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An Experimental Study of the Effects of Inequality and Relative Deprivation on Trusting Behavior

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Abstract

Several non-experimental studies report that income inequality and other forms of population-based heterogeneity reduce levels of trust in society. However, recent work by Glaeser et al. (2000) calls into question the reliability of widely used survey-based measures of trust. Specifically, survey responses regarding trust attitudes did not reflect subjects' actual behavior in a trust game. In this paper, we conduct a novel experimental test of the effects of inequality on trust and trustworthiness. Our experimental design induces inequality by varying the show-up fees paid to subjects, in contrast to previous experiments that focus on broad cultural or national differences in trust. We do not find robust support for the hypothesis that inequality per se dampens trusting behavior among all subjects; however, we do find some evidence that trust and trustworthiness are influenced by an individual's relative position in the group. Finally, we confirm previous findings that common survey-based measures of social trust are not associated with actual trusting behavior.

JEL Codes: C9, H4

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I. Introduction

The level of trust in society has long been recognized as an important economic resource (Arrow 1972); more recently, a plethora of empirical findings from across the social and behavioral sciences suggest that social trust has a beneficial impact on a wide-range of economic, political and social phenomena (e.g., Putnam 2000). This burgeoning literature has in turn inspired several studies of the determinants of trust. A repeated finding from national and cross-national surveys is that income inequality and other forms of population heterogeneity, especially race and ethnicity, are associated with lower levels of trust (e.g., Alesina and La Ferarra 2002). Theoretical explanations for a causal link between inequality and trust differ, but most are behavioral (e.g., Wilkinson 1996). While several recent experimental studies examine cultural or national differences in trusting and trustworthy behavior (e.g., Koford 2001), to date the experimental literature has not addressed the question of whether inequality itself affects trust. In addition, a recent study by Glaeser et al. (2000), which combines survey and experimental methods, raises serious doubts about whether the survey-based measures of trust employed in much of the non-experimental literature actually reflect trusting behavior.

We conduct a novel test of the hypothesis that group heterogeneity reduces trust by varying the show-up fees paid for participation in the experiment; this does not affect the equilibrium in the trust game, but it does generate a focal source of heterogeneity among our subjects. In addition, in some of our experiments, we award show-up fees in a public ceremony in order to heighten the salience of this source of inequality. We analyze the effects of this inequality treatment on both trusting and trustworthy behavior in the experimental setting. Further, in contrast to the previous literature on inequality and trust, we also investigate whether group-based measures of inequality

versus individual measures of relative standing are better predictors of subject behavior. Finally, like Glaeser et al., we also administer surveys to our subjects; this allows us to examine the association between actual trusting behavior in the lab and self-reported trusting attitudes and experiences.

Our subsequent analysis provides some weak evidence that group-based measures of induced inequality reduce both trust and trustworthiness, that is, until we control for relative standing of individuals via a "relative deprivation index." In fact, we find that relative deprivation is significantly associated with lower levels of trustworthiness; this is noteworthy, since the non-experimental literature on trust has ignored both trustworthiness and measures of relative standing. In addition, we find little support for claims that either attitudinal or behavioral-based survey measures of trust predict trusting or trustworthy behavior in our experiments. These findings accentuate the doubts raised by Glaeser et al. about the reliability of survey-based measures employed in most non-experimental studies of trust.

In Sections II and III we review the non-experimental literature on heterogeneity and trust and the relevant experimental literature, respectively. Section IV describes the experiment; our analysis and results are presented in Section V. We conclude with a discussion of our findings and suggestions for further research.

II. Non-Experimental Studies Linking Trust and Inequality

Several prominent authors argue that trust reduces the transaction costs of economic activity and facilitates the realization of collective goals (e.g., Arrow 1972, Coleman 1990, Fukuyama 1995 and Putnam 2000). Consistent with these claims, numerous empirical studies report that survey measures of social trust can be used to predict a host of phenomena, from economic growth (Knack

and Keefer 1997, and Zak and Knack 2001) and political corruption (La Porta et al. 1999) across countries, to population health (Kawachi et al. 1998) and crime (Kennedy et al. 1998) across U.S. states. In light of such findings, social scientists have begun to investigate the factors that produce differences in trust across countries, states, or individuals.

Several theories suggest that population heterogeneity reduces trust. For example, Alesina and La Ferarra (2002) posit a general aversion to heterogeneity among individuals that serves to increase the transaction costs of social experiences with members of other groups; likewise, Coleman (1990) and Barr (1999) argue that "familiarity breeds trust." Other explanations rely on the psychology of envy, or even evolutionary biology (e.g., see Wilkinson 1996 and Zak and Knack 2001). However, tests of these theories require that researchers have some meaningful way to quantify individual or group trust.

