Social Interactions and Giving

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This thesis is dedicated to Ted, Ginny, John, and Maryanna McConnell.

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Abstract

This thesis presents four experimental studies addressing theories of social interactions and charitable contributions. Social interactions have been identified as an important nonmarket determinant of economic outcomes. My research provides theoretically motivated experimental evidence to advance our understanding of strategic communication and voluntary contributions.

I consider a model of communication in the presence of investment opportunities with uncertain returns and positive social externalities. The model predicts that welfare improving communication can only occur when individuals can communicate by sending a costly signal. I test this model with experiments. While the model predicts that individuals need to "burn money" in order to effectively communicate, in our experiments individuals overcommunicate when messages are free and undercommunicate when they are costly. Therefore, we do not see welfare improvements from costly communication.

In joint work with Jacob Goeree, Leeat Yariv, Tiffany Mitchell, and Tracy Tromp, we consider the relationship between social closeness and the tendency to be generous to others in an actual social network. We find that dictator offers are primarily explained by social distance: giving follows a simple inverse distance law. Our results suggest that social closeness is a more important predictor of generosity than individual demographic characteristics.

In another study conducted with Sera Linardi, we adapt Benabou and Tirole's (2006) model in order to address the role of honor, stigma and visibility on contri-

butions of time. We consider the effect of excuses and monitoring on the willingness to volunteer in an experiment combining elements of lab and field. We find that removing available excuses for not volunteering significantly increases the willingness to volunteer without negatively affecting productivity.

In further work on charitable giving with Jacob Goeree and Antonio Rangel, we provide experimental evidence consistent with morally motivated charitable giving. We find that providing subjects with a suggested contribution amount increases the willingness to give and that framing the suggestion with moral language further increases contributions. However, moral framing language does not impact the share of individuals who make no contributions, suggesting that individuals may value contributions above a moral reference point differently from contributions below it.

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Chapter 1

Introduction

Economists have recently begun to consider how social structures and social interactions affect decision-making. The literature points to a variety of ways that individual behavior might be affected by social interactions. For example, through social interactions, individuals may receive information which has direct impact on their decisions. An extensive literature has examined the possibility of social learning, where individuals obtain information directly from others, or by observing the decisions of others (Banerjee and Fudenberg 1994; Ellison and Fudenberg 1993; Callander and Horner 2005; Bikhchandani, Hirschleifer, and Welch 1998).

Another way that individual behavior is affected by social interactions is that individual appear to have preferences over various dimensions of social interactions. For example, individuals have preferences related to the well-being of others (Andreoni and Miller 2002; Charness and Rabin 2002; Fehr and Schmidt 1999; Bolton and Ockenfels 2000). Furthermore, theoretical models have proposed that individuals' utility may also depend on others' good opinion of them (Benabou and Tirole 2006; Andreoni and Bernheim 2009). Similarly a natural tendency to compare oneself to others creates the possibility that individual preferences for conformity stem from a desire to fit in (Bernheim 1994; Austen Smith and Fryer 2005).

In addition to affecting individuals directly via individual preferences and indirectly via the transmission of information, social interactions also provide opportunities for strategic communication. Crawford and Sobel (1982) first considered the possibility that information transmission may not be possible in communication when individual incentives are not aligned. Understanding when informative communication is possible is an important component of understanding the role played by social interactions in economic decision-making.

In chapter 2, I consider theory and experimental evidence on individuals' willingness to make costly investments to communicate with others when there are strategic benefits from influencing others' behavior. I provide a theoretical model which illustrates that, in the presence of externalities from individual actions, individuals may be willing to send costly signals in order to influence others. The predictions of the model are tested empirically with an experiment comparing outcomes when there is no communication to two communication institutions: one in which communication is free and another in which communication is costly. While results from the model suggest that welfare improvements are possible from communication only when individuals can communicate at some cost, in our experiments we see the greatest welfare gains when communication is free. This is because in our experiments individuals already appear to take actions that benefit their neighbors, softening the strategic incentives to spend money influencing the actions of others.

Chapter 3 examines how network structure and social closeness affect the willingness to be generous to others. In joint work with Jacob Goeree, Tracy Tromp, Tiffany Mitchell, and Leeat Yariv, we combine survey data on friendship networks and individual characteristics with experimental observations from dictator games. Dictator offers are primarily explained by social distance: giving follows a simple inverse distance law. While student demographics play a minor role in explaining offer amounts, individual heterogeneity is important for network formation. In particular, we detect significant homophilous behavior: students connect to others similar to them. Moreover, the network data reveal a strong preference for cliques – students are more likely to form connections to those already close.

Chapter 4 contains joint work with Sera Linardi, in which we consider how the visibility of contributions of time affects the willingness to give. Specifically, we investigate the effect of excuses and monitoring on voluntary contributions of time and effort. We extend the theoretical framework proposed by Benabou and Tirole (2006) to consider the role of honor, stigma and visibility on contributions of time. We design an experiment that retains the advantages of laboratory control while incorporating the context of the field by engaging subjects in an actual nonprofit's operation. We find that when excuses for deciding not to volunteer are not available, the number of minutes volunteered increases without affecting the quality of work. Our experimental evidence suggests that social image concerns are complex. While the presence of a larger audience of peers increases the willingness to volunteer, the presence of a monitor reduces volunteering. Furthermore, we see evidence of nonlinearities in stigma over time; subjects avoid being the first to stop volunteering but are more likely to stop once others have stopped.

Chapter 5 represents joint work with Jacob Goeree and Antonio Rangel providing experimental evidence that supports a theory of morally-motivated giving. We test a two-part strategy often used by charities to solicit gifts: a specific suggested contribution combined with language framed by a sense of moral responsibility. Our experimental design isolates the effect of the suggested contribution from the additional impact of framing the suggested contribution in terms of moral responsibility. We find that providing a suggested contribution significantly increases the average gift and makes individuals more likely to give at the suggested level. Framing the suggested contribution as a moral responsibility further increases average contributions and the share of individuals contributing at the suggested level. However, the moral framing language does not impact the share of individuals who make no contributions at all, suggesting that individuals may value contributions above a moral reference point differently from contributions below a moral reference point.

Chapter 2

Costly Influence

2.1 Introduction

The role of communication in economic decision making is the subject of recent theoretical research. While a growing body of models address the possibility for informative communication in social interactions when payoffs are interrelated, relatively few studies consider equilibria in which individuals attempt to strategically influence the actions of others by "burning money" in order to reveal their own information. However, in practice we see many examples of individuals investing costly effort to convince others to take action. Furthermore, individuals appear to be more willing to make costly investments to convince others when they themselves will benefit from others' actions. For example, an individual proposing a risky but mutually beneficial business plan to a partner might travel a long distance at great cost to have face-to-face meetings, even though the same information would be exchanged regardless of where the meeting was conducted. These meetings can be seen as "burning money" in order for an individual to convince his parter that he has private information about high returns on investment.

Another example of the potential for communication by "burning money" is efforts to convince friends to join a social networking service (such as Facebook or MySpace). Social networking services require costly action and only provide returns when other members of one's social network also participate (which is usually a matter of uncertainty when you make the investment yourself). Therefore, while individuals might receive a number of invitations to join different social networking services from their friends, they would be more likely to be influenced to join a service when a friend has invested significant effort in convincing them (whether by attempting to convince them by repeatedly discussing the benefits, or taking the time to sit them down to show them the benefits of the social networking service or enlisting other friends to attempt to influence them).

In both of these examples, individual communication occurs in the presence of uncertainty about the returns to investment and complementarities from others' investments. These examples suggest that in the presence of both uncertainty and complementarities, certain kinds of communication can be informative while other communication may be seen as merely cheap talk. Because of individuals' private information, it may be possible to influence others' action with communication that is welfare improving. However, because of the complementarities between individual decisions, in order to be informative, communication must be able to reveal truthful information in an incentive compatible way.

In this chapter, I provide a model of communication and test the theoretical predictions of the model with an experiment. In the model of communication presented here, individuals must make a costly investment decision in the presence of uncertainty and social complementarities. Individuals receive additive payoffs for each member of their network who invests but they pay the cost of their own investment only. In the model, individuals first communicate with their networks. After sending and receiving messages, all individuals make simultaneous investment decisions. I show that effective communication is possible only when individual have access to a costly communication technology. In equilibrium, I predict that a separating equilibrium will exist, where individuals with good information will "burn money" in order to send a credible and informative message about their private information. I also show that when individuals derive heterogeneous returns from their investment, the amount that they invest to send a message is increasing in their return from investing and decreasing in the return of the person they are communicating with.

The experimental test of this model is done in the laboratory with two person networks. I compare a treatment with no communication to two communication treatment: one in which messages are free and another in which messages vary in positive costs. I find that while subjects in the experiment are more likely to be influenced by messages that are more costly, they do not fully take this into account when deciding whether to send a message. In fact, subjects are most willing to send messages when messages are free or low cost, even though these messages are not as influential. As a result, while the ability to send messages improves welfare, the probability of choosing the correct action is the same regardless of whether messages are free or costly. Hence, welfare improvements are found only in the free messages treatment.

Crawford and Sobel (1982) provide one of the first analyses of strategic communication. In their model, there is an asymmetry between information and action. One individual (the sender) receives private information and he must decide what information to provide to the receiver, who is the only one with the power to make decisions. This work on strategic communication is advanced by Austen Smith and Banks (2000) who illustrate that a larger set of communication equilibria are possible when individuals can send costly signals. Austen Smith and Banks (2002) illustrate what kind of communication occurs when both cheap talk and costly signaling are possible.

The Crawford and Sobel model is designed to provide insight into a class of problems related to bargaining when individuals have different information and decision making power. In my model, while the possibility for welfare improving communication comes from individuals' private information, there is an incentive compatibility problem due to positive externalities from the investment decisions of others. My model addresses situations where symmetric agents both make decisions about whether to communicate and make an investment. While communication can occur in Crawford and Sobel's model for free in equilibrium, in my model communication will only be effective if individuals send a costly signal in order to illustrate their private information.

My model is also related to a class of models explaining advice. These models compare the welfare properties when individuals can learn by passively observing individual actions (social learning) to when individuals are given advice.[1] In models of advice, the benefit to providing advice is exogenous and occurs only if the advice results in the right decision. These models differ from a situation where individuals attempt to socially influence their friends for strategic reasons. In my model, individuals pay to develop a communication network because there are strategic benefits due to the complementarities between individual choices. Individuals have something to gain from others' investment, regardless of whether that investment was cost effective.

Cai and Wang (2006) provide an empirical test of the Crawford and Sobel strategic communication model. Their evidence suggests that while the comparative statics of the model fit behavior well, individuals appear to be more likely to send messages that provide information to another party than the theory would predict. As a result, receivers of information are able to rely on this information more than would be predicted in the theory. In related work, Dickinson, Hafer and Landa (2008) find that subjects given positions on an issue scale and asked to deliberate have a tendency to overcommunicate, expressing their positions even when strategically they would benefit more from being silent. In the experiment, subjects tend to send informative messages more than the model would predict when messages are free. However, when

[1] Çelen, Kariv and Schotter (2007) and Chaudhuri, Schotter and Sopher (2001)

messages come at some cost, individuals do not invest as often as we would predict. For this reason, the highest levels of welfare occur when messages are free.

Other experimental and theoretical studies that feature "burning money" and cheap talk as part of their equilibrium concept have focused primarily on coordination games.[2] In many of these games, individuals benefit from others' actions only when they can coordinate, creating incentives to send costly signals as a way to select between equilibria. In these experiments, the possibility of burning money is sufficient and on the equilibrium path, individuals will not burn money (Hurkens 1994). Nonetheless, similar to evidence from the experiment presented here, experimental evidence on whether subjects act strategically is mixed. In experiments designed to determine whether individuals act optimally when they have the possibility to burn money, Huck and Müller's (2005) results indicate that subjects' behavior is not consistent with equilibrium predictions, except when the game is played in extensive form. Since these experiments do not predict that subjects should burn money, it could be that deviations from strategic behavior may be magnified when burning money occurs on the equilibrium path.

The structure of my model is adapted from the work of Calvo-Armengol and Jackson 2009 (hereafter CJ) on peer pressure (2009). In their model, individuals make a costly investment in order to reduce the cost of taking action for their networks. There is a direct analogy between the the direct benefit to others' utility that is possible in the CJ model of pressure and the information benefit of costly communication in the model I develop here. Individuals will pay for costly messages if they are effective. In my model, messages are effective because individuals have private information, while in CJ's model, pressure has a direct impact on decisions by changing the costs of taking action. The focus of the CJ model of peer pressure is primarily participation games, games in which individuals receive payoffs only if they participate. In con-

[2] Charness and Grosskopf (2001) and Duffy and Feltovich (2002); Palfrey and Rosenthal (1991)

trast, my model focuses on a game in which individuals may benefit from the actions of others' even when they do not participate. It is not always clear that individuals with common interests will be able to credibly affect the costs of others' actions as assumed in the CJ model. In the example provided here, it is more natural to assume that individuals may be able to communicate but not to directly influence the costs and benefits of others. In the presence of uncertainty, it may be possible to send costly signals as a way of communicating private information that will influence others, even when it is not possible to directly change their cost-benefit calculation.

2.2 Model

In this model, individuals make a simultaneous decision about whether to make an investment that externalities. An individual's investment decision is represented by the binary variable x. Individuals also benefit from others' investment. The cost of an individual's investment is represented by c and paid only by the individual making the investment.

Before individuals decide whether to invest, they are uncertain about the benefits of investment. The benefit to investment varies by a factor a_ω, where the state of the world ω is either G or B and $a_G > a_B$. Individuals receive a private signal s_i about the state of the world. Their signal is correct with probability $q > \frac{1}{2}$, so that $P(a_\omega = a_G | s_i = G) = q$. The payoffs to investing can be represented by

$$U(x_i, x_{-i}, a_\omega) = a_\omega \left(\sum_j x_j \right) - cx_i.$$

I will first consider a two person group. For any individual who receives a good signal, the expected utilities from investing and not investing are

$$\begin{aligned} EU(x_i = 1|s_i) &= E\left(a_\omega \left(1 + E(x_j)\right)|s_i\right) - c \\ EU(x_i = 0|s_i) &= E\left(a_\omega \left(E(x_j)\right)|s_i\right). \end{aligned}$$

When individuals have only their private information, they will invest if the expected utility is greater than the expected utility of not investing. That is, individuals will invest if and only if

$$E(a_\omega|s_i) \geq c.$$

It is useful to note that an individual's decision about whether to invest does not depend on others' investment decisions. Furthermore, individuals are uniformly better off when their partners invest. Ex-ante social welfare when individuals have only their private information can be written as

$$W(x, \omega) = \left(E(x_i|s_i) + E(x_j|s_j)\right)(a_\omega - c).$$

Individuals receive welfare from the expected benefits from investing minus the cost of the investment.

Naturally, improvements in welfare would be possible if individuals could share information. When individuals share all information, ex-ante social welfare with two individuals can be defined as

$$W_s(x, \omega) = \left(E(x_i|s_i, s_j) + E(x_j|s_i, s_j)\right)(a_\omega - c).$$

Because individuals have more information, they are more likely to invest when there are positive returns and not to invest when those returns are negative. Therefore, welfare improvements are possible when information is shared. However, because individuals uniformly benefit from the investment of others, regardless of whether the benefits justify the costs, individuals cannot credibly communicate their private information. If individuals were prevented from lying, whether by repercussions or by some cost of lying, it would be possible to achieve the best-case scenario for welfare. However, as long as individual communications are discounted because of the incentives to misrepresent, it is not possible to achieve this outcome.

2.2.1 Social Influence

In the presence of complementarities from others' actions, individuals have an incentive to influence the actions of their friends. I define influence as any communication that changes behavior. When information is free, informative communication is not possible. However, if there were some way for individuals to reveal their private information credibly, welfare improvements could be made by sharing information. I provide a model of communication between symmetric individuals making investment decisions with positive social externalities.

I adapt a model proposed by Jackson and Calvo-Armengol (2007) that was designed to analyze the theoretical implications of peer pressure. Unlike the CJ model, individuals in my model cannot change the cost of others in their network. Instead they hope to influence the actions of others in their social group by communicating with them. Individuals first chose whether to pay to send a message to others in their network and if so, to whom to send a message. Individuals then receive any messages sent from others in the group. Individuals may send one of two messages: {Invest, Don't invest}. After receiving all messages, all individuals in the group make their investment decision simultaneously.

I assume there is some fixed cost $\rho > 0$ of influence and individuals may choose to send a message or not. If individual i choses to send a message to j, $p_{ij} = 1$ and if individual i does not send a message to individual j, then $p_{ij} = 0$. The cost of a message is chosen from $\rho \in \{0, 1, \ldots, \Psi\}$. Individual utility when individuals can communicate by investing in messages can be represented as

$$U(x_i, x_{-i}, a_\omega, \rho, p_{ij}) = a_\omega \left(\sum_j x_j \right) - cx_i - \rho \sum_j p_{ij}.$$

For simplicity, I assume that individuals who are indifferent between sending and not sending a message will choose not to communicate.

2.2.2 Free Messages

In order to consider how individual behavior changes in the presence of messages, I again focus on the two individual case. Let us first consider what happens in equilibrium when messages are free. Since individuals benefit from their friend's investment, regardless of the true state of the world, individuals will have an incentive convince each other to invest regardless of their private signal, it is impossible for messages to reveal any actual information. Therefore, in equilibrium, a message is not credible and no messages will be sent.

2.2.3 Costly Messages

While no information can be effectively communicated when messages are free, it is possible to share information when individuals can send costly signals. In order to be effective in influencing others, costly messages must be able to credibly separate those who have received a good signal from those who have received a bad signal.

In order for a message to separate individuals with good signals from those with bad signals, the message cost ρ must satisfy the following two conditions

$$\begin{aligned} E(a_\omega|s_i = G, s_j)\left(E(x_j|s_i = G, p_{ij} = 1) - E(x_j|s_i = G, p_{ij} = 0)\right) &\geq \rho; \\ E(a_\omega|s_i = B, s_j)\left(E(x_j|s_i = B, p_{ij} = 1) - E(x_j|s_i = B, p_{ij} = 0)\right) &< \rho. \end{aligned}$$

That is, it must be that the expected benefit of sending a message exceeds the cost for individuals who have received a good signal, while the expected benefit is below the cost for those who have received a bad signal.

In the presence of such a separating equilibrium, all private information will be revealed. Individuals who have received a good signal will send a message that reveals their signal. Individuals who have received a bad signal will not send a message, also revealing their signal. Therefore individual investment decisions will be made with full shared information. In the presence of full shared information, an agent will invest if the expected benefit given all available information exceeds the cost

$$E(a_\omega|s_i, s_j) \geq c.$$

In equilibrium, messages will only be sent if they are expected to change behavior. Therefore, the existence of a costly influence equilibrium depends on the cost of investment. In the two-person example, there are two regions where a separating equilibrium exists, one in which individuals are influenced by messages if they have received a bad signal and another in which individuals are influenced by messages only if they have received a good signal. Cost parameters consistent with a signaling equilibrium either fall in the range c_G, where

$$c_G = \{c : E(a_\omega|s_i = G, s_j = B) < c < E(a_\omega|s_i = G, s_j = G)\}$$

so that the cost falls between the expected value of investing with one good signal and two good signals or c_B where

$$c_B = \{c : E(a_\omega|s_i = B, s_j = B) < c < E(a_\omega|s_i = G, s_j = B)\}$$

and the cost falls within the range where at least one good signal is needed to invest.

Therefore for costs in the range c_G, the expected change in investment by one's partner as a result of sending a credible message is

$$E(x_j|s_i = G, p_{ij} = 1) - E(x_j|s_i = G, p_{ij} = 0) = P(s_j = G|s_i = G)). \tag{2.1}$$

When costs fall in range c_G, the expected change in investment from a message is simply the probability that one's partner has received a good signal, conditional on receiving a good signal. For costs in the range c_B, the expected expected change in investment by one's partner as a result of sending a credible message is

$$E(x_j|s_i = G, p_{ij} = 1) - E(x_j|s_i = G, p_{ij} = 0) = P(s_j = B|s_i = G). \tag{2.2}$$

The expected change in investment from a credible message in cost range c_B is simply the probability of one's partner having received a bad signal, given that the individual received a good signal.

A separating equilibrium will always exist when costs fall in the range of c_G. When costs fall in this range, an individual j will change their investment behavior after receiving a message only when they have received a good signal. Therefore, the benefit of sending a signal is simply the expectation of the benefit from investment which will always be higher when individuals i has received a good signal. Furthermore, the probability of individual j responding to a message (which occurs only when individual j already received one good signal) is higher when individual i received a good signal: $P(s_j = G|s_i = G) > P(s_j = G|s_i = B)$. Therefore, when individuals receive a good signal in cost range c_G, the benefits to sending messages will always be higher when $s_i = G$ than when $s_i = B$ and therefore a separating equilibrium will always exist.

When costs fall in the range of c_B, it is only possible to successfully influence the actions of individuals who have received a signal of B. Therefore, while the expected benefit from influencing another individual is higher when an individual receives a good signal: $E(a_\omega|s_i = G, s_j = B) > E(a_\omega|s_i = B, s_j = B)$, the probability that the attempt to influence will be successful is lower: $P(s_j = B|s_i = G) < P(s_j = B|s_i = G)$. Therefore, the existence of ρ that constitutes a separating equilibrium depends on q, the quality of information. Figure **??** illustrates that there are a range of q for which this condition is met. Note that as q approaches 1, it will no longer be possible that there exists some ρ that satisfies the condition that only an individual with a good signal will be willing to send a message. This is because as q approaches 1, it becomes very unlikely that investing in a message will be effective in changing behavior, since it is the probability of an individual receiving a bad signal is very low when another agent has received a good signal. Let us define

$$\begin{aligned}
\rho_{GG} &= E(a_\omega|s_i = G, s_j = G)P(s_i = G|s_j = G);\\
\rho_{GB} &= E(a_\omega|s_i = B, s_j = G)P(s_i = G|s_j = B);\\
\rho_{BB} &= E(a_\omega|s_i = B, s_j = B)P(s_i = B|s_j = B);
\end{aligned}$$

and $\bar{q} = \frac{a_G}{a_G + a_B}$. With these definitions, I formalize these intuitions with proposition 2.2.1.

I can characterize the existence of a separating equilibrium as follows

Case 1: For costs $c \in c_G$

- **Separating Equilibrium:** $p_{ij}(s_i = G) = 1$ and $p_{ij}(s_i = B) = 0$ when $\rho_{GB} < \rho < \rho_{GG}$
- **Silent Equilibrium:** $p_{ij}(s_i = G) = p_{ij}(s_i = B) = 0$ otherwise

Case 2: For costs $c \in c_B$

- **Separating Equilibrium:** $p_{ij}(s_i = G) = 1$ and $p_{ij}(s_i = B) = 0$ when $\rho_{BB} < \rho < \rho_{GB}$ and $q < \bar{q}$
- **Silent Equilibrium:** $p_{ij}(s_i = G) = p_{ij}(s_i = B) = 0$ otherwise

Case 3: For costs $c \notin c_B \cup c_G$

- **Silent Equilibrium:** $p_{ij}(s_i = G) = p_{ij}(s_i = B) = 0$.

All proofs are provided in the appendix.

2.2.4 Costly Messages with Known Heterogeneity

Now I consider what happens if individual benefits from investing differ, so that the individual benefit from the sum of others' investment is multiplied by $t_i \in [t_L, t_H]$ where $t_H > t_L$, so that t denotes the vector of types. I assume that individual types are common knowledge. Individual utility can now be represented as

$$U(x_i, x_{-i}, a_\omega, \rho, p_{ij}, t_i) = t_i a_\omega \left(\sum_j x_j \right) - c x_i - \rho \sum_j p_{ij}.$$

When individuals receive different returns from the investment and these returns are common knowledge, the required cost of effective communication will differ according

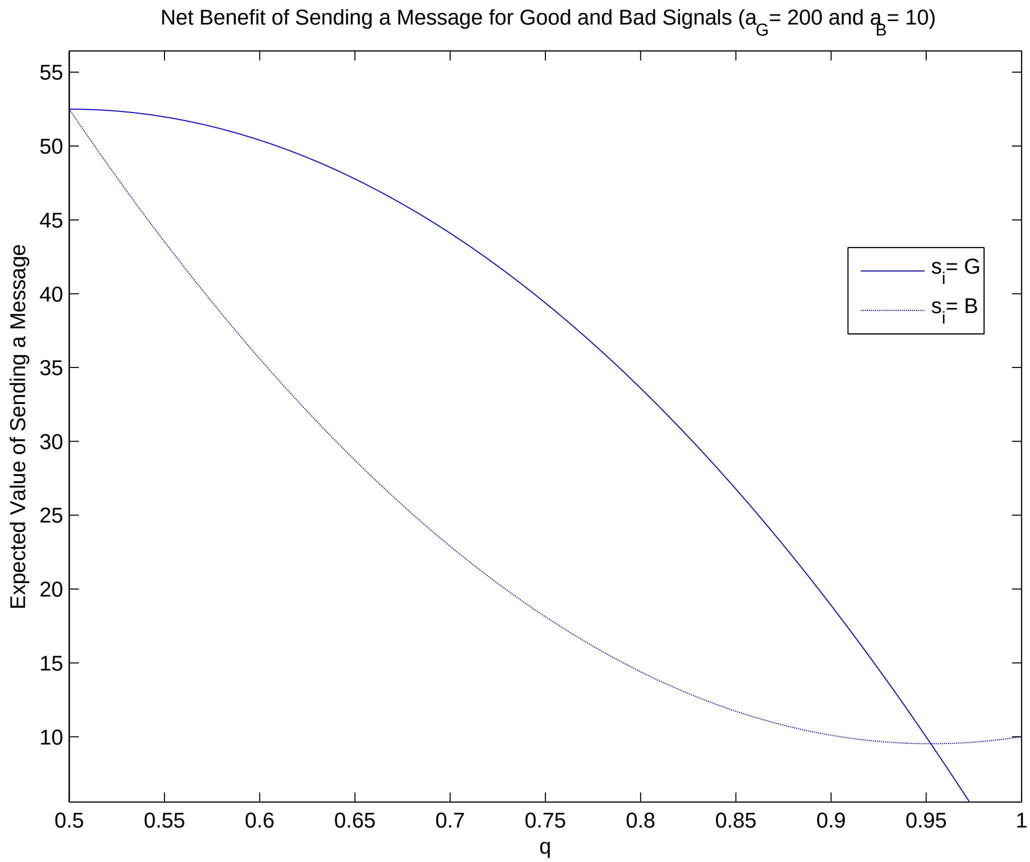


Figure 2.1. Representation of gains from communicating

to the types of the individuals sending and receiving messages. Now, in order for a separating equilibrium to exist, the following conditions must be met:

$$
\begin{aligned}
t_i E(a_\omega | s_i = G, p_{ij} = 1)\left(E(x_j | s_i = G, p_{ij} = 1) - E(x_j | s_i = G, p_{ij} = 0)\right) &\geq \rho; \\
t_i E(a_\omega | s_i = B, p_{ij} = 1)\left(E(x_j | s_i = B, p_{ij} = 1) - E(x_j | s_i = B, p_{ij} = 0)\right) &< \rho.
\end{aligned}
$$

As before, the expected benefit of sending a message must be higher than the cost for individuals who have received a good signal and the expected benefit of sending a message must be below the cost for individuals who have received a bad signal.

However, with individual heterogeneity, the expected benefit from sending a message depends on the the type of both the sender and receiver of the message. The expected benefit depends on the receiver's type because the receiver's type will determine when they might be influenced by a message (i.e., whether they are more likely to be influenced when they have received good or bad private information). The expected benefit depends on the sender's type because the benefit to their partner's investment depends on their type.

The existence of a separating equilibrium will again depend on the level of cost required to invest. However, there are now four relevant ranges of costs to consider:

$$c_B(t_i) = \{c : E(a_\omega|s_i = B, s_j = B) < \frac{c}{t_i} \leq E(a_\omega|s_i = G, s_j = B)\} \text{ for } t_i = H, L;$$
$$c_G(t_i) = \{c : E(a_\omega|s_i = G, s_j = B) < \frac{c}{t_i} \leq E(a_\omega|s_i = G, s_j = G)\} \text{ for } t_i = H, L.$$

The possibility of changing the investment decisions of one's partner depends on one's partner's type.

