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Abstract:
Earlier studies have found that a substantial part of the contributions in public good games can be explained by subjects misperceiving the game's incentives. Using a large-scale public good experiment, we show that subtle changes in how the game is framed substantially affect such misperceptions and that this explains major parts of framing effect on subjects' behavior. When controlling for the different levels of misperception between frames, the framing effect on subjects' cooperation preferences disappears.

Keywords: Public goods; Cooperation; Misperception; Framing effects; Internet experiment

JEL-codes: H41, C90, D03

1. Introduction

Laboratory experiments have repeatedly shown that the way a decision problem is presented to subjects can have a great impact on observed behavior. Such presentation, or framing, effects have been studied by economists and psychologists because they reveal how behavior is shaped by institutions and conditions surrounding the decision makers. While it is well known that complexity of such incentive schemes induces misperception our paper makes a contribution, by showing that the observed level of misperception can, to a substantial degree, be affected by how a given decision problem is framed. In line with many earlier framing studies, our paper uses a public good game, which places subjects in the classical social dilemma that embodies the trade-off between personal income and collective income. The interest in these games is easy to understand, since cooperative forms of behavior are essential in many aspects of human life. Starting with the early contributions of Bohm (1972) and Marwell and Ames (1979, 1980, 1981), there is now a large body of experimental evidence, which points out that many subjects do contribute to the public goods, even though this reduces their personal income. Different presentations of the public good game have been used in order to understand the structure of these preferences. In this literature, a number of studies have found significant differences in contributions when a game is framed as 'giving to a public good' compared with an economically equivalent framing of the game as 'taking from a public good' (Andreoni 1995b, McCusker and Carnevale 1995, Sonnemans et al. 1998, van Dijk and Wilke 2000, Iturbe-Ormaetxe et al. 2011, Dufwenberg et al. 2011, Cubitt et al. 2011). The general impression from the literature is that positive frames, in which subjects give to a public good, induce greater cooperation compared to negative, or take frames. Yet, examples of the opposite also exist. McCusker and Carnevale (1995) find that the take frame is associated with around 50 percent more cooperation than the give frame.

Several potential explanations of the observed framing effects have been put forth. Some authors interpret framing effects as manifestations of underlying *preferences*. McCusker and Carnevale (1995) and Iturbe-Ormaetxe et al. (2011) argue that subjects have reference dependent utility and are loss averse. van Dijk and Wilke (2000) suggest that differences in framing may induce shifts in subjects' 'focus' on personal and group outcomes when subjects solve their decision problem

(i.e. that preferences not only depend on outcomes, but also on the specific process through which these outcomes are generated).

Other studies find evidence for differences in subjects' *beliefs* about what other subjects will contribute as being an important explanation for framing effects (Sonnemans et al. 1998, Dufwenberg et al. 2011, Ellingsen et al. 2012). To the extent that subjects' preferred contributions are conditional on other group members' contributions, their behavior depends on their beliefs about the behavior of the other group members. Thus, if different frames induce different beliefs, it becomes difficult to make inferences about the underlying preferences from data on contributions in a public good game.

However, another possible explanation for framing effects has been suggested by Köszegi and Rabin (2008, pp. 1829) who write: "Framing and focusing effects can be interpreted in two ways. Under one interpretation, the frame or focus of an individual affects her preferences, and these preferences are translated into frame-sensitive choices. In the alternative view, preferences do not depend on the momentary frame or focus, but some decision situations lead people to make mistakes in implementing their stable preference". If subjects find it more difficult to understand their decision problem in one frame, then the difference in contributions between frames might be explained by more subjects misperceiving their optimal behavior in the frame. Moreover, several studies have found that misperceptions are an important explanation for cooperative behavior in public good games (Andreoni 1995a, Houser and Kurzban 2002, Bayer et al. 2009 and Ferraro and Vossler 2010). Thus, if framing does affect the degree of misperception, it could potentially explain a substantial part of the framing effects which have been attributed to shifts in preferences and beliefs in other studies. The purpose of this paper is to experimentally investigate whether participants' understanding of the game (Chou et al. 2009 label this effect 'game form recognition') depends on the way the public good game is presented to them and whether this in turn affects their cooperation preferences and behavior.

Our experiment disentangles preferences and beliefs about other subjects' contributions and misperceptions. We do this by eliciting conditional public good contribution profiles through the strategy method (Fischbacher et al. 2001), thereby controlling for beliefs about other group members' contributions. In addition to controlling for beliefs, this elicitation method provides a much richer insight into the behavioral strategies underlying public good contributions,

compared to merely studying unconditional contributions. To measure the degree of misperception, we ask incentivized ex-post questions that elicit the conditional contribution profiles that the subjects believe would maximize personal income and would maximize group income. We assume that incorrect answers to these questions indicate that a subject has misperceptions about what his optimal behavior is. As a natural way to relate our findings to the existing literature on framing effects in public good games, we also include a one-shot unconditional public good decision in our experiment.

Our experimental evidence highlights that the give/take framing dramatically affects the rate of misperception. 53% of the subjects in the give treatment were able to identify the payoff maximizing free riding strategy, whereas this was true for 68% of the subjects in the take treatment. We also find that this difference in misperception between frames has a substantial effect on the observed differences in contribution profiles between frames. The highly significant difference between the proportions of subjects choosing the free riding strategy in the two frames becomes insignificant when we control for misperception. We find a corresponding effect for the unconditional contributions in the one-shot public good game. The share of zero or low contributions differs significantly across frames, but this effect becomes insignificant when we control for misperception. Finally, we use an extensive battery of intelligence and personality tests combined with information on subjects' drop out through the experiment to shed light on *why* changes in framing induce changes in the level of misperception. Our data suggests that the subjects in the take frame make relatively higher effort to understand the game, presumably because they perceive a lower benefit if they misperceive in the take frame than if they misperceive in the give frame.

Our results generally imply that it is important to be aware of (and control for) shifts in misperceptions between frames in studies which attempt to identify framing effects on underlying variables such as beliefs or preferences. This would certainly seem to be the case in the public good type games that we have studied, although we suspect that it may also be important for studies of framing effects in other settings. Our results may also have important policy implications. Charges, fines, subsidies and tax systems are often complex constructions (characterized by progression, deductions etc.) and recent field studies suggest that simplifying such systems can substantially affect behavior, possibly by reducing misperceptions (e.g.