Empirical studies of trust typically make use of survey data from sources such as the General Social Survey (GSS), the World Values Survey (WVS) or the International Social Survey Program (ISSP). Survey respondents are classified as trusting or not according to their answers to questions about whether in general, "most people can be trusted." Aggregating these responses into measures of "generalized trust," several authors note a strong negative association between trust and income inequality across countries (e.g., Knack and Keefer 1997 and Knack 2003) and across U.S. states (e.g., Putnam 2000). Of course, such ecological analyses may not accurately reflect causal processes at the individual-level. However, more recent work that analyzes individual survey responses confirms this finding.

Both Alesina and La Ferrara (2002) and Costa and Kahn (2003) use the GSS to estimate models of individual trust as a function of individual attributes and metropolitan area characteristics,

while Leigh (2004) exploits WVS and ISSP data to estimate individual trust as a function of individual attributes and country-level characteristics. In each case, the probability that an individual responds that "most people can be trusted" is positively associated with own income and education, but negatively associated with income inequality. These same studies also suggest that sources of population heterogeneity, in addition to income inequality, reduce the propensity of individuals to express faith in the trustworthiness of others. Racial, ethnic, linguistic and religious fragmentation are also negatively associated with individual trust – at least in some specifications, and albeit not necessarily so when multiple measures of population heterogeneity are included simultaneously in these models. Nevertheless, there appears to be an emerging consensus that population heterogeneity in general (and income inequality in particular) reduces social trust.[2]

Despite the recent attention to the relationship between income inequality and trust, these studies are silent on two points. First, all of the studies cited above employ the gini coefficient as the only measure of income inequality. None of these studies explore whether it is in fact group-based measures of inequality that matter versus an individual's relative standing within an unequal community.[3] In other words, controlling for absolute income, does inequality have a differential effect on the trust of the haves versus the have nots? Second, if inequality influences one member's perceptions of the trustworthiness of others in their group, shouldn't inequality also have some effect on that individual's own trustworthiness? Yet, we know of no empirical evidence regarding the

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[2] One exception is Leigh's (2003) analysis of individual trust in Australia; he does not find that neighborhood inequality reduces trust. However, Leigh does not control for individual income, so this study is not comparable to those cited in the text.

[3] For example, two recent studies (Eibner and Evans (forthcoming) and Wagstaff and van Doorslaer 2000) attempt to distinguish between the consequences of group inequality versus relative deprivation on the health of individuals.

effects of income inequality on either self-reported trustworthiness or trustworthy behavior.

A very different sort of criticism of the trust literature is found in Glaeser et al. (2000); these authors integrate survey and experimental data to demonstrate that "generalized trust" is not significantly associated with trusting behavior in laboratory experiments. This disconnect calls into question much of the non-experimental evidence on inequality and trust, since the dependent variables in those studies may bear no relationship to actual trusting behavior. For this reason, we induce heterogeneity among our experimental subjects in order to conduct a novel test of whether inequality influences trusting *behavior* in the experimental lab.

III. Experimental Studies of Trust

Before describing our experimental design, we first provide a brief overview of the classic trust game, also known as the investment game. The first experiment to explicitly study trust was designed by Berg, Dickhaut and McCabe (hereafter, BDM 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.[4]

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

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

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.[5] Considerable attention 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. Trust experiments have been conducted in Belgium (Bouckaert and Dhaene 2004), Brazil (Csukas et al.2003), Bulgaria (Koford 2001), China (Croson and Buchan 1999 and Buchan and Croson 2004), France (Willinger et al. 2003), Germany (Fehr et al. 2003 and Willinger et al. 2003), Greece (Csukas et al. 2003), Hungary (Csukas et al. 2003), Israel (Fershtman and Gneezy 2001), the Netherlands (Bellemare and Kroger 2003), Russia (Gächter, Herrmann and Thöni 2003 and Ashraf, Bohnet and Piankov 2003, Csukas et al. 2003), South Africa (Ashraf, Bohnet and Piankov 2003), Tanzania (Danielson and Holm 2004), Turkey (Bouckaert and Dhaene 2002) and Zimbabwe (Barr 1999 and 2003). While two papers report significant differences in behavior across countries (Koford 2001 and Willinger et al. 2003), the majority of studies conclude that cultural differences do not significantly influence behavior in the trust game.[6]

We build on this literature by examining the effect of induced heterogeneity in a trust experiment. Heterogeneity that is induced in laboratory settings has been studied extensively in other experiments, most notably in public goods games.[7] No experimental study has looked at the effects of similarly-induced heterogeneity on trust behavior, despite the evidence that inequality

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

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

[7] See, for example, Anderson, Mellor, and Milyo (2004a, b).

affects survey-based measures of trust. Even in the rare cases in which cross-cultural differences have been identified, they may be the combined result of political, social, and economic factors.

