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Exposure to inequality may cause under-provision of public goods: Experimental evidence

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ABSTRACT

Economic inequality is rising globally and due to developments in information technologies and globalization, nowadays individuals are more exposed to such an inequality than ever. Recent studies show that exposure to inequality may shape economic decisions. In this article, we test whether contributions in the public goods game are sensitive to information about inequality of personal benefits between groups. Our results show that learning the return levels of another group with higher (lower) benefits decreases (increases) the contributions with a stronger reaction by low benefit groups. Our further tests show that this effect diminishes when the benefit difference between the groups gets smaller. These results suggest that exposure to inequality might have a detrimental impact on public goods provision, yet this effect gets negligible as economic inequality reduces.

1. Introduction

Insufficient cooperation lies at the core of a vast number of local and global economic challenges such as tax evasion, climate change, economic and social inequality and poverty. This is why economists pay particular attention to finding ways to sustain cooperation and identifying the drivers of free-riding in public goods provision. One suggested reason is that individuals who gain lower benefits from cooperating free-ride more both in homogeneous and heterogeneous groups (see Ledyard, 1995 and Zelmer, 2003 for reviews) as people do not always gain equal benefits from cooperating (Olson, 2009).[1] This inequality in personal benefits might be due to limited access to certain goods and services, discrimination, people's needs or their individual preferences. In a macro example, one can think of rising regional inequalities in the world causing unequal distribution of public investments among different regions of countries (e.g. Southern and Northern Italy).[2] Such inequalities in public investments may lead disadvantageous individuals to be less cooperative, which would yield inefficiency in public goods provision through tax evasion.

Beside rising regional inequalities, with transparency laws, developments in information technologies (e.g. internet, mass and social media) and globalization, individuals are exposed to economic and social inequalities much more frequently today. As a result, they are not only exposed to *local* but also to *global* inequalities. In regards to the earlier example, a taxpayer in Northern Italy is not only informed about public investments in the Southern Italy today but also about other countries, such as Germany or Greece. Exposure to such social information regarding other countries or external groups may affect how individuals evaluate their relative returns and therefore re-shape their contribution decisions. To test this hypothesis, we conducted a public goods experiment, where we investigated whether individuals' contribution decisions are affected by the information about personal benefits of another group. Keeping the marginal per capita return (m) within the groups constant, we informed our treatment group members that there existed two different m levels and that they would be allocated to one of the two types. This information is economically irrelevant for them as all group members have identical m's and payoff functions. Our results show that the cooperation difference between low and high benefit groups are significantly larger when they are informed about the existence of each other. However, when the inequality between groups gets

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[1] Note that Olson's theoretical work did not rely on experiments.

[2] See the report of OECD (2018) for a detailed analysis of regional inequalities in the world.

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smaller, the impact of social information also gets weaker. Our results suggest that transparency might have counter effects in environments with large inequalities even if they are not affected by the inequality at a local level.

2. Literature Review

2.1. Motives Behind Contributing in Public Goods Games

Behavioral game theory literature investigating the motives behind contributing to public goods has suggested various explanations. For Andreoni (1990), it is the *warm glow of giving*, which is a term describing a positive utility in the payoff function caused by an emotional reward of giving to others. Both inequity aversion model of (Fehr & Schmidt, 1999) and the fairness model of Rabin (1993) suggest that individuals have preferences for fairness, which might also shape contributions in the public goods game. Furthermore, conditional cooperation has also been reported to explain cooperative behavior in these environments (see also Fischbacher, Gächter, & Fehr, 2001; Chaudhuri, 2011). Public goods experiments suggest that contributions are sensitive also to other factors such as group size (Isaac, Walker, & Williams, 1994; Isaac & Walker, 1988; Pereda, Capraro, & Sánchez, 2019), sanctions (Fehr & Gachter, 2000; Carpenter, 2007; Nikiforakis, 2008), repetition (Andreoni, 1988), communication (Kerr & Kaufman-Gilliland, 1994; Haruvy, Li, McCabe, & Twieg, 2017), framing (Willinger & Ziegelmeyer, 1999; Rege & Telle, 2004) and also public goods being financed with lotteries and prizes (Morgan & Sefton, 2000; Corazzini, Faravelli, & Stanca, 2010; Orzen, 2008).

Heterogeneous distribution of endowments and marginal per capita return (m) among subjects are also reported to affect contributions in the public goods game. While the findings on the endowment heterogeneity is inconclusive (e.g. Buckley & Croson 2006; Cherry, Kroll, & Shogren 2005; Sadrieh & Verbon 2006; Van Dijk, Sonnemans, & van Winden 2002), the evidence on the positive effect of m on contributions is rather established. Regarding the heterogeneity in m, studies that are focusing on homogeneous group analysis (comparison of groups A and B where $m_A > m_B$, but identical among the members of each group) show that the groups with higher m contribute more (See Isaac & Walker, 1988, the survey of Ledyard, 1995 and the meta study of Zelmer, 2003). Also, if detailed instructions explaining the function of m are provided to subjects, reducing their m levels decreases the contribution (Laury, Walker, & Williams, 1999). Similarly, m has a positive effect on contributions in heterogeneous groups (comparison of group members i and j where $m_i > m_j$) (Tan, 2008; Reuben & Riedl, 2009; Kölle, 2015; Reuben & Riedl, 2013; Lugovskyy, Puzzello, Sorensen, Walker, & Williams, 2017). Fellner-Röhling, Kröger, and Seki (2019) study the information asymmetry in heterogeneously formed groups of six. Although the awareness of heterogeneity in m decreases the contributions in general, subjects with low m contribute more than the high-benefit subjects, which contradicts the previous findings on the effects of m. However, they use a modified version of public goods game where each subject's contribution is excluded from her own payoff function. Studies on within group heterogeneity do not solely focus on m heterogeneity but sanctions (Tan, 2008; Reuben & Riedl, 2009; Kölle, 2015), contribution norms (Reuben & Riedl, 2013), or uncertainty and conditional cooperation (Fischbacher, Schudy, & Teyssier, 2014). Besides, there is also heterogeneity in dominant strategies in Reuben and Riedl (2009) and Kölle (2015).

