OBSERVING OTHERS: THE EFFECT OF
BEHAVIORAL INFORMATION ON COLLECTIVE ACTION
IN SOCIAL DILEMMAS

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There has been an increasing interest in non-pecuniary measures to encourage pro-social behavior. Among these is the use of social comparison, or behavioral information. Individuals often conform their behavior to that exhibited by their peers. This dissertation explores the impact of this behavioral information on collective action in social dilemmas.

The first chapter systematically reviews the extensive, yet fragmented, literature on social comparison in order to begin assessing their applicability as a policy tool. To do so, the review draws on literature from a number of different disciplines, paying particular attention to experimental literature, to begin to identify the conditions under which behavioral information may be successful in encouraging welfare improving behavior.

The second chapter explores (in a lab experiment utilizing a linear public goods game) the use of voluntary disclosure to provide behavioral information when such information is not otherwise available. It finds that individuals tend to disclose their contributions when given the option, suggesting that voluntarily disclosed behavioral information remains a possible policy option when the cost of information collection is high. In addition, voluntarily revealed contributions are significantly higher than contributions
under mandated disclosure, ultimately leading to greater cooperation, under certain conditions, when voluntary disclosure occurs.

The final chapter is an experimental test of the impact of networked behavioral information on collective action. Social networks can be critical in the spreading of pro-social behavior, but their effect is diverse and complex. Focusing on one particular aspect of this effect, this study examines how information on others’ behavior affects individuals’ donation behavior in a threshold public good setting. The study finds that in these settings, when behavioral information networks span minimal coalitions (i.e., the minimal number of individuals necessary to provide a discrete public good), groups are more likely to be successful in overcoming the social dilemma problem.

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# TABLE OF CONTENTS

## Introduction

Behavioral Sciences and Public Policy ................................................................. 1  
Behavioral Information Interventions ................................................................. 2  
Dissertation Overview ....................................................................................... 3  
References ......................................................................................................... 5  

## Chapter I: Social Comparison as a Policy Tool: A Review of the Literature on Peer Effects in Social Dilemmas

Brief Overview ...................................................................................................... 7  
Introduction ........................................................................................................ 7  
Using Social Comparison to Address Social Dilemmas .................................. 11  
  Social Dilemmas ............................................................................................ 11  
  How Are Social Comparisons Used? – Research Approaches and Behavioral Interventions ................................................................. 16  
  Peer Group Effects and Interventions ......................................................... 17  
  Social Norms Marketing and Peer Information Interventions ......... 17  
The Effectiveness of Social Comparison: Behavioral (Ir)Regularities ............. 21  
  Indifference to Social Norm ....................................................................... 22  
  Boomerang Effect .................................................................................... 25  
Discussion: Sketching a Path Forward ............................................................ 34  
  Personalized Feedback ............................................................................... 34  
  Long Term Effects of Social Comparison Interventions ..................... 35  
  Cross-Disciplinary Synthesis .................................................................... 36  
References ......................................................................................................... 38  

## Chapter II: Voluntary Disclosure of Contributions: An Experimental Study on Non-Mandatory Approaches for Improving Public Good Provision

Brief Overview ...................................................................................................... 46  
Introduction ....................................................................................................... 48  
Social Information ............................................................................................. 50  
Experimental Design and Hypotheses .............................................................. 57  
  Experimental Design ................................................................................. 57  
  Hypotheses ............................................................................................... 60  
  Experimental Implementation ................................................................. 62  
Results ................................................................................................................ 63  
  General Observations ............................................................................... 63  
  Revisiting Hypotheses ............................................................................ 64  
Conclusion and Discussion ............................................................................... 70  
References ......................................................................................................... 73  
Tables ............................................................................................................... 78  
Figures ............................................................................................................... 84

Brief Overview ................................................................. 85
Introduction ................................................................. 86
Theoretical Model .............................................................. 92
  The Threshold Public Goods Game ........................................ 92
  Payoff Functions .............................................................. 95
  Nash Equilibria ................................................................. 95
  Minimal Coalitions ............................................................ 96
  Information Network Structure ............................................ 96
Experimental Treatments and Conjectures ................................ 97
  Experimental Design .......................................................... 97
    The Threshold Public Good Decision Setting ......................... 98
    Part I – One-shot Menu Games ........................................... 99
    Part II – Baseline Repeated Game – Aggregate Information Only .................................................. 100
    Part III – Repeated Game – Information Network Treatments .................................................. 100
  Experimental Implementation ............................................. 101
  Conjectures ...................................................................... 101
Results .............................................................................. 110
  Results from Part I – Menu Game ........................................ 110
    Overview ........................................................................ 110
    C1 – Endowment Effects ................................................. 112
  Results from Parts II and III – Repeated Game ........................ 113
    Overview ........................................................................ 113
    C1 – Endowment Effects ................................................. 115
    C2 - Network Structure Effects .......................................... 117
    C3 – Interaction Effects (between Endowments and Network Structure) ..................................... 120
    Convergence Behavior .................................................... 120
Conclusion and Discussion .................................................. 123
References ........................................................................ 126
Tables ............................................................................... 131
Figures ............................................................................ 139

Curriculum Vitae
TABLES

TABLES IN CHAPTER II:
Table 2.1: Overview of Treatments ................................................................. 78
Table 2.2: Summary Statistics: Group Level Contributions by Stage and Treatment ........................................................................................................ 78
Table 2.3: Panel Logit Regression on the Decision to Reveal Contributions...... 79
Table 2.4: Regression Analyses of Individual Level Contributions Given Public and Private Contributions ............................................................................. 80
Table 2.5: Regression Analysis of Group-Level Contributions in Voluntary Treatments ..................................................................................................... 81
Table 2.6: Regression Analyses of Individual Level Contributions in Voluntary Treatments ..................................................................................................... 81
Table 2.7: Regression Analyses of Change in Individual Level Contributions in Vol_Sim and Vol_Before Treatments .......................................................... 82

TABLES IN CHAPTER III:
Table 3.1: Between-Subject Experimental Design for Part III ....................... 131
Table 3.2: Summary Statistics: Contributions in Menu Game by Endowment Level ........................................................................................................ 131
Table 3.3: p-values of Two-sample Kolmogorov-Smirnov for Equality of Contribution Distributions in Endowment Settings in the Menu Game: .... 132
Table 3.4: p-values of Two-sample Wilcoxon Rank-Sum Test Comparing Contributions in Endowment Settings in the Menu Game: ......................... 132
Table 3.5: Logit Analysis of the Likelihood of Groups Meeting the Threshold in the Menu Game based on Endowment Setting ........................................... 132
Table 3.6: Summary Statistics: Contributions in Repeated Games by Part and Treatment .................................................................................................... 133
Table 3.7: p-values of Paired T-tests of Group Contributions Within Treatments, Across Periods .............................................................. 134
Table 3.8: p-values of Two-sample Wilcoxon Rank-Sum Test Comparing Contributions in LOW and HIGH Endowment Treatments ...................... 134
Table 3.9: Likelihood of Groups Meeting the Threshold in Part III .................. 135
Table 3.10.a: Random Effects Logit Models – Likelihood of Groups Contributing 1) Below; At 2) Asymmetric; 3) Symmetric); and 4) Above the Threshold in Part III ........................................................................................................ 136
Table 3.10.b: Pooled Multinomial Logit Models – Likelihood of Groups Contributing 1) Below; At 2) Asymmetric; 3) Symmetric); and 4) Above the Threshold in Part III ........................................................................................................ 137
Table 3.11: Random Effects Regression on Change in Individual Contributions in Part III by Endowment Level................................................................. 138
TABLES IN APPENDICES:

Table A.2.2.1: Overview of Treatments (including Vote before Treatment).... 151
Table A.2.2.2: Summary Statistics: Group Level Contributions by Stage and Treatment (including Vote before Treatment)......................................................... 153
Table A.2.2.3: Summary Statistics: Individual Level Contributions by Treatment and Publicity (including Vote before Treatment)............................................. 154
Table A.2.2.4: Regression Analyses of Individual Level Contributions Given Public and Private Contributions.................................................................................. 158
Table A.2.2.5: Paired Significance Tests of Coefficients in Models in Table 6. 159
Table A.3.1.1: Part I Decision Scenario Ordering................................................. 176
Table A.3.1.2: Summary Statistics: Mean Individual Contributions in Menu Game by Endowment Level and Position Seen in tokens (Standard Deviation). 177
Table A.3.1.3 p-values from Two-sample Wilcoxon (Mann-Whitney) Test ..... 180
Table A.3.1.4 p-values from Two-sample Wilcoxon (Mann-Whitney) Test ..... 181
Table A.3.1.5: Regression Analysis of Individual Contributions and Group Likelihood of Meeting the Threshold in Menu Game................................. 182
Table A.3.1.6: p-values of Wald Tests Comparing Coefficient Equality in Previous (Table A.3.1.5) Regressions: ................................................................. 183
FIGURES

FIGURES IN CHAPTER II:
Figure 2.1: Group-level contributions (in percent) to the Group Fund by treatment ................................................................. 84
Figure 2.2: Average Private, Combined, and Public Individual Contributions across Treatments .................................................................................................................. 84

FIGURES IN CHAPTER III:
Figure 3.1: LOCAL and COMPLETE Information Networks ........................................ 139
Figure 3.2: Mean Individual Contributions in Menu Game by Endowment Level ................................................................................................................................. 139
Figure 3.3: Histogram of Individual Contribution Decisions in Menu Game (Part I) by Endowment Setting .......................................................... 140
Figure 3.4.a: Mean Group Contributions in the Repeated Game in LOCAL and COMPLETE Treatments, Part II and III (in tokens) .......................... 141
Figure 3.4.b: Mean Group Contributions in the Repeated Game in LOW and HIGH Treatments, Part II and III (in tokens) ............................................ 142
Figure 3.5.a: Individual Contributions and Total Contributions in All Groups in the LOCAL Network LOW Endowment Treatment .................. 143
Figure 3.5.b: Individual Contributions and Total Contributions in All Groups in the COMPLETE Network, LOW Endowment Treatment .......... 144
Figure 3.5.c: Individual Contributions and Total Contributions in All Groups in the LOCAL Network, HIGH Endowment Treatment .................. 145
Figure 3.5.d: Individual Contributions and Total Contributions in All Groups in the COMPLETE Network, HIGH Endowment Treatment .......... 146
Figure 3.6.a: Histogram of Individual Contribution Decisions in Part II (Repeated Game, Periods 1-10) by Endowment Setting ...................... 147
Figure 3.6.b: Histogram of Individual Contribution Decisions in Part III (Repeated Game, Periods 11-20) by Endowment Setting and Information Treatment ........................................................................ 147
Figure 3.7: Individual Mean Contributions in Part II and Part III ...................... 148
Figure 3.8: Percentage of Groups Meeting the Threshold in Part II and Part III 148
Figure 3.7: Percentage of Groups Contributing i) BELOW, ii) AT, and iii) ABOVE the Threshold ................................................................................ 149

FIGURES IN APPENDICES:
Figure A.2.2.1: Percentage of Individuals Voting in Favor of Disclosure ........ 155
Figure A.2.2.2: Mean Individual Contributions in Vote_before Treatment (in Tokens) by Voting Decision and Vote Outcome ......................... 156
Figure A.3.1.1.a: Mean Contributions in the Menu Game by Endowment and Position .................................................................................. 177
Figure A.3.1.1.b: Mean Contributions in the Menu Game by Position and Endowment ................................................................................ 178
APPENDICES

SUPPLEMENTS TO CHAPTER II:
Appendix 2.1: Vote Treatment ................................................................. 150
Appendix 2.2: Experimental Instructions ................................................. 163

SUPPLEMENTS TO CHAPTER III:
Appendix 3.1: Ordering Effects in Part I (Menu Game) ............................. 175
Appendix 3.2: Experimental Instructions .................................................. 185
Introduction

Behavioral Sciences and Public Policy

There has been a recent increase in interest in utilizing the insights from behavioral economics and psychology to inform public policymaking (see, for example, Amir et al. 2005; Gowdy 2008; Thaler and Sunstein 2008; Riedl 2010; Croson and Treich 2014). There are two major ways in which the behavioral sciences may inform policymaking. First, they can highlight the conditions under which the rational actor model fails to predict human behavior (e.g., Amir et al. 2005; Gowdy 2008). Identifying these “irrationalities” may help fine-tune traditional policy tools (such as taxes, fines, and production or technology standards) so that the intended regulatory outcome may be achieved. In a similar vein, knowing of the limits of rationality allows for more accurate forecasts of policy outcomes.

The second role the behavioral sciences have played in policymaking is extending the non-pecuniary policy tool set. These tools, also known as “nudges” (Thaler and Sunstein 2008), leverage known behavioral tendencies/irrationalities to encourage welfare enhancing behavior. They do so without limiting an individual’s choice set as would, for instance, command and control instruments, such as production standards or outlawing certain behavior. Rather, they redesign an individual’s choice set (ibid). There are numerous nudges and other behavioral tools that apply to a number of different circumstances (Johnson et al. 2012). Perhaps one of the most prominent examples of this
type of “choice architecture” (Thaler and Sunstein 2008) would be changing prevailing
defaults from opting in to a welfare enhancing program/option to opting out. For
example, Johnson and Goldstein (2003) highlight the different rates of organ donation
consent (given when applying for a driver’s license) in Europe. Countries that are
culturally similar, such as Germany and Austria, differed vastly in consent percentage –
with Germany at 12% and Austria at 99.98%. One of the reasons for this divergence is
the different default settings these countries follow: in Germany, individuals have to opt
in to the organ donation program, whilst in Austria individuals have to opt out. This
striking difference in outcomes is a compelling illustration of the benefits of integrating
behavioral science into policymaking.

**Behavioral Information Interventions**

This dissertation explores a policy tool that has received somewhat less attention that
other nudges: the use of social comparison or behavioral information to encourage
welfare enhancing behavior. This tool leverages the tendency (given certain conditions)
of individuals to conform to social norms (i.e., observed or reported behavior by the
majority of others) (Turner 1991). By providing individuals with (selective) information
on how others are behaving (e.g., Cialdini et al. 1990), welfare enhancing behavior might
be encouraged without mandating any particular behavior. This approach has been used
in various policy areas, such as household electricity consumption (for an overview, see,
for example, Abrahamse et al. 2005; and Fischer 2008). The outcomes, although mixed,
have been encouraging. For example, in a large scale social comparison intervention
(facilitated by OPOWER), where more than 300,000 households received information on
the energy consumption by similar household, Alcott (2011) finds an average reduction of energy consumption of 2%. Although this may not seem large, it is estimated that it would require a (short-run) price increase of 11 to 20% to achieve the same effect (ibid). Depending on the political climate, a tax of that magnitude may not be a possibility. Thus, social comparison interventions (and other nudge-type policy tools) expand our ability to respond to public problems.

**Dissertation Overview**

There remains much we do not yet know about the application and impact of behavioral information interventions. This dissertation seeks to supplement the literature on social comparison effects by reviewing the literature to highlight policy applications and by exploring (via lab experiments) two areas that help us fine-tune social comparison interventions: i) behavioral information collection, and ii) conditions under which behavioral information may lead to successful coordination.

The dissertation consists of three chapters. The first reviews the literature on social comparison interventions, from a number of different fields, to assess their applicability as a policy tool. Given the breadth of the literature, this review focuses on the application of behavioral information to encourage collective action exclusively in the context of social dilemmas. Throughout the chapter, I integrate specific policy implications and potential areas of research. In doing so, I highlight the benefit of future cross-disciplinary synthesis. Chapter II explores the use of voluntary disclosure of contributions to a public good to provide behavioral information when alternative mechanisms must be considered.
because information is costly to collect and disseminate. Voluntary disclosure is studied in a lab experiment where the social dilemma is operationalized as a linear public goods game. Revealed contributions tend to be relatively high, suggesting that voluntary disclosure may be one way to construct a favorable social comparison, which in turn may make the behavioral information intervention more effective (Shang and Croson 2009).

Finally, Chapter III studies how information networks might impact a group’s ability to overcome collective action problems in a social dilemma setting (here, a provision point public goods game). In particular, it explores how behavioral information allows group members to coordinate on social welfare enhancing outcomes and shows that individuals need information on a sufficient number of others to be able to arrive at the social optimum.
REFERENCES


Chapter I: Social Comparison as a Policy Tool: A Review of the Literature on Peer Effects in Social Dilemmas

**BRIEF OVERVIEW**

This paper reviews the literature on social comparison interventions aimed at addressing large scale social dilemmas to assess their applicability as a policy tool. In doing so, I draw on research from a number of different fields to highlight the benefit of future cross-disciplinary synthesis. Given the extent of the literature, this review is by no means complete, but rather uses studies to identify concrete policy advice and potential avenues of research. The paper contains three major sections. I first conceptualize social dilemmas – the context in which this paper analyzes the effectiveness of social comparison – and then describe how, generally, social comparison works. The second section outlines the conditions under which social comparisons may be more or less effective tools by identifying behavioral regularities in the literature. This section explains two general ways in which social norm interventions may fail and, for each, identifies corresponding design features that may help overcome these failures. Finally, the concluding section tries to pave a path forward in the study of peer effects in social dilemmas by highlighting three important areas of research. Throughout the paper, I integrate specific policy implications and potential areas of research.

**INTRODUCTION**

Institutions can restructure incentives and may thus help overcome collective action problems in social dilemmas (see, for example, Ostrom 1990, 2005; Barrett 2003). Among these institutions, social norms have been identified as conveying appropriate action without (necessarily) imposing penalties for deviant behavior (Crawford and
adherence to social norms arises from numerous sources, such as fear of social ostracism, avoidance of guilt, conscientiousness, expectation of future collaboration, and the belief that norms convey information about effective strategies. Evidence suggests that these social norms are particularly effective in helping smaller communities overcome collective action problems (such as common pool resource management – see Ostrom 1990). For larger populations, however, there is still some uncertainty about how social norms may be utilized effectively to encourage social-welfare improving behavior in large-number, small-payoff social dilemmas, such as reducing household water and electricity consumption (Carlson 2001; Ferraro et al. 2011; Abrahamse and Steg 2013; Sacerdote 2014).

The impact of social norms has been studied, among other ways, by analyzing how individuals respond to observing (or being provided with information on) the behavior of others. In public policy, these social comparison effects have received renewed interest with the rise of “nudges” as a tool to encourage welfare improving decisions without coercive measures (Thaler & Sunstein, 2008). Subsequently, the use of social comparison has been tested in various recent field experiments (for example, Frey and Meier 2004; Schultz et al. 2007; Alcott 2011; Ferraro and Price 2013; Delmas and Lessem 2014).

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1 One might argue that behavior divergent from social norms will be met with social penalties, such as ostracism. However, these penalties are not specified with regards to norms (Crawford and Ostrom 1995), but, instead, are implicit. Further, the form social penalties take may be difficult to foresee.

2 See, for example, Ostrom’s design principles for conditions favorable to the evolution of effective (in terms of collective resource management) shared norms and rules (Ostrom 2005; Cox et al. 2010). Ostrom highlights the importance of small close-knit communities in the evolution of shared norms (Ostrom 2000).

3 Social norms may be interpreted broadly (see, for example, Cialdini et al. 1990). In the context of this review, social norms refer to general codes of conduct (as used by Blanton et al. 2008). These social norms may be captured by simple statistics, such as “X% of a given population engage in a given act Y.”
However, the literature on these effects is expansive and fragmented. For example, in sociology and political science, social comparison effects have been studied in such contexts as contagion models to explain how behavior (such as political revolutions) and opinions might spread through a society. In economics and psychology, social comparison effects have been studied in a number of experimental and field settings. For instance, social comparison has been studied extensively with regards to its impact on tax filing behavior (e.g., Fortin et al. 2007), education outcomes (see, for example, Sacerdote 2014 for an overview), binge drinking behavior (e.g., Blanton et al. 2008) and pro-environmental behavior (see, for example, Abrahamse and Steg 2013 for an overview).

Although these literatures study similar phenomena – in how far behavior is affected by the behavior of others – they seemingly study these in isolation, rarely breaching disciplinary and research methodological divides. Further, many studies focus on individual aspects of social comparison (in part to isolate and test conditions that impact the effectiveness of social comparison) and do not gather findings across conditions. To the author’s knowledge, there have been very few studies working to synthesize findings across these different areas to systematically assess the conditions under which social comparison may be an effective policy tool.4 Given the breadth of the literature, the

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4 There are several papers that discuss the conditions under which various nudges are effective. For instance, Johnson et al. (2012) analyze the circumstances under which to use a set of different nudges. Loewenstein et al. (2013) review the application of information disclosure and Truelove et al. (2014) analyze when pro-environmental behavioral spillovers are more likely. This paper begins to sketch out a similar synthesis, but it is somewhat more limited in scope and focuses specifically on social comparison effects. With regards to social comparison effects, I have identified three papers that work to synthesize findings and are thus related to this paper: Abrahamse and Steg (2013); Sacerdote (2014); and Miller and Prentice (2016). Abrahamse and Steg (2013) use meta-analytic tools to synthesize findings in social psychology on the use of social influence to encourage resource conservation. The current paper differs from the aforementioned studies in that it focuses on social comparison (a subcategory of social influence) only, which allows for a more finely grained analysis of the conditions under which social comparison
The current review will be by no means exhaustive, but it will draw on the literature in various different fields to begin to highlight conditions under which the successful application of social comparison is more or less likely. In doing so, it sketches how cross-disciplinary synthesis might, in the future, provide clear policy advice in using social comparison to overcome large scale collective action problems.

The paper contains three major sections. In the first section, I conceptualize social dilemmas – the context in which this paper analyzes the effectiveness of social comparison – and describe how, generally, social comparisons work. Specifically, I describe i) how the strategic setting of collective action problems impacts incentives and individuals’ motivations to react to social norms and ii) describe the primary ways in which social comparison interventions have been used in social science research. The second section outlines the conditions under which social comparisons may be more or less effective tools by identifying behavioral regularities in the literature. This section, while not designed to be fully comprehensive, explains two general ways in which social norm interventions may fail and, for each, identifies corresponding design features that
may help overcome these failures. Finally, the concluding section discusses a path forward in the study of peer effects in social dilemmas. It begins by offering two areas of potentially policy-relevant and fruitful research, concerning: i) the promise of personalized feedback and ii) the conditions under which behavioral change persists in the long-term following social comparison interventions. Finally, I describe the need for cross-disciplinary synthesis to leverage findings across policy areas and disciplinary divides, in order to construct effective policy interventions and identify research gaps. Throughout the paper, I integrate specific policy implications and potential areas of research.

**USING SOCIAL COMPARISON TO ADDRESS SOCIAL DILEMMAS**

**Social Dilemmas**

Social comparison effects, in the context of this review, refer to changes in behavior of individuals after observing, or receiving information about, the behavior of others. Both behavior changes toward, and away from, the observed behavior is being considered. The literature on these effects is expansive, hence the review will focus on social comparison effects in social dilemma situations only, where incentives encourage individual behavior that is at odds with behavior beneficial to the group as a whole. Thus, I assess the effectiveness of social comparison to encourage pro-social behavior (i.e., behavior that benefits the group).

Social dilemmas include a variety of different strategic settings (most prominently, perhaps, public goods games and common pool resource games). The defining feature of
a social dilemma is the mismatch between individual-level incentives and group welfare. Benefits from an action accrue only to the individual whilst costs are shared across the group. This leads to a suboptimal provision of a public good and overexploitation of a common pool resource (for a detailed explanation, see Bergstrom et al. 1986; and Hardin 1968, respectively). Further, if contributions toward a public good (or extractions from a common pool resource) are strategic substitutes, then others may free ride on an individual’s contributions (or refrainment). Hence, individuals may not contribute as much as they otherwise would in fear of being taken advantage.\(^5\) In the lab, social dilemmas are clearly defined. In the real world, however, it is not always clear whether a certain situation may be classified (or is perceived) as such (see, for example, Attari et al. 2014). Consider, for instance, household energy conservation and donations to NPR (examples taken from Attari et al. 2014). Donating to public radio is a classic example of a public goods game: Individuals are better off when enough donations are contributed to fund public broadcasting, but are even better off if these funds are contributed only by other members of the community. Consequently, donation behavior (i.e., charitable giving) has motivated numerous linear-public goods lab experimental studies (see, for example, Isaac and Davis 2006). The classification of household energy conservation, however, is less straightforward. Reducing household energy consumption provides a public good via its role in mitigating climate change. However, individuals actually save money if they reduce their energy consumption, thus the underlying conflict between individual and group incentives is not necessarily given.\(^6\) Individuals also may not

\(^5\) Note that suboptimal contributions may result from many different preference specifications. See, for example, Fehr and Schmidt (1999) for a discussion of inequality aversion that would explain this behavior.

\(^6\) However, in an equilibrium, individuals would balance the marginal benefit derived from cost savings from curbing energy use with the marginal cost of doing so (e.g., being warmer in summer, colder in
perceive it as a social dilemma; they may not consider their energy consumption as impacting anyone else’s welfare and hence do not consider free-riding in the first place. Indeed, Attari et al. (2014) demonstrate that the motivations for cooperating or defecting in these two areas are substantially different, with free-riding behavior being a greater concern in donations for public radio than for energy conservation. Interestingly, although reviews of social comparison effects distinguish between different policy areas and thus different strategic settings, they do not, to the author’s knowledge, systematically distinguish how problems might be perceived within a strategic setting. Drawing these distinctions may provide insights into the conditions under which particular types of social comparison interventions may be more effective.

**POTENTIAL RESEARCH AREA: The Impact of Problem Perception on the Effectiveness of Social Comparison Interventions**

For more detailed policy recommendations, it may be useful to combine the literature on how issue areas are perceived by individuals and their motivations for action (e.g., Attari et al. 2014) with the literature on social comparison. Given different motivations for action, social comparison may be targeted to, for example, allay fears of free-riding or play up reputational concerns (see discussion below). This will thus have a direct effect on the design of the policy intervention.

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winter, etc.). Thus, individuals would still have a net (non-pecuniary) cost to bear when reducing energy consumption – misalignment between individual and group incentives does occur. However, this depends on the assumption that individuals are maximizing their utility at the current energy consumption level. This may not be the case if there are informational gaps (as to how to reduce energy consumption; for example, Attari et al. 2010) or if individuals exhibit one of many possible behavioral “irrationalities” (see Kahneman 2011 for an overview).
As mentioned above, social dilemmas may be framed in terms of appropriation or provision games (Ostrom et al. 1994). In the former, the pro-social action is to reduce one’s consumption of the common pool resource; in the latter, the pro-social action is to increase one’s contribution toward the public good. Likewise, in environmental economics, in situations of pollution, the actions that lead to social optimal levels of pollution may be framed as increases in abatement or reductions in the polluting activity. To avoid confusion with regards to what might be considered a positive change (i.e., a pro-social action) and what it means to be below average, I will frame the discussion in pro-social action terms. This means that all individuals who act less pro-socially than the average (e.g., high household energy consumers, low public good donators) are below average on a pro-social scale. Thus to improve, they must increase their pro-social activities. Likewise, energy efficient households (low energy users) and high donators are considered above average and would decrease their pro-social actions by converging on the social norm/observed average contribution.


Given a number of conditions (discussed below), on average, individuals will change their behavior to converge toward the observed (or provided) behavioral norm (Festinger 1954; Asch 1956; Cialdini et al. 1990; Turner 1991, Cialdini and Goldstein 2004). There are several reasons why individuals might change their behavior following information on social norms. These fall into three broad categories: intrinsic, extrinsic, and image motivations (Bénabou and Tirole 2006; Ariely et al. 2009). Intrinsic motivation refers to

Note that Ostrom’s delta parameter, symbolizing the cost of abiding/disregarding a social norm, captures motivations from all three categories (Crawford and Ostrom 1995; Ostrom 2005).
a personal preference for a particular action (in this case, norm conformance or divergence). This includes deriving direct utility from norm conformance (Beshears et al. 2015), considering oneself a rule-abiding individual (i.e., identity considerations – Akerlof and Kranton 2000), and having concern for the wellbeing of others (i.e., social preferences – see, for example, Andreoni 1989; Fehr and Schmidt 1999; Bolton and Ockenfels 2000). Extrinsic motivation occurs when rewards or punishments, such as monetary fines and praise, are received for partaking in a particular action. In this category, we might also place strategic reasons such as inciting reciprocal behavior by others. Image motivation (also reputation motivation – Delmas and Lessem 2014) arises when actions may be used to signal virtue (which cannot be observed directly); this, in turn, affects social standing/reputation, which provides utility for an individual (Bernheim 1994; Bénabou and Tirole 2006). Aside from these three categories, norms might convey private information about what is more optimal behavior (Deutsch and Gerard 1955; Banerjee 1992; Beshears et al. 2015). For instance, in household energy conservation applications, individuals may not be aware that greater energy conservation is possible (Miller and Prentice 2016) or may misjudge what actions are most effective in curbing energy consumption (Attari et al. 2010).

The relevance of any of these particular motivations will depend on the (perceived) strategic incentives (see discussion above). This will have implications for how to target

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8 For a discussion of conditional cooperation, see Fischbacher et al. 2001; and for a discussion of trust and reciprocity see Ostrom 2003.
9 For this review I draw a distinction between learning which action leads to a desired outcome (i.e., information on the relationship between actions and outcomes) and learning a new skill. The latter may arguably be situated in the social learning literature (see, for example, Bandura 1977) and is those beyond the scope of this review.
social comparisons. Take, for example, the final motivation – informational content of the social norm. In a situation with high uncertainty or where individuals lack expertise – such as in retirement savings\textsuperscript{10} – the informational content of social norms may be high. In turn, this motivation may explain a large component of mimicry. By contrast, Miller and Prentice (2016) argue that information content plays a smaller role (in particularly) in social dilemma-type situations because presenting electric consumption information publicly is more effective in reducing consumption than private information (Delmas and Lessem 2014).\textsuperscript{11} In support of that argument, Ferraro and Price (2013) find no significant difference in household water consumption across treatments with and without technical information on water saving techniques. Regardless, many intervention studies aimed at household utility consumption include technical information to ensure that customers are able to reduce their consumption if they intend to do so (Abrahamse et al. 2005; Alcott 2011; Ferraro and Price 2013).