The optimal message now depends on t_i (because individuals who benefit more must spend more to signal) and t_j (because individuals with a higher type will be influenced to act at a lower information threshold than individuals with a lower type). The cost at which a separating equilibrium exists for credible messages is increasing in t_i and decreasing in t_j. Furthermore, when individuals have differing returns from investment, not all individuals will make the same decision with respect to sending and receiving messages. It may be that certain types do not receive messages in equilibrium, while other types never send messages in equilibrium. Let us define

$$\begin{aligned}
\rho_{GG}(t_i) &= t_i E(a_\omega | s_i = G, s_j = G) P(s_i = G | s_j = G); \\
\rho_{GB}(t_i) &= t_i E(a_\omega | s_i = B, s_j = G) P(s_i = G | s_j = B); \\
\rho_{BB}(t_i) &= t_i E(a_\omega | s_i = B, s_j = B) P(s_i = B | s_j = B).
\end{aligned}$$

These results are summarized in proposition 2.2.2:

I can characterize the existence of a separating equilibrium as follows:

Case 1: For costs $c \in c_G(t_j)$

- **Separating Equilibrium:** $p_{ij}(s_i = G) = 1$ and $p_{ij}(s_i = B) = 0$ when $\rho_{GB}(t_i) < \rho < \rho_{GG}(t_i)$
- **Silent Equilibrium:** $p_{ij}(s_i = G) = p_{ij}(s_i = B) = 0$ otherwise.

Case 2: For costs $c \in c_B(t_j)$

- **Separating Equilibrium:** $p_{ij}(s_i = G) = 1$ and $p_{ij}(s_i = B) = 0$ when $\rho_{BB}(t_i) < \rho < \rho_{GB}(t_i)$ and $q < \bar{q}$
- **Silent Equilibrium:** $p_{ij}(s_i = G) = p_{ij}(s_i = B) = 0$ otherwise.

Case 3: For costs $c \notin c_B(t_j) \cup c_G(t_j)$

- **Silent Equilibrium:** $p_{ij}(s_i = G) = p_{ij}(s_i = B) = 0$.

2.3 Experimental Design

The theoretical model was tested in a laboratory experiment using groups of size two.

2.3.1 Experimental Treatments

In all three treatment, individuals decide whether to make an investment. The success of their investment depends on the realization of the state of the world. They receive payoffs from their own investment and their partner's investment. In all of our treatments the value of investing in the good state of the world (purple) was 200 points, while the value of investing when the state of the world was bad (green) was 10 points. The cost of investing was fixed at 125 points and paid only for their own investment.[3]

In the **Control** treatment, individuals made their decisions about whether to invest without receiving any information from their partner. In the **Free Messages** treatment, individuals may send a message at no cost. In the **Costly Messages** treatment, the price of the message is drawn from a uniform distribution between 10 and 120 points. The states, information signals and messages were simulated in the experiment as follows:[4]

States: Each of the two states was a jar of marbles called the green jar and the purple jar.[5] Purple represents a good state of the world ($\omega = G$) while green represents a bad state of the world ($\omega = B$). In the experiment, each jar contained 10 marbles: the green jar consisted of 6 green marbles and 4 purple marbles while the purple jar consisted of 6 purple marbles and 4 green marbles. At the outset of each period, one of the jars was chosen at random, with each jar being equally likely. A representation of the image seen by subjects to explain how the state of the world is chosen is presented in figure **??**.

Signals: In each round of the experiments, subjects received a signal consisting of one draw from the jar of marbles with replacement. The probability of the marble chosen being the same as the color of the jar was 60%. We therefore simulate a quality of information $q = 0.6$.

Messages: In some treatments, subjects could send a message to their partner con-

[3] In all experiments, one point was equivalent to 1 cent.
[4] The full experimental instructions are available at: http://www.hss.caltech.edu/~mmcconnell/instructions.zip
[5] Purple and green were considered to be neutral colors with no obvious external affiliation.

taining a suggestion about which action to take. If a subject decided to send a message, they could send one of the following two messages:

Suggestion A: Invest

Suggestion B: Don't Invest

A body of experimental evidence suggests that subjects are averse to lying (Sanchez-Pages and Vorsatz 2009; Kartik 2009; Hurkens and Kartik 2009; Gneezy 2005). This experiment was designed to abstract away from lying in order to focus specifically on whether subjects attempt to influence others. Therefore, the messages were chosen so that subjects did not have to make statements about what their own decision would be, thereby removing the possibility that subjects made decisions that depended on norms about lying. The messages were chosen to approximate attempts to influence the actions of others as closely as possible.

The costs and benefits of investment, the distribution of message costs and the distribution of types were all common knowledge. Message costs were identical for every person in an experimental session and this was common knowledge. In each round, individuals were randomly assigned a partner from the pool of all other subjects in their experiment. This assignment process was common knowledge.

Prior to the experiment, subjects were given a worksheet which calculated the expected value of investing in two cases: (a) the subject receives only one signal (i.e., only their private information) and (b) the subjects receives two signals (i.e., their information and their partner's information).[6] At the start of the experiment, subjects received instructions. After the end of the instructions, subjects participated in 30 periods of decisions. Each period consisted of the following five steps:

(1) Information In the information stage, subjects did not take any action. They

[6]Subjects were provided with calculations of expected value in order to reduce the number of simple calculation errors in the experiment.

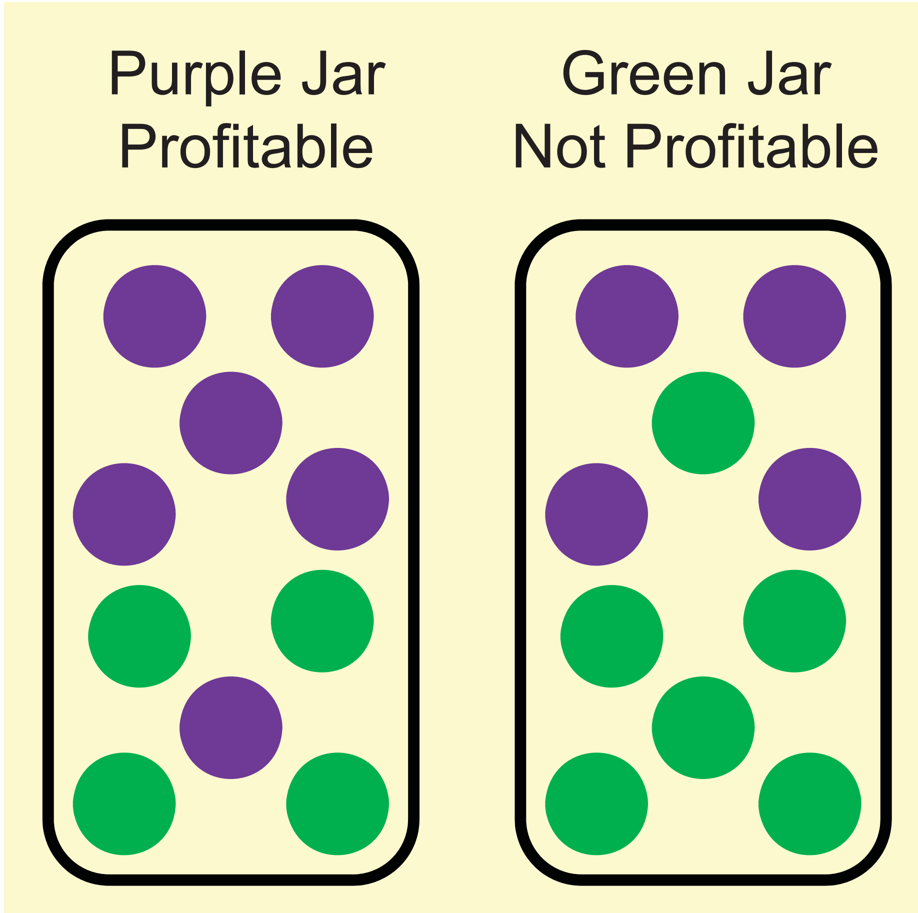


Figure 2.2. States and information

were informed about: the signal they received that period, their payoff parameter in that period, their partner's payoff parameter in that period, the two messages that could be sent and the cost of sending a message (if applicable).

(2) Sending In the sending stage, subjects could choose (simultaneously) to send a message to their partner.

(3) Receiving In the receiving stage, subjects were informed of whether they received a message from their partner and which message they received.

4) Decision In the decision stage, subjects decided whether or not to invest.

5) Feedback In the feedback stage, subjects received information about the decisions made by their partner and the payoff they received in that period. Subjects recorded their decisions and the payoff they received in each period on a record sheet.[7]

Subjects in all treatments first participated in 15 rounds of a **Homogeneous** condition and then participated in 15 rounds of a **Heterogeneous** condition. In the **Homogeneous** condition, all subjects had the same payoff parameter of one. In the **Heterogeneous** condition, subjects were assigned to either a payoff parameter of one (1) or a payoff parameter of one point five (1.5). Subjects were assigned to one payoff parameter for each condition and subjects were assigned to a different payoff parameter than their partner.

Table 2.1 represent the parameters implemented in the experiment.

Table 2.1. Parameters

Variable	**Value**
q	0.60
a_G	200
a_B	10
c	125
t_H	1.5
t_L	1
p	$\sim U[10, 120]$

2.3.2 Theoretical Predictions

The payoff and strategic structure of the model is maintained throughout the experiment. We can use the theoretical model to make predictions about behavior for the parameters chosen in the experiment.

[7] The record sheet was designed to facilitate subjects' understanding of the experiment, as they would be able to observe the outcomes for different decisions.

Investment Predictions

First we consider the predictions made by the model about individual investment in the three treatments. In table 2.2 we provide the expected value of investing when individuals receive only one signal (only the private information) and when individual receive two signals (their information and their partner's information). Based on these expected values, we also provide theoretical predictions for when individuals should invest, given their information. In the Control, individuals have only their own signal and no ability to communicate and will be constrained to make the best decision given only their own signal. Therefore, in the Control treatment, we would expect that individuals with a low type should never invest, since investing always has negative expected value. Individuals with a high type should invest in the Control treatment if and only if they have received a good signal.

Table 2.2. Theoretical predictions about investment behavior

Low Type		
Signal(s)	EV of Investment	Theoretical Investment Prediction
Good	124	NO
Bad	86	NO
Good, Good	142	YES
Good, Bad	105	NO
Bad, Bad	68	NO
High Type		
Signal(s)	EV of Investment	Theoretical Investment Prediction
Good	186	YES
Bad	129	YES
Good, Good	212	YES
Good, Bad	158	YES
Bad, Bad	103	NO

In the Free Messages treatment, messages cannot be informative, since individuals have an incentive to misrepresent their information and no way to credibly signal their information. Therefore, the theoretical model predicts that there will be no difference in the investment rate in the Control treatment and the Free Messages treatment.

When messages are costly, individuals may be able to send messages that are informative, provided that the message cost is high enough so that individuals who receive a bad signal will not be willing to send a message. If the price of a message is above the expected benefit of sending a message for an individual who has received a bad signal, the message will be credible. In table 2.2, we present the expected benefit of sending a message by individual type and signal. Therefore, we predict an individual should consider a message to be informative in the following ranges:

- They are a low type, their partner is a low type and the cost of the message is $\rho > 50.4$;
- They are a low type, their partner is a high type and the cost of the message is $\rho > 35.6$;
- They are a high type, their partner is a low type and the cost of the message is $\rho > 75.6$.

When these conditions are met in the experiment, we will call this the *Informative Message Range (receive).*

Within these ranges, individuals will effectively receive two signals. They will receive their own signal directly and they will be informed about the signal received by their partner indirectly. This is because if individuals receive a message suggesting that they invest, they know it can only be truthful since sending a message would have negative expected value if their partner had received a bad signal. If they do not receive a message, they can infer that their partner has received a bad signal. In the presence of two independent signals, individuals with a low type should invest if and only if they receive two good signals. Individuals with a high type should invest if and only if they receive at least one good signal.

Communication Predictions

In addition to considering what the theory predicts about individual investment decisions, we also consider the predictions made by the theory in terms of when individuals should communicate in the Free Messages and Costly Messages treatments. As discussed above, informative communication is not possible in the Free Messages treatment - since no messages can be effective, individuals will not communicate.[8]

Table 2.3. Theoretical predictions about separating equilibrium for sending a message

Signal	Low to Low	Low to High	High to Low
Good	73.6	50.4	110.4
Bad	50.4	35.6	75.6

In the Costly Messages treatment, individuals will be able to send informative messages if and only if the cost of the message exceeds the expected benefit of sending a message for an individual receiving a bad signal. Individuals will therefore be willing to send a message if the cost exceeds the level required to signal their good information but still has positive net expected benefit. From table 2.3, we can see that individuals will be willing to send a message if and only if they following conditions are met:

- They are a low type, their partner is a low type and the cost of the message is $50.4 < \rho < 73.6$ and they have received a good signal.
- They are a low type, their partner is a high type and the cost of the message is $35.6 < \rho < 50.4$ and they have received a good signal.
- They are a high type, their partner is a low type and the cost of the message is $75.6 < \rho < 110.4$ and they have received a good signal.

When these conditions are met in the experiment, we will call this the *Informative Message Range (send)*.

[8]This is because we have assumed that when indifferent, individuals will not communicate.

2.3.3 Implementation

A total of 54 subjects recruited from Caltech participated in the experiments. Subjects were recruited by emails in the standard procedure for the laboratory. Six subjects participated in each session for 30 rounds of decision making. The implementation of the sessions is summarized in table 2.4. Total payments (including a $5 show-up fee) ranged from a minimum of $12.35 to a maximum of $33.45.

Table 2.4. Experimental sessions

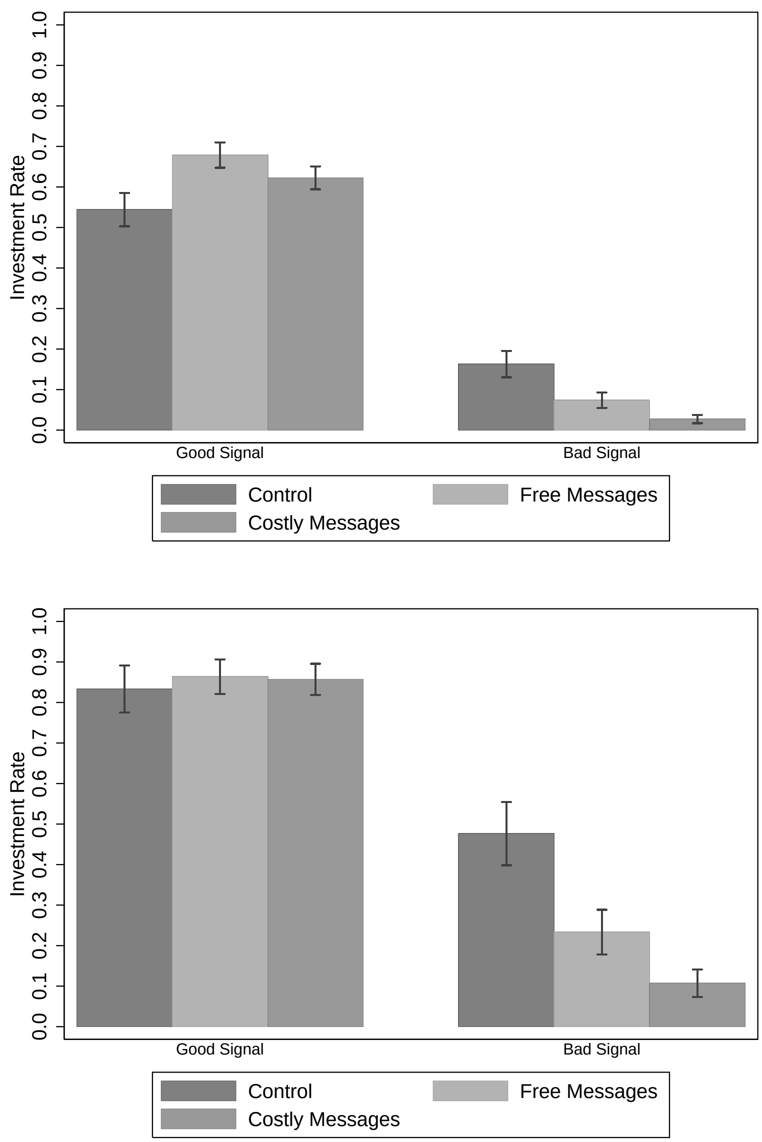
Treatment	# of Sessions	# of Subjects	Average Payoff
No Messages (Control)	4	24	$20.33
Free Messages	3	18	$24.33
Costly Messages	2	12	$20.40

2.4 Results

2.4.1 Aggregate Behavior

We first consider investment rates across the treatment. Figure **??** illustrates investment rates across treatments for good and bad signals. We notice significantly higher rates of investment than what the theory would predict. This over-investment means that individuals may not have had a strong incentive to influence the behavior of others, since investment often occurred without any additional information from communication.

We also consider the likelihood of communication across the treatments. Figure **??** illustrates communication rate (the probability of sending a message in a given period) across the Free and Costly Messages treatments for low types communicating to low types, low types communicating to high types and high types communicating to low types. We see substantially higher communication rates in the Free Messages



Key: (A) Investment Rates by Signal for Low Types. (B) Investment Rates by Signal for High Types. Error bars denote SEMs.

Figure 2.3. Treatment effects on investment rates

treatment than the Costly Messages treatment. Furthermore, we see that individuals in both treatments are more likely to send a message when they have received a good signal. This suggests that the messages in Free Message treatment contain more information than we would predict.

In order to consider the welfare effects of treatment, we provide summary statistics of profits throughout the experiment in table 2.5.

Table 2.5. Summary statistics (means with standard errors in parentheses)

Treatment	Profits
No Messages (Control)	51.11
	(7.26)
Free Messages	64.44
	(6.10)
Costly Messages	51.32
	(4.83)
N	1620

We do not see any evidence that the outcomes across treatments are consistent with a separating equilibrium, where messages are informative only when individuals invest above some threshold which reveals their type. In fact, it appears that the ability to communicate with another person when making an investment decision is likely to improve an individual's decision making, regardless of whether those messages are costly. Since messages are costly and similar outcomes can be achieved when messages are free, messages that are costly will not necessarily improve welfare as our model would have predicted. We can see confirmation of this intuition in table 2.5 where we present summary statistics on profits throughout the experiment. As further evidence, in figure **??**, we present the empirical distribution functions of profits in each of the three treatments. We can see that payoffs in the free messages treatment stochastically dominate payoffs in the control. In the paid treatment, the fact that there is no improvement in the ability to communicate, but an increased

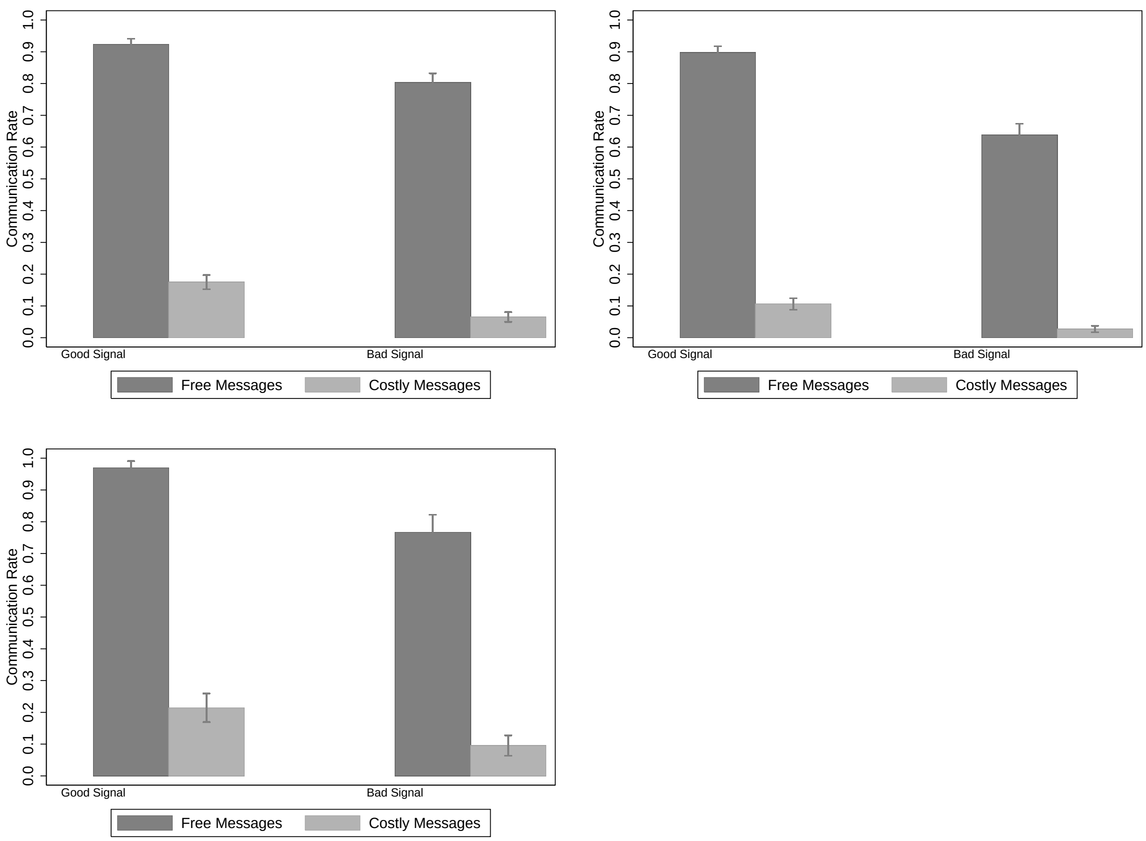


Figure 2.4. Treatment effects on communication rates

Key: (A) Communication rates for low types communicating with low types (by signal). (B) Communication rates for low types communicating with high types (by signal). (C) Communication rates for high types communicating with low types (by signal). Error bars denote SEMs.

cost to communication implies lower profits in the aggregate.[9]

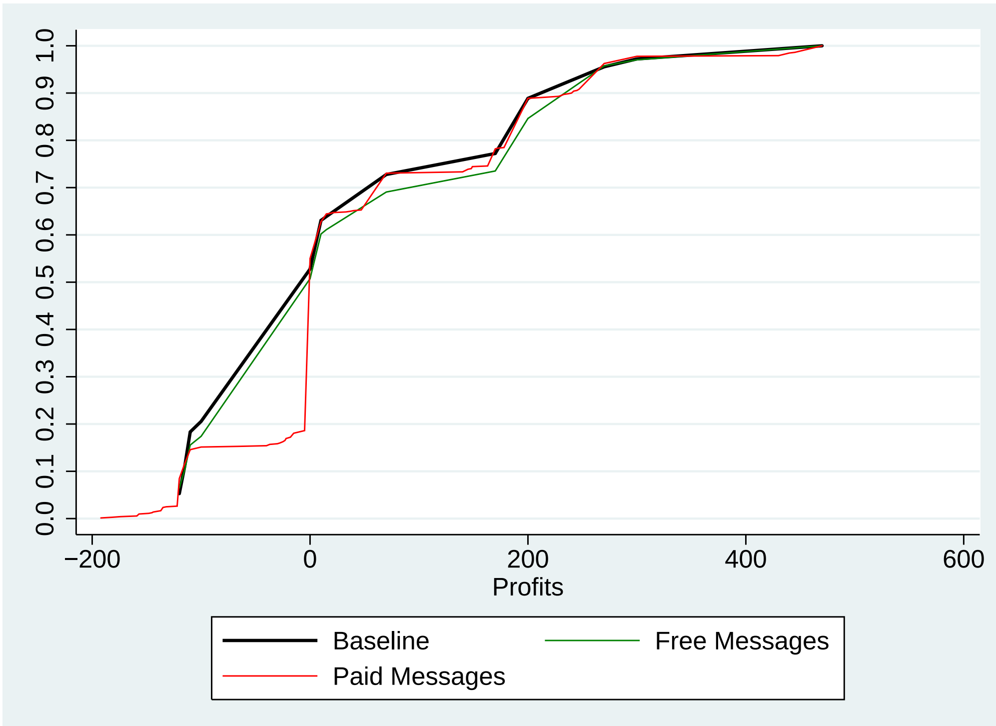


Figure 2.5. Empirical CDF of profits

We then provide a measure of whether individuals made the correct decision from the point of view of the decision that provides greater expected value. We define a "correct" decision as the decision to adopt when the aggregate information from both individuals implies that investing has positive expected value and not to adopt when the information available implies negative expected value. This measure allows us to compare decisions to the experiment to the ones that would have been made in the presence of complete information sharing.[10]

Using a nonparametric Mann-Whitney test, we can see that the subjects are significantly more likely to make the correct decision when they have the ability to

[9] A Mann-Whitney test confirms that profits are significantly lower in the paid treatments, z=1.89.

[10] This measure also considers decision making within the actual draws of the experiment. That is, subjects can only do as well as the information they are given.

send messages, regardless of whether the messages are costly.[11] Furthermore, subjects are equally likely to make the correct decision, regardless of whether communication is costly.[12]

However, this analysis does not consider possible dependencies that arise because the same individual makes many decisions in the experiment. Furthermore, additional dependences may arise at the level of the experimental session since through the random assignment process, individuals will repeatedly interact with the same individuals in their experimental session. Therefore we develop an estimation strategy that allows us to account for variation due to differences in individuals and at the level of the session. We estimate regressions using random effects at the level of the individual, clustering the standard errors at the level of the experimental session.

In table 2.6 model 1 we consider the effects of our treatments on the probability of making the "correct" decision. We consider the effect of the ability to send any message (free or paid) and find that the ability to send messages increases the probability of making the correct decision by 10%. Confirming the results from nonparametric tests, we see no additional effect of the probability of making the correct decision when messages are costly. Model 2 of table 2.6 also suggests that the price of sending a message does not affect the probability of making a correct decision.

2.4.2 Individual Behavior

In order to understand why costly messages did not improve accuracy and welfare, we examine two components of individual behavior: the decision to invest and the decision to send a message.

[11] Mann-Whitney tests yield z-statistics of -3.29 and -2.36 when comparing free and paid treatments to control.

[12] Mann-Whitney test statistic of 1.25.

Table 2.6. Correct decisions

Random Effects Estimation		
	(Correct decision)	
Variable	Model 1	Model 2
High Type	-0.027	-0.024
	(0.036)	(0.037)
Partner High Type	-0.016	-0.013
	(0.028)	(0.028)
Message Treatment	0.103**	0.095*
	(0.047)	(0.051)
Paid Message Treatment	-0.032	-0.007
	(0.035)	(0.045)
Message Cost	-	-0.000
	-	(0.000)
ρ	0.03***	0.04***
Breusch Pagan LM statistic	(21.47)	(21.52)
Constant	0.646***	0.653***
	(0.032)	(0.036)
Observations	1620	1620
Number of individuals	54	54

Robust Clustered standard errors in parentheses
*significant at 10%, **significant at 5%, ***significant at 1%

2.4.2.1 Investment Decisions

In table 2.7 we can see that being in a treatment where it is possible to communicate with your partner decreases the probability of making an investment by approximately 10%. However, when we consider the investment decisions of subjects who *did* receive a message, we see that on the whole, they are approximately 4% more likely to invest. Investment responses are consistent with the intuition of the equilibrium strategies predicted by our model: subjects who are in the message treatment and receive no message are less likely to invest, while subjets who do receive a message are more likely to invest. Furthermore, in model 3 of table 2.7 we can see that subjects who receive messages are 4% more likely to invest for every 10 point increase in the cost of sending that message. This evidence suggests that subjects' investment decisions do respond more to messages when individuals pay some cost to send those messages.

