Ecological Economics 184 (2021) 106998

Contents lists available at ScienceDirect

Ecological Economics

journal homepage: www.elsevier.com/locate/ecolecon

Methodological and Ideological Options

Too different to solve climate change? Experimental evidence on the effects of production and benefit heterogeneity on collective action

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ARTICLE INFO

Keywords:
Public goods game
Heterogeneity
Climate change
Collective action
Reciprocity

ABSTRACT

Though a global phenomenon, climate change will impact different countries to varying degrees. Different countries and industries also vary in how cost effectively they can mitigate climate change. These heterogeneities—one in marginal benefits derived from greenhouse mitigation ("benefit heterogeneity"), the other in marginal productivity in organizing collective action toward greenhouse mitigation ("production heterogeneity")—have not been sufficiently studied, nor have they been directly compared. The paper tests for the effects of these two heterogeneities in a linear public goods setting, allowing the identification of different drivers of cooperative behavior. We find that heterogeneous assemblies are less able to collectively provide a public good such as greenhouse gas mitigation. Crucially, the type of heterogeneity matters. When there are less-productive mitigators, or when mitigation benefits other actors more than oneself—scenarios that mirror the incentives facing many developed nations—collective action is least effective. Results suggest that emphasizing reciprocity may improve collective action toward mitigation, but this depends on whose behavior is reciprocated. In addition to these empirical findings, the paper advances a methodological innovation. Whereas previous studies manually sorted individuals into contribution groups, which is impractical in larger data sets and yields difficult-to-replicate classifications, this paper uses machine learning to classify players according to their conditional contribution behavior.

1. Understanding heterogeneity in climate negotiations

1.1. Heterogeneity among climate actors

Greenhouse gas mitigation is arguably one of the most challenging collective action problems humanity has faced. In a recent landmark study, the UN warns that carbon dioxide emissions would have to drop 45% by 2030 to limit temperature increases to 1.5 °C and avoid potentially catastrophic consequences (IPCC, 2018). The requisite changes in behavior and economic composition, advancements in technology, and adjustments to carbon-heavy infrastructure would be enormous (ibid.). Exacerbating the already daunting tasks is the incentive structure surrounding this transformation. Climate mitigation is a public good; while the costs of mitigation efforts are born by the mitigator,[1] the benefits from such mitigation are shared across countries and generations. This benefit-cost distribution results in a social dilemma where each individual country's best response is to free-ride on other countries' mitigation efforts, resulting in climate tragedy (Levin et al., 2012). While collective action problems can be overcome under specific circumstances (Barrett, 2003; Ostrom, 2005), those conditions are unlikely to hold for this issue. Among the numerous factors[2] that make the global climate a particularly challenging social dilemma is the considerable heterogeneity across actors, which has been hypothesized to negatively influence the likelihood of successful mitigation attempts (Tavoni et al., 2011; Waichman et al., 2018). In particular, many countries that contribute relatively little to the climate problem—and hence are able to contribute relatively little to mitigation efforts—are also most vulnerable to the effects of climate change.[3] While other

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[1] i.e., an individual country or industry

[2] For example, inter-generational distribution of costs and benefits of mitigation (Gardiner, 2006), uncertain thresholds (Barrett and Dannenberg, 2012), and human psychology ill-equipped to deal with long-term problems (Oppenheimer and Todorov, 2006; Swim et al., 2009).

[3] Be it by their geographic characteristics, such as small island states, or by having insufficient economic buffers to finance adaptation efforts.

https://doi.org/10.1016/j.ecolecon.2021.106998
Received 20 December 2019; Received in revised form 29 January 2021; Accepted 8 February 2021
Available online 11 March 2021
0921-8009/

countries, such as the United States—which in 2016 emitted about 16% of global carbon dioxide emissions (International Energy Association, 2019)—contribute a large share of emissions but are comparatively less affected by changes in global temperature (Bathiany et al., 2018; Nunn et al., 2019). This imbalance further limits the incentives that the largest greenhouse gas emitters have to contribute toward the mitigation effort.

In this setting, behavioral economics can assist in developing collective action strategies to mitigate climate change by identifying whether and what types of heterogeneity drive cooperative deficiencies (Milinski et al., 2008; Milinski et al., 2006). While no model or experiment can capture all the nuances of the institutions and incentive structures at play in an issue as complex as climate change mitigation, pared down analyses of underlying behavioral tendencies are useful for theory testing (Ostrom, 2006) and can, at least qualitatively, shed light on the factors that need to be considered when designing effective policy (Kessler and Vesterlund, 2015). Hitherto, this literature has predominantly focused on modeling heterogeneity in terms of i) varying endowments[4] (Milinski et al., 2011; Tavoni et al., 2011); ii) varying impacts of climate change (Waichman et al., 2018), and iii) varying likelihoods of suffering damages from a climate catastrophe[5] (Burton-Chellew et al., 2013). These behavioral economics studies that assess barriers to collective climate action have paid considerably less attention to two types of heterogeneity; heterogeneity in marginal mitigation *productivity* and *benefit*.

Marginal *productivity* heterogeneity (MPH) implies that countries differ in how many tons of CO_2 each unit of effort or set monetary value can mitigate. This type of productivity might be governed by where on the marginal abatement cost curve countries are located,[6] available technology (which would shift the abatement curve down), or the choice of mitigation method.[7] Marginal *benefit* heterogeneity (MBH), in contrast, implies that countries differ in how much benefit they derive from each ton of CO_2 mitigated, sequestered, etc. In other words, the social cost of carbon is not distributed uniformly. While marginal benefits will significantly depend on the current greenhouse gas concentration and proximity to potential climate tipping points, heterogeneity is largely determined by how sensitive given locations are to climate impacts. For example, the IPCC report indicates that different regions will experience different temperature increases, precipitation changes, and magnitudes of sea-level rise (IPCC, 2018). Even if climate impacts were to be distributed evenly, regions differ in how reliant they are on resources and infrastructure affected by climate change. To illustrate, the U.S. with significant population centers near coast lines will derive greater damages from sea-level rise than land-locked Switzerland. In addition, there may be interaction effects between MPH and MBH. For example, while the Small Island States may not be effective in curbing greenhouse gas emissions given their marginal contribution to the problem and/or unavailability of cheap green alternatives (an example of MPH), these countries may benefit significantly from mitigation given their vulnerability to sea-level increases.

While efficiency dictates that, in cases of MPH and MBH, high abatement productive countries should shoulder the bulk of the mitigation effort and be compensated by high abatement benefit countries, it is not clear how these types of actors would respond given the social dilemma setting and lacking externally enforceable compensation mechanisms.[8] Additionally, it is not evident how these different types of heterogeneities affect a group's ability to collectively overcome a social dilemma. Previous studies have accounted for variations in benefits from a public good based on who benefits (MBH) and who contributed (MPH), but there are no studies that directly compare the effects of these types of heterogeneities (see Table 1 below). Rather, scholars seemingly assume that these types of heterogeneity have differing impacts on prosocial behavior (i.e., mitigation effort) and study them separately. Table 1 provides an overview of the studies that incorporate some kind of marginal benefit and production heterogeneity in variants of linear public goods games. Linear public goods games provide a simple incentive structure to assess collective action behavior under varying conditions. Consequently, a wealth of studies employ this design (for overviews, see Chaudhuri, 2011; Ledyard, 1995; Zelmer, 2003) and its prevalence is as a testament to its effectiveness in capturing the conflicting individual and group incentives inherent to social dilemma settings. The first row in Table 1 identifies studies that explore the effect of MBH and MPH (and variations thereof) on cooperative behavior by comparing contributions in those settings with contributions in homogeneous settings. The second row lists studies that use heterogeneous

Table 1
Overview of studies exploring marginal benefit and production heterogeneity.[a]

Focus of study	Marginal benefit heterogeneity	Marginal production heterogeneity
Test for the impact of heterogeneity[b]	Fisher et al. (1995)	Blanco et al. (2016) Blanco et al. (2018) Carter et al. (2001) Goeree et al. (2002) Goetze and Galderisi (1989) Güth and Sääksvuori (2012)[c]
Test for interaction between heterogeneity and institutional factors	Baggio and Mittone (2014) Cardenas et al. (2002) Chan et al. (1999) Chan et al. (2012) Dasgupta and Orman (2014) Fischbacher et al. (2014) Helland et al. (2018) Kesternich et al. (2014) Reuben and Riedl (2013)	Brick and Visser (2012) Fellner et al. (2014) Noussair and Tan (2011) Tan (2008)

[a] This is study is also similar to a growing literature on multi-level or nested public goods games where individuals receive different benefits from contributions to local and global public goods (Blackwell and McKee, 2003; Buchan et al., 2009; Fellner and Lünser, 2014; Gallier et al., 2019).
[b] In other words, the null hypothesis is H_0: $x_i(\pi^{\beta}) = x_i(\pi^{\beta_{i(\beta)}})$; see discussion below.
[c] Güth and Sääksvuori's (2012) design varies includes both benefit and production heterogeneity, but they do not systematically test for the different effects. The parameters in all their treatments emulate those in our LOCAL treatment, and they do not test for effects when global externalities are greater than local externalities.

