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What Drives Conditional Cooperation in Public Goods Games?*

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Abstract

Extensive experimental research on public goods games documents that many subjects are "conditional cooperators" in that they positively correlate their contributions with (their belief about) contributions of other subjects in their group. The goal of our study is to shed light on what preference and decision-making patterns drive this observed regularity. We consider four potential explanations, including reciprocity, conformity, inequality aversion, and residual factors such as confusing and anchoring, and aim to disentangle their effects. We find that, of the average conditionally cooperative behavior in the sample, about two thirds is accounted for by residual factors, a quarter by inequality aversion and a tenth by conformity, while reciprocity plays virtually no role. These findings carry important messages about how to interpret conditional cooperation as observed in the lab and ways it can be exploited for fundraising purposes.

Keywords: conditional cooperation, public goods game, reciprocity, conformity, inequality aversion, anchoring, fundraising

JEL classification: H41, C91, D64

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1 Introduction

Casual observation as well as an extensive experimental literature Ledyard (1995) document that people voluntarily contribute to public goods. This observation is squarely at odds with the traditional model of self-regarding preferences. Under this model, each individual has a strictly dominant strategy of free-riding (i.e., contributing zero). Most existing explanations of this empirical regularity rely on the existence of social preferences.[1] Although positive voluntary contributions can be explained by maximization of social welfare (Laffont, 1975) or altruistic/warm-glow preferences (Becker, 1974, Andreoni, 1989, 1990), the predictions of these theories within the linear public goods game, a workhorse of research in this area, do not square well with empirical evidence. In particular, while these theories predict that an individual contributes the same amount no matter how much others contribute, Fischbacher, Gächter and Fehr (2001) (henceforth FGF) document that a sizable group of subjects contribute more if others on average contribute more as well. They call this empirical pattern "conditional cooperation" (henceforth CC). The authors find that about one half of their subjects are conditional cooperators (henceforth CCs), one third are free-riders (contributing zero regardless of the average contribution of the other group members), and the rest do not fit either pattern. These findings have since been replicated by numerous studies (see Thöni and Volk, 2018, for a list). Moreover, multiple studies in the lab and in the field (see Gächter (2007)and Chaudhuri (2011) for surveys) document a positive correlation between contributions and historical contributions, or beliefs about current contributions of others, suggesting the presence of CC .

CC is a very interesting observation from the point of view of designing fundraising campaigns for public goods or other social causes such as charities. It suggests that a fundraiser can increase contributions by relying on would-be contributors' CC along with convincing them (credibly, preferably) that others are contributing high amounts. However, we argue that without fully understanding social preference and other decision-making drivers of CC, such suggestion might be premature.

CC could be driven by several preference and decision-making patterns, such as reciprocity (to others whose contributions benefit oneself), conformity (with others' contributions regardless of payoff consequences), disadvantageous inequality aversion (to others falling behind materially relative to oneself because of their contributions) and other residual factors.

[1] A leading alternative explanation is experimental subject confusion; see Andreoni (1995), Houser and Kurzban (2002).

Reciprocity is a kind (unkind) response to an action by others that is perceived to be driven by kind (unkind) intentions towards oneself (Sugden, 1984, Rabin, 1993, Dufwenberg and Kirchsteiger, 2004, Falk and Fischbacher, 2006). *Conformity* is an act of following an observed behavior of others. It can arise due to adherence to a (perceived) social norm (Axelrod, 1986, Bernheim, 1994, Fehr and Fischbacher, 2004, a.k.a. "normative conformity") or due to social learning about an optimal decision (Bikhchandani et al., 1998, a.k.a. "informational conformity"). *Inequality aversion* (IA) is a willingness to redistribute material payoff among involved individuals in order to reduce material payoff inequality between oneself and the others. Such redistribution is desirable even if it involves a modest degree of efficiency loss. Importantly, unlike in the case of reciprocity, it is irrelevant whether the inequality originates from actions or intentions of the others or not (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000). *Residual factors* include any other alternative explanation for CC.[2]

When it comes to residual factors, we speculate that the most important ones include anchoring and subject confusion. *Anchoring* is an act of allowing one's decisions be influenced by payoff- and belief-irrelevant numerical cues (Tversky and Kahneman, 1974). *Subject confusion* (Andreoni, 1995, Houser and Kurzban, 2002) can be thought of as an imperfect "game form recognition" (Chou et al., 2009) in that subjects fail to properly understand how players' strategies map to their payoff vectors. The possibility that CC is driven by confusion has been vividly illustrated by Ferraro and Vossler (2010) and Burton-Chellew et al. (2016). These two studies find that when subjects play the public good game against computers using the FGF design, with nobody else (apart from the experimenter) benefiting from their contributions, the classification into conditional contribution types results in a distribution remarkably similar to that of FGF and its replications. Specifically, the share of CCs is 50%. All this happens despite subjects answering questions designed to ensure that they understand the instructions. Burton-Chellew et al. (2016) also document that CCs, as opposed to free-riders, are more likely to misunderstand the game.

The relative strength of the four potential drivers of CC as observed in the lab has important implications for how to best exploit CC to optimize fundraising campaigns. Specifically, if CC is driven by reciprocity or (disadvantageous) IA, exploiting it indeed requires that a campaign designer has high unconditional contributions from a subgroup of early contributors in order to generate high contributions from others.[3] If CC is driven by conformity, then exploiting it can also rely on advertising

[2]An additional alternative explanation for conditional cooperation that is similar to reciprocity is betrayal aversion (Bohnet et al., 2008). Although betrayal aversion could be distinguished from reciprocity in a sequential contribution game, the two cannot be distinguished in the simultaneous contribution game considered in this paper.

[3]One channel through which such early contributions, or "seed money", can affect later would-be contributors is that

high documented contributions from other (historical) peer groups that are not necessarily involved in the current campaign. If CC is driven by the residual factors, however, it is not clear what the message for fundraising is. Anchoring would imply suggesting a higher rather than a lower level of contributions to would-be contributors. On the other hand, confusion is an artefact of experimental design as implemented in the lab, without clear implications for field design.

The aim of our study is to disentangle the four potential drivers of CC. We utilize a modified version of the FGF design (detailed in Section 3). Each subject, after contributing unconditionally (treatment 1), is also faced with four conditional contribution treatments, making a total of five treatments. In treatments 2 through 4, subjects condition on the average contribution of three other members of their contribution group. What differs across these three treatments is how the contributions of the other three group members are determined. In treatment 2, the other group members' contributions are equal to their unconditional contributions from treatment 1, as in the original design of FGF. All four explanations play a potential role here. In treatment 3, the other group members' contributions are equal to unconditional contributions of three randomly chosen group *non*-members from treatment 1. This treatment eliminates reciprocity as an explanation for CC. In treatment 4, the other group members' contributions are randomly generated by computer. Comapred with treatment 3, this treatment also eliminates conformity as an explanation. Finally, in treatment 5, subjects condition on the average of three randomly drawn numbers. The other group members' contributions are independently randomly generated by computer. Campared with treatment 4, this treatment also eliminates inequality aversion and leaves only anchoring as a potential explanation. We identify the impact of reciprocity by comparing conditional contributions in treatments 2 and 3; that of conformity by comparing treatments 3 and 4; and that of inequality aversion by comparing treatments 4 and 5. Treatment 5 identifies the impact of the residual factors. We do not attempt to separate anchoring from confusion, as this is inherently difficult. Whenever anchoring is present, some type of confusion is very likely to be present as well.[4] Whenever confusion is present, there are some *ex post* patterns of conditional contributions that would allow us to strongly argue that anchoring is not present.[5] However, it is hard to think of a reliable way to rule out anchoring by design *ex ante*.

it signals that the campaign is worthy. By design, this channel in not present in documented CC behavior in the lab. We therefore omit it from our analysis.

[4]The only case to the contrary we can think of is if a subject is indifferent across several levels of his contribution and uses anchoring on the computer-generated random conditioning variable to implement a mixed strategy. We do not consider such a scenario to be likely.

[5]For example, when playing against computers as in Ferraro and Vossler (2010) and Burton-Chellew et al. (2016), a non-zero contribution that is independent of how much the three computers contribute on average suggests confusion, but not anchoring on the conditioning variable.

