ESSAYS ON SOCIAL PREFERENCES

A Dissertation

by

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ABSTRACT

This dissertation includes three essays in the fields of public and behavioral economics with a special focus on social preferences using both lab and field experiments. The first essay investigates the impact that information about the value of a public good has on voluntary contributions. It is costly for organizations to provide detailed information about their projects. Thus, organizations would ideally like to spend their resources on information provision only if it would help increase the contributions. We find that the impact of information depends on the generosity level of the population. While providing more information increases average contributions in a relatively less generous donor population, it actually hurts contributions in a relatively more generous population. Thus, these findings suggest targeting information provision towards less generous donor groups.

The second essays studies the impact that scarcity of resources has on cheating and in-group favoritism using a two-stage lab-in-the-field experiment with low-income coffee farmers in a small, isolated village in Guatemala. Using the distinctive variance in income that comes from seasonal coffee harvesting, we first conducted our experiment before the harvest (Scarcity period) and then during the harvest season (Abundance period). First, we find that subjects cheat at high levels in both periods when they are the beneficiary of the cheating. Scarcity does not impact this cheating behavior. Secondly, we find significant in-group favoritism towards fellow villagers for cheating in the Abundance period, which disappears during the Scarcity period. Finally, using a dictator game, we show that this finding holds even when the cost of favoring an in-group member is monetary.

The last essay studies whether workers exert more effort when they work for a mission-oriented job using a modified gift-exchange experiment. We find that workers exert more effort when they work for a non-profit organization rather than a for-profit one, but only for high wages. Thus, higher wages generate significantly higher profits in the non-profit firm compared to the for-profit firm. We contribute to the literature by studying how intrinsic motivations may impact effort choices in the workplace.

DEDICATION

To my parents, Emel and Sahin Aksoy

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NOMENCLATURE

FGF	Fischbacher et al., (2001)
MPCR	Marginal Per Capita Return
NE	Nash Equilibrium
OLS	Ordinary Least Squares
ORSEE	Online Recruitment System for Economic Experiments
Q	Quetzales

TABLE OF CONTENTS

Page

LIST OF FIGURES

FIGURE Page

LIST OF TABLES

1. GENERAL INTRODUCTION

This dissertation includes three essays in the fields of public economics and behavioral economics with a special focus on social preferences using both lab and field experiments.

Most donors make contributions to public goods without doing any research. Organizations that aim to increase cooperation can encourage more informed giving by providing more and detailed information. However, information provision is costly and organizations have limited resources. So, it is important to consider the benefits of such provision. The first essay investigates the capacity of information to increase public good contributions. We examine the impact of information provision on voluntary contributions to a linear public good with an uncertain individual benefit (i.e. uncertain marginal per-capita return (MPCR)). Uninformed subjects make contribution decisions based only on the expected MPCR (i.e. the prior distribution), while informed subjects observe the realized MPCR before contributing. Using a theoretical model of other-regarding preferences, we find that the impact of information on average contributions crucially depends on the generosity level of the population, modeled as a stochastic increase in the pro-social preferences. In particular, a less generous population substantially increases contributions in response to good news of higher than expected MPCR and reduces contributions relatively little in response to bad news of lower than expected MPCR. Thus, the overall impact of information is to increase average contributions when the population is less generous. The opposite is true for a more generous population. We test these theoretical predictions using a two-stage lab experiment. First, we measure subjects' levels of generosity in the public good game using an online experiment. Then using the data collected in the online experiment, we control for the level of generosity in the lab. Our findings are in line with the theoretical predictions, and suggest that a more targeted information provision may be a successful strategy to improve contributions to public goods.

The second essay studies the impact of scarcity on cheating and in-group favoritism using a two-stage lab-in-the-field experiment with low-income coffee farmers in a small, isolated village in Guatemala. During the coffee harvesting months, farmers in this village experience a significant income boost from selling their coffee beans. However, during the non-harvesting months, they experience a substantial decline in income, inducing a pronounced state of scarcity, while other factors remain similar. Using this variance in income, we first conducted our experiment before the coffee harvest (*Scarcity*), then repeated the experiment with the same group of subjects during the harvest season (*Abundance*). First, using the Fischbacher and Föllmi-Heusi (2013) die-roll paradigm, we find that subjects cheat at high levels in both periods when they are the beneficiaries of the cheating. Scarcity does not impact this cheating behavior. Secondly, using subjects' natural village identity, we find significant in-group favoritism for cheating in the Abundance period, which disappears during the Scarcity period. Finally, using a dictator game, we show that this finding holds when the cost of favoring an in-group member is monetary rather than moral.

When workers decide on the effort level to exert, they take many factors into account, including not only extrinsic factors, such as the salary, but also the type of work and the mission of the organization. In the third essay, we study whether workers exert more effort when they work for a mission-oriented job using a modified gift-exchange experiment. In our experiment, there are workers, managers and firm owners. Managers decide how much to pay to their workers, and observing this, workers decide how much effort to provide. These decisions determine the profits created for the firm. Firm owners receive a share of the profits along with the managers. There are two treatments: profit and non-profit. The difference between these treatments is the identity of the firm owner. In the profit treatment, the firm owner is another student in the lab who has been randomly selected to be a firm owner and does not make any decisions but collects their share of the profits. In the non-profit treatment, the firm owner is a non-profit organization. At the end of this treatment, the accumulated earnings for the non-profit organization are donated online. While we find that managers' behavior across the two treatments is similar, workers exert more effort in the non-profit treatment when the wage paid is high. This results in more profits being generated in the non-profit treatment at high wage levels. We contribute to the literature by studying how other motivations, such as altruism, may greatly impact effort choices in the workplace, particularly when the job involves *doing good*.

2. WHEN DOES LESS INFORMATION TRANSLATE INTO MORE GIVING TO PUBLIC GOODS?

2.1 Introduction

Private voluntary contributions have been increasingly viewed as a vital source of funding for public goods. For example, DonorsChoose, a fundraising platform for public school projects, has quickly gained popularity since its inception in 2000 and has raised close to $640 million up-to-date.[1,2] Other crowdfunding platforms that fundraise for public projects include Public Good[3], Razoo[4], and Pledge Music[5]. Interestingly, while the non-profit sector is growing, with the number of non-profits surpassing 1.5 million, recent evidence suggests that individual donors are often poorly informed when making contributions. According to 2015 Camber Collective survey about private charitable giving in the U.S., "49% of donors don't know how nonprofits use their money".[6] Such lack of information may have a significant effect on contributions if donors care about the impact of their giving. Lab experiments find that this is indeed the case with subjects contributing higher amounts to more valuable projects (see Ledyard et al., 1995 and Cooper and Kagel, 2016). This suggests that donors would respond to more information by increasing contributions upon finding out good news of higher than expected value of the public project and decrease contributions upon observing bad news of lower than expected value. Thus, the overall impact of more information on expected giving depends crucially on the relative response to good and bad news.

In this paper, we investigate theoretically and experimentally the impact of more information on total contributions in the context of a linear public good game with an uncertain return. We restrict our attention to public goods whose provision is always desirable from a social standpoint but free-

[1] For more information, visit https://www.donorschoose.org/about.

[2] According to Charity Navigator, the overall contributions to education related causes in the US amounted to $59.77 billion in 2016. For more information, see https://www.charitynavigator.org/index.cfm?bay=content.view&cpid=42.

[3] www.publicgood.com

[4] www.razoo.com

[5] https://www.pledgemusic.com/

[6] See http://www.cambercollective.com/moneyforgood/

riding incentives are present at the individual level. The public good provided increases linearly with total contributions, and the magnitude of this increase depends on the marginal per-capita return (MPCR) of the public good. To determine the impact of information about the MPCR, we consider two information environments corresponding to informed and uninformed populations. With an uninformed population, subjects do not know the realized value of the MPCR, but only know its prior distribution when making a contribution decision. With an informed population, subjects observe the realized MPCR prior to contributing. This allows us to compare uninformed and informed giving by studying how subjects respond to good and bad news about the MPCR.

On the theory side, the linear structure of the public good implies that for any value of the MPCR, it is socially optimal to contribute all of the endowment, while it is individually payoff maximizing to contribute nothing. Since lab experiments reveal that most of the contributions are in-between the two extremes (Ledyard et al., 1995; Cooper and Kagel, 2016), we incorporate other-regarding preferences into the agents' utility function in spirit of Arifovic and Ledyard (2012). In particular, agents are assumed to have pro-social motivations for giving, captured by agents' preference for higher average contributions to the public good. We refer to agents with stronger pro-social preferences as more generous since they have stronger propensity to contribute. In addition, agents exhibit fairness concerns, which are captured by a dis-utility from contributing a higher amount than the average contributions by others. In equilibrium, contributions increase with the MPCR and the generosity level of the agent.

Interestingly, we find that the impact of information on expected contributions crucially depends on the generosity level of the agent population, modeled as a stochastic increase in the pro-social preferences. While information has the potential of increasing average contributions for a less generous population, it may in fact reduce average contributions when the population is more generous. The reason for this is in the differential response to good and bad news in the two population types. For both types, the equilibrium contributions decrease upon observing bad news of lower than expected MPCR and increase upon observing good news of higher than expected MPCR. Moreover, the equilibrium contributions feature increasing returns to MPCR when the MPCR is low (i.e. contributions are a convex function of the MPCR for low values) and diminishing returns when the MPCR is high (i.e. contributions are a concave function of the MPCR for high values). This is because at low MPCR, an increase in the marginal return induces a large number of agents to contribute, generating a substantial increase in overall giving. In contrast, at high MPCR, a further increase induces a relatively small response since most agents are already contributing large amounts and thus are less willing to further increase their giving. However, a more generous population reaches diminishing returns faster since most of the agents are giving significant amounts even at lower values of the MPCR. As a result, a more generous population is less responsive to good news and more responsive to bad news and thus information has an overall negative effect on expected contributions. The opposite is true for a less generous population, which features increasing returns for a wider range of the MPCR and thus is more responsive to good news than bad news.

The novel findings of our model give rise to testable hypotheses, which we experimentally investigate in the lab. Since our theoretical model suggests that the generosity level of the population plays a vital role in how donors respond to information, a defining feature of our experimental design is controlling for the generosity level of the sessions. We accomplish this by running our experiment in two stages. First, we conduct an online experiment to elicit subjects' generosity levels in the public good game prior to the lab experiment. Using this data, we create more and less generous groups in the lab, and inform the subjects about the generosity level of their session by using a neutral language. Subjects play a linear public good game in groups of three with uncertain MPCR (either high (0.60) or low (0.40) with equal probability). There are two information treatments. In the informed treatment, subjects know the randomly chosen MPCR before they make their contribution decisions. In the uninformed treatment, they are only informed about the distribution of the MPCR, and asked to make their contributions without knowing which MPCR is chosen.

The experimental findings are in line with the theoretical predictions. In the sessions with more generous subjects, average contributions in the uninformed treatment are significantly higher than the ones in the informed treatment. Subjects' contribution level in the uninformed treatment is closer to the contribution level in the informed treatment under good news (MPCR of 0.60) than under bad news (MPCR of 0.40). Thus on average, information reduces contributions to the public good in the relatively more generous sessions. The opposite is true for the less generous sessions. Uninformed contributions to the public good are closer to the informed contributions under bad news than under good news. Thus, information is good for giving in the relatively less generous sessions.

The findings of this study have significant implications for fundraising. They suggest that targeted information provision may be a successful strategy that improves contributions to public goods. In particular, the model and experimental results reveal that less generous donors are more responsive to good news about the returns to public goods. Thus, focusing on better informing these donors, who are often overlooked in fundraising campaigns, may be a more fruitful strategy than uniform information provision.

2.2 Related Literature

This paper connects two research strands that investigate factors that impact public good provision and cooperation: 1) information, 2) social preference composition of groups. In the following section, we briefly review the related literature.

2.2.1 Information

Much of the earlier literature on public good provision assumes that donors operate under complete and perfect information (Ledyard et al., 1995; Andreoni and Payne, 2013; Vesterlund, 2016). In reality, however, information is often limited, which has given rise to a more recent trend of studying public good provision under incomplete and imperfect information.

On the theoretical front, there is sparse literature that studies public good provision under incomplete information about the public good's value. In particular, in the context of discrete public goods, Menezes et al. (2001), Laussel and Palfrey (2003), and Barbieri and Malueg (2008, 2010) introduce private information about donors' heterogeneous valuations of the public good, while Krasteva and Yildirim (2013) endogenize the choice of information acquisition and find that more information about one's own value improves giving. In contrast, our current setting features a public good with homogeneous returns and finds that more information about the return is not always beneficial.

Our paper is closer to the literature on continuous public goods under incomplete information, which has modeled the public good as having uncertain (but homogeneous) returns (e.g. Vesterlund, 2003; Andreoni, 2006; Lange et al., 2017). This literature, however, has mainly focused on the information transmission about the quality of the public good to uninformed donors via leadership giving (Vesterlund, 2003; Andreoni, 2006) [7] or costly gift provision to donors Lange et al. (2017). Instead, our focus here is on studying the impact of more information on average total provision.

Our model and experimental set-up is cast as a continuous linear public good with uncertain MPCR. In this respect, our paper is closest to the experimental literature that considers limited information about the returns. Although some of this literature focuses on information about others' valuation and/or endowment by incorporating heterogeneity in a non-linear public good environment (e.g. Marks and Croson, 1999; Chan et al., 1999), most of the focus has been on the impact of *uncertainty* about the MPCR. In particular, Gangadharan and Nemes (2009), Levati et al. (2009), Fischbacher et al. (2014), Stoddard et al. (2015), Boulu-Reshef et al. (2017), Butera and List (2017) and Théroude and Zylbersztejn (2017) study how increasing the *riskiness* of the returns, in terms of mean preserving spread, affects contributions. Although the findings are mixed, Levati and Morone (2013) and Stoddard (2017) show that the parameterization of the public good game can play an important role in determining the direction of this effect.

In contrast, we are interested in the impact of *information* about the MPCR on contributions. Because of that, we keep the distribution of the MPCR fixed and vary the amount of information that people receive, which more closely represents people's response to information. To the best of our knowledge, our paper is the first to investigate public good contributions in this environment.

[7] Potters et al. (2005, 2007) experimentally investigate the information revelation through leadership giving.

It is worth highlighting that our work is also related to an emerging literature studying the role of information on charitable giving. Most of this literature studies the impact of variety of information (such as cost-to-donation ratio, recipients' or non-profits' characteristics, or other donors' giving and so on) on donations (e.g. Eckel et al., 2007; Shang and Croson, 2009; Fong and Oberholzer-Gee, 2011; Null, 2011; Karlan and Wood, 2014; Exley, 2015, 2017; Metzger and Günther, 2015; Brown et al., 2017; Butera and Horn, 2017; Portillo and Stinn, 2018). In many of these studies, however, donors' beliefs in the absence of information are unobservable and outside the experimenter's control. In reality, donors may adopt different beliefs about non-profits' characteristics. Some may hold very optimistic beliefs, while others may hold very pessimistic beliefs in absence of sufficient information. Thus, donors' response to information is ambiguous and heavily influenced by their prior beliefs. Without means of controlling for these beliefs, it is difficult to gain a deeper insight into the channels through which information impacts giving. Indeed, the findings of the existing studies are mixed, with donors sometimes using information to tailor their donations up or down.

To gain more insight into the impact of information on donors' giving, we control both for the information that donors receive and the interpretation of this information by donors. To accomplish this, we use the linear public good game, in which subjects are assigned their valuations for the public good by the experimenter and compensated based on these assigned values. By varying people's information about their induced values (Smith, 1976), we are able to determine how they respond to information about the value and how the informed contributions compare to the uninformed contributions.

Finally, there is also charitable giving research studying how donors may strategically create a "moral wiggle room" (Dana et al., 2007) to justify selfish behavior. For example, research shows how donors use risk (Exley, 2015), ambiguity (Haisley and Weber, 2010), beliefs about others (Di Tella et al., 2015), and performance metrics (Exley, 2017) as an excuse not to give. Unlike our public goods framework, most of these studies use a dictator game type of environment where subjects are given an endowment and asked to make a donation. In this respect, our study is more representative of cooperation rather than altruism. Additionally, in our study subjects are either *exogenously* informed or uninformed depending on the treatment. Thus, information avoidance as an excuse not to give is not a viable explanation for our findings. Granted, it is plausible that subjects in the uninformed treatment could use the lack of information as an excuse not to give despite knowing that each MPCR is equally likely. Although this could provide an alternative explanation for our findings in the less generous sessions, it fails to explain the behavior observed in more generous sessions.[8]

2.2.2 Social Preferences Composition of Groups

The second strand of related literature studies social preferences (i.e. other-regarding preferences) for giving. This literature has established that people have different motivations for giving and they can be classified into different types based on these motivations (see the following surveys: Camerer, 2011; Fehr and Schmidt, 2006; Cooper and Kagel, 2016). While some people are selfish and do not give anything, others are conditional cooperators whose contributions depend on what others give (e.g. Brandts and Schram, 2001; Fischbacher et al., 2001; Kurzban and Houser, 2005).

Groups consist of individuals with different social preferences (i.e. types). The existing literature mainly focuses on how group composition changes the level of cooperation and finds that the composition of social preference types in groups matters in achieving and maintaining high levels of cooperation (e.g. Burlando and Guala, 2005; de Oliveira et al., 2015; Gächter and Thöni, 2005; Page et al., 2005; Gächter, 2006; Gunnthorsdottir et al., 2007; Ones and Putterman, 2007). One common finding in this literature is that contributions are higher in homogeneous groups with members who are more generous. Moreover, the existence of one selfish person in the group is enough to harm the groups' ability to cooperate (de Oliveira et al., 2015).

Our theoretical model suggests that people's reaction to information depends on the level of generosity of their group (more in Section 2.3). We contribute to this research strand by studying

[8] If moral wiggle room is an explanation for our findings, it is not clear to us why it may yield different results across treatments. Potentially, it is possible that information may be changing the *social norms* differently in more and less generous sessions. Since this is beyond the scope of this paper, we leave this to future research.

the impact of information across two groups with different levels of generosity. Finally, it is important to note that our findings about the impact of group composition on response to information might also explain the mixed results regarding the impact of information on giving in the charitable giving literature.

2.3 Theory and Hypotheses

The linear public good environment consists of groups of $N \geq 2$ agents. Each agent i is endowed with wealth W and chooses an amount g_i to allocate to a public good that benefits everyone equally in their groups. The monetary payoff of agent i is

$$M_i = W - g_i + v \sum_{k=1}^{N} g_k$$

where $v \in (\frac{1}{N}, 1)$ denotes the marginal per-capita return (MPCR) of the public good. Clearly, the payoff maximizing strategy is $g_i = 0$ and the socially optimal strategy is $g_i = W$. Therefore, in absence of other-regarding preferences all agents contribute zero in the unique Nash equilibrium.

