Three Essays on Cooperation in a Public Good Experiment

by

Dana K. Jackman

A dissertation submitted in partial fulfillment
of the requirements of the degree of
Doctor of Philosophy
(Natural Resources and Environment)
University of Michigan
2016

Doctoral Committee:

Professor Michael R. Moore, Chair
Adjunct Associate Professor Gloria E. Helfand
Assistant Professor Erin L. Krupka
Professor Bobbi S. Low

Dedication

For

Jane Frances, the light

and

Nancy Lee, who chose the light

Acknowledgements

First I would like to thank my dissertation committee members, Michael Moore, Gloria Helfand, Erin Krupka, and Bobbi Low. I would also like to thank Emeritus committee member, Rick Riolo; former committee member, Ted Parson; and informal mentors, Carl Simon and Michaela Zint. They are fundamentally kind people and truly gifted mentors. I am deeply grateful. Thank you.

I would also like to thank Ashley Hazel and Joyce Lawrence. How does one thank women so powerfully loving and generous? There are no words.

I am grateful to my sister, Eileen Jackman, and brother, Glenn Jackman, for keeping me in their hearts and in the real world.

Finally, I thank my family and many dear friends. I could not have done what I have done without the uncountable kindness of so many.

Table of Contents

List of Tables

List of Figures

Abstract

At the heart of every environmental dilemma, there are people engaged in the conflict between what is good for the individual and what is good for the group. These dilemmas include public goods – like clean air, biodiversity, and a stable climate – that, regardless of who provides the public good and how much, are shared equally. Public goods are most often provided through mandates, less often through price-based incentives and charitable giving. Recently, we have begun to see innovations in the energy sector that leverage behavioral research to motivate individuals to support renewable energy and conserve electricity.

In support of those innovations, this research advances our understanding of voluntary public goods provision by individuals. How? In an extensive public good experiment, I weave together three separate threads of behavioral research to achieve the following objectives: 1) I identified four differently motivated, behavior-based cooperative types – non-cooperators (19%), conditional cooperators (51%), high cooperators (7%), and a previously unclassified group of low cooperators (23%). 2) I established that non-cooperators were not influenced by frame, but conditional, low, and high cooperators were; and the different dimensions of framing design yielded different behavioral responses. Non-neutral language yielded lower cooperation among conditional and low cooperators when they were giving to the public good. Taking instead of giving yielded lower cooperation among low cooperators when neutral language was employed. 3) I also showed that frames indirectly influenced cooperation through beliefs about others' cooperation. For conditional cooperators, evidence of a beliefs pathway was unequivocal, but not at all present for non-cooperators and mixed for low and high cooperators.

Chapter 1

Heterogeneous Cooperation and Motivation in a Public Good Dilemma

Abstract: We know from decades of research that people cooperate in social dilemmas and cooperation varies considerably. These findings have led researchers to categorize individuals into cooperative types, often preconceived categories that conflate behavior and motives, ignore the possible effects of framing, and do not reliably predict behavior in repeated public good dilemmas. Without pre-specifying the number or nature of cooperative types, I assessed cooperative type via an incentivized, one-shot public good experiment in which four different sets of participants were subject to four different frames. I also separately assessed participants' motives via responses reported in a questionnaire. Analyses revealed four, differently motivated, behavior-based cooperative types and no framing effects. Some individuals did not cooperate (19%), many approximately matched the cooperation of others (51%), and a few consistently cooperated at relatively high levels (7%). In addition, a relatively large and previously unclassified group of individuals consistently cooperated at low but non-trivial levels (23%). At the same time, motives varied considerably and systematically according to cooperative type. Specifically, non-cooperators and low cooperators were similarly self-interested and significantly more self-interested than conditional and high cooperators. In other regards, non-cooperators were significantly less motivated by feelings of obligation and perceptions of others' actions than low, conditional, and high cooperators. Low cooperators also were significantly less motivated by feelings of obligation than conditional and high cooperators. Both cooperative type and motives were not sensitive to frame.

1 Introduction

At the heart of every social dilemma is a conflict between individual and group interests. Incentivized public good experiments exploit that conflict to reveal differences in decision-making when real monetary payoffs are at stake, individuals' earnings depend on others' decisions, and others' decisions are unknown. These properties of public good experiments are at the core of the real-world public good dilemmas that motivate this study – dilemmas such as climate stability, public parks, biodiversity, water and air quality, public schools, defense, and disaster relief. Public goods are created and maintained through voluntary cooperation, which is the focus of this research, and through public policy, which can be informed by studies like this one.[1]

What are the most robust findings from public good experiments? First, we know that behavior in social dilemma experiments deviates from rational actor predictions.[2] Second, every dilemma experiment shows that behavior differs dramatically among individuals. Third, we know that individuals behave differently when decisions are presented differently. In brief, many people appear to be motivated by something other than self-interest, behavior among individuals is heterogeneous, and framing influences behavior. This research integrates these separate strands and addresses the following questions: What is the appropriate typology of behavior-based and statistically distinct types of decision-makers in a public good dilemma? How are different types of decision-makers differently motivated? Are types and motives frame-dependent?

The novelty of this research lies in its experimental design and methodology, as well as its findings. This study empirically establishes behavioral types based on study participants' decisions, assesses participants' motives via questionnaire response, identifies relationships

[1] For example, many environmental goods are public goods suffer chronic under-provision. Environmental policies are dominated by mandatory standards (a.k.a. "command and control" regulations) plus relatively few price-based policies (e.g., taxes and markets). Very few policies target individuals and leverage social interactions. OPOWER's program of injunctive and descriptive norms among electric utility customers is a promising example of a nontraditional energy conservation program "informed by insights from behavioral science" (Allcott, 2011).

[2] Theoretically, a rational actor is an individual who is concerned exclusively with his or her own well-being, makes choices that best satisfy his or her preferences, and forms correct beliefs about the world (Camerer & Fehr, 2006). This definition typically excludes preferences over others' actions or outcomes. A rational actor would not cooperate in a linear public good dilemma.

between behavioral types and motives; and test for framing effects. Specifically, the experiment employed two instruments to separately assess type and motives, a one-shot public good dilemma[3] and an original questionnaire. Each participant was subject to one of four strategically equivalent but qualitatively different frames, so that the effects of frame could be captured.[4] Empirical methods included the first application of group-based trajectory modeling (GBTM) to social dilemma experiments, exploratory factor analysis (EFA), and nonparametric tests.

In contrast to previous studies (e.g., Kurzban & Houser, 2005; Liebrand 1984; Offerman, Sonnemans, & Schram, 1996; Park 2000; Parks, 1994; Sonnemans, Schram, & Offerman, 1998), this study does not fit individuals into preconceived categories, a practice that a priori limits full representation of heterogeneity. Instead, GBTM allows cooperative types to emerge from behavioral data, without pre-specifying the number or nature of each type. Each cooperative type represented a group of individuals, within which behavior was similar and between which behavior was distinct. This study also did not define cooperative type in terms of motives explicitly – as in Liebrand (1984), Offerman et al. (1996), and Sonnemans et al. (1998) – or implicitly – as in Fischbacher, Gächter, and Fehr (2001) Fischbacher and Gächter (2010), and Kurzban & Houser (2005). Why? Behavior is observable; motives are not. In addition, previous research suggests that motives-based typing schemes do not reliably predict behavior in public good experiments (c.f., Park, 2000; Parks 1994; Sonnemans et al., 1998), but previous behavior does (e.g. Fischbacher and Gächter, 2010; Kurzban & Houser, 2005). Thus, instead of presuming or inferring motives, this study assessed motives retrospectively via EFA of questionnaire responses. Non-parametric tests revealed systematic differences in motives among cooperative types.

This study revealed four, differently motivated, behavior-based cooperative types and no framing effects. As in other experiments, some individuals did not cooperate (19%), many approximately matched the cooperation of others (51%), and a few consistently cooperated at

[3] This incentivized, two-stage, one-shot public good dilemma was based on Fischbacher & Gächter's (2010) P-experiment and Fischbacher, Gächter, and Fehr's (2001) "strategy-method."

[4] Frame is defined as both the "formulation to which decision makers are exposed" and "the interpretation that they construct for themselves" (Dufwenberg et al., 2011 citing Kahneman, 2000, p. xiv). In this paper, "frame" refers only to the first definition.

relatively high levels (7%). In addition, a relatively large and previously unclassified group of cooperators consistently cooperated at low, but non-trivial levels (23%).

Overall, self-reported reactions to the experiment varied considerably and also systematically according to type. Generally, motives of non-cooperators differed dramatically from all three types of cooperators (i.e., low cooperators, conditional cooperators, and high cooperators), and cooperators differed in the level and ways in which they were other-regarding. Interestingly, however, low-level cooperators were similar to non-cooperators with regard to self-interest, but otherwise were more strongly motivated by feelings of obligation and perceptions of others' actions than non-cooperators. Low cooperators also distinguished themselves from conditional and high cooperators as significantly more self-interested and less motivated by feelings of obligation. Lastly, none of the results were sensitive to the frames imposed, suggesting that cooperative type and motives could be attributed to individuals, not to frame.

This paper is the first in a series of three papers all of which are based on a multi-part public good experiment. The experiment included a one-shot public good dilemma, a repeated public good dilemma, and a questionnaire. It was designed to assess and classify heterogeneity in behavior and motives among decision-makers (paper 1), examine how framing effects differ for different types of decision-makers (paper 2), parse the role of reference dependence and the language in framing effects (paper 2), and examine the channel/mechanism through which frames operate and how those channels differ for different types of decision-makers (paper 3). In this paper, I address the first of these four objectives through analyses of participants' decisions in the one-shot game and responses to the questionnaire. I analyze data from the repeated dilemma in the subsequent two papers.[5]

In the remaining sections, I first provide background related to four intersecting streams of research. Second, I describe experimental design and statistical analyses, both of which contain novel elements. Then, I present results and conclude with a discussion of next steps.

[5] These three papers make-up the three chapters of this dissertation. In paper 1, papers 2 and 3 are cited as Jackman (2016b) and Jackman (2016a), respectively.

2 Background

Social dilemma research is a multidisciplinary pursuit, and the laboratory experiment is a foundational tool used to understand decision-making. This research derives primarily from experimental economics, behavioral economics, and social psychology with specific lines of inquiry into public goods experiments, social preferences, heterogeneity, and framing.

2.1 Public Good Experiments

Public good experiments are simultaneous games with more than two players, and are intended to reproduce the essential attributes of pure public goods. Public goods are non-excludable, meaning that everyone has access to the good, and they are non-rival, meaning that consumption of the good by one individual does not affect others' consumption. Cooperation consists of providing or maintaining the public good. In the standard experiment with linear payoffs (p_i), every individual (i) decides how to distribute her or his personal endowment (w_i) between herself or himself and the group. Allocations to the group (g_i) constitute cooperation and are increased by a positive factor, k, and then divided among n participants. The result is a marginal per capita return (MPCR) of $\gamma = k/n$. Payoffs are typically given by the following:

$$p_i = w_i - g_i + \gamma \sum_{j=1}^{n} g_j \quad (1)$$

Under the usual assumptions of rational choice theory, the dominant strategy in a public good game with linear payoffs is to allocate nothing to the group even though everyone is better off if everyone cooperates. This is clearly not what we observe in public good experiments nor in real world situations such as donations to the Red Cross, purchases of carbon offsets, or voluntary participation in renewable electricity programs where contributions to the public good are common.[6]

[6] See Chauduri (2011) for a recent, selective survey of laboratory public good experiments and Ledyard (1995) for an older and exhaustive review of public good experiments.

2.2. Prosocial Behaviors and Social Preferences

Decades of experimental evidence show that standard rationality assumptions do not uniformly apply to everyone.[7] Some people do not cooperate and are assumed to be motivated by self-interest. However, some people do cooperate, and levels of cooperation differ significantly across individuals. These prosocial behaviors are often attributed to preferences over the actions and outcomes of others, known as other-regarding or social preferences. Other regarding preferences include preferences for being kind or doing good such as altruism (Andreoni & Miller, 2002; Fehr & Rockenback, 2004), impure altruism and "warm glow" (Andreoni, 1990, 1995), and strong reciprocity (Gintis, Bowles, Boyd, & Fehr, 2003); preferences over own-other comparisons of actions and outcomes such as reciprocity, fairness, and equity (Charness & Rabin, 2002; Fehr & Schmidt, 1999; Henrich & Henrich 2007); preferences related to harming others or foregoing opportunities to help others (Andreoni, 1995; Park, 2000; Sonnemans et al. , 1998; Willinger & Ziegelmeyer, 1999); and preferences over prosocial behaviors or obligations such as prevention of a "public bad" (Sonnemans et al., 1998) and guilt aversion (Dufwenberg, Gächter, & Hennig-Schmidt, 2011). In this research, I employ a questionnaire designed to expose social preferences and assess motives retrospectively. I also examine the relationships between motives and different types of decision-makers.

2.3. Heterogeneity and Typing Strategies

Together, public goods and social preferences research demonstrates that there are different types of decision makers in public good dilemmas who appear to be differently motivated (Chaudhuri, 2011). The practice of classifying decision-makers is decades old, and cooperative types have been assessed via a variety of ways. I focus on two opposing approaches that appear to have the most traction in social dilemma research in both social

[7] "Non-standard" contrasts with standard economic assumptions of self-interest and rationality wherein individuals are concerned exclusively with their own well-being, make choices that best satisfy their preferences, and form correct beliefs about the world (Camerer & Fehr 2006). Well-being in the context of public good dilemmas relates to tangible, often monetary payoffs and excludes any sort of social benefit or utility one might experience from helping others. Della Vigna (2009) provides an excellent and relatively brief summary of the literature on "non-standard" preferences, decision rules, and beliefs at the intersection of economics and psychology. Also see Camerer, Lowenstein, & Rabin (2004), Diamond & Vartiainen (2007), Laibson & Zeckhauser (1998), and Rabin (2002).

psychology and experimental economics – a motives-based approach and a behavior-based approach.

The classification strategy most commonly implemented in motives-based approaches is the Decomposed Game in which study participants choose between a series of hypothetical own and other outcomes that are then translated into predetermined categories corresponding to social motives, social values, or value orientation. For example, Liebrand (1984) defined "social motives" to be "the preferences of actors for specific outcome distributions" and classified individuals using the Decomposed Game into discrete, predefined types. Other researchers similarly defined social values (Parks, 1994) and value orientation (Offerman et al., 1996; Park 2000; Sonnemans et al., 1998) and assessed individuals' types. Results were similar across studies. Generally, there were larger groups of cooperators, individualists, and competitive types, and much smaller groups of altruists and aggressive types.[8]

Behavior-based approaches group individuals into types based on similarities in behavior in an actual dilemma (e.g., Fischbacher & Gächter, 2010; Fischbacher et al., 2001; and Kurzban & Houser, 2005). For example, Fischbacher and Gächter (2010) and Fischbacher et al. (2001) used an incentivized, two-stage, one-shot public good dilemma in which individuals report how much they would cooperate in response to all possible average levels of cooperation. Types were assessed and assigned via inspection of cooperation plotted against others' cooperation.[9] Using a repeated public good dilemma and a "statistical-type classification algoritm," Kurzban and Houser (2005) regressed each individual's cooperation on the average level of cooperation observed in the previous round for each individual. They assigned individuals to types based on the whether the regression lay everywhere above, everywhere below, or crossed the horizontal line at half of the initial endowment.[10] These and

[8] Using the Decomposed Game, Liebrand (1984) identified 4% altruists, 53% cooperators, 32% individualists, and 11% competitive; Parks (1994) found 9% altruists, 55% cooperators, 15% individualists, and 21% competitive; Offerman et al. (1996) found 27% cooperators, 65% individualists, 4% competitive, 1% aggressive, and 3% random; and Park (2000) classified 32% as cooperative, 65.5% as individualists, 0.5% as competitive, and 2% random.
[9] Using a two-stage, one-shot public good game, Fischbacher et al. (2001) found 50% conditional cooperators, 30% free-riders, 14% triangle cooperators, and 6% unclassifiable individuals; and Fischbacher and Gächter (2010) found 55% conditional cooperators, 23% free-riders, 12% triangle cooperators, and 10% unclassifiable individuals.
[10] Using a repeated public good game and predetermined types, Kurzban and Houser (2005) found 13% cooperators, 20% free-riders, and 63% reciprocators. Three individuals (4%) were not classified.

other studies typically find that the majority of participants in public good experiments are conditional cooperators (Chaudhuri, 2011). "Free-riders" also make up a relatively large group, with smaller numbers of individuals who cooperate at high levels. Importantly, these typing strategies can leave non-negligible numbers of individuals "unclassified."

Regardless of approach, most methods for classifying decision makers attempt to fit individuals into predetermined categories or categories explicitly or implicitly based on presumed motives. As a result, some individuals are left unclassified or incorrectly classified. For example, Offerman et al. (1996) classified 3% of study participants as random. Park (2000) classified 2% of subjects as random. 6% of study participants were unclassifiable in Fischbacher et al. (2001), 10% in Fischbacher and Gächter (2010), and 4% in Kurzban and Houser (2005).

Two additional challenges for these typing schemes – motives-based and behavior-based – are that they do not correlate with each other and may not predict behavior in public good dilemmas.[11] Recognizing these challenges, Burlando and Guala (2005) implemented four different classification methods to obtain a "data-driven classification" of subject heterogeneity plus self-reported, post-hoc explanations for behavior. However, they also imposed a typology of cooperative types in advance, and as a result, 15% of their study participants were deemed "unclassifiable,"[12] thus revealing a third challenge. Even when motives-based and behavior-based approaches are simultaneously implemented, specifying categories of decision-makers in advance can inhibit a complete representation of differences in behavior and motives among decision-makers in public good dilemmas.

In this research, I implement group based trajectory modeling (GBTM described in Section 3.3.1) to classify individuals into behavior-based types without pre-specifying types or

[11] More specifically, the Decomposed Game yields ambiguous results with regard to its ability to predict cooperation in a public good dilemma (Park, 2000; Parks, 1994; Sonnemans et al., 1998). Kurzban and Houser (2005) showed that " the decisions subjects made in games used for classification purposes predict well the decisions made in games played afterward." However, they were more focused on how well group composition predicted group outcomes, which turned out to be quite well. Fischbacher and Gächter (2010) assessed discrete types as in Fischbacher et al. (2001), but did not use discrete types to predict behavior in the subsequent repeated dilemma. Instead, they predicted cooperation for every individual based on a regression of conditional responses from the one-shot dilemma, which proved to be a significant but not perfect predictor of cooperation in the repeated dilemma.

[12] Burlando and Guala (2005) identified 32% free-riders, 35% reciprocators, 18% cooperators, and 15% unclassifiable.

motives. GBTM assumes that the behavior of interest varies continuously and widely with the goal of summarizing the key features of the data in a representative and parsimonious way (Nagin, 2005, pp. 56-57). I separately assess motives and the relationship between cooperative types and motives.[13]

2.4 Framing

Lastly, all of these and other typing strategies could be sensitive to the way the decision is presented, and none account for possible framing effects. Whether imposed externally or conceived by the decision-maker, frames can subtly or profoundly influence outcomes (e.g., Andreoni, 1995; Bougherara, Denant-Boemont, & Mascelet, 2011; Brewer & Kramer, 1986; Cookson, 2000; Cubitt, Drouvelis, & Gächter, 2011; Dufwenberg et al., 2011; Ellingsen, Johannesson, Mollerstrom, & Munkhammer, 2012; Fleishman, 1988; Levin, Gaeth, Schreiber, & Lauriola, 2002; Levin, Schneider, & Gaeth, 1998; Park 2000; Rutte, Wilke, & Messick, 1987; Sonnemans et al., 1998; Willinger & Ziegelmeyer, 1999). Arguably, frame is inextricably implicit in every public good experiment, even when neutral framing is an explicit goal. Therefore, the likelihood of belonging to a cooperative type could depend not only on *what* information is presented to decision-makers but also on *how* information is presented. Thus, the task of developing a fully representative typology of cooperative types and a classification strategy that is unbounded by preconceived pairings of behavior and motives could be further confounded by framing effects. This research addresses those concerns in three ways: framing is included in the experimental design, type is assessed via decisions that were not expected to be influenced by frame, and the relationships between cooperative type and frame are examined.

In summary, no method or combination of methods is known to provide an independent and objective measure of cooperative type in public good dilemmas. Despite the enormous body of research regarding public good dilemmas, social preferences, heterogeneity, and framing, there are important unanswered questions. What is an appropriate typology of cooperative types that is conceptually tractable and fully representative of behaviors in public

[13] In the second paper in this series, I demonstrate that cooperative type predicts cooperation in a repeated public good dilemma.

good dilemmas? How are different cooperative types differently motivated? Can we assess cooperative type as an inherent characteristic of the individual and not an artifact of framing? This research addresses those questions.

3 Methods

The primary objectives of this study were to 1) develop a typology of behavior-based cooperative types that is conceptually and computationally tractable and fully representative of behavior in public good experiments, 2) identify motives underlying the differences in behavior among cooperative types, and 3) demonstrate that cooperative types and motives, as assessed here, are an attribute of the individual, not of the decision frame to which the individual was subject.

To those ends, this paper reports the results of an incentivized, one-shot public good dilemma and a questionnaire that make up two components of the public dilemma experiment illustrated in Figure 1.1. The one-shot public good dilemma and the questionnaire were intended to tease apart subject heterogeneity without sensitivity to framing, the first via behavior and the second via motives. For each participant, the one-shot dilemma measured two types of decisions, an unconditional decision and 21 decisions conditioned on the average behavior of others. The 23 item questionnaire solicited individuals' reactions to the experiment and collected demographic information (i.e., self-reported age, gender, economics training, and prior experience with a "study like this one"). The experiment was programmed and conducted using the software z-Tree (Fischbacher, 2007) in the School of Information experimental laboratory at the University of Michigan.

Modeling and analyses consisted of five components. First, I confirmed that cooperation in the one-shot public good game was consistent with other public good experiments and tested for associations between frame and self-reported age, gender, economics training, and prior experience with a "study like this one." Second, using the first application of group-based trajectory modeling (GBTM) to social dilemma behavior, I identified four cooperative types based solely on behavior. Third, I tested for a relationship between questionnaire responses and cooperative types. Then, I applied exploratory factor analysis (EFA) to the questionnaire

data to elicit underlying motives and evaluated relationships between motives and cooperative types. Finally, I tested the one-shot public good game and the questionnaire for framing effects.

In the following, I describe the study participants, experiment, and core analytical methods used, namely GBTM, EFA, and Kruskal-Wallis and Wilcoxon-Mann-Whitney tests. In all of my analyses, I used an alpha level 0.05 for all statistical tests and describe results with *p*-values less than 0.05 as "statistically significant." I use the phrases "highly significant" for *p*-values less than 0.01, "weakly significant" for *p*-values between 0.05 and 0.1, and "marginally significant" for *p*-values close to but greater than 0.1. All analyses for this paper were generated using SAS® software.[14]

3.1 Study Population

Experimental subjects were recruited from an existing pool of potential participants using the School of Information On-line Recruitment System for Experimental Economics (ORSEE) at the University of Michigan. Prospective participants were invited via email to participate in a "decision-making experiment" for which they would receive "$5 compensation for showing up" and "approximately $20 (including show-up compensation)" if they completed the experiment. Two hundred and fifty-six adults participated. Per Table 1.1, the average age of participants was 20.5 ($N = 252, SD = 4.16$), 54 percent were women, 37 percent reported having no training in economics, and 38 percent of participants reported having no previous experience with "a study like this one."

I ran 16 sessions, four sessions for each of four experimental treatments, called frames and described in Section 3.2.2. Each session included 16 people, and each participant took part in only one session. Experimental treatments were assigned with the expectation that subpopulations would be similar. As reported in Table 1.1, participants' age ($F(3, N = 252) = 1.04, p = 0.3745$) and training in economics ($X^2\ (3, N = 251) = 0.68, p = 0.8784$) did not differ significantly across the four frames, but there was a marginal association between gender

[14] Version 9.4 for Windows, Copyright © 2002-2012 by SAS Institute Inc. SAS and all other SAS Institute Inc. product or service names are registered trademarks or trademarks of SAS Institute Inc., Cary, NC, USA.

and frame ($X^2\ (3, N = 256) = 6.09, p = 0.1073$) and a highly significant association between experience and frame ($X^2\ (3, N = 245) = 13.35, p = 0.0039$).[15]

3.2 The Experiment

This study focuses on a two-stage, one-shot public good dilemma and a questionnaire that were part of an experiment consisting of five parts – instructions, practice questions, one-shot public good dilemma, repeated public good dilemma, and questionnaire – illustrated in Figure 1.1 and fully documented in the Appendix. Frames were imposed on every part of the experiment with the exception of the questionnaire.

Each of the 16 sessions of the experiment commenced when recruits arrived at the waiting room. The first 16 recruits to arrive participated. Additional recruits were paid $5 for showing up and were then dismissed. Study participants were then invited into the computer laboratory where each individual chose to sit at one of 16 desktop computers separated by partitions. Participants were provided with written, frame-specific instructions stating that "the experiment consist[ed] of a series of decision situations" and explaining the decision situation and payoffs (see the Appendix). Instructions were also read aloud. To further ensure participants understood the decision situation and before the experiment could proceed, all participants were required to answer ten practice questions correctly.

Then the one-shot public good dilemma and the repeated public good dilemma proceeded in randomly re-matched groups of four. The public good decisions in the one-shot and repeated dilemmas were identical in payoffs but structured differently. The one-shot dilemma consisted of two-stages with only one payoff relevant decision (see Section 3.2.3). The repeated public good dilemma consisted of ten rounds and ten payoff relevant decisions.[16] Throughout, individuals earned points based on their decisions and the decisions of others.

[15] This is the result of a participant recruitment and selection process that did not control the demographic composition of experimental sessions (see section 3.2). Supplemental analyses showed that these differences did not influence results and conclusions.

[16] Before the repeated dilemma commenced, participants were informed that they would encounter the same decision 10 times as part of different anonymous groups of four that would be randomly assigned and reassigned for each decision. Decisions in the repeated public good dilemma are explored in depth in the next two papers in this series.

Each individual's decision, points earned, and the average decision of other group members were provided to each individual after the one-shot game and between each round of the repeated game.

Participants then completed a questionnaire that measured reactions to the experiment and collected demographic information. At the end of the experiment, points were converted to dollars. On average, participants earned $15 from their decisions plus $5 for showing up.

3.2.1 The Decision

Every decision in the experiment was essentially the same. Specifically, everyone was given decision-making power over an initial endowment of 20 points that they could split between themselves and the group in any way, including allocating nothing at all to themselves or nothing to the group. In the language of the experiment, allocation to oneself went to one's "personal account." Allocations to the group went to the "project account."

After decisions were made, individuals kept everything they allocated to themselves, and every group member benefited equally from allocations to the group according to the marginal per capita return (MPCR). Thus, consistent with other linear public good dilemma games, payoffs were given by Equation (1), repeated below:

$$p_i = w_i - g_i + \gamma \sum_{j=1}^{n} g_j \quad (1)$$

In this study p_i represents points earned by person i, and g_i represents person i's allocation to the public good (a.k.a, cooperation). The initial endowment was $w_i = 20$, and each group was of size $n = 4$. Consistent with Fischbacher et al. (2001) and Fischbacher and Gächter (2010), points allocated to the group were increased by a positive factor, $k = 1.6$, and then divided among $n = 4$ participants, yielding an MPCR of $\gamma = k/n = 0.4$.

3.2.2 Framing

The decision to cooperate and how much may depend on how the decision situation was presented.[17] In response, the decision situation in this study was presented in four

[17] Frames are created by presenting the same information differently. Framing effects are observed when decision-makers subject to different frames choose differently. These definitions may seem to imply that frames are "objective" and imposed on decision-makers. However, Amos Tversky and Daniel Kahneman made the important

different ways according to a between-subject, 2×2 factorial framing design that distinguishes the action implied by the location of the initial endowment (i.e., give versus take) and the language used (i.e., neutral versus non-neutral). Thus, the frames imposed were give neutral (GN), give non-neutral (GNN), take neutral (TN), and take non-neutral (TNN) as illustrated in Figure 1.2. The structure and language used to differentiate the give non-neutral and take non-neutral frames mimic the English translation of the give and take frames employed by Dufwenberg et al. (2011), excluding use of the words "keep" and "leave." In the give treatment, the initial endowment was located in individuals' personal accounts. In the take treatment, the initial endowment was presented as a portion of the project account. Neutral frames were created by substituting "allocate" in place of "give" and "take" while retaining the implied action of giving or taking established with the location of the initial endowment.[18] The following will make this more clear.

Payoffs were always calculated in accordance with Equation (1) regardless of frame. In the give treatments of this experiment, each of the four individuals in a group began with 20 points in her or his personal account. Participants were then asked how many points, called points-income in the experiment, they would "allocate" or "give" to the project account. Equation (1) reflects the framing of give treatments, but payoffs were presented as in Equation (1a) for the give neutral frame and Equation (1b) in the give non-neutral frame.

$$\textit{Total points-income} = \textit{(20 – points you allocated to the project account)} + 0.4 \times \textit{sum of all points allocated to the project account} \quad \text{(1a)}$$

$$\textit{Total points-income} = \textit{(20 – points you gave to the project account)} + 0.4 \times \textit{sum of all points given to the project account} \quad \text{(1b)}$$

point that frames are also subjective and therefore based on individuals' perceptions (Kahneman & Tversky, 1979; Tversky & Kahneman 1981, 1986, 1991). In this study, frame refers only to how the frame is constructed.

[18] The framing design was motivated by fairly robust, but somewhat mixed, findings that give frames yield more cooperation than take frames (c.f., Apesteguia & Maier-Rigaud, 2006; Bougherara et al., 2011; Brewer & Kramer, 1986; Cubitt et al., 2011; Fleishman, 1988; Rutte et al., 1987; Sonnemans et al., 1998), and positive frames yield more cooperation than negative frames (c.f., Andreoni, 1995; Levin et al., 2002; Levin et al., 1998; ; Park 2000; Willinger & Ziegelmeyer, 1999).

For each individual, points he "allocated" or "gave" to the project account comprised his cooperation.

In the take treatments, each four-person group was presented with a project account consisting of 80 points. Each individual was permitted to "allocate" up to 20 of those points to his or her private account or "take" up to 20 of those points from the project account. Payoffs in the take frames are the same as in Equation (1), but are more faithfully represented in Equation (2)

$$\pi_i = x_i + \gamma\left[\sum_{j=1}^{n} w_j - \sum_{j=1}^{n} x_j\right] \quad (2)$$

where x_i represents personal allocations, $w_j = 20$ is the initial endowment, and $\sum_{j=1}^{n} w_j = 80$. The relationship between Equations 1 and 2 is given by $x_i = w_i - g_i$, where g_i is person i's allocation to the project account (a.k.a., cooperation) as in Equation (1). In the experiment, payoffs were presented as in Equation (2a) for the take neutral frame and Equation (2b) for the take non-neutral frame.

$$\begin{matrix}\textit{Total} \\ \textit{points-income}\end{matrix} = \begin{matrix}\textit{points you allocated to your personal account +} \\ \textit{0.4 × (80 – sum of all points allocated to personal accounts)}\end{matrix} \quad (2a)$$

$$\begin{matrix}\textit{Total} \\ \textit{points-income}\end{matrix} = \begin{matrix}\textit{points you took from the project account +} \\ \textit{0.4 × (80 – sum of all points taken from the project account)}\end{matrix} \quad (2b)$$

For each individual, the points not "allocated to [her] personal account" or not points not "[taken] from the project account" comprised her cooperation.

3.2.3 The One-Shot Public Good Game

The purpose of the one-shot public good dilemma was to generate behavioral data with which to assess cooperative type. It was based on Fischbacher & Gächter's (2010) P-experiment and Fischbacher, Gächter, and Fehr's (2001) "strategy-method." First, every participant was asked to make a cooperation decision without knowing others' decisions. This was called the "unconditional decision." Then each participant was asked to complete a "conditional decision table." Completing the table required each participant to decide how they would respond to 21

different average decisions by others. After the unconditional and all conditional decisions were made, one person was selected at random from each four-person group. For the three members of the group who were not selected, the unconditional decision was the payoff relevant amount. The average unconditional decision for those three group members was calculated, and based on that rounded average, the appropriate entry from the randomly selected individual's conditional cooperation table became the payoff relevant amount for that individual. Each individual then received results on his or her computer screen. Results included whether he or she was the randomly selected individual, his or her payoff relevant decision, and the average payoff relevant decision for the other members of the group. The Appendix documents the exact instructions provided to participants, inputs requested from participants, and results provided to participants.

The advantages of this one-shot dilemma are several. First, the decision situation is consistent with a standard public good game. Second, the conditional decision table yields 21 measures of cooperation per person in a very short period of time that are amenable to regression analyses. Third, decisions are incentivized. Fourth, Fischbacher et al. (2001) and Fischbacher and Gachter (2010) effectively used this dilemma to categorize individuals according to cooperation conditioned on others' decisions. This study shares that purpose with the additional objective that an individual's cooperative type be the same regardless of the frame to which she was subject. Because individuals make decisions in response to possible average decisions by the other members of their 4-person group, it is very unlikely that they form beliefs about others' decisions. Thus, based on the notion that framing effects emerge through a process of belief formation and preferences over beliefs about how others behave (c.f., Andreoni, 1995; Dufwenberg et al. 2011; Ellingsen et al., 2012; Park 2000; Sonnemans et al., 1998; Willinger & Zeigelmeyer, 1999), the conditional decision table is unlikely to yield framing effects. Thus, cooperative type, as assessed from the conditional decision table, would be an attribute of the individual, not the frame.

3.2.4 Questionnaire

This original questionnaire was designed specifically with some of the most robust findings of experimental and behavioral economics in mind. The questionnaire consisted of 18 statements intended to elicit feelings of ownership or entitlement;[19] feelings about one's own and other's outcomes, actions, and obligations that relate to social or other-regarding preferences[20] and social norms; [21] and concern about prospective losses that might expose loss aversion.[22] The questionnaire also included five statements about confusion, understanding, and satisfaction. All participants indicated their level of agreement with each statement on a 5-point Likert scale from "strongly disagree" to "strongly agree." The questionnaire also asked participants to report gender, training in economics, and previous experience with "a study like this one." The full questionnaire appears in the Appendix.

3.3 Modeling and Analyses

Recall that the objectives of this research were to identify a discrete number of cooperative types based on behavior, to illuminate the motives underlying the differences in behavior among cooperative types, and to determine if those types and motives were influenced by frames. To those ends I employed group based trajectory modeling (GBTM), exploratory factor analysis (EFA), and nonparametric tests, respectively.

---

[19] Reference dependence is a fundamental concept in prospect theory, pioneered by Kahneman and Tversky (1979). It refers to the notion that "the carriers of value are gains and losses defined relative to a reference point" (Tversky & Kahneman 1991). "The reference point may be interpreted as the default choice, as in the status quo bias or endowment effect literature; the aspiration level, as in aspiration adaptation models; the convention, norm, or belief about what one should choose, as, for example, in cognitive dissonance studies; past consumption, as in addiction, habit formation, status-seeking, or brand loyalty models; and others" (Apesteguia & Ballester, 2009).

[20] Other-regarding preferences (a.k.a., social preferences) refer to valuations, either positive or negative, of the actions and outcomes of others (Camerer & Fehr, 2006).

[21] Consistent with Krupka and Weber's (2013) interpretation of Jon Elster's (1989) definition of social norms, social norms consist of two elements, 1) "prescribed or proscribed behaviors or actions" that are 2) "jointly recognized, or collectively perceived, by members of the population."

[22] Loss aversion is a fundamental concept in prospect theory wherein people behave as though "losses loom larger than corresponding gains" (Tversky & Kahneman, 1991).

3.3.1 Group-Based Trajectory Modeling

Group-Based Trajectory Modelling (GBTM) was developed by Daniel S. Nagin, Bobby L. Jones, and others to empirically identify groups of individuals with similar patterns of behavior over time (Jones & Nagin, 2007; Jones, Nagin, & Roeder, 2001; Nagin, 1999, 2005; Nagin & Odgers, 2010).[23] GBTM is fairly well-known in the psychology and medical literature, especially with regard to group-based developmental trajectories. [24] This is the first application of GBTM to social dilemma experiments.

3.3.1.1 Model Basics

With the goal of grouping individuals according to behavior, I applied GBTM to the conditional decision table. Unlike existing classification strategies for cooperative types, GBTM does not presume a predetermined number of cooperatives types nor does it presuppose a relationship between behavior and motives. GBTM is an empirical method that simultaneously a) identifies groups of individuals with similar patterns of conditional cooperation, b) estimates the likelihood that an individual belongs to each group, and c) models within-group behavior. Each group corresponds to a different cooperative type, individuals are assigned to the cooperative type to which they have the highest likelihood of belonging, and the model of within-group behavior refers to the regression relationship between conditional cooperation and the possible average level of cooperation by others.

More specifically, GBTM is a specialized form of "finite mixture modeling" that simultaneously estimates a maximum likelihood model, Equation (3), the likelihoods of membership in each group, Equation (6), and the cooperation trajectory for each group Equation (7). Importantly, GBTM does not assume that a population actually consists of discrete groups. Instead, it assumes that the behavior of interest varies continuously and widely with

[23] Daniel S. Nagin and Bobby L. Jones are also responsible for developing the SAS procedure, PROC TRAJ, that was used to implement GBTM in this study

[24] GBTM was developed nearly thirty years ago to study the life course of criminal behavior. Since then, GBTM has been used in a wide variety of studies spanning criminology, psychology, epidemiology, and medical research (Nagin & Odgers, 2010), and in more recently to study network formation, poverty, fertility, and labor markets (e.g., Hasan, 2012; Kim & Shin, 2014; Londono et al., 2013; Low, Parker, Hazel, & Welch, 2013; Virtanen et al., 2011).

the goal of summarizing the key features of the data in a representative and parsimonious way (Nagin, 2005, pp. 56-57).

Consistent with the usual conventions of maximum likelihood estimation, GBTM maximizes the likelihood, L, of the observed data

$$L = \prod_{i=1}^{N} P(\mathbf{Y}_i) \tag{3}$$

where $\boldsymbol{Y}_i = \{y_{i1}, y_{i2}, \dots, y_{iT}\}$ represents T=21 measurements of conditional cooperation for individual i, and $P(\boldsymbol{Y}_i)$ is the unconditional probability of observing $\mathbf{Y}_\mathrm{i}$ for individual i. The unconditional probabilities are further deconstructed into the probability of belonging to group j, represented by π_j, and the conditional probability of observing $\boldsymbol{Y}_i$ assuming membership in group j, given by $P^j(\boldsymbol{Y}_i)$. Specifically,

$$P(\boldsymbol{Y}_i) = \sum_{j=1}^{J} \pi_j P^j(\boldsymbol{Y}_i) \tag{4}$$

In accordance with standard conventions in mixture modeling and for the purpose of tractability, conditional independence is assumed so that

$$P^j(\mathbf{Y}_i) = \prod_{t=1}^{T} p^j(y_{it}) \tag{5}$$

where $p^j(y_{it})$ is the probability distribution function of y_{it} for group j, which in this case, is assumed to be censored normal (Nagin, 2005, pp. 24-26).

Approximations of group membership probabilities, π_j, employ the logit model

$$\pi_j = \frac{e^{\theta_j}}{\sum_{j=1}^{J} e^{\theta_j}} \tag{6}$$

where $j = 1 \dots J$ indexes the groups, and θ_j represents the log odds of membership in group j relative to a reference group. Group membership probabilities could be estimated directly, but it is difficult to do in practice. In contrast, this approximation is computationally straightforward while also conforming to the requirements of group membership probabilities. Namely, each π_j is between 0 and 1, and all π_j's sum to 1 (Nagin, 2005, p. 41).[25]

[25] This approximation also allows for an extension of the model that includes estimation with time-invariant covariates, z_i, with the multinomial logit model, $\pi_j(z_i) = \frac{e^{z_i\theta_j}}{\sum_{j=1}^{J} e^{z_i\theta_j}}$ (Nagin, 2005, p. 97). In this study, I extended the four-group linear model to evaluate the effect of frame, gender, training, and experience on the probability of group membership relative to a baseline group, which in this case was non-cooperators. A series of multinomial logit regression models showed that frame did not influence the likelihood of belonging to a particular cooperative type; there was no significant relationship between frame, training, and the likelihood of belonging to a

Because conditional cooperation clustered at the top and bottom of the cooperation range (i.e., at 0 and 20 points), each group's trajectory was estimated to fit a polynomial using a censored normal regression model[26] of an unobservable, latent variable, y_{it}^*, on the possible average level of cooperation by others, x_{it}, on as follows:

$$y_{it}^* = \boldsymbol{\beta}^j \mathbf{X}_{it} = \beta_0^j + \beta_1^j x_{it} + \beta_2^j x_{it}^2 + \beta_3^j x_{it}^3 + \varepsilon_{it} \quad (7)$$

The latent variable, y_{it}^*, represents "what might have been" had cooperation been unbounded and relates to the measured variable for conditional cooperation, y_{it}, as follows (Nagin, 2005, pp.28-29):

$$\begin{aligned} y_{it} &= 0 && for\ y_{it}^* < 0 \\ y_{it} &= y_{it}^* && for\ 0 \le y_{it}^* \le 20 \\ y_{it} &= 20 && for\ y_{it}^* > 20 \end{aligned} \quad (8)$$

I describe implementation of GBTM, the essential elements of group-based models, and model selection below. I forego further technical details of GBTM and refer the reader to Daniel Nagin's comprehensive book (2005), summary papers (Jones & Nagin, 2007; Jones, Nagin, & Roeder, 2001; Nagin, 1999), and a survey of GBTM applications (Nagin & Odgers, 2010).

3.3.1.2 Model Implementation and Selection

I implemented GBTM with the TRAJ procedure using SAS software with censored normal regression models (MODEL CNORM).[27] Among the estimated statistics and standard fit statistics, essential outputs included estimates of trajectory parameters for each group, $\boldsymbol{\beta}^j$ in Equation (7), the probability that a randomly selected individual will belong to each group, π_j, and the probability of membership in each group for each individual conditioned on observed behavior, called posterior probabilities, $P(j|\mathbf{Y}_i)$. These outputs guide model selection,

cooperative type; there was no significant relationship between frame, experience, and the likelihood of belonging to a cooperative type; and there was no significant relationship between the interaction of frame and gender and the likelihood of belonging to a cooperative type. However, women were more likely to be low cooperators than men. Men were more likely to be non-cooperators than women.

[26] Also known as a double-limit Tobit model, Wooldridge (2010, p. 704) states that the double-limit Tobit can be appropriately estimated when "we actually see pileups at the two endpoints and then a (roughly) continuous distribution in between," as was the case for conditional cooperation at every possible average level of cooperation by others.

[27] The TRAJ procedure was developed by Bobby L. Jones, Daniel S. Nagin, and Kathryn Roeder (2001).

specifically selection of the number of groups and the functional form of each group's trajectory, keeping in mind that group corresponds to cooperative type.

The overarching goal of model selection is a balance between parsimony and explanatory power. Following the two-stage model selection process described by Nagin (2005, p. 66), I systematically estimated and evaluated dozens of potential models that combined different numbers of groups (i.e., 2 to 10) and polynomial trajectories (i.e., linear, quadratic, and cubic). First, I selected the appropriate number of groups. Then I determined the appropriate order for each group's trajectory.

In most applications of GBTM, selecting the number of groups prioritizes the Bayesian Information Criteria (BIC), a widely-accepted diagnostic that rewards parsimony and explanatory power in model selection. The BIC is given by

$$BIC = \log L - 0.5k \log N \tag{9}$$

As the number of parameters, k, increases, the log likelihood, $\log L$, also increases. Thus, the second term "exacts a penalty" for additional parameters in proportion to half the log of the sample size, N (Nagin, 2005, pp. 63-67). The number of parameters rises with the number of groups. The BIC is an approximation of the log of the Bayes Factor, $2 \times BIC \approx 2 \log_e B_{10}$. The Bayes factor gives the posterior odds that the alternative hypothesis is correct and is interpreted as the degree of evidence favoring the alternate model (Jones et al., 2001; Nagin, 2005, pp. 68-69). When comparing two models, the null hypothesis is that the model with fewer trajectories provides the best fit, and a "sufficient" increase in the BIC (ΔBIC) favors selection of the more complex model.[28]

While the BIC criterion often yields a clear cut choice, in some applications, it does not. In this case, the BIC continued to increase as the number of groups in the model increased for models with cubic, quadratic, and linear trajectories. However, Table 1.2 shows that the BIC had a distinctive pattern of very large increases from 2 to 3 to 4 groups; the increases from 4 to 5 groups were orders of magnitude less; and the BIC increases from 5 to 6 groups were large,

[28] According to general guidelines, increases in the BIC between 0 and 2 provide weak support for the more complex model, increases between 2 and 6 provide moderate evidence in favor of the more complex model, increases between 6 and 10 provide strong evidence in favor of the more complex model, and increases above 10 provide very strong evidence for the more complex model (Andruff, Carraro, Thompson, & Gaudreau, 2009; Jones et al., 2001). If the BIC decreases as complexity increases, the simpler model is preferred.

but the sixth group was very small (less than 3% of participants).[29] More importantly, the cooperative types identified in the four group models – non-cooperators, conditional cooperators, low cooperators, and high cooperators – persisted as more groups were added. Specifically, the conditional cooperator group splintered into less generalizable groups of conditional cooperators that were quite small (3% or less) and statistically insignificant; and the already small high cooperator group (i.e., 7%) split into even smaller groups (2% or less).[30]

After selecting the four-group model, choosing the order of the polynomial function for each groups' trajectory was less important (Andruff, Carraro, Thompson, & Gaudreau, 2009; Nagin, 2005, p. 66) and relatively simple. For models with 2 to 10 groups, I tested cubic, quadratic, and linear trajectories. After selecting the four-group cubic model, I sequentially removed non-significant cubic then quadratic terms, resulting in a model with only linear trajectories. The convention in GBTM is to retain linear terms regardless of significance.

3.3.2 Exploratory Factor Analysis

Recall that the questionnaire was designed to expose motives often associated with behavior in public good dilemmas. However, I did not assume that any single item in the questionnaire would necessarily provide evidence of a specific motive. Instead, associations among questionnaire responses were expected, notable differences among questionnaire were also expected, and the common nature of associated questionnaire items were expected to reveal underlying motives. Exploratory factor analysis (EFA) provides a means to identify associations among different questionnaire items and assess the motives that those items together represent.

Specifically, EFA is a data-driven, exploratory, and descriptive technique intended to reveal the "number and nature of latent [factors] that explain the variation and covariation in a set of measured variables" (Preacher & MacCallum, 2003). In this case, each measured variable, y_j, corresponded to one questionnaire item. Motives were represented by latent factors, η_m,

[29] Results for quadratic and cubic trajectories were similar.

