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Economic Dynamics and Distributive Choices

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Abstract

This study investigates how economic dynamics influences distributive choices. In the first part of our experiment, an initial surplus is shared unequally between participants, based on the outcome of a real-effort task. In the second part, participants make redistribution decisions involving a new surplus that may be smaller (degrowth treatment), larger (growth treatment), or equal to the surplus of the first part. The results show that the 'rich' (those who received a high share in the first part) are not affected by the dynamics of the surplus, while the 'poor' demand more when the surplus decreases. These findings highlight the role of economic dynamics in shaping distributive tensions, particularly under conditions of inequality.

Keywords: Inequality, distributional preferences, distributive conflicts, reference-dependent preferences, social preferences

JEL classification: D31, C91, O49, I30

1 Introduction

Preferences regarding the distribution of collective surplus are central to many economic interactions. This is particularly crucial in the context of inequality, where disagreements are likely to occur and may involve large costs. Examples can be found at all levels of the economy: at the macroeconomic level, redistributive policies shape the distribution of income (Bozio et al., 2024) and may reflect individuals' preferences for more or less inequality (Maréchal et al., 2025). At the microeconomic level, for example within firms, conflicts frequently arise over how to divide the value added between workers and owners, or between different groups of workers (Breza et al., 2017; Pfeffer, 2007). The same is true in the private sphere, for instance within households, where resources and chores must be shared between spouses with different time resources and market wages.

Preferences and economic circumstances jointly determine distributive choices (Alesina and Giuliano, 2011). Keeping economic circumstances fixed, social preferences, including inequality aversion and political preferences, are important explanations for distributive preferences and choices (see e.g. Fong, 2001; Kerschbamer, 2015; Fisman et al., 2017; Kerschbamer and Müller, 2020; Epper et al., 2024; Fehr et al., 2024). Regarding economic circumstances, the literature has shown that the two most important determinants of distributional decisions are probably the level of inequality (Cavaillé, 2023) and how fair (or legitimate) this level of inequality is perceived to be (Durante et al., 2014).

In this paper, we address an important question: whether and how general economic dynamics affect distributive choices. We are particularly interested in comparing decisions made when the resources to be shared become more abundant as opposed to scarcer. This question is clearly relevant at the macro-level, where growth determines the collective surplus to be shared. For instance, Fisman et al. (2015) argue that economic crises are often accompanied by the rise of political movements advocating major changes in redistributive politics, either in favor of more redistribution (Occupy Wall Street in the US, the "yellow vests" in France) or less (the Tea Party or, to a certain extent, Trump in the US after an episode of severe inflation). This suggests that distributive preferences may be influenced by economic dynamics. At the microeconomic level, the surplus to be shared between stakeholders in a firm varies depending on its economic performance: for instance,

it may decrease when the firm is confronted with the entry of new competitors into the market, suffers from poor strategic decisions, or faces increased regulatory constraints.

Theoretically speaking, there are several reasons why the dynamics of collective surplus may impact distributive choices. First, with a diminishing surplus, the trade-off between equality and individual enrichment is sharpened, since in such a context reducing inequality necessarily implies decreasing the surplus allocated to at least *some* stakeholders. Secondly, there is well-established evidence that people have reference-dependent preferences (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), and that recent income often serves as a reference point (Benistant and Suchon, 2021). Previous studies have shown that the trade-off between one's own income and others' income depends on how current income compares to reference points, and people who have recently experienced private income loss tend to behave more selfishly (Benistant and Suchon, 2021). However, only a few studies, summarized below, have addressed the effect of the dynamics of collective surplus on distributive choice in the context of inequality.

We address this question using a lab experiment. Our experiment varies the dynamics of the surplus to be shared and measures how distributive choices in a subsequent dictator game with inequality are affected when the economic surplus increases, decreases, or remains stable. We introduce inequality because it is a necessary condition for redistribution to be meaningful and one's position in the income distribution can interact with economic dynamics to influence distributive choices. This setup reproduces, in a stylized way, patterns observed at the macroeconomic level, but also at the microeconomic level, where firms are composed of individuals with different status and income.

In the first part of the experiment, participants perform a real-effort task. We then randomly form pairs of participants, who go through two steps. In the first step, we split a given surplus between the participants: the best performer in the pair receives 75% of the aggregate surplus, while the other receives 25%. This creates inequality within pairs and establishes reference points in terms of both collective surplus and individual income. The split is identical across treatments, so the initial level of inequality is held constant. In the second step, participants are told they will have to split an additional collective surplus. Each participant in the pair proposes a split, and one of the two proposals is implemented

at random. They make two such proposals: one that, if implemented, would apply to their own pair, and another that would apply to a randomly chosen different pair. Depending on the treatment, the aggregate surplus in the second step can be smaller (*Degrowth* treatments), larger (*Growth* treatments), or equal (*Stable* treatment) to the aggregate surplus exogenously split in the first step. Comparing the splits in the second step across treatments allows us to capture the effect of collective surplus dynamics on distributive choices between the rich and the poor.

Using a lab experiment is a good way to study the effect of the dynamics of the collective surplus on distributive choices. First, a lab experiment allows us to manipulate the dynamics of the surplus exogenously. Second, our design allows us to control for other potential determinants of distributive choices, which would have been difficult to achieve with natural data. For instance, we hold constant the level of inequality and the perceived fairness of initial inequalities across treatments. In addition, given how we induce inequality and determine who is rich and poor in our experiment, status is unlikely to be correlated with social preferences. We can therefore rule out differential social preferences between the rich and the poor. This allows us to cleanly identify the causal effect of surplus dynamics on the distributive choices of the rich and the poor.

Across treatments, we find that the rich claim a greater fraction of the aggregate income than the poor. This suggests that the rich feel a stronger entitlement to the aggregate surplus than the poor, confirming that the inequality induction phase was successful in establishing status. On the effect of the economic dynamics *per se*, our results indicate that only the choices of the poor are affected by changes in the aggregate surplus: they tend to claim a larger share when the surplus diminishes (*degrowth* treatments). By contrast, the rich claim a very similar share of the aggregate income across treatments. In addition, we observe that participants are prone to a self-serving bias as individuals are generally more greedy when making choices that directly affect themselves. But, we find no significant differences in this respect between treatments. Overall, our results show that the dynamics of collective surplus can shape the distributive conflict between the rich and the poor, potentially calling for specific policy responses.

Our results add to several strands of the literature. First, they contribute to the scant literature on the effect of aggregate income dynamics on distributive choices. Benistant and Suchon (2021) find that dictators who recently experienced a negative income shock become more selfish. In their study, no pre-existing inequality is introduced. Therefore, our results extend this finding by showing that inequality matters, and that the effect of income dynamics depends on status: in our case, only the distributive choices of the poor are affected by income dynamics.

Closely related to our paper, Hochleitner (2025) studies how negative income shocks impact distributive choices in pairs with inequality. Similar to us, she finds that earning history does matter. However, she also finds that the distributive choices of the rich, but not the poor, are affected by earning histories: when the rich experience a negative income shock, they become less generous, while when the recipient experiences a shock, the rich become more generous. Gagnon et al. (2021) report consistent results with a similar design. Importantly, Hochleitner (2025) finds no effect of income shocks on generosity in scenarios where both participants suffer an income shock. While both her paper and ours agree that external shocks may impact distributive choices under inequality, her design does not implement, strictly speaking, economic dynamics, but rather external (and possibly unequal) income shocks. In addition, in Hochleitner (2025), distributive choices are made over the total amount brought about by both participants, while in our experiment, distributive decisions are made over an additional surplus provided by the experimenter. This may generate different levels of entitlement. For instance, in her design, because the surplus is the result of effort, the rich may feel more entitled to dispose of it as they see fit, which may explain why they react more strongly to the treatment. Moreover, in her experiment, status induction leaves considerable room for participants to interpret their status self-servingly, i.e., as a consequence of merit for the rich and of luck for the poor. This may lead the poor to disregard the status-based initial allocation and seek to enforce equality irrespective of income dynamics. Our status induction arguably leaves much less room for motivated interpretations of status. Finally, in her experiment, shocks are attributed to one or both members of the pair, while in our case, the dynamics of the surplus cannot be attributed to either member. This attribution difference may trigger distinct notions of solidarity across our experiments.

Second, because a negative income dynamic can be thought of as a violation of one's reference income, our paper also contributes to the literature on social preferences in the loss domain (Thunström, 2019; Cochard et al., 2020; Cochard and Flage, 2024; Boun My et al., 2018; Fiedler and Hillenbrand, 2020; Antinyan et al., 2024). Some papers find that individuals become more generous in the loss domain (Thunström, 2019; Cochard et al., 2020; Cochard and Flage, 2024), while others find the opposite (Boun My et al., 2018; Fiedler and Hillenbrand, 2020; Drouvelis and Gavassa-Pérez, 2024). A potential explanation for this discrepancy is that the way losses are induced and the details of the framing solicit different norms (Antinyan et al., 2024). Namely, participants are more generous when they have to share a loss (i.e., when they actively reduce the payoff of the recipient) than when they share what remains of their endowments after experiencing a loss. While showing that reference points matter for social preferences, this strand of literature did not consider income dynamics as a way to induce losses, nor did it introduce inequality. Therefore, the interaction between income dynamics and status is not precisely documented by this literature either.

