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How does fundraising affect volunteering? Evidence from a natural field experiment

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ABSTRACT

Can fundraising solicitations have an effect on other types of giving, like volunteering? We report results from a field experiment in which a non-profit firm asked their current volunteers to donate money, in a randomly-staggered roll-out. We observe volunteers' entire portfolio of giving to the firm – both donations, and volunteer hours before and after the call. Overall, these results suggested there was some decline in volunteer hours among volunteers who were newer to the firm. By contrast, long-time volunteers maintained their volunteering after the call, and were also more likely to donate in the first place. Substitution was rare, as the donors themselves did not decrease their volunteer hours after donating. Our findings provide new evidence for how personal history can shape charitable giving decisions, and provide new practical guidance for fundraisers.

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1. Introduction

Fundraising drives are ubiquitous in the not-for-profit world – charitable contributions from individuals in the United States totaled roughly $218 billion in 2011 (Giving USA, 2012). They provide critical support to organizations that might not exist otherwise, and almost every non-profit relies on some degree of active solicitation from potential donors. A simple cost-benefit calculation for a firm would weigh the expenses of fundraising against their expected revenues. But this misses the fact that the solicitees might respond in other ways, that could affect the firm's bottom line.

In particular, many non-profits also rely on volunteering – free contributions of time to support an organization's activities (Wilson, 2000). Volunteers donated 65.7 million hours to charities in the United States in 2012 (Bureau of Labor Statistics, 2012). Fundraisers naturally see a firm's volunteers as a fruitful target – volunteers already support the firm's goals, and have an ongoing personal relationship with the firm (Bryant, Jeon-Slaughter, Kang, & Tax, 2003; Havens, O'Herlihy, & Schervish, 2006). And practitioners have often connected volunteering and fundraising, even considering volunteering as a "gateway drug" to future giving, including money donations. (Dietz & Keller, 2015; Fidelity Charitable, 2015). But this very relationship also complicates the firm's decision, because fundraising might also change how much time they are willing to volunteer in the future. In the current research we ask: might fundraising have a spillover effect on volunteering?

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There is a rich literature on how fundraising drives charitable giving (Bekkers & Wiepking, 2011; List & Price, 2011; Oppenheimer & Olivola, 2010). However, most of this research focuses on the direct effect of solicitations, *within* the dimension being queried – most often, by looking at how monetary donations are impacted by an ask for money. These results are limited because fundraising could have secondary consequences on other forms of giving, in addition to the direct donations. We test for these spillover effects in a natural field experiment, in a setting that provides a unique opportunity to estimate these effects. Specifically, we observe volunteers making decisions about their volunteering contributions before and after fundraising calls, the timing of which was randomly assigned.

There are many ways volunteers could adjust their time donations in response to a solicitation. They might substitute away from future time donations after giving money. Fundraising could also increase time donations if it is a reminder of the cause, or if time and money are complementary. People who do not give money might also adjust their volunteer time after receiving a fundraising call. And the distribution of these spillover effects may vary across the pool of volunteers, who come from different backgrounds and have different histories with the organization.

In this paper we present data collected from a non-profit firm that fundraised from their volunteer pool. Volunteers were randomly assigned to the time they received their fundraising call, across a four-month period in which they were also expected to donate time. The firm kept a daily log of volunteer hours, which we paired with data from the fundraising itself. The volunteering data was modeled in a panel regression to generate the counterfactual: what time donations should the firm have expected each volunteer to give, if they had not been called? This novel dataset allows us to simultaneously estimate the causal effect of a single fundraising call on both money donations and time donations, within individual volunteers.

Our experiment provides new evidence that soliciting charitable donations must also consider potential trade-offs, such as volunteer time. Furthermore, this aggregate trade-off can exist even when there is no individual-level substitution, due to a combination of different mechanisms that both increase and decrease giving. We also find evidence for a new consequence of personal history in charitable giving – not only do long-time volunteers give more money than new volunteers during fundraising, but they also reduce their volunteer hours less after being solicited. This demonstrates that while calls to long-term volunteers were still effective, calls to newer volunteers can have a negative expected value for the firm.

The rest of the paper proceeds as follows: Section 2 reviews relevant literature on the psychology and economics of charitable giving. Section 3 describes the firm, data collection, and the experimental design. Section 4 presents results from both money and time donations, and analyses the individual-level results in light of our predictions. Section 5 discusses the implications of these data for related theories of charitable giving, and practical consequences.

2. Theory and related literature

We are not aware of any previous research that has looked at the direct and indirect effects of fundraising among volunteers. However, the existing literature demonstrates several kinds of mechanisms that link these two behaviors in the field. First, past volunteering may moderate the direct effect of fundraising on monetary donations. Second, fundraising may have spillover effects on future volunteering. Third, past volunteering history may moderate the spillover effects on future volunteering. All of these mechanisms have consequences for estimating the total effects of fundraising among volunteers, and provide theoretical guidance for our experimental design and analyses. We review these literatures in the following sections.

2.1. Past volunteering and fundraising

The connection between volunteering and fundraising is understood by practitioners – as one research report stated, volunteering can be seen as a "gateway drug" to fundraising (Dietz & Keller, 2015). Although the percentage of fundraising targeted at past volunteers has not been estimated well, a review of recent survey evidence suggests that fundraising from volunteers is not rare. At the household level, volunteering and fundraising are positively correlated (Bryant et al., 2003; Grimm, 2007; Havens et al., 2006) and there is some evidence for broader complementarities across different kinds of prosocial behavior (Brown & Lankford, 1992; Greenberg, 2014).

This result can be explained by a classical model that assume donors simply want to increase the quantity of a public good that is provided (e.g. Hochman & Rodgers, 1969; Warr, 1983). That is, if someone has revealed a preference for supporting a cause, this preference should be fungible across other opportunities to give to that same cause. Existing evidence for these effects are typically limited to giving within a single dimension. For example, those who have given money in a past solicitation are more responsive to future solicitations for money than those who did not give money initially (Landry, Lange, List, Price, & Rupp, 2010; Meer, 2013; for a similar effect in blood donations, see Foss & Dempsey, 1979). But it is reasonable to think that this mechanism could extend across dimensions of giving, as long as it was targeted towards the same cause.

But beyond this simple model, prosocial behavior has many psychological antecedents and consequences (Andreoni, 1990; Batson & Shaw, 1991; Bekkers & Wiepking, 2011; Schwartz, 1973). And many of these channels could mediate the relationship between past volunteering and donations. For example, volunteers may build a personal relationship with the firm during their volunteering time, which is an important psychological driver of giving (Chen & Li, 2009; Kessler & Milkman, 2016; Prouteau & Wolff, 2008; Small & Simonsohn, 2008). And a time donation mindset can also prime monetary contributions, even if no time is actually donated (Liu & Aaker, 2008). Finally, on a practical level, past volunteers are easier to contact, and already understand the firm's impact, which reduces the upfront costs of fundraising.

