Induced Heterogeneity in Trust Experiments[1]

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ABSTRACT

Several non-experimental studies claim that heterogeneity among individuals reduces trust. However, few experimental studies have examined the effects of naturally-occurring differences among subjects on trusting behavior, and most have not supported these claims. We adopt a novel approach by inducing heterogeneity among subjects in a canonical trust experiment. We accomplish this by varying the show-up payments given to subjects for participating in the experiment. Our results also do not corroborate the contention that heterogeneity uniformly reduces trust, although we do observe some significant effects from this treatment that warrant further exploration.

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I. Introduction

Do differences among individuals reduce trust? And if so, what kind of differences matter? In his recent influential volume on social capital, Putnam (2000) suggested that heterogeneity within groups hinders the formation of social trust, but this idea has a long history. Toqueville (1945) famously argued that it was the absence of class distinctions in American society that made Americans more other-regarding than their European contemporaries. More recently, social scientists have posited that familiarity breeds trust (Coleman 1990), that people are hard-wired to feel a bond with those that are like them (Wilkinson 1996), or that people simply have a preference for within-group interactions (Alesina and La Ferrara 2000). In support of these contentions that differences matter, several non-experimental studies have found that various indicators of population heterogeneity, from income inequality to racial and ethnic dissimilarities, are negatively associated with the probability that survey respondents express generalized trust in others (Putnam 2000, Zak and Knack 2001, Alesina and La Ferrara 2002 and Costa and Kahn 2003). However, few experimental studies examine whether ethnic, national, or religious differences between pairs of subjects influence behavior in trust games, and of these, most find no significant effects of such differences on subjects' behavior.[2]

In contrast to previous experimental studies that focus on naturally-occurring heterogeneity, we induce heterogeneity in the laboratory by varying the show-up payments awarded to all participants. While it would be impossible for us to alter subjects' relative wealth or income in any

[2] These contradictory findings may be attributable to the fact that the concept of "generalized social trust" examined in the non-experimental social capital literature does not correspond well to behavior in experimental trust games (Glaeser et al. 2000, Anderson, Mellor, and Milyo 2004b). Alternatively, it may be that naturally-occurring sources of group heterogeneity are not focal differences among subjects in the unfamiliar and artificial laboratory environment.

meaningful sense, our design does result in large differences in the amount of money that subjects take away from the experiment. Therefore, our heterogeneity treatment introduces a particular form of inequality. In Anderson, Mellor, and Milyo (2004a), we found that this method of inducing heterogeneity results in an overall decrease in contributions to a public good. Further, Ball et al. (2001) showed that differentiating among subjects by awarding some of them small gold stars is sufficient to change behavior in a market setting.[3]

Because we know of no previous study to induce heterogeneity in a trust game, our experimental design follows an exploratory approach. For example, in addition to the baseline treatment of equality in show-up payments, we consider two distributions of show-up payments with varying degrees of inequality. In addition, we also varied the method used to award show-up payments. In half of the sessions show-up payments were awarded privately and in the other half they were awarded in a public ceremony.[4] Importantly, the Nash prediction for how subjects will behave does not vary across any of the treatments, so behavior can only be affected through potential psychological effects. To summarize our key result, we find that induced inequality does not consistently affect first or second-mover behavior in the classic trust game in the manner predicted by previous theoretical work and by empirical studies of survey-based measures of trust.

II. Experimental Studies of Trust

The classic trust experiment was designed by Berg, Dickhaut and McCabe (hereafter, BDM

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[3] In some cases, subjects believed they were awarded the star because they outperformed others on a trivia quiz, but the stars were actually awarded randomly. In other cases, subjects were told the stars were awarded randomly. In both cases, awarding stars changed behavior.

[4] This feature of the design was also motivated by findings from the Ball et al. (2001) study, in which significant behavioral effects were not observed when stars were awarded privately.

