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Highlights

- The study examines how comparative payments affect coworkers' prosociality
- Competition crowds out prosociality only when the payment dispersion is high
- This effect can be partly explained by feelings of rivalry and winner's entitlement
- When introducing incentives, managers should keep potential side effects in mind

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Competition and prosociality: A lab-in-the-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 can have subsequent negative spillovers on coworkers' prosocial behavior. We revisit this question in a lab-in-the-field experiment and examine whether competition negatively affects Social Value Orientation (prosocial attitudes) in addition to contributions to a public good (cooperative behavior). By considering the context of a developing country, we contribute to replicating previous findings in White, Educated, Industrialized, Rich, and Democratic (WEIRD) samples. We find that when the payment dispersion between winners and losers is high, competition reduces both cooperation and prosocial attitudes compared to a threshold payment. Mainly winners cooperate less under competition. A comparison with a random payment scheme suggests that rivalry might partly explain the crowding-out effect in other-regarding preferences. Under low payment dispersion, competition does not affect cooperation or prosocial attitudes.

JEL Codes: C93, D03, J33
Keywords: competitive payment, wage differences, prosociality, field experiment

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1 Introduction

Theoretical and empirical work suggest that competitive payment schemes can increase effort and productivity compared to piece-rate payments (e.g. Lazear and Rosen, 1981; van Dijk et al., 2001; Bogaard and Svejnar, 2018; Lazear, 2018). At the same time, they may induce a rival atmosphere that could be detrimental to good workplace relations. Evidence from real-effort experiments so far confirm negative side effects of competitive payment schemes on cooperative behavior in social dilemmas (Buser and Dreber, 2016; Brandts and Riedl, 2020). Our study revisits this question and examines whether the harmful effects of competition on cooperation extend to other non-strategic dimensions of prosociality and social contexts. While strategic cooperation motives can be affected by incentives, it is less clear if non-strategic other-regarding preferences are malleable and affected by competition, particularly so in poor economies where kinship networks are deeply embedded in social and economic life (Barrett et al., 2019; Cox and Fafchamps, 2007; La Ferrara, 2010; Guirkinger and Platteau, 2019).

In this paper, we consider the effect of competition on a critical measure of social preferences, as is Social Value Orientation (SVO) or distributional preferences for self and others. We focus on this measure as the empirical research shows that SVO is related to critical behavioral outcomes such as charitable giving, cooperation, honesty, and tax compliance, among others (Mischkowski and Glöckner, 2016; Balliet et al., 2009; Grosch and Rau, 2017; Brizi et al., 2015). If competition negatively affects SVO, the cost of competition can be higher than anticipated. Gaining a deeper understanding of the influence of different dimensions of wage differentials and workers' characteristics and preferences holds insights for workplace policy, and more specifically, for the design of incentives.

Our results are based on a lab-in-the-field experiment (i.e., an artefactual field experiment in the terminology of Harrison and List, 2004) with workers from an agribusiness in Ghana. Therefore, our work helps to overcome the reproducibility crisis (e.g. Camerer et al., 2016) by extending the evidence that has been previously obtained with WEIRD samples (Henrich et al., 2010). The eld context is particularly relevant as prosocial preferences play a critical role of overcoming market failures (Foster and Rosenzweig, 2001; Biener et al., 2018).

In our study, we randomly and anonymously match two participants for the duration of a three-stage experiment following van Dijk et al. (2002). In the *first* and *third stage*, we consider two incentivized measures of other-regarding preferences. The first measure is the social value orientation (SVO) by Murphy et al. (2011) which captures the individual's

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distributional concerns in a two-player money allocation task. Participants who maximize their gains are considered to be individualistic while participants who maximize the total amount of payments (minimize losses) are said to be prosocial. Since decisions in this game are non-strategic, this measure allows us to capture the distributional preferences towards coworkers that are likely to be more stable over time. The second measure is contributions to a two-person one-shot public goods game (PGG). Ample empirical evidence has shown that the majority of individuals behave as conditional cooperators in this game (e.g. Keser and Van Winden, 2000; Fischbacher et al., 2001; Martinsson et al., 2013). This second measure, thus, allows us to examine cooperativeness which is strategic and also depends on expectations about the coworkers' cooperativeness. Although our two measures are distinct from each other, we refer to them as "prosociality" or "other-regarding preferences" for brevity.

In the *second stage*, participants complete a real-effort task. To abstract from potential free-riding that could occur in team-based production tasks, productivity depended only on individual workers' effort. We implement a between-subjects design in which each participant is randomly assigned to either a competitive, a threshold, or a random payment scheme. We also vary the dispersion in payments by implementing treatments with low or high dispersion of earnings. The comparison of the third and first stage allows us to assess the evolution of cooperativeness and other-regarding attachment across different payment schemes and dispersion levels.

Our main hypothesis is that competitive payment generates a negative effect on coworkers' other-regarding preferences. Various mechanisms may explain this. First, competition generates a feeling of rivalry. Confrontations in the workplace might cause workers to see each other as opponents and thus, adopt a more individualistic behavior (e.g. Drago and Garvey, 1998; Brandts et al., 2009; Dechenaux et al., 2015; Snower and Bosworth, 2016). 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 cooperation (e.g. Chan et al., 1999; Cherry et al., 2005; Buckley and Croson, 2006). Lastly, competitive payments can be regarded as unfair generating a decrease in the incentives to be prosocial (e.g. Akerlof and Yellen, 1990).

Our results corroborate that when the dispersion of payments is high, competition crowds out other-regarding preferences. Both cooperation and prosocial attitudes are lower in competition than threshold payments. Buser and Dreber (2016) found similar effects for a zero-sum competition in which cooperation was lower than in a treatment

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with piece-rate payments. We add to this literature and show that when pay dispersion is low, competitive payment schemes neither affect cooperation nor prosocial attitudes relative to the threshold payment. These findings suggest that the level of rivalry, i.e., the more there is at stake in a competition, is the main mechanism at play. Further, individuals are more individualistic under competition than random payments suggesting that prosocial attitudes are sensitive to the rivalry induced by competition.

Assessing heterogeneous impacts, the effect is partly driven by those who win the competition. We find that winners under competition contribute less to the public good than winners under threshold. This may imply a feeling of entitlement and enhancement of status from winning against someone else (comparable to Schurr and Ritov, 2016; Gee et al., 2017). Robustness checks corroborate these main results.

Related literature has shown that competition can induce antisocial behavior. For example, people sabotage others to increase their chances of winning (Harbring and Irlenbusch, 2011) and redistribute payoffs between themselves and other market participants (Erkal et al., 2011; Fehr, 2018). More surprising are maybe the "subtle" side effects, i.e., a subsequent change in behavior when strategic motives can be ruled out. Here, several experimental studies suggest that competitive environments can alter people's mindset toward a more selfish and unethical one. For example, people cheat more in a die-rolling task (Schurr and Ritov, 2016), engage in mean behavior in a joy-of-destruction task (Jauernig et al., 2016; Jauernig and Uhl, 2019), and become more non-utilitarian (Chen, 2019) after being exposed to a real-effort competition.

Closest related to our study are studies by Buser and Dreber (2016) and Brandts and Riedl (2020) who focus on the effect of competition on cooperative behavior. Brandts and Riedl (2020) examine the effect of being a market loser or winner and the respective matched groups (two loser, two winners, mixed) on the effectiveness of markets measured by a social dilemma game. The study by Buser and Dreber (2016) compares ex-post behavior in a public goods game (PGG) in a competition with a random and a piece-rate payment scheme. We extend existing work by (1) demonstrating that cooperation only crowds out in a competition with a high level of rivalry (high dispersion of payments) but not under a low level of rivalry (low dispersion of payments), (2) showing that competition not only crowds out cooperation but also triggers selfish attitudes, and (3) shedding light on potential mechanisms. We demonstrate that winners of the competition are the ones who partly drive the crowding-out effect. We explore other mechanisms such as perceived unfairness and show that these behavioral drivers are not as important. We use a novel threshold treatment as a control, compared to commonly used piece-rate payments, to

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investigate the effect of rivalry, i.e., winning against somebody else compared to reaching a threshold. This design allows us to compare the development of prosociality of the two different earnings groups, i.e., winners and losers, across treatments and shed light on the effect of deserving a payment (threshold) compared to deserving a payment *and* prevailing in a competition. Moreover, the design allows us to check the robustness of our main crowding-out results with respect to inequality in payments within dyads. Since we conducted the experiment in a field context, this paper also replicates findings from prior work in non-WEIRD (white, educated, industrialized, rich, and democratic) societies (Henrich et al., 2010). Moreover, our study has implications for workplace policy. Given that the impact of competitive schemes (such as relative payment for performance) on workplace cooperation is likely to depend on the dispersion of payments, managers should keep the nuances of incentive systems in mind as they consider implementing them (as alluded to by for example Holmström, 2017).

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

Our lab-in-the-field experiment is implemented with workers from a banana-producing agribusiness in Ghana. The firm is fair-trade certified and exports all of its produce to Europe. The field context provides a pertinent setup for our study as the banana sector in Africa is under extreme pressure to increase productivity, to survive stiff competition from international markets. With the above concerns in mind, the firm was considering changing its system of incentives but was concerned about the potential effects of new incentives on employees' social relations. The firm already had a somewhat complex bonus system in place, which rewards employees when a target production level is reached. Approximately one-third of the workers reported being unaware of how the existing bonus system works. In light of this, the firm had great interest in a low-cost approach to testing the potential effect of different payment schemes on prosociality. The "lab" seemed like a reasonable starting point. The managers permitted us to carry out the experiments with the workers and facilitated the implementation of the sessions by providing an adequate location. At the end of the study, we communicated the results to the firm, which opted not to implement a competitive payment.

