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The Dynamic Interplay of Inequality and Trust - An Experimental Study

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Abstract

We study the interplay of inequality and trust in a dynamic game, where trust increases efficiency and thus allows higher growth of the experimental economy in the future. We find that trust is initially high in a treatment starting with equal endowments, but decreases over time. In a treatment with unequal endowments, trust is initially lower yet remains relatively stable. The difference seems partly due to the fact that equal start positions increase subjects' inclination to condition their trust decisions on wealth comparisons, whereas conditional trust is much less prevalent with unequal initial endowments. As a result, with respect to efficiency, the initially more unequal economy fares worse in the short run but better in the long run, and the disparity of wealth distributions across economies mitigates over time.

Keywords: inequality, trust, growth, laboratory experiments

JEL Classification: C73, C92, D63, E25, O15

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I Introduction

Trust makes economic agents more willing to engage in interactions involving the risk of being deceived. Thus, serving as a "lubricant", trust may positively influence efficiency and economic growth, and at the same time affect the distribution of wealth within an economy (see Section II for related literature). However, trust is difficult to measure both on the microeconomic and the macroeconomic level. Survey data frequently discover individual attitudes towards trust, but cannot easily identify to what extent such self-reported attitudes reflect actual economic behavior, and how trust interacts with the dynamics of efficiency and distribution. Furthermore, the causal relationship between trust and economic variables is not always clear, as argued by Durlauf (2002), who thus advocates the use of laboratory studies. This paper follows Durlauf's advice. It complements the empirical and survey literature on the relationship between inequality and trust with the help of experiments, which systematically investigate the dynamic interplay of trust, efficiency and distribution.

The working horse of our experiment is a variant of the trust game introduced by Berg, Dickhaut and McCabe (1995). In this game, a trustor can send an amount of money to an anonymous trustee. Before received by the trustee, the amount sent is multiplied by a factor greater than one, and thus yields efficiency gains. Subsequently, the trustee decides on how much of the amount received she sends back to the trustor. The amount sent can be interpreted as a measure of trust, while the amount returned measures the degree of trustworthiness.[1] In our experiment, participants start with an unequal or equal distribution of initial endowments within a group. In each of several rounds they play a trust game with a new anonymous partner. Round payoffs are immediately added to endowments, and therefore determine the amount that can be exchanged in future rounds. That is, trust and trustworthiness jointly affect the current and potential

[1] The game is sometimes called 'investment game', and the amount sent is interpreted as a measure for investment in risky projects. In this paper both interpretations fit equally well.

future growth rates of the economy, as well as the evolution of economic inequality.[2]

In the experiments, we observe that initial trust levels are lower in a treatment starting with unequal endowments (IEQ) compared to a treatment with equal endowments (EQ). However, in EQ trust behavior is strongly conditioned on wealth information, while this is not the case in IEQ. We suspect that this is partly due to the fact that, in IEQ, subjects cannot easily distinguish between exogenously and endogenously created wealth differences. As a result, investment rates decrease steadily and strongly over time in the initially equal experimental economies, yet remain rather stable in the treatment starting with unequal endowments. The wealth distributions across treatments converge to each other. However, wealth inequality in treatment EQ rises faster than implied by the randomness of market interactions, while income shifts towards the relative poor in treatment IEQ seem not based on deliberate distributional concerns.

In Section II we review the literature related to our experiment. Section III specifies the details of our experimental design and procedures, and sketches hypotheses based on previous empirical results and economic models. Our experimental data and statistical analysis are presented in Section IV. We discuss our results and conclude in Section V.

II Related Literature

There is a broad theoretical and empirical literature dealing with the interplay of inequality with economic growth and prosperity. The academic discussion started in the 1950s with the Kuznets-Curve (Kuznets, 1955), which proposed a relation between inequality and economic development in the form of an inverted U. More recently, it is predominantly assumed that the relation between inequality and growth is a negative one. Examples of the theoretical literature include the models of Galor and Zeira (1993), Persson and Tabellini (1994), and, surveying the differing strains of literature,

[2] Given that investments yield constant positive returns, the dynamic game allows initially rich subjects to increase their endowments much more in absolute terms than initially poor subjects.

Ros (2000) and Glaeser (2005). Bénabou (1996) gives an overview about a number of empirical studies, the majority of which find a negative link between income disparity and growth.[3]

Some authors have argued that trust is the key for understanding the negative relationship between inequality and economic prosperity. Inequality decreases the level of trust and trustworthiness in a society, which in turn negatively affects growth. Empirical evidence is provided by Knack and Keefer (1997) and Zak and Knack (2001), who found that countries with higher income dispersion (measured by the Gini coefficient for income) exhibit significantly lower values for a trust measure derived from the World Value Surveys (WVS).[4] Similarly, Alesina and Ferrara (2002) find a negative connection between social distance and trusting behavior in a study restricted to the United States. Furthermore, some empirical studies could establish a positive impact of generalized trust on economic development (Knack and Keefer, 1997; La Porta, Lopez-de-Silanes, Shleifer and Vishny, 1997; Zak and Knack, 2001).

Other authors see social preferences, specifically concerns for social status,as the relevant link between inequality and economic development, as these might discourage both poor and rich subjects to accumulate income in an unequal society and lower the political will for redistribution (Corneo and Grüner, 2000; Corneo and Jeanne, 2001).

