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Announcements of Support and Public Good Provision†

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Providing information about contributions to public goods is known to generate further contributions. However, it is often impossible to provide verifiable information on contributions. Through a large-scale field experiment and a series of laboratory experiments, I show that nonbinding announcements of support for a public good encourage others to contribute, even when actual contributions might not or cannot be made. Providing a way to easily announce support for a charity increases donations by $865 per workplace fundraising campaign (or 16 percent of average giving). I discuss implications for understanding prosocial behavior and for organizations aiming to increase contributions to public goods. (*JEL* C93, D64, D83, H41, L31)

A number of theories appeal to social forces in an attempt to explain why individuals contribute to public goods at levels well above what would be predicted by altruism (Becker 1974; Batson 1991). These theories suggest that individuals learn from the contributions of others (Vesterlund 2003) or that individuals can be motivated to contribute by reciprocity (Sugden 1984), conformity (Bernheim 1994), or the desire to do their "fair share" (Sen 1977; Rose-Ackerman 1982). These theories also predict that at least some individuals will contribute more to a public good when they learn that others contribute or observe others explicitly promising that they will contribute. This behavior is called conditional cooperation and significant laboratory and field evidence is consistent with conditional cooperation.[1]

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[1] For laboratory evidence see, e.g., Keser and van Winden (2000) and Fischbacher, Gächter, and Fehr (2001). For field evidence on conditional cooperation in charitable giving, see Frey and Meier (2004); Croson and Shang (2008); Martin and Randal (2008); and Shang and Croson (2009). On energy use, see Allcott (2011). On prosocial job choice, see Coffman, Featherstone, and Kessler (2017). For literature on individuals making explicit statements or promises about their intent to contribute in a social dilemma game, see, e.g., Ostrom, Walker, and Gardner (1992) and Parks, Henager, and Scamahorn (1996). See Sally (1995) for a meta-analysis of games with preplay communication. See Cason and Mui (1998) for related work on the effect of payoff-irrelevant information on charitable behavior.

But to what extent can social forces explain the pattern and amount of public good contribution observed in practice? Verifiable information about others' actions or others' explicit promises to contribute (e.g., through a pledge) is often unavailable. If these are necessary to influence behavior, social forces are likely to have limited impact. If individuals additionally respond to announcements of support that may or may not be associated with an actual contribution, then such social forces may be capable of generating public good provision more broadly. Indeed, announcements supporting a public good—made without proof or a promise of contribution—are widespread. They include posts on social media, petition signatures, yard signs, bumper stickers, worn apparel, and ubiquitous verbal and written statements supporting a charity, candidate, or cause.

Do such announcements of support affect the contributions of others even though they could be cheap talk? This paper finds that individuals respond to announcements of support and that the effect of announcements can be large in magnitude. It analyzes results from a large-scale field experiment, conducted with the local branch of a major national charity, and two complementary laboratory experiments. In all experiments, some subjects are given a way to announce support for a charity to others, and subjects exposed to such announcements of support make larger donations.[2]

In the field experiment, workplace fundraising campaigns were randomly selected to receive a bag of pins (i.e., buttons) along with other campaign materials provided by the charity. The pins were printed with "I Support" and the charity name and logo. The "I Support" language was chosen to avoid referencing a specific action or making an explicit promise. The employee running each workplace campaign was instructed to give pins to his or her coworkers and to encourage them to wear the pins around the office before donations were made. A two-stage least squares estimation strategy finds that pin use in a workplace campaign increases donations by an average of \$865 per workplace (a 16 percent increase on a mean of \$5,341 per workplace). Each \$0.17 pin sent to a workplace generated \$3.85 in extra donations to the charity.

To further demonstrate that announcements of support affect giving, to rule out alternative theories for why pins might have affected giving in the field, and to explore how announcements of support affect giving, we ran two complementary laboratory experiments. The first laboratory experiment gave subjects the opportunity to donate to United Way and provided subjects in some treatments with pins that read "I Support United Way." Subjects in all treatments were exposed briefly and silently to another subject at the front of the room before making donation decisions. In a treatment where subjects could choose to wear the pin during this interaction, subjects randomly exposed to a pin wearer donated 69 percent more than those exposed to a subject who did not wear the pin. The second laboratory experiment replicated the results of the first with more statistical power, over a wider array of

[2] The motivation of the paper is to investigate private provision of public goods. The experiments identify effects on charitable giving. Consequently, this paper enters a rich experimental literature on motivations for charitable giving both in the field (List and Lucking-Reiley 2002; Frey and Meier 2004; Landry et al. 2006; Falk 2007; Karlan and List 2007; Eckel and Grossman 2008; Croson and Shang 2008; Shang and Croson 2009; Ariely, Bracha, and Meier 2009; Landry et al. 2010; Gneezy et al. 2010; DellaVigna, List, and Malmendier 2012) and in the laboratory (Ottoni-Wilhelm, Vesterlund, and Xie 2017; for surveys, see Andreoni and Payne 2013 and Vesterlund 2016). Relative to this literature, we elucidate a mechanism that has a substantial impact on charitable donation and investigate how and why it influences behavior.

charities, using announcements in the form of the message "I support this charity" sent anonymously over the computer.

Results from the laboratory experiments also provide insight into why announcements of support affect donations. Subjects believe that those who announce support donate more. In an experimental treatment in which the announcer's donation is also revealed, exposure to an announcement of support has no additional effect on donations. We find that subjects respond to announcements of support for similar reasons as they engage in conditional cooperation. Observing an announcement of support increases beliefs about charity quality, beliefs about average giving, and beliefs about what subjects should donate. However, unlike previous work on conditional cooperation, we show that subjects respond to what others *might* donate, even when announcements could be cheap talk. In a further treatment, we find effects of announcements that are just as large when the announcer is not allowed to make a donation as part of the experiment, demonstrating that subjects additionally respond to what others *would* donate, even if they cannot do so. Taken together, these findings highlight that individuals look to others to decide about their own donations—even when others' announcements are disconnected from actual donations—implying significant additional power for social forces to influence public good provision.

This paper includes a field experiment and laboratory experiments. Often, experimental papers with both a lab and field component replicate the same experiment across settings to investigate the external validity of a result. Here, a field experiment is run in a rather complex environment (a workplace fundraising campaign) to show that an effect is economically significant, and controlled laboratory experiments investigate potential mechanisms that might be generating the result in the field, demonstrating another way laboratory and field experiments can be complements (see Kessler and Vesterlund 2015 on lab and field complementarities).

The paper is organized as follows. Section I reports on the field experiment. Sections II and III report on the first and second lab experiments, respectively. Section IV compares the lab and field settings and results. Section V discusses the implication of the results and concludes.

I. Field Experiment

The field experiment was conducted at a local branch of an anonymous national charity (when the charity name is required, "*Charity*" is used). The charity does not provide services directly, but serves the dual roles of raising funds and distributing those funds to nonprofits that provide services. While the charity operates nationally, its branches raise money locally and distribute those funds to local service organizations. The branch we partnered with raises the majority of its funds from workplace campaigns.[3] In workplace campaigns, a staffer at the charity makes contact with an employee campaign manager (or ECM, a workplace employee) who runs the campaign by asking their coworkers to donate money to the charity.

Workplace campaigns provide a unique opportunity for research, since they are settings in which individuals make charitable decisions in preexisting social groups.

[3] Donations in workplace campaigns are collected via payroll deduction, check, or credit card. Workplace campaigns typically last one to four weeks and are run between September and December.

The field experiment involved over 36,000 workers at 278 workplaces who donated a total of over $1.48 million in 2009 (an average of 21 employees per workplace made donations that averaged $255 each). Workplaces in the experiment were a select group of workplaces in the charity database that were randomized into treatments for the 2009 campaign.

Workplaces in the control condition received the standard campaign materials (the same materials as in the 2008 campaign: a list can be found in online Appendix Table A1). Workplaces in the *pins treatment* additionally received a bag of 1.25-inch diameter pins (i.e., buttons), one for each employee, on which was printed "I Support *Charity*." The instructions provided to the ECM were to give a pin to everyone at the workplace (to wear voluntarily if the employee wished) and not as a reward for contributing. Thus, the pins were not meant to generate a prestige motive (see Harbaugh 1998a, b) or to give explicit information about previous donations. Instead, pins used as part of the campaign lower the cost of announcing support for the charity.[4]

We could not mandate that pins be used by the workplaces assigned to the pins treatment. We addressed this limitation of the field setting in two ways. First, we attempted to maximize the likelihood that ECMs would use the pins by placing the bag of pins on top of all other materials in the box of campaign materials the ECM received from the charity and by instructing charity staffers to discuss the pins with the ECM at each workplace that received them. Second, we introduced a third experimental treatment, the *pins and raffle treatment*, which our charity partners thought would increase the rate of pin use. Workplaces in the pins and raffle treatment received tickets to run a raffle—with a prize of two movie passes to AMC Theatres—that was an excuse to gather workers together to hand out the pins. In contrast to expectations of the charity partners, this treatment did not increase pin use. Use of materials, as established by a phone survey of ECMs after the experiment, is reported on the bottom of Table 1 (see details in the next subsection), and including raffle materials along with the pins slightly decreased the use of the pins (from 45 percent to 39 percent). While this was the opposite of the intended effect of including raffle materials along with the pins, the differential use of the pins across treatments can still be leveraged in our empirical specifications investigating the effect of pin use on giving.