The remainder of this paper is organized as follows. Section 2 describes the experimental design and introduces our main hypotheses, Section 3 presents our results, and Section 4 concludes.

2 Experimental design and hypotheses

2.1 Experimental design

Our experimental design consists of three parts. In the first part, participants performed a real effort task. In the second part, participants were randomly matched in pairs and played a modified dictator game with inequality. The last part consisted of sociodemographics questionnaires.

2.1.1 Part 1: Real-effort task

Participants had to count zeros in matrices composed solely of zeros and ones (Abeler et al., 2011). They had 6 minutes to correctly count zeros in as many matrices as they can. The number of matrices correctly solved is our measure of performance. To make sure that

participants exert effort, they were told that their payoff for the rest of the experiment will be partly determined by their performance in the real effort task. However, the information was limited, and we did not specify that their *relative performance* will matter to avoid triggering competition. Participants were not informed of their performance: each time they validated an answer, a new matrix was presented, without any indication of whether the previous answer was right or wrong.

Real-effort tasks are commonly used in experimental economics research (Charness et al., 2018). "Counting zeros" presents several advantages. Firstly, it is tedious and not intrinsically interesting. Second, no gender effect is observed (Lezzi et al., 2015). Importantly, resolving this specific task does not require advanced cognitive abilities, which is a good feature because cognitive abilities might be correlated with social preferences (Burks et al., 2009; Chen et al., 2013).

2.1.2 Part 2: Modified dictator game

At the beginning of the second part, participants were matched with another anonymous and randomly chosen participant in their session for the duration of this part. The second part had 2 steps. In the first step, participants were told that an amount of money (*TotalAmount1*) would be distributed in each pair. The value of *TotalAmount1* was exogenous and, in particular, did not depend on participants' performance in the previous part. However, the distribution depended on relative performances: participants were informed that, within each pair, the participant who solved the most matrices during Part 1 would receive 75% of the amount (rich), while the other one would receive 25% (poor).[1] Therefore, the status within the pairs explicitly depends on performance in Part 1. We did so to reinforce the legitimacy of inequality, and to make participants feel that they have deserved their earnings, potentially in an asymmetric way between the rich and the poor. However, it means that status is not totally exogenous, and therefore differences in behavior of the rich and poor might be due to unobserved characteristics rather than our experimental manipulation. In Appendix D, we show that this concern is unfounded: the distribution of the performances of the poor and rich overlap very significantly, and our

[1] We refer here to rich and poor to ease understanding, but no such words were used in the instructions.

results hold if we focus on participants belonging to the overlap, for whom the status is as-good-as-random.

The first step had two main goals. The first one was to induce inequality between participants. The level of inequality is constant across pairs: the highest performer always had 75% of *TotalAmount1*, while the lowest performer always received the remaining 25%. The second one was to generate a reference point in terms both of the total surplus distributed and how the surplus was distributed.

In the second step, participants were informed that they would decide how to share an additional amount (*TotalAmount2*) between the two members of a pair. Between treatments, this additional amount could be greater, smaller, or equal to the amount distributed in the first step, thereby inducing variation in aggregate income dynamics. Distributive choices were elicited under two scenarios. In the involved scenario, participants' decisions applied to their own pair. In the neutral scenario, their decisions applied to another randomly chosen pair in the session. The order of these two scenarios was randomized at the participant level to allow testing for carry-over effects. Each participant made choices in both scenarios, but they were informed that only one decision would ultimately be payoff-relevant. Specifically, at the end of the experiment, one participant in each pair was randomly selected, and then one of the two scenarios (involved or neutral) was randomly drawn. The selected participant's decision in the chosen scenario determined the actual payoffs for the relevant pair. We collected both involved and neutral decisions to measure individuals' degree of self-serving bias.[2]

Treatments

Our objective is to explore how redistributive choices are affected by the surplus dynamics. To achieve this objective, we varied the surplus in both steps of Part 2, *TotalAmount1* and *TotalAmount2*, between treatments. Table 1 summarizes the different values we set for *TotalAmount1* and *TotalAmount2* across treatments. In the *Growth* and *Growth'* treatments, *TotalAmount1* was greater than *TotalAmount2*, i.e. the aggregate income was growing between step 1 and step 2. In the *Degrowth* and *Degrowth'* treatments, *TotalAmount1* was smaller than *TotalAmount2*, i.e. the aggregate income decreased

[2]See Appendix F for screenshots of decision interfaces.

between step 1 and step 2. Finally, in the *Stable* treatment, the income was constant across steps.

Table 1: Treatment conditions regarding aggregate income dynamics

Treatment condition	*TotalAmount1*	*TotalAmount2*	Income dynamic	Observations
Growth	10€	20€	Increasing	82
Degrowth	20€	10€	Decreasing	80
Stable	15€	15€	Stagnating	98
Growth'	10€	15€	Increasing	102
Degrowth'	20€	15€	Decreasing	96

Comparing *Growth*, *Degrowth*, and *Stable* allows us to identify the effect of the dynamic of income while holding the sum of the amounts distributed across steps (*TotalAmount1* + *TotalAmount2*) constant. However, and because we wanted to control for the effect of the total surplus to be distributed across parts, *TotalAmount2* is necessarily different between *Growth*, *Degrowth* and *Stable*. Previous evidence shows that increasing the endowment in the dictator game might increase selfishness, with participants giving a smaller share of their endowment (Engel, 2011; Larney et al., 2019). In our case, this would lead to more generosity in *Degrowth* than in *Growth*. Therefore, to assess the role of the amount to be shared in the second step, we ran treatments *Growth'* and *Degrowth'* where we hold the amount to be shared in the second step (*TotalAmount2*) constant. Note that keeping *TotalAmount2* constant means that the total surplus distributed across steps cannot be held constant, and is in fact, larger in the *Degrowth'* than in any other treatment. This difference is necessary to explore the effect of keeping *TotalAmount2* the same across treatments. In addition, we wanted to avoid the total surplus being too different between *Growth'* and *Degrowth'* (as well as compared to *Growth* and *Degrowth*), which is why we chose to implement a relatively small variation between *TotalAmount1* and *TotalAmount2*, a variation that is necessarily smaller between *Growth'* and *Degrowth'* than between *Growth* and *Degrowth*.[3]

[3]To illustrate this, if we wanted to keep the change in total surplus the same as in *Growth* or *Degrowth*, total surplus would need to be as low as €20 in *Growth* and as high as €40 in *Degrowth'*.

Individual satisfaction regarding distribution

We asked participants to report their satisfaction regarding the distribution of incomes in their pair at three different points in time. The first elicitation occurred just after the first step; therefore, it concerned only the exogenous allocation in the first step. We conjecture that satisfaction at this point could impact distributive choices in the second step, therefore we control for it in our regressions.

We also elicited satisfaction just after the second step, about the distribution arising from the choice implemented in their pair, and also regarding the distribution of total payoffs (from the first step and the second step of Part 2). Last, at the end of the experiment, participants were presented with the distribution of payoffs in their sessions and were asked how satisfied they were about it. These elicitations were done for exploratory purposes, and are notably endogenous to previous decisions. Therefore we will not use them in our regressions. Details on satisfaction at these different points in time are presented in Appendix E.

Participants reported their satisfaction on a 7-points Likert scale, from 1 corresponding to being extremely dissatisfied to 7 corresponding to being extremely satisfied. We add smileys whose facial expressions reinforced the meaning of each point. These reports were not incentivized (this method is inspired by Diaz et al. 2023).

2.1.3 Part 3: socio-demographics

In Part 3, participants were asked to complete a sociodemographic questionnaire including information on gender, age, occupation, amount of monthly consumption expenditures, and the number of times they had taken part in a laboratory experiment in the past.

2.2 Procedure

Our experiment was developed using oTree (Chen et al., 2016). All sessions were conducted Anthropo-Lab (Lille, France) during winter 2023/2024. We ran twenty-three sessions in total, five sessions for *Growth'*, *Degrowth'* and *Stable*, and four sessions for *Growth* and *Degrowth*, with 16-24 subjects in each. Participants were recruited using ORSEE (Greiner, 2015). In total, 458 subjects took part in the sessions: 98 in *Stable*, 82 in *Growth*, 80 in *Degrowth*, 102 in *Growth'* and 96 in *Degrowth'*. Our sample allows to detect medium

effect size with a power of 80% and a critical α of 0.05 (see Appendix A for details). Details on sociodemographic characteristics across treatments are provided in Table B.1 of Appendix B.