Durlauf (2002), however, notes that there are various problems of causality and identification in many of these empirical studies on the relationship between social capital, trust and economic indexes. He thus proposes the use of laboratory experiments to investigate the causal structure between these measures. Results from such economic experiments allow to build models of individual behavior to explain the relationship between social capital and

[3]However, there are also some studies, such as Forbes (2000), which question this view and suggest a positive relation instead.

[4]The World Values Surveys are repeated interview studies with representative population samples on the changes in moral values and beliefs, conducted in 80 countries all over the world since 1981. One question is: "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?". The percentage of positive responses is used as a measure of generalized trust in a country.

economic measures on the aggregate (see, for example, the model of Glaeser, Laibson and Sacerdote, 2002).

There is experimental evidence on the relationship of cooperation and inequality in public goods games, which share a couple of features with the trust game studied here. The evidence is, however, mixed. In a survey on repeated public goods games with complete information, Ledyard (1995) comes to the conclusion that economic heterogeneity among subjects generally lowers cooperation levels. Chan, Mestelman, Moir and Muller (1996) find that poor subjects contribute more to a public good than rich subjects. Buckley and Croson (2006) conduct a linear public good game with heterogenous endowments of the subjects. In their study, rich and poor subjects contribute on average the same absolute amount to a public good. Thus, as poor subjects contribute a higher share of their respective endowments, economic inequality increases within the experimental groups.

Other studies are devoted to the relationship between social distance (measured on various scales) and investment behavior in the trust game introduced by Berg et al. (1995). Glaeser, Laibson, Scheinkman and Soutter (2000) combined questionnaires on social backgrounds and trust attitudes with an experimental trust game. In their experiment, subjects interacting face-to-face with a partner of a different race or nationality exhibited a lower level of trustworthiness. In addition, a higher social status of the sender seems to be positively related to the earnings of a trusting decision. Hence, the results of this study indicate detrimental effects of social distance. However, survey measures of generalized trust were not found to be correlated with actual trusting behavior. Fershtman and Gneezy (2001) find significantly different degrees of trust towards different ethnical groups in the Israeli-Jewish society, although these groups did not differ concerning their trustworthiness. In a recent study, Haile, Sadrieh and Verbon (2006) conducted a trust game experiment with South-African students. They found negative effects of socio-economic differences, as low-income subjects trusted less when confronted with a high-income transaction partner from another ethnic group.

To our knowledge, there are only two experiments which specifically study the role of payoff inequality in the trust game. Contrary to the studies discussed above, social distance is induced by the experimental design. Brülhart and Usunier (2007) varied endowments of the trustees, which however did not affect trust. Anderson, Mellor and Milyo (2006) employed an equal as well as a symmetric and a skewed unequal distribution of show-up fees in a trust game. The distribution of show-ups was either private or public information. The authors observe only small and inconsistent effects of unequal endowments on trusting behavior.

III Experimental design and hypotheses

In our study, we focus on the dynamic interaction of trust and inequality. Therefore, we added a couple of modifications to the original trust game introduced by Berg et al. (1995). First of all, the game is played over 20 rounds. In each round, two randomly and anonymously matched subjects play the trust game. One of the subjects is randomly assigned the role of the trustor, the other the role of a trustee. Before decisions are made, each subject is informed about his own and the opponent's wealth in the current round. Wealth is defined as the initial endowment plus any payoffs that have been accumulated in earlier rounds. A player's wealth limits the amounts that he can send or return in the current round of the dynamic trust game in the following way. The trustor decides on an amount S, which is not allowed to exceed his current wealth, to be sent to the trustee. Any amount sent is multiplied by the factor 1.2, i.e. the trustee receives $1.2S$. Next, the trustee can decide on the amount R to be sent back to the trustor. The minimum amount to be returned is $0.9S$, or 90% of the amount sent.[5] The upper limit is given by the sum of the current wealth of the trustee plus the received amount. Because payoffs are accumulated during the course of the repeated trust game, our experimental economies could maximally grow by an expected factor of 6.2.[6]

[5]These restrictions make the one-round interaction in our game equivalent to the original trust game interaction with a sent amount multiplier of three, with the exception that the amount that can be sent is restricted to 10% of the trustor's wealth.

[6]As in each round only half of the subjects in the economy are randomly assigned to the role of the trustor, the expected maximum growth rate over 20 rounds with full

We varied the distribution of the initial endowments across our two treatments. Under the equality condition (EQ), all subjects were endowed with an amount of 500 ET (Experiment Talers) before the first round. In the inequality treatment (IEQ), half of the subjects in each matching group received 200 ET, and the other half received 800 ET.

The experimental sessions took place in May 2005 in the Cologne Laboratory for Economic Research. We conducted four sessions, two for each of our treatments. To allow for experience and to test robustness of behavior, after the first 20 rounds of the experiment we restarted the game for another 20 rounds. Subjects were told before the session that the experiment consisted of several runs, one of which would be randomly selected for payoff.

Subjects were recruited using the Online Recruitment System by Greiner (2004). Altogether 128 student subjects participated, most of them with a major in Economics, Business Administration or related fields. Each session consisted of 32 participants. Random matching per round was restricted to groups of 8 participants.[7] It was publicly known that two subjects would never interact with each other in consecutive rounds. Due to this procedure, we collected observations on 8 statistically independent 'economies' for each treatment. Overall, we collected 2,560 choices for each player role.

