

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

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Abstract

What triggers giving? We explore this in a randomized natural field experiment during the Salvation Army's annual campaign. Solicitors were at one or both of two main entrances to a supermarket, making the solicitation either easy or difficult to avoid. Additionally, solicitors were either silent, or asked "please give" to passersby. We observed over 17,000 passings over four days, and found dramatic avoidance of the solicitors, but only during a direct ask. Furthermore, asking increased donations 75%. Across all conditions, seeking the solicitor was exceedingly rare. The results do not support static views of altruism, such as inequity aversion, and instead highlight the importance of social cues and psychological features of the giver-receiver interaction. We argue that avoidance could evidence a lack of altruism or self-control strategy to deal with empathic impulses to give.

*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, Stefano DellaVigna, Randall Lewis, David Reiley and Michael Schwarz 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 people have a great capacity to be generous. They 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 selflessness, as in mutualistic cooperation (Grice, 1957), kin selection (Hamilton, 1964), repeated-game reciprocity (Trivers, 1971) and norm adherence through sanctions (Boyd and Richerson, 1992). When economists removed these incentives in anonymous, one shot, dictator games among unrelated strangers, the initial findings surprised many. Subjects often eschewed the dominant strategy to be selfish, with many even choosing equal splits (Forsythe et al., 1994; Roth, 1995). These findings provided inspiration to new theories of social preferences, such as "inequity aversion" (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), that is, people enjoy giving to others who are less fortunate.

Subsequent laboratory experiments found that factors other than inequality had significant effects on choices. For instance, 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), and when they are forced to reason empathically about recipients (Andreoni and Rao, 2011). Giving is also responsive to the visibility of the giver's actions; in fact, as the ability of recipients to detect selfish acts becomes more difficult, altruistic acts virtually disappear (Andreoni and Bernheim, 2009; Ariely et al., 2009), implying that maintaining a positive social-image is a central motive behind mending inequality. This suggests that giving is situational. That is, when an opportunity to give is imposed upon people they give, but they may not seek it out if it is not present. Indeed, when given a chance to silently opt out of a dictator game (at a small cost), Dana et al. (2006) and Lazear et al. (2006) find that many subjects avoid being placed in the role of a potential altruist.

Taken together, these new studies appear to indicate that people give for reasons that are more self-focused than the original dictator game studies suggested. This inference raises natural questions about giving and fundraising. Is giving an altruistic act? Is fundraising bad for welfare? If people make contributions when asked to give, then, by revealed preference, they are better off. It is thus hard to argue that the act of giving is itself deleterious. Rather, this behavior would suggest people don't actually avoid *giving*, but they are avoiding being *asked to give*. That is, it is the act of asking that may affect the behavior, and thus utility, of potential givers.

We use a natural field experiment to address these questions. The experiment involved an institution well-known for decades in America, the Salvation Army's annual fundraising campaign. In the Christmas season, volunteers for the Salvation Army stand at entrances to stores and shopping malls, with the trademark Red Kettle, ringing a bell and seeking donations. We positioned solicitors at one of or both of two main entrances to a grocery store in suburban Boston over four days, and counted how the presence of the solicitors affected traffic and donations when only one of the two doors was covered as compared to when both were covered. We combined this with

another manipulation in which the solicitors either verbally asked for donations, or were totally silent, avoiding even eye contact. Notice that the silent bell-ringer is obviously requesting a donation, but the verbal ask is adding a deeper layer social interaction by forcing the potential donor to acknowledge the request, typically by indicating a yes-or-no reply. By comparing traffic flows when coverage and requesting are varied, our design allows us to identify whether potential donors seek or avoid the solicitation, and whether the stronger social interaction of the verbal ask is more or less aversive.

Our experiment contained another subtle but revealing twist. While the store had two main entrances off the parking lot, there was also a third entrance that was inconveniently located on the side of the store. This door never had a solicitor, but required shoppers to go well out of their ways to enter or exit the store when accessing their cars. Traffic through this door is the most costly form of avoidance.

We found that there was a virtually no avoidance of silent solicitors. In sharp contrast, however, up to one third of patrons chose to avoid being verbally asked to give by changing the store entrance they used. In addition, we confirm laboratory findings that asking is powerful. When shoppers were presented with a short, scripted request to give, giving rose by 75 percent.

The fact that a verbal ask is so much more powerful than a silent opportunity to give suggests that asking has a social or psychological effect on the giver beyond the mere awareness of the solicitor. Nor can it be explained by the request, a simple "please give today," conveyed any meaningful new information. It is possible that giving may burnish a positive social- or self-image as a generous type. But this cannot explain the difference between a silent but obvious implicit request, and a short overt verbal request. Social- or self-image can be burnished equally well in both cases, yet both giving and avoiding are increased by the ask. Our results point to the need for deeper thinking about how the psychological process that triggers one to give.

What this or other research on this area cannot answer directly is whether avoiding being asked reveals a lack of altruism, or if fundraising is bad for society by creating uncomfortable social pressure. For example, we argue in the Section 4 that the avoidance behavior we observe is also consistent with a model in which individuals avoid being asked because they are *too* altruistic. In our experiment subjects do not avoid a charitable solicitation that has a low component of sensory, and perhaps empathic, stimulation. However, when the solicitation has only a modicum of sensory or empathic stimulation in the form of brief eye contact with a solicitor who says "please give today," then avoidance is dramatic. As psychologists have shown (Batson, 1991), altruistic acts are often preceded by empathic stimuli. What our subjects' behavior appears to suggest is that there is conscious avoidance of the first step, the empathic stimulation. Cognitively, this is analogous to a dieter avoiding exposure to chocolate. Under this interpretation, "good people" are avoiding empathic stimuli, such as an ask, as a means to regulate their altruism, and very few people give

in the absence of empathic stimuli, which explains the lack of positive sorting.¹

