Silence is Golden.
Suggested Donations in Voluntary Contribution Games

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Abstract

We report on the results of laboratory experiments of voluntary giving with suggested donations. Asking subjects to give the socially optimal amount *reduces* giving compared to no suggestion. Asking subjects not to give also depresses giving. However, moderate suggested donations produce modest positive effects. We offer a model using the fairness equilibrium framework of Rabin (1993) that is capable of rationalizing some of these results.

Keywords: Voluntary contributions, suggested donations, fairness

1 Introduction

In addition to being one of the greatest art museums in the world, New York's Metropolitan Museum of Art is also an exceptional value. Admission to the museum is free. Visitors are, however, asked to make a "suggested donation" of $20, an amount that was increased recently from its previous $15 level. Many visitors pay the suggested donation amount and, indeed, the museum derives considerable income from this source, over $29 million in 2009.[1] Suggested donations are a key tool in the arsenal of strategies used by various charities in fund-raising. Considerable thought and debate goes into selecting the right amount to suggest in fund-raising solicitations.

Given their widespread use, it stands to reason that suggested donations must be an effective means of increasing giving. However, there is scant evidence for this. Warwick (2005) offers examples based on his experiences where a lower suggested donation amount produced more giving. Smith and Weyant (2006) conduct field experiments and observe a similar effect. Karlan and List (2007) conduct field experiments which include suggested donation treatments but report no significant effects. Unlike the Metropolitan Museum, suggested donations in their treatments referenced previous giving rather than specifying an exact amount. Marks, Schansberg, and Croson (2003) conduct laboratory experiments and find some positive effect from suggested donations when contributors have differing payoffs from giving. In their study, the charity (credibly) commits to a *provision point mechanism* whereby, if it did not raise sufficient funds up to a specified level, it would return all donations to contributors. Such mechanisms are unusual in practice.

In light of this limited evidence, one might turn to theory as a guide for how suggested donations could possibly help. When individuals have purely pecuniary motives, voluntary contributions suffer from the familiar free-rider problem. Indeed, in the simplest possible specification, the problem is isomorphic to a prisoner's dilemma. Adding "cheap talk" in the form of a suggested donation on top of this game should, in theory, have no effect whatsoever.

Of course, a vast number of studies have shown that individuals do contribute to public goods despite these pecuniary incentives (see, e.g., Ledyard, 1995). This has led to a growing

[1] Metropolitan Museum of Art, 2009 Annual Report.

theoretical literature postulating models where non-pecuniary incentives play a key role. For example, Rabin and Charness (2002) offer a simple model incorporating a variety of non-pecuniary preferences that is capable of rationalizing many of the "puzzles" observed in laboratory experiments. One version of that model has the effect of transforming voluntary giving into a coordination game whereby individuals reciprocate giving behavior and punish non-giving behavior by also withholding contributions. In principle, a suggested donation might make giving more focal and thereby improve contributions.[2] Perhaps the suggested donation serves as a priming effect and thereby shapes subsequent choice behavior. There is a vast literature in psychology establishing the importance of priming across a variety of decisions (see, e.g., Wegner and Bargh, 1998). However, so far as we are aware, the priming effect (if any) of suggested donation amounts has not been examined. The nearest antecedent is Seely, van Huyck, and Battalio (2005). They study non-binding "assignments" in four person repeated public goods games, focusing on *credible* assignments—assignments that are followed by subjects. They find that efficient assignments are rarely credible.

In this paper, we use laboratory experiments to study the effects of suggested donations on giving. Our first set of experiments looks at standard voluntary contribution games and compares various suggested donation amounts to a control where no amount is suggested. We find that asking individuals to contribute the socially optimal amount is, at best, ineffective. It produces no rise in initial giving and significantly more "donor fatigue" over the course of the experiment. In contrast, a more moderate suggested donation amount produces modest gains compared to the control. However, long-run giving (i.e. giving at the end of the experiment) is not improved.

In light of this perverse negative effect of suggesting the socially optimal contribution amount, we formulate a theory model based on Rabin's (1993) notion of fairness equilibrium and show that it is capable of rationalizing our findings. However, for tractability reasons, the theory model departs in various ways from the setting of the experiment. Moreover, it is merely a post hoc rationalization of our findings. For these reasons, we subsequently conducted a second set of experiments where subjects were paired and played a 15 period prisoner's dilemma. The model predicts that suggesting that subjects should cooperate

[2] Van Huyck, Gillette, and Battalio (1992) find this effect in laboratory experiments of coordination games.

should reduce cooperation, and this is exactly what we find. Compared to remaining silent, suggesting cooperation reduces cooperative behavior by 11.1%.

The theory also offers a highly counterintuitive prediction—suggesting that players *not cooperate* should produce more cooperation than silence. The underlying idea is that, by setting a low threshold for "nice" behavior, the non-cooperative suggestion maximizes the scope for generous behavior which improves outcomes. The theory, however, performs extremely poorly in this regard. Suggesting non-cooperation reduces cooperative behavior dramatically. Now, only 29.7% of choices are cooperative compared to 39.8% when cooperation was suggested and 50.9% when no suggestion was offered.

Our results may be best summarized by the title of the paper, when it comes to giving, at least in laboratory settings, silence is golden. At best, suggested donations can modestly and temporarily raise giving if set correctly. At worst, they can depress giving significantly. Moreover, the intuitively appealing suggestion of contributing to maximize social surplus turns out to always be worse than no suggestion whatsoever.

In terms of practice, our results offer a number of insights. First, consistent with the common intuition, the suggestion does have an effect on giving behavior though not always in the expected direction. Second, an "aggressive" ask can depress giving, sometimes by considerable amounts. Third, even seemingly sensible suggestions such as donating the amount which maximizes social surplus can turn out to be too aggressive. On the other hand, too low a suggestion can be harmful as well.

Perhaps the most important insight is that, often times making no suggestion is best. Outside of the context of charitable giving, the Radiohead pricing experiment shares many similarities with our silence is golden finding. In releasing its album *In Rainbows*, the band Radiohead engaged in an audacious pricing strategy. Rather than offering the album for sale at some fixed price, they made it available for download at their website and allowed users to pay what they wished with no suggested payment amount.[3] Radiohead sold more than 3 million copies of *In Rainbows* (in all formats), vastly more than their previous offerings and

[3]When a user placed the album in the checkout bin, there a question mark appeared where the price would normally be. Clicking the question mark produced the dialog box "It's up to you." Clicking it a second time produced the dialog box "It's really up to you."

made considerably more money as well. [4]

The remainder of the paper proceeds as follows: Section 2 describes and reports the results from our voluntary contributions experiments. Section 3 offers a fairness-based theory that rationalizes these results. Section 4 then tests the theory using repeated prisoner's dilemma experiments and reports the results. Section 5 highlights other aspects of giving including demographics. Finally, section 6 concludes.

2 Experiment 1: Suggested Donations

To examine the effect of suggested donations on giving, we conducted a standard voluntary contribution experiment. Our only amendment from the standard design was to display a suggested giving amount at the top of the screen for each individual. Under the control treatment, subjects received no message. Our two treatments were a message indicating that a subject should contribute 14 (out of a possible 20) tokens to the public fund and a message indicating that a subject should contribute 20 (i.e., all) tokens to the public fund. The message was the same for all subjects, and this was common knowledge. Moreover, subjects were told that the message was not, in any way, binding. They were free to contribute as much or as little as they wished to the public fund. Figure 1 displays a screenshot of the interface seen by subjects including the display of the suggested donation message. Our justification for choosing a 20 token treatment is to determine whether suggesting the socially efficient contribution amount would reduce free-riding behavior. The 14 token level was selected to see whether one could raise contribution levels by suggesting a higher level of giving than was obtained in the control treatment.

