What Drives Conditional Cooperation in Public Good Games?*

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Abstract

Extensive experimental research on public good games documents that many subjects are "conditional cooperators" in that they positively correlate their contribution with (their belief about) contributions of other subjects in their peer group. The goal of our study is to shed light on what preference and decision-making patterns drive this observed regularity. We consider reciprocity, conformity, inequality aversion and residual factors, such as confusion and anchoring, as potential explanations. Effects of these drivers are separated by varying the informational content of the conditioning variable across treatments. We find that, of the average conditionally cooperative behavior, about two thirds is accounted for by residual factors, a quarter by inequality aversion and a tenth by conformity. Reciprocity plays little role. These findings carry an important message for how to interpret conditional cooperation as observed in the lab. We also discuss what these findings mean for exploiting conditional cooperation for fundraising in the field.

Keywords: conditional cooperation, reciprocity, conformity, inequality aversion, confusion, anchoring

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 of the existing explanations of this empirical regularity rely on 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), predictions of these theories within the linear public good 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 the others contribute, Fischbacher, Gächter and Fehr (2001) (henceforth FGF) document that a sizable group of subjects contribute more if the others on average contribute more as well. They call this empirical pattern "conditional cooperation" (henceforth CC). The authors classify about one half of their subjects as conditional cooperators (henceforth CCs), one third as free-riders (contributing zero regardless of the average contribution of the other group members), and the rest as fitting other (or no particular) patterns. These findings have later been replicated by numerous studies (Thöni and Volk 2018). Moreover, multiple studies in the lab and in the field document a positive correlation between contributions and historical contributions or beliefs about current contributions of others, suggesting presence of CC (Gächter 2007; Chaudhuri 2011).

Laboratory-observed CC is a very interesting observation with a potential application for designing fundraising campaigns for public goods or other social causes such as charities. It suggests that a designer can increase contributions by relying on would-be contributors' CC in combination with convincing them that others are contributing highly. However, we argue that without fully understanding social preference and other decision-making drivers of laboratory-documented CC, such suggestion might be premature.

The reason is that CC in laboratory could be driven by several preference and decision-making patterns such as reciprocity (to perceived intentions behind others' contributions), conformity (to others' contributions regardless of payoff consequences), aversion to payoff inequality (in comparison to others regardless of their intentions), and other residual factors. *Reciprocity* is a kind (unkind) response

[1] A leading alternative explanation applicable to observations from laboratory studies is experimental subject confusion (Andreoni 1995; Houser and Kurzban 2002).

to an action by others that is perceived to be driven by their kind (unkind) intention (Rabin 1993; Dufwenberg and Kirchsteiger 2004; Falk and Fischbacher 2006) or by their generous (ungenerous) type (Levine 1998; Rotemberg 2008; Gul and Pesendorfer 2016). *Conformity* is an act of following an observed behavior of others. It could 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* is a willingness to take action in order to reduce material payoff inequality between oneself and others irrespective of whether the inequality originates from intentions of the others or not (Fehr and Schmidt 1999; Bolton and Ockenfels 2000). *Residual factors* include any other alternative explanation of CC.

Regarding residual factors, we speculate that the most important ones include anchoring and subject confusion. *Anchoring* is an act of letting 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' strategy combinations map to their payoffs. The possibility that laboratory-observed CC is driven by confusion has been illustrated by Ferraro and Vossler (2010) and Burton-Chellew, El Mouden and West (2016). These two studies find that when subjects play the public good game against computers using the FGF design, with nobody else benefiting from their contributions, the classification into conditional contribution types results in a distribution remarkably similar to that of FGF and its replications. In particular, the share of CCs is approximately 50%. All this happens despite subjects having to answer control questions that are supposed to assure understanding of the instructions. Moreover, Burton-Chellew et al. (2016) document that CCs, as opposed to free-riders, are more likely to misunderstand the game.

Relative strength of the four potential drivers of laboratory-observed CC has important implications for how to exploit this observation for fundraising in the field. If CC is driven by reciprocity or inequality aversion, exploiting it for fundraising requires high unconditional contributions from a subgroup of early contributors in order to generate high contributions from the others.[2] If CC is driven by conformity, then exploiting it can additionally also rely on advertising high documented contributions from other (historical) peer groups that are not necessarily involved in the current campaign. If CC

[2]One channel through which such early contributions, or "seed money", can affect later would-be contributors is that it signals that the goal of the fundraising campaign is worthy (Vesterlund 2003; Andreoni 2006). By design, this channel in not present in laboratory-observed CC. We therefore omit it from our discussion.

is driven by residual factors, however, it is not clear what the message for fundraising in the field is. Anchoring would imply suggesting a higher level of contributions to would-be contributors. On the other hand, confusion is an artefact of experimental design as implemented in the lab, suggesting that the extent of CC observed in the lab is likely to be an over-estimate of the extent of CC expected to occur in the field.

The aim of our study is to disentangle the four potential drivers of laboratory-observed 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. In treatments 2 to 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 of CC since conditioning variable no longer reflects intentions of the other group members.[3] In treatment 4, the other group members' contributions are randomly generated by computer. On top of 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 the computer. On top of treatment 4, this treatment eliminates also inequality aversion and leaves only residual factors 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; that of inequality aversion by comparing treatments 4 and 5. Treatment 5 identifies the impact of residual factors.

We do not attempt to separate anchoring from confusion as it 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 one to argue that anchoring is not present.[5] However, it is hard to think of a reliable way to rule out anchoring by

[3]More specifically, this treatment eliminates *direct* reciprocity, but not necessarily generalized reciprocity. We discuss this point in more detail in Sections 3 and 7.

[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.

[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.

design *ex ante*.

We find a 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 conditional contribution schedules by treatment, we find that 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 the related literature. Section 3 outlines the experimental design. Section 4 reviews the utilized empirical methodology. Section 5 presents our results. Section 6 links the findings to the previous literature and discusses potential alternative explanations of the results in treatment 5. 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, $2/3$ are 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 utilized design. The issue is that *all* the members of the own group, even those who account for 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.[6] 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 that shares a similarity with FGF in terms of eliciting conditional contributions, but differs 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 that CC behavior is predominantly driven by anchoring and inequality aversion (by about the same amount), with reciprocity having a small and statistically marginal role.[7,8,9]

Our design attempts to overcome these shortcomings. First, we consider all four potential drivers of CC behavior in a single setting. Second, our design builds on the FGF design that uses a linear public good game and that is also used 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* unconditional contributions or randomly drawn numbers. We hence avoid information-cascade-like problems in interpreting various conditions.

Other authors have attempted to address similar questions using data from repeatedly-played public good games. Ashley, Ball and Eckel (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 game experiments with fixed-group matching and *ex post* observability of individual contributions in the previous period within own group only (baseline treatment) or 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

[6] See the comparison of average contributions in LH and HL in their Figure 1.

[7] See their regression-based analysis summarized in Result 3 and Table 4.

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

[9] As admitted by the authors themselves, 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 the 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, the results are sensitive to which sub-range one looks at.

alternative interpretations of the results based on dynamic strategizing and reputation-building.[10,11]

2.2 Confusion

As discussed in Section 1, subject confusion might play a significant role in explaining laboratory-observed CC. Burton-Chellew et al. (2016) list several reasons 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 the return to which depends on complementary "investment" of 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 assuring understanding, only one (question 3) illustrates the trade-off inherent in the social dilemma; (3) since 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 correlation (an experimenter demand effect). We use these suggestions 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 on a simulator (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 payoffs of all members of the group.[12]

More generally, instead of merely examining a potential presence of confusion in conditional contributions, our study integrates confusion into a fully-fledged CC decomposition exercise. Moreover, unlike Ferraro and Vossler (2010) and Burton-Chellew et al. (2016), which rely on subjects interacting with computerized players, all players in our design are humans. As a result, we avoid a criticism raised against the two studies that their findings are driven by subjects being uncertain about who, if

[10] There is also work on whether reciprocity or inequality aversion drives punishment in public good games. Dawes, Fowler, Johnson, McElreath and Smirnov (2007) and Johnson, Dawes, Fowler, McElreath and Smirnov (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, Fehr and Fischbacher (2005) conclude that punishment by cooperators is predominantly driven by reciprocity rather than inequality aversion.

[11] There is also a related literature that addresses the same research question in the domain of a common pool resource game. Velez, Stranlund and Murphy (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.

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

anyone, collects the payoffs.

3 Experimental Design and Identification Strategy

We build on the original design of FGF with some modifications. Subject play a linear public good game in groups of four. Each subject *i* independently decides how many of her 10 tokens (as opposed to 20 in FGF) to allocate into her private account $(10 - g_i)$ and how many to contribute (as opposed to "invest" in FGF) to a "group project" (g_i). Each subject receives a payoff from the public good equal to 0.75 (instead of 0.4 in FGF) times the sum of all the contributions to the group project. 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. The reason why we use the marginal per capita return of 0.75 instead of 0.4 is to secure a high share of CCs in order to increase statistical power of our decomposition exercise.

Subjects make contribution decisions in five different treatments, labeled to them as "scenarios," described in subsection 3.2. 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 group members.

3.1 Procedure

Each experimental session begins with one-page printed General Instructions (see Appendix B). Subjects are given information about the outline of the experiment, including the number of treatments and the fact that they will not be given any feedback on their or anyone else's decisions or earnings before a 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 groups of 4 subjects and that everyone will be paid based on the same *one* treatment (strategy method) randomly determined by a public draw at the end of the experiment. This is followed by another one-page printed instructions (see Appendix B) describing the public good 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 four treatments.

Subjects then get 3 minutes to interact with an on-screen simulator (see Figure C1 in Appendix C for a screenshot) using which they can simulate their earnings and the earnings of the other group members as a function of all four group members' contributions. Initial simulated values of the four contributions are randomly selected by computer in order to mitigate any potential anchoring bias. Subjects can add to or subtract from the individual contributions in the increments of 1 token. After each such incremental change, subjects can observe the change in everyone's payoffs. The design of the simulator aims to clarify to subjects what the marginal payoff consequences of their own contribution and of the other group members' contributions are. Afterwards, the experiment progresses to treatment 1 in which subjects decide on their unconditional contributions (see Figure C2 in Appendix C for the input screen).