This all points to something unique about the social act of a direct ask, and this paper can only speculate on the mechanisms at work. The results can, however, help sew together questions raised about the use of dictator games in a laboratory. Recently, authors have claimed that the laboratory presents a special kind of expectation to give, that absent these "experimenter demand effects" subjects would behave quite differently, perhaps questioning the validity of laboratory studies (Levitt and List, 2007; List, 2007; Bardsley, 2008). What our work shows is that it is the demand effect itself that makes dictator games ideal for studying giving behavior. Had the Salvation Army not been in front of the grocery store doors, it is likely that none of the givers would have made an effort to give. And when the social stakes of the interaction were raised by making an explicit verbal request, giving increased dramatically over the passive bell-ringing. The solicitors in the paths of the shoppers made them choose to give, just as a laboratory experiment that places one subject in a role to "allocate" money to another subject converts the pair from strangers to "givers" and "recipients." Our study shows that the laboratory and the field are actually quite similar. Moreover, our study allows a more fundamental question to emerge: Why are such subtle social cues so powerful in human beings? The answer, we believe, lies partly in the social interaction itself, but also in the unconscious brain mechanisms that turn empathic feelings into charitable acts.²

In the remainder of this paper, Section 2 presents the design of the field experiment, Section 3 presents the results, with a discussion in Section 4 and conclusion in Section 5.

2 Design of the Field Experiment

Our study partnered with the Salvation Army Red Kettle Campaign, one of the best known street fundraising campaigns in the United States. The campaign occurs annually in the weeks leading up to Christmas Day. Volunteers, clad in distinctive red aprons, 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 Red Kettle Campaign's prominence helps assure us that subjects viewed the solicitor as representing a legitimate and worthy cause.

We coordinated with the Salvation Army to choose a location in the Boston area to satisfy these criteria: First, the store had two main doors that were far enough apart to create a cost of sorting; Second, both main doors were visible from the parking lot; and third, traffic of at least 180 people per hour.

An aerial photo of the selected store is shown in Figure 1. From the parking lot, Door 1 was on

¹This explanation has much in common with theory of cue-triggered choices of Bernheim and Rangel (2004).

²Authors in the neuroscience literature have argued in favor of this view. See for instance Singer and Decety (2006) and Hare et al. (2010).

the left and Door 2 on the right, both 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. This door was marked "recycling"; it is the place people enter in order to recycle plastic bags. In our pre-screening this door was not used for accessing the store, rather it served its usual role for recycling. However, we did notice one could get from the recycling area to the store proper. The presence of this door, which was extremely inconvenient for most patrons, proved very useful in our analysis.



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

We implemented a 2×2 design. On one dimension, the solicitation was either silent, which we call the "Opportunity" condition, or included a verbal greeting and request, the "Ask" condition. In the Opportunity conditions solicitors rang the bell, but did not speak or attempt eye-contact, except to thank those that gave, as per Red Kettle custom. In the Ask condition, solicitors did as in the Opportunity condition but in addition they attempted eye-contact to each passerby and said simply, "Hi, how are you? Merry Christmas. Please give today".

The second dimension varied whether the solicitors were at one or both main doors. For shorthand, we will call our four conditions *Opp1*, *Opp1&2*, *Ask1*, and *Ask1&2*, with "Opp1" referring to the Opportunity solicitation at Door 1 only, and so on.³

³The 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 (although no longer 22, all are still white). Trachtman administered a 45-minute

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

Each solicitor discreetly recorded the number of *givers* using a counter in her apron pocket. Two additional research assistants recorded shopper traffic from cars parked outside each of the main doors. Only individuals who appeared 18 or over were counted.⁴ If two adults arrived together, we counted both. Since the third (side) door was not easily visible from the parking lot and because it was essentially unused in pre-screening, traffic was not recorded for it. In the results section we infer third door traffic through differences in the traffic that was 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, and total donations for the block were tallied.

Each of the 4 conditions was assigned to blocks according to schedule shown in Table 1. Blocks were balanced across days and times. 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. We cannot balance the interaction between these two factors, so in the analysis we use co-variates to control for remaining differences.

In order to eliminate contamination on the social interaction level, the two blocks in the morning/evening were either both Ask or both Opportunity sessions. This meant 1-door and 2-door treatments had to occur adjacently, 18-minutes apart. A minority of shoppers entered during a 2-door treatment and exited during a 1-door treatment, or vice versa. However, this only dilutes our results, rather than confounds them, as lack of (or false) knowledge of the solicitors' locations works against the ability to sort.⁵

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.

⁴Taxi drivers and store employees, who enter and exit the store many times during the day, but are not shoppers were not counted.

⁵If the shopper enters during a 2-door treatment and exits during a 1-door treatment, the door through which the

This paper is most closely related to recent research by DellaVigna, List, and Melmender (2011). Although conceived independently, many components are common. In their study, the fundraiser gave people an opportunity to opt out of a door-to-door solicitation by checking a box on card left hanging on front door of their urban Chicago homes. Like our study, they found that many people avoided the solicitation by either checking the box or not answering the door. However, for those who were willing to be solicited, the average gift was higher. The chief aim of their study is the welfare impact of opt out policies. As such, their data is not suitable for our question about the influences and motives for giving. For instance, avoidance can be due to many factors other than avoiding a charitable solicitation, such as the time cost or uncertainty about safety. Also, the increase in giving found by DellaVigna, et al., could be due to positive sorting (generous givers making an effort to be available when they know the solicitation is coming), but it could also be that knowing the solicitation is coming makes those willing to give more comfortable and allows them more time to verify the quality of the charity as compared to a "cold call." The demeanor of the solicitor himself could also be affected by moving from cold call to "warm call" mode. In evaluating the welfare impact, however, it does not per se matter if giving increases due to positive sorting, because of increased variability, or the demeanor of the solicitor. What matters is the increase itself and the fact that checking an "opt out" box is undoubtedly preferred than answering the door and not giving or not coming to the door.

