

**Inequality, Group Cohesion, and Public Good Provision:
An Experimental Analysis**

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ABSTRACT

Recent studies argue that inequality reduces group cohesiveness and dampens support for expenditures on public goods and social programs. In light of competing theoretical explanations and mixed empirical evidence of the effect of inequality on public goods provision, we conduct a test using a public goods experiment. 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.

I. Introduction

Social scientists have long been intrigued by the relationship between inequality and well-being, but the last few years have witnessed a flurry of empirical scholarship that examines the effects of inequality on various economic, political and social indicators.¹ One of the primary explanations for how inequality might negatively influence well-being is through its hypothesized effects on the content of public policy; in particular, several authors argue that inequality undermines group cohesiveness (a component of social capital) and reduces popular support for expenditures on public goods and social programs [e.g., Kaplan et al. 1996, Wilkinson 1996, Knack and Keefer 1997, and Kawachi et al. 1997].² In contrast to this social capital mechanism, economists have argued that increased inequality may yield a median voter that is more supportive of government spending [e.g., Meltzer and Richard 1981 and 1983]. Consequently, the theoretical influence of inequality on government provision of public goods is ambiguous.

The emerging empirical literature on inequality and public spending also provides mixed evidence on the nature of this relationship. More importantly, Osberg, Smeeding and Schwabish [2003] note that existing studies have yet to deal convincingly with several methodological challenges, including the comparability of international data, and the joint determination of both income distribution and public policy. In this paper, we present new empirical evidence based on a different approach that sidesteps these particular pitfalls. We examine the effects of 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. 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.³ Because the experimental setting also allows us to rule out the operation of a political mechanism, we are able to attribute any observed effects of inequality to changes in group cohesiveness. In half of the sessions in our experiment, each individual's placement in the distribution of fixed payments is revealed 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 combine the use of survey and experimental data in our analysis; as a result both the focus and design of our study complement 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 these games, 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).⁴ However, once we control for the treatment effect of inequality at the group level, relative deprivation is no longer significantly associated with contributions. Instead, inequality 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 impacts group cohesiveness and has important 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]. One explanation for such inconsistent findings regarding the importance of group heterogeneity is that social, historical or other factors may serve to make some differences more salient than others for cohesion in particular groups. For example, ethnic homogeneity may be salient for a group composed of first-generation Americans, but less so for the general population. Similarly, income inequality may be less salient for groups whose members are drawn from locales with more egalitarian income distributions. In like manner, we interpret the importance of public information for the magnitude and significance of the treatment effect of inequality on contributions as evidence that only salient forms of heterogeneity influence the efficacy of collective action.

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 experimental design, 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].⁵ 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, 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., Wilkinson 1996, Putnam 2000].

As noted, empirical studies of inequality and public goods provision produce mixed results. 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] show that state-level expenditures on education in the United States are inversely related to state-level inequality, but Alesina, Baqir and Easterly [1999] do not find a significant association between income inequality and expenditures on productive public goods across U.S. metropolitan areas.

In their review of this literature, Osberg, Smeeding and Schwabish [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. However, endogeneity is arguably less of a concern with other measures of heterogeneity, such as ethnic and racial fragmentation.

Alesina, Baqir and Easterly [1999] do find a significant association between such measures of heterogeneity 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 limited, support for the contention that inequality influences public good provision through group cohesiveness.

One 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) find 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.⁶ 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 cohesion 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.⁷

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. Financial constraints and other concerns prevent us from using payments that substantially alter an individual's (or their family's) disposable income. For this reason our treatment can be broadly interpreted as generating heterogeneity within the subject group.

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 points⁸ 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 interaction 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 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 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.⁹ 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 in two ways, their results differ from predicted outcomes. Richer than average people contribute less than predicted and poorer than average people contribute more than predicted.¹⁰

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.¹¹

Two additional studies in non-linear environments find no effect of inequality of 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 [2002] 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. 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 I 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.¹² 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.

V. Experimental Results

A. Descriptive Statistics

Figure I 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. This is slightly below the forty to sixty percent range reported in summary studies.¹³ Consistent with previous studies, contributions generally decline with repetition. This trend is less obvious when results are reported separately for each session, as in Table II. In three sessions (2, 3 and 4) the 10-round average contribution rate monotonically decreases with repetition. In the remaining sessions (1, 5 and 6) average contribution rates increase or remain steady over some periods. One possible explanation for these differences is that the order of the inequality treatments varied across sessions.

