EXPERIMENTS ON COLLECTIVE ACTION

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EXPERIMENTS ON COLLECTIVE ACTION

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This dissertation employs experimental methods to investigate some of the non-material incentives that exacerbate and help groups resolve problems of collective action. It is composed of three studies that investigate what motivates individuals to contribute to public goods, what mechanisms can be used to encourage giving, and the aggregate collective action problem of dishonesty and corruption. Chapter 1 provides an overview of each study. Chapter 2 presents work conducted with Kelsey Jack, which investigates leadership within the context of voluntary public good provision. We conduct a field experiment in 52 communities in rural Bolivia to investigate two questions: (1) Can leaders enhance voluntary public good provision by making a public donation before others?, and (2) If so, why? We explore whether leaders give more when they have the opportunity to influence others, and whether their behavior influences the contributions of others. Some of the mechanisms behind leadership influence are explored. Chapter 3 presents a study conducted with Arno Riedl and Lise Vesterlund, which investigates voluntary contributions to public goods in the laboratory. We conduct an experiment that varies the location of the unique Nash equilibrium in dominant strategies in piece-wise linear public good games and examine whether or not response time can be used to identify the intuitive action. The study gives particular attention to error, which manifests itself as generosity in the classic linear public good game, and may be negatively correlated with the time individuals take to make choices. Chapter 4 investigates a collective action problem of a different nature: dishonesty. It conducts two laboratory experiments to investigate whether or not dishonesty is contagious, and the role that tolerance plays in halting or facilitating the contagion of dishonest behavior.

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PREFACE

I thank my dissertation advisor, Lise Vesterlund, for her invaluable mentorship. I thank my dissertation committee members, Lise, John, Randy, and Sera, for their guidance and support. I thank my co-author, Kelsey Jack, for introducing me to the world of field experiments. Finally, I thank my family and friends for their encouragement and love.

1.0 INTRODUCTION

This dissertation employs controlled laboratory and field experiments to investigate non-material incentives that exacerbate and help groups resolve problems of collective action. Two chapters examine how the social features of the environment in which individuals make decisions affect choices. One chapter examines a behavioral measure that has been recently used to make inferences about preferences, response time.

Chapter 2, titled "Leadership and the voluntary provision of public goods: Field evidence from Bolivia", investigates the effect that democratically elected authorities have on voluntary public good provision when they make public contributions before others. Kelsey Jack and I conduct a controlled experiment in 52 communities in rural Bolivia that varies whether or not the elected local authority in a village makes a public voluntary contribution before others make private simultaneous contributions to environmental education books for the local school. The study compares contributions when an elected authority makes a public contribution first to two types of controls: one in which a randomly selected person contributes publicly first, and one in which all subjects make private simultaneous contributions. Results show that elected leaders increase public good provision by 20 percent when they lead by example. The effect is driven by two factors. First, elected authorities take advantage of leadership opportunities and give more than non-authorities when they give publicly first. Second, high leader contributions increase the probability that others follow. The chapter empirically identifies information signaling as a channel of leadership influence and explores the extent to which the observable characteristics of leaders matter. It contributes to the literature by showing that local authorities can influence voluntary public good provision in a development setting and by taking a first step towards bridging the gap that currently exists between field, theoretical, and laboratory work on sequential giving, which has never examined how actual leaders influence the voluntary contributions of the groups they lead.

Chapter 3, titled "Intuitive generosity and error prone inference from response time", investigates whether response time can be used to identify the intuitive action in public good settings. Lise Vesterlund, Arno Riedl, and I follow up on the work of Rand, Greene, and Nowak (2012), who document a negative correlation response time and giving in linear public good games and from this infer that cooperation is intuitive while greed is a calculated response. Noting that error is a strong confound in the standard linear public good game and may be correlated with the time individuals take to make choices, we conduct a laboratory experiment that identifies mistakes and generosity in two piece-wise linear public good games with unique interior Nash equilibria in dominant strategies. In one treatment subjects make contributions in a setting in which the equilibrium is located below the midpoint of the strategy space. In another treatment subjects make choices in a setting in which the equilibrium is located above the midpoint of the strategy space. The chapter shows that the correlation between response time and giving is sensitive to features of the strategic environment. We replicate existing findings that participants who make decisions quickly tend to be more cooperative when the Nash equilibrium is located below the midpoint of the strategy space, but find the reverse result when the Nash equilibrium is located above the midpoint of the strategy space. The chapter shows that response time cannot be used to identify intuitive responses. Fast decisions are associated with error and contributions that are scattered over the entire range of possible choices.

Chapter 4, titled "Dishonesty, Tolerance, and Social Information", conducts two experiments to investigate the effect that exposure to dishonest behavior has on the likelihood that individuals engage in and tolerate dishonesty. I construct a strategic setting that allows dishonesty to be studied in one-shot and repeated settings in the laboratory. Half of all participants in the study have the opportunity to engage in dishonesty and half are affected by the dishonest actions of others. Engaging in dishonesty implies lying and taking earnings from another equal participant who does not have the opportunity to engage in dishonest actions but who can punish dishonesty in a repeated setting at a cost. Treatments vary whether or not individuals receive information about the behavior of subjects in a previous session of the experiment and whether or not such information reflects honest or dishonest behavior. Results show that social information affects choices. Exogenous social information reflecting dishonest behavior increases the likelihood that individuals are dishonest, while exposure to social information reflecting honest behavior has a weak effect on choices. Exogenous social information reflecting dishonest behavior weakly decreases the likelihood that individuals tolerate dishonesty. The chapter contributes to the literature by documenting a non-material strategic complementarity that makes dishonesty contagious and by showing that tolerance does not facilitate the contagion of dishonest behavior.

2.0 LEADERSHIP AND THE VOLUNTARY PROVISION OF PUBLIC GOODS: FIELD EVIDENCE FROM BOLIVIA (CO-AUTHOR: B. KELSEY JACK)

2.1 INTRODUCTION

Leaders play a central role in the resolution of collective action problems. Existing evidence demonstrates that leaders affect growth at the aggregate level (Jones and Olken 2005) and influence the choice of public goods provided at the local level (Chattopadhyay and Duflo 2004). Most studies of leadership and public good provision focus on public goods that are provided by the government.[1] In spite of the importance of voluntary contributions for the resolution of local-level collective action problems, less is known about the effect leaders have on the voluntary provision of public goods. Recent work has shown that leaders can affect voluntary contributions through informal taxation (Olken and Singhal 2011), sanction enforcement (Grossman and Baldassarri 2012), and reciprocity (Beekman, Bulte, and Nillesen 2013). This paper examines another mechanism by which leaders may affect voluntary contributions to local public goods: leadership by example.

In a voluntary contribution setting, leadership by example arises when individuals make sequential decisions, and the choice made by the first mover (the leader) influences the contributions of others. A substantial theoretical and experimental literature has shown that first movers can affect

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[1] For example, Chattopadhyay and Duflo (2004) study the effect of female leadership on policy decisions in India; Reinikka and Svensson (2004) investigate the political capture of public education funds in Uganda; Humphreys, Masters, and Sandbu (2006) study leadership influence on public deliberations about future public resource use in São Tomé and Príncipe; and Besley, Pande and Rao (2012) analyze political influence in public resource allocation decisions in India.

voluntary contributions in sequential decision settings through free-riding (Varian 1994), information signaling (Hermalin 1998; Vesterlund 2003; Potters , Sefton, and Vesterlund 2005, 2007; Andreoni 2006; Hermalin 2007; Bag and Roy 2011), reciprocity (Andreoni, Brown, and Vesterlund 2002; Meidinger and Villeval 2002; Gächter, Nosenzo, Renner, and Sefton. 2010, 2012), and social status (Kumru and Vesterlund 2010; Eckel, Fatas, and Wilson 2010). Due perhaps to the challenge of empirical identification of leadership influence in field settings, no study has examined how the example set by individuals who occupy actual leadership positions affects the voluntary contributions of the groups they lead. Our paper begins to fill this gap in the literature by conducting a randomized field experiment in rural Bolivia that investigates two questions: (1) Do local leaders (authorities) affect voluntary public good provision through their example?, and (2) If so, why? Our experiment examines the effect of leadership on the contributions of both leaders and followers, and tests the role of information signaling about the quality of the public good as a causal mechanism of leadership influence.

We implement a controlled field experiment in 52 socially and politically independent communities, each of which has its own elected local authority.[2] In our experiment, a representative sample of community members pool resources to provide environmental education books for the local school.[3] We employ a between-subject design that solicits voluntary contributions in a natural decision setting and compare public good provision when an authority makes an initial public voluntary contribution—and other group members make private voluntary contributions after observing the authority's choice—to two types of controls: one in which a randomly selected community member makes an initial public contribution and one in which all contributions are private. Two of the three treatments are implemented simultaneously in each community, facilitating the use of fixed effects to address unobservable community-level confounds.[4]

Our results show that local authorities increase average voluntary public good provision when they give publicly before others. The effect is unique to authorities; randomly selected individuals have little effect on overall giving when they lead. We decompose treatment effects into leader and follower

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[2] We refer to the elected local leader as the "authority" to differentiate the leadership role assigned in the experiment from the formal authority position elected local leaders occupy at the community level.

[3] Environmental education books provided through the experiment are accessible to all community members (non-excludable), but exhibit rivalry. We consider them a pure public good from the donor's perspective. That is, because contributions impose a positive externality that is non-rival and non-excludable on anyone who cares about the provision of education material in the local school.

[4] Community fixed effects remove any spurious correlation between first and subsequent mover decisions which could be otherwise incorrectly interpreted as leadership influence.

responses to leadership. Our results show that authorities not only contribute more than non-authorities when they give publicly before others, they also influence follower contributions. Followers of authorities are more likely to make a low contribution after observing a low leader contribution, and the probability that their contribution is high is significantly greater after observing a high leader contribution. Randomly selected leaders do not exert the same level of influence.

We offer two pieces of evidence on why leaders affect voluntary public good provision in our setting. First, our study is designed to identify information signaling as a mechanism through which leaders influence followers. We exogenously vary whether or not participants receive information about the quality of the public good. Informed participants are less responsive than uninformed participants to the example set by randomly selected leaders, but not to the example set by community authorities. This result suggests that other mechanisms such as social status or reciprocity may also contribute to the observed authority leader effects. Second, we examine the relative importance of the authority's formal leadership position in the community and his or her observable characteristics. In our study, community members randomly selected to lead who are similar to local authorities on observable characteristics both make higher contributions to the public good and have a greater influence over the contribution decisions of others, i.e. they have the same effect on provision as authorities in a leadership role. This finding provides suggestive evidence that authorities are influential because of the types of individuals they are, not just the formal position they hold. The effect of authority contributions on followers can be thus attributed to a combination of authority status, observable characteristics, and the fact that authority leaders contribute more than do random individuals given the opportunity to lead.

Our study is the first to examine how local authorities affect voluntary public good provision without the use of sanctions or coercion and thus makes several contributions to the literature. First, we empirically identify leadership by example as a mechanism through which local authorities can affect real voluntary contributions in a development setting. Second, we show that the leadership influence of local authorities on aggregate public good provision is explained both by their own contribution and the effect that they have on the contribution decisions of others. Third, we offer novel support for one of the most studied channels underlying leadership by example—information signaling—but show that its empirical relevance depends on who is in the leadership role. Finally, we provide suggestive evidence that authorities are influential because of their formal leadership position, their elevated contributions when placed in a leadership role, and their observable characteristics; traits such as education and wealth, which are correlated with several potential mechanisms of authority influence, matter.

Our study relates to a small but growing number of controlled field studies that examine the relationship between leaders and voluntary contributions in developing countries. Using public good games in the field, Grossman and Baldassari (2012) find that individuals elected within the experiment—who are not local authorities—are more effective at sanctioning low voluntary contributions, while Beekman et al. (2013) show that voluntary contributions are lower in communities that have corrupt officials. More similar to our study, d'Adda (2012) conducts an artefactual field experiment in 6 villages in rural Colombia that investigates how social information interacts with social status, defined endogenously along leadership dimensions, in a repeated voluntary contribution setting. Her results show that high status individuals (leaders elected within the experiment) are more likely to make high contributions and are less influenced by the contribution decisions of others. Our study is unique in this literature in that we study actual authorities, vary leadership exogenously, and use a one-shot setting in which voluntary contributions acquire an actual public good and in which both leaders and followers can react to leadership opportunities.

In trading off the control of the laboratory with the realism of the field, our study encounters some limitations. First, in order to investigate both leader and follower responses to leadership we allow leader contributions to arise endogenously in our experiment. This design feature reveals whether or not authorities take advantage of leadership opportunities, but prevents us from cleanly separating the effect of leader contributions from leader characteristics and leader type when analyzing follower responses. Second, a small number of communities could not comply with treatment randomization for idiosyncratic reasons. Our findings are robust to correcting for any resulting selection bias.

The chapter proceeds as follows. Section 2.2 offers a conceptual framework for leadership in public good provision. Section 2.3 describes the experimental context and design. Section 2.4 describes the main results, treatment heterogeneities, and robustness checks. Section 2.5 concludes our work

2.2 CONCEPTUAL FRAMEWORK

Early theoretical literature on sequential giving showed that leadership by example is weakly detrimental for voluntary public good provision when information is perfect and individuals are solely motivated by altruism (Varian 1994). This result emerges because the positive externalities generated by voluntary contributions introduce a free-riding incentive that induces first movers, leaders, to make low initial public contributions that force followers to provide the public good. Subsequent theoretical and empirical studies have, nevertheless, shown that sequential giving can be beneficial for public good provision. Three primary classes of mechanisms underlie these positive results: (1) information signaling, (2) social preferences, and (3) social status. Our empirical design only facilitates the empirical identification of information signaling as a causal mechanism of leadership influence, but to help interpret our results, we describe all the mechanisms by which actual leaders may affect overall giving below.

First, models of information signaling have shown that sequential giving can have beneficial effects on voluntary public good provision when at least one of two types of informational asymmetries are present: (1) uncertainty about the common value of the public good, and/or (2) uncertainty about private valuations of the public good. If the common value of the public good is uncertain and the leader has an informational advantage over others, he or she may signal such information through his or her contribution decision (Hermalin 1998; Vesterlund 2003; Potters et al. 2005, 2007; Andreoni 2006; Hermalin 2007).[5] The leader can signal if a public good is of high (low) value by making a high (low) contribution that induces others to follow. Although level predictions are conditional on the underlying information in the hands of the leader, information signaling is always welfare enhancing in this setting. If individuals have independent private valuations for the public good and such information is not common knowledge, then leadership by example can, theoretically, positively affect provision through the resolution of the underlying strategic uncertainty (Bag and Roy 2011).

Second, sequential giving can positively affect public good provision when individuals have social preferences that include reciprocity, equity, and fairness concerns (Meidinger and Villeval 2002; Huck and Rey-Biel 2006; Potters et al. 2005, 2007). Leaders who make high contributions crowd-in the contributions of subsequent movers. They cannot use their first mover advantage to free-ride (as in Varian 1994) because reciprocal followers punish free-riding at a cost.[6] Social preferences could even transform

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[5] Field studies that have investigated information signaling within the context of voluntary contributions include Karlan and List (2012) and Smith, Windmeijer, and Wright (2014). While Karlan and List (2012) examine information signaling within the context of matching grants in charitable giving, Smith et al. (2014) investigate peer effects in charity fundraising conducted by individuals on-line. The form of leadership we study differs from these field studies in that we require the leaders to set an example by making a one-time costly and unrecoverable contribution before others; the leader does not observe the decisions of others and cannot make contributions at a later time.

[6] Andreoni et al. (2002) and Gächter et al. (2010) compare simultaneous and sequential giving with induced preferences in the laboratory when information is perfect and show that although leaders try to free-ride off of followers, followers punish free-riding by giving less than their best response function predicts.

the social dilemma into a coordination problem (Bicchieri 2006). Sequential giving can be beneficial for coordination because it improves equilibrium selection in groups.

Third, social status can affect public good provision when individuals with high social status lead and followers like to be associated with high status others (Kumru and Vesterlund 2010) or want to acquire status (Bracha et al. 2009).[7] Status can also be modeled as the location individuals occupy in a social network (center vs. periphery), and explain leadership influence on voluntary contributions through the number of agents that observe the leader's choice (Eckel et al. 2010).[8]

What is the effect of leadership by example on public good provision in the field, when a local authority assumes the role of first mover? Do Varian's (1994) free-riding predictions hold or are effects consistent with one or more of the channels underlying positive leadership by example effects? Each of the mechanisms that allow leadership to positively influence public good provision may be active when any individual leads by example, but the existing literature suggests that effects should be amplified when a local authority leads.

For example, authorities may possess superior information about the value of the public good. They may have been elected precisely because of this informational advantage, or may have acquired such information through their formal leadership role.[9] Authorities may generate more reciprocity among community members due to their authority position and may even cause a reduction in strategic uncertainty when coordination incentives are present. Authorities may have higher social status than the average community member. They may be wealthier, more educated, and even possess higher social status as a direct result of the formal leadership position they occupy in the community. Finally, other factors such as legitimacy and motives unrelated to leadership influence, may also affect local authorities' ability to lead.[10] Among motives that are unrelated to leadership, authorities could give more because they

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[7] Related to this literature is also the empirical work on prestige and visibility motives for giving. See, for example, Harbaugh (1998), Ariely, Bracha, and Meier (2009) and Karlan and McConnell (2012).

[8] Several laboratory experiments have investigated the importance of networks within the context of coordination games (Eckel and Wilson 2007) and have shown that high status leaders affect equilibrium selection.

[9] Formal authorities in our study setting attend workshops and meetings organized exclusively for local leaders and have experience making decisions on behalf of the community. Miller and Mobarak (2014) study this informational channel of leadership influence within the context of opinion leader influence on technology adoption decisions in Bangladesh. They show that when opinion leaders (including formal authorities) unanimously decide to adopt a new technology, the likelihood of adoption by other community members increases.

[10] A small number of papers have shown that legitimacy increases leadership influence and voluntary public good provision in laboratory settings. See, for example, Baldassarri and Grossman (2011) and Levy et al. (2011).

are more imaged concerned (Bénabou and Tirole 2006) than non-authorities, and/or could value the public good more by virtue of the position they occupy in the social network (Nielson and Wichmann 2013). The described mechanisms are not mutually exclusive. Our empirical identification of information signaling does not rule out the relevance of other drivers of leadership effects in the field.

2.3 EXPERIMENTAL DESIGN

We employ a between-subject design with three treatments that (a) identifies the effect authorities have on voluntary public good provision when they lead by example, (b) distinguishes the influence of the example set by authorities and non-authorities in the community, and (c) isolates the importance of one of the most studied mechanisms behind leadership influence: information signaling about the quality of the public good. Before turning to the details of the experiment and its implementation, we describe the study setting, which informs our design.

2.3.1 Study setting

The experiment was conducted in 52 communities located in the Rio Grande-Valles Cruceños region of Bolivia, in collaboration with a non-governmental organization, Fundación Natura. The setting is useful for the study of leadership by example in public good provision for three reasons. First, decentralization in Bolivia extends all the way to community-level administrative units called Organizaciones Territoriales de Base (OTBs).[11] OTBs are independent social and political units; in our study setting they are small in size, meet regularly as a group, and are poorly integrated with outside markets. Each OTB has an elected representative (OTB president) who serves as the formal authority in the community. OTB presidents are elected in public meetings through majority vote. They are in charge of requesting funds from the municipal government, of developing local projects, of interacting with outsiders, and of organizing collective work. The fact that these authorities exist in all communities allows us to analyze the behavior of OTB presidents both in and out of a contribution leadership role.

[11] We use the term OTB and community interchangeably in the remainder of the paper because each community in our study is considered a separate OTB. Communities in the study sample contained an average of 26 households.

Second, a detailed census of 130 communities was conducted in the area by Fundación Natura in 2010. The census includes household and community level information that facilitated the randomization of communities and households into treatment, and provides us with the controls used in our analysis of experimental results.

Third, political parties and organizations have little presence in the area.[12] Anecdotal evidence indicates that OTB presidents do not actively seek office and have no intention of pursuing a political career. They accept the authority position when selected by their peers, but find the responsibility costly in terms of the effort and time. We consider this beneficial for our study because it mitigates political factors that might confound our experimental design.[13]

The experimental design uses a naturally occurring decision setting—a community meeting—to solicit contributions to environmental education books for the local school.[14] Environmental education books were chosen as the public good in our experiment for several reasons. First, all communities in our study have a local primary school. Second, 40 percent of households in our study site identified environmental protection as one of the top values that should be taught to children in the community. All communities are located inside a watershed that was declared protected in 2007 due to severe soil erosion caused by agricultural practices. Environmental issues such as trash, water pollution, and soil erosion are thus very salient in the area. Third, although environmental education books are not a pure public good, they exhibit several relevant characteristics: books are non-excludable,[15] and generate social spillovers. Furthermore, books can be viewed as a pure public good from the donor's perspective. That is, because contributions generate a positive externality that is non-rival and non-excludable on anyone who cares about the provision of environmental education material in the local school.

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[12] Sixty percent of households in our study (located in 51 communities) indicated in the 2010 census that political syndicates do not exist at the OTB level. Six out of 41 local authorities indicated that they attend political syndicate meetings, but none indicated that they have occupied authority positions at the syndicate level in the past. Only 4 out of the 580 individuals who participated in our study indicated that they had held authority positions at the syndicate level.

[13] It also may increase the likelihood of observing leadership by example effects in our setting. Given the lack of evidence on leadership by example in the field, establishing its empirical relevance is best done in a setting where it is likely to affect public good provision.

[14] Community meetings occur regularly in our study setting, and are organized through local authorities to address community business or at the request of outside individuals or organizations. We followed the standard approach to organizing a meeting, and therefore consider it a natural decision setting.

[15] All community members can have access to the education material available in the local school. Teachers are present in the school during weekdays and can grant school access. Community authorities and members of the parent-teacher association also have keys to grant school access when teachers or school administrators are not present.

From a practical perspective, books made it possible for us to examine voluntary contributions to a local public good in a setting in which even small contributions could ensure positive levels of provision. They also minimized trust confounds by allowing us to deliver the public good on site at the end of the experiment. The books used in the experiment were purchased from a non-government organization that specializes in producing environmental education material in Bolivia. Seven different books were available, and were sold at a zero-profit price of 10 Bs. per book.

2.3.2 Treatments

Our experiment employs a between-subject design with three treatments. In each treatment, subjects complete a survey in exchange for money and are subsequently given the opportunity to make a voluntary contribution to environmental education books for the local school.

The treatments vary the way in which community members make voluntary contributions. In a No Leader Treatment (NL), individuals make private simultaneous contributions to the local public good. In a Random Leader Treatment (RL), a randomly selected individual is asked to make his or her voluntary contribution publicly before others. In an Authority Leader Treatment (AL), the formal community authority is asked to make his or her voluntary contribution publicly first. In both the RL and AL treatments, other participants make private voluntary contribution decisions after observing the contribution leader's public choice.[16]

The NL treatment establishes a benchmark scenario that we use as control in our experiment. Comparison of NL and AL determines if local authorities affect voluntary public good provision through their example. Comparison of the RL and AL treatments determines whether AL treatment effects are specific to authority leaders or are a generic response to leadership. We conduct two simultaneous treatments per community and use fixed effects to control for community characteristics that may affect both leader and follower contributions.

[16] We use the term contribution leader to refer to the first mover: the randomly selected individual in the RL treatment, and the authority in the AL treatment.

The design introduces an information manipulation in all treatments that gives half of all participants, and always the contribution leader, the opportunity to inspect the public good before making a voluntary contribution decision.[17] The information manipulation identifies the extent to which information signaling about the quality of the public good explains leadership influence in this setting. If the leader's contribution conveys information about the value or quality of the public good then uninformed follower contributions should move in the direction of the leader's contribution (Vesterlund 2003; Potters et al. 2005, 2007; Andreoni 2006). The effect should be muted or reversed for informed followers, who do not need to rely on the leader's contribution to update their beliefs about the quality of the public good.[18] Comparison of uninformed and informed follower decisions within treatment tell us whether information signaling about the quality of the public good drives the effect that leaders have on follower contributions. Comparison across treatments informs us about the differential importance that information signaling has for each type of contribution leader. While several mechanisms discussed in Section 2.2 generate similar predictions across leadership treatments, none other than information signaling result in a differential follower response by information condition.

2.3.3 Randomization

We used household and OTB level data from the 2010 census of study communities to balance treatment assignment. OTBs included in the census but missing OTB-level information or without a local primary school were excluded from our study, as were communities smaller than 15 or greater than 80 households in size. The final eligible sample consisted of 52 OTBs.

OTBs were randomly assigned to one of three possible pairwise combinations of the NL, RL and AL treatments and 12 households from each community were randomly sampled for participation. The randomization balance for OTB and household variables was tested for each of 1000 draws and the draw with the minimum maximum t-statistic for any single variable was used as the final study sample (Bruhn

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[17] The information manipulation was implemented in such a way that informed agents knew who else was able to inspect the public good, but uninformed agents had no knowledge of the informational advantage possessed by others.

[18] Of course, visual inspection may not resolve all uncertainty about the quality of the public good, in which case, informed followers will look more like uninformed followers in their response to leader contributions. A negative correlation between leader and follower contributions should arise when all informational asymmetries are eliminated, social preferences are absent, and free-riding incentives dominate (Varian 1994).

and McKenzie 2009).[19] The largest resulting t-statistic associated with treatment assignment was 1.50, associated with balance by municipality. The randomization process delivered both balanced characteristics across treatment and a representative sample of households for participation in the study.

2.3.4 Implementation

The study team visited each community 4 to 7 days prior to the intervention. The team met with community leaders, scheduled the experiment, and delivered invitations to a "meeting organized by researchers from Universities in the United States."[20] Written invitations were delivered in person to the heads of the 12 households selected through the randomization process in each community, which always included the OTB president.[21] At the time of invitation, individuals were told that they could earn up to 45 Bs. for attending the meeting and that only one person per household could attend.[22] On the day of the experiment, invited households were reminded of the time and location of the meeting. If households informed the study staff that they would not attend, a new household identified from the list of alternates generated through randomization was invited to participate.[23]

Two types of attrition affect the final study sample. The first is selection into the study, which occurred before the experimental session was conducted and does not affect the internal validity of our results. Appendix Table A1 provides a description of how the sample of households selected through randomization differs from the final sample of participants. The second is selection into treatment, which occurred in 6 sessions assigned to the Authority Leader treatment (AL), where the authority was not

[19] Randomization was balanced at the household level on: the number of rooms in a house, the education of the household head, the number of children under 16 per household, a stated preference for instilling environmental values in children, perceptions of community cooperation and decision-making, attitudes toward outsiders and participation in past community meetings. The distance to market, the number of households in the community, and the municipality were all balanced at the community level. We use the balancing variables as controls in most cases. In the analysis, a few variables are replaced with superior measures of the underlying characteristic of interest, such as the use of assets instead of number of rooms as a proxy for wealth.

[20] Community meetings in the study area are always organized with community leaders. We did not use the word experiment and deliberately called sessions meetings to minimize potential experimenter demand effects.

[21] Attrition could occur at the invitation stage if a household selected through randomization could not be located for invitation delivery. A household selected for participation required a substitute at the invitation stage if it had moved from the community or if no adult representative was available. A list of randomly chosen substitute households was used to identify replacements.

[22] 45 Bs. is approximately 6.50 US dollars and is equal to the daily wage for agricultural work in the study setting.

[23] In select cases, no alternates from the list were available and substitutions were based on convenience. Convenience replacements were made in 19 cases.

present on the day of the experiment.[24] These sessions were run with either the NL or RL treatment (whichever was not assigned to the other concurrent session in the same community). In four cases, to preserve balance in the number of sessions across treatments, the next available community scheduled to receive a combination of RL and NL treatments received an unplanned AL treatment. These incidents are documented in Appendix Table A.2.

Selection into treatment is potentially problematic for our empirical strategy.[25] Authority absences, however, seem to have been idiosyncratic. Authorities were not present the day of the experiment because they had to attend classes in the municipal capital, had to take care of medical emergencies, or were away harvesting crops. No systematic differences between sessions selected into and out of treatment are detected in our data (see Appendix Table A.2). We show balance on the characteristics of participants for the final experimental sample in Appendix Table A.3 The Authority Leader treatment has fewer females, and participants in the No Leader treatment have slightly fewer assets. These results persist when community fixed effects are included and are partly driven by the fact that authorities differ from other community members on a number of observable characteristics, and are more often present in the AL treatment (See Appendix Table A.4). We control for these and other observable characteristics throughout our analysis of experimental results.

Each experimental session consisted of three parts and took place at the local school or in another centrally located community building. Throughout the implementation, efforts were made to keep the process similar to a typical community meeting.

In Part 1 of the meeting, individuals arrived to the designated meeting place, registered, received an envelope, an ID number, and consent forms. IDs ranging from 0 to 11 were distributed at random to participants with the exception of ID 0, which was always given to the OTB authority.[26] Subjects were then informed that they would earn 35 Bs. by completing a questionnaire and 10 Bs. by attending the full meeting. At the time of soliciting consent, subjects knew that they would be asked survey questions but

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[24] In 5 communities the authority was not present in the community to participate in the experiment. In one community the authority refused to participate.

[25] Specifically, the types of communities in which authorities were absent are systematically less likely to appear in the within-community comparisons of AL and the other treatments, while the RL-NL comparison is more likely to occur.

[26] The subject that was randomly assigned ID 6 acted as contribution leader in the RL treatment. Like the authority, the subject with ID 6 was not aware that he or she would have a special role in the experiment.

were not aware that they would be asked to make a voluntary contribution. Part 1 of the experiment took approximately 20 minutes.

In Part 2, subjects were split into two groups based on their ID number, which allowed more seating space for each of the participants while also facilitating the implementation of two simultaneous treatments. The experimenter and assistant were rotated to ensure balance across treatments. In each group, subjects completed a survey containing questions unrelated to the study in exchange for their experimental earnings.[27] Questions were read out loud to participants, who answered using paper and pencil.

Regardless of the answers provided, all subjects were given 35 Bs. in 5 Bs. coins upon completion of the survey. Participants with even ID numbers were then asked to step out of the room; contribution leaders always had even ID numbers. Even numbered subjects were shown the environmental education books and were given the opportunity to inspect them, but were not told how the books would be used in the session. Subjects with odd ID numbers were not told the purpose of this interruption, but were asked to answer one additional survey question to pass the time.[28] Participants with even IDs returned to the room after 5 minutes.

Following the information manipulation, the contribution decision was presented to subjects. Subjects were told that the money earned by completing the survey was theirs to keep and that they could contribute as much or as little as they wanted to environmental education books for the local school. Books were displayed in front of the room and subjects were given general information about their cost and content. They were informed that for every 10 Bs. contributed by all community members (in both sessions of the experiment) the school would receive one book.[29] Participants knew that 7 different volumes of the books were available and that they would be delivered on-site at the end of the experiment.

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[27] The 17 survey questions covered topics such as place of birth, places they visit to access markets, seek medical attention, and make legal transactions.

[28] The additional survey question asked participants to indicate the communities they had visited the previous year. None of the 580 participants questioned the purpose of the interruption.

[29] Participants were additionally informed that contributions would be rounded up if the total amount contributed by all participants was not a multiple of 10. This ensured that we never kept any of the contributions made by subjects.

To make their voluntary contributions, subjects were asked to place the money they wished to contribute in an envelope that had their ID number marked on the inside. Contribution decisions were done in private behind a cardboard partition. If the session was assigned the Random Leader or the Authority Leader treatment, the contribution leader—referred to by his or her ID number—was asked to demonstrate the process to others and to publicly announce the amount of his or her contribution as it was placed in the envelope. All other participants were called one by one to make their private voluntary contribution in the back of the room.[30] The order by which subjects were called upon to make their contributions depended on the seating arrangement. Participants were not allowed to talk while contribution decisions were being made.

After all participants made their contributions, subjects were asked to complete a survey with 6 questions on household socio-demographics and perceptions of teaching quality in the local school.[31] Once the final survey was completed, subjects received a 10 Bs. show up fee. This marked the conclusion of Part 2, which took approximately 60 minutes.

Part 3 of the experiment started once both experimental sessions were over. All participants returned to the same room and the total amount contributed by subjects was announced. The environmental education books were counted in public and given to the community authority or school representative in front of all subjects. The final part of the experiment took approximately 10 minutes. The entire session lasted between 90 and 120 minutes.

2.4 RESULTS

We observe the decisions of 580 subjects in 104 sessions of the experiment, which were conducted between May and July 2011. Each session included between 4 and 6 subjects; a total of 9 to 12 individuals participated in the experiment in each of the 52 communities included in our sample.

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[30] All subjects knew at the time they were making their contribution decisions that their contributions would not be revealed to anyone, including the local authority. This was done to ensure that contributions would not be affected by anticipated sanctioning.

[31] The purpose of these questions was to collect individual-level information that was not available through the census or was outdated. The census was conducted almost a year before the experiment and asked questions only to the household head.

