The perils of democracy

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Abstract

In this work we examine a common social dilemma in experimental economics, the public goods game, to determine how voting impacts pro-social behavior. As noted in Markussen, Putterman & Tyran (2014, *Review of Economic Studies*), a democratic dividend exists. Couching the public goods game in a phenomenon that is playing out in much of the world – drastic income inequality – we examine the decision of groups to share local public goods with groups that have, effectively, no endowment to contribute toward public nor private consumption. Our results show the perils of democracy in that subjects in the position to vote use their advantageous situation to reward the ingroups at the expense of the less endowed outgroup members.

1 Introduction

Much research in economics, law and political science studies the conditions under which people choose to implement efficient institutions (i.e. institutions that would lead to Pareto efficient outcomes). While some argue that the reduction of transaction costs is essential in reaching efficient institutional arrangements (North 1990), others stress the need to better understand the cognitive underpinnings of institutional lock-in (Kuran, 1995). In the last two decades, a different

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yet complementary strand of literature emerged and is currently thriving in the experimental economics of cooperation in public goods games that shows that the way in which an institution is implemented matters. More specifically, the results of many laboratory experiments suggest that there is a "dividend of democracy" (Markussen et al., 2014): the opportunity of democratic choice leads to higher cooperation levels than when the same choice is made by a third-party.

We design a public goods experiment to study the "dividend of democracy" in a novel environment characterized by radical income-asymmetry between groups, a phenomenon that is receiving considerable recent interest in the scientific community (Atkinson et al., 2011) and in public policy circles. Indeed, a meta-analysis of the use of voting in experimental papers in the top 25 journals that publish experimental research finds that 133 papers utilize voting in the research. Of these manuscripts, only 21 of these papers examine voting as the object of interest in the research, with the remaining 112 papers simply utilizing voting to determine a state of the world. Thus, directly examining the role of voting in decision-making involving public funds merits further investigation. From a policy perspective, conflicts in the Middle East and North Africa pressed public authorities in many developed countries to develop different policy responses to face the growing concerns about how immigration in general, and the refugee crisis in particular, may affect social order. Solidarity with refugees may mean that the native population accepts to give access to various *local public goods* – congestible – to zero-income individuals who were not previously part of their community. This, in turn, is likely to affect their behavior in terms of how much they want to contribute to the provision of local public goods. If the majority of natives vote against welcoming refugees 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 natives 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.[1] Obviously, the question of how voting on immigration issues affects local public goods provision is not confined to the current situation in the European Union. 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 and how this will affect social cohesion. Mexican immigration in the United States of America is one bold example. More generally, Hatton (2016) shows how each year about 60 million people flee their home country and seek to cross into developed countries, thus urging the latter to develop different policy responses.

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

As such, in our main treatments 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 account with one other group composed of subjects who, due to an exogenous shock, have zero endowment. The voting outcome is decided by simple majority. We then examine how the positive endowment subjects vote and how the voting outcome affects their post-voting contributions to the public account as compared to a setting where the same outcome is implemented without a vote. Our results show the perils of democracy in that subjects in the position to vote use their advantageous situation to reward the ingroups at the expense of the less endowed outgroup members.

To the best of our knowledge, this is the first experiment to study the effects on cooperation from introducing voting in an environment with income inequality between groups. But we are not the first to examine the impact on cooperation from voting 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 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.[2] 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 mere act of voting whether or not to share the benefits from the group's account with zero-income outgroups.[3]

The second dimension that we manipulate in the experimental design is the costs and benefits of voting in favor (against) sharing the group account with one zero endowment group. While the first situation does not enable subjects endowed with zero funds to contribute to the shared

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

[3]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. We do acknowledge that immigrants have a different identity from native populations. However, since this is the first laboratory experiment to test for the effect of inter-group solidarity on ingroup cooperation, we wanted to study this question in a neutral environment so as to provide a lower bound on how local public goods provision could be affected by the refusal to share the social benefits with "others". It is reasonable to expect that when people belong to different group identities then the probability that one group agrees to share the benefits from its public account with another group is lower than in the absence of identity differences. Experimental research found, indeed, that subjects tend to protect the interests of their ingroups (Klor and Shayo, 2010).

public account, in a second treatment we implement a situation where the zero endowed subjects can work (put in effort in a real effort task) in the public interest. Thus, in case of a vote in favor of sharing, the efforts made by the zero endowed subjects would yield tokens placed automatically in the public account. This treatment intends to mimic a policy that would require incoming zero-endowed individuals to do public service work.

