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Inequality and public good provision: An experimental analysis

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Abstract

Recent studies report that economic inequality is associated with reduced government expenditures on social programs. Several prominent social scientists, including Putman (Putnam, R., 2000. Bowling Alone. Simon and Schuster, New York), attribute this to the detrimental "psychosocial effects" of group heterogeneity on cooperation. We test the hypothesis that inequality within a group reduces individual contributions in a public goods experiment. Unlike previous examinations of inequality and public good provision, we introduce inequality by manipulating the levels and distributions of fixed payments given to subjects. When made salient through public information about each individual's standing within the group, inequality reduces contributions to the public good for all group members.

JEL classification: JEL codes: C9; H4

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1. Introduction

Several recent empirical studies report deleterious effects of inequality on various economic, political and social indicators.[1] One of the more intriguing explanations for such findings is that inequality reduces the tendency of individuals to cooperate with fellow citizens, and thereby generates or exacerbates a host of societal ills (e.g., Putnam, 2000). We conduct a novel test of

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[1] Examples of subject-specific studies on the effects of inequality include Forbes (2000) on economic growth and investment, Mellor and Milyo (2001,2002) on health, Mayer (2001) on education and Fajnzybler et al. (2002) on crime.

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this hypothesis by examining the effects of induced inequality on the propensity of subjects to contribute in a canonical public goods game of the type examined by Isaac et al. (1984).

We introduce inequality by manipulating the level and distribution of a fixed payment given to subjects for participating in a public goods experiment; importantly, this treatment does not alter the set of feasible actions available to subjects, nor is it known to be otherwise associated with differences in expected behavior. This feature of our experimental design is in contrast to previous experimental studies of inequality and public good provision that introduce inequality by varying the ability to contribute (the endowment) or the value of the public good across subjects, and in doing so, alter the Nash equilibrium prediction in some cases. Consequently, this study also represents a contribution to the growing experimental literature on the effects of inequality in public goods games.

In half of the sessions in our experiment, each individual's placement in the distribution of fixed payments is revealed to all players prior to the start of play; this feature makes our work similar to Ball et al.'s (2001), which reports that status awards made in a pre-game ceremony and known to all parties influence subsequent behavior in a market experiment. In addition, we incorporate several survey-based measures of subject traits into our empirical analysis; as a result, our study complements previous experimental research on trust by Glaeser et al. (2000).

When information on the distribution of fixed payments is private, we observe no significant effect of inequality on contributions; that is, relatively deprived subjects do not contribute substantially different amounts than other subjects, nor are aggregate contributions lower in groups with unequal distributions of fixed payments. However, when inequality is made salient through public information about each individual's standing in the group, we obtain very different results. In some of our analyses, relative deprivation of subjects is associated with lower contributions, as might be expected for reasons of altruism (e.g., Becker, 1974) or asymmetric inequality aversion (e.g., Fehr and Schmidt, 1999).[2] However, once we control for the treatment effect of inequality at the group level, relative deprivation is no longer significantly associated with contributions. Instead, the presence of inequality itself reduces contributions to the public good for all group members, regardless of their relative position. As such, this study provides novel support for recent claims that inequality has implications for cooperation in collective action problems. The importance of the salience of inequality is also consistent with recent research demonstrating that some—but not all—measures of group heterogeneity are negatively associated with the efficacy of collective action (e.g., Alesina and La Ferrara, 2000; Cardenas, 2003; Costa and Kahn, 2003a,b).

The remainder of this paper is organized as follows. In Section 2, we review the literature linking income inequality to the provision of public goods, and in Section 3 we summarize the findings from public goods experiments in which inequality is introduced. Section 4 includes a description of our experiment, the results of which are reported in Section 5. The last section concludes with a discussion of the implications of our work.

2. Income inequality and public goods: the non-experimental literature

How is inequality related to the provision of public goods? Two distinct causal pathways have been proposed. One school of thought holds that inequality is linked to policy outcomes via its impact on the workings of the democratic process. For example, all else constant, an increase in inequality may imply that the median voter is less well-to-do, and so more favorably disposed

[2] On the implications of altruism and inequality aversion for public goods games, see Buckley and Croson (2003).

to public expenditures, especially those with a strong redistributive component (Meltzer and Richard, 1981,1983).[3] On the other hand, if public policy is driven by elite preferences, increased inequality may be associated with pressure to shrink the size and scope of government.

In contrast to these political mechanisms, a second school of thought is found within the recent literature on social capital. In this view, inequality undermines group cohesiveness thereby impeding collective efficacy and dampening other-regarding preferences (Putnam, 2000). Although the social capital hypothesis is nascent and evolving, it is supported by several empirical studies that find statistically significant associations between assorted measures of social capital (e.g., generalized trust or membership in voluntary associations) and various indicators of well-being including economic growth, improved health, and reduced crime.[4]

Like these theoretical underpinnings, the empirical findings on the relationship between inequality and public goods provision are also varied. For example, both Lindert (1996) and Moene and Wallerstein (2002) find that inequality across countries is associated with lower public spending, while Milanovic (2000) finds the opposite. In addition, Kaplan et al. (1996) shows that state-level expenditures on education in the United States are inversely related to state-level inequality, but Alesina et al. (1999) do not find a significant association between income inequality and expenditures on productive public goods across U.S. metropolitan areas. In a review of this literature, Osberg et al. (2003) note that divergent findings may result from the difficulties of comparing government programs and distributional data across countries, or from different choices of the particular measure of income inequality. Even so, a greater challenge to this literature is the fact that both inequality and levels of public good provision are jointly determined within the political process.

Other measures of heterogeneity, namely ethnic and racial fragmentation, are perhaps more immune to endogeneity concerns. Alesina et al. (1999) find a significant association between such measures and expenditures on public goods; in addition, Luttmer (2001) demonstrates evidence of racial group loyalty in support for social spending. When taken together with recent work showing that income inequality is a determinant of social capital (Brehm and Rahn, 1997; Kawachi et al., 1997; Alesina and La Ferrara, 2000), these findings provide additional, albeit limited, support for the contention that inequality influences public good provision.

