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 Putnam [2000], attribute this relationship to the detrimental "psychosocial effects" of group heterogeneity on cooperation. We test the hypothesis that inequality within a group reduces individual contributions in a canonical public goods experiment. Unlike previous examinations of inequality and public good provision, our design introduces inequality by manipulating the levels and distributions of fixed payments given to subjects for participating in the experiment. When made salient through public information about each individual's standing within the group, inequality in the distribution of fixed payments reduces contributions to the public good for all group members.

JEL Codes: C9, H4

Keywords: Inequality, Heterogeneity, Cooperation, Public goods, Experiments

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I. 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 [Putnam 2000]. We conduct a novel test of 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, Walker, and Thomas [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

[1] For a recent review of the social science literature on the consequences of inequality, see Thorbecke and Charumilind [2002]; recent examples of subject-specific studies on the effects of inequality include: Alesina and Rodrik [1994], Persson and Tabellini [1994] and Forbes [2000] on economic growth and investment; Mellor and Milyo [2001 and 2002] on health; Birdsall [1999] and Mayer [2001] on education; and Fajnzyber, Lederman and Loayza [2002] on crime.

fixed payments is revealed to all players prior to the start of play; this feature makes our work similar to Ball et al. [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 [Becker 1974] or asymmetric inequality aversion [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 [Alesina and La Ferrara 2000, Cardenas 2003 and Costa and Kahn 2003a,b].

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

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

II. 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 to public expenditures, especially those with a strong redistributive component [Meltzer and Richard 1981 and 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 [e.g., Kaplan et al. 1996, Wilkinson 1996, Knack and Keefer 1997, Kawachi et al. 1997 and especially, 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

[3] See Mulligan [2003] for a recent critique of the Meltzer-Richard hypothesis.

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, Baqir and Easterly [1999] does 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, Smeeding and Schwabish [2003] notes 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, Baqir and Easterly [1999] finds 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 and 2002], these findings provide additional, albeit

4 Some of the empirical studies linking social capital to measures of well-being include: Alesina and La Ferrara [2000 and 2002] on civic participation and trust; Knack and Keefer [1997] and Zak and Knack [2001] on economic growth; Knack [2002] on the quality of government; Kawachi, Kennedy and Glass [1999] and Mellor and Milyo [2004] on health; and Galea, Karpati and Kennedy [2002] on crime.

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 Ferarra [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] reports 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 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 lab 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

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[5] However, see Vigdor [2003] for a critique of the use of fragmentation indices in this literature.

[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.

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).

III. 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], and later adapted by Isaac, Walker and Thomas [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 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 $P < (M/N)*G$, 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 forty to sixty percent 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 1999], repeated interactions with the same subjects versus random pairings after each repetition [Andreoni and Croson 2001] and financial punishments for free riding [Anderson and Stafford 2003]. Ledyard [1995] and Anderson [2001] discuss these major findings from public goods experiments.

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

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

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 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]

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

[8] See Laury and Holt [1998] 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.

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. In contrast, our experimental design minimizes the potential for interactive effects in order to isolate the effect of inequality from the effects of other experimental features. 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.

This experimental design makes our work most comparable to studies 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.

IV. Experimental Design

A total of 48 students were recruited from undergraduate classes at the College of William and Mary to participate in 6 sessions of the experiment. 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 the Appendix). Each session consisted of 30 decision-making periods divided into three blocks of ten 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. In the "egalitarian" block, or treatment, all fixed payments were $7.50. In the "skewed" treatment, one person received a $20 fixed payment, four people received a $7 payment and three people received a $4 payment. In the "symmetric" treatment, three people received a $10 payment, two people received a $7.50 payment and three people received a $5 payment. Note that the average fixed payment was $7.50 for all three fee schedules. 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 the eight cards with the fixed payments written on them. Then we shuffled the fixed payment cards and drew one for each subject. In half of the sessions, the fixed payment draws were made in private so each person knew the distribution and only their own draw. In other sessions, the fixed payments were made in public, so 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.[11] 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 the Appendix) was displayed on computer screens and read aloud. 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 $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 $0.25 (= $1*2/8) for all eight members of the group. 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. On average, subjects earned $19.57 and sessions lasted 90-minutes.

