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A representative sample in the lab

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Cooperation Stability – A representative Sample in the Lab

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Abstract:

The ability to cooperate is a central condition for human prosperity, yet a trend of declining cooperation is one of the most robust observations in behavioral economics. The massive replication of declining cooperation has almost exclusively been carried out in student populations, which opens up for the question of whether the declining cooperation is predictive for the population at large. I make two steps to address this knowledge gap about cooperation stability in the general population. First, I measure repeated cooperation among students and a representative sample. Among the students, I confirm the usual decay effect of cooperation. However, among the non-students, the behavior is hugely different and approaches no decay. Secondly, I stress test the cooperation stability among non-students by manipulating the composition of preferences so that fast decay and no decay are predicted. I observe that the cooperation stability is remarkably unaffected by this manipulation.

Cooperation is the cornerstone of maintaining a well-functioning society (Fehr & Gächter, 2000; Gächter, Herrmann, & Thöni, 2004; Ledyard, 1995). Decisions such as active engagement in teamwork in the workplace or in education, giving way in the traffic, or being involved in unions are a few examples which fundamentally depend on people's voluntary decisions to be cooperative. In contrast to its obvious importance, cooperation is often in conflict with individual self-interest. In economics, the behavioral responses to this conflict, or social dilemma, are often studied in the public good game (Marwell & Ames, 1979). In this game, a small group of people simultaneously contributes to a public good. The size of the contribution, viewed as the sign of cooperation, constitutes a dilemma between pursuing maximum private windfall or maximum group outcome. For participants who are solely motived by economic motives, the dominant decision is not to contribute anything while enjoying the benefits derived from other contributions. In contrast, for participants with some sort of social preferences (Cooper & Kagel, 2009), positive contributions may be their best response. By employing this social dilemma, the public good game serves as a formal illustration of the tension underlying many important social interactions.

Two behavioral findings are routinely observed in the repeated public good game. *First*, in the initial rounds of the game, a substantial share of participants gives positive contributions. That is, a large fraction of people deviates from pure self-interest, thereby illustrating the importance of social preferences (Cooper & Kagel, 2009). The *second* robust findings is that cooperation is unstable (Chaudhuri, 2010; Herrmann, Thöni, & Gächter, 2008; Ledyard, 1995; Zelmer, 2003). The initial positive cooperation dramatically declines with repetitions of the game, and approaches behavior that is in line with pure self-interest over time. Obviously, this collapsing cooperation constitutes a tremendous loss of economic opportunity, and substantial research has not surprisingly been devoted to understanding unstable cooperation (J. Andreoni, 1995; James Andreoni, 1988; Fischbacher & Gächter, 2010; Houser & Kurzban, 2002).

In this paper, I challenge the finding of cooperation instability in two different steps. **Step 1:** The declining cooperation might be the result of the laboratory investigations that exclusively use student samples instead of representative samples. Using university students to draw inferences about general behavioral patterns has indeed been criticized for creating skewed results (Henrich, Heine, & Norenzayan, 2010). A widely promoted alternative is to bring the investigation into the field (Harrison & List, 2004; Levitt & List, 2007). Whereas field settings indisputably provide very important knowledge about naturally occurring cooperation (See e.g. Carlsson, Johansson-Stenman, & Nam, 2014), the approach, strictly speaking, expands the traditional laboratory investigation in at least two dimensions by studying behavior in more natural settings and by studying a more heterogeneous sample of people. Consequently, a clear separation is lacking. My approach is to break the investigation up into smaller steps, and to simply replace the student samples in the

laboratory with a representative sample. This ensures that the settings and the set of circumstances of the traditionally applied cooperation study are the same and, consequently, the pure effect of introducing the heterogeneous sample can be measured.

I invited both a representative sample of the population and a student sample to play the repeated public good game in a laboratory experiment. The students' cooperation stability exactly mimics the robust decay finding in the literature, whereas the behavior of the representative sample exhibits a much more stable cooperation path - almost fully stable cooperation. Furthermore, through a counterfactual simulation, I find that the more stable cooperation among non-students is driven by their response to beliefs about other people's cooperation.

Step 2: Having established that cooperation among non-students is much more stable than the usual observation among students, I aim to test how robust this cooperation stability is among non-students. In fact, I create pools of non-student subjects, which are predicted to have unstable or stable (or even increasing) cooperation over time. According to a prominent study by Fischbacher & Gächter (2010), the reason for the declining cooperation in the public good game is that subjects, on average, have preferences for conditionally cooperating less than what other subjects do. A prediction that results from this is that populations with preferences for cooperation (at least) as much as others should result in stable (or even) increasing cooperation, but if populations have average preferences for cooperating less than others, decay should occur.

I test this prediction by splitting participants into subject pools based on their preferences. As an additional enhancement of the test, I also randomize whether subjects are informed about this grouping based on preferences. Against the prediction, I observe that pools of non-student subjects with a preference for cooperation at least as much as others do not exhibit stable or increasing cooperation. In fact, they show a pattern that is very similar to the pool of non-students not divided on preferences; that is, they exhibit a tiny decay effect. For the pools of subjects with a preference for contributing less than others, I observe a more rapid decline in cooperation, but not of a dramatic magnitude. Providing the subjects with information about the division based on preferences did not change the resulting cooperation stability for the most social subjects. However, surprisingly, those with the least social preferences had a significantly smaller decay effect when informed about the splitting.

The findings from the two steps of my study contribute to the large literature on repeated cooperation. Considerable evidence for cooperation among student samples exists (Chaudhuri, 2010; Ledyard, 1995; Zelmer, 2003), while evidence for out-of-the-lab cooperation is increasing (Diederich, Goeschl, & Waichman,

2016; Fehr & Leibbrandt, 2011; Galizzi & Navarro-Martinez, 2018; Kosfeld & Rustagi, 2015; List, 2004; Noussair, Charles N., Daan van Soest, 2015; Noussair, Soest, & Stoop, 2015; Rustagi, Engel, & Kosfeld, 2010). However, how non-students behave in the typically applied laboratory setting remains largely unknown. A few related studies do exist. Belot & Duch (2015) explore how students and non-students differ in standard games, including the repeated public good game. They report that students are less generous, and that a larger drop of cooperation from the first to the last contribution is observed among students. A limitation of their study is their limited sample of subjects. A rather specific comparison was made by Cadsby & Maynes (1998) who contrasted the cooperation of economics and business students in the laboratory threshold public good games with the cooperation of nurses. They found that the students were less likely to reach the threshold. Although these previous studies support the assertion that cooperation stability may be different among non-students, compared with the present study, they do apply small and quite specific samples, and some of them do not explicitly compare non-students with students. In this paper, I contribute by using a very traditional laboratory approach and carefully compare students and a very heterogeneous non-student sample. Furthermore, I stress test the importance of preference compositions for cooperation among non-students.

The findings of this paper highlight the fact that the unstable cooperation replicated in hundreds and hundreds of experiments may be a special case which is mainly relevant to students. Here, I find that among non-students the decrease in cooperation is very different. In fact, almost no decrease exists and this pattern is surprisingly robust to alternative preference compositions.

Existing literature and hypothesis

To guide my study of cooperation stability, I form three hypotheses. In the first step of the analysis, I contrast the cooperation stability of students with that of ordinary people; non-students. Initiated by the 'weird' critique[1] (Henrich et al., 2010), a large group of studies have explored how students' behavior deviates from non-students' (Anderson et al., 2013; Cappelen, Nygaard, Sørensen, & Tungodden, 2015; Cleave, Nikiforakis, & Slonim, 2013; Exadaktylos, Espín, & Brañas-Garza, 2013; Falk, Meier, & Zehnder, 2013; Kettner & Waichman, 2016). Clearly, students are different from the population as a whole. Students are, for instance, typically relatively young, make faster decisions (Langan et al., 2010), and have better cognitive skills (Henrich et al., 2010). In Norway, a recent study compared the behavior of a representative sample with that of a student sample in standardized dictator and trust games (Cappelen et al., 2015). The authors find that behavior and moral concepts differ remarkably between the two, with the general population being more

[1] The 'weird' critique highlights that results in social science studies are biased as they systematically build on the behavior of Western Educated students from Industrialized, Rich and Democratic societies.

pro-social. In a similar vein, Bellemare, Kröger, & Van Soest (2008) and Kettner & Waichman (2016) studied decisions in the dictator game and found that non-students are more altruistic. Generosity in the form of donations to charity is also found to be lower among students (Carpenter, Connolly, & Myers, 2008). Of perhaps the most direct relevance to this study, a set of previous experiments have found that students are less cooperative than non-students (Anderson et al., 2013; Belot, M., R. Duch, 2015; Gächter et al., 2004). In a sequential public good game, Anderson et al (2013) find that students are less pro-social. Gächter et al. (2004), found lower contributions in a standard one-shot public good game among students compared with non-students. Given that students have previously been found to be less pro-social and cooperative compared with non-students, I predict the same to happen here. Furthermore, I predict that the less cooperative attitude also translates into more decay, assuming that the lower cooperation is a result of less cooperative preferences (Fischbacher & Gächter, 2010; Gunnthorsdottir, Houser, & McCabe, 2007), which in turn is expected to result in lower cooperation (Fischbacher & Gächter, 2010).

H1: Cooperation decays more among students compared to non-students.

The second focus of the paper is to challenge the cooperation stability of non-students by manipulating the composition of cooperation preferences. Fischbacher & Gächter (2010) found that the reason for decaying cooperation is that participants, on average, have less than perfect conditional cooperator preferences. A direct implication of their result is that stable, or even increasing, cooperation should occur if participants, on average, have at least perfect conditional cooperator preferences. In contrast, combining the composition so that people on average have preferences for less than perfect conditional cooperation should result in decay. This prediction is echoed in previous studies which identify that the composition of those who interact in the public good game is pivotal for the decay (Angela C. M. de Oliveira, Rachel T. A. Croson, 2015; Berg, Molleman, Junikka, Puurtinen, & Weissing, 2015; Gächter & Thöni, 2005; Gunnthorsdottir et al., 2007; Hartig, Irlenbusch, & Kölle, 2015). De Oliveira & Croson (2015) measure social preference in the lab and manipulate the composition of conditional cooperators and self-serving types in subsequent experimental sessions. They find that the composition affects cooperation with more stable, but still decaying, cooperation occurring when only conditional cooperators are present. Gunnthorsdottir et al. (2007) match people into groups based on their previous public good contributions and find that, overall, such sorting increases cooperation and that decay in cooperation, to a large extent, is the result of conditional cooperators reducing their initial contributions. Less interaction between free riders and conditional cooperators consequently leads to less decay. Gächter & Thöni (2005) use a similar approach by playing an initial one shot public good game and sort participants according to their decisions. Building on these previous findings and the prediction made in

Fischbacker and Gächter's study, I expect that the composition of the cooperation preferences will influence the decay effect, making it both fade and increase.

H2: Stable cooperation will be present in interactions between subjects with at least perfect conditional cooperator preferences, whereas (rapid) decay of cooperation will be present for subjects with less than perfect conditional cooperator preferences.

The final hypothesis concerns whether information about the division based on cooperation preferences affects cooperation stability. On the one hand, such information could be redundant because subjects will receive feedback about the other group members anyway. On the other hand, information about others' attitudes could be important given that cooperation among the majority of experimental subjects is conditional on the behavior of others (Fischbacher, Gächter, & Fehr, 2001; Fosgaard, Hansen, & Wengström, 2014; Kesser & van winden, 2000). Getting this information before the experiment begins could help to form more realistic expectations, although this may depend on the type of preferences. If a perfect conditional cooperator learns that he is interacting with people who are also (at least) perfect conditional cooperators, he can more safely assume that the others will contribute and, therefore, he is more likely to cooperate himself. For types who are less than perfect conditional cooperators, the impact of the information will be less important as this information systematically moves a relatively small number of contributions. At the extreme, a person with a free rider preference should be unaffected by the information, simply because this information does not have any influence on the behavioral strategy. Yet free riders may be concerned about the expectations of others. In this case, learning that the interaction will be among other free riders could potentially result in more rapid decay because they do not need to worry about others expecting them to cooperate. In line with these predictions, Angela et al. (2015) find a positive effect of providing information about fellow group members' cooperation type in groups dominated by conditional cooperators, whereas such information speeds up decay in groups with mainly selfish types. For the present experiment, I form expectations for types with at least perfect conditional cooperator preferences and types with less than perfect conditional cooperator preferences, but not necessarily free riders.

H3: Information about fellow group members' cooperation preferences has a positive effect on cooperation stability for subjects with at least perfect conditional cooperator preferences, but less of an effect on subjects with less than perfect conditional cooperator preferences.

Experimental procedure and design

A representative sample (n=5000) of the population in the greater Copenhagen area was selected by Statistics Denmark to participate in a laboratory experiment. The permitted age range was 18-70. An

invitation to participate in the study was sent via ordinary mail, and the recipients were instructed to login on to a recruitment website to pick a day and a time for participation if they were willing to accept the invitation. The invitation letter promised subjects a 300 DKK (≈ 54 USD) attendance fee, and explained that additional earnings based on decisions made during the experiment would be earned, typically in the range of 100 to 400 DKK (≈18-72 USD). The invitation letter is presented in the appendix. To facilitate the participation of people with regular day jobs, a 5pm and an 8pm session were scheduled on each day of the experiment. The seats were allocated on a first come first serve basis. The invitations were sent out in two waves so that the size of the sample could be adjusted based on the response rates. The invitation letters provided the participants with a unique id code (a combination of 5 letters and 5 numbers), which they had to use to login to the recruitment site. The login code served several purposes. First, it meant that people could be identified when they arrived at the laboratory, while still maintaining their anonymity. Second, the code allowed me to ex-post link participants' behavior in the experiment to their records in the Danish register data. Once the subjects had signed up for a session, they were offered to be reminded one day prior to the appointment by email or phone. The subjects then came to the laboratory on the selected day and time. Before being seated in front of individual computers, which were carefully placed in separate booths, the subjects were identified by their id code. The experiment was completed over 10 sessions. A total of 424 subjects from the representative sample participated.

Using the laboratory's existing ORSEE database of subjects (Greiner, 2004), a sample of students was selected and invited to complete the experiment in order to contrast the behavior of the representative sample. Special care was taken to ensure that the student sample represented many different lines of studies and that only Danish students participated. None had previous experience with playing the public good game. 81 student subjects took part in the experiment over 3 sessions.

Students vs. Non-students (Step 1). The cornerstone of the experiment is the repeated public good game. In this game, subjects are divided into groups of four and have to decide how to distribute an initial endowment of 20 DKK (≈3.7 USD) between them and a common pot. The subjects can decide freely how to split the endowment. The amount that they allocate to themselves is simply kept as individual earnings, whereas the

endowment that is allocated to the common pot is multiplied by 0.4, and the resulting amount is given to all group members. Therefore, the subjects face a dilemma between investing their endowment in the common pot, which while paying back less than the investment to the individual, benefits the group as a whole by a greater amount than the investment, and keeping the money for themselves.

The first task the subjects faced was a measure of their cooperative preferences, the so-called strategy version of the public good game (from hereafter the *Strategy game*). The design of this task follows that of Fischbacher, Gächter, & Fehr (2001).All four group members take two decisions; 1) Unconditional - subjects decide on an unconditional contribution to the public good, and; 2) Conditional - subjects decide on contributions conditioned on the average contribution made by the other three group members starting from 0 DKK and increasing in increments of 1 DKK to 20 DKK. Before playing the game, the subjects are told that the outcome of the Strategy game is determined by ex-post randomly selecting the unconditional choice for three of the four group members to be their contribution to the public good. However, for the final group member, the contribution is her/his conditional choice at the level of the average unconditional contribution of the three other group members. The main measure of the strategy game is the subjects' revealed profile of desired contributions conditioned on other group members' average contribution. This is considered to be the subjects' preference for cooperation.

The subjects then play the repeated public good game and have to decide how much of the 20 DKK endowment to contribute to the common pot. The decisions are made simultaneously. Once everyone has decided, feedback about the average contribution of the other group members is received, along with the resulting personal income. The choice is repeated 10 times and for each repetition new groups are formed - the so-called stranger matching protocol (James Andreoni & Croson, 2008).

Before receiving the feedback, the subjects are asked, on a separate screen, how they think the other group members behaved. The subjects had an incentive to reveal their beliefs accurately as they earned 5 DDK if their belief was not more than 1 DDK away (+/-) from the actual average. The belief decision was repeated in each period.