At the beginning of the sessions, general instructions were displayed on the screen and read aloud by the same experimenter. During the experiment, instructions for each part were only displayed on participants' screens (see Appendix F). Since the experiment is structured into three different parts, displaying specific instructions progressively facilitates understanding of each parts. Sessions lasted around 20 minutes on average, and the mean payoff was approximately €18 show-up fee included (min: €5.5, max: €33, sd: €6.22). Payments were made via Lydia, a very popular french banking app.

2.3 Hypotheses

Our hypotheses on distributive choices are based on two important behavioral traits: inequity aversion, where individuals agree to support a private cost to limit inequality (see e.g. Fehr and Charness, 2025); and reference dependence, where income is evaluated relative to a reference point, such as recent earnings, rather than in isolation (e.g. Kahneman and Tversky, 1979; Tversky and Kahneman, 1992).

Inequity aversion is why we expect most individuals to share some of the surplus with the other member of their pair in the second step. However, the dislike for inequality is determined by perceptions of legitimacy: when inequalities are deemed legitimate, people are more likely to accept some inequality (see e.g. Konow, 2000; Cappelen et al., 2007, 2020). In our experiment, because status is based on individual performance in a real-effort task, participants might think that inequality has some legitimacy. These considerations lead us to our first hypothesis.

Hypothesis 1: *The rich keep a higher share of the surplus for themselves than the poor.*

This hypothesis corresponds to testing the legitimacy of inequality. Note, however, that we do not hypothesize that perceived legitimacy will be symmetrical between the rich and the poor: typically, perceived legitimacy may depends on one's position (e.g. Deffains

et al., 2016; Amasino et al., 2023, 2024). For instance, we do not expect the poor to keep a share as small as what they got in the first step, because they may perceive the inequality as excessive given the relative triviality of the real-effort task. Validating Hypothesis 1 is important for interpreting the rest of the experiment, as it ensures that the status and payoff induced in the first step impact behavior.

Our second hypothesis builds on the widely recognized fact that individuals have reference-dependent preferences. Here, we expect that the amount participants receive in step 1 will serve as a reference point. Therefore, participants may be reluctant to let their private payoff in step 2 deviate significantly from this reference (i.e. payoff in step 1). As a result, they may share less of the surplus in step 2 when the surplus is decreasing.

Hypothesis 2: *Participants keep a higher share of the surplus for themselves when the income dynamics are decreasing than when they are increasing.*

However, the effect of a decreasing surplus may depend on individuals' status. First, the psychological cost of drifting away from the reference point might depend on the level of the reference income itself. Second, for the poor, inequity aversion and reference dependence push in the same direction: both encourage more selfishness. This is not the case for the rich. For them, reference dependence would push to keep more to maintain their income level, while inequity aversion would push to transfer more to reduce inequality. Taken together, this leads us to formulate sub-hypothesis 2a.

Hypothesis 2a: *The effect of the decreasing income dynamics is stronger for the poor.*

We also have two hypotheses related to the difference between choices impacting oneself and choices impacting others. These hypotheses are based on the idea that individuals have different distributive preferences when they decide as an impartial spectator than when their decision will impact them (see e.g. Konow, 2000). The idea is simple: when choosing as impartial spectators, individuals tend to decide mostly based on fairness ideals including equality or equity. When their choices impact their own payoffs, self-interest gets in the way and push them to be more selfish. This lays the ground for Hypothesis 3.

Hypothesis 3: *Both the rich and poor keep a greater share of the surplus than they distribute to a member of another pair in the same role as them.*

Finally, we conjecture that self-serving bias is stronger in the loss domain.

Hypothesis 4: *Self-serving bias is stronger when the income dynamics decreases.*

This relies on the idea that reference-dependent considerations will be stronger for oneself rather than for others.

3 Results

3.1 Overview

First, we analyze choices participants make when distributing resources in their own pair. Table 2 reports the share of the aggregate surplus participants keep, separated by role and treatment. Note first that participants in both role keep significantly more than half the surplus for themselves, in all the treatments (all the p-values are lower than 1% in sign-rank tests, except for the poor in *Degrowth'* where $p < 0.1$). This is in line with what is observed in dictator games (see e.g. Engel, 2011). We also checked for order effect between the involved and neutral decision (e.g. Dengler-Roscher et al., 2018) and found that participants who first decided on a distribution for another pair kept a similar share than those who first decided for their own pair in all treatments and for all status, with only one exception (details are provided in Appendix C.1). We conclude that order effect are not likely to play a role and we therefore pool observations from both orders in our analysis.

3.2 Test of the experimental hypotheses

First, we observe that Hypothesis 1 holds, an essential part of our experiment, as it requires participants to consider the exogenous distribution of the first step to be a reference:

Result 1: *Hypothesis 1 is supported by the data - On average, the rich keep a greater share for themselves than the poor.* On average, the rich kept 70.3% of the endowment and the poor kept 65.6% (Wilcoxon Rank-sum Test: $n = 458$, $p < 0.001$). If we run

Table 2: Descriptive statistics on share of the surplus kept, over treatment and roles.

	Stable	*Growth*	*Degrowth*	*Growth'*	*Degrowth'*	Total
Poor						
Mean	.653	.605	.776	.595	.668	.656
Standard deviation	.226	.191	.244	.214	.235	.229
Number of observations	49	41	40	51	48	229
Rich						
Mean	.712	.68	.708	.733	.677	.703
Standard deviation	.198	.187	.254	.205	.214	.211
Number of observations	49	41	40	51	48	229
Total						
Mean	.682	.642	.742	.664	.673	.679
Standard deviation	.213	.191	.25	.22	.224	.221
Number of observations	98	82	80	102	96	458

separate Mann-Withney (MW hereafter) tests for each treatment, this result holds for every treatments except *Degrowth* and *Degrowth'* (for which $p > 0.10$, which is explained by different treatment effects on the rich and poor, as explained below). This result is important because it confirms that the first part was successful in inducing inequality and reference points.

Result 2: *Hypotheses 2 is supported by the data - Participants keep a greater share of the endowment for themselves under decreasing surplus dynamics.*

Participants kept on average 68.2% of their endowment in the *Stable* condition, 65.4% in the condition with increasing surplus (*Growth* & *Growth'*) and 70.4% in the condition with decreasing surplus (*Degrowth* & *Degrowth'*). Comparing transfers in conditions with an increasing surplus to those with a decreasing surplus, we find that the difference is significant (MW, $p = 0.031$). The same holds if we compare the share kept in *Growth* and *Degrowth* (64.2% vs 74.2%MW, $p = 0.009$). However, the difference is not significant when we focus on *Growth'* and *Degrowth'* (66.4% vs 67.%, $p = 0.57$).

Result 2a: *Hypothesis 2a is supported – the negative effect of a decreasing surplus is entirely driven by "poor" participants: the poor keep a greater share for themselves with*

decreasing income dynamics while the rich keep similar shares across treatments.

The share kept by the rich range from 67.6% in *Growth'* to 73.3% in *Degrowth'*. Overall, there is no statistically significant difference in the share kept by the rich across treatments (Kruskal-Wallis test: $p = 0.79$). None of the pairwise comparisons of shares kept by the rich across treatments yield a significant result either (All MW tests $p > .1$)

Regarding the choices of the poor, first of all, Table 2 shows important variations of the share kept by the poor across treatments. The overall between-treatment difference is significant (Kruskal-Wallis test: $p = 0.002$). Pooling data from treatments with decreasing trends (*Degrowth* and *Degrowth'*) on the one hand, and those with non-decreasing trend on the other hand (*Growth*, *Growth'* and *Stable*), we find that the poor keep 71% (n= 141) of the aggregate income when the latter is decreasing, and 61.7% (n=88) when it is non-decreasing (MW test: $p = 0.002$).[4] Focusing on *Growth* and *Degrowth* conditions, which keep the total of aggregate incomes constant across the income trend (€30), we find that the poor keep 77.5% in the *Degrowth* and 60.5% in the *Growth* condition (MW test: $p = 0.001$).[5] Focusing on the *Growth'* and *Degrowth'* conditions only, which keep the aggregate income in the second step constant (€15), we find that Player Bs keep 66.8% in the *Degrowth'* and 59.4% in the *Growth'* condition (MW test: $p = 0.082$). Note that the difference between *Growth'* and *Degrowth'* is somewhat smaller than the difference between *Growth* and *Degrowth*. A possible explanation is that the income trend is steeper in *Growth* and *Degrowth* than in *Growth'* and *Degrowth'*, as explained and justified in the design section. Overall, we conclude that this series of non-parametric tests supports Result 2a.

We further assess the robustness of Results 2 and 2a with regression analysis. We use OLS models to explain the share kept.[6] The outcomes of these regressions are reported

[4] Another way of pooling the treatment conditions would be to pool treatments with non-increasing trend (*Degrowth*, *Degrowth'* and *Stable*) and these with increasing trend (*Growth* and *Growth'*). Results are consistent: 69.4% *vs.* 59.9% (MW test: $p = 0.003$).

