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## Understanding the Nature of Cooperation Variability

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Working Paper 2013:6

Department of Economics  
School of Economics and Management

# Understanding the Nature of Cooperation Variability

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March 2013



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# Understanding the Nature of Cooperation Variability

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Abstract:

We investigate framing effects in a large-scale public good experiment. We measure indicators of explanations previously proposed in the literature, which when combined with the large sample, enable us to estimate a structural model of framing effects. The model captures potential causal effects and the behavioral heterogeneity of cooperation variability. We find that framing only has a small effect on the average level of cooperation but a substantial effect on behavioral heterogeneity and we show that this can be explained almost exclusively by a corresponding change in the heterogeneity of beliefs about other subjects' behavior. Preferences are on the other hand stable between frames.

Keywords: Framing, Public Goods, Internet Experiment, Simulation.

JEL codes: C13, C71, C93, H41

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## 1. Introduction

Cooperation between people is decisive for human welfare, but also a malleable endeavor in which the reasons for success or failure can be elusive. A stylized finding from economic experiments is that cooperation is situational in that people's behavior responds to economically irrelevant changes in the description of the decision situation (see for instance: Andreoni 1995a, Sonnemans et al. 1998, Park 2000, Cubitt et al. 2011a, Dufwenberg et al. 2011, Cubitt et al. 2011b, Fosgaard et al. 2011, Ellingsen et al. 2012, and Cappelen et al. 2013). Such insights might help to explain seemingly inconsistent phenomena such as why the same person can be very cooperative in teamwork at the workplace, but uncooperative when driving home from work.

An important puzzle in this literature is what drives the variability of cooperation. The exploration of this question has implications for several deep-rooted issues in economics. One such issue is whether human preferences are robust to contextual changes (Camerer and Thaler 2005, Levitt and List 2007), while another is the importance of changes in beliefs about other people's behavior (Nyarko and Schotter 2002, Dufwenberg and Battigalli 2009, Fischbacher and Gächter 2010). Finally, it also touches upon the relation between limited cognition and behavior (Köszegi and Rabin 2008a, Köszegi and Rabin 2008b, Bernheim and Rangel 2009, Choe et al. 2009). The connection between these issues and cooperation has been a lively research topic during recent decades. However, one limitation of the previous studies is however that they typically study one determinant at a time. Several studies have investigated to what extent preferences are context dependent (Brewer and Kramer 1986, McCusker and Carnevale 1995, Weber et al. 2004, Goerg and Walkowitz 2010, Iturbe-Ormaetxe et al. 2011). Another strand of the literature has focused on how context influences beliefs about others' behavior (Sonnemans et al. 1998, Dufwenberg et al. 2011 and Ellingsen et al. 2012), and while yet another set of studies have explored context-specific perceptions of the incentive structure (Ferarro and Volsler 2010, Fosgaard et al. 2011).

Our goal is to evaluate the relative importance of *all of the determinants* previously documented as being important, *within the same study* to determine their relative importance. We do this by conducting a large-scale experiment which measures cooperation in public good games in two distinct, but economically equivalent, contexts. More specifically, we investigate

the effect of framing the cooperation decision as *taking* from a public good vs. *giving* to a public good (Andreoni 1995a). We measure the level of cooperation along with the main determinants: preferences, beliefs, and perception. With this data in hand, we are able to identify and estimate a structural model that allows us to decompose the framing effect into parts which are explained by each of the determinants.

Our estimation results bring new insights about the nature of framing effects. We find that changes in beliefs about others' behavior are the main determinant of framing effects. However, the belief effect is not visible in the summary statistics, which only indicate a small framing effect on mean contributions. Our estimation strategy reveals that the belief effect is counteracted by an equally sized unexplained effect. Hence, one important message of our study is that framing may substantially affect behavior even though it is not visible in the summary statistics of the final outcome. Moreover, we find that changes in cooperation preferences and incentive perceptions have negligible effects.

Another finding of our study is that the relatively small average framing effect masks substantial shifts in the underlying distribution; a finding that has not been reported previously in the literature, presumably because of limited sizes and compositions of the applied subject samples. More specifically, framing has a significant effect on the variance of cooperation levels and we find that essentially all of this effect can be explained by changes in beliefs.

The remainder of this study is organized as follows. The next section introduces our conceptual model. Section 3 then outlines our experimental design. In Section 4 the model is estimated and section 5 outlines how we disentangle the determinants. The decomposition of the determinants is presented in section 6 while section 7 concludes the paper with a discussion of our findings.

## **2. Our conceptual model**

The conceptual model that we use to guide our study is illustrated in figure 1. Its core is suggested by Fischbacher and Gächter (2010). We extend their model to accommodate misperception of incentives and framing.

When cooperating about the production of a public good, Fischbacher and Gächter argue that subjects formulate a *contribution strategy* based on their conditional *cooperation*

*preferences* (arrow 1 in the figure). The contribution strategy states the subjects' preferred contribution conditional on different levels of contributions made by other subjects<sup>1</sup>. Subjects then determine their actual *contribution* to the production of a public good by combining their *contribution strategy* with their *belief* about other subjects' contributions (arrows 3 and 5 in the figure).

We extend this core model because a number of studies have found that many subjects *misperceive* the incentives to contribute to the production of a public good (Andreoni 1995b and Houser and Kurzban 2002) and a recent study by Fosgaard *et al.* (2011) shows that framing can substantially affect the level of this misperception.

Logically, a subject's *misperception* of incentives may affect both the *contribution strategy* that he thinks reflects his preferences and the *beliefs* he has about other peoples' contributions (arrows 1 and 2 in Figure 1).

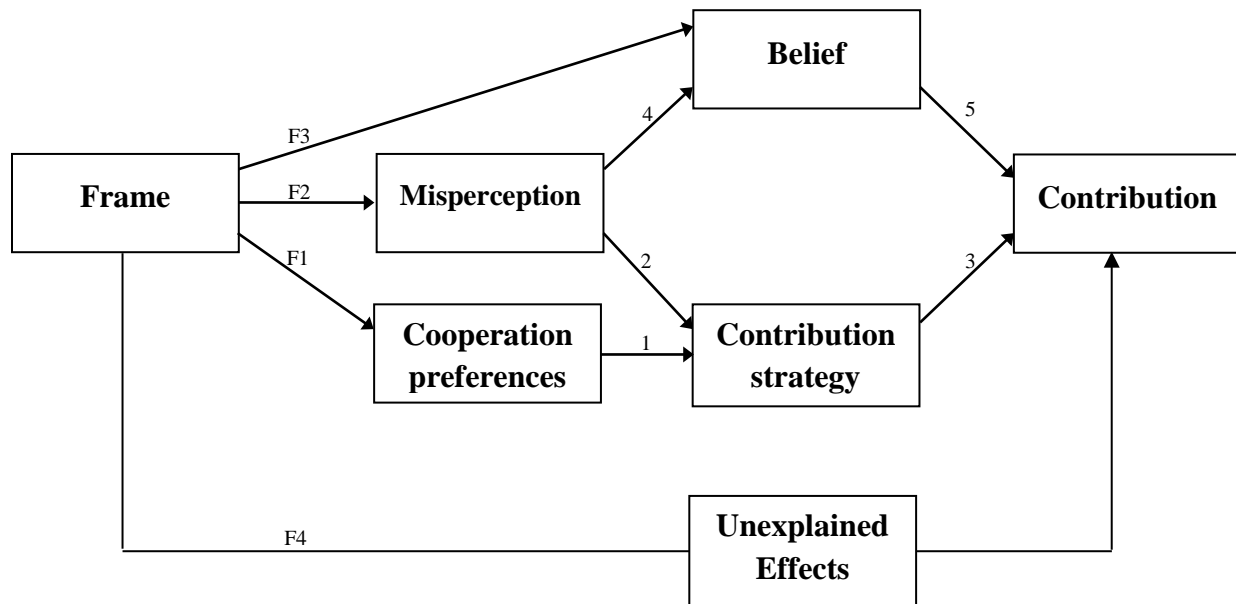


Figure 1: Framing and causal effects in our conceptual model

<sup>1</sup> The strategy indicates the subject's preferred contribution if others on average contribute nothing, if they contribute 1 dollar, etc.

Finally, our conceptual model allows for three possible gates through which framing effects can enter, which correspond to the three types of framing effect that have been suggested in prior experimental literature:

**a) *Framing effects through cooperation preferences:*** This effect reflects a shift in the subject's underlying preferences for cooperation caused by the change in framing and is captured by arrow F1. McCusker and Carnevale (1995) and Iturbe-Ormaetxe *et al.* (2011) argue that subjects have reference dependent utility and are loss averse, while van Dijk and Wilke (2000) suggest that subjects' 'focus' on personal and group outcomes may shift. If subjects have reference or 'focus' dependent utility functions (e.g. exhibit loss aversion) and these are affected by framing, we should find a significant effect (arrow 1) from framing on contribution strategies when controlling for changes in misperception.

**b) *Framing effects through beliefs:*** Sonnemans *et al.* (1998), Dufwenberg *et al.* (2011) and Ellingsen *et al.* (2012) have suggested that framing effects on peoples' beliefs are an important mechanism behind framing effects on contribution behavior. In our model, framing can directly affect beliefs (arrow F3) or this affect can go via misperception (arrow 4). Prior studies do not distinguish between these so if they turn out to be significant, it would be consistent with these studies.

**c) *Framing effects through misperception:*** Fosgaard *et al.* (2011) suggest that it is meaningful to distinguish between subjects' underlying cooperation preferences and subjects' *perception of the game* (e.g. the extent to which subjects correctly understand which contribution strategy maximizes their own income). The authors find that give/take framing has important effects on subjects' perception of the game and that this explains most of what looks like framing effects on underlying contribution preferences. Finding significant effects of misperception which work through subjects' contribution strategies would be consistent with this study (arrow 2). Here we also allow misperception to influence contributions via beliefs (arrow 4).

**d) *Unexplained framing effects:*** Finally, there could, in principal, be framing effects which work through other mechanisms than those proposed in the literature.

In conclusion, the model presented in Figure 1 incorporates elements which the literature has suggested are the core mechanisms through which framing affects contributions. As such, the model does not add any new elements, but rather it attempts to structure the existing elements and their interactions. One thing that the model does illustrate is the danger of investigating framing effects with an incomplete model. If key variables are omitted from the analysis, an estimated framing effect may become biased, because it may pick up effects which work through the omitted variables, and which are then *not* controlled for in the analysis.

The idea of our experiment (that we report on in the following section) is to generate sound indicators for the key variables in Figure 1, for a large subject pool that we randomly allocate to two different frames of the public good game. With this data, we then estimate the causal effects (the arrows) indicated in Figure 1, including the supplementary unexplained framing effect which captures framing effects that are not explained by our conceptual model.

### **3. Experimental design**

#### *General outline of the experiment*

Our artefactual field experiment was conducted over the Internet in the summer of 2008.<sup>2</sup> Initially, the Danish national bureau of statistics (Statistics Denmark) randomly selected 18,027 potential subjects from the Danish Central Person Register (CPR), which contains all individuals legally residing in Denmark. An invitation to participate in the experiment was sent to each selected potential subject via ordinary mail.<sup>3</sup> In the invitation letter, subjects were given the internet address of the experiment and a personal login code. The experiment was open for one week, and during this week, subjects could log in and out as they wished. After the experiment closed, subjects could log in again to receive feedback on the experimental results and type in their bank account number after which their earnings during the experiment were transferred to the account.

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<sup>2</sup> See [http://www.econ.ku.dk/cee/iLEE/iLEE\\_home.htm](http://www.econ.ku.dk/cee/iLEE/iLEE_home.htm) for a detailed description of the experiment platform. The platform has been used for numerous studies on different topics; see, e.g. Thöni et al. (2012).

<sup>3</sup> See the Supplementary Information Document for a translation of the invitation letter and screenshots from the experiment.



After logging onto the webpage with their personal log-in code, subjects were randomly allocated to either the ‘give to the Public Good frame’ or to the ‘take from the Public Good frame’ (Andreoni 1995a, Cubitt *et al.* 2011a, 2011b, Dufwenberg *et al.* 2011, Park 2000). Subjects first played a standard one shot public good (PG) game and then played a ‘strategy version’ of the same game (see descriptions below). Subjects were then asked about their perception of game incentives. Finally, they were asked to complete a series of cognitive ability and personality trait tests and background questions. Only the PG games and the game perception questions were framed. The remaining tests were completely identical for all subjects.

A total of 2,042 subjects completed the experiment, with 1,366 subjects in the give treatment and 676 in the take treatment.<sup>4</sup> On average subjects earned 300 Danish kroner (DKK), approximately \$60, during the course of the entire experiment (including the payments from a risk elicitation task that we do not use). Subjects spent on average 50 minutes completing the experiment.