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The company's workforce comprises approximately 1815 men and 230 women, all of whom are employed full-time. Most of the employees perform basic jobs such as bunchcare, harvesting, packaging, and quality control which do not require any specific qualifications but are crucial for the company's banana output. The workers are divided into eight sectors with identical 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 in 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 banana bunches, cutting leaves 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. The company emphasized the importance of prosocial behavior across employees of different tasks to allow for smooth processes from harvesting to packaging. Therefore, maintaining prosocial behavior was a top priority for the company's managers.

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.[1] The objective of the pre-survey was to elicit incentive-compatible measures of individual and social preferences that are not influenced by the main experimental conditions. 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, 2016), 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. We informed subjects about their outcomes only at the very end of the experiment session to avoid spillover effects on behavior in the core experiment. As these measures were elicited before the experiment, they can be argued to be exogenous to the treatment. We thus use them to further explore the drivers of behavioral change. Unfortunately, the firm did not share data on worker productivity and/or pay, so we only have limited administrative data (see below) to complement some work-related measures in the pre-survey.

[1]Complete instructions can be found at https://sites.google.com/view/kerstin-grosch/research.

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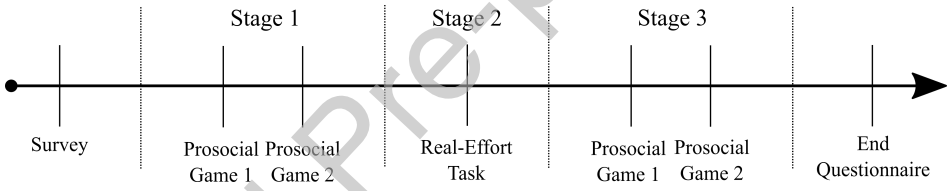
At the beginning of the experiment, we randomly and anonymously matched two participants (i and j). While participants knew that the experiment had different stages, the specific instructions for each task were presented stage by stage. In the first stage, we elicited the baseline level of prosociality. To avoid learning effects or strategic behavior, we did not provide feedback on the decisions in this stage till the end of the experiment. 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 greater detail 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.

Figure 1: Study session



Stage 1: Baseline measures of other-regarding preferences

Other-regarding preferences were measured through two incentivized games: a one-shot PGG and an SVO game, the order of which was randomized. In the PGG (Figure 4), subjects received an endowment of 10 Ghanaian cedi (GHS; 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.[2] The return on investment in the private account was 1 while the marginal per-capita return from the joint account was 0.7.

The SVO game (Figure 5) is based on Murphy et al. (2011). We selected this task as it has a lower number of distributional choices than the original Liebrand et al. (1986) and has a high degree of internal consistency.[3] Participants were matched in dyads again and presented with six different distributional decisions simultaneously. In each situation, there were two roles – an active decision-maker and a passive player. Subjects in the active role had to choose the preferred money allocation for themselves (i) and their

[2]At the time of the experiment, GHS 10 was approximately equivalent to USD 2.66.
[3]The original amounts were divided by 12.5 such that incentives were similar across SVO and PGG.

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matched partner (j) who had to accept the decision of the other person. Using the strategy method, all participants first took decisions as active players. Yet, we informed participants that at the end, one player in the dyad would be randomly assigned the role of the active decision maker (more when discussing information revelation in Section 2.3). The preferred amounts across the six decision sets were summed up for i and j. A completely individualistic person always maximizes the income for herself whereas a prosocial person maximizes the outcome for both i and j. These "prosocial attitudes" are represented by the so-called SVO angle which is the inverse tangent of the ratio of the mean of the payoffs allocated to the other and the mean of the payoffs allocated to oneself. A low angle represents individualistic preferences whereas a high angle refers to prosocial preferences.[4]

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. Each participant received components for up to 65 ballpoint pens. This task was chosen for several reasons. First, it is simple to assess quality: A properly functioning (high-quality) pen was able to eject/retract; anything else was of low quality. For purposes of payment, only properly functioning pens were counted. Second, the task could easily be completed regardless of education level; thus, resembling the majority of tasks at the company. Third, we wanted to mitigate comparative advantage by part of the workforce, e.g., a packing task would have been easier for workers who typically pack the bananas versus workers who are typically in the field. Finally, this task was likely to reduce workers' concerns that performance in the experiment would be tied to their day-to-day remuneration.

Stage 3: Ex-post level of other-regarding preferences

In the last stage, other-regarding preferences were measured again using the same procedures as in Stage 1. As a reminder, the order of the PGG and SVO was randomized. Moreover, we randomized the order of allocation decisions in the SVO task in Stage 1 and Stage 3 to reduce anchoring effects. Learning should not be an issue since participants did not receive feedback about their payoffs or decisions of others until the very end of the session. If any, the effects from repetition should kick in equally across treatments, so

[4]We do not observe competitive or altruistic persons as defined by Murphy et al. (2011) in our sample, as these are people at the outer spectrum of the angle. Therefore, we refrain from explaining extremely low angles ("competitive people") and extremely high angles ("altruists") for brevity.

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the diff-in-diff comparison should isolate differential effects.

Information revelation occurred as follows. Subjects were informed that either Stage 1 or Stage 3 and only one of the activities in each stage would be selected at random for payment (stage 2, the real-effort task would always be paid) once participants completed the task. Within each activity, the active role and the decision to be implemented was also 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 other-regarding preferences were unlikely to be due to endowment, learning, or reputation effects. In Stage 2, the RE task, participants received feedback after completing the task. They were privately informed of their individual earnings. 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.

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). We randomized subject-pairs to the resulting six treatments at the session level (Table 1). In the threshold scheme (T), the main control treatment, any participant who assembled forty 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 receive a low payment. In the experimental session, we used neutral framing and referred to the payoff from the task as "earnings". Yet, in this document, 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 participant (in the pair) who assembled most pens correctly won/earned the high payment. Finally, in the random scheme (R), the second control treatment, the winner of a dyad was determined at random with a 50 percent chance. 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 similar payment levels (mean and variance) across treatments as the threshold level was calibrated so that about 50 percent of the participants should be winners. For pairs in which there is one winner and one loser, payment distribution is similar in threshold and competition payments allowing us to control for

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Table 1: Experimental 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

the effect of rivalry (winning against another person versus winning against a level) on cooperativeness and prosocial attitudes. While we were mainly interested in the differential effect of competitive versus threshold payments on changes in other-regarding preferences, we included the random payment to isolate the potential effect of rivalry induced by competing with another person from the effect of receiving a differential payment. If other-regarding preferences were to decrease more in competition than in random, we could attribute such an impact to being exposed to relative payments. If the effect was of similar magnitude, then we could argue that it is not competition that affects other-regarding preferences, but rather the inequality it generates.

To boost participants' understanding of the different stages of the experiment, we used trivia quizzes after each stage. Two subjects in a session volunteered to answer some questions in public. We introduced several scenarios depending on the stage. We then asked them about the respective payoffs for the two participants. We also used written 'control' questions, which were checked by the experimenter as soon as the participants finished answering. If a participant had not answered correctly, s/he (1) was approached by one of the research assistants, (2) received an additional explanation, and (3) could answer the questions one more time. These procedures enhanced understanding in the different stages of the experiment.

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 experiment sessions. We prepared invitation lists with employee names and identification numbers for each experiment session and distributed them through all sectors. We called the section heads several times and reminded them to release the invited workers at the corresponding times. Due to the production line, packing-house employees tended to be available during the morning. However, we made sure that only one employee per working team was invited to a specific session. Descriptive data from our survey shows that, on average, people knew one other person within the experimental session (see Table 4), suggesting that this was a viable strategy to preserve independence of observations.

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2.4 Empirical strategy and hypotheses

Our main outcome of interest is the change in cooperativeness and prosocial attitudes between Stages 1 and 3 (i.e., at baseline/pre-treatment and follow-up/post-treatment). Therefore, we estimate our treatment effects using two alternative specifications. The first specification considers the following model:

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

where ΔY_i is the difference in other-regarding preferences between Stages 1 and 3 at the individual level i. C_i and R_i are dummies for individual-level exposure to the competition and random treatments, respectively. Y_{i0} is the initial level of prosociality in Stage 1; Z_i is a set of covariates comprising the unbalanced characteristics in Table 4 in the Appendix; and ϵ_i is an error term. Our variable of interest is $\beta_C C$ which captures the differential change in other-regarding preferences between threshold and competitive payments.

The second specification considers differential effects of competition according to the dispersion of payments using the following specification:

$$\Delta Y_i = \beta_0 + \beta_1 C_i + \beta_2 R_i + \beta_3 H_i + \beta_4 C_i \times H_i + \beta_5 R_i \times H_i + \beta_6 Y_{i0} + \beta_Z Z_i + \epsilon_i, \tag{2}$$

where, H_i is a dummy variable that indicates high dispersion of payments. Under this specification, we can compare the differential effects of competition for the low dispersion of payments (β_1) and high dispersion of payments (β_4).

We run these specifications for both contributions to PGG and the SVO angle for the pooled sample. While contributions to PGG measure the effect of competition on strategic interaction, the SVO angle measures the degree of care towards the other. We also run specification 1 separately for the low- and high-dispersion subsamples.