The experiment was computerized using the zTree software (Fischbacher, 2007). After subjects arrived and were randomly assigned to a cubicle, instructions were distributed.[8] Questions were answered privately. At the end of the experiment subjects filled in a post-experimental questionnaire asking for demographical data and containing open questions for motivations of subjects' decisions. Finally, either run 1 or run 2 was selected for payoff by publicly rolling a die. Participants were paid out privately and left the laboratory. The exchange rate was fixed at 150 ET = 1 Euro. The average payoff was 12.25 Euros (including a show-up fee of 2.50 Euros) with a stan-

investments corresponds to 1.2^{10}.

[7]Subjects were not informed that the matching procedure was restricted in such a way, conveying the impression that being matched with the same opponent more than once is very unlikely.

[8]Instructions are included in the appendix.

dard deviation of 5.09 Euros. Each session lasted approximately one and a half hours.

The standard game theoretic prediction is trivial. Because of the finiteness of the game, there is no trust and no trustworthiness among selfish and rational players if selfishness and rationality are common knowledge. However, starting with Berg et al. (1995), numerous experiments have shown that subjects are willing to send and return non-trivial amounts of money in the trust game. For a survey of the trust game literature see, for example, Camerer (2003).

While the experimental one-shot version of the trust game is by now well-analyzed and -understood, the dynamic interplay of inequality and trust is not easily predicted. However, observe that both of our treatments start with identical *average* endowments. If inequality does not affect subjects' willingness to send and return money, relative to their endowments, the two treatments may be expected to yield equivalent results with respect to growth rates.[9] On the other hand, the empirical and experimental literature on social and economic heterogeneity cited in Section II suggests that we may observe a negative impact of inequality on trust in our setting. Dispersion of wealth could increase social distance between economic agents and, as a result, trust and trustworthiness may decrease. To the extent our experiment captures some of the underlying mechanisms assumed in this literature, we should expect less growth and lower efficiency in treatment IEQ.

Finally, we note that theories of social preferences can organize some of the deviations from standard equilibrium behavior observed in the trust game. For instance, inequity aversion models (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999) can explain both trust and trustworthiness in the trust game by assuming that selfishness and fairness motives interact within and between subjects.[10] However, these models do not yield unambiguous

[9]In the beginning of the first run, the average endowment of trustors in treatment EQ is equal to 500, as it is in treatment IEQ. Thus, if the same share is sent and returned, expected overall invested amounts are the same, as well as the amounts returned. Therefore, the expected endowments of trustors in round 2 are the same in both treatments. The same reasoning applies to all consecutive rounds of the game.

[10]See Bolton and Ockenfels (2000), page 187, for a detailed description of the mechanics

comparative static predictions across our two treatments. To see why, observe for instance that a rather fair-minded trustor who is matched with a relatively poor trustee may send money to equalize payoffs, while a rather selfish trustor may not send money because he cannot expect to get anything back from a relatively poor opponent. Thus, the predictions of inequity aversion models will depend on the distribution of preferences. It appears, though, that 'myopic', straightforward concerns for equal payoffs lead to more trust and trustworthiness in IEQ in the following sense: Even when an inequality averse subject assumes that everybody else behaves in a completely selfish manner, he still has reason to trust and to be trustworthy towards relatively poor opponents in the inequality treatment (where, in the beginning of round 1, the payoff distribution is unfair), but no such incentive exists in the equality treatment (where the payoff distribution is fair if everybody behaves selfishly).

IV Experimental Results

IV.1 Aggregate Data

Figures 1 and 2 depict the evolution of averages of send and return quotas over time. The send quota of a particular round is the share of the trustor's wealth in this round that she invests in the transaction. The return quota is defined as the amount returned minus the mandatory 90% ($R - 0.9S$), divided by the amount received minus the mandatory 90% ($1.2S - 0.9S$). For example, a return quota of 1/3 implies that the trustee returns exactly the amount invested by the trustor. (The dashed horizontal line in Figure 2 indicates this 'break-even line'.) For figures and non-parametrical tests the send quota averages are calculated by adding up all amounts sent in a matching group, and dividing the sum by the total wealth of the senders.[11]

of the fairness models in the context of Berg et al. (1995)'s trust game.

[11] This procedure creates a weighted measure for the trust level, which seems appropriate since here our focus lies on aggregate behavior. However, our conclusions from statistical tests do not depend would not be different, if unweighted averages would be used.

