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United we stand: On the benefits of coordinated punishment

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Department of Economics

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Abstract

Coordinated punishment requires a specific number of punishers to be effective (otherwise, no damage is inflicted on the target) but it also exhibits *returns to scale*. While societies often rely on this punishment device, its benefits are unclear compared with uncoordinated punishment, where punishment decisions are substitutes. We argue that coordinated punishment can prevent the free-riding of punishers and show, both theoretically and experimentally, that this may be beneficial for cooperation in a team investment game, compared with uncoordinated punishment. Nevertheless, efficiency is not enhanced since punishment is more extensively used when it is coordinated.

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"If we are together nothing is impossible.
If we are divided all will fail."
Winston Churchill

I. Introduction

Members of society do often need to coordinate their actions to inflict punishment on others. Hence, workers who decide over a work stoppage to protest at unfair wages or poor labor conditions can refuse to attend work or picket outside the workplace, but what crucially matters for the protest to be successful is that a sufficient number of them go on strike. Otherwise, workers will inflict no damage on their principal despite bearing the cost of the protest (e.g., workers will not be paid for the period they withdraw their job). In fact, any protest or mobilization is doomed to fail if the number of participants falls below a certain threshold (Chwe 2000, De Mesquita 2010, Edmond 2013, Kiss et al. 2017). Thus, punishment may require a critical mass to be effective. Arguably, and consistent with the idea that the "strength is in numbers", members of society inflict more damage when they punish together *as a group* than when they do it individually or *fight their own battles*. This, in turn, suggests that punishment may also exhibit increasing returns to scale. In this paper, we incorporate these two features to study what we call *coordinated* punishment.

We consider a team investment game that resembles the principal-agent relationship. In this game, there are two investors and one allocator (Cassar and Rigdon, 2011; Olcina and Calabuig 2015). Investors can be thought to be the workers of a firm, who choose whether or not to put effort in a joint project, while the allocator is the principal that controls the proceeds of the investment and decides how to share the returns of investment. Our aim is to compare the efficacy of the coordinated punishment device with the (traditional) uncoordinated punishment, where *i*) punishers can always inflict some damage to others by incurring a cost and, *ii*) the inflicted damage depends (usually linearly) on the number of punishers. In our setting, investors can punish the allocator at a cost upon observing the returned amount. If punishment is uncoordinated, the allocator's earnings are reduced by 30% (60%) if only one of the (both) investor(s) decides (decide) to punish. The two characteristic aspects of coordinated punishment determine the rest of our treatment conditions. In these treatments, investors do not reduce the allocator's earnings unless they both decide to punish; i.e., the allocator's payoffs are not reduced if only one investor punishes. In one of the treatments, the allocator's earnings are reduced by 60% if both investors punish, therefore punishment requires coordination to be effective. In the other treatment, investors –by coordinating their actions and punishing together– can reduce the allocator's earnings by 80%, therefore coordinated punishment is more effective than uncoordinated in this treatment when both investors punish. While we are mainly interested in assessing the efficacy of coordinated punishment, this device allow us to tease apart its two distinct features: the fact that it requires coordination to be effective and the fact that it exhibits increasing returns to scale.

To derive our testable predictions, we assume that subjects can have other-regarding preferences (Fehr and Schmidt 1999, Bolton & Ockenfels 2000, Charness and Rabin 2002). We propose a two-sided incomplete information model with two possible types of investors (selfish and reciprocators) and two possible types of allocators (fair-minded and profit-maximizer). We assume that reciprocal investors care about inequality and will be willing to punish the allocator if they do not observe a fair return; while selfish investors simply focus on their own material payoff and consequently never punish.[1] The fair-minded allocator has a dominant strategy that consists in choosing the fair return, while the profit-maximizer allocator has to choose between returning nothing (and then being possibly punished) or returning the minimum positive amount that prevents him from being punished. We prove the existence of efficient pooling equilibria in which both the selfish and the reciprocal investor decide to invest in equilibrium. We also show that joint investment is more likely to occur when punishment is coordinated. While the intuition is that requiring coordination in the punishment stage can hinder joint investment in the case of coordinated punishment because reciprocal investors cannot reduce the inequality by their own, we show that requiring coordination prevents the free-riding behavior on punishment when investors are reciprocal. As a result, joint investment will be more likely when punishment is coordinated if the proportion of reciprocal investors is sufficiently high. Allocators anticipate that (for certain values of the proportion of reciprocal investors) investors will be more willing to punish when punishment is

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[1] Reciprocal investors consider that it is fair to receive at least half of the surplus that is generated after their investment decision. Our results, however, are robust to other definitions of fair return as discussed in Section III.

coordinated, thus they return a larger amount when punishment is coordinated than when it is uncoordinated. The key of these results lies on the fact that investors' beliefs about the proportion of fair-minded allocators need to be higher in the uncoordinated punishment than in the coordinated one in order for investment to be profitable.