A. *Data*

Results are from the 278 workplaces that were eligible to participate in the experiment and generated money for the charity in at least one of the six years preceding the experiment. These workplaces either received the standard campaign materials and are in the control condition (40 percent of the workplaces were randomized into the control condition) or received additional materials (30 percent in each of the

[4] It is already possible to announce support in workplace campaigns (e.g., employees can email each other or talk about the charity at the water cooler). The test here is whether pin use, which lowers the cost of announcing support by providing an easy, noninvasive way to make such announcements, affects giving. Finding an effect of pin use on giving serves as a proof-of-concept that announcements of support have the potential to affect behavior in a field setting.

Table 1—Pretreatment Summary Statistics and Material Use by Treatment (*Field Experiment*)

	Control	Pins	Pins and raffle
Workplaces	108	86	84
Share (40%/30%/30%)	39	31	30
Average number of donors 2008	23.08	18.14	27.07
	(29.39)	(25.59)	(34.98)
Average number employees 2008	117.05	103.22	176.45*
	(129.70)	(109.90)	(314.66)
Average $ donation 2008	4,817.63	5,821.70	7,242.33
	(9,049.68)	(14,499.67)	(13,816.06)
Participation 2008	0.36	0.29	0.29
(number of donors 2008/number of employees 2008)	(0.36)	(0.32)	(0.31)
Nonprofit: may receive grants from the charity	48	38	42
	(0.50)	(0.49)	(0.50)
Responded to mailed pre-survey (July 2009)	26	21	24
	(0.44)	(0.41)	(0.43)
Reached via phone post-survey (in March to May 2010)	69	72	73
	(0.47)	(0.45)	(0.45)
Use pins only		45	13
		(0.50)	(0.34)
Use raffle only			17
			(0.37)
Use both			26
			(0.44)

Notes: Standard deviations are in parentheses. * indicates that the data are significantly different from control condition at 10 percent level using a t-test. The pre-survey was sent to ECMs in July 2009 before the experiment. The phone post-survey was conducted in March 2010 and established use of materials. College student volunteers and full-time staffers for the charity conducted the post-survey. Workplaces that did not respond to the post-survey are assumed not to have used the included materials.

other two treatments). Online Appendix A.2 provides a detailed description of the process by which workplaces were selected and treated.

Dependent variables come from administrative data on the amount of money donated by workplaces to the charity from 2003 to 2009. Covariates come from the charity's administrative data (number of donors, number of employees, staffer assigned to the workplace, workplace industry, and whether the workplace is a nonprofit and so may receive funding from the charity). Table 1 shows pretreatment data for the 278 workplaces across the three treatments. The conditions are well balanced.[5]

B. *Results*

Figure 1 shows the kernel densities of the amount donated in 2009 for workplaces in each treatment. The kernel density for the two treatments that received the pins—the pins treatment and the pins and raffle treatment—are to the right of the control density. Figure 2 shows the change in the amount donated for workplaces from 2008

[5] The pins and raffle treatment has two outlier workplaces with 1,200 and 2,500 employees, which gives it a higher average number of employees than the control group ($p < 0.1$). Since outliers are driving the difference, it is not significantly different using a rank-sum test ($p = 0.18$).

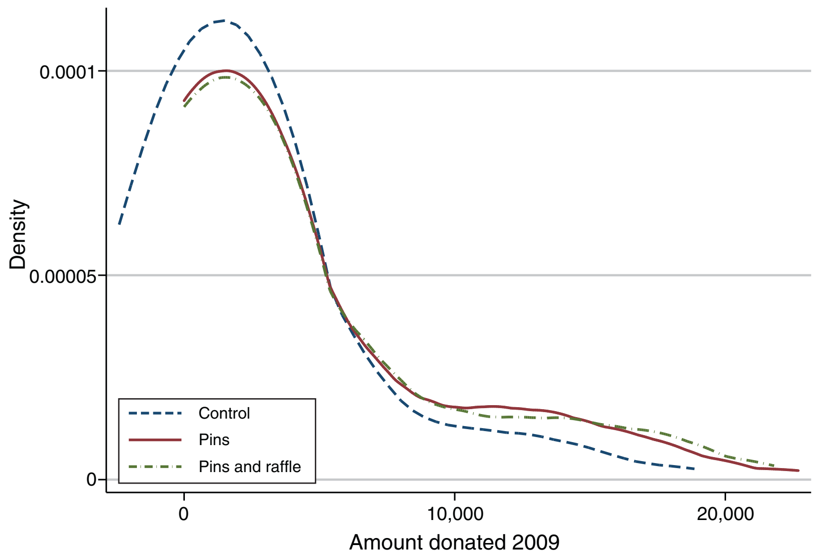

Figure 1. Amount Donated by Treatment (*Field Experiment*)

Note: Bandwidth is \$2,400; for display purposes excludes the top 5 percent of workplaces (> \$23,000).

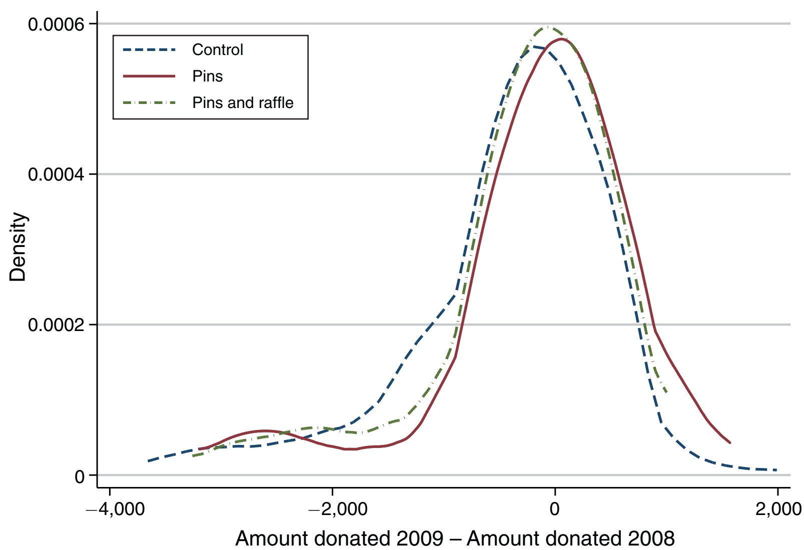

Figure 2. Difference in Amount Donated by Treatment (*Field Experiment*)

Note: Bandwidth is \$400; for display purposes excludes the top 5 percent (> \$1,600) and bottom 5 percent (< −\$3,275) of changes in donations.

to 2009 for each treatment. The kernel density for the pins treatment is to the right of the control density, and the pins and raffle density falls in between the other two (see online Appendix Figures A1 and A2 for the same data presented in histograms).

As noted, we could not force workplaces to use the pins in their campaigns. However, workplaces in the control condition did not have access to pins for their campaigns and so random assignment to the pins treatment or pins and raffle treatment vastly increased the likelihood of pin use from 0 percent to 45 percent and 39 percent, respectively. Since we are explicitly interested in pin use at the workplace and how it affects giving, we adopt a two-stage least squares specification, which allows us to estimate the effect that we care about, how pin use at the workplace affects giving to the charity (this approach is common in experimental settings with imperfect compliance, see, e.g., Duflo and Saez 2003). The basic two-stage least squares specification we estimate is

$$\text{(1)} \qquad \textit{Use pins}_j \;=\; \alpha_1 \times \textit{pins}_j + \alpha_2 \times \textit{pins and raffle}_j + \boldsymbol{\alpha}_3' \mathbf{X}_j + u_j,$$

$$\text{(2)} \qquad D_j \;=\; \beta_1 \times \textit{Use pins}_j + \boldsymbol{\beta}_2' \mathbf{X}_j + v_j,$$

where j indexes workplaces and D_j is the dependent variable of interest: either the amount donated by workplace j in 2009 or the difference in donation for workplace j between 2008 and 2009. While the workplaces are rather similar across treatments on average, there is significant variation in the size and success of workplace campaigns in the experiment. Consequently, the empirical specifications include a vector of controls, $\mathbf{X}_j$, for both historic giving by year and other workplace characteristics.[6] In (1), $\textit{pins}_j$ is a dummy equal to 1 if workplace j was in the pins treatment and $\textit{pins and raffle}_j$ is a dummy equal to 1 if workplace j was in the pins and raffle treatment.

In the first stage, random assignment to treatment induces significant variation in pin use (the F-statistic is 54.8). However, interpreting the coefficients estimated in the second stage as purely the effect of pin use on giving, as is being done here, requires that the only effect the randomly assigned treatment has on giving, D_j, is through its effect on pin use. This seems a reasonable assumption for the pins treatment (the existence of the pins in the box of campaign materials is unlikely to affect giving unless the pins are used). One might worry, however, that the pins and raffle treatment affects giving not just through the use of the pins, but also through the use of the raffle materials. The online Appendix includes a number of robustness checks, including a specification where we use random assignment to treatment to predict both use of pins and use of raffle materials in the first stage and investigate the effect of both pin use and raffle material use on giving in the second stage (online Appendix Table A2). We do not find a significant effect of raffle use on giving. In addition, in an ordinary least squares (OLS) specification, getting raffle materials in addition to pins does not statistically significantly affect giving (online Appendix Table A3). We find no evidence that use of raffle materials affects giving and argue that any difference between the treatments is due to differential use of the pins.