After treatment 1 is finished, we distribute additional printed instructions that are common to treatments 2-5 (see Appendix B) that are labeled as "conditional treatments." They explain the principle of conditional contributions as follows. There are three "Type X" participants and one "Type Y" participant in each group. Types of all subjects are chosen by computer at the *end* of the experiment, with each participant in a group 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." In this table, subjects specify how much they wish to contribute conditionally on the rounded average of three numbers. Subjects are told that what these numbers are will be announced to them at the beginning of each treatment. The conditioning variable takes values from the set $\{0, 1, ..., 10\}$. The task in each treatment is to specify the conditional contribution for each possible value of the conditioning variable for the case 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 input into the contribution table becomes relevant for the group members' earnings. 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 treatment on the screen.

Subjects are then sequentially presented with treatments 2-5 in a scrambled order (see Appendix B for on-screen instructions) and make 11 conditional contribution decisions in each treatment (see Figures C3-C6 in Appendix C for the input screens in treatment 2-5). Subjects are never aware of the content of the upcoming treatments while making their decisions for the current treatment. The on-screen instructions and the input screens inform 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 graphical schemes illustrating how the contributions are determined in that particular treatment (see Appendix B).[13]

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.

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 a 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 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 subjects.

3.2 Treatments

In **treatment 1**, subjects simply decide how much to contribute unconditionally. This treatment is the first treatment presented to *all* subjects. In treatments 2 through 4, the conditioning variable is equal to the rounded average contribution of the three other 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 groupmates' contributions are equal to their unconditional contributions from treatment 1. In **treatment 3**, the groupmates' contributions are equal to unconditional contributions of three randomly chosen group *non*-members from treatment 1. In **treatment 4**, the groupmates' contributions are randomly and independently generated by computer from the uniform distribution on $\{0, 1, ..., 10\}$. This is the case also in **treatment 5**. However, in this treatment subjects condition on the average of another three randomly and independently drawn numbers from the uniform distribution on $\{0, 1, ..., 10\}$ that are independent from the groupmates' contributions.

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

3.3 Identification strategy

This design allows us to disentangle the impact of reciprocity, inequality aversion, conformity and residual factors on the conditional contribution behavior in treatment 2. Behavior in this treatment is potentially affected by all four drivers. To outline the argument, note that, *ceteris paribus*, each additional token contributed by members 2, 3 and 4 on average increases π_1 by 2.25 tokens and π_j, for $j \in \{2, 3, 4\}$, by 1.25 tokens. This has two implications. First, an additional token of $\bar{g}_{234}$ might be viewed by member 1 as a kind marginal act of her groupmates toward herself, triggering intention-based reciprocity.[14] Alternatively, it might be seen by member 1 as a marginal signal of the groupmates' generosity, triggering an increased generosity by member 1 herself within the context of interdependent-type reciprocity. In either case, the resulting reciprocity increases g_1. Second, member 1 might take a normative or an informational que from $\bar{g}_{234}$. If so, an additional token of $\bar{g}_{234}$ increases g_1 by conformity. Third, an additional token of $\bar{g}_{234}$ increases the payoff of member 1 relative to her groupmates by 1 token on average. If averse to payoff inequality, member 1 will counteract such increase by increasing g_1. Fourth, if member 1 is unsure about what conditional contributions to pick, $\bar{g}_{234}$ might serve as an anchor and hence g_1 will be positively correlated with $\bar{g}_{234}$.

Treatments 3, 4 and 5 eliminate reciprocity as a driver since contributions of the groupmates are not determined by themselves. As a result, the conditioning variable does not carry any information about groupmates' intentions or generosity types. Treatments 4 and 5 also eliminate conformity as a driver since the conditioning variable is computer-generated and hence does not carry information about any human decisions. Treatment 5 in addition eliminates inequality aversion as a driver since the conditioning variable does not carry information about anyone's contribution. The identification strategy is summarized in Table 1. Assuming additive separability among the impacts of the four drivers, the impact of reciprocity is identified by differencing conditional contributions between treatments 2 and 3; that of conformity by differencing between treatments 3 and 4; and that of inequality aversion by differencing between treatments 4 and 5. Treatment 5 identifies the impact of residual factors. This way, the conditional contribution behavior in treatment 2 can be decomposed into the four components corresponding to the four respective behavior drivers.

Some discussion is in order before proceeding. First, regarding a potential confound in treatment 3, although subjects cannot directly reciprocate to the subjects whose intentions lie behind the group-

[14] The kindness of this act seems intuitively obvious. To consider kindness of a higher groupmates' average contribution within formal definitions introduced in the literature, see Appendix A.

Table 1: Presence of behavior drivers in the four treatments

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

mates' contributions, they might "generally" reciprocate to other subjects. If so, behavior in treatment 3 might be driven by "generalized" reciprocity to some extent.[15] Distinguishing generalized reciprocity from conformity is difficult in lab conditions under anonymity and random assignment of subjects to groups or roles. Hence, to the extent it is present, we subsume generalized reciprocity under the "conformity" label.

Second, regarding another potential confound, to the extent that a higher $\bar{g}_{234}$ in treatment 2 might come hand-in-hand with a higher second-order belief of the conditional contributor about how much her groupmates expect their groupmates to contribute,[16] an increasing conditional contribution schedule could (partly) be driven by guilt aversion (Charness and Dufwenberg 2006; Battigalli and Dufwenberg 2007) instead of reciprocity. Some previous studies have tried to induce exogenous variation into second-order beliefs while keeping material payoffs constant and the results are inconclusive (Ellingsen, Johannesson, Tjøtta and Torsvik 2010; Al-Ubaydli and Lee 2012; Engler, Kerschbamer and Page 2018). Since we want to stay close to the FGF design blueprint, we do not elicit and manipulate beliefs and therefore have no way of distinguishing the two drivers. In our setting, they arguably work in the same direction, so we will subsume guilt aversion under the "reciprocity" label.

Third, we opt for a within-subject design as opposed to a between-subject design because of noise reduction. Previous studies point to a significant heterogeneity in conditional contribution behavior among subjects in the FGF design across many different populations.[17] Anticipating such heterogeneity also in our subject population, the within-subject design reduces the resulting noise in the estimates of the impact of the various behavior drivers. In order to mitigate impact of potential treatment order

[15] The usage of the terms "indirect" and "general" reciprocity is somewhat confused in the literature. We follow the terminology used by Herne, Lappalainen and Kestilä-Kekkonen (2013). According to this terminology, *direct* reciprocity refers to B reciprocating to A after having been a target of an action by A. *Indirect* reciprocity refers to B reciprocating to A after having observed A acting toward C. *Generalized* reciprocity refers to B reciprocating to C after B having been a target of an action by A.

[16] We label this belief $b(\bar{g}_{234})$ in Appendix A.

[17] See (Thöni and Volk 2018) for a list of references.

effects on our inference, we evenly balance all 24 possible orderings of the four conditional treatments in the sample.

3.4 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 emphasis 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 of 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. The gender ratio is almost exactly balanced.[18] One experimental token was worth 10 Czech koruna (CZK).[19] The mean and median cash payoff, including a CZK 75 show-up fee, was CZK 280[20] for approximately 1 hour of participation.[21]

4 Methodology for Data Analysis

We use two different methods to judge on what drives CC. The first method is based on the average conditional contributions given each value of the conditioning variable. This method estimates the slope of the average conditional contributions in the value of the conditioning variable in treatment 2 and decomposes this slope into analogous slopes due to the four constituent drivers. The second method classifies subjects into types according to the pattern of their conditional contributions in a given treatment. It then traces how the type distribution changes across different treatments and what

[18] There are 95 men and 96 women in the sample. We recruited men and women separately in order to achieve an approximately gender-balanced sample, but we did not insist on the particular proportion of each gender when subjects arrived to the lab.

[19] €1 was equal worth around CZK 25.8 and $1 was worth around CZK 22 at the time of the experiment.

[20] This was approximately €10.9 or $12.7 at the time of the experiment.

[21] 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 CZK 100 to CZK 120.

that reveals about the four constituent drivers of CC.

4.1 Slope decomposition

Formally, let i index subjects, $j \in \{2,3,4,5\}$ index the conditional treatments and $c \in \{0,1,...,10\}$ index the value of the conditioning variable. Let g_{ijc} be the conditional contribution of subject i in treatment j if the value of the conditioning variable is c. Then the extent to which average conditional contributions increase with c in the given treatment can be estimated by the slope coefficient in the regression

$$g_{ijc} = \alpha_j + \beta_j c + u_{ijc}. \tag{1}$$

With $\widehat{\beta}_2$ being the OLS estimate of β_2, the extent of CC can then be measured by $\widehat{\beta}_{CC} \equiv \widehat{\beta}_2$. Using the identification strategy presented in subsection 3.3, the extent of CC attributable to the four drivers can be estimated by $\widehat{\beta}_R \equiv \widehat{\beta}_2 - \widehat{\beta}_3$ for reciprocity, $\widehat{\beta}_C \equiv \widehat{\beta}_3 - \widehat{\beta}_4$ for conformity, $\widehat{\beta}_{IA} \equiv \widehat{\beta}_4 - \widehat{\beta}_5$ for inequality aversion and $\widehat{\beta}_{RF} \equiv \widehat{\beta}_5$ for residual factors. By construction, we then have that

$$\hat{\beta}_{CC} = \hat{\beta}_R + \hat{\beta}_C + \hat{\beta}_{IA} + \hat{\beta}_{RF}. \tag{2}$$

This equation describes the slope decomposition. When performing statistical tests, we cluster standard errors at subject level.

4.2 Subject type classification

The slope decomposition at the sample level that we described in the previous subsection can also in principle be done at the individual level. However, each such coefficient estimate is then based on only 11 conditional contributions of one subject in question. Given the small sample size and a lack of independence, no statistically meaningful conclusions can be drawn about such coefficients using conventional statistical methods.

In order to gain at least some insight into subject heterogeneity, we turn to the classification method of Thöni and Volk (2018), which is itself a slight modification of the method used by FGF. Given the power difficulty mentioned in the previous paragraph, instead of capturing the extent of CC quantitatively, this method focuses on qualitatively distinguishing various types of conditional contribution schedules. The method distinguishes five conditional contribution patterns. In particular, a subject is classified to be a: (1) *conditional cooperator* if g_{i2c} is weakly monotonically increasing in c without being flat in c, or the estimated Pearson correlation coefficient between g_{i2c} and c is at least 0.5; (2) *free-rider* if $g_{i2c} = 0$ for all c; (3) *unconditional cooperator* if $g_{i2c} = g > 0$ for all c; (4) "*triangle cooperator*" if there is a value $\bar{c} \in \{1, .., 9\}$ such that g_{i2c} is weakly monotonically increasing on $c \in \{0, .., \bar{c}\}$ and weakly monotonically decreasing on $c \in \{\bar{c}, .., 10\}$, without being flat in c in either of the two regions, or there is a value $\bar{c} \in \{2, .., 8\}$ such that the Pearson correlation coefficient between g_{i2c} and c is at least 0.5 for $c \in \{0, .., \bar{c}\}$ and at most -0.5 for $c \in \{\bar{c}, .., 10\}$; (5) *other* if subject i is not classified as any of the previous four types. Moreover, if it happens that subject i satisfies the conditions for being both a CC and a triangle cooperator, then the subject is classified as a CC if and only if $g_{i2\,10} > \frac{1}{11} \sum_{c=0}^{10} g_{i2c}$.

