

# Avoiding the Ask: A Field Experiment on Altruism, Empathy, and Charitable Giving

---

James Andreoni

*University of California San Diego and National Bureau of Economic Research*

Justin M. Rao

*HomeAway, Inc.*

Hannah Trachtman

*Yale University*

If people enjoy giving, then why do they avoid fund-raisers? Partnering with the Salvation Army at Christmastime, we conducted a randomized field experiment placing bell ringers at one or both main entrances to a supermarket, making it easy or difficult to avoid the ask. Additionally, bell ringers either were silent or said “please give.” Making avoidance difficult increased both the rate of giving and donations. Paradoxically, the verbal ask dramatically increased giving but also led to dramatic avoidance. We argue that this illustrates sophisticated awareness of the empathy-altruism link: people avoid empathic stimulation to regulate their giving and guilt.

## I. Introduction

It is beyond debate that humans have a great capacity to be generous. People are polite to strangers, give money to charities, volunteer to help oth-

A version of this paper constituted Trachtman’s senior thesis at Harvard College, for which it was awarded the best thesis prize and summa cum laude honors. We would like to note that Trachtman took the lead responsibility in coordinating and executing the ex-

Electronically published April 28, 2017

[*Journal of Political Economy*, 2017, vol. 125, no. 3]

© 2017 by The University of Chicago. All rights reserved. 0022-3808/2017/12503-0001\$10.00

ers, and sometimes even risk their lives in heroic acts of selflessness. Such apparent altruism was initially attributed to indirect selfishness.<sup>1</sup> But when economists removed these incentives in anonymous, one-shot dictator games among unrelated strangers, the initial findings surprised many. Subjects often eschewed selfish choices, with many even choosing equal splits (Forsythe et al. 1994; Roth 1995). If giving cannot be explained by indirect selfishness, then perhaps it can be explained by a taste for altruism (Becker 1974). We learned quickly, however, that individuals were concerned not only with final allocations of consumption but also with the means to that allocation (Andreoni 1988; Andreoni, Brown, and Vesterlund 2002). The implication is that utility depends on the act of giving: that there is some warm glow from helping others (Andreoni 1989, 1990).<sup>2</sup> But the warm glow hypothesis provides a direction for research rather than an answer to the puzzle of why people give: the concept of warm glow is a placeholder for more specific models of individual and social motivations.

Subsequent laboratory experiments clarified the ways in which people experience utility from the act of giving. In particular, social concerns often underlie the warm glow. First, giving tends to increase when social distance is reduced (Roth 1995; Hoffman, McCabe, and Smith 1996; Bohnet and Frey 1999), when subjects communicate (Xiao and Houser 2005; Andreoni and Rao 2011), or when the recipient is identified specifically rather than statistically (Small and Loewenstein 2003). This indicates that warm glow increases with the vividness of the recipient in the mind of the giver. This process would be consistent with the idea that individuals wish to maintain self-images as fair or moral people. Second, giving is more likely when givers believe their donations will be vivid in the mind of the recipient. As the ability of recipients to detect unselfish acts becomes easier, altruistic acts increase sharply (Andreoni and Bernheim 2009; Ariely, Bracha, and Meier 2009), implying that maintaining a positive social image—where the recipient perceives the giver as generous—is another component of warm glow. Third, when people see a way to avoid an opportu-

---

periment. Erzo Luttmer provided exceptionally helpful advice along the way. We would also like to thank Ned Augenblick, Douglas Bernheim, Rob Boyd, Stefano DellaVigna, Daniel Fessler, Ed Glaeser, David Laibson, Randall Lewis, John List, Stephan Meier, Stephanie Preston, David Reiley, Alison Sanchez, Michael Schwarz, Joan Silk, and Lise Vesterlund for helpful comments and Harvard College (Trachtman) and the National Science Foundation, grant SES-1427355 (Andreoni), for financial support. We especially thank the Salvation Army for cooperation in running the experiment. This research was conducted under full Institutional Review Board approval. Data are provided as supplementary material online.

<sup>1</sup> Examples include mutualistic cooperation (Grice 1957), kin selection (Hamilton 1964), repeated-game reciprocity (Trivers 1971), and norm adherence through sanctions (Boyd and Richerson 1992).

<sup>2</sup> Many experiments have demonstrated that preferences depend on more than the outcomes of consumption, beginning with Andreoni (1993), up to most recently Crumpler and Grossman (2008) and Gronberg et al. (2012).

nity to be generous, they are sometimes willing to incur a cost to do so, while those who are not given the opportunity to avoid often decide to give. For example, in the context of the dictator game, many “dictators” choose to pay a portion of their endowment for the right to exit the game (Dana, Cain, and Dawes 2006; Broberg, Ellingsen, and Johannesson 2007; Lazear, Malmendier, and Weber 2012). This avoidance has led some people to wonder whether giving really does increase utility. Work by Andreoni and Rao (2011), however, suggests that both giving and avoiding can be consistent with warm glow. The authors asked one group of potential givers to participate in a brief written conversation with recipients and asked another group to simply write the message they would send in this conversation if placed in the role of the recipient. Both the real and the imagined conversations substantially increased giving relative to a control group. Surprisingly, imagined conversations were just as effective as real conversations. The two results can be consistent if subjects in the control naturally “avoided” considering what receivers would think is fair, whereas the empathic stimulation in the conversation treatments rendered this emotional avoidance infeasible and thus led to a dramatic increase in giving. Moreover, analysis of the conversations and post-questionnaires indicated that those who gave were happy to have done so.<sup>3</sup>

This suggests a new psychological mechanism for warm-glow giving. Psychologists posit that giving is initiated by a stimulus that elevates sympathy or empathy in the mind of the potential giver, much as the smell of freshly baked bread can pique appetite. Resolving this feeling comes either by giving and feeling good or by not giving and feeling guilt. However, those with (implicit or explicit) knowledge of their vulnerability to such stimuli, by controlling the input of those stimuli, can control both their emotions and the actions that result. Just as we should not eat our favorite dessert at every opportunity, we also cannot give at every opportunity, even though we might wish we could do both. Just as a sophisticated eater will avoid exposure to the chocolate cake, a sophisticated altruist can avoid being asked.<sup>4</sup> In contrast, a purely selfish person gains nothing from avoidance. Importantly, this mechanism can explain both avoidance and positive utility from giving.

We explore these ideas with a natural field experiment on charitable giving. Our experiment partnered with the globally renowned Salvation Army’s annual Red Kettle Campaign. In the Christmas season, volunteers for the Salvation Army stand at entrances to stores and shopping malls. They ring a bell, implicitly inviting people to put cash into their trademarked “Red Kettle.” We positioned solicitors at one or both of two main

<sup>3</sup> This evidence is also consistent with more recent evidence on happiness and giving. See Anik et al. (2011).

<sup>4</sup> We use the word “sophisticated” in the sense used by Strotz (1956), Laibson (1997), and O’Donoghue and Rabin (1999).

entrances to a grocery store in suburban Boston over 4 days and measured how the presence of the solicitors at one or both doors affected traffic and donations. We combined this with another manipulation: solicitors either asked for donations by saying “please give today” or were verbally silent (they still rang the bell), avoiding even eye contact. Notice that the silent bell ringer is still obviously requesting a donation, but the verbal ask is adding a more stimulating layer of social interaction.

Our primary question is whether avoiding the ask is an indication that altruistic people are (consciously or subconsciously) attempting to control their empathic emotions. Our experimental data inform this question in three ways. First, when we strengthen the social interaction by verbally asking for money, does this extra appeal to empathy increase the frequency of giving and the amount donated? Second, when we make it easier to avoid the solicitation, how many people do so, and are they avoiding giving or simply avoiding being asked? Finally, how many potential givers go out of their way to “seek” an inconveniently located solicitor?

The results are as follows. When the solicitors were silent, there was no discernible impact on store traffic: few people avoided the solicitors, and fewer still sought them out in order to give. Putting a solicitor at both doors, rather than just one, nearly doubled the total number of givers and amount given. In the presence of a verbal request, the results were dramatically different. Asking had a powerful effect on giving rates and total donations, which increased 45–69 percent in the verbal request conditions as compared to their “silent” counterparts. When avoidance was easy because only one door had a solicitor, nearly one-third of those intending to pass through the occupied door instead chose to use an unoccupied entrance. That is, they avoided the explicit verbal ask but not the ask implicit in ringing the bell. Doubling the solicitation did not double the number of givers or total donations—the increase was 65–80 percent—indicating that some givers sought out the solicitor when only one was present.

In general, giving was highly sensitive to our conditions: donations varied from \$0.30 per minute in the one-door silent condition to \$1.00 per minute in the two-door verbal ask condition. Why do so many people reveal a preference to give in one setting and then reveal the opposite preference in another? Our findings provide two clues. First, the dramatic increases in giving when a second solicitor is added suggest that “passive givers”—those who do not avoid or seek giving opportunities—account for a large fraction of giving. Second, the ask is indeed powerful: the data indicate that asking is both effective when experienced and aversive from afar. Moreover, our data rule out several common explanations for the power of the ask. The request script intentionally did not convey any information, which implies that the effect of asking cannot be explained by increased awareness about the fund-raiser. Social image or self-image concerns are also unable to explain the power of asking in our setting: posi-

tive image can be burnished equally well in both a silent opportunity and an active request.