How Are Social Comparisons Used? – Research Approaches and Behavioral Interventions

The way that social comparisons have been studied and, in part, used to encourage welfare-increasing behavior can be divided into two general categories: i) manipulating peer groups (or natural experiments regarding group composition) to change the prevailing social norm, and ii) providing subjects with information on (artificial) social norms. I provide a brief overview of both these approaches in turn.

\textsuperscript{10} Operationalized as, for example, information on which fund others are investing in (Beshears et al. 2015).
\textsuperscript{11} Miller and Prentice (2016) argue that this implies that image motivation is of greater importance the informational content of the social norm.
Peer Group Effects and Interventions

Using natural experiments that place individuals in peer groups or leveraging cross-group differences, this approach is widespread across many policy areas, with scholars across disciplines seeking to identify and measure the impact of peers on the behavior of others (Manski 1993). To illustrate this approach, consider the education literature: The study of peer effects is particularly wide-spread in the education literature, where test scores and school assignments (or even random college roommate assignments) can be leveraged to measure peer effects (see, for example, Sacerdote 2014 for an overview). The results, however, have been mixed: peer effects are only sometimes significant in explaining grades and test scores (ibid). Further, the approach runs into difficulty disentangling peer effects from homophily (Lazarsfeld and Merton 1954; Jackson 2014) – the tendency of individuals to associate based on shared characteristics. Thus, even when peer groups are manipulated (e.g., random assignment to classes or teams), endogenous peer selection takes place, making accurate measuring of peer effects difficult (Carrell et al. 2013). This approach has thus not yet resulted in targeted group interventions to encourage welfare-enhancing behavior (see discussion in Sacerdote 2014).

Social Norms Marketing and Peer Information Interventions

Social norms marketing (also termed peer information interventions – Beshears et al. 2015) informs subjects of a (possibly artificial) social norm and has thus taken a much more interventionist approach to studying social comparison effects compared to the previous approach. This approach arose from seminal works in social psychology on
social influence that have shown the profound effects of social norms and peer pressure on opinion and behavior (Asch 1956; Turner 1991). Building on these insights, social psychologists, Robert Cialdini in particular, began manipulating social norms and providing subjects with this information to encourage pro-social actions (e.g., Cialdini et al. 1990). This approach has now spread to several different disciplines and problem areas such as curbing college binge-drinking (for overviews: Wechsler et al. 2003; Borsani and Carey 2003; Blanton et al. 2008; Miller and Prentice 2016), encouraging pro-environmental actions (Cialdini et al. 1990; for overviews: Cialdini et al. 2006; Abrahamse and Steg 2013; Asensio and Delmas 2015; Miller and Prentice 2016), improving voter turnout (Gerber et al. 2008; Gerber and Rogers 2009), and encouraging saving for retirement (Duflo and Saez; Beshears et al. 2015).

Cialdini (1988) distinguishes between two types of norms that may have differing impacts on behavior: **descriptive norms** and **injunctive norms**. Descriptive norms\(^\text{12}\) (Cialdini et al. 1990) refer to the actions individuals in a given reference group take. This is often expressed as a frequency statistic.\(^\text{13}\) As such, this is similar to behavioral information subjects receive in several public goods and common pool resource lab experiments (see, for example, Sell and Wilson 1991; Croson 2001; and Janssen 2013) or in field experiments on donation behavior (e.g., Shang and Croson 2009). Injunctive norms\(^\text{14}\) (Cialdini et al. 1990) are the values held by society or individuals in a given

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\(^{12}\) Also referred to as behavioral norms (Blanton et al. 2008).

\(^{13}\) For example, Goldstein et al. (2008), in their study of encouraging hotel guests to reusing towels, operationalize descriptive norms as follows: “Almost 75% of guests who are asked to participate in our new resource savings program do help by using their towels more than once.” (p.474)

\(^{14}\) Also termed prescriptive norms (Miller and Prentice 1996) and attitudinal norms (Miller and Prentice 2016).
reference group.\footnote{15 Injunctive norms may also be expressed using approval frequencies for a particular action within a given group. However, they may also be kept vaguer: For example, Schultz et al. (2008), in a similar study of encouraging guests to reuse hotel towels (see \cite{12}), express an injunctive norm as follows: “Many of our guests have expressed to us their approval of conserving energy. Because so many guests value conservation and are in the habit of conserving, this hotel has initiated a conservation program.” (p.8)} Injunctive norms thus convey information regarding the degree to which an action is approved of by individuals in a group.

Studies using social norms marketing have relied on two behavioral tendencies: First, individuals tend to overestimate the frequency with which others engage in undesirable actions and thus tend to underestimate the pro-social norm (Schultz et al. 2007). For example, college students routinely misperceive how often and how much alcohol their peers consume (e.g., Prentice and Miller 1993; Borsari and Carey 2003; Wechsler et al. 2003; Blanton et al. 2008). Providing subjects with the true norm, they will now tend to conform to the new social norm, thus improving overall behavior.\footnote{16 With regards to misperceptions of the social norm, the \textit{distribution} of the bias in judgment will impact the effectiveness of social comparison as a policy tool. For example, if individuals who engage in less social welfare enhancing behavior systematically believe the norm to be lower by a greater extent than high performers believe the norm to be higher, then presenting the population with the true mean will still result in positive social welfare effects even if both groups converge on the newly presented social norm. (See discussion about the boomerang effect below).} Second, norm saliency increases the likelihood of individuals complying with a given social norm (Cialdini et al. 1990). Therefore, even if there is no misperception of the prevailing social norm, focusing individuals on what is considered appropriate behavior impacts subjects’ behavior (Cialdini et al. 1990, Kallgren et al. 2000). Miller and Prentice (2016) indicate that studies applying social norms marketing to social dilemmas often do not rely on rectifying a misperceived norm, suggesting that the second factor (norm saliency) may be more important in this area.\footnote{17 Rather, they rely on social norms signaling to individuals which actions are seen as appropriate (Ostrom 2001). Thus, negative deviation from the norm can be seen as a moral failing, which may result in social punishments and lack of reciprocal behavior. By contrast, individuals who contribute above and beyond the...}
Broadly speaking, the social norms marketing approach consists of two parts (Blanton et al. 2008): First, researchers collect information on the prevailing social norm/behavior. This information may be gathered via surveys\(^{18}\) or observation\(^{19}\). Alternatively, researchers have also utilized artificially constructed norms (see Croson and Treich for a discussion). Second, researchers (or policymakers) disseminate the previously collected social norms information. In doing so, researchers distinguish between two different dissemination approaches: social norms marketing and personalized normative feedback (Blanton et al. 2008, Miller and Prentice 2016). Social norms marketing (also “social norm information and feedback” Abrahamse and Steg 2013) is a more generalized approach where subjects receive a message about the prevailing norm only (e.g., Nolan et al. 2008; Schultz et al. 2008; Goldstein et al. 2008). This form of social comparison feedback is particularly relevant where there is no easily accessible behavioral history on subjects (e.g., hotel towel usage – Goldstein et al. 2008) or where the targeted behavior is a one-time event (e.g., removing petrified wood from Arizona’s Petrified Forest National Park as a souvenir – Cialdini et al. 2006). Personalized normative feedback (also “socially comparative feedback” – Abrahamse and Steg 2013) put the subject’s personal behavioral history into context of the prevailing social norm and provide the subject with relative information (e.g., Jones and McKee 2004; Schultz et al. 2007; Ferraro and Price 2013). This approach has been used when trying to encourage behavioral change in repeated decision-making scenarios and may be particularly effective when individuals

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\(^{18}\) Used frequently in studies of, for example, college drinking behavior – e.g., Blanton et al. 2008

\(^{19}\) Used frequently in studies of, for example, retirement saving – e.g., Beshears et al. 2015
are not aware of their own behavior (as may often be the case in household energy consumption).

**THE EFFECTIVENESS OF SOCIAL COMPARISON: BEHAVIORAL (IR)REGULARITIES**

Having discussed the mechanics of social comparison interventions, let us now turn to their effectiveness. Although there are numerous studies that provide evidence of successful interventions, the impact of social comparisons is still contested. In a meta-analysis of studies of the impact of social influence on pro-environmental behavior, Abrahamse and Steg (2013) find that social norms marketing has a small but positive impact on behavior. In education and curbing of college drinking, the evidence is mixed (Sacerdote 2014; and Blanton et al. 2008, respectively). Abrahamse et al. (2005) find mixed evidence for the impact of information campaigns in household energy conservation.

From the literature it seems that there are two major ways in which social comparison fails to encourage pro-social behavior. Generally speaking, these are: i) individuals do not respond to social norms and/or ii) individuals respond to them in undesirable ways (i.e., the boomerang effect). I begin by discussing each of these failings in turn, before, based on behavioral regularities observed in individual studies, identifying design features that may help avoid the aforementioned problems.
Indifference to Social Norm

Individuals will be motivated to different extents by social comparison interventions. If image motivation (see above) is not central to some individuals, they will tend to be left unmoved by social norms marketing. We should thus expect that social comparison interventions will not result in a homogenous treatment effect (Miller and Prentice 2016). In particular, if we expect convergence toward the norm (see discussion above), then some individuals (those who are close to the descriptive norm) will not change their behavior in response to receiving information on others’ behavior. However, aside from increasing the salience of social norms (Cialdini et al. 1990; Kallgren et al. 2000; see above), there are a number of factors that have been shown to make it more or less likely that individuals will be impacted by social norms marketing. Three such factors and their policy implications are discussed below.

**Policy Implication: Carefully Construct Reference Groups**

The reference group (i.e., the group to whose behavior one chooses to conform) has a significant impact on the success of social comparison (Turner 1991). Several different studies concerning college binge-drinking have shown that individuals are most likely to be swayed by social norms held by individuals most like themselves (see Miller and Prentice 2016 for an overview). Further, the same social norm may result in diametrically opposed effects depending on the reference group that is used. For instance, Gino et al. (2009) find that students are less likely to cheat (in a math task during a psychology experiment) when they observe a student wearing a sweater of a rival university cheating than when they observe a cheater wearing a sweater from their own university. This fits
with Turner’s (1991) argument that individuals either converge or diverge from a social norm depending on whether or not they want to be associated with a given group that holds said social norm.

The task of identifying the appropriate reference group is, however, not always straightforward. Consider, for example, the study by Goldstein et al. (2008) on encouraging hotel guests to reuse their hotel towels through informing guests that a certain share of others does. They impose five different treatments that differ with regards to the reference group (e.g., citizens, hotel guests, hotel guests in this room, men and women). The frame referring to hotel guests that stayed in the same room as the subject elicited the highest response, suggesting that, counter to the self-ascribed identity by guests, a geographically proximate reference group was most effective. Thus, individuals will not always latch on to the reference group that may seem most relevant to the policymaker – few people would naturally identify with nameless hotel guests that stayed in the same room previously – meaning that careful consideration must be given to selecting the appropriate reference frame.

**Policy Implication: Provide Attainable Social Norms**

Even after identifying the most relevant reference group, the distance to the reported social norms may be critical to encouraging behavioral change: If the behavior from individuals diverges too much from social norms, then presenting individuals with behavioral information may not result in convergence. Individuals may not attempt to change/improve their behavior (Croson and Shang, 2013) under these circumstances.
They may actually reduce their efforts and move away from the social norm due to discouragement (Beshears et al. 2015). This effect might be counteracted by favoring personalized feedback over general social norms marketing. This allows policy makers to manipulate the social norm to remain achievable for the individual by reporting the behavior of a particular quartile of the population (see discussion below). Alternatively, policy makers can provide a personalized suggested contribution level which has been shown to be effective in increasing cooperation in public goods games (Jones and McKee 2004).

**Policy Implication: Scrutiny May Enhance Social Comparison Effects**

Returning to the different types of motivation, it is clear that image motivation (i.e., utility received from how one is perceived by others) is particularly important when actions are public/identifiable. When this is the case, individuals are more likely to engage in the pro-social action (Levitt and List 2007). Individuals who are not otherwise impacted by social norms may now have sufficient incentive to change their behavior. This is supported by several social dilemma lab experiments: Identifiability is well documented to raise contributions in public goods experiments (see, for example, Croson and Marks 1998; Andreoni and Petrie 2004). In a study on energy consumption by college students, Delmas and Lessem (2014) replicate this effect – when their energy efficiency is publicly ranked in their dorm, students exhibit greater effort to reduce their energy consumption. This suggests that social comparison interventions may be particularly effective in changing highly visible behavior (such as excessive lawn watering) rather than hidden behavior (such as lengthy showers). Social comparison may
be similarly effective where improvements in technology now make publicizing of behavior more feasible.

There are, however, caveats to using increased scrutiny to enhance the effect of social comparisons. For instance, if behaviors that are considered substitutes (such as excessive watering of lawn and lengthy showers in terms of water savings potential) receive different amounts of scrutiny, individuals might compensate for additional effort in the public domain by reducing their effort in the hidden domain – there is a negative spillover from engaging in a pro-social behavior (Truelove et al. 2014). Further, when engaging image motivation, policy makers must recognize the potential for crowding out pro-social behavior by providing monetary incentives (perhaps intended to encourage further pro-social behavior). When one receives monetary benefits from engaging in a pro-social public act, engaging in this act is no longer a clear signal of virtue (Bénabou and Tirole 2006). The image incentive and the monetary incentive cancel each other out. It may even result in less pro-social effort if individuals are concerned about signaling greed by engaging in a virtuous act for the “wrong” (i.e., not pro-social) reasons. Experimental studies support this theory, demonstrating that contributions toward a public good decline when scrutiny and monetary incentives are combined (Ariely et al. 2009).

**Boomerang Effect**

The boomerang effect refers to behavior unintended by a particular intervention (Schultz et al. 2007; Alcott 2011). With regards to social comparison, boomerang effects (also
called rebound effect – Fischer 2008; Ferraro and Price 2013, or relaxing of efforts – Aitken et al. 1994) occur when above average contributors, presented with the social norm, reduce their contributions. In effect, individuals who discover that they outperform the social norm reduce their effort to converge on the norm from above (Schultz et al. 2007). Empirical support for this behavior is mixed: Aitken et al. (1994), Schultz et al. (2007), and Fischer (2008) find evidence for the boomerang effect. Ferraro and Price (2013) and Delmas and Lessem (2014), however, do not. Theory, too, suggests reasons why we may or may not observe a boomerang effect: Miller and Prentice (2016) argue that in social dilemmas individuals are concerned about others free-riding (see also Ostrom 2003). Hence, high contributors are likely to reduce their contributions in response to learning that they are contributing more than others. Evidence for lab experiments provides support for this argument: a significant proportion of individuals has been classified as conditional cooperators (Fischbacher et al. 2001, Fischbacher and Gächter 2010). These individuals make their contribution dependent on those made by others. By contrast, on the basis of image motivation (see discussion above), Bénabou and Tirole (2006) argue that convergence toward the norm is more likely in fads and fashions (Bernheim 1994) where social approval depends on conformance. In social dilemmas, however, social standing depends on being seen as altruistic; thus, individuals would seek to outperform the social norm in certain contexts. Experimental data supports this view as well, with, for example, Andreoni and Petrie (2004) finding that individuals increase their contributions toward a public good when these contributions can be traced back to them. Alternatively, Schultz et al. (2007) argue that the boomerang effect is conditional upon conducting social norms marketing using descriptive norms only.
Again, however, the evidence is mixed. For example, Ferraro and Price (2013) use descriptive norms only but do not detect a boomerang effect.

**POTENTIAL RESEARCH AREA: Limits to the Boomerang Effect**

Although there are a number of theories that discuss the boomerang effect, there are few papers that systematically compare and contrast the conditions in the studies that detect or fail to detect the boomerang effect. In addition, such a research agenda might benefit from drawing on the numerous common pool resource and public goods experiments conducted (for overviews, see Ostrom et al. 1994; Ledyard 1995; Zelmer 2003; Chaudhuri 2011) that test conditions under which individuals contribute in social dilemmas. Finally, experiments to test for the limits of the boomerang effect might prove useful to providing conditions under which social comparison interventions would have to be designed to avoid rebound behavior by high contributors.

Although we are not yet able to predict when the boomerang effect might occur, there are various design precautions policy makers might take that have been shown to be effective in improving the likelihood of successful social comparison interventions. Three of these are as follows:

**POLICY IMPLICATION: Combine Congruent Descriptive and Injunctive Norms**

In their study applying personalized feedback to curb household energy consumption, Schultz et al. (2007) find that above average households (i.e., those that are particularly energy efficient) do not converge down to the norm (i.e., increase their electricity usage)
when they receive a measure of social approval (a smiley face) alongside information on their energy consumption relative to average energy consumption. This may be due to individuals being reminded of what the “right” behavior might be. This might make the norm more salient (Cialdini et al. 1990, Krupka and Weber 2009), trigger pride in pro-social action (Miller and Prentice 2016), or signal which behavior results in increased social standing (Bénabou and Tirole 2006), which in turn motivates individuals to maintain their pro-social efforts. Using both descriptive and injunctive norms is consequently used in several utility consumption studies (see, for example, Alcott 2011\textsuperscript{20}; and Loock et al. 2012).

In practice, descriptive and injunctive norms cannot always be clearly separated. For example, observing (or receiving information on) predominantly homogenous behavior may signal to individuals that deviant behavior may be condemned and may incur some form of social backlash (Blanton et al. 2008).\textsuperscript{21} Perhaps more importantly, though, studies suggest that it is critical to ensure norm congruence (Smith et al. 2012) in order to achieve the desired behavioral impact. Misalignment of descriptive and injunctive norms will reduce the likelihood of individuals engaging in pro-social behavior (ibid). Consider, for example, if individuals interact with peers who engage in risky or anti-social behavior, such as excessive alcohol consumption or theft. In this case, the observed local descriptive norm is at odds with the injunctive norm in society at large (i.e., society

\textsuperscript{20} It is important to note, however, that Alcott (2011) does not detect an impact from different types of injunctive messages, suggesting that not the injunctive norms (emoticons in this case) but some other trigger may have reduced the potential boomerang effect. (All treatments included injunctive norms, so it is not possible to determine whether the boomerang effect would have occurred otherwise.)

\textsuperscript{21} In fact, receiving information on energy efficient household in Alcott (2011) and Ayres et al. (2013) may convey social approval of those particular households and their energy conservation behavior – descriptive and injunctive norms are combined. This may explain the lack of impact of the emoticons (Alcott 2011 – see footnote above).
generally disapproves of these actions, and, in the latter case, punishes such behavior). Receiving information on society’s disapproval is less likely to lead greater pro-social behavior, be it through mixed messaging from norms or peer pressure, than when the local norm was in alignment with the injunctive norm. Under these circumstances, social comparison interventions may not be sufficient to change behavior (Blanton et al. 2008).

*POLICY IMPLICATION: Provide Favorable Norms*

If individuals tend to converge on the norm irrespective of whether they perform above or below the norm, then, provided the true norm is not public knowledge, presenting individuals with an artificially pro-social norm will result in overall improvements in the targeted behavior. Consequently, in a number of studies, the social norms that are provided to subjects are either artificial (e.g., Goldstein et al. 2008), or artificially inflated (e.g., Aitken et al. 1994); alternatively, convey information about a selective pro-social subgroup (e.g., Jones and McKee 2004; Shang and Croson 2009, Alcott 2011).

The artificial norm may be utilized when information is difficult to attain (e.g., information about illegal behavior such as removing petrified wood from Arizona’s Petrified Forest National Park– Cialdini et al. 2006). Further, they allow the policymaker to set a target level of his or her choosing. However, this approach may be considered deceitful (Croson and Treich 2014). If the deceit is revealed, the impact of the social

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22 Chapter 3 is related to this area: It explores the impact of local descriptive norms on a group’s ability to provide a threshold public good.
norms marketing may be attenuated, leaving the policy tool less effective. A similar argument can be made for artificially inflated norms.

Conveying information about a selective subgroup may achieve the same result as artificial norms – providing individuals with a higher social norm to follow – but can be considered less deceitful. For example, Shang and Croson (2009) present individuals with average donations made by the highest contributors in a fund raising campaign, and the OPOWER studies (Allcott 2011; Ayres et al. 2013) provide individuals with energy consumption information of the most efficient households. In both cases, the social norm presented is of the upper quartile (i.e., the more pro-social subgroup), but this is clearly stated. An alternative, especially when information is difficult to attain, may be to allow for voluntary provision of behavioral information. If high contributing individuals are more likely than low contributors to reveal their contributions (because they wish to receive social acknowledgement or incite reciprocal behavior), then the presented norm will be raised, which, in turn, may reduce the boomerang effect (see Kreitmair 2015 – i.e., Chapter 3).

**Policy Implication: Refocus Attention to Identity Motivations**

Identity (i.e., how individuals might think of themselves) can have a profound impact on motivations and behavior (Akerlof and Kranton 2000). Individuals who consider themselves pro-social are more likely to be high contributors and remain high contributors even (or especially) after receiving information on the social norm. Delmas and Lessem (2014) show that individuals who self-identify as pro-environmental are less
likely to increase their energy consumption after knowing that they are outperforming others. However, when making frequent small decisions (such as whether to run the A/C at any given time), individuals may lose sight of this identity. Similar to the discussion about the saliency of social norms (Cialdini et al. 1990 – also see above), one may refocus an individual’s attention on their identity to make it a salient factor in the decision-making process. Aitken et al. (1994) provide households with reminders of their self-ascribed pro-environmental identity with their water usage information. These reminders led to reductions in the boomerang effect.

Naturally, this approach will work only with individuals who do identify as pro-social. This means that identity must be ascertained through surveys and these reminders must be provided through personalized feedback measures. Consequently, this approach may be more effective in smaller scale social comparison interventions. Further, increasing identity saliency must be done carefully. Triggering guilt instead may be problematic given that it is more likely to motivate pro-social action only when these actions can be observed and may, in turn, trigger negative spillovers (i.e., individuals will compensate for the cost of a pro-social action by engaging in less pro-social behavior in a related field) (Truelove et al. 2014).

In addition to further exploring the limits of the boomerang effect, there are other avenues of potentially fruitful research. I expand on two of these below.
With regards to the boomerang effect, it may be prudent to distinguish between dichotomous and continuous actions. Dichotomous actions are binary decisions such as whether to litter at any given time (Cialdini et al. 1990), whether to invest in a particular retirement fund (Beshears et al. 2015), etc. Continuous actions refer to actions that can be done to varying degrees. These include household electricity and water consumption (see, for example, Alcott 2011; and Ferraro and Price 2013).

In binary actions, converging on the norm means that more individuals will engage in the action presented as the social norm (e.g., X% of individuals reuse hotel towels). We should, thus, see an overall improvement in the incidence of pro-social actions. With continuous actions, however, convergence toward the norm (e.g., similar households consume X kWh of electricity) means that high energy users (for example) will reduce their consumption but low energy users will increase their consumption – the aforementioned boomerang effect. Thus, there may be no difference in overall household electricity consumption, but the distribution changes. However, there are, to the knowledge of the author, no studies that compare the relative effectiveness of these different types of social norms in promoting pro-social behavior.
POTENTIAL RESEARCH AREA: Distribution of Behavior

The distribution of behavior before the social comparison intervention may be important in a number of ways. Consider the following\textsuperscript{23}: If it is a normal distribution, then there will be no significant improvement if there is symmetric convergence toward the norm from above and below. In other words, the average action remains the same, but the distribution is now leptokurtic (the distribution has a higher peak and smaller tails). If the original distribution of is negatively (or left) skewed – the mean is below the median and the mode (i.e., there are more large-outliers toward the left) – social comparison intervention may result in \textit{negative} overall impact. This is because if individuals are too far from the social norm they are less likely to increase their efforts (Croson and Shang 2013; Beshears 2015; see discussion above). Conversely, if the distribution is positively (right) skewed, then social comparison may lead to a positive overall effect.

POTENTIAL POLICY IMPLICATION: Distribution of Original Behavior

The distribution of original behavior matters when designing the social comparison intervention. When the distribution is unfavorable (i.e., left skewed), using the personalized normative feedback design may be particularly beneficial. Personalized feedback, in combination with information from different subsamples of the population (see discussion above), would allow for the setting of different comparisons for the population left and right the curve. Thus, negative outliers might still be motivated to change their behavior leading to overall improvements in behavior.

\textsuperscript{23} Given no systematic differences in misperception of the social norm by above or below average actors.
DISCUSSION: SKETCHING A PATH FORWARD

Although the current paper was able to identify a number of findings that might provide actionable policy advice, let me highlight two areas that might be of particular interest going forward: personalized feedback and the persistence of social comparison effects.

Personalized Feedback

A common factor to several of the policy implications offered above, is the importance of personalized feedback. With having to navigate such failings of social comparison interventions as indifference to a social norm if one’s own behavior is too far from the norm, and the boomerang effect it seems unavoidable that social comparison interventions must be carefully targeted in order to be effective. Further, how an individual perceives a problem may change his or her motivations for action. In addition, individuals might face very different local norms compared to social norms that prevail in society at large. This suggests that policy makers may need to consider personalized feedback over general social norms marketing. Studies on household utilities consumption already tend to favor personalized feedback (e.g., Alcott 2011, Schultz et al. 2014). However, questions remain about how personalized feedback should be structured. For example, on which dimension should such feedback be personalized? Should it change reference groups based on geographic or socio-economic factors (Loock et al. 2012)? Should it (and how would it) consider problem perception? With the rise of data mining, researchers and policymakers have more and more information available to carefully craft and target feedback problems. More research is necessary to improve our ability to sway behavior using personalized feedback.
Long Term Effects of Social Comparison Interventions

A crucial aspect that remains to be examined is the persistence of the induced behavioral change. Although there has been a rapid growth in the analysis of social comparison as a policy tool, few studies have explored the long-term impact of these interventions (Ferraro et al. 2011). This research area is critical for the increased use and feasibility of the social comparison policy tool. It is necessary to determine whether one-time interventions are effective or whether they must be implemented on a regular basis. This, in turn, impacts their cost effectiveness. If interventions are repeated, it is not clear whether they might lose their effectiveness over time. Nevertheless, there is a relative dearth of long-term studies. This lack may be attributed to the cost and administrative challenge of implementing follow-up studies, in particular, in non-automated (i.e., where individuals do not receive computer generated messages) settings. Given improvements in technology, however, there is reason to be optimistic about the possibility for research in this area.

Those studies that have explored persistence of social comparison effects, show promising results. Alcott (2011) finds the impact of social comparison lessens over time but can be retriggered with every new application. Regardless of attenuation, there remains a difference between the treatment and control group: After several months, households that received the social comparison message still used less energy than untreated households (Ayres et al. 2013). Ferraro et al. (2011) suggest that the persistence of these effects (in their case, with regards to household water consumption) may be the
result of investments in more efficient durable technologies in addition to short term behavioral change. Hence, social norms approaches may be particularly useful in situations where they can trigger investments that lock in positive outcomes (ibid). Thus, they may be more effective with regards to household utility consumption than, for example, hotel towel usage. However, if the initial intervention allows for habit formation, the social comparison intervention may still have long-lasting effects. Aside from this tentative policy advice, however, much remains to be explored with regards to the conditions under which social comparison interventions lead to long-lasting behavioral change.

Cross-Disciplinary Synthesis

The study of peer effects on, and the use of social comparison interventions to encourage, pro-social behavior is a burgeoning field (Sacerdote 2014). However, Sacerdote (2014) argues that “we do not yet know enough about the nature of peer effects to engage in social engineering of peer groups to affect students’ outcomes in a desired direction” (p.269). Similarly, we do not yet fully understand the impact of social norms marketing to curb risky behavior (e.g., Wechsler et al. 2003; Blanton et al. 2008) or encourage pro-social behavior (e.g., Abrahamse and Steg 2013; Miller and Prentice 2016). This highlights the need for synthesis and review articles, that, supplementing one another, draw on different areas of the extensive and growing literature on social comparison. It would be beneficial to identify the areas still left underexplored, in order to allow for targeted research studies to most effectively move this research area forward. This would
result in more actionable advice for policy makers seeking to use non-pecuniary policy tools.

In particular, there is much to be gained from synthesis across disciplines. Disciplines tend to study different policy areas (e.g., political scientists are more concerned with peer effects in voter turnout than, for example, education outcomes). In addition, disciplines tend to utilize different approaches and methodologies (e.g., economists tend to avoid deceptive methods in experiments – Smith 1976; Dickson 2011). By contrasting behavior and the effectiveness of social comparison interventions across these different policy areas and approaches, we can leverage these differences to identify when social comparison may be more or less effective. The current paper has attempted to begin this process. Given the breadth of the field, however, a systematic cross-disciplinary synthesis will be an ongoing endeavor.
REFERENCES


CHAPTER II: Voluntary Disclosure of Contributions: An Experimental Study on Non-Mandatory Approaches for Improving Public Good Provision

BRIEF OVERVIEW

As discussed in Chapter I, there has been an increasing interest in social comparison as a policy tool to encourage pro-social behavior. Often studies of this measure have so far relied on the assumption of the availability of social information. In situations where information is costly to collect and disseminate, alternative mechanisms must be considered. This study explores the use of voluntary disclosure to provide social information in a linear public goods game in a lab experiment.

To explore voluntary disclosure in a social dilemma (here operationalized as a linear public goods experiment), I vary the information about other participants’ contribution behavior available to participants in five treatments:

1. **No Disclosure treatment** – individuals receive only group-level contribution information each round
2. **Mandatory treatment** – individuals receive information on their group members’ individual contributions
3. **Voluntary Simultaneous to Contribution treatment** – every round, individuals may choose to reveal their contribution to the group
4. **Voluntary Prior to Contribution treatment** – every round, individuals choose whether to reveal their contributions. Before making their contribution decision, participants receive a message informing them how many group members have chosen to reveal their contributions.
5. **Vote Prior to Contribution treatment** – every round, individuals vote on whether everyone is required to reveal their contributions. Before making their contribution decision, participants receive a message informing them whether the majority voted in favor of disclosure, thus informing them whether their contribution decision

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24 This chapter has been published (with minor editing and formatting changes) as:

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is public or private. (Results not reported in paper, but analysis was included in Appendix 2.1.)

