]We do not consider descriptive norms or empirical expectations, i.e. expectations about what others actually do. For a discussion of both concepts see also Bicchieri (2010; 2016); Bicchieri et al. (2019). Several studies have shown that both, injunctive and descriptive norms, can influence behavior (e.g., Bicchieri and Chavez (2010); Bicchieri et al. (2020); Bursztyn et al. (2020); Cialdini et al. (1990); Krupka and Weber (2009)).

In this context, our results contribute to four different literatures. First, our work complements a handful of previous papers that examine factors in the institutional environment that can cause long-standing social norms to change. For instance, recent research has shown that social norms can change quickly as a result of policy interventions. One example are studies of so-called "norm-nudges" (for overviews see Bicchieri et al., 2019; Hauser et al., 2018). In particular, our work adds to a recent literature that examines how public decision-making processes (such as elections, initiatives, or referenda) can lead to very rapid changes in social norms, which in turn can lead to changes in behavior. Jung and Tavits (2021) argue, based on results of a panel survey, that the outcome of the 2018 Irish abortion referendum changed Irish citizens' perceptions of the social norm regarding abortion. Baskaran and Hessami (2018) and Kedia and Pareek (2020) show that elections have the potential to affect norms regarding gender as well as gender-related behavioral outcomes. Using an experimental setup, Bursztyn et al. (2020) show that Donald Trump's victory in the 2016 Presidential election increased individuals' willingness to publicly express xenophobic views as well as accept related expressions by others.[5] In a similar vein, Albornoz et al. (2020) argue that the increase in hate crime following the Brexit referendum should be attributed to a change in social norms: They show that hate crime spiked especially in regions in which the outcome of the election came as a surprise and thus, can be explained by an update of beliefs about whether xenophobic views are extreme or mainstream (Albornoz et al., 2020). In the existing studies, the effect of elections on social norms is inferred indirectly from observed behavior or revealed preferences. Our experimental design instead allows us to *directly* elicit social norms and thus examine the immediate response of norms to elections. Using a well-established norm elicitation method (Krupka and Weber, 2013), we highlight the role of shifts in the social appropriateness of actions in bringing about behavioral change. To our knowledge, our paper is the first to directly measure changes in social norms in response to the election of a behavioral rule.

Second, our paper contributes to a growing experimental literature on the effect of social norms on behavior. This literature assumes that most individuals tend to learn and follow social norms, leading, for instance, to a willingness to constrain selfish behavior (Ostrom, 2000b, p.143,149). Following this conjecture, a number of recent experiments show that many people do indeed have an intrinsic preference to conform to what is collectively perceived as socially appropriate, and that norm conformity can explain behavior in a variety of social contexts (Kimbrough and Vostroknutov, 2016; Krupka et al., 2017; Gaechter et al., 2017). Krupka and Weber (2013) find that social norms vary with different framings in dictator games and argue that this variation can provide a plausible explanation for observed differences in behavior. By providing information about the

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[5]For related results see Crandall et al. (2018) and Huang and Low (2017).

moral views of others, Bursztyn et al. (2020) manage to directly manipulate perceived social norms and show that this in turn changes behavior. Importantly, these studies suggest that there is a fairly stable preference for following social norms across different settings. Our results are consistent with these findings: In our experiment, we observe systematic shifts in individual perceptions of social norms and present evidence that—by assuming a stable preference for following social norms exists—these shifts are well suited to explain behavioral changes after elections. Note that we are primarily interested in *changes* in perceived social norms in a society, which is conceptually distinct from studies that examine changes in individual adherence to existing *stable* norms (e.g. Bicchieri et al., 2022).

Third, our work adds to the literature on determinants of prosocial behavior and the analysis of the broader set of motives that shape people's social conduct. Norms and social pressure have been found to be important driving factors of altruistic behavior by attaching honor to good deeds and shame to selfish behavior (Bénabou and Tirole, 2006). A number of experimental studies has since then investigated the crucial role of social norms for prosocial behavior in various contexts (Krupka and Weber, 2009; Gaechter et al., 2012; Kimbrough and Vostroknutov, 2016). Our results confirm the view that prosocial behavior can be highly context-dependent and that a shift in social norms can lead to large shifts in prosocial outcomes. We add to the literature by showing that norms regarding prosocial behavior can be influenced by elections.

Fourth, we link to an interdisciplinary literature that examines how rules and laws can change attitudes and social norms and thus influence behavior beyond the imposition of explicit sanctions. There are a number of different theoretical approaches to explain the expressive power of rules (see, e.g., Cooter, 1998; Bénabou and Tirole, 2011; McAdams, 2015). Using survey data, Galbiati et al. (2021) show that social distancing rules during the Covid-19 pandemic causally affected social norms regarding social interactions. Several other studies empirically investigate how laws shape attitudes on morally controversial issues, e.g., by studying the efficacy of anti-discrimination laws (Aksoy et al., 2020; Barron and Hebl, 2010; 2013). However, these studies are usually not able to disentangle precise channels to explain where changes in behavior or personal opinions stem from. An exception is Lane et al. (2021), who provide direct evidence that the legal status of an action causally affects its normative appropriateness. We investigate how *democratically elected* rules, in particular, affect social norms and thus behavior.

The paper proceeds as follows. In the next section, we explain the experimental setup in detail. In section 2.3 we present our results. In subsection 2.4.4, we discuss our findings with a focus on possible mechanisms and ways in which elections can influence and change norms. Section 2.5 concludes.

2.2 Design

Our experimental approach is based on the Krupka-Weber method (Krupka and Weber, 2013), which identifies social norms by asking subjects to estimate the degree of social approval for different actions in a hypothetical decision situation. We apply this method to examine how social norms concerning prosocial vs. selfish behavior are affected by the outcome of majority elections in which a "code of conduct" is elected.

2.2.1 The decision context

The specific context for our study of norms is an earlier behavior experiment (Apffelstaedt and Freundt, 2022) in which we investigate how elected rules can change behavior. For the present paper, we use the decision environment of the behavior experiment to investigate the extent to which such elections can also shift norms. To provide an understanding of the actions for which we elicit social norms and the different situations in which these actions were evaluated, we now briefly introduce the behavior experiment on which the norms experiment is based. After this, we describe in detail how we elicit the social norms for this setting.

Figure 2.1, panel a) shows the timeline for the behavior experiment. The experiment revolves around a simple paradigm:

Action: Give or Don't Give. Among the 100 subjects of each treatment, income is distributed unequally. Before learning whether one is rich or poor, each subject has to decide privately whether to *Give* or *Don't Give*, where "Give" means that, conditional on being rich, the subject shares her income with another poorer subject and "Don't Give" means that the subject does not share her income. We operationalize this paradigm using a lottery: In each treatment, we raffle a cash prize of £100 among the 100 participating subjects. At the beginning of the experiment, subjects learn that lottery tickets for the raffle will be distributed unequally: While 50 subjects receive 10 lottery tickets each, the remaining 50 subjects receive no (zero) lottery tickets. Actions Give and Don't Give are then introduced as follows: "*If you happen to be a receiver of lottery tickets, do you want to GIVE or DON'T GIVE 3 of your 10 lottery tickets to a randomly selected participant who has received no tickets?*"[6]

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[6]Technically, this is a binary dictator game with role uncertainty which uses lottery tickets as the experimental currency. For possible problems with role uncertainty, see Iriberri and Rey-Biel (2011). We took great care in explaining the game to subjects, emphasizing that "*if you happen to be a nonreceiver (50% chance), your choice whether to GIVE or DON'T GIVE does not play a role*", and employed multiple control questions to make sure that they understood the role uncertainty correctly.

Stages 1 and 2: No rule vs. elected rule. Each subject takes the decision to Give/Don't Give twice. In a first stage, right after being informed about the unequal distribution of income, and in a second stage, after taking part in a referendum in which a "code of conduct" is elected. The code of conduct can either ask that "*everyone should choose GIVE*" (Rule:Give) or, contrarily, that "*everyone should choose DON'T GIVE*" (Rule:Don't). Subjects can cast a vote for either of the two rules. Subsequently, they decide, for each of the two potential election outcomes (strategy method) whether they want to Give or Don't Give. Subjects are informed that the final rule is non-binding: "*Once a rule has been set, each individual can decide privately and anonymously whether he/she wants to follow the rule or not.*"

Elections: StdMajority, Pay4Vote, MoneyOffer, ExcludePoor. Between treatments, we vary the exact procedure with which the code of conduct is elected. The baseline treatment StdMajority implements a simple majority vote among the 100 subjects of the treatment ("*all 100 individuals who take part in the lottery are asked to vote for the rule they prefer to have implemented as the code of conduct. The rule that receives more votes in total will be implemented as the code of conduct.*"). The other three treatments introduce salient deficiencies to the standard majority vote in order to analyze the extent to which behavioral changes hinge on the election being "free and fair": Treatment Pay4Vote introduces a voting fee or "poll tax" (subjects need to pay £0.20 to make their vote count). Treatment MoneyOffer involves a bribe (subjects are offered a bonus payment of £0.20 if they reverse their vote). Finally, treatment ExcludePoor excludes all subjects from the election who have a household income below £40,000 (about half of our subjects). Subjects are informed that these conditions apply to all participants in the treatment. They are not informed, however, about the share of votes that are ultimately uncounted or manipulated, nor about the extent to which the intervention affects the final vote share.

2.2.2 The norms experiment

We aim to understand how the election outcome (stage 2) affects the normative evaluation of actions Give and Don't Give and how the introduction of an elected rule changes social norms compared to the case of NoRule (stage 1). Toward this end, we invited 500 subjects into five treatments. 400 subjects (100 per treatment) provided normative evaluations for the situation in which a rule had been elected (election treatments), while 100 additional subjects provided normative evaluations for the case of no rule (NoRule treatment). Table 2.1 provides an overview of the treatments in the norms experiment.

Figure 2.1, panels b) and c) show the timeline for the norms experiment. We let subjects progress through the decision process as far as possible as active decision makers

Figure 2.1: Timelines for behavior (panel a) and norms experiment (panels b and c). The behavior experiment is introduced and analyzed in detail in Apffelstaedt and Freundt (2022). The norms experiment (this paper) uses the decision context of the behavior experiment to investigate the extent to which elections can change social norms.

a) Behavior Experiment (Apffelstaedt and Freundt, 2022)

Unequal Distribution of Income

Stage 1: No Rule

Treatments · Stage 2: Elected Rule

Choice: Give/Don't Give

Election: Rule:Give/Rule:Don't

Choice: Give/Don't Give | Rule:Give Give/Don't Give | Rule:Don't

b) Norms Experiment: **Election** Treatments

Unequal Distribution of Income

Stage 1: No Rule

Treatments · Stage 2: Elected Rule

Choice: Give/Don't Give

Hypothetical Election: Rule:Give/Rule:Don't

Normative Evaluation: Give/Don't Give | Rule:Give Give/Don't Give | Rule:Don't

c) Norms Experiment: **NoRule** Treatment

Unequal Distribution of Income

Stage 1: No Rule   (End of experiment)

Normative Evaluation: Give/Don't Give | NoRule

(until the point at which they are asked to provide normative evaluations). In both, the election treatments and the NoRule treatment, subjects participate in a raffle for a cash prize worth £100, identical to the subjects in the behavior experiment. We begin the experiment by distributing lottery tickets (i.e. income) unequally, meaning that 50% of subjects will be "poor" and 50% will be "rich" prior to redistribution. Before learning about their own position, subjects in the NoRule treatment (Figure 2.1, panel c) then provide social norm ratings for a "hypothetical decision situation" in which subjects can redistribute income by choosing to Give/Don't Give (stage 1). Subjects in the election treatments (Figure 2.1, panel b) move through stage 1 as active decision makers, choosing themselves whether to Give or Don't Give. After this, they provide social norm ratings for a "hypothetical decision situation" in which a code of conduct has been elected (stage 2).[7]

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[7]Letting subjects progress through the experiment as active decision makers until the point at which they are asked to provide normative evaluations helps us measure social norms at the different natural stages of a society's development. The norms so obtained thus correspond exactly to the norms which arguably regulate individual behavior at the respective stages of this process. Letting subjects experience the unequal distribution of income themselves arguably helps them understand the hypothetical situation and the stakes involved better, meaning that again, the elicited norms should correspond better to those norms that also regulate behavior. In Appendix 2.6.2, we study the role of stage 1 behavior for our

Social norms: Elicitation. We elicit social norms using the coordination game method suggested by Krupka and Weber (2013). We let subjects progress through the behavior experiment until the relevant point in the timeline. Following Krupka and Weber (2013), the respective decision for which they have to provide evaluations is then presented to subjects as a "hypothetical choice situation".[8] For this situation, we ask subjects to evaluate the "social appropriateness" of actions Give and Don't Give on a 6-point scale.[9] The scale allows subjects to evaluate the action negatively as "very socially inappropriate", "socially inappropriate", or "somewhat socially inappropriate", or positively as "somewhat socially appropriate", "socially appropriate", or "very socially appropriate". Two measures are taken to ensure that the elicited rating reveals a social norm—that is, a coordinated belief about what is wrong and what is right: First, we tell subjects that "*by socially appropriate, we mean behavior that most people agree is the 'correct' or 'ethical' thing to do*". Second, subjects are incentivized to provide a rating that is identical with how most of the other subjects evaluate the action. Specifically, we pay the subject a bonus payment of £2.00 if, for a randomly selected rating, the subject's rating matches the modal rating among the 99 other subjects in her treatment.

Table 2.1: Overview of treatments in the norms experiment

Description	Treatment	Elicits social norms for:
Social norms in the absence of election (benchmark rating)	NoRule	Give/Don't Give | NoRule
Social norms after standard majority vote (baseline election)	StdMajority	Give/Don't Give | Rule:Give
		Give/Don't Give | Rule:Don't
Social norms after simply majority vote in which...		
Voters have to pay £0.20 to make vote count	Pay4Vote	——————— " ———————
Voters are offered £0.20 to vote for the opposite rule	MoneyOffer	——————— " ———————
Voters with household inc. < GBP 40K excluded from ballot	ExcludePoor	——————— " ———————

Social norms after elections: Election treatments. We run four election treatments corresponding to the four different election procedures implemented in the behavior

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results. We show that stage 1 behavior cannot explain our main findings about norm shifts with respect to the election outcome.

[8] According to the definition by Krupka and Weber (2013), a social norm is an empirically measurable collective judgment that assigns to each action a degree of appropriateness or inappropriateness. The idea is to elicit this "collective judgment" as the focal point of a coordination game in which each agent is incentivized to guess the modal normative assessment in society. Framing the situation as "hypothetical" helps to ensure that the focal point is indeed the modal *normative* assessment and nothing else, such as, for instance, the answer to the question what most people do (behavioral expectation) rather than what they ought to do (normative expectation).

[9] Note that the definition and measurement of social norms suggested by Krupka and Weber (2013) differs from previous binary conceptions of social norms by allowing for actions to vary in the degree to which they are regarded as socially (in)appropriate. This is especially important for our purpose of studying changes in these perceptions dependent on the election procedure.

experiment. The four election treatments elicit social norms in stage 2, that is, for the situation in which a code of conduct has been elected asking people to Give (Rule:Give) or Don't Give (Rule:Don't). Subjects are presented with a "hypothetical decision situation" which describes the election procedure and are then asked, using the strategy method, to provide social approval ratings for actions Give and Don't Give conditional on Rule:Give/Rule:Don't being elected into power.[10] Our baseline election treatment is StdMajority, which provides us with the social norms after a standard simple majority vote. Treatments Pay4Vote, MoneyOffer, and ExcludePoor resemble the respective election treatments in the behavior experiment described above and allow us to investigate in how far salient deficiencies in the voting process (a voting fee or "poll tax", introducing bribes, excluding voters) affect the power of elected rules to shift norms.

Social norms before/in the absence of elections: NoRule treatment. To elicit social norms before/in the absence of an election (stage 1), we implement a fifth treatment (NoRule). Subjects in this treatment are presented with a "hypothetical decision situation" describing the basic choice between Give and Don't Give, and are then asked to provide a social approval rating for each action. The choice situation resembles stage 1 in the election treatments and in the behavior experiment. Consistent with stage 1 of the behavior experiment, no mention is made of an election (or rule) when describing the decision situation.

Implementation. Detailed instructions and screenshots can be found in appendix 2.6.3. The experiment was conducted on the online survey platform Prolific using a randomly drawn sample of international participants. Prolific automatically provides us with basic (self-declared) demographic information about individual subjects. Additional to this basic information, which includes gender, age, and student status, we required that participants had filled in information about their nationality and country of residence. The four election treatments were conducted over a period of two weeks in September

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[10]Using the strategy method is in line with how choices conditional on Rule:Give and Rule:Don't, respectively, are elicited in the behavior experiment. In a survey of the literature, Brandts and Charness (2011) find little evidence that the strategy method generates systematically different behavioral responses than a direct elicitation. Most importantly, they do not find any case in which a treatment effect found with the strategy method is not observed with the direct-response method. Rauhut and Winter (2010) argue that when measuring social norms, the strategy method is preferable because the conditionality of responses can help make counterfactual states of the world more salient and thus improve the measurement of complex elements, such as the conditionality or the level of consensus of social norms. In our particular case, we do not believe that the results of a direct method would be very different from the results obtained by the strategy method. Even when not asking for a conditional response, the simple binary policy space (Rule:Give/Rule:Don't), together with the information that one of the two rules is chosen by election, will lead subjects to be aware of the counterfactual rule when making a normative judgment. We believe this "counterfactual thinking" is also consistent with the way people reason after a real-world election in which two conflicting policies (or politicians) were up for election.

2018. On average, subjects spent about 15 minutes to go through the experiment. In addition to the chance to win a cash prize of £100 and a possible bonus payment of £2.00 in the social norm task, subjects received a base payment of £1.60 for completing the experiment. The benchmark treatment (NoRule) was conducted as a separate treatment in November 2020. Since this experiment took only 10 minutes to complete, we reduced the base payment to £1.10. Data collection for each of the five treatments was preset to stop when the number of subjects reached 100.

The entire sample of 500 participants has a mean age of 28.73 years (SD 9.59), 46.60 percent of participants are female, and 38.87 percent are students. The largest share of participants have a British nationality (38.08 percent), followed by 11.62 percent US Americans.[11] The total share of "Western" subjects is 78.16 percent.[12] In treatments StdMajority, Pay4Vote, MoneyOffer and ExcludePoor, the share of subjects choosing action Give in stage 1 is .63, .67, .62, and .65, respectively. These shares are near-identical and not statistically different from each other.

2.2.3 Predictions

There are (at least) two theoretical mechanisms through which majority-elected rules may influence public consensus about how socially appropriate an action is: (1) Rules that are elected by a majority vote provide information about which action has greater support in society (see, e.g., McAdams, 2015). (2) Rule compliance itself may be considered socially appropriate (see, e.g., Nadler, 2017). Both mechanisms should lead to an increase in social approval for the action that conforms with the rule, while at the same time decreasing social approval for the action that opposes the rule.[13] Our first prediction is therefore:

Prediction 1. *Majority-elected rules shift social norms. The election of Rule:Give (Rule:Don't) will shift upward (downward) the social approval rating of action Give and will shift downward (upward) the social approval rating of action Don't Give.*

Salient deficiencies in the voting process are likely to obstruct both of the above-mentioned channels. When a considerable share of voters does not participate in the election, or their votes are manipulated, the elected rule becomes less indicative of what action enjoys majority support in society.[14] Such deficiencies also make the rule itself

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[11]Not every participant had filled out all questions about demographics. Of 500 subjects, 494 subjects filled in information on their student status and 499 provided their nationality.

[12]Western = 1 if Nationality = United Kingdom (190 participants), United States (58), Austria (4), Australia (7), Belgium(6), Canada (19), Denmark (4), Finland (5), Germany (11), Greece (12), Ireland (4), Italy (30), Netherlands (8), Norway (1), New Zealand (1), Portugal (25), Sweden (4), Switzerland (1).

[13]The two mechanisms and a follow-up treatment designed to shed light on their relevance are discussed in more detail in section 2.4.

[14]In the behavior experiment, 35% of participants in Pay4Vote refused to pay a fee to make their vote

seem less legitimate, which is why rule compliance may be perceived as less socially appropriate following a flawed election process (Tyler, 2006; Norris, 2014). Our second prediction follows:

Prediction 2. *Salient deficiencies in the voting process (introducing a voting fee, bribing voters, or disenfranchising poor voters) decrease the power of elections to change norms. The effect of elected rules on social approval ratings will be lower in Pay4Vote, MoneyOffer, and ExcludePoor than in StdMajority.*

2.3 Results

2.3.1 Do elections shift norms?

To what extent can elections change social norms? We begin our analysis by comparing the social approval ratings after a "free and fair" majority election of Rule:Give or Rule:Don't (StdMajority) to each other and to those obtained without an election (NoRule).