Our results confirm that a dividend from democracy exists in that subjects are inclined to increase their contributions to the public account in a voting environment. However, they are not inclined to share the public account with the zero endowment group that cannot work in the public interest. Alternatively, when the zero endowment group can work in the public interest, positively endowed subjects are now inclined to share but they significantly reduce their contributions after voting in favor of sharing. An analysis of individual behavior reveals that subjects voting in favor of sharing the public account reduce their post-voting contributions in the two scenarios, when the zero endowed subjects can and cannot work in the public interest, but the rationale is different. When subjects in favor of sharing are in the minority, they subsequently reduce their contributions because they treat the voting outcome as a signal about the prosocial type of the other group members. Indeed, when subjects in favor of sharing learned that the majority voted against, their contributions converged to the contribution level of the week free-riders who had voted against sharing. We conjecture that voting against helping a less endowed group was treated as a signal that the majority in the group has no (or very low) altruistic inclinations, feels little warm-glow from helping others, and does not care about high levels of inequality in income. Absent these three motives identified in the literature as the main drivers of prosocial behavior, conditional cooperators responded by substantially reducing their own contributions.[4]

In the treatment where the zero endowed subjects can work, the subjects that vote in favor of sharing are in the majority but since the incentives to vote in favor are now different, those voting in favor of sharing are not necessarily prosocial (or inequity averse). The post-voting behavior in this treatment reflects the majority's intention to take advantage of the subjects in the zero endowment group that can work to contribute to the public account.

Thus, contrary to the extant literature that emphasizes the benefits of democratically chosen

[4]As noted by a referee, there are alternative explanations for why subjects would vote against sharing in our game. First, the game is subject to the typical problem of majority voting games: there are multiple equilibria. In particular, if j thinks nobody will vote in favor of sharing anyway, then sharing or not sharing does not make any difference. If we assume that j expects his vote to determine the voting outcome, then his voting decision reveals his willingness to share the group's account with one zero-endowment group.

changes in the context of public goods games, we find that the implementation of voting has unintended negative consequences. This is, of course, not to say that democracy has a dark side. We have implemented a particular set-up to simulate voting in a laboratory context where communication – to mention but one important institutional aspect of democracy – was not allowed. The study of intergroup helping when there is radical income asymmetry should be pursued in more ecologically relevant contexts. Our experiment acts as a first attempt to isolate the effect of voting to (not) share public goods with less-endowed others on cooperation within the voting group.

The remainder of the paper is structured as follows: in the next Section, we review the literature on the behavioral effects of allowing people to choose a specific institution as compared to when people are assigned to the same institution, Section 3 presents the experimental design and the way the experiment was implemented, Section 4 discusses the results and Section 5 concludes.

2 The effect of democratic choice on cooperation

In this section we review the extensive experimental literature on the behavioral effects of allowing people to choose a specific environment in a social dilemma as compared to when people are assigned to the same environment. The bottom line is that there are a number of studies finding that the opportunity of collective choice leads to higher cooperation levels than when the same choice is made by an unaffected third-party. This seems to be a robust result.

The experimental literature on the freedom of choice in a social dilemma burgeoned with oft-cited article "Achieving compliance when legal sanctions are non-deterrent" by Tyran and Feld (2006). The authors investigate whether the effect of a mild sanction depends on the sanction being exogenously imposed or endogenously enacted. They implement a public goods game with groups of 3 playing a one-shot game under different conditions. The experiment has two main design features. First, there are three sanction levels: (1) in the "no law" condition, the sanction for free-riding is zero, (2) in the "severe law" condition, the sanction is high enough to deter a subject who solely cares about his/her own monetary gains, and (3) in the "mild law" condition, the sanction is too law to deter rational and egoistic agents from free-riding. The second design feature concerns how the law is enacted: (i) it is either exogenously implemented – in which case the sanction is meted out by the experimenter – or (ii) or endogenously – in which case subjects decide on the enactment of the law by anonymous majority vote. Each subject participates in either the exogenous or the endogenous condition. In the exogenous condition, each subject makes a contribution decision for each of the sanction levels: no law, mild law, severe law. In the endogenous condition the sequencing is mild law, severe law. After each treatment, in the endogenous condition, subjects first participate in the referendum on the enactment of law and then make their contribution decision for all possible outcomes of the referendum. It is worth noting that subjects are not informed about the outcomes (the outcome of the referendum, the contribution level in their group, their earnings) of either sanction level until the end of the experiment. This is equivalent to playing a one-shot game.

The bold result of their study is that mild law works to change people's behavior solely when enacted endogenously. Further, contributions are significantly higher if either law was accepted than if it was rejected. However, cooperation levels appear to be lower when the mild law is rejected compared to the no-sanction treatment, even though the strategic environment is identical in the two conditions. While the authors do not report whether this difference is significant, it is important to highlight that in groups that rejected the mild law, the vote appears to have an overall negative effect on cooperation. Since the majority of subjects vote in favor of the enactment of the law, Tyran and Feld as well as the literature that followed focus on the positive effects on cooperation from voting to choose a specific environment.

In a related study, Sutter et al. (2010) investigate the comparative attractiveness of sanctions as compared to rewards. The authors use a design that systematically varies the following three factors: (i) the institution governing the provision of the public good (sanctions, rewards, no-sanction and no-reward), (ii) the leverage of the sanction or reward, (iii) the way in which the institution is determined (exogenously by the experimenter or endogenously by the group itself through a voting procedure). Sutter et al. (2010) find that the endogenous institutional choice has a large and positive effect on cooperation levels compared to a setting in which the sanctions or rewards have been determined exogenously. Additionally, the reward option was chosen almost exclusively when the two institutions were highly effective (defined as the ratio of monetary consequences of being sanctioned or rewarded to the costs of sanctioning or rewarding).