We find strong CC behavior even in treatment 5 in which only the residual factors play a role. Adding inequality aversion in treatment 4 further increases the extent of CC behavior. Adding conformity in treatment 3 leads to a small further increase in CC behavior with borderline statistical significance. Finally, adding reciprocity in treatment 2 has a minimal impact on CC behavior. Based on the estimated slopes of the average contribution schedules by treatment, we find that the residual factors account for about two thirds, inequality aversion for one quarter and conformity for one tenth of the CC behavior. Reciprocity is estimated to play virtually no role.

The paper proceeds as follows. Section 2 reviews related literature. Section 3 outlines the experimental design. Section 4 reviews the empirical methodology utilized. Section 5 presents our results. Section 6 discusses the results and links them to the previous literature. Finally, Section 7 concludes.

2 Related Literature

2.1 Reciprocity, Conformity, Inequality Aversion and Anchoring

This study is most closely related to the work of Bardsley and Sausgruber (2005) and Cappelletti, Güth and Ploner (2011). Bardsley and Sausgruber (2005) attempt to distinguish the roles of reciprocity and conformity in driving CC. They analyze conditional contribution behavior of subjects who see possible vectors x_{-i} of contributions of other members of their own group and possible vectors y of members of another group. They identify conformity by reaction to changes in y, holding x_{-i} constant. They identify the combined CC effect of reciprocity and conformity by reaction to changes in x_{-i}, holding y constant. Assuming additive separability of the two drivers, they conclude that, of the combined effect, 2/3 is accounted for by reciprocity and 1/3 is accounted for by conformity. This identification strategy requires that the strength of conformity with x_{-i} and that with y is the same. However, this is unlikely to be the case given the design utilized. The issue is that *all* the members of the 'own' group, even those who comprise x_{-i}, see y before deciding on their contributions. As a result, especially in cases when the level of contributions in x_{-i} and y is very different, it is reasonable to expect that conformity with x_{-i} is stronger than that with y because the decision-maker is likely to infer that if the other group members chose to deviate from the level of contributions in y, there is probably a good reason to do so (informational conformity). Indeed, this reasoning appears to be confirmed by the data (see the comparison of average contributions in LH and HL in Figure 1). As a result, the estimate of 1/3 of the total CC effect is likely to be an underestimate of the true effect of conformity in the combined effect of conformity and reciprocity. Also, the paper does not attempt to experimentally isolate the roles of inequality aversion and residual factors.

Cappelletti et al. (2011) attempt to disentangle the roles of reciprocity, inequality aversion and anchoring, but not that of conformity. They use a design to that of FGF in terms of eliciting conditional contributions, but differing from it by making payoffs non-linear in contributions (with a strictly increasing marginal cost of contributions) and using repeated play based on a stranger-matching protocol. They find (see their regression-based analysis summarized in Result 3 and Table 4) that CC behavior is predominantly driven by anchoring and inequality aversion (by about the same amount), with reciprocity playing a small and statistically marginal role.[6] As admitted by the authors themselves, however, their non-linear design is likely to be overly complex for subjects, as reflected in an atypically low incidence of CC relative to studies based on a linear public good game. This design also complicates the analysis of contribution data, as different sub-ranges of contributions need to be analyzed separately. Consequently, results are somewhat sensitive to which sub-range one looks at.

Our design attempts to overcome these shortcomings. First, we consider all four potential drivers of CC behavior in a single setting. Second, we build our design on the FGF design based on a linear public good game that is utilized in many existing replications (Thöni and Volk, 2018). This makes our study directly comparable to many other studies in the literature. Third, our conditioning variable is always the average of three *independent* human-made or computer-made unconditional contribution decisions or randomly drawn numbers. We hence avoid information-cascade-like problems in interpreting results of the various conditions.

Other authors have attempted to address similar questions using data from repeatedly-played public good games. Ashley et al. (2010) attempt to distinguish the roles of reciprocity and inequality aversion, but not those of conformity or other factors, using data from repeated public good experiments with fixed-group matching and *ex post* observability of individual contributions in the previous period within the 'own' group only (baseline treatment) and also across groups (alternative treatment). They conclude that the dynamics of contributions are more consistent with inequality aversion than with reciprocity. However, the fixed-group design with repeated interaction allows for alternative

[6]Reciprocity appears to play a somewhat more important role in their type of classification analysis, summarized by Result 1 and Tables 2 and 3. However, no statistical tests are provided with this analysis.

interpretations of the results based on dynamic strategizing and reputation-building.[7,8]

2.2 Confusion

As discussed in Section 1, subject confusion might play a significant role in explaining CC as observed in the lab. Burton-Chellew et al. (2016) list several conditions they think lead to subject confusion in the original FGF design: (1) using the verb "invest" to describe the act of contribution might invoke a sense of a risky endeavor, with returns dependent upon a complementary 'investment' by others.; (2) subjects might not be fully aware of the private cost of contributing and hence might not realize the social dilemma that they face; for example, of the four control questions aimed at ensuring understanding, only one (question 3) illustrates the trade-off inherent in the social dilemma; (3) since they are asked to contribute conditionally, subjects might think that the value of the conditioning variable is important and that their conditional contribution *should* vary with it even though they cannot see an obvious reason for such a correlation (an experimenter demand effect). We use these comments as a guideline for our experimental design. We develop an alternative set of instructions that uses the verb "contribute" instead of "invest" to describe the act of contribution. Instead of using control questions, which both Ferraro and Vossler (2010) and Burton-Chellew et al. (2016) find to be ineffective in preventing confusion, we aid understanding of the game by giving subjects an opportunity to simulate their and other group members' payoffs (see the next section). The simulator gives subjects a simple interface to perform a *ceteris paribus* analysis of how a marginal change in their or another subject's contribution affects the payoffs of all members of the group.[9]

The emerging confusion literature also has another message for our experimental design and underlying identification assumptions. Our design is based on variation in the definition of the conditioning variable. The existing literature gives no hint on how confusion correlates with such variation or whether this correlation, if any, affects the extent of CC. We believe that such correlation would be

[7]There is also work on whether reciprocity or inequality aversion drives punishment in public goods games. Dawes et al. (2007) and Johnson et al. (2009) find that a significant part of punishment in public good games is driven by inequality aversion rather than reciprocity. On the other hand, Falk et al. (2005) conclude that punishment by cooperators is predominantly driven by reciprocity rather than inequality aversion.

[8]There is also a related literature that addresses the same research question in the domain of a common pool resource game. This game features two differences relative to the public good game. First, participants extract from rather than contribute to the public good/resource. Second, the marginal cost of extraction typically increases with the extraction activity of others, resulting in a decreasing rather than a flat best response function based on self-regarding preferences. Velez et al. (2009) conduct a framed field experiment with fishermen in Colombia and find an upward-sloping best response. Based on this monotonicity, they conclude that observed behavior is best-explained by conformity.

[9]We come back to the experimenter demand effect in Section 6.

hard to identify given that computer players can no longer be used. We therefore make an identification assumption that the extent of confusion is independent of the variation in the definition of the conditioning variable that we use. Under this assumption, confusion introduces a significant additional source noise (but not bias) into a between-subject design that is avoided (differenced out) in a within-subject design. This reasoning leads us to rely on a within-subject design in our study.

Our study provides a link between the existing FGF-based literature on CC decomposition and the emerging literature indicating that CC is driven by confusion. Unlike the latter literature, which purely aims to document that confusion does drive CC, we integrate confusion into the decomposition exercise by lumping it together with anchoring into “residual factors." Importantly, we identify all four potential drivers, including the residual factors, from treatments that differ only in how the unconditional contributions are determined and what information is contained in the conditioning variable. In all other aspects, the various conditional contribution treatments are identical. In particular, there are always four payoff-collecting human players and this is common knowledge. As a result, treatment comparisons are not confounded by uncertainty about who, if anyone, collects the payoffs (Camerer, 2013).

3 Experimental Design

We build on the original design of FGF with some modifications. Subject play a linear public goods game in groups of four. Each subject *i* independently decides how many of her 10 tokens (as opposed to 20 in the original design of FGF) to allocate into her private account ($10 - g_i$) and how many to contribute to the public account, termed a “group project” (g_i). Each subject receives a payoff from the public good equal to 0.75 (instead of 0.4 in the original design of FGF) times the sum of all contributions to the public account. Hence the material payoff in tokens of subject *i* is given by $\pi_i = 10 - g_i + 0.75 \sum_{j=1}^{4} g_j$, where j indexes the members of the same contribution group.[10]

Subjects make contribution decisions in five different treatments, denoted as ’scenarios’, described in subsection 3.1. The underlying public good game is the same across all five treatments and subjects are informed that any decision they make in the experiment has a positive chance of being payoff-relevant for them and the other three group members.