These treatments are used to examine the following questions:

i) With what frequency do individuals choose, when given the chance, to reveal to their group members their contributions to a public good?

ii) Does the opportunity to voluntarily reveal contributions improve cooperation in comparison to scenarios with only group level information and with mandated disclosure of contributions?

iii) To what extent is this effect driven by “moral” motivations (in terms of the behavioral model proposed by Levitt and List, 2007) as opposed to the ability to encourage reciprocal behavior from group members?

There are several main findings of the experiment:

1. Given the opportunity, individuals choose to reveal their contributions more often than not. Those individuals who do reveal their transfers to the Group Fund contribute significantly more to the group than individuals who keep their contributions hidden. Thus, the visible contribution average is higher than the actual contribution average. This difference could potentially be used strategically to counteract the Boomerang effect (i.e., individuals reducing their conservation effort/contributions/pro-social behavior in response to observing mean behavior that is less pro-social than their own – Schultz et al., 2007).

2. Greater information leads to higher cooperation. This effect is particularly pronounced when individuals are able to voluntarily reveal contributions (as compared to having this disclosure mandated). This means that, in the two voluntary conditions, as more individuals make their contributions public, group contributions increase.

3. The most important factor in improving cooperation in these experiments seems to be signal quality, or the inclusion of a message informing individuals of how many other group members volunteered to reveal their contributions. This form of signaling led to the largest contributions to the public good.
INTRODUCTION

A growing literature explores the use of non-pecuniary measures to encourage welfare-enhancing behavior (see, for example, Thaler and Sunstein 2008 or Johnson et al. 2012). One strand of this literature explores the impact of “descriptive norms” (Cialdini et al. 1990), or “social information” (Croson and Shang 2008, and Shang and Croson 2009), on pro-social behavior (McDonald and Crandall 2015). In other words, they examine the impact on behavior of individuals being provided with information on how others behave in similar circumstances. These descriptive norms have been used to study and promote pro-social behavior in a number of different environmental applications such as increased curb-side recycling, reuse of hotel towels, natural resource conservation, household energy conservation, and household water conservation (see Schultz 1999, Goldstein et al. 2008, Cialdini et al. 2006, Allcott 2011, and Ferraro and Price 2013, respectively). This literature indicates that descriptive norms can be an effective means of encouraging pro-social behavior although this is dependent on the characteristics of the target population (Bao and Ho 2015).

Given that large-scale policy application of these “informational nudges” is a recent phenomenon, much remains to be researched. One such area that has yet to receive much attention is the voluntary disclosure of behavioral information. Often the information necessary for providing descriptive norms is not publicly available or must be compiled at high cost. For instance, consider the difficulty in assessing the precedence and distribution of illegal behavior, such as the theft of petrified wood from Arizona’s Petrified Forest National Park (Cialdini et al. 2006). In these scenarios providing social
information might be difficult without voluntary disclosure of behavior. However, the impact of voluntarily provided behavioral information on pro-social behavior has not been studied systematically. This study seeks to address this gap by using a laboratory experiment to explore the voluntary disclosure of social information in a linear public goods game where participants decide, as a direct measure of prosocial behavior, how many tokens to contribute toward a group fund. In particular, the experiment utilizes four different information treatments: i) “no disclosure” – where individuals receive only aggregate information group contributions, ii) “mandatory disclosure” – where individuals receive information on every group member’s contributions, iii) “voluntary simultaneous to contribution” – where individuals may voluntarily disclose their contribution information, and iv) “voluntary prior to contribution” – where individuals are notified about how many individuals have chosen to reveal their contributions before making their contribution decision. These treatments are used to examine the following questions: i) With what frequency do individuals choose, when given the chance, to reveal to their group members their contributions to a public good? ii) Does the opportunity to voluntarily reveal contributions improve cooperation in comparison to scenarios with only group level information and with mandated disclosure of contributions? iii) To what extent is this effect driven by moral motivations as opposed to the ability to encourage reciprocal behavior from group members?

The paper proceeds as follows. The first section introduces linear public goods games, briefly reviews related experimental work, and discusses behavioral implications of social information. The second section describes the experimental design and derives
testable hypotheses. Thereafter, experimental results are provided. Finally, the paper concludes with a discussion of the results—in particular, highlighting the importance of signaling as a means of inducing cooperation in voluntary contribution settings and the broader policy relevance of the study.

**SOCIAL INFORMATION**

The study focuses on the impact of social information on behavior in social dilemmas. Social dilemmas have been studied extensively in laboratory and field experiments. One way to model these dilemmas is using a linear public goods game. Consider the following (notation based on Isaac et al. 1994): In a group of size $N$ each individual receives an endowment of $Z$. They choose to contribute any amount, $m$, to a group fund (the public good). The payoffs from this group fund depend on the contributions by all group members (i.e., $\sum m$), and the payoff to each individual from the group fund is an equal share of total contributions multiplied by a rate of return multiplier $G$. If $m < Z$, the remainder ($Z - m$) is transferred to the individual’s private fund with a rate of return of $p$. Therefore, an individual’s payoff function is:

$$\pi_i = p(Z_i - m_i) + \frac{G}{N(m_i + \sum_{j \neq i} m_j)}$$  \hspace{1cm} (Equation 2.1)

This payoff function reflects a social dilemma setting when $p > G/N$ and $G > p$, which implies that it is rational, but not socially optimal, for individuals to transfer their endowment to the private fund. A concept that is often used to capture the relationship between $p$ and $G/N$ is the marginal per capita return (henceforth MPCR) on investment to
the public good (Isaac et al. 1984). To capture incentives surrounding social dilemmas, the MPCR (which is $G/pN$) is less than one. Accordingly, the Nash equilibrium prediction is zero contributions to the public good.

Although the experimental literature on public goods games is extensive (see Ledyard 1995, Zelmer 2003, and Chaudhuri 2011 for reviews of broad experimental findings), few lab experiments explicitly test the impact of social information – operationalized here as individual-level contribution information. Sell and Wilson (1991), Weimann (1994), Wilson and Sell (1997), and Croson (2001) contrast treatments with information on aggregate group contributions with full social information at the individual level (meaning that after each decision round subjects are informed about how much each individual contributed). The results are mixed. Sell and Wilson (1991) found that contributions to the public good increase significantly; Weimann (1994) and Croson (2001) found no significant difference; and Wilson and Sell (1997) found that individual-level social information reduces contributions. This divergence may, in part, be related to Weimann including information on earnings as well as information on contributions, which may trigger less cooperative tendencies in participants (Bigoni and Suetens 2012). In line with Bigoni and Suetens, other lab experiments have highlighted the importance of selective presentation of social information. Clark (2002), for instance, provides subjects with information on the highest contribution each round, and Jones and McKee (2004) explore whether relative information (i.e., how the subject ranks with respect to contributions in the group) improves cooperation. Croson and Shang (2008 and 2013) and Shang and Croson (2009) explore these effects in the field. Broadly, these studies
find that presenting subjects with information on the highest contribution increases contribution rates if the difference between the subject’s contribution and highest contribution is not too large. Likewise, information on the lowest contribution depresses contributions, suggesting convergence behavior toward the observed contribution level. This is the “social comparison effect.”

Field studies corroborate findings by Croson and Shang: In field experiments where the underlying game structure may be described as an environmental social dilemma (see, for example, Schultz 1999, Cialdini et al. 2006, Goldstein et al. 2008, Allcott 2011, Ayres et al. 2013, and Ferraro and Price 2013), individuals are swayed by social information. For instance, in the study by Ferraro and Price (2013), households that were presented with information on mean household water consumption—as well as technical advice on how to conserve water—reduced their water usage by more than households without this social comparison. In studies on household electricity use, Schultz et al. (2007) and Fischer (2008) corroborate this, but also found that behavior converges toward the mean. In other words, social comparison affects individuals differently, i.e., high consumers of electricity reduce their usage, but low electricity users increase their consumption. This increase in consumption, also called the “boomerang effect” (Schultz et al. 2007), can diminish the positive influence of social information depending on the composition of the group (Ostrom 2003, Bao and Ho 2015) and the information that is displayed. To avoid the boomerang effect, one might provide individuals with praise for pro-social behavior that exceeds the displayed social information (Schultz et al. 2007).
An alternative method to attenuate the boomerang effect is to increase the availability of information about relatively high contributions (Shang and Croson 2009) by allowing individuals to volunteer their contribution information. Voluntary disclosure of contribution information will result in the visible contribution average being higher than the actual mean of contributions if i) individuals choose to make their contributions public, and ii) public contributions are higher than hidden contributions. To see why this might be the case, consider the following behavioral model: Levitt and List (2007) include a moral component to explain behavior in differing contexts. In this model, utility is derived from pecuniary wealth ($W$) and moral behavior ($M$). These in turn are functions of the action taken ($a$) the financial externality imposed on others ($v$), the strength of social norms disproving of said action ($n$), and the scrutiny under which the action is placed ($s$). As $v$ increases, financial gain increases, given that more of the cost of engaging in $a$ is externalized. However, $M$ is negatively related to $v$, meaning that with greater financial externalities come greater guilt in engaging in $a$. This alone may not be sufficient to sway behavior toward abstaining from engaging in $a$ to the extent which would be optimal to maximize $W$; however, as social norms and scrutiny of one’s actions increase, individuals may alter their behavior.

$$U_i(a, v, n, s) = M_i(a, v, n, s) + W_i(a, v) \quad \text{(Equation 2.2)}$$

For this study, $a$ constitutes contributions toward the public good. On the basis of this preference structure and $M$ being positively correlated with $a$, the following conjectures arise:
**C1. Individuals will voluntarily disclose contributions with significant frequency.**

If individuals can choose to reveal their contributions, s becomes endogenously determined. The decision to disclose contributions is therefore a decision to increase s, which increases the impact moral considerations have on behavior. Disclosure is therefore a costly action that can be interpreted as a credible signal of the willingness to cooperate. This might then be used as a means of avoiding the assurance problem (Runge 1984, Isaac et al. 1989), where individuals wish to contribute only if others do so as well. The experimental literature on endogenous institutional choice corroborates this conjecture by finding that subjects often self-select into various governance institutions that restrict viable contribution choices, which ultimately leads to greater contributions overall (see, for example, Botelho et al. 2005, Kroll et al. 2007, Kosfeld et al. 2009, Sutter et al. 2010, and Hamman et al. 2011).

**C2. Voluntarily revealed contributions will be higher than hidden contributions.**

Given a higher value of scrutiny, s, when contributions are public, the relative impact of M increases, leading to individuals choosing to contribute greater amounts. However, individuals need not derive utility from moral actions in order to engage in them. If a self-regarding, and financially motivated, actor suspects that others might be motivated by contribution norms, improving this social norm by increasing contributions may be profitable and hence rational (Kreps et al. 1982). This suggests that: i) contributions in the mandatory treatment, and disclosed contributions in the voluntary treatments, will be higher than
contributions in the no disclosure treatment; ii) public contributions in the voluntary treatments will be higher than hidden contributions in the voluntary treatments; and iii) average public contributions in the voluntary treatments will be higher than average contributions in the mandatory treatment, because individuals in the voluntary treatments who are not sufficiently motivated by increased scrutiny, but believe that group members might respond negatively to free-riding by others, can self-select into hiding their low contribution.

C3. Voluntary disclosure leads to greater contributions at the group level.

If C1 and C2 occur at sufficiently high levels, then, given the evidence that individuals converge on the mean (see discussion above), voluntary disclosure will lead to greater contributions than in both the no disclosure and mandatory disclosure treatments.

C4. Greater social information (i.e., information on more individuals’ contributions) leads to more contributions at the group level.

If C1 holds, then, as more individuals face a high value of \(s\), group contributions increase. Thus, contributions in the no disclosure treatment will be lower than in all other treatments, given non-zero disclosure in the voluntary treatments.

C5. Increased ability to signal leads to higher group contributions.

This relates directly to the extent to which low contributions are related to the assurance problem, as opposed to free-riding preferences (i.e., high relative
weight of $W$ as compared to $M$). If the assurance problem motivates behavior, then individuals knowing, before making their contributions, how many others have chosen to make contributions public (see C1 and C2) will lead to increased contributions. In this case, contributions in the voluntary prior to contribution treatment will be higher than contributions in the voluntary simultaneous to contribution treatment, in which signaling may occur but must be done over two periods, and is hence less likely to be effective. Therefore, the difference in contributions between these two treatments measures the extent to which contributions are driven by the ability to encourage reciprocal behavior from group members as opposed to the ability to self-select into a disclosure institution.

Although voluntary disclosure may arguably lead to greater cooperation, there is very little, if any, research directly exploring this effect in linear public goods games. Andreoni and Petrie (2004) explore the impact of identifiability in connection with behavioral information. In their optional-reporting treatment (a supplement to treatments that present participants with photos of their group members), subjects may divide their contributions between two different public goods, one of which makes their contributions public. Two findings are of particular interest to this study: i) if subjects contribute to the public good, they do so via the visible public good—which can be seen as evidence for C1; and ii) optional reporting increases contributions—which may be interpreted as support for C3. In contrast, this study differs from Andreoni and Petrie in maintaining a single public good to separate any effects that may derive from having multiple public goods (Corazzini et al. 2013). In addition, there are no opportunities to identify group
members so as to mitigate any confounding effects between voluntary disclosure and identification. Finally, this study disentangles the effects of self-selection behavior from that of signaling.

**Experimental Design and Hypotheses**

Experimental Design

All experimental treatments in this paper utilize the standard VCM design as put forward by Isaac et al. (1984) and described, briefly, above. Parameters have been modified to parallel the experimental studies on information and to ease subsequent comparison of results. The experiment consists of two stages that are each ten rounds long. Stage 1 is the baseline public goods game (which is the same as the “no information” treatment), whilst Stage 2 varies across treatments. There are four treatments, each described below. A fifth treatment, where a vote took place in order to determine whether or not disclosure would be mandated, was also run. As this treatment is not central to the present analysis, details and results are not reported here but in Appendix 2.1. The two-stage design was implemented to detect and account for any group effects. Participants in all treatments were informed about the number of rounds in the experiment.25

Participants were randomly assigned into groups of five (i.e., $N = 5$). Within these groups, each subject received a subject number (1-5). Participants remained in these groups and retained these subject numbers until the end of the experiment. At the beginning of every round, individuals received 25 tokens, which they were to distribute

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25 Instructions for all five treatments may be found in Appendix 2.2
between an Individual Fund and a Group Fund. For every token placed in the Individual Fund, the individual received 2¢ (i.e., \( p = 2 \)). For every token he/she placed in the Group Fund, every member in the group received 1¢ (i.e., \( G/N = 1 \)) – this implies MPCR = 0.5, which is a commonly used value in literature (see, for example, Weimann 1994, Croson 2001, and Bigoni and Suetens 2012). Once everyone had made a transfer decision, subjects received information, the content of which varied across treatments. Subjects received this information every round and had access to their own contribution information from previous rounds when making their transfer decisions. At the end of the experiment, subjects received the sum of their per-round earnings and a US$5 show-up payment.

**No Disclosure (no_discl)**

All subjects participated in this treatment as the baseline in the first ten rounds. Thereafter, subjects who participated in the no_discl treatment participated in another ten rounds in Stage 2. The defining feature of the treatment was that once everyone had made a transfer decision, subjects received information on their own contribution to the Group Fund, the total contributions to the Group Fund by the group, and their own earnings for the round.

**Mandatory Disclosure (mandatory)**

The mandatory treatment is the same as the baseline information treatment with the exception that at the end of each round participants also received information on the individual transfers made by their group members. This treatment mirrors the full
information treatments used by Sell and Wilson (1991) and Croson (2001) to analyze the impact of social information.

*Voluntary Simultaneous to Contribution (vol_sim)*

The vol_sim treatment is similar to the no_discl treatment but differs in that, at the time of the contribution decision, subjects were required to also decide whether they wished to reveal the amount of their contribution to their group members. At the end of each round, participants received—along with the information received in the no_discl treatment—information on individual transfers by those group members who chose to reveal their contributions.

*Voluntary Prior to Contribution (vol_before)*

The vol_before treatment adds a further step to the vol_sim treatment. Every round, participants decided whether to reveal their contributions to the Group Fund before making their transfer decisions. Once all participants had decided whether to publicize their transfers, all participants received a message stating how many individuals in the group decided to reveal their contributions. Then subjects decided how to distribute their tokens. The information received at the end of the round was identical to that received in the vol_sim treatment.
Hypotheses

The hypotheses mirror the general conjectures outlined in the discussion of the literature. This section provides testable hypotheses and details as to how each is tested. The results section will provide further details on the analytical tools used.

\textit{H1: A majority of individuals in the vol\_sim and vol\_before treatments will choose to disclose their contributions.}

This assertion is explored by summing the instances when individuals choose to reveal contributions across periods in Stage 2. This information is displayed as a percentage of the number of instances and individual could disclose contributions. Logit regression models are used to explore the decision to disclose in these treatments.

\textit{H2: Voluntarily revealed contributions are higher}

\textit{H2a: Voluntarily public contributions are higher than mandated public contributions}

To test this assertion, average contributions in the mandatory treatment are compared to public contributions in the vol\_sim and vol\_before treatments. To account for confounding effects of group composition and contribution history, this conjecture will be tested in a panel regression model accounting for Stage 1 contributions.

\textit{H2b: Voluntarily public contributions are higher than voluntarily hidden contributions}
This analysis will mirror the analysis for H2a, but will contrast public contributions with hidden contributions in the vol_sim and vol_before treatments.

H3: Group contributions in the voluntary treatments are higher than in the no_discl and mandatory treatments.

Stage 2 total contributions to the group fund in the voluntary treatments are contrasted with total contributions in the no_discl and mandatory treatments. Behavior from Stage 1 is used to account for any group effects.

H4: Social information leads to greater contributions

H4a: More social information leads to greater contributions at the group level.

More social information is operationalized through the number of individual contribution decisions that are public to the group. As only the voluntary treatments have variation in social information, data from these is used to assess the role of social information.

H4b: More social information leads to greater contributions at the individual level.

Greater group-level contributions with increased social information may be the result of a combination of effects. The first effect is that public contributions are higher (H2); thus, with more public contributions, average contributions will be higher. In addition, individuals who choose to keep contributions hidden may also increase contributions in response to others revealing their contributions. To
distinguish between these different effects, data from the vol_sim and vol_before treatments is used and further separated into public and private contributions. Individual contribution decisions are then regressed on the number of other individuals choosing to reveal contributions. If these coefficients are positive and significant, there is evidence that higher group contributions are the result of both effects.

**H5: The effect of voluntary disclosure will be more pronounced when signal quality is greater (i.e., when subjects can signal within a period rather than across periods).**

This hypothesis is tested by comparing group-level contributions across the vol_sim and vol_before treatments. The number of individuals disclosing contributions is controlled for to account for group-composition effects.

**Experimental Implementation**

The computerized experiment was programmed and conducted with the software z-Tree (Fischbacher 2007). Subjects received instructions for each stage via their computer terminal. To ensure common information among participants, subjects received handouts and the experimenter reviewed the instructions publicly. To ensure that participants were comfortable with the decision task, everyone was required to answer a short quiz. Subjects could not advance without answering the quiz questions correctly.

The sessions were conducted at Indiana University in the Interdisciplinary Experimental Lab in 2013. Subjects were recruited from the Indiana University undergraduate
population using ORSEE (Online Recruitment System for Economics Experiments). A total of 190 students from various majors participated in the sessions. On average, subjects received $20.53 (including a $5 show-up payment) in experimental sessions that typically lasted about 45 minutes. Table 2.1 shows the distribution of subjects per treatment.

< Table 2.1 >

RESULTS

General Observations
Corroborating prior public goods experiments, all treatments show greater than zero average contributions (see Figure 1 and group-level summary statistics in Table 2.2), and thus contributions go beyond the Nash prediction. In Stage 1, the average contributions rate is 32.24%, which is similar to comparable studies with the same MPCR, such as 33.2% observed by Andreoni (1988). During Stage 2, the average contribution rate increased substantially to 44.38% with large variations across treatments (rates range between 29.27% and 58.16% in Stage 2), as shown in Figure 2.1 and Table 2.2. A common trend is the increase in group-contributions in Stage 2 for all treatments, except the no_discl treatment. Total transfers to the Group Fund actually decreased in that treatment. Using paired t-tests, these changes in contributions across stages prove to be statistically significant (at the 5% and 10% levels) for all treatments, suggesting that the social information treatments improve upon the baseline setting in Stage 1.
Revisiting Hypotheses

H1: A majority of individuals in the vol_sim and vol_before treatments will choose to disclose their contributions.

Given the opportunity to reveal, the majority of individuals chose to do so: 64% of the opportunities to disclose contributions are used to reveal information in the vol_sim treatment; in the vol_before treatment, the figure is 75%. This suggests that voluntary disclosure policies can be effective in creating transparency. The disclosure decision is studied using panel logit regression with data from the vol_sim and vol_before treatments (see Table 2.3). Models 1-4 use the decision to reveal as the dependent variable. Models 1-3 explore the decisions in periods 12-20, while Model 4 examines the first disclosure decisions (in period 11) when no feedback about others’ disclosure decision is available. The models test for differences in behavior across vol_sim and vol_before by including a dummy variable for vol_before. In addition, they include variables for how many others in the group are revealing their contributions and how much others (who have disclosed their behavior) are contributing. Both are lagged to reflect the information available to subjects when they make their own disclosure decision. Further, these lagged variables are interacted with the vol_before dummy to test for differences in behavior in response to this information across the two voluntary treatments. Model 2 also includes an individual’s decision in the prior period, and Model 3 further includes the individual’s contribution to the Group Fund in period 1 (as a proxy for behavioral type). In these three
models, there is no significant difference between how individuals make disclosure decisions between the two treatments. Individuals seem to exhibit reciprocal behavior – they are more likely to reveal their contribution if others have done so in the past. Further, once an individual has made the decision to disclose, she is more likely to continue to do so. Finally, Model 3 indicates that more generous individuals, as measured by period 1 contributions, are also more likely to reveal their contributions. Exploring behavior in period 11, it is evident that the difference in disclosure rates between vol_sim and vol_before arises largely from the initial decision in period 11.

\< Table 2.3 \>

**H2a & H2b:** Voluntarily public contributions are higher than mandated public contributions and voluntarily hidden contributions, respectively.

Figure 2 displays the private contributions, combined contributions, and public contributions across the four treatments as well as standard error bars. There is a dramatic difference between voluntarily hidden contributions and voluntarily revealed contributions, leading to visible average contributions being significantly higher than actual average contributions. In addition, the voluntarily disclosed contributions in both the vol_sim and vol_before are significantly higher than public contributions in the mandatory treatment. To further explore this, examine the random effects model and the pooled Tobit model in Table 2.4. The independent variables are i) a dummy variable for the mandatory treatment, ii) two dummy variables for public contributions, one for the vol_sim treatment and one for the vol_before treatment, and iii) two dummy variables for
hidden contributions in the two voluntary treatments. Both models include a variable to control for group history in Stage 1: total group contributions in Stage 1 averaged across periods. In both models, both the public and private contributions are significantly different from the contributions in the no_discl treatment. Contributions in the mandatory treatment are not significantly higher in Model 5, but given the number of censored contributions, the Tobit model may be the better specification. In this model, mandatory contributions are significantly higher than in the no_discl treatment. Paired significance tests of the coefficients in the Tobit model indicate that all contribution levels are significantly different (at the 1% level) except for the private contributions in vol_sim and vol_before. Therefore, both $H2a$ and $H2b$ are supported.

$H3$: Group contributions in the voluntary treatments are higher than in the no_discl and mandatory treatments.

Returning to Table 2.2, group-level contributions in Stage 2 across treatments are compared (there are no significant differences in Stage 1 contributions across the different treatments). Using Wilcoxon-Mann-Whitney tests, it is found that only the vol_before treatment results in group contributions that are significantly higher than the no_discl treatment (p-value 0.0109). Contributions in this treatment are also significantly higher than in the mandatory treatment (at the 5% level). This implies that the ability to
signal, rather than to voluntarily reveal contributions, improves cooperation. This is further explored below.

**H4a: More social information leads to greater contributions at the group level,**
The regression models in Table 2.5 use data from vol_sim and vol_before. Both a random effects and a pooled Tobit model (Models 7 and 8) are run for contributions at the group level. The main variable of interest here is number_revealed, which ranges from 0 to 5 depending on how many members of a group decided to reveal their contributions. Both models indicate that number_revealed is positive and significant, which suggests that more social information leads to higher contributions at the group level. But, as indicated above, this finding may be the result of two separate effects. Hence, individual contribution decisions must be considered.

< Table 2.5 >

**H4b: More social information leads to greater contributions at the individual level.**
The models in Table 2.6 explore the effect of social information at the individual level. Separate models are run for the two voluntary treatments to account for possible different mechanisms in play due to the timing of social information being transmitted. Both random effects estimations and pooled Tobit models were used. The following variables were used to distinguish between the effects of information content and information amount:
i) public contribution – indicates whether the contribution that was made was voluntarily revealed;

ii) lagged average of visible contributions made by others – a proxy for information content;

iii) lagged average other visible contributions interacted with public contribution, to assess whether individuals utilized this information differently depending on their disclosure decision;

iv) number of others choosing to reveal, i.e., a proxy for the amount of social information (lagged in vol_sim case given that these subjects did not know how many others would reveal their contribution before making their current round contribution decision);

v) number of others revealing interacted with public contributions (also lagged for vol_sim); and

vi) average contributions made in Stage 1 – to account for group effects.

The main finding in these models is that the amount of social information (i.e., how many others disclose contributions) does not significantly affect contributions, but the informational content (i.e., how much others are contributing) does. Further, individuals in the vol_sim treatment, whether they disclosed contributions or not, were swayed by the contributions made by others. Meanwhile, in the vol_before treatment, only individuals who revealed their contributions made their contributions dependent on visible contributions by others.

< Table 2.6 >
Finally, consider Models 13-16 (Table 2.7). These random effects models explore
collection changes in the vol_sim (Models 13 and 14) and vol_before (Models 15 and
16) treatments. They are used to examine convergence behavior toward the mean as is
observed in the field studies discussed above. Models 13 and 15 use data only from
public contributions, and Models 14 and 16 use data from hidden contributions to account
for different mechanisms at play. The following variables are included:

i) difference between last period’s average visible contributions made by
others and the individual’s own contributions (positive if the individual’s
contribution was less than the average);

ii) a dummy variable equal to 1 when an individual contributed more than the
visible average and 0 otherwise; and

iii) an interaction between these two variables.

A positive coefficient on the first variable implies convergence behavior. The second and
third variables explore differences in convergence behavior based on individuals being
high contributors. In all four models, individuals converge towards the mean. This occurs
irrespective of whether the individual decided to disclose contributions. Thus, low
contributors increase contributions and high contributors lower their contributions in
response to social information – the latter being the boomerang effect. However, in the
vol_before treatment, individuals who disclose their contributions seem to exhibit a
weaker boomerang effect, even though their convergence behavior does not change. This
may indicate that improved opportunities to reciprocate behavior may help attenuate the
boomerang effect.
H5: The effect of voluntary disclosure will be more pronounced when signal quality is greater

Finally, once the number of public contributions is accounted for, there is no significant difference in contribution behavior at the group level between vol_sim and vol_before (Table 2.5). Higher contributions in the vol_before treatment (Table 2.2) are thus an artifact of higher disclosure rates (see discussion for H1) given that public contributions are higher than private contributions (Table 2.4). Therefore, higher signal quality leads to greater transparency, which in turn leads to greater contributions.

CONCLUSION AND DISCUSSION

The experiment presented here allows the testing of the impact of voluntary information disclosure in a linear public goods setting. The treatments have been designed to assess the impact of voluntary disclosure of contributions on contribution levels. In addition, the design allows one to distinguish between the impact of voluntary disclosure of contribution decisions (and the scrutiny this entails) and the impact of signaling to other group members one's willingness to cooperate.

There are several main findings of this experiment: First, given the opportunity, individuals choose to reveal their contributions more often than not. Those individuals who do reveal their transfers to the Group Fund contribute significantly more to the group
than individuals who keep their contributions hidden. Second, greater information leads to higher cooperation at the group level. This effect is particularly pronounced when individuals are able to voluntarily reveal contributions (as compared to having this disclosure mandated). This means that, in the two voluntary conditions, as more individuals make their contributions public, group contributions increase. In contrast to Weimann (1994), Wilson and Sell (1997) and Croson (2001), it is found that providing subjects with full information on individual contribution decisions significantly increases contribution levels (although this is dependent on model specification). This may result from the structure of the experiment, given that the design utilized here has all subjects participate in the no_discl treatment in the first ten rounds. However, this design is key in determining whether there are any group effects that may cause one to over- or under-state effects. Finally, the most important factor in improving cooperation in these experiments seems to be signal quality, or the inclusion of a message informing individuals of how many other group members volunteered to reveal their contributions. This form of signaling led to the largest contributions to the public good. This highlights the potential for future research to explore the boundaries of using signaling to improve cooperation in social dilemmas.