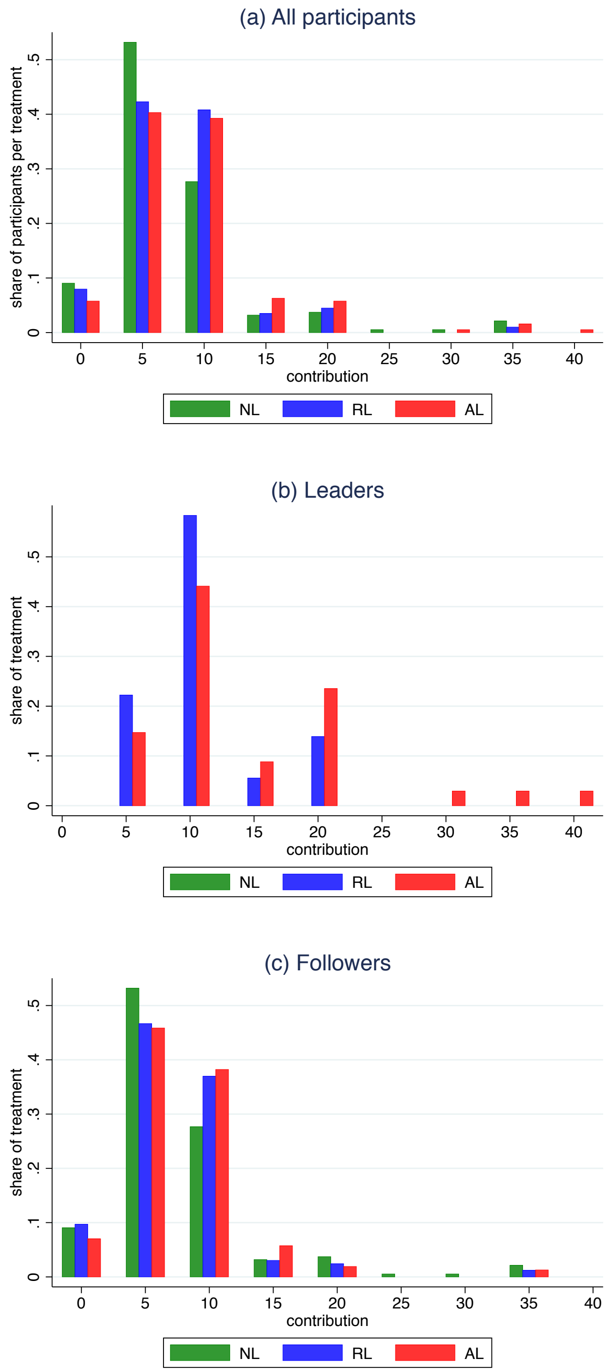

Figure 2.1: Histogram of contributions by participant type and treatment

Notes: Figures represent histograms of contributions in bins of 5 Bs. Each of the figures describes contributions for different samples of participants: all (top), contribution leaders only (middle), and followers only (including the NL treatment, bottom). The shading describes each of the three experimental treatments, and the histogram plots the share of each treatment in the different contribution bins.

Figure 2.1 shows histograms of contribution decisions by participant type and experimental treatment.[32] Panel (a) combines all participants and shows that 5 and 10 Bs. constitute the most common contribution levels across treatments. A contribution of 5 Bs. is the median contribution in the study population and the minimum non-zero contribution level. Ten Bs, on the other hand, is the smallest contribution that has a direct impact on the provision of the public good.[33] The AL treatment shows first order stochastic dominance over the RL treatment, which in turn dominates the NL treatment.[34] Panels (b) and (c) break contributions down by participant type. This presentation of raw data suggests that authority leaders give more than individuals randomly selected to lead by example (Panel b), and that contribution leaders make higher contributions than do followers (Panel b vs. c).[35]

Although the raw data suggests that public good provision increases in the presence of a leader, these results are informative only of aggregate outcomes. Observable and unobservable factors that drive both leader and follower contributions need to be accounted for. To quantify treatment effects parametrically we regress contributions on the experimental treatments and a vector of individual- and session-level controls. All regressions include community fixed effects and cluster standard errors at the community level. Community fixed effects address any spurious correlation between leader and follower giving, driven by unobservable community-level factors. Even with fixed effects, controlling for individual- and session-level characteristics is important given the imbalance discussed in Section 2.3.

We estimate treatment effects using three different specifications, which we describe generally here and in detail immediately preceding each set of results. First, to examine changes in mean contributions, we consider a continuous measure of contributions and estimate treatment effects using ordinary least squares. Second, we take into account that the experiment was implemented using 5 Bs. coins and implement an ordered logit model in which each 5 Bs. interval constitutes a separate categorical

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[32] Contributions are classified in 5 Bs. bins that reflect the monetary unit used to pay subjects in the experiment. A small number of subjects made contributions using their own coins. These are rounded to the closest 5 Bs. interval in Figure 2.1 but not in the remainder of the analysis.

[33] Ten Bs. is also the value of the show-up fee, which could induce artificially high focal contributions of 10 Bs. The modal contribution of 5 Bs. in the No Leader treatment implies that this is highly unlikely.

[34] Wilcoxon Mann-Whitney rank-sum tests reject the null hypothesis that the contributions of all participants in AL and NL or RL and NL were drawn from the same underlying distribution ($p<0.01$ and $p<0.10$ respectively). Differences between AL and RL are not statistically significant; but the Wilcoxon Mann-Whitney rank-sum test has a p-value <0.15.

[35] Wilcoxon Mann-Whitney rank-sum tests provide p-values <0.05 and <0.01 respectively.

giving bin.[36] Third, given that the greatest mass of contributions occurs at 5 and 10 Bs. (see Figure 2.1) treatment effects may be concentrated around the median level of giving. We therefore estimate treatment effects on the probability of giving above the median (≥10 Bs). We revert to OLS for the median regressions. However, our results are qualitatively similar if we use a conditional logit model with fixed effects.

In our main analyses, we assume that the selection documented in the implementation section is idiosyncratic. Robustness checks that address selection into treatment, including an instrumental variables correction and a sample restriction to the compliant sub-sample of communities, are presented in Section 2.4.3. They are consistent with our main results.

2.4.1 Main results

We begin by analyzing the effect of leadership by example on total and individual contributions, then split the analysis to focus on the behavior of leaders and on the response of followers. We explore treatment heterogeneities that help to explain the mechanisms underlying the main results next and conclude our analysis with a series of robustness checks. All tables show OLS and ordered logit estimates, include fixed effects, and show results with and without controls, though we focus on the specifications that include controls in our discussion of the results.

Total contributions

We begin by estimating:

$$y_{ic} = \alpha + \beta_1 AL_{ic} + \beta_2 RL_{ic} + \Gamma X_{ic} + \eta_c + \varepsilon_{ic} \tag{1}$$

[36] To accommodate community-level fixed effects in the ordered logit specification, we use Baetschmann et al.'s (2011) “blow up and cluster” (BUC) approach, which generates dichotomous outcomes at each of k thresholds and estimates each binary outcome using conditional maximum likelihood. The method relies on the restriction that the log odds associated with each threshold is the same, but is shown to be robust to outcome categories with few observations. In our data, the dependent variable acquires values k $\in\{0,5,10,15,20\}$ when contributions fall respectively in [0,5), [5,10), [10,15), [15,20), and [20,40]. Contributions of 20 Bs. or more are grouped together because few observations exceed the value of 20, and no random leader makes contributions in excess of this amount (see Figure 2.1).

where y_{ic} represents the contribution made by individual *i* in community *c*, AL denotes the Authority Leader treatment, RL the Random Leader treatment, X_{ic} is the vector of individual- and session-level controls shown in the balance table, η_c is a community fixed effect, and ε_{ic} is an error term clustered at the community level.

Results are shown in Table 2.1. Columns 1 and 2 show estimates aggregated at the session level, where *i* indexes the experimental session and X_{ic} is a vector of average session-level characteristics. Having an authority lead by example increases the total contributions in an experimental session by 9.11 Bs. (s.e. 4.92), or approximately one environmental education book (column 2). Columns 4 and 6 show that this translates to an average individual-level increase of 1.07 Bs. (s.e. 0.69) or a 0.50 (s.e. 0.28) increase in the log odds of contributing an additional 5 Bs. coin. The median regressions in columns 7 and 8 indicate that the likelihood that a contribution exceeds the median increases by 15 percent (s.e. 0.07) when a local authority leads (column 8).

Table 2.1: Total contributions (all participants)

	Total		Individual					
	Continuous		Continuous		Ordered logit		≥ 10 Bs.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RL	-0.941	-0.579	-0.183	-0.286	0.136	0.099	0.100*	0.093*
	(3.865)	(4.613)	(0.652)	(0.662)	(0.235)	(0.251)	(0.055)	(0.055)
AL	6.952	9.110*	1.257*	1.065	0.607**	0.499*	0.174**	0.155**
	(4.280)	(4.924)	(0.739)	(0.695)	(0.270)	(0.276)	(0.066)	(0.065)
N	104	104	580	580	580	580	580	580
Test: RL = AL (p-value)	0.102	0.034	0.072	0.052	0.045	0.062	0.273	0.323
Controls		Yes		Yes		Yes		Yes
Dep. Variable mean, NL	42.947		7.767		7.649		0.378	

Notes: N=104 in columns 1 and 2, N=580 in columns 3-6. Columns 1 and 2 show OLS estimates of treatment effects on total session contributions. Columns 3-4 and 7-8 show OLS estimates of treatment effects on individual contributions. Columns 5-6 show log odds ratios from an ordered logit regression. See text for details. RL refers to the Random Leader treatment; AL to the Authority Leader treatment. The omitted category is the No Leader (NL) treatment. All specifications include community fixed effects and standard errors clustered at the community level. Controls refer to the full set of individual- and session-level controls shown in the balance table. *p<0.10, **p<0.05, ***p<0.01.

Treatment effects differ by the type of contribution leader. Community members randomly selected to lead by example do not affect total contributions to the public good (columns 1 through 6). Random leaders do, however, increase the probability that contributions exceed the median by approximately 9 percent (s.e. 0.06, column 8). As shown by the p-value for a test of the equality, the RL and AL coefficients are statistically significantly different from each other in all but the linear probability model of giving 10 Bs. or more (columns 7 and 8). Leadership by example therefore has a consistent and positive impact on public good provision when an elected local authority leads.

Leader contributions

To examine how authority and randomly selected contribution leaders adjust their behavior when leading by example, we compare each type of leader's contribution behavior when they lead and when they give in private. We restrict our analysis to leaders in the RL and AL treatments and to individuals in the NL treatment, which includes elected authorities. The NL participants thus form a counterfactual for leader behavior, when contribution decisions are private. We regress contribution decisions on leadership treatment and authority status:

$$y_{ic} = \alpha + \delta Authority_{ic} + \beta_1 AL_{ic} + \beta_2 RL_{ic} + \Gamma X_{ic} + \eta_c + \varepsilon_{ic} \quad (2)$$

where $Authority_{ic}$ represents an indicator for whether individual i in community c is the elected authority, and all other variables use the same notation described in equation 1. The coefficient on $Authority_{ic}$ captures any difference in contribution behavior between authorities and other community members when they give in private. The coefficients on the RL_{ic} and AL_{ic} treatment indicators reflect the change in contribution behavior displayed by random individuals and authorities when they give in public relative to non-authorities who give in private in the No Leader treatment. The regression is analogous to a difference in difference set up that includes leadership position and authority status, where AL represents the total effect for an authority in a contribution leadership position. The regression does not describe differences in the contribution behavior of authorities across treatments because community fixed effects are used and authorities are always present in either the NL or AL treatment. With this caveat in mind, the test for Authority public = private reported in the last row of Table 2.2 tests whether authority contributions are significantly different when they are made in public in the AL treatment from when they are made in private in the NL treatment.

Table 2.2 shows that both authorities and non-authorities increase their contributions when they lead by example. Authorities give 6.09 Bs. (s.e. 2.35) more when they lead by example than the average individual in the NL treatment (Columns 1 and 2). Randomly selected contribution leaders, on the other hand, give an additional 1.64 Bs. (s.e. 1.39). In all specifications, the coefficient on AL (β_1) is larger in magnitude than the coefficient on RL (β_2). The difference between authority and non-authority leader contributions is marginally significant in some specifications, which suggests that part of the total increase in public good provision generated by the AL treatment is explained by the direct effect of the contribution of authorities who give publicly first.

Table 2.2: Leader contributions

	Continuous		Ordered Logit		≥ 10 Bs.	
	(1)	(2)	(3)	(4)	(5)	(6)
Private giving by an authority	-0.921	-0.477	-0.005	0.330	0.006	-0.027
	(2.257)	(1.947)	(0.815)	(0.791)	(0.171)	(0.153)
Public giving by a random leader (RL)	2.400*	1.643	1.076***	1.139**	0.347***	0.317***
	(1.266)	(1.387)	(0.391)	(0.464)	(0.080)	(0.097)
Public giving by an authority leader (AL)	7.192***	6.092**	2.144**	1.676*	0.424**	0.374**
	(2.637)	(2.347)	(0.917)	(0.892)	(0.196)	(0.178)
Controls		Yes		Yes		Yes
Dep. Variable Mean, No Leader treatment	7.751		7.701		0.372	
			Tests (p-values)			
Public giving RL = AL	0.098	0.097	0.244	0.544	0.706	0.756
Authority private = public	0.084	0.107	0.190	0.379	0.242	0.203

Notes: N=258. The sample consists of individuals who led by example in RL and AL treatments and all subjects in the NL treatment. Authority refers OTB presidents in NL. Columns 1-2 and 5-6 present OLS estimates. Columns 3 and 4 show log odds ratios from an ordered logit regression (see text for details). All regressions include community fixed effects and standard errors clustered at the OTB level. Controls refer to the full set of individual- and session-level controls shown in the balance table. *p<0.10, **p<0.05, ***p<0.01.

Interestingly, the differences between authority and non-authority giving arise solely in response to leadership. The coefficient on the authority status indicator (δ) is small and imprecisely estimated, indicating that authority contributions are not different from the contributions of other community members when they contribute privately in the NL treatment. The final row of Table 2.2 shows that the difference in authority contributions in public and in private is imprecisely estimated, though large in magnitude.[37]

Follower contributions

Now we turn to the behavior of followers to test whether they respond to the contribution decisions of leaders, and whether the response differs by leader type. We exclude contribution leaders from the

[37] It is important to note that we only observe 8 authorities giving in the NL treatment, so the authority status indicator variable is identified off of a very small number of observations, resulting in large standard errors.

analysis and compare the contribution behavior of followers in RL and AL treatment to individuals who contribute privately in NL.[38] The estimating equation is:

$$y_{ic} = \alpha + \beta_1 AL_{ic} + \beta_2 RL_{ic} + \beta_3 AL_{ic} \times y_c^{AL} + \beta_4 RL_{ic} \times y_c^{RL} + \Gamma X_{ic} + \eta_c + \varepsilon_{ic} \quad (3)$$

where y_{ic} represents the contribution made by follower i in community c, and y_c^T for T $\in$ {AL,RL} represents the contribution made by the contribution leader in treatment T. The effect of the different leader types cannot be completely separated from the fact that leader type (RL or AL) is correlated with leader contribution decisions and leader characteristics in our experimental setting (as shown in Appendix Table A.4). Thus, treatment effects on followers should be interpreted as the combined effect of the leader type, leader characteristics, and an endogenous leader contribution.[39]

Columns 1 and 2 of Table 2.3 show OLS estimates of equation (3) with a continuous measure of leader contribution on the right hand side. The linear effect of continuous leader contributions on follower giving is statistically insignificant for both authority leaders (AL × leader contribution) and randomly selected leaders (RL × leader contribution), as are the level effects of the leadership treatments (AL and RL). The specification is restrictive in that it estimates the average effect of an increase in leader contributions on follower responses linearly. Potential theories underlying a leadership by example effect require neither monotonicity nor linearity in the best response function of followers. Before turning to the categorical measures of leader giving used in the rest of Table 2.3, we examine the less parametric analyses presented in Figure 2.2, which shows the marginal effects of leader contributions on follower giving, including a quadratic term. The relationship is imprecisely estimated in Panel (a), where follower response is continuous, but is similar and more precise when follower response is binary in Panel (b).

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[38] As noted in the preceding analysis of leader contribution decisions, some NL treatments included local authorities. Controlling for the presence of an authority not in the contribution leader role does not substantially change any of the results.

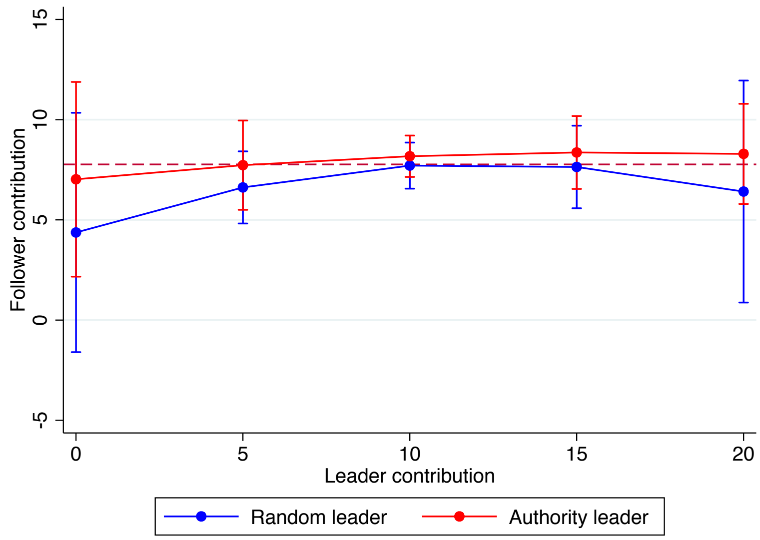
[39] We chose not to exogenously vary the amount authorities and non-authorities give when they lead by example because doing so would require letting subjects know that the leader is not freely choosing the amount they wish to contribute (in order to avoid using deception). This may generate a different response to leadership by example and would not be able to capture the leader response to leadership opportunities that we analyze in the previous sub-section.

Table 2.3: Follower contributions

	Continuous				Ordered Logit		≥ 10 Bs. (OLS)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Random leader (RL)	-0.388	-0.364	-0.896	-0.775	-0.441	-0.374	-0.086	-0.085
	(2.411)	(2.421)	(0.834)	(0.903)	(0.470)	(0.469)	(0.100)	(0.103)
Authority leader (AL)	-0.010	0.352	-1.512***	-1.124	-1.113***	-1.077***	-0.208***	-0.189**
	(1.023)	(1.164)	(0.361)	(0.882)	(0.201)	(0.387)	(0.049)	(0.080)
RL x leader contribution	-0.027	-0.034						
	(0.238)	(0.235)						
AL x leader contribution	0.005	-0.022						
	(0.078)	(0.084)						
RL x leader contribution ≥ 10 Bs.			0.448	0.228	0.492	0.405	0.212*	0.206*
			(1.117)	(1.117)	(0.507)	(0.497)	(0.112)	(0.111)
AL x leader contribution ≥ 10 Bs.			1.856*	1.379	1.420***	1.336***	0.372***	0.343***
			(0.968)	(1.282)	(0.374)	(0.516)	(0.085)	(0.104)
RL total effect: Leader contribution ≥ 10 Bs.			-0.448	-0.547	0.051	0.031	0.126**	0.121**
			(0.791)	(0.769)	(0.269)	(0.270)	(0.062)	(0.060)
AL total effect: Leader contribution ≥ 10 Bs.			0.344	0.255	0.307	0.258	0.164**	0.154**
			(0.898)	(0.880)	(0.315)	(0.327)	(0.070)	(0.069)
Controls		Yes		Yes		Yes		Yes
Dep. Variable mean, NL treatment	0.489				7.728		0.378	
	Tests (p-values)							
RL=AL	0.891	0.797	0.501	0.787	0.188	0.253	0.279	0.444
RL x leader contrib. = AL x leader contrib.	0.912	0.966	0.420	0.556	0.177	0.227	0.284	0.408
RL total effect = AL total effect			0.489	0.443	0.450	0.480	0.619	0.653

Notes: N = 510. Columns 1-4 and 7-8 show OLS estimates. Columns 5 and 6 show log odds ratios from an ordered logit regression with fixed effects (see text for a details). All specifications include community fixed effects and standard errors clustered at the OTB level. Controls refer to the full set of individual- and session-level controls shown in the balance table. *p<0.10, **p<0.05, ***p<0.01.

(a) Effect of leader contributions on average follower giving



(b) Effect of leader contributions on the probability that followers give above the median

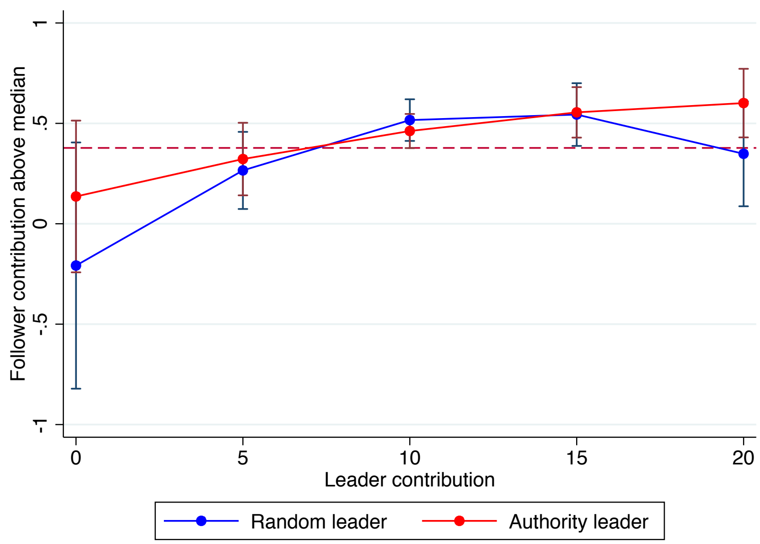


Figure 2.2: Leader influence over followers

Notes: Figures represent the marginal effects of regression coefficients for random and authority leader contributions. See text for a description of regressions. We present estimates for leader contributions up to 20 Bs. because this is the region of common support.

Specifically, Figure 2 suggests that authority leader contributions have a positive and approximately linear effect on the probability that followers give above the median (≥10 Bs.), while random leader contributions have a concave effect that is increasing up to 10 Bs. To accommodate these non-linearities the remainder of Table 2.3 tests for asymmetries in the response to high and low leader contribution, with the split at 10 Bs.

We examine the effect of high and low leader contributions on continuous follower contributions in columns 3 and 4 and categorical giving levels in columns 5 through 8. Authority leaders who give less than 10 Bs. (coefficient on AL) insignificantly decrease follower giving by approximately 1.12 Bs. (s.e. 0.88, column 4) relative to the NL treatment. They significantly decrease the log odds that followers give an additional coin by 1.08 (s.e. 0.39, column 6), and significantly reduce the probability that follower contributions exceed the median by 19 percent (s.e. 0.08, column 8). Relative to a lower authority leader contribution, a high AL contribution has a positive and significant effect on follower contributions. Specifically, a follower of an authority who gives above the median gives 1.4 more Bs. (insignificant, column 4), has a significant 1.3 greater log odds of giving an additional coin and is 34 percent more likely to also give above the median than is a follower who observes an authority leader contribute below the median.

Is the influence of authorities on followers different than that of randomly selected community members who lead by example? In general, the coefficients on the random leader treatment variables in Table 2.3 are of the same sign, smaller magnitude and less precisely estimated than the corresponding authority leader effects. Column 8 shows that a random leader who gives at least 10 Bs. increases the probability that followers give at least 10 Bs. by 12.1 percent (s.e. 0.06), which is the only specification in which random leaders can be seen to have a significant influence over follower contributions. In spite of the relatively more consistent influence of authority leaders, the differences between the random and authority leader effects are statistically indistinguishable; p-values from the relevant t-tests are reported in the table. In neither case is the response of followers consistent with free riding: both authority and random leaders show some evidence of positively affecting follower contributions.

2.4.2 Heterogeneous treatment effects

We turn next to the question of why leadership affects voluntary contributions in our setting, by exploring treatment heterogeneities in both leader and follower contribution decisions. First, we test whether followers' response to the leader's example differs based on their exposure to information. Second, we examine heterogeneities in leader influence based on the observable characteristics of leaders.

Followers: Information signaling

Recall that the information manipulation generated exogenous variation in the information available to study participants about the quality of the public good in all treatments. If leadership by example serves as an information signal, then the contribution decision of session leaders should influence informed and uninformed followers differently. Specifically, the contributions of uninformed followers should demonstrate a more positive correlation with the contribution decisions of the leader, because uninformed followers are more dependent on the quality information conveyed by the leader's decision than are informed followers. Table 2.4 replicates the analysis conducted in columns 3 to 8 of Table 2.3, but adds an additional interaction to differentiate between followers who did and did not have the opportunity to inspect the public good. We break the results out by information condition and show total effects for uninformed and informed followers relative to the contribution decisions of uninformed followers in the No Leader treatment. Note that informed and uninformed followers in the NL treatment make statistically indistinguishable contributions. In Table 2.4, the treatment effects are followed by a series of p-values, shown at the bottom of the table, from tests of the equality of coefficients across information conditions and by leader type.

Beginning with uninformed followers, the top panel of Table 2.4 shows that an authority leader who makes a low initial contribution (less than 10 Bs.) insignificantly decreases the average contributions made by uninformed followers by 1.04 Bs. (s.e. 1.17), significantly lowers the log odds that follower contributions fall in the next categorical giving bin by 1.44 (s.e. 0.77) and significantly decreases the probability that followers give at least 10 Bs. by 29.3 percent (s.e. 0.16). An authority who makes a high initial contribution, on the other hand, insignificantly increases average uninformed follower contributions and the log odds that followers contribute an additional coin, but significantly increases the probability of giving above the median by 14.4 percent (s.e. 0.08). Authorities therefore influence uninformed follower contribution decisions. They decrease follower giving when they set a negative example as leader, but also increase the probability that followers give above the median when their contributions are high. Random leaders do not affect the contributions of uninformed followers when they make low contributions, but are just as influential as authorities when they make initial contributions of 10 Bs. or more.

Table 2.4: Heterogeneous treatment effects – information

	Continuous		Ordered logit		≥ 10 Bs.	
	(1)	(2)	(3)	(4)	(5)	(6)
Uninformed follower						
RL: leader contribution < 10 Bs.	0.191	0.317	0.123	0.190	0.025	0.026
	(1.027)	(1.154)	(0.564)	(0.577)	(0.127)	(0.132)
RL: leader contribution ≥ 10 Bs.	0.270	0.351	0.323	0.356	0.175***	0.180***
	(0.827)	(0.780)	(0.301)	(0.290)	(0.062)	(0.063)
AL: leader contribution < 10 Bs.	-1.506*	-1.043	-1.454**	-1.440*	-0.315**	-0.293*
	(0.772)	(1.170)	(0.671)	(0.774)	(0.120)	(0.159)
AL: leader contribution ≥ 10 Bs.	0.849	0.619	0.510	0.417	0.172**	0.144*
	(1.005)	(0.900)	(0.397)	(0.372)	(0.082)	(0.078)
Informed follower						
RL: leader contribution < 10 Bs.	-1.611	-1.648*	-1.180**	-1.153**	-0.216**	-0.233**
	(0.989)	(0.879)	(0.526)	(0.520)	(0.117)	(0.109)
RL: leader contribution ≥ 10 Bs.	-0.521	-0.924	-0.055	-0.185	0.097	0.064
	(1.127)	(1.209)	(0.409)	(0.413)	(0.090)	(0.092)
AL: leader contribution < 10 Bs.	-0.691	-0.403	-0.539	-0.496	-0.027	-0.027
	(0.751)	(1.065)	(0.426)	(0.539)	(0.104)	(0.089)
AL: leader contribution ≥ 10 Bs.	0.537	0.567	0.301	0.270	0.187**	0.185**
	(0.933)	(0.965)	(0.351)	(0.382)	(0.087)	(0.089)
Individual and session controls		Yes		Yes		Yes
Dependent Variable mean, excluded category	7.351		7.728		0.360	
			Tests (p-values)			
RL, leader cont.<10Bs: uninformed=informed	0.105	0.126	0.014	0.023	0.093	0.057
RL, leader cont.≥10Bs: uninformed=informed	0.395	0.201	0.244	0.087	0.333	0.184
AL, leader cont.<10Bs: uninformed=informed	0.398	0.565	0.351	0.371	0.148	0.212
AL, leader cont.≥10Bs: uninformed=informed	0.684	0.946	0.499	0.624	0.877	0.665
Uninformed, leader contrib. <10Bs: RL = AL	0.152	0.408	0.066	0.094	0.052	0.133
Uninformed, leader contrib. ≥10Bs: RL = AL	0.624	0.782	0.622	0.853	0.977	0.664
Informed, leader contrib. <10Bs: RL = AL	0.405	0.357	0.308	0.360	0.203	0.138
Informed, leader contrib. ≥10Bs: RL = AL	0.491	0.351	0.464	0.359	0.437	0.300

Notes: N = 510. Columns 1-2 and 5-6 show OLS estimates. Columns 3-4 show log odds ratios from ordered logit estimates. See text for details. The excluded category is uninformed subjects in NL. All specifications include community fixed effects and standard errors clustered at the OTB level. Controls refer to the full set of individual- and session-level controls shown in the balance table. *p<0.10, **p<0.05, ***p<0.01.

As shown in the middle panel, the effects on informed followers are rather different. The negative effect of low authority leader contributions becomes statistically insignificant. High authority leader contributions, however, continue to affect the probability that followers give above the median, with a similar magnitude and statistical significance as in the case of uniformed followers. AL treatment effects are not significantly different across informed and uninformed followers. For random leaders, the results are reversed. While uninformed followers were unresponsive to low leader contributions, informed followers make significantly lower contributions when the leader makes a low contribution.[40] High contributions from random leaders do not increase the contributions of informed followers, as they did for uninformed followers, though the difference in the coefficient is significant only in one specification.

Overall, the analysis of heterogeneous treatment effects on the basis of information shows four things. First, the leadership influence exerted by random leaders is consistent with information signaling: random leaders who make high contributions increase the likelihood that uninformed but not informed followers make contributions that exceed the median. Second, the patterns of authority influence cannot be fully explained by information signaling. While the negative influence of a low authority contribution appears to be mitigated by exogenous information provision, high authority contributions increase the contributions of even informed followers.[41] The first two results can be seen in the t-tests for the difference in the effects on uninformed and informed followers, which are significant in a number of specifications for random leaders but never for authority leaders. Third, while uninformed followers are marginally more likely to be influenced by a low authority leader contribution than by a low random leader contribution, informed followers are not differentially responsive to leaders of different types. These differences disappear when contributions are high; random leaders who contribute 10 Bs. or more are just as likely to influence followers as are authorities. Generally, these tests take us to the edge of what the study is powered to investigate, and we interpret the differences as suggestive evidence of a role of information signaling in leadership by example, which appears more prevalent among random leaders than among authority leaders.

Leaders: Individual characteristics

As discussed in Section 2.3, authorities differ from the average community member on a number of dimensions, including gender, education, assets, and community participation (see Appendix Table A.4). As a result, the influence of authority leaders may be driven not by the position that they hold in the community but by their observable characteristics. Some relevant traits, such as education and wealth, may allow leaders to generate better information signals, trigger more reciprocity, and have stronger

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40 The effect that random leaders who make low contributions have on informed followers is inconsistent with the predictions of information signaling about the quality of the public good, but could be explained by normative component of information signaling or non-information channels such as reciprocity and conformism.

41 The persistent influence of authority leaders over even informed followers could be explained by a superior information signal, for which inspecting the books is not a good substitute. We therefore cannot rule out that information signaling fully explains the results, though the nature of the signal offered by random and authority leaders must differ.

social influence regardless of their status as elected authorities. Holding an authority position may, on the other hand, convey an additional influence that extends beyond the observable characteristics of the leader.

Though our study is not designed to explicitly investigate how the observable characteristics of leaders explain leadership influence, we take advantage of the fact that randomly selected contribution leaders vary in the degree to which they resemble the average elected authority. We construct an "authority propensity score" using a probit regression of authority status on the five characteristics where authorities significantly differ from the rest of the community: gender, education, wealth, participation in community meetings, and trust in NGOs.[42] Each contribution leader is assigned an authority propensity score between 0 and 1, which describes the resemblance of each contribution leader to the average authority in the study.

