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Competition and prosociality: A field experiment in Ghana

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Abstract

Competitive bonuses are commonly used to promote higher productivity in the workplace. Yet, these types of incentives could have negative spillovers on coworkers' prosocial behavior in subsequent tasks. To investigate this question, we conduct a lab-in-the-field experiment in Ghana. In a between-subjects design, participants complete a real-effort task under a competitive, threshold, or random payment while holding payment differentials constant across treatments. Before and after, we measure prosociality through a public goods and a social value orientation game. Competition reduces prosociality when the dispersion of payments is high. However, when there is less at stake, competition does not affect prosociality.

JEL Codes: C93, D03, J33

Keywords: competitive payment, wage differences, prosociality, field experiment

1 Introduction

The success of an organization critically depends on social relations among coworkers (Beal et al., 2003). Good interpersonal relations are associated with greater willingness of people within an organization to help each other, share information and cooperate on joint projects (Brief and Motowidlo, 1986). In addition, such relations facilitate communication, enhance employee commitment, foster individual learning, and result in improved

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organizational performance (Leana and Van Buren, 1999; Adler and Kwon, 2002; Bright et al., 2006). Employees also report higher job satisfaction and organizational commitment as well as less absenteeism when they perceive that coworkers are friendly (Rhoades and Eisenberger, 2002). Hence, managers should try to create friendlier work environments.

At the same time, managers are under extreme pressure to increase individual productivity and thus, turn to incentives for boosting performance. Relative payment schemes, in which workers compete for a bonus for example, are a very common instrument used in the workplace. Both theoretical and empirical literatures suggest that competitive, relative performance schemes can increase effort and productivity compared to piece-rate payments, but necessarily relative to fixed-payment schemes (e.g. Lazear and Rosen, 1981; Erev et al., 1993; van Dijk et al., 2001; Irlenbusch and Ruchala, 2008; Bandiera et al., 2011). Yet, it is unclear whether competitive incentives negatively affect the ex-post quality of coworker relationships.

In this paper, we assess how competitive versus individual payments obtained from a *non-collaborative* task impact cooperation and prosocial attitudes *after* such a task. Our setting can be thought of as an environment in which individuals work independently and the best performer receives a bonus. We thus consider the effect that this payment has on interactions among coworkers afterwards. Our main hypothesis is that competitive payment schemes generate a negative effect on the quality of coworker relationships. There are various channels that might give rise to this effect. First, competition generates a feeling of rivalry among competitors. Confrontations in the workplace might cause workers to see each other as opponents and thus, adopt more individualistic behavior (e.g. Drago and Garvey, 1998; Brandts et al., 2009; Dechenaux et al., 2012). Second, there are always winners and losers in a competition. This generates inequality in endowments and status. Empirical evidence suggests that those two forms of heterogeneity are associated with lower levels of prosociality and lower incentives to enter into a competition (e.g. Chan et al., 1999; Cherry et al., 2005; Buckley and Croson, 2006). Moreover, it has been shown that people with a pronounced aversion to inequality are less likely to select into a competition (e.g. Bartling et al., 2009). So, if forced to work under a competitive payment scheme, these individuals may become unsatisfied with the workplace environment and ultimately reduce their prosociality. Lastly, competitive payments can be regarded as unfair. This in turn may decrease incentives to act prosocially after individuals have been exposed to a competition (e.g. Akerlof and Yellen, 1990). Particularly those who usually work in teams might perceive an individual bonus scheme as unfair; this could cause frustration and less prosocial behavior.

To test the effect of competition on prosociality, we conduct a lab-in-the-field experiment (artefactual field experiment in the terminology of Harrison and List, 2004) with workers from a banana-producing agribusiness in Ghana. The field context is particularly relevant given the research question for two reasons. First, due to low productivity at the time the experiments were being designed, the firm was considering introducing a competitive bonus system. Second, teamwork is important for the performance of the firm. About 50% of the tasks (e.g. bunchcare, harvesting, and quality control in packaging) rely on teams.[1] Accordingly, the firm permitted us to do an experiment with a sample of its workers in order to test the potential effectiveness (on productivity) and side effects (on prosociality) of a competitive bonus system.

In our experimental design, we randomly and confidentially match two participants for the duration of a three-stage experiment.[2] In the *first stage*, we measure baseline prosociality via a one-shot public goods game (PGG) and a social value orientation (SVO) game (à la Murphy et al., 2011). To mitigate income and reputational effects, feedback on the outcomes of the first stage is provided only after subjects complete the post-questionnaire. In the *second stage*, participants complete a real-effort/output task where they individually assemble ballpoint pens. We implement a between-subjects design in which each subject is randomly allocated to either a competitive, a threshold, or a random payment scheme. In the competitive scheme, the participant who assembles most pens correctly earns a high payment; in the threshold scheme, every participant who correctly assembles more than a given number pens earns a high payment; and in the random scheme, the high earner is determined by chance. We refer to high- and low-income earners as "winners" and "losers" regardless of treatment. We also vary the difference between the winner's and loser's payoffs, i.e. the dispersion in payments. In the high-dispersion condition, the winner's payoff is 3 times the loser's while in the low-dispersion condition, it is 1.5 times. These variations lead to a 3×2 design. In the *third stage*, we measure prosociality again by means of the PGG and the SVO. In the analysis, we first compare effort and then, changes in prosociality (PGG or SVO) between the first and the third stage across payment schemes and dispersion levels. We also assess the effect of the scheme across winners and losers. The behavior in competitive and random relative to threshold allows us to disentangle the effect of winning against another person from the pure effect of receiving a higher or lower payment by luck. To test for other mechanisms, we also elicit preferences for risk, competition, and inequality aversion pre-experiment.

[1] Of course, the whole production line could also be considered one large team as it is an integrated production system where failure in one setting affects another.

[2] This is comparable to van Dijk et al. (2002).

We find the following. When much is at stake, i.e. the dispersion between the winner's and loser's payoffs is high, competition crowds out prosociality. This is in line with prior findings (e.g. Buser and Dreber, 2015). The effect seems to be mainly driven by those who (1) win the competition (comparable to Schurr and Ritov, 2016; Gee et al., 2016), (2) are more inequality averse, (3) usually work in teams, and (4) are unaware of the existing bonus system. However, when there is less at stake, competition does not affect prosociality. The random payment scheme also seems to crowd out prosociality when the dispersion of payments is high while having no impact when dispersion is low. However, this crowding-out effect is less than in the competitive treatment; at least when it pertains to one of the outcomes, SVO. This suggests that competition does have a distinguishable effect on prosociality from random. Overall, our findings suggest that the impact of competitive schemes (such as relative pay for performance) on workplace cooperation is likely to be context-specific. So, managers should keep the nuances of incentive systems in mind as they consider implementing such schemes in the workplace (as alluded to by for example Holmström, 2017).

Theoretical and lab-experimental evidence suggest that individuals respond to incentives induced by competition. First, in order to win a competition people may (1) sabotage competitors (e.g. Lazear, 1989; Harbring and Irlenbusch, 2011), (2) behave more dishonestly (e.g. Gill et al., 2013; Kilduff et al., 2016), (3) be less trusting/trustworthy (e.g. Dirks and Ferrin, 2003; Keck and Karelaia, 2012), and (4) cooperate and coordinate more (e.g. Bornstein and Erev, 1994; Bornstein et al., 2002). Second, some papers have examined whether exposure to competition or awareness of it could crowd out ex-post prosociality by increasing dishonesty, destructive behavior and individualism, or by affecting moral judgment (e.g. Chen, 2011; Buser and Dreber, 2015; Schurr and Ritov, 2016; Jauernig et al., 2016).[3] Third, a related line of literature argues that the potential downside effects of relative pay for performance depend on organizational transparency (e.g. Ockenfels et al., 2015; Breza et al., 2016; Cullen and Pakzad-Hurson, 2017).