[5] The amount that participants are asked to distribute is greater in *Growth* and in *Degrowth*. Past evidence shows that higher endowments lead dictator to transfer lower share of their endowment (Engel, 2011; Larney et al., 2019). Therefore, this difference is unlikely to explain our results, and actually may counteract the negative effect of loss we observe.

[6] The use of Tobit models gives very similar results.

in Table 3. Models (1) and (2) use the full sample. Models (3) and (4) restrict the sample to the decisions of rich participants, and Models (5) and (6) restrict the sample to the decisions of the poor participants. Model (1), (3) and (5) estimates the effect of a dummy variable that indicates a decreasing surplus (*i.e.*, indicating an observation from either *Degrowth* or *Degrowth'*). Model (2), (4) and (6) estimates a categorical variable indicating the treatment, with *Stable* as the reference category. In every models, we control for sociodemographic characteristics (age, gender, number of previous participations in economic experiments, monthly consumption expenses, professional status) and performance in the first part of the experiment (*i.e.*, the number of correctly solved matrices). We also control for elicited satisfaction over the distribution of payoffs within the pair in Step 1, as we conjecture it may condition distributive choices in Step 2. In Columns (2), (4) and (6), we report the p-values of Wald tests comparing the estimates for the *Growth* vs *Degrowth* conditions, as well as for the *Growth'* vs *Degrowth'*. In column (6), both Wald tests yield a p-value smaller than 0.05, which supports our results.

The effect of satisfaction relative to the Step 1 distribution is also worth commenting on. Pooling all participants, it is positive: the more satisfied a participant is, the more they keep for themselves in Step 2. However, the effect is heterogeneous: it is positive for the rich but negative for the poor. Our interpretation is as follows: the more a participant is satisfied with the initial distribution, the more likely they are to reproduce it in Step 2, which means keeping more for the rich, and less for the poor.

Overall, we conclude that the dynamics of the surplus to be shared does indeed matter for distributive choice, but only for the relatively poor who claim a greater share of the aggregate surplus under decreasing income.

3.3 Results on the discrepancy in own-other pair decisions

We now turn to the comparison of the choices made for one's own pair with those made for another pair. For each individual, we compute the individual self-serving bias as the difference between the share kept and the share allocated to the player in the same Role in the pair that would be impacted by her neutral choice. This allows us to identify the role of objective preferences and self-serving motivations in distributive choices.

Table 3: Regressions on the share of the surplus kept

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Kept	Share Kept	Share Kept	Share Kept	Share Kept	Share Kept
Negative trend (pooled)	0.038^{+}		-0.040		0.098**	
	(0.022)		(0.035)		(0.030)	
Satisfaction	0.011*	0.011*	0.055***	0.055***	-0.046***	-0.044***
	(0.005)	(0.005)	(0.012)	(0.012)	(0.011)	(0.011)
Stable		Ref.		Ref.		Ref.
Growth		-0.041		-0.034		-0.039
		(0.030)		(0.036)		(0.043)
Degrowth		0.059^{+}		-0.025		0.111*
		(0.035)		(0.045)		(0.049)
Growth'		-0.018		0.017		-0.062
		(0.030)		(0.038)		(0.041)
Degrowth'		-0.013		-0.053		0.024
		(0.031)		(0.040)		(0.044)
Sample	rich & poor	rich & poor	rich	rich	poor	poor
p-value *growth – degrowth*		0.004		0.834		0.003
p-value *growth' – degrowth'*		0.861		0.093		0.043
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.019	0.033	0.137	0.144	0.115	0.136
Observations	458	458	229	229	229	229

OLS models. The dependent varialbe is the share of the available surplus kept by the participant. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$ Robust standard errors in parentheses. Controls include age, gender, number of previous participations in economic experiments, monthly consumption expenses, professional status and performance in the first part of the experiment (i.e. the number of correctly solved matrices). Negative trend (Pooled) is the effect of Degrowth and Degrowth' pooled.

Result 3: *Hypothesis 3 is supported – participants generally keep more for themselves than they allocate to players in the same role but in an other pair.*

Overall 199 participants (43.45%) show a self-serving bias (*i.e.*, they keep more for themselves than they attribute to the member of the other pair in the same role as them). 174 (37.8%) transfer exactly the same share, and 85 participants (18.56%) keep less than they attribute to the participants in the other pair in the same role as them. These proportions are not significantly different between treatments (χ^2 test,$p = 0.129$), or between the rich and poor (χ^2 test, $p = 0.881$). Mean self-serving biases by treatment and status are presented

Table 4: Descriptive statistics on self-serving bias over treatment and roles

	Stable	*Growth*	*Degrowth*	*Growth'*	*Degrowth'*	*Total*
Poor (player B)						
Mean	.102	.111	.0862	.0474	.121	.0926
Standard deviation	.264	.261	.213	.185	.2	.225
Number of observations	49	41	40	51	48	229
Rich (player A)						
Mean	.085	.029	.0631	.0758	.0726	.0665
Standard deviation	.211	.116	.248	.181	.237	.203
Number of observations	49	41	40	51	48	229
Total						
Mean	.0934	.07	.0747	.0616	.0967	.0795
Standard deviation	.238	.205	.230	.183	.219	.215
Number of observations	98	82	80	102	96	458

in Table 4. On average, participants keep 7.95% more for themselves than they give to the member of the other pair who is in the same role (sign-rank test, $p < 0.001$),

Result 4: *Hypothesis 4 is not supported –self-serving bias does not depend on treatment or status.*

We reported above that the proportions of individuals showing self-serving bias are not different across status our treatment. The same hold if we focus on mean self-serving bias across role (MW rank-sum test: $p = 0.201$). Similarly, the dynamics of the aggregate surplus has no impact on the intensity of the bias (Kruskal-Wallis test: $p = 0.804$). This result holds whether we consider the five treatments separatly or if we pool the treatments with increasing trends together. In Table C.3 in Appendix C.2, we confirm these results with a series of regressions.[7]

We conclude this result section by asking whether we can reproduce our main treatment effect, namely that the poor are more selfish when the aggregate surplus decreases, if we consider neutral choices instead of choice for one's own pair as our variable of interest. To do so, we run the exact same models as in Table 3, only changing the dependent variable, to

[7]The use of Tobit models gives very similar results.

explain distributive choices made for another pair. More precisely, the dependent variable here is the share of the surplus attributed to the participants with same status as the decision maker, but from another pair. The outcome of these regressions are presented in Table 5. It shows that our results are broadly reproduced if we focus on choices made for another pair, or "neutral" choices.

Table 5: Regressions on decisions for another pair

	(1) Share to same status	(2) Share to same status	(3) Share to same status	(4) Share to same status	(5) Share to same status	(6) Share to same status
Negative trend (pooled)	0.026		-0.019		0.070*	
	(0.020)		(0.035)		(0.028)	
Satisfaction	0.016**	0.016**	0.042***	0.042***	-0.028**	-0.024*
	(0.005)	(0.004)	(0.008)	(0.008)	(0.010)	(0.010)
Stable		Ref.		Ref.		Ref.
Growth		-0.018		0.022		-0.061
		(0.028)		(0.037)		(0.040)
Degrowth		0.077*		0.002		0.119*
		(0.036)		(0.048)		(0.050)
Growth'		0.014		0.027		-0.004
		(0.027)		(0.035)		(0.039)
Degrowth'		-0.017		-0.037		-0.006
		(0.026)		(0.037)		(0.037)
Sample	rich and poor	rich and poor	rich	rich	poor	poor
p-value *growth* – *degrowth*		<0.001		0.67		< 0.001
p-value *growth'* – *degrowth'*		0.21		0.07		0.95
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.051	0.074	0.096	0.099	0.122	0.166
Observations	458	458	229	229	229	229

OLS models. The dependent variable is the share of the available surplus attributed to the participant with the same role in another pair. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$ Robust standard errors in parentheses. Controls include age, gender, number of previous participations in economic experiments, monthly consumption expenses, professional status and performance in the first part of the experiment (i.e. the number of correctly solved matrices). Negative trend (Pooled) is the effect of *Degrowth* and *Degrowth'* pooled.

4 Conclusion

At both macro- and micro-levels, the aggregate surplus available for distribution among economic agents closely follows economic fluctuations, with especially marked effects during crises. These dynamics may therefore have substantial implications for redistribution preferences. Using a lab experiment, we studied how such dynamics affect distributive choices, in particular whether negative trends make people more or less pro-social. Our study introduces variation in a dictator game to capture the interaction between aggregate surplus dynamics and inequality. Our main result is that the dynamics matter: the poor become more selfish when aggregate income decreases, while the pro-sociality of the rich is unaffected. Importantly, the effect of aggregate surplus dynamics is observed not only when distributive decisions impact the decision maker, but also when the decision concerns another pair and therefore has no direct consequence for the decision-maker.