Our experiment carefully controls the cost to avoid being asked, makes it trivially simple to keep the giver-receiver interactions consistent across conditions, and leaves little room for avoiding for reasons unrelated to our treatment variables. Moreover, by being simple and subtle, our experiment shows the deep power of the social interactions within the economic exchange between givers and receivers. One may argue that both the expected contributions and costs of avoidance are low in our experiment, making it less consequential for policy. The natural counter is that the huge effects these "small" costs are having on behavior only deepens the questions about altruistic preferences raised by our findings.

3 Results

We begin by discussing how the solicitations affected traffic at the store. Do people seek or avoid giving? Next we look at how giving responds to the presence of solicitors as well as to asking. Examining giving rates allows us to identify the presence of positive sorting even when sorting is on net negative. Third, we look at how asking affects the size of each gift. Finally, we comment on the implications of this work for social welfare.

shopper exits should be neutral with respect to the treatments since the belief is there are solicitors at both doors; if the shopper enters during a 1-door treatment and exits during a 2-door treatment, the shopper may choose to exit through Door 2 in belief that there is no solicitor there.

Table 2: Store Traffic

	Asking Condition				Total
	Silent Opportunity		Direct Ask		
	Door 1 (Opp1)	Doors 1&2 (Opp1&2)	Door 1 (Ask1)	Doors 1&2 (Ask1&2)	
Gross Traffic					
Door 1	2,563	2,508	1,728	1,918	8,717
Door 2	2,284	2,174	2,321	2,166	8,945
Total Passings	4,847	4,682	4,049	4,084	17,662
Imputed Traffic, Preferred Specification, Opp1 Total as Baseline					
Total Passings	4,847	4,847	4,847	4,847	19,388
Door 3 Increase	0	181	798	763	1,742
Traffic As Percent of Total					
Door 1	52.8%	51.7%	35.6%	39.5%	
Door 2	47.1%	44.8%	48.1%	44.6%	
Door 2 Increase	2.3%	-	3.5%	-	
Door 3 Increase	0%	3.7%	16.4%	15.7%	

3.1 Traffic Patterns and Avoiding Asks

Table 2 summarizes the traffic flows across our experimental conditions. We observed a total of 17,662 passings over four days. This averages to 276 passings through the supermarket doors during each 23-minute block. Since there were breaks between blocks, it is likely that a small number of shoppers were counted on only one of their passings, making it impossible to know exactly how many unique people are represented. Hence we add two assumptions. First, we assume that the number not counted going in equals the number not counted going out. Second, we assume that individuals do not give both entering and exiting, but give at most once (an assumption corroborated by the solicitors). Under these conditions, the best estimate of the number of individuals we counted is half of 17,662 or 8,831 people. The analysis has also been done simply considering each "passing" as the unit of observation, and all the conclusions and statistical significance remain unchanged.

One can note already that the raw results point to a large effect of solicitations on traffic flows. In the Opportunity sessions, traffic falls from Door 2 when a second solicitor is added there. Also, in Opp1&2 total traffic falls slightly, suggesting perhaps greater use of Door 3. In the Ask conditions the results are even more sharp. There were 836 fewer passing through Door 1 in the Ask1 condition than in the Opp1 condition. Combining Ask and Opp conditions, we observed 1397 fewer passings in the Ask conditions, indicating a likely important role for Door 3 in our analysis.

In Table 3 we confirm these impressions with regressions of traffic per session on treatment

Table 3: OLS Regressions of Traffic on Conditions

Dependent Variable	Traffic per Session		
	Total Traffic	Door 1	Door 2
Opportunity at Door 1 (Opp1)	10.31 (12.71)	3.438 (10.34)	6.875 (7.958)
Ask at Door 1 (Ask1)	-39.56*** (12.73)	-48.75*** (11.33)	9.188** (3.821)
Ask at Doors 1 & 2 (Ask1&2)	-37.37*** (11.62)	-36.87*** (8.969)	-0.500 (7.621)
Mean of Omitted Treatment (Opp1&2)	293.63	156.75	135.88
Day & Time Fixed Effects	yes	yes	yes
Observations	64	64	64
R^2	0.760	0.664	0.802

Note 1: Standard errors clustered by block in parentheses.

Note 2: Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

and on day and time fixed effects. The omitted condition is Opp1&2. Column (1) demonstrates that the decreased Door 1 and 2 traffic for Ask1 and Ask1&2 is highly significant. As compared to Opp1&2, Ask1&2 and Ask1 had 39.6 and 37.4 fewer counted passings per 23-minute session, respectively. Opp1 had 10.3 more passings than Opp1&2, indicating some seeking of solicitors, but the effect is not significant. People were also much more likely to use the third door in the presence of verbal solicitation. Column (1) shows this effect is almost entirely driven by people that would have entered Door 1 | avoiding Door 2 solicitors is indeed the most difficult. In particular, reaching Door 3 from Door 2 requires one to walk directly past Door 1, which always had a solicitor and thus is not only inconvenient, but also does not effectively avoid a solicitation. Column (3) shows that in Ask1 there is also significant avoidance to Door 2, which explains why Ask1 has the fewest passings through Door 1 over the four conditions.

Given that having a silent opportunity at only Door 1 was appeared to have no significant overall effect on net traffic flows, then, relying on our randomization across days and times, the number of passings in the Opp1 conditions appears to be the most natural choice for a baseline of the expected total passing per condition. We can then use this baseline to impute traffic through Door 3 and then calculate the effects of solicitors on the total store traffic. This imputation is presented in the center panel of Table 2. We note that a valid alternative baseline could be to average Opp1 and Opp1&2 traffic. The results are nearly identical to what we report here, thus we reserve this alternative for appendix, Table A1.