FIELD RESULT 1*: Pin use at the workplace increases the amount donated.*

[6] These workplace controls are: amount donated in each campaign year 2003–2007 (when the dependent variable is 2009 giving, the amount donated in 2008 is also included as a control), a dummy equal to 1 if there was no campaign run in each year 2003–2008, the number of employees in each workplace in 2009 and in 2008, whether the workplace replied to a pre-survey in July 2009, a dummy for each charity staffer who contacted employee campaign managers, dummies for the industry of the workplace, and a dummy for whether the workplace is itself a nonprofit that might receive money from the charity.

Table 2—Amount Donated in Field Experiment

	\$ donated in 2009		\$ donated in 2009 – \$ donated in 2008	
	(1)	(2)	(3)	(4)
Use pins	864.60 (429.10)		1,048.8 (420.11)	
Use pins × low participation 2008		341.02 (1,187.9)		424.83 (1,267.5)
Use pins × medium participation 2008		2,006.4 (686.93)		2,120.1 (671.51)
Use pins × high participation 2008		−7.9262 (493.92)		252.52 (496.76)
Low participation 2008		786.73 (858.02)		758.00 (953.83)
Medium participation 2008		−1,405.4 (1,305.5)		−1,539.1 (1,420.4)
High participation 2008		−1,113.5 (1,367.0)		−1,365.8 (1,471.5)
Total \$ donated in 2008	0.8883 (0.04961)	0.9001 (0.04808)		
No campaign in 2008	299.18 (310.61)	−1,331.2 (773.94)	724.11 (287.05)	−1,083.0 (779.99)
Workplace controls	Yes	Yes	Yes	Yes
Observations	278	278	278	278
R^2	0.98	0.98	0.64	0.67

Notes: Robust standard errors are in parentheses. *Use pins* is determined based on a phone survey of employee campaign managers (ECMs). Positive responses to "Did you use the pins?" are coded as 1, ECMs who were not reached are coded as 0. *Workplace controls* include amount donated in each campaign year 2003–2007, a dummy equal to 1 if there was no campaign run in each year 2003–2007, the number of employees in each workplace in 2009 and in 2008, whether the workplace replied to a pre-survey in July 2009, a dummy for each charity staffer who contacted employee campaign managers, dummies for the industry of the workplace, and dummy if the workplace was a nonprofit that could receive money from the charity. *Low participation 2008* indicates that the participation rate in 2008 (the number of donors in 2008 divided by the number of employees in 2008) was $<$ 4 percent in 2008 (bottom tercile), *Medium participation 2008* indicates the participation rate was $\geq$ 4 percent and $<$ 42 percent in 2008 (middle tercile), *High participation 2008* indicates the participation rate was $\geq$ 42 percent in 2008 (top tercile). Among the 278 workplaces: mean amount donated was \$5,341, median was \$1,688. For low participation: mean = \$830, median = \$0; for medium: mean = \$8,203, median = \$3,332; for high: mean = \$6,865, median = \$2,573. Regressions (1) and (3) show using the pins had a significant positive effect on donations. Regressions (2) and (4) show the effect arises from workplaces with 4 percent to 42 percent participation rates in 2008.

Table 2 shows the estimates for the amount donated in the workplace campaigns. The positive and significant coefficient of *Use pins* in regression (1) indicates that each workplace that used pins as part of the campaign increases the total amount donated by \$864.60. Regression (3) finds similar results analyzing the difference between donation in 2008 and 2009 for each workplace (essentially requiring the coefficient of Total \$ donated in 2008 to be 1) and also estimates a significant effect of pins being used as part of the campaign.

One limitation of the two-stage least squares approach is that, while we observe random assignment to treatment perfectly, we do not perfectly observe pin use at the workplace. We establish pin use through a post-study survey of ECMs, but were not able to reach about 25 percent of the workplaces in the pins treatment and pins and raffle treatment even after repeated phone calls. In our primary specification, workplaces that did not respond to the post survey are assumed not to have used the pins as part of the campaign. While it might be reasonable to assume that workplaces that do not respond to repeated phone calls from the charity are unlikely to use extra materials sent to them by the charity, this is not essential for the results. We get the same pattern of results and significance levels if we assume instead that all workplaces we did not reach in the post survey used the pins (see online Appendix Table A4). Since the two-stage least squares approach scales up the reduced-form estimates based on the percentage of workplaces that used the pins, we can think of the results from Table A4, which assumes pin use whenever we are unsure of pin use, as a conservative lower bound.

Heterogeneous Treatment Effects.—To better understand how announcements of support affect giving behavior, we investigate which workplaces demonstrate an increase in giving due to pin use. Regressions (2) and (4) show the effect of pin use broken down for each of the terciles of 2008 participation rate (where participation rate is the number of donors divided by the number of employees). The entire positive effect comes from the middle tercile, suggesting that pin use was especially effective at increasing giving at workplaces with 2008 participation rates between 4 percent and 42 percent.

FIELD RESULT 2*: Pin use generates an increase in amount donated at workplaces with participation rates between* 4 *percent and* 42 *percent.*

This result suggests that the there may be settings in which facilitating announcements of support can have a particularly large, positive effect on giving. In workplaces with historic giving rates that are too low, there may not be enough people in support of the charity to wear the pin and induce giving from others. In workplaces with historic giving rates that are too high, there might be a ceiling effect whereby pin use does not induce additional gifts since everyone already knows that their coworkers support the charity. With historic giving rates that are low, but not too low, announcements of support may serve to catalyze a potential donor base that learns others support the charity by observing them wear the pins.

C. *Discussion*

Providing workers with a way to easily announce support for the charity through pin use as part of the workplace campaign increased donations by $865, or roughly 16 percent of the average amount collected by workplaces in the experiment ($5,341).[7] This increase in donation is large relative to the cost of the pins (less than $0.17 each). Even though many workplaces that received pins did not use them, each $0.17 pin sent out in the experiment generated average of $3.85 in extra donations for the charity.

The purpose of including the pins in the campaign materials was to lower the cost of announcing support for the charity. Wearing the pin allows an employee to

[7] Using the conservative lower bound from online Appendix Table A4, the effect is $598 per workplace that used the pins, or 11 percent of the average workplace giving.

easily broadcast his support for the charity. While our preferred explanation for the increase in giving at workplaces that use the pins is that they lower the cost of making announcements of support, there are other potential mechanisms, unrelated to employees communicating about the charity, which might be at play. For example, a gift exchange motive might arise if employees at workplaces where pins were used felt obligated to make a return gift to the charity after receiving the pin. Falk (2007) documents a gift exchange effect in the setting of a charitable direct mail campaign where he finds a positive effect on donation rates associated with sending postcards "from the children" who benefit from any donations. Unlike the postcards in the Falk (2007) setting, the pins in this experiment are not a gift from the ultimate recipient of donated funds and do not have explicit value outside of the campaign, so they may be less likely to induce a gift exchange motive. Alternatively, seeing a coworker wear a pin might increase awareness or serve as a reminder that the campaign is taking place. Conversations with staff at the charity, however, suggest that pins are unlikely to play this role as campaigns are well publicized at the workplaces and usually involve significant communication from the ECM to their coworkers in the form of repeated emails about the campaign and numerous reminders to return the campaign's reply card.

To better understand why pin use increased donations, and to test the hypothesis that pin use affected donations by allowing individuals to more easily announce support for the charity, we ran two complementary laboratory experiments.

II. Laboratory Experiment 1

In the first laboratory experiment, subjects have the opportunity to make an anonymous donation to a charity. The experiment has four treatments that differ in whether subjects receive pins before the donation decision and rules about wearing the pins if received.

A. *Design*

Subjects begin the study by making 25 anonymous dictator game allocations without feedback.[8] Subjects are then provided with an information sheet about United Way of Massachusetts Bay and Merrimack Valley from real United Way marketing materials. Subjects in the three pin treatments are then provided with pins as discussed in the next subsection.

All subjects in all treatments are then called to the front of the room at the same time as a randomly selected other subject to collect an envelope containing $15. Each envelope has a subject's seat number written on it, and each subject is given the envelope associated with his or her seat, which allows private donations to be linked to a particular subject.

[8] Subjects are told that 1 of the 25 decisions will be paid out in cash at the end of the study and that with 50 percent probability they be in the role of the dictator (and so their choices will affect earnings) and with 50 percent they will be in the role of receiver. The dictator game decisions provide data on prosocial inclination and give subjects a task to complete before the donation decision.

Subjects are encouraged to look at the person who is with them at the front of the room, but are not explicitly instructed to do so. Subjects are told: "You will come up to the front of the room at the same time as someone else. You will later answer questions about this person." The experiment was designed so subjects have limited exposure to anyone besides the person who they are with at the front at the room (see online Appendix Figure A3 and associated discussion).