[30] In a similar situation, Nagin and Tremblay (2001) determined that the "addition of more groups was not informative" and chose the less complex model. Also, Nagin (2005, p. 62) states that "model selection based on the mechanical and rigid application of formal statistical criterion may lead to an inferior choice. Good judgement and real world domain knowledge are also required in the model selection process."

and factor loadings, λ_{jm}, indicate how strongly underlying motives influenced a questionnaire item. In general, the common factor model represents each measured variable, y_j, in terms of latent factors, η_m, with factor loadings, λ_{jm}, and error term, ε_j:

$$y_j = \lambda_{j1}\eta_1 + \lambda_{j2}\eta_2 + \cdots + \lambda_{jM}\eta_M + \varepsilon_j \quad (10)$$

Large factor loadings reveal which measured items are good indicators of the various latent factors (Brown, 2015, p. 18).

Ultimately, exploratory factor analysis (EFA) included 14 questionnaire items and all 256 participants. First, I confirmed that correlations between many pairs of questionnaire items were large and significant (i.e., above 0.3 in absolute value and significant at the 5 percent level). Then, I extracted three factors using the iterated principal factor method (METHOD=PRINIT) via the FACTOR procedure using SAS software. Using an oblique rotation (ROTATE=PROMAX), I achieved high loading on non-overlapping subsets of measured variables and low loadings otherwise (Preacher & MacCullum, 2003). After selecting a three factor model, I generated factor scores which can be thought of as the values of the latent factors if they were measurable.

Preliminary factor models that included all 23 questionnaire items yielded factors with three or fewer items. A common rule of thumb requires at least three indicators per factor, so 7 items related to confusion, satisfaction, and reference dependence were excluded. Two additional measured variables were also excluded from the EFA, one measured variable that yielded high and nearly equal factor loading on more than on factor and another measured variable with relatively small factor loadings (i.e., near 0.3).

3.3.4 Non-Parametric and Parametric Tests

Because most measured variables were not normally distributed, nonparametric tests were used to compare behavior and motives across frames and cooperative types. The Kruskal Wallis test is a non-parametric test for differences in the medians of a non-normal dependent variable (e.g., cooperation and questionnaire responses) for more than two categories of an independent variable (e.g., frame and cooperative type). The Kruskal Wallis test reduces to the Wilcoxon-Mann-Whitney test when the independent variable is dichotomous (e.g., gender,

experience, and training). Nonparametric tests were conducted using SAS software and the NPAR1WAY procedure with the WILCOXON and DSCF options. Parametric tests (ANOVA and *t*-tests) were used to assess differences in factors among and between cooperative types and were implemented using SAS software and the GLM and TTEST procedures.

4 Results

First, cooperation in the one-shot public good dilemma proved to be consistent with other public good experiments. Second, four robust, empirically distinct, and behavior-based cooperative types emerged via group-based trajectory modeling (GBTM). Cooperative types included a largely overlooked group of individuals who consistently cooperated at low but non-negligible levels. Third, exploratory factor analysis (EFA) revealed three distinct factors underlying questionnaire responses that correspond to feelings about own and others' obligations, perceptions of others' actions, and self-interest. Fourth, non-parametric and parametric tests exposed highly significant differences in reported reactions and motives among and between cooperative types. Generally, reactions and motives of non-cooperators differed dramatically from each of the three cooperator types (i.e., low cooperators, conditional cooperators, and high cooperators), and the three cooperator types differed from each other in the level and nature of other regarding preferences.

4.1 Descriptive Statistics

As expected, many individuals cooperated at highly variable and clearly non trivial levels either by giving to the project account or taking less from the project account than they could have.[31] Table 1.3 and Figure 1.3 summarize these results. Specifically, average unconditional cooperation was 41% of the initial endowment, ($M = 8.16, SD = 6.84$), and mean conditional cooperation averaged to 34% of the initial endowment ($M = 6.79, SD = 4.45$). [32] Results like these are consistent with previous public goods experiments where cooperation varied from

[31] T-tests and Wilcoxon signed rank tests resoundingly showed average conditional cooperation to be nontrivially greater than zero. All tests were highly significant (i.e., $p < 0.0001$).

[32] Mean conditional cooperation is the within-person mean of conditional cooperation according to each individual's conditional decision table.

not at all to 100%, and average cooperation typically fell between 40 to 60% of the initial endowment (Ledyard, 1995 as cited in Chaudhuri, 2011). In addition, mean and median conditional cooperation increased with the possible average level of cooperation by others, as did the variance and interquartile range, results that are consistent with the known co-existence of non-cooperators and conditional cooperators. Furthermore, unconditional and conditional cooperation "stacked-up" at focal levels of cooperation, especially at the allowable extremes of 0 and 20 points. Finally, the distributions of all cooperation variables were skewed and clearly not normally distributed.

4.2 Cooperative Types

Behavior in the one-shot public good dilemma revealed considerable heterogeneity in cooperative behavior. Figure 1.4 provides a sample of actual decisions of six individuals in the one-shot public good dilemma. These examples are similar to what Fischbacher et al. (2001) and Fischbacher and Gächter (2010) observed and representative of the four cooperative types identified in this study. Specifically, there were individuals who did not cooperate (Panel A), individuals who cooperated more on average than others (Panel B), individuals who perfectly and "imperfectly" matched the cooperation of others (Panels C and D), "hump-shaped" cooperators (Panel E), and others (Panel F).

Unpacking heterogeneity was the foremost objective of this research and was achieved using group-based trajectory modeling (GBTM).[33] Four robust and distinct cooperative types emerged to create a comprehensive, well-delineated, descriptive, and relatively simple representation of heterogeneous cooperation. Table 1.4 presents that best-fitting group-based trajectory model. Trajectories were characterized by highly significant intercepts and linear

[33] To characterize heterogeneity, Fischbacher and Gächter (2010) regressed conditional cooperation on other's possible average level of cooperation for each individual. Then they plotted the frequencies of pairs of slope and mean conditional cooperation. This same procedure was performed with the data from this experiment. Results were similar overall. In addition, type-specific bubble scatter plots confirmed that non-cooperators clustered near the origin, and conditional cooperators clustered around slope of 1 and mean conditional cooperation of 10 points. In addition high cooperators occupied the upper region of the regression slope versus mean conditional cooperation plot with shallow slopes and above average mean conditional cooperation. Low cooperators included individuals who generally had shallow individual linear regression coefficients and below average mean conditional cooperation.

coefficients, probabilities of group membership were highly significant, and trajectories were quite obviously distinct.

Figure 1.5 plots average and predicted conditional cooperation for each cooperative type and gives an intuitive representation of regression results.[34] Study participants were assigned to the cooperative type for which the posterior probability of membership was highest. Unsurprisingly, actual cooperation corresponded quite nicely to predicted cooperation. Non-cooperators did not cooperate ($N = 48$)[35] and are not expected to cooperate ever (i.e., predicted values for the latent variable were all less than zero, so that predicted values for the observed variable were all zero). Conditional cooperators approximately matched the assumed level of cooperation by others ($N = 131$) and are expected to cooperate less than, but in lock step with, the possible average level of cooperation by others (i.e., regression of the latent variable yielded a negative intercept, and a linear coefficient close to 1).[36] Low cooperators consistently cooperated at low but non-trivial levels ($N = 60$) and are expected to cooperate at consistently low levels with a negligible response to the possible average level of cooperation by others (i.e., regression of the latent variable yielded a positive, small, and nontrivial intercept and a linear coefficient that, although statistically significant, was near 0).[37] High cooperators cooperated at a relatively high level regardless of others' assumed level of cooperation ($N = 17$) and are expected to cooperate more on average than others (i.e., regression of the latent variable yielded a positive, relatively large, and highly statistically significant intercept), while

[34] Predicted conditional cooperation for the *j*th group is given by $E(y_{it}^j) = \Phi_{min}^j S_{min} + \hat{\beta}^j x_{it}(\Phi_{max}^j - \Phi_{min}^j) + \sigma(\phi_{min}^j - \phi_{max}^j) + (1 - \Phi_{max}^i)S_{max}$, where ϕ and Φ are the density function and cumulative distribution function of a normal distribution with mean $\beta^j X_{it}$ and standard deviation σ. Φ_{min}^j and Φ_{max}^j are $\Phi^j[(S_{min} - \hat{\beta}_{it}^j)/\sigma]$ and $\Phi^j[(S_{max} - \hat{\beta}_{it}^j)/\sigma]$, respectively, and $\hat{\beta}^j$ is the maximum likelihood estimate of β^j (Nagin, 2005, p. 32). In this case, $S_{min} = 0$ and $S_{max} = 20$.

[35] Cooperation was not exactly zero for all non-cooperators, but statistically trivial.

[36] Fischbacher and Gächter (2010) used the term "imperfect conditional cooperators" to describe conditional cooperation that was in proportion to and somewhat less than the *beliefs they formed about others*. In this study, I use the term "conditional cooperators" to describe individuals who cooperated on average in proportion to and either equal to or somewhat less than *possible average level of cooperation by others*.

[37] Via Fischbacher et al.'s (2001) and Fischbacher and Gächter's (2010) behavior-based methodology, most low cooperators would have been identified as triangle cooperators and unclassifiable. Via Kurban and Houser's (2005) behavior-based methodology, 45 low cooperators would have been assessed as free-riders, three would have been identified as reciprocators, and 12 would have been unclassifiable. Without expanding the experiment and to collect additional data, it is impossible to assess how low cooperators would have been classified via motives-based methods like the Decomposed Game.

simultaneously conditioning their decisions on others behavior to some extent (i.e., the linear term in the regression was positive, highly statistically significant, and less than 1).

Figure 1.5 also illustrates the dramatically different patterns of behavior among the four cooperative types as is also evident among the individual behavior show in Figure 1.4. In Figure 1.4, the individual in Panel A was a non-cooperator, panel B shows a high cooperator, panels C and D depict conditional cooperators, and panels E and F depict low cooperators. Lending additional persuasive evidence, Kruskal Wallis tests of unconditional cooperation, mean conditional cooperation, and every measurement of conditional cooperation from the conditional decision table confirmed highly significant differences in cooperation across cooperative types (all p-values were negligible).

4.3 Questionnaire Analysis

Questionnaire analyses focused on two goals: 1) identifying the motives behind decision-making in public goods dilemmas, and 2) assessing the relationships among questionnaire responses, motives, and cooperative types. Nonparametric tests of questionnaire responses among and between cooperative types confirmed that study participants experienced and conducted decision-making very differently, and those differences corresponded with cooperative types. Exploratory factor analysis (EFA) demonstrated that at least three distinct motives drove decision-making that included feelings about own and others' obligations, others' actions, and self-interest. In combination, these analyses revealed that non-cooperators were more self-interested, less concerned with others' actions, and had weaker or no feelings of obligation compared to conditional and high cooperators. Low cooperators were similar to non-cooperators with regard to self-interest, but otherwise were more strongly motivated by feelings of obligation and perceptions of others' actions. Low cooperators, however, also differed from conditional and high cooperators. Low cooperators were significantly more self-interested than conditional and high cooperators and less motivated by feelings of obligation.

4.3.1 Questionnaire Descriptive Results

Table 1.5 reports questionnaire descriptive statistics. In general, participants in this study responded neutrally (i.e., near 3 on a 5-point Likert scale) to questionnaire items regarding own and others' outcomes (Q03-Q06), actions (Q07-Q10), and obligations (Q11-Q16), with few exceptions. They responded more strongly, on average, to questionnaire items related to ownership (Q01 and Q02), prospective losses (Q17 and Q18), understanding and confusion (Q19-Q21), and satisfaction (Q22-Q23). Specifically, individuals were roughly neutral with regard to others' earnings (Q04), competition (Q05), and pure self-interest (Q06), but clearly wanted to earn as many points as they could (Q03). Individuals were also generally neutral with regard to their own cooperative and non-cooperative choices and to others' non-cooperative behavior (Q07, Q08, and Q10), but felt strongly and positively about others' cooperative behavior (Q09). In addition, neutrality prevailed on average among responses to statements regarding one's own and others' obligations including one's own reciprocal obligations, (Q11, Q12, Q13, and Q15). However, individuals generally agreed, albeit weakly, that other's reciprocal obligations were important (Q14 and Q16). Individuals generally felt ownership of their personal endowments (Q01) but not of all possible project points (Q02). Finally, individuals generally were not concerned with prospective losses (Q17 and Q18), not confused during the study (Q19, Q20, and Q21), satisfied with the results of their decisions (Q22), and believed others were satisfied as well (Q23).

4.3.2 Nonparametric Tests of Questionnaire Responses

Averages, however, do not tell a complete story. Substantial heterogeneity is evident in the standard deviations and interquartile ranges given in Table 1.5. As shown in Table 1.6, aggregating questionnaire responses by cooperative type and conducting Kruskal Wallis tests revealed highly significant differences across cooperative types with regard to own and others' outcomes (Q03-Q06), own actions (Q07-Q08), and own and others' obligations (Q11-Q16). Individual questionnaire items related to ownership of one's personal account (Q02),

understanding (Q19), and satisfaction (Q22) were also significantly different across cooperative types. [38]

Focusing on the three categories of questionnaire items where statistically significant differences were observed among all four cooperative types – namely outcomes, actions, and obligations – non-cooperators often distinguished themselves from at least one other type as more self-interested (Q03, Q06, and Q07), more competitive and less altruistic (Q05, Q04, and Q08), and less concerned with obligations (Q11-Q16). At the extreme, non-cooperators' responses differed significantly from every other cooperative type on four measures (Q06, Q08, Q11, and Q12). Non-cooperators most strongly express pure self-interest while other types were roughly neutral or disagreed (Q06). Non-cooperators also disagreed with feeling good about allocating points to the project account, but low, conditional, and high cooperators were neutral or agreed (Q08). In addition, non-cooperators were the least concerned with their obligations to the project (Q11 and Q12). In contrast, other types were roughly neutral.

Focusing still on the three categories of questionnaire items where statistically significant differences were observed among all four cooperative types, pairwise comparisons of cooperative types revealed a subtle gradient of self-interest to other-regarding among conditional, low, and high cooperators, where other-regarding refers to both preferences over others' actions and outcomes and feelings of obligation. First, responses of low cooperators were sometimes similar to non-cooperators. Non-cooperators and low cooperators were similarly self-interested (Q03) and competitive (Q04 and Q05). Both non-cooperators and low cooperators also generally reported feeling better about allocating points to their personal accounts than conditional and high cooperators (Q07), and were least concerned with others fulfilling obligations to the project (Q13 and Q14). However, conditional cooperators were similar to low cooperators with regard to some self-interest and competition measures (Q03, Q05, and Q06), different on others (Q04 and Q07). Approximately neutral feelings about obligations, however, were nearly always similar for low and conditional cooperators (Q11,

[38] For items Q02, Q13, Q19, and Q22, Kruskal Wallis tests among all four types were not significant when the Bonferonni correction for multiple comparisons was applied. The Bonferroni correction is a modification of the threshold for statistical significance, α, based on the number of measurements, n. In this case statistical significance at the 0.05 level requires a p-value less than $\alpha/n = 0.05/23 = 0.0022$.

Q12, Q13, Q15, and Q16). Overall, low cooperators were sometimes similar to high cooperators with regard to self-interest (Q06 and Q07) and obligations (Q11-Q16), but conditional cooperators and high cooperators were statistically indistinguishable on every measure (Q03-Q16).

4.3.3 Exploratory factor analysis

Questionnaire differences among cooperative types became more salient and easier to characterize by way of exploratory factor analysis (EFA). Exploratory factor analysis revealed three distinct factors underlying questionnaire responses. Those factors included feelings about own and others' obligations, perceptions of others' actions, and self-interest.

Table 1.7 presents factors and factor loadings. Attending to one factor at a time, we see that four of the five questionnaire items in the first factor relate to feelings of obligation (Q11-14) that are reminiscent of social norms and fairness, equity, and reciprocity preferences. The fifth questionnaire item (Q08) in the first factor relates to the notion of feeling good about doing good, a concept sometimes referred to as "warm glow," "altruism," or "impure altruism." On average, responses to those questionnaire items were neutral ($M = 3.03$), but every indicator in the obligation factor differed significantly among cooperative types according to Kruskal Wallis tests (as shown in Table 1.6).

Unsurprisingly, estimates of factor scores (i.e., the estimated value of the factor for each individual) were highly significantly different among cooperative types (ANOVA: $F(3{,}255) = 22.37, p < 0.0001$ and Kruskal Wallis: $\chi^2(3, N = 256) = 51.26, p < 0.0001$). Pairwise t-tests and Wilcoxon-Mann-Whitney tests of the first factor, given in Table 1.8, showed statistically significant differences between every pair of cooperative types except for conditional and high cooperators.[39] Thus, even though participants on average did not report strong feelings of obligation, feelings of obligation distinguished non-cooperators from conditional, low, and high cooperators and differentiated low cooperators from conditional and high cooperators. Non-cooperators appeared to be least motivated by feelings of obligation, and high and conditional

[39] Controls for multiple comparisons ($n = 6$) were applied using the Bonferroni correction for t-tests and the Dwass, Steel, Critchlow-Fligner (DSCF) correction for Wilcoxon-Mann-Whitney tests.

cooperators appeared to be most influenced by feelings of obligation. Low cooperators were in between.

As shown in Table 1.7, the unifying concept in the questionnaire items that make up the second factor is perceptions of other actions (Q16, Q15, Q17, Q10, and Q9). In general, participants agreed weakly ($M = 3.48$) with negative feelings about others' non-cooperative behavior. However, only the questionnaire items about feeling "cheated" (Q15 and Q16) differed significantly across cooperative types according to Kruskal Wallis tests, a result driven by non-cooperators who disagreed with feeling cheated and low and conditional cooperators who were approximately neutral or weakly agree with feeling cheated (see Table 1.6). Thus, while, the perception factor did differ significantly among cooperative types (ANOVA: $F(3{,}255) = 17.71, p < 0.0001$ and Kruskal Wallis: $\chi^2(3, N = 256) = 19.19, p < 0.0003$), only pairwise differences between non-cooperators and each of the other types were present. Factor scores between low, conditional, and high cooperators did not differ significantly (see Table 1.8).

Finally, five questionnaire items related to self-interest (Q03, Q06, and Q07) and competition (Q05) formed the third factor (see Table 1.7). In general, study participants agreed with self-interested and competitive statements ($M = 3.78$). As in the first factor, differences among cooperative types were highly significant among all four questionnaire items making up the third factor (see Table 1.6) as were factor scores (ANOVA: $F(3{,}255) = 9.25, p < 0.0001$ and Kruskal Wallis: $\chi^2(3, N = 256) = 37.39, p <$0.0001). Pairwise tests showed that non-cooperators and low cooperators were similarly self-interested and more self-interested than conditional cooperators and high cooperators who were statistically indistinguishable with regard to self-interest (see Table 1.8).

These results make clear that at least three underlying factors drove decision-making in the experiment, namely feelings of obligation, perceptions of others' actions, and self-interest.[40] These results also make clear strong associations between behavior and motives that

[40] Furthermore, evidence from the process of factor model selection and Kruskal Wallis tests suggested that additional factors were also at work. Specifically, if more questionnaire items regarding reference dependence (Q01 and Q022) and warm glow (Q04 and Q08) preferences had been included in the questionnaire, two additional factors may have emerged that could have played a role in differentiating motives among cooperators. Evidence from discriminant and regression analyses support the future implementation of a more extensive questionnaire.

are not easily inferred, especially with regard to low-cooperators who distinguish themselves from each of the other cooperative types with regard to obligations. Low cooperators are more concerned with obligations that non-cooperators, but less than conditional and high cooperators. Low cooperators are also self-interested, much like non-cooperators, and but more self-interested than conditional and high cooperators.[41]

4.4 Frame Immunity

In this section, I briefly report two nested results that complement the core objectives of this research. First, overall the one-shot public good dilemma and the questionnaire showed no framing effects. The absence of framing effects in the one-shot dilemma is consistent with the notions that frames influence beliefs which influence cooperation, as hypothesized in Section 3.2.3, and frames act through beliefs, not through preferences (Ellingsen et al., 2012). Second, there was not a statistically significant association between cooperative type and frame. Thus, the identification of four cooperative types and three underlying motives as well as the systematics relationships between cooperative type and motives were not influenced by frame.

Specifically, the Kruskal Wallis tests reported in Tables 1.9 and 1.10 show that behavior in the one-shot public good dilemma and the reactions reported via the questionnaire did not differ significantly across the four frames after the Bonferroni correction for multiple comparisons was applied. Pairwise tests, controlling for multiple comparisons, also revealed that there were no framing effects between pairs of frames in the one-shot dilemma and in the questionnaire with one exception.[42] Factors scores also did not differ among or between frames (Factor 1: $X^2\ (3, N = 256) = 3.59, p = 0.3088$; Factor 2: $X^2\ (3, N = 256) = 4.79, p = 0.1876$; Factor 3: $X^2\ (3, N = 256) = 0.5853, p = 0.8998$). Lastly, a Chi Square test

[41] In addition, separate factor analyses for distinct subpopulations (i.e., women, men, trained, not trained, experienced, and not experienced) yielded similar factors and factor loadings.
[42] The exception is a statistically significant difference between take non-neutral and give neutral for Q09.

($X^2\ (9, N = 256) = 11.29, p = 0.2566$) and Fisher's Exact test ($p = 0.2651$) showed no association between frame and cooperative type.[43]

5 Conclusion

Results of this study strongly support the conclusion that the decision-makers in this public good dilemma were heterogeneous, in behavior and in motives. This study revealed and modeled the within group behavior of four distinct, behavior-based types of decision-makers that expressed clearly discernible and different motives when engaged in a public good dilemma. These behavior-based cooperative types included non-cooperators who did not cooperate or cooperated at negligible levels regardless of peer behavior, conditional cooperators whose cooperation was on average less than their peers but increased in equal proportion as others' possible average cooperation increased, and high cooperators who cooperated more on average than their peers with small increases in cooperation as others' possible average cooperation increased. In addition, this study revealed a relatively large and robust group of low cooperators, not previously differentiated in the experimental literature. These low cooperators consistently cooperated at low levels regardless of others' possible average cooperation. Conditional cooperators were the largest group (51%), but this previously unrecognized group of low cooperators (23%) was larger than the non-cooperator group (19%) and the high cooperator group (7%). Furthermore, the low cooperator type was a robust group. They consistently emerged as a statistically distinct group in group-based trajectory models with 4 or more groups, and low cooperators were highly significantly different from each of the other three cooperative types according to pairwise Wilcoxon-Mann-Whitney tests of mean conditional cooperation.[44]

[43] Recall that frames were assigned to experimental sessions with the expectation that subpopulations would be similar. While this held unequivocally for age and training in economics, there was a marginal and weak association between gender and frame ($X^2\ (3, N = 256) = 6.09, p = 0.1073$) and a highly significant association between experience and frame ($X^2\ (3, N = 245) = 13.35, p = 0.0039$). In response, multinomial logit regression models were employed to demonstrate further that frame did not influence the probabilities of belonging to a cooperative type.

[44] All six pairwise tests of mean conditional cooperation yielded $p < 0.0001$, and all tests controlled for multiple comparisons using the Dwass, Steel, Critchlow-Fligner (DSCF) correction.

In addition to identifying cooperative types and assessing cooperative type for each individual, this study revealed subtle and systematic differences in motives among types that could not have been predicted and could not have been convincingly inferred from behavior via the notion of revealed preference. Overall, conditional, low, and high cooperators were more "other-regarding" than non-cooperators, but "other-regarding" proved to be different from "not self-interested." Non-cooperators were more self-interested than conditional and high cooperators, but not more than low cooperators. In fact, low cooperators were self-interested just like non-cooperators, and more self-interested than conditional and high cooperators. Compared to non-cooperators, low, conditional, and high cooperators were all more concerned with others' actions and had stronger feelings regarding their own and others' obligations. However, low cooperators also distinguished themselves from conditional and high cooperators as less concerned with obligations. In a sense, low cooperators were a hybrid of non-cooperators and conditional and high cooperators – a large, robust, cooperating, and therefore important hybrid who could be compelled to cooperate more via very different means than other types of decision-makers.

Identification of low cooperators and the ways in which they are differently motivated are fundamental contributions of this research. Many previous attempts to classify heterogeneity share a limitation that a priori excludes low cooperators. Those studies pre-specified the number and nature of types and presumed associations between behavior and motives. In those cases, low cooperators likely would have been "unclassified" or lumped with non-cooperating, free-riding, or individualistic types based on their low levels of cooperation and self-interested motives. Even Fischbacher et al. (2001) and Fischbacher and Gächter (2010), who did not pre-specify types or motives, did not identify low cooperators. Their classification methods allowed the differences among individuals identified as low cooperators to obscure the trend in low cooperator behavior. Namely, whether cooperation was triangular, hump-shaped, or in other ways non-linear as others' level of cooperation increased, cooperation among low cooperators overall tended to be low, but non-trivial. Furthermore, when I assessed type using Kurzban and Houser's "statistical-type classification method" (see Section 2.3), most of the low cooperators identified in this study were instead classified as "free-riders."

Although a unique contribution to the experimental literature, an analogous group of minimum threshold cooperators with a "buy-in" mentality have been differentiated and studied in the charitable giving literature (Rose-Ackerman, 1982) and more recently in the green-electricity literature (Jacobsen, Kotchen, & Vandenbergh, 2012). Rose-Ackerman (1982) defined "buy-in" mentality in the context of charitable giving, where a minimum contribution led those with a "buy-in" mentality to feel entitled to the full range of services provided by the charity. Jacobsen et al. (2012) reinterpreted this concept in the context of green-electricity to represent households who feel as though they have "done their part" after purchasing green electricity.[45] In the context of this public good dilemma, low cooperators may be operating via a "buy-in" mentality as well, in the sense that they cooperate just enough to assuage feelings of obligation toward others that they might feel as beneficiaries of the public good. Similarly, one might characterize the mentality of low cooperators in terms of guilt aversion, a "desire not to let others' down" (Dufwenberg et al., 2011), or as avoiding feeling bad about not cooperating (i.e., the "cold prickle") rather than seeking to feel good about contributing to group well-being (i.e., "warm glow") (Andreoni, 1995).

Finally, framing is present in every decision and until now has received little, if any, attention in assessing cooperative type and motives. The final contribution of this study is that identification and assessment of cooperative types and motives achieved via group-base-trajectory modeling and exploratory factor analysis were not sensitive to framing. Type and motives, as determined in this study, appear to be attributes of the individual, like gender or level of education, not of the frame.

Within the context of existing policy and in environmental policy in particular, non-cooperators and high cooperators seem to overshadow policy-making even though they made up only a quarter of study participants. The spectrum of approaches intended to provide and protect environmental goods is fairly narrow and dominated by regulations (a.k.a. mandatory standards and command-and-control), but also includes price-based policies (e.g., taxes, subsidies, and markets) and voluntary provision (e.g., charitable giving, environmental offsets,

[45] In Jacobsen et al. (2012), 45% of 910 households participated minimally in the energy conservation program and therefore in a way that was consistent with a "buy-in mentality."

payments for ecosystem services, participation in renewable energy programs). Regulations and price-based policies typically target firms and are based on the assumption that all decision-makers are self-interested, which is unequivocally false, or the notion that prioritizing the purely self-interested minority (i.e., the non-cooperators in this study) in policy design and implementation is optimal.

In contrast, voluntary provision most often targets individuals, appealing to altruistic motives and therefore to a small minority of decision-makers (i.e., the high cooperators in this study) who engage in environmentally friendly behaviors at an material cost to themselves, without regard for their peers' participation or lack thereof. Allcott (2011) presents a notable exception in which a non-price energy conservation program, run by the company OPOWER, employed "insights from behavioral science" and "carefully crafted psychological cues" such as social norms and competition to reduce household electricity consumption without price incentives or mandates. These tactics are likely to be especially effective with conditional cooperators, whose cooperation was on average less than their peers but increased in equal proportion as others' possible average cooperation, and non-cooperators who were more competitive than other decision-makers. Having produced a comprehensive and tractable classification of decision-makers engaged in a public good dilemma, this study has real-world traction in the context of innovative policy-making as illustrated by the OPOWER example.

Furthermore, by identifying four distinct behavioral types and differentiating motives, this study contributes to our existing understanding of heterogeneity among decision-makers engaged in public good dilemmas. Other studies that focused on heterogeneity among decision-makers showed that group composition (e.g., Burlando & Guala, 2005; Kurzban & Houser, 2005), interactions between different types of cooperators (Chaudhuri, 2011), and the strategic incentives that emerge in heterogeneous populations (Camerer & Fehr, 2006) greatly influence aggregate cooperation. While Kurzban and Houser (2005) demonstrated that greater proportions of cooperative types lead to more cooperation, Camerer and Fehr (2006) argued that strategic incentives can trigger cooperation even when cooperators are in the minority. Whether it is mass, strategy, or both, the development of a comprehensive, coherent, and a conceptually tractable representation of heterogeneous behavior and motives in social

dilemma experiments advances behavioral science, models of decision-making, and our understanding of real-world dilemmas. This study demonstrates that models of decision-making that embrace heterogeneity are not only possible, but necessary. Similarly, policy-making that reflects population heterogeneity will diverge from the existing a silver-bullet approach, in which a single intervention targets the majority of decision-makers or lowest common denominator (i.e., the least cooperative). Instead, policy-making will exploit the differences and interactions between different types of individuals by way of several simultaneous interventions, deliberately designed to complement each other.

Important questions remain. Most importantly, recall from Section 2.3 that the association between cooperation in public good dilemmas and cooperative type determined via other methodologies was ambiguous at best. Is cooperative type, as determined in this study, relevant to public good dilemmas? To start, is there a demonstrable relationship between the cooperative types identified here and cooperation in repeated public good experiments? If so, how robust is that relationship? For example, even though frames did not influence the results of this study, every public good decision has a frame and therefore may be vulnerable to framing effects. When framing effects are present, does cooperative type moderate the effect of frame? In other words, is the effect of frame different for different types of decision makers? I explore these questions in my subsequent work (Jackman, 2016b).

In addition, this study and the vast literature on social preferences resoundingly demonstrates that what individuals know or believe about others' actions and outcomes in public good dilemmas influences how much individuals cooperate. Conditional cooperation is the quintessential example of this behavior. In this study, individuals were asked to report how much they would cooperate in response to every possible level of cooperation by others, but in real-world dilemmas and in most public good experiments, individuals do not know how much others cooperate when making their own decisions. If some individuals include in their decision process how much others cooperate, as they clearly appear to do, then forming expectations of others' behavior is a critical piece of that process. Do different types of decision-makers form beliefs differently, or do they respond to the same beliefs differently when making cooperation decisions?

Finally, evidence from several studies suggests that framing effects emerge via the process of forming beliefs about others' likely behavior (Dufwenberg et al. 2011; Ellingsen et al. 2012; Sonnemans et al., 1998). In other words, different frames yield different beliefs about how much others cooperate. Could it also be true that for some types of decision-makers frame influences beliefs, but for other types, frame does not? These are some of the questions that motivated the design of the full experiment from which this study is derived. These are the kinds of questions regarding the interplay of heterogeneity, frame, beliefs, and cooperation in public good dilemmas that are explored in depth in Jackman (2016a).

References

Allcott, H. (2011). Social norms and energy conservation. *Journal of Public Economics, 95*(9-10), 1082-1095.

Andreoni, J. (1990). Impure altruism and donations to public-goods - A theory of warm-glow giving. *Economic Journal, 100*(401), 464-477.

Andreoni, J. (1995). Warm-glow versus cold-prickle: The effects of positive and negative framing on cooperation in experiments. *The Quarterly Journal of Economics, 110*(1), 1-21.

Andreoni, J., & Miller, J. (2002). Giving according to GARP: An experimental test of the consistency of preferences for altruism. *Econometrica, 70*(2), 737-753.

Andruff, H., Carraro, N., Thompson, A., & Gaudreau, P. (2009). Latent class growth modelling: A tutorial. *Tutorials in Quantitative Methods for Psychology, 5*(1), 11-24.

Apesteguia, J., & Ballester, M. A. (2009). A theory of reference-dependent behavior. *Economic Theory, 40*(3), 427-455.

Apesteguia, J., & Maier-Rigaud, F. P. (2006). The role of rivalry: Public goods versus common-pool resources. *The Journal of Conflict Resolution, 50*(5), 646-663.

Bougherara, D., Denant-Boemont, L., & Masclet, D. (2011). Cooperation and framing effects in provision point mechanisms: Experimental evidence. *Ecological Economics, 70*, 1200-1210.

Brewer, M. B., & Kramer, R. M. (1986). Choice behavior in social dilemmas: Effects of social identity, group size, and decision framing. *Journal of Personality and Social Psychology, 50*(3), 543-549.

Brown, T. A. (2015). *Confirmatory factor analysis for applied research* (Second ed.). New York, New York USA: Guilfor Publications.

Burlando, R., & Guala, F. (2005). Heterogeneous agents in publics goods experiments. *Experimental Economics, 8*(1), 35-54.

Camerer, C. F., & Fehr, E. (2006). When does "economic man" dominate social behavior? *Science, 311*(5757), 47-52.

Camerer, C. F., Lowenstein, G., & Rabin, M. (Eds.). (2004). *Advances in behavioral economics*. Princeton, NJ: Princeton University Press.

Charness, G., & Rabin, M. (2002). Understanding social preferences with simple tests. *The Quarterly Journal of Economics, 117*(3), 817-869.

Chaudhuri, A. (2011). Sustaining cooperation in laboratory public goods experiments: A selective survey of the literature. *Experimental Economics, 14*, 47-83.

Cookson, R. (2000). Framing effects in public goods experiments. *Experimental Economics, 3*, 55-79.

Cubitt, R. P., Drouvelis, M., & Gachter, S. (2011). Framing and free riding: emotional responses and punishment in social dilemma games. *Experimental Economics, 14*(2), 254-272.

Della Vigna, S. (2009). Psychology and economics: Evidence from the field. *Journal of Economic Literature, 47*(2), 315-372.

Diamond, P., & Vartiainen, H. (Eds.). (2007). *Behavioral economics and its applications*. Princeton, NJ: Princeton University Press.

Dufwenberg, M., Gächter, S., & Hennig-Schmidt, H. (2011). The framing of games and the psychology of play. *Games and Economic Behavior, 73*(2), 459-478.

Ellingsen, T., Johannesson, M., Mollerstrom, J., & Munkhammar, S. (2012). Social framing effects: Preferences or beliefs? *Games and Economic Behavior, 76*, 117-130.

Elster, J. (1989). Social Norms and Economic Theory. *Journal of Economic Perspectives, 3*(4), 99-117.

Fehr, E., & Rockenbach, B. (2004). Human altruism: Economic, neural, and evolutionary perspectives. *Current Opinion in Neurobiology, 14*(6), 784-790.

Fehr, E., & Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. *Quarterly Journal of Economics, 114*(3), 817-868.

Fischbacher, U. (2007). z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics, 10*(2), 171-178.

Fischbacher, U., & Gächter, S. (2010). Social preferences, beliefs, and the dynamics of free riding in public goods experiments. *American Economic Review, 100*(1), 541-556.

Fischbacher, U., Gächter, S., & Fehr, E. (2001). Are people conditionally cooperative? Evidence from a public goods experiment. *Economics Letters, 71*(3), 397-404.

Fleishman, J. A. (1988). The effects of decision framing and others' behavior on cooperation in a social dilemma. *The Journal of Conflict Resolution, 32*(1), 162-180.

Gintis, H., Bowles, S., Boyd, R., & Fehr, E. (2003). Explaining altruistic behavior in humans. *Evolution and Human Behavior, 24*(3), 153-172.

Hasan, S. (2012). Group based trajectories of network formation and dynamics. *Social Networks, 34*(4), 506-514.

Henrich, N., & Henrich, J. (2007). Cooperation through reciprocity and reputation, *Why humans cooperate: A cultural and evolutionary explanation* (pp. 109-132). New York, New York USA: Oxford University Press, Inc.

Jackman, D. K. (2016a). *Beliefs pathway of framing effects in a repeated public good dilemma*. Ph.D. Dissertation Chapter. School of Natural Resources and Environment. University of Michigan. Ann Arbor, MI.

Jackman, D. K. (2016b). *Heterogeneous framing effects in a repeated public good dilemma*. Ph.D. Dissertation Chapter. School of Natural Resources and Environment. University of Michigan. Ann Arbor, MI.

Jacobsen, G. D., Kotchen, M. J., & Vandenbergh, M. P. (2012). The behavioral response to voluntary provision of an environmental public good: Evidence from residential electricity demand. *European Economic Review, 56*, 946-960.

Jones, B. L., & Nagin, D. S. (2007). Advances in group-based trajectory modeling and an SAS procedure for estimating them. *Sociological Methods and Research, 35*(4), 542-571.

Jones, B. L., Nagin, D. S., & Roeder, K. (2001). A SAS procedure based on mixture models for estimating developmental trajectories. *Sociological Methods and Research, 29*(3), 374-393.

Kahneman, D. (2000). Preface. In D. Kahneman & A. Tversky (Eds.), *Choices, values, and frames*. Cambridge: Cambridge University Press.

Kahneman, D., & Tversky, A. (1979). Prospect theory: Analysis of decision under risk. *Econometrica, 47*(2), 263-291.

Kim, D. H., & Shin, D. M. (2014). Identifying and explaining different poverty trajectories ssing a group-based trajectory analysis - A case study of south korea. *Social Policy & Administration, 48*(7), 826-847.

Krupka, E. L., & Weber, R. A. (2013). Identifying social norms using coordination games: Why does dictator game sharing vary? *Journal of the European Economic Association, 11*(3), 495-524.

Kurzban, R., & Houser, D. (2005). Experiments investigating cooperative types in humans: A complement to evolutionary theory and simulations. *Proceedings of the National Academy of Sciences of the United States of America, 102*(5), 1803-1807.

Laibson, D., & Zeckhauser, R. (1998). Amos Tversky and the ascent of behavioral economics. *Journal of Risk and Uncertainty, 16*, 7-47.

Ledyard, J. (1995). Public goods: A survey of experimental research. In A. E. Roth & J. H. Kagel (Eds.), *The handbook of experimental economics* (pp. 111-194). Princeton, NJ: Princeton University Press.

Levin, I. P., Gaeth, G. J., Schreiber, J., & Lauriola, M. (2002). A new look at framing effects: Distribution of effect sizes, individual differences, and independence of types of effects. *Organizational Behavior and Human Decision Processes, 88*(1), 411-429.

Levin, I. P., Schneider, S. L., & Gaeth, G. J. (1998). All frames are not created equal: A Typology and critical analysis of framing effects. *Organizational Behavior and Human Decision Processes, 76*(2), 149-188.

Liebrand, W. B. G. (1984). The effect of social motives, communication and group-size on behavior in an n-person multi-stage mixed-motive game. *European Journal of Social Psychology, 14*(3), 239-264.

Londono, D., Chen, K. M., Musolf, A., Wang, R. X., Shen, T., Brandon, J., Herring, J.A., Wise, C.A., Zou, H., Jin, M., Yu, L., Finch, S.J., Matise, T.C., Gordon, D. (2013). A novel method for analyzing genetic association with longitudinal phenotypes. *Statistical Applications in Genetics and Molecular Biology, 12*(2), 241-261.

Low, B. S., Parker, N., Hazel, A., & Welch, K. B. (2013). Life expectancy, fertility, and women's lives: A life-history perspective. *Cross-Cultural Research, 47*(2), 198-225.

Nagin, D. S. (1999). Analyzing Developmental Trajectories: A semiparametric, group-based approach. *Pyschological Methods, 4*(2), 139-157.

Nagin, D. S. (2005). *Group-based modeling of development*. Cambridge, MA USA: Harvard University Press.

Nagin, D. S., & Odgers, C. L. (2010). Group-based trajectory modeling (nearly) two decades later. *Journal of Quantitative Criminology, 26*(4), 445-453.

Nagin, D. S., & Tremblay, R. E. (2001). Analyzing develomental trajectories of distinct but related behaviors: A group-based method. *Psychological Methods, 6*(1), 18-34.

Offerman, T., Sonnemans, J., & Schram, A. (1996). Value orientations, expectations and voluntary contribution in public goods. *Economic Journal, 106*, 817-845.

Park, E.-S. (2000). Warm-glow versus cold prickle: A further experimental study of framing effects on free-riding. *Journal of Economic Behavior & Organization, 43*, 405-421.

Parks, C. (1994). The predictative ability of social values in resource dilemmas and public goods games. *Personality and Social Psychology Bulletin, 20*(4), 431-438.

Preacher, K. J., & MacCallum, R. C. (2003). Repairing Tom Swift's electric factor analysis machine. *Understanding Statistics, 2*(1), 13-43.

Rabin, M. (2002). A perspective on psychology and economics. *European Economic Review, 46*, 657-685.

Rose-Ackerman, S. (1982). Charitable giving and excessive fund-raising. *Quarterly Journal of Economics, 97*(2), 193-212.

Rutte, C. G., Wilke, H. A. M., & Messick, D. M. (1987). The effects of framing social dilemmas as give-some or take-some games. *British Journal of Social Psychology, 26*, 103-108.

Sonnemans, J., Schram, A., & Offerman, T. (1998). Public good provision and public bad prevention: The effect of framing. *Journal of Economic Behavior & Organization, 34*, 143-161.

Tversky, A., & Kahneman, D. (1981). The framing of decisions and the psychology of choice. *Science, 211*(4481), 453-458.

Tversky, A., & Kahneman, D. (1986). Rational choice and the framing of decisions. *The Journal of Business, 59*(4), S251-S278.

Tversky, A., & Kahneman, D. (1991). Loss aversion in riskless choice - A reference dependent model. *Quarterly Journal of Economics, 106*(4), 1039-1061.

Virtanen, P., Lipiainen, L., Hammarstrom, A., Janlert, U., Saloniemi, A., & Nummi, T. (2011). Tracks of labour market attachment in early middle age: A trajectory analysis over 12 years. *Advances in Life Course Research, 16*(2), 55-64.

Willinger, M., & Ziegelmeyer, A. (1999). Framing and cooperation in public good games: An experiment with an interior solution. *Economic Letters, 65*, 323-328.

Wooldridge, J. M. (2010). *Econometric Analysis of Cross Section and Panel Data* (Second ed.). Cambridge, Massachusetts: The MIT Press

Tables

Table 1.1: Sample characteristics

			Frames				ANOVA			
			GN	GNN	TN	TNN	df	F	p	
Age (in years)							3	1.04	0.3745	
Number	252		61	64	63	64				
Mean	20.54		20.15	20.23	21.44	21.31				
Standard Deviation	4.16		1.68	3.47	2.99	6.68				
			Frames				Chi Square Test			
	N	%	GN	GNN	TN	TNN	df	X^2	p	
Gender							3	6.09	0.1072	
Men	117	46%	25	37	30	25				
Women	139	54%	39	27	34	39				
Training										
No	94	37%	26	22	22	24	3	0.68	0.8784	
Yes	157	63%	37	40	41	39				
Don't know (excluded)	5	--	1	2	1	1				
Experience							3	13.35	0.0039	***
No	94	38%	34	15	21	24				
Yes	151	62%	26	44	42	39				
Don't know (excluded)	11	--	4	5	1	1				

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: Age was not reported for 3 individuals, and one age was misreported (age = 92) and excluded. Most participants were in their late teens (18 or 19) and early twenties. Eight were over 25.

Table 1.2: Comparing the Bayesian Information Criterion for group-based trajectory models with linear trajectories

Number of Groups		Linear Trajectories	
Model	Null	ΔBIC (N=256)	ΔBIC (N=5376)
3	2	528.18	519.86
4	3	534.89	526.57
5	4	74.80	66.48
6	5	425.63	417.31
7	6	81.52	73.20
8	7	50.58	42.26
9	8	65.10	56.78
10	9	322.38	314.06

Note 1: The "model" column gives the number of groups (i.e., cooperative types) in the model of interest. The "null" column gives the number of groups in the model to which the model of interest is compared (i.e., the simpler model).
Note 2: BIC is reported for $N = 256$ and $N = 5376$, the first of which is the number of participants and the second is the number of total measurements (i.e., 21 measurements per person). In theory, N represents the number of independent measurements. The number of participants underestimates that theoretical value. Because intra-individual measurements are not totally independent, the number of measurements over estimates that theoretical value. The true value falls somewhere in between (Nagin, 2005, p. 68).
Note 3: In general, the pattern of changes in the BIC, group size, and group significance for linear, quadratic, and cubic models were very similar.

Table 1.3: Summary statistics for unconditional and conditional decisions

	Mean	Standard Deviation	Median	Interquartile Range	
Unconditional cooperation	8.16	6.8492	8.0	1.5	12.0
Mean conditional cooperation	6.79	4.4513	7.6	2.9	10.0
Conditional cooperation					
Others' cooperation = 0	2.00	5.0289	0.0	0.0	0.0
Others' cooperation = 1	2.21	4.7954	0.0	0.0	1.0
Others' cooperation = 2	2.52	4.5512	1.0	0.0	2.0
Others' cooperation = 3	3.01	4.5249	1.5	0.0	3.0
Others' cooperation = 4	3.29	4.2946	2.0	0.0	4.0
Others' cooperation = 5	4.27	4.6047	4.0	0.0	5.0
Others' cooperation = 6	4.57	4.4072	5.0	0.0	6.0
Others' cooperation = 7	5.13	4.6251	5.0	0.0	7.0
Others' cooperation = 8	5.78	4.7600	7.0	0.0	8.0
Others' cooperation = 9	6.08	4.8968	8.0	0.0	9.0
Others' cooperation = 10	7.04	5.3058	9.0	0.0	10.0
Others' cooperation = 11	7.73	5.6950	9.5	1.5	11.0
Others' cooperation = 12	8.29	5.8096	10.0	2.5	12.0
Others' cooperation = 13	8.68	6.0674	10.0	3.0	13.0
Others' cooperation = 14	8.98	6.3884	10.0	2.0	14.0
Others' cooperation = 15	9.52	6.8215	10.0	2.0	15.0
Others' cooperation = 16	9.89	7.3032	11.5	2.0	16.0
Others' cooperation = 17	10.24	7.6876	12.0	1.0	17.0
Others' cooperation = 18	10.72	7.9900	13.5	1.0	18.0
Others' cooperation = 19	11.00	8.3321	14.5	1.0	19.0
Others' cooperation = 20	11.54	8.7885	15.0	0.0	20.0

Note 1: Mean conditional cooperation is the within-person mean of conditional cooperation according to each individual's conditional decision table.
Note 2: Others' cooperation refers to the possible average level of cooperation by others.
Note 3: Interquartile range consists of the middle quartiles.

Table 1.4: GBTM trajectory estimates for the four-group model

	Dependent variable: Conditional cooperation							
	Group 1 Non-Cooperators		Group 2 Conditional Cooperators		Group 3 Low Cooperators		Group 4 High Cooperators	
Intercept	-9.986	(0.732) ***	-3.309	(0.204) ***	5.192	(0.289) ***	11.6774	(0.607) ***
Others' cooperation	0.214	(0.056) ***	1.180	(0.017) ***	-0.082	(0.026) ***	0.4566	(0.051) ***
Group size								
Probability of group membership	18.7%	(2.444) ***	51.1%	(3.154) ***	23.4%	(2.674) ***	6.7%	(1.585) ***
Proportion assigned to group	18.8%	NA	51.2%	NA	23.4%	NA	6.6%	NA

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Note 1: The model for each trajectory was a censored normal linear regression model where an uncensored, latent variable for conditional cooperation was regressed on the possible average levels of cooperation by others. Thus, coefficients correspond to the latent variable.
Note 2: Conditional cooperation was measured at all possible average levels of cooperation ranging from 0 to 20 points.
Note 3: Standard errors are in parentheses.
Note 4: Group size is measured in two ways, the probability of group membership and the proportion assigned to the group via posterior probabilities.
Note 5: Goodness of fit for this model was assessed with the Bayesian Information Criteria. However, the nearly identical values for probabilities of group membership and proportion assigned to the group also demonstrate the goodness of fit of the four-group model.
Note 6: Group corresponds to cooperative type.