We now turn to the details of the design. We conducted nine sessions (three of each treatment) at Princeton University in the fall of 1998 using the voluntary contribution framework of Isaac and Walker (1988). Subjects were recruited via E-mail from the graduate and undergraduate population, and were told that they would earn "about $10" for participation in a decision-making study. Each session consisted of 12 subjects, seated at individual computer terminals. A set of instructions was distributed and read, subjects were given an

[4] See Tyrangiel (2007).Sales figures based on Radiohead press release of October 20, 2008.

opportunity to ask questions, and then the decision-making component of the experiment began. This portion of each session consisted of 20 rounds, during which communication among the subjects was not allowed and during which all decision and payoff information was transmitted and recorded via computer terminals.[5] The first five rounds of each session were designated as practice rounds, meaning that decisions made during these rounds would not affect earnings. Other than this special designation for the first five rounds, all rounds of a particular session were identical.

At the beginning of each round, subjects were randomly divided into three groups of 4 individuals and each was endowed with 20 tokens. Subjects did not know which of the other 11 participants were also in their group, and it was emphasized that group assignments were random and changed from one round to the next.[6] Each subject was asked to decide how many tokens from his 20 token endowment to allocate to a group account, with any remaining tokens allocated to a private account. Payoffs were calculated as follows: each token in the private account yielded 100 points to the subject, and each token in the group account yielded 75 points to each individual in that subject's group. After all subjects made their choices, they were informed of the total number of tokens placed in their group account and of the points gained from each account. During each round, each subject recorded his decisions as he made them; subjects also kept a record of their point earnings, which were calculated for them by the computer.

At the end of the session, a number from 6 to 20 was chosen randomly to designate the round which determined earnings. Subjects were paid according to the total points gained in this round at a rate of 3¢ for every 10 points earned, rounded up to the nearest multiple of 25¢. All sessions were completed in under 90 minutes, and earnings averaged $11.50 per subject.

[5]Code for the program was written in Microsoft Visual Basic 5.0 and is available from the authors.

[6]In fact, the process generating the groups was generated using the same random number seed for each session. Thus, all sessions used an identical algorithm for matching subjects into groups.

2.1 Results from Experiment 1

Table 1 summarizes the giving in the experiment. Pooling all sessions (excluding practice rounds), subjects made 1,620 individual giving decisions divided equally among the three treatments. As figure 1 shows, average giving under the control treatment was 8.97 tokens while average giving under the "14" treatment rose to 10.01. In contrast, average giving under the "20" treatment fell to 9.21. Thus, the subjects appeared to be modestly influenced (upwards) by the suggestion. Further evidence of this influence may be seen in the frequency with which subjects gave exactly the suggested amount. As the table shows, when we suggested that subjects give 14 tokens, subjects took our "advice" 11% of the time (compared to other treatments where giving 14 tokens was chosen 1-2% of the time). Likewise, suggesting to subjects that they give all of their tokens to the public goods produced this level of giving about 20% of the time (compared to 12-14% of the time in other treatments).

It is common to observe a (downward) trend in giving over the course of the experiment. Presumably, this is due to the fact that, as the endgame draws near, subjects are less inclined to be generous in hopes that this spurs giving in the future. While Table 1 offers some evidence of a downward trend in giving, Figure 2, which displays five round moving average giving for each treatment, highlights this pattern more clearly. As expected, the control displays a modest downward trend. Under the "14" treatment, the tendency to reduce giving over time seems to be more muted. In contrast, the "20" treatment displays a distinctly different pattern. Here, giving falls steeply from initially high levels. It appears that subjects suffer more from "donor fatigue" in this treatment than the others. Of course, both differences in average giving and trends are merely suggestive. There is considerable variability in individual behavior that is hidden from view in Table 1 and Figure 2. In the remainder of this section, we offer a number of more formal results and provide statistical support for each result.

One might expect that the main effect of the suggested donation message would occur at the beginning of the experiment. After all, at that point the message is novel and presumably most salient. In fact, we find no statistical treatment effect on giving in the first (real) round of the experiment.

Result 1: Suggested donations have no effect on initial giving.

Support for Result 1: Using a t-test with unequal variances against the null hypothesis of no treatment effect produces a test statistic of 0.35 when comparing the "14" treatment to the control. Similarly, if we instead perform a Wilcoxon Rank-Sum test, we obtain a test statistic of 0.23. In neither case are the results significant at conventional levels. Likewise, if we compare initial giving in the "20" treatment to the control, we obtain test scores of 1.46 and 1.51, which are significant at the 15 and 13 percent, respectively. Thus, there is a modest lift in giving under this treatment, but it does not rise to significance at conventional levels.

What effect do suggested donations have on overall giving? Obviously, the strategy of offering a suggested donation is designed to raise giving, but, as we saw in Result 1, this had only a modest effect at least initially. What is the cumulative effect of suggested donations? Result 2 shows that an aggressive suggestion produces no lift in giving while moderate suggestion can be effective.

Result 2: A moderate suggested donation (14 tokens) increases overall giving relative to the control. An aggressive suggested donation (20 tokens) does not.

Support for Result 2: We may straightforwardly compare giving under the various treatments by using a t-test with unequal variances or a rank-sum test to compare giving over all rounds. A test of average giving under the "14" treatment compared to the control under the null hypothesis of no difference in average giving yields test statistics of 2.44 and 2.32. Both of these statistics are significant at the 1.5% level. Performing the same comparison for the "20" treatment compared to the control yields statistics of 0.55 and 0.53, respectively, neither of which comes close to statistical significance. Of course, neither of these tests accounts for subject interactions occurring throughout the experiment. Thus, we also perform a simple random effects regression of giving on treatment dummies using robust standard errors which is reported in Column 1 of Table 2. This regression yields a coefficient estimate on the "14" treatment of 1.04 which is highly significant. In contrast, the coefficient estimate for the "20" treatment is only 0.24 and not significant.

Next, we turn to trends in giving. As we saw in Figure 2, the suggested treatments seemed to have opposite effects. The "14" treatment seemed to reduce "donor fatigue" while the "20" treatment seemed to exacerbate it relative to the control. Result 3 formalizes these observations.

Result 3: An aggressive suggested donation (20 tokens) causes more donor fatigue while a moderate suggested donation (14 tokens) causes less donor fatigue compared to the control.

Support for Result 3: To examine this question, we regress giving in each round on the interaction between time and treatment as well as each variable separately. To account for possible heteroskedasticity and autocorrelation of the regressors, we use robust standard errors. Column 2 of Table 2 displays the results of this specification.[7] As the table reveals, donor fatigue is considerably greater under the "20" treatment as compared to the control—the coefficient estimate indicates that this treatment led to an additional reduction of 0.30 tokens per round. In contrast, the "14" treatment reveals a reduction in donor fatigue. Relative to the control, the coefficient estimate indicates that giving increases by 0.06 tokens per round though it cannot be statistically distinguished from a zero effect. Thus, the main effect of an aggressive suggested donation is to reduce giving (considerably) in later rounds.

Finally, we turn to the "long-run" effects of various suggested donation strategies. We treat giving in the last round as a proxy for the long run.

Result 4: An aggressive suggested donation (20 tokens) depresses long-run giving compared to the control. A moderate suggested donation (14 tokens) has no long-run effect.