After all subjects return to their seats, they are asked to remove the money from their envelope and put back in the envelope any money they would like to donate to United Way. The experimenter then collects an envelope from each subject. Finally, subjects answer a series of questions—some of which are incentivized—and a demographic survey.[9]

No Pin Control and Four Pin Treatments.—In the *no pin control treatment* (132 subjects in 11 sessions), subjects do not receive pins. In the three pin treatments, subjects are each given a pin by the experimenter that read "I Support" followed by the United Way logo, which includes the name "United Way." The pin is handed out before subjects are exposed to each other at the front of the room. In all pin treatments, subjects are told: "You will now be given a United Way pin, which is yours to keep." The pin treatments differ in the instructions about whether or not the subjects should wear the pin.

In the *gift only treatment* (96 subjects in 8 sessions), subjects are instructed: "Please do not put the pin on now. No one will wear the pin for the remainder of the session." Pins are a gift to the subject but are not worn during the session, so the pin cannot provide information about a subject's support for the charity.

In the *must wear treatment* (96 subjects in 8 sessions), subjects are instructed: "Please put the pin on now. Everyone will wear the pin for the remainder of the session." Pins are a gift to the subject and every subject wears the pin during the rest of the session, so (as in the gift only treatment) the pin cannot provide information about a subject's support for the charity.

In the *pin option treatment* (144 subjects in 12 sessions), subjects are told: "Please feel free to put the pin on now." Pins are a gift to the subjects and subjects can choose whether to wear them when they come up to the front of the room. Consequently, in the pin option treatment, the pins can provide information about a subject's support for the charity. In the pin option treatment, 35 percent of subjects choose to wear the pin to the front of the room. We identify the effect of exposure to a subject announcing support for United Way by wearing the pin by comparing donations of subjects randomly called up with a pin wearer to those randomly called up with a subject who chose not to wear the pin (i.e., a "non-wearer").

B. *Results*

Laboratory results are from 468 subjects, students at Boston-area colleges and universities. Subjects participated in 1 of 39 sessions at the Computer Lab for

[9] Subjects are physically handed the information about United Way and the United Way pin (in the relevant treatments), and subjects make the charity donation decision by placing cash into an envelope. All other parts of the experiment are conducted on the computer using z-Tree (Fischbacher 2007).

Experimental Research at Harvard Business School.[10] The experiment lasted less than an hour and on average subjects earned $20.06.

There are three goals of this laboratory experiment. The first is to investigate a set of mechanisms that might have led pins to increase giving in the field experiment but are unrelated to employees announcing support for the charity. The second is to demonstrate that exposure to announcements of support can affect donations. The third is to understand why announcements of support affect donations in the laboratory experiment, which provides insight into how announcements of support may be affecting behavior in the field. We address each of these three goals in a separate subsection.

Mechanisms That Might Have Affected Donations in the Field Experiment.—The no pin control, gift only, and must wear treatments were run to investigate various mechanisms that could have influenced charitable giving in the field experiment. In particular, by comparing donation in the no pin control treatment to donation in the gift only and must wear treatments—in which each subject gets a pin as a gift but cannot use it to announce support since they either cannot wear it or must wear it—we can identify the effect of gift exchange (since they all get the pin as a gift) and of exposure to the charity imagery (since they all see the United Way logo on the pin they receive). By comparing the latter two treatments, we can identify the effect of being forced to wear a pin, which may trigger cognitive dissonance.

LAB RESULT 1*: Receiving the pin as a gift*, *being exposed to the charity logo*, *and being forced to wear the pin do not statistically significantly impact donation.*

Figure 3 shows average donations in the no pin control, gift only, and the must wear treatments. As can be seen in the figure, the donation levels are nearly identical across the treatments ($1.32 in control, $1.31 in gift only, $1.47 in must wear: all t-tests of pairwise differences have $p > 0.48$), demonstrating that in the laboratory setting there is not strong evidence of an effect due to gift exchange, exposure to charity imagery, or mandatory pin wearing. While finding null results for these mechanisms is not conclusive evidence that such forces are not active, the tests that compare donations between two treatments have at least 192 observations and are well powered to identify an effect of a similar magnitude as the effects on exposure to announcements of support that are discussed in the next subsection.

Do Announcements of Support Affect Donation?—In the pin option treatment, subjects decided whether to wear the pin. To test if announcements of support affect donation decisions, we ask whether donations differ when individuals are randomly called up to the front of the room with a pin wearer rather than a non-wearer. In the pin option treatment, 50 subjects were exposed to someone at the front of the room

[10] Subjects participated in the main experiment in May and June 2010 (14 sessions); April and May 2011 (19 sessions); February and October 2014 (6 sessions). There were no statistical differences in behavior within any treatment across years and so all data have been combined for analysis. A fifth treatment, not included in the main analysis but discussed at length in the online Appendix, was additionally run in February, March, April, and October 2014 (17 sessions) and August 2016 (5 sessions).

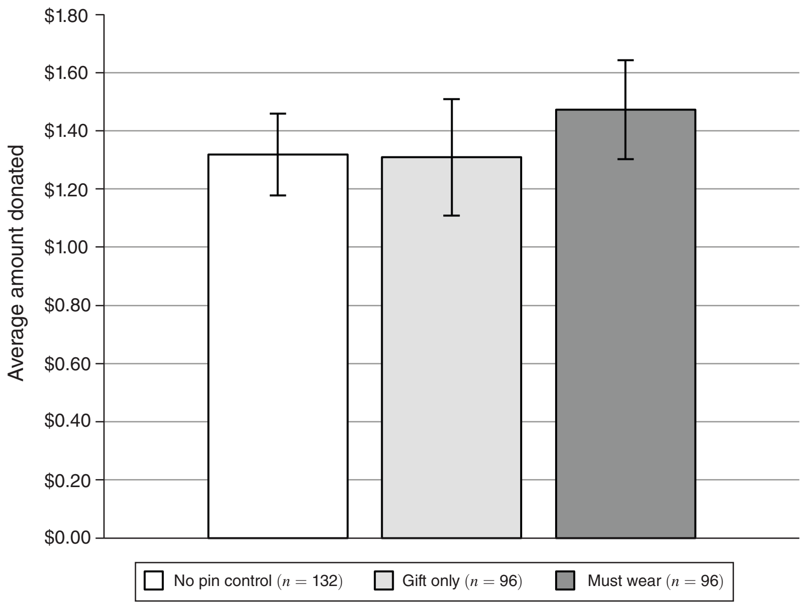

Figure 3. Donation by Treatment (*Laboratory Experiment 1*)

Notes: Average amount donated by treatment in Experiment 1. Standard error bars are shown around each mean.

who announced support for United Way by wearing the pin and 94 subjects were exposed to a non-wearer.[11]

LAB RESULT 2: *Being randomly exposed to a subject who chooses to wear the pin, rather than a non-wearer, increases donations.*

Figure 4 shows average donations across the treatments and exposure. The first two bars compare the donations of subjects in the pin option treatment who are exposed to non-wearers (bar 1) and who are exposed to pin wearers (bar 2). For reference, the figure also shows average donations across the control, gift only, and must wear treatments where there was no way to announce support for the charity (bar 3, which combines all data in Figure 3) and shows the average donation in the pin option treatment (bar 4, which combines data from bars 1 and 2).

[11] These numbers include pairs of two non-wearers, two pin wearers, and one pin wearer and one non-wearer. Pin wearing is established by whether the experimenter saw the subject wearing the pin at the front of the room. Subjects also report whether they wore the pin. Comparing my record to these self-reports, I saw the pin on two subjects who reported not wearing it, and I did not see the pin on five subjects who reported wearing it. Many of these differences are likely in interpretation (one subject who reported wearing the pin noted in a questionnaire that he put it on his bag, which he did not wear to the front of the room). Results are similar if data from these subjects and those paired with them are dropped. One subject carried the pin up in his hand to the front of the room. This subject is coded as not having worn the pin; results are nearly identical if the subject is coded as having worn the pin. Non-announcement-based explanations for why exposure to a pin wearer might affect giving are addressed and ruled out in online Appendix A5.

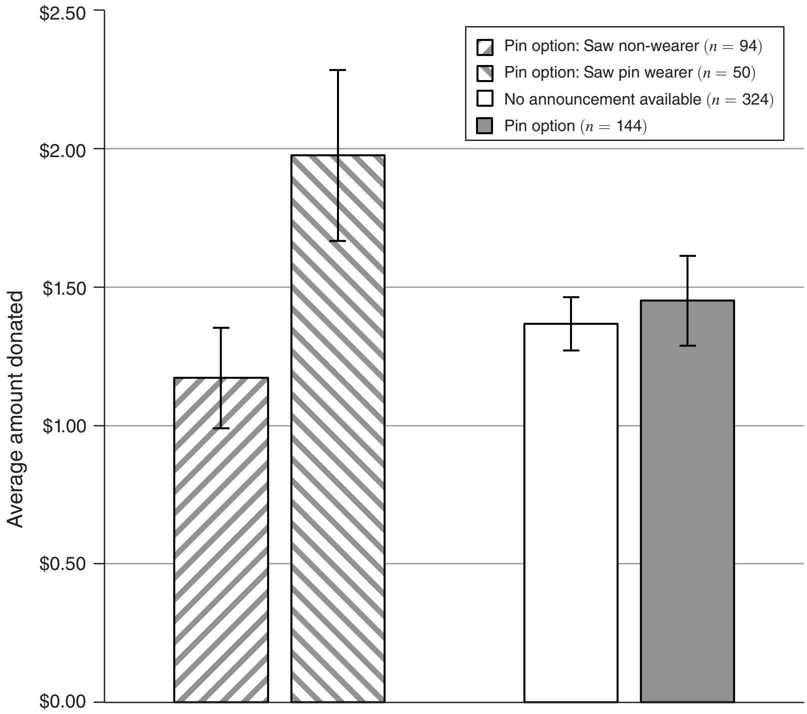

FIGURE 4. DONATION BY EXPOSURE TO PIN WEARER AND TREATMENT (*Laboratory Experiment 1*)

Notes: Average amount donated by exposure and by condition in Experiment 1. Standard error bars are shown around each mean.