An important advantage of our experimental design is that it allows us to test the effect of

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

inequality on group account contributions using two forms of variation in subject behavior. By re-assigning the fixed payments cards over the course of each session, we can observe variation in behavior within subjects as each subject experienced all three inequality treatments. Moreover, by varying the inequality treatment for each individual subject, we can 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 treatment, as opposed to unobservable characteristics of the subjects who participated in the particular treatments.

It is possible that subject experiences under one type of inequality treatment may have persistent effects on subject behavior under a new treatment. For this reason, we also conduct separate analysis using each subject's experiences in only the first block of ten rounds. In the discussion below, we refer to the examination of behavior using the first block of 10 rounds for each subject as the *between-subjects* analysis. When we use decisions made by subjects in all three blocks (all 30 rounds), we refer to this as the *within-subject* analysis.

V. Experimental Results

Figure 1 reports the total amount contributed to the group account in each round as a percent of the initial endowment, combining results for all sessions. In the first round, subjects contributed approximately thirty seven percent of the total available endowment to the group account. The average first-round contribution is slightly below the forty to sixty percent range reported in summary studies.[12] 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

[12] See, for example, Anderson [2001] and Ledyard [1995].

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 tenth round with a *status quo* fixed payment 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.[13]

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 inequality treatment varied across sessions: in sessions one and four for example, the first treatment was egalitarian, whereas in sessions two and five 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, applying the between-subjects comparison we noted above. 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

[13] 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.

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 using data from all three blocks. Differences in these mean contribution rates provide a descriptive version of the within-subject analysis. 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 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, such as the fixed payment, the effect of repetition and the order of the treatments, and possibly subject traits. In order to identify the effect of inequality on group account contributions holding all else equal, we next conduct multivariate analyses of our data. Before turning to the question of 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 our basic model, the dependent variable is the number of tokens contributed to the group account by a subject in a given round of play. The explanatory variables include the fixed payment, or the dollar amount 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.[14] In this model, 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 i, and a disturbance term for the decision made by the subject in the given round of play.

Table 4 reports the results from the estimation of the basic model, first using data from all six sessions. We then report results of the model estimated separately for the private and public inequality sessions. In each case, we use separate samples based on the first block of 10 decisions and then all 30 decisions in order to illustrate the basic form of both the between- and within-subject models. In all models, we include a variable representing the fixed payment received by the subject. In most cases this coefficient is positive but not statistically significant. This suggests that our show-up fees were sufficiently low to avoid wealth effects.

When we estimate the 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. When we use the 10-round sample, we control for the effects of repeated play using dummy

[14] 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.

variables for rounds 2 through 10. In the sample based on all six sessions, the estimated effects for rounds 2 through 4 are positive and significant, and the coefficients on dummy variables for rounds 7 through 10 are negative and significant. When the 10-round model is estimated using separate samples by public and private inequality, we observe a few differences in the estimated effects of the round dummies. For example, the negative effect of repeated play is observed at earlier rounds in the sessions with public inequality. Specifically in the public sessions, the coefficients for round dummies 6 through 10 are negative and significant at the 10% level or better; in contrast, only the dummy for round 10 has a negative and significant effect in the private inequality sessions. These results suggest that the public manner of revealing inequality indirectly affected contributions by changing the effects of other variables. It did not have a direct effect on contributions, since in both the 10- and 30-round analysis, the public coefficient is negative but statistically insignificant.

When estimating the model using data from all 30 rounds, 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 Figure 1, we also include dummy variables to identify decisions made in the first round of blocks 2 and 3. In general, the estimated effects of the round, block, and reset variables have the expected signs. 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. As in the 10-round analysis, there are some differences 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 the Table 4 results, 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.

Interestingly, the results shown in Table 4 are largely similar when all observations are used in the estimation versus only those from the first ten rounds. Nonetheless, we conduct separate analyses of the effect of inequality using the existing variation in treatments and behavior both between subjects and within subjects. In Table 5 we report the results of several different models estimated using only the observations from the first block of ten rounds. Each model is based on a sample of 10 rounds for each subject; twenty-four subjects participated in sessions in which inequality was revealed in a private manner and another 24 participated in sessions in which inequality was revealed publicly. Each subject experienced only one of three inequality treatments. In Table 6, we report results from a series of models estimated using all 30 rounds of data for the same number of subjects in the public and private sessions. The 30 rounds of data are comprised of 10 decisions made in each of three separate inequality treatments. In each of the models shown in Tables 5 and 6, the results of Hausman tests support the use of random-effects estimation.

