

Avoiding The Ask:  
A Field Experiment on Altruism, Empathy,  
and Charitable Giving\*

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May 1, 2015

**Abstract**

If people get joy from giving, then why might they avoid fundraisers? We explore this in a randomized natural field experiment during the Salvation Army’s annual campaign. The familiar bell-ringers were placed at one or both of two main entrances to a supermarket, making the ask for a charitable donation either easy or difficult to avoid. Additionally, solicitors either simply rang the bell, or asked “please give” to passersby. Verbally asking dramatically increases the number of givers and the amount of giving, as does having solicitors at both main entrances. However, we also found dramatic avoidance of verbal solicitation, between 26.2% and 32.6%, but negligible avoidance of non-verbal solicitation. Asking has a powerful effect on both giving when asked, and on avoidance. We argue that this pattern likely illustrates givers’ sophisticated awareness of the empathy-altruism link, rather than pernicious social costs of fund-raising.

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\*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 experiment. 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, Stephanie Preston, David Reiley, Alison Sanchez, Michael Schwarz, Joan Silk, and Lise Vesterlund for helpful comments and the NSF and Harvard College for financial support. We especially thank the Salvation Army for cooperation in running the experiment. This research was conducted under full IRB approval.

# 1 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 others, 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 et al., 2002). The implication is that utility depends on the *act* of giving; that there is some warm-glow from helping others (Andreoni, 1989; Andreoni, 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 et al., 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 et al., 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 opportunity 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 et al., 2006; Lazear et al., 2012; Broberg et al., 2007). 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.

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<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 Luccasen et al. (2012)

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 those that 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 like the smell of freshly baked bread can pique appetite. Resolving this feeling comes either by giving and feeling good, or not giving and feeling guilt. However, someone with (implicit or explicit) knowledge of their vulnerability to such stimuli can, by controlling the input of that stimuli, 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 of or both of two main entrances to a grocery store in suburban Boston over four 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 solicitor(s) were silent, there was no discernible impact on

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<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 as used by O’Donoghue and Rabin (1999), Laibson (1997), and Strotz (1956).

store traffic—few people avoided the solicitor(s), 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% to 69% 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% to 80%—indicating that some givers sought out the solicitor when only one was present.

In general, giving appeared 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 fundraiser. Social- or self-image concerns are also unable to explain the power of asking in our setting—positive 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 fundraising have a negative impact on welfare? In our setting, requests generated a positive net impact on donations because the increase in giving more than offset the increased avoidance. More generally 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

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<sup>5</sup>This explanation has much in common with theory of cue-triggered choices of Bernheim and Rangel (2004) and the will power depletion model of Ozdenoren et al. (2011).

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

of “saying no.” We use our data to determine the range of parameters for which fundraising is a net welfare enhancement in our setting. This range includes most of the parameter combinations that we consider reasonable based on intuition and prior studies.

In related work, DellaVigna, List, and Malmendier (2012) use a design similar to ours (though conceived independently) with the chief aim of estimating this welfare impact. 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. The authors found that many people opted out of being solicited and the average gift was higher for those who actively consented to solicitation. We view the our study as complementary to theirs. They attempted to measure time costs, and to define “social pressure” costs in order to estimate the welfare impact of the opting-out manipulation in their door-to-door fund-raising campaign. Our study is aimed more squarely at understanding the motives behind giving.<sup>7</sup>

The paper proceeds as follows. Section 2 presents the design of the field experiment. Section 3 presents the results. Section 4 offers a discussion, and Section 5 concludes.

## 2 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 [www.ringbells.org](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; 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 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>8</sup>

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<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. The increase in giving might be due to the fact that givers make an effort to be available when they know the solicitor is coming, or the fact that people have more time to deliberate and prepare, as indicated by Landry et al. 2010. In evaluating an opt-out policy, it does not *per se* matter 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>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.



Figure 1: The Store Studied. Doors 1 and 2 were the main entrances, while door 3 was the side “recycling” door.

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, receptively. In the Opportunity conditions solicitors rang the bell as usual, but did not speak nor attempt eye-contact, except to thank those that 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>9</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

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<sup>9</sup>Taxi drivers and store employees were not counted. They enter and exit the store many times during the day but are not shoppers.

Table 1: Experiment Schedule: December 7th to 10th, 2009

	Monday	Tuesday	Wednesday	Thursday
	12/7	12/8	12/9	12/10
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

each block, when the solicitor appeared to be taking a break, in order to minimize any unnatural behavior. This means that donations are only observed at the block level.<sup>10</sup>

Conditions were assigned to blocks according to the Latin square configuration shown in Table 1. The configuration ensures blocks were balanced across days and time-of-day. Daily balance helps ensure that factors such as weather, day-of-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 Monday evening at 4pm was a particularly busy time, and that a particular door is favored during busy periods. Although we guarded against this confound by choosing non-Friday weekdays, it cannot directly test for it with our original data. As such, we later returned to the store and collected data on customer traffic for four days without any solicitation, which we refer to as the placebo data.<sup>11</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 visa 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>12</sup>

Unfortunately, we did not count traffic through Door 3. The reason is simple: in our initial selection of the story 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 only became known to us after the data

<sup>10</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.