We also consider whether individuals are more likely to respond to a message when it falls in the equilibrium range. In model 3 of table 2.7 we consider the subset of individuals in the paid message treatment who received a message. The variable *Informative Message Range (receive)* is a dummy equal to 1 if the cost of the message received was above the level required to signal that a good message was received. We find that individuals are significantly more likely to listen to a message that falls within the equilibrium range and that the increase is large. Subjects are 62.7% more likely to invest if they receive a message in the equilibrium range. We also include an interaction between *Informative Message Range (receive)* and the message cost. Once individuals have gotten above the threshold required to signal their good informaiton, our model would predict that messages would be equally effective, regardless of their price. We see that subjects do not respond significantly more to messages that are more costly within the equilibrium range. This also provides evidence against the idea that subjects might be more likely to invest when they receive a costly message because of reciprocity.

2.4.2.2 Communication Decisions

We now examine individual decisions about when to communicate in our experiment. We use regression analysis to explain subjects' decisions of whether to send a message to their partner. For this analysis, we confine our analysis to treatments where subjects could send messages. In model 4 if table 2.7, we see that subjects are 50% less likely to send messages in the paid treatment than when messages are free. Furthermore, subjects' willingness to send messages *decreases* with message costs. For each 10 point increase in the cost of sending a message, subjects are 3.7% less likely to send a message.

In model 4 of table 2.7 we consider whether subjects are more likely to send a message under the conditions predicted by our theory: a) they receive a good signal and b) the message price implies the existence of a a separating equilibrium.[13] The variable *Informative Message Range (send)* indicates a message cost within the equilibrium range **and** a good signal. We find that subjects are 8% more likely to send messages when the message cost falls within the equilibrium range. Within this equilibrium range, model 5 of table 2.7 shows that subjects are 2% less likely to send a message for every 10 point increase in the cost of the message.

If we consider the upper tail of the 95 % confidence interval on the probability of sending a message in the equilibrium range, subjects are at most 18% more likely to send a message when the cost is in the equilibrium range. However, from our analysis in model 3 of table 2.7, we know that subjects who received costly messages in the equilibrium range were more than 60% more likely to act on those messages. This asymmetry suggests that we see more of the intuition behind the burning money equi-

[13]Note that the definition of an equilibrium for sending messages is different from the equilibrium for acting on messages. When considering how investment decisions respond to messages, we consider an equilibrium price to be any price above the price required to separate a good signal from a bad signal. When considering how individuals decide whether to send messages, we consider an equilibrium price range to be above the price required for a separating equilibrium and below the price at which the message will no longer provide increases in expected returns.

Table 2.7. Determinants of investment and communication

Random Effects Estimation					
	(Invest)			(Communicate)	
Variable	Model 1	Model 2	Model 3	Model 4	Model 5
High type	0.166***	0.166***	0.207***	0.045	0.067*
	(0.046)	(0.046)	(0.074)	(0.028)	(0.037)
Partner High Type	-0.108***	-0.110***	-0.085	-0.032	0.036
	(0.046)	(0.046)	(0.074)	(0.028)	(0.037)
Good Signal	0.572***	0.574***	-	0.141***	0.155***
	(0.044)	(0.045)	-	(0.033)	(0.034)
Message Treatment	-0.097*	-0.083	-	-	-
	(0.061)	(0.075)	-	-	-
Received Message	0.147***	0.097	-	-	-
	(0.061)	(0.075)	-	-	-
Message Cost	-	0.004***	-	-0.004***	-0.004***
	-	(0.002)	-	(0.001)	(0.001)
Received Message*Message Cost	-	-	0.007	-	-
	-	-	(0.006)	-	-
Informative Message (Receive)	-	-	0.627**	-	-
	-	-	(0.280)	-	-
Informative Message (Receive)*Message Cost	-	-	-0.010	-	-
	-	-	(0.008)	-	-
Costly Messages	-	-	-	-0.495***	-0.496***
	-	-	-	(0.067)	(0.067)
Informative Message (Send)	-	-	-	-	0.089**
	-	-	-	-	(0.045)
Informative Message (Send)*Message Cost	-	-	-	-	-0.003**
	-	-	-	-	(0.001)
ρ	0.14**	0.14**	0.00	0.17**	0.18**
Breusch Pagan LM statistic	(407.95)	(399.97)	(0.02)	(496.67)	(513.14)
Constant	0.120***	0.119***	0.321***	0.769***	0.757***
	(0.061)	(0.061)	(0.082)	(0.019)	(0.021)
Observations	1620	1620	79	1260	1260
Number of individuals	54	54	22	42	42

Robust Clustered standard errors in parentheses
*significant at 10%, *significant at 5%, *significant at 1%
Model 3 represents the subset of individuals in the paid treatment who sent a message.
Models 4 and 5 represent the subset of individuals in the free or paid message treatments.

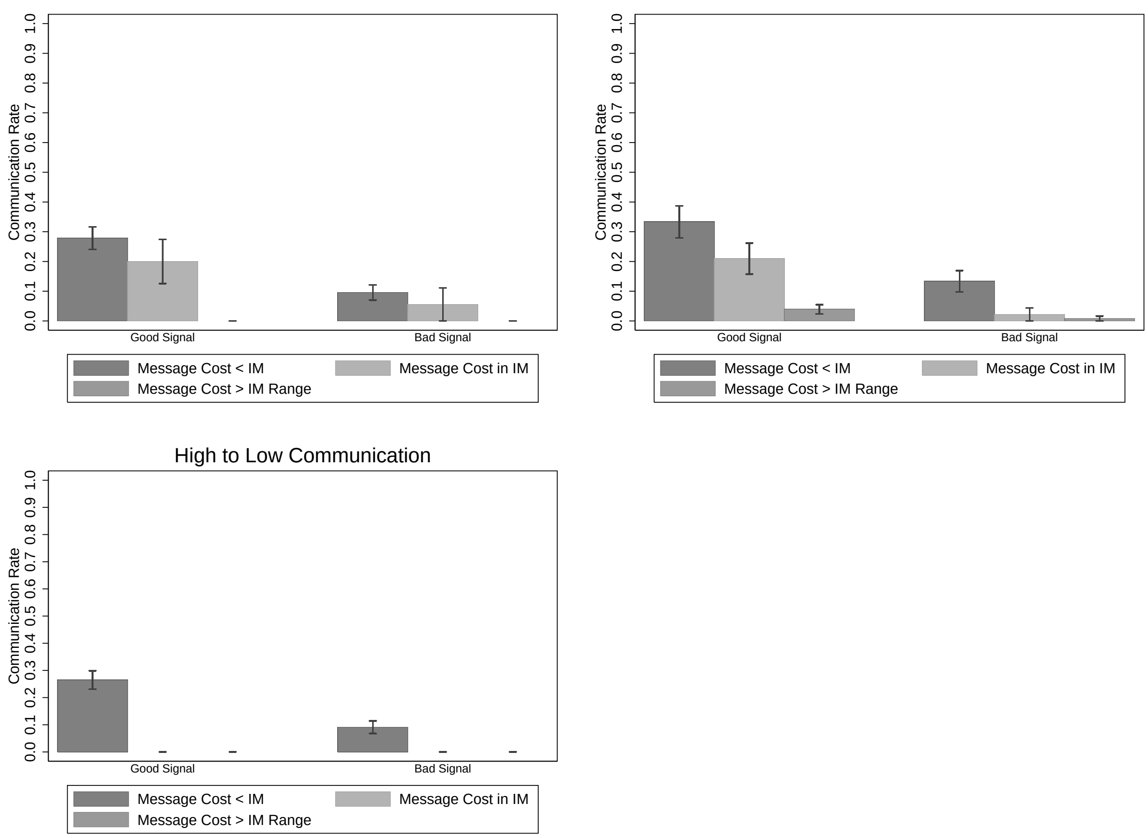
librium for subjects' behavior as receivers than as senders. When sending messages, subjects did not fully account for the relatively greater effectivness of messages sent at a cost and favored messages that are sent at low cost.

By examining individual behavior in different cost ranges more closely, we can see that subjects are actually more likely to send messages when the cost fall below the range of informative messages than when the cost falls within the range where a separating equilibrium exists. The adherence of individual communication behavior to the predictions of the theory can be seen in figure **??** where we examine communicate rates for costs below the *Informative Message Range*, in the *Informative Message Range* and above the *Informative Message Range*. We can see that for all communication by all types, individuals are not more likely to send messages when the cost falls in the range where a separating equilibrium exists, than they are when costs fall below the *Informative Message Range*.

2.4.2.3 Learning

In order to determine whether subjects' behavior changes over time, we consider estimations that examine the possibility of learning in the experiment. We consider how subjects investment decisions differ over time in models 1 and 2 of table 2.8. Subjects are between 5% and 6% less likely to invest in later periods. Subjects are also more willing to invest if they have been assigned a high type. Subjects are not statistically significantly more likely to invest when their previous investment decision gave them positive payoffs. Furthermore, there is no difference in the responsiveness to a successful past investment decision when costly messages are available.

We also see no evidence that subjects are more likely to invest when their partner has invested in the previous period and no evidence that responsiveness to past investment success is greater depending on the cost of messages in that period. There is limited evidence suggesting that subject behavior with respect to investment adapts



Key: (A) Communication rates for low types communicating with low types (by signal). (B) Communication rates for low types communicating with high types (by signal). (C) Communication rates for high types communicating with low types (by signal). Error bars denote SEMs.

Figure 2.6. Communication rates for message costs below, within and above the cost range in which a separating equilibrium exists

or that learning about when to invest differs depending on the cost of messages.

In model 2 of table 2.8 we consider how subjects' willingness to send messages changes over time. On average, subjects do appear to exhibit some learning in the experiment with respect to decisions about communication. Subjects are 10% more likely to send a message if they sent a message that resulted in investment by their partner in the previous period. We also examine whether subjects are more responsive to prior success based on the cost of the message sent, but do not see any statistically significant evidence of a different propensity to learn from the past that depends on the cost of a message. We see no evidence that subjects are more likely to send messages when their partner has sent a message, regardless of the cost of that message.

2.4.2.4 Informative Messages

The main intuition behind the separating equilibrium predicted by our theory is driven by the fact that individuals are payoff maximizers; they will not reveal their signal truthfully when they have incentives to misrepresent their information. However, the relative success of the free messages treatment suggests that the problem of noninformative messages was not nearly as large as we would expect. In fact, we find 61% of the time, individuals in the free treatment either accurately revealed their signal or did not send any message at all. In contrast, 95% of the time, individuals in the paid treatment accurately revealed their signal or did not send a message at all. Furthermore, as we can see in model 3 of table 2.8, the probability of individuals revealing their information accurately[14] is increasing in the message cost. Individuals are 1% more likely to reveal their information accurately for every 10 point increase in message cost.

[14] We consider a message to reveal information accurately if you send a message that corresponds to your private information if you did send a message, or sending no message at all.

Table 2.8. Learning and truthful revelation

Random Effects Estimation			
	Learning		Revelation
	(Invested)	(Sent a Message)	(Informative)
Variable	Model 1	Model 2	Model 3
Successful investment in previous period	0.076	-	-
	(0.056)	-	-
Successful investment in previous period*Message cost	-0.001	-	-
	(0.001)	-	-
Partner invested in previous period	-0.006	-	-
	(0.007)	-	-
Partner invested in previous period*Message Cost	0.000	-	-
	(0.000)	-	-
Period	-0.006***	-0.003	-
	(0.002)	(0.003)	-
High Type	0.259***	0.104***	0.027
	(0.055)	(0.019)	(0.014)
Partner High Type	-0.010	0.024	-0.031**
	(0.036)	(0.018)	(0.014)
Good Signal	0.566***	0.144***	0.328***
	(0.047)	(0.034)	(0.108)
Message Cost	-0.001	-0.005***	-0.002**
	(0.001)	(0.001)	(0.000)
Messages	-0.027	-	-
	(0.051)	-	-
Successful message in previous period	-	-0.107***	-
	-	(0.030)	-
Successful message in previous period*Message cost	-	0.000	-
	-	(0.001)	-
Partner sent message in previous period	-	-0.001	-
	-	(0.008)	-
Partner sent message in previous period*Message cost	-	-0.000	-
	-	(0.000)	-
Messages Paid	-	-	0.226***
	-	-	(0.035)
ρ	0.00***	0.15***	0.02**
Breusch Pagan LM statistic	(353.84)	(585.77)	(4.80)
Constant	0.181**	0.555***	0.440***
	(0.088)	(0.122)	(0.064)
Observations	1566	1218	1260
Number of individuals	54	42	42

Robust Clustered standard errors in parentheses
*significant at 10%, *significant at 5%, *significant at 1%
First period decisions are excluded from model 1 and 2.
Model 2 and 3 are the subset of individuals who could send a message.

2.5 Conclusion

The current chapter provides a model that sheds light on when it is possible for individuals to influence the actions of others via communication. In our model, individuals can effectively influence their neighbors because of their private information, unlike other models of influence like Calvo-Armengol and Jackson where individuals can directly affect the costs of their neighbors. Therefore, my model can be applied to a class of problems in which individuals may benefit from others' investment because of complementarities but might not have be able to change the cost of their investment.

In my model, I illustrate that communication can only be informative when individuals are able to communicate by incurring some cost, thereby sending a signal which reveals their private information. In fact, practices designed to influence the actions of others, such as a willingness to travel or take time out for face-to-face meetings do not provide additional information content, but often come at a nontrivial cost.

I test the implications of this model using laboratory experiments which compare individuals' decisions with no ability to communicate, when only cheap talk is possible, and when they can send costly messages designed to influence. I find that individuals do appear to send informative messages in the cheap talk treatment, in contrast to the theoretical prediction. As a result, messages improve information quality even when they are merely cheap talk. When messages are costly, individuals are more likely to be influenced by messages that were sent at a higher cost, as predicted by the theory. However, when sending messages, individuals do not burn money to the extent that our theory would predict. As a result, we see welfare improvements over the control when messages are free but not when messages are costly.

Overall, our experimental results suggest that the ability to communicate can improve outcomes, even when individuals have some incentive to misrepresent their private information because of complementarities from their friends' decisions. While the model predicts that individuals need to "burn money" in order to effectively communicate, it may not always be welfare improving to develop institutions for costly communication, especially if individuals tend to reveal their information accurately, even without the ability to send a costly signal.

2.6 Appendix

Proposition 2.2.1 Proof

Let us define the benefit of individual i sending a message to j as $b(p_{ij} = 1|s_i)$.

Case 1

When $c \in c_G$, j will not invest unless he receives two good signals. Hence if messages are credible:

$$E(x_j|p_{ij} = 1, s_j = B) = 0;$$
$$E(x_j|p_{ij} = 1, s_j = G) = 1;$$
$$E(x_j|p_{ij} = 0) = 0.$$

Using equation (2.1) from section 2.2.3 we can see that $b(p_{ij} = 1|s_i) = \rho_{GG}$ and $b(p_{ij} = 1|s_i) = \rho_{GB}$:

$$\begin{aligned}
b(p_{ij} = 1|s_i = G) &= E(a_\omega|s_i = G, s_j = G)\left(E(x_j|p_{ij} = 1, s_j = G) - E(x_j|p_{ij} = 0, s_j = G\right) \\
&= E(a_\omega|s_i = G, s_j = G)P(s_i = G|s_j = G) \\
&= \rho_{GG}; \\
b(p_{ij} = 1|s_i = B) &= E(a_\omega|s_i = B, s_j = G)\left(E(x_j|p_{ij} = 1, s_j = G) - E(x_j|p_{ij} = 0, s_j = G\right) \\
&= E(a_\omega|s_i = B, s_j = G)P(s_i = B|s_j = G) \\
&= \rho_{GB}.
\end{aligned}$$

In order to illustrate that a separating equilibrium exists for all ρ in the range $\rho_{GB} < \rho < \rho_{GG}$, we need only show that $\rho_{GG} > \rho_{GB}$ for all q. Because we have shown that

$\rho_{GG} = b(p_{ij} = 1|s_i = G)$ and $\rho_{GB} = b(p_{ij} = 1|s_i = G)$ we can write

$$\begin{aligned}\rho_{GG} &= E(a_\omega|s_i = G, s_j = G)P(s_i = G|s_j = G) &&= \tfrac{a_G q^2 + (1-q)^2 a_B}{(1-q)^2+q^2}\left((1-q)^2 + q^2\right) \\ & &&= a_G q^2 + (1-q)^2 a_B; \\ \rho_{GB} &= E(a_\omega|s_i = G, s_j = G)P(s_i = G|s_j = G) &&= \left(\tfrac{1}{2}a_G + \tfrac{1}{2}a_B\right)(1 - (1-q)^2 - q^2).\end{aligned}$$

Therefore we need only to show that $a_G q^2 + (1-q)^2 a_B > (q - q^2)a_G + (q - q^2)a_B$ for all q. This follows from the observation that

$$a_G q^2 + (1-q)^2 a_B - (q - q^2)a_G - (q - q^2)a_B = q(2q-1)a_G - (1-q)(2q-1)a_B.$$

Since $q > 1 - q$ for all $q > \frac{1}{2}$ and $a_G > a_B$, we know that $\rho_{GG} > \rho_{GB}$ and we have shown that a separating equilibrium exists for all ρ such that $\rho_{GG} > \rho > \rho_{GB}$ and outside of this range, there will be no communication.

Case 2

When $c \in c_B$, j will not invest unless he receives at least one good signal. Hence if messages are credible:

$$E(x_j|p_{ij} = 1, s_j = B) = 1;$$
$$E(x_j|p_{ij} = 1, s_j = G) = 1;$$
$$E(x_j|p_{ij} = 0, s_j = B) = 0;$$
$$E(x_j|p_{ij} = 0, s_j = G) = 1.$$

Using equation (2.2) in section 2.2.3 we now show that $b(p_{ij} = 1|s_i = G) = \rho_{GB}$ and $b(p_{ij} = 1|s_i = B) = \rho_{BB}$:

$$\begin{aligned}
b(p_{ij} = 1|s_i = G) &= E(a_\omega|s_i = G, s_j = B)\,(E(x_j|p_{ij} = 1, s_j = B) - E(x_j|p_{ij} = 0, s_j = B) \\
&= E(a_\omega|s_i = G, s_j = G)P(s_j = G|s_i = G) \\
&= \rho_{GB}; \\
b(p_{ij} = 1|s_i = B) &= E(a_\omega|s_i = B, s_j = B)\,(E(x_j|p_{ij} = 1, s_j = B) - E(x_j|p_{ij} = 0, s_j = B) \\
&= E(a_\omega|s_i = B, s_j = G)P(s_j = G|s_i = G) \\
&= \rho_{BB}.
\end{aligned}$$

In order to illustrate that a separating equilibrium exists in the range $\rho_{GB} < \rho < \rho_{GG}$, we need only show that $\rho_{GB} > \rho_{BB}$ for all q. Because we have shown that $\rho_{GG} =$

$b(p_{ij} = 1|s_i = G)$ and $\rho_{GB} = b(p_{ij} = 1|s_i = G)$ we can write

$$\begin{aligned}
\rho_{GB} &= b(p_{ij} = 1|s_i = G) &=& E(a_\omega|s_i = G, s_j = B)P(s_j = B|s_i = G) \\
& &=& \left(\tfrac{1}{2}a_G + \tfrac{1}{2}a_B\right)(2q - 2q^2); \\
\rho_{BB} &= b(p_{ij} = 1|s_i = B) &=& E(a_\omega|s_i = B, s_j = B)P(s_j = B|s_i = B) \\
& &=& \tfrac{a_G(1-q)^2 + q^2 a_B}{(1-q)^2+q^2}\left((1-q)^2 + q^2\right) \\
& &=& a_G(1-q)^2 + q^2 a_B.
\end{aligned}$$

Therefore we need to show that:

$$\left(\frac{1}{2}a_G + \frac{1}{2}a_B\right)(2q - 2q^2) > a_G(1-q)^2 + q^2 a_B$$

if and only if $q < \bar{q}$. Let us now consider:

$$\begin{aligned}
\rho_{GB} - \rho_{BB} &= \tfrac{1}{2}(a_G + a_B)(2q - 2q^2) - a_G(1-q)^2 - q^2 a_B \\
&= (-2a_G - 2a_B)q^2 + (3a_G + a_B)q - a_G.
\end{aligned}$$

The roots of this quadratic function are $q = \frac{1}{2}$ and $q = \frac{a_G}{a_G + a_B} = \bar{q}$.

$\rho_{GB} - \rho_{BB}$ is a quadratic function that is positive in the interval $(\frac{1}{2}, 1]$. We know that $\rho_{GB} - \rho_{BB} > 0$ when $q = 1$ since $P(s_j = B|s_i = G) = 0$ when $q = 1$. Therefore $\rho_{GB} - \rho_{BB} \geq 0$ when $q \in (\frac{1}{2}, \bar{q}]$ and $\rho_{GB} - \rho_{BB} < 0$ when $q \in (\bar{q}, 1]$ and a separating equilibrium exists for all ρ in $\rho_{BB} < \rho < \rho_{GB}$ if and only if $q < \bar{q}$.

Case 3

For costs c below costs in the range c_B, $E(x_j|p_{ij} = 1) = 0$ and for costs c above costs in the range c_G, $E(x_j|p_{ij} = 1) = 1$. Therefore for $c \notin c_G \cup c_B$, j will never change his investment decisions based on a message therefore $p_{ij} = 0$ for all i. □

Proposition 2.2.2

Case 1

When $c \in c_G(t_j)$, j will not invest unless he receives two good signals. Hence if messages are credible:

$$E(x_j|p_{ij} = 1, s_j = B, t_j) = 0;$$

$$E(x_j|p_{ij} = 1, s_j = G, t_j) = 1;$$

$$E(x_j|p_{ij} = 0, t_j) = 0.$$

Since $\rho_{GG}(t_i) = t_i\rho_{GG}$ and $\rho_{GB}(t_i) = t_i\rho_{GB}$, the proof of proposition 2.2.1 follows directly.

Case 2

When $c \in c_B(t_j)$, j will not invest unless he receives at least one good signal. Hence if messages are credible:

$$E(x_j|p_{ij} = 1, s_j = B, t_j) = 1;$$

$$E(x_j|p_{ij} = 1, s_j = G, t_j) = 1;$$

$$E(x_j|p_{ij} = 0, s_j = B, t_j) = 0;$$

$$E(x_j|p_{ij} = 0, s_j = G, t_j) = 1.$$

Since $\rho_{GB}(t_i) = t_i\rho_{GB}$ and $\rho_{BB}(t_i) = t_i\rho_{BB}$, the proof of proposition 2.2.1 follows directly.

Case 3

For costs c below costs in the range $c_B(t_j)$, $E(x_j|p_{ij} = 1, t_j) = 0$ and for costs c above costs in the range c_G, $E(x_j|p_{ij} = 1, t_j) = 1$. Therefore for $c \notin c_G(t_j) \cup c_B(t_j)$, j will never change his investment decisions based on a message therefore $p_{ij} = 0$ for all i.$\square$

Chapter 3

The 1/d Law of Giving

This chapter is forthcoming in the American Economic Journal: Microeconomics and represents joint work with Jacob Goeree, Leeat Yariv, Tiffany Tromp, and Tracy Mitchell.

3.1 Introduction

The recent empirical literature has identified the importance of social networks in diverse environments such as technology adoption, job search, and crime.[1] Despite the wealth of fascinating data there are several inherent problems that appear in the empirical analysis of networks. First, the strategic interactions that underlie field observations are usually hard to pinpoint. Second, the attributes of individual nodes in the network tend to be restricted or missing altogether. Third, the endogeneity of the network structure itself is difficult to account for. These problems draw a wedge between the developing theory and the extant empirical work studying the impacts of social structure on individual and collective outcomes.[2] The current chapter con-

[1] E.g., Oriana Bandiera, Iwan Barankay, and Imran Rasul (2009), Antoni Calvo-Armengol, Eleonora Patacchini, and Yves Zenou (2009), Timothy Conley and Christopher Udry (2009), Edward Glaeser, Bruce Sacerdote, and Jose Scheinkman (1996), Mark Granovetter (1994), and Giorgio Topa (2001).

[2] Recent theoretical works investigating strategic interactions on networks include Glaeser and Scheinkman (2002), Andrea Galeotti, Sanjeev Goyal, Matthew Jackson, Fernando Vega-Redondo and Leeat Yariv (2010), and Arun Sundararajan (2007).

tributes a first step toward connecting social network structure and strategic behavior by combining standard survey techniques with controlled experimentation.

We collected data from a unique population of 10 to 18 year old students in an all-girls school in Pasadena, California. Our data set was assembled in two stages. In the initial stage, we elicited the entire network of friendships, as well as a wide range of personal characteristics of each of the girls, including height, race, confidence, shyness, etc. In the second stage, we conducted an array of experimental dictator games with fifth and sixth graders, varying the social distance between dictator and recipient as determined by the length of the shortest path connecting them in their (elicited) network of friends.

We chose this subject pool since we wanted to conduct experiments in a self-contained network where peer effects are important. Indeed, the impact of social networks on behavior can likely be discerned only when the information about the network is accurate and complete, which requires high levels of participation by the entire *relevant* population. Our design allows us to play the dictator experiments with almost all students in the fifth and sixth grades. Because 95% of them completed the social network survey we are able to account for the entire network structure when analyzing giving behavior.[3]

Several insights come out of the analysis of our data. We find that dictator offers are poorly explained by individual characteristics alone. The few characteristics that are significant indicate that shy subjects give and receive less while popular subjects (as measured by the number of subjects naming them as friends) give less but receive more. We find no significant differences between kids in the fifth and sixth grades.[4]

[3]By playing the game in the field we are able to sidestep many of the selection problems identified by Steven Levitt and John List (2006) and we control for some of the experimental "context" by explicitly measuring the nature of the existing network that the games are played within.

[4]In important studies with children ranging in age between 7 and 18, William Harbaugh and Kate Krause (2000) show that students who have been at the same school longer give more to classmates and Harbaugh, Krause and Steven Liday (2003) find that older children make larger offers in dictator and ultimatum games. These authors suggest that children internalize social norms during childhood.

The model's explanatory power dramatically improves once social distance is included: the regressions reveal a simple *inverse distance law of giving*.

The second set of results pertains to the endogenous structure of the network itself. Using a logit discrete choice model applied to the data from all grades, we assess what affects each individual link's creation, given the network in place (a notion reminiscent of stability). We find that by and large links are significantly more likely between students with similar attributes. This is consistent with the wide sociology literature identifying homophily, the tendency of people to connect with those similar to them (for a survey and references, see Miller McPherson, Lynn Smith-Lovin, and James Cook, 2001). Hence, while personal characteristics do not directly affect strategic outcomes, they may have an important indirect effect by determining the friends one ultimately interacts with. We also uncover evidence for a form of preferential attachment manifesting itself as a strong preference for cliques: students like to link to those that are already "close."

To summarize, our study serves as one of the first to identify the importance of the underlying social network for dictator generosity. More generally, the combination of survey techniques (to elicit student demographics and friendship networks) and controlled experimentation allows for a careful measurement of network or peer effects in strategic situations. As such, our approach should prove useful in evaluating some of the recent theoretical work that investigates the interplay of social structure and strategic play.

Furthermore, the analysis suggests a mechanism by which individual attributes affect outcomes. Namely, attributes can help determine one's neighborhood of friends (i.e., the number of direct friends, friends of friends, etc.), which can in turn affect outcomes.

Since their study does not include information about the children's friends, they cannot determine to what extent student behavior stems from generalized norms or from social preferences.

Finally, this chapter relates to a strand of the anthropology literature investigating giving behavior. Our findings suggest a possible alternative to the "culture" effects observed in dictator games played with members of small scale societies around the world (Joseph Henrich et al. 2001). It seems reasonable to assume that these small scale societies differ in terms of the underlying network structure, in particular in terms of average distances. Our results predict that in a more tightly-knit society, more generous dictator offers can be expected.