Support for Result 4: Again, using t-tests and rank sum tests to compare last round giving, we find test statistics of 0.48 and 0.48, respectively, neither of which rises to statistical significance at conventional levels, in comparing the "14" treatment to the control. In contrast, comparing the "20" treatment to the control yields test statistics of 2.01 (p-value = 0.05) and 1.90 (p-value = 0.06), respectively.

To summarize, suggested donations have an effect on giving, but not always in the expected direction. Overall, offering a modest suggested donation seems to have a modest positive effect on giving. It has no effect on donor fatigue, raises giving levels somewhat,

[7] We also ran several nonlinear specifications for the time trend and found that these performed similarly to the more parsimonious linear model.

but is of little consequence in terms of long-term giving. In contrast, making an aggressive suggestion has the perverse effect of *reducing* voluntary giving. It offers no initial lift in giving, increases donor fatigue and depresses long-run giving levels. Standard theory would, of course, predict that merely offering a non-binding suggestion should produce no change in behavior. After all, it remains a dominant strategy to contribute nothing regardless of the suggested donation amounts if subjects only have pecuniary concerns. In the next section, we offer a model where we introduce the possibility of fairness considerations that can rationalize some aspects of the data.

3 Theory

We consider a simple model of voluntary giving where there are two types of individuals: non-psychological types, who care only about monetary payoffs obtained during the game, and psychological types, who care both about monetary and psychological compensation. We use the framework of Rabin (1993) to model psychological types, and extend his notion of fairness equilibria to our dynamic game of incomplete information. It is widely believed, in light of the evidence contradicting payoff maximizing predictions in voluntary contribution games, that factors other than monetary payoffs must affect giving behavior. A variety of models have been offered to help explain these observations (see Ledyard, 1995 for an overview). Most of these explanations enrich the standard model of voluntary giving by introducing a variety of psychological motives such as kindness, warm glow, caring about social surplus, and reciprocity into the utility functions of individuals being modeled. We view Rabin's framework as a step toward weaving together these various psychological strands into a tractable model; hence, we adopt it here. To facilitate comparison, we adopt the notation of Rabin exactly.

Players and Preferences Two players participate in a T period voluntary contribution game. In each period, players must simultaneously decide how much to contribute $z_i \in \{0, 1\}$ to the public good. The monetary payoff player i receives in a given round is given by

$$\pi(z_i, z_j) = 1 - z_i + \beta(z_i + z_j)$$

where β is the return to each player from contributions to the public good. We assume that there is a free-rider problem and the public good is socially desirable, that is $\frac{1}{2} < \beta < 1$. Throughout, we shall consider the mixed extension of this game, and, slightly abuse notation by letting $\pi(a_i, a_j)$ denote the expected monetary payoff to player i when playing the strategy $a_i \in \Delta$ when her co-player chooses strategy $a_j \in \Delta$ in a given period. In addition, player i holds belief b_j strategy employed by her opponent in a given period. Finally, player i holds the belief c_i about j's beliefs regarding i's strategy for the given round.

Player i can be one of two types: economic ($\theta = 0$) or psychological ($\theta = 1$). Let p denote the common prior probability that an individual is a psychological type; however, an individual's type is private information. An individual of type θ earns per period expected utility of

$$U_i = \pi(a_i, a_j) + \theta\left[\bar{f}_j(b_j, c_i)(1 + f_i(a_i, b_j))\right]$$

where $f\ _i(a_i, b_j)$ measures how kind i is to j and $\bar{f}_j(b_j, c_i)$ measures how kind i perceives j is being to him.

Rabin defines the kindness function $f_i(a_i, b_j)$ to be

$$f_i(a_i, b_j) \equiv \frac{\pi(b_j, a_i) - \pi^e(b_j)}{\pi^h(b_j) - \pi^{\min}(b_j)}$$

where $\pi^e(b_j)$ is a reference payoff when b_j is chosen, $\pi^h(b_j)$ is the highest Pareto optimal payoff that j can obtain when selecting b_j and $\pi^{\min}(b_j)$ is the minimum payoff j can be held to by selecting b_j. In our case, it is straightforward to verify that

$$\pi^h(b_j) = 1 - b_j + \beta(b_j + 1)$$

and

$$\pi^{\min}(b_j) = 1 - b_j + \beta b_j.$$

The value for $\pi^e(b_j)$ depends upon shared perceptions as to what the reference payoff is. We shall assume that the use of suggested donation amounts on the part of public goods providers is an attempt to influence this perception. For simplicity, we restrict attention to the case where

$$\pi^e(b_j) = 1 - b_j + \beta(b_j + e).$$

Here, $e \in [0,1]$ denotes the reference level of giving by player i. The function $\bar{f}_j$ is defined similarly. Thus, we have

$$f_i(a_i, b_j) = a_i - e$$

and

$$\bar{f}_j(b_j, c_i) = b_j - e.$$

The interested reader should consult Rabin for details.

Finally, we assume that there is no discounting, so that payoffs over the whole game are simply the sum of the per round payoffs.

Thus, a (possibly mixed) strategy for player i, $a_i(\theta_i, h)$ maps player i's type and the history of moves, h, into the unit simplex, likewise for b_j and c_i. Also, let player i's beliefs about j's type be given by $P_i(h) \equiv \Pr(\theta_j = 1|h)$.

Solution Concept We extend the notion of a fairness equilibrium to dynamic games of incomplete information. Specifically, in a *Perfect Bayesian Fairness Equilibrium (PBFE),*

1. Contributions must maximize expected payoffs given beliefs at each decision node.

2. Beliefs must be formed using Bayes' rule wherever possible

3. Beliefs about strategies must be correct along the equilibrium path.

3.1 Equilibrium Analysis

We do not characterize the set of equilibria. Rather we study properties of several equilibria that may arise in this game. We begin by demonstrating the existence of equilibrium.

Proposition 1 *A complete free-riding equilibrium exists.*

Proof. Suppose that in all periods, players of all types choose to give zero and that beliefs are consistent with this fact for every history. Further, for all histories and all players $P_i(h) = p$. It is routine to verify that there are no profitable deviations for economic types.

Next, notice that deviations by psychological types have no effect on continuation payoffs; thus, we need only show that deviations be psychological types do not benefit them in a given round. By choosing a strategy a_i in period t a psychological type earns

$$1 - a_i + \beta a_i - e\left(1 + a_i - e\right) + \left(T - \left(t + 1\right)\right)\left(1 - e\left(1 - e\right)\right).$$

Since this is a decreasing function of a_i, it is then immediate that choosing $a_i = 0$ maximizes the above expression; hence there are no profitable deviations for psychological types either.

■

Now we examine equilibria that entail cooperation. We begin by offering sufficient conditions guaranteeing the existence of an equilibrium that maximizes expected contributions to the group account in the set of all equilibria. First, notice that in the last period of the game, regardless of beliefs, it is a dominant strategy for economic types to free-ride. Under the following conditions, this is the only circumstance in which any type of player will free ride.

Proposition 2 *Suppose that $\beta > e$ and $p \geq \max\left(1 + e - \beta, \frac{1-\beta}{\beta}\right)$, then there exists an equilibrium where all types contribute 1 in every period save the last, where economic types free-ride and psychological types still contribute 1.*

Proof. Consider the case where equilibrium strategies call for full cooperation up until the final period and free-riding by economic types. In the event of any deviation, play the equilibrium contained in Proposition 1 for the remaining subgame.

Suppose that there was full cooperation up until the last period of the game and that the equilibrium calls for psychological types to cooperate completely in the last period. This strategy is optimal for psychological types provided that

$$\beta\left(1 + p\right) + \left(p - e\right)\left(2 - e\right) \geq 1 + \beta p + \left(p - e\right)\left(1 - e\right).$$

Or equivalently,

$$p \geq 1 + e - \beta.$$

It is straightforward to show that if $\beta > e$, then deviations are not profitable for psychological types in any earlier period.