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) finds a small negative, but statistically insignificant, effect on amount sent 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) conduct 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 (2002) conduct a similar experiment using businessman of Turkish or Belgian origin, but they report no evidence of ethnic differences in the amount sent or returned. Finally, Willinger et al. (2003) pair French and German students and find no difference in behavior when subjects knew they were playing with someone from a country other than their own. Thus, half of these studies provide evidence that 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 income measure, and by inducing heterogeneity within a controlled laboratory setting.

IV. 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 and 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.[8] Roles remained constant throughout the experimental session but subjects were randomly re-paired at the beginning of each new round.

Heterogeneity was introduced to this setup through the use of three different show-up payment distributions as described in Table 1. Each distribution was applied for a set of 10 rounds in the game. Giving subjects a fixed 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. In two cases, which we refer to as "skewed" and "symmetric," show-up payments varied across participants; in the "egalitarian" treatment, all subjects received an identical payment. All distributions had an average payment of $7.50.

In Table 1, the "type of inequality" treatment refers to the manner in which fixed payments were assigned to participants. In the private treatment, all subjects were told the distribution of fixed payments, but were privately given a card with their specific fixed payment written on it. In the public treatment, the fixed payments were awarded in a "ceremony." When the payments varied, each subject's name was recorded on a card and placed in a container. All subjects watched as we drew one name from the container and awarded that person the highest fixed payment. The remaining fixed payments were awarded in a similar manner, starting with the second highest and

[8] 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.

finishing with the lowest. This type of ceremony is similar to one used by Ball et al. (2001), which examined status effects on market interaction.

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 adopt this approach as well. In some studies subjects are surveyed prior to the experiment and in other studies, at the conclusion of the session. The advantage of conducting the survey first is that an individual's survey responses will not be influenced by her experiences in the experiment. A disadvantage is that survey responses might influence behavior in the experiment. For example, a subject who strongly agrees with the statement "I am trustworthy" might feel compelled to return a large amount of money as a second mover. In comparing correlations between behavior and survey responses across both types of paired studies, we were unable to identify any obvious order effect, but as we later discuss, these studies differ in multiple dimensions. Since our primary focus is on how heterogeneity affects behavior in the trust experiment, we conducted the survey at the end of the experiment. Thus, any biases caused by the pairing would be less likely to affect actual behavior, rather than survey responses, which are only one feature of our analysis. We describe the survey questions in more detail later in the paper.

V. Results

The results from the experiment are presented below in three parts. First, we discuss our empirical examination of trust, as indicated by first-mover decisions regarding the amount sent. This is followed by an analysis of trustworthiness, measured by the ratio of the amount returned to the amount available to return by second movers. These analyses employ descriptive statistics and a

series of regression models that control for the repeated nature of the game and subject fixed effects. In a third section, we turn our attention to assessing the relationship between both of these subject behaviors and the trusting attitudes and experiences elicited by our survey.

A. Inequality and Trust

Across all twelve sessions and 30 decision-making periods, first movers sent an average of $4.97 (out of $10) to their randomly-matched partners. The mean amount sent is somewhat lower in the sessions in which the fixed payments were distributed in a less public manner, at $4.41, compared to an average of $5.53 in sessions that used a public ceremony to award fixed payments. A rank-sum test reveals that the difference in means is not significant at conventional levels ($p=0.152$), however, in the skewed treatment there is a significant difference in the amount sent in public versus private inequality ($p=0.05$). We adopt the use of separate samples in our subsequent analysis to identify whether the public nature of the award ceremony influenced the effects of other experimental conditions on behavior. For ease of discussion, we use the terms "private" and "public" to reference the separate samples.

In Table 2, we present mean amounts sent by the type of inequality treatment induced in the 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. This is consistent with survey-based evidence that heterogeneity reduces the level of trust. However, these differences are not significant according to Wilcoxon rank-sum tests, and a similar pattern is not found in the public sessions. Table 2 also reports mean amount sent by the subject's fixed payment within the inequality treatments. In both private and public sessions, the amount sent is largest among subjects who received the median payment in the symmetric distributions. In the

skewed distributions, the highest paid subjects sent relatively less than other participants in the private sessions, but relatively more in the public sessions.