### *Details*

Initially, subjects played a **standard** PG game, in which subjects were randomly divided into groups of four. Each subject was given control of 50 DKK ( $\approx$ \$10) and was allowed to allocate this sum freely between a contribution to a common pool (i.e. the public good) or private income reserved for one self. In the **give** frame, subjects were initially given the 50 DKK as a private endowment, and were then asked what part of this endowment they wanted to contribute to a common pool. In the **take** frame, the 50 DKK was initially allocated to the common pool and subjects were then asked how much they wanted to withdraw from the common pool and instead reserve for themselves as private income. Under both frames, the money allocated to the common pool was doubled and shared equally among all group members. Hence, subjects’ earned the amount they reserved as private income plus a quarter of the final value of the contributions to the common pool. If subjects only care about personal income, we expect everyone to contribute 0, since the marginal private return of contributing 1 DKK to the common

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<sup>4</sup> Two thirds of the participating subjects were allocated to the give treatment and the remaining one third were allocated to the take treatment. The reason for the uneven (but still random) allocation of subjects between the treatments is that the data from the give treatment have also been used for other papers, and hence these data received a higher priority.

pool is only ½ DKK. After the standard PG game, subjects were asked to state their beliefs about the average contribution of the other three group members. Subjects were rewarded for belief accuracy using the quadratic scoring rule.<sup>5</sup>

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 game developed by Fischbacher *et al.* (2001), where a profile of PG contributions conditional on different levels of contributions from other group members is elicited from each subject. Initially, subjects were divided into new groups of 4 and informed of this. Each subject was asked to make both an unconditional contribution and a profile of conditional allocations to the PG. First, the unconditional contribution was elicited in exactly the same way as in the previous standard PG. Subjects were then asked to indicate their contribution conditional on values of the three other group members' average contributions which varied from 0 to 50 DKK in steps of 5 DKK. When calculating payoffs, we used the elicited unconditional contributions for three randomly selected group members while the contribution of the fourth subject was based on the elicited conditional contribution profile using the average of the unconditional contributions from the other three group members.

Since contribution profiles are conditional on other group members' contributions, these profiles should not be affected by beliefs about the other group members' contributions. As shown by Fischbacher *et al.* (2001), the strategy method gives incentives to disclose precisely the conditional contribution profile, which underlies the unconditional contribution elicited in the standard PG game.<sup>6</sup>

Right after the strategy game, the subjects were asked incentivized control questions to test for **misperception**. We used the conditional setup introduced in the strategy game, and asked subjects to state the contribution profile they believed would maximize their personal income

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<sup>5</sup> Subjects received an additional payment in DKK of  $10 - 0.004 d^2 \geq 0$ , where  $d$  is the difference between the belief and the true value.

<sup>6</sup> 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 the 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.

and the contribution profile they believed would maximize other group members' income. The test consisted of six questions in all. It was emphasized that each question only had one correct answer and that subjects earned 5 DKK for each correct answer. The first three questions asked subjects what public good contribution they should make if they wanted to maximize their own income, when the others, on average, contributed 0 DKK (question 1), 25 DKK (question 2) and 50 DKK (question 3). In the last three questions, subjects were asked what contribution they should make if they wanted to maximize the income of the group, when the others, on average, contributed 0 DKK (question 4), 25 DKK (question 5) and 50 DKK (question 6). We interpret incorrect answers to these questions as misperceptions about how to implement the specified goals in the public good game.

#### **4. Estimating a model for decomposing framing effects**

In this section, we specify and estimate the structural model of framing, developed in Section 2, using our experimental data. In Section 6, we then use this model to decompose the total framing effect on contributions measured in our experiment into parts working through misperception, beliefs and preferences. This allows us to quantify the relative importance of these three different possible mechanisms through which framing could work and to quantify the importance of any remaining unexplained framing effect on contributions.

In the experiment, we measure the exogenous framing variable as well as 4 out of the 5 endogenous variables (misperception, contribution strategies, beliefs and contributions) introduced in the conceptual model (See the document with Supplementary Information for a detailed data description). We are not able to elicit cooperation preferences directly and so we are not able to estimate the framing effect on cooperation preferences directly.

Although we do not observe cooperation preferences, we can nevertheless estimate the framing effect which works through them (arrow F1 in Figure 1). Our conceptual model implies that any framing effect on contribution strategies, which does not go via misperception (which we observe), must go through cooperation preferences. By including a direct framing effect on contribution strategies in our estimations, we can therefore (under the assumptions of our conceptual model) interpret this as the effect of framing which works through cooperation preferences (and hence arrow F1 is solid in Figure 1).

Finally, we do not measure an indicator of unexplained framing effects directly. However, the theoretically blocked direct causal links between framing and contributions provide us with a test of our conceptual model. If we do find a significant direct effect of framing on contributions (effect F4 in Figure 1) it will indicate that the conceptual model is incomplete and that there are important unexplained mechanisms which transmit the effect of framing into contributions. We therefore include this (conceptual blocked) direct effect in the empirical model.

We also include a number of exogenous controls including cognitive ability, big five personality traits, gender, and age as explanatory variables for the estimation of all four endogenous variables.<sup>7</sup>

Formally, the empirical specification of our model is the following recursive system:

$$\text{Misperception (pe):} \quad pe = g1(fr_{F2}, x, e1)$$

$$\text{Contribution strategy (cs):} \quad cs = g2(pe_2, fr_{F1}, x, e2)$$

$$\text{Belief (bl):} \quad bl = g3(pe_4, fr_{F3}, x, e3)$$

$$\text{Contribution(cn):} \quad cn = g4(bl_4, cs_3, fr_{F4}, x, e4)$$

Where  $fr$  is a frame dummy,  $x$  is a vector of exogenous control variables and  $e1, \dots, e4$  are stochastic variables which capture the effects of unobserved exogenous variables. The subscripts to the framing indicator and other endogenous explanatory variables indicate the corresponding arrow in Figure 1. Our extensive battery of control variables includes potentially highly important causes of correlation between equations such as cognitive abilities and personality traits. When estimating the system, we assume that the stochastic variables ( $e1$ ,  $e2$ ,  $e3$  and  $e4$ ) are independent.

In the estimations, misperception ( $g1$ ) is modeled as a binary probit. Beliefs ( $g3$ ) are modeled as a multi-nominal probit.<sup>8</sup> Contribution strategies ( $g2$ ) are modeled as a two-step

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<sup>7</sup> See the Supplementary Information Document for a full description of the exogenous variables.

process where a subject's strategy type follows a multi-nominal probit and the specific profile is selected randomly among the set of observed profiles for subjects of this strategy type and with the subject's set of explanatory variables. Finally, contributions ( $g4$ ) are modeled as a multi-nominal probit<sup>9</sup> where we followed Fischbacher and Gächter (2010) and first combined belief ( $bl$ ) and contribution strategy ( $cs$ ) to generate the subjects' preferred contribution ( $pc$ ), and then used both preferred contribution and beliefs as explanatory variables in the estimation of the actual contributions. That is, by including both preferred contributions (which depend on beliefs) and beliefs in the final estimation, we allow beliefs to affect contributions beyond their role in generating the preferred contribution. Fischbacher and Gächter (2010) find clear evidence for this, and we want our empirical model to follow theirs. Thus, this equation becomes:

$$\text{Contribution: } \quad cn \quad = \quad g4(pc, bl_4, fr_{F4}, x, e4) \quad \text{where} \quad pc=f(bl_4, cs_3)$$

The preferred contribution,  $pc=f(bl_4, cs_3)$ , is found as the contribution indicated in the subject's contribution strategy that corresponds to his stated belief about the contributions of others.

We include first-order effects of controls and both first and second-order effects of endogenous explanatory variables (i.e. we include squared endogenous variables as regressors in all probit models). These specifications are quite flexible and do not impose restrictive functional relationships on the key endogenous variables. Further, in addition to the linear functional form assumed by Fischbacher and Gächter (2010) for the contribution equation, a large class of other functional forms is accommodated. A detailed description of the empirical specification of the four equations is found in the Supplementary Information Document, Section 3.

## 5. Simulating framing effects using the estimated model

Because we have access to a large dataset, we are able to estimate our structural model of the underlying relationships without restrictive assumptions about the functional form. This makes it possible for us to use the model to simulate behavior with reasonable precision.

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<sup>8</sup> Beliefs are in practice categorized since almost all subjects report beliefs that are divisible by 5 and with some values attracting a large proportion of reported beliefs. The few observations that were not a multiple of 5 (around 2% of the observations) were rounded to the nearest 5 kroner.

<sup>9</sup> Like beliefs, contributions were in practice rounded to the nearest multiple of 5.

When simulating, we first use the estimated parameters for the misperception equation  $g1(\cdot)$  to calculate the probability of misperceiving, when a subject with a specific set of personality traits, cognitive abilities, gender, and age is placed in a given frame. For our simulations, we then select the specific realization of the misperception variable through a random draw between the two possible realizations which reflect their calculated probabilities. Using the simulated realization of the subject's misperception variable, we then simulate the subject's belief about others' contributions. Using the estimated parameters for the belief equation  $g3(\cdot)$ , we calculate the probability of each possible value of this belief for a subject with the given combination of personal characteristics, frame and the simulated value of the misperception variable. The realized belief is again chosen through a random draw among the possible realizations which reflect the calculated probabilities. The subject's contribution strategy is simulated in basically the same way. First, the subject's strategy type is simulated using the estimated multinomial probit parameters and a random draw in the same way that beliefs are simulated. Then the specific profile is selected randomly among the set of observed profiles for subjects of this strategy type and with the same frame and the simulated value of the misperception variable. Finally, the simulated contribution strategy and belief are combined to find the preferred contribution, which together with the framing variable, beliefs and controls are used to simulate the subject's contribution (for more details on the simulation procedure see the Supplementary Information Document, Section 4).

Before using the estimated structural model to simulate how subjects react to a change in frame (in the following section), we tested the estimated model's ability to reproduce the original experimental data in a number of ways. We did this by comparing the distributions of contributions in the experiment, with the distribution simulated by our model. A comprehensive description of the tests and results can be found in the Supplementary Information Document, Section 5. We do not find significant differences between simulated and original distributions in any of our tests and we conclude that our model captures the systematic variation in our data well.

## **6. Disentangling framing effects**

In this section, we decompose the observed framing effect on contributions into framing effects which work through misperception, preferences, beliefs and remaining unexplained framing

effects. We do this by simulating the change in contributions for subjects who are moved from the give frame into the take frame. By changing the frame variable for one equation at a time, we are able to simulate the marginal effect on contributions for each of the suggested paths in Figure 1.

Initially we simulate the *total framing effect* on contributions for our sample. Using the estimated coefficients of our system of equations, we first simulate the contributions for all subjects in the give frame (i.e. all frame variables set to give and all endogenous variables which enter equations are those simulated for the give frame). We then simulate the corresponding distribution of contributions when all subjects are ‘placed’ in the take frame (i.e. all frame variables set to take and all endogenous variables which enter equations are simulated for the take frame). The total simulated framing effect on contributions is then the difference between the simulated distributions for the two frames (note that both simulated distributions use all subjects). In the following, we focus on how moving from the take to the give frame affects the mean and the standard deviation of the distribution of contributions.<sup>10</sup> This simulated total framing effect on the mean of contributions is shown in Figure 6 (right bar), while the simulated total framing effect on the standard deviation of contributions is shown in Figure 7 (right bar). The simulated effects correspond almost exactly to the observed framing effects in our data (see the Supplementary Information Document, Section 1). The indicated 5% confidence intervals around the simulated effects are larger than for the observed effects (see the Supplementary Information Document, Section 1) because these take the estimation uncertainty of our model parameters into account.<sup>11</sup>

We now *decompose the total framing effect* into its four parts using the simulation model to generate marginal effects. Starting with all subjects in the take frame, we only let the framing affect one of the underlying mechanisms at a time. The four partial effects that we simulate in this way are:

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<sup>10</sup> Framing also significantly effects higher order distribution moments but the quantitatively most important effects are on the first two ditribution moments.

<sup>11</sup> Standard errors for simulated effects illustrated in figures 6 and 7 are found by the standard bootstrapping procedure (randomly drawing subjects from the original subject pool with replacement and re-estimating and resimulating 250 times. See the Supplementary Information Document for details) .

Misperception ( $F2 \rightarrow 4 \rightarrow 5$ ,  $F2 \rightarrow 2 \rightarrow 3$ )

Beliefs ( $F3 \rightarrow 5$ )

Preferences ( $F1 \rightarrow 3$ )

Unexplained framing effect ( $F4$ )

The *misperception* mechanism is made up of two sub-effects. First, misperception can affect contribution strategies, which then carry over to contributions ( $F2 \rightarrow 2 \rightarrow 3$ ). Second, misperception can affect beliefs ( $F2 \rightarrow 4 \rightarrow 5$ ). The *belief* mechanism explains the part of the total framing effect on the distribution of contributions which works directly through beliefs ( $F3 \rightarrow 5$ ), but excludes the part of the change in beliefs that is caused by a framing effect on misperception ( $F2 \rightarrow 4 \rightarrow 5$ ). The *preference* mechanism explains the part of the total framing effect which is caused by a framing induced change in basic preferences for cooperation ( $F1 \rightarrow 3$ ). Finally, the *unexplained* effect captures the part of the total framing effect that is not explained by the three modeled causal mechanisms. In Figure 2, we show the simulated partial framing effects on mean distributions and in Figure 3 the corresponding partial effects on the contribution distributions standard deviation.