The top panel of Figure 2.3 shows how leader contributions vary with leader characteristics and types by plotting the marginal effects from a regression of leader contributions on authority propensity score interacted with leader treatment that includes community fixed effects and standard errors clustered at the community level. The figure shows that random leaders give less than authority leaders in general, with contributions that are increasing in their authority propensity score. The slope of the random leader regression is positive but statistically insignificant. The giving gap between authority leaders and random leaders, however, narrows as the authority propensity scores increases, suggesting that the positive influence random leaders exert when their contributions are high (see Table 2.3) may be coming both from their observable characteristics and the amount they contribute as leaders. Since the authority propensity score was generated from the sample of authority leaders included in the regression, we plot the authority leader results only to provide a basis for comparison. The slope of the authority leader regression is negative, but statistically insignificant.[43]

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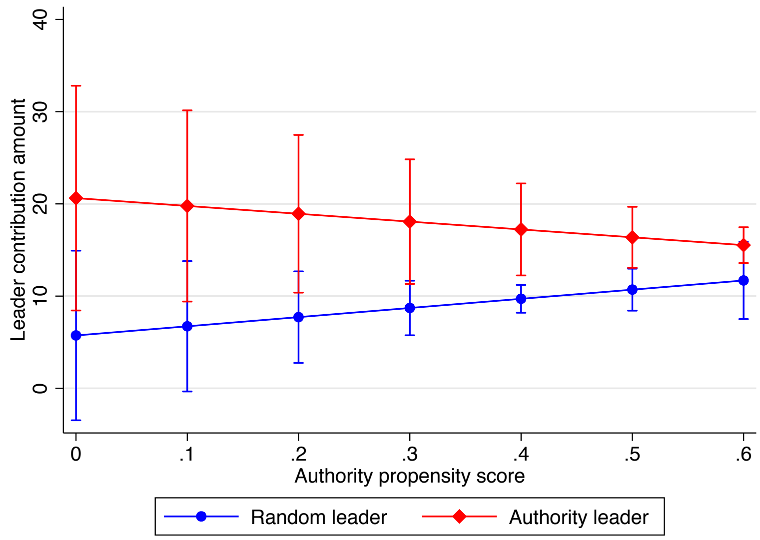
[42] Note that authorities differ from the rest of the population on several participation-related characteristics, including participation in OTB meetings and projects, and agreement with OTB decisions. We focus on one of these to avoid redundancy. Each of the covariates used in the probit regression is balanced after imposing common supports. We implement the propensity score matching using the algorithm developed by Becker and Ichino (2002).

[43] The negative and insignificant slope could indicate that authorities compensate for looking less like a leader by contributing more.

(a) Authority characteristics and leader contributions



(b) Authority characteristics and leader influence over followers

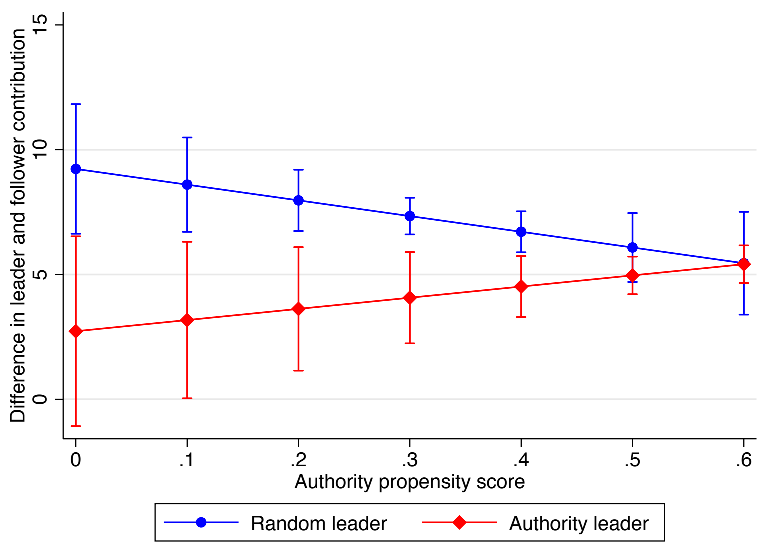


Figure 2.3: Authority characteristics vs. size of leader contributions

Note: Figures represent the marginal effects of regression coefficients for random and authority contribution leaders. See text of a description of the regressions. We do not report results for authority propensity scores beyond 0.6 because differences are not statistically significant across treatments.

To more directly examine the relationship between leader characteristics and leader influence, we construct a new outcome variable: the absolute difference between leader and follower contributions. The lower panel of Figure 2.3 shows the marginal effects from a regression of this measure of leader influence on an interaction of authority propensity score and leadership treatment, controlling for the leader contribution amount, individual- and session-level characteristics and community fixed effects, and with standard errors clustered at the community level. The figure shows that random leaders are more influential the more they look like the typical authority in the study. The slope of the authority propensity score among random leaders is marginally significant (p=0.102). Together, the results in Figure 2.3 highlight two factors underlying leadership by example in our setting. First, random leaders are more influential if they resemble authorities, both because they give more as leaders and because of their characteristics. Second, at least some of the influence that authorities have when they lead by example is driven by their observable characteristics. This last point may indicate that communities choose their leaders based, in part, on observable characteristics that are correlated with influential leadership.

2.4.3 Robustness checks

We use two types of robustness checks to address possible selection bias resulting from non-compliance with the assigned treatment in some communities. First, we present two stage least square estimates of treatment effects that use treatment assignment to the AL treatment as an instrument for administered AL treatment, as follows:

$$y_{ic} = \delta_1 RL_{ic} + \delta_2 \hat{AL}_{ic} + \Gamma X_{ic} + \eta_c + \varepsilon_{ic} \quad (3)$$

The fitted values of $\hat{AL}_{ic}$ are obtained from the first stage regression

$$AL_{ic} = \theta D_{ic} + \Gamma X_{ic} + \varepsilon_{ic} \quad (4)$$

where D_{ic} is an indicator for assignment to the AL treatment for individual *i* in community *c*. Second, we restrict our analysis to the sample of communities that complied with treatment assignment and estimate treatment effects directly as in our main specifications.

Appendix Table A.5 presents revised estimates of treatment effects on total contributions. Overall, the results look similar to the main specifications, and are stronger under the instrumental variables specification in most cases. This strengthening of the results under the IV specification is due to the relatively low contributions among the four replacement authority leaders. The limited sample analysis sometimes lacks statistical power because of the loss of sample size. The same robustness specifications are carried out for the leader and follower results. These are presented in Appendix Tables A.6 and A.7 respectively, and are also consistent with our main results.

2.5 CONCLUSION

Local authorities in developing countries often wield substantial power, and some evidence shows large authority fixed-effects in community development outcomes, including the provision of public goods (Chattopadhyay and Duflo 2004, Miguel and Gugerty 2005). What role do local authorities play? Do they help communities overcome collective action problems and sustain higher levels of voluntary public good provision? If so, how? A number of channels present themselves: sanctioning or rule enforcement, moral suasion, liaison with outside resources, reciprocity, and leadership by example. Our study offers novel evidence on the latter mechanism.

We implement small group experiments in 52 communities in rural Bolivia to examine the role that locally elected authorities play in the voluntary provision of public goods when they lead by example. In our setting, authorities exert a significant influence over voluntary public good provision even without the ability to monitor, sanction or coerce. On average, provision increases by approximately 20 percent when the group is led by an elected authority who makes an initial public contribution. In our setting, authorities significantly increase their contributions when they lead by example relative to when they give in a private, simultaneous decision setting where their contributions do not differ from those of the average community member. Authorities also influence the contribution decisions of followers, to a marginally greater extent than do random individuals who lead by example.

Our design explores one of the best-studied mechanisms underlying a positive effect of leadership by example on public good provision: information signaling. We find that the predictions of information signaling are consistent with the influence that randomly selected contribution leaders have on their followers, but do little to explain the influence of elected authorities. The additional influence of local authorities who lead by example suggests an information signal that goes above and beyond the information manipulation offered in the experiment, and with other channels such as reciprocity, legitimacy, and social influence. Further research is needed to identify other mechanisms by which leadership by example affects voluntary public good provision in field settings and to investigate how the observable characteristics of individuals correlate with channels of leadership influence. We generate suggestive evidence in this direction by showing that leader characteristics play an important role in determining their influence over followers. Our results highlight the multiple facets of the differential influence that authorities wield. In addition to the formal position that they hold within the community, authorities also make higher contributions when given the opportunity to lead and have different observable characteristics than the average community member.

Methodologically, our study offers an innovative approach to studying endogenously arising behavior within groups in field settings. The inclusion of community fixed effects allows us to address many of concerns associated with unobservable similarities between leaders and their followers within communities. We also employ best practices in a number of other design features, including precise measurement of selection in to the study, using a voluntary contribution mechanism with earned money rather than house money, and making contributions to an actual public good. Combining the rigor and insights of laboratory studies with the complexities of social interactions in the field offers a promising direction for future research. Particularly where leadership is concerned, stepping outside of the laboratory can generate insights about how actual leaders influence their followers and how the characteristics of individuals and groups interact.

While taking the study of leadership by example to the field offers a number of benefits, it also has some drawbacks. Most notably for our study, some communities were unable to comply with treatment assignment. The differences between the OLS results and the IV robustness checks suggest potential selection associated with the experimental treatments. We choose to lead with the OLS results given that treatment non-compliance appears idiosyncratic and the IV strategy strengthens our findings in most cases. Another area where the study gives up some control is the endogeneity of leader contributions. While the use of community fixed effects eliminates endogeneity concerns at the community level, they may still exist at the session level. We test for session level correlates of leader contributions and find only one significant explanatory variable out of 13 tested.[44]

[44] Specifically, we see that out of the full set of individual- and session-level controls, only average session-level assets is associated with leader contributions, which confirms that leaders are not systematically adjusting their

Like all field studies, we generate evidence for a particular area of Bolivia at a particular point in time. By testing specific causal mechanisms, nevertheless, we identify what may be more generalizable results. In other settings, with less decentralization or with more corrupt leaders, other actors within the community may be relatively more influential. Our results hold constant other means of influence that authorities have at their disposal, such as sanctioning power, which may be relatively more or less important than leadership by example in sustaining the voluntary public provision in different settings. While other studies have described the relationship between ethnic heterogeneity and local public good provision (e.g. Miguel and Gugerty 2005), our study is relatively homogeneous, making issues such as ethnic tensions less relevant. Leader characteristics, which play a role in shaping influence in our setting, may be even more important in settings with greater heterogeneity.

behavior based on the random group of followers to which they were assigned. The full table of results is available on request.

3.0 INTUITIVE GENEROSITY AND ERROR PRONE INFERENCE FROM RESPONSE TIME (CO-AUTHORS: ARNO RIEDL AND LISE VESTERLUND)

3.1 INTRODUCTION

Models of dual selves and dual process reasoning are increasingly being used in economics and psychology to explain decision making (see e.g., Evans 2008; Kahneman 2003, 2011; Shefrin and Thaler 1988; Loewenstein and O'Donoghue 2004; Benhabib and Bisin 2005; Bernheim and Rangel 2004; Fudenberg and Levine 2006, 2012). While the precise characterizations of these models differ, central to the framework is the premise that decisions are influenced by an intuitive system which is responsible for fast, impulsive, emotional, and rule-based choices, and by a deliberative system, through which slow, calculated, cognitive, and controlled decisions are made.

Dual-self models have been used to explain choices over a large range of domains (e.g., time and risk) and researchers have recently begun to apply this framework to the study of charitable giving (see e.g., Martinsson, Myrseth, and, Wollbrant 2012; Kocher, Martinsson, and Wollbrant 2012; Kinnunen and Windmann 2013; Kessler and Meier 2014). Of key interest has been the question of whether individuals intuitively are generous or selfish. Is it a fast, impulsive, emotional response to throw a dollar in a panhandler's hat? Or, does giving require a slow, calculated, and cognitive decision?[45] Understanding whether our generosity is intuitive or calculated is important from a theoretical and practical perspective because different predictions and policy prescriptions arise depending on the action that constitutes the intuitive response.

The authors thank seminar participants at SITE (Stanford), SPI (University of Chicago), ESA, ASSA, and the University of Pittsburgh for helpful comments

[45] Indeed an alternative interpretation of recent evidence that individuals donate due to social pressure is that they are tempted to do good, e.g., Vesterlund (2012) argues that temptation can help explain why individual's give more when there is an option to opt out of giving in DellaVigna, List and Malmendier (2010).

The question of whether individuals are predisposed toward generosity or selfishness is intriguing, but it is far from clear how we as researchers can find an answer. Existing evidence is scarce and contradictory. Using methods from psychology and neuroscience some studies suggest that generosity is intuitive (Sanfey, Rilling, and Aronson 2003; Ruff, Ugazio, Fehr 2013, Kinnunen and Windmann 2013) while others find evidence in favor of the deliberate generosity hypothesis (Knoch et al. 2006, Martinsson et al. 2012, Kocher et al. 2012, Strang et al. 2013). In a recent study Rand, Greene, Nowak (2012) propose the use of response time to identify whether individuals are intuitively generous. Examining transfers in a linear public good game they find that participants who quickly decide how much to give are significantly more generous than those who spend more time making their decisions. From this negative correlation between response time and giving they conclude that cooperation is spontaneous and intuitive while greed is a calculated response.[46]

The use of response time to infer whether individuals are intuitively generous or selfish is tempting as it is cheaply and easily gathered in controlled experiments. An obvious concern with it, however, is that confusion or mistakes may also be correlated with response time.[47] Such concerns are particularly relevant in the linear public good game where mistakes can be erroneously identified as other regarding behavior. In the classic linear public good game n individuals form a group and each allocate an endowment between a private and a group account. While a unit allocation to the private account generates a private payoff of 1, a unit contribution to the group account secures a payoff of r to each group member, where $1/n < r < 1$. Thus, from a selfish perspective, it is a dominant strategy to place the endowment in the private account.[48] An unfortunate consequence of this setting is that the core of the strategy space constitutes the entire range of available choices. Any (erroneous) deviation from the dominant strategy can therefore be seen as consistent with other-regarding preferences.[49] When using response time to draw inference on innate preferences we thus need to account for the correlation between response time and error. Given the comparative static we have to be particularly weary of the possibility that error prone participants make fast decisions, as quick mistakes can attribute to (if not account for) the negative correlation between giving and response time that has been documented in the literature.

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[46]The negative correlation between response time and contributions in the linear public good game has been replicated by Lotito, Migheli, and Ortona (2012) and Nielsen, Tyran, and Wengström (2013).
[47] For evidence on the negative correlation between response time and error in the beauty contest game see Kocher and Sutter (2006), Rubinstein (2007), Agranov, Caplin, and Tergiman (2012).
[48] Throughout the paper we use the term 'dominant strategy' to refer to payoff maximizing choices from a selfish perspective.
[49] Another issue in the linear public good game is that the efficient outcome requires full contribution. Therefore, it is impossible to determine whether positive donations result from error or generosity.

To explore whether error may confound the inference on intuitive generosity we conduct a laboratory experiment consisting of two public good games with an interior equilibrium in dominant strategies. In one game, the equilibrium is below the midpoint of the strategy space and relatively far away from the group welfare maximizing strategy, and in the other game it is above the midpoint of the strategy space and relatively close to the group welfare maximizing strategy. We refer to the first game as the "Low" treatment and the second game as the "High" treatment.

Relying on a between-subject design, we replicate the Rand et al. result in the Low treatment. When it is a dominant strategy to contribute an amount below the midpoint of the strategy space, fast decisions are correlated with higher contributions. However, the correlation between time and contributions is reversed in the High treatment. When the equilibrium is located above the midpoint of the strategy space, fast decision makers are shown to be less generous than those who take time to decide.

Intriguingly, comparing the Low and High treatments we find no significant differences in the contributions made by fast decision makers, but find significant treatment differences among slow decision makers. That is, the different payoff structures in the Low and High treatments only affect the behavior of slow decision makers. Importantly, in both treatments, fast decision makers are more likely to select contributions that are dominated and to contribute an inefficiently large amount. By contrast, slow decision makers are more likely to make the equilibrium contribution and to contribute an amount that falls inside the core of the strategy space.

In both treatments fast contributions are spread over the whole range of possible choices. This characteristic and the similarity of contributions by fast decision makers in both treatments suggest that fast decision makers are insensitive to the payoffs presented in the Low and High treatments. The pattern of contributions is consistent with fast decision makers being more prone to error. Whether these choices result from mistakes, inattention, or indifference to the incentives, the insensitivity to treatment suggests that choices made by fast decision makers unlikely reflect preferences over payoffs. Consequently, if payoff insensitive behavior correlates negatively with response time, then response time is likely to be a poor, or at least imprecise, measure of preferences over payoffs. In particular, a negative correlation between response time and generosity need not reflect that giving is spontaneous, but rather that 'confused' participants quickly select a contribution that lies, on average, in the middle of the strategy space.

3.2 RELATED LITERATURE

Economists have just recently begun to use response time to study individual decision making. Wilcox (1993) analyzed choices in risky environments and viewed response time as a proxy for decision cost. Rubinstein (2007) used response time to study the deliberation process employed by individuals. He put forward the idea that fast responses are intuitive and examined the correlation between non-incentivized choices and response time in seven different strategic environments. While his work often is cited as showing that intuitive responses are emotion-based, a large variation in the types of choices associated with fast responses is documented across strategic environments.[50]

The use of response time to identify the intuitive choice in pro-social settings started with the work of Rand et al. (2012). In a series of studies that use linear public good games (and prisoner dilemma games), Rand et al. find that fast choices are generous while slow choices are more selfish. They document a negative correlation between response time and giving and infer from this correlation and a study that primes either intuition or deliberation that cooperation is the intuitive response. The negative correlation between response time and contributions in the linear public good game has been replicated by Lotito, Migheli, and Ortona (2012) and, Nielsen, Tyran, and Wengström (2014).[51]

To infer causality between response time and contributions Rand et al. (2012) also vary whether or not subjects make decisions under time pressure. The results suggest that time pressure increases contributions in the linear public good game. Tinghög et al. (2013), however, note that the analysis presented in the paper is problematic because half of the observations in the time pressure treatment are excluded from the sample. They conduct a series of binary public good experiments in three different countries to reexamine the effect of time pressure on cooperation, but do not find a robust relationship. Rand et al. (2013a) reanalyze data from 15 experiments that manipulate time pressure and show that the effect of time pressure is only sometimes positive and statistically significant. However, they also note that time pressure is never found to have a statistically significant negative effect on giving.

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[50] In Rubinstein (2007) fast decisions are associated with fair outcomes in some settings, with equilibrium and efficiency maximizing choices in others, and even with the use of strictly dominated choices in other environments. In many of the strategic settings investigated, however, focal choices coincide with fair, equilibrium, efficiency maximizing, and strictly dominated strategies. It is thus unclear what constitutes the fast or intuitive response.

[51] Nielsen et al. (2014) use the strategy method to classify subjects as free-riders, conditional cooperators, and other cooperator types. They show that free riders make slower choices than other cooperator types. Their results could be driven by error if confused participants make fast choices that are ‘arbitrarily coherent’ and increase with the contributions of others. See Ariely, Loewenstein, and Prelec (2003) for evidence of coherent arbitrariness in other settings.

In examining both the negative correlation between response time and giving, as well as the response to time pressure, the literature has ignored the role that error may play in these settings. This is of particular concern in the public good game where deviations from the dominant strategy will benefit others and therefore can be seen as generous. If errors are uncorrelated with response time this is, of course, not an issue. However, it becomes an important confound if error is negatively correlated with response time. If fast decision makers more frequently are confused or inattentive, then response time will be a poor measure of preferences.[52]

Although no work in economics has analyzed the correlation between response time and mistakes specifically, several studies have shown that a lower frequency of dominated choices is associated with larger response times. Sutter, Kocher, and Strauß (2003), for example, show that forcing subjects to make decisions quickly increases the rates of rejection in the first round of a repeated ultimatum game. Kocher and Sutter (2006) show that guesses in the beauty contest game increase with time pressure. Rubinstein (2007) shows that choices equal to or above the midpoint of the strategy space in the 2/3 beauty contest game are associated with faster response times than any other strategy. Agranov, Caplin, and Tergiman (2012) use a strategy-type method that maps out individual choices over response time in the 2/3 beauty contest game and show that while the guesses of strategic players decrease with response time, non-strategic players make average guesses that coincide with the midpoint of the strategy space and do not change with time. Related to this work is also the psychology literature developed after Kahneman and Tversky (1974), which has shown that intuitive thinking and heuristics are often times associated with error.[53]

Our study contributes to the literature by analyzing whether response time can be used to identify the intuitive action. While we do this in the context of public good games, our results are informative of inference from response time in more general settings.

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[52] For a discussion of confusion and other-regarding behavior see Andreoni (1995) and Houser and Kruzban (2002).
[53] See for example Kahneman (2011).

3.3 EXPERIMENTAL DESIGN

In the classic linear public good game (aka voluntary contribution mechanism, VCM) an individual's private monetary payoff is maximized by contributing nothing irrespective of others contributions. In contrast, the efficient outcome is secured when all group members contribute their entire endowment. A problematic feature of the VCM is that any deviation from equilibrium increases the payoffs of the other group members, which makes it difficult to determine whether an individual's positive contribution is made in error or due to non-selfish inclinations. If we want to draw inference on preferences from the correlation between response time and giving, it is therefore essential to account for the possible correlation between error and response time.

To better disentangle errors from innate generosity we design a public good game where both the equilibrium and the efficient outcome are in the interior of the strategy space. Similar to many classic VCM experiments we examine 4-person groups and secure the Nash equilibrium in dominant strategies. However, in contrast to the classic setting the core is a subset of the strategy space. That is, there are contributions that simultaneously lower the payoffs of the individual and the group, and are therefore dominated from an individual as well as group payoff maximizing perspective.

In addition to modifying the public good game to allow for individually and socially dominated contributions, we also compare public good games with different payoff structures to assess first whether the difference in payoff structures affects the finding that fast decision are more generous, and second to determine whether error is responsible for the documented negative correlation between response time and generosity.

3.3.1 Payoffs

To secure an interior equilibrium we rely on a piece-wise linear payoff structure. Specifically we extend the two-person framework of Bracha, Menietti, and Vesterlund (2010) to a 4-person group. Participants are given a $10 endowment, which they can contribute in $1 increments to a group account. Contributions to the group account generate a constant and equal benefit to the other group members. The private benefit of contributing, however, is concave using a linear approximation. The participant's payoffs are given by the following function:

$$\pi_i(g_i, G_{-i}) = \begin{cases} 10 + \alpha g_i + \sigma G_{-i} & if \quad 0 \le g_i \le g^L \\ 10 + \alpha g^L + \beta(g_i - g^L) + \sigma G_{-i} & if \quad g^L < g_i \le g^H \\ 10 + \alpha g^L + \beta(g^H - g^L) + \gamma(g_i - g^H) + \sigma G_{-i} & if \quad g^H < g_i \le g^P \\ 10 + \alpha g^L + \beta(g^H - g^L) + \gamma(g^P - g^H) + \delta(g_i - g^P) + \sigma G_{-i} & if \quad g^P < g_i \le 10 \end{cases} \quad (1)$$

where π_i denotes the payoff individual i receives from his or her contribution g_i to the group account and the sum of contributions G_{-i} made by the three other group members. Threshold contributions g^L, g^H, and g^P denote respectively the individual equilibrium contribution in the Low and High treatments, and the individual contribution associated with the unique group welfare maximizing outcome. Parameter σ remains constant across treatments, while α, β, γ, and δ vary. That is, across treatments we hold constant the benefit to other group members from contributing while varying the individual return from giving.[54] Table 3.1 shows the specific parameters used in each treatment.

Table 3.1: Payoff function parameters by treatment

Treatment	Parameter							
	α	β	γ	δ	σ	g^L	g^H	g^P
Low	1.45	-0.25	-0.5	-3.25	0.25	3	7	9
High	0.116	0.25	-0.5	-1.25	0.25	3	7	9

Parameters were chosen to secure that the strategic settings fulfilled four requirements. First, there is a unique interior Nash equilibrium in dominant strategies, which varies by treatment. In our Low treatment the equilibrium contribution is \$3, and, thus, located below the midpoint of the strategy space. In our High treatment the equilibrium contribution is \$7, and, thus, located above the midpoint of the strategy space. Second, there is a unique interior group welfare maximizing contribution of \$9, which is the same across treatments. Third, equilibrium payoffs as well as the boundary payoffs associated with contributing \$0 and \$10 are held constant across treatments. Payoffs on the boundary are chosen such that contributions in the core range from \$3 to \$10 in the Low treatment, and from \$7 to \$10 in the High treatment. Fourth, the cost of deviating from the equilibrium contribution toward the middle of the strategy space (between \$3 and \$7) is held constant in the two treatments. The strategic environments thus separate equilibrium contributions from the boundaries and midpoint of the strategy space, while holding constant across treatments key features of the environment.

[54] We also conduct a robustness check that changes σ for $g_i > 9$. See section 3.4.5.

This set-up allows us within each treatment to assess whether fast deciding participants more frequently make choices outside of the core of the strategy space; that is, make choices that decrease both the payoffs of the decision maker and of the other group members. Moreover, by comparing the pattern of contributions of fast and slow decision makers between treatments we can assess whether fast decisions are always more generous or whether the correlation between contributions and response time depends on the strategic environment. If mistakes are more frequently made by fast decision makers and if these mistakes on average result in contributions in the middle of the strategy space, then we would expect to find a negative correlation between contributions and response time in the Low treatment, and to find the reverse correlation in the High treatment.

Building on Rand et al. (2012), we are primarily interested in documenting behavior in one-shot interaction. To assess stability and possible convergence, however, we also examine the results of repeated interaction. Therefore, the decision making phase of our study consists of two parts. Participants are in Part 1 matched in groups of four. Each group member makes a one-time contribution decision in a public good game. In Part 2 participants are informed that they are to play the same public good game for an additional ten periods, and that they will be randomly re-matched after every period (with the stipulation that they could not be matched with the same participants twice in a row). Participants were at the beginning of the experiment informed that there would be two parts of the study and that they would be paid for only one part of the experiment. They were only informed of the content of each part immediately before reaching that part of the decision phase. That is, while contributing in Part 1, participants did not know that Part 2 would be a repeated version of Part 1.

3.3.2 Experimental procedures

The experiment was conducted in April 2013 at the Pittsburgh Experimental Economics Laboratory (PEEL) at the University of Pittsburgh. Using a between-subject design we conducted four sessions of each of the two treatments. With 20 subjects per session a total of 160 undergraduate students participated in the study. Each session lasted approximately 45 minutes with average payments being $22.50 per subject (including a $6 show up fee).

Upon entering the lab, participants were seated in a pre-marked cubicle, and were asked to provide informed consent to participate in the study. We then distributed instructions and read them out loud. The instructions provided a general description of the strategic setting. Participants were informed that there would be two parts of the experiment and that one would count for payment. They were given no information on what Part 2 would entail.

For Part 1 they were told that they would be matched in groups of four and that they would each be given an endowment of $10 that they could invest in $1 increments in a group account. Participants were told that investment decisions would affect their payoffs and the payoffs of other group members, but were given no details on the actual payoff structure. They were told that payoff information would be presented to them via payoff tables displayed on the computer screen. The instructions explained to participants how they should read the payoff table and informed them that they would have to complete a tutorial before proceeding with Part 1. Subjects were also informed in the instructions that they would receive feedback on their choices and the choices made by other group members.[55]

After completion of the instructions participants proceeded with the tutorial. Interfaces for the tutorial and for the decision making part of the experiment were programmed using z-Tree (Fischbacher 2007). The tutorial used a payoff table in which subjects had two investment options. The payoffs in each cell were denoted using matrix notation. That is, no monetary payoffs were presented but rather combinations of letters and numbers (e.g., $A11). Participants had to answer six questions in the tutorial, which asked subjects to identify the payoffs associated with different investment choices made by all group members. The tutorial allowed subjects to enter incorrect answers, but presented solutions to ensure proper understanding.

Having completed the tutorial, Part 1 of the decision making phase began. Participants were anonymously matched in groups of four and were shown individual computer screens, which displayed the payoff table and asked participants to make a contribution decision.[56] For a given average contribution made by the other three group members, the payoff table listed the individual's payoff of contributing between $0 and $10 and the average payoffs of the other group members. Time was recorded as the number of seconds it took participants to make a decision after seeing the payoff table and was not presented in the decision screen. Once all contribution decisions had been made participants were shown a payoff screen informing them of the total amount contributed by all group members. Subjects received specific feedback on their contribution, the total and average contribution made by other group members, their payoff and the average payoff of the other group members.

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[55] For the instructions see the Appendix.
[56] The payoff tables used in the experiment are presented in Appendix Table B.1 and B.2.

Having completed Part 1, participants received instructions for Part 2. They were informed that Part 2 consisted of 10 periods of the same decision scenario as in Part 1. They were also informed of the random re-matching procedure and were told that if Part 2 was selected for payment only one randomly selected period would be paid. At the end of each period participants received the same feedback as in Part 1.

At the completion of the decision phase all participants were given a brief demographic questionnaire to determine their age, gender, nationality, year in college, and college major.

3.4 RESULTS

In reporting the results of the experiment we begin by examining behavior in Part 1. For this part of the study we have a precise measure of response time. We then proceed to a brief discussion of Part 2, where we check the stability and convergence of the behavior observed in Part 1.

3.4.1 Part 1: Contributions

We have clear evidence that on average participants responded to the different incentives in the Low and High treatment. Figure 3.1 presents a histogram of contributions by treatment. It shows that the modal contribution in each treatment is precisely the equilibrium prediction. 35 percent of participants in the Low treatment contribute \$3 and 36 percent of participants in the High treatment contribute \$7.[57]

[57] Of the 80 participants in each treatment the number of participants selecting the payoff maximizing strategy equaled 28 in the Low treatment and 29 in the High treatment. The frequency of equilibrium play is greater than that typically documented in the linear VCM, suggesting that our setting is not too difficult for subjects to understand, but is also lower than the frequency of equilibrium play documented in Bracha et al. (2012) who use a piece-wise linear VCM similar to the one used in our experiment but with groups of 2 rather than 4 participants. Bracha et al. document a frequency of equilibrium play of 60 percent across 14 rounds of a comparable treatment. Isaac, Walker, and Thomas (1984) document a frequency of equilibrium play of 30 percent (across 10 rounds) in a linear VMC with various group sizes and various marginal per capita returns (19 percent in the first round of play when group size is four). For a general description of contribution behavior in the linear VCM see Ledyard (1995) and Chaudhuri (2011).

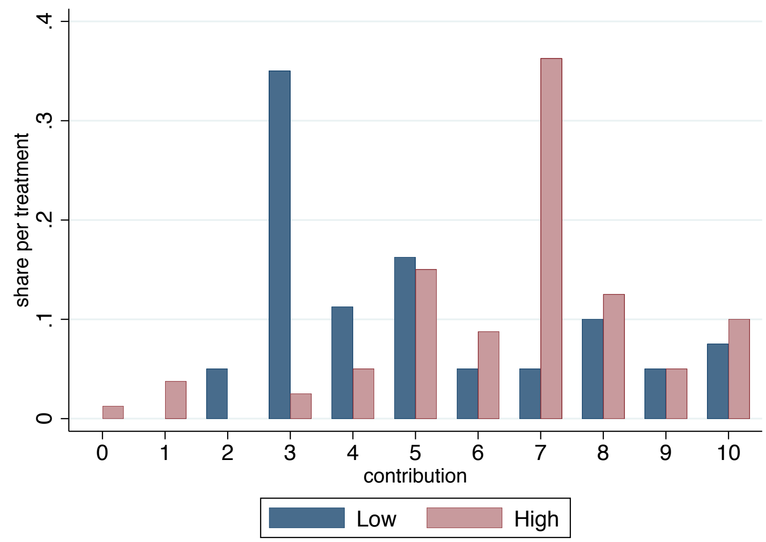

Figure 3.1: Histogram of contributions by treatment, Part 1

In the Low treatment the average contribution exceeds the equilibrium prediction of $3 (mean = $5.06, one-sample t-test p<0.01), whereas in the High treatment it falls short of the equilibrium prediction of $7 (mean = $6.57, one-sample t-test p<0.10). Thus, relative to the equilibrium prediction, participants overcontribute in the Low treatment and undercontribute in the High treatment. Participants in the Low treatment contribute, on average, less than participants in the High treatment (one-sample t-test p<0.01).[58]

Importantly, many participants in both the Low and High treatment make dominated contributions. That is, they contribute an amount that lowers both individual and group payoffs. Furthermore, we see contributions that exceed the Pareto efficient contribution of $9. While benefitting the recipients, the individual cost of giving $10 rather than $9 is so large that the group's aggregate payoff decreases. Interestingly, looking solely at non-equilibrium play the contribution distributions appear to be rather insensitive to treatment.

In fact, absent contributions of $3 and $7 we cannot reject the null hypothesis that the average of the remaining contributions are the same across treatments (the mean contribution in the Low treatment is $6.10 and the mean contribution in the High treatment is $6.47; two-sample t-test p=0.479). Nor can we reject the null hypothesis that (absent contributions of $3 and $7) the two samples come from the same

[58] A Wilcoxon Mann-Whitney rank-sum test provides p<0.01. We also find statistically significant differences in the distribution of contributions across treatments (Kolmogorov-Smirnov p<0.01).

underlying distribution (Kolmogorov-Smirnov p=0.714). This similarity in contributions is particularly striking when considering contributions of $4, $5, and $6. These contributions lie in the core of the strategy space in the Low treatment, but are outside of the core of the strategy space in the High treatment. The incentives associated with these contributions, therefore, differ substantially by treatment.

The frequency of contributions that cannot be justified by altruistically inclined participants, and the similarity in contributions across treatments suggest that some choices may not be reflective of individual preferences over (own and others') payoffs. Combined with the finding that non-equilibrium play results in overcontributions in the Low treatment and undercontributions in the High treatment, this suggests that we need to examine how non-equilibrium play correlates with response time.