A consistent finding in the literature on social capital is that not all forms of group heterogeneity are important determinants of the efficacy of collective action. For example, using the General Social Survey, Alesina and La Ferrara (2000) finds that participation in one or more voluntary membership organizations is negatively associated with metropolitan area income inequality, as well as racial and ethnic fragmentation, but not age fragmentation.[5] Costa and Kahn (2003a,b) conduct a similar analysis using data from several surveys; they find that membership is negatively associated with metropolitan area income inequality, racial fragmentation and birthplace fragmentation; however, neither the magnitude nor significance of these measures is consistent across surveys. In a study of shirking within companies of the Union Army, Costa and Kahn (2003c) report desertions and other acts of cowardice are positively associated with group heterogeneity based on income, ethnicity, occupation or age, albeit not significantly so for income inequality. These findings suggest that not all forms of heterogeneity are salient for group cooperation in

[3] See Mulligan (2001) for a recent critique of the Meltzer–Richard hypothesis.

[4] Some of the empirical studies linking social capital to measures of well-being include Alesina and La Ferrara (2000) on civic participation, Zak and Knack (2001) on economic growth, Knack (2002) on the quality of government, Mellor and Milyo (2005) on health and Galea et al. (2002) on crime.

[5] However, see Vigdor (2002) for a critique of the use of fragmentation indices in this literature.

all times or places. Research on the consequences of heterogeneity on the efficacy of collective action must then distinguish between salient and non-salient forms of heterogeneity. This also raises concerns about whether those forms of heterogeneity that are salient in the laboratory are likewise salient in the field, and vice versa.[6]

In this paper, we conduct a new empirical test of the relationship between inequality and public goods contributions using data from a laboratory experiment. Our experimental approach offers several advantages over non-experimental investigations. First, we are able to manipulate the form and salience of inequality within a particular reference group. Second, the use of an experimental design allows us to isolate the causal effect of inequality on public goods contributions and to avoid the problem of endogeneity present in some of the non-experimental literature. Finally, we can assess the degree of inequality for a given group and each individual's placement in the group distribution without concern about measurement error. Of course, financial constraints, as well as other concerns, prevent us from using payments that substantially alter an individual's disposable income. For this reason, our treatment is perhaps better interpreted as generating heterogeneity within the subject group (as opposed to income inequality).

3. Public goods experiments

The public goods experiment used in this study is a variation of the game first introduced by sociologists Marwell and Ames (1979, 1980, 1981), and later adapted by Isaac et al. (1984). Each individual in a group of N members is given a number of tokens to divide between a private account and a group account (i.e., the public good). The private account earns a return of P per token to the individual. The sum of all contributions made to the group account, denoted by G, is multiplied by some amount M and shared equally by all members of the group. Hence, each group member earns $(M/N)*G$ from the group account. In the standard design of this game, the return to the group account is a linear function of the total number of tokens in that account. If $P > M/N$, it is individually optimal to put all tokens in the private account. Additionally, if $P < M$, it is socially optimal for all subjects to put all tokens in the public account, making this a prisoner's dilemma game.

Variations of this public goods game have been used extensively in economics experiments for more than two decades, and a number of empirical regularities have been documented. Contrary to the Nash prediction of zero, contributions to the public good generally start in the range of 40–60% of the endowment. Repetition reduces contributions, but rarely to zero; on the other hand, provision points[7] and communication among subjects generally increase contributions to the group account. Contributions also increase as the return from allocating one token to the public good (M/N) rises, holding the return from allocating one token to the private good (P) constant. This is an intuitively obvious result that is not predicted by theory (for the set of $P > M/N$). Other factors have mixed effects on contributions, including gender (Eckel and Grossman, in press), repeated interactions with the same subjects versus random pairings after each repetition (Andreoni and Croson, in press) and financial punishments for free riding (Anderson and Stafford, 2003). Ledyard (1995) and Anderson (2001) discuss these major findings from public goods experiments.

6 For example, Cardenas (2003) studies commons games with experimental subjects draw from rural Colombian villages; wealth inequality among subjects is associated with less cooperation when subjects are allowed to communicate with each other, but not otherwise.

7 Provision points are threshold amounts that must be reached before anyone can receive a benefit from the group account.

Studies examining the effect of inequality (of some sort) in public goods experiments can be broadly classified along two dimensions—the structure of the game and the source of the inequality. In *linear public goods experiments*, the marginal value of the public good is constant, and the Nash equilibrium predicts zero contributions to the public good. In *non-linear public goods experiments*, the marginal value of the public good declines with the size of the group account, and the Nash equilibrium generally predicts positive contributions to the public good. Within these two structures, inequality has been introduced either in the *endowment* that subjects must split between private consumption and the public good, or in the *value of the public good* relative to some fixed value of private consumption. However, variations in endowments change the feasible set of alternatives to individuals (and are known to influence individual behavior), while changes in the value of the public good might alter the predicted Nash outcome. Therefore, previous studies have not isolated the treatment effect of inequality on group cohesion; we are able to do so, because unlike the extant literature, we introduce inequality in the distribution of fixed payments to subjects.

Several studies examining inequality (among endowments or the value of the public good) in linear public goods setting are reviewed by Ledyard (1995). For example, Bagnoli and McKee (1991) and Rapoport and Suleiman (1993) find that inequality reduces contributions to the group account, while Marwell and Ames (1979,1980) report that inequality has no effect on contributions. These studies interact inequality with threshold provision levels in different ways, which may in part explain the mixed nature of their findings. In another linear public goods game, Brookshire et al. (1993) interact inequality in the value of the public good with information; in some cases, group account contributions are unaffected by inequality, while in others, contributions increase.