We are left to conclude that the power of the ask derives from the emotional reaction it induces in a potential giver. This is consistent with other research demonstrating that altruistic acts are often preceded by empathic stimuli (Batson 1991; Preston and deWaal 2002; Andreoni and Rao 2011).<sup>5</sup> Under this motivation, the desire to give must follow the ask, and not precede it, and by controlling exposure to the ask, one can regulate both emotions and giving. Those who avoid the ask are not callous or selfish, but rather are “good people” who are avoiding empathic stimuli, such as an ask, as a means to regulate their giving and guilt.<sup>6</sup>

This research raises a natural policy question. If so many people avoid the ask, might fund-raising have a negative impact on welfare? The answer to this question will depend critically on parameters that are difficult or impossible to measure, such as the marginal utility of money for recipients of charity and the psychological costs of saying yes or saying no. Because in our setting people actually have to exert physical effort to avoid being asked to give, we can put bounds on some of these otherwise unobservable parameters. For instance, if a person prefers to go out of his or her way by about 70 feet, we can view that cost as a lower bound on the cost of facing a charitable solicitation. Using what we learn from subjects sorting across entrances, we can say that in our context, the cost of saying no plus the cost of avoiding being asked is almost equal in value to the donations collected in the campaign. Furthermore, we are able to provide somewhat informal bounds on the utility from saying yes and on the value of donations to society that would make fund-raising welfare enhancing or not.

To arrive at the above conclusions, we took very seriously three shortcomings of our experimental design. First, and most importantly, there was a third door around the corner that led to a recycling area, and we neglected to realize that patrons were actually using this door to enter and exit the store. Second, although we observe 17,622 passings, we have only 16 treatment blocks. Third, in the original experiment we had no “pure” control treatment with no solicitors and measured avoidance relative to the treatment with solicitors at both doors. Throughout the paper we will describe the ways in which these flaws might be problematic for our results and address each in detail.

In related work, DellaVigna, List, and Malmendier (2012) use a design similar to ours (though conceived independently) with the chief aim of

<sup>5</sup> This explanation has much in common with the theory of cue-triggered choices of Bernheim and Rangel (2004) and the willpower depletion model of Ozdenoren, Salant, and Silverman (2012).

<sup>6</sup> Authors in the neuroscience literature have argued in favor of this view. See, e.g., De Vignemont and Singer (2006) and Hare et al. (2010).

estimating the welfare impact of fund-raising. In their field experiment, some residents were given an opportunity to opt out of door-to-door solicitation by checking a “do not disturb” box on a card left at their front doors. Many residents took this option, while the average gift was higher for those who actively consented to solicitation. We view our study as complementary to theirs. With regard to empirics, their study was designed to measure time costs and define “social pressure” costs in order to estimate the welfare impact of the opting-out manipulation. Our study is aimed more squarely at understanding the motives behind giving.<sup>7</sup> With regard to theory, DellaVigna et al. put forward a model with social pressure as the driving force, where social pressure is defined to be a utility cost that is decreasing in donations and that is felt only in the presence of solicitation. Indeed, this model is also capable of explaining our results (provided that social pressure can be “avoided”) and shares many similarities with the mechanism we have in mind.<sup>8</sup> Despite predicting similar behavior in many contexts, the two models are distinct. Our notion of “empathic stimulation” captures the idea of inner conflict between the “planning” self and the self who, when asked, has an emotional response that tempts her to give. Under this view, the ask is not a direct cost, but rather a cue that triggers suboptimal deviations in giving behavior.

Finally, recent work on the reliability of “positive results” has highlighted the importance of using related literature to form priors and conducting independent replications (Doyen et al. 2012; Open Science Collaboration 2012; Maniadis, Tufano, and List 2014). In terms of priors, this study was motivated by the “power of the ask” found by Andreoni and Rao (2011) in a laboratory dictator game, which provides a relatively strong prior on this dimension. Indeed, we designed this experiment very much as a “field replication” of the lab findings. Castillo, Petrie, and Wardell (2014) also subsequently found the power of asking in a field fund-raising context. With regard to finding avoidance, DellaVigna et al. (2012) provide a strong prior in a fund-raising setting similar to ours. Finally, the statistically significant results in the only known replication of our protocol, by Trachtman et al. (2015), dramatically increase our confidence in the finding on avoidance in this particular context, since a false positive would require

<sup>7</sup> For example, avoidance of solicitation in their study might be attributable to factors such as the time cost of answering the door or uncertainty about safety. Both are unrelated to motives for giving. Giving might be increased because givers make an effort to be available when they know the solicitor is coming, or people might have more time to deliberate and prepare, as indicated by Landry et al. (2010). In evaluating an opt-out policy, it does not matter per se if giving increases because it allows people to “seek” or because of increased verifiability; what matters is the impact on donations and overall time saved for the solicitor and solicitee.

<sup>8</sup> This applies under two conditions. First, increased anticipated social pressure makes the solicitation both more effective and more aversive. Second, one needs to accept that the social pressure is higher during the verbal ask than in simple bell ringing. We think both of these are sensible and defensible applications of the DellaVigna et al. model.

two independent outcomes with very low probability. Thus we conclude that the collective evidence places our findings on very firm ground.

The paper proceeds as follows. Section II presents the design of the field experiment. Section III provides a framework for analysis. Section IV shows the results. Section V discusses our findings, while Section VI is a conclusion.

## II. Design of the Field Experiment

The Salvation Army Red Kettle Campaign is one of the best-known and largest street fund-raising campaigns in the United States. The campaign occurs annually in the weeks leading up to Christmas Day. Volunteers, clad in distinctive red aprons and a Santa hat, ring bells to solicit passersby for donations, which are placed in a locked red kettle. The campaign raises over \$100 million annually, and the funds help provide “food, toys and clothing to over 6 million people” (see <http://www.ringbells.org>). The prominence of the Red Kettle Campaign makes it likely that subjects viewed the solicitor as representing a legitimate and worthy cause.

We chose a location in the Boston area to satisfy the following criteria: (1) the store had two main doors that were far enough apart to create a meaningful opportunity to seek or avoid a solicitation, (2) both main doors were visible from the parking lot, and (3) traffic amounted to at least 180 people per hour. An aerial photo of the selected store is shown in figure 1. In our text, we will refer to the doors as labeled in this figure, with door 1 on the left and door 2 on the right. Both doors opened in the direction of the main parking lot. As identified in the figure, the store also had a side



FIG. 1.—The store studied. Doors 1 and 2 were the main entrances, while door 3 was the side “recycling” door.

door, door 3, which was around the corner from door 1. Door 3 was marked “recycling” because it led directly to an area for recycling plastic bags.<sup>9</sup>

We implemented a  $2 \times 2$  design. Solicitation occurred in two modes: only bell ringing or bell ringing with a verbal request. We will refer to these two as opportunity, or “Opp,” and “Ask,” respectively. In the opportunity conditions, solicitors rang the bell as usual but did not speak or attempt eye contact, except to thank those who gave, as per Red Kettle custom. The ask condition was the same as the opportunity condition except that solicitors attempted eye contact with each passerby and said, “Hi, how are you? Merry Christmas. Please give today.” The other dimension is whether we had solicitors at only door 1 or at both doors 1 and 2. Hence, for ease of exposition we will refer to our four conditions as Opp1, Opp1&2, Ask1, and Ask1&2.

Each solicitor discreetly recorded the number of givers using a counter in her apron pocket. Two additional research assistants recorded shopper traffic in and out of doors 1 and 2 from cars parked nearby. Only individuals who appeared 18 or over were counted.<sup>10</sup> If two adults arrived together, both were counted. The study was conducted from 11:00 a.m. to 7:00 p.m. over four weekdays (Monday through Thursday), December 7–10, 2009. Each day was divided into four treatment “blocks” lasting 1 hour and 32 minutes each. Each block was further divided into 23-minute “sessions.” The solicitors and observers all carried synchronized watches that beeped at the end of each session. At this juncture, solicitors recorded session tallies for traffic and givers. The counters were then quickly reset and the new session began. The kettles were switched after each block, when the solicitor appeared to be taking a break, in order to minimize any unnatural behavior. This means that donations are observed only at the block level.<sup>11</sup>

Conditions were assigned to blocks according to the Latin square configuration shown in table 1. The configuration ensures that blocks were balanced across days and time of day. Daily balance helps ensure that factors such as weather, day of the week, and solicitor identity were evenly distributed. Time slot balance ensures that time of day effects were also evenly distributed across the four treatments. This design does not, however, eliminate potential day of week by time of day effects. For example, suppose that Monday evening at 4:00 p.m. was a particularly busy time

<sup>9</sup> Door 3 was different from the other doors only in its visibility (it was around the corner) and the fact that its users had to pass through a small recycling area before entering the store. It was similar in every other way.

<sup>10</sup> Taxi drivers and store employees were not counted. They enter and exit the store many times during the day but are not shoppers.