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By any of these channels, a firm should primarily expect that fundraising calls to volunteers will produce greater revenue than calls to non-volunteers. However, the spillover effects may also be larger among volunteers than non-volunteers, if the firm is expecting future volunteering. So it is important to understand the direction, magnitude, and composition of those spillover effects.

2.2. Fundraising and future volunteering

The most consequential spillover effects of fundraising are ones that would suppress donations of volunteer time. And standard economic theory suggests one clear possibility: a substitution effect. That is, the fundraiser may simply provide a way for some people to shift their contributions from one dimension (volunteer time) to another (the fundraiser), according to their preference and budget constraint (Duncan, 1999; Feldman, 2010). Previous work has demonstrated evidence for intertemporal substitution, in which fundraising at time $T + 1$ was less effective among people who had given to fundraisers at time T (Cairns & Slonim, 2011; Meier, 2007). It is unclear whether this might also translate across dimensions of giving. But substitution should be expected if the solicitation simply makes it easier to give money (reducing the price of money, relative to giving time), or if the solicitee infers that the firm prefers donations in the queried dimension over the unqueried one. A related mechanism is "moral licensing" (Merritt, Effron, & Monin, 2010; Sachdeva, Iliev, & Medin, 2009), in which past prosocial behavior is used as a justification to pass up future prosocial behavior. In this domain, volunteers who donate money could feel licensed to shirk on their future volunteering, thus behaving in a manner consistent with substitution effects.

Another negative spillover effect – distinct from substitution – would be a decrease in volunteering among those who do not give money. For example, some research has found that being solicited is itself an aversive experience (Dellavigna, List, & Malmendier, 2012; Van Diepen, Donkers, & Franses, 2009). Solicitees may feel pressure to comply with the asker, or else violate the asker's expectations and impugn their self-image by rejecting an opportunity to be charitable (Cain, Dana, & Newman, 2014; Dana, Cain, & Dawes, 2006; Flynn & Lake, 2008). If solicited volunteers infer that other solicitations are likely in the future, they might then be apprehensive about any future contact (Andreoni, Rao, & Trachtman, 2011; Kamdar, Levitt, List, & Suyverson, 2014; Knutsson, Martinsson, & Wollbrant, 2013; Lin, Schaumberg, & Reich, 2016). Furthermore, active fundraising might signal negative qualities about firm decision-making (Bowman, 2006; Rose-Ackerman, 1982) or conflict with personal motivations for volunteering (Andreoni, 1990; Exley, 2017; Prouteau & Wolff, 2008; Unger, 1991). Self-perception might also play a role, if declining a fundraiser also erodes volunteers' identification with the cause (Bem, 1972; Grant & Dutton, 2012; Kessler & Milkman, 2016).

It is also possible that fundraising could have positive spillover effects, and increase the hours given by some volunteers. For example, time and money donations might increase concurrently if the two types of giving are complements of one another (Brown & Lankford, 1992; Greenberg, 2014). Here, too self-perception may induce a spillover effect if giving money reinforces a volunteer's self-concept as a supporter of the firm. Likewise, there might be knock-on effects of escalating commitment – the "foot in the door" effect (Burger, 1999; Freedman & Fraser, 1966). Finally, volunteering could even increase among those who make no money donations. For example the solicitation might simply act as a reminder of their earlier intention to volunteer. Likewise the fundraising might motivate volunteers to increase their intended support, but those solicitees who are money-constrained may give a second-best response and donate more time instead (Cialdini et al., 1975). Given the range of possible effects suggested by theory – increasing and decreasing volunteering, among donors and non-donors – our field experiment can contribute to the theoretical and practical implications of these literatures.

2.3. Past volunteering and future spillovers

Volunteers at any firm are by definition a self-selected group. But within a firm's volunteer pool, there will very often be wide variation in tenure at that firm. For example, any long-term volunteering operation will maintain a pool of returning volunteers even as they recruit and onboard new volunteers. And this variation in volunteers' personal history with the firm will often have important effects on how those volunteers give to the firm in the future. The existing empirical literature gives strong theoretical guidance to suggest this is a potent individual difference in charitable giving. People who have given to previous fundraisers are more responsive to future solicitations than those who did not give the first time (Landry et al., 2010; Meer, 2013). And personal history can increase people's psychological connection to a firm (Grant & Dutton, 2012; Kessler & Milkman, 2016; Piliavin & Callero, 1991; Small & Simonsohn, 2008). Personal history also changes how people respond to direct incentives for volunteering (Exley, 2017).

Given that personal history is implicated in many of the direct effects of fundraising, it is reasonable to wonder whether personal history might also affect the spillovers between fundraising and volunteer time. Similar trends have been suggested in earlier work that has focused on spillover effects within a single dimension (e.g. Karlan, List, & Shafir, 2011; Karlan & Wood, 2014). The current study provides a new opportunity to estimate these spillover effects across dimensions, where the effects of history on preferences for giving can be more cleanly separated from mechanical spillover effects like anchoring or habit formation.

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3. Experimental design

3.1. The firm

This experiment was conducted by the Center for Economic Progress (henceforth "CEP") a non-profit organization based in Chicago that provides free tax preparation assistance to low-income families and individuals[1] trying to navigate the complexities of the US Income Tax Code. The structure of the tax code means that most of these individuals are in fact owed money by the government, but many people still do not file, for various reasons (including lack of information, effort costs, and stigma; see Bhargava & Manoli, 2015; Chetty & Saez, 2013). CEP trains volunteers to assist their clients in centers throughout the state.

On weekday evenings and weekends leading up to the filing deadline, clients can bring their tax materials to their local center where a volunteer will guide them through their tax return. In 2011, CEP helped prepare 28,134 tax returns at 29 locations across Illinois. However, their funding was temporarily cut during a state budget crisis, so the firm decided to conduct an active fundraising drive for the first time among their volunteers.[2] CEP consulted with the authors on how to conduct and analyze a field experiment, however the execution and data collection were handled internally, and a data sharing agreement was procured in advance to ensure all personally-identifying information would be protected.

3.2. Sample selection

CEP volunteer recruitment is organized around discrete tax seasons, which run from sign-up in December and January, through to the filing deadline in mid-April. Prospective volunteers registered online during the November-January period preceding that year's tax season and filled out a demographics questionnaire. There were no specific experience or qualification prerequisites for volunteers, though every volunteer was required to complete a six-hour online training course before they were allowed to sign up for volunteer shifts, and new volunteers attended an additional in-person training session before the tax season began.