Figure 2.2 displays the mean and median of social approval ratings across treatments NoRule and StdMajority. The left-hand side of the figure shows how subjects rate action Give in the absence of an election (NoRule), when Rule:Give is elected by majority vote (StdMajority) and when Rule:Don't is elected by majority vote (StdMajority). The right-hand side of the figure (grey bars) shows how subjects rate action Don't Give under the same conditions. Following the convention introduced by Krupka and Weber (2013), we have converted subjects' responses into numerical scores. A rating of "very socially inappropriate" received a score of -1, "socially inappropriate" a score of -2/3, "somewhat socially inappropriate" a score of -1/3, "somewhat socially appropriate" a score of 1/3, "socially appropriate" a score of 2/3, and "very social appropriate" a score of 1.

Recall our main prediction (Prediction 1): We predicted that majority-elected rules will shift upward the social approval of actions that comply with the rule and will shift downward the social approval of actions opposed to the rule. Specifically, we predicted that the election of Rule:Give (Rule:Don't) will shift upward (downward) the social approval of action Give and will shift downward (upward) the social approval of action Don't Give. This is exactly what we find. Consider first action Give (left-hand side of Figure 2.2): In the absence of an election (NoRule), the action is rated as "socially appropriate" (mean: .72). The election of Rule:Give shifts the rating of action Give moderately upward toward "very socially appropriate" (mean: .74), whereas the election of

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count, 39% of participants in MoneyOffer were willing to reverse their vote in exchange for the small bonus payment, and 50% of voters were excluded due to a low household income in ExcludePoor (see Apffelstaedt and Freundt, 2022). Subjects in our norms experiment were not informed of these shares.

Figure 2.2: Elicited social approval (mean and median) of actions Give (left panel) and Don't Give (right panel) in the absence of an election (NoRule) and after a standard majority election of Rule:Give or Rule:Don't (StdMajority).
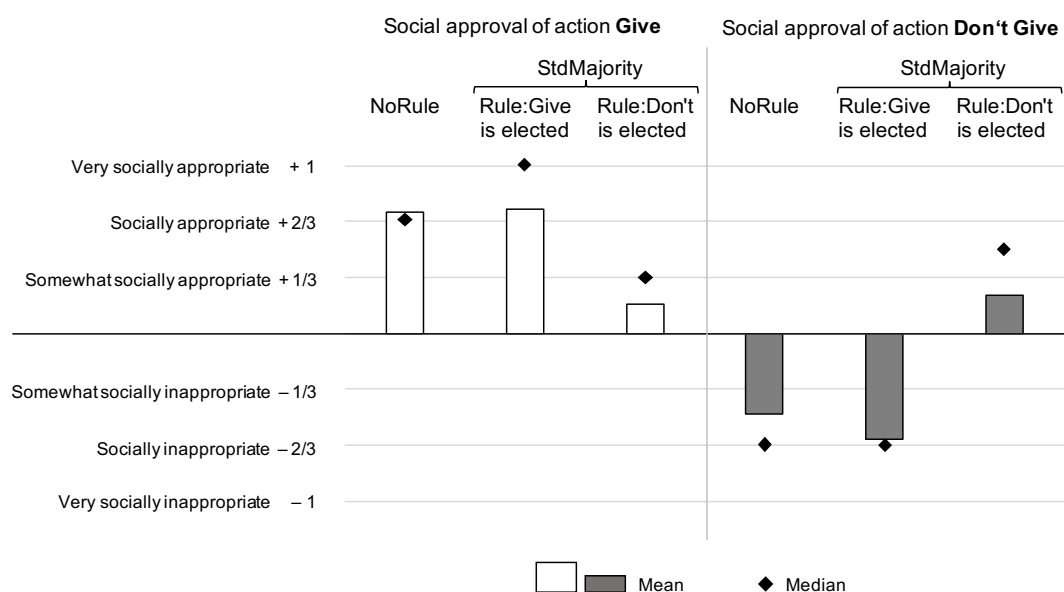
Rule:Don't leads to a strong downward shift to at or below "somewhat socially appropriate" (mean: .17). Although the mean and median ratings of action Give remain positive throughout, close to 40% of subjects rate action Give *negatively* under Rule:Don't, which is an increase of 35 and 32 percentage points, respectively, compared to NoRule and Rule:Give. We find a similar, flipped version of this pattern for action Don't Give (right-hand panel of Figure 2.2): In the absence of an election (NoRule), action Don't Give is rated as moderately socially *in*appropriate (mean: -.48). The election of Rule:Give pushes this rating slightly further into the negative (mean: -.63), whereas the election of Rule:Don't produces a strong effect in the opposite direction. Under Rule:Don't, mean and median social approval of action Don't Give are *positive* (mean: .23), *and even higher* than that of the opposite action Give. Two thirds (66%) of subjects rate action Don't Give positively under Rule:Don't, an increase of 54 and 58 percentage points, respectively, compared to NoRule and Rule:Give. This finding shows that a majority election can cause actions previously judged socially inappropriate (Don't Give) to become socially appropriate.

Detailed information on the distributions of approval ratings can be found in Table 2.2. Similar shifts to those observed in the mean and median are also observed in the modal rating of actions Give and Don't Give (shaded values in Table 2.2). Non-parametric rank-sum and signed rank tests reported at the bottom of the table verify that social norms are significantly altered by the outcome of the majority election: Compared to NoRule, Rule:Give significantly shifts upward the social approval of action Give ($z = 2.24$,

Table 2.2: Elicited social approval (full distribution and non-parametric tests) of actions *Give* and *Don't Give* in the absence of an election (NoRule) and after a standard majority election of Rule:Give or Rule:Don't (StdMajority).

	StdMajority				NoRule	
	Rule:Give		**Rule:Don't**		**NoRule**	
Rating	Give	Don't Give	Give	Don't Give	Give	Don't Give
− − −	5%	36%	8%	10%	0%	15%
− −	1%	41%	13%	11%	2%	41%
−	0%	15%	17%	13%	1%	32%
+	5%	4%	24%	16%	11%	8%
+ +	34%	1%	20%	32%	49%	4%
+ + +	55%	3%	18%	18%	37%	0%
Mean	.74	−.63	.17	.23	.72	−.48
Median	1.00	−.67	.33	.50	.67	−.67
Rating $\gtreqless$ 0 (Signed rank test (z))	7.64***	−7.70***	2.49**	3.16***	8.60***	−7.40***
vs. NoRule (Rank-sum test (z))	2.24**	−3.68***	−6.34***	7.00***		
vs. Rule:Give (Signed rank test (z))			−6.41***	7.48***		

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$; all two-tailed.
Ratings are: "very socially inappropriate" (− − −), "socially inappropriate" (− −), "somewhat socially inappropriate" (−), "somewhat socially appropriate" (+), "socially appropriate" (+ +), "very socially appropriate" (+ + +); modal ratings are shaded. For means and medians, responses are converted into numerical scores -1 (− − −), $-2/3$ (− −), $-1/3$ (−), $+1/3$ (+), $+2/3$ (+ +), $+1$ (+ + +).

$p = .025$) and significantly shifts downward the social approval of action Don't Give ($z = -3.68$, $p < .001$). Analogously, Rule:Don't significantly shifts downward the social approval of action Give ($z = -6.34$, $p < .001$) and significantly shifts upward the social approval of action Don't Give ($z = 7.00$, $p < .001$). Under Rule:Don't, action Don't Give is evaluated positively ($z = 3.16$, $p = .002$).[15] We summarize our findings below:

Result 5. *Majority-elected rules (StdMajority) shift social norms. The election of Rule:Give makes action Give (Don't Give) more (less) socially appropriate. The election of Rule:Don't makes action Don't Give (Give) more (less) socially appropriate. Majority-elected rules can cause actions previously judged socially inappropriate (Don't Give) to become socially appropriate.*

2.3.2 Do norm shifts require "free and fair" elections?

Having established that an inclusive and unbiased majority election has the power to shift social norms, we now ask whether this power is sensitive to salient deficiencies in the voting process that can make the election appear less democratic: Are norms affected

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[15]A notable additional observation (which we did not predict) is that social approval ratings become more dispersed following the election of Rule:Don't (see Table 2.2). That is, compared to the case of Rule:Give or NoRule, people seem to agree less on which action constitutes "the right thing to do" if Rule:Don't has been elected into power. One possible interpretation of this finding is that elections can sometimes lead to a fragmentation rather than to a consolidation of normative expectations. We will elaborate on this additional finding and the particular aspects of our setting that may be driving it in subsection 2.4.4.

less if there is a voting fee (Pay4Vote), voters are bribed (MoneyOffer), or parts of the electorate are excluded from the ballot (ExcludePoor)?

Figure 2.3 displays mean and median ratings of actions Give (left panel) and Don't Give (right panel) following the election of Rule:Give or Rule:Don't, respectively, across the four election procedures StdMajority, Pay4Vote, MoneyOffer, and ExcludePoor. In this figure, the benchmark average rating for the case where there exists no rule (Give: .72, Don't Give: $-.48$) is represented by a dashed line. Complementing the figure, in Table 2.3 we present OLS estimates of the effect of elected rules on mean approval ratings by treatment. Column (1) shows estimates for the effect of rules on the mean social approval of action Give. Column (3) shows estimates for the effect of rules on the mean social approval of action Don't Give. Here, the benchmark rating of treatment NoRule serves as the constant. Columns (2) and (4) replicate and add individual-specific controls for gender, age, student status and country of origin.[16]

Figure 2.3: Elicited social approval (mean and median) of actions *Give* (left panel) and *Don't Give* (right panel) across different election procedures. Dashed line denotes mean social approval in the absence of an election (treatment NoRule; Give: .72, Don't Give: -.48). Stars denote significant results of Rank-sum tests comparing the social approval after a standard majority election (StdMajority) with the social approval after a non-standard majority election (Pay4Vote, MoneyOffer, ExcludePoor): $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.
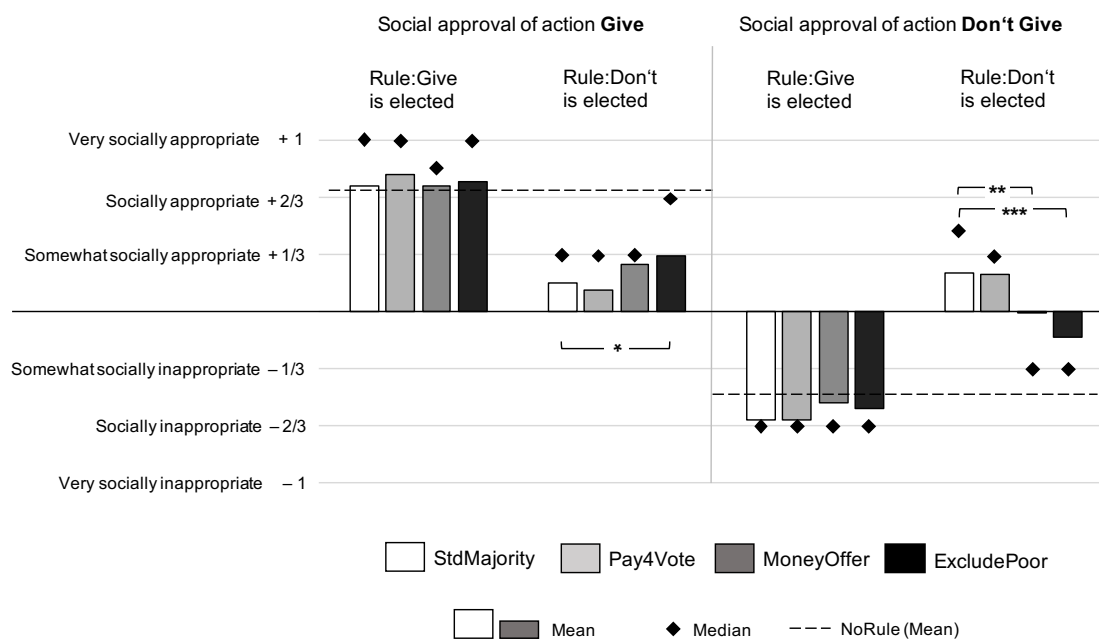
Analyzing Figure 2.3 and Table 2.3, we first see that *all* four election procedures have the power to change norms: Relative to the baseline rating of NoRule, the election of Rule:Give shifts the social approval of action Give (Don't Give) slightly upward (slightly

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[16]The entire distribution of ratings for each of the four treatments StdMajority, Pay4Vote, MoneyOffer, and ExcludePoor, including non-parametric Rank-sum test vs. NoRule and StdMajority can be found in Table 2.6 in the Appendix.

Table 2.3: Marginal effects of elected rules (Rule:Give and Rule:Don't) on mean social approval of actions Give and Don't Give across different election procedures: OLS Regressions. Mean ratings in the absence of an election (NoRule; Give: .72, Don't Give: -.48) serve as the baseline.

Dep. Var.	Mean social approval			
	Give		Don't Give	
	(1)	(2)	(3)	(4)
Election of Rule:Give				
StdMajority	.02	.03	-.16**	-.12*
	(.056)	(.059)	(.061)	(.064)
Pay4Vote	.09*	.09*	-.16**	-.12*
	(.045)	(.051)	(.064)	(.067)
MoneyOffer	.02	.02	-.06	-.02
	(.053)	(.056)	(.066)	(.067)
ExcludePoor	.04	.04	-.09	-.04
	(.049)	(.051)	(.068)	(.069)
Election of Rule:Don't				
StdMajority	-.55***	-.54***	.71***	.75***
	(.073)	(.075)	(.079)	(.082)
Pay4Vote	-.59***	-.59***	.70***	.74***
	(.071)	(.076)	(.076)	(.078)
MoneyOffer	-.44***	-.44***	.47***	.51***
	(.066)	(.071)	(.075)	(.077)
ExcludePoor	-.39***	-.39***	.33***	.38***
	(.070)	(.073)	(.076)	(.075)
Constant	.72	.63	-.48	-.30
	(.031)	(.080)	(.041)	(.087)
Mean rating NoRule	.72		-.48	
Controls	No	Yes	No	Yes
Observations	900	900	900	900
(Subjects)	(500)	(500)	(500)	(500)
R^2	.219	.224	.270	.279

Robust standard errors (clustered at subject level) in parentheses:
$*p < 0.1$; $**p < 0.05$; $***p < 0.01$.
Controls in columns (2) and (4) are: female (1/0), age (continuous), student (1/0), and Western (1/0).

downward) relative no NoRule, while the election of Rule:Don't shifts ratings strongly into the opposite direction. Qualitatively, this is true for all four treatments, i.e, regardless of whether voting is costly (Pay4Vote), voters are bribed (MoneyOffer), or parts of the electorate are excluded from the ballot (ExcludePoor). As Table 2.3 shows, the effect of Rule:Don't on mean approval ratings is always highly significant, while the effect of Rule:Give is only sometimes weakly so. On average, the election of Rule:Give shifts the mean approval rating of action Give upward by .05 points and the mean approval rating of action Don't Give downward by −.12 points, which is about one-tenth and one-third, respectively, of a discrete step in the approval rating (where a discrete step means, e.g., going from "weakly socially appropriate" to "socially appropriate"). In comparison, the

election of Rule:Don't leads to average shifts of $-.49$ points and $.55$ points, respectively, which converts to between one and two discrete steps on the rating scale.

A second observation we make is that not all of the elections shift norms to the same extent as the inclusive and unbiased majority election StdMajority. While Pay4Vote has virtually the same power as StdMajority, MoneyOffer and ExludePoor perform significantly worse in shifting social norms. Throughout Table 2.3, MoneyOffer and ExludePoor show systematically smaller coefficients than Majority. In Figure 2.3, stars denote statistically significant differences to StdMajority according to non-parametric Rank-sum tests (for detailed test results, see Table 2.6 in the Appendix). We see that shifts of social approval ratings are significantly smaller following the election of Rule:Don't when this rule comes into force with an election in which voters received bribes (MoneyOffer) or in which voters with a low household income were excluded from the ballot (ExludePoor). The results are particularly pronounced and meaningful for the social approval ratings of action Don't Give (right panel in Figure 2.3): While in StdMajority and in Pay4Vote, the election of Rule:Don't pushes the evaluation of action Don't Give significantly into the positive (signed rank test $z = 3.16$, $p = .002$, and $z = 3.33$, $p < .001$, respectively), this is not the case following the election of Rule:Don't in MoneyOffer and ExcludePoor ($z = -.06$, $p = .954$, and $z = -2.36$, $p = .019$, respectively). This comparison shows that the extent to which elections can change social norms depends on the election process: Elections that entail too large deficiencies in the voting process can still significantly shift norms, but may no longer be able to completely turn around prior normative evaluations.

Result 6. *Elected rules can shift social norms, but bribing voters (MoneyOffer) or excluding parts of the electorate (ExcludePoor) weaken this ability. While Pay4Vote has virtually the same power as StdMajority, elected rules shift social approval ratings significantly less in MoneyOffer and ExludePoor.*

2.3.3 Do election-induced norm shifts predict behavior change?

The literature on social norms typically argues that people have an intrinsic preference to conform to what is collectively perceived as socially appropriate and, in a variety of social contexts, refrain from maximizing material profits in order to comply with social norms (Elster, 1989; Bicchieri, 2006; López-Pérez, 2008; Kimbrough and Vostroknutov, 2016; Krupka et al., 2017; Gaechter et al., 2017). If elections can change norms, can these changes predict how people adapt their behavior to the election outcome? To answer this question, we draw on data from the behavior experiment (Apffelstaedt and Freundt, 2022) in which we elicited actual choices (Give or Don't Give) following the election instead of

social approval ratings.[17]

Figure 2.4: Share of subjects choosing action Give: data vs. predicted. Data from behavior experiment (Apffelstaedt and Freundt, 2022). Predictions using norms elicited through rating experiment (this paper), utility parameters according to specification Table 2.4, column (1).
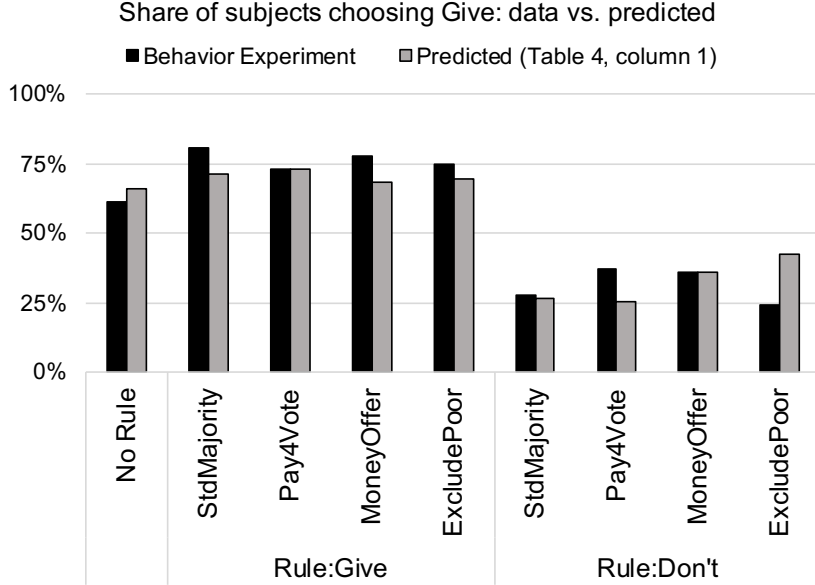
How much of the change in behavior can be *directly* predicted by changes in social norms? Note that we use a setup without strategic incentives to follow a social norm. Rather, we assume that behavioral changes are driven by changes in the moral appeal associated with each action. Let us denote by $N(\text{Give}) \in [-1, 1]$ the elicited mean social approval of action Give in a given situation and by $N(\text{Don't Give}) \in [-1, 1]$ the elicited mean social approval of action Don't Give in the same situation. We seek to understand how much the propensity to choose action Give over Don't Give in the behavior experiment depends on the difference in social approval, $N(\text{Give}) - N(\text{Don't Give})$, elicited in the rating experiment. For this, assume that the utility from taking action Give takes the form

$$U_{\text{Give}} = const + \gamma \cdot [N(\text{Give}) - N(\text{Don't Give})], \qquad (2.1)$$

and normalize the utility from taking action Don't Give to zero (i.e., $U_{\text{Don't Give}} = 0$).[18] In this simple utility framework, γ measures the weight that individuals attach to norms:

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[17]The behavior experiment, which was conducted in spring 2017 with a separate group of subjects on the same online platform as our rating experiment, forms the core of another paper (Apffelstaedt and Freundt, 2022). In that paper, we analyze the effects of different voting procedures on people's willingness to comply with elected rules. For a detailed description of the experiment and its results, see Apffelstaedt and Freundt (2022).

[18]Because subjects can only choose between two actions, Give and Don't Give, only differences in utility matter for decisions. The normalization of $U_{\text{Don't Give}} = 0$ is thus without loss of generality.