In both, the threshold and the competitive payment, participants work for their payoff and, hence, payoffs reflect merit. It has been shown that when payoffs reflect the effort, people support redistribution ex-post (Krawczyk, 2010; Gee et al., 2017). This could be a hint that our two payment schemes of competition and threshold are seen as equally fair. However, winning over somebody else (competitive treatment) compared to surpassing a threshold (threshold treatment) may feel like a triumph stimulating self-centered views (Piff et al., 2012; Snower and Bosworth, 2016). Moreover, Buser and Dreber (2016) have

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shown that cooperation after a real-effort competition is lower compared to piece-rate payments. Hence, we expect the coefficient β_C to be negative indicating a crowding-out effect of competitive relative to the threshold payment scheme. Along the lines of the conceptual framework by Lazear (1989) and based on the evidence of more pronounced sabotage at high stakes (Drago and Garvey, 1998; Harbring and Irlenbusch, 2011), we expect the decrease in other-regarding preferences to be more pronounced under relatively high rivalry (where winners earn GHS 15 and losers earn GHS 5) than under relatively low rivalry (where they earn GHS 12 and GHS 8, respectively).

Hypothesis 1: *We expect that contributions to the PGG and the degree of prosocial attitudes in SVO will be lower in the competitive treatment compared with the threshold payment. The negative effect of competition is more pronounced within high payment dispersion than within low payment dispersion.*

Different mechanisms could explain why other-regarding preferences are lower in the competition than in the threshold payment. One potential mechanism is associated with the rivalry created by competition. Another potential mechanism is inequality as competition is associated with high and low earnings. For example, Kilduff et al. (2016) find that increased rivalry is related to "competitors" being more performance-oriented and concerned with their status. Brandts et al. (2009) find that competition induces negative emotions making people less willing to help others.

To disentangle these two motives, we compare the random and the competition treatment. In the random and the competition treatment, the payment distribution is similar as well as the knowledge about the other person's income within a pair. If rivalry is not generated by competitive payments, we would expect that the crowding-out effect of competition and the random treatment is similar in magnitude, i.e., $\beta_C \approx \beta_R$. However, if competition generates a feeling of rivalry and confrontation, we would expect a larger drop in other-regarding preferences in competition than in random treatment, 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 rivalry generated by competition rather than the merely induced income inequality.

Hypothesis 2: *A competitive payment scheme generates rivalry among coworkers which decreases prosocial attitudes and cooperation compared with a random payment scheme.*

Another mechanism by which competition might crowd out other-regarding preferences is that winning the competition against somebody else compared to reaching a threshold creates a higher sense of superiority. This may make winners feel more entitled

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to take advantage of losers. For example, Erkal et al. (2011), Schurr and Ritov (2016), and Jauernig et al. (2016) find that winners of a competition tend to behave more antisocial than losers. Schurr and Ritov (2016) in particular demonstrate that merely remembering the moment of winning a competition is sufficient to increase cheating behavior. Winning over somebody else (competitive treatment) compared to reaching a threshold (threshold treatment) may feel like a triumph and a status uplift that stimulates self-centered views and harms cooperation (Piff et al., 2012; Snower and Bosworth, 2016). Our design with similar payment levels allows us to compare participants with the same earnings but differing feelings of superiority/entitlement.

Hypothesis 3: *Winners of the competition are more likely to decrease other-regarding preferences compared with winners in the threshold payment scheme.*

3 Results

3.1 Descriptives

In total, we conducted 51 sessions, typically one in the morning and one in the afternoon on Mondays through Fridays, over five weeks. The sessions were announced as "workshops" and supervisors were informed of selected employees a week in advance to release them at a given time. A total of 619 individuals (589 of whom were men) participated in the experiment. Table 2 shows the number of individuals across treatment conditions for the sample used in the analysis.[5] Sessions lasted approximately three hours and paid GHS 26.31 (USD 7), relative to a daily wage equivalent of GHS 18.

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 4 in the Appendix presents the results. We find that overall there is good balance in most of the demographic and behavioral measures. Yet, subjects appear to be significantly different across treatments in a few sociodemographic (age, sex, education) and some other characteristics (preferences for risk and competition, length of employment, awareness of existing bonuses, job satisfaction, close relations to co-workers). We control for baseline imbalances by including these covariates in our regressions.

[5]Of the 619 participants in the sessions, we drop a total of 80 observations due to missing variables in the questionnaires. Sixty six entries are missing because of the variables education level, household income, and risk aversion. Other responses such as job satisfaction, gender, and age are missing for 14 participants.

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Table 2: Number of observations

Treatment	Sessions	Individuals	Winner		Loser	
$C(HD)$	8	94	48	$(0.51)^*$	46	$(0.49)^*$
$C(LD)$	9	107	50	$(0.47)^*$	57	$(0.53)^*$
$T(HD)$	10	117	66	(0.56)	51	(0.44)
$T(LD)$	10	105	69	(0.66)	36	(0.34)
$R(HD)$	7	93	46	(0.49)	47	(0.51)
$R(LD)$	7	103	54	(0.52)	49	(0.48)
Total	51	619	333	(0.54)	286	(0.46)
Sample**	51	539	294	(0.55)	245	(0.45)

*If the number of subjects in a session was uneven, the "last" subject was randomly assigned to an existing group to compare performance in the competition treatment. However, each participant worked only once and was paid according to relative performance in only one of the groups.

**Due to the field context, we used pen and paper to elicit variables such as awareness of bonuses in the company in the ex-post questionnaire. These questions were not answered by all 619 subjects. To use the same data set in all regressions, we drop observations with missing data. All regressions and tests in this paper are based on 539 observations.

In terms of the two measures of other-regarding preferences, we find that there are no significant differences in prosocial attitudes. However, the initial PGG contributions are highest in our threshold control treatments ($T(HD)$ and $T(LD)$) and lowest in our main treatments of interest ($C(HD)$ and $C(LD)$).[6]

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. On average, subjects completed about 40 pens. Consistent with other empirical evidence (e.g., Buser and Dreber, 2016), there are limited statistically significant impacts on performance across payment schemes. The variance in effort is also unaffected by the treatment (Levene's-test, $p > 0.45$). This result holds regardless of payment dispersion. We also find that there are no significant differences in performance between R and the other treatments. While the finding may seem surprising initially, there are several possible explanations. For example, subjects may be (1) reciprocating the announced payment, (2) exerting effort because they have already chosen to attend the session, and (3) complying with a moral obligation to work.

The PGG contributions and SVO angles by treatment are presented in Figure 2. In

[6]Given there is more room to decrease contributions when initial contributions are higher, these differences in contributions may lead to underestimates of the true treatment effects. For example, if the initial contribution is 9 in Stage 1, the participant can decrease contributions by as many as 9 units, but only increase contributions by as much as 1 unit in Stage 3. Meanwhile, a participant with an initial contribution of 3 has more (less) room to increase (decrease) contributions. Therefore, in the analysis, we control for the initial level of other-regarding preferences.

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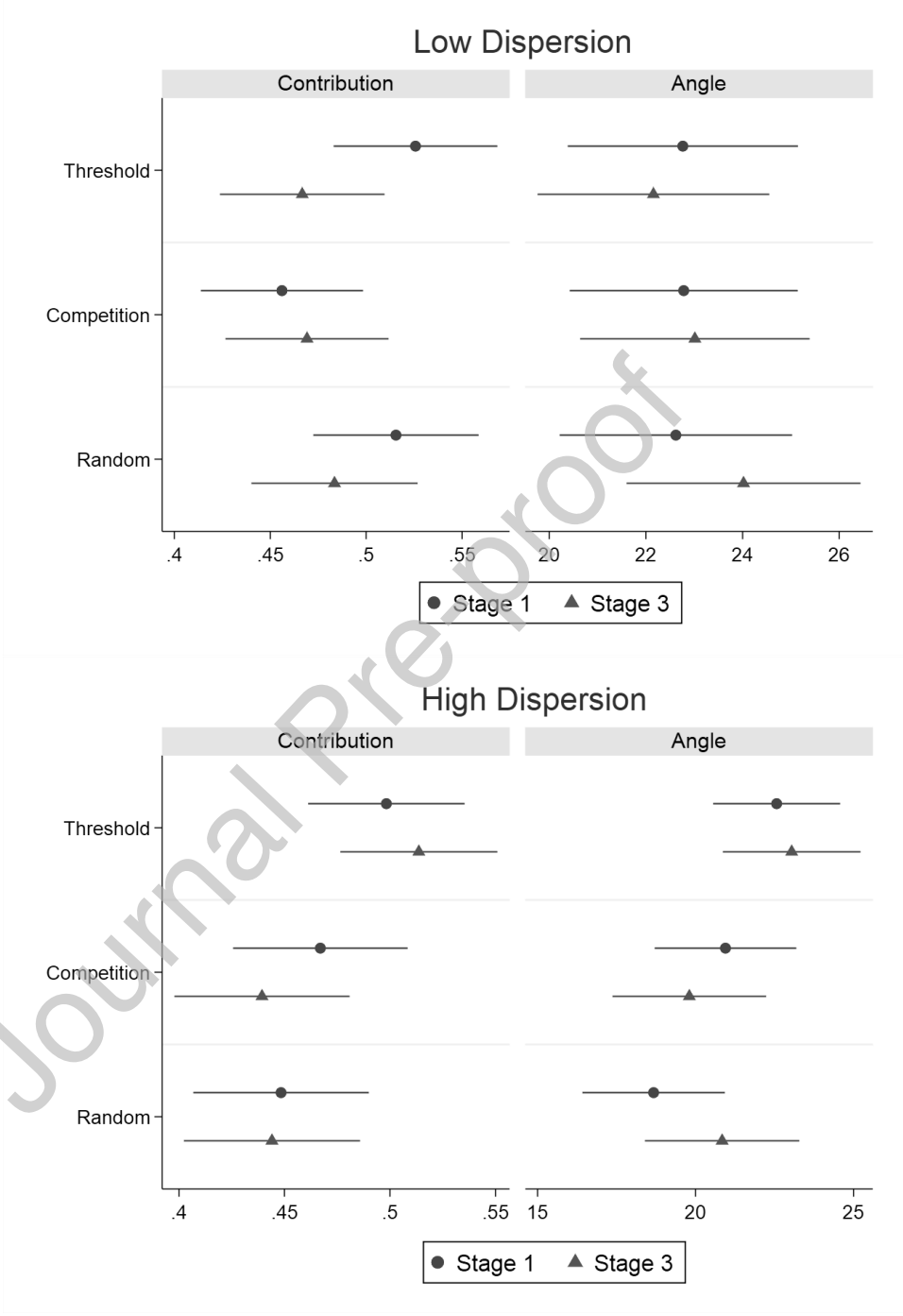
Figure 2: Contribution to PGG and SVO angle by treatment