We decided to include all 1691 volunteers registered to participate in the 2012 tax season with CEP. Registration was open throughout the season – however, we imposed a strict cut-off, and only included tax preparers who completed their training and were registered as of January 18, which was the latest date we could perform the necessary random assignment.[3] This ensured that sign-up date would not be correlated with treatment assignment, which might otherwise create a composition bias in our treatment effect estimates.

This population is intrinsically self-selected. That is, we do not assign people to volunteer for the cause – instead they chose to register on their own, before the experiment. However, this self-selection is actually critical for the theoretical implications of this work. These volunteers have already revealed a preference for contributing to this particular charitable good. Furthermore, they have expressed a desire to volunteer, so the firm's expectation of future volunteering is strictly positive. Without this positive expectation, it would be impossible to observe any substitution or spillover costs that result from our treatment.

3.3. Volunteer hours

During the tax season, volunteers allocated their efforts to tax prep locations through a central website, and all records of volunteer contributions were verified on-location by CEP staff. Crucially, the website allowed volunteers' schedules to be flexible throughout the season. The site showed them upcoming time shifts at locations in their area that were available, and was updated frequently by CEP. Active volunteers logged in periodically to track and modify their commitments, and they could sign up or cancel up to two days before each shift. This rolling sign-up structure allowed us to observe a treatment effect, because decisions to volunteer (or not) could be made or adjusted after treatment.

There were some exogenous pressures on volunteer time that were unrelated to our experiment. For instance, all volunteers who completed the online training were asked to pledge that they would sign up for at least 20 h. But there was no actual enforcement or penalty,[4] and most volunteers either comfortably exceeded their pledged amount, or did not show up at all. Additionally, all volunteers were also sent periodic reminders via email, regardless of their sign-up history. Furthermore, some volunteers made their time commitments well in advance. However, any exogenous variation in hours signups (due to the pledge, reminders, advance scheduling, or external time demands) would decrease volunteers' sensitivity to our treatment, and bias our treatment effects estimates towards zero.

3.4. Experimental treatment – Call timing

All volunteers were given a fundraising call during one of three week-long waves across the tax season. Fig. 1 shows CEP's "editorial calendar", which puts the fundraising calls in context, alongside all other unprompted communications sent from

[1] Eligibility for the program was capped at $25,000 annual income for a single filer; or $50,000 for joint filers.
[2] Some fundraising had been targeted towards select corporate sponsors in previous years, which we do not observe.
[3] Our records indicate that 97 volunteers who contributed hours were not on the list of volunteers at the cut-off date. They were not called, and they were not included in the randomization.
[4] There were small incentives tied to the pledge, such as t-shirts and partial credit towards CPA certification.

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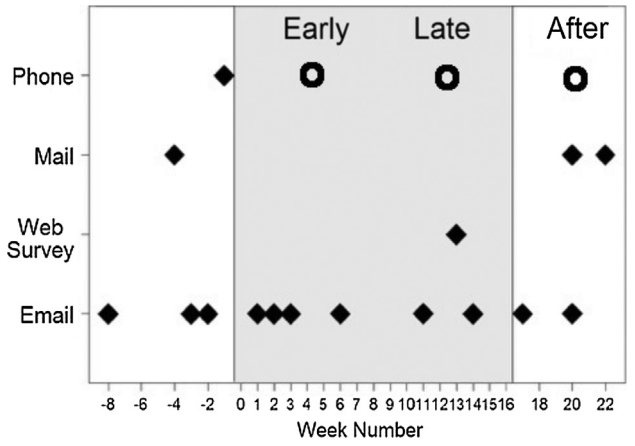

Fig. 1. The firm's internal "editorial calendar" shows every single unprompted communication that is sent to volunteers, across four different media, during the tax season. The actual window in which volunteers can donate time is shaded in grey, and the filled diamonds show messages that were sent to everyone. The three different fundraising phone call windows are plotted as open circles, and every volunteer was randomly assigned to be called during only one of these three windows.

CEP to volunteers during the tax season. While the other communications routinely reminded volunteers about opportunities to sign-up for hours, the fundraising calls did not mention volunteer hours, and were the only time that money donations were mentioned.

A third of the calls were made in January, after volunteers completed training and tax centers were opening (*Early* condition), another third in March before the late-season rush began (*Late* condition), and the final third was called once the whole season was over (*After* condition). The *After* condition was effectively a control condition for the two treatment conditions, and still allowed CEP to raise some funds from this group. We chose to use two treatment conditions because a volunteer's personal history with a firm grows considerably over a single season – especially first-time volunteers. We decided it was better to estimate the effect of that trajectory directly, rather than choosing a single calling time and assuming our estimates would generalize. This staggered design allows us to compare the effects of solicitation on subsequent volunteer hours using two kinds of control observations – both within people, before and after the call; and between people, comparing those in a given week who had already been called to those who had not yet been called.

3.5. Fundraising calls

Fundraising was performed over the phone. To ensure that the solicitations were done in a professional fashion and performed exactly according to the script, CEP contracted the task through a third-party call center, who were blind to the purpose of the experiment. They tracked all responses from callers, which were merged with CEP records.

The solicitation script was based on initial drafts by CEP (see Appendix A), describing who might benefit from the donation and CEP's short-term funding shortfall, neither of which would be new information for volunteers. These funding pressures had already been conveyed to the volunteers before the tax season began, to explain some service reductions. However, by the time the calls were made the schedule of service provision that year was completely locked in, and not contingent on the success of the fundraising. Furthermore, the funding pressure was due to a well-publicized budget shortfall in the Illinois state government in late 2011, affecting all publicly-funded programs state-wide, which mean that the funding cut was not a signal about the quality of the firm itself. There were also some slight variation in how the beneficiaries are described in the script.[5] However, this variation is completely orthogonal to the focus of this paper, and we collapse across different scripts in all analyses here.

After the script was read, if the volunteer agreed to make a donation, they were able to request a follow-up either through post or e-mail, with logistical details on how to donate. If they were not interested, the caller quickly concluded the call. We recorded final contributions as they were received by CEP, counting those who said they would donate but did not follow up as non-donors. The firm's intent was to deliver the solicitation in conversation, but this was logistically impossible because not all phone calls are answered. As a compromise, the protocol was to call each volunteer up to three times during their assigned solicitation week, and if, after three calls, the volunteer did not answer, the solicitation script was left as a voicemail, if possible. Our main analyses are all conducted at the level of Intent To Treat, including those who could not be reached directly, to guard against effects of selection into treatment. However, we confirm in Appendix B that these results are robust when we analyze at the level of Treatment on the Treated, among only those who picked up the phone.

[5] A full analysis of the differences between scripts is conducted in a separate paper by the authors [citation temporarily withheld to protect the double-blind review process]. Described in Appendix A here.