2.2. Transparency and Social Information

Studies investigating the role of transparency in public goods games provide contradictory results. On the one hand it is argued that transparency increases contributions. Informing subjects about decisions of others can make it easier to detect free-riders and non-cooperative actions (e.g. Friedman 1971; Fudenberg & Maskin 2009). Further, it can also foster signaling and conditional cooperation in repeated public goods games (Fischbacher et al., 2001; Davis, Korenok, & Reilly, 2010). On the other hand, the meta-analysis of Fiala and Suetens (2017) shows that although transparency about other group members' choices increases contributions, transparency on individual payoffs decreases the contributions. Nikiforakis (2010) similarly finds that giving feedback about the earnings of other subjects lowers contributions. The negative effect of information on contributions can be explained by the fact that the monetary feedback might crowd out intrinsic motivation, such as warm glow and fairness concerns (see for example Reeson & Tisdell, 2008). Burton-Chellew, Nax, and West (2015) indeed show that payoff based learning yields contribution decline in repeated public goods games.

The common feature of those studies investigating the role of information on public goods provision is that they manipulate the information and feedback schemes within groups. That is, the payoffs of the group members are directly affected by the decisions of other members of the same group. However, both economic and social decisions outside the laboratory are also affected by *economically irrelevant* information as a body of literature has documented. Literature on relative income is maybe the most visited topic in this respect. For example, Luttmer (2005) shows that people become unhappier with richer neighbors and a rise in income may sometimes not increase happiness due to relative income concerns (Easterlin, 2001). Using a similar design with the current study, Schmidt, Neyse, and Aleknonyte (2019) show that higher income yields higher risk-taking only if the relative income is common knowledge. This suggests that even non-social economic decisions are sensitive to such type of social information regarding other subjects' endowments. Further examples regarding economic behavior suggest that social information shapes work productivity (Gächter & Thöni, 2010; Cohn, Fehr, Herrmann, & Schneider, 2014; Charness, Cobo-Reyes, Lacomba, Lagos, & Pérez, 2016), risky decisions (Linde & Sonnemans, 2012; Gamba, Manzoni, & Stanca, 2017) and charitable giving (Frey & Meier, 2004). These findings should not be surprising for social psychologists as numerous studies have shown that social comparisons shape decisions since the seminal study of Festinger (1954) (See also Corcoran, Crusius, & Mussweiler, 2011 for a detailed review). The impact of social comparisons on economic decisions were even confirmed through direct brain imaging techniques (Fliessbach et al., 2007).

Investigating the role of social information in economic behavior is a matter of great importance as economic and social inequality is rising not only in the developing but also in the developed countries today (Piketty, 2014). As a result, people engage in social comparisons more frequently. The study of Cheung and Lucas (2016), involving 1.7 million US citizens, show that people are concerned about their neighbors income more in those US counties that have higher income inequality levels. This is not surprising, given the theoretical foundations of the relationship between inequality and social comparisons that date back to the 1980s (Panning, 1983). Yet, those studies were largely concerned about relative deprivations (discontent arising from upward social comparisons) caused by inequality or injustice in society. In an experimental study Nishi, Shirado, Rand, and Christakis (2015) show that inequality and visibility of wealth decreases cooperation in social networks. However, income and wealth are not the only factors behind economic inequality.[3] Furthermore, with increasing globalization and urban population more encounters between diverse socioeconomic groups occur and this may shape decision-making (Sands, 2017). Finally, psychology literature (Haferkamp & Krämer, 2011; Vogel, Rose, Okdie, Eckles, & Franz, 2015) suggests a strong correlation between social media use and social comparisons.

In a nutshell, individuals are exposed to economic inequality and social information more severely today and these exposures have im-

[3] Sociologist Claude S. Fischer explains that *"Inequality is about access to public goods, not income"* (Fischer, 2017).

Table 1
Experimental Treatments.

Treatment	m	Social-Info.
	High-Inequality	
T1	0.4	No
T2	0.9	No
T3	0.4	Yes
T4	0.9	Yes
	Low-Inequality	
T5	0.5	No
T6	0.75	No
T7	0.5	Yes
T8	0.75	Yes

Table 2
Regressions by the Size of Inequality.

	High-Inequality				Low Inequality			
	All	All	No-Info	Info	All	All	No-Info	Info
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
m	0.788***	0.484**	0.484**	1.091***	0.306	0.419	0.419	0.252
	(0.152)	(0.220)	(0.225)	(0.173)	(0.244)	(0.540)	(0.557)	(0.252)
info	-0.021	-0.415**			0.007	0.111		
	(0.076)	(0.163)			(0.074)	(0.363)		
period	-0.010*	-0.010*	-0.017*	-0.004	-0.027***	-0.027***	-0.037***	-0.023***
	(0.005)	(0.005)	(0.009)	(0.006)	(0.005)	(0.005)	(0.007)	(0.007)
mXinfo		0.606**				-0.167		
		(0.277)				(0.595)		
constant	0.001	0.197	0.233*	-0.252**	0.471***	0.400	0.453	0.486***
	(0.110)	(0.132)	(0.138)	(0.109)	(0.154)	(0.328)	(0.354)	(0.167)
Observations	720	720	360	360	1110	1110	360	750
N	72	72	36	36	111	111	36	75
R^2	0.307	0.351	0.135	0.560	0.073	0.074	0.100	0.060

Panel regressions. Dependent variable: Contributions [0,1]. Independent variables: (i) *m*; (ii) *info* is a binary variable; (iii) *period* [0,10]; (iv) *mXinfo* interaction variable. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; regressions were clustered by experimental groups of three.

pacts on their economic decisions. In our experiment, we first investigate whether contribution decisions in the public goods game are sensitive to social information about other groups' return levels (*m*). Then, we test the relevance of social information between less unequal groups.