One linear study that looks exclusively at the effect of inequality is that of Fisher et al. (1994). Contributions to the group account are found to be higher when subjects vary in their valuation of the public good, but features of this study make it difficult to attribute the result to inequality *per se*. In particular, their result is also consistent with another common finding in the literature—that contributions to the group account increase as the value of the public good rises, holding the value of private consumption fixed.

Several other studies have introduced inequality into non-linear public goods games; as noted earlier, this often changes the Nash prediction and makes the optimal contribution to the public good generally greater than zero.[8] Chan et al. (1996) present a design in which increasing the degree of inequality (from equality to moderate inequality to extreme inequality) in endowments usually predicts higher levels of contribution to the group account. Their experimental results are in part consistent with this Nash prediction (i.e., inequality sometimes results in a larger group account), but their results differ from predicted outcomes in two ways. Richer-than-average people contribute less than predicted and poorer-than-average people contribute more than predicted.[9]

In Chan et al. (1999), inequality treatments are introduced in non-linear games by creating variation in both the endowment and the value of the public good. In addition, inequality is introduced under several communication and information conditions. When subjects are fully informed and not allowed to communicate, adding a single type of inequality (endowment or value) does not change the amount contributed to the group account, but incorporating both types of inequality at once increases contributions to the group account.[10]

[8] See Laury and Holt (in press) for a review of non-linear public goods studies.

[9] Similar results are reported in Chan et al. (1997).

[10] Chan et al. (2003) analyze individual-level data for the same experiments.

Two additional studies in non-linear environments find no effect of inequality on public goods contributions. van Dijk and Grodzka (1992) report that inequality in endowments does not affect contributions to the group account in a step-level (threshold) public goods game. Sadrieh and Verbon (2004) vary endowments in a dynamic setting, where each round's earnings are added to the available endowment in the following round. In this design, which did not include a baseline treatment of equality, they found that contribution levels did not vary with the degree of inequality.

In summary, the small but growing literature on inequality in public goods experiments varies considerably in both design features and conclusions. There is no robust support for an effect of inequality, and existing results suggest complicated interactions between inequality and other treatment variables. Our study design differs in several ways from previous work. First, we adopt a linear framework, since varying the endowment or the value of the public good in non-linear games generally changes the Nash prediction, and makes it difficult to separate behavioral changes that are explained by theory from changes that are motivated by inequality *per se*. Second, we do not introduce inequality in the value of the public good, since even in linear settings with homogeneous preferences there is convincing experimental evidence that contributions to a public good are affected by its value relative to private consumption. Instead, we introduce inequality by varying the levels and distributions of a fixed payment made to subjects for participating in the experiment. The advantage of this experimental design is that it isolates the effect of inequality separately from the effects of other experimental features (e.g., the endowment, value of the public good).[11]

Of all the previous studies in this area, our work is most comparable to those that introduce inequality in endowments in a linear setting. To our knowledge, the only published inequality studies that vary endowments in linear settings are the threshold public goods studies by Bagnoli and McKee (1991) and Rapoport and Suleiman (1993). Both report that the public good is provided less often when endowments vary across individuals. However, Rapoport and Suleiman (1993) also report that as endowments vary in size, participants contribute some fixed proportion of their endowment to the group account. In unpublished work, Buckley and Croson (2003) obtain similar results for a linear public goods experiment without a threshold provision level. In contrast to these studies, our decision to introduce inequality through a fixed payment does not alter the set of feasible actions available to subjects. Finally, our experiment is the first to examine the importance of making inequality salient to subjects.

4. Experimental design

A total of 48 students were recruited from undergraduate classes at the College of William and Mary to participate in six sessions of the experiment.[12] At the beginning of each laboratory session, we distributed and read aloud instructions describing the payoff structure of the game (see the first page of Appendix A). Each session consisted of 30 decision-making periods divided into three blocks of 10 rounds; the blocks differed only in the "fixed payment" distribution. The fixed payments served as show-up fees and, as explained to the subjects, were completely unrelated to the decision-making phase of the game. Participants were not promised any specific payment

[11] In reality, individuals may use accumulated wealth to contribute to public goods, so by introducing inequality in the fixed payment as opposed to the endowment, our design is better interpreted as introducing heterogeneity within the subject pool, rather than income inequality.

[12] There were no controls on whether subjects in a given session knew one another. Slightly more than half of the subjects were women (56%), about 19% of the subjects were non-White, and the average age was 19 years.

Table 1
Experimental design

Session	Block 1 (10 rounds)	Block 2 (10 rounds)	Block 3 (10 rounds)	Type of inequality	Number of subjects
1	Egalitarian	Skewed	Symmetric	Private	8
2	Skewed	Symmetric	Egalitarian	Private	8
3	Symmetric	Egalitarian	Skewed	Private	8
4	Egalitarian	Skewed	Symmetric	Public	8
5	Skewed	Symmetric	Egalitarian	Public	8
6	Symmetric	Egalitarian	Skewed	Public	8
		Total subjects			48

Notes: Egalitarian fixed payments = (8 at US$ 7.50); Skewed fixed payments = (1 at US$ 20, 4 at US$ 7, 3 at US$ 4); Symmetric fixed payments = (3 at US$ 10, 2 at US$ 7.50, 3 at US$ 5).

amount at the time of recruitment. In the "egalitarian" block, or treatment, all fixed payments were US$ 7.50. In the "skewed" treatment, one person received a US$ 20 fixed payment, four people received a US$ 7 payment and three people received a US$ 4 payment. In the "symmetric" treatment, three people received a US$ 10 payment, two people received a US$ 7.50 payment and three people received a US$ 5 payment. Note that the average fixed payment was US$ 7.50 for all three payment distributions. Table 1 summarizes this experimental design.