<sup>11</sup>The placebo data was collected in July of 2013.

<sup>12</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 belief that there is no solicitor there, which would bias the estimate of sorting downward.

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 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 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 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 0.97 and 0.44 for total traffic and residuals respectively), indicating our test statistics rest on empirically valid assumptions.

In the remainder of the paper, we will use “bell-ringing conditions” to refer the conditions where a solicitor was actually present and “placebo conditions” to refer to the same day-time 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.

### 3 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  $u_0$  the utility from avoiding one a solicitor and giving 0. 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  $g^*$ , which could be 0. 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  $g^* = 0$  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 0, 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-non-givers* ( $u_s > u_0 - c$  and  $g_s = 0$ ), that is, people who use their endowed entrance but don’t 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 now 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. Based on 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 backwards, if any of these three

effects lower 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.

## 4 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.

### 4.1 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 0.55 givers 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 givers per minute in 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 three-fold difference across conditions, \$0.33 to \$0.99 per minute. This is our first evidence that relatively small changes in the fundraising 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 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% and total donations by 69%. The power of asking was also observed in the two-door conditions, increasing givers by 53% (column 1) and donations by 45% (column 2). All of the above differences are significant beyond the 0.01 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, giving comes from three types: seeking-givers, giving-avoiders, and passive-givers. The first two types change their preferred door based on the level of solicitation, and thus giving depends on sorting (or the inability to do so). Differential giving rates across conditions identifies the relative importance of givers that sort. In the Opportunity conditions the second solicitor increased the number of givers by 67% (5 givers per session) and dollars donated

Table 2: OLS Regressions of Total Giving in Bell-Ringing Treatments

VARIABLES	(1) Number of Givers per 23-minute Session	(2) Dollars of Donations per 92-minute Block
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-squared	0.726	0.977
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%
<i>F</i> -tests, <i>p</i> values:		
Opp1&2 = Ask1&2	0.000	0.000
Ask1 = Ask1&2	0.000	0.000
Opp1&2 = Ask1	0.395	0.008

Column 1: standard errors clustered by block in parentheses

Column 2: robust standard errors in parentheses

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$

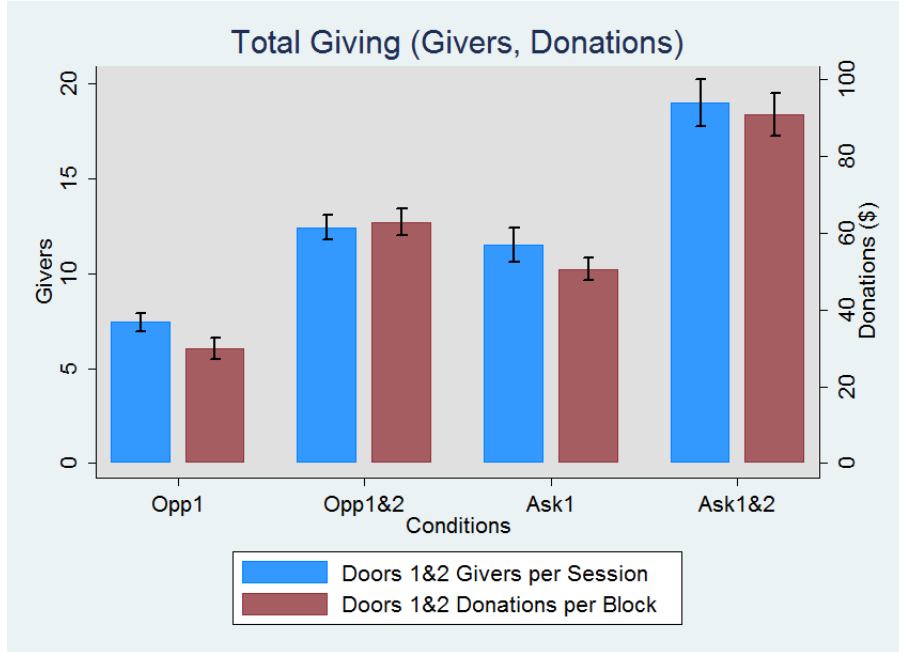


Figure 2: Total givers and donations by condition. Bars give 1.96 standard errors.

by 110% (\$32.86 per block). In the Ask conditions, the second solicitor raised giving by 65% (7.5 givers per session) and donations by 80% (\$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 of 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.

## 4.2 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 1 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 the second panel of Table 3 we impute avoidance to Door 3 under the 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% of total passings, whereas the adjustment to both Ask conditions is much more substantial, at 17%.