FIGURE 1
Average Send Quotas over Rounds

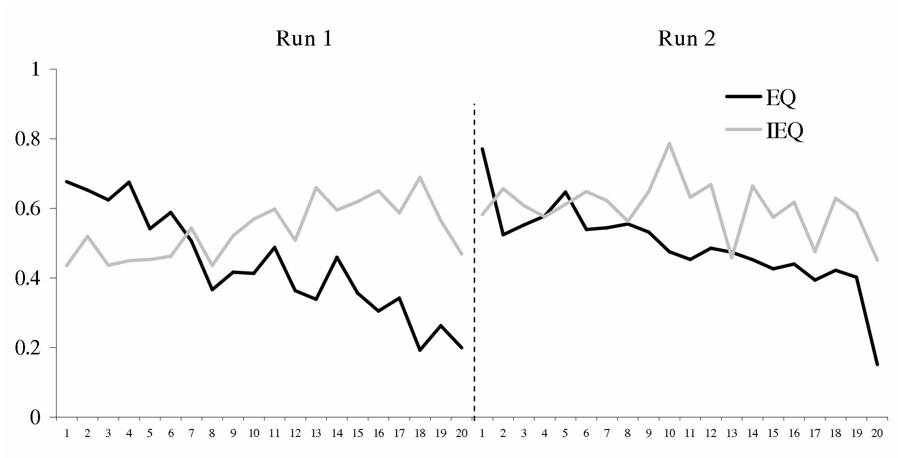


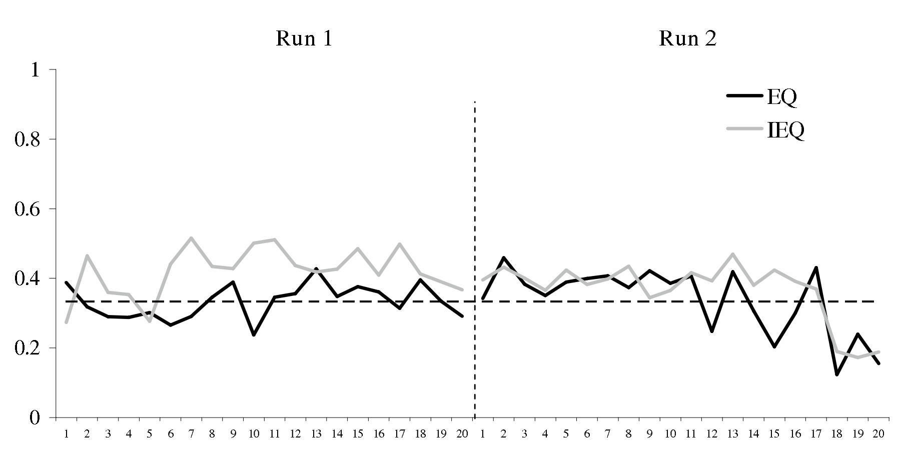
Figure 1 shows that the dynamics of trust differ markedly between the treatments. In the first round of the games, the equal distribution of wealth leads to higher trust (54% more, to be exact) than the unequal distribution.[12] This finding is in line with previous empirical findings and theoretical work suggesting that inequality hampers efficiency.

However, send quotas in treatment EQ strongly and steadily decrease over time from 68% in round 1 to 20% in round 20 in run 1, and from 77% to 15% in run 2, while send quotas in IEQ increase slightly in run 1 and decrease slightly in run 2. Correspondingly, in EQ average send quotas of the matching groups are significantly and negatively correlated to the number of rounds (Pearson-R=-.586, $p = .000$ and Pearson-R=-.394, $p = .000$ for run 1 and 2, respectively) while this is not (strongly) so in IEQ (Pearson-R=.061, $p = .442$ and Pearson-R=-.154, $p = .051$ for run 1 and 2, respectively).[13]

[12]One-sided Mann-Whitney-U (MWU) tests applied to (statistically still independent) individual send quotas and to respective matching group data in round 1 all yield $p < 0.1$. The reason for the low significance of these between-treatment tests lies in the significant heterogeneity of subjects in treatment IEQ. Specifically, poor subjects send absolutely less than EQ subjects ($p < 0.01$), but not relatively, while rich subjects send relatively less ($p < 0.05$), but not absolutely. Our analysis of individual behavior in the next subsection controls for these wealth effects.

[13]A similar conclusion is reached when applying Wilcoxon Matched Pairs Signed Ranks

FIGURE 2
Average Return Quotas over Rounds



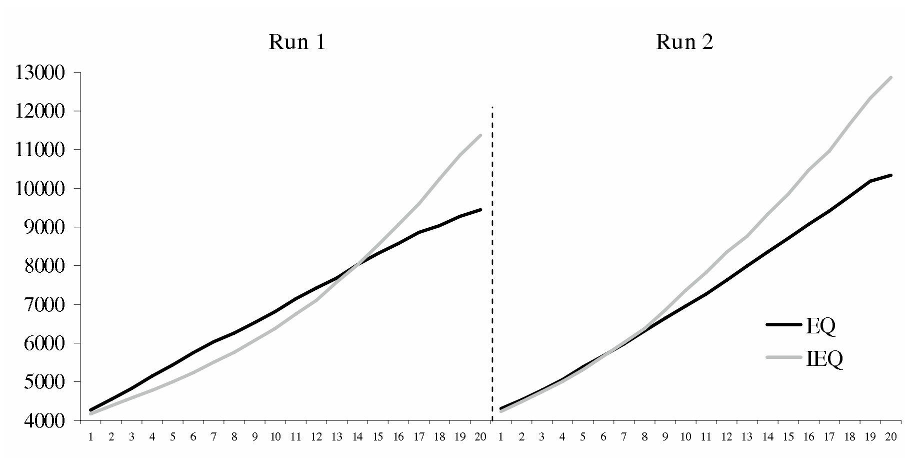
Differences in the return quotas, visualized in Figure 2, are generally less pronounced than differences in the send quotas. However, average return quotas in the first run are significantly lower in treatment EQ than in treatment IEQ (a one sided MWU test applied to independent matching group averages yields $p = .007$). The differences in run 2 are not statistically significant. A look at the individual data reveals higher average trustworthiness in treatment IEQ: The share of responders with an average return quota of more than 1/3 in run 1 is 56.3% in treatment IEQ and only 34.4% in treatment EQ ($p = .021$, two-sided χ^2-test). Thus, from a trustor‘s point of view, the overall probability that a trusting decision is profitable is higher in IEQ. Differences in run 2 with 57.8% for IEQ and 45.3% for EQ are again existent, but not significantly different ($p = .216$, two-sided χ^2-test).