3.1.1 Related Literature

There are several recent papers that connect experimental games with social networks. Specifically, Steve Leider, Markus Mobius, Tanya Rosenblat, and Quoc-Anh Do (2009) pioneered the methodology of network elicitation followed by a controlled altruism experiment. They obtained a social network of college students and illustrated that dictators give more to "friends," i.e., recipients with social distance equal to 1.[5] Pablo Brañas-Garza, Miguel Durán, and Maria Espinosa (2005) replicate this finding under weaker conditions: they compare giving behavior when dictators are matched with *one of their friends* (not knowing which friend) or with a stranger.[6] This alternative setup allows them to rule out the possibility that generous giving occurs because the dictator knows the recipient's identity and personal characteristics.[7]

Our study directly adds to this literature by looking at a very different subject

[5]Leider, Mobius, Rosenblat, and Do (2009) study several versions of the dictator game in which they vary the amount received by the recipient when the dictator gives up part of the pie (see James Andreoni and John Miller, 2002). Their main interest is to define and measure "social capital," defined as the extent to which subjects are able to internalize the externalities that arise from dictator giving.

[6]The experiments were conducted during class and subjects were paid in terms of credit toward their grades.

[7]Brañas-Garza, Ramon Cobo-Reyes, Natalia Jiménez, and Giovanni Ponti (2006) report results from a related setup in which dictators have some (known) chance of being matched with one of their friends (not knowing which friend) or a stranger. In this case, dictator giving does not significantly increase with the chance of being matched with a friend. The aforementioned studies are further evaluated by Brañas-Garza and Espinosa (2005).

pool that, in particular, allows us to analyze network effects across ages. Importantly, our methods and results differ in several crucial ways. First, our evidence comes from a self-contained social network. Compared to the previous studies our results show a dramatic effect of social distance on dictator giving, possibly because the social networks of 10 - 12 year olds are concentrated at their school (unlike, for example, college students who may have some friends at home, at the place they work, etc.). Second, besides eliciting data on friendships, we also gathered information about individual students' characteristics. This allows us to correct for the effects of the recipient's characteristics on the dictator's offer (if any such effects exist). Third, there are several elements of our design that mitigate or eliminate the possibility of strategic reciprocity.[8]

Another related strand of literature pertains to the formation of social networks. Denise Kandel (1978) followed adolescents over time and documented the interplay between social connections and four behavioral attributes: frequency of current marijuana use, level of educational aspirations, political orientation, and participation in minor delinquency. David Marmaros and Bruce Sacerdote (2006) illustrated that geographical and racial proximity are key determinants of friendships in a population of students and recent graduates of Dartmouth. Similarly, Aldabert Mayer and Steven Puller (2008) use Facebook data on Texas A & M college students and document that proximity of major, dorm, and race are significant proxies for friendship formation. These studies are consistent with the similarity-based connections that we observe when eliciting a wide range of demographic and psychological characteristics across different age groups. Interestingly, we also find evidence for a form of preferential

[8]Dictators were randomly matched with three of their friends, three friends of friends, and four strangers. Dictators made ten allocation decisions in our design, one of which was randomly selected by us to determine actual payments. Moreover, all subjects played the roles of dictators as well as recipients simultaneously, which should balance out any claims for *ex-post* favors. Finally, subjects' payments from being a dictator or a recipient were collected in envelopes, which they were supposed to take home before opening them several days after the experiment.

attachment that complements the tendency to connect to those who are similar, and is in line with recent theoretical models of network formation (e.g., Albert-Laszlo Barabasi and Reka Albert 1999, and Jackson and Brian Rogers 2007).

Our study also contributes to an ongoing debate regarding giving behavior in the dictator "game," where the typical outcome is that dictators give up non-negligible amounts.[9] "Dictator generosity" has inspired theories of other-regarding preferences that incorporate notions of fairness into the standard utility-maximizing paradigm.[10] Complementing these preference-based explanations, further experimentation has investigated the effects of "social context" on dictator giving. Elizabeth Hoffman, Kevin McCabe, and Vernon Smith (1996) vary the instructions and administration of the dictator game so that each variation makes the game a closer approximation of standard social interactions. They find that lowering the degree of the dictator's anonymity results in more generous offers, and conjecture that a less-anonymous experimental design evokes levels of strategic reciprocity common to everyday repeated social interactions. Iris Bohnet and Bruno Frey (1999), however, provide evidence that dictator generosity is driven not by reciprocity but by the ability to identify with the recipients, whether by knowing something about them or seeing their faces. Likewise, Charness and Uri Gneezy (2008) show that recipients (located in a different city) identified by their family names receive significantly larger amounts.[11]

These experiments are suggestive of the importance of "social distance" in explaining dictator giving, where social distance is taken to be synonymous with anonymity.[12] However, Martin Dufwenberg and Astrid Muren (2006) demonstrate that reducing

[9] See Gary Bolton, Elena Katok, and Rami Zwick (1998) for an overview of early dictator game experiments, including Daniel Kahneman, Jack Knetsch and Richard Thaler (1986) and Robert Forsythe, Joel Horowitz, Savin, and Martin Sefton (1994).

[10] See, e.g., Ernst Fehr and Klaus Schmidt (1999), Bolton and Axel Ockenfels (2000), and Gary Charness and Matthew Rabin (2002).

[11] Deborah Small and Uri Simonsohn (2009) find that knowing a "victim" increases dictator giving to another victim of the same misfortune.

[12] In the words of Hoffman, McCabe, and Smith (1996), social distance is "...a sense of coupling between the dictator and her counterpart, or others who know the dictator's decision."

anonymity, by paying dictators in public, *lowers* the amount given even when recipients are visible or known to the dictator. Dufwenberg and Muren conclude that "...it is problematic to organize experimental data in terms of social distance if this notion is taken to vary one to one with anonymity." In this chapter, we follow their suggestion and instead formally define social distance as the geodesic distance in the elicited network of friends.

3.1.2 Structure of the Paper

The chapter is organized as follows. Section 3.2 describes the survey used to elicit the network of friendships and students' demographics and the protocol for the dictator experiments. In section 3.3, we report regression results to explain observed dictator offers. Section 3.4 analyzes the determinants of network formation, i.e., what causes a link between two subjects to be formed. Section 3.5 reports simulation results showing the effects of network position and individual characteristics on students' welfare. Section 3.6 concludes. Summary statistics of our data can be found in appendix A. Appendix B contains the instructions, dictator decision sheet, and survey.

3.2 Design and Protocols

We collected data from students at an all-girls school in Pasadena, California. Our design was comprised of two components. We first conducted a survey among all students (grades 5 through 12) eliciting their network of friends and their personal characteristics. Four months later, we conducted an array of dictator experiments with girls from grades 5 and 6.

3.2.1 Student Characteristics and Friendship Survey

The survey was conducted in January 2006. We approached teachers with the request to give up 10 to 15 minutes of class time at the start of a class. Students were instructed not to talk while filling out the survey nor glance at others' responses. One of us was present to monitor and to answer any questions about the survey. The response rate in the entire population was 77%. In grades five and six, two out of forty students were absent, so for this group the response rate was 95%.

The survey (see appendix B) consists of two parts: questions 1 - 11 concerning individual characteristics and questions 12 - 14 concerning friendships. The former include height, age, number of siblings, personality traits, and a few questions regarding physical appearance (e.g., hair and eye color and whether the student wears braces). In the final three questions, students were asked to name up to five friends and indicate how much time was spent with each. In addition, they were asked how much time they spent with *other* friends (possibly from a different school) not addressed in questions 12 - 14 and how much time they spent doing extracurricular activities (e.g., playing an instrument, sports, etc.).

The summary statistics for the entire population are contained in appendix A. Here we list some relevant statistics for fifth and sixth graders that play a role in our analysis of giving behavior below. The grades 5-6 subject pool is predominantly Caucasian (51%), followed by Asian (27%), and Mixed (16%). The remaining 6% are split between African-American, Middle-Eastern, and Hispanic. The average height is 4' 11" (ranging from 4' 1" to 5' 9"). Ages range from 10 to 12 years old (with an average of 10.8); the number of siblings ranges from 1 to 4 (average 1.1); 30% wear glasses and 40% wear braces. The questions concerning personality characteristics show answers ranging from 1 (corresponding to the left-most bubble in question 11) to 5 (corresponding to the right-most bubble in question 11) with means of 2.9

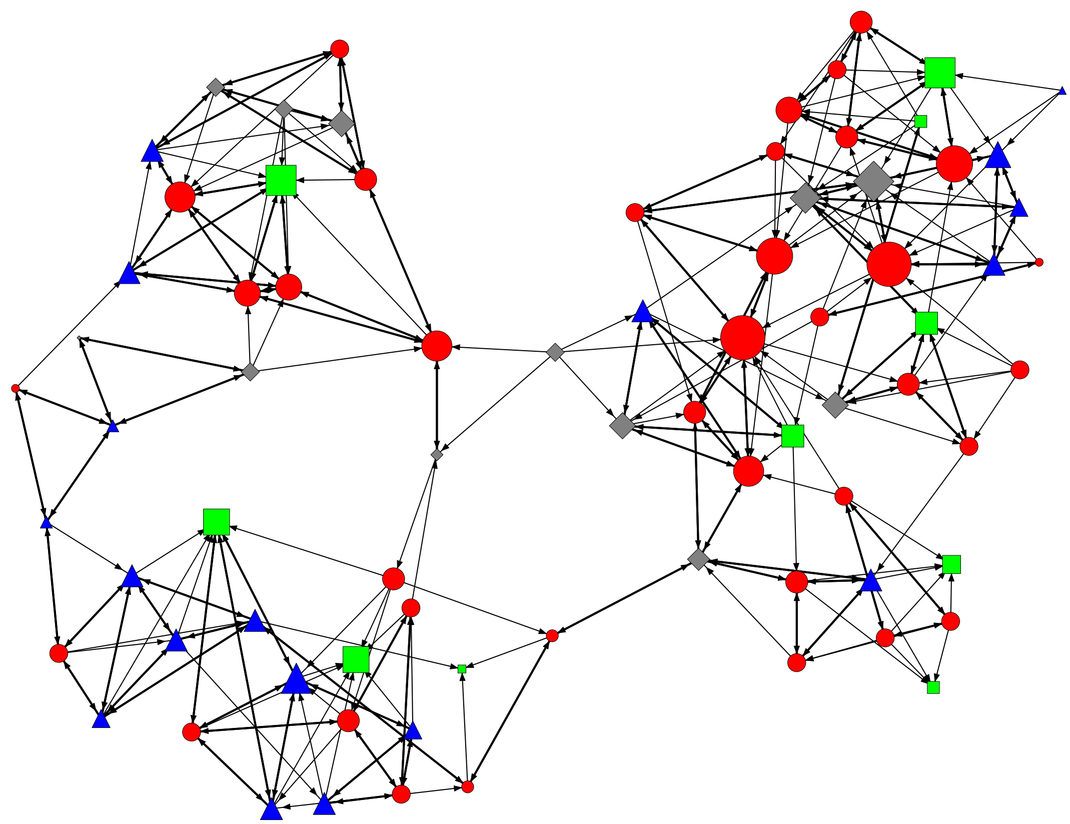


Figure 3.1. Network of fifth and sixth graders
Key: Nodes are sized according to degree and the different symbols indicate race: Caucasian (red circle), Asian (blue triangle), Mixed (gray diamond), and Other (green square). Thin (light grey) lines indicate one-way links, thick (dark grey) lines indicate two-way links.

(optimistic), 2.5 (extroverted), 2.4 (confident), and 2.3 (outgoing), respectively.[13]

Subjects reported anywhere from 2 to 5 friends (where 5 is the maximum by design)[14] with an average of 4.4. The resulting friendship network is displayed in figure **??**, where the nodes are sized by degree and the different symbols represent race. The left cluster corresponds to grade 6, whereas the right cluster corresponds to grade 5. A thin (light grey) directed arrow is drawn from subject i to j when i names j as a friend but not vice versa, and a thick (dark grey) line is drawn when both i and j name each other as a friend. The data from the time estimates (questions 13 and 14) are used in the simulation approach of section 3.5 to provide a measure of how much time is spent with friends vis-à-vis friends of friends.

3.2.2 Dictator Game Experiment

The experiments were conducted on April 20th, 2006. We ran the dictator games with fifth and sixth graders during four separate classes, each of size 20. In each of these classes the teacher would start by taking 10 subjects outside after which we would read the instructions to the remaining ten subjects in the class (the instructions for the dictator game are simple and standard, see appendix B). Subjects were allowed to ask questions during the instructions phase and afterward. We then handed out envelopes (labeled by name) that contained 10 decision sheets (see appendix B), each sheet indicating the name of the dictator and that of the recipient.[15] Each dictator had to take a numbered decision sheet out of her envelope, record her allocation

[13] One possible concern about these psychological measures is that answers would cluster around a focal answer. It is not the case that the majority of individuals chose the median answer. The percentages choosing the median answer of 3 were 38% (optimistic), 33% (extroverted), 35% (confident), and 23% (outgoing).

[14] While the majority of subjects do report five friends, this constraint seems to have had little effect on the substantive conclusions. All of the analysis reported in this chapter were repeated on a sample restricted to those who reported less than five friends, yielding similar estimates and conclusions. The results are available from the authors upon request.

[15] In other words, decision making is not anonymous: the dictator knows the identity of the recipient but not vice versa. Of course, the recipient's name had to be disclosed in order to capture network effects.

decision (i.e., how to split $6 between her and the recipient) and then take out the next decision sheet. After all ten allocation decisions had been made the decision sheets were put back in the envelopes.[16] One of us would then roll a ten sided die and mark the decision sheet to be used for actual payment. Once all ten dictators were finished they were asked to join their teacher outside, at which point the other ten subjects played the game. The allocated class time (one hour) proved more than sufficient to run the games with two groups of ten dictators.

After the dictator game experiments were completed in all four classes, we took the envelopes with us to determine actual payments. We returned the envelopes the next day, which now contained only the subjects' payments from the experiment (total payment from all selected decisions in which the subject appeared in the role of dictator or recipient) plus an additional $2 that served as a "show up fee."[17] All subjects played the role of both dictator and recipient so it would be hard for a dictator to extract favors from anyone that appeared on her list of recipients, since the recipient could credibly claim to have already returned that favor. The subjects received their envelopes a few days later and were instructed to bring these home before opening them.

Of special interest is the matching protocol we used. To be able to discern the effects of social distance on giving, we matched each dictator with three friends (distance 1), three friends-of-friends (distance 2), and four others (distance 3 or higher). We borrowed this design element from the innovative study of Leider, Mobius, Rosenblat and Do (2009) who conducted dictator games and network surveys among college undergraduates.

[16]This within-subject design enables us to more accurately measure the effects of social distance on giving behavior and to generate enough data from the sample of fifth and sixth graders.

[17]This amount was suggested to ensure no subject had to feel her friends were not generous toward her.

3.3 Experimental Results

In this section, we describe the results derived from the experimental segment of our design. Here, we take the elicited network of friendships as given. In section 3.4 below we analyze the determinants of the network itself.

3.3.1 Explaining Giving Behavior by Individual Characteristics

Average offers in our experiment were 34% (approximately $2 of the $6), which is larger than standard results but comparable to average offers of 27% reported by Charness and Gneezy (2003), where subjects make offers to a recipient identified by family name.[18] Furthermore, offers to strangers (defined as those of distance 3 or greater) in our experiment were 18% on average, which is comparable to average amounts reported in other dictator experiments. In our experiment, the game theoretic prediction of making no offers is seen in 36% of the offers to strangers, which is consistent with numbers reported by Hoffman, McCabe and Smith (1996) for their single-blind treatment.

We first analyze offer amounts using only individual characteristics collected from our survey to explain the share of the pie ($6) given to the recipient. *Height* controls for each individual's deviation from the mean height, *shy* controls for individuals' deviation from mean shyness, *asian* is a dummy equal to 1 if the participant's race is Asian, and *popular* proxies for a student's popularity by counting the number of people who call that student a friend. We also control for the recipient's characteristics: *shy_recipient* represents the recipient's deviation from the mean shyness, *popular_recipient* controls for the deviation of the recipient from the mean in-degree

[18] This difference has an intuitive explanation: Charness and Gneezy's subjects were playing with individuals in a different town, while our subjects know the recipients socially.

measure, *samerace* is 1 if the dictator and recipient are of the same race and *sameheight* is 1 if both dictator and recipient are above or below the mean height. *Sameconf* is 1 if both dictator and recipient are above or below the average on ranking how confident they are. The results are summarized in table ??.[19] Since each individual makes 10 separate decisions, we report heteroskedasticity-robust standard errors that take into account the cluster structure of the data.[20]

Of the demographic and network variables, only the survey measure assessing the "shyness" of a subject is significant at the 5% level. Popular students (as measured by their indegrees) receive slightly larger offers suggesting a "popularity premium."[21] Note that the individual and pair characteristics all represent relatively small effects. Moreover, the explanatory power of the model is poor ($R^2 = .05$).[22]

3.3.2 Explaining Giving Behavior by Individual and Network Characteristics

To glean some insight into the importance of social distance on giving behavior we computed the mean amount given for distances ranging from 1 to 10 (we pooled the data for distances greater than 10 for which we have relatively fewer data points).[23] The results are displayed in figure ??, where each of the 11 bars reflects the mean share

[19]Table ?? reports results from an OLS regression. We find qualitatively similar results from an ordered probit regression: asian 0.248 (0.164), shy -0.157 (0.075), shy_recipient -0.034 (0.042), popular -0.051 (0.039), popular_recipient 0.048 (0.020), samerace -0.058 (0.104), sameheight 0.002 (0.091), sameconf 0.100 (0.092).

[20]Similar estimates (not reported) were obtained using a random effects model. We also estimated a fixed-effects model but could not reject the null hypothesis that the fixed-effects model and the random-effects model were similar (using a Hausman test at the 5% significance level).

[21]If the dictator game proxies for a typical social interaction where giving is common, one possible explanation is that shy students give less because they participate less in standard social interactions.

[22]We also considered the importance of individual demographic variables in giving, restricted to offers made to friends only and offers made to strangers only. Even after this conditioning on social distance, dictator and recipient demographics cannot explain offer amounts.

[23]Recall that we measure social distance as the minimum number of "steps" between any two individuals in figure ??. A distance of 1 indicates (direct) friends, a distance of 2 indicates indirect friends (friends of friends who are not direct friends), etc. When individuals are not connected by the network their distance is coded 1001.

Table 3.1. Explaining dictator offers by personal traits only (model 1) and by including network variables (model 2).

Variable Share of 6$	**Coefficient** (Standard Error)	
	Model 1	**Model 2**
delta (δ)		0.356***
		(0.040)
gamma (γ)		-0.852***
		(0.153)
order		-0.047***
		(0.009)
height	-0.002	-0.003
	(0.004)	(0.004)
asian	0.057	0.039
	(0.039)	(0.040)
shy	-0.037**	-0.036**
	(0.018)	(0.018)
shy_recipient	-0.010	-0.001
	(0.01)	(0.009)
popular	-0.011	-0.017
	(0.009)	(0.012)
popular_recipient	0.010**	-0.003
	(0.005)	(0.004)
samerace	-0.014	-0.022
	(0.024)	(0.022)
sameheight	0.005	-0.006
	(0.021)	(0.018)
sameconf	0.028	0.006
	(0.022)	(0.019)
closeness		-0.011
		(0.009)
betweenness		0.003
		(0.002)
power		-0.002
		(0.007)
Constant	0.324***	0.156***
	(0.020)	(0.037)
Observations	629	629
R-squared	0.05	0.29

Robust clustered standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

of the pie that was offered and the error bars indicate twice the standard deviation (the theoretical black line is discussed below). Clearly, offers significantly decline with distance.

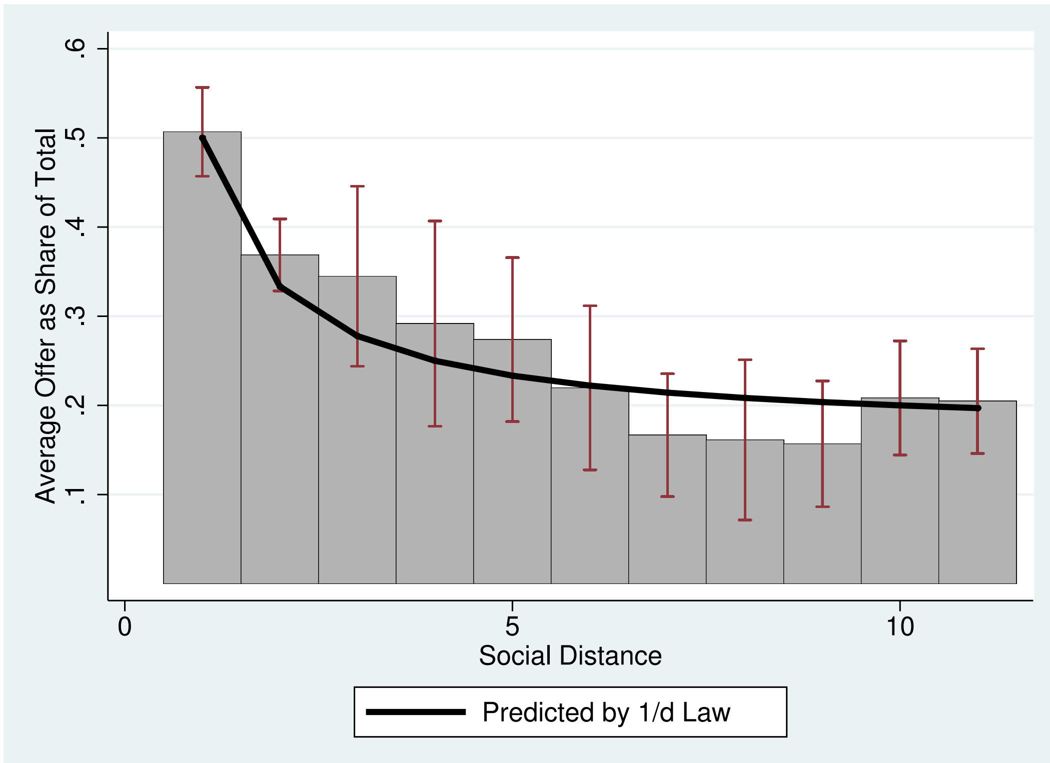


Figure 3.2. Observed and predicted offers by social distance

The effect of distance is captured in the regressions by including a term δd^{γ}, where $\gamma < 0$ reflects the decaying effect of social connection as distance increases. An additional measure of social distance is captured by the variable *order*, which refers to the order (1 - 5) a first-degree friend was listed (see question 12 of the survey in appendix B). In the estimations, the variable *order* is coded as the deviation from the mean. We also include three standard measures of network structure. *Betweenness* measures the share of times an individual is between any two other individuals on a path over all paths in the network, *closeness* is the sum of the inverse distance from i to all other individuals in the network and *power* is a measure of the centrality of other individuals when i is removed from the network (see Phil Bonacich, 1987). For each of these three network variables we take the deviation from the mean as the explanatory variable. We take into account the panel structure, using a clustered design, and report heteroskedasticity-robust standard errors. The results are summarized in the right-most column of table **??**.

Shy is still significant, but remains small in size. The three social network structure measures are not statistically significant and represent very weak effects. One possible explanation is that for these measures to play a role, subjects would effectively need to understand the entire network structure, which is unlikely the case in practice. The social distance measures, in contrast, represent very large effects. The model estimates that strangers, i.e., those of infinite distance, will receive 16% of the pie while a second-degree friend receives an additional 20% and another 16% is added for the median first-degree friend. Moving one deviation below the mean in the ordering of friends is equivalent to losing 5% of the pie. None of the partner characteristics are significant when we include controls for social distance. In particular, the effects of the partner's popularity vanish. Finally, note the dramatic improvement in fit once social distance is included ($R^2 = .29$).

The variation in offer amounts across different recipients is not driven by only a few individuals. Most individuals showed substantial variation in offer amounts across the recipients they faced. Only 5% of the individuals who participated in the experiment offered the same amount to all recipients. We also consider the distribution of offers conditional on social distance. Figure **??** shows that the distribution of offers for recipients of distance 1 (friends), distance 2 (friends-of-friends), and those of distance greater than 3 (strangers). Note that the offer distributions are ranked in the sense of first-degree stochastic dominance, which illustrates that dictators make a clear distinction between friends, friends-of-friends, and strangers.

Compared to other studies our results show a dramatic effect of social distance

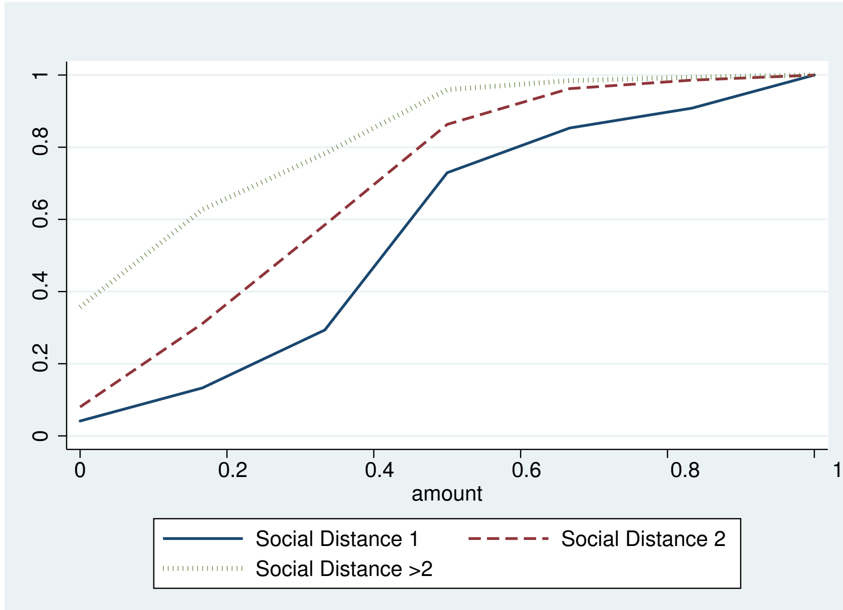


Figure 3.3. Empirical distributions by social distance.

on dictator giving, possibly because the social networks of 10 - 12 year olds are concentrated at their school. Another reason could be that ingroup/outgroup effects are much more pronounced among adolescents - relative differences between giving to friends and others may be exaggerated at age 12. To summarize, introducing controls for social distance significantly improves the fit of the model and introduces large and significant effects, particularly in comparison with the individual and pair effects reported above. The equation governing dictator giving in terms of social distance may be neatly summarized as[24]

$$\text{share given} = \frac{1}{6}\Big(1 + \frac{2}{d}\Big).$$

The predictions of this *inverse distance law of giving* are superimposed in figure **??**.

[24]Note that the estimated parameters governing the social distance part are not significantly different from $\frac{1}{6}$, $\frac{1}{3}$, and 1 respectively.

3.4 Determinants of Network Formation

The previous section suggests the potential significance of the underlying social network for individual outcomes. In fact, personal characteristics had far weaker predictive power than network attributes in determining strategic behavior. Nonetheless, personal characteristics may have an important (albeit indirect) role in determining outcomes by affecting the social network at place, and thus the type, in terms of social distance, of interactions an agent with certain characteristics experiences.

Our data allow us to measure the effects of individual characteristics on linking choices. Furthermore, we observe links made by students from *all* grades (373 students in total), not just grades 5 and 6. We analyze subjects' linking choices with a logit discrete choice model: for every link made by a subject we evaluate its "value" and compare it to the values of other links the subject could have made (holding fixed all other links in the network). This has the flavor of stability in the sense that if a subject had a more valuable link available than one of her existing links, we would expect her to shift social resources to that link and the network would not be stable.

Consider first a model where link values are determined by individual characteristics alone. In particular, suppose a link from subject i to j has a value v_{ij} that is a function of i's and j's characteristics $v_{ij} = f(X_i, X_j)$. Then, using a logit model, the probability that this link occurs is given by

$$P_{ij} = \frac{e^{v_{ij}}}{\sum_k e^{v_{ik}}}. \tag{3.1}$$

Estimating this model using a clustered structure to take into account that each individual makes five independent decisions to link to a friend, we find that individual and partner characteristics a role primarily when the characteristics of i and j match up, see table **??**.[25] This is consistent with the extensive sociology literature on homophily,

[25] We restrict logit choices to students within the same grade, which captures almost all data. The

a phenomenon referring to people's apparent tendency to connect with others similar to them (for an overview, see McPherson, Smith-Lovin, and Cook, J. M., 2001).[26] The one individual characteristic that appears to affect the probability of linking is the height of the network partner. Individuals are more likely to link to make friends with girls who are taller.