Manipulating preferences (Step 2). The aim of the second part of the experiment is to assess the robustness of the non-students' cooperation stability. Stress testing the stability is achieved by changing the preference composition of the interacting people. The experimental design of part 2 extends part 1 merely by the way in which groups are formed in the repeated public good game. Whereas in part 1 the subjects were randomly re-matched with any subjects, the subjects in part 2 are matched together with subjects who have similar preferences. In practice, the matching procedure splits the pool of subjects into two: one with the most

pronounced preference for cooperation and one with the least pronounced preference. The split was made so that the resulting sub-populations were of equal size[2]. Within each of the resulting sub-populations, stranger matching is applied across the 10 repetitions of the game. This procedure is carried out in two different treatments. In the 'PrefSplit' treatment, the matching procedure is unknown to the subjects, and they simply have to work out for themselves that they are interacting with people who have a similar preference. However, in the 'PrefSplitInfo' treatment, the same matching procedure is in place except that before the repeated game begins, the subjects are informed that they will interact with people who answered in a similar way to them in the strategy game. Besides these modifications, everything else is identical to part 1.

All instructions were provided on the individual computer screens and the subjects had the opportunity to review the instructions during the experiment. The instruction screen is presented in the appendix. The experiment was programmed in z-tree (Fischbacher, 2007). An overview of the treatments, number of participants, their age and gender is provided in Table 1.

Table 1 – Summary statistics

Treatment	n	Age	Female share
Step 1:			
Non-student	89	42.91	0.63
Student	81	25.77	0.47
Step 2:			

[2] The criteria for selection for the sub-population with the pronounced preferences for cooperation is based on previous evidence on Danish citizens (Fosgaard et al., 2014), to be a perfect conditional cooperator. This means that subjects in this sub-population preferred to contribute at least the same as they expected others to do. The experiment was designed so that if the sub-population did not contain half the total number of subjects, the subjects who were closest to the other sub-population were moved until the sub-population was of an equal size.

PrefSplit	Social Pref.	81	44.89	0.52
	Less social pref	84	42.52	0.49
PrefSplitInfo	Social Pref.	85	46.13	0.52
	Less social pref	85	46.33	0.55
Total		505	41.53	0.53

Selection bias

Selection bias is a possible concern when inviting ordinary people to the lab. In the current study, a unique opportunity for testing such selection bias exists, since register data on all non-student participants is available. This means that via Denmark's Statistics, I have access to the following information on all potential participants: background characteristics on home location, work location, type of work, income, civil status, number of children and their ages, etc. Importantly, such information is not limited to those who completed the experiment, but instead it is available for all invited participants.

The selection bias consists of several hurdles, which all need to be successfully passed in order to become a participant. The first selection hurdle is that the invited individual has to make a decision regarding whether to opt for interview protection, which Statistics Denmark offers all Danish citizens. The arrangement implies that a protected individual cannot be contacted for research purposes. The second hurdle is that the invited individual has to respond to the invitation letter, log in to the recruitment website and read about the experiment. If that hurdle is successfully passed, the next is to actually sign up for participation, which is followed by the final hurdle – actually showing up for the chosen session.

To assess the effect of these hurdles, a probit for each hurdle that explains whether an individual has passed the hurdles is regressed on the register variables. Several of the variables turn out to be significant explanations for passing the hurdles. A full overview of the results is available in the appendix. To account for selection bias from each hurdle, a Mills ratio of the regressions is created and then added to regressions that explain public good contributions in the experiment. The results from these regressions are also listed in the appendix. Importantly, none of the Mills ratios are significant in explaining the contributions and including them does not affect the magnitude of the coefficient, which suggests that the detected selection bias has no significant effect on contributions.

Step 1: Comparing students and non-students

The result of the repeated public good game is illustrated in Figure 1. As predicted, a dramatic difference in cooperation stability exists among students and non-students. Evaluated by means of a Mann-Whitney non-parametric test, the individual average contributions in the two groups are found to differ significantly

(z=5.747, p=0.000). In a simple OLS regression with period as explanatory variable (see appendix), cooperation stability is found to decrease significantly more among the students than the non-students (Coef.: -0.405, p=0.000). The difference in stability is also highlighted by calculating the individual change in contribution from the first to last period. The median change for the students is -5 DKK, whereas it is 0 DKK for the non-students.

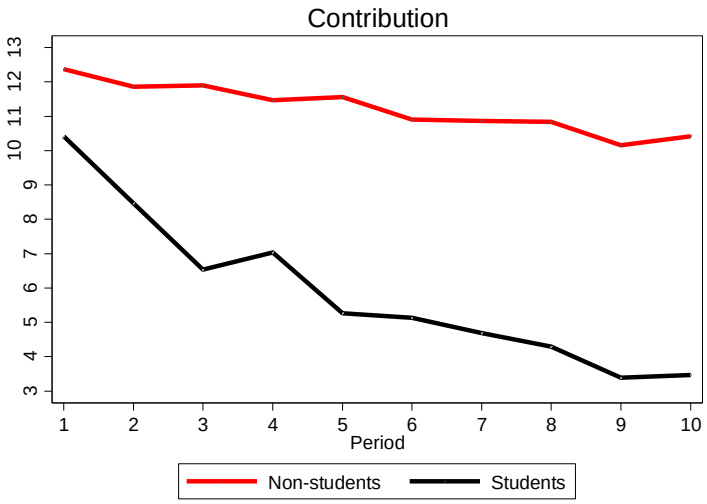

Figure 1 - Repeated cooperation

The heterogeneity of the individual cooperation stability in the two samples is illustrated in Figure 2, which shows the time trend from individual regressions on the horizontal axis and the individual mean contribution across all periods on the vertical axis. It is immediately clear from the figure that a large degree of variation exists in both samples, but it is also noticeable that a large fraction engage in free rider behavior (zero slope and zero mean) and unconditional full cooperation (zero slope and positive mean). Importantly, despite these similarities, the plots differ according to how the slopes are generally distributed around zero. The non-students are more or less equally distributed around the zero slope, whereas the students' slopes are much more frequently located in the negative domain: The slopes are found to differ significantly (Mann-Whitney, z=4.194, p=0.0000).

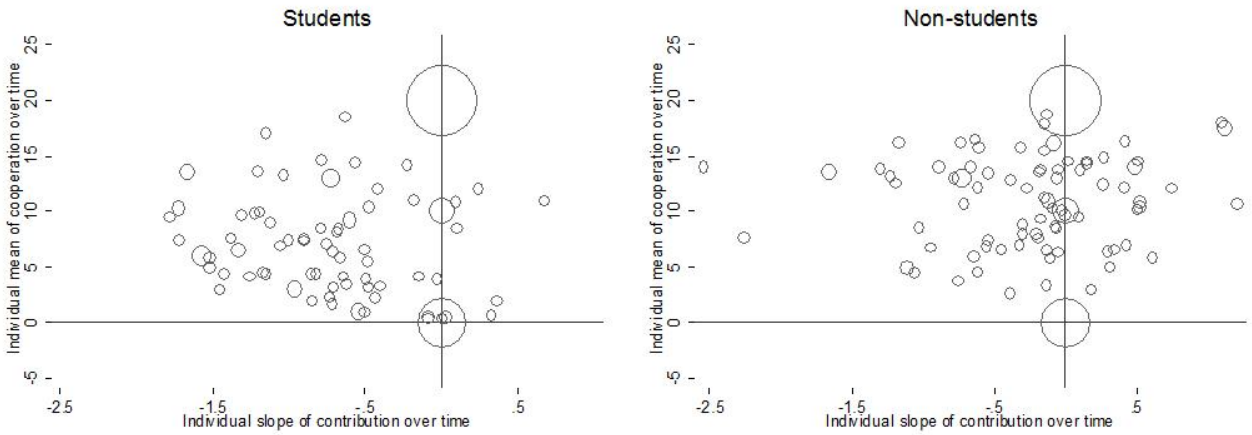

Figure 2 - Stability heterogeneity (the bigger the bobble, the more observations it represents)

To better understand what causes the difference in cooperation stabilities, studying the underlying belief as to how others will behave and the cooperation preferences can potentially provide insight into the different processes taking place in the two populations (Dufwenberg, Gächter, & Hennig-Schmidt, 2011; Fischbacher & Gächter, 2010; Fosgaard et al., 2014).

The individually measured **belief** (expectations) of other group members' average contribution, which was measured in every repetition of the game, is illustrated in Figure 3a. The belief patterns clearly differ between students and non-students (Mann-Whitney, z=20.307, p=0.000), with the non-students' being significantly more stable over time than the students' beliefs, as evaluated by the time trend in an OLS regression (see appendix). Interestingly, the non-students' beliefs and contributions do not differ significantly (Signrank test, z=0.949, p=0.3427), whereas the students' beliefs and contributions do (Signrank test, z=-4.642, p=0.000). In sum, belief formation seems to be an important explanation for the varying cooperation stability of students and non-students.

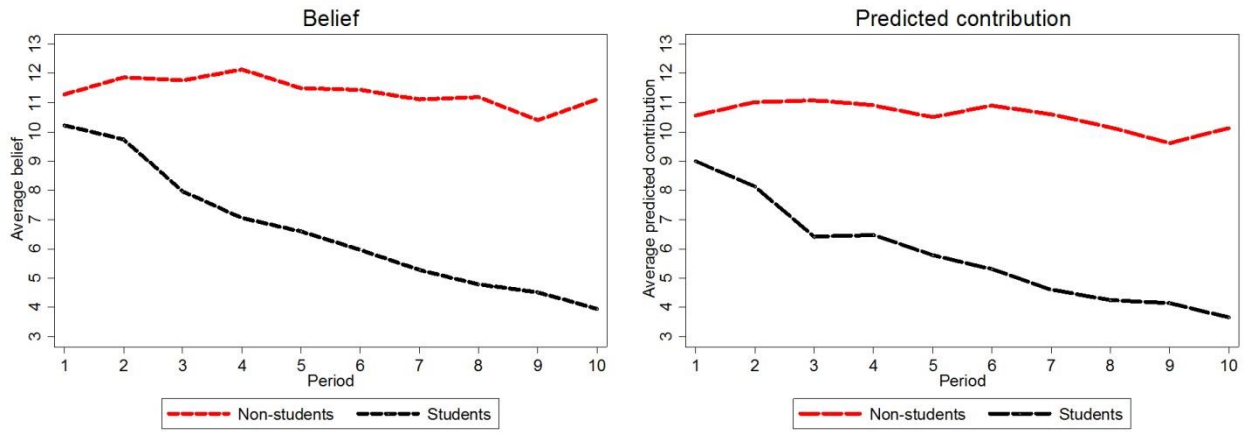

Figure 3 - A) Expectations regarding other groups' average contribution B) Preferred contribution based on belief measure and preference profiles

Preferences for cooperation is another potential explanation for the difference in cooperation stability across students and non-students. The participating subjects' preferences are measured in the strategy game, and the resulting profiles are categorized as different types (such as free riders, conditional cooperators, see Fischbacher et al. 2001). The distribution of these preference profiles is presented in Table 2. The preference categorization differs significantly across the two samples (Pearson chi square test, chi2(6)=14.1889, p=0.028). Among non-students, there are fewer conditional cooperators and free-riders, but more unconditional cooperators.

Table 2 – Distribution of preference types

%	Non-student	Student
Conditional Cooperator	47.19	56.79
Perfect Conditional cooperator	25.84	24.69
Free Rider	5.62	11.11
Unconditional Cooperator	12.36	0.00
Triangle Cooperator	5.62	6.17
Negative Conditional Cooperator	2.25	0.00
Other types	1.12	1.23

Another possible way to operationalize the preference measure is to find each subject's preferred contribution, in each period, by looking up the subject's preferred contribution in the strategy method for a given stated belief. The preference profile is stable and measured once, but the belief can change during the course of the repeated public good game, which means that the preferred contribution measure may vary.

The preferred contribution measure over time is illustrated in Figure 3b. Similar to the beliefs, the preferred contributions differ dramatically across the students and non-students, which is further supported by a Mann-Whitney test (z=14.869, p=0.000). For both students and non-students, the preferred contribution differs significantly from the contributions derived when considering all periods (students: z =2.029, p=0.0425, non-student: z=4.969, p=0.000). However, in the latter half of the periods, they do not differ for any of the samples (students: z=-0.121, p=0.9037, non-student: z=1.398, p=0.1622). These findings indicate that preferences may also be a potential explanation for the difference in cooperation stability.

So far, both the belief and the preference (preferred contribution) measures have shown potential for explaining the difference in cooperation stability. In order to judge their influence jointly, the two measures are used as explanatory variables in a regression. Table 3 lists the results. A separate regression for each sample is performed with belief, preferences, and period as explanatory variables. A subsequent joint regression with sample interaction terms was performed, which resulted in significant differences in the effect of the explanatory variable. The result of the joint regression is presented in the last column of Table 3.

Table 3- OLS regressions explaining contributions

	Contribution		
	(1)	(2)	
			Sign. Δ
Sample:	Non-students	Students	
Belief	0.562***	0.267***	Yes
	(0.0501)	(0.0627)	
Preferred contribution	0.417***	0.668***	Yes
	(0.0301)	(0.0464)	
Period	-0.118**	-0.134**	No
	(0.0556)	(0.0633)	
Constant	1.091*	1.271**	Yes
	(0.621)	(0.582)	
Observations	890	810	
R-squared	0.426	0.490	

What is evident from the regressions is that the contributions in both samples are significantly and positively affected by both belief and preferred contribution, albeit to different extents. The effect of belief is much greater for the non-students than it is for the students (significantly different). A similar effect, but in the opposite direction, is found for the preferences; students react significantly more to a unit increase in preferred contribution.

Interestingly, the regressions also reveal that when these two choice determinants are accounted for, there is still downward sloping trend in the contributions over time, while the remaining decay effect is not

significantly different among students and non-students, which suggests that the difference in cooperation stability is indeed associated with belief and preferences.

Another interesting avenue of research which is inspired by the analysis of (Fischbacher & Gächter, 2010), is to understand how beliefs themselves are determined. In Table 4, a separate regression for the student and the non-student populations is presented. The regression treats belief as dependent variable and the lagged belief and the lagged average contribution of the other group members as independent variables. The third column presents the results from a joint regression with the same explanatory variables, but also with interaction terms to determine whether the influence varies across the two populations. What is apparent from the table is that the belief formation is enduring in the sense that an important determinant of a belief in a certain period is the belief held in the previous period. This influence is not significantly different across students and non-students. However, the belief is also updated based on the average contribution of other group members in the past period. The coefficient is significantly larger for students than it is for the non-students. In other words, students adjust their belief more for a certain change in group members' average contribution. Furthermore, the non-students have a significant constant which the students do not.

Table 4- OLS regressions explaining contributions

	Belief		
	(1)	(2)	
			Sign. Δ
Sample:	Non-students	Students	
Belief [t-1]	0.434***	0.401***	No
	(0.0272)	(0.0208)	
Other contribution [t-1]	0.398***	0.525***	Yes
	(0.0272)	(0.0204)	
Constant	1.912***	0.278	Yes
	(0.449)	(0.176)	
Observations	801	729	
R-squared	0.371	0.705	

Simulation. Ultimately, the goal of the present analysis is to understand what causes the cooperation stability difference to occur. Until now, both belief and preferred contribution have had the potential to explain the cooperation. To get closer to the source of the difference, I set up a counter-factual simulation in order to determine their relative importance.

The basic structure of the simulation follows that of Fischbacher & Gächter (2010). In practice the contributions are simulated by the models regressed in Tables 3 and 4. The aim is to simulate the contribution of the non-students over time. In practice, this is done in the following way: First, belief is determined by model 1 in Table 4: that is the sum of lagged belief (multiplied by the coefficient), other group members' contribution (multiplied by the coefficient), and the constant determines this period's belief. The actual

contribution of other group members and the belief in period 1 are taken as given, but in subsequent periods, belief is determined in accordance with model 1 in Table 4.

Cooperation is determined as follows: In period 1, the actual contribution is taken as given, hereafter the contribution is determined by model 1 in Table 3. That is the sum of simulated belief (resulting from the determination outlined above) multiplied by the coefficient, predicted contribution (using simulated belief, as explained above) multiplied by the coefficient, period multiplied by the coefficient, and the constant.

Dynamically, the simulated measures of contribution, belief, and others' contribution are used as inputs to predict belief and contribution in the following period. This successive calculation continues for all remaining periods. In the end, I have a sequence of recursively simulated contributions over all periods.

