# Competition and prosociality: A field experiment in Ghana

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**Abstract**

Competitive payment mechanisms are commonly used to promote higher productivity in the workplace. Yet, these types of incentives could negatively affect the quality of co-workers' relations, thus reducing their willingness to cooperate in subsequent tasks. In this paper, we explore this question by conducting a lab-in-the-field experiment with workers from an agribusiness in Ghana that is interested in establishing a competitive payment scheme. To investigate how prosociality towards a co-worker evolves, we use a between-subjects design where participants complete a real-effort task under a competitive, threshold, or random payment. We measure prosociality before and after this task through (1) a public goods and (2) a social value orientation game. We find the effect of competition on changes in prosociality to be context-specific. Compared to other payment schemes, competition reduces prosociality when the dispersion of payments is high, an effect that is driven by those who (1) win the competition; (2) are more inequality averse; (3) are used to working in teams; and (4) are not aware of existing bonus incentives in the workplace. However, when there is less at stake, competition does not affect prosociality. Finally, performance is similar across payment schemes. Our findings suggest that managers should keep dispersion of payments (i.e. strength of the competition) and worker types in mind when contemplating whether or not to introduce relative performance schemes in the workplace, particularly in development-field contexts.

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# 1 Introduction

The success of an organization critically depends on social relations among co-workers (Beal et al., 2003). Good interpersonal relations are associated with more willingness of people within an organization to help each other, share information and cooperate in joint projects (Brief and Motowidlo, 1986). In addition, good interpersonal relations facilitate communication, enhance employee commitment, foster individual learning, strengthen relationships and result in improved organizational performance (Leana and Van Buren, 1999; Adler and Kwon, 2002; Bright et al., 2006). Employees report higher job satisfaction, organizational commitment, and less absenteeism when they perceive that co-workers are friendly (Rhoades and Eisenberger, 2002). Hence, managers should try to create and maintain a more friendly working environment.

Despite the importance of workplace relations, managers are also under extreme pressure to increase individual productivity and thus, turn to incentives for boosting performance. Relative payment schemes, where workers compete for a bonus, are a very common instrument used in the workplace. Both theoretical and empirical literatures suggest that competitive payment schemes can increase effort and productivity (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 not clear if such type of incentives could negatively affect the ex-post quality of coworker relationships. In this paper, we address this question by investigating the effects that competitive payment schemes have on *subsequent* prosocial attitudes between coworkers.

Our main hypothesis is that competitive payment schemes generate a negative effect on the quality of coworker relationships. There are various channels that might explain 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 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 enter a competition (e.g. Bartling et al., 2009). Compelled to work under a competitive payment scheme, these workers might 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). Especially workers usually exposed to teamwork might perceive an individual bonus scheme as unfair. This might lead to frustration and less prosocial actions.

To test the effect of competition on prosociality, we conducted 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 for this question for three reasons. First, at the time the experiments were being designed, the firm was considering introducing a competitive bonus system with relative incentives. Second, a non-competitive bonus system was already in place; however, our baseline (pre) survey data suggested that about one third of the employees were unaware of this. Third, teamwork is important for the performance of the firm. In fact, about 50% of the tasks (e.g. bunchcare, harvesting, and quality control in packaging) rely on teams. So, to test the potential effectiveness of a competitive bonus system and evaluate the potential side effects on (un)cooperative behavior among team members, the firm permitted us to do an experiment with a sample of their workers.

In our experimental design, we randomly and confidentially match two participants for the duration of a three-stage experiment.[1] 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 eliminate income or reputational effects, feedback on the outcomes of the first stage is not provided until after the third stage. In the second stage, participants complete a real-effort/output task in which they individually assemble ballpoint pens. We implement a between-subjects design where 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. The second dimension that we vary in our design is the difference between the winner's and loser's payoffs (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. This gives rise to a 3×2 design. Finally, in the third stage, we measure prosociality again by means of the PGG and the SVO. In the analysis, we thus compare changes in prosociality (PGG or SVO) between the first and the third stage across the payment schemes and dispersion levels.

Since all treatments have a high-income earner, which we refer to as "winner", and

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

a low-income earner, which we refer to as "loser", we can further compare the effect of the payment scheme on each group (winners and losers) and also across dispersion levels. Moreover, the comparison of behavior in the competitive and the random payment schemes relative to the threshold payment scheme allows us to disentangle the effect of "entitlement" (winning against another person) and "self-efficacy" (winning by independently achieving a goal). To test for additional, potential mechanisms underlying changes in prosociality, we also elicit preferences for risk, competition, and inequality aversion prior to the experiment. Moreover, we elicit perceived fairness of the payment schemes as well as feelings of rivalry and attachment towards coworkers through a post-experiment survey.

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 confirms prior lab findings. The effect seems to mainly be driven by (1) those who win the competition (comparable to Erkal et al., 2011), (2) those who are more inequality averse, (3) those who usually work in teams, and (4) those who are not aware of the existing bonus system. However, when there is less at stake, we find that competition does not affect prosociality. Overall, our findings suggest that the impact of competitive schemes (such as bonuses and merit pay based on relative performance) on workplace cooperation is likely to be context-specific. So, overall managers should keep the complexities of incentive systems in mind as they consider implementing such schemes in the workplace (as alluded to by for example Lazear, 1989; Holmström, 2017).