3. Experimental Procedures and Design

3.1. Experimental Procedures

The experiment was run in EGEO, the experimental lab of University of Granada in Spain and conducted with Z-tree software (Fischbacher, 2007). The sessions were conducted between June 2012 and October 2013. The subjects were recruited through ORSEE (Greiner, 2015). The total number of subjects was 183 (female=106), who were students of the Faculty of Economics and Business.[4] In each session, subjects were randomly assigned to the computers and their groups. After they read the general instructions and the instructions of the public goods game, experimenters read them out loud once again. Following several control questions and two trial periods, subjects were informed about their own *m* and also the *m* of the other two members in their groups on their screens. Then they began the experiment. After completing the experiment, subjects filled out a follow-up questionnaire including items on gender, generosity, trust and risk preferences (see Appendix for the whole questionnaire and information regarding variable generation). Subjects were paid confidentially for a randomly selected round and left the lab. The duration of a session was about 1 hour. Subjects earned € 13.41 in average (including € 3 show up fee).

3.2. Experimental Treatments

In our design each group consists of three subjects who are randomly matched.[5] As the standard public goods game procedure, each subject makes a contribution decision from her endowment of 20 tokens ($c_i \in [0, 20]$; exchange rate of a token is € 0.33). Thus, subjects decide how much to keep for themselves and how much to contribute to the public pool. Any amount of money contributed to the public pool is multiplied by a multiplication factor and divided equally between the members of the group, which gives the marginal per capita return (*m*). It defines the personal benefit that the subjects receive from the public pool. A subject *i*'s payoff function is as follows:

$$\pi_i = (w_i - c_i) + \mathrm{m}_i \times \sum_{j=1}^{3} c_j \quad (1)$$

In the equation w_i is the initial endowment of the subject *i*, c_i is her contribution to the public pool, m_i is her marginal per capita return, and c_j stands for the contributions of the other subjects in the group. The experiment is run for 10 periods.[6] In all treatments, subjects are given full information about their own payoffs and also a vector showing contributions of each group member after each period.

There are four *m* levels in the experiment: 0.4, 0.5, 0.75, 0.9 and 8 treatments (See Table 1). Note that if subjects were self-interested, since $m < 1$ in all our treatments, the dominant strategy for all of them would be $c_i = 0$. In the first four treatments we investigate the *High-Inequality* environment. In these treatments, subjects have either a low (0.4) or a high (0.9) *m*. The subjects in treatments T1 and T2 only know their own group's *m* level, identical for all the members of the group. The subjects in treatments T3 and T4, however, are also aware of the *m* level of each

[4] Each treatment contained 18 subjects (6 groups) except for T7 and T8 (see Table 1). In T7 and T8 there are 39 and 36 subjects, respectively, as we included the observations from the pilot sessions, which were identical to the experimental protocols. Two-sample Wilcoxon rank-sum tests do not suggest statistically significant contribution differences between pilot and actual sessions neither for groups with $m = 0.5$, nor for those with $m = 0.75$, ($p = 0.09$ for both levels of *m*).

[5] We chose a group size of three, following a similar design as Reuben and Riedl (2009) and Reuben and Riedl (2013).

[6] The experimental protocol involved two sets of 10 rounds. After completing first 10 rounds, participants were invited to played another 10 rounds of PGG with different *m*'s. This study uses the first part of the experiment.

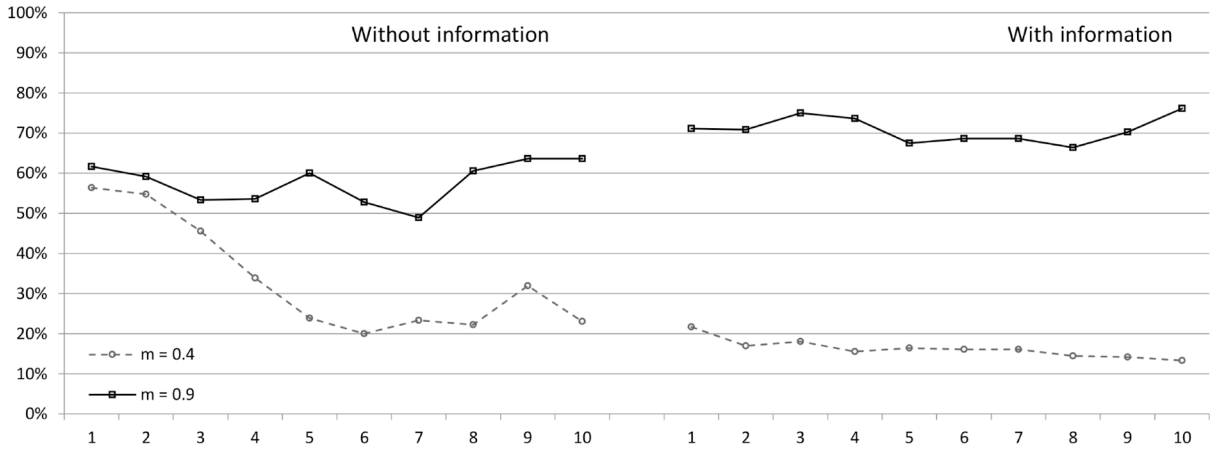


Fig. 1. Contributions along 10 periods: High-Inequality.

other, which is different. In the latter four treatments (T5-T8) we study the *Low-Inequality* environment. These *Low-Inequality* treatments are symmetrical to *High-Inequality* ones, except for the levels of high and low *m*, which are 0.75 and 0.5, respectively. Note that in the *High-Inequality* environment there exists a larger difference between the levels of *m* (0.5), while in the *Low-Inequality* environment the difference is precisely the half (0.25).