We extend this methodology from treatment 2 to all four conditional treatments. This way we can estimate the distribution of types in each treatment and examine how it shifts across treatments. In doing so, we pay special attention to how the share of CCs shifts across the four treatments.

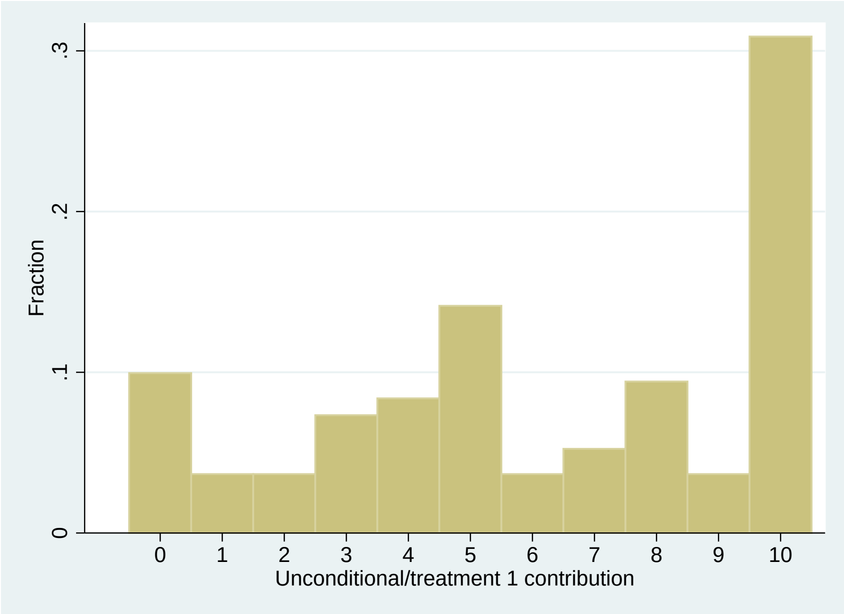
5 Results

5.1 Preliminary analysis

Figure 1 presents a histogram of unconditional (i.e., treatment 1) contributions. The mean (median) unconditional contribution is 6.13 (6) out of 10. This is at the upper boundary of the range typically found in the literature (Ledyard 1995). We attribute this finding to a MPCR of 0.75 that is also higher than what is usually found in the literature. A high MPCR makes contributing to the public good cheap and hence, for example, a given distribution of reciprocity or inequality aversion in the population leads to a higher level of unconditional contributions on average.

Figure 2 plots the average conditional contribution across all subjects by the value of the conditioning variable c and treatment (2 through 5). In treatment 2, we observe that that the pattern of conditional contributions is monotonically increasing with c, suggesting presence of CC. In particular, the average conditional contribution for $c = 10$ is by about 4.5 tokens larger than the average conditional contribution for $c = 0$. This suggests that the extent of CC is quantitatively sizeable on average at

Figure 1: Histogram of unconditional contributions

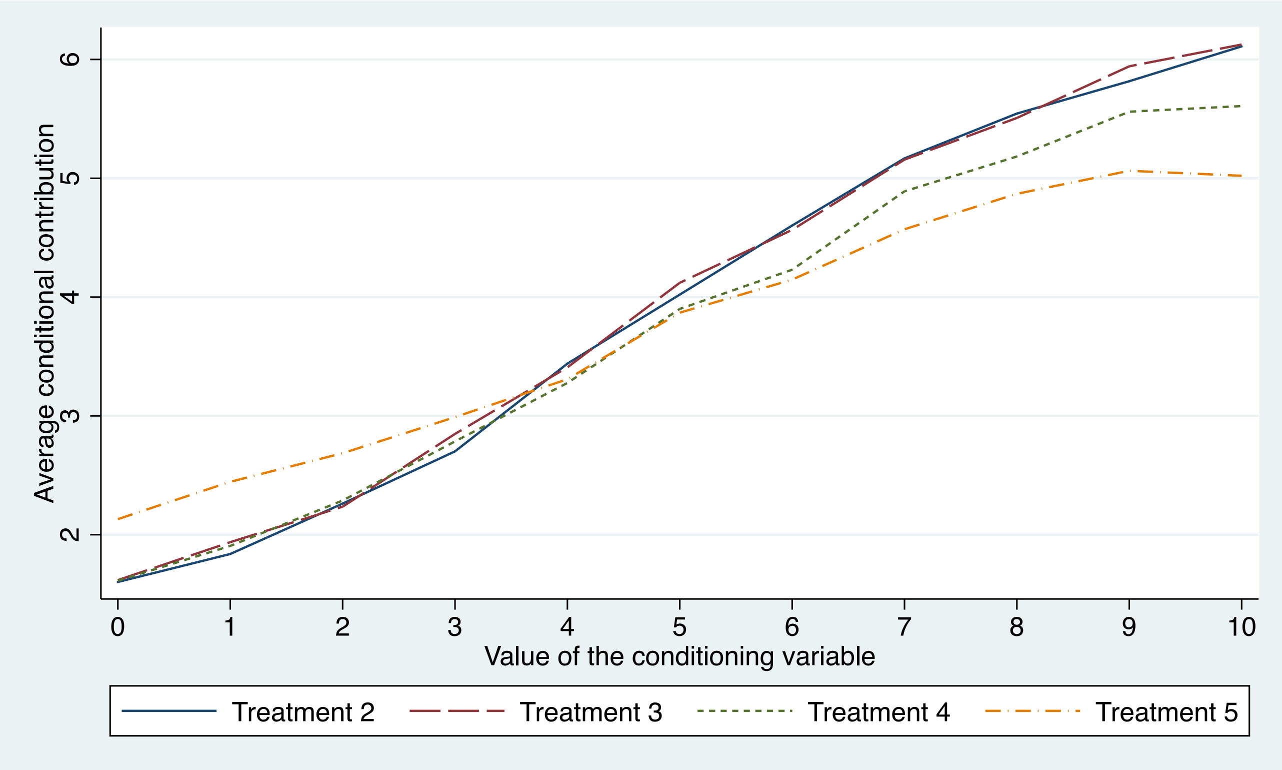


almost one-half-for-one. The pattern of the average conditional contributions in treatment 3 is almost identical, suggesting that reciprocity plays little role in explaining CC. The pattern of the average conditional contributions in treatment 4 is also monotonically increasing with c. It is almost identical to treatments 2 and 3 for up to $c = 3$, but it diverges from the previous two treatments downwards for higher values of c. At $c = 10$, the gap is about 0.5 tokens. This suggests that conformity does play a role in explaining CC, albeit not a quantitatively very large one. The pattern of the average conditional contributions in treatment 5 is also increasing in c, but the slope is smaller than in treatment 4. The difference between the average conditional contributions at $c = 0$ and $c = 10$ is about 3 tokens, as opposed to about 4 tokens in treatment 4. This suggests that inequality aversion plays an important role in explaining CC. Finally, somewhat unexpectedly, the pattern of average conditional contributions is (almost) monotonically increasing with c also in treatment 5. The difference between the average conditional contributions at $c = 10$ and $c = 0$ is almost 3 tokens, two thirds of the analogous difference in treatment 2. This suggests that not only are residual factors present as a driver of CC, but they actually account for a major part of it.

5.2 Slope decomposition

Results of the slope decomposition along the lines of equation (2) are presented in Table 2. In the left panel, columns "Intercept" and "Slope" report estimates of the intercept and the slope, respectively,

Figure 2: Average conditional contribution by value of the conditioning variable and treatment



of the average conditional contribution schedule by treatment (equation 1). The right panel presents how $\widehat{\beta}_{CC}$ decomposes into $\widehat{\beta}_R$, $\widehat{\beta}_C$, $\widehat{\beta}_{IA}$ and $\widehat{\beta}_{RF}$, both in absolute and in proportional terms.[22] In line with our preliminary observations in Figure 2, we find that the average conditional contribution in treatment 2 increases with c at the rate of approximately one half (precisely $\widehat{\beta}_{CC} = 0.495$). That is, we observe imperfect (slope less than 1) but sizeable (slope more than 0) CC. In treatment 3, $\widehat{\beta}_3$ is 0.492, almost as high as $\widehat{\beta}_2$. As shown in the right panel, the resulting difference of 0.002 (after rounding) accounts for only 0.5% of $\widehat{\beta}_{CC}$ and is not statistically significant (t-test $p = 0.924$). That is, reciprocity plays little role in explaining CC. In treatment 4, $\widehat{\beta}_4$ is 0.44, which is by 0.052 less than $\widehat{\beta}_3$ ($p = 0.082$). Translated into proportional terms, this means that conformity accounts for about one tenth of $\widehat{\beta}_{CC}$. In treatment 5, where only residual factors play a role, the slope estimate $\widehat{\beta}_5$ drops to 0.322, which is by 0.118 less than $\widehat{\beta}_4$ ($p = 0.001$). In proportional terms, this implies that inequality aversion accounts for almost one quarter of $\widehat{\beta}_{CC}$. Finally, $\widehat{\beta}_5$ is equal to 0.322, statistically significantly different from 0 ($p < 0.001$). In proportional terms, residual factors account for almost two thirds $\widehat{\beta}_{CC}$.

[22] We define the proportional impact of reciprocity, conformity, inequality aversion and anchoring by $\widehat{\beta}_R/\widehat{\beta}_{CC}$, $\widehat{\beta}_C/\widehat{\beta}_{CC}$, $\widehat{\beta}_{IA}/\widehat{\beta}_{CC}$ and $\widehat{\beta}_{RF}/\widehat{\beta}_{CC}$, respectively. We obtain standard errors by the Delta method.

Table 2: Regression-based estimates of CC

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 in two-tailed tests at: * 10%, ** 5 %, *** 1%.

To summarize the slope decomposition, the main driver of CC are the residual factors, accounting for approximately two thirds of CC. The second most important driver is inequality aversion, accounting for about a quarter of CC. Conformity accounts for approximately a tenth of CC, with the evidence for its presence being mildly statistically significant. Reciprocity plays virtually no role in driving CC.