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4. Results

By the cut-off date for our sample, 1691 volunteers had registered online with CEP and completed the on-line training module, which collected their contact information. This group formed the basis of the call list, and their basic descriptive statistics are reported in Table 1. Assignment to waves was randomized, and analyses of the observables confirmed that assignment was balanced, with the curious exception that more people answered the phone in March than in January or May. The calling protocol was identical for each wave, so this may be a seasonal effect (which could not be controlled for in the random assignment). Furthermore, we explicitly control for this variable in Appendix B, in which we conduct robustness checks by estimating Treatment On the Treated among only those who pick up the phone.

4.1. Demographics

Demographic information was not mandatory so some of our data are incomplete – however, internal CEP records allowed us to perfectly observe whether volunteers were among the 34% who had participated as volunteers in a previous year, or whether they were among the 66% from whom this was their first year with CEP. It is worth noting that the typical long-time volunteer has a different demographic profile from the typical first-time volunteer, and Table 1 reports a simple contingency test for each demographic variable. Long-term volunteers tend to have higher household income, are more likely to be married, to be caucasian, to work in accounting/finance, and less likely to be a student or unemployed.

To distinguish the direct role of volunteer history from simple differences in composition, our primary results include alternative specifications in which we control for the effect of income – the demographic most relevant for fundraising – on both overall rates of volunteering, and on the post-call treatment effect. Income is specified using the (centered) ordered index of 15 categories in which participants actually gave their response (<20 k, 20–25 k, 25–30 k, and so on, up to >100 k). Though not shown here, we confirm that our results are robust under other transformations of the income variable (such as division into quartiles, or a log-transformation of category midpoints). In addition, Appendix C reports fixed effect models, which control for unobserved individual-level differences in volunteer behavior, and these analyses all come to the same conclusions as those reported in the main body.

Table 1
Descriptive statistics of the volunteer pool. Response rates are reported where applicable, and table quantities reflect the percentages among those who chose to respond. The final column reports chi-squared statistics for demographic differences between first-time and long-time volunteers.

	Response Rate	Total	By Call Timing			By Volunteer History		
			Early	Late	After	First-time	Long-time	x^2
No. of Volunteers	–	1691	567	570	554	1117	574	–
First Time Volunteer	–	34%	37%	31%	34%	100%	0%	–
Answered the Phone	–	45%	43%	51%	41%	47%	40%	6.4*
Gender-Male	99%	37%	38%	37%	36%	37%	37%	0.05
Married	87%	28%	25%	27%	31%	24%	35%	26***
Children in House	27%	33%	30%	39%	29%	36%	31%	0.73
Bachelor's Degree	90%	69%	67%	70%	71%	65%	77%	23***
Working Full-Time	97%	62%	61%	62%	64%	59%	68%	12
Ethnicity	84%							36***
White		38%	39%	34%	41%	35%	44%	–
Black		23%	24%	23%	23%	24%	22%	–
Hispanic		17%	16%	18%	16%	17%	16%	–
Asian		13%	13%	15%	12%	14%	12%	–
Other		9%	8%	10%	8%	10%	6%	–
Household Income	61%							40***
less than $20 k		24%	27%	24%	22%	28%	18%	–
$20 k–$40 k		21%	21%	20%	22%	24%	16%	–
$40 k–$80 k		28%	29%	27%	28%	26%	32%	–
more than $80 k		26%	24%	29%	27%	22%	34%	–
BLS Job Sector	83%							106***
Accounting/Finance		29%	29%	29%	29%	25%	35%	–
University Student		17%	16%	18%	16%	20%	10%	–
Unemployed		10%	11%	9%	10%	13%	6%	–
Professional/Manager		6%	4%	7%	6%	5%	7%	–
Retired		5%	6%	4%	6%	4%	8%	–
Government		5%	5%	5%	5%	5%	6%	–
Other		28%	29%	28%	28%	28%	28%	–

^ = $p < .10$.
* = $p < .05$.
** = $p < .01$.
*** = $p < 0.005$.

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4.2. Money donations

The descriptive statistics of the fundraising drive are summarized in Table 2. In total, only 30 volunteers who were solicited actually made donations (1.77% of the sample). However, the average donation among those who gave was high ($62.70 per donor), and the raw donation rates were higher (3.1%) among those who actually talked to the fundraiser over the phone. The small number of donors limits our ability to test many hypotheses about what factors predict higher donations. In Table 3, we report a series of regressions to predict donation amounts- measured both as (log-transformed) total donation amounts per person, as well as a dummy variable for the extensive margin, indicating whether a volunteer made any donation at all. The right hand side of these regressions include various standardized demographic covariates, as well as indicator variables for our randomized treatments (which were dummy-coded in comparison to the after-season group that was called in May).

We do not observe much difference in revenues across treatment waves. And, as predicted, there was a considerable effect of the volunteer's history. Volunteers who had a longer relationship with the firm – as indicated by whether they had also volunteered in a previous year – gave more on average, and were more likely to donate than first-time volunteers. It is important to emphasize, though, that the small number of actual donors in our sample means that regardless of statistical significance, these estimates explain only a small portion of the variance in this sample.

One possible concern is that this is an artifact of composition effects, and while the sample is not large enough for a full slate of robustness checks, we can test whether this is due to higher income among returning volunteers. Those analyses, reported in columns 3 and 6, show that when we include income as a covariate the effect of volunteer history is virtually identical. As a rough estimate of effect size, these analyses imply that in expectation, a call to a returning volunteer will generate 11% more revenue than a call to a first-time volunteer.

4.3. Volunteer time

The distribution of total hours per volunteer is shown in Fig. 2. There is wide variation in volunteer contributions over the season, and that figure does not include the 58.9% of registered volunteers who did not donate a single hour after training. We binned the hours data into weekly totals, for two reasons: firstly, the daily data exhibit heavy weekly cyclicality (mainly driven by weekend spikes). The second reason is that we do not know the exact day on which volunteers were reached by

Table 2
Fundraising results from the solicitation drive.

Call window	Number of calls	Total funds	Average per call	Number of donors	Percent donating	Average per donor
Early	567	$675	$1.19	8	1.41%	$84.38
Late	570	$772	$1.35	13	2.28%	$59.38
After	554	$435	$0.78	9	1.62%	$48.33
Total	1691	$1882	$1.11	30	1.77%	$62.73

Table 3
Regression models of donation decisions made in response to fundraising calls. All coefficients are unstandardized. The first three columns estimate the effect on total donations (i.e. log-transformed donation amount). The last three columns estimate the effect on probability of donating, using a binary variable indicating whether someone did (or did not) donate any money, irrespective of amount.

