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

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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 giving, as does having solicitors at both main entrances. However, we also found dramatic avoidance of verbal solicitation,32.6%, but negligible avoidance of non-verbal solicitation. Asking has a powerful effect on both giving when asked, and on avoiding being asked. We argue that this pattern likely illustrates givers' sophisticated awareness of the empathy-altruism link, rather than pernicious social costs of fund-raising.

*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 to 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.[1] 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).[2] 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), 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 a fair or moral people. Second, giving is greater and more likely when subjects communicate (Xiao and Houser, 2005; Andreoni and Rao, 2011), and when the ability of recipients to detect unselfish acts becomes easier(Andreoni and Bernheim, 2009; Ariely et al., 2009). This indicates that warm-glow increases with the vividness of the giver in the mind of the *recipient*. This is consistent with the notion that maintaining a positive *social*-image 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" chose 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 increases 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.

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

[2] 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 mental 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.[3]

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.[4] 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. We partnered with the globally renowned Salvation Army to conduct an experiment during their 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 this: Is avoiding the ask an indication that altruistic people are (consciously or subconsciously) attempting to control their empathic emotions? We hope to inform this hypothesis in two ways. First, when we strengthen the social interaction by verbally asking for money, does this extra appeal to empathy increase the frequency and amount of giving? Second, when we create the opportunity to select solicitation, how many givers seek the ask, how many avoid it, and are avoiders avoiding giving or simply avoiding being asked?

We find that both of our manipulations had interesting effects. When the solicitors were silent, there was no discernible impact on store traffic–few people avoided then, and fewer still sought them

[3]This evidence is also consistent with more recent evidence on happiness and giving. See (Anik et al., 2011).

[4]We use the word sophisticated in the sense as used by O'Donoghue and Rabin (1999), Laibson (1997), and Strotz (1956).

out. Putting solicitors at both doors, rather than one, nearly doubled the total number of givers and amount given. However, when the bell-ringers also asked "please give today," the impacts were dramatic. When they were at both doors, those who passed were far more likely to give, and donations went up significantly. However, when only one door had a solicitor, and that bell-ringer was verbally asking for a donation, we found that nearly a third of those intending to pass through that door instead chose to use an entrance without a solicitor. That is, they avoided the explicit verbal ask, but not the ask implicit in ringing the bell.

This suggests that it is the emotions surrounding the interactions, and thus the economic outcome, that is being controlled by avoidance. This interpretation is reinforced by the near doubling of giving observed when solicitors were at both doors, but the subsequent lack of seeking a chance to donate when solicitors are at one door. Why would one reveal a preference to give in one setting, then reveal the opposite preference in the other? The answer appears to be that the desire to give must follow the ask, and not precede it, and by not seeking being asked, one can again control their emotions and regulate giving.

An important application of our findings is that we gain a deeper understanding of the "power of the ask." Giving in our study ranges from from \$0.30 per minute in the one-door "silent" condition to \$1.00 per minute in the two-door verbal ask condition. The fact that small changes in fund-raising produced such large increases in giving indicates that very few people had a strong, stable desire to give. Second, we note that our data rules out several common explanations for the power of the ask. The fact that the verbal ask does not convey any information 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.

Our results also motivate a new sensitivity to the role of emotions in economic models of giving. Researchers have shown that altruistic acts are often preceded by empathic stimuli (Batson, 1991; Preston and deWaal, 2002; Andreoni and Rao, 2011). Empathy can also explain our main finding that asking is both effective when experienced, yet aversive when anticipated from afar.[5] Under this interpretation, those who avoiding the ask are not callous or selfish, but rather are "good people" who are avoiding an ask as a means of controlling their altruistic impulses and guilt.[6]

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 depends 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 no." If one is willing to accept as reasonable certain relative values of these parameters, we show one can claim that the presence of

[5]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) .

[6]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).

the fundraisers is a net welfare enhancement.

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.[7]

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). 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 was 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.[8]

We implemented a 2×2 design. Solicitation occurred in two modes, only bell ringing or bell-

[7] An example of these difference is that avoidance of solicitation in their study might be attributable to factors such as the time cost of answering the door or uncertainty about safety that are unrelated to motives for giving. The increase in giving could be due to generous givers making an effort to be available when they know the solicitation is coming or the fact that potential givers have more time to deliberate and prepare, making them more comfortable when the doorbell rings, 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.

[8] 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.

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.[9] If two adults arrived together, both were counted. The study was conducted from 11:00 a.m. to 7:00 p.m. over 4 weekdays (Monday through Thursday), December 7-10, 2009. Each day was divided into 4 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

[9]Taxi drivers and store employees, who enter and exit the store many times during the day, but are not shoppers were not counted.