While this literature shows how subjects democratically choose institutions so as to affect behavior, it does not necessarily disentangle the effect of the democratic choice from the effect through the choice of incentives that subjects will face. Dal Bo et al. (2010) present a laboratory experiment designed to disentangle these effects and test for a direct effect of democracy. Their experiment consists of two sequences. In the first sequence, groups of 4 subjects were created and each subject played 10 periods of a prisoner's dilemma with the other group members. The second sequence is similar to the first one but subjects could vote to implement a tax on unilateral defection. While under the initial payoffs the unique Nash equilibrium is mutual defection, under the modified payoffs both mutual defection and mutual cooperation are Nash equilibria.

The novelty in their experiment is that after subjects voted on the implementation of the tax, the computer randomly chose whether to consider the votes in each group. If the computer considered the votes, then the majority won[5]. Conversely, if the computer did not consider the votes, it randomly chose whether to implement the tax in that group. The subjects were then informed whether the computer considered their group's vote – i.e. whether the environment within which they will interact is endogenously chosen – and whether the tax is adopted or not.

The authors present a decomposition of the total effect of implementing the tax on unilateral defection into a selection effect (the fact that subjects that voted to implement the tax are likely to behave differently than subjects who voted against it), a treatment effect and a direct effect from democracy. They find that of the total effect, 8% is due to the selection effect, 66% is due to the treatment effect and 26% is due to the direct effect of democracy. That is, the democracy dividend represents slightly less than 50% of the treatment effect.

The literature following the aforementioned studies extend the "dividend of democracy" hypothesis to choices between formal and informal sanctions (Markussen et al., 2014, Kamei et al., 2015). What is more, Markussen et al. (2014) suggest that voting affects behavior because people treat the voting outcome as a signal about the prosocial type of the other members of the community. In other words, for conditional cooperators, the voting outcome acts as a signal about the prosocial type of the other group members. In our experiment, the vote outcome may signal the extent to which subjects in the group identify with "others". If subject i voted in favor of sharing but was informed that the majority in his group voted against sharing, i may infer that there is a majority of selfish individuals in the 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

[5] In case of a tie, the computer broke the tie.

cooperators may want to respond by substantially reducing their own contributions.[6]

3 Experimental design

3.1 Experimental game

Standard public goods 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. To correct for a surprise restart-effect (Andreoni 1988), subjects were informed that a second sequence would be played. However, written instructions were provided for the first sequence only.

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.

[6] As noted by a referee, subjects may also reduce their contributions if the group votes against sharing because zero contributions at the group level reduce the difference in earnings between the positive-endowment subjects and the zero-endowment ones. However, our results at the individual level do not support this conjecture. Indeed, we observe that solely subjects who voted in favor, while the majority in their group voted against, reduce significantly their post-voting contributions.

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).

3.2 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.

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 five treatments. In the five 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 no endowment 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*.[7] 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 contributions of the positive endowment group members.

[7] The language used in the instructions was neutral, we 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.

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 one zero endowment group or (ii) to refuse to do so.[8] Each session of the three test treatments were conducted with 20 subjects divided into 4 groups of 5 subjects each. Therefore, we always had two zero endowment groups and two positive endowment groups in the room. Thus, in the *VOTE* treatment each positive endowment group could vote in favor or against sharing the benefits from the group's account with one randomly chosen zero endowment group. The voting outcome was decided by simple majority. The instructions explained that if the majority of the positive endowment group votes against sharing the group's 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). Subjects also knew in advance that if the majority in the positive endowment group votes in favor of sharing the group's public account, then the benefits from the group's public account would be divided by 10 for the entire 10 rounds of sequence 2.[9] 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.

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 aggregate number of votes against and in favor of sharing the public account. 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

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

[9]If the majority votes in favor of sharing, the group optimum still requires that subjects contribute all tokens to the group account; the dominant strategy, however, is to contribute nothing. We decided to divide the amount contributed by 10 because we did not want to assume that the technology to produce the public good changes with the sharing decision. In our experiment, the "internal" and "external" MPCR are equal (Goeree et al., 2002). These changes might influence the voting decision in different ways. However, in this experiment, we want to examine post-voting behavior and not voting itself.

outcome.

The *VOTE* treatment was introduced to test for the effect of voting on the subsequent contribution decisions of the positive 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 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).

Conditional solidarity (VOTE_Conditional and SOL_Conditional): In our *VOTE* treatment, there is a high cost of sharing the group's public account with a zero endowment group because the members of the latter group are totally inactive. In the real-world, the less-endowed ones can put in some effort in the public interest. To investigate the voting and post-voting behavior of positive endowment subjects when the zero endowment subjects can put in some effort that would yield tokens placed directly on the shared public account, we implement the *VOTE_ Conditional* treatment. This treatment is similar to the *VOTE* treatment with the following exception: if the positive endowment group votes in favor of sharing the group's account with a zero endowment group, then there is common knowledge that the zero endowed subjects will have the possibility to do 5 real-effort tasks at each period and for each completed task 2 tokens will be placed on the shared group account. The task consists of counting all the zeros in a matrix consisting of 1s and zeros. This way, each member of a zero endowment group has zero endowment but can work to increase the amount on the shared group account with up to ten tokens.[10] While in the *VOTE* treatment voting in favor or against sharing the group account does not matter if subjects act on the belief that all would keep their tokens on their private accounts, that is no longer the case in the *VOTE_ Conditional* treatment. Indeed, subjects from the positive endowment groups who seek to merely maximize their monetary earnings should vote in favor of sharing if they expect the subjects from the zero endowment group to put in some effort.