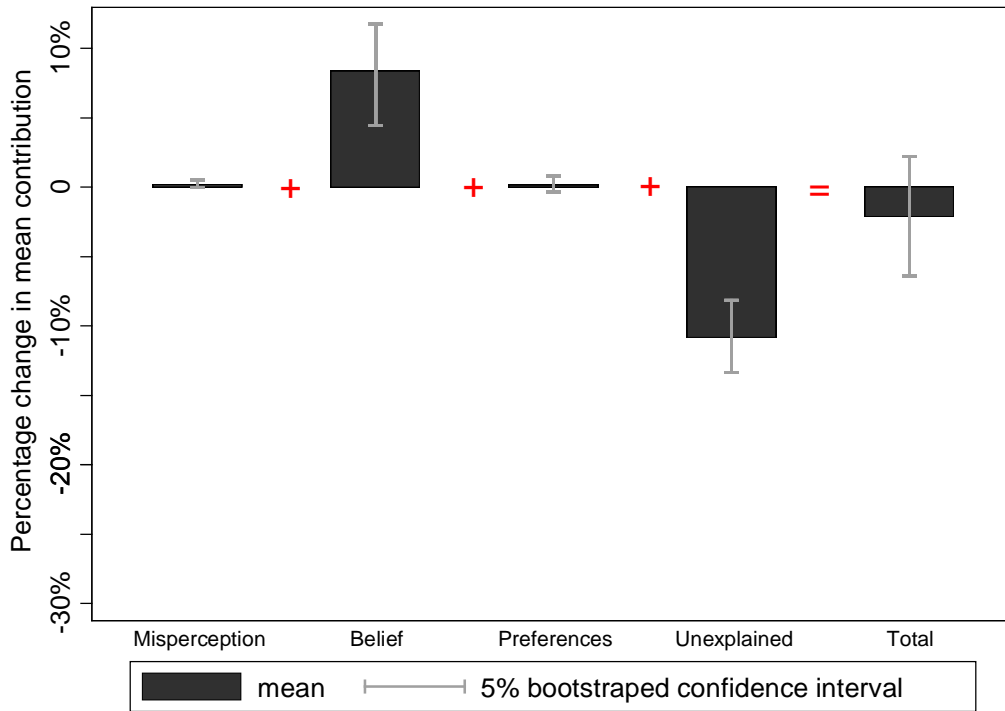


Figure 2: Simulated effects on mean contribution when moving all subjects from the take frame to the give frame

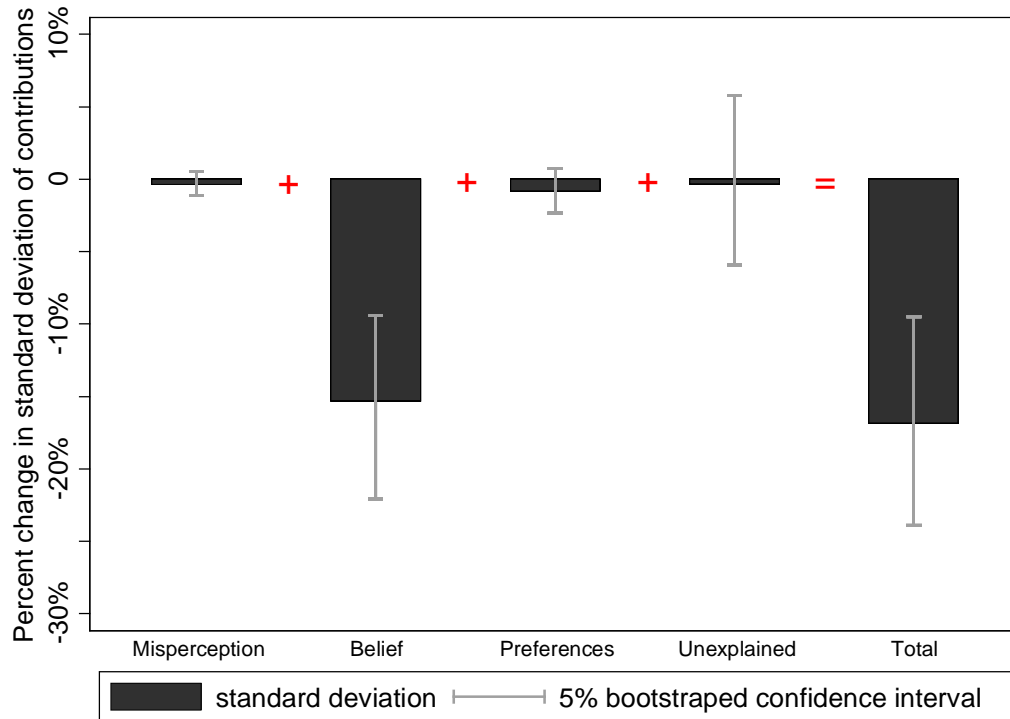


Figure 3: Simulated effects on the standard deviation of contributions when moving all subjects from the take frame to the give frame

Looking at the total framing effects in Figure 2 and 3, the first interesting observation is that a relatively small framing effect on mean contributions masks a relatively large framing effect on the standard deviation of the distributions. Thus, even though mean contributions do not change much; the subjects' contribution behavior is in fact affected substantially by framing.

Looking at the decomposed effects in Figure 2 and 3, we see that essentially no framing effects work through misperception or preferences. The estimated effects are in both cases close to zero and standard errors are quite tight around this. On the other hand, beliefs explain a substantial and significant part of the total framing effects. Thus, it seems clear that there are substantial framing effects on subjects working through beliefs, while the effects which work through misperception and preferences are negligible in comparison. This is not obvious from the outset since there are substantial framing effects on misperception and on elicited contribution strategies (see the Supplementary Information Document, section 1). In addition, Fosgaard *et al.* (2011) show that without controlling for misperceptions, more subjects give low

contributions in the take frame, which is consistent with more subjects understanding how to maximize own income in the take frame. Furthermore, they show that, when controlling for misperceptions, this framing effect is removed. Our study reveals that the importance of this mechanism for explaining the change in average contributions is small compared to the importance of the belief mechanism.

From Figure 3 it seems clear that framing effects on beliefs can explain virtually the entire framing effect on the standard deviation of the contribution distribution. The remaining unexplained effect is small and insignificant (though the confidence interval around this estimate is relatively large). Thus, the substantial reduction in belief variances in our data can explain the reduction in contribution variance when we control for other possible explanations.

Finally, looking at Figure 2, we see that though the net framing effect is small, there is a substantial framing effect through beliefs, which drives mean contributions up and a substantial unexplained framing effect, which drives mean contributions down. Thus, the increase in mean beliefs that we see as we move to the give frame has the expected effect of increasing contributions. However, a stronger *unexplained* framing effect counteracts this and ends up causing a (small) net reduction in mean contributions.

Comparing with earlier literature, our results are in line with recent studies that find important framing effects which work through beliefs (Dufwenberg *et al.* (2011), Ellingsen *et al.* (2012), and Sonnemans *et al.* (1998)). We add to this in several ways. First, we provide sound evidence that indicates that the two other proposed explanations (framing induced changes in preferences and misperception) are negligible in comparison. Our results also show that framing which works through beliefs has an important effect on the variance of the contribution distribution in addition to the effect mean contributions (which has been the sole focus of earlier investigations). Finally, we find that, in addition to beliefs, there are important unexplained framing effects on mean contributions.

These unexplained effects could be caused by incorrect specifications of the mechanisms we have modeled. Though our functional forms are flexible, in general, it is possible that, for example, the process of generating preferred contributions by combining beliefs with the strategy profile is too simple. We assume that subjects hold point beliefs about others' contributions and

choose the contribution in their profile that corresponds to this belief. In reality, few subjects are certain about what others will do and, hence, it may be more appropriate to ask subjects how they believe the contributions of others will be distributed. Using belief distributions, rather than point beliefs, would open up new ways of understanding our results. For example, if subjects feel worse about giving too little than giving too much compared to others, increased belief uncertainty could make them contribute more (to insure against giving less than others). In our case, subjects may become more certain about their beliefs as they are moved to the give frame (the reduction in contribution variance suggests this). If so, this would reduce the expected cost of giving too little and subjects would reduce their contributions. This effect could outweigh the effect of the increase in expected beliefs. Thus, the unexplained negative effect on mean contributions that we identify could be caused by subjects being more certain about what others contribute in the give frame, and thereby which action is appropriate. However, this is just speculation.<sup>12</sup> The unexplained framing effect could also be caused by mechanisms outside our model transferring framing effects to contributions. At any rate, the fact that we find two strong underlying framing effects with opposite signs fits well with the mixed results on the direction of the net framing effect in the literature. When a relatively small net effect is caused by two large opposite underlying effects of the same magnitude, small changes in setting, subject pool, etc. can cause the sign of the net effect to change.

## 7. Conclusions

In our study, we investigate the effect of changing the default state in a game of public good production. We measure indicators of the three main explanations proposed in the literature and we use a substantially larger subject pool for our experiment than prior studies. This makes it possible for us to estimate a structural model which captures all three causal effects and the behavioral heterogeneity of our subject pool. We find that framing has only a small effect on the average level of cooperation. However, we find a substantial effect of this change on behavioral

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<sup>12</sup> One way to view this is that, in the give treatment, there is less uncertainty regarding the social norms governing the choice situation. This is consistent with people spending more effort on understanding the game in the take frame which was argued in Fosgaard et. al (2011). Regarding the effects of norm uncertainty and framing, Dreber *et al.* (2012) argue that higher norm uncertainty in the generalized ultimatum game (in which the responder can only reduce the proposer's payoff by a small fraction) compared to the dictator game (in which the responder cannot affect the proposer's payoff at all) may explain why framing matters in the former game but not in the latter. Their reasoning is that framing is less influential in situations with a clearly established norm such as in the dictator game.

heterogeneity across our subjects: cooperative behavior is much more *heterogeneous* in some settings than in others and we show that this can be explained almost exclusively by a corresponding change in the heterogeneity of beliefs about other subjects' cooperative behavior. Our results suggest that it is easier for subjects to identify the behavioral norm in some settings than in others and that *this* is the main explanation for the variability in cooperation that we see in prior studies and in the real world. Preferences are on the other hand stable across contextual changes in our experiment.

Our results suggest that cooperation preferences are stable over a wider range of presentation and context changes which implies that underlying incentives are of primary importance for the long-term success of cooperation. The stability of preferences across different settings should also imply a corresponding stability of behavioral norms. However, presentation and context appear to be very important for how easy it is for people to *identify* the behavioral norm in a given setting.

Thus, our results suggest a new understanding of how context and presentation affect the success or failure of cooperation. Even if the mean level of cooperation is the same across different settings, our results imply that the *variance* in outcomes for initial rounds of the same basic cooperative venture may be substantially larger for some settings than for others. Because of the dynamics of cooperative processes that depend on conditional preferences, this can be critical. Fundamentally, sound ventures may fail in the initial rounds of cooperation, or never get started, if many (potential) participants in a given venture have overly pessimistic initial expectations of others' behavior. A greater variance in expectations will cause a larger proportion of cooperative ventures in this setting to fail in the critical upstart phase because of overly pessimistic initial expectations. Initially, this is compensated by high cooperation levels in other ventures where participants have overly optimistic expectations. But as the venture continues and expectations are updated, cooperation will stabilize at a level which reflects the underlying preferences. In the long run, those ventures that have survived the critical initial rounds will end up cooperating at the same level in both settings (reflecting cooperation preferences and the actual behavior of others). However, fewer ventures will survive in the setting in which people initially found it more difficult to identify the behavioral norm.

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# Supplementary Information

This document provides supplementary information to the paper “Understanding the Nature of Cooperation Variability” by T. Fosgaard, L. G. Hansen, and E. Wengström.

Selected screenshots can be found at <http://www.nek.lu.se/nekewe>.

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# 1 Description of our experimental data

In this section, we report the effects of framing on cooperation (measured as the chosen contribution to the public good) and on the indicators of the different underlying causes that we measure in our experiment. Figure SI-1 presents the distributions of contributions for subjects exposed to the give and the take frame. Using the Kolmogorov-Smirnov test, we conclude that the distributions are clearly different ( $p=0.000$ ) and that the give distribution of contributions exhibits substantially less variance than the take distribution (tested with Levene's robust test statistic for the equality of variances,  $p=0.0000$ ). There is also a slightly higher mean contribution level in the take treatment (35.51) compared to the give frame (34.75). Despite being small, the difference is significant ( $p=0.0163$ , two tailed Mann-Whitney test). Some prior studies of this type of framing find a framing effect in the same direction as we do (E.g. McCusker and Carnevale 1995), but most prior studies report a framing effect on mean contributions in the opposite direction.