3.4.2 Part 1: Response time and contributions

The time it takes participants to make a decision varies substantially. Some participants spend as little as 4 seconds making a decision whereas others spend more than 3 minutes deciding. There is, however, no evidence that response time differs by treatment (Kolmogorov-Smirnov p=0.49). The mean and median response times of 50.61 and 41 seconds in the Low treatment and of 46.78 and 42.5 seconds in the High treatment do not differ statistically significantly between treatments (two-sample t-test p=0.497; Wilcoxon Mann-Whitney rank-sum test z=0.753, p=0.452).

To test the hypothesis that response time is negatively correlated with contribution we regress contributions on response time using OLS.[59] Column 1 of Table 3.2 shows the results of the regression for participants in the Low treatment. The negative and statistically significant coefficient on response time is consistent with the existing findings (Rand et al. 2012). When the equilibrium is located below the midpoint of the strategy space fast decision makers contribute more than slow decision makers. Although the size of the coefficient appears small, it indicates that participants who delay their decision by 1 minute on average contribute $1.14 less than those who make a contribution decision right away.

The correlation between generosity and response time is, however, sensitive to treatment. Column 2 of Table 3.2 shows that in the High treatment the correlation is positive and statistically significant. While of opposite sign, the coefficient on response time is of similar magnitude as that estimated in the

[59] Tobit regressions that take into account the censoring at $0 and $10 are presented in the Appendix and provide similar results.

Low treatment. A participant who delays the decision by 1 minute will, on average, contribute \$0.96 more than someone who makes a contribution right away. Column 3 of Table 2 pools the data from the two treatments to test whether treatment effects are statistically significant. Using a difference-in-difference regression of contributions on response time and treatment column 3 shows two things. First, the mean treatment effect is not significantly different from zero. Second, the correlation between contribution and response time differs significantly by treatment. The coefficient on the interaction of treatment High and response time is positive and statistically significant. The differences in contributions documented in Figure 3.1 thus result with increases in response time.

Table 3.2: OLS regression of contributions on response time, Part 1

Dep. Var.: Contribution to group account	Treatments		
	Low	High	All
	(1)	(2)	(3)
Response time	-0.019**	0.016**	-0.019***
	(0.016)	(0.013)	(0.010)
High			-0.205
			(0.732)
High x response time			0.035***
			(0.001)
Constant	6.024***	5.819***	6.024***
	(0.000)	(0.000)	(0.000)
Total effect response time: High			0.016**
			(0.023)
N	80	80	160

Note: P-values reported in parentheses. *p<0.10, **p<0.05, ***p<0.01.

The results reported in Table 3.2 demonstrate that the strategic environment influences whether fast decision makers are found to be more or less generous than slow decision makers. Looking at contributions by response time it becomes clear why the comparative static reverses with treatment. Figure 3.2 presents a scatterplot of response time and contributions by treatment. The solid vertical line indicates the location of the equilibrium contribution (\$3 and \$7 in the Low and High treatment respectively) and the dashed vertical line indicates the location of the efficiency maximizing contribution (\$9 in both treatments). The horizontal line indicates the median response time of the pooled sample (41.5 seconds). For the remainder of the analysis we refer to participants who used less than the median response time to make a decision as being fast decision makers, and we refer to participants who used more than the median response time to make a decision as being slow decision makers.

The two panels of Figure 3.2 mirror each other and show that fast decision makers choose contributions that are scattered over the entire range of possible choices. In particular, contributions that are dominated from a selfish as well as social perspective are associated with fast response times in both treatments. That is, fast decision makers are more likely to contribute below the Nash equilibrium and above the group welfare maximizing contribution of $9. By contrast slow decision makers are more likely to select a contribution at or close to the equilibrium prediction. Slow decision makers also account for almost all of the efficiency maximizing contributions.

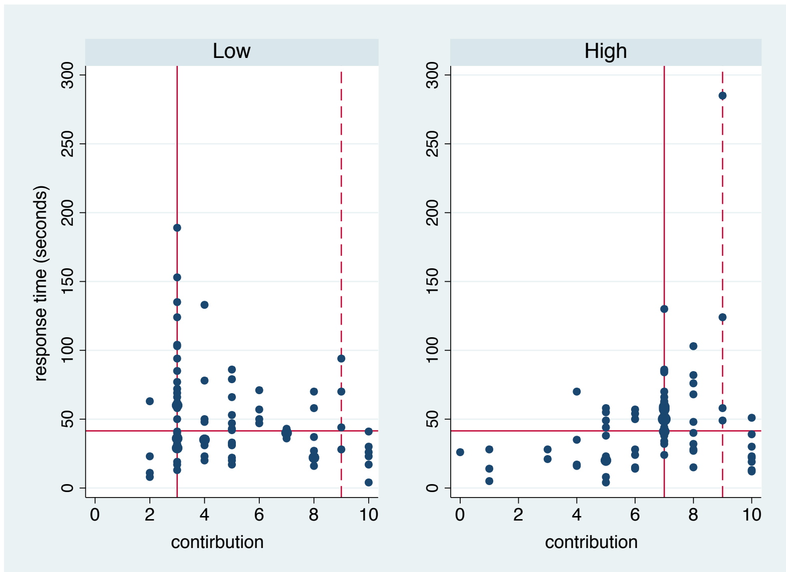

Figure 3.2: Scatterplot of contributions and response time by treatment, Part 1

Note: The solid vertical line indicates the Nash contribution, the dashed vertical line indicates the efficiency maximizing contribution, and the solid horizontal line indicates the median response time of the pooled sample (41.5 seconds).

Table 3.3 summarizes the information contained in Figure 3.2. Defining mistakes as choices that are outside the core, i.e., contributions that simultaneously lower both the individual's and the group's payoffs, we see in both treatments that a large majority (> 70%) of such choices are made by fast decision makers. Hence, we reject the null hypothesis that slow and fast decision makers are equally likely to make mistakes (1-sided Fisher's exact test $p<0.01$ in the pooled sample; $p=0.327$ in the Low treatment and $p<0.01$ in the High treatment). By contrast, the observed equilibrium contributions are more likely to be made by slow decision makers (1-sided Fisher's exact test $p<0.10$ in the Low treatment and $p<0.01$ in the High treatment). Contributions of $10, which at a very high cost increase the payoffs of others and thus are dominated from a group welfare maximizing perspective, are also associated with fast responses (1-sided Fisher's exact test $p<0.05$ in both treatments).[60]

Table 3.3: Contributions by treatment, Part 1

Choices	Low NE n	Low NE (% fast)	High NE n	High NE (% fast)	All n	All (% fast)
Mistakes	4	(0.75)	29	(0.72)	33	(0.72)
Nash equilibrium	28	(0.39)	29	(0.21)	57	(0.30)
Above Nash & below welfare max.	38	(0.53)	10	(0.50)	48	(0.52)
Welfare maximizing	4	(0.25)	4	(0.00)	8	(0.13)
Welfare reducing	6	(1.00)	8	(0.88)	14	(0.93)
Midpoint of strategy space	13	(0.46)	12	(0.33)	25	(0.56)
All	80.00	(0.51)	80.00	(0.49)	160.00	(0.50)

Note: Fast indicates that contribution decisions were made in less than the median response time.

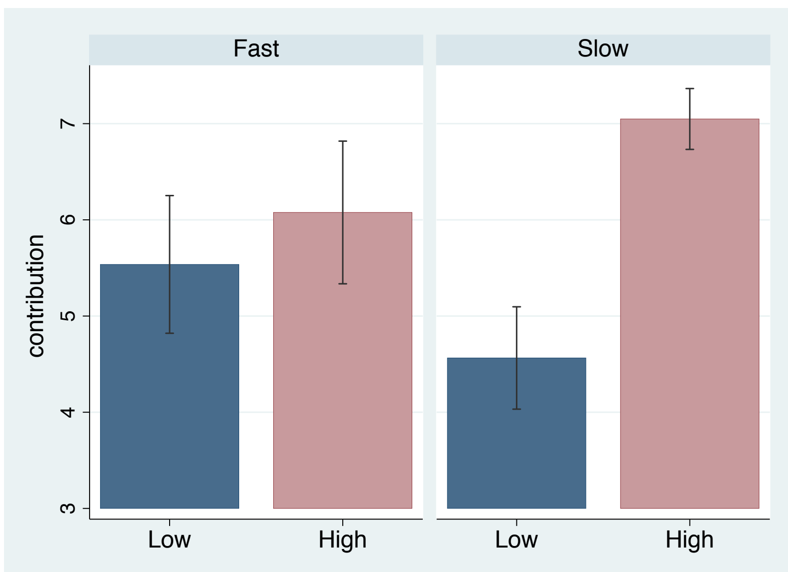

Figure 3.3: Mean contribution share by fast and slow decision makers, Part 1

Note: Fast indicates that choices were made in less than the median response time (41.5 seconds), and slow indicates that choices were made in more than the median response time.

Figure 3.3 presents the mean contributions made by fast and slow decision makers in each treatment. Looking for treatment effects we note that while there are no differences in the average

[60] Additional sessions of the experiment conducted as a robustness check examine whether \$10 choices truly constitute mistakes by making the core of the strategy space strictly interior. See section 3.4.5.

contributions made by fast decision makers (Mean Low = \$5.54, Mean High = \$6.08, one-sided t-test p=0.380), there are substantial differences in the average contributions made by slow decision makers (Mean Low = \$4.56, Mean High = \$7.05, one-sided t-test p<0.01). Consistent with the results presented in Table 3.2 we find in the Low treatment that slow decision makers contribute less than fast decision makers, but that the reverse holds true in the High treatment (one-sided t-test p=0.072 and 0.042 in each treatment respectively). We also note that in both treatments standard errors are smaller for slow decision makers. Tests of differences in the standard deviation of contributions by response time category reject the null hypothesis that the variance of the contributions is the same for fast and slow decision makers (Brown - Forsythe robust test p<0.05 in both treatments).

Are the results reported so far robust with respect to adding other explanatory variables? We examine whether age, gender, the number of tutorial questions answered correctly, training in economics, and experience with laboratory experiments affect response time and contributions in Table B.4 of the Appendix. Results show a non-systematic correlation between variables. Answering a higher number of tutorial questions correctly seems to increase response time in both treatments, but coefficients are statistically insignificant. Importantly, the difference in correlation between response time and contribution documented in Table 3.2 is robust to controlling for all of these variables as well as to excluding outlier observations.[61]

Part 1 has shown that the correlation between response time and giving is sensitive to treatment. Despite the vastly different payoff structures, we find that fast decision makers contribute essentially the same amounts in the High and Low treatments. From this we draw two conclusions. First, fast decision makers are more likely to make mistakes. Second, they are not necessarily more generous. To further investigate the potential role of error we next examine the choices participants make when the game is repeated in Part 2 of the experiment.

3.4.3 Part 2: Contributions

At the completion of Part 1 participants were informed that Part 2 would be a ten period version of Part 1. Behavior in this setting helps determine the robustness of the results documented in Part 1 and the extent

[61] We define outlier observations as the choices made by participants who took more than 150 seconds to decide. The coefficients on response time reported in Table 3.2 do not change when outlier observations are excluded regardless of whether or not the full set of additional controls are included in the regressions.

to which behavior converges in our strategic setting. Response time in Part 2, however, is no longer the only variable capturing the time individuals have had to deliberate about their choices. The period of play, experience acquired by participants, and feedback are all likely to affect both contributions and response time.

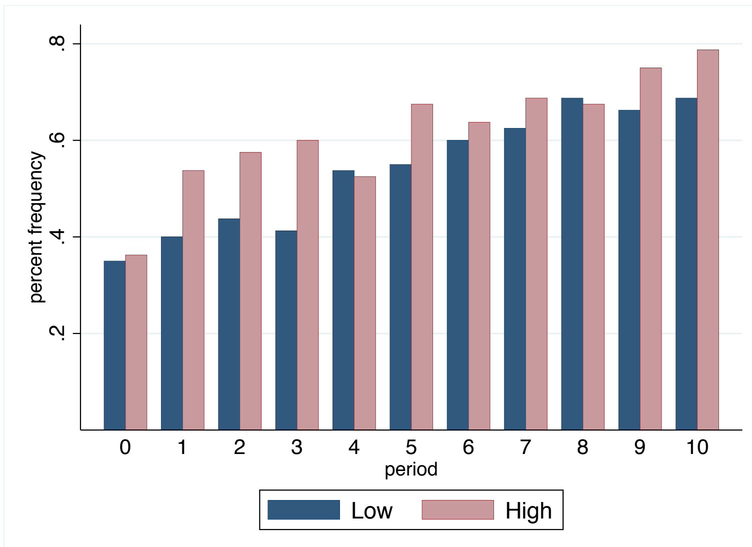

Figure 3.4: Frequency of equilibrium contributions by treatment

(Low $g^L = 3$, High $g^H = 7$)

Note: Period 0 denotes Part 1

We begin by examining how contributions change over the course of the experiment. Figure 3.4 illustrates the share of equilibrium contributions seen in each of the two treatments. Period 0 refers to contributions in Part 1, and period 1 through 10 refer to the ten contribution decisions made in Part 2. Strikingly, we see the frequency of equilibrium contributions doubling from one third in Part 1, to more than two thirds by period 10 of Part 2.

Convergence to equilibrium is also seen in Figure 3.5, where we show the mean and median contribution by period and treatment. Starting in Part 1, participants overcontribute in the Low treatment and undercontribute in the High treatment. However, with repeated play we see mean contributions converging to the equilibrium prediction from above in the Low treatment and from below in the High treatment. While undercontributions are on average eliminated in the High treatment, overcontributions persist in the Low treatment. By period 4, however, the median contribution of each of the two treatments equals the respective equilibrium prediction. While convergence from above in the Low treatment can be justified by other-regarding preferences, there is no such justification for the convergence from below in the High treatment. No selfish or altruistically inclined participant should contribute below the equilibrium prediction in the High treatment.

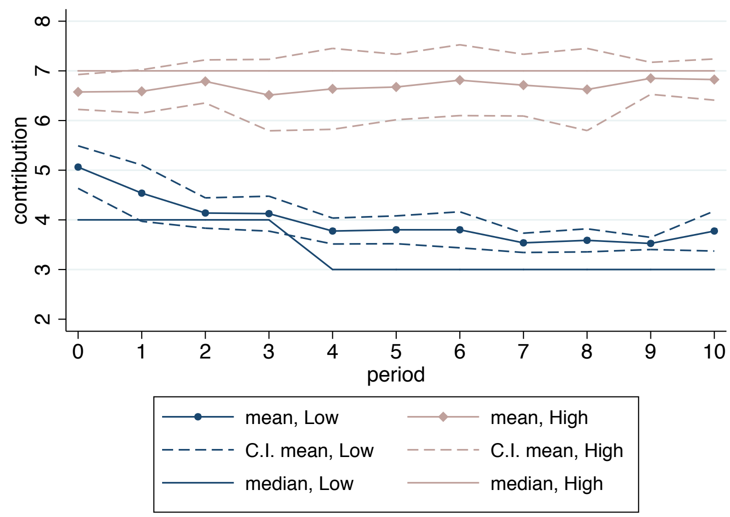

Figure 3.5: Mean and median contribution by period and treatment

Note: 90% confidence interval of mean session contribution shown in graph. Period 0 denotes Part 1.

3.4.4 Part 2: Response time and contributions

In moving from Part 1 to Part 2 we no longer have proper control over deliberation and response time. Nonetheless, we ask if individual response time in any given period of Part 2 correlates with the amount contributed in that period. As response time decreases with repetition the definition of what it means to be a fast and slow decision maker, however, changes over the course of the experiment. Figure 3.6 demonstrates how the median response time changes by period, with the largest decrease being from Part 1 to Part 2 (i.e., between period 0 and 1).[62]

[62] The decrease in response time between Part 1 and 2 is similar in magnitude and statistically significant in both treatments (session-level Wilcoxon Mann-Whitney rank-sum test p<0.05 in each treatment). Response time in period 1 of Part 2 is not statistically different between treatments when session-level tests are conducted (Wilcoxon Mann-Whitney rank-sum test p=0.753). Differences are marginally significant when subject-level tests are performed, which do not take into account the potential correlation introduced by feedback (Wilcoxon Mann-Whitney rank-sum test p= 0.079).

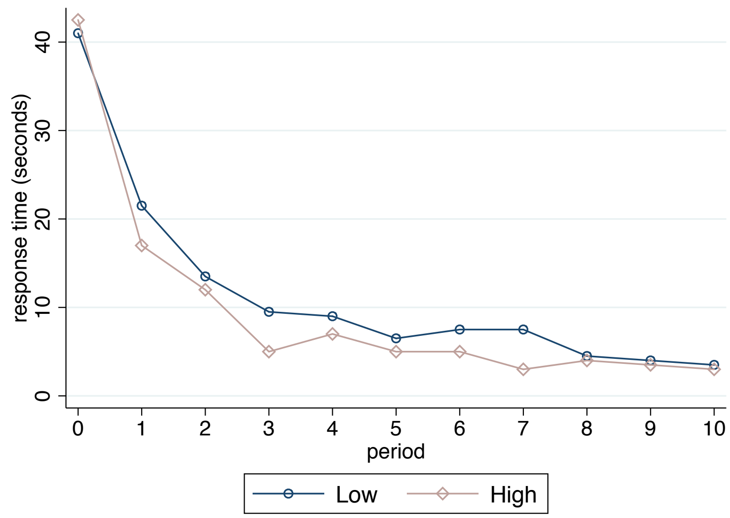

Figure 3.6: Median response time by period and treatment

Note: Period 0 denotes Part 1.

Using Part 2 data we examine the correlation between response time in a particular period and the amount given in that period. Regressing contributions on response time and period and clustering the standard errors at the session level, we secure the OLS regression reported in Table 3.4. The coefficients on response time reveal that neither in the Low nor in the High treatment does per period response time affect per period contributions. Reflective of convergence from the middle the coefficient on period reveals that contributions decrease with each repetition of the Low treatment, and that contributions increase with each repetition of the High treatment. Column 3 of Table 3.4 includes the relevant interaction terms and demonstrates that by the beginning of Part 2 contributions are significantly larger in the High treatment than in the Low treatment and that the differential response to repetition is significant.[63]

[63] Appendix Table B.5 shows regressions of response time and contribution decisions that include history of play variables, comprehension questions, socio-demographic characteristics, training in economics, and experience with laboratory experiments. Estimates of the correlation between response time and contribution do not change when controls are included.

Table 3.4: OLS regression of contributions on response time, Part 2

	Treatment		
	Low	High	All
	(1)	(2)	(3)
Response time	0.014	0.022	0.014
	(0.340)	(0.278)	(0.261)
Period	-0.068**	0.047*	-0.068***
	(0.047)	(0.052)	(0.010)
High NE			2.143***
			(0.003)
High NE x Response time			0.009
			(0.668)
High NE x period			0.115***
			(0.002)
Constant	4.082***	6.225***	4.082***
	(0.001)	(0.001)	(0.000)
Total effect response time: High NE			0.022
			(0.196)
Total effect period: High NE			0.047**
			(0.012)
N	800	800	1600

Note: P-values reported in parentheses.*p<0.10,**p<0.05, ***p<0.01.

The results reveal that the within period correlation between response time and giving is eliminated when participants repeatedly make the same contribution decision. The more important question is, however, whether the classification of participants as slow and fast decision makers in Part 1 helps predict behavior in Part 2. If making a fast decision is reflective of individual preferences over payoff distributions then we may anticipate that fast and slow decision makers in Part 1 also differ in their behavior in Part 2. We see in Figures 3.7 and 3.8 that in the Low treatment the initial difference in response time and contributions is eliminated by period 1 of Part 2. This suggests that if response time is reflective of the individual's type then such differences are eliminated after a short period of reflection. The insensitivity to the initial classification as a fast or slow decision maker in Part 1 of the Low treatment holds when looking at response time as well as mean and median contributions in Part 2.

Looking at the High treatment we see that the differences between fast and slow decision makers decrease with repetition, but some differences persist for the duration of Part 2. Fast decision makers continue to make fast decisions and to make lower mean contributions than slow decision makers. The median contribution is, however, not sensitive to the response-time classification from Part 1. Combined with the convergence toward equilibrium from the middle that we observed in Part 2, we see Part 1 response time not as reflective of preferences over payoffs but rather as evidence of confusion and/or inattention.

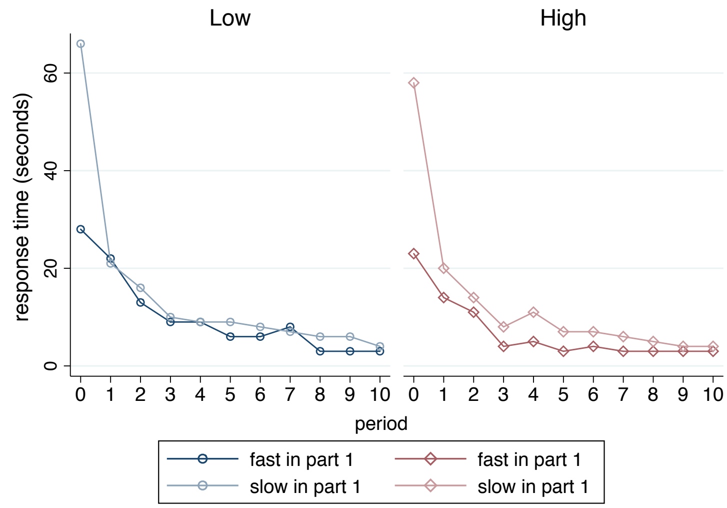

Figure 3.7: Median response time by fast and slow decision makers in Part 1

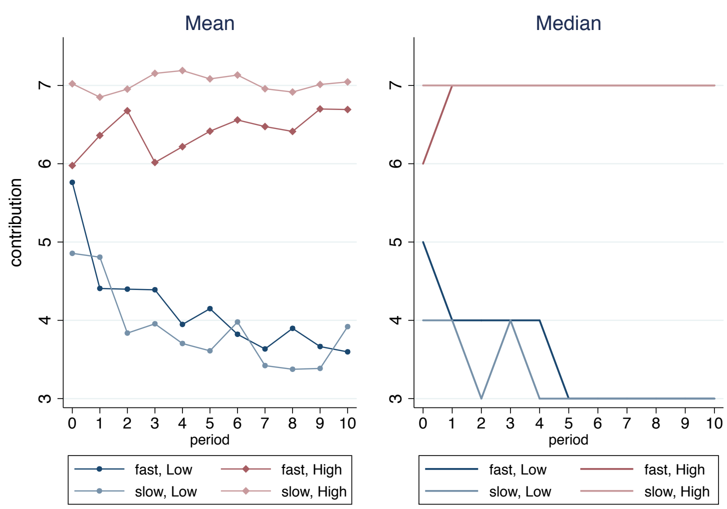

Figure 3.8: Mean and median contribution by fast and slow decision makers in Part 1

Note: Fast and slow denote decision maker type in Part 1. Period 0 denotes Part 1.

3.4.5 Robustness checks

While response time plays a limited role in repeated interaction, our results from Part 1 demonstrate significant and substantial differences in contribution behavior across fast and slow decision makers. Examining public good games with interior dominant strategy equilibria we replicate the Rand et al. (2012) finding when the equilibrium is located below the midpoint of the strategy space, but find the opposite correlation when the equilibrium is located above the midpoint of the strategy space. Fast decision makers are less generous than slow decision makers when the equilibrium is above the midpoint. Despite the substantial differences in payoffs between the Low and High treatments, we are for fast decision makers unable to distinguish contribution distributions by treatment. We also find that fast decision makers are more likely to make contributions outside the core of the strategy space, and that they account for the vast majority of $10 contributions (which decrease the group's aggregate payoffs). Based on these findings we conclude that the correlation between response time and contributions may not reflect `spontaneous giving´, but rather the fact that confused participants quickly select a contribution that lies, on average, in the middle of the strategy space.

An argument, however, can be made to oppose the claim that our results shed light on the negative correlation between response time and giving uncovered by Rand et al. (2012). Although the interior dominant strategy equilibrium and interior Pareto efficient outcome we employ make it easier to draw inference on what motivates individuals to contribute, the resulting payoff structure is more complicated than that seen in the linear VCM examined by Rand et al. This more complicated payoff structure may, in and off itself, have confused participants. The reversal of the correlation between response time and giving may not arise in simpler and more transparent settings similar to the linear VCM.

We investigated behavior in Low and High versions of the linear VCM to determine whether the reversal of the correlation between response time and giving in the Low and High treatment arises with a simpler payoff structure. That is, we replaced the piece-wise linear payoff structures described in Section 3.3.1 with Low and High versions of the standard 4-person linear VCM. To secure a payoff range comparable to our earlier study we provided each participant with an $8 endowment. Following past VCM procedures the payoffs were characterized without the use of a payoff table. Incentives in the Low-VCM were identical to those employed in Rand et al. (2012), every dollar contributed to the group account was doubled and split equally between group members. With a marginal per capita return (MPCR) of $0.50, the unique dominant strategy equilibrium contribution in the Low-VCM is on the lower boundary of the strategy space (contribution of $0). The incentives in the High-VCM instead placed the unique dominant strategy equilibrium contribution on the upper boundary of the strategy space (contribution of $8). Participants in the High-VCM were informed that every dollar contributed to the group account would be doubled and split equally between group members, and that any individual who contributed a dollar would get an additional individual bonus of 60 cents. Hence the MPCR in High-VCM remained $0.50, with an added individual bonus for contributing of 60 cents. Payoffs ensured that the welfare maximizing choice, $8, and the marginal benefit to others form contributing, $0.50, was kept constant cross treatments.[64]

Relative to our piece-wise linear payoff structure, the linear payoffs of the Low-VCM and the High-VCM make it more difficult to infer what motivates a contribution. In the Low-VCM deviations from the dominant strategy may result from mistakes or concern for others; contributions outside the core of the strategy space cannot arise. In the High-VCM deviations from the dominant strategy lie outside the core, while the dominant strategy simultaneously maximizes individual and group payoffs. In the Low-VCM treatment it is thus not possible to separately identify mistakes and generosity, while in the High-VCM treatment it is not possible to separately identify generosity and payoff maximizing behavior.

We conducted six sessions of the linear VCM, two sessions of the Low-VCM treatment and four sessions of the High-VCM treatment.[65] 112 individuals participated in the linear VCM sessions. The results corroborate our findings. Fast responses in Part 1 continue to be associated with contributions that are scattered over the entire range of possible choices. Furthermore, we also find that mean contributions by fast decision makers are not distinguishable across treatments (Mean Low-VCM = $3.53, Mean High-VCM = $3.81, two-sample t-test p=0.70). By contrast, we can distinguish the mean contributions made by slow decision makers by treatment (Mean Low-VCM = $4.05, Mean High-VCM = $5.71, two-sided t-test p=0.03). As in our piece-wise linear High treatment, we find in the High-VCM that slow decision makers contribute more than fast decision makers. In the Low-VCM we do not find that response time decreases giving. While not consistent with Rand et al.'s finding, this is consistent with the results from our piece-wise linear treatments where slow decision makers are more likely to select both the equilibrium and group welfare maximizing choices.[67] The results from the linear VCM are thus consistent with our findings in the initial piece-wise linear treatments. Fast decision makers, on average, make choices in the middle of the strategy space while slow decision makers are more likely to make contributions that are

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[64] The exact instructions and payoff descriptions employed in the linear VCM treatments are included in the Appendix. As in our main design payoff descriptions were provided on the screen used to make contribution decisions. Response time was measured from the moment the decision screen was displayed to subjects.

[65] Two of the High-VCM sessions presented the bonus as an individualized 60 cent bonus for contributing, and two of the High-VCM sessions added the comment that the return from contributing was $1.10. While average contributions are slightly higher in the second High-VCM treatment (Mean High-VCM 1 = $.4.19, Mean High-VCM 2 = $5.29, t-test p-value = 0.09) the general insights from the two treatments are the same. For the purposes of this discussion we therefore pool the data for the two High-VCM treatments.

[67] Only 38 percent of Nash and welfare maximizing choices were made by fast decision makers in the Low-VCM treatment, while 33 percent of choices were made by fast decision makers in the High-VCM treatment.

reflective of preferences over payoffs. Rather than revealing less confusion than in our piece-wise linear treatment, behavior in the linear VCM experiments suggest limited convergence to equilibrium.[68]

In analyzing the results form Part 1 of our piece-wise linear treatment we noted that contributions of $10 were predominantly made by fast decision makers. Noting that the individual cost of contributing $10 rather than $9 was substantial and only resulted in a limited increase in earnings for others, we argued that these group payoff-reducing contributions may be seen as mistakes.[69] However, the $10 contributions may be justified for someone who values payoffs to others much more than payoff to self. To further determine whether $10 contributions should be seen as mistakes we modified the payoff structure of the original experiment to secure that individuals in increasing their contributions from $9 to $10 not only lowered their own payoff but also lowered the payoff of every other member of the group. That is, we made the marginal benefit of contributing $10 negative for both the individual and each of the other group members.[70] This change in payoffs places the core entirely in the interior of the strategy space in both treatments and makes it possible to determine whether $10 contributions reflect altruistic preferences or mistakes. Four sessions were conducted with this alternative payoff structure, two with the new-Low and two with the new-High treatment (40 individuals participated in each treatment). All other parameters and experimental procedures were unchanged relative to our original piece-wise linear design.

Results from the new-Low and new-High treatments confirm the fact that $10 contributions constitute mistakes, not altruistic choices. Out of 40 individuals observed in Part 1 in each of the new treatments, 10 percent contributed $10 in the new-Low treatment and 15 percent contributed $10 in the new-High treatment. As in the original experiments we find that $10 contributions are made primarily by

[68] Results from Part 2 of the linear VCM treatments show convergence to equilibrium from above in the Low treatment but no convergence to equilibrium in the High treatment. Participants in the High treatment contribute on average, between $4 and $5 across all periods of Part 2. The lack of convergence in the High treatment seems to be driven by the fact that confused participants never learn the payoff maximizing choice while participants who understand the payoff structure get frustrated and preemptively punish free-riding at a cost. Using Part 1 response time and measures of payoff understanding to analyze Part 2 choices supports this conjecture. Fast decision makers in Part 1 make choices that are closer to the middle of the strategy space in Part 2 as do participants who did not understand the payoff maximizing choice. Two measures of payoff understanding were used to analyze Part 2 behavior: past equilibrium play, and answers to post-decision questionnaires. Participants who did not understand the equilibrium gave between $4 and $5 throughout Part 2, while participants who understood the payoff structure gave between $5 and $8.

[69] In the original treatments (with piece-wise linear payoff structures) the marginal cost of giving $10 rather than $9 was $2.25 in the Low treatment and $1.25 the High treatment. The marginal benefit to others from contributing was $0.75, $0.25 per group member. Contributions of $10 rather than $9 thus decreased total group payoffs by $1.50 and $0.50 in the Low and High treatments respectively.

[70] The payoff tables used in these additional sessions of the experiment are reported in Appendix Table B.6 and B.7. They changed δ to -2.25 and -0.25 in the new-Low and new-High treatments respectively and made σ=-0.15 in both treatments when $g_i > 9$.

fast decision makers. Fast decisions are associated with 75 percent and 83 percent of the $10 contributions in the new-Low and new-High treatment respectively.[71]

The results of both of our robustness checks reveal behavior consistent with our interpretation of the data in our piece-wise linear design. Fast decision makers are more likely to select contributions that lie, on average, in the middle of the strategy space, and the characteristics of these fast decisions are such that it is difficult to see them as reflective of preferences over payoffs to self and to others.

3.5 CONCLUSION

Our study questions whether response time can be used to identify the intuitive choice in pro-social settings. Building on Rand et al. (2012) we investigate whether the correlation between response time and giving is sensitive to the strategic environment. Examining contributions in two public good games with interior equilibria, we show that the correlation between response time and giving changes with the location of the equilibrium. The correlation is negative when the equilibrium is located below the midpoint of the strategy space, and positive when the equilibrium is located above the midpoint of the strategy space. Our setting allows us to identify mistakes as choices that lower both individual and group payoffs. We show that the frequency of mistakes decreases with response time. This suggests that fast responses, rather than being reflective of an intuitive action, result from fast decision makers quickly selecting contributions that lie, on average, in the middle of the strategy space.

Our study offers a possible explanation for the mixed relationship between response time and generosity that has been documented in other strategic settings. In particular, it may help explain why the correlation between giving and response time is positive in simple environments such as the dictator game

[71] The modification of the payoffs resulting from a $10 contribution does not alter the direction of the documented comparative statics. With the smaller sample size some results are, however, not statistically significant. The correlation between response time and contributions in Part 1 is negative in the new-Low treatment and positive in the new-High treatment. The mean contributions made by fast and slow decision makers show that fast decision makers make undistinguishable choices (Mean new-Low = $6.22, Mean new-High = $6.00, one-sided t-test p=0.599), while slow decision makers make contributions that are statistically significantly different (Mean new-Low = $5.00, Mean new-High = $6.88, one-sided t-test p<0.01). Differences within treatment show that contributions move in the direction of the Nash prediction but are marginally insignificant in the new-High treatment (one-sided t-test p=0.053 and p=0.133 in the new-Low and new-High treatments respectively). Part 2 results show the same convergence documented in section 3.4.3. Contributions of $10 are scarce in Part 2, and disappear after period 8 in both treatments.

