# Inequality, Fairness and Social Capital

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**Abstract**

We study the impact of unjust inequality on social trust and trustworthiness, and how it interacts with economic status in a large-scale controlled experiment. We document that unfair economic inequality is detrimental for social interactions, resulting in a significant decline in trust and trustworthiness. Probing the boundaries of this effect, we demonstrate that this erosion of social capital critically depends on the context: if an economically successful person is not directly responsible for the outcome of the unsuccessful person, we observe no negative effects on trust and trustworthiness in the aggregate. Finally, our data do not support the view that higher status or wealth leads to an erosion of pro-social attitudes: the successful are always more generous, whereas unsuccessful persons display the least efficient and generous behavior.

KEYWORDS: inequality, fairness, social capital, experiment

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## 1. Introduction

The recent surge of income and wealth inequality in many developed countries is a widely discussed topic in the media and academic research. Much of these discussions revolve around the gains of the top-income decile and the stagnation of income for the bottom half of the distribution and its implications for society (e.g., Piketty and Saez, 2003; Autor, Katz, and Kearney, 2008; Piketty, 2014; Piketty and Saez, 2014; Piketty, Saez and Zucman, 2016; World Inequality Report, Alvaredo et al., 2017). Indeed, inequality deriving from competitive economic environments is often associated with negative societal consequences (Stiglitz, 2012; Verhaeghe, 2014). In particular, it is sometimes conjectured that inequality may harm the social fabric, destroying social capital (trust, honesty, cooperation) and subsequently affecting economic outcomes (Wilkinson and Pickett, 2010). Two hypotheses can be derived from the literature in economics and the social sciences. The first hypothesis states that higher inequality, if perceived as unjust and caused by competition, hampers economic interaction (Alesina and Perotti, 1996; Bénabou, 1996; IPSP, 2017, Section 3; Camera, Deck and Porter, 2017). The second hypothesis states that those who are in an advantageous position (of higher status or wealth) in an unequal society, become self-focused and greedy (Piff et al., 2010; 2012; Fisman et al., 2015, Guinote et al., 2015; Martinsson et al., 2015; Nishi et al., 2015). That is, negative social consequences are caused by the egoistic behavior of the successful.

Both of these hypotheses are contested in the literature. However, empirical assessments of the effects of inequality and the role of the successful often suffer from an absence of counterfactuals and the endogeneity of status. Experimental methods offer an alternative approach for assessing the consequences of inequality as they make exogenous variation of inequality and its underlying causes possible. While potentially having lower external validity, experiments thus provide a clear identification of causal effects and underlying processes.

We use experimental methods to study the impact of perceived unjust inequality on subsequent social interactions, differentiating between the behavior of the economically successful and the unsuccessful. Our design thus aims to test both hypotheses within the same setting. We create income inequality in dyads, using a real-effort procedure with varying payment schemes. Subsequently, we let these dyads interact in a modified trust game allowing us to measure both players'

social trust and trustworthiness. Social trust has been interpreted as an important component of social capital in the literature (Glaeser et al., 2000; Bellemare and Kröger, 2007; Björnskov, 2017; Langer et al., 2017). As higher social capital is typically associated with better-functioning institutions and society in general (Putnam, 2000), social trust is a center piece in the debate on whether inequality erodes the social fabric.[1] In addition, our experimental measure for trustworthiness allows us to quantify subjects' greed or altruism absent strategic motives. It directly tests the hypothesis that higher inequality has a negative impact on social interactions because successful people become less generous, in particular less generous than the unsuccessful.

We create exogenous variation in income inequality in the real-effort task by randomly assigning subjects to two different payment schemes. In our baseline condition subjects receive a piece-rate payment. This results in relatively low inequality and is typically not perceived as unjust. We compare the trust-game outcomes in this setting with an environment that features high and unjust inequality. To generate such an environment, we implement a relative-payment scheme that additionally gives an undue time advantage to one participant in the dyad.[2] This undermines equality of opportunity and the payment scheme can thus be seen as unfair from a normative perspective (e.g., Roemer, 1998). This view is supported by participants' fairness judgments showing that the time-adjusted relative-payment scheme is perceived a substantially less fair than the piece-rate payment. In a third condition, we employ the same relative-payment scheme to generate unjust inequality as before, but randomly rematch participants in the trust-game stage (keeping earnings information constant across conditions). This eliminates the direct responsibility for each other's

[1] More precisely, social capital can be defined as values and shared beliefs that help groups to cooperate in situations where contracts are difficult or impossible to enforce (cp., Guiso, Sapienza, and Zingales, 2010). Accordingly, it is possible to measure social capital by eliciting values and beliefs using experimental tools (see e.g., Fehr (2009) for an extensive account of the measurement of trust and trust beliefs). In the economic literature social capital has been positively associated with a plethora of economic outcomes, such as economic growth (e.g., Knack and Keefer, 1997), the size of firms (e.g., Bloom, Sadun, and Van Reenen, 2012) or financial development (e.g., Guiso, Sapienza, and Zingales, 2004).

[2] There is evidence documenting that (high) inequality is not per se seen as unfair (e.g., Bortolotti et al., 2017; Breza, Kaur, and Shamdasani, 2018; Fehr, 2018). For example, Fehr (2018) illustrates that an increase in inequality leads to more antisocial behavior but only if higher inequality cannot be clearly attributed to work effort and is possibly the result of immoral behavior.

outcomes in the dyads and has the advantage of observing matches with equal and unequal outcomes from the real effort stage.