Table 1.5: Questionnaire summary statistics

Questionnaire Item with Organizational Headers	Summary Statistics (N =256)				
	Mean	SD	Median	Interquartile Range	
Reference Dependence					
Q01 I felt ownership of all of the possible project points	2.50	1.20	2.0	2.0	3.0
Q02 I felt ownership of only my 20 points	4.20	1.06	5.0	4.0	5.0
Own and Others' Outcomes					
Q03 I wanted to earn as many points as I could	*4.77*	*0.54*	*5.0*	*5.0*	*5.0*
Q04 I wanted others to earn as many points as they could	2.91	1.31	3.0	2.0	4.0
Q05 I wanted to earn more points than others earned	*3.46*	*1.48*	*4.0*	*2.0*	*5.0*
Q06 I felt responsible only for myself, not for others, and not for the project	*3.35*	*1.36*	*4.0*	*2.0*	*5.0*
Own and Other's Actions					
Q07 Allocating points to my personal account made me feel good	*3.54*	*1.27*	*4.0*	*3.0*	*5.0*
Q08 Allocating points to the project account made me feel good	*3.22*	*1.14*	*3.0*	*2.5*	*4.0*
Q09 I felt good when others allocated points to the project account	*4.26*	*0.95*	*5.0*	*4.0*	*5.0*
Q10 I felt bad when others allocated points to their personal accounts	*3.21*	*1.33*	*3.0*	*2.0*	*4.0*
Own and Others' Obligatory Actions					
Q11 I felt obligated to allocate points to the project account	*2.74*	*1.35*	*3.0*	*2.0*	*4.0*
Q12 I felt like I had to allocate as many points to the project account as others did	*2.76*	*1.35*	*3.0*	*2.0*	*4.0*
Q13 I felt that others were obligated to allocate points to the project account	*2.97*	*1.42*	*3.0*	*2.0*	*4.0*
Q14 I felt that others were obligated to allocate as many points to the project account as I did	*3.44*	*1.38*	*4.0*	*2.0*	*5.0*
Q15 I felt cheated when others allocated points to their personal accounts	*2.94*	*1.39*	*3.0*	*2.0*	*4.0*
Q16 I felt cheated when others allocated more points to their personal accounts than I did	*3.24*	*1.41*	*4.0*	*2.0*	*4.0*
Concern about Prospective Losses					
Q17 I was worried that others would allocate nothing to the project account	*3.73*	*1.36*	*4.0*	*3.0*	*5.0*
Q18 I had to allocate points to my personal account because I expected the project account to be small	4.11	1.18	5.0	4.0	5.0

Table 1.5 continued: Questionnaire summary statistics

Questionnaire Item with Organizational Headers	Summary Statistics (N =256)				
	Mean	SD	Median	Interquartile Range	
Confusion and Understanding					
Q19 I fully understood the decision situation throughout the study	4.05	0.98	4.0	4.0	5.0
Q20 I understood the decision situation better over time	4.10	1.08	4.0	4.0	5.0
Q21 I was confused throughout the study	1.68	0.98	1.0	1.0	2.0
Satisfaction					
Q22 I was very satisfied with the results of the decision situations	3.24	1.11	3.0	3.0	4.0
Q23 I believe that others were very satisfied with the results of the decision situations	3.18	0.89	3.0	3.0	4.0

Note 1: Questionnaire responses scaled from 1 for Strongly Disagree (SD) to 5 for Strongly Agree (SA).
Note 2: Organizational headers not shown to study participants.
Note 3: Items in italics were included in the exploratory factor analysis (EFA).

Table 1.6: Questionnaire means by cooperative type with Kruskal Wallis statistics

Questionnaire Item with Organizational Headers		Means NC	CC	LC	HC	Krukal Wallis (N=256, df = 3) X^2	p	
	N =	48	131	60	17			
Reference Dependence								
Q01 I felt ownership of all of the possible project points		2.33 [a]	2.43 [a]	2.67 [a]	2.88 [a]	5.86	0.1188	
Q02 I felt ownership of only my 20 points		4.46 [a]	4.11 [a,b]	4.32 [a,b]	3.76 [b]	9.45	0.0239	**
Own and Others' Outcomes								
Q03 I wanted to earn as many points as I could		*4.92* [a]	*4.73* [a,b]	*4.87* [a]	*4.35* [b]	*16.64*	*0.0008*	***
Q04 I wanted others to earn as many points as they could		2.17 [a]	3.24 [b]	2.55 [a]	3.76 [b]	37.10	<.0001	***
Q05 I wanted to earn more points than others earned		*4.00* [a]	*3.23* [b,c]	*3.75* [a,b]	*2.71* [c]	*16.38*	*0.0010*	***
Q06 I felt responsible only for myself, not for others, and not for the project		*4.25* [a]	*3.01* [b]	*3.55* [b]	*2.76* [b]	*34.72*	*<.0001*	***
Own and Other's Actions								
Q07 Allocating points to my personal account made me feel good		*3.94* [a]	*3.25* [b]	*3.97* [a]	*3.18* [a,b]	*20.83*	*0.0001*	***
Q08 Allocating points to the project account made me feel good		*2.35* [a]	*3.46* [b,c]	*3.18* [b]	*4.00* [c]	*38.84*	*<.0001*	***
Q09 I felt good when others allocated points to the project account		*4.21* [a]	*4.28* [a]	*4.20* [a]	*4.47* [a]	*3.43*	*0.3300*	
Q10 I felt bad when others allocated points to their personal accounts		*2.92* [a]	*3.37* [a]	*3.08* [a]	*3.29* [a]	*4.59*	*0.2046*	
Own and Others' Obligations								
Q11 I felt obligated to allocate points to the project account		*1.81* [a]	*3.05* [b]	*2.67* [b]	*3.29* [b]	*34.05*	*<.0001*	***
Q12 I felt like I had to allocate as many points to the project account as others did		*1.85* [a]	*3.07* [b]	*2.63* [b]	*3.35* [b]	*33.21*	*<.0001*	***
Q13 I felt that others were obligated to allocate points to the project account		*2.44* [a]	*3.13* [b]	*2.93* [a,b]	*3.41* [a,b]	*10.03*	*0.0183*	**
Q14 I felt that others were obligated to allocate as many points to the project account as I did		*2.54* [a]	*3.83* [b]	*3.17* [a,c]	*3.94* [c,b]	*33.43*	*<.0001*	***
Q15 I felt cheated when others allocated points to their personal accounts		*2.17* [a]	*3.15* [b]	*3.03* [b]	*3.18* [a,b]	*19.18*	*0.0003*	***
Q16 I felt cheated when others allocated more points to their personal accounts than I did		*2.40* [a]	*3.50* [b]	*3.37* [b]	*3.24* [a,b]	*19.65*	*0.0002*	***
Concern about Prospective Losses								
Q17 I was worried that others would allocate nothing to the project account		*3.40* [a]	*3.91* [a]	*3.68* [a]	*3.53* [a]	*5.46*	*0.1412*	
Q18 I had to allocate points to my personal account because I expected the project account to be small		4.38 [a]	4.00 [a]	4.15 [a]	4.12 [a]	4.66	0.1983	

Table 1.6 continued: Questionnaire means by cooperative type with Kruskal Wallis statistics

		Means				Krukal Wallis		
		NC	CC	LC	HC	(N=256, df = 3)		
Questionnaire Item with Organizational Headers	N =	48	131	60	17	X^2	p	
Confusion and Understanding								
Q19 I fully understood the decision situation throughout the study		4.40 [a]	4.07 [a,b]	3.93 [b]	3.29 [b]	14.18	0.0027	***
Q20 I understood the decision situation better over time		3.83 [a]	4.09 [a]	4.37 [a]	3.94 [a]	3.98	0.2638	
Q21 I was confused throughout the study		1.58 [a]	1.62 [a]	1.87 [a]	1.76 [a]	4.02	0.2593	
Satisfaction								
Q22 I was very satisfied with the results of the decision situations		3.65 [a]	3.09 [a]	3.27 [a]	3.12 [a]	8.02	0.0456	**
Q23 I believe that others were very satisfied with the results of the decision situations		3.35 [a]	3.15 [a]	3.07 [a]	3.29 [a]	3.19	0.3636	

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Note 1: Means based on a Likert scale from 1 for Strongly Disagree (SD) to 5 for Strongly Agree (SA).
Note 2: NC=non-cooperator, CC=conditional cooperators, LC=low cooperator, and HC=high cooperator.
Note 3: Organizational headers not shown to participants.
Note 4: Items in italics were included in the exploratory factor analysis (EFA).
Note 5: For items Q02, Q13, Q19, and Q22, Kruskal Wallis tests among all four types were not significant when the Bonferonni correction for multiple comparison was applied. The Bonferroni correction is a modification of the threshold for statistical significance, α, based on the number of measurements, n. In this case statistical significance at the 0.05 level requires a p-value less than $\alpha/n = 0.05/23 = 0.0022$.
Note 6: The results of pairwise nonparametric tests between cooperative types are summarized by the superscripts appearing to the right of each mean. Within a row, when two types share superscripts, we cannot reject the hypothesis that medians are equal. Within a row, when two types do not share a superscript, the difference between medians is statistically significant at the 0.05 level. These pairwise tests employed the Dwass, Steel, Critchlow-Fligner (DSCF) multiple comparison analysis, which is based on pairwise two-sample Wilcoxon comparisons.

Table 1.7: Questionnaire factors and factor loadings

Questionnaire Item		Mean Response	Factor Loadings: Own-Other Obligations	Factor Loadings: Others' Actions	Factor Loadings: Self Interest
Factor 1: Feelings about own and others' obligations					
Q11	I felt obligated to allocate points to the project account	2.74	0.7902	0.2825	-0.3377
Q12	I felt like I had to allocate as many points to the project account as others did	2.76	0.7543	0.2765	-0.2944
Q13	I felt that others were obligated to allocate points to the project account	2.97	0.6299	0.3476	-0.1679
Q14	I felt that others were obligated to allocate as many points to the project account as I did	3.44	0.6901	0.5867	-0.1831
Q08	Allocating points to the project account made me feel good	3.22	0.5737	0.3716	-0.3781
Factor 2: Perceptions of others' actions					
Q16	I felt cheated when others allocated more points to their personal accounts than I did	3.24	0.4773	0.8643	-0.0758
Q15	I felt cheated when others allocated points to their personal accounts	2.94	0.4763	0.8041	-0.1358
Q17	I was worried that others would allocate nothing to the project account	3.73	0.2954	0.6216	-0.0871
Q10	I felt bad when others allocated points to their personal accounts	3.21	0.2775	0.5766	-0.1721
Q09	I felt good when others allocated points to the project account	4.26	0.0634	0.3615	0.0423
Factor 3: Self-interest					
Q05	I wanted to earn more points than others earned	3.46	-0.2201	0.0385	0.7027
Q07	Allocating points to my personal account made me feel good	3.54	-0.2666	-0.1516	0.6703
Q06	I felt responsible only for myself, not for others, and not for the project	3.35	-0.5502	-0.1888	0.6177
Q03	I wanted to earn as many points as I could	4.77	-0.1319	-0.1014	0.3248

Note 1: Mean response was based on a Likert scale from 1 for Strongly Disagree to 5 for Strongly Agree.
Note 2: Items regarding confusion (Q19 – Q21), satisfaction (Q22 and Q23), and ownership (Q01 and Q02) were removed.
Note 3: Item Q04 loaded on two factors and was removed.
Note 4: Item Q18 was removed because absolute factor loadings were small (<0.3).

Table 1.8: Pair-wise comparisons of cooperative types

Baseline Cooperative Type		Comparison Cooperative Type: Conditional Cooperators (N=131) t p	Comparison Cooperative Type: Low Cooperators (N=60) t p	Comparison Cooperative Type: High Cooperators (N=17) t p
Non-Cooperators				
	Factor 1: Own-other obligations	-More motivated by obligations [a,1]	-More motivated by obligations [a,1]	-More motivated by obligations [a,1]
	Factor 2: Others' actions	-More concerned with others' actions [a,1]	-More concerned with others' actions [b,2]	-More concerned with others' actions [3]
	Factor 3: Self-interest	-Less self-interested [a,1]		-Less self-interested [a,1]
Conditional Cooperators				
	Factor 1: Own-other obligations		-Less motivated by obligations [b,1]	
	Factor 2: Others' actions			
	Factor 3: Self-interest		-More self-interested [a,1]	
Low Cooperators				
	Factor 1: Own-other obligations			-More motivated by obligations [b,3]
	Factor 2: Others' actions			
	Factor 3: Self-interest			-Less self-interested [a,1]

Note 1: Comments describe statistically significant differences in factors score of the comparison type (column) relative to the baseline type (row) according to paired t-tests and Wilcoxon-Mann-Whitney factor scores.
Note 2: Superscript a, b, and c refer to statistically significant t-tests at the 0.01, 0.05, and 0.1 levels with the Bonferroni correction for $n = 6$ comparisons.
Note 3: Superscript 1, 2, and 3 refer to statistically significant Wilcoxon-Mann-Whitney test with the Dwass, Steel, Critchlow-Fligner (DSCF) correction for multiple comparisons.

Table 1.9: Cooperation means by frame and Kruskal Wallis tests for framing effects

	Means by Frame (N=64 each)				Framing Statistics Kruskal Wallis		
Measurement	GN	GNN	TN	TNN	X^2	df	p
Unconditional cooperation	8.80	8.06	7.77	8.02	1.10	3	0.7779
Mean conditional cooperation	7.29	6.22	6.71	6.92	2.08	3	0.5555
Conditional cooperation							
Others' cooperation = 0	1.33	0.52	2.69	3.48	7.61	3	0.0549
Others' cooperation = 1	1.89	0.94	2.39	3.61	2.27	3	0.5178
Others' cooperation = 2	2.22	1.45	2.56	3.83	2.32	3	0.5094
Others' cooperation = 3	2.81	1.94	3.05	4.25	3.84	3	0.2796
Others' cooperation = 4	3.14	2.48	3.20	4.34	1.87	3	0.5999
Others' cooperation = 5	4.39	3.09	4.16	5.45	4.11	3	0.2502
Others' cooperation = 6	4.53	3.67	4.55	5.52	3.44	3	0.3283
Others' cooperation = 7	5.22	4.20	5.08	6.02	3.10	3	0.3763
Others' cooperation = 8	5.95	4.86	5.77	6.55	3.56	3	0.3131
Others' cooperation = 9	5.95	5.39	6.00	6.97	2.88	3	0.4105
Others' cooperation = 10	7.52	7.02	6.89	6.72	1.31	3	0.7275
Others' cooperation = 11	8.39	7.36	7.67	7.50	2.22	3	0.5275
Others' cooperation = 12	8.84	7.73	8.31	8.27	1.81	3	0.6128
Others' cooperation = 13	9.45	8.23	8.47	8.58	1.87	3	0.5992
Others' cooperation = 14	9.97	8.66	8.64	8.66	2.01	3	0.5711
Others' cooperation = 15	10.89	9.13	9.41	8.67	3.51	3	0.3201
Others' cooperation = 16	11.14	9.73	9.75	8.94	2.87	3	0.4129
Others' cooperation = 17	11.50	10.27	10.13	9.06	2.76	3	0.4294
Others' cooperation = 18	12.33	10.73	10.48	9.34	3.47	3	0.3249
Others' cooperation = 19	12.69	11.42	10.63	9.28	5.03	3	0.1700
Others' cooperation = 20	12.98	11.69	11.19	10.30	3.75	3	0.2894

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: Mean conditional cooperation is the within-person mean of conditional cooperation according to each individual's conditional decision table.
Note 3: Others' cooperation refers to all possible average level of cooperation by others.
Note 4: The results of pairwise nonparametric tests between frames using the Dwass, Steel, Critchlow-Fligner (DSCF) multiple comparison analysis yielded no statistically significant differences at the 0.05 level.

Table 1.10: Questionnaire means by frame and Kruskal Wallis tests for framing effects

Questionnaire Item with Organizational Headers	Means by Frame (N=64)				Kruskal Wallis (N=256, df = 3)		
	GN	GNN	TN	TNN	X^2	p	
Reference Dependence							
Q01 I felt ownership of all of the possible project points	2.41	2.36	2.66	2.56	2.57	0.4620	
Q02 I felt ownership of only my 20 points	4.22	4.33	4.17	4.09	1.45	0.6941	
Own and Others' Outcomes							
Q03 I wanted to earn as many points as I could	*4.81*	*4.77*	*4.75*	*4.75*	*1.86*	*0.6009*	
Q04 I wanted others to earn as many points as they could	3.14	2.69	2.86	2.97	4.35	0.2258	
Q05 I wanted to earn more points than others earned	*3.50*	*3.44*	*3.39*	*3.52*	*0.23*	*0.9720*	
Q06 I felt responsible only for myself, not for others, and not for the project	*3.22*	*3.55*	*3.23*	*3.41*	*2.51*	*0.4730*	
Own and Other's Actions							
Q07 Allocating points to my personal account made me feel good	*3.66*	*3.42*	*3.56*	*3.53*	*0.64*	*0.8883*	
Q08 Allocating points to the project account made me feel good	*3.19*	*3.09*	*3.22*	*3.39*	*1.35*	*0.7170*	
Q09 I felt good when others allocated points to the project account	*4.47*	*4.34*	*4.17*	*4.06*	*9.65*	*0.0218*	**
Q10 I felt bad when others allocated points to their personal accounts	*3.38*	*3.27*	*2.84*	*3.36*	*6.65*	*0.0839*	*
Own and Others' Obligatory Actions							
Q11 I felt obligated to allocate points to the project account	*2.97*	*2.73*	*2.45*	*2.81*	*4.53*	*0.2100*	
Q12 I felt like I had to allocate as many points to the project account as others did	*2.75*	*2.81*	*2.56*	*2.91*	*2.24*	*0.5249*	
Q13 I felt that others were obligated to allocate points to the project account	*3.14*	*3.11*	*2.66*	*2.98*	*4.68*	*0.1972*	
Q14 I felt that others were obligated to allocate as many points to the project account as I did	*3.53*	*3.48*	*3.28*	*3.47*	*1.33*	*0.7217*	
Q15 I felt cheated when others allocated points to their personal accounts	*3.23*	*2.86*	*2.80*	*2.86*	*3.94*	*0.2681*	
Q16 I felt cheated when others allocated more points to their personal accounts than I did	*3.48*	*3.14*	*2.98*	*3.36*	*4.27*	*0.2336*	
Concern about Prospective Losses							
Q17 I was worried that others would allocate nothing to the project account	*3.86*	*3.83*	*3.58*	*3.67*	*1.85*	*0.6050*	
Q18 I had to allocate points to my personal account because I expected the project account to be small	3.92	4.36	4.19	3.98	8.47	0.0372	*

Table 1.10 continued: Questionnaire means by frame and Kruskal Wallis tests for framing effects

Questionnaire Item with Organizational Headers	Means by Frame (N=64 each) GN	GNN	TN	TNN	Kruskal Wallis (N=256, df=3) X^2	p	
Confusion and Understanding							
Q19 I fully understood the decision situation throughout the study	4.03	4.23	4.08	3.84	4.90	0.1794	
Q20 I understood the decision situation better over time	3.92	4.28	4.08	4.11	3.08	0.3789	
Q21 I was confused throughout the study	1.64	1.36	1.73	1.98	9.63	0.0220	**
Satisfaction							
Q22 I was very satisfied with the results of the decision situations	3.25	3.17	3.33	3.20	0.89	0.8285	
Q23 I believe that others were very satisfied with the results of the decision situations	3.13	3.13	3.28	3.19	1.55	0.6708	

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Note 1: Questionnaire responses scaled from 1 for Strongly Disagree (SD) to 5 for Strongly Agree (SA).
Note 2: GN=give neutral, GNN=give non-neutral, TN=take neutral, and TNN=take non-neutral
Note 3: Organizational headers not shown to participants.
Note 4: Items in italics were included in the exploratory factor analysis.
Note 5: None of the questionnaire responses differed significantly across frame when the Bonferroni correction ($\alpha/n = 0.05/23 = 0.0022$) was applied.
Note 6: The results of pairwise nonparametric tests between frames using the Dwass, Steel, Critchlow-Fligner (DSCF) multiple comparison analysis yielded only one statistically significant difference at the 0.05 level. Namely, questionnaire response to Q09 differed statistically between the take non-neutral and give neutral frames.

Figures

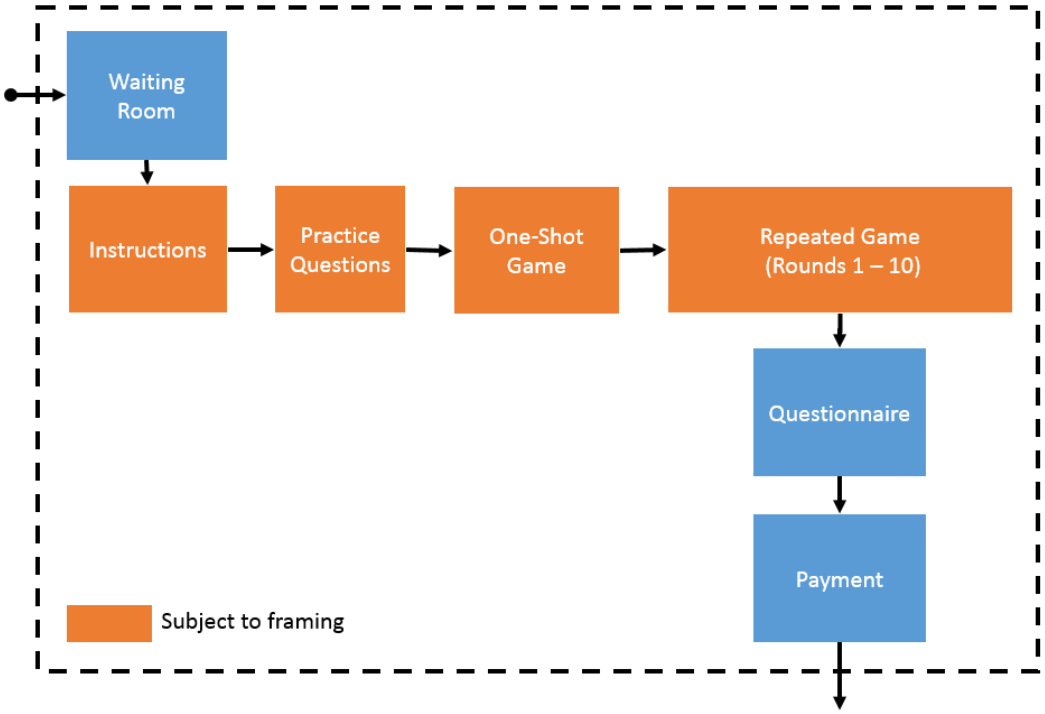
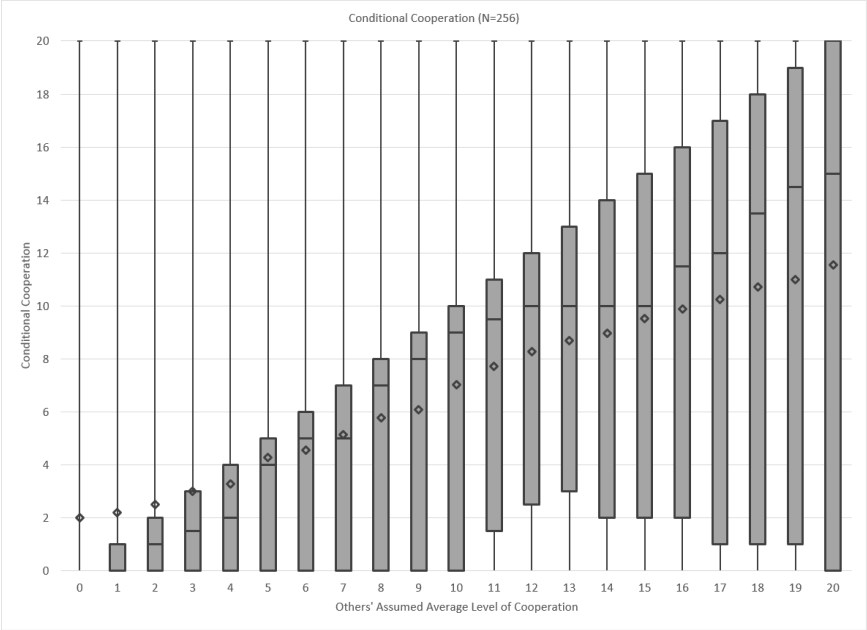


Figure 1.1: The experiment

		Language: Neutral	Language: Non-Neutral
Action	Give	Give-Neutral (GN)	Give Non-Neutral (GNN)
	Take	Take Neutral (TN)	Take Non-Neutral (TNN)

Figure 1.2: 2×2 Factorial framing design
Note 1: Action and language are the two experimental treatments used to create four different frames.
Note 2: Each of the frames was assigned to a different set of 4 sessions.



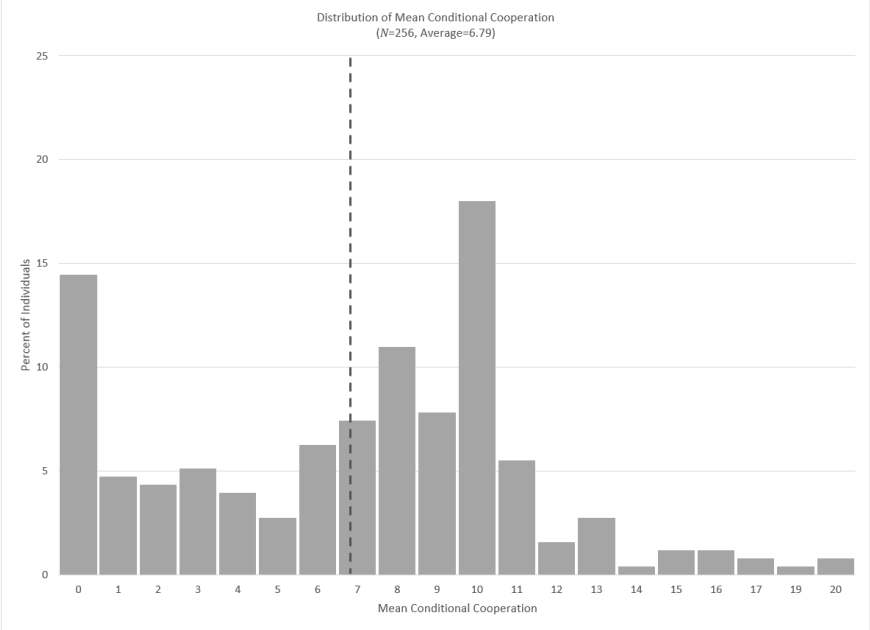


Figure 1.3: Distributions of conditional cooperation and mean conditional cooperation

Note 1: Each box and whisker plot represents the distribution of conditional cooperation for all 256 at each possible average level of cooperation by others. Boxes show the interquartile range (25^{th} to 75^{th} percentile). Whiskers extend to the maximum and minimum. The median is represented with a horizontal line, and the mean is represented with a diamond.

Note 2: Mean conditional cooperation is the within-person mean of conditional cooperation according to each individual's conditional decision table.

Note 3: Average mean conditional cooperation was 6.79 points.

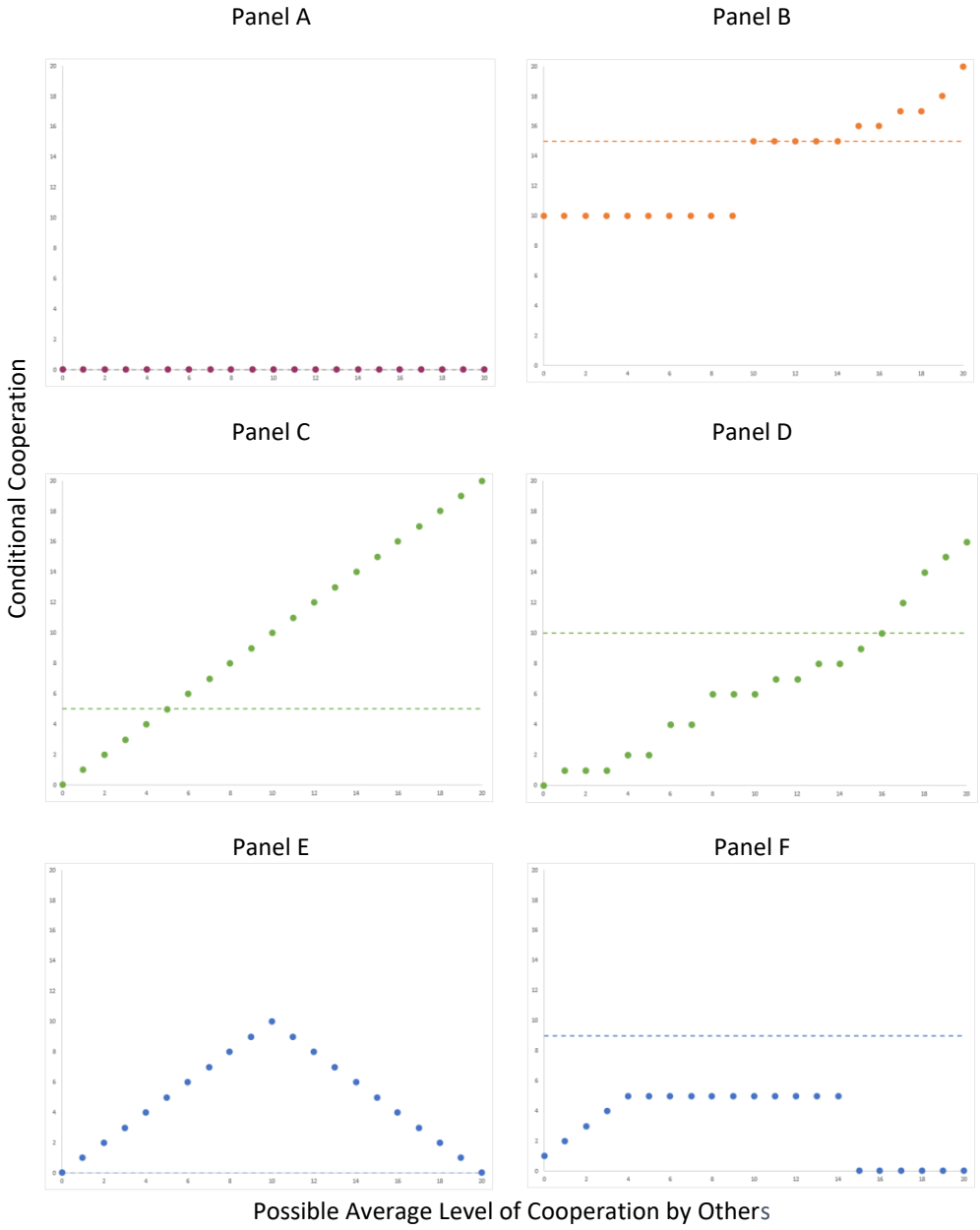


Figure 1.4: Unconditional and conditional cooperation for selected individuals

Note 1: The dashed line represents unconditional cooperation. Dots represent conditional cooperation.

Note 2: Individuals were selected to represent the four cooperative types that emerged via group-based trajectory modeling (GBTM).

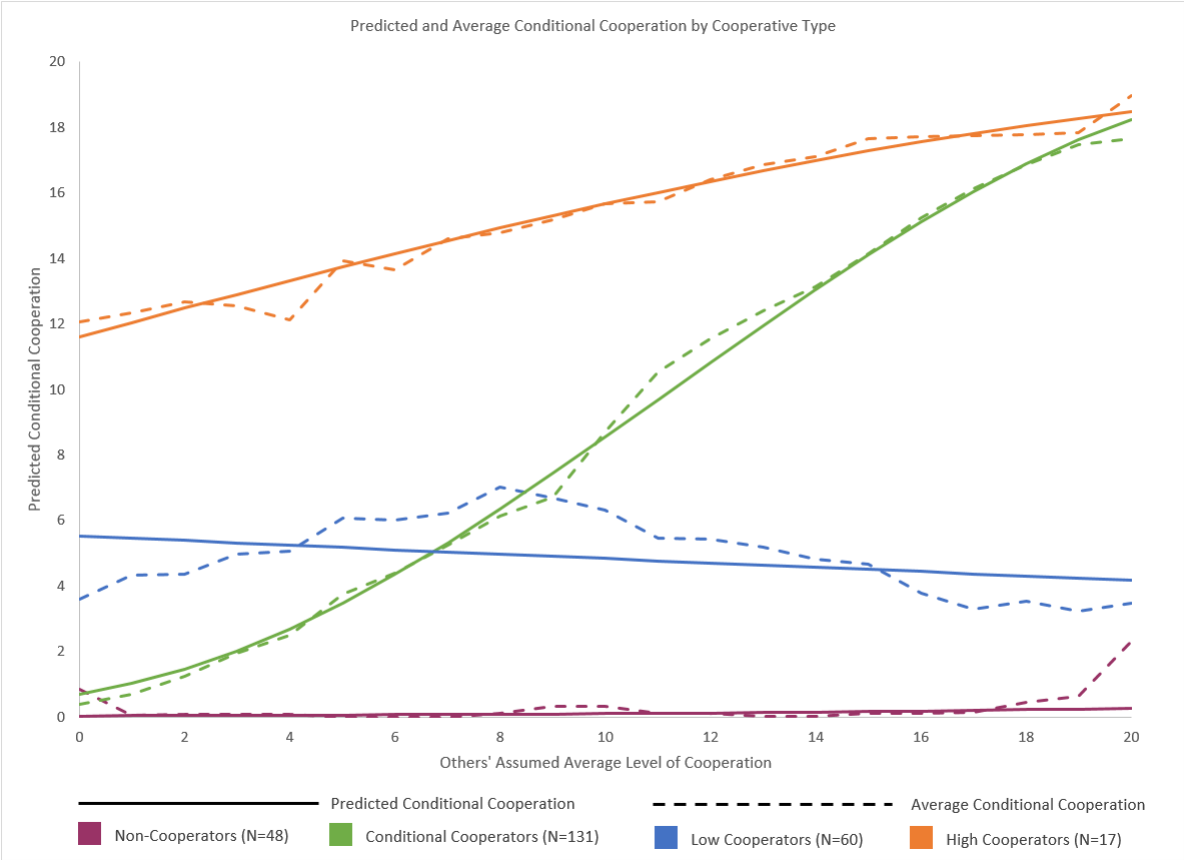


Figure 1.5: Predicted and average conditional cooperation by cooperative type

Note 1: Predicted values are based on the four-group linear trajectory model.

Note 2: Individuals were assigned to cooperative type according to the highest posterior probability then average conditional cooperation was computed.

Chapter 2

Heterogeneous Framing Effects in a Repeated Public Good Dilemma

Abstract: This paper focuses on how give and take frames are constructed in a linear, repeated public good experiment, how those formulations influence behavior, and how framing effects differ among different types of decision makers. Previous framing studies create frame via a wide variety of techniques, often "stacking the deck" in favor of a framing effect. As a result, it is not often clear what element(s) of the framing design is (are) responsible for the framing effect. The inherent heterogeneity among decision-makers adds another layer of complexity to understanding framing. With few exceptions, framing and heterogeneity among decision makers in public good dilemmas have been examined separately. Where they have been jointly explored, results have been ambiguous. In this research, I establish frames via a 2×2 factorial design that decomposes frame along two dimensions, the action implicit in the decision (i.e., give or take) and the language used to describe the decision (i.e., neutral or non-neutral). Each of four frames was imposed on a different set of study participants, whose decision-making types were previously determined via a separate dilemma. Among the four types of decision-makers, individuals who were expected not to cooperate cooperated trivially or not at all and demonstrated no sensitivity to framing. Individuals previously assessed as low, conditional, and high cooperators cooperated at distinct and non-trivial levels that declined over the course of the repeated dilemma. Where framing effects were observed, the two dimensions of frame did not uniformly influence every type of decision-maker. Non-neutral language yielded lower cooperation in the give treatment among conditional and low cooperators. Taking yielded lower cooperation in the neutral treatment among low cooperators.

1 Introduction

Decades of research on decision-making in public good experiments resoundingly conclude that cooperation varies considerably in public good dilemmas, and individuals respond differently to different presentations of otherwise identical decision situations. With few exceptions, however, heterogeneity among decision makers and framing have been examined separately. Within those mostly separate bodies of research, existing studies that classified decision-makers into groups provided categories that were not fully representative of heterogeneity and/or did not predict cooperation in repeated public good dilemmas (Jackman, 2016b). Among framing studies, frames often simultaneously differ along more than one dimension, combining notions of positive versus negative, gains versus losses, giving versus taking, and so on. Casting information in positive versus negative light, in terms of gains versus losses, or as giving versus taking generally leads to more or less cooperation. However, the complexity of public good frames and the layering of design elements to create frames confound understanding how different elements of the framing design influence behavior. Where heterogeneity and framing have been jointly explored, results have been ambiguous (e.g., Park, 2000; Sonnemans, Schram, & Offerman, 1998).

Via a novel 2×2 factorial design that decomposed frame along two dimensions, action (i.e., give or take) and language (i.e., neutral or non-neutral), this research examines the differential effects of frame on different types of decision-makers in an incentivized, linear repeated public good experiment. This research distinguishes itself from previous work in three ways. First, independent of the repeated dilemma, decision makers were classified into four different types that proved to be highly predictive of behavior in the repeated public good dilemma. Individuals who were not expected to cooperate cooperated trivially or not at all. Individuals previously assessed as low, conditional, and high cooperators cooperated at distinct and non-trivial levels that declined over the course of the repeated dilemma. Second, different types of decision-makers responded to framing differently. Specifically, the decisions of individuals previously identified as non-cooperators did not differ across frames, but individuals previously identified as low, conditional, and high cooperators did. Third, framing effects emerged from the interaction of the two dimensions of frame and did not uniformly influence

every type of decision-maker. Non-neutral language yielded lower cooperation in the give treatment among conditional, low, and high cooperators. The take treatment yielded lower cooperation in the neutral treatment among low and high cooperators.

This paper is the second in a series of three papers, all of which are based on the same multi-part linear public good experiment. The experiment included a one-shot public good dilemma, a repeated public good dilemma, and a questionnaire. It was designed to assess and classify heterogeneity in behavior and motives among decision-makers (paper 1), examine how framing effects differ for different types of decision-makers (paper 2), parse the role of reference dependence and the language in framing effects (paper 2), and examine the channel/mechanism through which frames operate and how those channels differ for different types of decision-makers (paper 3). In this paper, I address the second and third of these objectives. I analyze cooperation decisions in the repeated public good dilemma only and incorporate decision-maker types and motives as assessed in the first paper[46]

In the following, I first describe the existing landscape of research upon which this study builds, including public good experiments, heterogeneity among decision-makers, and framing. I then describe the study participants, experimental design, and empirical methods. I present results, then close with a discussion and conclusions.

2 Background

Although stylized models, repeated public good experiments nevertheless capture the fundamental and essential elements of real-world public good dilemmas, like public education, disaster relief, defense, public radio, and environmental goods. In the laboratory and in the real world, public goods must be created or maintained by decision-makers subject to a conflict between own and group interests, and public goods are shared, without exclusion, by everyone. Real-world public goods are often provided through charitable giving, where appeals to social responsibility and social comparisons are important tools for motivating cooperation, or through policy. Mandates dominate in the policy sphere where price-based incentive also

[46] These three papers make-up the three chapters of this dissertation. In this paper, papers 1 and 3 are cited as Jackman (2016b) and Jackman (2016a), respectively.

play a role in public goods provision. However, we are beginning to see private market interventions that echo approaches to charitable giving and leverage findings from behavioral research. Examples include voluntary green electricity programs that appeal to a sense of altruism or feelings of obligation (Jacobsen, Kotchen, & Vandenbergh, 2012) and voluntary energy conservation efforts motivated through injunctive and descriptive norms (Allcott, 2011). Framing is a potential tool for motivating greater provision of public goods that, in the context of an inherently heterogeneous populations of decision-makers, is not yet well-understood. This study moves that frontier.

2.1 Public good experiments and heterogeneous cooperation

Public good experiments are simultaneous games with more than two players that intend to reproduce the essential non-excludable and non-rival attributes of pure public goods.[47] In the standard public good experiment,[48] every individual decides how to distribute her personal endowment between herself and the group. Allocations to the group constitute "cooperation." In the standard public good experiment with linear payoffs (p_i), every individual (i) decides how to distribute her personal endowment (w_i) between herself and the group. Allocations to the group (g_i) constitute cooperation and are increased by a positive factor, k, and then divided among n participants. The result is a marginal per capita return (MPCR) of $\gamma = k/n$. Payoffs are typically given by the following:

$$p_i = w_i - g_i + \gamma \sum_{j=1}^{n} g_j \quad (1)$$

Under the usual assumptions of rational choice theory, the dominant strategy is to allocate nothing to the group even though everyone is better off if everyone cooperates. This is clearly not what we observe in real world situations where contributions to the Red Cross, purchases of carbon offsets, or voluntary participation in renewable electricity programs are common, nor in public good experiments where levels of cooperation differ significantly across individuals.[49]

[47] Non-excludable means that everyone has access to the public good. Non-rival means that consumption by one individual does not affect others' consumption. Cooperation consists of providing or maintaining the public good.
[48] I refer to public good dilemmas that match the rules and payoffs described here as "standard" and public good dilemmas that do not as "non-standard."
[49] See Chauduri (2011) for a recent, selective survey of laboratory public good experiments and Ledyard (1995) for an older and exhaustive review of public good experiments.

The heterogeneity in cooperation observed in public good experiments (Chaudhuri, 2011; Ledyard, 1995) combined with substantial evidence for a wide variety of preferences, decision rules, and beliefs (Della Vigna, 2009) very strongly suggests that different decision processes are underway for different types of decision-makers. Going back to 1996, "evidence from economics as well as psychological experiments on the voluntary provision of public goods indicates that substantial differences exist in the attitudes of subjects toward contributing" (Offerman, Sonnemans, & Schram, 1996). Many efforts to characterize heterogeneity have attempted to match behavior to preconceived categories of decision-makers that underrepresent their respective study populations and/or are not good predictors of behavior in repeated public good dilemmas (Jackman, 2016b). In contrast, Jackman (2016b) employed a combination of group-based trajectory modeling and exploratory factor analysis to identify four differently motivated, behavior-based types of decision-makers and assessed type for all of the participants in this study. This paper demonstrates that those types actually predict behavior in a repeated public good dilemma. In other words, different types of decision-makers as assessed in Jackman (2016b) behave in statistically distinct ways in a repeated public good dilemma.

2.2 Framing and heterogeneous decision-makers

Another robust finding of public good experiments is that decision-makers often respond differently to the same information based on how the decision is presented, a phenomenon called a "framing effect." However, there are two important challenges to advancing the science. First, framing experiments almost always contrast frames that differ in more than one way, making it difficult to match the framing effect to its source. Second, most framing studies ignore heterogeneity among decision-makers, which could conceal effects and underlying mechanisms.

To be clear, frames are created by presenting the same information differently. Framing effects are observed when decision-makers subject to different frames choose differently. These definitions may seem to imply that frames are "objective" and imposed on decision-makers. However, Amos Tversky and Daniel Kahneman made the important point that frames are also subjective and therefore based on individuals' perceptions (Kahneman & Tversky,

1979; Tversky & Kahneman 1981, 1986, 1991). In other words, frame is defined as both the "formulation to which decision makers are exposed" and "the interpretation that they construct for themselves" (Dufwenberg, Gächter, & Hennig-Schmidt, 2011 citing Kahneman, 2000, p. xiv). In this study, frame refers only to how the frame is constructed.

The standard frame for a public good experiment describes the decision as a voluntary contribution or public good provision. Many dilemmas are presented as counterpoints to the voluntary contribution frame, including resource dilemmas (e.g., Parks, 1994), commons dilemmas (e.g., Apesteguia & Maier-Rigaud, 2006; Brewer & Kramer, 1986; Ostrom, 2006), take-some games (e.g., Bougherara, Denant-Boemont, & Masclet, 2011; Fleishman, 1988; Rutte, Wilke, & Messick, 1987; van Dijk & Wilke, 2000), and negative frames (e.g., Andreoni, 1995; Park, 2000; Sonnemans et al., 1998; Willinger & Ziegelmeyer, 1999). As in contribution experiments, these studies typically show that many individuals cooperate and cooperation varies considerably across individuals. However, cooperation in these counterpoint dilemmas tends to be lower even when, from the perspective of a perfectly rational actor, the decision situations were strategically identical to decision situations in contribution experiments.

Framing studies that maintain the essential non-rival and non-excludable nature of public goods contrast giving versus taking, leaving versus keeping, being generous versus employing restraint, achieving gains versus avoiding losses, positive versus negative, doing good versus doing or preventing bad, cooperative versus competitive, and so on. Though the evidence is somewhat mixed, several patterns emerge: 1) contribution frames generally yield more cooperation than take frames (c.f., Bougherara et al., 2011; Brewer & Kramer, 1986; Cubitt, Drouvelis, & Gächter, 2011; Dufwenberg et al., 2011; Fleishman, 1988; Rutte et al., 1987), 2) avoiding losses motivates more action than pursuing gains (e.g., Levin, Gaeth, Schreiber, & Lauriola, 2002; Levin, Schneider, & Gaeth, 1998; Sonnemans et al., 1998), 3) positive frames yield more cooperation than negative frames (c.f., Andreoni, 1995; Cookson, 2000; Levin et al., 2002; Levin et al., 1998; Park 2000; Willinger & Ziegelmeyer, 1999), and 4) wording like "I" instead of "We" and labels like "Wall Street" and "Stock Market" instead of "Community" or no label at all yield different levels of cooperation (c.f., Cookson, 2000;

Dufwenberg et al., 2011; Ellingsen, Johannesson, Mollerstrom, & Munkhammer, 2012; Liberman, Samuels, & Ross, 2004; Rege & Telle, 2004).[50]

Scrutinizing the myriad ways frames are constructed in public good dilemmas reveals three differences typically present between contrasting frames: the size and/or location of the initial endowment, the words used to label and describe the decision, and the characterization of the externality resulting from decisions to provide or not to provide for the public good. The following examples illustrate these three attributes of framing design and how these approaches are intertwined. Andreoni (1995) contrasted cooperation in positive and negative frames of a standard repeated public good dilemma with equivalent linear payoffs. Frames differed in the imposition of positive versus negative externalities, which was the focus of the study, but initial allocations also differed. In both frames, an individual received an initial "budget" of 60 tokens; individuals in the negative frame also received 120 tokens in "automatic earnings." Andreoni found more cooperation in the positive frame that he attributed to the positive framing of the externality. However, if individuals evaluate gains and losses relative to some reference value, then automatic earnings could have played a role in the framing effect in addition to or instead of the imposition of positive or negative externalities.