[10]The change in the marginal per capita return from 0.4 to 0.75 is driven by an attempt to secure a high share of CCs so as to increase statistical power of our analysis.

3.1 Treatments

In **treatment 1**, subjects simply decide how much to contribute unconditionally. This is the first treatment presented to *all* subjects. Treatments 2 through 5 are labeled “conditional treatments”. In each of these four treatments, subjects fill out a “contribution table” in which they specify how much they wish to contribute conditionally on the average rounded contribution of the other three group members, or the average of three random numbers in treatment 5, and they are informed about a specific way in which the contributions of the other group members are determined. The conditioning variable takes values from the set $\{0, 1, ..., 10\}$ and subjects are asked to specify their contribution for each possible value of the conditioning variable. In treatments 2 through 4, the conditioning variable is equal to the rounded average contribution of the other three group members. What differs across these three treatments is how the contributions of the other group members are actually determined. In **treatment 2**, as in FGF, the other group members’ contributions are equal to their unconditional contributions from treatment 1. In **treatment 3**, the other group members’ contributions are equal to the unconditional contributions of three randomly chosen non-group members from treatment 1. In **treatment 4**, the other group members’ contributions are randomly generated by a computer from the uniform distribution on $\{0, 1, ..., 10\}$. In **treatment 5**, subjects condition on the average of three randomly drawn numbers from the uniform distribution on $\{0, 1, ..., 10\}$. The other group members’ contributions are independently randomly generated by a computer from the same distribution. The four conditional treatments are presented to subjects in different orders, with the number of participating subjects being the same for all of the 24 permutations.

This design allows us to disentangle the impacts of reciprocity, conformity, inequality aversion and residual factors on the conditional contribution behavior in treatment 2. Behavior in this treatment is potentially affected by all four explanations. Treatments 3, 4 and 5 eliminate reciprocity as an explanation since the other three group members do not determine their own contribution amounts. Treatments 4 and 5 also eliminate conformity as an explanation since the contributions of the other three group members are computer-generated. Treatment 5 further eliminates inequality aversion as an explanation since the conditioning variable is independent of anyone’s contribution. This identification strategy is summarized in Table 1. It follows that the impact of reciprocity is identified by comparing conditional contributions in treatments 2 and 3; that of conformity by comparing treatments 3 and 4; that of inequality aversion by comparing treatments 4 and 5. Treatment 5 identifies the impact of residual factors.

Table 1: Identification Strategy

	Treatment 2	Treatment 3	Treatment 4	Treatment 5
Reciprocity	x			
Conformity	x	x		
Inequality Aversion	x	x	x	
Residual factors	x	x	x	x

3.2 Procedure

Each experimental session begins with one page of printed General Instructions (see the Appendix). Subjects are given information about the outline of the experiment, including the number of treatments, labeled "decision scenarios", and that they will not be given any feedback on their own or anyone else's decisions or earnings before the feedback stage at the end of the experiment. They are also given standard logistical instructions and are informed about the exchange rate between experimental tokens and cash. Finally, they are also informed that in each treatment they will interact in the same group of 4 subjects and that everyone will be paid based on the same *one* treatment (strategy method) randomly determined by a public draw toward the end of the experiment. This is followed by another page of printed instructions (see the Appendix) describing the public goods game and its payoffs. This is the game played in treatment 1. Subjects are also notified that payoffs are calculated in the same way also in the following treatments. The subjects then have 3 minutes to use an on-screen interactive simulator (see the Appendix for a screenshot) on which they can simulate their earnings and the earnings of the other group members as a function of all four group members' contributions. The initial values of the four contributions are randomly computer-selected in order to mitigate any potential anchoring bias. Subjects can add to or subtract from the individual contributions in increments of 1. After each incremental change, subjects can observe the change in everyone's payoffs. This simulator design aims to make it clear to subjects what the marginal payoff consequences of their own contributions and those of others, are. After the simulator time, the experiment progresses to treatment 1 in which subjects decide on their unconditional contributions (see the Appendix for a screenshot).

After treatment 1 is finished, we distribute additional printed instructions common to treatments 2-5 (see the Appendix). They explain the principle of conditional contribution as follows. There are three Type X participants and one Type Y participant in each group. Types are randomly chosen by a computer, with each participant having the same chance of being the Type Y participant. The Type X participants contribute to the public good according to the rule announced for each treatment. The Type Y participant contributes to the public good based on his/her decisions in the "contribution table". The task in each treatment is to fill out the contribution table for the case when one is selected to be the Type Y participant. The instructions then describe what the contribution table looks like and, by means of examples, which of the conditional contributions input into the table becomes relevant for the group members' earnings. The subjects are also told that treatments 2-5 will be presented to them in a random order and that they will receive instructions for each scenario on the screen. The subjects are then sequentially presented with treatments 2-5, and make 11 conditional contribution decisions in each. The subjects are never aware of the content of upcoming treatments while making their decisions in the current treatment. The on-screen instructions inform the subjects about how the actual contributions of the three Type X participants are determined and about the definition of the conditioning variable. In order to further aid understanding, the text instructions are complemented by a graphical scheme illustrating how the contributions are determined in that particular treatment (see the Appendix).[11]

After all subjects have finished entering their conditional contributions, we administer a demographic questionnaire. We elicit gender, age, country of origin, number of siblings, academic major, the highest achieved academic degree so far, and an estimate of monthly spending.

The subjects are paid based on their decisions in one treatment chosen randomly by a public draw of a chip numbered from 1 to 5 at the end of the experiment. If treatment 1 is chosen to be payoff-relevant, the contributions are determined according to the decision of each group member in that treatment. If one of the other four conditional treatments is chosen to be payoff-relevant, then one group member is randomly chosen by a computer to be the Type Y participant, with the remaining three group members being assigned the role of Type X participants. Everyone's contributions and earnings are then determined according to the rules described above. At the end of the experiment, experimental earnings in tokens are converted into cash and paid privately to the subjects.

[11]The instructions and the graphical schemes were tested during three pilot sessions in order to ensure understanding by subjects.

3.3 Logistics

We collected data for 192 subjects over 9 sessions. There are 8 participating subjects for each of the 24 orders in which the four conditional treatments were presented. Due to a technical problem, the decisions of one subject for one of the scenarios were not recorded. Given our stress on within-subject design, we decided to drop this subject from our data set. The dataset we utilize therefore contains 191 subjects. All sessions were conducted in the *Laboratory of Experimental Economics* (LEE) at the University of Economics in Prague in May and June 2018. The experiment used a computerized interface programmed in zTree (Fischbacher, 2007). Subjects were recruited using the Online Recruitment System for Economic Experiments (Greiner, 2015) from a subject database belonging to the lab. Our subjects are students from various universities in Prague, mostly from the University of Economics. Almost 72% of the subjects report "Economics or Business" as their field of study, with the remaining subjects reporting other fields. The gender ratio is almost exactly balanced.[12] One experimental token was worth 10 Czech koruna (CZK).[13] The average cash payoff, including a 75 CZK show-up fee, was 280 CZK[14] for approximately 1 hour of participation.[15] It is cumbersome to perform ex-ante power calculations as there are no previous results using a comparable design for identification of the determinants. Using the results of Bardsley and Sausgruber (2005) as a prior belief for the expected effect size for conformity, our design is able to identify an effect of the same size ($\alpha = 0.05$) with power $(1 - \beta) = 0.87$.[16]

4 Methodology for Data Analysis

We use two methodologies to evaluate the extent to which conditional contributions increase with the value of the conditioning variable. The first one is based on the method proposed by Thöni and Volk (2018), which is itself a slight modification of the method used by FGF. It evaluates statistical strength of the relationship for each individual subject in a given treatment by means of the estimated Pearson correlation coefficient. If and only if the coefficient estimate is at least 0.5, the subject is

[12]There are 95 males and 96 females in the sample. We recruited males and females separately in order to achieve an approximately gender-balanced sample, but we did not insist on a specific proportion of males and females when the subjects arrived at the lab.

