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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 reflexes to give.

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1 Introduction

It is beyond debate to 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 selfishness, 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 finding provided inspiration to new theories of social preferences, such as "inequity aversion" (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), the idea that 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 diminish sharply (Andreoni and Bernheim, 2009; Ariely et al., 2009), implying that maintaining a positive social-image is a central motive behind mending inequality. These results suggest that giving is situational—it is difficult to explain with static preferences alone. That is, when a giving context is imposed upon people they give, but they typically do not seek out giving opportunities and may even prefer to manipulate the context observed by others by taking steps to avoid being placed in the position of "giver" in the first place. 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, once you have been asked, is itself deleterious. Rather, this behavior would suggest people don't actually avoid *giving*, but they are avoiding being *asked to give*. If asking to give affects people's behavior such that they avoid being asked, then we are left to conjecture about why people would avoid being asked, and what can we infer about the welfare effects of asking. As we argue below, there could be reasons to expect the cost is noteworthy, and other reasons to suggest it may be trivial.

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 by saying "please give today," 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.

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—a short, scripted request in the field increased giving 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. This effect seemingly cannot be explained by increased awareness or information: the bell ringing ensures that the potential giver is made aware of the solicitor, and "please give today" does not convey any information. Social- or self-image concerns also struggle to explain the power of asking—in a formal model, positive image can be burnished equally well in both a silent opportunity and an active request, unless it is truly the case that one can "not notice" the solicitor in the opportunity conditions, that is, the implied ask allows for some plausible deniability. Our results point to the need for deeper thinking about the role of asking in the psychological process that triggers one to give.

One possible mechanism that might drive the difference between the verbal ask and the silent opportunity is the degree of sensory, and perhaps empathic, stimulation. In the verbal ask, we introduce this stimulation in the form of brief eye contact with a solicitor who says "please give today." 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 (if they knew they would give) or guilt (if they knew they would not). And very few people give in the absence of empathic stimuli, which could explain the lack of positive sorting.[1]

Our results have an additional methodological implication that helps sew together questions

[1]This explanation has much in common with theory of cue-triggered choices of Bernheim and Rangal (2004) and the will power depletion model of Ozdenoren et al. (2011) .

raised about the use of dictator games in a laboratory. Recently, authors have expressed concerns 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 fidelity of laboratory studies (Levitt and List, 2007; List, 2007; Bardsley, 2008). Our work shows that the "demand effect" itself may be a critical part of the giving interaction. 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. Just as laboratory dictator games convert a pair of strangers to "givers" and "recipients," the Salvation Army places a solicitor in the path of a shopper to create a context of giving that otherwise probably would not have existed. In many cases, so-called "experimenter demand effects" might be better referred to as "experimenter-cues" that elicit natural mechanisms at work in real-world giving situations.

These findings inspire important questions about the psychological mechanisms involved in giving and and the welfare impact of fundraising. With regards to the psychological mechanisms, what this or other research on this area cannot answer directly is whether avoiding being asked reveals a lack altruism. For example, we argue in the Section 4 that the behavior we observe is consistent with a model in which individuals avoid being asked as a self-regulation mechanism to guard *against* (excessive) altruism. Yet we show that one could argue that the behavior is also consistent with the natural reaction that avoidance is a sign of deficient altruism; this explanation posits that giving is a result of social image concerns and thus requires differential social signaling contexts of the opportunity versus ask conditions. Furthermore, 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] With regards to fundraising: Does fundraising create an uncomfortable social pressure that is bad for society? Although we cannot definitively answer these questions, in the discussion section we flesh out how our field experiment provides important clues and present our thoughts on future work.

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

[2]Authors in the neuroscience literature have argued in favor of this view. See for instance Singer and Decety (2006) and Hare et al. (2010). Note also, the empathic feelings we mention here do not rule out avoiding feelings of guilt. Guilt at not giving may, we conjecture, be part of the same mechanism and could be a product of sympathy with the cause colliding with an attempt to moderate giving.

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 the left and Door 2 on the right, both opened in the direction of the main parking lot. As identified in the figure, the 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 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 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,

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

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]

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

[3]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 (although no longer 22, all are still white). 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.