In another example, Dufwenberg et al. (2011) examined framing according to a 2×2 factorial design, give frame versus take frame and community labeling versus neutral labeling.[51] Focusing on the first dimension, in the give treatment, "every participant receive[d] an 'endowment' of 20 Thaler" – their word for monetary units – and were instructed to "contribute" up to 20 Thaler to the project. In the take treatment, instructions stated that "there [were] 60 Thaler in a project of your group," and individuals were instructed to "take" up to 20 Thaler from the project. Their frames differed in both the location of the initial endowment, and the words used to describe the decision. However, in addition to contrasting "give" and "take" across frames, "give" and "keep" were used interchangeably within frames,

[50] Ellingsen et al., (2012), Liberman et al. (2004), and Rege & Telle (2004) were Prisoners' Dilemma experiments.
[51] Neutral and community refer to the ways that Dufwenberg et al. (2011) labelled their experiment for different treatment groups. In some treatment groups, the experiment was labelled as a "the community experiment." In other treatment groups, the experiment was labelled as "the experiment." Note that quoted text is there English translation from German.

as were "take" and "leave."[52] Though logically equivalent, these language differences could comprise another difference between frames with important behavioral consequences (van Dijk & Wilke, 2000).

Synthesizing the design lessons from Andreoni (1995) and Dufwenberg et al. (2011), the framing design implemented in this research first recognized the relationship between the location of the initial endowment and the action implied by locating the endowment with the individual (i.e., give) or with the group (i.e., take), making action the first dichotomous dimension of the framing design. This framing design also structured decisions so that the size, or perceived size, of the initial endowment was equal and not confounded by "initial earnings." In addition, this framing design acknowledged the potential power of the words imbedded in the instructions and avoided the assumption that logically equivalent wording yields equivalent outcomes. Thus, language was identified as the second dichotomous dimension of frame. "Allocate" was used consistently in neutral treatments, and only the words "give" and "take" were used in non-neutral treatments.[53] Furthermore, the framing design was intended to allow at most two differences between any two frames, a difference in action and/or in language. Thus, in every frame, the externality implicit in giving, not taking, or making allocations to the group was always characterized as weakly positive. The result was the 2×2 factorial framing design illustrated in Figure 2.1 and four frames – give neutral (GN), give non-neutral (GNN), take neutral (TN), and take non-neutral (TNN) – that were quite similar, but also held the potential for significantly different behavior.

The 2×2 framing design in this study also acknowledged two related theories regarding the mechanisms underlying framing effects: 1) reference-dependence and loss aversion (Kahneman & Tversky, 1979; Tversky & Kahneman 1981, 1986, 1991) and 2) valence-based attribute framing (Levin et al., 2002; Levin et al., 1998).[54] Reference dependence and loss

[52] Quoted text is from the English translation.
[53] To my knowledge, there is no evidence that "allocate" is neutral or more neutral than the words "give" or "take." This is an assumption.
[54] Cookson (2000) describes the theory of reference dependence, per Kahneman and Tversky (1979) and Tversky and Kahneman (1991), as the "most well-developed theory of framing." Levin et al. (1998) and Levin et al. (2002) provide a typology of framing effects in which they link the positive and negative ways in which frames are created (i.e., through attributes, goals, and risky choices) to different levels of cooperation. They attribute goal and risky

aversion state that "the carriers of value are gains and losses defined relative to a reference point" and "losses loom larger than gains" of the same magnitude (Tversky & Kahnman, 1991). Valence-based attribute framing "casts the same critical information in a positive or negative light" that evokes positive or negative associations (Levin et al., 1998).[55]

Neither theory captures the full breadth and complexity of public good frames. Recall that public good frames incorporate at least three elements: the location of the initial endowment, the wording and labels used to describe the decision; and the characterization of the externality implicit in the public good. In addition, a persistent challenge for both theories is that reference points, gains and loss, and positive and negative associations ultimately depend upon the perceptions of decision-makers. By separating the effects of the location of the initial endowment from the effects of language, this framing design delineates the effects of reference points and valence in a way not previously explored in the framing literature.

The second challenge for framing research is that few studies address the interaction of framing and heterogeneity among decision-makers. Exceptions include Park (2000), who replicated Andreoni's (1995) warm-glow and cold prickle experiment, and incorporated value orientation determined via the Decomposed Game.[56] Like Andreoni, Park found more cooperation in the positive frame. He also found that the negative frame was most salient among individuals with an individualistic orientation. Sonnemans et al. (1998) studied provision of a public good contrasted with prevention of a public bad via non-standard, step-level public good games. Value orientation was also assessed via the Decomposed Game. They observed greater cooperation in later rounds in the positive frame, but they also found little predictive value in value orientation. Parks (1994) casts some doubt on the applicability of the Decomposed Game to public good dilemmas. He found that value orientation determined via the Decomposed Game was not correlated with cooperation in a standard public good dilemma. Avoiding the apparent shortcomings of the Decomposed Game and based on the

---

choice framing effects to reference dependence, loss aversion, and prospect theory (Kahneman & Tversky, 1979). They ascribe attribute framing effects to associative processing.

[55] Cox and Beland (2013) offer an alternate definition of valence, namely "the emotional quality of an idea that makes it more or less attractive."

[56] Value orientation corresponds to "preferences regarding one's own welfare relative to the welfare of others" (Park 2000) and was assessed with Decomposed Game as in Liebrand (1984) and Offerman et al. (1996).

notion that the best predictor of future behavior is past behavior, Jackman (2016b) assessed heterogeneity in via the incentivized, two-stage, one-shot public good dilemma that preceded the repeated public good dilemma in this experiment.

3 Methods

This paper focuses on behavior in the repeated public good dilemma that is a core component of the experiment illustrated in Figure 2.2. The primary objectives of this research are to examine how the different elements of framing design influence behavior in the repeated public good dilemma and how framing effects differ for different types of decision-makers in a repeated public good dilemma. An important, secondary objective includes testing if the typing strategy for decision-makers developed in Jackman (2016b), and based on the one-shot public good dilemma, actually predicts behavior in the repeated public good dilemma.

In the following, I describe the study participants, experiment, decision situation, framing design, and modeling and analyses. When I report statistics, I use an alpha level of 0.05 for all statistical tests and describe results with *p*-values less than 0.05 as "statistically significant." I use the phrases "highly significant" for *p*-values less than 0.01, "weakly significant" for *p*-values between 0.05 and 0.1, and "marginally significant" for *p*-values close to but greater than 0.1. All analyses for this paper were generated using SAS® software.[57]

3.1 Study Participants

Experimental subjects were recruited from an existing pool of potential participants using the School of Information On-line Recruitment System for Experimental Economics (ORSEE) at the University of Michigan. The vast majority of prospective participants were students. Prospective participants received an email inviting them to participate in a "decision-making experiment" for which they would receive "$5 compensation for showing up" and "approximately $20 (including show-up compensation)" if they completed the experiment.

[57] Version 9.4 for Windows, Copyright © 2002-2012 by SAS Institute Inc. SAS and all other SAS Institute Inc. product or service names are registered trademarks or trademarks of SAS Institute Inc., Cary, NC, USA.

In total, 256 adults participated in the experiment via 16 different sessions consisting of 16 people per session. Each participant took part in only one session. As reported in Table 2.1, the average age of participants was 20.5 years ($N = 252, SD = 4.16$), 54 percent were women, 37 percent reported having no training in economics, and 38 percent of participants reported having no previous experience with "a study like this one." Table 2.1 also reports the distribution of participants among the four types of decision-makers assessed via the one-shot dilemma and Group-Based Trajectory Modeling (GBTM)[58] in Jackman (2016b). Nineteen percent of participants were classified as non-cooperators (NC), 51 percent were conditional cooperators (CC), 23 percent were low cooperators (LC), and 7 percent were high cooperators.[59]

Four sessions were assigned to each of the four frames in advance and with the expectation that the individuals assigned to each frame would, on average, be similar with regard to age, gender, training in economics, prior experience with studies like this one, and type of decision-maker. As reported in Table 2.1, participants' age ($F(3, N = 252) = 1.04, p = 0.3745$), training in economics ($X^2\ (3, N = 251) = 0.68, p = 0.8784$), and cooperative type ($X^2\ (9, N = 256) = 11.29, p = 0.2566$) did not differ significantly across the four frames, but there was a marginal association between gender and frame ($X^2\ (3, N = 256) = 6.09, p = 0.1073$) and a highly significant association between experience and frame ($X^2\ (3, N = 245) = 13.35, p = 0.0039$). Subsequent analyses controlled for these differences.

[58] Group-Based Trajectory Modeling (GBTM) was developed by Daniel S. Nagin, Bobby L. Jones, and others to empirically identify groups of individuals with similar patterns of behavior over time (Jones & Nagin, 2007; Jones, Nagin, & Roeder, 2001; Nagin, 1999, 2005). It is an empirical method that simultaneously 1) identifies groups of individuals with similar patterns of behavior, 2) estimates the likelihoods that an individual belongs to each group, and 3) models within-group behavior. Technically, GBTM is a specialized form of "finite mixture modeling" that simultaneously a) estimates a maximum likelihood model, b) the likelihoods of membership in each group, and c) the cooperation trajectory for each group via a double-limit censored normal regression model (a.k.a., double limit Tobit model).

[59] Jackman (2016b) also showed that motives differed systematically among the four types of decision-makers. Specifically, non-cooperators reported more self-interest, less concern with others' actions, and weaker or no feelings of obligation compared to conditional and high cooperators. Low cooperators were similar to non-cooperators with regard to self-interest, but otherwise were more strongly motivated by feelings of obligation and perceptions of others' actions. Low cooperators, however, also differed from conditional and high cooperators. Low cooperators were significantly more self-interested than conditional and high cooperators and less motivated by feelings of obligation.

3.2 The Experiment

The full experiment consisted of five components – instructions, practice questions, a one-shot public good dilemma, a repeated public good dilemma, and a questionnaire. See Figure 2.2 for an illustration of the experiment and the Appendix for the researcher script and participant instructions. The experiment was conducted via 16 separate sessions. Every session included exactly 16 people, and each participant took part in only one session. Each of the four frames was assigned to a different set of four sessions. The experiment was programmed and conducted using the software z-Tree (Fischbacher, 2007) in the School of Information experimental laboratory at the University of Michigan.

Each session commenced when recruits arrived at the waiting room. The first 16 recruits to arrive participated in the session. Additional recruits were paid $5 for showing up and left. Study participants were then invited into the computer laboratory where each individual chose to sit at one of 16 desktop computers separated by partitions. Participants were provided with written, frame-specific instructions stating that "the experiment consist[ed] of a series of decision situations" and explaining the decision situation and payoffs (see the Appendix). Instructions were also read aloud. To further ensure participants understood the decision situation and before the experiment could proceed, all participants were required to answer ten practice questions correctly.

At this point, the one-shot public good dilemma[60] proceeded in randomly assigned anonymous groups of four. In the one-shot public good dilemma, participants encountered two formulations of the same decision situation, an unconditional decision in which participants did not know others' decisions and 21 decisions conditioned on the hypothetical average decisions of others. To conclude the one-shot dilemma, each participant was shown his decision, his earnings, and the average decision of others in his four-person group.

The repeated public good dilemma, which is the focus of this paper, proceeded in randomly re-matched, anonymous groups of four. It began with onscreen instructions that

[60] The one-shot dilemma was patterned after the "P-experiment" employed by Fischbacher and Gächter (2010) and Fischbacher, Gächter, and Fehr, (2001). In Fischbacher and Gächter (2010), the one-shot dilemma preceded repeated dilemma decisions for some subjects and followed repeated dilemma decisions for others. They found no statistically significant difference in cooperation between subjects who encountered the one-shot dilemma before or after the repeated dilemma.

were also read aloud. Before the repeated dilemma commenced, participants were informed that they would encounter the same decision 10 times as part of different anonymous groups of four that would be randomly assigned and reassigned for each decision. Participants then responded to a series of ten identical decision situations, called rounds. Cooperation decisions in each round were made simultaneously, without knowledge of the current decisions of other group members. Individuals also provided estimates for the average level of others' decisions and approximations of others' estimates (i.e., first and second order beliefs). Each individual's decision, points earned, average decision of other group members, and additional points corresponding to the accuracy of estimates were provided after each round.

Following the repeated dilemma, participants then completed a questionnaire that measured reactions to the experiment and collected demographic information. At the end of the experiment, points were converted to dollars and paid to participants. On average, participants earned $15 from their decisions plus $5 for showing up. The experiment took approximately one hour.

3.3 Decision Situation and Framing

In every decision in the experiment, every participant was randomly assigned to an anonymous group of four individuals and given decision-making power over an initial endowment of 20 points. Every decision required each individual to choose how to split her endowment between herself and the group knowing that individuals kept everything they allocated to themselves, and group members benefited equally from allocations to the group. Consistent with other linear public good experiments, payoffs were given by Equation (1), repeated below:

$$\pi_i = w_i - g_i + \gamma \sum_{j=1}^{n} g_j \quad (1)$$

where π_i represents points earned by person i, and g_i represents person i's allocation to the group. In other words, g_i represents cooperation by person i. Each individual's initial endowment was $w_i = 20$, groups were of size $n = 4$, and the marginal per capita return (MPCR) was $\gamma = 0.4$ as in Fischbacher and Gächter (2010).

The decision situation in this study was presented in four different ways according to two experimental treatments, according to the 2×2 factorial framing design illustrated in Figure 2.1. The design distinguishes 1) the action implied by the location of the initial endowment from the 2) the language used to describe the action. Neutral frames were nearly identical, differentiated only by the location of the initial endowment. The word "allocate" was used in both neutral frames to describe the action taken. "Allocate" was replaced with "give" and "take" to create the non-neutral frames. The result was four frames, give neutral (GN), give non-neutral (GNN), take neutral (TN), and take non-neutral (TNN). The following makes the differences among frames more clear

The give treatment located the initial endowment with the individual (i.e., in "personal accounts") and instructions stated the following: "To start, **your personal account contains 20 points**. Every individual has 20 points in their personal account that they can distribute between their personal account and the project account in any way they like." In contrast, take treatments located the initial endowment with the group (i.e., in the "project account") and instructions stated the following: "To start, **the project account contains 80 points.** Every individual may distribute 20 of the project points between their personal account and the project account in any way they like." In neutral frames, "allocate to" was used to describe the action, whereas "give to" and "take from" were used in non-neutral treatments. Therefore in the neutral treatment individuals were asked how many points they would "allocate" to the "project account" or to her "personal account". In the non-neutral treatments, individuals were asked how many points they would "give" to the project account or "take" from the project account.

Payoffs were always calculated in accordance with Equation (1) regardless of frame. Equation (1) reflects the framing of the give treatment, but payoffs were presented to participants via Equation (1a) in the give neutral frame and Equation (1b) in the give non-neutral frame.

$$\textit{Total points-income} = \textit{(20 – points you allocated to the project account)} + 0.4 \times \textit{sum of all points allocated to the project account} \quad \text{(1a)}$$

$$\textit{Total points-income} = \textit{(20 – points you gave to the project account) + 0.4 × sum of all points given to the project account} \quad \text{(1b)}$$

For each individual, the points he "allocated" or "gave" to the project account comprised his cooperation.

Payoffs in the take treatment are the same as in Equation (1), but are more faithfully represented in Equation (2)

$$\pi_i = x_i + \gamma\left[\sum_{j=1}^{n} w_j - \sum_{j=1}^{n} x_j\right] \quad (2)$$

where π_i represents earnings for person i, and x_i represents points allocated by person i to his personal account. The same parameters apply; the initial endowment is given by $w_j = 20$, group size is $n = 4$, and marginal per capita return is $\gamma = 0.4$. Importantly, Equation (2) is equivalent to Equation (1) via the relationship, $x_i = w_i - g_i$ where g_i represents person i's cooperation. In the experiment, payoffs were presented as in Equation (2a) in the take neutral frame and Equation (2b) in the take non-neutral frame.

$$\textit{Total points-income} = \textit{points you allocated to your personal account + 0.4 × (80 – sum of all points allocated to personal accounts)} \quad \text{(2a)}$$

$$\textit{Total points-income} = \textit{points you took from the project account + 0.4 × (80 – sum of all points taken from the project account)} \quad \text{(2b)}$$

For each individual, points not "allocated to personal accounts" or not "taken from the project account" make up her cooperation.

In all other regards, oral and written instructions were nearly identical in all frames. Following Dufwenberg et al.'s (2011) example, the externality generated via the public good was always presented positively but weakly. Specifically, all frames included the following statement: "You will earn points from both accounts in every decision situation. You are the only one who will earn points from your personal account. However, everyone will profit

equally from the total amount in the project account." In addition, wording very strictly excluded any use of arguably equivalent terminology. For example, "keep" and "leave" were never used in place of "give" and "take" since "keep" and "leave."

3.4 Modeling and Analyses

Modeling and analyses consisted of two components, nonparametric tests and various specifications of a linear unobserved effects panel data model. In the following, I first describe the data. Then I examine the objectives, advantages, and limitations of non-parametric tests and linear unobserved effects panel data models.

3.4.1 Data

Table 2.2 presents the three level hierarchy of the data, the dependent variable, the independent variables at each level (i.e., covariates), and the time-constant, unobserved effects for session and individual. The units of analysis were the 256 study participants ($N = 256$) nested within 16 clusters ($S = 16$) that correspond to session. Each session was comprised of 16 individuals ($M_s = 16$). Each of the four different frames was applied to four sessions. Cooperation was the dependent variable of interest and was measured for each individual over the 10 rounds of the repeated public good dilemma, where round is analogous to time ($t = 0, \ldots, 9$). Session level covariates included the two experimental treatments, action (i.e., give or take) and language (i.e., neutral or non-neutral), that together created frames. Individual level covariates included type of decision-maker, gender, training in economics, and prior experience with behavioral experiments. Type of decision-maker was previously assessed in Jackman (2016b). Study participants self-reported gender, training in economics, and prior experience with behavioral experiments in the questionnaire. The result was a balanced cluster sample with unit-specific, balanced panel data and two sources of correlation – among individuals within the same session and among repeated measures within individuals.

3.4.2 Nonparametric Tests

Nonparametric tests were used to test for statistically significant differences in cooperation between or among groups. The Kruskal Wallis test compares the medians of a non-normal dependent variable (i.e., cooperation) for more than two categories of an independent variable (i.e., frame and cooperative types). The Kruskal Wallis test reduces to the Wilcoxon-Mann-Whitney test for dichotomous independent variables (i.e., action, language, gender, experience, and training). Pairwise tests between two frames employed the Dwass, Steel, Critchlow-Fligner (DSCF) multiple comparison analysis (Critchlow & Fligner, 1991; Dwass, 1960; Steel, 1960), which is based on pairwise two-sample Wilcoxon comparisons. Nonparametric tests were conducted using SAS software and the procedure NPAR1WAY with the WILCOXON and DSCF options.

There are challenges that nonparametric analyses do not address. Kruskal Wallis and Wilcoxon-Mann-Whitney tests only allow us to reject the hypothesis that the groups being compared are drawn from the same non-normal distribution.[61] They also do not estimate effects, do not control for known differences in the demographics of treatment groups, and cannot incorporate unobservable effects associated with session and individual. Linear unobserved effects panel data models overcome these challenges as well as allow for robustness checks. However, nonparametric tests do not require any assumptions of normality, asymptotic or otherwise; they are conceptually quite simple; and they lend weight to the findings of the unobserved effects model, which are quite complex and arguably require normality assumptions (West, Welch, & Gałecki, 2015, p. 17), at least asymptotically (Wooldridge, 2010, pp. 7-9).

3.4.3 Linear unobserved Effects Panel Data Model and Estimation[62]

The aim of the unobserved effects model was to quantify the relationship between cooperation ($COOPERATION_{sit}$) and the observed, explanatory variables of time (t), action

[61] These tests assume the same spread and non-normal distribution.

[62] Linear unobserved effects panel data models (Wooldridge, 2010, p. 281) often go by at least two other names, random effects models and linear mixed models (West et al., 2015). Implementation is identical, but the underlying normality assumptions are more restrictive.

(ACT_s), language ($LANG_s$), and type of decision-maker ($TYPE_i$) while also controlling for the time-constant, observed variables of gender ($GENDER_i$), training in economics ($ECON_i$), and prior experience with studies like this one (EXP_i). In addition to observed explanatory variables, the linear unobserved effects panel data model explicitly included unobserved, time-constant variables for session (σ_s) and individual (υ_i).

In this model, unobserved effects were treated as random variables, consistent with the notion that these unobserved effects represent "omitted variables or neglected heterogeneity" (Wooldridge, 2010, p. 286).[63] For example, session effects (σ_s) captured unobservable but palpable differences among sessions such as mood and energy level that were most evident in the waiting room. Individual effects (υ_i) captured time-constant, unobserved effects associated with each individual such as cognitive ability, country of origin, and socio-economic status. In addition, unobserved effects were assumed to have zero correlation with observed explanatory variables, and as such, estimation assumed a "random effects framework." Therefore, the unobserved effects are most appropriately identified as "random session effects" and "random individual effects" (Wooldridge, 2010, p. 286).[64]

Analyses focused on four specifications of the model, all of which include unobserved effects for session and individual. Three model specifications included demographic information, and one did not. The preferred specification of the model is given by the following:

$$\begin{aligned} COOPERATION_{sit} = {} & \beta_0 + \beta_1 t + \\ & \beta_2 ACT_s + \beta_3 LANG_s + \beta_4 ACT_s \times LANG_s + \\ & \boldsymbol{\beta_5 TYPE}_i + \\ & \boldsymbol{\beta_6} ACT_s \times \boldsymbol{TYPE}_i + \\ & \boldsymbol{\beta_7} LANG_s \times \boldsymbol{TYPE}_i + \\ & \boldsymbol{\beta_8} ACT_s \times LANG_s \times \boldsymbol{TYPE}_i + \end{aligned} \tag{3}$$

[63] Unobserved effects do not merely account for associations among individuals within session and among repeated measures for each individual as would be the case with cluster robust standard errors.

[64] This assumption is correct in an experimental context where the session level covariates are the experimental treatments (ACT_s and $LANG_s$) and appropriate for well delineated individual level covariates ($TYPE_i$, $GENDER_i$, $ECON_i$, and EXP_i). This assumption contrasts with the so-called "fixed effects" framework in which unobserved effects may be correlated with observed explanatory variables. Importantly, in "modern econometric parlance", "fixed effects" implies that time-constant unobserved effects are correlated arbitrarily with observed explanatory variables (Wooldridge, 2010, p. 286).

$$\boldsymbol{\beta_9} t * \boldsymbol{TYPE}_i +$$
$$\sigma_s + \upsilon_i +$$
$$\varepsilon_{sit}$$

where $COOPERATION_{sit}$ is the number of points, out of 20, that person i in session s allocated to the group project at time t. Individuals are indexed by $i = 1, \dots, N$ with $N = 256$ study participants. Sessions are indexed by $s = 1, \dots, S$ with $S = 16$. The ten rounds of the repeated public good dilemma are represented by time, $t = 0, \dots, 9$. ε_{sit} represents idiosyncratic error.

Note that with the exception of time, all observed variables are categorical. Therefore, all of the regression coefficients, except the intercept and the coefficient on t, represent a net effect, or some part thereof, relative to the reference type (i.e., non-cooperator), action (i.e., give), and language (i.e., neutral). In particular, ACT_s and $LANG_s$ are indicator (i.e., dummy) variables for action and language, respectively, where give (i.e., $ACT_s = 0$)and neutral (i.e., $LANG_s = 0$) are the reference values (i.e., omitted variables). $\boldsymbol{TYPE}_i$ represents a vector of three indictor (i.e., dummy) variables for conditional, low, and high cooperators where non-cooperator is the reference type (i.e., omitted variable). Thus every coefficient on $\boldsymbol{TYPE}_i$ and every interaction thereof, is also a vector. I have bolded $\boldsymbol{TYPE}_i$ and all associated coefficients to denote that these are vectors.[65] By removing terms that evaluate to 0, each cell in Table 2.3 simplifies Equation (3) for each type, action, and language combination. Figure 2.3 illustrates the relationships between type and frame combinations and net effects. Inspection of Table 2.3 and Figure 2.3 makes clear that, with few exceptions, single regressions coefficients do not fully represent framing effects. Most regression coefficients represent only a portion of the difference in cooperation between any two frames and/or types.

All specifications of the linear unobserved effects panel data model were estimated using SAS software and the MIXED procedure with observed effects in the MODEL statement and unobserved effects in the RANDOM statements. Estimation employed the restricted maximum likelihood method (METHOD = REML). Individuals' repeated measures were assumed

[65] Non-cooperators have $TYPE_i = (0,0,0)'$, conditional cooperators have $TYPE_i = (1,0,0)'$, low cooperators have $TYPE_i = (0,1,0)'$, and high cooperators have $TYPE_i = (0,0,1)'$. Corresponding coefficients on $TYPE_i$ and interactions with $TYPE_i$ are given by $\boldsymbol{\beta_5} = (\beta_5^1, \beta_5^2, \beta_5^3)$, $\boldsymbol{\beta_6} = (\beta_6^1, \beta_6^2, \beta_6^3)$, $\boldsymbol{\beta_7} = (\beta_7^1, \beta_7^2, \beta_7^3)$, $\boldsymbol{\beta_8} = (\beta_8^1, \beta_8^2, \beta_8^3)$, and $\boldsymbol{\beta_9} = (\beta_9^1, \beta_9^2, \beta_9^3)$.

to be correlated and best represented via first order autoregressive correlation matrix (TYPE=AR(1) in the REPEATED STATEMENT). I invoked the EMPIRICAL option to allow for heteroscedasticity-robust inference.

4 Results

In addition to demonstrating the presence of framing effects, this paper focuses on three nested results: 1) framing effects differ among types of decision-makers, 2) framing effects emerge as the result of the interaction between the action (i.e., give versus take) and language (i.e., neutral versus non-neutral) used to create frames, and 3) the interaction effect is not uniform among types. In the following, I first provide a general description of cooperation in the repeated public good dilemma. Then I examine framing effects beginning with the results of nonparametric tests and concluding with the results of the linear unobserved effects panel data model.

4.1 Descriptive Statistics

Table 2.4 provides descriptive statistics for mean per person cooperation – an individual's average level of cooperation over the ten rounds of the repeated public good dilemma – for all study participants and by frame. Table 2.4 also provides descriptive statistics for mean per person cooperation by type of decision-maker, gender, training, and experience. Figures 2.4, 2.5, and 2.6 illustrate the distribution of cooperation in every round overall, by frame, and by type of decision-maker. Clearly, many individuals cooperated at non-trivial levels;[66] cooperation declined over the course of the repeated public good dilemma overall, for each frame, and for each cooperative type; [67] and cooperation varied considerably, overall, for each frame, and for each type. The important takeaway here is that participants in this study

[66] Average cooperation in every round was non-trivial (statistically distinct from 0) overall, for every frame, and for every cooperative type according to Wilcoxon Signed Rank Sum Tests. However, we can clearly see in Figures 2.4, 2.5, and 2.6 that the median, which may be a better measure of center, did reach zero overall and for most subgroups.

[67] The decline in cooperation was statistically significant overall, for each frame, for each cooperative type, and for every demographic group. The decline in cooperation was evaluated via fixed effects analysis (Allison, 2005; Wooldridge 2010, pp. 300-302) clustered by individual with robust standard errors. Fixed effects analyses were implemented using SAS software with the GLM and GLIMMIX procedures.

behaved in much the same way as observed in other public good experiments. There appears to be no reason to suspect that results in this study are anomalous.

In addition, the first three columns of Table 2.4 under the heading "All Frames" show that mean per person cooperation differed significantly among types of decision-makers ($\chi^2(3, N = 256) = 58.09, p < 0.0001$), a result made even more evident in Figure 2.6. Pairwise tests showed that all differences in mean per person cooperation between types were significant except for conditional versus high cooperators. Furthermore, mean per person cooperation among men and women appeared to be similar ($\chi^2(1, N = 256) = 0.09, p = 0.7695$), training in economics was associated with lower cooperation ($\chi^2(1, N = 251) = 5.41, p = 0.0200$), and experience appeared not to influence cooperation ($\chi^2(1, N = 244) = 0.97, p = 0.3235$). Finally, Table 2.4 also provides descriptive statistics for all participants and for each demographic category by frame. These results show that the differences among types and between men and women, trained and untrained, and experienced and unexperienced were not consistent across frames.[68] However, looking within demographic categories and across frames, we see that give neutral was the most cooperative frame overall and for every subgroup except non-cooperators and individuals with no training in economics.

4.2 Framing Effects

4.2.1 Nonparametric Tests

Nonparametric tests support the following three conclusions. First, there was a statistically significant framing effect.[69] Second, unpacking results according to the 2×2 factorial framing design suggests that locating the initial endowment in personal accounts (i.e., giving) had a positive effect on cooperation, but the introduction of non-neutral language, (i.e., "give" or "take" instead of "allocate") reduced cooperation. However, action and language

[68] For example, when comparing individuals with and without training, mean and median mean per person cooperation were lower among individuals without training in the take-neutral frame, but individuals without training were more cooperative in all other frames and overall than individuals with training.

[69] This contrasts with results for the one-shot game where no framing effects were found (Jackman, 2016b). Framing effects were not expected for the conditional decision table because the conditional decision table did not require individuals to form beliefs about others' cooperation. In contrast, framing effects were expected in the repeated public good dilemma because individuals did form beliefs about others' cooperation. However, the hypothesis that belief formation is necessary for framing effects was not tested.

interact. The effect of language differed between give and take treatments, and the effect of action differed between neutral and non-neutral treatments. Specifically, non-neutral language reduced cooperation in the give treatment but had no discernible effect in the take treatment. The take treatment reduced cooperation in the neutral treatment, but had no discernible effect in the non-neutral treatment. Third, the effect of frame was quite different for different types of decision makers. Non-cooperators – who were more self-interested, unconcerned with others' actions, and unmotivated by feelings of obligation (Jackman, 2016b) – were not sensitive to frame. In contrast, conditional and low cooperator types – who were mindful of others' actions (Jackman, 2016b) – were sensitive to frame.

Specifically, the Kruskal Wallis test reported in the first row of Table 2.4 provides very strong evidence of framing effects among all four frames. Pairwise tests between every combination of frames, adjusted for multiple comparisons, and denoted by superscripts "a" and "b," show that participants in the give neutral frame cooperated significantly more than individuals in the other frames, and cooperation in the give non-neutral, take neutral, and take non-neutral frames was statistically indistinguishable. In fact, nonparametric analyses that excluded the give neutral frame provided no evidence of framing effects. Figure 2.5 illustrates the difference in the distribution of cooperation across frames, particularly how the give neutral frame stands out from the others.

Kruskal Wallis tests within demographic categories also revealed framing effects. In most categories, mean per person cooperation differed significantly among frames; give neutral was the most cooperative frame; and when pairwise differences between frames were significant, the give neutral frame was always implicated. Focusing on framing effects within cooperative types, nonparametric tests suggest that non-cooperators and high cooperators were not influenced by frame,[70] but Kruskal Wallis tests were significant for conditional and low cooperators. Pairwise tests showed that conditional and low cooperators cooperated less when non-neutral language was employed in the give treatment. Narrowing to just low cooperators,

[70] Differences in mean and median across frames for high cooperators were dramatic, but not significant, possibly due to the limited statistical power available with only seventeen high cooperators.

we also see that low cooperators in the neutral treatment cooperated significantly more when giving than when taking.

4.2.2 Linear Unobserved Effects Panel Data Model

Tables 2.5, 2.6, and 2.7 present results of four specifications of the linear unobserved effects panel data model. Each table serves different purpose. In short, Table 2.5 provides estimates of effects relative to reference points and variance parameter estimates. Therefore, it primarily shows that results are robust and variance within and between sessions was significant. Via Type III F-tests, Table 2.6 supports the selection of the specification I as the most preferred and very concisely reveals that the three-way interaction of action, language, and type drives framing effects. Table 2.7 estimates framing effects according to that three way interaction as well as their significance. For all three tables, specification I matches the model as given in Equation (3), Section 3.4.3. Specifications II, III, and IV include demographic variables, and estimation excluded 15 participants who did not report gender, training, and/or experience.

Before delving into details, first notice that estimates are clearly robust. The sign, magnitude, and significance of estimates in Table 2.5 and 2.7 are consistent across specifications. The significance of Type III F-tests given in Table 2.6 are also consistent across specifications. Note especially that robustness spans specifications with and without demographic variables, demonstrating that the "uneven" distribution of men and women and of experienced and inexperienced participants reported in Table 2.1 did not undermine analyses. Robustness is accompanied, unsurprisingly, by statistically significant variance parameters documented in Table 2.5. As expected, cooperation varied significantly across sessions and among individuals within session, and there was significant unexplained variance (i.e., idiosyncratic error).[71]

[71] Inspection of outliers revealed that some behavior was nonlinear, and the behavior of some individuals appeared to be at odds with their type. For example, some conditional cooperators did not cooperate even when others in their group(s) did. Other outliers appeared to be engaged in an effort to influence others' behavior by cooperating at very high levels or executing a tit-for-tat strategy in early rounds before defecting for the remaining rounds.

Second, recall from Section 3.4.3 that regression coefficients in Table 2.5 must be interpreted relative to reference categories, that is, the non-cooperator type in the give action and neutral language. As such, they represent one of three possible effects – an effect for a reference category only, a net effect, or some portion of a net effect. Thus, the regression coefficients and corresponding statistics in Table 2.5 suggest, but do not reveal, the level of interaction at which framing effects are evident and significant. In contrast, the Type III F-tests reported in Table 2.6 show that the three-way interaction of action, language, and type is a weakly significant predictor of cooperation.[72] In other words, the interaction of action and language yielded framing effects that differed across types of decision makers. In addition, these Type III F-tests, supported by likelihood ratios tests, revealed that two, three, and four-way interactions with time, action, language, and type were not statistically significant. Thus, Specifications III and IV were not preferred.[73] Controlling for the level of other factors, Type III F-tests also show that models that included demographic were inferior, a result also supported by likelihood ratio tests.

Third, despite the challenge of interpreting results in Table 2.5, these results nevertheless set the stage for identifying framing effects due to the three-way interaction of action, language, and type and via mean differences. To that end, revisit Table 2.3. Each cell in Table 2.3 simplifies Equation (3) for each type, action, and language combination. For example, the upper, left cell contains only the relevant (i.e., nonzero) terms for the non-cooperator type in the give neutral frame. Focusing on the non-cooperator type in the first row of Table 2.3, we see that the model reduces to the following for each of the four frames:

$$\text{Give Neutral:} \qquad \beta_0 + \beta_1 t \tag{4a}$$

$$\text{Give Non-Neutral:} \qquad \beta_0 + \beta_1 t + \beta_3 \tag{4b}$$

$$\text{Take Neutral:} \qquad \beta_0 + \beta_1 t + \beta_2 \tag{4c}$$

[72] Type III F-tests test for the presence of an effect for one factor while controlling for the level of other factors. These "tests correspond to hypotheses about the linear functions of true parameters and are evaluated using the sum of squares of the estimated parameters" (SAS Institute, Inc., 2008, p. 271). "When no missing cells exist in the factorial model [as is the case here] Type III SS coincide with the Yates' weighted squares-of-means technique" (SAS Institute, Inc., 2008, p. 284).

[73] Likelihood ratios tests were conducted on a series of nested model specifications in accordance with West et al., (2015, pp. 34-36, 77-80) with a significance threshold of 0.05.

Take Non-Neutral: $\beta_0 + \beta_1 t + \beta_2 + \beta_3 + \beta_4$ (4d)

Equations 4a through 4d make clear that β_1 is the slope for the non-cooperator type, and within the non-cooperator type, β_0 is the intercept for give neutral only. According to rows 1 and 2 in Table 2.5, both the intercept and slope for non-cooperators in the give neutral frame are highly significant. Non-cooperators in the give neutral frame initially cooperated at a low but highly significant level that decreased at a slow but highly significant rate. For non-cooperators only, β_2 represents the effect of locating the initial endowment in the project account instead of in personal accounts in the neutral language treatment (i.e., $TN - GN|NC$). Similarly, β_3 represents the effect on cooperation of non-neutral language relative to neutral language in the give treatment (i.e., $GNN - GN|NC$). According to rows 3 and 4 in Table 2.5, neither difference is significant.

Similarly, the coefficient on the interaction of action and language also is not significant (see Table 2.5 row 5). In isolation, however, β_4 reflects a partial, net effect and is not especially meaningful; it is merely a part of the difference in the intercept for the non-cooperator type between take non-neutral and any of the other frames. Like β_4, none of the remaining coefficients in Table 2.5 independently indicate a framing effect or lack thereof. Thus, to test the significance of the difference in cooperation between frames, I estimated mean differences and employed joint tests to evaluate statistical significance.

Table 2.7 presents estimated mean differences and the results of joint tests that concisely reveal heterogeneity in framing effects among types and differential effects of the action and language treatments. Results of the linear unobserved effects panel data model were mostly consistent with nonparametric tests. There was no framing effect among non-cooperators, a weak framing effect among conditional cooperators, and significant framing effects among low cooperators. High cooperators were the exception. Non-parametric tests revealed no framing effects among high cooperators, but the linear unobserved effects panel data model did. Wherever a significant pairwise effect was present, the give neutral frame was implicated.

Specifically, conditional cooperators in the give treatment cooperated less in the non-neutral treatment than in the neutral treatment, a result that was weakly significant in the first

specification without correction for multiple comparisons ($GNN - GN|CC = -2.18, p = 0.0749$) and weakly significant with a Bonferroni correction for multiple comparisons in the second specification ($GNN - GN|CC = -2.84, p = 0.0250$). Similarly, low cooperators in the give treatment cooperated less in the non-neutral treatment than in the neutral treatment (I: $GNN - GN|LC = -3.21, p = 0.0082$; II: $GNN - GN|LC = -3.28, p = 0.0058$), a result that was significant at the 0.05 level with the within-type Bonferroni correction for multiple comparisons. In addition, low cooperators in the neutral treatment cooperated less when taking than when giving (I: $TN - GN|LC = -2.75, p = 0.0078$; II: $TN - GN|LC = -2.75$, $p = 0.0079$), a result that was significant at the 0.05 level with the within-type Bonferroni correction. Finally, and keeping in mind that model results for high cooperators are based on very few data, high cooperators in the give treatment cooperated less in the non-neutral treatment (I: $GNN - GN|HC = -5.19, p = 0.0028$; II: $GNN - GN|HC = -4.75, p = 0.0020$), and high cooperators in the neutral treatment cooperated less in the take treatment (I: $TN - GN|HC = -3.94, p = 0.0252$; II: $TN - GN|HC = -3.30, p = 0.0117$).

4.3 Type Predicts Cooperation and Elucidates the Framing Effect

A secondary result of this paper is that the typing strategy implemented via the one-shot dilemma and group-based trajectory modeling in Jackman (2016b) proved to be a strong predictor of cooperation in the repeated dilemma. Returning to Table 2.3 and focusing on the first column, the linear unobserved effects panel data model reduces to the following for each type of decision-maker in the give neutral frame:

$$\text{Non-Cooperator:} \quad \beta_0 + \beta_1 t \tag{5a}$$

$$\text{Conditional Cooperator:} \quad \beta_0 + \beta_1 t + \beta_5^1 + \beta_9^1 t \tag{5b}$$

$$\text{Low Cooperator:} \quad \beta_0 + \beta_1 t + \beta_5^2 + \beta_9^2 t \tag{5c}$$

$$\text{High Cooperator:} \quad \beta_0 + \beta_1 t + \beta_5^3 + \beta_9^3 t \tag{5d}$$

Coefficients in Table 2.5 corresponding to conditional cooperators, low cooperators, and high cooperators are given by β_5^1, β_5^2, and β_5^3, and the interaction of time and non-reference types are given by β_9^1, β_9^2, and β_9^3. All are highly significant. Therefore, intercepts are higher and slopes are steeper for all types relative to the non-cooperator type.

In fact, type alone was unequivocally a significant predictor of cooperation. Type I F-tests (i.e., sequential sum of squares) for type and for time interacted with type were highly significant (I: $F(3, 2300) = 39.08, p < 0.0001$; II: $F(3, 2165) = 25.85, p < 0.0001$). Type actually elucidates the framing effect in this study. Not only did the model specification without type prove to be inferior via a likelihood ratio tests,[74] but when type was excluded from the linear unobserved effects panel data model, Type III F-tests of action, language, and their interaction were not significant.

5. Conclusion

Just about anyone – marketers, advertisers, speech writers, and friends as well as those who study communication, literature, and rhetoric, among others – will agree that "it's not just *what* you, it's *how* you say it" where "it" is a complicated message that includes nuanced information about own and others' actions, outcomes, obligations, expectations, and more. This paper gives new insight into the behavioral consequences of different presentations of a repeated public good decision among different types of decision makers. It also reveals important interactions between reference-dependence and valence, and calls into question spontaneous valence assumptions that may in fact be normative, not real.

Results of this study showed that there was a strong framing effect overall in which participants subject to the give neutral frame were significantly more cooperative than individuals in the other three frames. Cooperation among the remaining three frames (i.e., give non-neutral, take neutral, and take non-neutral) was statistically indistinguishable. In addition, conditional cooperators, low cooperators, and high cooperators responded to framing differences whereas non-cooperators did not. Furthermore, framing effects could not be attributed solely to either dimension of the framing design; the interaction of action and language yielded framing effects that differed across types of decision-makers.

These results were purely descriptive and were intended to be. They simply described the relationship between the ways frames were created and the behavioral responses of

[74] A likelihood ratio test of the first specification of the unobserved effects model versus a nested specification that excludes type indicated that the nested specification was inferior ($p < 0.0001$).

different types of decision-makers. However, via this theory-based framing design and behavior-based categorization of subject heterogeneity, results also provide insight into the underlying decision-processes at work. Results strongly suggest important interactions between reference-dependent decision-making and valence-based attribute framing, and a relationship between framing effects and regard for others' action or outcomes. Considered in light of the applied literature, results also suggest relevance to field experiments and real-world public good dilemmas and policy.

To better understand the interaction of reference-dependent decision-making and attribute framing, consider four hypothetical two-frame studies. First, imagine that this study had been limited to the give neutral and take neutral frames only, and results were based on the data collected in the give neutral and take neutral frames. In every regard except the location of the initial endowment, these frames were identical. However, the cooperation in the give neutral and take neutral frames differed significantly, a result that could be attributed entirely to the location of the initial endowment. In addition, the framing effect in this hypothetical, two-frame study would have been driven by low cooperators, a previously undifferentiated group of decision-makers.

Continuing on this tack, now imagine a study with only the give neutral and give non-neutral frames. The initial endowment was located in personal accounts in both frames, and differed only in the use of the word "allocate" instead of "give." Based on the data from the give neutral and give non-neutral frames, individuals in the neutral frame of this hypothetical, two-frame study would have cooperated more on average than individuals in the non-neutral frame but only among conditional and low cooperators.[75] To anyone who subscribes to the notion of attribute framing and perceives the word "give" as having purely positive associations, replacing the arguably neutral word "allocate" with the presumably positive word "give" should have led to more cooperation, not less. Instead, the word "give" appears to have carried a negative valence with negative associations and/or evoked a feeling of loss that led to

[75] Even though the linear unobserved effects panel data model indicated statistically significant differences in cooperation between the give neutral and take neutral frames for high cooperator, I am ignoring high cooperators here. There were only 17 high cooperators out of 256 participants, and non-parametric tests revealed no framing effects among high cooperators.

lower levels of cooperation. In contrast, there was no effect of language in the take frames, suggesting that the word "take" had neither positive nor negative associations relative to the word "allocate" or that the effect of non-neutral language was undiscernibly small relative to the effect of presenting individuals' endowments as part of the project account.

To conclude this thought experiment, imagine a study that consisted of only the give non-neutral and take non-neutral frames. Instructions for this study would have been a close approximation to the English language translation of Dufwenberg et al.'s (2011) neutral give and take frames with the critical difference that this hypothetical experiment would have been a repeated dilemma. Because researchers observe framing effects more often in repeated games (Cubbit & Drouvelis, 2011) and because frames differ on more than one dimension, I would have predicted a significant framing effect. However, there was no statistically significant difference in cooperation between the give non-neutral and take non-neutral frames. Could the negative effect of non-neutral language counteract the positive effect of reference point in the give frame? In other words, could the effect of valence and reference-dependence have opposite signs, leading to an insignificant net effect? In light of what this thought experiment has revealed already, this seems plausible.

In addition, the difference in framing effects among different types of decision makers is notable in and of itself because this study joins a very small number of previous studies that simultaneously examine framing effects and subject heterogeneity (e.g., Sonnemans et al., 1998; Park, 2000; Parks, 1994). However, this study also stands out because the categorization of decision-makers implemented here yields types that unequivocally predict behavior in the repeated public good dilemma. Thus, the type and corresponding motives, assessed in Jackman (2016b), were relevant to the repeated public good dilemma. Only individuals who were motivated by perceptions of others' actions and outcomes were influenced by frame (i.e., conditional, low, and high cooperators). In addition, framing effects differed between low cooperators, who were more self-interested and less motivated by feelings of obligation, and conditional and high cooperators, who were less self-interested with stronger feelings of obligation.

Since participants who were sensitive to framing were also individuals who were more inclined to report some regard for others' actions and/or outcomes, these results also appear to be consistent with the notion that expectations of others' cooperation, also called first order beliefs, are an intermediary between frame and cooperation (Dufwenberg et al., 2011; Ellingsen et al. 2012; Fischbacher & Gächter, 2010; Sonnemans et al., 1998; Willinger & Ziegelmeyer, 1999).[76] Because framing effects differed for differently motivated cooperators, the indirect effect of beliefs could differ for different dimensions of frame and for different types of decision-makers. These are the questions I explore in Jackman (2016a).

Finally, this study focused very intentionally on behavior in a public good dilemma as a highly stylized model for real-world public goods. In the laboratory environment, this study provides evidence that even very subtle differences in frame can yield significant differences in cooperation and that framing effects are the result of interacting design elements. These results suggests that bundling subtle and significant framing differences into the social, political, cultural, and ecological context of a field experiment or real-world policy could lead to a collection of increases and decreases in cooperation, for which the net effect could not be known in advance.