<sup>11</sup> The data collection was overseen by Trachtman. Across all conditions, Trachtman acted as the solicitor at door 1. The solicitor at door 2 was a paid research assistant. All the bell ringers in this study were 22-year-old white females at the time of the study. Trachtman administered a 45-minute training session prior to the study.

TABLE 1  
EXPERIMENT SCHEDULE: DECEMBER 7–10, 2009

	Monday December 7	Tuesday December 8	Wednesday December 9	Thursday December 10
Block 1: 11:00 a.m. to 12:32 p.m.	Ask1&2	Opp1	Ask1	Opp1&2
Block 2: 12:50 p.m. to 2:22 p.m.	Ask1	Opp1&2	Ask1&2	Opp1
Block 3: 3:40 p.m. to 5:12 p.m.	Opp1	Ask1&2	Opp1&2	Ask1
Block 4: 5:30 p.m. to 7:02 p.m.	Opp1&2	Ask1	Opp1	Ask1&2

and that a particular door is favored during busy periods. Although we guarded against this confound by choosing non-Friday weekdays, we cannot directly test for it with our original data. As such, we later returned to the store and collected data on customer traffic for 4 days without any solicitation, which we refer to as the placebo data.<sup>12</sup> As we discuss in detail further on, the placebo data reveal that day of week by time of day interactions are not a concern in our setting.

In order to minimize the possibility of a shopper entering with verbal asks and leaving with silent opportunities (or vice versa), the two blocks in the morning were either both ask or both opportunity sessions, as were the blocks in the afternoon. This meant that one-door and two-door treatments had to be interspersed throughout the day, making it possible for a shopper to enter during a two-door treatment and exit during a one-door treatment, or vice versa. We note, however, that this only dilutes our results and cannot confound them, as lack of (or false) knowledge of the solicitors' locations works against the ability to sort.<sup>13</sup>

Unfortunately, we did not count traffic through door 3. The reason is simple: in our initial selection of the store we did not realize that one could actually get from the recycling area to the store proper. That shoppers could enter and exit door 3 became known to us only after the data collection. The main drawback that this oversight creates is that we are unable to directly measure avoidance to door 3 and must instead infer it from traffic patterns at the two main doors. For example, if the two main doors have a surprisingly low number of people for a certain condition, provided that the experiment is balanced across other traffic factors, this indicates avoidance to door 3. "Surprisingly low," however, has to be relative to some baseline. In our primary analysis we treat traffic under Opp1 as the "baseline." This means we are unable to measure avoidance to

<sup>12</sup> The placebo data were collected in July 2013.

<sup>13</sup> If the shopper enters during a two-door treatment and exits during a one-door treatment, the door through which the shopper exits should be neutral with respect to the treatments since the shopper believes there are solicitors at both doors. If the shopper enters during a one-door treatment and exits during a two-door treatment, the shopper may choose to exit through door 2 in the belief that there is no solicitor there, which would bias the estimate of sorting downward.

door 3 in this condition, but we can measure avoidance to door 2, which turns out to be very close to zero, indicating this is not major concern.

A potentially larger concern is that the door 3 inference rests heavily on the measurement of total traffic through doors 1 and 2. Since experimental blocks were randomized in a balanced fashion, this is an unbiased inference, and the variation in traffic captures the statistical uncertainty around the measurement. However, the fact that there were only 64 experimental sessions (traffic measurements) assigned to 16 four-session blocks raises two concerns. First, there might be day of week by time of day anomalies in certain experimental cells. Second, these statistics cannot rely solely on asymptotics and instead rest on normality assumptions that we cannot directly verify with the original data since observed traffic is endogenous to the type of solicitation. The placebo data provide traffic measurements that are not contaminated by varying charitable solicitation, allowing us to address both concerns. They enable us to directly control for the interaction between day of week and time of day by using fixed effects in a regression framework. Furthermore, in the online appendix, we show that both total traffic and the residuals from a regression on day and time dummies are well approximated by a normal distribution (Shapiro-Wilk tests give  $p$ -values of .97 and .44 for total traffic and residuals, respectively), indicating that our test statistics rest on empirically valid assumptions.

In the remainder of the paper, we will use “bell ringing conditions” to refer to the conditions in which a solicitor was actually present and “placebo conditions” to refer to the same daytime slots in the absence of solicitation (each bell ringing treatment cell is matched to the placebo cell from the same day of week and time of day). Finally, we note that while the oversight in measurement of door 3 does not directly affect our analysis of the giving behavior, the placebo data lend valuable reassurance about our statistical inference in this domain as well.

### III. A Simple Framework for Giving, Avoiding, and Seeking

Imagine a shopper stepping from her car and immediately hearing the ring of the Salvation Army’s bell. Depending on the shopper’s self-awareness, she may imagine what she would do and how she would feel if she passed a bell ringer. She can also evaluate how she would feel if she instead chose an entrance without a solicitor and did not donate on this occasion. Finally, she considers the cost of each path. From her car, the shopper chooses the path with the highest utility *ex ante*. That is, she makes the choice before feeling the impact of the social and emotional incentives that guide her choice. This can be viewed as an intertemporal choice problem: in the “cold state” of the parking lot, she chooses which “hot state” to put her future self in.

Let  $u_s$  be the utility from passing a solicitor and choosing the optimal gift,  $g_s$ , conditional on passing. Likewise, let  $u_0$  be the utility from avoiding a solicitor and giving zero. Clearly, avoiding an ask has the benefit of saving money but may come with some guilt. Passing a solicitor may result in a donation if the ask is persuasive enough or it may result in guilt at saying no. If the solicitation is “passive” in the sense of not creating any more than a simple opportunity to give, then perhaps the shopper can choose her cold-state optimum, say  $g_0$ , which could be zero. If there is a heightened emotional appeal to give, which could come from the solicitor making eye contact and saying “please give today,” then giving  $g_0 = 0$  according to the original plan may produce feelings of guilt and may even motivate the shopper to give  $g_s > 0$  instead. Depending on the individual’s preferences for giving, tolerance for guilt, vulnerability to asking, and costs of changing entrances, the shopper in the parking lot may decide to choose a door without a solicitor as a means of commitment. Otherwise, she anticipates that her emotional short-run self will either feel guilty or overrule her calculating planner self and give too much.

It is fair to treat parking spots as approximately randomly assigned, and thus we can think of shoppers as endowed with a “most convenient door.” Normalize the cost of going to the most convenient entrance at zero, and let  $c > 0$  be the cost of changing entrances. Then people who give can be of two types: those who pass through their endowed door, *passive givers* ( $u_s > u_0 - c$  and  $g_s > 0$ ), and those who change from their endowed door to seek an opportunity to give, *seeking givers* ( $u_s - c > u_0$  and  $g_s > 0$ ). What about people who don’t give? These can be of three types. First is *passive nongivers* ( $u_s > u_0 - c$  and  $g_s = 0$ ), that is, people who use their endowed entrance but do not give. Second are *giving avoiders* ( $u_0 - c > u_s$  and  $g_s > g_0$ ). These are people who, if they passed a solicitor, would give more than the planner-self would prefer and thus steer themselves away from the ask. Finally, there are *saying no avoiders* ( $u_0 - c > u_s$  and  $g_s = g_0 = 0$ ). As the name suggests, these people have the self-control to say no if they pass, but the cost of guilt at saying no makes it worth paying the cost to choose another entrance.

Our experiment provides exogenous variation to help identify the relative sizes of these groups. First, by adding a second solicitor at the other main entrance, we are increasing the cost of avoiding being asked, as it is now harder to find an unoccupied door. On the basis of the physical layout of the store, this cost will be much higher for those endowed with door 2 as compared to door 1. This can be seen in figure 1: those parked near door 2 have to actually walk past door 1 to find the unmanned door 3. Next we vary the intensity of the ask. A simple bell ringer is a familiar sight and is seen by many as a “passive” ask. By contrast, the direct verbal ask of “please give today” is expected to have heightened psychological consequences (Andreoni and Rao 2011). This has three effects

conditional on being asked: relative to bell ringing alone, it raises the probability of giving, the size of donations, and the guilt of saying no. Working backward, if any of these three effects lowers the planner's utility enough, avoiding the ask becomes more attractive in the cold state of the parking lot. We can measure this avoidance by exploiting the variation in the intensity of requests and costs of avoidance induced by the experiment. The net impact of a more emotional ask will depend on the distribution of types in the population. In the next section we characterize the general preferences of our sample using this framework.

#### IV. Results

We begin by looking at giving in the bell ringing treatments. We next examine how charitable solicitation affected traffic patterns. Finally, we comment on what our results imply about the psychological mechanisms behind giving and the welfare effects of fund-raising.