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

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

that donations are only observed at the block level.[10]

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 for four days without any solicitation, which refer to as the placebo data.[11] 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.[12]

[10]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. The fact that Trachtman was always the solicitor at door 1 is unlikely to have any consequence for our study. On the one hand, it means that door 1 appeared identically in all conditions, which should give power to the results. On the other hand, it means that an author on the study also interacted with the subjects directly and thus was not blind to the hypothesis. For this reason we will attempt to control for any "Trachtman effect" in our data. We report this in footnotes and, as we show, there was indeed no measurable Trachtman effect.

[11]The placebo data was collected in July of 2013.

[12]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.

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 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, since there were only 64 experimental sessions (traffic measurements) assigned to 16 four-session blocks, these statistics cannot rely solely on asymptotics and instead rest, to some degree, on normality assumptions that we cannot directly verify with the original data since observed traffic is endogenous to the type of solicitation.

The main failure of normality we are concerned with is that unidentifiable day-of-week by time-of-day anomalies may occur. The placebo data provides traffic measurements that are not contaminated by varying charitable solicitation. The results of these diagnostics, documented in the appendix, 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. Additionally, the placebo data allow us to directly control for the interaction between day-of-week and time-of-day by using fixed effects in a regression framework.

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" will refer to the same day-time slots in the absence of solicitation (that is, 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. The shopper then faces a decision problem. Depending on the shopper's self-awareness, she may imagine what she would do if she passed a bell-ringer and how she would feel. She can also

evaluate how she would feel if she instead chose an entrance without a solicitor, and thus failed to donate. Finally, each path has a cost. 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. Framed in an intertemporal context, she makes her choice in the "cold state" over which "hot state" to put her future self in.

Let u_s be the utility from passing a solicitor, and u_0 the utility from avoiding one. 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 even greater 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 a person can choose their 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, they anticipate that their emotional short run self will either feel guilty or overrule their 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, *non-avoiding-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 *non-avoiding-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, thus are steered 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 must walk past door 1 to find the reach 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 will have the effect

of making more people givers and make donations bigger *conditional* on being asked. Working backwards, if this increased chance of giving, larger gift conditional on giving or heightened guilt of saying no makes the planner worse off, then it will make avoiding the ask more attractive when deciding from the cold state in the parking lot. We can measure this avoidance by exploiting the variation in emotional intensity of the solicitation and the cost 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 in the context of 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 across our four conditions (right axis).[13] Across all four conditions, the average number of givers per session was 12.6, or about 0.5 givers per minute, and $58.57 in donations per block, or about $0.55 per minute. Overall giving ranged from 0.36 givers per minute in one-door silent condition (Opp1) to 0.87 per minute in the two-door asking condition (Ask1&2). Total money donated showed three-fold 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 OLS framework that allows for day-of-week and time-of-day fixed effects. All the differences we cite here are significant beyond the 0.01 level. 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. 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 dollars given 45% (column 2). 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.

Table 2 also provides the first evidence of endogenous sorting. In our framework, giving comes from three types: "giving-seekers," "giving-avoiders," and "non-avoiding-givers." The first two

[13] We were only able to count money in the kettles every four sessions, four times per day, so here each observation is a block of four sessions, or 92 minutes. This primarily so nothing seemed unusual to shoppers.

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***	32.86***
	(1.164)	(4.166)
Ask1	4.06***	20.63***
	(1.135)	(4.785)
Ask1&2	11.56***	60.90***
	(1.308)	(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%
F-tests, p 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