Figure 2.5: Predicted probability of choosing action Give as a function of $N(\text{Give}) - N(\text{Don't Give})$. Predictions using norms elicited through rating experiment (this paper) and actions from behavior experiment (Apffelstaedt and Freundt, 2022). Conditional Logit prediction with utility parameters according to specification Table 2.4, column (1).
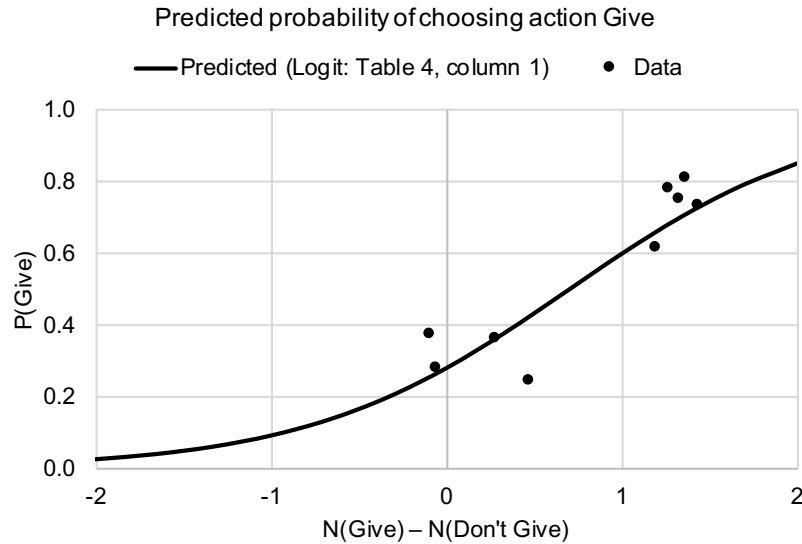
A positive weight γ implies a utility gain from following that action (Give or Don't Give) which yields a higher social approval. The constant (*const*) captures the average utility individuals derive from choosing action Give over Don't Give that is independent of norms. Following the procedure in Krupka and Weber (2013), we combine the data from the behavior experiment with the data from the rating experiment to estimate the parameters of the utility function using conditional Logit. The results of this estimation are found in Table 2.4: In column (1), we estimate γ by fitting the utility function to the share of Givers in the behavior experiment using as only explanatory variable the elicited difference in social approval, $N(\text{Give}) - N(\text{Don't Give})$. We find a large, positive and highly significant estimate, $\gamma = 1.347$ ($p < .001$). This estimate tells us that, on average, the relative utility from taking action Give strongly increases with the difference in social approval between actions Give and Don't Give. Vice versa, if that difference in ratings becomes smaller or even turns negative as, for instance, when Rule:Don't is elected, the propensity to choose action Don't Give will become larger. Columns (2)-(4) show that the estimate of γ is robust to including demographic controls and does not vary significantly if we estimate it separately by treatment (column 3).

To get a better sense of the estimated relationship between norms and behavior, Figure 2.5 plots the predicted probability of choosing action Give according to the model specification in Table 2.4, column (1): When there is no difference between the social approval of actions Give and Don't Give, $N(\text{Give}) - N(\text{Don't Give}) = 0$, $P(\text{Give})$ is predicted at 28%. That is, in the absence of clear guidance by a social norm, our model

Table 2.4: Conditional logit estimation of choice determinants (utility according to Eq. (2.1)). Choice data from behavior experiment (Apffelstaedt and Freundt, 2022). Includes mean appropriateness ratings $N(\text{Give})$ and $N(\text{Don't Give})$ from rating experiment (this paper) as explanatory variable.

Dep. Var.	Utility according to Eq. (2.1)			
	(1)	(2)	(3)	(4)
Appropriateness rating (γ)	1.347***	1.371***	1.403***	1.444***
	(.103)	(.106)	(.143)	(.148)
Appropriateness rating X				
Pay4Vote			-.194	-.233
			(.192)	(.198)
MoneyOffer			.248	.203
			(.216)	(.221)
ExcludePoor			-.154	-.140
			(.199)	(.199)
Constant (*const*)	-.944	-1.656	-.970	-1.671
	(.125)	(.393)	(.130)	(.393)
Controls	No	Yes	No	Yes
Observations	1200	1182	1200	1182
(Subjects)	(400)	(394)	(400)	(394)
Log-likelihood	-746.8	-725.9	-742.7	-722.4

Robust standard errors (clustered at subject level) in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.
Controls in columns (2) and (4) are: female (1/0), age (continuous), student (1/0), and Western (1/0).

predicts that the majority of subjects will choose selfishly. From this position, increasing the social approval of action Give (or, equivalently, decreasing the social approval of action Don't Give) will lead on average to a 10 percentage point increase in the probability of taking action Give for every step on the rating scale (e.g., going from "weakly socially appropriate" to "socially appropriate"). This is a strong relationship: In the case of a standard majority election of Rule:Give, for instance, for which we elicit a difference in social approval of $N(\text{Give}) - N(\text{Don't Give}) = 1.37$, the probability of action Give is predicted at 71%—an increase of more than 40 percentage points relative to the case without normative guidance.

Finally, in Figure 2.4, we plot—next to the original data from the choice experiment— the share of Givers predicted by the model specification in Table 2.4, column (1), for each of our experimental settings. The figure demonstrates that, overall, the simple model of norm-dependent utility specified in Eq. (2.1) performs well in reproducing behavioral changes across different choice situations, i.e., going from NoRule to Rule:Give to Rule:Don't. Behavioral differences between treatments *within* a given rule (within Rule:Give/Rule:Don't) are not predicted as consistently as differences between NoRule, Rule:Give and Rule:Don't. In line with the analysis in Apffelstaedt and Freundt (2022), we suggest that these differences should be attributed to intrinsic preferences regarding

the rule-selection procedure rather than social norms.[19] We summarize our results on the relationship of norms and behavior as follows:

Result 7. *Election-induced norm shifts predict behavior change. Using choice data from the behavior experiment in Apffelstaedt and Freundt (2022), we predict a one-step increase in the mean approval rating of an action to increase the probability of taking that action by on average 10 percentage points.*

2.4 Mechanisms of Social Norms Change

Our results show that majority-elected rules can shift collective perceptions of what constitutes socially appropriate behavior. But what exactly is the cause for these shifts? In this section, we shed light on the role of two mechanisms, both of which are inherent to elections that select rules using a majority vote, but which are not easily separated in a natural setting (and in our experiment so far).[20] The first mechanism pertains to the informational value contained in majority elections, the second to the social appropriateness of following rules per se. Both mechanisms are extensively discussed in an interdisciplinary literature on the expressive function of law (see, e.g., Sunstein, 1996; Cooter, 1998; McAdams and Rasmusen, 2007). We present the results of an additional treatment that can shed light on the importance of the two mechanisms in the context of our experiment.

To fix ideas, consider the election of Rule:Don't in StdMajority. In line with our predictions, we showed in subsection 2.3.1 that such an election increases the social approval of action Don't Give and decreases the social approval of action Give. However, is this due to (1) the information that a majority of subjects prefer Rule:Don't over Rule:Give or (2) the fact that *regardless* of what the majority prefers, it is socially appropriate to take action Don't Give now that it is mandated by the rule?

2.4.1 Mechanism 1: The informational content of majority elections

Outcomes of elections and referenda can serve as a public signal which contains information about the distribution of preferences and values in society (Bursztyn et al., 2020; McAdams, 2015). In our setting, the election of Rule:Give signals broad public support for action Give, while the election of Rule:Don't signals broad public support for action

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[19]For a detailed analysis of treatment effects on rule compliance (i.e. on behavior conditional on Rule:Give or Rule:Don't being elected) and possible mechanisms we refer the interested reader to Apffelstaedt and Freundt (2022).

[20]We thank an anonymous reviewer for pushing this argument further and motivating us to investigate the mechanisms behind our observation in more detail.

Don't Give. If most people agree that whether an action is socially appropriate depends positively on the level of support for that action in society, then this information— *regardless* of whether or not the elected rule is actually implemented—will shift collective perceptions of what constitutes socially appropriate behavior. It follows that when being asked to estimate the social approval of actions Give and Don't Give conditional on Rule:Don't being elected by a majority vote, subjects in our experiment will provide a different rating than when being asked to rate the same actions conditional on Rule:Give being elected.[21] Depending on their beliefs regarding public support for actions Give and Don't Give when *not* receiving information about a voting outcome, they will also give a different evaluation than in NoRule. Belief updating processes may play an important role in the coordination game of our experiment, as subjects have an incentive to consider any information they believe will change the modal answer.

2.4.2 Mechanism 2: The social appropriateness of following rules

An alternative explanation is based on the idea that the mere existence of a rule itself— irrespective of its formation process or underlying normative foundations—can influence collective opinion of what is ethically right and what is wrong. Rather than addressing information processes, this mechanism posits that subjects are morally obligated to obey any rule that is in place (Nadler, 2017). If such a general obligation is collectively acknowledged, this can lead to a social norm of *unconditional* rule compliance. There are various intuitive explanations for why such norms may exist. For instance, Benabou and Tirole (2011) propose a (self-)signaling model in which individuals reveal private information about themselves by (not) abiding by the law. If rule compliance is prime, subjects in our experiment will rate that action as socially appropriate that complies with the rule and will rate that action as socially inappropriate that opposes the rule—irrespective of the nature of actions and irrespective of the social norm that governs actions in the absence of a rule.

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[21]While the strength of the behavioral response to conditional information might differ, the logic, and hence the potential relevance, of this information mechanism holds irrespective of whether we elicit norms using a direct method or the strategy method. Let $S(\text{Don'tGive}|\text{Rule:Don't})$ be a subject's Bayesian posterior belief of the support for action Don't Give conditional on the information that Rule:Don't is elected. This posterior is the same whether we ask for conditional ratings (strategy method) or only present subjects with one (final) election outcome for which they give their rating (direct method).

2.4.3 Uncoupling mechanism 1 and 2: Treatment ExoRule×MajVote

Our experiment so far cannot disentangle the two mechanisms.[22] To shed light on their role, we designed and implemented a follow-up treatment, ExoRule×MajVote. The new treatment was run in July 2021 with 100 new subjects on Prolific, using the same subject pool as our main experiment. Treatment ExoRule×MajVote inherits the structure and procedures of our election treatments (see section 2.2). In stage 2, subjects are asked to rate the social approval of actions Give and Don't Give conditional on a code of conduct (Rule:Give or Rule:Don't) being implemented. However, instead of presenting subjects with an election procedure, we present them with the following situation: "*To determine the rule, a coin is flipped by the computer. If the coin lands heads (50%chance), RULE: GIVE is implemented. If the coin lands tails (50% chance), RULE: DON'T is implemented. Note: Whether RULE: GIVE or RULE: DON'T is implemented depends purely on chance. That is, the rule is random.*" When asking for their evaluations, we *additionally* ask them to condition their answer on whether the majority of individuals who take part in the lottery would vote for Rule:Give or Rule:Don't.[23] In total, we thus elicit the social norms for four different scenarios:

- Scenario 1: ExoRule (Coin flip) = Rule:Give; MajVote = Rule:Give

- Scenario 2: ExoRule (Coin flip) = Rule:Give; MajVote = Rule:Don't

- Scenario 3: ExoRule (Coin flip) = Rule:Don't; MajVote = Rule:Give

- Scenario 4: ExoRule (Coin flip) = Rule:Don't; MajVote = Rule:Don't

Hence, the information on the majority vote is now independent of the actual rule that is implemented. This design allows us to decouple the role of mere rule compliance (ExoRule) from the role of social information (MajVote).

We present the main findings from treatment ExoRule×MajVote in Table 2.5, and Figures 2.6 and 2.7. Figure 2.6 displays elicited mean and median approval ratings of action Give (left panel) and action Don't Give (right panel) across the four scenarios. In Table 2.5 we estimate the effect of ExoRule and MajVote on mean approval ratings using OLS regressions. Figure 2.7 graphically illustrates the heterogeneity in individual reactions to shifts in ExoRule and MajVote.

Looking at Table 2.5, we see that both, a change in the exogenous rule *and* a change in the majority vote have a very significant, similar and sizeable effect on social norms. Going

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[22]Real world elections, in contrast to simple polls, are typically followed by an adjustment of the institutional framework, i.e. by a change of the rules. Hence, in normal practice, elections and referenda combine the two aspects described above, as did our experiment.

[23]Detailed instructions can be found in Appendix A.X

Figure 2.6: Elicited social approval (mean and median) of actions *Give* (left panel) and *Don't Give* (right panel) across different scenarios in ExoRule×MajVote. Dashed line denotes mean social approval in the absence of an election (treatment NoRule; Give: .72, Don't Give: -.48).
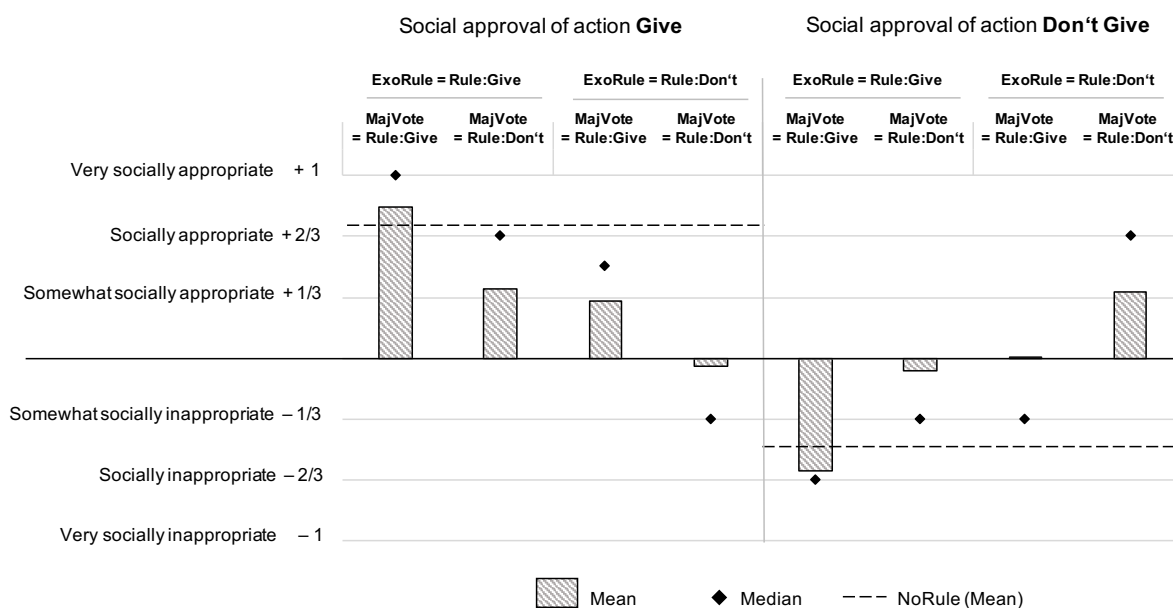
from ExoRule = Rule:Give to ExoRule = Rule:Don't, ceteris paribus, shifts mean social approval ratings by −.52 (action Give) and +.62 (action Don't Give) points, respectively. Similarly, going from MajVote = Rule:Give to MajVote = Rule:Don't, ceteris paribus, shifts mean social approval ratings by −.45 (action Give) and +.55 (action Don't Give) points, respectively.[24]

Looking at Figure 2.6, we see that in absolute terms, the social approval ratings for scenarios 1 (ExoRule = MajVote = Rule:Give) and 4 (ExoRule = MajVote = Rule:Don't) are very similar to the social approval ratings which we document in StdMajority following a majority election of Rule:Give and Rule:Don't, respectively. Going from scenario 1 (on the very left) to scenario 4 (on the very right), we find that—as in StdMajority, when going from Rule:Give to Rule:Don't—the approval ratings can flip, making action Don't Give (instead of action Give) the relatively more appropriate action. As in StdMajority, action Don't Give is rated significantly positively following such a change (signed rank test, $z = 4.87$, $p < .001$). However, for such a major shift in ratings to occur, both a change in the formal rule (ExoRule=Rule:Don't) and a majority vote confirming that rule (MajVote=Rule:Don't) are required. This observation suggests that both mechanisms also contribute significantly to the shift in social norms that we observe in our main experiment.

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[24]The interaction term in Table 2.5 indicates that the marginal effects of changes in ExoRule and MajVote are somewhat smaller when the initial situation is one in which both variables are set to Rule:Don't (ExoRule = MajVote = Rule:Don't) rather than Rule:Give (ExoRule = MajVote = Rule:Give). This suggests that social norms associated with Rule:Don't are somewhat harder to change.

Table 2.5: Marginal effects of ExoRule and MajVote on mean social approval of actions Give and Don't Give in treatment ExoRule×MajVote: OLS Regressions.

Dep. Var.	Mean social approval	
	Give (1)	Don't Give (2)
ExoRule = Rule:Give	baseline	baseline
MajVote = Rule:Give	baseline	baseline
ExoRule = Rule:Don't	-.52***	.62***
	(.068)	(.069)
MajVote = Rule:Don't	-.45***	.55***
	(.065)	(.066)
ExoRule x MajVote = Rule:Don't x Rule:Don't	.09	-.19**
	(.093)	(.080)
Constant	.83	-.61
	(.034)	(.050)
Observations	400	400
(Subjects)	(100)	(100)
R^2	.236	.247

Robust standard errors (clustered at subject level) in parentheses.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Last but not least, looking at Figure 2.7, we document that while subjects are quite heterogeneous in their reactions to ExoRule and MajVote, the majority of subjects show responses that are broadly in line with the aggregate effects reported above. Only 9 (10) out of 100 subjects do not adjust their rating of action Give (Don't Give) to either of the two aspects (bubbles at the origin). A minority of subjects (18 of 100 in both panels) only respond to either the rule or the majority vote (bubbles on the axes). The large majority of subjects (56 (50) out of 100) are found in the interior of the first quadrant: Consistent with the aggregate effects reported above, their ratings respond positively to both a change in the exogenous rule endorsing the action and a change in the majority decision endorsing the action. There seems to be some agreement among subjects, then, that both, the compliance with rules *and* the compliance with majority opinions, are relevant to the moral evaluation of behavior.

2.4.4 Interpreting the data in light of the two mechanisms

The results of our follow-up treatment ExoRule×MajVote suggest that both the informative value of elections as well as a general obligation to comply with rules are drivers of election-induced norm shifts. Below, we discuss how these two mechanisms may relate to three interesting patterns that we observe in the data of our main experiment.

Asymmetric effects of Rule:Don't vs. Rule:Give. We observe that the election of Rule:Don't seems to shift social norms relative to NoRule to a greater extent than

Figure 2.7: Graphical illustration of shifts in individual norm ratings for action Give (left) and action Don't Give (right) as a consequence of a change in the exogenous rule (y-axis) or the majority vote (x-axis): Treatment ExoRule×MajVote. Bubble sizes correspond to subject densities (n=100). The further away a bubble from the origin, the stronger the reaction. The strongest possible positive reaction is a change from "very socially inappropriate" (-1) to "very socially appropriate" (+1), which converts into a reaction of +2. The strongest possible negative reaction is a change from "very socially appropriate" (+1) to "very socially inappropriate" (-1), which converts into a reaction of −2.

Shifts in individual ratings of action **Give**
(if Rule:Don't → Rule:Give)

Shifts in individual ratings of action **Don't Give**
(if Rule:Give → Rule:Don't)

X-Axis:	[Avg.Rating(Give) | MajVote = Rule:Give] − [Avg.Rating(Give) | MajVote = Rule:Don't]
Y-Axis:	[Avg.Rating(Give) | ExoRule = Rule:Give] − [Avg.Rating(Give) | ExoRule = Rule:Don't]

X-Axis:	[Avg.Rating(Don't Give) | MajVote = Rule:Don't] − [Avg.Rating(Don't Give) | MajVote = Rule:Give]
Y-Axis:	[Avg.Rating(Don't Give) | ExoRule = Rule:Don't] − [Avg.Rating(Don't Give) | ExoRule = Rule:Give]

the election of Rule:Give (see, for instance, Table 3). This observation lends itself to an interpretation using either the first or the second mechanism.[25] Assume, first, that subjects primarily care about the informative value of elections (mechanism 1). Compared to NoRule, the election of a rule then affects ratings to the extent that the elected rule changes subjects' beliefs about the degree of public support for actions Give and Don't Give. Subjects will react asymmetrically to rules if the election of one rule confirms their prior and the election of the other rule rejects their prior. It follows that if subjects have the prior (NoRule) belief that most participants would vote for Rule:Give, then the election of Rule:Give will shift social norms less than the election Rule:Don't. Assume,

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[25]We do not mean to suggest that other mechanisms are not at work. An alternative explanation is that there is less room for a shift in ratings in response to Rule:Give than for a shift in ratings in response to Rule:Don't Give, because the social approval ratings for Give and Don't Give are already relatively close to the upper and lower limits, respectively, in the absence of a rule. Another explanation is that Rule:Don't and Rule:Give differ in the basic mechanisms by which they affect social norms, e.g., because Rule:Don't can also be used as an "excuse" for selfish behavior. For instance, Bénabou et al. (2018) model how such narratives can influence prosocial behavior.

instead, that subjects primarily care about rule compliance (mechanism 2). Elected rules then shift norms to the extent to which rule compliance after the election contradicts any prior assessment of social appropriateness. In the case of Rule:Give, rule compliance and prosocial behavior (which is arguably the most important factor when evaluating actions Give and Don't Give in the absence of a rule) coincide. This may lead to a reinforcement of the previous (NoRule) norm, but should not greatly change the basic evaluation of the actions. The election of Rule:Don't, on the other hand, leads to a clash of the two rating dimensions. The more important rule compliance is to the subjects, the more strongly norms will shift in this case. As long as rule compliance is an important criterion, Rule:Don't will shift social norms more than Rule:Give.