The basic simulated contribution should follow the actual contribution if the regressions are sufficiently accurate. If the simulation does indeed predict the actual contribution of the non-students, then the framework allows the coefficients in front of each of the variables entering the simulation to be manipulated, and the resulting effect on contribution can be observed, i.e. a counter-factual analysis. There are five coefficients across the two models in Tables 3 and 4, which can potentially be altered. Essentially, the applied simulation allows me to determine the importance of the coefficients by changing them one at a time.

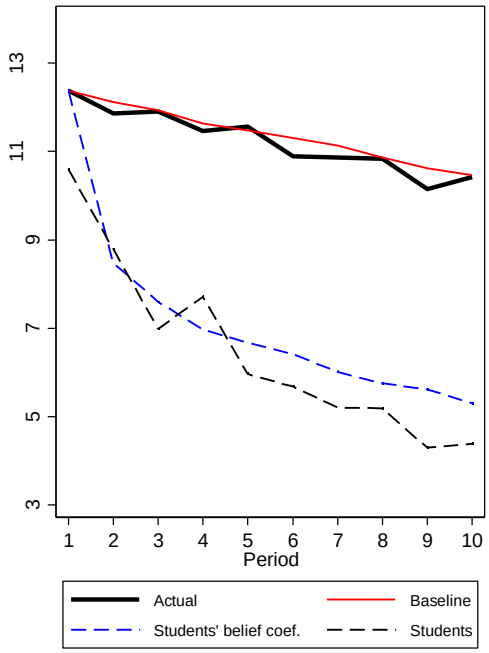

Figure 4 - Counterfactual simulation of contribution stability

The simulated contribution over time does approximate the actual data very well. This is illustrated in Figure 4, where the bold black line is the actual data and the thinner red line is the simulated behavioral patterns based on the regressions. Using a Mann-Whitney test, it is observed that the simulated and the actual contribution do not differ (z=-0.322,p=0.748).

With the simulation in place, it is possible to manipulate one coefficient of the simulation at a time and observe the resulting cooperation over time. In order to understand what is causing the difference between cooperation stability among non-students and students, I investigate what happens to the non-students' simulated cooperation over time if I impose the coefficients from the students' regressions one by one. There are five coefficients which I can manipulate, two in the belief regression (table 4) and three in the cooperation regression (table 3).

Concretely, my aim is to observe which of the coefficient manipulations changes the non-students' behavioral patterns so that they look like the students'. In the appendix, I present the results of all 5 possible coefficients which I change. However, it turns out that inserting the students' belief coefficient into the non-students' simulation results in simulated contribution that is very close to that of the students. This manipulation is illustrated in Figure 4. The dashed blue line is the non-student simulation, but with the students' belief coefficient and the dashed black line is the students' actual behavior. As seen, the dashed black and blue lines follow each other closely, which suggests that the key determinant of the difference between the students' and non-students' cooperation stability is the behavioral response to a certain belief. Importantly, similar manipulations with the students' coefficients for lagged belief, others' contribution, preference or period do not create the same patterns (see the results in the appendix).

To compare the different simulated contributions, I have created a regression of contribution over time for each possible simulation and included a dummy for the simulated results and an interaction between simulation and period. If the simulated contribution perfectly matches the observed data, the dummy and the interaction variable should be insignificant. The result of these regressions can be found in the appendix. The conclusion from these regressions is that the only coefficient variation of the non-students' simulation which gives a result similar to the students' behavior is when the students' belief coefficient is inserted. In sum, the simulation of the non-students reproduces the actual non-student data well, but once the students' belief coefficient is inserted into the non-student simulation it mimics the students' behavior.

Step 2: Stress testing cooperation stability

Having established that cooperation is much more stable among non-students compared with students, step 2 of the analysis tests the robustness of this stability. As hypothesized earlier in the paper, the preference is

expected to matter for cooperation stability. Varying the preference composition allows me to test how sensitive the cooperation stability is to such ‘’shocks”. The simulation from step 1 of the analysis can be used to empirically illustrate the hypothesis. In the basic simulation of the non-students’ repeated cooperation, I set the preference to be of perfect conditional cooperator types. Figure 5 illustrates the result of making this change to the simulation. After an initial adjustment, the cooperation becomes entirely stable, which further supports the assertion that letting people with at least perfect conditional cooperator preferences interact should result in stable cooperation.

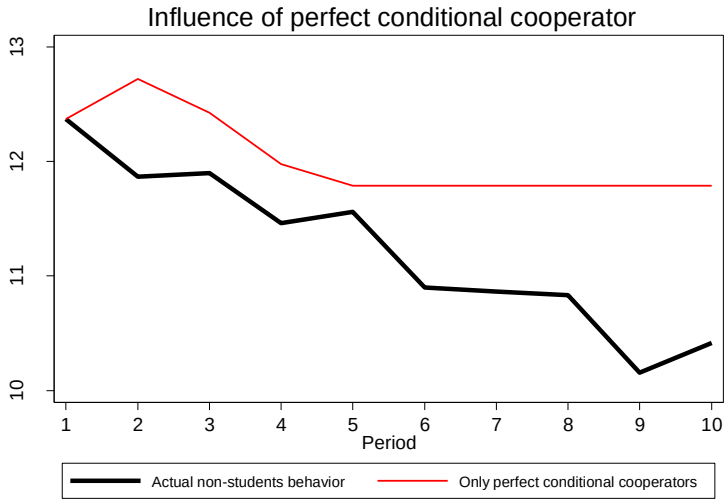

Figure 5 – Simulation of non-students’ cooperation if perfect conditional cooperation is assumed

Before reporting the cooperation result, I check to see whether varying the preference composition did, in fact, work. To do so I use the data from the strategy game and plot the average response of each level of contribution given by others. The result is illustrated in Figure 6. It can be seen that splitting based on preference did work. The average of conditional cooperation is systematically higher in the ‘Social’ groups compared to the ‘Less social’ group in both the PrefSplit and the PrefSplitInfo treatments, and the intersection with the diagonal is close to 20 for the socially-minded population compared to an intersection below 5 for the less social population. In order words, the preferences in the two groups predict contribution patterns at very high and very low levels respectively.

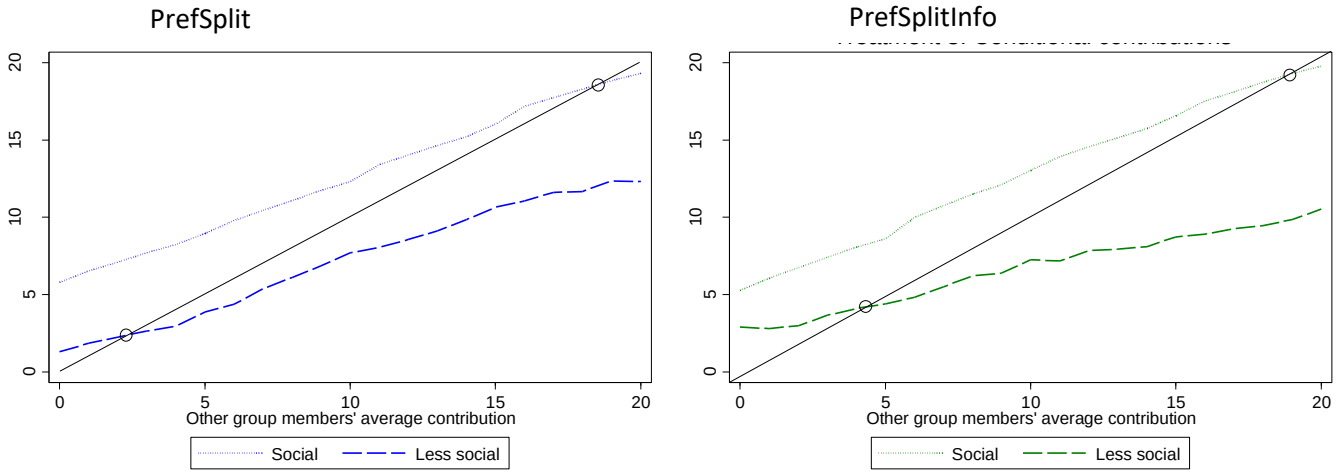

Figure 6 - Average conditional contributions

Cooperation. I label the data from the non-students without any spliting as Baseline (which was reported above) when analyzing the result in step 2. I find that the Baseline and the PrefSplit treatment are almost entirely parellel, see Figure 7. In support of this, the average individual contributions are found not to be significantly different (Mann Whitney test: z=0.455, p=0.6492), allthough there is some tendency for PrefSplit to decline in the last half of the periods compared to the baseline, which suggests a similar initital process, although deteriorating interaction on average as the participants experience the collaboration. However, the difference in the last half is not significantly different (Mann Whitney: z=1.005, p=0.3148).

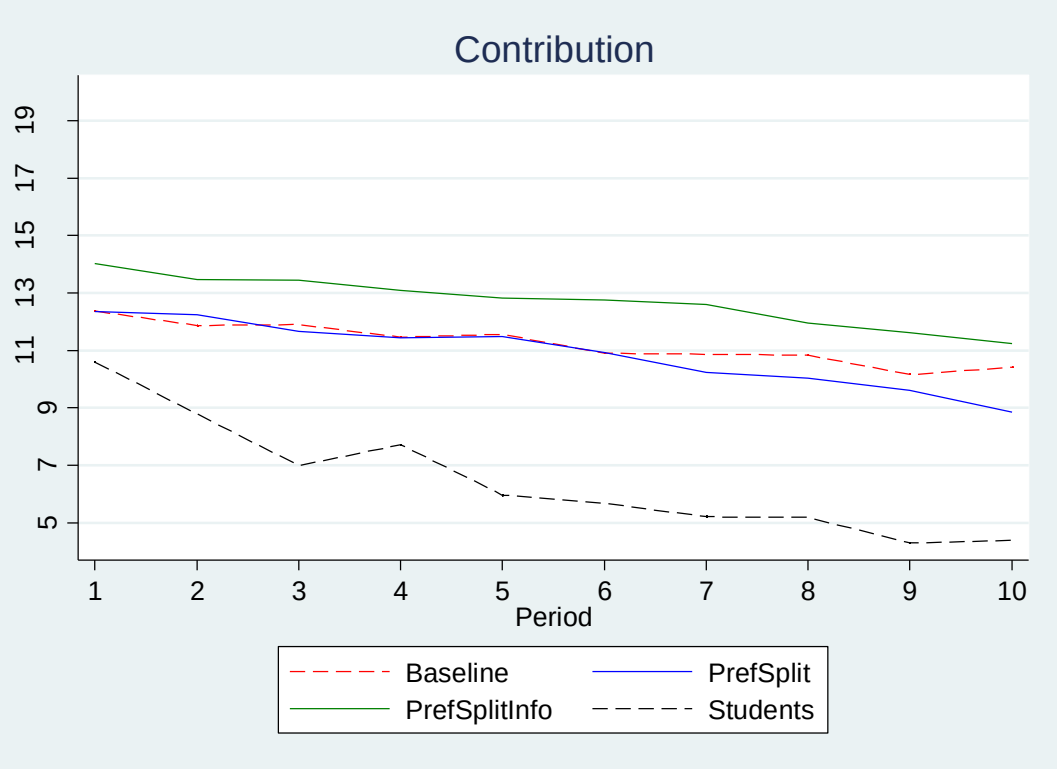

Figure 7 – Cooperation over time

The PrefSplitInfo treatment exhibits a very similar pattern over time, alltough the cooperation level in this treatment systematically is at a high level (Mann Whitney: z=-3.283, p=0.0010). In order to draw some conclusions about the cooperation stability, I carry out a random effect OLS regression explaining the cooperation with treatment dummies, period, and interactions. The result can be found in the second column of Tabel 4. The regression confirms that cooperation is systematically at a higher level in the PrefSplitInfo, thus suggesting that, overall, letting people collaborate with like-minded individuals and telling them about it is beneficial. But is the positive level effect eroded by the cooperation (in)stability? An overall decay effect is indeed found - and it turns out to be even stronger when subjects are split according to their preferences (Period x PrefSplit). However, this effect vanishes when the split is acompanied by information about the split (Period x PrefSplitInfo). Therefore, in general, it is possible to conclude that, in terms of cooperation stability, letting people interact in groups of like-minded individuals does not affect the decay when information is provided, while the split has a positive level effect. At this stage, I have addressed the average effect, but I still do not know what effect the splitting had on the separate groups of preferences.

Cooperation – types. In the following, I address the cooperation stability of the sub-groups in PrefSplit and PrefSplitInfo and contrast them with the baseline treatment of non-students. As a reference, the baseline treatment is also split into social and less social types using the same procedure as used in the other

treatments. The only differences is that the social and less social types *did* interact in the baseline treatment, which makes it possible to measure the effect of letting the same types interact, while adding information about this.

It is evident from Figure 8 that the social types in both the PrefSplit and the PrefSplitInfo treatments do not have stable, or increasing, cooperation as otherwise predicted by the composition of their preferences. In fact, the individual average contributions are not different across the social types in the baseline and PrefSplit (Mann-Whitney: z=-0.932, p=0.3514), but they are significantly higher for the social types in PrefSplitInfo compared to the rest of the social types (Mann-Whitney: z=-3.055, p=0.0023). To determine the cooperation stability, I carry out a random effect OLS regression with period as the explanatory variable, and an interaction term between period and the treatment. The result is presented in the third column of Table 5. I find that a significant decay effect exists, although it is not significantly different across the social types in baseline PrefSplit, and PrefSplitInfo. These findings suggest that against the odds, groups with social preferences for stable or increasing cooperation also result in (minor) downward sloping interactions.

For the groups of subjects who have a tendency to be "less social" for whom rapidly decaying cooperation is expected, I also find surprising results. Their cooperation over time is remarkably stable in light of their social preferences. Whereas the stability is not very different across the groups, the levels of cooperation do differ. For the less social type, average individual contributions in PrefSplitInfo are significantly higher than those in PrefSplit (Mann-Whitney, z=-3.879, p=0.0001). The baseline is not significantly different from the PrefSplit or the PrefSplitInfo treatment. But what about stability? In a regression similar to that of the social types, the results (presented in the fourth column of Table 5) reveal that the decay effect is significantly stronger among the less social types in PrefSplit compared to the less social types in the baseline. Interestingly, when the split of subjects is announced, the negative effect on the decay is neutralized. It seems that letting types with less social preferences interact does result in a more rapid collapse in cooperation, as predicted. However, 'the steeper decay effect returns to the starting point when the subjects are aware of the, which is consistent with an in-group effect, which supports the contention that people are more sympathetic towards people who belong to the same group (C. Bram Cadsby, Du, & Song, 2016; Fu et al., 2012).

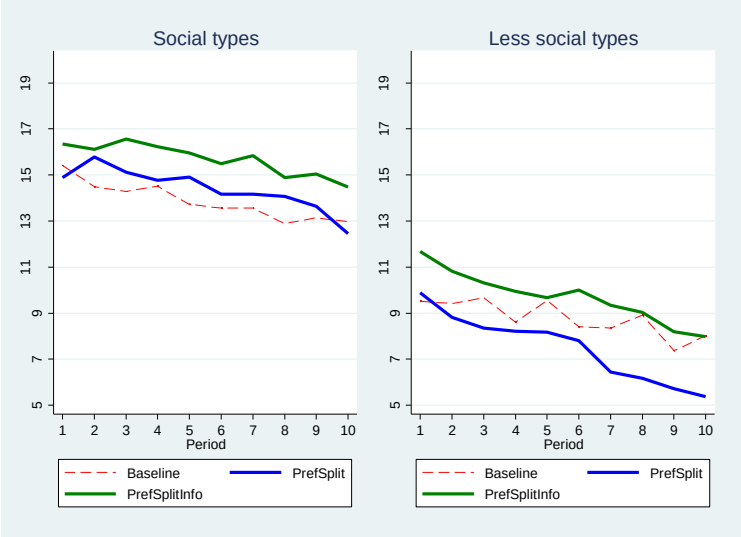

Figure 8 – Contribution stability across social types and treatments.