##### A. Total Giving

Figure 2 displays the total number of givers per 23-minute session (left axis) and total money donated per 92-minute block (right axis) by condition. The global average number of givers per session was 12.6, or about

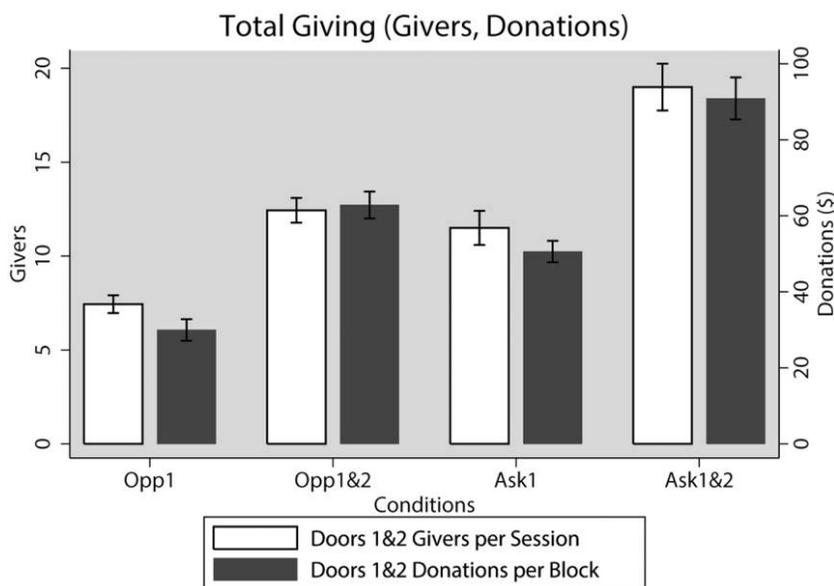


FIG. 2.—Total givers and donations by condition. Bars give 1.96 standard errors.

0.55 giver per minute, which netted \$58.57 in donations per block, or about \$0.64 per minute. The number of givers varied widely across conditions, from 0.32 giver per minute in the one-door opportunity condition (Opp1) to 0.83 per minute in the two-door ask condition (Ask1&2). Total donations showed similar variation, exhibiting a threefold difference across conditions, \$0.33 to \$0.99 per minute. This is our first evidence that relatively small changes in the fund-raising approach can lead to large differences in giving rates and money raised.

Table 2 confirms the statistical significance of the differences across conditions using an ordinary least squares (OLS) framework that allows for day of week and time of day fixed effects. The number of givers is the dependent variable in column 1, donations is the dependent variable in column 2, and Opp1 is the omitted group. First, comparing the simple bell ringing to the verbal ask, we see that asking is indeed powerful. The coefficient on Ask1 reveals that in the one-door conditions, the presence of a verbal request raised the number of donors by 55 percent and total donations by 69 percent. The power of asking was also observed in the two-door conditions, increasing givers by 53 percent (col. 1) and donations

TABLE 2  
OLS REGRESSIONS OF TOTAL GIVING IN BELL RINGING TREATMENTS

Variable	Number of Givers per 23-Minute Session (1)	Dollars of Donations per 92-Minute Block (2)
Opp1&2	5.00*** (1.164)	32.86*** (4.166)
Ask1	4.06*** (1.135)	20.63*** (4.785)
Ask1&2	11.56*** (1.308)	60.90*** (4.901)
Observations	64	16
R <sup>2</sup>	.726	.977
Mean of Opp1	7.44	29.97
Date and time fixed effects	Yes	Yes
Predicted levels:		
Opp1	7.44	29.97
Opp1&2	12.44	62.83
Increase	67%	110%
Ask1	11.51	50.60
Ask1&2	19.00	90.87
Increase	65%	80%
F-tests, <i>p</i> -values:		
Opp1&2 = Ask1&2	.000	.000
Ask1 = Ask1&2	.000	.000
Opp1&2 = Ask1	.395	.008

NOTE.—In col. 1, standard errors clustered by block are in parentheses. In col. 2, robust standard errors are in parentheses.

\* *p* < .1.

\*\* *p* < .05.

\*\*\* *p* < .01.

by 45 percent (col. 2). All of the above differences are significant beyond the .01 level. For both giving rates and donations, Ask1 does not differ significantly from Opp1&2, which indicates that the short verbal request was about as effective as adding an additional silent solicitor.

Second, table 2 provides initial evidence about the extent to which giving can be attributed to people who sort. In our framework, potential givers come in three types: seeking givers, giving avoiders, and passive givers. The first two types change their preferred door on the basis of the level of solicitation, and thus giving depends on sorting (or the inability to do so). Differential giving rates across conditions identify the relative importance of givers who sort. In the opportunity conditions the second solicitor increased the number of givers by 67 percent (five givers per session) and dollars donated by 110 percent (\$32.86 per block). In the ask conditions, the second solicitor raised giving by 65 percent (7.5 givers per session) and donations by 80 percent (\$40.27 per block). Since the doors had roughly equal baseline usage rates, in the absence of any sorting, we would expect the two-door conditions to do twice as well as their one-door counterparts. We observe statistically significant deviations from this prediction, especially in the ask conditions. However, the evidence also rules out the possibility that sorting accounts for all giving. If giving were dominated by seeking givers, then the one- and two-door conditions should perform similarly. Conversely, if giving avoiders accounted for the bulk of giving, then one-door conditions, when avoiding was easy for all shoppers, would be expected to garner very few donations. We strongly reject both these hypotheses as well. In the next two subsections we incorporate traffic patterns to further understand sorting and giving behavior.

### *B. Avoiding the Ask*

We define traffic to be the total “passings” in and out of the store. A single shopper thus accounts for two passings, a fact we account for in our statistics. Panel A of table 3 reports the raw traffic for the bell ringing conditions. Since our late discovery of door 3 meant that we did not count traffic through this door, the totals at doors 1 and 2 do not represent total shoppers but are rather endogenous to avoidance induced by each condition. Total counted passings were highest in the condition Opp1, with the most passive and most easily avoided solicitation (to door 2 as well as door 3). Comparing total traffic in Opp1 to Ask1 and Ask1&2, we observe a steep decline under the ask conditions. Around 800 fewer people passed through doors 1 and 2 under Ask1 relative to Opp1, with the entire difference between the two totals due to traffic through door 1 (where the solicitor was located). Ask1&2 sees a similar drop in total traffic that is more evenly distributed between doors 1 and 2. Using Opp1 as a baseline, in panel B of table 3 we impute avoidance to door 3 under the

TABLE 3  
GROSS TRAFFIC IN BELL RINGING CONDITIONS AND PLACEBO CONDITIONS

	SILENT OPPORTUNITY		DIRECT ASK		TOTAL
	Door 1 (Opp1)	Doors 1 and 2 (Opp1&2)	Door 1 (Ask1)	Doors 1 and 2 (Ask1&2)	
A. Actual Bell Ringing Traffic					
Door 1	2,563	2,508	1,728	1,918	8,717
Door 2	2,284	2,174	2,321	2,166	8,945
Total doors 1 and 2	4,847	4,682	4,049	4,084	17,662
B. Imputed Door 3 Bell Ringing Traffic, Opp1 Total as Baseline					
Imputed total all doors	4,847	4,847	4,847	4,847	19,388
Imputed door 3 increase	0	165	798	763	1,726
C. Actual Placebo Traffic					
Door 1	2,223	2,092	2,242	2,249	8,806
Door 2	2,119	2,224	2,088	2,194	8,625
Door 3	871	901	867	853	3,492
Total doors 1 and 2	4,342	4,316	4,330	4,443	17,431
Total all doors	5,213	5,217	5,197	5,296	20,923
D. Imputed Door 3 Placebo Traffic, Opp1 Total as Baseline					
Imputed total all doors	4,342	4,342	4,342	4,342	17,368
Imputed door 3 increase	0	26	12	-101	-63

NOTE.—Panel A reports observed traffic under the bell ringing conditions (December 2009). Panel B imputes traffic in door 3 as the deviation in doors 1 and 2 traffic from that of Opp1 (4,847). Panel C reports observed traffic under the placebo conditions, i.e., without solicitation (July–August 2013). Panel D imputes traffic in door 3 in the same way as panel B, using the placebo data.

other conditions. Insofar as there was indeed avoidance in Opp1, these measurements will understate total avoidance. The additional avoidance imputed for Opp1&2 is relatively small, at 3.4 percent of total passings, whereas the adjustment to both ask conditions is much more substantial, at 16 percent.

The surprising lack of shoppers entering doors 1 and 2 under the ask conditions is our first evidence of net avoidance. A standard statistical test, such as a *t*-test in an OLS framework, reveals that this difference is significant beyond the .01 level. Such a test relies on two important assumptions. First, the null hypotheses require that the treatment blocks be independent and identically distributed conditional on the day of the week, time of the day, and the product. We consider the first two reasonable assumptions because the Latin square configuration ensured that each 92-minute treatment block occurred once in each of the four time slots and once on each of the four days (Monday–Thursday). It is possible, however, that a certain time was particularly popular on a certain weekday. For example, it might be that the ask conditions just happened to be assigned to unpopular day of week by time of day spots, which would explain the low

counted passings. Since there were only 16 treatment blocks, random assignment does not ensure the independence of treatment blocks from the daytime slot. Moreover, with only 16 treatment blocks, traditional test statistics require a normality assumption.<sup>14</sup>

We use the placebo data to validate both of these assumptions. In panel C of table 3 the “conditions” in the placebo data occupy the same time of day and day of week positions as they did in the original data, but there is no charitable solicitation. Total traffic is stable in the placebo conditions; counted traffic through doors 1 and 2 is around 4,350 in all conditions. In the placebo data we have an accurate count of traffic at door 3, which is stable at about 17 percent of traffic. In panel D we conduct the same exercise as panel B with the placebo data, that is, supposing that we did not have access to door 3 data and had to infer it as deviations using Opp1. The deviations range from 26 to  $-101$  and sum to  $-63$ , which stands in stark contrast to the total 1,726 displaced passings in the bell ringing conditions. These results suggest that time of day by day of week interaction effects cannot explain the substantial differences in traffic we observe in the bell ringing treatments. Second, we show in the online appendix that both total traffic and the residuals from a regression on day and time dummies are well approximated by a normal distribution (Shapiro-Wilk tests give  $p$ -values of .97 and .44 for total traffic and residuals, respectively), indicating that this assumption was not problematic either.