Our main novelties relative to such literature are (1) experimental variation of the strength of competition by having high- and low-dispersion conditions; (2) the field context, which allows us to assess how relevant pre-characteristics or conditions "outside of the lab" impact responses to our treatments; (3) the use of a threshold payment scheme, which enables us to keep the distribution of payments (mean and variance) similar across competitive and non-competitive payments; (4) the pre- and post-elicitation of measures

[3]This has also led to a tangential research agenda exploring the 'thin line' between competition and prosociality (e.g. Savikhin and Sheremeta, 2013; Milkman et al., 2014), as tends to be the case in most naturally-occurring workplace environments.

of prosociality, which allow us to control for baseline differences that could confound the treatment estimates; and (5) the exploration of underlying mechanisms that prior literature has not.

The remainder of the paper proceeds as follows. Section 2 discusses the field context and study design. Section 3 presents the main findings. Finally, Section 4 concludes with some discussion, potential policy implications, and avenues for future work.

2 Study design

2.1 Field context

We recruit workers from a major banana-producing agribusiness in Ghana. The firm is fair-trade certified and exports all of its produce, which constitutes 95% of Ghana's national production of bananas to Europe. Its workforce comprises approximately 1815 men and 230 women, all of whom are employed full-time. Most of the employees complete basic jobs such as bunchcare, harvesting, packaging and quality control.

Banana production is divided into eight sectors. All sectors have the same structure: a field with a cableway system moving the banana bunches to one of eight packing houses. The majority of employees are specialized in a specific job and work in a specific sector. Sectors 1-7 employ 200 to 250 people every day from Monday to Friday. About 45 people are employed on sector 8, where organic bananas are cultivated. The remaining workers are not attached to a specific sector and get assigned based on need every morning. Apart from being assigned to a sector, workers also specialize in a certain type of job such as caring for and harvesting of banana bunches, cutting leafs off the banana trees, and packaging bananas for transport. Workers in several of these jobs – bunchcare, harvesting, and quality control of packaging – report that they regularly work in teams.

In order to foster higher productivity, the firm established a rather complex bonus system that rewards employees when a target production level is reached. Approximately one third of the workers report being unaware of how the existing bonus system works. At the time these experiments were being designed, the firm was considering revising its existing bonus (i.e., relative/competitive payment) scheme. Therefore, the managers gave our research team permission to carry out the experiments with their workers. The results of the study were also presented to them.

2.2 Experimental design

A study session comprised a pre-survey, an experiment with three stages (the crux of the session), and a post-questionnaire as shown in Figure 1. At the beginning of the experiment, we randomly and confidentially matched two participants (i and j). Groups remained fixed throughout the experiment. Instructions were presented stage by stage. In the first stage, we elicited the baseline level of prosociality. In the second stage, participants engaged in an individual real-effort task under one of six treatments with either an individual or a relative payment scheme (as explained in Section 1 and reiterated in Section 2.3). In the third stage, we elicited participants' ex-post level of prosociality using the same measures as in the first stage. We thus assess the change in prosociality from the first to the third stage as a result of being exposed to an individual versus a relative payment scheme. Below we explain the procedures used in each of the stages.

Stage 1: Baseline measures of prosociality

Prosociality was measured through two games: a one-shot public goods game (PGG) and a social value orientation (SVO) game, the order of which was randomized. In the PGG (Figure 2), subjects received an endowment of GHS 10 (represented by 10 paper coins during the task) and had to decide how much to invest in an individual or a joint account (represented by two envelopes). The return on investment in the private account was 1 while the marginal per-capita return from the joint account was 0.7. After making a decision, each subject i was asked to guess the amount the other person j contributed to the group account. Correct guesses earned GHS 2; guesses that deviated by one unit earned GHS 1; and guesses that deviated by two units earned GHS 0.5.

The SVO game (Figure 3) is based on Murphy et al. (2011).[4] Due to time constraints, we used the reduced version in which subjects compare six distinct money allocations for themselves (i) and their partners (j). To calculate the so-called SVO angle, the preferred amounts across the six decision sets are summed up for i and j respectively and then, the inverse tangent of the proportion of the sums is used to determine the angle. The higher the angle, the more altruistic/prosocial a person is. This game was played via the strategy method and accordingly, the role of payoff-allocator (dictator) was randomly assigned to one of the participants (more when discussing information revelation further below).

[4]The original amounts were divided by 12.5 such that incentives were similar across SVO and PGG.

Stage 2: Real-effort task

Subjects completed a real-effort (RE) task in which we exogenously varied the incentives for performance. The task entailed assembling ballpoint pens for eight minutes (Figure 4). Each participant received components for up to 65 ballpoint pens. This task was chosen since it can easily be completed regardless of education level and it is simple to assess quality: A properly functioning (high-quality) pen was one that was able to eject/retract; anything else was of low quality. For purposes of payment, only properly functioning pens were counted.

Stage 3: Ex-post level of prosociality

In the last stage, prosociality was measured again using the same procedures as in Stage 1. The only difference was that the decision sets for the SVO game were presented in a different order to mitigate mere mimicking/repetition of the decisions made previously.

2.3 Treatments and procedures

We implemented a 3×2 between-subjects design with three different payment schemes (threshold, competitive, and random) and two different dispersion levels between winners and losers (high and low). Subject-pairs were randomly assigned to one of the resulting six treatments (Table 1). In the threshold scheme (T), any participant who assembled 40 or more pens correctly (the median output observed during pilot sessions of the competition, high-dispersion treatment) received a high payment while those who did not received a low payment. Recall that we refer to participants who received the *high payment* as winners and those who received the *low payment* as losers (not to be confused with high- and low-dispersion treatments). In the competitive scheme (C), payments were based on relative performance. The subject (in the pair) who assembled most pens correctly won/earned the high payment. Finally, in the random scheme (R), the winner was determined at random. In the high-dispersion treatments (H), the winner and loser received 15 and 5 respectively and in the low-dispersion treatments (L), they received 12 and 8 respectively.

Our experimental design ensures that, in the second stage, winners and losers in the threshold treatments have the same monetary payoffs as winners and losers in the competitive and random treatments. Therefore, conditional on being a winner or loser, differences in behavior across treatments cannot be driven by differential payoffs. Moreover, this design should give rise to similar distributions of payments (mean and variance) across treatments. While we were mainly interested in the differential effect of competitive

versus threshold payments on changes in prosociality, we included the random payment in order to isolate the potential effect of being confronted with another person from the effect of receiving a differential payment.[5] If prosociality were to decrease more in competition than in random, we could attribute such an impact to being exposed to relative payments. If the effect were of similar magnitude, then we could argue that it is not competition that affects prosociality, but rather the inequality it generates.

As mentioned previously, while participants knew that the experiment had different tasks, instructions were presented stage by stage. Information revelation occurred as follows. Subjects were informed that either Stage 1 or Stage 3 and only one of the prosociality games (either PGG or SVO) would be selected at random for payment. If the SVO was selected, the role of dictator and one of the six decisions would also be selected at random (given the strategy method was used). Feedback on these stages (in particular Stage 1) was given only after subjects completed the post-questionnaire such that changes in prosociality were unlikely to be due to endowment, learning, or reputation effects. Participants did receive feedback immediately after the RE task and these earnings were paid with certainty (unlike those for Stages 1 and 3). This was done to enhance the salience of the main treatments, i.e. exposure to different payment schemes.

To further investigate the drivers of changes in prosocial behavior and complement the findings from our treatments, we also had subjects complete a pre-survey. This included questions on (1) basic socioeconomic characteristics, (2) work-related measures such as job satisfaction, and (3) behavioral measures such as social preferences (including inequality aversion), risk and time preferences (à la Charness and Viceisza, 2015), competitive preferences (à la Gneezy et al., 2009), Schwartz-values (à la Schwartz, 1992), and self-esteem. Inequality aversion and competitive preferences were elicited in an incentivized way. As these measures were elicited before the experiment, they can be argued to be exogenous to treatment. We thus use them to further explore the drivers of behavioral change. We also obtained limited administrative data (e.g. job type and sector) from the firm to validate/complement (some of) the work-related measures in the pre-survey.

