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Intergroup Solidarity and Local Public Goods Provision: An Experiment*

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Abstract

The effects from voluntarily choosing to accept or reject solidarity with outgroup members on ingroup cooperation has not been studied from a behavioral perspective yet. Our contribution to the literature on intergroup solidarity and ingroup cooperation is twofold. First, building on the public goods game, we design a novel experiment that allows us to study intergroup solidarity by allowing higher endowed groups to voluntarily share the benefits from their group account with less endowed groups. Second, we show that voluntarily – by voting – refusing solidarity with a less endowed group results in a breakdown in cooperation within the group that voted. The mere thought that sharing with outgroups is costly spills over to the ingroup contribution decisions.

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1 Introduction

Conflicts in the Middle East and North Africa pressed more than a million people to cross into Europe in 2015.[1] This phenomenon created divisions over how responsibility for refugees should be distributed across the European Union (EU) member states. The European Commission allocated quotas on asylum seekers to each EU member state in order to redistribute people who arrived in Greece, Hungary or Italy. However, the push to allocate refugees is a source of constant contention. The Hungarian government, for example, has called an anti-immigration referendum. Hungarians will be asked to cast a vote on whether they accept the EU quota schemes.[2] The Hungarian government is obviously not the only government overtly opposed to welcoming refugees. The question of immigrants in general and of refugees in particular has been a defining issue in the United Kingdom's June 2016 referendum on EU membership. The hostility to immigration has been argued to be an important contributing factor to the British voted to leave the EU.[3]

The extant literature in economics, political science and psychology points to different reasons why the indigenous population would refuse to welcome refugees, or immigrants more generally (Fong 2001). In economics, the costs and benefits of hosting immigrants have been investigated from a macroeconomic (Tumen 2016) and a market design perspective (Moraga and Rapoport 2014). In political science, it has been shown that elite discourse shape mass opinion and action on immigration policy (Brader *et al.* 2008) and that cultural differences matter (Hopkins 2015). Psychology research focuses on intergroup prejudices (Abrams *et al.* 2005). However, economic, psychological, and political science research are devoid of studies of individual reactions to voluntarily refusing to host immigrants that might have social consequences. One such neglected question concerns the effect of voluntarily (by voting) accepting or refusing solidarity with zero-income immigrants on the indigenous population's contributions to the provision of

[1] See the International Organization for Migration's database on http://migration.iom.int/europe/.

[2] More specifically, the question on the ballot will ask whether you are in favor of the EU being allowed to make the settlement of non-Hungarians obligatory in Hungary even if the parliament does not agree. The ballot additionally asks whether voters are for mandatory quotas or for the independence of Hungary.

[3] See $https : //www.cer.org.uk/sites/default/files/bulletin_{1}05_{s}t_{a}rticle1.pdf$

local public goods or public services.[4] Local public goods, by definition, benefit members of a group or community even when these individuals did not or could not contribute. However, adding a zero-income individual who cannot make his or her own contribution to the group or community is costly because of local public goods' congestible nature (Scotchmer 2002).

Solidarity with immigrants might mean that the indigenous population accepts to give access to various local public goods to zero-income individuals who were not previously part of their community. This, in turn, is likely to affect the decision of the indigenous population in terms of how much they want to contribute to the provision of local public goods. Similarly, if the majority votes against welcoming immigrants and therefore against letting them benefit from local public goods – such as hospitals, museums, or even sidewalks – the voting outcome could strengthen cooperation among the indigenous population because refusing solidarity may express one's commitment to preserve local public goods for natives, or conversely it might communicate one's egoistic type, thereby destroying group cohesion.[5]

Obviously, the question of how voting on immigration issues affects local public goods provision is not confined to the current situation in the EU. One could think of many economic, political and social crises that pressed millions of people to cross the border and led governments to raise the question of whether and how local citizens should be consulted. Mexican immigration in the United States of America is one bold example. More generally, Hatton (2016) perceptively shows how every year about 60 million people flee their home country and seek to cross into developed countries. The latter have developed different policy responses.

This article investigates the indigenous population's willingness to share the benefits from local public goods with zero-income individuals under a *voting procedure* and asks how the voting outcome impacts subsequent cooperation rates.[6] To study these questions we build on a firmly

[4]It should be noted that there are theoretical models predicting how local public goods contributions might be affected when a district attracts new immigrants. For instance, Schultz and Sjostrom (2001) developed a two-community model in which a district might experience congestion in the consumption of local public goods because it attracts new immigrants. However, there are no empirical studies of how the effect of immigration on the indigenous' population's willingness to contribute to the provision of local public goods.

[5]On the power of commitment in public goods games, see Croson (2007) and Dannenberg et al. (2014).

[6]As acknowledged in the previous paragraph, countries have developed different policy responses to immigration

established paradigm from experimental economics, namely, the public goods game. The public goods game has been extensively used to study cooperation in environments with income and wealth inequalities – an essential feature of the interaction between immigrants and the hosting population. Buckley and Croson (2006) were among the first to study the effect of inequality on group cohesion. They induced inequality by offering participants from the same group different endowments, holding equal between-groups income allocation. Another relevant work is Maurice *et al.* (2013) who varied the distribution of income within groups after some periods were played under symmetric income allocation, again holding equal between-group income levels. Both of these papers found that overall contributions do not decrease with the introduction of within-group income inequality.