(Piovesan and Wengström, 2008; Fiedler, Glöckner, Nicklish and Dickhert, 2013)[72] but negative in more complex environments such as the linear public good game (Rand et al. 2012, Lotito et al. 2012, Nielsen et al. 2013) and the ultimatum game (Brañas-Garza, Meloso, and Miller 2012).[73]

The results of our paper extend beyond the study of response time and intuitive choices in the pro-social setting. In particular, it suggests that independent of the environment caution is warranted when trying to draw inferences about preferences from response time. Our results demonstrate that fast decision makers are rather insensitive to the payoffs associated with their choices. Absent a model of error, the choices made by slow decision makers may therefore be a better indicator of the types of preferences we should expect in environments where individuals have had experience and time to decide.

[72] Fiedler et al. (2013) also document a positive correlation between response time and giving in the linear public good game. They use eye-tracking technology to inspect information search patterns and show that pro-social types take longer times to make decisions and search more information than selfish types in both the modified dictator game and the linear public good game.

[73] Rubinstein's (2007) analysis of response time and ultimatum game offers shows a negative correlation between response time and offers only when offers above the 50-50 threshold are excluded. Offers in excess of 60 percent of the endowment are associated with fast response times, while offers between 50 and 60 percent of the endowment are associated with long response times.

4.0 DISHONESTY, TOLERANCE, AND SOCIAL INFORMATION

4.1 INTRODUCTION

Dishonesty is a complex collective action problem that affects all cultures. It originates at the individual level, with the decision to engage in dishonesty but has severe aggregate consequences that affect social, political, and economic life. Even though dishonesty is disapproved by society and typically condemned through laws, the prevalence of this type of behavior suggests that for some individuals the benefits outweigh the costs. Standard economics has given attention to the material costs and benefits that influence decisions in this setting. Only recently have authors recognized that non-material rewards matter and that they can also affect the individual decision to engage in dishonesty.[74] This paper investigates dishonesty from the latter perspective. It conducts two experiments to investigate: (1) whether individuals are more likely to engage in dishonest behavior when they observe others engage in dishonesty, and (2) whether individuals are more likely to tolerate dishonest behavior when they observe others engage in dishonesty. The primary goal of this study is to investigate the role of tolerance in halting or possibly facilitating the contagion of dishonest behavior.

The strategic setting employed has subjects play a modified investment game in pairs. A first mover first decides whether or not to invest an endowment earned earlier in the experiment with a second mover. Investment with the second mover is always beneficial for the first mover and can generate either a high or a low return on the investment. The second mover observes the return on the investment, when the first mover invests with the second mover, and announces a return to the first mover. Truthful announcements split the net profit equally between players. Untruthful announcements allow the second mover to take an unequal share of earnings. The probability that the return observed by the second mover is high is common knowledge among participants. The first mover, however, only learns the return

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I thank Lise Vesterlund for supporting this study, and seminar participants at the UCSD Deception, Incentives, and Behavior Symposium , NYU International ESA meeting, and the University of Pittsburgh for helpful comments.

[74] See, for example, Gneezy (2005), Mazar, Amir, Ariely (2008), and Gino, Ariely, and Ayal (2009).

announced by the second mover. Tolerance is studied through the behavior of first movers, who can make inferences about the honesty of second movers and forego investment in a repeated setting at a cost.

Treatments vary whether or not subjects observe the returns announced by four second movers in one phase of a previous session of the experiment. In an Honest Information Treatment (HIT) subjects observe information reflecting honest behavior. In a Dishonest Information Treatment (DIT) subjects observe information reflecting dishonest behavior. Experiments vary the return to the outside option available for the first mover and how transparent implementation procedures make it for participants to know that dishonesty is permitted in the experiment. Participants in Experiment 1 need to recognize that they can engage in dishonesty and decide on their own whether or not they want to do so. Experiment 2 uses instructions and comprehension questions that go over the payoffs associated with all possible announcement scenarios to rule out learning and confusion as a possible explanation of treatment effects. It does so at the expense of priming selfishness, which can affect the moral conflict associated with engaging in dishonest behavior.

Results show that social information about the prevalence of dishonest behavior affects the likelihood that individuals engage in and tolerate dishonesty. In Experiment 1, exposure to social information reflecting dishonest behavior increases the likelihood that individuals engage in dishonesty, while exposure to social information reflecting honest behavior has a weak effect on behavior. Experiment 2 seems to hit a lower bound in the rate of honesty that can be observed in the experiment, and does not show any treatment effects generated by social information. A subset of individuals can, nevertheless, be identified in Experiment 2 who become more likely to engage dishonesty when they observe others engage in dishonesty. The behavior of these movers cannot be explained by confusion or learning given that the implementation procedures used in Experiment 2 ensure that subjects understand not only that dishonesty is permitted in the experiment but also the consequences associated with engaging in dishonest behavior. Results on first mover behavior suggest that tolerance does not facilitate the contagion of dishonesty. First movers become more likely to forego investment with the second mover (and thus punish dishonesty) when they are exposed to social information reflecting dishonest behavior. Tolerance results are robust in Experiment 2, where punishing dishonesty is less costly, but weak in Experiment 1 where exposure to dishonesty causes first movers to invest more in initially but invest less subsequently in the experiment.

This paper makes several contributions to literature. First, it constructs a strategic setting suitable for the study of dishonesty in one-shot and repeated settings in the laboratory. Subjects can inherently recognize the dishonest nature of their actions and inflict a direct negative externality on another equal participant by engaging in dishonest behavior. Second, this paper extends the set of conditions under which dishonesty has been found to be contagious. Previous studies in psychology (Gino, Ayal, Ariely 2009) and economics (Innes and Mitra 2013) have studied whether or not dishonesty is contagious in one-shot settings that include cheating on performance tests and truth telling in sender-receiver games. This paper investigates the contagion of dishonesty in setting designed to encompass a larger class of social dilemmas, such as theft and corruption, where dishonesty has direct harmful consequences on other equal participants. Third, the paper investigates for the first time the role that tolerance plays in halting or possibly facilitating the contagion of dishonest behavior. To the best of my knowledge no study has ever investigated the effect that social information has on tolerance, which can attenuate or exacerbate the effect that social information has on the likelihood that individuals engage in dishonesty.

The structure of the paper is the following. Section 4.2 describes the existing literature. Section 4.3 presents the experimental design. Section 4.4 discusses the results, and section 4.5 concludes.

4.2 RELATED LITERATURE

The experimental economics literature on dishonesty and corruption is fairly recent. It emerged in 2000 with the work of Frank and Schulze (2000) who used a hypothetical question to study dishonesty. The work that developed thereafter has primarily used modified versions of the trust game of Berg et al. (1995) to study corruption. Abbink et al. (2002) were the pioneers in this approach and used the trust game to study bribing in the laboratory. In their experiment a firm decides whether or not to bribe a public official. The public official decides whether or not to accept a bribe and whether or not to grant a favor to the bribee. The original experiment used neutral instructions where actions did not have the negative connotations implied by corruption. Subsequent papers tested this implication by assigning subjects hypothetical roles. Abbink and Hennig-Schmidt (2006) showed no statistically significant differences in behavior generated by role assignment. Barr and Serra (2009), however, showed that in a petty corruption experiment where subjects were assigned the more familiar role of citizens instead of firms, loaded instructions matter. The experiment I conduct overcomes the challenges encountered by this early literature by employing a modified trust game in which the nature of dishonesty can be inherently recognized by subjects and where there is no need to assign hypothetical roles.

Although an array of issues within bribery have been investigated using modifications of Abbink et al. (2002), related to this study are the papers that focus on the role of social norms and culture. Cameron et al. (2009) studied the propensity to engage in bribery and to punish it using a modified trust game with role assignment in four different countries: Australia, India, Indonesia, and Singapore.[75] The study documented no clear and robust relationship between the prevalence of corruption in a country and subject's behavior in the experiment. Barr and Serra (2010) conducted a bribery experiment using another modified trust game with role assignment in the United Kingdom. They recruited subjects from different nationalities and found that that the individual likelihood to engage in bribery correlates with the prevalence of corruption in subjects' home countries.[76] The same type of correlation between the prevalence of corruption in a nation and socially undesired behavior was found by Fisman and Miguel (2007), who examined the parking behavior of UN officials protected by diplomatic immunity in New York City. Unlike these papers, which investigate social norms through the broad definition of culture, I manipulate the perceived social norm of honesty exogenously in the laboratory.

Other popular settings that have been used to study dishonesty in the laboratory include truth telling in sender-receiver games (Gneezy 2005) and cheating when self-reporting scores on performance tests (Mazar, Amir, and Ariely 2008). The deception game of Gneezy (2005) has a sender inform a receiver which of two options yields a higher payoff. The receiver takes an action that determines the payoffs of both players but has no knowledge about the structure of incentives. The sender can thus tell the truth or lie; the receiver has to choose whether or not to believe the message sent by the sender before making a choice. The strategic setting employed in this paper is superior to that of Gneezy (2005) because it eliminates any moral wiggle room by which subjects may justify their behavior as honest. In Gneezy (2005) lying can be rationalized as other-regarding if the sender believes that the receiver will not believe the message. Independent of beliefs, the choices made by the potential victims of dishonesty determine the final payoffs. Engaging in dishonesty by lying thus does not directly inflict a negative externality on another participant. In Mazar et al. (2008) subjects self-report scores on a performance test. Scores determine payoffs; and subjects can cheat to earn more money by inflating scores. The setting employed in this paper is superior to Mazar et al. (2008) because engaging in dishonesty implies lying and taking money from another equal participant rather than from the experimenter. Dishonesty can be rationalized

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[75] They used a three-player one-shot bribery game where a firm chooses whether or not to bribe a public official, the official chooses whether or not to accept the bribe, and subsequently a third party affected by the bribery exchange decides whether or not to punish the briber and bribee.

[76] This correlation, however, is only statistically significant among undergraduates. The authors argue that socialization and time spent in another country changes the norms that individuals use to guide their behavior.

as welfare enhancing in Mazar et al. (2008) if participants care only about the total money earned by subjects in the experiment.

The two papers that have investigated whether dishonesty is contagious have used the strategic settings of Gneezy (2005) and Mazar et al. (2008). Gino et al. (2009) have subjects self-report scores on a performance test and manipulate the perceived social norm of honesty in the laboratory exogenously by hiring an actor who cheats bluntly in front of participants. Results show that observing a group member cheat increases the prevalence of cheating but observing an outsider do so decreases it. Interestingly, observing someone cheat without the group identity element increases honesty in the experiment.[77] Innes and Mitra (2013) study whether lying is contagious. They conduct an experiment in Arizona and India where subjects play the deception game of Gneezy (2005) and are given information about the percentage of players that sent untruthful messages in a previous session of the experiment. Their results show that the prevalence of truth telling decreases when subjects in the Arizona experiment are told that 85 percent of subjects in another session were untruthful (does not change when that percentage is 15, 40 or 60) and increases in India when subjects are told that 85 percent of subjects were truthful. Additional sessions of the experiment that allowed subjects to draw messages from a box containing all messages sent in previous sessions of the experiment show that observing 3 of 5 participants be untruthful makes individuals more likely to lie. This is not driven by an effect of social information on preferences over allocations.

Unlike the existing studies on contagion, the present study does not use deception or loaded terms in the instructions. Subjects are presented with the sequence of choices made by other participants in a previous session of the experiment and have to individually interpret such information. More importantly, the present study investigates contagion in a repeated setting for the first time, which allows tolerance to halt or facilitate the contagion of dishonest behavior.[78]

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[77] The authors argue that this results from the fact that observing someone else cheat increases attention to standards, which increases honesty.

[78] Related to this study is also work on social influence and social information conducted within the context of socially desired and undesired behavior (e.g., Cason and Mui 1998; Cialdini, Reno, and Kallgren 1990, 2000; Cialdini et al. 2006; Cialdini 2007; Croson and Shang 2008; Duffy and Kornienko 2010).

4.3 EXPERIMENTAL DESIGN

This study employs a between subject design that varies whether or not subjects are exposed to social information about the behavior of other participants and whether or not such information reflects honest or dishonest behavior. It conducts two experiments, each of which employs the same session structure and basic experimental design. In Phase 1 subjects perform the real effort task of Niederle and Vesterlund (2007) to secure an endowment for subsequent phases. The task consists of adding as many sets of five two-digit numbers as possible in five minutes. Participants secure \$0.50 in endowment for every problem they solve correctly. Additionally, they receive an additional fixed sum of \$2.00 that is independent of performance. Subjects know before performing the real effort task that the endowment they secure determines the earnings that they can receive in the experiment. They are not informed in Phase 1 about the activities that will be carried out in Phases 2 and 3 and are not aware of how their endowment will affect the earnings that they can receive in the experiment.

Instructions for Phases 2 and 3 are distributed after the completion of Phase 1. At the beginning of Phase 2 participants are randomly assigned roles as first and second movers. They are then matched with a participant of the opposite role. Phase 2 has subjects play an investment game repeatedly with the same participant ten times. Phase 3 rematches subjects with a different participant and has them play the same investment game repeatedly ten additional times. Roles are maintained throughout the duration of the session and no participant is matched with the same person twice. The re-matching is done in such a way that the number of independent observations (subgroups) within each session is maximized. Subjects are informed at the beginning of Phase 2 that one of the 20 decision rounds that make up Phases 2 and 3 will be randomly selected to count for payment. They are given a quiz to check their understanding and are subsequently provided a solution key that is read out loud by the experimenter. The quiz and solutions are given to participants before any decisions are made.

4.3.1 Investment game

The investment game played repeatedly by subjects in Phases 2 and 3 is depicted in Figure 4.1. A first mover (I) first decides whether or not to invest with a second mover (II) the endowment (x_1) earned in Phase 1. Foregoing investment with the first mover provides a constant return on the investment (y) that is kept entirely by the first mover. Investment with the second mover generates a return on the investment that is shared between movers and can be either low or high. A low return doubles investment and generates a net profit equal to the invested endowment. A high return triples investment and generates a net profit equal to twice the invested endowment. When the first mover invests with the second mover, the second mover observes the return on the investment and announces it to the first mover. The second mover can tell the truth or lie. Truthful announcements provide an equal split of the net profit between movers. Untruthful announcements allow the second mover to take a larger share of the earnings when return is high, and to give up earnings when return is low. The return observed by the second mover is randomly selected by the computer and acquires a high value 3/4 of the time. The likelihood that the return selected by the computer is high is common knowledge among participants. The second mover, however, is the only participant who observes the actual return on the investment. The first mover thus never learns if the second mover told the truth or lied.

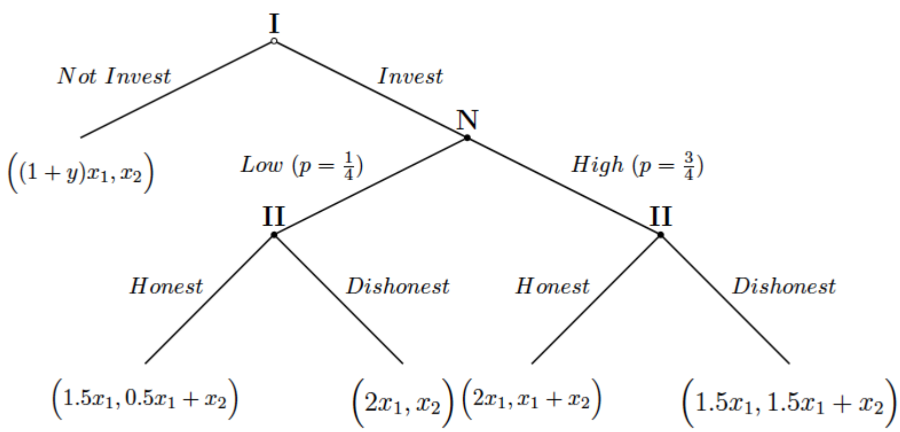

Figure 4.1: Investment game

The Nash equilibrium of the investment game depicted in Figure 4.1 is unique if $y<0.5$, which is secured by the parameterization employed in the experiments. It consists of the first mover investing with the second mover and of the second mover always announcing a low return on the investment. The unique Nash and subgame perfect equilibrium of the finitely repeated game is for first movers to always invest with the second mover and for second movers to always announce a low return on the investment.

Deviations from the Nash prediction can emerge in the stage game if second movers value honesty, or alternatively care about payoff differentials. Two features of the design minimize the last channel. First, subjects earn their endowment in Phase 1. Second, honest announcements provide an equal split of net profits.[79] Second movers valuing honesty can be modeled by letting individuals have preferences denoted by $U(x_1, x_2) = m(x_1, x_2) - c(\theta, x_1, x_2)$, where $m(x_1, x_2)$ is the material payoff of the stage game and $c(\theta, x_1, x_2)$ is the cost of engaging in dishonesty. Parameter θ captures the strength of the social norm of honesty and can be interpreted as the prevalence of honest behavior in a reference group or in society. Suppose that $c(\theta, x_1, x_2)$ is positive and increasing in θ when lying inflicts a negative externality on another equal participant. Then second movers should engage in dishonesty when return is high only if $1.5x_1 + x_2 - c(\theta, x_1, x_2) \geq x_1 + x_2$. Since θ is not perfectly known by participants, subjects will engage in dishonesty when $x_1 \geq 2E[c(\theta, x_1, x_2)]$. Social information provides information about θ to participants, and thus predicts that honesty should increase with the prevalence of honesty reflected in the social information.

First mover behavior in the stage game can be affected by social information about θ directly and indirectly. A direct effect may be present if first movers experience a disutility from being the victims of dishonesty that is increasing in the prevalence of honesty in society. Indirectly, information about θ may affect the beliefs that first movers have about the behavior of second movers and thus their investment.

The finitely repeated game introduces strategic considerations that can mute or exacerbate the direct effects of social information on first and second mover behavior. For example, social information reflecting dishonest behavior may decrease $c(\theta, x_1, x_2)$ making dishonesty more permissible for second movers, but may also affect the likelihood that first movers punish dishonesty. If first movers become more likely to punish dishonesty when dishonesty is prevalent, then this may counteract the incentive to engage in dishonesty that is generated by the fall in $c(\theta, x_1, x_2)$. If first movers become less likely to punish dishonesty when exposed to dishonest others, then this may further increase the likelihood that individuals engage in dishonest behavior.

4.3.2 Treatments

Treatments in each experiment vary whether or not subjects observe the behavior of other participants in a previous session of the experiment and whether or not such information reflects honest or dishonest behavior. In a No Information Treatment (NIT) subjects are not presented with any social information. In

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[79] Individuals would need to be extremely inequality averse to justify dishonesty through fairness or equality considerations

an Honest Information Treatment (HIT) subjects are shown the sequence of returns announced by four second movers who behaved honestly in one phase of a previous session of the experiment. In a Dishonest Information Treatment (DIT) subjects are shown the sequence of returns announced by four second movers who behaved dishonestly in one phase of a previous session of the experiment. The sequences of returns presented to subjects in each of the treatments are displayed in Appendix Tables C.1 and C.2. A Computer Treatment (CT) has subjects play the investment game as second movers against the computer. This treatment seeks to rule out confusion as a possible explanation for the honesty observed in the experiment and does not provide subjects any social information.

Experiments vary the payoff associated with the first mover's outside option and some implementation procedures. In Experiment 1 the return on the outside option was 0 and thus provided no additional earnings. Subjects were only quizzed in Experiment 1 about the payoffs associated with truthful announcements. Considerable efforts were made not to prime dishonesty in Experiment 1 for two reasons. First, to let subjects realize on their own that they could lie and take earnings from the second mover; and second, not to affect the moral conflict associated with engaging in dishonesty. In Experiment 2, the return to the outside option was increased to 0.4 percent to give tolerance a superior chance of playing a role. The strategy method was also employed in Experiment 2 to avoid losing observations when first movers chose not to invest with the second mover. Experiment 2 also used instructions and comprehension questions that went over the payoffs associated with all possible announcement scenarios. This was done to rule out confusion as a possible explanation of the treatment effects documented in Experiment 1, but may have primed selfishness and possibly affected the moral conflict associated with engaging in dishonesty.

The specific treatments conducted in each experiment are depicted in Table 4.1. The information presented to subjects is the same across experiments within treatment; it was collected during pilot phases of the study. Experiment 1 presented the social information to subjects once at the beginning of Phase 2. Experiment 2 also gave participants a handout with the social information that could be consulted any time throughout the duration of Phases 2 and 3. Incentivized questionnaires conducted at the end of Experiment 2 validate the social information presented to subjects.[81]

[81] Subjects correctly assess that the sequence of investment returns shown to participants in HIT and DIT reflects honest and dishonest behavior respectively.

Table 4.1: Experimental design

Treatments	Experiment 1 y=0	Experiment 2 y=0.4
No Information (NIT)	X	
Honest Information (HIT)	X	X
Dishonest Information (DIT)	X	X
Computer (CT)		X

4.4 RESULTS

A total of 216 individuals participated in 18 sessions of the study conducted at the Pittsburgh Experimental Economics Laboratory (PEEL) at the University of Pittsburgh. 108 individuals participated in the 9 sessions that make up Experiment 1 and 216 individuals participated in the 9 sessions that make up Experiment 2. Experiment 1 conducted 3 sessions per treatment while Experiment 2 conducted 4 sessions per Information Treatment and 1 session of the Computer Treatment. Each session included 12 participants. The experiment was programmed using z-Tree (Fischbacher 2007).

Table 4.2 shows summary statistics by experiment and treatment. Half of all participants in each of the Information Treatments were assigned the role of first movers and half were assigned the role of second movers. Gender was balanced within sessions but not across roles. Participants solved an average of 11.23 problems correctly in Phase 1 and earned an average endowment of $7.60. Interestingly, the number of problems solved correctly in Experiment 2 was higher than in Experiment 1 (Wilcoxon Mann-Whitney rank-sum z=1.861, p=0.02).[82] Individuals made on average $18.50 dollars for participating in one session of the study, which lasted approximately 1 hour.[83]

[82] Differences may have emerged as a result of unwanted changes in the implementation, but are not statistically different across treatments within each of the experiments. Experiment 1 was conducted in the Fall of 2011 while Experiment 2 was conducted in between April 2012 and October 2013.

[83] Earnings were higher in Experiment 2 than in Experiment 1 (subject-level Wilcoxon Mann-Whitney rank-sum test z=-1.503 ,p<0.15, subgroup-level Wilcoxon Mann-Whitney rank-sum test z=-1.973 p=0.049), in large part due to the high earnings made by individuals in the computer treatment. Pairwise comparisons across treatments in experiment 2 show that subjects in the Computer Treatment earn more than subjects who participated in the other two treatments (subgroup-level Wilcoxon Mann-Whitney rank-sum tests provide z=2.022, p=0.043 and z=1.906, p=0.057 in the honest and Dishonest Information Treatments respectively).

Table 4.2: Summary statistics by experiment and treatment

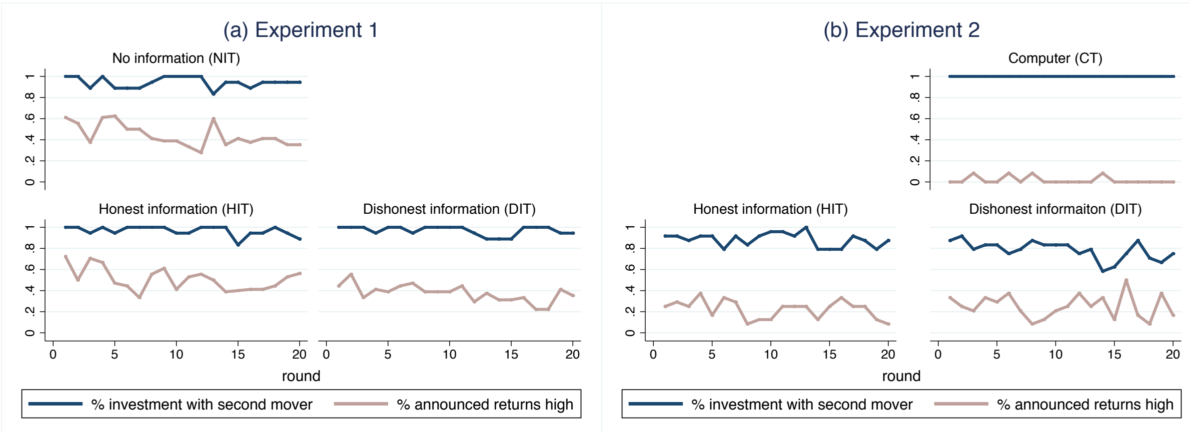
Variable	Experiment 1				Experiment 2				Total
	NIT	HIT	DIT	All	CT	HIT	DIT	All	
N sessions	3	3	3	9	1	4	4	9	18
N subjects	36	36	36	108	12	48	48	108	216
% female	0.50	0.50	0.50	0.50	0.42	0.44	0.46	0.44	0.47
% first movers	0.50	0.50	0.50	0.50	0.00	0.50	0.50	0.45	0.47
% second movers	0.50	0.50	0.50	0.50	1.00	0.50	0.50	0.56	0.53
n problems correct ph 1	10.31	10.25	11.36	10.64	11.17	11.08	12.71	11.82	11.23
(sd)	(3.91)	(4.26)	(3.42)	(3.88)	(2.62)	(4.52)	(5.27)	(4.74)	(4.36)
endowment	7.15	7.13	7.68	7.32	7.58	7.54	8.35	7.91	7.61
(sd)	(1.96)	(2.13)	(1.71)	(1.94)	(1.31)	(2.26)	(2.63)	(2.37)	(2.18)
total earnings	17.61	17.74	18.40	17.92	21.92	18.34	19.21	19.13	18.52
(sd)	(3.92)	(3.94)	(3.66)	(3.82)	(4.46)	(4.70)	(5.48)	(5.11)	(4.54)

4.4.1 Aggregate results

Figure 4.2 shows the aggregate rate of first mover investment and second mover announcements of high returns by round and treatment. Panel (a) shows the results of Experiment 1 and panel (b) shows the results of Experiment 2. Experiment 1 shows that first movers almost never chose to forego investment with the second mover; they invested with the second mover 96 percent of the time. Deviations from the subgame perfect prediction of always investing with the second mover are, nevertheless, statistically significant (subgroup-level sign tests $p<0.05$ in each treatment). First movers, thus, did not invest with the second mover 100 percent of the time in Experiment 1. No differences in the aggregate rate of investment can be detected in Experiment 1 across treatments.

Experiment 2 shows that first movers are less likely to invest with the second mover when the return to the outside option is larger (subgroup-level Wilcoxon Mann-Whitney rank sum tests $p<0.01$ across experiments within each of treatments). In fact, first movers invest with second movers only 83 percent of the time. Deviations from the subgame perfect prediction of full investment with the second mover are also statistically significant (subgroup-level sign tests $p<0.01$ within each of the treatments). Differences in the rate of investment across treatments show a lower rate of investment in the Dishonest Information Treatment than in the Honest Information Treatment but are not statistically significant.

Figure 4.2: First and second mover behavior by experiment and treatment



Note: Investment in the Computer Treatment is included as a reference. All subjects in CT were assigned the role of second movers and played against a computer that was programmed to always invest with the second mover.

Second movers in Experiment 1 announce high returns 44 percent of the time. Deviations from the subgame perfect equilibrium strategy of no high announcements are statistically significant (subgroup-level sign test $p<0.01$ in each of the treatments). Differences in the rate of high announcements across treatments suggest that announcements move in the direction of social information, but are not statistically significant when non-parametric tests are employed. The insignificant differences in behavior across treatments may be a product of the size of the subtle treatment effects, which may require more statistical power, but may also reflect the fact that strategic considerations need to be controlled for.

Experiment 2 shows that the rate of high announcements observed in Experiment 1 is not a product of confusion. Subjects continue to announce high returns (24 percent of the time) when they play against a human participant even when dishonesty is primed. They play the subgame perfect equilibrium of the game when matched against the computer. Deviations from the subgame perfect prediction of no high announcements are statistically significant in both of the Information Treatments (subgroup-level sign tests $p<0.05$ within each treatment) but not in the Computer Treatment (subgroup-level sign test $p=0.25$). Differences in the rate of high announcements are statistically significant across treatments in Experiment 2 (subgroup-level Kruskal-Wallis $X^2=19.593$, $p<0.01$) due to the zero rate of high announcements made by subjects in the Computer Treatment.[84] Second mover behavior, however, does

[84] Wilcoxon Mann-Whitney rank-sum tests of differences in behavior across treatments show that the rate of high announcements is lower in the Computer Treatment than in the Honest and Dishonest Information Treatments (z=-3.510, p<0.01 and z==-4.212, p<0.01 respectively).

not vary across Information Treatments. The lack of treatment differences generated by the nature of the social information presented to subjects in Experiment 2 may reflect the fact that priming dishonesty may have caused behavior to hit a lower bound in the aggregate rate of honesty that can be observed in the experiment, which prevents social information from moving average behavior.

Comparison of second mover behavior across experiments shows that the rate of high announcements is different across experiments within the Honest Information Treatment (subgroup-level Wilcoxon Mann-Whitney rank-sum z=2.775, p=0.006) but not the Dishonest Information Treatment. High announcements are not even statistically significantly different between the Dishonest Information Treatment in Experiment 1 and the Honest Information Treatment in Experiment 2. That is, social information reflecting dishonest behavior in Experiment 1 has the same effect on behavior as the implementation procedures employed in Experiment 2. The lack of aggregate differences across treatments in Experiment 2 may also be interpreted as evidence that confusion or learning explains the differences in behavior documented in Experiment 1 across treatments. Further analysis of second mover behavior presented in section 4.4.3 will show that this is unlikely the case.

The aggregate rate of first mover investment and second mover announcements of high returns presented so far does not take into account the strategic considerations present in the experiment or any heterogeneity in individual behavior. The next subsections address these issues by providing an in-depth analysis of behavior by mover type.

4.4.2 First movers

Table 4.3 shows investment statistics of first movers by treatment. Although subjects chose to invest with second movers 98 percent of the time in Experiment 1, only 65 percent of participants (n=35) chose to always invest with the second mover and thus played the subgame perfect equilibrium of the game. That is, 35 percent of participants (n=19) chose to forego investment with the second mover at least once. Foregone investment cannot be explained by confusion because 20 percent of participants (n=11) chose to forego investment with the second mover more than once. A much lower fraction of participants (27 percent, n=13) played the subgame perfect equilibrium game in Experiment 2. Out of the 35 participants who chose to forego investment with the second mover in Experiment 2, 28 did so more than once. Differences in the proportion of players who chose to always invest with the second mover are statistically significant across experiments within treatment (subgroup level Wilcoxon Mann-Whitney rank sum tests provide $p<0.05$) but not across treatments within each of the experiments.

Table 4.3: First mover behavior by experiment and treatment

	Experiment 1				Experiment 2			Total
	NIT	HIT	DIT	All	HIT	DIT	All	
Aggregate behavior								
% investment with the second mover	0.94	0.97	0.97	0.96	0.88	0.78	0.83	0.90
Strategies employed								
% who play the SPE	0.56	0.67	0.72	0.65	0.21	0.33	0.27	0.47
% who forego investment once	0.11	0.22	0.11	0.15	0.17	0.13	0.15	0.15
% who forego investment more than once	0.33	0.11	0.17	0.20	0.63	0.54	0.58	0.38
N subjects	18	18	18	54	24	24	48	102
N independent subgroups	9	9	9	27	12	12	24	51

Histograms of investment rates by subject are presented in Figure 4.3. Whereas at most a first mover chose to forego investment with the second mover 6 times in Experiment 1, the lowest rate of investment with the second mover in Experiment 2 was 0. That is, one participant exposed to social information reflecting dishonest behavior never chose to invest with the second mover. The behavior of this participant was extreme but not unique, 4 participants exposed to social information reflecting dishonest behavior in Experiment 2 chose to forego investment with the second mover at least 10 times (Kolmogorov Smirnov tests provide $p<0.05$ across experiments and within information treatment).