Our findings complement the experimental literature on the effect of aggregate income dynamics on pro-sociality. They are consistent with studies showing that negative trends reduce generosity (Benistant and Suchon, 2021) and extend these results to situations where inequality is present. At the same time, our results stand in contrast to those of Hochleitner (2025), which provide a different perspective. In an online experiment, she finds that the rich are more affected than the poor by negative income shocks: they become more selfish when they experience the shock themselves and more generous when the recipient experiences it. Notably, she reports no effect when both rich and poor experience a shock, which is the situation closest to ours.

Several design differences may account for these contrasting results. First, in our experiment aggregate income dynamics are manipulated by reducing the surplus to be shared in the dictator game, which limits feelings of entitlement over the surplus. In contrast, in Hochleitner (2025) participants experience shocks on their own income before making a transfer. Second, income trends in her study take the form of shocks attributed to individual participants, which is not the case in our design. This higher scope for attribution may trigger different notions of solidarity than in our experiment, where negative income trends cannot be attributed to either participant in a pair. Third, in her experiment the induction of status leaves ample room for motivated appraisal. As explained by the author,

the rich tend to interpret status as merit-based, while the poor interpret it as luck-based. In our experiment, such motivated beliefs play a more limited role.

Both our results and those of Hochleitner (2025) and Gagnon et al. (2021) confirm that aggregate income dynamics influence distributive choices. An important implication is that periods of crisis may require specific taxation or redistributive policies to preserve social cohesion. At the same time, the sharply contrasting results suggest that the precise effect of income dynamics on distributive choices depends on the perceived source of status (merit or luck), feelings of entitlement over the surplus, and attribution of shocks. Taken together, this discussion highlights the need for further research on the interaction between income dynamics and status in shaping pro-social choices. The role of attribution and entitlement in particular appears to be a promising avenue for designing policies that preserve social cohesion in times of crisis.

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A Statistical Power

We perform our statistical power analysis with GPower. Figure A.1 below reports the total number of observations required to achieve a power of 0.8 in a Mann-Whitney test, as a function of the effect size.

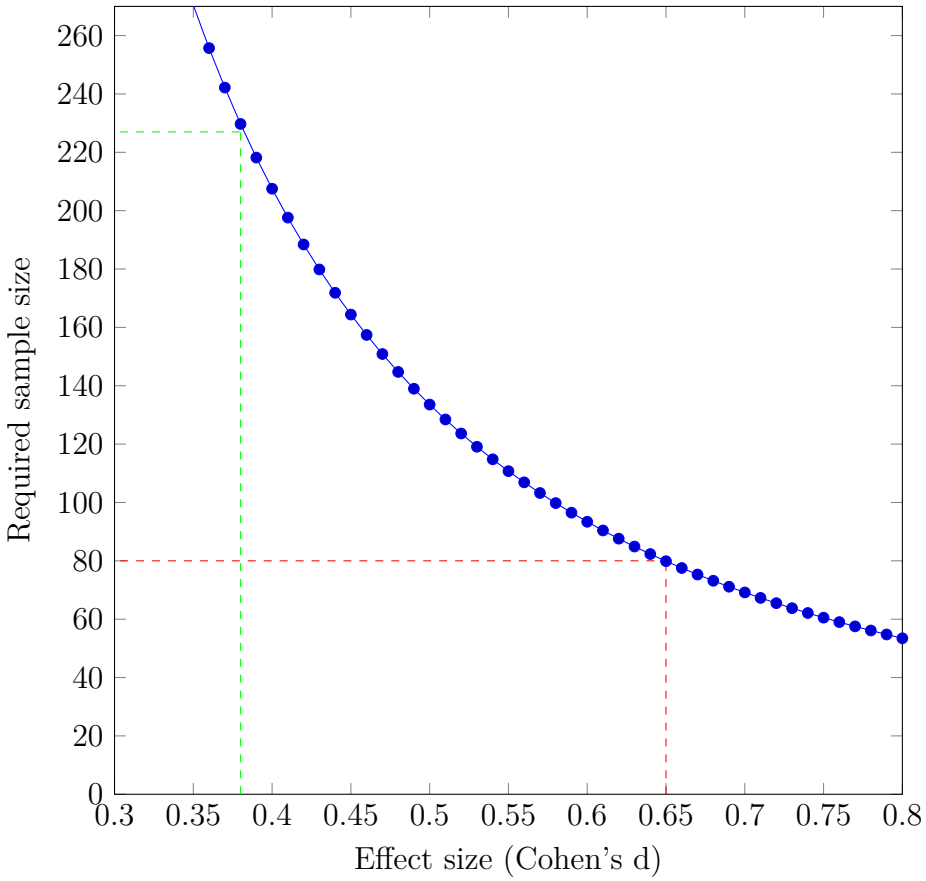


Figure A.1: Required sample size to reach a power of .8 as a function of the effect size.

Distributive choices of the rich *vs* the poor. If we pool all the treatments together, our sample includes 227 choices made by poor participants and 227 choices made by rich participants. Figure A.1 shows that we have a .80 power to detect effect size larger that $d = .38$ (green lines). If we consider the treatment for which we have the fewest observations (80 observations overall), we have a .80 power to detect and effect size of at least $d = .65$ (red lines). Both these values are generally considered to be medium effect sizes.

Distributive choices depending on the dynamics of the surplus. If we pool the treatments according to the dynamics of the economic surplus, we have 184 observations for both growing (*Growth* + *Growth'*) and decreasing (*Degrowth* + *Degrowth'*) dynamics. Considering this sample, we have a power of .8 to detect an effect size of $d = .42$. If we now consider the comparison of the choices of the "poor" between *Growth* and *Degrowth* - the combination for which we have fewest observations, with 81 observations- we have a power of .8 to detect an effect size of at least $d = .65$ (red lines).

We conclude from this power analysis that our experiment has proper power to detect medium effect size, according to the classification of Cohen (2013).

B Summary statistics

Table B.1: Summary statistics

	Stable	*Growth*	*Degrowth*	*Growth'*	*Degrowth'*	Total	Differences
Age	21.81 (2.92)	21.89 (5.62)	21.75 (3.51)	22.50 (6.23)	21.21 (2.85)	21.84 (4.47)	*ns*
Sex							*all**
Male	21 (21.43%)	23 (28.05%)	9 (11.25%)	29 (28.43%)	21 (21.88%)	103 (22.49%)	*(Growth') vs (Growth)****
Female	77 (78.57%)	58 (70.73%)	70 (87.50%)	71 (69.61%)	74 (77.08%)	350 (76.42%)	*(Growth') vs (Degrowth)**
NA	0 (0.00%)	1 (1.22%)	1 (1.25%)	2 (1.96%)	1 (1.04%)	5 (1.09%)	*(Growth') vs (Degrowth')***
Student							*ns*
No	14 (14.29%)	7 (8.54%)	5 (6.25%)	9 (8.82%)	5 (5.21%)	40 (8.73%)	
Yes	84 (85.71%)	75 (91.46%)	75 (93.75%)	93 (91.18%)	91 (94.79%)	418 (91.27%)	
Experience							*ns*
None	26 (26.53%)	22 (26.83%)	11 (13.75%)	26 (25.49%)	26 (27.08%)	111 (24.24%)	
1	20 (20.41%)	11 (13.41%)	12 (15.00%)	17 (16.67%)	24 (25.00%)	84 (18.34%)	
2 & more	52 (53.06%)	49 (59.76%)	57 (71.25%)	59 (57.84%)	46 (47.92%)	263 (57.42%)	
Consumption expenditure							*ns*
Less than €500	43 (43.88%)	38 (46.34%)	34 (42.50%)	35 (34.31%)	45 (46.88%)	195 (42.58%)	
500-1000€	42 (42.86%)	35 (42.68%)	34 (42.50%)	49 (48.04%)	40 (41.67%)	200 (43.67%)	
More than 1000€	13 (13.27%)	9 (10.98%)	12 (15.00%)	18 (17.65%)	11 (11.46%)	63 (13.76%)	
Total	98 (21.40%)	82 (17.90%)	80 (17.47%)	102 (22.27%)	96 (20.96%)	458 (100%)	

Note: observations and frequency in % for categorical variables, mean and standard-deviation for continuous variables. Chi-2 test and Fisher exact's test for categorical variables, Kruskal-Wallis rank test for continuous variables.

C Additional tests

C.1 Test for order effects

Table C.1 and C.2 below reports the share kept by participants according to the elicitation order, i.e. whether participants decided for their own pair or another pair first. The only case in which we observe a significant difference between elicitation order is for the rich in the *Degrowth'* treatment, who keep more when they start by deciding for another pair (Neutral first). We have no good explanation, besides it being a statistical artifact.