For example, Spence and Pidgeon (2010) tested gain and loss frames in the context of climate change mitigation and found that "gain frames were superior to loss frames in increasing positive attitudes toward climate change mitigation … that [were] partially suppressed by lower fear responses and poorer information recall." Loss aversion predicts the more positive response to the gain frame, risky choice framing could explain the negative effect

[76] Indeed, in a one-shot public good dilemma, Dufwenberg et al. (2011) found a positive correlation between cooperation and beliefs and a stronger association between beliefs and cooperation than between frames and cooperation. They concluded that frames influence cooperation through "a subtle interplay of framing, beliefs, and choice." Similarly, Sonnemens et al. (1998) attributed framing effects in repeated, step-level dilemmas to the dynamic environment in which beliefs about others were evolving over time in response to information about others' cooperation. In a similar vein, Willinger and Ziegelmeyer (1999) concluded that the framing effects in the later rounds of positively versus negatively framed dilemmas to different, context-dependent reactions to others' contributions. Those different "reactions" may have consisted largely of different context-dependent beliefs that precipitate different choices. In fact, in a contribution-framed, repeated public good dilemma Fischbacher and Gätcher (2010) actually demonstrated that information about others' cooperation is an important factor in belief formation, and beliefs are important predictors of behavior. Via a Prisoners' Dilemma experiment, Ellingsen et al. (2012) described social frames as coordination devices, concluding that frames "enter people's beliefs rather than their preferences."

of fear, and positive associations in the gain frame may have relaxed focus leading to poorer recall. Arguably, these are reasonable conclusions in hindsight, but could we have predicted these results in advance? Could we predict the net effect? And could very subtle changes in policy-frames tweak these interactions and yield different net effects? If framing is among the overlooked low hanging fruit in policy-driven public goods provision,[77] then understanding interactions within the framing design and in the relevant context are important trajectories for future research that must include laboratory and field studies.

[77] Cox and Beland (2013) argued that policy entrepreneurs are already employing frame as a policy tool. For example, they found that as the emotional quality (what they call valence) of sustainability increased, "policy entrepreneurs have used the idea [well beyond its origins in environmental policy]" to reframe problems and promote reforms in policy areas like pension reform, public finance, labor markets, and energy security.

References

Allcott, H. (2011). Social norms and energy conservation. *Journal of Public Economics, 95*(9-10), 1082-1095.

Allison, P. D. (2005). *Fixed effects regression methods for longitudinal data using SAS*. Cary, NC: SAS Institute, Inc.

Andreoni, J. (1995). Warm-glow versus cold-prickle: The effects of positive and negative framing on cooperation in experiments. *The Quarterly Journal of Economics, 110*(1), 1-21.

Apesteguia, J., & Maier-Rigaud, F. P. (2006). The role of rivalry: Public goods versus common-pool resources. *The Journal of Conflict Resolution, 50*(5), 646-663.

Bougherara, D., Denant-Boemont, L., & Masclet, D. (2011). Cooperation and framing effects in provision point mechanisms: Experimental evidence. *Ecological Economics, 70*, 1200-1210.

Brewer, M. B., & Kramer, R. M. (1986). Choice behavior in social dilemmas: Effects of social identity, group size, and decision framing. *Journal of Personality and Social Psychology, 50*(3), 543-549.

Chaudhuri, A. (2011). Sustaining cooperation in laboratory public goods experiments: A selective survey of the literature. *Experimental Economics, 14*, 47-83.

Cookson, R. (2000). Framing effects in public goods experiments. *Experimental Economics, 3*, 55-79.

Cox, R. H., & Beland, D. (2013). Valence, policy ideas, and the rise of sustainability. *Governance-an International Journal of Policy Administration and Institutions, 26*(2), 307-328.

Critchlow, D. E., & Fligner, M. A. (1991). On distribution-free multiple comparisons in the one-way analysis of variance. *Communications in Statistics-Theory and Methods, 20*, 127-139.

Cubitt, R. P., Drouvelis, M., & Gachter, S. (2011). Framing and free riding: Emotional responses and punishment in social dilemma games. *Experimental Economics, 14*(2), 254-272.

Della Vigna, S. (2009). Psychology and economics: Evidence from the field. *Journal of Economic Literature, 47*(2), 315-372.

Dufwenberg, M., Gächter, S., & Hennig-Schmidt, H. (2011). The framing of games and the psychology of play. *Games and Economic Behavior, 73*(2), 459-478.

Dwass, M. (1960). Some k-sample rank-order tests. *Contributions to Probability and Statistics* (pp. 198-202). Stanford, California: Stanford University Press.

Ellingsen, T., Johannesson, M., Mollerstrom, J., & Munkhammar, S. (2012). Social framing effects: Preferences or beliefs? *Games and Economic Behavior, 76*, 117-130.

Fischbacher, U. (2007). z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics, 10*(2), 171-178.

Fischbacher, U., & Gächter, S. (2010). Social preferences, beliefs, and the dynamics of free riding in public goods experiments. *American Economic Review, 100*(1), 541-556.

Fischbacher, U., Gächter, S., & Fehr, E. (2001). Are people conditionally cooperative? Evidence from a public goods experiment. *Economics Letters, 71*(3), 397-404.

Fleishman, J. A. (1988). The effects of decision framing and others' behavior on cooperation in a social dilemma. *The Journal of Conflict Resolution, 32*(1), 162-180.

Jackman, D. K. (2016a). *Beliefs pathway of framing effects in a repeated public good dilemma*. Ph.D. Dissertation Chapter. School of Natural Resources and Environment. University of Michigan. Ann Arbor, MI.

Jackman, D. K. (2016b). *Heterogeneous cooperation and motivation in a public good dilemma*. Ph.D. Dissertation Chapter. School of Natural Resources and Environment. University of Michigan. Ann Arbor, MI.

Jacobsen, G. D., Kotchen, M. J., & Vandenbergh, M. P. (2012). The behavioral response to voluntary provision of an environmental public good: Evidence from residential electricity demand. *European Economic Review, 56*, 946-960.

Jones, B. L., & Nagin, D. S. (2007). Advances in group-based trajectory modeling and an SAS procedure for estimating them. *Sociological Methods and Research, 35*(4), 542-571.

Jones, B. L., Nagin, D. S., & Roeder, K. (2001). A SAS procedure based on mixture models for estimating developmental trajectories. *Sociological Methods and Research, 29*(3), 374-393.

Kahneman, D. (2000). Preface. In D. Kahneman & A. Tversky (Eds.), *Choices, Values, and Frames*. Cambridge: Cambridge University Press.

Kahneman, D., & Tversky, A. (1979). Prospect theory: An analysis of decision under risk. *Econometrica, 47*(2), 263-291.

Ledyard, J. (1995). Public goods: A survey of experimental research. In A. E. Roth & J. H. Kagel (Eds.), *The Handbook of Experimental Economics* (pp. 111-194). Princeton, NJ: Princeton University Press.

Levin, I. P., Gaeth, G. J., Schreiber, J., & Lauriola, M. (2002). A new look at framing effects: Distribution of effect sizes, individual differences, and independence of types of effects. *Organizational Behavior and Human Decision Processes, 88*(1), 411-429.

Levin, I. P., Schneider, S. L., & Gaeth, G. J. (1998). All frames are not created equal: A Typology and critical analysis of framing effects. *Organizational Behavior and Human Decision Processes, 76*(2), 149-188.

Liberman, V., Samuels, S. M., & Ross, L. (2004). The name of the game: Predictive power of reputations vs. situational labels in determining Prisoners' Dilemma game moves. *Personality and Social Psychology Bulletin, 30*, 1175-1185.

Liebrand, W. B. G. (1984). The effect of social motives, communication and group-size on behavior in an n-person multi-stage mixed-motive game. *European Journal of Social Psychology, 14*(3), 239-264.

Nagin, D. S. (1999). Analyzing Developmental Trajectories: A Semiparametric, Group-Based Approach. *Pyschological Methods, 4*(2), 139-157.

Nagin, D. S. (2005). *Group-based modeling of development*. Cambridge, MA USA: Harvard University Press.

Offerman, T., Sonnemans, J., & Schram, A. (1996). Value orientations, expectations and voluntary contributions in public goods. *Economic Journal, 106*, 817-845.

Ostrom, E. (2006). The value-added of laboratory experiments for the study of institutions and common-pool resources. *Journal of Economic Behavior & Organization, 61*(2), 149-163.

Park, E.-S. (2000). Warm-glow versus cold prickle: A further experimental study of framing effects on free-riding. *Journal of Economic Behavior & Organization, 43*, 405-421.

Parks, C. (1994). The predictative ability of social values in resource dilemmas and public goods games. *Personality and Social Psychology Bulletin, 20*(4), 431-438.

Rege, M., & Telle, K. (2004). The impact of social approval and framing on cooperation in public good situations. *Journal of Public Economics, 89*(7-8), 1625-1644.

Rutte, C. G., Wilke, H. A. M., & Messick, D. M. (1987). The effects of framing social dilemmas as give-some or take-some games. *British Journal of Social Psychology, 26*, 103-108.

SAS Institute, Inc. (2008). *SAS/STAT 9.2 User's Guide*. Cary, NC: SAS Institute Inc. .

Sonnemans, J., Schram, A., & Offerman, T. (1998). Public good provision and public bad prevention: The effect of framing. *Journal of Economic Behavior & Organization, 34*, 143-161.

Spence, A., & Pidgeon, N. (2010). Framing and communicating climate change: The effects of distance and outcome frame manipulations. *Global Environmental Change-Human and Policy Dimensions, 20*(4), 656-667.

Steel, R. G. D. (1960). A rank sum test for comparing all pairs of treatments. *Technometrics, 2*, 197-207.

Tversky, A., & Kahneman, D. (1981). The framing of decisions and the psychology of choice. *Science, 211*(4481), 453-458.

Tversky, A., & Kahneman, D. (1986). Rational choice and the framing of decisions. *The Journal of Business, 59*(4), S251-S278.

Tversky, A., & Kahneman, D. (1991). Loss aversion in riskless choice - A reference dependent model. *Quarterly Journal of Economics, 106*(4), 1039-1061. doi: 10.2307/2937956

van Dijk, E., & Wilke, H. (2000). Decision-induced focusing in social dilemmas: Give-some, keep-some, take-some, and leave-some dilemmas. *Journal of Personality and Social Psychology, 78*(1), 92-104.

West, B. T., Welch, K. B., & Galecki, A. T. (2015). *Linear mixed models: A practical guide using statistical software* (Second edition ed.). Boca Raton, FL: CRC Press.

Willinger, M., & Ziegelmeyer, A. (1999). Framing and cooperation in public good games: An experiment with an interior solution. *Economic Letters, 65*, 323-328.

Wooldridge, J. M. (2010). *Econometric analysis of cross section and panel data* (Second ed.). Cambridge, Massachusetts: The MIT Press.

Tables

Table 2.1: Participant characteristics

	Total		Frames				ANOVA			
			GN	GNN	TN	TNN	df	F	p	
Age							3	1.04	0.3745	
Number	252		61	64	63	64				
Mean	20.54		20.15	20.23	21.44	21.31				
Standard Deviation	4.16		1.68	3.47	2.99	6.68				
			Frames				Chi Square Test			Fisher's Exact
	Total	%	GN	GNN	TN	TNN	df	X^2	p	p
Type							9	11.29	0.2566	0.2651
NC	48	19%	9	11	14	14				
CC	131	51%	40	37	28	26				
LC	60	23%	13	14	16	17				
HC	17	7%	2	2	6	7				
Gender							3	6.09	0.1072 *	
Men	117	46%	25	37	30	25				
Women	139	54%	39	27	34	39				
Training							3	0.68	0.8784	
No	94	37%	26	22	22	24				
Yes	157	63%	37	40	41	39				
Don't know (excluded)	5	--	1	2	1	1				
Experience							3	13.35	0.0039 ***	
No	94	38%	34	15	21	24				
Yes	151	62%	26	44	42	39				
Don't know (excluded)	11	--	4	5	1	1				

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: NC = non-cooperator. CC = conditional cooperator, LC = low cooperator, and HC = cooperator.
Note 3: Age was not reported for 3 individuals, and one age was misreported (age = 92) and excluded. Most participants were in their late teens (18 or 19) and early twenties. Eight were over 25.

Table 2.2: Data structure

Level of Data		**Variables**		**Unobserved Effects**
Level 3:	Cluster of Units ($S = 16$) ($M_s = 16$)	Cluster ID: Covariates:	session (s) action (ACT_s) language ($LANG_s$)	session (σ_s)
Level 2:	Unit of Analysis ($N = 256$)	Unit ID: Covariates:	individual (i) type ($TYPE_i$) gender ($GENDER_i$) training ($ECON_i$) experience (EXP_i)	individual (v_i)
Level 1:	Round ($T = 10$)	Round: Dependent Variable:	time ($t = 0, \dots, 9$) cooperation ($COOPERATION_{sit}$)	

Note 1: S is the number of clusters/sessions, indexed by s. M_s is the number of units/individuals in every cluster. N is the number of units/individuals in the study, indexed by i. T is the number of rounds, indexed by $t = 0, \dots 9$, for which measurements were made for every individual.

Table 2.3: Linear unobserved effects panel data model by type and frame

Type ($TYPE_i$)	Action and Language (ACT_s and $LANG_s$)			
	Give Neutral	Give Non-Neutral	Take Neutral	Take Non-Neutral
Non Cooperator	$\beta_0 + \beta_1 t$	$\beta_0 + \beta_1 t + \beta_3$	$\beta_0 + \beta_1 t + \beta_2$	$\beta_0 + \beta_1 t + \beta_2 + \beta_3 + \beta_4$
Conditional Cooperator	$\beta_0 + \beta_1 t + \beta_5^1 + \beta_9^1 t$	$\beta_0 + \beta_1 t + \beta_5^1 + \beta_9^1 t + \beta_3 + \beta_7^1$	$\beta_0 + \beta_1 t + \beta_5^1 + \beta_9^1 t + \beta_2 + \beta_6^1$	$\beta_0 + \beta_1 t + \beta_5^1 + \beta_9^1 t + \beta_2 + \beta_3 + \beta_4 + \beta_6^1 + \beta_7^1 + \beta_8^1$
Low Cooperator	$\beta_0 + \beta_1 t + \beta_5^2 + \beta_9^2 t$	$\beta_0 + \beta_1 t + \beta_5^2 + \beta_9^2 t + \beta_3 + \beta_7^2$	$\beta_0 + \beta_1 t + \beta_5^2 + \beta_9^2 t + \beta_2 + \beta_6^2$	$\beta_0 + \beta_1 t + \beta_5^2 + \beta_9^2 t + \beta_2 + \beta_3 + \beta_4 + \beta_6^2 + \beta_7^2 + \beta_8^2$
High Cooperator	$\beta_0 + \beta_1 t + \beta_5^3 + \beta_9^3 t$	$\beta_0 + \beta_1 t + \beta_5^3 + \beta_9^3 t + \beta_3 + \beta_7^3$	$\beta_0 + \beta_1 t + \beta_5^3 + \beta_9^3 t + \beta_2 + \beta_6^3$	$\beta_0 + \beta_1 t + \beta_5^3 + \beta_9^3 t + \beta_2 + \beta_3 + \beta_4 + \beta_6^3 + \beta_7^3 + \beta_8^3$

Note 1: This is the linear unobserved effects panel data model by type and frame as specified in Equation (3) in Section 3.4.3.
Note 2: Give is the reference action, neutral is the reference language, and non-cooperator is the reference type.
Note 3: Coefficients on $TYPE_i$ and interactions with $TYPE_i$ are given by $\boldsymbol{\beta_5} = (\beta_5^1, \beta_5^2, \beta_5^3)$, $\boldsymbol{\beta_6} = (\beta_6^1, \beta_6^2, \beta_6^3)$, $\boldsymbol{\beta_7} = (\beta_7^1, \beta_7^2, \beta_7^3)$, $\boldsymbol{\beta_8} = (\beta_8^1, \beta_8^2, \beta_8^3)$, and $\boldsymbol{\beta_9} = (\beta_9^1, \beta_9^2, \beta_9^3)$.

Table 2.4: Descriptive statistics for mean per person cooperation and non-parametric tests for framing effects

	All Frames			Frame											Kruskal Wallis (df = 3)		
				GN			GNN			TN			TNN					
	N	Mean	Median	N	Mean	Median	N	Mean	Median	N	Mean	Median	N	Mean	Median	X^2	p	
All (N =256)	256	3.98	2.85	64	5.49	4.95 [a]	64	3.19	2.05 [b]	64	3.55	2.20 [b]	64	3.68	3.30 [b]	13.08	0.0045	***
Type (N =256) ***																		
NC	48	1.13	0.00	9	0.66	0.00 [a]	11	0.86	0.00 [a]	14	1.06	0.40 [a]	14	1.72	0.65 [a]	1.88	0.5976	
CC	131	4.97	4.20	40	6.47	5.20 [a]	37	4.06	2.90 [b]	28	4.83	4.05 [a,b]	26	4.13	4.40 [a,b]	8.22	0.0416	**
LC	60	3.20	2.25	13	5.18	5.40 [a]	14	2.45	1.80 [b]	16	2.61	2.00 [b]	17	2.85	1.80 [a,b]	10.49	0.0148	**
HC	17	7.09	8.00	2	9.75	9.75 [a]	2	5.15	5.15 [a]	6	5.87	6.10 [a]	7	7.94	10.00 [a]	2.29	0.5141	
Gender (N =256)																		
Men	117	3.99	2.50	25	5.34	2.80 [a]	37	3.64	2.00 [a]	30	3.56	2.10 [a]	25	3.21	1.30 [a]	1.66	0.6452	
Women	139	3.97	3.20	39	5.59	5.20 [a]	27	2.57	2.10 [b]	34	3.54	2.60 [b]	39	3.98	3.50 [a,b]	14.91	0.0019	***
Training (N =251, "Don't know" excluded) **																		
No	94	4.66	3.65	26	5.57	4.90 [a]	22	3.41	2.95 [a]	22	3.49	1.90 [a]	24	5.75	5.40 [a]	11.13	0.0110	**
Yes	157	3.63	2.30	37	5.26	4.90 [a]	40	3.21	2.00 [a,b]	41	3.64	2.50 [a,b]	39	2.50	1.30 [b]	8.71	0.0334	**
Experience (N =244, "Don't Know" excluded)																		
No	94	4.17	3.15	34	6.00	4.85 [a]	15	3.48	2.90 [a,b]	21	2.14	1.90 [b]	24	3.76	3.50 [a,b]	10.09	0.0179	**
Yes	151	3.92	2.90	26	4.88	4.95 [a]	44	2.74	2.00 [b]	42	4.34	2.75 [a,b]	39	3.73	3.20 [a,b]	6.80	0.0785	*

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: NC = non-cooperator. CC = conditional cooperator, LC = low cooperator, and HC = cooperator.
Note 3: Mean per person cooperation is an individual's average level of cooperation over the ten rounds of the repeated public good dilemma.
Note 4: Statistics and significance for Kruskal Wallis tests among all four frames appear at the far right.
Note 5: The results of pairwise nonparametric tests between frames are summarized by the superscripts appearing to the right of each median. Within a row, when two frames do not share a superscript, the difference between medians is statistically significant at the 0.05 level. These pairwise tests employed the Dwass, Steel, Critchlow-Fligner (DSCF) multiple comparison analysis, which is based on pairwise two-sample Wilcoxon comparisons.

Table 2.5: Estimates and variance parameters for the linear observed effects panel data model

	I		II		III		IV	
Solution for Observed Effects	*Est.*	*(SE)*	*Est.*	*(SE)*	*Est.*	*(SE)*	*Est.*	*(SE)*
Intercept	1.83	(0.46) ***	2.39	(0.56) ***	2.33	(0.71) ***	1.92	(0.33) ***
Round/Time (t = 0, …, 9)	-0.21	(0.07) ***	-0.23	(0.08) ***	-0.22	(0.13) *	-0.13	(0.04) ***
Action (reference = Give)	0.16	(0.37)	0.15	(0.43)	0.45	(0.77)	0.45	(0.62)
Language (reference = Neutral)	0.26	(0.55)	0.32	(0.61)	0.03	(0.88)	0.58	(1.40)
Action X Language	0.41	(0.94)	0.69	(1.22)	0.84	(1.62)	1.31	(2.37)
Type (reference = Non-Cooperator)								
Conditional Cooperator (CC)	7.13	(1.31) ***	7.04	(1.36) ***	7.08	(1.38) ***	7.44	(1.39) ***
Low Cooperator (LC)	5.81	(0.97) ***	5.63	(1.07) ***	5.62	(1.06) ***	6.32	(0.84) ***
High Cooperator (HC)	12.58	(1.60) ***	11.76	(1.42) ***	11.67	(1.42) ***	14.60	(0.81) ***
Action, Language, & Type								
Action X CC	-1.67	(1.55)	-1.83	(1.58)	-1.83	(1.58)	-1.57	(1.86)
Action X LC	-2.91	(0.99) ***	-2.90	(1.04) ***	-2.90	(1.04) ***	-3.07	(0.89) ***
Action X HC	-4.11	(1.77) **	-3.46	(1.23) ***	-3.46	(1.23) ***	-6.39	(2.30) ***
Language X CC	-2.45	(1.40) *	-3.16	(1.59) **	-3.16	(1.59) **	-3.67	(2.07) *
Language X LC	-3.47	(1.50) **	-3.60	(1.47) **	-3.60	(1.47) **	-4.33	(2.53) *
Language X HC	-5.45	(1.54) ***	-5.07	(1.33) ***	-5.07	(1.33) ***	-9.07	(1.52) ***
Action X Language X CC	0.90	(1.73)	1.29	(1.90)	1.29	(1.90)	0.46	(2.87)
Action X Language X LC	3.44	(1.93) *	3.36	(1.87) *	3.36	(1.87) *	2.61	(3.51)
Action X Language X HC	6.52	(2.92) **	5.63	(2.67) **	5.63	(2.67) **	9.28	(4.46) **
Time & Type								
Time X CC	-0.38	(0.10) ***	-0.35	(0.11) ***	-0.36	(0.10) ***	-0.44	(0.17) ***
Time X LC	-0.25	(0.11) **	-0.25	(0.12) **	-0.25	(0.12) **	-0.40	(0.11) ***
Time X HC	-0.80	(0.24) ***	-0.76	(0.26) ***	-0.74	(0.26) ***	-1.39	(0.04) ***
Time, Action, & Language	No		No		Yes		Yes	
Time, Action, Language, & Type	No		No		No		Yes	
Demographics	No		Yes		Yes		Yes	
Observations	2560		2410		2410		2410	
Residual Log Likelihood	13108.6		13106.1		13103.4		13097.8	
Variance Parameters	*Est.*	*(SE)*	*Est.*	*(SE)*	*Est.*	*(SE)*	*Est.*	*(SE)*
Session Intercept	2.42	(1.29) **	2.66	(1.39) **	2.66	(1.39) **	2.66	(1.39) **
Individual w/in Session Intercept	9.30	(1.06) ***	8.54	(1.03) ***	8.54	(1.03) ***	8.52	(1.03) ***
AR(1) Row(Correlation)	0.27	(0.03) ***	0.26	(0.03) ***	0.26	(0.03) ***	0.26	(0.03) ***
Residual	11.93	(0.42) ***	12.11	(0.44) ***	12.1	(0.44) ***	12.15	(0.44) ***

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: Specification I matches the model as presented in Equation 3 in Section 3.4.3. Estimates include all 256 participants with 10 measures per participant.
Note 2: Specifications II, III, and IV excluded 15 participants with missing demographic information and therefore included 10 measurements for each of 241 participants.
Note 3: All specifications included unobserved effects for session and individuals within session.
Note 4: Standard errors are robust.
Note 5: To facilitate comparison across specifications, -2 residual log likelihood was estimated via the maximum likelihood method with the 241 participants for whom full demographic information was reported.

Table 2.6: Type III F-tests for the linear unobserved effects panel data model

	I (N=256)		II (N=241)		III (N=241)		IV (N=241)	
F Statistics	*F*	*p*	*F*	*p*	*F*	*p*	*F*	*p*
Round/Time (t = 0, …, 9)	74.79	<.0001 ***	65.88	<.0001 ***	62.3	<.0001 ***	155.55	<.0001 ***
Action (reference = Give)	0.33	0.5636	0.13	0.7219	0.02	0.9017	0.01	0.9040
Language (reference = Neutral)	1.72	0.1896	1.80	0.1794	1.9	0.1679	2.52	0.1127
Action X Language	4.08	0.0434 **	5.00	0.0254 **	3.72	0.0540 *	5.81	0.0160 **
Type (reference = Non-Cooperator)	39.08	<.0001 ***	28.13	<.0001 ***	28.6	<.0001 ***	39.05	<.0001 ***
Action X Type	0.72	0.5410	0.61	0.6117	0.61	0.6117	0.37	0.7767
Language X Type	1.90	0.1279	2.43	0.0633 *	2.43	0.0633 *	2.28	0.0778 *
Action X Language X Type	2.45	0.0615 *	2.13	0.0947 *	2.13	0.0947 *	1.72	0.1603
Time X Type	13.32	<.0001 ***	8.69	<.0001 ***	10.2	<.0001 ***	17.38	<.0001 ***
Time X Action X Language								
Time X Language					0.35	0.5561	1.29	0.2569
Time X Action					0.99	0.3197	0.13	0.7159
Time X Action X Language					0.04	0.8424	1.81	0.1790
Time, Action, Language, & Type								
Time X Language X Type							0.86	0.4602
Time X Action X Type							0.16	0.9230
Time X Action X Language X Type							1.00	0.3910
Demographics								
Gender (reference = Man)			0.69	0.4079	0.69	0.4078	0.68	0.4080
Economics (reference = No Training)			1.97	0.1611	1.97	0.1611	1.97	0.1610
Experience (reference = No Experience)			0.39	0.5301	0.39	0.5301	0.39	0.5301

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: Specification I matches the model as presented in Equation 3 in Section 3.4.3.
Note 2: Specification I includes all 256 participants. Specification II includes only the 241 participants for which gender, training, and experience information was available.
Note 3: Type III F-tests test for the presence of an effect for one factor while controlling for the level of other factors, where in this case "factors" are categorical variables and two-, three-, and four-way interactions of categorical variables. These "tests correspond to hypotheses about the linear functions of true parameters and are evaluated using the sum of squares of the estimated parameters" (SAS Institute, Inc., 2008, p. 271). "When no missing cells exist in the factorial model [as is the case here] Type III SS coincide with the Yates' weighted squares-of-means technique" (SAS Institute, Inc., 2008, p. 284). The advantage of the Type III F-test, relative to other F-tests, is that it is valid in the presence of significant interactions. The limitation is that Type III F-tests for main effects generally should not interpreted when significant interaction effects are present. In keeping with this convention, these F-tests revealed weakly significant framing effects resulting from the interaction of language and action and type (i.e., Language X Action X Type). Type III F-tests also revealed highly significant interaction of time and type (i.e., Time X Type).
Note 3: These tests were used in combination with likelihood ratio tests to compare different specifications of the unobserved effects model. The first specification was most preferred.

Table 2.7: Estimated mean differences between frames within type per the linear unobserved effects panel data model

	I					II				
	Est.	*(SE)*	*t*	*F*	*p*	*Est.*	*(SE)*	*t*	*F*	*p*
Non-Cooperators										
GNN - GN	0.26	(0.55)	0.48	0.23	0.6296	0.32	(0.61)	0.53	0.28	0.5992
TN - GN	0.16	(0.37)	0.45	0.20	0.6561	0.15	(0.43)	0.35	0.12	0.7265
TNN - GNN	0.57	(0.87)	0.66	0.44	0.5084	0.84	(1.11)	0.76	0.57	0.4496
TNN - TN	0.67	(0.76)	0.88	0.78	0.3775	1.01	(1.09)	0.93	0.86	0.3539
Conditional Cooperators										
GNN - GN	-2.18	(1.23)	-1.78	3.18	0.0749	-2.84	(1.26)	-2.24	5.03	0.0250 [a]
TN - GN	-1.51	(1.55)	-0.97	0.94	0.3324	-1.68	(1.58)	-1.06	1.13	0.2880
TNN - GNN	-0.19	(0.99)	-0.19	0.04	0.8458	0.30	(0.98)	0.3	0.09	0.7615
TNN - TN	-0.87	(1.38)	-0.63	0.40	0.5282	-0.86	(1.36)	-0.64	0.40	0.5247
Low Cooperators										
GNN - GN	-3.21	(1.21)	-2.64	6.99	0.0082 [b]	-3.28	(1.19)	-2.76	7.62	0.0058 [b]
TN - GN	-2.75	(1.03)	-2.66	7.09	0.0078 [b]	-2.75	(1.03)	-2.66	7.07	0.0079 [b]
TNN - GNN	1.10	(1.51)	0.73	0.53	0.4658	1.31	(1.39)	0.94	0.88	0.3495
TNN - TN	0.65	(1.37)	0.47	0.22	0.6370	0.77	(1.26)	0.61	0.38	0.5398
High Cooperators										
GNN - GN	-5.19	(1.74)	-2.99	8.94	0.0028 [c]	-4.75	(1.53)	-3.1	9.61	0.0020 [c]
TN - GN	-3.94	(1.76)	-2.24	5.02	0.0252 [a]	-3.30	(1.31)	-2.52	6.36	0.0117 [b]
TNN - GNN	2.99	(2.61)	1.15	1.31	0.2521	3.01	(2.59)	1.16	1.35	0.2463
TNN - TN	1.74	(2.62)	0.66	0.44	0.5068	1.56	(2.51)	0.62	0.38	0.5351

[a] Significant at the 0.10 level with a Bonferroni correction for multiple comparisons within type.
[b] Significant at the 0.05 level with a Bonferroni correction for multiple comparisons within type.
[c] Significant at the 0.05 level with a Bonferroni correction for multiple comparisons across all types.

Note 1: Specification I matches the model as presented in Equation 3 in Section 3.4.3.
Note 2: Specification I includes all 256 participants. Specification II includes only the 241 participants for which gender, training, and experience information was available.
Note 3: Estimates are mean differences.
Note 4: Four hypotheses were tested within each type of decision-maker. With a Bonferroni correction for multiple comparisons within type ($m = 4$), the $\alpha = 0.01$ threshold for significance becomes $\alpha/m = 0.01/4 = 0.0025$, the $\alpha = 0.05$ threshold for significance becomes $\alpha/m = 0.05/4 = 0.0125$, and the $\alpha = 0.10$ threshold becomes $\alpha/m = 0.10/4 = 0.0250$.
Note 5: Across types of decision-makers, sixteen hypothesis were tested. With a Bonferroni correction for multiple comparisons across all types ($m = 16$), the $\alpha = 0.05$ threshold for significance becomes $\alpha/m = 0.05/16 = 0.003125$, and the $\alpha = 0.10$ threshold becomes $\alpha/m = 0.1/16 = 0.00625$.

Figures

		Language: Neutral	Language: Non-Neutral
Action	Give	Give-Neutral (GN)	Give Non-Neutral (GNN)
	Take	Take Neutral (TN)	Take Non-Neutral (TNN)

Figure 2.1: 2×2 Factorial framing design
Note 1: Action and language are the two experimental treatments used to create four different frames.
Note 2: Each of the frames was assigned to a different set of 4 sessions.

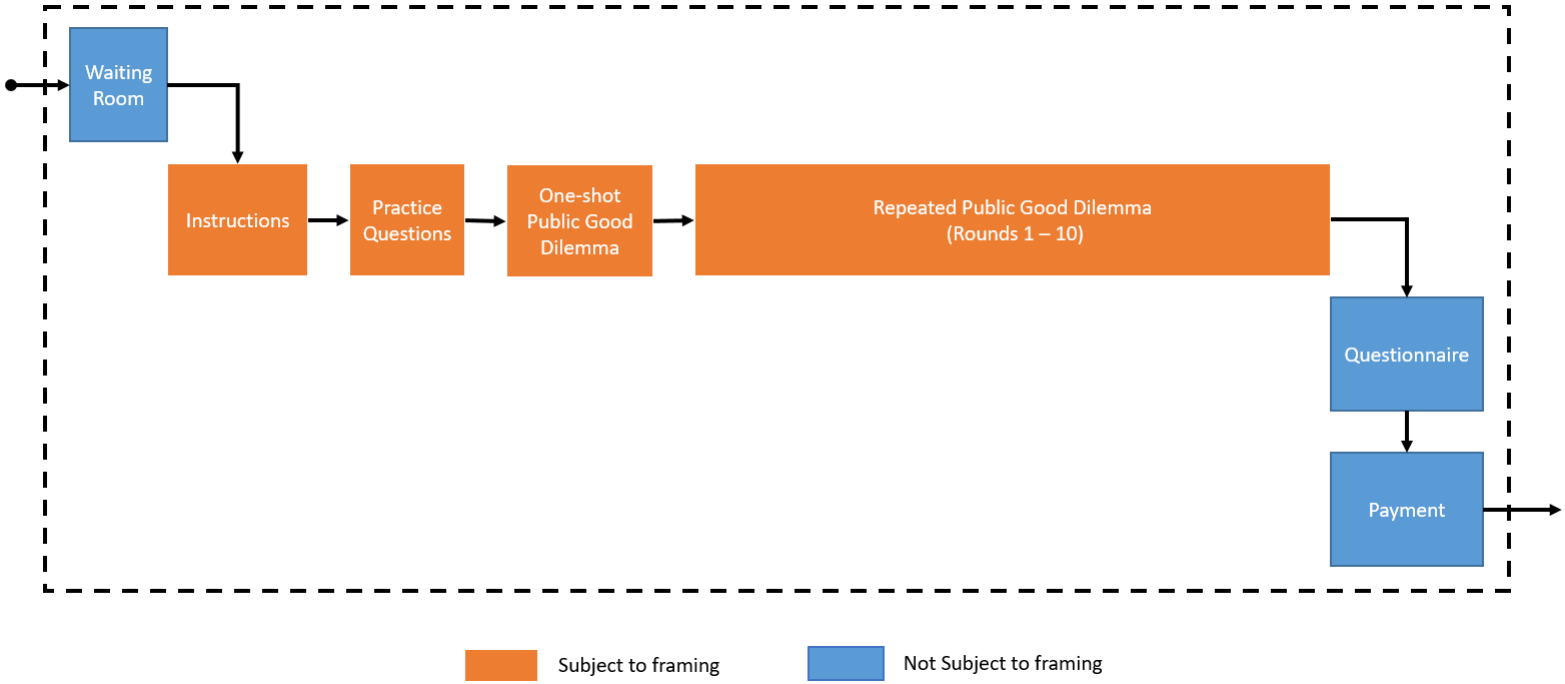


Figure 2.2: The experiment

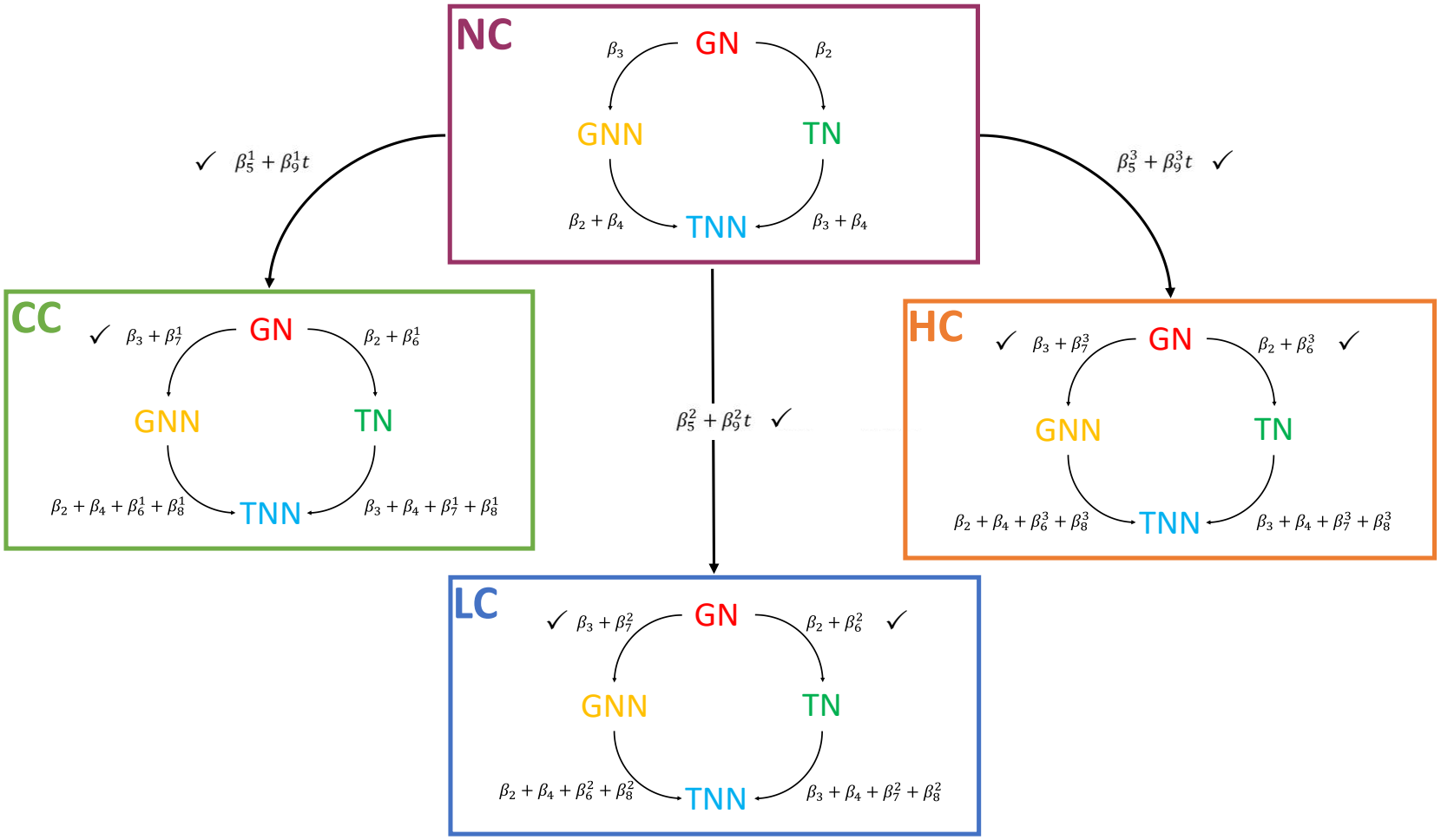


Figure 2.3: Diagram of the linear unobserved effects panel data model

Note 1: Give is the reference action, neutral is the reference language, and non-cooperator is the reference type.

Note 2: Net effects are represented by arrows between types (between boxes) or frames (within boxes) and correspond to Equation (3) (Section 3.4.3).

Note 3: Check marks indicate differences that were at least weakly significant according to pairwise nonparametric tests (Section 4.2.1) and/or the first or second specification of the linear unobserved effects panel data model (Section 4.2.2).

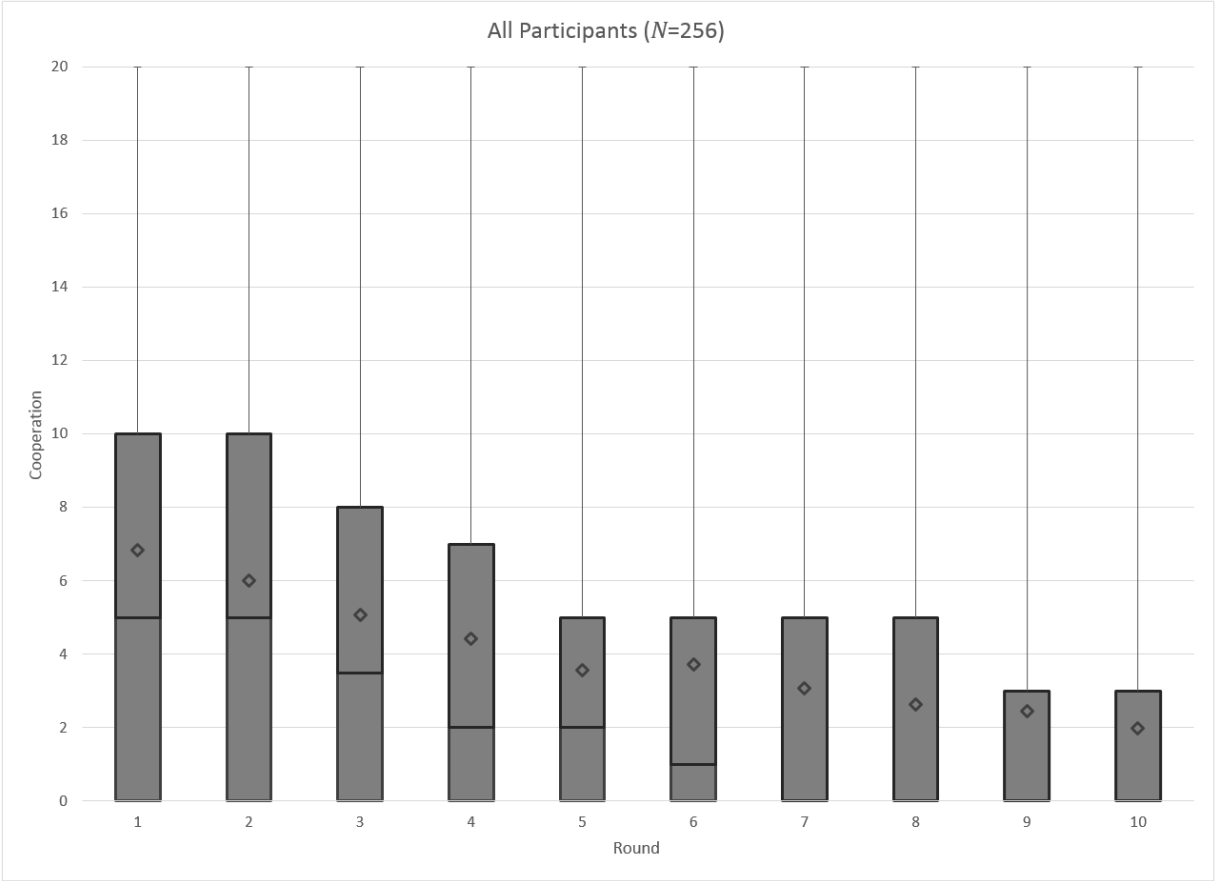


Figure 2.4: Distribution of cooperation in every round for all participants
Note 1: Boxes represent the middle quartiles for cooperation for each round of the repeated public good dilemma. The horizontal line in each box represents the median. Boxes without a horizontal line have median equal to zero. The diamond represents mean. The whiskers extend to the maximum and minimum.

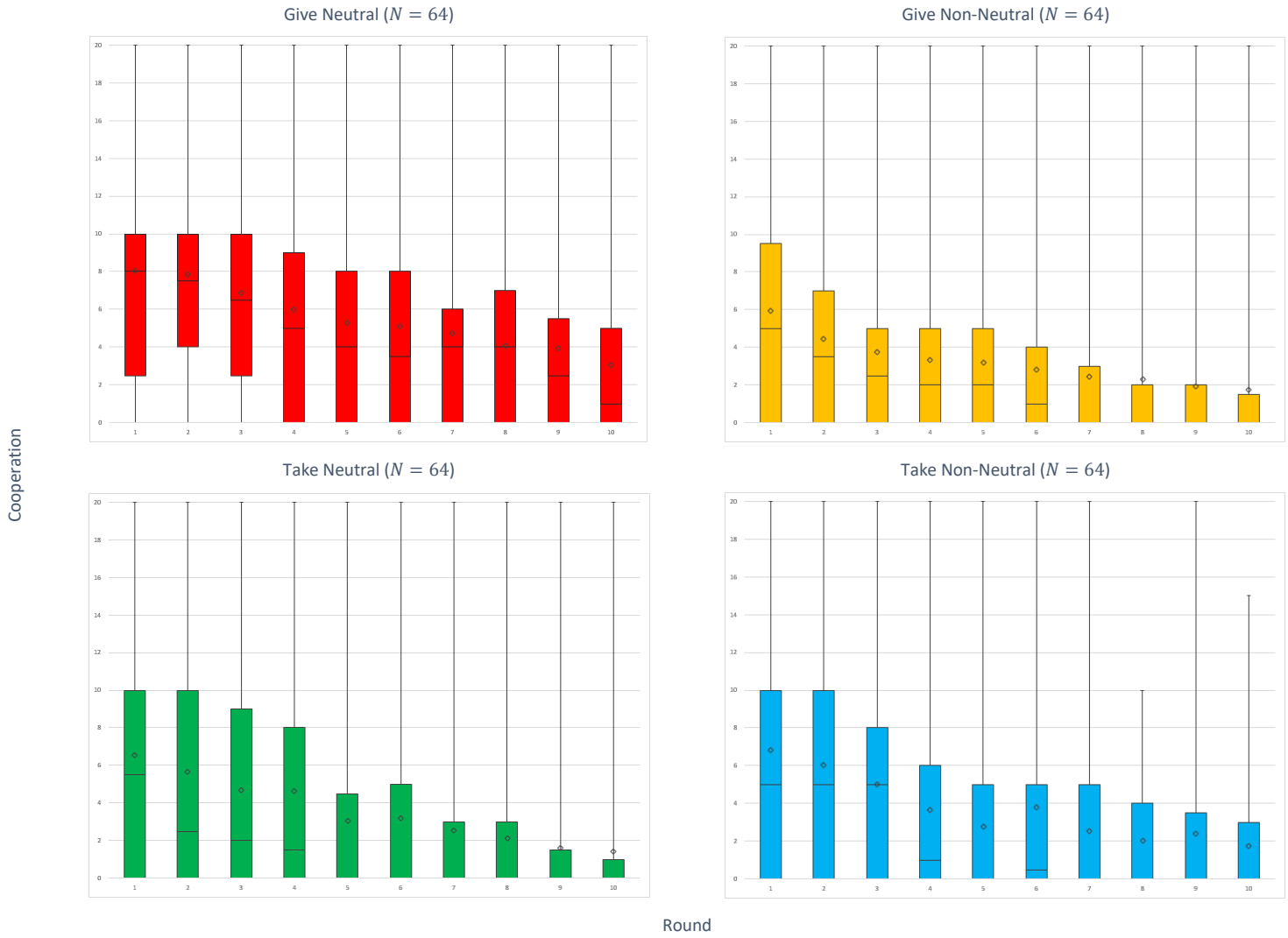


Figure 2.5: Distribution of cooperation in every round by frame

Note 1: Boxes represent the middle quartiles for cooperation for each round of the repeated public good dilemma. The horizontal line represents the median, and the diamond represents mean. Boxes without horizontal lines have median equal to zero. The whiskers extend to the maximum and minimum levels of cooperation.