Repeating a set of ten rounds under a new fixed payment distribution resulted in a positive “reset effect” in rounds 11 and 21, as shown in Figure I. The reset effect is measured 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. Table III shows that there is considerable variation in reset effects across treatments and sessions. 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. We found no significant difference in the average reset effects as treatments varied from more to less equal, or from less to more equal.

Table IV provides more insight about the effect of the fixed payment distribution on contributions to the group account. Combining results from the private and public inequality treatments (the far right column in table IV), we found that contributions were highest with the egalitarian distribution (3.01 versus 2.63 for symmetric and 2.61 for skewed). These differences are more pronounced in the public inequality sessions, and do not hold for the private inequality sessions. In order to test whether subject contributions differed significantly across treatments, we conducted a series of Wilcoxon signed rank tests for matched pairs. Results suggest that 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) can we reject the null hypothesis that contributions in the egalitarian treatment are the same as contributions in either of the unequal treatments considered separately.

B. Tobit Models

Since the order of inequality treatments varied across subjects, the results of Wilcoxon signed rank tests may be confounded by subject experiences over the course of the experiment. To identify the effect of inequality on group account contributions holding all else equal, we use data from the experiment to conduct multivariate analysis. We begin with a basic model of contributions, C^* , as specified in equation (1) below:

$$(1) C_{ibr}^* = \alpha + \beta FP_{ib} + \sum_{b=2}^3 \gamma_b BLOCK_b + \sum_{r=2}^{10} \lambda_r ROUND_r + \eta_1 RESET_1 + \eta_2 RESET_2 + \omega_{ibr}$$

where I references individual subjects (1 through 48), b references the block (1, 2 or 3), and r references the round within the block (1 through 10). The fixed payment (FP) is the dollar amount received by subject I in block b . In addition to an intercept, α , we include two dummy variables equal to 1 if the subject's decision is made in either block 2 or 3 and nine dummies to represent decisions made in each of rounds 2 through 10. To capture the two reset effects depicted in Figure I, we include $RESET_1$, equal to 1 in the first round of the second block (and 0 otherwise) and $RESET_2$, equal to 1 in the first round of the third block (and 0 otherwise). Finally, ω_{ibr} is a composite error term that is assumed to be distributed normally with mean zero. To allow for unobserved subject-specific differences in group contributions, we estimate the model with random-subject-effects, where ω_{ibr} is defined by equation (2) below, in which v_i is a random disturbance term for subject I , and ϵ_{ibr} is the disturbance term for the decision made by subject I in block b , round r :

$$(2) \omega_{ibr} = v_i + \epsilon_{ibr}$$

The variable C_{ibr}^* in equation (1) is a latent variable which represents the subject's unobserved tendency to contribute tokens to the group account in each period of play. Values of C_{ibr}^* greater than 10 are not observed, and negative values are observed as zeros. For this reason, we construct C_{ibr} , the dependent variable used in estimation, according to equation (3) below:

$$(3) C_{ibr} = \begin{cases} C_{ibr}^* & \text{if } 0 < C_{ibr}^* < 10 \\ 10 & \text{if } C_{ibr}^* \geq 10 \\ 0 & \text{if } C_{ibr}^* \leq 0 \end{cases}$$

Given that the dependent variable is both right- and left-censored, we estimate the model as a tobit with a lower limit of 0 and an upper limit of 10.

Table V reports the results from the estimation of the basic model as a random-effects tobit using data from all six sessions, and then separately using data from sessions in which the inequality was revealed in a private or public manner. The effects of all explanatory variables are reported as marginal effects, calculated as the tobit coefficient multiplied by the predicted probability that the dependent variable is uncensored, which is evaluated at the mean of all explanatory variables. When we estimate the model using data from all sessions, we also include a dummy variable (*PUBLIC*) equal to 1 in sessions in which fixed payments to all subjects were publicly revealed. In column (2) the marginal effect of this variable is negative but not statistically significant. To test whether public inequality has an interactive effect with other explanatory variables, we estimate our models separately using either data from public inequality or private inequality sessions (shown in columns 3 through 6).