The fifth treatment, *SOL_ Conditional*, is the control for our VOTE_Conditional treatment. In the former treatment sharing is automatically decided by the computer, similarly to the *SOL* treatment.

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
120	24	Partner	linear PGG	VOTE	12
80	16	Partner	linear PGG	SOL	8
80	16	Partner	linear PGG	VOTE_Cond	8
80	16	Partner	linear PGG	SOL_Cond	8

Total N of subjects: 470

3.3 Practical procedures

The experiment consists of 24 sessions conducted in a computerized laboratory at LEEM laboratory in Montpellier, France. The sessions were conducted between March 2016 and April 2017.[11] Twenty subjects participated in each session conducted for the five test treatments, while for the Baseline treatment there were 15 subjects for each session. The 470 subjects, invited via the ORSEE software (Greiner, 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

[10] In case of a vote in favor of sharing, all subjects that share the same group account would be informed about the total number of tokens on the group account. We decided to fix the maximum contribution of the zero endowed subjects to ten tokens in order to keep the inequality between the maximum possible of subjects from the positive endowment groups and subjects from the zero endowment groups.

[11] It is worth noting that the implementation of a given treatment was always decided randomly at the very beginning of each experimental session.

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.

4 Data

We introduce our data by first identifying the treatments by pseudoname. There are six treatments within our design that are separately run in the second sequence of the experiment. In addition, for the VOTE and VOTE_Conditional treatments, we separately consider the two voting outcomes – that is, when groups voted in favor and against sharing. First, we ran sessions where the *Baseline* treatment was run in the second sequence, which acts as our comparison treatment. Second, a treatment where subjects were not permitted to share the local public good with the zero endowment group, which we label *No_Sol* for no solidarity. Third, a treatment where subjects could share their local public good with the zero endowment group that could not make contributions to the public account, which we label *Sol*. Fourth, a treatment where subjects could share their local public good with the zero endowment group that could work to make contributions to the public account, which we label *Sol_Conditional*. Fifth, a treatment where subjects could vote to share their local public good with the zero endowment group that could not make contributions to the public account, which we label *Vote*. We break these data into groups that vote in favor, *Vote_Favor*, and those that vote against, *Vote_Against*. Finally, a treatment where subjects could vote to share their local public good with the zero endowment group that could work to make contributions to the public account (*Vote_Conditional*), which we break apart into *Vote_Condional_Favor* for those voting in favor and *Vote_Conditional_Against* for those voting against.[12]

Table 2 breaks down the average number of public goods contributions across the treatments within our experiment, while Figure 1 visually displays the distribution of contributions.[13] Two observations are immediately obvious even upon visual inspection. First, in the *Vote_Against* treatment the average contributions to the public account are significantly lower than the average

[12]Note that no group ever voted to not share the public account with the zero endowment group in the treatment where the zero endowment group could work to contribute to the public account.

[13]We do not display the boxplot of the *Vote_Favor* treatment since only one group ever voted in favor of sharing the public account with a zero endowment group that was incapable of working to contribute to the public account.

Table 2: Contribution by treatment and sequence

Treatment	Sequence	Public Account Contributions							
		Mean	Std	Min	25%	Median	75%	Max	Obs.
Baseline	1	8.19	6.90	0.00	1.00	7.00	14.00	20.00	450
	2	8.32	6.92	0.00	1.00	8.00	15.00	20.00	450
No_ Sol	1	7.48	6.66	0.00	0.00	7.00	11.25	20.00	400
	2	7.19	6.83	0.00	0.00	5.00	11.00	20.00	400
Sol	1	9.11	7.89	0.00	1.00	8.00	19.00	20.00	400
	2	6.24	6.78	0.00	0.00	5.00	10.00	20.00	400
Sol_ Conditional	1	9.81	6.99	0.00	4.00	10.00	16.00	20.00	350
	2	6.45	6.12	0.00	0.00	5.00	10.00	20.00	350
Vote_ Favor	1	13.06	9.07	0.00	2.00	20.00	20.00	20.00	50
	2	5.02	7.45	0.00	0.00	0.00	10.00	20.00	50
Vote_ Against	1	5.43	6.37	0.00	0.00	3.00	8.00	20.00	550
	2	3.98	5.32	0.00	0.00	2.00	6.00	20.00	550
Vote_ Cond_ Favor	1	7.19	6.34	0.00	1.00	5.50	11.25	20.00	400
	2	5.39	5.73	0.00	0.00	4.00	9.00	20.00	400

contributions in nearly every other treatment.[14] Specifically, contributions in the *Vote_ Against* treatment are significantly different from contributions in the *Baseline* and *No_ Sol* treatments. Second, contributions in the *Vote_ Conditional_ Favor* treatment are significantly different from contributions in the *Baseline* treatment. So, even in simple summary statistics we find some evidence that voting treatments could be altering the decision-making process. However, these summary statistics could be masking treatment, session and period effects that are capturing some of the heterogeneous effects that are being observed in the summary statistics. In the following section we more carefully dissect the impact of voting on the choice to share the public account with our two types of zero endowment groups.