The models reported in Table 5 introduce various measures of inequality in separate specifications. 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. Model 1 includes an indicator variable for an unequal distribution of fixed payments to subjects; the omitted category represents the egalitarian distribution in which all subjects received a fixed payment of $7.50. The results suggest that the inequality treatment had no effect in the private sessions, but depressed contributions in the public treatment. This evidence is consistent with the model of Alesina, Baqir and Easterly [1999], in which heterogeneity dampens the ability to provide public goods. 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. To simplify the presentation, we report results using the combined dummy variable.[15]

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 given in Deaton [2001].[16] In contrast to the results for inequality, relative deprivation

[15] We did the same hypothesis tests for Models 3 through 5, and Models 1, 3, 4, and 5 in Table 6 and could not reject the null hypothesis that the coefficients on the skewed and symmetric treatment dummies were identical in any case using the public session data.

[16] The index is calculated as:

$$RDI_i = 1 - F(x_i) \quad \frac{\mu^+(x_i) - x_i}{\mu_r}$$

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. 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 $4 fixed payment in the skewed distribution).

has a negative, but insignificant influence on contributions. In Model 3 we control for both the relative deprivation index and the nature of the payment distribution. Once again, when fixed payments are drawn from an unequal distribution, subjects in the public sessions reduced contributions to the group account by about three tokens. However, even after controlling for the nature of the distribution, the subject's placement within the distribution (indicated by the RDI) does not have a significant effect.

Finally, Models 4 and 5 employ two alternate subject-specific measures of relative income; both models also control for the nature of the fixed payment distribution. First we include a dummy equal to one if the subject received the maximum payment in the distribution. Another measure of relative income is calculated by dividing the subject's payment by the maximum payment in the fixed payment distribution. The results for Model 4 are similar to those seen in Model 3, except that the inequality effect is attenuated. Model 5 exhibits a similar qualitative pattern in the coefficients as Models 3 and 4, but generates larger point estimates. The fixed payment relative to the maximum is large and significant in the private sessions and negative (albeit insignificant) in the public sessions. In addition, inequality is associated with significantly greater contributions in the private session, but lower contributions in the public session.

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 several subject traits using a survey administered at the conclusion of the experiment. Columns (2) and (5) include controls for the subjects' race and gender, while columns (3) and (6) also add controls for the subjects' academic major and political ideology. In each case, our main result – that inequality dampens contributions to the group account in the public sessions – withstands these additional controls. As an additional check, not shown here, we added a control for subjects' attitudes for government spending; this measure was based on responses to a survey question about whether the government should spend more or less on several major federal programs. Our main result was not altered when this variable was introduced. Finally, we added a variable capturing the level of contributions made by other members of the subject's group in the previous round, and the negative and significant effect of inequality in the public sessions was unchanged.

The addition of subject traits like race and gender allows us to check our main finding against possible bias caused by observable subject-specific traits omitted from the initial models shown in columns (1) and (4). These results demonstrate that the inequality effect shown in Table 5 is not driven by the mix of subjects by race/gender/academic major/ideology within our inequality treatments. As a further test of the robustness of our main finding, we now turn to the within-subject variation in our experiment, and estimate a similar series of models using data from all 30 rounds of subject decisions.

Table 6 reports largely the same set of specifications described above, using various measures of group inequality and the subject-specific relative payment. We estimate the models first without subject traits in columns (1) and (4), then including race, gender, major, and ideology in columns (2) and (5). Finally, we present the results of fixed-subject-effects models in columns (3) and (6). The latter strategy estimates the effect of inequality holding constant all fixed subject-specific traits, even those we are unable to adequately measure in our survey. Once again, the only robust finding across these models is that inequality is associated with reduced contributions in the public treatment. The size of this effect ranges from about -0.8 to -1.1; these magnitudes are much smaller than the estimates from the first ten rounds. This suggests that either unmeasured subject-specific traits contribute to the Table 5 results, or that the ordering of the inequality treatment across blocks dampens its overall effect. The estimates in Model 2 suggest that relative deprivation has a large and significant negative influence on contributions in the public treatment; however this effect is not observed after including the indicator for inequality in the distribution of fixed payments, nor is it seen for the alternative measures of relative standing. As in Table 5, we added a control for the level of contributions made by other members of the subject's group in the previous round. We get substantively similar results in four of the five models; only in Model 3 does the coefficient for the unequal distribution dummy lose its significance when we add this control.