Table 3 presents the results of the first laboratory experiment in a regression framework, investigating the effect of exposure to a pin wearer or a non-wearer on donations. The basic regression specification we estimate is

$$(3) \qquad D_i = \alpha_0 + \alpha_1 \times \textit{Pin option}_i + \alpha_2 \times \textit{Exposed to announcement of support}_i + \varepsilon_i \,,$$

where D_i is the dependent variable of interest: the amount donated by subject i; α_0 is the constant, indicating average giving in the three treatments where pins cannot be used to announce support; $\textit{Pin option}_i$ is a dummy for being in the pin option treatment, and α_1 is the coefficient associated with being exposed to a non-wearer in the pin option treatment relative to being in a treatment where announcements of support were not possible; $\textit{Exposed to announcement of support}_i$ is a dummy for being exposed to a pin wearer in the pin option treatment, so α_2 is the coefficient that reflects the relative effect of being exposed to a pin wearer rather than a non-wearer in the pin option treatment. We cluster the random error, ε_i, at the session level.

Regression (1) estimates this specification exactly, testing whether the differences between the bars in Figure 4 are statistically significant. *Exposed to announcement of support* is positive and significant with a coefficient of \$0.80, indicating an increase

Table 3—Exposure to Announcement of Support on Donation (*Laboratory Experiment 1*)

	Donation	
	No controls	Controlling for beliefs
	(1)	(2)
Exposed to announcement of support	0.804	0.324
	(0.265)	(0.204)
Pin option	−0.190	−0.105
	(0.247)	(0.157)
Constant	1.361	
	(0.0882)	
	Controls	
Beliefs about donation of person at front of room	No	Yes
Observations	468	468
Clusters	39	39
R^2	0.014	0.521
$P(\textit{Exposed to announcement of support} + \textit{Pin option} = 0)$	0.021	0.179

Notes: Robust standard errors clustered by session are in parentheses. *Exposed to announcement of support* is a dummy equal to 1 if the subject was exposed to a pin wearer at the front of the room in the pin option treatment. *Pin option* is a dummy equal to 1 if the subject was in the pin option treatment. Subjects in the no pin control, gift only, and must wear treatments are the excluded group. Beliefs about donation of person at front of room include dummy variables for each dollar amount between \$0 and \$15. $P(\textit{Exposed to announcement of support} + \textit{Pin option} = 0)$ reports the p-value associated with a test of whether those exposed to an announcement of support in the pin option treatment donate the same amount as subjects in the other three treatments. Regression (1) shows that subjects who are randomly exposed to a pin wearer donate significantly more than subjects in other treatments and subjects who are exposed to a non-wearer. Regression (2) shows that adding the endogenous beliefs of the donation of the person at the front of the room as a control decreases the estimated effect of being exposed to a pin wearer.

in donations when subjects are exposed to a pin wearer rather than a non-wearer. *Pin option* has a coefficient of −\$0.19, indicating a slight, but not statistically significant, decrease in donations associated with seeing a non-wearer in the pin option treatment as compared to being in a treatment where announcements of support are not allowed. A directionally negative effect on *Pin option* is not that surprising. Since it is publicly announced that every subject has received a pin and has the option to wear it up to the front of the room, anyone who is with a non-wearer gets a negative signal about that subject's support for the charity. That is, subjects know that the non-wearer could have chosen to wear the pin and explicitly chose not to do so. We find additional evidence of this negative effect in the second laboratory experiment.

The row $P(\textit{Exposed to announcement of support} + \textit{Pin option} = 0)$ shows the p-value associated with a test of whether the sum of the coefficients is equal to 0, effectively asking whether subjects exposed to a pin wearer in the pin option treatment donate more than subjects in the treatments where it is not possible to announce support. In regression (1), this row reports that the combined effect (\$0.61) is statistically different from 0 with $p = 0.021$, indicating that subjects exposed to a pin wearer in the pin option treatment give more than subjects in treatments where announcements of support are not allowed.

Why Do Announcements of Support Affect Donation?—Do announcements of support trigger conditional cooperation even though the announcements need not be associated with a donation? After making donation decisions, subjects were asked to report beliefs about the United Way donation of the person they were with at the front of the room. Beliefs about donation significantly increase when that person announces support for the charity by wearing the pin. Subjects at the front of the room with a pin wearer in the pin option treatment believe that person donates on average \$2.74 whereas subjects at the front of the room with a non-wearer believe that person donates on average \$1.87 (144 observations: t-test, $p < 0.01$).[12] Can this change in belief about the donation of the person at the front of the room help explain the increase in donations associated with exposure to a pin wearer?

To test whether beliefs about the donation of announcer can help explain donation decisions of the subject exposed to an announcement of support, regression (2) of Table 3 controls nonparametrically for beliefs about the donation of the other person at the front of the room (i.e., it includes a dummy variable for each dollar amount belief between \$0 and \$15). Controlling for how much subjects believe the person at the front of the room with them donated generates a much smaller estimate of the effect of exposure to an announcement of support on donation. The coefficient in regression (2) is less than one-half of the magnitude of the effect in regression (1).

LAB RESULT 3: *Subjects exposed to a pin wearer think that subject donates more and this can help explain the increase in donation.*

These results suggest that announcements of support affect beliefs about how much the announcer donates and that this helps to explain why announcements of support affect donation. We further explore this hypothesis in second experiment, discussed in the next section.[13]

III. Laboratory Experiment 2

The first laboratory experiment allowed us to use the same form of announcements of support in the lab as in the field, but the experiment was limited in that it each session involved only 12 subjects who made one donation decision to a single charity. The second laboratory experiment tests for the effect of announcements of support in a new paradigm that allows for a different way of announcing support (i.e., an anonymous message sent over the computer), includes a wider range of charities, generates more statistical power, and introduces additional treatments. The second experiment was run to replicate the main result from the first experiment and to better understand the mechanism underlying how announcements of support affect giving.

[12] The belief that pin wearers donate \$2.74 is also higher than the belief of subjects in the control, gift only, and must wear treatments who believe the subjects they are with at the front of the room donate \$2.02 on average (374 observations: t-test, $p = 0.016$). Beliefs about donation in the pin option treatment are directionally correct (pin wearers donate more than non-wearers) but are above the actual amounts donated. The 50 pin wearers donate \$1.76 on average (below the average belief of \$2.74); the 94 non-wearers donate \$1.28 on average (below the average belief of \$1.87).

[13] We additionally ran a fifth treatment in the first laboratory experiment that was similar to the pin option treatment but in which one-half of subjects were not allowed to donate to the charity. This treatment was designed to investigate whether subjects respond to announcements of support when they know announcers cannot donate. Results were inconclusive (see discussion in online Appendix A6), which motivated a similar treatment in the second laboratory experiment.

A. *Design*

The second laboratory experiment involves 20 rounds. Subjects are asked about a different, randomly selected charity in each round.[14] In each round, subjects are shown a few sentences about that round's charity and are anonymously and randomly paired with another subject in the session. As in the first laboratory experiment, subjects get $15 as a guaranteed payment for participating in the experiment from which they can donate to the charity. They are told that one round will be randomly selected for their donation decision to be implemented. Subjects sit at isolated computer terminals and do not interact face-to-face with anyone else in the lab: all announcements of support are made anonymously through the computer. The experiment involves four treatments.

In the *control treatment*, subjects have the opportunity to donate money to the designated charity for that round but no announcements of support are allowed. This treatment is designed to mirror the no pin control treatment from the first laboratory experiment.

In the *announce treatment*, subjects have the opportunity to announce support for the charity by sending the message "I support this charity" to the person they are paired with in the round. Each subject sees whether the person they are paired with sent the message before they make donation decisions. This treatment is designed to mirror the pin option treatment from the first laboratory experiment.

In the *donation-and-announce treatment*, one subject has the opportunity to announce support to the charity by sending the message "I support this charity" and to make a donation. The other subject is shown both the donation amount, which may be zero, and the support message, if sent, before making a donation decision. This treatment is designed to investigate whether announcements of support still have an effect once donation amounts are known.

In the *restricted-announce treatment*, subjects have the same opportunity to announce support as in the announce treatment, but only one subject in each pair is allowed to make a donation in each round. Subjects are informed that if they are able to donate in the round, the person they are paired with cannot donate in the round. This treatment is designed to investigate whether announcements of support still have an effect when the announcer is restricted from donating in the experiment.