There are network effects that seem intuitively appealing and do not appear in the estimation of equation (3.1). Following the network formation literature on *preferential attachment* (see, e.g., Barabasi and Albert, 1999, and Jackson and Rogers, 2007, and references therein), it is natural to entertain subjects' preference to form cliques, i.e., subjects' preference to link to friends of friends. We model this by posing that the probability that i links to j depends on the distance between i and j without the link. Table **??** incorporates these network or distance effects into our logit model.[27] For example, the dummy variable $d2$ is equal to 1 if, without the direct link between i and j, there is already a path from i to j of length two. In other words, j is already a friend of a friend and a direct link from i to j closes the "triad." Finally, the dummy variables $d3$ and $d4$ are 1 if, without the direct link from i to j there is already a path from i to j of length 3 or 4 respectively.

As in the previous section, the inclusion of network variables results in a dramatically improved fit (the log likelihood increases by roughly 1400 or 25%). Students seem to have a strong preference to link to those that are already close. The resulting "cliques" are apparent from the network graph in figure **??**. These observations

dummy variable *sameboyfriend* is 1 if the girls that form the link both have a boyfriend. Other recipient characteristics we tried include *confident*, *shy*, *boyfriend*, *Asian*, *height*, *braces*, *glasses*, and *only child* but none of them are significantly different from 0, and neither are *sameshy*, *sameonlychild*, *samebraces*, *sameglasses*, *sameoptimistic*, and *sameextroverted*.

[26]There are several recent studies that explore the foundations and impacts of similarity-based connections, e.g., Sergio Currarini, Jackson, and Paolo Pin (2009) and Mariagiovanna Baccara and Yariv, 2008.

[27]There are potential endogeneity issues when incorporating these variables in the estimations. Note, however, that the non-network estimates do not change significantly when the network variables are included, which provides evidence that the resulting estimates are not biased.

Table 3.2. Explaining linking decisions by personal traits only (model 1) and by including network variables (model 2).

Variable Link	**Coefficient** (Standard Error)	
	Model 1	**Model 2**
samerace	0.618***	0.491***
	(0.084)	(0.084)
sameheight	0.263***	0.289***
	(0.064)	(0.069)
sameconf	0.158**	0.158**
	(0.066)	(0.071)
sameboyfriend	0.683**	0.562**
	(0.324)	(0.225)
shy_recipient	-0.019	0.030
	(0.032)	(0.035)
height_recipient	0.023**	0.010
	(0.011)	(0.013)
d 2		3.657***
		(0.130)
d 3		0.940***
		(0.169)
d 4		0.473***
		(0.169)
Log Likelihood	-5451.151	-4027.450
Links	1753	1753

Robust clustered standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

are important when considering the distance of agents one chooses to interact with. Indeed, girls in one's clique are of short social distance. Our experimental results suggest that dictator-like interactions with them are expected to yield high returns relative to interactions with girls outside of the clique. Once we account for network structure, the height of a partner is no longer a significant predictor of making a link. In our data, height is weakly correlated with popularity (significant at 10%). Nicola Persico, Andrew Postlewaite and Dan Silverman (2004) find that height affects early childhood confidence, which could also suggest a tendency for taller individuals to have more friends. This relationship between height and popularity may also explain the fact that once we control for the network position of existing partners, height is no longer an important predictor of making a link.[28]

The homophilic preferences underlying linking choices together with the dependence of giving behavior on social distance identified in the previous section, suggest the potential (indirect) significance of individual characteristics on outcomes. To illustrate, note from figure **??** that Asian students tend to form cliques. Moreover, table **??** shows that Asian students tend to give more (although the result is not significant, possibly because of our matching protocol). If each girl interacts with a fixed number of closest friends (or, alternatively, with all the girls that are of particular distance from her) Asian students will, on average, receive higher benefits in dictator-like settings. In other words, personal characteristics affect the type of agents one

[28] One natural question (raised by a referee) is "if similar characteristics determine network formation (model 1 in table **??**), and network positions determine giving (model 2 in table **??**), then why do we not see a big effect of individual characteristics on the amounts given (model 1 in table **??**)?" For example, one could characterize a certain clique in the network by two characteristics, i.e. a group of tall Asian girls. In this clique, girls are "close" and give a lot to each other, so the interaction term *sameheight* × *Asian* could be expected to be a significant explanatory variable for the amount given. Other cliques could be described similarly by constructing interaction terms that involve two or more individual characteristics. We tried several models that included interaction terms of this type, but found no improvement in fit. Basically, the interaction terms filter out few subjects that satisfy multiple criteria, and our data set is not large enough to produce significant effects. We do think that in a larger data set the network effects should be reproducible, although less perfectly so, by considering individual characteristics.

interacts with and, hence, social distances, which in turn affect earning outcomes.

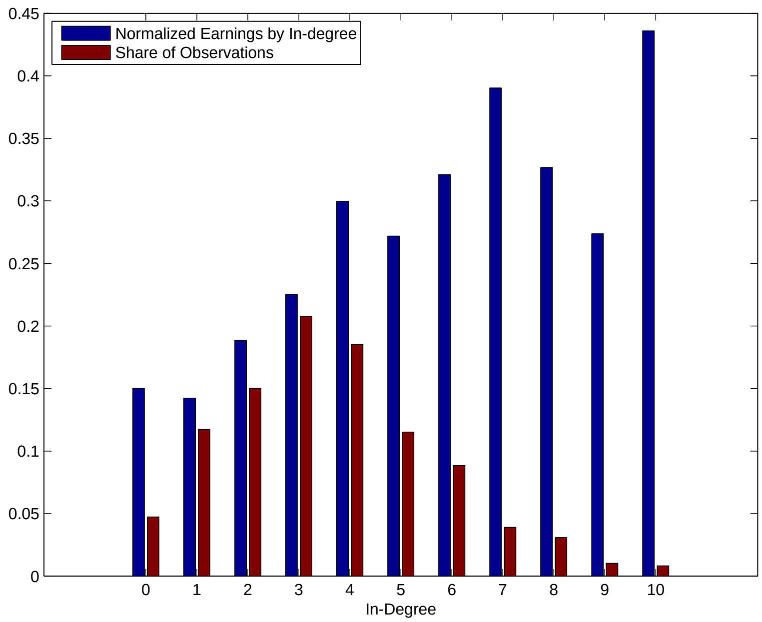


Figure 3.4. Simulated earnings by popularity.

Actual earnings in the experiment exhibit some variance, but our design of randomly choosing one of ten decisions effectively dampens out network or demographic effects. To gain more insight into the connection between network position, individual characteristics, and earnings, we simulate interaction over the entire network and generate a normalized measure of the share of the \$6 each individual is expected to receive. In particular, first we predict offer amounts from i to j for all possible interactions in the network using the results from estimating the inverse distance model in section 3.2. Using survey responses, we next generate a weight that represents the ratio of time spent on average with direct friends to the total time spent with all friends. We then generate a weighted sum that represents the value any individual is expected to receive from a pairwise interaction with any other member of the network (we assume an individual is as likely to be a dictator as a recipient). We find that these simulated average earnings have a strong positive relationship with in-degree or popularity, see figure **??**.

Using the simulated earnings we are able to measure the importance of individual and network characteristics, see table **??**. First, note that without the inclusion of network variables such as popularity, betweenness, closeness, and power, the model's predictive power is weak (left column). Including the network variables (right column) improves the fit dramatically and predicts that the few most popular girls earn close to four times as much as the least popular ones (see also figure **??**). In particular, each time a student is named by someone else as a friend, their in-degree goes up by 1 and their normalized earnings by 2%. This raises the normalized earnings from roughly 10% for someone who was never listed as a friend to 32% for someone who was listed eleven times (the maximum in our sample), a more than three fold increase.

Note that the value of being listed as a friend by one extra person is roughly the same as the value of being Asian. The underlying reason is quite different, however, and ties back to the homophilic preferences discussed above. Asian girls tend to form small cliques and they tend to give more on average - as a result the normalized earnings of Asian girls are higher. This illustrates our earlier argument for why individual traits (which do not explain the amount given) are important in explaining subjects' earnings - they affect linking choices and average distances, which are the main determinants of giving behavior.

3.5 Conclusion

We collected survey data on friendship networks and individual characteristics from the entire student body at an all-girls school in Pasadena, California. In addition, we conducted several dictator games with 5th and 6th graders, varying social distances between proposers and receivers (using the *elicited* social network structure).

Table 3.3. Explaining earnings by personal traits only (model 1) and by including network variables (model 2).

Variable Simulated Received Earnings	**Coefficient** (Standard Error)	
	Model 1	**Model 2**
height	-0.000	-0.001
	(0.002)	(0.002)
asian	0.026	0.025
	(0.021)	(0.019)
shy	-0.016*	-0.013
	(0.009)	(0.008)
confident	0.002	-0.002
	(0.007)	(0.007)
only child	0.012	0.006
	(0.022)	(0.022)
optimistic	-0.001	-0.001
	(0.007)	(0.006)
braces	0.018	0.020
	(0.016)	(0.015)
glasses	-0.029**	-0.019
	(0.014)	(0.014)
popular		0.020***
		(0.003)
between		-0.003*
		(0.001)
close		0.003
		(0.002)
Power		-0.000
		(0.000)
Constant	0.200***	0.122***
	(0.009)	(0.015)
Observations	330	330
R-squared	0.03	0.15

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

There are two main insights that shine through. First, network effects are extremely important in explaining dictator behavior, far more so than any individual characteristic. In fact, the data reveal a simple $1/d$ law of giving, where d denotes social distance between a proposer and receiver. Second, individual characteristics are important in explaining the network formation process. We identify strong homophilous behavior in that girls tend to link to others similar to them.

These two insights suggest that social networks may constitute an important channel through which personal characteristics affect outcomes. Indeed, personal characteristics may affect the agents one faces (say, the number of direct friends, friends of friends, etc.) in a variety of strategic interactions. These, in turn, play a crucial role in determining the resulting outcomes.

More generally, the study contributes to the rapidly expanding literature pointing to the importance of social networks to economic consequences. It provides one of the first to elicit both network and personal attributes and tie them to a controlled strategic interaction.

The chapter also provides a contribution to the literature on social capital (see, e.g., James Coleman, 1990, Robert Putnam, Robert Leonardi, and Raffaella Nanetti, 1992, Glaeser, David Laibson, and Sacerdote, 2002, and references therein). While our population of subjects is very particular, in view of the social capital literature, our results suggest that social network characteristics may serve as a useful proxy for social capital (coarse network characteristics, such as joint memberships in organizations, have, in fact, already been used in some empirical work). In our setup, social capital captured in that way have power in explaining outcomes, namely dictator giving. Furthermore, our results suggest that social capital formation may be impacted by non-malleable physical characteristics. In particular, having an attribute that is common in the population can ease the creation of close connections, since similar individuals are easier to find, and consequently raise one's potential for acquiring social capital.

3.6 Appendix

Instructions: Welcome to this experiment! If you have any questions you may ask _______. You are not allowed to talk with anyone except _______ during the experiment.

We will ask everyone from this class to make a series of 10 decisions. For each decision, you get a separate sheet of paper (numbered 1 - 10) that lets you divide $6 between yourself and another student from this school: the name of the other student is printed on the sheet. The other student might be someone in your class or someone from a different grade. On each sheet you should write down how much you want to keep for yourself and how much you want to give to the other student: any division is allowed as long as the numbers add up to $6. When you are done, we will pick up your sheet and give you a new sheet, which will have the name of a different student printed on it.

Once you made all 10 decisions, we will roll a ten-sided die to decide which of the 10 decision sheets to use. The amount you wrote down on this sheet to keep for yourself is put in your "money envelope" and the amount you wrote down on this sheet for the other student is put in the other student's "money envelope." No student in your class (or in a different grade) will ever know their name was on your sheet or how much money you gave to them.

After the experiment is done, each student will receive her own money envelope. The amount of money in your envelope depends on how much you decided to keep for yourself and how much others decided to give to you. The money in your envelope is yours to keep and you do not have to tell anyone how much money you got.

Decision sheet (#1 out 10): Hello dictator's name! Please choose how you want to divide $6 between you and recipient's name.

Amount for You	**Amount for Recipient's Name**
$___	$___

Please make sure that the amounts in the boxes add up to **$6**.

Table 3.4. Summary statistics for the entire population (and grades 5 and 6).

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
grade	8.9 (5.5)	2.2 (0.5)	5 (5)	12 (6)	487 (80)
age	13.9 (10.9)	2.2 (0.7)	10 (10)	18 (12)	373 (76)
height	63.2 (58.8)	3.7 (3.4)	49 (49)	73 (69)	370 (75)
siblings	1.2 (1.1)	0.9 (0.8)	0 (1)	6 (4)	373 (76)
boyfriend	0.07 (0)	0.26 (0)	0 (0)	1 (0)	487 (76)
optimistic	3.0 (2.9)	1.0 (0.9)	1 (1)	5 (5)	368 (75)
extroverted	2.6 (2.5)	1.0 (1.0)	1 (1)	5 (5)	366 (75)
confident	2.7 (2.4)	1.0 (0.9)	1 (1)	5 (5)	368 (74)
outgoing	2.5 (2.3)	1.0 (1.0)	1 (1)	5 (4)	370 (74)
hours: friend 1	25.1 (25.0)	20.0 (17.4)	0 (1)	155 (64)	368 (75)
hours: friend 2	22.2 (21.7)	18.0 (15.6)	0 (1)	147 (56)	368 (75)
hours: friend 3	21.3 (21.0)	18.7 (15.5)	0 (1)	189 (50)	367 (74)
hours: friend 4	20.1 (19.6)	18.8 (15.4)	0 (0)	148 (50)	361 (69)
hours: friend 5	19.5 (21.0)	19.0 (16.1)	0 (0)	168 (56)	345 (63)
socializing	15.6 (12.8)	16.7 (15.7)	0 (0)	100 (80)	365 (72)
white	59.2% (50.7%)	-	-	-	368 (75)
black	3.8% (2.7%)	-	-	-	368 (75)
asian	22.6% (26.7%)	-	-	-	368 (75)
mixed	9.5% (16.0%)	-	-	-	368 (75)
hispanic	2.4% (2.7%)	-	-	-	368 (75)
in-degree	3.6 (4.4)	2.1 (2.4)	0 (0)	11 (10)	487 (76)
Number of friends	4.8 (4.7)	0.6 (0.7)	1 (2)	5 (5)	370 (76)
Number of Surveys	-	-	-	-	373 (76)

Survey

Thank you for participating in this survey. Please be assured that your answers will be kept completely confidential and your identity will be protected. While your name is required for this survey, we assure you that your identity will not be disclosed to any third parties nor published.

1. What is your first and last name: ________________________________
2. How old are you? ___________
3. What grade are you in? ____________
4. How would you describe your race/ethnicity? ________________________
5. How tall are you? __________ft ___________in
6. How many siblings do you have? ____________
7. What color are your eyes? Please circle one:

   Blue Brown Green Hazel

8. What color is your hair? Please circle one:

   Brown Blonde Black Red

9. Do you currently wear braces? Please circle one: Yes No
10. Do you currently wear glasses? Please circle one: Yes No

11. Please select a bubble closest to the word which best describes your personality.

Optimistic	○	○	○	○	○	Realistic
Extroverted	○	○	○	○	○	Introverted
Confident	○	○	○	○	○	Self-conscious
Outgoing	○	○	○	○	○	Shy

12. In the spaces below, list up to five of your closest friends **that currently attend your school**. Please include their full names, ages, and grade. In addition, please include the approximate number of hours spent with this friend during the week. If you have a best friend, please place his/her name at the top of the list.

Name	Age	Grade	Hrs/Week
1.			
2.			
3.			
4.			
5.			

13. On average, how many hours per week do you spend socializing with **other** friends (not listed under 12): ______________

14. Please list some of your team sports, activities and hobbies. (For example: basketball, knitting, playing an instrument, cheerleading, drawing etc…)

Activity	Hrs/Week
1.	
2.	
3.	
4.	
5.	

Chapter 4

No Excuses for Good Behavior

This chapter represents joint work with Sera Linardi.

4.1 Introduction

Economists have long been interested in the motivation behind prosocial behavior such as volunteering or donating money. Andreoni (1989) proposes a model in which individuals derive a "warm glow" from their contribution to others' wellbeing. However, empirical evidence that people act differently when prosocial behavior is publicly observable motivated theoretical models (Seabright 2004; Benabou and Tirole 2003, 2006; Ellingsen and Johannesson 1998) positing that individuals may also give in order to improve their social image. A range of experimental evidence from Andreoni and Petrie (2004), Rege and Telle (2004), Ariely, Bracha, and Meier (2007), and Carpenter and Myers (2007) is consistent with models linking social image and prosocial behavior.[1]

Nonprofits have long provided image rewards as a strategy to encourage contribution. The effect of an audience on monetary contributions has been studied,[2] however

[1] Andreoni and Petrie (2004), Rege and Telle (2004) find that removing the anonymity of gifts in a public goods game increases contributions. Ariely, Bracha, and Meier (2009) find that in a lab experiment where keyboard clicks are translated to donations for charity, subjects click more when they have to announce their donation to the room. Carpenter and Myers (2007) discover that having a vanity license plate is positively correlated with volunteering as a firefighter.

[2] Andreoni and Petrie (2004), Rege and Telle (2004), Harbaugh (1998)

these studies cannot be readily applied to labor contributions for three reasons. First, labor consists of two dimensions: time and effort. Work on multitask agency problems (Dewatripont, Jewitt, Tirole 2000) illustrates that rewarding one dimension of a task can increase the emphasis on the rewarded dimension to the detriment of unrewarded dimensions. For example, image rewards may encourage contribution of time, which is readily visible, but harm productivity. Second, monetary contribution are often studied in a static social environment, thus missing the dynamic changes of social environment that can occur during contributions of labor. Third, the importance of image rewards may depend on the degree of personal engagement in the social task. Manipulations that are effective in encouraging monetary contributions may not be effective in encouraging volunteering given the higher degree of personal involvement inherent in labor contribution.

This chapter provides experimental evidence about two ubiquitous feature of the volunteering environment: the availability of unverifiable excuses and the presence of an organization's representative. We use Benabou and Tirole's (2006) binary participation model as a starting point in predicting how stigma and visibility influence contribution of time. We test three propositions. First, removing unverifiable excuses increases average contributions. Second, volunteers are more likely to leave in clusters when no excuses are available. Third, average contributions decrease without the presence of an authority figure. We partner with the nonprofit School on Wheels to design an experiment where the realism and context of volunteering can be integrated into a setting with a precisely controlled social environment.

We find that removing excuses induces subjects to contribute more, but this effect is weakened as soon as a single member of the group stops contributing. Second, we find evidence that individuals leave in clusters only in the absence of excuses. Third, removing the monitor does not reduce volunteering, in fact it slightly increases volunteering but the effect is not robust. On the other hand, having a larger audience of peers (other subjects) increases the willingness to contribute, indicating that volunteers may care more about the presence of peers than the presence of a monitor (experimenter). Importantly, we find that our image manipulation does not affect volunteers' productivity. Social image manipulations generate a greater quantity of contribution without affecting their average quality. Overall our findings suggest that, while social image can be manipulated to increase prosocial behavior, the effect is sensitive to the details of the environment.

Existing literature provides evidence that individuals exhibit behavior consistent with stigma avoidance. Unconditional transfers in games (dictator game in Andreoni and Bernheim 2009 and modified trust game in Tadelis 2008) are less generous when players are able to obscure their decision behind a random mechanism. While the stigma of making small transfers is static, we posit that in a group volunteering situation the stigma of being the first person to quit is particularly high. To investigate this, we discuss an alternative model where contribution are made dynamically by individual decisions to quit in the current social environment. This alternative model makes distinct predictions for behavior motivated by imitation and stigma avoidance; if individuals are motivated by avoiding stigma, cascade behavior will be less pronounced in the presence of excuses. On the other hand, behavior driven by imitation will result in cascades regardless of whether excuses are available.

Nonprofits often send representatives to potential volunteers or donors, under the assumption that the presence of an observer will make it more difficult for the donor to reject the request to give.[3] Two separate strands of literature examining the effect of the presence of an authority figure give conflicting predictions. The literature on crowding out from monitoring argues that the presence of a monitor may be interpreted as distrust and could therefore decrease prosocial contributions (Dickinson and Villeval 2008; Frank and Schultze 2002; Falk and Kosfeld 2004).

[3]DellaVigna, List and Malmendier (2009)

However, the experimenter demand effect literature posits that altruism seen in the lab is motivated by subject's desire to please an authority figure (the experimenter), and hence subjects act more generously when the experimenter is present (Levitt and List, 2006 and Zizzo 2009).[4] We address the experimenter demand literature directly by varying whether the experimenter is present.

Our work considers the possibility that the salient audience for the subjects is not the experimenter but other subjects. Volunteers often perform their tasks not only in the presence of a leader but also in the presence of other volunteers. Researchers have often considered the social environment by using double-blind procedures, which protect subjects decisions from *both* the experimenter and other subjects. As a result, there is little evidence of the differential impact of experimenters and peers.[5] Frank's (1998) finding that the decisions of experimental subjects in the lab are not sensitive to the payoffs of the experimenter suggests that subjects are not concerned about what the experimenter thinks of them. The chapter proceeds as follows. In section 4.2 we describe the theoretical background for our experimental treatments and make predictions about what we expect to see in our experiment. In section 4.3 we describe our experimental design and survey. section 4.4 presents the results. Section 4.5 concludes. Proofs for section 4.2 and experimental materials (instructions, software screen shots, and survey questions) can be found in the appendix.

4.2 Theoretical Framework

In the typical charitable giving environment, a representative from an organization solicits contribution from a group of individuals, each of whom may or may not be

[4]See Fleming, Townsend, Lowe and Ferguson (2007) and Paulhus (1991) for surveys of demand effects in psychology.

[5]The introduction of a double blind protocol does not change the outcome of the voluntary contribution game (Holt and Laury 1997) but it does for the dictator game (Hoffman, McCabe, and Smith 1996).

limited in their ability to contribute by unverifiable external circumstances. Using Benabou and Tirole's (2006) binary participation model (henceforth BT), we illustrate the basic intuition of the image signaling mechanisms in this environment and derive testable predictions. In the Appendix we show that the results of this binary model extend to a decision of how much to contribute as subjects make when deciding how much to volunteer.

BT relies on the assumption that individuals know not only of their own altruism but also the distribution of altruism in the population. This information allows them to solve for an equilibrium contribution level. In section 4.2.2 we provide an alternative model where individuals do not know the distribution of altruism in the population. The decision about how much to contribute is made in time as subjects dynamically decide whether to continue working or not.

4.2.1 Equilibrium model of volunteering

Let v be an agents intrinsic motivation to volunteer. We model v as a random variable with distribution function $g(v)$ and an associated density function $G(v)$.[6] C be the cost of volunteering, and x be the visibility of volunteering, which represent agent's awareness of being observed.

Let the decision to volunteer be represented by $a = \{0, 1\}$. An individual with type v who faces a choice to volunteer with visibility x has the following utility for volunteering:[7]

$$u(a = 1) = v - C + x(E(v|a = 1) - E(v|a = 0)) \tag{4.1}$$

Individuals participate if $v \geq C - x(E(v|a = 1) - E(v|a = 0)) \equiv v^*$ where the

[6] $g'(v)$ decreasing implies that there are fewer highly altruistic types in the population than less altruistic types.

[7] Note that $u(a = 0) = 0$.

equilibrium threshold of altruism v^* is implicitly defined by the equation

$$v^* - C + x(E(v|v \geq v^*) - E(v|v < v^*)) = 0. \tag{4.2}$$

Let $\Delta(v^*|x) = x(M^+(v^*) - M^-(v^*))$ be an agent's **image reward** from participating, where $M^+(v^*) \equiv E(v|v \geq v^*)$ is the *honor* for participating and $M^-(v^*) \equiv E(v|v < v^*)$ is the *stigma* of not participating.

Suppose with some probability $\delta \in [0, 1]$ individuals are prevented from volunteering by (unverifiable) external circumstances. It is straightforward to infer the type of agents who participate, but more difficult to determine the type of agents who do not. This is because there are two reasons that an agent might not participate: with probability δ he has been prevented by circumstances, and with probability $1-\delta$ he is not altruistic enough to participate. The honor of participating remains unchanged, but the stigma of not participating is now lessened:

$$M^-(v^*|\delta) \equiv \frac{\delta E(v) + (1-\delta)G(v^*)E(v|v < v^*)}{\delta + (1-\delta)G(v^*)} \tag{4.3}$$

When $g(v)$ is decreasing or constant in v, participation can be described by a unique equilibrium threshold v. With this assumption on the population parameter,[8] the binary model identifies two elements in the typical volunteering environment that affect contributed time. First, the presence of the representative may increase volunteering by increasing subjects' awareness of being observed (x).[9] Second, the availability of excuses (δ) may have reduced volunteering by reducing the stigma of not volunteering. We extend this binary participation model to discrete levels of contributions in the Appendix and formally derive the two predictions below.

[8]Without this assumption (e.g when $g(v)$ is decreasing or unimodal in v), then multiple equilibria exists for a large range of C and $g(v)$, making it difficult to derive theoretical predictions.

[9]In section 4.3.2 we ask if instead of the monitor, subjects signal their altruism towards their peers.

- **Excuses Prediction:** Removing excuses increases average time volunteered.
- **Monitor Prediction:** Reduced monitoring decreases average time volunteered.

4.2.2 Dynamic model of volunteering

Benabou and Tirole's signaling model requires an individual to know the distribution of altruism in the population. However, individuals may not know how altruistic they are relative to other people but still be sensitive to what they think others perceive them. Let $\Delta_C(t) = C(t) - C(t-1)$ be the increase in cost from working an additional minute at time t and x be the visibility of the volunteering activity. Individual i's utility for volunteering an extra minute is

$$U_i(t) = v_i - \Delta_C(t) + S(t|x, \delta).$$

where $S(t|x)$ is the image utility from the current social environment.

We posit that in a group volunteering setup there is a particular stigma attached to being the least altruistic person in the group. Normalizing the honor of working an additional minute to 1, an individual suffers disutility $B > 1$ if he is the first person to stop working. The equation for image utility is therefore:

$$S(t|x, \delta) = x(1 - (-(1-\delta)B)) \text{ if no one has left, } x \text{ otherwise}$$

A reduced awareness of being observed (x) in this dynamic model will reduce volunteering as it does in the equilibrium model. Removing excuses (δ) will increase volunteering, but will also introduce cascades of people leaving.

The existence of unverifiable external circumstances (δ) lowers volunteering in two ways. First, it lessens the bad apple stigma to $(1-\delta)B$. Second, it introduces a probability that someone will be constrained by circumstances to be the first person to leave, instead of leaving because of the bad apple stigma. When excuses are removed ($\delta = 0$), the bad apple stigma is fully present (B) before the least altruistic person decides he will rather suffer B than work any more. Since there are more people staying just to avoid the bad apple stigma when excuses are removed, cascades of people leaving are more likely there than in a social environment where excuses are available. This distinguishes a model of stigma avoidance from one of conformity, where individuals have a simple preference for doing what others do[10] and will therefore always leave in cascades, regardless of the availability of excuses. We will test this prediction from the dynamic model in our experiment.

- **Cascade Prediction:** Cascades are more likely to happen when there are no excuses.

4.3 Experiment

In order to test the predictions of theories of social environment, we need to precisely isolate opportunities for social signaling. In the field, potential image benefits are more difficult to isolate due to challenges in controlling or observing the visibility of volunteering. In empirical studies, the preference to be seen as a good person may be confounded with strategic image building such as bolstering a college application, resume or career contacts. The lab offers no strategic image benefits and allows us to isolate the social signaling audience to only observers present in the lab. Realism and context are integrated into the lab by partnering with the nonprofit School On Wheels (SOW) for orientation, training, and the choice of task.