Next, we show that cooperation in the penultimate period by economic types is optimal. Contributing in the penultimate period is incentive compatible provided that:

$$\begin{aligned} 2\beta + 1 + \beta p &\geq 2 + \beta \\ p &\geq \frac{1-\beta}{\beta} \end{aligned}$$

Similarly, since economic types cooperate in the penultimate period, then deviations are never profitable. ■

Notice that as the reference point increases it becomes increasingly difficult to sustain full cooperation. In contrast, as the public good becomes more valuable (β increases) the conditions given in the Proposition become easier to satisfy.

Finally, we examine a reciprocating equilibrium.

Proposition 3 *Suppose* $\beta > e$ *and* $p < \min\left(1 + e - \beta, \frac{1-\beta}{\beta}\right)$. *Then for* T *sufficiently large, there exists a PBFE such that*

a) For all $t \leq T$, $\theta_i = 1$ *types contribute 1 in* t *provided no free-riding has occurred in any preceding period.*

b) For $t \neq T, k$, $\theta_i = 0$ *types contribute* 1 *in* t *provided no free-riding has occurred in any preceding period.*

c) For $t = T$, $\theta_i = 0$ *types free-ride.*

d) For $t = k$, $\theta_i = 0$ *types randomize between contributing and free-riding.*

e) For all t *and for all* θ_i, *if free-riding has occurred in any preceding period, revert to the equilibrium in Proposition 1.*

Proof. Suppose that $t > k$. Consider any history h where there has been no free-riding in any preceding period. Let the posterior beliefs of a player following any such history be denoted by $\pi = P(h)$.

First, consider any history h' where free-riding has occurred in some preceding period. Following any such history, the strategies of both types of players conform to Proposition 1 and hence are incentive compatible. The rest of the proof concerns itself with showing incentive compatibility of strategies absent any earlier free-riding.

We establish that if $\theta_i = 1$, the above strategies are incentive compatible. In period T, by an argument analogous to Proposition 2, cooperation is incentive compatible provided

$$\pi \geq 1 - \beta + e.$$

In all periods $t < T$, it is straightforward to show that cooperation is incentive compatible provided $\beta > e$.

Next, we establish that if $\theta_i = 0$, then above strategies are incentive compatible. The case where $t = T$ is obvious. Suppose $t \in \{1, 2, ..., k-1, k+1, ..., T-1\}$then analogous to the proof of Proposition 2, it is straightforward to show that conforming is optimal provided

$$\pi \geq \frac{1-\beta}{\beta}$$

Finally, choose π such that if $t = k$ then a $\theta_i = 0$ type is indifferent between cooperating and free-riding. This yields the expression

$$\bar{\pi} = \frac{p\,(T-k)\,(2\beta - 1)}{1 - \beta\,(1+p)}. \tag{1}$$

Note that if $k = T$ then $\bar{\pi} = 0$ and the $\bar{\pi}$ is a decreasing function of k. Finally, for a fixed k, as T increases $\bar{\pi}$ increases. Define $\bar{T}$ to be the smallest integer such that

$$\frac{p\left(\bar{T}\right)(2\beta - 1)}{1 - \beta\,(1+p)} \geq 1.$$

Then if $T \geq \bar{T}$, there exists $\bar{\pi}$ and k satisfying:

$$\max\left(\frac{1-\beta}{\beta}, 1 - \beta + e\right) \leq \bar{\pi} \leq 1$$

which ensures that all of the sufficient conditions for incentive compatibility hold. ■

From equation (1) two comparative statics are noteworthy. First, as e increases, $\bar{\pi}$ increases; hence the incidence of free-riding increases. As T decreases, k decreases and hence the incidence of free-riding also increases. Thus we see that increases in the reference point or decreases in the duration of the game adversely affect contributions. We test these implications experimentally.

Notice that the equilibrium identified in Proposition 3 has several properties consistent with existing experimental data.

- Giving behavior decreases over time. To see this, notice that expected contributions in periods after k are lower than in earlier periods.

- Giving behavior drops dramatically in the last period of the game. Expected contributions in period T are lower than in any other period.

Perhaps more importantly, the model implies that

- Choosing too high a suggested donation level reduces giving.

The reason is that, if the suggested donation level acts as a reference for what constitutes generous behavior, then setting too high a threshold makes it impossible for an individual to distinguish herself by being generous. Thus, even fairness-minded players do not benefit from giving generously. Since economic types only give to imitate these fairness-minded types, giving collapses completely for a sufficiently high reference point.

The model also offers a counterintuitive prediction:

Remark 1 *All else equal, a lower suggested giving level (i.e. e smaller) is more likely to produce giving by economic and psychological types.*

The main implication of this remark is that, by suggesting non-cooperation, one should see at least as much giving as making no suggestion and strictly more giving than when cooperation is suggested. In the first set of experiments, there was no treatment where we intentionally suggested a *low* level of giving. Thus, a test of this remark represents an out of sample prediction of the model.

Of course, the model departs from the experiment in significant ways. First, the model consists of only two players and two strategies. Second, and more importantly, the model postulates repeated interaction among the same individuals and relies on this to sustain giving. Clearly, this was not the case in Experiment 1 where subjects were rematched after each round. Finally, the theory also relies on the fact that, somehow, the subjects view the suggestion as a legitimate reference point. While this may be plausible in some cases, we have no direct way to test the reference point directly.

4 Experiment 2: Prisoner's Dilemmas with Suggestions

With these limitations in mind, we embarked on an additional set of experiments designed to match the theory more closely. Specifically, we ran standard 2×2 repeated prisoners' dilemma games with matched pairs. Under the control, we made no suggestion. Under the "cooperate" treatment, we suggested the obvious – that subjects should cooperate. (Actually, to remove context as much as possible, we suggested that they choose Bin A, which corresponds to cooperation). This treatment is actually quite similar to the 20 token recommendation in Experiment 1, since in each we are recommending the maximum amount of cooperation, which is also the socially efficient level. However, it is also analogous to "paradox of obvious" type treatments in the stag hunt game where the explicit suggestion that we maximize social surplus seems to have an effect. Under the "defect" treatment, we recommended that participants choose Bin B; that is, we recommended that they not cooperate. Figure 3 displays a screenshot of the interface seen by subjects including the display of the suggested donation message.

These experiments were conducted at Muhlenberg College during the Fall 2004, Fall 2009, and Spring 2010 semesters. Participants were recruited via email and posting on the campus website and told that they would "earn some money" for participating in a decision-making study. We conducted 12 sessions, with a number of participants ranging from 2 to 22. Whether a particular session was control, Treatment A or Treatment B was determined randomly. A set of instructions was distributed and read, subjects were given an opportunity to ask questions, and then the decision-making component of the experiment began. This portion of each session consisted of 20 rounds, during which communication among the subjects was not allowed and during which all decision and payoff information was transmitted and recorded via computer terminals.[8] The first five rounds of each session were designated as practice rounds, meaning that decisions made during these rounds would not affect earnings. Other than this special designation for the first five rounds, all rounds of a particular session were identical. Participants were matched with a partner at the

[8] Code for the program was written in Microsoft Visual Basic 6.0 and is available from the authors.

beginning of the experiment, and pairs were maintained throughout the entire 20 rounds. Payoffs were according to the following matrix:

		Co-Player	
		Bin A	Bin B
Player	Bin A	6,6	3,7
	Bin B	7,3	4,4

During each round, each subject recorded his decisions as he made them; subjects also kept a record of their point earnings, which were calculated for them by the computer. At the end of the session, a number from 6 to 20 was chosen randomly to designate the round which determined earnings. Subjects were paid according to the total points gained in this round at a rate of $1 for every point earned. All sessions were completed in under 40 minutes, and subjects earned an average of $9.80 per hour.