We present two main results. First, our findings support the view that unjust inequality can negatively affect social interactions. That is, we document a significant decline in trust and trustworthiness when income inequality is the result of an income-generating process that is eminently perceived as unfair. However, we also find that this observed decline depends on a direct interaction in the first stage, i.e., when the well-off ("successful") player causing the poor outcome of the worse-off ("unsuccessful") player. If we take away the direct interaction by re-matching participants in the trust game, we find that especially the successful players maintain a high level of trust and trustworthiness, in particular when interacting among themselves. That is, the detrimental impact of unjust inequality on social interactions critically depends on contextual factors.

Second, we find that successful players are consistently more generous than the unsuccessful in absolute terms. This is in strong contrast to an influential literature claiming that the successful behave more selfish (e.g., Piff et al. 2010, 2012) and are responsible for the negative societal consequences of inequality. In general, our results indicate that both successful and unsuccessful players narrowly frame their distribution decisions in the trust game, which echoes recent findings by Exley and Kessler (2018) showing that people have a strong tendency to such narrow framing of equality. In our setting, this is particularly evident for the successful, as they are always less generous relative to their total wealth than the unsuccessful. This highlights an important normative question about whether generosity should be judged in absolute or relative terms. While third-party observers may take a broader perspective incorporating total wealth and thus setting a high normative bar, our stakeholders take a different and narrower perspective focusing primarily on the specific social interaction. In any case, that subjects seemingly frame equity concerns narrowly has significant implications, as such behavior can result in a more unequal society.

In the next section, we introduce the experimental paradigm and design of our study followed by a description of how we induce perceived unjust inequality. Section 3 shows that our experimental paradigm successfully induces inequality differences and a polarization of fairness perceptions. Clearly, neither inequality nor competitiveness have to be perceived negatively per se

(e.g., Cappelen et al., 2007; Cappelen et al., 2013; Cappelen et al., 2014; Bartling et al., 2017, Bartling, Grieder, and Zehnder, 2017). Rather, it is the combination of inequality with unequal opportunity in a competitive environment that triggers strong feelings of injustice in our experimental setup and guarantees a powerful prime to reliably quantify the effects of unjust inequality on social interactions. This mirrors many settings outside the laboratory, as inequality arising from unequal opportunities and competition is, for example, an inherent feature of school education, universities, workplaces or labor markets more generally.[3] Section 4 discusses the effects of unjust inequality in fixed dyads and Section 5 discusses the effects when direct attributions of responsibility for others' outcomes cannot be made. We discuss these results in the context of the related literature in section 6.

## 2. Experimental Paradigm and Design

We employ an experimental paradigm in which dyads of participants interact in two stages. In the first stage, we use a repeated real-effort task involving either an individual piece-rate payment inducing low inequality, or a relative payment scheme (tournament) with preferential treatment of the initial tournament winner, resulting in high inequality (in a between-subjects design). In the second stage participants then interact in a trust game. Consequently, we observe trust and trustworthiness depending on stage-1 conditions (low versus high inequality), and depending on stage-1 income. This allows us to gauge the impact of higher inequality on social interactions and how this relates to income.

### 2.1. Stage 1: Inequality Manipulation

We implement a repeated real-effort slider task (Gill and Prowse, 2012) and vary the payment scheme to manipulate inequality, i.e., low inequality versus high and unjust inequality. In the slider task, participants see a number of sliders on their computer screen and have to adjust each slider to exactly the middle position within a certain time limit (see Figure A.1 in the Appendix). The goal in this task is to maximize the number of correctly positioned sliders before the allotted time

[3] For example, Lemieux, MacLeod, and Parent (2009) document an economy-wide increase of performance-pay jobs in the U.S. labor market, along with a substantial increase in wage inequality.

runs out. Participants are only allowed to use their mouse to drag the sliders into the correct position.[4] The task requires little a-priori knowledge and skills such that outcomes mainly depend on the expended effort of subjects. Unfairness or concerns about unequal opportunities arise only through institutional features, i.e., the details of the two distinct payment schemes we implemented.

In the piece-rate payment scheme (low-inequality condition), participants complete four rounds of this task, each lasting for 120 seconds. In each round, they receive a flat payment of €0.50 plus €0.05 per correctly placed slider. Total earnings are calculated by summing up the earnings in the four rounds. Note that each subject in a dyad individually determines her own earnings, i.e., there is no interaction. However, at the end of each round both subjects in the dyad are informed about the correctly positioned sliders and the resulting earnings of each other. Thus, social comparison is also salient in this setting.

In the relative-payment scheme (high-inequality condition), participants in a dyad also complete the slider task four times, with the first round lasting for 120 seconds. In contrast to the piece-rate payment scheme, participants' payoffs in each round are determined through their relative performance. That is, the subject with the higher number of correctly placed sliders in a round receives €3.00, while the subject with the lower number of correctly placed sliders receives €0.30. In the case of equal performance, the two payments are randomly allocated. As in the piece-rate payment scheme, participants receive information on the performance of each subject and the resulting payoffs after each round. In addition to the high payoff, the subject with the higher performance receives a time bonus. More specifically, the winner of the first round obtains a time bonus of 8 seconds. The time bonus is subtracted from the time budget of the tournament loser such that the time for completing the second-round task is 128 (112) seconds for the first-round winner (loser). The winners of the second and third round get a time bonus of 6 and 4 seconds, respectively, which is again subtracted from the time budget of the loser in the respective round.