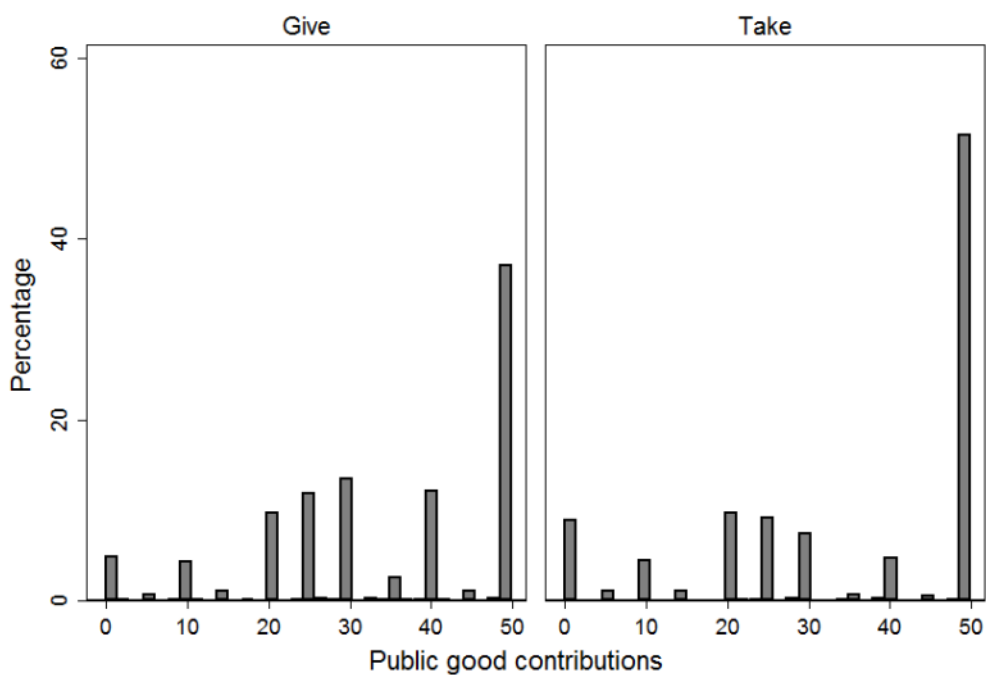


Figure SI-1: Distribution of contributions for each frame

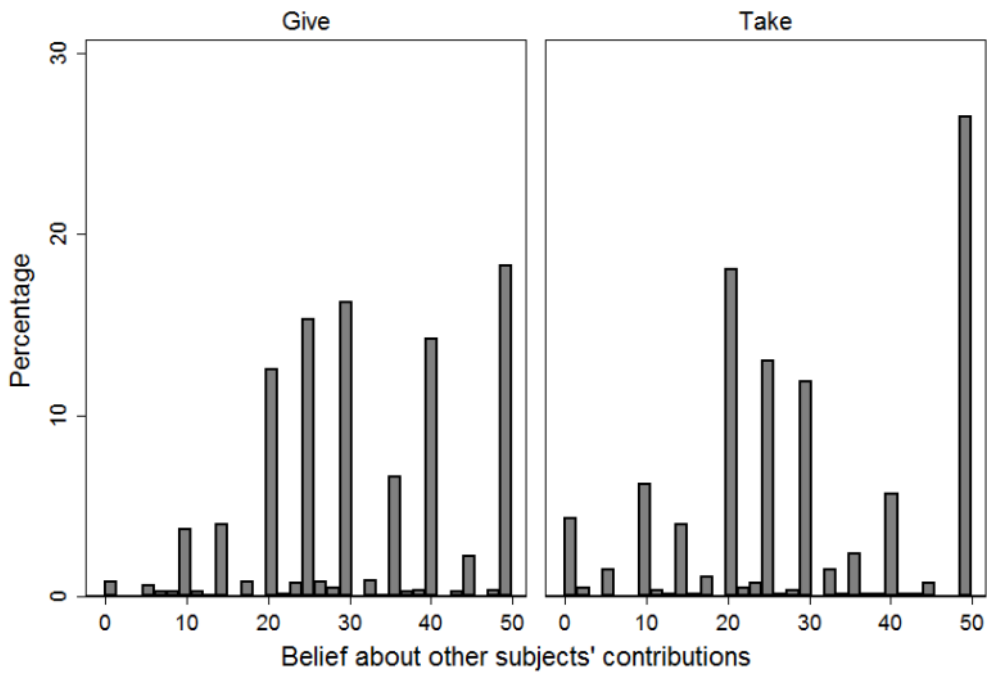


Figure SI-2: Distribution of beliefs for each frame

The subjects' distributions of **beliefs** about what other group members on average contribute are presented in Figure SI-2. These distributions also differ significantly between frames ( $p=0.000$  with the Kolmogorov-Smirnov test), with the give distribution again exhibiting substantially less variance than the take distribution (tested with Levene's robust test statistic for the equality of variances,  $p=0.0000$ ). The average belief about what others contribute is slightly *lower* in the take treatment (29.79), compared to the give frame (31.81) with the difference being clearly significant ( $p=0.0009$ , two tailed Mann-Whitney test). This direction of the framing effect on mean beliefs is also found by, e.g. Dufwenberg *et al.* 2011. Thus, in our experiment, the framing effects on mean beliefs and mean contributions go in opposite directions; going from give to take increases contributions but leads to lower beliefs. In contrast, the direction of the framing effect on the variance is the same for contributions and beliefs, with the take treatment generating a higher variance.

To obtain a summary picture of how framing affects contribution strategies we categorize our subjects into the following three groups (in the spirit of Fischbacher *et al.* 2001):

*Conditional cooperators*: subjects whose contribution strategies indicate a positive correlation between their own contribution and that of other subjects.<sup>1</sup>

*Free riders*: Subjects whose contribution strategies indicate a zero contribution irrespective of what others contribute, and

*Others*: subjects who do not fall into any of the two categories above.

The effect of framing on this categorization of contribution strategies is presented in Table SI-1.

*Table SI-1: Distribution of contribution strategies for each treatment*

	<b>Give</b> <b>(n=1366)</b>	<b>Take</b> <b>(n=676)</b>
<b>Conditional operators</b>	68%	56%
<b>Free riders</b>	15%	21%
<b>Others</b>	17%	23%
$\Sigma$	100%	100%
<b>Pearson's Chi<sup>2</sup>(2)</b>	p=0.000	

There is a substantial framing effect with less conditional cooperators in the take than in the give frame. The difference in distributions between frames is highly significant using the Pearson's chi square test (p=0.000).

*Table SI-2: Level of misperception for each treatment*

	<b>Give</b> <b>(n=1366)</b>	<b>Take</b> <b>(n=676)</b>
<b>Misperception</b>	51%	41%
<b>Correct perception</b>	49%	59%
<b>Pearson's Chi<sup>2</sup>(1)</b>	p=0.000	

<sup>1</sup> More precisely, our definition is that the contributions are monotonely increasing and the relation between the contribution of the average of other group members' contributions has a positive and significant (at 10 percent level) Spearman rank.

Table SI-2 presents the proportion of subjects who have misperceptions about how to implement the two contribution strategies we ask about. It is clear that a large proportion of subjects have such misperceptions and that there is substantially more misperception in the give frame than in the take frame ( $p=0.000$ , Pearson's chi square test).

To sum up, framing has a highly significant effect on both contributions and on our indicators of all three underlying causes or mechanisms through which this framing effect may work. Thus, off hand, it seems as if all three possible mechanisms for transmitting the effect of framing to contributions could be important. To disentangle these and evaluate their relative importance, we estimate the model developed in the paper.

## 2 Data for the estimated model

For each subject in the experiment, we measure the exogenous variable:

**Framing** variable: a dichotomous variable which indicates the frame the subject received as well as 4 out of the 5 endogenous variables.

**Misperception**: an indication of whether or not the subject misperceives how to maximize his own or the group's outcome in the public good game (See appendix section 5.4.3 for measurement details).

**Contribution strategy**: an 11 number conditional contribution table which indicates condition contributions from the strategy version of the public good game (See appendix section 5.4.2 for measurement details).

**Beliefs**: an integer between 0 and 50 which indicates the point expectation of the average contribution of the other group members (See appendix section 5.4.2 for measurement details).

**Contributions**: an integer between 0 and 50 which indicates the contribution made in the public good game (See appendix section 5.4.2 for measurement details).

We do not elicit cooperation preferences directly and so we are not able to get a direct estimate of the framing effect on cooperation preferences. In Figure SI-3, the conceptual model from the paper is reproduced with the absent preference elicitation illustrated as a broken box around the cooperation preferences.

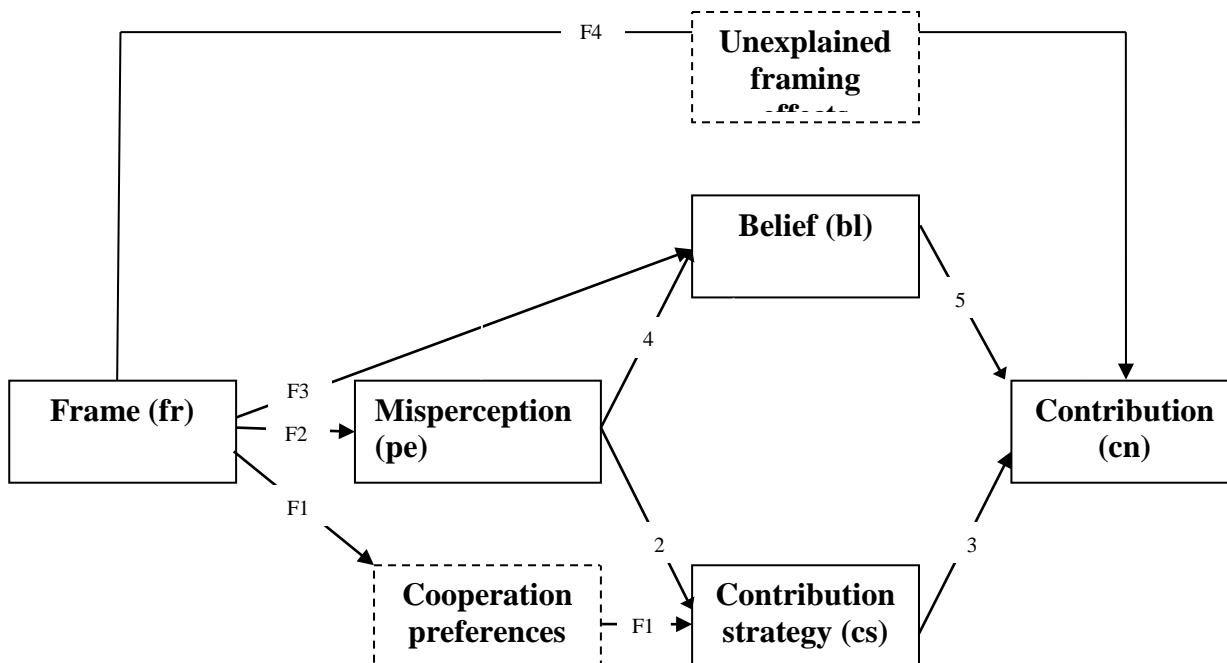


FIGURE SI-3: THE EMPIRICAL MODEL AND ESTIMATED EFFECTS

The figure indicates which endogenous variables are directly affected by the framing variable and other endogenous variables which therefore must be included in our estimation models. By including the framing variable as an explanatory variable for contribution strategy (as indicated in the figure), we are able to obtain an indirect measure of the framing effect on cooperation preferences. By including the framing variable as an explanatory variable for contributions (as indicated in the figure), we are in the same way able to get an indirect measure of any remaining unexplained framing effect on contributions.

We also measure and include the following control variables:

**Gender**

**Age**

**Cognitive abilities:** a number which indicates the test score from the visual part (Group 9 - “Matrices”) of the IST 2000 R intelligence test (See appendix section 8 for measurement details).

**Agreeableness:** a number which indicates the tendency to be pleasant and accommodating in social situations (See appendix section 8 for measurement details).

**Conscientiousness:** a number which indicates the degree of carefulness, thoroughness, self-organization, deliberation (See appendix section 8 for measurement details).

**Extraversion:** a number which indicates attitude characterized by concentration of interest on the external object (See appendix section 8 for measurement details).

**Neuroticism:** a number which indicates the tendency to experience negative emotional states (See appendix section 8 for measurement details).

**Openness:** a number which indicates active imagination, aesthetic sensitivity, attentiveness to inner feelings, preference for variety, and intellectual curiosity (See appendix section 8 for measurement details).

The table below summarizes the descriptive statistics of our set of control variables (for descriptive statistics of the 4 endogenous variables, please see the paper):

*Table SI-3: Summary statistics of control variables*

	<b>Give (n=1366)</b>	<b>Take (n=676)</b>	<b>Entire sample (n=2042)</b>
Gender	0.49	0.47	0.48
Age	45.77	45.84	45.79
Intelligence	8.73	8.72	8.73
Agreeableness	32.23	32.46	32.31
Conscientiousness	33.04	32.53	32.87
Extroversion	30.41	30.58	30.47
Neuroticism	19.32	19.25	19.3
Openness	27.09	27.12	27.1

*All numbers are mean values*

### 3 Estimation of our model

Formally, the empirical specification of our model is the following recursive system:

$$\text{Misperception:} \quad pe = g1(fr_{F2}, x, e1) \quad (1)$$

$$\text{Contribution strategy:} \quad cs = g2(pe_2, fr_{F1}, x, e2) \quad (2)$$

$$\text{Belief:} \quad bl = g3(pe_4, fr_{F3}, x, e3) \quad (3)$$

Contribution:  $cn = g4(bl_4, cs_3, fr_{F4}, x, e4)$  (4)

Where  $fr$  is a frame dummy,  $x$  is a vector of our exogenous control variables and  $e1, \dots, e4$  are stochastic variables which capture the effects of unobserved exogenous variables. The subscripts to the framing indicator and other endogenous explanatory variables indicate the corresponding causal node in Figure 5.