[4] That is, ability to contribute in the mitigation effort.
[5] That is, if groups fail to collectively prevent climate change there is a risk that they will lose their remaining resources.
[6] For example, low carbon economies have few low-cost mitigation opportunities left, or less developed countries are able to sequester carbon at a lower price than developed countries.
[7] For example, the same amount of CO_2 mitigated from air travel results in comparably greater mitigation benefit compared to CO_2 mitigated from vehicles because emissions in higher altitudes are more damaging (Lee et al., 2010).
[8] International environmental agreements are designed with the goal of creating self-enforcing treaties but often, as in the case of the Kyoto Protocol, fall short of this goal (Barrett, 2003). Arguably, it may thus be useful to explore behavioral tendencies in the absence of these institutions.

groups as their baseline and test what type of institutional manipulation is effective in encouraging pro-social contribution behavior. Noussair and Tan (2011), for example, study how effective democratically determined sanctions are in eliciting collective action when actors vary in their productivity in providing the public good.

To the best of our knowledge, there are no studies that directly compare these different forms of heterogeneity. Also absent are studies that test the effects of an interaction between these distinct types of heterogeneity. A direct test of the effect of these types of heterogeneity is critical to precisely identify the source of collective action failures, and to effectively design policies that might overcome these problems. This test would also allow us to assess whether some of the institutions studied in the second row of Table 1 would similarly apply to the other heterogeneity setting. To fill these critical gaps and aid institutional design by correctly specifying and testing sources of heterogeneity, we measure the effects of different types of heterogeneity on group- and individual-level contributions by varying how productive each contribution (in a linear public goods game) is in mitigating climate change, and how much benefit each individual receives from the public good. Individual incentives vary while the group-level return on investment—which Isaac et al. (1994) propose as one of the main determinants of group-level contributions—remains constant across treatments. We explore four heterogeneity treatments as specified below.

1.2. Using clustering to classify player types – A methodological contribution

In addition to our empirical contribution, we offer a methodological contribution to the literature. We introduce clustering techniques to classify player types based on conditional cooperation preference elicitation data gathered during our experiment. Conditional cooperation is one of the leading theories to explain contribution behavior in social dilemmas. Fischbacher et al. (2001) posit that individuals are willing to cooperate to overcome a social dilemma if others reciprocate these contributions. While support for the theory has been found in the lab (Fischbacher et al., 2001; Fischbacher and Gächter, 2010; Keser and Van Winden, 2000) and in the field (Kraft-Todd et al., 2015), the prevailing means by which researchers classify individuals according to these preferences are limited. Previous researchers have manually assigned participants to one or another ad hoc behavioral categories. These categories tend to correspond to archetypical behavioral tendencies that researchers identify in their subjects' contribution schedules. For example, Fischbacher et al. (2001) and Fischbacher and Gächter (2010) manually place their subjects into three categories that they name "free-riders", "conditional cooperators", and "hump-shaped/triangle"[9] cooperators, with an additional taxon to hold subjects they deem "unclassifiable" or whose contributions exhibit "other patterns". This approach is workable when the number of subjects is relatively small, but becomes intractable as the number of subjects grows, given its reliance on visually inspecting the shape of subject contribution schedules. Further, this scheme for classifying behavioral tendencies is often reliant on subjective interpretation. While subjectivity is unavoidable and not problematic per se, it would be preferable for researchers to have a means of (i) validating the classifications used by their peers, (ii) testing alternative classification schemes, and (iii) nuancing those classification schemes by introducing additional variables on which to base classification.

Our use of clustering allows for the following classification improvements. First, our data and R clustering code are publicly available for replication. Second, our code instructs researchers on how to remove or alter the variables on which we based behavior classification, and instructions on how to incorporate additional variables as relevant to other studies but not included in our classification scheme. Because cluster analysis requires researchers to make a series of decisions regarding technical specifications, any one of which alone—and all of which in concert—can influence the final sorting into categories, our code specifies the technical decisions we made, invites researchers to consider alternate specifications, and automatically generates metrics with which to asses our and proposed specifications.[10] Finally, because our clustering approach identifies subjects exhibiting behavioral tendencies based on a researcher-specified set of theoretically relevant inputs rather than working backwards from a predetermined set of archetypical types, previously difficult-to-classify subjects can be systematically grouped with similar subjects, exposing new or potentially intermediary behavioral categories unidentified by earlier researchers. In addition to the aforementioned improvements, our introduction of behavioral tendency classification using clustering techniques can be adapted to take full advantage of automation and machine learning.

2. Methods and materials

2.1. Marginal cost and benefit heterogeneity in a linear public goods games

To explore the impact of production and benefit heterogeneity, we utilize a linear[11] public goods game. Homogeneous linear public goods games have the following set-up: a set of players $i, j = \{1, 2, \ldots, n\}$, each receives a token endowment of e tokens and decides how many of these tokens, x_i, to invest in a group fund, which supplies the public good to the whole group. For each token that an individual places in her private account she receives a rate of return α. For each token in the group account, irrespective of the origin of the contribution, each individual receives a per token return of β. Linking this back to climate change negotiations, the endowment would represent a state's resources that could be committed to climate mitigation. The decision to invest in the group fund mirrors a state's mitigation efforts, which have positive externalities to the global community of size β. α represents the benefits that can be received from investing available resources in projects, policies, etc., unrelated to greenhouse gas emissions mitigation. This results in the following payoff function of:

Eq. 1: Linear Public Goods Payoff Function in Homogeneous Groups

[9] Triangle cooperators exhibit conditional cooperative tendencies, increasing contributions as other group members increase theirs, up to a given point. Thereafter these individuals reduce their contributions, seemingly substituting their group members' contributions for their own.

[10] Among the decisions an analyst must make: whether and how to scale data, which kernel and how many bandwidths to use, method of aggregation, measure of distance, class of clustering (e.g., k-means, partitioning around medoids, and hierarchical), etc. There are not always clear theoretical grounds or even general rules of thumb directing analysts on which specifications to use. In these cases, researchers can move forward by assessing various options against one another using various metrics. Different metrics, however, may buttress or impugn a given partitioning, so researchers must be cautious in determining which metrics to utilize. To help other researchers assess our and their clustering specifications, our code uses Charrad et al.'s (2014) "NbClust" package, which in turn relies on a type of popularity heuristic. By presenting and summarizing over thirty measures of cluster fit and resilience, researchers can make informed decisions about which parameters they specify, and have a ready means by which to compare their and others' final clusters.

[11] Many climate change experimental studies analyze climate change mitigation behavior in provision point public goods settings, where a failure to contribute a given amount results in climate disaster (e.g., Barrett and Dannenberg, 2012; Milinski et al., 2008). In contrast, we utilize a *linear* public goods setting to align our study with the incentive structure used by previous studies on marginal heterogeneity (see Table 1) and because mitigation, at this point, will limit temperature increases but will not avoid them (IPCC, 2018).

Table 2
Overview MPCR heterogeneity types and experimental treatments.[a]

Heterogeneity type	Relationship between βs	Treatment	$\beta_{1,1}$	$\beta_{1,2}$	$\beta_{2,1}$	$\beta_{2,2}$
Homogeneity	$\beta_{i,G_i} = \beta_{i,G_{-i}} = \beta_{-i,G_i} = \beta_{-i,G_{-i}}$	HOM	0.6	0.6	0.6	0.6
Marginal Benefit	$\beta_{i,G_i} = \beta_{i,G_{-i}} \neq \beta_{-i,G_i} = \beta_{-i,G_{-i}}$	MBH	0.8	0.8	0.4	0.4
Marginal Production	$\beta_{i,G_i} = \beta_{-i,G_i} \neq \beta_{i,G_{-i}} = \beta_{-i,G_{-i}}$	MPH	0.8	0.4	0.8	0.4
In-group Externalities	$\beta_{i,G_i} = \beta_{-i,G_{-i}} > \beta_{i,G_{-i}} = \beta_{-i,G_i}$	LOCAL	0.8	0.4	0.4	0.8
Out-group Externalities	$\beta_{i,G_{-i}} = \beta_{-i,G_i} > \beta_{i,G_i} = \beta_{-i,G_{-i}}$	LONG	0.4	0.8	0.8	0.4

[a] α was set at 1 for the experiment thus β = MPCR in this table.

$$\pi_i = \alpha(e - x_i) + \beta \sum_i^n x_i$$

When $\alpha > \beta$ but $n\beta > \alpha$, the payoff structure emulates the well-known social dilemma conditions in which game theory predicts rational individuals motivated by personal monetary payoffs (and expecting like-minded individuals in the group) will transfer no tokens to the group account.[12] This outcome is not socially optimal as the highest total group payoffs occur when all tokens are placed in the group account. These conditions are often expressed in terms of the marginal per capita return (MPCR) which is calculated as $\frac{\beta}{\alpha}$. Thus, in a social dilemma, $MPCR < 1$ and $n(MPCR) > 1$.