Dynarski and Scott-Clayton 2008, Gallagher and Muehlegger 2011). Though simplification may be a substantial advantage in this respect, distributional effects and other considerations often make this infeasible. However, our results suggest that merely changing the framing of a given system (without reducing its objective complexity or distributional effects as such) can potentially result in substantial reductions in the general degree of misperception.Experimental design:

2.1 General outline of the experiment

Our data comes from an internet experiment conducted through iLEE (internet Laboratory for Experimental Economics) at the Department of Economics at the University of Copenhagen in spring 2008[1]. Using the Central Person Register (CPR), which contains all persons legally residing in Denmark, 18,027 potential subjects were randomly selected by the Danish National Bureau of Statistics (Statistics Denmark) and were sent an invitation to participate in the experiment by ordinary mail. In the invitation letter, subjects were given the internet address of the experiment and a personal login code.[2] It was explained to the subjects that they had a week to respond to the invitation. During this week, subjects could log in and out as they wished. After this week, we matched the participants who had completed the experiment into groups, and these participants could log in again to receive feedback on the experimental results and type in their bank account number, to which their earnings during the experiment were transferred.

[1] See http://www.econ.ku.dk/cee/iLEE/iLEE_home.htm for a detailed description of the iLEE project. The platform has been used for numerous studies on different topics, see Thöni et al. (2012) for an example.

[2] A translation of the invitation letter is included in the appendix together with other supplementary materials such as screenshots, instructions and more thorough descriptions of the recruitment process and the experimental design.

When logging on to ILEE website for the first time, subjects were randomly allocated to either the give, or the take frame.[3] The applied framing follows the design of Andreoni (1995b). The subjects were then given general information about the scientific purpose of the experiment and told that they would earn money. After this introduction, the subjects were asked to type in their sex, age, and highest completed education level. Subsequently, the subjects met more specific instructions for the public good (PG) games. After reading these instructions, the subjects were asked traditional control questions to test whether they had read and understood the provided information. Having passed the control questions, the subjects faced their first actual experience with the PG game, a standard one-shot PG game. Hereafter, the subjects were told that they had been matched into new groups and that they should complete a strategy method version of the PG game (strategy game), which measured subjects' conditional willingness to cooperate. Immediately after the strategy game, the subjects met the misperception test. After this, the subjects filled out a series of personality and cognitive ability tests and background questions.

Attrition between frame allocation and completion of the experiment was substantial (34%) and much larger than is usual in laboratory experiments. The reason for this is that our experiment was run over the Internet where subjects can easily quit by simply closing the internet browser. There is a small difference in attrition rates between the treatments, with slightly fewer subjects dropping out in the give frame (67% vs. 63%). This will be discussed more thoroughly in the section 3. For the analysis in our paper, we use subjects who answered all control questions correctly and who completed the entire experiment.

2.2 The Public Good Games

Initially, subjects played a standard PG game with a group size of four. Each subject was given control of 50 DDK (≈6.7 €) and allowed to freely allocate the sum by either contributing it to the

[3] Two thirds of the subjects were allocated to a 'give to the public good' framing of the PG game (Give treatment), whereas the remaining one third were allocated to a 'take from the public good' framing of the game (Take treatment). The reason for the uneven (but still random) allocation of subjects between the treatments is that the data from the give treatment are also used for other papers, and hence more weight on these data is prioritized.

PG, or keeping it for themselves. In the **give** frame, the subjects were initially given the 50 DDK as a private endowment, and they were then asked what part of the endowment they wanted to contribute to the common pool. In the **take** frame, the 50 DKK were initially allocated to the common pool and subjects were then asked how many of the 50 DKK they wanted to withdraw from the common pool and instead keep as private income. Under both frames, the money allocated to the PG was doubled and shared equally among all group members. Hence, the marginal private return of a one DKK contribution to the PG is ½ DKK. In addition to private income and the return on their own PG contribution, the subjects in both frames also earned an amount corresponding to half of the PG contributions made by the other three group members.

After completing the standard PG game, each subject played the strategy version of the PG game (strategy game) with the same framing. We applied the **strategy game** developed by Fischbacher et al. (2001) in which a profile of PG contributions, conditional on different levels of contributions from other group members, is elicited from each subject. The subjects were divided into new groups of four[4] and asked to make an unconditional contribution and a profile of conditional allocations to the PG. Prior to this, the subjects were informed that there was a 25% chance that their payoff would be calculated based on their conditional contribution and a 75% chance that it would be calculated based on their unconditional contribution profile. First, the unconditional contribution was elicited in exactly the same way as the contribution in the previous standard PG game. The subjects were then asked to indicate their contribution conditional on the values of the other three group members' average contributions varying from 0 to 50 DKK in steps of 5 DKK. Thus, each subject was asked what his contribution was if the other group members on average contributed 0 DKK, if they on average contributed 5 DKK, and so on up to 50 DKK. When calculating payoffs, we used the elicited unconditional contributions for three randomly selected group members, while the forth subject's contribution was calculated based on the elicited conditional contribution profile using the average of the unconditional contributions from the other three group members.

[4] Recall, however, that the actual matching took place after the subjects had finished the experiment and only included subjects who had completed the experiment.

Since contribution profiles are conditional on other group members' contributions, these profiles are assumed to be unaffected by beliefs about the other group members' contributions. As shown by Fischbacher et al. (2001), the strategy method provides an incentive to disclose precisely the conditional contribution profile which underlies the unconditional contribution elicited in the standard PG game.[5]

2.3 The Misperception Measure

Right after the strategy game, the subjects were asked incentivized control questions to test for misperception. We used the contribution profile setup introduced in the strategy game to ask participants to delineate the contribution profiles of imaginary subjects who either only care about their own payoff or only care about the payoffs of others.[6] The test consists of six questions. It was emphasized that each question only had one correct answer and that the subjects would earn 5 DDK (≈0.7 €) for each correct answer. The first three questions asked the subject what public good contributions a person, who only cares about their own payoff, would choose if the other subjects, on average, contribute 0 DKK (question 1), 25 DKK (question 2) and 50 DKK (question 3). In the last three questions, the subjects were asked what contribution a person who only cares about the payoff to other group members would choose, when the others on average contribute 0 DKK (question 4), 25 DKK (question 5) and 50 DKK (question 6). We interpret incorrect answers to these questions as an indication that the subject has misperceptions about how to implement the specified goals. Our method for measuring misperceptions (or

[5] This assumes that the subjects assign non-degenerate probabilities to each of the given average contributions of the others. In case this is not true and the subjects assign a zero probability to one of the proposed contribution levels, the subject is indifferent with regards to what amount to state. In principle, it could also be that contribution preferences do not depend on the average contributions, but rather on the distribution of contributions in the group or the strategy profiles of the others.