[10] Goeree and Yariv (2006), Andreoni and Bernheim (2009)

4.3.1 Experimental Design

Subjects received an email publicizing an opportunity to participate in an experiment on decision making that did not mention volunteering. The experiment consisted of two stages: training and volunteering.

The training session lasted 15-20 minutes. The experimenter started by introducing SOW and distributing SOW promotional materials.[11] After all subjects indicated they have adequate time to read the materials, the experimenter explained the volunteering task. SOW has requested help in building a database of educational resources. This task consists of doing internet searches and entering the information into a database; up to seven entries (subject, website address, grade level, etc.) could be made per resource. Each subject received a task sheet listing the areas in the database assigned to them. Subjects were aware that they were all working on different portions of the database and that their work would not be redundant.[12] Subjects then practiced the task by performing one directed internet search and one data entry task. After everyone had completed the training session, we announced they had earned their show-up fee ($20) and were free to go; if they chose to, they could stay in the lab and volunteer for SOW by performing the task they had just practiced for as long as they like (up to 90 minutes). We clearly stated that no additional monetary incentives would be forthcoming.

[11] Promotional materials included a People magazine article on SOW and a thank you letter from SOW's lead volunteer coordinator to the lab volunteers.

[12] The list contained several choices of grade levels and school subjects that has been randomly drawn, adjusted to minimize overlap between subjects. Since the value of an individual's database entries is independent of other subjects', concerns of free riding present in traditional public goods experiment were likely minimal in this setting.

4.3.2 Treatments

Baseline=Excuses+Monitored

Excuses: A random mechanism embedded in the database software provided subjects with excuses to quit volunteering. Subjects clicked on a button on their screen to 'roll a die' after the training session. This die determined an individual's maximum time limit; a subject could stop at any point before the time limit but could not make any further database entries afterwards.[13] This random mechanism introduced the probability δ of being prevented from working by external circumstances described in section 4.2. Subjects were aware that each person could be limited by the randomly determined maximum time but were unaware of the true probability distribution of time limits. This approximates the natural occurrence of excuses where the true distribution of obstacles to prosocial behavior is unknown; all that is known is that $E(\delta) > 0$.

In our experiment, $\delta = 0$ with probability $\frac{2}{3}$, ensuring that a large share of the data was generated from subjects who did not have excuses and could be compared directly to subjects in the Remove Excuses treatment. In order for it to be credible to subjects that there was a randomly generated stopping point, we set $\delta = 1$ with probability $\frac{1}{6}$, meaning that some subjects would be observed leaving the lab right away. The remaining $\frac{1}{6}$ of subjects received a time limit randomly chosen between 1 and 90. Neither the experimenter nor other subjects in the room know for certain if a subject had stopped by choice or because of the random mechanism.

Monitored: The experimenter stays at the front of the room for the entire period of volunteering and answers subjects' questions in person.[14]

[13] We explained to subjects that this protected their anonymity.
[14] A lab technician was available to deal with computer problems if they arose.

Remove Excuses: No Excuses + Monitored

In this treatment, the random mechanism was disabled. After training, subjects were told that they could stay in the lab and volunteer for any amount of time they chose, up to 90 minutes.

Remove Monitor: Excuses + Unmonitored

In this treatment, the experimenter left the room after training. In case questions about lab protocol or the volunteering task arose throughout the experiment, subjects could initiate contact with the experimenter through an anonymous chat software. Subjects randomly selected chat IDs out of a paper cup, thus fully assuring that their identity was protected from the monitor.[15]

Implementation

Pilot tests of the laboratory experiments took place at Claremont McKenna College in 2007 and the full set of experiments was run at UCLA[16] in Spring 2008 and Spring 2009.[17] The full set of experiments were run as 13 separate sessions with a total of 156 subjects. Each session has between 10 and 16 subjects. We ran 4 sessions of Remove Excuses, 5 sessions of Baseline, and 4 sessions of Remove Monitor. The average number of subjects per session in each treatment is 12. We consider two outcome variables: the number of minutes worked by subjects and the number of entries completed per minute.

Over the course of running the experiments, we completed a database of lesson

[15] Note that we do not implement a treatment with Excuses and Monitored. The theories we consider here do not make predictions about the interaction between monitoring and excuses. Therefore we focus on the effectiveness of each treatment in isolation.

[16] We attempted to replicate our experiment with actual SOW tutors, however logistical restrictions resulted in inadequate participation.

[17] The experiments ran at Claremont include only a subset of the treatments discussed in the chapter. The pilot results support our findings from the main experiments at UCLA and are available from the authors upon request.

plans before continuing on to educational activities.[18] The task had to change once the first task was completed to ensure that subjects' volunteered efforts continue to be useful for the organization. All data analysis controls for the task change.

We also collected data on demographic characteristics that have been found to be important determinants of prosocial behavior.[19] To control for past volunteering experience, we ask subjects to report the length of time since their last volunteering experience, the organization they worked with, and the rating they assign to that experience. We also asked them to rate the value of the work done in the lab volunteering task and to report whether they think volunteers should be paid for their time. To control for the relevance of social connections or peer pressures, we asked the subjects to report the number of people in the room they knew by name. The data collection is conducted by an online survey; subjects are automatically directed to that page when they click on a 'Finish Volunteering' button on the database software.

4.4 Results

Of these 156 subjects in the experiment, 121 subjects were not affected by the random mechanism, receiving a time limit of 90 minutes. We classify these subjects as *unrestricted* and treat them as our primary sample.[20] We see a range of behavior in the experiment, with some subjects leaving right away while others remain to volunteer for nearly 90 minutes. The largest and most significant treatment effect comes from a comparison of subjects in the Excuses and Remove Excuses treatments. Figure 4.1

[18] The complete database of the results of subjects' volunteer work is available at http://www.hss.caltech.edu/∼mmcconnell/data.xls

[19] Schady, 2001, Freeman, 1995, Mellstrom and Johannesson, 2005) for gender and (Brooks, 2006) for religious activity

[20] Excluding subjects whose volunteering time was restricted does not introduce selection effects since these subjects were randomly chosen by our random mechanism. For the dynamic model, regression models with the full sample controlling for the restricted time limit are also included in the appendix.

presents a comparison of the empirical distributions of minutes volunteered. Table 4.1 shows the average minutes volunteered in each of the three treatment groups.

Table 4.1. Average minutes volunteered by treatment

	Remove Excuses No Excuses Monitored	Baseline Excuses Monitored	Remove Monitor Excuses Unmonitored
Average	38.76	20.02	26.97
Standard Error	(3.06)	(1.78)	(2.19)
N	49	41	31

Only Subjects Unrestricted by Time Limit

Consistent with the **Excuses Prediction**, removing excuses increases the total minutes volunteered. The difference between Remove Excuses and Baseline is positive and statistically significant at the 1% level using a nonparametric Wilcoxon (Mann-Whitney) test (z=4.26). Figure **??** illustrates that the distribution of minutes worked in the Remove Excuses treatment stochastically dominates the distribution of minutes worked in the Baseline. This suggests that unrestricted volunteers contribute more time in a social environment where contributions are not limited by external circumstances.

The **Monitoring Prediction** was not supported by the data. A comparison of Baseline and Remove Monitor shows that more volunteering happens in the *absence* of the monitor. This difference is statistically significant at the 5% level (Mann-Whitney test statistic of $z = 2.41$). Furthermore, the distribution of minutes worked in the Remove Monitor treatment stochastically dominates the minutes worked in the Baseline treatment. This suggests that observation by a monitor does not necessarily increase prosocial behavior.[21] (Mann-Whitney test statistic of $z = 2.41$)

Table 4.2 reports session level summary statistics for the 13 sessions for unrestricted subjects. We see a consistent pattern of higher average minutes worked in

[21] We do not make theoretical predictions about the effect of excuses relative to monitoring. However, using the Mann-Whitney test, we find that minutes worked under Remove Excuses is higher than Remove Monitor at the 5% level (z=2.10).

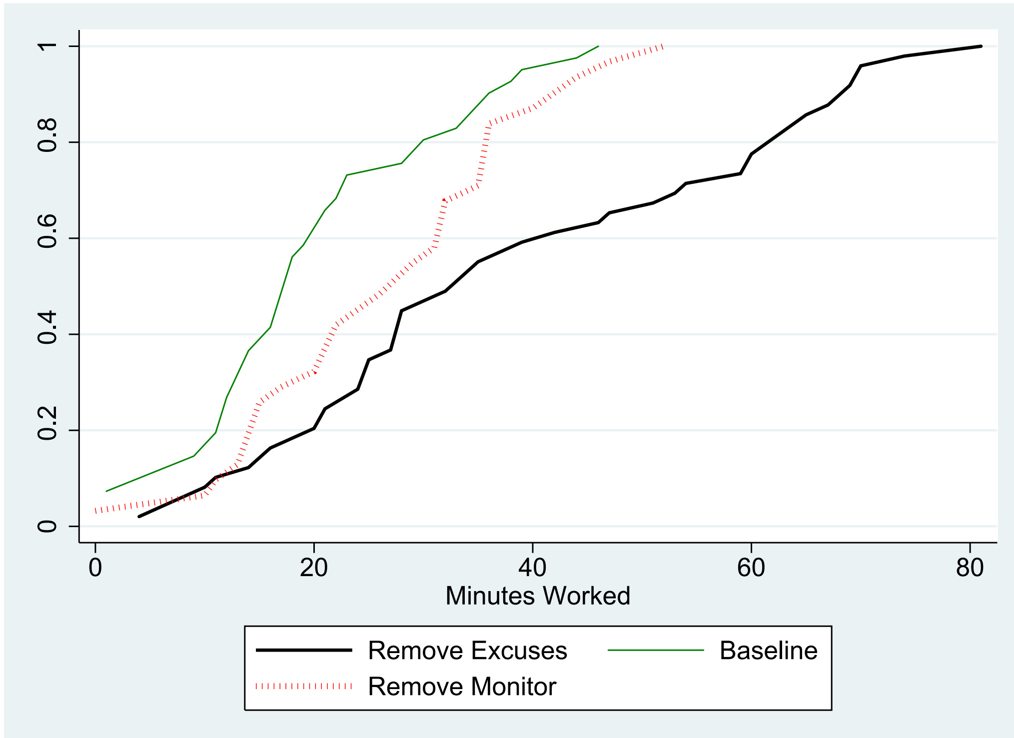


Figure 4.1. CDF of minutes volunteered

the Remove Excuses treatment. The Mann-Whitney test of the difference in average minutes worked at the session level across Excuses and Remove Excuses treatments yields a z-statistic of 1.389 which has a p-value of 0.08 for a one-sided test. We do not see a consistent pattern of higher average minutes worked in the Remove Monitor treatment when compared to the Monitored treatments (Mann-Whitney test statistics $z = 0.77$). We conclude that the effect of removing excuses is robust to localized social dynamics occurring at the level of the experimental session.

Table 4.2. Session level statistics

Treatment	Average Minutes	Standard Deviation	Min	Max	Unrestricted Individuals	All Individuals	Average of Session Averages
Excuses Monitored	18	13	1	39	6	12	
	18	9	9	35	10	13	
	31	9	18	44	6	11	
	22	14	1	46	10	13	
	15	7	1	23	9	12	
							21
Excuses Unmonitored	26	21	0	52	5	10	
	28	11	11	42	7	12	
	29	13	13	47	10	13	
	25	7	15	36	9	11	
							27
No Excuses Monitored	53	14	32	74	10	10	
	23	15	4	53	16	16	
	67	6	60	81	10	10	
	25	5	15	35	13	13	
							42

All Summary Statistics refer to individuals unrestricted by time limit

4.4.1 Consistency of Lab Behavior with Natural Volunteering Behavior

We perform several robustness checks to confirm that lab behavior is consistent with volunteering behavior in a natural setting. First we verified that subjects were actually working during the experiment by examining each subject's output. Figure **??** shows the relationship between the number of minutes worked and the entries completed. The strong positive trend between minutes worked and entries completed suggests that subjects were actually working instead of merely pretending to work.[22]

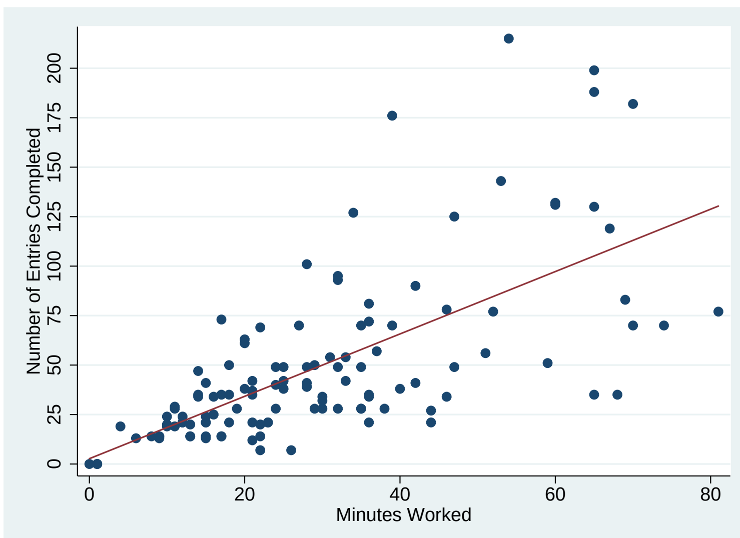


Figure 4.2. Time volunteered and amount of work completed

We then examine the relationship between the number of minutes worked and subjects' self-reported valuation for the task. As we would expect, the higher subjects rated the task, the longer they work (figure **??**).[23]

[22]We also manually checked for evidence of internet searches or webpage visits unrelated to the task at hand after subjects finished working. We saw only 4 - 5 cases where subjects were doing work unrelated to the experiment.

[23]One concern is that some subjects who indicated that they had never volunteered before *do* volunteer in the lab. After conversing with the subjects at the end of the experiment, we believe this is due to the lower cost of volunteering in the lab. All the usual transactions costs for volunteering such as searching for a cause to work for, learning the task, and traveling to the site has been removed in our setting.

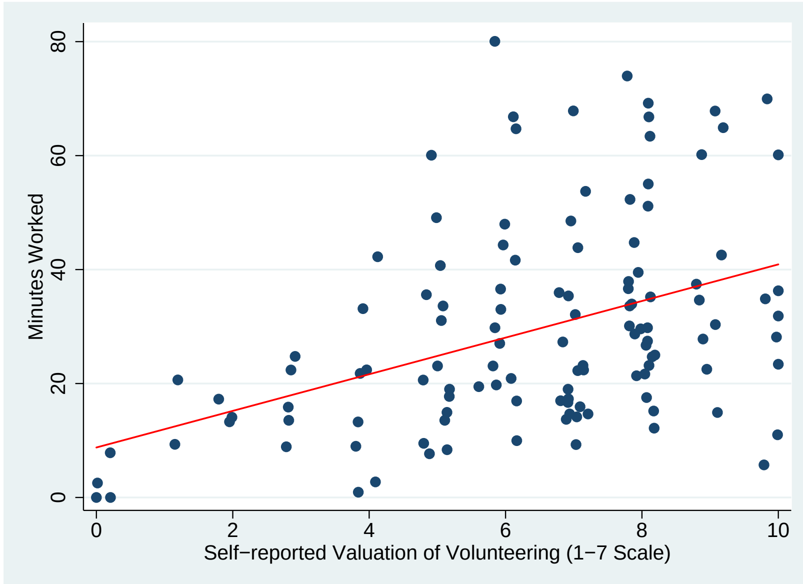


Figure 4.3. Time volunteered and self-reported value of volunteering

In model 1 of table 4.3 we report results with covariates from least-squares regressions examining the **Excuses Prediction** and the **Monitoring Prediction**.[24] In models 2, we included random effects for experimental sessions to allow for the possibility of group specific norms, or other correlation in behavior in each experiment. The estimated coefficient on Remove Excuses suggests that removing excuses doubles the time volunteered above the Baseline.[25]

In both models, demographic characteristics do not have predictive power in explaining time volunteered. Tests for the joint significance of all of the demographic controls yields an F-statistic of 0.53 for model 1 and a χ^2-statistic of 3.67 for model 2. While empirical studies suggest that demographic variables such as gender and

[24]One subject who finished volunteering early failed to complete the survey and we therefore impute the values for their demographic characteristics. See regression without the covariates in the Appendix.

[25]Subject in earlier experiments searched for worksheets (*task1*), while those in later experiments searched for educational activities. We estimated a separate intercept for subjects working on worksheets, *task1*, which is negative and statistically significant. This may be because subjects may have found searching for activities less tedious than searching for worksheets and therefore were more willing to spend time on activities. The results of separately estimating the treatments on the task 1 and task 2 data are qualitatively similar.

Table 4.3. Main treatment effects

	Least Squares	Random Effects		
	Model 1	Model 2	Model 3	Model 4
	Minutes Worked		Entries	Entries/Min
(Intercept)	21.975***	21.907***	33.461***	1.393***
	(4.066)	(7.172)	(11.715)	(0.230)
Remove Excuses	19.959***	22.124**	36.465**	0.195
	(4.216)	(10.240)	(14.409)	(0.248)
Remove Monitor	7.724**	6.815	1.152	-0.309
	(3.218)	(10.280)	(14.656)	(0.256)
Task 1	-5.26	-4.231	18.655	0.805***
	(3.551)	(8.826)	(12.465)	(0.215)
Random Effects (by experiment)	-	YES	YES	YES
ρ	-	0.612***	0.190***	0.058
Breusch Pagan LM statistic	-	(185.97)	(15.46)	(0.45)
Covariates:				
Male	-2.622	-2.48	-12.232*	-0.105
	(3.065)	(2.190)	(6.652)	(0.158)
Religious	0.432	2.062	-1.479	-0.044
	(3.163)	(2.247)	(6.823)	(0.162)
Recent Volunteer	1.716	1.454	3.045	0.013
	(3.103)	(2.154)	(6.589)	(0.157)
Know other subjects	-2.783	-2.814	3.533	0.250
	(3.145)	(2.793)	(8.561)	(0.205)
N	121	121	121	121
Test Statistic	4.880	8.740	17.850	22.830
P-Value	0.000	0.272	0.013	0.002
Test	F-Test	Wald test	Wald	Wald

* significant at 10%; ** significant at 5%; *** significant at 1%

Robust standard errors in parenthesis

Subset of subjects whose volunteering was unrestricted by random time limit

,

religion are correlated with volunteering activity, they are not a central determinant of behavior in our experiments.[26]

4.4.2 Alternative Explanations

So far we have interpreted the findings that unrestricted subjects work more in the Remove Excuses treatment as evidence that subjects work less when the stigma of not working is alleviated by unverifiable excuses. We now consider several alternative explanations. First, subjects with higher time limits in the Excuses treatment may have worked less because they were motivated by a conditional cooperation motive and were willing to work only while others were working. In other words, it may be that introducing a random time limit from our experiments changed subjects' interpretation of the social correct contribution. We made efforts to ensure that the language across the treatments is identical in the instructions (provided in the Appendix). In each treatment, subjects are told that they can stop volunteering whenever they like regardless of their assigned time limit. The random mechanism is explained as a way of ensuring subjects' privacy and not linked in any way to what subjects should do.[27]

It may also be that subjects perceive different time limits to be unfair and might be less willing to work if they perceived that they were being asked to work more than others. In order to consider the possibility of this fairness motive, we check for negative correlation between the random maximum time limit of the 35 *restricted* subjects and the actual minutes worked. Figure **??** illustrates that higher time limits

[26]Demographic trends in our experiment follow field evidence to a certain extent. For example, many studies have shown that women volunteer more than men – the negative coefficient of *Male* suggests that this may be true in our population. We also see further evidence of a negative relationship between males and volunteering in our duration model analysis presented in section 4.4.4.

[27]It may also be that the random time limit served served as an anchor that influenced subjects' behavior mechanically. However, the fact subjects were responsive to the behavior of others and the size of the audience, suggests that our treatment did successfully alter the social environment (and that anchoring was not the main factor in determining the treatment's effectiveness).

did not induce lower volunteered time (the 45 degree line is provided as a point of comparison).[28] Instead, we see that that subjects work more when their work time is less restricted. This result is consistent with nonprofits insistence on 'the power of the ask,'[29] or the idea that people will give more if asked.

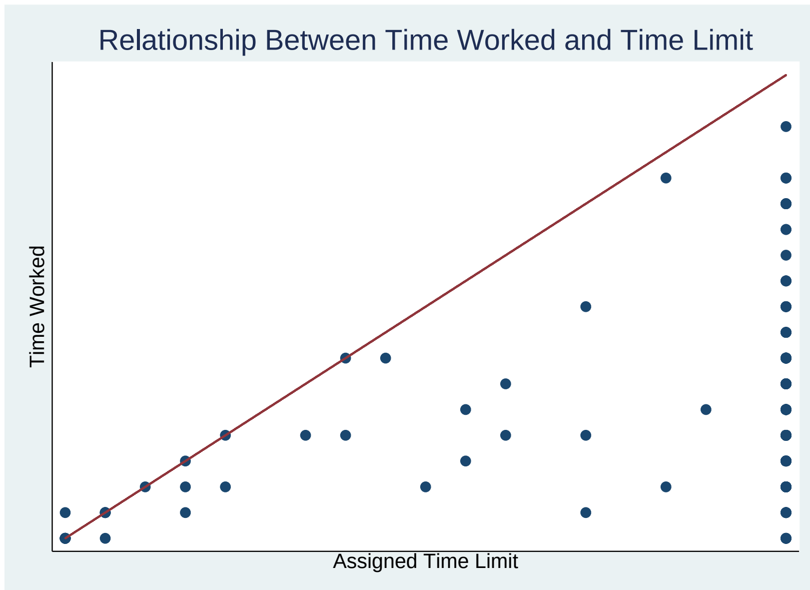


Figure 4.4. Time restriction and time volunteered grouped in 5 minute intervals

Another possibility is that the maximum time limits lowered volunteering by increasing subjects' attention to the value of their time. However, we find no evidence in the survey that subjects in the Excuses treatment are equally likely to favor compensation for volunteers in their situation as subjects in the Remove Excuses treatment.[30] We would expect subjects to consider volunteering time to have a different value in the Excuses treatment if it called their attention to the value of their time.

[28]One subject stayed in the lab even though he was assigned a 0 time limit and therefore falls above the 45 degree line.

[29]Giving and Volunteering in the United States 2001, The Independent Sector; Andreoni (2006); Andreoni and Payne (2003)

[30]The Mann-Whitney test statistic for the comparison is $z = 0.11$.

4.4.3 Quantity and Quality of Contribution: Time and Productivity

We now consider the effect of the image treatments on 'quality' of work, the invisible dimension of volunteer's contribution. In model 3, we consider the total number of data entries completed, while in model 4 we consider productivity (the number of entries per minute). Results considering the total number of data entries completed is consistent with results when we consider the number of minutes worked.

When we consider productivity, the coefficient on Remove Excuses is close to zero and not significant, as we would expect with an invisible dimension of volunteer work. While not significant, the coefficient on Remove Monitor is negative, suggesting that while we see more time volunteered in the unmonitored sessions, the time volunteered may be slightly less productive. We conclude that while removing excuses has a powerful impact on increasing volunteering time, it does not decrease the quality of work (productivity).[31]

4.4.4 Duration Model: Volunteers Response to Changes in Social Environment

We now more carefully examine how volunteers respond to their dynamically changing social environment. We first consider predictions from the bad apple model. In order to consider the possibility that subjects consider the decision to stop working differently when they are the first person to leave, we consider the relationship between the number of people who have left and the average number of minutes until someone else leaves in figure **??**. The figure illustrates an overall positive relationship between

[31] Unlike our estimation of treatment effect on visible output, we do not see any evidence of statistically significant random effects at the level of the experiment using a Breusch Pagan test (test statistic=0.45). Norms or dynamics at the level of the experimental session play a role only when behavior is visible.

the number of subjects who have left and the time until the next subject leaves. However, subjects wait much longer to leave when no one else in the room has left.

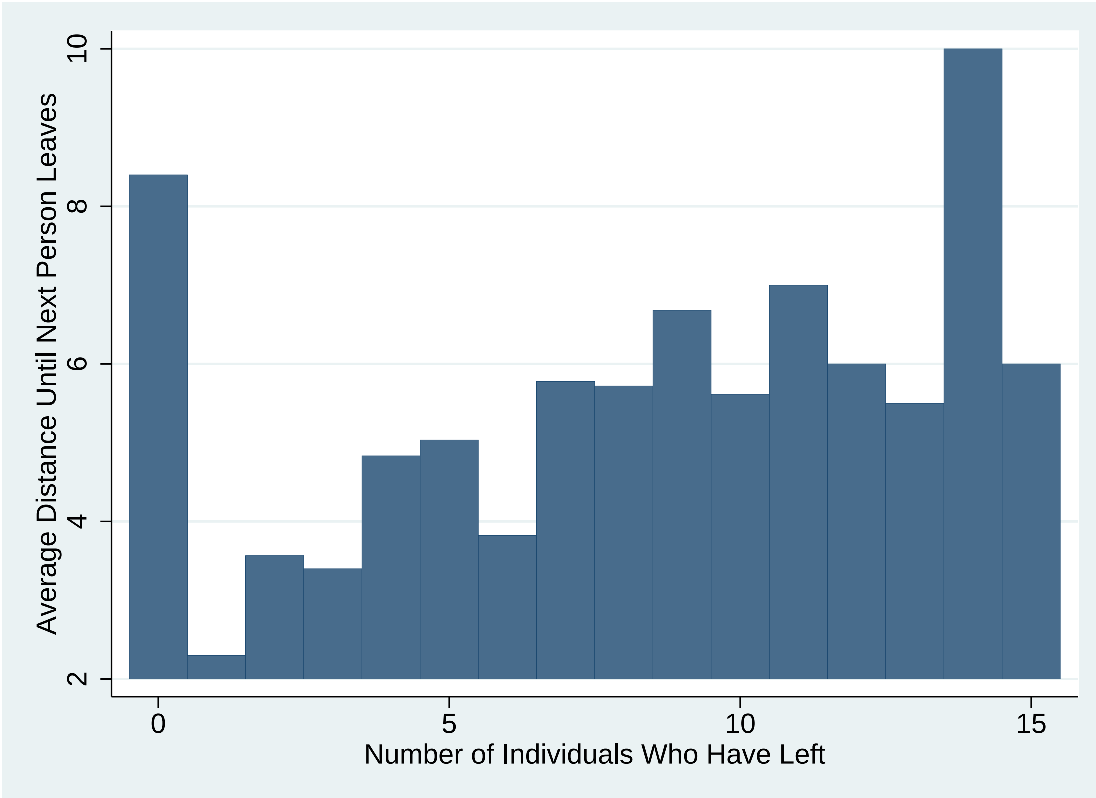


Figure 4.5. Relationship between the order of leaving and clustering

We also consider the bad apple norm within the framework of the duration model. Specifically, we examine whether anyone else had left in the preceding time interval and the number of individuals leaving in the current period. We then consider the possibility that the salient audience is the number of remaining peers at the start of the time interval.

For our discrete time model, we consider time in five minute intervals.[32] In table 4.4 we consider the subsample of 121 unrestricted individuals.[33] Model 1 estimates the

[32]Since it takes less than five minutes to complete a unit of the volunteering task, intervals larger than five minutes would be too large to capture the effect of changes in the social environment on decisions. The results are robust to smaller intervals of time.

[33]The full sample of 156 individuals is presented in appendix table 4.5 where we added an addi-

baseline discrete time duration model when no time varying social image variables are included. In any time interval, subjects are 24% more likely to continue volunteering when excuses are removed and 10% more likely to continue working when not observed by a monitor. We see the willingness to work decline over time: with every additional five minutes, subjects probability of continuing to work decreases by 6%.

4.4.4.1 Evidence – Bad Apple Stigma

In our alternative model of volunteering (section 4.2.2) subjects, uninformed about the distribution of altruism in their population, are unable to solve for the equilibrium time contribution and instead decide whether to continue volunteering or not on the basis of the current social environment. We focus in particular on the stigma of stopping work before everyone else, which we refer to as the bad apple stigma. This stigma cause a buildup of subjects who would like to leave the room but are uncomfortable doing so unless someone else leaves first. In this model, the action of someone stopping will lead to a cluster of people leaving. The availability of excuses reduces this stigma, thus making the fact that nobody has stopped less relevant and reducing the buildup of volunteers waiting to leave.