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

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 that 1-door and 2-door treatments had to be interspersed throughout the day, making it possible for a shopper to enter during a 2- door treatment and exit 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]

This paper is most closely related to recent research by DellaVigna, List, and Melmendier (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 was to evaluate 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. Since the authors' main objective was to evaluate the welfare impact, it does not per se matter if giving increases due to positive sorting, because of increased verifiability, 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 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.

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

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.

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 observed 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. If we look at total passings under Ask versus under Opportunity conditions (combining both door treatments), 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 and on day and time fixed effects. The omitted condition is *Opp1&2*. In column (1), we see that *Opp1* had 10.3 more passings than *Opp1&2*, indicating some seeking of solicitors, but the effect is not significant. Door 1 and 2 traffic (total observed) shows a sharp and highly significant decrease in both ask conditions. As compared to *Opp1&2*, *Ask1&2* and *Ask1* had 39.6 and 37.4 fewer counted passings per 23-minute session, respectively. People were also much more likely to use the third door in the presence of verbal solicitation. The fact that total traffic decreases more under *Ask1* than it does under *Ask1&2* implies that avoidance to Door 3 is entirely driven by people who would have otherwise used Door 1; this is confirmed in column (3), which shows that Door 2 traffic did not decrease in *Ask1&2*. Avoiding the solicitor at Door 2 was the most difficult for two reasons. First, a solicitor was only at Door 2 when there was one at Door 1 as well. Secondly, Door 3 was

Table 2: Store Traffic

	Asking Condition				
	Silent Opportunity		**Direct Ask**		
	Doors with Solicitors		Doors with Solicitors		
	Door 1 (Opp1)	Doors 1&2 (Opp1&2)	Door 1 (Ask1)	Doors 1&2 (Ask1&2)	Total
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%	

not visible from the parking lot facing Door 2 and reaching it from Door 2 required walking past Door 1 (or very far around through the parking lot), thus making it difficult to discreetly avoid. Column (3) also 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 affects 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 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 percent 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 solic-

Table 3: OLS Regressions of Traffic on Conditions

	Traffic per Session		
Dependent Variable	Total Traffic	Door 1	Door 2
Opportunity at Door 1	10.31	3.438	6.875
(Opp1)	(12.71)	(10.34)	(7.958)
Ask at Door 1	-39.56***	-48.75***	9.188**
(Ask1)	(12.73)	(11.33)	(3.821)
Ask at Doors 1 & 2	-37.37***	-36.87***	-0.500
(Ask1&2)	(11.62)	(8.969)	(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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

itation in *Ask1* and *Ask1&2*. However these figures tend to understate the true level of avoidance in the population because of the difficulty of avoidance for Door 2 shopper. In Table 2 we see that Door 2 does not show a drop in traffic in *Ask1&2* (2166) relative to *Opp1&2* (2174). This implies that there was no avoidance of Door 2 when the solicitor there applied the verbal ask. Moreover, the Door 3 increase is not larger under *Ask1&2* relative to *Ask1*, which implies that the avoidance to Door 3 does not come from potential Door 2 users. Both of these facts suggest that the avoidance through Door 3 comes from potential Door 1 users. If we Isolate the traffic that would, without solicitations, have preferred to use Door 1, we can more precisely measure avoidance.[6] We do so using the traffic in *Opp1* to give the base rate for Door 1 as shown in Table 3, Column 2. The results are displayed graphically 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 so disliked being asked to give 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* $F(1,15) =$

[6]Since Door 3 was not visible from the Door 2 area, some patrons were likely unaware of any possibility to avoid. Patrons familiar with the store would have been aware of the avoidance opportunity. The fact that we do observable avoidance in this case indicates that the desire to avoid is not strong enough to motivate a large inconvenience cost or that avoidance doesn't work when one is acutely aware of it, such as would be the case when circumnavigating the parking lot.