5.3 Subject type classification

Table 3 displays distribution of the conditional contribution type by treatment based on the method of Thöni and Volk (2018) (see subsection 4.2).[23] In treatment 2, which corresponds to the setting considered in the previous literature, we classify 57.6% of subjects as conditional cooperators, 12.6% of subjects as triangle cooperators, 12.0% of subjects as free-riders, 9.4% of subjects as unconditional cooperators and 8.4% of subjects as having the "other" type. Regarding the incidence of CC and triangle cooperation, our results are consistent with the range of type distributions estimated in many

[23]We implement the classification using the STATA routine *cctype* supplied as a companion to Thöni and Volk (2018).

Table 3: Conditional contributor type classification by treatment (% 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

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 a 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 fits our objective of increasing the power of the CC decomposition analysis.

Looking beyond treatment 2, we observe that the type distribution in treatment 3 is almost identical to that in treatment 2, suggesting that reciprocity plays little role on average in driving conditional contribution behavior in treatment 2. This is confirmed by formal tests. Neither the type distribution (Stuart-Maxwell test $p = 0.546$), nor being classified as a CC (paired sign test $p = 0.690$)[24] is statistically significantly different across the two treatments. Moving on to treatment 4, there are some mild differences in the type distribution vis-à-vis treatment 3, such as a drop in the fraction of CCs from 56% to 52.9%. The difference in the type distribution is marginally statistically significant (Stuart-Maxwell test $p = 0.053$), but the difference in the fraction of CCs is not (paired sign test $p = 0.392$). This suggests that conformity plays at most a minor role in driving conditional contribution behavior in treatment 2. Moving on to treatment 5, there are relatively large differences in the type distribution vis-à-vis treatment 4. For example, there is a drop in the fraction of CCs from 52.9% to 40.8%. Both this difference (paired sign test $p = 0.003$) and the difference in the type distribution (Stuart-Maxwell test $p = 0.001$) are now statistically significant. This suggests that inequality aversion plays an important role in driving conditional contribution behavior in treatment 2. Again, the most unexpected finding in Table 3 is that 40.8% of subjects in treatment 5 behave as CCs, suggesting a large role of residual factors in driving conditional contribution behavior in treatment 2. Indeed, the paired sign test rejects the hypothesis that this fraction is zero ($p < 0.001$). In quantitative terms, residual factors seem to be the main driver of CC in treatment 2, with inequality aversion playing a secondary role, conformity playing a minor role and reciprocity playing virtually no role. These observations mirror

[24]In the current application with a binary outcome variable, the paired sign test is equivalent to the McNemar test.

our earlier observations drawn from Figure 2 and Table 2.

5.4 Conditioning on conditional cooperators

An inviting idea is to apply either of our two methodologies only on those subjects who are classified as CCs in treatment 2 according to the classification from subsection 4.2. After all, we are interested in knowing what drives CC. We report results of such exercise in this subsection. However, one needs to be cautious when interpreting these results because they are based on an endogenously selected sample. We expect that such selection tends to overstate the role played by reciprocity. To illustrate the point, consider an example in which the true expected conditional contribution schedule is completely flat in each treatment. However, due to noise, a fraction $p \in (0, 1)$ of subjects, chosen randomly and independently in each treatment, submits a conditional contribution schedule that has a slope $s > 0$. These subjects are then classified as CCs in the given treatment. The other subjects report flat conditional contribution schedules and are not classified as CCs. If using the full sample for either of the two analyses, we would in expectation correctly conclude that there is some CC, but that it is fully driven by residual factors, while reciprocity, conformity and inequality aversion play no role. However, when conditioning on those classified as CCs in treatment 2, we would in expectation incorrectly conclude that CC is partly attributable to reciprocity and partly to residual factors. Even though this example is very stylized, it gives a flavor of the direction of the potential bias.

Applying the slope decomposition analysis only on the subjects classified as CCs in treatment 2, we find that reciprocity accounts for 8.3% of CC (t-test $p = 0.012$), conformity accounts for 9.7% ($p = 0.025$), inequality aversion for 24% ($p < 0.001$) and residual factors for 58% ($p < 0.001$). The relative effects of conformity and inequality aversion are very similar to the ones based on the full sample. The relative effect of reciprocity is higher here, and it comes at the expense of a smaller relative effect due to residual factors. Hence, overall, even if ignoring the potential sample selection bias, the results of the slope decomposition do not become dramatically different compared to the full sample. The most important driver of CC are residual factors, accounting for at least 58%, followed by inequality aversion (quarter), conformity (tenth) and reciprocity (twelfth). The increased role of reciprocity relative to the full-sample results might be driven by the sample selection bias, though.

When performing the type classification analysis on the subjects classified as CCs in treatment 2, we find that the share of CCs drops from 100% in treatment 2 to 87.3% in treatment 3 (paired sign test $p < 0.001$), 78.2% in treatment 4 ($p = 0.041$ relative to treatment 3) and 60.0% in treatment 5 ($p = 0.001$ relative to treatment 4, $p < 0.001$ relative to 0). Because we condition on being a CC in treatment 2, we can interpret the results directly as relative shares of CC driven by the respective groups of drivers. In particular, residual factors account for 60% of CC, residual factors and inequality aversion combined account for 78.2% and residual factors, inequality aversion and conformity combined account for 87.3%. Even if ignoring the potential sample selection bias, in qualitative terms, the results are broadly consistent to the results drawn from the full sample. Residual factors are the main driver of CC, with inequality aversion playing a secondary role, while conformity and reciprocity play only minor roles. In quantitative terms, reciprocity now plays a larger role, whereas the relative roles of the other three drivers are approximately unchanged. Again, this difference might be driven by the sample selection bias, though.

6 Discussion

6.1 Relation to findings in the previous literature

Our results document that there is a lot of CC in treatment 5 even though the conditioning variable is meaningless. As a reminder, the average conditional contribution in treatment 5 has a slope of approximately $1/3$ in the conditioning variable. Also, 41% of subjects in this treatment are classified as CCs. As outlined in section 1, such CC-like behavior can only be attributed to residual factors such as anchoring and confusion. In this respect, our result to some extent mirror the findings of Ferraro and Vossler (2010) and Burton-Chellew et al. (2016). However, while the implicit message of Burton-Chellew et al. (2016) is that *all* of CC can be accounted for by confusion and is hence an artefact of experimental design, we find that this is not the case. Our results suggest that about one third of CC is indeed driven by inequality aversion and conformity.

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, their "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 slope decomposition analysis suggests that residual factors (that include anchoring) play a much bigger 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 also interesting to contrast our results with findings from field experiments on fundraising. Alpizar, Carlsson and Johansson-Stenman (2008) investigate to what extent donations to a national park are driven by conformity, reciprocity and anonymity. Similarly to us, they find that conformity (to a pretended modal contribution in the past) does have an effect, albeit not a large one, whereas reciprocity (to a small gift) has a very small effect. This is in contrast to Falk (2007) who finds that reciprocity (to a gift) has a large effect. Whatever the effect of reciprocity to a gift might be in the field, this gift-exchange setting is different from the setting studied by us in at least three important aspects. First, it involves reciprocity between a potential donor on one side and the recipient or the fundraiser on the other side. Donors do not materially benefit from their contributions. Second, each gift is exclusively targeted toward a specific potential donor who is the only one who can reciprocate it. Third, if the gift is not followed by a (sufficiently) generous response by the potential donor, the recipient/fundraiser is left worse off. This might trigger guilt aversion and give a strong incentive to return the favor. Our setting is different. First, potential contributors are also beneficiaries of everyone's contributions. Hence the roles of gift-givers and gift-reciprocators are not sharply defined. Second, contributions cannot be targeted toward specific individuals. Hence players might free-ride on expected reciprocity by other players. Third, there is a specific information structure. Kindness of the other group members is communicated through their higher *average* contribution. But, given the parameterization of the game, whenever the other group members are kinder, they are also better off even if the conditional contributor does not reciprocate. This might mitigate feelings of guilt aversion and hence reduce the incentive to return the favor. In general, this is the case whenever $(n-1) \times \text{MPCR} > 1$. Among FGF and its 19 replications considered by Thöni and Volk (2018), this condition is satisfied in 19 studies, including FGF.[25]

[25]In the remaining study, $(n-1) \times \text{MPCR} = 1$.

6.2 Potential alternative explanations for behavior in treatment 5

There are several possible concerns that the strong effect of residual factors that we associate with confusion and anchoring is an artefact of *our* experimental design. First, confusion might be driven by the within-subject design. In particular, if subjects see treatment 5 after some, or all, of the other three conditional treatments, they might fail to notice or appreciate the difference in the conditioning variable. As a result, they might erroneously believe that they are conditioning on informative contingencies, and hence respond by an increasing conditional contribution schedule due to reciprocity, conformity or inequality aversion. Even though such confusion is possible in principle, we argue that it does not account for the increasing pattern of conditional contributions in treatment 5. In particular, those subjects who see treatment 5 as the first conditional treatment cannot be affected by this type of confusion. As a result, they should not exhibit an increasing pattern of conditional contributions in treatment 5. To the contrary, we observe that the slope of the average conditional contribution schedule is 0.473 (with the standard error of 0.071) and the share of subjects classified as CCs is 47.9%, statistically significantly different from 0 (paired sign test $p < 0.001$). Moreover, these figures are higher than the corresponding full-sample figures in Tables 2 and 3 (with the difference not being statistically significant). The strong effect of residual factors is therefore does not appear to be an artefact of the within-subject design.

Second, anchoring might be driven by an experimenter demand effect. Subjects might wonder what the experimenter expects of them in treatment 5 and conclude that it is an increasing conditional contribution schedule. If equally present in all conditional contribution treatments, such effect is a valid part of residual factors. There is an identification problem only if the experimenter demand effect is stronger in treatment 5 than in treatments 2 to 4. If so, we would expect the effect to be the strongest among subjects who see treatment 5 before treatments 2 to 4. On the other hand, we would expect such effect to be smaller if treatment 5 comes after some or all three other conditional treatments. This is because subjects are then also likely to wonder how the experimenter wants them to *change* their contributions relative to the previous conditional treatment(s). Since the conditioning variable is no longer relevant, subjects might conclude that contributions should be less responsive 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. 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. Also, we cannot reject the null hypothesis that these shares are identical across all four groups (Pearson's χ^2 test $p = 0.124$). Hence the available evidence is not supportive of the experimenter demand effect.