While the piece-rate scheme leads to modest inequality depending on individual performance, the relative-payment scheme leads to a more spread pay distribution (and thus more inequality)

[4] We used a keyboard locker to prevent students from using the arrow keys or the mouse wheel.

than the observed performance differences justify. There are two features of relative-payment scheme that render the resulting income inequality unjust. First, the substantial time gap (16 seconds) that arises after the first round makes it nearly impossible for the first-round loser to catch up in the subsequent rounds, and thus magnifies the income difference over the rounds. Second, conditional on exerting effort, winning the first round largely depends on matching luck (i.e., the random assignment of the interaction partner). Therefore, the condition induces inequality, caused by a competitive procedure that is difficult to justify on fairness grounds as there are no reasons that would warrant a winner-takes-all allocation (no obvious indivisibilities).

Note that our relative-payment scheme includes two components – competition and unjust procedure – that are absent in the piece-rate payment scheme. These two aspects go often hand in hand in real-world settings, where initial (unfair) advantages are often amplified in competitive contexts, leading to enhanced inequality (e.g., Frank and Cook, 1995; Stiglitz, 2012). For example, it is empirically well-established that being born rich is a gateway to better primary and secondary schooling and subsequently to college, resulting in better jobs and higher earnings (Chetty et al., 2011) as well as highly persistent socio-economic status (Chetty et al, 2017). At the same time, combining inequality with an unjust and competitive procedure helps us to identify any effects of perceived unjust income inequality on social interactions. This is important as previous evidence suggests that inequality effects are subtle (see discussion in Section 6). As such, our goal is to maximize the impact of inequality in the relative-payment scheme in comparison to the inequality in the piece-rate payment scheme, and not to compare different aspects of inequality.

### 2.2 Stage 1: Measurement of Fairness Perception

We measure subjects' fairness evaluations of the payment schemes to assess whether the piece rate versus tournament manipulation was successful in creating perceptions of unfair inequality. To gauge the impact of the procedures on participants, we measure fairness perceptions both before and after the stage-1 real-effort task. At the beginning of the experiment, participants receive the detailed instructions about the stage-1 real-effort task and the payment procedures of their condition. They then answer three control questions about the procedure. Next, they are asked to indicate on a scale from 0 (very unfair) to 10 (very fair) how fair they consider the payment procedures in

stage 1. They also indicate their gender, age, and field of study. After that they start with the real-effort task.

The first assessment provides a fairness judgment based on a verbal description of the mechanism, absent any experience of the task and the outcomes. Our second measurement takes place immediately after the end of stage 1. Subjects have then completed four rounds of the real-effort task and received feedback on the number of correctly placed sliders and the corresponding payoffs of both subjects in the dyad. Thus, we can observe whether and how experiencing the task and the resulting feedback affects subjects' fairness evaluations. Because we collected fairness judgments in all conditions, demand effects cannot drive any observed treatment differences.[5]

### 2.3. Stage 2: Measurement of Social-Interaction Effects

In the second stage, we use a two-player trust game to measure the effects of the exogenous income variation on social interactions. In this game, we endow the first mover (trustor) with €6 and the second mover (trustee) with €0. The first mover decides whether or not to transfer the endowment to the second mover. If she does not transfer, the game ends and the earnings will be €6 for the first mover and €0 for the second mover. In contrast, if she transfers her endowment, the experimenter triples the endowment such that the second mover receives €18 (and first mover has €0 now). The second mover then decides how much of the €18 to send back to the first mover (by the cent). Payoffs follow directly from the second mover's decision.

To obtain information on both decisions and the underlying processes, we use the strategy method. More precisely, we first elicit from each player in the dyad their decision as a first mover, and then their decision as a second mover conditional on having received a transfer (because otherwise there is no decision to be made). The player roles in the game are randomly determined after all decisions have been made and subjects are well aware of this fact. Therefore, this modification allows us to answer our first research question (i.e., the effect of inequality on trust in other individuals in a group; first mover) and the second research question (i.e., the greediness of individuals as a function of stage-1 income; second mover), within the same context.

[5] It is still conceivable though that fairness is made salient by asking. However, saliency of fairness issues is conducive to our goal of studying downstream behavior following allocations perceived as unfair.

We also measure participants' beliefs regarding the behavior of the other player in this stage. Specifically, we ask subjects to indicate whether they believe the other player in the dyad transferred her endowment when acting as a first mover (yes/no), and to indicate how much they think the other player sends back when acting as second mover (in six ranges: €0 to €3.00; €3.01 to €6.00; …; €15.01 to €18.00). We do not incentivize beliefs because the preclusion of hedging opportunities would have required rather complex randomizations. Given the randomization in the implementation of the strategy method, we did not want to complicate matters further.

### 2.4. Treatments

We implemented three treatments. In condition *Piece Rate* (first-stage piece rate – same partner) the piece-rate payment scheme determined subjects' earnings whereas in *Tournament* (first-stage tournament – same partner) we used the relative-payment scheme. In both conditions, stage-1 dyads remain intact and proceed together to stage 2 to play the trust game as explained above. We emphasized at the very beginning of the experiment that subjects will interact with the same partner throughout the whole experiment. At the start of stage 2, subjects are reminded of this fact. They also receive a reminder of their own and the other person's stage-1 earnings before making any choices in the trust game. Condition *Tournament-New* (first-stage tournament – new partner) is identical to *Tournament*, except that the dyads are re-matched in stage 2, such that each person will play the trust game with a person with whom she did not interact in stage 1. Again, we made clear at the beginning of the experiment that they interact with different person in the two stages. At the beginning of stage 2, they were informed about the new match and they received information on their own and the other persons' (the new partner in the dyad) earnings from stage 1.[6] This design precludes attributions of responsibility for each other's stage-1 outcomes (Lien et al., 2018). Moreover, as only earnings (but not effort) are communicated, is it not possible to attribute high or low stage-1 earnings to luck or effort.

[6] Note that the information at the beginning of the trust-game stage was exactly identical in all treatments, i.e., we displayed both players stage-1 earnings on the screen at the beginning of stage 2.