Using the imputed Door 3 avoidance, we are now in a position to calculate the percentage of

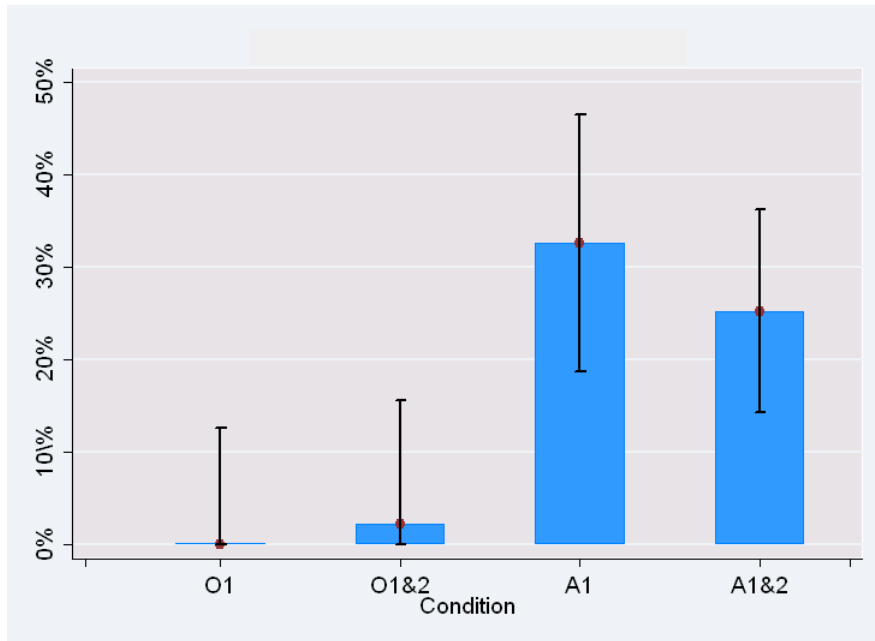


Figure 2: Door 1 Avoidance by Condition

patrons entering each door by condition. We see that Door 1 is favored in the face of a silent opportunity. In both Opp1 and Opp1&2 about 52 per cent of individuals used Door 1. In stark contrast, only 35.5 percent of shoppers used Door 1 in Ask1. In Ask1&2 avoiding was more difficult, due to the solicitor at Door 2, and accordingly Door 1 saw more traffic (39.6 percent) than Ask1, but still far less than either opportunity condition.

In the bottom row of Table 2, we report that 16.4 and 15.7 percent of all shoppers avoided solicitation in Ask1 and Ask1&2. However these figures tend to understate the true level of avoidance in the population. The reason is that would-be Door 2 users in the Ask1 condition were not constrained by a solicitation at Door 1. This is reflected in Table 2 where we Door 2 does not show a drop in traffic in Ask1&2 (2166) as opposed to Opp1&2 (2174), and that the Door 3 increase is virtually the same in the two Ask conditions (798 and 763). Rather, if we isolate the traffic that would, without solicitations, have preferred to use Door 1, we can measure the aversiveness of the fundraising. We show this in Figure 2.

The results in Figure 2 are striking. First, over one third of patrons preferred to avoid solicitation in Ask1, the verbal request condition for which avoidance was easiest. In Ask1&2, over 25 percent were deterred from Door 1. The implication is that over a quarter of the population dislikes being asked to give so much that they took a side entrance to avoid it. The avoidance estimates for the ask conditions are significantly different from Opp1 (Ask1 $F(1, 15) = 19.04, p < 0.0006$, Ask1&2 $F(1, 15) = 17.07, p < 0.0009$) and Opp1&2 (Ask1 $F(1, 15) = 18.52, p < 0.0007$, Ask1&2 $F(1, 15) =$

16.90, $p < 0.0009$), but they do not significantly differ from each other ($F(1, 15) = 1.21, p < 0.28$)

Having found net-negative sorting in the presence of a verbal request, we examine the opportunity treatments more closely. Although Door 1 traffic for Opp1 is slightly higher than in Opp1&2, the regression in Table 3 shows the difference is within one standard deviation from zero. Moreover, the imputed Door 3 increase in Opp1&2 is modest. It appears that a verbal rather than implied ask is a key driver of avoidance. A silent opportunity that one can easily walk past is not nearly as aversive as a short, polite, verbal request to give. This result is a close parallel to the lab results from Dictator Games found by Andreoni and Rao (2011). When lab "recipients" could ask "allocators" for a share of \$10, giving was much more likely, but when allocators were given a chance to avoid by writing an excuse, while recipients were silent, the likelihood of giving fell from 65 percent to 32 percent, and the share given fell from 24 percent to 6 percent. As we show next, asking had a similar effect on dollars donated in the field.

3.2 Rates of Giving and Solicitor Seeking

Table 4 shows the number of times someone passing chose to give. The top panel reports the observed givers. The second panel reports this as a percent of counted passings, while the bottom panel is perhaps most informative by reporting giving as a fraction of imputed traffic. As one would expect, giving is much more likely when solicitors are asking than when they are silent. In Ask1&2 gifts are made by 6.27 percent of shoppers, as compared to 4.1 percent in Opp1&2, a 53 percent increase. Making giving harder to avoid also increase the likelihood of giving. In the silent opportunity conditions, 2 solicitors saw 67 percent more givers, and with an ask the increase was nearly the same, at 65 percent. As before, however, these raw numbers are best understood if we focus on projected Door 1 entrants.

Figure 3 displays the rates of giving on the individual level, that is, assuming one individual makes two passings. For reference, the left panel of Figure 3 (blue bars) shows giving probabilities for all would-be entrants (actual entrants plus those calculated in the prior section as avoiders). The center panel shows the likelihood of giving for only Door 1 entrants, while the right panel adjusts the data for those we calculated to avoid Door 1.