Since the above equilibrium behavior is a drastic departure from the experimental evidence (see Ledyard et al., 1995), the existing literature has considered the possibility of other-regarding preferences (e.g. Rabin, 1993; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Falk and Fischbacher, 2006; Arifovic and Ledyard, 2012). In particular, following the model of inequality aversion by Arifovic and Ledyard (2012)[9], agent i's utility function is given by

$$u_i(M_i, \overline{M}) = M_i + \beta_i \overline{M} - \gamma_i \max\{\overline{M} - M_i, 0\} \tag{2.1}$$

where $\overline{M} = \frac{1}{N} \sum_{k=1}^{N} M_k$ is the average earnings in the game. The agent-specific parameter β_i captures the agent's preference for higher average earnings. In other words, β_i is the strength

[9]We adopt the preference specification proposed by Arifovic and Ledyard (2012) since it most closely fits our public good framework. However, the utility function given by eq. (2.1) is closely related to alternative preference specifications proposed by the existing literature (e.g. Fehr and Schmidt (1999); Bolton and Ockenfels (2000); Charness and Rabin (2002)), with utility representations that are equivalent up to linear transformations of one another. For further discussion of this equivalence, see Arifovic and Ledyard (2012).

of i's pro-social motives for giving which we refer to as the individual i's generosity level[10]. The parameter γ_i captures her inequality aversion, which generates disutility if i's earnings fall below the average.

Letting $\overline{g}(v)$ denote the expected average giving in the public good game, i's best response function is given by

$$g_i = \begin{cases} 0 & \text{if} \quad \beta_i \leq \beta_1(v) \\ \overline{g}(v) & \text{if} \quad \beta_i \in [\beta_1(v), \beta_2(v, \gamma_i)] \\ W & \text{if} \quad \beta_i \geq \beta_2(v, \gamma_i) \end{cases} \tag{2.2}$$

where

$$\beta_1(v) = \frac{N(1-v)}{Nv-1}, \beta_2(v, \gamma_i) = \frac{N(1-v)}{Nv-1} + \gamma_i \frac{N-1}{Nv-1}.$$

The best response function reveals that selfish agents (low β_i) give 0, highly generous agents (high β_i) give all their endowment, and moderately generous agents (intermediate β_i) are conditional cooperators and match the expected average contributions by others.

Given this best response function, the expected equilibrium giving solves

$$\overline{g}(v) = \Pr(\beta_i \geq \beta_2(v, \gamma_i))W + \Pr(\beta \in [\beta_1(v), \beta_2(v, \gamma_i)])\overline{g}(v), \tag{2.3}$$

where the total expected giving is simply the weighted average giving of the highly generous agents, who give all their endowment, and the moderately generous agents, who match the expected average giving in the population. Rearranging terms, we can re-write eq. (2.3) as

$$\overline{g}(v) = \frac{1}{1 + \frac{\Pr(\beta \leq \beta_1(v))}{\Pr(\beta \geq \beta_2(v, \gamma_i))}} W \tag{2.4}$$

Thus, the expected equilibrium giving depends on the relative likelihood of the payoff maximizing (selfish) giving and socially optimum (generous) giving, i.e. $R(v) = \frac{\Pr(\beta \leq \beta_1(v))}{\Pr(\beta \geq \beta_2(v))}$. As

[10]In Arifovic and Ledyard's paper, this term is referred to as the level of altruism. Due to different definitions of altruism in the economics and psychology literature, we opt to avoid confusion by referring to β_i as the individual's generosity.

expected, the average giving is decreasing in the relative likelihood of selfish giving (i.e. $R(v)$) since it causes the conditional contributors to adopt more pessimistic beliefs about the average giving in the population.

To determine how the expected equilibrium giving varies with the MPCR, v, we need to take into account the distribution of other-regarding preferences since it affects the relative likelihood of selfish giving, $R(v)$. In particular, in order to focus attention on the comparative statics with respect to the population's generosity level, we simplify the model by letting $\gamma_i = \gamma$ be identical across the population.[11] Furthermore, we model the pro-social preferences in the population as distributed according to an exponential distribution $\beta_i \sim Exp(1/\lambda)$ where higher λ represents a (stochastically) more generous population.[12] This specification allows us to conduct comparative statics with respect to the generosity level of the population, captured succinctly by the parameter λ.

Given the expected equilibrium giving function and the distribution of pro-social preferences in the population, the following lemma describes how expected giving varies with the MPCR, v, and the population's generosity level λ.

Lemma 1. *$\overline{g}(v)$ is increasing in $v \in \left(\frac{1}{N}, 1\right)$ with $\lim_{v\to\frac{1}{N}} \overline{g}(v) = 0$ and $\lim_{v\to 1} \overline{g}(v) = W$. Moreover, there exists a unique $\tilde{v}(\lambda) \in \left(\frac{1}{N}, 1\right]$ with the following properties:*

1) $\overline{g}''(v) > 0$ for $v < \tilde{v}(\lambda)$ and $\overline{g}''(v) < 0$ for $v > \tilde{v}(\lambda)$;

2) $\tilde{v}(\lambda)$ is decreasing in λ with $\lim_{\lambda\to 0} \tilde{v}(\lambda) = 1$ and $\lim_{\lambda\to\infty} \tilde{v}(\lambda) = \frac{1}{N}$.

The formal proof of Lemma 1 is relegated to Appendix A. Intuitively, it reveals that the equilibrium giving is increasing in the MPCR since higher v increases the net social benefit of giving, captured by $Nv-1$, and decreases the individual cost of giving, captured by $(1-v)$. Moreover, expected giving approaches zero as the net social benefit of giving becomes negligible (i.e. $v \to \frac{1}{N}$)

[11]The results in this section readily generalize to a stochastic inequality aversion parameter γ_i as long as γ_i and β_i are independently distributed.

[12]The exponential distribution gives a convenient way of capturing heterogeneity in pro-social preferences as its domain covers all non-negative real numbers and the parameter λ allows us to stochastically change the pro-social preferences of the population in terms of first order stochastic dominance.

and it approaches W as the marginal cost of giving becomes negligible (i.e. $v \to 1$).

Interestingly, the first property reveals that the marginal benefit of increasing the MPCR is non-monotone and tends to diminish at higher values of the MPCR. In particular, the average giving $\overline{g}(v)$ exhibits increasing returns of higher MPCR for low values ($v < \tilde{v}(\lambda)$), but diminishing returns for high values ($v > \tilde{v}(\lambda)$). To grasp the intuition behind these dynamics, note that for low values (i.e. $v < \tilde{v}(\lambda)$), there is a significant number of agents who do not contribute. Thus, raising the MPCR in this case has an increasing marginal impact as it shifts a growing number of agents away from selfish to conditional and generous giving. However, this impact of increasing the MPCR eventually levels off as the number of selfish agents dwindles. Consequently, for high values of the MPCR ($v > \tilde{v}(\lambda)$), the marginal impact of further increasing the MPCR is diminishing as it induces a smaller number of agents to move away from selfish giving.

The second property further reveals that a more generous population, characterized by a larger λ, reaches diminishing returns of higher MPCR faste (i.e. $\tilde{v}(\lambda)$ is decreasing in λ). The reason is that for a more generous population, composed of individuals with relatively high β_i, inducing most agents to give requires only a modest increase in the MPCR. The opposite is true for a less generous population, in which significant portion of agents require a large increase in the MPCR in order to contribute.

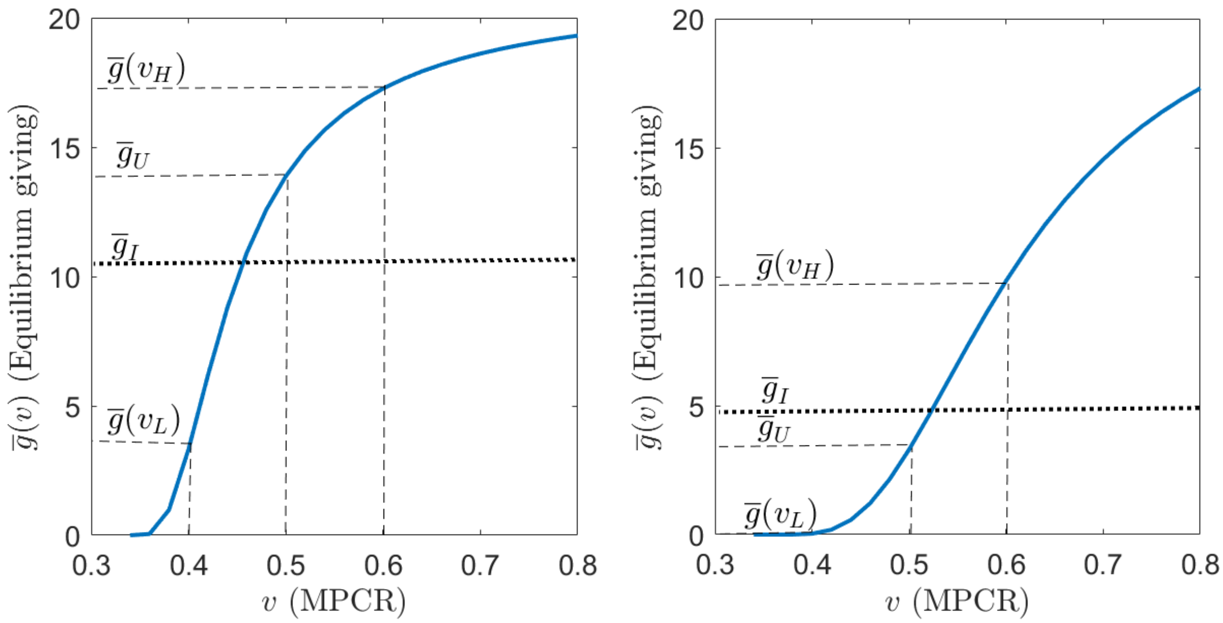
Figure 2.1 illustrates a numerical example for two different values of λ (low and high generosity levels) and provides visual support for Lemma 1.[13] It is evident from Figure 2.1 that $\overline{g}(v)$ is increasing in v for both generosity levels. While $\overline{g}(v)$ is convex at low values of v, it is concave at high values. Moreover, a more generous population (Figure 2.1 (a)) reaches diminishing returns of higher MPCR faster as illustrated by the fact that it is concave for a wider region of v (i.e. $\tilde{v}(16) < \tilde{v}(6)$).

The shape of the giving function described by Lemma 1 has an important implication on the impact of information provision. To see this, suppose that, as in the experimental design in Section 2.4, the MPCR (v) is drawn from a discrete distribution with $v = \{v_L, v_H\}$, where $\frac{1}{N} < v_L <$

[13]This numerical example is constructed by using the following parameters: $\gamma = 4$, $v_L = 0.4$, $v_H = 0.6$, and $p_L = 0.5$

Figure 2.1: Informed and Uninformed Giving

(a) High Generosity ($\lambda = 16$) (b) Low Generosity ($\lambda = 6$)



$v_H < 1$, and $\Pr(v = v_r) = p_r$ for $r = \{L, H\}$.[14] In absence of information, the agent's giving ($\overline{g}_U$) is based on the expected MPCR, $E[v]$. In contrast, an informed agent gives based on the realized MPCR, v, and thus the expected informed giving ($\overline{g}_I$) is the weighed average contributions under high and low MPCR.

$$\overline{g}_U = \overline{g}(E[v]); \quad \overline{g}_I = p_L \overline{g}(v_L) + p_H \overline{g}(v_H) \tag{2.5}$$

Clearly, information can either decrease giving by revealing low value v_L (bad news), or increase giving by revealing high value v_H (good news). The relative magnitude of the response to good and bad news depends of the shape of the giving function described by Lemma 1 and illustrated in Figure 2.1. In particular, Figure 2.1 (a) illustrates the case of generous population for which the giving function is mostly in the concave region. It is evident from the figure that expected equilibrium giving responds more to bad news than good news, i.e. $|\overline{g}_U - \overline{g}(v_L)| > |\overline{g}_U - \overline{g}(v_H)|$.

[14] To ease the exposition, we present the theoretical results using a two-point distribution since it corresponds to our experimental design in Section 2.4, but the theoretical results extend to any arbitrary non-degenerate distribution.

Consequently, when the population is rather generous, information is on average bad for giving, i.e. $\overline{g}_U > \overline{g}_I$. The opposite is true for a more selfish population that is likely to feature a convex giving function for a wider range of v. Thus, as Figure 2.1 (b) illustrates, the response to good news in this case is larger than the response to bad news (i.e. $|\overline{g}_U - \overline{g}(v_H)| > |\overline{g}_U - \overline{g}(v_L)|$), causing information to be on average beneficial for giving (i.e. $\overline{g}_I > \overline{g}_U$). The following Proposition formalizes this dynamics.

Proposition 1. *There exist generosity levels $0 < \lambda_1 \leq \lambda_2 < \infty$ such that expected informed giving exceeds uninformed giving for $\lambda \leq \lambda_1$, while uninformed giving exceeds expected informed giving for $\lambda \geq \lambda_2$.*

The proof of Proposition 1 follows immediately from the Jensens' inequality and is relegated to Appendix A. The proposition states that while informed giving exceeds uninformed giving for a less generous population, information is detrimental for giving if the population is more generous. As discussed above, the key driver for these dynamics is that less generous population is more responsive to good news than bad news, while the opposite is true for more generous population.

Lemma 1 and Proposition 1 provide testable hypotheses that we investigate by using a lab experiment described in Section 2.4. In particular, our experimental design aims to test the following hypotheses.

Hypothesis 1. *In a less generous population, the agents' average response to good news is higher than their response to bad news.*

Hypothesis 2. *In a more generous population, the agents' average response to good news is lower than their response to bad news.*

The implication of Hypothesis 1 is that the average contributions are higher when agents are informed and thus information is good for giving. Hypothesis 2 implies just the opposite for a more generous population- average uninformed giving exceeds the informed one and information is bad for giving. In the next section, we describe the experimental design that we use to test these hypotheses in the lab.

2.4 Experimental Design

In order to test our hypotheses, we need to control for the level of generosity in each session. We do this by conducting the experiment in two stages.[15] In Stage 1, we measure subjects' generosity level in the public good game by using an online experiment. One to two weeks later, using the information obtained from Stage 1, we invite some of these participants to the lab to participate in the second stage of the experiment.

Our experiment is a 2x2 between subjects design[16]: *Selfish* vs. *Generous* and *Informed* vs. *Uninformed*. Using the data collected in Stage 1, we create relatively more and less selfish sessions in the lab. More specifically, we only invite subjects who were classified as relatively less generous to the *Selfish* treatment; and we only invite subjects who were classified as relatively more generous to the *Generous* treatment.

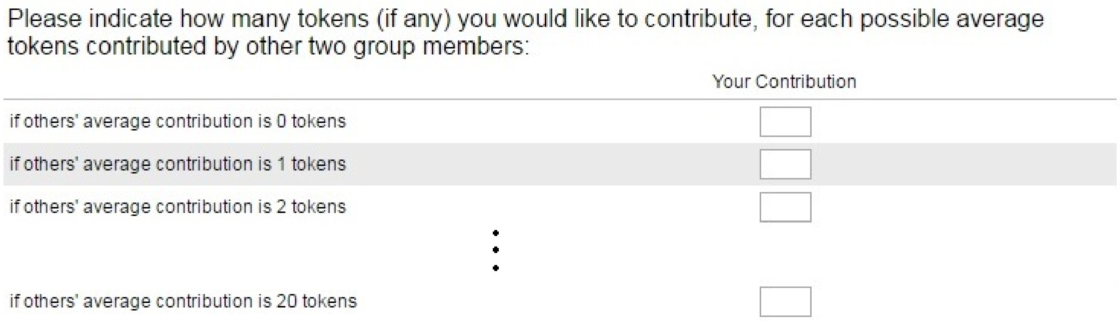
In Stage 2, subjects come to the lab to participate in a linear public good game described in Section 2.3. Subjects are placed in groups of three and play a one shot linear public good game with uncertain MPCR, which takes values of 0.4 or 0.6 with equal probability. They play the game for 10 rounds with random rematching. In the *Uninformed* treatment, subjects make a contribution decision without knowing which MPCR will be used for that round. However, subjects know that each outcome of the MPCR is equally likely. In the *Informed* treatment, subjects are informed about the realized MPCR for that round when they make their decisions. We pay subjects for one of the rounds picked at random at the end of the experiment. Finally, they fill out a survey. Below, we provide detailed information about each stage of the experimental design.

Stage 1: First, invitees receive an invitation email to participate in an incentivized online experiment with a possibility of being invited to an experiment in our lab. The online experiment, programmed in Qualtrics, consists of Fischbacher et al. (2001) (henceforth FGF) conditional con-

[15]This aspect of our design is inspired by and similar to Burlando and Guala (2005), Gächter and Thöni (2005), and de Oliveira et al. (2015).

[16]We ran the Informed and Uninformed treatments within subjects. Although subjects knew that experiment had two parts, they did not know anything about the second part when they played the first part. We only report the data from the first treatment played, since the behavior in the first treatment contaminated the data from the second treatment (i.e. ordering effect).

Figure 2.2: Conditional Contribution Table


Please indicate how many tokens (if any) you would like to contribute, for each possible average tokens contributed by other two group members:

	Your Contribution
if others' average contribution is 0 tokens	
if others' average contribution is 1 tokens	
if others' average contribution is 2 tokens	
⋮	
if others' average contribution is 20 tokens	

tribution game. In this game, each subject is endowed with 20 tokens (1 token=$0.40), assigned to a group consisting of three other members, and asked to play a linear public good game with an MPCR of 0.50. Each subject makes two decisions: Decision 1 and Decision 2. In Decision 1, subjects state how many of their tokens, if any, they would like to contribute to a group project (unconditional contribution) that benefits everyone in their group equally. Next, in Decision 2, they fill out a conditional contribution table (see Figure 2.2). In this table, they indicate how many tokens they would like to contribute to the group project *conditional* on the other group members' average contribution in Decision 1. For example, they state how much they would like to contribute if the other group members contributed 0 tokens on average in Decision 1, how much they would like to contribute if others contributed 1 token on average in Decision 1, and so on. Thus, in Decision 2, subjects make a total of 21 conditional contribution decisions.

After all subjects participate in the online experiment, we randomly construct groups of three. Next, for each group, we randomly pick two group members for which Decision 1 will be implemented. We implement Decision 2 for the other group member. In other words, we randomly determine which two group members' unconditional contribution decisions will be implemented. Depending on the average unconditional contribution made by these two group members, we implement the other group member's conditional contribution as indicated in her conditional contribution table. Then, we calculate the earnings accordingly. Payments for the online experiment are delivered by Venmo, Paypal or cash. In order to avoid any potential contamination that may be created by the outcome of this stage, the subjects are not informed about the outcome and receive their payments for the online experiment only after Stage 2 is conducted.

The FGF conditional contribution game described above is a good way to measure the generosity level (β_i) of the subjects in the public good game. It is commonly used in the literature (with over 2,000 citations) to classify subjects into types in the public good game: selfish (or free-riders who contribute zero), conditional cooperator (subjects whose contributions depend on the others' average contribution) and pro-social (or full cooperators who contribute everything).[17] As described in Section 2.3, each subject's type is determined by their level of generosity, β_i. Those with a relatively low β_i are selfish, those with a high β_i are pro-social and others with a β_i somewhere in between are conditional cooperators. Since one of our goals is to create more and less generous groups in the lab, we calculate a measure for each subject by using the data collected from the conditional contribution game in Stage 1. More specifically, we calculate the following parameter, that works as a proxy for the subject's level of generosity (i.e. β_i), for each subject:

$$\hat{\beta}_i = \frac{\sum_{j=0}^{20}(g_j^i - j)}{\sum_{j=0}^{20} j}$$

where g_j^i is subject i's stated conditional contribution in Decision 2 for an average contribution by others, $j = 0, 1..., 20$. If a subject is selfish, whose contribution is always zero independent of others' giving, then her $\hat{\beta}_i$ is equal to -1. If a subject is pro-social, who always contributes all of her endowment independent of others' giving, her $\hat{\beta}_i$ is equal to $+1$. If a subject is a perfect conditional cooperator, whose giving perfectly matches others' average contribution, then her $\hat{\beta}_i$ is equal to 0. In general, if a subject is more generous, she tends to contribute higher amounts for any average contribution level of the other group members resulting in a larger $\hat{\beta}_i$.