[13]1 EUR was equal around 25.8 CZK and 1 USD was worth around 22 CZK at the time of the experiment.

[14]This was approximately 10.9 EUR or 12.7 USD at the time of the experiment.

[15]For a comparison, the hourly wage that students could earn at the time of the experiment in research assistant or manual jobs typically ranged from 100 to 120 CZK.

[16]Using the GPower software (Faul et al., 2009).

proclaimed to be a CC in the given treatment.[17] Formally, let i index subjects, $j \in \{2, 3, 4, 5\}$ index treatments and $c \in \{0, 1, ..., 10\}$ index the value of the conditioning variable. Also, let g_{ijc} be the conditional contribution of subject i in treatment j if the value of the conditioning variable is c and let $\bar{g}_{ij\cdot} \equiv \frac{1}{11}\sum_{c=0}^{10} g_{ijc}$ and $\bar{c} \equiv \frac{1}{11}\sum_{c=0}^{10} c = 5$. The estimated correlation coefficient for subject i in treatment j is then given by

$$\begin{aligned}
\hat{\rho}_{ij} &\equiv \frac{\sum_{c=0}^{10}(c-\bar{c})(g_{ijc}-\bar{g}_{ij\cdot})}{\sqrt{\sum_{c=0}^{10}(c-\bar{c})^2}\sqrt{\sum_{c=0}^{10}(g_{ijc}-\bar{g}_{ij\cdot})^2}} \\
&= \hat{\beta}_{ij}\sqrt{\frac{\sum_{c=0}^{10}(c-\bar{c})^2}{\sum_{c=0}^{10}(g_{ijc}-\bar{g}_{ij\cdot})^2}} \\
&= \sqrt{\frac{\hat{\beta}_{ij}^2}{\hat{\beta}_{ij}^2 + \sum_{c=0}^{10}\hat{\varepsilon}_{ijc}^2/\sum_{c=0}^{10}(c-\bar{c})^2}} \\
&= \sqrt{\frac{\hat{\beta}_{ij}^2}{\hat{\beta}_{ij}^2 + [\text{s.e.}(\hat{\beta}_{ij})]^2}},
\end{aligned}$$

where $\hat{\beta}_{ij}$ is the estimated slope coefficient from the OLS regression of g_{ijc} on c for the given individual-treatment pair ij, $\hat{\varepsilon}_{ijc}$ are the estimated residuals from this regression and s.e.$(\hat{\beta}_{ij})$ is the standard error of $\hat{\beta}_{ij}$ without the small sample adjustment. It follows that being categorized as a CC is equivalent to the z-statistic $\hat{\beta}_{ij}/\text{s.e.}(\hat{\beta}_{ij})$ exceeding $\sqrt{1/3} \doteq 0.58$ or to the t-statistic $\hat{\beta}_{ij}/[\sqrt{1.1}\text{s.e.}(\hat{\beta}_{ij})]$ that includes the small-sample adjustment exceeding $\sqrt{10/33} \doteq 0.55$. We then compare the incidence of CC (a binary variable) across treatments using the paired sign test that accounts for a possible within-subject correlation in noise across different treatments that might be present in a within-subject design.

The second approach takes the path of evaluating the average relationship between conditional contributions and the conditioning variable for the entire sample (and hence inferring it for the population). To parsimoniously capture the strength of the average CC behavior, we approximate the average conditional contribution schedule in each treatment by means of a separate affine function and estimate this function using OLS. That is, for each treatment j, we regress g_{ijc} on c using data for all subjects i. Since the conditioning variable realizations are identical for each subject-treatment pair, this is equivalent to allowing each subject in each treatment to have a different affine average contribution schedule and estimating the average intercept and slope of these schedules across all subjects sepa-

[17] Thöni and Volk (2018) use an additional minor tie-breaker that determines whether a subject is categorized as a CC or a triangle cooperator. We do implement the tie-breaker in our classification but ignore it in the exposition for the sake of tractability.

Table 2: Type Classification by Treatment, Full Dataset (% of all subjects)

	Treatment 2	Treatment 3	Treatment 4	Treatment 5
Conditional cooperator	**57.6**	**56.0**	**52.9**	**40.8**
Triangle cooperator	12.6	13.6	15.7	12.6
Free-rider	12.0	10.5	15.2	19.4
Unconditional cooperator	9.4	8.9	7.3	17.3
Other type	8.4	11.0	8.9	10.0

rately in each treatment. We then evaluate both the magnitude and the statistical significance of the estimated slope coefficients $\hat{\beta}_j$. Again, to account for a possible within-subject correlation in noise across different treatments, the standard errors are adjusted for clustering at subject level.

5 Results

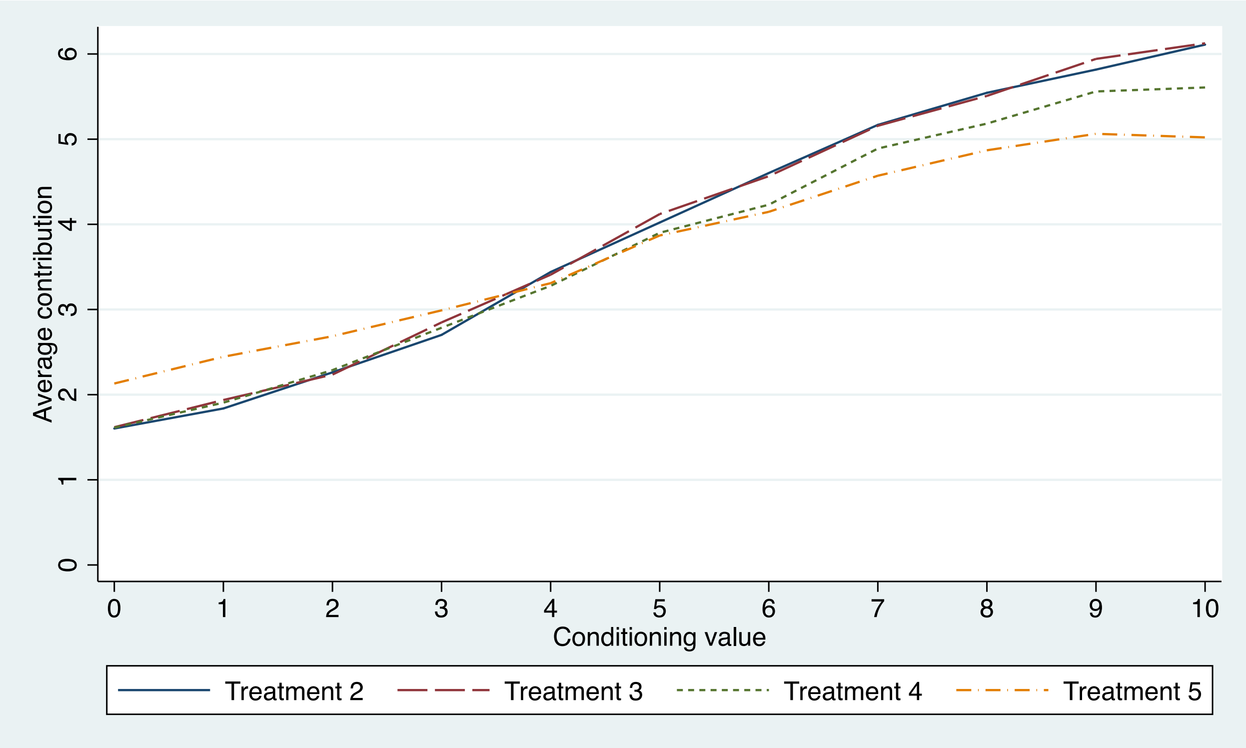
5.1 Type Classification Method

Table 2 displays conditional contribution type classification by treatment using the full dataset based on the method of Thöni and Volk (2018) (and their STATA routine). Based on the classification in treatment 2, the one considered in the previous literature, we classify 110 subjects (57.6%) as conditional cooperators, 24 subjects (12.6%) as triangle cooperators, 23 subjects (12.0%) as free-riders, 18 subjects (9.4%) as unconditional cooperators and 16 subjects (8.4%) as "other" type. Regarding the incidence of the first two types, our results are consistent with the range of type distributions recorded in many previous studies (Thöni and Volk, 2018). Regarding the incidence of free-riding, our finding lies toward the bottom edge of the range identified in the literature. We speculate that this is primarily driven by the high MPCR of 0.75 in our study, which coincides with the upper boundary of the range used in the literature. Minimization of free-riding does fit our objective of increasing the prevalence of conditional cooperation and hence increasing the power of our analysis.[18]

Looking beyond treatment 2, the most striking observation in Table 2 is that even in treatment 5, where only residual factors are at play, 40.8% of subjects are classified as CCs, meaning that they have a (roughly) increasing pattern of conditional contributions. This fraction is statistically highly

[18]Regarding unconditional contributions, 90% of subjects choose positive contributions in treatment 1. The mean (median) unconditional contribution is 6.13 (6).