A final robustness check is to look at the distributions of session-level traffic by treatment. Of the 32 opportunity sessions, the one with the lowest traffic was a one-door session with 235 passings. In comparison, 15 of 32 ask sessions had fewer than 235 passings. That is, nearly half the ask sessions had less traffic through doors 1 and 2 than the lowest opportunity session. Given the Latin square design, the probability of this happening by chance is less than one in 10,000. In figure A5 of the online appendix, we present kernel density plots that further illustrate the stark differences in the traffic distributions.

With our measures of statistical uncertainty on solid ground, we now formally quantify avoidance. Column 1 of table 4 uses an OLS specification with Opp1 as the omitted condition. The coefficients thus represent deviations in total counted traffic under the bell ringing conditions (reflecting avoidance to door 3) as compared to Opp1. We find substantial and statistically significant avoidance to door 3 under both ask conditions. The increase is 16.5 percent for Ask1 and 15.7 percent for Ask1&2. These coefficients are statistically significant beyond the .01 level but are not statistically distinguishable from each other. This indicates that avoidance to

<sup>14</sup> This is less of a concern because the significance is so strong. Standard bootstrapping procedures and other methods that get around normality assumptions still easily produce statistical significance.

TABLE 4  
OLS REGRESSIONS OF DOORS 1 AND 2 TRAFFIC ON BELL RINGING AND PLACEBO CONDITIONS

Variable	Doors 1 and 2 Traffic under Bell Ringing Conditions (1)	Doors 1 and 2 Traffic under Bell Ringing and Placebo Conditions <sup>a</sup> (2)
Opp1	Omitted group	4.425 (30.08)
Opp1&2	-10.31 (12.71)	-4.100 (21.33)
Ask1	-49.87*** (15.11)	-44.62* (23.26)
Ask1&2	-47.69*** (14.18)	-50.21*** (14.77)
Observations	64	128
R <sup>2</sup>	.760	.807
Mean of omitted group	302.93	299.59
Date and time fixed effects	Yes	No
Date × time fixed effects	No	Yes
<i>F</i> -tests, <i>p</i> -values:		
Opp1&2 = Ask1&2	.006	.096
Ask1 = Ask1&2	.880	.842
Opp1&2 = Ask1	.007	.219
Opp1 = Opp1&2		.820
Opp1 = Ask1		.217

NOTE.—Standard errors clustered by block are in parentheses.

<sup>a</sup> Bell ringing traffic if 2009,  $\theta \times$  (placebo traffic) if 2013.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

door 3 was not influenced by the presence of a solicitor at door 2. This has a natural explanation given the physical layout of the store, as shown in figure 1: shoppers parked between doors 1 and 2 who observe a solicitor at both doors would have to walk in front of door 1 to reach door 3. The evidence indicates that, as anticipated, this was not an attractive avoidance strategy. Finally, we observe that the coefficient on Opp1&2 is within one standard error of zero.

In column 2 we incorporate the placebo data directly into our regression framework in order to include 64 day of week by time of day interactions. To do so we must overcome a hurdle. The counted traffic in doors 1 and 2 in the placebo conditions was stable at about 4,350 per condition, less than the 4,800 or so we observed in the opportunity bell ringing conditions. This likely represents a level shift in popularity of the store between the two collection periods. In particular, the evidence in Section IV.A rules out that the higher traffic in the bell ringing conditions is due to seeking of the solicitor.<sup>15</sup> Given these differences, we need to inflate total

<sup>15</sup> We are assuming that people seek only in order to give. That is, they gain no pleasure from saying no.

traffic under the placebo sessions to make it comparable to the bell ringing sessions; otherwise all the bell ringing condition coefficients will be biased upward. We do so by specifying a multiplier,  $\theta$ , to apply uniformly to all placebo sessions. Since the counted traffic in the bell ringing conditions is endogenous, it is not possible to perfectly identify  $\theta$ . Our strategy is to compute the multipliers under three reasonable assumptions about displacement under the bell ringing treatments.<sup>16</sup> The three assumptions generate multipliers of 1.1163, 1.1006, and 1.0848, respectively (see app. table A1 for more details).<sup>17</sup> We interpret this as consistent evidence that there were about 10 percent more shoppers in the time period of the bell ringing treatment; as such we specify a preferred multiplier of 1.1.

Returning to table 4, in column 2 the placebo data, multiplied by  $\theta$ , form the omitted group, and we include 64 temporal fixed effects. Given the large number of regressors, the standard errors naturally rise, but the point estimates are similar to those in column 1. This is what we would expect since in both specifications the opportunity conditions were effectively used as a baseline. The reason is that we used the opportunity conditions to calculate the multiplier, meaning we are unable to speak to baseline avoidance in Opp1, but report the estimate for completeness, noting that it is constrained to be close to zero by construction. In tables A2 and A3 in the online appendix, we show that the main results are robust to using any plausible multiplier. Overall, the direct inclusion of time of day by day of week fixed effects further confirms our finding of substantial avoidance.

We have thus far focused on avoidance to door 3 by examining deviations in total traffic in doors 1 and 2. In table 5 we use the same regression framework to examine the distribution of traffic between doors 1 and 2. Column 1 shows that relative to Opp1, door 1 traffic drops by 32.6 percent in Ask1 and by 25.2 percent in Ask1&2. The larger drop in Ask1 traffic is expected (since avoidance to door 2 is possible), but the magnitude of the difference is not statistically significant. Column 2 indicates why: there is little variation at door 2 overall. In Ask1&2, the solicitor at door 2 has little impact on door 2 traffic, consistent with the idea that for a shopper endowed with door 2, avoiding through door 3 is not an attractive strategy.

Finally, in column 3, we regress the fraction of observed traffic (doors 1 and 2) at door 1 on the bell ringing conditions. Importantly, we have already shown that the denominator falls substantially in Ask1 and Ask1&2

<sup>16</sup> First, we take displacement to be zero in doors 1 and 2 under Opp1, since under this condition displacement effects were the smallest. Second, to use more data, we take displacement to be zero in doors 1 and 2 under both Opp1 and Opp1&2 (which also had low displacement). Finally, we take displacement to be zero in door 2 under Opp1, since we expect door 2 displacement to be especially low given the layout of the doors.

<sup>17</sup> For example,  $1.1006 = (4,847 + 4,682)/(4,342 + 4,316)$ .

TABLE 5  
OLS REGRESSIONS OF TRAFFIC DISTRIBUTION ON BELL RINGING CONDITIONS

Variable	Door 1 Traffic (1)	Door 2 Traffic (2)	Door 1 Traffic as Fraction of Doors 1 and 2 Traffic (3)
Opp1&2	-3.44 (10.34)	-6.88 (7.958)	.0046 (.0268)
Ask1	-52.19*** (11.96)	2.318 (7.61)	-.111*** (.027)
Ask1&2	-40.31*** (9.757)	-7.375 (10.07)	-.067** (.025)
Observations	64	64	64
R <sup>2</sup>	.664	.802	.689
Mean of omitted group	160.19	142.75	.537
Date and time fixed effects	Yes	Yes	Yes
<i>F</i> -tests, <i>p</i> -values:			
Opp1&2 = Ask1&2	.865	.865	.008
Ask1 = Ask1&2	.410	.410	.085
Opp1&2 = Ask1	.335	.335	.000

NOTE.—Standard errors clustered by block are in parentheses.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

as a result of avoidance to door 3, so the coefficients will be biased toward zero.<sup>18</sup> Consistent with this logic, the avoidance estimates are lower than in column 1 but highly significant nonetheless, at 20.7 percent and 12.5 percent under Ask1 and Ask1&2, respectively.

### C. Seeking and the Types of Avoidance

In the last section we estimated avoidance net of seeking behavior. Avoidance dominates seeking in terms of magnitudes, but this does not rule out seeking. Do most donations come from seeker types? When people avoid the solicitor, are they avoiding giving, or are they avoiding “saying no”? In this section we try to disentangle underlying motives by examining giving rates and avoidance patterns together.

Since door 1 always had a solicitor, we start by looking at giving rates under differential traffic patterns induced by the experiment. Figure 3 shows the number of givers and total donations at door 1 across conditions. The placebo data indicate that traffic is nearly equally split across

<sup>18</sup> To see this consider the following example. Suppose that in the baseline, 50 people use door 1 and 50 people use door 2. The door 1 share is 50 percent. In the treatment, 40 use door 1, 50 use door 2, and 10 use door 3. The true share of traffic through door 1 is 40 percent, suggesting avoidance of 10 percentage points; but calculating just using doors 1 and 2, we get 44 percent, or avoidance of only 6 percentage points.