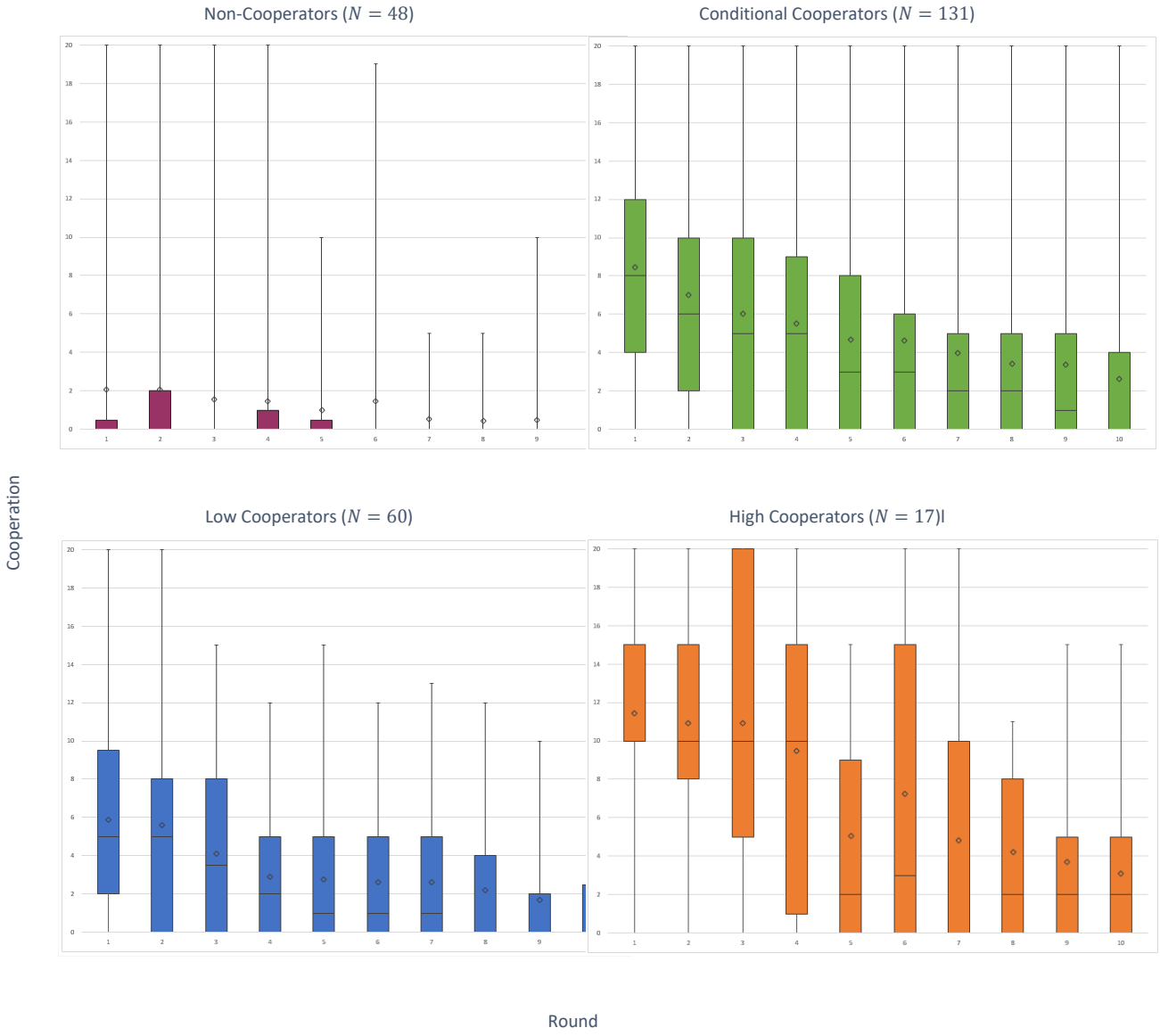


Figure 2.6: Distribution of cooperation in every round by cooperative type

Note 1: Boxes represent the middle quartiles for cooperation for each round of the repeated public good dilemma. The horizontal line represents the median, and the diamond represents mean. Boxes without horizontal lines have median equal to zero. The whiskers extend to the maximum and minimum.

Chapter 3

The Beliefs Pathway of Framing Effects in a Repeated Public Good Dilemma

Abstract: A persistent challenge for framing researchers is that decision processes depend upon the perceptions of decision-makers. Previous studies strongly suggest that beliefs about others' cooperation provide insight into underlying decision processes, and framing passes through beliefs in decision process where framing effects are observed. However, none of these studies provides a pathway analysis of that mechanism, and none addresses how those pathways differ among different type of decision-makers. This study examines framing effects and the relationships between beliefs and cooperation among different types of decision makers in a repeated linear public good dilemma. Using moderation and mediation analyses, I show that frames influence beliefs and beliefs influence cooperation. However, the relevance of beliefs differs among types of decision-makers. For conditional cooperators, who made up 51% of the study population, frame unequivocally exerts influence on cooperation through beliefs; there was no evidence that frame influences cooperation independent of beliefs for conditional cooperators. In contrast, for the study participants who were not expected to cooperate, mediation analyses provides no evidence of a beliefs pathway even though their estimates of others' cooperation were quite good and appeared to be sensitive to framing. For other types of decision-makers, evidence for a beliefs pathway was mixed. This study also shows that in general individuals cooperated slightly less on average than they believed others would, but this result differed across types of decision makers. Individuals previously identified as high cooperators (7%) cooperated more on average than they believed others would. Individual previously identified as non-cooperators (19%) cooperated much less on average than they believed others would.

1 Introduction

Although greatly simplified representations of real-world public good dilemmas, repeated public good experiments capture the essential elements of real-world public good decisions. In the laboratory and in the real world, public goods must be created or maintained by decision-makers subject to a conflict between own and group interests. Real-world public goods are sometimes provided voluntarily or through charitable giving, where appeals to social responsibility, social comparisons, gains to society, and the benefits to individuals are important tools for motivating cooperation. Public goods are more often and primarily provided through public policy mandates and less often through price-based incentives.

In the policy sphere, we are beginning to see interventions that employ findings from behavioral research. Examples include voluntary green electricity programs that appeal to altruism, feelings of obligation, and environmental benefits (e.g., Jacobsen, Kotchen, & Vandenbergh, 2012) and voluntary energy conservation efforts motivated through information about others' energy consumption, normative declarations, and competition (e.g., Allcott, 2011). These emerging techniques overtly exploit what decision-makers know or believe about others. They employ framing by casting pro-environment behaviors and outcomes in a positive light. And by simultaneously invoking several different approaches, they implicitly leverage heterogeneity among decision-makers.

In support of policy innovations, the objectives of this research are to understand how frames influence cooperation and beliefs about others' cooperation overall and among different types of individuals, and under what conditions (i.e., for what types and in which frames) the cooperation decision passes through beliefs. This research examines the differential effects of frame on cooperation and beliefs about others' cooperation among different types of decision-makers in a laboratory setting using an incentivized repeated, linear, public good experiment.

This paper is the third in a series of three papers, all of which are based on the same multi-part linear public good experiment. The experiment included a one-shot public good dilemma, a repeated public good dilemma, and a questionnaire. It was designed to assess and classify heterogeneity in behavior and motives among decision-makers (paper 1), examine how

framing effects differ for different types of decision-makers (paper 2), parse the role of reference dependence and the language in framing effects (paper 2), and examine the channel/mechanism through which frames operate and how those channels differ for different types of decision-makers (paper 3). In this paper, I address the last of these objectives. I analyze cooperation decisions and beliefs about others' decisions in the repeated public good dilemma only and incorporate decision-maker types and motives as assessed in the first paper.[78]

Public good experiments are simultaneous games with more than two players that intend to reproduce the essential non-excludable and non-rival attributes of pure public goods.[79] In the standard experiment with linear payoffs (π_i), every individual (i) decides how to distribute her personal endowment (w_i) between herself and the group. Allocations to the group (g_i) constitute "cooperation" and are increased by a positive factor, k, and then divided among n participants. The result is a marginal per capita return (MPCR) of $\gamma = k/n$. Linear payoffs are typically given by the following:

$$\pi_i = w_i - g_i + \gamma \sum_{j=1}^{n} g_j \quad (1)$$

Under the usual assumptions of rational choice theory, the dominant strategy is to allocate nothing to the group even though everyone is better off if everyone cooperates. This is clearly not what we observe in real world situations or in public good experiments where levels of cooperation vary considerably across individuals.[80]

Heterogeneity in cooperation among participants in public good experiments is well documented (Chaudhuri, 2011; Ledyard, 1995). Classifications of decision-makers typically attempt to match behavior to preconceived categories of decision-makers, efforts that ultimately underrepresent their respective study populations and/or do not predict cooperation in a repeated public good dilemma (Jackman, 2016a). In contrast, Jackman (2016a) employed a combination of group-based trajectory modeling and exploratory factor analysis to identify four differently motivated, behavior-based types of decision-makers among the

[78] These three papers make-up the three chapters of this dissertation. In this paper, papers 1 and 2 are cited as Jackman (2016a) and Jackman (2016b), respectively.
[79] Non-excludable means that everyone has access to the public good. Non-rival means that consumption by one individual does not affect others' consumption. Cooperation consists of providing or maintaining the public good.
[80] See Chauduri (2011) for a recent, selective survey of laboratory public good experiments and Ledyard (1995) for an older and exhaustive review of public good experiments.

participants in this study. Types included non-cooperators (NC, 19%, $N = 48$), conditional cooperators (CC, 51%, $N = 131$), low cooperators (LC, 23%, $N = 60$), and high cooperators (HC, 7%, $N = 17$). Jackman (2016b) demonstrated that those types predict cooperation in a repeated public good dilemma.[81]

Another robust finding of public good experiments is that decision-makers often respond differently to the same information based on how the decision is presented; this phenomenon is called a "framing effect."[82] Though the evidence is somewhat mixed, several patterns emerge for experimental frames: 1) contribution frames generally yield more cooperation than take frames (c.f., Bougherara, Denant-Boemont, & Masclet, 2011; Brewer & Kramer, 1986; Cubitt, Drouvelis, & Gächter, 2011; Dufwenberg, Gächter, & Hennig-Schmidt, 2011; Fleishman, 1988; Rutte, Wilke, &Messick, 1987), 2) avoiding losses motivates more action than pursuing gains (e.g., Levin, Gaeth, Schreiber, & Lauriola, 2002; Levin, Schneider, & Gaeth, 1998; Sonnemans, Schram, & Offerman, 1998), 3) positive frames yield more cooperation than negative frames (c.f., Andreoni, 1995; Cookson, 2000; Levin et al., 2002; Levin et al., 1998; Park 2000; Willinger & Ziegelmeyer, 1999), and 4) wording like "I" instead of "We" and labels like "Wall Street" and "Stock Market" instead of "Community" or no label at all yield different levels of cooperation (c.f., Cookson, 2000; Dufwenberg et al., 2011; Ellingsen, Johannesson, Mollerstrom, & Munkhammer, 2012; Liberman, Samuels, & Ross, 2004; Rege & Telle, 2004).[83]

Explanations for framing effects vary, but most invoke at least one of two related theories: 1) reference-dependence, loss aversion, and prospect theory[84] and 2) valence-based

[81] Jackman (2016b) analyzed cooperation in the same repeated public good dilemma, focusing specifically on cooperation, framing effects, and the difference in framing effects among different types of cooperators. This paper extends Jackman's (2016b) results to beliefs as well as examines the relationships between beliefs and cooperation and the circumstances under which those relationships differ.

[82] Frames are created by presenting the same information differently. However, frame is defined as both the "formulation to which decision makers are exposed" and "the interpretation that they construct for themselves" (Dufwenberg et. al,2011 citing Kahneman, 2000, p. xiv). Framing effects are observed when decision-makers subject to different frames respond differently. In this study, the phrases "frame" and "subjective frame" distinguish the frames imposed by researchers from the subjective frames that individuals construct for themselves.

[83] Ellingsen et al., (2012), Liberman et al. (2004), Rege & Telle (2004), and Ross & Ward (1996) were Prisoners' Dilemma experiments.

[84] According to prospect theory, decisions occur in two stages, framing and valuation, where framing in this context refers to how individuals decide on the "acts, contingencies, and outcomes that are relevant to the decision" (Kahneman& Tversky, 1979; Tversky & Kahneman, 1992). The valuation stage invokes the notions of

framing.[85] Despite the robustness and endurance of these theories, a persistent challenge for framing researchers is that decision processes, which include how individuals select reference points, how they measure gains and losses, and what they evaluate as positive versus negative, ultimately depend upon the perceptions of decision-makers. Without the ability to observe "subjective frame", results from a series of public good experiments suggest that beliefs about others' cooperation provide some insight into underlying decision processes and are an important intermediary in understanding framing effects.

For example, Jackman (2016a; 2016b) found that individuals who were sensitive to framing were also more motivated by others' actions and/or outcomes, which in a simultaneous dilemma like this one, almost certainly includes beliefs about others' decisions. In a one-shot public good dilemma, Dufwenberg et al. (2011) found a stronger association between beliefs and cooperation than between frames and cooperation, and concluded that frames influence cooperation through "a subtle interplay of framing, beliefs, and choice." Similarly, Sonnemans et al. (1998) attributed framing effects in repeated, step-level dilemmas to the dynamic environment in which beliefs about others were evolving over time in response to information about others' cooperation. In a similar vein, Willinger and Ziegelmeyer (1999) concluded that the framing effects emerged in response to different, context-dependent reactions to others' contributions. Those different "reactions" may have consisted largely of different beliefs that precipitated different choices. In fact, in the contribution-framed, repeated public good dilemma upon which this study was based, Fischbacher and Gätcher (2010) demonstrated that information about others' cooperation was an important factor in belief formation, and beliefs are important predictors of behavior. Via a Prisoners' Dilemma experiment, Ellingsen et al. (2012) described social frames as coordination devices, concluding that frames "enter people's beliefs rather than their preferences."

---

reference dependence and loss aversion, which together mean that "the carriers of value are gains and losses defined relative to a reference point" and "losses loom larger than gains" of the same magnitude (Tversky & Kahnman, 1991).

[85] Levin et al. (1998) and Levin et al. (2002) categorized valence-based framing effects into a three part typology consisting of attribute framing, goal framing, and risky choice framing each due to different underlying mechanisms. They ascribed attribute framing effects to associative processing in which positive associations yield more action and negative associations yield less action. They attributed goal framing effects to reference dependence and loss aversion and risky choice framing effects to prospect theory.

These results strongly suggest that framing passes through beliefs in decision processes where framing effects are observed. However, none of these studies provides a pathway analysis of that mechanism, and none addresses how those pathways differ among different types of decision makers. Through the combined use of simple non-parametric tests and recent advances in the marriage of moderation and mediation models, this study does.

This research 1) provides evidence that frames indirectly influenced cooperation through beliefs about others' cooperation, and 2) shows that this beliefs pathway was present for individuals previously assessed as conditional cooperators but absent for individuals previously assessed as non-cooperators. This work also 3) demonstrates framing effects in cooperation and in beliefs that differed across types of decision-makers; 4) shows that study participants cooperated somewhat less on average than their expectations of others, although this effect differed by type of decision-maker; and 5) reveals that individuals who were expected to cooperate very little or not at all provided the best estimates of others' cooperation, while more cooperative types tended to overestimate others' cooperation.

Methodologically, this research distinguishes itself from previous work in at least three ways. First, separately from the repeated public good dilemma, decision-makers were classified into four differently motivated, behavior-based types (Jackman, 2016a) that predict cooperation in the repeated public good dilemma (Jackman, 2016b). Second, the public good decision was presented in four different ways to four different sets of individuals according to a novel 2×2 factorial framing design that decomposes frame into the action implied by the location of the initial endowment and the language used to describe the action. Third, this study treated beliefs about others as an outcome variable in non-parametric tests and as an intermediary in moderation and mediation analyses (a.k.a. path analysis, conditional process analysis).

In the following, I describe the study participants, experimental design, and empirical methods. I present results, and conclude with a discussion of results and next steps.

2 Methods

This study employed non-parametric tests and moderation and mediation analyses. Results derive from cooperation and stated beliefs in the repeated public good dilemma, a core component of the full experiment illustrated in Figure 3.1. The frames examined in this study – give neutral (GN), give non-neutral (GNN), take neutral (TN), and take non-neutral (TNN) that – correspond to the 2×2 factorial framing design illustrated in Figure 3.2 and are described in depth in Section 2.3. Each study participant's decision-making type was assessed previously in Jackman (2016a) based on their choices in the one-shot public good dilemma that preceded the repeated dilemma. Types include non-cooperators who cooperated at trivial levels or not at all regardless of others' average cooperation; conditional cooperators who approximately matched others' average cooperation; low cooperators who cooperated at low but non-trivial levels regardless of others' average cooperation; and high cooperators who cooperated at high levels regardless of others' average cooperation.

When I report statistics, I use an alpha level of 0.05 for all statistical tests and describe results with *p*-values less than 0.05 as "statistically significant." I use the phrases "highly significant" for *p*-values less than 0.01, "weakly significant" for *p*-values between 0.05 and 0.1, and "marginally significant" for *p*-values close to but greater than 0.1. All analyses for this paper were generated using SAS® software.[86]

2.1 Study Participants

Experimental subjects were recruited from an existing pool of potential participants using the School of Information On-line Recruitment System for Experimental Economics (ORSEE) at the University of Michigan. Prospective participants received an email inviting them to participate in a "decision-making experiment" for which they would receive "$5 compensation for showing up" and "approximately $20 (including show-up compensation)" if they completed the experiment.

[86] Version 9.4 for Windows, Copyright © 2002-2012 by SAS Institute Inc. SAS and all other SAS Institute Inc. product or service names are registered trademarks or trademarks of SAS Institute Inc., Cary, NC, USA.

In total, 256 adults participated in the experiment via 16 different sessions consisting of 16 people per session. Each participant took part in only one session. As reported in Table 3.1, the average age of participants was 20.5 years ($N = 252, SD = 4.16$), 46 percent were women, 37 percent reported having no training in economics, and 38 percent of participants reported having no previous experience with "a study like this one."[87] Table 3.1 also reports the distribution of participants among four types of decision-makers. Per Jackman (2016a), 19 percent of participants in this study were non-cooperators who were not expected to cooperate in the repeated dilemma; 51 percent were conditional cooperators who were expected to match approximately their beliefs about others' cooperation; 23 percent were low cooperators who were expected to cooperate consistently at low levels; and 7 percent were high cooperators who were expected to cooperate more than they believed others would.

Four sessions were assigned to each of the four frames in advance, with the expectation that the individuals assigned to each frame would, on average, be similar with regard to age, gender, training in economics, prior experience with studies like this one, and type of decision-maker. As reported in Table 3.1, participants' age ($F(3, N = 252) = 1.04, p = 0.3745$), training in economics ($X^2\ (3, N = 251) = 0.68, p = 0.8784$), and cooperative type ($X^2\ (9, N = 256) = 11.29, p = 0.2566$) did not differ significantly across the four frames, but there was a marginal association between gender and frame ($X^2\ (3, N = 256) = 6.09, p = 0.1073$) and a highly significant association between experience and frame ($X^2\ (3, N = 245) = 13.35, p = 0.0039$). Subsequent analyses controlled for these differences. Specifically, regression analyses were robust across model specifications with and without demographic variables.

2.2 The Experiment

The full experiment illustrated in Figure 3.1 consisted of five components – instructions, practice questions, a one-shot public good dilemma, a repeated public good dilemma, and a questionnaire. A complete script and instructions for the experiment are given in the Appendix.

[87] These study participants were quite young; based on age, most were born in the late 1990's and only 8 were over the age of 25. Most had studied economics, and most had participated previously in a study like this one. While a microcosm of decision-makers in general, which is a challenge for many laboratory experiments, these results, at minimum, cast light onto a very interesting microcosm of educated, late generation millennials.

The experiment was conducted via 16 separate sessions. Every session included exactly 16 people, and each participant took part in only one session. Each of the four frames was assigned to a different set of four sessions. The experiment was programmed and conducted using the software z-Tree (Fischbacher, 2007) in the School of Information experimental laboratory at the University of Michigan.

Each session commenced when recruits arrived at the waiting room. The first 16 recruits to arrive participated in the session. Additional recruits were paid $5 for showing up and then were dismissed. Study participants were invited into the computer laboratory where each individual chose to sit at one of 16 desktop computers separated by partitions. Participants were provided with written, frame-specific instructions explaining the decision situation and incentives (see Appendix). Instructions clearly stated that "the experiment consist[ed] of a series of decision situations." Instructions were also read aloud. To further ensure participants understood the decision situation and before the experiment could proceed, all participants were required to answer ten practice questions correctly.

At this point, the one-shot public good dilemma proceeded in randomly assigned anonymous groups of four.[88] In the one-shot public good dilemma, participants encountered two formulations of the same decision situation, an unconditional decision in which participants did not know others' decisions and 21 decisions conditioned on the hypothetical average decisions of others. To conclude the one-shot dilemma, each participant was shown her decision, her earnings, and the average decision of others in her four-person group.

The repeated public good dilemma, the focus of this paper, proceeded in randomly re-matched, anonymous groups of four. It began with onscreen instructions that were also read aloud. Instructions advised participants that there would be 10 rounds in this stage of the experiment. Then participants were presented with a series of ten identical decision situations, called rounds. Cooperation decisions in each round were made simultaneously, without knowledge of the current decisions of other group members. Individuals also provided

[88] The one-shot dilemma was patterned after the "P-experiment" employed by Fischbacher and Gächter (2010) and Fischbacher, Gächter, and Fehr, (2001). In Fischbacher and Gächter (2010), the one-shot dilemma preceded repeated dilemma decisions for some subjects and followed repeated dilemma decisions for others. They found no statistically significant difference in cooperation in the repeated dilemma between subjects who encountered the one-shot dilemma before or after the repeated dilemma.

estimates for the average decision of the other three members of their 4-person group, called first order beliefs, and approximations of the average estimate of the three other members of their 4-person group, called second order beliefs.

Using the give neutral frame and *italics* to highlight the phrases that differ among frames, cooperation decisions were collected via the question, "How many points will you *allocate to the project* account?" To measure first order beliefs, individuals were asked, "How many points on average do you think others will *allocate to the project* account?" To measure second orders beliefs, individuals were asked, "On average, what do you think the other three members of your group will estimate for *others' average allocations to the project* account?" Therefore, first order beliefs reflect an individual's expectations of others and their estimate of others' cooperation. In this research, I explore the role of first order beliefs in decision processes, specifically the role of beliefs as an intermediary between frame and cooperation. Second order beliefs are conceptually quite different.[89] Specifically, second order beliefs reflect an individual's assessment of others' ability to accurately estimate others' behavior and are not relevant to the objectives of this study.[90]

Decisions were incentivized by the payoff function given in Equation (1), which is further discussed in Section 2.3. Providing correct estimates of others' decisions and of others' estimates was encouraged by awarding additional points for correctness. After each round, each individual's decision, points earned, average decision of other group members, and additional points based on the accuracy of his estimates were provided on the computer screen.

Following the repeated dilemma, participants then completed a questionnaire that measured reactions to the experiment and collected demographic information. At the end of the experiment, points were converted to dollars and paid to participants. On average,

[89] According to Wilcoxon signed rank sum tests, mean per person second order beliefs were statistically significantly greater than mean per person first order beliefs ($S = 4483, p < 0.0001$). Fixed effects regression also support the conclusion that first and second order beliefs were conceptually distinct; both variables were statistically significant as was their interaction.

[90] This notion of second order beliefs is quite different from Dufwenberg et al.'s (2012) notion of second order beliefs.

participants earned $15 from their decisions plus $5 for showing up. The experiment took approximately one hour.

2.3 Decision Situation and Framing

In every decision in the experiment, every participant was randomly assigned to an anonymous group of four individuals and given decision-making power over an initial endowment of 20 points. Every decision required each individual to choose how to split her endowment between herself and the group knowing that individuals kept everything they allocated to themselves, and group members benefited equally from allocations to the group. Consistent with other linear public good games, payoffs were given by Equation (1), repeated below:

$$\pi_i = w_i - g_i + \gamma \sum_{j=1}^{n} g_j \quad (1)$$

where π_i represents points earned by person i, and g_i represents person i's allocation to the group. In other words, g_i represents cooperation by person i. Each individual's initial endowment was $w_i = 20$, groups were of size $n = 4$, and the marginal per capita return (MPCR) was $\gamma = 0.4$ as in Fischbacher and Gächter (2010).

The decision situation in this study was presented in four different ways according to the 2×2 factorial framing design illustrated in Figure 3.2. This design distinguishes 1) the action implied by the location of the initial endowment from 2) the language used to describe the action. Neutral frames were nearly identical, differentiated only by the location of the initial endowment. The word "allocate" was used in both neutral frames to describe the action taken. "Allocate" was replaced with "give" and "take" to create the non-neutral frames. The result was four frames, give neutral (GN), give non-neutral (GNN), take neutral (TN), and take non-neutral (TNN). The following discussion of will make this more clear. [91]

The give treatment located the initial endowment with the individual (i.e., in "personal accounts"), and instructions stated the following: "To start, **your personal account contains 20 points**. Every individual has 20 points in their personal account that they can distribute

[91] There is no evidence, to my knowledge, that the word "allocate" is neutral relative to the words "give" and "take." In this context, neutral is shorthand for the uniformity of the language used in the neutral frames.

between their personal account and the project account in any way they like." In contrast, take treatments located the initial endowment with the group (i.e., in the "project account") and instructions stated the following: "To start, **the project account contains 80 points.** Every individual may distribute 20 of the project points between their personal account and the project account in any way they like." In neutral frames, "allocate to" was used to describe the action; "give to" and "take from" were used in non-neutral treatments. Therefore in the neutral treatment individuals were asked how many points they would "allocate" to the "project account" or to her or his "personal account". In contrast, in the non-neutral treatments, individuals were asked how many points they would "give" to the project account or "take" from the project account.

Payoffs were always calculated in accordance with Equation (1) regardless of frame. Equation (1) reflects the framing of give treatments, but payoffs were presented to participants via Equation (1a) in the give neutral frame and Equation (1b) in the give non-neutral frame.

$$\textit{Total points-income} = \textit{(20 – points you allocated to the project account)} + \textit{0.4} \times \textit{sum of all points allocated to the project account} \quad (1a)$$

$$\textit{Total points-income} = \textit{(20 – points you gave to the project account)} + \textit{0.4} \times \textit{sum of all points given to the project account} \quad (1b)$$

For each individual, the points she or he "allocated" or "gave" to the project account comprised her or his cooperation.

Payoffs in the take frames are the same as in Equation (1), but are more faithfully represented in Equation (2)

$$\pi_i = x_i + \gamma\left[\sum_{j=1}^{n} w_j - \sum_{j=1}^{n} x_j\right] \quad (2)$$

where π_i represents earnings for person i, and x_i represents points allocated by person i to her/his personal account. The same parameters apply: the initial endowment is given by $w_j = 20$, group size is $n = 4$, and marginal per capita return is $\gamma = 0.4$. Importantly, Equation (2) is equivalent to Equation (1) via the relationship, $x_i = w_i - g_i$ where g_i represents points allocated to the project account by person i (i.e., cooperation for person i). In the experiment,

payoffs were presented as in Equation (2a) in the take neutral frame and Equation (2b) in the take non-neutral frame.

$$\begin{matrix} \textit{Total} \\ \textit{points-income} \end{matrix} = \begin{matrix} \textit{points you allocated to your personal account +} \\ \textit{0.4 × (80 – sum of all points allocated to personal accounts)} \end{matrix} \quad (2a)$$

$$\begin{matrix} \textit{Total} \\ \textit{points-income} \end{matrix} = \begin{matrix} \textit{points you took from the project account +} \\ \textit{0.4 × (80 – sum of all points taken from the project account)} \end{matrix} \quad (2b)$$

For each individual, points not "allocated to personal accounts" or not "taken from the project account" make up her or his cooperation.

In all other regards, oral and written instructions were nearly identical in all frames. Following Dufwenberg et al.'s (2011) example, the externality generated via the public good was always presented positively but weakly. Specifically, all frames included the following statement: "You will earn points from both accounts in every decision situation. You are the only one who will earn points from your personal account. However, everyone will profit equally from the total amount in the project account." In addition, wording very strictly excluded any use of arguably equivalent terminology. For example, "keep" and "leave" were never used in place of "give" and "take" since "keep" and "leave" may have different behavioral consequences (van Dijk & Wilke, 2000).

2.4 Modeling and Analyses

In addition to descriptive statistics, modeling and analyses consisted of two components: nonparametric tests and moderation and mediation analyses. In the following, I first describe the data. Then I examine the objectives, advantages, and limitations of non-parametric tests. Third, I discuss moderation and mediation models and how they are combined to reveal the beliefs pathway for framing effects.

2.4.1 Data

Table 3.2 describes the structure of the data. The units of analysis were the 256 unique study participants ($N = 256$) nested within 16 clusters ($S = 16$) that correspond to experimental session. Each session was comprised of 16 individuals ($M_s = 16$). Each of the four different frames was applied to four sessions via the 2×2 factorial combinations of the experimental treatments, action (ACT_s) and language ($LANG_s$). Cooperation and first and second order beliefs were measured for each individual in all 10 rounds of the repeated public good dilemma, which proceeded in randomly re-matched groups of four (see Figure 3.1 and Section 2.2). For each individual, mean per person cooperation ($MEANCOOP_i$) was calculated by averaging cooperation over all ten rounds. Mean per person first order beliefs ($MEANFOB_i$) and mean per person second order beliefs ($MEANSOB_i$) were also calculated. Demographic variables for each individual (i) included one categorical variable for the four types of decision-makers ($\boldsymbol{TYPE}_i$), and three dichotomous variables for gender ($GENDER_i$), training in economics ($ECON_i$), and prior experience with a study like this one (EXP_i). Type was assessed in Jackman (2016a) using behavior recorded during the one-shot public good dilemma, and demographic information was collected in the questionnaire that bracketed the repeated public good dilemma in this experiment (see Figure 3.1).

2.4.2 Nonparametric Tests

Nonparametric tests were used to test for statistically significant differences in mean per person cooperation and beliefs between or among groups.[92] Specifically, Kruskal Wallis tests were used to compare the medians of non-normal dependent variables (i.e., mean per person cooperation and beliefs) for more than two categories of an independent variable (i.e., frame and type). Pairwise tests between frames and between types employed the Dwass, Steel, Critchlow-Fligner (DSCF) multiple comparison analysis (Critchlow & Fligner, 1991; Dwass, 1960; Steel, 1960), which is based on pairwise two-sample Wilcoxon comparisons. In general, the Kruskal Wallis test reduces to the Wilcoxon-Mann-Whitney test in the two-category case. All

[92] Non-parametric tests for mean per person cooperation and mean per person beliefs were consistent with results by round.

non-parametric tests were conducted using SAS software via the procedure NPAR1WAY with the WILCOXON and DSCF options.

Nonparametric tests are conceptually quite simple; and most important, they guided subsequent analyses and lend weight to the findings of the moderation and mediation analyses. Nonetheless, there are challenges that nonparametric analyses do not address. Kruskal Wallis tests do not estimate treatment effects, do not control for known differences in the demographics of treatment groups, do not control for within session correlation, and do not reveal the direct effects of frame on cooperation and indirect effects of frame on cooperation through beliefs. Moderation and mediation analyses overcome these challenges.

2.4.3 Moderation, Mediation, and Conditional Process Analyses

Moderation and mediation analyses are used to test hypotheses regarding the circumstances under which relationships between variables hold – moderation – and the mechanisms or pathways by which variables exert influence – mediation (Hayes, 2013, p. 3). Moderation is also known as interaction (Hayes, 2013, p. 8). Mediation analysis partitions the effects of independent variables on an outcome variable into indirect and direct effects and compares those effects to total effects. Conditional process analysis combines moderation and mediation to test hypotheses regarding the circumstances under which those mechanisms or pathways differ (Hayes, 2013, p. 327).[93]

In this paper, moderation, meditation, and conditional process analyses were implemented to test three relationships among action, language, type, mean per person cooperation, and mean per person first order beliefs.[94] Because first and second order beliefs ultimately proved to be so closely correlated (see Section 3) and results for mean per person

[93] There is an important distinction and some debate in the methodology literature regarding what comprises mediated moderation versus moderated mediation (Hayes, 2013, p. 9-10). These distinctions relate to appropriately matching the model type (i.e., moderated mediation versus mediated moderation) to research questions. However, in this case, the mathematical specifications are identical. As a result, I avoid jargon and therefore avoid the possible debate by describing interactions and direct, indirect, and total effects rather than labeling models that combine moderation and mediation analyses, as well as invoke the more general terminology, conditional process analysis.

[94] Results reported in this paper are consistent with results of linear unobserved effects panel data models (LUEPDMs). LUEPDMs include every measure of cooperation and first order beliefs for every individual as well as unobserved effects for sessions and individuals.

second order beliefs were similar to results for mean per person first order beliefs, the following model specifications include only mean per person first order beliefs.

First, I examined how language (i.e., non-neutral versus neutral) influenced mean per person cooperation and mean per person first order beliefs in give versus take treatments using the model illustrated in Figure 3.3. In the moderation model depicted in the top panel, the relationship between language and the dependent variable (i.e., cooperation or beliefs) is represented by the horizontal arrow. Moderation in this context means that the effect of language on the dependent variable depends upon action. Moderation is represented by the arrow pointing from action to the horizontal arrow. Statistically, moderation is simply an interaction. Thus, language moderated by action and action moderated by language are equivalent, and both formulations correspond to the statistical model given in the lower panel of Figure 3.3 and specified in Equation (3).

$$Y_{is} = i_{is} + c_1 ACT_s + c_2 LANG_s + c_3 ACT_s \times LANG_s + \varepsilon_{is} \quad (3)$$

Y_{is} represents mean per person cooperation ($MEANCOOP_{is}$) or mean per person first order beliefs ($MEANFOB_{is}$). The intercept and error term are represented by i_{is} and ε_{is}, respectively. Note that ACT_s and $LANG_s$ are indicator (i.e., dummy) variables for action and language respectively where give (i.e., $ACT_s = 0$) and neutral (i.e., $LANG_s = 0$) are the reference values (i.e., omitted variables). Therefore, regression coefficients on action and language represent an effect relative to the reference action (i.e., give) and language (i.e., neutral).

Second, I examined the effects of language moderated by action on mean per person cooperation through mean per person first order beliefs by partitioning the total effect, c, into indirect, ab, and direct, c', effects such that $c = c' + ab$. Thus, the indirect effect, ab, comprises the portion of the total effect arising from the pathway of influence from language onto beliefs, a, and from beliefs onto cooperation, b. The direct effect, c', comprises the portion of the total effect that is independent of the influence of beliefs on cooperation.

Indirect and direct effects are illustrated in the upper panel of Figure 3.4. The indirect effect, ab, consists of two components represented by the upward sloping arrow between language and first order beliefs and the downward sloping arrow between first order beliefs and cooperation. The direct effect, c', is represented by the horizontal arrow between language

and cooperation. The relationships between language and beliefs and between language and cooperation are both moderated by action, and depicted by the arrows originating from action.[95]

The indirect and direct effects are given by Equations (4) and (5).

$$MEANFOB_{is} = i_{is}^{M} + a_1 ACT_s + a_2 LANG_s + a_3 ACT_s \times LANG_s + \varepsilon_{is}^{M} \quad (4)$$

$$MEANCOOP_{is} = i_{is}^{Y} + c_1' ACT_s + c_2' LANG_s + c_3' ACT_s \times LANG_s + bMEANFOB_i + \varepsilon_{is}^{Y} \quad (5)$$

Total effects are given by equation (6),

$$MEANCOOP_{is} = i_{is}^{T} + c_1 ACT_s + c_2 LANG_s + c_3 ACT_s \times LANG_s + \varepsilon_{is}^{T} \quad (6)$$

Thus, the indirect effect, $a_2 b$, estimates the difference in cooperation between non-neutral and neutral language in the give treatment that results from the influence of language on beliefs, which in turn influences cooperation. It is equivalent to the estimated difference between cooperation in the give non-neutral and give neutral frames ($GNN - GN$). The direct effect, c_2', estimates the difference in cooperation between non-neutral and neutral language in the give treatment that is independent of the influence of beliefs on cooperation. The direct effect is the difference in cooperation between the give non-neutral and give neutral frames ($GNN - GN$) that is in no way attributable to the influence of beliefs on cooperation.

The third relationship examined explored if and how the beliefs pathway established via the above model differed for different types of decision makers. This model adds the categorical variable type ($\boldsymbol{TYPE}_i$) to the above model as an additional moderator as depicted in Figure 3.5 and given by Equations (7), (8), and (9).

$$\begin{aligned} MEANFOB_{is} = {} & i_{is}^{M} + a_1 ACT_s + a_2 LANG_s + a_3 ACT_s \times LANG_s + \\ & \boldsymbol{a_4 TYPE_i} + \boldsymbol{a_5} ACT_s \times \boldsymbol{TYPE_i} + \boldsymbol{a_6} LANG_s \times \boldsymbol{TYPE_i} + \\ & \boldsymbol{c_7'} ACT_s \times LANG_s \times \boldsymbol{TYPE_i} + \varepsilon_{is}^{M} \end{aligned} \quad (7)$$

$$\begin{aligned} MEANCOOP_{is} = {} & i_{is}^{Y} + c_1' ACT_s + c_2' LANG_s + c_3' ACT_s \times LANG_s + \\ & \boldsymbol{c_4' TYPE}_i + \boldsymbol{c_5'} ACT_s \times \boldsymbol{TYPE}_i + \boldsymbol{c_6'} LANG_s \times \boldsymbol{TYPE}_i + \\ & \boldsymbol{c_7'} ACT_s \times LANG_s \times \boldsymbol{TYPE}_i + \end{aligned} \quad (8)$$

[95] This model assumes a causal relationship from language through first order beliefs to cooperation ($LANG \rightarrow MEANFOB \rightarrow MEANCOOP$) that is consistent with the experimental nature of this study, the design of the decision situation in which individuals are asked to estimate other's decisions before submitting their own decisions, as well as the generally accepted theory of social preferences in which individuals include a valuation of others' actions or outcomes in their decision processes.

$$b_1 MEANFOB_i + \boldsymbol{b_2} MEANFOB_i \times \boldsymbol{TYPE}_i + \varepsilon_{is}^{Y}$$

Total effects are illustrated in the lower panel of Figure 3.5 and are given by the coefficients in Equation (9).

$$\begin{aligned} MEANCOOP_{is} = i_{is}^{T} + c_1 ACT_s + c_2 LANG_s + c_3 ACT_s \times LANG_s + \\ \boldsymbol{c_4 TYPE}_i + \boldsymbol{c_5} ACT_s \times \boldsymbol{TYPE}_i + \boldsymbol{c_6} LANG_s \times \boldsymbol{TYPE}_i + \\ \boldsymbol{c_7} ACT_s \times LANG_s \times \boldsymbol{TYPE}_i + \varepsilon_{is}^{T} \end{aligned} \quad (9)$$

Note that $\boldsymbol{TYPE}_i$ represents a vector of three indictor (i.e., dummy) variables for conditional, low, and high cooperators where non-cooperator is the reference type (i.e., omitted variable). Thus, $\boldsymbol{TYPE}_i$, the two- and three-way interactions with $\boldsymbol{TYPE}_i$, and their coefficients are vectors that I represent with bold type-face. Regression coefficients on $\boldsymbol{TYPE}_i$ and interactions with $\boldsymbol{TYPE}_i$ represent effects, or some part thereof, relative to the reference category or reference categories.[96]

These models were implemented as generalized estimating equations (GEE) clustered on session. Model estimation was conducted using SAS software and the procedure GENMOD with the REPEATED statement to calculate cluster-robust standard errors (CRSE).

3 Results

These approaches provide new insight into how frames differently influence cooperation and beliefs about others' cooperation, how those framing effects differ for different types of decision-makers, and under what conditions (i.e., for what types and in which frames) framing effects pass through beliefs. To begin, I report descriptive statistics and results of non-parametric tests for cooperation, first order beliefs, and second order beliefs. Because only cooperation and first order beliefs are relevant to the objectives of this research moderation and mediation analyses included only cooperation and first order beliefs.

[96] Interpretation of total, indirect, and direct effects is identical to the previous model, but requires attention to non-cooperator as the reference category for type. As with give and neutral reference values for action and language, using other reference values for type was investigated; those alternate specifications and results were equivalent. However, using non-cooperator as the reference category for type yielded the most illustrative results.

3.1 Descriptive Statistics

Figure 3.6 plots the average and median cooperation, first order beliefs, and second order beliefs for all 256 study participants. In general, cooperation and beliefs declined over time; on average, cooperation and beliefs were non-trivial; cooperation and first and second order beliefs track one another; and cooperation was less than beliefs in every round except the first. Results were similar by frame and by type as shown in Figures 3.7 and 3.8. In addition to plotting cooperation and beliefs, Figures 3.7 and 3.8 also include average cooperation for all 256 participants.

Consistent with Fischbacher and Gächter (2010), Figure 3.6 reveals that study participants were "imperfect conditional cooperators." Their cooperation appeared to be conditioned on but also somewhat less than their beliefs about others' cooperation. In fact, cooperation strongly correlated with first order beliefs in every round ($r(254) > 0.47, p < 0.0001$) and Wilcoxon signed rank sum tests showed that mean per person cooperation was statistically significantly less than mean per person first order beliefs ($S = 6249, p < 0.0001$); average (median) mean per person first order cooperation was 0.65 (1.6) points less than average (median) mean per person first order beliefs.

These results held for every frame, but not for every type. Among non-cooperators, mean per person cooperation was not strongly correlated with mean per person first order beliefs ($r(46) = 0.31, p = 0.0327$). The differences between mean per person cooperation and mean per person first order beliefs for non-cooperators was not only highly significant according to Wilcoxon signed rank sum tests ($S = 518, p < 0.0001$), the difference between beliefs and cooperation was much larger for non-cooperators than for all study participants, 2.68 points on average (3.20 points in the median). For high cooperators mean per person cooperation and mean per person first order beliefs were strongly and significantly correlated ($r(15) = 0.72, p = 0.0011$), but high cooperators cooperated more, not less, than they believed others would. Among high cooperators, mean per person cooperation was 2.16 points greater on average (2.9 points in the median) than mean per person first order beliefs.

Focusing on average first order beliefs as an estimate of others' average cooperation, consider how well study participants estimated others' behavior. As reported above, study

participants generally overestimated others' cooperation. However, comparing average estimates of others' behavior for each type of decision maker to actual average cooperation of all participants, shows that non-cooperators and low cooperators provided very good estimates on average, but conditional and high cooperators overestimated others' behavior. Actual average (median) mean per person cooperation was 3.98 (2.85) points. Average mean per person first order beliefs for non-cooperators and low cooperators were 3.81 (3.20) and 3.90 (3.30) points, respectively, and 5.23 (5.20) and 4.93 (5.10) points for conditional and high cooperators, respectively.

Table 3.3 condenses Figures 3.6, 3.7, and 3.8 into descriptive statistics for mean per person cooperation, mean per person first order beliefs, and mean per person second order beliefs, for all study participants across frames and for each type of decision-maker across frames. Overall, study participants' average mean per person cooperation was 3.98 points and median mean per person cooperation was 2.85 points. Mean per person beliefs were higher. Across frames, mean per person cooperation and mean per person beliefs were largest for individuals in the give neutral frame overall and for each type of decision-maker. Across cooperative types, high cooperators cooperated the most. Non-cooperators cooperated least. Mean per person beliefs were also highest for high cooperators, and lowest for non-cooperators, but the range of mean per person beliefs among types of decision-makers was smaller than the range of mean per person cooperation among types of decision-makers.

3.2 Non-parametric tests

Table 3.3 also shows the results of non-parametric tests among and between frames for all participants and within type. Kruskal Wallis tests for all participants showed statistically significant differences among frames in mean per person cooperation and mean per person first and second order beliefs. Frame appears to have influenced cooperation and beliefs. According to pairwise non-parametric tests, framing effects were driven by the give neutral frame, which almost always yielded the highest levels of cooperation and beliefs.

However, framing effects differed across types. Specifically, framing effects in mean per person cooperation were significant among conditional cooperators and low cooperators,[97] framing effects in mean per person first and second order beliefs were significant among conditional cooperators, and framing effects were significant in mean per person second order beliefs for low cooperators. Kruskal Wallis tests were not statistically significant for non-cooperators and high cooperators, not in mean person cooperation and not in mean per person beliefs. Where pairwise tests were significant, the give neutral frame often yielded the highest levels of cooperation and differed significantly from other frames in pairwise tests.

Focusing on trends, rather than statistical significance, the give neutral frame produced the highest levels of cooperation for every type *except non-cooperators*. The give neutral frame also produced the highest beliefs for every type of decision-maker, *without exception*. In other words, framing effects in mean per person cooperation appear to be present for conditional, low, and high cooperators. Framing effects in mean per person first and second order beliefs are evident for every type of decision-maker. The "failure" to detect these framing effects may be due to the small number of each type (i.e., 48 non-cooperators, 60 low cooperators, and 17 high cooperators) and not due to the absence of framing effects.

Among different types of decisions makers, non-cooperators distinguished themselves from the other types in mean per person cooperation but conditional cooperators distinguished themselves from other types in mean per person beliefs. Focusing on the left-most columns of Table 3.3 under the heading "All Frames", mean per person cooperation differed significantly among types of decision-makers ($\chi^2(3, N = 256) = 58.09, p < 0.0001$). In addition, pairwise tests showed that mean per person cooperation among non-cooperators was statistically significantly different from each of the other types.[98] Mean per person first order beliefs ($\chi^2(3, N = 256) = 17.46, p = 0.0006$) and mean per person second order beliefs ($\chi^2(3, N = 256) = 13.00, p = 0.0046$) also differed among all four types of decision-makers.

[97] Non-parametric tests results for mean per person cooperation are identical to results reported in Jackman (2016b) and consistent with the results of linear unobserved effects models also reported in Jackman (2016b).
[98] Mean per person cooperation also differed between low cooperators and high cooperators, but both low cooperators and high cooperators were not statistically significantly different from conditional cooperators.

In pairwise tests, mean per person beliefs among conditional cooperators differed significantly from non-cooperators and low cooperators, but not from high cooperators.

3.3 Moderation and Mediation

The descriptive statistics and results of non-parametric tests provide a road map for the moderation and mediation analyses that follow. First, framing effects were significant in mean per person cooperation and mean per person first order beliefs, suggesting strong relationships between frame (i.e. action, language, and their interaction) and cooperation and between frame and beliefs. Second, the give neutral frame drove the framing effects in mean per person cooperation and in mean per person beliefs and therefore implicates the interaction of action and language in producing framing effects. Third, first order beliefs and cooperation were strongly and significantly correlated.

Altogether, these results suggest that the effect of non-neutral language on cooperation and on beliefs differs in the give and take treatments, frame influences beliefs, and beliefs influence frame. In the language of moderation and mediation, action appears to moderate the effect of language on cooperation and on beliefs as illustrated in Figure 3.3, specified in Equation (3), and described in Section 3.3.1. Frame appears to indirectly influence cooperation through beliefs as illustrated in Figure 3.4, specified in Equations (4), (5), and (6), and described in Section 3.3.2.

Returning to descriptive statistics and non-parametric tests for motivation, recall that framing effects differed among types and for cooperation versus beliefs (see Table 3.3),[99] and beliefs and cooperation were strongly correlated for every type except non-cooperators. These results show that the effect of action, language and their interaction on beliefs and cooperation differed across types, and suggested that action and language exerted an effect on cooperation

[99] Framing effects in cooperation were clearly evident among conditional and low cooperators with give neutral as the most cooperative frame, and results were similar but not significant among high cooperators. Framing effects in beliefs were also clearly evident among conditional cooperators and weakly among low cooperators; for conditional and low cooperators beliefs were highest in the give neutral frame. Again, high cooperators demonstrated the same pattern of results, though effects were not statistically significant. Non-cooperators also appeared to form beliefs differently in different frames in much the same ways as other types of decision-makers. Among non-cooperators, beliefs in the give neutral frame were higher than in other frames.

through beliefs (i.e., mediation) for conditional cooperators, but not for non-cooperators (i.e., moderation). In other words, type moderates the indirect, direct, and total effects of frame on cooperation through beliefs, as illustrated in Figure 3.5, specified in Equations (7), (8), and (9), and described in Section 3.3.3.