Table 5 - Random effect OLS regressions

	All	Social types	Less social types
PrefSplit	0.475	0.647	0.228
	(0.62)	(0.66)	(0.27)
PrefSplitInfo	1.783*	1.604	1.782*
	(2.35)	(1.64)	(2.11)
Period	-0.226***	-0.251***	-0.203**
	(-4.76)	(-3.76)	(-3.01)
Period x PrefSplit	-0.150*	-0.02	-0.276**
	(-2.55)	(-0.24)	(-3.28)
Period x PrefSplitInfo	-0.058	0.044	-0.157
	(-0.98)	-0.53	(-1.88)
Constant	12.477***	15.234***	9.900***
	(20.26)	(19.15)	(14.55)
R-squared	0.035	0.029	0.068
Individuals	424	209	215
N	4240	2090	2150

** p<0.05, ** p<0.01,*** p<0.001*

Note: Dependent variable is public good contributions. The second column refers to a regression of all non-student subjects, whereas columns three and four regress the contributions of subjects with the most and least social preferences respectively. Subjects in the baseline treatment are used as the reference group.

The evidence in this second step of the analysis was intended to test the cooperation stability among non-students. It turned out that the findings were largely not as hypothesized. Although the less social types do

show a steeper decay effect as predicted, this disappears when they are informed that they are interacting. Step 2 failed to produce stable or increasing cooperation for the group with preferences, which should have resulted in such patterns, regardless of whether information was supplied. The evidence highlights the fact that the remarkable cooperation among non-students observed in step 1 is robust to quite substantial changes in the preference composition.

The findings in part 2, to a certain extent, challenge the existing literature. Gunnthorsdottir et al. (2007) find that matching subjects based on their social preferences resulted in more stable cooperation. I do not find this. Furthermore, Angela C. M. de Oliveira, Rachel T. A. Croson (2015) found that stable cooperation occurred when a sufficiently high share of conditional cooperators were present in a group - a finding which is not supported in my data. However, what *does* echo the existing literature is the fact that information on preference division has a positive effect on cooperation.

Conclusion

In this paper, I had the unique opportunity of bringing a representative sample of the population in to the lab to play a repeated public good game. I find that repeated cooperation among non-students is dramatically more stable than cooperation among students. In fact, they approach almost stable cooperation. Belief about others' cooperation and preferences can potentially explain this difference in cooperation stability. I show that the non-students have a much smaller reaction to belief compared with the students. The importance of this difference in belief is underlined in a simulation of the non-students' cooperation behavior. If the non-students belief reactions are swapped with the students' reactions to belief, the non-students' simulated contribution becomes identical to the students' actual contribution. In the second step of the analysis, I explore how sensitive the cooperation stability of non-students is by deliberately creating pools of subjects with preferences that should result in decreasing or stable/increasing cooperation. I observe that employing these different subject pools has remarkably little influence on the resulting cooperation stability, although the subject pool with the least social subjects did exhibit slightly faster decay in cooperation. Randomly, I inform subjects about these different pools. For the majority of social subjects, this information does not affect their cooperation stability, but for the least social subjects it does, surprisingly, slow down their decay effect.

The findings in the present study contribute to the large literature on repeated cooperation. The very robust finding of rapidly decaying cooperation is not found among the representative sample who complete the public good game under the exactly same circumstances as the regular lab studies with students. Essentially, my findings question the external validity of the cooperation decay effect observed among students.

The study also adds to the dominate view of why cooperation decays. Fischbacher & Gächter (2010) conclude that the decay effect is the result of subjects having, on average, less than perfect conditional cooperator preferences. Yet, in contrast to this conclusion, I show that cooperation is not stable if I ensure that the interacting subjects have, at least, perfect conditional cooperation preferences, regardless of whether the subjects are informed about this. This suggests that the preference composition is not the only explanation for the decay. Through my simulation of the non-students' cooperation, I show that belief about others' behavior has a critical influence on the decay effect. It is interesting to consider why students and non-students respond differently to the beliefs. A recent theory by Bénabou and Tiróle (2016) states that a trade-off occurs between accuracy and desirability for having a certain belief. One interpretation could be that non-students put relatively more emphasis on desirability compared with students.

Finally, the findings may also be relevant for policy makers who want to influence cooperation stability without disciplinary mechanisms such as punishment. This paper suggests that voluntary cooperation is not as far from being stable as previously thought, and that a potentially fruitful channel for creating stable cooperation is by influencing reactions to belief about other people's behavior - perhaps by stimulating a greater focus on desired beliefs.

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Appendix A: Invitation letter (In Danish)

DANMARKS STATISTIK

1. maj 2012

Undersøgelse fra Danmarks Statistik.

Danmarks Statistik og Københavns Universitet inviterer dig hermed til at deltage i et eksperiment om hvordan danskere samarbejder med hinanden.

For at opnå et repræsentativt billede har Danmarks Statistik udvalgt et stort antal personer fra hele Københavnsområdet, som får muligheden for at deltage i eksperimentet. Du er blandt de tilfældigt udtrukne. Din deltagelse er naturligvis frivillig, men vi håber meget at du vil deltage i dette forskningsprojekt, og dermed bidrage til dansk grundforskning.

For at dække dine transportomkostninger får du 300 kr. for at møde op til eksperimentet. Derudover har du mulighed for at tjene flere penge i selve eksperimentet. Vi kan ikke garantere dig, at du vil tjene et bestemt beløb i eksperimentet, idet din indtjening vil afhænge af dine egne samt andre deltageres beslutninger. Men typisk vil du tjene mellem 100 kr. og 400 kr. ekstra. Til eksperimentet serverer vi desuden kaffe, te, sodavand, frugt og kage.

Eksperimentet foregår på Københavns Universitet, Øster Farimagsgade 5, 1353 København K, Bygning 5, i perioden fra 23. maj til 7. juni 2012. Eksperimentet varer to timer, og du kan kun deltage én gang. For at tilmelde dig, bedes du **hurtigst muligt**, logge ind på vores tilmeldingshjemmeside. Her kan du vælge de dage og de tidspunkter der passer dig bedst. På grund af et begrænset antal pladser, fungerer tilmeldingen efter først-til-mølle princippet.

Du tilmelder dig på følgende hjemmeside:

www.ku-eksp

med din login kode: SBPDW20367

Til selve eksperimentet skal du **medbringe dette brev** – så gem det, og pas godt på det. Du skal også medbringe dine **kontooplysninger**, da de penge du tjener udbetales som en bankoverførsel efter eksperimentet er gennemført.
Skulle du have problemer med at logge ind eller har du yderligere spørgsmål, er du velkommen til at kontakte Københavns Universitet på email: tf@foi.ku.dk eller telefon 35 33 68 67.

Anonymitet og spørgsmål
Danmarks Statistik står inde for, at dine svar behandles fortroligt og at du optræder fuldstændig anonymt. Du vil derfor ikke på nogen måde kunne identificeres.

Venlig hilsen

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Chef for Interviewservice
Danmarks Statistik

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dst@dst.dk
www.dst.dk

Referencenr.: 11188

Invitation letter (Translation):

Study from Statistics Denmark

Dear XXX

Statistics Denmark and University of Copenhagen hereby invite you to participate in an experiment about how Danes cooperate with each other.

To obtain a representative result, Statistics Denmark has selected a large sample of people from the greater Copenhagen area, who have been given the opportunity to participate in the experiment. You are among those who have been randomly selected. Your participation is obviously voluntary, but we sincerely hope you will participate in this research project, and thereby contribute to important Danish research.

To cover your travel costs, you will receive 300 DKK for showing up for the experiment. Besides this, you will have the opportunity to earn more money during the actual experiment. We cannot guarantee that you will earn a certain amount in the experiment because your earnings will depend on your and others' decisions. But typically you will earn between 100 DKK and 400 DKK in addition to the initial 300 DKK. We will also serve coffee, tea, soft drinks, fruit, and cake at the experiment.

The experiment will be carried out at the University of Copenhagen, Øster Farimagsgade 5, 1353 København K, building 5 in the period from Maj 23rd until June 7th 2012. The experiment lasts for two hours and you can only participate once. To register, we kindly ask you to log on to our recruitment website **as soon as possible**. Here you will be able to choose the day and timeslot which suits you the best. Due to the limited number of seats, the registration is organized on a first come first served basis.

You can register on the following website:

www.ku-eksperiment.dk with your login code.

For the actual experiment, you should bring this letter – so please keep it somewhere safe. You also have to bring your bank account details because your earnings from the experiment will be paid out as a bank transfer after the experiment.

If you have problems logging in or have any further questions, please do not hesitate to contact the University of Copenhagen by email tf@foi.ku.dk or phone: 35 33 68 67.

Anonymity and questions
Statistics Denmark guarantees that your answer will be completely confidential and that you will be anonymous. Therefore, it will not be possible to identify you personally.

Best regards,

Isak Isaksen
Head of interview service
Statistics Denmark

Toke Fosgaard
Researcher
University of Copenhagen.

Appendix B: Instructions & selected screen shots (in Danish)

Welcome page

Translation:

Thanks for your interest in the experiment.

Your participation is very valuable because you by completing the experiment is contributing to research in social science.

In the experiment you earn money. This is the standard in economic experiments to ensure that the decisions have real consequences. You get 300 DKK for showing up for the experiment. In addition, you earn more. The addition earnings depend on your and others' decision, and will typically be between 100 DKK and 400 DKK. The amount is paid out as a bank transfer after the experiment is completed.

You remain entirely anonymous towards the other participants and the researchers behind the experiment. To maintain your anonymity, we do not know your login code. We begin with you reading some instructions about the experiment, and you should answer some questions about these instructions.

Press continue for go on to the instructions.

General instruction

Translation:

Instructions

We begin with you reading some instructions and answering some questions about them. After this the experiment will begin. It is important that you read the instructions carefully. You are randomly in a group with 3 other participants. None of you will know who the others are. The other participants are here today, but might sit in another room.

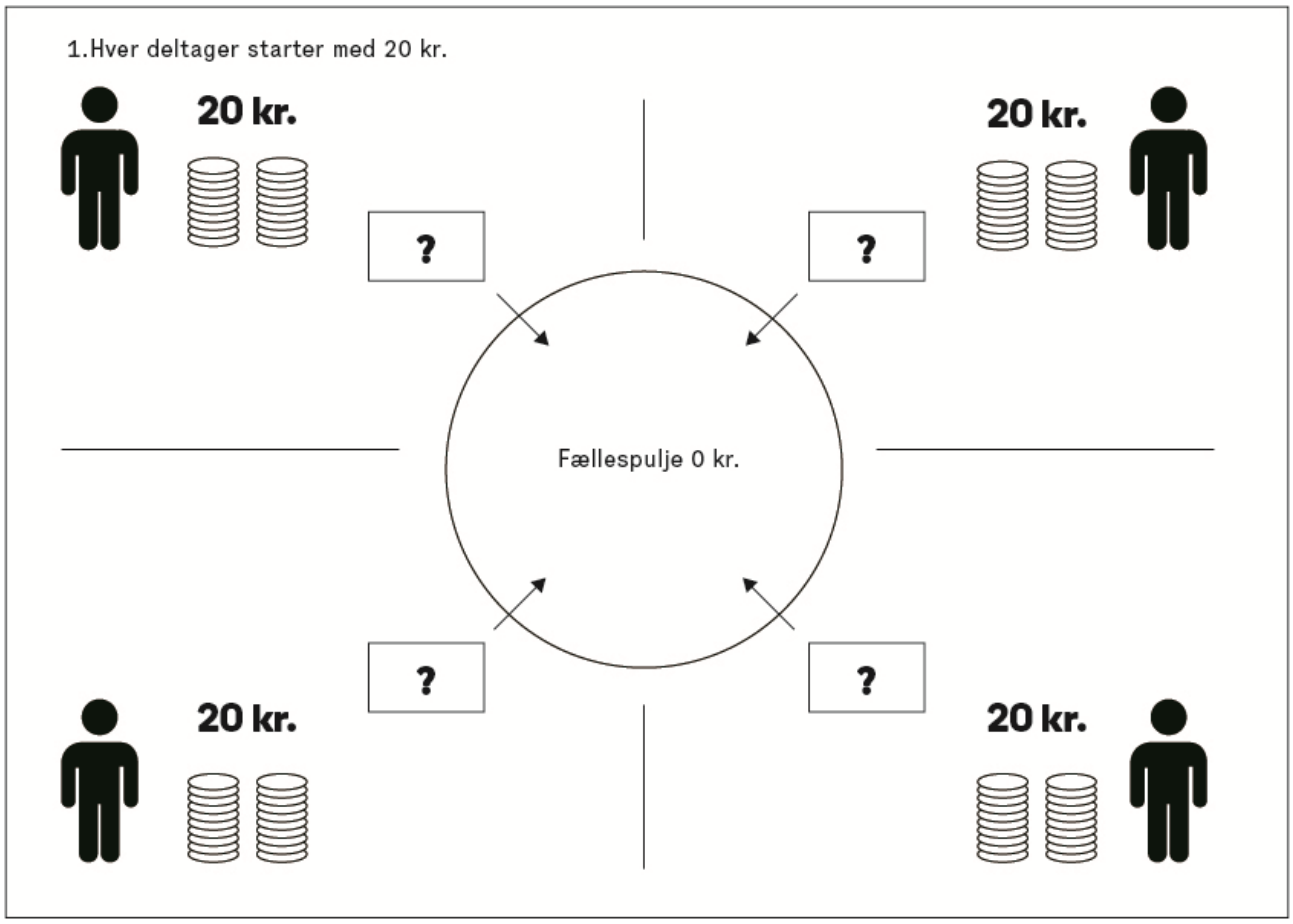
Every participant receive a starting amount of 20 DKK from us. You and the other group members have to make a decision about keeping the money or contribute some, or all, of them to a common pot. Every group member is facing the same decision.

The money you decide to keep is simply yours. The amount contributed to the common pot will first be increased by 60% and then shared equally among all four group members. Every group members get her/his equal share regardless of how much the person herself/himself put in the common pot.

Below a slideshow present an example of how the game works. Please click on “Start slideshow”.

Notice that each participant is deciding without knowing what the other group members decide.

Slideshow page 1

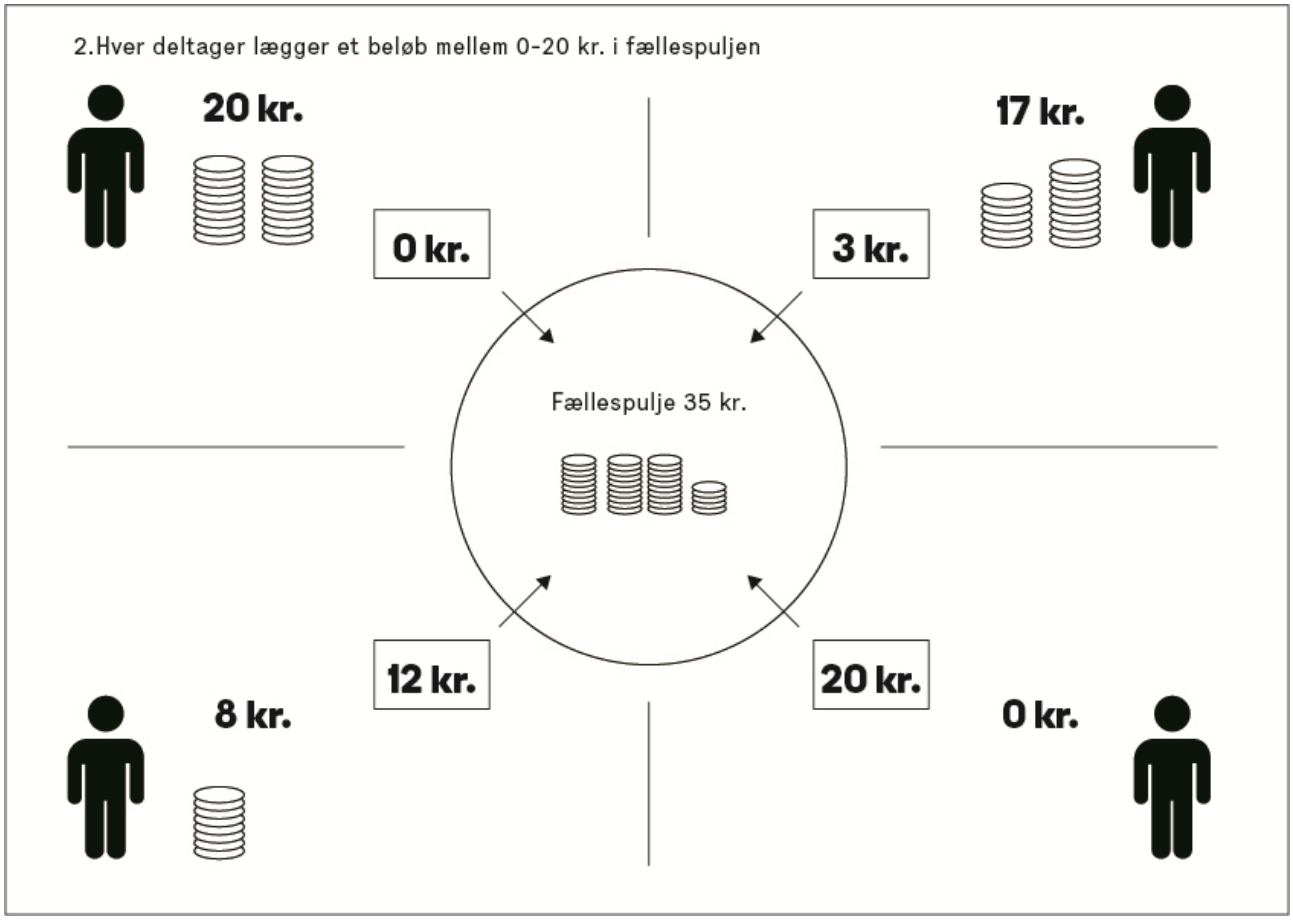


Translation:

Every participants starts with 20 DKK.