Predictors	1	2	3	4	5	6
Early (vs. After) Treatment	−0.002	−0.005	0.034	−0.143	−0.181	0.604
	(0.030)	*(0.030)*	*(0.045)*	*(0.490)*	*(0.491)*	*(0.636)*
Late (vs. After) Treatment	0.023	0.027	0.092^	0.346	0.393	1.265*
	(0.030)	*(0.030)*	*(0.044)*	*(0.438)*	*(0.440)*	*(0.582)*
Long-Time Volunteer		0.092***	0.107*		1.123**	1.107*
		(0.026)	*(0.037)*		*(0.377)*	*(0.447)*
Household Income			0.011*			0.130*
			(0.004)			*(0.050)*
Dependent Variable	log (1 + total dollars given)			binary: any donation [0/1]		
Regression Model	Linear	Linear	Linear	Logistic	Logistic	Logistic
Observed Volunteers	1691	1691	1035	1691	1691	1035
Adjusted- /pseudo-R^2	0.000	0.009	0.019	0.005	0.040	0.107

^ = $p < .10$.
* = $p < .05$.
** = $p < .01$.
*** = $p < 0.005$.

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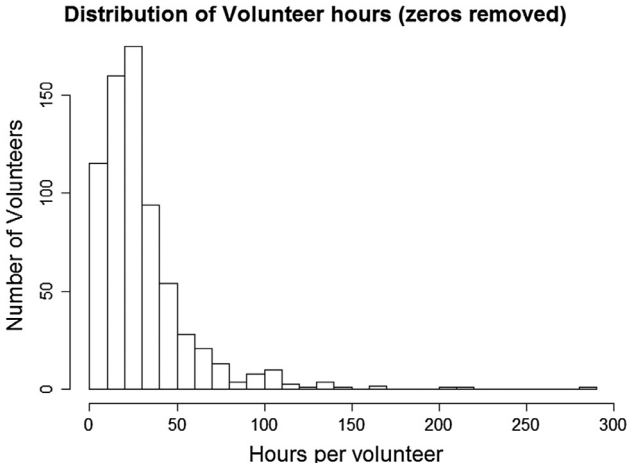

Fig. 2. Distribution of total hours donated by volunteers across the entire tax season. Note that this does not include 995 registered volunteers who had no hours on record.

phone, only the week during which they were assigned to be called. We conduct our analyses as though each volunteer was called simultaneously on the last day of their assigned calling period (week 4 or week 12, respectively). This is a conservative cut-off point between pre- and post-solicitation windows, and is certain to include some post-call data in the final pre-call week, which would bias our estimates of treatment effects towards zero.

The time course of raw average hours per week per volunteer, split by condition and across all 16 weeks of the tax season, is shown in Fig. 3, and the two mid-season calling periods are demarcated with vertical dashed lines (the third wave, in what would be week 21, is not shown). At first glance, these raw totals suggest that overall, volunteering levels are declining after fundraising calls, compared to the trends in un-called volunteers over the same time frame.

Formally, we estimate the effects of fundraising on volunteer hours using a panel regression model (Croissant & Millo, 2008), consisting of sixteen observations of (log-transformed) weekly hour totals for each volunteer in the sample. In the tables in the main text, we model individual-level differences in volunteering as random effects - this allows us to explicitly estimate the effect of time-invariant individual-level covariates. As a specification check we report Hausman test statistics for each model (1978). In many cases, we cannot reject the null hypothesis that random effects are sufficient. However in some cases we do reject this null. Accordingly, we report the corresponding fixed effects specification for every model in Appendix C, and the treatment effects estimates are similar across models.

The post-solicitation treatment effect is defined using dummy variables indicating all post-solicitation weeks (5–16 in the Early treatment, and 13–16 in the Late treatment). In all models we include weekly fixed effects, to control for the effects of the tax season's time course that are unrelated to treatment. All standard errors and statistical tests in all models are cluster-adjusted to be robust for both heteroskedasticity across weeks and autocorrelation within individuals (Arellano, 1987;

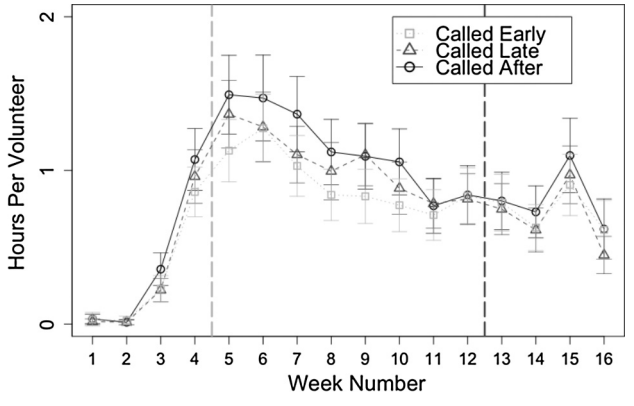

Fig. 3. Total hours donated per volunteer, by condition, divided into weekly bins. Dashed vertical lines represent the last day of each window for fundraising calls. Bars represent 95% confidence intervals for the raw mean of all observations within each cell.

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Bertrand, Duflo, & Mullainathan, 2004; White, 1980). Finally, in models where we are estimating more than one treatment effect at once – when we separately compare early and late calls to control, or when we estimate treatment effects within subgroups – we correct all our p-values to control for false discovery rate across multiple comparisons (Benjamini & Hochberg, 1995; Fink, McConnell, & Vollmer, 2014; List, Shaikh, & Xu, 2016).

Substitution of Time and Money Donations: An initial set of regression models are reported in Table 4. The first column tests for the presence of an average post-call effect. We find that on average, volunteers tend to slightly *reduce* their hours contributions after being called, though this main effect does not reach significance at standard levels. The remaining columns test for substitution, with an indicator of whether the volunteer had donated money during fundraising. There is a clear relationship between the two types of giving, in that those who donated money were also more likely to donate time. Furthermore, volunteers' previous donations had a significant interaction with the treatment effect of fundraising (columns 2 and 4). That is, the small subsample of volunteers who donated money *increased* their hours contributions after solicitations by roughly 40%, even beyond their initially higher baseline (columns 3 and 5).

We are very cautious about over-interpreting a result that is identified on a subsample of only 30 people. Still, our results suggest that individual-level substitution was not responsible for any aggregate firm-level substitution. Furthermore, there may be important complementarity for those who choose to donate – though results from such a small sample should be considered preliminary.

Even putting aside concerns about sample size, it is very hard to determine the causality in this effect due to endogeneity – that is, volunteers who donate money are *ex ante* different from volunteers who do not donate, as our own results above can attest. Thus, the donations themselves may not have any direct impact, over and above these other differences. However, we can at least rule out some obvious confounding variables, such as income (columns 4 and 5) and other individual-level differences (reported in Appendix C). The null result on income has special theoretical significance for two reasons. First, higher-income people are more likely to donate money - in our results, and in most other settings. Second, people who earn higher wages should have higher opportunity costs of time, in which case they would gain the most from substituting monetary donations for volunteer hours. Thus, we expected that high-income people would be the most likely to want to substitute from time to money, and yet we found no evidence that they used the fundraising calls to conduct this substitution.