Figure 1: Average Contribution by Conditioning Value and Treatment



significant (two-sided proportion test $p < 0.001$). In treatment 4, 52.9% of subjects are classified as CCs, 12.1 percentage points higher than in treatment 5. This difference is statistically highly significant (two-sided paired sign test $p = 0.003$). In treatment 3, 56% of subjects are classified as CCs, 3.1 percentage points higher than in treatment 4. This difference is statistically insignificant ($p = 0.392$). In treatment 2, as mentioned, 57.6% of subjects are classified as CCs, 1.6 percentage points higher than in treatment 3. This difference is also statistically insignificant ($p = 0.69$). These results suggest that two significant drivers of CC are residual factors and inequality aversion. On the other hand, conformity and reciprocity do not individually contribute significantly to conditional cooperation.

5.2 Regression-Based Method

Figure 1 plots the average conditional contribution across all subjects by the conditioning value c and treatment. Corresponding to our findings in the previous subsection, we observe that the pattern of conditional contributions is increasing with c in all four treatments. Also, it is approximately linear, *ex post* justifying the use of a linear function approximation. The regression results are presented in

Table 3. In the left panel, columns "Intercept" and "Slope" report estimates of the intercept and the slope, respectively, of the average conditional contribution schedule by treatment. The right panel presents how the slope estimate of CC in treatment 2 decomposes into the portions due to the four hypothesized drivers, both in absolute and in proportional terms. In treatment 5, where only residual factors play a role, the slope estimate is 0.322 and is statistically highly significant ($p < 0.001$). Consistent with the findings in the previous subsection, we find that residual factors play an important role in driving CC behavior. The estimate of the slope coefficient then increases to 0.440 in treatment 4, with the difference of 0.118 being statistically highly significant ($p = 0.001$). That is, inequality aversion also significantly affects CC behavior. Going to treatment 3, the slope coefficient increases further to 0.492, but the difference of 0.052 is only weakly statistically significant ($p = 0.082$). Hence conformity appears to be only a mild contributor to CC behavior. Finally, going to treatment 2, the slope coefficient increases only cosmetically to 0.495. As a result, reciprocity does not contribute to CC behavior. The last column shows that approximately two thirds of the slope coefficient in treatment 2 comes from residual factors, one quarter from inequality aversion and one tenth from conformity. Reciprocity accounts for virtually none of the CC behavior.[19]

6 Discussion

Our type-classification results suggest that there is a lot of CC even when the conditioning variable is meaningless. In particular, 41% of subjects are CCs in treatment 5 in which such correlation can only be attributed to residual factors. Recall from Section 1 that we expect that a major part of the residual factors is accounted for by confusion. In this respect, our results to some extent mirror the findings of Ferraro and Vossler (2010) and Burton-Chellew et al. (2016). However, while the implicit message of these papers is that *all* of CC can be accounted for by confusion, we find that this is not the case. Our results suggest that inequality aversion is another significant driver of CC on top of residual factors. Using the regression-based results for magnitude comparisons, we estimate that residual factors account for two thirds of CC. Taking into account a possible role of non-confused anchoring, this magnitude gives an upper bound on the possible importance of confusion in driving CC. That said, it does appear that potentially more than one half of CC as observed in the lab is an

[19]With $\hat{\beta}_j$ being the slope estimate for treatment $j \in \{2, .., 5\}$, we define the proportional impact of reciprocity, conformity, inequality aversion and anchoring by $(\hat{\beta}_2 - \hat{\beta}_3)/\hat{\beta}_2$, $(\hat{\beta}_3 - \hat{\beta}_4)/\hat{\beta}_2$, $(\hat{\beta}_4 - \hat{\beta}_5)/\hat{\beta}_2$ and $\hat{\beta}_5/\hat{\beta}_2$, respectively. The respective standard errors are obtained using the Delta method.

Table 3: Regression-Based Estimates of CC, Full Dataset

Conditional Contribution Schedule			**Decomposition**		
Treatment	**Intercept**	**Slope**	**Driver**	**Slope**	**Percent**
2	1.446***	0.495***	Overall effect	0.495***	100.0
	(0.241)	(0.036)		(0.036)	(0.0)
			Reciprocity	0.002	0.5
				(0.024)	(4.8)
3	1.490***	0.492***			
	(0.235)	(0.037)			
			Conformity	0.052*	10.5*
				(0.030)	(6.0)
4	1.547***	0.440***			
	(0.245)	(0.038)			
			Inequality aversion	0.118***	23.8***
				(0.036)	(7.1)
5	2.124***	0.322***	Residual factors	0.322***	65.2***
	(0.250)	(0.035)		(0.035)	(6.0)

Notes: Standard errors adjusted for clustering at subject level in parentheses. Statistically significant at: * 10%, ** 5 %, *** 1%.

artefact of experimental design.

In terms of the relative impact of the four potential drivers, our results are qualitatively similar to those of Cappelletti et al. (2011). Since they do not consider subject confusion, in their classification, “anchoring” accounts for what we call residual factors. Their results suggest that anchoring (residual factors) and inequality aversion are the only statistically significant drivers of CC. Unlike us, they estimate their relative contribution to be about the same. Our regression-based results suggest that residual factors (that include anchoring) play a much larger role than inequality aversion. Recall, however, that their design is complex and the results are quite sensitive to which sub-range of the conditioning variable one looks at. We therefore speculate that the magnitude difference in the two sets of estimates is driven by their design complexity. In terms of the relative impact of reciprocity and inequality aversion, our results are also in accordance with those of Ashley et al. (2010). On the other hand, our results are different from the findings of Bardsley and Sausgruber (2005). They find reciprocity to have twice as large an effect as conformity, whereas we find that reciprocity has a minimal effect, while conformity has a statistically borderline significant effect. As we have argued in Section 2, however, their estimate of conformity is likely to be downward-biased. We speculate that the difference with our results is driven by this bias.

It is possible to raise a concern that the strong effect of residual factors that we associate with confusion and anchoring might be at least partly driven by by an experimenter demand effect. The hypothesis here is that subjects in treatment 5 might ask themselves what the experimenter expects of them, given the irrelevance of the conditioning variable. Some might conclude that an increasing pattern of conditional contributions is expected. However, to the extent subject behavior in treatment 5 is indeed driven by such a demand effect, we would expect the effect to be the strongest among subjects who see treatment 5 before the other three conditional treatments. On the other hand, we would expect the effect to be smaller if treatment 5 comes after some or all three other conditional treatments. The reason is that, in the latter case, subjects are also likely to think how the experimenter wants them to *change* their behavior relative to the previous conditional treatment(s). Given the irrelevance of the conditioning variable in treatment 5, unlike in the previous treatments, subjects are more likely to conclude that contributions should be unresponsive to the conditioning variable. Based on between-subject comparisons, the share of CCs (and its standard error) for those subjects who see treatment 5 first, second, third or fourth is 47.9% (0.036), 40.4% (0.036), 27.1% (0.032) or 47.9% (0.036) respectively. These shares are statistically indistinguishable. In particular, there is no difference in the share of subjects classified as CCs between those subjects who see treatment 5 first and those who see it last. We therefore conclude that an experimenter demand effect is not likely to be a driver of the strong effect of residual factors.

A reader might also wonder why we do not condition our decomposition exercise only on those subjects who are classified as CCs in treatment 2. The reason is that such an endogenous sample selection poses an important identification problem that tends to overstate the role played by reciprocity. To illustrate the point, imagine that the behavior of subjects is random and uncorrelated across treatments, with a fraction p classified as CCs in any given treatment due to noise. Then, conditioning on those who are classified as CCs in the treatment in which reciprocity plays a role erroneously implies a reciprocity-driven treatment effect of $1 - p$ on the share of CCs. To avoid this problem, we perform our analysis on the full sample.