Salient deficiencies in the voting process. In subsection 2.3.2, we document differences in the extent to which social norms are affected by elections in which participants are offered a bribe (MoneyOffer) or excluded from the election (ExcludePoor) relative to the case of a standard majority vote. While we can only speculate about this, it seems intuitive that when evaluating the social appropriateness of actions, the more democratic the election of the rule, the greater the weight that should be given to rule compliance (mechanism 2). Following this argumentation, controversial practices such as vote buying and the disenfranchisement of poor voters should decrease the power of elections to change social norms. It is also possible that the effect is due to a signal extraction problem (mechanism 1): If the election result is biased or no longer representative, it reveals less about the public support for a given policy. Again, social norms should react to a lesser extent to the rule that was elected. We also observe that the introduction of a voting fee or "poll tax" (Pay4Vote) leaves the power of elected rules to shift norms largely unaffected. In line with our reasoning above, it could be that a voting fee is not seen as such a massive interference with democratic principles, rendering the obligation to comply with the elected rule more important than in treatments MoneyOffer and ExcludePoor. It also seems plausible that the informative value of the election is higher in this treatment, as the election result is potentially less biased and still (somewhat) representative.

Elections and norm consensus. In the results so far, we have focused on modal, mean, and median ratings to analyze effects of elections on social norms. Another noteworthy finding which we have only referred to in a footnote (see footnote 14) is an observation that may, prima facie, appear counterintuitive: We find that elections do not only have the potential to cause a shift in modal, median and mean social appropriateness ratings but also an increase in the variance of the distribution of individual ratings (see Table 2.2 in the main text and Table 2.6 in the Appendix). We interpret this finding as a decrease in *norm consensus*, i.e. in the degree to which members of a society agree

on which action constitutes "the right thing to do". How can we explain the fact that social norms may become *less* clear even though elections are thought to provide *better* information about the moral preferences in society? In our data, this finding is again particularly prevalent in the case of Rule:Don't.

We can make sense of this finding by acknowledging that people may be heterogeneous when it comes to weighting the social appropriateness of following elected rules (mechanism 2) against the social appropriateness of taking prosocial actions. In the case of Rule:Don't, these two dimensions disagree regarding the evaluation of actions Give (high on prosociality, low on rule compliance) and Don't Give (high on rule compliance, low on prosociality). A subject who weights rule compliance strongly (and believes other people to do so as well), will rate action Give socially inappropriate and action Don't Give socially appropriate. At the same time, however, a respondent who strongly weights prosociality over compliance will give the opposite rating. As people become unsure or begin to disagree about what the social norm is, rating decisions diverge as a consequence of the election of Rule:Don't. Ultimately, either the shift in modal, median, and mean social appropriateness ratings that we observe under Rule:Don't *or* the observed erosion of norm consensus could be responsible for why people's behavior is affected by the election.

2.5 Conclusion

We have investigated how elected rules can affect what is perceived as socially appropriate behavior. Participants in our online experiment rate the social appropriateness of sharing versus not sharing experimental income with other participants. We find that majority-elected rules that ask people to share or not to share, respectively, can change social norms: They shift the modal appropriateness perception of actions and, as a result, can cause actions previously judged socially inappropriate (not sharing) to become socially appropriate. Comparing different voting procedures, we show that this power prevails, albeit in a weakened form, even if the election process is flawed (introducing a voting fee or "poll tax", bribing voters, disenfranchising poorer voters). Using behavioral data from a related experiment (Apffelstaedt and Freundt, 2022), we show that the norm shifts we observe are able to predict changes in behavior that result from the election of rules. A follow-up treatment suggests that both the informational value contained in election results and the social appropriateness of following rules *per se* play a role in shifting social norms.

We hope that our paper will stimulate future research on the importance of democratic procedures in general, and elections in particular, for the formation and dissolution of social norms in a society. Our data show that elected rules not only shift modal appropriateness ratings of behavior, but can also alter their distribution and lead to an

erosion of a previously existing norm consensus. Psychological research underscores the importance of perceived social consensus or "norm clarity" in shaping one's opinions and the ability of norms to guide behavior (Lewandowsky et al., 2019; Zitek and Hebl, 2007). Thus, to fully understand how elections influence norms and how these norms in turn shape behavior, we believe it is an important task of future research to shed light on the role of norm consensus as well as the role of individual uncertainty about social norms in driving behavioral responses.

2.6 Appendix

2.6.1 Distribution of ratings across treatments

Table 2.6: Elicited social approval of actions *Give* (panel a) and *Don't Give* (panel b) across all treatments.

Panel (a): Action **Give**	Rule:Give				Rule:Don't				NoRule
Rating	StdMaj	P4Vote	MOffer	ExPoor	StdMaj	P4Vote	MOffer	ExPoor	NoRule
− − −	5%	1%	3%	2%	8%	9%	6%	10%	0%
− −	1%	1%	2%	1%	13%	11%	4%	3%	2%
−	0%	1%	0%	1%	17%	21%	21%	13%	1%
+	5%	4%	10%	6%	24%	23%	25%	19%	11%
+ +	34%	36%	31%	40%	20%	23%	28%	37%	49%
+ + +	55%	57%	54%	50%	18%	13%	16%	18%	37%
Mean	.74	.80	.74	.76	.17	.13	.27	.33	.72
Median	1.00	1.00	1.00	.83	.33	.33	.33	.67	.67
Rating $\geqslant$ 0 (Signed rank test (z))	7.64***	8.61***	7.98***	8.28***	2.49**	2.00**	4.36***	4.50***	8.60***
vs. NoRule (Rank-sum test (z))	2.24**	2.88***	1.82*	1.80*	−6.34***	−7.08***	−5.99***	−4.94***	
vs. StdMaj (Rank-sum test (z))		0.52	−0.31	−0.51		−0.49	1.06	1.80*	

Panel (b): Action **Don't Give**	Rule:Give				Rule:Don't				NoRule
Rating	StdMaj	P4Vote	MOffer	ExPoor	StdMaj	P4Vote	MOffer	ExPoor	NoRule
− − −	36%	43%	35%	37%	10%	5%	11%	12%	15%
− −	41%	32%	25%	35%	11%	14%	15%	30%	41%
−	15%	16%	27%	15%	13%	17%	25%	17%	32%
+	4%	2%	7%	4%	16%	20%	20%	19%	8%
+ +	1%	4%	3%	5%	32%	26%	22%	16%	4%
+ + +	3%	3%	3%	4%	18%	18%	7%	6%	0%
Mean	−.63	−.63	−.53	−.57	.23	.22	−.01	−.15	−.48
Median	−.67	−.67	−.67	−.67	.50	.33	−.33	−.33	−.67
Rating $\geqslant$ 0 (Signed rank test (z))	−7.70***	−7.45***	−7.02***	−6.88***	3.16***	3.33***	−.06	−2.36**	−7.40***
vs. NoRule (Rank-sum test (z))	−3.68**	−3.87***	−1.70*	−2.92***	7.00***	7.36***	5.30***	3.27***	
vs. StdMaj (Rank-sum test (z))		−0.51	1.45	.42		−0.24	−2.73**	−4.00***	

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$; all two-tailed.
Ratings are: "very socially inappropriate" (− − −), "socially inappropriate" (− −),"somewhat socially inappropriate" (−), "somewhat socially appropriate" (+), "socially appropriate" (+ +), "very socially appropriate" (+ + +); modal ratings are shaded. For means and medians, responses are converted into numerical scores −1 (− − −), −2/3 (− −), −1/3 (−), +1/3 (+), +2/3 (+ +), +1 (+ + +).

Table 2.7: Elicited social approval (full distribution and non-parametric tests) of actions *Give* (Panel a) and *Don't Give* (Panel b) across choice environments in ExoRule×MajVote.

	ExoRule×MajVote			
Panel (a): Action **Give**	**ExoRule = Rule:Give**		**ExoRule = Rule:Don't**	
Rating	MajVote = Rule:Give	MajVote = Rule:Don't	MajVote Rule:Give	MajVote Rule:Don't
− − −	1%	3%	6%	13%
− −	1%	11%	7%	19%
−	2%	11%	12%	21 %
+	3%	17%	25%	18%
+ +	26%	34%	37%	19%
+ + +	67%	24%	13%	10%
Mean	.83	.38	.31	−.04
Median	1.00	.67	.50	−.33
Rating ≷ 0 (Signed rank test (z))	8.73***	5.40***	4.67***	−.57
vs. NoRule (Rank-sum text (z))	4.060***	−4.000***	−5.842***	−8.181***
Panel (a): Action **Don't Give**	**ExoRule = Rule:Give**		**ExoRule = Rule:Don't**	
Rating	MajVote = Rule:Give	MajVote = Rule:Don't	MajVote Rule:Give	MajVote Rule:Don't
− − −	42%	10%	7%	6%
− −	32%	24%	25 %	11%
−	14%	20%	20%	10%
+	5%	20%	17%	13%
+ +	6%	20%	18%	34 %
+ + +	1%	6%	13%	26%
Mean	−.61	−.07	.00	.36
Median	−.67	−.33	−.33	.67
Rating ≷ 0 (Signed rank test (z))	−7.58***	−1.09	.13	4.87***
vs. NoRule (Rank-sum text (z))	−3.557***	4.449***	5.056***	8.181***

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$; all two-tailed.
Ratings are: "very socially inappropriate" (− − −), "socially inappropriate" (− −), "somewhat socially inappropriate" (−), "somewhat socially appropriate" (+), "socially appropriate" (+ +), "very socially appropriate" (+ + +); modal ratings are shaded. For means and medians, responses are converted into numerical scores -1 (− − −), $-2/3$ (− −), $-1/3$ (−), $+1/3$ (+), $+2/3$ (+ +), $+1$ (+ + +).

2.6.2 Subgroup Analysis (stage 1 Behavior)

We analyze the role played by stage 1 choices (Give vs. Don't Give) for ratings elicited in the election treatments. We call a subject who chose action Give in stage 1 a "Giver" and a subject who chose action Don't Give in stage 1 a "Nongiver". Table 2.8 reports the distribution of types (Giver and Nongiver) across the four election treatments. The distributions do not statistically differ from each other (Fisher's exact tests, smallest $p = .55$ (two-sided) for Pay4Vote vs. MoneyOffer).

Table 2.8: Distributions of choices (Give vs. Don't Give) in stage 1 of election treatments

stage 1 choice	Number of Subjects				
	StdMajority	Pay4Vote	MoneyOffer	ExcludePoor	Total
Give (Giver)	63	67	62	65	257
Don't Give (Nongiver)	37	33	38	35	143
Total	100	100	100	100	400

Decisions in stage 1 cannot account for our main results. Below, we show that: First, differences in ratings for actions Give and Don't Give with respect to the election outcome in StdMajority are highly significant and go into the same direction in *both* subsamples. That is, both Givers and Nongivers agree that the election of Rule:Give makes action Give (Don't Give) more (less) socially appropriate, while the election of Rule:Don't makes action Give (Don't Give) less (more) socially appropriate. Second, norm shifts between StdMajority and NoRule cannot be explained by stage 1 behavior. Significant differences in ratings with respect to the existence of an elected rule are found in both subsamples. Third, differences in ratings between the election treatments cannot be explained by stage 1 behavior since the distribution of Givers and Nongivers is near identical across treatments. Controlling for stage 1 behavior in a regression on ratings across the four election treatment thus leaves treatment effects unaffected.

Table 2.9 reports the results of OLS regressions on ratings across the four election treatments by type (Giver, NonGiver) and pooled. Ratings conditional on Rule:Give in treatment StdMajority serve as the baseline. We see that changing the election outcome (Rule = Rule:Don't) has highly significant effects on the social approval ratings of both actions (Give and Don't Give) in both the Giver and the Nongiver subsample. In both subsamples, the effects are large, highly significant and go into the same direction: The election of Rule:Don't makes action Give (Don't Give) less (more) socially appropriate. Effect sizes are higher for Nongivers than for Givers, meaning that Nongivers react somewhat stronger to a change in the election outcome than Givers. We also find that Givers, on average, rate action Don't Give worse than Nongivers (column (8), Giver = 1). In sum, while we find differences between types, these differences do not challenge our main

finding that election outcomes (on average and across the entire population) shift norm ratings. Regarding treatment effects (Pay4Vote, MoneyOffer, ExcludePoor), we also do not see large differences across types. If anything, Nongivers react a bit stronger to salient deficiencies in the voting process, especially when interacted with a change of the election outcome (Rule = Rule:Don't). Since the distribution of Givers and Nongivers is near identical across treatments, controlling for stage 1 behavior (Giver = 1) in the pooled regressions leaves average treatment effects unaffected (columns (4) and (8)).

Table 2.9: Marginal effects on social approval ratings across election treatments by type (Giver vs. Nongiver) and pooled: OLS Regressions. Ratings conditional on Rule:Give in treatment StdMajority serve as the baseline.

Dep.Var.	Mean social approval							
	Give				Don't Give			
	Giver (1)	Nongiver (2)	Pooled (3)	Pooled (4)	Giver (5)	Nongiver (6)	Pooled (7)	Pooled (8)
Pay4Vote	.14 (.077)	-.07 (.083)	.07 (.057)	.06 (.058)	-.08 (.080)	.16 (.123)	.00 (.067)	.01 (.067)
MoneyOffer	.02 (.090)	-.04 (.080)	-.00 (.064)	-.00 (.064)	.08 (.090)	.13 (.101)	.10 (.068)	.10 (.098)
ExcludePoor	.06 (.087)	-.04 (.061)	.02 (.060)	.02 (.060)	.03 (.090)	.12 (.113)	.07 (.070)	.07 (.070)
Rule = Rule:Don't (StdMajority)	-.46*** (.098)	-.76*** (.129)	-.57*** (.079)	-.57*** (.079)	.67*** (.101)	1.20*** (.118)	.86*** (.081)	.86*** (.081)
×Pay4Vote	-.16 (.133)	-.05 (.168)	-.11 (.106)	-.11 (.106)	.18 (.142)	-.33* (.197)	-.01 (.117)	-.01 (.117)
×MoneyOffer	.05 (.124)	.20 (.173)	.10 (.102)	.10 (.102)	-.23* (.142)	-.53*** (.177)	-.34*** (.114)	-.34*** (.114)
×ExcludePoor	.08 (.130)	.22 (.174)	.14 (.105)	.14 (.105)	-.32** (.141)	-.65*** (.209)	-.44*** (.120)	-.44*** (.120)
Giver = 1				.03 (.040)				-.18*** (.040)
Constant	.68 (.068)	.84 (.051)	.74 (.047)	.72 (.050)	-.62 (.064)	-.66 (.054)	-.63 (.045)	-.52 (.048)
Observations (Subjects)	514 (257)	286 (143)	800 (400)	800 (400)	514 (257)	286 (143)	800 (400)	800 (400)
R^2	.169	.315	.214	.215	.232	.380	.274	.291

Robust standard errors (clustered at subject level) in parentheses:
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

In Table 2.10 we report the overall distribution of ratings in StdMajority separately for Givers and Nongivers alongside the ratings in NoRule. We see that differences in ratings for actions Give and Don't Give in StdMajority compared to NoRule go into the same direction for Givers and Nongivers. Within StdMajority, the only significant difference in the rating distributions between Givers and Nongivers is found for action Don't Give in the case where Rule:Don't has been elected. Here, the social approval of

action Don't Give is significantly higher in the Nongiver compared to the Giver subsample. Even here, however, the effect of the elected rule compared to NoRule goes into the same direction: The election of Rule:Don't makes action Don't Give significantly more appropriate compared to NoRule. We conclude that norm shifts between NoRule and Rule:Give/Rule:Don't are not attributable to one subgroup only, but are driven by a reaction of both types.

Table 2.10: Elicited social approval of actions *Give* (panel a) and *Don't Give* (panel b) by subgroup (Givers vs. Nongivers): StdMajority vs. NoRule.

Panel (a): Action **Give**		StdMajority				NoRule
	Rule:Give		**Rule:Don't**			**NoRule**
Rating	Givers	Nongivers	Givers	Nongivers		all subjects
− − −	8%	0%	5%	14%		0%
− −	0%	3%	14%	11%		2%
−	0%	0%	17%	16%		1%
+	6%	3%	19%	32%		11%
+ +	37%	30%	25%	11%		49%
+ + +	49%	65%	19%	16%		37%
Mean	.68	.84	.22	.08		.72
Median	.67	1.00	.33	.33		.67
Rating ≥ 0 (Signed rank test (z))	5.50***	5.39***	2.67***	.68		8.60***
vs. NoRule (Rank-sum test (z))	1.10	2.87***	−5.08***	−5.53***		
vs. Givers (Rank-sum test (z))		1.70*		−1.17		

Panel (b): Action **Don't Give**		StdMajority				NoRule
	Rule:Give		**Rule:Don't**			**NoRule**
Rating	Givers	Nongivers	Givers	Nongivers		all subjects
− − −	40%	30%	16%	0%		15%
− −	37%	49%	16%	3%		41%
−	14%	16%	11%	16%		32%
+	3%	5%	21%	8%		8%
+ +	2%	0%	25%	43%		4%
+ + +	5%	0%	11%	30%		0%
Mean	−.62	−.66	.05	.54		−.48
Median	−.67	−.67	.33	.67		−.67
Rating ≥ 0 (Signed rank test (z))	−5.75***	−5.26***	.34	4.66***		−7.40***
vs. NoRule (Rank-sum test (z))	−3.28***	−2.63***	4.37***	7.79***		
vs. Givers (Rank-sum test (z))		.46		3.76***		

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$; all two-tailed.
Ratings are: "very socially inappropriate" (− − −), "socially inappropriate" (− −), "somewhat socially inappropriate" (−), "somewhat socially appropriate" (+), "socially appropriate" (+ +), "very socially appropriate" (+ + +); modal ratings are shaded. For means and medians, responses are converted into numerical scores −1 (− − −), −2/3 (− −), −1/3 (−), +1/3 (+), +2/3 (+ +), +1 (+ + +).

2.6.3 Experimental Instructions

Welcome and Consent Form

This study is hosted by the University of Hamburg [Fribourg/Cologne].
Thank you for participating in our study! Your participation is very important to our research. The study takes about 15 minutes to complete and we ask you to please finish the study in one sitting.
Please read the following consent form before continuing:

- I consent to participate in this research study. I am free to withdraw at any time without giving a reason (knowing that any payments only become effective if I complete the study).

- I understand that all data will be kept confidential by the researchers. All choices are made in private and anonymously. Individual names and other personally identifiable information are not available to the researchers and will not be asked at any time. No personally identifiable information will be stored with or linked to data from the study.

- I consent to the publication of study results as long as the information is anonymous so that no identification of participants can be made.

If you have any questions about this research, please feel free to contact us at [email hidden for manuscript].
To proceed, please give your consent by ticking the box below:

☐ I have read and understand the explanations and I voluntarily consent to participate in this study.

General Instructions

Please read the following instructions very carefully before proceeding with the study.

- This study has 100 participants. You are one of them.

- Each participant receives a base payment of GBP 1.60 for completing the study.

- One participant will receive an extra cash prize of GBP 100. The winner of this cash prize is determined by a lottery. The chance of a participant to win the lottery depends on how many lottery tickets he/she holds at the end of the study.

- The number of lottery tickets you receive depends partly on luck and partly on yours and other participants' choices during this study. The final number of lottery tickets a participant holds ranges from 0 to 10. Each lottery ticket has the same chance to be the winning ticket.

- The winner of the GBP 100 cash prize will be drawn once all 100 participants have completed the study and will be notified one week from now at the latest. You receive all payments through your Prolific.co account.

- Completion of the study at normal pace should not take more than 15 minutes.

Please tick this box when you are done reading the information and want to proceed.

☐ I have read the information and want to proceed.

Instructions about the Lottery

- 500 lottery tickets will be distributed among the 100 participants. One of these lottery tickets is the winning ticket. The winning ticket yields the holder of the ticket a cash prize of GBP 100. The final distribution of lottery tickets depends partly on luck and partly on the choices you and other participants make.

- You will begin with task 1 on the next screen.

Please tick this box when you have read the instructions and want to proceed:

☐ I have read the information and want to proceed.

Instructions about the Distribution of Lottery Tickets

The lottery tickets are distributed in two steps.
Step 1: The computer picks 50 receivers and 50 nonreceivers:

- The computer randomly selects 50 out of 100 participants to be "Receivers". Each receiver gets 10 lottery tickets from the computer.

- The other 50 participants are "Nonreceivers". Nonreceivers get no tickets from the computer.