Model 2 of table 4.4 estimates the probability that a person continues to volunteer in a particular time interval given AnyoneLeft, a binary variable that is 1 if someone has left the room. AnyoneLeft by itself is negative but not significant, however, its interaction with RemoveExcuses is negative and significant which is consistent with the **Cascade Prediction**. Subjects are more likely to leave after the first person left when there are no excuses available.

In model 3 we estimate the probability that subjects continue working when a cluster of other individuals leaves within that time period. We find that the Number

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tional control for the number of time periods remaining in each individuals' maximum time limit. All conclusions hold qualitatively.

Table 4.4. Discrete time model for unrestricted subjects

	Model 1	Model 2	Model 3	Model 4
Probability of working	0.161	0.178	0.157	0.194
	dy/dx	dy/dx	dy/dx	dy/dx
Variable				
Remove Excuses	0.244***	0.417***	0.337***	0.178**
	(0.062)	(0.117)	(0.071)	(0.079)
Remove Monitor	0.108**	0.008	0.105**	0.080
	(0.045)	(0.063)	(0.049)	(0.086)
Period #	-0.057***	-0.058***	-0.058***	-0.027***
	(0.005)	(0.005)	(0.005)	(0.006)
Task1	-0.072**	-0.065*	-0.072**	-0.063
	(0.033)	(0.034)	(0.031)	(0.037)
Remaining periods before time limit				
Time varying social factors				
Anyone left in prior periods	-	-0.121	-	-
	-	(0.077)	-	-
Anyone left in prior periods	-	-0.165**	-	-
x No Excuses	-	(0.057)	-	-
Anyone left in prior periods	-	0.117	-	-
x Unmonitored	-	(0.087)	-	-
# subjects leaving in period	-	-	0.009	-
	-	-	(0.008)	-
# subjects leaving x No Excuses	-	-	-0.052***	-
	-	-	(0.014)	-
# subjects leaving x Unmonitored	-	-	0.006	-
	-	-	(0.012)	-
# subjects remaining in period	-	-	-	0.053***
	-	-	-	(0.010)
# subjects remaining x No Excuses	-	-	-	-0.012
	-	-	-	(0.009)
# subjects remaining x Unmonitored	-	-	-	-0.005
	-	-	-	(0.009)
Demographic controls				
Male	-0.051*	-0.069**	-0.048	-0.086**
	(0.030)	(0.032)	(0.029)	(0.037)
Religious	0.011	0.025	0.010	0.041
	(0.031)	(0.033)	(0.030)	(0.040)
Recent Volunteer	0.016	0.037	0.018	0.023
	(0.031)	(0.033)	(0.030)	(0.038)
Know other subjects	-0.013	-0.023	-0.012	-0.002
	(0.031)	(0.036)	(0.030)	(0.042)
AIC	0.584	0.539	0.515	0.538
N	2299	2299	2299	2299

Standard errors are clustered by individuals
Marginal effects after glm (Bernoulli distribution with complimentary log-log link function)
Periods are defined in minute intervals (0, 1-5, 6-10)
* significant at 10%; ** significant at 5%; *** significant at 1%
Subset of Subjects who faced no time restriction

of Subjects Leaving does not have a significant effect on the probability of leaving, but its interaction with RemoveExcuses is negative and significant. The presence of the monitor has little effect on both models. We find that when excuses are not available, subjects are 16.5% more likely to leave when someone else has left and 5% more likely to leave for every subject that leaves within that time period. The fact that we do not observe a significant effect of the number of other individuals in the room when excuses are present suggests that subjects are not merely imitating each other's behavior and is consistent with our model of bad apple stigma avoidance.

RemoveExcuses increases the probability that individuals continue to volunteer by 42% even after attempts to estimate the bad apple stigma. While the bad apple norm is important, we estimate that only one third of the treatment effect of removing excuses derives from the reluctance to be the first person to leave. The utility of continuing to work seems to be higher throughout the experiment when external circumstance cannot be blamed for stopping work. Similarly, Remove Excuses continues to have a powerful effects on behavior after taking cascades of subjects leaving into account, increasing the probability of volunteering by 27% - 34%.

4.4.4.2 Evidence on Audience Effects

So far, following the experimenter demand effects literature and the emphasis of practical literature on the presence of volunteer leadership, we have focused on an authority figure as the salient audience for image signaling. Our experimental results on removing the monitor are surprising: we see evidence weakly suggestive of crowding out from monitoring. Motivated by findings from Falk and Ichino (2006) where individuals are more willing to work more when working alongside others, we consider the possibility that the salient audience for image signaling are fellow volunteers. A peer group may provide individuals with camaraderie (Rotemberg, 1994) or in the case of image signaling, higher image benefits from a larger audience.

In model 4 of table 4.4, we estimate the effect of the number of peers present at the beginning of the period on the probability of continuing to volunteer in that period. We see evidence consistent with the hypothesis that subjects are signaling their altruism to peers, not to the monitor. An additional peer observer present during the period will increase the probability of continuing to work by 4% - 5% and there is no differential effect of Remove Excuses or Remove Monitor. The coefficient on RemoveMonitor is consistently positive (though not always significant), suggesting that being observed by an authority figure has the opposite effect from being observed by a large peer group.[34] One implication of this finding is that it is important to carefully identify the potential audience to gauge the effectiveness of internventions designed to manipulation social image. In particular, nonprofits may want to look at the effect of members of a volunteer group on each other in addition to the effect of a volunteer leader on the group.

This suggests that the RemoveExcuses treatment works in part because it maintains a larger peer group, since the random mechanism limits the amount of time that some of the subjects can stay in the room. However, we find that even after controlling for the number of subjects in the room, subjects are 20% more likely to continue working in each time interval in RemoveExcuses.

4.5 Conclusion

Recent theoretical and empirical studies have shown that image concerns play a central role in prosocial behavior. However, while a large body of literature addresses financial contributions, only a small literature exists on contributions of time and

[34] This is consistent with crowding out from monitoring; an audience of peers does not communicate distrust, thus increasing image concerns without crowding out intrinsic motivation. An alternative interpretation is that the pleasure from additional peers comes from additional intrinsic motivation and hence does not affect image signaling.

effort.[35] We focus on volunteering, a prosocial activity performed by a quarter of Americans on a weekly basis (Bureau of Labor Statistics, 2006).[36]

Our experiments attempt to unpack various components of the social environment, with a focus on social visibility and stigma. We provide experimental evidence on the effectiveness of two important features of the social environment common to many volunteering settings: 1) the availability of excuses for not volunteering and 2) the presence of a monitor. We consider predictions from a model of image signaling behavior as well as a dynamic model where individuals respond to features of the social environment.

Working closely with the nonprofit School on Wheels, we test these theoretical predictions with an experiment that translates the core components of institutional volunteering into a carefully controlled laboratory setting. By using the nonprofit's own promotional material and volunteering task, we engage student subjects directly in the social mission of the nonprofit. The lab setting allows us to control recruitment, task training, and more importantly, the observability of volunteers' actions and availability of excuses while precisely measuring both time and effort contributed.

Subjects contributed substantial time and effort in our experiment, producing several large databases of internet resources. Providing subjects with an excuse to leave early reduced the average minutes worked by half. Our experimental evidence also shows that prosocial behavior is slightly lower when a monitor (the experimenter) is present.

Our evidence suggests that subjects' response to the social environment is complex. First, the identity of the observer matters: while a larger audience of peers makes individuals more likely to volunteer, the presence of a monitor can be coun-

[35]Some examples of studies of financial contribution include Karlan and List (2007), Landry et al (2006) and Shang and Croson (2009). Studies in labor contribution include Gneezy and Rustichini (2000), Ariely, Bracha, and Meier (2009), Carpenter and Myers (2007)

[36]Hodgkinson and Weitzman (1994) report that in 1990, Americans gave $100 billion in funds, and an estimated $182.3 billion worth of volunteer labor.

terproductive. Second, when we consider the dynamic context of volunteering more closely, we see that subjects are highly sensitive to many different features of the environment. On the one hand we see behavior consistent with a dynamic response to social norms – in the absence of excuses, subjects are more likely to leave after someone else has left and more likely to leave when others are leaving.[37] However, even when we estimate subjects' response to components of the social environment such as whether anyone has left, the clusters of people leaving, or the number of subjects left in the room, the availability of excuses remains an important predictor of the willingness to continue volunteering.

Volunteers' productivity was unaffected by changes in the availability of excuses. Image treatments targeting the observable component of the contribution (time) do not affect the unobservable component of the contribution (productivity). This suggests a dual purpose of prosocial behavior as both an expression of intrinsic altruism and as a way of publicly signaling this altruism.

Our results have implications for interventions aimed encouraging prosocial behavior. Restricting the ability of potential volunteers to give excuses can increase the amount of time volunteered without impacting the quality of work. Therefore common nonprofit practices, such as asking for contributions of time or money in public (i.e., church collection plates), or precommiting contributions (such as pledges or organizing work retreats) are likely to be effective. However the success of eliminating excuses may be reduced by the presence of a single bad apple who openly refuses to contribute. In other words, while social image can be manipulated to increase prosocial behavior, the level of success is sensitive to the details of the social environment.

[37] The fact that we see this heightened sensitive only in the absence of excuses makes it less likely that this behavior represents pure conformity.

4.6 Appendix

Proofs for section 4.2

$\Delta(v^*|\delta, x)$ *is increasing in* v^*. Let $[v_L, v_H] \in \mathbb{R}_+$ indicates the interval where v is drawn from. By Proposition 6 (Benabou and Tirole, 2006), the assumption that $g(v)$ is decreasing implies that $\Delta(v^*|\delta, x)$ is increasing in v^* when $\delta = 0$. Since M^+ is unaffected by δ, we only need to show that the slope of M^- when $\delta > 0$ lies beneath the slope of $E(v|v < v^*)$.

Let $f(v) \equiv E(v|v < v^*)$ and $f'(v)$ be its derivative. Let $f_H \equiv E(v|v < v_H) = E(v)$. Also define $e(v^*) \equiv \delta + (1-\delta)G(v^*)$ and $h(v^*) \equiv \frac{(1-\delta)G(v^*)}{e(v^*)}$. Rewrite $M^-(v^*|\delta) = \delta\ f_H e(v^*)^{-1} + g(v^*)f(v^*)$ and take its derivative:

$$\frac{\partial M^-(v^*|\delta)}{\partial v} = -\frac{\delta f_H}{e(v^*)^2} + h'(v^*)f(v^*) + h(v^*)f'(v^*). \tag{4.4}$$

Taking the derivate of $h(v^*)$ and substituting with $e(v^*)$ we get

$$h'(v^*) = \frac{(1-\delta)G'(v^*)e(v^*) - (1-\delta)G(v^*)e'(v^*)}{e(v^*)^2} = \frac{(1-\delta)G'(v^*)\delta}{e(v^*)^2}. \tag{4.5}$$

Substituting equation (**??**) into equation (**??**) and simplifying, we are left to show that

$$\frac{\delta(1-\delta)G'(v^*)f(v^*) - \delta f_H}{e(v^*)^2} < f'(v^*)(1 - h(v^*)).$$

Since $0 < h(v^*) < 1$ and $f'(v^*) > 0$, $f'(v^*)(1-h(v^*)) > 0$. Since by assumption $g'(v^*) < 0$, $(1-\delta)G'(v^*)f(v^*) < f_H$, which implies that the slope of $M^-(v^*|\delta > 0)$ is smaller than $M^-(v^*|\delta = 0)$. Hence $\Delta(v^*|\delta > 0, x)$ must be increasing in v^*.□

Let $\bar{a}(\delta, x) \equiv N(1 - G(v^*))$ denote the total participation among a population of

N individuals. Removing excuses increases total participation.

$$0 = \delta < \delta' \Rightarrow \bar{a}(\delta, x) > \bar{a}(\delta', x).$$

Let v' the solution to $v + \Delta(v|\delta', x) - C = 0$. Honor remains unchanged by excuses while stigma is lowered, hence $\Delta(v|\delta', x) < \Delta(v|\delta, x)$. When excuses become unavailable $v' + \Delta(v'|\delta, x) - C > 0$, which implies v' will still participate. By lemma **??** we know that $\Delta(v^*|\delta, x)$ increases in v^*, hence the new cutoff type v^* whom is now indifferent about volunteering must be a lower type. Since participation is decreasing in type, $v^* < v'$ implies higher total participation when $\delta = 0$.

Reduced monitoring decreases participation.

$$0 < x < x' \Rightarrow \bar{a}(\delta, x) < \bar{a}(\delta, x')$$

Let v' the solution to $v + \Delta(v|\delta, x') - C = 0$. When visibility is decreased, $v' + \Delta(v'|\delta, x') - C < 0$ hence type v' will no longer participate. By lemma **??** we know that $\Delta(v^*|\delta, x)$ increases in v^*, hence the new cutoff cannot be smaller than v'. Hence $v^* > v'$, and since participation is decreasing in type, this implies lower total participation.

We now extend this binary participation model to our volunteering setup. Suppose there are t level of contribution: participate 1 minute, 2 minutes, up to a maximum of T minutes. Let $C(t)$ be the cost function for contribution level t where $C'(t) \geq 1$ (cost does not decrease in time). Let v_t^* be the threshold type for participation level t. Individuals contribute at level t if

$$u(t) = vt - C(t) + \Delta(v_t^*|\delta, x) \geq 0.$$

Treating each individual as facing t binary participation decision, let $v^* = (v_1^*, .., v_t^*, .., v_T^*)$ be the equilibrium threshold types induced by environment (δ, x). We can show that higher levels of participation induce strictly higher thresholds than lower levels of participation; in other words individuals who do not choose to volunteer in level t will also not participate in level t' where $t' > t$. The monotonicity of v_t^* allows total time volunteered to be computed in intervals. This allows us to extend lemma **??** and lemma **??** to t levels of contribution.

Level t threshold type v_t^* is strictly higher than level $t-1$ threshold type v_{t-1}^*. The utility of the cutoff type at each level is zero:

$$v_t^* t - C(t) + \Delta(v_t^*|\delta, x) = v_{t-1}^*(t-1) - C(t-1) + \Delta(v_{t-1}^*|\delta, x) = 0.$$

Note that $v_t^* = \frac{C(t)-\Delta(v_t^*|\delta,x)}{t}$. Subtracting the utilities we get

$$(v_t^* - v_{t-1}^*)(t-1) + v_t^* - (C(t) - C(t-1)) + \Delta(v_t^*|\delta, x) - \Delta(v_{t-1}^*|\delta, x) = 0. \quad (4.6)$$

Substituting v_t^* into equation (**??**) and simplifying we arrive at

$$(v_t^* - v_{t-1}^*)(t-1) + \Delta(v_t^*|\delta, x) - \Delta(v_{t-1}^*|\delta, x) = \frac{\Delta(v_t^*|\delta, x)}{t} + C(t) - C(t-1) - \frac{C(t)}{t}.$$

From the assumption that $C'(t) \geq 1$, $C(t) - C(t-1) - \frac{C(t)}{t} \geq 0$. Since $\frac{\Delta(v_t^*|\delta,x)}{t} > 0$ the entire right hand expression is positive. By lemma **??** we know that $\Delta(v^*|\delta, x)$ increases in v^*, hence v_t^* cannot be smaller than $v_{t-1}^* < 0$ since this implies $\Delta(v_t^*) - \Delta(v_{t-1}^*) < 0$ and that the left hand expression is negative. Hence $v_t^* > v_{t-1}^*$.$\square$

In a volunteering setup involving T levels of participation we can make the following two predictions.

- **Excuses Prediction:** Removing excuses increases time volunteered.

$$0 = \delta < \delta' \Rightarrow \bar{a}_T(\delta, x) > \bar{a}_T(\delta', x).$$

- **Monitoring Prediction:** Reduced monitoring decreases time volunteered.

$$0 < x < x' \Rightarrow \bar{a}_T(\delta, x) < \bar{a}_T(\delta, x').$$

Let $v' = (v'_1, \ldots, v'_t, \ldots, v'_T)$ denotes the vector of cutoff types induced by environment (δ', x) while $v^* = (v^*_1, \ldots, v^*_t, \ldots, v^*_T)$ denotes the vector of cutoff types induced by environment (δ, x). Hence v'_t is the solution to $v_t t + \Delta(v_t|\delta', x) - Ct = 0$ while v_t solves $v_t t + \Delta(v_t|\delta, x) - Ct = 0$. Following the proof of the binary case lemma **??** we arrive at $v^*_t < v'_t$. Letting N be the total number of agents in the population, total time volunteered is

$$\bar{a}_T(\delta, x) \equiv N \sum_{t=1}^{T-1} t(G(v^*_{t+1}) - G(v^*_t)).$$

This implies that $\bar{a}_T(\delta, x) > \bar{a}_T(\delta', x)$. Using same steps and application of lemma **??** we show that $\bar{a}_T(\delta, x) < \bar{a}_T(\delta, x')$ for $0 < x < x'$.$\square$

Removing excuses increase volunteering but makes cascades more likely.

$$0 < x < x' \Rightarrow \bar{a}(\delta, x) < \bar{a}(\delta, x').$$

Cascade Prediction Let $\{v_1, v_2, \ldots, v_N\}$ represent the altruism of a group of N volunteers, listed in increasing order. Utility of volunteering an extra minute is $U_i(t) = v_i - \Delta_C(t) + S(t|x)$ where $S(t|x) = x(1 - (-(1-\delta)B))$ if no one has left and x otherwise. Let t^*_i be the solution to $v_i - \Delta_C(t) = 0$, person i's optimal stopping time without the bad apple stigma. The least altruistic individual, unaware that he is of the lowest type v_1, will continue working past t^*_1 up to time $\hat{t}_1$ which solves $v_1 - \Delta_C(t) + x(1 + (1-\delta)B) = 0$. At $\hat{t}_1$, he will leave (and suffer B) to prevent negative utility. This first act of leaving immediately removes B as a constraint to the individual optimization problem of the remaining individuals.

Depending on the distribution of altruism, this can induce a "cascade": all people of type v_i whose optimal stopping time has passed $t_i^* < \hat{t}_1$ would now also leave. Unverifiable external circumstances (δ) lowers volunteering in two ways. First, it lessens the bad apple stigma to $(1-\delta)B$. Second, it introduces a probability that a person of type $v_j > v_1$ is forced to leave at an earlier time than $\hat{t}_1$ (v_1's stopping time), thus completely eliminating the bad apple stigma early on. Cascades of people leaving are less likely in a social environment where excuses are available than one where no excuses are available since fewer people are staying to avoid the bad apple stigma.□

Table 4.5. Discrete time model for all subjects

	Model 1	Model 2	Model 3	Model 4
Probability of working	0.128	0.141	0.125	0.156
	dy/dx	dy/dx	dy/dx	dy/dx
Variable				
Remove Excuses	0.197***	0.381**	0.273***	0.139*
	(0.058)	(0.119)	(0.070)	(0.079)
Remove Monitor	0.079**	0.025	0.068*	0.057
	(0.022)	(0.045)	(0.037)	(0.071)
Period #	-0.036***	-0.036***	-0.038***	-0.010*
	(0.004)	(0.004)	(0.004)	(0.005)
Task1	-0.045*	-0.039	-0.045*	-0.031
	(0.025)	(0.027)	(0.024)	(0.029)
Remaining periods before	0.011***	0.012***	0.011***	0.015***
time limit	(0.002)	(0.003)	(0.002)	(0.003)
Time varying social factors				
Anyone left in prior periods	-	-0.027*	-	-
	-	(0.042)	-	-
Anyone left in prior periods	-	-0.127***	-	-
x No Excuses	-	(0.035)	-	-
Anyone left in prior periods	-	0.018	-	-
x Unmonitored	-	(0.047)	-	-
# subjects leaving in period	-	-	-0.001	-
	-	-	(0.007)	-
# subjects leaving x No Excuses	-	-	-0.036**	-
	-	-	(0.011)	-
# subjects leaving x Unmonitored	-	-	0.009	-
	-	-	(0.010)	-
# subjects remaining in period	-	-	-	0.039***
	-	-	-	(0.008)
# subjects remaining x No Excuses	-	-	-	-0.003
	-	-	-	(0.007)
# subjects remaining x Unmonitored	-	-	-	-0.003
	-	-	-	(0.007)
Demographic controls				
Male	-0.032	-0.041*	-0.030	-0.054*
	(0.023)	(0.024)	(0.022)	(0.029)
Religious	0.009	0.018	0.009	0.032
	(0.023)	(0.024)	(0.022)	(0.030)
Recent Volunteer	0.003	0.015	0.004	0.004
	(0.023)	(0.024)	(0.022)	(0.029)
Know other subjects	-0.008	-0.015	-0.008	-0.031
	(0.024)	(0.028)	(0.024)	(0.029)
AIC	0.607	0.547	0.516	0.541
N	2964	2964	2964	2964

Standard errors are clustered by individuals
Marginal effects after glm (Bernoulli distribution with complimentary log-log link function)
Periods are defined in minute intervals (0, 1-5, 6-10)
* significant at 10%; ** significant at 5%; *** significant at 1%

Master Subject Instructions

1. Thank you for coming. During this experiment, please do not talk, or use the web for any activities outside of the experiment. If you have any questions please raise your hand and an experimenter will come to you to answer it in private. This experiment is different from other experiments you may have participated in because we will be actually be working with a local nonprofit. Today's session will consist of a 15 minute training session, for which you will earn $10 and another $10 showup fee. After the training session, you may stay and volunteer **unpaid** as long as you like up to 90 minutes.

   After volunteering, you will complete a brief survey.

   > Random Mech: (No Experimenter in parenthesis)
   > This experiment is completely anonymous, not only to other subjects but also to the experimenter (who will not be present during the experiment). Your decisions and answers to the survey will be tagged by only an ID number, allowing us to analyze the data without using any identifying personal information.
   >
   > (Again, it is important that you do not communicate with each other. After the training session, the experimenter will have no further involvement with anyone in this experiment. However, you may ask questions to the experimenter throughout today's session using the AIMExpress. You have received a piece of paper with a username and password for the chat software. The experimenter will be on your buddy list when you sign in. If you have any problems signing in, raise your hand and a lab assistant will help you.)

2. On your keyboard there is information about School On Wheels, the nonprofit that we will be working with today. Please read the article about the organization. Our job today is to compile a database of educational activities for School on Wheels tutors. These tutors often do not have teaching background and may find it difficult to come up with age appropriate activity for kids that can be done with their limited resources. The list of activities you suggest today will help the tutors connect with homeless kids more effectively.

3. I will now pass along a sheet of paper on the type of activity that you are in charge of finding. We have staggered your task for minimum overlap with other students so that we get to cover as many areas as possible. Please take a look at your task and ask me any questions you have.

4. Before we start the actual work, we will do a five minute practice task. Click *Start Practice Task*. You are now in the database window. Please take extra care to not close this screen during the ENTIRE experiment.

5. Random Mech:

   > Notice that in the bottom of the screen there's a button that says "Roll Dice".
   > You will click this LATER when you have completed your practice task. This mechanism protect the privacy of your choice of how long to volunteer. When you click *Roll Dice*, the computer will roll a dice and randomly pick the maximum number of minutes you will volunteer today. This number will be between 0-90 minutes and the computer will automatically stop you from starting a new entry once time is up. It will not interrupt you so do not worry about losing any work. Remember: **you can stop at any point before this time (there will be a button that says Finish Volunteering).** You do not have to do the number of minutes the computer picked: how long you want to work is completely up to you. Again, your privacy is guaranteed: when you leave the room, nobody will know whether you chose

to finish or were forced to by the time limit. Before you leave, please complete the survey. After rolling the dice, if your time limit is zero, click *Start Survey*. Any questions?

6. Please press Ctrl T to open a new tab, look online for an art project using recycled materials, and input the information you found into the database window. When you are finished with your practice task please wait for further instructions before you click on anything.

7.

Public:

You can click ***Start Volunteering*** now. Now you will look for the activity listed in your sheet. Remember that you choose how much you want to work, the maximum is 90 minutes. If you chose to stay to volunteer, it is very important that you work carefully, since the information you produce today be given out to tutors as a searchable database of educational activities. Please keep the database window open and fill the survey before you go.

DB:

You can click on ***Roll Dice*** now and start the real volunteering task. Now you can look for any activity that is listed in your sheet. If you chose to stay to volunteer, it is very important that you work carefully, since the information you produce today be given out to tutors as a searchable database of educational activities. Please remember to keep the database window open and fill the survey before you go.

(I will now leave the room. To reach me at any time you can ask me through AimExpress. Please open the AimExpress, and send thx.experimenter a test message. If you have any software problems, raise your hand and a lab assistant will help you.)

Educational Activity Resource Database

Help us build a database of targeted educational activities to help tutors engage their students. Please work carefully. If you cannot find the information from the webpage, please write "N/A". Click **Next** to proceed to the next entry. Click **Finish Volunteering** if you have completely finished working.

Your practice task today is to find *instructions for an art activity using recycled materials*.
Please open another tab (Ctrl T) to perform searches and use this screen to enter information. Do not close this screen.
Use this practice session as an opportunity to ask any questions you have.

1. Subject:

2. Grade level:

3. Description/topic area (algebra, history, painting, etc):

4. Website address:

5. Approximate duration of time needed to complete (please estimate):

6. Description of online resource or the activity itself (worksheet, field trip, experiment, etc):

7. (Optional) What is interesting about this resource? What advice do you have for the tutor who chooses to do this activity with his/her student? Does it require special preparation/skills?

Chapter 5

Moral Framing and Contributions to Public Goods

This chapter represents joint work with Jacob Goeree and Antonio Rangel.

5.1 Introduction

A wide array of theory and empirical evidence[1] suggests that explaining charitable giving is not as simple as determining whether individual preferences include altruism toward others or individual benefits from the provision of public goods. In particular, recent research has considered a variety of ways in which the context of a charitable contribution may affect the willingness to give. Benabou and Tirole (2006) provide a theoretical model demonstrating that when individuals are concerned about their social image, greater social visibility and opportunities for social signaling make individuals more willing to give. Fong and Luttmer (2009) show evidence that the demonstrated worthiness of the recipient of charity makes larger gifts more likely.

While this evidence suggests that many features of the context of a gift may affect the willingness to give, we propose a different notion of how context may affect charitable contributions. Both the evidence surrounding charitable giving and the strategies of nonprofits suggest that individuals might give partly because they feel

[1] Andreoni and Miller 2002; Charness and Rabin 2002

some moral responsibility to give.

Consider for example an individual who is approached by Oxfam and asked to donate $25 to help alleviate the effect of famine on children in Africa. Many well-fed individuals living a comfortable life feel some moral responsibility to contribute when children are starving and when the cost to themselves is relatively small. However, if an individual had recently made a $25 gift to an appeal to end children's hunger from another organization, the feeling of moral responsibility might be diminished when asked to donate again and they might decline to contribute more. The difference in the success of an appeal for a contribution in these two situations cannot be explained by a difference in the worthiness in the recipient or any change in the visibility of individual generosity to others. Nonetheless, it is apparent that the context of the gift, specifically the sense of moral responsibility influence the propensity to give.

A look into the fundraising efforts of nonprofits suggests that they are aware that a sense of moral responsibility is an important determinant of individual willingness to give and that they attempt to frame the decision to contribute in these terms. Charities appeal to these motives in two ways: 1) they frequently provide individuals with a suggested contribution levels and 2) they often make an outright appeal to morality and responsibility. In figure **??**, we provide examples of charities using language that frames the contribution decision in a moral way: in each of the examples we provide individuals are asked to "do your part."

The goal of this chapter is to provide evidence on how morally induced giving is modulated by the two methods of fundraising described in the previous paragraph. Our experimental design allows us to separate the effect of simply providing a suggested contribution from the effect of providing a suggested contribution that is framed as a morally responsible choice. We find evidence that providing a suggested contribution increases the average gift and the share of individuals making a gift at the suggested level. Furthermore, we find that framing a suggested contribution

Figure 5.1. Examples of "do your part" language from nonprofits

Key: From left to right: (A) An email from the Obama presidential campaign (B) Southwest Florida Water Management District (C) National Parks Conservation Association (D) Do Your Part Recycling Co (E) A newspaper article from the Christian Post and (F) A fundraising request from a children's cancer society.

as a moral responsibility further increases the provision of the public good and the likelihood of making a gift at the suggested level.