5 Results

We now dig further into our results to identify the impact of voting on public goods contributions decisions. As a starting point, we display the evolution of average group contributions in Figure 2. We first note that no matter the treatment, all contributions decay with the number of rounds in a sequence. However, the decay is much more pronounced in the non-voting treatments.

Concentrating on the *Baseline* treatment, we observe the common restart effect between the first and second sequence of the experiment (Andreoni, 1995). Importantly, the *Sol*, *No_ Sol* and, to a lesser extent, the *Sol_ Conditional* treatments also display the restart effect. However, both of our voting treatments (*Vote_ Against* and *Vote_ Conditional_ Favor*) do not display the restart

[14] Appendix Table 1 displays the Mann-Whitney tests for each of the comparisons of public goods contributions across all treatments.

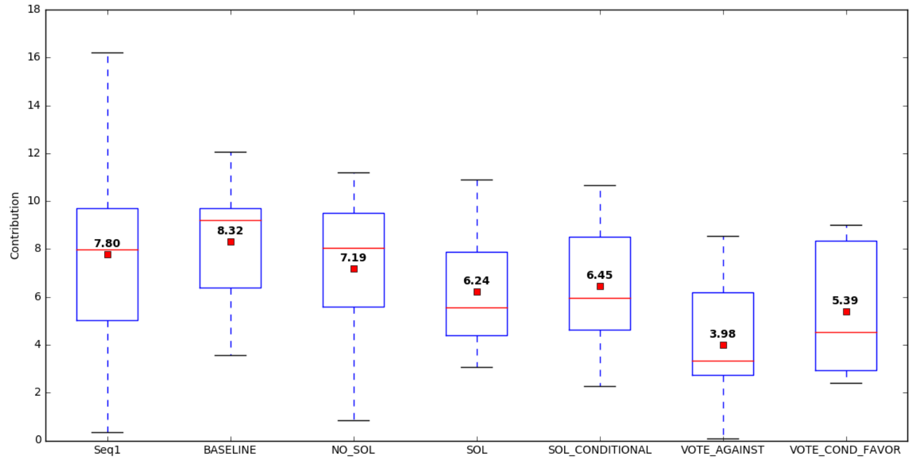

Figure 1: Boxplot of contribution levels by treatment

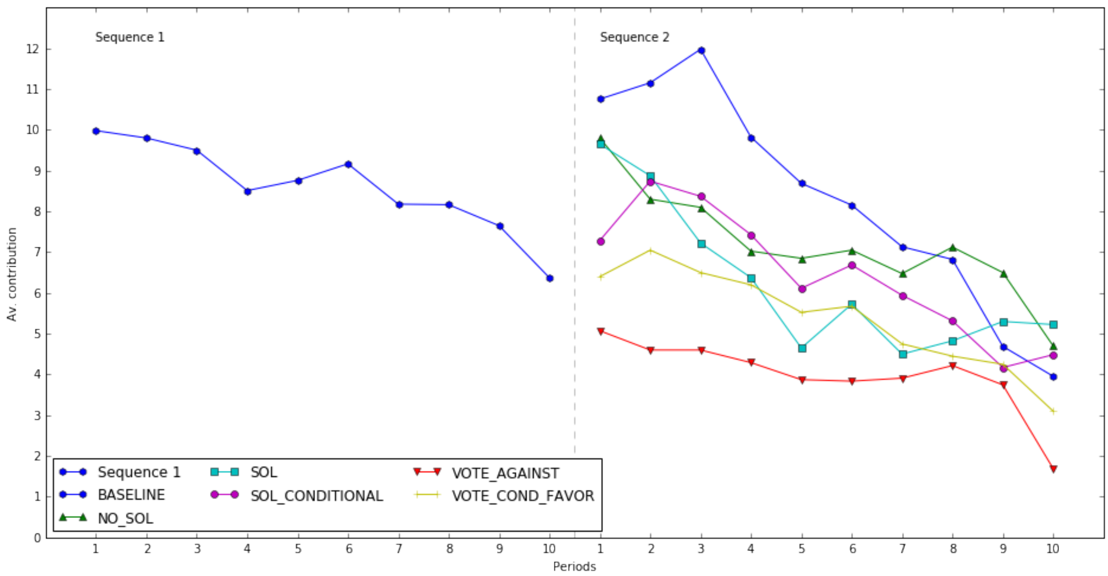

Figure 2: Evolution of average group contributions across treatments

effect. To further examine the differences in contribution decisions across these treatments, we now turn to a formal econometric analysis.