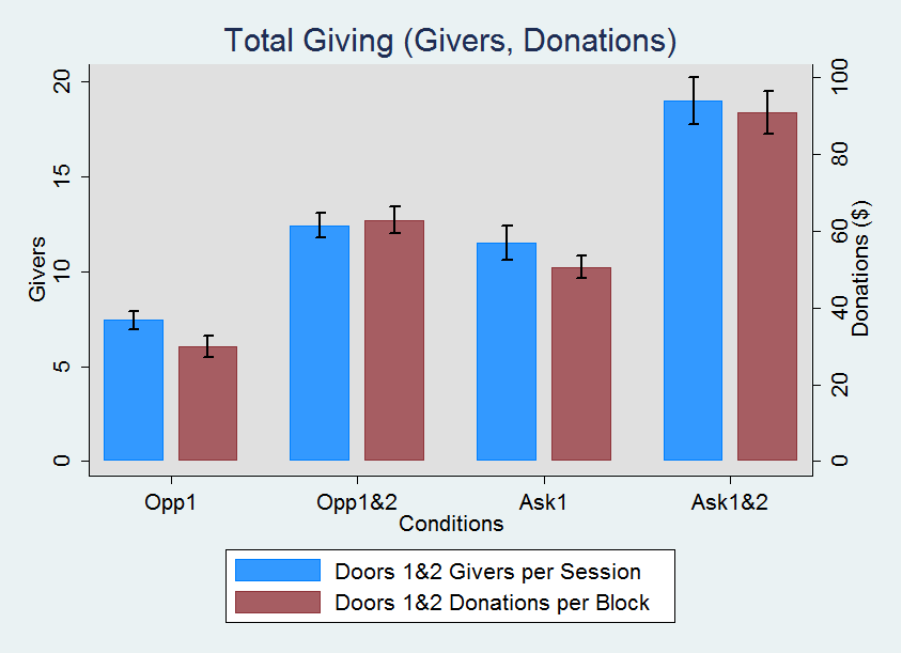

Figure 2: Total givers and donations by condition. Bars give 1.96 standard errors.

types change their preferred door based on the level of solicitation. We refer to this as sorting. Differential giving rates across conditions identifies sorting behavior on net. In the opportunity conditions the second solicitor increased the number of givers by 67% (5 givers per session) and dollars donated 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, meaning all giving is from passive types, we would expect the the two-door conditions to simply 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 that either of the sorting types account for all of giving. If giving was dominated by seekers, 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 raise very little. 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 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. Comparing total traffic in Opp1 to Ask1 and Ask1&2, we observe a steep decline for the ask conditions. In the one-door conditions, about 800 fewer people passed through doors 1 and 2 in Ask1, with the entire difference between the two totals being accounted for by door 1 traffic. This is expected based on the avoidance hypothesis because the solicitor was located at door 1. 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 for 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 asking conditions is much more substantial, 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 Door3 Bell-Ringing Traffic, Opp1 Total as Baseline					
Total Doors 1 and 2	4847	4847	4847	4847	19388
Imputed door3 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					
Total Doors 1 and 2	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 door3 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 door3 in the same way as Panel 2, using the placebo data.

The surprising lack of shoppers entering doors 1 and 2 in 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 requires that the treatment blocks are independent and identically distributed conditional on the day of the week and time of the day. We do not require unconditional independence 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). Time-of-day and day-of-week effects are thus equal in expectation. 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. Random assignment of treatment blocks renders this unlikely, but there was a relatively small number of treatment blocks. The relatively small number of treatment blocks also means a traditional test statistic requires a normality assumption.[14]

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 actual charitable solicitation. Total traffic is stable in the placebo conditions; counted traffic through doors 1 and 2 is around 4,300 in all conditions. In the placebo data we have an accurate count of 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 the door 3 data and had to infer it as deviations using Opp1. The deviations range from 26 to -101, and are -63 in total, which stands in stark contrast to the total 1726 estimated displaced passings in the bell-ringing conditions. 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. In aggregate, 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.

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 due 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 by condition.

Given that the placebo data places 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 in the

[14]This is less of a concern primarily 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&2 Traffic on Bell-Ringing and Placebo Conditions

VARIABLES	(1) Doors 1&2 Traffic under Bell-Ringing Conditions	(2) Doors 1&2 Traffic under Bell-Ringing and Placebo Conditions†
Opp1	omitted group	4.425
		(30.08)
Opp1&2	-10.31	-4.100
	(12.71)	(21.33)
Ask1	-49.87***	-44.62*
	(15.11)	(23.26)
Ask1&2	-47.69***	-50.21***
	(14.18)	(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 × Time fixed effects	no	yes
F-tests, p 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

†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

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 by the presence of a solicitor at door 2. This is has a quite natural explanation given the physical layout of the store—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 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. This

allows us to include 64 day-of-week by time-of-day interactions, however we must overcome a hurdle to yield interpretable coefficients. The counted traffic in doors 1 & 2 in the placebo conditions was stable at about 4300 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 the solicitor.[15] Given these differences, we need to inflate the placebo traffic to make the total traffic comparable to the bell-ringing sessions, otherwise all the bell-ringing condition coefficients will be biased upwards. We do so by simply specifying a multiplier, θ, to apply to uniformly to all placebo sessions. Since the counted traffic in the bell-ringing conditions is endogenous, it is not possible to perfectly identify θ. Our strategy is to compute the multipliers under three reasonable assumptions about displacement under the bell-ringing treatments. Specifically, we take displacement to door 3 to be zero in Opp1, in Opp1&2, or in Opp1 and Opp1&2 combined, since Table 3 suggests that under these conditions displacement effects were smallest. The three assumptions generate multipliers of 1.1163, 1.0848, and 1.1006 respectively (see Appendix Table A1 for more details). 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[16].