Next, we rank all the subjects based on their $\hat{\beta}_i$ and divide them into two equally populated samples using the median. This gives us two samples, one below the median and one above the median. The first sample includes selfish subjects as well as conditional cooperators, thus it is

[17]Boosey et al. (2018) shows the validity of this procedure to explain behavior in public goods games. Also see Thöni and Volk (2018) that review 17 replication studies of FGF and show that FGF findings are stable.

relatively more selfish. The second sample is relatively more generous since it includes pro-social subjects who contributed everything as well as conditional cooperators. More information on the distribution of types in our experiment is provided in Section 2.5. Next, we use these two samples to control for the generosity level in the public good game in the lab as explained below.

Stage 2: After dividing the subjects into two equally populated samples, we invite them to participate in the second stage in the Economic Research Lab at Texas A&M University.

Subjects play a one shot linear public good game in groups of three for ten rounds in the lab. The groups in each round are constructed randomly (stranger matching design). In each round, subjects start out with 20 tokens (1 token =\$0.50) in their individual accounts and are asked to decide how many of these 20 tokens, if any, they would like to contribute to a group project (g_i). The monetary payoff function for this game is as follows:

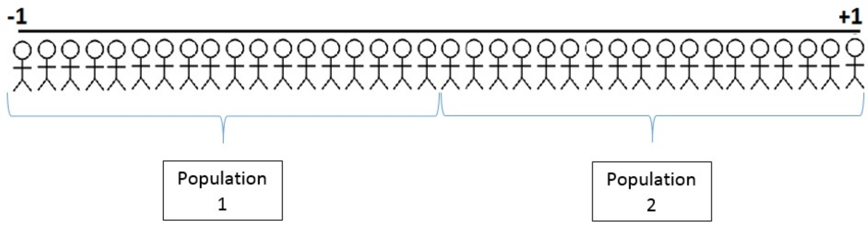
$$M_i = 20 - g_i + v \sum_{k=1}^{3} g_k$$

The MPCR (v) of the public good is either 0.40 with 0.5 probability or 0.60 with 0.5 probability, which is determined randomly and independently for each group in each round.[18] In the *Uninformed* treatment, subjects make their contribution decision about the public good without knowing which MPCR is selected for that round, but they know that it is either 0.40 or 0.60 with equal probability. In the *Informed* treatment, subjects are informed about the randomly chosen MPCR for that round and then are asked to make their contribution decisions about the public good. In both treatments, at the end of each round subjects receive feedback about their earnings, other group members' average contribution, and the randomly determined MPCR in the round.

In the *Selfish* (*Generous*) treatment, we only invite subjects whose $\hat{\beta}_i$ was below (above) the median. This is how we control for the level of generosity in each session. At the end of the instructions, before the experiment starts, we remind subjects about their participation in the online experiment and provide them with information about the level of generosity in their session. More

[18]The independent draw of the MPCR on the round and the group level eliminates any potential effect coming from the order of the MPCR.

Figure 2.3: Providing Info About the Session's Generosity Level



specifically, using a neutral language, we explain how we have created a measure (i.e. $\hat{\beta}_i$) using their responses in the online experiment and ranked everyone based on their measure as shown in Figure 2.3. In the *Selfish* (*Generous*) treatment, we tell subjects that participants from Population 1 (2) were invited for that session.

2.5 Results

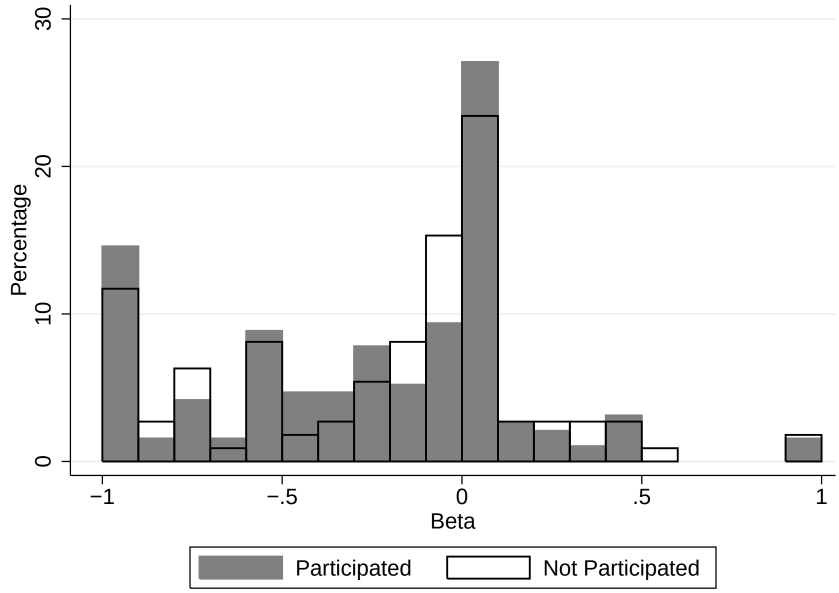
Six experimental sessions were conducted in the Economic Research Lab at Texas A&M University in April 2017. Subjects were recruited through ORSEE (Greiner et al., 2004), and the lab experiment was coded in z-Tree (Fischbacher, 2007). Average earnings were \$9.85 in the first stage and \$21 in the second stage (including a show up fee of \$8 in the second stage).

A total of 360 subjects participated in the online experiment and 44 of these preferred not to be invited to the lab experiment. From the remaining group, we excluded 13 as their behavior in the online experiment seemed to be random. The final pool of subjects for Stage 2 was 303 and 111 of those participated in the second stage.

2.5.1 Stage 1 Findings

The attrition rate from Stage 1 to Stage 2 is high since when recruiting for the online experiment, it was impossible to predict whether a subject would be assigned to the *Selfish* or *Generous* treatment sessions. This made it difficult to schedule session times that would be convenient for a large number of subjects. Nevertheless, it is important to confirm that there is no systematic difference in the generosity level of the subjects who participated in both stages versus the ones who participated in the first stage only. For this purpose, we look at Figure 2.4 that presents the

Figure 2.4: Distribution of $\hat{\beta}_i$

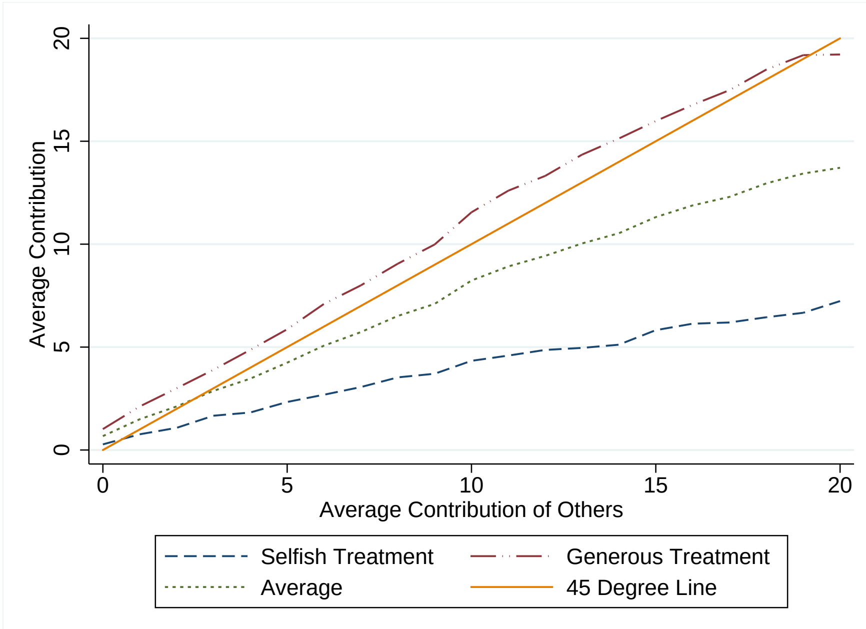


percentage distribution of $\hat{\beta}_i$, as computed using (6), for the 303 participants who were invited to Stage 2 in our experiment. The darker color represents the participants who participated in both stages (111 subjects), whereas the lighter color- the participants who only participated in the first stage (192 subjects). Using the Kolmogorov-Smirnov equality of distributions test, we confirm that the difference between the distribution of $\hat{\beta}_i$ across these two samples is not statistically significant (*p-value* is 0.536).

The mean and median of $\hat{\beta}_i$ from the online experiment are -0.25 and -0.11 respectively. The median is the cut-off point for the *Generous* and *Selfish* sessions. The subjects whose $\hat{\beta}_i$ is below (above) the median are invited to the *Selfish (Generous)* treatment sessions. This is the only difference between these two treatments.

Figure 2.5 illustrates the conditional contribution decisions made in Stage 1 by those who also participated in Stage 2. A perfect conditional cooperator who always matches the others' average contribution would be located on the 45 degree line. If a subject is located above this 45 degree line, it indicates that the subject contributed more than others for all possible average

Figure 2.5: Average Contributions in Stage 1 for Each Possible Average Contribution of Others



contributions made by other group members. On the contrary, a subject who contributed less than the average would be located below this line. The average conditional contributions made for each possible contribution level of others looks almost identical to FGF data. Figure 2.5 also illustrates the average conditional contributions made by the subjects in *Generous* and *Selfish* treatments separately.

2.5.2 Stage 2 Findings

First, we compare the average contributions made across treatments. We do this by taking the average amount of tokens contributed across all ten periods by each subject and compare them using bootstrap t-test.[19] Table 2.1 presents these average contributions made across treatments. First of all, it is not surprising to see that average contributions in *Generous* treatment is always higher than the *Selfish* treatment (*p-value* < 0.000 for all three conditions). Next, in the *Selfish* treatment we find that *Uniformed* average contributions are not different than *Informed* contributions with low MPCR (*p-value* is 0.333). However, *Uninformed* contributions are significantly different than

[19]Mann-Whitney U test also yield very similar p-values.

Informed contributions with high MPCR (*p-value* is 0.015). Subjects in the *Selfish* treatment do not respond to information when they receive bad news (MPCR of 0.40), but they significantly increase their contributions when they receive good news (MPCR of 0.60). This is line with Hypothesis 1. In the *Generous* treatment, we see the opposite as stated in Hypothesis 2. Average *Uninformed* contributions are not statistically different from the *Informed* contributions with high MPCR (*p-value* is 0.838), but they are different from the *Informed* contributions with low MPCR (*p-value* is 0.039). Contrary to the *Selfish* treatment findings, subjects in the *Generous* treatment do not respond to good news, but they significantly decrease their contributions upon obtaining bad news.

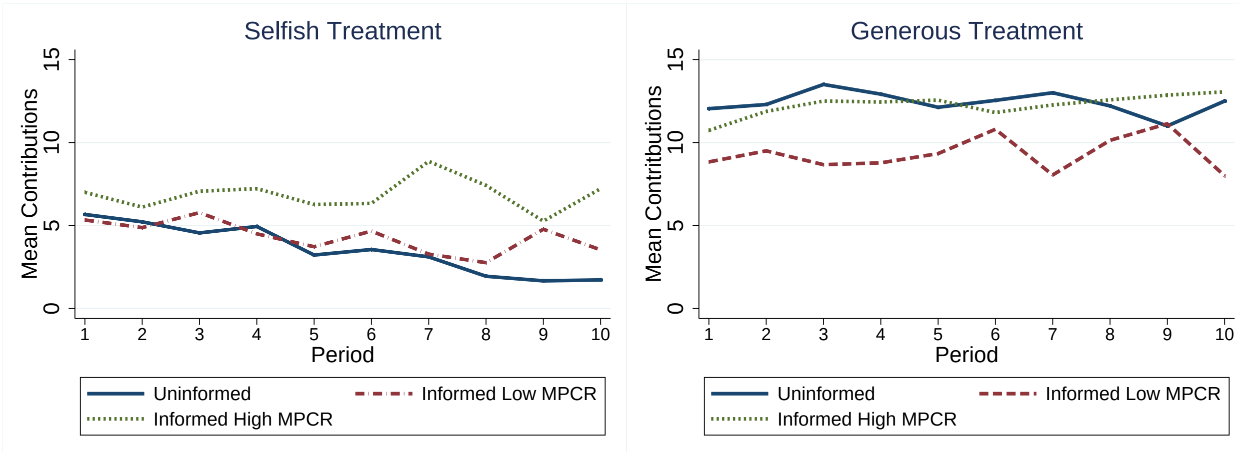
Table 2.1: Average Contributions Across Treatments

	Uninformed	**Informed**	
		High MPCR	**Low MPCR**
Selfish	3.56 (n=18)	6.67 (n=33)	4.37 (n=33)
Generous	12.41 (n=24)	12.14 (n=36)	9.24 (n=35)

This is also evident in Figure 2.6. Figure 2.6 shows the average contributions in all ten periods in both *Selfish* (left) and *Generous* (right) treatments. As you can see in Figure 2.6, in the *Selfish* treatment, uninformed contributions follow a similar path as the informed contributions for the low MPCR over time. However, there is a jump in the level of average informed contributions for the high MPCR. On the other hand, in the *Generous* treatment, while the average uninformed contributions follow a similar path as the informed average contributions for the high MPCR, there is a decrease in average informed contributions for the low MPCR relative to the uninformed contributions. Finally, as expected, average informed contributions are always higher for the high MPCR for both *Selfish* and *Generous* treatments.

To check the robustness of our findings, we next present the regression results. Since the lowest possible contribution amount is zero tokens and the highest possible contribution amount is

Figure 2.6: Mean Contributions



20 tokens, we need to control for potential censuring. Although Tobit model is useful in order to account for censoring, it restricts the data by not allowing different motives behind the contribution of zero.[20] In other words, Tobit model does not differentiate between the subjects who are selfish and would always contribute zero no matter what, and those who contribute zero due to treatment (for example due to receiving bad news). Following (Moffatt, 2015, Ch 11.), we use a double hurdle model (also see Brown et al., 2017 for another example of using hurdle model in experimental data).

The double hurdle model treats the probability of being a contributor and the extent of contribution separately. Thus, by using this model, we can examine the impact of information on the extensive and intensive margin. The results are reported in Table 2.2. We first run a Probit model regression using the cross section of all 111 subjects to analyze the factors that impact whether subjects contribute or not (i.e. being a potential contributor or not). The dependent variable in this probit model is Contributed which takes the value of one if the subject contributed a positive amount in any of the ten periods; and zero otherwise. The estimates of the first hurdle are presented in the first column of Table 2.2. There are a total of six subjects who contributed zero in all ten periods. Being in *Informed* treatment does not affect the *probability* of contributing to the

[20]A similar reasoning can also apply to subjects who contribute everything. Since we have only one subject who contributed everything in all periods, we restrict our attention to only selfish types.

public good. This means that information does not impact contributions on the extensive margin. However, being in the *Selfish* treatment significantly decreases the probability of contributing. This is not surprising given that we created the *Selfish* vs. *Generous* treatments based on the subjects' level of generosity measured in Stage 1.

Table 2.2: Double Hurdle Model Regression Results

	Probit	Selfish Treatment Tobit		Generous Treatment Tobit	
	(1)	(2)	(3)	(4)	(5)
Selfish	-0.836*				
	(0.470)				
Informed	0.00403	2.332**	0.865	-2.559*	-4.675***
	(0.431)	(1.144)	(1.009)	(1.379)	(1.179)
Informed*High MPCR			3.211***		3.555***
			(0.773)		(0.580)
Lagged Others' Average		0.258***	0.275***	0.149***	0.148***
		(0.0741)	(0.0705)	(0.0460)	(0.0536)
Beta		-0.976	-0.816	9.695***	9.709***
		(2.365)	(2.082)	(2.367)	(2.126)
Period		-0.253**	-0.288***	-0.0822	-0.0203
		(0.111)	(0.102)	(0.118)	(0.116)
Constant	2.126***	2.891*	3.150*	11.91***	11.51***
	(0.458)	(1.704)	(1.612)	(1.692)	(1.393)
Observations	111	414	414	531	531

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The robust standard errors clustered at the individual level are in parentheses.

The dependent variable for the Probit model is Contributed which takes the value of 1 if the subject contributed at least once, and otherwise zero. The dependent variable for the panel data Tobit models is Contributions. The number of observations in column 1 is the total number of subjects participated in this study. The numbers of observations in the remaining four columns are the number of decisions made in 9 periods by subjects who contributed at least once across all 10 periods.

Next, we run a Tobit model for *Selfish* and *Generous* treatments separately to study the factors that impact contributions *conditional* on contributing at least once (i.e. conditional on being a potential contributor). Thus, we exclude the subjects who failed the first hurdle. In columns (2)-(5), we report the marginal effects of the coefficients on the uncensored latent variable. The second and the third columns are created using the data collected in the *Selfish* treatment and the last two columns are created using the data collected in the *Generous* treatment. The dependent variable for all four columns is the contributions to the public good in each round.

The first model in columns 2 and 4 shows the average impact of information on contributions. The variable *Informed* is a dummy variable for *Informed* treatment sessions. Thus, it takes the value of 1 if the subjects were informed about the realized MPCR for that round before they made their decisions. The model also controls for the following variables: *Lagged Others' Average* which is the average contributions made by other group members in the previous round, *Beta* which is $\hat{\beta}_i$ that was computed using the data from the online experiment (i.e. Stage 1), finally *Period* which is simply the time trend. It is evident from these columns that, in *Selfish* treatment, information has a positive and significant impact on contributions for those who are potential contributors. On the other hand, in *Generous* treatment, information hurts the average contributions.

The second model in columns 3 and 5 studies the impact of information separately for good and bad news. The variable *Informed*High MPCR* is the interaction term between *Informed* and *High MPCR*. The baseline in columns 3 and 5 is the *Uninformed* treatment. Thus, the coefficient of *Informed* shows the impact of receiving bad news. And, the coefficient of *Informed*High MPCR* shows the impact of receiving good news *relative* to receiving bad news. Thus, the impact of receiving good news relative to the *Uninformed* treatment is the summation of the coefficients of *Informed* and *Informed*High MPCR*.

In the *Selfish* treatment, we see that when subjects are informed and if they find out that the MPCR is low, then they do not significantly change their giving behavior relative to being uninformed. In other words, they do not respond to bad news. However, when they are informed and receive good news, then they respond to information by increasing their contributions. As suggested by Hypothesis 1, the relative response to good news is larger than bad news, thus information is good for contributions.

On the other hand, in *Generous* treatment, when subjects are informed and if they receive bad news, they significantly decrease their contributions relative to the contribution levels when uninformed. When they receive good news, they respond to it by significantly increasing their contributions relative to receiving bad news. Furthermore, as suggested by Hypothesis 2, the negative response to bad news is stronger than the positive response to good news. Thus, on average, information hurts contributions significantly on the intensive margin.