7 Conclusion

We use a laboratory experiment to decompose CC, as identified in the design of FGF, into parts driven by reciprocity, conformity, inequality aversion and residual factors. We associate residual factors mostly with subject confusion and anchoring. We find that around 40% of subjects are categorized as CCs even in the treatment where only residual factors play a role. This is more than two thirds of the 58% share of subjects who are classified as CCs in the treatment in which all four drivers play a role, and that has been considered by the previous literature. Inequality aversion is found to play an important role too, accounting for 12 percentage points of the difference between the two previously mentioned shares. Conformity accounts for another 3 percentage points, with this effect being only marginally statistically significant. Reciprocity plays a minimal role, accounting for only about 1.6 percentage points of the difference.

To better gauge the extent of CC, we also use a regression analysis that identifies the sensitivity of the conditional contributions to the conditioning variable. The results match those of the type classification analysis discussed above. Two thirds of CC is accounted for by residual factors, a quarter by inequality aversion and a tenth by conformity, with reciprocity playing virtually no role.

Our results confirm that subject confusion is likely to be a significant driver of CC as observed in the lab, but also suggest that it cannot account for all CC observed in FGF and its replications. Our results also suggest that reciprocity has a minimal role in driving CC. Instead, the part of CC unaccounted for by residual factors appears to be predominantly driven by inequality aversion, and, possibly to some extent, by conformity.

Our results have implications for how to exploit CC as observed in the lab for fundraising. The main message is that one should be less optimistic about the true strength of CC relative to what is suggested by laboratory studies. This is because a major part of CC as observed in the lab, which we label as residual factors, appears to be an artefact of experimental design that is that is not likely to hold up in the field. To some extent, however, residual factors might also be driven by unconfused anchoring. Such anchoring can be exploited for fundraising design by suggesting high(er) contributions to would-be contributors. Indeed, casual observation suggests that many charities use such a strategy, and several field experiments for the most part confirm its effectiveness (see, for example, Charness and Cheung, 2013, Edwards and List, 2014). Our results suggest that reciprocity, conformity and inequality aversion only account for about one third of CC and, of this effect, most is accounted for by inequality aversion. Therefore, to the extent that a fundraiser wants to leverage CC accounted for by these three drivers, it appears to be most important to stress to would-be contributors that their contribution will ensure that the financial burden of the campaign is shared more equally (fairly) among the relevant population of potential contributors.

It is important to stress that all of these suggestions are derived from decomposition of CC as observed in the lab. We therefore do not suggest that reciprocity is irrelevant for fundraising in the field. Indeed, there is strong evidence to the contrary (Falk, 2007). We speculate that the FGF environment might not provide a suitable setting for reciprocity to manifest itself. The reason is driven by the nature of the conditioning variable and equal sharing of contribution benefits. Even though a higher average contribution of the other three group members does benefit the conditional contributor, it is not a yet-unrewarded act of kindness. In particular, as long as the product of $n-1$ and the MPCR exceeds 1, the three unconditional contributors are already gaining, on average, from their contributions relative to free-riding. The conditional contributor might therefore not feel the reciprocal drive that would be present if the conditional contributions were yet-unrewarded, as is the case in typical implementations of gift-exchange settings (such as Falk, 2007).[20] This observation underlines the fact that CC as observed in the lab might be more relevant for field settings in which contributors are also beneficiaries of the fundraising effort as opposed to settings in which the two groups are distinct.

[20]Indeed, of the 20 papers surveyed by Thöni and Volk (2018), $(n-1)\times$MPCR exceeds 1 in 19 studies is equal to 1 in the remaining study.

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Appendix

On the following pages, we present the following:

1. General instructions (printed)
2. Game description and instructions for Treatment 1 (printed)
3. Earning simulator (on-screen)
4. Common instructions for treatments 2-5 (printed)
5. Treatment-specific instructions for treatments 2-5 (on-screen)

GENERAL INSTRUCTIONS

OUTLINE OF THE EXPERIMENT

The experiment consists of the following **parts**:

1. **Instructions**.
2. **Five decision scenarios**. You will receive printed instructions for the first scenario. Instructions for the following scenarios will be distributed later. After going through the instructions at your own pace, you will enter your decisions.
3. **Demographic Questionnaire**, in which you will be asked a few questions about your demographic and academic background.
4. **Feedback** about your earnings. You will not be given any feedback on your or anyone else's decisions or earnings before this.

LOGISTICS

- During the experiment, **please do not communicate with other participants**. Please turn off the ringer on your mobile phone at this moment.
- **There are no time restrictions for submitting your decisions during the experiment. You are free to progress at your own pace as you see fit. However, if progressing slowly, you may be asked by an experimenter to enter your decision(s) more quickly.** Note that you might at times need to wait until other participants submit their decisions.
- If you think that your computer is frozen anytime during the experiment, please raise your hand. We will assist you.
- Your earnings and earnings of the other participants in this experiment will be measured in **experimental points**. At the end of the experiment, experimental points you earn will be converted into CZK and paid out in cash (**1 experimental point = 10 CZK**). Individual earnings will be kept confidential.

SCENARIOS, PARTICIPANT MATCHING AND PAYMENT SCHEME

- In each of the five decision scenarios, you will be matched to **another three participants**. With them, you will form a **group of four participants**. No participant will know the identity of the other group members in his or her group.
- Your earnings in a given scenario will depend on your decisions and on decisions of the three other members of your group, and possibly also on a random draw.
- You and every other participant will be paid according to your point earnings in **one and only one of the five scenarios**. However, you do not know which one of the five it will be. Near the end of the experiment, one of the participants will draw a chip from a bag of chips numbered from 1 to 5. The drawn chip will determine which of the five scenarios is relevant for everyone's earnings.
- It is therefore important that you consider your decisions in each scenario **separately from your decisions in the other scenarios**.

DECISION SITUATION

- We first introduce you to the basic decision situation.
- You will be a member of a group consisting of **4 participants**. Each group member has to decide on the allocation of 10 tokens. You can put these 10 tokens into your **private account** or you can contribute them **fully or partially** to a **group project**. Each token you do not contribute to the group project will automatically remain in your private account.
- The total amount of tokens allocated to the **group project** is equal to the **sum of contributions of the four group members**.

YOUR EARNINGS FROM THE PRIVATE ACCOUNT

- **You will earn one point for each token you put into your private account**. For example, if you put 10 tokens into your private account (and therefore do not contribute to the group project), your earnings from the private account will amount to exactly 10 points. If you put 6 tokens into your private account, your earnings from this account will be 6 points. **No one except you earns anything from your private account**.

YOUR EARNINGS FROM THE GROUP PROJECT

- **Each group member will profit equally from the amount you contribute to the group project**. You will also benefit from the other group members' contributions. The earnings of each group member from the group project will be determined as follows:

$$\boxed{\textbf{\textit{Earnings from the group project}} = \textit{0.75} \times \textit{sum of all the contributions}}$$

- If, for example, the sum of all contributions to the group project is 28 tokens, then you and the other members of your group each earn 0.75×28 = 21 points out of the group project. If the four members of the group contribute a total of 4 tokens to the group project, you and the other members of your group each earn 0.75×4 = 3 points.

YOUR TOTAL EARNINGS FROM SCENARIO 1

- Your total earnings from this scenario will be the **sum of your earnings from your private account and from the group project**:

Earnings from your private account (= 10 – your contribution to the group project)

+ Earnings from the group project (= 0.75 × sum of the contributions to the group project)

= Total earnings from the scenario

EARNINGS OF THE OTHER GROUP MEMBERS

- Earnings of the other group members are computed in an analogous way.

Please note that all the numbers used in these examples are selected for illustrative purposes only. They do not indicate how anyone decides or should decide. You will have an opportunity to use a "Simulator" of your earnings and earnings of other group members at the beginning of the experiment (without any consequences for your earnings).

Instructions for other scenarios will be shown on the screen. However, the calculation of your earnings from the private account and the group project in each scenario is as described on this page.