1995). In this game, one subject (the first mover) is given some amount of money and offered the opportunity to pass some, all or none to a partner (the second mover). All passed money is multiplied by some predetermined amount before being received by the second mover. Finally, the second mover has the opportunity to pass some, all or none of the money she receives back to the first mover. Using backward induction, it is straightforward to show that the Nash equilibrium for this game is that no money will be passed in the first stage since second movers have no incentive to return money in the second stage.[5]

The original BDM (1995) trust experiment revealed that game theory does not predict actual behavior in this environment. On average, first movers sent around half of their endowment ($5.16 out of $10) to their second-mover partner. Second movers returned around one-third of what they received ($4.66 out of $15.48). Subsequent studies have reported some variation in the amounts sent and returned, but in general they confirm that the stark Nash equilibrium prediction does not hold up in these games.[6]

A common interpretation of this result is that first movers exhibit trust in their partners by passing a non-trivial amount of money and second movers exhibit trustworthiness by returning some of the money that was passed. However, there has been considerable debate among experimental economists regarding this interpretation, since the design does not allow one to rule out altruism as an alternative motive for sending and returning money. A recent study by Cox (2004) found evidence of altruism, trust, and reciprocity using a variation of the BDM (1995) design that disentangles the separate effects of altruism and trust.

[5] This analysis applies to a one-shot game, but can also be extended to a repeated game with a known endpoint.

[6] An excellent source for other experimental trust studies is Ostrom and Walker (2003).

Considerable attention also has been devoted to studying whether the difference between theory and behavior can be accounted for by culture, making the trust game one of the most well-traveled economics experiments.[7] While two recent studies have found significant differences in behavior across countries (Koford 2001 and Willinger et al. 2003), the majority of these studies have concluded that cultural differences do not significantly influence behavior in the trust game.[8]

A small number of trust experiments have looked at naturally-occurring heterogeneity in the form of pre-existing differences among subjects. Glaeser et al. (2000) found a small negative, but statistically insignificant, effect on the amount sent by first movers when players interacted face-to-face with a partner of a different nationality. This type of interaction also produced a negative, and in this case significant, effect on the amount returned by second movers. Fershtman and Gneezy (2001) conducted a trust game with Israeli college students, in which subjects were told the last name of their partner as a means of revealing their ethnicity. In this study, significantly less money was transferred to Eastern origin players by partners from both the East and the West, a finding that held for males but not females. Bouckaert and Dhaene (2004) conducted a similar experiment using businessman of Turkish or Belgian origin, but they reported no evidence of ethnic differences in the amount sent or returned. Willinger et al. (2003) paired French and German students and found no difference in behavior when subjects knew they were playing with someone from a country other than their own. Finally, Johansson-Stenman et al. (2005) matched Hindu and Muslim subjects from rural Bangladesh, and found no difference in behavior when subjects were paired with a partner with the same or different religious beliefs. Thus, these studies provide, at best, mixed evidence that

[7] Trust experiments have been conducted in 16 countries.

[8] Many cross-cultural studies using different experiments also find insignificant differences in behavior (e.g., Roth et al. 1991).

heterogeneity in the players' ethnicity or national origin reduces trusting behavior.

In this study, we build on these earlier works by examining the effect of heterogeneity in a readily-observed measure, and by inducing heterogeneity within a controlled laboratory setting. Heterogeneity that is induced in the laboratory has been studied extensively in other experiments,[9] but we are aware of no previous study that examines the effects of induced heterogeneity in the classic trust experiment.[10]

III. Experimental Design

For each of twelve sessions we recruited eight subjects to participate in the trust game established in BDM (1995). For each session of the experiment, subjects were recruited from undergraduate classes at the College of William and Mary.[11] Subjects were randomly assigned to be a first mover or a second mover in the game. Each subject participated in 30 decision-making rounds of the game; in each round, the first mover received a new $10 starting balance and amounts passed to the second mover were tripled.[12] Roles remained constant throughout the experimental session but subjects were randomly re-paired at the beginning of each new round. At the end of the session, one of the 30 rounds was randomly chosen for payment. In addition, each subject received

[9] For example, heterogeneity has been studied in public goods games (Chan et al. 1996 and 1999), in ultimatum games (Armantier 2003), and in two-stage bargaining games (Goeree and Holt 2000).

[10] Maximiano et al. (2004) reported that inequality (in the form of higher possible earnings) does not significantly decrease the reciprocity of workers in a gift-exchange model of a labor market.

[11] All subjects were recruited from large introductory-level classes that satisfy college-level general education requirements. Students in the authors' classes were not eligible to participate in the experiment.