To test the separate effects of both the fixed payment level and its distribution on subject behavior, we employ multivariate regressions on separate samples from the private and public sessions. For each sample, we start with 720 observations of amount sent, based on six sessions with four first movers per session and 30 decisions per subject. In our basic regression model, we define the dependent variable as the amount sent in each round, and we use subject fixed effects to allow for unobserved subject-specific differences in behavior. Following Cochard, Van, and Willinger (2004) who also model output from a repeated trust game, we control for the fraction returned by the second mover in the previous round.[9] For cases in which the first mover sent nothing in the previous round, we code the fraction returned in the previous round as 0 and include a dummy variable to pick up this effect. This requires omitting the initial round of play for all subjects, and results in two samples of 696 observations. We include a set of 28 dummy variables to control for the round of play, with the second round serving as the omitted category. To this set of explanatory variables, we sequentially add measures of the fixed payment and the type of inequality.

Table 3 reports the results of several fixed effects models of amount sent.[10] As shown in the

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

[10] Since the dependent variable ranges from 0 to 10, some econometric analyses of trust games have employed Tobit models to address right- and left-censoring. We estimated a series of random effects Tobit models using STATA software, and as recommended, also checked the sensitivity of the quadrature approximation used in the random-effects estimators. The results of these checks suggested that the random-effects Tobit should not be employed with our data set, possibly due to the degree of correlation within subject observations. Fixed effects can not be easily introduced to the Tobit model, and GLS models with random effects were not appropriate as indicated by Hausman tests. For these reasons, we use fixed effects models.

first two rows of the table, the amount sent in a given round increases with the fraction returned from the matched partner in the previous round, and decreases when subjects sent nothing in the previous round. Although not shown here, the coefficients of the round dummies are largely insignificant individually, but are jointly significant at the 0.01 level or better in each model.

Before discussing the effects of the inequality treatments, we first examine the effect of the fixed payment itself on trust behavior. As shown in columns (1) - (3), larger payments reduce the amount sent in the private sample; however, data from the public sessions in columns (4) - (6) report the opposite relationship. In the survey-based literature on trust, several studies report that trusting attitudes are positively associated with income and years of schooling among individuals (e.g., Alesina and La Ferrara 2002), and GDP across countries (e.g., Zak and Knack 2001). Thus, our public sample findings are consistent with this type of income effect. Our finding in the private sample is more consistent with an entitlement effect as described in Fahr and Irlenbusch (2000), who find that when first movers performed a task to earn their role in the trust game they were more likely to send nothing to their second-mover partners. Perhaps when high payments are received privately, the sense of entitlement reduces what subjects send to their partners. Alternatively, the public revelation of a high fixed payment might "shame" the recipients into sending more.

To the base model, we next add controls for the presence of inequality; results are reported in columns (2) and (4). Dummy variables are used to indicate when decisions were made during a skewed or symmetric fixed payment distribution; the omitted category is when all subjects in the group received an equal payment. In the private sample, we see some indication that heterogeneity introduced in the experiment had a dampening effect on trust behavior: the amount sent is significantly lower in both symmetric and skewed distributions relative to egalitarian. However, this

pattern is not observed in the public sessions, where respondents in the skewed treatment sent significantly more than those in the egalitarian treatment.

We next seek to determine whether these significant effects of inequality are the result of heterogeneity *per se*, versus the subject's relative position in the group. Previous studies of individual trust report that group-based measures of heterogeneity, such as income inequality, racial fragmentation, and linguistic fractionalization are associated with lower levels of trust among individuals (e.g., Alesina and La Ferrara 2002, and Leigh 2004). It is also possible that heterogeneity affects trust for certain group members more so than others. For example, Leigh (2003) examines the effects of inequality separately in samples of rich and poor individuals, but reports that inequality has similar effects on trust in both groups.

We test whether the effect of inequality is due to differences in relative ranking, and whether those at the bottom of the distribution are most affected by those differences. This distinction is of particular relevance to the trust game, where individual payoffs are based on decisions made between one person and her partner, as opposed to the group's actions overall. For this reason, we add a subject-specific measure of relative income to the model, the relative deprivation index (RDI).[11] 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

[11] Following Deaton (2001), we calculate this index for person i as:

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

where x_i is the fixed payment for individual i, $[1 - F(x_i)]$ is the proportion of the subjects in the session 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 session.

(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).

When we include this measure in the model in the private sample (column 3), we find that the coefficients on the inequality treatment variables are no longer significant, whereas the RDI has a large negative coefficient. This suggests that the more deprived members of the group were less trusting, however, this effect is not statistically significant.[12] Moreover, in the public sample, none of the coefficients for either inequality treatment or the individual's relative placement is statistically significant in column (6).