The estimated effects of the round, block, and reset variables generally have the expected signs. The full sample results show that repeated play has an initial positive and significant effect in Round 2, no effect on contributions in the middle rounds, and in final rounds results in reduced contributions to the group account. The negative effect of repeated play sets in earlier in the sessions with public inequality: in columns (5) and (6) the effects of dummies for rounds 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. In both types of sessions, decisions made in the second and third blocks are lower relative to the first round of

play (although both block effects are significant only in the private inequality sessions). The effects of $RESET_1$ and $RESET_2$ suggest that subjects increase their tokens in the first round of a new block in the private inequality sessions. Also of interest in Table V is the finding regarding the fixed payment, which has no significant effect on contributions to the group account in sessions with either public or private inequality. This suggests that our show-up fees are sufficiently low to avoid wealth effects.

Table V establishes first, that experience generally has the predicted negative effect observed in previous studies, second, that there are some differences in subject behavior according to the manner in which inequality was revealed, and third, that the level of the fixed payment has no independent effect on contributions to the group account. We now evaluate whether the distribution of the fixed payment has an effect on contributions; to do this, we add various measures of the fixed payment distribution to the basic model specified in equation (1) in separate specifications reported in Table VI. Models 1 through 5 include the explanatory variables measuring inequality that are shown in the relevant rows, as well as controls for fixed payment, round, order, and reset effects.

Model 1 includes two measures describing the distribution of the fixed payments in the form of a dummy variable equal to 1 if the distribution is skewed (and 0 otherwise), and a dummy variable equal to 1 if the distribution is symmetric (and 0 otherwise). The omitted category represents the egalitarian distribution in which all subjects received a fixed payment of \$7.50. The results suggest that the distribution of fixed payments had a significant effect on contributions to the group account, but only in the sessions in which inequality was public. Here, compared to decisions made under egalitarian distributions, contributions were three-

quarters of a token smaller in the skewed distributions and almost a full token smaller in the symmetric distributions. This evidence is consistent with the model of Alesina, Baqir and Easterly [1999], in which heterogeneity dampens the ability to provide public goods. A test of the null hypothesis that the coefficients of the two distribution dummies were equal could not be rejected. Thus, it appears that the presence of inequality, but not the level, affects contributions to the group account.

In Model 2, we include a subject-specific measure called the relative deprivation index, following a definition given in Deaton [2001]. The index is calculated according to equation (4) below:

$$(4) \quad RDI_i = 1 - F(x_i) \frac{\mu^+(x_i) - x_i}{\mu_r}$$

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). As we found for the dummy variables measuring the distribution, we find a significant effect for the RDI only in the public sessions; specifically, subjects with a higher relative deprivation index

contributed significantly fewer tokens to the group account.

In Model 3 we control for both the relative deprivation index and the nature of the payment distribution. Since we could not reject the equality of the two distribution coefficients in Model 1, we control for inequality with a dummy equal to 1 if the distribution was either skewed or symmetric, and 0 if egalitarian. When fixed payments are drawn from an unequal distribution, subjects in the public sessions reduced contributions to the group account by two-thirds of a token (column 4). However, once we control for the nature of the distribution, the subject's placement within the distribution (indicated by the RDI) no longer has a significant effect.

Finally, Models 4 and 5 employ two alternate 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. A second measure of relative income is calculated by dividing the subject's payment by the maximum payment in the fixed payment distribution. These individual-specific measures had no significant effect on group account contributions in either public or private settings upon controlling for the nature of the distribution. Similar to our results from Models 1 and 3, we observe that subjects in the public sessions gave fewer tokens to the group account when fixed payments were unequally and publicly distributed.

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 contribution, the result is a lower overall level of public goods provision. This result is also consistent with the work of Ball et al. [2001] who award 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 random or believed to be 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.

We next test the sensitivity of our results in Table VI to a number of additional controls, beginning with various subject traits measured by a survey instrument completed at end of each experimental session. First, we add explanatory variables representing the subject's gender and race to the tobit models. Gender is represented with a dummy variable equal to 1 if the subject was female, and race is measured with a dummy variable equal to 1 if the subject was nonwhite. The estimated inequality effects from this exercise are shown in columns 2 and 4 (the full results are available upon request). Our main results are robust to the inclusion of controls for gender and race; inequality in the fixed payment distribution dampened contributions to the group account only when publicly revealed. The estimated effect of gender was mixed, which is consistent with results from other studies. Compared to males, female subjects generally made larger contributions to the group account in the public sessions, and smaller contributions in the private sessions. The estimated effect of the nonwhite dummy also varied across the sessions. Compared to whites, nonwhite subjects generally contributed fewer tokens to the group account

in the private sessions; in the public sessions the effect of race varied across Models 1 through 5, from negative and significant, positive and significant, to zero.