-

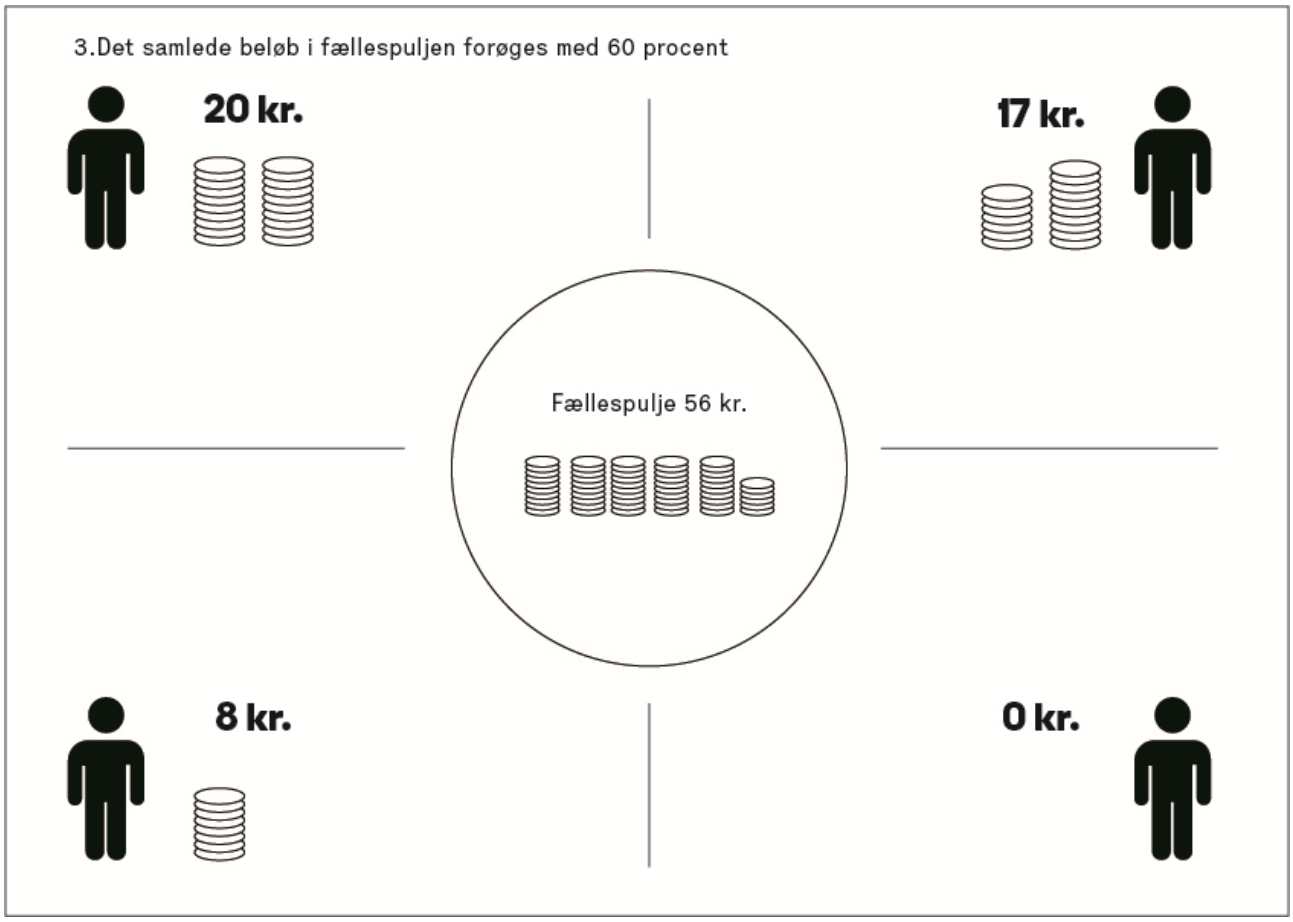
Slideshow page 2



Translation:

Every participant put an amount between 0-20 DKK in the common pot.

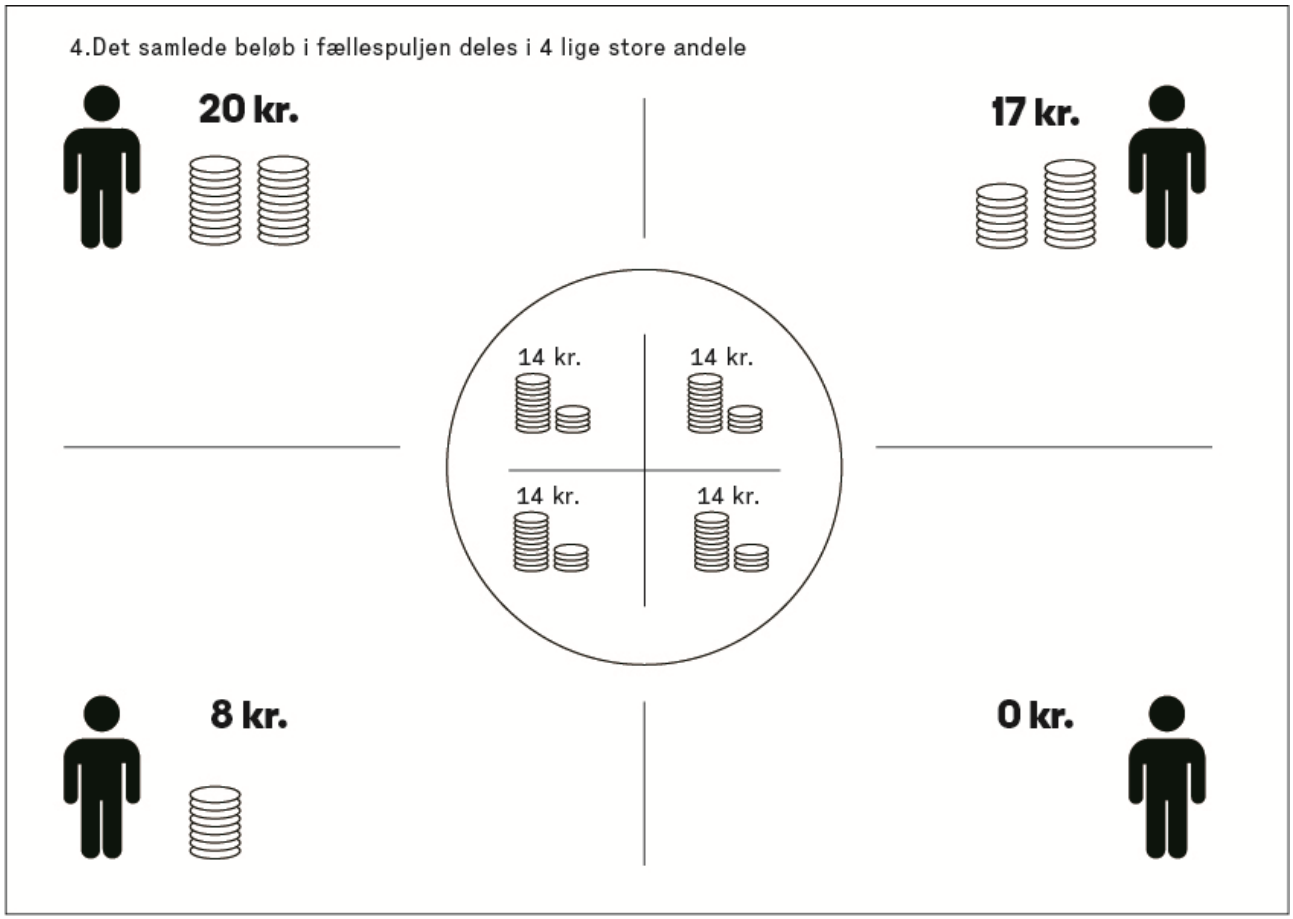
Slideshow page 3



Translation:

The total amount in the common pot increases with 60 percent.

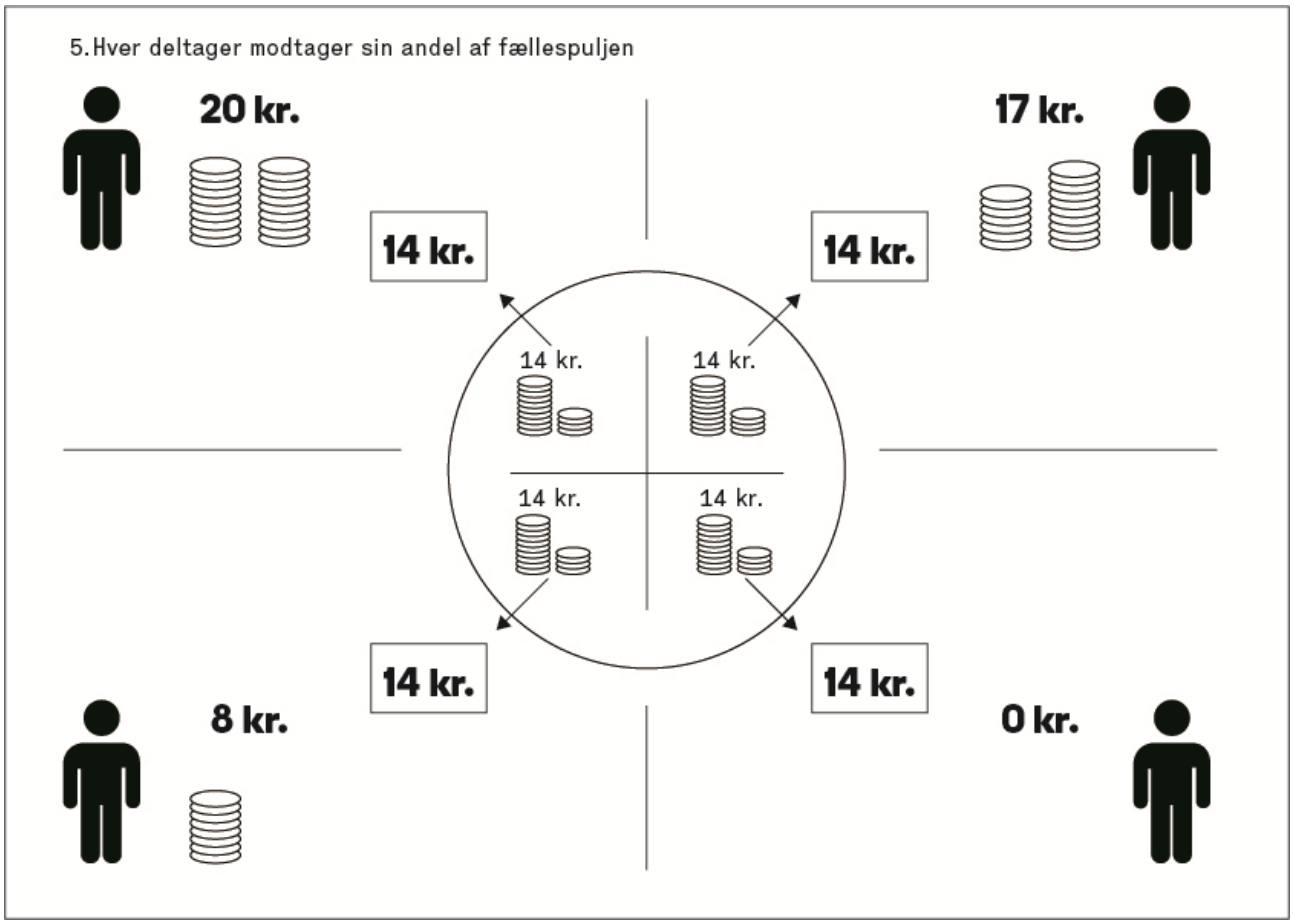
Slideshow page 4



Translation:

The total amount in the common pot is split in 4 equal shares.

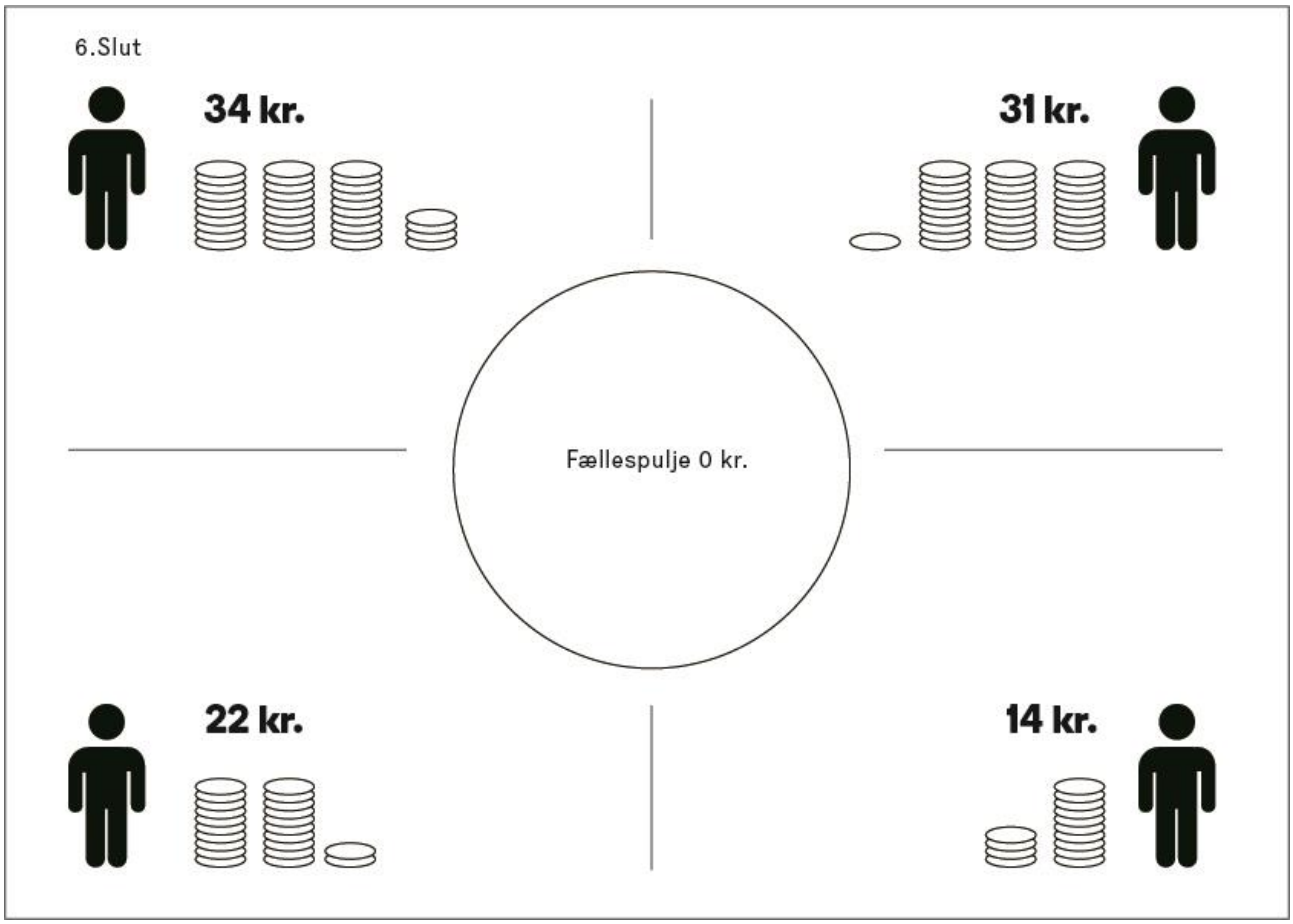
Slideshow page 5



Translation:

Every participant get her/his share of the common pot.