Remaining time [sec]: 115
Earning simulation
You can use buttons bellow to simulate your earnings and earnings of other players depending on the contributions to the group account
Contributions of other players
-1
0
+1
-1
0
+1
-1
0
+1
Earnings of other players
10.0
10.0
10.0
Average contribution of three other players
0
Your contribution
-1
0
+1
Your earnings
10.0

INSTRUCTIONS FOR SCENARIOS 2-5

The method of payoff calculation from the private accounts and the group project is the same as in Scenario 1. In each of these scenarios, there will be three **Type X** participants and one **Type Y** participant in each group. Your type is randomly chosen by the computer, with each participant having the same chance of being the **Type Y** participant. The **Type X** participants contribute to the group project according to the rule which will be announced for each scenario. The **Type Y** participant contributes to the group project based on his/her decisions in the CONTRIBUTION TABLE (see below). Your task in each scenario is to fill out the Contribution table for the case you are selected to be the **Type Y** participant.

CONTRIBUTION TABLE

The Contribution table lets you condition your **Type Y** contribution on the rounded average of three numbers between 0 and 10. Details of what these numbers are will be provided for each scenario. Here is what such table looks like before you fill it out:

Rounded average of the three numbers	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Your contribution if you are the Type Y participant											

In each scenario, the rounded average takes one of the values 0, 1, .., 10, but you do not know which one it is when you fill out the table. Therefore, please carefully consider how much to contribute for each potential value of the average. If you are drawn to be the **Type Y** participant in that scenario, your contribution will be the value you filled in below the value of the average that was actually realized in that scenario.

On the next page, we present several examples. We use letters instead of numbers to denote your conditional contributions in these examples. **Please note that all the values of the average used in these examples are selected for illustrative purposes only. They do not indicate how anyone decides or should decide.**

Example 1: Suppose the rounded average of three numbers is 0. Then your contribution to the group project (if you are the Type Y participant) is **f.**

Rounded average of the three numbers	0	1	2	3	4	5	6	7	8	9	10
Your contribution if you are the Type Y participant	f	g	h	i	j	k	l	m	n	o	p

Your contribution to the group project = f

Example 2: Suppose the rounded average of three numbers is 10. Then your contribution to the group project (if you are the Type Y participant) is **p.**

Rounded average of the three numbers	0	1	2	3	4	5	6	7	8	9	10
Your contribution if you are the Type Y participant	f	g	h	i	j	k	l	m	n	o	p

Your contribution to the group project = p

Example 3: Suppose the rounded average of three numbers is 5. Then your contribution to the group project (if you are the Type Y participant) is **k.**

Rounded average of the three numbers	0	1	2	3	4	5	6	7	8	9	10
Your contribution if you are the Type Y participant	f	g	h	i	j	k	l	m	n	o	p

Your contribution to the group project = k

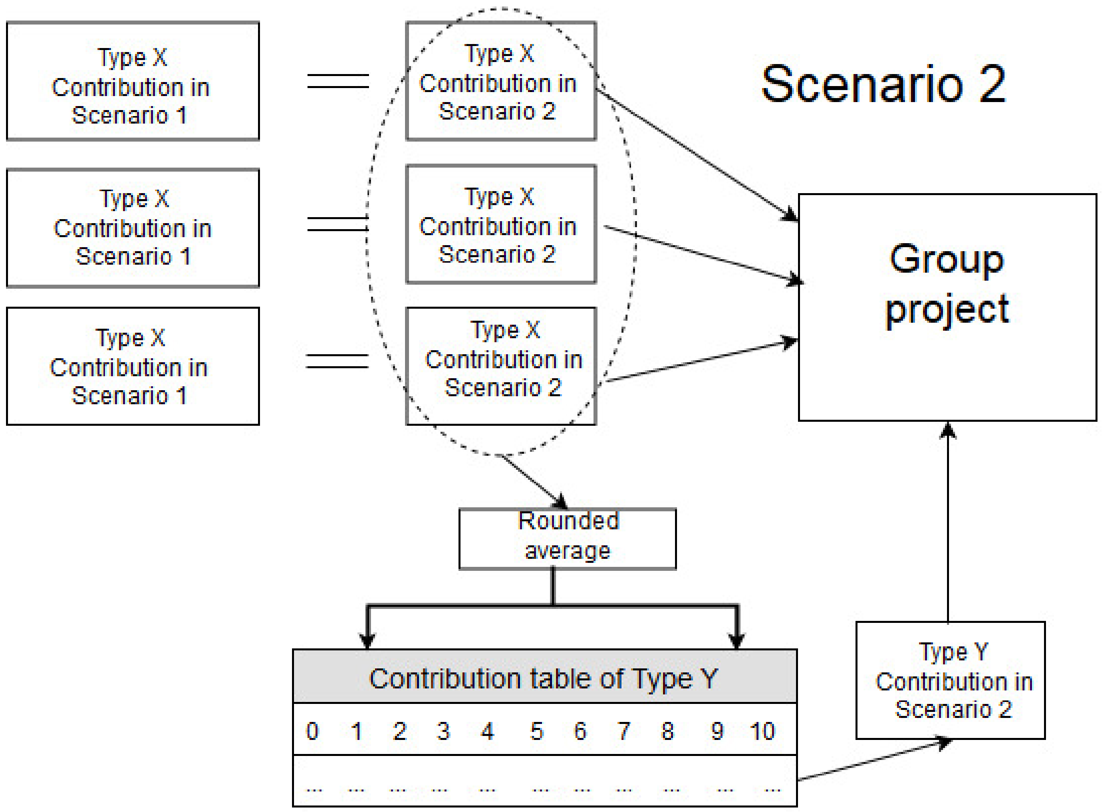
SCENARIO-SPECIFIC INSTRUCTIONS

Scenarios 2 to 5 will be presented to you in a random order. You will receive instructions for each scenario on the screen. They are complemented by a graphical scheme illustrating how the contributions are determined in that particular scenario.

SCENARIO 2

Type X contributions to the group project: Their own contributions in Scenario 1.

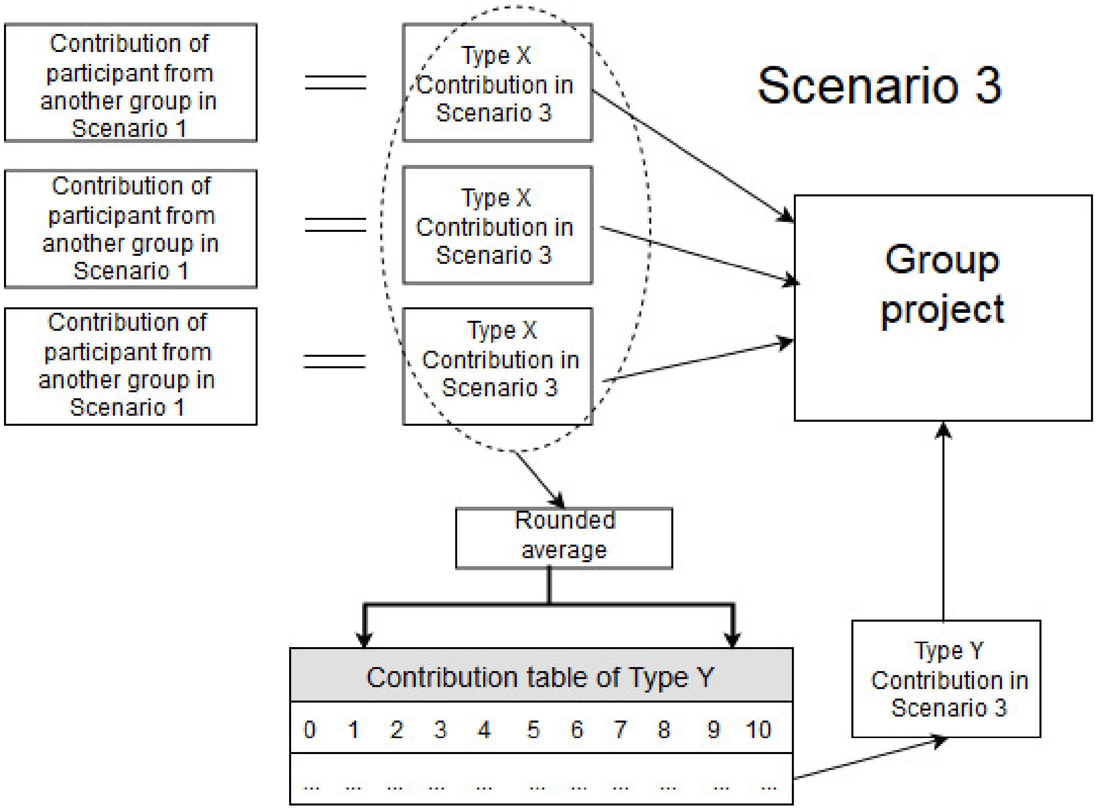
Type Y contribution to the group project: Based on the rounded average of the Type X contributions and the Contribution table.