Looking first at the left panel, two-results are immediately clear. First, having 2 solicitors greatly increases the propensity to give. Second, asking is extremely powerful, which replicates the laboratory findings Andreoni and Rao (2011). Giving is 57 percent more likely in the presence of simple and polite request. We estimate that the baseline solicitation, *Opp1*, elicits donations from 4.92 percent of people. This figure jumps to 8.50 percent for *Opp1&2* ($t = 4.61, p < 0.0001$). Giving increases 2.7 percentage points moving from *Opp1* to *Ask1*, which comes in at 7.60 percent givers ($t = 3.86, p < 0.001$). *Ask1* does not significantly differ from *Opp1&2* ($t = 0.76$); in other words, adding the simple verbal request of "please give," is as about as effective as adding an additional silent solicitor at the store. Adding a verbal request at both doors generates more givers

Table 4: Numbers and Rates of Giving, by Condition

	Silent Opportunity		Direct Ask		Total
	Doors with Solicitors		Doors with Solicitors		
	Door 1 (Opp1)	Doors 1&2 (Opp1&2)	Door 1 (Ask1)	Doors 1&2 (Ask1&2)	
Givers					
Door 1	119	110	184	159	572
Door 2		89		145	234
Total	119	199	184	304	806
Giving As Percent of Counted Passings					
Door 1	4.64%	4.39%	10.65%	8.29%	
Door 2		4.07%		6.69%	
Total	2.46%	4.25%	4.54%	7.44%	
Giving As Percent of Imputed Traffic					
Door 1	4.64%	4.11%	6.85%	5.93%	
Door 2		4.09%		6.69%	
Total	2.46%	4.10%	3.80%	6.27%	

than any other condition; in Ask1&2 12.54 percent of people gave, which is significantly higher than *Opp1&2* ($t = 4.99, p < 0.0001$) and all the other conditions, at greater levels of significance.

The center panel of Figure 3 gives the rates of giving conditional on *actually using* Door 1. Under the hypothesis of positive sorting we would expect more giving in Opp1 than Opp1&2 and more in Ask1 than Ask1&2. This is because some altruistic individuals seek the solicitor with the intent to give. If, in addition to altruism some are influenced by audience effects (Andreoni and Bernheim, 2009), and if we think the verbal requests are a stronger cue to social-image, then we might expect a larger difference between giving at Door 1 for the Ask treatments than the Opportunity treatments. While all of these predictions are met at the means, the 0.7 percentage point higher giving in Opp1 is not significantly different from Opp1&2 ($t = 0.67, p < 0.51$). Comparing Ask1 and Ask1&2 a different story emerges. Giving conditional on actually entering Door 1 in Ask1 is 21.2 percent, which is significantly higher than Ask1&2, 16.5 percent ($t = 3.06, p < 0.002$). This would also suggest social image concerns may also have been operating in the Ask conditions.

We must be careful, however, in making this comparison. First, the giving rates in the Ask conditions are artificially inflated because they do not include negative sorters who avoided being asked. Table 3 shows that avoidance to Door 3 was significant in the Ask conditions and overall avoidance was highest for *Ask1*. This net negative sorting lowers the denominator in conditions in which more people have sorted out and are thus not counted. Implicitly the 21.2 percent vs. 16.5 percent comparison in the Ask conditions assumes that avoiders give at the same rate as those that

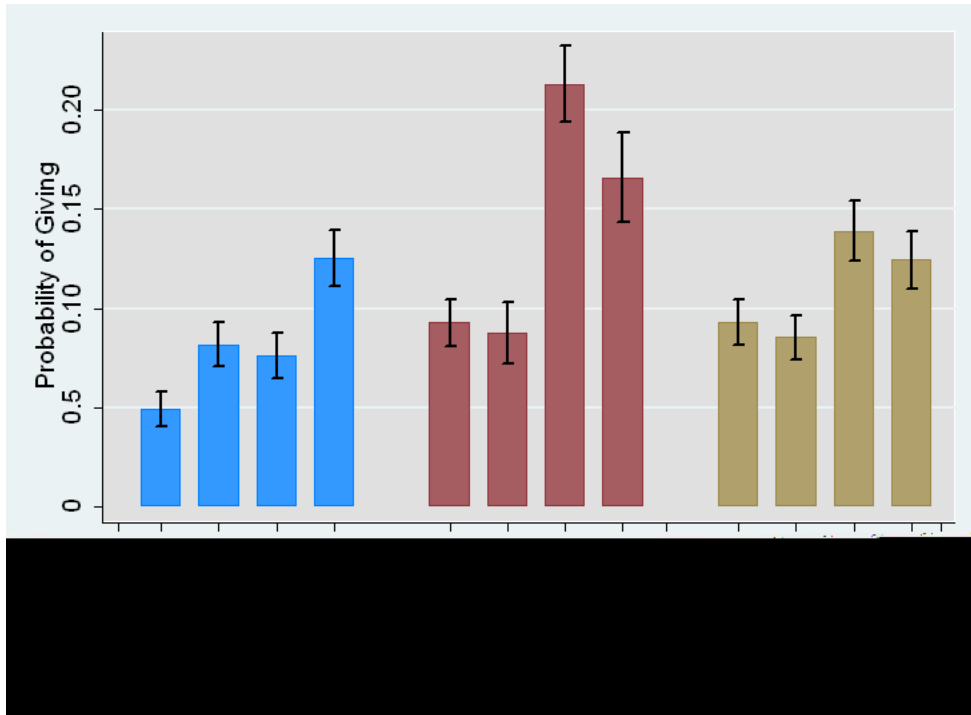


Figure 3: Rates of giving by condition. Left Panel: Conditional on arriving at the store. Center Panel: Conditional on those people who actually entered Door 1. Right Panel: Conditional on Door 1 entrants plus Door 1 avoiders, that is adjusted for negative sorting. Bars give 1.96 standard errors, as given by Table 3.

chose to walk through Door 1.

In Panel 3 we try to give an accurate picture of Door 1 giving rates accounting for avoidance. This will allow use to assess whether there was positive sorting or not in the face of an ask. To do so, we need to adjust the denominator to reflect the loss in traffic due to avoidance and adjust the numerator to account for the loss in giving due to avoidance. We collect the necessary data to do so in Table 5. Columns (1) and (2) of Table 5 present giving and recorded traffic for *Ask1&2*. Column (3) adjusts Door 1 traffic using the estimates from Table 3. Column (4) shows the unadjusted frequency of giving. The rate is 0.165 for Door 1 and 0.134 for Door 2. In Column (5), we compute the rate of giving assuming that negative sorters never give. In this case, the rate of giving at Door 1 is actually less than Door 2: 0.122 vs. 0.134.