2.4 Recruitment and sample

The firm provided a listing of its employees. This list included employee names and identification numbers, sector numbers, and the type of job. A sample of employees was randomly selected and assigned to experimental sessions. However, there was imperfect compliance in terms of actual attendance. Employees had to be released by their sector

[5]Like treatments have also been compared to "murky" bonus schemes (e.g. Buser and Dreber, 2015).

supervisors, some of whom were less cooperative. In addition, due to the nature of the tasks, packing-house employees tended to be available during the morning. So, while relatively substantial compared to the population of employees, our sample is not necessarily representative of all sectors and job types across the firm.

In total, we conducted 51 sessions, one in the morning and one in the afternoon on Mondays through Fridays, over the course of five weeks. The sessions were announced as "workshops" and supervisors were informed of selected employees a week in advance in order to release them at a given time. Table 2 shows the number of sessions, individuals, winners, and losers across treatment conditions. Sessions lasted approximately three hours and paid 26.31 Ghanaian cedi/GHS (USD 7), relative to a daily wage equivalent of GHS 18. A total of 619 individuals (589 of whom were men) showed up.

For purposes of internal validity, we run balancing tests across a wide range of pre-characteristics as well as baseline levels of the outcome variables, PGG and SVO. Table 3 contains a select set of variables, in particular those that are significantly different at the 5% level and below. As expected, subjects appear to be significantly different based on some firm/work-related variables such as length of employment, bonus awareness, and the number of other subjects they have "close" relationships with. In addition, subjects appear to be different on age, education, and preferences for risk and competition. Finally, subjects contribute differently to the PGG at baseline across treatments. In the following section, we discuss how our estimation strategy deals with this unbalancedness.

Overall, the average participant is 31 years old, lives in a household with 5 persons (including children), has been employed by the firm for 43 months, and has a close relationship with 1 other person in the session.

2.5 Empirical strategy

Given we collected measures of prosociality in Stages 1 and 3 (i.e., at baseline/pre-treatment and follow-up/post-treatment) and there is evidence of some baseline imbalance, we estimate our treatment effects according to the following specification:

$$\Delta Y_i = \beta_0 + \beta_C C_i + \beta_R R_i + \beta_{Y_0} Y_{i0} + \beta_Z Z_i + \epsilon_i, \tag{1}$$

where ΔY_i is the difference in prosociality between Stages 1 and 3 at the individual level i; C_i and R_i are dummies for individual-level exposure to treatment, competition and random respectively. So, threshold (T_i) is taken as the control. Y_{i0} is the initial level of prosociality in Stage 1; Z_i is a set of covariates comprising the unbalanced characteristics

in Table 3; and ϵ_i is an error term. We run these specifications for both PGG and SVO, for the pooled sample as well as separately for the low- and high-dispersion subsamples.

To further tease apart mechanisms, we expand Equation 1 by adding interactions between the treatment dummies (C_i and R_i) and covariates of interest X_i. Among these covariates are (1) whether or not the subject is a winner (i.e., earned 15 or 12 depending on whether s/he is in the high- or low-dispersion condition); (2) typical behavioral measures such as risk and inequality aversion; (3) preferences for competition (see for example Brandts et al., 2009; Gneezy et al., 2009); and (4) potentially relevant administrative/external variables such as (i) whether or not the subject engages in teamwork (i.e., a more prosocial context) in her/his usual job and (ii) whether or not the subject is aware of the bonus the firm currently has in place.

We thus run the following specification:

$$\Delta Y_i = \beta_0 + \beta_C C_i + \beta_R R_i + \beta_X X_i + \beta_{CX} C_i X_i + \beta_{RX} R_i X_i + \beta_{Y_0} Y_{i0} + \beta_Z Z_i + \epsilon_i, \qquad (2)$$

where all is as defined previously.

2.6 Hypotheses

In lieu of a theoretical framework, we elaborate on the potential mechanisms for our hypothesized effects in the context of existing literature. For example, consistent with Buser and Dreber (2015), we expect the coefficient β_C to be negative indicating a larger decrease in prosociality in the competitive relative to the non-competitive (threshold) payment scheme. We also expect the decrease in prosociality to be more pronounced in the high-dispersion treatments (where winners earn 15 and losers earn 5) than in the low-dispersion treatments (where they earn 12 and 8 respectively). This would be consistent with Lazear (1989), although some of the mechanisms are likely to be different, given sabotage is not possible in our context.

Erkal et al. (2011) and Schurr and Ritov (2016) find that winners of a competition tend to behave in a less prosocial way than losers. Schurr and Ritov (2016) in particular demonstrate that merely remembering the moment of winning a competition is sufficient to increase cheating behavior. The implication for our context is that winning in a competition, thus alters prosocial behavior. So, winners might keep more money for themselves (in PGG or SVO) in Stage 3 if they think that their "superior" (winner) status entitles them to do so. Consistent with this finding, we would expect winners in competition to decrease prosociality more in Stage 3 than winners in threshold, i.e. $\beta_C < 0$.

The coefficient β_R further allows us to pin this down. In the random treatment, the outcome is determined by sheer luck. So, a comparison of the effect of random and competition enables us to assess whether income inequality alone explains the decrease in prosociality. If that were the case, we would expect the effect of competition and random to be similar in magnitude, i.e. $\beta_C \approx \beta_R$. However, if competition generates a feeling of rivalry and confrontation, we would expect the crowding-out effect of prosociality to be larger in competition than in random, i.e. $\beta_C > \beta_R$ in absolute terms. In summary, a significant effect for β_C but not for β_R would be more solid evidence that changes in prosociality are due to competition.

Evoked feelings of rivalry from competition (relative to the threshold treatment) could also lead to a decrease in prosocial behavior. For example, Kilduff et al. (2016) find that increased rivalry is related to "competitors" being more concerned with their status and performance-oriented. Similar mechanisms could be at play here.

Finally, perceived unfairness of the competitive payment scheme could also affect prosocial behavior (e.g Akerlof and Yellen, 1990). Beliefs about unfairness could lead to frustration and anger, which in turn could discourage worker effort and demotivate them to behave prosocially. Subjects might perceive a competitive payment scheme as unfair (relative to threshold), since there is an exclusive bonus that ultimately only one worker in the dyad will benefit from. This perception might be particularly pronounced for those who are (1) more inequality averse (e.g. Bartling et al., 2009); (2) less used to incentive schemes as part of their day-to-day work environment (as proxied by not being aware of the firm's existing bonus system or not being used to working in teams); and (3) more inclined to compete (as proxied by our measure of preferences for competition).

3 Results

3.1 Descriptives

To get a sense of potential unconditional treatment effects, we start with some graphs. Figure 5 looks at the difference in PGG contributions and SVO angle between Stages 1 and 3 across threshold (T), competition (C), and random (R) by low (L) and high (H) dispersion. Two aspects are striking:

1. The bars for T and C typically point in opposite directions. Evidence is somewhat mixed for C versus R.

2. Dispersion seems to matter. The bars for H point in the expected direction, specif-

ically the contributions (and angle) in C *decreased* between Stages 1 and 3 while they *increased* in T. However, the bars for L show the opposite.

Collectively, these findings suggest that competition led to a greater decrease in prosociality across Stages 1 and 3 relative to the threshold treatment, but only when the dispersion between the winner's and loser's payoffs is high (i.e., when there is much at stake). When the dispersion between the winner's and loser's payoffs is low, this effect is reversed. Indeed, statistical tests confirm these findings for PGG (t-test, p-value < 0.1).