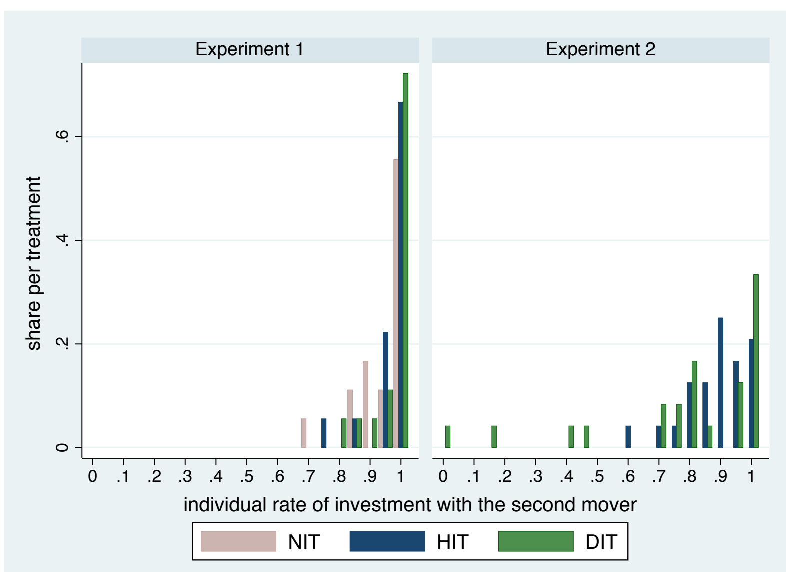


Figure 4.3: Histogram of individual rates of investment

Table 4.4: Marginal effects of probit regressions of first mover investment

Dep. Var.: Invest with SM	Experiment 1			Experiment 2		
	Phase 2	Phase 3	Phase 2 vs. 3	Phase 2	Phase 3	Phase 2 vs. 3
	(1)	(2)	(3)	(4)	(5)	(6)
HIT	0.002	-0.582	-0.460			
	(0.822)	(0.582)	(0.753)			
DIT	1.000***	-0.999***	-1.000***	-0.299	-0.620***	-0.259
	(0.000)	(0.000)	(0.000)	(0.108)	(0.003)	(0.421)
perceived honesty SM	0.006	0.195**	0.026	0.501***	0.832***	-0.026
	(0.645)	(0.032)	(0.334)	(0.009)	(0.003)	(0.883)
HIT X perceived honesty SM	-0.006	-0.007	0.027			
	(0.642)	(0.945)	(0.431)			
DIT X perceived honesty SM	0.054	-0.062	-0.106*	-0.177	-0.395	
	(0.487)	(0.511)	(0.060)	(0.392)	(0.175)	
% past investment	0.012	0.055	-0.014	0.231*	0.26	-0.069
	(0.630)	(0.328)	(0.367)	(0.061)	(0.298)	(0.779)
HIT X % past investment	-0.000	0.054	0.013			
	(0.987)	(0.573)	(0.744)			
DIT X % past investment	-0.129	0.003	0.214	0.345**	0.296	-0.187
	(0.630)	(0.960)	(0.330)	(0.044)	(0.288)	(0.477)
period	0.000	0.005***	0.000	0.018***	0.020**	-0.007
	(0.623)	(0.006)	(0.604)	-	(0.013)	(0.481)
endowment	0.000		-0.002	-0.014**	0.004	0.020**
	(0.622)		(0.312)	(0.017)	(0.690)	(0.040)
female						
perceived honesty SM ph2		-0.055			-0.718	
		(0.452)			(0.112)	
HIT X perceived honesty SM ph2		-0.186				
		(0.176)				
DIT X perceived honesty SM ph2		-0.010			-0.111	
		(0.918)			(0.878)	
% past investment		0.046			0.283	
		(0.255)			(0.135)	
HIT X % past investment ph2		0.122				
		(0.525)				
DIT X % past investment ph2		0.322			0.435*	
		(0.160)			(0.080)	
N obs.	486	486		432	432	864
N clusters	54	27		61.949	92.986	1233.176
L	-52.92	-82.60		48	24	24
Tests HIT vs. DIT:						
treatment	0.000	0.468				
perceived honesty SM	0.105	0.565				
% past investment	0.987	0.838				
perceived honesty SM ph2		0.272				
% past investment ph2		0.497				

Note: P-values reported in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table 4.4 shows the marginal effects of probit regressions of investment on treatment that include history of play controls and clustered standard errors at the subgroup level. A lot of structure is imposed on the regression to control for the strategic considerations present in the experiment. Period 1 observations are excluded due to the nature of regressors; including them does not change results. The analysis is conducted separately for each phase because a phase constitutes a different finitely repeated game. Columns 1 and 2 show that social information reflecting dishonest behavior affects investment choices in Experiment 1. There is a level effect caused by exposure to social information reflecting dishonest behavior that makes participants more likely to invest with the second mover in Phase 2 but less likely to invest with the second mover in Phase 3. Level differences are statistically significant between the two information treatments in Phase 2 ($p<0.01$).

First mover behavior does not seem to react to second mover behavior considerably. The coefficient on perceived honesty, which measures the probability that the number of high returns announced by the second mover so far in the game is true, is positive but only statistically significant in Phase 3. Treatment differences in the intensity with which first movers react to second mover behavior is captured by the interaction term between perceived honesty and treatment, which shows marginally statistically insignificant differences in Phase 2 ($p=0.105$). Column 3 tests differences in the coefficients of the regressors across phases.

Regressions results for Experiment 2, shown in columns 4 and 5, show a negative level effect of the Dishonest Information Treatment that does not vary by phase. This indicates that first movers are less likely to invest with second movers when they are exposed to social information reflecting dishonest behavior in Experiment 2 ($p=0.105$ in Phase 2, $p=0.003$ in Phase3 and in the joint regression). The coefficient on perceived honesty is positive and statistically significant, suggesting that subjects react to second mover behavior. A bifurcation of types seems to be generated by exposure to dishonest behavior in Phase 2, which is captured by the coefficient of percent past investment. First movers are more likely to invest with the second mover the more they have invested in the past, and the intensity with which they do so is higher when they are exposed to social information reflecting dishonest behavior than when they are exposed to social information reflecting honest behavior. The size of the endowment decreases the likelihood that individuals invest in Phase 2 but has no effect on investment in Phase 3 of the experiment.

Overall, Table 4.4 shows that social information reflecting dishonest behavior does affect the likelihood that individuals invest with the second mover and thus punish dishonesty. Individuals become less likely to invest with the second mover (more likely to punish dishonesty) when they are exposed to exogenous social information reflecting dishonest behavior.

4.4.3 Second movers

Table 4.5 provides detailed information about second mover behavior that differentiates announcements when return was high from announcements when return was low. The returns observed by second movers were randomly and independently drawn for each participant in each period of play in Experiment 1. The percentage of high returns observed by second movers thus varies across sessions, individuals, and rounds. Experiment 2 removed the unnecessary variation by choosing the random draws shown to participants ex-ante and keeping them constant across sessions.[85] In particular, sequences of returns were chosen that showed a high return to second movers during the first period of play of each phase.

Table 4.5: Second mover behavior by experiment and treatment

	Experiment 1				Experiment 2				Total
	NIT	HIT	DIT	All	CT	HIT	DIT	All	
Aggregate behavior									
% returns observed high	0.76	0.76	0.75	0.76	0.80	0.80	0.80	0.80	0.78
% announced high	0.44	0.51	0.38	0.44	0.02	0.22	0.25	0.18	0.33
% honest when return high	0.54	0.62	0.48	0.55	0.01	0.26	0.30	0.20	0.40
% honest when return low	0.85	0.83	0.93	0.87	0.94	0.91	0.94	0.93	0.90
Strategies employed									
% who play the SPE	0.06	0.06	0.39	0.17	0.75	0.38	0.25	0.40	0.29
% always dishonest when return high	0.17	0.06	0.39	0.20	0.92	0.38	0.29	0.45	0.33
% always honest when return low	0.50	0.39	0.72	0.54	0.83	0.79	0.79	0.80	0.68
% always honest	0.11	0.11	0.11	0.11	0.00	0.00	0.04	0.02	0.06
N returns observed	340	348	349	1,037	240	480	480	1,200	2,237
N subjects	18	18	18	54	12	24	24	60	113
N independent subgroups	9	9	9	27	12	12	12	36	63
N sessions	3	3	3	9	1	4	4	8	18

As Table 4.5 shows, second movers were not always honest when return was high or low. Participants were honest when return was high 55 percent of the time in Experiment 1 and 20 percent of the time in Experiment 2. They were honest when return was low 87 percent in Experiment 1 and 90 percent of the time in Experiment 2. The lack of complete honesty when return was low suggests that second movers used announcements when return was low strategically. That is, to possibly maintain their perceived image of honesty or even to erase the consequences of past actions. Announcing high when return was low allowed second movers to undo the effect of having announced low when return was high and even gave them the opportunity to reward first mover investment.

[85] The percentage of high returns observed by second movers is higher than 0.75 because it is determined by 6 randomly generated sequences of returns.

Only 17 percent of participants (n=9) played the subgame perfect equilibrium strategy of never announcing a high return in Experiment 1. 20 percent of participants (n=11) were always dishonest when return was high, and 54 percent (n=29) were always honest when return was low. The fraction of subjects who played the subgame perfect equilibrium strategy of never announcing a high return was significantly lower in the No Information and Honest Information Treatments than in the Dishonest Information Treatment (subgroup-level Wilcoxon Mann-Whitney rank-sum tests z=-2.015, p=0.044 in both cases). Exposure to dishonesty thus increased the likelihood that individuals never announced a low return on the investment in Experiment 1.

Experiment 2 shows a larger fraction of subgame perfect equilibrium play across experiments and within treatment (subgroup-level test Wilcoxon Mann-Whitney rank-sum z=-2.487, p=0.013 within each of the treatments). The fraction of subjects who never announce a high return varies across treatments in Experiment 2 (subgroup-level Kruskal-Wallis test X^2=9.256, p=0.01). 75 percent of subjects (n=9) played the subgame perfect equilibrium strategy in the Computer Treatment, 38 percent (n=9) did so in the Honest Information Treatment, and 25 percent (n=6) did so in the Dishonest Information Treatment. Differences are statistically significant between each of the Information Treatments and the Computer Treatment (subgroup-level Wilcoxon Mann-Whitney rank-sum test z=2.309, p=0.021 between CT and HIT and z=2.769, p=0.006 between CT and DIT) but not across Information Treatments. Deviations from subgame perfect play in the Computer Treatment are driven by a few high announcements made by subjects when return was low but are not statistically significant.

The results from Experiment 1 suggest that exposure to dishonesty makes individuals more likely to play the sub-game perfect equilibrium of never announcing a high return. Are these differences present if we look at the individual decision to announce a high return? Table 4.6 presents the marginal effects of probit regressions of high announcements on treatment that control for the return observed by the second mover, for the history of play, and cluster standard errors at the subgroup level. Period 1 observations are excluded, due to the nature of the variables included in the regression. Columns 1 through 3 show the results of Experiment 1. Columns 4 through 6 show the results of Experiment 2. Social information has a level effect on the likelihood that individuals make high announcements in Experiment 1. Exposure to social information reflecting honest behavior (HIT) makes subjects more likely to announce high in Phase 2, but less likely to announce high in Phase 3 relative to the control treatment of no information (NIT). Exposure to social information reflecting dishonest behavior (DIT), on the other hand, makes subjects less likely to announce high returns in Phases 2 and 3 the experiment. Differences in level effects are statistically significant across Information Treatments (p<0.01).

Table 4.6. Marginal effects of probit regressions of second mover announcements

Dep. Var.: High announcement	Experiment 1			Experiment 2		
	Phase 2	Phase 3	Phase 2 vs. 3	Phase 2	Phase 3	Phase 2 vs. 3
	(1)	(2)	(3)	(4)	(5)	(6)
return high	0.454***	0.444***	0.004	0.199***	0.200***	-0.015
	(0.000)	(0.000)	(0.740)	(0.000)	(0.000)	(0.832)
HIT	0.783*	-0.825**	-0.780***			
	(0.052)	(0.015)	(0.000)			
DIT	-1.000***	-1.000***	0.627***	0.292	-0.089	-0.275
	(0.000)	(0.000)	(0.000)	(0.223)	(0.825)	(0.207)
perceived honesty	1.385***	0.790**	-0.375	0.982***	0.216	-0.788***
	(0.000)	(0.010)	(0.303)	(0.000)	(0.261)	(0.000)
HIT X perceived honesty	-0.484*	-0.050	0.391			
	(0.072)	(0.895)	(0.410)			
DIT X perceived honesty	0.996***	0.925*	0.139	-0.290	0.023	0.318
	(0.006)	(0.061)	(0.788)	(0.108)	(0.922)	(0.256)
% past investment FM	0.095	0.921*	0.969	0.466**	-0.072	-0.546
	(0.913)	(0.074)	(0.128)	(0.038)	(0.793)	(0.122)
HIT X % past investment FM	-0.811	-0.311	0.395			
	(0.399)	(0.642)	(0.648)			
DIT X % past investment FM	16.598***	-1.302*	-16.895***	-0.268	0.126	0.396
	(0.000)	(0.062)	(0.000)	(0.290)	(0.707)	(0.332)
period	0.044***	0.039***	0.394***	0.015*	-0.008	-0.023**
	(0.001)	(0.000)	(0.006)	(0.056)	(0.345)	(0.015)
endowment	-0.002	0.011	0.014	0.003	-0.010	-0.013
	(0.889)	(0.561)	(0.514)	(0.812)	(0.537)	(0.320)
investment	-0.010	-0.009	-0.002	-0.004	0.002	0.007
	(0.586)	(0.662)	(0.949)	(0.627)	(0.858)	(0.699)
perceived honesty ph2		1.882***			2.793***	
		(0.001)			(0.000)	
HITXperceivedhonestyph2		0.955				
		(0.325)				
DIT X perceived honesty ph2		-0.377			-2.227***	
		(0.546)			(0.000)	
% investment FM ph2		0.041			-0.026	
		(0.925)			(0.909)	
HIT X % investment FM ph2		1.823				
		(0.126)				
DIT X % investment FM ph2		14.030***			0.119	
		(0.000)			(0.677)	
N obs.	472.000	458.000	930.000	432.000	432.000	864.000
N clusters	54.000	27.000	27.000	48.000	24.000	24.000
L	-224.850	-166.918	-391.768	-185.278	-174.316	-359.595
R squared	0.311	0.455	0.384	0.203	0.265	0.235
Tests HIT vs. DIT:						
treatment	0.000	0.000				
perceived honesty	0.000	0.023				
% investment FM	0.000	0.109				
perceived honesty ph2		0.101				
% investment FM ph2		0.000				
phase 3 X treatment						
phase 3 X perceived honesty						
phase 3 X % investment FM						

Note: P-values reported in parentheses. *p<0.10, **p<0.05, ***p<0.01.

The variable perceived honesty controls for the history of high announcements made by second movers in previous rounds of the experiment. It is equal to the probability that the number of high announcements made by the second mover and observed by the first mover so far in the game is true. The positive coefficient on perceived honesty suggests that, on average, subjects do not behave strategically in the experiment. They are more likely to announce high when they have announced high returns. This may capture the fact that there are subject types who are more likely to announce high returns. Treatments seem to alter this division of types, which is weaker in the Honest Information Treatment than in the No Information Treatment, but stronger in the Dishonest Information Treatment than in the No Information Treatment. Differences in the coefficient of perceived honesty are statistically significant across Information Treatments ($p<0.01$ in Phase 2 and $p=0.023$ in Phase 3).

Subjects do not react strategically to first mover behavior in the experiment. The coefficient on percent investment is positive in Phases 2 and 3 suggesting that second movers are more likely to announce a high return the more the first mover has invested. This could be due to reciprocity or to the existence of second mover types that are more likely to announce high returns and less likely to be punished by second movers. Exposure to social information reflecting dishonest behavior strengthens this relationship in Phase 2 and eliminates it in Phase 3. The total effect of percent past investment is negative in Phase 3 but not statistically significant.

Columns 4 through 6 of Table 4.6 exclude observations where subjects play against the computer and show that in Experiment 2 social information has no aggregate effect on behavior. Subjects continue to be more likely to announce high when they observe a high return on the investment in Phases 2 and 3, but are not affected by the nature of the social information. The coefficients on perceived honesty and percent past investment are similar in sign to those documented in Experiment 1, but are only statistically significant in Phase 2 of Experiment 2.

Does this suggest that the treatment effects captured in Experiment 1 are the result of learning to play Nash equilibrium? Not necessarily, due to three reasons. First, extreme punishers exist in the Dishonest Information Treatment in Experiment 2 so the behavior of some second movers may exhibit higher levels of honesty than if announcements were binding. Second, if priming dishonesty causes behavior to hit a lower bound in the rate of honesty that can be observed in Experiment 2, then exposure to dishonesty cannot push behavior any further. Third, the aggregate analysis presented so far masks the fact that subject types may exist who are affected by social information. The first explanation is ruled out by excluding second movers who were matched with extreme punishers from the analysis, which does not change the estimated magnitude of treatment effects in the regressions. The third explanation can be investigated by examining individual heterogeneity in announcement decisions and classifying subjects into types.

Figure 4.4 shows histograms of the individual rate of high announcements by experiment and treatment. There is a large degree of heterogeneity in the rates of high announcements made by subjects within experiments and treatments. The maximum rate of high announcements is 100 percent in Experiment 1 and 85 percent in Experiment 2. Approximately 35 percent of subjects in Experiment 1 (n=14) and 7 percent of participants in Experiment 2 (n=5) exhibit honesty and announce high at least 3/4 of the time. The minimum rate of high announcements made by subjects in all experiments and treatments is 0, the subgame perfect equilibrium of the game.

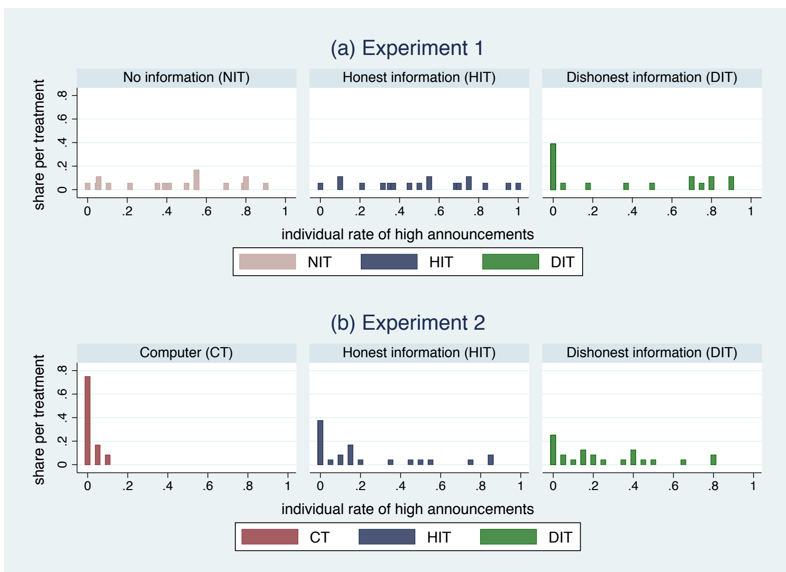


Figure 4.4. Histogram of individual rates of high announcements

The heterogeneity in second mover behavior suggests that there may be honest and dishonest types in the experiment who are not swayed by social information. Any treatment effects in Experiment 1 must thus come from individuals who are affected by social information. Who are these participants? Can they be identified? Unfortunately, experimental procedures did not collect any information prior to treatment that could be used to generate an exogenous classification of subject types. An endogenous classification is therefore employed using the choices made by participants the first time they observed a high return. I define honest types as those who announced high honestly the first time they observed a high return in Phase 2. Dishonest types are those who announced low. A total of 51 participants were classified as honest types by this standard, 37 in Experiment 1 and 14 in Experiment 2.[86] Appendix Table C.3 shows statistics by honest type category. Honest and dishonest types react to social information in Experiment 1, while only honest types react to social information in Experiment 2. Parametric analysis of the announcements made by honest types in Experiment 2, in fact, shows a robust and statistically significant negative level effect of exposure to social information reflecting dishonest behavior (see Appendix Table C.4). Honest types in Experiment 2 are less likely to announce high returns when they are exposed to social information reflecting dishonest behavior than when they are exposed to social information reflecting honest behavior. The effect of social information on honest types in Experiment 2 cannot be explained by learning or confusion given the experimental procedures employed.

4.5 CONCLUSION

This study investigates whether dishonesty is contagious in a repeated setting in the laboratory. Strategic considerations are present that may enhance or counteract the direct effect that social information has on the likelihood that individuals engage in dishonesty. Treatments expose participants to honest and dishonest behavior using exogenous social information collected in a previous session of the experiment. Results show that exposure to dishonest others increases the likelihood that individuals engage in dishonesty and weakly decreases the likelihood that individuals tolerate dishonesty.

The contributions of this paper are several. First, the study provides a strategic setting that can be used to investigate dishonesty in the laboratory. Second, the study extends the set of conditions under which dishonesty has been found to be contagious. Third, the study investigates for the first time the role that tolerance plays in halting or possibly facilitating the contagion of dishonesty. To the best of my knowledge, no study has ever examined the role of tolerance in fueling contagion.

The findings of this paper have implications that extend beyond the two-person repeated game investigated in this paper. They suggest that a non-material strategic complementarity exists that makes dishonesty contagious. Including it in dynamic models of dishonesty could help explain why dishonesty and corruption are so prevalent in some cultures and not in others and why it is so difficult to curb this

[86] The classification of types is clean in Experiment 2, where all participants observed a high return during the first period of play. In Experiment 2 some subjects saw the first high return in periods 2 and 3 of Phase 2.

type of behavior. The study of tolerance suggests that tolerance does not fuel the downward spiral behavior. More research is needed to understand when tolerance can facilitate and when it can prevent the contagion of dishonest behavior.

APPENDIX A: CHAPTER 2

Table A.1: Household level selection into the study

	Eligible (1)	Invited (2)	Participated (3)	Diff. (3) – (1)
Household head's education	4.44	4.128	4.264	-0.176
	[3.679]	[3.507]	[3.564]	(0.177)
Household assets	2.05	1.941	1.972	-0.078
	[1.647]	[1.570]	[1.551]	(0.078)
Caring for environment is top value	0.386	0.385	0.395	0.009
	[0.487]	[0.487]	[0.489]	(0.024)
Participated in all OTB meetings this year	0.321	0.366	0.366	0.045*
	[0.467]	[0.482]	[0.482]	(0.023)
Participates in OTB projects	0.582	0.636	0.659	0.077***
	[0.493]	[0.482]	[0.475]	(0.024)
Always agrees with community decisions	0.641	0.671	0.672	0.031
	[0.480]	[0.470]	[0.470]	(0.023)
Always trusts NGOs	0.394	0.404	0.407	0.013
	[0.489]	[0.491]	[0.492]	(0.024)
Held past leadership position in community	0.074	0.094	0.103	0.029**
	[0.263]	[0.292]	[0.305]	(0.014)
N	1438	673	580	

Notes: Columns 1-3 show means with standard deviations in brackets for sample of participants at each stage of the experiment. The column "Diff." shows the mean difference in between households that were eligible and those that participated, with estimated standard errors in parentheses. Significance levels are for a two-sided t-test: *p<0.10 **p<0.05 ***p<0.01. See text for discussion of covariates omitted from the table.

Table A.2: Treatment non-compliance

	Switched out of			Switched in to		
Binary dependent variable is:	AL	RL	NL	AL	RL	NL
	(1)	(2)	(3)	(4)	(5)	(6)
Individual contribution decision	0.0019	-0.002	0.0015	-0.004	0.0041	0.0008
	(0.0046)	(0.0026)	(0.0016)	(0.0034)	(0.0043)	(0.0009)
Female	0.1177*	-0.019	-0.038	-0.016	0.0259	0.0325
	(0.0641)	(0.0284)	(0.0385)	(0.0461)	(0.0378)	(0.0319)
Years of education	-0.005	-0.002	0.0071	0.0041	-0.0120*	0.0045
	(0.0088)	(0.0062)	(0.0068)	(0.0083)	(0.0068)	(0.0045)
Household assets	0.0058	-0.0350*	0.0408	-0.014	0.0169	-0.007
	(0.0363)	(0.0200)	(0.0332)	(0.0340)	(0.0310)	(0.0075)
Number of children attending local school	0.0316	-0.001	0.0046	0.0199	0.0117	-0.005
	(0.0319)	(0.0104)	(0.0051)	(0.0222)	(0.0188)	(0.0052)
Evaluated the teacher as good or excellent	0.000	0.0292	0.0061	0.0638	-0.056	0.027
	(0.0625)	(0.0618)	(0.0086)	(0.0625)	(0.0641)	(0.0271)
Caring for environment is top value	-0.119	0.0134	-0.002	-0.007	-0.069	-0.045
	(0.0878)	(0.0729)	(0.0054)	(0.0719)	(0.0809)	(0.0449)
Participated in all OTB meetings this year	0.018	-0.013	-0.04	-0.037	-0.003	0.0057
	(0.0721)	(0.0598)	(0.0401)	(0.0705)	(0.0710)	(0.0087)
Participates in OTB projects	-0.013	0.0364	-0.035	-0.009	1.272	-0.011
	(0.0529)	(0.0603)	(0.0350)	(0.0722)	(0.0519)	(0.0128)
Always agrees with community decisions	-0.088	-0.019	-0.033	-0.016	-0.09	-0.033
	(0.0574)	(0.0637)	(0.0329)	(0.0614)	(0.0573)	(0.0329)
Always trusts NGOs	0.0401	0.0398	-0.001	0.0617	-0.004	-0.001
	(0.0518)	(0.0581)	(0.0047)	(0.0685)	(0.0480)	(0.0047)
Held past leadership position in community	0.0307	-0.04	-0.029	-0.073	0.0277	0.032
	(0.0863)	(0.0593)	(0.0293)	(0.0618)	(0.0975)	(0.0338)
Experimenter indicator	-0.107	-0.058	-0.053	-0.113	-0.043	-0.053
	(0.1238)	(0.1046)	(0.0533)	(0.1139)	(0.1136)	(0.0533)
Session size	-0.161	0.1363*	-0.054	0.0659	-0.104	-0.054
	(0.1327)	(0.0745)	(0.0552)	(0.1082)	(0.1236)	(0.0552)
Community size	0.0034	0.000	0.008	0.0072	0.0011	0.0019
	(0.0062)	(0.0018)	(0.0056)	(0.0054)	(0.0046)	(0.0024)
Travel time to nearest market	0.000	0.0003	0.000	4.711	0.000	0.0003
	(0.0004)	(0.0004)	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Pupils provide their own books	0.1295	-0.004	0.045	0.0638	0.051	0.045
	(0.1174)	(0.1140)	(0.0449)	(0.1126)	(0.1179)	(0.0449)
N	165	158	188	157	165	188
Number of individuals with dep var = 1	27	15	5	19	22	5
Number of communities with dep var = 1	6	3	1	4	5	1

Notes: OLS regressions of binary treatment non-compliance on individual-level characteristics. Each cell is a separate regression. The sample in columns 1-3 are all sessions assigned to AL, RL and NL, respectively. The sample in columns 4-6 are all sessions that received AL, RL and NL, respectively. Columns 1, 2, 4 and 5 are estimated for followers only. Standard errors in parentheses are clustered at the OTB level. *p<0.10 **p<0.05 ***p<0.01.

Table A.3: Treatment balance among study participants

	NL mean (sd)	RL - NL	AL - NL	AL - RL
	(1)	(2)	(3)	(4)
Individual received information	0.527	0.001	0.007	0.007
	[0.501]	(0.011)	(0.011)	(0.011)
Female	0.330	0.004	-0.120***	-0.124***
	[0.471]	(0.038)	(0.043)	(0.036)
Years of education	4.500	0.241	0.453	0.212
	[3.352]	(0.429)	(0.399)	(0.351)
Household assets	1.676	0.359*	0.523**	0.164
	[1.302]	(0.183)	(0.198)	(0.212)
Number of children attending local school	0.601	0.086	0.027	-0.058
	[1.027]	(0.116)	(0.083)	(0.107)
Evaluated the teacher as good or excellent	0.548	0.104	0.070	-0.034
	[0.499]	(0.070)	(0.070)	(0.065)
Caring for environment is top value	0.415	-0.052	-0.007	0.045
	[0.494]	(0.059)	(0.053)	(0.053)
Participated in all OTB meetings this year	0.351	0.027	0.015	-0.012
	[0.479]	(0.061)	(0.069)	(0.056)
Participates in OTB projects	0.691	-0.080	-0.016	0.063
	[0.463]	(0.054)	(0.049)	(0.041)
Always agrees with community decisions	0.676	-0.029	0.021	0.050
	[0.469]	(0.052)	(0.050)	(0.047)
Always trusts NGOs	0.410	0.008	-0.017	-0.025
	[0.493]	(0.057)	(0.053)	(0.051)
Held past leadership position in community	0.096	0.009	0.014	0.005
	[0.295]	(0.037)	(0.032)	(0.031)
Experimenter indicator	0.505	-0.013	-0.008	0.005
	[0.501]	(0.148)	(0.150)	(0.150)
Session size	5.585	0.042	0.075	0.033
	[0.536]	(0.119)	(0.097)	(0.106)
Community size	23.931	4.457*	4.064	-0.393
	[9.908]	(2.227)	(2.539)	(1.750)
Travel time to nearest market	171.755	-5.760	7.669	13.429
	[138.141]	(24.430)	(20.011)	(23.331)
Pupils provide their own books	0.590	0.101	0.022	-0.079
	[0.493]	(0.084)	(0.083)	(0.084)

Notes: N = 580. Column 1 shows means with standard deviations in brackets. Columns 2 to 4 show coefficients from linear regressions of each covariate on a binary treatment variable with standard errors clustered at the OTB level in parentheses. *p<0.05 **p<0.01 ***p<0.001.

Table A.4: Participant characteristics by leadership role

	Non-leaders	RL - Non-leaders	AL - Non-leaders	AL - RL
	(1)	(2)	(3)	(4)
Female	0.298	0.0264	-0.060***	-0.408***
	[0.458]	(0.0243)	(0.0154)	(0.1239)
Years of education	4.547	0.0035	0.0122***	0.0253*
	[3.339]	(0.0034)	(0.0035)	(0.0139)
Household assets	1.896	0.0036	0.0284***	0.0752***
	[1.490]	(0.0065)	(0.0071)	(0.0270)
Number of children attending local school	0.639	-0.006	0.0061	0.0591
	[1.084]	(0.0069)	(0.0099)	(0.0444)
Evaluated the teacher as good or excellent	0.598	0.0248	0.0119	-0.053
	[0.491]	(0.0193)	(0.0208)	(0.1267)
Caring for environment is top value	0.382	0.0161	0.0361	0.0849
	[0.486]	(0.0219)	(0.0231)	(0.1281)
Participated in all OTB meetings this year	0.351	0.0176	0.0452**	0.113
	[0.478]	(0.0194)	(0.0192)	(0.1036)
Participates in OTB projects	0.661	-0.0430*	0.0352*	0.3200***
	[0.474]	(0.0227)	(0.0194)	(0.1118)
Always agrees with community decisions	0.676	-0.0630**	0.0481***	0.4444***
	[0.468]	(0.0244)	(0.0177)	(0.1025)
Always trusts NGOs	0.396	0.0052	0.0395*	0.1421
	[0.490]	(0.0215)	(0.0206)	(0.1155)
Held past leadership position in community	0.108	-0.034	-0.012	0.123
	[0.310]	(0.0283)	(0.0326)	(0.1817)
N	510	546	544	70

Notes: Column 1 shows means with standard deviations in brackets for all subjects in NL and followers in RL and AL. Columns 2 to 4 show coefficients from univariate regressions of binary leadership role on each explanatory variable (each cell is a separate regression) with standard errors clustered at the OTB level shown in parentheses. *p<0.05 **p<0.01 ***p<0.001.

Table A.5: Robustness checks – Total contributions

	Total		Individual				
	Continuous		Continuous		Ordered Logit	≥ 10 Bs.	
	IV	Limited Sample	IV	Limited Sample	Limited Sample	IV	Limited Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RL	2.476	-4.231	0.085	-0.850	-0.134	0.126**	0.066
	(4.424)	(5.503)	(0.724)	(0.816)	(0.308)	(0.054)	(0.067)
AL	17.663***	10.470*	1.797**	0.949	0.426	0.219***	0.167**
	(5.720)	(5.265)	(0.864)	(0.841)	(0.318)	(0.073)	(0.074)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	104	84	580	469	469	580	469
Test: RL=AL (p-value)	0.000	0.002	0.011	0.010	0.008	0.139	0.119
Dep. Variable mean, excluded category	42.947	43.968	7.767	7.965	7.784	0.378	0.377

Notes: Columns 1 and 2 show OLS estimates of treatment effects on total session contributions. Columns 3-4 and 6-7 show OLS estimate of treatment effects on individual contributions. Column 5 shows log odds rations from an ordered logit regression (see text for details). IV indicates that 2SLS instrumental variable analysis was used. Limited sample indicates that the sample of observations is restricted to communities that complied with treatment assignment. All specifications include community fixed effects and standard errors clustered at the community level. Controls refer to the full set of individual- and session-level controls shown in the balance table. *p<0.10, **p<0.05, ***p<0.01.

Table A.6: Robustness checks – Leader contributions

	Continuous		Ordered Logit	≥ 10 Bs.	
	IV	Limited Sample	Limited Sample	IV	Limited Sample
	(1)	(2)	(3)	(4)	(5)
Private giving by an authority	-2.990	-0.298	0.262	-0.159	-0.027
	(3.015)	(1.910)	(0.786)	(0.226)	(0.162)
Public giving by a random leader (RL)	1.921	0.806	0.864*	0.331***	0.293**
	(1.373)	(1.686)	(0.498)	(0.094)	(0.114)
Public giving by an authority leader (AL)	9.491**	6.093**	1.689*	0.551**	0.380*
	(4.060)	(2.411)	(0.897)	(0.273)	(0.191)
Controls	Yes	Yes	Yes	Yes	Yes
Dep. Variable Mean, No Leader treatment	7.751	7.955	7.855	0.372	0.369
			Tests (p-values)		
Public giving RL = AL	0.053	0.063	0.356	0.401	0.670
Authority private = public	0.070	0.113	0.343	0.144	0.221

Note: The sample consists of individuals who led by example in RL and AL treatments and all subjects in the NL treatment. Authority refers OTB presidents in NL. Columns 1-2 and 4-5 present OLS estimates. Column 3 shows log odds ratios from an ordered logit regression (see text for details). IV indicates that 2SLS instrumental variable analysis was used. Limited sample indicates that the sample of observations is restricted to communities that complied with treatment assignment. All regressions include community fixed effects and standard errors clustered at the OTB level. Controls refer to the full set of individual- and session-level controls shown in the balance table. *p<0.10, **p<0.05, ***p<0.01.