Our contribution to the literature is twofold. First, we design a novel, two-part public goods experiment in which we introduce, in the second part of the experiment, income asymmetry *between groups*. That is, we keep symmetric the within-group endowment but vary the between-groups endowment. The between-groups asymmetry results in one-half of the groups being randomly selected to continue receiving an endowment similar to the first part of the experiment, while the other half of the groups have their endowments reduced to zero for the entire second part of the experiment.

Our second contribution is that in our main treatment of interest we ask subjects in each group with a positive endowment to vote on whether they are willing to share the benefits from their group's public account with one other group composed of subjects who, due to an exogenous shock, have zero endowment for the second part of the experiment. The voting outcome is decided by simple majority. To the best of our knowledge, this is the first experiment to study the effect on cooperation from introducing voting in an environment with income inequality. But we are not the first to examine the impact of voting on cooperation in the context of the public goods game. Our work is closely related to Dal Bo *et al.* (2010) who experimentally showed that the effect of a policy on the level of cooperation is greater when it is chosen democratically by the subjects rather than when it is exogenously imposed. Also Feld and Tyran (2006), Ertan

and asking citizens to vote on such issues is one of the many observed arrangements.

et al. (2010), Sutter *et al.* (2010) and Markussen *et al.* (2014) found that punishments and rewards in public goods games have a greater impact on behavior when they are allowed democratically.[7] There are, however, important differences with the aforementioned works. First, we investigate the effect of voting on cooperation in the context of endowment inequalities between groups. Second, our focus is on how ingroup cooperation is affected by the voting outcome rather than on how people behave under alternative policies, democratically or undemocratically adopted.[8]

In line with the aforementioned literature, we find a strong effect from allowing people to vote. However, we show how the implementation of voting significantly reduces cooperation for groups whose members were asked to vote (i.e., within the positive endowment groups).[9] We find, indeed, that subjects in the positive endowment groups who voted against sharing subsequently cooperated one-half as much within their own group compared to the control treatment without the vote. We further show how the mere act of voting against sharing resulted in a breakdown in cooperation within the positive endowment groups. The existing models of altruism (Becker 1974), warm-glow (Andreoni 1990), inequality-aversion (Fehr and Schmidt 1999), and conditional cooperation (Markussen *et al.* 2014) fail to point to a coherent explanation of the post-voting behavior we observe. The former three models would suggest that post-voting behavior should not be different compared to the treatment where positive endowment groups were not asked to vote, but were merely informed that there were zero endowment groups in the room. The strategic environment in the two treatments is the same. As for the conditional cooperation model proposed by Markussen *et al.* (2014), we should observe pre-vote conditional

[7]Other studies on the benefits of voting in public goods games include Dal Bo (2010), Kamei (2016a, 2016b)

[8]We should note from the outset that, contrary to the literature on the minimal group paradigm (see Hargreaves Heap and Zizzo 2009, Pan and Houser 2013), we did not induce group identity other than the random formation of groups, as is common in standard public goods experiments. In this sense, ingroup identity is minimal in our experiment. Indeed, the only difference between ingroups and outgroups is that they belong to different groups that have been randomly formed.

[9]To the best of our knowledge, the only other experiment showing that voting might have a negative effect on group cohesion is Cappelen *et al.* (2014). However, they investigate the effect of free-choice on people's willingness to redistribute but not on the voluntary contributions to the provision of public goods or of local public goods.

cooperators to decrease their post-vote contributions because the voting outcome may signal the egoistic type of the majority in one's own group. However, our data shows that, prior to voting, subjects who subsequently voted against sharing did not behave differently than subjects who voted in favor of sharing. The divide clearly appears after voting took place: contribution rates are considerably lower only for subjects who voted against sharing (which makes 70% of our subjects in the voting treatment), but not for those who voted in favor of sharing (30% of our subjects). One explanation for such effects has been called the "perception shift" hypothesis (Li *et al*. 2009), where a subject makes decisions based on social norms and shifts to gain-driven choices because the mere thought that one's self-interest is served when one refuses to share his/her group's account with a zero endowment group spills over to ingroup contribution decisions.

In the next section we describe the experimental parameters and implementation. Section 3 formulates a number of predictions stemming from the literature on social preferences. The results are presented in Section 4. Section 5 provides an explanation for observed behaviors, and the paper concludes in Section 6.

2 Experimental design

2.1 Experimental game

The basic structure of our experimental game follows the well-established design of a repeated linear public goods game employing standard parameters. Ledyard (1995) and more recently Chaudhuri (2011) provide elaborate descriptions of how public good games are implemented. Our experiment consists of two sequences of ten rounds each. Written instructions were provided for the first sequence only. Subjects were aware that a second sequence of ten rounds would be played but were not given any specific instructions about the game that would be played until the first sequence was completed.

In the first sequence of the game, subjects were randomly arranged into groups of five. Each group played ten rounds of the public goods game with the same partners. At the beginning of each round, each subject received an income of 20 tokens. These incomes stayed constant throughout the first sequence of the game and were common knowledge. Subjects then decided how many tokens to contribute to a public account. The remaining tokens were automatically placed in each subject's private account. Parameters were chosen to be consistent with those used in previous experiments. For every token that the group allocated to the public account, each of the five subjects in the group received 0.5 Experimental Currency Units (ECUs). For every token that the subject kept in his private account, he received 1 ECU (the exchange rate was 20 ECU = €1). The marginal per capita return (MPCR) from the public good is thus 1/2, as in previous studies (Andreoni 1990, Croson 1998). Consequently, the individual payoff function (π_i) is the following:

$$\pi_i = 20 - g_i + 0.5 \sum_{j=1}^{5} g_j$$

where g_i corresponds to the individual contribution decision to the public account and g_j is the group contribution.