3.3.1 Non-neutral language yielded lower levels of cooperation and first order beliefs in the give treatment

Table 3.4 reports the results of the moderation analysis illustrated in Figure 3.3 and tests the hypothesis that action moderates the effect of language on mean per person cooperation and on mean per person first order beliefs.[100] Neither the location of the initial endowment nor the use of the words "give" or "take" instead of "allocate" fully accounted for the differences in cooperation among different frames. How do we know? Model specifications that included the interaction of action and language were preferred, and the effect of non-neutral language on mean per person cooperation and mean per person first order beliefs was negative and statistically significant in the give frame only.

To interpret Table 3.4, first note that mean per person cooperation was the dependent variable in the first three model specifications (I, II, and III), and mean per person first order beliefs was the dependent variable in the last three model specifications (I, II, III). Second, specification III and VI were preferred according to the Quasilikelihood under the Independence model Criterion (QIC),[101] but specifications II and V, which exactly match Equation (3), yielded nearly identical results. Third, independent variables were all dichotomous and therefore

[100] The results in Table 3.4 were consistent 1) in magnitude but not statistical significance with ordinary least square models, 2) in magnitude and statistical significance with linear unobserved effects models that included unobserved random effects for each session, and 3) in statistical significance with linear unobserved effect panel data models that included repeated measures for each individual (i.e., cooperation and first order beliefs were the dependent variables, not the means), unobserved random effects for each individual, and unobserved random effects for each session.

[101] Since the generalized estimating equations (GEE) method used here is not a likelihood-based method, the Akaike's Information Criterion (AIC) statistic used for comparing models fit with likelihood-based methods is not available. Instead, model fit is given by the Quasilikelihood under the Independence model Criterion (QIC) statistic. QICu approximates QIC when the GEE model is correctly specified. QICu, defined as $Q + 2p$, adds a penalty ($2p$) to the quasilikelihood (Q), where p is the number of parameters in the model. When using QIC or QICu to compare two structures or two models, the model with the smaller statistic is preferred (SAS Institute, Inc., n.d.).

coefficients represent effects relative to references categories (i.e., give, neutral, man, no training, and no experience).

Understanding regression coefficients reported in Table 3.4 was simplified through mean differences also reported in Table 3.4. Statistically significant differences were found between the give neutral and give non-neutral frames for mean per person cooperation and mean per person first order beliefs, but not between other frames. Figure 3.9 illustrates these results for specifications II and V via the statistically significant slope of the give line for cooperation and beliefs. Figure 3.9 also makes clear the relationships between regression coefficients and mean differences. In particular, the slope of the give line corresponds to the coefficient on language and the mean difference between give non-neutral and give neutral.

3.3.2 Non-neutral language in the give treatment exerts an effect on mean per person cooperation through mean per person first order beliefs

Table 3.5 reports the results of the mediation analysis illustrated in Figure 3.4 and specified in Equations (4), (5), and(6) in which frame (i.e. action, language, and their interaction) influenced cooperation through beliefs.[102] Note that pairs of columns report estimates without demographic variables, as specified in Equations (4), (5), and (6), and with demographic variables. In every pair of results, the specification with demographic variables was preferred according to the QIC, but results were nearly identical without demographic variables. For simplicity, results focus on specifications without demographic variables. Figure 3.10 illustrates results for the give treatment, where statistically significant effects were found.

These results reveal a core finding of this paper. From a simple mediation analysis, non-neutral language in the give treatment appears to have indirectly influenced mean per person cooperation through mean per person first order beliefs. In other words, using the word "give" instead of "allocate" led to lower expectations of others' cooperation that then precipitated lower levels of cooperation. As can be seen in Table 3.5 and Figure 3.10, non-neutral language in the give treatment reduced beliefs ($a = -2.65, p = 0.0204$), and lower beliefs led to lower

[102] The results reported in Table 3.5 are consistent with linear unobserved effects panel data models that include every measure of cooperation and first order beliefs for every individual as well as unobserved effects for sessions and individuals.

levels of cooperation ($b = 1.11, p < 0.0001$) yielding the indirect effect ($ab = -2.94, p = 0.3321$).[103] Though the indirect effect is not statistically significant, results reveal that the statistically significant and negative total effect of language on mean per person cooperation in the give treatment ($c = -2.30, p = 0.0317$) vanished when mean per person first order beliefs was introduced as an intermediary ($c' = 0.65, p = 0.1597$).[104] In other words, there was no evidence of a direct effect of language on mean per person cooperation in the give treatment independent of the effect of language on mean per person first order beliefs in the give treatment.[105]

In detail, the first two columns of results in Table 3.5 correspond to Equation (5) and are identical to estimates in Table 3.4, but here they serve a different purpose. Here, they are components of the indirect effects (i.e., $a_1 = -2.15^{*}$, $a_2 = -2.65^{**}$, and $a_3 = 2.97$) that quantify the relationships between frame (i.e., action, language, and their interaction) and mean per person first order beliefs. Recall that because variables are dichotomous, the statistically significant coefficient on language shows that non-neutral language yielded mean per person beliefs that were 2.65 points lower than neutral language in the give treatment. This difference corresponds to the estimate of mean differences reported at the bottom of Table 3.5 ($GNN - GN = -2.65^{**}$),[106] and corresponds to the upward sloping arrow in Figure 3.10 between "Non-Neutral" and "First Order Beliefs."

---

[103] The p-value reported here employs the Sobel test. The Sobel test – also known as the product of coefficients approach, delta method, and normal theory approach – is conservative. The standard error of the product, ab, is given by $se_{ab} = \sqrt{a^2 se_b^2 + b^2 se_b^2}$ and test statistic, $Z = \frac{ab}{se_{ab}}$ (Hayes, 2013, pp. 102-105).

[104] Recall that total (c), direct (c'), and indirect (ab) effects are related as follows: $c = c' + ab$. The indirect effect is ab; a and b are components of the indirect effect.

[105] The statistical significance of a and b are not especially relevant; statistical significance of ab typically is (Hayes, 2013, p.201). Nevertheless, the marked different in sign and statistical significance in the relationship between frame and cooperation when the mechanism through beliefs *is not* taken into account (i.e., total effects) versus when the mechanism through beliefs *is* take into account (direct effects), led me to conclude in the absence of a statistically significant indirect effect, that non-neutral language in the give treatment influenced cooperation through beliefs.

[106] Note that mean differences also show that mean per person first order beliefs were lower in the take neutral frame were than in the give neutral frame, but only weakly ($TN - GN = -2.15^{*}$). In other words, in the neutral language treatment, taking yielded lower beliefs than giving. In general, $GNN - GN$ corresponds to the effect of the non-neutral language versus neutral language on the dependent variable in the give treatment. $TN - GN$ corresponds to the effect of taking versus giving in the neutral treatment. $TNN - GNN$ corresponds to the effect of taking versus giving in the non-neutral treatment. $TNN - TN$ corresponds to the effect of non-neutral language versus neutral language in the take treatment.

The last two columns of results in Table 3.5 correspond to Equation (6) and are identical to specifications II and III in Table 3.4. Again, they serve a different purpose here. Here these results represent the total effects of language on mean per person cooperation (i.e., $c_1 = 1.95$, $c_2 = -2.30^{**}$, and $c_3 = 2.43$) where the only statistically significant difference occurred between the give non-neutral and give neutral frames ($GNN - GN = -2.30^{**}$). The total effects in Figure 3.10 are labelled accordingly.

Finally, the third and fourth columns of Table 3.5 correspond to Equation (5). These results report the direct effects ($c_1' = 0.46$, $c_2' = 0.65$, and $c_3' = -0.88^{*}$) of language moderated by action on mean per person cooperation and the second component of the indirect effect ($b = 1.11^{***}$). Mean differences show no statistically significant differences between any frames. Figure 3.10 shows and labels these findings for the give treatment.

3.3.3 Mediation differs across types of decision-makers

Tables 3.6 and 3.7 report indirect, direct, and total effects for the model depicted in Figure 3.5, and specified with Equations (8), (9), and (10). Results for conditional cooperators are depicted in Figure 3.11.[107] These results reveal the final contribution of this paper. Namely, non-neutral language in the give treatment appears to have exerted an effect on mean per person cooperation through mean per person first order beliefs for conditional cooperators.

Focusing on conditional cooperators in the give treatment, refer to Figure 3.11 and Table 3.8 which condenses relevant results from Tables 3.5, 3.6, and 3.7. Among conditional cooperators non-neutral language reduced beliefs ($a = -3.09, p = 0.0023$), and lower beliefs led to lower levels of cooperation ($b = 1.27, p < 0.0001$) yielding the indirect effect ($ab = -3.94$, $p = 0.2077$). Though the indirect effect is not statistically significant, Table 3.8 and Figure 3.11 show that the statistically significant and negative effect of language on mean per person cooperation ($c = -2.41, p = 0.0294$), that was evident in the absence of the beliefs pathway changed sign when mean per person first order beliefs was introduced as an intermediary ($c' = 1.51, p = 0.0132$). In other words, even though the total effect of non-

[107] The results reported in Tables 3.6 and 3.7 are consistent with linear unobserved effects panel data models that include every measure of cooperation and first order beliefs for every individual as well as unobserved effects for sessions and individuals.

neutral language on cooperation was negative among conditional cooperators, the direct effect of language on cooperation when the beliefs pathway was taken into account was positive. Among conditional cooperators, using the word "give" instead of "allocate," increased cooperation when the beliefs pathway was taken into account. But, using the word "give" instead of "allocate" substantially lowered estimates of others' cooperation and subsequently their own level of cooperation, yield a total effect that was negative.

Among non-cooperators, indirect, direct, and total effects were not statistically significant, and there was no evidence of a beliefs pathway for non-cooperators. Among low and high cooperators, however, evidence was mixed. Indirect effects were not significant, and direct and total effects were significant, negative, and of similar magnitude.[108]

4 Conclusion

A persistent challenge for framing researchers is that decision processes depend upon the perceptions of decision-makers. Previous studies suggest that beliefs about others' cooperation provide insight into underlying decision processes and are an important intermediary in understanding framing effects (Dufwenberg et al., 2011; Ellingsen et al. 2012; Fischbacher & Gächter, 2010; Sonnemans et al., 1998; Willinger & Ziegelmeyer, 1999). None of these studies, however, tested for a beliefs pathway between frame and cooperation. The primary contribution of this research is that frames influenced cooperation through beliefs. Specifically, using the word "give" instead of "allocate" when the initial endowment was located in personal accounts, lowered expectations for others' cooperation that in turn led to lower levels of cooperation. Furthermore, when analyzed by type of decision-maker, the beliefs pathway was evident among individuals previously assessed as conditional cooperators, but not for individuals previously assessed as non-cooperators. Results were mixed for other types of decision-makers.

This research also demonstrated framing effects in cooperation and in beliefs that differed across types of decision-makers; mean per person cooperation differed significantly

[108] There is evidence that taking exerts influence on mean per person cooperation in the non-neutral treatment through mean per person first order beliefs among low cooperators and among high cooperators ($TNN - GNN$).

among frames for conditional and low cooperators, and mean per person first order beliefs differed significantly among frames only for conditional cooperators. However, the give neutral frame yielded the highest levels of cooperation for every type except non-cooperators and highest expectations of others' cooperation for every type, without exception. Jackman (2016b) attributed framing effects to a combination of reference dependence, where locating the initial endowment in personal accounts yielded higher levels of cooperation that was then diminished by the surprisingly negative valence of the word "give" relative to the word "allocate." Mediation analysis in this paper may offer additional insight into this counter-intuitive result. When the initial endowment was located in personal accounts, the total effect of the word "give" was negative and significant, but the effect when the beliefs pathway was taken into account was positive and significant. Perhaps the word "give" does evoke positive associations,[109] and a positive direct effect on cooperation, but individuals do not ascribe those positive associations and outcomes to others.

In addition, this study showed that overall study participants cooperated somewhat less than they expected others to cooperate. This result is consistent with Fischbacher and Gächter's (2010) finding that on average people are "imperfect conditional cooperators;" they cooperate somewhat less than they expect others to cooperate. This study extends those results. In this study, conditional and low cooperators were "imperfect conditional cooperators" and cooperated slightly less than they expected others to cooperate. High cooperators cooperated more than they expected others to cooperate, and non-cooperators cooperated very little or not at all even though their estimates of others' cooperation were well above zero.

In fact, individuals who were expected to cooperate at low levels or not at all, provided the best estimates of others' cooperation, while more cooperative types tended to overestimate others' cooperation. This is consistent with Orbell and Dawes (1991), as cited by Ledyard (1995), who reported that cooperators expect more cooperation than defectors. Explanations dating back to the 1970's relate to the idea that people believe others are like themselves and/or overemphasize similarities between themselves and others, such as

[109] This is an example of attribute framing per Levin et al. (1998).

cognitive misers (Orbell & Dawes, 1991), false consensus (Ross, Greene, & House, 1977), and egocentrism. However, the dramatic difference and low correlation between cooperation and beliefs among non-cooperators in this research suggest that non-cooperators know that other decision-makers are not like themselves.

Furthermore, this study revealed a beliefs pathway between frame and cooperation that was clearly evident among conditional cooperators and not among non-cooperators. The question of how beliefs are formed, how they evolve over time, and how information received over time influences beliefs and cooperation remain open questions. While it has long been suggested that beliefs are a product/result of prior beliefs and information about others, such as a weighted average,[110] how frames differently influence the evolution of beliefs and/or how beliefs differently evolve over time for different types of decision-makers is a fruitful and unexplored avenue of research that follows from this research.

Finally and revisiting the relevance of this research to policy innovation, note that conditional cooperators make up the majority of decision-makers here and in other experiments (Chaudhuri, 2011). Based on these findings, frame-based policies intended to inform or raise expectations for cooperation among other participants could motivate a very large number of decision-makers. This research also suggests that such efforts could influence other cooperators as well. Thus, more research into how frames can raise expectations of others' cooperation is clearly warranted. However, narrow policy inceptions of this and related research could ignore a large minority of decision-makers that includes self-interested, competitive types, altruistic types, and individuals with a buy-in mentality, all of whom might cooperate more given appropriately designed and complementary incentives. This research also showed that the language dimension of frame passed through beliefs when individuals were giving, not when they were taking; and the effect of giving versus taking did not pass through beliefs. Thus, the beliefs pathway of valence-based framing almost certainly is not the only viable pathway for leveraging framing in policy design.

[110] Ledyard (1995) noted that this idea dates back to Dawes, McTavish, and Shaklee (1977) who studied a commons dilemma situation.

References

Allcott, H. (2011). Social norms and energy conservation. *Journal of Public Economics, 95*(9-10), 1082-1095.

Andreoni, J. (1995). Warm-glow versus cold-prickle: The effects of positive and negative framing on cooperation in experiments. *The Quarterly Journal of Economics, 110*(1), 1-21.

Bougherara, D., Denant-Boemont, L., & Masclet, D. (2011). Cooperation and framing effects in provision point mechanisms: Experimental evidence. *Ecological Economics, 70*, 1200-1210.

Brewer, M. B., & Kramer, R. M. (1986). Choice behavior in social dilemmas: Effects of social identity, group size, and decision framing. *Journal of Personality and Social Psychology, 50*(3), 543-549.

Chaudhuri, A. (2011). Sustaining cooperation in laboratory public goods experiments: A selective survey of the literature. *Experimental Economics, 14*, 47-83.

Cookson, R. (2000). Framing effects in public goods experiments. *Experimental Economics, 3*, 55-79.

Critchlow, D. E., & Fligner, M. A. (1991). On distribution-free multiple comparisons in the one-way analysis of variance. *Communications in Statistics-Theory and Methods, 20*, 127-139.

Cubitt, R. P., Drouvelis, M., & Gachter, S. (2011). Framing and free riding: Emotional responses and punishment in social dilemma games. *Experimental Economics, 14*(2), 254-272. doi: 10.1007/s10683-010-9266-0

Dawes,R,M., McTavish, J., Shaklee, H. (1977). Behavior, communication and assumptions about other peoples' behavior in a commons dilemma situation. Journal of Personality and Social Psychology, 35, 1-11.

Dufwenberg, M., Gächter, S., & Hennig-Schmidt, H. (2011). The framing of games and the psychology of play. *Games and Economic Behavior, 73*(2), 459-478.

Dwass, M. (1960). Some k-sample rank-order tests. *Contributions to Probability and Statistics* (pp. 198-202). Stanford, California: Stanford University Press.

Ellingsen, T., Johannesson, M., Mollerstrom, J., & Munkhammar, S. (2012). Social framing effects: Preferences or beliefs? *Games and Economic Behavior, 76*, 117-130.

Fischbacher, U. (2007). z-Tree: Zurich toolbox fo ready-made economic experiments. *Experimental Economics, 10*(2), 171-178.

Fischbacher, U., & Gächter, S. (2010). Social preferences, beliefs, and the dynamics of free riding in public goods experiments. *American Economic Review, 100*(1), 541-556.

Fischbacher, U., Gächter, S., & Fehr, E. (2001). Are people conditionally cooperative? Evidence from a public goods experiment. *Economics Letters, 71*(3), 397-404.

Fleishman, J. A. (1988). The effects of decision framing and others' behavior on cooperation in a social dilemma. *The Journal of Conflict Resolution, 32*(1), 162-180.

Hayes, A. F. (2013). *Introduction to mediation, moderation, and condtional process analysis: A regression-based approach*. New York, NY: The Guilford Press.

Jackman, D. K. (2016a). *Heterogeneous cooperation and motivation in a public good dilemma*. Ph.D. Dissertation Chapter. School of Natural Resources and Environment. University of Michigan. Ann Arbor, MI.

Jackman, D. K. (2016b). *Heterogeneous framing effects in a repeated public good dilemma*. Ph.D. Dissertation Chapter. School of Natural Resources and Environment. University of Michigan. Ann Arbor, MI.

Jacobsen, G. D., Kotchen, M. J., & Vandenbergh, M. P. (2012). The behavioral response to voluntary provision of an environmental public good: Evidence from residential electricity demand. *European Economic Review, 56*, 946-960.

Jones, B. L., & Nagin, D. S. (2007). Advances in group-based trajectory modeling and an SAS procedure for estimating them. *Sociological Methods and Research, 35*(4), 542-571.

Jones, B. L., Nagin, D. S., & Roeder, K. (2001). A SAS procedure based on mixture models for estimating developmental trajectories. *Sociological Methods and Research, 29*(3), 374-393.

Kahneman, D. (2000). Preface. In D. Kahneman & A. Tversky (Eds.), *Choices, Values, and Frames*. Cambridge: Cambridge University Press.

Kahneman, D., & Tversky, A. (1979). Prospect theory: Analysis of decision under risk. *Econometrica, 47*(2), 263-291.

Ledyard, J. (1995). Public goods: A survey of experimental research. In A. E. Roth & J. H. Kagel (Eds.), *The Handbook of Experimental Economics* (pp. 111-194). Princeton, NJ: Princeton University Press.

Levin, I. P., Gaeth, G. J., Schreiber, J., & Lauriola, M. (2002). A new look at framing effects: Distribution of effect sizes, individual differences, and independence of types of effects. *Organizational Behavior and Human Decision Processes, 88*(1), 411-429. doi: 10.1006/obhd.2001.2983

Levin, I. P., Schneider, S. L., & Gaeth, G. J. (1998). All frames are not created equal: A typology and critical analysis of framing effects. *Organizational Behavior and Human Decision Processes, 76*(2), 149-188.

Liberman, V., Samuels, S. M., & Ross, L. (2004). The name of the game: predictive power of reputations vs. situational labels in determining Prisoners' Dilemma game moves. *Personality and Social Psychology Bulletin, 30*, 1175-1185.

Nagin, D. S. (1999). Analyzing developmental trajectories: A semiparametric, group-based approach. *Pyschological Methods, 4*(2), 139-157.

Nagin, D. S. (2005). *Group-based modeling of development*. Cambridge, MA USA: Harvard University Press.

Orbell, J., & Dawes, R. M. (1991). A “cognitive miser” theory of cooperators advantage. *American Political Science Review, 85*(2), 515-528

Park, E.-S. (2000). Warm-glow versus cold prickle: A further experimental study of framing effects on free-riding. *Journal of Economic Behavior & Organization, 43*, 405-421.

Rege, M., & Telle, K. (2004). The impact of social approval and framing on cooperation in public good situations. *Journal of Public Economics, 89*(7-8), 1625-1644.

Ross, L., Greene, D., House, P. (1977). The "false consensus effect": An egocentric bias in social perception and sttribution processes. *Journal of Experimental and Social Psychology, 13*, 279-301.

Rutte, C. G., Wilke, H. A. M., & Messick, D. M. (1987). The effects of framing social dilemmas as give-some or take-some games. *British Journal of Social Psychology, 26*, 103-108.

SAS Institute, Inc. (n.d.). 26100 - QIC goodness of fit statistic for GEE models. Retrieved October 09, 2016 from http://support.sas.com/kb/100.htm.

Sonnemans, J., Schram, A., & Offerman, T. (1998). Public good provision and public bad prevention: The effect of framing. *Journal of Economic Behavior & Organization, 34*, 143-161.

Steel, R. G. D. (1960). A rank sum test for comparing all pairs of treatments. *Technometrics, 2*, 197-207.

Tversky, A., & Kahneman, D. (1991). Loss aversion in riskless choice - A reference dependent model. *Quarterly Journal of Economics, 106*(4), 1039-1061. doi: 10.2307/2937956

Tversky, A., & Kahneman, D. (1992). Advances in prospect theory: Cumulative representation of uncertainty. *Journal of Risk and Uncertainty, 5*, 297-323.

van Dijk, E., & Wilke, H. (2000). Decision-induced focusing in social dilemmas: Give-some, keep-some, take-some, and leave-some dilemmas. *Journal of Personality and Social Psychology, 78*(1), 92-104.

Willinger, M., & Ziegelmeyer, A. (1999). Framing and cooperation in public good games: An experiment with an interior solution. *Economic Letters, 65*, 323-328.

Tables

Table 3.1: Participant characteristics

			Frames				ANOVA			
			GN	GNN	TN	TNN	df	F	p	
Age (in years)							3	1.04	0.3745	
Number	252		61	64	63	64				
Mean	20.54		20.15	20.23	21.44	21.31				
Standard Deviation	4.16		1.68	3.47	2.99	6.68				
			Frames				Chi Square Test			
	N	%	GN	GNN	TN	TNN	df	X^2	p	
Gender							3	6.09	0.1072	
Men	117	46%	25	37	30	25				
Women	139	54%	39	27	34	39				
Training										
No	94	37%	26	22	22	24	3	0.68	0.8784	
Yes	157	63%	37	40	41	39				
Don't know (excluded)	5	--	1	2	1	1				
Experience							3	13.35	0.0039	***
No	94	38%	34	15	21	24				
Yes	151	62%	26	44	42	39				
Don't know (excluded)	11	--	4	5	1	1				

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: NC = non-cooperator. CC = conditional cooperator, LC = low cooperator, and HC = cooperator.
Note 3: Age was not reported for 3 individuals, and one age was misreported (age = 92) and excluded. Most participants were in their late teens (18 or 19) and early twenties. Eight were over 25.

Table 3.2: Data Structure

Level of Data		**Variables**	
Level 2:	Cluster of Units	Cluster ID:	session (s)
	($S = 16$)	Covariates:	action (ACT_s)
	($M_s = 16$)		language ($LANG_s$)
Level 1:	Unit of Analysis	Unit ID:	individual (i)
	($N = 256$)	Covariates:	type ($TYPE_i$)
			gender ($GENDER_i$)
			training ($ECON_i$)
			experience (EXP_i)
		Continuous Variables:	mean cooperation ($MEANCOOP_i$)
			mean first order beliefs ($MEANFOB_i$)
			mean second order beliefs ($MEANSOB_i$)

Note 1: S is the number of clusters/sessions, indexed by s. M_s is the number of units/individuals in every cluster. N is the number of units/individuals in the study, indexed by i. Mean cooperation and mean first and second order beliefs are per person averages over the ten rounds of the repeated public good dilemma.

Table 3.3: Descriptive statistics for mean per person cooperation, first order beliefs, and second order beliefs by frame and type with non-parametric tests for framing effects

	All			Frame											Kruskal Wallis (df = 3)	
				GN			GNN			TN			TNN				
	N	Mean	Median	N	Mean	Median	N	Mean	Median	N	Mean	Median	N	Mean	Median	X^2	p
All (N =256)																	
Mean Cooperation	256	3.98	2.85	64	5.49	4.95 [a]	64	3.19	2.05 [b]	64	3.55	2.20 [b]	64	3.68	3.30 [b]	13.08	0.0045 ***
Mean FOB	256	4.63	4.45	64	6.29	6.80 [a]	64	3.64	3.30 [b]	64	4.14	3.55 [b]	64	4.46	4.60 [b]	38.62	<.0001 ***
Mean SOB	256	4.82	4.60	64	6.58	6.90 [a]	64	4.03	3.75 [b]	64	4.23	3.90 [b]	64	4.45	4.25 [b]	44.24	<.0001 ***
Type (N =256)																	
Non-Cooperators																	
Mean Cooperation	48	1.13	0.00	9	0.66	0.00 [a]	11	0.86	0.00 [a]	14	1.06	0.40 [a]	14	1.72	0.65 [a]	1.88	0.5976
Mean FOB	48	3.81	3.20	9	4.92	5.40 [a]	11	2.74	3.00 [a]	14	3.85	3.30 [a]	14	3.89	3.60 [a]	5.20	0.1575
Mean SOB	48	4.26	3.75	9	5.66	6.40 [a]	11	4.00	3.80 [a]	14	4.14	3.65 [a]	14	3.69	3.30 [a]	4.09	0.2524
Conditional Cooperators																	
Mean Cooperation	131	4.97	4.20	40	6.47	5.20 [a]	37	4.06	2.90 [b]	28	4.83	4.05 [a.b]	26	4.13	4.40 [a.b]	8.22	0.0416 **
Mean FOB	131	5.23	5.20	40	6.91	7.20 [a]	37	3.82	3.70 [b]	28	4.60	4.55 [b,c]	26	5.35	5.90 [a,c]	30.94	<.0001 ***
Mean SOB	131	5.34	5.40	40	7.06	7.10 [a]	37	4.01	3.80 [b]	28	4.60	4.70 [b]	26	5.37	5.85 [a,b]	35.13	<.0001 ***
Low Cooperators																	
Mean Cooperation	60	3.20	2.25	13	5.18	5.40 [a]	14	2.45	1.80 [b]	16	2.61	2.00 [b]	17	2.85	1.80 [a,b]	10.49	0.0148 **
Mean FOB	60	3.90	3.30	13	5.38	6.40 [a]	14	3.43	3.20 [a]	16	3.84	3.10 [a]	17	3.19	2.80 [a]	6.84	0.0773 *
Mean SOB	60	4.13	3.65	13	5.78	6.60 [a]	14	3.67	3.35 [b]	16	3.98	3.35 [a,b]	17	3.38	3.30 [b]	10.12	0.0176 **
High Cooperators																	
Mean Cooperation	17	7.09	8.00	2	9.75	9.75 [a]	2	5.15	5.15 [a]	6	5.87	6.10 [a]	7	7.94	10.00 [a]	2.29	0.5141
Mean FOB	17	4.93	5.10	2	6.00	6.00 [a]	2	6.75	6.75 [a]	6	3.42	3.15 [a]	7	5.40	5.30 [a]	5.00	0.1715
Mean SOB	17	4.90	4.30	2	6.20	6.20 [a]	2	7.25	7.25 [a]	6	3.40	3.05 [a]	7	5.14	4.60 [a]	5.26	0.1537

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.

Note 2: The results of pairwise nonparametric tests between frames are summarized by the superscripts appearing to the right of each median. Within a row, when two frames share superscripts, we cannot reject the hypothesis that the medians are equal. Within a row, when two frames do not share a superscript, the difference between medians is statistically significant at the 0.05 level. These pairwise tests employed the Dwass, Steel, Critchlow-Fligner (DSCF) multiple comparison analysis, which is based on pairwise two-sample Wilcoxon comparisons.

Table 3.4: Action moderates language

Dependent Variable:	Mean Per Person Cooperation						Mean Per Person First Order Beliefs					
	I		II		III		IV		V		VI	
	Est.	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*
Estimates												
Intercept	4.88	(0.83) ***	5.49	(0.94) ***	6.03	(1.21) ***	5.55	(0.92) ***	6.29	(1.05) ***	6.54	(1.18) ***
Action (reference = Give)	-0.73	(0.88)	-1.95	(1.21)	-1.73	(1.21)	-0.67	(0.94)	-2.15	(1.29) *	-2.18	(1.31) *
Language (reference = Neutral)	-1.09	(0.88)	-2.30	(1.07) **	-2.47	(1.08) **	-1.16	(0.94)	-2.65	(1.14) **	-2.68	(1.17) **
Action X Language			2.43	(1.66)	2.62	(1.65)			2.97	(1.73) *	3.01	(1.74) *
Gender (reference = Man)					-0.25	(0.36)					-0.14	(0.16)
Economics (reference = No Training)					-1.06	(0.65)					-0.42	(0.43)
Experience (reference = No Experience)					0.25	(0.65)					0.15	(0.23)
Observations	256		256		241		256		256		241	
QIC/QICu	272/259		275/260		264/248		315/259		329/260		313/248	
Mean Differences												
GNN-GN	-1.09	(0.88)	-2.30	(1.07) **	-2.47	(1.08) **	-1.16	(0.94)	-2.65	(1.14) **	-2.68	(1.17) **
TN-GN	-0.73	(0.88)	-1.95	(1.21) *	-1.73	(1.21)	-0.67	(0.94)	-2.15	(1.29) *	-2.18	(1.31) *
TNN-GNN	-0.73	(0.88)	0.49	(1.14)	0.88	(1.13)	-0.67	(0.94)	0.82	(1.15)	0.83	(1.14)
TNN-TN	-1.09	(0.88)	0.13	(1.27)	0.15	(1.27)	-1.16	(0.94)	0.33	(1.30)	0.33	(1.27)

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: The dependent variable in specifications I, II, and III was mean per person cooperation. The dependent variable in specifications IV, V, and VI was mean per person first order beliefs. Results when mean per person second order beliefs is the dependent variable were consistent with first order beliefs.
Note 3: Standard errors are cluster robust (CRSE), clustered on session.
Note 4: Based on the QIC and the QICu, specifications that control for demographic effects (specifications III and VI) were most preferred.
Note 5: Because the independent variables are all dichotomous, coefficients represents effects relative to references categories (i.e., give, neutral, man, no training, and no experience).
Note 6: $GNN - GN$ represents the effects of the non-neutral language versus neutral language in the give treatment. $TN - GN$ represents the effects of taking versus giving in the neutral treatment. $TNN - GNN$ represents the effect of taking versus giving in the non-neutral treatment. $TNN - TN$ represents the effects of non-neutral language versus neutral language in the take treatment

Table 3.5: Mediation by beliefs

	Direct and Indirect Effects								Total Effects			
	$MEANFOB_i$				$MEANCOOP_i$				$MEANCOOP_i$			
	Eqn. (4)				Eqn (5)				Eqn. (6)			
	w/o demographics		w/ demographics		w/o demographics		w/ demographics		w/o demographics		w/ demographics	
	Est.	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*
Estimates												
Intercept	6.29	(1.05) ***	6.54	(1.18) ***	-1.51	(0.72) **	-1.13	(0.76)	5.49	(0.94) ***	6.03	(1.21) ***
Action (reference = Give)	-2.15	(1.29) *	-2.18	(1.31) *	0.46	(0.43)	0.66	(0.39) *	-1.95	(1.21)	-1.73	(1.21)
Language (reference = Neutral)	-2.65	(1.14) **	-2.68	(1.17) **	0.65	(0.46)	0.47	(0.46)	-2.30	(1.07) **	-2.47	(1.08) **
Action X Language	2.97	(1.73) *	3.01	(1.74) *	-0.88	(0.54) *	-0.68	(0.52)	2.43	(1.66)	2.62	(1.65)
Mean Per Person First Order Beliefs					1.11	(0.08) ***	1.10	(0.08) ***				
Gender (reference = Man)			-0.14	(0.16)			-0.10	(0.34)			-0.25	(0.36)
Economics (reference = No Training)			-0.42	(0.43)			-0.60	(0.33) *			-1.06	(0.65) *
Experience (reference = No Experience)			0.15	(0.23)			0.09	(0.52)			0.25	(0.65)
Observations	256		241		256		241		256		241	
QIC/QICu	329/260		313/248		255/261		242/249		275/260		264/248	
Mean Differences												
GNN-GN	-2.65	(1.14) **	-2.68	(1.17) **	0.65	(0.46)	0.47	(0.46)	-2.30	(1.07) **	-2.47	(1.08) **
TNN-GNN	0.82	(1.15)	0.83	(1.14)	-0.42	(0.24) *	-0.02	(0.26)	0.49	(1.14)	0.88	(1.13)
TN-GN	-2.15	(1.29) *	-2.18	(1.31) *	0.46	(0.43)	0.66	(0.39) *	-1.95	(1.21) *	-1.73	(1.21)
TNN-TN	0.33	(1.30)	0.33	(1.27)	-0.23	(0.25)	-0.21	(0.19)	0.13	(1.27)	0.15	(1.27)

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: In the first two columns, the dependent variable is mean per person first order beliefs. Otherwise, the dependent variable is mean per person cooperation.
Note 3: Standard errors are cluster-robust (CRSE), clustered on session.
Note 4: $GNN - GN$ represents the effects of the non-neutral language versus neutral language in the give treatment. $TN - GN$ represents the effects of taking versus giving in the neutral treatment. $TNN - GNN$ represents the effect of taking versus giving in the non-neutral treatment. $TNN - TN$ represents the effects of non-neutral language versus neutral language in the take treatment.

Table 3.6: Type moderated mediation: Coefficient estimates

	Direct and Indirect Effects				Total Effects	
	$MEANFOB_i$		$MEANCOOP_i$		$MEANCOOP_i$	
	Eqn. (7)		Eqn. (8)		Eqn. (9)	
Effects	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*	*Est.*	*(CRSE)*
Estimates						
Intercept	4.92	(1.08) ***	-0.89	(0.64)	0.66	(0.33) **
Action (reference = Give)	-1.07	(1.25)	0.75	(0.44) *	0.41	(0.37)
Language (reference = Neutral)	-2.19	(1.16) *	0.89	(0.66)	0.21	(0.56)
Action X Language	2.22	(1.46)	-0.25	(0.83)	0.45	(0.76)
Conditional Cooperator (CC)	1.99	(0.53) ***	-1.43	(0.89)	5.81	(1.11) ***
Low Cooperator (LC)	0.45	(0.35)	1.28	(0.66) *	4.53	(0.96) ***
High Cooperator (HC)	1.08	(1.00)	1.88	(0.86) **	9.09	(1.86) ***
Action X CC	-1.24	(0.74) *	0.55	(0.40)	-2.05	(1.46)
Action X LC	-0.46	(0.50)	-1.96	(0.44) ***	-2.99	(1.10) ***
Action X HC	-1.51	(1.10)	-0.86	(1.44)	-4.29	(2.31) *
Language X CC	-0.90	(0.75)	0.62	(0.51)	-2.62	(1.22) **
Language X LC	0.24	(0.85)	-1.89	(1.08) *	-2.94	(1.60) *
Language X HC	2.94	(1.16) **	-6.59	(0.60) ***	-4.81	(2.12) **
Action X Language X CC	1.61	(1.06)	-2.91	(1.02) ***	1.26	(1.72)
Action X Language X LC	-0.92	(1.04)	2.07	(1.41)	2.53	(2.04)
Action X Language X HC	-0.99	(1.41)	5.12	(2.19) **	6.23	(3.37) *
Mean First Order Beliefs			0.31	(0.10) ***		
Mean First Order Beliefs X CC			0.96	(0.15) ***		
Mean First Order Beliefs X LC			0.58	(0.11) ***		
Mean First Order Beliefs X HC			1.15	(0.14) ***		
Gender (reference = Man)						
Economics (reference = No Training)						
Experience (reference = No Experience)						
Observations	321/272		252/276		274/272	
QIC/QICu	256		256		256	

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: NC = non-cooperator. CC = conditional cooperator, LC = low cooperator, and HC = cooperator.
Note 2: Standard errors are cluster robust (CRSE), clustered on session.
Note 3: Models that included demographic variables were also estimated and yielded similar results.

Table 3.7: Type moderated mediation: Mean differences

	Direct and Indirect Effects				Total Effects	
	$MEANFOB_i$		$MEANCOOP_i$		$MEANCOOP_i$	
	Eqn. (7)		Eqn. (8)		Eqn. (9)	
Mean Differences	*Est.*	*Chi. Sq.*	*Est.*	*Chi. Sq.*	*Est.*	*Chi. Sq.*
Non-Cooperator						
NC GNN-GN	-2.19	(1.16) *	0.89	(0.66)	0.21	(0.56)
NC TN-GN	-1.07	(1.25)	0.75	(0.44) *	0.86	(0.66)
NC TNN-GNN	1.15	(0.76)	0.50	(0.65)	0.41	(0.37)
NC TNN-TN	0.04	(0.89)	0.65	(0.46)	0.66	(0.52)
Conditional Cooperator						
CC GNN-GN	-3.09	(1.01) ***	1.51	(0.61) **	-2.41	(1.11) **
CC TN-GN	-2.31	(1.27) *	1.30	(0.45) ***	0.07	(1.05)
CC TNN-GNN	1.52	(1.17)	-1.86	(0.56) ***	-1.64	(1.48)
CC TNN-TN	0.75	(1.40)	-1.65	(0.47) ***	-0.70	(1.44)
Low Cooperator						
LC GNN-GN	-1.95	(1.30)	-1.00	(0.65)	-2.73	(1.42) *
LC TN-GN	-1.53	(1.46)	-1.21	(0.26) ***	0.40	(1.52)
LC TNN-GNN	-0.23	(1.15)	0.61	(0.76)	-2.58	(1.25) **
LC TNN-TN	-0.65	(1.33)	0.82	(0.57)	0.24	(1.36)
High Cooperator						
HC GNN-GN	0.75	(1.85)	-5.70	(0.59) ***	-4.60	(2.27) **
HC TN-GN	-2.58	(1.66)	-0.11	(1.46)	2.79	(2.53)
HC TNN-GNN	-1.35	(1.57)	4.76	(0.95) ***	-3.88	(2.33) *
HC TNN-TN	1.98	(1.34)	-0.82	(1.76)	2.08	(2.58)

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: NC = non-cooperator. CC = conditional cooperator, LC = low cooperator, and HC = cooperator.
Note 3: Standard errors are cluster robust (CRSE), clustered on session.
Note 4: Models that included demographic variables were also estimated and yielded similar results.
Note 5: $GNN - GN$ represents the effects of the non-neutral language versus neutral language in the give treatment. $TN - GN$ represents the effects of taking versus giving in the neutral treatment. $TNN - GNN$ represents the effect of taking versus giving in the non-neutral treatment. $TNN - TN$ represents the effects of non-neutral language versus neutral language in the take treatment.

Table 3.8: Indirect, direct, and total effects of type moderation beliefs mediation

	Indirect Effects									Direct Effects			Total Effects		
	a	*(CRSE)*	*p*	*b*	*(CRSE)*	*p*	*ab*	*Sobel (SE)*	*p*	*c'*	*(CRSE)*	*p*	*c*	*(CRSE)*	*p*
Mediation (Eqns, 4, 5, & 6)															
GNN-GN	-2.65	(1.14)	0.0204 **	1.11	(0.08)	<.0001 ***	-2.95	(3.03)	0.3321	0.65	(0.46)	0.1597	-2.30	(1.07)	0.0317 **
Moderated Mediation (Eqns. 7, 8, & 9)															
NC GNN-GN	-2.19	(1.16)	0.0586 *	0.31	(0.10)	0.0024 ***	-0.69	(2.53)	0.7872	0.89	(0.66)	0.1734	0.21	(0.56)	0.7096
CC GNN-GN	-3.09	(1.01)	0.0023 ***	1.27	(0.12)	<.0001 ***	-3.92	(3.12)	0.2077	1.51	(0.61)	0.0132 **	-2.41	(1.11)	0.0294 **
LC GNN-GN	-1.95	(1.30)	0.1334	0.89	(0.06)	<.0001 ***	-1.73	(2.53)	0.4902	-1.00	(0.65)	0.1245	-2.73	(1.42)	0.0539 *
HC GNN-GN	0.75	(1.85)	0.6853	1.46	(0.12)	<.0001 ***	1.10	(1.40)	0.4354	-5.70	(0.59)	<.0001 ***	-4.60	(2.27)	0.0427 **

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.10 percent level.

Note 1: GN = give neutral, GNN = give non-neutral, TN = take neutral, and TNN = take non-neutral.
Note 2: NC = non-cooperator. CC = conditional cooperator, LC = low cooperator, and HC = cooperator.
Note 3: Standard errors are cluster robust (CRSE), clustered on session.
Note 4: $GNN - GN$ represents the effects of the non-neutral language versus neutral language in the give treatment. $TN - GN$ represents the effects of taking versus giving in the neutral treatment. $TNN - GNN$ represents the effect of taking versus giving in the non-neutral treatment. $TNN - TN$ represents the effects of non-neutral language versus neutral language in the take treatment.
Note 5: The p-values for indirect effects (ab) employed the Sobel test (Hayes, 2013, pp. 102-103).