Table C.1: Poor

	Stable	*Growth*	*Degrowth*	*Growth'*	*Degrowth'*
Own pair first					
Mean	.61	.598	.835	.658	.66
Number of nonmissing values	21	21	22	21	24
Neutral first					
Mean	.685	.613	.703	.551	.676
Number of nonmissing values	28	20	18	30	24
***p*-value diff. (MW test)**	.27	.46	.13	.19	.76

Table C.2: Rich

	Stable	*Growth*	*Degrowth*	*Growth'*	*Degrowth'*
Own pair first					
Mean	.714	.657	.719	.74	.615
Number of nonmissing values	28	20	18	30	24
Neutral first					
Mean	.709	.701	.699	.724	.738
Number of nonmissing values	21	21	22	21	24
***p*-value diff. (MW test)**	.89	.39	.80	.81	.03

C.2 Econometric analysis of the self-serving bias

Table C.3: Regressions regarding the self-serving bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Growth	0.033	0.031	0.056	0.059	0.009	0.007		
	(0.035)	(0.035)	(0.035)	(0.037)	(0.055)	(0.059)		
Degrowth	0.003	-0.002	0.022	0.027	-0.015	-0.023		
	(0.037)	(0.038)	(0.049)	(0.052)	(0.051)	(0.053)		
Growth'	-0.023	-0.033	0.009	0.015	-0.054	-0.055		
	(0.032)	(0.033)	(0.039)	(0.041)	(0.046)	(0.048)		
Degrowth'	0.016	0.017	0.012	0.029	0.019	0.023		
	(0.035)	(0.036)	(0.046)	(0.048)	(0.047)	(0.049)		
Degrowth trends							0.015	0.017
							(0.049)	(0.039)
Neutral first							-0.010	0.062
							(0.029)	(0.041)
Degrowth trends × Neutral first							-0.014	0.018
							(0.061)	(0.061)
Sample	rich & poor	rich & poor	rich	rich	poor	poor	rich	poor
p-value *Growth* – *Degrowth*	0.411	0.370	0.429	0.495	0.640	0.599		
p-value *Growth'* – *Degrowth'*	0.216	0.109	0.939	0.738	0.060	0.059		
Controls	No	Yes	No	Yes	No	Yes	Yes	Yes
R^2	0.006	0.046	0.009	0.052	0.014	0.039	0.045	0.048
Observations	458	458	229	229	229	229	229	229

OLS models. Dependent variable: self-serving bias. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$. Standard errors in parentheses. Controls include age, gender, number of previous participations in economic experiments, monthly consumption expenses, professional status and performance in the first part of the experiment (i.e. the number of correctly solved matrices).

D Robustness check

D.1 Performance and status.

In our experiments, the status (rich or poor) depends on one's performance in the real-effort task. We chose a real-effort task that does not require high cognitive skills to avoid that cognitive skill would confound our effects. However, it is still possible that the rich differ from the poor on unobserved characteristics that are not captured in the regressions presented in Table 3 in the main text. In this Appendix, we show that differences in unobserved characteristics of the rich and poor do not drive our results.

Figure D.1 plots the distribution of performances in the first task of the rich and poor. A striking pattern is that the distributions of performance of the poor and the rich largely overlap: 392 participants (85.6% of the sample) solved between 11 and 23 matrices – that is, at least as many as the lowest-performing rich participant and at most as many as the highest-performing poor participant. For these participants, their relative rank within a pair could have been different had they been matched with someone else. As a result, their status is as-good-as-random. Table D.1 below replicates the analysis reported in Table 3 but focuses on the subset of participants whose performance fall within the overlap. Despite a certain loss of power, our results are mostly replicated, therefore we are confident that our results are not driven by differences in unobserved characteristics of the rich and poor.

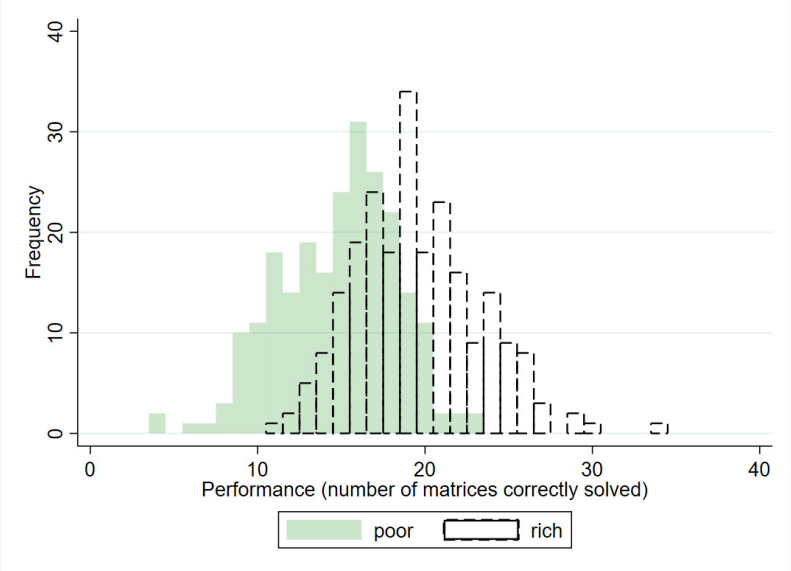


Figure D.1: The distribution of performances of the rich and poor in the first part of the experiment

Table D.1: Regressions on the share kept restricted to subsample of participants whose performance belongs to the overlap.

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Kept	Share Kept	Share Kept	Share Kept	Share Kept	Share Kept
Negative trend (Pooled)	0.047*		-0.018		0.101**	
	(0.023)		(0.038)		(0.032)	
Satisfaction	0.007	0.008	0.049**	0.049**	-0.052***	-0.050***
	(0.005)	(0.005)	(0.013)	(0.013)	(0.011)	(0.011)
Stable		0.000		0.000		0.000
		(.)		(.)		(.)
Growth		-0.033		-0.028		-0.026
		(0.031)		(0.038)		(0.044)
Degrowth		0.090*		0.009		0.136*
		(0.038)		(0.048)		(0.052)
Growth'		0.000		0.045		-0.052
		(0.033)		(0.042)		(0.042)
Degrowth'		-0.000		-0.038		0.034
		(0.033)		(0.043)		(0.044)
Sample	Rich and Poor	Rich and Poor	Rich	Rich	Poor	Poor
p-value *Growth Degrowth*		0.001		0.442		0.003
p-value *Growth' Degrowth'*		0.988		0.073		0.056
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.017	0.036	0.118	0.136	0.141	0.165
Observations	392	392	191	191	201	201

OLS models. The dependent variable is the share of the available surplus kept by the participant. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$ Robust standard errors in parentheses. Controls include age, gender, number of previous participations in economic experiments, monthly consumption expenses, professional status and performance in the first part of the experiment (i.e. the number of correctly solved matrices). Negative trend (Pooled) is the effect of *Degrowth* and *Degrowth'* pooled.

E Descriptive statistics on satisfaction data

As explained in Section 2, we elicited participants satisfaction using a 7-points likert scale four times during the experiment. The first elicitation occurred just after the first step, and participants were asked to report their satisfaction about the (exogenous) distribution of the surplus in the first step. The second elicitation occurred just after the second step: at this point, participants were informed of the distributive choice implemented in their pair and were asked to report their satisfaction about it. The third elicitation concerned the total payoff (step 1 + step 2). For the fourth elicitation, we presented participants with the distribution of payoff in their sessions and asked them to report

their satisfaction about it.The first elicitation is exogenous because it was performed before any distributive choices by participants, and we therefore use it as a control in our regressions. The other three elicitations are endogenous, and were collected for exploratory purpose only. Figure E.1 below describes satisfaction reported by participants, separated by treatment and roles.