To compute the latent giving rate of avoiders for the ask conditions, we exploit the fact that there was significant Door 1 avoidance in *Ask1&2*, as people who would have entered Door 1 chose Door 3 instead, but little avoidance of Door 2, as shown in Table 2 Column 3 (these shoppers were basically roadblocked, which is further confirmed by the aerial photo shown in Figure 1). This means that Door 2 includes would-be avoiders, whereas Door 1 does not (or includes much fewer).

By assuming that giving rates would have otherwise been equal between Door 1 and Door 2, we can get an estimate of the giving rate of avoiders.⁶ The giving rate of avoiders needed to make the giving rates equal across doors can be easily calculated to be 0.057, or 5.7%, which is less than those who chose to enter Door 1.⁷ The evidence seems to rule out avoiders being particularly likely to give, indicating that the "avoid saying no" effect dominates the "avoid giving" effect.

Table 5: Calibrating the Counterfactual Traffic and Giving Rates in Ask1&2.

	Observed Givers (1)	Traffic Unadjusted (2)	Traffic Adjusted* (3)	Frequency of Giving Unadjusted (4)	Frequency of Giving Adjusted for Avoiders** (5)
Door 1	159	959	1299.5	0.165	0.122
Door 2	145	1083	1083	0.134	0.134

* These calculations are taken from Table 2 and the regressions in Table 3.

** This counts avoiders as using their intended entrance but not giving.

We are now in position to calculate the degree of *positive* sorting, that is, those who go out of their way to make a donation. In *Ask1*, the Door 1 giving rate of actual entrants was $184/864 = 0.212$ and in *Ask1&2* it was $159/959 = 0.165$, which significantly differ from each other. Note, there were more givers in *Ask1* despite there being less traffic, consistent with some degree of positive sorting. However, if we adjust the denominator to account for avoidance and adjust the numerator to account for the estimated fraction of avoiders who would have given using the estimates from Table 2 and Table 4 respectively, we get a point estimate of 0.139 for the conditional Door 1 giving rate in *Ask1* and 0.122 in *Ask1&2*. The results are shown graphically in right panel of Figure 4. The adjusted rates for the opportunity conditions are given as well, which are quite similar to Panel 2, due to the limited negative sorting in these conditions. However for the ask conditions, the imputed Door 1 giving rates are much lower than the raw giving rates. This is because the raw figures implicitly assume avoiders give at the same rate as those who choose to enter and since the empirical estimates indicate that would-be avoiders give at a lower rate, the adjusted giving rates naturally fall. The difference between *Ask1&2* and *Ask1* implies 1.46 percent of people are positive sorters in the presence of a verbal greeting and request, which is double the 0.74% estimate of positive sorting in the Opportunity conditions. It is difficult to devise the appropriate statistic to test the significance of this difference, given the multiple steps in calculation, but even using the best case scenario in terms of precision (Figure 2, Panel 3, right-most bars), there is still significant

⁶There are a few important ways in which this assumption could be violated. We find it unlikely that more generous people happen to park near Door 1. Also, since the same solicitor always occupied Door 1, if that solicitor was always more (or less) effective, it lead to data that would appear to violate the assumption of equal propensities to give at the two doors. Hence, we are comforted by the fact that the opportunity conditions showed no differences across doors (Table 4 Panel 2).

⁷Based on the calculation giving rate of avoiders= $[(1299.5 - 1083)/1299.5] \times (0.134 - 0.122) = 0.057$.

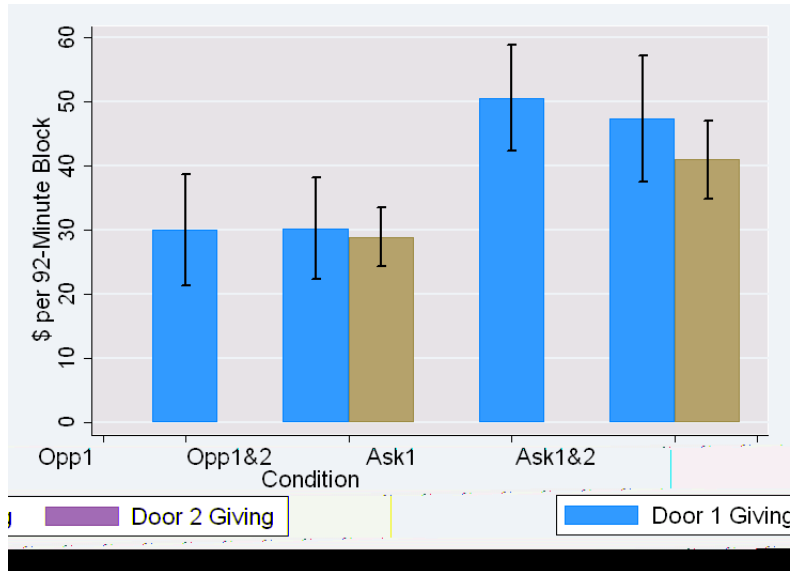


Figure 4: Mean donations per 92-minute block, by condition and door. Bars give 1.96 standard errors, as given by Column (4) of Table 3.

overlap in the confidence intervals.⁸ Thus our conclusion is that the evidence provides support for a small degree of positive sorting in the presence of an ask, and scarcely measurable positive sorting in the opportunity conditions. This also suggests the large differences shown in the center panel of Figure 3 were the result of an implicit assumption about the giving rates of avoiders that do not appear to be supported by a more careful treatment of the data.