Treatments T3-T4 in the High-Inequality environment and the treatments T7-T8 in the *Low-Inequality* environment are our *Social-Information* treatments. As we have mentioned before, in addition to their groups' *m*, subjects in these treatments were informed in the instructions about the existence of another group with a different *m*. In the *High-Inequality* environment, subjects with low *m* (0.4) were informed about the existence of subjects with a higher *m* (0.9) and vice-versa. Likewise, in the *Low-Inequality* environment, low *m* (0.5) and high *m* (0.75) groups were informed in the instructions about the existence of each other. The exact wording was as follows:

"In this experiment you may have a multiplication factor of 0.4 or 0.9."[7] The numbers were 0.5 and 0.75 in the Low-Inequality environment respectively.

4. Results

In this section we present the results of our experiment in three subsections. In the first subsection, we focus on treatments T1-T4 where the *m* difference is large (0.4 and 0.9). In the second subsection, we analyze the treatments T5-T8 where the *m* difference between groups is smaller (0.5 and 0.75). Both subsections follow an identical analysis which begins with the descriptive results and non-parametric tests based on Mann-Whitney-U Tests, which are followed by regression analyses. In the final subsection, we investigate the effect of social information on each *m* level. Please note that in all analyses contributions are shown as the percentage of the endowment [0,1] to make the interpretation of the results easier. Average contributions in absolute units [0,20] can be found in the Appendix Table A1.

Table 2 shows our panel regression analyses. The first four models are devoted to High-Inequality treatments and the latter four to Low-Inequality treatments. Our dependent variable is the contribution levels in [0,1]. The variable *m* is the marginal per capita return, *info* is a dummy variable that takes the value of 1 in the social information treatments (T3-T4-T7-T8). *Period* is the time variable and shows the experimental periods [1,10]. The interaction variable, *mXinfo*, is included in the regressions in models (2) and (6) to test the joint impact of *m* and social information on the dependent variable. Columns (1) and (2) involve all four treatments in the High-Inequality environment. Column (3) focuses on the no information treatments (T1-T2) and column (4) involves the model for treatments with information (T3-T4). Models (5) to (8) present regressions for the Low-Inequality treatments in the identical order as High-Inequality columns.

4.1. High-Inequality

Fig. 1 shows the contribution trends in the High-Inequality treatments ($m = 0.4$ and $m = 0.9$) where the left panel shows treatments without comparative information and the right one shows the treatments with comparative information. Without information, the low-benefit subjects (*m=0.4*) start contributing at 56% and reduce until 22%. The high-benefit subjects (*m=0.9*) also start around 60% but do not exhibit a decline. In the information condition, informed low-benefit subjects start contributing at remarkably low rates and they exhibit a gentle decline. In summary, the contributions of informed low-benefit subjects are significantly lower than uniformed low-benefit ones (means: $Low_{noinfo} = 33.50\%$; $Low_{info} = 16.28\%$; $p = 0.003$). On the contrary, high-benefit subjects (*m=0.9*) start contributing at a very high level (70%) and continue contributing consistently until the end of the game. Hence, there is no decline. Although the contribution pattern of high-benefit gets more stable with information, the statistical tests do not detect a significant difference between informed and uninformed high-benefit subjects on average (means: $High_{noinfo} = 57.72\%$; $High_{info} = 70.81\%$; $p = 0.195$). The difference between low-benefit and high-benefit subjects are significant in both no-information and information cases ($p < 0.01$; $z = -2.595$ and $z = -5.000$ respectively).

In the High-Inequality environments, the difference between low and high-benefit subjects is not significant until the fifth round in treatments without information ($p = 0.01$ and $z = -2.699$ in round 5). In the information treatments, however, the difference is significant from the first period onwards. Fig. 2 and Fig. 3 show average contributions in the first 5 and last 5 periods.

Results from our panel regression analyses are in line with our non-parametric results. Column (1) shows that *m* has a significant positive impact on contributions ($p < 0.01$, coefficent=0.788). The contributions decay along the experiment is not very strong as *period* variable is negative and significant only at $p < 0.1$, (coefficient= − 0.010). When the interaction variable, *mXinfo* is introduced in model (2), all variables

[7] Note that the experimental protocol involved two sets of 10 rounds. After completing first 10 rounds, participants were invited to played another 10 rounds of PGG with different values of *m*. The current study uses only the first part of the experiment. The data collected in the second 10 rounds would focus on changes in group formations. However, for a scientifically sound analysis, we needed many more observations as changes in *m* would change the group structures (i.e. heterogeneous groups with various types in majority), so that part of the project has been postponed for further research.