Table A.7: Robustness checks – Follower contributions

	Continuous				Ordered Logit	≥ 10 Bs.	
	IV	Limited Sample	IV	Limited Sample	Limited Sample	IV	Limited Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Random leader (RL)	-0.364	-0.026	-1.209	-1.200	-0.601	-0.144	-0.089
	(2.357)	(3.056)	(1.248)	(1.109)	(0.585)	(0.152)	(0.135)
Authority leader (AL)	0.352	0.082	0.666	-1.203	-1.064**	0.009	-0.162*
	(1.134)	(1.456)	(2.325)	(1.161)	(0.486)	(0.211)	(0.082)
RL x leader contribution	-0.034	-0.120					
	(0.229)	(0.310)					
AL x leader contribution	-0.022	-0.006					
	(0.082)	(0.090)					
RL x leader contribution ≥ 10 Bs.			0.695	0.053	0.423	0.259*	0.184
			(1.446)	(1.197)	(0.582)	(0.152)	(0.134)
AL x leader contribution ≥ 10 Bs.			-0.110	1.323	1.285**	0.161	0.332***
			(2.710)	(1.613)	(0.631)	(0.241)	(0.121)
RL total effect: Leader contribution ≥ 10 Bs.			-0.515	-1.148	-0.179	0.115*	0.095
			(0.823)	(0.952)	(0.330)	(0.064)	(0.075)
AL total effect: Leader contribution ≥ 10 Bs.			0.555	0.120	0.220	0.170**	0.170**
			(0.982)	(1.047)	(0.377)	(0.077)	(0.082)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	510	410	510	410	410	510	410
Dep. Variable mean, NL treatment	7.767	7.965	7.767	7.965	7.887	0.378	0.377
				Tests (p-values)			
RL=AL	0.791	0.975	0.553	0.999	0.557	0.632	0.669
RL x leader contrib. = AL x leader contrib.	0.965	0.753	0.830	0.580	0.358	0.778	0.462
RL total effect = AL total effect			0.357	0.225	0.213	0.516	0.308

Notes: The sample consists of followers in the RL and AL treatments and all contributors in the NL treatment. Columns 1-4 and 6-7 show OLS estimates. Column 5 shows log odds ratios from an ordered logit regression with fixed effects (see text for details). IV indicates that 2SLS instrumental variable analysis was used. Limited sample indicates that the sample of observations is restricted to communities that complied with treatment assignment. All specifications include community fixed effects and standard errors clustered at the OTB level. Controls refer to the full set of individual- and session-level controls shown in the balance table. *p<0.10, **p<0.05, ***p<0.01.

APPENDIX B: CHAPTER 3

Instructions [Piece-wise linear VCM]

This is an experiment on decision making. The earnings you receive today will depend on the decisions made by you and by other participants in this room. Please do not talk or communicate with others in any way. If you have a question please raise your hand and an experimenter will come to where you are sitting to answer you in private.

Earnings

There will be two parts of the experiment. Only one of the two parts will count for payment. Once part 1 and 2 are completed we will flip a coin to determine which part counts for payment. Your earnings in the experiment will be the sum of a $6 payment for showing up on time and your earnings from either part 1 or part 2. We will first explain how earnings are determined in part 1. Once part 1 is completed we will explain how earnings in part 2 are determined. Decisions in part 1 only affect possible earnings in part 1, and decisions in part 2 only affect possible earnings in part 2. Your total earnings will be paid to you in cash and in private at the end of the experiment.

Part 1

In part 1 you will be matched in groups of four. That is the computer will randomly match you with three other participants.

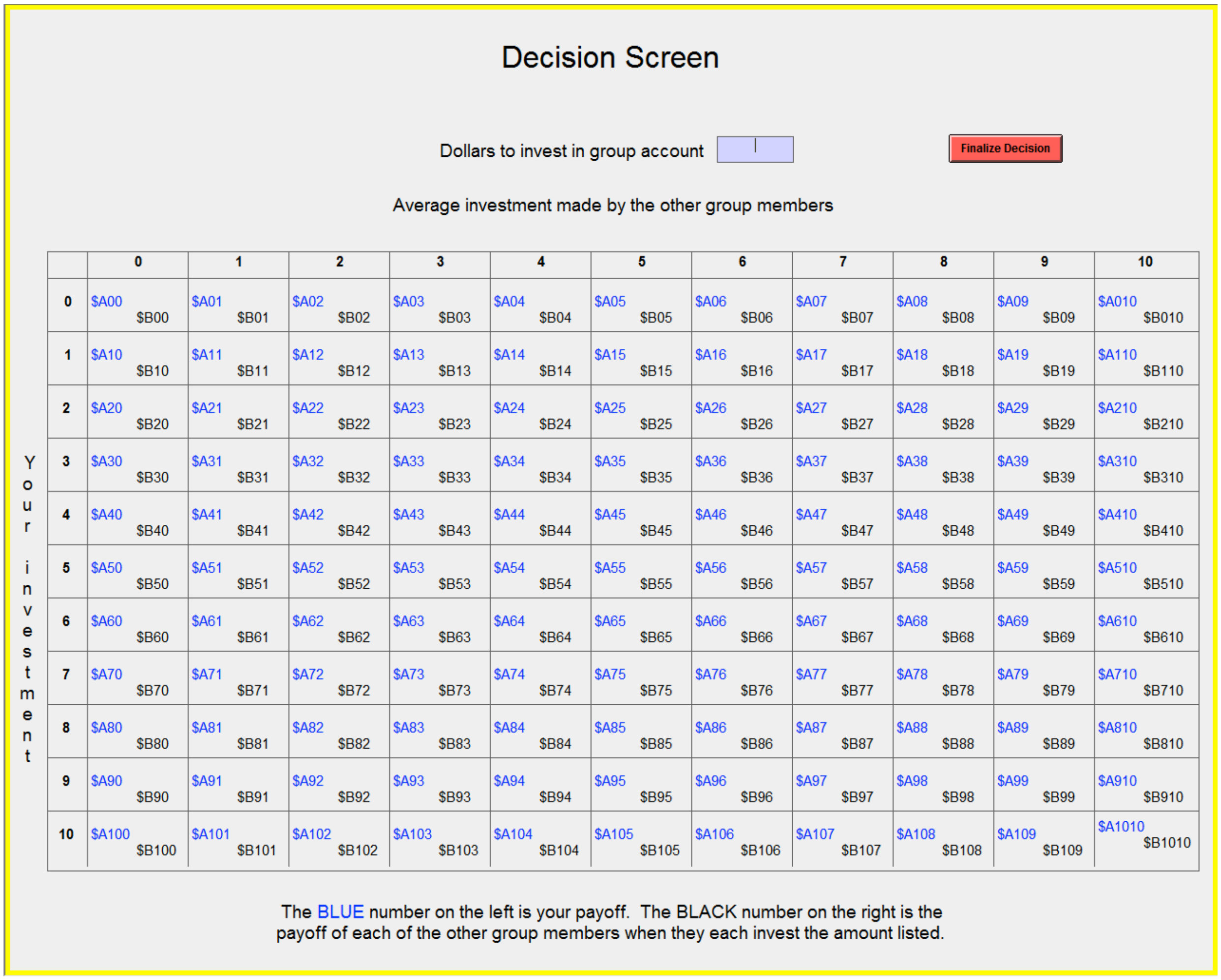
You will each have to make one decision, and earnings will depend on the decision made by you and the decisions made by other members of your group. Neither during nor after the experiment will you get to know who the other members of your group are or what decisions they make. Likewise, no one in your group will know who you are and what decision you make.

You and each of the other group members will be given $10 and asked to make an investment decision. You may select to invest any dollar amount between $0 and $10 in a group account. Investments in the group account affect both your earnings and those of the other members of the group. That is, individual earnings depend on the individual investment in the group account and the investment by the other group members.

Decision Screen

Your investment decision will be made using a decision screen. You make a decision by entering the number of dollars you wish to invest in the group account in the area labeled: *Dollars to invest in group account*. Once you have made your investment decision, please click the red *Finalize Decision* button. You will not be able to modify your decision once your choice is finalized.

A decision screen is shown below. The actual decision screen will include a payoff table with the earnings that result from the investments made by you and the three other group members. We will use the screenshot below to demonstrate how to read the table. The first column shows all possible investments by you. The first row shows all possible average investments by the other group members. If the average investment by the other group members is say $2, then it may result from each investing $2, or from one member investing $0, another investing $2, and a third investing $4.



Decision Screen

Dollars to invest in group account

Finalize Decision

Average investment made by the other group members

Your investment	0	1	2	3	4	5	6	7	8	9	10
0	$A00 $B00	$A01 $B01	$A02 $B02	$A03 $B03	$A04 $B04	$A05 $B05	$A06 $B06	$A07 $B07	$A08 $B08	$A09 $B09	$A010 $B010
1	$A10 $B10	$A11 $B11	$A12 $B12	$A13 $B13	$A14 $B14	$A15 $B15	$A16 $B16	$A17 $B17	$A18 $B18	$A19 $B19	$A110 $B110
2	$A20 $B20	$A21 $B21	$A22 $B22	$A23 $B23	$A24 $B24	$A25 $B25	$A26 $B26	$A27 $B27	$A28 $B28	$A29 $B29	$A210 $B210
3	$A30 $B30	$A31 $B31	$A32 $B32	$A33 $B33	$A34 $B34	$A35 $B35	$A36 $B36	$A37 $B37	$A38 $B38	$A39 $B39	$A310 $B310
4	$A40 $B40	$A41 $B41	$A42 $B42	$A43 $B43	$A44 $B44	$A45 $B45	$A46 $B46	$A47 $B47	$A48 $B48	$A49 $B49	$A410 $B410
5	$A50 $B50	$A51 $B51	$A52 $B52	$A53 $B53	$A54 $B54	$A55 $B55	$A56 $B56	$A57 $B57	$A58 $B58	$A59 $B59	$A510 $B510
6	$A60 $B60	$A61 $B61	$A62 $B62	$A63 $B63	$A64 $B64	$A65 $B65	$A66 $B66	$A67 $B67	$A68 $B68	$A69 $B69	$A610 $B610
7	$A70 $B70	$A71 $B71	$A72 $B72	$A73 $B73	$A74 $B74	$A75 $B75	$A76 $B76	$A77 $B77	$A78 $B78	$A79 $B79	$A710 $B710
8	$A80 $B80	$A81 $B81	$A82 $B82	$A83 $B83	$A84 $B84	$A85 $B85	$A86 $B86	$A87 $B87	$A88 $B88	$A89 $B89	$A810 $B810
9	$A90 $B90	$A91 $B91	$A92 $B92	$A93 $B93	$A94 $B94	$A95 $B95	$A96 $B96	$A97 $B97	$A98 $B98	$A99 $B99	$A910 $B910
10	$A100 $B100	$A101 $B101	$A102 $B102	$A103 $B103	$A104 $B104	$A105 $B105	$A106 $B106	$A107 $B107	$A108 $B108	$A109 $B109	$A1010 $B1010

The BLUE number on the left is your payoff. The BLACK number on the right is the payoff of each of the other group members when they each invest the amount listed.

Each cell reports the payoff you and the other group members receive given your investment and the average investment by the other group members. Your payoff will be depicted in blue and located in the upper left corner of each cell. The average payoff of the other group members will be depicted in black and located in the bottom right corner of each cell. To determine the payoffs from a specific combination of investments you look at the cell where the row of your investment crosses the column of the average investment by the other group members. In this cell you will see your payoff on the left (in blue) and the average payoff of the other group members on the right (in black). The average payoff for the other group members refers to the payoff they each get when they invest the same amount in the group account.

Consider an example where you invest $1 and the average investment by the other group members is $4. Your earnings from this investment decision will be $A14, where the first number refers to your $1 investment and the second to the $4 average investment by the other group members. Similarly the earnings of each of the three other group members will be $B14. If you were to increase your investment to $2 you move down one row to see that your earnings would become $A24 and the average earnings of the other group members would become $B24. Likewise if the average investment of the other group members increased by $1, such that you invest $2 and the other group members on average invest $5, you move over one column to see that your earnings would become $A25 and the average payoff to the other group members would be $B25. Before we begin we will give you a tutorial on how to read the payoff table.

Results Screen

After everyone has made an investment decision you will see a results screen. The results screen will indicate the investments made by you and the other group members and will summarize the earnings you and the other group members receive if part 1 counts for payment. The average earnings for the other group members reported in the payoff table refer to the earnings that result when the three other group members make the same investment decision. In the event that they do not invest the same amount their actual average earnings may differ slightly from that reported in the table. Your own payoff from the listed investment combination will be precisely that listed in the payoff table.

Instructions Part 2

Part 2 is very similar to part 1. The only difference is that you now must make investment decisions over a sequence of ten rounds. At the beginning of each round you will be randomly matched with three other people to form a new group of four. You will never be matched with the same three people twice in a row. It is also unlikely that you will meet the same set of three other group members twice. You will not get to know who the other members of your group are nor will you be informed of their past investment. Likewise, no one will know who you are and what investments you made in the past.

Just as for part 1 you will be presented with a decision screen which reports the earnings that you and the other group members get from the different investments. The decision screen will be the same in each round. That is, the earnings are the same for each of the ten rounds and are identical to those seen in part 1.

After each round is complete you will be shown a result screen which reports the investments made by you and the other group members in that round, as well as the earnings you and the other group members made in that round.

If part 2 is selected for payment we will randomly select a number between one and ten. The earnings for the corresponding round will be paid to the participants along with the $6 show up fee. The part that counts for payment will be determined by the flip of a coin. The round that counts in part 2 will be determined by having a participant draw a number between 1 and 10.

Table B.1: Payoff table Low treatment

Average investment made by the other group members

Your Investment	0	1	2	3	4	5	6	7	8	9	10
0	10.00 10.00	10.75 11.95	11.50 13.90	12.25 15.85	13.00 16.10	13.75 16.35	14.50 16.60	15.25 16.85	16.00 16.85	16.75 16.85	17.50 14.10
1	11.45 10.25	12.20 12.20	12.95 14.15	13.70 16.10	14.45 16.35	15.20 16.60	15.95 16.85	16.70 17.10	17.45 17.10	18.20 17.10	18.95 14.35
2	12.90 10.50	13.65 12.45	14.40 14.40	15.15 16.35	15.90 16.60	16.65 16.85	17.40 17.10	18.15 17.35	18.90 17.35	19.65 17.35	20.40 14.60
3	14.35 10.75	15.10 12.70	15.85 14.65	16.60 16.60	17.35 16.85	18.10 17.10	18.85 17.35	19.60 17.60	20.35 17.60	21.10 17.60	21.85 14.85
4	14.10 11.00	14.85 12.95	15.60 14.90	16.35 16.85	17.10 17.10	17.85 17.35	18.60 17.60	19.35 17.85	20.10 17.85	20.85 17.85	21.60 15.10
5	13.85 11.25	14.60 13.20	15.35 15.15	16.10 17.10	16.85 17.35	17.60 17.60	18.35 17.85	19.10 18.10	19.85 18.10	20.60 18.10	21.35 15.35
6	13.60 11.50	14.35 13.45	15.10 15.40	15.85 17.35	16.60 17.60	17.35 17.85	18.10 18.10	18.85 18.35	19.60 18.35	20.35 18.35	21.10 15.60
7	13.35 11.75	14.10 13.70	14.85 15.65	15.60 17.60	16.35 17.85	17.10 18.10	17.85 18.35	18.60 18.60	19.35 18.60	20.10 18.60	20.85 15.85
8	12.85 12.00	13.60 13.95	14.35 15.90	15.10 17.85	15.85 18.10	16.60 18.35	17.35 18.60	18.10 18.85	18.85 18.85	19.60 18.85	20.35 16.10
9	12.35 12.25	13.10 14.20	13.85 16.15	14.60 18.10	15.35 18.35	16.10 18.60	16.85 18.85	17.60 19.10	18.35 19.10	19.10 19.10	19.85 16.35
10	9.10 12.50	9.85 14.45	10.60 16.40	11.35 18.35	12.10 18.60	12.85 18.85	13.60 19.10	14.35 19.35	15.10 19.35	15.85 19.35	16.60 16.60

The BLUE number on the left is your payoff. The BLACK number of the right is the payoff of each of the other group members when they each invest the amount listed.

Table B.2: Payoff table High treatment

Average investment made by the other group members

Your Investment	0	1	2	3	4	5	6	7	8	9	10
0	10.00 / 10.00	10.75 / 10.62	11.50 / 11.23	12.25 / 11.85	13.00 / 12.60	13.75 / 13.35	14.50 / 14.10	15.25 / 14.85	16.00 / 14.85	16.75 / 14.85	17.50 / 14.10
1	10.12 / 10.25	10.87 / 10.87	11.62 / 11.48	12.37 / 12.10	13.12 / 12.85	13.87 / 13.60	14.62 / 14.35	15.37 / 15.10	16.12 / 15.10	16.87 / 15.10	17.62 / 14.35
2	10.23 / 10.50	10.98 / 11.12	11.73 / 11.73	12.48 / 12.35	13.23 / 13.10	13.98 / 13.85	14.73 / 14.60	15.48 / 15.35	16.23 / 15.35	16.98 / 15.35	17.73 / 14.60
3	10.35 / 10.75	11.10 / 11.37	11.85 / 11.98	12.60 / 12.60	13.35 / 13.35	14.10 / 14.10	14.85 / 14.85	15.60 / 15.60	16.35 / 15.60	17.10 / 15.60	17.85 / 14.85
4	10.60 / 11.00	11.35 / 11.62	12.10 / 12.23	12.85 / 12.85	13.60 / 13.60	14.35 / 14.35	15.10 / 15.10	15.85 / 15.85	16.60 / 15.85	17.35 / 15.85	18.10 / 15.10
5	10.85 / 11.25	11.60 / 11.87	12.35 / 12.48	13.10 / 13.10	13.85 / 13.85	14.60 / 14.60	15.35 / 15.35	16.10 / 16.10	16.85 / 16.10	17.60 / 16.10	18.35 / 15.35
6	11.10 / 11.50	11.85 / 12.12	12.60 / 12.73	13.35 / 13.35	14.10 / 14.10	14.85 / 14.85	15.60 / 15.60	16.35 / 16.35	17.10 / 16.35	17.85 / 16.35	18.60 / 15.60
7	11.35 / 11.75	12.10 / 12.37	12.85 / 12.98	13.60 / 13.60	14.35 / 14.35	15.10 / 15.10	15.85 / 15.85	16.60 / 16.60	17.35 / 16.60	18.10 / 16.60	18.85 / 15.85
8	10.85 / 12.00	11.60 / 12.62	12.35 / 13.23	13.10 / 13.85	13.85 / 14.60	14.60 / 15.35	15.35 / 16.10	16.10 / 16.85	16.85 / 16.85	17.60 / 16.85	18.35 / 16.10
9	10.35 / 12.25	11.10 / 12.87	11.85 / 13.48	12.60 / 14.10	13.35 / 14.85	14.10 / 15.60	14.85 / 16.35	15.60 / 17.10	16.35 / 17.10	17.10 / 17.10	17.85 / 16.35
10	9.10 / 12.50	9.85 / 13.12	10.60 / 13.73	11.35 / 14.35	12.10 / 15.10	12.85 / 15.85	13.60 / 16.60	14.35 / 17.35	15.10 / 17.35	15.85 / 17.35	16.60 / 16.60

The BLUE number on the left is your payoff. The BLACK number of the right is the payoff of each of the other group members when they each invest the amount listed.

Table B.3: Tobit regression of contribution, Part 1

Dep. Var.: Contribution to group account	Treatments		
	Low	High	All
	(1)	(2)	(3)
Response time	-0.021**	0.015**	-0.021***
	(0.013)	(0.037)	(0.009)
High			-0.199
			(0.759)
High x response time			0.035***
			(0.001)
Constant	6.190***	5.970***	6.180***
	(0.000)	(0.000)	(0.000)
Total effect response time: High			0.015**
			(0.046)
N	80	80	160

Note: P-values in parentheses. *p<0.10, **p<0.05, ***p<0.01

Table B.4: OLS regression of response time and contribution, Part 1

OLS, Dependent Variable:	response time (seconds)			contribution to group account		
	Low	High	All	Low	High	All
	(1)	(2)	(3)	(4)	(5)	(6)
response time				-0.018**	0.011*	-0.018**
				(0.031)	(0.098)	(0.018)
% tutorial correct	22.121	30.526	22.121	-0.756	3.504***	-0.756
	(0.328)	(0.165)	(0.331)	(0.639)	(0.008)	(0.610)
experiments	-0.824	-0.948	-0.824	-0.021	0.003	-0.021
	(0.125)	(0.148)	(0.126)	(0.587)	(0.935)	(0.554)
econ courses	1.175	5.763***	1.175	-0.105	0.038	-0.105
	(0.396)	(0.004)	(0.398)	(0.288)	(0.750)	(0.247)
age	2.353	2.094	2.353	0.091	0.256	0.091
	(0.448)	(0.524)	(0.451)	(0.682)	(0.185)	(0.655)
female	-13.854*	5.358	-13.854*	0.034	0.423	0.034
	(0.075)	(0.501)	(0.075)	(0.952)	(0.363)	(0.948)
High			-25.062			-7.571
			(0.779)			(0.193)
High x response time						0.029***
						(0.006)
High x % tutorial correct			8.404			4.260**
			(0.789)			(0.039)
High x experiments			-0.123			0.024
			(0.883)			(0.663)
High x econ courses			4.588*			0.143
			(0.055)			(0.372)
High x age			-0.259			0.165
			(0.954)			(0.574)
High x female			19.212*			0.389
			(0.083)			(0.591)
Constant	-2.218	-27.280	-2.218	5.234	-2.337	5.234
	(0.972)	(0.667)	(0.972)	(0.248)	(0.527)	(0.207)
Total effect response time: High						0.011
						(0.131)
Total effect % tutorial correct: High			30.526			3.504**
			(0.159)			(0.015)
Total effect experiments: High			-0.948			0.003
			(0.142)			(0.941)
Total effect econ courses: High			5.763***			0.038
			(0.003)			(0.772)
Total effect age: High			2.094			0.256
			(0.520)			(0.228)
Total effect female: High			5.358			0.423
			(0.496)			(0.408)
N	80	80	160	80	80	160

Note: P-values reported in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table B.5: OLS regression of response time and contribution, Part 2

Dependent Variable:	response time (seconds)			contribution to group account		
	Low	High	All	Low	High	All
response time				0.013	0.016	0.011
				(0.145)	(0.242)	(0.141)
period	-1.564***	-1.249**	-1.573***	-0.037	0.016	-0.052**
	(0.004)	(0.021)	(0.000)	(0.112)	(0.261)	(0.017)
% past equilibrium play	1.422	3.377*	1.835**	-1.494***	0.873	-0.889*
	(0.213)	(0.075)	(0.020)	(0.004)	(0.127)	(0.055)
mean contribution others	-0.130	0.353	-0.135	0.019	0.240***	0.012
	(0.843)	(0.215)	(0.816)	(0.810)	(0.004)	(0.864)
% tutorial correct	-1.546	1.838	-1.547	-0.586*	0.283	-0.590***
	(0.703)	(0.511)	(0.661)	(0.055)	(0.534)	(0.005)
experiments	-0.268**	-0.136**	-0.274***	-0.008*	0.000	-0.017**
	(0.031)	(0.045)	(0.005)	(0.068)	(0.986)	(0.035)
econ courses	0.087	0.542	0.072	-0.009	-0.063	-0.031
	(0.641)	(0.367)	(0.668)	(0.690)	(0.352)	(0.235)
age	0.419	0.065	0.423	-0.004	0.158	0.003
	(0.244)	(0.824)	(0.157)	(0.935)	(0.495)	(0.952)
female	-1.901	0.752	-1.865	-0.225	-0.202	-0.176
	(0.278)	(0.629)	(0.207)	(0.329)	(0.329)	(0.349)
High			-6.528			-4.489
			(0.599)			(0.285)
High x response time						0.007
						(0.624)
High x period			0.322			0.072***
			(0.338)			(0.008)
High x % past equilibrium play			1.142			1.165**
			(0.330)			(0.014)
High x mean contribution others			0.484			0.231**
			(0.439)			(0.011)
High x % tutorial correct			3.515			1.035**
			(0.414)			(0.034)
High x experiments			0.142			0.023
			(0.114)			(0.202)
High x econ courses			0.502			0.010
			(0.332)			(0.853)
High x age			-0.342			0.179
			(0.370)			(0.384)
High x female			2.630			-0.015
			(0.203)			(0.949)
Constant	15.762	9.532	15.639	5.277***	1.247	5.119***
	(0.178)	(0.394)	(0.100)	(0.008)	(0.792)	(0.000)
Total effect response time: High						0.018
						(0.134)
Total effect period: High			-1.251***			0.020
			(0.002)			(0.131)
Total effect % past equil. play: High			2.284			2.330**
			(0.330)			(0.014)
Total effect mean contrib. others: High			0.349			0.243***
			(0.115)			(0.000)
Total effect % tutorial correct: High			1.969			0.445
			(0.410)			(0.259)
Total effect experiments: High			-0.132**			0.006
			(0.011)			(0.682)
Total effect econ courses: High			0.574			-0.021
			(0.247)			(0.701)
Total effect age: High			0.082			0.182
			(0.744)			(0.366)
Total effect female: High			0.765			-0.191
			(0.576)			(0.248)
N	800	800	1600	800	800	1600

Note: P-values reported in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Instructions [Linear VCM]

This is an experiment on decision making. The earnings you receive today will depend on the decisions made by you and by other participants in this room. Please do not talk or communicate with others in any way. If you have a question please raise your hand and an experimenter will come to where you are sitting to answer you in private.

Earnings

There will be two parts of the experiment. Only one of the two parts will count for payment. Once part 1 and 2 are completed we will flip a coin to determine which part counts for payment. Your earnings in the experiment will be the sum of a $6 payment for showing up on time and your earnings from either part 1 or part 2. We will first explain how earnings are determined in part 1. Once part 1 is completed we will explain how earnings in part 2 are determined. Decisions in part 1 only affect possible earnings in part 1, and decisions in part 2 only affect possible earnings in part 2. Your total earnings will be paid to you in cash and in private at the end of the experiment.

Part 1

In part 1 you will be matched in groups of four. That is, the computer will randomly match you with three other participants.

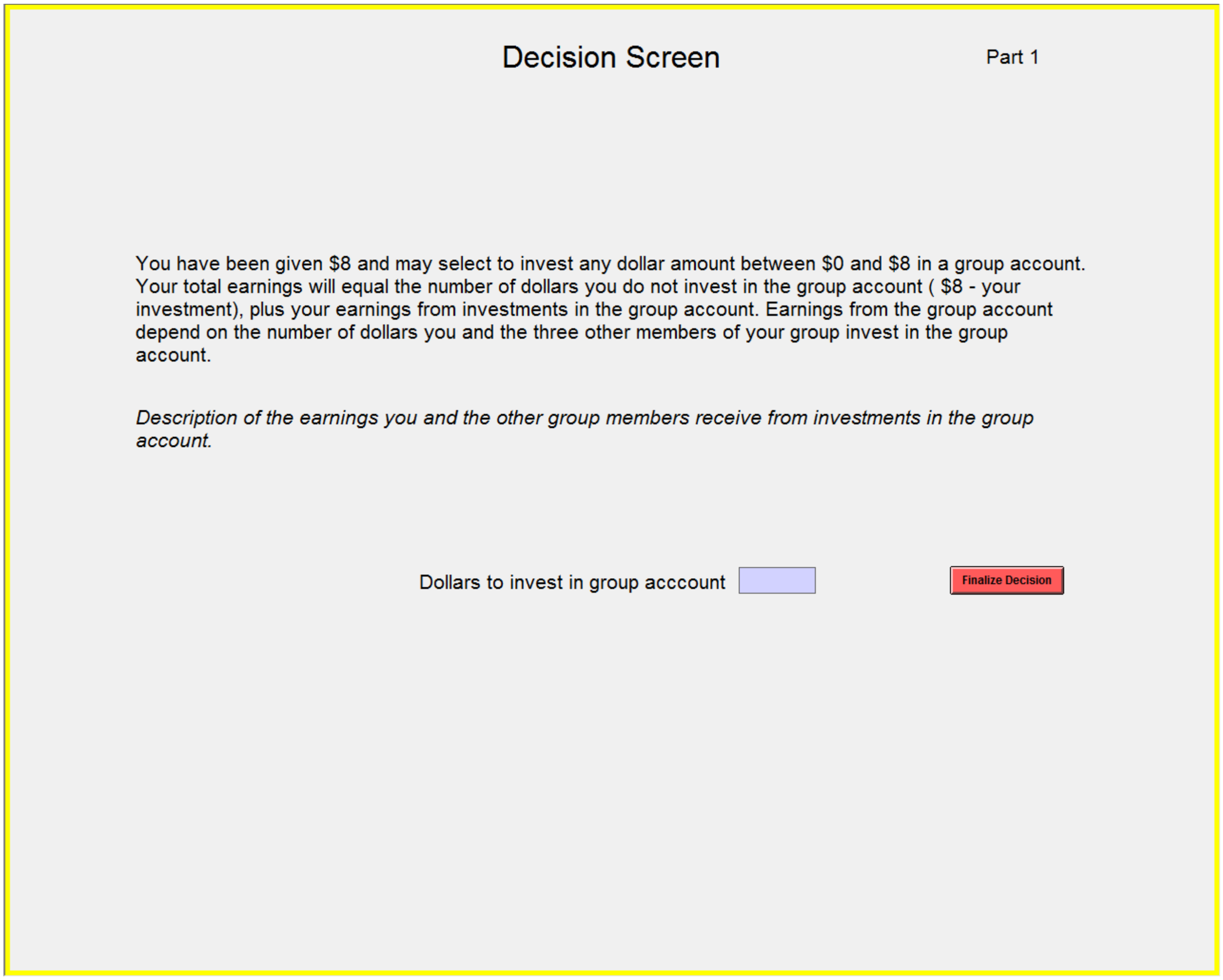
You will each have to make one decision, and earnings will depend on the decision made by you and the decisions made by other members of your group. Neither during nor after the experiment will you get to know who the other members of your group are or what decisions they make. Likewise, no one in your group will know who you are and what decision you make.

You and each of the other group members will be given $8 and asked to make an investment decision. You may select to invest any dollar amount between $0 and $8 in a group account. Investments in the group account affect both your earnings and those of the other members of the group. That is, individual earnings depend on the individual investment in the group account and the investment by the other group members.

Decision Screen

Your investment decision will be made using a decision screen. You make a decision by entering the number of dollars you wish to invest in the group account in the area labeled: *Dollars to invest in group account*. Once you have made your investment decision, please click the red *Finalize Decision* button. You will not be able to modify your decision once your choice is finalized.

A decision screen is shown below. The actual decision screen will include a description of the earnings you and the other group members receive from investing in the group account. Your total earnings will equal the number of dollars you do not invest in the group account ($8 – your investment) plus your earnings from investments in the group account. Earnings from the group account depend on the number of dollars you and the three other members of your group invest in the group account.



Results Screen

After everyone has made an investment decision you will see a results screen. The results screen will indicate the investments made by you and the other group members and will summarize the earnings you and the other group members receive if part 1 counts for payment.

Instructions Part 2

Part 2 is very similar to part 1. The only difference is that you now must make investment decisions over a sequence of ten rounds. At the beginning of each round you will be randomly matched with three other people to form a new group of four. You will never be matched with the same three people twice in a row. It is also unlikely that you will meet the same set of three other group members twice. You will not get to know who the other members of your group are nor will you be informed of their past investment. Likewise, no one will know who you are and what investments you made in the past.

Just as for part 1 you will be presented with a decision screen, which reports the earnings that you and the other group members get from investing in the group account. The decision screen will be the same in each round. That is, the earnings are the same for each of the ten rounds and are identical to those seen in part 1.

After each round is complete you will be shown a result screen which reports the investments made by you and the other group members in that round, as well as the earnings you and the other group members made in that round.

If part 2 is selected for payment we will randomly select a number between one and ten. The earnings for the corresponding round will be paid to the participants along with the $6 show up fee. The part that counts for payment will be determined by the flip of a coin. The round that counts in part 2 will be determined by having a participant draw a number between 1 and 10.

Description of payoffs - linear VCM treatments

Low-VCM treatment:

“Every dollar invested in the group account by you or any other member of your group will secure the group a payoff of $2 which is divided equally between you and the three other group members. Thus, for every dollar any group member invests in the group account you and each of the other group members will receive 50 cents.”

High-VCM 1 treatment:

Same as Low-VCM + “In addition, you will get a bonus of 60 cents for every dollar you personally invest in the group account.”

High-VCM 2 treatment:

Same as High-VCM 1 + “That is, you will get a total of $1.10 for every dollar you invest in the group account.”