Theoretical and in particular lab-experimental evidence suggest that individuals respond to the incentives induced by competition. Hence, in order to win a competition people might either sabotage competitors, behave more dishonestly, be less trustworthy, cooperate, coordinate and trust more (e.g. Lazear, 1989; Bornstein and Erev, 1994; Bornstein et al., 2002; Dirks and Ferrin, 2003; Harbring and Irlenbusch, 2011; Keck and Karelaia, 2012; Gill et al., 2013). Apart from these incentive effects, 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).[2]

Our main novelties relative to previous literature are (1) the pre- and post-elicitation of measures of prosociality, which enable us to control for baseline differences that may

[2]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.

confound the treatment estimates; (2) the use of a threshold payment scheme, which enables us to keep the distribution of payments (mean and variance) similar between competitive and non-competitive payments; (3) experimental variation in the strength of competition by having high- and low-dispersion conditions; and (4) the field context, which allows us to assess how relevant pre-characteristics or conditions outside "the lab" impact response to treatment. We thus seek to understand underlying mechanisms that previous literature has not, particularly in a labor-intensive context and industry where there is intense global competition. So, our results could have immediate implications for workplace and development policy.

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 and potential policy implications.

# 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 plantation 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.

## 2.2 Experiments and surveys

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. Sessions lasted approximately three hours and paid 26.31 Ghanaian cedi/GHS (USD 7), relative to a daily wage equivalent of GHS 18. In what follows, we first discuss the experiment and then elaborate on the surveys. Complete instructions and survey instruments are available at the authors' websites as online appendices.

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 (more below). 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.

### 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 normalized to 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).[3] 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

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

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).

**Stage 2: Real-effort task and differential payment schemes**

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. Each participant received components for up to 65 ballpoint pens. This task was chosen since 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.[4] For purposes of payment, only properly functioning pens were counted.

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. 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 and 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 this 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

[4]We did not choose a RE task that entails teamwork. While this could have enabled us to study prosociality during the main task in the presence of competition (simultaneously, if you will), we wanted to disentangle the two for ease of interpretation. In this sense, the experiments test the impact of a competitive payment scheme on prosociality in a *subsequent* task.

scheme in order to isolate the potential entitlement effect from merely winning.[5] In other words, one might expect limited to no differential impacts across the random and threshold treatments on changes in prosociality, since payment does not depend on relative performance in either instance.

### Stage 3: Ex-post level of prosociality

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.

### Information revelation, other procedures, and surveys

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 were 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). The latter was done to enhance the salience of the main treatments, i.e. exposure to the payment schemes.

To further investigate the drivers of changes in prosocial behavior and complement the findings from our experiment 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.

Prior to departure, subjects also completed a post-questionnaire. This included perceptions on (1) fairness of the payment scheme, (2) feelings of rivalry, and (3) attachment

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

to one's partner. As these measures were collected after exposure to treatment, we use them as complementary outcome measures to further assess the mechanisms by which differential payment schemes impact behavior.

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.

## 2.3 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 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. 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 will 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.

## 2.4 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 $\Delta Y_i$ 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 $\epsilon_i$ 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 potentially relevant administrative/external variables such as (4) whether or not the subject engages in teamwork (i.e. a more prosocial context) in her/his usual job and (5) whether or not the subject is aware of the bonus the firm currently has in place.

We thus run the following specification:

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

where all is as defined previously.

Finally, we examine whether the payment schemes differentially impact perceptions of fairness and feelings of rivalry or attachment towards coworkers. Also, these perceptions may be affected more vigorously for workers/subjects with specific characteristics such as those who are more inequality averse.

We thus run an additional specification that is identical to equation 2 except for the outcome variables:

$$E_i = \beta_0 + \beta_C C_i + \beta_R R_i + \beta_X X_i + \beta_{CX} C_i X_i + \beta_{RX} R_i X_i + \beta_{Y_0} Y_{i0} + \beta_Z Z_i + \epsilon_i, \tag{3}$$

where $E_i$ is either the measure of fairness, rivalry, or attachment collected in the post-questionnaire (recall Section 2.2) and all else is as defined previously.

## 2.5 Hypotheses

In line with prior literature (e.g. Buser and Dreber, 2015), we expect the coefficient $\beta_C$ 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). In lieu of a theoretical framework, we elaborate on the potential mechanisms for these hypothesized effects in the context of existing literature.

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 a feeling of "entitlement" ensues from winning against another person, thus altering 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.

To disentangle the effect of "self-efficacy" (i.e. one's ability to achieve a goal independently, see for example Bandura and Locke, 2003) from that of "entitlement" (i.e. achieving a goal relative to another person), we can compare winners across the competition and threshold treatments. If entitlement indeed is a major factor, we would expect winners in the competitive payment scheme to decrease prosociality more in Stage 3 than winners in the non-competitive (threshold) payment scheme, i.e. $\beta_C < 0$.

The coefficient $\beta_R$ further allows us to pin this down. The notion of "self-efficacy" is at play in both the competitive and threshold treatments. However, it is in principle ruled out in the random treatment where the outcome is determined by sheer luck. So, a comparison across the random and threshold treatments enables us to assess if self-efficacy in isolation triggers a change in prosocial behavior. Specifically, a significant effect for $\beta_C$ but not for $\beta_R$ is more solid evidence that changes in prosociality are due to competition, and its ensuing sense of entitlement towards a coworker, rather than self-efficacy.