As an additional test of the robustness of our main results, we add variables to control for the subject's academic concentration and political ideology (results are shown in columns 3 and 6). Academic concentration was represented by a dummy variable equal to 1 if the subject was an economics major, and 0 otherwise. We measure political ideology with a dummy variable equal to 1 if the subject reported that his or her interests were best represented by the Democratic party, and a dummy variable equal to 1 if the subject's interests were not closely represented by either Democrats or Republicans (the omitted category represents Republican party interests). As shown in column (3) and (6) of Table VI, our substantive results regarding the dampening effect of inequality on public goods contributions are unchanged when these variables are included. As for the effects of these additional controls, we observe that subjects majoring in economics make smaller contributions to the group account, as reported in previous studies [e.g., Marwell and Ames 1981]. The effect of political ideology varied; in the some models, subjects whose interests aligned with the Democratic party made higher contributions.

Our survey also queried respondents about their attitudes regarding the level of government spending on a number of programs. From these responses, we define a dummy variable equal to 1 if the respondent felt that the government spending was too low for at least 5 of the 8 programs we listed.¹⁴ In all five models estimated using the data from the private inequality sessions, preferences for more government spending were positively associated with public goods contributions. In the public inequality sessions, the effect of this variable was positive and significant in two models, and negative and significant in two other models. In no

case did controlling for government-spending preferences alter the sign or significance of the estimated effects for the inequality measures.¹⁵

Finally, we test the sensitivity of our results by including in our model a measure of the total amount contributed to the group account by other players in the previous period. Some analyses of public goods experiments have included a similar measure to control for subject experience over time. We prefer our initial specification for two reasons. First our round, block and reset effects already account for experience; furthermore, other players' contributions in the previous round may also be a function of the distribution of fixed payments in the block. Nonetheless, to rule out the possibility that the significance of our distribution dummies are driven by subject reactions to the previous round's result, we include the amount that other subjects contributed to the group account in the previous period. This requires dropping the first round observations for all sessions, for a total loss of 24 observations in the public sessions, and 24 in the private sessions. The results (available upon request) reveal two patterns: first, only in the public sessions is there a significant (and positive) effect of the other players' contributions on individual contributions to the group account; and second, the inclusion of this variable does reduce the size of the inequality effects somewhat, but these marginal effects retain their negative sign and statistical significance.

In summary, the main results from tobit models presented in Table VI are robust to various changes in model specification, including the addition of controls for gender, race, academic concentration, political ideology, preferences for government spending, and past contributions to the group account. In addition, several of our results regarding these controls are consistent with findings reported in some previous empirical analyses of public goods

experiments.

VI. Conclusions

Several researchers have asserted that inequality influences public spending, but perspectives on the direction of and the causal mechanism behind this relationship differ considerably. Some theories posit that inequality results in a less well-to-do median voter and thereby increases spending, while other explanations suggest that inequality reduces spending (especially on public goods) by decreasing the cohesiveness of the group. Moreover, the empirical evidence is difficult to categorize due to both data and methodological challenges. Yet, establishing the direction, size, and sign of the relationship has important implications for research on the potential effects of inequality on economic growth, education, and health status. In this paper, we test the effect of inequality on public goods provision in an experimental setting that offers two advantages over non-experimental empirical analyses. First, our design allows us to sidestep a number of empirical challenges, especially the potential problem of endogeneity due to the effect public spending asserts on inequality. Second, our experiment allows us to examine one specific pathway through which inequality and public spending are linked. By using a standard public goods game devoid of political mechanisms, we are able to attribute any observed effects of inequality to changes in group cohesiveness. Because we introduce the treatment of inequality in a way that does not alter the set of choices available to subjects, our design differs from most existing experimental studies on 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. This finding is observed only when the fixed payment is distributed publicly, that is, fixed payment draws are known to all subjects. Once we control for the nature of the distribution across the group, the individual's relative standing in the group does not affect contributions. The importance of public signals for the manifestation of a treatment effect of inequality on contributions in the public goods game is 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 cohesion. Consequently, future work on the determinants of social capital should investigate the factors that render some group characteristics salient.