[12] Following BDM (1995), second movers were also given a $10 starting balance each round, but they could not return any of their $10 starting balance to their first-mover partner. The $10 starting balance for second movers was designed to prevent very small earnings for second movers and to equalize earnings across roles in the experiment.

a show-up payment. Giving subjects a flat payment for showing-up is a standard practice in many experiments. This payment supplements what subjects earn based on their decisions and serves as a lower bound on their compensation for participating in the experiment. Each session lasted about one hour and earnings averaged $22.74.[13]

We used three different show-up payment distributions to introduce heterogeneity into the experimental design. As shown in Table 1, each distribution was applied for a set of 10 rounds in each session. In the "egalitarian" treatment, all subjects received the same show-up payment. To gain insight into how the degree and form of inequality might affect trust, we used two treatments in which show-up payments varied across participants. The symmetric distribution had smaller differences in payments across subjects: only $5 separated the lowest ($5) and highest ($10) payments, and three of the eight subjects received the highest possible payment. In contrast, in the skewed distribution there was a $16 difference between the highest ($20) and the lowest ($4) payment, and only one subject received the highest possible payment. In this treatment, more than half of the subjects received less than the mean payment, which is consistent with the type of skewness observed in survey-based data on income inequality. In all three distributions, the average payment was $7.50.

In Table 1, the "type of inequality" treatment refers to the manner in which show-up payments were assigned to participants. In the private treatment, all subjects were told the distribution of payments, but were privately given a card with their specific show-up payment written on it. In the public treatment, the show-up payments were awarded in a "ceremony." When the

[13] Starting with Glaeser et al. (2000), a number of trust experiments have been paired with surveys to measure the predictive power of the standard questions used to gauge trust and trustworthiness. We adopted this approach by having subjects complete a survey at the end of each session.

payments varied, each subject's name was recorded on a card and placed in a container. All subjects watched as one name was drawn from the container and that person was awarded the highest payment in the distribution. The remaining payments were awarded in a similar manner, starting with the second highest and finishing with the lowest.[14] This type of ceremony is similar to one used by Ball et al. (2001), which examined the effect of heterogeneity, in the form of status, on market interaction. Ball et al. (2001) reported that market interactions were only significantly affected by status when it was made salient through a public award ceremony.

IV. Results

The survey-based literature posits that heterogeneity has a dampening effect on generalized trust, so we first focus our attention on the amount sent by first movers in our experiments. We find that on average, first-mover behavior is comparable to results reported in BDM (1995). The mean amount sent by first- mover subjects in all sessions and rounds of our experiment is \$4.97 out of a possible \$10. The mean amount sent is smaller in the sessions in which the show-up payments were distributed in a more private manner, at \$4.41, compared to \$5.53 in the sessions where we used a public ceremony to award payments. According to rank-sum tests, this difference is not significant at conventional levels (p=0.15) for the combined treatments, but is significantly different in the public and private versions of the skewed treatment (p=0.05). Therefore, we used separate samples in our subsequent analysis to identify whether the private or public nature of the award ceremony influenced the effects of other experimental conditions on behavior.[15]

[14] Although show-up payments were awarded publicly, subjects did not know who they were matched with when making decisions. Hence, they never knew the show-up payment of any particular partner as they played the game.

[15] A comparable approach would be to interact a public indicator variable with each explanatory variable in the model, but given the number of explanatory variables, including round dummies, this

We first examine whether the presence or degree of inequality in the show-up payments affected the mean amount sent in ways consistent with findings from survey-based studies of trust. Survey-based evidence shows that generalized trust in the population decreases with increased degrees of heterogeneity. In Table 2, we present means and standard deviations for amount sent by the type of inequality treatment induced in our experiment. As shown on the left-hand side of the bottom row (for the private sessions), mean amount sent is greater in the egalitarian treatment than in either of the two unequal treatments. However, these differences are not significant according to Wilcoxon rank-sum tests, and a similar pattern is not exhibited by data from the public sessions. In the public sessions, the amount sent is largest in the skewed distribution, where the degree of inequality is most pronounced. In that treatment, the amount sent increases with the size of the show-up payment. In short, the data presented in Table 2 do not offer clear evidence that inequality in the show-up payment reduces amount sent, but do reveal some important differences in the method (private or public) of inducing inequality.