Returning to the Table 4, in column (2) the placebo data are multiplied by θ to form the omitted group and the regression includes 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 provied column (1) is properly specified because the calculation of the multiplier used the opportunity conditions as a baseline. This creates algebraic similarity to the specification in column (1) where Opp1 formed the omitted group. Note using the opportunity conditions to calculate the multiplier means we are unable to speak to baseline avoidance in $Opp1$; we report the estimate for completeness even though 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 core findings on 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 as given in column (1) of Table 4 to examine the distribution of traffic between doors 1 and 2. Column 1 shows that door 1 traffic drops by 32.6% in Ask1 and by 25.2% in Ask1&2 as compared to Opp1. We observed the expected larger drop in Ask1 traffic (since avoidance to door 2 is possible) but the magnitude of the difference is not statistically significant. Column 2 gives an idea why—there is little movement overall at Door 2. In Ask1&2, the solicitor at door 2 has little impact on door 2 traffic, whereas there is still

[15] We are assuming that people only seek in order to give. That is, they gain no pleasure from saying no.

[16] Under the assumption that there was no displacement in either door under either Opp1 or Opp1&2. 1.10 = (4847+4682)/(4342+4316)

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	-6.88	0.0046
	(10.34)	(7.958)	(0.0268)
Ask1	-52.19***	2.318	-0.111***
	(11.96)	(7.61)	(0.027)
Ask1&2	-40.31***	-7.375	-0.067**
	(9.757)	(10.07)	(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
F-tests, p 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

substantial avoidance at door 1. This difference is explained by the physical layout of the store, as shown in Figure 1. A shopper parked near door 2 that wished to avoid solicitation would have to walk past door 1 and around the corner of the store to door 3, which is not even visible from their parking spot. The evidence indicates this was quite uncommon—these shoppers were effectively "captured" by our solicitors. Similarly, avoiding to door 2 from door 1 appeared less attractive than opting for the side door, out of sight of the solicitor.

In column 3 of Table 5 we approach the data from a slightly different angle by regressing traffic the share of traffic through door 1 on the bell-ringing conditions. Share is defined as simply the fraction of observed traffic (doors 1 and 2) at door 1. Importantly, we have already shown that the denominator in this calculation falls substantially in Ask1 and Ask1&2, due to avoidance to door 3. This implies the coefficients, which reflect differences as compared to a where the denominator is higher, are biased towards zero.[17] Consistent with this logic, the avoidance estimates are lower than in column 1—but nonetheless still highly significant—-in at 20% and 12.5% under Ask1 and Ask1&2, respectively. [[JA doesn't see where 20% adn 12.5% come from?? Also a sentence fragment in the first red sentence]]

4.3 Seeking and the Types of Avoidance

It the last section we estimated avoidance net of seeking behavior. Avoidance dominates seeking in terms of magnitudes, but this certainly 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 look at giving rates under differential traffic patterns induced by experimental assignment. Figure 3 shows the number of givers and total donations at door 1 across conditions. The placebo measurements indicate that traffic is nearly equally split across doors when there is no solicitation (50.6% go to door 1).[18] 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 types—those givers that neither actively avoid nor seek the solicitor—we'd 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 be double in the one-door conditions (givers that would enter door 2 re-route to door 1). Third, if all donations came from people that choose to avoid when possible, then given that door 2 shoppers were "roadblocked," we'd expected door 1 to raise very little, if any, money in one-door

[17] 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. True door 1 share is 40%, but calculating just using doors 1 and 2, we get 44%, or a difference of only 6%.

[18] The fraction going to each door does depend a little 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.