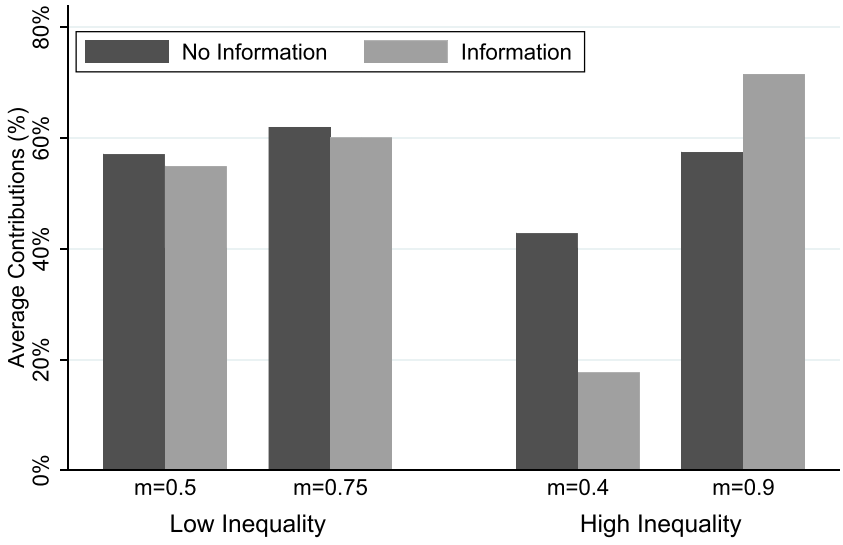


Fig. 2. Average contributions: First 5 periods.

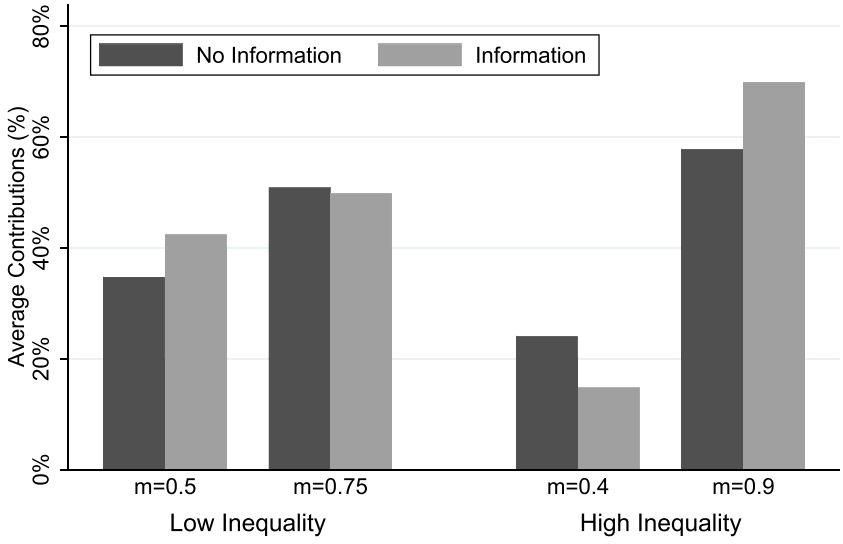


Fig. 3. Average contributions: Last 5 periods.

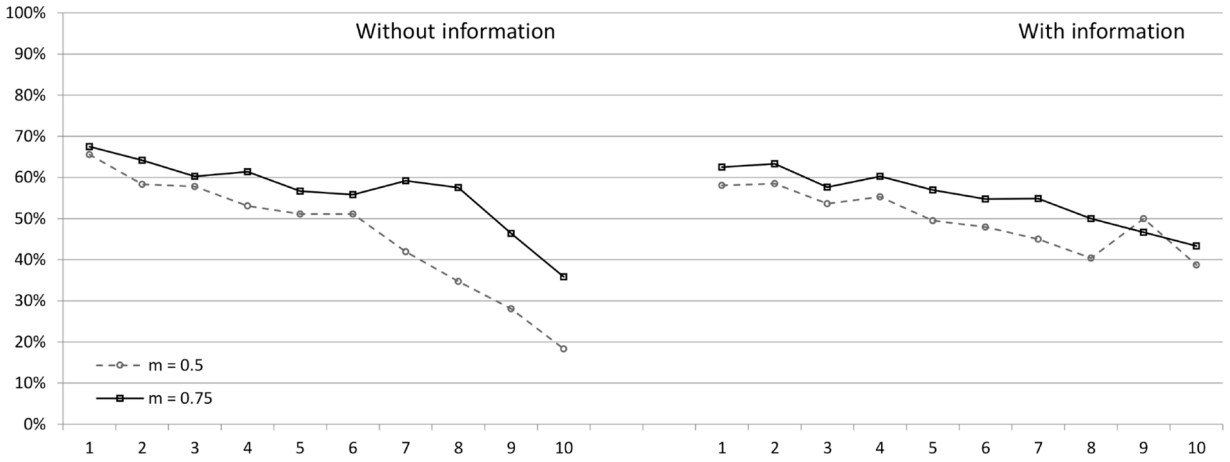


Fig. 4. Contributions along 10 periods: Low-Inequality.

Table 3
Regressions by m levels.

	$m = 0.4$	$m = 0.5$	$m = 0.75$	$m = 0.9$
	(1)	(2)	(3)	(4)
info	-0.172**	0.027	-0.014	0.131
	(0.076)	(0.094)	(0.118)	(0.120)
period	-0.022***	-0.032***	-0.023***	0.001
	(0.006)	(0.007)	(0.007)	(0.008)
constant	0.455***	0.634***	0.689***	0.569***
	(0.058)	(0.099)	(0.113)	(0.093)
Observations	360	570	540	360
N	36	57	54	36
R^2	0.200	0.085	0.040	0.035

Panel regressions. Dependent variable: Contributions [0,1]. info=1 for comparative information treatments, period is the time variable [1,10]. Robust standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; regressions were clustered by groups.

get significant, suggesting that 0.4 groups are the ones who are affected by the social-information. Models (3) and (4) repeat the same analysis for No-Information and Information treatments separately and confirm that m has a significant positive effect on contributions. However, the *period* variable gets insignificant in model (4), showing that the cooperation is sustained along 10 periods in the 0.9 groups as Fig. 1 illustrates.