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 (1) concerned with their status and (2) 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 discourage worker effort and demotivate them to behave prosocially. Subjects might perceive a competitive payment scheme as unfair (relative to the threshold payment), since there is an exclusive bonus at play that ultimately only one worker in the dyad will benefit from directly. 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. Figures 4 and 5 look at the difference in PGG contributions and SVO angle between Stages 1 and 3 across threshold ($T$), competition ($C$), and random ($R$); low ($L$) and high ($H$) dispersion; and losers and winners (see panels B and C). A few aspects are striking:

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

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

3. The bars for $H$ resemble those for winners (right-hand side of panel C) and the bars for $L$ resemble those for losers (left-hand side of panel B).

Collectively, these findings suggest that competition led to a greater decrease in prosociality across Stages 1 and 3 relative to the other treatments, but only when the dispersion between the winner's and loser's payoffs is high (i.e. when there is much at stake). Further, these effects seem to be driven by whether or not one wins the competition. Indeed, statistical tests confirm these findings as we shall see when we present the conditional regression estimates.

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 2.3, 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 2.4.

## 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. This finding is thus consistent with the hypothesis that competition indeed can erode prosociality (à la Lazear, 1989; Buser and Dreber, 2015; Holmström, 2017).

Under low dispersion (columns 5 and 6), prosociality may also increase 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 PGG or SVO, across the competitive and threshold treatments. While we expected that competition under low dispersion would have less of an impact on prosociality relative to high dispersion, we did not expect this effect to be statistically insignificant. These results thus suggest that competition may not always lead to a decrease in prosociality. It depends on the context; notably, how well/badly off the competition leaves winners relative to 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 competition and threshold for individuals of different characteristics $X$ under high dispersion. In other words, 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). The first column presents the effects for changes in PGG contribution and the second column presents the effects for changes in the SVO angle. The results for low dispersion are included as Table 7 in Appendix C.

As described in Section 2.5, we further examine if select characteristics affect subjects' perceptions of fairness of the payment scheme as well as rivalry and attachment towards coworkers. These results are presented in Table 6 across columns (1), (2), and (3) respectively.[6] Since we do not find significant effects for low dispersion, we only present results for high dispersion (the other results are available from the authors upon request). Generally, Table 6 reveals that workers exposed to competition, perceive the payment as less fair than workers in threshold. Interestingly, exposure to competition does not have

[6]We ran the model specification 3 as described in Section 2.4. However, for the sake of brevity, we only present the variables and interaction terms of interest. In particular, we do not report interaction effects for the random treatment or the coefficients for the full list of covariates. For the outcome variable *attachment*, one observation is missing because the question was skipped when completing the survey.

a significant effect on the perceived rivalry or attachment towards the subject-coworker.

More specifically, 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 all of the $X$ and $Z$ variables 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. 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. Regression results also point in that direction as winners from the competition report higher perceived fairness of their pay compared to winners in the threshold (Table 6).

2. *Inequality aversion:* Based on an easy distribution task from (Fehr et al., 2008), we classify individuals as inequality averse when they preferred the equal distribution over the 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. The effect from inequality aversion does not seem to be mediated by differential, perceived fairness of the competitive payment scheme (Table 6). However, compared to the threshold treatment, inequality averse subjects felt less rivalry and attachment towards the subject-coworker.

3. *Risk seeking:* We find no significant effect of risk preferences on changes in prosociality between competition and threshold, for neither PGG nor 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:* We find that those who like competition (i.e. self-select into competitive environments based on a separate task) behave no differently in

PGG or SVO across competition and threshold. Moreover, disliking competition also has no significant heterogeneous effect on changes in prosociality.

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. Future work should explore whether inter-team competition has a differential effect on in- versus out-group members. We do not find any heterogeneous effects on perceived fairness of the payment scheme or rivalry and attachment.

6. *Bonus awareness:* Participants who are unaware of the firm's existing bonus also decrease prosociality in the competition compared to the threshold. The effect is similar for PGG and SVO. 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. 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). These results are reported in Table 7 in Appendix C.

# 4 Conclusion

In this study, we conduct lab-in-the-field experiments with workers from an agribusiness in Ghana to test whether competitive (relative to individual threshold) payment schemes crowd out prosociality (cooperation and concern for another person's payoff) in subsequent tasks. We thus revisit a question that has been addressed in pure lab environments (e.g. Buser and Dreber, 2015), in a context where the findings have the potential to immediately inform workplace and development policy. We also 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 lab findings: Competition crowds out prosociality. This effect seems to be mainly driven by (1) those who win the competition (comparable to

Schurr and Ritov, 2016), (2) those who are inequality averse, (3) those who usually work in teams and (4) those who are unaware of an 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 when the level of competition is low (i.e. when the dispersion between the winner's and loser's payoffs is low), subjects' willingness to be prosocial seems unaffected. However, when subjects are paid individually for their performance in the threshold treatment, they are less reluctant to cooperate with others and thus, appear less prosocial than in the competitive environment. This result could behaviorally be related to Schurr and Ritov (2016) who find that subject's (dis)honesty is impacted by exposure to competitive environments. Furthermore, an empirical study from Drago and Garvey (1998) demonstrates that strong promotion incentives can 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 in the workplace is likely to be context-specific. When there is much to gain from competition, prosociality tends to be crowded out. However, when there is little to gain, prosociality need not be crowded out. These effects seem to be mediated by factors such as whether the worker wins the competition, her or his type (notably, inequality aversion), and work-related factors such as whether the worker is used to teamwork or related incentive schemes. So, overall managers should keep the complexities of incentive systems generally and these indicators in particular in mind as they consider implementing such schemes in the workplace (as alluded to by for example Lazear, 1989; Holmström, 2017).