Table 3: Gross Traffic in Bell-Ringing Conditions and Placebo Conditions

	Silent Opportunity		Direct Ask		
	Door 1 (Opp1)	Doors 1 & 2 (Opp1&2)	Door 1 (Ask1)	Doors 1 & 2 (Ask1&2)	Total
Panel 1. Actual Bell-Ringing Traffic					
Door 1	2563	2508	1728	1918	8717
Door 2	2284	2174	2321	2166	8945
Total Doors 1 and 2	4847	4682	4049	4084	17662
Panel 2. Imputed Door 3 Bell-Ringing Traffic, Opp1 Total as Baseline					
Imputed Total All Doors	4847	4847	4847	4847	19388
Imputed Door 3 Increase	0	165	798	763	1726
Panel 3. Actual Placebo Traffic					
Door 1	2223	2092	2242	2249	8806
Door 2	2119	2224	2088	2194	8625
Door 3	871	901	867	853	3492
Total Doors 1 and 2	4342	4316	4330	4443	17431
Total All Doors	5213	5217	5197	5296	20923
Panel 4. Imputed Door 3 Placebo Traffic, Opp1 Total as Baseline					
Imputed Total All Doors	4342	4342	4342	4342	17368
Imputed Door 3 Increase	0	26	12	-101	-63

Note: Panel 1 reports observed traffic under the bell-ringing conditions (December 2009). Panel 2 imputes traffic in Door 3 as the deviation in Doors 1 and 2 traffic from that of Opp1 (4847). Panel 3 reports observed traffic under the placebo conditions; i.e. without solicitation (July-August 2013). Panel 4 imputes traffic in Door 3 in the same way as Panel 2, using the placebo data.

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 a t-test in an OLS framework, reveals this difference is significant beyond the 0.01 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 (Mon–Thurs). 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 day-time slot. Moreover, with only 16 treatment blocks, traditional test statistics require a normality assumption.<sup>13</sup>

We use the placebo data to validate both of these assumptions. In Panel 3 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% of traffic. In Panel 4 we conduct the same exercise as Panel 2 with the placebo data, that is, supposing 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 1726 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 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 0.97 and 0.44 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 is less than 1 in 10,000. In Figure A5 of the 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 in both Ask conditions. The increase is 16.5% for Ask1 and 15.7% for Ask1&2. These coefficients are statistically significant beyond the 0.01 level, but are not statistically distinguishable from each other. This indicates that avoidance to Door 3 was not influenced

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<sup>13</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&amp;2 Traffic on Bell-Ringing and Placebo Conditions

VARIABLES	(1)	(2)
	Doors 1&2 Traffic under Bell-Ringing Conditions	Doors 1&2 Traffic under Bell-Ringing and Placebo Conditions <sup>†</sup>
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-squared	0.760	0.807
Mean of omitted group	302.93	299.59
Date and Time fixed effects	yes	no
Date $\times$ Time fixed effects	no	yes
<i>F</i> -tests, <i>p</i> values:		
Opp1&2 = Ask1&2	0.006	0.096
Ask1 = Ask1&2	0.880	0.842
Opp1&2 = Ask1	0.007	0.219
Opp1 = Opp1&2		0.820
Opp1 = Ask1		0.217

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

Standard errors clustered by block in parentheses

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$

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 4800 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 4.1 rules out that the higher traffic in the bell-ringing conditions is due to seeking of the solicitor.<sup>14</sup> Given these differences, we need to inflate total traffic under the placebo sessions to make it comparable to the bell-ringing sessions, otherwise all the bell-ringing condition coefficients will be biased upwards. 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>15</sup> The three assumptions generate multipliers of 1.1163, 1.1006, and 1.0848 respectively (see Appendix Table A1 for more details).<sup>16</sup> We interpret this as consistent evidence that there were about 10% 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. This is because we used the Opportunity conditions to calculate the multiplier, meaning are unable to speak to baseline avoidance in Opp1, but report the estimate for completeness, noting that it is constrained to be close to 0 by construction. In Tables A2 and A3 in the Appendix, we show that the main results are robust to using any plausible multiplier. Overall, the direct inclusion of time-of-day cross 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% in Ask1 and by 25.2% 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 due to avoidance to Door 3, so the coefficients will be biased towards zero.<sup>17</sup> Consistent with this logic, the avoidance estimates are lower than in column (1),

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<sup>14</sup>We are assuming that people only seek in order to give. That is, they gain no pleasure from saying no.