A fixed β, however, fails to account for the production and benefit heterogeneity observed in the climate change mitigation problem.[13] First, *benefit heterogeneity* occurs when one ton of mitigated greenhouse gases yields different benefits to countries or regions. This is the case when geographic regions are more or less prone to the harmful effects of climate change, such as sea-level rise. In this scenario, β in Eq. 1 varies across beneficiaries. Second, *production heterogeneity* occurs when the same amount of effort or financial investment results in different quantities of greenhouse gases mitigated, as is the case among technologically diverse countries. Thus, β varies across *contributions* because contributions toward a public good are no longer perfect substitutes. Finally, production and benefit heterogeneity may interact in two ways: first, *local externalities* arise when those with more effective mitigation abilities also benefit more greatly from mitigation; second, and more reflective of the climate change incentive structure, *long-distance externalities* arise when those who are able to mitigate greenhouse gas emissions most effectively benefit less from these mitigation efforts. This incentive structure mirrors those faced by many developed countries.[14] For them, unlike small island countries, climate change poses a considerable threat, but not necessarily an existential one (Ciscar et al., 2011). To account for these differences, we allow β to vary across individuals as both *recipients* (i.e., $\beta \rightarrow \beta_i$) and *sources* of public goods contributions (i.e., $\beta \rightarrow \beta_{i,j}$ where $\beta_{i,j}$ refers to the benefit derived by individual i from j's contribution). Payoffs can thus be captured by the following payoff function:

Eq. 2: Linear Public Goods Payoff Function with Idiosyncratic MPCR

$$\pi_i = \alpha(e - x_i) + \sum_j^n \beta_{i,j} x_j$$

To specify the heterogeneity in the model, we partition the main group ($n = 4$) into two subgroups: i and one other subject are in i's group G_i, while the remaining two subjects are in group G_{-i} of which i is not a member. Group membership determines the MPCR and payoffs in the following way:

Eq. 3: Linear Public Goods Payoff Function with Group-membership determined MPCR

$$\pi_i = \alpha(e - x_i) + \beta_{i,G_i} \sum_{j \in G_i}^n x_j + \beta_{i,G_{-i}} \sum_{j \in G_{-i}}^n x_j$$

This allows for succinct specification of heterogeneity as identified in the climate negotiations above, summarized here in Table 2. We structure our treatments around these heterogeneity types and, in groups of four individuals with two subgroups, set the parameters as indicated in the right columns of Table 2.

2.2. Experimental design

The experiment consisted of two stages. In Stage I, subjects participated in a preference elicitation game based on Fischbacher et al. (2001) and Fischbacher and Gächter (2010). This data was used (post experiment and for the purpose of analysis) to classify subjects based on conditional contribution tendencies using clustering techniques described below. In this stage, participants were randomly assigned to groups of four to play a linear public goods game with the following parameters: $e = 12$, $\alpha = 5$, and $\beta = 3$. However, rather than regularly playing the game, subjects were asked to provide contribution decisions to a linear public goods game for all possible average contributions

[12] In other words, the Nash Equilibrium implies no contributions given the linear cost and benefit functions. This result holds both for one-shot settings and repeated games with known end-point (using backward induction).

[13] It is important to note that marginal benefit and production heterogeneity are not the only important sources of heterogeneity that impact actor incentives. While we are unable to study these sources of heterogeneity in this experiment, it is important to acknowledge their role in shaping mitigation incentives. Differences in endowments, e, change how much a country can contribute to mitigation in the first place. This source of heterogeneity has been studied by Milinski et al. (2011) and Tavoni et al. (2011). In addition, variations in the private return, α, would represent differences in opportunity cost of mitigation. Any given amount spent on mitigation by less developed countries will have an outsized marginal impact, relative to more developed countries, on these countries' ability to promote economic growth and alleviate poverty. A variant of this type of heterogeneity was studied by Blanco et al., 2016.

[14] This incentive structure is also reminiscent of many of the long-range sulfur dioxide pollution problems which motivated the 1985 Helsinki Protocol (Ringquist and Kostadinova, 2005); The UK emitted large amounts of sulfur dioxide that, due to prevailing winds, resulted in acid deposition in Norway and Sweden. Scandinavian forests were significantly damaged while the impact on UK ecosystems was less pronounced. Hence, Scandinavia, for its own well-being relied on emission cuts in the UK.

(rounded to the nearest token) by their group members. (This method is based on the strategy method pioneered by Selten, 1965.) In other words, participants were asked how many tokens they would transfer to the group account if their group members transferred a mean of 0, 1, …, 12 tokens. This resulted in 13 *conditional* contribution decisions from each subject. They were also asked for an *unconditional* contribution decision (i.e., a contribution decision if they did not know how much their group members were contributing). Subjects received no information on their group member decisions and how much they had earned until the end of the experiment. Payoffs from Stage I were calculated by first randomly assigning three of the four group members to take the roles of the *unconditional* contributors. The remaining group member was selected to act as the *conditional* cooperator. The corresponding three *unconditional* contribution decisions were used to calculate average contributions by group members and then to determine the corresponding conditional contribution by the fourth group member. These contributions were summed and used to determine returns from the group account and subsequently payoffs for each individual group member.

Stage II consisted of a multi-round, linear, voluntary contribution game (Isaac and Walker, 1988a) with MPCR heterogeneity treatments as described in Table 2. At the beginning of Stage II, subjects were randomly assigned to a new group of four individuals. Within that group they were randomly assigned to a *player type*: either a Type 1 player or Type 2 player. In the game, Type 1 and Type 2 players were described as belonging to different color groups—individuals were either orange or purple.[15] This random assignment technique and reference to colors rather than numbers were used to limit group identity effects (Chen and Li, 2009; Eckel and Grossman, 2005) in determining cooperative behavior. Subjects maintained these player type designations for the duration of Stage II, which lasted 15 periods. In this setting, an endowment of 25 tokens ($e = 25$) was received each period and did not vary across participants. The private return, α, was set to 1 and did not vary across subjects. β, however (and thus the MPCR) varied by treatment (see Table 2). After allocating their tokens each period, subjects saw the following information: i) their contribution decision that period, ii) their earnings that period, iii) the total number of tokens transferred to the Group Fund that period, and iv) the individual transfers to the Group Fund made by all other group members that period.[16] A history of this information was displayed in table format on their screen while subjects made their contribution decisions in subsequent periods.[17] The five treatments were as follows: i) homogeneous returns (HOM) treatment in which all participants received the same benefit from contributions made to the group account ($\beta = 0.6$); ii) marginal benefit heterogeneity (MBH) treatment in which β varied across the *receiver* of the public benefit, with Type 1 individuals always receiving greater payoffs from the public good (i.e., $\beta_{1,1} = \beta_{1,2} = 0.8$ and $\beta_{2,1} = \beta_{2,2} = 0.4$); iii) marginal production heterogeneity (MPH) treatment in which β varied across the *producer* of the public good, with Type 1 individuals producing greater benefit for the group with their contribution (i.e., $\beta_{1,1} = \beta_{2,1} = 0.8$ and $\beta_{1,2} = \beta_{2,2} = 0.4$); iv) the *LOCAL* treatment in which β varied across both the receiver and producer of the public good with in-group members receiving greater benefit from their own contributions, hence $\beta_{1,1} = \beta_{2,2} = 0.8$ and $\beta_{2,1} = \beta_{1,2} = 0.4$; and v) the *LONG* treatment which emulated the long-distance externality scenario described above where β varied across both the receiver and producer of the public good with out-group members receiving greater benefit from contributions, hence $\beta_{1,2} = \beta_{2,1} = 0.8$ and $\beta_{1,1} = \beta_{2,2} = 0.4$.

2.3. Hypotheses

We use the experimental treatments described above to test the following group-level and individual-level hypotheses. Group hypotheses compare how cooperative groups are across the different treatments, while individual hypotheses test individual motivations to contribute toward the public good.

G1: Heterogeneity Hypothesis: *Heterogeneity does not affect group-level contribution rates; contributions will be similar in the homogeneous treatment compared to the heterogeneity treatments.*

G2: Type of Heterogeneity Hypothesis: *There is no difference in group contributions across the different heterogeneity treatments.*

As indicated by Zelmer's (2003) meta-analysis, MPCR is strongly positively correlated with contribution levels in public goods games. Isaac et al. (1994) hypothesize that group return (a summation of individual MPCR, i.e., $\delta = \sum_{i,j \in N}^{n} \beta_{i,j}$) is thus determinative of group-level contributions. While this finding has been refuted in experiments that vary group sizes while keeping group returns constant (Diederich et al., 2016; Nosenzo et al., 2015; Weimann et al., 2019), there is evidence that when keeping group size and group returns constant but introducing MPCR heterogeneity there is no change in contribution behavior. Fisher et al. (1995), comparing contributions in heterogeneous groups with contributions averaged across low and high MPCR homogeneous groups, find no statistical difference.[18] In addition, Zelmer (2003) determines that MPCR heterogeneity is not a significant factor of overall group contributions.[19] Thus, given that the group return has been held constant across treatments ($\delta = 2.4$), the literature seems to indicate that there should be no significant difference between contributions in any of our heterogeneous treatments and the HOM treatment. However, these findings largely apply to marginal benefit heterogeneity. Studies of marginal production heterogeneity either use different game or heterogeneity set-ups (Blanco et al., 2016; Goeree et al., 2002), or have additional limits to contributions (e.g., Blanco et al., 2018), limiting their predictive quality for this experiment. Further, there is evidence to suggest that the relationship between MPCR and contributions is not linear (Gunnthorsdottir et al., 2007; Weimann et al., 2019). Weimann et al. (2019) indicate that the relationship is concave, meaning that the difference in contribution levels between individuals with an MPCR of 0.4 and individuals with an MPCR of 0.6 is greater than the difference in contributions between an MPCR of 0.6 and an MPCR of 0.8. Conse-

Table 3
Isolating MPCR effects.

Effects	Type 1	Type 2
Own benefits received from own type's contribution – *own effects*	$\beta_{1,1}$	$\beta_{2,2}$
Own benefits received from other type's contribution – *dependence effects*	$\beta_{1,2}$	$\beta_{2,1}$
Other type's benefits received from own type's contribution – *positive externalities*	$\beta_{2,1}$	$\beta_{1,2}$
Other type's benefits received from their contribution – *outsider effects*	$\beta_{2,2}$	$\beta_{1,1}$

[15] The colors orange and purple were chosen as these are not readily connected with existing social groups that might have preexisting behavioral expectations (such as red for Republicans and blue for Democrats).