We further run the same regression analysis with control variables that we elicited through the follow-up questionnaire (Table A2). We do not find a robust impact of gender, risk, trust or generosity variables on contribution behaviors. The only statistically significant control variable in our regression analysis is *risk* in column 3 (coefficient=0.07; $p < 0.01$), which suggests that risk-seeking subjects in T1 and T2 contributed more than relatively risk-averse subjects. Nevertheless, we do not see such an effect again in any other treatments.

4.2. Low-Inequality

Fig. 4 presents the Low-Inequality treatments ($m = 0.5$ and $m = 0.75$). The common contribution decline patterns are observed in all treatments. Contributions begin around 60% of the endowment in period 1 and they decrease gradually. In Low-Inequality case, we do not observe any significant treatment effects caused by comparative information neither for low-benefit (Means: $Low_{noinfo} = 46.00\%$; $Low_{info} = 48.73\%$; $p = 0.619$) nor for high-benefit subjects (Means: $High_{noinfo} = 56.47\%$; $High_{info} = 55.03\%$; $p = 0.614$).

The low-benefit subjects contribute nearly 2 tokens less than high-benefit ones on average. However, the difference is not significant in No-Information treatments ($p = 0.200$) and it is significant only at 90% ($p = 0.090$) in Information treatments. See Figs. 2 and 3 for average contributions in the first 5 and last 5 periods.

In line with the non-parametric tests, regression analyses (Table 2) do not detect a significant effect of *info* or m on contributions in the Low-Inequality treatments, but decline pattern is significant in all models ($p < 0.01$).

To test whether the null results of Low-Inequality analysis are due to a low statistical power, we further run an ex-post power calculation for each sub-sample (columns 7 and 8 in Table 2. For the No-info sample ($n = 36$), we find a minimum detectable effect (MDE) of 0.33 (with a power of 0.8, an *alpha* of 0.05 and a SD of 0.35). As column 7 suggests, our effect is 0.42, higher than the MDE. For the Info sample ($n = 75$), we can detect a MDE of 0.23, lower than the estimated effect found in column 8. In a nutshell, our ex-post power calculation does not suggest that the null effects in this section are due to a lack of statistical power. Similar to the previous subsection, we repeated our analysis with more control variables (Table A2). Again, results do not indicate a statistically significant impact of gender, risk, trust or generosity.

4.3. Effect of Social Information per m

As a final test, we investigate the role of social information in different m levels. To do so, we run simple panel regression models for groups with an identical m, but who are in different social information treatments (See Table 3). In line with the previous regression analysis, the dependent variable is the contribution level, represented in [0,1]. The two independent variables are *info* and *period*: *info* variable is significant only in model 1 ($p < 0.05$; coefficient= -0.172) suggesting that the contributions of informed subjects with $m = 0.4$ are significantly lower than the uninformed ones. *Period* variable is significant in all models except for model (4) which suggests that the cooperation was sustained along the experiment when subjects have the highest level of m.

Table A3 presents the same analysis with control variables. Risk variable is significant in the first column (coefficient=0.042; $p < 0.05$) suggesting a positive relationship between contributions and risk preferences in the groups where $m = 0.4$. We also observe statistically significant coefficients in column 3, where $m = 0.75$. The coefficient of *female* is -0.113 ($p < 0.05$) and the coefficient of *trust* is 0.222 ($p < 0.05$), meaning that men and more trusting subjects tend to contribute higher. Considering the sample size of our experiment and the fact that our questionnaire items are self-reported or hypothetical, more detailed research with a larger sample size would be necessary to identify the underlying mechanisms thoroughly.

5. Conclusion

In this experimental study, we investigated whether being exposed to social information, what makes the economic inequality salient, affects individuals' contribution decisions in a linear public goods game. Since the members of each group have identical marginal per capita return levels (i.e. homogeneous groups) the social information that a *selfish* agent receives should not affect her contribution level. Yet, our results in High-Inequality treatments show something different. The contribution difference between 0.4 and 0.9 groups widens when they learn about the existence of each other. Our analyses and robustness tests show that this effect is mainly due to a decrease in 0.4 groups' contributions. Notably, the difference between low and high benefit subjects is significant starting from the first round in the Information treatments, while the difference becomes significant only starting from the fifth period in the No-Information treatments. This immediate effect of social information is important as many public goods games in the literature are played one-shot. In such situations the salience about inequality might seem more plausible as low-benefit subjects contribute as much as the high-benefit subjects. However, in real life social interactions and also tax-payments are usually repeated actions. In those situations where economic inequality is not salient, agents and policy makers might not spot the contribution differences between low and high benefit agents and might choose to avoid transparency. However, the difference might emerge in time as our results indicate.

In the treatments where the inequality is lower, we see that the difference between 0.5 and 0.75 groups are not significant in No-Information treatments and significant only at $p = 0.09$ level in the Information treatments. When we look at the contribution trends, the usual decaying pattern is observable in Low-Inequality treatments and also in 0.4 groups without information. However, groups with $m = 0.9$ sustain cooperation along 10 periods. This is not surprising as m is almost 1, which is the point where dominant strategy switches from free-riding to full contribution. In the social information treatments in the High-Inequality environment, we observe even a more stable pattern in 0.9 groups. 0.4 groups contribute very low starting from the first period and keep their contribution low along 10 periods.