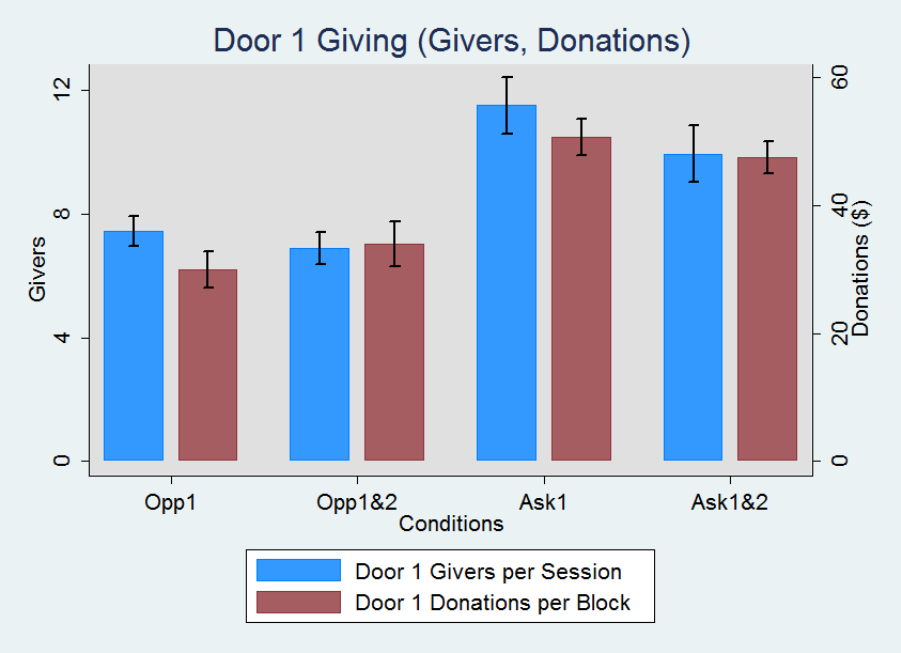

Figure 3: Door 1 givers and donations by condition. Bars give 1.96 standard errors.

conditions. If there is a mix sorting types, then observing similar giving rates at door 1 could be a function of two offsetting effects. The large increase in door 1 avoidance reduces giving, because some would have gave, but the presence of only one solicitor raises giving, due to seeking behavior.

Across all four conditions we observe slightly more givers and no difference in money raised at door 1 when only one solicitor is present. Taken in conjunction with the substantial giving at door 2, this patterns overwhelmingly rejects the hypothesis that most giving is attributed to seekers. Similarly, the fact that the magnitude of avoidance swamps giving rates and the higher giving rates in the one-door conditions strongly rules out that most avoiders would have given. The evidence instead indicates that either passive givers account for all donations, or that most giving comes from passive types, but there is some seeking that is offset by a similarly sized segment of avoider-givers.

Looking at the opportunity conditions more closely, 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. Table 2 further showed that doubling solicitation doubles donations. Finally, we see minimal evidence of avoidance in these conditions. The implication is that under the opportunity conditions, giving is dominated by non-avoider-givers. In other words, passive solicitation induced passive giving patterns.

We now turn to the ask conditions. At door 1, the number of givers per 23-minute session in Ask1 is 1.56 givers than in Ask 1&2. Looking at the F-test in Table 6, this difference is not

Table 6: OLS Regressions of Door 1 Giving on Bell-Ringing Treatments

VARIABLES	(1) Givers Door 1	(2) Donations Door 1
Opp1&2	-0.56	3.98
	(0.91)	(3.56)
Ask1	4.06***	20.63***
	(0.96)	(4.52)
Ask1&2	2.50**	17.46***
	(1.04)	(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
F-tests, p 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

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 a sufficient statistic for seeking. Unfortunately, we cannot identify the degree of seeking separately from the number of avoider-givers. We can, however, give some useful calibrations. If there are no avoider-givers, 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 would have given if avoidance was not possible. This in turn implies that 11.0% of givers are seekers. As a final benchmark, if we assume that the fraction of avoider-givers is the same as global base rate of givers, which is roughly 14% in the ask conditions, then the estimate of seekers is a significant at 12.6% share of givers.

We can narrow our focus to more realistic calibrations by incorporating data from door 2. When considering door 1 alone, roughly equal proportions of seekers and avoider-givers can explain most observed giving rates. But when we consider the fact that total donations rise 80% when a second solicitor is added in the ask conditions, short of doubling but still a substantial increase, this rules out both the hypothesis that most giving is generated by seekers and the hypothesis that there are no seeker givers whatsoever. Taken as a whole, we have statistically significant evidence that seeking generates a relatively small fraction of giving, which in turn implies that the fraction of avoider-givers is small (we cannot rule out that it is zero). 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.

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. Put another way, most people who actively changed the door they entered were avoiding turning down the request to give and the associated psychological cost. 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 and control of emotional aspects of fund-raising and 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.

5.1 Self- and Social-Signalling

Can models of social- or self-signaling explain avoidance? One channel is if avoiding provides a credible excuse for *not* sending a signal as to 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. Our 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 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.