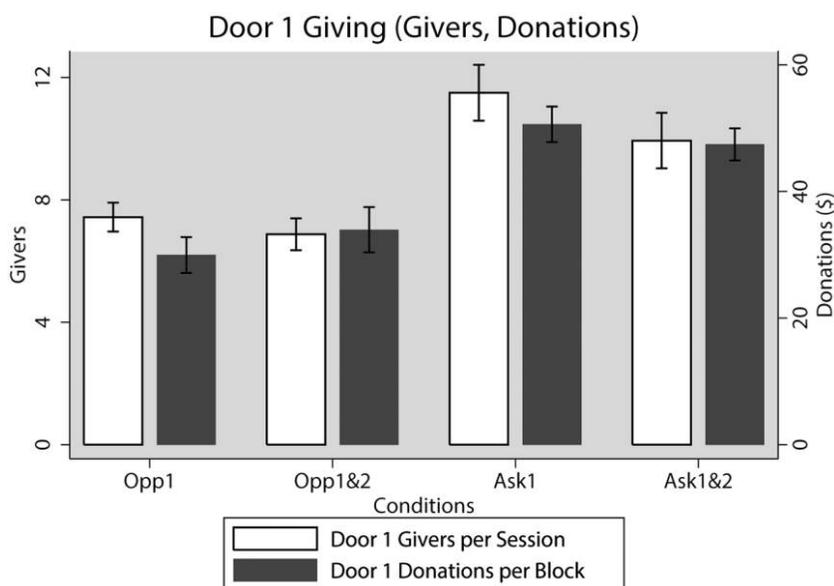


FIG. 3.—Door 1 givers and donations by condition. Bars give 1.96 standard errors.

doors when there is no solicitation (50.6 percent go to door 1).<sup>19</sup> There are three extreme outcomes we could observe if giving is dominated by one of the three giving types. First, if all giving came from passive givers, we would expect givers and donations at door 1 to be the same in the one- and two-door conditions. Second, if all giving came from seeking givers, then we would expect the number of givers at door 1 to double in the one-door conditions (givers that would enter door 2 reroute to door 1). Third, if all donations came from giving avoiders, people who choose to avoid when possible, then we would expect door 1 to raise very little money in one-door conditions.

We start by examining the silent opportunity conditions. Since these conditions produced minimal avoidance, giving cannot be dominated by giving avoiders. At door 1 there are 7.44 givers per 23 minutes in Opp1 and 8.0 givers per 23 minutes in Opp1&2. These estimates are statistically indistinguishable. The implication is that under the opportunity conditions, giving is dominated by passive givers. This is consistent with the evidence in table 2 that adding a second solicitor doubles donations.

We now turn to the ask conditions, which require closer examination since giving avoiders may well constitute a large share of givers. In column 1 of table 6 we see that the number of givers per 23-minute session

<sup>19</sup> The fraction going to each door does depend a bit on the lagged volume of traffic, presumably because of how the parking lot fills up. However, the impact is very small, and the results are robust to using any estimates in the range observed.

TABLE 6  
OLS REGRESSIONS OF DOOR 1 GIVING ON BELL RINGING TREATMENTS

Variable	Givers Door 1 (1)	Donations Door 1 (2)
Opp1&2	-.56 (.91)	3.98 (3.56)
Ask1	4.06*** (.96)	20.63*** (4.52)
Ask1&2	2.50** (1.04)	17.46*** (3.23)
Observations	64	16
$R^2$	.523	.901
Mean of Opp1	7.44	29.97
Date and time fixed effects	Yes	Yes
<i>F</i> -tests, <i>p</i> -values:		
Opp1&2 = Ask1&2	.005	.004
Ask1 = Ask1&2	.132	.464
Opp1&2 = Ask1	.000	.008

NOTE.—In col. 1, standard errors clustered by block are in parentheses. In col. 2, robust standard errors are in parentheses.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

in Ask1 is 1.56 givers higher than in Ask 1&2. The *F*-test below indicates that this difference is not statistically significant ( $p = .13$ ). However, since there are 5.94 more avoiders per 23 minutes than in Ask1 as compared to Ask1&2, this test is not sufficient to conclude that there is no evidence of seeking. Unfortunately, we cannot separately identify seeking givers from giving avoiders. We can, however, give some useful calibrations. If there are no giving avoiders, then the above comparison reveals statistically insignificant evidence of seeking. To raise the *F*-statistic to the .05 significance level, we require that at least 8.87 percent ( $0.527/5.94$ ) of avoiders be giving avoiders.<sup>20</sup> This in turn implies that 11.0 percent of givers are seeking givers. As a final benchmark, if we assume that the fraction of giving avoiders is the same as the global base rate of givers, which is roughly 14 percent in the ask conditions, then the estimate of seeking givers is comparable at 12.6 percent of givers.

We can provide a more specific measure of seeking by returning to our estimates on total giving. When considering door 1 alone, roughly equal proportions of seeking givers and giving avoiders can explain observed giving rates. But recall that in the ask conditions, the second solicitor raised giving by 65 percent and donations by 80 percent, both short of doubling. This rules out both the hypothesis that most giving is generated by seeking givers and the hypothesis that there are no seeking givers whatsoever. This can perhaps most clearly be seen by the observation that

<sup>20</sup> This calculation assumes constant standard errors.

despite the high avoidance and limited overall evidence of seeking, the solicitor at door 1 in Ask1 raised the most money per minute and garnered the highest giving participation. Taken as a whole, seeking givers generate a relatively small fraction of giving, which in turn implies that the fraction of giving avoiders is small.

In summary, we find that the verbal ask raises total giving but also generates substantial avoidance. Seeking is negligible under the opportunity conditions and positive but small under the ask conditions. Evidence of limited seeking in combination with high levels of giving at door 1 under Ask1 suggests that most avoiders would not give if asked, which is consistent with the fact that the verbal ask raises total giving on net. In other words, most people who actively changed the door they entered were avoiding the psychological cost of saying no. In the next section we discuss the psychological motives for giving and avoidance that can reconcile this constellation of findings.

## V. Understanding Avoidance and the “Power of the Ask”

In this section we first explore two potential psychological mechanisms behind the utility of being solicited,  $u$ , from Section III, and the decision to avoid the ask. The first concerns the managing of signals of generosity to oneself or to others. The second is about strategic manipulation of the impulse to give. Next, we present evidence from a recent replication of our experiment that indicates that avoidance responds to cost. Finally, we discuss what our findings imply about the overall welfare impact of this form of fund-raising.

### A. *Self- and Social Signaling*

Can models of social or self-signaling explain avoidance? What if avoiding provides a credible excuse for not sending a signal about one’s generosity? Suppose, for instance, that giving is not determined by altruistic feelings but instead by personal or social goals, such as appearing generous in the eyes of the solicitor or other patrons of the store or maintaining a self-image as a generous person. In the formal model of Andreoni and Bernheim (2009), individuals care about matching social expectations of generosity, although the “higher” types care more than others. In addition, people get utility from others believing they are a high type. In our context, the model predicts that “low” types look for credible excuses to not give. Our treatments could be seen as providing shoppers with credible excuses: that they happened to “naturally” use a door without a solicitor. The easier it is to access a credible excuse, the more likely it is that someone of a lower type will pool with other lower types at giving nothing. By contrast, high types may be willing to give, and extremely high

types, for whom the cost of seeking solicitation is less than the gain in utility from both warm glow and social image, may actually go out of their way to give.

In order for this model to explain our data, we would have to reconcile the lack of avoidance in the opportunity conditions. This is possible only by appealing to a second credible excuse, namely, that people legitimately did not notice the solicitor or felt unobservable when the solicitation lacked a verbal request. One could further argue that the rise in giving under the ask conditions is driven by higher “visibility.” The strength of this explanation relies on the plausibility of not noticing the verbally silent solicitor. In the end, the physical parameters at play render this highly unlikely. The solicitor was a few feet from the store entrance, was wearing a bright red apron, and was loudly ringing a bell. Moreover, the Salvation Army regularly conducted solicitation in this manner in the area and would be known to virtually anyone.

Since self-signaling requires these assumptions and an additional caveat that avoiders are somehow unaware that they are going out of their way to avoid, we conclude that the evidence is not consistent with signaling explanations. A far more likely story is that nongivers in the opportunity conditions noticed the solicitor but did not experience an unanticipated or strong emotional response.

### *B. Avoidance as the Mediation of Altruism*

Psychological models of altruism claim that the act of giving is a struggle between empathy and executive function, that is, between the pull of the heartstrings and the pull of the family budget. Why, for instance, does one avert the gaze of a beggar? A psychologist’s answer would be that eye contact stimulates an empathic response in the brain, either making the altruistic act harder to resist or heightening the guilt associated with not giving. Like the children in Walter Mischel’s famous self-control tasks who successfully avoided eating the marshmallow by physically turning their backs, thus reducing the emotional but not the cognitive awareness of the temptations (Mischel, Shoda, and Rodriguez 1989), it is distinctly possible that our subjects are exhibiting a natural avoidance of an emotional stimulus, which makes it easier to keep their empathy from being engaged.