2.6 Conclusion

In this paper, we investigate the impact of information about the MPCR of a linear public good on contributions. The theoretical model predicts that information provision has differential impact on less and more generous groups. While information increases average contributions by less generous subject groups, it reduces average contributions by more generous subject groups. We experimentally test these hypotheses in the lab and the findings are in line with the theoretical expectations. We find that information does not impact public good contribution on the extensive margin. However, information impacts public good contributions on the intensive margin and the sign of this impact depends on the generosity level of the sessions. In the relatively selfish sessions, subjects who contributed at least once contribute more on average when they are informed compared to when they are uninformed of the value of the public good. However, just the opposite is true for the relatively generous sessions. In these sessions, subjects who are potential contributors contribute less to the public good when they are informed. This is because their relative response to bad news is greater than their response to good news.

The findings of this study have significant implications for fundraising. In particular, they suggest that targeted information provision may be a more fruitful strategy of increasing public good contributions than uniform information provision. Since donors themselves may be able to acquire information by conducting research about non-profits prior to contributing, an important direction for future research includes endogenizing the choice of information acquisition by donors. This would allow us to glean further insight about the impact of information on public good provision by studying how information acquisition incentives differ across donors.

3. THE EFFECTS OF SCARCITY ON CHEATING AND IN-GROUP FAVORITISM

3.1 Introduction and Literature Review

Over 10% of the world's population lives under extreme poverty.[1] Even in developed countries, a significant proportion of the population suffers from scarcity of resources. For example, in the United States, 41.2 million people (12.3% of the population) were food insecure in 2016, meaning they did not have enough money or other resources to buy sufficient food to meet the needs of all their household members (Coleman-Jensen et al., 2017). In addition to obvious detrimental effects, such as poor nutrition intake and health, an emerging literature proposes that living under a prolonged state of scarcity impairs decision-making (Shah et al., 2012; Mani et al., 2013; Mullainathan and Shafir, 2013; Haushofer and Fehr, 2014). Individuals living in poverty engage in suboptimal behavior, such as excessive borrowing at high interest rates (Bertrand and Morse, 2011; Dobbie and Skiba, 2013), playing lotteries (Haisley et al., 2008a,b), bad management of personal finances and low saving rates (Barr, 2012). They are also less productive at work (Kim et al., 2006), more impatient (Lawrance, 1991; Carvalho, 2010), more risk averse (Gloede et al., 2015) and have lower self-control (Banerjee and Mullainathan, 2010; Spears, 2011; Bernheim et al., 2015).

There is a considerable amount of literature that connects poverty and crime, although causality has not been robustly established (Ellis and McDonald, 2001; Sharkey et al., 2016). Notorious criminals, from Al Capone to Pablo Escobar, use a lack of resources to justify initiating a lifetime of illegal activities. For decades, the economic environment has been recognized as a critical factor in criminal behavior (Sharkey et al., 2016). It should be noted, however, that recent literature suggests a potential genetic predisposition to antisocial behavior and crime (Joseph, 2001; Raine, 2008; Mead et al., 2009; Raine, 2013; van Gelder and de Vries, 2014). The question of whether criminal behavior is rooted in individual traits or influenced by scarcity is important to understand in order to reduce criminal behavior.

[1] According to the World Bank, 766 million people live in extreme poverty with less than $1.90 per day. http://www.worldbank.org/en/publication/poverty-and-shared-prosperity.

In general, economic models that study criminal behavior suggest that an individual commits a crime if the benefits outweigh the costs (i.e. potential punishments). In his seminal work, Gary Becker (1968) argues that those who engage in criminal behavior do so not because their motivations differ from those of noncriminals but because their benefits and costs differ. Although crime is more generally associated with violent felonies, the same economic rationale applies to other types of lesser misconduct, such as cheating.

Cheating has recently received a considerable amount of interest from economists. Using incentivized games, researchers have shown that people cheat far less than standard economic theoretical predictions (e.g. Gneezy, 2005; Mazar et al., 2008; Hurkens and Kartik, 2009; Sutter, 2009; Fischbacher and Föllmi-Heusi, 2013; Jiang, 2013).[2] In these games, subjects have the opportunity to increase their own monetary payoff by cheating. However, people do not cheat maximally and exhibit an aversion to lying (Dufwenberg and Gneezy, 2000; Charness and Dufwenberg, 2006; Mazar and Ariely, 2006; Lundquist et al., 2009; Battigalli et al., 2013; Erat, 2013). Many factors impact dishonesty, including self-image (e.g. Mazar et al. 2008), anonymity of decisions (Fischbacher and Föllmi-Heusi, 2013; Gneezy et al., 2018), size of the stakes and incentives (Fischbacher and Föllmi-Heusi, 2013; Kajackaite and Gneezy, 2017; Martinelli et al., 2018; Rahwan et al., 2018), and fairness (Houser et al., 2012). Furthermore, research shows that cheating behavior in the laboratory correlates with cheating behavior in the real world (Gächter and Schulz, 2016; Potters and Stoop, 2016; Dai et al., 2018).

In this paper, we study the extent to which scarcity, in the form of a substantial reduction in available resources, impacts cheating. We investigate this question by implementing a two-stage lab-in-the-field experiment with poor coffee farmers from a small and relatively closed community in Guatemala. Our participants derive their income almost exclusively from harvesting coffee beans. As such, a sharp decline in their income during non-harvesting months provides a natural variation in scarcity levels while other observables remain similar. We conducted our experiment in two stages by using this distinctive variance in income. The first stage took place before the

[2] See Rosenbaum et al. (2014); Abeler et al. (2016); Capraro (2017); Jacobsen et al. (2018) for a more comprehensive literature review.

coffee harvest started (*Scarcity* period). We then repeated the same experiments, with the same group of subjects, at the peak of the coffee harvest season (relative *Abundance* period).

We study differences in cheating behavior between the Scarcity and Abundance periods by using the die-roll game (Fischbacher and Föllmi-Heusi, 2013). Similar to the die-under-cup paradigm (Shalvi et al., 2011), we place a fair six-sided die in a cup with a closed lid. Subjects roll the six-sided die by shaking the cup and are asked to report the outcome to determine their earnings. The experiment is designed such that it is not possible to detect cheating behavior at the individual level; thus, no retribution can be pursued, and the full cost of cheating is exogenously borne by the experimenters. Thus, if individual characteristics are the main driving force behind cheating, there should be no change in the cheating behavior across periods. However, if the economic environment influences cheating behavior, then we expect higher levels of cheating during the Scarcity period.

Although standard economic theory suggests otherwise, people may also cheat to help others. A student taking an online exam or writing an essay in place of his/her friend, a person taking the blame for a minor traffic accident because his/her friend does not have insurance, a teenager lying to his/her friend's parents to help with his/her cover up story could be examples of such behavior. The motives behind this kind of dishonesty may be due to generosity or could be driven by past or expected reciprocity. In this paper, we also study the impact of scarcity on cheating for others by using the same die-roll game. We ensure that reciprocity cannot be a driving force by keeping the identities of the beneficiaries anonymous.

According to the social identity theory (Tajfel and Turner, 1979), individuals place themselves and others into groups, such as female, Caucasian, American, economists, poor, and so on. People also show favoritism (i.e. bias or preferential treatment) toward others within their group. This is called in-group favoritism (or in-group bias). In-group favoritism has been studied in the psychology and economics literature mainly by using people's natural identities (e.g. Klor and Shayo, 2010; Ockenfels and Werner, 2014; Cadsby et al., 2016) or by experimentally inducing identities (i.e. minimal group paradigm) (e.g. Eckel and Grossman, 2005; Buchan et al., 2006; Chen and Li, 2009; Chen and Chen, 2011; Harris et al., 2015). In this paper, we use the subjects' natural village identities to study how scarcity impacts in-group favoritism in terms of cheating.

Economic research on pro-social dishonesty is fairly new (Lewis et al., 2012; Gino et al., 2013; Okeke and Godlonton, 2014; Cadsby et al., 2016; Lupoli et al., 2017). Cadsby et al. (2016) ask whether people cheat for an in-group member at the expense of an out-group member and report significant cheating behavior. However, in-group favoritism in the absence of an externality to an out-group member has not been studied. In our study, the cost of favoring an in-group member is entirely borne by the experimenters. Furthermore, we compare in-group favoritism in cheating across Scarcity and Abundance periods.

The geographical location and sample population of the experiment were carefully selected. First, the residents of the village derive most of their yearly income from seasonal coffee harvest. This ensures that participants experience a financially worse situation in Scarcity relative to the Abundance period. Second, coffee is a perennial crop continuously harvested and sold weekly or biweekly. As such, the coffee harvest provides steady income during the harvest season. Finally, the village is relatively isolated. With limited transportation options, participants' mobility for the purposes of procuring outside income is impaired. All of these factors ensure that available resources are indeed scarce during the Scarcity period relative to the Abundance period. Meanwhile, other factors such as stress level, number of recent celebratory events attended, interactions with others outside of the village, level of physical activity, and so on remain similar. We confirm this by comparing survey measures across the two periods.

We contribute to the literature by studying how scarcity impacts dishonesty and in-group favoritism in terms of cheating using a lab-in-the-field experiment; to the best of our knowledge, we are the first to study these questions.[3] Our results show that subjects cheat the most for themselves and that this cheating behavior is not impacted by scarcity. We find that subjects also cheat for the in-group member (although less) and that this cheating is also not impacted by scarcity. Subjects

[3]While we were in the process of writing this paper, we became aware of a working paper, Boonmanunt et al. (2018), that studies poverty, social norms, and cheating. Their experiments were conducted around the same time as ours; however, they focus on the impact of social norms on cheating and how this changes due to poverty.

do not cheat for the out-group member in the Abundance period. Thus, we find in-group favoritism in terms of cheating in the Abundance period.

However, in-group favoritism disappears in the Scarcity period. Although scarcity does not impact the cheating behavior for oneself and for the in-group member, it significantly increases cheating for the out-group member. In the Scarcity period, subjects cheat for the out-group member just as much as they do for the in-group member.

We also contribute to the literature by studying the impact of scarcity on in-group favoritism in terms of generosity. In our cheating game, the cost of favoring an in-group member is purely moral. We also investigate the effects of scarcity on in-group favoritism when the cost of the preferential treatment is monetary. We do this by using a dictator game where the recipient is either an in-group member or an out-group member. In line with recent research (e.g. Ben-Ner et al., 2009; Whitt and Wilson, 2007; Chen and Li, 2009; Chen and Chen, 2011; Balliet et al., 2014), we also find in-group favoritism, but only in the Abundance period. While subjects send significantly more money to the in-group member in the Abundance period, the difference vanishes during the Scarcity period. In the Scarcity period, subjects are significantly more generous towards the out-group member which abolishes the in-group favoritism.

Earlier papers studying the correlation between scarcity and other-regarding preferences have mixed findings (e.g. Piff et al., 2010; Haushofer and Fehr, 2014; Andreoni et al., 2017). Bartos (2016) exploits a shock in income similar to ours during an agricultural harvest season, and he finds that the amount sent to an in-group member in a dictator game remained unchanged during scarcity and abundance periods. This is in line with our findings. We contribute to the literature by studying the *causal* impact of scarcity on other-regarding preferences as well as in-group favoritism.

3.2 Experimental Design and Procedures

3.2.1 Selection of Participants and Recruitment Procedure

The experiment was conducted in a small and relatively isolated village in Guatemala. The village is home to about 190 families whose main source of income is derived from harvesting coffee beans. Coffee is a perennial crop that is continuously harvested and sold during a period of five to six months (depending on the amount of rain and general climate conditions). In this part of Guatemala, harvesting normally occurs between late September and early March. A few studies have used agricultural harvest to separate scarce and abundant periods (e.g. Bartos, 2016; Mani et al., 2013; Boonmanunt et al., 2018). However, they use annual crops (such as sugar cane and rice), which means there is a one-time harvest and a single lump sum payment. In our case, our subjects sell their coffee beans to their local cooperative and receive steady weekly or bi-weekly payments during the five to six months long harvest season.

The selection of coffee farmers in this isolated community is crucial for identification purposes. During the non-harvesting months, participants live mainly on accumulated savings made during the harvest season. During this time, they also work on subsistence crops planted for self-consumption and the maintenance of the coffee plants such as pruning, weeding, and fertilizing. This is mostly a self-sustaining community. The closest settlement is about 45 minutes away by car. However, villagers have limited transportation options since most of them do not own motor vehicles. Thus, their mobility for the purposes of procuring outside income is severely impaired. About 95% of our subjects derive the majority of their income from harvesting coffee, and their interaction with people outside of their village is fairly constant across harvesting and non-harvesting months. All of these factors provide an ideal environment to study our research questions.

We employed five local assistants from the vicinity of the community to help recruit participants to our study. During the recruiting process, the assistants informed potential participants that the study consisted of economic decision-making and that they would be compensated with 20Q (Quetzales, about $3) for their participation. Prospective participants were also informed that they would have the opportunity to earn more money based on their decisions, the decisions of others, and luck. However, they were not provided with any details about the experimental procedures. Although Spanish is the most commonly used language in Guatemala, people in the rural areas also speak other languages such as K'iche' and Kaqchiquel. Thus, we instructed our study assistants to only recruit people who could understand and speak Spanish. The assistants were also instructed to recruit people who were at least numerate.

Our decision sheets, script, and experimental procedures were prepared so that people with low education levels could understand all parts of the experiment. Our decision sheets included visual illustrations and were prepared based on de Oliveira et al. (2012, 2016). We used large, poster-size laminated copies of each page in the booklets. While one of the experimenters was reading the instructions from a script, an assistant illustrated examples and instructions on the large laminated copies using a dry erase-marker. This helped the participants become familiar with each page in the booklet and ensured that all participants understood how the game worked and where they were supposed to indicate their decisions. Other study assistants were trained regarding the experimental procedures and were available to go around and privately help participants with any questions.

The experiment was conducted in two stages using a lab-in-the-field framework. The first stage took place in mid-September 2017, before the coffee harvest season (*Scarcity* period). The second stage took place in early December 2017, during the harvest season (*Abundance* period). In both periods, subjects played a sequence of games in the same order.[4] Because of the limitations that we faced in the field, we did not control for the potential order effects. However, Abeler et al. (2016), in their meta analysis, show that playing the cheating game repeatedly does not significantly change the cheating behavior. Additionally, our main research interest is the comparison of scarcity and abundance periods. Thus, we do not think that order is an issue for this paper. In this paper, we only use the data collected from two games: cheating games and dictator games. Below, we provide the experimental design and details of each game.

[4]This project is part of a larger study we conducted in the field. The same subjects played a sequence of games without feedback in the same order across all sessions in both periods. The games and the exact order is as follows: trust game with an in-group member (game 1), trust game with an out-group member (game 2), dictator game with an in-group member (game 3), dictator game with an out-group member (game 4), Eckel and Grossman (2002, 2008) risk elicitation task (game 5), time preference elicitation task (game 6), and finally three Fischbacher and Föllmi-Heusi (2013) cheating game treatments (games 7–9)(see Section 3.2.2. for details), and a survey. At the end of the experiment, one game out of the first six games was randomly chosen to be the paying game. The payment details for cheating games 7–9 are provided in Section 3.2.2.

Table 3.1: Cheating Game Payoffs

Number Reported	**Payoff**
1	5Q
2	10Q
3	15Q
4	20Q
5	25Q
6	0Q

Note: Q refers to Guatemalan Quetzales.
5 Q is equivalent to 0.70 USD.

3.2.2 Cheating Game

We used the Fischbacher and Föllmi-Heusi (2013) die-roll paradigm. In this game, subjects are provided with an opaque cup with a closed lid, containing a fair six-sided die (similar to the Shalvi et al. (2011) die-under-cup game). The cup is designed to ensure privacy. The only person who can see the die (and the number rolled) inside the cup is the person holding it. This process guarantees to participants that not even the experimenters would know the actual number rolled. Subjects are instructed to shake the cup (thus roll the six-sided die) twice but to report only the outcome of the *first* shake. The number reported determines the payment for completing a survey. Table 3.1 reports the payment scheme used in this game.

We have a 3x2 within-subjects design: 1) Cheating for self (CheatingSelf), 2) Cheating for an in-group member (CheatingInGroup), and 3) Cheating for an out-group member (CheatingOutGroup) during 1) Abundance and 2) Scarcity periods. First, subjects played the cheating game for themselves (CheatingSelf), which determined their earnings for completing the survey at the end of the experiment. Then they played the same game for an anonymous person from the subject's own village (AP-InGroup), which is the CheatingInGroup treatment. Finally, they participated in the CheatingOutGroup treatment and played the same game for an anonymous person from outside of the village (AP-OutGroup). Thus, the only difference among these three treatments is the identity of the beneficiary. The cheating games were played at the end of the experiment and were used to determine the payments for completing the survey, similar to Fischbacher and Föllmi-Heusi

(2013).

We used our subjects' naturally occurring village identity to study in-group favoritism. Prior to the experimental sessions, with the help of one of our local contacts, we randomly chose one person from the village to be the AP-InGroup. This person was discretely approached by one of the experimenters and asked to make decisions, not relevant to this paper, and to answer the same survey questions as the participants. The AP-InGroup was informed that it was very important for his/her identity to remain strictly confidential. Hence, he/she was instructed to avoid mentioning anything about our visit to anyone. We followed the same procedure with the AP-OutGroup.

The only information we provided to the subjects about the identity of the AP-InGroup (or AP-OutGroup) was that they were someone from their own village (or another village). The real identities of the AP-InGroup and AP-OutGroup remained unknown to the subjects. We opted to use an anonymous person as the out-group member mainly for the ease of implementation, since traveling across villages is cumbersome. Thus, it was not feasible to bring together subjects from different villages. We used the same procedure for an in-group member (i.e. AP-InGroup) in order to keep the procedure consistent across treatments and to prevent contamination from other potential effects. For example, if subjects knew the identity of the in-group member, then their behavior toward the in-group member might be biased in an unpredictable way based on their personal interaction, experience, and beliefs about this person.

Every participant was paid for their earnings in the CheatingSelf treatment. At the end of the experiment, one subject was randomly chosen, and his/her decision in the CheatingInGroup treatment determined the earnings for AP-InGroup. Similarly, another person was randomly chosen to determine the payment of the AP-OutGroup.

3.2.3 Dictator Game

In the Dictator Game, there are two players: a dictator and a recipient. The dictator is given an endowment of 30Q (about $4.2) and asked to decide how much, if any, to send to the recipient. The recipient does not have any endowment.

We employ a 2x2 within-subjects design: 1) In-group recipient (InGroup) and 2) Out-group recipient (OutGroup) during 1) Abundance and 2) Scarcity periods. Subjects were always the dictators, and the only difference between the InGroup and OutGroup treatments is the identity of the recipient. In the InGroup (OutGroup) treatment, the recipient is the AP-InGroup (AP-OutGroup).

As previously mentioned (see footnote 5), subjects played a total of nine games (including two dictator games and three cheating games) and were paid for their decisions in the dictator game only if one of the two dictator games was randomly selected for payment. Thus, if the InGroup or OutGroup treatments were randomly chosen to be the paying game, subjects' earnings were calculated according to their decisions. Furthermore, we randomly chose one subject whose decisions determined the earnings for the AP-InGroup or AP-OutGroup depending on the randomly chosen game. The APs were paid for their total earnings after we finished all the sessions.