[6] We tested the sensitivity of the wording of these questions in a follow-up laboratory experiment, which also used an alternative wording asking subjects directly to state which contributions would maximize their own earnings. The results will be commented on in the next section, but it is worth pointing out that the main message of the paper does not appear to depend on the way these questions were phrased.

confusion) is different compared to the previous literature. Andreoni (1995a) measures misperception by conducting a repeated public good game in which subjects receive payment based on their ranking among the participants and then comparing this rank payment treatment to normal payment treatments. Andreoni finds that confusion accounts for about half of all contributions, although with a declining trend over time. Hence, a learning effect is found. Houser and Kurzban (2002) and Ferraro and Vossler (2010) confirm the findings of Andreoni using setups in which subjects play against computers. Any positive contribution towards the computers is viewed as an act of confusion. They again find a substantial amount of confusion, but also that it decreases with experience.[7] None of these studies has addressed the relation between framing and misperceptions. The study which is most related to ours is Andreoni (1995b), who, after his experiments, asks the subjects two non-incentivized questions about what behavior would maximize personal benefit. In contrast to our results he is not able to detect any framing effects on misperception. One reason for this could be that his questions were asked after subjects played the game for 10 rounds with feedback after each round. Still, there could have been framing differences in misperception in the early rounds of the experiment and they may have had persistent effects on outcomes since behavior in early rounds is likely to affect behavior in later rounds[8]. Another difference between our design and Andreoni's is that his questions (in contrast to ours) were not incentivized. A recent public good experiment with an incentivized misperception test conducted by Fosgaard, Reinholt, and Foss at the University of Copenhagen, found that a substantial degree of misperception was present even after 20 periods of repetition[9]. An additional benefit of our study is that we use a very large sample (2,042

[7] Bayer et al. (2009) point out that the amount of confusion detected in Houser and Kurzban (2002) should be viewed as an upper bound on confusion.

[8] It should be noted that addressing these types of dynamic aspects is difficult using the type of direct misperception measurement that we do. In repeated settings, one is restricted to either measure misperception at the end of the experiment when experience may have alleviated the effects, or one has to interfere and ask for misperception between rounds which naturally can affect misperception and behavior in later rounds.

[9] This project is work in progress. More information about the findings is available upon request.

subjects) consisting of participants who are much more representative of the general population than the traditional university subject pool.

2.4 Measures of Cognitive Ability and Personality

After completion of the misperception test, our subjects were asked to complete a number of well established standardized cognitive ability and personality tests. These tests were completely identical for both treatments.

First, the subjects completed the visual IST 2000R[10] **IQ test** (referred to in the following as the IQ test). This test asks the subjects to solve 20 different logic puzzles. The task in each puzzle is to identify one of five candidate symbols, which would finalize a sequence of pictures constituting a logical graphical string (for a snapshot example, see the appendix). For instance, subjects see three solid square boxes in a row as the logical string. Subjects are asked which of five suggested symbols would logically prolong the presented string. If subjects, for instance, can choose between a triangle, a line, a circle and a squared solid box, the correct answer is to choose the solid box, which is the only logical continuation of the sequence of symbols. The subjects were given 10 minutes to solve as many of the puzzles as possible, and were allowed to jump back and forth between the puzzles as they wished. The assumption is that the higher the number of puzzles solved, the higher the cognitive ability of the participant.

We also used an alternative measure of cognitive ability referred to as the **Cognitive Reflection** (CR) test (Frederick 2005). The test simply consists of three questions that have immediate and intuitive (but incorrect) answers and more cognitively demanding (but correct) answers. The three questions are shown in the appendix. The test measures whether a subject tends to give fast intuitive answers, rather than carefully trying to derive the correct answer. In other words, the test captures the individual's willingness to engage in cognitively demanding tasks (Grimm et al. 2009). The more correct, as oppose to immediate and intuitive, answers a subject gives, the more cognitive reflected he is.

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[10] Used with permission from the Danish Psychology Publisher, www.dpf.dk.

The first IQ test captures a general ability to think logically about complex and unfamiliar concepts. That is, basic cognitive abilities that do not depend on prior knowledge or acquired skills - often referred to as fluid intelligence (see Borghans et al. 2008). In contrast, the results of tests like the CR test are, to a much larger extent, dependent on acquired skills and so these tests are said to measure 'crystallized' intelligence (see Borghans et al. 2008). For instance, reading and math skills are certainly important when answering the CR test, but not as important for completing the IQ test. Prior to both tests, the subjects were informed that there was only one correct answer to each posed question or problem. The Cognitive reflection test (CR test) was incentivized, whereas the IQ test was not.

Finally, we applied a Danish version of the Big 5 **personality test**.[11] The test consists of 60 statements covering personality traits in five dimensions: agreeableness, conscientiousness, extraversion, neuroticism, and openness.[12] Based on the answers to these statements, each subject is assigned a score for each of the big 5 dimensions. A high score for a given trait indicates that the trait is an important part of the subject's personality.

2. Results

Our experiment was conducted in May-June 2008 with a total of 2,042 subjects completing the experiment. 1,366 subjects completed the give treatment and 676 the take treatment. On average, subjects earned 300 DKK (≈ 40 Euros) during the entire experiment. This amount included

[11] We used the Danish NEO-PI-R Short Version test by permission of Danish Psychology Publishing (www.dpf.dk).