This pathway to avoidance is also evident in the laboratory experiments of Andreoni and Rao (2011). They asked subjects to play dictator games with controlled degrees of communication. When “receivers” could ask “allocators” for a share of the pie but dictators could not respond, the receivers tended to get what they asked for. By contrast, when dictators could explain what they chose and receivers were kept silent, dictators nearly always gave nothing and offered an apology. However, in a condition designed to heighten empathy, the experiment required all players to make

decisions as recipients (and ask) and as dictators (and explain) but were told that their true roles would be assigned randomly after both decisions were made. Putting oneself in the other's shoes causes the empathy-inducing ask to completely erase the effects of the would-be apologetic explanation. Having thought of what they themselves would ask for, dictators were far more generous, and the messages were more likely to center around fairness. Taken together, this experiment indicates that verbal requests engage empathy but that people take steps to avoid thinking about what others would request if given a chance. When they are forced to consider requests, either explicitly through communication or implicitly by delaying the assignment of roles, giving goes up dramatically.

What if someone with high "empathic vulnerability" were to pass a silent solicitation by a Red Kettle bell ringer? Would he have the strength or willpower to resist if he stopped and chatted with the solicitor about the Salvation Army? Perhaps not. Instead, like Mischel's child subjects, he can turn his gaze, look straight ahead, and walk on by. However, when the solicitor is making a verbal request and attempting eye contact, this strategy of avoidance is defeated by the social norm of acknowledging a request, leaving only three options: pass and give, pass and feel guilty, or use another door.<sup>21</sup> The evidence indicates that some subjects opt for another door while others are induced to give.

### *C. An Extension: The Cost of Avoidance*

Is avoidance in this context sensitive to changing the cost of eliminating contact with the solicitor? In our experiment, people avoid more when there are more escape routes, but the costs of doing so were roughly constant across days and conditions. A recent replication of our protocol (Trachtman et al. 2015) provides both a robustness check of our primary finding and evidence on cost responsiveness. Trachtman et al. ran their experiment at a large supermarket in Anchorage, Alaska. The supermarket had only two doors, and the researchers used two conditions to study sorting: treatment, in which a solicitor was positioned at one of the doors and made a verbal request to shoppers, and control, in which there was no solicitation. Another important difference is that instead of using the well-known Salvation Army, the researchers raised money for a far lesser-known group, the Polycystic Kidney Disease Foundation, by selling awareness buttons for \$1. Finally, solicitors were male instead of female.

<sup>21</sup> Psychologists have also shown that eye contact alone is a powerful stimulus to helping, as initially shown by Ellsworth and Langer (1976). Thus we view the "verbal request" as a combination of both the actual call to action to give and eye contact. Future work could determine if eye contact alone is sufficient to generate an increase in giving. It would be highly unlikely to generate as much avoidance, as it is not as noticeable as verbal requests.

The results of the replication support the findings in this paper in several ways. First and most importantly, the researchers found significant avoidance of the solicitor in the presence of a verbal request. The magnitude of the avoidance was lower; a likely explanation, at least in part, is the lack of an escape route as convenient and well hidden as door 3 in our study. It also may indicate that the degree of avoidance depends on the recognizability of the charity, visibility of the solicitors, or factors such as the gender of the solicitor (as in Landry et al. [2010]). Second, temperature variation provided an instrument for costs. When it was just above freezing—a temperature the locals reportedly consider rather balmy—avoidance was relatively high. However, when it dropped to 0° Fahrenheit—uncomfortably cold by anyone’s standards—avoidance disappeared entirely. These findings lend important insights to the internal cost-benefit decision governing the decision to avoid.

#### *D. Avoidance, Asking, and Welfare*

The significant levels of avoidance in this study and the replication just discussed raise the question of how such fund-raising might affect welfare, since the physical and psychic costs of avoidance appear to be pure deadweight loss, as is the guilt of turning down a request. DellaVigna et al. (2012) do a careful job of estimating the welfare implications of door-to-door solicitation. Their approach is to use a secondary study to estimate the opportunity cost of participants’ time and avoidance and then apply the estimates to a structural model of utility, resulting in a calculation of the welfare consequences of an “opt-out” policy for door-to-door fund-raising. They conclude that first canvassing a neighborhood to notify residents when a solicitor will be present and then offering an opt-out is superior to simply knocking on doors unannounced, both for residents and for the charity.

Our design could not accommodate pricing time and avoidance this precisely; however, we can still construct informative bounds on the welfare impact of this type of solicitation. A detailed description of these derivations is presented in online appendix D. Here we focus on the most intrusive solicitation condition, Ask1&2, and we will draw traffic and donation information from tables 2 and 4. We merge our five types into three broader categories: givers, nongivers, and avoiders.

First, we can estimate the cost of avoidance. One component of this is the time cost of going to the less favored entrance. We first note that to avoid by going to door 3 from door 1 required about 70 feet of extra walking. According to Wikipedia, the average American’s speed of walking is about 4.4 feet per second. Since wages in this area average about \$18.00 per hour, the time cost of avoiding can be roughly estimated to be \$0.08. For a person on the margin between avoiding and passing the so-

licitor (the “marginal nonavoider”), the cost of avoidance (\$0.08) should equal the cost of “saying no.”

If we assume that the “saying no” costs of nonavoiders are uniformly distributed between zero and \$0.08, then the mean cost is \$0.04 for a nonavoider. What about the psychic cost to avoiders? They are saying no but are not facing any social pressure from being asked; thus their psychic cost is lower than that of nonavoiders who say no. To be conservative we set it at \$0.03. Finally, there are the costs of the donations themselves. Conditional on donating, the average donation is \$1.20. Averaging these together by their population weights, the average cost, including donations, to shoppers is \$0.12. However, 40 percent of this cost is from either saying no (25 percent) or avoiding (15 percent).

On the benefits side, we can assume that those who donate get some utility  $y$  from saying yes, from either warm glow or possible self- and social image. Likewise, the recipient of the donation will receive an amount  $\mu$  per dollar donated. Without frictions,  $\mu$  would represent the marginal utility of money for the recipient, relative to the giver. This would be fair to assume to be greater than one. However, if the givers are fairly poor and if the Salvation Army pays some friction costs of getting benefits to the poor, then it could also be that  $\mu < 1$ .<sup>22</sup>

The variables  $y$  and  $\mu$  represent our free parameters. Since a giver chose to give, however, we know that this must yield more utility than passing a solicitor and saying no. Thus  $y - 1.20 \geq -0.04$ , which means that  $y \geq 1.16$ . Notice that this means it is possible for some givers to feel worse off by giving than had the Salvation Army not been at the store. Summing across givers, nongivers, and avoiders, weighted appropriately, we find that fund-raising will be welfare improving if it is also true that  $y \geq 1.91 - 1.2\mu$ .

Consider these benchmarks. Suppose that all donors are indifferent to giving, that is,  $y = 1.16$ . Then fund-raising will be welfare improving only if  $\mu \geq 0.625$ . The value of  $\mu$  could be this low if, for instance, there are large friction costs associated with the transfer. Conversely, another way to say this is that as long as  $\mu > 0.625$ , our fund-raising experiment was surely welfare improving. Suppose instead that the value of saying yes and giving \$1.20 is at least 1.91, on average, for all donors. Then as long as  $\mu > 0$ , fund-raising will increase welfare.

In sum, this exercise shows that the social costs of saying no or of avoiding the ask can be a significant share of the total cost of the donations. In our particular experiment there is no clear answer to whether these fund-raising costs were large enough to make the endeavor welfare diminish-

<sup>22</sup> For instance, we do not explicitly count the opportunity cost of the bell ringers' time or the other costs absorbed by the Salvation Army and the grocery store in coordinating solicitations and disbursements of donations to the needy. All of these costs will be reflected in lower values of  $\mu$ .

ing or whether the benefits of more charity were great enough to swamp the extra costs. This, in our view, remains an open question for all fundraising, and one worth serious further research.

## VI. Discussion and Conclusion

We study how giving and avoidance respond to the presence of Salvation Army bell ringers at the doors of a large supermarket. We find that verbal requests increase the number of givers by 55 percent and total donations by 69 percent. Adding a second solicitor has similarly large impacts on givers and total donations. Shoppers do little to avoid the bell ringers who do not verbally engage or make eye contact with them, but we estimate that the simple act of looking at shoppers and saying “please give today” causes between 25.2 percent and 32.6 percent of would-be entrants to “avoid the ask.” Asking, it seems, is both aversive and effective. There is no significant evidence of seeking in the absence of a verbal ask. However, the fact that adding a solicitor under the ask conditions falls short of doubling total giving suggests that seeking exists, though it cannot explain the bulk of giving. Still, in order to justify limited seeking together with the high levels of giving at door 1 when there is just one verbal solicitor, we infer that most avoiders do not give when they are unable to avoid, meaning they are in fact avoiding saying no to the request. More broadly, we see that every outcome of interest—giving, avoidance, and seeking—is transformed by the verbal request.