All subjects in all sessions participate in the control treatment for five rounds and the announce treatment for five rounds. Depending on the session, subjects participate in either the donation-and-announce treatment for ten rounds or the restricted-announce treatment for ten rounds. In five of the donation-and-announce rounds, subjects make the donation decision first and in five they observe the donation decision and support message of the other subject before donating. Similarly, in five of the restricted-announce rounds, subjects are allowed to donate and in five they are not. The order of the treatments varies by session.[15]

[14] The 20 charities selected for use in the experiment were the 20 US charities that receive the most private funding as identified by *Forbes* (www.forbes.com/top-charities/list/, accessed June 16, 2014). The list of these charities, and the accompanying descriptions of the charities shown to subjects, is presented in online Appendix Table A6.

[15] There are six possible orders for the control (C), announce (A), and donation-and-announce (D) rounds: CAD, CDA, DAC, DCA, ADC, and ACD. There are similarly six orders for the sessions when the restricted-announce treatment replaces the donation-and-announce treatment. For balance, each of the orders was used in 5 sessions,

B. *Results*

Results are from 938 subjects, students at the University of Pennsylvania, who participated in 1 of 60 sessions run at the Wharton Behavioral Lab.[16] The experiment lasted less than one hour and was run entirely on z-Tree.

Since subjects made multiple donation decisions and faced 20 different charities, results are presented in regression analysis, shown in Table 4. The regression specification we estimate for the second laboratory experiment is

$$(4)\ D_{ir} = \alpha_1 \times Announce_{ir} + \alpha_2 \times Exposed\ in\ announce_{ir}$$
$$+ \beta_1 \times Restricted\text{-}announce_{ir} + \beta_2 \times Exposed\ in\ restricted\text{-}announce_{ir}$$
$$+ \gamma_1 \times Donation\text{-}and\text{-}announce_{ir}$$
$$+ \gamma_2 \times Exposed\ in\ donation\text{-}and\text{-}announce_{ir}$$
$$+ \boldsymbol{\gamma}_3' \, \mathbf{Other}_{ir} + \mu_i + \mu_r + \mu_{charity(ir)} + \varepsilon_{ir},$$

where i indexes subject and r indexes round of the experiment. The regressions include subject fixed effects, μ_i, to control for differential generosity of subjects; a dummy variable for each round, μ_r, since willingness to donate may depend on the round of the experiment; and a dummy variable for each charity, $\mu_{charity(ir)}$, since different charities may have different levels of baseline support. The coefficients α_1, β_1, and γ_1 respectively compare donation in the control treatment to donation when the subject is not exposed to an announcement of support in the announce treatment, restricted-announce treatment, and donation-and-announce treatment (when the person they are paired with donates 0). The coefficients of interest, α_2, β_2, and γ_2 compare donation when subjects are exposed to an announcement of support to when they are not in each of the three treatments, respectively. Since in the donation-and-announce treatment subjects are exposed to the donation made by the person they are paired with, we additionally include a dummy for each potential donation amount greater than 0 they might be exposed to (denoted as the vector $\mathbf{Other}_{ir}$). Note that these dummies are all necessarily 0 in all treatments besides the donation-and-announce treatment. We cluster the random error, ε_{ir}, at the session level.

Regression (1) shows how donations change when subjects are randomly exposed to a subject who chose to announce support for a charity. The coefficient *Exposed in announce* reports how giving is affected by receiving an "I support this charity" message rather than not receiving one in the announce treatment. It is positive and

leading to 30 sessions of each type and 60 sessions total. Due to issues with exclusion criteria at the laboratory, an additional five sessions of the main experiment were run that inadvertently allowed a previous participant to participate. All results are robust to also including data from these sessions in the analysis as shown in online Appendix A8. Finally, eight additional sessions were run with a fifth experimental treatment described in Section V and discussed in detail in online Appendix A9.

[16] Subjects participated in the experiment in September, October, and November 2014, and January, February, July, September, and December 2016. As in the first experiment, there were no statistical differences in behavior within any treatment across years and so all data have been combined for analysis.

Table 4—Exposure to Announcement of Support (*Laboratory Experiment 2*)

	Donation (1)	Overall quality beliefs (2)	Average donation beliefs (3)	Should donate beliefs (4)
Exposed in announce	0.12	0.16	0.28	0.23
	(0.029)	(0.024)	(0.022)	(0.052)
Exposed in restricted-announce	0.11	0.20	0.36	0.21
	(0.045)	(0.034)	(0.032)	(0.078)
Exposed in donation-and-announce	−0.023	0.086	0.076	0.056
	(0.044)	(0.031)	(0.027)	(0.062)
Announce	−0.15	−0.064	−0.16	−0.13
	(0.027)	(0.021)	(0.026)	(0.046)
Restricted-announce	−0.12	−0.11	−0.21	−0.22
	(0.043)	(0.025)	(0.032)	(0.077)
Donation-and-announce	−0.11	−0.100	−0.28	−0.22
	(0.038)	(0.029)	(0.038)	(0.066)
	Controls			
Subject fixed effects	Yes	Yes	Yes	Yes
Charity dummies	Yes	Yes	Yes	Yes
Round dummies	Yes	Yes	Yes	Yes
Other donation dummies	Yes	Yes	Yes	Yes
Observations	14,070	14,070	14,070	14,070
Subjects	938	938	938	938
Sessions (clusters)	60	60	60	60
R^2	0.071	0.301	0.171	0.103

Notes: Fixed effect specifications with robust standard errors clustered by session in parentheses. *Exposed in announce* is a dummy equal to 1 if the subject was exposed to an announcement of support in the announce treatment. *Exposed in restricted-announce* is a dummy equal to 1 if the subject was exposed to an announcement of support in the restricted-announce treatment. *Exposed in donation-and-announce* is a dummy equal to 1 if the subject was exposed to an announcement of support in the donation-and-announce treatment. *Announce* is a dummy equal to 1 in the announce treatment. *Restricted-announce* is a dummy equal to 1 in the restricted-announce treatment. *Donation-and-announce* is a dummy equal to 1 in the donation-and-announce treatment. The control treatment is the excluded group. *Overall quality beliefs* is on a 1–4 scale. *Average donation beliefs* is a number from 0 to 15. *Should donate beliefs* is a number from 0 to 15. All regressions include subject fixed effects, dummies for each of the 20 charities, dummies for each of the 20 rounds, and dummies for the other subject's donation in the donation-and-announce treatment.

significant and estimates that being exposed to an announcement of support leads subjects to donate \$0.12 more than if they are not exposed to an announcement of support (a 30 percent increase over the donation in the announce treatment when subjects are not exposed to an announcement of support), replicating laboratory result 2 from the first experiment. The significant, negative coefficient of *Announce* indicates that subjects donate less in the announce treatment when they do not see an announcement of support than they donate in the control treatment. A similar countervailing negative effect was present in the first experiment; in this experiment, it is statistically significant.

The additional treatments in the second laboratory experiment allow us to explore why announcements of support affect donation decisions. The coefficient of *Exposed in donation-and-announce* is small, close to 0, and directionally negative. Indeed, it is statistically significantly smaller than the *Exposed in announce* coefficient ($p < 0.01$ in a post-estimation F-test), highlighting that once subjects know the donation of the person they are paired with, whether that person announces support no longer affects giving. This finding reinforces the results from the first laboratory experiment, which suggested that subjects respond to announcements of support because they contain information about what the announcer might donate.

LAB RESULT 4*: Once subjects know the amount donated by the announcer*, *an announcement of support does not affect donation.*

Finally, the coefficient of *Exposed in restricted-announce* shows how announcements of support affect donation decisions when the announcer is not allowed to donate money as part of the experiment. The coefficient is \$0.11 and is statistically significant ($p = 0.030$). This result suggests that announcements of support affect behavior even whether the announcer cannot make a contemporaneous donation.

LAB RESULT 5*: Subjects exposed to an announcement of support donate more*, *even when they know the person announcing support cannot make a donation as part of the experiment.*

Taken together, these results suggest that announcements of support provide information on what a subject might donate or would donate if able to do so. Once that information is available, as in the donate-and-announce treatment, there is little additional information contained in the announcement.

An additional feature of the donation-and-announce treatment is that we can measure the effect of observing an actual donation of another subject on donation behavior. Regressing a subject's donation on the donation the subject observes in the donation-and-announce treatment yields a highly significant coefficient of \$0.061 ($p < 0.01$). This suggests that the \$0.12 effect of announcements of support in the announce treatment is equivalent to observing a \$2 donation in the donation-and-announce treatment.

Why Do Subjects Respond to What Others Might Donate?—Above, we establish that subjects respond to announcements of support because they provide information about what others might donate. Here we investigate why subjects care about what others might donate—or would donate if allowed to do so. One possible explanation is that beliefs about someone else's potential donation affects giving by making subjects believe the charity is of higher quality (e.g., as in Vesterlund 2003 and Potters, Sefton, and Vesterlund 2005). Other possible explanations are that subjects use the donation of the announcer as a proxy for donations of all subjects, potentially triggering a reciprocity (Sugden 1984), conformity (Bernheim 1994), or "fair share" motive (Sen 1977; Rose-Ackerman 1982), or that subjects take it as an indication of what donation they should make (i.e., what is socially appropriate).

Regressions (2) through (4) of Table 4 use the regression specification shown above but replace donation amount with other variables to show how exposure to announcements of support affects beliefs about the overall rating of the charity in regression (2), the average donation of subjects who are eligible to donate in regression (3), and what subjects should donate in regression (4).[17] In all three regressions, the *Exposed in announce* and *Exposed in restricted-announce* coefficients are positive and significant. The *Exposed in donation-and-announce* coefficient is only positive significant in regressions (2) and (3) and it is significantly smaller than the other two coefficients, suggesting it has less of an impact on these beliefs. These regressions find that exposure to an announcement of support moves all of these beliefs dramatically, unless the announcer's donation decision is also known.