<u>Scenario 3</u>

Type X contributions to the group project: Contributions of randomly chosen participants from other groups in Scenario 1.

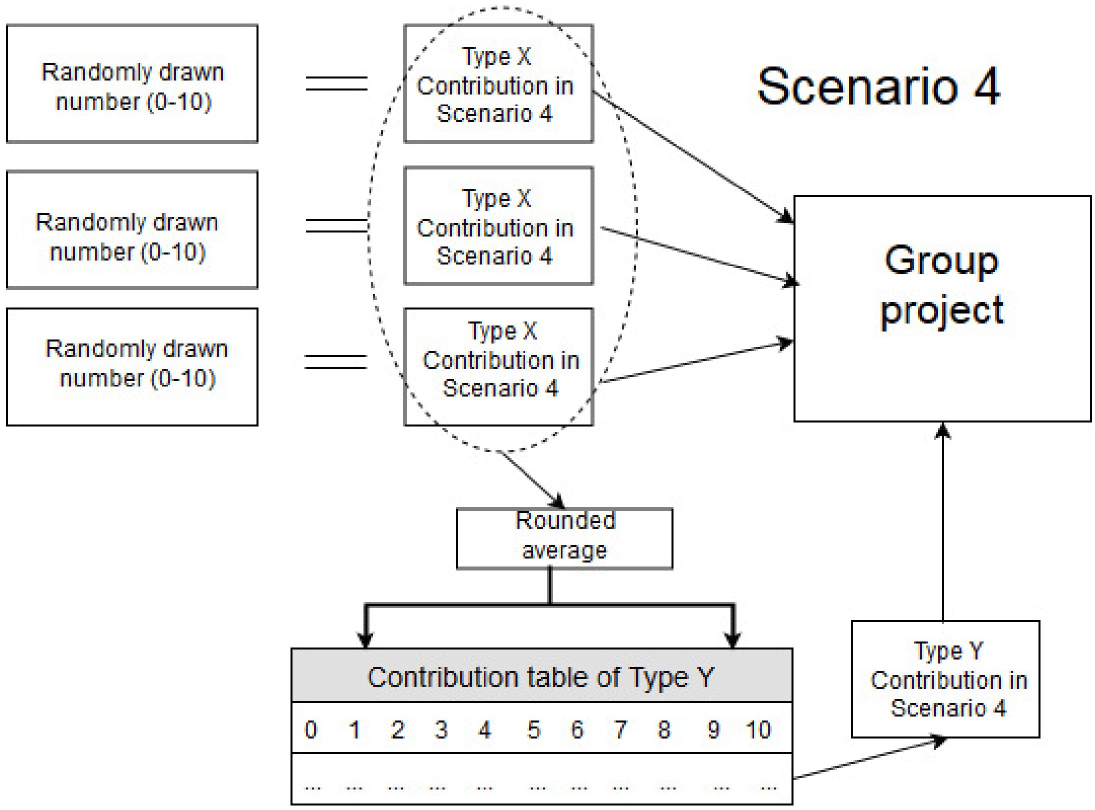
Type Y contribution to the group project: Based on the rounded average of the Type X contributions and the Contribution table.



SCENARIO 4

Type X contributions to the group project: Randomly selected by the computer from values 0, 1, …, 10. Each value has the same chance to be drawn. The three draws for the three Type X participants are independent.

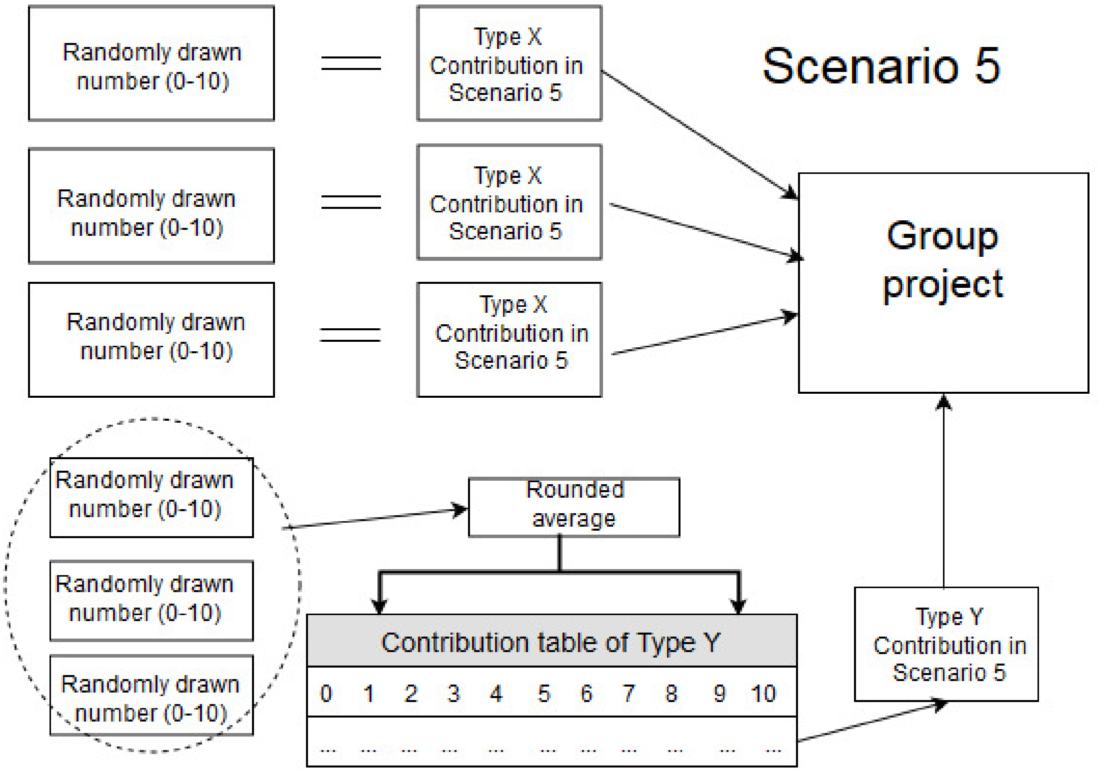
Type Y contribution to the group project: Based on the rounded average of the Type X contributions and the Contribution table.



SCENARIO 5

Type X contributions to the group project: Randomly selected by the computer from values 0, 1, …, 10. Each value has the same chance to be drawn. The three draws for the three Type X participants are independent.

Type Y contribution to the group project: Based on the rounded average of ANOTHER three randomly drawn values from 0, 1, ..., 10 and the Contribution table. Each value has the same chance to be drawn. The three draws are independent from each other and also from the contributions of Type X participants.



Abstrakt

Rozsáhlý experimentální výzkum her na společné dobro dokumentuje, že mnoho účastníků je "podmíněnými spolupracovníky", jelikož jejich příspěvky (spolu s jejich očekáváními) pozitivně korelují s příspěvky ostatních subjektů v jejich skupině. Cílem naší studie je osvětlit, jaké preference a rozhodovací vzorce vedou k této pozorované pravidelnosti v chování. Zvažujeme čtyři potenciální vysvětlení, včetně reciprocity, konformity, averzi k nerovnosti a reziduálních faktorů, jako je například zmatení a kotvení, s cílem oddělit jejich účinky. Zjistili jsme, že z průměrného podmíněně kooperativního chování v našem vzorku se dají vysvětlit asi dvě třetiny pomocí zbytkových faktorů, dále čtvrtina averzí k nerovnosti a desetina konformitou, zatímco reciprocita nehraje prakticky žádnou roli. Tyto výsledky přinášejí důležitá sdělení o tom, jak interpretovat podmíněnou spolupráci standardně pozorovanou v laboratoři, a nastiňují způsoby, jak je lze využít pro účely fundraisingu.

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