Third, unlike FGF and many other previous studies of CC, we do not use control questions before the experiment. As mentioned before, this decision choice is motivated by both Ferraro and Vossler (2010) and Burton-Chellew et al. (2016) finding control questions to be ineffective in rooting out confusion. Instead, we use a payoff simulator to reduce confusion. Within the time window of 3 minutes, our subjects make on average, and in the median, 91 clicks (about one click each two seconds) in the simulator. Each click comprises a one-token change in own or another group member's contribution and if followed by an immediate display of changed payoffs. Such intensive use of the simulator suggests that it is a useful tool in aiding subject comprehension. Although we cannot assure that the simulator results in less confusion than the control questions do, our results in treatment 2 suggest that this is indeed so. In particular, as mention before, Burton-Chellew et al. (2016) document a positive correlation between being a CC and misunderstanding the game. This suggests that if the simulator is less effective in eliminating confusion than the control questions are, we would expect to see a higher fraction of CCs in treatment 2 than in FGF and its replications that do use control questions. This is, however, plainly not the case. Among FGF and its 19 replications considered by Thöni and Volk (2018), 9 find a higher share of CCs than we do, while the other 11 a lower one. Moreover, this happens despite our MPCR being at the upper boundary of the range used in these studies, which should push the share of CCs up on its own.

7 Conclusion

We use a laboratory experiment to decompose CC, as identified by FGF and its replications, into parts driven by reciprocity, conformity, inequality aversion and residual factors. We associate residual factors mostly with subject confusion and anchoring. Using the methodology proposed by Thöni and Volk (2018), which is a slight modification of the methodology used by FGF, we find that 40.8% of subjects are categorized as CCs even in the treatment where only residual factors play a role. This is about $7/10$ of the share 57.6% of subjects who are classified as CCs in the "baseline" treatment in which all four drivers potentially play a role and that has been considered by FGF and the follow-up literature. Inequality aversion is found to play an important role too. When it is added to residual factors as a potential driver, the share of subjects categorized as CCs increases by 12.1 percentage points, or one fifth of the share of CCs in the baseline treatment, from 40.8% to 52.9%. Adding conformity increases this share by another 3.1 percentage points, or one twentieth of the share of CCs in the baseline treatment, from 52.9% to 56%. This effect is only marginally statistically significant, though. Finally, adding reciprocity as a potential driver plays a minimal role, increasing the share of CCs only by 1.6 percentage points, or $1/36$ of the share of CCs in the baseline treatment, from 56% to 57.6%.

We also use an alternative method that does not depend on an arbitrary threshold used to judge whether an individual subjects is a CC or not. In particular, we examine how the slope of the average linear conditional contribution schedule estimated by OLS changes as we vary the presence of the potential drivers. We find that the slope is 0.322 even in the treatment where only residual factors play a role. This is about two thirds of the slope of 0.495 in the baseline treatment. When inequality aversion is added to residual factors as a potential driver, the slope increases by 0.118, or one quarter of the slope in the baseline treatment, from 0.322 to 0.44. Adding conformity to the list, the slope increases by further 0.052, or one tenth of the slope in the baseline treatment, from 0.44 to 0.492. Again, this effect is only marginally statistically significant. Finally, adding reciprocity as a potential driver leaves the slope almost unchanged, increasing it by only 0.002, or half a percent of the slope in the baseline treatment, from 0.492 to 0.495.[26]

Both sets of results paint a unified picture. Under the assumption of additive separability of the effects of the four drivers, about 65% to 70% of CC is accounted for by residual factors, further 20% to 25% by inequality aversion, 5% to 10% by conformity, with reciprocity accounting for at most a few percent. Even if we drop the assumption of additive separability, we can conclude that residual factors, which we associate with confusion or anchoring, appear to be a major force behind CC as observed in the lab. But, unlike some previous literature suggesting that confusion is the sole driver of CC in the lab (Burton-Chellew et al. 2016), our results suggest that this is not the case. Inequality aversion and conformity appear to play a significant role too.

In terms of potential confounds, the potential effect of reciprocity is, in our experiment, observationally equivalent to the potential effect of guilt aversion. This confound is not, however, important in the interpretation of the results *ex post* since we find little effect of reciprocity. Also, the potential effect of conformity is observationally equivalent to the potential effect of generalized reciprocity. As a result, the 5-to-10% share of CC that we have so far attributed to conformity might also (partly) be

[26]These results do not add up at the third decimal place due to rounding.

driven by generalized reciprocity.

Our results have implications for exploiting CC, as observed in the lab, for fundraising in the field. The main message is that one should be less optimistic about the strength of CC in the field relative to what is suggested by laboratory studies. This is because a major part of CC, as observed in the lab, appears to be driven by subject confusion and anchoring. These are, for the most part, artefacts of experimental design that are unlikely to apply in the field. However, anchoring might be present in the field and can be exploited by suggesting high(er) contributions to would-be contributors. Indeed, casual observation suggests that many charities exploit such strategy.[27] The second message is that inequality aversion and, at least to some extent, conformity or generalized reciprocity appear to drive CC as well. Therefore appealing to a fair allocation of the contribution burden and model role of previous contributors might be an effective fundraising strategy. On the other hand, reciprocity appears to not have much impact on CC, suggesting that appealing to kindness of other contributors might not be very effective. It is important to stress, however, that these suggestions only apply to larger-scale interactions in which contributors are also beneficiaries, contributions cannot be targeted to specific individuals and one's knowledge of others' generosity implies that the others mutually benefit from their generosity even if one does not reciprocate.

[27] There are also several field experiments that, for the most part, confirm fundraising effectiveness of suitably suggested contributions (Charness and Cheung 2013; Edwards and List 2014).

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Appendices

A Kindness of a higher average contribution of the groupmates

Within the context of existing theories of intention-based reciprocity, intentions are modelled via second-order beliefs. Formally, let b be the (mean of the) second-order belief of member 1 about how much the groupmates (on average) expect him to contribute. In general, b might (and we would speculate that is likely to) depend on $\bar{g}_{234}$. Therefore in what follows, we will refer to this second-order belief as $b(\bar{g}_{234})$. Under such belief and conditional on $\bar{g}_{234}$, member 1 expects his groupmates to expect that his payoff is $\pi_1^e(\bar{g}_{234}) \equiv 10 - 0.25b(\bar{g}_{234}) + 2.25\bar{g}_{234}$. We would expect that the slope of $b(\cdot)$ is less than 9, implying that $\pi_1^e(\cdot)$ is strictly increasing. Hence, member 1 expects that the expectation of his payoff by the groupmates is increasing with $\bar{g}_{234}$, making a higher value of $\bar{g}_{234}$ to be more kind. This reasoning is also borne out by using the literal definition of kindness from Rabin (1993) and Dufwenberg and Kirchsteiger (2004). Given whatever second-order belief b, member 1 expects that his groupmates expect his payoff to range from $10 - 0.25b$ at the low end to $10 - 0.25b + 22.5$ at the high end, depending on how much they contribute. Kindness of a particular value of $\bar{g}_{234}$ is then given by the relative location of 1's payoff in the range of possible payoffs. This measure is given by

$$\frac{(10 - 0.25b + 2.25\bar{g}_{234}) - (10 - 0.25b)}{(32.5 - 0.25b) - (10 - 0.25b)} = 0.1\bar{g}_{234}.$$

That is, a higher value of $\bar{g}_{234}$ is perceived to be kinder. Falk and Fischbacher (2006), on the other hand, define kindness by difference in the payoff of member 1 and (adjusted to the present application) the average payoff of the groupmates given $b(\cdot)$ and $\bar{g}_{234}$. This measure is given by

$$[10 - 0.25b(\bar{g}_{234}) + 2.25\bar{g}_{234}] - [10 + 0.75b(\bar{g}_{234}) + 1.25\bar{g}_{234}] = \bar{g}_{234} - b(\bar{g}_{234}).$$

According to this definition, a higher value of $\bar{g}_{234}$ is perceived to be kinder if and only if the slope of $b(\cdot)$ is less than 1. We would speculate that, at least for most subjects, $b(\cdot)$ is either an identity (expecting that the groupmates expect exactly matching contributions), or its slope is less than 1 (expecting that the groupmates expect some selfish bias away from exactly matching contributions). As a result, at least in a weak sense, we speculate that a higher value of $\bar{g}_{234}$ is perceived to be kinder under this approach as well.

B Instructions

In this appendix, we present the following:

1. General instructions (printed, one page)
2. Game description and instructions for Treatment 1 (printed, one page)
3. Common instructions for treatments 2-5 (printed, two pages)
4. Treatment-specific instructions for treatments 2-5 (on-screen, 1 page per treatment)

GENERAL INSTRUCTIONS (one page)

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**.

GAME DESCRIPTION AND INSTRUCTIONS FOR TREATMENT 1
(one page)

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:

Earnings from the group project *= 0.75 × 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.

COMMON INSTRUCTIONS FOR TREATMENTS 2-5 (two pages)