At the end of each round the subjects received feedback about the number of tokens they contributed to the public account, the total contributions to the public account by their group, their earnings for that round, and their earnings to date (wealth).

2.2 Treatments

After the tenth round, subjects were given the set of instructions for the second sequence of the game. In the second sequence of the game subjects could be placed into one of four treatments, which we now outline.

Baseline treatment In the Baseline treatment, subjects were informed that the second sequence of the experiment is identical to the first sequence: the second sequence has ten rounds and the group composition does not change.

In addition to the Baseline treatment, we implemented three test treatments. In the three test treatments, after the first sequence of the game, subjects were informed that half of the groups in the room will be randomly selected to receive endowment and endowment of 0 ECUs for the next ten rounds. Henceforth, we will refer to these groups as the *zero endowment groups* and to the groups that would continue to receive an endowment identical to the first sequence of the experiment as the *positive endowment groups*. The language used in the instructions did not refer to lucky, positive or zero endowment groups, but informed subjects that the computer will randomly select half of the groups in the room to have no endowment for the entire second sequence of the experiment. The loss seriously impacted the zero endowment group's final earnings since in all of our treatments subjects knew that one of the two sequences would be randomly selected for payment at the end of the experiment.

No-solidarity treatment (NOSOL) In the NOSOL treatment, each positive endowment group played the same game as in sequence 1, while each zero endowment group was completely inactive and earned zero for this part of the experiment.When the second sequence of the experiment was selected for the final payment, the members of the zero endowment group earned €0 for the experiment and were paid only the €6 show-up fee. The *NOSOL* treatment was implemented in order to isolate the effect of the mere existence of zero endowment groups on the positive endowment groups members' contributions.

Voting treatment (VOTE) In the second test treatment, subjects in the positive endowment groups were asked to vote. They could vote for one of the following two options: (i) to share the benefits yielded by their group's public account with the zero endowment group or (ii) to refuse to do so.[10] Each session of the three test treatments were conducted with 20 subjects divided into 4 groups of 5 subjects. Therefore, we always had two zero endowment groups and two positive endowment groups in the room. Thus, when subjects were asked to vote in the *VOTE* treatment each positive endowment group could vote in favor or against sharing the benefits from their group's public account with one randomly chosen zero endowment group. The voting outcome

[10]The voting procedure was common knowledge. Subjects in the zero and positive endowment groups were aware that the positive endowment groups had to vote.

was decided by simple majority. The instructions explained that if the majority of the positive endowment group votes against sharing the public account then the game played by the positive endowment group would be exactly the same as in sequence 1, while the zero endowment group would be inactive and would earn €0 for the second sequence of the experiment (and possibly for the entire experiment). If the majority in the positive endowment group voted in favor of sharing the public account, then subjects were informed that the benefits from the group's public account would be divided by 10. We employed the same explanation of how benefits from the public account would be divided among group members as in the other treatments, except that instead of an equal division by 5, subjects were informed that if the majority votes in favor of sharing then the benefits from their group's public account would be divided by 10. It is also worth noting that we made sure not to give the impression that the group size changes in case the majority votes in favor of sharing. After every subject in the group voted, they were privately informed about the outcome of the vote, but not about the individual voting decision of the other group members. That is, subjects knew only whether the majority in the group voted for/against sharing the benefits from their public account with one zero endowment group. This way, we capture the essential features of voting on collective issues in real-life settings where only the voting outcome is publicly known, but individual voting decisions are not directly observable. Additionally, we decided to not inform subjects about the margin of votes against over votes in favor of sharing. This was done in order to keep constant the information given in each group that voted against or in favor of sharing, thereby increasing the number of independent observations for each voting outcome.[11]

The *VOTE* treatment was introduced to test for the effect of voting on the subsequent contribution decisions of the zero endowment group members. In case the majority votes against sharing, the game's strategic environment is identical to the *NOSOL* treatment. The *VOTE* treatment, in this case, isolates the effect of the expression of one's preferences regarding solidarity with out-group individuals on the subsequent contribution decisions within the positive endowment group. However, another possible outcome is that the majority in the group votes in favor of

[11] Future work could be conducted in order to test for the effect of providing information about how many members in one's group voted against sharing the group's public account with one zero endowment group.

sharing. The vote in favor of sharing implies a reduction in the individual return from the public account for subjects' in the positive endowment group from 0.5 to 0.25 – yet, the reduction preserves the nature of the interactions as a social dilemma. This is why we need to control for a change in the MPCR effect, independently of the voting procedure.

Imposed solidarity treatment (SOL) Our third test treatment, the *SOL* treatment, introduces solidarity exogenously. Each positive endowment group was asked to share the benefits yielded by the group's public account with one zero endowment group. Thus, in this treatment, the group contributions to the public account were automatically divided by 10 (i.e., the MPCR was changed from 0.5 to 0.25 automatically).