Table B.6: Payoff table New-Low treatment

Average investment made by the other group members

Your Investment	0	1	2	3	4	5	6	7	8	9	10
0	10.00 10.00	10.75 11.95	11.50 13.90	12.25 15.85	13.00 16.10	13.75 16.35	14.50 16.60	15.25 16.85	16.00 16.85	16.75 16.85	16.30 14.30
1	11.45 10.25	12.20 12.20	12.95 14.15	13.70 16.10	14.45 16.35	15.20 16.60	15.95 16.85	16.70 17.10	17.45 17.10	18.20 17.10	17.75 14.55
2	12.90 10.50	13.65 12.45	14.40 14.40	15.15 16.35	15.90 16.60	16.65 16.85	17.40 17.10	18.15 17.35	18.90 17.35	19.65 17.35	19.20 14.80
3	14.35 10.75	15.10 12.70	15.85 14.65	16.60 16.60	17.35 16.85	18.10 17.10	18.85 17.35	19.60 17.60	20.35 17.60	21.10 17.60	20.65 15.05
4	14.10 11.00	14.85 12.95	15.60 14.90	16.35 16.85	17.10 17.10	17.85 17.35	18.60 17.60	19.35 17.85	20.10 17.85	20.85 17.85	20.40 15.30
5	13.85 11.25	14.60 13.20	15.35 15.15	16.10 17.10	16.85 17.35	17.60 17.60	18.35 17.85	19.10 18.10	19.85 18.10	20.60 18.10	20.15 15.55
6	13.60 11.50	14.35 13.45	15.10 15.40	15.85 17.35	16.60 17.60	17.35 17.85	18.10 18.10	18.85 18.35	19.60 18.35	20.35 18.35	19.90 15.80
7	13.35 11.75	14.10 13.70	14.85 15.65	15.60 17.60	16.35 17.85	17.10 18.10	17.85 18.35	18.60 18.60	19.35 18.60	20.10 18.60	19.65 16.05
8	12.85 12.00	13.60 13.95	14.35 15.90	15.10 17.85	15.85 18.10	16.60 18.35	17.35 18.60	18.10 18.85	18.85 18.85	19.60 18.85	19.15 16.30
9	12.35 12.25	13.10 14.20	13.85 16.15	14.60 18.10	15.35 18.35	16.10 18.60	16.85 18.85	17.60 19.10	18.35 19.10	19.10 19.10	18.65 16.55
10	10.10 12.10	10.85 14.05	11.60 16.00	12.35 17.95	13.10 18.20	13.85 18.45	14.60 18.70	15.35 18.95	16.10 18.95	16.85 18.95	16.40 16.40

The BLUE number on the left is your payoff. The BLACK number of the right is the payoff of each of the other group members when they each invest the amount listed.

Table B.7: Payoff table New-High treatment

Average investment made by the other group members

Your Investment	0	1	2	3	4	5	6	7	8	9	10
0	10.00 10.00	10.75 10.62	11.50 11.23	12.25 11.85	13.00 12.60	13.75 13.35	14.50 14.10	15.25 14.85	16.00 14.85	16.75 14.85	16.30 14.30
1	10.12 10.25	10.87 10.87	11.62 11.48	12.37 12.10	13.12 12.85	13.87 13.60	14.62 14.35	15.37 15.10	16.12 15.10	16.87 15.10	16.42 14.55
2	10.23 10.50	10.98 11.12	11.73 11.73	12.48 12.35	13.23 13.10	13.98 13.85	14.73 14.60	15.48 15.35	16.23 15.35	16.98 15.35	16.53 14.80
3	10.35 10.75	11.10 11.37	11.85 11.98	12.60 12.60	13.35 13.35	14.10 14.10	14.85 14.85	15.60 15.60	16.35 15.60	17.10 15.60	16.65 15.05
4	10.60 11.00	11.35 11.62	12.10 12.23	12.85 12.85	13.60 13.60	14.35 14.35	15.10 15.10	15.85 15.85	16.60 15.85	17.35 15.85	16.90 15.30
5	10.85 11.25	11.60 11.87	12.35 12.48	13.10 13.10	13.85 13.85	14.60 14.60	15.35 15.35	16.10 16.10	16.85 16.10	17.60 16.10	17.15 15.55
6	11.10 11.50	11.85 12.12	12.60 12.73	13.35 13.35	14.10 14.10	14.85 14.85	15.60 15.60	16.35 16.35	17.10 16.35	17.85 16.35	17.40 15.80
7	11.35 11.75	12.10 12.37	12.85 12.98	13.60 13.60	14.35 14.35	15.10 15.10	15.85 15.85	16.60 16.60	17.35 16.60	18.10 16.60	17.65 16.05
8	10.85 12.00	11.60 12.62	12.35 13.23	13.10 13.85	13.85 14.60	14.60 15.35	15.35 16.10	16.10 16.85	16.85 16.85	17.60 16.85	17.15 16.30
9	10.35 12.25	11.10 12.87	11.85 13.48	12.60 14.10	13.35 14.85	14.10 15.60	14.85 16.35	15.60 17.10	16.35 17.10	17.10 17.10	16.65 16.55
10	10.10 12.10	10.85 12.72	11.60 13.33	12.35 13.95	13.10 14.70	13.85 15.45	14.60 16.20	15.35 16.95	16.10 16.95	16.85 16.95	16.40 16.40

The BLUE number on the left is your payoff. The BLACK number of the right is the payoff of each of the other group members when they each invest the amount listed.

APPENDIX C: CHAPTER 4

INSTRUCTIONS [PHASE 1 – EXPERIMENTS 1 AND 2]

INTRODUCTION

This is an experiment about decision making and economic interaction. The experiment will consist of three phases. In phase 1 you will perform individually to secure earnings for later phases. In phases 2 and 3 you will be paired in groups of two people and will have to make decisions using the earnings you secured in phase 1.

The money you make in today's experiment will depend on your performance in phase 1 and on one of the decisions you [INFORMATION TREATMENTS - and the person you are matched] with make during the two subsequent phases. In addition you will receive $5.00 for showing up to the experiment. Your earnings in today's experiment will be paid to you in cash at the end of the session.

The instructions for phase 1 are listed below. The instructions for phases 2 and 3 will be distributed once we have completed phase 1.

No talking is allowed in today's experiment. If you have a question, please raise your hand and wait for an experimenter to come answer your question in private.

PHASE 1

In phase 1 of today's session you will be asked to perform a task. The task is to add as many sets of five two-digit numbers as you can in five minutes. Your performance on this task will determine your endowment in phases 2 and 3 [EXPERIMENT 1 - and will affect the money you can make in the experiment]. Your endowment in phases 2 and 3 will equal $2.00 plus $0.50 times your performance in phase 1. For example, if you solve 10 problems correctly your endowment will equal $2.00 + $0.50 × 10 = $7.00. If you solve 20 problems correctly your endowment will equal $2.00 + $0.50 × 20 = $12.00.

Each problem presented to you will appear as follows:

23	14	57	78	90

The five two-digit numbers displayed will be selected at random. For each new problem you have to provide an answer in the blank cell on the right. You may use paper and pencil to solve the problems, but the answer must be recorded in the blank cell. You must click the "OK" button to submit your answer and proceed to the next problem. Every time a new problem is presented, you will receive feedback on your performance. You will find out if the last answer you submitted was correct and will be notified of the number of problems you have solved correctly and incorrectly up until that point.

You will not be able to skip a problem and come back to it later. If you skip a problem by clicking "OK" when the right cell is left blank you will have spent time providing an incorrect answer. Once the five minutes are over no additional problems will be presented to you. A screen will indicate the number of problems you solved correctly in phase 1.

We are now ready to perform the task. Are there any questions before we begin?

INSTRUCTIONS [PHASES 2 AND 3 – EXPERIMENT 1]

PHASES 2 & 3

Phases 2 and 3 will each consist of 10 rounds. Your endowment in each round equals \$2.00 plus \$0.50 times your performance in phase 1. For example, if you solved 16 problems correctly in phase 1 you will have a \$2.00 + \$0.50 × 16 = \$10.00 endowment.

At the beginning of phase 2 you will be randomly assigned the role of first mover or second mover. Your role will be the same in phases 2 and 3 of the experiment. Half of the people in this room will be assigned the role of first mover and half will be assigned the role of second mover.

In both phases you will be randomly matched with a person of the opposite role. For the ten rounds of phase 2 you will be matched with one person and for the ten rounds of phase 3 you will be matched with another person. You will not know the identity of the person you are matched with and he or she will not know who you are.

Phases 2 and 3 consist of a total of 20 rounds. At the end of the experiment we will have one participant randomly select one of the 20 rounds to count for payment. Your payment will equal your earnings from the selected round.

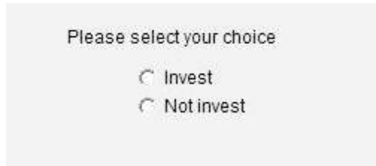
Investment decision

In every round you will be matched with a person and the two of you will participate in an investment decision.

The round proceeds as follows. The first mover first decides whether or not to invest his or her endowment with the second mover. The second mover then learns the first mover's investment decision, observes the return on the investment, and informs the first mover of his or her earnings.

The return on the investment can be either HIGH or LOW. When the return is HIGH the return on the investment is 200%, when the return is LOW the return on the investment is 100%. For example, if a first mover with an endowment of \$10.00 decides to invest and the return is HIGH, then the return on the investment is 200% × \$10.00 = \$20.00. If the return is LOW, then the return on the investment is 100% × \$10.00 = \$10.00. The chance that the return is HIGH is 3/4 and the chance that the return is low is 1/4. The computer will randomly determine whether the return is HIGH or LOW. We call the return selected by the computer the actual investment return.

The first mover sees the following screen and makes an investment decision.



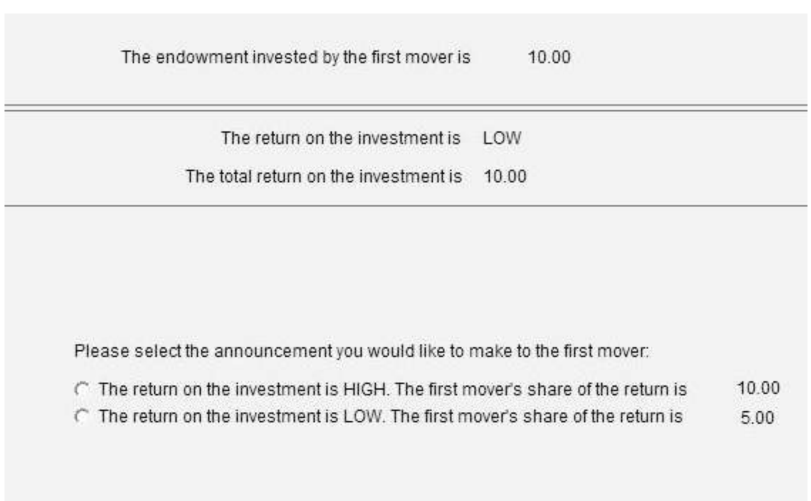
The second mover learns whether the first mover invested and finds out whether the actual investment return is HIGH or LOW. The second mover then announces a HIGH or LOW return on the investment to the first mover. We call this the announced investment return. The first mover only learns the announced investment return.

The total earnings the movers receive equals their initial endowment plus a share of the investment return. The first mover receives half of the investment return announced by the second mover. The second mover receives a share equal to the actual investment return that was not paid to the first mover.

The first mover's total earnings, when he or she invests, are thus equal to the invested endowment plus half of the announced investment return. For example, if the announced investment return is LOW, then the first mover's total earnings are his or her invested endowment ($10.00) plus half of the announced investment return (50% × $10.00 = $5.00), which equals $15.00. When the first mover decides not to invest, his or her total earnings from the round are simply equal to the initial endowment.

The second mover's total earnings, when the first mover invests, are equal to the second mover's initial endowment plus the actual investment return that was not paid to the first mover. For example, if the first mover's invested endowment is $10.00, the second mover's endowment is $9.00, and the actual and announced investment return is LOW, then the second mover's total earnings are equal to his or her endowment ($9.00) plus the actual investment return that was not paid to the first mover ($10.00 − $5.00 = $5.00), which equals $14.00. When the first mover does not invest, the second mover's total earnings are equal to his or her initial endowment.

When announcing the return on the investment the second mover knows the size of the investment and the share of the actual investment return that the first mover receives. For example, if the endowment invested by the first mover is $10.00 and the actual investment return is low, then the second mover sees the following decision screen.



[INFORMATION TREATMENTS –

At the beginning of phase 2 you will see the sequence of returns announced by four second movers in one phase of a previous session of the experiment. Announcements of high returns are denoted by H and announcements of low returns are denoted by L.]

Now I ask that you answer some questions to check your understanding. We will go over the correct responses before phase 2 begins.

CHECKING YOUR UNDERSTANDING [PHASES 2 AND 3 - EXPERIMENT 1]

1. In any given round, what is the probability that the return observed by the second mover is HIGH?

   3/4

2. In any given round, what is your endowment if you solved 6 problems correctly in phase 1?

   $2.00 + $0.50 × 6 = $5.00

3. Suppose you are the FIRST MOVER and your endowment equals $5.00.

a. What are your total earnings if you decide NOT to invest?

   $5.00

b. What are your total earnings if you invest and the return announced by the second mover is HIGH?

2 × $5.00 = $10.00	*because*	*First mover's invested endowment*	*$5.00*
		Announced investment return	*200% × $5.00 = $10.00*
		First mover's share of the return (half of the announced investment return)	*50% × $10.00 = $5.00*
		First mover's total earnings	*$5.00 + $5.00 = $10.00*

c. What are your total earnings if you invest and the return announced by the second mover is LOW?

1.5 × $5.00 = $7.50	*because*	*First mover's invested endowment*	*$5.00*
		Announced investment return	*100% × $5.00 = $5.00*
		First mover's share of the return (half of the announced investment return)	*50% × $5.00 = $2.50*
		First mover's total earnings	*$5.00 + $2.50 = $7.50*

4. Suppose you are the SECOND MOVER matched with the first mover described above. Your endowment equals $4.00.

a. What are your total earnings if the first mover decides NOT to invest?

   $4.00

b. What are your total earnings if the return you observe and announce to the first mover is HIGH?

\$4.00 + \$5.00 = \$9.00	*because*	*First mover's invested endowment*	*\$5.00*
		Actual investment return	*200% × \$5.00 = \$10.00*
		Announced investment return	*200% × \$5.00 = \$10.00*
		Return paid to the first mover (half of the announced investment return)	*50% × \$10.00 = \$5.00*
		Remaining actual investment return	*\$10.00 − \$5.00 = \$5.00*
		Second mover's total earnings	*\$4.00 + \$5.00 = \$9.00*

c. What are your total earnings if the return you observe and announce to the first mover is LOW?

\$4.00 + \$2.50 = \$6.50	*because*	*First mover's invested endowment*	*\$5.00*
		Actual investment return	*100% × \$5.00 = \$5.00*
		Announced investment return	*100% × \$5.00 = \$5.00*
		Return paid to the first mover (half of the announced investment return)	*50% × \$5.00 = \$2.50*
		Remaining actual investment return	*\$5.00 − \$2.50 = \$2.50*
		Second mover's total earnings	*\$4.00 + \$2.50 = \$6.50*

INSTRUCTIONS
[PHASES 2 AND 3 – EXPERIMENT 2]

PHASES 2 & 3

Phases 2 and 3 will each consist of 10 rounds. Your endowment in each round equals \$2.00 plus \$0.50 times your performance in phase 1. For example, if you solved 8 problems correctly in phase 1 you will have a \$2.00 + \$0.50 × 8 = \$6.00 endowment.

[INFORMATION TREATMENTS –

At the beginning of phase 2 you will be randomly assigned the role of first mover or second mover. Your role will be the same in phases 2 and 3 of the experiment. Half of the people in this room will be assigned the role of first mover and half will be assigned the role of second mover.

In both phases you will be randomly matched with a person of the opposite role. For the ten rounds of phase 2 you will be matched with one person and for the ten rounds of phase 3 you will be matched with another person. You will not know the identity of the person you are matched with and he or she will not know who you are. At the beginning of phase 2 everyone in this room will be randomly assigned an identification number. The identification number of the person you are matched with will be displayed on your screen every round.]

[COMPUTER TREATMENT –

In every round you will have to make a decision. Your decision will determine the earnings that you secure for the round.]

Phases 2 and 3 consist of a total of 20 rounds. At the end of the experiment we will have one participant randomly select one of the 20 rounds to count for payment. Your payment will equal your earnings from the selected round.

Investment decision

[INFORMATION TREATMENTS –

In every round you will be matched with one person and the two of you will participate in an investment decision.]

[COMPUTER TREATMENT –

In every round a first and a second mover will participate in an investment decision.]

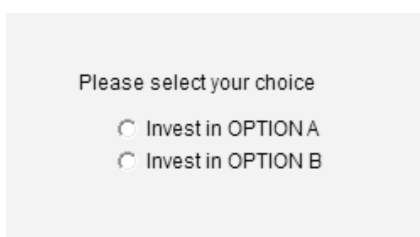
The round proceeds as follows. The first mover first decides whether to invest his or her endowment in Option A or B.

Investment in Option A generates a 40% return. This return is kept entirely by the first mover. For example, if a first mover with a $6.00 endowment invests in Option A his or her earnings for the round equal $8.40; the invested endowment ($6.00) plus the entire investment return (40% × $6.00 = $2.40). The second mover gets nothing from the first mover's investment in Option A. The second mover's earnings for the round equal his or her initial endowment.

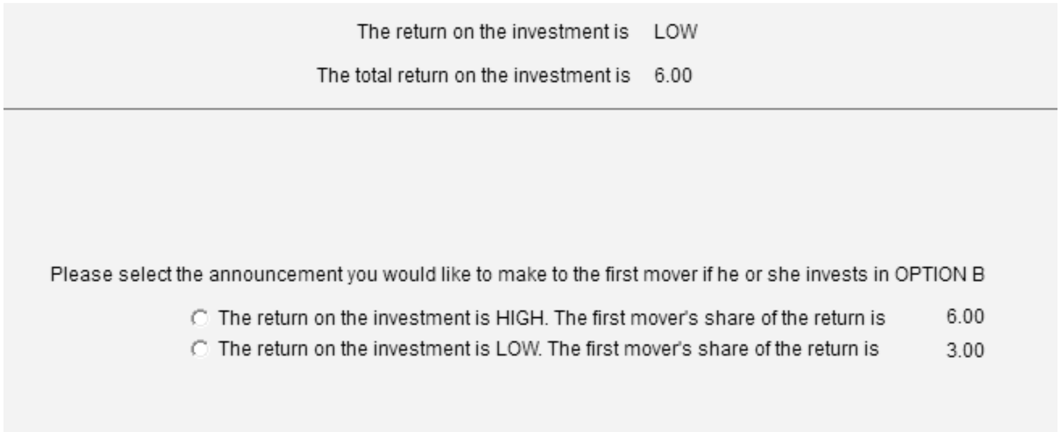
Investment in Option B can generate either a LOW or a HIGH return. This return is shared between the first and second mover. When the first mover invests in Option B, the second mover observes the actual return on the investment and announces a return to the first mover. The first mover learns the investment return announced by the second mover, but does not observe the actual return on the investment. The first mover receives half of the investment return announced by the second mover. The second mover keeps the investment return that is not paid to the first mover. The total earnings that each mover receives equal their initial endowment plus a share of the investment return.

The return from Option B, to be shared between the first and second mover, is 100% when the return is LOW and 200% when the return is HIGH. For example if a first mover with a $6.00 endowment invests in Option B, the total return on the investment is 100% × $6.00 = $6.00 when the return is LOW and 200% × $6.00 = $12.00 when the return is HIGH. The chance that the return is LOW is 1/4 and the chance that the return is HIGH is 3/4. The computer randomly determines whether the return is LOW or HIGH. The return selected by the computer is what we refer to as the actual investment return.

The first mover sees the following screen and makes an investment decision.



When the first mover invests in Option B, the second mover learns the actual return on the investment and announces a return to the first mover. Consider the example where a first mover invests a $6.00 endowment in Option B and the actual investment return is LOW. In this case, the second mover sees the following screen and decides whether to announce a LOW or a HIGH return on the investment.]



The announcement decision determines the share of the actual investment return (in this case \$6.00) that each mover receives. If the second mover announces a LOW return, the first mover's earnings for the round equal \$9.00; the invested endowment (\$6.00) plus half of the announced investment return (1/2 × \$6.00 = \$3.00). If the second mover announces a HIGH return, the first mover's earnings for the round equal \$12.00; the invested endowment (\$6.00) plus half of the announced investment return (1/2 × \$12.00 = \$6.00).

The second mover keeps the investment return not paid to the first mover. In the above example if the second mover has a \$5.00 endowment and he or she announces a LOW return, then the second mover's earnings for the round equal \$8.00; the initial endowment (\$5.00) plus the actual investment return that was not paid to the first mover (\$6.00 − 1/2 × \$6.00 = \$3.00). If instead the second mover announces a HIGH return, then his or her earnings for the round equal \$5.00; the initial endowment (\$5.00) plus the actual investment return that was not paid to the first mover (\$6.00 − 1/2 × \$12.00 = \$0.00).

[INFORMATION TREATMENTS –

Although choices are effectively made in sequence, the second mover will be asked to decide which return to announce before knowing the first mover's investment choice. The second mover's announcement becomes relevant and is learned by the first mover only when the first mover invests in Option B.

In each round the actual investment return is independently and randomly determined for each second mover. The actual investment returns observed by second movers therefore differ across participants and rounds.

At the beginning of phase 2 you will see the sequence of investment returns announced by four second movers in one phase of a previous session of the experiment. Announcements of high returns will be denoted by H and announcements of low returns will be denoted by L.]

[COMPUTER TREATMENT –

IN TODAY'S EXPERIMENT EVERYONE IN THIS ROOM WILL BE ASSIGNED THE ROLE OF SECOND MOVER. THE COMPUTER WILL MAKE FIRST MOVER CHOICES IN EVERY ROUND.

In every round of phases 2 and 3 the computer is programmed to have an \$8.00 endowment and to invest in Option B. The decisions that you make as second mover will therefore have NO impact on the investment option selected by the computer.

The earnings secured by the computer will NOT be paid to a subject. Your decisions will only affect YOUR payoff. They will NOT affect the payment that any other participant receives today.]

Before proceeding I ask that you answer some questions to check your understanding. We will go over the correct responses before phase 2 begins.

CHECKING YOUR UNDERSTANDING
[INFORMATION TREATMENTS - EXPERMENT 2]

1. In any given round, what is the probability that the return observed by the second mover is HIGH?

   3/4

2. Suppose you are the FIRST MOVER and your endowment equals $5.00.

a. What are your total earnings if you invest in Option A?

1.4 × $5.00 = $7.00	*because*	*First mover's invested endowment*	*$5.00*
		Investment return	*40% × $5.00 = $2.00*
		First mover's total earnings	*$5.00 + $2.00 = $7.00*

b. What are your total earnings if you invest in Option B and the return announced by the second mover is LOW?

1.5 × $5.00 = $7.50	*because*	*First mover's invested endowment*	*$5.00*
		Announced investment return	*100% × $5.00 = $5.00*
		First mover's share of the return	*1/2 × $5.00 = $2.50*
		First mover's total earnings	*$5.00 + $2.50 = $7.50*

c. What are your total earnings if you invest in Option B and the return announced by the second mover is HIGH?

2 × $5.00 = $10.00	*because*	*First mover's invested endowment*	*$5.00*
		Announced investment return	*200% × $5.00 = $10.00*
		First mover's share of the return	*1/2 × $10.00 = $5.00*
		First mover's total earnings	*$5.00 + $5.00 = $10.00*

3. Suppose you are the SECOND MOVER matched with the first mover described above. Your endowment equals $4.00.

a. What are your total earnings if the first mover invests in Option A? *$4.00*

b. What are your total earnings if the first mover invests in Option B, the return you observe is LOW, and you announce a LOW return to the first mover?

$4.00 + $2.50 = $6.50	*because*	*First mover's invested endowment*	*$5.00*
		Actual investment return	*100% × $5.00 = $5.00*
		Announced investment return	*100% × $5.00 = $5.00*
		Return paid to the first mover	*1/2 × $5.00 = $2.50*
		Remaining actual investment return	*$5.00 − $2.50 = $2.50*
		Second mover0s total earnings	*$4.00 + $2.50 = $6.50*

c. What are your total earnings if the first mover invests in Option B, the return you observe is LOW, and you announce a HIGH return to the first mover?

\$4.00 + \$0.00 = \$4.00 because	*First mover's invested endowment*	*\$5.00*
	Actual investment return	*100% × \$5.00 = \$5.00*
	Announced investment return	*200% × \$5.00 = \$10.00*
	Return paid to the first mover	*1/2 × \$10.00 = \$5.00*
	Remaining actual investment return	*\$5.00 − \$5.00 = \$0.00*
	Second move'0s total earnings	*\$4.00 + \$0.00 = \$4.00*

d. What are your total earnings if the first mover invests in Option B, the return you observe is HIGH, and you announce a HIGH return to the first mover?

\$4.00 + \$5.00 = \$9.00 because	*First mover's invested endowment*	*\$5.00*
	Actual investment return	*200% × \$5.00 = \$10.00*
	Announced investment return	*200% × \$5.00 = \$10.00*
	Return paid to the first mover	*1/2 × \$10.00 = \$5.00*
	Remaining actual investment return	*\$10.00 − \$5.00 = \$5.00*
	Second mover's total earnings	*\$4.00 + \$5.00 = \$9.00*

e. What are your total earnings if the first mover invests in Option B, the return you observe is HIGH, and you announce a LOW return to the first mover?

\$4.00 + \$7.50 = \$11.50 because	*First mover's invested endowment*	*\$5.00*
	Actual investment return	*200% × \$5.00 = \$10.00*
	Announced investment return	*100% × \$5.00 = \$5.00*
	Return paid to the first mover	*1/2 × \$5.00 = \$2.50*
	Remaining actual investment return	*\$10.00 − \$2.50 = \$7.50*
	Second mover's total earnings	*\$4.00 + \$7.50 = \$11.50*

CHECKING YOUR UNDERSTANDING
[COMPUTER TREATMENT – EXPERIMENT 2]

1. What role will you be assigned at the beginning of phase 2?

Everyone in this room will be assigned the role of second mover. The computer will make first mover choices in every round.

2. What is the endowment that the first mover will have in every round?

The computer is programmed to have an endowment of \$8.00 in every round.

3. Will the decisions that you make affect the behavior of the mover you are matched with?

NO, the computer is programmed to ALWAY S invest in Option B.

4. Will the decisions that you make today affect the payoff of another participant?

No, the payment secured by the computer will NOT be paid to another subject. Your decisions will only affect the payment that YOU receive today.

5. Suppose you are the SECOND MOVER, your endowment equals $4.00, and the first mover invests in Option B.

a. What are your total earnings if the return you observe is LOW and you announce a LOW return to the first mover?

$4.00 + $4.00 = $8.00 because	*First mover's invested endowment*	*$8.00*
	Actual investment return	*100% × $8.00 = $8.00*
	Announced investment return	*100% × $8.00 = $8.00*
	Return paid to the first mover	*1/2 × $8.00 = $4.00*
	Remaining actual investment return	*$8.00 − $4.00 = $4.00*
	Second mover's total earnings	*$4.00 + $4.00 = $8.00*

b. What are your total earnings if the return you observe is LOW and you announce a HIGH return to the first mover?

$4.00 + $0.00 = $4.00 because	*First mover's invested endowment*	*$8.00*
	Actual investment return	*100% × $8.00 = $8.00*
	Announced investment return	*200% × $8.00 = $16.00*
	Return paid to the first mover	*1/2 × $16.00 = $8.00*
	Remaining actual investment return	*$8.00 − $8.00 = $0.00*
	Second mover's total earnings	*$5.00 + $0.00 = $5.00*

c. What are your total earnings if the return you observe is HIGH and you announce a HIGH return to the first mover?

$4.00 + $8.00 = $12.00 because	*First mover's invested endowment*	*$8.00*
	Actual investment return	*200% × $8.00 = $16.00*
	Announced investment return	*200% × $8.00 = $16.00*
	Return paid to the first mover	*1/2 × $16.00 = $8.00*
	Remaining actual investment return	*$16.00 − $8.00 = $8.00*
	Second mover's total earnings	*$4.00 + $8.00 = $12.00*

d. What are your total earnings if the return you observe is HIGH and you announce a LOW return to the first mover?

$4.00 + $12.00 = $16.00 because	*First mover's invested endowment*	*$8.00*
	Actual investment return	*200% × $8.00 = $16.00*
	Announced investment return	*100% × $8.00 = $8.00*
	Return paid to the first mover	*1/2 × $8.00 = $4.00*
	Remaining actual investment return	*$16.00 − $4.00 = $12.00*
	Second mover's total earnings	*$4.00 + $12.00 = $16.00*

Table C. 1: Social information - Honest Information Treatment (HIT)

The table below shows the investment returns announced by four second movers in one phase of a previous session of the experiment. H represents HIGH and L represents LOW.

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10
Second Mover 1	H	H	L	H	H	H	H	H	H	L
Second Mover 2	H	H	H	H	H	H	H	H	H	H
Second Mover 3	H	H	H	H	L	L	H	H	H	H
Second Mover 4	H	L	H	H	H	L	H	H	H	L

Table C. 2: Social information - Dishonest Information Treatment (DIT)

The table below shows the investment returns announced by four second movers in one phase of a previous session of the experiment. H represents HIGH and L represents LOW.

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10
Second Mover 1	H	L	H	L	L	L	L	H	L	L
Second Mover 2	L	L	L	L	L	L	L	L	L	L
Second Mover 3	L	H	L	L	L	L	H	L	L	L
Second Mover 4	L	L	L	L	L	L	L	L	L	L

Table C. 3: Second mover behavior by honest type category

Experiment 1	Dishonest				Honest			
	NIT	HIT	DIT	All	NIT	HIT	DIT	All
Aggregate behavior								
% returns observed high	0.82	0.78	0.74	0.77	0.72	0.76	0.77	0.75
% announced high	0.25	0.29	0.03	0.14	0.50	0.55	0.71	0.57
% honest announcements when return high	0.29	0.31	0.04	0.16	0.62	0.67	0.89	0.70
% honest announcements when return low	0.88	0.93	1.00	0.95	0.86	0.79	0.84	0.83
Strategies employed								
% who play the SPE	0.20	0.33	0.78	0.53	0.00	0.00	0.00	0.00
% always dishonest when return high	0.60	0.33	0.78	0.65	0.00	0.00	0.00	0.00
% always honest when return low	0.60	0.67	1.00	0.82	0.46	0.33	0.44	0.41
% always honest	0.20	0.00	0.00	0.06	0.08	0.13	0.22	0.14
N returns observed	100	60	180	340	260	300	180	740
N subjects	5	3	9	17	13	15	9	37
Experiment 2	**Dishonest**				**Honest**			
		HIT	DIT	All		HIT	DIT	All
Aggregate behavior								
% returns observed high		0.80	0.80	0.80		0.80	0.80	0.80
% announced high		0.08	0.14	0.11		0.66	0.48	0.55
% honest announcements when return high		0.07	0.16	0.11		0.80	0.58	0.67
% honest announcements when return low		0.90	0.94	0.92		0.92	0.94	0.93
Strategies employed								
% who play the SPE		0.50	0.38	0.44		0.00	0.00	0.00
% always dishonest when return high		0.50	0.44	0.47		0.00	0.00	0.00
% always honest when return low		0.83	0.81	0.82		0.67	0.75	0.71
% always honest		0.00	0.00	0.00		0.00	0.13	0.07
N returns observed		360	320	680		120	160	280
N subjects		18	16	34		6	8	14

Table C. 4: Marginal effects of probit regressions of announcements - Honest types, Experiment 2

Dep. Var.: High announcement	Honest types		
	Phase 2	Phase 3	Phase 2 vs. 3
	(1)	(2)	(3)
return high	0.684***	0.591***	-0.169
	(0.000)	(0.000)	(0.584)
DIT	-0.796***	-1.000***	-1.000***
	(0.000)	(0.000)	(0.000)
perceived honesty	0.169	0.024	-0.146
	(0.715)	(0.958)	(0.824)
DIT X perceived honesty	0.627	0.613	-0.017
	(0.243)	(0.261)	(0.984)
% past investment FM	-0.144	-15.229***	-15.094***
	(0.382)	(0.000)	(0.000)
DIT X % past investment FM	0.467	15.540***	15.081***
	(0.266)	(0.000)	(0.000)
period	-0.017	-0.034	-0.017
	(0.547)	(0.128)	(0.546)
endowment	0.077	0.060	-0.018
	(0.207)	(0.311)	(0.833)
investment	0.028	0.008	-0.019
	(0.407)	(0.878)	(0.742)
perceived honesty ph2		3.204***	
		(0.000)	
DIT X perceived honesty ph2		-2.053**	
		(0.013)	
% investment FM ph2		1.859	
		(0.122)	
DIT X % investment FM ph2		-1.585	
		(0.203)	
N obs.	126	126	252
N clusters	14	13	13
L	-53.668	-54.342	-108.01
R squared	0.385	0.378	0.381

Note: P-values reported in parentheses. Standard errors clustered at the subgroup level. *p<0.10, **p<0.05, ***p<0.01. See text for description of variables.

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