Figure 6 shows the distribution of the number of ballpoint pens assembled across treatments. On average, subjects completed about 40 pens. Both, on average and over the whole distribution, there are limited statistically significant impacts across treatments. So, effort appears to be unaffected by the type of payment scheme, regardless of dispersion. Perhaps this is not so surprising when comparing T and C as both treatments create an incentive for higher performance. However, the finding that effort in R (the random treatment) is similar to that in T and C is more striking, given the outcome is determined by sheer luck. That said, this could be because subjects have exerted effort to attend the session or feel observed by the experimenters (and indirectly, the firm) and thus have the need to "do something" while sitting in the session.

As stated in Section 2.4, there are some baseline imbalances across treatments. So, the claims made in this section should be taken with caution. Next, we present conditional effects according to the specifications in Section 2.5.

3.2 Treatment effects

Table 4 presents the estimates of the treatment effects according to the specification in Equation 1. Panel A presents the impacts on changes in PGG and Panel B presents the impacts on changes in SVO. For the sake of brevity, the table does not explicitly report the coefficients for the Z covariates; however, the table footnote lists the covariates that are included when applicable. Results are available from the authors upon request.

In columns (1) and (2), we pool observations across high and low dispersion for the three payment schemes: competition, random, and threshold. The constant term is positive and significant, indicating that in the threshold treatment (the omitted category) there is an increase in prosociality from Stage 1 to Stage 3. The negative and significant effect of the baseline level of prosociality (PGG and SVO) indicates that in Stage 3 the dispersion in prosociality decreases. In other words, individuals with initially low levels of prosociality cooperate more in the third stage than in the first stage, while individuals with initially high levels of prosociality cooperate relatively less. Looking at the pooled

data, competition seems to have no effect on prosociality. To investigate whether these effects may be heterogeneous with respect to high and low dispersion, we run separate regressions (columns 3-6).

Once we disaggregate by high and low dispersion, a different picture emerges. Under high dispersion (columns 3 and 4) there seems to be an increase in prosociality in the threshold treatment from the first to the third stage. Focusing on column (4), which controls for the full spectrum of baseline imbalances, PGG contributions are 16.8% higher post-treatment (although not statistically significant) and the SVO angle increases by 10.99 points post-treatment. However, PGG contributions increase by 5.6% less in the competition than in the threshold between Stages 1 and 3. Similarly, the SVO angle increases by 4.2 degrees (about 38%) less under competition than under threshold. Both of these effects are significant at the 5% level. Prosociality increases less under random than threshold for PGG (by 3.3%), but it increases more for SVO (by 0.5 degrees). These comparisons are not significant. Across competition and random, the coefficients are statistically similar for PGG, but different for SVO – the angle increases by 4.7 points less in competition than in random (significant at the 5% level). These results suggest that competition has a larger crowding-out effect on prosociality than random; at least as far as SVO is concerned. This finding is consistent with the hypothesis that competition indeed can erode prosociality (à la Lazear, 1989; Holmström, 2017).

Under low dispersion (columns 5 and 6), prosociality also increases in the threshold treatment from the first to the third stage. However, contrary to the case of high dispersion, there are no differential changes in prosociality, be it for PGG or SVO, across competition and threshold or random and threshold (once we control for socioeconomic characteristics). The change in PGG or SVO across competition and random is also insignificant. While we expected competition to have less of an impact on prosociality in the presence of low rather than high dispersion, we did not expect this effect to be statistically insignificant. In particular, these results suggest that competition need not always lead to a decrease in prosociality. It depends on the context; notably, how well or badly off the competition leaves winners and losers.

3.3 Mechanisms

To investigate potential mechanisms beyond those that are feasible using only our experimental variations, we first run the specification in Equation 2. Table 5 summarizes the effects for changes in prosociality across treatments for individuals of different character-

istics X under high dispersion.[6] The first, third and fifth columns present the effects for changes in PGG and the second, fourth and sixth columns present the effects for changes in SVO for comparisons between competition and threshold (columns 1 and 2), random and threshold (columns 3 and 4), and these two former comparisons (columns 5 and 6). The results for low dispersion are included in Table 6 in the Appendix.

We find the following under high dispersion:

1. *Winners:* Those who win the competition are less prosocial after having been exposed to the competition than those who win in the threshold treatment. This effect is only significant for changes in PGG contributions. This is consistent with Erkal et al. (2011) who find that winners are more likely to behave in a selfish manner. However, in contrast to Erkal et al. (2011), this does not seem to be due to selection of less prosocial types into the winner position as this specification controls for various individual and social preferences as discussed previously. So, we think this is more likely due to winner-subjects feeling more entitled and thus, believing they deserved their payments more than had they been in the threshold treatment. High earners in the random treatment, however, do not behave less cooperatively relative to high earners in the threshold treatment. This suggests that competition indeed has a distinct effect on winners, which is not only due to higher income. This also relates to Gee et al. (2016) who find that when income is earned through performance, individuals use income differences as a heuristic to infer relative deservingness.

2. *Inequality aversion:* Based on an easy distribution task by Fehr et al. (2008), we classify individuals as inequality averse if they preferred an equal distribution over an unequal distribution in all three questions (and *not* inequality averse otherwise). We find that those who are inequality averse decrease prosociality (PGG) in competition compared with threshold. The effect for SVO is also negative, but not statistically significant. This is consistent with findings from Cherry et al. (2005) who find that contribution levels are significantly lower when groups have heterogeneous (rather than homogeneous) endowments. In the random treatment, we do not observe differences in contributions to PGG or SVO for inequality averse individuals relative to the threshold treatment. So, again, competition seems to trigger a different behavior compared to the random treatment.

3. *Risk seeking:* We find no significant effect of risk preferences on changes in prosocial-

[6]The effects reported in this table are equivalent to $(\beta_C + \beta_{CX}) + (\beta_0 + \beta_X)$ in Equation 2 (aka contrasts, e.g. in Stata).

ity between competition and threshold or across random and threshold, for either PGG or SVO. This is different from for example Teyssier (2012) who finds that risk aversion is significantly and negatively correlated with contributions of first movers; although we recognize that our impacts are identified on changes in prosociality and not contribution levels.

4. *Preferences for competition:* Competitive preferences are measured in the pre-survey with a simple marble game in three stages à la Niederle and Vesterlund (2007). We find that preferences for competition do not explain differences in PGG or SVO across treatments.

5. *Working in teams:* We find that participants who are used to working in teams, reduce prosociality in competition relative to threshold. As before, the effect is only significant for changes in PGG. This finding could imply that the erosion of prosociality may be exacerbated when competition is induced between members of same team rather than between teams. No such effects are observed in the random treatment.[7]

6. *Bonus awareness:* Participants who are unaware of the firm's existing bonus also decrease prosociality in competition relative to threshold and in random compared to threshold. The magnitude of this effect is higher for competition than for random. This suggests that lack of prior exposure to related schemes can increase the negative impacts of newly implemented relative-performance schemes.

Finally, under low dispersion, none of the covariates significantly predict differential behavior across competition and threshold or random and threshold. That said, losers do have a greater (positive) point estimate (albeit statistically insignificant) than winners, possibly suggesting an effect à la Schurr and Ritov (2016).

4 Conclusion

In this study, we conduct a lab-in-the-field experiment with workers from an agribusiness in Ghana to test whether competitive (relative to individual) payment schemes crowd out prosociality in subsequent tasks. We thus partially revisit a question addressed by Buser and Dreber (2015), but in a context where the findings have the potential to immediately

[7]Future work should explore whether inter-team competition has a differential effect on in- versus out-group members.

inform workplace and development policy. In particular, we seek to understand underlying mechanisms by (1) experimentally varying the strength of the competition through the dispersion between the winner's and loser's payoffs and (2) interacting treatment variation with survey covariates as well as external, work-related variables.