[16] This information was presented using the player color and subject number.

[17] Participants thus had full contribution information as would be the case in international negotiations when parties have information on mitigation targets states have previously committed themselves to, and whether they are meeting these targets.

[18] Note: Fisher et al. compare heterogeneous group contributions to an average of homogeneous groups with low and homogeneous groups with high MPCR. In addition, unlike our experiment, Fisher et al. (1995) do not inform their subjects about the differences in MPCR, which may lead to different behavior.

[19] It is not clear, however, which studies in her sample exhibit MPCR heterogeneity.

quently, heterogeneity should result in lower contributions as the individuals facing a lower MPCR will reduce their contributions by greater amounts than individuals facing a higher MPCR will increase theirs. This effect should be particularly pronounced for the MBH treatment where, unlike in the other treatments, two individuals receive lower MPCR for all contributions regardless of the origin. Thus, the evidence for group-level heterogeneity hypotheses is mixed.

In the individual-level contributions hypotheses, we isolate the effects of returns from the public good by both recipient (i.e., to whom benefits accrue) and source (i.e., who made the contribution). The experimental design implies each individual receiving a benefit from their own type's contributions and from contributions made by the subjects of the other type. Similarly, one's own contribution affects payoffs both to subjects of their type (themselves included) and to subjects of the other type. This results in four distinct effects that are summarized in Table 3 and are discussed below.

I1: MPCR Effects Hypothesis: *Higher MPCR results in higher contributions by individuals.*

This hypothesis derives from the discussion above. MPCR effects should result in higher contributions by Type 1 individuals in MBH than by HOM individuals and by Type 2 MBH individuals. Hypothesizing non-linear MPCR effects (Weimann et al., 2019) should result in the difference between MBH Type 2 contributions and HOM contributions to be more pronounced than the difference between MBH Type 1 contributions and HOM contributions.

I2: Own Effects Hypothesis: *Keeping other effects constant, subjects will contribute more to the group account in treatments with higher rather than lower "own effects" ($\beta_{1,1}$ and $\beta_{2,2}$).*

Several studies (e.g., Goeree et al., 2002; Güth and Sääksvuori, 2012) conclude that individuals tend to contribute more as own effects increase. This is because the opportunity cost of investing in the group fund is reduced for the contributing individual (Goeree et al., 2002).[20]

I3: Dependence effects Hypothesis*: Keeping other effects constant, when subjects receive a greater return from other group members' contributions, they will increase their own contributions to encourage higher contributions by others.* Signaling and conditional cooperation have been found to be a motivation for higher contributions both inside (Fischbacher and Gächter, 2010; Fischbacher et al., 2001; Isaac et al., 1994) and outside (Kraft-Todd et al., 2015) the lab. Signaling theories suggest that individuals increase their contributions to demonstrate their trustworthiness and elicit higher contributions by their group members, while conditional cooperation theories posit that individuals are willing to mirror contributions by others. In settings where positive externalities are high, individuals will benefit to a greater extent from being able to convincingly signal their cooperativeness and maintain high contributions from the other type of players. Likewise, because of the high opportunity cost to the other player of foregoing one's contribution, reducing one's own contributions is a more effective sanction to keep contributors in line when positive externalities are high. This implies higher contributions in the presence of high positive externalities when conditional cooperators are present.

I4: Altruism Hypothesis*: Keeping other effects constant, subjects will contribute more to the group account in treatments with higher rather than lower positive externalities ($\beta_{2,1}$ and $\beta_{1,2}$).*

A number of theories posit that human behavior is partly driven by altruistic motives, pure or impure[21] in nature (Andreoni, 1990; Bolton and Ockenfels, 2000; Cooper and Kagel, 2016; Fehr and Schmidt, 1999). These theories suggest that individuals are motivated by their concern for others' wellbeing. Part of the contributions in a public goods game result from this concern (Croson, 2007), implying higher contributions in settings with higher positive externalities.

2.4. Experimental implementation

Subjects received on-screen and printed instructions,[22] which they could read at their own pace. All instructions were reviewed publicly. Before proceeding to the decision-making phase of these stages, subjects participated in quizzes to ensure that they fully understood all features of the game. Subjects could not proceed without correctly answering all quiz questions. Participants received payoffs in terms of Experimental Currency Units (ECU) during the experiment. At the end of the experiment, subjects received, privately, the sum of their per-stage earnings (converted into US dollars at a rate of 100 ECU = $1) as well as a $5 show-up payment. All sessions were programmed and implemented using z-Tree (Fischbacher 2007). Sessions were conducted at a large Midwestern research university.[23] Subjects were recruited using ORSEE recruiting software and consisted of undergraduate students from a diversity of majors who expressed interest in participating in economics experiments. To preserve anonymity and ensure that individuals did not know who was in their group, all sessions were conducted with at least two groups per session. All treatments contained eight subject groups each, with the exception of MPH which covered seven groups and HOM with six groups. Subjects earned an average of $18.57, with the experiment taking, on average, less than 45 min.[24]

3. Results

3.1. Group-level effects: Heterogeneity (type) matters for collective action

Fig. 1 provides an overview of group-level contribution behavior. As is common in public goods games, there is a gradual decline in contributions—an endgame effect (Ledyard, 1995; Neugebauer et al., 2009). Of note, however, is that groups in the HOM treatment tend to contribute more toward the public good.[25]Table 4 displays two-sample *t*-tests, illustrating that contribution levels are significantly different in all treatments except in case of MBH and LONG. To leverage the panel structure of the data we use a random effects regression with treatment dummies (and period as a control variable). The HOM treatment is used as the reference case. The results in Table 5 largely confirm the visual

[20] In this particular experiment, given the focus on group heterogeneity, the other individual from of the same type also benefits from greater returns, suggesting that there may be an element of altruism or conditional cooperation at play in higher contributions under these conditions as well (see discussion below).

[21] Impure altruism implies that others' wellbeing is a source of utility for individuals, meaning that pro-social behavior actually improves the actor's *own* wellbeing – which implies that the motivation is not *purely* altruistic (Andreoni, 1989, 1990)

[22] Instructions available in full in Appendix A.

[23] Funding for the study was received through a Sustainability Research Development Grant from Indiana University Office of Sustainability.

[24] The data set and corresponding Stata do file can be found in (Kreitmair and Bower-Bir, 2021).

[25] For the analysis we dropped an outlier group from the HOM treatment. In group 7073, random assignment of type roles resulted in two "free-riders" being assigned to be type 1 individuals who contributed an average of 2.6 tokens per round. Meanwhile, the individuals assigned to type 2 roles contributed an average of 16.8 tokens. This discrepancy in contribution behavior made it appear as though there were significant differences in contribution behavior across type 1 and type 2 subjects in the HOM treatment although incentive structures were identical across types in this treatment. In all remaining HOM groups, there was no significant difference between type 1 and type 2 individual contribution behavior. Significance levels and directions of effects in Tables 5 and 6 remain the same with and without group 7073 (see online analysis file for details).