The importance of public signals is also consistent with the work of Ball et al. [2001], which finds that status awards had a significant effect on market outcomes only when both sellers and buyers knew of each others' status. 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. It may be the case that our pooled analysis masks 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]. To explore whether repetition affected individual responses to inequality, we estimated our model using only observations from the first round of play, thus simulating a one-shot game. Although the sample size was limited, the qualitative results from this exercise were the same as those reported in Tables V and VI. Even in the first round of play, inequality dampens contributions by all subjects in the group, and relative standing is insignificant.

In conclusion, the results of this study provide novel support for recent claims that inequality impacts group cohesiveness and has important implications for cooperation in collective action problems. We find support for one of the primary explanations for how inequality might negatively influence well-being in the form of reduced growth, lower

educational attainment and worse health status. In addition, the design of our experiment allows us to attribute this result to the reductions in group cohesiveness or social capital, rather than a political process.

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 \times 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 \times 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 \times 1 = \1.00 from the token kept, plus

$\$0.25 \times 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. 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.

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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Notes

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 Fajnzlyber, Lederman and Loayza [2002] on crime.
2. On the general efficacy of social capital, see especially Putnam [2000]; other studies of the effects of social capital 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 [2003] on health; and Galea, Karpati and Kennedy [2002] on crime.
3. This is in contrast to previous experimental designs 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.
4. On the implications of altruism and inequality aversion for public goods games, see Buckley and Croson [2003].
5. See Mulligan [2003] for a recent critique of the Meltzer-Richard hypothesis.
6. However, see Vigdor [2003] for a critique of the use of fragmentation indices in this literature.
7. For example, Cardenas [2003] studies commons games with experimental subjects drawn 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.
8. Provision points are threshold amounts that must be reached before anyone can receive a benefit from the group account.
9. See Laury and Holt [1998] for a review of non-linear public goods studies.
10. Similar results are reported in Chan et al. [1997].
11. Chan et al. [2003] analyze individual level data for the same experiments.
12. Since fixed payments were awarded randomly, these sessions most closely match the random status treatment used by Ball et al. [2001].
13. See, for example, Anderson [2001] and Ledyard [1995].

14. The survey question is worded: “Use the following scale to indicate your opinion about government spending on each program (1=too little, 2=about right, 3=too much): National defense, Foreign aid, Welfare, Education, Transportation, Social Security, Medicare, Police and Prisons.”
15. Results are available from the authors upon request.

Table I
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
<i>Total Subjects</i>					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 II
Descriptive Statistics for Tokens Contributed, by Session and Block

Private Inequality	Mean [Min, Max]	Public Inequality	Mean [Min, Max]
<i>Session 1</i>	2.63 [0, 10]	<i>Session 4</i>	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]
<i>Session 2</i>	2.01 [0, 8]	<i>Session 5</i>	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]
<i>Session 3</i>	4.00 [0, 10]	<i>Session 6</i>	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 III
Reset Effects by Session

Private Inequality	Reset Effect	Public Inequality	Reset Effect
<i>Session 1</i>		<i>Session 4</i>	
From Egalitarian to Skewed	0.25	From Egalitarian to Skewed	0
From Skewed to Symmetric	2.38	From Skewed to Symmetric	2.25
<i>Session 2</i>		<i>Session 5</i>	
From Skewed to Symmetric	0.13	From Skewed to Symmetric	3
From Symmetric to	1.13	From Symmetric to	0.25
Egalitarian		Egalitarian	
<i>Session 3</i>		<i>Session 6</i>	
From Symmetric to	1.25	From Symmetric to	4.88
Egalitarian		Egalitarian	
From Egalitarian to Skewed	3.13	From Egalitarian to Skewed	4.38

Notes: The reset effect is the calculated by subtracting the mean of tokens contributed in the tenth round of the previous block from the mean of tokens contributed in the first round of the second or third block. The mean reset effect is 1.92 tokens.

Table IV
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 V
Random Effects Tobit Models of Contributions
(Marginal Effects of Explanatory Variables Reported; absolute values of t-statistics in parentheses)