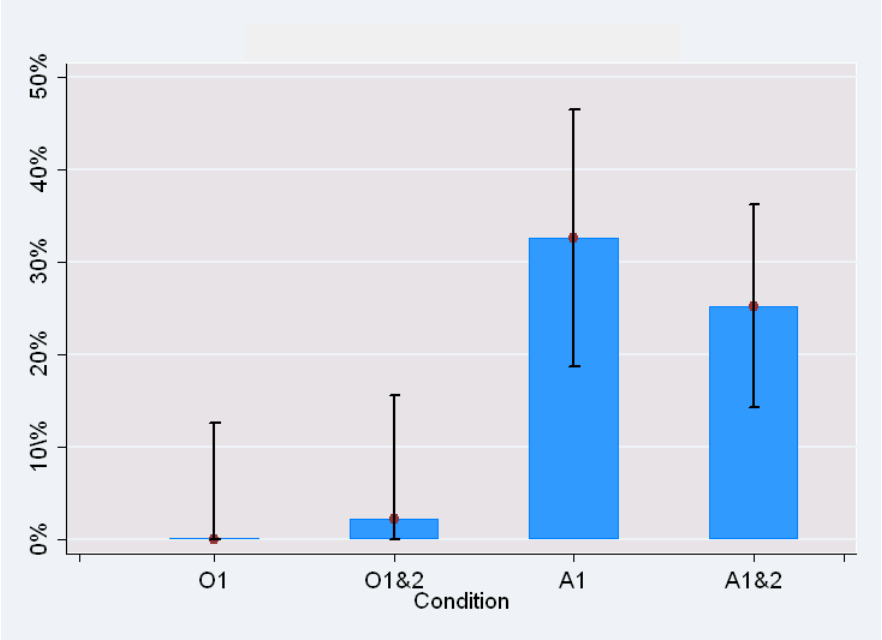

Figure 2: Door 1 Avoidance by Condition

$16.90, p < 0.0009$), 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 total 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 those shoppers that chose to enter Door 1. In the right-panel we present our estimates of what Door 1 giving rates would have been accounting for avoidance (intuitively avoidance artificially increases the giving rate, even in the absence of seeking, because it lowers the denominator). We explain our adjustment procedure in detail below.

Looking first at the left panel, two-results are immediately clear. First, having 2 solicitors greatly increases the 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 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 que to social-image, then we might expect a larger difference between giving at Door 1 for the Ask treatments then 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

Table 4: Numbers and Rates of Giving, by Condition

	Silent Opportunity		**Direct Ask**		
	Doors with Solicitors		Doors with Solicitors		
	Door 1 (Opp1)	Doors 1&2 (Opp1&2)	Door 1 (Ask1)	Doors 1&2 (Ask1&2)	Total
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%	

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 chose to walk through Door 1.

In Panel 3 we try to give an accurate picture of Door 1 giving rates accounting for avoidance, which will allow us 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 what avoiders would have done had they been unable to avoid. 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. 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.[7]

We collect the necessary data 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

[7]There 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).