3.3 Results

A total of 109 low-income coffee farmers participated in our experiment.[5] Nearly all participants (95%) derive the majority of their income from harvesting coffee beans, with an average yearly income of 8,399 Quetzales (about 1,120USD). About 41% of participants are female. Additionally, 35% are 18–30 years old, 36% are 30–50 years, and the rest are older than 50. Finally, 28% have no formal education, while 63% hold either an elementary or a middle school diploma, and 9% hold a high school diploma.

In the results presented below, unless stated otherwise, the reported *p*-values are derived by either McNemar's χ^2 test (for binary variables) or Wilcoxon signed rank test (for non-binary variables).

[5] A total of 144 subjects participated in the first stage (Scarcity period). We exclude 3 subjects from the analysis since they either did not understand Spanish or slept during the experiment. Of the remaining 141 subjects, 109 also participated in the second stage (Abundance period). Table C.1 in the appendix compares observables between the 109 subjects who participated in both stages and the 32 subjects who participated in the first stage only. We do not find a systematic difference between these two groups, which suggests that self-selection is not an issue.

3.3.1 Comparison of Scarcity and Abundance Periods

At the end of the experiment, subjects completed a survey. By comparing self-reported measures, we show that the main difference between the Scarcity and the Abundance periods is purely financial; other observables are fairly constant across the two periods. See Table C.2 for the description of the survey measures, and Tables C.3 and C.4 for a more detailed comparison of these measures across periods. The survey questions are provided in the online supplementary materials.

We asked participants to indicate whether they had experienced lack of money for various needs in the preceding month. By using an index created with answers to these questions, we find that a significantly higher proportion of subjects experienced lack of money in the Scarcity period relative to the Abundance period (p-value = 0.004).[6] While participants reported similar financial conditions relative to others in the village (p-value = 1.000), they also indicated a worse state of finances (p-value = 0.000) in the Scarcity period. This means that our participants experienced harsher financial conditions in the Scarcity period. Additionally, they reported that everyone else in the village was also experiencing similar financial situations. On the other hand, the proportion of participants taking a credit/loan in the preceding six months is not significantly different (p-value = 0.134). (It is important to note that farmers' access to credit is limited.) Furthermore, there is no difference in the frequency of celebratory events attended/organized (p-value = 0.414), and subjects reported similar stress levels (p-value = 0.525) across the two periods. Finally, consistent with the findings of Carvalho et al. (2016), participants' risk preferences, measured by an incentivized gamble (Eckel and Grossman, 2002, 2008), did not change across periods (p-value = 0.531).

In summary, these findings suggest that participants experienced more financial challenges and hardship during the Scarcity period. However, other observables did not significantly differ across the two periods.

[6] This index is created by summing the responses to four questions regarding lacking money in the preceding month for the following situations: food, basic expenses (non-food), medical expenses, and farm.

3.3.2 Cheating Game Findings

Table 3.2 provides detailed information about the data collected in the cheating game treatments. Columns 4-9 report the frequency of each number reported across all treatments and periods.[7] A visual comparison of these distributions can be found on Figure B.1. in the appendix. First, we compare the distribution of reported numbers in each treatment to a uniform distribution and report the *p*-values in the third column. Next, we compare the expected probability of each number occurring (16.7%) to the reported frequencies by using a one-sided binomial test. The resulting *p*-values are indicated with stars in each cell. Finally in the last column, we report the average number reported in each treatment and period.[8]

Additionally, similar to Wang et al. (2017), we also examine cheating behavior as the high-paying numbers (3, 4, and 5) being reported more often than the random occurrence of 50%. In other words, if the subjects are honest and report the observed outcome, then on expectation, the high payoffs should occur half of the time. Thus, reporting high payoffs more often than 50% represents the prevalence of cheating in order to increase earnings. Figure 3.1 shows the frequencies of high payoffs reported across all treatments and periods.

Result 1: In the Abundance period, subjects cheat for themselves and for the in-group member but not for the out-group member.

First, we compare the distribution of reported numbers in each treatment to a uniform distribution (see *p*-values in the third column of Table 3.2). Only the CheatingOutGroup treatment in the Abundance period is not significantly different from a uniform distribution. This means that the only treatment in which subjects did not cheat was the CheatingOutGroup treatment during the Abundance period.[9]

[7] We conducted a simulation analysis to assess the randomness of the sample with 109 subjects. We found that our sample size provides a statistically valid random uniform distribution (*p*-value = 0.046). The details of the simulation procedure are available in Appendix D.

[8] The expected number reported is 2.5 since six is coded as zero.

[9] Although our research questions (thus the experimental design) are different, Cadsby et al. (2016) also found that people cheat not only for themselves but also cheat for an in-group member. However, it is important to note that Cadsby et al. (2016) conducted their study in a lab and did not investigate the role of scarcity. While their environment could be more analogous to our Abundance period, we need to be cautious about a one-to-one comparison of our findings to theirs (or those of other similar papers).

Table 3.2: Proportion of Subjects who Reported the Corresponding Numbers

		p-values	0	1	2	3	4	5	Average Number
					Number Reported†				
Abundance	**Self**	0.000	2.75***	2.75***	5.50***	8.26***	27.52***	53.21***	4.15
	InGroup	0.000	6.42***	7.34***	12.84	19.27	22.94*	31.19***	3.39
	OutGroup	0.276	11.01*	21.10	14.68	12.84	19.27	21.10	2.72
Scarcity	**Self**	0.000	1.85***	2.78**	9.26**	12.96	19.44	53.70***	4.07
	InGroup	0.000	6.48***	6.48***	12.04*	26.85***	25 **	23.15*	3.27
	OutGroup	0.002	9.26**	12.04*	10.19**	20.37	28.70***	19.44	3.06

† Since reporting a 6 paid nothing, it is coded as 0. * < 0.10, ** < 0.05, and *** < 0.01.
The *p*-values reported on the third column are obtained by Chi-Square Goodness of Fit test run against a uniform distribution. The *p*-values indicated with stars in columns 4-9 are obtained from one-sided binomial probability tests for the proportion being larger (smaller) than 16.67%. See Figure B.1 in the appendix for a visual comparison of the distributions of each number reported across treatments and periods.

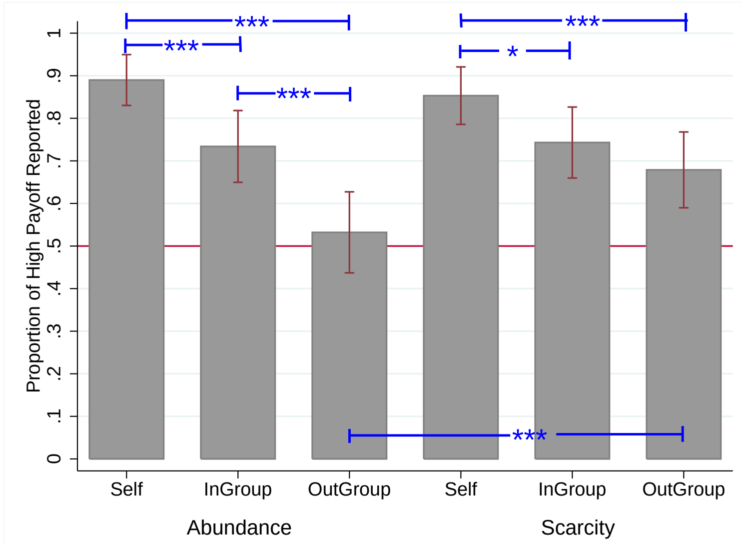
We also find supporting evidence for Result 1 when we compare the high payoffs reported across treatments. Figure 3.1 shows that, in the Abundance period, high payoffs are reported significantly more often than random chance would predict in both CheatingSelf (89%) and CheatingInGroup (73%) treatments (one-sided binomial probability test *p*-value is 0.000 for both). Moreover, the high payoffs are not reported significantly more than half of the time in CheatingOutGroup (53%) treatment (*p*-value = 0.2829).

Result 2: In the Abundance period, subjects exhibit in-group favoritism.

Comparing the average number reported across treatments (reported on the last column in Table 3.2), we find evidence of in-group favoritism in the Abundance period. The average number reported for the in-group member (3.39) is significantly higher than the one reported for the out-group member (2.72) (*p*-value = 0.0002).

This in-group favoritism is also evident in Figure 3.1. Subjects favor the in-group member in the Abundance period by reporting high payoffs significantly more often for the in-group member (73%) than for the out-group member (53%) (*p*-value = 0.0005). Subjects behave more favorably toward an anonymous person from their own village relative to an anonymous person from another village. This finding in the Abundance period is in line with the social identity theory (Tajfel and

Figure 3.1: Proportion of Subjects who Reported High Payoffs Across Treatments



Turner, 1979).

Result 3: Scarcity does not impact cheating for oneself or for the in-group member.

The average numbers reported for oneself and the in-group member are 4.15 and 3.39 in the Abundance period, and 4.07 and 3.27 in the Scarcity period respectively. The differences between the Scarcity and the Abundance periods are not significant for neither CheatingSelf (p-value = 0.5492) nor CheatingInGroup (p-value = 0.4641) treatments.

This can also be seen in Figure 3.1. Participants' cheating behavior for themselves is not statistically different across the two periods (89% vs. 85%) (p-value = 0.4142).[10] Additionally, we also find that cheating behavior for the in-group member is not different across the two periods (73% vs. 74%) (p-value = 0.8618). Although subjects cheat less for the in-group member than for themselves, this behavior is not different across periods, implying that scarcity does not affect participants' cheating behavior for themselves or for the in-group member.

Result 4: In-group favoritism fades in the Scarcity period.

In the Scarcity Period, the average numbers reported for the in-group member and the out-group member are 3.27 and 3.06 respectively and the difference is not statistically significant (p-value =

[10]This finding is line with Boonmanunt et al. (2018). In their experiment, when subjects were not reminded of social norms, their cheating behavior was not impacted by poverty.

0.3899).

Figure 3.1 shows that, in the Scarcity period, participants cheat for the out-group member (68%) (i.e. the frequency of high-paying numbers being reported is significantly different than 50%, *p*-value = 0.000) as much as they do for the in-group member (74%) (the difference is not significant, *p*-value = 0.2623). Scarcity sweeps away in-group favoritism. In-group favoritism disappears not because cheating for the in-group member decreases but because subjects cheat for the out-group member at the same rate as they do for the in-group member. In other words, subjects cheat significantly more for the out-group member in the Scarcity (68%) compared to the Abundance period (53%) (*p*-value = 0.0061). These findings suggest that scarcity produces a general empathy toward out-group members. We further explore this issue in a dictator game context in the following section.

Subjects cheat for themselves as well as for the in-group member, and this is not impacted by scarcity. However, even in an experiment like ours, where there is no risk of being caught and punished, participants do not cheat for others as much as they do for themselves. There are two potential explanations. First, people may be envious and prefer others to earn less than they do, which could also result in anti-social cheating. However, we do not see evidence for such behavior. Second, there may be non-monetary costs associated with cheating behavior, which is in line with lying aversion (Dufwenberg and Gneezy, 2000; Charness and Dufwenberg, 2006; Mazar and Ariely, 2006; Lundquist et al., 2009; Battigalli et al., 2013; Erat, 2013). The costs of favoring the in-group member in the cheating game treatments are non-monetary. In the next section, we also study the impact of scarcity on in-group favoritism when the cost of this preferential treatment is monetary.

3.3.3 Dictator Game Findings

In this section, we study the impact of scarcity on in-group favoritism using the dictator game described in Section 3.2.3.

Result 5: In the Abundance period, subjects are more generous toward the in-group member relative to the out-group member.

Figure 3.2: Average Dictator Giving Across Treatments

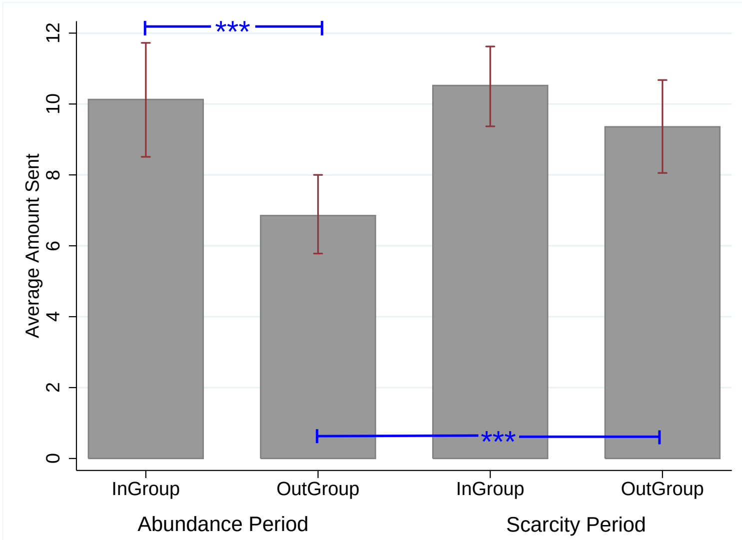


Figure 3.2 illustrates the average amount sent in the dictator game in each treatment across the Abundance and Scarcity periods. The amount sent to the in-group member (10.13Q) is significantly higher than the amount sent to the out-group member (6.85Q) during the Abundance period (p-value = 0.000). This is in line with the findings in the literature (e.g. Whitt and Wilson, 2007; Ben-Ner et al., 2009; Chen and Li, 2009; Chen and Chen, 2011; Balliet et al., 2014). While the environment in these papers is more analogous to our Abundance period, we need to be cautious about a one-to-one comparison of our findings to others that did not study for scarcity.

Result 6: In-group favoritism fades in the Scarcity period. This change is driven by a significant increase in giving toward the out-group member.

There is no significant in-group bias in pro-social behavior during the Scarcity period. While subjects send about 10.52Q to the in-group member, they send 9.36Q to the out-group member, and the difference is not statistically significant (p-value = 0.1219). Scarcity eliminates the in-group bias in pro-social behavior.

Again, and similar to the results of the cheating game, in-group bias disappears due to an increase in giving to the out-group member rather than a decrease in giving to the in-group member. The amount sent to the out-group member during the Scarcity period (9.36Q) is statistically higher

Table 3.3: OLS Regression of the Amount Sent in the Dictator Game

Variable	**Abundance**		**Scarcity**	
	(1)	(2)	(3)	(4)
Out-group Member	-3.275***	-3.433***	-1.165	-1.217
	(0.959)	(1.016)	(0.880)	(0.932)
Female		0.518		-2.541***
		(1.066)		(0.969)
Number of People in Household		-0.444**		0.371*
		(0.217)		(0.198)
Coffee Main Source of Income		-0.475		-2.630
		(2.164)		(1.968)
Risk		-0.0458		0.370
		(0.270)		(0.289)
Celebrations		2.112*		-1.788
		(1.195)		(1.190)
Stress		-0.730		-1.978*
		(0.914)		(1.071)
Constant	13.40***	16.36***	11.69***	17.14***
	(1.516)	(3.758)	(1.391)	(3.854)
No. Observations	218	194	218	184
No. people	109	97	109	92

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses. The dependent variable is the amount sent in the dictator game.

than the amount sent during the Abundance period (6.85Q) (*p*-value: 0.0069). Meanwhile, there is no difference in the amount sent to the in-group member between the Abundance and Scarcity periods (*p*-value = 0.5594). The latter finding is in line with Bartos (2016), who also looked at the impact of scarcity on giving behavior in the dictator game and found that scarcity does not impact giving. In his study, the recipient was someone from the same village as the participants. Thus, his findings can be compared to our InGroup treatment findings.

Table 3.3 presents the OLS regression results of the amount sent in the dictator game. We run the regressions separately for each period. The first two columns report Abundance period results while the last two report Scarcity period results. The dependent variable in all columns is the amount sent in the dictator game. The reference group is the InGroup treatment. Looking at the first column, we see that subjects sent about 3.3Q less to the out-group member compared to the in-group member in the Abundance period. This finding holds even after we control for some observables. This result indicates that subjects show a clear in-group favoritism in the dictator game by sending significantly less to the out-group member. Furthermore, this in-group favoritism goes away in the Scarcity period. The coefficient for the OutGroup treatment is no longer significant. Thus, in the Scarcity period, we do not find a significant in-group favoritism in dictator giving.

3.4 Discussion and Conclusion

Previous literature documents that people living under precarious conditions of scarcity tend to make suboptimal economic and financial decisions. Motivated by this emerging literature, we study the impact of scarcity on moral and pro-social behavior. More specifically, we study whether an individual's propensity to cheat originates mostly in individual characteristics or in the surrounding economic environment (i.e. scarcity). In addition, we also study the impact of scarcity on in-group favoritism in cheating and pro-social behavior.

People engage in dishonest behavior in various forms. In this paper, we focus on two types of cheating behavior. The first type results in a personal gain. While the plausible moral cost is borne by the individuals, the monetary cost is entirely assumed by the experimenters. Although this type of cheating does not create a negative externality on another subject, technically it cannot be considered a Pareto improvement since the increase in earnings is compensated by the experimenters from their research budgets. This is relevant in many economic settings. For example, people often misreport their income in order to pay lower taxes (Kettle et al., 2017), business executives misuse corporate accounts and make unnecessary charges (Litzky et al., 2006). In most of these cases, the monetary cost of cheating may not be salient to the individuals since the dishonesty hurts a large corporation or institution rather than another individual (Smigel, 1956).

The second type of cheating studied in this paper is pro-social cheating. Subjects have the opportunity to cheat to increase the payoff of another person, either an in-group or an out-group member, with neither monetary costs nor benefits to the decision maker. In this case, the cheating decision is made by comparing the utility coming from the pro-social act of increasing someone's earnings and the disutility coming from the moral cost of cheating.

Using a lab-in-the-field experiment, we study these two types of cheating behavior across periods of Scarcity and Abundance. A significant increase in our subjects' income during the Abundance period allows us to study the role of scarcity on cheating and pro-social behavior. In order to control for other potential factors changing across Scarcity and Abundance periods, we carefully selected a rural community located in Guatemala that experiences similar conditions across the two periods in terms of stress, risk, and physical activity levels.

We find that scarcity does not affect participants' cheating behavior for themselves. Contrary to Aristotle's quote at the beginning of the paper, our findings suggest that cheating in an effort to increase the participant's own well-being is not impacted by the economic environment. However, we also find that people cheat for others even though they do not directly benefit from it. While people cheat more for the in-group member relative to the out-group member during the Abundance period, this in-group favoritism in cheating vanishes during the Scarcity period. In fact, subjects do not cheat at all for the out-group member during the Abundance period, but they cheat for the out-group member during scarcity.

We also use a dictator game to study in-group favoritism in pro-social behavior. This allows us to study the impact of scarcity on in-group favoritism when the cost of this preferential treatment is monetary rather than moral. We find a similar pattern of behavior. While subjects send significantly more to the in-group member during the Abundance period, this gap is no longer statistically significant in the Scarcity period. Furthermore, the in-group favoritism in pro-social behavior is swept away by an increase in giving to the out-group member rather than a reduction in giving to the in-group member. Looking at the findings from both experimental games, we conclude that scarcity eliminates in-group bias in terms of pro-social and moral behavior.