It has been suggested that, e.g. personality traits are important determinants of behavior in social dilemmas (Borghans et al. 2008). Empirical support for this has been found in a large German survey, where Dohmen et al. (2008) noted that personality traits (measured by the big five personality test) are important explanations for trust and reciprocal attitudes. Thus, such personal characteristics are probably an important explanation of the variation in our endogenous variables between subjects and so they are also an important cause of any correlation between them. Since we have included an extensive battery of control variables including potentially important causes of correlation between equations such as intelligence and personality traits, assuming that the stochastic variables ( $e1, e2, e3$  and  $e4$ ) are independent does not seem critical. This assumption ensures unbiased estimation and also implies that we can estimate each equation of the recursive system independently.

### 3.1 Misperception (equation 1)

Since misperception is a binary variable, we can estimate the probability of each outcome as a function of the explanatory variables directly using an ordinary probit model, assuming that the stochastic variable  $e1$  is normally distributed. The framing variable is dichotomous and we assume the standard linear functional form (first order approximation) for explanatory variables i.e.:

$$\Pr(pe = 0) = \Phi([fr, x] \beta^{pe})$$

with the following estimation presented in Table SI-4 below.



Table SI-4: Estimation result for misperception

<b>Multi nominal probit model</b>	
Dependent variable: Misperceiving (=1) or not (=0)	
Treatment (1: give, 0: take)	-0.136*** (0.0302)
Agreeableness	0.00459 (0.00535)
Conscientiousness	-0.0112* (0.00580)
Extraversion	0.0143*** (0.00530)
Neuroticism	0.00930* (0.00503)
Openness	-0.00642 (0.00484)
Intelligence	-0.0902*** (0.0101)
Age	0.000700 (0.00224)
Sex (1: female, 0: male)	0.0251 (0.0615)
Constant	0.699* (0.377)
Observations	2,042
Wald test	129.7
Probability	0
Standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

### 3.2 Contribution strategy (Equation 2)

When modeling contributions (in equation 4), we follow Fischbacher and Gächter (2010) and use the *preferred contribution* as an explanatory variable. The preferred contribution is the contribution stated in the subject's contribution strategy (*cs*) table which corresponds to his stated belief about others' contributions. In order to do this, we must model the subject's selection of his specific contribution strategy. The problem we face in equation 3 is that the set of possible contribution strategies that a subject can choose from is very large. This makes it infeasible to model the probabilities of choosing each possible strategy directly, e.g. using a multinomial probit model. Instead we model the probability in two steps. *First* the possible contribution strategies are

categorized according to the five contribution strategy types (*cs-type*)<sup>2</sup>. The probability of choosing a contribution strategy from one of these categories (*cs-type*) can be modeled using a multi-nominal probit (If we again assume that  $e_2$  is normally distributed). Conditional on this choice we *then*, in principal, estimate the probability of choosing a *specific* profile within that category (*cs* conditional on *cs-type*), as a function of the subject's explanatory variables. In other words, given the *cs-type*, the probability that a subject chooses a specific contribution strategy is given by the contribution strategy's proportion of all observed subjects' strategies with the same combination of explanatory variables and *cs-type*. The probability of choosing a specific profile is then the probability of choosing the relevant category multiplied by the conditional probability of choosing the specific profile within that category, i.e.:

$$\Pr(cs) = \Pr(cs | cs\text{-type} = j) * \Pr(cs\text{-type} = j)$$

where

$$\Pr(cs\text{-type} = j) = \Pr(V_j = \text{Max}(V_1, \dots, V_n)) , \quad V_i = \Phi([fr, ps, bl, x] \beta_i^{cs}) \quad \forall i$$

and

$$\Pr(cs | cs\text{-type} = j) = \Pr(cs | fr, ps, bl, x, cs\text{-type} = j)$$

The first step of the procedure captures all explanatory variable effects under the unrestrictive assumption of normally distributed  $e_2$ . The first step estimation results are displayed in Table SI-5.

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<sup>2</sup> See Fischbacher *et al.* (2001) for the specific categorization criteria.

Table SI-5: First step estimation of cooperation preferences

<b>Multi nominel probit model</b>				
Dependent variable: Cooperation preference	Free rider	Unconditional cooperator	Perfect conditional cooperator	Conditional cooperators
Treatment (1: give, 0: take)	-0.0925 (0.0581)	-0.231*** (0.0703)	-0.283*** (0.0537)	-0.224*** (0.0524)
Misperception	-1.375*** (0.126)	0.193 (0.137)	-0.241** (0.105)	0.219** (0.104)
Agreeableness	0.00294 (0.0108)	0.0362*** (0.0129)	0.0238** (0.00976)	0.0157* (0.00949)
Conscientiousness	0.0109 (0.0116)	-0.00681 (0.0138)	0.0136 (0.0105)	0.00552 (0.0103)
Extraversion	-0.0281*** (0.0107)	-0.0234* (0.0127)	-0.0235** (0.00972)	-0.00977 (0.00949)
Neuroticism	-0.0202** (0.0100)	-0.0280** (0.0120)	-0.00863 (0.00899)	-0.00990 (0.00886)
Openness	0.00851 (0.00976)	0.0132 (0.0114)	0.0209** (0.00889)	0.00724 (0.00873)
Intelligence	0.0529*** (0.0203)	0.0287 (0.0235)	0.0403** (0.0181)	0.0351** (0.0178)
Age	0.00540 (0.00450)	0.00810 (0.00534)	-0.00741* (0.00405)	-0.00933** (0.00396)
Sex (1: female, 0: male)	-0.292** (0.123)	-0.300** (0.143)	-0.270** (0.111)	-0.102 (0.108)
Constant	0.834 (0.750)	-0.713 (0.895)	0.414 (0.678)	0.790 (0.664)
Observations	2,042			
Wald	346.4			
Probability	0			

Standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

The second step of the procedure is in principal less restrictive since it allows the estimation of conditional probabilities without making distributional assumptions. The probability is simply the observed rate of the specified contribution strategy out of the entire set of strategies which satisfy the conditioning variables ( $fr, ps, bl, x, cs-type = j$ ). The procedure also has another substantial advantage. Since we are mainly interested in decomposing the framing effect through simulation, we do not have to actually estimate any second step conditional probabilities for all types of profiles. Instead we can, when simulating, simply allocate profiles by drawing randomly from the observed set of profiles which satisfy the appropriate conditioning variables. In practice, however, the second step procedure requires a sizable number of strategies in each subsample. Thus we end up only being able to allow the framing and perception variables to affect the conditional probabilities of choosing specific profiles. Thus even though we comprehensively model the choice of contribution profile, our model of effects on the variation within each profile categorization is quite basic and only captures the effects of the primary variables of interest. This more basic model of second step probabilities is unavoidable since we want to follow Fischbacher and Gächter (2010) in using predicted contributions as an explanatory variable for contributions.

### 3.3 Belief (equation 3)

Beliefs are, in theory, continuous variables. However, as seen in the data section of the paper, this variable is in practice categorized since almost all subjects report beliefs that are divisible by 5 and with some values attracting a large proportion of reports<sup>3</sup>. This makes it possible for us to estimate a model of the probabilities of these categorized outcomes using a multi-nominal probit (If we again assume that  $e3$  is normally distributed and the linear functional form for explanatory variables):

$$\Pr(bl = j) = \Pr(V_j = \text{Max}(V_1, \dots, V_n))$$

$$\text{where } V_i = \Phi([fr, pe, x] \beta_i^{bl}) \quad \forall i$$

The results of the Beliefs estimations are displayed in Table SI-6.

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<sup>3</sup> Prior to estimation, the few observations which were originally not reported in the steps of 5 DKK (around 2% of the observations) were rounded to the nearest 5 DKK. Thus, an observation of 14 was moved to 15, one for 37 to 35 and so on. This re-categorization did not have any noticeable effect on the mean or other moments of distribution of beliefs.

Table SI-6: Estimation results for Beliefs

<b>Multi nominal probit model</b>										
Dependent variable - Belief about others' contribution	0	5	10	15	20	25	30	35	40	45
Treatment (1: give, 0: take)	0.303*** (0.0899)	0.0855 (0.0989)	0.00396 (0.0648)	-0.0734 (0.0674)	0.00522 (0.0520)	-0.190*** (0.0527)	-0.212*** (0.0532)	-0.339*** (0.0697)	-0.382*** (0.0603)	-0.388*** (0.103)
Misperception	0.144 (0.178)	0.154 (0.199)	-0.0688 (0.131)	0.119 (0.133)	0.264** (0.103)	-0.0919 (0.102)	0.0970 (0.103)	0.0496 (0.125)	0.321*** (0.109)	-0.0837 (0.172)
Agreeableness	-0.0440*** (0.0163)	-0.00392 (0.0184)	-0.0320*** (0.0120)	0.00174 (0.0123)	-0.0181* (0.00949)	-0.0195** (0.00938)	-0.0101 (0.00941)	-0.00835 (0.0114)	0.00912 (0.0102)	0.0296* (0.0161)
Conscientiousness	0.0326* (0.0180)	-0.00573 (0.0192)	0.0105 (0.0127)	-0.00833 (0.0129)	0.0130 (0.0102)	0.000855 (0.0101)	0.000794 (0.0102)	0.0126 (0.0124)	0.00117 (0.0109)	-0.0235 (0.0167)
Extraversion	-0.0215 (0.0157)	-0.0372** (0.0180)	-0.00847 (0.0117)	-0.0275** (0.0119)	-0.0137 (0.00940)	-0.0122 (0.00927)	-0.0110 (0.00938)	-0.0150 (0.0111)	-0.00468 (0.00995)	-0.00464 (0.0151)
Neuroticism	0.0162 (0.0147)	-0.0142 (0.0170)	0.0128 (0.0111)	0.00739 (0.0113)	0.00909 (0.00889)	0.00250 (0.00880)	-0.0141 (0.00894)	0.00127 (0.0108)	-0.00189 (0.00945)	-0.00493 (0.0149)
Openness	-0.00581 (0.0146)	0.00460 (0.0162)	-0.0152 (0.0109)	-0.0196* (0.0112)	-0.00897 (0.00861)	-0.00856 (0.00848)	-0.00265 (0.00851)	0.00442 (0.0105)	-0.0171* (0.00907)	-0.00326 (0.0139)
Intelligence	-0.00438 (0.0305)	0.0586* (0.0353)	-0.00269 (0.0219)	0.0240 (0.0230)	0.0246 (0.0179)	0.0202 (0.0175)	0.00547 (0.0178)	-0.000951 (0.0215)	0.0292 (0.0189)	-0.0219 (0.0293)
Age	0.000996 (0.00673)	-0.0207*** (0.00768)	-0.00477 (0.00499)	-0.0216*** (0.00506)	-0.0104*** (0.00397)	-0.0127*** (0.00394)	-0.0150*** (0.00398)	-0.0258*** (0.00484)	-0.00924** (0.00425)	-0.0258*** (0.00659)
Sex (1: female, 0: male)	0.163 (0.187)	0.315 (0.213)	0.311** (0.137)	0.211 (0.140)	0.481*** (0.108)	0.273** (0.108)	0.482*** (0.108)	0.242* (0.131)	0.219* (0.116)	0.0663 (0.178)
Constant	-1.324 (1.135)	0.0546 (1.264)	0.360 (0.824)	1.268 (0.839)	0.325 (0.674)	1.630** (0.663)	1.517** (0.668)	1.007 (0.810)	0.450 (0.718)	0.771 (1.125)
Observations	2,042									
wald	297.1									
probability	0									

Standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 3.4 Contributions (equation 4)

The core idea of the Fischbacher and Gächter (2010) model is that subjects decide on their contribution by looking up their preferred contribution in their strategy profile for the belief that they have about other group members' contributions. The preferred contribution is the contribution indicated in the subject's contribution strategy for his belief about others' contributions ( $pc = f(cs, bl)$ ). For example, if the subject believes that the other group members on average will contribute 20 DKK and his contribution strategy indicates that his preferred contribution is 15 DKK when others on average contribute 20 DKK, then his predicted contribution will be 15 DKK. To allow for errors they model actual contributions as a linear function of beliefs and *predicted contribution* ( $pc$ ). However, contributions are like beliefs categorized since almost all subjects' contributions are divisible by 5. We therefore, in the same way as for beliefs, estimate a model of the probabilities of these categorized outcomes using a multi-nominal probit, assuming that  $e4$  is normally distributed. We include squared belief and predicted contribution variables to avoid making restrictive functional form assumptions about these variables (note that our specifications allow the strictly linear relationship that Fischbacher and Gächter assume). Thus, in sum, we estimate a model of the probabilities of categorized contributions using a multi-nominal probit assuming that  $e4$  is normally distributed and the linear functional form for other explanatory variables, i.e. :

$$\Pr(cn = j) = \Pr(V_j = \text{Max}(V_1, \dots, V_n))$$

$$\text{where } V_i = \Phi\left(\left[ fr, pc, pc^2, bl, bl^2, x \right] \beta_i^{cn} \right) \quad \forall i$$

$$\text{and } pc = f(cs, bl)$$

The estimates are presented in Table SI-7 below.