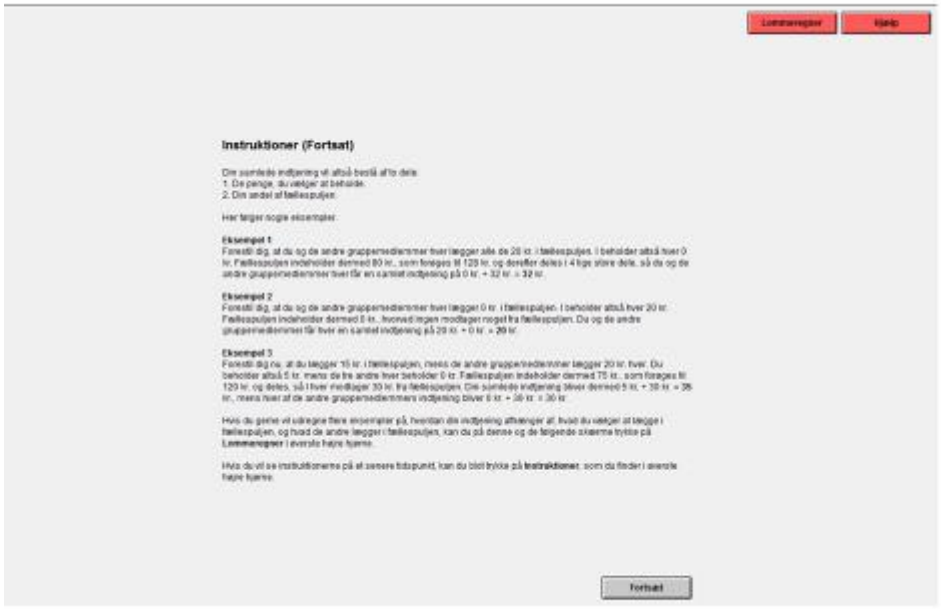
Slideshow page 6



Translation:

Done

General instructions - continued



Translation:

Instructions (continued)

Your total earning is made up by two parts:

1. The money you choose to keep
2. Your share of the common pot.

Here are some examples:

Example 1:
Imagine that you and the other group members each put 20 DKK in the common pot. That is, you each keep 0 DKK. The common pot therefore contains 80 DKK, which is increased to 128 DKK and after that divided by 4, so you and the other group members each get a total earning of 0 DKK + 32 DKK = 32 DKK.

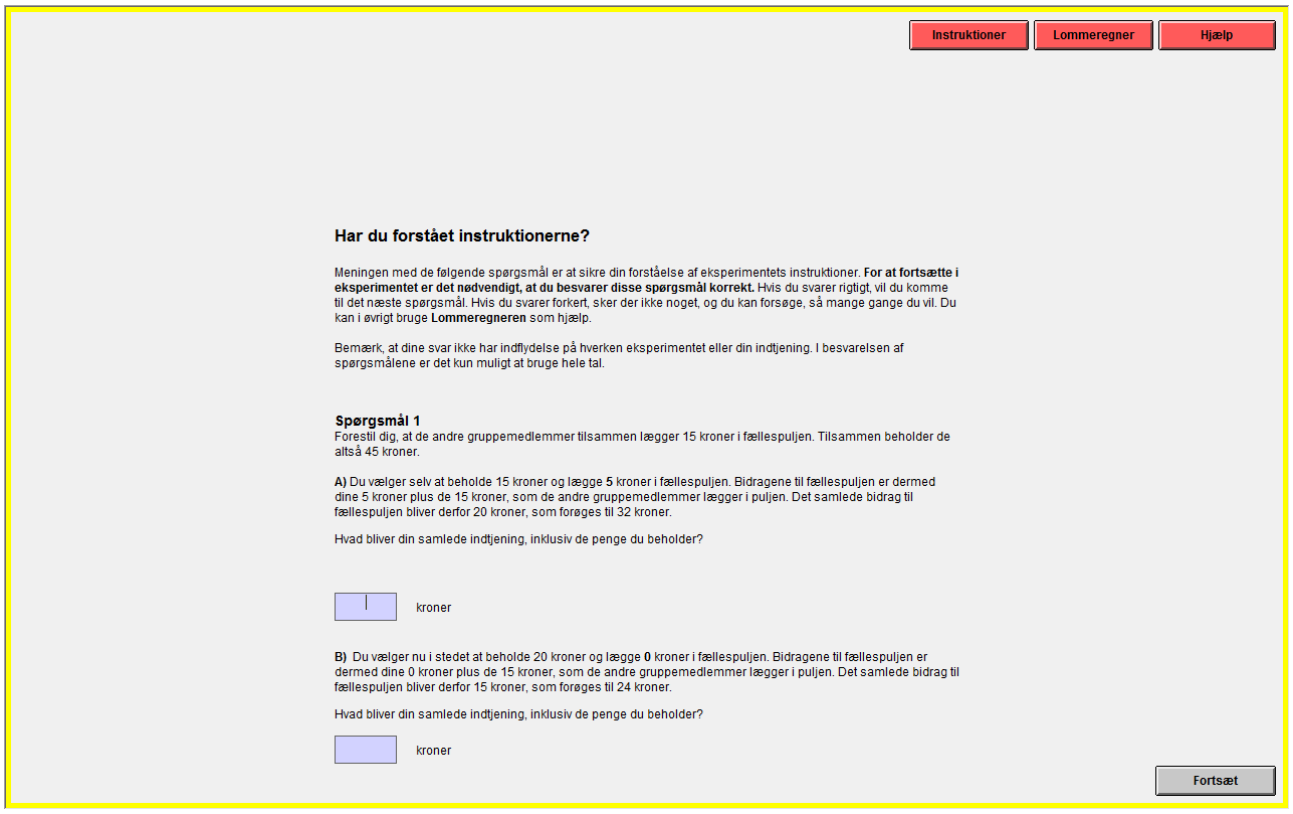
Example 2:
Imagine that you and the other group members each put 0 DKK in the common pot. That is you each keep 20 DKK. The common pot thus contains 0 DKK, and nobody gets anything from the common pot. You and the other group members each get a total earning of 20 DKK + 0 DKK = 20 DKK.

Example 3:
Imagine that you put 15 DKK in the common pot, while the other group members puts 20 DKK each. That is you keep 5 DKK, while the others keep 0 DKK. The common pot contains 75 DKK which increases to 120 DKK and is shared and each receive 30 DKK from the common pot. Your total earnings is therefore 5 DKK + 30 DKK= 35 DKK, while the other group members total earning is 0 DKK + 30 DKK = 30 DKK.

If you would like to calculate more examples of how you earnings depend on what you and the other group member choose to put in the common pot, you can just click on “Calculator” in the upper right corner of this, and the subsequent, screens.

If you want to see the instructions at a later point, just click on “Instructions”, which you will find in the upper right corner of the screen.

Control question 1



Translation

Did you understand the instructions?

The purpose of the following questions is to ensure your understanding of the experimental instruction. To continue in the experiment it is require that you answer these questions correct. If you answer correct, you will continue to the next question. If you answer incorrect nothing happens, and you can try as many times as you want. You can by the way use the calculator as a help.

Notice, your answers will not impact neither the experiment nor your earnings. When answering the questions it is only possible to use integers.

Question 1: Imagine that the other group members in total put 15 DKK in the common pot. In total they keep 45 DKK.

A) You choose to keep 15 DKK and put 5 DKK in the common pot. The contributions to the common pot is therefore your 5 DKK plus the 15 DKK which the other group members put in the pot. The total contribution to the common pot is therefore 20 DKK, which is increased to 32 DKK.
What is your total earning, including the money you keep?
B) Now, you chooses instead to keep the 20 DKK and put 0 DKK in the common pot. The contributions to the common pot is therefore your 0 DKK plus the 15 DKK from the other group members. The total contribution to the pot is therefore 15 DKK, which increases to 24 DKK.
What is your total earning, including the money you keep?

Control question 2

Translation:

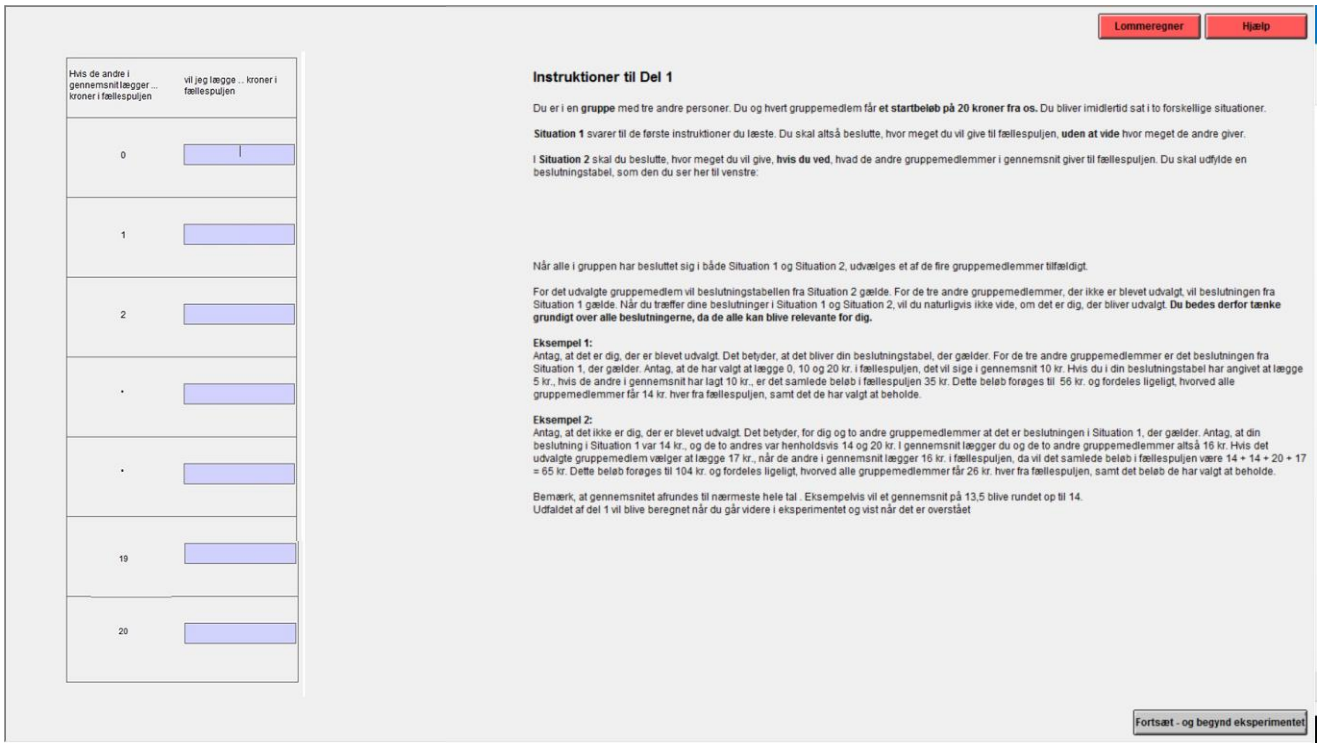
Did you understand the instructions?

Question 2:

Imagine that you keep 10 DKK and put 10 DKK in the common pot.

A) The other group members in total put 30 DKK in the common pot, and thus in total they keep 30 DKK. The total amount in the common pot is therefore 40 DKK, which increases to 64 DKK. What is your total earning, including the money you keep?
B) The other group members in total put 0 DKK in the common pot, and thus in total they keep 60 DKK. The total amount in the common pot is therefore 10 DKK, which increases to 16 DKK. What is your total earning, including the money you keep?

Instructions for part 1



Translation:

Instructions for Part 1.

You are in a group with three other people. You and each other group member gets a start amount of 20 DKK from us. You will however be set in two different situations.

Situation 1 is similar to the first instructions you read. That is, your task is to decide how much you want to give for the common pot, without knowing how much the others give.

In situation 2 you have to decide how much you want to give when you know what the other group members on average gives to the common pot. You have to fill out a decision table similar to the one you see to the left.

When all group members have decided in both situation 1 and 2, one group member is randomly picked.

For the chosen group member the decision table in situation 2 counts as her/his decision. For the three other group members, who were not picked, the decision in situation 1 counts. When you make your decisions in situation 1 and 2, you do not know if you will be picked. Therefore, think carefully about all decisions, as they can be relevant for you.

Example 1:

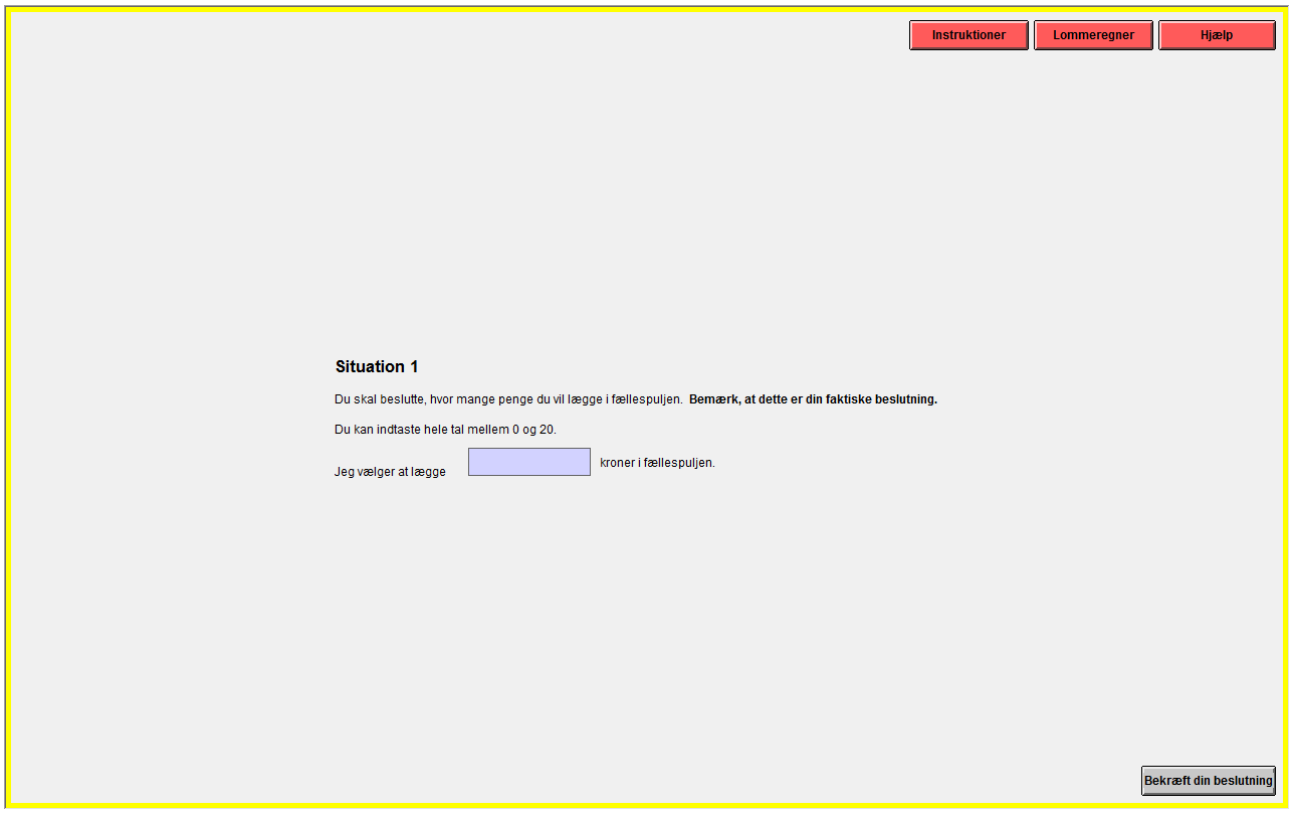
Imagine you are picked. It means that your decision table counts as your decision. For the three other group members it is the decision in situation 1 which counts. Assume they decided to put 0, 10, 20 DKK in the common pot, which is 10 DKK on average. If you in your decision table have stated that you want to put 5 DKK if the others on average give 10 DKK, then the total amount in the common pot is 35 DKK. This amount increases to 56 DKK, and is shared equally, and thereby each group member get 14 DKK from the common pot plus what they decided to keep.