Table 1 provides detailed information about the experimental design as well as the number of subjects that participated in each treatment of the experiment.

Table 1: Experimental design

Subjects	**Groups**	**Matching**	**Sequence 1 Periods 1-10**	**Sequence 2 Periods 11-20**	**Active groups in sequence 2**
30	6	Partner	linear PGG	Baseline	6
80	16	Partner	linear PGG	NOSOL	8
80	16	Partner	linear PGG	VOTE	8
80	16	Partner	linear PGG	SOL	8

2.3 Practical procedures

The experiment consists of 14 sessions conducted in a computerized laboratory at LAMETA-LEEM laboratory in Montpellier, France.[12] The sessions were conducted between March and May 2016.[13] Twenty subjects participated in each session conducted for the three test treat-

[12]The computer program was developed with LE2M, the software dedicated to experimental economics developed by the engineers of the LAMETA.

[13]It is worth noting that the implementation of a given treatment was always decided randomly at the very beginning of each experimental session. Also, we should note that from March to May 2016 there was no "particular"

ments, while for the Baseline treatment there were 15 subjects for each session. The 270 subjects, invited via the ORSEE software (Grenier 2015), were randomly selected from a pool of more than 4,000 volunteers from the University of Montpellier. Nine out of ten subjects participated previously in a laboratory experiment. We ensured, however, that none had previously participated in a public goods game. Terminals were separated by lateral partitions to ensure complete anonymity. Payments were made privately at the end of the session. Subjects earned an average of €20. Sessions lasted about one hour, including initial instruction and payment of subjects.

3 Theoretical background and hypotheses

The benchmark game theoretic model tested in most laboratory experiments assumes away the impact that social preferences or social norms may have on behavior.[14] The model predicts that in our Baseline, *NOSOL*, *VOTE*, and *SOL* treatments none of the subjects will contribute to the public good. Indeed, agents for whom $\frac{\partial U_i}{\partial U_j} = 0, \forall j \neq i$ will contribute nothing and in the *VOTE* treatment will vote against sharing their group's public account with one positive endowment group (where U_i represents the utility of individual i and U_j represents the utility of the other individuals in and outside i's own group).

However, a number of competing models have been developed which are consistent with behaviors observed in laboratory public goods experiments. Becker's (1974) theory of altruism represents individual utilities as a function of the individual's own income and the income of other members within or outside of her own group. If individuals are equally concerned with all other individuals affected by their actions ($\frac{\partial U_i}{\partial U_w} = \frac{\partial U_i}{\partial U_o} > 0$, where U_w denotes the utility of

event relevant to the refugee crisis that could have changed people's attitudes regarding refugees or immigrants more generally.

[14]It is important to keep in mind that game theory, in general, is mute about the content of agents utility. The experimental literature testing game theoretic predictions chose to use self-regarding preferences in formulating the standard Nash equilibrium (Cox 2004). It is, however, possible to include a preference for others' income in an agent's utility functions and then apply Nash or subgame perfection to the resulting game.

members within one's group and U_o represents the utility of members outsides one's own group), they will contribute positive amounts to the group's public account and, in the *VOTE* treatment, will vote in favor of sharing the public account with one zero endowment group. However, if individuals are more concerned with those from their own-group than with those outside of their own group ($\frac{\partial U_i}{\partial U_w} > \frac{\partial U_i}{\partial U_o} > 0$), then they will contribute positive amounts to the public account, but will vote against sharing in the *VOTE* treatment. Andreoni's (1990) theory of warm-glow yields similar predictions.

The second type of social preferences that could affect individual contributions and voting decisions correspond to models of inequality-aversion. Fehr and Schmidt (1999) modeled an individuals aversion to the difference (positive and negative) between own and group average payoff. This model was amended by Bolton and Ockenfel (2000) who introduced a combination of self-interest with a concern for relative standing. If inequality-averse members of the positive endowment group believe that members from the zero endowment group have baseline payoffs less than the payoffs of the positive endowment group, then they will vote in favor of sharing their public account with the zero endowment group.

Our predictions are based on these models of social preferences. First, based on existing evidence from public goods experiments, we expect that subjects will contribute positive amounts in the Baseline treatment and cooperation rates will decrease with the repetition of the game (Gaechter and Herrmann 2009). Second, Becker's model of altruism and Fehr and Schmidt's model of inequality aversion imply that the *mere existence* of zero endowment groups should not affect the contribution decisions of members of the positive endowment group. We expect, therefore, that cooperation rates in the treatment in which the positive endowment group is technologically constrained not to share its public account with one zero endowment group – the *NOSOL* treatment – will not differ from cooperation rates in the Baseline treatment. Third, the aforementioned models would predict that subjects from positive endowment groups might change their behavior when given the opportunity to share their public account with the zero endowment group. If subjects from the positive endowment group are equally concerned with all other subjects affected by their actions, within or outside their own group, they will vote in favor of sharing their public account with one zero endowment group in the *VOTE* treatment. Their subsequent contribution decisions should be similar to the Baseline treatment.[15] However, if subjects from the positive endowment group are more concerned with ingroup members compared to those from another (zero endowment) group, as the literature on ingroup favoritism (Hargreaves, Heap and Zizzo 2009) suggests, subjects will vote against sharing their public account with the zero endowment group in the *Vote* treatment. Their subsequent contribution decisions should, nonetheless, be similar to the Baseline treatment.