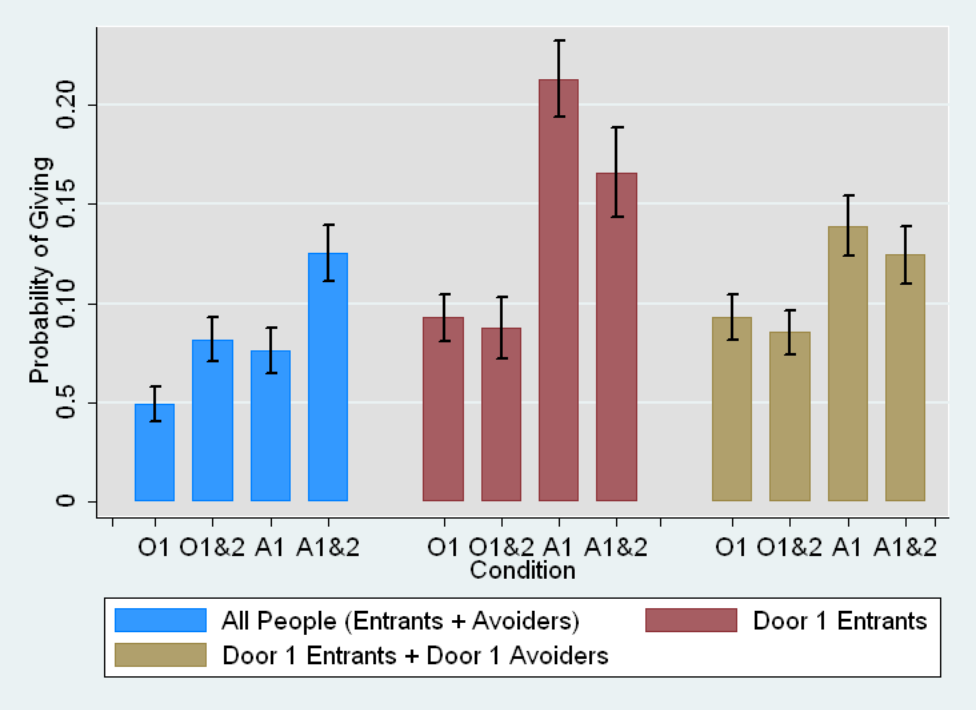

Figure 3: Rates of giving by condition. Left Panel: Conditional on arriving at the store. Center Panel: Conditional on those people who actually entered Door 1. Right Panel: Conditional on Door 1 entrants plus Door 1 avoiders, that is adjusted for negative sorting. Bars give 1.96 standard errors, as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

in order to account for Door 1 avoidance (recall there was not Door 2 avoidance). Column (4) shows the unadjusted frequency of giving, that is the frequency of giving for those people who actually entered the respective door. In this case, the rate of giving is 0.165 for Door 1 and 0.134 for Door 2. The higher rate of giving at Door 1 reflects an artificially low denominator due to some shoppers sorting out. In Column (5) we add these avoiders back in (so that the denominator is now accurate) and compute the rate of giving assuming that the negative sorters never give. Under this assumption, the rate of giving at Door 1 is actually less than Door 2: 0.122 vs. 0.134. To build intuition, we are trying to identify the "rate of giving of avoiders" by assuming that Door 1 and 2 would otherwise show the same giving rates (in *Ask1&2*) if avoidance at Door 1 was not possible. If after adding avoiders back to the denominator the giving rates were equal, this would mean that avoiders never give. In actuality, when we add avoiders back in, Door 1 does worse than Door 2, indicating some avoiders do give. The giving rate of avoiders is calculated as the fraction of avoiders that must give to render the giving rates equal across Door 1 and Door 2. This can be easily calculated to be 5.7 percent.[8] Giving by avoiders is significantly less than the percentage of

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

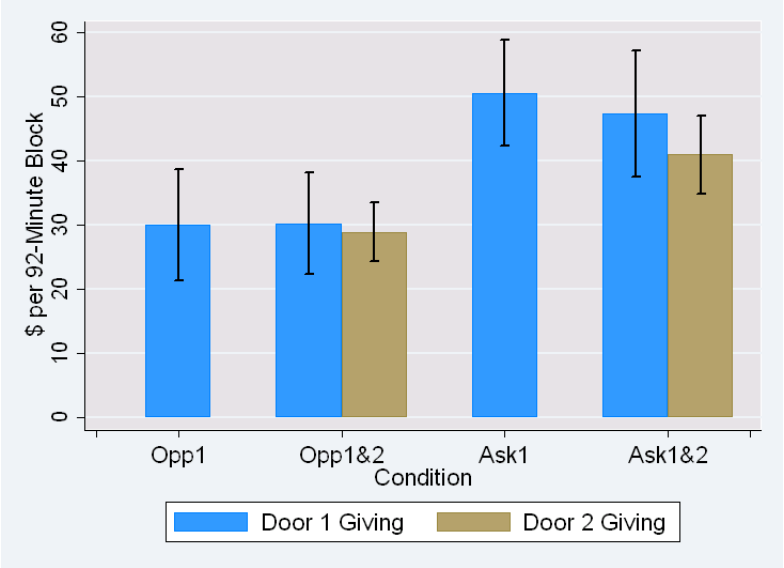

Figure 4: Mean donations per 92-minute block, by condition and door. Bars give 1.96 standard errors, as given by Column (4) of Table 3.

givers for shopppers who do not avoid.