VI. 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 show-up fees paid 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 the nature of the distribution across the group, the individual's relative standing in the group does not affect contributions. Our findings are supported by two types of econometric analysis, a between- subjects analysis in which we examine the variation in contributions to the group account as different groups of subjects experienced different inequality treatments, and a within-subjects model that varied the inequality treatments experienced by each subject and allowed for the estimation of subject fixed-effect models.

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. In some cases, subjects are told that they earned the stars by scoring high in a trivia quiz. In other cases, subjects observe as names are randomly drawn to award stars. In reality, all of the stars are awarded randomly. Ball et al. [2001] finds 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 Ball et al.

study 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 to 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, we demonstrate that our results are present even when we estimate our models using only data from the first block of ten rounds (indeed, the inequality effects are more pronounced). 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.

Instructions Appendix

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.

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 3 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.

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 $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.

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 6-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.

Part B: Game Specific Instructions

Screen 1

Matchings: The experiment consists of a series of rounds. In each round, you will be matched with the same group of 7 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 7 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:

$1.00 for each token you keep,

$0.25 for each token you invest, and

$0.25 for each token invested by the 7 other people who you are matched with.

Subsequent Matchings: The groups of 8 people will be the same in all subsequent rounds, so the 7 other people you are matched with in one round are the same people that you are matched with in the next round.

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 $1.00, $0.25, and $0.25 respectively.

(1) If you keep both tokens, then your earnings will be: $1.00 x 2 = $2.00 from the tokens kept, plus $.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: $0.25 x 2 = $0.50 from the tokens kept, plus $.25 times the number of tokens invested by the other people in your group.

(3) If you keep one and invest one, then your earnings will be:

$1.00 x 1 = $1.00 from the token kept, plus

$0.25 x 1 = $1.00 for the token invested, plus

$0.25 times the number of tokens invested by the other people in your group.

Note: In each of the 3 above cases, what you earn from the others' investments is: $0.00 if the others invest 0 tokens, $0.25 if the other people invest 1 token (in total) and keep the rest, $0.50 if the other people invest 2 tokens (in total), etc.

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 7 other people in your group will also have 10 tokens. Everybody earns money in the same manner: $1.00 for each token kept, $0.25 for each token invested, and $0.25 for each token invested by the 7 other people.

At the start of a new round, you will be given a new endowment of 10 tokens. Your 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.

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 7 other people is Y tokens.
a) Then you earn (10 - X)*$1.00 + X*$0.25 .
b) Then your earnings will be at least as high as (10 - X)*$1.00 + X*$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.
Bottom of Form 1

Screen 5
Question 1: Suppose you invest X of your 10 tokens and the total number invested by the 7 other people is Y tokens.
(a) Then you earn (10 - X)*$1.00 + X*$0.25 .
(b) Then your earnings will be at least as high as (10 - X)*$1.00 + X*$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.

Screen 6
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 $1.00 each) or invest (and earn $0.25 each), knowing that they will also earn $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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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 payoffs = (8 @ $7.50)
Skewed payoffs = (1 @ $20, 4 @ $7, 3 @ $4)
Symmetric payoffs = (3 @ $10, 2 @ $7.50, 3 @ $5)

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]

Table 3
Contribution Means and Standard Deviations, By Form of Inequality and Distribution

Distribution of Payments	Private Inequality	Public Inequality	Both Private and Public Inequality
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