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

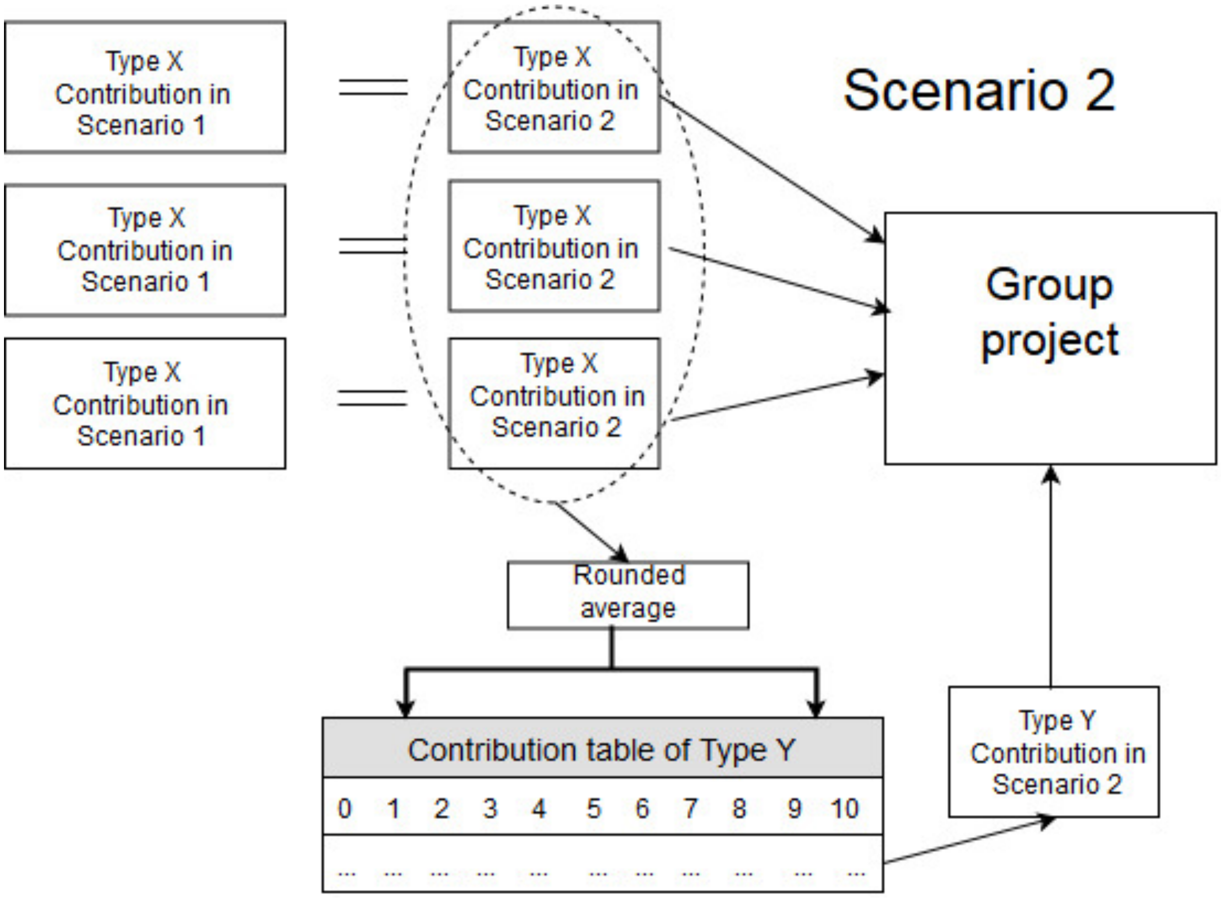
SCENARIO-SPECIFIC INSTRUCTIONS

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.

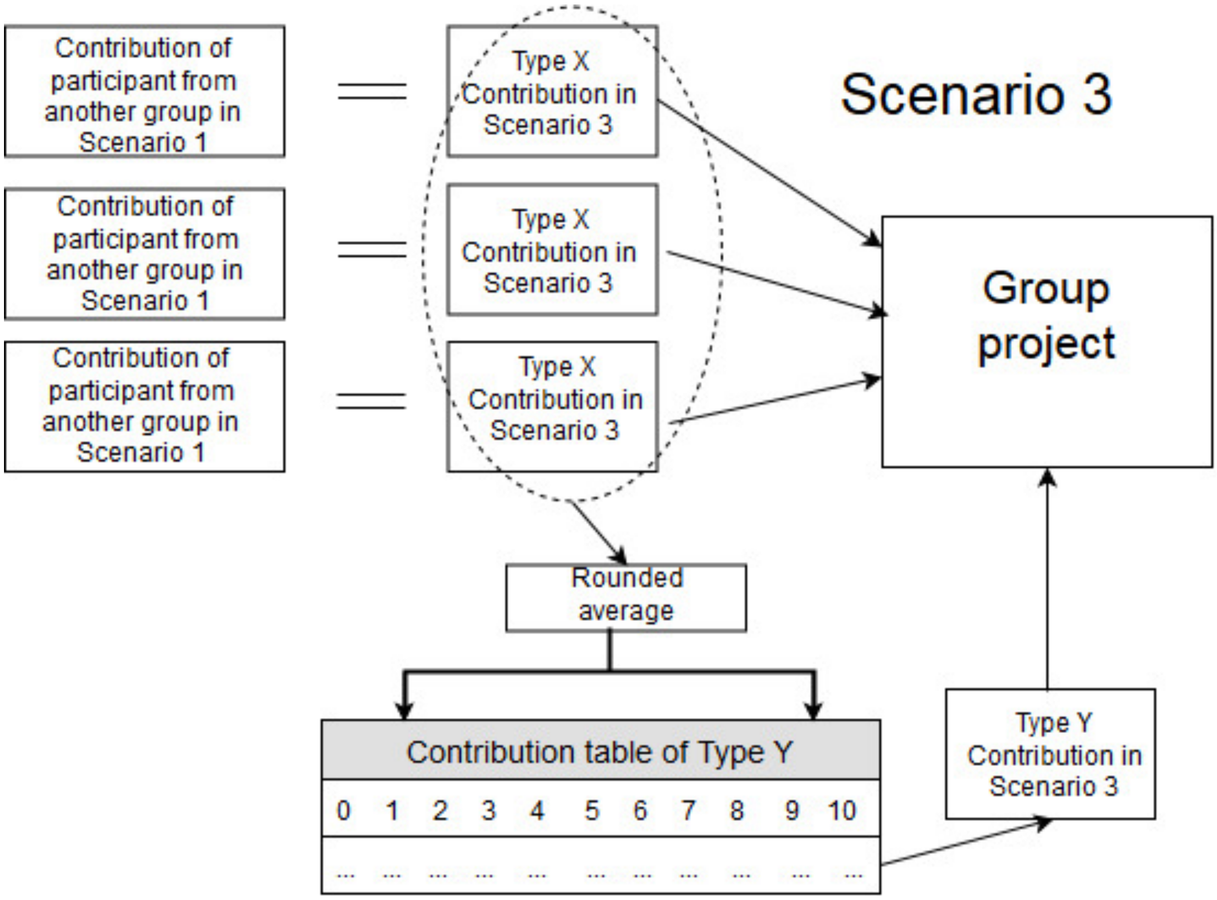
Type Y contribution to the group project: Based on the rounded average of the Type X contributions and the Contribution table.



Scenario 3

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

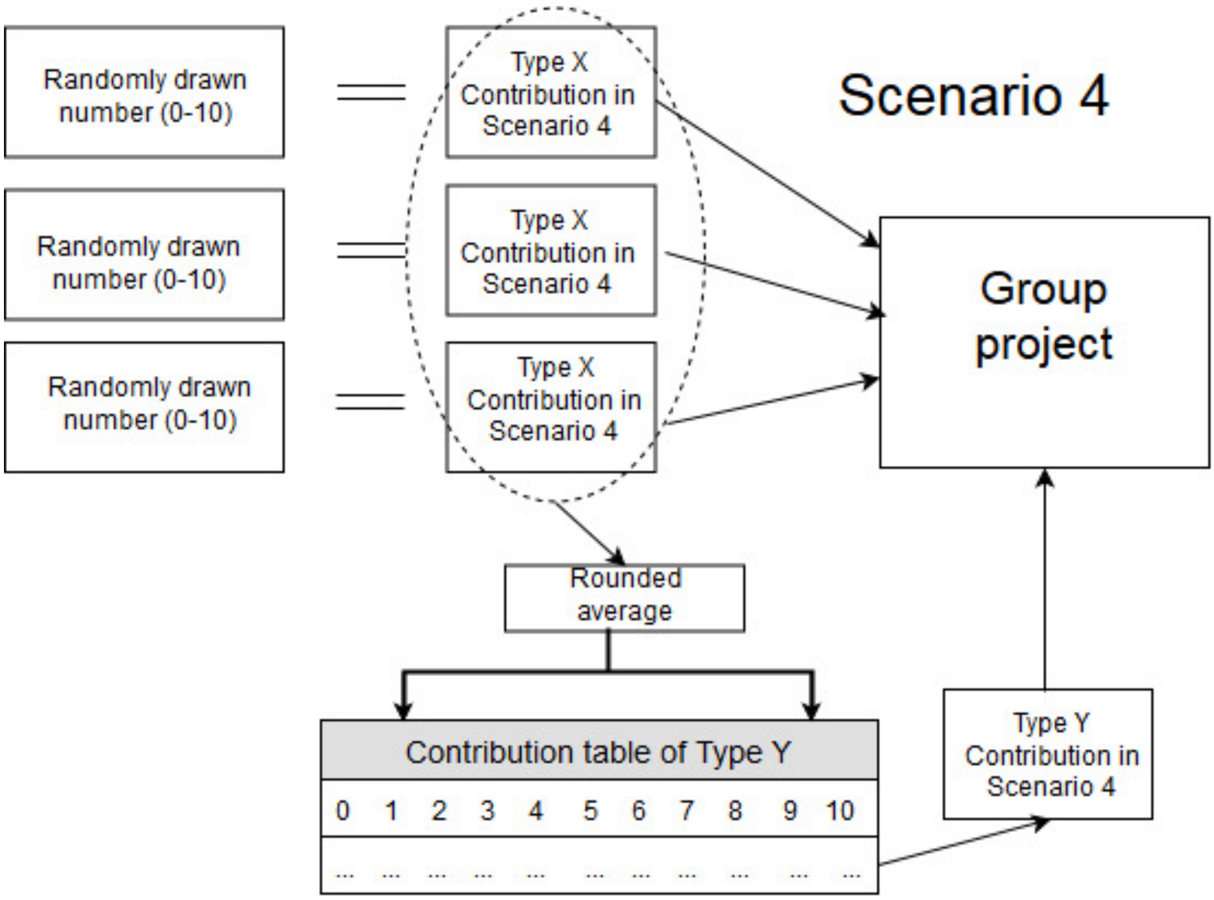
Type Y contribution to the group project: Based on the rounded average of the Type X contributions and the Contribution table.



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.