Effect of Volunteering History: The next set of regression models were designed to test for heterogeneity in the treatment effect with respect to the volunteer's history with the firm. The regressions in Table 5 test two specifications of volunteer history, first by estimating separate treatment effects for long-time volunteers and first-time volunteers (columns 3 and 4), and second by further dividing treatment effects by early-season and late-season calls (columns 5 and 6). The results indicate that there is indeed a significant spillover effect in a subsample of the volunteer pool, whereby the spillover costs of

Table 4
Regression models of hours volunteered per week, per person, testing for heterogeneity in treatment effects with respect to donation status. All models estimate the effects on (log-transformed) total hours donated per week, and all coefficients are unstandardized.

Predictors	1	2	3	4	5
After-Call Effect (all volunteers)	−0.019	−0.024		−0.031	
	(0.016)	*(0.016)*		*(0.020)*	
Donated Money		0.283***	0.283***	0.250**	0.250**
		(0.089)	*(0.089)*	*(0.093)*	*(0.093)*
After-Call Effect *x* Donated Interaction		0.365***		0.338***	
		(0.105)		*(0.108)*	
After-Call Effect (donors only)			0.341***		0.307***
			(0.105)		*(0.107)*
After-Call Effect (non-donors only)			−0.024		−0.031
			(0.016)		*(0.020)*
Household Income				0.018***	0.018***
				(0.003)	*(0.003)*
Income *x* After-Call Interaction				0.003	0.003
				(0.003)	*(0.003)*
Dependent Variable	log (1 + hours per week per person)				
Observed Volunteers	1691	1691	1691	1035	1035
Total Observations	27,056	27,056	27,056	16,560	16,560
Week Dummies	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES
X^2-test: RE vs. FE	2.89^	5.51	5.51	4.63	4.63
Adjusted R^2	0.410	0.411	0.411	0.427	0.427

^ = $p < .10$.
* = $p < .05$.
** = $p < .01$.
*** = $p < 0.005$.

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Table 5
Regression models predicting hours volunteered per week, per person, testing for heterogeneity in treatment effects with respect to volunteer history. All models estimate the effects on (log-transformed) total hours donated per week, and all coefficients are unstandardized.

Predictors	1	2	3	4	5
First-Time Volunteer	−0.192***	−0.182***	−0.173***	−0.172***	−0.166***
	(0.029)	(0.022)	(0.028)	(0.022)	(0.028)
After Call		−0.046*	−0.046^		
(first-timers only)		(0.017)	(0.021)		
After Call		0.034	0.013		
(long-timers only)		(0.026)	(0.031)		
After Call - Early				−0.075***	−0.074^
(first-timers only)				(0.023)	(0.031)
After Call - Early				0.066	0.032
(long-timers only)				(0.038)	(0.043)
After Call - Late				−0.015	−0.016
(first-timers only)				(0.020)	(0.025)
After Call - Late				−0.010	−0.014
(long-timers only)				(0.030)	(0.036)
Household Income	0.017***		0.016***		0.016***
	(0.003)		(0.002)		(0.002)
Household Income			0.004		
x After Call			(0.003)		
Household Income					0.007
x After Early Call					(0.005)
Household Income					0.000
x After Late Call					(0.004)
Dependent Variable	log (1 + hours per week per person)				
Observed Volunteers	1035	1691	1035	1691	1035
Total Observations	16,560	27,056	16,560	27,056	16,560
Week Dummies	YES	YES	YES	YES	YES
Clustered SEs	YES	YES	YES	YES	YES
X^2-test: RE vs. FE	0.0	5.69	4.50	25.9***	24.3
Adjusted R^2	0.426	0.410	0.425	0.411	0.087

^ = $p < .10$.
* = $p < .05$.
** = $p < .01$.
*** = $p < 0.005$.

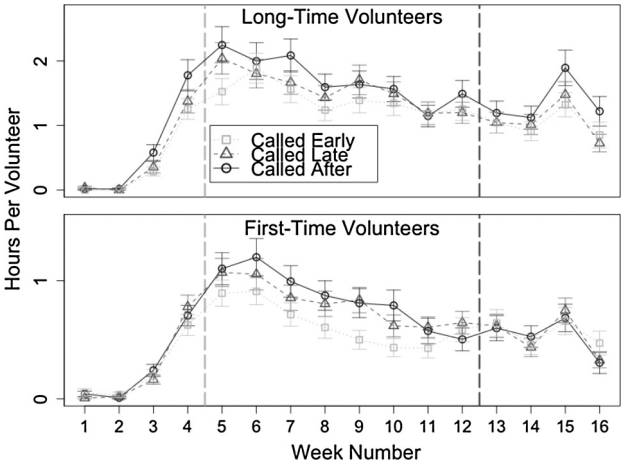

Fig. 4. Total hours donated per volunteer, by condition, divided into weekly bins, split into long-time volunteers (top panel) and first-time volunteers (bottom panel). Dashed vertical lines represent the last day of each window for fundraising calls. Bars represent 95% confidence intervals for the raw mean of all observations within each cell.

fundraising are concentrated almost exclusively among first-time volunteers, and even more specifically, among first-time volunteers who were called early in their experience with the firm. That is, the shorter a volunteer's tenure with the firm, the more likely they are to respond to a fundraising call by reducing their future volunteering.

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This heterogeneity can be seen in Fig. 4, which plots, separately, the time course of donated hours among long-time and first-time volunteers – the drop in hours among first-time volunteers after the early-season call is evident, in contrast to the relatively stable trends after the early-season call among long-time volunteers, and after the late-season call in both groups. The difference between first-time and long-time volunteers is not due to composition effects, as the estimated effects are essentially unchanged when we include income in the model as a demographic control. Additionally, these differences are not just driven by the long-time volunteers who donated money, as the main results hold when we focus strictly on non-donors as well.

These estimates imply that an early call to a first-time volunteer reduces their future volunteering by 8%, relative to expectation. As a rough estimate of magnitude, we consider a hypothetical in which every first-time volunteer would have been called during the early calling window. Over the following 12 weeks, the effect of these calls would add up to a total decline of 0.91 h per first-time volunteer. For comparison, this magnitude is equivalent to the average amount of volunteering that would be lost if the firm simply shortened the tax season by 1.3 weeks. For another perspective, consider that the average donation from first-time volunteers was only $0.35 per call. Together, these two results imply that fundraising calls to new volunteers during the early calling window could only be net-positive if the firm valued their time below $2.60 per hour – a very low wage for high-skill labor.