Table SI-7: Estimates for contribution equation

<b>Multi nominal probit model</b>										
Dependent variable - Public good contribution:	0	5	10	15	20	25	30	35	40	45
Treatment (1: give, 0: take)	-0.443*** (0.0858)	-0.408*** (0.139)	-0.564*** (0.0895)	-0.530*** (0.133)	-0.486*** (0.0728)	-0.511*** (0.0704)	-0.560*** (0.0706)	-0.649*** (0.120)	-0.500*** (0.0738)	-0.450*** (0.156)
Belief	-0.125*** (0.0258)	0.0349 (0.0639)	-0.0605* (0.0329)	0.409** (0.161)	0.124*** (0.0345)	0.190*** (0.0377)	0.227*** (0.0403)	0.204*** (0.0698)	0.346*** (0.0465)	0.284** (0.126)
Belief <sup>2</sup>	0.000385 (0.000473)	-0.00345* (0.00183)	-0.00130* (0.000738)	-0.0136*** (0.00450)	-0.00478*** (0.000744)	-0.00553*** (0.000768)	-0.00542*** (0.000730)	-0.00437*** (0.00117)	-0.00597*** (0.000694)	-0.00465*** (0.00180)
Cooperation preference	-0.0517*** (0.0169)	-0.0462 (0.0302)	0.0303 (0.0187)	0.0253 (0.0295)	0.00641 (0.0141)	0.0208 (0.0135)	0.0358*** (0.0132)	0.0284 (0.0201)	0.000138 (0.0131)	0.0385 (0.0283)
Cooperation preference <sup>2</sup>	0.000377 (0.000389)	0.000276 (0.000810)	-0.00125*** (0.000474)	-0.00134 (0.000825)	-0.000805** (0.000338)	-0.00106*** (0.000322)	-0.00127*** (0.000300)	-0.00110** (0.000465)	-0.000377 (0.000279)	-0.00118** (0.000599)
Agreeableness	-0.0374*** (0.0140)	-0.0144 (0.0249)	0.00880 (0.0149)	-0.0452** (0.0223)	0.0136 (0.0124)	-0.00102 (0.0119)	0.0148 (0.0117)	0.000474 (0.0165)	0.00283 (0.0118)	0.000648 (0.0225)
Conscientiousness	0.0240 (0.0157)	0.00553 (0.0262)	0.0216 (0.0163)	0.0254 (0.0243)	0.00657 (0.0135)	0.0142 (0.0128)	0.00685 (0.0127)	-0.00474 (0.0174)	0.0336*** (0.0130)	0.0312 (0.0242)
Extraversion	0.00266 (0.0145)	-0.0510* (0.0264)	0.00819 (0.0147)	0.0263 (0.0234)	0.0107 (0.0122)	-0.0154 (0.0117)	0.00851 (0.0114)	0.0234 (0.0163)	0.0119 (0.0114)	0.0141 (0.0207)
Neuroticism	0.00486 (0.0137)	-0.0244 (0.0245)	0.0144 (0.0140)	0.00694 (0.0222)	0.0231* (0.0118)	0.0144 (0.0112)	0.00808 (0.0111)	0.0304* (0.0155)	0.0284** (0.0110)	0.0461** (0.0206)
Openness	-0.0353*** (0.0136)	-0.00493 (0.0238)	-0.0389*** (0.0140)	-0.0254 (0.0220)	-0.0320*** (0.0115)	-0.00617 (0.0110)	-0.0195* (0.0107)	-0.0162 (0.0153)	-0.00496 (0.0105)	-0.0215 (0.0201)
Intelligence	-0.0219 (0.0268)	-0.0182 (0.0454)	-0.0100 (0.0277)	-0.0734* (0.0420)	-0.0592*** (0.0227)	-0.00944 (0.0218)	-0.0191 (0.0214)	-0.0439 (0.0305)	-0.0169 (0.0216)	-0.00338 (0.0414)
Age	-0.00454 (0.00598)	-0.00598 (0.0101)	0.00712 (0.00625)	-0.00101 (0.00950)	0.00953* (0.00519)	0.00821 (0.00502)	0.0110** (0.00493)	-0.0114 (0.00722)	0.00514 (0.00494)	-0.00372 (0.00969)
Sex (1: female, 0: male)	-0.162 (0.173)	0.204 (0.299)	-0.00845 (0.174)	0.409 (0.276)	0.151 (0.142)	-0.00498 (0.137)	0.262** (0.132)	0.00834 (0.194)	0.0432 (0.133)	0.0986 (0.256)
Constant	5.102*** (1.070)	3.696** (1.741)	1.913* (1.103)	-1.358 (2.096)	0.547 (0.970)	-0.303 (0.946)	-2.085** (0.999)	-1.916 (1.549)	-6.120*** (1.155)	-6.813** (2.710)
Observations	2,042									
wald	1072									
probability	0									

### 3.5 Summarizing estimation results

We see that all four estimated equations are highly significant as are most explanatory variables. To assess the importance of the different explanatory variables, a Wald test for each variable in each equation is summarized in Table SI-8 below.

*Table SI-8: Wald tests of explanatory variables*

	Equation 1 Contribution	Equation 2 Belief	Equation 3 Cooperation preferences	Equation 4 Misperception
Frame (1: give, 0: take)	0.000 (102.8)	0.000 (116.2)	0.000 (35.5)	0.000 (20.4)
Misperception		0.007 (24.2)	0.000 (215.0)	
Belief	0.000 (207.2)			
Belief <sup>2</sup>	0.000 (213.0)			
Preference (cooperation from :	0.000 (37.2)			
Preference <sup>2</sup>	0.000 (37.4)			
Agreeableness	0.020 (21.1)	0.001 (29.2)	0.013 (12.7)	0.391 (.7)
Conscientiousness	0.342 (11.2)	0.363 (10.9)	0.466 (3.6)	0.054 (3.7)
Extroversion	0.123 (15.3)	0.448 (9.9)	0.036 (10.3)	0.007 (7.2)
Neuroticism	0.105 (15.8)	0.325 (11.4)	0.115 (7.4)	0.064 (3.4)
Openness	0.073 (17.1)	0.515 (9.2)	0.162 (6.5)	0.185 (1.8)
Intelligence	0.376 (10.8)	0.487 (9.5)	0.102 (7.7)	0.000 (79.6)
Age	0.031 (19.8)	0.000 (51.3)	0.000 (24.6)	0.755 (.1)
Gender (1: female, 0: male)	0.376 (10.8)	0.001 (30.5)	0.037 (10.2)	0.684 (.2)

The shown numbers are the test probabilities  
Chi2 values are presented in the parentheses

Generally, the Wald tests show that the key explanatory variables are significant in all the equations. Some of the controls are not significant in all of the equations, while for others they are only significant in one equation. The patterns we find seem reasonable. Intelligence for instance is not significant for the equations that determine contribution, belief and preference, but is significant for the misperception equation. This seems reasonable since misperception concerns the ability to understand the game for which intelligence is key, whereas the other equations are more about the



subjects' behavioral attitude for which the level of intelligence is not necessarily an important explanation.

## 4 Simulation procedure

To disentangle the different possible causes of the framing effect on public good contributions, we have developed a simulation procedure based on the estimated model. The procedure is graphically presented in the Figure SI-4 below. The procedure has 3 steps, and includes a repeated random draw routine and a bootstrapping procedure that allows us to gauge the precision of our estimated causal decomposition of the framing effect.

*Step 1.* Using the standard bootstrapping approach (see, e.g. Efron and Tibshirani 1993 or Varian 2005), we randomly select from the original dataset obtained in the experiment, a sample of the same size and distribution between frames *with* replacement. Thus some observations from the original dataset are selected more than once, while others are not selected at all. This selection process mirrors the random sampling variation from a population with a distribution over subjects corresponding to our original sample. This allows us to simulate sampling variation in the estimated parameters we are interested in.

*Step 2.* For this bootstrapped sample, we estimate our 4 equation probit models as described in section 2.

*Step 3.* With the estimated coefficients, we then simulate realizations of each of the four endogenous variables for each subject in the sample. In principal we calculate the probability of each possible outcome for a given subject by combining the estimated parameters from the equation in question with the specific subject's values of the exogenous variables for this equation. We then randomly draw a realization among the possible outcomes which reflect the calculated outcome probabilities. Technically we do this in the standard way (Alfnes 2004, Brownstone and Train 1999) by calculating the probit value for each outcome (multiplying the vector of parameters with the vector of subject exogenous variable values and adding a number drawn randomly from a normal distribution) and then selecting the realization with the highest probit value. For each subject, we simulate the outcome for each of the four equations recursively: We first use the estimated parameters for the misperception equation to simulate a realization of the misperception variable for the subject (with her/his specific set of personality traits, intelligence score, gender, age) placed in the given frame. Using the simulated realization of the subject's misperception variable,

we then simulate the subject's belief about others' contributions. The subject's contribution strategy is then simulated in basically the same way. First the subject's strategy type is simulated using the estimated multinomial probit parameters. Then the specific profile is selected randomly among the set of observed profiles for subjects of this strategy type and with the same frame and simulated value of the misperception variable. Finally the simulated contribution strategy and belief are combined to find the preferred contribution, which together with the framing variable, beliefs and controls are used to simulate the subject's contribution. This gives us a simulated realization of the contribution for each subject in the sample. We then calculate the *mean* and the *variance* of this distribution.

To study the framing effect, we ask what happens when we move subjects from one frame to another. In order to decompose the total effect into underlying frame effects which work through misperception, beliefs and preferences, we simulate the model five times for a given set of random normal distribution draws:

*Baseline simulation* Simulate with the frame variable in all equation sets to give,

*Simulation a)* We only change the framing variable in the misperception equation to take (the difference to the baseline simulation is merely the marginal effect of the framing effect which works through misperception).

*Simulation b)* We set the framing variable to take in both the misperception and belief equations (the difference to simulation *a* is the marginal effect of the framing effect which works through beliefs).

*Simulation c)* We set the framing variable to take in the misperception, belief and cooperation strategy equations (the difference to simulation *b* is the marginal effect of the framing effect which works through preferences).

*Simulation d)* Finally, we change the framing variable to take in all equations (misperception, belief, cooperation strategy and contribution). This gives the simulated total framing effect (the difference to simulation *c* is the marginal effect of any remaining unexplained framing effect which does not work through any of the three explanations suggested in the literature).

When taking the differences between the mean and the variance of these five simulated distributions we get:

A) The framing effect on the mean and variance of the contribution distribution which works through misperception (Simulation *a* minus Baseline simulation).

B) The framing effect on the mean and variance of the contribution distribution which works through beliefs (Simulation *b* minus Simulation *a*).

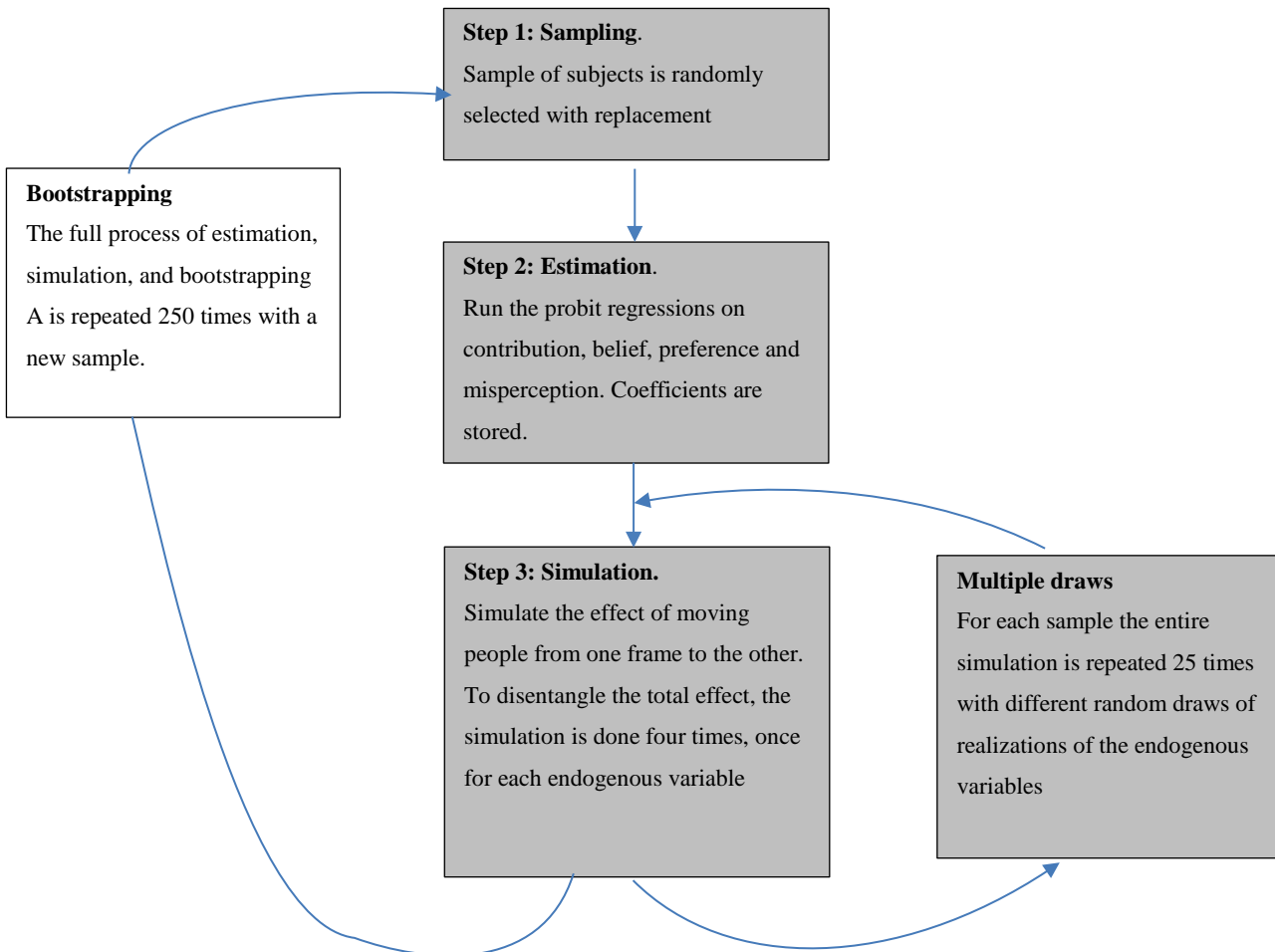
C) The framing effect on the mean and variance of the contribution distribution which works through preferences (Simulation *c* minus Simulation *b*).