Figures

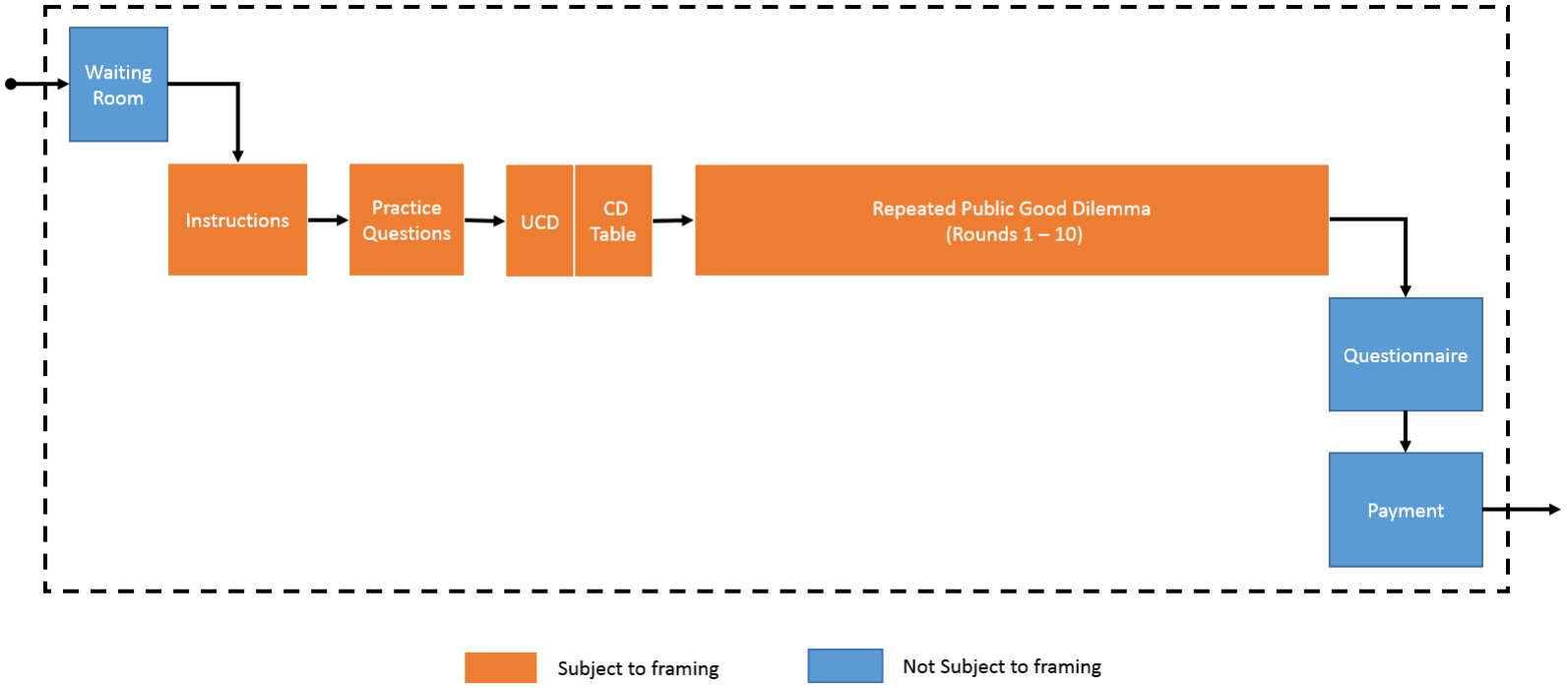
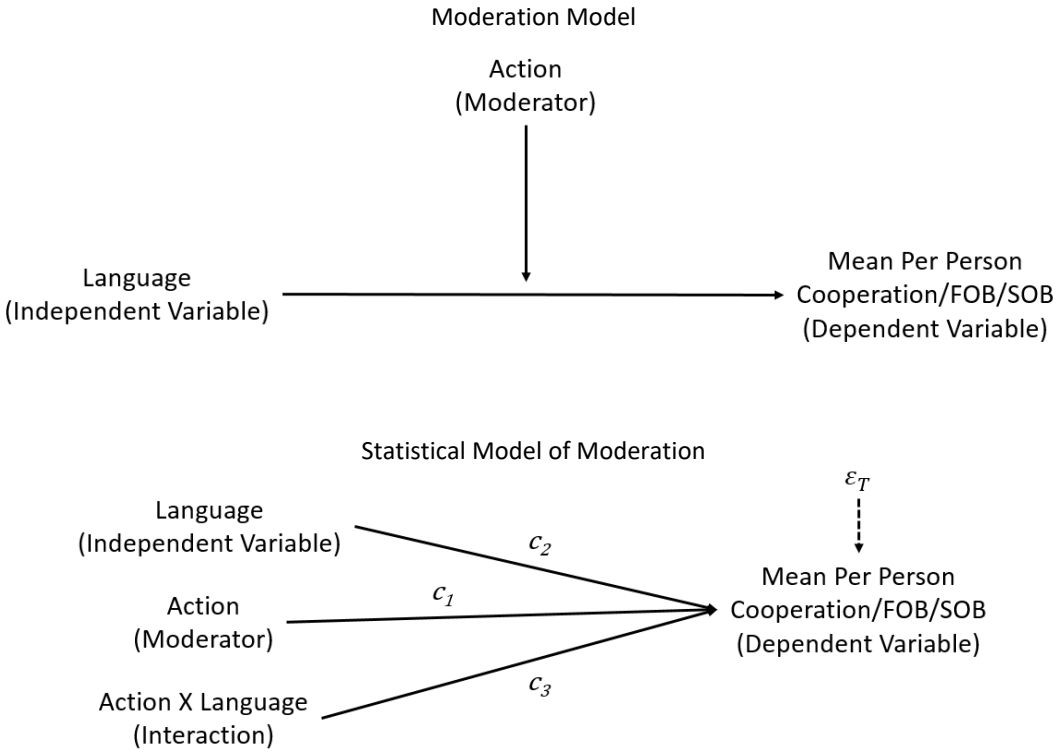


Figure 3.1: The experiment

		Language: Neutral	Language: Non-Neutral
Action	Give	Give-Neutral (GN)	Give Non-Neutral (GNN)
	Take	Take Neutral (TN)	Take Non-Neutral (TNN)

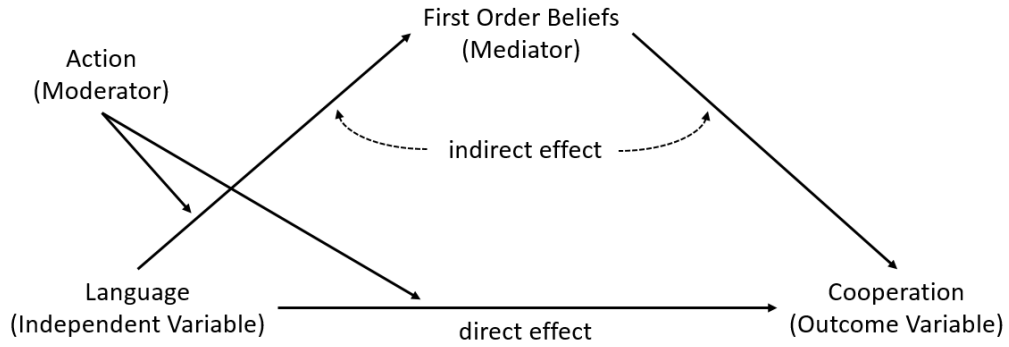
Figure 3.2: 2×2 Factorial framing design
Note 1: Action and language are the two experimental treatments used to create four different frames.
Note 2: Each of the frames was assigned to a different set of 4 sessions.



$$Y_{is} = i_T + c_1 ACT_{is} + c_2 LANG_{is} + c_3 ACT_{is} \times LANG_{is} + \varepsilon_T$$

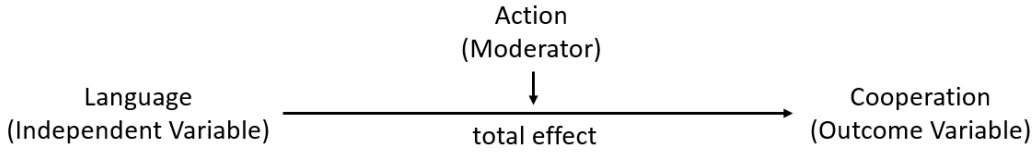
Figure 3.3: Moderation

Direct and Indirect Effects



$$MEANFOB_{is} = i_M + a_1 ACT_s + a_2 LANG_s + a_3 ACT_s \times LANG_s + \varepsilon_M$$
$$MEANCOOP_{is} = i_Y + c_1' ACT_s + c_2' LANG_s + c_3' ACT_s \times LANG_s + bMEANFOB_i + \varepsilon_Y$$

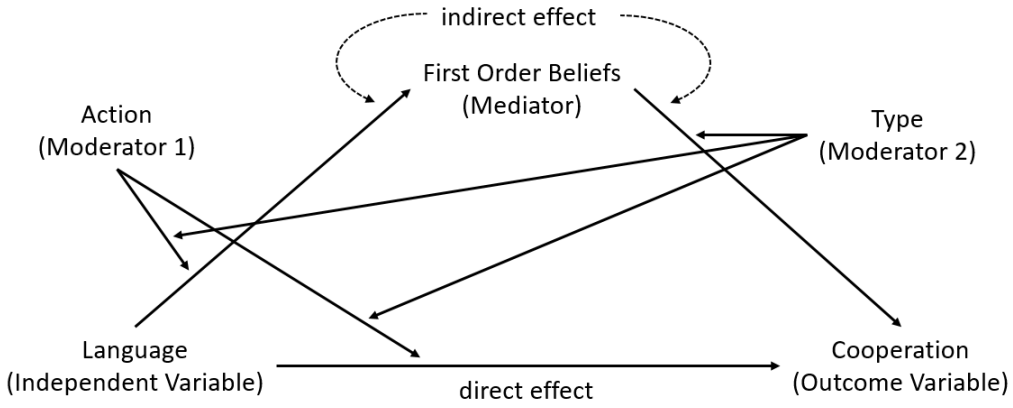
Total Effect



$$MEANCOOP_{is} = i_T + c_1 ACT_{is} + c_2 LANG_{is} + c_3 ACT_{is} \times LANG_{is} + \varepsilon_T$$

Figure 3.4: Mediation

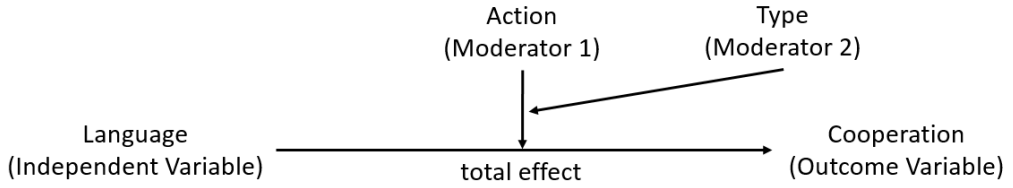
Direct and Indirect Effects



$$MEANFOB_{is} = i_M + a_1 ACT_s + a_2 LANG_s + a_3 ACT_s \times LANG_s + \boldsymbol{a_4 TYPE_i} + \boldsymbol{a_5} ACT_s \times \boldsymbol{TYPE_i} + \boldsymbol{a_6} LANG_s \times \boldsymbol{TYPE_i} + \boldsymbol{c_7'} ACT_s \times LANG_s \times \boldsymbol{TYPE_i} + \varepsilon_M$$

$$MEANCOOP_{is} = i_Y + c_1' ACT_s + c_2' LANG_s + c_3' ACT_s \times LANG_s + \boldsymbol{c_4' TYPE_i} + \boldsymbol{c_5'} ACT_s \times \boldsymbol{TYPE_i} + \boldsymbol{c_6'} LANG_s \times \boldsymbol{TYPE_i} + \boldsymbol{c_7'} ACT_s \times LANG_s \times \boldsymbol{TYPE_i} + b_1 MEANFOB_i + \boldsymbol{b_2} MEANFOB_i \times \boldsymbol{TYPE_i} + \varepsilon_Y$$

Total Effect



$$MEANCOOP_{is} = i_T + c_1 ACT_s + c_2 LANG_s + c_3 ACT_s \times LANG_s + \boldsymbol{c_4 TYPE_i} + \boldsymbol{c_5} ACT_s \times \boldsymbol{TYPE_i} + \boldsymbol{c_6} LANG_s \times \boldsymbol{TYPE_i} + \mathrm{ACT_s} \times \mathrm{LANG_s} \times \mathbf{TYPE_s} + \varepsilon_T$$

Figure 3.5: Type moderated mediation

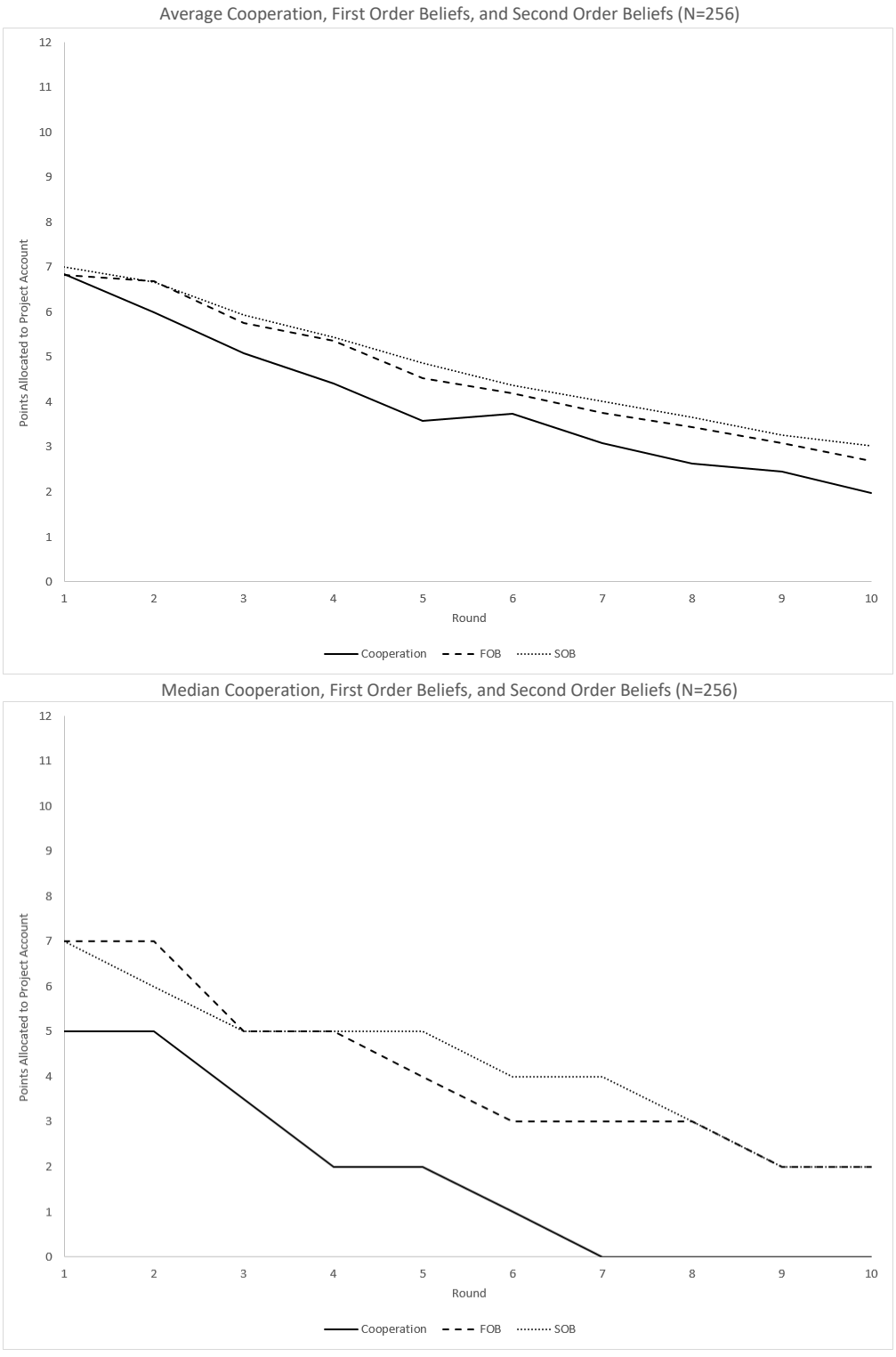


Figure 3.6: Average and median cooperation and beliefs for all participants
Note 1: Cooperation (solid line); FOB = first order beliefs (dashed line). SOB= second order beliefs (dotted line).

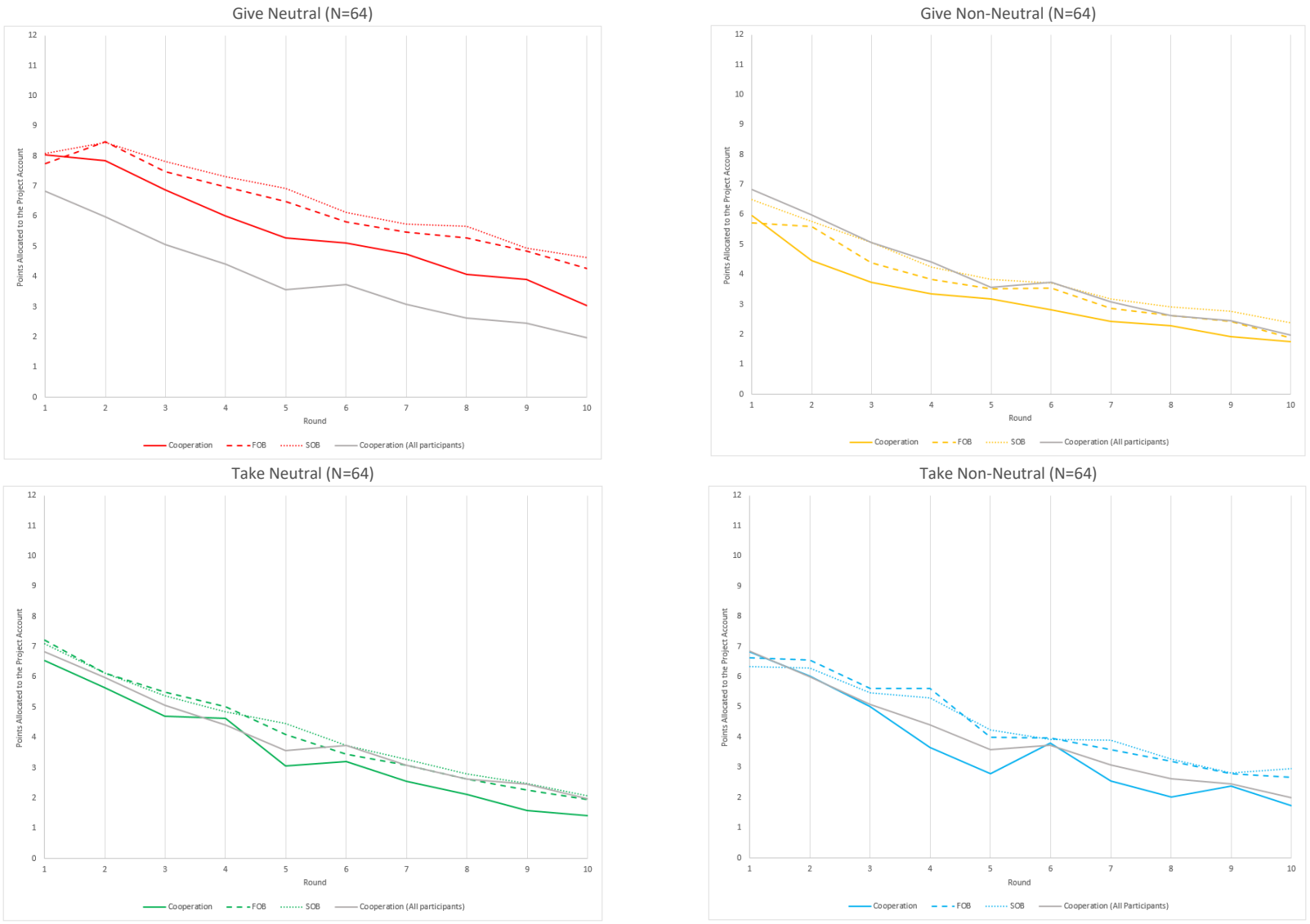


Figure 3.7: Average cooperation, first order beliefs, and second order beliefs for every round paneled by frame

Note 1: Cooperation (solid lines). FOB = first order beliefs (dashed lines). SOB= second order beliefs (dotted lines).

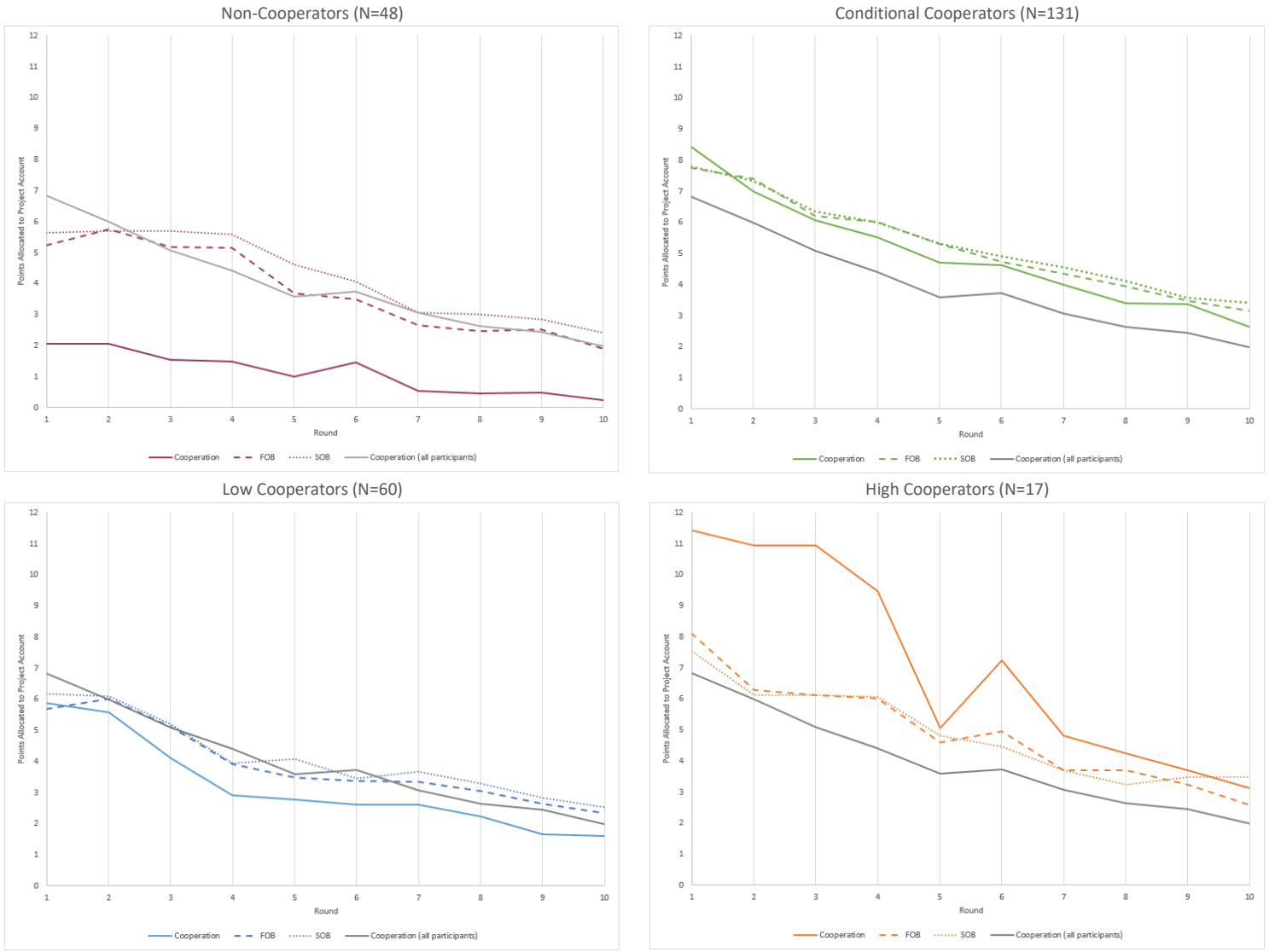


Figure 3.8: Average cooperation, first order beliefs, and second order beliefs for every round paneled by type

Note 1: Cooperation (solid lines). FOB = first order beliefs (dashed lines). SOB= second order beliefs (dotted lines).

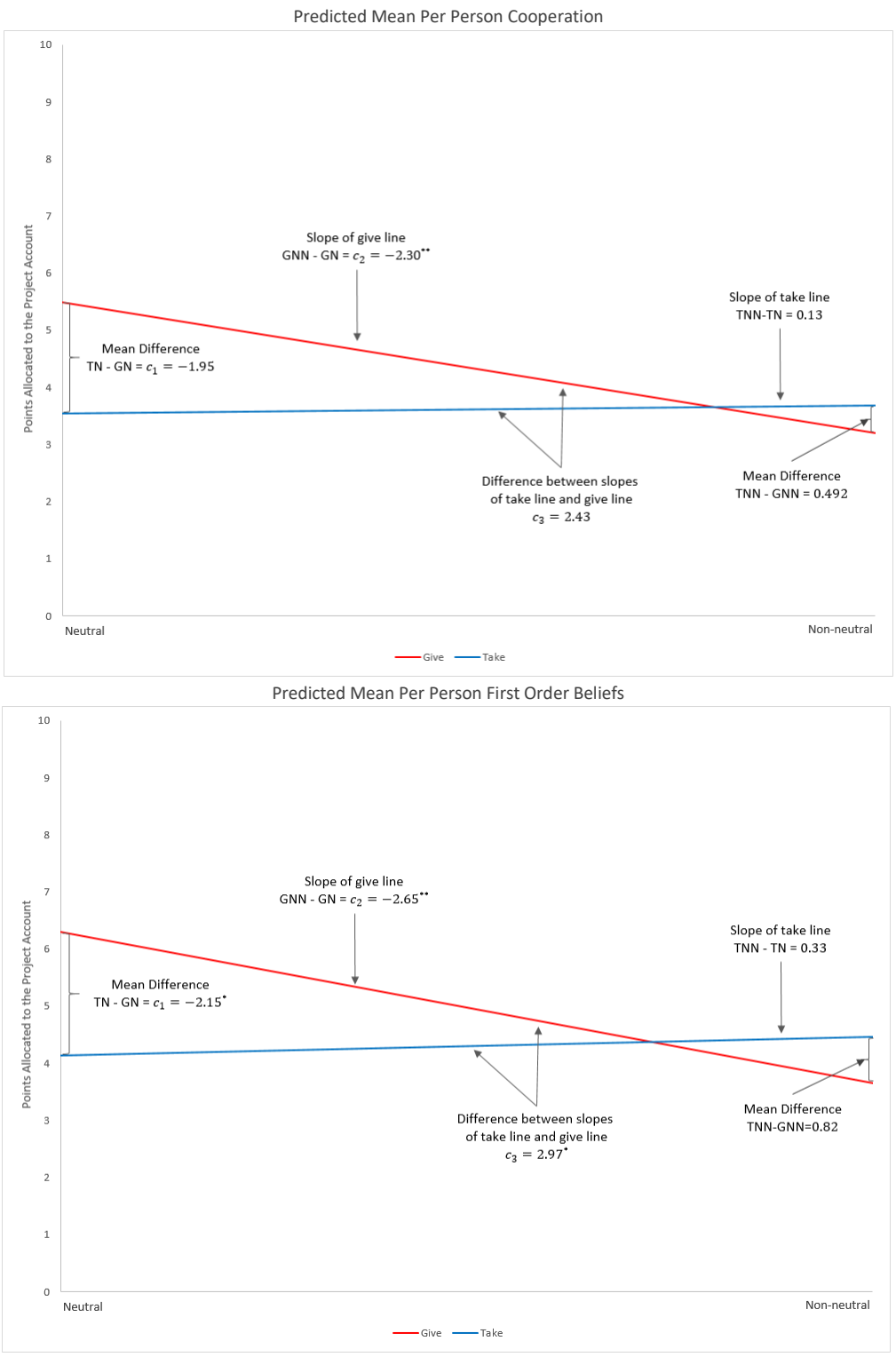
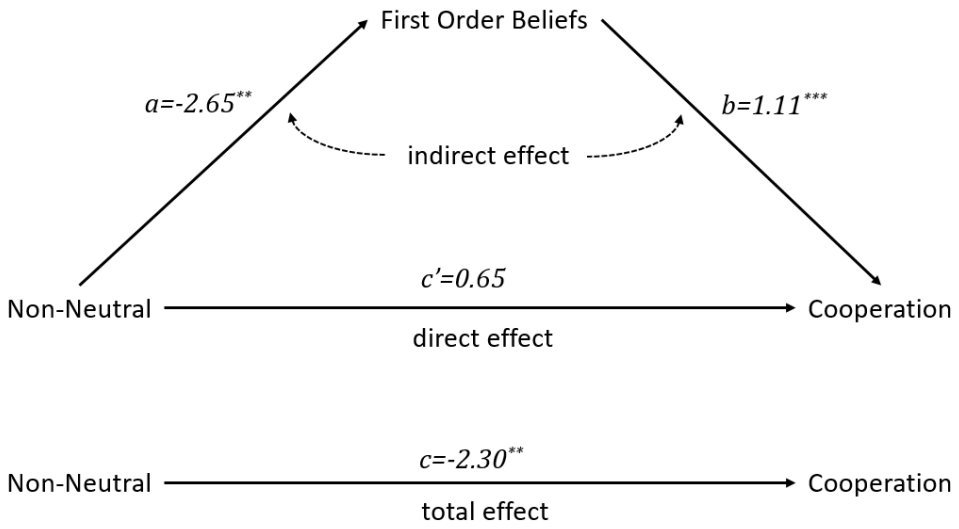


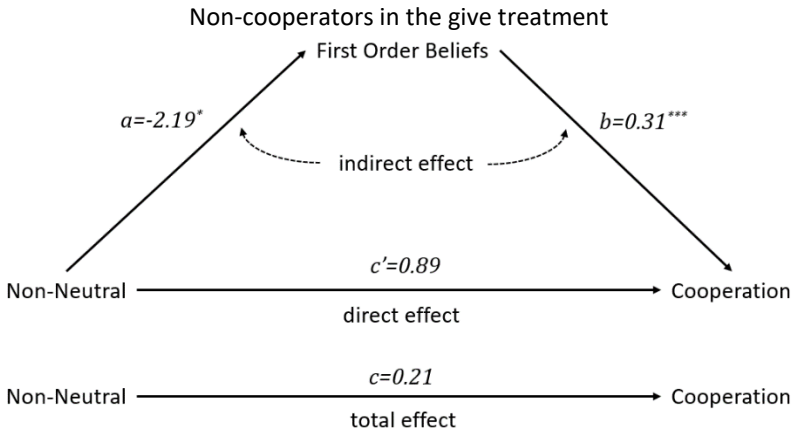
Figure 3.9: Predictions of mean per person cooperation and mean per person first order beliefs



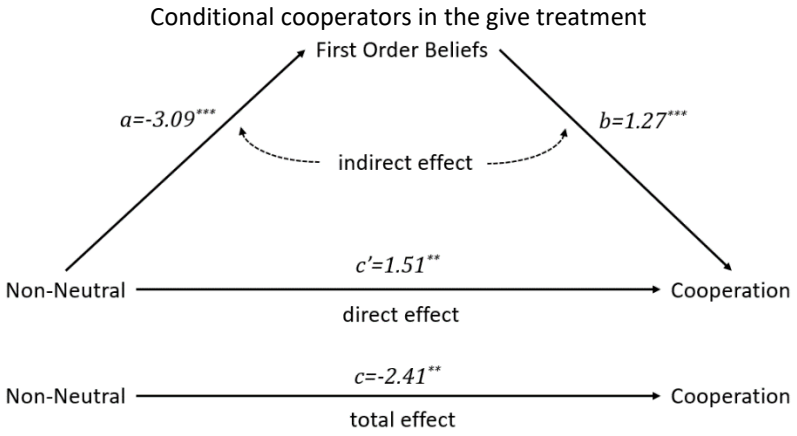
IndirectEffect:	$ab =$	-2.94	$(p = 0.3321)$
Direct Effect:	$c' =$	-2.30	$(p = 0.0317)$
Total Effect:	$c =$	0.65	$(p = 0.1597)$

Figure 3.10: Mediation in the give treatment

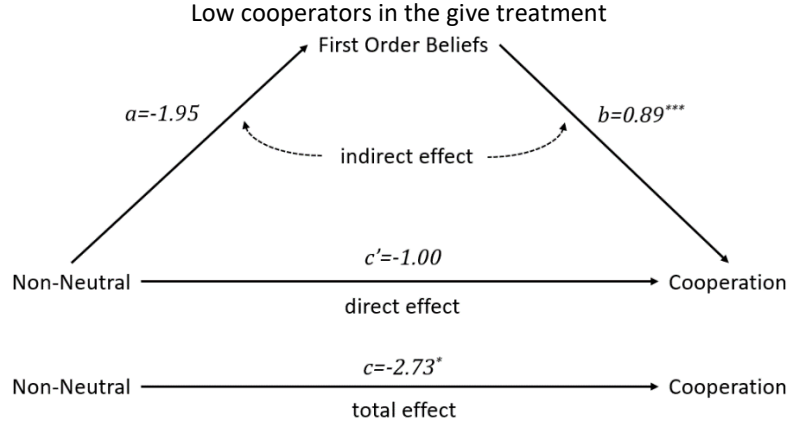
Note 1: ***Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.01 level.
Note 2: The p-values for the direct and total effects were generated via regression analysis with cluster robust standard errors. The p-value for the indirect effect employed the Sobel test (Hayes, 2013, pp. 102-103).



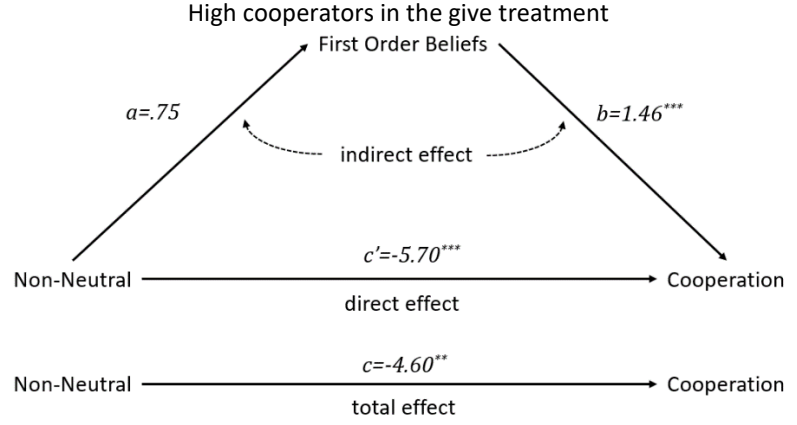
Indirect Effect: $ab = -0.69$ $(p = 0.7872)$
Direct Effect: $c' = 0.89$ $(p = 0.1734)$
Total Effect: $c = 0.21$ $(p = 0.7096)$



Indirect Effect:$ab = -3.92$ $(p = 0.2077)$
Direct Effect: $c' = 1.51$ $(p = 0.0132)$
Total Effect: $c = -2.41$ $(p = 0.0294)$



Indirect Effect: $ab = -1.73$ $(p = 0.4902)$
Direct Effect: $c' = -1.00$ $(p = 0.1245)$
Total Effect: $c = -2.73$ $(p = 0.0539)$



Indirect Effect: $ab = 1.10$ $(p = 0.4354)$
Direct Effect: $c' = -5.70$ $(p < 0.0001)$
Total Effect: $c = -4.60$ $(p = 0.0427)$

Figure 3.11 Median of non-neutral language in the give treatment by type
Note 1: ***Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.01 level.
Note 2: The p-values for the indirect effects (ab) employed the Sobel test (Hayes, 2013, p. 102-103).

Appendix

Researcher Script and Instructions

Notes on Framing

This appendix contains a complete copy of the researcher script and instructions for the give non-neutral frame (GNN). Subtle, but important, differences in wording were used to create the give non-neutral (GNN), give neutral (GN), take non-neutral (TNN), and take neutral (TN) frames. Note that framing language was applied to every component of the experiment except the questionnaire. The questionnaire was identical for all frames. The earnings equations for each frame are given below to illustrate important differences:

Give non-neutral:

Total points-income = *(20 – points you gave to the project account) + 0.4 × sum of all points given to the project account*

Give neutral:

Total points-income = *(20 – points you allocated to the project account) + 0.4 × sum of all points allocated to the project account*

Take non-neutral:

Total points-income = *Points you took from the project account + 0.4 × (80 - sum of all points taken from the project account)*

Take neutral:

Total points-income = *Points you allocated to your personal account + 0.4 × (80 - sum of all points allocated to personal accounts)*

Notes on the Script and Instructions

Opening comments and requests were read to participants. The introduction and practice questions were provided to participants in hard copy, exactly as they appear below, and shown on participants' computer screens. The introduction was also read aloud. The instructions for the practice question were read aloud, but the practice questions themselves were not. The remaining instructions, examples, requests for inputs, and results all appeared on the participants' computer screens. Instructions and examples were read aloud, including the instructions preceding input requests. Results screens were not read aloud. Throughout, comments in square brackets were notes to the researcher that were not read aloud.

GIVE NON-NEUTRAL
OPENNING COMMENTS AND REQUESTS

Hello. Thank you all for coming today. Before we begin, I would like to make a few requests.

First, please turn off your cell phones. [Pause to give them time to do this.]

Please also put away all of your belongings, including your phone, so that the only things on your desk are

- a packet labelled “Instructions”
- a packet labelled “Receipts” and
- a pencil.

When the study has completed, I will ask you to leave, one by one. When you leave, please be sure to

- take all of your belongings, and
- leave the instructions and pencil at your station.

Please listen to the following instructions closely. Follow along on the computer. Please also note that the first three pages or screens of instructions are also on your desk. You may reference them throughout the experiment, if you wish. I have also written some relevant information on the board behind me.

INTRODUCTION

Please read the following instructions carefully. **Depending on your decisions and the decisions of others, you can earn money in addition to the $5 that you receive for showing up.** The entire amount of money you earn will be added up and paid to you in cash at the end of the study.

These instructions are solely for your private information. **You are *not* allowed to communicate during the study.** Violation of this rule will lead to exclusion from the study. If excluded, you will be paid $5 for showing up plus current earnings. If you have questions, please raise your hand. A member of the research team will come to you and answer them in private.

We will not speak of dollars during the study, but rather of points. Your income will be calculated in points, which will be called points-income. At the end of the study, total points-income will be converted to dollars at the following rate:

1 point = 5 cents.

All participants will be assigned randomly to groups of four members. **Except for the research team, no one knows who is in which group.**

The experiment

The experiment consists of a series of decision situations, as described below, that vary in how decisions are made. For every decision situation – called rounds - everyone is randomly assigned to a new group. As a result, **it is almost certain that every decision you make will be made as part of a different 4-person group**.

The decision situation

You will be a member of a group consisting of 4 people. To start**, your personal account contains 20 points**. Every individual has 20 points in their personal account that they can distribute between their personal account and the project account in any way they like. For example, you can put all 20 points into your personal account, you can put all 20 points into the project account, or you can split the 20 points between the accounts.

You will earn points from both accounts in every decision situation. You are the only one who will earn points from your personal account. However, everyone will profit equally from the total amount in the project account. The next screen explains in more detail how earnings are calculated in every decision situation.

Please click OK.

INTRODUCTION (continued)

The following describes how earnings are calculated in every decision situation. **Please pay close attention.** You will have an opportunity to check your understanding on the next screen.

Your points-income from your personal account

You will earn one point for each point in your personal account. For example, if you give 0 points to the project account, your points-income from your personal account will be exactly 20 – 0 = 20 points. If you give 14 points to the project account, your points-income from your personal account will be 20 – 14 = 6 points. No one except you earns something from your personal account.

Points-income from your personal account = *20 – points you gave to the project account*

Your points-income from the project account

Each group member will profit equally from the total amount in the project account. The points-income from the project account for each group member will be determined as follows:

Points-income from the project account = *0.4 × sum of all points given to the project account*

If, for example, the sum of all points given to the project account is 60 points, then you and the three other members of your group each earn 0.4 × 60 = 24 points from the project account. If the sum of all points given to the project account is 10 points, you and the three other members of your group each earn 0.4 × 10 = 4 points from the project account.

Your total points-income

Your total points-income is the sum of your points-income from your personal account and your points-income from the project account.

Total points-income = *(20 – points you gave to the project account) + 0.4 × sum of all points given to the project account*

Please click OK.

PRACTICE QUESTIONS

The following questions will help you understand the calculation of your total points-income. **Please provide all of your answers in points and enter your responses on the computer.**

You must answer all the questions correctly before we continue with the study. Raise your hand if you have questions. A member of the research team will answer your questions privately. There is a calculator at the bottom right of your screen for your use.

For each question, everyone's personal account contains 20 points. Every individual may distribute the 20 points in their personal account between their personal account and the project account in any way they like.

Once you have answered all of the questions, click OK.

1. Everyone, including you, gives 0 points to the project account.

   a) What will *your total points-income* be? _____
   b) What will the *total points-income* be for each of the other group members? _____

2. Everyone, including you, gives 20 points to the project account.

   a) What will *your total points-income* be? _____
   b) What will the *total points-income* be for each of the other group members? _____

3. The other 3 members give a total of 30 points to the project account.

   a) What will *your total points-income* be, if you give 0 points to the project account? _____
   b) What will *your total points-income* be, if you give 10 points to the project account? _____
   c) What will *your total points-income* be, if you give 15 points to the project account? _____

4. Assume that you give 8 points to the project.

   a) What will *your total points-income* be if the other group members give a total of 7 points to the project account? _____
   b) What will *your total points-income* be if the other group members give a total of 12 points to the project account? _____
   c) What will *your total points-income* be if the other group members give a total of 22 points to the project account? _____

INSTRUCTIONS FOR THE UNCONDITIONAL DECISION AND CONDITIONAL DECISION TABLE

You have been randomly assigned to a group of 4 individuals. The decision situation is exactly as originally described (see handout). In this round, each individual has to make **two types** of decisions, an **"unconditional decision"** and **"conditional decisions."**

There are 20 points in your personal account.

First, you decide how many of the 20 points in your personal account you want to give to the project account. **You have no knowledge of others' decisions.** This is the **"unconditional decision."**

Again, **there are 20 points in your personal account.**

Your second task is to fill in a **"conditional decision"** table. **Suppose you know the average number of points that the three other members of your group gave to the project.** You will enter the number of points out of 20 in your personal account that you would give to the project account for each possible average number of points given to the project account by the other three group members. For example, you have to decide how many points you would give to the project account knowing that the others in your group each gave 0 points to the project account. You also will have to decide how much you would give to the project account if the average number of points given by others to the project account was 1, 2, 3, etc.

After all participants have made an "unconditional" decision and have filled in their "conditional decision" table, a group member will be selected randomly from every group. **The "conditional decision" table will be the payoff-relevant decision for the randomly determined subject. The "unconditional decision" will be the payoff-relevant decision for the other three group members.** You obviously do not know whether the random mechanism will select you. Think carefully about both types of decisions because either one could be relevant for you.

Two examples should make this clear. Click OK to see the examples.

EXAMPLES

EXAMPLE 1: Assume that **the random mechanism selected you. This implies that your relevant decision will come from your "conditional decision" table**. The "unconditional decision" is the relevant decision for the other three group members. Suppose that the average number of points given to the project account by the other three group members was 2 points. If you indicated in your "conditional decision" table that you will give 1 point to the project account if others gave 2 points on average to the project account, then the total amount given to the project account is 3×2+1=7 points. Therefore, all group members earn 0.4×7=2.8 points from the project account plus their respective points from their personal accounts. Your total points-income would be (20 – 1) + 2.8 = 21.8 points.

EXAMPLE 2: Assume **that the random mechanism did not select you, so that the "unconditional decision" is the payoff-relevant decision for you and two other group members.** Assume your unconditional decision was to give 13 points to the project account, and the "unconditional decisions" of two other group members were 0 and 20 points. Therefore, the average number of points given to the project account by you and the two other group members was (13 + 0 +20)/3 = 11 points. If the group member whom the random mechanism selected indicated in her "conditional decision" table that she would give 17 points to the project account if the other three group members on average gave 11 points to the project account, then the total number of points given to the project account was 13+0+20+17=50 points. Therefore, all group members earn 0.4×50=20 points from the project account plus the points in their respective personal accounts. Your total points-income would be (20 – 13) + 20 = 27 points.

Click OK. Then read and complete the "unconditional decision" and "conditional decision" table.

UNCONDITIONAL DECISION

There are 20 in your personal account.

Your "unconditional decision" is the number of points, up to 20, that you would give to the project account without any knowledge of others' decisions. Please enter your "unconditional decision."

You may enter any whole number from 0 to 20. Then click OK.

How many points would you give to the project account? [ ]

[Verbally remind participants to click OK if there appears to be a delay.]

CONDITIONAL DECISION TABLE

To start, **there were 20 points in each personal account.**

The numbers in the table below are possible averages of the number of points given to the project account by the three other group members. In the boxes to the right of each average, enter the number of points, up to 20, that you would give to the project account based on the average number of points given to the project account by others.

You may enter any whole number from 0 to 20. After you have made an entry in every box, click OK.

Average Number of points others gave to the project account	Number of points you would give to the project account	Average number of points others gave to the project account	Number of points you would give to the project account
0		10	
1		11	
2		12	
3		13	
4		14	
5		15	
6		16	
7		17	
8		18	
9		19	
		20	

[Verbally remind participants to click OK if there appears to be a delay.]

RESULTS

You were (were not) randomly selected. Therefore, your "conditional (unconditional) decision" was the payoff-relevant decision for you. After you have reviewed the results, please click OK.

Number of points you gave to the project account:	
Average number of points others gave to the project account:	
Your total points-income based on your conditional (unconditional) decision:	

[Verbally remind participants to click OK if there appears to be a delay. Results are rounded to the nearest whole number.]

INSTRUCTIONS FOR THE NEXT TEN ROUNDS

In the next 10 rounds, groups will be reassigned at the beginning of each round so that you are part of a different 4 person group. The decision situation is exactly as originally described (see handout).

There are 20 points in your personal account. Every individual has 20 points in their personal account to distribute between their personal account and the project account in any way they like. Total points-income for each round will be calculated as originally described. Specifically,

Total points-income = *(20 - points you gave to the project account) +*
0.4 × sum of all points given to the project account

In each of the next 10 rounds, you have to make three inputs:

First, **decide how many points you will give to the project account**. That is, you have to decide how to distribute the 20 points in your personal account between the project account and your personal account. Your decision is "unconditional." You must make this decision without knowing what others will do.

Second, **estimate the average number of points given to the project account by the other three group members**, rounded to the nearest whole number.

Third, **estimate the average that the other three group members stated as their estimate for the average number of points given to the project account**, rounded to the nearest whole number.

You will be awarded points in addition to your total points-income based on the accuracy of your estimates. For each of your two estimates:

- If your estimate is exactly right (after the actual average amount has been rounded to the nearest whole number), you will get 3 points in addition to your total points-income.
- If your estimate deviates by 1 point from the actual, rounded average, you will get 2 points in addition to your total points-income.
- If your estimate deviates by 2 or more points from the actual, rounded average, you will not get any additional points.

Click OK. Then read and complete the following ten rounds of decisions and estimates.

ROUND (round number) DECISION AND ESTIMATES

You have been randomly assigned to a new group.

There are 20 points in your personal account. You may distribute those 20 points between your personal account and the project account in any way you like.

Please enter the number of point you will give to the project account. Enter your estimate for the average number of points the other three group members' will give to the project account. Enter your estimate of the average of the other three group members' estimates of the average number of points others will give to the project account.

Enter whole numbers from 0 to 20. After you have made your entries, click OK.

How many points will you give to the project account?	
How many points on average do you think others will give to the project account?	
On average, what do you think the other three members of your group will estimate for the average number of points others will give to the project account?	

[Verbally remind participants to click OK if there appears to be a delay.]

ROUND (round number) RESULTS

Below are the results of round (round number). After you have reviewed the results, click OK.

Number of points you gave to the project account	
Average number of points others gave to the project account	
Your total points-income from this round, not including additional points from estimates	
Your additional points from your two estimates	

[Verbally remind participants to click OK if there appears to be a delay. Results are rounded to the nearest whole number.]

QUESTIONNAIRE

Please indicate the degree to which you agree with each of the following statements. **You must respond to every statement.** Then click OK.

Before making my decisions,	Strongly Disagree				Strongly Agree
I felt ownership of all of the possible project points.	○	○	○	○	○
I felt ownership of only my 20 points.	○	○	○	○	○

During the study:	Strongly Disagree				Strongly Agree
I wanted to earn as many points as I could.	○	○	○	○	○
I wanted others to earn as many points as they could.	○	○	○	○	○
I wanted to earn more points than others earned.	○	○	○	○	○
I felt responsible only for myself, not for others, and not for the project.	○	○	○	○	○
Allocating points to my personal account made me feel good.	○	○	○	○	○
Allocating points to the project account made me feel good.	○	○	○	○	○
I felt good when others allocated points to the project account.	○	○	○	○	○
I felt bad when others allocated points to their personal accounts.	○	○	○	○	○
I felt obligated to allocate points to the project account.	○	○	○	○	○
I felt like I had to allocate as many points to the project account as others did.	○	○	○	○	○
I felt that others were obligated to allocate points to the project account.	○	○	○	○	○
I felt that others were obligated to allocate as many points to the project account as I did.	○	○	○	○	○
I felt cheated when others allocated points to their personal accounts.	○	○	○	○	○
I felt cheated when others allocated more points to their personal accounts than I did.	○	○	○	○	○
I was worried that others would allocate nothing to the project account.	○	○	○	○	○
I had to allocate points to my personal account because I expected the project account to be small.	○	○	○	○	○

QUESTIONNAIRE (continued)

Please indicate the degree to which you agree with each of the following statements. **You must respond to every statement.** Then click OK.

Thinking back over the study,	Strongly Disagree				Strongly Agree
I fully understood the decision situation throughout the study.	○	○	○	○	○
I understood the decision situation better over time.	○	○	○	○	○
I was confused throughout the study.	○	○	○	○	○

Overall	Strongly Disagree				Strongly Agree
I was very satisfied with the results of the decision situations.	○	○	○	○	○
I believe that others were very satisfied with the results of the decision situations.	○	○	○	○	○

QUESTIONNAIRE (continued)

Please answer as many of the following questions as you like. **Responding to these questions is optional.** Then click OK.

What is your age? _____

What is your gender? ○ Male ○ Female

Are you a student? ○ US Student ○ International Student ○ Non-student

QUESTIONNAIRE (continued)

Please answer as many of the following questions as you like. **Responding to these questions is optional.** Then click OK.

If you are not a student, in what occupation do you currently work? Please answer according to your most recent employment if you are not employed.

- Management, Business, and Financial
- Computer, Engineering, and Science
- Education, Legal, Community Service, Arts, and Media
- Healthcare Practitioners and Technicians
- Service
- Sales and Related
- Office and Administrative Support
- Farming, Fishing, and Forestry
- Construction and Extraction
- Installation, Maintenance, and Repair
- Production
- Transportation and Material Moving
- Military

If you are a student, in what school or program are you currently enrolled.If you are enrolled in more than one, select the one that most closely matches your area of concentration.

- Architecture and Urban Planning
- Art & Design
- Business
- Dentistry
- Education
- Engineering
- School of Information
- Kinesiology

- Law
- Literature, Science, and the Arts
- Medicine
- Music, Theatre, and Dance
- Natural Resources and Environment
- Nursing
- Pharmacy
- Public Health
- Public Policy
- Social Work

QUESTIONNAIRE (continued)

Please answer as many of the following questions as you like. **Responding to these questions is optional.** Then click OK.

Have you previously participated in a study like this one?

- ○ No
- ○ Yes, once, not including this study
- ○ Yes, more than once, not including this study
- ○ Don't know

Are you trained in economics?

- ○ No, I have never taken any courses in economics
- ○ Yes, I have taken one or two courses in economics
- ○ Yes, I have taken more than two courses in economics
- ○ Don't Know

CONCLUSION

The entire experiment is finished. Your income from the entire experiment includes:

- Your earnings from your unconditional decision and conditional decision table *(total points-income × $0.05).*
- Your earnings from rounds 1 through 10 *(total points-income × $0.05 for every round).*
- Your earnings for correct estimates in rounds 1 through 10 *(additional points × $0.05 for every round).*
- $5 compensation just for showing-up.

Therefore, your total earnings from the entire study, rounded up to the next dollar are _____.

Thank you for your participation! Please

- Complete both copies of the paper receipt at your station,
- Quietly pack up your things, and
- Leave the paper instructions and pencil at your station.

When your station number is called, come to the researcher area in the back of the room for payment. Bring the completed receipts and all of your things.

Click OK when you are ready to clear this screen.

[Verbally remind participants to click OK if there appears to be a delay. Call participants to the back of the room one-by-one to receive payments in a blank envelope.]