### 2.5. Procedural details and variable definitions

In total, 636 subjects took part in the experiment that was programmed using z-Tree (Fischbacher, 2007): 160 in condition *Piece Rate*, 134 in condition *Tournament*, and 342 in condition *Tournament-New*. The first two conditions were run on a subject pool at the Universities in Heidelberg and Mannheim (balanced across conditions). For condition *Tournament-New* we recruited 202 new subjects from the same subject pool, and additionally 140 subjects from the laboratory at the Technical University Berlin to increase power for the analysis of various subgroups of matching stage-1 winners and losers in this treatment.[7] Participants were undergraduate students from a wide range of different majors, who were recruited with ORSEE (Greiner, 2015) in Berlin and Mannheim and with Hroot (Bock, Nicklisch, and Baetge, 2012) in Heidelberg.

Final payoffs were determined by adding payoffs from both the real-effort stage and the trust game. A typical session lasted about 50 minutes, and subjects earned, on average about €13.40 (approximately $14.70 at that time), with final payoffs ranging from €1.20 to €30. There was no show-up fee in addition to the incentivized payoffs; that is, incentives were very salient.

At the beginning of a session we matched participants in equal-gender dyads, with one mixed dyad if there was an uneven number of (fe)males. This was done based on the information about each subjects' gender from the initial questionnaire. Subjects were not aware of this matching procedure; they were only informed that they were matched with another person in the lab. We implemented this matching procedure in the background to control for possible gender differences in the performance in the multiple-round slider task (Gill and Prowse, 2014) and in the behavior in the trust game (Bellemare and Kröger, 2007).

In the presentation of the results we use the following conventions. In the fixed dyads conditions *Piece Rate* and *Tournament* we will call the person with the higher income in a dyad "successful" and the person with the lower income "unsuccessful." In the *Tournament-New* condition, participants encounter new partners, leading to various matches based on the stage-1 income. In

[7] Note that the results reported in Section 5 remain qualitatively unaffected if we exclude the Berlin sample (for more details, see Section 5).

the presentation, we denote subjects as "successful" if stage 1 income equals €12.00 and as "unsuccessful" if stage-1 income equals €1.20. This definition reflects the typical payoff pattern for the successful and unsuccessful in condition *Tournament* (results are robust to alternative definitions). In our analysis using the successful-unsuccessful denomination, we drop individuals from dyads with equal income (in *Piece Rate* and *Tournament*, in total N=12) and unclassified subjects with an income between €12.00 and €1.20 (in *Tournament-New*, N=54).

## 3. Results: Income Inequality Manipulation

We first provide evidence on effort levels, i.e., the number of correctly positioned sliders, in the different conditions. The *Piece Rate* and *Tournament* conditions did not result in different levels of effort with an average number of correctly solved sliders of 75 in *Piece Rate* and 76 in *Tournament* in all four rounds (p=0.795, two-sided t-test). Effort in *Tournament-New* was somewhat higher at 81 compared to *Tournament* (t=2.28, p=0.023). Importantly though, the average difference in effort levels between the two players in a dyad in the first slider task does not differ in all three conditions (3.93 in *Piece Rate*, 4.33 in *Tournament*, and 4.54 in *Tournament-New*, two-sided t-tests, all p>0.28).

**Table 1**: Stage-1 Earnings

| | *Piece Rate* | *Tournament* | *Tournament-New* |
|---|---|---|---|
| Earnings: mean | 5.77 | 6.60 | 6.60 |
| Earnings: median | 5.75 | 6.60 | 6.60 |
| Earnings: 10% percentile | 4.93 | 1.20 | 1.20 |
| Earnings: 90% percentile | 6.70 | 12.00 | 12.00 |

*Notes*: Entries are in €.

Table 1 displays stage-1 earnings and shows that the tournament condition has the intended effect on inequality. While average earnings are comparable across the different treatments, the variation in earnings is much larger in *Tournament* and *Tournament-New* than in *Piece Rate*. That is, small initial differences in effort translate into vast income inequality in *Tournament* and *Tournament-New*, but not in *Piece Rate*.

It is conceivable that subjects perceive the high reward for the tournament winner as justified, taking a meritocratic perspective and focus on incentives for performance (see e.g., Cappelen et al., 2007). This is not what happens in the current context. Table 2 shows that participants perceive the tournament mechanism as substantially less fair than the piece-rate mechanism. We observe strong treatment differences both before and after the experience of the task and for both the successful and the unsuccessful: the piece-rate scheme always receives much higher fairness evaluations than the two tournament schemes. Experiencing the task leads to lower evaluations compared to the mere verbal description for all three conditions. In all three conditions, the unsuccessful perceive the task as less fair than the successful.

**Table 2**: Fairness Evaluation of Payment Mechanism

| Point of evaluation | Evaluators | *Piece Rate* | *Tournament* | *Tournament-New* |
|---|---|---|---|---|
| Before experience | All | 7.17 (n=160) | 3.69$^{***}$ (n=134) | 3.91$^{***}$ (n=342) |
| After experience | All | 6.78$^{\wedge\wedge\wedge}$ (n=160) | 2.44$^{\wedge\wedge\wedge,***}$ (n=134) | 2.90$^{\wedge\wedge\wedge,***}$ (n=342) |
| After experience | Successful | 7.32 (n=78) | 2.98$^{***}$ (n=63) | 3.57$^{***}$ (n=144) |
| | Unsuccessful | 6.36$^{\#\#}$ (n=78) | 1.92$^{\#\#,***}$ (n=63) | 2.00$^{\#\#\#,***}$ (n=144) |

*Notes*: Entries are fairness ratings ranging from 0 (perceived as very unfair) to 10 (perceived as very fair); *,**,*** indicates significant difference between *Piece Rate* and *Tournament* conditions; #,##,### indicates significant difference between successful and unsuccessful; and ^,^^,^^^ indicates significant difference between evaluation before and after experience; at the 10%, 5%,1% level, t-test; pairs with equal earnings excluded in analyses of successful and unsuccessful. Note that there are no statistically significant differences between entries for *Tournament* and *Tournament-New* (this is robust to restricting to the same locations).