[12] The Danish NEO-PI-R Short Version consists of five 12-item scales which measure each of the 5 domains. The 12 items for each domain are chosen from the original 48 items (of the full NEO-PI-R test) as follows: for each facet, the two items (out of eight) with the highest correlation with the total factor score are chosen (this is different from the American 60-item version of NEO-PI-R, called NEO-FFI, where the 12 items with the highest correlation with the total factor score are picked, without regard to which facet the single items belong to). In the Danish short version, all facets are therefore equally represented within each domain.

payment for some tests that are not reported here. The subjects spent on average 50 minutes completing the experiment.

We first present results on how framing affects misperceptions. We then report how differences in misperception between frames affect contributions in the standard public good game and investigate, in-depth, how important framing effects on misperception are for framing effects in the strategy version of the game. We conclude with a discussion of possible explanations for our results.

3.1 Framing effects on misperception

Table 1 summarizes the proportion of subjects in the give and in the take frame who understood how to implement specific goals. The first row displays the proportion of subjects who correctly understood which contribution strategy would maximize their personal income (i.e. the proportion of subjects who answered all three questions about this strategy correctly). The second row gives the proportion of subjects who correctly understood which contribution strategy would maximize the group income. The third row displays the proportion of subjects who understood how to implement both strategies.

Table 1 - Percent of subjects who understood how to implement specific goals

Goals:	Give treatment (n=1366)	Take treatment (n=676)	Chi2 Test
Maximize personal income:	53%	68%	p=0.000
Maximize group income:	78%	76%	p=0.364
Both goals:	49%	59%	p=0.000

Note: Chi2 test refers to Pearson's Chi2 asymptotic test.

We see that a substantial proportion of subjects suffered from some form of misperception about how to implement the specified goals – even though all participating subjects answered the usual instruction control questions correctly. Overall, subjects are considerably more confused about how to maximize personal income than group income. Furthermore, we see a substantial and clearly significant difference in the degree of misperception about how to maximize personal income between the two frames: 68% of the subjects in the take treatment understood how to maximize personal outcome, whilst this was the case for only 53% of the subjects in the give

frame. On the other hand, there is no significant framing affect on misperception about how to maximize group outcome.

The framing effect on misperceptions is confirmed by the logit regressions presented in Table 2 below. The table displays the marginal effects of different sets of regressors on the probability of misperceiving in the sense of answering at least one question wrongly. We note that the treatment dummy for the take treatment is highly significant across all specifications and that being in the take treatment results in about a 10% lower probability of misperceiving. Moreover, the effect of the covariates appears robust and intuitive. Subjects with long education and high scores on the cognitive reflection and the IQ tests have a higher probability of understanding the game form. In addition, the propensity to misperceive increases with the number of times the subject typed a wrong answer to the control questions and decreases with the number of times the subject used the profit calculator. The misperception probability is also significantly related to the personality traits extraversion and conscientiousness. Extravert subjects are more likely to fail the misperception test, which seems intuitive, as extrovert subjects tend to be active and spontaneous rather than deliberate people.[13] There is a positive relation between conscientiousness and passing the misperception test. Again, this appears rather intuitive, as conscientiousness is associated with being careful, reliable, self-disciplined and scrupulous (see for example McCrae, and Costa, 1987).[14] We will come back to how conscientiousness relates to misperception and frames when we discuss our findings.

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[13] One manifestation of this relationship in our data is that there is a strong negative relationship between extroversion and the number of correct answers on the cognitive reflection test. For example using an ordered probit to regress the cognitive reflection score on a set of regressors including extroversion gives a z-value around -4 for extroversion.

[14] Again, this is visible in our data if we use an OLS model to predict the time spent on the instructions. Having a high conscientious score is associated with spending more time reading the instructions. Similarly, using a tobit model, we note that conscientiousness is significantly and positively related to the number of clicks on the profit calculator

Table 2 – Logit regressions on the probability of misperceiving

	(1)	(2)	(3)
Take	-0.0969***	-0.0963***	-0.0950***
	[0.0228]	[0.0219]	[0.0217]
Female	0.0273	-0.0183	-0.0167
	[0.0222]	[0.0234]	[0.0231]
Age	-0.00273	-0.00190	-0.00332
	[0.00434]	[0.00424]	[0.00421]
Age^2	0.0642	0.0358	0.0498
	[0.0467]	[0.0454]	[0.0450]
Basic Education	0.0550	0.0355	0.0237
	[0.0397]	[0.0384]	[0.0381]
Short Tertiary Education	-0.0789***	-0.0655**	-0.0636**
	[0.0276]	[0.0267]	[0.0264]
Long Tertiary Education	-0.199***	-0.138***	-0.133***
	[0.0342]	[0.0340]	[0.0336]
IQ score		-0.0217***	-0.0211***
		[0.00377]	[0.00375]
CRT score		-0.0879***	-0.0790***
		[0.00966]	[0.00981]
Big 5: Agreeableness		0.00175	0.00197
		[0.00197]	[0.00195]
Big 5: Conscientiousness		-0.00389*	-0.00397*
		[0.00214]	[0.00211]
Big 5: Extraversion		0.00361*	0.00324*
		[0.00195]	[0.00193]
Big 5: Neuroticism		0.00164	0.00122
		[0.00186]	[0.00183]
Big 5: Openness		0.00100	0.000883
		[0.00182]	[0.00181]
# Control questions wrong			0.0187***
			[0.00526]
# Calculator clicks			-0.00686***
			[0.00121]
Observations	2042	2042	2042

Note: The Table shows marginal effects based on logit estimates using misperception (at least on wrong answer to the misperception questions) as dependent variable. Independent variables are a dummy for the *Take* treatment, a gender dummy *Female*, the *Age* of the participants, age squared, *Age^2* (divided by 1000). The *Basic Education* category contains those with primary education only, *Short Tertiary Education* those with tertiary education up to 4 years and *Long Tertiary Education* those with a tertiary education of at least 4 years. *IQ score* is the number of correct answers (0-20) to the IQ test and *CRT-score* is the score on the cognitive reflection test. The *Big 5* variables each give a score between 0 and 48 for each of the give personality dimensions.*# Control questions wrong* indicates how many times the subject typed a wrong answer to any of the control questions and *# Calculator clicks* describes the number times the subject used the profit calculator. The figures reported are marginal effects, with corresponding standard errors are given in brackets.; * denotes significance at 10 percent, ** at 5 percent, *** at 1 percent.