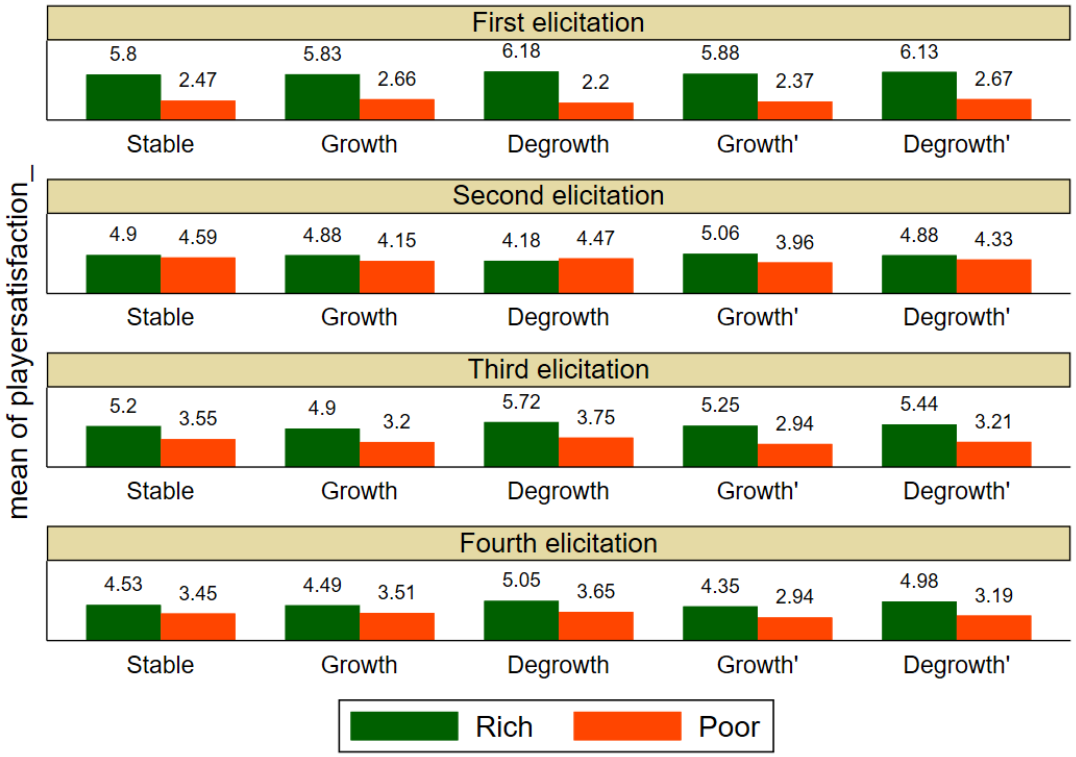


Figure E.1: Satisfaction data

F Instructions

Note: in the instructions below, *player.totalamount1* and *player.totalamount2* are replaced by the values reported in Table 1. *player.shareA* is replaced by 0.75**player.totalamount1* and *player.shareB* is replaced by 0.25**player.totalamount1*.

General Instructions

We thank you for your participation.

From this point on, you are not allowed to communicate with other participants for the entire duration of the experiment.

Please turn off your mobile phones and put them away.

Throughout the experiment, all decisions are anonymous.

Your decisions, as well as those of other participants, may result in financial gains. In addition, you will receive a fixed show-up fee of €3.

Your experimental earnings and the €3 show-up fee will be paid via Lydia within two weeks, following the lab's standard procedure.

The experiment consists of four parts.

The instructions will appear progressively on your screen and are identical for all participants.

There may be waiting times during the experiment – this is completely normal.

As a reminder, in all our experiments, the information provided to participants is truthful (i.e., no deceptive content is used).

If you have any questions during the experiment, please raise your hand, and the experimenter will come to assist you privately.

If you finish before the end of the session, please remain seated quietly.

The experimenter will let you know when the session is over.

START

PART 1

Instructions

In Part 1, your task is to count the number of zeros ("0") in grids composed exclusively of zeros ("0") and ones ("1").

Each grid contains 7 rows and 7 columns, for a total of 49 cells.

Here is an example of a grid:

In this example, there are 24 zeros and 25 ones. Therefore, the correct answer is 24. You should enter this number and click "Submit". A new grid will then appear.

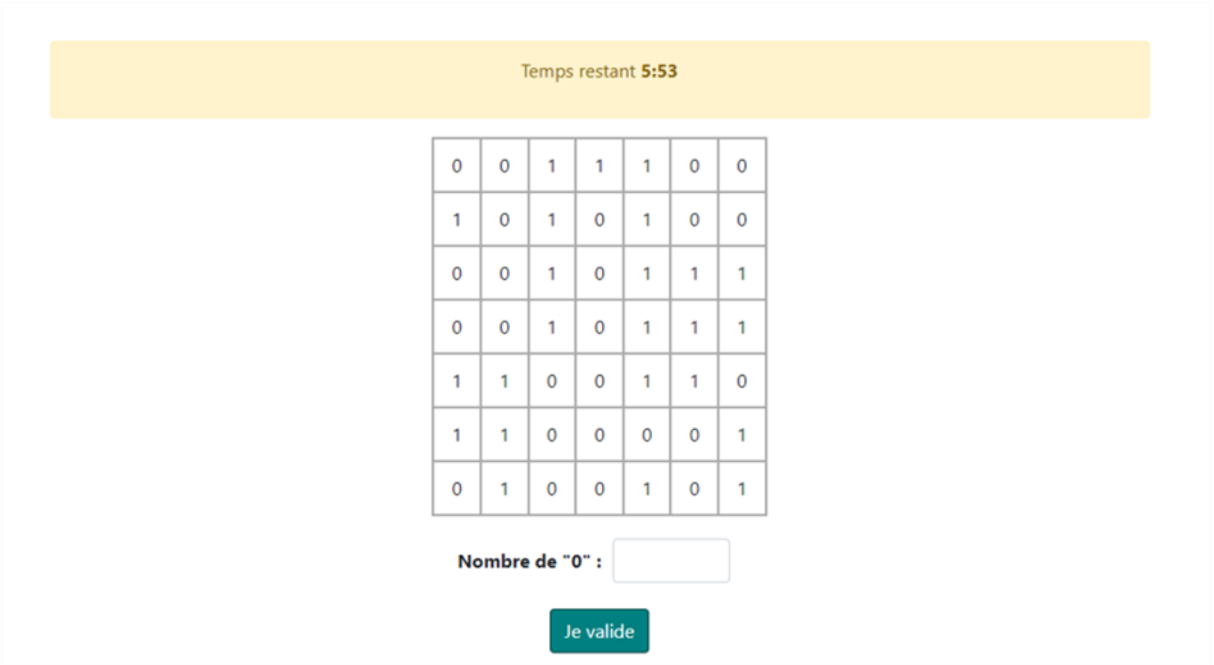
Note: you can only submit one answer per grid.

You have 6 minutes to correctly solve as many grids as possible. A countdown timer is displayed at the top of the screen.

The number of correct answers determines your score in Part 1. This score will influence your payment in the remainder of the experiment.

I'M READY TO START

Real-effort task decision interface



PART 2

General Information

At the beginning of this part, you are randomly paired with another participant in the session. This pair remains fixed for the remainder of the experiment. The other participant is selected randomly by the computer.

You will not be able to identify your matched participant, and he/she will not be able to identify you. No other participants in the session will be able to identify you either.

In this part, an amount of $player.totalamount1$ euros is allocated between you and your matched participant.

The allocation depends on your score in the previous part: the participant with the higher score in Part 1 receives $player.share_A$ euros, while the participant with the lower score receives $player.share_B$ euros.

In other words:

If your score is higher than that of your matched participant, you will receive $player.share_A$ euros, and your partner will receive $player.share_B$ euros.

Conversely, if your score is lower, you will receive $player.share_B$ euros, and your partner will receive $player.share_A$ euros.

In the case of a tie, the participant receiving the higher amount is selected at random by the computer.

This allocation rule is the same for all pairs in the session.

You will be informed of your payment for this part on the next screen, after which you will be asked to answer a question.

I UNDERSTAND

Results of Part 2

Your pair:

- You are participant *player.anonymid*.
- Your paired participant is *player.otheranonymid*.

Your payment for this part:

{*Displayed for the higher-scoring participant, i.e. Player A*}

Your score in Part 1 is higher than that of your paired participant.

You earn $player.share_A$ euros. Your paired participant *player.otheranonymid* earns $player.share_B$ euros.

{*Displayed for the lower-scoring participant, i.e. Player B*}

Your score in Part 1 is lower than that of your paired participant.

You earn $player.share_B$ euros. Your paired participant *player.otheranonymid* earns $player.share_A$ euros.

OK

Recap of earnings in Part 2

{*Displayed for the higher-scoring participant, i.e. Player A*}

Your score in Part 1 is higher than that of your paired participant.

You earn $player.share_A$ euros. Your paired participant $player.otheranonymid$ earns $player.share_B$ euros.

{*Displayed for the lower-scoring participant, i.e. Player B*}

Your score in Part 1 is lower than that of your paired participant.

You earn $player.share_B$ euros. Your paired participant $player.otheranonymid$ earns $player.share_A$ euros.

Are you satisfied with this allocation?

PART 3

General Information

Part 3 consists of two consecutive phases. Each phase will be explained on the screen step by step.

As in Part 2, a certain amount must be divided within each pair between the higher-scoring participant and the lower-scoring participant.

In Part 2, this amount was *player.totalamount*1 euros. In this part, the amount to be divided is *player.totalamount*2 euros.

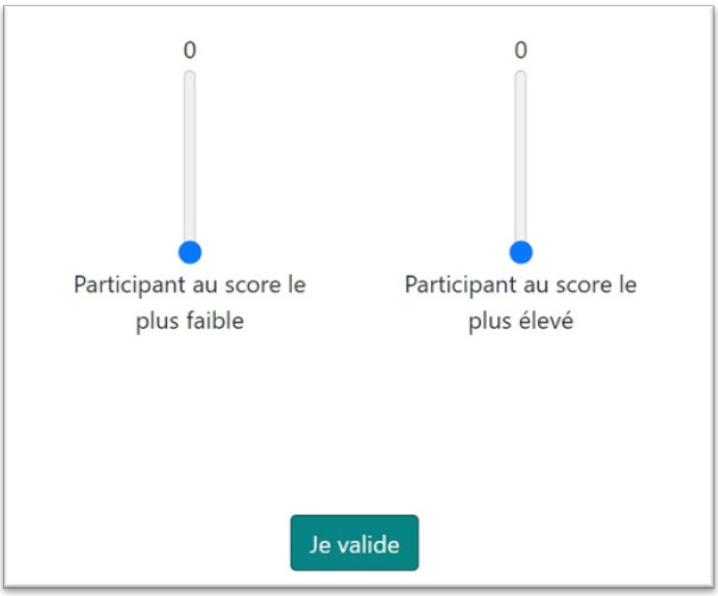
At the end of this part, one of the two phases will be randomly selected by the computer. The selected phase will be used to determine your earnings, as well as those of the other participants. Each phase has the same probability of being chosen.