When there is much at stake, i.e. when the dispersion between the winner's and loser's payoffs is high, we confirm prior findings: Competition crowds out prosociality. This effect seems to mainly be driven by those who (1) win the competition, (2) are inequality averse, (3) usually work in teams and (4) are unaware of an already existing bonus scheme at the firm. However, when there is less at stake, we find quite the opposite: Competition does not affect prosociality. Closer analysis indicates that prosociality is also eroded under the random payment scheme. However, this erosion (when measured using the social value orientation game) is stronger under competition than under random.

Our results only partly replicate the findings by Buser and Dreber (2015), but are in line with for example Schurr and Ritov (2016) who find that subject's (dis)honesty is impacted by exposure to competitive environments and Drago and Garvey (1998) who find that strong promotion incentives crowd out helping behavior among coworkers. Overall, our findings suggest that the impact of competitive schemes (such as bonuses and merit pay based on relative performance) on cooperation is likely to be context-specific. So, managers should keep the complexities and potential side effects of such incentive systems in mind as they design and implement them in the workplace (as alluded to by for example Lazear, 1989; Holmström, 2017).

Since we report results from a stylized experiment, we need to be cautious in deriving implications for workplace policy. For example, typically coworkers interact frequently and repetitively. They may also get feedback on their performance (say in form of bonus pay) several times during the year. For example, Bandiera et al. (2013) have shown that, in the short run, workers who once won a competition are more motivated to perform on subsequent tasks. However, over a longer term, the bonus incentive may wear off and workers may reduce their effort. Furthermore, the workers who rarely receive a bonus may become dissatisfied with their job, which in turn can have negative consequences for organizational efficiency (e.g. Miceli and Mulvey, 2000) or crowd out intrinsic motivation (e.g. Bowles and Polanía-Reyes, 2012). Finally, incentives may not work as intended because they may interact with the degree of social ties among coworkers (e.g. Ashraf and Bandiera, 2017).

In light of the above, our findings leave some avenues for future research. First, our setting could be extended to include collaborative tasks where strategic complementarities

are important. Second, the effect of different threshold levels on effort and subsequently prosociality could be explored. Third, it would be interesting to look at environments in which individual performance is not perfectly observable and could lead to perceived discrimination (e.g. Grosch and Rau, 2017). Fourth, in lieu of a measure of cooperation and prosociality, future work could look at the impact of different payment incentives on subsequent effort provision/productivity tasks. Fifth, in light of the literature on gender differences in competition (e.g. Gneezy et al., 2003, 2009) and uncertainty aversion (e.g. Croson and Gneezy, 2009), future research could explore the differential impact of competitive payments on cooperation across women and men (we did not have sufficient variation in our sample to shed some light on this issue).

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A Figures

Figure 1: Study session

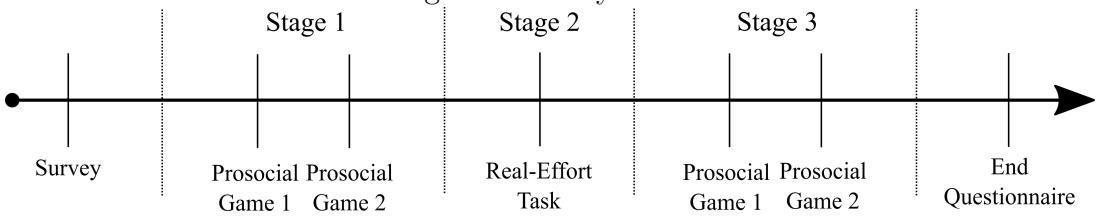
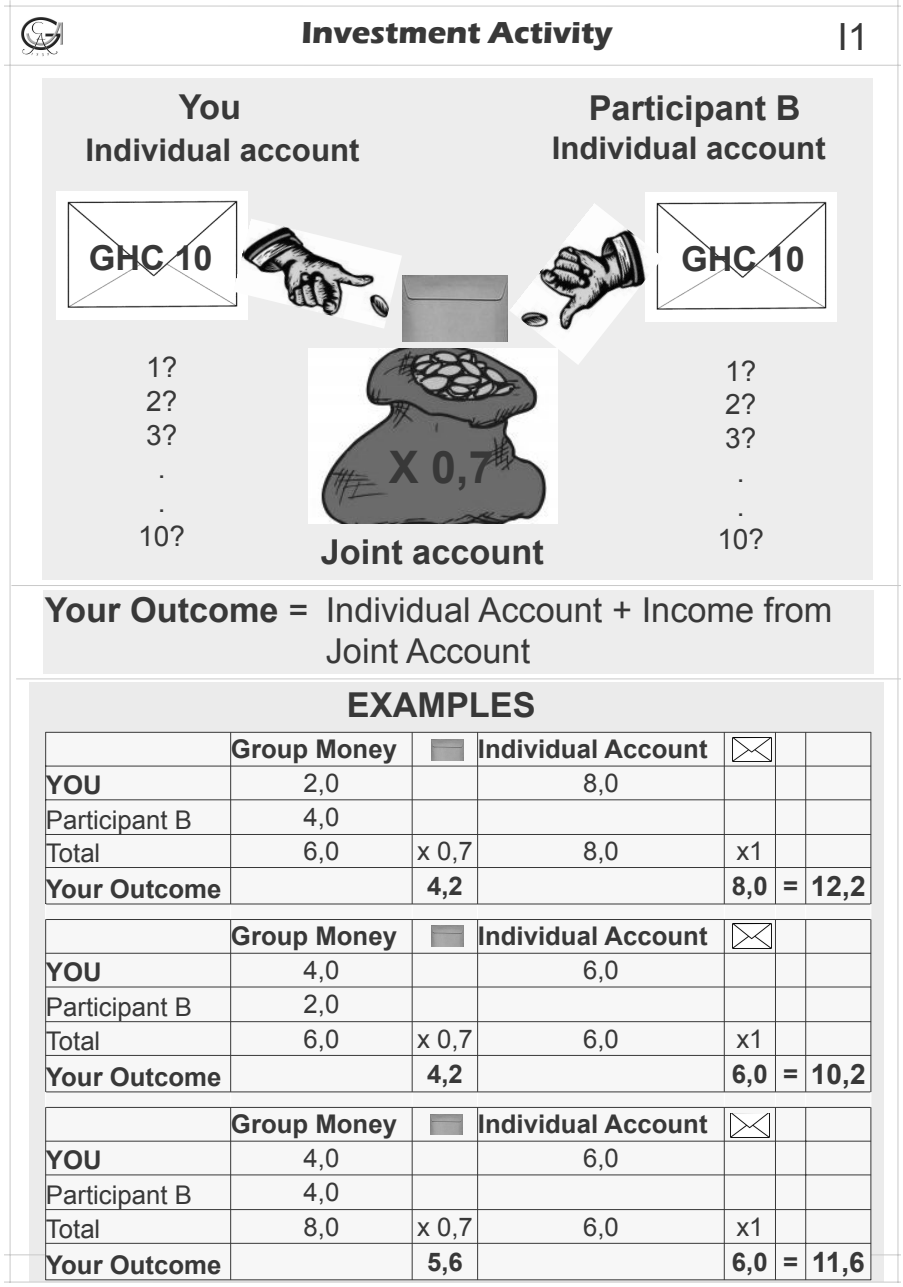


Figure 2: PGG poster



	Group Money		Individual Account			
YOU	2,0		8,0			
Participant B	4,0					
Total	6,0	x 0,7	8,0	x1		
Your Outcome		**4,2**		**8,0**	**=**	**12,2**

	Group Money		Individual Account			
YOU	4,0		6,0			
Participant B	2,0					
Total	6,0	x 0,7	6,0	x1		
Your Outcome		**4,2**		**6,0**	**=**	**10,2**