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Stage 1 participants contributed on average 50 percent of the endowment and the average SVO angle is 22, indicating a low degree of prosocial attitudes. There is a positive but neither strong nor significant correlation between the two outcome measures in the first

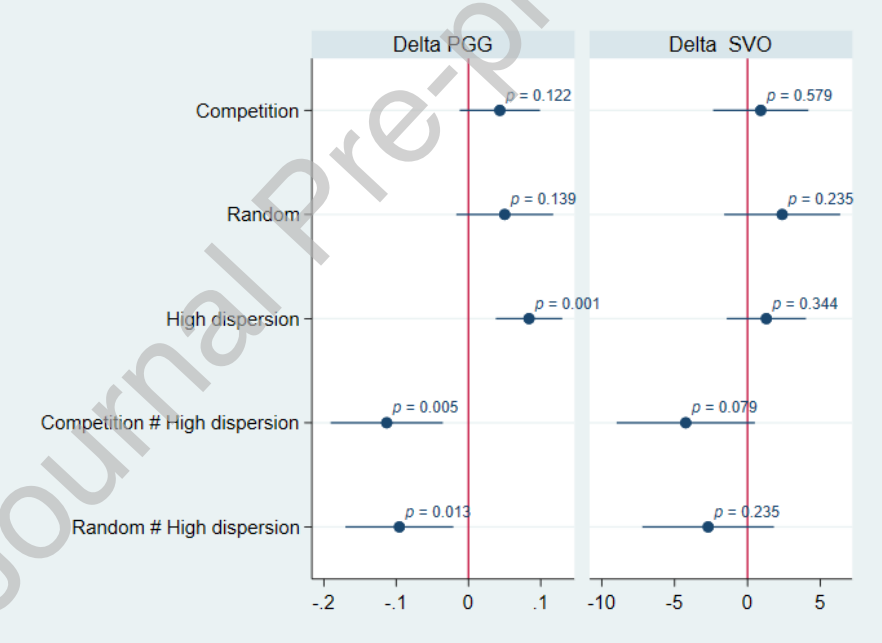
(Spearman's $\rho = 0.061$, $p = 0.158$) and the second stage of the experiment (Spearman's $\rho = 0.013$, $p = 0.771$), suggesting that these two measures capture different dimensions of other-regarding preferences.

To assess the impact of the different payments on cooperation and prosocial attitudes, we estimate the Equations 1 and 2. We discuss these findings in the next section.

3.2 Treatment effects

Figure 6 presents the estimated treatment effects according to the specification in Equation 1. The left panel presents the estimates for changes in PGG contributions while the estimates on changes in SVO angle are presented in the right panel. The corresponding regressions are presented in Table 5 in the Appendix.

Figure 3: Treatment effects for model with interaction effects



We estimate the treatment effects pooling observations across high and low dispersion treatments and for the subsamples with high and low dispersion of payments. Looking at the pooled data across treatments, we find that the mean change in cooperation and prosocial attitudes between Stages 1 and 3 is not significantly different for competition compared to the threshold payment scheme (see columns 1 and 2 in Table 5). Moreover, there is no significant change from zero in the threshold treatment. Once we disaggregate by high and low dispersion, we gain further insights into the mechanisms.

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Under high dispersion, PGG contributions decrease by 5.6 percent in the competition compared with the threshold payment between Stages 1 and 3. As shown in columns 4 and 5 in Table 5, there is no significant change in other-regarding preferences in the threshold treatment from the first to the third stage. Similarly, the SVO angle decreases by 4.2 degrees (about 38 percent) under competition compared with the threshold payment. Both of these effects are significant at the 5 percent level.

Interestingly, we find that random payments also have a negative effect on cooperation compared with the threshold payment. Yet, this effect vanishes once socioeconomic controls are included. Comparing the magnitude of the effect, we find that the competition treatment reduces contributions by 0.135 standard deviations and reduces the angle by 0.139 standard deviations relative to the threshold treatment. This indicates that competitive payments have a similar effect on both measures of other-regarding preferences.

Under low dispersion (see columns 6 and 7 in Table 5), contrary to high dispersion, there are no differential changes in other-regarding preferences, be it for PGG or SVO, across competition and threshold or random and threshold, once we control for socioeconomic characteristics. While we expected competition to have less of an impact on other-regarding preferences 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 does not always lead to a decrease in cooperation and prosocial attitudes. It depends on the context; notably, how well or badly off the competition leaves winners and losers. This adds to the literature that examines zero-sum competitions (e.g., Buser and Dreber, 2016) and is an interesting extension since few competitions in work environments will be winner-take-all competitions.

Alternatively, Equation 2 allows us to compare the relative effects of payment dispersion on changes in other-regarding preferences by treatment. Figure 3 and Column 3 in Table 5 in the Appendix present the estimated coefficients. Confirming our previous results, we find that at low dispersion of payment, competition does crowd-out other-regarding preferences. We find that the change in PGG contributions and SVO angle have a similar magnitude in competition and threshold payments. Yet, when the dispersion of payment is high, competition results in a significantly lower increase in PGG contributions and SVO angle than the threshold payment. In conclusion we find:

Result 1: *Compared with a threshold payment, competitive payments decrease prosocial attitudes and cooperation but only when dispersion of payment is high. When the dispersion of payments is low, we do not find a crowding-out effect under competition.*

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The positive and significant constant demonstrates that other-regarding preferences increases across treatments from the first to the third stage in the threshold payment. The negative and significant effect of the baseline PGG or SVO indicates that prosociality decreases across stages with increasing initial levels of prosociality. In other words, individuals with initially high levels of prosociality decrease prosociality to a higher extent than individuals with a relatively low baseline prosociality level.

3.3 Mechanisms

One mechanism that could explain the crowding-out effects of competitive payment when dispersion of payments is high, is rivalry created by competition. If competitive payments generate rivalry, we would expect the crowding-out effect to be larger in competition than in the random treatment (Hypothesis 2). To test this hypothesis, we estimate Equation 1 with competition as the omitted variable. The estimates are presented as C versus R in Table 5. In line with Buser and Dreber (2016), we find that when the dispersion of payments is high, the coefficient for PGG is similar between both treatments. Yet, there is a distinguishable effect on changes in attitudes towards others demonstrated by a larger decrease in SVO in competition than in random (significant at the 5 percent level). Hence, the estimated coefficient from Equation 1 indicates larger crowding-out effects on prosociality by competition than by random payments. This finding is consistent with Hypothesis 2 stating that competition can erode prosociality by generating rivalry (à la Lazear, 1989; Holmström, 2017). When the dispersion of payments is low, the estimated coefficients on competition and random are not statistically different for any of the two outcomes. This suggests that in this case no rivalry is induced.

Result 2: *A competitive payment has a more pronounced negative effect on prosocial attitudes (SVO angle) than a random payment. This suggests that the former generates rivalry. However, there are no such effects on cooperative behavior (contributions to a public good) in line with recent literature.*

Another mechanism by which competition might decrease prosociality is the sense of entitlement that may come from winning. This in turn may induce winners to feel entitled to take advantage (Hypothesis 3). Moreover, perceived unfairness of the competitive payment scheme may also translate into a drop in other-regarding preferences (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 cooperate and behave prosocially. Subjects might perceive a competitive payment scheme as unfair (relative to

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the threshold payment) 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; Grosch and Rau, 2020); (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) less inclined to compete (as proxied by our measure of preferences for competition).[7]

To tease apart these mechanisms, we extend Equation 1 by adding interactions between the treatment dummies (C_i and R_i) and the 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 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. The last two variables are our firm-administrative measures. We thus run the following specification:

$$\Delta Y_i = \beta_0 + \beta_C C_i + \beta_R R_i + \beta_X X_i + \beta_{CX} C_i X_i + \beta_{RX} R_i X_i + \beta_{Y_0} Y_{i0} + \beta_Z Z_i + \epsilon_i, \qquad (3)$$

where all is as defined previously.

Table 3 summarizes the effects for changes in other-regarding preferences across treatments under high dispersion. We compare behavior of individuals with characteristics X_i in the treatment groups (competition and random payments) to individuals with similar characteristics in the control treatment (threshold payment). Therefore, this measure captures the effect of competition on otherwise similar individuals.[8] The first and third columns are for changes in PGG and the second and fourth columns are for changes in SVO. These are for comparisons between competition and threshold (columns 1 and 2) as well as random and threshold (columns 3 and 4). 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 contribute less in the PGG after having

[7] While a given participant i is aware of participant j's payoff in the competitive and random treatment, this is not the case in the threshold treatment. Therefore, inequality aversion should play a role in the former treatments but not the latter.

[8] The effects reported in this table are equivalent to $(\beta_C + \beta_{CX}) - (\beta_X)$ in Equation 3 (e.g. aka contrasts in Stata).