We next tested the effects of induced inequality in econometric models of amount sent using separate samples of data from the private and public sessions. For each sample, we started with 720 observations of amount sent, based on six sessions with four first movers per session and 30 decisions per subject. We defined the dependent variable as the amount sent by a subject in a given round. Following Cochard, Van, and Willinger (2004) who also modeled output from a repeated trust game, we controlled for the fraction returned by the second mover in the previous round.[16] For

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approach proved unwieldy.

[16] Anderhub, Engelmann, and Güth (2002) and Engle-Warnick and Slonim (2003) also conducted repeated trust games; these games differ in design from ours and thus the analysis of results is not comparable.

cases in which the first mover sent nothing in the previous round, we coded this variable as the fraction returned in the most recent previous round in which some money was sent. This required omitting the initial round of play for all subjects and two second round observations for subjects who sent zero in the first round. We also included a set of 28 dummy variables to control for the round of play, with the second round serving as the omitted category. Along with this set of explanatory variables, we included one of two sets of variables to test the effects of inequality.

Table 3 reports the results from these models.[17] We used linear fixed subject-effects models to capture unobserved subject-specific differences in behavior, and we adjusted the standard errors of the estimates for clustering by subject.[18] As shown in the first row of the table, the coefficient on the fraction returned variable is large, positive, and highly significant in all four models. This indicates that first movers who were rewarded with a higher return in past rounds sent more in a subsequent round. Further, these experiences of the first movers appear to be more important in the public inequality treatment than in the private inequality treatment as indicated by the size difference in the coefficients.

In columns (1) and (3), we report results from models using two indicator variables for the level of inequality in the show-up payment distribution. One indicator variable equals 1 for all

[17] For ease of exposition, we do not report the coefficients of the round dummy variables. These are jointly significant at the 0.01 level or better in each model.

[18] We considered several different econometric specifications. Since the dependent variable ranges from 0 to 10, some analyses of trust experiment data use Tobit models to address right- and left-censoring. Fixed effects can be introduced to the Tobit model with dummy variables for each subject, but produce biased and inconsistent results. We estimated random effects Tobit models using STATA software, but tests of the sensitivity of the quadrature approximation used in the random-effects estimators suggested that this strategy should not be employed with our data, possibly due to the degree of correlation within subject observations. Finally, GLS models with random effects were not appropriate as indicated by Hausman tests. For these reasons, we used linear robust-cluster fixed effects models.

subjects and rounds in which show-up payments were assigned from the skewed distribution, and 0 otherwise; another indicator variable equals 1 for all subjects and rounds in which show-up payments where assigned from the symmetric distribution, and 0 otherwise. The omitted category represents observations in which all subjects received an equal payment. If inequality reduces trusting behavior for all subjects in the group as the survey-based literature suggests, then we would expect to see negative and significant coefficients for the skewed and symmetric indicators.

The results from the econometric models confirm inferences that were drawn from the descriptive statistics in Table 2. In the private sample, we again see some indication that inequality induced in the experiment had a dampening effect on trust behavior: the amount sent is lower in both symmetric and skewed distributions relative to egalitarian. However, only the coefficient of the symmetric indicator variable is significant, and it is larger in absolute value than that for the skewed distribution, suggesting a larger response to less pronounced inequality. More importantly, this pattern is not observed in the public sessions, where both indicator variables are insignificant.[19] To summarize, these results are not consistent with the non-experimental literature on inequality and trust which suggests that inequality reduces trust. However, it is possible that heterogeneity affects trust for certain members more so than others, although few studies in the survey-based literature test this directly.

We tested whether the significant effects of the inequality indicators are the result of inequality *per se*, versus the subject's specific ranking in the group. To examine this, we replaced the set of inequality dummies with a set of six dummy variables that capture all possible relative standings among subjects. For example, subjects who received the lowest payment in the skewed

[19] In both the public and private sessions, we were not able to reject the null hypothesis that the coefficients for the two inequality variables were jointly equal to zero.

distribution have a value of 1 for the "Low-Skewed" indicator, and 0 otherwise. The remaining five variables represent the lowest payment in the symmetric distribution, the median payment in the skewed distribution, the median payment in the symmetric distribution, the highest payment in the skewed treatment, and the highest payment in the symmetric treatment. The omitted category in the model represents decisions made in the egalitarian distribution. If inequality affects all subjects in the same manner, as much of the survey-based literature on trust suggests, then we expect that the coefficients on all six dummy variables will be negative.