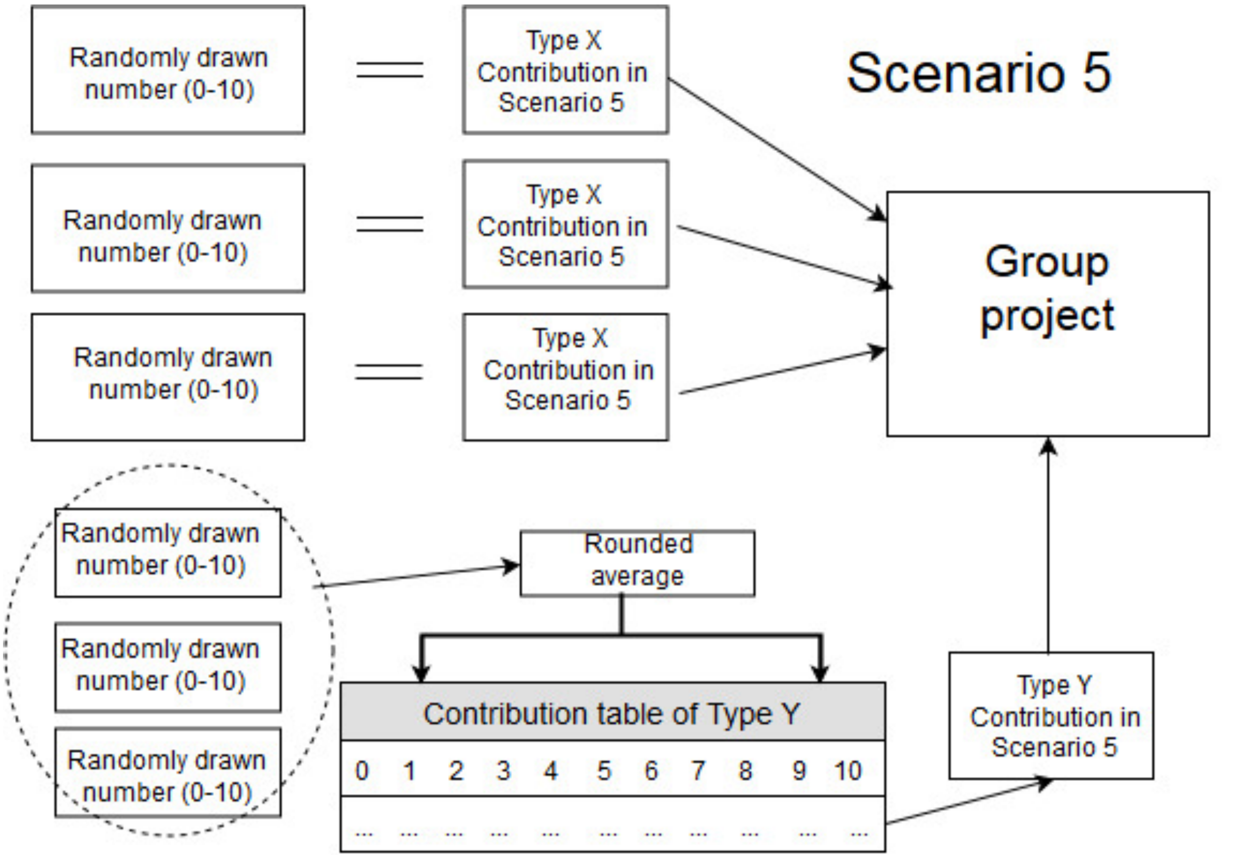
Type Y contribution to the group project: Based on the rounded average of the Type X contributions and the Contribution table.



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.



C Screenshots

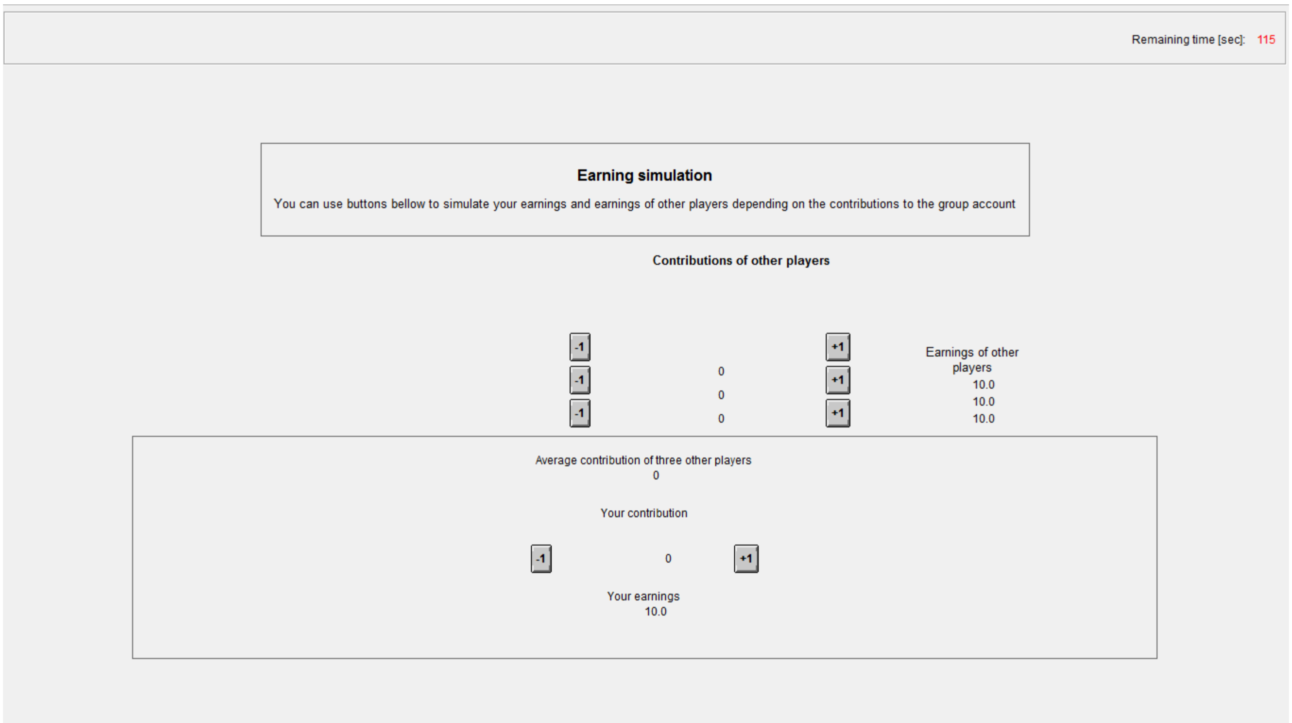


Figure C1: Screenshot of the payoff simulator

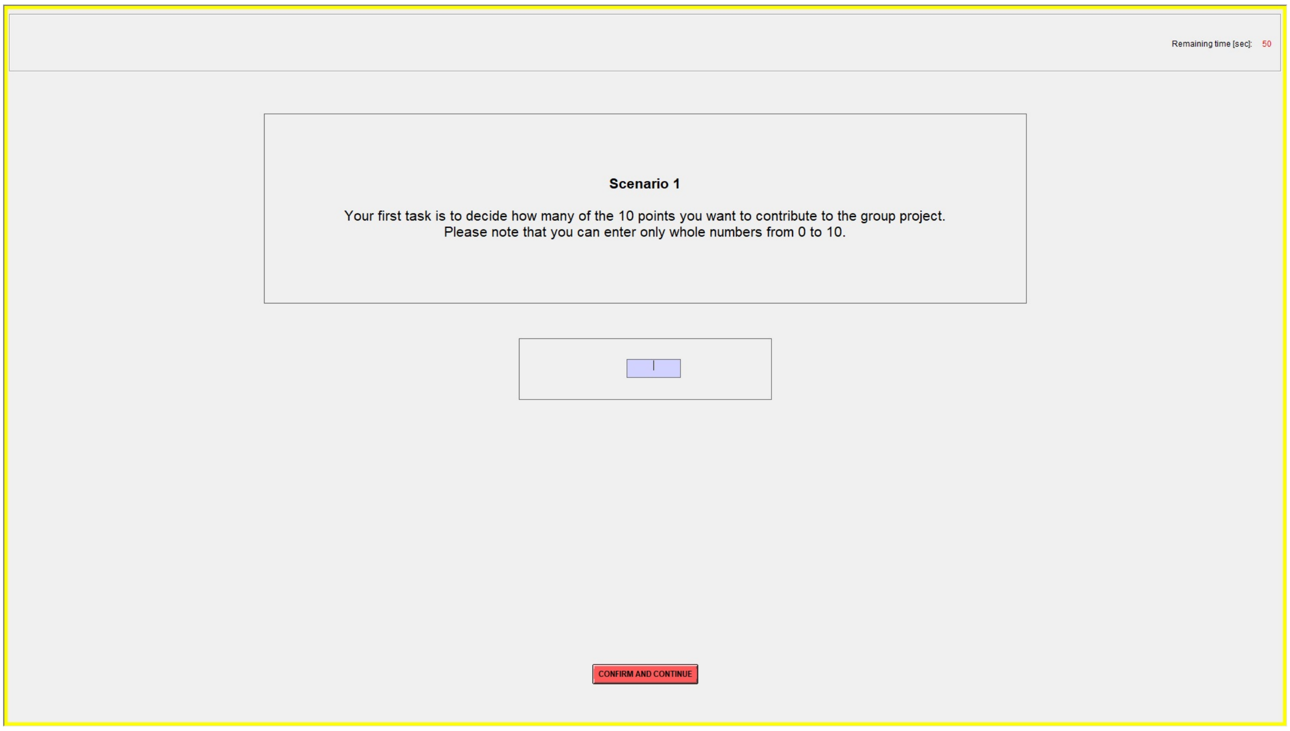


Figure C2: Screenshot of the treatment 1 (unconditional contribution) input screen