- No participant learns whether he/she has been chosen to be a receiver or a nonreceiver until the end of the study.
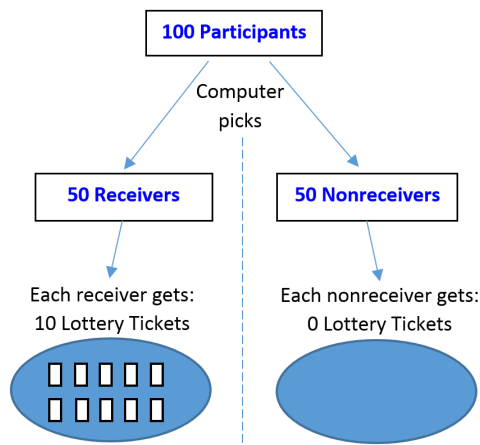
Step 2: Participants decide whether they want to share tickets with nonreceivers:

- All participants decide—for the case they happen to be a receiver—whether they want to give 3 lottery tickets to a nonreceiver

- This decision (GIVE or DON'T GIVE) has the following consequences:
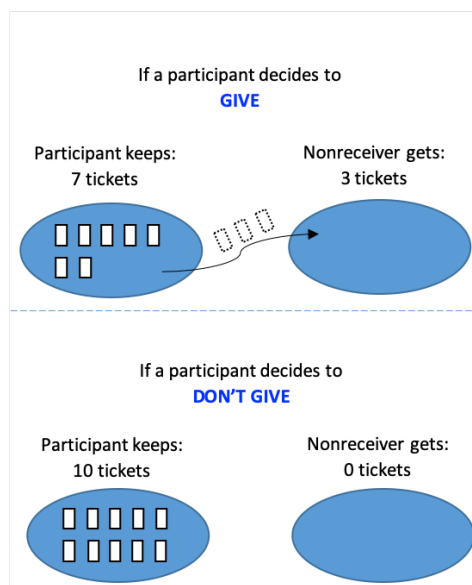
When taking the decision whether to GIVE or DON'T GIVE, you will not know whether you have been selected to be a receiver or a nonreceiver. Nor will anybody else. You will receive a message with this information after all participants have finished the study.

If you happen to be a receiver (50% chance), your choice whether to GIVE or DON'T GIVE determines the final number of lottery tickets for you and for one other participant. If you happen to be a nonreceiver (50% chance), your choice whether to GIVE or DON'T GIVE does not play a role. In this case, the choice of another participant (who happens to be a receiver) determines the number of lottery tickets that you will receive.

Please make sure that you have understood the instructions given above. Once you are sure to have understood the instructions, please tick here to proceed.

☐ I have read and understood the instructions and would like to proceed.

To show that you have read and understood the instructions, please answer the following control questions:

If you happen to be a receiver and choose GIVE, you will hold: (numbers appear in random order)

○ 3 tickets

○ 10 tickets

○ 7 tickets (correct)

○ 0 tickets

If you happen to be a receiver and choose DON'T GIVE, you will hold:

○ 7 tickets

○ 10 tickets (correct)

○ 0 tickets

○ 3 tickets

If you happen to be a nonreceiver and the other participant chooses GIVE, you will hold:

○ 0 tickets

○ 10 tickets

○ 3 tickets (correct)

○ 7 tickets

If you happen to be a nonreceiver and the other participant chooses DON'T GIVE, you will hold:

○ 0 tickets (correct)

○ 3 tickets

○ 7 tickets

○ 10 tickets

Instructions for The Giving Decision (Stage 1)

Task 1
Your Choice: Give or Don't Give
If you happen to be a receiver, do you want to GIVE or DON'T GIVE 3 of your 10 lottery tickets to a randomly selected participant who has received no tickets?

- We ask all participants to make this choice.

- If you happen to be a receiver, your choice will be automatically implemented.

- If you happen to be a nonreceiver, your choice does not play a role.

- Your choice remains private and anonymous to other participants.

Click here to be reminded of how lottery tickets are distributed to all participants of this study.

✓ Remind me of the way lottery tickets are distributed.

Lottery tickets are distributed in two steps:

Step 1: The computer randomly selects 50 receivers and 50 nonreceivers. Each receiver gets 10 lottery tickets. Nonreceivers get no lottery tickets. No participant will learn whether he/she has been selected to be a receiver or a nonreceiver until the end of the study.

Step 2: Each participant decides privately whether he/she wants to GIVE or DON'T GIVE 3 lottery tickets to a nonreceiver for the case that he/she happens to be a receiver.

Please choose now:

◯ GIVE 3 lottery tickets to a nonreceiver.

◯ DON'T GIVE 3 lottery tickets to a nonreceiver.

Once you have made your decision, please tick below:

☐ This is my final answer. Please proceed.

Instructions for the Evaluation Stage (Stage 2)

Task 2
Evaluate choices in a similar situation
On the following screen, you will read the description of a hypothetical choice situation that is very similar to the choice situation you just faced: 100 individuals take part in a lottery that has the exact same structure as the lottery you just took part in. Similar to

your choice, each of these individuals has to decide whether to GIVE or DON'T GIVE 3 out of 10 lottery tickets to a nonreceiver.

In this new situation, however, you will NOT be asked to choose yourself. Instead, you will be asked to EVALUATE the different choices available to the other individuals. For each of the possible actions, you will have to decide whether taking that action would be

- "socially appropriate" and "consistent with moral or proper social behavior", or

- "socially inappropriate" and "inconsistent with moral or proper social behavior."

By socially appropriate, we mean behavior that most people agree is the "correct" or "ethical" thing to do.

All of the 99 other participants of today's study will evaluate the same choices in the same hypothetical situation. We will compare your evaluation with the evaluation of the 99 other participants. **If your evaluation is the same as the evaluation most frequently given by the other 99 participants, then you will receive an additional payment of GBP 2.00!**

Note: You and the other 99 participants will evaluate several choices. For the extra payment of GBP 2.00 we will select one of these choices at random. If you evaluate this choice the same way as most of the other 99 participants do then you will receive an additional payment of GBP 2.00. Each of your evaluations has the same chance to be selected for your payment. That is, you maximize your chances to earn GBP 2.00 by trying to always match the most common evaluation in your group.

Note: Your evaluation in Task 2 does NOT influence your chances to win the lottery! The lottery tickets for your group have been distributed in Task 1.

☐ I have read and understood the instructions and would like to proceed.

To show that you have read and understood the instructions, please answer the following control questions:

	True	False
You will receive GBP 2.00 if your evaluation is the same as the evaluation provided by most of the 99 other participants.	◯	◯
By socially appropriate, we mean behavior that most people agree is the "correct" or "ethical" thing to do.	◯	◯

Description of the Hypothetical Situation

Consider the following hypothetical situation: 100 other individuals take part in a lottery that has exactly the same structure as the lottery you took part in a few minutes ago. However, in this new lottery, **before anyone of the 100 individuals decides whether to choose GIVE or DON'T GIVE, a code of conduct will be set**. The code of conduct says whether everyone should choose GIVE ($\rightarrow$ RULE: GIVE) or whether everyone should choose DON'T GIVE ($\rightarrow$ RULE: DON'T GIVE). Only one of the two rules will be implemented.

In the choice situation you have to evaluate, the rule for the code of conduct will be determined in the following way:

Treatment StandardMajority:

- All 100 individuals who take part in the lottery are asked to vote for the rule (RULE: GIVE or RULE: DON'T GIVE) they prefer to have implemented as the code of conduct. The rule that receives more votes in total will be implemented as the code of conduct.

Treatment Pay4Vote:

- All 100 individuals who take part in the lottery are asked to vote for the rule (RULE: GIVE or RULE: DON'T GIVE) they prefer to have implemented as the code of conduct. The rule that receives more votes in total will be implemented as the code of conduct.

- However, only the votes of those participants who pay GBP 0.20 to make their vote count will be counted in the election.

Treatment MoneyOffer:

- All 100 individuals who take part in the lottery are asked to vote for the rule (RULE: GIVE or RULE: DON'T GIVE) they prefer to have implemented as the code of conduct. The rule that receives more votes in total will be implemented as the code of conduct.

- However, before the final votes are counted, all participants are offered an extra payment of £0.20 to vote for the rule that is opposite to what they originally wanted to vote for.

Treatment ExcludePoor:

- All 100 individuals who take part in the lottery are asked to vote for the rule (RULE: GIVE or RULE: DON'T GIVE) they prefer to have implemented as the code of conduct. The rule that receives more votes in total will be implemented as the code of conduct.

- However, only the votes of participants with a household income above GBP 40,000 will be counted in the election.

Once a rule has been set, each individual can decide privately and anonymously whether he/she wants to follow the rule or not.

This is the situation you will be asked to evaluate on the next screen. Please make sure to remember it will. In particular, **on the next screen, you will be asked whether it is socially appropriate to follow the rule under the circumstances it has come into force.**

☐ I have read and understood the instructions and would like to proceed.

To show that you have read and understood the instructions, please answer the following control questions: In the hypothetical choice situation, the code of conduct will equal... Please choose only one of the following: *[StdMajority, items appear in random order]*

◯ the rule that the majority of individuals voted for.

◯ the rule that was selected using a coin-flip by the computer.

◯ the rule that was selected by the researcher.

In the three malpractice treatments, the additional (correct) items were, respectively:

◯ the rule that the majority of those individuals who pay £0.20 voted for. *[Pay4Vote]*

◯ the rule that the majority of individuals finally voted for—after being offered GBP 0.20 to change their vote. *[MoneyOffer]*

◯ the rule that the majority of individuals with annual household income above GBP 40,000 voted for. *[ExcludePoor]*

The Evaluation Decision

Please evaluate: Choice in the presence of a code of conduct
Here is a reminder of how the rule for the code of conduct is determined: *[Example StdMajority]*

- All 100 individuals who take part in the lottery are asked to vote for the rule (RULE: GIVE or RULE: DON'T GIVE) they prefer to have implemented as the code of conduct. The rule that receives more votes in total will be implemented as the code of conduct.

StdMajority, Pay4Vote, MoneyOffer, ExcludePoor:

For each of the two possible rules (RULE: GIVE and RULE: DON'T GIVE), please indicate below how socially appropriate you believe it is to follow the rule and how socially appropriate you believe it is to not follow the rule. Remember that you will earn money (GBP 2.00) if your evaluation is identical with the most common evaluation given by the other 99 participants of this study.

StdMajority, Pay4Vote, MoneyOffer, ExcludePoor:

RULE: GIVE. Below you see the choices available for each individual if RULE: GIVE is implemented as the code of conduct. Please indicate how socially appropriate you believe each choice to be.

	Very socially inappropriate	Socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Socially appropriate	Very socially appropriate
Follow the rule and GIVE.	○	○	○	○	○	○
Don't follow the rule and DON'T GIVE.	○	○	○	○	○	○

RULE: DON'T GIVE. Below you see the choices available for each individual if RULE: DON'T GIVE is implemented as the code of conduct. Please indicate how socially appropriate you believe each choice to be.

	Very socially inappropriate	Socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Socially appropriate	Very socially appropriate
Follow the rule and DON'T GIVE.	○	○	○	○	○	○
Don't follow the rule and GIVE.	○	○	○	○	○	○

All treatments:

Feedback Screen and End of the Study

Thank you very much for your participation. Your evaluations have been saved.

Did you feel that it makes a large difference whether to choose GIVE or DON'T GIVE or were you very much indifferent between choosing any of the two?

◯ Was very much indifferent/did not care.

◯ Felt that it makes a large difference.

Below, you have the opportunity to leave a more general personal comment. We greatly appreciate your feedback.

Please make sure to click "Submit" and then the link on the next page in order to prove that you have completed the study! Thank you!

Treatment variation ExoRule×MajVote

The instructions in treatment variation ExoRule×MajVote were identical to the StdMajority treatment, except for the following parts:

Description of the Hypothetical Situation

- To determine the rule, a coin is flipped by the computer. If the coin lands heads (50% chance), RULE: GIVE is implemented. If the coin lands tails (50% chance), RULE: DON'T GIVE is implemented. <u>Note:</u> Whether RULE: GIVE or RULE: DON'T GIVE is implemented depends purely on chance. That is, the rule is random.

In ExoRule×MajVote, we asked the following two control questions:

If RULE: GIVE is implemented as the code of conduct, this is because...

○ ... the majority of individuals who take part in the lottery prefer RULE: GIVE

○ ... by chance, the coin flip landed heads.

○ ... RULE: GIVE is the only rule that can be selected.

If RULE: DON'T GIVE is implemented as the code of conduct, this is because...

○ ...the majority of individuals who take part in the lottery prefer RULE: DON'T GIVE

○ ... by chance, the coin flip landed tails.

○ ... RULE: DON'T GIVE is the only rule that can be selected.

In ExoRule×MajVote, the information given on the evaluation decision page differed from the other treatment variations:

The Evaluation Decision

Please evaluate now:
How socially appropriate is it to follow/not follow **RULE: GIVE** if it is implemented?
How socially appropriate is it to follow/not follow **RULE: DON'T GIVE** if it is implemented?
Below, we ask you to answer these questions **under two different scenarios**:

- **Scenario 1:** The majority of individuals who take part in the lottery prefer RULE: GIVE. That is, in a majority vote, RULE: GIVE would win against RULE: DON'T GIVE.

- **Scenario 2:** The majority of individuals who take part in the lottery prefer RULE: <u>DON'T</u> GIVE. That is, in a majority vote, RULE: DON'T GIVE would win against RULE: GIVE.

In total there are <u>4 possible situations</u> that you have to evaluate.

Please evaluate each situation according to what you think <u>most people</u> find socially appropriate <u>in that particular situation</u>

Remember that you will earn money (£2.00) if your evaluation is identical with the most common evaluation given by the other 99 participants of this study.

ExoRule×MajVote: example of the rating decision table

Evaluation 1:

Please evaluate the following situation:

- The coin flip lands heads. **RULE: GIVE is implemented.**

- **The majority prefers RULE: GIVE.** That is, in a majority vote, RULE: GIVE would win against RULE: DON'T GIVE.

How socially appropriate is it to follow/not follow RULE: GIVE in this case?

	Very socially inappropriate	Socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Socially appropriate	Very socially appropriate
To follow the rule and GIVE.	○	○	○	○	○	○
To not follow the rule and DON'T GIVE.	○	○	○	○	○	○

Example Screenshots: StdMajority

Figure 2.8: Screenshot: Description of the evaluation decisions 1

Figure 2.9: Screenshot: Description of the evaluation decisions 2 (*StdMajority*)

Figure 2.10: Screenshot: Evaluation decisions (*StdMajority*)

Example Screenshots: NoRule

Figure 2.11: Screenshot: Description of the Evaluations 1

Figure 2.12: Screenshot: Description of the Evaluations 2

If a participant decides to
GIVE
and happens to be receiver:

Participant keeps: Nonreceiver gets:
7 tickets   3 tickets

If a participant decides to
DON'T GIVE
and happens to be a receiver:

Participant keeps: Nonreceiver gets:
10 tickets   0 tickets

- **Note that everyone makes this decision without knowing whether he/she is a receiver or nonreceiver of lottery tickets.** In the case that the participant is a non-receiver, his/her decision to GIVE or DON'T GIVE does not play a role. The number of lottery tickets this participant holds then depends on the choice of another participant.

On the following screen, we will ask you to evaluate <u>how socially appropriate</u> you believe each of the two possible actions (GIVE and DON'T GIVE) to be:

You will have to decide whether you think that taking action GIVE would be

- "socially appropriate" and "consistent with moral or proper social behavior", or
- "socially inappropriate" and "inconsistent with moral or proper social behavior"

and whether you think that taking action DON'T GIVE would be

- "socially appropriate" and "consistent with moral or proper social behavior", or
- "socially inappropriate" and "inconsistent with moral or proper social behavior".

By socially appropriate, we mean behavior that most people agree is the "correct" or "ethical" thing to do.

Please note: How you evaluate actions GIVE and DON'T GIVE does NOT influence your chances to win the lottery. In your group, lottery tickets will only be distributed by the computer and not by anyone else.

Figure 2.13: Screenshot: Evaluations of Giving Decisions 1

Your Evaluation

Your answers to the control questions were correct.

Below, we ask you to evaluate actions GIVE and DON'T GIVE in the hypothetical choice situation explained on the previous screen.

For each of those two actions, we ask you to decide whether you think that taking that action would be

- "socially appropriate" and "consistent with moral or proper social behavior", or
- "socially inappropriate" and "inconsistent with moral or proper social behavior".

By socially appropriate, we mean behavior that most people agree is the "correct" or "ethical" thing to do.

With your evaluation you can earn an additional £2.00 to your base payment!

Whether you receive the additional £2.00 is determined as follows:

- All of the 99 other participants of today's study evaluate the same actions in the same hypothetical situation.
- We will compare your evaluation with the evaluation of the 99 other participants. If your evaluation is the same as the evaluation most frequently given by the other 99 participants, then you will receive an additional payment of £2.00.*

*For the extra payment of £2.00 we will select one of the two evaluations (GIVE or DON'T GIVE) at random. Both of your evaluations have the same chance to be selected for the extra payment. You maximize your chances to earn an additional £2.00 by trying to always match the most common evaluation among the other 99 participants. Neither your base payment of £1.10, nor your chances to win the cash prize of £100.00 in the lottery is affected by whether you receive the extra payment.

Figure 2.14: Screenshot: Evaluations of Giving Decisions 2

✱Evaluation 1. Please indicate how socially appropriate you believe action **GIVE** to be:

	Very socially inappropriate	Socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Socially appropriate	Very socially appropriate
Action **GIVE** is	○	○	○	○	○	○

❷ Remember that you will earn £2.00 if your evaluation is identical with the most common evaluation given by the other 99 participants of this study!

✱Evaluation 2. Please indicate how socially appropriate you believe action **DON'T GIVE** to be:

	Very socially inappropriate	Socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Socially appropriate	Very socially appropriate
Action **DON'T GIVE** is	○	○	○	○	○	○

❷ Remember that you will earn £2.00 if your evaluation is identical with the most common evaluation given by the other 99 participants of this study!

Chapter 3

Identity Priming, Cognitive Spillovers and Charitable Giving

Note: This chapter was written jointly with Luca Henkel and Frederik Schwerter. All three authors contributed to the design and implementation of the research, analysis of the results, and writing of the manuscript.

3.1 Introduction

Fundraising managers design charitable campaigns with the overall goal of maximizing the amount of money raised. The literature on the demand side of charitable giving shows that several aspects of the decision environment can influence individual giving decisions, and that many of these aspects can be actively shaped by campaign designers through targeted behavioral interventions.[1] While many interventions alter the decision problem a potential donor faces by changing the economic constraints and incentive structure, other interventions are more subtle and affect giving by strategically influencing the way potential donors cognitively process information and drawing attention to certain features of the decision environment. The main advantage of the latter approach is its low implementation cost and ease of scalability, which has led to its widespread adoption by practitioners. However, it is often difficult to accurately predict which associations

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[1]Working together with charities, scholars have explored various fundraising strategies based on different mechanisms to increase donations, such as seed money (List and Lucking-Reiley, 2002), lottery incentives (Landry et al., 2006), matching mechanisms (Karlan and List, 2007; Eckel and Grossman, 2008; Huck and Rasul, 2011; Karlan et al., 2011; Adena and Huck, 2017), gifts (Falk, 2007), rebate rules (Spencer et al., 2009), and fundraising appeals (List et al., 2021). Literature on the *social* decision environment has studied peer pressure (Landry et al., 2006; 2010; Meer and Rosen, 2011; Meer, 2011; DellaVigna et al., 2012; Andreoni et al., 2017) and social information (Frey and Meier, 2004; Martin and Randal, 2008; Shang and Croson, 2009; Kessler, 2017) and social identity (Shang et al., 2008; Charness and Sutter, 2012; Kessler and Milkman, 2018).

will be triggered by a particular intervention.

We study the effect of an intervention that features a group appeal on charitable giving in a large-scale natural field experiment. Unlike standard appeals, which focus on the donor as an individual, group appeals focus potential donors' attention on the fact that they are giving together with others. They are widely used by practitioners and organizations, and previous literature has found evidence that group appeals increase giving. We provide evidence that group appeals can backfire: in our experiment, the group appeal causes a significant reduction in donations. Using a tightly controlled online experiment, we show that the intervention fails because the group appeal triggers cognitive material that is detrimental to the actual target behavior.

In Section 2, we describe the natural field experiment. We work with a well-known nonprofit organization dedicated to helping children in need around the globe. For the experiment, we vary the details of a solicitation letter that the organization's regular donors receive. In total, 105,318 donors received one of two versions of the letter: Donors in the treatment group received a group appeal letter ($GROUP$). The letter's headline emphasized that donors are making their decision as members of potentially "generous neighborhoods". In particular, the text highlighted that donations would be made "as a team with other donors" from their postal code region. In contrast, the control condition received a standard individual appeal letter ($CONTROL$) that made no reference to the subjects' region or to other donors. Building on previous work examining the effects of a similar intervention on giving, local communities are typically associated with prosocial behavior, leading us to hypothesize that the group appeal would increase giving relative to the individual appeal by priming a facet of a donor's identity associated with helpfulness and generosity (Kessler and Milkman, 2018).

The main result of the field experiment is intriguing: We find substantial negative effects of our intervention on both the probability of donating and the revenue per letter. The group appeal in the letter significantly decreased donors probability to donate by 33% and reduced the total amount of donations the charity collected by 43%. Hence, contrary to expectations, the group appeal decreased giving.