Table 4
Random Effects GLS Models of Contributions

	All Sessions		**Private Inequality**		**Public Inequality**	
Explanatory Variable	*10 rounds n=480*	*30 rounds n=1440*	*10 rounds n=240*	*30 rounds n=720*	*10 rounds n=240*	*30 rounds n=720*
Public	-0.879 (1.47)	-0.267 (0.58)				
Fixed Payment	0.036 (0.38)	0.024 (1.01)	-0.078 (0.64)	-0.017 (0.62)	0.150 (1.02)	0.068* (1.74)
Round 2	0.667* (1.64)	0.729* (1.90)	0.750 (1.38)	1.019** (2.26)	0.583 (0.98)	0.440 (0.71)
Round 3	0.792* (1.95)	0.444 (1.16)	1.417*** (2.60)	0.949** (2.11)	0.167 (0.28)	-0.060 (0.10)
Round 4	0.708* (1.75)	0.000 (0.00)	0.958* (1.76)	0.352 (0.78)	0.458 (0.77)	-0.352 (0.57)
Round 5	0.063 (0.15)	0.007 (0.02)	0.583 (1.07)	0.727 (1.62)	-0.458 (0.77)	-0.713 (1.16)
Round 6	-0.542 (1.33)	-0.438 (1.14)	0.042 (0.08)	0.199 (0.44)	-1.125* (1.89)	-1.074* (1.74)
Round 7	-0.688* (1.69)	-0.493 (1.28)	0.500 (0.92)	0.352 (0.78)	-1.875*** (3.14)	-1.338** (2.17)
Round 8	-1.000** (2.46)	-0.660* (1.72)	-0.375 (0.69)	0.046 (0.10)	-1.625*** (2.72)	-1.366** (2.22)
Round 9	-1.125*** (2.77)	-0.792** (2.06)	-0.458 (0.84)	-0.301 (0.67)	-1.792*** (3.00)	-1.282** (2.08)
Round 10	-1.729*** (4.26)	-1.653*** (4.31)	-1.042* (1.91)	-0.968** (2.15)	-2.417*** (4.05)	-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	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.

Table 5
Random Effects GLS Models of Contributions
10 Round (Between-Subject) Analysis

	Private Inequality			Public Inequality		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Model 1						
Unequal Distribution	0.313 (0.38)	-0.016 (0.02)	0.207 (0.24)	-1.550* (1.65)	-1.463* (1.67)	-1.394 (1.60)
Model 2						
Relative Deprivation Index	-0.342 (0.13)	-1.589 (0.61)	-0.850 (0.32)	-0.919 (0.30)	-1.742 (0.60)	-0.792 (0.28)
Model 3						
Relative Deprivation Index	-2.797 (0.68)	-3.909 (0.93)	-4.705 (0.96)	7.219 (1.61)	5.265 (1.17)	5.774 (1.48)
Unequal Distribution	1.014 (0.76)	0.938 (0.71)	1.467 (0.94)	-3.359** (2.32)	-2.814* (1.95)	-2.872** (2.19)
Model 4						
Maximum Payment	1.694 (1.01)	1.725 (1.05)	1.681 (0.88)	-0.713 (0.36)	-0.187 (0.10)	-1.647 (0.96)
Unequal Distribution	1.583 (1.05)	1.278 (0.86)	1.497 (0.89)	-2.084 (1.19)	-1.603 (0.97)	-2.663* (1.67)
Model 5						
Payment Relative to Max	4.478** (2.02)	4.948** (2.36)	5.398** (2.04)	-3.676 (1.39)	-1.343 (0.47)	-3.328 (1.29)
Unequal Distribution	2.272* (1.84)	2.105* (1.80)	2.665* (1.86)	-3.158** (2.13)	-2.062 (1.32)	-2.902** (2.01)
Control for race and gender	no	yes	yes	no	yes	yes
Control for major and political ideology	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, and the round of play. The number of observations used in each model's estimation is 240. The subject traits added to some models are indicator variables measuring subject sex (female), race (nonwhite), 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.

Table 6
Random and Fixed Effects GLS Models of Contributions
30 Round (Within-Subject) Analysis