In summary, our analysis of amount sent suggests that the presence of heterogeneity in the fixed payments had a statistically significant negative effect on trust in some sessions, specifically when fixed payment awards were made privately. When we added controls to the model for the subject's relative position in the distribution, we found that the inequality effect was driven by subjects who received low payments relative to their group members. These subjects were less trusting, but this effect was not statistically significant. These findings add to a number of previous studies on the determinants of trust by highlighting the contribution of heterogeneity, and identifying the importance of the individual's relative position in the group. We now turn to the data on second-mover behavior.

B. Inequality and Trustworthiness

The decision of interest for second movers in our experiment is the return ratio, defined as

[12] In a variation of model (3) that does not include the two distribution dummies as explanatory variables, the coefficient on RDI is negative and significant at the .01 level. However, an F-test on model (3) shows that the distribution dummies are jointly significant at the 0.01 level.

the amount returned divided by three times the amount sent. For the combined twelve sessions, the return ratio has a mean of 0.355; the mean is slightly higher in the public sessions at 0.377 compared to 0.330 in the private sessions.[13] Mean values of return ratio by inequality treatment and fixed payment are shown in Table 4. Examining values in the bottom row, there does not appear to be a consistent relationship between the type of inequality and mean levels of trustworthiness. In the private sessions, return ratio is greatest in the symmetric treatment, and in the public sessions, it is largest (but barely so) in the egalitarian treatment.

As we did for the analysis of amount sent, we also estimate a series of multivariate regression models to separately identify the effects of various aspects of the experimental design. We use fixed effects regressions of the return ratio on the amount sent and a set of round dummy variables. We then add controls sequentially for the fixed payment, distribution dummies, and the RDI. The results are presented in Table 5; because we do not observe a value for return ratio when nothing was sent by a first mover, these samples are smaller than those used to estimate the models of amount sent.

One explanatory variable consistently and significantly associated with the return ratio is the amount sent by the first mover. In all models, receiving a larger amount from the first mover leads to an increase in the return ratio. In every case, the fixed payment has a negative effect on the return ratio, but in only two cases is the effect significant. While none of the inequality variables is significant in the private sessions, column (5) reports a negative and significant coefficient for the symmetric distribution dummy in the public sessions. When we subsequently control for the subject's relative payment in column (6), we find a negative and significant effect for the subject-

[13] The difference in means is not significant, but mirroring our analysis of trust we examine the samples separately. This also allows the effects of inequality and other controls to differ across the two samples.

specific inequality, but no negative effect for either group inequality measure. This suggests that subjects who receive relatively small fixed payments, all else equal, return less to their partners, or, in other words, are less trustworthy. In column (6), the skewed distribution dummy is instead positive and significant. Recall that in the skewed session, first movers with the highest payments sent significantly more to their partners, an effect that could lead second movers in the skewed treatment to reciprocate with larger percentage returns.

In summary, the results from models of return ratio provide some limited evidence that heterogeneity in the subjects' payments reduced trustworthiness. Specifically, in the public sessions we found a significant negative effect for the symmetric inequality treatment variable. However, when we added the RDI variable to the model, the effects of group-level inequality were no longer negative, and instead we found that the subjects with relatively low fixed payments returned less to their partners. This evidence adds to existing findings from survey-based studies of heterogeneity and trust; to date such studies have not examined heterogeneity as a determinant of trustworthiness.

C. Analysis of Trust Survey Questions

As we noted earlier, subjects completed a survey at the end of each experimental session. The survey included a number of questions designed to elicit trusting attitudes and behaviors, and several recent papers have paired these types of questions with trust experiments to gauge their validity. The first study to do this, Glaeser et al. (2000), produced several interesting findings. First, questions regarding subjects' general trust in others, and about the perceived helpfulness or fairness of others (so-called attitudinal questions) were found to have no significant effect on trust behavior.[14] In contrast, survey questions about past trusting acts (like leaving doors unlocked and loaning money

[14] Specifically, of 12 attitudinal questions used by Glaeser et al. only two had significant effects on trust behavior, and both of these questions pertained to trust in strangers, a narrowly-defined group.

to others) were significantly associated with trusting behavior in the experiment. Another reported finding from the Glaeser et al. study is that attitudinal measures of trust were significantly associated with trustworthiness (measured by the return ratio).