<sup>15</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>16</sup>For example,  $1.1006 = (4847+4682)/(4342+4316)$

<sup>17</sup>To see this consider the following example. Suppose in the baseline 50 people use Door 1 and 50 people use Door 2. Door 1 share is 50%. In the treatment, 40 use Door 1, 50 use Door 2 and 10 use Door 3. The true share of traffic



Table 5: OLS Regressions of Traffic Distribution on Bell-Ringing Conditions

VARIABLES	(1) Door 1 Traffic	(2) Door 2 Traffic	(3) Door 1 Traffic as Fraction of Doors 1&2 Traffic
Opp1&2	-3.44 (10.34)	-6.88 (7.958)	0.0046 (0.0268)
Ask1	-52.19*** (11.96)	2.318 (7.61)	-0.111*** (0.027)
Ask1&2	-40.31*** (9.757)	-7.375 (10.07)	-0.067** (0.025)
Observations	64	64	64
R-squared	0.664	0.802	0.689
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	0.865	0.865	0.008
Ask1 = Ask1&2	0.410	0.410	0.085
Opp1&2 = Ask1	0.335	0.335	0.000

Standard errors clustered by block in parentheses

\*\*\**p*<0.01, \*\**p*<0.05, \**p*<0.1

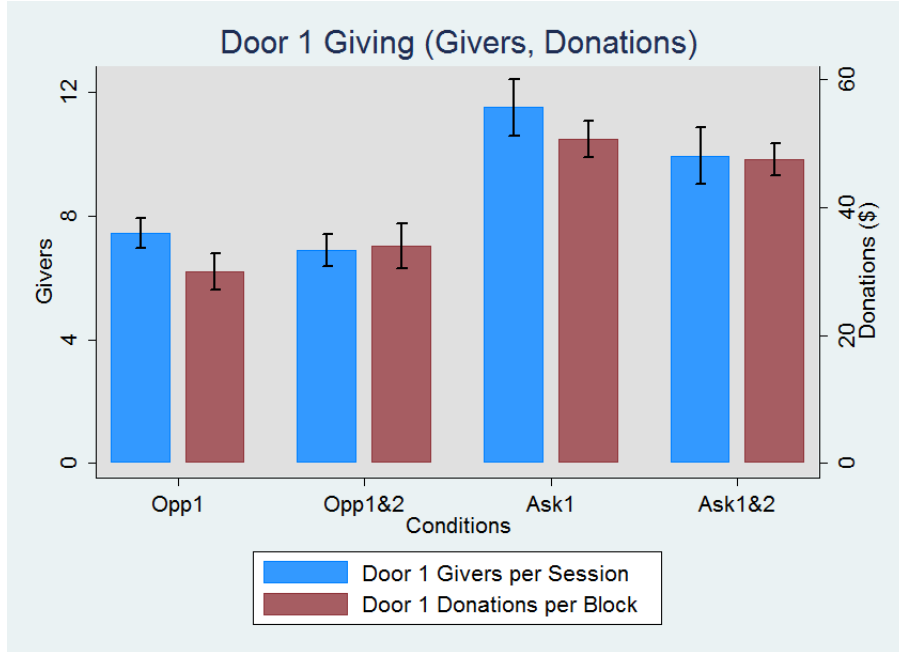


Figure 3: Door 1 givers and donations by condition. Bars give 1.96 standard errors.

but highly significant nonetheless, at 20.7% and 12.5% under Ask1 and Ask1&2, respectively.

### 4.3 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 indicates that traffic is nearly equally split across doors when there is no solicitation (50.6% go to Door 1).<sup>18</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-

through Door 1 is 40%, suggesting avoidance of 10 percentage points, but calculating just using Doors 1 and 2, we get 44%, or avoidance of only 6 percentage points.

<sup>18</sup>The fraction going to each door does depend a bit on the lagged volume of traffic, presumably due to 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

VARIABLES	(1)	(2)
	Givers Door 1	Donations Door 1
Opp1&2	-0.56 (0.91)	3.98 (3.56)
Ask1	4.06*** (0.96)	20.63*** (4.52)
Ask1&2	2.50** (1.04)	17.46*** (3.23)
Observations	64	16
R-squared	0.523	0.901
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	0.005	0.004
Ask1 = Ask1&2	0.132	0.464
Opp1&2 = Ask1	0.000	0.008

Column 1: standard errors clustered by block in parentheses

Column 2: robust standard errors in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

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 re-route to Door 1). Third, if all donations came from giving-avoiders, people that 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 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 in Ask1 is 1.56 givers higher than in Ask 1&2. The F-test below indicates that this difference is not statistically significant ( $p = 0.13$ ). However, since there are 5.94 more avoiders per 23-minutes than in Ask1 as compared to Ask 1&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 0.05 significance level, we require that at least 8.87% ( $0.527/5.94$ ) of avoiders be giving-avoiders.<sup>19</sup> This in turn implies that 11.0% 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% in the Ask conditions, then the estimate of seeking-givers is comparable at 12.6% 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% and donations by 80%, 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 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.

## 5 Understanding Avoidance and the “Power of the Ask”

In this section we first explore two potential psychological mechanisms behind the utility of being solicited,  $u_s$  from Section 3, 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 avoidance responds to cost. Finally, we discuss what our findings imply about the overall welfare impact of this form of fundraising.