# 5 Acknowledgments

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# A Figures

Figure 1: Study session

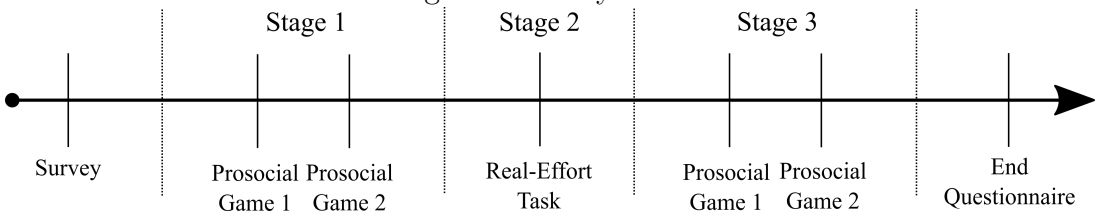


Figure 2: PGG poster

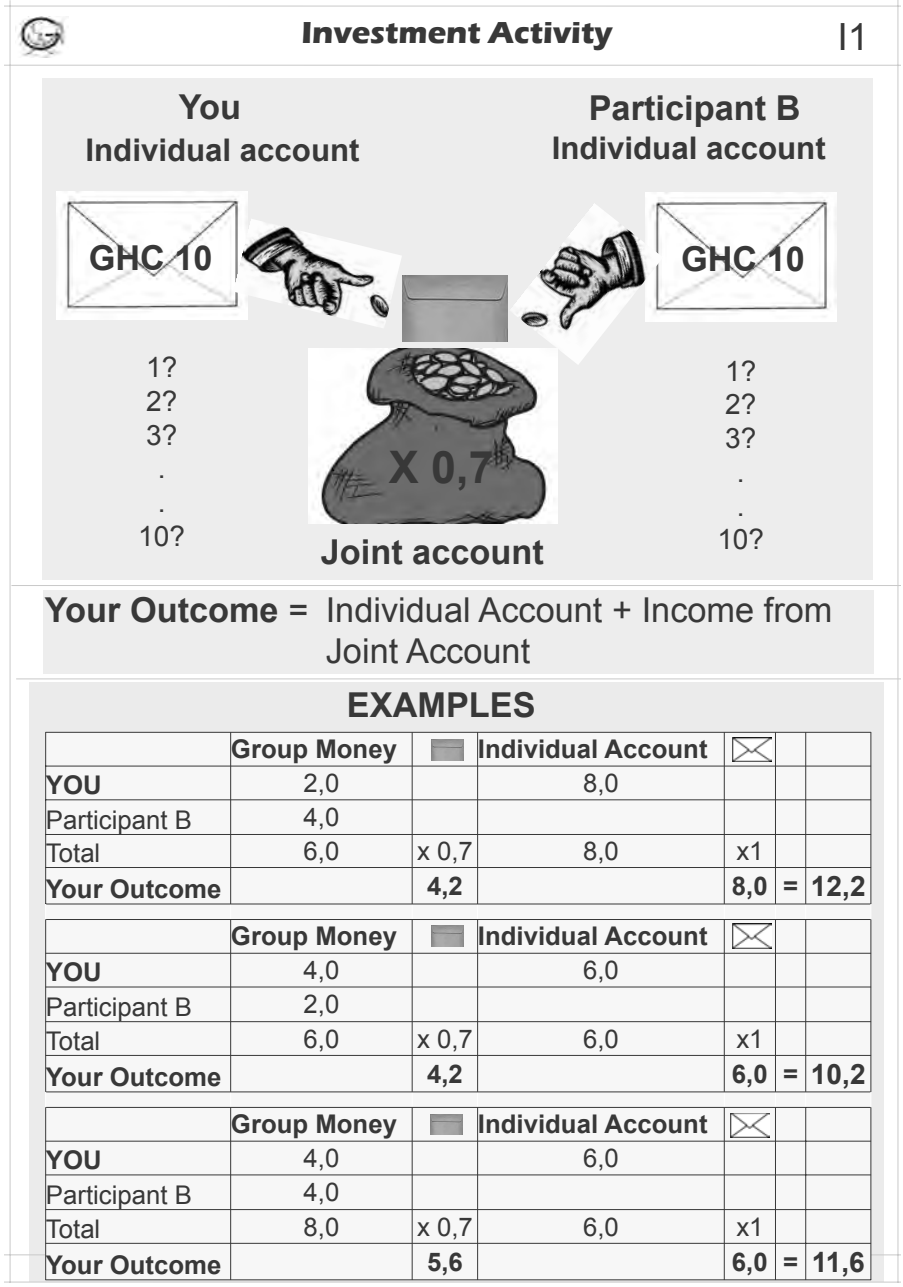


Figure 3: SVO poster

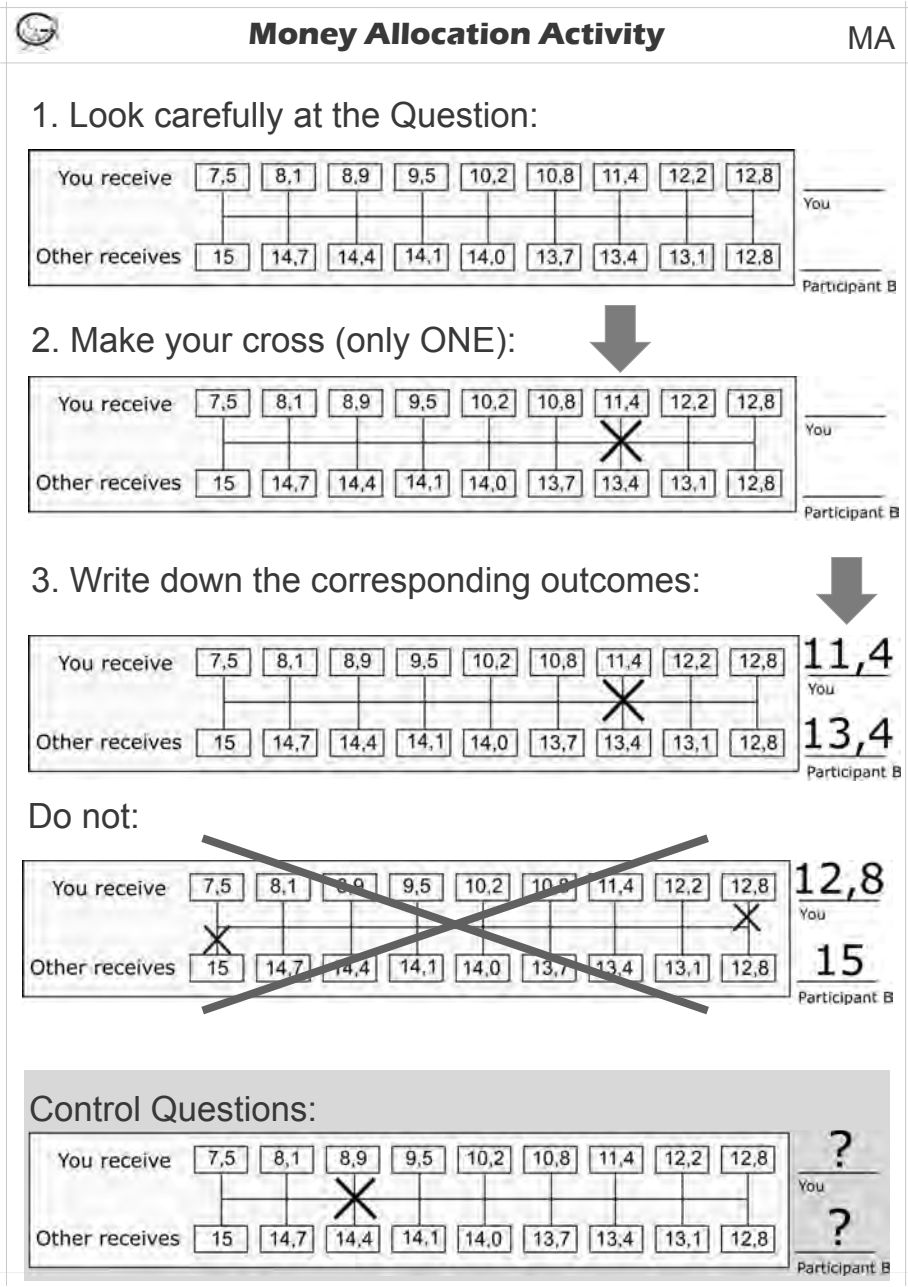


Figure 4: Changes in PGG contributions across treatments