Our experimental results show that contribution decisions in the public goods game are sensitive to exposure to information about economic inequality even if the agents are not directly affected by the inequality. This result is in line with previous findings in different economic contexts such as risk-taking or redistribution (e.g. Schmidt et al., 2019; Sands, 2017). A recent meta-analysis by Gerber, Wheeler, and Suls (2018) shows that people tend to compare themselves with people who are superior than them (i.e. upward social comparison). Our result on the higher impact of social information on 0.4 groups supports this finding. The main message of our paper is that exposure to inequality in individual returns might affect individuals' support for public goods, which eventually might cause inefficiencies in public goods provision. However, the impact of inequality exposure gets weaker as inequality levels decrease. As the social information flow about other groups, regions or countries is unavoidable today, the policy makers should focus on decreasing the inequality in public goods returns. Aiming to decrease access limitations and to redistribute taxes more fairly might overcome detrimental impacts of social information on public goods provision.

We show that individuals do not only care about their own returns from the public goods but also take their relative positions into consideration. This social comparison effect is significant if the benefit difference in two societies is large. Although the main conclusions of this paper are supported by (ex-post) power calculations, still the results should be tested in a larger sample. Our study is only exploratory as many other questions remain to be answered. On the one hand, field evidence would be useful to be able to provide a concrete policy advice. During the Covid-19 pandemic, for example, citizens of different countries have been exposed to news about social health systems and insurance schemes in other countries. Does such information affect the public trust in the government and tax compliance behavior? If it does, such an effect should force malfunctioning governments to catch up with the better functioning ones. Alternatively, they could follow a non-democratic path and try to prevent citizens from getting unbiased information (e.g. media control), which should not be a long-lasting strategy. On the other hand, our study remains silent about the underlying mechanisms of the social comparison effect. With a more detailed study, it would be possible to detect what drives people to change their behavior. Decreasing contributions could be due to envy, disappointment, or simply not feeling responsible for the group because of the inequality. Analysis of our follow-up questionnaire does not show any statistically significant impact of gender or economic preferences such as risk, trust or generosity.

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Appendix A

A1. Questionnaire

In our analysis, we use four additional control variables that rely on the follow-up questionnaire.

- Gender: We generated the binary *female* variable with Item 1.
- Risk: We use the sum of lottery items to generate our *risk* variable (questions 9-15) Note that we corrected the reversed ordered item 11. Note that item 8, asking the frequency of lottery ticket purchase did not have a meaningful variance in a student sample. This is why we do not use this question in our analysis. We neither use the hypothetical Holt and Laury task (Item 16) as a large number of students made multiple switches in the task.
- Trust: We use the average of Items 6 and 7 to generate the *trust* variable.
- Generosity: We use Item 2 (hypothetical dictator game) to generate the *generosity* variable

1. What is your sex? M/F
2. Imagine you are given 10 euros. You have to divide it between you and an unknown person who has received nothing. How much would you give her/him?
3. How many close friends would you say that you have?
4. If you had 1,000 euros, to how many of your close friends would you lend your money?
5. And if you had 10.000?
6. Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people? (Most people can be trusted = 1 / You can't be too careful in dealing with people = 0)

7. Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? (Most people would try to take advantage of you if they got a chance = 0 / Most people would try to be fair = 1)
8. How often do you buy lottery tickets or enter a draw or a raffle? (Weekly/ More than one time in a month / Monthly / 6-10 times a year / Rarely / Never or almost never / Dont know)
9. We flip a coin. Choose one of the following options:
   a. Take 1,000 euros no matter if it is heads or tails = 0
   b. Take 2,000 euros if it is heads and nothing if it is tails = 1
10. Choose one of the following options:
   a. Take a lottery ticket with 80% chance of winning 45 euros and 20% chance of winning nothing
   b. Take 30 euros
11. Choose one of the following options:
   a. Take a lottery ticket with 25% chance of winning 100 euros and 75% chance of winning nothing
   b. Take a lottery ticket with 20% chance of winning 130 euros and 80% chance of winning nothing
12. Choose one of the following options:
   a. Take a lottery ticket with 2% chance of winning 3000 euros and 98% chance of winning nothing
   b. Take a lottery ticket with 1% chance of winning 6000 euros and 99% chance of winning nothing
13. We flip a coin. Would you take this deal? Take 1,500 euros if it is heads and lose 1,000 euros if it is tails. (Yes/ No)
14. Consider a balanced six-sided dice. Choose one of the following options
   a. You win 50 euros no matter what number comes up
   b. You win 50 euros if it comes up a 1,2 or 3; you win nothing if it comes up a 4 or 5; you win 200 euros if it comes up a 6
15. Consider a balanced six-sided dice. Choose one of the following options
   a. You win 50 euros if it comes up a 4,5 or 6; else you win nothing
   b. You win 200 euros if it comes up a 6; else you win nothing
16. Please choose one of the options in each row:

Option A	**Option B**
Win 2 Euros with 1/10 chance and win 1.60 Euros with 9/10 chance	Win 3.85 Euros with 1/10 chance and win 0.10 Euros with 9/10 chance
Win 2 Euros with 2/10 chance and win 1.60 Euros with 8/10 chance	Win 3.85 Euros with 2/10 chance and win 0.10 Euros with 8/10 chance
Win 2 Euros with 3/10 chance and win 1.60 Euros with 7/10 chance	Win 3.85 Euros with 3/10 chance and win 0.10 Euros with 7/10 chance
Win 2 Euros with 4/10 chance and win 1.60 Euros with 6/10 chance	Win 3.85 Euros with 4/10 chance and win 0.10 Euros with 6/10 chance
Win 2 Euros with 5/10 chance and win 1.60 Euros with 5/10 chance	Win 3.85 Euros with 5/10 chance and win 0.10 Euros with 5/10 chance
Win 2 Euros with 6/10 chance and win 1.60 Euros with 4/10 chance	Win 3.85 Euros with 6/10 chance and win 0.10 Euros with 4/10 chance
Win 2 Euros with 7/10 chance and win 1.60 Euros with 3/10 chance	Win 3.85 Euros with 7/10 chance and win 0.10 Euros with 3/10 chance
Win 2 Euros with 8/10 chance and win 1.60 Euros with 2/10 chance	Win 3.85 Euros with 8/10 chance and win 0.10 Euros with 2/10 chance
Win 2 Euros with 9/10 chance and win 1.60 Euros with 1/10 chance	Win 3.85 Euros with 9/10 chance and win 0.10 Euros with 1/10 chance