Again, the results from this exercise reveal differences in behavior depending on whether the inequality was induced privately or publicly. In the private sessions (column 2), all but one of the coefficients is negative, lending some support for the notion that inequality erodes trust for all members of the group; however, only two of the six coefficients are statistically significant. Specifically, subjects who received the lowest payment in the symmetric distribution or the highest payment in the skewed distribution sent less to their partners compared to subjects in the egalitarian treatments. Thus, these results suggest that the observed effect of inequality in the private sessions holds only for some subjects, in particular, for some subjects at the extreme ends of the show-up payment distribution. The coefficients of the indicator variables for the median payment in the two unequal treatments do not approach statistical significance.

The results from the public sample are not consistent with the hypothesis that inequality dampens trusting behavior for all members in the group. None of the coefficients on the relative standing indicators is negative and significant, and four coefficients are positive. The only significant effect in column (4) is for the indicator assigned to subjects who received the highest payment in the skewed distribution, which has a positive coefficient. This is consistent with the

column (3) estimate of a positive coefficient for the skewed distribution dummy.

To summarize, findings related to sender behavior in this trust experiment are not consistent with theoretical predictions or survey-based findings that heterogeneity erodes trust. One possible explanation for this discrepancy is that survey measures such as generalized trust in others, or beliefs about fairness and helpfulness in others may not correspond well to first-mover behavior in the classic trust game. For example, some studies (e.g., Glaeser et al. 2000) have shown that questions such as these are more closely related to the second-mover's behavior than the first mover's decision.[20] So, if trustworthiness is reflected in survey-based measures, we might expect to see more pronounced effects of inequality on second-mover behavior.

With this concern in mind, we also examined the data on second-mover decisions in our experiments.[21] In our combined sessions, the return ratio, calculated as the amount returned divided by three times the amount sent, has a mean value of 0.355. This is strong evidence that second-movers returned roughly the amount that was sent to them. The mean return ratio is not statistically different in the public and private samples, and there are only small differences in mean return ratio by type of inequality or show-up payment. In a set of fixed subject-effects models of the return ratio similar to those shown in Table 3, we do not find significant coefficients on the two inequality indicator variables.[22] In models using the six categorical dummies for relative standing, the only

[20] In contrast, Burks, Carpenter, and Verhoogan (2003) and Fehr et al. (2003) reported that several measures of generalized trust did not have significant associations with the second mover's transfer. We also examined the correspondence between survey-based questions and behavior in this study, and consistent with some of the studies in this literature, we also find mixed results.

[21] These results are available from the authors upon request.

[22] These models also included controls for the amount sent, a set of round dummy variables, and when the inequality variables were included, we also controlled for the subject's show-up payment. Standard errors were also adjusted for subject clustering.

significant coefficient is in the public sample, where the low-symmetric indicator has a negative and significant coefficient (p<0.10). The only explanatory variable to have a consistent significant association with the return ratio is the amount sent by the first mover. In all models, receiving a larger amount from the first mover led to an increase in the return ratio. In summary, we find little evidence that inequality induced in the lab had an across-the-board negative effect on subjects' trustworthiness. Thus, if survey-based measures of trust are more closely aligned with second-mover behavior, we remain unable to corroborate non-experimental findings of the effects of inequality on trust.

V. Discussion

There is a growing non-experimental literature linking inequality and other forms of heterogeneity to lower levels of trust. This literature has employed survey-based measures of generalized trust, and has focused exclusively on the common effects of group-based measures of inequality (i.e., the gini coefficient) on individual trust, while ignoring individual-based measures of relative standing. In a novel test of the importance of induced inequality, we find that inequality in subject show-up payments has effects on subject behavior that in some cases are very different than those observed in survey-based studies. We find that inequality in the distribution of show-up payments has a dampening effect only when show-up payments are awarded privately, and in that case, the negative effect is significant for less pronounced inequality but insignificant for more pronounced inequality. Further, the support for the hypothesis that inequality dampens trust for all members of a group is also weak. We find only two subject-specific measures of relative standing to be negative and significant. When show-up payments were awarded publicly, we find no support for the trust-dampening effect of inequality. In fact, subjects who received the highest fixed payment

in the most unequal distribution sent more to their randomly matched second mover partners.