Example 2:

Imagine you were not picked. It means that for you and two other group members it is the decision in situation 1 which counts. Assume your decision in situation 1 was 14 DKK, and the two others' decisions were respectively 14 and 20 DKK. That means, on average you and the two other group put 16 DKK in the common pot. If the picked group members want to give 17 DKK when other on average give 16 DKK, then the total amount in the common pot is: 14+14+20+17= 65 DKK. This amount increases to 104 DKK and is shared equally, resulting in each group member getting 26 DKK each from common pot, plus what they decided to keep.

Notice, that the average is rounded to closest integer. For example, an average of 13.5 will be rounded to 14. The result of part 1 will be calculated and shown when part 2 of the experiment is completed.

Unconditional decision in part 1



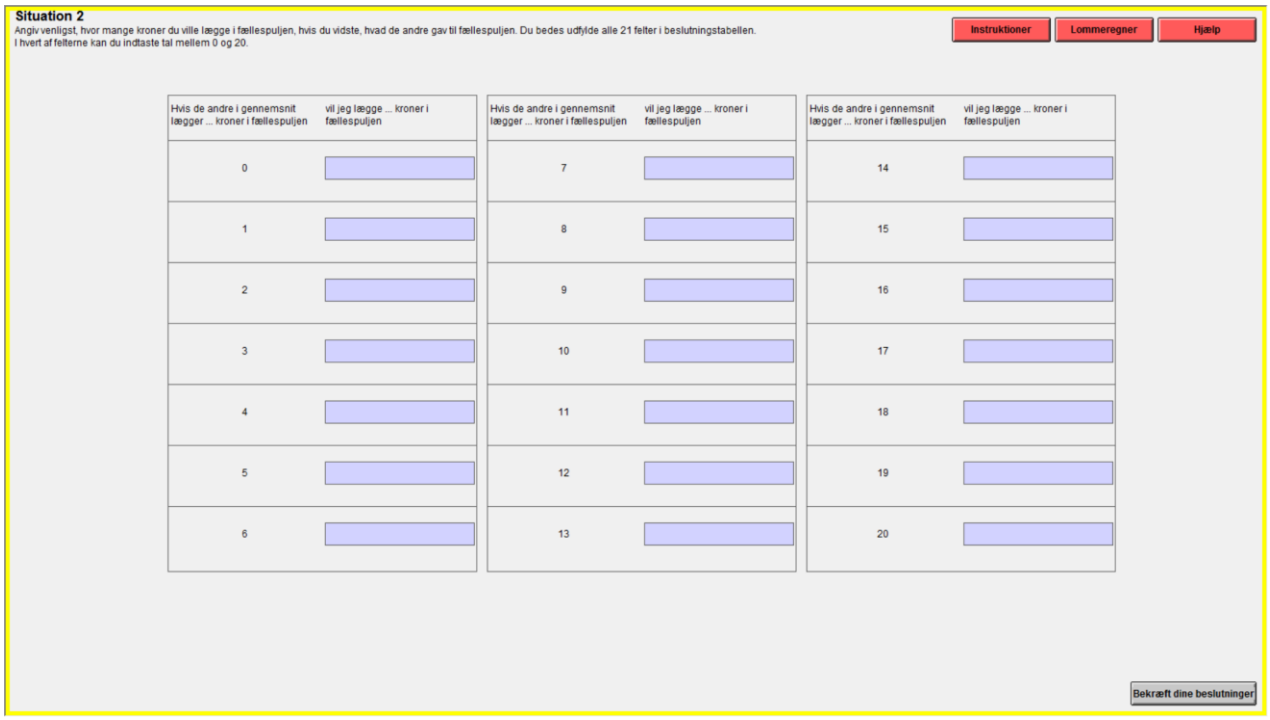
Translation:

Situation 1

You have to decide how much money you put in the common pot. Notice, this is your actual decision. You can type in integers between 0 and 20.

I decide to put [box] DKK in the common pot.

Conditional decision in part 1



Translation:

Situation 2

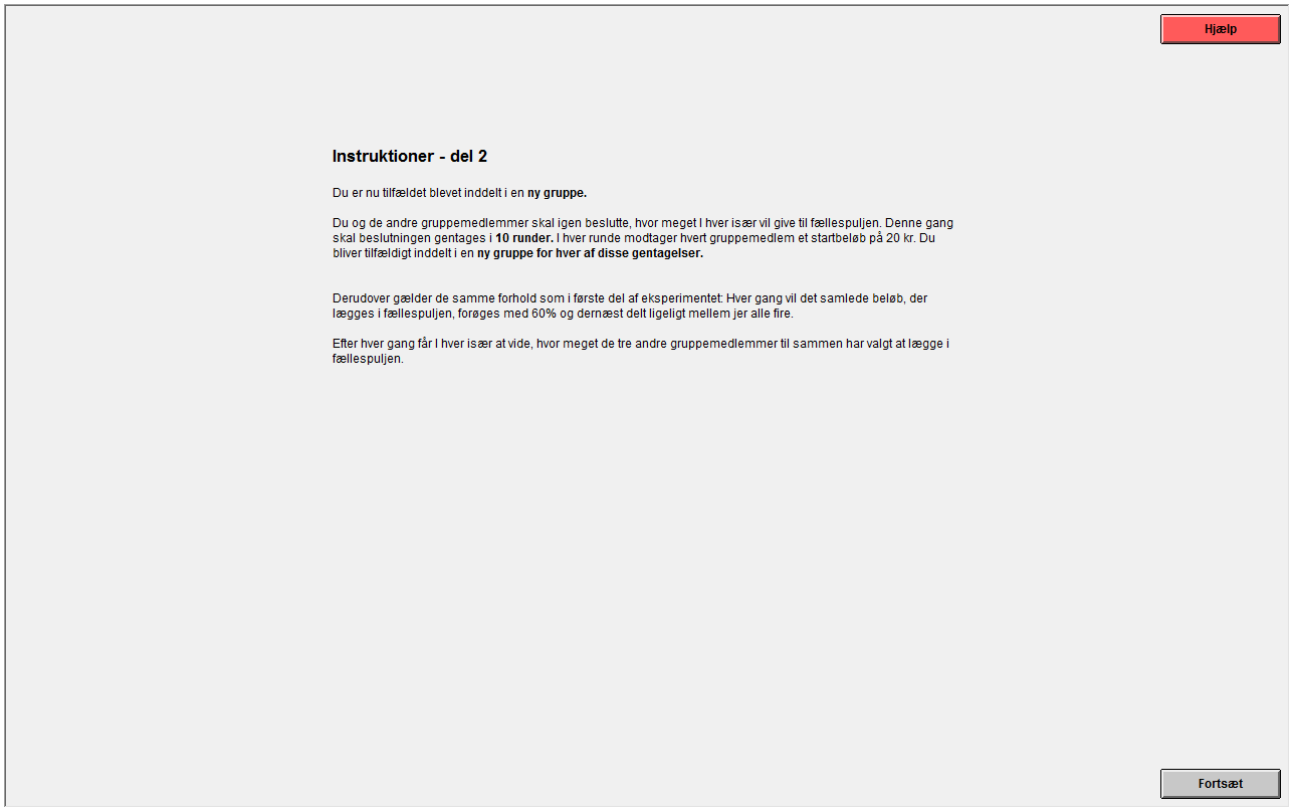
Please state how many DKK you will put in the common pot if you know what the others give. Please type in an answer in all 21 fields in the decision table. In each field, you can type a number between 0 and 20.

[Table headings]

[Column 1] If the others on average put… DKK in the common pot.

[Column 2] I will put … DKK in the common pot.

Instructions part 2 (Baseline & PrefSplit only)



Translation:

Instructions – Part 2:

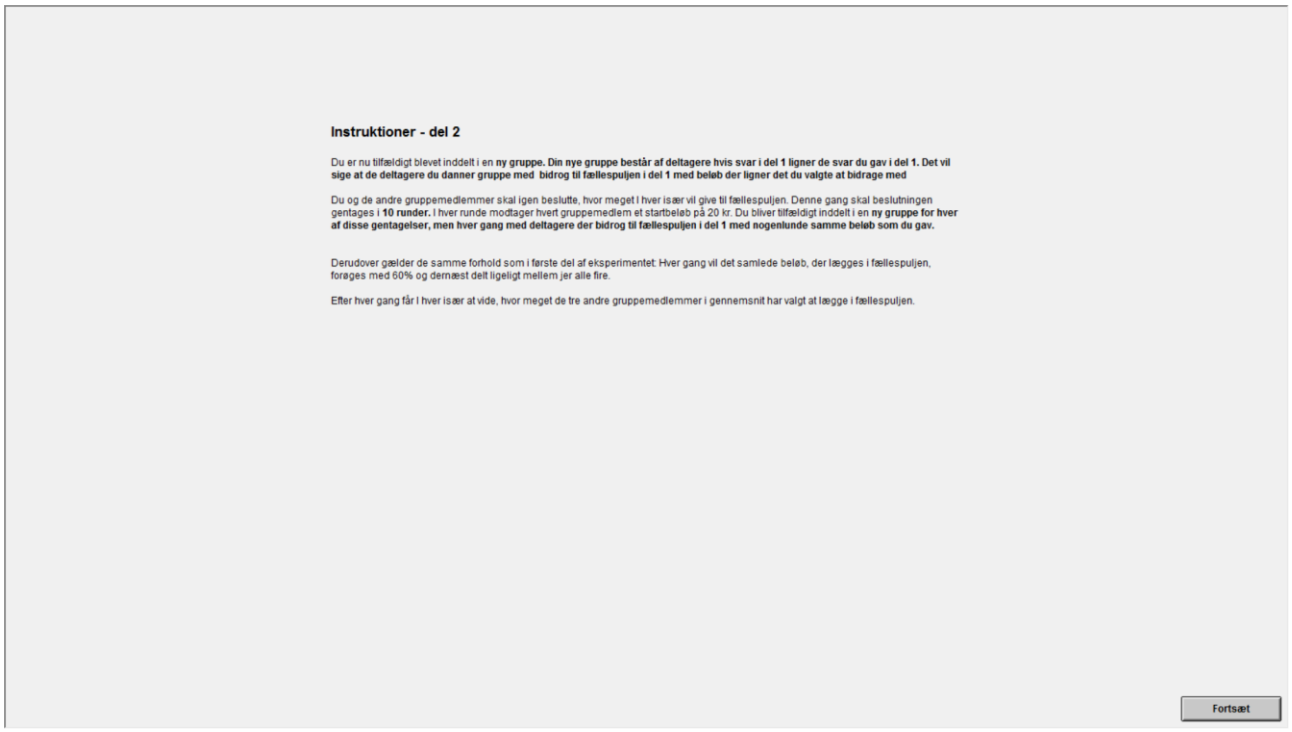
You have now randomly been assigned to a new group.

You and the other group members again have to decide how much each of you want to give to the common pot. This time, the decision is repeated for 10 rounds. In each round, each group member receives a staring amount of 20 DKK. You will randomly be assigned to a new group for each of the repetitions.

Besides this the conditions are the same as in the first part of the experiment: Every time the total amount in the common pot will increase by 60% and will be shared equally between all four.

After each repetition you will each know how much the other three group members together chose to put in the common pot.

Instructions part 2 (PrefsplitInfo only)



Translation:

(The difference of the instruction compared to Baseline and SplitPref is marked in yellow).

Instructions – Part 2

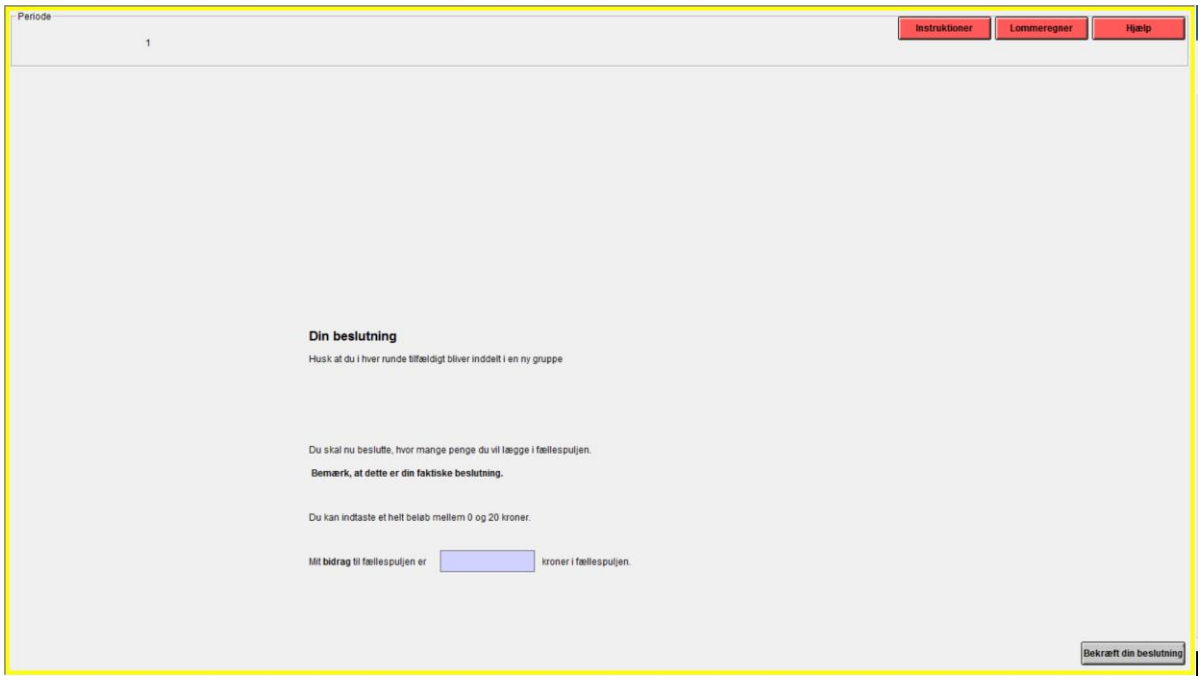
You have now randomly been assigned to a new group. Your new group consists of participants who answered similar to you in part 1. That means, you are in a group with participants who gave similar amounts to the common pot in part 1.

You and the other group members again have to decide how much each of you want to give to the common pot. This time, the decision is repeated for 10 rounds. In each round, each group member receives a staring amount of 20 DKK. You will randomly be assigned to a new group for each of the repetitions, but every time with participants who gave contributions to the common pot in part 1 similar to you.

Besides this the conditions are the same as in the first part of the experiment: Every time the total amount in the common pot will increase by 60% and will be shared equally between all four.

After each repetition you will each know how much the other three group members together chose to put in the common pot.

Decision part 2



Translation:

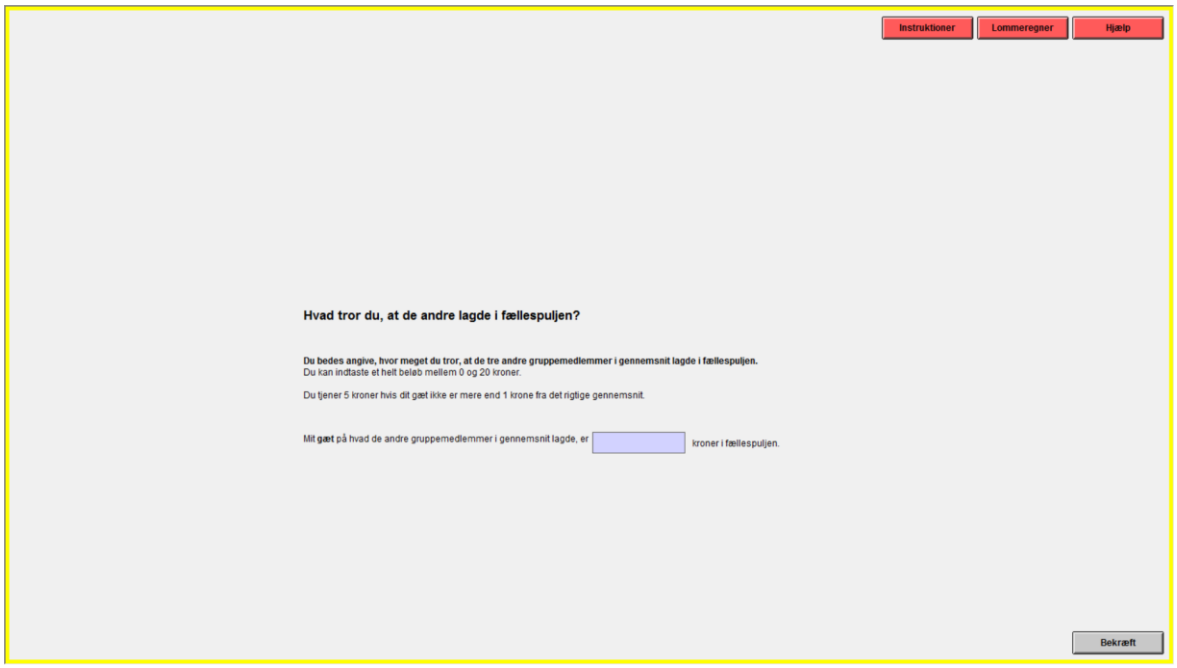
Your decision:

Remember that you in each round will be assigned to a new group.