At the beginning of each of the three blocks, we wrote the eight possible fixed payments on the board at the front of the room and showed subjects eight cards with the payment amounts written on them. We then shuffled the cards and drew one for each subject. In another experimental study in which differences among subjects were induced, Ball et al. (2001) reported that the way in which differences were denoted mattered greatly. Therefore, in half of the sessions, fixed payment draws were made in private, so each person knew the distribution and only their own draw. In other sessions, the draws were made in public, so that each person saw which fixed payment was drawn by all of the participants. The public draw of fixed payments was a subtle version of the "award ceremony" method described in Ball et al. (2001). Fixed payment cards were ranked from highest to lowest and distributed in that order by drawing names from a box.[13] Subjects who were awarded higher than average fixed payments were congratulated, while others were simply presented with the card.

Once the first block's fixed payment cards were distributed, subjects were seated at computer terminals where large foam board partitions prevented subjects from seeing one another. At that point, a second set of instructions (also in Appendix A) was displayed on the computer screens and read aloud.[14] These instructions describe the decisions that subjects were required to make during each of the 30 rounds in the experiment. Specifically, in each round, each subject was given 10 tokens to allocate between a private account and a public account. Each token allocated to the private account resulted in US$ 1 in earnings for that individual participant. All tokens allocated to the public account were doubled and split equally between the eight group members. Therefore, one token allocated to the public account earned US$ 0.25 (=US$ 1*2/8) for all eight

[13] Since fixed payments were awarded randomly, these sessions most closely match the random status treatment used by Ball et al. (2001).

[14] These instructions included two pre-game questions to gauge the subjects' understanding (see Screen 4). Subjects were informed of the correct answers, but subject responses to these questions were not recorded.

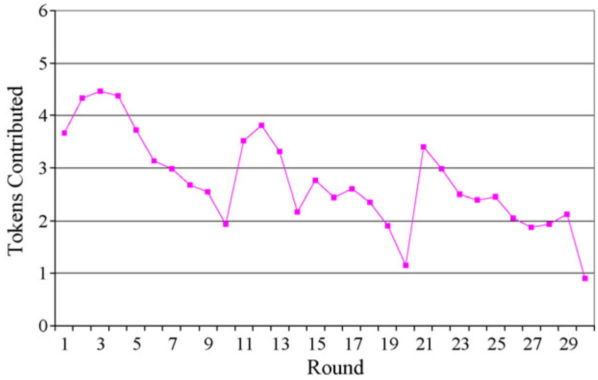

Fig. 1. Mean tokens contributed, by round.

members of the group.[15] After subjects made 10 allocation decisions under the first of the three inequality treatments, fixed payment cards were redistributed and the process was repeated under two additional inequality treatments. After the third block of decisions, subjects were asked to complete a survey of demographic traits and political attitudes. At the end of the experiment, one round was randomly chosen for payment.[16] On average, subjects earned US$ 19.57 and sessions lasted 90 minutes.

An important advantage of our experimental design is that it allows us to test the effect of inequality on group account contributions using variation in behavior within subjects. By re-assigning the fixed payments cards in each session, we can observe subject behavior in all three inequality treatments, and control for the role of fixed subject-specific traits in the propensity to contribute to the group account. This allows us to attribute any observed effect of inequality to the specific inequality treatment, as opposed to unobservable characteristics of the subjects who participated in particular treatments.

5. Experimental results

Fig. 1 reports average contributions to the group account in each round of play. In the first round, subjects contributed approximately 37% of the total available endowment to the group account. The average first-round contribution is slightly below the 40–60% range reported in summary studies.[17] In contrast, our finding that contributions generally decline with repetition is quite consistent with previous work. In a small number of rounds, average contributions rose rather than fell from the previous round, most noticeably in rounds 11 and 21. We refer to these two increases in particular as reset effects, measured here as the difference between the average amount contributed to the group account in the 10th round with a *status quo* fixed payment

15 Subjects were informed of the total contribution to the group account, but information about the individual contributions made by the seven other subjects was not revealed. Hence, contributions made by individual subjects could not be linked to fixed payments.

16 This is a common approach that has the advantage of reducing experimenter costs, and at the same time, may induce subjects to think carefully about each decision since only one matters in determining their earnings. There is no consensus as to whether or not subjects' behavior is affected by this method.

17 See, for example, Anderson (2001) and Ledyard (1995).

Table 2
Descriptive statistics for tokens contributed, by session and block

Private inequality	Mean (min, max)	Public inequality	Mean (min, max)
Session 1	2.63 (0, 10)	*Session 4*	2.39 (0, 10)
Block 1 (egalitarian)	3.61 (0, 10)	Block 1 (egalitarian)	3.98 (0, 10)
Block 2 (skewed)	2.01 (0, 10)	Block 2 (skewed)	2.00 (0, 10)
Block 3 (symmetric)	2.28 (0, 10)	Block 3 (symmetric)	1.20 (0, 10)
Session 2	2.01 (0, 8)	*Session 5*	3.32 (0, 10)
Block 1 (skewed)	2.88 (0, 8)	Block 1 (skewed)	3.11 (0, 10)
Block 2 (symmetric)	1.86 (0, 6)	Block 2 (symmetric)	3.71 (0, 10)
Block 3 (egalitarian)	1.29 (0, 6)	Block 3 (egalitarian)	3.14 (0, 10)
Session 3	4.00 (0, 10)	*Session 6*	2.13 (0, 10)
Block 1 (symmetric)	4.98 (0, 10)	Block 1 (symmetric)	1.74 (0, 7)
Block 2 (egalitarian)	3.64 (0, 9)	Block 2 (egalitarian)	2.39 (0, 10)
Block 3 (skewed)	3.40 (0, 10)	Block 3 (skewed)	2.28 (0, 10)

structure and the average amount contributed to the group account in the first round with a new fixed payment structure. The average reset effect across all treatments and sessions is 1.92 tokens. Although there is considerable variation in the size of reset effects across treatments, we do not find a statistically significant difference in the average reset effect as treatments varied from more to less equal, or from less to more equal.[18]