3.3 Levels of Giving

Figure 4 presents average donations per 92-minute block by door and condition. The results are consistent with the findings in Figure 3. In the opportunity conditions, Door 1 giving per 92-minute block differs by only 23 cents: \$30.20 for Opp1 and \$29.97 for Opp1&2. This is evidence against positive sorting. Placing two bell ringers at the very same store appears to be just as good as distributing them to different supermarkets.

In Ask1&2, the presence of second solicitor lifts total contributions from \$50.60 to \$88.37, a 75 percent increase. Examining Door 1 giving, we see a similar increase in collected donations to match the slight increase in the propensity to give, however the statistical significance is lower due to the coarser unit of observation. As we saw in Figure 2, the power of the ask is evident. Asking conditions outstripped their silent counterparts by about 60 percent on average and these differences are highly significant (*Ask1* vs. *Opp1* $F(1,6) = 19.44, p < 0.0045$; *Ask1&2* vs. *Opp1&2*

⁸These error bars assume independence across people and that the adjustments were made perfectly.

Table 6: OLS Regressions of Donations on Conditions

Dependant Variable:	Donations per Block (\$)
Opportunity at Door 1 (Opp1)	-29.11*** (5.117)
Ask at Door 1 (Ask1)	-8.48* (4.139)
Ask at Doors 1 & 2 (Ask1&2)	29.29*** (5.392)
Mean of Omitted Treatment (Opp1&2)	59.09
Day & Time Fixed Effects	yes
Observations	16
R^2	0.961

Note 1: Standard errors clustered by block in parentheses.

Note 2: Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

$F(1, 6) = 29.50, p < 0.0016$). We can see these effects in Table 6 where we regress donations per block on the treatment variables, and day and time fixed effects.

4 Asking, Avoiding, and Social Welfare

In this field study we observed people passing 17,662 times through a supermarket's entrances over a four day period. Of these, 5812 passed a solicitor who made a polite and simple request to "please give," and 8.4 percent of them did so. The average contribution of those who were asked and gave was \$1.69. Yet, despite the small financial costs of giving, the fraction avoiding being asked was dramatic, as high as one third of patrons who would otherwise have chosen Door 1 in *Ask1*.

This naturally raises the question of what such fundraising's effect on utility is and what the social costs could be. If people are avoiding an opportunity to give, they must feel better off in the process, hence the physical and psychic costs of avoidance would appear to be pure deadweight loss. In this section we argue that such calculations, if possible, depend deeply on the assumptions one makes on the motives and intent individuals have in avoiding the ask.

4.1 Do People Lack Altruism?

One natural interpretation of avoiding the ask (and the minimal-to-no positive sorting) is that giving is not determined by altruistic feelings but instead by selfish or social goals, such as appearing generous in the eyes of the solicitor or other patrons of the store, or perhaps in maintaining a self-

image as a generous person. Thus, we would look to ideas of social- or self-signaling to explain avoidance.

One model of this is provided by Andreoni and Bernheim (2009). In this model, individuals care (that is, are altruistic or have a high warm-glow of giving) about matching social expectations, although the "higher" types care more than others. In addition, people care about what others believe their type to be| everyone wants others to think they are a high type. Andreoni and Bernheim show, both theoretically and experimentally, that such tastes can result in a signaling equilibrium in which one pool forms at full compliance by higher types, another pool forms at complete noncompliance by lower types, with a center group revealing their true types with partial compliance. The interesting twist in the Andreoni-Bernheim approach is to add noise to the signal value of being non-compliant. For instance, suppose there is an idiosyncratic cost of compliance with the norm that is known only to the potential donor. When this is added, many more people pool with the non-compliant.

Our experiment has many parallels to this model. It is more credible that a busy shopper didn't notice the silent solicitation than that a direct ask was not heard. Likewise, when only Door 1 has a solicitor, it becomes more credible that another door was used innocently, and not as avoidance of the ask. Hence, the easier is the avoidance, 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 a solicitation is less than the gain in utility from both warm-glow and social-image, may actually go out of their ways to give.

Some of the behavior observed in this field experiment could be explained with a signaling model, with avoidance of the solicitation indicating low types in the population. However, the lack of those seeking solicitation undermines the argument that high types will desire to signal, although it could be that avoidance costs were so low that only one the lower pool was supported in equilibrium. The bigger challenge to the signaling explanation, however, comes with the significant effect of the ask. The fact of being observed giving, either by others or by oneself, is the same across the Ask and Opportunity conditions, yet giving and avoiding were greatly affected by the ask.

4.2 Do People Have Too Much Altruism?

The grocery store where we conducted our study at was in suburban Boston, with hundreds of individuals passing through each hour. While social pressures are surely present even in such largely anonymous circumstances, it is worth entertaining the notion that the choice to give or avoid is as a simple individual decision problem devoid of overt social persuasion.

Psychological models of altruism begin with a concept that economists are only beginning to discuss: empathy. The act of giving is a struggle between empathy and executive function, that is, between the pull of the heartstrings and the draw on the family budget. Why, for instance, does one avert the gaze of a beggar? The psychologists' answer would be that a direct look will

stimulate an empathic response in the brain, making the altruistic act harder to resist. Like the children in Walter Mischel's famous self-control tasks who successfully avoided eating the tempting marshmallow by turning their backs (and thus reducing the emotional, but not cognitive awareness of the temptations) (Mischel et al., 1989), it is distinctly possible that our subjects are avoiding the ask in order to turn away from the emotional stimulus that makes it easier to keep one's empathy from being engaged, and thus easier to resist the ultimate ask.

This pathway to avoidance also has precedence in economic laboratory experiments. Andreoni and Rao (2011) asked subjects to play Dictator games with controlled degrees of communication. When "receivers" could ask "dictators" for a share, keeping dictators silent, the receivers tended to get what they asked for. By contrast, when dictators could explain what they chose, keeping receivers silent, dictators nearly always gave nothing and offered an apology. However, in a condition designed to heighten empathy, we required all players to make decisions as a recipient (and ask) and as a dictators (and explain) but were told that their true roles (just one of the two decisions) 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 avoidant-apology of the explain.