	Group Money		Individual Account			
YOU	4,0		6,0			
Participant B	4,0					
Total	8,0	x 0,7	6,0	x1		
Your Outcome		**5,6**		**6,0**	**=**	**11,6**

Figure 3: SVO poster

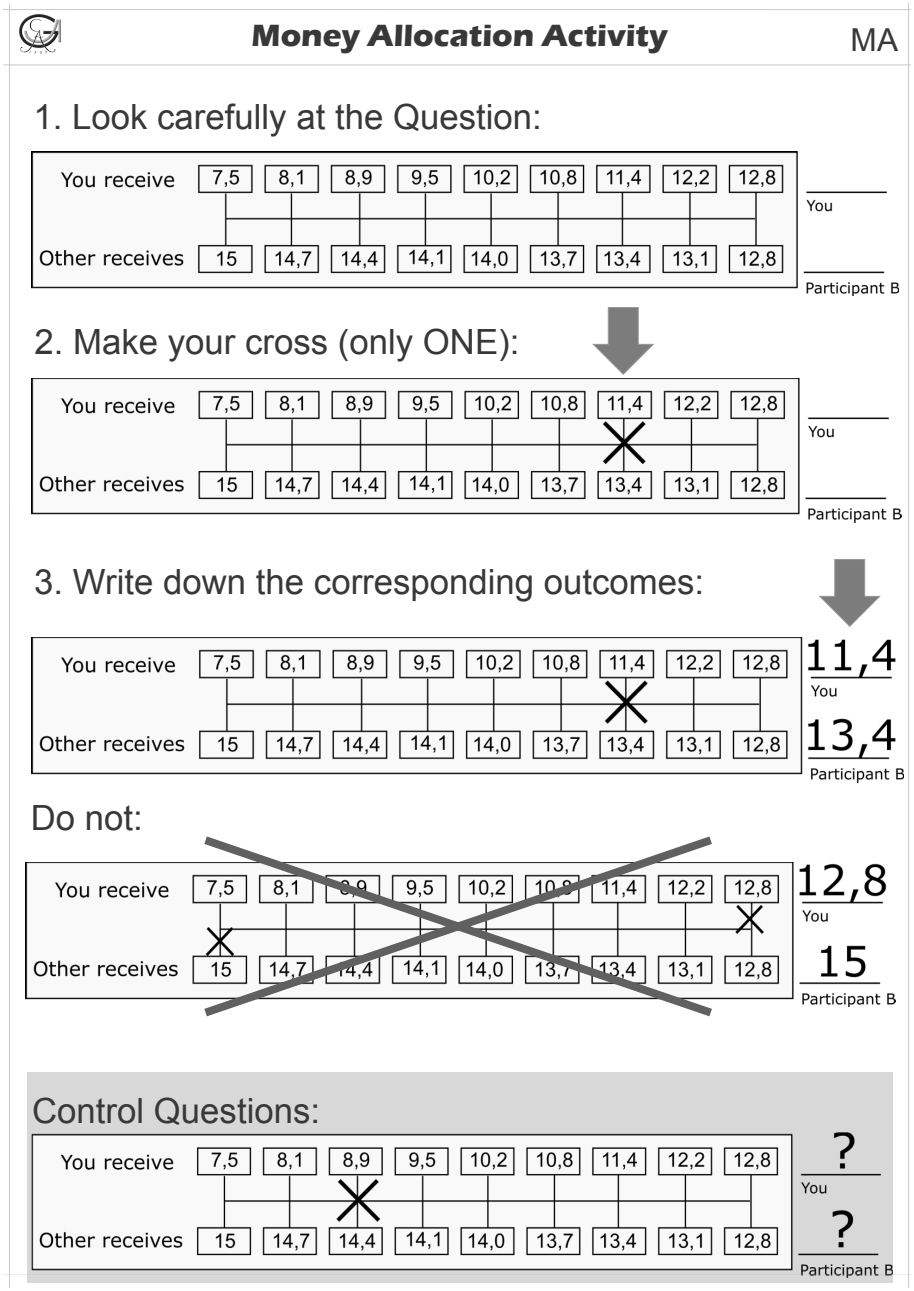


Figure 4: Instruction phase

Figure 5: Changes in PGG contributions (left) and SVO angle (right) across treatments

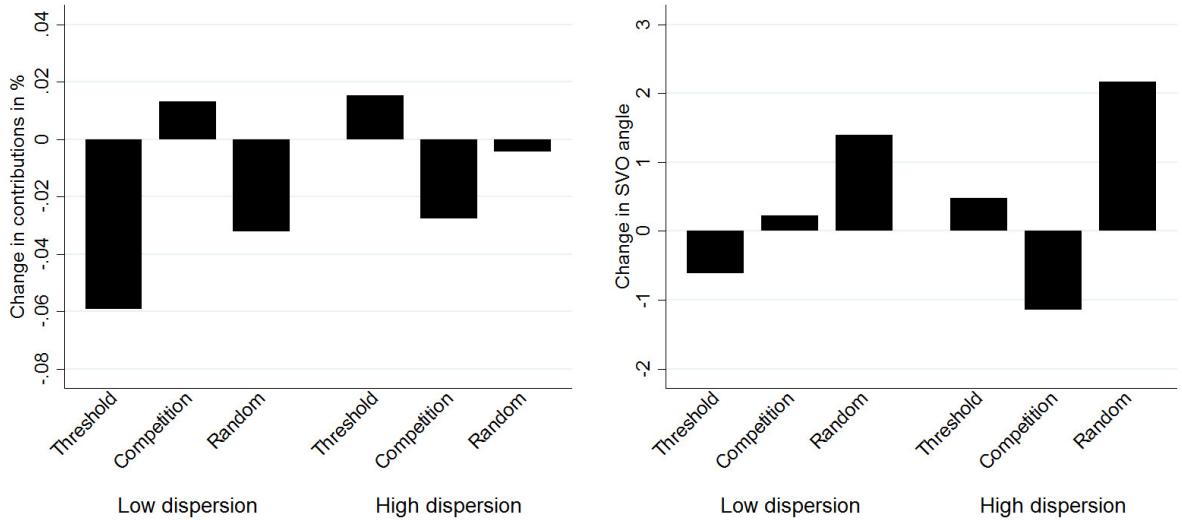
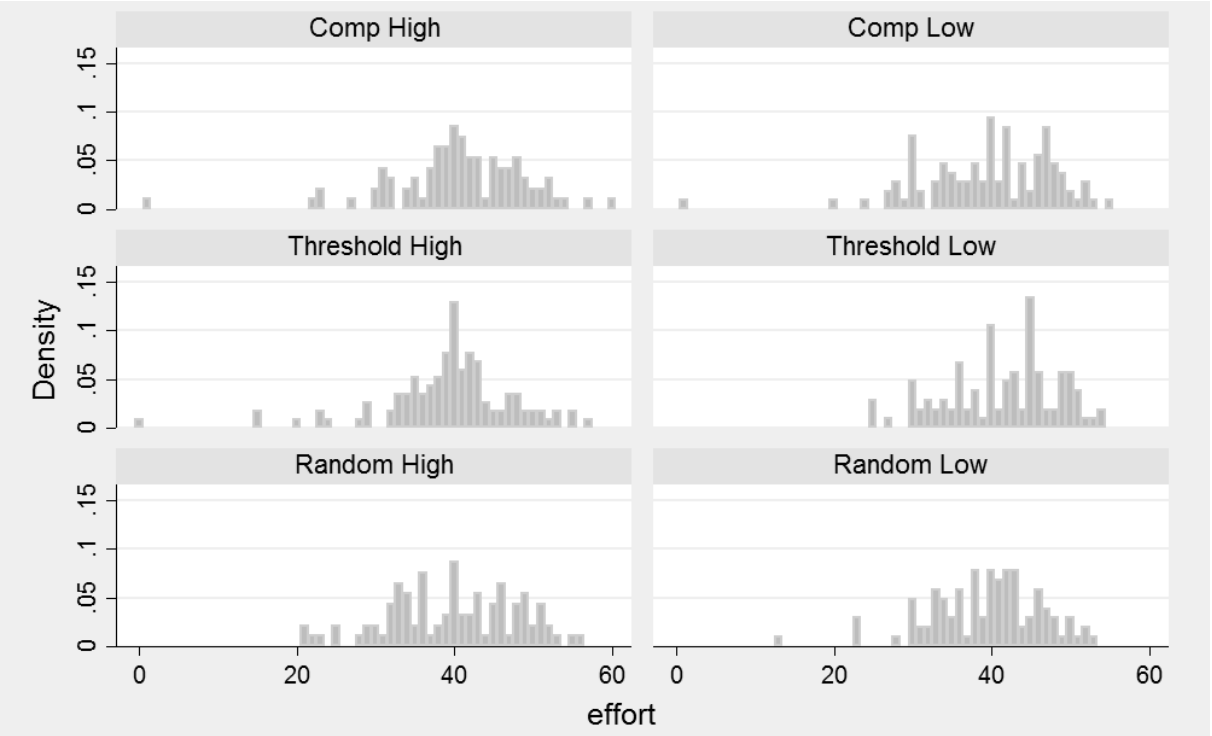