	Private Inequality			**Public Inequality**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Model 1						
Unequal Distribution	0.054	0.054	0.054	-0.827***	-0.827***	-0.827***
	(0.37)	(0.37)	(0.37)	(4.16)	(4.16)	(4.16)
Model 2						
Relative Deprivation Index	0.044	0.060	0.054	-2.172***	-2.156***	-2.134***
	(0.09)	(0.13)	(0.11)	(3.35)	(3.32)	(3.27)
Model 3						
Relative Deprivation Index	-0.277	-0.234	-0.253	0.044	0.110	0.214
	(0.35)	(0.29)	(0.31)	(0.04)	(0.10)	(0.19)
Unequal Distribution	0.123	0.113	0.118	-0.838**	-0.855**	-0.881**
	(0.50)	(0.45)	(0.47)	(2.45)	(2.49)	(2.55)
Model 4						
Maximum Payment	0.419	0.410	0.483	-0.323	-0.303	-0.331
	(1.22)	(1.19)	(1.40)	(0.70)	(0.66)	(0.71)
Unequal Distribution	0.368	0.361	0.417	-1.069***	-1.055***	-1.075***
	(1.24)	(1.22)	(1.40)	(2.68)	(2.64)	(2.67)
Model 5						
Payment Relative to Max	0.587	0.579	0.596	-0.660	-0.661	-0.687
	(1.36)	(1.34)	(1.38)	(1.14)	(1.14)	(1.18)
Unequal Distribution	0.311	0.307	0.315	-1.116***	-1.116***	-1.128***
	(1.30)	(1.28)	(1.32)	(3.46)	(3.45)	(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 (nonwhite), 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.

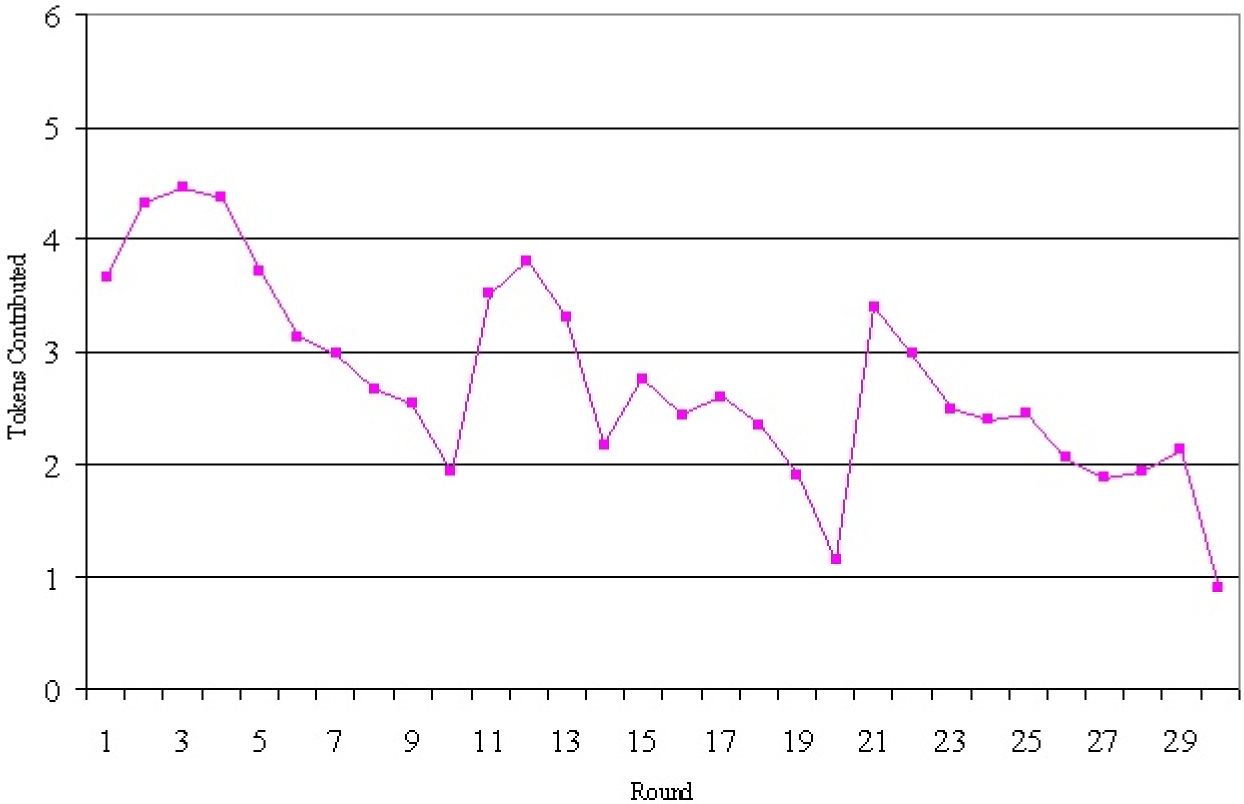

Figure 1
Mean Tokens Contributed, By Round