Table A1
Average contributions.

m	Soc. Info.	Period 1	Period 1-5	Period 6-10	All Periods
			Large-Inequality		
0.4	No	11.278	8.578	4.822	6.700
		(5.676)	(3.574)	(3.668)	(3.104)
0.9	No	12.334	11.511	11.578	11.544
		(6.278)	(5.933)	(6.326)	(5.871)
0.4	Yes	4.334	3.544	2.967	3.256
		(3.395)	(2.311)	(2.501)	(2.363)
0.9	Yes	14.222	14.322	14.000	14.161
		(4.479)	(4.481)	(5.576)	(4.478)
			Small Inequality		
0.5	No	13.111	11.433	6.967	9.200
		(6.902)	(5.963)	(3.445)	(4.259)
0.75	No	13.500	12.400	10.189	11.294
		(6.243)	(6.237)	(6.971)	(6.414)
0.5	Yes	11.507	10.640	8.147	9.393
		(5.305)	(4.203)	(4.678)	(4.090)
0.75	Yes	12.222	12.122	10.256	11.189
		(5.228)	(4.161)	(4.423)	(3.817)

Note: Contributions are presented as number of tokens contributed [0,20]. Standard deviations are in parentheses.

Table A2
Regressions by the Size of Inequality (with control variables).

	Large-Inequality				Small-Inequality			
	All	All	No-Info	Info	All	All	No-Info	Info
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
m	0.749***	0.441**	0.488**	1.113***	0.320	0.388	0.364	0.276
	(0.158)	(0.214)	(0.191)	(0.190)	(0.241)	(0.534)	(0.553)	(0.248)
info	-0.038	-0.419***			0.005	0.068		
	(0.079)	(0.150)			(0.073)	(0.363)		
period	-0.010*	-0.010*	-0.017*	-0.004	-0.027***	-0.027***	-0.037***	-0.023***
	(0.005)	(0.005)	(0.009)	(0.006)	(0.005)	(0.005)	(0.007)	(0.006)
mXinfo		0.598**				-0.101		
		(0.275)				(0.586)		
female	-0.012	-0.029	0.129*	-0.086	-0.019	-0.017	-0.006	-0.019
	(0.062)	(0.053)	(0.074)	(0.057)	(0.035)	(0.035)	(0.091)	(0.036)
risk	0.034*	0.028	0.070***	-0.021	-0.001	-0.001	-0.015	0.006
	(0.020)	(0.020)	(0.020)	(0.017)	(0.014)	(0.015)	(0.036)	(0.014)
trust	-0.086	-0.061	0.003	-0.099	0.115*	0.114*	0.192	0.095*
	(0.068)	(0.074)	(0.099)	(0.090)	(0.062)	(0.060)	(0.218)	(0.049)
generosity	0.002	0.008	-0.008	0.014	-0.004	-0.004	-0.017	0.001
	(0.009)	(0.009)	(0.013)	(0.011)	(0.001)	(0.010)	(0.022)	(0.012)
constant	-0.030	0.165	-0.008	-0.188	0.469***	0.424	0.552	0.443**
	(0.122)	(0.136)	(0.148)	(0.126)	(0.174)	(0.348)	(0.428)	(0.179)
Observations	720	720	360	360	1110	1110	360	750
N	72	72	36	36	111	111	36	75
adj. R^2	0.325	0.366	0.207	0.591	0.086	0.086	0.123	0.072

Panel regressions. Dependent variable: Contributions [0,1]. Independent variables: i) *m*; ii) *info* is a binary variable; iii) *period* [0,10]; iv) *mXinfo* interaction variable. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; regressions were clustered by experimental groups of three.

Table A3
Regressions by *m* levels (with control variables.

	$m = 0.4$	$m = 0.5$	$m = 0.75$	$m = 0.9$
	(1)	(2)	(3)	(4)
info	-0.166***	0.048	0.010	0.130
	(0.060)	(0.089)	(0.108)	(0.138)
period	-0.022***	-0.032***	-0.023***	0.001
	(0.006)	(0.007)	(0.007)	(0.008)
female	0.037	0.091*	-0.113**	-0.097
	(0.057)	(0.047)	(0.052)	(0.082)
risk	0.042**	0.021	-0.026	0.015
	(0.021)	(0.016)	(0.022)	(0.030)
trust	-0.127*	0.001	0.222**	0.021
	(0.067)	(0.078)	(0.113)	(0.115)
generosity	0.003	-0.001	-0.010	0.008
	(0.012)	(0.017)	(0.011)	(0.012)
constant	0.348***	0.516***	0.797***	0.541***
	(0.093)	(0.133)	(0.138)	(0.160)
Observations	360	570	540	360
N	36	57	54	36
adj. R^2	0.264	0.111	0.121	0.062

Panel regressions. Dependent variable: Contributions [0,1]. info=1 for comparative information treatments, period is the time variable [1,10]. Robust standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; regressions were clustered by groups.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.socec.2021.101679.

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