5 Results

Table 3 summarizes cooperation in this set of experiments. Pooling all sessions (excluding practice rounds), subjects made 1,960 cooperate/defect decisions. Of these, 18 subject pairs participated in the control treatment, 19 pairs in cooperate and 12 pairs in the defect treatment. The panel is unbalanced for two reasons. First, sessions differed in the number of no-shows giving rise to differences across treatments. Perhaps more importantly, about 2/3rds of the cooperate and control pairs were run in 2004 for a separate experiment. The defect sessions were added later to test the (counterintuitive) prediction of the theory. At that time, we also ran additional sessions of control and cooperate to ensure that our findings for these treatments replicated across time. We found no significant time effect in cooperation behavior for a given treatment.[9] As Table 3 shows, there were considerable differences in giving behavior across treatments. Specifically, under the control treatment, the average subject cooperated 50.9% of the time. When we asked subjects to cooperate, average co-

[9] Formally, treating average giving for a pair over non-practice periods as a unit of observation, we observed no significant difference in sessions conducted in 2004 and those conducted in 2009-2010 for a given treatment.

operation fell to 39.8%. Finally, when we asked subjects not to cooperate, they were very obliging—on average, a subject cooperated only 29.7% of the time.

These differences are even more starkly illustrated Figure 4, which graphs five-round moving average cooperation for each treatment. As the figure shows, average giving is consistently higher in control than under either of the suggestion treatments. Suggesting cooperation reduces giving, though not by as much as suggesting non-cooperation. Unlike Figure 2, there is less evidence of donor fatigue in these experiments. All of the treatments exhibit a modest reduction in giving in later rounds, but it is not noticeably more pronounced under "cooperate" than either of the other treatments. Of course, both Table 3 and Figure 4 obscure variation in individual behavior giving rise to these average results. The remainder of this section examines giving patterns more formally, taking account of this variation.

As with the first set of experiments (and much of the priming literature), we would expect that the strongest effect of our experimental treatment would occur at the start of the experiment when the message is most salient. Here, we find an effect, though not in the intuitive direction.

Result 5: Suggesting anything reduces initial giving.

Support for Result 5: We examine cooperation in the first (non-practice) round of the experiment. Using a t-test with unequal variances against the null hypothesis of no treatment effect produces a test statistic of 1.92 when comparing the "cooperate" treatment to the control. A Wilcoxon Rank-Sum test produces a test statistic of 1.88. Under either statistical test, we can reject the null hypothesis of no treatment effect in favor of a two-sided alternative at the 6% level. Similarly, a t-test produces a test statistic equal to 3.46 comparing "defect" to control. A Wilcoxon Rank-Sum test yields a statistic of 3.14. Both are significant at the 1% level.

The theory model predicts a sharp decrease in giving in the final round of the experiment. While in Experiment 1, we interpreted this as the "long run," here, owing to the repeated interaction, the last period is better interpreted as the "endgame". In contrast to theory and results from Experiment 1, here we find that an aggressive suggested donation has *no effect* on last round giving.

Result 6: Suggestions have no effect on final round giving.

Support for Result 6: Again, we use t-tests and Wilcoxon Rank-Sum tests to compare last round giving. Comparing "cooperate" to control yields test statistics of 0.65 and 0.65, respectively. Neither is significant at conventional levels. Comparing "defect" to control yields test statistics of 1.53 and 1.46, respectively. This difference is significant at the 15% level.

Along the same lines, we show that donor fatigue is less severe when partners repeatedly interact than it was under random rematching of players.

Result 7: Suggestions have no effect on donor fatigue.

Support for Result 7: To examine this question, we perform a probit regression taking cooperation in each round as our dependent variable and regressing it on the interaction between time and treatment as well as each variable separately. Since subjects repeatedly interacted with a single partner, we cluster by subject pair. Table 4 reports the results of this analysis. Notice that the coefficients on the interaction terms are both modest in magnitude and not close to statistical significance. Thus, while giving trends downward over the course of the experiment, these trends do not differ with the treatment.

Now we come to the heart of the matter—what effect do suggested donation levels have on cooperation? Recall that the theory model predicted that the cooperate treatment should reduce giving while the defect treatment should increase it.

Result 8: Suggestions reduce overall giving. Suggesting non-cooperation produces the lowest level of giving.

Support for Result 8: Returning to Table 4, since the interaction terms are not significant, the mean effect of each treatment is given by the coefficient on each of the treatment dummies. As the table shows, suggesting that subjects cooperate reduces cooperation by 22.2% while suggesting that subjects defect reduces cooperation by 41.6% relative to the control.[10] As the table shows, these effects are significant at around the 5% level.

To summarize, when we amend the experiments to tie more closely to the model, the qualitative effects of suggestions are similar to our first set of experiments—suggestions do not produce more giving regardless of whether we suggest that subjects cooperate or

[10] To arrive at these figures, we evaluated the regression coefficients at the average round (7.5) and them computed percentage differences.

the opposite. While the former is consistent with the theory, the latter is not (though it is consistent with common sense.) Our main conclusion is that, by and large, silence is golden—the most consistently effective "treatment" is to make no suggestion whatsoever.

6 Other Effects

We now turn to several other aspects of the data: path dependence of giving and demographics of giving. While these were not our main concern in the experimental design, they hold some interest in relation to the broader literature on giving. It is important to note that our demographic data is confined to sessions run in 2009-2010. Prior to this, we did not collect demographic information and, since these experiments were anonymized, have no basis to recover this information retrospectively. Thus, one should think of these results as more suggestive than definitive.

Path Dependence

We saw previously that suggesting either cooperation or defection reduced initial giving. That is, it reduced the possibility that subject pairs got off to a "good start" in cooperating with one another. The theory predicts that getting off to a good start is critical for sustaining cooperation later in the game. To examine this question, we examine giving after the first round and create a binary variable labeled "good start." Good start equals 1 if a subject pair managed to mutually cooperate in the first round and equals zero otherwise. We then run a probit regression of giving in later rounds on good start, clustering by subject pair. To control for the possibility that some subjects are inherently "nice" and others not so nice, we add individual fixed effects, thus eliminating an individual's mean giving from consideration. Since the effects of good start as well as individual effects likely differ by treatment, we run separate regressions for each treatment with the exception of the "defect" treatment, which we omit.[11] The results of this analysis are shown in Table 5 (suppressing the individual fixed effects coefficients). Notice that the coefficient on good start is highly significant and large

[11] This treatment is omitted since the effect of getting a good start cannot be identified. Only one subject pair got off to a good start and subsequent behavior did not vary over time. Thus, the regressor good start washes out of any regression with individual fixed effects.

under the control and the cooperate treatment. Getting off on the right foot is strongly predictive of later cooperation. This implies a transmission mechanism for the suggested donation to continue to matter later in the experiment even if the suggestion itself is largely ignored by subjects. By undermining initial cooperation, the effect of the suggestion may persist through path dependence.

Demographics

Many studies report gender differences in giving behavior.[12] Here, we examine the effects of gender and other demographic characteristics on giving. Since our treatments were not balanced by gender, we regress giving on gender separately for each treatment. Since giving declines over time, we include a time trend. Finally, we cluster by subject pairs to account for correlation effects on the error terms. The results of these three regressions are reported in Table 6. Consistent with previous findings, women are significantly more likely to cooperate than men under the control. However, making any suggestion whatsoever seems to eliminate this predilection towards cooperation. The coefficient estimates for these regressions fall by a factor of 5 and are nowhere close to significance. To summarize, once any suggestion is made for how to behave, women and men behave similarly.