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Table 3: Heterogeneous effects on change in PGG and SVO (high dispersion)[a]

	(C vs. T)		(R vs. T)	
	(1)	(2)	(3)	(4)
	Δ PGG	Δ SVO	Δ PGG	Δ SVO
Income effect				
loser	-0.0239	-4.3625	-0.0173	-6.8019
	(0.0562)	(3.3742)	(0.0579)	(3.6976)
winner	-0.1309***	-4.0296	-0.0419	-1.5370
	(0.0403)	(2.8920)	(0.0389)	(1.6196)
Behavioral variables				
not inequality averse	-0.0335	-3.7620	-0.0065	-4.9172
	(0.0322)	(3.2476)	(0.0337)	(3.6099)
inequality averse	-0.1214*	-4.6301	-0.0527	-3.4217
	(0.0682)	(3.4913)	(0.0603)	(2.7935)
risk seeking	0.0028	0.0439	-0.0075	1.1677
	(0.0087)	(0.5227)	(0.0087)	(0.5078)
dislikes competition	-0.0871	-3.3085	-0.0411	-3.4614
	(0.0543)	(3.3379)	(0.0411)	(2.4085)
likes competition	-0.0676	-5.0836	-0.0182	-4.8775
	(0.0441)	(3.3031)	(0.0438)	(3.4188)
Work-related variables				
does not work in teams	-0.0411	-4.5677	-0.0467	-1.1657
	(0.0373)	(3.1905)	(0.0326)	(2.3815)
works in teams	-0.1138*	-3.8243	-0.0125	-7.1732
	(0.0617)	(3.2144)	(0.0602)	(3.4140)
is not aware of bonus	-0.1081**	-6.4739**	-0.0930**	-3.8571
	(0.0489)	(3.0383)	(0.0396)	(1.9608)
is aware of bonus	-0.0466	-0.9182	0.0338	-4.4818
	(0.0455)	(3.3706)	(0.0544)	(3.5504)
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.

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

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been exposed to the competition than those who win in the threshold treatment. This is consistent with Erkal et al. (2011) who find that winners are more likely to behave selfishly. However, in contrast to Erkal et al. (2011), this does not seem to be due to the selection of less prosocial types into the winner position as this specification controls for various individual and social preferences as discussed previously. This effect is more likely to be due to winners feeling more entitled and, thus, believing they deserved their payments more than the winners who have not triumphed against someone else, as in the threshold treatment. Winners in the random treatment, however, do not behave less cooperatively compared to winners 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. (2017) who find that when income is earned through performance, individuals use income differences as a heuristic to infer relative merit.

An alternative explanation is that winners in the competition treatment might anticipate that losers would contribute less and behave strategically by reducing contributions. Indeed, we find that winners in the competition expect significantly lower contributions under competition than under threshold payment (Mann-Whitney test, $p = 0.029$) whereas the losers' contributions do not differ between the treatments (Mann-Whitney test, $p = 0.7462$). However, when we control for changes in expected contributions of others in the regression, the winner coefficient in model 1 drops slightly to -0.1284 but remains strongly significant ($p = 0.008$). These analyses imply that expectations of others' contributions only partly explain the winner effect.

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 the competition compared with the threshold payment. The effect for SVO is not statistically significant. This is consistent with the idea that perceived unfairness in the payment crowds-out prosociality. For example, Grosch and Rau (2020) find that antisocial behavior increases with more pronounced inequality aversion after being exposed to a competition. Our result is in line with that since prosocial behavior can be seen as the flipside of antisocial behavior. In the random treatment, we do not observe differences in contributions to PGG or SVO for inequality-averse individuals relative to the threshold treatment.

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3. *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.

4. *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.

5. *Working in teams:* We find that participants who are used to working in teams, reduce other-regarding preferences 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 the same team rather than between teams. No such effects are observed in the random treatment.[9]

Result 3: *The negative effect of competition on cooperativeness is associated with (1) a feeling of entitlement generated among winners of the competition and (2) to a lesser extent, perceived unfairness of payments.*

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3.4 Robustness tests

First, we investigate whether the main results still hold after correcting for multiple hypothesis testing. We use the method proposed by Romano and Wolf (2005) since it allows us to control for baseline level of other-regarding preferences (similar to the main estimation strategy). In Table 5, we present the results in square brackets underneath the respective treatment coefficients and standard errors. The main result, i.e., a decrease in other-regarding preferences under competition relative to threshold at high dispersion, remains significant in the specification that does not include controls (Column 4). The Romano-Wolf p-value for the competition compared to the threshold treatment is 0.040 for Δ PGG as well as for Δ SVO. Once we add controls on socioeconomic characteristics

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

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(Column 5) the Romano-Wolf p-value drops to 0.119 (for Δ PGG) and 0.267 (for Δ SVO).[10]

Second, our identification strategy is based on a difference-in-difference approach where we regress the change in prosocial measures on treatment dummies. To test the robustness of the findings, we also run two alternative specifications that account for the panel data structure. To account for multiple decisions across individuals in Stages 1 and 3, we use a random effects model and a fixed effects model. The specification for the random effects model is:

$$Y_{it} = \alpha_0 + \alpha_C C_{it} + \alpha_R R_{it} + \alpha_\tau \tau_{it} + \alpha_{C\tau} C_{it}\tau_{it} + \alpha_{R\tau} R_i \tau_{it} + \alpha_Z Z_i + \mu_i + \epsilon_{it}, \quad (4)$$

and that for the fixed effects model is:

$$Y_{it} = \alpha_i + \alpha_0 + \alpha_\tau \tau_{it} + \alpha_{C\tau} C_{it}\tau_{it} + \alpha_{R\tau} R_i \tau_{it} + \epsilon_{it}, \quad (5)$$

where Y_{it} is the value of prosociality by individual i in Stage t, as a function of the treatment (C for competition and R for random), τ is the time indicator, and Z represents the individual socioeconomic characteristics. Our variable of interest is the interaction term. The parameters of interest in the previous specifications are $\alpha_{C\tau}$ and $\alpha_{R\tau}$ that indicate differences in the trends of prosociality between the treatments. If competition crowds out PGG and SVO, we would expect the estimated coefficients to be negative.

As presented in Table 7 in the Appendix, we find that the results for PGG are robust to the new specifications. When considering the pooled data, we find that exposure to the threshold payment does not change contributions to PGG significantly in the last stage. Compared with this control treatment, the magnitude of the change is not statistically different in the competitive and random payments. Yet, if we consider the sub-sample under high dispersion of payments, we find that participants exposed to the competitive payment decrease cooperation in the third stage compared with participants in the threshold payment. On the other hand, for participants under the low dispersion of payments, there is a crowding-in effect on contributions to PGG. This effect was not significant in the previous specification and points at increases in cooperativeness under competition when there is low dispersion of payments.

For the second outcome variable of interest, SVO, we find that under this new speci-

[10]These estimations are *comparable* to the specifications in Table 5. However, to correct for multiple hypothesis testing we have to include simultaneously the baseline levels of SVO angle and PGG contributions. Model specification 3 cannot be tested with Romano Wolf due to the interaction term between treatments.

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fication, there are no significant effects of competition relative to the threshold payment. This holds for the pooled sample as well as the subsamples with high and low dispersion. Instead, we find that under random payment, individuals display higher values in the SVO, indicating more pronounced cooperative preferences.

While in the competitive and random payment, there is always a high and a low earner in each group, in the threshold treatment, random matching could imply that in one group there are also two low or two high-income earners. To test whether the effects of competition on prosociality are explained by income differences (Brandts and Riedl, 2020), we estimate equation 1 restricting the sample to 65 percent of the pairs where there is inequality of payments in the threshold group. Table 8 presents the estimated coefficients which confirm the main results from Table 5. At high dispersion of payments, competition hurts cooperation and prosocial attitudes compared with the threshold payment. Yet, for low dispersion, it does not have perverse effects in either measure.

Finally, Table 9 tests for robustness once interaction of payment schemes and dispersion are included. Models 1 and 2 present the results with and without covariates, respectively. Hence, model 2 reproduces the results presented in model 3 of Table 5. Model 3 presents the results without controlling for contributions and SVO in the first stage. The last two columns present the results when we estimate random effects and fixed effects models. The results are quite consistent and competition is associated with negative effects in contributions when the dispersion of payments is high, but not when there is low dispersion of payments. For SVO, the results are robust only in the specifications that control for baseline levels of prosocial attitudes. This suggests that rivalry generated by competition mainly affects mindsets about others in strategic settings as opposed to (prosocial) attitudes towards others without strategic concerns.

4 Conclusion

In this study, we conduct a lab-in-the-field experiment with workers from an agribusiness in Ghana to test whether competitive payment schemes subsequently crowd out other-regarding preferences. We thus partially revisit a question addressed by Buser and Dreber (2016) and seek to understand underlying mechanisms of the thin line between competition and cooperation (Cárdenas et al., 2015). We do so by (1) using two measures of other-regarding preferences, cooperation in a PGG (as in the former study) and prosocial attitudes in a SVO game (which abstracts from strategic concerns); (2) experimentally varying the strength of the competition through payment dispersion between winners and

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losers (while keeping payment differences constant across treatments); and (3) interacting treatment variation with survey covariates and 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 cooperation (as proxied by PGG contributions) and prosocial attitudes (as proxied by SVO angle). This finding is in line with an empirical study by Drago and Garvey (1998) who find that strong promotion incentives at work crowd out helping behavior among coworkers. In our study, the crowding-out effect is mainly driven by those who win the competition. The winner effect is in line with prior studies on *antisocial* behavior such as Schurr and Ritov (2016) who find that participants' (dis)honesty is impacted by exposure to competitive environments and Jauernig et al. (2016) who find that winners of a competition punish more than losers. Yet, winners do not display less prosocial attitudes in the SVO.