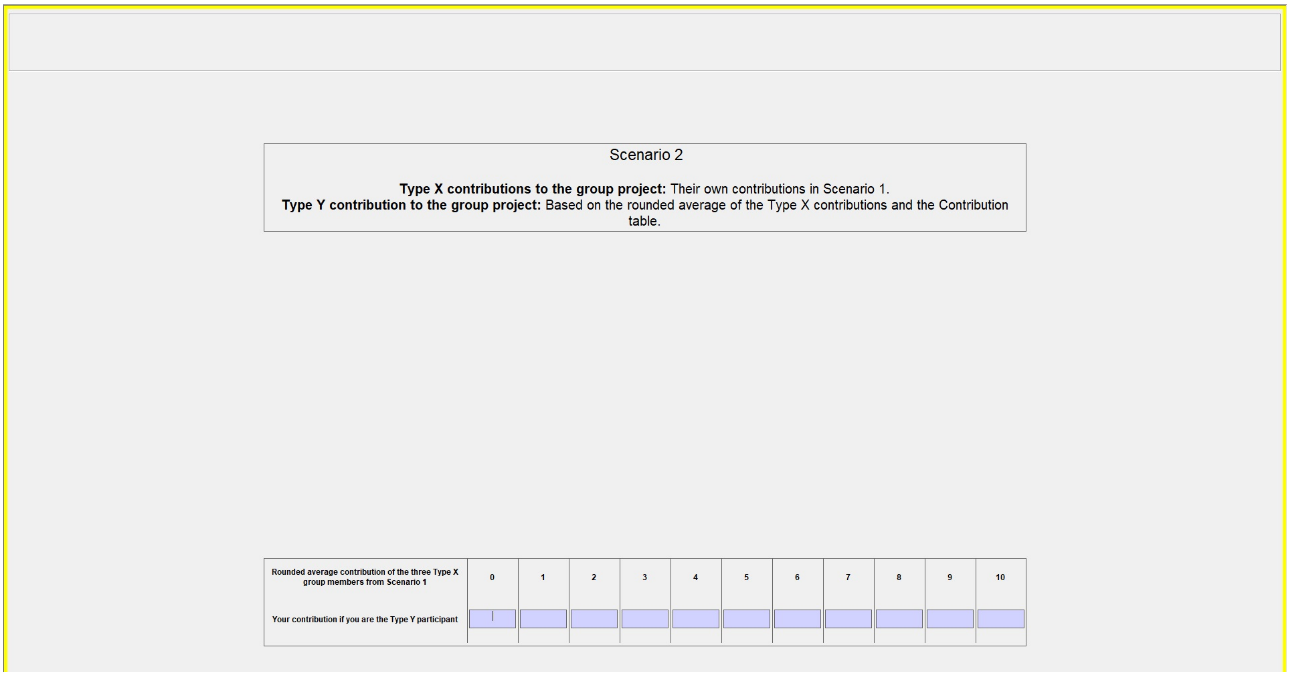


Figure C3: Screenshot of the treatment 2 input screen

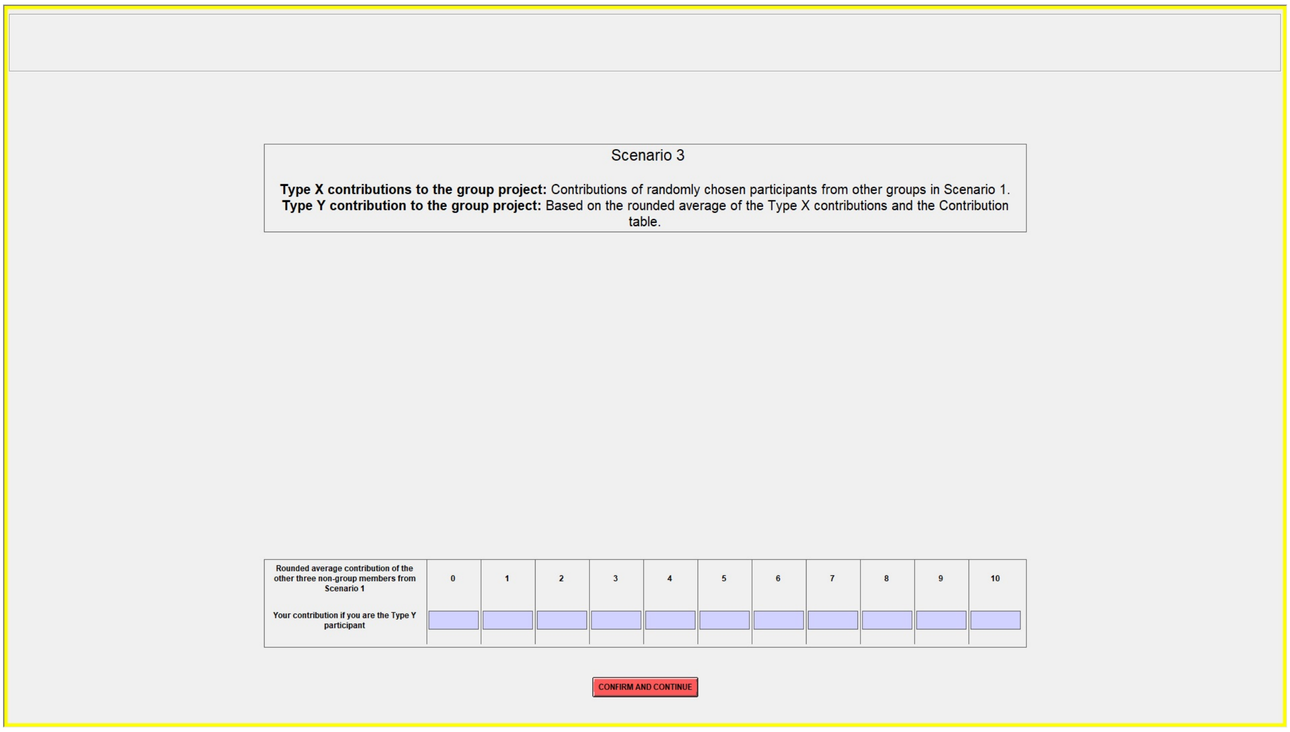


Figure C4: Screenshot of the treatment 3 input screen

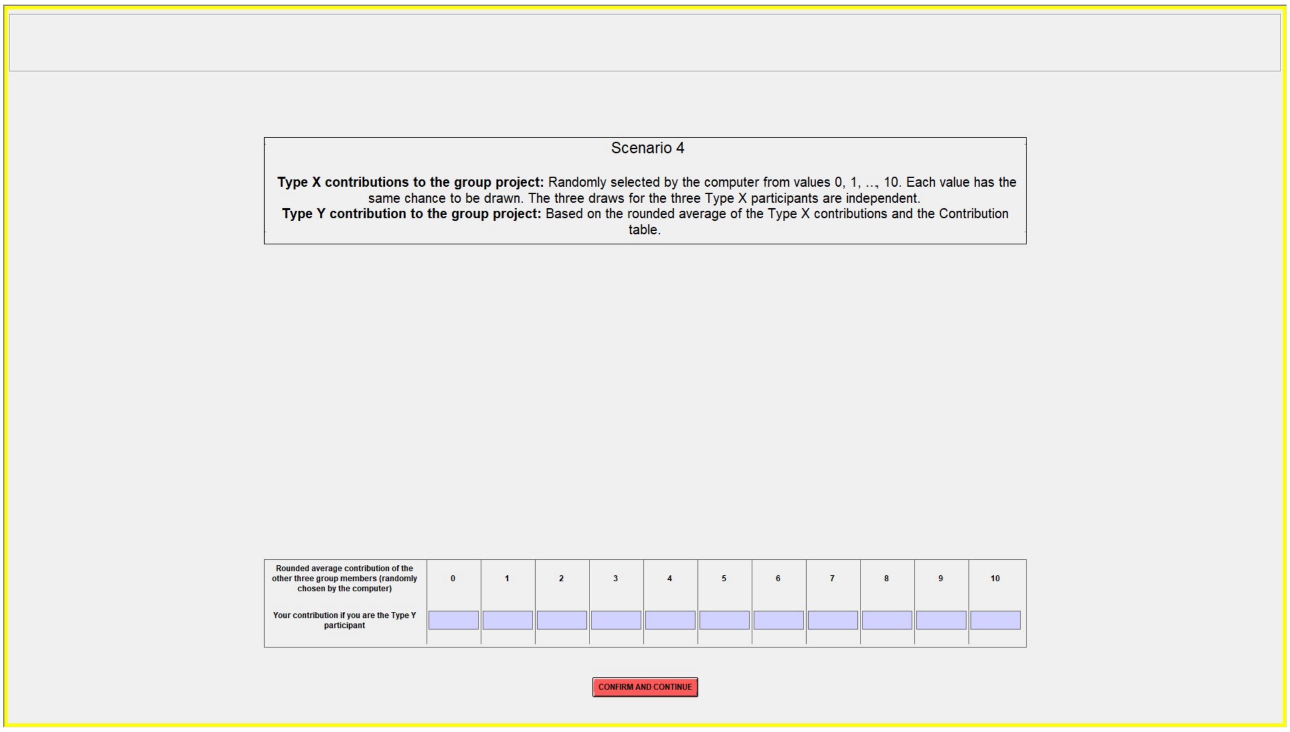


Figure C5: Screenshot of the treatment 4 input screen

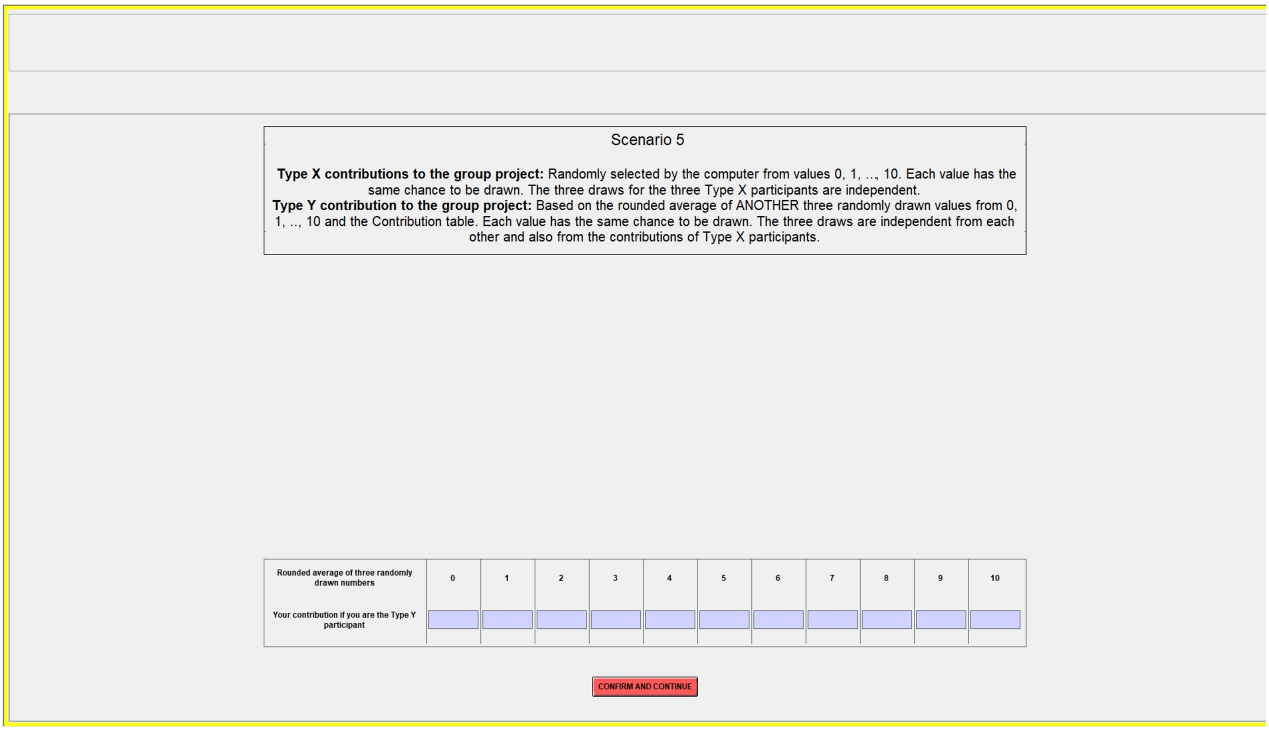


Figure C6: Screenshot of the treatment 5 input screen