One limitation of our study is that we do not study the mechanism behind these results. One potential explanation for our findings could be that scarcity may change and shift people's social identities. Future research can study how scarcity may impact social identity.

4. DO WORKERS EXERT MORE EFFORT FOR MISSION-ORIENTED JOBS?

4.1 Introduction and Literature Review

There is a large literature that studies worker motivation and the factors that encourage workers to exert higher levels of effort in the workplace. In general, this literature shows that there is a reciprocal relationship between the employee and employer, and employees provide more effort for higher wage levels. But workers may also exert more effort if their work has a mission. In this paper, we are interested in understanding the nature of the relationship between the employee and employer in two types of firms: for-profit and non-profit. More specifically, we ask whether workers exert more effort, for a given a wage level, when they work for a non-profit firm rather than a for-profit firm. We also ask whether managers who determine the wages offer different wage levels across these two types of firms. We study these questions by using a modified gift exchange environment where the decisions made by the worker and manager generate a payment to a third party who is either another subject in the lab (which represents working for a for-profit firm) or a non-profit organization.[1]

Our paper is closely related to the literature that studies the role that pro-social preferences play on worker motivation. For example, Banuri and Keefer (2016) find that workers with higher pro-social motives exert more effort in pro-socially motivated tasks. Similarly, Carpenter and Myers (2010) find that the decision to volunteer as a firefighter is correlated with altruism.[2] Tonin and Vlassopoulos (2010) reports that warm glow altruism and pure altruism have been the two sources of workers' pro-social motivation considered in the literature. They disentangle these two sources by using a controlled field experiment and find that men do not exhibit either of these pro-social motivations. On the other hand, women exert more effort due to warm glow altruism, but there is no additional impact coming from pure altruism.

[1]For a survey of lab labor experiments including gift-exchange game which is utilized in this paper, please see Charness and Kuhn (2011).

[2]A recent paper by Brown et al. (2018) investigate why people donate their time although the opportunity cost of their time is probably higher than the benefit created to the charity. They explain this by showing how people may have differential warm glow preferences depending on the form of the donation.

There are also some studies that compare worker's behavior across for-profit and non-profit sectors. Gregg et al. (2011) examine whether workers in non-profit firms behave more pro-socially than workers in for-profit firms by comparing the amount of unpaid overtime labor provided across these types of firms. They find that workers in the non-profit sector are more likely to do unpaid overtime. Cowley and Smith (2014), using data from world values survey, show that intrinsically motivated workers are more likely to work in the public sector. However, they report some variation across countries and argue that this variation could be partially explained by public corruption at those countries.

This raises the question of causality of whether more pro-social individuals select into the non-profit sector or whether they become more pro-social as a result of working in this sector. By comparing individuals' pro-social behavior after they change their sector, Gregg et al. (2011) shows that more pro-social individuals self-select into non-profit and public sectors. Banuri and Keefer (2016) also find that a real world pro-social organization attracts workers who are more pro-social. Additionally, Dur and Zoutenbier (2014, 2015) show that altruism plays a role into sorting into public sector. In a related strand of literature, researchers have studied how employers' decision to make a donation (i.e. corporate social responsibility (CSR)) impacts workers' motivation (e.g. Koppel and Regner, 2014, 2015; Tonin and Vlassopoulos, 2015; Charness et al., 2016; Kajackaite and Sliwka, 2017; Cassar, 2018).[3]

Literature also shows that although most workers care about the positive externality that their firms create, they may also care about working for the right mission (i.e. mission alignment). For example, Besley and Ghatak (2005) developed a theory regarding mission alignment and its impacts on worker motivation. They predict that workers self select into missions, and this mission-match enhances their efficiency at work. They show that if the workers are matched with the right mission, they work hard even when the financial incentives are little. However, high-powered incentives are needed to get workers to exert effort in the case of a mission mis-match. There have been some studies testing the implications of this model and the findings are generally in line

[3]See Kitzmueller and Shimshack (2012) for a comprehensive literature review on CSR.

with the predictions (e.g. Serra et al., 2011; Dur and Zoutenbier, 2014; Carpenter and Gong, 2016; Smith, 2016; Banuri et al., 2018).

In more closely related literature, Fehrler and Kosfeld (2014) study whether workers exert more effort if they choose the mission of their job. Using an experimental design that is very similar to ours, they do not find any impact. The biggest difference between their design and ours is that the random matching rule between the rounds. While they use a partners-matching design, ours is a stranger-matching design. We suggest that this difference in the matching rule is the driving force behind the differences between our findings. However, this claim should be approached with caution since further investigation is needed. In a second experiment, they introduce endogeneity where all subjects are assigned as workers and they decide whether they want to work for a profit (generate donations to another student) or a non-profit (generate donations to an NGO) firm.[4] They find that subjects who choose to work for a non-profit firm exert more effort. As a result, they state that self-selection into the non-profit sector is an important factor that could explain the empirical findings in this sector.

In another related paper, Gerhards (2015) finds that mission-match increases workers' efforts. In their experiments, subjects are either matched with a mission of their preference (mission match treatment) or a randomly and exogenously chosen mission (low mission match). Subjects exert more effort in the mission match treatment. They have another experiment which is similar to our paper. In this second experiment, subjects participate in the mission match and low mission match treatments (within-subjects design) and play multiple rounds with the perfect stranger matching rule. When the game is played repeatedly like this, they do not find any difference between the two treatments. On the contrary, in another closely related study, Cassar (2018) does not find any difference in the effort when the mission is matched compared to random mission assignment. She suggests that increasing the quality of the mission-match does not generate any further gains.

In this paper, we study whether workers exert more effort when they are *randomly* assigned to an exogenously chosen mission-oriented job. In our study, we randomly assign workers into

[4]Modifying the gift-exchange game like this takes away the reciprocity between the worker and the employer. It would be interesting to test the robustness of these findings by using a design similar to their first experiment.

either a non-profit firm or a for-profit firm. Thus, in our environment, self-selection into a mission is not possible. In line with the prior literature, workers exert more effort for high wages in both treatments. Similar to Cassar (2018), we also find that pro-social mission results in higher effort, but only if the wage paid is high. In contrast to Cassar (2018), we find that managers offer the same wages across the two treatments and thus their behavior is not impacted by the mission of the firm. This results in higher profits generated in the non-profit treatment.

4.2 Experimental Design

We use a modified version of the Charness et al. (2004) gift exchange experiment. In this modified version, there are three roles a subject can take to which they are randomly assigned: a worker, a manager, and a firm owner. Subjects are put in groups of three that consist of one worker, one manager, and one firm owner. First, the manager determines a wage level to be paid to the worker. Then, the worker observes the wage and decides how much effort to provide. Both the wage paid and the effort level provided determines the earnings for all three group members. The payoff functions are as follows:

$$\pi_W = \text{wage} - c(e) \tag{4.1}$$

$$\pi_M = 0.40 \ \times \text{Profit} \tag{4.2}$$

$$\pi_F = 0.60 \ \times \text{Profit} \tag{4.3}$$

$$\text{Profit} = 2 \times e \times (100 - w) \tag{4.4}$$

where W, M, and F represent worker, manager and firm owner respectively; and $c(e)$ denotes the cost of providing the effort level, e. Worker receives the wage ($wage \in \{10, 20, 30, 40, 50, 60\}$) determined by the manager and bears the cost of their chosen effort level. We use the Charness et al. (2004) cost of effort schedule which is shown in Table 4.1.

While the wage increases the worker's payoff, it decreases the profit. Both wage and effort

Table 4.1: Worker's Cost of Effort Schedule

e	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
c(e)	0	1	2	4	6	8	10	12	15	18

determine the profit which in turn determines the earnings for the manager and the firm owner. The profit is calculated according to eq. (4.4) and is shared between the manager and the firm owner. The firm owner receives 60% of the profit and the manager receives the remaining 40%. In this game, the firm owner does not make any decisions. She simply collects her share of the profit.

First, the roles are assigned randomly at the beginning of the experiment and kept the same for the duration of the experiment. Next, subjects are placed in groups of three that consist of one worker, one manager and one firm owner. Subjects play this game for 20 rounds and are paid at the end for two randomly selected rounds. Although the roles are fixed, groups are re-matched randomly in each round. At the end of each round, we provide feedback about the wage chosen, effort provided, and the earnings.

We have two treatments: Profit Treatment and Non-Profit Treatment. The only difference between the two treatments is the identity of the firm owner. In the profit treatment, the firm owner is another subject in the lab. Whereas in the non-profit treatment, the firm owner is a non-profit organization. We chose Operation Kindness, which is the largest and oldest no-kill animal shelter in North Texas, as the non-profit organization for this experiment. At the end of the experiment, we randomly pick one of the subjects to be the monitor. The monitor is paid an extra $5 to stay a little longer to make sure that earnings generated for Operation Kindness are donated to Operation Kindness on the organization's website.

4.3 Results

We ran a total of eleven sessions in the Economic Research Lab at Texas A&M University in February and March 2018 with a total of 251 subjects. 141 subjects participated in the profit treatment and the remaining 110 participated in the non-profit treatment. Thus, we have 47 workers and managers in the profit treatment; and 55 workers and managers in the non-profit treatment.

Table 4.2: Average Effort

Treatment	Wage Paid					
	10	20	30	40	50	60
Profit	0.17 (0.18) n=41	0.21 (0.13) n=38	0.31 (0.18) n=47	0.41 (0.19) n=47	0.51 (0.23) n=43	0.59 (0.33) n=42
Non-Profit	0.15 (0.10) n=46	0.21 (0.11) n=45	0.34 (0.18) n=53	0.49 (0.16) n=55	0.64 (0.21) n=55	0.72 (0.28) n=47
p-values †	*0.422*	*0.968*	*0.364*	*0.030*	*0.014*	*0.042*
p-values ‡	*0.882*	*0.694*	*0.286*	*0.031*	*0.006*	*0.054*

Standard deviations are in parentheses. †Bootstrapped t-test ‡ Mann-Whitney test

The experiment was programmed in z-tree (Fischbacher, 2007), and the undergraduate students at Texas A&M University were recruited through ORSEE (Greiner et al., 2004). Subjects earned $19 on average including a $10 show-up fee.

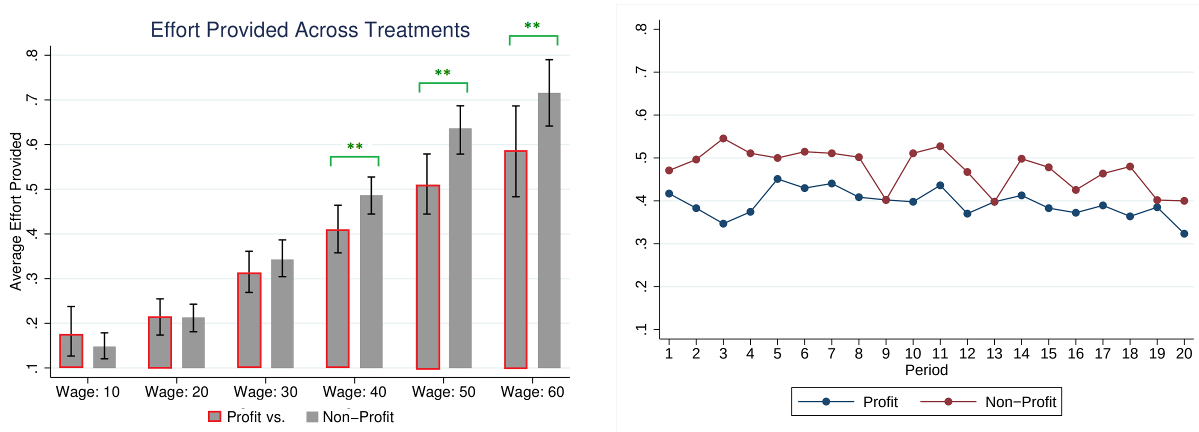
Similar to previous studies using the gift-exchange game (or variants of it), we do not find support for Nash equilibrium (NE) predictions either. Although the managers are predicted to offer the lowest possible wage of 10, the average wage offered across both treatments is 38.65 tokens. Similarly, the workers provide significantly higher efforts (the average across both treatments is 0.44) than the NE of 0.1. In what follows, we first present findings on workers' behavior, then we present the findings on managers' behavior and finally present and discuss the impact of these observed behavior on firm profits.

4.3.1 Workers

As mentioned above, workers provide significantly higher efforts on average than the NE of 0.1. Table 4.2 and Figure 4.1 (a) show the average effort provided across treatments and for each wage offered. In both treatments, there is a positive relationship between the wage offered and the effort provided. This reciprocal relationship that we observe is similar to the findings in the literature. When we compare the effort levels across treatments, we notice that the treatment does not have an impact on effort for wages lower than 40. However, workers provide significantly

Figure 4.1: Average Effort Provided

(a) (b)



higher levels of effort if the wage offered is 40 or higher.

The distribution of effort levels provided across treatments can be found in Figure B.3 for low wages and Figure B.4 for high wages in the appendix. Using the Epps-Singleton test[5], we compare these distributions across profit and non-profit treatments. We find that the distributions are not statistically significantly different if the wage offered is 10 (*p-value*: 0.105) or 20 (*p-value*: 0.311). On the other hand, the distributions of efforts are significantly different across profit and non-profit treatments if the managers offer 30 (*p-value*: 0.000), 40 (*p-value*: 0.000), 50 (*p-value*: 0.001), or 60 (*p-value*: 0.002).

Next, we look at the average effort provided over time. Looking at Figure 4.1 (b), we see that the behavior seems fairly consistent with slight decline over time. Although there are some fluctuations, the average effort provided in the non-profit treatment is mostly above the average effort in the profit treatment.

To check the robustness of our findings, we also present the regression results. In our experiment, workers cannot provide an effort lower than 0.1 or higher than 1. Thus, by using a panel data Tobit model, we take this censuring into account. Table 4.3 presents the results. In both Panel A and B, the dependent variable is *Worker Effort* which is the level of effort provided by the worker.

[5]Findings are similar if we use the Kolmogorov-Smirnov test.

Table 4.3: Panel Data Tobit Regression Results

	(1)	(2)	(3)	(4)
Panel A				
DV: Worker Effort				
Wage	0.0149***	0.0151***	0.0151***	
	(0.000771)	(0.000859)	(0.000798)	
Non-Profit	0.106**	0.0987**	0.473**	
	(0.0536)	(0.0432)	(0.187)	
Period		-0.00776***	-0.00777***	
		(0.00137)	(0.00152)	
Female		-0.0848*	-0.0665	
		(0.0479)	(0.0563)	
Society Oriented			0.110***	
			(0.0393)	
Society Oriented*Non-Profit			-0.0987**	
			(0.0474)	
Constant	-0.247***	-0.126**	-0.550***	
	(0.0485)	(0.0643)	(0.156)	
Observations	2040	2040	2040	
Panel B				
DV: Worker Effort				
High Wage (40-60)	0.376***	0.334***	0.344***	0.344***
	(0.0222)	(0.0214)	(0.0333)	(0.0304)
Non-Profit	0.113***	0.0622*	0.0580	0.444**
	(0.0405)	(0.0366)	(0.0508)	(0.173)
High Wage*Non-Profit		0.0733**	0.0701	0.0695*
		(0.0340)	(0.0434)	(0.0365)
Period			-0.00794***	-0.00795***
			(0.00118)	(0.00132)
Female			-0.0775*	-0.0587
			(0.0404)	(0.0495)
Society Oriented				0.113***
				(0.0333)
Society Oriented*Non-Profit				-0.102**
				(0.0450)
Constant	0.0858**	0.114***	0.237***	-0.198
	(0.0410)	(0.0368)	(0.0464)	(0.141)
Observations	2040	2040	2040	2040

Robust errors standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Wage is the wage offered by the manager to the worker in that period. *Non-Profit* is the dummy variable that takes the value of 1 for the non-profit treatment, otherwise 0. *Period* is the trend variable, *Female* is the dummy variable for females. *Society Oriented* is constructed by using the answers to the following item from the PSM (Public Service Measure) (Perry, 1996): "Making a difference in society means more to me than personal achievements".[6] It is between 1 (Strongly Disagree) and 5 (Strongly Agree).

First, looking at Panel A, we see that workers are responsive to the wages offered. Workers provide significantly higher effort for higher wage levels. Additionally, we see that workers provide significantly higher effort when they are in the non-profit treatment compared to the profit treatment. Interestingly, we also find that caring about making a difference in society (i.e. being society oriented) does not impact behavior in the non-profit treatment (the summation of the coefficients of *Society Oriented* and *Society Oriented*Non-Profit* is not significantly different from zero). On the other hand, society-oriented individuals provide significantly higher levels of effort when they are in the profit treatment.

We are also interested in the workers' responsiveness to the wages in the non-profit treatment compared to the profit treatment. We do not find a significant difference across the two treatments (see Table C.5 in appendix).[7]

In Panel B of Table 4.3, we use a different measure for the wages. Instead using the actual wage offered, we construct a dummy variable which takes the value of 1 if the wage offered was high (i.e. 40, 50, or 60), and otherwise zero. We find that subjects are more responsive to high wages in the non-profit treatment compared to the profit treatment.

4.3.2 Managers and Profits

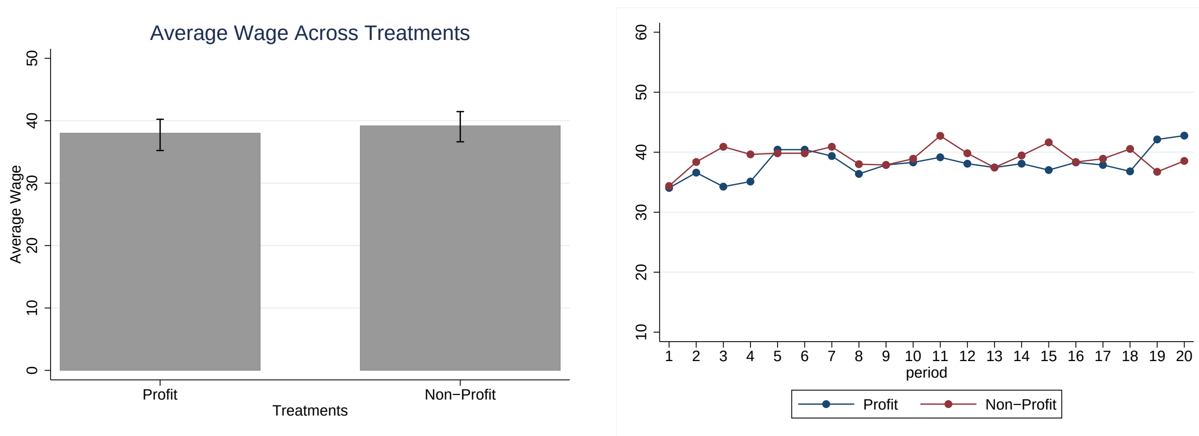
In this section, we first study the managers' behavior and then compare the profits created across two treatments. Figure 4.2 (a) shows the average wage paid across treatments. Managers paid workers 38 and 39 tokens, on average, in the profit and non-profit treatments respectively, and they are not statistically different from one another (Mann-Whitney test *p-value:* 0.207). Figure

[6]This item is listed as PSM1 under the self-sacrifice subscale in Perry (1996).

[7]However, when we only include the first 15 periods, we find that workers in the non-profit treatment are significantly more responsive to the wages compared to the profit treatment. Regressions using the first 15 periods are presented on Table C.6.