Explanatory Variable	All Session (n=1440)		Private Inequality (n=720)		Public Inequality (n=720)	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Fixed Payment</i>		0.011 (0.49)		-0.009 (0.34)		0.042 (0.74)
<i>Public</i>		-0.302 (1.27)				
<i>Round 2</i>	0.912 (2.17)	0.912 (2.17)	1.105 (2.26)	1.106 (2.26)	0.687 (1.15)	0.693 (1.14)
<i>Round 3</i>	0.512 (1.24)	0.512 (1.24)	0.994 (2.04)	0.995 (2.04)	0.109 (0.20)	0.110 (0.20)
<i>Round 4</i>	0.062 (0.15)	0.062 (0.15)	0.315 (0.66)	0.316 (0.66)	-0.099 (0.19)	-0.103 (0.20)
<i>Round 5</i>	0.085 (0.21)	0.086 (0.21)	0.773 (1.60)	0.773 (1.60)	-0.380 (0.79)	-0.388 (0.79)
<i>Round 6</i>	-0.407 (1.05)	-0.412 (1.06)	0.198 (0.42)	0.198 (0.42)	-0.730 (1.68)	-0.739 (1.66)
<i>Round 7</i>	-0.445 (1.16)	-0.450 (1.16)	0.358 (0.75)	0.358 (0.75)	-0.858 (2.07)	-0.872 (2.05)
<i>Round 8</i>	-0.675 (1.79)	-0.681 (1.79)	-0.008 (0.02)	-0.008 (0.02)	-0.956 (2.38)	-0.973 (2.35)
<i>Round 9</i>	-0.788 (2.12)	-0.794 (2.12)	-0.367 (0.82)	-0.366 (0.81)	-0.904 (2.21)	-0.923 (2.19)
<i>Round 10</i>	-1.769 (5.50)	-1.793 (5.50)	-1.105 (2.70)	-1.105 (2.69)	-1.751 (6.15)	-1.789 (5.79)
<i>Second Block</i>	-0.839 (5.48)	-0.844 (5.49)	-1.386 (7.98)	-1.385 (7.98)	-0.300 (1.44)	-0.309 (1.46)
<i>Third Block</i>	-1.289 (8.54)	-1.294 (8.54)	-1.692 (9.81)	-1.692 (9.82)	-0.779 (3.79)	-0.786 (3.74)
<i>Reset 1</i>	0.725 (1.36)	0.725 (1.36)	0.980 (1.57)	0.980 (1.57)	0.446 (0.60)	0.449 (0.60)
<i>Reset 2</i>	1.146 (2.10)	1.142 (2.10)	1.632 (2.55)	1.633 (2.55)	0.672 (0.88)	0.676 (0.88)
Likelihood ratio test of $\sigma_u=0$	438.76	428.03	330.67	330.78	148.80	147.47

Notes: Marginal effects are calculated as the Tobit coefficient of the explanatory variable multiplied by the predicted probability that the dependent variable is uncensored (evaluated at the mean of explanatory variables).

Table VI
Random Effects Tobit Models of Contributions, by Form of Inequality
(Marginal Effects of Explanatory Variables Reported; absolute Values of t-statistics in Parentheses)

	Private Inequality			Public Inequality		
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Model 1</u>						
<i>Skewed Distribution</i>	-0.089 (0.50)	-0.074 (0.43)	-0.069 (0.42)	-0.780 (3.25)	-0.712 (3.32)	-0.804 (3.20)
<i>Symmetric Distribution</i>	0.108 (0.60)	0.119 (0.68)	0.103 (0.63)	-0.939 (3.97)	-0.854 (4.04)	-0.976 (3.90)
<u>Model 2</u>						
<i>Relative Deprivation Index</i>	0.010 (0.02)	0.012 (0.03)	-0.159 (0.34)	-2.577 (3.67)	-2.689 (3.79)	-2.243 (3.69)
<u>Model 3</u>						
<i>Relative Deprivation Index</i>	-0.036 (0.04)	-0.107 (0.14)	0.106 (0.14)	-0.362 (0.36)	-0.504 (0.46)	-0.224 (0.19)
<i>Unequal Distribution</i>	0.018 (0.07)	0.049 (0.20)	-0.007 (0.03)	-0.664 (1.98)	-0.700 (2.02)	-0.833 (2.29)
<u>Model 4</u>						
<i>Maximum Payment</i>	0.327 (0.90)	0.264 (0.81)	0.277 (0.85)	-0.198 (0.38)	-0.206 (0.44)	-0.212 (0.43)
<i>Unequal Distribution</i>	0.253 (0.82)	0.216 (0.81)	0.212 (0.77)	-1.05 (2.29)	-1.01 (2.36)	-1.07 (2.41)
<u>Model 5</u>						
<i>Payment Relative to Max</i>	0.417 (0.91)	0.378 (0.88)	0.344 (0.77)	-0.382 (0.71)	-0.378 (0.65)	-0.583 (0.99)
<i>Unequal Distribution</i>	0.190 (0.76)	0.183 (0.77)	0.173 (0.71)	-0.947 (2.88)	-0.997 (2.96)	-1.10 (3.31)
Control for race and gender	no	yes	yes	no	yes	yes
Control for major and political ideology	no	no	yes	no	no	yes

Notes: All models control for the fixed payment in the shuffle, and also include dummy variables for rounds, blocks and reset effects. The number of observations used to estimate each model is 720. Marginal effects are calculated as the Tobit coefficient multiplied by the predicted probability that the dependent variable is uncensored (evaluated at the mean of explanatory variables).