This model can help explain why avoidance was most popular at the out-of-sight side door. But the model struggles 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 in our opinion. 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 they are going out of their way to avoid, we conclude the evidence is not consistent with either signaling explanations. A far more likely story is that non-givers in the opportunity conditoins noticed the solicitor but simply felt that this familiar fundraising strategy was "passive" in the sense of not eliciting a 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 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 Michel's famous self-control tasks who successfully avoided eating the marshmallow by physically

turning their backs, thus reducing the emotional but not 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, as it were, 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 by 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's? 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.[19] 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 avoidance? 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: 1) in the treatment condition a

[19] 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.

solicitor was positioned at one of the doors and made a verbal request to shoppers; 2) in the control there is was no solicitation. Another important difference is that instead of using the well-known Salvation Army the researchers raised money for the far lesser-known group Polycistic 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, which indicates that the degree of avoidance likely depends on the recognizably of the charity, visibility of the solicitors, and potentially 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 high incidence of avoidance in this study and the replication just discussed raises 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). In a related paper, (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 a 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 "street solicitation." We'll focus on the most effective (and intrusive) solicitation condition, (Ask1&2), which raised about $0.15 per shopper. 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. Given that the average American's speed of walking is about 4.4 feet per second,[20] avoiding to door 3 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 $0.10. For a person

[20]This figure is from Wikipedia.

on the margin between avoiding and passing the solicitor (the "marginal non-avoider"), the cost of avoidance ($0.10) should equal the cost of "saying no."[21] If we assume that the "saying no" costs of non-avoiders are uniformly distributed between 0 and $0.10, then the mean cost is $0.05 for a non-avoider and $0.10 for an avoider. Finally, we assume no utility of giving.[22] By multiplying each cost estimate by the fraction of the population occupying each group, we get a lower bound of $0.07 for Ask 1&2. If we only consider time costs, the bound is $0.03. 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, $1.69. Through the same reasoning, it's also defensible to assume that the cost of "saying no" is less than $1.69 for the non-giving entrants.[23] 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 $0.85. If we ignore the welfare impact on givers, then upper bound is calculated as $0.72. 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 $0.07 and $0.72 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 0.46 (that is 0.07/0.15), then fund-raising 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 4.8 (that is, 0.72/0.15) 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

[21] Using only the time cost of walking is problematic because presumably some "guilt cost" motivates avoidance in the first place. However, we note that 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).

[22] The marginal giver suffers a utility loss, by construction, but seekers might have utility gains.

[23] It's 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).

not take an explicit stance on what should count, but note that our bounds include cases where fund-raising of this sort is socially efficient and cases where it is 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 32.6% of would-be entrants to "avoid the ask." Asking, it seems, is both aversive and effective. There is no 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

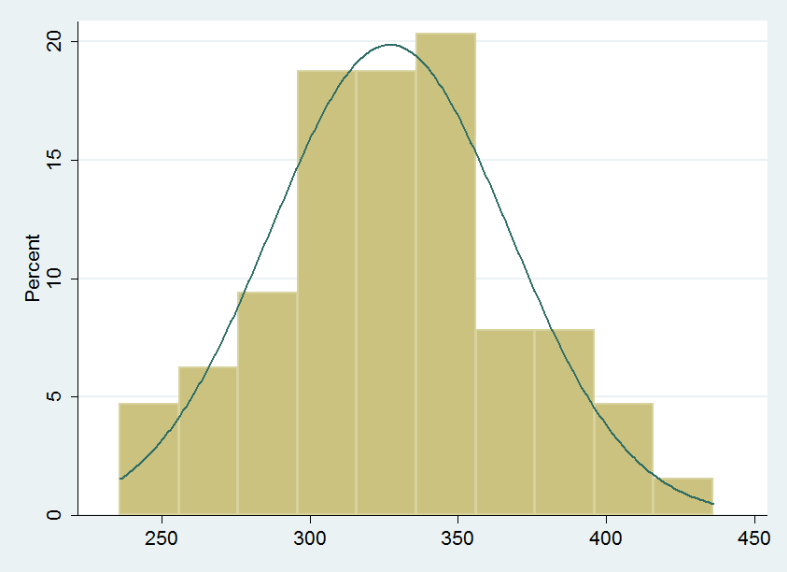

Figure A1: Total Placebo Traffic per 23-Minute Session (Doors 1, 2, and 3)