When there is less at stake, competition does not hurt either cooperation (PGG) or prosocial attitudes (SVO). Further analysis indicates that prosocial attitudes also seem to be eroded under the random payment scheme with high dispersion. This erosion is stronger under competition than under random payments. This suggests that rival feelings induced by competition as opposed to inequality aversion explain the erosion of prosocial attitudes. The concept of rivalry may also explain why a competition with a larger pay dispersion decreases prosociality while a relatively low pay dispersion does not.

Jakiela (2015) also conducted an experiment in a developing country with less-educated participants and finds that money shared in a dictator game is independent from how the funds were earned, i.e., by effort or luck. In contrast, we look at spillover effects that are independent of the payoff earned in the real-effort task under different payment schemes. Moreover, the finding that prosociality decreases in our context emphasizes that even among coworkers with a common identity, prosociality can (quite easily) be undermined. 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 depend on the design of the competition, e.g., the payment dispersion level. 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).

In light of the above, our findings leave some avenues for future research. First, our setting could be extended to include ex-post effects of competitive payments on collaborative tasks where strategic complements are important. Alternatively, the effects of competition on community interactions as participation in risk-sharing networks could

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be analyzed. Second, while we focused on the impact of competitive payments on the group, where we suspect that the effect of competition will be larger, this analysis can be extended to consider the effect of competitive payments on external bystanders. 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, 2020). Fourth, in lieu of measures of cooperation and prosociality, future work could look at the impact of different payment incentives on subsequent effort provision/productivity tasks (e.g. McGee and McGee, 2019). Fifth, in light of the literature on gender differences in competition (e.g. Gneezy et al., 2003, 2009; Booth, 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). Finally, a natural field experiment, in which participants are unaware of the incentives could provide additional external validity to the results and allow for a distinction between the short- and long-term impacts of these types of interventions on prosociality.

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A Appendix

Table 4: Internal validity balancing tests

	(1) Competition	(2) Threshold	(3) Random	(4) (1) vs. (2), p-value	(5) (1) vs. (3), p-value	(6) (2) vs. (3), p-value	(7) p-value[a]
Demographics							
Age	31.43	31.97	29.65	0.56	0.05	0.01	0.03
Female	0.07	0.06	0.01	0.80	0.00	0.00	0.01
Education	10.05	9.44	10.36	0.02	0.22	0.00	0.00
Ethnicity[b]	2.71	2.63	2.60	0.46	0.33	0.77	0.59
Marital status[c]	1.49	1.49	1.52	0.99	0.66	0.67	0.89
HH size[d]	5.43	5.29	5.07	0.67	0.32	0.51	0.57
Contribution to HH income	1.38	1.39	1.32	0.82	0.40	0.27	0.53
HH income equivalent	144.30	138.19	163.57	0.63	0.17	0.02	0.10
Poverty	1.40	1.40	1.27	0.98	0.13	0.05	0.18
Behavioral							
Trust[e]	0.11	0.10	0.16	0.88	0.16	0.11	0.20
Fairness[f]	0.39	0.43	0.45	0.39	0.25	0.76	0.49
Risk Seeking[g]	3.40	3.53	4.46	0.65	0.00	0.00	0.00
Inequality aversion[h]	2.42	2.56	2.42	0.36	0.99	0.35	0.57
Time preference[i]	204.87	188.92	252.82	0.67	0.40	0.23	0.41
Preference for competition[j]	0.57	0.58	0.72	0.94	0.01	0.01	0.01
Schwartz values[k]							
Benevolence	4.53	4.52	4.55	0.90	0.77	0.66	0.90
Conformity	4.60	4.58	4.53	0.83	0.32	0.44	0.58
Collectivism	0.69	0.74	0.76	0.34	0.14	0.59	0.33
Firm-related							
Months worked at company	41.86	44.89	35.61	0.42	0.07	0.00	0.02
Awareness of Bonuses	0.60	0.66	0.79	0.24	0.00	0.00	0.00
Job satisfaction[l]	4.47	4.43	4.57	0.55	0.09	0.02	0.06
Close relations[m]	1.12	1.44	0.98	0.05	0.29	0.01	0.01
Outcomes							
PGG (Stage 1)	0.45	0.51	0.48	0.01	0.21	0.20	0.04
SVO (Stage 1)	22.15	23.20	20.55	0.38	0.23	0.03	0.10
Observations	171	189	179				539

[a] p-value from joint orthogonality test of treatment arms.

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

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Table 5: Treatment effects on change in PGG and SVO (pooled, high, low dispersion)

Panel A:	Pooled			High		Low	
Δ PGG contributions	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Competition (C)	-0.018	-0.011	0.043	-0.077**	-0.056**	0.041	0.058
	(0.023)	(0.022)	(0.028)	(0.028)	(0.022)	(0.029)	(0.035)
	[0.713]	[0.960]		[0.040]	[0.119]	[0.401]	[0.753]
Random (R)	-0.018	0.002	0.050	-0.053**	-0.033	0.022	0.059
	(0.021)	(0.023)	(0.033)	(0.021)	(0.019)	(0.031)	(0.039)
	[0.515]	[0.683]		[0.099]	[0.505]	[0.515]	[0.782]
High dispersion (HD)			0.084***				
			(0.023)				
Competition × HD			-0.113***				
			(0.039)				
Random × HD			-0.096**				
			(0.037)				
Baseline PGG (Y_{i0})	-0.447***	-0.458***	-0.457***	-0.422***	-0.427***	-0.462***	-0.476***
	(0.045)	(0.046)	(0.046)	(0.066)	(0.070)	(0.063)	(0.059)
Constant	0.213***	0.180**	0.148*	0.234***	0.168	0.184***	-0.144
	(0.027)	(0.076)	(0.078)	(0.039)	(0.108)	(0.033)	(0.105)
R-squared	0.218	0.242	0.257	0.226	0.308	0.237	0.320
Threshold (Mean)	-0.016	-0.016	-0.016	0.020	0.020	-0.056	-0.056
(Std.Dev.)	(0.090)	(0.096)	(0.104)	(0.082)	(0.103)	(0.097)	(0.115)
C vs. R^a	0.000	-0.013	0.007	-0.023	-0.024	0.019	-0.001
	(0.022)	(0.020)	(0.027)	(0.027)	(0.025)	(0.031)	(0.023)
C vs. R^b			0.017				
			(0.039)				
Panel B:Δ SVO angle							
Competition (C)	-1.060	-1.040	0.909	-3.586**	-4.199**	1.292	1.037
	(1.103)	(1.108)	(1.626)	(1.409)	(1.854)	(1.543)	(2.019)
	[0.713]	[0.634]		[0.040]	[0.267]	[0.455]	[0.753]
Random (R)	1.489	0.921	2.378	-0.151	0.490	3.114*	2.821
	(1.004)	(1.233)	(1.980)	(1.189)	(1.201)	(1.604)	(2.227)
	[0.317]	[0.495]		[0.792]	[0.663]	[0.198]	[0.713]
High dispersion (HD)			1.293				
			(1.353)				
Competition × HD			-4.235*				
			(2.360)				
Random × HD			-2.697				
			(2.246)				
Baseline SVO (Y_{i0})	-0.661***	-0.652***	-0.658***	-0.655***	-0.657***	-0.680***	-0.635***
	(0.050)	(0.051)	(0.052)	(0.069)	(0.074)	(0.074)	(0.074)
Constant	14.880***	12.921**	13.518**	15.443***	10.995**	14.509***	22.939**
	(1.366)	(5.121)	(5.350)	(1.787)	(5.048)	(2.148)	(8.870)
R-squared	0.325	0.347	0.351	0.302	0.341	0.358	0.406
Threshold (Mean)	-0.461	-0.461	-0.461	0.109	0.109	-1.089	-1.089
(Std.Dev.)	(7.057)	(7.358)	(7.414)	(6.253)	(6.844)	(8.040)	(8.604)
C vs. R^a	-2.550**	-1.961	1.469	-3.435**	-4.689**	-1.822	-0.001
	(1.165)	(1.212)	(1.758)	(1.510)	(1.915)	(1.631)	(0.023)
C vs. R^b			1.538				
			(2.414)				
Observations	539	539	539	262	262	277	277
Covariates	NO	YES	YES	NO	YES	NO	YES

[+] Robust standard errors clustered at the session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Notes: The model specifications in this table refer to equation 1 in section 2.4; except model 3 which refers to equation 2. Accordingly, we control for baseline PGG contributions in Panel A and for baseline SVO angle in Panel B. Numbers in square brackets are Romano-Wolf p-values controlling for multiple hypotheses testing. Covariates: age, female, education, risk seeking, inequality aversion, poverty, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session. C *vs.* R refers to the comparison of the relative effect of competition and random payments. We report the estimated coefficients and corresponding robust standard errors when Equation 1 is estimated with competition as the baseline (omitted) category. [a]In column 3, we estimate the model with interactions and report the estimated coefficient for random payments when the dispersion of payments is low. [b]In column 3, we present the estimated coefficient for random payments when the dispersion of payments is high.