In Table 2, we report average contributions separately for each session, and within each session, we show the average contribution for the three separate inequality treatments. The initial treatment varied across the sessions: in sessions 1 and 4 for example, the first treatment was egalitarian, whereas in sessions 2 and 5, the first treatment was skewed. Patterns in the first-treatment (or first-block) averages give an indication of how contributions to the group account varied with inequality. In the first three sessions, in which fixed payments were awarded privately, the first-block egalitarian contributions averaged 3.61 tokens and the first-block skewed and symmetric treatment contributions averaged 2.88 and 4.98 tokens, respectively. Taking the average of the latter two estimates, subjects who initially experienced the inequality treatments contributed more tokens (3.93) to the group account compared to subjects who initially experienced the egalitarian treatment. The opposite pattern is suggested by the data from the sessions in which inequality was awarded in a public manner. There, contributions by subjects whose first-block treatment was egalitarian were larger (3.98 tokens) on average compared to first-block subject contributions under the skewed treatment (3.11 tokens) or the symmetric treatment (1.74 tokens).

Table 3 reports the variation in group account contributions by treatment, this time pooling data from all three blocks. Results from the combined private and public inequality sessions (the far right column in Table 3) show that contributions were highest in the egalitarian treatment (3.01 versus 2.63 for symmetric and 2.61 for skewed). Like the previous table of results, the higher level of contributions under the egalitarian treatment is present only in the public inequality sessions and does not hold for the private inequality sessions. We tested for statistically significant differences

[18] The largest reset effect was observed in session 6; the average contribution to the group account increased by 4.88 (almost half of each individual's endowment) when the fixed payment structure changed from symmetric to egalitarian. The smallest reset effect was zero, and occurred in session 4 when the fixed payment structure switched from egalitarian to skewed.

Table 3
Contribution means and standard deviations, by form of inequality and distribution

Distribution of payments	Private inequality	Public inequality	Both private and public inequalities
Egalitarian	2.85 (2.45), $n = 240$	3.17 (3.26), $n = 240$	3.01 (2.89), $n = 480$
Symmetric	3.04 (2.69), $n = 240$	2.22 (2.66), $n = 240$	2.63 (2.70), $n = 480$
Skewed	2.76 (2.43), $n = 240$	2.46 (3.24), $n = 240$	2.61 (2.86), $n = 480$
All	2.88 (2.52), $n = 720$	2.62 (3.09), $n = 720$	2.75 (2.82), $n = 1440$

in subject contributions by inequality treatment using a series of Wilcoxon signed rank tests for matched pairs. Contributions in the egalitarian treatment were significantly different (at the 95% level) from contributions in the two unequal (symmetric and skewed) treatments combined, but only in the public inequality sessions. In no case (public or private inequality) could we reject the null hypothesis that contributions in the egalitarian treatment were the same as contributions in either of the unequal treatments considered separately.

Differences in mean contributions by treatment can reflect variation in factors other than inequality *per se*, such as the level of the fixed payment a subject received, the effect of repetition and the order of the treatments, and possibly subject traits. To identify the effect of inequality on group account contributions holding all else equal, we next conduct multivariate analyses of our data. Before examining the effects of inequality on contributions, we first estimate several variants of a basic model of contributions to demonstrate the effect of some of these other factors on subject decisions. In this model, the dependent variable is the number of tokens contributed to the group account by a subject in a given round of play, and the explanatory variables include the fixed payment received by the subject and controls for the round of play. To allow for unobserved subject-specific differences in group contributions, we estimate the model as a generalized least squares model (GLS) with random-subject-effects.[19] In this specification, the error term is assumed to be distributed normally with mean zero, and composed of two parts, a random disturbance term for the individual subject and a disturbance term for the decision made by the subject in the given round. Table 4 reports the base model results first using the full data set and then using separate samples from the private inequality sessions and public inequality sessions. In most models, the coefficient of the fixed payment variable is positive but not statistically significant. This suggests that the absolute amount of the fixed payment does not explain the differences in contributions to the group account reported in Table 3, and that our fixed payments were sufficiently low as to avoid wealth effects.

When we estimate the base model using data from all sessions, we use a dummy variable to capture the effect of the public manner of revealing the inequality in the subject fixed payments. We control for repeated play with round dummy variables as well as additional dummy variables to identify decisions made in the second and third blocks. To capture the two reset effects depicted in Fig. 1, we also include two dummy variables, one is equal to 1 for the first round of the second block and 0 otherwise, and the second is equal to 1 for the first round of the third block and 0 otherwise. In general, the estimated effects of the round, block, and reset variables have the expected signs.

[19] Since the dependent variable ranges from 0 to 10, some previous empirical analyses of public goods contributions employ Tobit models. However, when we used our data to estimate Tobit models with random effects, specification tests suggested that the estimation results were sensitive to the number of quadrature points used in the random-effects estimation process. For this reason, we employ the GLS model with random-effects, since it is not estimated via quadrature.

Table 4
Random effects GLS models of contributions

Explanatory variable	All sessions[a]	Private inequality[b]	Public inequality[b]
Public	−0.267 (0.58)		
Fixed payment	0.024 (1.01)	−0.017 (0.62)	0.068* (1.74)
Round 2	0.729* (1.90)	1.019** (2.26)	0.440 (0.71)
Round 3	0.444 (1.16)	0.949** (2.11)	−0.060 (0.10)
Round 4	0.000 (0.00)	0.352 (0.78)	−0.352 (0.57)
Round 5	0.007 (0.02)	0.727 (1.62)	−0.713 (1.16)
Round 6	−0.438 (1.14)	0.199 (0.44)	−1.074* (1.74)
Round 7	−0.493 (1.28)	0.352 (0.78)	−1.338** (2.17)
Round 8	−0.660* (1.72)	0.046 (0.10)	−1.366** (2.22)
Round 9	−0.792** (2.06)	−0.301 (0.67)	−1.282** (2.08)
Round 10	−1.653*** (4.31)	−0.968** (2.15)	−2.338*** (3.79)
Second block	−0.850*** (5.57)	−1.407*** (7.87)	−0.292 (1.19)
Third block	−1.213*** (7.95)	−1.648*** (9.22)	−0.778*** (3.18)
Reset 1	0.704 (1.46)	0.907 (1.61)	0.500 (0.65)
Reset 2	0.942* (1.95)	1.481*** (2.62)	0.403 (0.52)
Hausman Test *p*-value	1.000	1.000	1.000

Note: Absolute values of *t*-statistics are shown in parentheses. Statistical significance is indicated by *** for the 0.01 level, ** for the 0.05 level and * for the 0.10 level.