In contrast to the evolution of send quotas, average return quotas appear to be relatively stable over time. In neither treatment there is a significant difference between the first and second half of a run (Wilcoxon Matched

(WMPSR) tests to averages per matching group or to individual averages for the first and the second half of each run, respectively.

Pairs Signed Ranks test, $p > 0.1$ for all tests). However, the fact that return quotas tend to go down in the last few rounds, especially in run 2, suggests that subjects exhibit an end-game effect.

FIGURE 3
Average Total Economy Wealth over Rounds

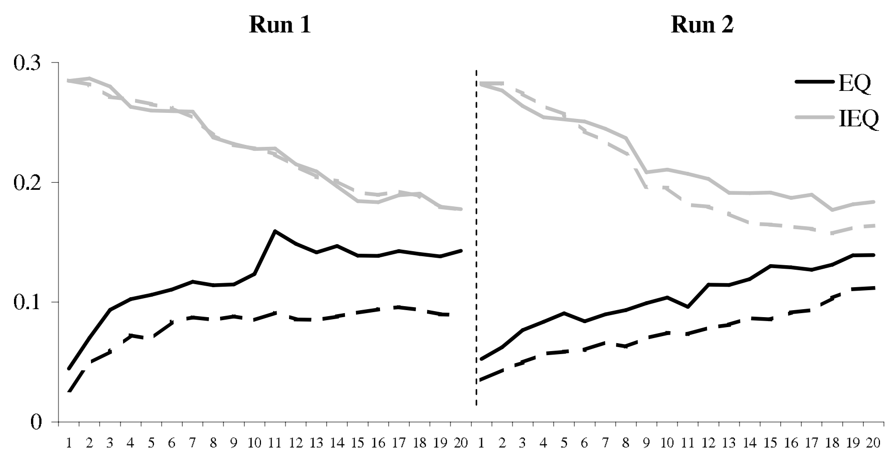


The different dynamics in trust are reflected in the growth rates of overall wealth in our experimental economies. Recall that more trust directly expands overall wealth in our experimental design, because the latter is a cumulative measure of the former. Figure 3 depicts average economy wealth over time. There are substantial efficiency gains in both treatments and runs, with total average wealth more than doubling in all runs of both treatments. Initially, wealth in treatment IEQ lags behind the one in treatment EQ. However, as average send quotas remain on a relatively high level in treatment IEQ and decrease in treatment EQ, the lag is eventually counterbalanced and reversed in the last few rounds. In run 2 we do not observe large initial differences, and after the first few rounds treatment EQ lags behind indelibly. Accumulated wealth in IEQ finally accounts for more than 300 % of initial endowments.

Not only efficiency gains but also the distributions of wealth in our experimental economies evolve endogenously through sending and returning

FIGURE 4

Observed and Simulated Gini Coefficients over Rounds



decisions. We use Gini coefficients to analyze the dispersion of individual wealth levels.[14] Figure 4 depicts average matching group Gini coefficients in treatments EQ and IEQ (solid lines). We observe that Gini coefficients strongly and steadily decrease (increase) in treatment IEQ (EQ), and tend to converge to each other towards the end of a run. In the last round of a run, IEQ and EQ Ginis are not or only weakly significantly different (MWU, $p = .462$ and $p = .093$ for run 1 and 2, respectively).

Redistribution in our setting might have two different sources: on the one hand, it could be the result of random, homogenous interaction, in the sense that quotas are not conditioned on individual wealth states or wealth comparisons. Because, given the quotas, richer subjects send more in absolute terms than poorer subjects, such behavior moves the economy towards more equality when starting with unequal endowments. On the other hand, redistribution could be the result of quotas which systematically depend on

[14]The Gini coefficient as a measure for disparity takes the value of zero if the income is equally distributed among the subjects and $(n-1)/n$ if all wealth is concentrated on only one subject. Here, the maximum value of the Gini coeeficient accounts for 7/8, as the number of subjects per matching group is $n = 8$).

others' wealth or on own wealth in the current state. Depending on the nature of conditional behavior and the heterogeneity of the behavioral patterns, the resulting system behavior may increase or decrease equality relative to what can be expected from unconditional homogenous interaction.

In order to isolate the effects of these two mechanisms, we simulate Gini coefficients for unconditional, homogenous behavior. More specifically, simulations are based on the same role and group matchings as implemented in our experiment. Furthermore, we assume that in every round all participants in a matching group behave identical – like the group average.[15] If actual behavior is unconditional with respect to wealth levels and differences, simulations and actual behavior cannot differ. If behavior is conditional on these factors, the actual system deviates from the simulated system.

Moreover, we use the same growth rates as realized in each respective round to calculate the evolution of the income distribution separately for all experimental groups.

The simulated Gini coefficients (see the dotted lines in Figure 4) follow the same general pattern as the observed ones. In treatment IEQ, simulated and observed Ginis curves are nearly the same (run 1) or differ only slightly (run 2). Consequently we find no differences between round 20 observation and simulation values (WMPSR, $p = .674$ and $p = .327$ for run 1 and 2, respectively). In treatment EQ, observed values are constantly higher than the simulated values, yielding (weakly) significant differences of final values (WMPSR, $p = .025$ and $p = .093$ for run 1 and 2, respectively).

Thus, on the aggregate level, we find no evidence for systematic and deliberate redistributive behavior from rich to poor in treatment IEQ. Contrary, inequality rises faster than expected in treatment EQ, suggesting that there are indeed heterogeneous behavioral patterns that systematically affect wealth distribution.