We conclude that the stage-1 manipulation succeeded in inducing strong differences in income inequality and fairness perceptions across piece rate and tournament conditions. Moreover, successful and unsuccessful subjects strongly differ in their fairness perceptions, reflecting a self-serving bias that might have lead the successful to perceive the procedures and resulting positional differences as more justifiable than the unsuccessful. Note that self-serving fairness judgments by the successful should facilitate self-centered behavior in the trust game.

## 4. Results: Social Interaction Effects for Fixed Dyads

### 4.1. Main Effects

We now turn to the analysis of whether the strong differences in payoff inequality and fairness perception between *Piece Rate* and *Tournament* affect behavior in the stage-2 trust game. Table 3 shows our main results. We observe strong treatment effects, with the share of trusting participants (i.e., transferring their endowment to the second mover) being almost 20 percentage points lower in *Tournament* than in Piece rate (top panel, Table 3). Trust is significantly lower in *Tournament* for both the successful and the unsuccessful. However, we do not detect significant differences in trust between these subgroups in either treatment.

**Table 3**: Social Interaction Effects of Payment Mechanism

| | Participants | *Piece Rate* | | *Tournament* | |
|---|---|---|---|---|---|
| Trusting | All | 71% | (n=160) | 53%$^{***}$ | (n=134) |
| | Successful | 71% | (n=78) | 49%$^{***}$ | (n=63) |
| | Unsuccessful | 71% | (n=78) | 56%$^{*}$ | (n=63) |
| Amount returned | All | €6.41 | (n=154) | €5.50$^{**}$ | (n=134) |
| | Successful | €6.30 | (n=78) | €6.10 | (n=63) |
| | Unsuccessful | €6.55 | (n=78) | €4.65$^{\#\#,***}$ | (n=63) |

*Notes*: *,**,*** indicates significant difference between treatment; #,##,### indicates significant difference between successful and unsuccessful; at the 10%, 5%,1% level, two-sided t-test for amounts returned, test of proportion for trust; pairs with equal earnings excluded in analyses of successful and unsuccessful.

***Result 1****: Unjust inequality in stage 1 is detrimental for social trust in stage-2 interaction for fixed dyads.*

The bottom panel of Table 3 shows the amounts returned by the second mover. Remember that there are no strategic considerations at this stage and that these amounts are conditional on the trust decision of the first mover resulting in a budget of €18 for the second mover and €0 for the first-mover. We observe that amounts returned are almost €1 lower in the *Tournament* than in the *Piece Rate* condition (6.4 vs 5.5). Thus, transferring the budget implies an expected loss for the first mover in *Tournament*. This effect is mainly driven by the behavior of the unsuccessful stage-1 subjects. While there is no difference in the amounts returned across conditions for the successful,

the stage-1 losers strongly reduce these amounts in *Tournament*. Consequently, amounts returned are significantly lower for the unsuccessful than for the successful in *Tournament*.

While reduced trustworthiness (generosity) affects the distribution of trust game earnings resulting in a higher variance and skewness, reduced trust affects overall welfare because of the inefficiency of forgoing the tripled payoffs after transfer. Indeed, we observe that the welfare effects are substantial. Expected trust game earnings are €1.08 lower in the *Tournament* condition (€7.26 vs. €6.18), a 15% loss compared to the *Piece Rate* condition.

***Result 2****: Unjust inequality in stage 1 is detrimental for absolute generosity in stage-2 interaction for fixed dyads, which is driven by lower generosity of the unsuccessful.*

In general, the observed returns in the trust game indicate hardly any attempts to equalize overall experimental wealth in our setting in both *Piece Rate* and *Tournament*. It appears that participants narrowly frame their decisions in stage 2 and do not fully take stage-1 income into consideration when deciding about how much to return to the trustor. This raises an interesting normative aspect of the observed generosity. Although the successful in *Tournament* are more generous in the trust game than the unsuccessful, they fall short of relevant normative benchmarks. First, they give less than the unsuccessful relative to their total wealth. Second, in spite of having typically earned €12 in stage 1 (vs. €1.20 for their partner), they are far from sharing the stage-2 income (return €9), or overall income (return €14.40) equally. However, failure to meet such normative criteria is not restricted to the successful. In *Piece Rate*, stage-1 payoff differences are modest in most dyads, and both the successful and the unsuccessful fail to share their income equally (return €9) and thus to equalize their overall earnings. The observer's higher normative expectations towards the stage-1 winners make this behavior look less acceptable for the successful in *Tournament*. Yet, conditional on a narrow framing of decisions in stage 2, the successful return more than the unsuccessful in *Tournament*.

### 4.2. The Role of Beliefs

In stage 2 we measured subjects' beliefs regarding the other player's behavior as a trustor and as a trustee in a dyad. In the Appendix (Table A.1), we show that the *Tournament* condition induces more pessimistic beliefs regarding both trust and amounts returned. These effects are significant

for the whole sample, but only significant for the successful subgroup when differentiating by stage-1 outcome. That is, the stage-1 condition affects subjects' beliefs. In Tables 4 and 5 we investigate whether these beliefs can explain the treatment effects on trust and trustworthiness. The tables provide four specifications: Specifications 1 and 2 verify the raw comparisons discussed above including various controls. Specifications 3 and 4 include beliefs about trust and trustworthiness. All specifications include controls for gender, location, and session size.