While Figure 2 displays the average effects across all subjects within our treatments, individual, group or session-specific effects could be influencing the observed behavior. To account for these potentially confounding effects, we conduct a Tobit regression analysis that examines the effect of each of these treatments on public account contributions in the second sequence of our experiment by using the following specification for each subject i in group g facing treatment j:

$$Contribution_{igj} = \alpha + \beta_j Treatment_j + \theta_g Group_g + \gamma_i controls_i + \delta Period + \phi Session + \epsilon_{igj} \quad (1)$$

Equation 1 controls for group- and session-specific variation as well as period fixed effects. Additionally, we include controls for the previous period contribution, average contribution of other group members and average contribution in the first sequence of the experiment in our analysis so that β_j identifies the between subjects effect of each treatment relative to the omitted treatment (*Baseline*).[15] As such, we can think of the coefficients for each of the treatments variables as the average treatment effect.[16]

Column 1 of Table 3 utilizes the full sample to assess the impact of the separate treatments. A couple results are worth noting. First, relative to the *Baseline* treatment, the *Vote* and *Vote_ Conditional* treatments generate higher contributions to the public account. Alternatively, the *Sol* treatment generates fewer public account contributions than the *Baseline* treatment.[17] Importantly, the non-voting analog to the *Vote* treatment (*No_ Sol*) and the non-voting analog to the *Vote_ Conditional* treatment (*Sol_ Conditional*) yield statistically significantly different results when comparing these coefficient estimates. We therefore find some evidence that the voting environment matters in determining whether there is a democratic dividend. To control for restart and end-of-sequence effects, we restrict our sample to only periods 3-8 in column 2 and re-run the same econometric specification. Our results remain largely unchanged and the analogous voting and non-voting treatments continue to yield statistically different coefficients.

To further explore the effect of the voting treatments, we break down the voting treatments

[15] We cluster standard errors at the group level.

[16] The results presented in Table 3 are qualitatively similar if we instead use OLS regression analysis. See Table 2 in the Appendix

[17] Tests for equality of average treatment effects are all rejected at the 1% level.

into those individuals that voted in favor of sharing the public account, and those that did not. Since there were two environments where voting occurred, we further split voting decisions by those treatments where the zero endowment individuals can conditionally contribute and the treatments where they cannot. Given that only one group ever shared the public account with the zero endowment group in the *Vote* treatment, the *Vote_ Against* variable can be viewed as the majority vote within the *Vote* treatment. Similarly, in the *Vote_ Conditional* treatment, we can view the *Vote_ Conditional_ Favor* as the majority vote since all groups voted in favor of sharing the public account with the zero endowment group. The results of breaking voting decisions apart are presented in columns 3 & 4.

Separating the vote conditions into those that did and did not vote in favor of sharing the public account with the zero endowment group reveals an interesting relationship between voting behavior and public contributions. In the *Vote* condition, we find that those voting against and in favor of sharing the public account both increase their contributions to the public account, although the contributions are greater in the majority group (those voting against sharing the public account).[18]

A different phenomena is observed in the *Vote_ Conditional* treatments, however. That is, those individuals voting in favor of sharing the public account (the majority group) reduce their contributions to the public account, whereas those voting against sharing the public account actually increased their contributions.[19] Thus, the majority group (those voting in favor of sharing the public account) voted to share the public account, then reduced their contributions by approximately one unit relative to the baseline treatment.

In sum, when we examine the average treatment effects, controlling for time-invariant features of each session, as well as period-specific fixed effects across sessions, we learn a few intriguing features about the role of voting in experiments. First, the *Vote* treatment yields higher average contributions to the public account than the equivalent non-voting treatment (*No_ Sol*). Second, when we enable the zero endowment group to work to contribute to the public account, the

[18]Note that there are only 19 subjects in total that voted in favor of sharing the public account, which could be driving the magnitude of these results. Moreover, these individuals appear to be behaving in manner that is consistent with conditional cooperation (Fischbacher et al., 2001). The pairwise t-test of the difference in contributions is significant at the 5% level, indicating that contributions are higher among those voting against sharing. In addition, comparing first to second sequence decisions, we observe that relative to behavior in the first sequence those voting against sharing the public account tend not to change their contribution behavior, while those voting in favor of sharing tend to contribute significantly less. For the comparison of first to second sequence behavior, see Table 3 in the Appendix

[19]The pairwise t-test of equality of contributions to the public account across these two treatments rejects the null hypothesis at the 0.01% level.

Table 3: Treatment Effects of Public Account Contributions

	1	2	3	4
Sol	-1.554^{***}	-1.291^{***}	-1.559^{***}	-1.298^{***}
	(0.251)	(0.112)	(0.248)	(0.111)
No_ Sol	-0.687	-0.948	-0.694	-0.958
	(0.722)	(0.656)	(0.726)	(0.662)
Vote	1.774^{**}	1.840^{***}		
	(0.790)	(0.555)		
Sol_ Conditional	-0.146	-0.072	-0.153	-0.083
	(0.673)	(0.483)	(0.679)	(0.486)
Vote_ Conditional	1.458^{***}	1.382^{***}		
	(0.305)	(0.231)		
Vote_ Against			2.057^{*}	2.238^{***}
			(1.25)	(0.800)
Vote_ Favor			0.901^{**}	1.653^{***}
			(0.383)	(0.471)
Vote_ Conditional_ Against			1.666^{***}	1.532^{***}
			(0.286)	(0.252)
Vote_ Conditional_ Favor			-0.938^{***}	-0.966^{***}
			(0.013)	(0.250)
Intercept	-13.409^{***}	-12.424^{***}	-9.165^{***}	-8.159^{***}
	(1.819)	(1.632)	(1.080)	(0.932)
Num. obs.	3,730	2,625	3,730	2,625
Pseudo-R^2	0.194	0.219	0.194	0.218

Robust standard errors clustered at the session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1. We include session and period fixed effects, as well as the previous period contribution, average contribution of other group members and average contribution in the first sequence of the experiment in our analysis. Columns 1 and 3 include the full sample, while columns 2 and 4 use only periods 3-8.

behavior of the treatment effects becomes slightly more complicated. Now, the majority group votes in favor of sharing the public account with the relatively poorer group, but their contributions now decline in the treatment where they are sharing the public account (*Vote_ Conditional* treatments) relative to the treatment where subjects are not sharing the public account (*Vote* treatments).