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<sup>19</sup>This calculation assumes constant standard errors.

## 5.1 Self- and Social-Signalling

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 a credible excuse: 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 only possible 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 non-givers in the Opportunity conditions noticed the solicitor, but did not experience an unanticipated or strong emotional response.

## 5.2 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 et al., 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 they have the strength or willpower to resist if they stopped and chatted with the solicitor about the Salvation Army? Perhaps not. Instead, like Michel’s child subjects, they can turn their 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>20</sup> The evidence indicates that some subjects opt for another door while others are induced to give.

### 5.3 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. (In Press), 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, AK. 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

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<sup>20</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.

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.

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.

#### 5.4 Avoidance, Asking and Welfare

The significant levels of avoidance in this study and the replication just discussed raises the question of how such fundraising 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 an 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 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. We will focus on the most effective (and intrusive) solicitation condition, (Ask1&2), which raised about \$0.15 per shopper.<sup>21</sup> To construct the lower bound, we assume that “saying no” and giving both come at a lower cost than avoidance. We note that this does not follow from revealed preference, since these are potentially different types of people (avoiders may have a higher marginal utility of income, for instance). We next assume that we can price avoidance through time cost alone. Note that the average American’s speed of walking is about 4.4 feet per second,<sup>22</sup> and avoiding to Door 3 from Door 1 required about 70 feet of extra walking. Since average wages for this area of town are \$18 per hour, the time-cost of avoidance per person can be roughly estimated to be \$0.08. For a person

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<sup>21</sup>In line with our analysis, we use the number of shoppers under Opp1 as the denominator ( $0.15=22.73/302.93$ )

<sup>22</sup>This figure is from Wikipedia.

on the margin between avoiding and passing the solicitor (the “marginal non-avoider”), the cost of avoidance (\$0.08) should equal the cost of “saying no.”<sup>23</sup> If we assume that the “saying no” costs of non-avoiders are uniformly distributed between 0 and \$0.08, then the mean cost is \$0.04 for a non-avoider and \$0.08 for an avoider. Finally, we assume no utility of giving.<sup>24</sup> By multiplying each cost estimate by the fraction of the population occupying each group (recall that avoidance of Door 1 was 25.2% under Ask1&2), we get a lower bound of \$0.05 for Ask 1&2. If we only consider time costs, the bound is \$0.02. Both figures are lower than the per person funds raised.

What is the upper bound for solicitation costs? It’s reasonable to assert, due to revealed preference, that the avoidance cost is less than the mean donation among those who were asked to give and gave, \$1.20 under A1&2. Through the same reasoning, it’s also defensible to assume that the cost of “saying no” is less than \$1.20 for the non-giving entrants.<sup>25</sup> This value represents the maximum loss for the avoider and “saying no” groups—it corresponds to the loss of a person on the margin of giving and refusing, or on the margin of giving and avoiding. If we again assume that these costs are uniformly distributed, we get an average loss for both groups of \$0.75. If we ignore the welfare impact on givers, then the upper bound is calculated as \$0.71. This represents the maximum average loss to customers assuming no warm-glow or private benefit from giving, and no social benefit from signaling.

We have identified \$0.05 and \$0.71 as approximate lower and upper bounds for the solicitation cost imposed on customers. Let’s normalize the marginal utility of money to be 1 for donors. If the marginal utility of money to recipients is less than 0.33 (that is,  $0.05/0.15$ ), then fundraising is inefficient. Since the Salvation Army helps the impoverished, this sufficient condition is unlikely to be met. On the other extreme, if money is worth at least 4.7 (that is,  $0.71/0.15$ ) times more to recipients at the margin, then fund-raising is welfare enhancing. As is clear from this exercise, making precise welfare statements is quite delicate—our bounds are sensitive to a number of assumptions. However, our methodology lets one take a stand on the issue with simple assumptions on the ratio of marginal utility between recipients and donors, and outlines which costs should count in welfare calculations. For instance, if one takes the stance that guilt and other psychological costs should not enter welfare calculations, but time costs of avoidance should count, then the Ask and Opportunity treatments were both welfare positive and in fact similarly efficient, since the increase in donations under the Ask conditions offsets the utility loss due to avoidance. If “refusal guilt” was assumed to be present only with verbal requests, then, provided this utility impact was counted, the Opportunity conditions would be viewed as the efficient solution. We do not take an explicit stance on what should count, but note that our bounds include cases where fund-raising of

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<sup>23</sup>Using only the time cost of walking is problematic because presumably some “guilt cost” motivates avoidance in the first place. However, it is unclear if we should include guilt cost in welfare considerations (see Andreoni (2006) for a review of the difficulties of determining welfare consequences in the presence of warm glow and guilt).