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

	(C vs. T)		(R vs. T)	
	(1)	(2)	(3)	(4)
	Δ PGG	Δ SVO	Δ PGG	Δ SVO
Income effect				
loser	0.0646	0.5790	-0.0590	2.0844
	(0.0859)	(4.1416)	(0.0814)	(6.1982)
winner	-0.0220	0.1261	-0.0867	1.2186
	(0.0774)	(2.9798)	(0.0745)	(4.9874)
Behavioral variables				
not inequality averse	0.0022	-0.8651	-0.0566	-0.1196
	(0.0715)	(3.3825)	(0.0729)	(4.7944)
inequality averse	0.0404	1.5702	-0.0892	3.4226
	(0.0979)	(4.2841)	(0.0904)	(6.4010)
risk seeking	0.0143	0.4117	0.0230*	0.5119
	(0.0147)	(0.3598)	(0.0118)	(0.5250)
dislikes competition	0.0238	-0.8102	-0.0493	-0.3645
	(0.0858)	(3.6660)	(0.0927)	(6.2201)
likes competition	0.0188	1.5153	-0.0965	3.6675
	(0.0808)	(3.9717)	(0.0745)	(5.0919)
Work-related variables				
does not work in teams	-0.0005	-0.2476	-0.0696	1.8968
	(0.0713)	(4.0153)	(0.0782)	(6.1735)
works in teams	0.0431	0.9527	-0.0762	1.4061
	(0.0929)	(3.8317)	(0.0870)	(5.3102)
is not aware of bonus	0.0403	1.8906	-0.1398	3.9170
	(0.0895)	(5.1206)	(0.0918)	(7.5238)
is aware of bonus	0.0023	-1.1855	-0.0060	-0.6140
	(0.0823)	(3.2735)	(0.0823)	(3.7850)
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.

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

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Table 7: Robustness Checks: Random and Fixed effects models

Panel A: Δ PGG	Pooled		High		Low	
Contributions	(1)	(2)	(3)	(4)	(5)	(6)
Competition (C)	-0.010		0.002		-0.009	
	(0.008)		(0.014)		(0.014)	
Random (R)	0.003		-0.003		0.018	
	(0.007)		(0.011)		(0.014)	
Period 2	-0.016	-0.016	0.020	0.020	-0.056***	-0.056**
	(0.016)	(0.016)	(0.014)	(0.013)	(0.021)	(0.021)
Competition × Period	0.009	0.009	-0.059**	-0.059**	0.076**	0.076**
	(0.025)	(0.025)	(0.027)	(0.026)	(0.034)	(0.033)
Random × Period	-0.005	-0.005	-0.027	-0.027	0.023	0.023
	(0.022)	(0.022)	(0.020)	(0.019)	(0.034)	(0.033)
Constant	0.098**	0.483***	0.074	0.475***	-0.044	0.492***
	(0.038)	(0.005)	(0.057)	(0.005)	(0.053)	(0.007)
R-squared Overall	0.606	0.000	0.654	0.020	0.605	0.000
R-squared Adj		0.003		0.012		0.025
Panel B: Δ SVO Angle						
Competition (C)	-0.337		-0.974		-0.176	
	(0.615)		(0.840)		(1.074)	
Random (R)	-1.159*		-1.289*		-0.347	
	(0.596)		(0.663)		(1.085)	
Period 2	-0.461	-0.461	0.109	0.109	-1.089	-1.089
	(0.735)	(0.727)	(0.632)	(0.617)	(1.403)	(1.371)
Competition × Period	-0.366	-0.366	-2.250	-2.250	1.389	1.389
	(1.326)	(1.311)	(1.572)	(1.534)	(2.154)	(2.104)
Random × Period	3.240***	3.240***	3.067**	3.067**	3.516*	3.516*
	(1.201)	(1.187)	(1.476)	(1.440)	(1.968)	(1.923)
Constant	6.691***	21.984***	5.443**	21.236***	12.014***	22.691***
	(2.499)	(0.265)	(2.360)	(0.319)	(4.244)	(0.416)
R-squared Overall	0.446	0.002	0.442	0.002	0.465	0.005
R-squared Adj		0.012		0.019		0.007
Observations	1078	1078	524	524	554	554
Number Groups	539	539	262	262	277	277
Covariates[a]	YES	NO	YES	NO	YES	NO
Fixed Effects	NO	YES	NO	YES	NO	YES
Random Effects	YES	NO	YES	NO	YES	NO

Notes: Clustered Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. [a] Covariates: age, female, education, risk seeking, inequality aversion, poverty, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

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Table 8: Robustness Checks: Restriction of dataset to dyads with one winner and one loser

	(1) Pooled	(2) Pooled	(3) High	(4) High	(5) Low	(6) Low
Panel A: Δ PGG contributions						
Competition (C)	-0.035	-0.030	-0.079**	-0.040*	0.010	0.019
	(0.0240)	(0.0207)	(0.0300)	(0.0195)	(0.0339)	(0.0382)
Random (R)	-0.035	-0.011	-0.056**	-0.021	-0.008	0.025
	(0.0219)	(0.0224)	(0.0237)	(0.0220)	(0.0361)	(0.0452)
Baseline PGG (Y_{i0})	-0.469***	-0.481***	-0.446***	-0.463***	-0.480***	-0.495***
	(0.0522)	(0.0522)	(0.0689)	(0.0745)	(0.0755)	(0.0737)
Constant	0.241***	0.230**	0.247***	0.209*	0.223***	-0.0712
	(0.0299)	(0.0868)	(0.0414)	(0.116)	(0.0411)	(0.136)
R-squared	0.240	0.273	0.243	0.337	0.251	0.335
C vs. R	0.000	-0.018	-0.023	-0.020	0.018	-0.006
	(0.024)	(0.020)	(0.027)	(0.022)	(0.031)	(0.022)
Panel B: Δ SVO angle						
Competition (C)	-1.605	-1.450	-3.821**	-4.035*	0.457	-0.422
	(1.235)	(1.246)	(1.713)	(2.199)	(1.661)	(2.228)
Random (R)	0.951	0.140	-0.435	0.332	2.288	1.241
	(1.147)	(1.416)	(1.548)	(1.563)	(1.710)	(2.401)
Baseline SVO (Y_{i0})	-0.657***	-0.645***	-0.671***	-0.675***	-0.659***	-0.595***
	(0.0510)	(0.0530)	(0.0729)	(0.0800)	(0.0756)	(0.0731)
Constant	15.33***	11.88*	16.04***	8.966	14.86***	20.64*
	(1.488)	(5.916)	(2.197)	(6.072)	(2.103)	(10.59)
R-squared	0.322	0.355	0.315	0.367	0.342	0.412
C vs. R	-2.560**	-1.590	-0.386**	-4.367**	-1.832	-1.663
	(1.164)	(1.215)	(1.505)	(1.752)	(1.625)	(1.454)
Observations	481	481	238	238	243	243
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.
Note: The model specifications refer to equation (1) in section 2.4. In this analysis, we reduce the data set to the dyads with one winner and one loser only and exclude winner-winner and loser-loser matches. Per design, in competition and random there were only winner-loser dyads. Hence, the data set is reduced by 58 observations from 189 to 131 under threshold (from 539 to 481 for the whole sample).
[a] Covariates: age, female, education, risk seeking, inequality aversion, poverty, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

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Table 9: Robustness Checks: Models with interaction on incentives and dispersion

Panel A	(1)	(2)	(3)	(4)	(5)
PGG contributions	Δ PGG	Δ PGG	Δ PGG	Contrib.	Contrib.
Competition (C)	0.042	0.043	0.087*	-0.016	
	(0.029)	(0.028)	(0.029)	(0.011)	
Random (R)	0.022	0.050	0.059	0.014	
	(0.030)	(0.033)	(0.036)	(0.009)	
High dispersion (HD)	0.071*	0.084*	0.086*	0.000	
	(0.024)	(0.023)	(0.024)	(0.020)	
C × HD	-0.120*	-0.113*	-0.130*	0.011	
	(0.039)	(0.039)	(0.040)	(0.015)	
R × HD	-0.077**	-0.096**	-0.075***	-0.023*	
	(0.036)	(0.037)	(0.040)	(0.011)	
Period				-0.056**	-0.056**
				(0.021)	(0.021)
C × Period				0.076**	0.076**
				(0.033)	(0.032)
R × Period				0.023	0.023
				(0.033)	(0.032)
Period × HD				0.076**	0.076**
				(0.025)	(0.025)
C × Period × HD				-0.136**	-0.136**
				(0.042)	(0.042)
R × Period × HD				-0.050	-0.050
				(0.038)	(0.038)
Baseline PGG (Y_{i0})	-0.444*	-0.457*			
	(0.046)	(0.046)			
Constant	0.174*	0.148***	-0.041	0.102**	0.483*
	(0.027)	(0.078)	(0.089)	(0.039)	(0.004)
R-squared	0.233	0.257	0.048	0.025	0.025
Observations	539	539	539	1078	1078
Groups	539	539	539	539	539
Covariates[a]	NO	YES	YES	YES	NO
Fixed Effects	NO	NO	NO	NO	YES
Random Effects	NO	NO	NO	YES	NO

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Panel B	(1)	(2)	(3)	(4)	(5)
SVO Angle	Δ SVO	Δ SVO	Δ SVO	Angle	Angle
Competition (C)	1.294	0.909	0.497	0.179	
	(1.522)	(1.626)	(2.122)	(2.427)	
Random (R)	3.121***	2.378	1.498	0.329	
	(1.583)	(1.980)	(2.068)	(2.217)	
High dispersion (HD)	1.523	1.293	1.077	0.267	
	(1.262)	(1.353)	(1.550)	(1.569)	
C × HD	-4.908**	-4.235***	-2.350	-2.221	
	(2.058)	(2.360)	(2.610)	(2.760)	
R × HD	-3.342***	-2.697	0.031	-3.907*	
	(1.972)	(2.246)	(2.460)	(2.310)	
Period				-1.089	-1.089
				(1.375)	(1.358)
C × Period				1.389	1.389
				(2.111)	(2.084)
R × Period				3.516*	3.516*
				(1.929)	(1.904)
Period × HD				1.198	1.198
				(1.508)	(1.489)
C × Period × HD				-3.639	-3.639
				(2.611)	(2.578)
R × Period × HD				-0.448	-0.448
				(2.409)	(2.378)
Baseline SVO (Y_{i0})	-0.669*	-0.658*			
	(0.051)	(0.052)			
Constant	14.253*	13.518**	-6.266	27.486***	21.984***
	(1.656)	(5.350)	(6.469)	(4.507)	(0.262)
R-squared	0.332	0.351	0.065	0.018	0.018
Observations	539	539	539	1078	1078
Groups	539	539	539	539	539
Covariates[a]	NO	YES	YES	YES	NO
Fixed Effects	NO	NO	NO	NO	YES
Random Effects	NO	NO	NO	YES	NO

[+] 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 seeking, inequality aversion, poverty, preference for competition, months employed, bonus awareness, job satisfaction, close relations, order of PGG and SVO, day and time of the session.