The evidence seems to rule out avoiders being particularly likely to give, indicating that the "avoid saying no" effect dominates the "avoid giving" effect. The intuition for the estimate is that in Door 1, we have only non-avoiders, and giving rate is higher. This means that the capturing the avoiders at Door 2 lowers the the giving rate, meaning avoiders are not particularly likely to give—thus the "avoid saying no" effect drives most avoidance.

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

	Observed Givers	Traffic Unadjusted	Traffic Adjusted*	Frequency of Giving Unadjusted	Frequency of Giving Adjusted for Avoiders**
	(1)	(2)	(3)	(4)	(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 percent 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 overlap in the confidence intervals.[9] 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* $F(1,6) = 29.50, p < 0.0016$). We can see these effects in Table 6 where we regress donations per block on the treatment variables, and day and time fixed effects.

[9] These 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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 regarding the motives that drive an individual to avoid 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. 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 on giving by non-avoiders. This explanation requires that the opportunity conditions impose an idiosyncratic compliance cost in the form of "noticing." If people can credibly claim they did not notice the silent opportunity but cannot do so for a verbal request, this means they will be judged less harshly for not giving in the opportunity setting (in equilibrium) and more people will pool with the non-compliant. The strength of this explanation relies on the plausibility of (legitimately) not noticing the bell-ringing (but otherwise silent) solicitor.

4.2 You Cannot Give Every Time: Mediating 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 or by heightening the

quilt associated with doing so. Like the children in Walter Michel'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 prescience 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 recipients (and ask) and as 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 would-be apologetic explain—dictators were far more generous and the messages were more likely to center around fairness just by thinking of what they would themselves ask for.

What if someone with high empathy were to pass a silent solicitation by a Red Kettle bell-ringer? Would they have the strength or willpower to resist? Perhaps not. Then, like Michel'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: 1) pass and give 2) feel guilty 3) 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 using a less convenient door, and 53.6 percent prefer to use 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 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 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. In this section we construct "reasonable" bounds on the welfare impact of solicitation. We'll focus on the most

effective (and intrusive) solicitation condition (*Ask1&2*) For the lower bound, we first 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. We next assume that we can price avoidance through time-cost alone. That is, we zero out any related costs that may motivate avoidance in the first place. Given that the average American's speed of walking is about 4.4 feet per second,[10] 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 time-cost of avoidance per-person can be roughly estimated to be $0.10. This value should equal the cost of "saying no" for a person on the margin between avoiding and entering and refusing (the "marginal non-avoider").[11] If we assume that the "saying no" costs of non-avoiders are uniformly distributed between 0 and $0.10, then the mean cost to a non-avoider is $0.05. For avoiders the cost is $0.10 and we further assume that givers' break-even (since our sorting results show that vast majority of givers would have rather not given, we view the zero-cost to givers assumption as consistent with the lower-bound methodology).[12] 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.

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.[13] 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 either giving and refusing or 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 lost to customers assuming no warm-glow or private benefit from giving, and no social benefit from signaling.

Ask1&2 raised $0.15 per shopper. We have identified $0.07 and $0.72 as reasonable 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 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 4.8 (that is, 0.72/0.15) times more to recipients at the margin, then fundraising is clearly welfare enhancing.

As is clear from this exercise, making precise welfare statements is quite delicate—our bounds

[10] We got this figure from Wikipedia.

[11] There 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).

[12] The marginal giver clearly suffers a utility loss, however.

[13] 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).

are sensitive to a number of assumptions. We view welfare effects as interesting an important question, but one that is fundamentally difficult to answer. However, our methodology lets one take a stand on the issue as long as one is comfortable specifying the ratio of marginal utility between recipients and donors and outlining which costs should count in welfare calculations.[14]

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 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 is inequity aversion, would predict high a degree of solicitor *seeking*. Furthermore, these theories are silent on the impact of 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 can only be fully captured by this story if it is plausible that one could not notice a solicitor loudly ringing a bell in the opportunity conditions, but this sort of "not noticing" is not possible in the ask conditions. Another hypothesis is simple social pressure. However, the interaction here is brief—only a second or two—and easy to avoid simply be 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 difficult 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

[14]For instance, if one takes the stance that guilt and other psychological costs should not enter welfare calculations, then the lower bound we calculate 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.

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