Now you have to decide how much money you want to put in the common pot. Notice, that this is your actual decision. You can type in an amount between 0 and 20 DKK.

My contribution to the common pot is: [box] DKK

Belief measure in part 2



Translation:

What do you think others were putting in the common pot?

Please, state how much you think the three other group members on average contributed to the common pot. You can type in an integer amount between 0 and 20.

You earn 5 DKK if your guess is not more than 1 DKK from the actual average.

My guess of what the other group members on average were contributing, is [Box] DKK to the common pot.

Appendix C: Regression of cooperation and belief over time

Table C1:

Dependent variable: Contribution	
Period	-0.226**
	(0.074)
Student X Period	-0.405***
	(0.107)
Student	-0.505***
	(0.135)
Constant	12.982***
	(0.545)
R-squared	0.159
N	1700

* p<0.05, ** p<0.01, *** p<0.001

Table C2:

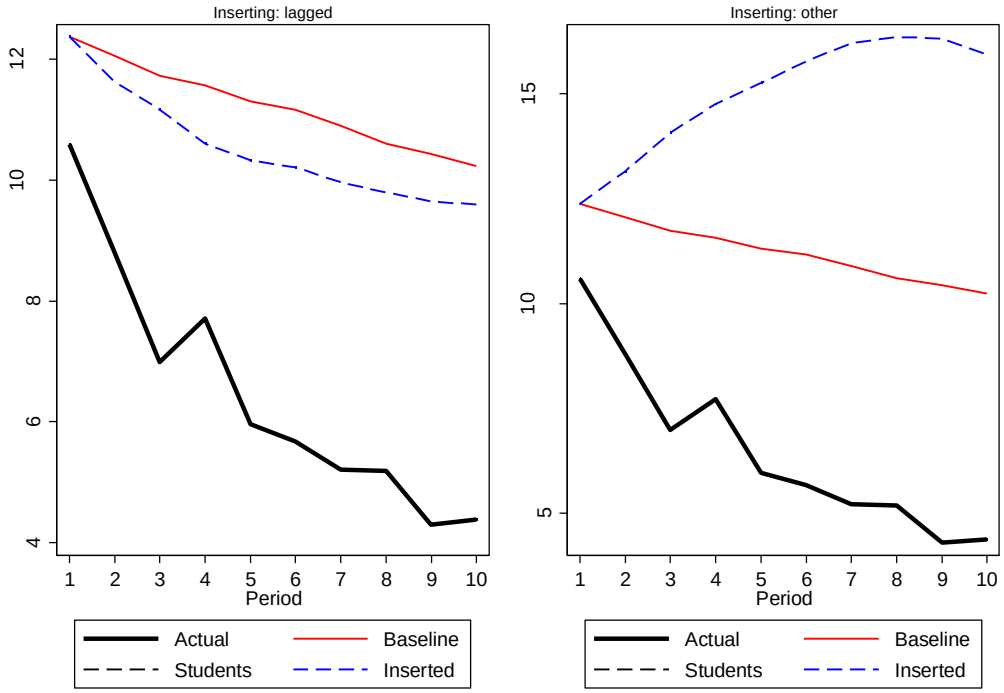
Dependent variable: Belief	
Period	-0.107*
	(0.046)
Student_x_Period	-0.518***
	(0.067)
Student	-0.291***
	(0.088)
Constant	12.262***
	(0.366)
R-squared	0.309
N	1700

* p<0.05, ** p<0.01, *** p<0.001

Appendix D: Simulation of non-students' contribution

In the graphs below, the simulation of the non-students' behavior is shown for all possible variations. In particular, I insert coefficients from the students' regressions one at a time. The resulting contributions can be observed. The following coefficient variations have been tested, each of which correspond to a separate graph:

- Lagged: Insert the non-students' lagged belief coefficient in the belief prediction
- Other: Insert the non-students' coefficient for the contribution of others in the belief prediction
- Belief: Insert the non-students' coefficient for the belief in the contribution prediction
- Preferences: Insert the non-students' coefficient for the preference in the contribution prediction
- Period: Insert the non-students' coefficient for period in the contribution prediction



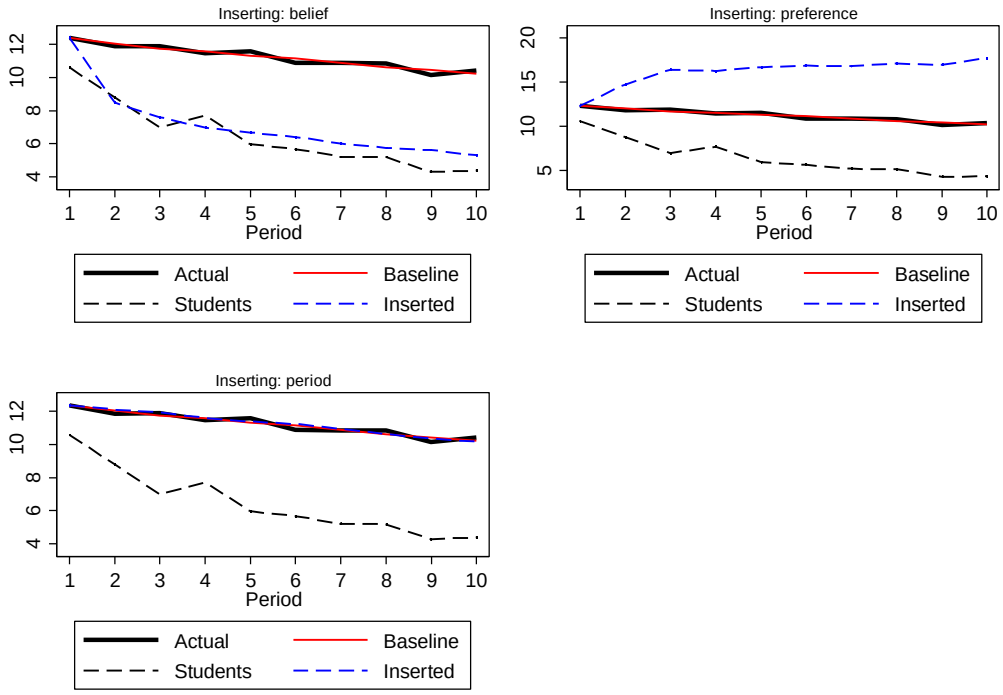


Table C1 – Non-students' simulation compared to students' behavior

	Belief regression		Contribution regression		
	Lagged belief	Others' contribution	Belief	Preference	Period
Period	-0.631***	-0.631***	-0.631***	-0.631***	-0.631***
	(0.036)	(0.037)	(0.037)	(0.048)	(0.036)
Simulation	2.171**	2.661***	0.367	3.934***	2.675***
	(0.666)	(0.690)	(0.665)	(0.813)	(0.671)
Period x Simulation	0.342***	1.076***	0.049	1.055***	0.385***
	(0.050)	(0.052)	(0.052)	(0.067)	(0.050)
Constant	9.952***	9.952***	9.952***	9.952***	9.952***
	(0.482)	(0.499)	(0.481)	(0.588)	(0.485)
R-squared	0.205	0.450	0.117	0.419	0.243
N	1700	1689	1700	1679	1700

* p<0.05, ** p<0.01, *** p<0.001

Appendix D: Selection analysis

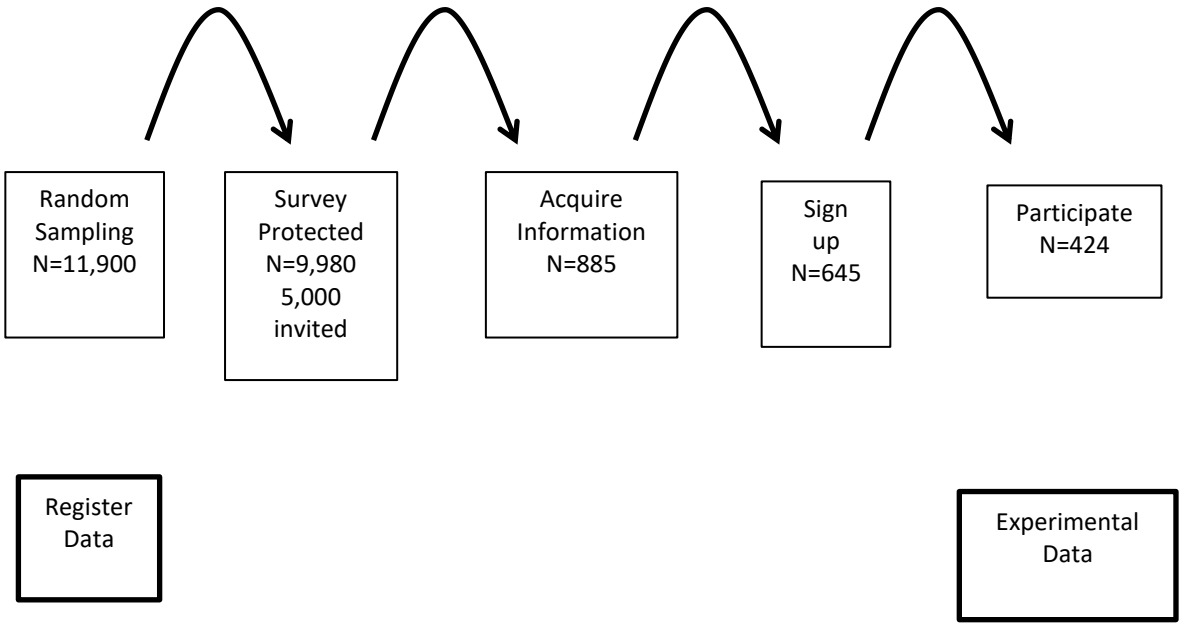
The purpose of the main experiment is to investigate cooperation among ordinary people. However, when a representative sample of subjects was invited into the lab, a much more complex selection process emerges than the one which occurs within the usual student sample.

Recently, the issue of selection bias of experimental participants has been addressed. Slonim, Wang, Garbarino, & Merrett (2013) studied selection among students, and compared the characteristics of those who were invited to participate in an experiment with those who actually did participate. They find that those who have a lower income and more leisure time are more likely to participate. In contrast, Falk, Meier, & Zehnder (2013) find that students who are more likely to make a generous donation are not more likely to sign up as participants, which suggests that the selection bias is not a huge concern. Cleave, Nikiforakis, & Slonim (2013) find similar results. Interestingly, Falk et al., (2013) also compare trust measured among student subjects and non-student subjects, and find that students are less trusting (there is no difference in initial trust, but there is in subsequent repayment), and conclude that students' social behavior can be seen as a lower estimate of the actual level of trust. The present analysis seeks to determine whether the selection process among the representative sample accounts for the behavioral patterns observed in the experiment.

In the present experiment, I break down the selection process into several steps as illustrated in Figure B1. Each of the steps shows sub-processes which may potentially affect selection. Concretely, I identify the following selection process: A random sample is initially selected by Statistics Denmark, which is followed by these 4 selection steps:

1) Protected. A sub-sample of the initial sample are survey protected and cannot be invited to participate in research studies as stipulated in Danish legislation. From the resulting non-research protected sample, 5000 people are randomly sent an invitation letter.
2) Acquire info. Of those who received an invitation letter, only some login to the recruitment website and read about the experiment.
3) Sign up. Of those who logged in to the website, a sub-sample decide to sign-up to participate in the experiment.
4) Participate. Of those who signed up, a sub-sample do actually participate in the experiment.

Figure D1 - Selection process for the experiment



To determine whether the selection bias that I identify is relevant, I run a probit regression for each step that addresses the likelihood of people being in the sample (1: in the sample, 0: not) on key choice determinants based on the register data obtained for all individuals who were invited to the experiment. The results of the four regressions are presented in table D1 below.

Protected. In Denmark, you can decide not to be contacted by survey and research institutions. In 2012, 16% of the population had signed up for such protection. It can be seen that new signups increased dramatically between 2000 and 2007. The reason seems to be that, during this period, the Danish authorities had included an easy solution to survey protection in the official form citizens had to sign when they moved house. Thus, people who moved in the period 2000-2007 were automatically exposed to the protection signup, whereas before 2000 and after 2007, individuals had to acquire a separate form to get the protection. From the second column of Table D1, it is apparent that people who moved in the period 2000-2007 are indeed much more likely to be protected, which introduces a selection bias.

Acquire info. If an individual is invited, what are the determinants for whether they subsequently login to the recruitment website to read about the experiment? When analyzing this decision from a cost-benefit point of view, I included the distance from the household to the lab (the address of the lab was mentioned in the invitation letter), and also whether people have children. As presented in column 3 of Table D1, the greater the distance to the lab, the less likely the people are to login. And similarly, they are significantly less likely to login if they have small children, 0-5 years old.

Table D1 – Selection biases	Protected	Acquire info	Sign up	Participate
Moved from 2000-2007	1.221***			
	(33.48)			
ln(Distance to Lab)		-0.112***	-0.136***	
		(-4.79)	(-5.38)	
Children aged 0 to 5		-0.0989*	-0.116*	
		(-2.07)	(-2.25)	
Children aged 6 to 10		-0.0866	-0.107*	
		(-1.72)	(-2.01)	
Children aged 11 to 16		-0.0286	-0.0317	
		(-0.67)	(-0.69)	
Invitation wave 2			-0.102*	-0.689***
			(-2.20)	(-6.74)
ln(Gross Income)			-0.0420*	
			(-2.01)	
Labor Market participation			0.0906	
			(1.60)	
Constant	-1.827***	0.123	0.682*	0.541***
	(-56.47)	(0.59)	(2.10)	(8.53)
N	11900	4865	4788	682

Probit estimates, robust standard errors
t statistics in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Sign-up. The decision to sign-up for an experiment session is also significantly negatively associated with distance and having children (now from 0-10). In this regression, I also included whether people had received the invitation letter in the second wave of invitations, as opposed to the first. Being in the second wave significantly reduced the likelihood of signing up. This finding is quite intuitive since many places had already been taken when participants were invited in the second wave to login in and consider participating. Income was also included as an explanation for signing-up, and I find that higher gross income reduces the likelihood of participation, probably because a higher income implies higher opportunity costs. Interestingly, having a regular job as opposed to being on social benefits does not influence the decision.

Participate. Finally, there is a bias for actual participation based on invitation waves, in that the likelihood of participation is significantly reduced if individuals were invited in the second wave.

In sum, significant selection bias seems to occur at all four steps, which may potentially influence the resulting behavioral measure. To determine whether the selection biases affects the public good results, I now run regressions on the contributions in the first period of the repeated public good game. I have chosen the first period as this decision is not biased by the potential dynamics occurring during the repeated game. The results are presented in Table D2.

Table D2 – effect of selection biases on public good outcome	**Public good contribution (first period)**				
Response time on website	-0.0885	-0.0806	-0.0745	-0.0784	-0.0675
	(-1.73)	(-1.56)	(-1.41)	(-1.52)	(-1.28)
Gender (1: female, 0: male)	0.635	0.599	0.665	0.608	0.687
	(1.06)	(1.00)	(1.10)	(1.02)	(1.13)
Mills (Survey protected)	-0.791				-0.788
	(-1.36)				(-1.33)
Mills (Acquire info)		3.615			4.079
		(1.27)			(0.54)
Mills (Sign up)			1.659		-0.576
			(0.78)		(-0.10)
Mills (Participate)				-1.232	-1.266
				(-0.77)	(-0.62)
Constant	14.53***	7.914	10.52**	13.86***	10.18*
	(13.19)	(1.89)	(3.12)	(12.95)	(2.03)
N	415	414	407	415	407

OLS estimates, robust standard errors

t statistics in parentheses

* p<0.05, ** p<0.01, *** p<0.001

It is observed in Table B2 that none of the Mills ratios are significant in explaining the public good contributions, which means that the observed selection bias that I do observe it not impacting the result of the experiment.