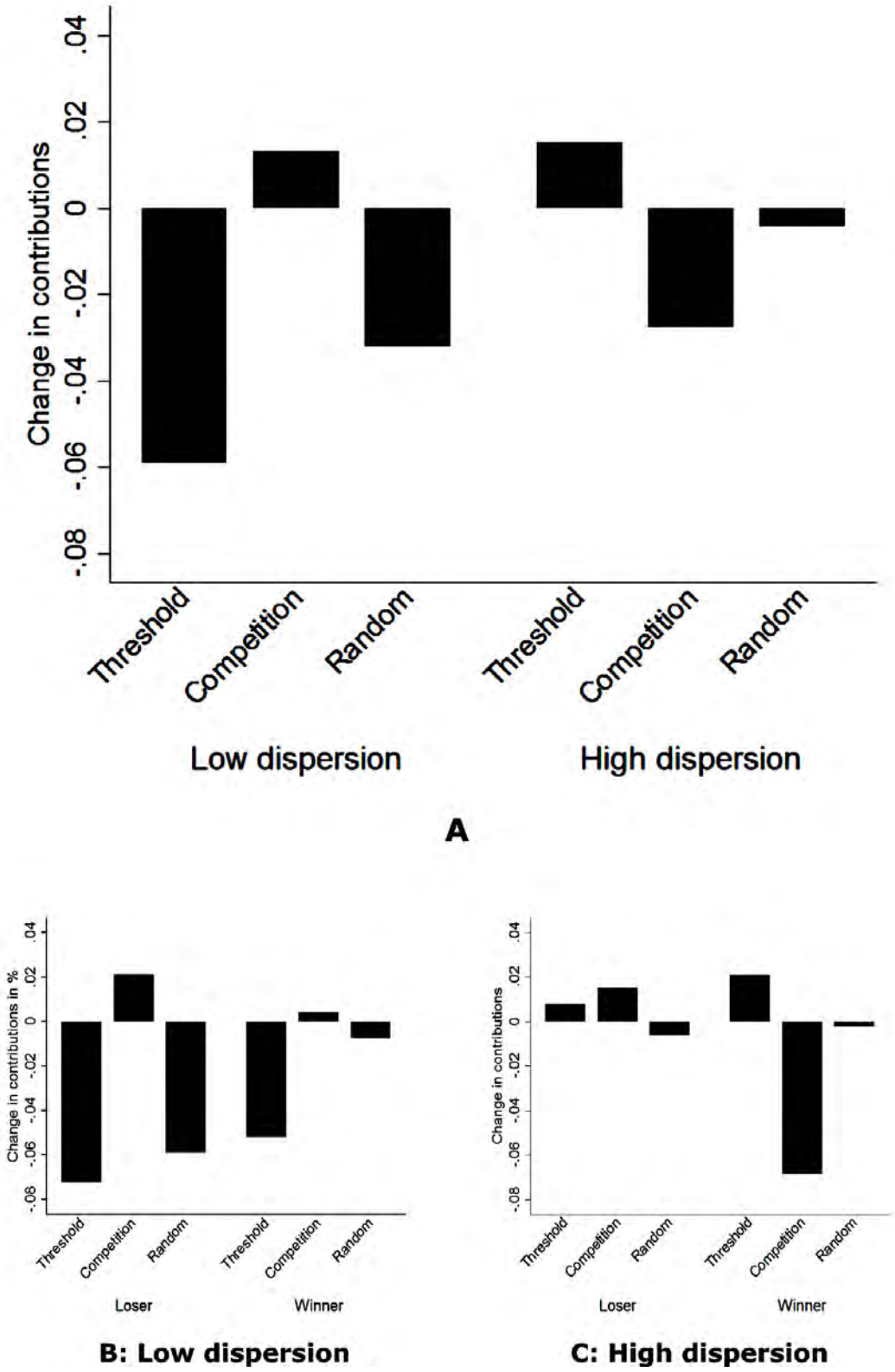


Figure 5: Changes in SVO angle across treatments

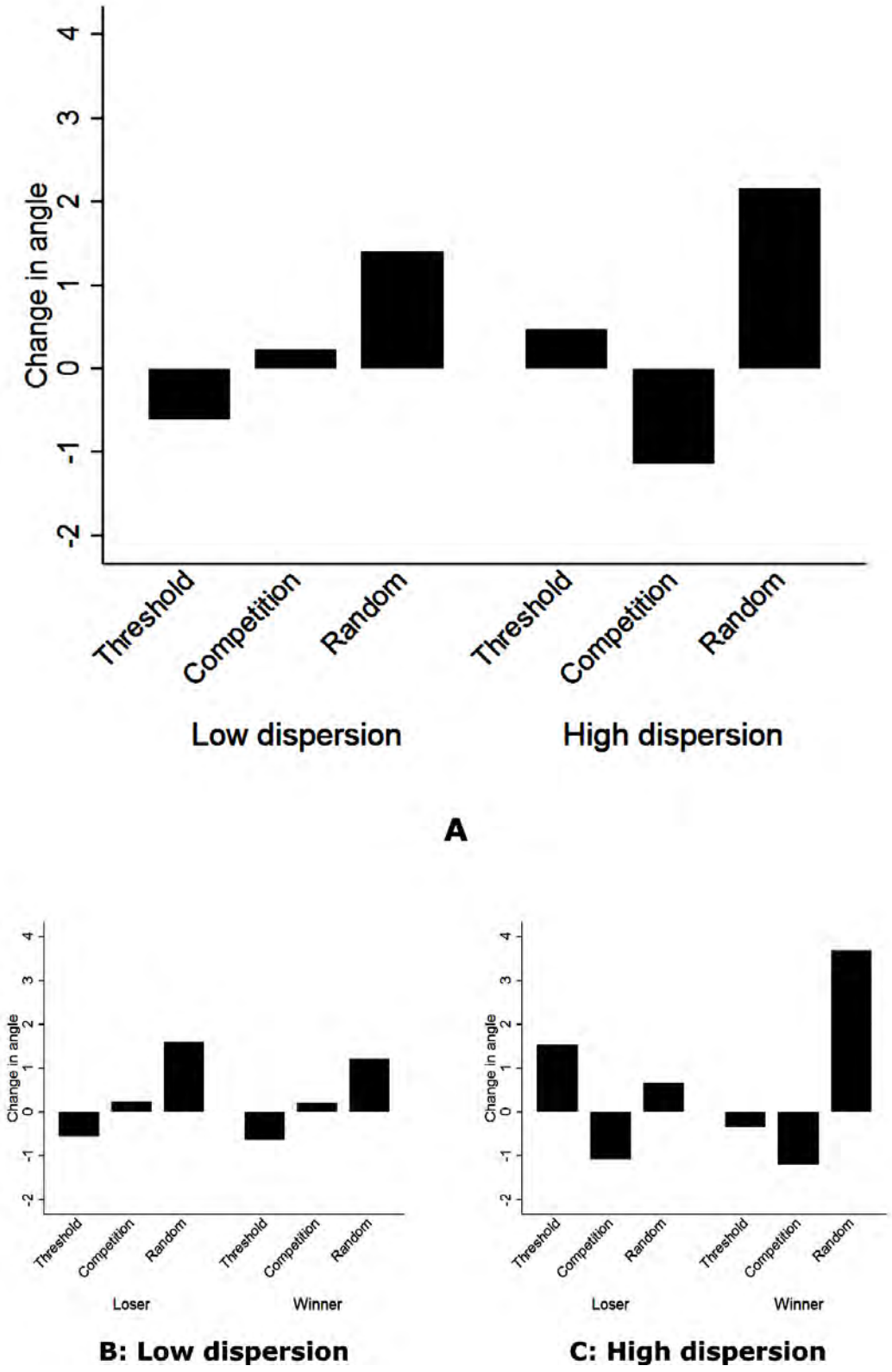
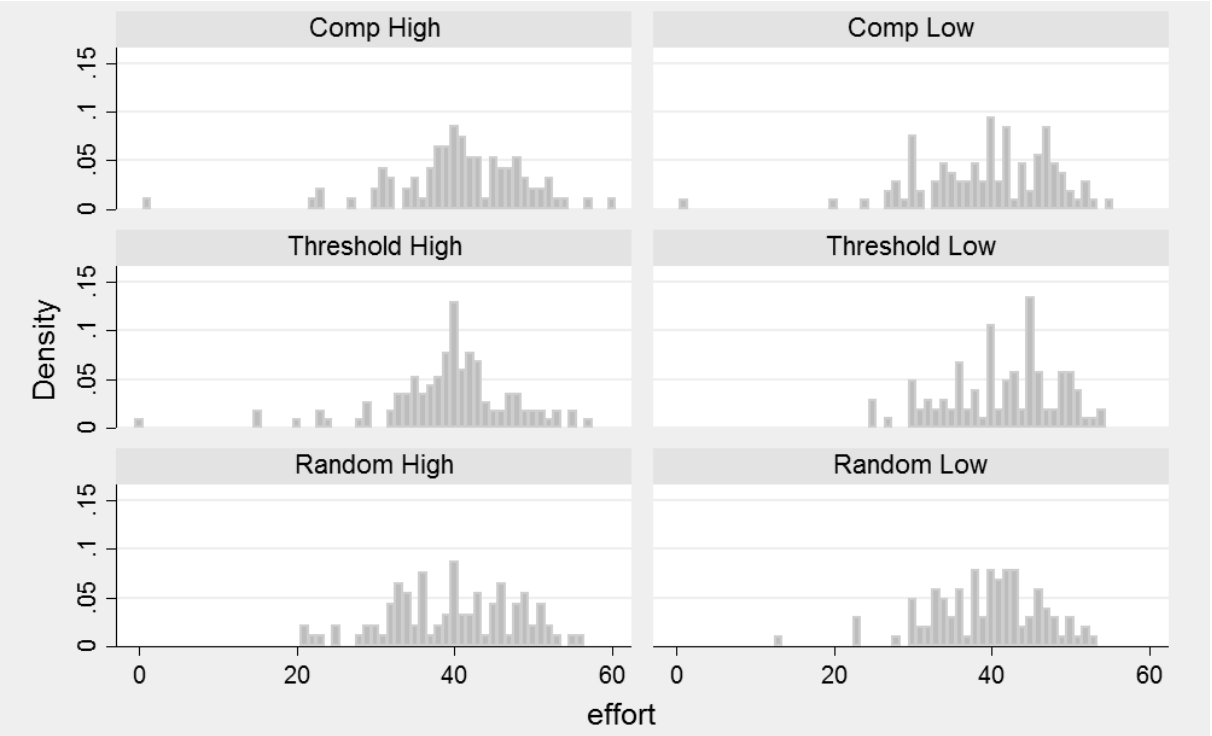