A potential concern with our measure of misperception is that there is misperception concerning the misperception questions themselves. To address this issue, we ran a follow-up laboratory experiment at the University of Copenhagen, Department of Economics in November 2010. In this experiment, we tested the sensitivity of our results to a variation in the wording of the misperception questions. The subjects in one treatment had an exact copy of the misperception test from the main experiment, whereas the subjects in another treatment had an alternatively worded misperception test.[15] The difference in misperception between the give and take frames was approximately of the same magnitude in the laboratory, as it was in the main experiment, not splitting on the wording (about 10 percentage points more correct answers in the take frame than in the give frame). Additionally, more subjects answered correctly with the original wording than with the alternative wording (65% versus 48%, pooled over the frames). The laboratory results suggest that the wording we used in the main experiment minimizes the misperception relative to the alternative wording and we therefore have little reason to suspect that our measure does not reflect subjects' misperceptions.[16]

Do framing effects on misperception influence framing effects in the strategy game?

We now look at how framing affects our subjects' contribution strategies in the strategy game and how misperception influences this result. The main advantage of the strategy game is that the elicited contribution profiles do not depend on beliefs about other subjects' contributions. Thus,

[15] To illustrate the wording applied in the follow-up experiment, the two different wordings in the give frame (of the first question) are reprinted here: *The original wording*: A person, who only cares about own earnings and believes that the others put 0 DDK in the common pool on average, will choose to put [answer] DDK in the common pool. *The alternative wording*: When I think the others on average put 0 DDK in the common pool, my own income is maximized when I put [answer] DDK in the common pool.

[16] It should be noted that this follow-up experiment used a sample of university students who may behave differently from the more heterogeneous sample in the internet experiment. However, in our setting, we believe treatment effects on misperception measured in a laboratory sample give a conservative estimate of the treatment effects in the general population, as students are typically more familiar with reading and processing new information.

we obtain a measure of the subjects' cooperation preferences. Inspired by Fischbacher et al. (2001), we categorize the stated profiles into two groups:

> *Free riders*: Subjects who always choose a 0 contribution in their conditional contribution profile, irrespective of other group members' contributions.
> *Conditional cooperators*: Subjects whose conditional contributions are positively affected by other group members' contributions.[17]
> *Rest:* All remaining subjects who do not meet the criteria for being categorized as free riders or conditional cooperators.

The idea behind the categorization of Fischbacher et al. (2001) is that subjects in the free rider group appear not to derive any utility from (partially) matching cooperative behavior exhibited by other group members, whereas this seems to be the case for conditional cooperators. Our focus here is not on a specific aspect of the underlying behavioral differences, but more generally on whether misperception is important for framing effects on such differences. For this, the well established and widely used aggregated categorization developed by Fischbacher et al. seems to be a natural choice.

The distributions of subjects over this categorization are listed in *Table 3*. In the first two columns of the Table, we see the distributions of subjects completing the experiment under the two frames. The next two columns present the distributions that result when deleting the subjects who, according to our test, have some form of misperception about how to implement goals (i.e. the last two distributions characterize the subsets of subjects who, according to the third row in Table 1, understood how to implement both the goal of maximizing personal income and the goal of maximizing group income).

[17] More precisely, subjects are defined as conditional cooperators when there is a significant (at the 10 percent level) positive spearman rank correlation between subjects' conditional contributions and the conditioning average contribution of other group members.

Table 3 – Distribution of types in Strategy game

	Full sample (All subjects, n=2042)		**Subset without misperception** (subjects with misperception deleted, n=1060)	
Type	**Give** (n=1366)	**Take** (n=676)	**Give** (n=664)	**Take** (n=396)
Free riders	15%	21%	28%	31%
Conditional cooperators	68%	56%	58%	54%
Rest	17%	23%	14%	15%
1. Chi2 test:	p= 0.000 (n=1366, n=676)		p=0.359 (n=664, n=396)	
2. Sample-size adjusted Chi2 test:	p=0.0002 (n=664, n=396)			
3. Chi2 test, comparing the full sample to the subset without misperception within same frame:	p=0.0000 (n=1366, n=664)		p=0.0001 (n=676, n=396)	

Note: Chi2 test refers to Pearson's Chi2 asymptotic test.

Comparing the first two columns of Table 3 (in which distributions include subjects with misperception), we see that the distributions of contribution strategies differ substantially between the frames. The main difference is that the take frame induces more subjects to choose the free rider strategy and less to choose conditional cooperation strategies, compared to the give frame.

The next two columns of Table 3 present the corresponding distributions of contribution strategies after deleting subjects who, according to our test, exhibit some form of misperception. Comparing the first with the last two sets of distributions (in which subjects with misperception have been deleted), we see that controlling for misperception dramatically affects the distributions. When controlling for misperception, the proportion of free riders increases in both frames and the proportion of conditional cooperators is reduced (statistically the differences between the two give distributions and the two take distributions are in both cases highly significant, as indicated by the Chi2 tests reported in the last row of Table 3). Furthermore, the framing effect on free riders and conditional cooperators is reduced substantially (two thirds and

half of the original magnitude, respectively). Statistically, the observed differences between the two distributions for 'all subjects' are highly significant by the standard Pearson Chi2 test (the first two reported test statistics in Table 2) and when deleting subjects who misperceive, the distributions no longer differ significantly between frames. However, when we remove observations by deleting the subjects that misperceive, we also systematically reduce the power of the Chi2 test. To check whether the reduction in test power is driving the difference in Pearson Chi2 significances, we also re-calculated the Pearson Chi2 significance for the 'Full sample' distributions with scaled-down sample size corresponding to the size of the sub-sample without misperception (the second chi square test 'all subjects' in Table 2).[18] This adjusted statistic is also highly significant and so we conclude that the shift in test significance is due to the smaller differences between the distributions that we observe, rather than to a decrease in test power.

The main reason for the strong effect of removing subjects who misperceive is that more conditional cooperators in the original sample misperceive how to implement the free rider strategy in the give frame (58%), than in the take frame (44%). Had they not suffered from this misperception, some of them may have chosen the free riding strategy. In comparison, only a little over 10% of the subjects who chose a free riding strategy misperceive how to implement the strategy with no difference between frames.