4 Results

4.1 Baseline results

We first check if our data is consistent with those of previous public goods experiments. Figure 1 plots group contributions over the 20 periods composing the two sequences. The data for the first sequence – periods 1-10 – is pooled over the 14 sessions since the game implemented was identical across sessions in this sequence and we do not observe any statistically significant differences.

Consistent with previous experimental results, in our first sequence, contributions begin high and decrease over time. In the second sequence, our Baseline treatment also replicates the standard pattern observed in other public goods games. Indeed, we observe a restart effect in the first periods with contribution rates similar to the first periods of the first sequence followed by a decrease over time (Andreoni 1988). At the aggregate level, over the ten periods, the wilcoxon signed rank test shows that contribution rates are similar in our Baseline treatment from the second sequence to the contribution rates in the first sequence of the game (p=0.753).

[15] If we assume in the *VOTE* treatment that subjects are concerned with the other subjects affected by their contribution decisions, this assumption should also hold in our Baseline treatment. Contribution decisions should, therefore, be similar in the two treatments, independent of the voting procedure.

Figure 1: The evolution of average group contributions by treatment

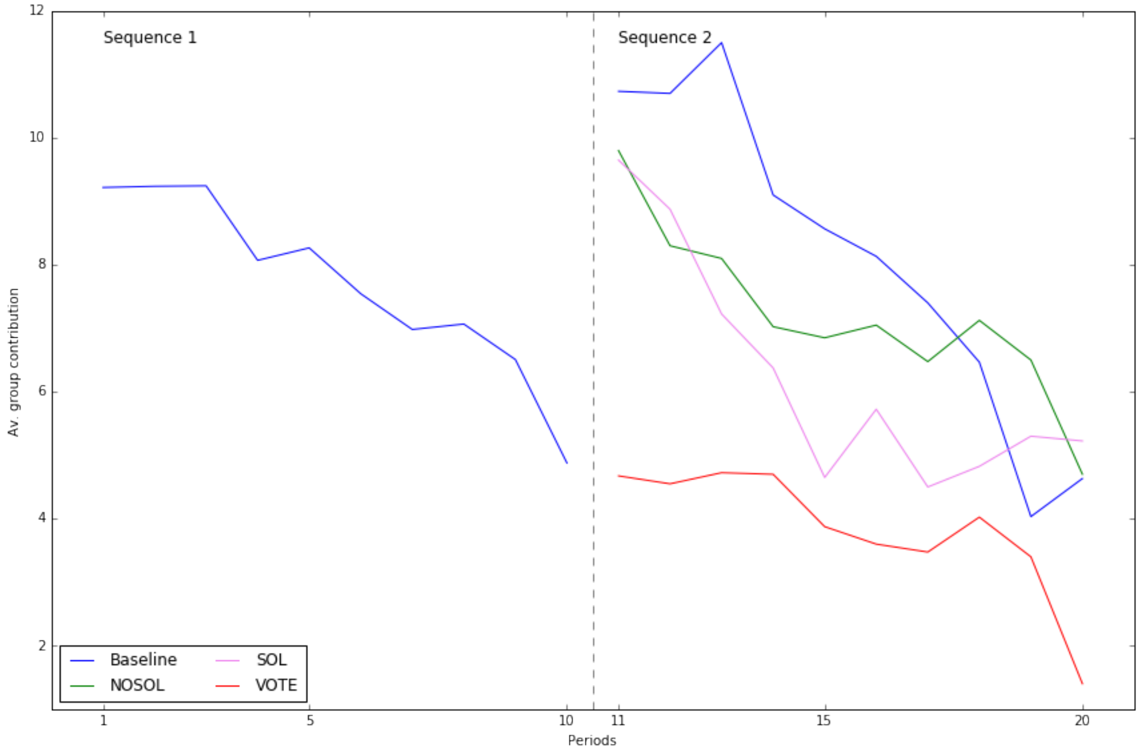
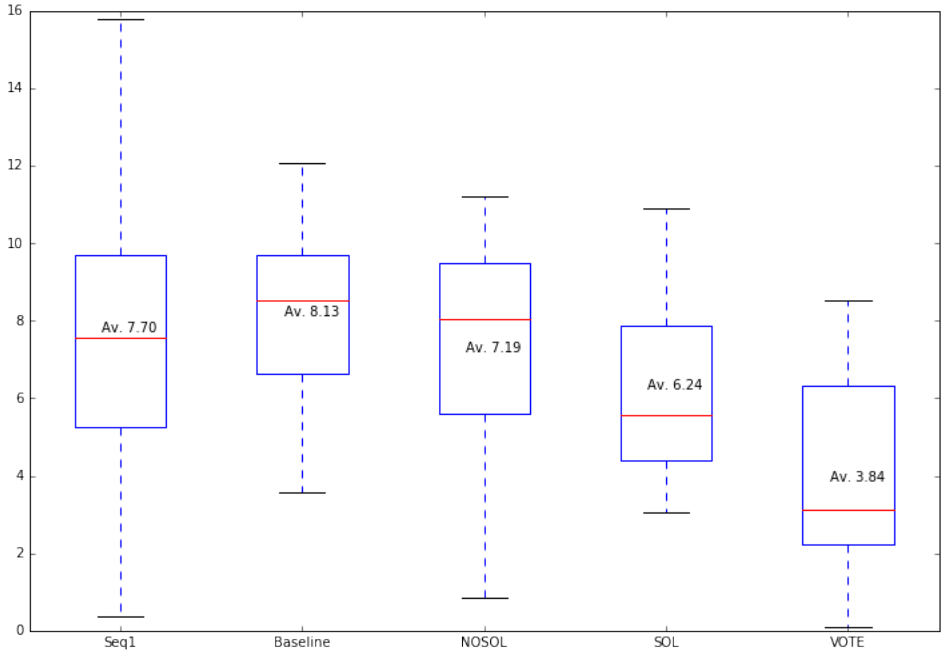