To shed light on possible mechanisms underlying our findings, we conduct an online experiment and investigate potential cognitive effects of our intervention (Section 3). In the experiment, subjects first receive either a group appeal or an individual appeal, similar to the field experiment. Subjects then allocate money in a donation decision between a local and an international charity. Again, we observe a negative effect of the group appeal on giving to the international charity, as subjects shift their donation to the local charity. Next, we conduct two additional conditions that include open-ended questions before the decision but after the appeals. We find that the group appeal shifts attention away from global towards local issues, which fully mediates the effect of the group appeal on giving

behavior. We then show that the same effects on attention and behavior occur when we compare two primes, one of which contains similar cues to local matters as the group appeal, but without group framing. These results provide evidence that the effect in our field experiment is indeed cognitively rooted and point to an attention-based rationale.

Taken together, our results show that an intervention that relies on cognitive effects can severely backfire if it triggers cues that are detrimental to the target behavior. In particular, priming a group identity based on regional affiliation can shift donors' attention away from global issues to local ones. Such "cognitive spillovers" produced by interventions that induce people to reallocate scarce cognitive resources across domains may have positive effects on giving to local causes but, as observed in our field experiment, negative effects on giving to global causes (Altmann et al., 2022).

In Section 4, we discuss alternative explanations for our results. In particular, we argue that the negative treatment effect is unlikely to be driven by norms associated with local communities that are detrimental to giving, since the group appeal explicitly states that groups are formed among regular donors from the community who arguably share similar values in favor of donating to the organization's causes. Furthermore, we can confidently rule out that responsibility diffusion due to increased salience of other donors led subjects to refrain from bearing the costs of a donation themselves. In another project in the same setting, information about other donors leads to significant positive effects on individual giving, contradicting a negative effect of responsibility diffusion. We also show that the negative effects of the group appeal on giving are short-lived, i.e., they do not affect the organization's future solicitations. This finding also addresses the concern that donors update their beliefs about the quality of the organization or its projects based on the group appeal, which would lead to persistently lower donations.

Our study contributes to the existing literature on interventions in charitable giving and to a literature on the cognitive effects of policy interventions. Prominent economic theories posit that social identity is an important driver of human behavior (Akerlof and Kranton, 2000; Bénabou and Tirole, 2011; Bonomi et al., 2021). These theories are deeply rooted in psychological accounts of the matter (Tajfel and Turner, 1979). Empirical research has shown that a strong sense of belonging to a group of people can lead individuals to make different choices than they would otherwise and that this can be successfully exploited by decision architects (e.g. Benjamin et al. (2010), Cohn et al. (2014); see Charness and Chen (2020) for a review).[2] Identity has also been shown to drive behavior in the area of charitable giving: Shang et al. (2008) examine the role of identity congruence in social influence on charitable giving and show that donors are influenced by

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[2]Field evidence on the relevance of social identity for economic behavior has been documented in various applications, including worker productivity in firms (Hjort, 2014; Ghosh, 2022), consumption choices (Atkin et al., 2021), labor market supply (Oh, 2023), and stock market participation (Henkel and Zimpelmann, 2023).

the behavior of a predecessor who shares their identity. In a minimal group experiment, Charness and Holder (2019) find that experimental induction of group identity combined with intergroup competition has positive effects on giving in the laboratory. Sánchez (2022) find similar results and highlight the role of identity strength for group appeals to influence decisions in the laboratory. Chapman et al. (2020) provide survey evidence for identity motives in charitable giving based on a global donor survey. Li et al. (2017) use a laboratory experiment to examine how community identity affects contributions to local public goods and show that the effect is sensitive to community context. Most closely related to our study is Kessler and Milkman (2018), who show that priming a donor's identity facet based on regional affiliation can increase giving. Importantly, their partner organization is widely associated with projects at the regional and national levels, but not abroad. We add to this literature by providing, to our knowledge, the first field evidence of a negative effect of a group appeal intervention on giving. We argue that the cognitive material triggered by an intervention is not always as straightforward as one might think, and show that our findings are cognitively grounded. In doing so, we relate our findings to a vibrant recent literature on the cognitive foundations of economic decision making, which focuses on the allocation of scarce cognitive resources in economic decision making and how this may be affected by policy interventions (Altmann et al., 2022; Maćkowiak et al., 2023; Enke, 2024).

Section 5 summarizes our findings and contribution and concludes.

3.2   Field experiment

When planning a fundraising campaign, fundraising managers need to carefully design their appeal to potential donors. One wide-spread strategy is to appeal to individual donors as part of a larger group of people (see Appendix 3.6.3 for real-world examples). Recent research has shown that appealing to donors as part of a group indeed has the potential to significantly increase giving (Kessler and Milkman, 2018; Charness and Holder, 2019). Proposed mechanisms are that the appeal primes a facet of their identity associated with a norm of generosity or induces donors to not want to "let their team down." We conduct a large-scale controlled natural field experiment to examine the effects of a broadly applicable appeal strategy on giving based on a donor's membership in a local community.[3]

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[3]The field experiment received approval from the Ethics Committee of the Faculty of Management, Economics, and Social Sciences at the University of Cologne and was preregistered at the AEA RCT registry (#7724 and #7962). The instructions are available in Appendix 3.6.4.

3.2.1 Setting and experimental design

Setting. We partner with a well-known nonprofit organization that operates in 192 countries and focuses on humanitarian and development aid for children. Accordingly, donations to the organization serve a global purpose not tied to a specific country or region. The charity is organized through national committees, each of which is established as an independent local nonprofit. We work with the German committee, which regularly uses fundraising letters to solicit donations from private donors for its global cause. For the experiment, we randomize the content of the letters as part of a regular fundraising round for the charity. The purpose of the letter is to motivate individuals who actively and regularly donate to the charity to sign another pledge in addition to their existing one. Specifically, donors are asked to sign an Emergency Response Sponsorship, which enables the organization's staff to quickly and effectively help children in emergency situations around the world. The one-page solicitation letters are sent in three waves at different times between June and August 2021.

Sample. Our sample frame consists of 105,318 individuals in Germany. Each individual has donated to the charity in the past and has made an ongoing commitment to donate a certain amount of money to the organization on a regular basis. All individuals have agreed to be contacted by the organization for future fundraising rounds and are part of the charity's pledge file. On average, individuals in our sample donate €172 per year to the organization as part of their regular commitment (median €120) and have been donors for an average of 16 years. Overall, 51% are male and 19% were originally acquired through face-to-face fundraising, with the remainder acquired through other acquisition channels. In addition, the average population of the zip code they live in is 18,540, with 13% living in a city of >1 million.

Treatments. As part of the experiment, we randomly assigned individuals to receive letters that differed slightly in wording. Specifically, donors assigned to the treatment group received a group appeal (*GROUP*), while donors assigned to the control group received a standard individual appeal (*CONTROL*). The wording was varied in two places: (a) the headline at the top of the letter, and (b) the last paragraph of the letter, which repeated the headline in slightly different words. In all other respects, the letters were identical.

In the *CONTROL* condition, the headline was "Emergency Helpers needed" and the prompt was "Help us now." In the *GROUP* treatment, the headline read "Emergency Helpers needed from <ZIP Code + Region>" and further emphasized that donors were making their decision as members of potentially "generous neighborhoods."[4] The last

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[4]Because potential donors agreed to be contacted by direct mail fundraising campaigns, the organi-

paragraph of the letter repeats the appeal with different wording. Most importantly, the wording in the *GROUP* treatment emphasized that donations would be made "as a team with other donors from <ZIP Code + Region>". In contrast, the *CONTROL* treatment made no reference to the subjects' region or other donors. The *GROUP* treatment had a further variation in which a subset of the letters contained an explicit notion of group competition that was added to the wording of the *GROUP* treatment. For our main analysis, we pool both variations of the treatment and return to the analysis of differences within the treatment in section 3.4.

The rest of the letter, written and designed by the organization, followed their typical fundraising practices, drawing attention to pressing development issues and the work of the organization's staff in the field. The letters also included a link and QR codes that directed recipients to the organization's general home page (see the Appendix section 3.6.4 for the exact wording of the letters). Of the individuals in our sample, 28% had previously provided their email address to the organization. They received an additional email two weeks after the solicitation letters were sent. The email was a digitized version of the solicitation letter, with the subject line being the treatment-specific appeal.

Treatment assignment. In total, 38,414 subjects (36% of the sample) were part of the *CONTROL* condition, while 66,904 subjects (64%) were part of the *GROUP* treatment. Within the latter group, 38,576 donors received the mere group appeal (*GROUP:NOCOMP*) and 28,328 donors received the *GROUP:COMP* letter version. When assigning treatment status, we adjusted for potentially relevant donor baseline characteristics according to Athey and Imbens (2017). Subjects' annual donation amount, whether they provided their email, whether they were recruited face-to-face, their gender, the population of their zip code, and the years they had been a donor served as strata variables. These variables had previously been found to be associated with giving behavior in our sample, so we randomized within strata for each of the three waves. To check the effectiveness of randomization in generating treatment and control assignments orthogonal to our baseline information, see table 3.3 in the appendix. As expected, both conditions are balanced on the baseline variables.

3.2.2 Results

Variables. For the analysis, we focus on two outcome variables: first, we examine the treatment effect on donation incidence, a binary variable that equals one if a subject signs a pledge as a result of the letter and zero otherwise (extensive margin). Second,

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zation had address information readily available. Since the donor's address is also printed in the address field at the top of the letter, forming teams based on regional affiliation should not seem strange to donors.

we examine the average amount donated per subject, which captures the overall effect of the letter on giving behavior (extensive + intensive margin). Our pledge request is for a regular giving commitment, so donation size refers to the annual amount a subject gives to the charity.

Main result. Table 3.1 presents our results. In response to the letter, 0.28% of subjects in the *CONTROL* condition sign the annual pledge. This percentage drops to 0.19% in the *GROUP* condition (column 1), an effect that is robust to the inclusion of controls (column 2). Thus, the group appeal in the letter decreased the probability that subjects would donate by 33%, a significant decrease ($p = 0.003$, two-sample test of proportions). Moreover, the group appeal decreased the total amount of donations. While subjects in *CONTROL* donate an average of €0.66 annually, they donate €0.38 in *GROUP*, a decrease of 43% (columns 5 and 6). Extrapolating these averages, the result implies that the charity could have raised €70,019 in annual donations from donors with its standard letter, but ended up raising only €50,975. Using the group appeal for the entire group of subjects would have further reduced the amount to €40,040. Thus, the small change in the wording of the letter had a significant impact on the amount of money the charities raised.

Table 3.1: Field experiment: Main results

	Dependent variable:			
	Donation Probability		Donation Size	
	(1)	(2)	(3)	(4)
GROUP	−0.093***	−0.086***	−0.285***	−0.267**
	(0.032)	(0.032)	(0.110)	(0.107)
Constant (*CONTROL*)	0.281***		0.665***	
	(0.027)		(0.101)	
Controls	No	Yes	No	Yes
Observations	105,318	105,318	105,318	105,318

Notes: The table shows OLS estimates. The dependent variable in columns (1) and (2) is a variable equal to 100 if a subject signs the annual pledge solicited in the fundraising letter and zero otherwise and in columns (3) and (4) the amount in Euro that is pledged. "*GROUP*" is equal to 1 when the subject is part of the *GROUP* treatment and zero if the subject is part of the *CONTROL* condition. Additional independent variables ("Controls") added in columns (2) and (4) are subjects' annual donation amount, whether they provided their email, whether they were recruited face-to-face, their gender, the population of their ZIP code, the years they have been a donor as well as wave fixed effects. See Appendix Table 3.4 for all coefficients. Robust standard errors in parentheses. Significance levels: *p<0.1, **p<0.05 and ***p<0.01.

Robustness.   We perform several exercises to ensure that the observed treatment effect is robust to the choice of regression model and outliers. First, we rerun the OLS regression on the probability of donation using a probit regression, replicating the negative effect (see Appendix table 3.5). Second, we rerun the regression on total donations (i) excluding outliers identified by a Cook's distance of $1/4N$, and (ii) using log donations. We obtain a significant negative treatment effect in both cases (see Appendix Table 3.6).

3.3   Online experiment

The previous section showed the negative effect of group appeals on giving behavior in a natural field experiment. This may be surprising in light of previous research that has found positive effects of group appeals on charitable giving. One possible explanation for the negative treatment effect is based on a reallocation of potential donors' scarce cognitive resources in response to the intervention. In particular, appealing to donors as members of a local community may have activated associations with their regional affiliation and focused their attention on local issues related to their region or country. At the same time, it may have diverted cognitive resources away from the importance of global issues such as poverty alleviation in developing countries. Since the cause of giving was a global one - reducing child poverty and suffering, especially in the developing world - the diversion of attention may explain the decline in giving.

To test this mechanism, we conduct an online experiment. In the experiment, we first attempt to replicate the negative effect of the group appeal and then investigate its attentional origins. We use an online experiment because it allows us to (i) tightly control the decision problem and the corresponding trade-offs a donor faces, (ii) measure donors' attention, and (iii) vary the prime used in the fundraising appeal.[5]

3.3.1   Experimental design

Decision.   In the experiment, subjects face a donation decision. They are given £10 to divide between the British Red Cross and the International Red Cross in increments of 2. The British Red Cross provides health care and disaster relief to people living in Britain, and the International Red Cross does the same around the world. We chose the Red Cross as the recipient because it is a well-known charity, and it allows us to vary the location of potential recipients while keeping the cause of the donation constant. The choice thus involves a direct tradeoff between donating to a local cause and donating to

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[5]The online experiment obtained approval from the German Association for Experimental Economic Research and was preregistered at aspredicted (#181776). The instructions are available in Appendix 3.6.4.

a global cause. For convenience, in the following we code higher values as the amount of the donation made to the International Red Cross.

Treatments. Our design includes three treatment comparisons, each with two conditions. Thus, there are a total of six conditions to which subjects are randomly assigned. The conditions vary the information that subjects receive before making the donation decision.

The first treatment comparison includes the conditions *Group prime* and *Control*, in which we experimentally vary parts of the text that introduces subjects to the decision context. In the *Group prime* condition, we tell subjects that they form a group with other people in their zip code area, closely mimicking the group appeal of the field experiment. Specifically, subjects receive the following introduction to the decision context:

> This survey is fielded to people who live in [UK postcode region] and are active on Prolific. You and the other people from [UK postcode region] who participate in this Prolific survey form a group. Your group faces a decision, which will be explained on the next pages.

In contrast, in the *Control* condition the decision context is introduced without the group framing:

> In this survey, you face a decision, which will be explained on the next pages.

Afterwards, subjects receive information about their endowment and the incentives, and then make the donation decision. We maintain the group versus neutral framing throughout. Thus, as in the field experiment, subjects are asked to donate either in a neutral framing or in the presence of a group appeal.

In our second treatment comparison, which contains the conditions *Free form group prime* and *Free form control*, we build on the previous design and add an open-ended text question. The question is inserted after the introductory text, but before the donation decision. That is, subjects in *Free form group prime* receive the same group appeal as in *Group prime*, while subjects in *Free form control* receive the same neutral introduction as in *Control*. Importantly, the introductory text mentions only the endowment and the incentives, not the recipients. Accordingly, we ask them to state in their own words, if they could choose any person or group as the recipient of the donation, (i) whom they would like to help, and (ii) where the recipient is located. After providing their answer in two open-ended text boxes, subjects are presented with the donation decision. This procedure allows us to capture subjects' considerations that are on top of their mind immediately after receiving either the group appeal or the neutral introduction. By making the question open-ended, we obtain a detailed and uninterrupted lens into respondents' thoughts and considerations (Haaland et al., 2024).

In the third comparison, *Free form local prime* and *Free form global prime*, we test whether we can replicate the cognitive activation and its effect on behavior of the group prime without the group framing. The setup is the same as in the previous treatment comparison with one difference: instead of the group appeal versus neutral introduction, subjects receive information about one of the two recipients of the decision. We also tell them (truthfully) that we randomly select the recipient with equal probability. In *Free form local prime* they receive the information that "One of the recipients is a charity that is operating locally in Britain.", while in *Free form global prime* they receive the information that "One of the recipients is a charity that is operating in regions affected by war." Afterwards, they are presented with the same open-ended question as in the previous treatment comparison, and then make their donation choice. Thus, we can measure the effect of these primes on both the open-ended responses and the donation decision. This allows us to directly test whether the prime directs attention toward a specific target group and whether it influences donation decisions.

Sample. In total, 1,794 subjects participated in the experiment: 300 in *Group prime*, 297 in *Control*, 296 in *Free form group prime*, 300 in *Free form control*, 305 in *Free form global prime*, and 296 in *Free form local prime*. Participants were adults living in the United Kingdom and were recruited using Prolific. We chose Prolific due to its status as one of the leading market research companies used in social science research and because we could pre-screen and target participants based on the UK zip code area in which they live.[6] All experiments were pre-registered at aspredicted.org.[7] We used oTree (Chen et al., 2016) to program the graphical user interface. The instructions can be found in the Appendix 3.6.4. Subjects spent a median time of 3 minutes in the experiment and received £0.5 as compensation, equivalent to an hourly wage of $13. We implemented the donation choice of one out of 25 subjects (between-subject random incentivized system).

3.3.2 Results

In the following, we discuss our results for each of the three treatment comparisons. For a summary of the main results, see Figure 3.1. For the distribution of decisions across all conditions, see Appendix Figure 3.2.

***Group prime* versus *Control*.** In *Control*, subjects donate on average £4.96 to the International Red Cross. In contrast, in *Group prime*, subjects donate on average £4.52.

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[6]At the time of the experiment, this feature was not available in other countries.

[7]Details can be found under `https://aspredicted.org/K32_KCJ`. The preregistration includes details on the experimental design, the planned sample size, exclusion criteria, hypotheses, and the main analyses.

The group appeal thus decreases donations to a global cause relative to a national one by 9%, a significant decrease ($p = 0.039$, t-test).

***Free form group prime* versus *Free form control*.** Does the prime really shift attention to local matters? The analysis of subjects' open-ended responses shows that in the absence of the group appeal (*Free form control*), 67% of subjects mention a local recipient, while 24% mention a global recipient. When the group appeal is provided before the open-ended question, 90% of subjects mention a local recipient and only 6% mention a global recipient.[8] Thus, the appeal increases attention to local causes and decreases attention to global causes (both cases $p < 0.001$, two-sample t-test of proportions). Are the mentions in the question related to choices? We find that subjects who mention a local recipient donate £2.17 (34%) less to the International Red Cross than those who do not ($p < 0.001$, t-test). Subjects who mention a global recipient donate £2.56 (61%) more than those who do not ($p < 0.001$, t-test). Mention in the open-ended question thus predicts voting behavior. Consequently, we again find a negative effect of the group appeal on donations to the global recipient: subjects' donations decrease from £4.82 in *Free form control* to £4.30 in *Free form group prime*, again a significant decrease ($p = 0.021$, t-test). Interestingly, the treatment effect is entirely mediated by the open-ended responses: when controlling for either local or global mentions in an OLS regression, there is no longer a treatment effect (see Panel A of Appendix Table 3.10 for details).

***Free form local prime* versus *Free form global prime*.** Finally, we test whether we can replicate the previous results with a prime that directs attention to local issues without group framing. Telling subjects that one of the recipients is a charity operating in Britain (*Free form local prime*) leads 87% of subjects to mention a local recipient, while 14% mention a global recipient. Telling them instead that one of the recipients is a charity working in war zones reduces local mentions to 34%, while global mentions increase to 65% (both cases $p < 0.001$, two-sample test of proportions). Again, mentions predict behavior, with local mentions associated with 29% lower international donations and global mentions associated with 45% higher international donations (both $p < 0.001$, t-test). Importantly, we find a treatment effect on behavior: subjects donate 14% or £0.77 less in *Free form local prime* compared to *Free form global prime* ($p = 0.001$, t-test). As before, the treatment effect is mediated by the open-ended responses (see Appendix Table 3.10 Panel B).

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[8]Two research assistants, blinded to treatment status and hypotheses, coded the questions. Agreement was 97%; disagreements were resolved by the researchers. See Appendix Table 3.9 for details.