Although experimental results, in particular lab experimental results (Levitt and List 2007), by themselves are not a sufficient basis to design or justify policy measures, they may be used to highlight behavioral tendencies and focus additional policy-relevant research. In light of this, the above results have several implications for policy: The level at which information was provided in the endogenous disclosure treatments indicates that
voluntary disclosure policies can result in transparency without requiring external enforcement measures. Hence, where enforcement is prohibitively costly, a voluntary disclosure policy may be more efficient. Furthermore, contributions that are voluntarily kept private are more likely to be lower than those that are made public. As a result, if actors have the ability to mask free-riding behavior, reciprocators are less likely to respond with uncooperative behavior because the observable average contribution rate is higher than when free riders are mandated to disclose their contributions. This may indicate that voluntary disclosure measures may be particularly effective in environments with numerous free riders. In other words, free riding is more deleterious to cooperation when others can observe it. On this basis, it is clear that voluntary disclosure policies may be useful tools, and further research is necessary regarding the conditions under which these may be more or less effective.
REFERENCES


### Table 2.1: Overview of Treatments

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Stage 1</th>
<th>Stage 2</th>
<th>Number of groups</th>
<th>Number of subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td>No_Discl</td>
<td>No information</td>
<td>No_Discl</td>
<td>11</td>
<td>55</td>
</tr>
<tr>
<td>Mandatory</td>
<td>No information</td>
<td>Mandatory</td>
<td>11</td>
<td>55</td>
</tr>
<tr>
<td>Vol_Sim</td>
<td>No information</td>
<td>Vol_Sim</td>
<td>7</td>
<td>35</td>
</tr>
<tr>
<td>Vol_Before</td>
<td>No information</td>
<td>Vol_Before</td>
<td>9</td>
<td>45</td>
</tr>
</tbody>
</table>

### Table 2.2: Summary Statistics: Group Level Contributions by Stage and Treatment

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Stage 1</th>
<th>Stage 2</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td># of tokens (standard deviation)</td>
<td># of tokens (standard deviation)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>% of max</td>
<td>% of max</td>
<td></td>
</tr>
<tr>
<td>No_Discl</td>
<td>396.18 (227.84)</td>
<td>365.82 (203.14)</td>
<td>-2.42%</td>
</tr>
<tr>
<td>Mandatory</td>
<td>327.36 (117.85)</td>
<td>474.27 (327.01)</td>
<td>11.75%*</td>
</tr>
<tr>
<td>Vol_Sim</td>
<td>376.29 (151.09)</td>
<td>512.29 (275.21)</td>
<td>10.88%**</td>
</tr>
<tr>
<td>Vol_Before</td>
<td>451.89 (156.97)</td>
<td>727 (292.25)</td>
<td>22.01%***</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

**Note:** N refers to the number of groups. The percentages refer to group-level contributions to the Group Fund compared to the maximum possible contributions (1250 over ten rounds).
Table 2.3: Panel Logit Regression on the Decision to Reveal Contributions

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>0.233</td>
<td>-0.319</td>
<td>-2.190*</td>
<td>-1.06***</td>
</tr>
<tr>
<td></td>
<td>(0.853)</td>
<td>(0.793)</td>
<td>(0.068)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Vol_Before dummy</td>
<td>1.381</td>
<td>0.906</td>
<td>0.607</td>
<td>1.25***</td>
</tr>
<tr>
<td></td>
<td>(0.416)</td>
<td>(0.575)</td>
<td>(0.689)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Lagged other reveal</td>
<td>0.524*</td>
<td>0.513*</td>
<td>0.539*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.081)</td>
<td>(0.079)</td>
<td>(0.062)</td>
<td></td>
</tr>
<tr>
<td>Lagged other reveal x vol_before</td>
<td>-0.0963</td>
<td>-0.0384</td>
<td>-0.0262</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.803)</td>
<td>(0.919)</td>
<td>(0.944)</td>
<td></td>
</tr>
<tr>
<td>Lagged average other contributions</td>
<td>0.0452</td>
<td>0.0400</td>
<td>0.0397</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.310)</td>
<td>(0.345)</td>
<td>(0.325)</td>
<td></td>
</tr>
<tr>
<td>Lagged average other contributions x vol_before</td>
<td>-0.0132</td>
<td>-0.00532</td>
<td>-0.00820</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.824)</td>
<td>(0.926)</td>
<td>(0.882)</td>
<td></td>
</tr>
<tr>
<td>Prior period’s decision</td>
<td>0.676*</td>
<td>0.648*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.073)</td>
<td>(0.082)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Contribution in period 1</td>
<td></td>
<td></td>
<td>0.191***</td>
<td>0.21***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
</tbody>
</table>

| Periods included:        | 12-20   | 12-20   | 12-20   | 11      |
| Observations             | 715     | 715     | 715     | 80      |

* p<.1, ** p<.05, *** p<.01  
P-values in parantheses.  
Dummies for periods were included in the regressions but omitted in table.
Table 2.4: Regression Analyses of Individual Level Contributions Given Public and Private Contributions

<table>
<thead>
<tr>
<th></th>
<th>(5) RE</th>
<th>(6) Tobit</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Constant</strong></td>
<td>2.03 (0.421)</td>
<td>-3.46 (0.201)</td>
</tr>
<tr>
<td><strong>Mandatory</strong></td>
<td>3.12 (0.156)</td>
<td>5.701** (0.014)</td>
</tr>
<tr>
<td><strong>Public (Vol_Sim)</strong></td>
<td>6.87*** (0.000)</td>
<td>11.82*** (0.000)</td>
</tr>
<tr>
<td><strong>Private (Vol_Sim)</strong></td>
<td>-3.40*** (0.007)</td>
<td>-10.26*** (0.001)</td>
</tr>
<tr>
<td><strong>Public (Vol_Before)</strong></td>
<td>9.40*** (0.000)</td>
<td>18.25*** (0.000)</td>
</tr>
<tr>
<td><strong>Private (Vol_Before)</strong></td>
<td>-2.32 (0.119)</td>
<td>-13.04*** (0.000)</td>
</tr>
<tr>
<td><strong>Average Group Contributions in Stage 1 (averaged over 10 periods)</strong></td>
<td>0.14*** (0.001)</td>
<td>0.27*** (0.000)</td>
</tr>
<tr>
<td><strong>Periods included:</strong></td>
<td>11-20</td>
<td>11-20</td>
</tr>
<tr>
<td><strong>R-sq:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Within</strong></td>
<td>0.2543</td>
<td></td>
</tr>
<tr>
<td><strong>Between</strong></td>
<td>0.4468</td>
<td></td>
</tr>
<tr>
<td><strong>Overall</strong></td>
<td>0.3643</td>
<td></td>
</tr>
<tr>
<td><strong>Observations:</strong></td>
<td>1900</td>
<td>1900</td>
</tr>
<tr>
<td><strong>Left-censored</strong></td>
<td>620</td>
<td></td>
</tr>
<tr>
<td><strong>Uncensored</strong></td>
<td>912</td>
<td></td>
</tr>
<tr>
<td><strong>Right-censored</strong></td>
<td>368</td>
<td></td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01
P-values in parentheses.
Random effects models use errors clustered at the group-level.
Tobit model errors clustered at the subject-level. Lower limit = 0, upper limit = 25
Dummies for rounds were included in the regressions but omitted in table.
Table 2.5: Regression Analysis of Group-Level Contributions in Voluntary Treatments

<table>
<thead>
<tr>
<th></th>
<th>(7) RE</th>
<th>(8) Tobit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>0</td>
<td>-23.39*</td>
</tr>
<tr>
<td></td>
<td>(.)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>Vol_Before dummy</td>
<td>-5.42</td>
<td>-13.72</td>
</tr>
<tr>
<td></td>
<td>(0.643)</td>
<td>(0.424)</td>
</tr>
<tr>
<td>Number_reveal</td>
<td>9.13***</td>
<td>14.75***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Vol_Before x Number_reveal</td>
<td>4.33</td>
<td>6.34</td>
</tr>
<tr>
<td></td>
<td>(0.192)</td>
<td>(0.196)</td>
</tr>
<tr>
<td>Average Group Contributions in Stage 1 (averaged over 10 periods)</td>
<td>0.079**</td>
<td>0.074***</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.007)</td>
</tr>
</tbody>
</table>

Periods included: 11-20 11-20

R-sq:
Within 0.5987
Between 0.7561
Overall 0.6947

Observations: 160 160

Left-censored 3
Uncensored 148
Right-censored 9

* p<.1, ** p<.05, *** p<.01
P-values in parentheses.
Dummies for rounds were included in the regressions but omitted in table.
Random effects and Tobit models with errors clustered at the group level.
Tobit model - lower limit = 0, upper limit = 125
**Table 2.6: Regression Analyses of Individual Level Contributions in Voluntary Treatments**

<table>
<thead>
<tr>
<th></th>
<th>Vol Sim</th>
<th>Vol Before</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(9) RE</td>
<td>(10) Tobit</td>
</tr>
<tr>
<td>Constant</td>
<td>-6.23***</td>
<td>-17.52***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Public Contribution</td>
<td>9.99***</td>
<td>15.87***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Lagged Average Visible Contribution by Others</td>
<td>0.26**</td>
<td>0.61**</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Lagged Average Visible Contribution by Others x Public Contribution</td>
<td>0.10</td>
<td>0.14</td>
</tr>
<tr>
<td></td>
<td>(0.436)</td>
<td>(0.585)</td>
</tr>
<tr>
<td>(Lagged) Number_reveal</td>
<td>0.68</td>
<td>0.08</td>
</tr>
<tr>
<td></td>
<td>(0.455)</td>
<td>(0.946)</td>
</tr>
<tr>
<td>(Lagged) Number_reveal x Public Contribution</td>
<td>0.29</td>
<td>0.47</td>
</tr>
<tr>
<td></td>
<td>(0.690)</td>
<td>(0.710)</td>
</tr>
<tr>
<td>Average Group Contributions in Stage 1 (averaged over 10 periods)</td>
<td>0.11*</td>
<td>0.16</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.112)</td>
</tr>
<tr>
<td>R-squared:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Within</td>
<td>0.5311</td>
<td>0.3625</td>
</tr>
<tr>
<td>Between</td>
<td>0.7343</td>
<td>0.7948</td>
</tr>
<tr>
<td>Overall</td>
<td>0.6544</td>
<td>0.5929</td>
</tr>
<tr>
<td>Observations:</td>
<td>313</td>
<td>313</td>
</tr>
<tr>
<td>Left-censored</td>
<td>102</td>
<td>107</td>
</tr>
<tr>
<td>Uncensored</td>
<td>170</td>
<td>139</td>
</tr>
<tr>
<td>Right-censored</td>
<td>41</td>
<td>156</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

Standard errors are in parentheses.
Random effects models use errors clustered at the group-level.
Tobit model errors clustered at the subject-level. Lower limit = 0, upper limit = 25
Dummies for rounds were included in the regressions but omitted in table.
Table 2.7: Regression Analyses of Change in Individual Level Contributions in Vol_Sim and Vol_Before Treatments

<table>
<thead>
<tr>
<th></th>
<th>Vol_Sim</th>
<th>Vol_Before</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Public (13)</td>
<td>Private (14)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.766** (0.014)</td>
<td>-8.492** (0.012)</td>
</tr>
<tr>
<td>Lagged difference</td>
<td>0.417*** (0.000)</td>
<td>0.425** (0.041)</td>
</tr>
<tr>
<td>contributions</td>
<td>0.123 (0.146)</td>
<td>-0.498 (0.861)</td>
</tr>
<tr>
<td>Lagged contribute more</td>
<td>-0.218 (0.570)</td>
<td>0.437 (0.875)</td>
</tr>
<tr>
<td>Lagged difference x</td>
<td>0.0712 (0.570)</td>
<td>0.103 (0.875)</td>
</tr>
<tr>
<td>observations</td>
<td>201</td>
<td>112</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.4248</td>
<td>0.2645</td>
</tr>
<tr>
<td>Within</td>
<td>0.2802</td>
<td>0.2289</td>
</tr>
<tr>
<td>Overall</td>
<td>0.3349</td>
<td>0.2082</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01
Standard errors are in parentheses.
Random effects models use errors clustered at the group-level.
FIGURES

Figure 2.1: Group-level contributions (in percent) to the Group Fund by treatment

Note: The maximum (100%) is 125 tokens (5 x 25 tokens). Nash equilibrium is 0 tokens (i.e., 0%).

Figure 2.2: Average Private, Combined, and Public Individual Contributions across Treatments

BRIEF OVERVIEW

Social networks can be critical in the spreading of pro-social behavior. Social peers may act as role models, may hold individuals accountable, or may convey which actions are appropriate. Furthermore, these effects differ depending on the setting (e.g., charitable giving versus problem solving) and the composition of the network (e.g., network density, relationship between actors, etc.). This study examines how information on others’ behavior (i.e., information networks) affects individuals’ donation behavior in a provision point public good setting, when decision makers are anonymous.

The experiment reported in this chapter utilizes a 2x2 treatment design, in which two different information network treatments are combined with two endowment levels, as follows:

- Two information network treatments
  - LOCAL network – subjects receive individual contribution information of two neighbors (the group is arranged on a circle network)
  - COMPLETE network – subjects receive individual contribution information for all group members

- Two endowment levels:
  - LOW treatment – $e = 30$ tokens – this requires at least four individuals in order to arrive at threshold of 120 tokens
  - HIGH treatment – $e = 50$ tokens – this requires at least three individuals in order to arrive at threshold of 120 tokens

---

26 This chapter arises out a joint research project with James Walker (Indiana University) and Simanti Banerjee (University of Nebraska).
This design allows us to answer the following questions:

- What is the impact of endowment on the likelihood of groups meeting the public good provision point?
- What is the effect of local behavioral information (as compared to information on all group members) on the ability of a group to collectively provide a threshold public good?
- Is this effect dependent on the ratio of group endowment to threshold value (i.e., the minimal number of individuals necessary to contribute for a successful meeting of the provision point)?

There are several main findings of the experiment:

- As the endowment increases, groups contribute more to the public good and are more likely to meet the threshold.
- In later periods, groups are more likely to meet the threshold if they are in the COMPLETE network treatments than if they are in the LOCAL treatments.
- Groups in the COMPLETE network treatments are more likely to arrive at symmetric provision of the public good (i.e., every individual contributes 20 tokens) than groups in the LOCAL treatments.
- Groups in the COMPLETE LOW treatment are more likely to meet the threshold than groups in the LOCAL LOW treatment, but there is no significant difference between the LOCAL and COMPLETE treatments in the HIGH endowment setting. This suggests that information needs to span the minimal number of necessary contributors to aid in coordination.

---

**INTRODUCTION**

Social networks are critical in the spread and magnitude of pro-social behavior. Consider the rapid diffusion of the ‘Ice Bucket Challenge’ utilized to raise funds to combat amyotrophic lateral sclerosis. The donation campaign spread through social networks, with people nominating others to participate in the challenge.

With others’ behavior demonstrated so publicly, participants were made aware of what customary behavior looked like (in terms of donations and dousing with ice water). The
campaign raised more than twice as many funds in 2014 for the ALS Association as in the previous year (ALS Association 2014), suggesting that social networks are a powerful means to encourage charitable contributions.

The effect of social networks is diverse and complex (see, for example, Monge and Contractor 2003; Jackson 2008). For instance, social peers may act as role models (see, for example, Bosma et al. 2012), may hold individuals accountable (see, for example, Sparrowe et al. 2001), or may convey which actions are appropriate (see, for example, March and Olsen 2004). Furthermore, the effect will likely differ depending on the setting (e.g., charitable giving versus problem solving) and the composition of the network (e.g., network density, relationship between actors, etc.) (Jackson 2008).

This experimental study focuses on one particular aspect of the effect of social networks on charitable giving. It examines how information on others’ behavior (i.e., information networks) affects individuals’ donation behavior in a provision point public good setting, when decision makers are anonymous. A provision point public good setting is one where contributions must meet a publicly known threshold for provision of the public good. In the game setting studied here, subjects receive endowments which can be used as contributions to a public good. Contributions in excess of the threshold do not increase the level of the public good and are not returned; and, if the threshold is not met, contributions are returned to the contributors. In this setting, we vary endowments and information networks to address the following questions: (i) Do endowment levels impact a group’s ability to provide the public good? (ii) To what extent does success in
providing the public good depend on knowledge of contributions of other group members? (iii) Does information network density impact provision success? (iv) Does the impact of information networks depend on the endowment levels of group members?

Studying the effect of information networks in *threshold* public goods is critical given that the provision of goods with substantial social benefits often requires a target or threshold level of funds in order to become economically feasible. Thresholds are particularly relevant to environmental public goods because of non-linear ecological processes. For example, given non-linear ecological processes, preserving biodiversity requires the creation of nature reserves of some critical minimum size. Similarly, the EPA specifies air and water quality standards as minimum thresholds up to which pollutant emissions should be abated for acceptable levels of public health safety.

From a theoretical perspective, threshold public goods provide a rich setting for examining strategic behavior. Based on standard assumptions of own income maximization and common beliefs of others having such preferences, the strategic environment of linear public goods yields a Nash equilibrium where everyone free rides and contributes nothing. Thresholds, however, create additional Nash equilibria. In particular, many combinations of contributions that equal the threshold level are socially optimal Nash equilibria.²⁷ In addition, under a money-back guarantee in case of non-provision (i.e., private contributions are refunded if the threshold is not met), there are many Nash equilibria where total group contributions are below the threshold, but not socially optimal. Even though there is evidence to suggest that information networks are

²⁷ Given certain conditions presented below.
significant in equilibrium selection (see below), there have been no studies that explore their impact in threshold public goods games.

The large experimental literature on voluntary contributions to linear public goods (for overviews see, for example, Ledyard 1995, Zelmer 2003, Chaudhuri 2011) explores conditions under which groups are more, or less, successful at collectively providing said goods, in cases where a threshold level of contributions is not required for provision. These game settings are generally constructed in a manner where there is only one Nash equilibrium (zero contributions).\textsuperscript{28} In this setting, the behavioral information of group members has proven ambiguous. Contrasting information on individual contribution behavior (i.e., complete information networks) with information on group-level contributions only (empty information networks), Sell and Wilson (1991) and Kreitmair (2015) find a positive effect of individual-level information on donation behavior. Weimann (1994) and Croson (2001), on the other hand, find no effect, whilst Wilson and Sell (1997) find individual-level behavior information reduces contributions. This divergence might be attributed to differences in Marginal Per Capita Return (MPCR)\textsuperscript{29} (which ranges between 0.3 and 0.75), in endowments\textsuperscript{30} (between 10 and 40 tokens), or group size\textsuperscript{31} (between four and six individuals) – all of which may interact with

\textsuperscript{28} Under the assumptions that subjects make decisions based on maximizing their own pecuniary gains in the experiment and this is common information.

\textsuperscript{29} MPCR, marginal per capita return, as defined by Isaac, et al. 1984, is the ratio between the return from a token contributed toward the public good and the return to a token invested in a private fund – it is a measure of opportunity cost. Isaac and Walker (1988) find that MPCR correlates positively with average contributions.

\textsuperscript{30} Laury et al. (1999) find that greater endowments lead to significantly greater contributions.

\textsuperscript{31} Studies by Isaac and Walker (1988) and Isaac et al. (1994), however, show little evidence to support that group size is negatively correlated with contributions.
information settings to affect contribution behavior. These possible interaction effects have not yet been systematically tested. In this study, we begin to unpack the relationship between behavioral information and endowments.

Not all experiments exploring the impact of information compare behavior in complete and empty information networks. A number of experimental studies provide subjects with selective behavioral information (where individuals receive, for example, information on the highest contribution only). In effect, these studies explore (complicated) incomplete information networks where the structure of the network is essentially dependent on previous behavior. For example, in some treatments, Jones and McKee (2004) and Croson and Shang (2008, 2013) provide subjects with individual contribution information of the highest or lowest contributor, respectively, and thus test information networks where links are a function of the contribution level. They find that information on the highest contribution increases donations (if the difference between the displayed contribution and one’s own contribution is not too large) and information on the lowest contribution reduces contributions. Kreitmair (2015) explores the impact of information networks where individuals may choose to voluntarily display their contribution behavior to others. The information network is thus dependent on voluntarily provided information and may range between empty and complete. She finds that the ability to voluntarily provide behavioral information improves upon the mandatory display of information (a complete information network) only if the willingness to disclose information is

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32 Laury et al. (1999) find that the impact of greater endowments changes with the provision of more or less detailed information on the payoff structure. Although they do not study behavioral information, they do provide evidence to suggest that endowment effects are context dependent. Similarly, the effect of information networks may be context dependent as discussed in more detail in the section on conjectures.
transmitted before contribution decisions are made. Within this literature, there is only one study that explicitly tests the impact of information network structure on contribution behavior: Fatas, et al. (2010). They find that behavior significantly varies across the range of information network structures they study. Network structures that provide more group members with information on an individual’s contributions tend to arrive at higher contributions toward the public good.

Based on differences in game equilibria in linear versus threshold public goods games, it is unclear to what extent these observations from experiments studying linear public goods would transfer to threshold public goods. Looking more broadly to the literature on coordination games, experimental studies suggest that denser information networks, in which subjects receive information from more nodes, increase the likelihood of efficient coordination compared to when information is available from fewer nodes (Banerjee, et al. 2014). These results, however, are not robust when the game structure is altered such that payoffs are dependent on the entire group’s coordination success (as is the case in the coloring game in Kearns, et al. 2006). In this case, increases in information improve likelihood of successful coordination only if the game is not too complex. Hence, with threshold public goods games incorporating an element of coordination, the literature does not provide clear predictions about what results to expect regarding information network effects.

33 There are a small number of experimental studies that play social dilemma games on networks. However, in these games (mostly prisoner’s dilemmas), payoffs are dependent on neighbors’ contributions rather than on donations made by the group at large, as is the case in the linear and threshold public goods literature. These differences in the payoff structure result in substantially different strategic settings, and subsequently, a different role to be played by information networks. These studies (Boosey 2011, Suri and Watts 2011, Cassar 2007) are thus not directly linked to our game setting. In addition, only Boosey alters information available to subjects. However, Boosey’s information treatments do not include incomplete networks of individual level behavioral information.
To the authors’ knowledge, there is only a single study, Croson and Marks (1998), that explores the impact of behavioral information in threshold public goods games. That study contrasts complete information networks with empty networks. They conclude that behavioral information improves the likelihood of successful provision of a threshold public good only if it is combined with identifiers as to who made the contribution. Our study is distinct from Croson and Marks by exploring the impact of incomplete (in addition to complete) information networks. Crucially, we interact these networks with two endowment settings\(^{34}\), arguing that information effects cannot be studied in absence of contextual factors that impact the strategic importance of the information received.

The paper proceeds as follows: The next section provides a formal model of the threshold public good and introduces network notation. Subsequently, the experimental design and conjectures are discussed. This is followed by discussion of hypotheses and results regarding the effects of endowments and information network variation on behavior and efficiency. The final section provides some conclusions.

**Theoretical Model**

**The Threshold Public Goods Game**

Individual, \(i\), is part of a group \(N\) (i.e., \(i \in N\) where \(N = \{1, ..., n\}\)). She chooses how much of her endowment, \(e_i\) (this could be income, wealth, or effort), to contribute toward a public good. Her contribution, \(m_i\), in combination with her group members’

\(^{34}\) To study possible interaction effects between information and endowments which is motivated, in part, by the contrasting findings in the linear public goods literature.
contributions, determine the payoffs for herself and her group: For each unit of $e_i$ that she does not contribute towards the public good (i.e., $e_i - m_i$) she receives a private return of $p_i$. If the sum of contributions by all group members (i.e., $\sum_{i \in N} m_i$) is greater than the publicly known threshold level, $T$, the discrete public good is provided resulting in a payoff of $b_i$ (the individual benefit derived from the public good). When the threshold is met, $b_i$ is provided to all group members regardless of their own contribution.

Refund rules (i.e., payoffs received for tokens when $\sum_{i \in N} m_i < T$) and rebate rules (i.e., payoffs received for tokens above the threshold when $\sum_{i \in N} m_i > T$) impact Nash equilibrium strategies and thus must be determined beforehand to allow calculation of Nash equilibria. In this study, individuals receive full refunds if the threshold target is not met but receive no rebates, meaning that excess tokens are lost. The game design implies that greatest payoff is received when groups contribute an amount exactly equal to the threshold.

There are three reasons why we chose to study this combination of refund and rebate rules: First, this design keeps our study consistent with a number of previous experimental studies of threshold public goods. See, for example: Isaac et al. 1989; Croson and Marks, 1998; Marks and Croson 1998, 1999; Cadsby and Maynes 1999; Croson and Marks, 2000; Cadsby et al. 2008. Second, it adds saliency to the provision point. There is reason to believe that confusion about payoffs may explain a part of observed contribution behavior in linear public goods games (see, for example, Andreoni 1995; Houser and Kurzban 2002), let alone in games with interior solutions. To minimize

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35 See also a number of other papers listed in the meta-analysis section in Croson and Marks (2000).
the impact of confusion, we emphasize the provision point making it easy for individuals to identify the provision point as the social optimum. This allows us a cleaner exploration of how information networks impact the provision of discrete public goods. Third, this combination of refund and rebate rules lends real world applicability to our experiment. With regards to full refunds, consider crowdfunding websites. On kickstarter.com, for instance, users may post information on a project they seek to fund and minimal funding goals. If this target is not made, donations pledged by individuals are not collected – i.e., contributions are fully refunded. In addition, numerous international agreements, notably the Kyoto Protocol and the Paris Agreement, only become binding when the required number of parties signs on. One could argue that this institution may be simulated via a full refund rule. Incidentally, international negotiations have been simulated using threshold public goods experiments with and without minimal participation constraints (see, for example, McEvoy, 2010; Tavoni et al., 2011; Barrett and Dannenberg, 2012). With regards to utilizing a “no-rebate rule”, consider scenarios where there are no additional benefits from contributing beyond a threshold as occurs in the presence of satiation points or discrete public goods. For example, the EPA sets National Ambient Air Quality Standards at levels deemed safe for human health. Reducing pollution below these levels may be considered insignificant to human (or even ecosystem) health.

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36 We also provide subjects with a paid training round – see Part I in the Experimental Design section below.
Payoff Functions

Define \(e_i\), \(p_i\), and \(b_i\) as individual subject endowment in a given decision round in tokens, earnings from per token not contributed to the public good, and an individual’s earnings from the public good if it the threshold is met. Assuming homogeneity across actors in \(e_i\), \(p_i\), and \(b_i\), this design results in the following payoffs:

\[
\pi_i = \begin{cases} 
pe & \text{if } \sum_{i \in N} m_i < T \\
p(e - m_i) + b & \text{if } \sum_{i \in N} m_i \geq T 
\end{cases} \quad (\text{Equation 3.1})
\]

Nash Equilibria

Assuming rational individuals motivated only by experimental earnings (with the understanding that other actors are similarly motivated), these payoffs result in the following Nash equilibria:

\[
\sum_{i \in N} m_i = 0
\]

\[
0 < \sum_{i \in N} m_i < T - (e - \min_i(m_i)), \text{ where } m_i \leq \frac{b}{p}
\]

\[
\sum_{i \in N} m_i = T, \text{ where } m_i \leq \frac{b}{p} \quad (\text{Equation 3.2})
\]

For thusly motivated and rational individuals to contribute toward the public good at all, the value of the benefit derived from the public good must be greater than the private return on the contribution (i.e., \(p \leq b\)). With full refund, it is profitable to continue contributing as long as \(m_i \leq \frac{b}{p}\). If \(\frac{b}{p} > e\) this constraint is non-binding.
Minimal Coalitions

The coalition formation literature (see Barrett 2003 for an example particularly relevant to public goods and environmental policy) suggests that not all individuals must cooperate to provide a non-linear public good. Given that all contributions are strategic substitutes (i.e., contributions are equally effective in providing the public good and thus fungible) and come with a private cost (forgone private earnings of $p_i$), $i$ prefers to have her group members meet the threshold without her own contribution ($\pi_i \sum_{i \in N} m_i = T (m_i = 0) > \pi_i \sum_{i \in N} m_i = T (m_i > 0)$). As long as $T - \sum_{i \in N} m_{-i} \leq (N - 1)e - \sum_{i \in N} m_{-i}$ (the contribution shortfall is less than or equal to the remaining funds of the contributors) and $m_i \leq \frac{b}{p_i}$, it is possible and profitable for other group members to make up any lost contributions if one individual were to contribute nothing. Rearranging the first condition, we find the lower bound on the number of group members who need to contribute, making free riding a viable strategy for the remaining others. The minimum number of contributors, $N^{min}$, necessary to offset non-contributions is thus:

$$N^{min} = \left\lfloor \frac{pT}{b} \right\rfloor$$

or $N^{min} = \left\lceil \frac{T}{e} \right\rceil$ if $\frac{b}{p} > e$ \hspace{1cm} (Equation 3.3)

Information Network Structure

Now consider that individual actors may receive information on group members’ contributions through information networks. To formalize networks: $l_{ij}$ represents undirected links or relationships between each pair of actors in a group. Hence, if $l_{ij} =$
Given this, \( i \)'s information neighborhood, \( G_i \), is defined as the set of actors to which \( i \) is linked (i.e., \( i \) sees all contributions made in her information neighborhood):

\[
G_i(l) = \{j : l_{ij} = 1\}
\]  

(Equation 3.4)

**Experimental Treatments and Conjectures**

**Experimental Design**

The experiment consisted of three parts: In Part I, subjects participated in a “menu game” – four one-shot threshold public goods games presented simultaneously. In Parts II and III, subjects participated in a repeated game setting with fixed groups. The menu game setting of Part I was designed to provide subjects the opportunity to familiarize themselves with a threshold public goods setting, with varying token endowments, holding the threshold constant. There was no feedback on game decisions before subjects proceeded to Part II. Results from the menu game will also be used to test endowment conjectures discussed below. Part II was designed as an experimental control to assess possible group specific effects and to be used to compare results from the current experiment to findings from previous studies. Part III was designed to explore primary and interaction effects between endowment levels and information networks. Results

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\(^{37}\) This was implemented to minimize the role of confusion in contribution behavior in Parts II and III.
from predominantly this stage will be used to test the conjectures described below. The full set of instructions are provided in Appendix 3.2.

In summary, and as discussed further below, Part III yields a 2x2 factorial design summarized in Table 3.1.

< Table 3.1 >

At the beginning of the experiment, subjects were randomly assigned to groups of six (N=6) and received a subject number (1-6) within the group. Subjects remained in the same group and retained the subject number during the experiment. This subject number was used to determine information neighborhoods for every subject (see below). Before participating in any stage, each subject received on-screen and printed instructions, which they could read at their own pace. All instructions were reviewed publicly. Before proceeding to the decision-making phase of each stage, subjects participated in quizzes to ensure that they fully understood the different features of the game. Participants received payoffs in terms of Experimental Currency Units (ECU) and at the end of the experiment these payoffs were exchanged into US dollars at a rate of 100 ECU = $1. This amount was added to the $5 show-up payment and was paid out in private.