Table 5 investigates whether any of these belief variables—or the combination of all three—can statistically explain the effect of announcements of support on donation decisions. Regression (1) is the same specification as the first regression in Table 4, and regressions (2) through (5) add as controls the beliefs analyzed in regressions (2) through (4) of Table 4.

Regression (2) estimates that there is still a statistically significant effect of exposure to an announcement of support in the announce treatment when controlling for charity quality (although the coefficient has decreased to 0.076, a decrease of roughly 38 percent). This decrease suggests some scope for announcements of support to communicate information about charity quality in this experimental paradigm.[18]

The effect of being exposed to an announcement of support in the announce treatment also persists when controlling for beliefs about average donations of others, as in regression (3), or when controlling for beliefs about what subjects should donate, as in regression (4). The effect of an announcement of support is close to zero and insignificant in regression (5), which includes all three sets of controls. These results suggest that subjects are motivated by announcements of support through a combination of changes in information about charity quality, changes in beliefs about average donation (i.e., the descriptive norm), and changes in beliefs about what individuals should donate to the charity (i.e., an injunctive norm). This finding implies that subjects respond to announcements of support for similar reasons as they engage in conditional cooperation, but they respond even though announcements are disconnected from actual donations and only indicate what announcers might donate or what they might have donated if allowed to do so.

[17] Subjects in the announce treatment were also asked their belief of how much the subject they are paired with donated. As in the first experiment, being exposed to an announcement of support increases this belief and if we control for this belief, there is no longer a positive effect of an announcement of support on donation (we do not explore this variable in Table 4 because subjects in the restricted-announce treatment and in the donation-and-announce treatment were not asked this question). As in the first experiment, subjects are correct that those who announce support donate more. Average donations in the announce treatment are \$0.12 when subjects do not announce support and \$0.93 when they do. In a regression with subject fixed effects and controls for charity and round of the experiment, we estimate that a subject announcing support is associated with that subject donating \$0.57 more.

[18] In the first laboratory experiment, exposure to a pin wearer does not affect beliefs about charity quality (either United Way's rating overall or specific dimensions of "organizational efficiency" and "organization capacity" asked about in that experiment). In addition, nonparametrically controlling for these beliefs does not change at all the estimated coefficient on exposure to a pin wearer in the first experiment. There is a key difference between the experiments that could explain the difference in how beliefs about charity quality respond to announcements of support. In the first experiment, subjects are faced with one charity (i.e., United Way), provided with a page of information about it, and given a few minutes to read it. In the second experiment, subjects are faced with 20 charities and are only provided with a few sentences about each charity. Consequently, subjects in the first experiment may generate precise beliefs about charity quality before they are exposed to an announcement of support, while subjects in the second experiment may have more scope to learn about charity quality from announcements of support.

TABLE 5—EXPOSURE TO ANNOUNCEMENT OF SUPPORT ON DONATION (*Laboratory Experiment 2*)

	Donation				
	No controls (1)	Overall quality controls (2)	Average donation controls (3)	Should donate controls (4)	All three (5)
Exposed in announce	0.12	0.076	0.053	0.074	0.016
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
Exposed in restricted-announce	0.11	0.053	−0.013	0.053	−0.025
	(0.045)	(0.042)	(0.041)	(0.037)	(0.034)
Exposed in donation-and-announce	−0.023	−0.027	−0.037	−0.036	−0.042
	(0.044)	(0.039)	(0.039)	(0.040)	(0.034)
	Controls that might eliminate effect of exposure to announcer				
Beliefs about charity quality	No	Yes	No	No	Yes
Beliefs about average donation	No	No	Yes	No	Yes
Beliefs about how much one should donate	No	No	No	Yes	Yes
	Controls				
Treatment dummies	Yes	Yes	Yes	Yes	Yes
Subject fixed effects	Yes	Yes	Yes	Yes	Yes
Charity dummies	Yes	Yes	Yes	Yes	Yes
Round dummies	Yes	Yes	Yes	Yes	Yes
Other donation dummies	Yes	Yes	Yes	Yes	Yes
Observations	14,070	14,070	14,070	14,070	14,070
Subjects	938	938	938	938	938
Sessions (clusters)	60	60	60	60	60
R^2	0.071	0.108	0.233	0.244	0.340

Notes: Fixed effect specifications with robust standard errors clustered by subject in parentheses. *Exposed in announce* is a dummy equal to 1 if the subject was exposed to an announcement of support in the announce treatment. *Exposed in restricted-announce* is a dummy equal to 1 if the subject was exposed to an announcement of support in the restricted-announce treatment. *Exposed in donation-and-announce* is a dummy equal to 1 if the subject was exposed to an announcement of support in the donation-and-announce treatment. Beliefs about charity quality include dummy variables for each star rating for overall charity quality interacted with treatment. Beliefs about average donation include dummy variables for each amount between $0.00 and $15.00 and are the belief about how much the average person in the session donated, interacted with treatment. Beliefs about how much one should donate include dummy variables for each dollar amount between $0 and $15 interacted with treatment. All regressions include subject fixed effects, dummies for each of the 20 charities, dummies for each of the 20 rounds, dummies for treatment, and dummies for the other subject's donation in the donation-and-announce treatment.

IV. Comparing Laboratory and Field Settings and Results

The first laboratory experiment found no evidence that gift exchange, exposure to charity imagery, or mandatory pin wearing affected donations; instead, it demonstrated that subjects respond to announcements of support.[19]

[19] Results on the effect of announcements of support are relatively similar across the two laboratory experiments. Subjects in the pin option treatment of the first laboratory experiment who were not exposed to an announcement of support donated $1.17, while subjects who were exposed to an announcement of support donated $1.98, a 69 percent increase. Meanwhile, subjects in the announce treatment of the second laboratory experiment donated an average of $0.42 when they were not exposed to an announcement of support and $0.71 when they were exposed to announcement of support, a 70 percent increase. Note, however, that since the latter comparison does not control for charity, it combines both the effect of announcements and the fact that announcements are more likely to be made for desirable charities, to which subjects are more likely to donate whether or not they observe an announcement. When we rerun the exercise and average the means of those exposed to announcements and those not exposed to

We take this evidence to suggest that the effect of pin use on donations in the field experiment is likely a result of pins decreasing the cost of announcing support for the charity. However, while pin use in the field experiment led to an increase in average donations, the similar pin option treatment did not generate a significant increase in donations relative to control in the first laboratory experiment.[20] There are a number of ways the field setting is different from the lab setting that have the potential to influence the effect of announcements of support and could potentially lead pins to have a larger effect on giving in the field experiment.

First, in the laboratory, subjects are only exposed to one other person before donating; in the field, subjects are exposed to many coworkers. Consequently, one announcer in the field experiment might be able to increase donations from an entire workplace, whereas each announcer in the laboratory experiments is only exposed to one potential donor.

Second, campaigns in the field last a few weeks and announcements have the potential to have a dynamic effect such that employees decide to announce support after observing a coworker announce support, which could further affect giving. In the laboratory, there is no scope for dynamic effects of this kind.

Third, the field may be less likely to display, or display more weakly, the countervailing negative effect we observed in the laboratory when subjects were exposed to someone who failed to announce support. In the first laboratory experiment, it is publicly announced that everyone has a pin and a chance to wear it. In the field, a coworker not wearing a pin around the office might have worn the pin yesterday but left it at home today, might be displaying the pin at his desk, or might have never received a pin in the first place.[21] In the field, positive announcement of support are explicit and clear while failure to announce support is a much noisier signal. An additional treatment of the second experiment, the s*urprise-announce treatment* (discussed and analyzed in online Appendix A9), was run to attempt to eliminate this countervailing negative effect in a lab setting. Donations in this treatment are 17 percent higher than in the control treatment of the second experiment, very similar to the 16 percent increase observed in the field experiment.

Fourth, subjects in the laboratory interact with other subjects who are almost always strangers. Subjects in the field know, and may care about, their coworkers who are announcing support. It is reasonable to believe that subjects will respond more to announcements when made by closer friends (see, e.g., Leider et al. 2009 about how social ties affect giving). We have some scope for testing this hypothesis in the data from the first lab experiment. In particular, at the end of the study, subjects were asked whether they recognized the person who they were with at the front of the room. Among the 234 pairs in the experiment, in 10 pairs at least one of the

announcements, weighting the 20 charities equally for both nonexposure and exposure to an announcement, we get an average of \$0.46 for those not exposed to an announcement and \$0.65 for those exposed to an announcement, a 41 percent increase.

[20] The pin option treatment did increase giving by 10 percent relative to control, a similar percentage change to the 16 percent associated with pin use in the field experiment, but the increase was not statistically significant. The second laboratory experiment also did not find an increase in giving in the announce treatment relative to the control, however a treatment that mitigated the negative effect of being exposed to a non-announcer, described in online Appendix A9, generated an increase of giving of 17 percent over control giving.