Subsequent studies that have paired the trust game with a survey have produced mixed results.[15] For example, both Ashraf, Bohnet and Piankov (2003) and Danielson and Holm (2004) were able to replicate the Glaeser et al. finding that generalized trust was not significantly associated with amount sent. In contrast, Bellemare and Kroger (2004) found that generalized trust had a positive and significant association with trusting behavior. As for the use of questions about past behaviors, Danielson and Holm (2004) found that a trusting behavior index had no effect on the amount sent, a result that differs from Glaeser et al. Thus, there is no consensus about which survey questions best predict trusting behavior. However, one finding that has been reproduced in most subsequent work is that questions about trust predict trustworthiness. Five of these seven paired studies have reported that variables measuring generalized trust were positively and significantly related to the percent returned by the second mover.[16]

The mixed nature of results from paired studies is perhaps not surprising given the variation in the study designs. Some studies conduct the survey before the experiment, others after, and one study simultaneously conducted the survey and the experiment. The experimental designs vary in

[15] The paired studies include Ashraf, Bohnet, and Piankov (2003), Bellemare and Kroger (2003), Burks, Carpenter, and Verhoogan (2003), Csukas et al. (2003), Danielson and Holm (2004), and Fehr et al. (2003).

[16] Both Burks, Carpenter, and Verhoogan (2003) and Fehr et al. (2003) reported that several measures based on generalized trust did not have significant associations with the second mover's transfer. However an important difference between these studies and the others is that models of second-mover transfers included several different measures of trust attitudes. It is possible that the degree of correlation between these measures led to multicollinearity and insignificant coefficients.

other important ways, such as the number of decisions made (one-shot versus repeated) and the method for assessing second-mover responses.[17] Like trust experiments in general, these paired studies also have considerable variation across subject pools, with a mix of student and non-student subjects from Brazil, Germany, Greece, Holland, Hungary, Russia, South Africa, Tanzania, and the United States. Studies also vary greatly in terms of the use and wording of specific survey questions, and the way in which they were entered in the model (ie., as dummy variables or indexes). For these reasons, direct comparisons are difficult. Nonetheless, the debate continues about the importance of these pre-existing attitudes or tendencies in subject decisions.

From a series of common questions eliciting attitudes and experiences involving trust, we constructed eleven variables including several measures of general trust (e.g., agreeing that most people can be trusted) and several indicators of trusting behaviors (e.g., leaving doors unlocked). Table 6 lists the eleven trust variables and their means in samples of first and second movers. In the first half of the table, we include each of these measures in a separate regression of amount sent with controls for round dummies, whether nothing was sent in the last round, what fraction was returned in the last round, as well as subject race and gender. Because the trust survey variables were fixed for each respondent, we estimated OLS models with clustered standard errors in lieu of fixed effects. Separate columns report regression coefficients for the relevant trust variable in the private and public samples. In all but three cases, we find no significant relationship between trust responses in the survey and trusting behavior in our experiment. One of the three significant cases is incorrectly-signed: self-reported trustworthiness is associated with lower amounts sent in the public

[17] In some cases, second movers responded to the actual amount send by their first-mover partners. In others, the "strategy method" was used. This required second movers to report return amounts for every possible amount that a first mover might send.

sessions. Only the variables measuring perceived fairness and not having a loan repaid have significant effects in the expected direction.

We examine the association of trust responses with trustworthiness, or the fraction returned by second movers, in the second half of Table 6. These regressions include controls for round dummies, the amount sent, and the race and gender of the subject. Here, we find only one instance where a coefficient is statistically significant, and in that case, the direction of the effect is opposite the expected sign. However, in spite of their statistical insignificance, the majority of the coefficients in Table 6 do have the expected sign (29 of 43 models).

D. Sensitivity Tests

Recall that in our experiment, subjects made ten decisions in each of three different inequality treatments, skewed, symmetric, and egalitarian. This feature allows us to use within-subject variation to identify the effects of inequality on behavior, and to attribute any observed effect to the inequality treatments, as opposed to unobservable characteristics of the subjects who participated in the particular treatments. However, it is possible that subject experiences under one inequality treatment may have had persistent effects on subject behavior under subsequent treatments. For this reason, we also conduct separate analyses using decisions from the first ten rounds of the experiment during which subjects experienced only the initial distribution of show-up fees. We estimated the models in Tables 3 and 5 using approximately one-third of the observations, and using clustered-OLS models in lieu of subject fixed effects (since the inequality variables were fixed over the subjects' first ten decisions). The results of this exercise conform with our full-sample findings, in that we do not find robust support for the hypothesis that inequality *per se* dampens either type of trusting behavior, and we find some evidence that behavior is influenced by the

subject's relative deprivation. In addition, the survey-based measures of social trust are generally not significantly associated with actual trusting behavior.

Finally, we checked the sensitivity of our results to an alternate measure of the subject's relative ranking in the group, defined as the subject's payment divided by the maximum payment within the distribution. The basic pattern of results shown in the full models reported in Tables 3 and 5 is unchanged. For example, controlling for relative payment attenuates the negative effect of inequality on amount sent in the private sessions, and subjects who received low payments relative to the maximum were less trustworthy in the public sessions.