After both phases are completed, you will be informed of your earnings. You will then be asked to answer a question.

I UNDERSTAND

Allocation for another pair - Phase 1 or 2

You must allocate the amount of *player.totalamount*2 euros between two participants from another pair, randomly selected by the computer from among the other pairs in the session. This allocation is made using the interface shown in the example below.



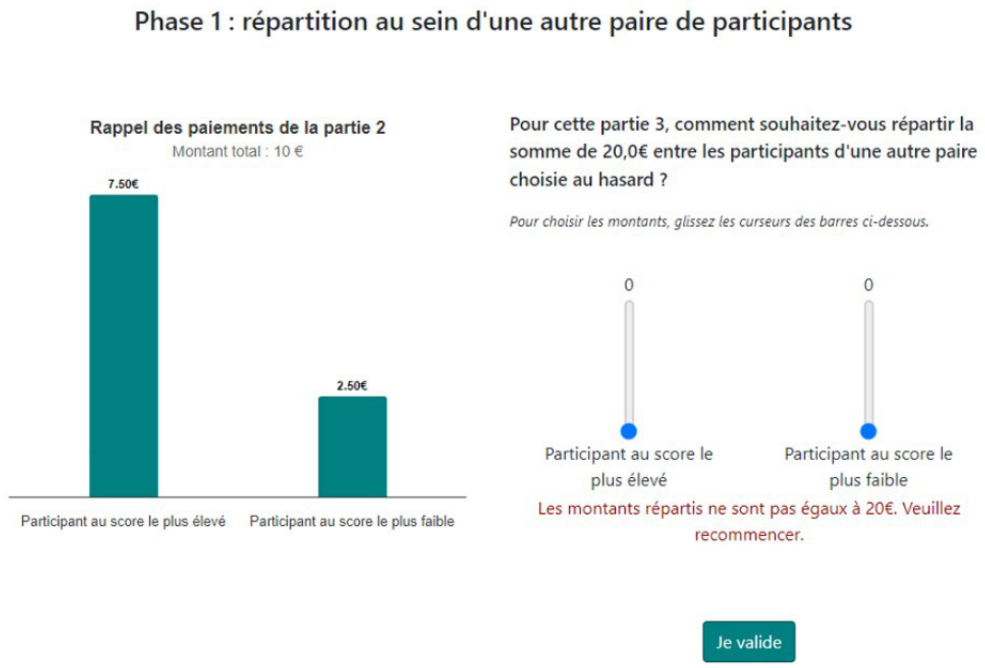
If this phase is randomly selected by the computer to determine earnings:

- The computer will randomly decide whether it is your allocation or your paired participant's allocation that will be used to determine the earnings of the members of the other pair. Each allocation has an equal chance of being selected.
- Similarly, your own earnings will be determined by the allocation of a member of another pair, randomly chosen by the computer.

Note that the pair whose allocations determine your earnings is different from the pair for whom your allocations determine earnings.

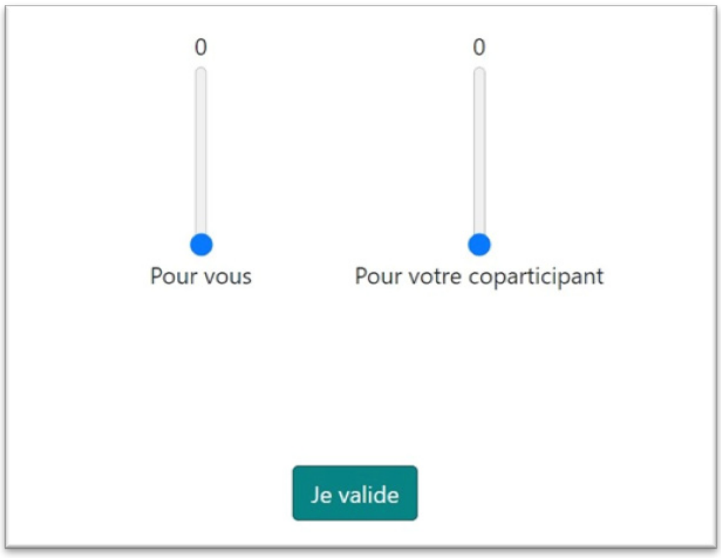
I UNDERSTAND

Example interface for allocating within another pair of participants



Allocation within your own pair - Phase 1 or 2

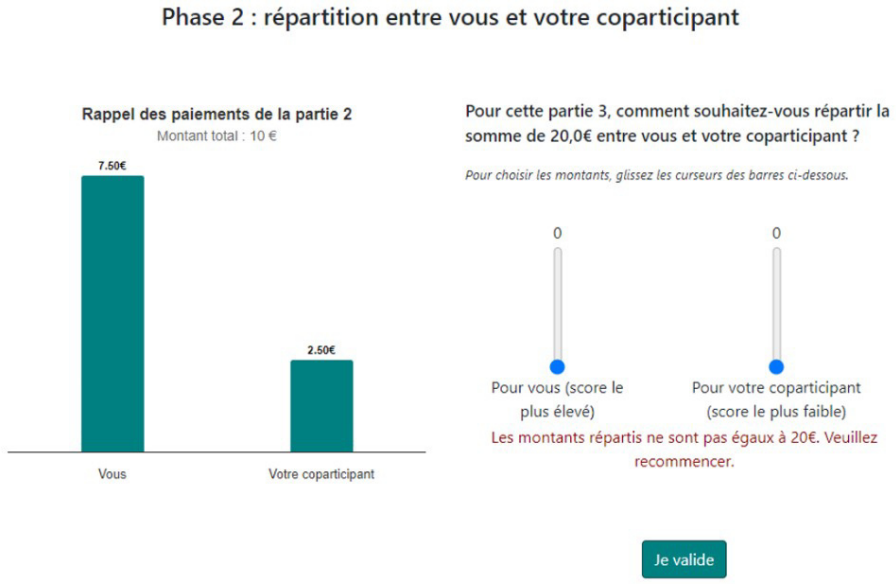
You remain paired with the same participant, *player.otheranonymid*, as before. As in Part 2, the amount of *player.totalamount*2 euros must be divided between you and your paired participant. Using the interface shown in the example below, you must indicate the allocation you prefer.



If this phase is randomly selected, the computer will randomly decide whether your allocation or your paired participant's allocation is used to determine the earnings. In that case, each allocation has an equal chance of being selected.

I UNDERSTAND

Example interface for allocating within the own pair



Results of Part 3

The amount to be divided between you and your paired participant is *player.totalamount*2 euros.

As explained earlier, the allocation used to determine earnings in Part 3 is randomly selected by the computer from either Phase 1 or Phase 2.

In other words, there is an equal chance that the allocation comes from your pair or from another pair. If the selected allocation comes from your pair, there is an equal chance that it is yours or your paired participant's that is used.

Your earnings in this part:

The allocation used to determine the division within your pair is *your own allocation / your paired participant's allocation / the allocation of a participant from another pair*, namely:

- For you: *player.payof*2 euros
- For your paired participant: *otherplayerpayof*2 euros

OK

Recap of earnings in Part 3

You earned *player.payof*2 euros.

Your paired participant *player.otheranonymid* earned *otherplayerpayof*2 euros.

Are you satisfied with this allocation?

Je valide

PART 4

Recap of earnings in Part 2 & 3

You earned *player.payoff* euros.

Your paired participant *player.otheranonymid* earned *otherplayerpayoff* euros.

Are you satisfied with this allocation?

Je valide

Here is the distribution of earnings for all participants from Parts 2 & 3. Your own earnings are highlighted in red.

{*Histogram of the distribution of participants' earnings in the session*}

Are you satisfied with the overall distribution of earnings?

Je valide

Additional questions

You are :

- ☐ male
- ☐ female
- ☐ non-binary
- ☐ prefer not to answer

How old are you ? ...

Your current status:

- ☐ student
- ☐ employed
- ☐ unemployed
- ☐ retired

How many times have you previously participated in a lab experiment?

- ☐ 0
- ☐ 1
- ☐ 2 or more

Your total monthly expenses are:

- ☐ $<$ €500
- ☐ €500- €1000
- ☐ €1000- €1500
- ☐ $>$ €1500