<sup>24</sup>The marginal giver suffers a utility loss, by construction, but seekers might have utility gains.

<sup>25</sup>It is unclear whether the cost of saying no is less than the avoidance cost because of selection. Grossman (2010) presents a nice discussion of how selfish self-deception relies on decision awareness.



this sort is socially efficient and cases where it is possibly not.

## 6 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% and total donations by 69%. 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% and 32.6% 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 experiment 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.

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## Appendix

To accompany J. Andreoni, J. Rao, and H. Trachtman,  
“Avoiding The Ask: A Field Experiment  
on Altruism, Empathy, and Charitable Giving”  
for online publication only.

### A Verifying Normality of Placebo Traffic and Residuals

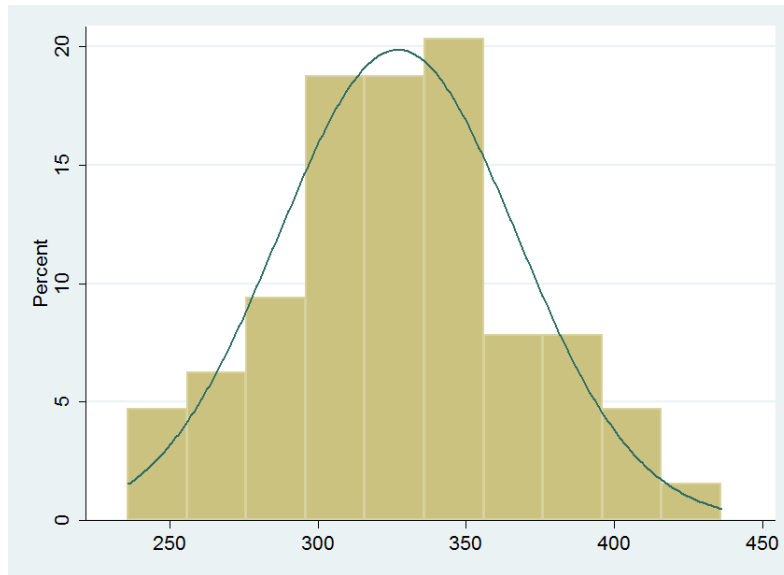


Figure A1: Total Placebo Traffic per 23-Minute Session (Doors 1, 2, and 3)

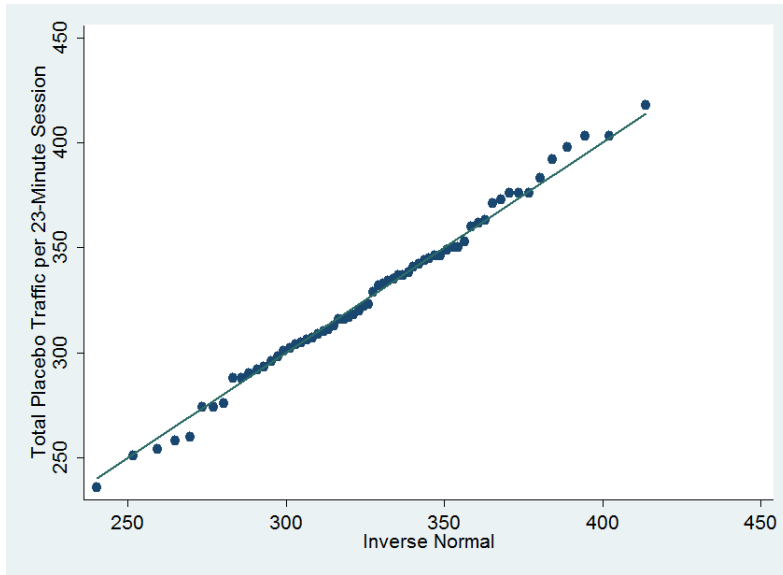


Figure A2: Quantiles of Total Placebo Traffic per 23-Minute Session against Quantiles of Normal Distribution

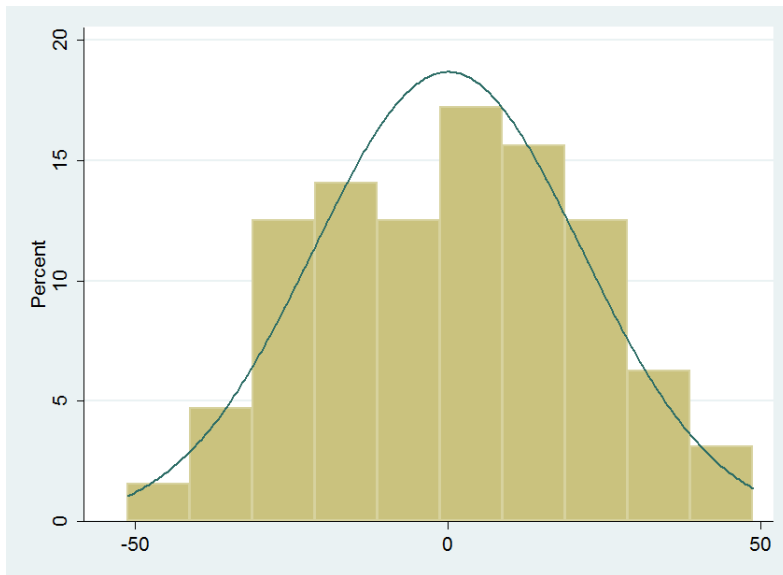


Figure A3: Residuals from Regression of Total Placebo Traffic (Doors 1, 2, and 3) on Day and Time Dummies