_The Threshold Public Good Decision Setting_

During each part of the experiment, subjects received an endowment of \( e \) tokens. For every token in the individual account, subjects received 1 ECU \( (p=1) \). If group
contributions amounted to 120 tokens or higher, the threshold public good was provided (i.e., \( T=120 \)) and subjects received 60 ECU (i.e., \( b=60 \)) plus any earnings from their individual account, regardless of how much he or she had transferred to the group account. In cases where the group contributed more than 120 tokens, the excess tokens were lost. If the group transferred less than 120 tokens, the public good was not provided and all tokens were refunded back to the private accounts earning subjects \( e \) ECU. The values of \( T \) and \( b \) were chosen deliberately: The threshold of 120 tokens provides a straightforward focal point of 20 tokens – each person’s share of the goal that must be met. This value could be seen as a social norm against which individual contributions can be judged. The value of \( b \), 60 ECU, was chosen partly to allow a subject to contribute any or all of her tokens: Since endowment levels (in the repeated game settings) are lower than 60 ECU, the return on the public good (in the event that it is provided) always exceeds the private value of one’s endowment of tokens; thus, there is no value at which a contribution would be become irrational (as explained in the discussion of Nash equilibria above). This allows subjects to focus more attention on overcoming the social dilemma and coordinating on the social optimum.

*Part I – One-shot Menu Games*

In the menu game setting, subjects were asked to make contribution decisions in four different endowment scenarios (labeled A, B, C and D). In one scenario, subjects received 25 tokens, in another 45 tokens, in a third 65 tokens, and in a final scenario 85
Decisions were made simultaneously and subjects received no feedback on their earnings or the contribution decisions made by others before the end of the experiment.

**Part II – Baseline Repeated Game – Aggregate Information Only**

Subjects made decisions across 10 decision periods where both group composition and the parameters of the decision environment were held constant. Information environments remained unchanged for all groups but the endowment levels varied. In half of the groups, individuals received LOW endowments ($e=30$ tokens) and in the other half individuals received HIGH endowments ($e=50$ tokens). This means that in the LOW treatments $N^{\text{min}}=4$ and in the HIGH treatments $N^{\text{min}}=3$. In Part II, information feedback was at the aggregate level; once subjects made their contributions decisions, they received summary information regarding:

- Their current period contributions
- The total contributions by their group in the current period
- History of group contributions for all previous periods
- Total earnings for the current period (in ECU).

**Part III – Repeated Game – Information Network Treatments**

In Part III, groups and endowments remained the same as in Part II. The decision setting in Part III is identical to Part II except that subjects received individual contribution information from their information neighborhoods, between decision periods.

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38 To avoid ordering effects, the order in which the scenarios were presented was changed randomly across sessions according to a Latin square design. Discussion and analysis of ordering effects is not included here but in Appendix 3.1.
Information neighborhoods varied by treatment. In groups termed LOCAL, information neighborhoods consisted of two neighbors, resulting in individuals receiving information on individual contributions by two others in the group. For example, as shown in Figure 2, subject 1 received information about subject 2’s and subject 6’s current period contribution (after everyone had made their decisions). In the remaining groups, termed COMPLETE, information neighborhoods encompassed all other group members and subjects were thus provided with individual contribution values for all group members. The diagrams below illustrate the information networks in the two information treatments.

< Figure 3.1 >

Experimental Implementation

The experiment was programmed and conducted using Z-Tree (Fischbacher, 2007). Sessions were conducted in late 2014 and early 2015 at Indiana University. The 144 subjects were undergraduate students with various majors. On average, participants made $22.17, including the $5 show-up fee, for an experiment that lasted about 1 hour.39

Conjectures

C1. Endowments

a. Contributions – As endowments increase, groups will, on average, contribute more tokens toward the public good.

39 These earnings are high enough to make it likely that subjects were motivated to do well in the experiment. As a reference point, the minimum hourly wage in Indiana was $7.25 at the time.
b. **Meeting the Threshold** – As endowments increase, groups are more likely to meet the threshold.

Social dilemma experiments that explore the impact of endowments suggest that the number of tokens that subjects receive impacts group-level contribution decisions. For example, Laury et al. (1995) show that contributions toward the public good increase significantly as endowments increase. In social dilemmas with interior solutions, Ostrom et al. (1994) find that in environments with the same social optimum extraction rate, groups with higher endowments extract more from a common pool resource than groups with lower endowments. There are a number of reasons why one might expect these findings to translate to the current experimental study.

One reason to expect higher contributions in higher endowment settings begins with the robust finding in the experimental social dilemma literature that there is considerable variance in individual contribution behavior (see, for example, Isaac et al. 1994 and Ostrom et al. 1994; see also Figures 3.5.a, 3.5.b, 3.5.c, and 3.5.d below). The literature has begun classifying subjects into behavioral types (see, for example, Fischbacher et al. 2001, Burlando and Guala 2005, Fischbacher and Gächter 2010) such as free-riders (who predominantly contribute 0 tokens), conditional cooperators (whose contribution is positively correlated with contributions made by others), and altruists (who contribute at a high level throughout the experiment). In appropriation social dilemmas (Ostrom et al. 1994), such as common pool resource games, higher endowments increase the capacity of free-riders to behave non-cooperatively by increasing their potential extraction amount.
Thus, with similar distributions of subject types across treatments, group extraction rates increase as endowments increase (as shown by Ostrom et al. 1994). Conversely, in a linear public goods game, when endowments increase, altruists now have the opportunity to contribute more toward the public good. Subsequently, group-level contributions increase as endowments increase (as suggested by Laury et al. 1995).

With regards to threshold public goods, assume, due to randomization of treatments, that there are similar proportions of behavioral types across endowment treatments. If altruists make their contribution decisions in terms of the proportion of their endowments to invest in the public good, contributions will increase as endowments increase. In turn, as endowments increase, contributions by altruists are more likely to make it possible for only one other individual to make up the difference between the altruists’ contributions and the threshold (see the Nash equilibria in Equation 3.2). In this case it is rational, even for a self-regarding individual, to contribute so that the group might meet the threshold, thus making groups in higher endowment settings more likely to provide the public good. Finally, recall (from Equation 3.3) that as endowments increase minimal coalition sizes decrease. Thus, it requires fewer individuals to cooperate, and more groups are likely to contain a sufficient number of cooperative individuals to meet the threshold.

Finally, there is strong evidence to suggest that, in situations of (even mild) uncertainty, individuals tend to base their estimates of value (of a good or an exhibited behavior) on any number provided, even if irrelevant (see, for example, Kahneman et al. 1999, Ariely

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40 In the menu game this assumption is given by default because all subjects take part in all endowment settings.
et al. 2006). This anchoring bias (Tversky and Kahneman 1974) is consistent with the group-level behavior observed in social dilemma experiments described above. There is reason to believe that anchoring may also play a role in the current experiment. There is significant uncertainty in the game with regards to other subjects’ behavior and what might be considered a socially acceptable contribution behavior. Thus, subjects may anchor their contribution decision on the endowment level resulting in higher contributions as endowments increase.

Previous studies of threshold public goods have not explored the impact of endowments directly. However, in their meta-analysis of 19 threshold public goods experiments, Croson and Marks (2000) provide evidence that the proportion of a group’s endowment necessary to meet the threshold (i.e., $\frac{T}{N_0}$) is not significant in determining whether a group will successfully meet the provision point. Rather, contribution levels, and hence a group’s likelihood of meeting the threshold, are shown to be dependent on the Step Return, henceforth SR. The SR is the ratio of the relative benefit (as compared to the value of the private good) received by the group when the public good is provided to the threshold level, $T^{d^5}$:

---

41 Note, however, that these studies (Ostrom et al. 1994 and Laury et al. 1995) do not test for anchoring effects. Given that endowments have an impact on payoffs received by subjects, changes in contribution behavior across endowment treatments distinguish between anchoring effects and “behavioral type effects.” Thus, results from these studies are merely consistent with anchoring effects, not indicative of them.
42 None of the studies included in the meta-analysis explicitly tests the impact of endowments on the likelihood of public good provision success. Rather, the meta-analysis utilizes cross-study variation in endowments to estimate its impact on the likelihood of groups meeting the threshold.
43 For additional evidence of the impact of SR see also Cadsby et al. (2008). The SR was initially designed as a measure to mimic MPCR in linear public goods (Croson and Marks 2000).
44 $N_0$ in a symmetric game.
45 I.e., the cost of providing the public good.
$$SR = \frac{N_b}{pT}$$  \hspace{1cm} (Equation 3.5)

However, all studies in the meta-analysis, save one\(^{46}\), vary the proportion of the group’s endowment necessary to meet the threshold by altering the threshold level. As a result, the SR also varies. The covariation between these two variables makes it difficult to ascertain the effect the endowment level has on a group’s ability to meet the provision point, suggesting that results from the meta-analysis might not be indicative of the impact of endowment on contributions. In the current experiment the threshold (and thus the SR) remains constant across all treatments while we vary the endowment level, allowing for a direct test of the impact of endowment levels on the likelihood of meeting the threshold.

\textbf{C2. Network Structure}

As discussed above, the impacts of networked information are diverse and complex. With regards to the impact of network structure, scholars have devised a number of different statistics that capture different aspects of the structure of the network (see, for example, Wasserman and Faust 1994 and Jackson 2008). Among these, we will use average path length and network density to motivate the following conjectures.

Average path length, a mark of network efficiency, is defined as the average number of steps along the shortest paths between all possible pairs of actors. It thus measures how efficiently information may pass between two actors (i.e., nodes) in a network. For

\(^{46}\) Isaac et al. (1989) varies the proportion necessary without altering the threshold level. They find that proportion of endowment necessary is negatively correlated with the likelihood of success. However, the payoff function in Isaac et al. is different in that every token in the group fund, once the threshold is met, returns a set value – in other words, unlike in this study or in the majority of the previous literature (Croson and Marks 2000), subjects receive a rebate.
example, once \( N > 3 \), a star network will always have shorter average path length than a line network with the same number of nodes even though the network density is the same. Average path length is calculated as follows:

\[
v(g) = \frac{1}{N(N-1)} \sum_{i \neq j} k(n_i, n_j)
\]

(Equation 3.6)

Where \( k(n_i, n_j) \) is the shortest path between actors (\( n \), or node) \( i \) and \( j \).

Density of a given network (\( g \)), \( d(g) \), is a measure of how many of the possible links in a group are present. It is thus an indication of how closely individuals are connected and, in part, how quickly information may pass between nodes. Density is directly related to the size (i.e., cardinality) of individuals’ neighborhoods and is calculated in the following way:

\[
d(g) = \frac{\sum_{i \in N} \# G_i(I)}{N(N-1)}
\]

(Equation 3.7)

Note, however, that the experiment was not designed to distinguish between the effects of these different network attributes. In the design, these attributes co-vary perfectly (i.e., COMPLETE networks are both denser and have shorter average path lengths than LOCAL networks). Rather, we use these attributes, and findings in the corresponding literature, solely to explain why we expect differing behavior across the LOCAL and COMPLETE treatments. As such, average path length is used to explain C.2.a and network density motivates both C.2.b and C.2.c.
a. **Meeting the Threshold** – Groups with lower average path lengths are more likely to meet the threshold. COMPLETE networks are more likely to provide the public good compared to LOCAL networks.

Fatas et al. (2010) suggest that groups with some individuals whose contributions are highly visible contribute significantly more in linear public goods and thus come closer to the social optimum. Given the structure of the networks being studied, this visibility positively correlates with network efficiency in terms of is average path lengths.\(^\text{47}\) The information treatments implemented here allow a test of whether the findings of Fatas et al. are also supported in a threshold public goods environment. Thus, we expect groups in LOCAL network treatments (average path length = 1.8) to be less likely to meet the threshold than groups in COMPLETE network (average path length = 1) treatments within each endowment setting.

b. **Meeting the Threshold Exactly** – Groups with denser information networks are more successful in coordinating on the social optimum. Groups in COMPLETE networks are more likely to contribute exactly 120 tokens than groups in LOCAL networks.

\(^\text{47}\) Fatas et al. do not define efficient networks and do not calculate average path length, however average path length is consistent with their findings in that the complete and the star network have lower average path lengths than the circle and line networks and result in significantly higher contributions. Note also that individuals never receive direct information on the behavior of their neighbor’s neighbors suggesting that average path length might not be an appropriate statistic in this setting. However, in many settings where average path length is calculated individuals do not receive this information (see Jackson 2008). Further, in a sense, this information is transferred via a neighbor’s reaction to their neighbor’s behavior.
Meeting the threshold is, in part, a coordination effort. The experimental literature on coordination games on networks suggests that denser information networks improve the likelihood of coordination on payoff dominant strategies (Banerjee et al. 2014). Information on more individuals (i.e., denser networks) allow for more accurate adjustment of contributions, making coordination on the social optimum more likely. Therefore, COMPLETE network groups (with $d(g)=1$) are more likely to contribute at $\sum_{i \in N} m_i = T$ than LOCAL network groups (with $d(g)=0.4$).

c. **Equitable Distribution of Contributions** – The presence of increased oversight/scrutiny results in more equitable contributions. COMPLETE networks lead to more equitable contributions relative to LOCAL networks.

Levitt and List (2007) argue that scrutiny is a critical factor in predicting behavior. In particular, higher scrutiny amplifies the utility received from complying with social norms. Thus, as scrutiny increases so should convergence, resulting, in the described experiment, in more equitable contributions. COMPLETE networks provide greater scrutiny given that one’s behavioral information is available to all group members (i.e., it is a denser network than the LOCAL network – see above). Hence, variation in contributions in low density LOCAL network groups is expected to be lower than in COMPLETE network groups (where $d(g)=1$). Further, COMPLETE network groups are expected to coordinate on the symmetric equilibrium ($\sum m_i = T$ and $m_i = m_j$, $i \neq j$) more often than LOCAL network groups.
C3. Interaction between Endowments and Network Structure

Groups where information networks span minimal coalitions necessary to meet the provision point (i.e., $\#N_i(l) \geq N^{\text{min}}$) are more likely to meet the threshold. COMPLETE LOW are more likely to meet the threshold than LOCAL LOW, but there is no difference between COMPLETE HIGH and LOCAL HIGH.

This conjecture posits that the effect of network density on successfully meeting the threshold is sensitive to the endowment level (or, rather, the number of individuals necessary to arrive at the provision point). In particular, information networks that span the smallest subgroup necessary to provide the public good allow for successful coordination, because the information available is sufficient for an individual to coordinate with the minimal number of others necessary for meeting the threshold. Consider that in HIGH endowment groups the threshold can be met by three individuals. Therefore, in the LOCAL treatment where $\#N_i(l)=2$, any one individual has information on three individuals’ behavior (herself included) and can thus coordinate with a sufficient number of individuals to meet the threshold. The COMPLETE treatment also provides sufficient information. Hence, in the HIGH endowment groups there should be no significant difference in the likelihood of groups meeting the threshold across the two information treatments. In contrast, in the LOW endowment treatment contributions from at least four individuals are necessary to provide the public good ($N^{\text{min}}=4$). In the LOCAL information treatment, the information exchanges do not span this minimal coalition. In the COMPLETE treatments, however, it does. Therefore, we expect significant increase
in likelihood of meeting the threshold in the LOW endowment COMPLETE information groups compared to the LOW endowment LOCAL information groups.

**Results**

This section discusses the results from Part I and the results from Part II and III separately. Within the analysis of the menu game (Part I), we first provide an overview of contributions before exploring the impact of endowments on contributions. Subsequently we will discuss findings from Parts II and III, again providing summary statistics and figures. Thereafter, results will be structured around testing the conjectures discussed above using data from the repeated game\textsuperscript{48}. Finally, convergence behavior (toward the threshold level) is explored.

**Results from Part I – Menu Game**

*Overview*

Figure 2 displays mean contributions in the menu game by endowment and Table 3.2 provides additional overview statistics of the behavior exhibited in Part I. Consistently, higher endowments result in higher contributions towards the public good. The table also provides an overview of the distribution of contributions across the four endowment levels: A similar percentage of individuals free-ride in the four endowment settings. Strong freeriding (Isaac et al. 1984), where individuals contribute 0 tokens toward the public good, is observed by about 5-6% of subjects across all endowments. Similar amounts of weak freeriding, where individuals contribute more than 0 but fewer than the

\textsuperscript{48} Thus, C1 will be tested using both data from the menu game and data from Parts II and III.
focal point of 20 tokens, is exhibited in the three higher endowment settings. In the lowest endowment setting, contributions tend to be skewed toward weak freeriding; in the higher endowment settings, more individuals contribute more than the focal point. The modal contribution levels, however, remains the same across endowment settings. The histogram of individual contribution decisions in Figure 3.3 reinforces this observation. Two-sample Kolmogorov-Smirnov tests comparing the distributions of contributions in the four endowment settings (see Table 3.3) indicate distributions are only statistically significantly different when comparing \( e=25 \) and \( e=65 \), \( e=25 \) and \( e=85 \), and \( e=45 \) and \( e=85 \). These results are largely driven by individuals having the opportunity to invest more tokens in the higher endowment settings and doing so (see right tail in contribution distributions in Figure 3.3). Thus, the change in mean contributions is not the result of changes in the number of strong free riders but rather in “altruists” having the opportunity to increase their contributions, suggesting that these individuals may make contribution decisions based on proportion of endowment.

The percentage of groups meeting the threshold increases with endowment. However, as endowments increase, groups become more likely to overshoot the threshold. The groups that do meet the threshold in the highest endowment setting all contribute more than the required 120 tokens. These results will be analyzed in more detail in the following section when testing C1.\textsuperscript{49}

\textsuperscript{49} Note also that no individual makes contributions above 60 tokens, which suggests that individuals understand the game sufficiently to not make unprofitable contributions (i.e., where \( m_i > \frac{b}{p} \)). This suggests that behavior is indicative of preferences in the sample rather than the result of confusion (see discussion above).
C1 – Endowment Effects

In the menu game, as the endowment level increases, there is an increase in contributions. Both individual contributions and group contributions are significantly different (at the 1% level) across endowment levels (see Table 3.4). Thus, conjecture C1.a is supported in the context of one-shot games. Regarding the impact of endowments on the likelihood of successfully providing the threshold public good we run a simple logit regression (results reported, as output ratios, in Table 3.5). Dummy variables for the different endowment settings are the explanatory variables. Errors are clustered at the group-level to account for systematic differences across groups. Groups are more successful in meeting the threshold in higher endowment settings compared to the lowest endowment setting (e=25 tokens). Comparing the coefficients on the endowment dummies using Wald tests we find that groups are not significantly more likely to meet the threshold when they receive 45 tokens as compared to 65 tokens (p=0.1501). However, they are significantly more likely to meet the threshold when they receive 85 tokens compared to 45 tokens (p=0.0169) and 65 tokens (p=0.0730). Thus, even though there is no significant difference between the 45 token and 65 token endowment settings, there is a trend where, as endowments increase, the likelihood of meeting the threshold increases as well.
Therefore, C1.b is also supported in a one-shot setting. We test these conjectures in a repeated setting below.

< Table 3.4 >

< Table 3.5 >

Results from Parts II and III – Repeated Game

Overview

Figures 3.4.a and 3.4.b show mean contributions in Parts II and III across the different treatment conditions. Figures 3.5.a, 3.5.b, 3.5.c, and 3.5.d display all individual contribution decisions made in the Parts II and III of the experiment, respectively. These figures corroborate the significant variance in individual decisions found in previous experiments (see discussion above). Table 3.6 displays summary statistics of the behavior exhibited in the repeated game (Parts II and III). Unlike in linear public goods games, there is no consistent contribution decay across periods (i.e., the “endgame effect”) (see results of paired t-tests in Table 3.7). Rather, LOW endowment groups tend to increase contributions (comparing average contributions in period 1 and period 10) and HIGH endowment groups tend to decrease their contributions. This effect is lessened when comparing period 2 and period 10, indicating that convergence occurs. In Part III, this effect disappears in all but the LOCAL LOW treatment (comparing mean contributions in period 11 and period 20), suggesting that once groups meet the threshold group contributions remain relatively constant (see also discussion below). Likewise, there is no
consistent “restart effect” (Andreoni 1988\textsuperscript{50}) in which groups significantly change their contributions at the beginning of a new stage (comparing mean contributions in period 10 and period 11). This effect is only observed in the LOCAL LOW treatment where groups tend to have not met the threshold in period 10.

< Figure 3.4.a >
< Figure 3.4.b >

< Figure 3.5.a >
< Figure 3.5.b >
< Figure 3.5.c >
< Figure 3.5.d >

< Table 3.6 >
< Table 3.7 >

Returning to the summary statistics in Table 3.6, the percentage of individuals contributing 0 and their full endowment, respectively, decreases from Part II (aggregate information only) to Part III (information treatments). At the same time, there are more individuals that contribute at the focal point, or equitable level (i.e., $T/N$). This finding is further supported by comparing the histograms of individual contribution decisions in

\textsuperscript{50} Note that Andreoni (1988) first identifies the restart effect when subjects face a “surprise restart.” In other words, they do not know that they will be participating in a second sequence of decision rounds. However, the restart effect has since been observed in numerous settings even when subjects know that they will be participating in additional sequences (see, for example Isaac and Walker 1988).
Figures 3.4.a and 3.4.b. These findings explain the reduction in variance in individual contributions (see Table 3.6), suggesting that contributions become more equitable as the experiment progresses. In addition to being more successful in meeting the threshold, groups do so more efficiently; groups more often coordinate on exactly the threshold (i.e., \( \sum_{i \in N} m_i = T \)) in all but treatment LOCAL HIGH. Finally, consistent with results from the menu game, higher endowments result in higher contributions toward the public good in both Part II and Part III. This result will be analyzed in more detail in the next section.

< Figure 3.6.a >

< Figure 3.6.b >

**C1 – Endowment Effects**

Having found evidence to support C1.a and C1.b in a one-shot setting, we now explore the impact of endowments in a repeated setting. Figures 3.7 and 3.8 suggest that endowments impact contributions as well as likelihood of meeting the threshold. To test for significance of the effect of endowments on contribution levels, Wilcoxon Signed Rank Tests are run on individual and group contributions in period one of Part II\(^{51}\), all periods in Part II, and all periods in Part III. On average, in the first period of Part II individuals contributed 16.43 tokens (54.77\% of their endowment) in the LOW endowment setting and 22.21 tokens (44.42\% of their endowment) in the HIGH endowment setting. With a p-value of 0.0003 we can reject that contributions are the same across the LOW and HIGH endowment treatments in the first period. This finding

\(^{51}\) Data from period one is analyzed separately to test for endowment effects free from confounding effects from feedback and group interaction.
is mirrored at the group level: On average, groups contribute 98.58 tokens in the LOW treatments and 133.25 tokens in the HIGH treatments in the first period. These are significantly different at the 1% level (p=0.0004). When considering contributions across all of Part II and Part III, there is no longer a significant difference between individual contributions in the LOW and HIGH settings. Group contributions, however, remain significantly different – the result of differences in within-group variance in contributions across treatments. These findings are in agreement with the results reported in the menu game, suggesting that endowments are significant in increasing group contributions toward the public good irrespective of the (anticipated) repetition of the decision setting – C1.a is thus supported in a repeated setting as well.

To explore the impact of endowment on the likelihood of meeting the threshold (C1.b), consider the Logit models in Table 3.9. The regressions model the likelihood of groups meeting the threshold given whether they received a high endowment (HIGH Dummy), were in a complete network (COMPLETE Dummy), or belonged to both these categories (HIGH * COMPLETE). The first variable allows for testing of the endowment conjectures, the second tests the network structure conjectures, and the third explores whether information affects groups differently dependent on their endowment (and thus allows for testing of the interaction conjectures). Model 1 uses data from period 1 only so

52 This is likely a testament to the existence of free riders in both endowment settings.
as to avoid confounding effects from feedback and group interaction (see above). Model 2 explores the impact of endowment in all of Part II. Models 3-7 utilize data from Part III to test the impact of information networks. Group behavior in Part II is controlled for through the inclusion of a dummy variable capturing whether the group met the threshold in period 10. These models vary with regards to which periods are being considered. This rationale for separating the analysis is discussed below (see C2 – Network Structure Effects). The output is displayed as odds ratios.

Based on Table 3.9, we can conclude that HIGH endowment groups are 25 and 2.1 times as likely to meet the threshold in period 1 and in all of Part II, respectively, than LOW endowment groups (Models 1 and 2). This effect prevails across parts; in Part III groups in the HIGH endowment treatments are more than 3 times as likely to meet the threshold (see Model 3). Thus, endowments have a positive effect both on contributions and on the likelihood of meeting the provision point, meaning that both C1.a and C1.b are supported in the repeated settings.

C2 - Network Structure Effects

Table 3.9 also provides evidence on the role of information networks in a group’s ability to provide a provision point public good. In Model 3, the odds ratio of the COMPLETE

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53 Groups tend to remain at the threshold level once they meet it, especially when they meet the threshold exactly and symmetrically (see Overview Section, Figures 3.5.a, 3.5.b, 3.5.c, and 3.5.d, and Tables 3.10.a and 3.10.b). Therefore, behavior in period 10 captures much of the relevant behavior of Part II necessary for predicting behavior in Part III.
dummy is not significant. However, from Figures 3.5.a, 3.5.b, 3.5.c, and 3.5.d there is evidence of a difference in convergence toward the threshold between the LOCAL and COMPLETE treatments in the two endowment settings. In the COMPLETE network, LOW endowment treatment, five groups (Groups 3031, 3032, 3051, 3052, 3053) met the threshold consistently (and arrive at stable contributions) toward the end of the experiment. In the LOCAL network LOW endowment treatment, only one group exhibits this behavior (Group 1012). Likewise, in the HIGH endowment settings: In the corresponding COMPLETE treatment four groups arrive at stable contributions (Groups 4042, 4071, 4072, 4073) compared to three groups in the LOCAL treatment (Groups 2021, 2022, 2023). Hence, we separate the data by periods. Model 4 utilizes data from periods 11 through 16 only and Model 5 examines data from periods 17 through 20. In earlier periods (Model 4), the likelihood of meeting the provision point is driven by the endowment: HIGH endowment groups are 4.2 times as likely to meet the threshold (see also discussion above). In later periods (Model 5), however, provision success is largely driven by the information available to the groups: Groups in the COMPLETE treatments are 3.8 times as likely to meet the threshold. Thus, C2.a is supported.

To assess whether COMPLETE networks allow for coordination on the social optimum (C2.b) and doing so more equitably (C2.c), we must distinguish among instances in which a group i) meets the threshold exactly but with asymmetric contributions (i.e., $\sum m_i = T$ where $m_i \neq m_j$, $i \neq j$), ii) meets the threshold exactly and everyone contributes the same (i.e., $\sum m_i = T$ where $m_i = m_j$, $i \neq j$), and iii) overshoots the threshold (i.e., $\sum m_i > T$). The models in Table 3.9 do not differentiate among these cases. Table 3.10a shows the results
of random effects logistical regressions using data from Part III. The models differ with respect to the dependent variable. Model 1 examines the likelihood of groups contributing below the threshold and Models 2-4 examine the likelihood of meeting the threshold in the three different ways described above. The explanatory variables of interest are, as before, a) a dummy variable for the HIGH endowment treatment, b) a dummy variable for the COMPLETE network, and c) an interaction term between the two dummy variables. To control for Part II behavior (group history effects), dummy variables for whether the group contributed i) below, ii) asymmetrically at, iii) symmetrically at, and iv) above the provision point in period 10 are included. Table 3.10.b explores the same dependent variables but uses a pooled multinomial Logit specification. The results in both tables are expressed as odds ratios.

The significance of variables is robust to the model specification. Models in both tables (Model 3 and 6) suggest that COMPLETE information groups are significantly more likely to arrive at the symmetric equilibrium than groups in the other treatments. There is no significant difference, however, in terms of over- or under-shooting the provision point. This means that C2.c is supported to a certain extent whilst there is mixed evidence for C2.b.

54 I.e., only data from periods 11-20 are used.
C3 – Interaction Effects (between Endowments and Network Structure)

Returning to Table 3.9, Models 1-4 suggest that there is no difference as to how information networks impact behavior in the different endowment settings – the interaction effect is not significant. However, separating the data into LOW and HIGH endowment sets (Models 6 and 7), it is evident that the information effect identified previously (Model 5) derives from the LOW endowment treatments. In other words, groups in the COMPLETE LOW treatment are more likely to meet the threshold than groups in the LOCAL LOW treatment, but there is no significant difference between the LOCAL and COMPLETE treatments in the HIGH endowment setting. This result supports C3.

Convergence Behavior

Finally, Table 3.11 provides evidence regarding convergence behavior toward the social optimum by modeling changes in individual contributions in two random effects OLS models. The data from the LOW and HIGH treatments are modeled separately (Model 1 and 2, respectively) to account for the possibility that information networks may affect behavior differently in these settings.\textsuperscript{55} The dependent variable in the models is the change in individual contributions from the previous period (i.e., $m_{i,t} - m_{i,t-1}$).\textsuperscript{56} To explain changes in contributions, the following independent variables are included: A LOCAL treatment dummy is included to explore the impact of information networks. The difference between group contributions and the threshold in the previous period (i.e., $\left(\sum_{i \in N} m_{i,t-1} - T\right)$) is included to examine response to...

\textsuperscript{55} Although the same may be achieved by interacting the independent variables with an endowment dummy, this approach was selected for ease of interpretability of the results.