[15] This procedure yields the same individual economy growth rates in the simulation as in the experiment.

IV.2 Individual Decisions

We regressed the individual send and return quotas on a number of independent variables. First, we use the *Round* number (1-20), and create two dummies for *Treatment* (0 for EQ, 1 for IEQ) and *Run* (0 for 1st, 1 for 2nd run). We also include the relative wealth standing of the sender (W_{SDR}) and the responder (W_{RSPR}) prior to the transaction. Both variables are derived by dividing the respective decision maker's wealth by the average wealth in her economy (matching group). For the regression on the return quota we include the send quota of the counterpart. To account for group-specific as well as for subject-individual characteristics, we use Mixed Effects models. Due to the rather complex dynamic nature of our game, we cannot exclude any interaction effects between the independent variables. We approach the selection problem in the following way: in our main regressions, we start with the full factorial set of potential interaction effects. We then iteratively throw out insignificant effects. After two iterations we ended up with the models presented in Table 1. Note that, by construction, all included interactions effects are significant. As a second approach we ran regressions on the 2-factorial set of interaction factors. The results are presented in Table 2 in the Appendix and basically confirm the analyses shown here.

The model for sender decisions, presented in Table 1, reveals important differences in the role of relative wealth variables between the treatments.[16] Wealth positions influence trusting behavior in treatment EQ, but are of minor importance for trustors in treatment IEQ. In treatment EQ, the effect of both direct wealth variables is negative. That is, participants send less the richer they are and the richer the responder is. However, the positive interaction effect indicates that the more equal sender and responder are, the less pronounced are the wealth effects. The lowest send quotas are found for poor senders towards rich responders and for rich senders towards poor responders. Contrary, in treatment IEQ all these three effects are largely neutralized. It seems that here wealth of the sender or responder

[16] We had to exclude 6 and 314 observations in the models on the send quota and return quota, respectively, because the send quota is only defined for positive wealth of the sender, and the return quota is only defined for positive amounts sent.

TABLE 1

Regressions of individual send and return quotas

Dependent Variable	Send Quota		Return Quota	
	Coefficients	(Std.Error)	Coefficients	(Std.Error)
Round	-0.017**	(0.001)	-0.005**	(0.001)
W_{SDR}	-0.950**	(0.152)	-0.080**	(0.023)
W_{RSPR}	-0.922**	(0.145)	0.029	(0.030)
$W_{SDR} * W_{RSPR}$	0.662**	(0.142)		
Treatment	-1.255**	(0.198)	0.075*	(0.033)
Treatment*Round	0.013**	(0.002)		
Treatment*W_{SDR}	0.929**	(0.160)		
Treatment*W_{RSPR}	0.892**	(0.151)		
Treatment*$W_{SDR} * W_{RSPR}$	-0.631**	(0.147)		
Run	0.036**	(0.011)	0.016	(0.033)
Run*W_{SDR}			-0.077*	(0.038)
Run*$W_{SDR} * W_{RSPR}$			0.063*	(0.027)
Send quota			0.154**	(0.015)
Constant	1.866**	(0.173)	0.275**	(0.050)
Random Effects				
Group StdDev	0.186	(0.045)	0.0001	
Subject StdDev	0.264	(0.019)	0.174	(0.013)
Residual StdDev	0.272	(0.004)	0.246	(0.004)
No. of obs.	2554		2246	
Wald χ^2	305.10		249.81	
Log-restricted likelihood	-529.993		-207.612	

Standard errors are given in parentheses. * and ** denote significance on the 5% and 1%-level, respectively. Regression models are derived by starting with a full factorial set of interaction effects and iteratively throwing out insignificant effects.

play only a very minor role for decisions on send quotas. These observations are consistent with the simulation results of the Gini coefficient dynamics shown above. While the trust decisions in EQ systematically affect the wealth distribution in the economy, this is not the case in IEQ.

The effect of the repetition of the game (Run) is positive and corresponds to an increase of average send quotas across the treatments in the second run of the game. With respect to the evolution of investments over time, we find a negative effect of the number of rounds for treatment EQ, whereas in IEQ the effect of time is somewhat mitigated. This result is in line with the reported matching group averages. Finally, the coefficient of the treatment dummy is large and highly significant, pointing out a negative effect of *initial* inequality introduced by the variation of endowments.

The model for responder decisions indicates that return quotas are generally higher in treatment IEQ, and shrink over time. We find that participants reciprocate high investments, as the coefficient for the send quota is positive and significant: The more of his wealth the first mover sends, the higher his expected profit margin. Rich senders can expect to earn less from their trusting decisions than poor senders, while there seems to be no effect of the wealth of the responder herself. Also, there are no significant interaction effects between treatment and the relative wealth indicators as found in the send quota regression.

V Discussion and Conclusions

We have conducted an experimental trust game to analyze the dynamics of economic inequality and trust. In our experimental economies, participants start with either an equal or unequal endowment distribution. They then repeatedly play a trust game and accumulate their payoffs. In each round, both transaction partners are informed about the current wealth of their opponent.

We find that trust and efficiency is initially high in the economy starting with equal endowments (EQ), but decreases over time; when endowments are unequal (IEQ), trust is initially lower yet remains relatively stable. While wealth distributions converge towards each other, IEQ eventually outperforms the initially equal economy EQ. The differences seem partly due to the fact that conditional trust is much more prevalent in EQ than in IEQ. Two explanations for the different role of conditional behavior across treatments suggest themselves.