Though the framing differences remaining after we control for misperception are not significant for the categorization we have presented, or for various alternatives we have investigated[19], we cannot rule out that some framing effects may still remain. However, it is apparent that a substantial part of the framing effects that we find in our original subject sample on conditional

[18] Thus, here we base the Pearson chi square on samples with the same *relative* distribution of cooperation types as in the original sample, but with a sample size equal to the size of the sample remaining after the misperceiving subjects have been eliminated.

[19] We have performed the analysis for a number of more detailed categorizations (with free riders, conditional cooperators, perfect conditional cooperators, triangle cooperators, negative conditional cooperators, unconditional cooperators) with the same result.

contributions can be attributed to framing effects on misperception. We conclude that most of the framing effect that we see in our study should be interpreted as a change in the amount of mistakes made by subjects, rather than as a change in their preferences, which is the alternative interpretation of framing effects suggested by Közegi and Rabin (2008).

Do framing effects on misperception influence framing effects in the standard PG game?

Up to now, we have focused on the strategy-game, which has the main advantage of disentangling preferences for cooperation from beliefs about other group-members' contributions. We now present results for the standard public good game, in which the same subjects give unconditional contributions to the public good. The one-shot PG game captures many real-life situation were cooperative decisions in relation to certain people are only made once (traffic, nightlife etc.). Moreover, the one-shot game also has the analytical advantage of eliminating all dynamic reputational effects which might occur in repeated interactions.

Table 4 presents the distributions of unconditional contributions made by subjects in the initial standard one-shot PG game (proportions of subjects whose contributions to the PG fell within 0-10, 11-40 and 41-50 DDK respectively and the distributions mean contribution). The first and second columns in Table 2 present the distributions for all subjects in the give and take framing, respectively, while the third and fourth columns present distributions for subsets in which the subjects who misperceived were deleted.

Looking at the full sample, we see that even though mean contributions only differ slightly between frames, the distributions differ substantially. There is a larger proportion of subjects who give very low amounts (≤10), as well as a substantially larger proportion of subjects who give very high contributions (>40) in the take treatment (all differences being significant).

Table 4: Unconditional contributions by treatment

	Full sample (All subjects, n=2042)			**Subset without misperception** (Subjects with misperception deleted, n=1060)		
Contribution between	**Give** (n=1366)	**Take** (n=676)	**P-value***	**Give** (n=664)	**Take** (n=396)	**P-value***
0-10	10%	14%	0.002	13%	14%	0.594
11-40	52%	34%	0.000	48%	30%	0.000
41-50	38%	52%	0.000	40%	56%	0.000
Mean contribution	34.8	35.5	0.016	34.4	36.4	0.004

*P-values refer to the Pearson's chi2 asymptotic test for the top three rows and the Mann-Whitney test for the last row. The Pearson's chi2 tests compare each group of unconditional contributions to the remaining two groups, considered as one group. The chi test results are not sensitive to the shown division of the unconditional contributions; this has been verified by various alternative divisions.

When we restrict the sample to the participants who passed the misperception test in the same way as above, the framing difference in the low contributions interval is virtually eliminated (the slight remaining difference between frames is no longer significant). On the other hand, removing the misperceiving subjects does not reduce the framing effect among higher contribution intervals, if anything they are magnified somewhat. On an aggregated level, the difference in average contributions increases slightly after controlling for misperception.

When comparing with the results on contribution strategies (reported in Table 3), we note that the subjects presumably arrive at the unconditional contributions reported in Table 4 by combining their contribution strategy with their beliefs about other group members' contributions. Thus, framing effects on unconditional contributions 'aggregate' the framing effects on contribution strategies, with framing effects on beliefs making a direct comparison difficult. However, the subjects with a free rider contribution strategy are characterized by not contributing, regardless of their beliefs about other group members contributions. Since a substantial part of the very low contributions in Table 3 are from subjects with free rider strategies, we would expect to find a similar effect, as above, when controlling for misperception. This is, in fact, what we see. When we remove the misperceiving subjects, the larger proportion of low contributions in the take frame disappears, which mirrors the equalization of the proportions of free riding strategies that we see in Table 3. For contributions

over 10 (presumably dominated by subjects with a conditional cooperation strategy), controlling for misperception does not reduce framing effects, which is in contrast to the above results. One reason could be that there actually are some framing effects within the conditional cooperation strategy category after controlling for misperception that are not captured in the strategy game investigation above. Another reason for the difference could be framing effects on beliefs. Since most subjects in this part of the distribution are conditional cooperators where beliefs about other subjects' contributions are important for their choice of contribution, the latter seems a likely explanation. Even if there were no framing effects on contribution strategies after controlling for misperception, contributions could still differ if there are remaining framing effects on beliefs.

Taken together, the two sets of results are consistent in the part of the distribution where this can be tested. The differences in results for the remaining part of the distribution suggest that, while framing effects on contribution strategies may be small after controlling for misperception, there are important remaining framing effects on subjects' beliefs about other group members' contributions. In conclusion, it is again apparent that a substantial part of the framing effects that we find in our original sample of contributions can be attributed to framing effects on misperception.

3.2 Understanding framing effects on misperception

The existence of substantial framing effects on misperceptions, even though the two versions of the game are logically identical and objectively equally demanding, is somewhat surprising and calls for an explanation. In this section, we argue that the lower rate of misperception in the take frame is caused by participants exerting more effort to understand the game in the take frame. Before spelling out the details of this argument, we will consider two other potential explanations, which, based on our data, we find less likely. First, we note that the different degrees of misperception between frames are most probably not driven by the take frame being cognitively easier to understand. If this was the case, we would expect fewer people to drop out at the control questions in the take frame, but we observe the opposite. Table 5 summarizes the number of subjects who dropped out at various stages of the experiment. During the initial information and background questions, about 13% dropped out in both frames and during the 'rest of the experiment' after control questions there was again no significant difference in drop out (8-9%). However, during the screens with control questions, which tested whether the

subjects had understood instructions, 12% dropped out in the give frame and 16% dropped out in the take frame. Thus, there is a substantial and significant difference in drop out the first time that subjects were required to make an effort to understand the game that they were going to play. Subjects could return and re-read instructions and make several attempts to answer. This finding indicates that the lower degree of misperception in the take frame cannot be explained solely by the take frame begin easier to understand.