Several studies report differences in selfishness depending on training in economics or allied fields.[13] Since we have the self-reported majors of our subjects, we can examine differences in cooperativeness in the data. More importantly, we can examine whether training in economic thinking affects how subjects respond to various suggestions. We replicate the regressions in Table 7, replacing gender with a dummy variable which equals one if a subject's self-reported major is accounting, business, economics, or finance. In all of these majors, subjects are exposed to one or more microeconomics courses. As the table shows, under the control treatment economics training has no effect on giving behavior nor does it have any effect under the defect treatment. However, when we suggested cooperation, subjects trained in economics were systematically less likely to cooperate than those not so trained. Evaluated at the mean value for the data, a subject trained in economics was 28% less likely

[12] See Oortman and Tichy (1999).

[13] See, for example, Marwell & Ames (1981) and Frank *et al.* (1996).

to cooperate than a subject whose major was not accounting, business, economics, or finance. Thus, training seems to lead to skepticism with respect to suggestions but not differences in mean giving absent such suggestions.

Perhaps intellective differences are driving giving behavior. Our summary data indicate that women in the sample have significantly lower math SAT scores than their male counterparts while verbal SAT scores are about the same.[14] Students whose self-reported majors are accounting, business, economics, and finance do not differ in their math and verbal SAT scores compared to those in other majors. To examine whether gender is merely a proxy for differences in this dimension, we replicate Table 6 adding quadratic terms for the math and verbal SAT scores of each subject. These results are reported in Table 8. As that table shows, under the control, women are still more likely to cooperate. Higher math SAT scores lead to more cooperation initially with cooperation peaking at 680, the median math SAT score in the data. Thereafter, cooperation falls modestly. On the other hand, higher verbal SAT scores are associated with less cooperation. The lowest level of cooperation is achieved at a verbal SAT score of 680, well above the median verbal SAT score in the data (640). Thus, higher verbal skills (as measured by SAT) seem to coincide with less cooperative behavior. Of course, these regressions merely represent correlations between giving and demographic variables rather than representing causality.

Turning to the cooperate treatment, gender remains insignificant while higher math SAT scores now lead to *lower* levels of cooperation. Indeed, for the bulk of the sample, higher math or verbal SAT scores are associated with less giving. Finally, under the defect treatment, cooperation is sufficiently rare that none of the demographic variables is significant. Thus, as we saw for the case of economics training, greater "sophistication" (as measured by SAT score) seems to lead to a greater level of distrust of the suggestion and lower levels of cooperation.

[14] A t-test comparing scores by gender with unequal variances results in a difference in math SAT scores that is significant at the 9% level and no statistical difference in verbal SAT scores.

7 Conclusions

In charitable fund-raising, using a suggested donation amount to influence giving is ubiquitous. In this paper, we ask what would seem the most important question: Do suggestions actually work to increase giving? Using laboratory experiments, we investigated the effects of different suggested donation amounts, as well as no suggestion, on giving. We found that suggested donations do indeed influence giving, but not always in the expected direction. Suggesting that individuals contribute the socially optimal amount *reduced* giving relative to no suggestion. Suggesting that individual not give also reduced giving. However, suggesting a moderate contribution amount produced modest gains in giving. We showed that a fairness-based theory model is capable of explaining some of these findings.

Our findings suggest that suggested donations can increase giving, but not as much and not as well as one might have expected. Indeed, getting the suggested donation amount wrong can be extremely costly. In many instances, silence is golden—the best suggestion may be no suggestion at all. In terms of practice, we would conjecture, based on our results, that fund-raisers would be better served by having fewer and lower suggested donation amounts.

While our results are directly applicable for charities seeking to raise money by enticing undergraduates to play games in a computer lab, our study suffers from the usual concerns about external validity common to laboratory experiments. There are, however, several aspects of the study that suggest its applicability more broadly. First, our study is consistent with field experiments and practice in terms of the phenomenon of getting more by asking for less. Second, despite differences in the subject pool (Princeton versus Muhlenberg), time (1998 versus 2009), and interaction (random rematching versus partners), responses to suggested donations amounts were quite similar. Third, previous studies, such as Rondeau and List (2008) note many similarities in giving behavior exhibited in the lab and in the field. Thus, there are some reasons to be optimistic in the applicability of our findings outside the lab.

Regardless, our study should mainly be viewed as a first step toward understanding the effects of suggested donations on giving. Much remains to be investigated. An important next step would be to conduct field experiments comparing not only various suggested donation amounts but also including a "silence" treatment, i.e. no suggested donation. An aspect emphasized in practice but missing from our experiment is the need to match the suggested donation amount with the income of the potential donor. Introducing heterogeneity in endowments of various subjects could, in principle, shed light on this aspect. Still another aspect arising in practice for public goods that are "consumed" like a visit to the Metropolitan Museum is that the suggested donation might be viewed as a signal of the quality of the good. The Metropolitan's decision to raise its suggested donation amount was, at least in part, driven by the fact that the rival MOMA was now charging a $20 entry fee.

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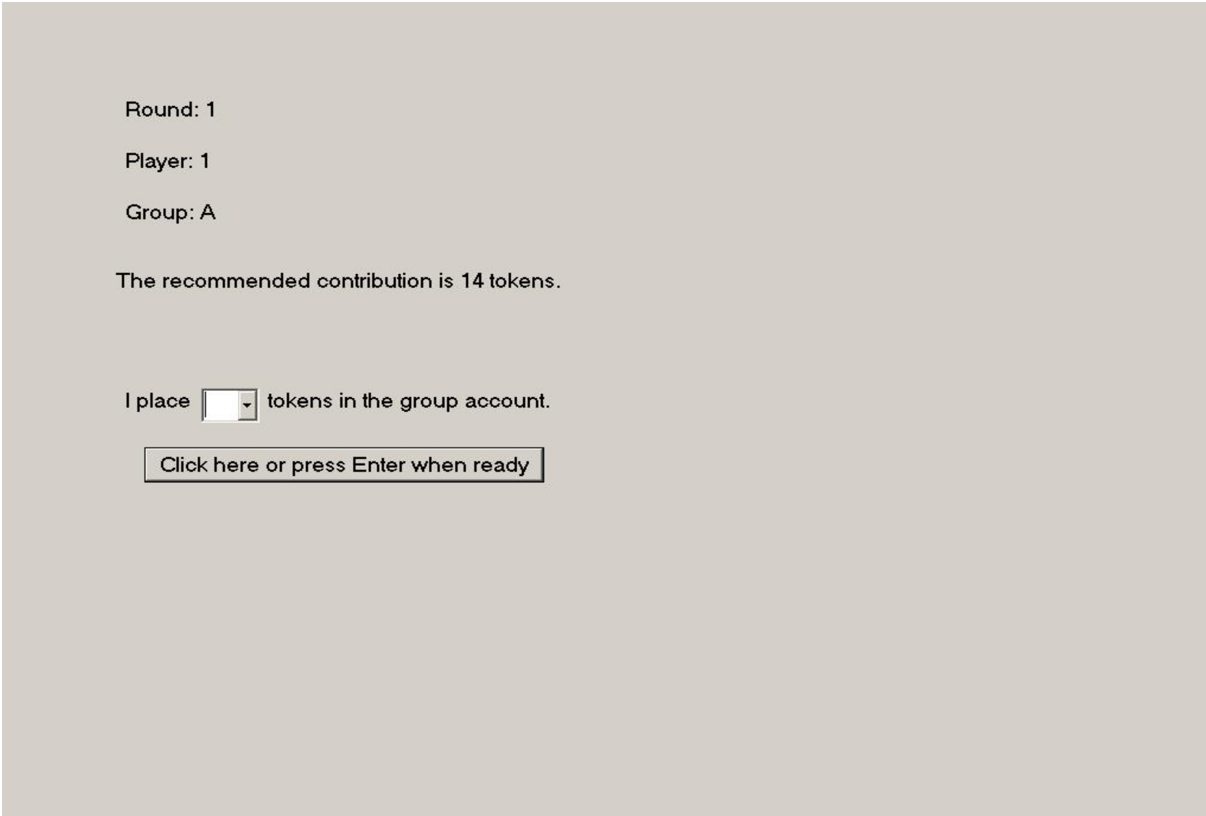
Figure 1: Screenshot of Experiment 1