[a] $n = 1440$.

[b] $n = 720$.

The full sample results show that repeated play had an initial positive and significant effect in round 2, no effect on contributions in the middle rounds, and negative and significant effects in rounds 8 though 10. In both the public and private session samples, contributions made in the second and third blocks were lower relative to the first round of play. Some differences exist in the coefficients of the control variables across the public and private session samples. For example, the estimated reset effects are positive and significant only in the private inequality sessions.

To summarize, we find that repeated play generally had the predicted negative effect observed in previous studies, that there are some differences in subject behavior according to the manner in which inequality was revealed, and that the level of the fixed payment had little independent effect on contributions to the group account. In addition, the results of Hausman tests reported in the bottom row of Table 4 support the use of a random-effects specification for our data.

We next test the effect of inequality on contributions to the group account by introducing additional explanatory variables to the base model, as shown in Table 5. We begin by adding only explanatory variables to account for the inequality in different model specifications; these are reported in columns (1) and (4). Each model includes the explanatory variables measuring inequality that are shown in the relevant rows, as well as controls for fixed payment and round of play. In each of the models shown, the results of Hausman tests support the use of random-effects estimation.

Model 1 includes an indicator variable for an unequal distribution of fixed payments; the omitted category represents the egalitarian distribution in which all subjects received a fixed payment of US$ 7.50. The results suggest that the inequality treatment had no effect in the private sessions but depressed contributions in the public treatment by about 32% of the average contribution in the public sessions, or 0.827/2.62. The negative coefficient on the inequality variable is consistent

Table 5
Random and fixed effects GLS models of contributions

	Private inequality			Public inequality		
	(1)	(2)	(3)	(4)	(5)	(6)
Model 1						
Unequal distribution	0.054 (0.37)	0.054 (0.37)	0.054 (0.37)	−0.827*** (4.16)	−0.827*** (4.16)	−0.827*** (4.16)
Model 2						
Relative deprivation index	0.044 (0.09)	0.060 (0.13)	0.054 (0.11)	−2.172*** (3.35)	−2.156*** (3.32)	−2.134*** (3.27)
Model 3						
Relative deprivation index	−0.277 (0.35)	−0.234 (0.29)	−0.253 (0.31)	0.044 (0.04)	0.110 (0.10)	0.214 (0.19)
Unequal distribution	0.123 (0.50)	0.113 (0.45)	0.118 (0.47)	−0.838** (2.45)	−0.855** (2.49)	−0.881** (2.55)
Model 4						
Maximum payment	0.419 (1.22)	0.410 (1.19)	0.483 (1.40)	−0.323 (0.70)	−0.303 (0.66)	−0.331 (0.71)
Unequal distribution	0.368 (1.24)	0.361 (1.22)	0.417 (1.40)	−1.069*** (2.68)	−1.055*** (2.64)	−1.075*** (2.67)
Model 5						
Payment relative to max	0.587 (1.36)	0.579 (1.34)	0.596 (1.38)	−0.660 (1.14)	−0.661 (1.14)	−0.687 (1.18)
Unequal distribution	0.311 (1.30)	0.307 (1.28)	0.315 (1.32)	−1.116*** (3.46)	−1.116*** (3.45)	−1.128*** (3.49)
Control for race and gender, political ideology and major	No	Yes	No	No	Yes	No
Control for subject random effects	Yes	Yes	No	Yes	Yes	No
Control for subject fixed effects	No	No	Yes	No	No	Yes

Notes: Absolute values of *t*-statistics are shown in parentheses. All models control for the subject's fixed payment, the round of play, the order of the treatment and reset effects. The number of observations used in each model's estimation is 720. The subject traits added to some models are indicator variables measuring subject sex (female), race (non-White), academic major (economics or not) and political ideology (Democratic and neither party relative to Republican). Statistical significance is indicated by *** for the 0.01 level, ** for the 0.05 level and * for the 0.10 level.

with the model of Alesina et al. (1999), in which heterogeneity dampens the ability to provide public goods.[20]

In Model 2, we examine whether inequality affected all subjects in an identical manner, or whether the subject's relative payment within the distribution mattered. To do this, we add to the base model a subject-specific measure called the relative deprivation index, or RDI, following a definition provided in Deaton (2001).[21] The index is calculated as:

$$\mathrm{RDI}_i = 1 - F(x_i)\frac{\mu^{+}(x_i) - x_i}{\mu_{\mathrm{r}}} \tag{1}$$

where x_i is the fixed payment for individual i, $(1 - F(x_i))$ is the proportion of the group with payments greater than x_i, $\mu^{+}(x_i)$ is the mean of all payments to subjects with payments greater than x_i and μ_{r} is the mean of all payments in the reference group. In the public sessions, the relative deprivation measure has a negative and significant influence on contributions. In Model 3, we control for both the relative deprivation index and the nature of the payment distribution. All else equal, when fixed payments are drawn from an unequal distribution, subjects in the public sessions reduced contributions to the group account, again by about 32%. After controlling for the presence of inequality, the subject's placement within the distribution (indicated by the RDI) does not have a significant effect.