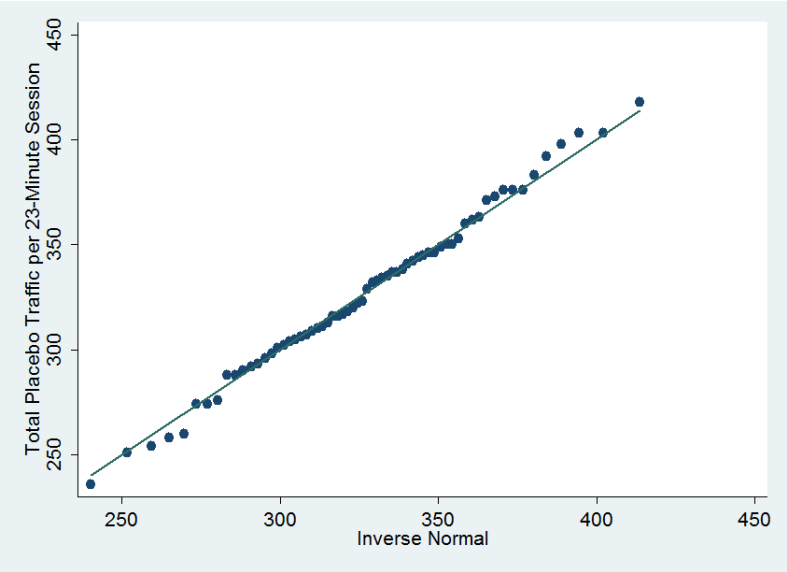

Figure A2: Quantiles of Total Placebo Traffic per 23-Minute Session against Quantiles of Normal Distribution

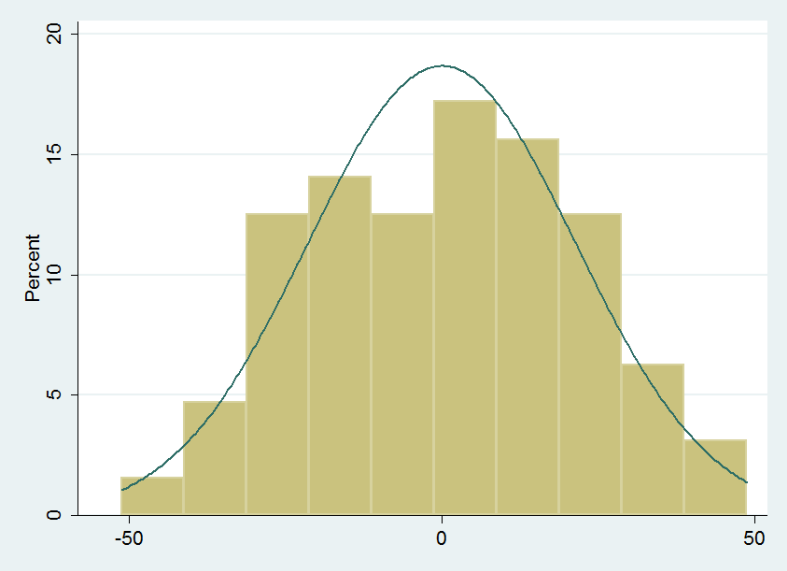

Figure A3: Residuals from Regression of Total Placebo Traffic (Doors 1, 2, and 3) on Day and Time Dummies

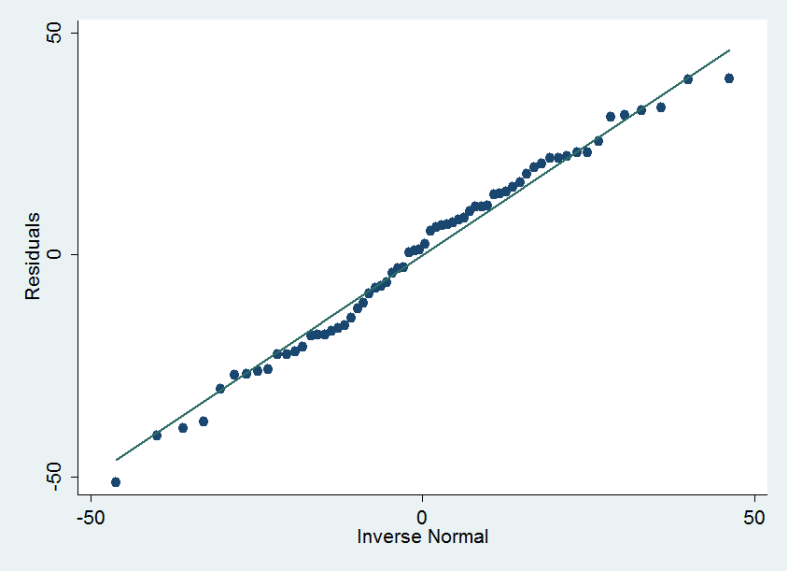

Figure A4: Quantiles of Residuals from Regression of Total Placebo Traffic (Doors 1, 2, and 3) on Day and Time Dummies against Quantiles of Normal Distribution