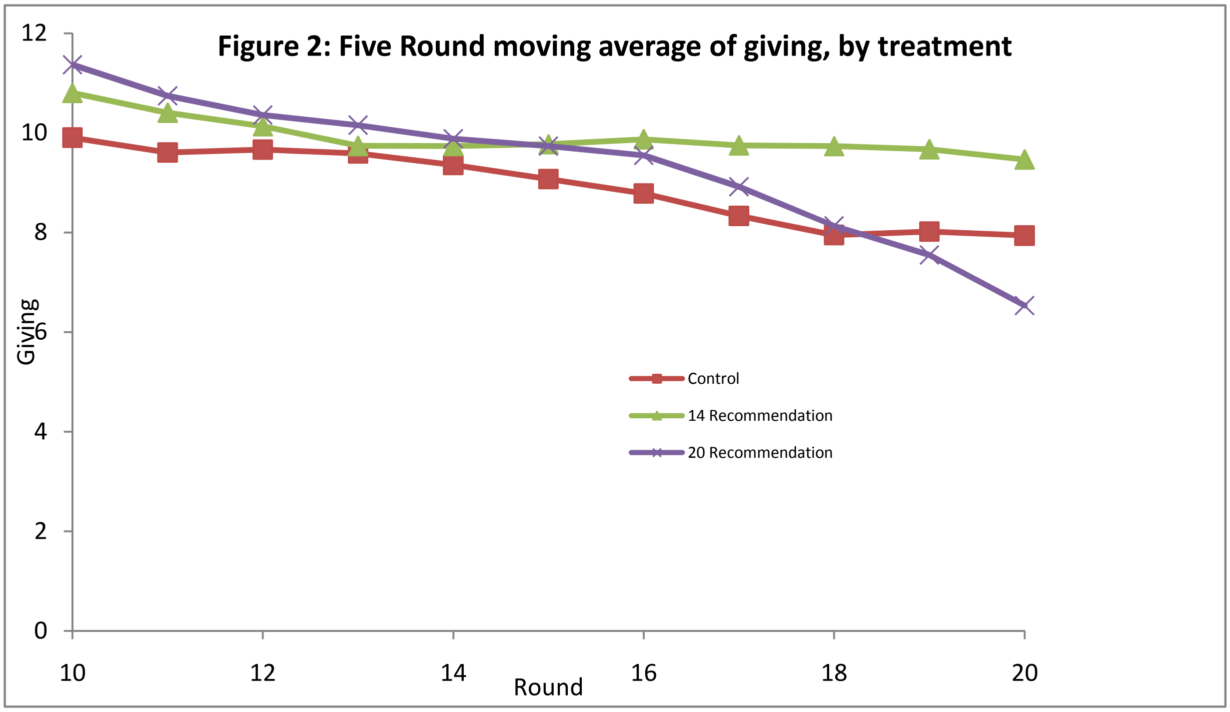
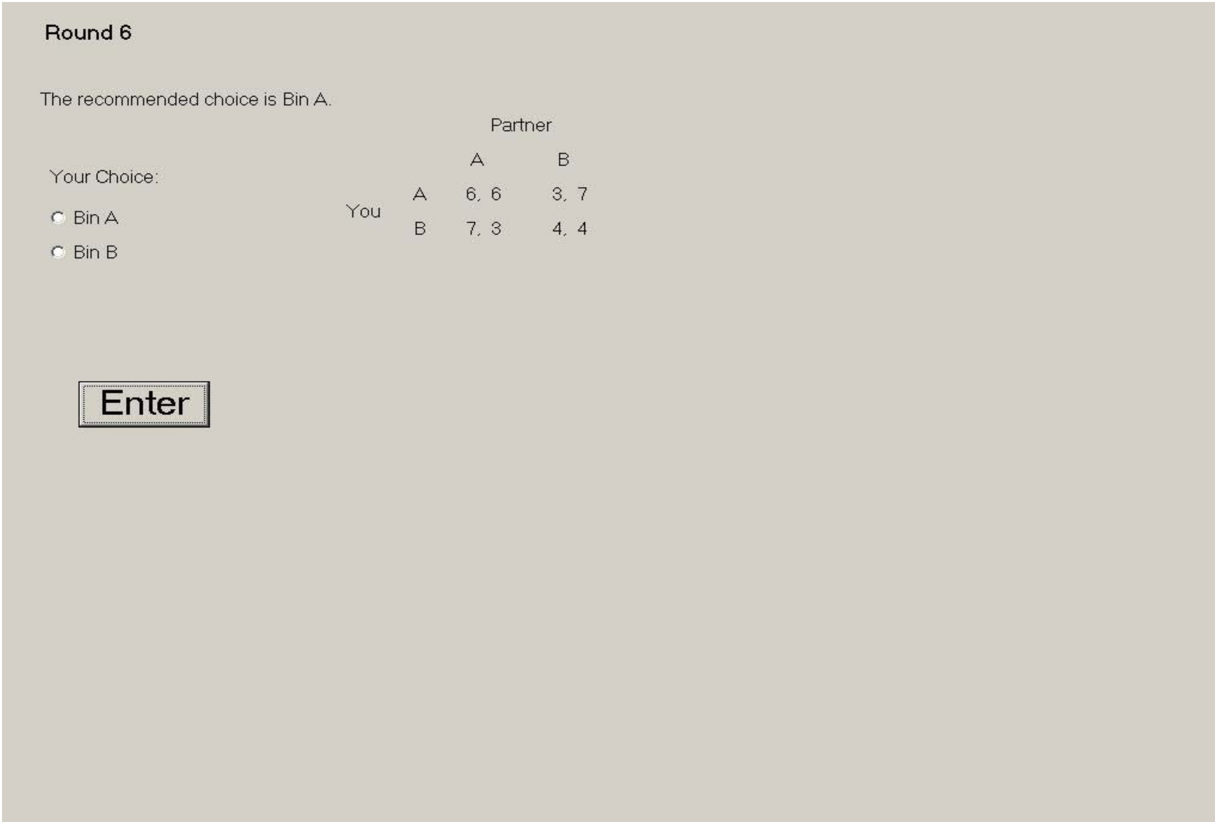


Figure 2: Five Round moving average of giving, by treatment
Giving
Round
Control
14 Recommendation
20 Recommendation
0
2
4
6
8
10
12
10
12
14
16
18
20