Figure 6: Distribution of effort across treatments



B Tables

Table 1: Experiment treatments

	Competition (C)	Threshold (T)	Random (R)
High (H)	most pens earns 15 other earns 5	≥ 40 pens earns 15 < 40 earns 5	randomly earns 15 other earns 5
Low (L)	most pens earns 12 other earns 8	≥ 40 pens earns 12 < 40 earns 8	randomly earns 12 other earns 8

Table 2: Number of observations

Treatment	Sessions	Individuals	Winner		Loser	
CH	8	94	48	(0.51)*	46	(0.49)*
CL	9	107	50	(0.47)*	57	(0.53)*
TH	10	117	66	(0.56)	51	(0.44)
TL	10	105	69	(0.66)	36	(0.34)
RH	7	93	46	(0.49)	47	(0.51)
RL	7	103	54	(0.52)	49	(0.48)
Total	51	619	333	(0.54)	286	(0.46)

*If the number of subjects in a session was uneven, the "extra" subject was randomly assigned to an existing group.

Table 3: Internal validity balancing tests

Variables	N_i	All	CH	CL	TH	TL	RH	RL	p-value
Demographics									
Age	618	31.26	31.94	30.80	32.92	32.13	29.88	29.62	0.03**
Female	617	0.05	0.10	0.05	0.05	0.07	0.01	0.00	0.02**
Years of schooling	586	9.98	10.07	10.21	9.19	9.80	10.47	10.29	0.00***
Ethnicity[a]	617	2.66	2.67	2.70	2.74	2.51	2.62	2.65	0.71
Marital Status[b]	610	1.52	1.56	1.54	1.46	1.52	1.48	1.53	0.85
HH size	603	5.20	5.91	4.82	5.56	4.92	4.74	5.23	0.06
Poverty[c]	601	1.34	1.40	1.49	1.49	1.32	1.26	1.31	0.40
Behavioral									
Trust[d]	619	0.89	0.95	0.87	0.92	0.89	0.86	0.85	0.22
Fairness[e]	618	0.39	0.34	0.38	0.37	0.40	0.39	0.46	0.67
Risk seeking[f]	606	3.88	3.41	3.70	3.37	3.86	4.59	4.43	0.00**
Inequality averse[g]	619	2.51	2.70	2.35	2.75	2.44	2.37	2.42	0.18
Time[h]	605	202.20	191.68	186.75	157.57	192.07	184.13	300.70	0.26
Competition[i]	619	0.62	0.46	0.70	0.52	0.62	0.71	0.73	0.00**
Schwartz values[j]									
Benevolence	617	4.53	4.39	4.61	4.50	4.51	4.66	4.48	0.07
Conformity	618	4.57	4.54	4.64	4.60	4.55	4.53	4.51	0.67
Collectivism	619	0.73	0.62	0.73	0.71	0.78	0.75	0.76	0.15
Firm-related									
Months employed	611	42.60	41.18	45.74	49.41	45.17	42.75	30.20	0.00***
Monthly wage	610	374.19	377.49	390.38	373.99	369.43	365.85	366.87	0.18
Aware of bonus	619	0.69	0.46	0.72	0.62	0.71	0.77	0.82	0.00***
Works in team	614	0.45	0.55	0.45	0.51	0.38	0.46	0.36	0.06
Job satisfaction[k]	615	4.50	4.33	4.60	4.51	4.38	4.64	4.56	0.00***
Job type	606	4.79	4.61	4.92	4.56	4.89	5.39	4.44	0.16
Sector	602	4.64	4.89	4.49	4.26	4.97	4.96	4.41	0.08
Close relations[l]	615	1.18	1.16	1.14	1.53	1.24	0.58	1.30	0.00***
Outcomes									
PGG (stage 1)	619	0.49	0.47	0.46	0.50	0.53	0.45	0.52	0.04**
SVO (stage 1)	619	21.82	20.95	22.78	22.57	22.76	18.67	22.62	0.09
PGG (stage 3)	619	0.47	0.44	0.47	0.51	0.47	0.44	0.48	0.13
SVO (stage 3)	619	22.23	19.80	23.01	23.04	22.16	20.84	24.02	0.16

*** $p < 0.01$, ** $p < 0.05$. The last column is obtained by running a one-way ANOVA test. These values are also robust to running a seemingly unrelated regression model for continuous variables and a χ^2-test for categorical variables. The tests for baseline equivalence of outcomes (PGG and SVO) are additionally robust to a Wilcoxon rank-sum test.

Variable definitions (see questionnaires for additional detail): [a] 1=Akan, 2=Ewe, 3=Ga/Dangbe, 4=Krobo, 5=Hausa; [b] 1=married, 2=single, 3=separated, 4=divorced, 5=widowed; [c] number of adults per bedroom in the home; [d] 0=most people can be trusted, 1=need to be very careful trusting; [e] 0=most people take advantage, 1=most people try to be fair; [f] number of seeds out of 10 chosen that are risky; [g] based on payoff equalization or not (aka Fehr allocation activity); [h] average GHS needed in one month to sacrifice 100 GHS tomorrow; [i] based on choice to be paid relative to someone else (compete) in a marble activity; [j] based on Schwartz (1992); [k] 1=terrible, 2=unhappy, 3=mixed, 4=mostly satisfied, 5=pleased; [l] number of people known during experiment session.

Table 4: Treatment effects on change in PGG and SVO (pooled, high, low dispersion)