\textsuperscript{56} A positive value indicates an increase in contributions.
undershooting, meeting, or overshooting the threshold in the previous period.\textsuperscript{57} The interaction term, LOCAL dummy * Distance to T, is included to account for a difference in how individuals react to undershooting or overshooting the threshold in the previous period based on the information available to them. The change in viewable contributions (i.e., Eq. 8) allows for examining how individuals react to changes in average viewable contributions (i.e., changes in average contributions by group members in their information network).\textsuperscript{58} The interaction between LOCAL and viewable contributions accounts for any differences in how individuals react to contribution changes in their neighborhood based on the network they are in.

\[ \left( \frac{\sum_{i \in N_i(t)} (m_i(t-1) - m_i(t-2))}{|N_i|} \right) \]  
(Equation 3.8)

\(< Table 3.11 >\)

From the results reported in Table 3.11, it can be inferred that individuals try to make up for any contribution shortcomings that occurred in the previous period – the coefficient on the lagged distance to T variable is negative in both LOW and HIGH endowment

\textsuperscript{57} Consider that the game has an interior solution. Hence, behavior is likely to change depending on whether a group missed or met the threshold: Under-contribution in the previous period should lead to increases in contributions, meeting the threshold exactly should lead to no change in contributions, and over-contribution should lead to decreases in contributions. Given the specification of this variable, a positive coefficient indicates divergence from the optimum (i.e., when groups under-contribute in the previous period they decrease their contributions in response), and a negative coefficient indicates convergence behavior (i.e., when groups under-contribute in the previous period they increase their contributions in response).

\textsuperscript{58} The variable is negative if group members decrease average contributions and positive if they increase average contributions. Therefore, if the coefficient is negative, individuals tend to make up the shortfall from contribution reductions by others. If the coefficient is positive, individuals “punish” contribution reductions by reducing contributions themselves.
environments. However, in the HIGH endowment treatments individuals increase their contributions, on average, by \( \frac{1}{4} \) of a token for every token that is missing, while in the LOW treatments individuals increase their contributions by a little more than \( \frac{1}{6} \) (or \( \frac{1}{N} \)) of a token per token missing. Hence, HIGH groups tend to overshoot after missing the provision point. In contrast, groups in the LOW endowment treatments converge more efficiently. However, this latter result is only the case for COMPLETE LOW groups (based on the significant coefficient on the interaction effect LOCAL dummy * Distance to T in Model 1). In the LOCAL LOW treatment, individuals increase their contributions by 0.073 (the sum of the coefficients on the lagged distance variable and on the interaction variable\(^9\)) tokens per tokens missing. Thus, these groups tend to make up only about half of the shortfall (0.073 x 6 = 0.438) and tend to miss the threshold after missing it the previous period. In the high endowment groups, individuals in different network structures react similarly to contribution shortfalls, suggesting that there is no interaction effect between information networks and HIGH endowments in terms of convergence behavior.

Table 3.11 also provides evidence on how individuals tend to react to contribution information. In the LOW endowment groups, changes in viewable contributions elicit no significant response. Individuals in the HIGH endowment groups, however, react strongly to changes in average viewable contributions. In this case, if individuals have full individual level behavioral information, they tend to make up about 80% of reductions in average viewable contributions (coefficient on change in viewable

\(^{99}\) A joint test of the coefficients (i.e., H₀: Last period distance to T + LOCAL * Distance to T = 0) is rejected with a p-value of 0.0020.
contributions). In the LOCAL HIGH treatments, however, individuals make up only about 15% of the short falls (combination of the change in viewable contributions variable and the interaction variable\(^{60}\)). This suggests that freeriding in COMPLETE HIGH treatments has less impact on meeting the threshold, because other group members will make up more of the shortfall than in the LOCAL HIGH treatments.

**CONCLUSION AND DISCUSSION**

This experimental study supplements the growing literature on the impact of providing individuals with behavioral information about others. In particular, it was designed to explore the impact of information networks on groups’ abilities to collectively provide a threshold public good. The experimental design interacted two types of information networks (LOCAL and COMPLETE) with two endowment levels, to explore how the impact of information networks might change based on the resources at one’s disposal. The main findings are summarized below.

In contrast to findings by a meta-analysis of threshold public goods experiments (Croson and Marks 2000), increases in endowments lead to both significantly more contributions and significantly higher likelihood of meeting the provision point. As discussed above, this difference in findings may be an artifact of near perfect covariance between endowments and the step return found in previous studies to be a strong indicator of provision success (Croson and Marks (ibid.)). The significance of endowments, however,

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\(^{60}\) A joint test of the coefficients (i.e., \(H_0: \text{Change in viewable contributions} + \text{LOCAL} \ast \text{Change in viewable contributions} = 0\)) is rejected with a p-value of 0.0106.
does agree with previous findings regarding the anchoring effect (Tversky and Kahneman 1974) and is corroborated by results in linear public goods games (Laury et al. 1999).

Over time, denser information networks increase the likelihood of groups successfully providing a discrete public good. Although we cannot distinguish among possible mechanisms through which this effect occurs (e.g., increased scrutiny, greater ability to coordinate), it is clear that groups in denser information networks are better able to coordinate on meeting the threshold in later rounds. In addition, groups in denser networks are more likely to meet the threshold symmetrically suggesting a more equitable distribution of contributions in these networks. This finding may suggest that individuals have an innate preference for fairness and are better able to coordinate on these outcomes when they receive adequate information. Alternatively, they are no longer able to hide social norm divergent behavior in the COMPLETE network and may thus conform to the social norm. This finding, however, may be sensitive to a strong focal point (here 20 tokens), which may act as a fair contribution level upon which groups can converge.

Finally, the results suggest that in provision point public goods settings, information effects cannot be studied in isolation of contextual factors. The experiment indicates that the impact of information networks is dependent on the endowment level, or rather, on minimal coalitions (i.e., the minimum number of individuals that must contribute to meet the threshold – which is a function of the threshold level and individual endowments). As

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See, for example, Fehr and Schmidt 1999 for a discussion of social preferences.

For a discussion, see, for example, Bernheim 1994 and Levitt and List 2007.
long as information networks span these minimal coalitions, groups are more likely to be able to coordinate on meeting the threshold irrespective of whether they are in more or less dense network settings. Thus, fundraising campaigns should be careful to not just be mindful to target communities/households with high endowments or dense information networks, but rather to consider both aspects in combination.
REFERENCES


### Table 3.1: Between-Subject Experimental Design for Part III

<table>
<thead>
<tr>
<th>Information Network</th>
<th>Endowment Level</th>
<th>LOCAL</th>
<th>COMPLETE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>LOW</td>
<td>LOCAL-LOW</td>
<td>COMPLETE-LOW</td>
</tr>
<tr>
<td></td>
<td>HIGH</td>
<td>LOCAL-HIGH</td>
<td>COMPLETE-HIGH</td>
</tr>
</tbody>
</table>

### Table 3.2: Summary Statistics: Contributions in Menu Game by Endowment Level

<table>
<thead>
<tr>
<th>Individual Endowment Level</th>
<th>Proportion of Group Endowment necessary to meet $T=120$</th>
<th>25 tokens</th>
<th>45 tokens</th>
<th>65 tokens</th>
<th>85 tokens</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(80%)</td>
<td>(44%)</td>
<td>(31%)</td>
<td>(24%)</td>
<td></td>
</tr>
<tr>
<td><strong>INDIVIDUAL CONTRIBUTIONS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean contribution (st. dev.)</td>
<td>16.65</td>
<td>18.93</td>
<td>20.41</td>
<td>22.17</td>
<td></td>
</tr>
<tr>
<td>% of endowment contributed</td>
<td>66.61%</td>
<td>42.07%</td>
<td>31.40%</td>
<td>26.08%</td>
<td></td>
</tr>
<tr>
<td>Min contribution (strong freeriding) (%)</td>
<td>0 (6.25%)</td>
<td>0 (4.86%)</td>
<td>0 (5.56%)</td>
<td>0 (6.25%)</td>
<td></td>
</tr>
<tr>
<td>% of contributions, where $0 &lt; m_i &lt; 20$ (weak freeriding) (%)</td>
<td>(23.61%)</td>
<td>(16.67%)</td>
<td>(15.97%)</td>
<td>(14.58%)</td>
<td></td>
</tr>
<tr>
<td>% of contributions, where $m_i = 20$</td>
<td>(58.33%)</td>
<td>(61.81%)</td>
<td>(51.39%)</td>
<td>(50%)</td>
<td></td>
</tr>
<tr>
<td>% of contributions, where $20 &lt; m_i &lt; \text{max}$</td>
<td>(1.39%)</td>
<td>(15.98%)</td>
<td>(26.39%)</td>
<td>(27.78%)</td>
<td></td>
</tr>
<tr>
<td>Max contribution (%)</td>
<td>25 (10.42%)</td>
<td>45 (0.69%)</td>
<td>60 (0.96%)</td>
<td>60 (1.39%)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>144</td>
<td>144</td>
<td>144</td>
<td>144</td>
<td></td>
</tr>
<tr>
<td><strong>GROUP CONTRIBUTIONS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean contribution (st. dev.)</td>
<td>99.92</td>
<td>113.58</td>
<td>122.46</td>
<td>133.00</td>
<td></td>
</tr>
<tr>
<td>Groups Meeting Threshold</td>
<td>16.67%</td>
<td>41.67%</td>
<td>50.00%</td>
<td>62.50%</td>
<td></td>
</tr>
<tr>
<td>% of successful groups where $\sum_{i=1}^{N} m_i \geq T$</td>
<td>50.00%</td>
<td>70.00%</td>
<td>100%</td>
<td>100%</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>24</td>
<td>24</td>
<td>24</td>
<td>24</td>
<td></td>
</tr>
</tbody>
</table>
Table 3.3: p-values of Two-sample Kolmogorov-Smirnov for Equality of Contribution Distributions in Endowment Settings in the Menu Game:

<table>
<thead>
<tr>
<th>Endowment=25</th>
<th>Endowment = 45</th>
<th>Endowment = 65</th>
<th>Endowment = 85</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Observations)</td>
<td>0.505 (288)</td>
<td>0.009*** (288)</td>
<td>0.000*** (288)</td>
</tr>
<tr>
<td>Endowment=45</td>
<td>--</td>
<td>0.211 (288)</td>
<td>0.069* (288)</td>
</tr>
<tr>
<td>(Observations)</td>
<td>--</td>
<td>--</td>
<td>0.209 (288)</td>
</tr>
<tr>
<td>Endowment=65</td>
<td>--</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>(Observations)</td>
<td>--</td>
<td>--</td>
<td>--</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

Table 3.4: p-values of Two-sample Wilcoxon Rank-Sum Test Comparing Contributions in Endowment Settings in the Menu Game:

<table>
<thead>
<tr>
<th>Endowment = 25 vs Endowment = 45</th>
<th>Endowment = 45 vs Endowment = 65</th>
<th>Endowment = 65 vs Endowment = 85</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Observations)</td>
<td>0.0000*** (144)</td>
<td>0.0003*** (144)</td>
</tr>
<tr>
<td>Individual Contributions</td>
<td>0.0001*** (24)</td>
<td>0.0036*** (24)</td>
</tr>
<tr>
<td>(Observations)</td>
<td>--</td>
<td>--</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

Table 3.5: Logit Analysis of the Likelihood of Groups Meeting the Threshold in the Menu Game based on Endowment Setting

Output reported as Odds Ratios

<table>
<thead>
<tr>
<th>Dependent Variable: Meeting the Threshold</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dummies:</td>
</tr>
<tr>
<td>E = 45</td>
</tr>
<tr>
<td>E = 65</td>
</tr>
<tr>
<td>E = 85</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>P &gt; Chi²</td>
</tr>
</tbody>
</table>

Errors clustered at the group-level (24 clusters)
p-values in brackets
* p<.1, ** p<.05, *** p<.01
<table>
<thead>
<tr>
<th>Endowment</th>
<th>LOW (30 tokens)</th>
<th>HIGH (50 tokens)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Information</td>
<td>AGG</td>
<td>LOCAL</td>
</tr>
<tr>
<td>Part</td>
<td>II</td>
<td>III</td>
</tr>
<tr>
<td><strong>INDIVIDUAL CONTRIBUTIONS</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean contribution (st. dev.)</td>
<td>19.29 (7.44)</td>
<td>19.11 (7.24)</td>
</tr>
<tr>
<td>Min contribution (strong freeriding if $m_i=0$) (%)</td>
<td>0 (8.06%)</td>
<td>0 (7.22%)</td>
</tr>
<tr>
<td>% of contributions, where $0&lt;m_i&lt;20$ (weak freeriding)</td>
<td>10.83%</td>
<td>9.45%</td>
</tr>
<tr>
<td>% of contributions, where $m_i=20$</td>
<td>56.94%</td>
<td>61.11%</td>
</tr>
<tr>
<td>% of contributions, where $20&lt;m_i&lt;e$</td>
<td>12.78%</td>
<td>14.44%</td>
</tr>
<tr>
<td>Max contribution (%)</td>
<td>30 (11.39%)</td>
<td>30 (7.78%)</td>
</tr>
<tr>
<td>Observations</td>
<td>360</td>
<td>360</td>
</tr>
<tr>
<td><strong>GROUP CONTRIBUTIONS</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean contribution (st. dev.)</td>
<td>115.75 (13.04)</td>
<td>114.68 (13.49)</td>
</tr>
<tr>
<td>Groups Meeting Threshold</td>
<td>55.00%</td>
<td>60.00%</td>
</tr>
<tr>
<td>% of successful groups where $\sum_{i \in N} m_i \geq T$</td>
<td>36.36%</td>
<td>61.11%</td>
</tr>
<tr>
<td>Observations</td>
<td>60</td>
<td>60</td>
</tr>
</tbody>
</table>
Table 3.7: p-values of Paired T-tests of Group Contributions Within Treatments, Across Periods

<table>
<thead>
<tr>
<th></th>
<th>Period 1 versus 10</th>
<th>Period 2 versus 10</th>
<th>Period 10 versus 11</th>
<th>Period 11 versus 20</th>
</tr>
</thead>
<tbody>
<tr>
<td>LOCAL, LOW</td>
<td>0.038**</td>
<td>0.621</td>
<td>0.050**</td>
<td>0.032**</td>
</tr>
<tr>
<td>LOCAL, HIGH</td>
<td>0.917</td>
<td>0.076*</td>
<td>0.565</td>
<td>0.778</td>
</tr>
<tr>
<td>COMPLETE, LOW</td>
<td>0.020**</td>
<td>0.387</td>
<td>0.244</td>
<td>0.149</td>
</tr>
<tr>
<td>COMPLETE, HIGH</td>
<td>0.097*</td>
<td>0.026**</td>
<td>0.638</td>
<td>0.693</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

Table 3.8: p-values of Two-sample Wilcoxon Rank-Sum Test Comparing Contributions in LOW and HIGH Endowment Treatments

<table>
<thead>
<tr>
<th></th>
<th>Period 1</th>
<th>Periods 1-10</th>
<th>Periods 11-20</th>
</tr>
</thead>
<tbody>
<tr>
<td>Individual Contributions (Observations)</td>
<td>0.0003***</td>
<td>0.6244</td>
<td>0.7123</td>
</tr>
<tr>
<td>(Observations)</td>
<td>(144)</td>
<td>(144)</td>
<td>(144)</td>
</tr>
<tr>
<td>Group Contributions</td>
<td>0.0004***</td>
<td>0.0101**</td>
<td>0.0022***</td>
</tr>
<tr>
<td>(Observations)</td>
<td>(24)</td>
<td>(24)</td>
<td>(24)</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01
Table 3.9: Likelihood of Groups Meeting the Threshold in Part III

Output reported as Odds Ratios

<table>
<thead>
<tr>
<th>Dependent Variable: Meeting the Threshold</th>
<th>(1) Logit Per. 1</th>
<th>(2) RE Logit Per. 1-10</th>
<th>(3) RE Logit Per. 11-20</th>
<th>(4) RE Logit Per. 11-16</th>
<th>(5) RE Logit Per. 17-20 LOW</th>
<th>(6) RE Logit Per. 17-20 HIGH</th>
<th>(7) RE Logit Per. 17-20 HIGH</th>
</tr>
</thead>
<tbody>
<tr>
<td>HIGH Dummy</td>
<td>25.000*** [0.003]</td>
<td>2.137* [0.062]</td>
<td>3.381* [0.088]</td>
<td>4.224* [0.075]</td>
<td>2.089 [0.308]</td>
<td>2.445 [0.363]</td>
<td>2.445 [0.363]</td>
</tr>
<tr>
<td>COMPLETE Dummy</td>
<td>2.432 [0.196]</td>
<td>2.069 [0.339]</td>
<td>3.793* [0.087]</td>
<td>3.597* [0.094]</td>
<td>2.445 [0.363]</td>
<td>2.445 [0.363]</td>
<td>2.445 [0.363]</td>
</tr>
<tr>
<td>HIGH * COMPLETE</td>
<td>0.534 [0.539]</td>
<td>0.463 [0.498]</td>
<td>0.597 [0.671]</td>
<td>0.597 [0.671]</td>
<td>0.597 [0.671]</td>
<td>0.597 [0.671]</td>
<td>0.597 [0.671]</td>
</tr>
<tr>
<td>T Met in p10 Dummy</td>
<td>1.298 [0.653]</td>
<td>0.761 [0.677]</td>
<td>3.373** [0.040]</td>
<td>2.068 [0.321]</td>
<td>7.841** [0.034]</td>
<td>7.841** [0.034]</td>
<td>7.841** [0.034]</td>
</tr>
</tbody>
</table>

Observations 24 240 240 144 96 48 48

P > Chi² 0.0006 0.0619 0.2424 0.3587 0.0651 0.1681 0.0848

*p<.1, **p<.05, ***p<.01

Pooled errors clustered at the group level
Table 3.10.a: Random Effects Logit Models – Likelihood of Groups Contributing 1) Below; At 2) Asymmetric; 3) Symmetric); and 4) Above the Threshold in Part III

Output reported as Odds Ratios

<table>
<thead>
<tr>
<th>Independent Variable: Group Contributing:</th>
<th>(1) $\sum m_i &lt; T$</th>
<th>(2) $\sum m_i = T, m_i \neq m_j, i \neq j$</th>
<th>(3) $\sum m_i = T, m_i = m_j, i \neq j$</th>
<th>(4) $\sum m_i &gt; T$</th>
</tr>
</thead>
<tbody>
<tr>
<td>HIGH Dummy</td>
<td>0.531</td>
<td>0.0272*</td>
<td>0.847</td>
<td>6.505*</td>
</tr>
<tr>
<td></td>
<td>[0.380]</td>
<td>[0.052]</td>
<td>[0.955]</td>
<td>[0.059]</td>
</tr>
<tr>
<td>COMPLETE Dummy</td>
<td>0.382</td>
<td>0.233</td>
<td>690.8**</td>
<td>0.407</td>
</tr>
<tr>
<td></td>
<td>[0.142]</td>
<td>[0.236]</td>
<td>[0.032]</td>
<td>[0.406]</td>
</tr>
<tr>
<td>HIGH * COMPLETE</td>
<td>1.354</td>
<td>79.37**</td>
<td>0.0393</td>
<td>0.340</td>
</tr>
<tr>
<td></td>
<td>[0.757]</td>
<td>[0.048]</td>
<td>[0.431]</td>
<td>[0.406]</td>
</tr>
</tbody>
</table>

Group contribution in period 10 dummies:

<table>
<thead>
<tr>
<th>$\sum m_i &lt; 120$</th>
<th>0.582</th>
<th>4.04e-09***</th>
<th>0.472</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>[0.664]</td>
<td>[0.000]</td>
<td>[0.357]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>$\sum m_i = 120$ where $m_i \neq m_j$ where $i \neq j$</th>
<th>1.645</th>
<th>1.87e-07***</th>
<th>1.048</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>[0.461]</td>
<td>[0.000]</td>
<td>[0.957]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>$\sum m_i = 120$ where $m_i = m_j$ where $i \neq j$</th>
<th>0.270</th>
<th>3.85e-11</th>
<th>0.0197***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>[0.139]</td>
<td>[1.000]</td>
<td>[0.002]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>$\sum m_i &gt; 120$</th>
<th>0.608</th>
<th>1.945</th>
<th>4.95e-07***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>[0.435]</td>
<td>[0.577]</td>
<td>[0.000]</td>
</tr>
</tbody>
</table>

Observations | 240 | 240 | 240 | 240 |
P > Chi²      | 0.1526 | 0.6199 | 0.0013 | 0.0459 |

*p-values in brackets
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Data from periods 11-20
Table 3.10.b: *Pooled Multinomial Logit Models* – Likelihood of Groups Contributing 1) Below; 2) Asymmetric; 3) Symmetric; and 4) Above the Threshold in Part III

Output reported as Odds Ratios

<table>
<thead>
<tr>
<th>Independent Variable: Group Contributing:</th>
<th>(5) $\sum m_i = T$&lt;br&gt;$m_i \neq m_j$, $i \neq j$</th>
<th>(6) $\sum m_i = T$&lt;br&gt;$m_i = m_j$, $i \neq j$</th>
<th>(7) $\sum m_i &gt; T$</th>
</tr>
</thead>
<tbody>
<tr>
<td>HIGH Dummy</td>
<td>0.127*&lt;br&gt;[0.060]</td>
<td>2.233&lt;br&gt;[0.575]</td>
<td>3.468**&lt;br&gt;[0.017]</td>
</tr>
<tr>
<td>COMPLETE Dummy</td>
<td>0.865&lt;br&gt;[0.799]</td>
<td>11.98**&lt;br&gt;[0.021]</td>
<td>1.163&lt;br&gt;[0.644]</td>
</tr>
<tr>
<td>HIGH * COMPLETE</td>
<td>16.81*&lt;br&gt;[0.060]</td>
<td>0.322&lt;br&gt;[0.518]</td>
<td>0.551&lt;br&gt;[0.330]</td>
</tr>
</tbody>
</table>

Group contribution in period 10 dummies:

- $\sum m_i = 120$<br>where $m_i \neq m_j$, $i \neq j$<br>0.931<br>[0.892] | 0.323<br>[0.350] | 1.128<br>[0.757] |
- $\sum m_i = 120$<br>where $m_i = m_j$, $i \neq j$<br>1.78e-6***<br>[0.000] | 12.65*<br>[0.072] | 0.337**<br>[0.044] |
- $\sum m_i > 120$<br>2.723<br>[0.200] | 0.994<br>[0.996] | 1.897*<br>[0.061] |

Observations 240<br>Pseudo R² 0.184<br>P > Chi² 0.0000

*p-values in brackets*
* * $p<0.10$, ** $p<0.05$, *** $p<0.01$*

*Data from periods 11-20*
*Errors clustered at the group level*
*$\sum m_i < T$ outcomes used as baseline*
*$\sum m_i < 120$ in period 10 dummy used as comparison case*
### Table 3.11: Random Effects Regression on Change in Individual Contributions in Part III by Endowment Level

<table>
<thead>
<tr>
<th>Independent Variable: Change in $i$’s Contribution</th>
<th>(1) LOW</th>
<th>(2) HIGH</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>0.460 [0.470]</td>
<td>-0.362 [0.124]</td>
</tr>
<tr>
<td>LOCAL dummy</td>
<td>-0.289 [0.219]</td>
<td>0.262 [0.553]</td>
</tr>
<tr>
<td>Last period distance to T</td>
<td>-0.180*** [0.000]</td>
<td>-0.276*** [0.000]</td>
</tr>
<tr>
<td>LOCAL * Distance to T</td>
<td>0.107* [0.055]</td>
<td>0.00988 [0.832]</td>
</tr>
<tr>
<td>$\Delta$ in Viewable Contributions</td>
<td>0.413 [0.155]</td>
<td>0.827*** [0.000]</td>
</tr>
<tr>
<td>LOCAL * $\Delta$ in Viewable Contributions$^\dagger$</td>
<td>-0.252 [0.380]</td>
<td>-0.658*** [0.001]</td>
</tr>
<tr>
<td>Observations</td>
<td>576</td>
<td>576</td>
</tr>
</tbody>
</table>

*p-values in brackets*

* $p<0.10$, ** $p<0.05$, *** $p<0.01$*

*Data from periods 13-20*

*Period dummies included*

*Errors clustered at the group level*
FIGURES

Figure 3.1: LOCAL and COMPLETE Information Networks

Figure 3.2: Mean Individual Contributions in Menu Game by Endowment Level
Figure 3.3: Histogram of Individual Contribution Decisions in Menu Game (Part I) by Endowment Setting
Figure 3.4.a: Mean Group Contributions in the Repeated Game in LOCAL and COMPLETE Treatments, Part II and III (in tokens)
Figure 3.4.b: Mean Group Contributions in the Repeated Game in LOW and HIGH Treatments, Part II and III (in tokens)
Figure 3.5.a: Individual Contributions and Total Contributions in All Groups in the LOCAL Network LOW Endowment Treatment
Figure 3.5.b: Individual Contributions and Total Contributions in All Groups in the COMPLETE Network, LOW Endowment Treatment
Figure 3.5.c: Individual Contributions and Total Contributions in All Groups in the LOCAL Network, HIGH Endowment Treatment
Figure 3.5.d: Individual Contributions and Total Contributions in All Groups in the COMPLETE Network, HIGH Endowment Treatment
Figure 3.6.a: Histogram of Individual Contribution Decisions in Part II (Repeated Game, Periods 1-10) by Endowment Setting

Figure 3.6.b: Histogram of Individual Contribution Decisions in Part III (Repeated Game, Periods 11-20) by Endowment Setting and Information Treatment
Figure 3.7: Individual Mean Contributions in Part II and Part III

Figure 3.8: Percentage of Groups Meeting the Threshold in Part II and Part III
Figure 3.7: Percentage of Groups Contributing i) BELOW, ii) AT, and iii) ABOVE the Threshold
APPENDIX 2.1: VOTE TREATMENT

As described in the chapter, there were five treatments in the experiment. The fifth, which was not analyzed in the published paper, was a vote_before treatment. The treatment was designed to explore the impact group-decision-making (with regards to disclosing contributions) relative individual decision-making. As such it was designed to contrast with the vol_before treatment. Further, the treatment may be used to distinguish between selection and endogeneity effects (Dal Bó et al. 2010) of voluntary information disclosure – see discussion below.

Experimental Design

Vote Prior to Contribution (vote_before)

The vote_before treatment was based on a decision task identical to that of the other treatments. However, participants voted (every round prior to making their transfer decisions) on whether the group was required to make the individual contributions to the Group Fund public. If a simple majority (at least three individuals) voted in favor of publicizing contributions, the computer included individual transfer information at the end of each round. Once everyone had voted, all subjects received a message stating how many individuals had voted in favor of revealing transfers and whether individual contributions would be made public. At the end of the round, subjects received additional individual transfer information depending on the outcome of the vote.
The treatment was conducted during the same time frame as the treatments presented in Chapter II. Below is an overview of the treatments and how many groups were assigned to each treatment:

**Table A.2.2.1: Overview of Treatments (including Vote before Treatment)**

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Stage 1</th>
<th>Stage 2</th>
<th>Number of groups</th>
<th>Number of subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td>No_Discl</td>
<td>No information</td>
<td>No_Discl</td>
<td>11</td>
<td>55</td>
</tr>
<tr>
<td>Mandatory</td>
<td>No information</td>
<td>Mandatory</td>
<td>11</td>
<td>55</td>
</tr>
<tr>
<td>Vol_Sim</td>
<td>No information</td>
<td>Vol_Sim</td>
<td>7</td>
<td>35</td>
</tr>
<tr>
<td>Vol_Before</td>
<td>No information</td>
<td>Vol_Before</td>
<td>9</td>
<td>45</td>
</tr>
<tr>
<td>Vote</td>
<td>No information</td>
<td>Vote</td>
<td>8</td>
<td>40</td>
</tr>
</tbody>
</table>

**Conjectures**

*C.A1:* Groups will vote to disclose contributions with significant frequency.

This conjecture mirrors C1 from Chapter II, which posits that individuals will, more often than not, voluntarily reveal their contributions. The reasons for individuals to vote in favor of disclosure compared to unilaterally deciding to reveal their own contributions may be somewhat different, however. Consider that, on average, individuals may be swayed to do the ‘right’ thing when being observed (Levitt and List 2007), and this is common knowledge. Then, voting for disclosure becomes a strategy to change the incentives to encourage higher contributions. Thus individuals will tend to vote for disclosure.

*C.A2:* Voluntarily revealed contributions will be higher than hidden contributions.

This conjecture mirrors C2 from Chapter II. Recall that as scrutiny increases (when contributions are public), individuals will be more likely to be swayed by moral considerations (i.e., contribute highly). This suggests that average contributions in groups
that vote to reveal their actions will be higher than in groups that do not. However, not everyone in the group will have voted the same as the majority. It stands to reason that if someone voted to keep contributions hidden, but the majority voted in favor of disclosure, the individual may feel less inclined to be bound/affected by the institutional change. In fact, she may even resent not having had the majority on her side. Hence, individuals that vote against disclosing contributions, but where the group votes in favor will contribute less than those who are pro-disclosure.

C.A3: Groups in the vote_before treatment contribute, on average, similar amounts as groups in the vol_before treatment.

The results presented in Chapter II suggest that receiving a message about how many others are willing to disclose their contributions leads to the highest rate of cooperation in the experiment. The vote_before treatment also provides individuals with such a message. This treatment thus allows us to test the robustness of this messaging effect across different decision-making institutions.

Results

Comparing group-level contributions in Stage 2 across treatments, both the vol_before treatment and the vote_before treatment result in contributions that are significantly higher at the 5% level (Wilcoxon-Mann-Whitney test) than contributions in the no disclosure treatment (see Table A.2.2.2). This implies that receiving a message informing of others’ decisions to reveal (and correspondingly the ability to signal one’s own cooperativeness), rather than voluntarily reveal contributions, improves cooperation. To take account of possible group effects (there is a significant difference in Stage 1
contributions between the mandatory and vote_before treatments), I compare the
difference in contribution levels (between Stage 1 and Stage 2) across treatments. All
treatments result in a significantly greater (at the 10% level) increase in contributions
compared to the no information treatment. This indicates that greater information does
result in higher cooperation, with the effect magnified when subjects are better able to
signal.