VI. Conclusion

There is a growing non-experimental literature linking income inequality and other forms of heterogeneity to lower levels of trust. However, this literature 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 has a significant effect on trusting behavior, but the effect is negative only when fixed payments are awarded privately. This negative association is largely attenuated once we control for relative deprivation. In addition, when we examine second-mover behavior, salient inequality is weakly and *positively* associated with trustworthiness, while relative deprivation has a strong depressing influence on trustworthiness. These findings strongly suggest that future research should investigate the role of relative standing in determining both trust and trustworthiness.

These results contrast somewhat with our earlier work (Anderson, Mellor and Milyo 2004a)

which examines the effect of group inequality on contributions in a public goods experiment. In that study, the effect of inequality was robust to the inclusion of relative deprivation as a control variable. We suspect that the group nature of the public goods game, in contrast to the bilateral trust game, makes group attributes more important in determining behavior. However, in both experimental settings, inequality seems to have a more pronounced effect in the public treatment; this suggests that in order to induce heterogeneity in the lab, researchers should take pains to ensure that subjects find such differences to be salient. Nevertheless, the differential effect of inequality across these experimental games also merits further study.

Several additional findings in our trust experiments should be noted. First, we observe several differences in subject behavior in the private versus public information treatments. For example, public information on the identities of individual "haves" and "have-nots" is associated with both an increase in amounts sent (i.e., trust) and the fraction returned (i.e., trustworthiness); however, the size of the fixed payment is negatively associated with amounts sent on the private sessions and positively associated in the public sessions. Future work should explore to what extent the public nature of these awards influences risk-taking and inequality aversion. Further, relative deprivation appears to be a significant determinant of trustworthiness, but not trust. This is consistent with other studies that uncover systematic differences in the determinants of trust versus trustworthy behavior in similar experiments.

Our final set of results is perhaps the most troubling for the extant literature on the causes and consequences of social trust. We confirm Glaeser et al.'s finding that the most common attitudinal measures of social trust (e.g., "most people can be trusted," etc.) are not strongly associated with trusting or trustworthy behavior by our experimental subjects. However, like those

authors, we do observe that attitudinal measures of trust tend to be positively correlated with trustworthiness. However, in contrast to Glaeser et al., we do not find that behavioral-based survey measures of trust (e.g., "do you lock your door?") fare any better in predicting trust or trustworthiness. Once again, these findings are in contrast to our public goods experiments (Anderson, Mellor, Milyo 2004b); in those experiments, we find that both attitudinal and behavioral survey measures are positively associated with contributions. Nevertheless, given the extent to which non-experimental studies of trust rely on survey-based measures of social trust, more attention needs to be devoted to developing more reliable survey instruments for eliciting information on trust.

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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 Amount Sent, By Inequality Treatment

Fixed Payment	Private			Public		
	Egalitarian	Symmetric	Skewed	Egalitarian	Symmetric	Skewed
$4			4.34			4.77
$5		3.63			3.60	
$7			4.18			6.41
$7.50	4.99	4.74		5.41	7.25	
$10		4.01			5.59	
$20			3.80			7.14
All	4.99	4.04	4.19	5.41	5.27	5.92

Table 3. Results of Fixed Effects Models of Amount Sent, With Inequality Treatments

	Private *(n=696)*			**Public** *(n=696)*		
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction Returned Last Round	2.881*** (6.51)	2.880*** (6.55)	2.886*** (6.57)	3.226*** (7.27)	3.200*** (7.22)	3.194*** (7.21)
Sent Zero Last Round	-1.257*** (3.86)	-1.204*** (3.73)	-1.189*** (3.68)	-1.222*** (3.24)	-1.199*** (3.19)	-1.191** (3.17)
Fixed Payment	-0.092* (2.26)	-0.085* (2.10)	-0.173* (2.38)	0.082* (2.39)	0.0769* (2.25)	0.121* (2.01)
Symmetric Distribution		-0.869*** (3.84)	-0.456 (1.26)		-0.104 (0.48)	-0.315 (0.98)
Skewed Distribution		-0.525* (2.31)	0.277 (0.46)		0.417 † (1.91)	-0.006 (0.01)
RDI			-2.482 (1.45)			1.269 (0.89)

Notes: Absolute values of t-statistics reported in parentheses. All models also include controls for round of play. 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.