Why does asking have such a powerful, but nuanced, impact on behavior? We argue that the underlying psychological mechanism is empathy. Stimulating someone’s empathy through a direct and vocal ask can create an impulse to be generous that is difficult for humans to resist. While our experiment does not test this theory directly, it does guide the discussion of altruism toward the act of asking itself as the linchpin to understanding the costs and benefits of the giving interaction.

We believe that our paper serves a useful methodological purpose as well. Directly asking people to give to charity is a different frame than a donation booth with a “silent” solicitor, which in turn is a different frame than simply posting a sign with instructions as to how to give if one desires to do so. A recent application of our experimental protocol modified the frame to one in which verbal requests were used to sell “awareness buttons” for a little-known kidney disease foundation. Significant avoidance was observed in this frame as well, but less than in the verbal requests from Salvation Army volunteers. Laboratory experiments on the dictator games also provide a frame that allows individuals to “allocate” money to another player, and when players are allowed to make requests from each other, the ask greatly increases donations (Andreoni and Rao 2011), whereas total donations drop when dictators can “opt out” of the experi-

ment entirely (Lazear et al. 2012). Although differing in scope and magnitude, the patterns of results in both the field and lab are quite similar. This gives us some assurance that the frames we create in laboratory experiments do provide informative parallels to real-world giving and are not wholly contaminated by artificial experimenter demand effects.

## References

- Andreoni, J. 1988. "Privately Provided Public Goods in a Large Economy: The Limits of Altruism." *J. Public Econ.* 35:57–73.
- . 1989. "Giving with Impure Altruism: Applications to Charity and Ricardian Equivalence." *J.P.E.* 97:1447–58.
- . 1990. "Impure Altruism and Donations to Public Goods: A Theory of Warm-Glow Giving." *Econ. J.* 100:464–77.
- . 1993. "An Experimental Test of the Public-Goods Crowding-Out Hypothesis." *A.E.R.* 83 (5): 1317–27.
- Andreoni, J., and B. Bernheim. 2009. "Social Image and the 50-50 Norm: A Theoretical and Experimental Analysis of Audience Effects." *Econometrica* 77 (5): 1607–36.
- Andreoni, J., P. Brown, and L. Vesterlund. 2002. "What Makes an Allocation Fair? Some Experimental Evidence." *Games and Econ. Behavior* 40 (1): 1–24.
- Andreoni, J., and J. M. Rao. 2011. "The Power of the Ask: How Communication Affects Selfishness, Empathy and Altruism." *J. Public Econ.* 95:513–20.
- Anik, L., L. B. Aknin, M. I. Norton, and E. W. Dunn. 2011. "Feeling Good about Giving: The Benefits (and Costs) of Self-Interested Charitable Behavior." In *The Science of Giving: Experimental Approaches to the Study of Charity*, edited by D. M. Oppenheimer and C. Y. Olivola. New York: Psychology Press.
- Ariely, D., A. Bracha, and S. Meier. 2009. "Doing Good or Doing Well? Image Motivation and Monetary Incentives in Behaving Prosocially." *A.E.R.* 99 (1): 544–55.
- Batson, C. 1991. *The Altruism Question: Toward a Social Psychological Answer*. Hillsdale, NJ: Erlbaum.
- Becker, G. S. 1974. "A Theory of Social Interactions." *J.P.E.* 82 (6): 1063–93.
- Bernheim, B., and A. Rangel. 2004. "Addiction and Cue-Triggered Decision Processes." *A.E.R.* 94:1558–90.
- Bohnet, I., and B. Frey. 1999. "The Sound of Silence in Prisoner's Dilemma and Dictator Games." *J. Econ. Behavior and Org.* 38 (1): 43–57.
- Boyd, R., and P. Richerson. 1992. "Punishment Allows the Evolution of Cooperation (or Anything Else) in Sizable Groups." *Ethology and Sociobiology* 13 (3): 171–95.
- Broberg, T., T. Ellingsen, and M. Johannesson. 2007. "Is Generosity Involuntary?" *Econ. Letters* 94 (1): 32–37.
- Castillo, M., R. Petrie, and C. Wardell. 2014. "Fundraising through Online Social Networks: A Field Experiment on Peer-to-Peer Solicitation." *J. Public Econ.* 114:29–35.
- Crumpler, H., and P. J. Grossman. 2008. "An Experimental Test of Warm Glow Giving." *J. Public Econ.* 92 (5): 1011–21.
- Dana, J., D. Cain, and R. Dawes. 2006. "What You Don't Know Won't Hurt Me: Costly (but Quiet) Exit in Dictator Games." *Org. Behavior and Human Decision Processes* 100 (2): 193–201.
- DellaVigna, S., J. List, and U. Malmendier. 2012. "Testing for Altruism and Social Pressure in Charitable Giving." *Q.J.E.* 127 (1): 1–56.

- De Vignemont, F., and T. Singer. 2006. "The Empathic Brain: How, When and Why?" *Trends Cognitive Sci.* 10 (10): 435–41.
- Doyen, S., O. Klein, C.-L. Pichon, and A. Cleeremans. 2012. "Behavioral Priming: It's All in the Mind, but Whose Mind." *PLoS ONE* 7 (1): e29081.
- Ellsworth, P. C., and E. Langer. 1976. "Staring and Approach: An Interpretation of the Stare as a Nonspecific Activator." *J. Personality and Soc. Psychology* 33 (1): 117–22.
- Forsythe, R., J. Horowitz, N. Savin, and M. Sefton. 1994. "Fairness in Simple Bargaining Experiments." *Games and Econ. Behavior* 6 (3): 347–69.
- Grice, H. 1957. "Meaning." *Philosophical Rev.* 66:377–88.
- Gronberg, T. J., A. Luccasen, T. L. Turocy, and J. B. Van Huyck. 2012. "Are Tax-Financed Contributions to a Public Good Completely Crowded-Out? Experimental Evidence." *J. Public Econ.* 96 (7–8): 596–603.
- Hamilton, W. 1964. "The Genetical Evolution of Social Behaviour." *J. Theoretical Biology* 7 (1): 1–16.
- Hare, T., C. Camerer, D. Knoepfle, J. O'Doherty, and A. Rangel. 2010. "Vale Computations in Ventral Medial Prefrontal Cortex during Charitable Decision Making Incorporate Input from Regions Involved in Social Cognition." *J. Neuroscience* 30 (2): 583–90.
- Hoffman, E., K. McCabe, and V. L. Smith. 1996. "Social Distance and Other Regarding Behavior in Dictator Games." *A.E.R.* 86 (3): 653–60.
- Laibson, D. 1997. "Golden Eggs and Hyperbolic Discounting." *Q.J.E.* 112 (2): 443–78.
- Landry, C. E., A. Lange, J. A. List, M. K. Price, and N. G. Rupp. 2010. "Is a Donor in Hand Better than Two in the Bush? Evidence from a Natural Field Experiment." *A.E.R.* 100 (3): 958–83.
- Lazear, E. P., U. Malmendier, and R. A. Weber. 2012. "Sorting in Experiments with Application to Social Preferences." *American Econ. J.: Appl. Econ.* 4 (1): 136–63.
- Maniadis, Z., F. Tufano, and J. A. List. 2014. "One Swallow Doesn't Make a Summer: New Evidence on Anchoring Effects." *A.E.R.* 104 (1): 277–90.
- Mischel, W., Y. Shoda, and M. Rodriguez. 1989. "Delay of Gratification in Children." *Science* 244 (4907): 933–38.
- O'Donoghue, T., and M. Rabin. 1999. "Doing It Now or Later." *A.E.R.* 89 (1): 103–24.
- Open Science Collaboration. 2012. "An Open, Large-Scale, Collaborative Effort to Estimate the Reproducibility of Psychological Science." *Perspectives Psychological Sci.* 7 (6): 657–60.
- Ozdenoren, E., S. W. Salant, and D. Silverman. 2012. "Willpower and the Optimal Control of Visceral Urges." *J. European Econ. Assoc.* 10 (2): 342–68.
- Preston, S. D., and F. B. M. deWaal. 2002. "Empathy: Its Ultimate and Proximate Bases." *Behavior and Brain Sci.* 25:1–72.
- Roth, A. 1995. "Bargaining Experiments." In *Handbook of Experimental Economics*, edited by J. H. Kagel and A. Roth. Princeton, NJ: Princeton Univ. Press.
- Small, D. A., and G. Loewenstein. 2003. "Helping a Victim or Helping the Victim: Altruism and Identifiability." *J. Risk and Uncertainty* 26:5–16.
- Strotz, R. H. 1956. "Myopia and Inconsistency in Dynamic Utility Maximization." *Rev. Econ. Studies* 23 (3): 165–80.
- Trachtman, H., A. Steinkruger, M. Wood, et al. 2015. "Fair Weather Avoidance: Unpacking the Costs and Benefits of 'Avoiding the Ask.'" *J. Econ. Sci. Assoc.* 1:8–14.
- Trivers, R. 1971. "The Evolution of Reciprocal Altruism." *Q. Rev. Biology* 46 (1): 35–57.
- Xiao, E., and D. Houser. 2005. "Emotion Expression in Human Punishment Behavior." *Proc. Nat. Acad. Sci.* 102 (20): 7398–7401.