The significant effect of receiving the highest show-up payment in the skewed distribution warrants further consideration. In our design, only subjects with this $20 award could be certain they were always paired with subjects with lower awards since there was only one award of this magnitude in each session. This is the only subject-specific measure of relative standing that had a significant coefficient in both private and public treatments (although signs varied). Thus, providing each subject with information about the specific show-up payment awarded to their randomly-matched partner in each round might reveal additional inequality effects.

There are other possible explanations as to why our findings fail to support the survey- based literature on trust and inequality. One explanation for this finding is that survey measures of trust are based on self-reported attitudes and behaviors that have been found to be unrelated to sender behavior in some trust experiments. Since some analysis has found that survey questions correspond more closely to second-mover behavior, we also examined patterns in the return ratio. Again, we find no support for an overall negative effect of inequality on trusting behavior.

Another explanation for the difference in our findings and those reported in survey-based studies is that the inequality induced in our design is not large enough to be meaningful to subjects. One interesting approach for future research is to pair subjects with others of various income levels. For example, in the traditional university setting student subjects could play the trust game first with other students, and then with faculty or staff. Alternatively, faculty subjects could alternate between faculty partners and staff partners.

Another explanation for why our findings are not consistent with the survey-based literature on trust is that the classic trust experiment is unable to separate trusting behavior from either a

concern for fairness or altruism. As a result, these alternative motivations for sending money might be confounding the trust-dampening effect of inequality. If first movers are more motivated by fairness than trust, we would expect subjects with higher show-up payments to send more to second movers. We see some evidence of this, but it is limited to the case when show-up payments are awarded publicly. Alternatively, altruism may affect subject behavior in response to inequality in the trust game, but the direction of the effect is not obvious. In another game that captures altruistic tendencies, the public goods experiment, we found that inequality induced in a similar manner dampened contributions (Anderson, Mellor and Milyo 2004a). If altruistic motives work in the same way in the trust game, we would expect a similar dampening effect in this environment, and our results would be biased in favor of finding a negative inequality effect. Since we find no effect in most cases, we expect that fairness motives may be a greater concern. One area for future work is to implement our inequality mechanism in the experimental design described in Cox (2004). This will allow us to disentangle the effects of trust, altruism and reciprocity.

Our results also call for examining differences in these settings by the nature of the inequality. We find that the response to inequality, whether in terms of trusting behavior, or fairness or altruistic considerations, differs when show-up payments were either privately or publicly awarded. For example, the amount sent by first movers was significantly higher in the public version of the skewed treatment than in the private version. When payments are awarded privately our results regarding the effects of inequality are generally consistent in sign (although not in all cases significant) with the survey-based literature that suggests inequality erodes trust. However, comparing the effects of subject-specific relative standing across the public and private samples reveals an important distinction. Relative to subjects in the egalitarian treatment, subjects who

received the highest payment in the skewed treatment sent less to their partners when the subjects' specific payments were private knowledge, but sent more to their partners when the specific payments were publicly known. If fairness concerns lead "richer" subjects to send more to second movers in the presence of unequal payments, then our results also suggest that the public revelation of inequality heightens the fairness response. Thus, in addition to introducing inequality into the Cox (2004) design as discussed above, future research might examine how small differences in the type of knowledge of inequality affect subject behavior.

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Table 1. Experimental Design

Sessions	Block 1 (10 rounds)	Block 2 (10 rounds)	Block 3 (10 rounds)	Type of Inequality	Number of Subjects
1 – 2	Egalitarian	Skewed	Symmetric	Private	16
3 – 4	Skewed	Symmetric	Egalitarian	Private	16
5 – 6	Symmetric	Egalitarian	Skewed	Private	16
7 – 8	Egalitarian	Skewed	Symmetric	Public	16
9 – 10	Skewed	Symmetric	Egalitarian	Public	16
11 – 12	Symmetric	Egalitarian	Skewed	Public	16
				Total Subjects	96

Notes: Egalitarian show-up payments = (8 @ $7.50)
Skewed show-up payments = (1 @ $20, 4 @ $7, 3 @ $4)
Symmetric show-up payments = (3 @ $10, 2 @ $7.50, 3 @ $5)