[21] For example, many employee campaign managers reached in the post-experiment survey reported putting pins in a bowl (e.g., in the break room) for people to take if they wished. It is possible that not all employees observe the bowl to have a chance to get a pin.

two people said they recognized the person with them at the front of the room. Only 4.27 percent of subjects are in these pairs, but the 5 pairs in the pin option treatment donate an average of \$1.70 while the 5 pairs in the 3 other treatments only donate \$0.70. Regression results demonstrate that this \$1.00 difference in giving is significantly different from the \$0.05 difference in giving among pairs that don't recognize each other (i.e., \$1.70 in pin option versus \$0.70 in the other three treatments is significantly different from \$1.43 in pin option versus \$1.38 in the other three treatments, $p = 0.018$). The closeness of the relationship between the parties is likely to be important for the efficacy of announcements of support.

Fifth, in the laboratory experiments the announcements of support were very constrained. Subjects could only announce support for the charity by silently wearing the pin (first lab experiment) or by sending the message "I support this charity" (second lab experiment). In the field, announcements of support could be contextually richer; for example, the pins could break the ice and allow employees to talk more effusively about their support of the charity.

V. Conclusion

The findings discussed above teach us a great deal about individuals' motivations for giving and the extent to which social forces influence donation behavior. In the field experiment, facilitating announcements of support for a charity increases giving. In both laboratory experiments, subjects donate more when they observe an announcement of support than when they do not. These effects arise even though such announcements may not be associated with actual donations—subjects still respond even though announcers may choose not to donate or be restricted from donating as part of the experiment. Results from the second laboratory experiment reveal that announcements contain similar information as actual donations—an announcement has no additional effect when the announcer's actual donation is also revealed. Finally, individuals exposed to an announcement of support update their beliefs about the quality of the charity, how much all others donate, and how much people should donate. That individuals update beliefs about this information from what could be cheap talk reveals that social forces can be incredibly powerful motivators in public good provision.

In addition, our results demonstrate that altruism alone cannot explain giving patterns. To the extent that information about charity quality (and thus the social benefit generated by the charity) is communicated by the announcement, an increase in giving in response to an announcement of support could be consistent with altruism. We find, however, that announcements of support affect donations even when subjects do not believe the charity is of higher quality (as in the first lab experiment) or when controlling for beliefs of charity quality (as in the second laboratory experiment). In both laboratory experiments, we find evidence that announcements of support move beliefs about the donations of others and that controlling for these beliefs dampens the effect of announcements of support on giving. These results are consistent with models of giving based on reciprocity, conformity, or a desire to do one's fair share.

The laboratory work in this paper has focused on how individuals respond to being exposed to announcements of support. There is related academic work focusing on the effect of announcements of support on those who make them. Indeed, research on "slacktivism" (a portmanteau word combining slacker and activism) suggests that when opportunities for announcing support are available, individuals may announce support as a substitute for more costly activism (Kristofferson, White, and Peloza 2014). This notion may help explain cases of social media activism in which individuals "like" a cause or share a charity's story online but do not make donations or recruit others to join the cause (Lewis, Gray, and Meierhenrich 2014; Lacetera, Macis, and Mele 2014). While our experiment did not directly analyze the effect of announcing support on the donation of the announcer, the fact that subjects believe announcements are associated with higher donations from announcers suggests that our experimental subjects do not believe that announcements are complete substitutes for actual donations. Indeed, in the field experiment, giving increases when pins are used—suggesting that even if some subjects wear the pin as a substitute for making larger donations, the net effect, including increased donations from those exposed to the pins, is still positive.

How economically relevant are the announcements of support that we investigate here? Facilitating announcements in the field generated a 16 percent increase in giving. This effect size is comparable to results from a number of other field experiments on charitable giving. For example, Shang and Croson (2009) find a 12 percent increase in giving from telling donors calling into a radio fundraising drive that a recent donation was $300. In a direct-mail field experiment, Falk (2007) finds an 11 percent increase in average giving when a small gift (1 postcard) is added to the appeal; the effect is a much larger 60 percent increase when a large gift (4 postcards) is added. In another direct-mail field experiment, Karlan and List (2007) find a 19 percent increase in giving when they announce a matching grant of 1-to-1, 2-to-1, or 3-to-1 as compared to no match. In List and Lucking-Reiley (2002), offering a refund provision for a threshold public good increases giving by 20 percent. Other field experiments have found much larger effects on giving. The same List and Lucking-Reiley paper finds that announcing seed funding to a threshold public good increases giving by hundreds of percent. Landry et al. (2006) find that offering a lottery incentive for charitable donations in a door-to-door fundraising campaign increases giving by 50 percent and that having physically attractive solicitor generates an equivalent effect. Landry et al. (2010) find that potential donors offered a copy of *Freakonomics* (combining conditional and unconditional gifts) give 100 percent more in a door-to-door campaign and 200 percent more in a direct-mail campaign; however, they see no effect with a smaller gift of a bookmark in either setting. Compared to these results, it is clear that facilitating announcements of support is among field interventions generating modest but significant increases in giving.[22]

[22] An additional comparison of interest across experiments on charitable giving is the cost effectiveness of the intervention. In our field experiment, each $0.17 pin sent to a workplace generated $3.85 in extra donations to the charity (even though many workplaces did not use the pins)—a 2,165 percent return on investment for the charity. While this return may seem high, some of the other interventions described in the text effectively offer an infinite return since they are free for the charity (e.g., providing information about a large donation from another donor as in Shang and Croson 2009) or effectively free, conditional on existing donors (e.g., announcing a seed donor as in List and Lucking-Reiley 2002 or announcing a match as Karlan and List 2007 if funds used are already earmarked for the charity). The interventions in Falk (2007) both have a cost-effectiveness estimate comparable to ours. Each gift sent to donors in that experiment costs about 0.12 CHF. Sending one with a solicitation (as in the small gift

Consequently, in addition to teaching us about underlying motivations for giving, the results also provide guidance for charities, policymakers, and companies interested in public good provision.[23] That facilitating announcements of support increased giving in the field experiments suggests benefits to having discussions about the public good take place publicly. Results from the field experiment provide additional guidance by showing that announcements of support increased contributions even in environments where past rates of giving are low; the pins were most effective at workplaces with previous contribution rates between 4 percent and 42 percent. That results were strongest in the workplaces with these relatively low rates of giving suggests that facilitating announcements of support may work even when providing information about previous giving may backfire because historic rates are "bad news" (Coffman, Featherstone, and Kessler 2015).

In addition, results from the laboratory experiments reveal that subjects donate less when they are aware that an individual could have announced support but chose not to do so. This finding suggests that simply facilitating announcements of support may backfire. Organizations aiming to optimize the use of social forces like announcements of support may aim to control information more directly. For example, charities may aim to solicit new donors from previous donors' social networks (e.g., see Castillo, Petrie, and Wardell 2014). Some organizations that facilitate announcements of support already adopt this tactic. Organizing for America (OFA), a fundraising and advocacy organization of the Democratic National Committee, regularly encouraged its potential donors to announce support for a cause by entering their name on a website. They would then prompt the supporter to email news of their support to five friends (and simultaneously ask the supporter for a donation). The news of the support could be emailed even without a donation being made. OFA repeatedly used this strategy to generate support and collect donations for causes including: a repeal of "Don't Ask, Don't Tell," a "Clean Energy Future," and commitments to vote. In a similar spirit, Facebook began allowing members to announce, without validation, that they were organ donors on May 1, 2012, which generated thousands of new registrations in state registries in the subsequent days (Cameron et al. 2013). More research is necessary to better understand how such information structures can be designed to maximize the effects of social forces in general and announcements of support in particular.

An additional line of future work seems particularly worthwhile: investigating the limits of the announcements of support effect documented here. As might be expected, subjects in our laboratory experiments cannot predict whether an announcement of support is going to be followed by a donation. In the main announcement treatments

treatment) generates an additional 0.81 CHF on average per mailing, implying a 565 percent return on investment; sending four with a solicitation (as in the large gift treatment) generates an additional 4.65 CHF on average per mailing, an 854 percent return on investment. Finally, some interventions in the papers described above have a negative return (e.g., the lottery treatments in Landry et al. 2006 and the gift treatments in Landry et al. 2010) since they do not generate enough extra donations to cover the cost of the incentive being provided to encourage donations.

[23] In the field experiment, donations made to the charity funded services for the poor and provided an additional public good: the success of a company's workplace campaign, which may directly or indirectly benefit the company and its employees. Consequently, announcements of support may also encourage employees to provide work effort toward a company goal. This interpretation makes announcements of support a relevant tool not only for fund-raisers and charities but also for managers attempting to motivate workers. For example, employees working on a group project may be able to use announcements of support to encourage one another to provide effort, even if individual contributions are not monitored or individually rewarded.

of both experiments (i.e., the pin option treatment and the announce treatment), the increase in donations associated with an announcement of support is the same regardless of whether the announcer makes a subsequent donation. While announcements in these treatments contain information on average—announcers donate more on average than those who do not announce support—this will likely not be the case in general. Will individuals still respond to announcements of support if announcers are not more inclined to donate than non-announcers, as might be the case if subjects engage in slacktivism and announce support instead of donating? Understanding whether or not agents can systematically predict whether announcements contain relevant information will help us better understand how individuals make charitable giving decisions and how policymakers can use announcements of support to further encourage public good provision.

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