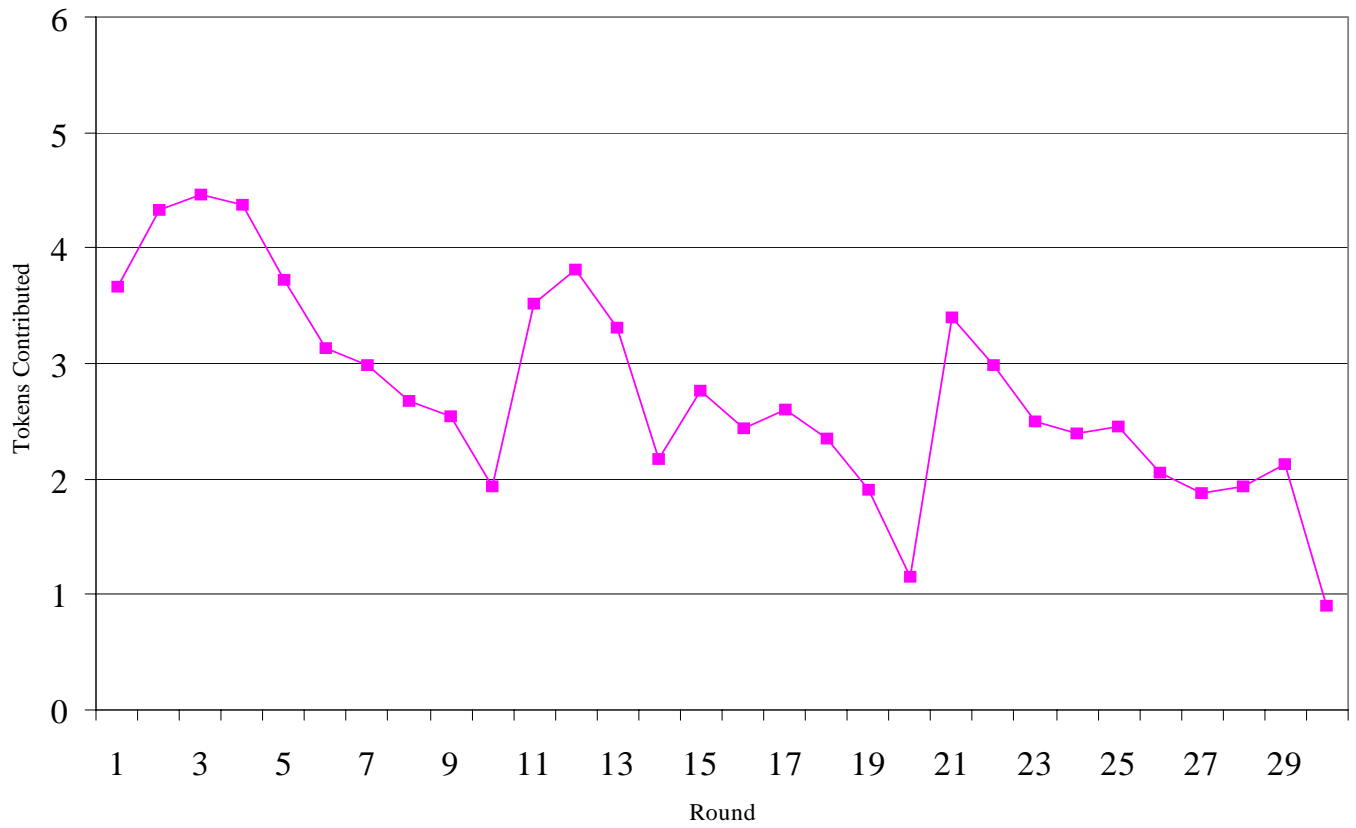


Figure I
Mean Tokens Contributed, By Round