Figure 2: Average contributions and their dispersion by treatment



Result 1 *Subjects contribute positive amounts in the Baseline treatment and cooperation rates decrease with the repetition of the game.*

Figure 2 shows average contribution rates and their dispersion in the first sequence of the game and in each of our second sequence treatments. It is worth-noting the high dispersion in the first sequence of the game, which is explained by the large variation in contribution decisions in the first periods – an outcome commonly observed in public goods experiments that has been explained by confusion and "attempts at kindness" (Andreoni 1995, p.892).

Our main results of interest, however, focus on the impact of voting on contribution decisions when there is income inequality between groups. We will now present the data from the second sequence of the experiment in which we implemented our test treatments. We first present the results from the *NOSOL* treatment controlling for the mere existence (and common knowledge about the existence) of zero endowment groups. We then focus our attention on whether voting affects solidarity with zero endowment groups and cooperation within positive endowment groups.

4.2 NOSOL and SOL treatments results

Figure 2 shows that the mere introduction of zero endowment groups in our *NOSOL* treatment does not affect average contribution rates in the positive endowment groups compared to the Baseline scenario. In the *NOSOL* treatment, subjects in the zero endowment groups contribute on average 7.19 tokens, which is not statistically different from the 8.13 tokens contributed on average in the Baseline (Mann-Whitney ranksum test, p=0.325 when we consider the ten periods; p=0.280 when we control for end-game effects by considering only the first eight periods).

The comparison between the Baseline and the *NOSOL* treatments suggests that ending up in a positive endowment group does not alter behavior of the individuals in this group. We therefore conclude that the random selection of positive and zero endowment groups did not change the behavior of the subjects in the positive endowment group.

Result 2 *Cooperation rates in the treatment in which subjects randomly ended up in the positive endowment group and are technologically constrained not to share their public account with the zero endowment group – the NOSOL treatment – do not differ from cooperation rates in the Baseline treatment.*

The next step is to study the effect of voting on contribution decisions in environments with income inequality between-groups by comparing behavior in the *NOSOL* treatment to the *VOTE* treatment. However, one major concern in comparing these two treatments is that the *VOTE* treatment differs from the *NOSOL* treatment along two dimensions. The first dimension is the existence of a voting procedure in the *VOTE* treatment. However, subjects are also informed that in case of a majority vote in favor of sharing the benefits from the positive endowment group's public account with one zero endowment group, the per capita return (MPCR) from the public account will be halved compared to a voting outcome that is against sharing. We are therefore concerned with the effect that the possibility that the MPCR might change could have on the subjects' behavior in the positive endowment group independently of the voting procedure.

To control for the effect of a change in the MPCR independently of the voting procedure, we implemented the *SOL* treatment. Figures 1 and 2 show that when solidarity is imposed without a voting procedure, which means that the MPCR is automatically halved, contribution rates are not substantially different in the *SOL* treatment (6.24 tokens on average) compared to the *NOSOL* treatment (7.19 tokens on average) in which the MPCR did not change (Mann-Whitney ranksum test, p=0.247 when we consider the ten periods; p=0.159 when we control for end-game effects). Thus, the automatic reduction in the MPCR does not impact the behavior of subjects in the positive endowment groups. Hence, the only way through which the *VOTE* treatment may affect the contribution decisions of subjects in the positive endowment group is through the expression of one's solidarity preferences. We further investigate the results from the *VOTE* treatment.

4.3 VOTE treatment results

The first result from our *VOTE* treatment is that the majority vote in each of our eight positive endowment groups was against sharing the group's public account with a zero endowment group. Overall, 28 subjects who ended up in one positive endowment group voted against sharing (70%), while 12 subjects voted in favor of sharing (30%). However, the majority in each positive endowment group always voted against sharing (we had at most 2 out of 5 subjects who voted in favor of sharing in each positive endowment group).

Result 3 *In each positive endowment group, the majority voted against sharing the group's public account with one zero endowment group.*

Table 2 shows that the probability of voting against sharing is not influenced by the following factors for which we control at the end of the experiment with a questionnaire: subjects' risk aversion[16], their preferences for redistribution[17], their political orientation[18], and their gender. The regression also includes a control variable for the mean contribution in each subject's group prior to the voting procedure.