Models 4 and 5 employ two alternate subject-specific measures of relative income; both models also control for inequality in the fixed payment distribution. One measure is a dummy equal to 1 if the subject received the maximum payment in the distribution; another measure is calculated by dividing the subject's payment by the maximum payment in the fixed payment distribution. Models 4 and 5 produce results that are qualitatively similar to Model 3, but generate larger point estimates for the inequality effect, here about 41–42% of the average contribution.

One persistent result across all specifications is that inequality substantially reduced contributions in the public sessions. We investigated the robustness of this finding by introducing additional controls to the model. We constructed measures of subjects' race, gender, academic major and political ideology from a survey administered at the conclusion of the experiment. Columns (2) and (5) include controls for these variables, and in each case, our main result—that inequality dampens contributions to the group account in the public sessions—persists with the additional controls. In columns (3) and (6), we present the results of fixed-subject-effects models that adjust for unobservable, fixed, subject traits. These results also show that inequality was associated with reduced contributions to the group account in the public treatment. Notably, the inclusion of these additional control variables does not appreciably alter the magnitude of the inequality effect in the public sessions.

We conducted additional robustness checks that are not reported in Table 5. To all of the models, we added a variable capturing the level of contributions made by other members of the subject's group in the previous round. Focusing on the public session results, we found that the significant effects of the inequality measures persisted in 12 of the 15 models reported in Table 5. In another

[20] We also estimated an alternate specification of Model 1 using two inequality dummy variables for the skewed and symmetric treatments; we were unable to reject the null hypothesis that the coefficients on the skewed and symmetric treatment dummies were identical.

[21] Values of the RDI can range from 0 to 1, with higher values assigned to subjects who are more deprived relative to their group members. In our experiment, the RDI ranged from a low of 0 (assigned to subjects who receive the largest fixed payment in the group and all subjects in the egalitarian distribution) to a high of 0.53 (assigned to subjects who received a U$ 4 fixed payment in the skewed distribution).

exercise, we estimated our models using only those observations from the first 10 rounds each subject played. These models generated a *between-subjects* analysis (in contrast to the *within-subjects* analysis reported in Table 5), and addressed the concern that subject experiences under one type of inequality treatment may have persistent effects on subject behavior under a new treatment. Our main result, that inequality dampens contributions to the group account by all members in the public sessions, was also observed in the 10-round sample.

6. Conclusions

Several researchers have asserted that inequality hinders collective efficacy. In this paper, we conduct a novel test of the effect of inequality on public goods provision in an experimental setting. Because we introduce inequality in a way that does not alter the set of choices available to subjects, our design differs from most existing experimental studies of inequality.

Our results suggest that inequality in the distribution of fixed payments made to subjects dampens public goods contributions. For an individual subject, being in a group with an unequal distribution of fixed payments significantly reduces contributions to the public good, all else equal. However, this finding is observed only when the fixed payment is distributed publicly, that is, fixed payment draws are known to all subjects. Further, once we control for group-level inequality, the individual's relative standing in the group does not affect contributions.

While not predicted by standard economic theory, the importance of public inequality is consistent with research on status in psychology and sociology (Berger et al., 1983). For example, if awarding a high fixed payment confers status on some people in the group, this may depress contributions made by low-status individuals as a means of protest. If high-status people reciprocate with low contributions, the result is a lower overall level of public goods provision. This result is also consistent with the work of Ball et al. (2001) which awards status via gold stars, rather than high fixed payments. Ball et al. (2001) find that subjects with stars earn a higher percent of surplus in a market experiment than those without stars, regardless of whether the status is believed to be random or earned. Further, when status is awarded privately to some people (with others unaware that it existed), it has no effect on the distribution of the surplus. However, our results have additional implications not shared by the Ball et al. analysis. Using a box market design with multiple equilibria, their experiment finds that status affected the selection of outcomes from a range of alternatives that differed in terms of equity (i.e., distribution of the total surplus) but were identical in terms of efficiency. In contrast, we find that publicly known inequality reduced subjects' propensity to contribute to a public good in a setting in which it is socially optimal to do so. Thus, our results suggest that inequality may have important efficiency implications as well.

A second difference regarding the effect of status in our setting compared to the study of Ball et al. pertains to the way particular individuals are affected by status. Prevailing theories suggest that higher status individuals believe themselves to be more deserving and demanding of rewards. Consistent with this, Ball et al. find higher status subjects receive a larger share of the surplus in market transactions. In contrast, we find that status differentials in the form of inequality affect all individuals in the group; that is, we found no independent effect of the individual's ranking on the propensity to make contributions to the group account upon controlling for the nature of the distribution. One concern is that our pooled analysis may mask an individual-specific effect in the early rounds of the game, and that repeated play creates a contagion effect, spreading this behavior to all members of the group. Such an effect could be generated by the initial reactions of individuals with large fixed payments who neglect to contribute due to perceptions of higher status, or by the behavior of individuals with low fixed payments who neglect to contribute due to spite (e.g., Saijo and Nakamura, 1995) or envy (e.g., Zizzo and Oswald, 2001). However, a robustness check demonstrated that our results are present even when we estimate our models using only data from the first block of 10 rounds. Consequently, we are confident that we are identifying a common effect of group inequality on the contributions of all individuals, regardless of their relative standing.

Finally, the importance of public signals for the manifestation of a treatment effect of inequality on contributions in the public goods game is also consistent with survey-based research on the effects of group heterogeneity on social capital. Group differences in income, ethnicity, race, birthplace and age do not exert a consistent effect on the efficacy of collective action, although in many instances, such group characteristics are quite important. In light of these findings and our experimental results, a reasonable conjecture is that not all forms of heterogeneity are salient for the group. Consequently, future work on the determinants of social capital should investigate the factors that render some group characteristics salient. In conclusion, the results of this study provide novel support for recent claims that inequality has important "psychosocial" effects that reduce the tendency for cooperation in collective action problems.