Table A.2.2.2: Summary Statistics: Group Level Contributions by Stage and
Treatment (including Vote_before Treatment)

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Stage 1</th>
<th></th>
<th></th>
<th>Stage 2</th>
<th></th>
<th></th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td># of tokens (standard deviation)</td>
<td>% of max</td>
<td></td>
<td># of tokens (standard deviation)</td>
<td>% of max</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No_Discl</td>
<td>396.18 (227.84)</td>
<td>31.69%</td>
<td></td>
<td>365.82 (203.14)</td>
<td>29.27%</td>
<td>-2.42%</td>
<td></td>
</tr>
<tr>
<td>Mandatory</td>
<td>327.36 (117.85)</td>
<td>26.19%</td>
<td></td>
<td>474.27 (327.01)</td>
<td>37.94%</td>
<td>11.75%*</td>
<td></td>
</tr>
<tr>
<td>Vol_Sim</td>
<td>376.29 (151.09)</td>
<td>30.10%</td>
<td></td>
<td>512.29 (275.21)</td>
<td>40.98%</td>
<td>10.88%**</td>
<td></td>
</tr>
<tr>
<td>Vol_Before</td>
<td>451.89 (156.97)</td>
<td>36.15%</td>
<td></td>
<td>727 (292.25)</td>
<td>58.16%</td>
<td>22.01%***</td>
<td></td>
</tr>
<tr>
<td>Vote_Before</td>
<td>484.75 (188.65)</td>
<td>38.79%</td>
<td></td>
<td>768.25 (398.69)</td>
<td>61.46%</td>
<td>22.68%**</td>
<td></td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

Note: N refers to the number of groups. The percentages refer to group-level
crowds to the Group Fund compared to the maximum possible contributions (1250
over ten rounds).

Table A.2.2.3 above shows the summary statistics for individual contribution rates for
each of the different treatments accounting for whether contributions were public or
private. Several interesting results are worth highlighting. First, in support of C.A2, there
is a pronounced difference in mean private and public contributions, indicating that
individuals seek to hide free-riding behavior. Further, in the vote_before treatment there
is a stark difference in contributions between those that vote for and against disclosure
(especially when the majority is in favor of disclosure). Finally, the data suggest that the
frequency of contributing the minimum and maximum number of tokens is also dependent on both the publicity of the contributions and the individual’s vote. These results will be further examined in the re-visititation of the hypotheses below.

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Contribution is:</th>
<th>Obs.</th>
<th>Mean Contr. (Std. Dev)</th>
<th>Min Contr. (Frequency)</th>
<th>Max Contr. (Frequency)</th>
</tr>
</thead>
<tbody>
<tr>
<td>No Disclosure</td>
<td>Private</td>
<td>550</td>
<td>7.32 (8.49)</td>
<td>0 (38.18%)</td>
<td>25 (10.55%)</td>
</tr>
<tr>
<td></td>
<td>Public</td>
<td>550</td>
<td>9.49 (9.54)</td>
<td>0 (34.18%)</td>
<td>25 (17.09%)</td>
</tr>
<tr>
<td>Mandatory</td>
<td>Public</td>
<td>550</td>
<td>14.85 (8.37)</td>
<td>0 (11.56%)</td>
<td>25 (20.89%)</td>
</tr>
<tr>
<td>Voluntary Simultaneous to Contribution</td>
<td>Public</td>
<td>225</td>
<td>1.96 (3.88)</td>
<td>0 (67.20%)</td>
<td>20 (0.80%)</td>
</tr>
<tr>
<td></td>
<td>Private</td>
<td>125</td>
<td>10.25 (9.41)</td>
<td>0 (31.43%)</td>
<td>25 (13.43%)</td>
</tr>
<tr>
<td></td>
<td>Combined</td>
<td>350</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Voluntary Prior to Contribution</td>
<td>Public</td>
<td>336</td>
<td>18.72 (8.10)</td>
<td>0 (8.93%)</td>
<td>25 (49.40%)</td>
</tr>
<tr>
<td></td>
<td>Private</td>
<td>114</td>
<td>2.23 (5.07)</td>
<td>0 (71.93%)</td>
<td>25 (2.63%)</td>
</tr>
<tr>
<td></td>
<td>Combined</td>
<td>450</td>
<td>14.54 (10.34)</td>
<td>0 (24.89%)</td>
<td>25 (37.56%)</td>
</tr>
<tr>
<td>Vote Prior to Contribution</td>
<td>Public (ALL)</td>
<td>350</td>
<td>17.14 (9.57)</td>
<td>0 (14.57%)</td>
<td>25 (48.29%)</td>
</tr>
<tr>
<td></td>
<td>Voted: YES</td>
<td>297</td>
<td>18.70 (8.49)</td>
<td>0 (8.42%)</td>
<td>25 (52.53%)</td>
</tr>
<tr>
<td></td>
<td>Voted: NO</td>
<td>53</td>
<td>8.38 (10.63)</td>
<td>0 (49.06%)</td>
<td>25 (24.53%)</td>
</tr>
<tr>
<td></td>
<td>Private (ALL)</td>
<td>50</td>
<td>2.96 (6.30)</td>
<td>0 (60%)</td>
<td>25 (6%)</td>
</tr>
<tr>
<td></td>
<td>Voted: YES</td>
<td>20</td>
<td>4.7 (7.60)</td>
<td>0 (45%)</td>
<td>25 (10%)</td>
</tr>
<tr>
<td></td>
<td>Voted: NO</td>
<td>30</td>
<td>1.8 (5.06)</td>
<td>0 (70%)</td>
<td>25 (3.33%)</td>
</tr>
<tr>
<td></td>
<td>All Combined</td>
<td>400</td>
<td>15.37 (10.35)</td>
<td>0 (20.25%)</td>
<td>25 (43%)</td>
</tr>
</tbody>
</table>

*Frequency of disclosure*
There are a high number of votes in favor of disclosure (see Figure A.2.2.1). At least 75% of individuals vote to reveal contributions throughout the treatment. Toward the end of the experiment (periods 19 and 20) all individuals in the treatment vote to disclose contributions. Actual disclose rates track these findings. There are only ten instances, in total, where a majority decides to keep contributions hidden.

Figure A.2.2.1: Percentage of Individuals Voting in Favor of Disclosure

Public Contributions

Figure A.2.2.2 shows the mean contributions in the vote_before treatment given whether contributions were public and how the individual voted. In support of C.A2, mean contributions are significantly higher when they are public than when the majority votes in favor to keep contributions hidden. However, within these scenarios, contributions from individuals that voted for an against disclosure vary widely. To explore this further, Table A.2.2.4 separates the vote_before treatment dummy variable into four separate

63 This information can also be inferred from Table A.2.2.3.
dummy variables: Variable $v1$ equals one if individuals vote in favor of disclosure and the majority does too; $v2$ equals one when an individual voted against making contributions public but the group decides to disclose information; $v3$ equals one when individuals favor public contributions but the group decides to keep this information private; and $v4$ equals one when both individuals and the group favor keeping contributions private. Two-tailed t-tests (significance results shown in A.2.2.5) reveal that there is a significant difference between contributions by $v1$ individuals and by $v3$ individuals. This effect might be attributable to expecting less cooperation from a group that voted to keep contributions private. Interestingly, the difference between $v2$ and $v3$ is not statistically significant (in the random effects model) suggesting that knowing that one is in a cooperative group may improve cooperation to a similar extent as being a cooperative type yet knowing that the group itself is uncooperative. Finally, contributions under $v4$ are significantly different from those when it has been endogenously decided to make contributions public ($v1$ and $v2$).

Group versus Individual Decision-making

A further conclusion may be drawn from the analysis above: There is no significant difference between contribution levels in the vol_before and vote_before treatments, even when accounting for the level of behavioral information (i.e., how many individuals decide to, or vote in favor to, disclose contributions). Hence, C.A3 is supported – a democratic implementation of disclosure is similarly effective as individual decisions to reveal contributions.

Figure A.2.2.2: Mean Individual Contributions in Vote_before Treatment (in Tokens) by Voting Decision and Vote Outcome
Table A.2.2.4: Regression Analyses of Individual Level Contributions Given Public and Private Contributions

<table>
<thead>
<tr>
<th></th>
<th>(1) RE</th>
<th></th>
<th>(2) Tobit †</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>10.29***</td>
<td>9.23***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.56)</td>
<td>(1.01)</td>
<td></td>
</tr>
<tr>
<td>Mandatory</td>
<td>3.33</td>
<td>5.79***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.05)</td>
<td>(0.95)</td>
<td></td>
</tr>
<tr>
<td><strong>Vol_sim</strong> Public</td>
<td>5.98***</td>
<td>8.97***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.94)</td>
<td>(1.17)</td>
<td></td>
</tr>
<tr>
<td><strong>Vol_sim</strong> Private</td>
<td>-1.62</td>
<td>-4.54***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.20)</td>
<td>(1.63)</td>
<td></td>
</tr>
<tr>
<td><strong>Vol_before</strong> Public</td>
<td>9.08***</td>
<td>14.68***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.90)</td>
<td>(1.06)</td>
<td></td>
</tr>
<tr>
<td><strong>Vol_before</strong> Private</td>
<td>-1.95</td>
<td>-5.15***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.21)</td>
<td>(1.60)</td>
<td></td>
</tr>
<tr>
<td>Vote for Public Public (v1)</td>
<td>8.84***</td>
<td>14.93***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.38)</td>
<td>(1.11)</td>
<td></td>
</tr>
<tr>
<td>Vote for Private Public (v2)</td>
<td>1.89</td>
<td>3.14</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.59)</td>
<td>(1.97)</td>
<td></td>
</tr>
<tr>
<td>Vote for Public Private (v3)</td>
<td>-1.74</td>
<td>-3.56</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.43)</td>
<td>(3.11)</td>
<td></td>
</tr>
<tr>
<td>Vote for Private Private (v4)</td>
<td>-2.21</td>
<td>-6.05*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.68)</td>
<td>(3.11)</td>
<td></td>
</tr>
</tbody>
</table>

Periods included: 1-20 1-20

R-sq:
Within 0.2107
Between 0.3446
Overall 0.2334

Observations: 4600 4600
Left-censored 1400
Uncensored 2437
Right-censored 763

* p<.1, ** p<.05, *** p<.01
Standard errors are in parentheses.
Random effects and fixed effects models use errors clustered at the group-level.
Dummies for rounds were included in the regressions but omitted in table.
† Tobit model errors not clustered at the group-level. Lower limit = 0, upper limit = 25
### Table A.2.2.5: Paired Significance Tests of Coefficients in Models in Table 6

<table>
<thead>
<tr>
<th></th>
<th>Public</th>
<th></th>
<th>Private</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Vote for Public (v1)</td>
<td>Vote for Private (v2)</td>
<td>Vote for Public (v3)</td>
<td>Vote for Private (v4)</td>
</tr>
<tr>
<td>No disclosure</td>
<td>***</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td></td>
<td>(***</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Mandatory</td>
<td>**</td>
<td>.</td>
<td>*</td>
<td>***</td>
</tr>
<tr>
<td></td>
<td>(***</td>
<td>.</td>
<td>(***</td>
<td>(***</td>
</tr>
<tr>
<td>Vol_sim</td>
<td></td>
<td></td>
<td>***</td>
<td>***</td>
</tr>
<tr>
<td>Public</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td></td>
<td>(***</td>
<td>(***</td>
<td>(***</td>
<td>(***</td>
</tr>
<tr>
<td>Vol_sim</td>
<td></td>
<td></td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Private</td>
<td>***</td>
<td>(**)</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Vol_before</td>
<td></td>
<td></td>
<td>***</td>
<td>***</td>
</tr>
<tr>
<td>Public</td>
<td>.</td>
<td>***</td>
<td>.</td>
<td>.</td>
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<tr>
<td></td>
<td>(***</td>
<td>(***</td>
<td>(***</td>
<td>(***</td>
</tr>
<tr>
<td>Vol_before</td>
<td></td>
<td></td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Private</td>
<td>***</td>
<td>(***</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Vote for Public</td>
<td></td>
<td></td>
<td>***</td>
<td>***</td>
</tr>
<tr>
<td>Public (v1)</td>
<td>***</td>
<td>(***</td>
<td>(***</td>
<td>(***</td>
</tr>
<tr>
<td>Vote for Private</td>
<td></td>
<td></td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Public (v2)</td>
<td>***</td>
<td>(***</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Vote for Public</td>
<td></td>
<td></td>
<td>***</td>
<td>(***</td>
</tr>
<tr>
<td>Private (v3)</td>
<td>***</td>
<td>(**</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>Vote for Private</td>
<td></td>
<td></td>
<td>***</td>
<td>.</td>
</tr>
<tr>
<td>Private (v4)</td>
<td>***</td>
<td>(***</td>
<td>(***</td>
<td>(***</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

Table shows paired test of significance levels from random effects model (Table A.2.2.4, Model 1) and from Tobit model (Table A.2.2.4, Model 2) shown in parentheses

**The Impact of Voting**

Using the data from the vote_before treatment, the effect from democratically implementing a disclosure policy can be further separated into the selection effect (the effect arising from individuals that vote in favor of disclosure have an inclination to contribute more highly) and the endogeneity effect (the effect of endogenously implementing the policy). Following the methodology put forward in Dal Bó et al. (2010) I use the data from Table A.2.2.3 to calculate these effects: If \( g(v) \) denotes the proportion
of individuals that found themselves in scenarios \( v \in \{ v_1, v_2, v_3, v_4 \} \) given a disclosure setting, and the \( c(v) \) denotes the corresponding the contribution levels by these individuals, the total effect from a democratically implemented disclosure policy is:

\[
Total \ Effect = g(v_1)c(v_1) - g(v_3)c(v_3) + g(v_2)c(v_2) - g(v_4)c(v_4)
\]

(Equation A1)

This results in 14.18 tokens being contributed to the public good on account of a democratically implemented disclosure policy, which is higher than the contribution rate in the no information treatment, suggesting the effectiveness of allowing institutional choice to increase cooperation. This effect may be further separated. The selection effect (Equation A2) accounts for 1.30 of the total effect. Which, in turn, implies that the majority of the effect of democratically implementing disclosure is due to the endogeneity effect (Equation A3). 12.88 tokens are the result of allowing individuals to choose disclosure.

\[
Selection \ Effect = g(v_1)c(v_3) - g(v_3)c(v_3) + g(v_2)c(v_4) - g(v_4)c(v_4)
\]

(Equation A2)

\[
Endogeneity \ Effect = g(v_1)c(v_1) - g(v_1)c(v_3) + g(v_2)c(v_2) - g(v_2)c(v_4)
\]

(Equation A3)

Inferring that institutional choice is a panacea, however, is premature because there are few observations of \( v_3 \) and \( v_4 \) (most groups chose to disclose contributions). Furthermore, the data does not allow me to contrast the endogenous treatment effect to the exogenous treatment effect (Dal Bó et al. 2010) because there is no information on voting behavior when disclosure is mandated (i.e. the vote always determines disclosure.
policy). Hence, it is not clear, from this analysis alone, how much institutional choice improves upon mandated policy.

Discussion

The vote_before treatment provides some interesting insights into disclosure and contribution behavior. When a group votes in favor of disclosure, average contributions are higher than when disclosure is mandated. The level of contributions is similar to that of individuals who choose to reveal their contributions in the vol_before treatment. Within democratically agreed upon disclosure, individuals that voted in favor contribute significantly more than those that did not. In fact, individuals that vote against disclosure contribute similar amounts to individuals in the mandatory treatment. In groups that vote to keep contributions private, however, individuals that vote for and against this notion do not contribute significantly different amounts to the public good. However, further research is necessary to estimate the full impact of voting for disclosure – data on individuals’ inclinations (i.e., how they would vote) while the disclosure decision is mandated. This would allow for a more truthful comparison between outcomes when disclosure mandated and when it is democratically elected (see discussion above and Dal Bô et al 2010).
References


APPENDIX 2.2: EXPERIMENTAL INSTRUCTIONS

[Please note: Writing in square brackets will not be seen by subjects. --- refers to a new screen.]

Now that the experiment has begun, please make sure that your cell phones are switched off. We ask that you do not talk with one another. If you have a question after reading the instructions, please raise your hand and the experimenter will answer your question in private. Also, during the experiment, please do not turn around or peer onto the screens of other participants.

------------------------------------------------------------------------------------------

Welcome
You will receive $5 for showing up to this experiment. In addition, you can earn up to about $30 by participating in this experiment, which will take about an hour. Please read the instructions carefully, as the amount of money you earn depends on your decisions during this experiment as well as the decisions of your group members.

Today’s experiment will consist of a number of stages. The instructions that follow describe the first stage. You will receive instructions for the second stage after completing the first.

------------------------------------------------------------------------------------------

[These are instructions for the “No Disclosure” baseline treatment as all subjects will be participating in these for the first ten rounds]

Stage 1 Instructions
Stage 1 will consist of several rounds of the decision task described below. Before making actual decisions that affect your earnings, you will answer a short quiz designed to check your understanding of the decision task.

You have been randomly assigned to a group of 5 persons. You will remain in this group until the end of the experiment. Each group member has been assigned a number, 1 through 5. You will keep this number throughout the experiment.

------------------------------------------------------------------------------------------
**Starting Balances**

Everyone in your group has an *Individual Fund* and your group of five has a *Group Fund*. Each **person** receives 25 tokens at the beginning of each round. Both the *Individual Fund* and the *Group Fund* begin with 0 tokens in them. At the end of each round, your earnings (described on a following screen) will be banked, and the number of tokens in each fund will reset to 0. Your final earnings for Stage 1 will be the sum of the amounts that you banked after each round.

---

**Decision Task**

Each person will decide privately how many (if any) of his/her 25 tokens to move to his/her *Individual Fund* and how many (if any) to move to the *Group Fund*. Each token that is placed in one’s own *Individual Fund* will increase one’s earnings by $0.02. Every token that is moved to the *Group Fund* will increase every group member’s earnings by $0.01 each. The total number of tokens moved must be 25, and each person’s decisions must be in whole tokens (0, 1, 2, ..., 23, 24, 25).

---

**Earnings**

Let us now explain how your earnings are calculated. In each group of five, an individual’s earnings per round will be:

\[
0.02 \times \text{Number of tokens in that person’s Individual Fund} + 0.01 \times \text{Number of tokens in the Group Fund}
\]

**Examples to illustrate earnings:**

<table>
<thead>
<tr>
<th>Example 1: Every person in the group moves 6 tokens to the Group Fund.</th>
</tr>
</thead>
<tbody>
<tr>
<td>• There are 30 tokens are in the Group Fund.</td>
</tr>
<tr>
<td>• Everyone receives $0.68 this round, because:</td>
</tr>
<tr>
<td>$0.02 \times 19 \text{[tokens in Individual Fund]}</td>
</tr>
<tr>
<td>\ + \ $0.01 \times 30 \text{[tokens in Group Fund]}</td>
</tr>
<tr>
<td>\ = \ $0.68</td>
</tr>
<tr>
<td>• The total amount earned in the group this round is $3.40.</td>
</tr>
</tbody>
</table>
Example 2: Person 1 moves 19 tokens to the Group Fund and everyone else moves 3 each.

- There are 31 tokens in the Group Fund.
- Person 1 receives $0.43 this round:
  $$0.02 \times 6 \text{ tokens in Individual Fund} + 0.01 \times 31 \text{ tokens in Group Fund} = 0.43$$
- Everyone else receives $0.75:
  $$0.02 \times 22 \text{ tokens in Individual Fund} + 0.01 \times 31 \text{ tokens in Group Fund} = 0.75$$
- The total amount earned in the group this round is $3.43.

Example 3: Person 1 moves nothing into the Group Fund, and all other group members move 18 tokens each.

- There are 72 tokens in the Group Fund.
- Person 1 receives $1.22 this round:
  $$0.02 \times 25 \text{ tokens in Individual Fund} + 0.01 \times 72 \text{ tokens in Group Fund} = 1.22$$
- The other group members receive $0.86 each:
  $$0.02 \times 7 \text{ tokens in Individual Fund} + 0.01 \times 72 \text{ tokens in Group Fund} = 0.86$$
- The total amount earned in the group this round is $4.66.

Information
At the end of each round, you will receive an overview of your decisions and earnings. You will also receive information on the total number of tokens moved to the Group Fund. You will not receive information on the individual decisions of your group members.

Anonymity
Please note that information about your transfer decisions will not be personally identifiable: Your decisions will not be shown to your group.

If you have any questions please raise your hand, and the experimenter will answer your questions in private. Do you have any questions so far?
QUIZ

[After 10 rounds of Stage 1, subjects will see the following instructions. Subjects will receive different instructions depending on the treatment to which they have been randomly assigned.]
Stage 2 Instructions [No Disclosure Treatment]
You have now completed Stage 1 of the experiment. You will now participate in Stage 2, which will also consist of several decision rounds. The decision task will be the same as in Stage 1. Before making actual decisions that affect your earnings, you will answer another short quiz designed to check your understanding of the decision task.

You are still in the same group of 5 persons to which you were randomly assigned at the beginning of the experiment. Your participant number is the same as in Stage 1.

------------------------------------------------------------------------------------------------------------

Decision Task
Each person will decide privately how many (if any) of his/her 25 tokens to move to his/her Individual Fund and how many (if any) to move to the Group Fund. Each token that is placed in one’s own Individual Fund will increase one’s earnings by $0.02. Every token that is moved to the Group Fund will increase every group member’s earnings by $0.01 each. The total number of tokens moved must be 25, and each person’s decisions must be in whole tokens (0, 1, 2, ..., 23, 24, 25).

Earnings
Earnings are calculated in the same way as they were calculated in the first stage of the experiment: In each group of five, an individual’s earnings per round will be:

$0.02 \times \text{Number of tokens in that person's Individual Fund} +
$0.01 \times \text{Number of tokens in the Group Fund}

------------------------------------------------------------------------------------------------------------

Information [No Disclosure Treatment]
At the end of each round, you will receive an overview of your decisions and earnings. You will also receive information on the total number of tokens moved to the Group Fund. You will not receive information on the individual decisions of your group members.

Anonymity
Please note that information about your transfer decisions will not be personally identifiable: Your decisions will not be shown to your group.

If you have any questions please raise your hand, and the experimenter will answer your questions in private. Do you have any questions so far?

------------------------------------------------------------------------------------------------------------

QUIZ
**Stage 2 Instructions [Mandatory Disclosure Treatment]**

You have now completed Stage 1 of the experiment. You will now participate in Stage 2, which will also consist of several decision rounds. The decision task will be the same as in Stage 1. Before making actual decisions that affect your earnings, you will answer another short quiz designed to check your understanding of the decision task.

You are still in the same group of 5 persons to which you were randomly assigned at the beginning of the experiment. Your participant number is the same as in Stage 1.

------------------------------------------------------------------------------------------------------------

**Decision Task**

Each person will decide privately how many (if any) of his/her 25 tokens to move to his/her Individual Fund and how many (if any) to move to the Group Fund. Each token that is placed in one’s own Individual Fund will increase one’s earnings by $0.02. Every token that is moved to the Group Fund will increase every group member’s earnings by $0.01 each. The total number of tokens moved must be 25, and each person’s decisions must be in whole tokens (0, 1, 2, ..., 23, 24, 25).

**Earnings**

Earnings are calculated in the same way as they were calculated in the first stage of the experiment: In each group of five, an individual’s earnings per round will be:

\[
$0.02 \times \text{Number of tokens in that person’s Individual Fund} + $0.01 \times \text{Number of tokens in the Group Fund}
\]

------------------------------------------------------------------------------------------------------------

**Information [Mandatory Disclosure Treatment]**

At the end of each round, you will receive an overview of your decisions and earnings. You will also receive information on the total number of tokens moved to the Group Fund. Furthermore, you will receive information on the individual decisions of your group members using their participant numbers.

**Anonymity**

Please note that information about your transfer decisions will not be personally identifiable: Your decisions will be shown to your group using only your participant number, not your name.

If you have any questions please raise your hand, and the experimenter will answer your questions in private. Do you have any questions so far?

------------------------------------------------------------------------------------------------------------

**QUIZ**
Stage 2 Instructions [Voluntary Simultaneous to Contribution]
You have now completed Stage 1 of the experiment. You will now participate in Stage 2, which will also consist of several decision rounds. The decision task for the next rounds is similar to the previous rounds, but some important changes have been made. Before making actual decisions that affect your earnings, you will answer another short quiz designed to check your understanding of the decision task.

You are still in the same group of 5 persons to which you were randomly assigned at the beginning of the experiment. Your participant number is the same as in Stage 1.

------------------------------------------------------------------------------------------------------------

Decision Task [Voluntary Simultaneous to Contribution]
Each person will decide privately how many (if any) of his/her 25 tokens to move to his/her Individual Fund and how many (if any) to move to the Group Fund. Each token that is placed in one’s own Individual Fund will increase one’s earnings by $0.02. Every token that is moved to the Group Fund will increase every group member’s earnings by $0.01 each. The total number of tokens moved must be 25, and each person’s decisions must be in whole tokens (0, 1, 2, ..., 23, 24, 25).

Every round, when a participant makes his/her decision on how many tokens to move to his/her Individual Fund and to the Group Fund, he/she must also decide whether he/she wishes to make their transfer to the Group Fund public. Along with the information everyone receives at the end of each round, the computer will include the individual transfer decisions of the group members who chose to make their decision public.

Earnings
Earnings are calculated in the same way as they were calculated in the first stage of the experiment: In each group of five, an individual’s earnings per round will be:

$0.02 \times \text{Number of tokens in that person’s Individual Fund} + $0.01 \times \text{Number of tokens in the Group Fund}

------------------------------------------------------------------------------------------------------------

Information [Voluntary Simultaneous to Contribution]
At the end of each round, you will receive an overview of your decisions and earnings. You will also receive information on the total number of tokens moved to the Group Fund. Furthermore, you will receive information on the individual decisions of the group members (using their participant numbers) who have agreed to reveal that information.

Anonymity
Please note that information about your transfer decisions will not be personally identifiable: Your decisions will be shown to your group (in the event that you decide to make them public) using only your participant number, not your name.
If you have any questions please raise your hand, and the experimenter will answer your questions in private. **Do you have any questions so far?**

---

**QUIZ**
Stage 2 Instructions [Voluntary Prior to Contribution]

You have now completed Stage 1 of the experiment. You will now participate in Stage 2, which will also consist of several decision rounds. The decision task for the next rounds is similar to the previous rounds, but some important changes have been made. Before making actual decisions that affect your earnings, you will answer another short quiz designed to check your understanding of the decision task.

You are still in the same group of 5 persons to which you were randomly assigned at the beginning of the experiment. Your participant number is the same as in Stage 1.

Decision Task [Voluntary Prior to Contribution]

Each person will decide privately how many (if any) of his/her 25 tokens to move to his/her Individual Fund and how many (if any) to move to the Group Fund. Each token that is placed in one’s own Individual Fund will increase one’s earnings by $0.02. Every token that is moved to the Group Fund will increase every group member’s earnings by $0.01 each. The total number of tokens moved must be 25, and each person’s decisions must be in whole tokens (0, 1, 2, ..., 23, 24, 25).

Every round, each participant must decide whether he/she wishes to make their transfer to the Group Fund public. Every group member receives a message stating how many individuals decided to make their contributions public. Then each participant makes his/her decision on how many tokens to move to his/her Individual Fund and to the Group Fund. Along with the information everyone receives at the end of each round, the computer will include the individual transfer decisions of the group members who chose to make their decision public.

Earnings

Earnings are calculated in the same way as they were calculated in the first stage of the experiment: In each group of five, an individual’s earnings per round will be:

$0.02 \times \text{Number of tokens in that person’s Individual Fund}$

$+\ 

$0.01 \times \text{Number of tokens in the Group Fund}$

Information [Voluntary Prior to Contribution]

At the end of each round, you will receive an overview of your decisions and earnings. You will also receive information on the total number of tokens moved to the Group Fund. Furthermore, you will receive information on the individual decisions of the group members (using their participant numbers) who have agreed to reveal that information.
Anonymity

Please note that information about your transfer decisions will not be personally identifiable: Your decisions will be shown to your group (in the event that you decide to make them public) using only your participant number, not your name.

If you have any questions please raise your hand, and the experimenter will answer your questions in private. Do you have any questions so far?

QUIZ
Stage 2 Instructions [Vote Treatment]
You have now completed Stage 1 of the experiment. You will now participate in Stage 2, which will also consist of several decision rounds. The decision task for the next rounds is similar to the previous rounds, but some important changes have been made. Before making actual decisions that affect your earnings, you will answer another short quiz designed to check your understanding of the decision task.

You are still in the same group of 5 persons to which you were randomly assigned at the beginning of the experiment. Your participant number is the same as in Stage 1.

------------------------------------------------------------------------------------------------------------

Decision Task [Vote treatment]
Each person will decide privately how many (if any) of his/her 25 tokens to move to his/her Individual Fund and how many (if any) to move to the Group Fund. Each token that is placed in one’s own Individual Fund will increase one’s earnings by $0.02. Every token that is moved to the Group Fund will increase every group member’s earnings by $0.01 each. The total number of tokens moved must be 25, and each person’s decisions must be in whole tokens (0, 1, 2, ..., 23, 24, 25).

Every round, each participant must vote on whether all individuals in the group have to make their transfer to the Group Fund public. If three or more participants (a majority) vote in favor of revealing transfers, the computer includes individual transfer decisions with the information everyone receives at the end of each round.

Every group member receives a message stating how many individuals voted for public contributions and whether the contributions will be public. Then each participant makes his/her decision on how many tokens to move to his/her Individual Fund and to the Group Fund. Along with the information everyone receives at the end of each round, the computer will include the individual transfer decisions of the group members who chose to make their decision public.

Earnings
Earnings are calculated in the same way as they were calculated in the first stage of the experiment: In each group of five, an individual’s earnings per round will be:

\[
\text{\$0.02 \times Number of tokens in that person's Individual Fund} + \text{\$0.01 \times Number of tokens in the Group Fund}
\]
**Information [Vote Treatment]**

At the end of each round, you will receive an overview of your decisions and earnings. You will also receive information on the total number of tokens moved to the Group Fund. Furthermore, you will receive information on the individual decisions of your group members (using their participant numbers) if the group has voted in favor of revealing transfers.

**Anonymity**

Please note that information about your transfer decisions will not be personally identifiable: Your decisions will be shown to your group (in the event that the group voted to make them public) using only your participant number, **not** your name.