**Table 4**: Determinants of Trust

| *Dependent variable: Transfer (yes/no) to second mover* | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Tournament* | -.178<br>(3.05)$^{***}$ | -.147<br>(1.74)$^{*}$ | -.133<br>(2.06)$^{**}$ | -.131<br>(1.45) |
| Successful | | .010<br>(.13) | | .070<br>(.81) |
| *Tournament* × Successful | | -.076<br>(.63) | | -.006<br>(.05) |
| Belief in trust by other | | | .428<br>(6.64)$^{***}$ | .411<br>(6.20)$^{***}$ |
| Belief in amount returned by other | | | .046<br>(3.27)$^{***}$ | .047<br>(3.33)$^{***}$ |
| Male | -.082<br>(1.42) | -.097<br>(1.61) | -.059<br>(.94) | -.071<br>(1.10) |
| N | 294 | 282 | 294 | 282 |
| Joint effect of tournament variable | | $\chi$=9.67, p<.01 | | $\chi$=4.05, p=.132 |

*Notes*: Marginal effects from probit regressions with robust z-statistics in parenthesis. All regressions control for gender, session size and location. Linear regressions support the sign of the interaction terms in the probit regressions. Belief in amount returned by other scaled to 100 cents.

We find a clear correlation between beliefs and behavior. For trust, beliefs about the other person's trust and her trustworthiness relate to higher trust (Table 4). The latter effect makes sense from a strategic point of view (expecting lower returns on trust), while the former effect suggests a conditionally-cooperative or reciprocal view (conditioning on behavior if the other person were in the trustor's position). Results on trustworthiness support the reciprocal view as well (Table 5). Higher beliefs on amounts returned by the other player relate to higher amounts returned. Because strategic aspects are absent for the second mover, beliefs about the other person's returns can only play a role in terms of reciprocal thinking. Note that while beliefs play a role for both trustor and

trustee behavior, the main treatment effects of the *Tournament* condition remain substantial when including the beliefs. That is, beliefs cannot fully explain the effect of unjust inequality on social interactions.

**Table 5**: Determinants of Amounts Returned

| *Dependent variable: Amount returned in cents* | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Tournament* | -101<br>(1.9)$^{*}$ | -198<br>(2.70)$^{***}$ | -40<br>(.85) | -166<br>(2.68)$^{***}$ |
| Successful | | -8<br>(.13) | | 17<br>(.31) |
| *Tournament* × Successful | | 167<br>(1.53) | | 253<br>(2.80)$^{***}$ |
| Belief in trust by other | | | 13<br>(.26) | 22<br>(.44) |
| Belief in amount returned by other | | | 77<br>(7.42)$^{***}$ | 84<br>(8.62)$^{***}$ |
| Male | -215<br>(4.05)$^{***}$ | -207<br>(3.77)$^{***}$ | -170<br>(3.63)$^{***}$ | -157<br>(3.33)$^{***}$ |
| N | 294 | 282 | 294 | 282 |
| Joint effect of tournament variable | | F=3.70, p=.026 | | F=4.59, p=.011 |

*Notes*: Tobit regressions with robust t-statistics in parenthesis. All regressions control for gender, session size and location. Linear regressions support the sign of the interaction terms in the tobit regressions. Belief in amount returned by other scaled to 100 cents. Amounts are coded in cents.

## 5. Results: Social-Interaction Effects in New Dyads

The previous analysis has revealed strong detrimental effects of unjust inequality on social interactions. In this section, we test the boundaries of this effect by rematching subjects into new dyads in stage 2 (*Tournament-New*). While the experience and perception of competition and unjust inequality is identical to the *Tournament* condition (see Section 3, Table 2 results), a direct attribution of "responsibility" for the mutual stage-1 outcomes is absent in this condition. A negative attribution of high stage-1 earnings for the successful to undeserved luck also becomes more difficult as effort information on the stage-1 dyad is not available.[8] Moreover, the rematching of dyads allows us to distinguish between the role of a player's own income and the income of the

[8] König-Kersting et al. (2017) find that outcome information biases the perception of the underlying process ("outcome bias"). They find that the bias is mainly driven by positive random outcomes being falsely attributed to the decision

matched partner: this was impossible in *Tournament* because these incomes were perfectly correlated.

We first compare behavior in *Tournament-New* (trust = 65%; amount returned = €6.61) and *Tournament* (trust = 53%; amount returned = €5.50) keeping in mind that we used an additional location to collect data for *Tournament-New*. Running simple probit/tobit regressions with a treatment dummy and controlling for locations, as well as other standard covariates shows that trust and generosity are significantly larger in *Tournament-New* than in *Tournament* (*p=0.016* and *p<0.001*). The results are robust to restricting the analysis to the same locations (i.e., excluding the Berlin sample).[9]

**Table 6**: Social Interaction Effects – *Tournament-New*

| | Participants | vs. all (1) | vs. successful (2) | vs. unsuccessful (3) |
|---|---|---|---|---|
| Trusting | All | 65% (n=342) | 64% (n=144) | 64% (n=144) |
| | Successful | 68% (n=144) | 71% (n=56) | 69% (n=67) |
| | Unsuccessful | 62% (n=144) | 61% (n=67) | 55% (n=56) |
| Amount returned | All | €6.61 (n=342) | €6.49 (n=144) | €6.51 (n=144) |
| | Successful | €7.37 (n=144) | €7.96 (n=56) | €6.98# (n=67) |
| | Unsuccessful | €5.74 (n=144)*** | €5.48*** (n=67) | €5.52** (n=56) |

*Notes*: *,**,*** indicates significant difference between successful and unsuccessful; #,##,### indicates significant difference between successful partner and unsuccessful partner; at the 10%, 5%, 1% level, test of proportion for trust, and two-sided t-test for amounts returned. Unclassified participants (*n=54*, i.e., those with an income between €12.00 and €1.20) are excluded when conditioning on successful and unsuccessful decision maker or successful and unsuccessful partner. This leads to different number of observations across cells, depending on stage-2 matches with unclassified subjects.

maker's skill. If this effect transfers to the current setting, we expect that good stage-1 outcomes should more likely be attributed to skill, rather than luck, by stage-2 players.