Figure 3.1: Online experiment: Main results
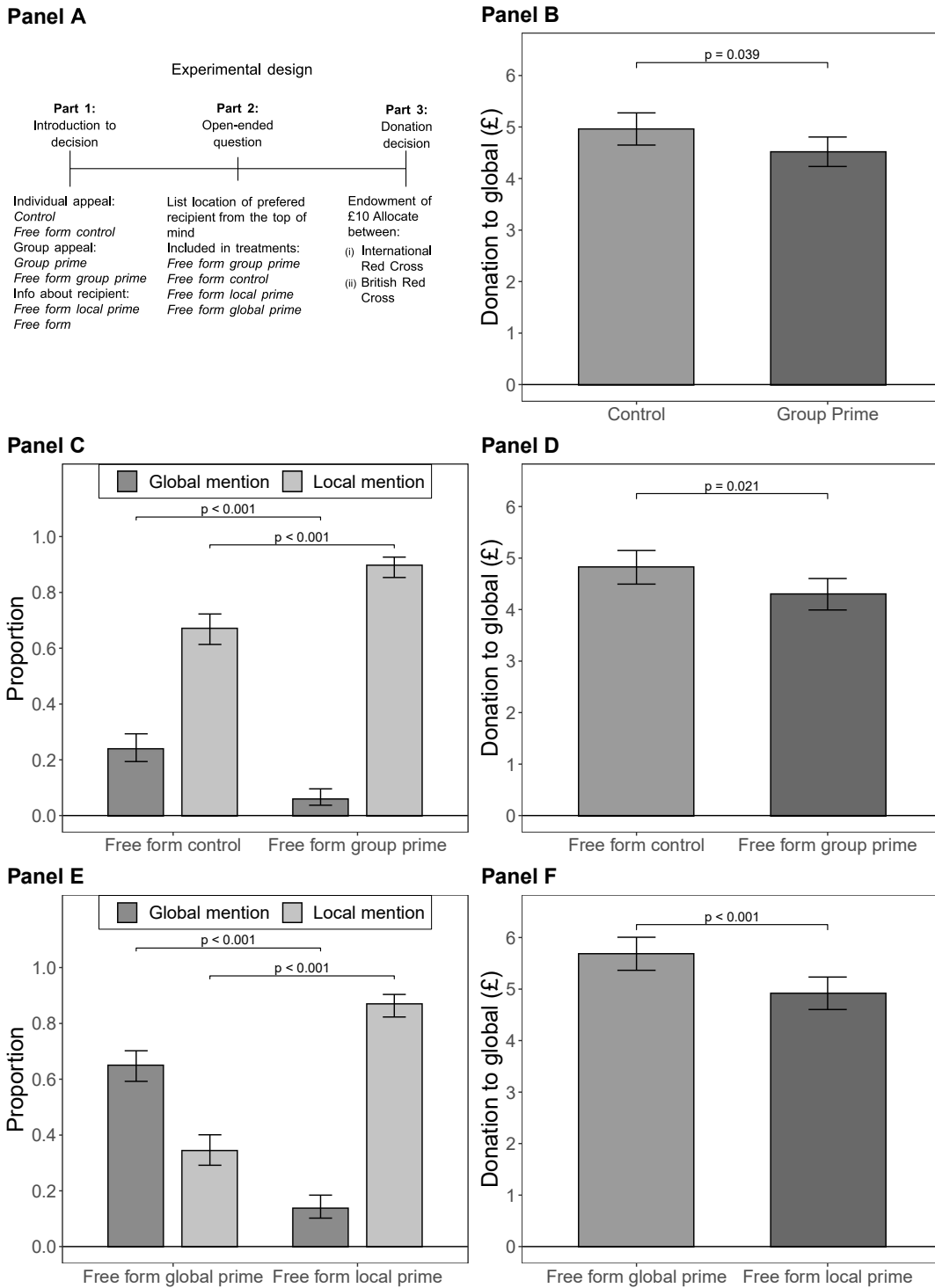
Notes: **Panel A** shows a summary of the experimental design. **Panel B** displays the treatment effect of the *Group prime* condition on donation behavior. The outcome variable denotes the amount of money (out of £10) that subjects allocate to the International Red Cross instead of the British Red Cross. **Panel C** displays the effect of the *Free form group prime* condition on open-ended text responses. "Global mention" and "Local mention" are indicator variables equal to one if the text response contains a globally located recipients and a locally located recipient, respectively. **Panel D** displays the treatment effect of the *Free form group prime* condition on donation behavior. **Panel E** displays the effect of the *Free form local prime* condition on open-ended text responses and **Panel F** its effect on donation behavior. Error bars indicate 95% confidence intervals. P-values are obtained from two-sample tests of proportions.

3.4 Discussion

By using conceptual priming, decision architects typically aim to activate specific associations (e.g., a particular self-concept) and memory cues. However, it is often not clear ex ante which associations are triggered by a particular prime. As a result, an intervention may activate different cognitive material than expected and affect individuals' allocation of scarce cognitive resources in unintended ways. This explanation is based on the intuition that people allocate a fixed amount of attention to different domains. As a consequence, directing cognitive resources to one domain may cause people to withdraw attention from another domain. Such 'cognitive spillovers' may have led to less active engagement in the area targeted by the donation request, resulting in fewer positive donation decisions (Altmann et al., 2022). Combining the results of the field and online experiments suggests that the group appeal used in our intervention had such unintended cognitive effects. In particular, the appeal seems to have drawn attention to aspects that were detrimental to the target behavior. The results of our online experiment help to explain the stark difference in the influence of a group appeal in our setting and the setting of Kessler and Milkman (2018). In their setting, since the charity is soliciting donations for the American Red Cross, focusing Americans' attention on local issues through a local identity appeal is consistent with the cause of the donation. In our case, the focus of attention is not aligned with the cause, as it shifts attention from global to local issues. Below, we discuss several alternative explanations that could explain the documented treatment effect independently of attention.

Social norms. A possible alternative explanation for why our intervention had a negative effect on giving is that the specific group was poorly chosen for the given setting. One could argue that the identity facet made salient by the prime is not associated with a norm of generosity toward children in need around the world, but with another norm that is detrimental to the target behavior. Note, however, that the appeal explicitly states that the subjects form a group with other regular donors. It is plausible to assume that, relative to the general population, there is a prevailing norm of universal generosity among regular donors, making it unlikely that social norms drive our main result.

Furthermore, if conformity to identity-based norms were driving our results, we should expect the intervention to be more effective for donors who are hypothesized to identify most strongly with the group.[9] We can test this hypothesis in the field experiment. We provide two pieces of evidence against it by showing that our results are robust to plausible variations in identity strength. First, our field experiment included an exoge-

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[9]Previous research has shown that the effectiveness of identity-based norm primes varies as a function of the strength of the individual's identification with the invoked identity (Eckel and Grossman, 2005; Goette et al., 2012).

nous manipulation of identity strength within the *GROUP* treatment. In total, 28,328 of the 66,904 subjects who were part of the treatment were assigned to the *GROUP: COMP* treatment. Here, we added an explicit intergroup competition to the group appeal through a neighborhood tournament.[10] Although we would expect this additional feature to increase identity strength and thus treatment effects, it did not have a significant effect on giving beyond the effect of the mere group appeal (labeled here *GROUP: NOCOMP*), see Table 3.2. Second, according to Kessler and Milkman (2018), people living in smaller regions care more and identify more strongly with their local community. Thus, if the negative treatment effects were driven by salient social norms, we should observe a stronger negative effect in smaller regions. To test this, we interact our treatment dummy with the logarithm of the population size of the donor's region. As Appendix Table 3.8 shows, we find no evidence that the negative group appeal effect is stronger in smaller regions. If anything, the effect goes in the opposite direction, being stronger in larger regions, although this effect is only marginally significant in one of the specifications.

Table 3.2: Field experiment: Effect of explicit group competition on giving within the *GROUP* condition

| | Dependent variable: | | | |
| | Donation Probability | | Donation Size | |
	(1)	(2)	(3)	(4)
GROUP:COMP	−0.021	−0.011	−0.054	−0.030
	(0.034)	(0.036)	(0.088)	(0.091)
Constant (GROUP:NOCOMP)	0.197***		0.403***	
	(0.023)		(0.060)	
Controls	No	Yes	No	Yes
Observations	66,904	66,904	66,904	66,904

Notes: The table shows OLS estimates. The dependent variable in columns (1) and (2) is a variable equal to 100 if a subject signs the annual pledge solicited in the fundraising letter and zero otherwise and in columns (3) and (4) the amount in Euro that is pledged. "*GROUP:COMP*" is equal to 1 when the subject is part of the *GROUP:COMP* treatment and zero if the subject is part of the *GROUP:NOCOMP* condition. Additional independent variables ("Controls") added in columns (2) and (4) are subjects' annual donation amount, whether they provided their email, whether they were recruited face-to-face, their gender, the population of their ZIP code, the years they have been a donor as well as wave fixed effects. Robust standard errors in parentheses. Significance levels: *p<0.1, **p<0.05 and ***p<0.01.

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[10]Specifically, we announced that the "most generous neighborhoods" would be recognized by being featured on the organization's website and social media. Fundraising managers call such a feature "friendly competition."

Diffusion of responsibility.   A possible alternative explanation for the negative effect of the group appeal is that approaching donors as part of a group makes the involvement of other donors more salient, leading to a reduced sense of personal responsibility. If individuals perceive the contributions of other donors as substitutes, this may lead to moral transgressions (Bartling and Fischbacher, 2012; Falk et al., 2020). Similarly, increasing the salience of others' involvement may trigger a "bystander effect" that reduces the likelihood that individuals will help (Darley and Latané, 1968; Cryder and Loewenstein, 2012). While this effect is certainly relevant in many settings of moral decision making, it is unlikely to drive behavior in our setting. While this effect is certainly relevant in many settings of moral decision making, it is unlikely to drive behavior in our setting because the content of the fundraising letters makes it clear, even in *CONTROL*, that the letters were also sent to other regular donors. Moreover, chapter 1 in this dissertation provides direct evidence that providing information about the giving behavior of other donors *increases* giving using a similar sample of regular donors as in our field experiment.[11] Thus, the mere mention of others inducing diffusion of responsibility is unlikely to explain the negative effect on giving in our setting.

Updating information.   Another potential explanation for the negative effect of the group appeal is that the appeal contains implicit information about the quality of the organization or its projects. For example, while the appeal itself does not contain explicit substantive information, donors in the treatment group may update their beliefs about the organization's fundraising management skills, leading to a decrease in donations. Since our sample consists of experienced donors who have had sufficient time to consolidate their prior beliefs about the organization, this effect seems unlikely. To empirically test whether updating behavior drives our treatment effect, we analyze donors' follow-up behavior. If donors update negatively about decision-relevant features of the choice environment, we should see a persistent negative effect of the group appeal. Instead, we find no long-lasting effects of the group appeal on follow-up behavior (see Appendix Table 3.7 for details). In particular, neither the probability of giving nor the total amount given differs between our treatment and control groups in response to the next fundraising letter. If anything, the group appeal leads to a small increase in both. We also find no treatment effect when looking at whether subjects donate in response to any of the next six fundraising appeals, or the total amount they donate over the next six rounds. Similarly, there is no effect on termination rates, i.e., whether they cancel their pledge in response to our intervention.

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[11]In the experiment, subjects receive information about the progress of the campaign toward a specific campaign goal. Across conditions, both the progress toward the goal and the number of other donors who have already donated are varied. Importantly, all of these interventions, whether the amount donated is close to the goal or far from it, and whether information about many or few other donors is provided, increase giving relative to a control in which no information was provided.

These results support the notion of an attentional effect that is present at the time of decision and is short-lived.

3.5   Conclusion

Results from a large-scale natural field experiment with donors to a charitable organization show that an intervention designed to increase giving can backfire in significant ways. In particular, a group appeal designed to emphasize a facet of a donor's identity associated with generosity has significant negative effects on giving. Results from an online experiment shed light on the underlying mechanism, suggesting that cognitive effects drive negative treatment effects on behavior. Decision architects designing behavioral interventions need to be very careful about what cognitive material an intervention may trigger in subjects' minds, as unwanted shifts of attention can have serious detrimental effects on the target behavior.

3.6 Appendix

3.6.1 Additional figures

Figure 3.2: Online experiment: Distribution of donation decisions across treatments
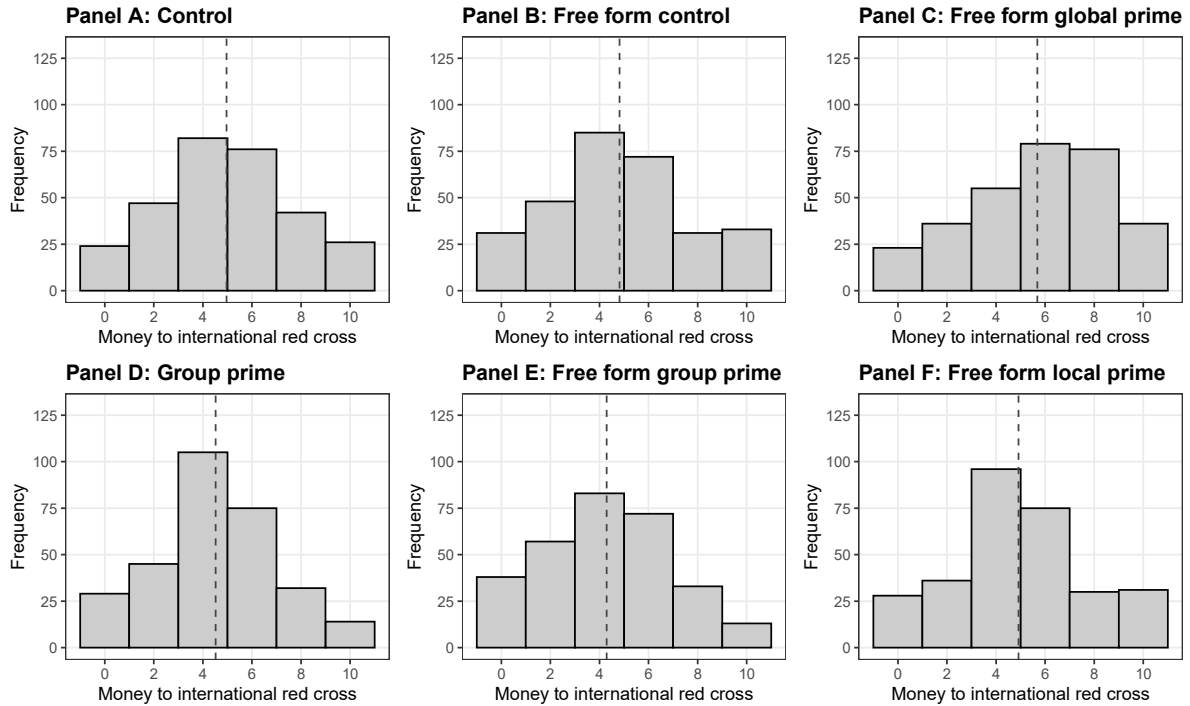
Notes: The figure displays histograms of the donation behavior for all six experimental conditions of the online experiment. The x-axis denotes the amount of money (out of £10) that subjects allocate to the International Red Cross instead of the British Red Cross.

3.6.2 Additional tables

Table 3.3: Field experiment: Balance check by wave

	Wave 1			Wave 2			Wave 3		
	Group (1)	Control (2)	$H_0:$ (1) = (2)	*Group* (3)	Control (4)	$H_0:$ (3) = (4)	*Group* (5)	Control (6)	$H_0:$ (5) = (6)
Annual donation amount	176	175	0.68	172	173	0.65	168	167	0.51
Provided Email	0.31	0.30	0.83	0.25	0.25	0.87	0.40	0.40	0.86
Recruited face to face	0.25	0.25	0.94	0.14	0.14	0.77	0.38	0.38	0.57
Men	0.51	0.52	0.16	0.51	0.51	0.98	0.48	0.48	0.84
Years donor	14.44	14.66	0.12	17.47	17.45	0.77	13.59	13.48	0.49
Local ZIP code population	18, 879	18, 984	0.47	18, 417	18, 445	0.73	18, 579	18, 477	0.58
Observations	10, 331	10, 269		47, 598	23, 733		8, 975	4,412	

Notes: The table presents summaries of the available baseline data by mailing wave and treatment condition. The third column in each wave panel presents p-values from t-tests (of proportions) and shows the effectiveness of the randomization method in generating treatment and control assignments orthogonal to the baseline information.

Table 3.4: Field experiment: Main results with and without reported controls

	Dependent variable:			
	Donation Probability		Donation Size	
	(1)	(2)	(3)	(4)
GROUP	−0.093***	−0.086***	−0.285***	−0.267**
	(0.032)	(0.032)	(0.110)	(0.107)
log(Population)		0.034*		0.141**
		(0.020)		(0.062)
Male		−0.064**		−0.188*
		(0.029)		(0.102)
Received by Email		−0.078**		−0.350***
		(0.031)		(0.097)
log(Annual donation amount)		0.080**		0.642***
		(0.034)		(0.219)
log(Years donor)		−0.007		−0.124
		(0.026)		(0.091)
Recruited face to face		−0.085*		−0.284
		(0.043)		(0.180)
Wave1		0.062		0.125
		(0.042)		(0.153)
Wave3		−0.012		−0.169**
		(0.042)		(0.083)
Constant (*CONTROL*)	0.281***	−0.365	0.665***	−3.320**
	(0.027)	(0.279)	(0.101)	(1.405)
Observations	105,318	105,318	105,318	105,318

Notes: The table shows OLS estimates. The dependent variable in columns (1) and (2) is a variable equal to 100 if a subject signs the annual pledge solicited in the fundraising letter and zero otherwise and in columns (3) and (4) the amount in Euro that is pledged. "*GROUP*" is equal to 1 when the subject is part of the *GROUP* treatment and zero if the subject is part of the *CONTROL* condition. "log(Population)" is the logarithm of the number of individuals living in subjects ZIP code region. "Male" is an indicator equal to one if the subject is a men and zero if female. "Received by Email" is an indicator if the subject also received the fundraising appeal vial email after the letter. "log(Annual donation amount)" is the logarithm of the amount subjects donate annually as their existing pledge. "log(Years donor)" is the logarithm of the years that subjects are donating to the organisation. "Recruited face to face" is an indicator equal to one if subjects were initially recruited via a face to face approach. "Wave 1" and "Wave 3" are indicator variables equal to one if subjects were part of the first and third wave, respectively, of sending the letters, relative to the second wave. Robust standard errors in parentheses. Significance levels: *p<0.1, **p<0.05 and ***p<0.01.

Table 3.5: Field experiment: Robustness of main results to model choice

	Dependent variable:			
	Has donated (Probit)		Has donated (Probit marginal effects)	
	(1)	(2)	(3)	(4)
GROUP	-0.128**	-0.120**	-0.00093**	-0.00082**
	(0.042)	(0.043)	(0.00032)	(0.00031)
Constant (CONTROL)	-2.769***	-3.714***		
	(0.031)	(0.420)		
Controls	No	Yes	No	Yes
Observations	105,318	105,318	105,318	105,318

Notes: The table shows Probit estimates in columns (1) and (2) and Probit marginal effects in columns (3) and (4). The dependent variable is a variable equal to 1 if a subject signs the annual pledge solicited in the fundraising letter and zero otherwise. "GROUP" is equal to 1 if the subject is part of the GROUP treatment and zero if the subject is part of the CONTROL condition. Additional independent variables ("Controls") added in column (2) and (4) are subjects' annual donation amount, whether they provided their email, whether they were recruited face-to-face, their gender, the population of their ZIP code, the years they have been a donor as well as wave fixed effects. Robust standard errors in parentheses. Significance levels: *$p<0.1$, **$p<0.05$ and ***$p<0.01$.

Table 3.6: Field experiment: Main results robustness to outliers

	Dependent variable:			
	Total Donation Size		log(Donation Size + 1)	
	(1)	(2)	(3)	(4)
GROUP	-0.155**	-0.149**	-0.005***	-0.005***
	(0.065)	(0.065)	(0.002)	(0.002)
Constant (CONTROL)	0.482***	-1.171**	0.015***	-0.026*
	(0.055)	(0.583)	(0.001)	(0.015)
Controls	No	Yes	No	Yes
Observations	105,308	105,308	105,318	105,318

Notes: The table shows OLS estimates. The dependent variable in columns (1) and (2) is the amount in Euro that subjects pledge annually to donate to the charity. In columns (3) and (4), this variable is logarithmized. To deal with the zeros, a constant of 1 is added beforehand. "GROUP" is equal to 1 when the subject is part of the GROUP treatment and zero if the subject is part of the CONTROL condition. Additional independent variables ("Controls") added in columns (2) and (4) are subjects' annual donation amount, whether they provided their email, whether they were recruited face-to-face, their gender, the population of their ZIP code, the years they have been a donor as well as wave fixed effects. In columns (1) and (2), we excluded 10 observations from the main sample which were identified using a Cook's distance of $1/4N$. Robust standard errors in parentheses. Significance levels: *$p<0.1$, **$p<0.05$ and ***$p<0.01$.