Tyran (2018) offers possible rationales for this behavior. In the vote treatment, those voting against sharing the public good become more pro-social toward the in-group once they determine that the public account will not be shared. Alternatively, those voting in favor of sharing learn, through the vote outcome, the prosocial type of the other group members, resulting in reductions in contributions to the public account. When we examine the *Vote_ Conditional* and *Sol_ Conditional* treatments, though, we see those in favor of (against) sharing the public good reducing (increasing) their public contributions (relative to the *Baseline* treatment), despite the group now voting in favor of sharing the public account. The rationale is now, however, different because the incentives to vote in favor are not the same as in the *Vote* treatment. Those voting in favor of sharing are not necessarily prosocial (or inequity averse). The post-voting behavior in the *Vote_ Conditional* treatment reflects the majority's intention to take advantage of the less endowed ones.

6 Conclusion

Voting is a commonly utilized democratic mechanism that is used to determine a state of the world, typically when more than one state of the world could be realized. The use of voting is ubiquitous in democratic environments, leading many to believe that it improves the well-being of societies. This sentiment is reflected generally in academic research, and more specifically in experimental public choice research (Tyran and Feld 2006).

In this work we examine a common social dilemma in experimental economics, the public goods game, to determine how voting impacts pro-social behavior. As noted in Markussen et al. (2014), a democratic dividend exists. Couching the public goods game in a phenomenon that is playing out in much of the world (drastic income inequality), we examine the decision of societies to share local public goods with a population that has, effectively, no endowment to contribute toward public goods. We test this social dilemma in two situations. The first situation does not enable subjects endowed with zero funds to contribute to the public good, while the second enables zero endowment subjects to work to contribute to the public account. Subjects that are positively endowed are given the option to vote to share their contributions to a public account with the zero endowment group in each treatment of the experiment. Moreover, these treatments are compared to environments where subjects are placed into situations where their contributions are automatically (not) shared with the zero endowment group.

Our results reflect the complexity of voting, as we confirm that a dividend from voting does exist, in that subjects are inclined to increase their public contributions in a voting environment. However, they are not inclined to share the public account with the zero endowment group that cannot work to contribute to the public account. Alternatively, when the zero endowment group can contribute to the public account, positively endowed subjects are now inclined to share the public account with the zero endowment group. However, the positively endowed subjects now reduce their contributions to the public account, preferring instead to free-ride on the zero endowment group's working contributions. Investigating the contribution decisions amongst those who voted in favor and against sharing the public account reveals that subjects voting in favor of sharing the public account reduce their contributions, but the rationale for the reductions likely differs. When subjects in favor of sharing are in the minority, they likely treat the voting outcome as a signal about the intentions (not) to cooperate of the majority in the group that voted against sharing. Alternatively, subjects voting against sharing the public account increase their contributions to the public account to enhance the payoffs of the in-group.

In the treatment where the zero endowed subjects can work, the subjects in favor of sharing are in the majority but since the incentives to vote in favor are now different, those voting in favor of sharing are not necessarily prosocial (or inequity averse). The post-voting behavior in this treatment reflects the majority's intention to take advantage of the subjects in the zero endowment group that can work to contribute to the public account.

This research has implications for the use of voting in social dilemmas, and contributes to a growing area of public choice research relating to the use of voting in democratic environments. While most research highlights the dividend gained in democratic environments, it is worth noting that there are potential dark sides to democracy that could result in socially undesirable environments. Voting on immigration is one domain where democracy may lead to undesirable effects on cooperation in the voting community. 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 and how this will affect social cohesion. Mexican immigration in the United States of America is one bold example. More generally, Hatton (2016) shows how each year about 60 million people flee their home country and seek to cross into developed countries, thus urging the latter to develop different policy responses to face the growing concerns about how immigration may affect social order. Our experiment shows that voting to help (or not) less endowed individuals may increase cooperation in the voting groups at the cost of refusing solidarity, but it may also decrease cooperation in the voting group and lead to an exploitation of less endowed citizens.