5. Discussion

The experiment in this paper was designed to estimate how volunteers at a charitable organization would respond to a fundraising solicitation from that organization. Our unique dataset comes from a non-profit firm that asked their pool of volunteers if they were willing to contribute money, and tracked both the monetary donations and the volunteer hours logged before and after the ask. The firm randomized the time when volunteers were called, allowing us to estimate the causal effects of fundraising, while adjusting for secular trends and composition effects. We found that fundraising generated more donations from long-time volunteers than first-time volunteers. We also found that after fundraising, there was no evidence for substitution between volunteering time and donating money. Furthermore, we found a spillover cost of fundraising – that is, a drop in volunteer hours – among volunteers who did not donate money and were new to the firm, while long-term volunteers kept up their high contributions of time, regardless of whether they donated money.

5.1. Theoretical contributions

This paper gives a novel demonstration of spillover costs for a fundraising firm, and this result is distinct from previous work on unintended consequences of solicitation. For example, some papers have shown that fundraising can impose spillover costs on its targets (Dellavigna et al., 2012; Van Diepen et al., 2009). Other work has shown that fundraising can suppress the efficiency of future fundraising in the same dimension, essentially a diminishing marginal return from repeated solicitation (Cairns & Slonim, 2011; Meier, 2007). Instead, we show that spillover costs can result in a negative marginal return for the firm, since spillover costs were largest among the people who *ex ante* were least likely to donate, and also those who *ex post* did not donate.

This research also contributes new evidence to the literature on how personal connection can affect charitable giving. Our fundraising results replicated many previous papers that have shown how person history relates to contribution increases (Kessler & Milkman, 2016; Landry et al., 2010; Meer, 2013; Small & Simonsohn, 2008). But our volunteering results suggest a new consequence, which is that that personal history can mitigate contribution decreases. This result is consistent with suggestive trends in related work that shows a similar interaction pattern[6] (Karlan & Wood, 2014; Karlan et al., 2011). Of course, personal history is not exogenous, and while we rule out composition effects on observable demographics, there may be relevant unobservables (such as attentiveness to firm communications, or initial support for the cause). But even if the estimated effect is completely endogenous, it could still enter into a firm's calculation of whom (and when) to solicit for fundraising.

Finally, this work reinforces the distinction between firm-level substitution as an aggregate result, and individual-level substitution as a behavioral mechanism. Economists and practitioners are often concerned about whether fundraising increases total contributions, or merely draws away from other kinds of giving (Duncan, 1999; Feldman, 2010; Meier, 2007). In these results we found firm-level substitution, but not individual-level substitution within the firm. This pattern of results implies that the firm-level substitution could have been reduced if the firm had known which volunteers would be most susceptible to spillover effects. It is well known that the direct effects of fundraising (on money donations) vary considerably across the population (Al-Ubaydli & Lee, 2011; Srnka, Grohs, & Eckler, 2003), and thus it is likely that there is similar heterogeneity in the spillover effects, as well, including firm- and context-specific influences. This information could have a significant impact on the firm's decision of who, when, and how to fundraise.

[6] Their estimates of spillover effects are not significant, and much smaller than ours. However, they were also looking at spillover effects on money donations, *within* the fundraising domain. Any spillover effects in their sample would have been difficult to detect, apart from the direct effect of asking for money. In contrast, our data reflect donations in volunteering, *outside* the fundraising domain, where the direct effects were minimal.

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5.2. Limitations and future directions

In these data we found no evidence for individual-level substitution, and preliminary evidence among a small sample that suggests individual-level complementarity. However, the there may be other moderators that affect how well these results generalize to other contexts. Within-firm solicitations may naturally imply complementarity, especially when the money donated might be used to make future volunteering more productive. This was also a one-time solicitation brought about by external pressures, but volunteers may shy away more quickly from a fundraiser that is more persistent, or less in need. Finally, our experiment provided a unique look into short-term substitution within a firm, but it is also possible that substitution might be more common over longer time periods, or between firms. We leave these important questions to future research.

Although the theoretical motivation for this paper is agnostic about the particular dimensions of giving in question, we only provide results for the effect of solicitation in one domain (money) on giving in another (volunteer time). Our data cannot tell us whether these results would be similar in situations where the dimensions were reversed (i.e. the effect of volunteer solicitations on money donations), or where other dimensions of giving may also be affected. Although there are reasons to believe that time and money have special psychological salience to donors (Lacetera & Macis, 2010; Liu & Aaker, 2008; Pfeffer & DeVoe, 2009), these are not necessary to explain the results here, and ultimately this is an empirical question for future work. Additionally, we also make no claim about the substitutability or complementary of giving between different firms, within an individual's entire portfolio of giving.

We were somewhat surprised by the low donation rates, which were closer to typical levels for cold-list fundraising, rather than for people who had given in the past. It may be that switching across dimensions of giving can disrupt habitual donations (Landry et al., 2010; Meer, 2013). Furthermore repeated fundraising is typically spaced over time (e.g. annually) while the fundraising and volunteering requests here were essentially overlapping. Previous research has also demonstrated that all else equal, many people would rather give time than give money (e.g. Brown, Meer, & Williams, 2016; Lilley & Slonim, 2014), so volunteering may have satiated their desire to give. Future research is needed in settings where the experimenter has greater control to, for example, manipulate the opportunity to volunteer.

The substitution effect we observe may give pause to decision-makers for future fundraising efforts. We think it is vitally important to explore the mechanisms and boundary conditions of this effect, so that practitioners can determine how much care (if any) should be taken to forestall future substitution effects. Our work showed that new volunteers were most likely to experience these effects, but it is critical to understand what inoculates longer-term volunteers from these effects, and whether this can be encouraged by the firm. If it is merely information about the worthiness of the cause, then perhaps this information could be delivered more effectively during recruitment. If the mechanism is more closely tied to identity concerns, then early volunteering efforts could be oriented towards activities that more clearly build that connection (Kessler & Milkman, 2016). Furthermore, in some contexts new volunteers will already have a deep personal connection to the cause – for example, close connections to those who have been affected (Small & Simonsohn, 2008). The presence and magnitude of spillover effects will vary across many situational factors, and future research in different settings can help develop a better understanding of this variation across contexts.

5.3. Concluding remarks

This research provides useful guidelines for fundraising decisions in other firms. In particular, they should be aware of the potential spillover effects in other aspects of their relationship with the solicitee (volunteering, reputation, retention, etc.). Our data were unique in that the spillover costs of fundraising were easily observed, but in other situations they may be hard to quantify. Even when the firm's marginal rates of substitution is clear, the overall value of a fundraising appeal depends not just on how they call, but who they call, and when. As a general rule, fundraisers should consider how a solicitation affects its targets, beyond the direct revenue stream – a proper accounting of the costs and benefits of fundraising may need to consider the spillover effects as well.