Table 4. Mean Return Ratio, By Inequality Treatment

Fixed Payment	Private			Public		
	Egalitarian	Symmetric	Skewed	Egalitarian	Symmetric	Skewed
$4			0.297			0.361
$5		0.368			0.382	
$7			0.337			0.417
$7.50	0.335	0.277		0.388	0.276	
$10		0.395			0.410	
$20			0.234			0.279
All	0.335	0.345	0.310	0.388	0.361	0.383

Table 5. Results of Fixed Effects Models of Return Ratio, With Inequality Treatments

	Private *(n=551)*			**Public** *(n=611)*		
	(1)	(2)	(3)	(4)	(5)	(6)
Amount Sent	0.010**	0.010***	0.010***	0.007**	0.007**	0.007**
	(3.18)	(3.32)	(3.32)	(2.59)	(2.67)	(2.68)
Fixed Payment	-0.002	-0.002	-0.001	-0.005	-0.005†	-0.020***
	(0.61)	(0.52)	(0.11)	(1.58)	(1.71)	(3.73)
Symmetric Distribution		0.022	0.017		-0.028†	0.034
		(1.07)	(0.49)		(1.87)	(1.42)
Skewed Distribution		-0.018	-0.026		-0.019	0.087*
		(0.85)	(0.50)		(1.28)	(2.48)
RDI			0.025			-0.340***
			(0.17)			(3.35)

Notes: Absolute values of t-statistics reported in parentheses. All models also include controls for round of play. 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.

Table 6. Models of Amount Sent and Return Ratio with Trust Survey Responses

Explanatory variable based on subject's survey response that he or she:	**Amount Sent Models**			**Return Ratio Models**		
	Mean (Std. Dev.)	**Private**	**Public**	**Mean (Std. Dev.)**	**Private**	**Public**
		β-hat	β-hat		β-hat	β-hat
Agrees that "most people can be trusted"	0.229 (0.425)	0.447 (0.49)	0.928 (0.57)	0.354 (0.483)	0.019 (0.31)	0.103 (1.25)
Agrees that "most people try to be fair"	0.292 (0.459)	1.591* (2.60)	-0.099 (0.09)	0.292 (0.459)	0.049 (0.68)	0.121 (1.63)
Agrees that "most people try to be helpful"	0.375 (0.489)	0.649 (1.01)	-1.140 (1.09)	0.333 (0.476)	-0.017 (0.22)	0.089 (1.19)
Strongly agrees/agrees that "you can't trust strangers anymore"	0.604 (0.494)	0.840 (1.23)	-0.045 (0.04)	0.333 (0.476)	-0.041 (0.48)	-0.085 (1.06)
Strongly agrees/agrees that "I am trustworthy"	0.938 (0.245)	0.054 (0.08)	-2.628** (3.59)	0.917 (0.279)	–	-0.071 (0.62)
Often leaves door unlocked	0.292 (0.459)	0.596 (0.81)	1.121 (1.00)	0.438 (0.501)	0.009 (0.13)	0.036 (0.45)
Has loaned money to strangers	0.25 (0.438)	-0.420 (0.83)	-0.430 (0.45)	0.313 (0.468)	-0.096† (1.68)	0.016 (0.25)
Often loans money or possessions to friends	0.458 (0.504)	-0.237 (0.33)	0.978 (0.96)	0.625 (0.489)	0.007 (0.07)	0.056 (0.49)
Has loaned money to someone who failed to repay	0.813 (0.394)	-0.088 (0.13)	-1.846* (2.49)	0.813 (0.394)	0.084 (0.69)	-0.069 (0.62)
Has been victim of a crime	0.458 (0.504)	-0.068 (0.11)	0.912 (0.94)	0.375 (0.489)	-0.040 (0.61)	-0.060 (0.93)
Never lies to either parents, friends, roommates, or other acquaintances	0.542 (0.504)	0.484 (0.69)	0.478 (0.48)	0.532 (0.504)	-0.096 (1.34)	-0.060 (0.72)

Notes: Each survey-based explanatory variable was used in a separate OLS model that also included controls for subject race (nonwhite) and gender (female), round of play. Standard errors were clustered by subject. In the amount sent models we also controlled for the fraction return last round, and whether nothing was sent last round. In the return ratio models we also controlled for the amount sent. Absolute values of t-statistics are reported in parentheses. Means and standard deviations of survey responses are based on a sample of subjects acting as either first or second movers in all sessions. Amount sent regressions are based on samples of 696 observations, except for the regression reported in the last row, where a missing response reduced the public session sample to 667 observations. In the return ratio regressions, the sample sizes of the private session were each 551 observations; in the public session samples included 611 observations, except for the regression reported in the last row, where a non- response reduced the sample to 582 observations. In the case of the trustworthy variable, no coefficient is reported for the private sessions because the variable took a value of 1 for all observations in that sample.

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.[18] 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.

[18] 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[19]

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.

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