	(1) Pooled	(2) Pooled	(3) High	(4) High	(5) Low	(6) Low
Panel A: Δ PGG contributions						
Competition (C)	-0.0177	-0.0113	-0.0768**	-0.0561**	0.0409	0.0580
	(0.0230)	(0.0220)	(0.0276)	(0.0216)	(0.0290)	(0.0348)
Random (R)	-0.0180	0.00156	-0.0534**	-0.0325	0.0220	0.0591
	(0.0208)	(0.0227)	(0.0208)	(0.0193)	(0.0306)	(0.0388)
Baseline PGG (Y_{i0})	-0.447***	-0.458***	-0.422***	-0.427***	-0.462***	-0.476***
	(0.0454)	(0.0460)	(0.0665)	(0.0705)	(0.0633)	(0.0592)
Constant	0.213***	0.180**	0.234***	0.168	0.184***	-0.144
	(0.0266)	(0.0757)	(0.0394)	(0.108)	(0.0326)	(0.105)
R-squared	0.218	0.242	0.226	0.308	0.237	0.320
C vs. R	0.0002	-0.0129	-0.0234	-0.0236	0.0189	-0.0010
	(0.0219)	(0.0196)	(0.0265)	(0.0248)	(0.0310)	(0.0233)
Panel B: Δ SVO angle						
Competition	-1.060	-1.040	-3.586**	-4.199**	1.292	1.037
	(1.103)	(1.108)	(1.409)	(1.854)	(1.543)	(2.019)
Random	1.489	0.921	-0.151	0.490	3.114*	2.821
	(1.004)	(1.233)	(1.189)	(1.201)	(1.604)	(2.227)
Baseline SVO (Y_{i0})	-0.661***	-0.652***	-0.655***	-0.657***	-0.680***	-0.635***
	(0.0498)	(0.0507)	(0.0687)	(0.0741)	(0.0740)	(0.0736)
Constant	14.86***	12.87**	15.44***	10.99**	14.45***	22.82**
	(1.368)	(5.123)	(1.787)	(5.048)	(2.155)	(8.972)
R-squared	0.325	0.347	0.302	0.341	0.358	0.406
C vs. R	-2.550**	-1.961	-3.435**	-4.689**	-1.822	-0.001
	(1.165)	(1.212)	(1.510)	(1.915)	(1.631)	(0.023)
Observations	539	539	262	262	277	277
Covariates[a]	NO	YES	NO	YES	NO	YES

[+] Robust standard errors clustered at the session level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
[a] Covariates: age, female, education, risk, inequality averse, poverty, competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

Table 5: Heterogeneous effects on change in PGG and SVO (high dispersion)[a]

	(C vs. T)		(R vs. T)		(C vs. T) vs. (R vs. T)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ PGG	Δ SVO	Δ PGG	Δ SVO	Δ PGG	Δ SVO
Income effect						
loser	-0.0239	-4.3625	-0.0173	-6.8019	-0.0066	2.4394***
	(0.0562)	(3.3742)	(0.0579)	(3.6976)	(0.006)	(0.373)
winner	-0.1309***	-4.0296	-0.0419	-1.5370	-0.0890***	-2.4926***
	(0.0403)	(2.8920)	(0.0389)	(1.6196)	(0.004)	(0.246)
Behavioral variables						
not inequality averse	-0.0335	-3.7620	-0.0065	-4.9172	-0.0270***	1.1552***
	(0.0322)	(3.2476)	(0.0337)	(3.6099)	(0.003)	(0.362)
inequality averse	-0.1214*	-4.6301	-0.0527	-3.4217	-0.0687***	-1.2084***
	(0.0682)	(3.4913)	(0.0603)	(2.7935)	(0.007)	(0.332)
risk seeking	0.0028	0.0439	-0.0075	1.1677	0.0103***	-1.1238***
	(0.0087)	(0.5227)	(0.0087)	(0.5078)	(0.001)	(0.054)
dislikes competition	-0.0871	-3.3085	-0.0411	-3.4614	-0.0460***	0.1529
	(0.0543)	(3.3379)	(0.0411)	(2.4085)	(0.005)	(0.306)
likes competition	-0.0676	-5.0836	-0.0182	-4.8775	-0.0494***	-0.2061
	(0.0441)	(3.3031)	(0.0438)	(3.4188)	(0.005)	(0.354)
Work-related variables						
does not work in teams	-0.0411	-4.5677	-0.0467	-1.1657	0.0056	-3.4020***
	(0.0373)	(3.1905)	(0.0326)	(2.3815)	(0.004)	(0.296)
works in teams	-0.1138*	-3.8243	-0.0125	-7.1732	-0.1013***	3.3489***
	(0.0617)	(3.2144)	(0.0602)	(3.4140)	(0.006)	(0.349)
is not aware of bonus	-0.1081**	-6.4739**	-0.0930**	-3.8571	-0.0151***	-2.6168***
	(0.0489)	(3.0383)	(0.0396)	(1.9608)	(0.005)	(0.268)
is aware of bonus	-0.0466	-0.9182	0.0338	-4.4818	-0.0804***	3.5636***
	(0.0455)	(3.3706)	(0.0544)	(3.5504)	(0.005)	(0.365)
R-squared	0.3476	0.3710	0.3476	0.3710		
Observations	262	262	262	262		
Covariates[b]	YES	YES	YES	YES		

[+] Robust standard errors clustered at the session level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
[a] This table presents contrasts across C and T in columns (1) and (2), i.e., $(\beta_C + \beta_{CX}) + (\beta_0 + \beta_X)$ as discussed in Section 3.3. Contrasts across R and T are shown in columns (3) and (4). In columns (5) and (6), we apply a t-test to examine significant differences between results from column (1) vs. column (3) and column (2) vs. column (4) respectively.
[b] Covariates: baseline PGG or SVO, age, female, education, risk, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

C Appendix

Table 6: Heterogeneous effects on change in PGG and SVO (low dispersion)[a]

	(C vs. T)		(R vs. T)		(C vs. T)	vs. (R vs. T)
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ PGG	Δ SVO	Δ PGG	Δ SVO	Δ PGG	Δ SVO
Income effect						
loser	0.0646	0.5790	-0.0590	2.0844	0.1236***	-1.5054***
	(0.0859)	(4.1416)	(0.0814)	(6.1982)	(0.009)	(0.551)
winner	-0.0220	0.1261	-0.0867	1.2186	0.0647***	-1.0925**
	(0.0774)	(2.9798)	(0.0745)	(4.9874)	(0.008)	(0.430)
Behavioral variables						
not inequality averse	0.0022	-0.8651	-0.0566	-0.1196	0.0588***	-0.7455*
	(0.0715)	(3.3825)	(0.0729)	(4.7944)	(0.008)	(0.434)
inequality averse	0.0404	1.5702	-0.0892	3.4226	0.1296***	-1.8524**
	(0.0979)	(4.2841)	(0.0904)	(6.4010)	(0.010)	(0.570)
risk seeking	0.0143	0.4117	0.0230*	0.5119	-0.0087***	-0.1002**
	(0.0147)	(0.3598)	(0.0118)	(0.5250)	(0.001)	(0.047)
dislikes competition	0.0238	-0.8102	-0.0493	-0.3645	0.0731***	-0.4457
	(0.0858)	(3.6660)	(0.0927)	(6.2201)	(0.009)	(0.534)
likes competition	0.0188	1.5153	-0.0965	3.6675	0.1153***	-2.1522***
	(0.0808)	(3.9717)	(0.0745)	(5.0919)	(0.008)	(0.477)
Work-related variables						
does not work in teams	-0.0005	-0.2476	-0.0696	1.8968	0.0691***	-2.1444***
	(0.0713)	(4.0153)	(0.0782)	(6.1735)	(0.008)	(0.545)
works in teams	0.0431	0.9527	-0.0762	1.4061	0.1193***	-0.4534
	(0.0929)	(3.8317)	(0.0870)	(5.3102)	(0.009)	(0.484)
is not aware of bonus	0.0403	1.8906	-0.1398	3.9170	0.1801***	-2.0264**
	(0.0895)	(5.1206)	(0.0918)	(7.5238)	(0.009)	(0.673)
is aware of bonus	0.0023	-1.1855	-0.0060	-0.6140	0.0083	-0.5715
	(0.0823)	(3.2735)	(0.0823)	(3.7850)	(0.009)	(0.370)
R-squared	0.3633	0.4152	0.3633	0.4152		
Observations	277	277	277	277		
Covariates[b]	YES	YES	YES	YES		

[+] Robust standard errors clustered at the session level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
[a] This table presents contrasts across C and T, i.e., $(\beta_C + \beta_{CX}) + (\beta_0 + \beta_X)$ as discussed in Section 3.3. Contrasts across R and T are shown in columns (3) and (4). In columns (5) and (6), we apply a t-test to examine significant differences between results from column (1) vs. column (3) and column (2) vs. column (4) respectively.
[b] Covariates: baseline PGG or SVO, age, female, education, risk, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.