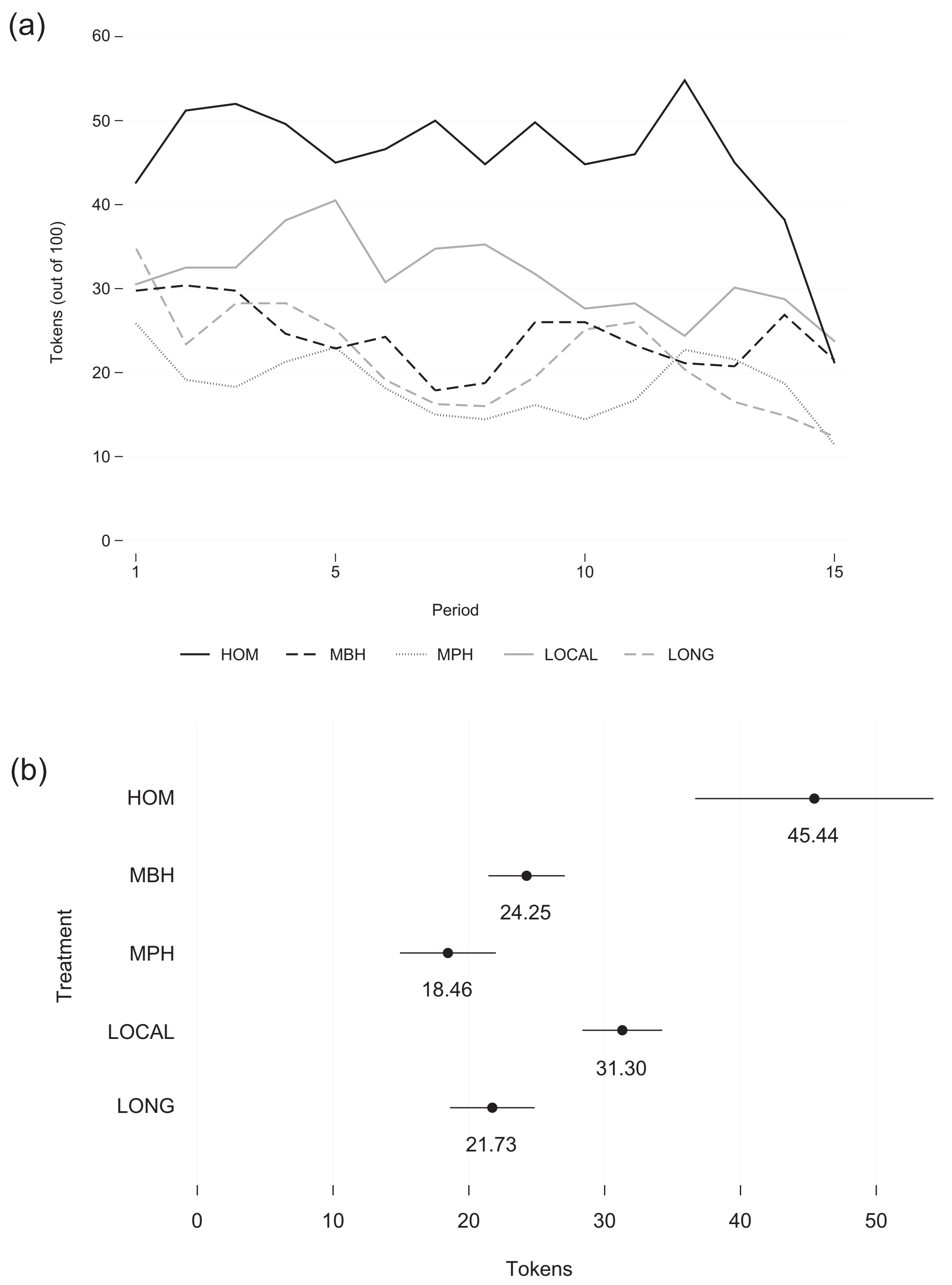

Fig. 1. a) Mean Group Contributions by Period; b) Mean Group Contributions by Treatment (with 95% Confidence Intervals).

findings above[26]; group-level contributions in the HOM treatment are significantly higher than contributions in all the heterogeneous treatments except for in the LOCAL treatment. This finding runs counter to findings by Fisher et al. (1995). This thus contradicts the theory that group-level contributions, even in the face of heterogeneity, are largely predicted by the overall group return. Rather, it may lend support for non-linear MPCR effects (Gunnthorsdottir et al., 2007; Weimann et al., 2019) or conditional cooperative dynamics that depress contributions in heterogeneous groups. Individual motivations for contributions are analyzed below. Further, the type of heterogeneity seemingly has a varying effect on group contributions, where heterogeneity represented in the LOCAL treatment does not result in significantly different levels of contributions compared to the HOM treatment. However, comparing regression coefficients for the different treatments, there are no significant differences across the heterogeneity.

[26] Non-parametric Mann-Whitney tests of equal contribution distributions have also been run; see Table B1 in the appendix. Results largely confirm insights from Table 4.

Table 4
Two-sample *T*-tests of group contributions across treatments (15 periods).

	MBH	MPH	LOCAL	LONG
HOM	5.411***	6.310***	3.576***	5.916***
	(195)	(180)	(195)	(195)
MBH	–	2.569***	−3.429***	1.191
		(225)	(240)	(240)
MPH	–	–	−5.582***	−1.380***
			(225)	(225)
LOCAL	–	–	–	4.425***
				(240)

Notes: t-statistic (combined observations). Negative values for t statistics indicate that contributions in the treatment in the left column were lower than contributions in the treatment in the top row.
* $p \leq 0.1$.
** $p \leq 0.05$.
*** $p \leq 0.01$.

Table 5
Regression analysis of group-level contributions.*

Dependent variable: Group contributions	
MBH	−21.19**
	[10.76]
MPH	−26.98**
	[11.05]
LOCAL	−14.14
	[10.76]
LONG	−23.71**
	[10.76]
Period	−0.661***
	[0.116]
Constant	50.73***
	[8.489]
Observations	540
χ^2	39.90***

Notes: random effects. HOM treatment used as the reference case.
Standard errors in brackets.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

3.2. Individual MPCR effects: Contributions by type

Fig. 2 provides an overview of individual-level contribution behavior by treatment and player type. While the results mirror those of group-level contributions, it is noteworthy that, using Mann-Whitney tests,[27] Type 1 individual contributions and Type 2 individual contributions do not differ statistically from one another except in the MBH treatment. While it is expected that contributions by Type 1 and Type 2 individuals differ in the MBH treatment given that incentives differ across types (Hypothesis I1), incentives are also asymmetric in the MPH treatment where contributions are not significantly different. We leverage the panel data structure in the regression analysis in Table 6 below to further explore these effects. In the regressions we include dummy variables for MBH Type 1 and Type 2 individuals, MPH Type 1 and Type 2 individuals, and for treatments LOCAL and LONG. We do not differentiate between types in these symmetric heterogeneity treatments because the incentive structure suggests no behavioral difference and Mann-Whitney tests confirm this empirically. Model 1 includes data only from period 1 to assess contribution behavior untainted by information about others' contributions and group dynamics. Model 2 is a random effects model which leverages data from all 15 contribution periods. The HOM treatment is the reference case in both models.

[27] Table B2 in the appendix provides Mann-Whitney test statistics comparing type 1 contributions to type 2 contributions within each treatment. The difference in the MBH treatment is significant with a *p*-value less than 0.01.

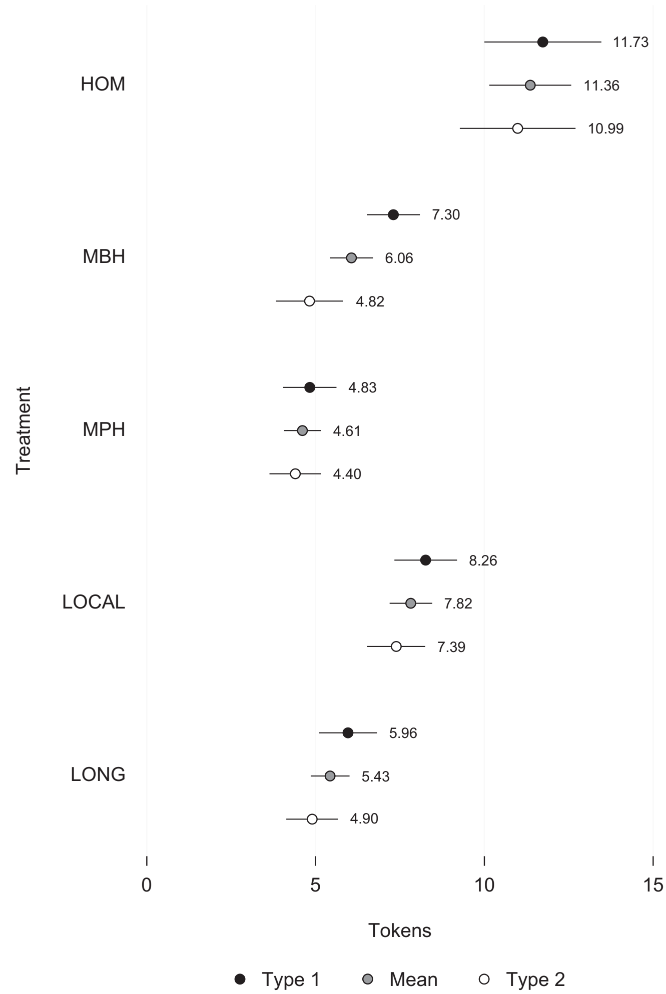

Fig. 2. Mean Individual Contributions by Treatment and Type (with 95% Confidence Intervals).

Table 6
Regression analysis of individual-level contributions by type.

Dependent variable: Individual contributions	(1)	(2)
MBH Type 1	−4.588**	−4.056*
	[2.269]	[2.163]
MBH Type 2	−1.838	−6.539***
	[2.269]	[2.376]
MPH Type 1	−2.579	−6.531***
	[2.357]	[2.346]
MPH Type 2	−5.793**	−6.960***
	[2.357]	[2.391]
LOCAL	−3.025	−3.535
	[1.928]	[2.183]
LONG	−1.963	−5.929***
	[1.928]	[2.119]
Period	–	−0.165***
		[0.000]
Constant	10.65***	12.68***
	[1.513]	[2.033]
Observations	144	2160
R^2/χ^2	0.0556	43.38***

Notes: Model 1 includes first period data only. Model 2 is a random effects model with errors clustered at the subject level. HOM treatment used as the reference case.
Standard errors in brackets.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Even though the explanatory power of Model 1 is low, contrasting these two models provides a number of interesting insights. First, contributions in the MBH treatment do not support Hypothesis I1. Initially, *Type 1* individuals (who receive higher returns from all contributions) contribute significantly *less* than individuals in the HOM treatment who face a lower MPCR. Type 2 individuals who have a lower MPCR than individuals in the HOM treatment do not contribute significantly less than individuals in the homogeneous groups. As the experiment progresses, contributions by Type 2 individuals fall significantly which, in combination with low contributions by Type 1 individuals, explains the low contribution levels in the treatment. Thus, in this setting, contributions by low MPCR individuals are depressed by initial low contributions by high MPCR individuals. This suggests that MPCR by itself cannot explain contribution behavior in marginal benefit heterogeneous groups. Further research will have to identify whether this behavior is the result of confusion, low expectations of contributions by the low MPCR group members, or something else altogether. Second, contributions in the MPH treatment provide some support for Hypothesis I1; in period 1, Type 1 individuals, who produce greater benefits via their contributions, contribute at similar levels[28] to HOM individuals while low productive individuals (Type 2) contribute significantly less. This effect is attenuated as Type 1 individuals reduce their contributions to significantly below contributions observed in the HOM treatment. This may be the result of mimicking contributions by low productive individuals (Type 2) spurred by conditional cooperation tendencies (Fischbacher et al., 2001). We will explore these processes in the regression analysis in Table 7 below.[29] Finally, the symmetric heterogeneity treatments mirror the results from the group-level analysis.