Literature Review

A comprehensive theoretical literature has proposed a model of charitable giving in which individual giving may not be motivated by simple (pure) altruism, but also from the simple good feeling one gets from giving: "warm glow." Andreoni (1989) proposes a model in which individuals receive utility from their gift, regardless of whether the public good is provided. Experimental evidence from Crumpler and Grossman (2008) and Null (2009) confirms that individuals make contributions even when there would be no incentive to do so if individuals were purely altruistic (when their own contributions perfectly crowd out public giving). Theoretical and empirical evidence from Ribar and Wilhelm (2002) also illustrates little evidence from crowd out of charitable giving, suggesting a large share of motivation for giving comes from warm glow motivations. The theory of warm glow giving is supported by evidence from neuroeconomics Harbaugh et al. (2007) suggests that individuals exhibit neural activity consistent with reward processing when they make a voluntary contribution. We do not argue that morally motivated giving is inconsistent with giving for warm glow. In fact, we propose that the two can coexist. However, it is difficult to explain the success of language that frames the problem in terms of moral responsibility if individuals care only about the size of their own gift.

Andreoni (1995) presents evidence that may be consistent with a sense of moral responsibility, showing that individuals are more likely to make contributions to support goods with positive externalities than to reduce equivalent negative externalities. Our work is related to evidence from Andreoni (1995) that the cold prickle felt from behavior that is ungenerous is not symmetric to the warm glow felt from being generous. The evidence presented here is consistent with Andreoni's proposed asymmetry between individual reactions to positive and negative deviations from a reference point. In this chapter, we argue that endogenous preferences are modulated by "exogenous" factors such as institutions. We consider how each component of a fundraising strategy that provides individuals with a suggested contribution and frames that contribution as a moral reference point adds to the success of the fundraising strategy.

A major component of consulting work for charitable organizations consists of determining optimal "ask strings" or suggested donations.[2] Andreoni and Payne (2003) model the effectiveness of "the ask" as a latent demand for contributions to public good. Dale and Morgan (2009) provide a dynamic model of charitable giving where high levels of contributions can be supported in a voluntary contribution game as individuals learn over time about the types of other subjects, thereby providing the information necessary to support a fairness equilibrium. They find support for the theory from experiments in which individuals give more when provided with a suggested contribution amount. They illustrate that their results are consistent with predictions of Rabin's (1993) fairness model. Experimental evidence from Marks, Schansberg and Croson (1999) and Croson and Marks (2001) suggests that providing individuals with a suggested donation level increases individual contributions and the provision of public goods. Our chapter takes these models a step further by proposing that the success of the suggested contribution stems in part from a sense of moral responsibility, and that these moral motivations can be further activated through simple framing language.

Our work is also related to the theoretical work by Duncan (2004), in which individuals gain utility based on the impact their contribution makes on a social problem. This model differs from a model of warm glow in that individuals care not just about their contribution, but also about whether their contribution "makes a difference." Duncan's model differs from a traditional model of warm glow because in

[2] Andreoni (2006)

contrast to warm glow, individuals may see no change in their impact utility as their contribution increases if the provision of the public good does not increase as well. In contrast, for the kind of moral motivation we propose, individuals care about the relative morality of their gift choice and not about the outcomes produced. While we demonstrate that framing language may change the relative costs and benefits of adhering to a moral reference point, Duncan's model of impact philanthropy would not predict that framing language would have any affect on contributions.

The role of emotions which we focus on here has been examined in prior work. Kahneman and Knetsch (1992) find a strong relationship between the willingness to pay for public goods and independent reports of the moral satisfaction of contributions to those public goods. Biel et al. (2006) provide experimental and survey evidence that the discrepancy between willingness to pay (WTP) and willingness to accept (WTA) can be explained by greater shame from not contributing to a public good when asked in the context of WTA than WTP. Dal Bo and Dal Bo (2009) examine how behavior in cooperative games responds to different kinds of philosophical appeals with and without the ability to punish deviators. We examine whether moral emotions can be manipulated by a simple institutional mechanism. By isolating the effect of providing a suggested contribution from the additional impact of the moral framing language, we are able to narrowly identify the effect of moral framing.

5.2 Experimental Design

In order to test the hypothesis that giving is partially motivated by wanting to adhere to some moral standard we design a public goods experiment with three treatments. In our experimental treatments, we test a) the simple effect of establishing a suggested contribution and b) whether framing language can change individuals' response to the suggested contribution. Our experimental treatments are modeled after a common strategy used by nonprofits, the combination of a suggested contribution with language to "do your part." With our treatments we isolate the effect of the suggested contribution effect from the additional effect of the framing language.

5.2.1 Experimental Predictions

Employing the idea of morally motivated giving, we develop hypotheses regarding the average contributions and the share of individuals giving at the suggested contribution level in our experimental treatments. Our first prediction concerns comparative statics for each of these metrics when we introduce a suggested contribution. We predict that instituting a suggested contribution will have the following effects:

- Average contributions will increase when individuals are provided with a suggested contribution.
- The number of individuals who make the suggested contribution will be higher when there is a suggested contribution.

We next consider what will occur when a suggested contribution is presented with moral framing. When the suggested contribution is framed in terms of moral responsibility, we predict that:

- Average contributions will increase when the suggested contribution is morally framed.
- The number of individuals who make the suggested contribution will be higher when the suggested contribution is morally framed.

5.2.2 Parameters of Public Good Experiment

The contribution decision made by individuals takes place in a simple threshold public goods game. Individuals had private values for the public good, which we will represent by v_i. In the experiment, individual values were uniformly and independently distributed over the range $[150, 210]$. Individuals were a member of a group of size 3, 6 or 12. Individuals received their private value only if the threshold contribution level was reached by the group and the public good was produced. If the threshold contribution level was not reached, individual contributions were not refunded. In each period of the experiment, every individual received an endowment of $\frac{\sum v_i}{N}$ where N was the number of people in the group. Individuals chose how much of their endowment to contribute. The threshold of contributions required for the public good to be provided was $0.6 \sum v_i$. Since the threshold is below the sum of values, provision of the public good is socially efficient. Subjects were told that endowments, thresholds and values were drawn from a fixed distribution. The full instructions are provided in the appendix.

In the Suggested Contribution (SC) and Moral Frame (MF) treatments, we generated a suggested contribution amount that was a percentage of individuals' private values from providing the public good. The suggested contribution was 70 percent of an individual's value (v_i) in that round. It was not a symmetric equilibrium strategy for all individuals to make a contribution at the suggested level. If individuals were to all provide contributions at the suggested level, the total contributions for the group would have reached $0.7 \sum v_i$. At this level of contributions, it would have been individually rational for subjects to have reduced their contribution. An individual could have reduced their contribution by up to $0.1 \sum v_i$ and the public good would still have been provided. Therefore it is clear that contributing at the suggested contribution level cannot be an equilibrium strategy if strategies are symmetric.

5.2.3 Information

The distribution of individual values in the experiment was common knowledge. Individuals were told that endowments and thresholds were drawn from a fixed distribution. In each period, individuals were told their endowment, their group's threshold, their value from providing the public good and the number of people in their group.[3]

5.2.4 Framing Language

In addition to information related to the public goods game, individuals in some treatments were provided with a suggested contribution. In order to determine whether framing language was able to change individual reaction to a suggested contribution we designed three treatments.

Control (C) In the control group, individuals were not provided with any suggested contribution.

Suggested Contribution (SC) In the suggested contribution treatment, individuals were provided with the suggested contribution which was called a "suggested investment."

Moral Frame (MF) In the moral frame treatment, individuals were provided with the suggested contribution which they are told was the "Amount required to DO YOUR PART."

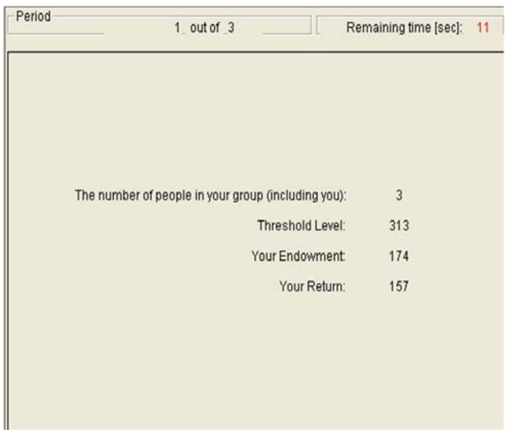
Figure 5.2 shows the screen seen by subjects in all three treatments.

5.2.5 Implementation

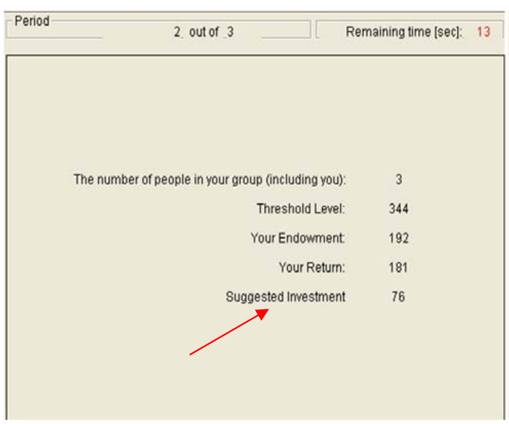
Individuals played 30 rounds of the public goods game. They received no feedback about the outcome in each period. Only one of the periods was randomly selected for

[3]After each round of the experiment, subjects answered a question intended to measure satisfaction. Subjects were asked "On a scale of 1-10 with 1 being the least satisfied and 10 being the most satisfied, how satisfied would you be if this is the round that gets implemented?" after their decision in each round. Because subjects received no feedback about the outcome of the game in each round, this self-reported satisfaction depended on their own choice only and not on the outcome of the game.

A



B



C

Figure 5.2. Screenshots used in the different experimental conditions. Key: (A) Control treatment. (B) Suggested Contribution. (C) Moral Frame.

payment. Subjects were not informed which round would be implemented until the end of the experiment. Subjects received 10 cents for every point they received in the experiment. The first 10 rounds were played with 3 person groups; the next 10 rounds were played with 6 person groups and the final 10 rounds were played with 12 person groups. In each experimental session there were 12 individuals. The experiments were run at UCLA in 16 sessions with a total of 192 subjects. The implementation of the sessions is summarized in table 5.1. Total payments (including a $5 show-up fee) averaged $13 and ranged from a minimum of $1 to a maximum of $44.

Table 5.1. Experimental sessions

Treatment	# of Sessions	# of Subjects
Control	5	60
Suggested Contribution	6	72
Moral Frame	5	60

5.3 Experimental Results

We consider the following results from our experiment: the effect of the treatments on individual giving and the treatments' effects on public good provision and efficiency for the group.

5.3.1 Treatment Effects on Individual Giving

To consider the effect of the treatments on individual contributions, we consider a measure of giving as the share of the suggested contribution. Figure **??**A illustrates the distribution of contributions across the three treatments. As predicted, we see a significant increase in the share of individuals giving the suggested value comparing the Control treatment and the Suggested Contribution treatment. Similarly, we see a further increase in the share of individuals giving the suggested contribution from the Suggested Contribution and Moral Frame treatments. In addition, the share of individuals making no contributions is more than twice as high in the Control treatment compared with the Suggested Contribution or Moral Frame treatments.

We then examine the average amount given by treatment (again considering the share of the suggestion contributed) in figure **??**C. Average giving in the Moral Frame treatment, 91% of suggestion, is statistically significantly higher than the average for the suggested contribution treatment, 88% of suggestion (the comparison of means t-test has a p-value=0.01). Both the Moral Frame treatment and the suggested contribution treatment are significant improvements on average contribution in the control group average of 73% of the suggestion (both t-test comparisons have p-values< 0.01). Therefore, it would appear that a large share of the increase in contribution based on the strategy of providing a morally framed contribution level comes from simply establishing the moral reference point. Nonetheless, framing the decision with explicitly moral language does provide additional increases in contributions.

In order to further examine individual decisions about whether to contribute, we consider four ranges of relevant behavior: the decision not to contribute, the decision to contribute less than the suggestion, contributions at the suggestion level and contributions over the suggested level. Specifically, we consider the average share of individuals who fall into the following definitions: those who gave between 0% and 10% of the suggested amount (Gave Nothing), those who gave between 10% and 90% of the suggested amount (Gave Some), those who gave between 90% and 110% of the suggested amount (Gave Suggestion) and those who gave more than 110% of the suggested amount (Gave More). Figure **??**B illustrates that the share of individuals giving the suggested amount (Gave Suggestion) is significant higher in the Suggested Contribution than the Control treatment (p-value for the t-test is < 0.01). Furthermore, there is a significant increase in the share giving the suggested amount with the moral framing treatment in the MF treatment (p-value for the t-test

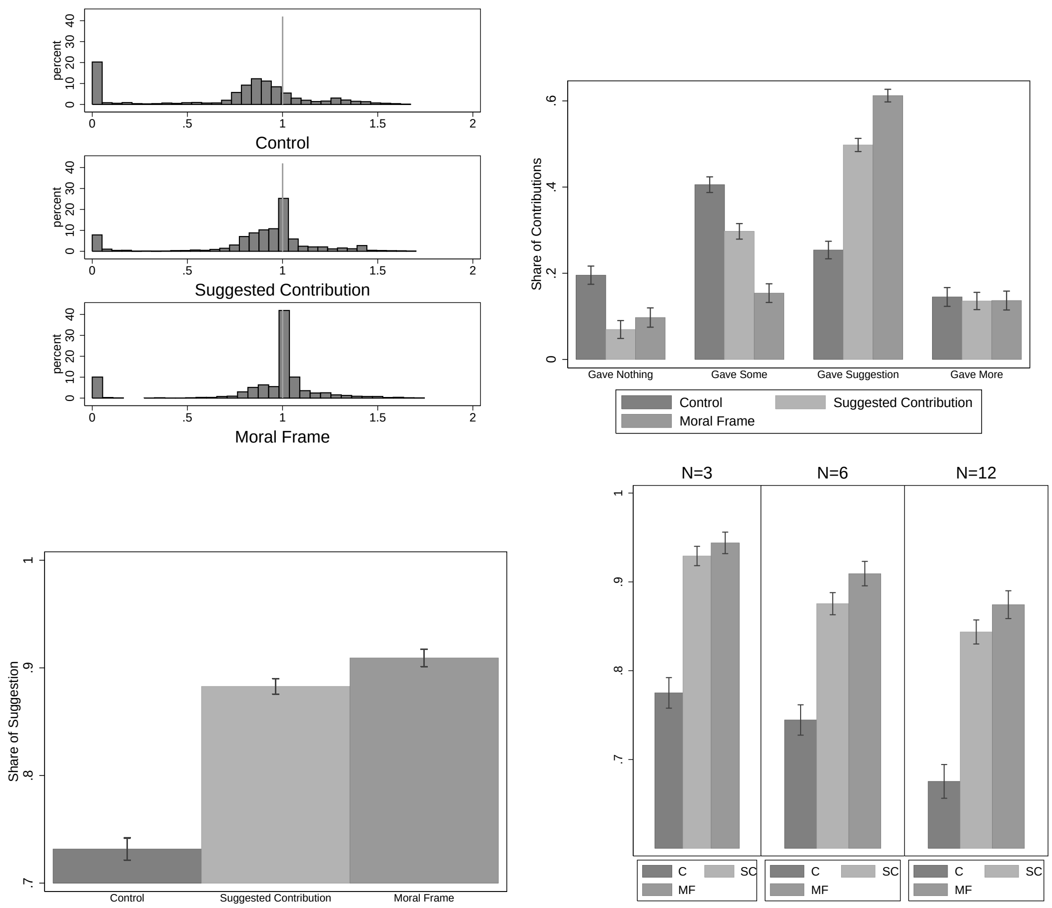


Figure 5.3. Treatment effects on individual giving

Key: (A) Histogram of donations by condition measured as the share of the suggestion contributed. (B) Fraction of individuals who gave less than the 10% of the suggested amount, 10%-90% of the suggested amount, 90%-110% of the suggested amount, or more than 110% of the suggested amount. (C) Average contributions by treatment. (D) Average contributions by group size. Error bars denote SEMs.

is < 0.01).

Figure **??**B also shows that individuals in the Moral Frame treatment were less likely to give some but not enough to meet the suggestion, (p-value for the t-test is < 0.01), suggesting that the Moral Frame treatment has better efficiency properties since individuals were the least likely to make contributions that would not be enough for the group to reach the threshold on average. While the Moral Frame treatment provides the greatest improvement in eliminating inefficient contributions that will not allow the group to reach the threshold, the Suggested Contribution treatment also provides improvement in the share of individuals giving less than the suggestion, the average of those who Gave Some in the Suggested Contribution treatment is significantly less than in the Control group (p-value for the t-test is < 0.01). There is also a significant decrease in the average share of individuals giving nothing in both Suggested Contribution treatment and Moral Frame treatment (p-values for both t-test are < 0.01). The Moral Frame treatment's framing language significantly increases the share of individuals who contribute toward the group meeting the goal but does not make individuals less likely to free ride and give nothing, as we can see by observing that there is no significant difference between the Suggested Contribution and Moral Frame treatments in the share of individuals in each treatment who gave zero.

In figure **??**D we examine how contributions change as the group size increases. The contributions to the public good drop off much more rapidly in the Control group than in either the Suggested Contribution or the Moral Frame treatments. While contributions in the control group drop from 74.4% of the suggestion for a group with 6 people to 67.5% of the suggestion for a group with 12 people (p-value for the t-test is < 0.01) , contributions drop from 87.5% to 84.4% of the suggestion (p-value for the t-test is 0.08) for the Suggested Contribution treatment and from 90.9% to 87.4% of the suggestion (p-value for the t-test is 0.09) for the Moral frame treatment when moving from a group size of 6 to a group size of 12. The Suggested Contribution and Moral Frame treatments help maintain contributions to the public good, even as group size increases.

5.3.2 Treatment Effects on Group Outcomes

Not only does the Moral Frame treatment have an impact on individual giving, it also has an impact on the ability of the group to provide the public good. Figure **??**A illustrates the share of times a group successfully reaches the threshold needed to provide the public good in each of the three treatments. The public good is provided 72% of the time in the Moral Frame treatment and 64% of the time in the Suggested Contribution treatment (a comparison of mean t-test has p-value 0.01). In the Control group, the public good is provided by only 29% of the groups which is significantly lower than when both Suggested Contribution and Moral Frame treatments (both comparison of mean t-tests have p-values < 0.01). Therefore, while a significant share of the improvement from a morally framed suggested contribution comes from merely providing a suggested contribution level, additional improvements in the ability to provide the public good are possible simple from framing the suggested contribution with moral language.

Figure **??**B illustrates that the ability of the group to provide the public good as group size increases drops off much more dramatically in the control group than in either Suggested Contribution treatment or Moral Frame treatments. The Moral Frame treatment does not statistically significantly improve the ability of the group to provide the public good above the Suggested Contribution treatment when the group size is small (the p-value for a t-test comparison of means for 3-person groups is 0.56). However, with groups of 12, with the Moral Frame treatment the public good continues to be provided 58% of the time while in the Suggested Contribution treatment it is provided only 38% of the time (a comparison of means t-test yields a

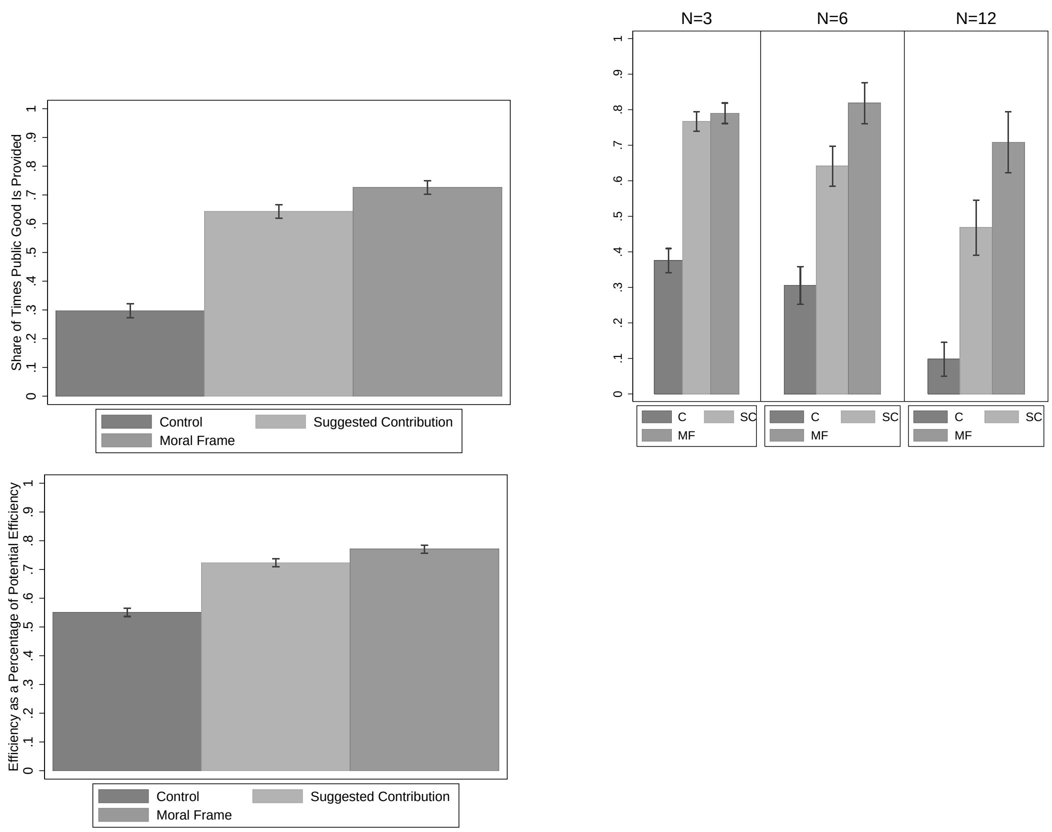


Figure 5.4. Treatment effects on group outcomes

Key: (A) Frequency with which the public good is provided by treatment. (B) Frequency with which the public good is provided by group size. (C) Fraction of potential efficiency achieved by treatment. Error bars denote SEMs.

p-value=0.04).

While provision of the public good drops from 25% of the suggestion for a group with 6 people to 8% of the suggestion for a group with 12 people (p-value for the t-test is 0.01) in the Control treatment, provision of the public good drops from 53% to 38% of the suggestion (p-value for the t-test is 0.07) for the Suggested Contribution treatment and from 67% to 58% of the suggestion (p-value for the t-test is 0.28) for the Moral Frame treatment when moving from a group size of 6 to a group size of 12. The Suggested Contribution and Moral Frame treatments help maintain provision of the public good, even as group size increases when compared to the Control Treatment. Furthermore, many of the improvements in the ability to provide the public good that are achieved with moral framing occur in larger groups, where the impact of the suggested contribution appears to weaken more quickly in the absence of morally framed language.

In figure **??**C we consider the sum of earnings in the group in each round as a percentage of the potential efficiency. We define potential efficiency as the sum of endowments plus the sum of value received from providing the public good minus the total amount of contribution needed to reach the threshold for the public good to be provided. The efficiency of the group's outcomes is 77% of the potential efficiency in the Moral Frame treatment; compared to 72% of the potential efficiency in the Suggested Contribution treatment (a comparison of mean t-test has p-value 0.02). In the control group, the efficiency is 55% of the potential which is significantly lower than when both Suggested Contribution and Moral Frame treatments (both comparison of mean t-tests have p-values < 0.01). Consistent with earlier evidence, a large share of the success of the strategy of providing a morally framed suggested contribution comes from introducing the suggested level, but additional gains are possible when language is framed explicitly in terms of moral responsibility.

5.3.3 Discussion

We consider the possibility that one determinant of individual charitable action comes from emotional feelings triggered by a sense of moral responsibility. We consider a common two-pronged approach by nonprofits that appears to rely on this insight about emotional responses to a sense of moral responsibility. Charities often combine a suggested contribution level with language that frames the contribution in the light of moral responsibility. Our experimental design allows us to separate the simple effect of providing a suggested contribution level from the additional impact of the framing language which emphasizes the moral responsibility behind making the suggested contribution.

We find that providing a suggested contribution both increases the average donation and increases the share of individuals giving the suggested value, as predicted by our hypotheses about morally motivated giving. Furthermore, we find additional, though smaller increases in contribution levels and the share of individuals giving the suggested values when the suggested contribution is framed in terms of moral responsibility. We conclude that a large part of triggering this emotional response is due to the simple act of setting a suggested contribution level. Nonetheless, simple moral framing language can intensify this reaction and bring further increases in donations. Finally, we see that improvements from the Moral Frame treatment act primarily to move individuals from the inefficient outcome of donating some but not enough to meet the group's threshold to giving the suggested value. Individuals are just as likely to give nothing in the Moral Frame treatment as the Suggested Contribution treatment. This suggests that one of the important determinants of moral motivations is the distance from the moral reference point. Individuals appear to display some asymmetry in their preferences with respect to giving above the moral reference point than they do to giving below the moral reference point. Evidence of asymmetry with respect to positively and negatively framed contributions would be consistent with evidence presented by Andreoni (1995) of a distinction between warm glow and cold prickle.

The success of appeals to moral emotions cannot be well explained by other theories. Since neither the suggested contribution nor the moral framing in our experiment have any impact on the choice set, they can only affect the selection among among multiple equilibria. In this experiment, it is a dominant strategy not to contribute the suggested value; therefore neither the suggested value nor the moral frame can be argued to act as merely coordination devices.

Furthermore, our evidence cannot be explained by models of warm glow. Appealing to a sense of morality is distinct from the notion of an egotistical warm glow derived from the sense of satisfaction from being generous. The introduction of a suggested contribution should have no impact on warm glow, since models of warm glow generally consider warm glow to increase in the actual contribution, regardless of institutions (such as a suggested contribution or language morally framing the problem). Nonetheless, if giving is motivated by emotional responses to a sense of moral responsibility, this does not imply that all giving can be explained by moral motives. In fact, giving may be motivated by *both* warm glow and a sense of moral responsibility.

Lastly, our results cannot be explained by existing theories on the social signaling motives for giving or models of social preferences. The moral framing was independent of any variation in the worthiness of recipients, opportunities for social signaling or sense of fairness as suggested by recent theories of more subtle motivation for charitable giving.[4]

One open question for research is how important these kind of moral motives are in predicting contributions when compared to neoclassical motives, altruism and

[4]Fong and Luttmer (2009), Benabou and Tirole (2006), Fehr and Gächter (2002)

warm-glow considerations. Understanding the importance of moral motivations for giving will depend on a better understanding of where a moral reference point comes from and whether individuals start with some moral reference point for contributing to charities, even in the absence of a suggested contribution level.

Gaining insight into how these moral motives are activated is also a crucial component of understanding the role they play in explaining charitable giving. As in the example provided in the introduction, where individuals are contacted with a morally framed request by two different and similar organizations, it could be that a moral frame is successful only in partial equilibrium but if it were to be adopted by all organizations it would no longer be successful. In the presence of multiple appeals to moral responsibility, their effect could be diminished. Similarly, it could be that individuals differ in their prior tendency to be morally motivated based on their beliefs. Moral framing might be expected to be successful only if individuals have a sense that a charitable cause is morally worthy and might not be moved by a cause they do not believe in.

Another possible area of research is whether moral motives are determine by some kind of social norm. For instance, it could be that individuals derive guilt or pride from their adherence to a moral reference point. If so, it could be that this kind of motivation could lead to interesting multiple equilibria. As suggested by Bernheim and Rangel's (2007, 2009) model of behavioral welfare economics, it is not clear whether this kind of moral framing represents a a welfare maximizing treatment, since individuals might feel manipulated ex post and the emotion response to moral framing may correspond to the manipulation of emotions instead of an improvement in well-being.

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