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Appendix A. Instructions Appendix

A.1. Part A: General Instructions

This experiment is a study of individual behavior. The instructions are simple. If you follow them carefully, you may earn a considerable amount of money, which will be paid to you privately in cash at the end of the experiment today.

A.1.1. Blocks and Rounds

In this experiment you will make a decision in each of 30 rounds. The specific details about these decisions will be displayed on your computer screens and we will read these details aloud before the decision-making rounds begin. The rounds will be divided into three blocks (A, B and C) with 10 decision-making rounds in each block. Notice that the block and round indicators are shown on the left side of your decision sheet.

A.1.2. Fixed Payment Cards

At the beginning of each block, we will shuffle and randomly distribute cards that assign your "fixed payment" for that block. We have eight fixed payment cards for each block and the numbers on those cards will be announced out loud and written on the board at the front of the room at the beginning of each block. Hence, everyone in the room will know what the eight fixed payments are, but only you will know which of the eight numbered cards was randomly distributed to you. (Alternative sentence for public information condition: Hence, everyone in the room will know what the eight fixed payments are and who is randomly assigned each payment.)

The number on your card represents your fixed payment for that block. For example, if you draw the 5, your fixed payment is US$ 5. Notice that there is only space for you to record one fixed payment amount for each block because you are only given one fixed payment for each block. Your fixed payment does not depend on decisions that you or other people make in this experiment.

A.1.3. Your Earnings in the Experiment

The computer will keep a cumulative total of the money you earn for every decision you make. Please disregard this amount, as it will not be relevant for your earnings. You should transfer other requested information from the computer screen to your record sheet. It will be important in determining your earnings at the end of the experiment today. At the end of the experiment, we will throw a six-sided die to determine which block of rounds will be used to determine your earnings. If we throw a 1 or 2, block A will be used; if we throw a 3 or 4, block B will be used; and if we throw a 5 or 6, block C will be used. You will receive the fixed payment associated with the block that we choose. In addition, we will throw a 10-sided die to pick the specific round within the chosen block that will determine your earnings in the decision-making phase of the experiment. If the die throw is 1, we will use round 1, and so on. The die throws guarantee that all rounds are equally likely to be chosen for payment, so you should think carefully about each decision.

A.2. Part B: Game-Specific Instructions

A.2.1. Screen 1

Matchings: The experiment consists of a series of rounds. In each round, you will be matched with the same group of seven other people. The decisions that you and the other people in your group make will determine the amounts earned by each of you.

Investments: You begin each round with a number of "tokens," which may either be kept or invested. At the same time, the seven people you are matched with will decide how many of their tokens to keep and how many to invest. Neither of you will be able to see the other's decision until after your decision is submitted.

Earnings: The payoff to you will equal:

US$ 1.00 for each token you keep,
US$ 0.25 for each token you invest, and
US$ 0.25 for each token invested by the seven other people who you are matched with.

Subsequent Matchings: The groups of eight people will be the same in all subsequent rounds, so the seven other people you are matched with in one round are the same people that you are matched with in the next round.

A.2.2. Screen 2

Example: Suppose you have only two tokens for the round, and the earnings from tokens kept, invested and invested by the others are US$ 1.00, 0.25 and 0.25 respectively.

(1) If you keep both tokens, then your earnings will be: US$ 1.00 × 2 = US$ 2.00 from the tokens kept, plus US$ 0.25 times the number of tokens invested by the other people in your group.

(2) If you invest both tokens, then your earnings will be: US$ 0.25 × 2 = US$ 0.50 from the tokens kept, plus US$ 0.25 times the number of tokens invested by the other people in your group.
(3) If you keep 1 token and invest 1 token, then your earnings will be:
    US$ 1.00 × 1 = US$ 1.00 from the token kept, plus
    US$ 0.25 × 1 = US$ 0.25 for the token invested, plus
    US$ 0.25 times the number of tokens invested by the other people in your group.

Note: In each of the above three cases, what you earn from the others' investments is: US$ 0.00 if the others invest 0 tokens, US$ 0.25 if the other people invest 1 token (in total) and keep the rest, US$ 0.50 if the other people invest 2 tokens (in total), etc.

A.2.3. Screen 3

There will be 10 rounds, and in all rounds you will begin with a new endowment of 10 tokens, each of which can either be kept or invested. The seven other people in your group will also have 10 tokens.

Everybody earns money in the same manner: US$ 1.00 for each token kept, US$ 0.25 for each token invested and US$ 0.25 for each token invested by the seven other people.

At the start of a new round, you will be given a new endowment of 10 tokens. You are free to change the numbers of tokens kept and invested from round to round.

Note: You will be matched with the same people in all rounds.

A.2.4. Screen 4

In the following examples, please use the mouse button to select the best answer.

Question 1: Suppose you invest X of your 10 tokens and the total number invested by the seven other people is Y tokens.

(a) Then you earn $(10 - X) \times$ US$ 1.00 + $X \times$ US$ 0.25.
(b) Then your earnings will be at least as high as (10 − X) × US$ 1.00 + $X \times$ US$ 0.25.

Question 2: Which is true?

(a) You may divide your 10 tokens any way you wish in each round, keeping some and investing some, or you may keep or invest them all.
(b) The more you invest in one period the less there is to invest in later periods.

A.2.5. Screen 5

There will be a total of 10 rounds in this part of the experiment.

All people will begin with 10 tokens which they may keep (and earn US$ 1.00 each) or invest (and earn US$ 0.25 each), knowing that they will also earn US$ 0.25 for each token invested by other people in the group. You will begin each round with a new endowment of 10 tokens, irrespective of how many tokens you may have kept or invested in previous rounds.

There will be a total of 10 rounds in this part of the experiment. Your earnings for each round will be calculated for you and added to previous earnings, as will be shown in the total earnings column of the record form that you will see next.

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