Figure 4.2: Average Wage Offered

(a) (b)



4.2 (b) shows the average wage offered over time across treatments. Average wages seem fairly consistent over time and across treatments.

To check the robustness of these findings, we also ran a panel data tobit model regression where the dependent variable is the wage paid in each period. Average marginal effects derived from these regressions are presented in Table 4.4. According to these results, wages paid across

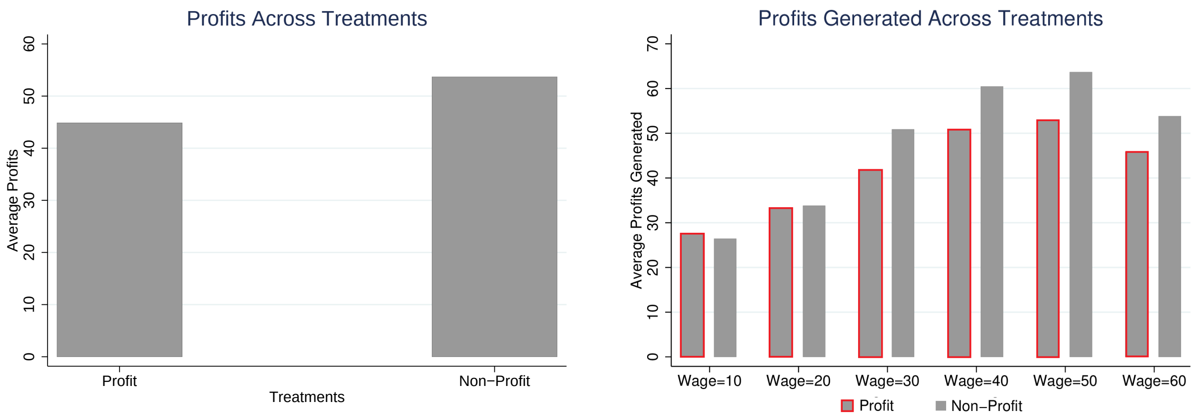
Table 4.4: Panel Data Tobit Regression Results for Wage

	(1)	(2)
DV:	*Wage*	*Wage*
Non-Profit	0.747	-0.447
	(2.411)	(2.625)
Female		-1.639
		(2.599)
Period		0.135*
		(0.0692)
Lagged Effort		13.82***
		(1.777)
Constant	38.68***	32.70***
	(1.815)	(2.583)
Observations	2040	2040

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4.3: Average Profits Generated Across Treatments

(a) (b)



treatments are not statistically different. Although we see that managers respond positively to the effort provided in the previous round, we do not find any evidence that managers respond to the treatment.

Next, we compare the profits generated in the profit and non-profit treatments. Figure 4.3 (a) shows the average profits generated across treatments. Average profits are 44.85 and 53.67 tokens in the profit and non-profit treatments respectively. Looking at the first column of Table 4.5, we see that profits are significantly higher in the non-profit treatment. We can also compare the profits generated across different wage levels. This is illustrated in Figure 4.3 (b), and regression analysis results are provided in the second column of Table 4.5. We find that wages of 40 and 50 result in the highest profits generated in both treatments.

4.4 Conclusion

In this paper, we study whether workers exert more effort when they work for a non-profit vs. a for-profit firm when they are randomly assigned to these firms. We find that workers exert higher levels of effort in the non-profit treatment only when the wages are high. For low wages, we do not find a significant difference in effort levels between the two treatments. Interestingly, managers do not respond to the treatment so the average wages paid across the two treatments are

Table 4.5: Panel Data Tobit Regression Results for Firm Profits

	(1)	(2)
DV: Profits		
Non-Profit	9.255**	0.291
	(4.035)	(5.448)
Period	-0.592***	-0.610***
	(0.128)	0.104
Wage 20		6.422
		(3.908)
Wage 30		13.513***
		(4.013)
Wage 40		20.689***
		(5.098)
Wage 50		21.891***
		(5.880)
Wage 60		16.269**
		(6.492)
Wage 20*Non-Profit		-1.273
		(5.020)
Wage 30*Non-Profit		5.214
		(5.124)
Wage 40*Non-Profit		10.372*
		(6.078)
Wage 50*Non-Profit		12.653*
		(7.387)
Wage 60*Non-Profit		10.220
		(8.161)
Constant	50.1060***	34.961***
	(3.080)	(4.185)
Observations	2,040	2,040

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

not statistically different. This results in higher profits generated in the non-profit treatment.

5. CONCLUSIONS

In the first essay of this dissertation, we investigate the impact of information about the MPCR of a linear public good on contributions. The theoretical model predicts that information provision has a differential impact on less and more generous groups. While information increases average contributions by less generous subject groups, it reduces average contributions by more generous subject groups. We experimentally test these hypotheses in the lab and the findings are in line with the theoretical expectations. We find that information does not impact public good contribution on the extensive margin. However, information impacts public good contributions on the intensive margin and the sign of this impact depends on the generosity level of the sessions. In the relatively selfish sessions, subjects who contributed at least once contribute more on average when they are informed compared to when they are uninformed of the value of the public good. However, just the opposite is true for the relatively generous sessions. In these sessions, subjects who are potential contributors contribute less to the public good when they are informed. This is because their relative response to bad news is greater than their response to good news.

The findings of this study have significant implications for fundraising. In particular, they suggest that targeted information provision may be a more fruitful strategy of increasing public good contributions than uniform information provision. Since donors themselves may be able to acquire information by conducting research about non-profits prior to contributing, an important direction for future research includes endogenizing the choice of information acquisition by donors. This would allow us to glean further insight about the impact of information on public good provision by studying how information acquisition incentives differ across donors.

Previous literature documents that people living under precarious conditions of scarcity tend to make suboptimal economic and financial decisions. Motivated by this emerging literature, the second essay studies the impact of scarcity on moral and pro-social behavior. In addition, we also study the impact of scarcity on in-group favoritism in cheating and pro-social behavior. Using a lab-in-the-field experiment, we study cheating behavior across periods of Scarcity and Abundance.

We find that scarcity does not affect participants' cheating behavior for themselves. Our findings suggest that cheating in an effort to increase the participant's own well-being is not impacted by the economic environment. However, we also find that people cheat for others even though they do not directly benefit from it. While people cheat more for the in-group member relative to the out-group member during the Abundance period, this in-group favoritism in cheating vanishes during the Scarcity period. In fact, subjects do not cheat at all for the out-group member during the Abundance period, but they cheat for the out-group member during scarcity.

In this study, we also use a dictator game to study in-group favoritism in pro-social behavior. This allows us to examine the impact of scarcity on in-group favoritism when the cost of this preferential treatment is monetary rather than moral. We find a similar pattern of behavior. While subjects send significantly more to the in-group member during the Abundance period, this gap is no longer statistically significant in the Scarcity period. Furthermore, the in-group favoritism in pro-social behavior is swept away by an increase in giving to the out-group member rather than a reduction in giving to the in-group member. Looking at the findings from both experimental games, we conclude that scarcity eliminates in-group bias in terms of pro-social and moral behavior.

The third essay investigates whether workers exert more effort when they work for a mission-oriented job using a modified gift-exchange experiment. We find that workers exert higher levels of effort in the non-profit treatment only when the wages are high. For low wages, we do not find a significant difference in effort levels between the two treatments. Interestingly, managers do not respond to the treatment so the average wages paid across the two treatments are not statistically different. This results in more profits being generated in the non-profit treatment at high wage levels.

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APPENDIX A

PROOFS

Proof of Lemma 1. To show that $\overline{g}(v)$ is increasing in v, note by eq. (4.3) that

$$\overline{g}'(v) = -\overline{g}(v)\frac{R'(v)}{1+R(v)} \tag{A.1}$$

Moreover, since $\beta_i \sim Exp(1/\lambda)$, $R(v) = \frac{1-e^{-\beta_1(v)/\lambda}}{e^{-\beta_2(v)/\lambda}}$. Therefore, differentiating $R(v)$ with respect to v yields

$$\begin{aligned} R'(v) &= \frac{1}{\lambda}\beta_2'(v)e^{\beta_2(v)/\lambda} - \frac{1}{\lambda}\left[\beta_2'(v) - \beta_1'(v)\right]e^{(\beta_2(v)-\beta_1(v))/\lambda} = \\ &= -\frac{N(N-1)}{(Nv-1)^2}\frac{1}{\lambda}\left[e^{\beta_2(v)/\lambda} + \gamma R(v)\right] < 0 \end{aligned} \tag{A.2}$$

where the last equality takes into account that $\beta_1'(v) = -\frac{N(N-1)}{(Nv-1)^2} < 0$ and $\beta_2'(v) = -\frac{N(N-1)}{(Nv-1)^2}(1+\gamma) < 0$. Given $R'(v) < 0$, eq. (A.1) implies that $\overline{g}'(v) > 0$.

To show that $\lim_{v\to\frac{1}{N}}\overline{g}(v) = 0$, we need to show that $\lim_{v\to\frac{1}{N}} R(v) = \infty$. Note that $\lim_{v\to\frac{1}{N}}\beta_1(v) = \lim_{v\to\frac{1}{N}}\beta_2(v) = \infty$. Therefore, $\lim_{v\to\frac{1}{N}} e^{-\beta_2(v)/\lambda} = \lim_{v\to\frac{1}{N}} e^{-\beta_1(v)/\lambda} = 0$, resulting in $\lim_{v\to\frac{1}{N}} R(v) = \infty$. To see that $\lim_{v\to 1}\overline{g}(v) = W$ note that $\lim_{v\to 1}\beta_1(v) = 0$ and $\lim_{v\to 1}\beta_2(v) = \gamma$. This implies that $\lim_{v\to 1} R(v) = 0$ and $\lim_{v\to 1}\overline{g}(v) = W$.

To establish the existence and uniqueness of $\tilde{v}(\lambda)$ and its corresponding properties, we first derive $\overline{g}''(v)$ by differentiating $\overline{g}'(v)$ with respect to v, yielding

$$\overline{g}''(v) = \frac{\overline{g}(v)}{(1+R(v))}\left[2\frac{[R'(v)]^2}{1+R(v)} - R''(v)\right] \tag{A.3}$$

Differentiating eq. (A.2) with respect to v and simplifying yields

$$R''(v) = \frac{N^2(N-1)^2}{\lambda^2(Nv-1)^4}\left[(e^{\beta_2(v)/\lambda} + \gamma R(v))\left(\frac{2\lambda(Nv-1)}{(N-1)} + \gamma\right) + (1+\gamma)e^{\beta_2(v)/\lambda}\right] \tag{A.4}$$

Subsituting for $R'(v)$ and $R''(v)$ in eq. (A.3) and simplifying results in

$$\begin{aligned} \overline{g}''(v) &= \frac{\overline{g}(v)}{(1+R(v))}\frac{N^2(N-1)^2}{\lambda^2(Nv-1)^4}[e^{\beta_2(v)/\lambda} + \gamma R(v)] \times \\ &\left[2\frac{e^{\beta_2(v)/\lambda} + \gamma R(v)}{1+R(v)} - \frac{(1+\gamma)e^{\beta_2(v)/\lambda}}{e^{\beta_2(v)/\lambda} + \gamma R(v)} - \frac{2\lambda(Nv-1)}{(N-1)} - \gamma\right] \end{aligned}$$

Note that

$$g''(v) \overset{sign}{=} \left[2\frac{e^{\beta_2(v)/\lambda} + \gamma R(v)}{1+R(v)} - \frac{(1+\gamma)e^{\beta_2(v)/\lambda}}{e^{\beta_2(v)/\lambda} + \gamma R(v)} - \frac{2\lambda(Nv-1)}{(N-1)} - \gamma\right] = \Omega(v,\lambda).$$

To show the uniqueness of $\tilde{v}(\lambda)$, we first show that $\Omega(v,\lambda)$ is strictly decreasing in v, implying that there is at most one solution to $\overline{g}''(v) = 0$. Substituting for $R(v)$ in the above expression and further simplifying yields

$$\Omega(v,\lambda) = 2\frac{1+\gamma(1-e^{-\beta_1(v)/\lambda})}{1+e^{-[\beta_1(v)+\beta_2(v)]/\lambda}} - \frac{1+\gamma}{1+\gamma(1-e^{-\beta_1(v)/\lambda})} - \frac{2\lambda(Nv-1)}{(N-1)} - \gamma \tag{A.5}$$

It is immediately evident that $\Omega(v,\lambda)$ is strictly decreasing in v since $\beta_1'(v) < 0$ and $\beta_2'(v) < 0$. Thus, there is at most one solution to $\Omega(v,\lambda) = 0$.

To establish the existence of $\tilde{v}(\lambda)$, note that

$$\lim_{v\to\frac{1}{N}} \Omega(v,\lambda) = 1+\gamma > 0, \tag{A.6}$$

since $\lim_{v\to\frac{1}{N}} \beta_1(v) = \lim_{v\to\frac{1}{N}} \beta_2(v) = \infty$, and

$$\lim_{v\to 1}\Omega(v,\lambda) = \frac{2}{1+e^{-\lambda/\gamma}} - 2/\lambda - 1, \tag{A.7}$$

since $\lim_{v\to 1}\beta_1(v) = 0$ and $\lim_{v\to 1}\beta_2(v) = \gamma$. It is straightforward to verify that $\lim_{v\to 1}\Omega(v,\lambda)$ is strictly decreasing in λ and takes negative values for all $\lambda > \tilde{\lambda}$ where $\tilde{\lambda} \in (0,\infty)$ solves

$$\lim_{v\to 1}\Omega(v,\tilde{\lambda}) = 0.$$

Thus, for $\lambda > \tilde{\lambda}$, $\tilde{v}(\lambda)$ uniquely solves $\Omega(\tilde{v}(\lambda),\lambda) = 0$ and $\tilde{v}(\lambda) \in (\frac{1}{N},1)$, while for $\lambda < \tilde{\lambda}$, $\Omega(v,\lambda) > 0$ for all $v \in (\frac{1}{N},1)$ and thus $\tilde{v}(\lambda) = 1$. This establishes the existence of a unique $\tilde{v}(\lambda) \in \left(\frac{1}{N},1\right]$ with $g''(v) > 0$ for $v < \tilde{v}(\lambda)$ and $g''(v) < 0$ for $v > \tilde{v}$, proving property 1).

To establish property 2, note first that for $\lambda < \tilde{\lambda}$ $\tilde{v}(\lambda) = 1$. For $\lambda > \tilde{\lambda}$ implicit differentiation of $\Omega(\tilde{v}(\lambda),\lambda) = 0$ results in

$$\tilde{v}'(\lambda) = -\frac{\partial\Omega(v,\lambda)/\partial\lambda}{\partial\Omega(v,\lambda)/\partial v}$$

Recall that $\partial\Omega(v,\lambda)/\partial v < 0$. Moreover, straighforward differentiation reveals that $\partial\Omega(v,\lambda)/\partial\lambda < 0$. Therefore, it follows immediately that $\tilde{v}'(\lambda) < 0$.

The property $\lim_{\lambda\to 0}\tilde{v}(\lambda) = 1$ follow immediately from the fact that $\tilde{v}(\lambda) = 1$ for $\lambda < \tilde{\lambda} \in (0,\infty)$.

Finally, to establish that $\lim_{\lambda\to\infty}\tilde{v}(\lambda) = \frac{1}{N}$, note that

$$\lim_{\lambda\to\infty}\Omega(v,\lambda) = \lim_{\lambda\to\infty} -2\frac{Nv-1}{N-1}\lambda$$

By definition, $\Omega(\tilde{v}(\lambda),\lambda) = 0$ for $\lambda > \tilde{\lambda}$. Therefore,

$$\lim_{\lambda\to\infty}\Omega(\tilde{v}(\lambda),\lambda) = \lim_{\lambda\to\infty} -2\frac{N\tilde{v}(\lambda)-1}{N-1}\lambda = 0 \Longrightarrow \lim_{\lambda\to\infty}\tilde{v}(\lambda) = \frac{1}{N}$$

□

Proof of Proposition 1. Given $\frac{1}{N} < v_L < v_H < 1$, by Lemma 1, there exist $\lambda_1 > 0$ be such that

$\tilde{v}(\lambda_1) = v_H$ and $\lambda_2 > \lambda_1$ such that $\tilde{v}(\lambda_2) = v_L$. Furthermore, by Lemma 1, $\overline{g}(v)$ is convex for all $v < v_H$ if $\lambda \leq \lambda_1$. Thus, by definition of convexity,

$$p_L \overline{g}(v_L) + p_H \overline{g}(v_H) > \overline{g}(p_L v_L + p_H v_H)$$

Analogously, for $\lambda \geq \lambda_2$, $\overline{g}(v)$ is concave for all $v \geq v_L$, implying the reverse inequality.