What if someone with high empathy were to pass a silent solicitation by a Red Kettle bell-ringer? Would they have the strength to resist? Perhaps not. Then, like Mischel's eight-year-olds, they can turn their gaze from the luring power of empathy, look straight ahead and walk on by. However, when the solicitor or making a verbal request and attempting eye contact, this level of avoidance is defeated or made more difficult due to the social norm of acknowledging a request, leaving only two options: pass and give or feel guilty, or use another door.

4.3 Can we Measure the Welfare Effects?

Our study indicates that 32.5 percent of patrons of the supermarket prefer to avoid being asked by exiting a less convenient door, 13.9 percent prefer to be asked and to give rather than exit a less convenient door, and 53.6 percent prefer exit the most convenient door, to be asked, and to refuse to give. In addition, almost half of the donors would not have given without the ask. What can this behavior tell us about the welfare effects of fundraising?

In a related paper, DellaVigna et al. (2011) do a nice 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 then apply the estimates to the avoidance of charitable solicitation, resulting in a calculation of the welfare consequences of an "opt out" policy.

Our design could not accommodate pricing time and avoidance in this way. However, we can put upper and lower bounds on the welfare cost of solicitation. For the lower bound, we assume that "saying no" and giving both come at a lower cost than avoidance (since these are potentially different types of people, it is not at all obvious this should be true). We further assume the

lower bound on avoidance is the time cost. Given that that average American's speed of walking is about 4.4 feet per second,⁹ avoiding to Door 3 required about 70 feet of extra walking, and that average wages for this area of town is \$18 per hour, the cost of avoidance per person can be roughly estimated to be \$0.10. Through a simple revealed preference argument, this corresponds to the maximum bound for those that did not avoid and chose not to give, and should equal that of the "marginal non-avoider."¹⁰ If we assume, for simplicity "saying no" costs of non-avoiders are uniformly distributed between 0 and \$0.10 (so mean costs are \$0.05 for non-avoiders and \$0.10 for avoiders and 0 for givers), we get a lower bound of \$0.07 for *Ask 1\$2* (if we only consider time costs, the bound is \$0.03).

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. It's also defensible to assume that the cost of "saying no" is less than \$1.69 for the non-givers.¹¹ The maximum loss is thus \$1.69, assuming no warm-glow or private benefit from giving, and no social benefit from signaling.

Ask 1\$2 raised \$0.15 per-person. We have identified the solicitation cost to be between \$0.07 and \$1.69. 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 fundraising is clearly 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 11.1 (that is, $1.69/0.15$) times more to recipients at the margin, then fundraising is clearly welfare enhancing.

As is obvious from this exercise, making precise welfare statements is quite delicate{our bounds are quite sensitive to a number of assumptions. We view welfare effects as interesting an important question, but one that is fundamentally difficult to answer.¹²

5 Conclusion

We study how avoidance and giving respond to the presence of the annual Salvation Army bell ringers at the doors of a large supermarket in suburban Boston. We find first, shoppers do little to avoid the bell ringers who do not verbally engage or make eye contact with them and only a tiny fraction appear to seek the solicitor by walking a few paces in order to give. In contrast, the simple

⁹We got this figure from Wikipedia.

¹⁰There are many reasons to view the lower bound as implausible. For instance, using 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).

¹¹It'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).

¹²If one takes these stance that guilt and other psychological costs should not enter welfare calculations, then the lower bound can be used, which endorses solicitation with verbal requests, since it is the most efficient way to raise money. Since avoidance was low in the opportunity conditions, one could argue that people did not face a cost of not giving (since they did not have to say no), so one might endorse this method of solicitation.

act of looking at shoppers and saying "please give today" causes over 30 percent of shoppers to avoid the ask, but increases average donations per giver by 75 percent. Asking, it seems, is both aversive and effective. Notice that static models of giving, such as inequity aversion, would predict high a degree of solicitor *seeking*. Furthermore, these theories are silent on the impact requests on either giving or avoidance. In contrast to this view, we argue that our results show that human altruism is driven by many proximate social cues and psychological factors that have, thus far, been largely unexplored by economists but are the likely drivers of the rather extreme behavior observed in our field study.

The key question raised is, why is asking so powerful? One answer operates through self-interest; asking might provide an opportunity for self- or social-signaling. While there is clear evidence that signaling concerns motivate giving elsewhere (Andreoni and Bernheim, 2009; Frey and Meier, 2004), in this circumstance it seems that the lack of positive sorting toward solicitors, and the huge differential effect of a verbal request on rates and amounts of giving cannot be fully captured by this story. Another hypothesis is simple social pressure. However, the interaction here is brief—only a second or two—and easy to avoid simply by keeping one's head down and walking by or politely declining. Thus, the external pressure and expectations to give seem minimal. This leaves the possibility of some pressure that comes from within the donor. The main psychological feature implicated here is empathy. Just as the smell of freshly baked bread can make it hard for a dieter to resist eating, stimulating one's empathy through a direct and vocal ask can create a temptation to be generous that is costly for humans to resist. While nothing in our data allows us to directly test any of these explanations, we feel our results usefully shift the discussion of altruism, fundraising, and charitable giving to focus on the act of asking itself as the linchpin to understanding both the costs and benefits of the giving interaction.

We feel our paper serves a useful methodological purpose as well. Allowing our Salvation Army solicitors to ask someone to give to charity openly and directly is a different "frame" than offering a silent opportunity to give, 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. 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. Although differing in scope and magnitude, the patterns of results in both the field and lab are quite similar. Thus, rather than creating an artificial experimenter demand effect, the lab could instead be seen as providing an informative parallel to real-world giving.

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6 Appendix

A1. Alternate Specification for Inferring Door 3 Traffic

Imputed Traffic, Average of Ask1 and Ask1&2 Used as Baseline

Total Passings	4,682	4,847	4,765	4,765	19,059
Door 3 Increase	0	0	681	716	1,397