A final curiosity; Type 1 individuals on average contribute more than Type 2 individuals. Even though these differences are not significant (see discussion above), it is surprising to see such a pattern. Incentive structures cannot explain this contribution pattern. In the HOM and symmetric heterogeneous treatments (LOCAL and LONG) individuals face the same incentives regardless of type. The experimental design purposefully avoided priming individuals based on group assignment by using innocuous type-names: “orange” and “purple.” While additional data might identify this contribution pattern as a random occurrence, further research might be necessary to unpack possible group identity effects (Chen and Li, 2009) that can sometimes be triggered by even minimal feelings of belonging (Tajfel and Turner, 1986).

3.3. Unpacking individual motivations for contributions

To identify the effect different types of heterogeneity has on individual motivations to contribute and to understand some of the behaviors exhibited in Table 6, we account for behavioral tendencies in assessing the effects of incentives in our regression analysis below. To do so, we first classified subjects according to the decisions they made in Stage 1. Importantly, we do not ascribe motivations to participants but use exhibited behavior to classify individuals using previously identified behavioral categories (Fischbacher et al., 2001; Fischbacher and Gächter, 2010). Before clustering we ran a simple regression for each subject with dependent variable being the conditional cooperation decisions made by them in Stage 1. Independent variables were average contributions by others (provided to the subjects in Stage 1) and average contributions by others squared. We used the resulting regression coefficient for average contributions by others and the squared term, along with a number of other variables, to classify subjects into behavioral clusters. We used the following variables: i) the summation of an individual’s conditional contributions—a measure of generosity; ii) the regression coefficient—this coefficient measures conditional cooperativeness[30]; iii) the regression coefficient on the squared term—identifies individuals with turning points, aka triangle contributors; iv) the conditional contributions when others contribute an average of 0 tokens “situated” the contribution scheme, because the regression coefficients indicate slope, but not the intercept; and v) a dummy variable for contribution decisions that were uniform across the different possible average contributions by others[31] ensured that the clustering would identify unconditional contributors, given that this behavior is fundamentally different to conditional cooperation behavior. All variables were normalized, so as to limit the importance of one variable over another.[32] Clustering resulted in *six* behavioral clusters (see Fig. B1 for a depiction of all contribution preference schedules and their groupings): 1) *conditional cooperators* are individuals who increased their contributions as average contributions by others increased; 2) *nominal triangle cooperators* are individuals who generally increased their contributions up to a given point, and then reduced contributions; 3) *reverse-conditional cooperators* are individuals who reduced their contributions as others increase theirs, suggesting a preference for maintaining a given amount of public goods; 4) *free-riders* are individuals who contribute the same low number of tokens regardless of contribution by others, this group includes ‘true’ free-riders who contribute zero tokens and low horizontal contributors; 5) *altruists* are individuals who contributed the maximum possible regardless of others’ contributions; and 6) *others* are individuals who do not present preferences that easily fit into a discernable category.[33] Additional details about clustering procedures can be found in (Bower-Bir and Kreitmair, 2017).

Using the clusters identified above, we estimate the following random effects model:

$$
\begin{aligned}
Y_{it} = {} & \alpha + \beta_1 OE_i + \beta_2 DE_i + \beta_3 PE_i + \beta_4 dummies_{icluster} + \beta_5 CST_{i,t-1} \\
& + \beta_6 COT_{i,t-1} + \beta_7 \left(OE_i{}^{*}CST_{i,t-1}\right) + \beta_8 \left(DE_i{}^{*}COT_{i,t-1}\right) + \beta_9 \left(PE_i{}^{*}COT_{i,t-1}\right) \\
& + \beta_{10} \left(dummies_{icluster}{}^{*}ACO_{i,t-1}\right) + \beta_{10} period + u_i + e_{it}
\end{aligned}
$$

Where Y_{it} is an individual’s contribution in a given period, α is a constant, and *period* is the time period in which the contribution took place. OE_i, DE_i, and PE_i are dummy variables that are 1 when, respectively, a subject’s own effects, dependence effects, and positive externalities are high[34] (i.e., the corresponding $\beta_{i,\, Gi} = 0.8$) and 0 otherwise. These variables are used to test the hypotheses discussed above. We also construct dummy variables for behavioral categories based on how individuals were classified during clustering ($dummies_{icluster}$). We include a dummy for the conditional cooperator cluster, the free rider cluster, and the altruist cluster. These particular cluster dummies were included because they are the most distinct behavioral categories and are thus

[28] Non-linear MPCR effects as identified by Gunnthorsdottir et al. (2007) and Weimann et al. (2019)—discussed under the group-level hypotheses—suggest that at high levels of MPCR (as is the case for 0.6 in the HOM treatment) we may not observe much of an effect on contributions as MPCR increases further.

[29] Additional analysis with treatment specific period effects is available in the supplementary materials.

[30] In other words, the greater the coefficient, the more do other individuals contributions determine one’s own contributions. Large coefficients indicate strong conditional cooperation, while small coefficients imply independent decisions.

[31] That is, on a graph with conditional contribution decisions on the y-axis and average contributions by others on the x-axis, these contributions schemes were horizontal lines.

[32] These variables are measured on different scales (e.g., compare the free-rider dummy and total contributions). Variables on larger scales can have an outsized impact on the clustering.

[33] Fischbacher et al. (2001) identified four groups: conditional cooperators, triangle cooperators, free-riders, and other. While clustering may recreate these groupings, visual inspection suggests a significant number of individuals with altruistic preferences and reverse conditional cooperation preferences. Including these individuals in the aforementioned groups would arguably limit behavioral predictions given that these individuals seem to have preferences that are considerably at odds with preferences captured in the other groups.

[34] This will be treatment and type specific – see Table 3

Table 7
Regression analysis of individual-level contributions.

Dependent variable: Individual contributions	(3)	(4)	(5)	(6)	(7)
Own effects dummy (OE)	1.456	1.114	1.187	0.217	0.247
	[1.214]	[1.223]	[1.051]	[1.095]	[1.071]
Dependence effects dummy (DE)	0.0448	0.0713	0.511	−0.0502	0.102
	[0.958]	[0.923]	[0.897]	[0.915]	[0.911]
Positive externalities dummy (PE)	−0.983	−1.198	−0.982	−1.663*	−1.590*
	[1.101]	[1.103]	[0.918]	[0.973]	[0.936]
Conditional cooperator dummy (CC)		0.869	1.035	0.962	−0.791
		[0.918]	[0.769]	[0.748]	[0.902]
Free-rider dummy (FR)		−3.304***	−3.099***	−2.986***	−1.492
		[1.147]	[1.078]	[1.046]	[1.227]
Altruist dummy (A)		3.436	3.920**	3.819**	−3.385
		[2.508]	[1.973]	[1.857]	[2.410]
Lagged contributions by *same* type individual (CST_{t-1})			0.199***	0.103***	0.102***
			[0.0411]	[0.0375]	[0.0396]
Lagged average contributions by *other* type individuals (COT_{t-1})			0.159***	0.0510	0.0132
			[0.0409]	[0.0611]	[0.0624]
Interaction between OE and CST_{t-1}				0.189**	0.168**
				[0.0745]	[0.0729]
Interaction between DE and COT_{t-1}				0.0923	0.101
				[0.0801]	[0.0754]
Interaction between PE and COT_{t-1}				0.137*	0.119
				[0.0815]	[0.0770]
Interaction between CC and lagged average contributions by others (ACO_{t-1})					0.299**
					[0.123]
Interaction between FR and ACO_{t-1}					−0.287***
					[0.105]
Interaction between A and ACO_{t-1}					1.641***
					[0.462]
Period	−0.157***	−0.157***	−0.0920**	−0.0984**	−0.0837**
	[0.0398]	[0.0398]	[0.0401]	[0.0399]	[0.0401]
Constant	7.023***	7.315***	4.089**	5.201***	5.317***
	[1.569]	[1.679]	[1.627]	[1.609]	[1.589]
Observations	1860	1860	1736	1736	1736
χ^2	31.24***	48.83***	95.08***	97.04***	358.4***

Notes: Data only from the heterogeneous treatments.
All models: random effects, errors clustered at the subject-level.
Standard errors in brackets.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

more able to explain behavioral differences. Including additional cluster dummies may thus needlessly complicate the regressions. $CST_{i,\ t-1}$, $COT_{i,\ t-1}$, and $ACO_{i,\ t-1}$ refer to lagged contribution of the other individual of the *same* type, lagged average contributions by individual of the *other* type, and lagged *average* contributions by others, irrespective of type, in the group, respectively. These variables are included to test for conditional cooperative behavior. Finally, there are a number of interaction effects included in the regression to determine how different groups may react to different incentives. u_i and e_{it} represent individual specific effects, and idiosyncratic error terms. Table 7 displays the regression coefficients for the models. All errors are clustered at the individual level.