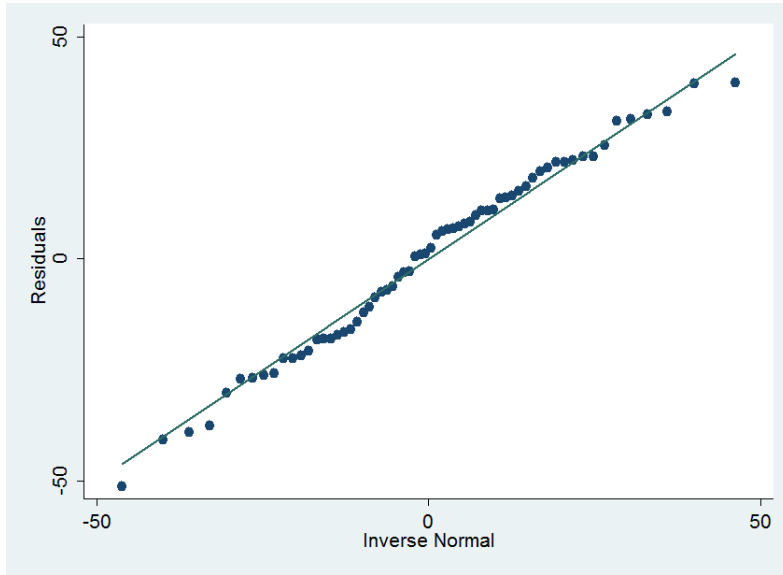


Figure A4: Quantiles of Residuals from Regression of Total Placebo Traffic (Doors 1, 2, and 3) on Day and Time Dummies against Quantiles of Normal Distribution

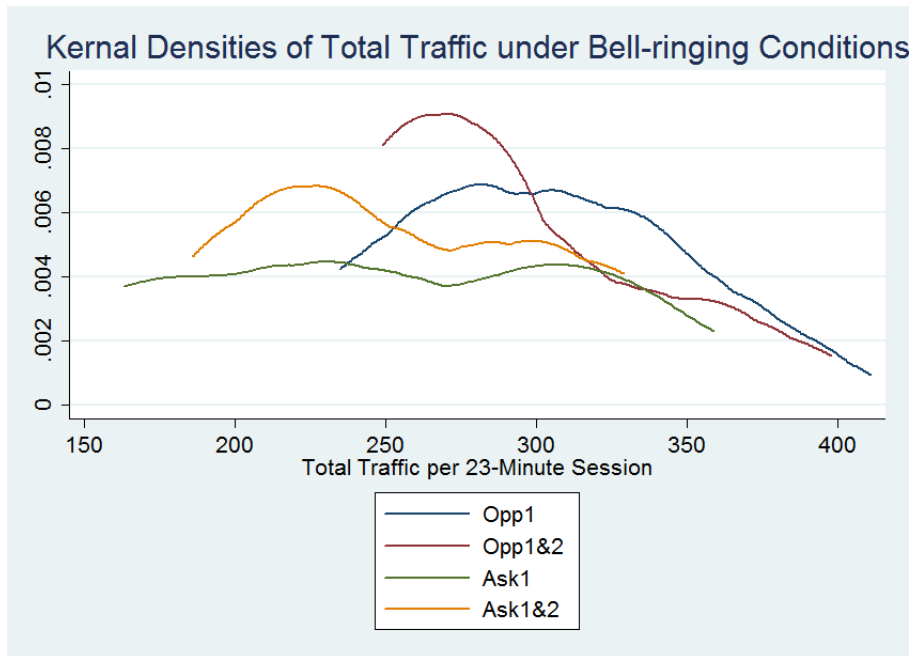


Figure A5: Kernel Density Estimates of Total Bell-ringing Traffic (Doors 1 and 2)



## B Computation of the Traffic Multiplier ( $\theta$ )

Table A1: Imputing Displacement Effects of Asking Treatments Using Placebo Traffic