Table 2. Mean and Standard Deviation of Amount Sent By Inequality Treatment

Show-up Payment	Private			Public		
	Egalitarian	Symmetric	Skewed	Egalitarian	Symmetric	Skewed
$4			4.34 (3.78)			4.77 (3.04)
$5		3.63 (3.30)			3.60 (3.60)	
$7			4.18 (3.33)			6.41 (3.89)
$7.50	4.99 (3.75)	4.74 (3.21)		5.41 (3.64)	7.25 (3.24)	
$10		4.01 (3.58)			5.59 (3.61)	
$20			3.80 (3.36)			7.14 (2.84)
All	4.99 (3.75)	4.04 (3.42)	4.19 (3.50)	5.41 (3.64)	5.27 (3.54)	5.92 (3.54)

Table 3. Results of Fixed Subject Effects Models of Amount Sent

	Private *(n=694)*		**Public** *(n=696)*	
	(1)	(2)	(3)	(4)
Fraction Returned	3.391***	3.216***	4.026***	4.036***
	(4.62)	(4.39)	(5.03)	(5.09)
Show-up Payment	–0.089		0.086	
	(1.02)		(1.32)	
Skewed Distribution	–0.586		0.409	
	(1.41)		(1.08)	
Symmetric Distribution	–0.895†		–0.075	
	(1.94)		(0.21)	
Low Skewed		–0.537		0.056
		(0.86)		(0.11)
Low Symmetric		–1.415*		–0.166
		(2.44)		(0.18)
Median Skewed		–0.233		0.311
		(0.44)		(0.45)
Median Symmetric		0.440		0.557
		(0.83)		(1.07)
High Skewed		–2.214†		1.749†
		(1.87)		(1.74)
High Symmetric		–1.196		–0.233
		(1.45)		(0.52)

Notes: Results from linear fixed subject effects models are presented; the standard errors of the coefficient estimates were adjusted for subject clustering. Absolute values of t-statistics reported in parentheses. All models also include controls for round of play. The variable "fraction returned" is defined as the fraction returned in the last round or in the most previous round in which some money was sent by the first mover. Statistical significance indicated by *** for the 0.001 level, ** for the 0.01 level, * for the 0.05 level, and † for the 0.10 level.

Appendix: 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.[23] 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.

[23] In the public sessions, this sentence was replaced with "Hence, everyone in the room will know what the eight fixed payments are and who is randomly assigned each payment."

Appendix: Game Specific Instructions[24]

Rounds and Matchings: The experiment consists of a number of rounds. Note: In each round, you will be matched with another person selected at random from the other participants. There will be a new random rematching each round.

Interdependence: The decisions that you and the other person make will determine the amounts earned by each of you.

Pass/Keep Decisions: One of you will be designated to move first, and that person will begin by receiving a specified amount of money $10.00. The first mover will decide how much money (if any) to pass on to the other person and how much (if any) to keep. All money passed gets multiplied by 3 before it is received by the second mover, who then decides how much (if any) to keep and how much (if any) to pass back to the first mover. These pass/keep decisions determine earnings for the round, as explained below.

Role: You have been randomly assigned to be a First Mover, and you will begin each round with an amount of money, $10.00. You will decide how much to keep and how much to pass.
OR Role: You have been randomly assigned to be a Second Mover. The other person (first mover) will begin each round by receiving $10.00 and deciding how much to keep and how much to pass.

Earnings from Pass/Keep Process: The first mover earns the amount kept initially plus the amount that is passed back by the second mover. All money passed by the first mover is multiplied by 3. The second mover earns the amount kept at this stage.

Matchings: At the beginning of each round, there will be a new random pairing of all participants, so the person who you are matched with in one round may not be the same person you will be matched with in the subsequent round. Matchings are random, and you are no more likely to be matched with one person than with another.

Decisions: The first mover begins each round with $10.00 and must decide how much (if any) to keep and how much (if any) to pass. What is passed gets tripled before being received by the second mover. The second mover in each pair then decides how much (if any) to keep and how much (if any) to pass back.

Earnings: The first mover earns the amount kept initially plus the amount passed back. The second mover earns the amount kept in the second stage.

Rounds: There will be a number of rounds, with random rematchings in each one.

[24] These instructions are taken from Charles Holt's VeconLab website at the University of Virginia (http://veconlab.econ.virginia.edu/admin.htm)