Figure 6: Distribution of effort across treatments




# B Tables

Table 1: Experiment treatments

| | Competition ($C$) | Threshold ($T$) | Random ($R$) |
|---|---|---|---|
| High ($H$) | most pens earns 15<br>other earns 5 | $\geq 40$ pens earns 15<br>$< 40$ earns 5 | randomly earns 15<br>other earns 5 |
| Low ($L$) | most pens earns 12<br>other earns 8 | $\geq 40$ pens earns 12<br>$< 40$ earns 8 | randomly earns 12<br>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 $\chi^2$-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 | -0.0197 | -0.0143 | -0.0768** | -0.0561** | 0.0372 | 0.0530 |
| | (0.0231) | (0.0222) | (0.0276) | (0.0216) | (0.0296) | (0.0375) |
| Random | -0.0200 | -0.00226 | -0.0534** | -0.0325 | 0.0180 | 0.0522 |
| | (0.0208) | (0.0227) | (0.0208) | (0.0193) | (0.0311) | (0.0421) |
| Baseline PGG ($Y_{i0}$) | -0.444*** | -0.455*** | -0.422*** | -0.427*** | -0.457*** | -0.473*** |
| | (0.0453) | (0.0459) | (0.0665) | (0.0705) | (0.0629) | (0.0591) |
| Constant | 0.214*** | 0.184** | 0.234*** | 0.168 | 0.185*** | -0.129 |
| | (0.0266) | (0.0766) | (0.0394) | (0.108) | (0.0328) | (0.113) |
| R-squared | 0.217 | 0.242 | 0.226 | 0.308 | 0.233 | 0.314 |
| **Panel B: Δ SVO angle** | | | | | | |
| Competition | -1.046 | -1.018 | -3.586** | -4.199** | 1.328 | 1.074 |
| | (1.102) | (1.110) | (1.409) | (1.854) | (1.540) | (2.062) |
| Random | 1.504 | 0.948 | -0.151 | 0.490 | 3.150* | 2.869 |
| | (1.003) | (1.235) | (1.189) | (1.201) | (1.598) | (2.281) |
| Baseline SVO ($Y_{i0}$) | -0.661*** | -0.651*** | -0.655*** | -0.657*** | -0.679*** | -0.635*** |
| | (0.0499) | (0.0507) | (0.0687) | (0.0741) | (0.0741) | (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 | 0.324 | 0.347 | 0.302 | 0.341 | 0.357 | 0.405 |
| Observations | 538 | 538 | 262 | 262 | 276 | 276 |
| 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, 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; $C$ vs. $T$)[a]