Table 3.7: Field experiment: Long-run treatment effect on giving in next solicitation

	Dependent variable:									
	Next call: Donation Probability		Next call: Donation Size		Next 6 calls: Donated in any		Next 6 calls: Donation Size		Has terminated contract	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
GROUP	0.015	−0.021	0.117	0.080	0.003	0.001	0.142	−0.021	−0.002	0.001
	(0.095)	(0.096)	(0.171)	(0.172)	(0.002)	(0.002)	(0.916)	(0.906)	(0.002)	(0.002)
Constant (*CONTROL*)	2.257***		2.321***		0.114***		21.755***		0.068***	
	(0.076)		(0.123)		(0.002)		(0.784)		(0.001)	
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	105,318	105,318	105,318	105,318	105,318	105,318	105,318	105,318	105,318	105,318

Notes: The table shows OLS estimates. The dependent variable in columns (1) and (2) is a variable equal to 100 if a subject signs an additional pledge in the next fundraising campaign after the field experiment and zero otherwise and in columns (3) and (4) the amount in Euro that is pledged in the next campaign. The dependent variable in columns (5) and (6) is an indicator equal to 1 if a subject signs an additional pledge in any of the next six fundraising campaigns after the field experiment and zero otherwise and in columns (7) and (8) the amount in Euro that is pledged in overall in the next six fundraising campaigns after the field experiment. Lastly, in the dependent variable in columns (9) and (10) is an indicator equal to one if a subject terminates his existing giving pledge. "*GROUP*" is equal to 1 when the subject is part of the *GROUP* treatment and zero if the subject is part of the *CONTROL* condition. Additional independent variables ("Controls") added in columns (2) and (4) are subjects' annual donation amount, whether they provided their email, whether they were recruited face-to-face, their gender, the population of their ZIP code, the years they have been a donor as well as wave fixed effects. Robust standard errors in parentheses. Significance levels: *p<0.1, **p<0.05 and ***p<0.01.

Table 3.8: Field experiment: Treatment effects by population size

	Dependent variable:			
	Donation Probability		Donation Size	
	(1)	(2)	(3)	(4)
GROUP	0.055	0.070	2.195	2.281
	(0.404)	(0.405)	(1.464)	(1.489)
log(Population)	0.043	0.044	0.314**	0.309**
	(0.035)	(0.035)	(0.148)	(0.144)
GROUP × log(Population)	−0.015	−0.016	−0.257	−0.265*
	(0.042)	(0.042)	(0.159)	(0.161)
Constant	−0.134	−0.465	−2.359*	−4.942**
	(0.336)	(0.407)	(1.351)	(2.234)
Controls	No	Yes	No	Yes
Observations	105,318	105,318	105,318	105,318

Notes: The table shows OLS estimates. The dependent variable in columns (1) and (2) is a variable equal to 100 if a subject signs the annual pledge solicited in the fundraising letter and zero otherwise and in columns (3) and (4) the amount in Euro that is pledged. "log(Population)" is the logarithm of the population of respective subjects ZIP code region. Additional independent variables ("Controls") added in columns (2) and (4) are subjects' annual donation amount, whether they provided their email, whether they were recruited face-to-face, their gender, the years they have been a donor as well as wave fixed effects. Robust standard errors in parentheses. Significance levels: *p<0.1, **p<0.05 and ***p<0.01.

Table 3.9: Online experiment: Results of the open-ended coding

	Coder 1	Coder 2	Final
Local mention = 1 & Global mention = 0	68.09%	68.42%	68.00%
Local mention = 0 & Global mention = 1	26.40%	26.82%	26.32%
Local mention = 0 & Global mention = 0	4.34%	4.09%	4.51%
Local mention = 1 & Global mention = 1	1.17%	0.67%	1.17%

Notes: The table shows the result of coding 1,197 open-ended text responses across the four conditions of the online experiment that contain open-ended questions. "Global mention" and "Local mention" are indicator variables equal to one if the open-ended text responses contain a globally located recipients and a locally located recipient, respectively.

Table 3.10: Online experiment: Open-ended responses as mediator of the treatment effect on giving

	Dependent variable:		
	Donation to the International Red cross		
	(1)	(2)	(3)
Panel A: *Free form group prime* vs. *Free form control*			
Constant (*Free form constant*)	4.820***	6.270***	4.211***
	(0.166)	(0.258)	(0.166)
Treatment *Free form group prime*	−0.523**	−0.035	−0.068
	(0.227)	(0.219)	(0.218)
Local mention		−2.164***	
		(0.280)	
Global mention			2.536***
			(0.320)
Observations	596	596	596
R^2	0.009	0.105	0.109
Panel B: *Free form local prime* vs. *Free form global prime*			
Constant (*Free form global prime*)	5.685***	6.379***	4.244***
	(0.164)	(0.178)	(0.239)
Treatment *Free form local prime*	−0.766***	0.290	0.367
	(0.229)	(0.266)	(0.260)
Local mention		−2.016***	
		(0.271)	
Global mention			2.220***
			(0.264)
Observations	601	601	601
R^2	0.018	0.105	0.126

Notes: The table shows OLS estimates. The dependent variable is the amount of money (out of £10) that subjects allocate to the International Red Cross instead of the British Red Cross. "Treatment *Free form group prime*" and "Treatment *Free form local prime*" are indicators equal to one if subjects are part of the respective treatment conditions. "Global mention" and "Local mention" are indicator variables equal to one if the open-ended text responses prior to the donation decision contain a globally located recipients and a locally located recipient, respectively. Robust standard errors in parentheses. Significance levels: $^*p<0.1$, $^{**}p<0.05$ and $^{***}p<0.01$.

3.6.3   Motivational examples of group appeals

The Australian Red Cross invites people to volunteer as a team with the slogan "nothing beats the feeling of saving lives together." On their website, potential donors can register a personal account, create a new Lifeblood Team or join an existing one, and even become a Lifeblood Champion. Contributions are tracked at the group level, as each blood donation counts toward the team's total.[12] The Australian charity MS Plus aims at improving the lives of people affected by multiple sclerosis and regularly organizes charity cycling events in which the top teams' fundraising totals are posted on its website.[13] Similarly, the charity Leukaemia & Blood Cancer New Zealand encourages donors to give in teams with features such as team profile pictures, introductory texts, and team fundraising goals.[14] Movember, a foundation dedicated to men's health projects around the world, motivates potential donors to create a "Movember Team" and join a field or industry-specific team challenge to compete against other teams representing, for example, schools, universities, companies, or sports clubs.[15] Monthly donors to the United Nations World Food Programme can join "Challenges" a feature within the charity's ShareTheMeal app—which provides a way to donate together with other donors in a group. [16] Not only charities, but also large fundraising platforms that serve as tools for smaller nonprofits to securely collect and process donations offer ways to create a sense of community by allowing donors to form teams (e.g. givelively.org or givebutter.com). In addition, a common feature of most of the above fundraising campaigns is that they feature leaderboards, i.e., a public ranking of team contributions.

3.6.4   Field experiment material

The following displays the text of the letters that were distributed in the field experiment.
[Standard letter head]

[Control]
Emergency Helpers needed!

[Group prime]
Emergency Helpers from <ZIP CODE> <REGION> needed! We are looking for generous neighborhoods

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[12]https://www.lifeblood.com.au/blood/donate-as-a-group ((02/28/2023))

[13]https://www.msgongride.org.au/fundraising/leaderboard (02/28/2023)

[14]https://www.shaveforacure.co.nz/teamsleaderboard (02/28/2023)

[15]https://ie.movember.com/leaderboards/network/ (02/28/2023)

[16]ShareTheMeal App (02/28/2023)

[Group competition]

Emergency Helpers from \<ZIP CODE\> \<REGION\> needed! We will award the most generous neighborhoods

Dear [Name of donor],

many children are currently having a harder time than ever. Poverty and hunger are continuing to affect girls and boys all over the world. Wars, natural disasters and the corona crisis are increasing the plight of children. It is our job to help them quickly. As [charity name] sponsors, you are making an important contribution to this.

Our helpers are currently in constant operation in many places, for example in [country]. The country is on the verge of collapse due to a long, terrible civil war. Millions of children are starving.

[charity name] is providing emergency aid in [country of operation]. This means that, for example, we are providing malnourished children with special food. For girls and boys, we are organizing medical aid that cannot be delayed. It is about ensuring that children can survive.

Every year, [charity name] teams carry out around 300 emergency aid missions in 100 countries. Whether currently in [country] or [event in other country] – wherever children urgently need help, our helpers are on site.

[Control]

Help us now: Become [charity name] Emergency Helper and make life-saving emergency aid possible with your contribution!

[Group prime]

Help us now as a team together with other donors from \<ZIP CODE\> and \<REGION\>: Become [charity name] Emergency Helper and make life-saving emergency aid possible with your contribution!

[Group competition]

Help us now as a team together with other donors from \<ZIP CODE\> and \<RÉGION\>: Become a [charity name] Emergency Helper and make life-saving emergency aid possible with your contribution!

We will crown the neighborhoods with the highest numbers of Emergency Helpers in our next newsletter as well as on our homepage and on social media.

Further information about the campaign can be found at: [link]

I would like to thank you very much for your generous support.

With warm thanks and kind regards [Picture, name and signature of director]

3.6.5 Online experiment instructions

This section provides screenshots of the instructions of the online experiment. Note that in order to avoid anchoring effects, the slider-thumbs of the donation decision are initially hidden and only appear once subjects click on the slider-scale.

Control condition

Figure 3.3: Control condition screen 1

Information

In this survey, you face a decision, which will be explained on the next pages.

After the study is completed, a computer will randomly choose one out of every 25 participants. For the selected participants, their decision is implemented with real monetary consequences. That is, each selected participant's choice in the decision is carried out exactly as described in the description of the decision.

Since your choice can have actual consequences, you should make the choice as conscientiously as possible.

Next

Figure 3.4: Control condition screen 2

Information

In the decision, you decide on how much to help others.

You receive a budget of £10. You then decide how much of the budget to help others.

On the next page, we inform you about the potential recipients of your help and ask you how you want to help.

Next

The Red Cross

The *Red Cross* is a humanitarian organization that is dedicated to preventing and alleviating human suffering in the face of emergencies. It plays a crucial role in responding to disasters and provides support to individuals and communities. It is also at the forefront of disaster response, providing shelter, food, and medical care to individuals affected by events such as rail crashes, floods, and fires. In addition to its disaster response efforts, it is also deeply involved in providing health services.

British Red Cross

The *British Red Cross* provides the services described above to individuals and communities **in Britain**.

International Red Cross

The *International Red Cross* provides the services described above to individuals and communities **all around the world**.

Your donation

You decide how much you want to donate from your budget of £10 to the *British Red Cross* and how much to the *International Red Cross*. The part of the budget you donate to the *British Red Cross* will help communities in Britain. The part of the budget you donate to the *International Red Cross* will help communities all around the world.

The charities are looking for donations. Help the charities: make high-impact aid possible with your donation.

How would you like to divide the money?
Please use the slider below to make your decision.

£-Click the scale- for the *British Red Cross*
£-Click the scale- for the *International Red Cross*

Your donation to the
British Red Cross

Your donation to the
International Red Cross

Confirm decision

Group prime condition

Figure 3.6: Group prime condition screen 1

Information

You indicated on Prolific that you currently live in East London. This survey is fielded to people who live in East London and are active on Prolific. You and the other people from East London who participate in this Prolific survey form a group. Your group faces a decision, which will be explained on the next pages.

After the study is completed, a computer will randomly choose one out of every 25 participants. For the selected participants, their decision is implemented with real monetary consequences. That is, each selected participant's choice in the decision is carried out exactly as described in the description of the decision.

Since your choice can have actual consequences, you should make the choice as conscientiously as possible.

Next

Figure 3.7: Group prime condition screen 2

Information

You and people in your group – **people from East London who are active on Prolific** – decide on how much to help others.

Every group member receives a budget of £10. Every group member then decides individually how much of the budget to contribute to your group's financial help to others.

On the next page, we inform you about the potential recipients of your group's help and ask you how you want to contribute to your group's help.

Next

The Red Cross

The *Red Cross* is a humanitarian organization that is dedicated to preventing and alleviating human suffering in the face of emergencies. It plays a crucial role in responding to disasters and provides support to individuals and communities. It is also at the forefront of disaster response, providing shelter, food, and medical care to individuals affected by events such as rail crashes, floods, and fires. In addition to its disaster response efforts, it is also deeply involved in providing health services.

British Red Cross

The *British Red Cross* provides the services described above to individuals and communities **in Britain**.

International Red Cross

The *International Red Cross* provides the services described above to individuals and communities **all around the world**.

Your group's donation

You decide how much you want to contribute from your budget of £10 to your group's donation to the *British Red Cross* and how much to the *International Red Cross*. The part of the budget you contribute to your group's donation to the *British Red Cross* will help communities in Britain. The part of the budget you contribute to your group's donation to the *International Red Cross* will help communities all around the world.

The charities are looking for donations from **East London**. Help the charities together with other Prolific donors from **East London**: make high-impact aid possible with your contribution to your group's donation.

How would you like to divide the money?
Please use the slider below to make your decision.

£-Click the scale- for the *British Red Cross*
£-Click the scale- for the *International Red Cross*

Your contribution to your group's donation to the *British Red Cross*

Your contribution to your group's donation to the *International Red Cross*

Confirm decision

Free form control condition

Figure 3.9: Free form control condition screen 1

Information

In this survey, you face a decision, which will be explained on the next pages.

After the study is completed, a computer will randomly choose one out of every 25 participants. For the selected participants, their decision is implemented with real monetary consequences. That is, each selected participant's choice in the decision is carried out exactly as described in the description of the decision.

Since your choice can have actual consequences, you should make the choice as conscientiously as possible.

Next

Figure 3.10: Free form control condition screen 2

Information

In the decision, you decide on how much to help others.

You receive a budget of £10. You then decide how much of the budget to contribute to help others. There are two different recipients.

Before we inform you about the other potential recipient of your help and ask you how you want to help, please respond to the following question.

From the top of your mind: whom would you like to help? Assume it is up to you and you could choose any person or group of people to be the recipient(s) of your help.

Please be specific about who is the recipient and where the recipients are located.

Who?

Where?

Next

The Red Cross

The *Red Cross* is a humanitarian organization that is dedicated to preventing and alleviating human suffering in the face of emergencies. It plays a crucial role in responding to disasters and provides support to individuals and communities. It is also at the forefront of disaster response, providing shelter, food, and medical care to individuals affected by events such as rail crashes, floods, and fires. In addition to its disaster response efforts, it is also deeply involved in providing health services.

British Red Cross

The *British Red Cross* provides the services described above to individuals and communities **in Britain**.

International Red Cross

The *International Red Cross* provides the services described above to individuals and communities **all around the world**.

Your donation

You decide how much you want to donate from your budget of £10 to the *British Red Cross* and how much to the *International Red Cross*. The part of the budget you donate to the *British Red Cross* will help communities in Britain. The part of the budget you donate to the *International Red Cross* will help communities all around the world.

The charities are looking for donations. Help the charities: make high-impact aid possible with your donation.

How would you like to divide the money?
Please use the slider below to make your decision.

£-Click the scale- for the *British Red Cross*
£-Click the scale- for the *International Red Cross*

Your donation to the
British Red Cross

Your donation to the
International Red Cross

Confirm decision

Free form group prime condition

Figure 3.12: Free form group prime condition screen 1

Information

You indicated on Prolific that you currently live in East London. This survey is fielded to people who live in East London and are active on Prolific. You and the other people from East London who participate in this Prolific survey form a group. Your group faces a decision, which will be explained on the next pages.

After the study is completed, a computer will randomly choose one out of every 25 participants. For the selected participants, their decision is implemented with real monetary consequences. That is, each selected participant's choice in the decision is carried out exactly as described in the description of the decision.

Since your choice can have actual consequences, you should make the choice as conscientiously as possible.

Next

Figure 3.13: Free form group prime condition screen 2

Information

You and people in your group – **people from East London who are active on Prolific** – decide on how much to help others.

Every group member receives a budget of £10. Every group member then decides individually how much of the budget to contribute to your group's financial help to others.

Before we inform you about the potential recipients of your group's help and ask you how you want to contribute to your group's help, please respond to the following question.

From the top of your mind: whom would you like to help with your group? Assume it is up to you and you could choose any person or group of people to be the recipient(s) of your groups' help.

Please be specific about who is the recipient and where the recipients are located.

Who?

Where?

Next

The Red Cross

The *Red Cross* is a humanitarian organization that is dedicated to preventing and alleviating human suffering in the face of emergencies. It plays a crucial role in responding to disasters and provides support to individuals and communities. It is also at the forefront of disaster response, providing shelter, food, and medical care to individuals affected by events such as rail crashes, floods, and fires. In addition to its disaster response efforts, it is also deeply involved in providing health services.

British Red Cross

The *British Red Cross* provides the services described above to individuals and communities **in Britain**.

International Red Cross

The *International Red Cross* provides the services described above to individuals and communities **all around the world**.

Your group's donation

You decide how much you want to contribute from your budget of £10 to your group's donation to the *British Red Cross* and how much to the *International Red Cross*. The part of the budget you contribute to your group's donation to the *British Red Cross* will help communities in Britain. The part of the budget you contribute to your group's donation to the *International Red Cross* will help communities all around the world.

The charities are looking for donations from **East London**. Help the charities together with other Prolific donors from **East London**: make high-impact aid possible with your contribution to your group's donation.

How would you like to divide the money?
Please use the slider below to make your decision.

£-Click the scale- for the *British Red Cross*
£-Click the scale- for the *International Red Cross*

Your contribution to your group's donation to the *British Red Cross*

Your contribution to your group's donation to the *International Red Cross*

Confirm decision

Free form global prime condition

Figure 3.15: Free form global prime condition screen 1

Information

In this survey, you face a decision, which will be explained on the next pages.

After the study is completed, a computer will randomly choose one out of every 25 participants. For the selected participants, their decision is implemented with real monetary consequences. That is, each selected participant's choice in the decision is carried out exactly as described in the description of the decision.

Since your choice can have actual consequences, you should make the choice as conscientiously as possible.

Next

Figure 3.16: Free form global prime condition screen 2

Information

In the decision, you decide on how much to help others.

You receive a budget of £10. You then decide how much of the budget to contribute to help others. There are two different recipients.

One of the recipients is a charity that is operating in regions affected by war.

The computer randomly selected which recipient you learn about on this screen. Each recipient had an equal chance of being chosen.

Before we inform you about the other potential recipient of your help and ask you how you want to help, please respond to the following question.

From the top of your mind: whom would you like to help? Assume it is up to you and you could choose any person or group of people to be the recipient(s) of your help.

Please be specific about who is the recipient and where the recipients are located.

Who?

Where?

Next

The Red Cross

The *Red Cross* is a humanitarian organization that is dedicated to preventing and alleviating human suffering in the face of emergencies. It plays a crucial role in responding to disasters and provides support to individuals and communities. It is also at the forefront of disaster response, providing shelter, food, and medical care to individuals affected by events such as rail crashes, floods, and fires. In addition to its disaster response efforts, it is also deeply involved in providing health services.

British Red Cross

The *British Red Cross* provides the services described above to individuals and communities **in Britain**.

International Red Cross

The *International Red Cross* provides the services described above to individuals and communities **all around the world**.

Your donation

You decide how much you want to donate from your budget of £10 to the *British Red Cross* and how much to the *International Red Cross*. The part of the budget you donate to the *British Red Cross* will help communities in Britain. The part of the budget you donate to the *International Red Cross* will help communities all around the world.

The charities are looking for donations. Help the charities: make high-impact aid possible with your donation.

How would you like to divide the money?
Please use the slider below to make your decision.

£-Click the scale- for the *British Red Cross*
£-Click the scale- for the *International Red Cross*

Your donation to the
British Red Cross

Your donation to the
International Red Cross

Confirm decision

Free form local prime condition

Figure 3.18: Free form local prime condition screen 1

Information

In this survey, you face a decision, which will be explained on the next pages.

After the study is completed, a computer will randomly choose one out of every 25 participants. For the selected participants, their decision is implemented with real monetary consequences. That is, each selected participant's choice in the decision is carried out exactly as described in the description of the decision.

Since your choice can have actual consequences, you should make the choice as conscientiously as possible.

Next

Figure 3.19: Free form local prime condition screen 2

Information

In the decision, you decide on how much to help others.

You receive a budget of £10. You then decide how much of the budget to contribute to help others. There are two different recipients.

One of the recipients is a charity that is operating locally in Britain.

The computer randomly selected which recipient you learn about on this screen. Each recipient had an equal chance of being chosen.

Before we inform you about the other potential recipient of your help and ask you how you want to help, please respond to the following question.

From the top of your mind: whom would you like to help? Assume it is up to you and you could choose any person or group of people to be the recipient(s) of your help.

Please be specific about who is the recipient and where the recipients are located.

Who?

Where?

Next

The Red Cross

The *Red Cross* is a humanitarian organization that is dedicated to preventing and alleviating human suffering in the face of emergencies. It plays a crucial role in responding to disasters and provides support to individuals and communities. It is also at the forefront of disaster response, providing shelter, food, and medical care to individuals affected by events such as rail crashes, floods, and fires. In addition to its disaster response efforts, it is also deeply involved in providing health services.

British Red Cross

The *British Red Cross* provides the services described above to individuals and communities **in Britain**.

International Red Cross

The *International Red Cross* provides the services described above to individuals and communities **all around the world**.

Your donation

You decide how much you want to donate from your budget of £10 to the *British Red Cross* and how much to the *International Red Cross*. The part of the budget you donate to the *British Red Cross* will help communities in Britain. The part of the budget you donate to the *International Red Cross* will help communities all around the world.

The charities are looking for donations. Help the charities: make high-impact aid possible with your donation.

How would you like to divide the money?
Please use the slider below to make your decision.

£-Click the scale- for the *British Red Cross*
£-Click the scale- for the *International Red Cross*

Your donation to the
British Red Cross

Your donation to the
International Red Cross

Confirm decision

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