D) The remaining unexplained framing effect on the mean and variance of the contribution distribution (Simulation *d* minus Simulation *c*).

E) The total framing effect on the mean and variance of the contribution distribution (Simulation *d* minus Baseline simulation).

*Multiple draws:* Step 3 is repeated 25 times, each time with different normal distribution draws, but with the same coefficients and variable values. Then the mean of the 25 sets of simulated decomposed framing effects *A-E* are calculated. This is our estimate of the *expected* values of the set of decomposed framing effects *A-E* that our simulation model will generate for this particular subject sample.

*Bootstrapping:* The entire procedure (steps 1, 2 and 3) is repeated 250 times, in order to generate distributions for each decomposed framing effect *A-E* reflection sampling variation when sampling from the experiment's subject pool. This allows us to gauge the accuracy (significance) of the estimated set of decomposed framing effects by the standard bootstrapping method (see, e.g. Efron and Tibshirani 1993 or Varian 2005).



*Figure SI-4: An illustration of the simulation procedure*

## 5 Testing the estimated model

Prior to using the simulation model for decomposing the framing effects, we tested its ability to accurately simulate the observed behavior of our subject pool. The table below provides an overview of the 3 sets of test we have undertaken.

*Table SI-9: Various tests of the model*

	<b>What is tested?</b>	<b>Description</b>	<b>Test</b>	<b>Conclusion</b>
1	Simulated Distributions conditional on observed endogenous explanatory variables	The distributions of observed contributions are compared with the distributions of the corresponding simulated variables using <i>observed</i> values of all endogenous explanatory variables in the four model equations.	Pearson's chi2 tests	Distributions do not differ significantly.
2	Simulated Distributions, when equations feed into each other	The distributions of observed contributions are compared with the distributions of the corresponding simulated variables using <i>simulated</i> values of all endogenous explanatory variables in the four estimated models. Thus we allow the equations to feed into each other basing predicted values on predicted values from prior parts of the system.	Pearson's chi2 tests	Distributions do not differ significantly
3	Frame swap	The distribution of predicted contributions and beliefs in the give frame for subjects originally in the take frame is compared with observed contributions of subjects in the give frame – and vice versa.	Pearson's chi2 tests	Distributions do not differ significantly

**Test 1:** We test how well contributions are predicted conditional on observed explanatory variables. We do this by comparing the distributions of contributions observed in the experiment, with the distributions of the contributions simulated by the contribution equation as described above where we use *observed* values of misperception, contribution strategy and beliefs as explanatory variables. We perform 100 simulations for each distribution (using different random draws each time) and each time compare with the observed distribution using Pearson’s chi square test. The resulting test score probabilities are graphed below where test scores below the 5% dotted line indicate that the distributions are different at the 5% significance level. None of the 100 *simulation* distributions differed from the observed at a significance level under 0.43.

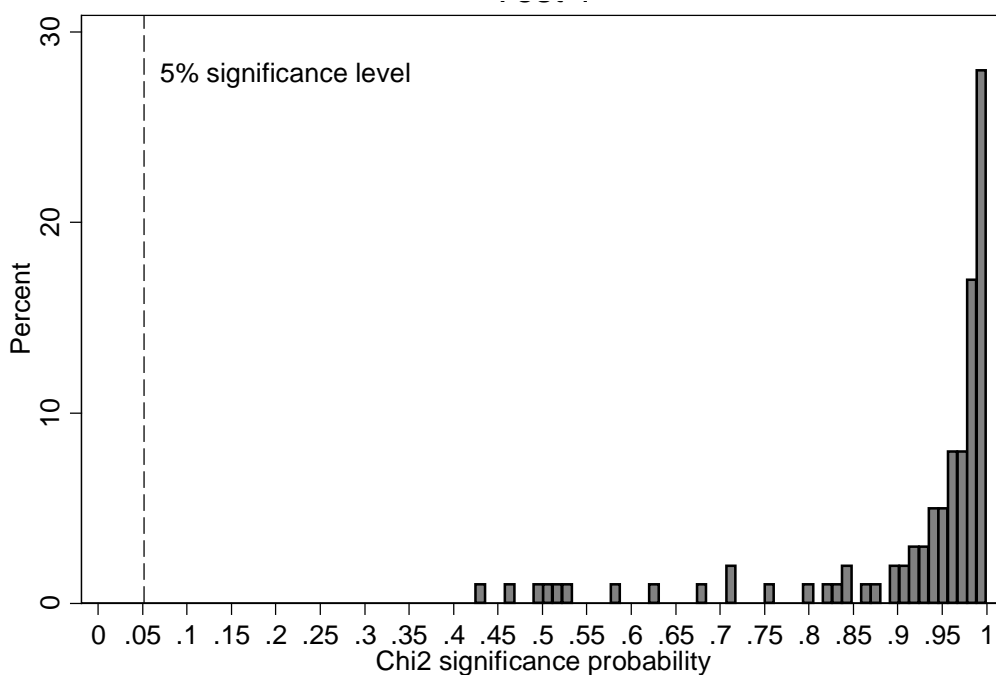
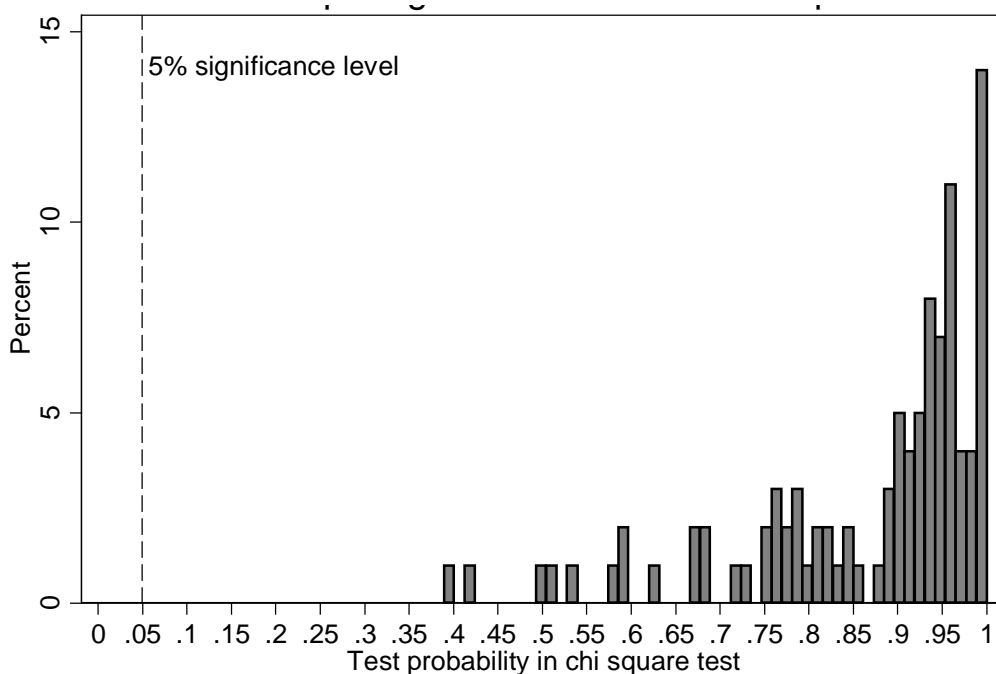


Figure SI-5: Test 1- Comparing actual and estimated distributions

**Test 2:** We test how well contributions are predicted conditional on simulated explanatory variables. This test is like test 1 except that we do not condition on observed endogenous variables, but on simulated endogenous variables. We do this by comparing the distributions of contributions observed in the experiment, with the distributions of the contributions simulated by the contribution equation as described above where we use simulated values of misperception, contribution strategy and beliefs as explanatory variables (as described above)

We perform 100 simulations of each distribution (using different random draws each time) and each time compare with the observed distribution using Pearson's chi square test. The resulting test score probabilities are graphed below where test scores below the 5% dotted line indicate that the distributions are different at the 5% significance level. None of the 100 simulated distributions differed from the observed at a significance level under 0.39.



*Figure SI-6: Test 2 - Comparing actual and estimated distributions (simulated endogenous variables)*

**Test 3:** generates distributions of simulated contributions for subjects who are moved from their original frame into the alternative frame as described above. Thus we simulate the contributions of subjects originally in the give frame, when their framing variable is changed to take in all equations. We then do the same simulated frame change for all subjects originally in the take frame. These simulated contribution distributions are then compared with the observed contributions of subjects originally in the take and give frames. We perform 100 simulations of the distribution (using different random draws each time) and each time compare with the observed distribution using Pearson's chi square test. The resulting test score probabilities are graphed below where test scores below the 5% dotted line indicate that the distributions are different at the 5% significance level. None of the 100 simulated distributions differed from the observed at a significance level under 0.33.

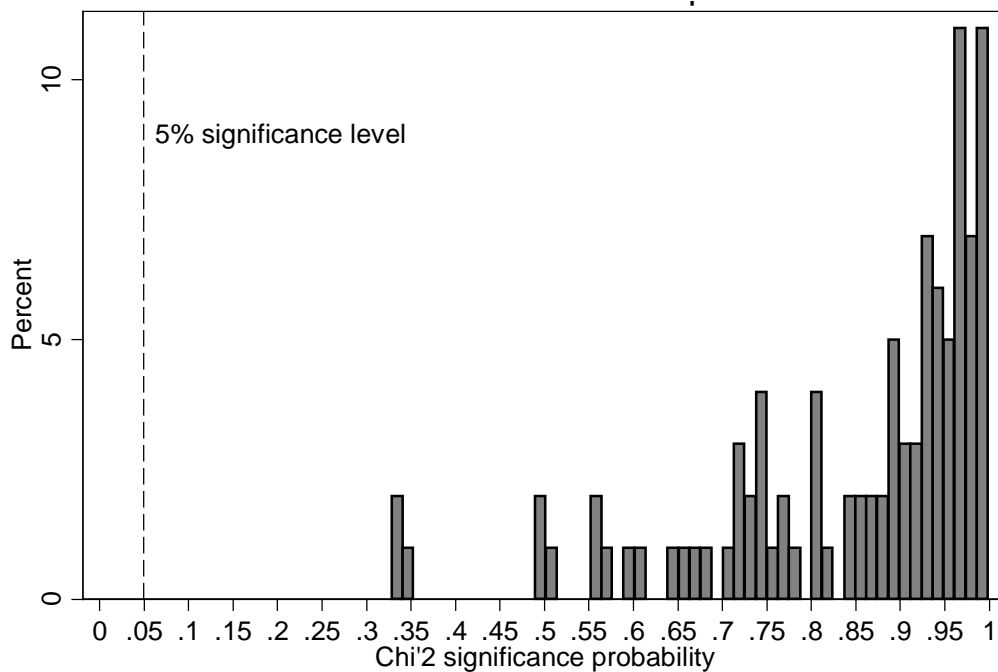


Figure SI-7: Test 3 – Comparing estimated distributions when actual frames are swapped

## 6 Recruitment and design

This section provides details about the recruitment of subjects and the experimental design.

### 6.1 Recruitment of subjects

The participants were recruited as follows:

- Statistics Denmark, the official statistics office in Denmark, randomly selected 40,000 individuals from the Danish population.<sup>4</sup>

<sup>4</sup> Note that this is not a completely random sample of the Danish population because all inhabitants have the right to refuse to be contacted for research purposes (this rule applies to all research conducted in Denmark when sampling from the Central Person Register). Individuals who have claimed this right are not included in the population from which our sample of 40,000 was drawn. Around 20-25% of people in the age group 20-39 years have claimed this right, while the percentage is much lower in other age groups (5-12%). More information about the issue and the characteristics of people claiming this right is available at ([http://www.dst.dk/upload/notat\\_om\\_forskerbeskyttelse\\_2008.pdf](http://www.dst.dk/upload/notat_om_forskerbeskyttelse_2008.pdf)). Unfortunately, this material is only available in Danish.