Table 5. Drop out and perception by frame.

Stage of the experiment	Give	Take
Information/Background	249	136
	13%	13%
Control questions	246	174
	12%	16%
Rest of the experiment	166	94
	8%	9%
Completed the experiment	1,366	676
	67%	63%

A second possible explanation that is natural to considered is that the specific control questions we ask in the take frame are harder to answer than those we ask in the give frame (even though the frames as such are equally cognitive demanding). This would induce the observed larger dropout rate in the take frame. Since the increase in the dropout rate would likely be greater among less capable subjects this would also explain why there is less misperception among the remaining subjects who complete the take frame. However, two observations contradict this explanation. *First*, if the control questions in the take treatment are more difficult, and this introduces a selection bias, we would expect a final sample with higher cognitive skills in the take treatment. Yet, the characteristics of the final sample displayed in Table 6 reveal no significant differences between frames with respect to our cognitive ability measures: IQ score

and CRT score.[20] *Second*, if harder control questions were the explanation behind the higher level of drop outs in the take frame, we would expect more subjects to answer the pre-game control questions incorrectly in the take treatment, which is not supported by the data.[21] Moreover, we observe that the vast majority of the subjects who dropped out at the control question stage did not even try to answer the questions.[22] This suggests that they dropped out not because of the difficulty of the questions, but because they wished to leave the experiment for other reasons.

[20] This suggests that the differences in the attrition process between the two frames had a reticent influence on the composition of the final sample in terms of inherent characteristics. This conclusion is consistent with our findings in the follow-up laboratory experiment. In this experiment, subjects could not drop out and we still found a framing effect on misperception of similar size. However, the support that this finding gives to our conclusion should not be over interpreted, because the number of subjects in the lab experiment was small. Still, there is nothing in these results that indicates that there are important differences in attrition between frames in the main experiment. In addition, it seems implausible that the 3 percentage point difference in dropout rates could explain the difference in game form recognition.

[21] Mann-Whitney tests using each subject's total number of wrong answers to the control questions as observations give p-values>0.4 irrespective of whether we use subjects who completed the experiment, subjects who dropped out during the control questions, or the full sample including drop outs and completers. Similar results are also obtained if we instead look at the number of times they used the calculator.

[22] 78% of the subjects who dropped out did not try to provide an answer.

Table 6. Characteristics of the final sample by treatment.

	Standard		Take	
Variable	Mean	Standard deviation	Mean	Standard deviation
Age	45.77	14.6	45.84	14.31
Female	0.49	0.5	0.47	0.5
CRT Score	1.46	1.11	1.5	1.07
IQ Score	8.73	3.18	8.72	3.09
Big 5: Agreeableness	32.23	5.67	32.46	5.54
Big 5: Conscientiousness	33.04	5.56	32.53	5.7
Big 5: Extraversion	30.41	6.47	30.58	6.2
Big 5: Neuroticism	19.32	7.07	19.25	7.05
Big 5: Openness	27.09	6.23	27.12	6.14

Our preferred explanation is instead that it is less pleasant to do the wrong thing in a game where you are asked to take money away from others, rather than giving to a common good. Doing the wrong thing could here most naturally be interpreted as some form of norm-violating behavior, such as taking more than what others expect from the common pool in the take treatment. More specifically, if subjects perceive the loss in benefit from performing badly in this sense (or the benefit from performing well) to be greater in the take frame, it will create a stronger incentive to exert effort in order to understand the game.[23] The resulting difference in effort levels would then naturally result in different levels of misperception about which contribution strategies implement their goals. More specifically, one could imagine that the subject's effort investment decision depends on the perceived utility difference between playing the game when understanding it, or when misperceiving it. The higher this difference is, the higher the incentive

[23] In the research area of risky decisions, Gonzalez et al. (2005) find neuro-economic evidence of a cognitive trade-off between finding the 'right' decision and the cognitive effort it requires. Interestingly for our suggested explanation of misperception, they find that this trade-off is affected by framing.

to invest effort. Let *G* denote the benefits from playing with a correct understanding of the game and *g* the benefits from playing with a misperception; then what matters for effort choices is the difference between understanding and misperceiving, i.e. *G-g*. Thus, the larger drop out in the take frame could be explained by perceived benefits in either state being smaller in the take frame than in give frame. However, if *G* was smaller in the take frame, then the allocated effort in the take frame would be smaller and so would the probability of correctly perceiving for all subjects. In fact, we see the opposite. This is, on the other hand, consistent with a lower *g*. A lower *g* would cause a greater drop out in the take frame and, since this increases the incentive to allocate effort for those who do not drop out, this could result in a larger proportion of the subjects perceiving correctly. Thus, this explanation suggests that an important cause of the framing effect on misperception is that subjects perceive a lower benefit if they misperceive in the take frame than in the give frame.

This argument is compatible with our data regarding dropout rates and misperception in the two treatments, as well as a number of other aspects of our data. *First*, there is a positive relationship between the time participants spent reading the instructions and being in the take frame.[24] *Second*, the framing effect on misperception is driven by the effect on misperception of how to maximize personal economic outcome (see Table 1). There is no corresponding framing effect with respect to maximizing the group outcome. Since it is presumably less attractive to be a free rider by mistake than to be a cooperator by mistake, this fact is consistent with the explanation that subjects in the take frame provided more effort in understanding the game, in particular in the dimension which has the greatest potential of being unattractive. *Third*, a comparison of the distributions of the big 5 conscientiousness scores in Table 6, indicates that the extra drop out in the take frame is among relatively more conscientious subjects (the difference in conscientiousness scores between frames is significant at the 5% level using the Mann-Whitney test). This finding is consistent with conscientious participants experiencing a lower benefit of participating in take while misperceiving (i.e. *g* in take is smaller for conscientious subjects). We

[24] Table A1 in the supplementary material document presents results from a regression on time spent on the instructions.