Table 2: GLM estimating the determinants of the probability of voting against sharing

	Estimate	**Std. Error**	**Z value**	**P value**
Risk aversion	-0.11700	0.12466	-0.939	0.348
Redistribution preferences	-0.08547	0.13652	-0.626	0.531
Political preferences	0.38260	0.25221	1.517	0.129
Gender	-0.46473	0.79121	-0.587	0.557
Mean contribution in sequence 1	-0.01417	0.07507	-0.189	0.850
N=40				

Due to the voting outcome, our *VOTE* treatment has exactly the same strategic environment as the *NOSOL* treatment. The only difference between the two treatments is that in the *VOTE* treatment subjects voluntarily refused to share the public account with one zero endowment group, while in the *NOSOL* treatment this was decided automatically (i.e., subjects were constrained not to share). In both treatments, 5-person groups played the public goods game with an MPCR of 0.5 knowing that there were zero endowment groups in the room who will earn zero for the second sequence of the experiment and possibly for the entire experiment, excluding the show-up fee.

Figure 1 shows that in the *VOTE* treatment there is a dramatic decrease in contribution decisions in the positive endowment groups over the ten periods that followed the voting. In effect, from period 11 to period 20, average contributions are always substantially lower in the *VOTE* treat-

[16]We measured risk aversion based on Vieider *et al.*'s (2015) survey question, which has been shown to correlate with incentivized lottery choices in most countries.

[17]We measure preferences for redistribution with a question taken from the World Value Survey. The question asks to what extent the responded agrees with the following statement: "The government should take measures to reduce income inequalities". This question has been used by others in the literature (see Alesina and Giuliano 2009).

[18]We asked subjects whether politically they situate themselves on the extreme left of the political spectrum, left, center, right, or extreme right.

ment compared to the *NOSOL* treatment. Figure 2 confirms this visual trend by showing that contributions in the *VOTE* treatment in periods 11-20 (3.84 tokens on average) are almost two times lower than in the *NOSOL* treatment (7.19 tokens on average). As shown in Table 3, where we summarize the between treatments comparison, the difference between the *VOTE* and the *NOSOL* treatments is highly statistically significant[19] (Mann-Whitney ranksum test, p=0.025 when we consider the ten periods; p=0.041 when we control for end-game effects).

Table 3: Summary of between treatments comparison in sequence 2

Treatment 0	**Treatment 1**	**P-value**
Baseline	NOSOL	0.325
Baseline	SOL	0.135
Baseline	VOTE	0.011
VOTE	NOSOL	0.025
VOTE	SOL	0.051
NOSOL	SOL	0.247

Note: In each cell N=8 (one independent observation by group), except for the Baseline where N=6. The null hypothesis in the two sided Mann-Whitney ranksum test is that the two samples come from the same population.

Result 4 *Contribution rates are nearly two times lower in the treatment in which positive endowment groups voluntarily refused to share their group's public account compared to the treatment in which the no-sharing outcome has been decided automatically without voting.*

5 What explains the breakdown in cooperation?

The mere act of voluntarily refusing to share with a zero endowment group resulted in a breakdown in cooperation within the positive endowment groups. The existing models of altruism (Becker 1974), warm-glow (Andreoni 1990), and inequality-aversion (Fehr and Schmidt 1999)

[19] A random-effects Tobit regression shows that risk aversion, redistribution preferences, subject's political orientation, and gender do not significantly affect individual contribution decisions.

fail to point to a coherent explanation of the post-voting behavior we observe. As explained in the previous section, the three models would suggest that post-voting behavior should not be different compared to the treatment where positive endowment groups were not asked to vote but were merely informed that there were zero endowment groups in the room. The strategic environment in the two treatments is the same.

Besides altruism and inequality-aversion, one of the most popular theories explaining observed behavior in public goods games is conditional cooperation (Fischbacher *et al.* 2001). For conditional cooperators the voting outcome may act as a signal about the prosocial type of the other group members (Markussen *et al.* 2014). If subject *i* voted in favor of sharing but was informed that the majority in his/her group voted against sharing, *i* may infer that there is a majority of selfish individuals in his/her group. Voting against sharing may indeed signal that one has no (or very low) altruistic inclinations, feels no warm-glow from helping others and does not care about high levels of inequality in income and wealth. Absent these three motives identified in the literature as the main drivers of prosocial behavior (Bowles and Gintis 2013), conditional cooperators may want to respond by substantially reducing their own contributions.