Figure 3: Screenshot of Experiment 2



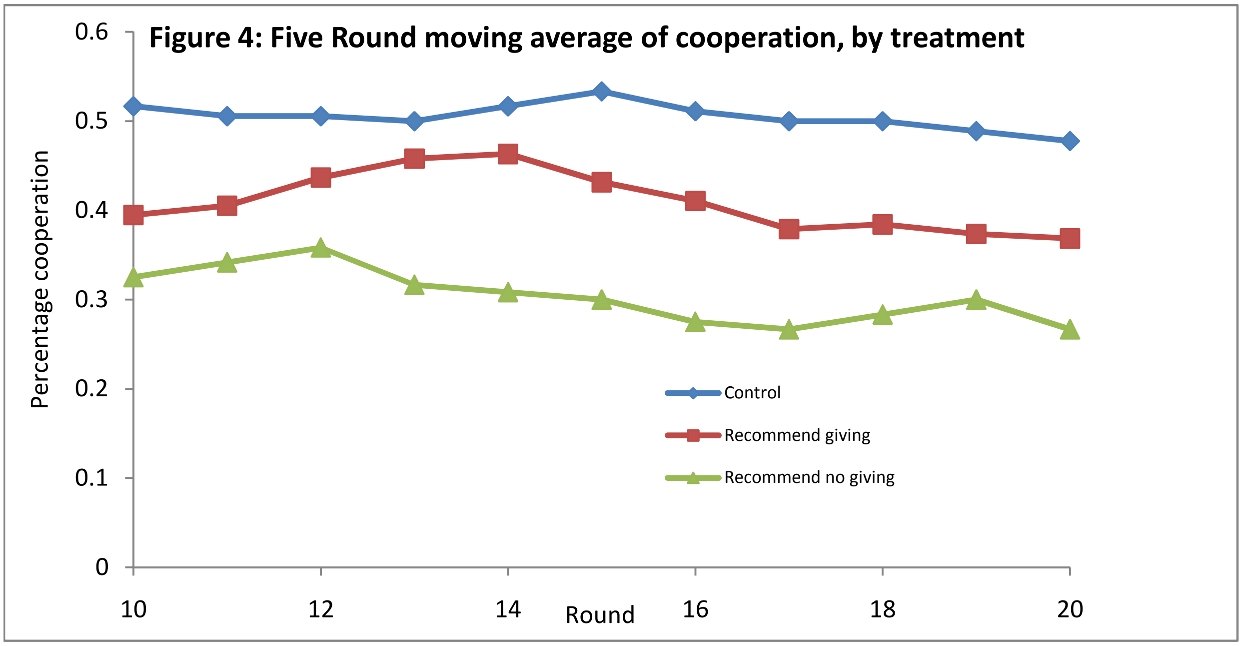


Figure 4: Five Round moving average of cooperation, by treatment
Percentage cooperation
0.6
0.5
0.4
0.3
0.2
0.1
0
10
12
14
16
18
20
Round
Control
Recommend giving
Recommend no giving

Table 1: Experiment 1 Summary Statistics

Variable	Control	Treatment 14	Treatment 20
Giving	8.97	10.01	9.21
Standard Deviation	6.84	7.16	7.49
Fraction of observations where subjects gave exactly 14 tokens	1.25%	10.97%	2.08%
Fraction of observations where subjects gave exactly 20 tokens	11.67%	13.47%	20.56%
Number of observations	540	540	540

Table 2: Regression results, Experiment 1

	Coefficient (standard error)			
Variable	**Simple model**		**With time trend**	
Constant	8.97	***	10.61	***
	(0.29)		(0.62)	
Treatment 14	1.04	**	0.60	
	(0.43)		(0.89)	
Treatment 20	0.24		2.62	**
	(0.44)		(0.89)	
Round			-0.21	**
			(0.07)	
Round*Treatment 14			0.06	
			(0.10)	
Round*Treatment 20			-0.30	**
			(0.10)	
Number of Observations	1620		1620.00	

Linear regression of giving, with robust standard errors.
** denotes statistical significance at 5% level.
*** denotes statistical significance at 1% level.

Table 3: Experiment 2 Summary Statistics

Treatment	Control	Recommend cooperation	Recommend non-cooperation
Sessions	4	5	4
Participants	36	38	24
Average cooperation	50.9%	39.8%	29.7%
Cooperation in Round 6	66.70%	44.74%	25%
Cooperation in Round 20	38.89%	31.58%	20.83%
Female	12.50%	60%	54.17%
Major in accounting, business, economics or finance	50%	10%	37.50%
Mean Verbal SAT	641	673	628
Mean Math SAT	681	644	652

Table 4: Regression results, Experiment 2

Variable	Coefficient (standard error)	
Treatment: Suggest cooperation	-0.15	*
	(0.08)	
Treatment: Suggest non-cooperation	-0.23	**
	(0.09)	
Round	-0.008	**
	(0.003)	
Round*Suggest cooperation	0.005	
	(0.004)	
Round*Suggest non-cooperation	0.004	
	(0.007)	
Number of Observations	1470	

Estimates from probit regression of cooperation, clustering on subject pair.
Coefficients are marginal effects.
* denotes statistical significance at 10% level.
** denotes statistical significance at 5% level.

Table 5: Regression results, Experiment 2 -- effect of initial cooperation

	Coefficient (standard error)	
Variable	**Control**	**Suggest cooperation**
Good start	1.000 ***	0.428 ***
	(0.000)	(0.006)
Round	-0.011	-0.002
	(0.007)	(0.006)
Number of Observations	406	532

Estimates from probit regression of cooperation with individual fixed effects. Coefficients are marginal effects. Result from treatment with suggested non-cooperation omitted due to lack of identification.
*** denotes statistical significance at 1% level.

Table 6: Regression results, Experiment 2 -- gender effects

Variable	Coefficient (standard error) Control		Suggest cooperation		Suggest non-cooperation
Female	0.322	**	0.062		0.067
	(0.200)		(0.126)		(0.157)
Round	-0.008		-0.006	*	-0.004
	(0.006)		(0.003)		(0.005)
Number of Observations	120		150		360

Estimates from probit regression of cooperation, clustering on subject pair. Coefficients are marginal effects.
* denotes statistical significance at 10% level.
** denotes statistical significance at 5% level.

Table 7: Regression results, Experiment 2 -- major effects

	Coefficient (standard error)		
Variable	**Control**	**Suggest cooperation**	**Suggest non-cooperation**
Major in Accounting, Business, Economics or Finance	0.034	-0.283 **	-0.001
	(0.069)	(0.100)	(0.145)
Round	-0.009	-0.006 *	-0.004
	(0.006)	(0.003)	(0.005)
Number of Observations	120	150	360

Estimates from probit regression of cooperation, clustering on subject pair. Coefficients are marginal effects.

* denotes statistical significance at 10% level.

** denotes statistical significance at 5% level.

Table 8: Regression results, Experiment 2 -- SAT effects

Variable	Coefficient (standard error)		
	Control	Suggest cooperation	Suggest non-cooperation
Math SAT/1000	32.03 *	-17.05	8.88
	(16.130)	(62.540)	(17.040)
Math SAT squared	-23.72 **	10.86	-6.16
	(11.390)	(50.230)	(12.680)
Verbal SAT/1000	-165.65 ***	-42.86 **	14.92
	(13.690)	(15.750)	(23.010)
Verbal SAT squared	125.04 ***	29.55 **	-12.69
	(9.790)	(10.990)	(18.300)
Female	0.52 ***	0.013	0.06
	(0.030)	(0.119)	(0.139)
Round	-0.014	-0.008 *	-0.005
	(0.011)	(0.004)	(0.005)
Number of Observations	120	135	330

Estimates from probit regression of cooperation, clustering on subject pair. Coefficients are marginal effects.
* denotes statistical significance at 10% level.
** denotes statistical significance at 5% level.
*** denotes statistical significance at 1% level.