find this plausible and believe it to be caused by conscientious subjects being more sensitive than non-conscientious to misperceiving the structure of the game in a setting where they are asked to take money. To explore the link between conscientiousness and misperception further, we re-estimate the models of Table 2 for each of the treatments separately. These estimation results are presented in Table A2 of the supplementary material document. We observe that the relationship between misperception and conscientiousness is weaker in the give frame than the take frame. In fact, we only find a significant (and negative) relationship between conscientiousness and misperception probability in the take frame. This is consistent with conscientious subjects exhibiting a lower g, than non-conscientious subjects in the take frame, but that this difference in g is smaller in the give frame. Again, we believe this supports the interpretation that the framing induced differences in misperception that we observe is driven by the fact that the take frame encourages subjects to invest more effort in understanding the game. Another way to look at this issue is to include an interaction term between conscientiousness and the take dummy in the specifications of Table 2. The corresponding marginal effect of take on the probability of misperceiving is plotted over the range of conscientiousness scores in Figure 1 below. In other words, Figure 1 plots the sum of the take frame dummy and the interaction effect (between conscientiousness and the take frame dummy), as a function of subjects' conscientiousness scores. In line with the previous analysis, the figure suggests that the misperception difference between frames is greater for those who score high on conscientiousness.

To sum up, we believe it is most likely that the difference in misperception between frames is driven by subjects investing less effort in understanding the game in the give treatment. This explanation is consistent with subjects investing more time to read the instructions in the take treatment, and appears to in line with the observed relationships between misperception, dropout rates and conscientiousness that we analyzed above.

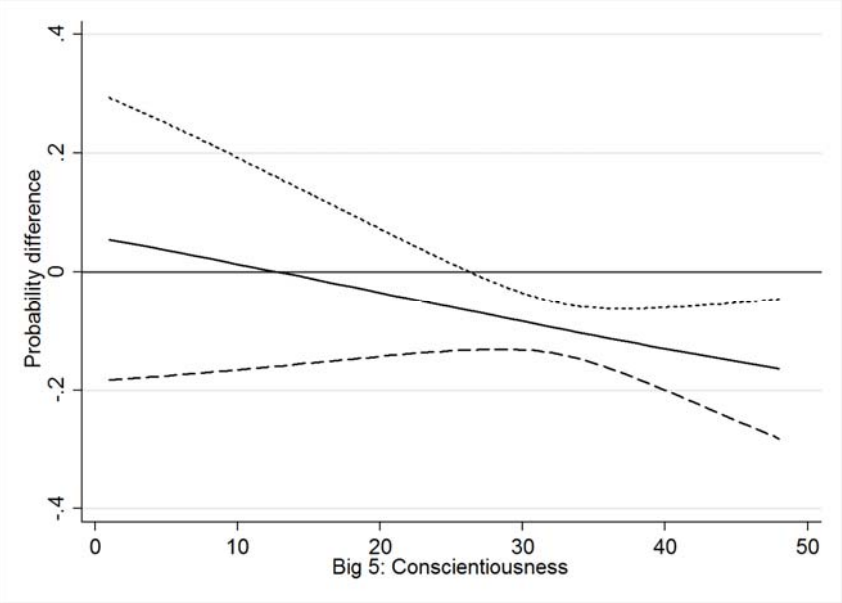

Figure 1. The marginal effects of take on misperception probabilities. Dashed lines indicate 95% confidence intervals.

3. Concluding discussion

We find a lot of misperception about which contribution strategies maximize personal and group outcome. Roughly half of our subjects have some degree of misperception about how to implement these specified goals. This result is consistent with the results of Andreoni (1995a) and Houser and Kurzban (2002) and suggests that misperception in public good games is extensive and that observed behavior does not necessarily reflect intended goals. Specifically, if we interpreted our results as reflecting intended goals without controlling for misperception, we would underestimate the proportion of free riders by over 10 percentage points and correspondingly overestimate the proportion of conditional cooperators. This is in itself an important result, since the extent to which subjects are conditional cooperators and not free riders, has been a focus of considerable interest in the behavioral and experimental literature (Fischbacher et al. 2001, Keser and van Winden 2000, Herrmann and Thöni 2009, Fischbacher and Gächter 2010).

Our main result is, however, that there is a dramatic framing effect on misperception about how to maximize personal income. Whereas 68% of the subjects in the take treatment understood

how to maximize personal outcome, this was only true for 53% of the subjects in the give frame. On the other hand, there is almost no framing effect on misperception about how to maximize group outcome.

We also find that these differences in misperception between frames have a substantial effect on the differences in contributions we see between frames. There is a substantial difference between the distributions of cooperation strategies for the two frames. This difference, however, becomes insignificant when we control for misperception. We also find – albeit to lesser degree - that controlling for misperception is important for framing effects on unconditional contributions in the one-shot public good game.

Our results imply that it is important to be aware of misperception when trying to identify and explain framing effects. Our study also shows that varying the framing of a 'simple' stylized choice situation may cause substantial changes in the 'optimizing mistakes' that people make. This suggests that using the rational choice assumption to make inferences about preferences from data may be problematic even in 'simple' choice situations, something which economists often do.

Finally, our results suggest a more general relevance of models of choice situations and empirical investigation strategies that allow/control for 'optimizing error' as suggested by, e.g. Közegi and Rabin (2008). For example, charges, fines, subsidies and tax systems are often complicated by progression, deductions and other complexities in order to address distributional considerations. As a number of prior studies have shown this may result in misperception of the resulting incentives. However, our results suggest that economically identical framings of a given tax or charge system could potentially result in substantial differences in the generated degree of misperception. Thus, there may be a substantial potential for welfare improvement if policymakers were to systematically consider how complex tax and charge systems could be framed so as to reduce misperception.Our results are also related to Chetty et al. (2009) who present results from a field experiment in a grocery store, where consumer demand decreased significantly when goods were presented with tax-inclusive price tags. Chetty et al. (2009) find that the additional tax information affects behavior and they suggest that this is explained by increased salience of the tax payment that a purchase implies. Our results suggest that the explanation of effects like those observed by Chetty et al. (2009) may go beyond issues of

salience and extend in to quite subtle framing differences and how this affects misperceptions in situations that do not otherwise differ with respect to information provision or complexity.

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5. Appendix

See separate document with supplementary material.