| | (1) Δ PGG | (2) Δ SVO |
|---|---|---|
| *Income effect* | | |
| loser | -0.0239 | -4.3625 |
| | (0.0562) | (3.3742) |
| winner | -0.1309*** | -4.0296 |
| | (0.0403) | (2.8920) |
| *Behavioral variables* | | |
| not inequality averse | -0.0335 | -3.7620 |
| | (0.0322) | (3.2476) |
| inequality averse | -0.1214* | -4.6301 |
| | (0.0682) | (3.4913) |
| risk seeking | 0.0028 | 0.0439 |
| | (0.0087) | (0.5227) |
| dislikes competition | -0.0871 | -3.3085 |
| | (0.0543) | (3.3379) |
| likes competition | -0.0676 | -5.0836 |
| | (0.0441) | (3.3031) |
| *Work-related variables* | | |
| does not work in teams | -0.0411 | -4.5677 |
| | (0.0373) | (3.1905) |
| works in teams | -0.1138* | -3.8243 |
| | (0.0617) | (3.2144) |
| is not aware of bonus | -0.1081** | -6.4739** |
| | (0.0489) | (3.0383) |
| is aware of bonus | -0.0466 | -0.9182 |
| | (0.0455) | (3.3706) |
| R-squared | 0.3476 | 0.3710 |
| Observations | 262 | 262 |
| Covariates[b] | 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 not shown.

[b] Covariates: age, female, risk, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

Table 6: Effects of payment/preferences on fairness, rivalry, attachment (high dispersion)[a]

| | (1) fairness | (2) rivalry | (3) attachment |
|---|---|---|---|
| Competition ($C$) | -0.287** | 0.219 | 0.0799 |
| | (0.134) | (0.168) | (0.438) |
| Random ($R$) | -0.395** | 0.0391 | -0.104 |
| | (0.193) | (0.208) | (0.447) |
| Winner | 0.0369 | 0.0433 | -0.0360 |
| | (0.0386) | (0.107) | (0.294) |
| $C\times$ Winner | 0.177* | -0.0637 | -0.271 |
| | (0.0924) | (0.136) | (0.374) |
| Inequality averse | -0.000741 | 0.198* | 0.651** |
| | (0.0560) | (0.114) | (0.277) |
| $C\times$ Inequality averse | 0.0876 | -0.257* | -0.614* |
| | (0.0983) | (0.154) | (0.368) |
| Works in teams | -0.00554 | -0.0201 | -0.228 |
| | (0.0348) | (0.0987) | (0.213) |
| $C\times$ Works in teams | -0.113 | 0.106 | -0.0330 |
| | (0.105) | (0.142) | (0.340) |
| Is aware of bonus | 0.0184 | -0.109 | -0.613** |
| | (0.0387) | (0.102) | (0.260) |
| $C\times$ Is aware of bonus | -0.0970 | 0.0636 | -0.216 |
| | (0.114) | (0.150) | (0.379) |
| Baseline PGG ($Y_{i0}$) | -0.295*** | -0.0228 | 0.123 |
| | (0.113) | (0.123) | (0.297) |
| Constant | 1.415*** | -1.124*** | 2.395*** |
| | (0.219) | (0.277) | (0.736) |
| R-squared | 0.364 | 0.359 | 0.280 |
| Observations | 262 | 262 | 261 |
| Covariates[b] | 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 only presents effects that were previously significant, particularly for interactions with the competitive treatment ($C$). Interactions with the random treatment dummy ($R$) are omitted.

[b] Covariates: age, female, 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 7: Heterogeneous effects on change in PGG and SVO (low dispersion; $C$ vs. $T$)[a]

| | (1) Δ PGG | (2) Δ SVO |
|---|---|---|
| *Income effect* | | |
| loser | 0.0609 | 0.6386 |
| | (0.0861) | (4.1840) |
| winner | -0.0311 | 0.2876 |
| | (0.0799) | (3.0165) |
| *Behavioral variables* | | |
| not inequality averse | -0.0056 | -0.7307 |
| | (0.0737) | (3.4142) |
| inequality averse | 0.0353 | 1.6570 |
| | (0.0983) | (4.3247) |
| risk seeking | 0.0137 | 0.4230 |
| | (0.0144) | (0.3580) |
| dislikes competition | 0.0228 | -0.7872 |
| | (0.0845) | (3.6898) |
| likes competition | 0.0069 | 1.7135 |
| | (0.0848) | (4.0513) |
| *Work-related variables* | | |
| does not work in teams | -0.0052 | -0.1767 |
| | (0.0723) | (4.0470) |
| works in teams | -0.0350 | 1.1029 |
| | (0.0949) | (3.8855) |
| is not aware of bonus | -0.0258 | 2.1395 |
| | (0.0951) | (5.1746) |
| is aware of bonus | 0.0040 | -1.2133 |
| | (0.0808) | (3.2864) |
| R-squared | 0.3633 | 0.4152 |
| Observations | 276 | 276 |
| Covariates[b] | 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 not shown.

[b] Covariates: age, female, risk, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.