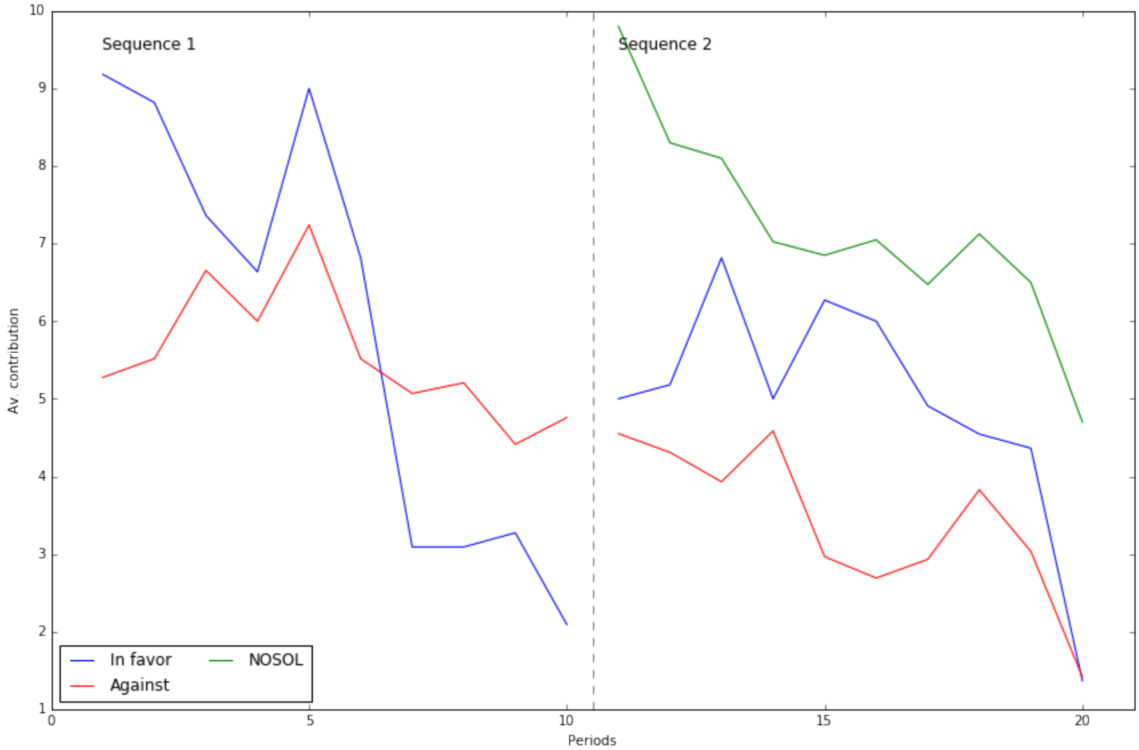
In our *VOTE* treatment, conditional cooperators, once informed about the voting outcome, should contribute less than contribution rates observed in the *NOSOL* treatment. The signal about the majority's social preferences in one's group[20] is indeed missing in the *NOSOL* treatment.

Figure 3 graphs average individual contributions for subjects who voted in favor of sharing their group's public account with one zero endowment group and for subjects who voted against, prior (in sequence 1) and after (in sequence 2) the voting took place.

First, Figure 3 suggests that subjects who voted against sharing after the tenth period, on average, did not behave as free-riders in the first sequence of the game – their contributions are rather stable over the first ten periods. In fact, in the first sequence of the experiment, average contribution rates are rather similar between those who subsequently voted against and those

[20] By social preferences, we refer here to theories about direct or indirect altruism, and inequality-aversion. This broad definition of social preferences can be found in Bowles and Gintis (2013).

Figure 3: Average individual contributions before (sequence 1) and after voting (sequence 2)



who voted in favor of sharing their group's public account. The divide in contribution decisions occurs after the tenth period – that is, after the voting stage. Contribution rates for subjects who voted in favor of sharing clearly exceed the amounts contributed by those who voted against sharing – an observation that is not congruent with the hypothesis emphasizing the signaling effect of voting. More specifically, Figure 3 shows that subjects who voted in favor of sharing their group's public account behave subsequently similarly to subjects in the *NOSOL* treatment, except for periods 11 and 12. The introduction of voting seems to have affected negatively only the behavior of subjects who voted against sharing their group's public account with a zero endowment group. This is consistent with the arguments presented by Bicchieri and Mercier (2014) evaluating the mechanisms that change social norms. Institutional interventions, such as voting to provide access to publicly-provided good as studied here, can affect behavior only through adjusting the empirical norms assessed in the community. The announcement of voting outcomes provide a new social comparison that can distort prior assessments. As they point out, changing norms presents its own collective action problem, and the inability to do so can have deleterious effects. Here, the evidence supports subjects shifting for norm compliance to gain-sensitive behavior. However, due to the small number of observations, this is solely a conjecture that we cannot support with robust statistical tests. The conjecture should be tested in future works.

The small number of subjects who voted in favor of sharing (12 subjects overall) and of those who voted against (28 subjects) does not allow us to make deterministic statements about the mechanisms that made subjects who voted against sharing contribute extremely low amounts in periods 11-20. Our results are, however, exploratory. One explanation for such effects has been called the "perception shift" hypothesis, where a subject makes decisions based on social norms and shifts to gain-driven choices because the mere thought that one's self-interest is served when one refuses to share his or her group's account with a zero endowment group spills over to ingroup contribution decisions (see Li *et al.* 2009, Vohs 2015).

6 Conclusion

Introducing intergroup endowment inequality and a voting mechanism in the public goods game, we have demonstrated that voluntarily refusing solidarity (by voting) with a less-endowed group results in a breakdown in cooperation within the group that voted. There is an aversive effect of voting against solidarity with outgroup subjects on ingroup cohesion as measured by the contribution rates to the public good. Recent models emphasizing people's social preferences (altruism, warm-glow, inequality-aversion) or the human inclination to conditionally cooperate fail to point to a coherent explanation of the post-voting behavior we observe. We hypothesized that the aversive effect from voting against intergroup solidarity on ingroup cooperation might be due to a "perception shift" from norm sensitive choices to gain-sensitive choices. Indeed, we found that voting affected the contribution decisions solely for subjects who voted *against* sharing their group's account with a zero-endowed group. The mere thought that sharing with outgroups is costly spilled over to the ingroup contribution decisions, changing the context of subject's decision from a social (or moral) to a gain-oriented one, whereas subjects who voted *in favor* of solidarity with a less endowed group contributed similar amounts as in our baseline treatment with no voting mechanism. Our results are, however, exploratory and the mechanisms underlying the behavior we observe should be examined with further controlled experiments.

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