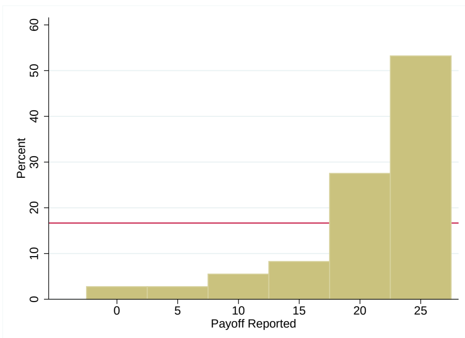
□

APPENDIX B

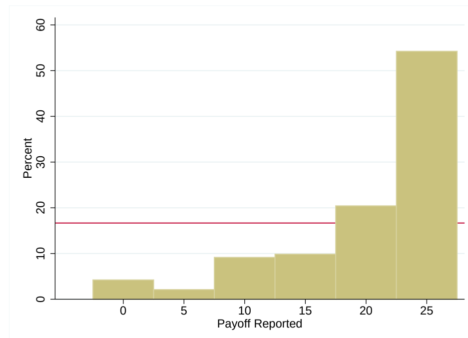
FIGURES

Figure B.1: Distributions of Payoffs Reported in Cheating Game Treatments

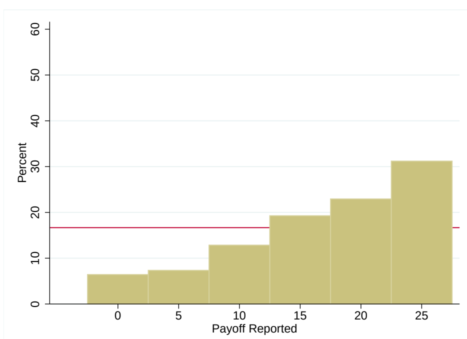
(a) Self Abundance



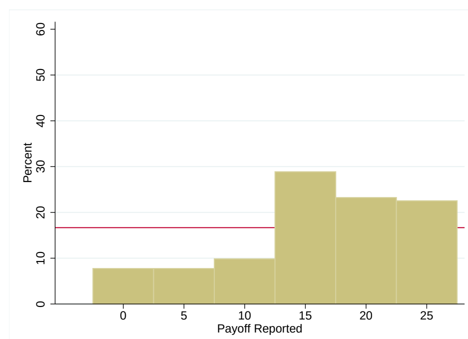
(b) Self Scarcity



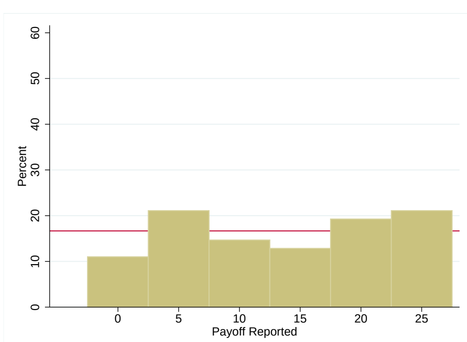
(c) In Group Abundance



(d) In Group Scarcity



(e) Out Group Abundance



(f) Out Group Scarcity

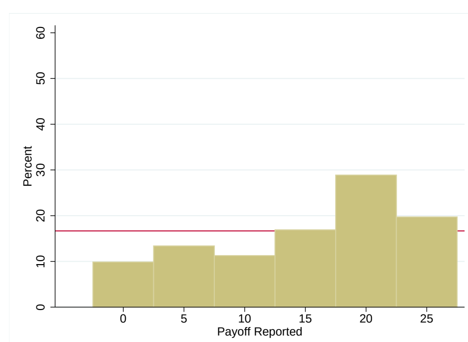
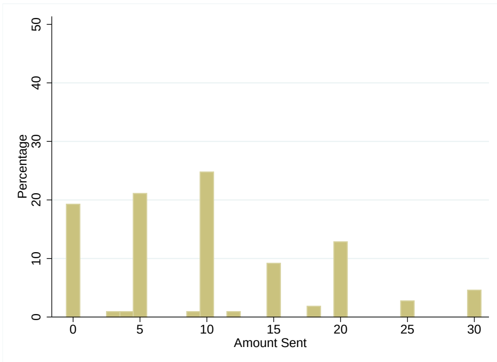
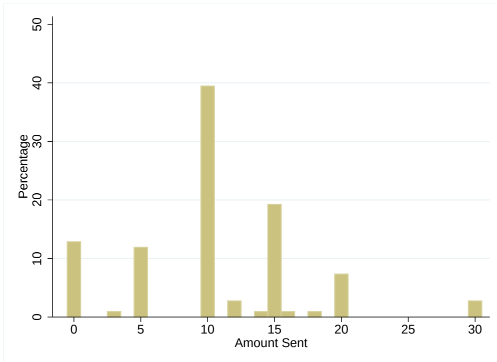


Figure B.2: Distributions of Amount Sent in Dictator Game Treatments

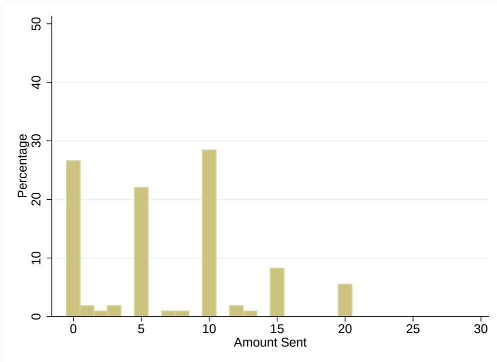
(a) In Group Abundance



(b) In Group Scarcity



(c) Out Group Abundance



(d) Out Group Scarcity

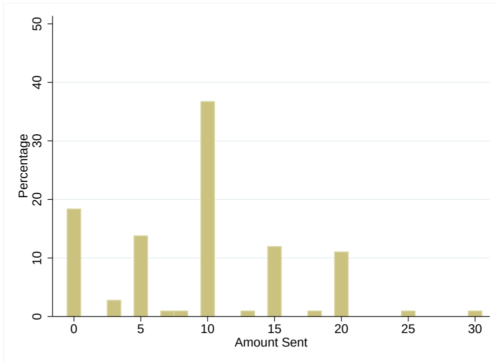
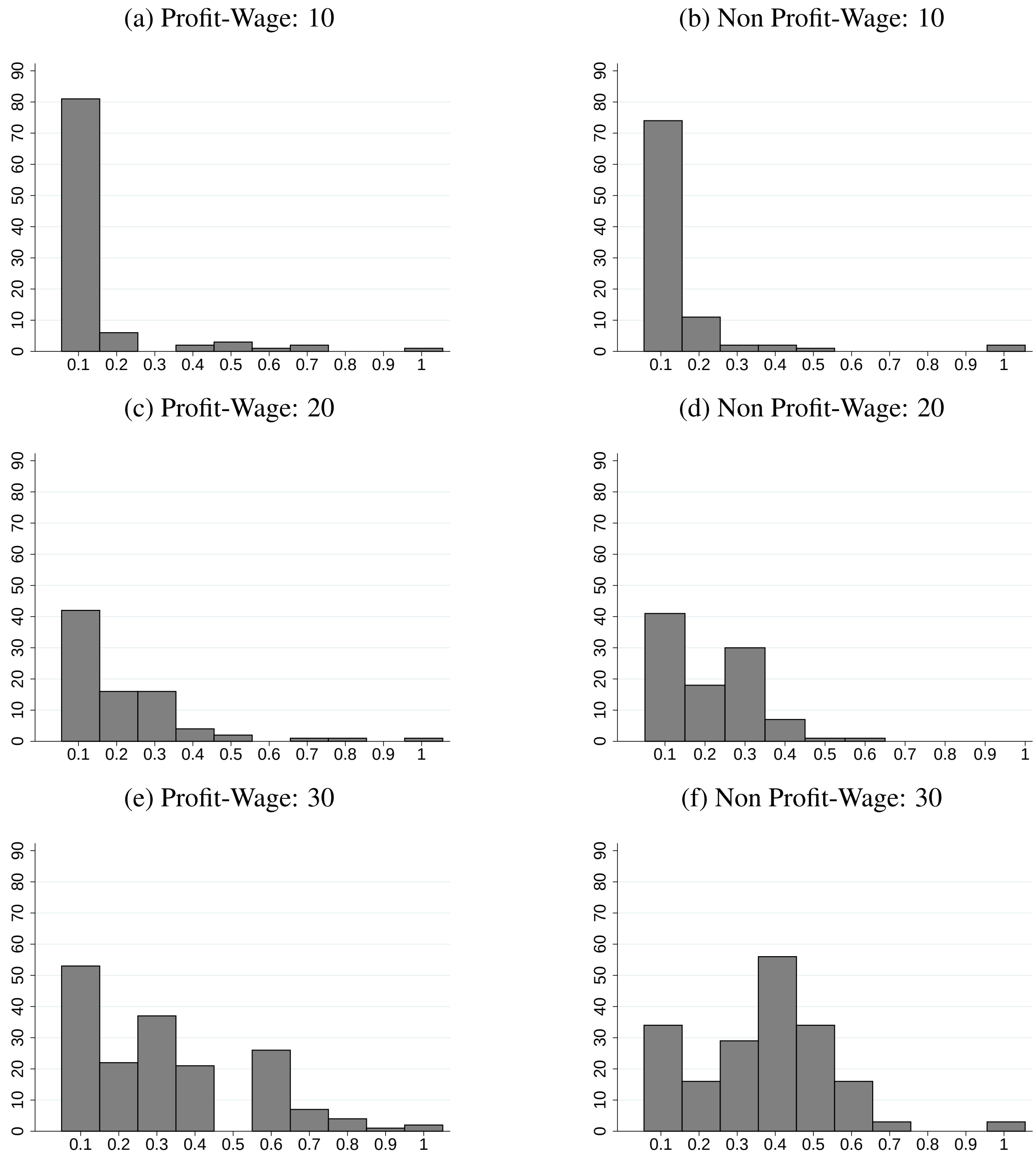


Figure B.3: Frequency Distributions of Worker Effort Provided Across Low Wages and Treatments



(a) Profit-Wage: 10

(b) Non Profit-Wage: 10

(c) Profit-Wage: 20

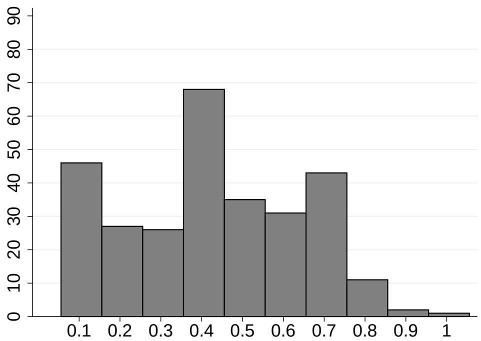
(d) Non Profit-Wage: 20

(e) Profit-Wage: 30

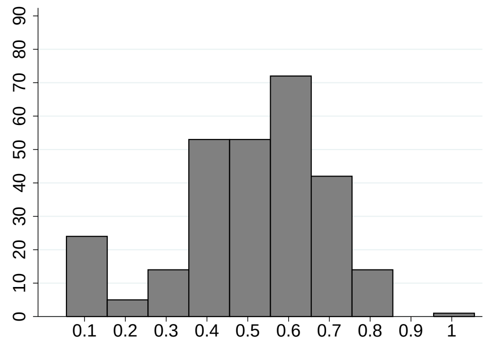
(f) Non Profit-Wage: 30

Figure B.4: Frequency Distributions of Worker Effort Provided Across High Wages and Treatments

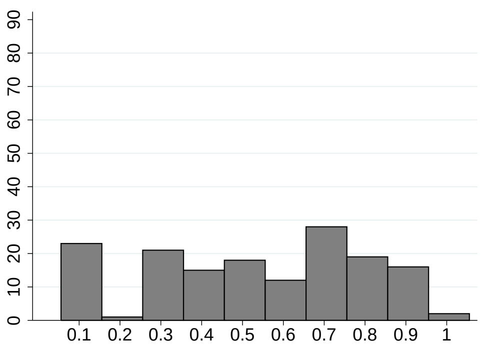
(a) Profit-Wage: 40



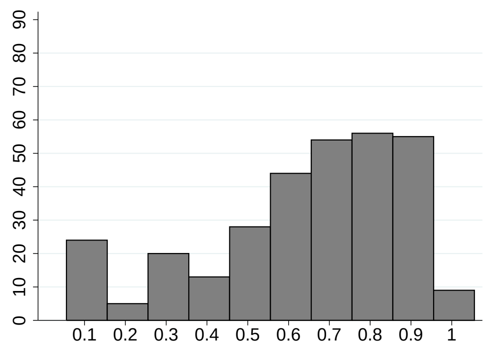
(b) Non Profit-Wage: 40



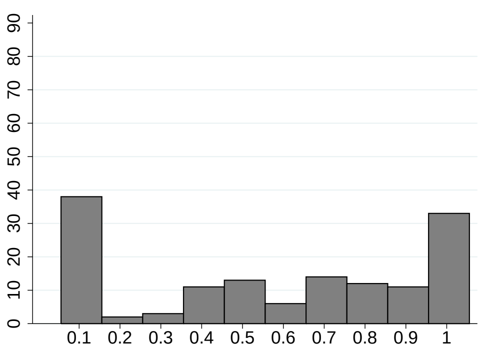
(c) Profit-Wage: 50



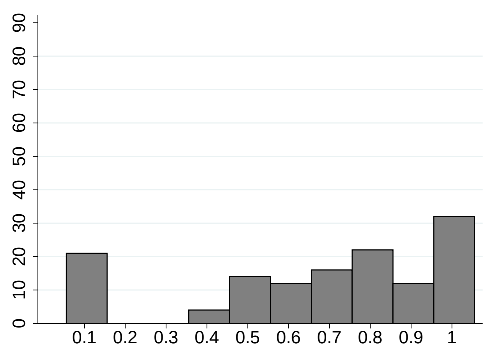
(d) Non Profit-Wage: 50



(e) Profit-Wage: 60



(f) Non Profit-Wage: 60



APPENDIX C

TABLES

Table C.1: Comparing Subjects who Participated in Scarcity Only vs. Both Periods

Variable	Scarcity Only	Both Periods	*p*-value
Female	0.27 (0.45)	0.41 (0.50)	0.1463†
Yearly Income	9,174 (7,906)	8,242 (7,794)	0.5531‡
Main Source of Income Coffee	0.97 (0.18)	0.94 (0.23)	0.5732 †
Finances Relative to Others	2.23 (0.43)	2.19 (0.57)	0.8219‡
Household Financial Situation	2.69 (0.65)	2.87 (0.61)	0.1595‡
No Money Index	2.22 (1.41)	2.17 (1.35)	0.9599 ‡
No Money for Food	0.41 (0.50)	0.40 (0.49)	0.9791 †
No Money for Basic Needs (non-food)	0.38 (0.49)	0.57 (0.50)	0.0536 †
No Money for Medical Expenses	0.56 (0.50)	0.48 (0.50)	0.3954 †
No Money for Farm	0.88 (0.34)	0.73 (0.45)	0.0807 †
Credit	0.19 (0.40)	0.17 (0.38)	0.8002†
Risk	3.16 (1.80)	2.91 (1.58)	0.5886 ‡
Stress Index	1.89 (0.48)	1.92 (0.46)	0.6019 ‡
Celebratory Events	0.78 (0.42)	0.82 (0.39)	0.6165 †
Cheating for Self	0.82 (0.39)	0.85 (0.36)	0.6261 †
Cheating for In Group	0.76 (0.44)	0.74 (0.44)	0.8672 †
Cheating for Out Group	0.58 (0.50)	0.68 (0.47)	0.2748 †
Dictator Giving -In Group	11.34 (7.38)	10.52 (6.38)	0.5758‡
Dictator Giving -Out Group	11.47 (6.22)	9.36 (6.61)	0.0969‡
Number of Subjects	31-33	97-109	

†Two-sample test of proportions. ‡Two-sample Wilcoxon rank-sum (Mann-Whitney) test. Standard deviations are in parentheses.

Table C.2: Description of the Survey Measures and Risk Preferences

Variables	Description
Finances Relative to Others	1-Better, 2-Similar, 3-Worse
Household Financial Situation	1-Excellent, 2-Good, 3-Not so good, 4-Poor
No Money Index	Summation of the following four
No Money for Food	1-Experienced this situation in the last month, 0-otherwise
No Money for Basic Needs (non-food)	1-Experienced this situation in the last month, 0-otherwise
No Money for Medical Expenses	1-Experienced this situation in the last month, 0-otherwise
No Money for Farm	1-Experienced this situation in the last month, 0-otherwise
Credit	1- took a credit/loan in the last 6 months 0- otherwise
Stress Index	Average of answers to ten stress related questions (Cohen et al., 1983)
Celebratory Events	1- attended/organized a wedding or a celebratory event in the last month, 0- otherwise
Risk	Scale: 1 (risk averse) -6 (risk lover) Incentivized Eckel and Grossman (2002, 2008) Gamble Task

Table C.3: Survey Measures of Financial Situation Across Abundance and Scarcity Periods

Variable	Abundance	Scarcity	*p*-value
Finances Relative to Others	2.19 (0.55)	2.19 (0.57)	1.0000‡
Household Financial Situation	2.58 (0.78)	2.87 (0.61)	0.0002‡
No Money Index	1.71 (1.46)	2.17 (1.35)	0.0041 ‡
No Money for Food	0.26 (0.44)	0.40 (0.49)	0.0061†
No Money for Basic Needs (non-food)	0.44 (0.50)	0.57 (0.50)	0.0433†
No Money for Medical Expenses	0.41 (0.50)	0.48 (0.50)	0.2623†
No Money for Farm	0.60 (0.49)	0.73 (0.45)	0.0348†

†McNemar's Chi Square test. ‡Wilcoxon matched-pairs signed-ranks test.
Standard deviations are in parentheses.This table includes all 109 participants who participated in both periods. However, not all participants provided an answer to all questions. Thus, the number of observations ranges between 97 and 109 depending on the period and the question.

Table C.4: Other Survey Measures and Risk Across Abundance and Scarcity Periods

Variable	Abundance	Scarcity	*p*-value
Stress Index	1.91 (0.60)	1.92 (0.46)	0.5251‡
Credit	0.92 (0.28)	0.83 (0.38)	0.1336†
Celebratory Events	0.77 (0.43)	0.82 (0.39)	0.4142†
Risk	3.10 (1.93)	2.91 (1.58)	0.5311‡

†McNemar's Chi Square test. ‡Wilcoxon matched-pairs signed-ranks test.

Standard deviations are in parentheses.This table includes all 109 participants who participated in both periods. However, not all participants provided an answer to all questions. Thus, the number of observations ranges between 97 and 109 depending on the period and the question.

Table C.5: Tobit Regression Results

	(1)	(2)	(3)
DV: Worker Effort			
Wage	0.0134***	0.0138***	0.0138***
	(0.00154)	(0.00135)	(0.00134)
Non Profit	0.000906	0.00178	0.368*
	(0.0824)	(0.0730)	(0.201)
Wage*Non Profit	0.00258	0.00238	0.00236
	(0.00198)	(0.00180)	(0.00171)
Period		-0.00766***	-0.00767***
		(0.00154)	(0.00134)
Female		-0.0848	-0.0666
		(0.0555)	(0.0469)
Society Oriented			0.109***
			(0.0376)
Society Oriented*Non Profit			-0.0964*
			(0.0504)
Constant	-0.188***	-0.0724	-0.491***
	(0.0611)	(0.0636)	(0.149)
Observations	2040	2040	2040

Robust errors standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Panel Data Tobit Regression Results Using the First 15 Periods

	(1)	(2)	(3)	(4)	(5)	(6)
DV: Worker Effort						
Wage	0.0129***	0.0130***	0.0130***			
	(0.00135)	(0.00140)	(0.00158)			
High Wage (40-60)				0.317***	0.321***	0.321***
				(0.0306)	(0.0344)	(0.0292)
Non Profit	-0.0474	-0.0559	0.291	0.0560	0.0486	0.402**
	(0.0804)	(0.0958)	(0.192)	(0.0514)	(0.0475)	(0.173)
Wage*Non Profit	0.00368*	0.00369*	0.00366**			
	(0.00201)	(0.00208)	(0.00186)			
High Wage*Non Profit				0.0855*	0.0858*	0.0849*
				(0.0474)	(0.0448)	(0.0468)
Period		-0.00552***	-0.00553***		-0.00519***	-0.00520***
		(0.00172)	(0.00166)		(0.00189)	(0.00191)
Female		-0.0936*	-0.0770		-0.0855**	-0.0687
		(0.0548)	(0.0476)		(0.0396)	(0.0439)
Society Oriented			0.100**			0.102***
			(0.0403)			(0.0357)
Society Oriented*Non Profit			-0.0911**			-0.0932**
			(0.0446)			(0.0410)
Constant	-0.138**	-0.0439	-0.430**	0.151***	0.241***	-0.153
	(0.0568)	(0.0615)	(0.184)	(0.0373)	(0.0460)	(0.152)
Observations	1530	1530	1530	1530	1530	1530

Robust errors standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

APPENDIX D

SIMULATION PROCEDURE TO ASSESS THE ACCURACY OF THE SAMPLE SIZE TO GENERATE RANDOM DISTRIBUTION

The Simulation Procedure:

- Step 1: Given our sample size of 109 subjects, we first draw 109 random integers between 1-6 (i.e., virtual die roll).
- Step 2: We test whether the distribution of the random draws differs from a categorical random uniform distribution using the Chi Square Goodness of Fit test.
- Step 3: We repeat the procedure in Steps 1 and 2 1000 times.
- Step 4: We record the number of times out of 1000 simulations that the distributions were indeed categorical random uniform.
- Step 5: We compute a statistical inference measure which is the number of simulations resulting in non-random distributions divided by the total number of simulations.