[9] All results in this section remain qualitatively identical if we exclude the Berlin sample in *Tournament-New*, but some of the within-treatment comparisons of successful and unsuccessful become insignificant due to small sample sizes in subgroups. Details are available in the appendix, see section A3.

Next, Table 6 shows detailed results for Trust and for Amounts Returned, separately for successful and unsuccessful decision makers, and successful and unsuccessful partners in the dyad. The upper panel of Table 6 shows trust behavior. There are no significant raw differences in trust between the successful and the unsuccessful (column 1), and neither between situations interacting with a successful partner (column 2), and an unsuccessful partner (column 3). However, there is a tendency to trust the stage-1 losers less and also for the losers to trust less. Accordingly, trust within dyads of unsuccessful participants is lower than trust within dyads of successful participants (55% vs. 71%, z=1.77, p<0.08). Regressions reveal that winners are 12.4 percentage points more likely to trust others than losers, which is a significant effect (see Table 7).

The lower panel of Table 6 shows that stage-1 winners are significantly more generous as second movers than stage-1 losers are. This holds for interactions with other winners and for interactions with losers. Winners give their partners in the dyad even more when facing a stage-1 winner than when facing a stage-1 loser (€7.96 vs. €6.98). As in the case of trust, these effects lead to an overall large difference of generosity within the group of unsuccessful people versus the group of successful people (€5.52 vs. €7.96, t=3.56, p<0.001).

**Table 7**: Determinants of Trust and Amounts Returned – *Tournament-New*

| | Trust | Trust | Amounts Returned | Amounts Returned |
|---|---|---|---|---|
| Successful | .124<br>(2.00)** | .141<br>(1.83)* | 234<br>(4.18)*** | 160<br>(3.53)*** |
| Successful Partner | .050<br>(.79) | -.062<br>(.82) | 56<br>(1.00) | -36<br>(.83) |
| Belief in trust by other | | .581<br>(7.42)*** | | 122<br>(2.08)** |
| Belief in amount returned by other | | .062<br>(4.51)*** | | 76<br>(6.08)*** |
| Male | -.127<br>(2.00)** | -.130<br>(1.75)* | -88<br>(1.52) | -82<br>(1.79)* |
| N | 246 | 246 | 246 | 246 |

*Notes*: Marginal effects from probit regressions for Trust with robust z-statistics in parenthesis. Tobit regressions for Amounts Returned with robust t-stats in parenthesis. Amounts are coded in cents. All regressions control for gender, session size, and location. Belief in amount returned by other scaled to 100 cents.

The result that dyads of stage-1 losers perform worst in terms of trust and trustworthiness suggests that the detrimental effect of inequality on trust and trustworthiness is not driven by inequality within dyads per se. Moreover, because of the reduced trust and trustworthiness within the group of dyads of stage-1 losers, stage-2 inequality is larger, and stage-2 welfare is lower in this group compared to the winner dyads. The expected welfare loss of the loser dyads amounts to €0.96, a 13% loss compared to the winner dyads. As in the case of trust, a regression analysis shows that the winners return significantly higher amounts in the trust game (Table 7).

***Result 3****: The detrimental effects of unjust inequality on social interactions are dampened in newly assembled dyads. Negative effects derive mainly from interactions among the unsuccessful.*

A closer look at the participants' beliefs explains the differences in trust game behavior between *Tournament* and *Tournament-New*. Table 7 shows that the effect of beliefs on trust and amounts returned emerge in *Tournament-New* just as in *Tournament*. However, while in *Tournament* there were substantial negative effects of the stage-1 interaction on beliefs, especially for the winning partners, there are no such negative effects in *Tournament-New* (see Appendix A.2). Moreover, in *Tournament-New* the successful stage-1 players hold more positive views than the unsuccessful ones, especially when paired with another successful person.

We also observe that social aspects must be relevant for the observed effects. That is, the negative effects for the unsuccessful stage-1 dyads cannot simply derive from higher risk aversion caused by their lower income. We observe negative effects for the loser in both trust (potentially affected by risk attitude) and the non-strategic behavior as second mover. Moreover, in the comparison between *Piece Rate* and *Tournament*, where unsuccessful players were always matched with successful ones, there were no differences between the successful and unsuccessful. Moreover, recent literature suggests that, if relative position is salient, inequality may lead the poor to take higher levels of risk (see Payne et al., 2017, and references therein). We therefore interpret our results in terms of reduced levels of social capital within groups of unsuccessful subjects, rather than in terms of risk attitudes.

## 6. Discussion

We present an experiment aimed at studying the effects of unjust economic inequality and the potential role of the economically successful in harming the social fabric, in a controlled lab setting. Obviously, there is no trivial mapping of laboratory results to behavior in society at large. Our study thus aims to complement results from less controlled settings and to provide insights into the underlying mechanisms, in particular by analyzing the behavior of the successful and unsuccessful as well as the impact of beliefs about others' behavior.