- Statistics Denmark prepared invitation letters and envelopes. See section 7 for a picture of the invitation letter. A translation of the invitation letter can also be found in Section 6.
- In total, 22,027 letters were randomly selected out of the 40,000 and sent out to the respondents in two waves on May 15 and May 30, 2008.
- The letters invited subjects to log on to our webpage, [www.econ.ku.dk/ilee](http://www.econ.ku.dk/ilee), using a personal identification number printed in the letter. Subjects had one week to complete the experiment.
- In total, 3,584 subjects logged on to our web page and out of these, 2,291 completed the experiment. We had several treatments and in the current paper, we use 2,042 observations (give and take treatments with incentives and no gifts). See Table SI-10 for details about treatments and how our sample was selected.

## 6.2 Overview of the experiment

In short, the participants were invited to log on to our web page twice, once during the period in which the experiment was open and once during a feedback period after the experiment was closed. The first time they logged in they participated in two public good games and completed a series of other questionnaires and tests. After the experiment closed, participants were matched together in groups for the public good game and payments were calculated. Participants logged on to our web page again to see the results of their group and provide us with the banking details necessary for distributing the payments.

## 6.3 Treatments and participation

The experiment had six treatments that varied with respect to the type of invitation letter, incentives and framing. There were three types of invitation letter, namely Standard, Support, and Support Gift. The Standard letter informs subjects that they can make money in the experiment, whereas the Support letters instead tell subjects that they will be contributing to scientific research. See below for a picture of the invitation letter and Section 7 for a translation of the text in the letters. The Support Gift letter also included a small gift (a foam puzzle with logos of the Internet laboratory of experimental economics (iLEE) and the University of Copenhagen printed on it).

Another variation concerned the actual incentives paid out to the subjects. In the Paid treatments, subjects actually received payment irrespective of which invitation letter they received. In the

Hypothetical treatments, subjects faced the same instructions throughout the experiment, but the welcome screen included a paragraph making clear that subjects would not actually receive payment and directing them to simply make their decisions as if they would be paid according to the instructions. Of course, only subjects who received the invitation letters Support or Support Gift participated in the Hypothetical treatment.

The third and last variation concerned the framing of the public good game part of the experiment, which was either a Give or a Take frame. Only the instructions for the public good game differed between treatments.

Table SI-9 breaks down the complete target subject pool into treatments. Upon logging in, a random number determined which treatment the subjects were routed to. 2/3 of the subjects who received the standard letter were assigned to the Give-Incentivized treatment and 1/3 to the Take-Incentivized treatment. For the other two letter types, 1/2 of the subjects were routed to Give-Incentivized and 1/2 to the Give-Hypothetical.<sup>5</sup>

The current paper only uses data from the Give treatment with incentives and the Take treatment. In addition, we only use subjects who received the standard letter and hence we have a sample with 2,042 (1366+676) subjects (corresponding to highlighted row in Table SI-10).

*Table SI-10: Number of letters sent out and number of Subjects in Each Treatment*

	<b>Treatment</b>			
	Letters	Give-Incentivized	Give-Hypothetical	Take-Incentivized
<b>Standard</b>	18,027	1,366 (2,027)	-	676 (1,080)
<b>Support</b>	2,000	47 (93)	68 (128)	-
<b>Support Gift</b>	2,000	85 (146)	49 (110)	-
<b>Total</b>	22,027	1,498 (2,266)	117 (238)	676 (1,080)

<sup>5</sup> It turned out that the random number generator we used failed to generate a perfectly uniform distribution, which explains why the number of observations does not exactly match our intended division between treatments.

*Note:* Figures in the first column refer to the number of letters sent out. Figures in the other columns refer to the number of subjects who completed the experiment for each treatment. Numbers in parenthesis refer to the number of subjects assigned to each experiment.

## 6.4 Detailed account of the core part of the experiment

This section describes the core part of the experiment in detail. Screenshots including translated instructions are available at the end of this appendix. Subjects had access to several forms of help to understand the instructions. Throughout the public good game part of the experiment, subjects could go back and read the instructions again at any time. In addition, from each screen, subjects could access a screen-specific help screen which provided further guidance about what to do. Subjects also had access to a profit calculator where they could see for themselves how the earnings of the four members of the group depended on the members' contributions (see Section **Error! Reference source not found.** for screenshots of the profit calculator in the give and the take frame). Finally, all help screens included a telephone number and an email address through which subjects could obtain further assistance.

### 6.4.1 Login and information screens

The first screen of the experiment that the subjects were taken to when they entered the URL from the invitation letter was a simple login screen where they had to enter their personal identification code which was printed in the invitation letter. Upon login, subjects saw a welcome screen which provided information about the experiment. They were informed that their participation in the experiment would be valuable to research in economics and were reminded of the importance that the person who participated was the one who was named in the invitation letter. Moreover, they were informed that they could earn money during the experiment (within the range of 8 to 510 DKK, corresponding to approximately 1.6 to 102 USD) and that this is standard procedure in economic experiments. They were also cautioned that they had to complete the experiment to get their money by electronic transfer. All subjects were then informed that the experiment would last approximately 50 minutes. Finally, they were reassured that they would be anonymous.

After answering some questions regarding their socioeconomic background (age, gender and highest completed education), the subjects proceeded to the public good game part of the experiment.

#### 6.4.2 The public good games

Subjects played two variants of the public good game. They first played a standard linear one-shot public good game involving one unconditional contribution choice (referred to as the Standard game). Afterwards they played a public good game using the strategy method which involves an unconditional choice as well as a series of conditional choices (referred to as the Strategy game). Both public good games were framed according to the treatment that the subjects were assigned to.

In both games, there were four members in each group, the endowment was 50 DKK (approximately 10 USD), and the marginal per capita return was 0.5. The subjects were asked to contribute between 0-50 DKK of the private endowment to a common pool. Everything in the pool was then doubled and shared equally between the four subjects in the group. There was no feedback during game play.

Subjects began by reading the instructions for the Standard game. In order to make the rules of the public good game easy to understand, the written instructions were complemented by a series of illustrations made by a professional illustrator.

After viewing the instructions, subjects were required to correctly complete four control questions testing their ability to calculate payoffs in the game. Subjects were allowed as many attempts as necessary, but could not proceed without entering the correct answer to each question. Subjects then made their choice. On the next screen, their beliefs about the average contribution of the other members of their group were elicited. The belief elicitation was incentivised using the quadratic scoring rule. Participants' payments, expressed in DKK, were determined by  $10 - 0.004 d^2 \geq 0$ , where  $d$  is the difference between the belief and the true value.

Subjects then read the instructions for the strategy method version of the public good game. The strategy method was adapted to the context of the public good game by Fischbacher *et al.* (2001). The idea behind the strategy method is to have subjects report the complete strategy of actions they would like to take in the event of each possible combination of actions that others could take.

After reading the instructions for the Strategy game, subjects first had to make an unconditional choice. This unconditional choice was necessary to determine the outcome of the game. Subjects then had to fill out a conditional contribution table in which they had to decide how much they would like to contribute for each of the 11 average contribution levels of the other group members that are multiples of 5 (0, 5, 10... 45, 50). Our design differs from Fischbacher *et al.* (2001) in this

respect. In that paper, the endowment was 20 tokens and all 21 possible integer average contribution levels were included in the conditional contribution table.

The outcome of the Strategy game was determined as follows: One member of the group is randomly selected. For the other three subjects, the second unconditional choice counts as their contribution. The average of their choices is rounded to the nearest multiple of 5, and the contribution of the selected member is then determined by referencing the relevant row of his or her conditional contribution table.

#### 6.4.3 Game misperception

After the public good games, subjects continued to a test of the relation between income motives and behavior in the public good game. The game perception test was framed according to the treatment that the subjects were assigned to.

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.<sup>6</sup> 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 ( $\approx 0.7$  €) 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, contributed 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 contributed 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.

#### 6.4.4 Measures of Personality

After completion of the game perception test, our subjects were asked to complete a well-established **personality test**. More specifically, we applied a Danish short version of the Big 5

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<sup>6</sup> 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 are discussed 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.

personality test.<sup>7</sup> The test consists of a battery of 60 statements which cover personality traits in five dimensions: agreeableness, conscientiousness, extraversion, neuroticism, and openness.<sup>8</sup> Based on the answers to these statements, each subject is assigned a score for each of the five big 5 dimensions. A high score for a given trait indicates that this trait is an important part of the subject's personality.

#### 6.4.5 Measure of Cognitive ability

First, the subjects completed the visual IST 2000 R 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.

#### 6.4.6 Additional tests

Subjects also performed a number of tasks that we do not use in our analysis, including risk and loss aversion, and cognitive ability scores.

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<sup>7</sup> We used the Danish NEO-PI-R Short Version test, provided to us with the permission of Dansk Psykologisk Forlag ([www.dpf.dk](http://www.dpf.dk)).

<sup>8</sup> 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, regardless of which facets the single items belong to). In the Danish short version, all facets are therefore represented equally within each domain.

## 7 The invitation letter



**Name**  
**Address**

Kære **Name**

Danmarks Statistik og Internet Laboratoriet for Eksperimentel Økonomi (iLEE) ved Økonomisk Institut på Københavns Universitet inviterer dig hermed til at deltage i et eksperiment vedrørende økonomiske beslutningsprocesser.

Eksperimenter er et vigtigt redskab inden for økonomisk forskning, idet de er med til at skabe en bedre forståelse for, hvordan mennesker træffer økonomiske beslutninger. I sidste ende kan dette være med til at forbedre den førte økonomiske politik. Et økonomisk eksperiment kan tage mange forskellige former – eksempelvis kan det gå ud på, at deltagerne skal købe og sælge varer på et fiktivt marked eller træffe beslutninger om at investere.

For at opnå et repræsentativt billede har Danmarks Statistik udvalgt et stort antal personer fra hele Danmark, som nu får mulighed for at deltage i eksperimentet. Du er blandt de tilfældigt udtrukne. Din deltagelse er naturligvis frivillig, men vi håber meget, at du vil deltage. Der kræves ingen særlig kendskab til hverken økonomi eller computere for at kunne deltage i eksperimentet, og dine beslutninger i eksperimentet bliver behandlet strengt fortroligt og anonymt.

Ved at deltage i eksperimentet får du mulighed for at tjene penge. Vi kan ikke garantere dig, at du vil tjene et bestemt beløb, idet din indtjening vil afhænge af dine egne samt andre deltageres beslutninger. De nærmere regler er beskrevet på hjemmesiden.

For at sikre deltagerne fuld anonymitet logger alle deltagere ind med et tilfældigt udvalgt nummer. Vi laver en række forskellige eksperimenter, og alle deltager derfor ikke i det samme eksperiment. For at se detaljerne i netop dit eksperiment, herunder opgaven, tidsforbrug mv., bedes du snarest muligt logge ind på vores hjemmeside:

[www.econ.ku.dk/ilee](http://www.econ.ku.dk/ilee) med dit login nummer: **28.826-6**

Hvis du har problemer med at logge ind eller har yderligere spørgsmål, er du velkommen til at kontakte Økonomisk Institut på e-mail [ilee@econ.ku.dk](mailto:ilee@econ.ku.dk) eller telefon 35 32 44 09.

Med venlig hilsen og på forhånd tak for din hjælp.

Isak Isaksen  
Kontorchef, Danmarks Statistik

Jean-Robert Tyran  
Professor, Økonomisk Institut

Danmarks Statistik  
Sejrøgade 11  
2100 København Ø

Tlf. 39 17 39 17  
Fax 39 17 39 99  
CVR 17-15-04-13

[dst@dst.dk](mailto:dst@dst.dk)  
[www.dst.dk](http://www.dst.dk)

## 7.1 Translation

**Dear [First name]**

Statistics Denmark and the Internet Laboratory for Experimental Economy (iLEE) at the Institute of Economics, Copenhagen University, hereby invite you to participate in an experiment on economic decision making.

Experiments are a vital tool in economic research, since they help gain a better understanding of how people make economic decisions. This can ultimately help improve economic policy making. An economic experiment can assume many forms – e.g. the participants could be asked to buy and sell hypothetical goods or make investment decisions.

In order to obtain a representative picture, Statistics Denmark has selected a large number of people from all of Denmark who have been given the opportunity to participate in the experiment. You are among the randomly chosen. Your participation is of course voluntary but we sincerely hope that you participate. No special knowledge of economics or computers is required to participate in the experiment and your decisions during the experiment will be kept strictly confidential and anonymous.

By participating in the experiment you will have the opportunity to earn money. We cannot guarantee that you will earn a specific amount since your earnings will depend on your decisions and the decisions of other participants. The specific rules are described on the web site.

To ensure complete anonymity, all contestants log on with a randomly selected number. We conduct a range of different experiments and therefore not everyone participates in the same experiment. To see the details of your experiment, including the task, duration and so forth, you are requested to log on to our web site at your earliest convenience:

**[www.econ.ku.dk/ilee](http://www.econ.ku.dk/ilee)** with your log in number: [ID number]

If you experience problems logging in or have any further questions, you are welcome to contact us either via email at **[ilee@econ.ku.dk](mailto:ilee@econ.ku.dk)** or by phone on 35 32 44 09.

Thanks in advance.

Kind regards,

Isak Isaksen

Kontorchef, Statistics Denmark

Jean-Robert Tyran

Professor, Institute of Economics



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