First, wealth might be a credible signal for trustworthiness: unfair agents become richer, and therefore richer people should be trusted less. However, in IEQ wealth information has less reputational value, because there wealth does not only depend on behavior but also on the exogenous endowments.[17] This implies that there is more reason to employ conditional trust strategies in EQ than in IEQ.

Second, social preferences may also produce conditional behaviors in EQ. Observe that, as trustees typically do not return more than what the trustor has sent, higher send amounts tend to yield more inequality when the responder is rich, and less inequality when the responder is poor. Thus, inequality aversion in treatment EQ might prevent participants from sending their money to rich responders. This hinders growth. But then, why don't we see the same mechanism in treatment IEQ?

We suspect that the answer to this question lies in the way inequality was created. A number of experimental studies (see for example Bolton, Brandts and Ockenfels (2005), Frey, Benz and Stutzer (2004)) emphasize the role of procedural fairness for economic decision-making. People are found to be more tolerant towards inequitable outcomes if inequality is the result of a fair procedure. This is the case in treatment IEQ in the sense that the assignment of initial endowments is done by a 'fair' chance move such that the initial distribution may be perceived as fair by the participants. Contrary, inequality emerging in treatment EQ is created endogenously by unfair behavior of some of the subjects and thus triggers inequality averse behavior of others. Summing up, inequality aversion seems to have counterintuitive effects in our experiment: it lowers the economy's potential to grow when starting with equality, and does not trigger redistribution when inequality is imposed by an exogenous and fair procedure.[18]

[17] In our regressions, we do not find a direct significant effect of a responder's wealth on her return quota. However, the effect may be concealed to some extent, because if the argument is right, rich people are trusted less and thus earn less.

[18] One puzzle remains: why, if inequality aversion plays a role, are send quotas lower with higher sender wealth? There are three potential explanations: First, as the regressions show, rich senders run a higher risk of being exploited than poor subjects, and therefore should be more cautious. Second, for the same send quota, rich senders put considerably more money on the table than poor senders. And third, inequality aversion is generally

Of course, a similar pattern of conditional trust may also emerge from reciprocity. Indeed, high wealth in EQ is a signal for prior unfair behavior; simple correlation analysis reveals that in treatment EQ there is a significant negative correlation between a person's wealth at the end of a half-run and her average return quota in the preceding periods.[19] These subjects are also less trusting.[20] Thus, negative reciprocity based on wealth signals is justified in treatment EQ. Contrary, in treatment IEQ the informational content of wealth is covered by the exogenous imposed inequality. Correspondingly, half-run correlations between average return quotas and wealth are low or insignificant here.[21] Thus, participants behave negatively reciprocal in treatment EQ on the basis of wealth levels, thereby hampering exchange and growth. In treatment IEQ, such conditional behavior is not feasible due to the ex-ante heterogeneity in wealth levels, endorsing trust, somewhat surprisingly.

Overall, the following picture emerges from our analyses: in the experimental EQ economy, trust is relatively prevalent at the beginning, maybe due to low social distance as measured by initial wealth comparisons. Sending money increases efficiency and rises accumulated wealth, but also necessarily yields some inequality. This results from the stochastic matching and role assignment on the one hand and from behavioral heterogeneity among subjects on the other hand. In fact, the difference between simulated and actual Gini values makes obvious that *heterogeneity* in trust and trustworthiness drives a non-trivial part of the increasing inequality in EQ. With increasing inequality, subjects start to condition their behavior on the opponent's wealth, motivated by inequality aversion, reciprocity or reputa-

assumed to be asymmetric with respect to own disadvantage and advantage.

[19]Results of Pearson correlations in treatment EQ are R=-.358, p=.004 and R=-.479, p=.000 for rounds 1-10 and rounds 11-20 in run 1, and R=-.397, p=.001 and R=-.309, p=.013 for rounds 1-10 and rounds 11-20 in run 2, respectively.

[20]Average trust and trustworthiness of a subject are generally highly correlated (treatment EQ: R=.592, p=.000 and R=.526, p=.000 for run 1 and 2, treatment IEQ: R=.526, p=.000 and R=.371, p=.000 for run 1 and 2, respectively), but the correlations are somewhat stronger in treatment EQ.

[21]In treatment IEQ Pearson correlation statistics are R=-.142, p=.262 and R=-.048, p=.704 for rounds 1-10 and rounds 11-20 in run 1, and R=-.240, p=.056 and R=-.152, p=.231 for rounds 1-10 and rounds 11-20 in run 2, respectively.

tional effects. Rich subjects do not trust poor subjects because of the risk of being exploited, and nobody trusts rich subjects, unable to distinguish between riches who made their fortune in a fair way and the ones who exploited others. Consequently, trust rates go down and growth is attenuated.

The situation is very different in the experimental IEQ economy: Initial exogenous inequality lowers mutual trust, but send quotas are still positive. Inequality decreases over time, but not because of deliberate redistribution from rich to poor. Of course there is heterogeneity in behavior,[22] but (at least senders' behavior) is not *systematically* affected by wealth levels. The initial fairly assigned endowment differences do not allow for fairness/reputation effects of accumulated income. As a result, trust levels remain stable, allowing for considerable efficiency gains until the end of the game.

We believe that our study yields promising questions for further research. On the individual level, further experimental work is needed to distinguish between alternative motives for the conditional behavior observed in treatment EQ. On the aggregate level, it might be worth studying experimental economies starting with more realistically unequal or asymmetric initial income distributions. The experimental economies studied here start with symmetric income distributions with a Gini factor of 0 and 0.30, respectively. However, the lowest Gini ever measured was 0.16 in Bulgaria, 1968, and the highest was 0.74 in Namibia, 1993. In 2000 the Gini for the United States was 0.41, and 0.28 for Germany.