Appendix

	Treat. 0	Treat. 1	p-value
0	BASELINE	NO_SOL	0.500
1	BASELINE	SOL	0.136
2	BASELINE	SOL_CONDITIONAL	0.289
3	BASELINE	VOTE_FAVOR	0.295
4	BASELINE	VOTE_AGAINST	0.005
5	BASELINE	VOTE_COND_FAVOR	0.038
6	NO_SOL	SOL	0.495
7	NO_SOL	SOL_CONDITIONAL	0.603
8	NO_SOL	VOTE_FAVOR	0.561
9	NO_SOL	VOTE_AGAINST	0.035
10	NO_SOL	VOTE_COND_FAVOR	0.270
11	SOL	SOL_CONDITIONAL	0.954
12	SOL	VOTE_FAVOR	0.846
13	SOL	VOTE_AGAINST	0.076
14	SOL	VOTE_COND_FAVOR	0.494
15	SOL_CONDITIONAL	VOTE_FAVOR	1.000
16	SOL_CONDITIONAL	VOTE_AGAINST	0.147
17	SOL_CONDITIONAL	VOTE_COND_FAVOR	0.524
18	VOTE_FAVOR	VOTE_AGAINST	0.772
19	VOTE_FAVOR	VOTE_COND_FAVOR	0.846
20	VOTE_AGAINST	VOTE_COND_FAVOR	0.386

Table 1: Mann-Whitney tests of average contribution (group as independent data)

Table 2: Environment Effects of Public Account Contributions - OLS

	1	2	3	4
Sol	-5.420^{***}	-6.0861^{***}	-5.420^{***}	-6.086^{***}
	(0.057)	(0.078)	(0.037)	(0.029)
No_ Sol	-5.412^{***}	-6.085^{***}	-5.412^{***}	-6.085^{***}
	(0.039)	(0.079)	(0.099)	(0.028)
Vote	0.800^{***}	0.343^{***}		
	(0.038)	(0.076)		
Sol_ Conditional	-5.402^{***}	-6.086^{***}	-5.402^{***}	-6.083^{***}
	(0.096)	(0.070)	(0.033)	(0.486)
Vote_ Conditional	2.760^{***}	0.524^{***}		
	(0.040)	(0.073)		
Vote_ Against			0.767	0.524
			(1.124)	(1.264)
Vote_ Favor			0.808^{***}	0.298
			(0.281)	(0.316)
Vote_ Conditional_ Against			3.716^{***}	3.346^{***}
			(0.641)	(0.663)
Vote_ Conditional_ Favor			1.326	0.910
			(0.961)	(0.995)
Intercept	3.833^{***}	5.381^{***}	3.833^{***}	5.381^{***}
	(0.161)	(1.174)	(0.295)	(0.932)
Num. obs.	3,730	2,625	3,730	2,625
R^2	0.423	0.454	0.427	0.457

Robust standard errors clustered at the session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1. We include session and period fixed effects, as well as the previous period contribution, average contribution of other group members and average contribution in the first sequence of the experiment in our analysis. Columns 1 and 3 include the full sample, while columns 2 and 4 use only periods 3-8.

Table 2 provides the equivalent OLS estimates to the Tobit estimates reported in Table 3. In general, the point estimates tend to be larger and more statistically significant than the Tobit results. This is to be expeeted, since the OLS estimates tend to downward-bias slope coefficient estimates and upward-bias intercept estimates due to the inconsistency of utilizing an OLS estimate on censored data.

Table 3: Comparing First to Second Sequence Decisions

	1	2	3	4
Baseline	−0.018	0.688	−0.017	0.688
	(1.361)	(1.435)	(1.361)	(1.434)
Sol	−10.158***	−10.293***	−10.153***	−10.287***
	(3.312)	(3.369)	3.311)	(3.367)
No_ Sol	−11.553***	−12.180***	−11.547***	−12.172***
	(2.328)	(2.369)	(2.326)	(2.367)
Vote	−4.248***	−4.262**		
	(1.922)	(2.080)		
Sol_ Conditional	−12.160***	−6.086***	−12.154***	−12.335***
	(2.641)	(0.070)	(2.639)	(2.807)
Vote_ Conditional	−2.638***	0.524***		
	(0.752)	(0.073)		
Vote_ Against			−4.542*	−4.105
			(2.498)	(2.567)
Vote_ Favor			−4.097**	−4.338**
			(1.926)	(2.151)
Vote_ Conditional_ Against			−1.689	−1.558
			(1.051)	(1.013)
Vote_ Conditional_ Favor			−5.005**	−5.271**
			(2.214)	(2.352)
Intercept	−0.140***	2.982***	−0.168	3.001***
	(1.026)	(1.035)	(1.019)	(1.028)
Num. obs.	8,480	5,950	8,480	5,950
Pseudo R^2	0.084	0.085	0.084	0.085

Robust standard errors clustered at the session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1. We include session and period fixed effects, as well as the previous period contribution, average contribution of other group members and average contribution in the first sequence of the experiment in our analysis. Columns 1 and 3 include the full sample, while columns 2 and 4 use only periods 3-8.

Table 3 displays the environment and treatment effects that include data from both the first and second sequences. This analysis, therefore, examines the environment/treatment effect by comparing behavior in the first sequence to behavior in the second sequence. By including additional controls, we can identify the environment/treatment effect that is separate from idiosyncratic group or individual behavior. As expected, contributions in the *Baseline* treatment do not vary from contributions in the first sequence. However, contributions are significantly lower in the *Sol*, *No_ Sol* and *Vote* treatments. Relative to behavior in the first sequence, those voting against sharing the public account tend not to change their contribution behavior, while those voting in favor of sharing tend to contribute significantly less. In the conditional voting environment the behavior flips. This behavior persists in the conditional voting environments.

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