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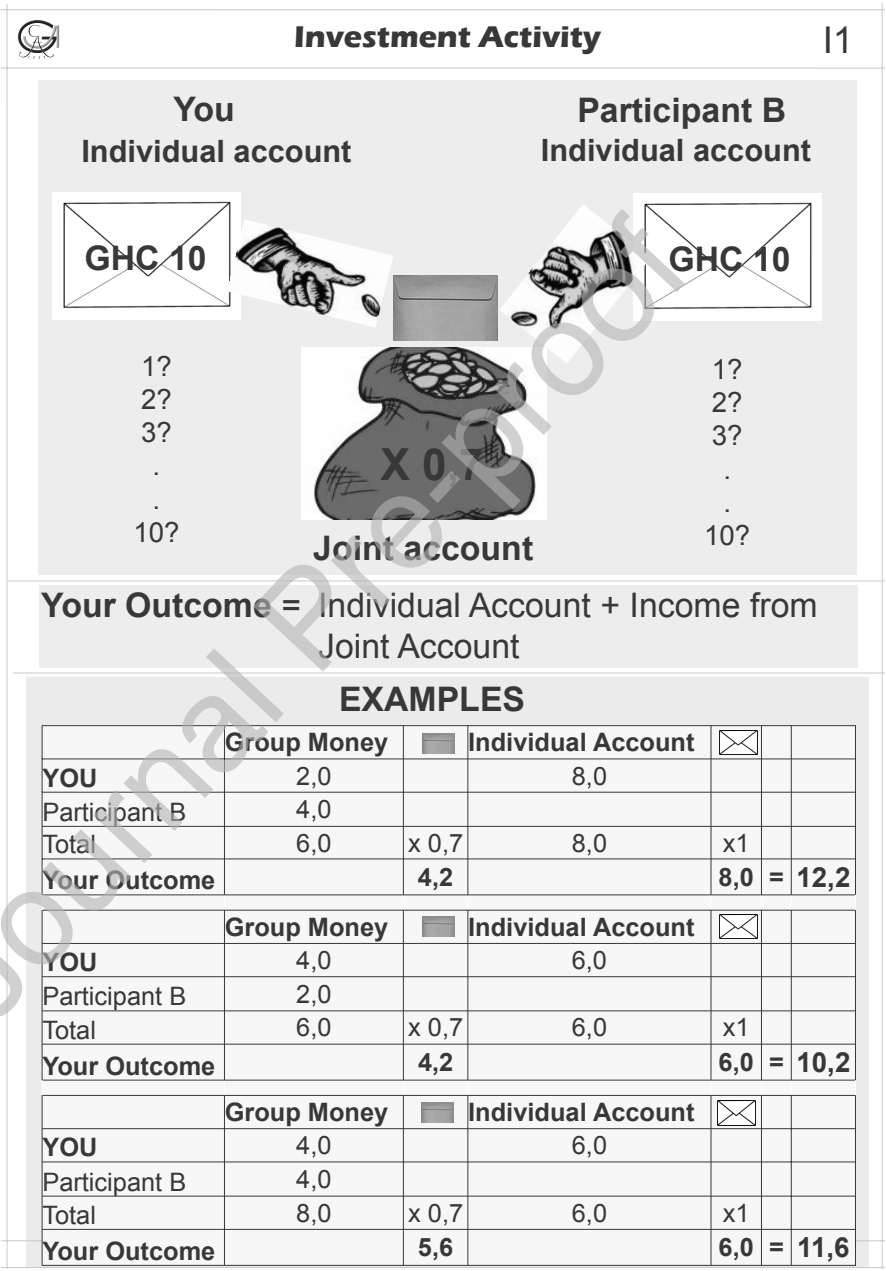
Figure 4: PGG poster



	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**

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Figure 5: SVO poster

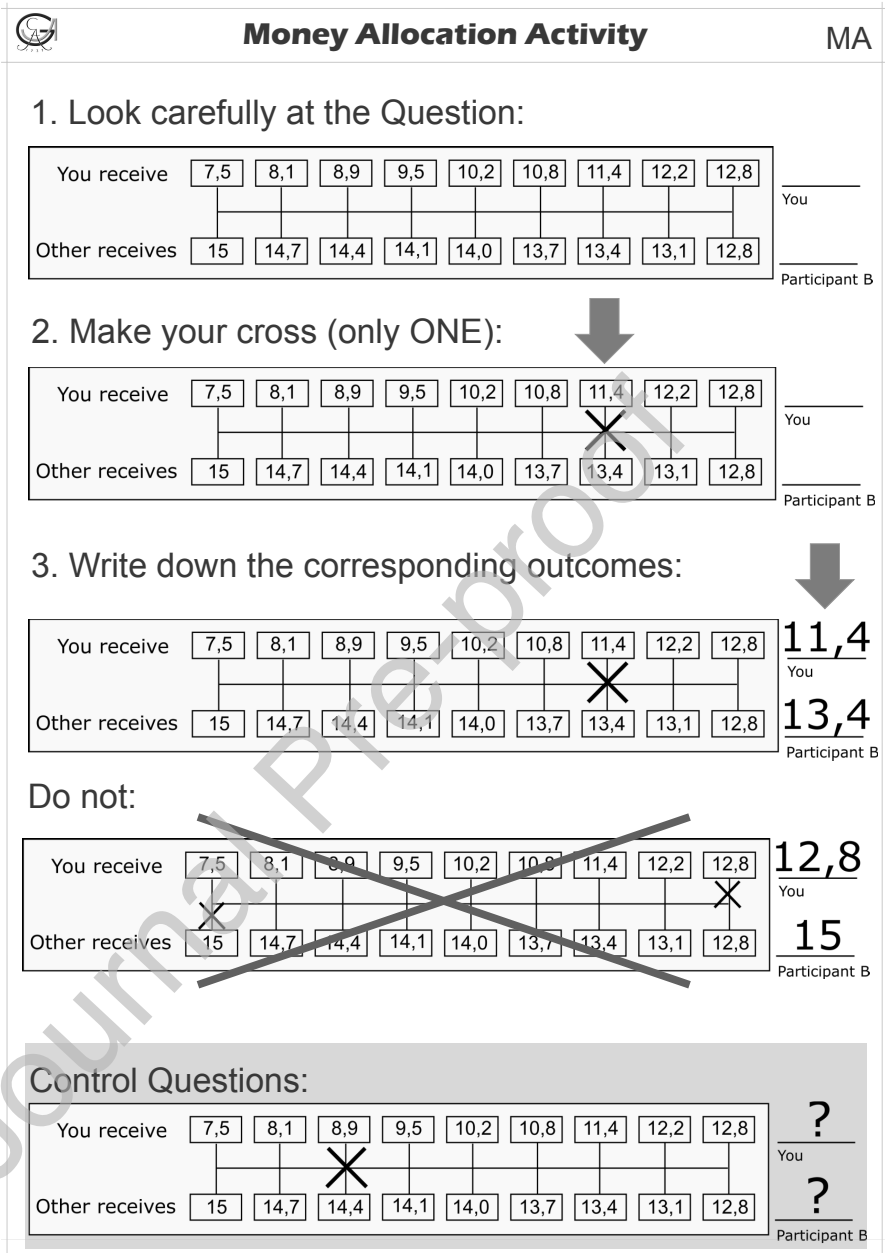

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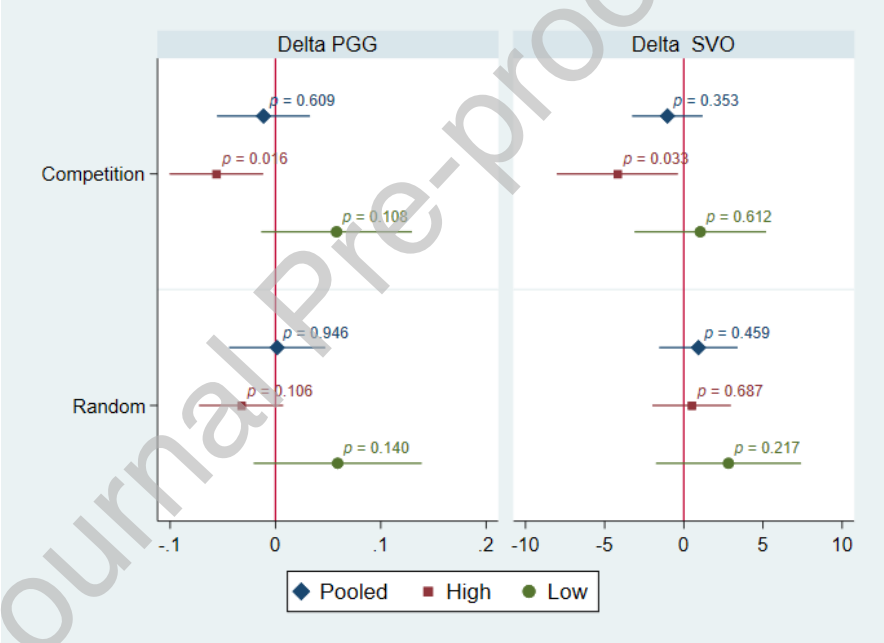
Figure 6: Treatment Effects by Payment Dispersion



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