The regression analysis confirms a number of findings in the literature and provides an insight into what may be driving the results detailed above. *Confirmatory findings*: As expected, many individuals exhibit conditionally cooperative tendencies, where one's own contribution is correlated to contributions made by others. In particular, subjects increase contributions if the other person of the *same* type increased contributions in the previous period (coefficient of CST_{t-1} in Models 5–7). Contributions by individuals of the *other* type (COT_{t-1}), by contrast, do not significantly affect contribution behavior once we account for the incentive structure of the treatment (contrast Model 5 with Models 6 & 7). Contributions by individuals who showcased conditional cooperation tendencies in the preference elicitation stage of the experiment exhibited even greater dependence on contributions by others (coefficient on CC X ACO_{t-1} in Model 7). Free-riders meanwhile are less generous (coefficient of FR in Models 4–6) and actually decrease their contributions in response to contributions by others (coefficient on FR X ACO_{t-1} in Model 7). Altruists, as expected, contribute at a higher rate relative to individuals in other behavioral clusters (coefficient of A in Models 4–6), although once we interact the dummy with lagged contributions by others, we see that altruists too are guided by contributions by their peers. Depending on the distribution of these types of individuals (conditional cooperators, free-riders, and altruists) we may be able to explain some of the depressed contributions in the heterogeneity treatments.

Insights into behavioral processes driving heterogeneity effects: We detect no direct MPCR effects. In other words, receiving higher returns on one's own contributions (own effects) or receiving higher returns from a different type's contributions (dependence effects) does not significantly change one's contribution behavior. This runs counter to the Hypotheses I2-I4 and, for example, altruist effects detected in one-shot settings in Goeree et al. (2002). However, these incentive structures significantly elevate the importance of conditional cooperation for all individuals regardless of exhibited behavioral tendencies. When we interact own effects, dependence effects, and positive externality dummies with the appropriate[35] contributions by others, we find that high own effects increase the effect of conditional cooperation. In other words, when one receives higher payoffs from contributions from one's own type, the

[35] Given the incentive structure, CST_{t-1} is arguably more relevant when own effects are high and COT_{t-1} is more relevant when dependence and positive externalities are high.

other person's contributions take greater weight in determining one's own contribution (Models 6 and 7). Likewise, in settings with high positive externalities, contributions by individuals of the other type play a greater role in one's own contributions (Model 6) – although this effect is not significant once behavioral tendencies are interacted with contributions by others. Interestingly this is not the case in settings with high dependence effects, suggesting that conditional cooperation is stronger when one has a greater impact on the benefits of the other type rather than them having a greater impact on one's own benefits.

Relating this back to contributions in heterogeneous groups, the regression results suggest that rather than MPCR effects per se, heightened conditional cooperation may be the driving force behind low contributions. As suggested by Fischbacher et al. (2001), Chaudhuri and Paichayontvijit (2006), and Chaudhuri (2011) conditional cooperation can sustain high levels of cooperation. On the reverse, it can also explain the steady decline in contributions observed in public goods games (Neugebauer et al., 2009). In the setting provided here the heterogeneous incentive structure amplifies conditional cooperation which may thus explain the decline in contributions identified in Table 6 and, in turn, explain the lower contributions in heterogeneous treatments over all. Both large own effects (i.e., when one receives higher payoffs from contributions from one's own type) and large positive externalities (i.e., the other type benefitting more from one's own contribution) increase conditional cooperative behavior while dependence effects (i.e., oneself benefitting more from the other type's contributions) do not. This may have implications for the settings in which reciprocity can be encouraged.[36]

4. Conclusion

We conduct a linear public goods game in which we vary how effectively individuals can provide the good and how much they benefit from its provision. In so doing, we study the effects of marginal productivity heterogeneity and marginal benefit heterogeneity on collective action to provide a public good. Our findings provide insight into the effects of heterogeneity on collective action public good provision in general, and on climate change mitigation in particular. Unlike previous experimental studies related to climate mitigation, we implement heterogeneity at the marginal level, meaning that *the benefit of each token* contributed toward the public good varies. We systematically identify different types of heterogeneity and assess the effect of these types of incentive structures on collective action. There are three major findings: First, heterogeneity significantly impacts a group's ability to provide a public good. This finding runs counter to previous linear public goods experiments (Fisher et al., 1995). Alternative theories which hypothesize lower contributions in the presence of MPCR heterogeneity can only partially explain these results. This calls for additional study of heterogeneity effects.

Second, and perhaps most importantly, the *type* of heterogeneity matters. LOCAL heterogeneity does not result in significantly different contributions compared to the HOM treatment. While random effects regression analysis does not indicate statistical differences across the different heterogeneity treatments, pair-wise t-tests and Mann-Whitney tests do, suggesting that additional studies are warranted to determine qualitative differences in types of heterogeneity. Pairwise t-tests indicate that public goods provision is lowest when actors differ in how effectively they can provide a public good—mitigating climate change, for example. In this setting (MPH treatment), the inability to provide more of the public good depresses contributions by the less productive segment, which in turn reduces the willingness of the more productive segment to contribute toward the public good. This scenario mirrors a recurring dynamic observed in international climate change mitigation, where countries with the greatest potential to cut emissions, notably the United States, have been wary to engage in mitigation efforts so long as other, potentially less able, countries do not limit their greenhouse gases to a similar extent. While more effective in providing the public good than the MPH treatment, settings where the benefits of one's action accrue disproportionally to others (LONG treatment)—again reflective of climate change—also result in less collective action success than do other experimental treatments. When benefits of action disproportionally accrue locally (LOCAL treatment), groups are able to provide more of the public good. This makes them similar to homogeneous groups in securing collective action success. This might imply that climate change mitigation may have a higher likelihood of success if attempted from the bottom up, where benefits have direct impacts.

Third, our analysis indicates that contributions observed are critically dependent on heightened conditional cooperation, moderated by the incentive structure. In other words, while cooperation by others encourages reciprocal behavior (Fischbacher et al., 2001), the rate at which it does depends on the *sources* of contributions and how those determine an actor's *benefits*. Thus, rather than tracing contribution behavior back to non-linear MPCR effects (Weimann et al., 2019), it seems that higher contribution benefits to oneself and other group members increases the importance of reciprocity. This increased importance of conditional cooperation in turn leads to more rapid decay in contributions and consequently lower contributions in heterogeneity treatments. To relate this to climate change, if an individual's benefits are strongly affected by local cooperation, then her own level of contributions will depend to a greater extent on contributions taken at that local level (i.e., by fellow type members). In contrast, when one's actions benefit outside groups to a greater extent, the willingness to continue cooperating depends on reciprocity by the individuals being disproportionately benefited. This suggests that climate mitigation success may be more likely if reciprocity can be fostered and emphasized. The Paris Agreement allowing for highly visible, successive ramping up of mitigation goals may be helpful in that regard. However, *who* makes these commitments is a critical consideration in determining whether encouraging reciprocity will be effective.

The study was designed to distinguish between different types of heterogeneities in general, and to model the heterogeneity found in climate change mitigation in particular. As such, there are a few caveats that need mentioning with regard to the applicability of our results to climate change. Specifically, climate change mitigation is a social dilemma that is characterized by low MPCRs and large groups. While both of these factors, individually and in combination, can change contribution behavior (Diederich et al., 2016; Isaac et al., 1994; Isaac and Walker, 1988b; Nosenzo et al., 2015; Weimann et al., 2019) we argue that our results remain meaningful for the following reasons: First, by virtue of being an abstract experiment, the study identifies *general* behavioral tendencies. Results of the study should lead us to more carefully consider underlying heterogeneities to improve our response to collective action failures, regardless of the particular social dilemma in question. Second, seminal studies on the effect of climate-change-type incentive structures on collective action also utilize small groups to uncover behavioral sticking points in international climate negotiations (e.g., Milinski et al., 2008). Third, while group-size effects have been identified (see above), group returns (i.e., the summation of individual MPCRs) remains a strong predictor of collective action success. This suggests that our results may still be relevant even if not fully predictive of climate mitigation behavior. Further, it is important to highlight that, while international settings are sparse in enforceable agreements, the international climate setting is rife with institutions and norms (e.g., the non-binding but possibly effective Paris Agreement), which alter and complicate incentive structures compared to the underlying social

[36] As is the case in any analysis, the results are, to a certain extent, determined by the measurement of variables. As such it is possible that different clustering techniques and/or clustering variables may alter the size of effects. As yet there is no agreed upon clustering procedure to model behavioral tendencies. Future research will be necessary to determine clustering procedures that result in the most robust behavioral predictions.

dilemma. No experiment can capture real world incentive structures in their entirety, but can still provide insights into general behavioral tendencies. Finally, it may be the case that our results are moderated by changes in MPCR based on evidence of non-linear MPCR effects (Gunnthorsdottir et al., 2007; Weimann et al., 2019) and our use of relatively high MPCR values. Our results, however, show little evidence of these effects, suggesting that additional studies may be warranted to assess whether MPCR non-linearities also hold in the presence of heterogeneity. In general, we believe that external validity of results, ours or any other study's, arise from systematic replication and testing of conditions under which effects hold (McDermott, 2011). As such, we welcome additional studies expanding our understanding of heterogeneity typology and investigating the effects of different types of heterogeneity on collective action behavior.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Experimental instructions

Now that the experiment has begun, please make sure that your cell phones are switched off. **We ask that you do not talk with one another and do not turn around or look at other participants' screens.**

If you have a question after reading the instructions, please raise your hand and the experimenter will answer your question in private.

—

Welcome

You will receive $5 for showing up for this experiment. You can earn additional money by participating in this experiment. This experiment should not take more than 1 hour. You are free to leave at any time. However, if you choose to do so before the end of the experiment, you will only receive the $5 show-up fee. The total amount of money you can earn during the experiment depends on your decisions during this experiment, as well as the decisions of your group members, so please read the following instructions carefully.