Finally, while most existing empirical studies of the relationship between inequality, trust and macroeconomic indicators concentrate on cross-country correlations, our results suggest that there might also be value in studying the dynamics of inequality *within* countries – as well as the interaction of trust and procedural or jurisdictional fairness perceptions.

[22]For example, 30 % of the subjects have an average send quota of less than 0.25, while approximately 35 % of the subjects send more than 75% of their respective round wealth.

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Appendix

A Additional tables

TABLE 2

Regressions of individual send and return quotas, including 2-factorial set of independents interaction effects

Dependent Variable	Send Quota		Return Quota	
	Coefficients	(Std.Error)	Coefficients	(Std.Error)
Round	-0.012*	(0.005)	-0.011*	(0.005)
W_{SDR}	-0.375**	(0.076)	-0.239**	(0.072)
W_{SDR}*Round	0.000	(0.003)	0.006	(0.003)
W_{RSPR}	-0.243**	(0.065)	0.010	(0.082)
W_{RSPR}*Round	-0.005	(0.003)	0.004	(0.003)
W_{RSPR}*W_{SDR}	0.069*	(0.035)	0.067	(0.037)
Treatment	-0.629**	(0.130)	0.075	(0.099)
Treatment*Round	0.013**	(0.002)	0.000	(0.002)
Treatment*W_{SDR}	0.308**	(0.065)	0.063	(0.047)
Treatment*W_{RSPR}	0.253**	(0.043)	-0.058	(0.062)
Run	0.120*	(0.056)	0.065	(0.063)
Run*Round	-0.003	(0.002)	-0.006**	(0.002)
Run*W_{SDR}	0.025	(0.033)	-0.027	(0.033)
Run*W_{RSPR}	-0.082*	(0.032)	0.076*	(0.032)
Run*Treatment	0.014	(0.022)	-0.059**	(0.021)
Send quota			0.293**	(0.074)
Send quota*Round			-0.003	(0.002)
Send quota*W_{SDR}			-0.013	(0.040)
Send quota*W_{RSPR}			-0.095*	(0.046)
Send quota*Treatment			0.039	(0.031)
Send quota*Run			-.0.038	(0.027)
Constant	1.190**	(0.123)	0.330**	(0.120)
Random Effects				
Group StdDev	0.171	(0.040)	0.000	(0.000)
Subject StdDev	0.265	(0.019)	0.173	(0.012)
Residual StdDev	0.272	(0.004)	0.243	(0.017)
No. of obs.	2554		2246	
Wald χ^2	301.43		299.70	
Log-restricted likelihood	-500.302		-155.981	

Standard errors are given in parentheses. * and ** denote significance on the 5% and 1%-level, respectively.

B Instructions

Below we show the instructions, translated from German, for the first run of treatment IEQ. Instructions for the other runs and treatments were worded in a similar way.

Welcome! You can earn money in this experiment. How much money you earn depends on your decisions and the decisions of the other participants. From now on, please do not communicate with the other participants. If you have a question concerning the experiment, please raise your hand! We will come over to you to answer your question. If you break this rule, we will have to exclude you from the experiment and all payments.

In the experiment, we will use "Experiment-Taler" (ET) as the currency. At the end of the experiment, your payoff will be converted into Euros and will be paid out in cash. The exchange rate is 150 ET = 1 Euro. In the experiment, all amounts in ET are rounded to whole numbers.

The experiment consists of several runs. The payoff of one of these runs will be paid out at the end of the experiment. After the experiment is over, it will be determined by the roll of a die which run is relevant for the payment. The following instructions refer to the first run of the experiment. After the first run is over you will receive new instructions.

In this run all participants receive an initial endowment. Half of the participants receive an initial endowment of 800 ET, the other half receives an initial endowment of 200 ET. It will be determined by chance which participant receives which initial endowment.

The run consists of 20 rounds. In each round pairs are formed randomly, each pair consisting of participant A and participant B. It is guaranteed that you do not interact with the same participant in two subsequent rounds. The roles A and B within the group are assigned randomly in every round. The identity of the participant you are interacting with is secret, and no other participant will be informed about your identity. Thus, your decisions are anonymous.

Every round proceeds as follows:

- At the beginning of the round both participants are informed about their roles (A or B), about the number of the round (1-20), their own current wealth and the current wealth of the other participant.

- Then participant A can decide how much of his/her wealth he/she wants to send to participant B.

- The amount sent by participant A is multiplied by 1.2. This means participant B not only receives the amount sent, but 120 % of the amount sent (1.2*amount sent).

- Then participant B can decide how much he/she sends back to participant A. He/she must send back at least 90 % of the amount sent (0.9*amount sent). The upper limit for the amount sent back is the wealth of participant B.

After that the round is over. Wealth at the end of the round is calculated as follows:

- Participant A: Wealth at the end of the round = wealth at the beginning of the round - amount sent + amount sent back (at least 0.9*amount sent)

- Participant B: Wealth at the end of the round = wealth at the beginning of the round + 1.2*amount sent - amount sent back (at least 0.9*amount sent)

Wealth at the beginning of a new round is equal to wealth at the end of the preceding round. The relevant payment for the run is determined by the wealth at the end of the last round in the run.