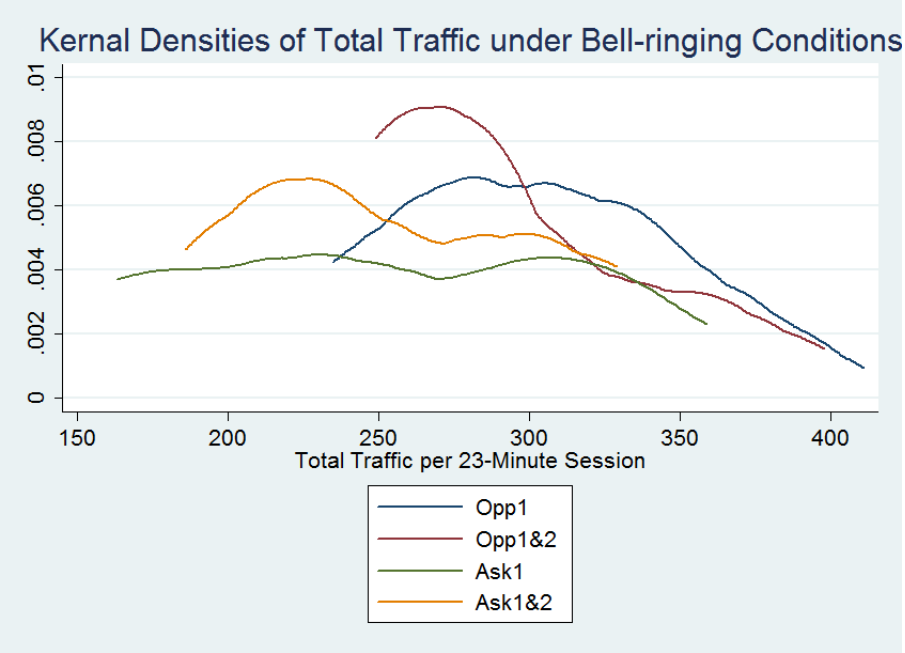

Figure A5: Kernel Density Estimates of Total Bell-ringing Traffic (Doors 1 and 2)

B Computation of the Traffic Multiplier (θ)

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

	Silent Opportunity		Direct Ask	
	Door 1 (Opp1)	Doors 1 & 2 (Opp1&2)	Door 1 (Ask1)	Doors 1 & 2 (Ask1&2)
Traffic in Asking Conditions as a Proportion of Traffic in Placebo				
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
Imputing Percent Displacement of Traffic Under Four Assumptions				
a. $\theta = 1$ *(assume traffic is the same in Bell-Ringing and Placebo)*				
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
b. $\theta = 1.1163$ *(assume displacement is 0 in Opp1, Doors 1 and 2)*				
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
c. $\theta = 1.1006$ *(assume displacement is 0 in Opp1 and Opp1&2, Doors 1 and 2)*				
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
d. $\theta = 1.0848$ *(assume displacement is 0 in Opp1, Door 2)*				
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[†]

VARIABLES	(1) $\theta = 1$	(2) $\theta = 1.05$	(3) $\theta = 1.1$	(4) $\theta = 1.15$
Opp1	31.56	17.99	4.425	-9.144
	(29.64)	(29.85)	(30.08)	(30.33)
Opp1&2	22.88	9.387	-4.100	-17.59
	(20.66)	(20.96)	(21.33)	(21.77)
Ask1	-17.56	-31.09	-44.62*	-58.16**
	(24.72)	(23.97)	(23.26)	(22.58)
Ask1&2	-22.44	-36.32**	-50.21***	-64.09***
	(14.97)	(14.81)	(14.77)	(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 × Time fixed effects	yes	yes	yes	yes
F-tests, p 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

[†]Bell-ringing traffic if 2009, θ×(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[†]

VARIABLES	(1) $\theta = 1$	(2) $\theta = 1.05$	(3) $\theta = 1.1$	(4) $\theta = 1.15$
Opp1	0.0869	0.0583	0.0324	0.00871
	(0.0559)	(0.0534)	(0.0512)	(0.0492)
Opp1&2	0.111	0.0831	0.0572	0.0336
	(0.0659)	(0.0629)	(0.0602)	(0.0577)
Ask1	-0.120*	-0.139**	-0.156***	-0.172***
	(0.0575)	(0.0550)	(0.0528)	(0.0507)
Ask1&2	-0.0736	-0.0942**	-0.113**	-0.130***
	(0.0436)	(0.0413)	(0.0392)	(0.0373)
Constant	0.540***	0.538***	0.537***	0.535***
	(0.0218)	(0.0207)	(0.0196)	(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
F-tests, p 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

[†]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