The finding that unjust inequality arising in a competitive environment has substantial effects on trust and trustworthiness supports the view that such an environment might be detrimental to social interactions, well-being, and more generally to social capital (Kawachi et al., 1997; Verhaeghe, 2014; Buser and Dreber, 2016). The increased pessimism about others' willingness to cooperate and the lower willingness to take the social risk of trusting a stranger is indicative for a decline in social capital. Indeed, we find that beliefs are correlated with behavior and that they are significantly more pessimistic if inequality is unjust. As a consequence, a vicious cycle of decreasing trust and cooperation may result, leading to a substantial loss of social capital.

Importantly, we find that the decline in trust and trustworthiness is mostly driven by the less well-off. Thus, we find no evidence for the hypothesis that the behavior of the successful is mainly responsible for the erosion of the social fabric. This is consistent with recent findings of Camera et al. (2017). They report that worse-off subjects discriminate against better-offs by cooperating less with them in a repeated helping game even if wealth is determined by chance, leading to an overall efficiency loss in the long run. Zheng (2017) similarly reports a higher degree of selfish behavior in a team production setting for low status subjects, where status is endowed in non-monetary terms (public praise). In Table A3 in the appendix, we summarize a larger set of experimental studies that relate to the question of the impact of inequality and competition on cooperation and trust. Although these studies greatly differ in terms of design, the overall picture is consistent with negative social capital effects being more likely. However, the table shows a rather mixed picture about which social status group may drive the observed effects. That is, differences in implementation of inequality are important for the relevant channel driving social capital effects.

A large literature in psychology has argued that rich, high-status individuals are less generous in *absolute* terms than poor, low-status individuals (e.g. Piff et al., 2010; 2012; Guinote et al., 2015). In particular, this literature makes the causal claim that increasing wealth induces less social behavior. In correlational field data, the existence of a negative correlation between status and prosocial behavior has been questioned (Trautmann et al., 2013), and various studies have recently shown that wealthy individuals are often more prosocial and more generous in absolute terms (e.g. Andreoni et al., 2017; Korndörfer et al., 2015; Smeets et al., 2015). Our results suggest that the behavior of the successful is indeed not responsible for the negative consequences of unjust inequality on social interactions.

If the, arguably modest, degree of competition and unjust inequality in a lab setting can induce strong effects on social behavior, we may expect the consequences to be even more severe in more significant situations outside the lab. However, our results also hint to the boundaries of such effects. Negative effects on trust and trustworthiness are overall reduced if the interaction partner has not directly contributed to the existing income inequality within a dyad. This happens despite the fact that subjects perceive the inequality-generating process as equally unfair in the two *Tournament* conditions. At a first glance, this result contradicts results in Buser and Dreber (2016) who report negative effects of competition on cooperation even in newly assembled groups. In contrast to Buser and Dreber, however, subjects in our new-dyads condition were aware of their own and the other player's income situation. Apparently, the uncertainty about stage-1 outcomes in Buser and Dreber induces a behavior closer to our condition of fixed dyads. Indeed, positive trust game effects emerge in the new-dyads condition especially in interactions between two stage-1 winners, i.e., in a situation with high income and income equality. If information about other's income is absent, positive effects on trust (and trustworthiness) may not be easily realized.

The observed differences between the fixed dyads and the newly assembled dyads hint at the volatility of the subtle psychological effects caused by inequality or fairness cues. Moreover, our manipulation combined strong inequality with a competitive and perceived unjust payment scheme. We have argued that this key feature of our setup is relevant in many contexts outside the lab such as in educational systems, labor markets or one's social environment (e.g., Chetty et al., 2011; Chetty, Hendren, and Katz, 2016; Hanushek and Woessmann, 2006; Lemieux, MacLeod, and Parent, 2009). The more modest inequality emerging in condition *Piece Rate* is perceived as

fair and allows players to maintain a high level of trust and trustworthiness. The perceived justice of the institution producing unequal outcomes thus seems to constitute an essential aspect, lending support to Starmans et al.'s (2017) conjecture that it is not inequality per se that bothers people in life, but economic unfairness. Indeed, dyads of unsuccessful participants in *Tournament-New* score low on trust and trustworthiness despite having equal outcomes; their experience of disadvantages caused by unfair economic allocations seems to affect behavior, rather than the experience of inequality per se. Such low social capital among the poor is also consistent with field data on deprived neighborhoods in the UK. Compared to wealthy neighborhoods, social capital is lower in deprived neighborhoods, measured by interactions among people in the same neighborhood and thus social class (Nettle et al., 2011). Jachimowicz et al. (2018) similarly report negative effects on social community fabric among low income households. Our results suggest that these field data may not simply derive from selection of people in or out of certain neighborhoods, but are caused by the local environment itself. The result is consistent with classic findings in social psychology that members of low-status status groups aim to move out of these groups if their group status cannot easily be increased (Tajfel and Turner, 1979): in the current study, betraying the other player's trust allows stage-1 unsuccessful players to move out of the group of low payoff subjects.

In conclusion, we find clear evidence for negative social capital effects of unjust inequality. We also find that most people frame fairness narrowly, consistent with Exley and Kessler (2018). As a consequence, despite being more generous in absolute terms, the successful appear more selfish from a broadly framed fairness perspective including all possible benefits. If the broad frame is normatively more compelling, the successful are likely to fall short of the potential normative expectations we may hold with respect to their behavior. This is not the case for the poor, simply because expectations are lower. Such an expectation-behavior gap for the rich may explain the appeal of picturing elites as immoral and selfish in popular discourses, which were eager to pick up the results supporting the view of the selfish elite.

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