Private decisions

Please note that your decisions and earnings are private. Your decisions are recorded using your experimental subject ID given to you by the experimenter, not your name or your student ID. At the end of the experiment, you will be asked to enter your name into the computer. This information is to process your payment only - it will not be used in any other way.

Payments

Your decisions will earn you **Experimental Currency Units** (ECUs). At the end of the experiment your ECUs will be exchanged into US dollars at a rate of 45 ECUs = $1. You will be paid in US dollars.

Stages

Today's experiment will consist of **two** stages. You will receive instructions at the beginning of each stage.

—

Stage 1 Instructions

Groups and member number

You have been randomly assigned to a group of 4 individuals for Stage 1. You will remain in this group until the end of Stage 1. Within the group, every member has been randomly assigned a member number 1 through 4. You will keep this number until the end of Stage 1.

You are member__

In Stage 1 you will make 14 decisions. The decision task is described below. Before making actual decisions that affect your earnings, you will answer a short quiz designed to check your understanding of the task.

Decision task

Every individual in your group has an Individual Fund, and your group of 4 has a Group Fund. Both your Individual Fund and the Group Fund have 0 tokens in them at the beginning of each decision setting. You and your group members will each receive 12 tokens for each decision setting. For each decision setting you must decide privately how many (if any) tokens to transfer to the Group Fund. The computer automatically places any tokens you did not transfer to the Group Fund in your own Individual Fund.

Unconditional Transfer Decision: In the first decision setting you must decide how many tokens to transfer when you do not know how many tokens your group members will transfer themselves.

Conditional Transfer Decisions: In the remaining settings you must decide how many tokens to transfer if your group members were to transfer, on average, a range of tokens. You will make a decision for every possible average level of transfers by your group members (0–12 tokens).

—

Returns to transfers

Each token that is in your Individual Fund will increase your earnings by 5 ECU. Each token that is transferred to the Group Fund will increase each group member's earnings by 3 ECU.

This means that your ECU earnings are calculated in the following way:

$$5\ \text{ECU} \times \text{Number of tokens in your Individual Fund} + 3\ \text{ECU} \times \text{Number of tokens in the Group Fund}$$

Earnings stage 1

You will not receive earnings for every decision you make in Stage 1. Instead, at the end of the experiment, the computer will calculate your payoffs for Stage 1 as follows:

One member will be randomly selected to be the "conditional" member. The three other members are the "unconditional" members.

For the unconditional members payoffs are calculated using their unconditional transfer decision.

For the conditional member his or her payoffs depend on the transfer decision made in the setting that corresponds to the average transfers (rounded to the nearest whole number) made by the unconditional members.

On the next screen you will find an example to illustrate these calculations.

To pick the conditional member the experimenter will draw a card from a shuffled deck of 4 cards (Ace through 4) with each card corresponding to a member number. Each member has the same probability of being picked. The draw will take place in public at the front of the room. The conditional member number that is picked is the same for every group in the room.

—

Stage 2 Instructions

You have completed Stage 1 of the experiment. Stage 2 consist of 15 decision rounds. The decision task is similar to that in Stage 1 but with important differences. Before making actual decisions that affect your earnings, you will answer another short quiz designed to ensure you understand the decision task.

Groups and member number

For Stage 2 you have been randomly assigned to a new group of 4 individuals. You will remain in this group until the end of Stage 2. Within the group, every member has been randomly assigned one of the following color and number combinations: Purple 1, Purple 2, Orange 1, and Orange 2. You will keep this color and number until the end of Stage 2.

You have been assigned type and number____.

—

Decision task

As in Stage 1, in every round of Stage 2 you will privately decide how to distribute a number of tokens between the Group Fund and your Individual Fund. (Remember: Any tokens not transferred to the Group Fund will be automatically placed in your Individual Fund.)

In Stage 2 you will receive 25 tokens each round with which to make your transfer decision.

Once everyone has made his or her decisions in a round, you will receive information about:

- Your transfer decision for that round,
- Your earnings for that round,
- The total number of tokens transferred to the Group Fund for that round, and
- The individual transfers to the Group Fund by all other group members.

A history of this information is available in a table while you make your decision.

—

Earnings in Stage 2 ***(HOM Treatment)***

Your Earnings in Stage 2 will be the sum of your per round earnings.

Returns to each token transferred have changed from Stage 1:

- Each token that you place in your Individual Fund will increase your earnings by 1 ECU.
- Every token that is moved to the Group Fund will increase every group member's earnings by 0.6 ECU each.

This means that your ECU earnings are calculated in the following way:

$$1\text{ ECU} \times \text{Number of tokens in your Individual Fund} + 0.6\text{ ECU} \times \text{Number of tokens in the Group Fund}$$

Please raise your hand if you have any questions.

On the next screen you will see some examples of how earnings are calculated.

—

Earnings in Stage 2 ***(MBH Treatment)***

Your Earnings in Stage 2 will be the sum of your per round earnings.

Returns to each token transferred have changed from Stage 1. Earnings differ for type Purple and type Orange individuals:

Purple

- Individuals receive 1 ECU for every token in his or her Individual Fund.
- And 0.4 ECU for every token in the Group Fund.

Orange

- Individuals receive 1 ECU for every token in his or her Individual Fund.

- And 0.8 ECU for every token in the Group Fund.

This means that your ECU earnings are calculated in the following way:
For PURPLE:
1 ECU × Number of tokens in your Individual Fund
+
0.4 ECU × Number of tokens in the Group Fund
For ORANGE:

$$1\ \text{ECU} \times \text{Number of tokens in your Individual Fund} + 0.8\ \text{ECU} \times \text{Number of tokens in the Group Fund}$$

Please raise your hand if you have any questions.
On the next screen you will see some examples of how earnings are calculated.
—

*Earnings in Stage 2**(MPH Treatment)***
Your Earnings in Stage 2 will be the sum of your per round earnings.
Returns to each token transferred have changed from Stage 1:

- Individuals receive 1 ECU for every token in his or her Individual Fund.
- Every group member receives 0.4 ECU for every token transferred to the Group Fund by PURPLE group members.
- And 0.8 ECU for every token transferred to the Group Fund by ORANGE group members.

This means your earnings are now calculated as follows:

$$1\ \text{ECU} \times \text{Number of tokens in your Individual Fund} + 0.4\ \text{ECU} \times \text{Number of tokens in the Group Fund transferred by PURPLE members} + 0.8\ \text{ECU} \times \text{Number of tokens in the Group Fund transferred by ORANGE members}$$

Please raise your hand if you have any questions.
On the next screen you will see some examples of how earnings are calculated.
—

*Earnings in Stage 2**(LOCAL Treatment)***
Your Earnings in Stage 2 will be the sum of your per round earnings.
Returns to each token transferred have changed from Stage 1. Earnings differ for type Purple and type Orange individuals:
Purple

- Individuals receive 1 ECU for every token in his or her Individual Fund.
- Purple individuals receive 0.8 ECU for every token transferred to the Group Fund by PURPLE group members.
- And 0.4 ECU for every token transferred to the Group Fund by ORANGE group members.

Orange

- Individuals receive 1 ECU for every token in his or her Individual Fund.
- Orange individuals receive 0.8 ECU for every token transferred to the Group Fund by ORANGE group members.
- And 0.4 ECU for every token transferred to the Group Fund by PURPLE group members.

This means that your ECU earnings are calculated in the following way:
For PURPLE:

$$1\ \text{ECU} \times \text{Number of tokens in your Individual Fund} + 0.8\ \text{ECU} \times \text{Number of tokens in the Group Fund by PURPLE} + 0.4\ \text{ECU} \times \text{Number of tokens in the Group Fund by ORANGE}$$

For ORANGE:

$$1\ \text{ECU} \times \text{Number of tokens in your Individual Fund} + 0.8\ \text{ECU} \times \text{Number of tokens in the Group Fund by ORANGE} + 0.4\ \text{ECU} \times \text{Number of tokens in the Group Fund by PURPLE}$$

Please raise your hand if you have any questions.
On the next screen you will see some examples of how earnings are calculated.
—

*Earnings in Stage 2**(LONG Treatment)***
Your Earnings in Stage 2 will be the sum of your per round earnings.
Returns to each token transferred have changed from Stage 1. Earnings differ for type Purple and type Orange individuals:
Purple

- Individuals receive 1 ECU for every token in his or her Individual Fund.
- Purple individuals receive 0.4 ECU for every token transferred to the Group Fund by PURPLE group members.
- And 0.8 ECU for every token transferred to the Group Fund by ORANGE group members.

Orange

- Individuals receive 1 ECU for every token in his or her Individual Fund.
- Orange individuals receive 0.4 ECU for every token transferred to the Group Fund by ORANGE group members.
- And 0.8 ECU for every token transferred to the Group Fund by PURPLE group members.

This means that your ECU earnings are calculated in the following way:
For PURPLE:

$$1\text{ ECU} \times \text{Number of tokens in your Individual Fund} + 0.4\text{ ECU} \times \text{Number of tokens in the Group Fund by PURPLE} + 0.8\text{ ECU} \times \text{Number of tokens in the Group Fund by ORANGE}$$

For ORANGE:

$$1\text{ ECU} \times \text{Number of tokens in your Individual Fund} + 0.4\text{ ECU} \times \text{Number of tokens in the Group Fund by ORANGE} + 0.8\text{ ECU} \times \text{Number of tokens in the Group Fund by PURPLE}$$

Please raise your hand if you have any questions.
On the next screen you will see some examples of how earnings are calculated.

Appendix B. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ecolecon.2021.106998.

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