Disclosure

The authors declare that this work is entirely their own, and not published or submitted for publication elsewhere. The authors declare that they have no conflict of interest with any of the material, and that no outside relationships had any influence on the results presented here. Both authors contributed to the design, analysis and preparation of this paper.

For each study, we report how we determined our sample size, all data exclusions, all manipulations, and all measures. The data and analysis code are available as Online Supplementary Materials at the following link: https://osf.io/f7e92/.

Appendix A

A representative script used for solicitations over the phone. Within each wave, the scripts were randomized so that some also included a specific name of a CEP client (as shown in square brackets) while others did not include the reference to a

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name but were otherwise identical. We collapse across this difference in our analyses here, so that all calls are treated the same as long as they were made in the same time window. If the volunteer responded affirmatively after this script, they were offered a follow-up through either post or email, through which they could send in their donation.

> "As you know we are a non-profit, and we want you to consider making a small donation to support our cause. There have been large statewide budget cuts that have forced us to cut back our services, preventing volunteers like you from helping more CEP clients [such as *insert name*] in need. Even a small donation will make it possible for us to reach others [like *insert name*] who need our services."

Appendix B

The regressions in this table re-estimate models from Tables 4 and 5 at the level of Treatment On the Treated – that is, among those people who answered the fundraising phone call. In general, these results validate the conclusions of the Intent To Treat analyses above.

Predictors	1	2	3	4
After Call Effect	−0.030		−0.049	
	'(0.024)		*'(0.054)*	
Donated Money		0.334***		
		'(0.104)		
After-Call Effect(donors only)		0.372**		
		'(0.130)		
After-Call Effect(non-donors only)		−0.037		
		'(0.024)		
First-Time Volunteer			−0.185***	−0.181***
			'(0.037)	*'(0.037)*
After Call(first-timers only)			−0.056*	
			'(0.025)	
After Call(long-timers only)			0.024	
			'(0.038)	
After Call - Early(first-timers only)				−0.123***
				'(0.034)
After Call - Early(long-timers only)				0.004
				'(0.055)
After Call - Late(first-timers only)				0.007
				'(0.031)
After Call - Late(long-timers only)				0.056
				'(0.045)
Dependent Variable	log (1 + hours per week per person)			
Observed Volunteers	757	757	757	757
Total Observations	12,112	12,112	12,112	12,112
Week Dummies	YES	YES	YES	YES
Clustered SEs	YES	YES	YES	YES
X^2-test: RE vs. FE	2.94^	3.94	5.63	32.3***
Adjusted R^2	0.427	0.428	0.427	0.427

Legend: ^ = $p < .10$; * = $p < .05$; ** = $p < .01$; *** = $p < .005$.

Appendix C

Robustness checks for all panel regression models reported above. Whereas the above specifications modeled individual-level variation as random effects, here we specify the corresponding fixed effects models. Although these models cannot estimate time-invariant effects (e.g. simple demographic effects), they can estimate our treatment effects while controlling for unobserved individual-level heterogeneity. Furthermore, they require fewer assumptions about the data-generating process. The estimates here generally corroborate those in the tables above, providing further evidence that our conclusions are robust across a range of modeling approaches.

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Table 4 with fixed effects

Predictors	1	2	3	4	5
After-Call Effect(all volunteers)	−0.017	−0.024		−0.031	
	(0.016)	*(0.016)*		*(0.020)*	
After-Call Effect *x*Donated Interaction		0.380***		0.354***	
		(0.105)		*(0.107)*	
After-Call Effect(donors only)			0.356***		0.323**
			(0.105)		*(0.107)*
After-Call Effect(non-donors only)			−0.024		−0.031
			(0.016)		*(0.020)*
Income *x* After-Call Interaction				0.005	0.005
				(0.003)	*(0.003)*
Dependent Variable	log (1 + hours per week per person)				
Observed Volunteers	1691	1691	1691	1035	1035
Total Observations	27,056	27,056	27,056	27,056	16,560
Week Dummies	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES
Adjusted R^2	0.447	0.448	0.448	0.463	0.463

Legend: ^ = $p < .10$; * = $p < .05$; ** = $p < .01$; *** = $p < .005$.

Table 5 with fixed effects

Predictors	1	2	3	4
After Call(first-timers only)	−0.047*	−0.049^		
	(0.017)	*(0.022)*		
After Call(long-timers only)	0.042^	0.025		
	(0.025)	*(0.030)*		
After Call - Early(first-timers only)			−0.083***	−0.087*
			(0.023)	*(0.032)*
After Call - Early(long-timers only)			0.087*	0.058
			(0.036)	*(0.042)*
After Call - Late(first-timers only)			−0.014	−0.014
			(0.021)	*(0.026)*
After Call - Late(long-timers only)			−0.013	−0.016
			(0.029)	*(0.036)*
Household Income*x* After Call		0.005		
		(0.003)		
Household Income*x* After Call - Early				0.010
				(0.005)
Household Income*x* After Call - Late				0.000
				(0.004)
Dependent Variable	log (1 + hours per week per person)			
Observed Volunteers	1691	1035	1691	1035
Total Observations	27,056	16,560	27,056	16,560
Week Dummies	YES	YES	YES	YES
Clustered SEs	YES	YES	YES	YES
Adjusted R^2	0.448	0.463	0.448	0.463

Legend: ^ = $p < .10$; * = $p < .05$; ** = $p < .01$; *** = $p < .005$.

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Appendix B with fixed effects

Predictors	1	2	3	4
After Call Effect	−0.037		−0.049	
	(0.024)		(0.054)	
After-Call Effect(donors only)		0.370**		
		(0.130)		
After-Call Effect(non-donors only)		−0.037		
		(0.024)		
After Call(first-timers only)			−0.055^	
			(0.026)	
After Call(long-timers only)			0.038	
			(0.037)	
After Call - Early(first-timers only)				−0.134***
				(0.035)
After Call - Early(long-timers only)				0.027
				(0.053)
After Call - Late(first-timers only)				0.006
				(0.032)
After Call - Late(long-timers only)				0.051
				(0.044)
Dependent Variable	log (1 + hours per week per person)			
Observed Volunteers	757	757	757	757
Total Observations	12,112	12,112	12,112	12,112
Week Dummies	YES	YES	YES	YES
Clustered SEs	YES	YES	YES	YES
Adjusted R^2	0.464	0.465	0.464	0.465

Legend: ^ = $p < .10$; * = $p < .05$; ** = $p < .01$; *** = $p < .005$.

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