	Silent Opportunity		Direct Ask	
	Door 1	Doors 1 & 2	Door 1	Doors 1 & 2
	(Opp1)	(Opp1&2)	(Ask1)	(Ask1&2)
<b>Traffic in Asking Conditions as a Proportion of Traffic in Placebo</b>				
Door 1	1.1529	1.1989	0.7707	0.8528
Door 2	1.0779	0.9775	1.1116	0.9872
Total Doors 1&2	1.1163	1.0848	0.9351	0.9192
<b>Imputing Percent Displacement of Traffic Under Four Assumptions</b>				
<i>a. <math>\theta = 1</math> (assume traffic is the same in Bell-Ringing and Placebo)</i>				
Door 1	0.065	0.080	-0.099	-0.063
Door 2	0.032	-0.010	0.045	-0.005
Door 3	-0.097	-0.070	0.054	0.068
<i>b. <math>\theta = 1.1163</math> (assume displacement is 0 in Opp1, Doors 1 and 2)</i>				
Door 1	0.016	0.034	-0.153	-0.115
Door 2	-0.016	-0.061	-0.002	-0.055
Door 3	0.000	0.027	0.154	0.169
<i>c. <math>\theta = 1.1006</math> (assume displacement is 0 in Opp1 and Opp1&amp;2, Doors 1 and 2)</i>				
Door 1	0.021	0.036	-0.131	-0.097
Door 2	-0.009	-0.048	0.004	-0.043
Door 3	-0.012	0.012	0.127	0.138
<i>d. <math>\theta = 1.0848</math> (assume displacement is 0 in Opp1, Door 2)</i>				
Door 1	0.029	0.044	-0.120	-0.086
Door 2	0.000	-0.039	0.012	-0.034
Door 3	-0.029	-0.005	0.108	0.121

## C Traffic Regressions with Placebo Data and Several Multipliers

Table A2: OLS Regressions of Doors 1&2 Traffic on Bell-Ringing and Placebo Conditions<sup>†</sup>

VARIABLES	(1) $\theta = 1$	(2) $\theta = 1.05$	(3) $\theta = 1.1$	(4) $\theta = 1.15$
Opp1	31.56 (29.64)	17.99 (29.85)	4.425 (30.08)	-9.144 (30.33)
Opp1&2	22.88 (20.66)	9.387 (20.96)	-4.100 (21.33)	-17.59 (21.77)
Ask1	-17.56 (24.72)	-31.09 (23.97)	-44.62* (23.26)	-58.16** (22.58)
Ask1&2	-22.44 (14.97)	-36.32** (14.81)	-50.21*** (14.77)	-64.09*** (14.86)
Observations	128	128	128	128
R-squared	0.791	0.795	0.807	0.822
Mean of Placebo	272.36	285.98	299.60	313.21
Date $\times$ Time fixed effects	yes	yes	yes	yes
<i>F</i> -tests, <i>p</i> values:				
Opp1&2 = Ask1&2	0.0961	0.0952	0.0959	0.0981
Ask1 = Ask1&2	0.868	0.855	0.842	0.829
Opp1&2 = Ask1	0.229	0.223	0.219	0.215
Opp1 = Opp1&2	0.813	0.817	0.820	0.824
Opp1 = Ask1	0.222	0.219	0.217	0.215

<sup>†</sup>Bell-ringing traffic if 2009,  $\theta \times$ (placebo traffic) if 2013, with  $\theta = 1.1$ .

Standard errors clustered by block in parentheses

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$

Table A3: Door 1 Traffic as Fraction of  $\theta \times$  Mean Doors 1&2 Traffic under Placebo<sup>†</sup>

VARIABLES	(1) $\theta = 1$	(2) $\theta = 1.05$	(3) $\theta = 1.1$	(4) $\theta = 1.15$
Opp1	0.0869 (0.0559)	0.0583 (0.0534)	0.0324 (0.0512)	0.00871 (0.0492)
Opp1&2	0.111 (0.0659)	0.0831 (0.0629)	0.0572 (0.0602)	0.0336 (0.0577)
Ask1	-0.120* (0.0575)	-0.139** (0.0550)	-0.156*** (0.0528)	-0.172*** (0.0507)
Ask1&2	-0.0736 (0.0436)	-0.0942** (0.0413)	-0.113** (0.0392)	-0.130*** (0.0373)
Constant	0.540*** (0.0218)	0.538*** (0.0207)	0.537*** (0.0196)	0.535*** (0.0186)
Observations	128	128	128	128
R-squared	0.689	0.691	0.701	0.718
Mean of Placebo	.506	.506	.506	.506
Date $\times$ Time fixed effects	yes	yes	yes	yes
<i>F</i> -tests, <i>p</i> values:				
Opp1&2 = Ask1&2	0.0335	0.0325	0.0316	0.0308
Ask1 = Ask1&2	0.532	0.527	0.523	0.518
Opp1&2 = Ask1	0.0184	0.0181	0.0177	0.0174
Opp1 = Opp1&2	0.780	0.769	0.758	0.747
Opp1 = Ask1	0.0211	0.0214	0.0217	0.0220

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

Standard errors clustered by block in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$