If you have any questions please raise your hand, and the experimenter will answer your questions in private. **Do you have any questions so far?**

------------------------------------------------------------------------------------------------------------

**QUIZ**
APPENDIX 3.1: ORDERING EFFECTS IN PART I (MENU GAME)

Overview

There is evidence that the order of presentation of the different games or setting in a multi-stage experiment affects behavior (see, for example, Rapoport 1997 and Camerer 2003). However, this has been predominantly tested in settings where individuals make their decision about one game before moving on to the next game. There have been few studies that test for ordering effects in menu game designs (such as the game in which subjects participated in Part I). Utilizing the randomization of orderings of endowment settings in Part I, we test for these ordering effects. We find little evidence for consistent ordering effects. This suggests that presenting subjects with all games/settings simultaneously may help alleviate ordering effects. However, further research needs to be conducted to determine the potential for ordering effects in situations with sequential decision-making when endowments vary. Further, our analysis indicates that order does not affect the endowment effect identified and discussed in Chapter 3. We hence do not include ordering effects in that discussion.

Decision Setting

In Part I subjects were presented all decision scenarios (i.e. different endowment levels) on one screen and made their contribution decisions on one screen as well. In order to test for any potential ordering effects arising from subjects facing decision scenarios in ascending or descending order of endowments, we implemented a Latin square design (see, for example, Cochran and Cox 1957) resulting in the following four menu decision
treatment orderings. These orderings are presented in Table A.3.1.1. Once subjects had made their contribution decisions, the experiment proceeded to the next stage without receiving any feedback on their earnings or their group members’ contributions.

### Table A.3.1.1: Part I Decision Scenario Ordering

<table>
<thead>
<tr>
<th>Decision Scenario</th>
<th>Ordering 1</th>
<th>Ordering 2</th>
<th>Ordering 3</th>
<th>Ordering 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Decision A</td>
<td>25</td>
<td>45</td>
<td>65</td>
<td>85</td>
</tr>
<tr>
<td>Decision B</td>
<td>45</td>
<td>65</td>
<td>85</td>
<td>25</td>
</tr>
<tr>
<td>Decision C</td>
<td>65</td>
<td>85</td>
<td>25</td>
<td>45</td>
</tr>
<tr>
<td>Decision D</td>
<td>85</td>
<td>25</td>
<td>45</td>
<td>65</td>
</tr>
</tbody>
</table>

**Analysis**

For the analysis we construct the variable “position,” that records in which position an endowment level setting was seen. For example, in Ordering 1 endowment = 25 tokens was seen first. This means that in Ordering 1 position = 1 for endowment = 25. Similarly, position = 4 for endowment = 85 tokens in Ordering 1. This allows testing whether seeing any endowment first or last (etc.) significantly changes a subject’s level of contributions. The summary statistics and graphs below present mean contributions across the different endowment levels and positions.

The summary statistics and graph (Figure A.3.1.1.a) suggest that there are no clear differences across the different positions within each endowment setting. With regards to within each position, Figure A.3.1.1.b suggests that the endowment effect (see Chapter 3, above) is not systematically effected by ordering. We explore these suggestions in two-sample Mann-Whitney tests below.
Table A.3.1.2: Summary Statistics: Mean Individual Contributions in Menu Game by Endowment Level and Position Seen in tokens (Standard Deviation)

<table>
<thead>
<tr>
<th>Position</th>
<th>Endowment=25</th>
<th>Endowment=45</th>
<th>Endowment=65</th>
<th>Endowment=85</th>
</tr>
</thead>
<tbody>
<tr>
<td>First</td>
<td>17.58 (6.09)</td>
<td>17.79 (9.38)</td>
<td>21.11 (7.28)</td>
<td>25.03 (13.88)</td>
</tr>
<tr>
<td>Second</td>
<td>15.69 (7.28)</td>
<td>19.11 (6.72)</td>
<td>18.67 (10.45)</td>
<td>22.36 (9.62)</td>
</tr>
<tr>
<td>Third</td>
<td>17.53 (6.55)</td>
<td>18.56 (9.21)</td>
<td>20.69 (8.26)</td>
<td>19.39 (12.02)</td>
</tr>
<tr>
<td>Last</td>
<td>15.81 (7.85)</td>
<td>20.28 (6.87)</td>
<td>21.17 (11.67)</td>
<td>21.89 (12.56)</td>
</tr>
</tbody>
</table>

Figure A.3.1.1.a: Mean Contributions in the Menu Game by Endowment and Position
Table A.3.1.3 shows p-values from two-sample Wilcoxon tests of individual level contributions within the same endowment setting across different positions. The null hypothesis states that contributions are identical across different positions (i.e., there are no ordering effects). In only one instance can we reject the null hypothesis. In the 85 token endowment setting, contributions are significantly different when individuals see this setting first and when they see it second. Thus, we detect no systematic ordering effects, suggesting that simultaneous menu games might alleviate ordering effects.

In addition, we test whether ordering impacts the endowment effect identified in Chapter 3. Table A.3.1.4 displays the p-values from two-sample Wilcoxon tests of individual level contributions across different endowment setting with the same ordering. The null hypothesis states that contributions are identical across different endowments (i.e., there is no endowment effect). Here the evidence is mixed. We find no consistent endowment
effect within each position. However, comparing contributions across endowment setting when these are seen first, contributions increase with increases in endowments.

To further explore the impact of ordering on the endowment effect we run the following regressions (see Table A.3.1.5 below).

Models 1 and 2 regress individual contributions dummies for the four different endowment settings.\textsuperscript{64} Model 2 also includes dummy variables for the position in which the endowment setting was seen. The inclusion of which does not change the magnitude or significance of the endowment coefficients, suggesting that the order in which subjects see the endowment settings does not impact the endowment effect. Nevertheless, subjects tend to contribute more when they see a setting first.\textsuperscript{65}

Models 3 and 4 explore the impact of endowment and ordering on the likelihood of a group to meet the threshold.\textsuperscript{66} Again, we see a largely consistent endowment effect.\textsuperscript{67} The inclusion of position dummy variables does not alter these findings. However, there is a significant difference between the likelihood of groups meeting the threshold when they see an endowment first versus third. This seems to be largely driven by the difference in contributions across position in the 85 token setting (see Figure A.3.1.1.a).

\textsuperscript{64} The 25-token-setting is the baseline.
\textsuperscript{65} However, this effect is not consistent across endowment settings. It seems to be driven by the differences in contributions in the 85 token setting (see Figure A.3.1.1.a and Table A.3.1.3). Further, as shown in Table A.3.1.6 coefficients are not significantly different across the remaining endowment settings.
\textsuperscript{66} Model 3 replicates the model in Table 3.5 in Chapter 3.
\textsuperscript{67} Although groups are not more likely to meet the threshold in the 45 tokens setting versus the 65 tokens setting (see Table A.3.1.6).
Table A.3.1.3 p-values from Two-sample Wilcoxon (Mann-Whitney) Test

H₀: Individual Contributions are the Same Within the Same Endowment Across Different Positions

<table>
<thead>
<tr>
<th>Endowment = 25 tokens</th>
<th>Endowment = 45 tokens</th>
<th>Endowment = 65 tokens</th>
<th>Endowment = 85 tokens</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>SEEN</strong></td>
<td><strong>Second</strong></td>
<td><strong>Third</strong></td>
<td><strong>Last</strong></td>
</tr>
<tr>
<td>First</td>
<td>0.2055</td>
<td>0.6553</td>
<td>0.8158</td>
</tr>
<tr>
<td>Second</td>
<td>0.4894</td>
<td>0.2001</td>
<td></td>
</tr>
<tr>
<td>Third</td>
<td>0.6010</td>
<td></td>
<td></td>
</tr>
<tr>
<td>First</td>
<td>0.3042</td>
<td>0.3289</td>
<td>0.1095</td>
</tr>
<tr>
<td>Second</td>
<td>0.9577</td>
<td>0.5645</td>
<td></td>
</tr>
<tr>
<td>Third</td>
<td>0.5038</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01

Observations: 72 for each test
### Table A.3.1.4 p-values from Two-sample Wilcoxon (Mann-Whitney) Test

**H0:** Individual Contributions are the Same Within the Same Position Across Different Endowments

<table>
<thead>
<tr>
<th></th>
<th>Seen FIRST</th>
<th></th>
<th></th>
<th>Seen SECOND</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>45 tokens</td>
<td>65 tokens</td>
<td>85 tokens</td>
<td>45 tokens</td>
<td>65 tokens</td>
<td>85 tokens</td>
<td>85 tokens</td>
</tr>
<tr>
<td>25 tokens</td>
<td>0.9797</td>
<td>0.0253**</td>
<td>0.0128**</td>
<td>0.0554*</td>
<td>0.2053</td>
<td>0.0040***</td>
<td></td>
</tr>
<tr>
<td>45 tokens</td>
<td>0.0717*</td>
<td>0.0271**</td>
<td>0.7648</td>
<td>0.7648</td>
<td>0.2356</td>
<td></td>
<td></td>
</tr>
<tr>
<td>65 tokens</td>
<td>0.4032</td>
<td>0.4032</td>
<td>0.4032</td>
<td>0.4032</td>
<td>0.1803</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Seen THIRD</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>45 tokens</td>
<td>65 tokens</td>
<td>85 tokens</td>
<td>45 tokens</td>
<td>65 tokens</td>
<td>85 tokens</td>
<td>85 tokens</td>
</tr>
<tr>
<td>25 tokens</td>
<td>0.9247</td>
<td>0.1654</td>
<td>0.9125</td>
<td>0.0675*</td>
<td>0.0726*</td>
<td>0.0620*</td>
<td></td>
</tr>
<tr>
<td>45 tokens</td>
<td>0.2283</td>
<td>0.9952</td>
<td>0.8155</td>
<td>0.8155</td>
<td>0.6235</td>
<td></td>
<td></td>
</tr>
<tr>
<td>65 tokens</td>
<td>0.3430</td>
<td>0.3430</td>
<td>0.7850</td>
<td>0.7850</td>
<td>0.7850</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* * p<.1, ** p<.05, *** p<.01

Observations: 72 for each test
Table A.3.1.5: Regression Analysis of Individual Contributions and Group Likelihood of Meeting the Threshold in Menu Game

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>(1) Tobit</th>
<th>(2) Tobit</th>
<th>(3) Logit</th>
<th>(4) Logit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>16.39***</td>
<td>17.29***</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td></td>
<td>[0.000]</td>
<td>[0.000]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>E = 45</td>
<td>2.351***</td>
<td>2.351***</td>
<td>3.571**</td>
<td>3.703**</td>
</tr>
<tr>
<td></td>
<td>[0.000]</td>
<td>[0.000]</td>
<td>[0.031]</td>
<td>[0.026]</td>
</tr>
<tr>
<td>E = 65</td>
<td>3.813***</td>
<td>3.812***</td>
<td>5.000***</td>
<td>5.255***</td>
</tr>
<tr>
<td></td>
<td>[0.000]</td>
<td>[0.000]</td>
<td>[0.002]</td>
<td>[0.002]</td>
</tr>
<tr>
<td>E = 85</td>
<td>5.555***</td>
<td>5.556***</td>
<td>8.333***</td>
<td>8.948***</td>
</tr>
<tr>
<td></td>
<td>[0.000]</td>
<td>[0.000]</td>
<td>[0.000]</td>
<td>[0.000]</td>
</tr>
<tr>
<td>Position Dummies:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>--</td>
<td>-1.483**</td>
<td>--</td>
<td>0.680</td>
</tr>
<tr>
<td></td>
<td></td>
<td>[0.023]</td>
<td></td>
<td>[0.256]</td>
</tr>
<tr>
<td>3</td>
<td>--</td>
<td>-1.392**</td>
<td>--</td>
<td>0.368**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>[0.048]</td>
<td></td>
<td>[0.037]</td>
</tr>
<tr>
<td>4</td>
<td>--</td>
<td>-0.729</td>
<td>--</td>
<td>1.000</td>
</tr>
<tr>
<td></td>
<td></td>
<td>[0.354]</td>
<td></td>
<td>[1.000]</td>
</tr>
<tr>
<td>ll / ul</td>
<td>0 / -</td>
<td>0 / -</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>N</td>
<td>576</td>
<td>576</td>
<td>96</td>
<td>96</td>
</tr>
<tr>
<td>P &gt; F/Chi²</td>
<td>0.0000</td>
<td>0.0000</td>
<td>0.0007</td>
<td>0.0030</td>
</tr>
</tbody>
</table>

Output of Models 3 and 4 presented as odds ratios
Errors clustered around the subject level (144 clusters) in Models 1 and 2 and clustered around the group level (24 clusters) in Models 3 and 4.
p-values in brackets
* p<.1, ** p<.05, *** p<.01
Table A.3.1.6: p-values of Wald Tests Comparing Coefficient Equality in Previous (Table A.3.1.5) Regressions:

<table>
<thead>
<tr>
<th>H₀</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>E₄₅ = E₆₅ (Observations)</td>
<td>0.0005***</td>
<td>0.0005***</td>
<td>0.1501</td>
<td>0.1053</td>
</tr>
<tr>
<td>E₄₅ = E₈₅ (Observations)</td>
<td>0.0001***</td>
<td>0.0000***</td>
<td>0.0169**</td>
<td>0.0063***</td>
</tr>
<tr>
<td>E₆₅ = E₈₅ (Observations)</td>
<td>0.0059***</td>
<td>0.0058***</td>
<td>0.0730*</td>
<td>0.0811*</td>
</tr>
<tr>
<td>Position 2 = Position 3 (Observations)</td>
<td>--</td>
<td>0.8850</td>
<td>--</td>
<td>0.1349</td>
</tr>
<tr>
<td>Position 2 = Position 4 (Observations)</td>
<td>--</td>
<td>0.3633</td>
<td>--</td>
<td>0.2897</td>
</tr>
<tr>
<td>Position 3 = Position 4 (Observations)</td>
<td>--</td>
<td>0.3598</td>
<td>--</td>
<td>0.0374**</td>
</tr>
</tbody>
</table>

* p<.1, ** p<.05, *** p<.01
References


APPENDIX 3.2: EXPERIMENTAL INSTRUCTIONS

[Please note: Writing in square brackets will not be seen by subjects. ___ denotes to a new screen.]

Now that the experiment has begun, please make sure that your cell phones are switched off. We ask that you do not talk with one another and do not turn around or look at other participants' screens.

If you have a question after reading the instructions, please raise your hand and the experimenter will answer your question in private.

Welcome

You will receive $5 for showing up for this experiment. You can also earn additional money by participating in this experiment. This experiment will not take more than 1 hour and 30 minutes. You are free to leave at any time. However, if you choose to do so before the end of the experiment, you will only receive the $5 show-up fee. Please read the following instructions carefully. The total amount of money you can earn during the experiment depends on your decisions during this experiment, as well as the decisions of your group members.

Private Decisions
Please note that your decisions and earnings are private. Your decisions are recorded using your experimental subject ID given to you by the experimenter, not your name or your student ID. At the end of the experiment, you will be asked to enter your name into the computer. This information is to process your payment only - it will not be used in any other way.

Today's Experiment

Payments
Your decisions will earn you Experimental Currency Units (ECUs). At the end of the experiment your ECUs will be exchanged into US dollars at a rate of 100 ECUs = $1. You will be paid in US dollars.

Stages
Today's experiment will consist of four stages. You will receive instructions at the beginning of each stage.
Groups and Member Number
You have been randomly assigned to a group of 6 persons. You will remain in this group until the end of the experiment. Each group member has been assigned a member number between 1 and 6. Your member number (and your group members' numbers) will remain the same throughout the experiment.

Please note: Both group assignments and member number assignments have been assigned randomly across all of the participants in today's experiment. Member numbers do not reflect the seating location of any particular group member.

Your Group: [empty diagram here]

You have been assigned Member Number: [subject ID here]

Stage 1 Instructions

Stage 1 will consist of four separate transfer decisions as described below. Before making actual decisions that affect your earnings, you will answer a short quiz designed to check your understanding of the decision task.

Decision Task
Every individual in your group has an Individual Fund, and your group of six has a Group Fund. Both your Individual Fund and the Group Fund have 0 tokens in them at the beginning of a decision task. You and your group members will each receive a number of tokens (in your starting balance) for each decision task. You will then decide privately how many (if any) tokens to transfer to the Group Fund. The computer automatically places any tokens you did not transfer to the Group Fund in your own Individual Fund.

Returns from Transfer Decisions
- Each token that you place in your Individual Fund will increase your earnings by 1ECU. This also means that for each token you transfer to the Group Fund, your earnings from the Individual Fund go down by 1ECU.
- If your group transfers at least 120 tokens to the Group Fund, every group member will receive 60ECU each, regardless of how many tokens each member transferred to the Group Fund.
- If your group transfers more than 120 tokens to the Group Fund, you will still earn 60ECU. This means that any additional token beyond 120 does not improve earnings for any individual.
• If your group transfers fewer than 120 tokens to the *Group Fund*, your transfers to the *Group Fund* will be automatically returned to you and placed in your *Individual Fund*.

The computer will store all your decisions in Stage 1. You will not know how many tokens your group members transferred to the *Group Fund*, nor how much you earned in this stage. At the end of the experiment (after Stage 4) the computer will sum up your earnings from Stages 1 to 4 to compute your total earnings.

---

**Starting Balances**

You will make four decisions. For every decision, you and your group member will have different numbers of tokens in your starting balance:

- In Decision A each group member has \[25 \text{ in } O1, 45 \text{ in } O2, 65 \text{ in } O3, 85 \text{ in } O4\] tokens in their starting balance.
- In Decision B each group member has \[45 \text{ in } O1, 65 \text{ in } O2, 85 \text{ in } O3, 25 \text{ in } O4\] tokens in their starting balance.
- In Decision C each group member has \[65 \text{ in } O1, 85 \text{ in } O2, 25 \text{ in } O3, 45 \text{ in } O4\] tokens in their starting balance.
- In Decision D each group member has \[85 \text{ in } O1, 25 \text{ in } O2, 45 \text{ in } O3, 65 \text{ in } O4\] tokens in their starting balance.

Please note that in each of the four Decisions everyone has the same number of available tokens.

**Earnings in Stage 1**

Your total earnings in Stage 1 will be the sum of your earnings from each of your four decisions in Stage 1. For each setting your earnings are calculated as follows:

If the number of tokens in the *Group Fund* is less than 120 your earnings are:

\[
1 \text{ECU} \times \text{Number of tokens initially received in your Starting Balance}
\]

If the number of tokens in the *Group Fund* is equal to or greater than 120 your earnings are:

\[
1 \text{ECU} \times \text{Number of tokens in your Individual Fund} + 60\text{ECU}
\]

Please note that your earnings from the *Group Fund* are always 60ECU anytime the target of 120 tokens is met.

Please raise your hand if you have any questions. Otherwise, click Continue to proceed to the quiz.

---

*QUIZ – Stage 1*
Stage 2 Instructions

You have now completed Stage 1. Stage 2 consists of 10 decision periods. The decision task is very similar to those in Stage 1, but with some differences. Before making actual decisions that affect your earnings, you will answer another short quiz designed to check your understanding of the decision task.

You are still in the same group of 6 persons to which you were randomly assigned at the beginning of the experiment.

Your member number is also the same as it was in Stage 1: You are Member [subject ID here]

Decision Task

As in Stage 1, you will decide privately how to distribute your starting balance between the Group Fund and your Individual Fund. (Remember: Any tokens not transferred to the Group Fund will be automatically placed in your Individual Fund by the computer.) At the beginning of every round of this stage both the Individual Fund and the Group Fund will contain 0 tokens.

As before:

- Each token in your own Individual Fund will increase your earnings by 1ECU.
- If your group transfers at least 120 tokens to the Group Fund, every group member will receive 60ECU each, regardless of how many tokens each member transferred to the Group Fund.
- If your group transfers more than 120 tokens to the Group Fund you will still earn 60ECU. This means that any additional token beyond 120 does not improve earnings for any individual.
- If your group transfers fewer than 120 tokens to the Group Fund, your transfers to the Group Fund will be automatically returned to you and placed in your Individual Fund.

In each round of Stage 2, you and your group members will have a starting balance of [30/50] tokens each.

Please note that in each of the 10 decision rounds, when you are making a decision about how many tokens to transfer to the Group Fund you will not know your group members' decisions.
Information You Will Receive
Once everyone has made his or her transfer decisions in a round, you will receive information about:

- Your transfer decision for that round,
- Your earnings for that round,
- The total number of tokens transferred to the Group Fund for that round.

Decisions from previous rounds will also be available to you in a History Table at the bottom of the screen.

Earnings in Stage 2
Your total earnings in Stage 2 will be the sum of your earnings from each of the 10 decision rounds in Stage 2. Earnings in every round are calculated in the same way as they were calculated in Stage 1 of the experiment:

If the number of tokens in the Group Fund is less than 120 your earnings are:

\[ 1 \text{ECU} \times \text{Number of tokens initially received in your Starting Balance} \]

If the number of tokens in the Group Fund is equal to or greater than 120 your earnings are:

\[ 1 \text{ECU} \times \text{Number of tokens in your Individual Fund} + 60 \text{ECU} \]

Please raise your hand if you have any questions. Otherwise, click Continue to proceed to the quiz.

QUIZ – Stage 2
Stage 3 Instructions

You have now completed Stage 2. You will now participate in Stage 3, which also consists of 10 decision periods. The decision task is very similar to that in Stage 2, but with some differences in the information you receive. Before making actual decisions that affect your earnings, you will answer another short quiz designed to check your understanding of these differences.

You are still in the same group of 6 persons to which you were randomly assigned at the beginning of the experiment.

Your member number is also the same as it was in Stage 1: You are member [subject ID here]


[LOCAL NETWORK]

**Decision Task and Information**

Number of tokens in the *starting balances* and the decision task are the same as in Stage 2. However once you have made your decision, you will now receive the following information:

- Your transfer decision for that round,
- Your earnings for that round,
- The total number of tokens transferred to the *Group Fund* for that round, AND
- The individual transfers to the *Group Fund* by two other group members. You are member [SubjectID] so:
  - You will receive information about the transfers made by member [left neighbor ID] and by member [right neighbor ID].
  - In turn, person [left neighbor ID] receives information about transfers by member [left neighbor ID – 1] and by YOU.
  - And person [right neighbor ID] receives information about transfers by YOU and by member [right neighbor ID + 1].

The diagram to the right shows your group.
The lines indicate who receives whose transfer information

As before, decisions from previous rounds will also be available to you in the History Table at the bottom of the screen.

Please raise your hand if you have any questions. Otherwise, click Continue to proceed to the quiz.
Decision Task and Information
Number of tokens in the *starting balances* and the decision task are the same as in Stage 2. However once you have made your decision, you will now receive the following information:

- Your transfer decision for that round,
- Your earnings for that round,
- The total number of tokens transferred to the *Group Fund* for that round, AND
- The individual transfers to the *Group Fund* by all other group members.

The diagram to the right shows your group.
The lines indicate who receives whose transfer information

As before, decisions from previous rounds will also be available to you in the History Table at the bottom of the screen.

Please raise your hand if you have any questions. Otherwise, click Continue to proceed to the quiz.

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*QUIZ – Stage 3*
Stage 4 Instructions

You have completed Stage 3. In Stage 4, you will make 10 decisions. Please note that your earnings in this stage do not depend on your group members' decisions or your decisions from the previous stages.

Decision Task
You have been given 10 different scenarios where you must choose between alternatives LEFT and RIGHT. In each of the 10 decisions the alternative LEFT gives a certain payment. If you choose alternative RIGHT, your payment depends on chance.

Example: In the following setting you must decide whether you prefer alternative LEFT in which you receive $1.75 for certain or alternative RIGHT in which there is a 50% chance that you receive $2.50 and a 50% chance that you receive $0.

<table>
<thead>
<tr>
<th>LEFT</th>
<th>Please indicate your choice</th>
<th>RIGHT</th>
</tr>
</thead>
<tbody>
<tr>
<td>$1.75 for certain</td>
<td>LEFT o o RIGHT</td>
<td>50% chance of $2.50 and 50% chance of $0.</td>
</tr>
</tbody>
</table>

Earnings in Stage 4
Only one of the 10 decisions you make will be used for computing your earnings in Stage 4. The decision task is selected at random - the experimenter will draw a card from a shuffled deck of cards number 1 through 10 (corresponding to the decision tasks 1-10). Each decision task has the same probability of being picked. The draw will take place in public at the front of the room. The decision task that is picked is the same for everyone in the room.

Once the decision task has been picked, another card will be randomly picked from a deck of two cards that contains one face card (a Jack) and one non-face card (#2). The card drawn will determine the earnings of everyone that picked alternative RIGHT in this decision task:

- If the Jack is picked, those that picked RIGHT will receive the high payoff ($2.50).
- If the #2 card is picked, those that picked RIGHT will receive $0.

Everyone that picked LEFT for this decision task will receive the certain payoff for the decision task.

Please raise your hand if you have any questions. Otherwise, click Continue to proceed to the quiz.

QUIZ – Stage 4
Curriculum Vitae

URSULA W. KREITMAIR

EDUCATION
   Fields: Public Policy, Environmental Policy, Methods
   Dissertation: Observing Others: The Effect of Behavioral Information on Collective Action in Social Dilemmas
   Dissertation Chairs: Elinor Ostrom (deceased), James Walker (Co-Chair), Michael McGinnis (Co-Chair)

M.Sc. University College London. Natural Resource and Environmental Economics. 2010

M.Sc. London School of Economics and Political Science. Environmental Policy, Planning, and Regulation. 2006

B.A. University of Oxford. Philosophy, Politics, and Economics. 2005

FURTHER TRAINING
Santa Fe Institute Graduate Workshop in Computational Social Science Modeling and Complexity. June 2013

Interuniversity Consortium for Political and Social Research (ICPSR) at University of Michigan. Summer 2012. Courses taken: Advanced Game Theory and Complex Systems

AWARDS AND HONORS
Recipient of scholarship from the Vincent and Elinor Ostrom Workshop in Political Theory and Policy Analysis to attend the Santa Fe Institute Graduate Workshop in Computational Social Science Modeling and Complexity. 2013. $2500

Recipient of School of Public and Environmental Affairs scholarship to attend the Interuniversity Consortium for Political and Social Research (ICPSR) at University of Michigan. 2012. $2300

RESEARCH

RESEARCH INTERESTS

• Environmental Policy
• Integration of behavioral economics research into public policy
• Institutional design (especially information policies)
• Experimental methods (particularly with regards to social dilemmas)
• Impact of information, communication, and social networks on cooperation
• Common pool resource management

PUBLICATIONS AND WORKING PAPERS


“Voluntary Disclosure of Contributions: An Experimental Study on Non-Mandatory Approaches for Improving Public Good Provision” Ecology and Society 20(4):33. (Experiment funded through National Science Foundation.)


“Production versus Use: An Experimental Study of Collective Action Among Heterogeneous Groups,” with Jacob Bower-Bir

“Communication and Monitoring in Collective Resource Management: A Computational Exploration of Local Information”


“Defining a Framework: Institutions, Resources and People in Social-Ecological Systems,” with Graham Epstein

RESEARCH GRANTS

Indiana University Office of Sustainability Graduate Student Sustainability Research Development Grant. 2015 (with Stefan Carpenter). “Conservation and Human-Wildlife Conflict: An Experimental Study of Collective Action Involving Discounting and Uncertainty.” Co-PI. $6,000
University of Nebraska Faculty Seed Grant. 2015 (with Simanti Banerjee, UNL and James Walker, IU). “Toward Successful Fundraising Campaigns: Social Networks and their Role in Threshold Public Goods Games.” Researcher. $10,000

Indiana University Office of Sustainability Graduate Student Sustainability Research Development Grant. 2013 (with Jacob Bower-Bir). “Groups, Dictators, and Natural Resources: An Experimental Study of Collective Action Among Heterogeneous Groups.” Co-PI. $3,500.


INVITED TALKS AND CONFERENCE PRESENTATIONS


“Voluntary Disclosure of Contributions: An Experimental Study on Non-Mandatory Approaches for Improving Public Good Provision”; Invited Talk presented at the Department of Agricultural Economics, University of Nebraska, Lincoln, March 2015

“Communication and Monitoring in Collective Resource Management: An Exploration of Local Information”; presented at Workshop on the Workshop 5, Bloomington, June 2014

“Communication and Monitoring in Collective Resource Management: An Exploration of Local Information”; presented at Western Political Science Association Annual Meeting, Seattle, April 2014


“Groups, Dictators, and Unequal Pay: An Experimental Study of Group Decision-Making and Membership Effects in Heterogeneous Groups”; co-presented with Jacob Bower-Bir at Wednesday Colloquium at Vincent and Elinor Ostrom Workshop in Political Theory and Policy Analysis; Bloomington, March 2013


“Information and Common Pool Resources: An Experimental Study of Conditional Cooperation”; presented at Wednesday Colloquium at Vincent and Elinor Ostrom Workshop in Political Theory and Policy Analysis; Bloomington, November 2012

**EMPLOYMENT AND TEACHING**

**EMPLOYMENT**

Lecturer. Department of Political Science. Oklahoma State University. 2015


Research Assistant to Haeil Jung. School of Public and Environmental Affairs. Indiana University. Fall 2010


**TEACHING EXPERIENCE**

POLS 4593 “Natural Resources and Environmental Policy.” Department of Political Science, Oklahoma State University.

Spring 2016, 24 students

POLS 4363 “Environmental Law and Policy.” Department of Political Science, Oklahoma State University.

Fall 2015, 33 students
E 162 “Environment and People.” School of Public and Environmental Affairs, Indiana University
   Spring 2014, 56 students
   Fall 2013, 53 students
   Spring 2011, 26 students

“An Introduction to Z-tree Experimental Economics Software for Graduate Students and Faculty” Indiana University.
   Fall 2015 Co-taught with Andrea Sorensen
   Fall 2014 Co-taught with Andrea Sorensen
   Summer 2013 Co-taught with Brock Stoddard

OTHER

SERVICE
   Reviewer for the International Journal of the Commons; Ecology and Society

   Coordinator of volunteers at Workshop on the Workshop 5, June 2014

   Student member of the Workshop Colloquium Committee AY2013-2014

RELEVANT SKILLS
   Stata, Z-tree (Economics Experiments Software), LaTeX, Python

REFERENCES

   References available upon request.