We conduct a laboratory experiment to test these predictions. As our examples on labor conditions or protests are characterized by long-term relationships, our experimental protocol considers the team investment game described above, where subjects interact across rounds in a fixed-matching protocol. Overall, our experimental data lend support for the benefits of coordinated punishment in that we observe more joint investment and higher returns when punishment is coordinated. One may expect that these findings translate into more efficiency gains when punishment is coordinated. Empirically, this does not seem to be the case. When punishment is uncoordinated, there is evidence of free-riding behavior in the punishment stage. In particular, investors tend to punish less if they observe that the other investor did punish in the previous round. Eventually, this discourages joint investment, thus investors end up not investing when punishment is uncoordinated. Investors are more likely to invest when punishment is coordinated, but they also more likely to punish together *as a group*; in fact, there is evidence of a solidarity effect in that investors are more likely to punish if they observe that the other investor in the team did it in the previous round. This, in turn, implies that the high levels of joint investment are sustained with high levels of joint punishment, thus there is a significant surplus destruction when punishment is coordinated. In fact, the higher the joint punishment in the coordinated devices is, the higher the levels of joint investment are which, eventually, turns into a higher level of destruction and, therefore, efficiency is not enhanced.

To the best of our knowledge, this is the first paper that investigates the benefits of coordinated punishment in an asymmetric environment like the investment game. The positive effects of individually costly punishment to achieve better outcomes have been already suggested in anthropology (Boyd et al., 2003; Hauert et al. 2007), evolutionary game theory (Fehr and Fischbacher, 2003; Olcina and Calabuig, 2015) or experimental economics (Ostrom et al. 1992; Fehr and Gächter, 2000; Chaudhuri, 2011). While these works highlight the benefits of punishment to sustain cooperation, punishment decisions in these models are always assumed to be uncoordinated or substitutes. In the current paper, we extend the discussion to the case in which punishment decisions are complement and can result in returns to scale.[2]

There is an existing body of works that systematically looks at the effects of punishment on the investment game (see, among others, Fehr and Rockenbach 2003, Fehr and List 2004, Houser et al. 2008, Rigdon 2009, Calabuig et al. 2016). Yet these papers rely on the dyad version of the game (Berg et al. 1995), thus they are not well suited to compare the efficacy of uncoordinated and coordinated punishment. The most closely related works are the theoretical models of Boyd et al. (2010) and Olcina and Calabuig (2015), who highlight the benefits of coordinated punishment in an evolutionary setting. Boyd et al. (2010) consider a prisoners dilemma and show that cooperation can be sustained as an equilibrium outcome when punishers divide the cost of the punishment if they coordinate their actions and decide to punish. In the model of Olcina and Calabuig (2015), there are two investors and one allocator who interact in an overlapping-generations dynamic model. As in Boyd et al. (2010), it is possible to sustain a cooperative equilibrium in the presence of coordinated punishment when the (individual) cost of punishment decreases as the number of punisher increases. In their setting, however, there is also the possibility of peer punishment as investors can punish each other after observing the punishment decision of the other team members. Our contribution to this literature is to highlight the benefits of coordinated punishment in an asymmetric situation that resembles the principal-agent relationship. In that vein, we show that coordinated punishment may be beneficial for the joint investment even if subjects do not divide the cost of the punishment and peer-punishment is not allowed.[3] In addition, we use a different approach to the previous studies with the experimental methodology to test the theoretical results in a simplified setting.

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[2] In a way, we extend the analysis of the minimum effort games to a setting in which subjects need to coordinate their actions to punish others. For evidence on the minimum-effort game see, among others, Harrison and Hirshleifer (1989), Van Huyck et al. (1990), Brandts and Cooper (2006) or Riedl et al. (2015).

[3] Somewhat related to the idea of that punishment requires coordination, Casari and Luini (2009), Ertan et al. (2009) or Noussair and Tan (2011), among others, allow subjects to vote over different punishment schemes in a public good game. This usually results in efficiency gains because subjects tend to punish below-average contributors and strong cooperators are barely punished.

The rest of the paper is organized as follows. Section II presents our experimental design. We present our theoretical predictions in Section III and summarize our findings in Section IV. Section V concludes. The proofs are relegated to the online appendix. This contains additional material such as the experimental instructions or further analysis on our data.

II. The team investment game with punishment

A. *Experimental Design*

We consider a team investment game with two investors and one allocator (Cassar and Rigdon 2011, Olcina and Calabuig 2015). Each player is initially endowed with 20 Experimental Currency Units (ECUs hereafter). They interact as follows:

- **STAGE 1 (Investment):** investors choose simultaneously whether to invest in a joint project. In a principal-agent relationship, this decision can be interpreted as agents deciding whether to exert effort in a common project. If none of the investors (or only one of them) invest, the game ends and the investor who decided to invest (if any of them) pays off the cost of the investment (5 ECUs). When both investors decide to invest, each pays the cost of the investment, but the total investment (10 ECUs) is multiplied by 3, and the game proceeds to stage 2.
- **STAGE 2 (Return):** the allocator chooses the amount of ECUs, $\in [0,30]$, to return. Any returned amount is equally divided between the two investors of the team, thus each investor receives$x/2$. The allocator adds the amount he keeps (30 - x) to his initial endowment.
- **STAGE 3 (Punishment):** investors are allowed to punish the allocator upon observing the returned amount. The punishment has a cost of 5 ECUs for each investor and reduces the allocator’s payoff in a given share $\lambda_n \in [0,1]$ depending on the number of investors that punish, $n \in \{0, 1, 2\}$. If none of the investors decide to punish, then no damage is inflicted to the allocator (λ_0 = 0). The

reduction in the allocator's payoffs if only one or both investors punish (λ_n for n = {1, 2}) varies across treatments.[4]

We summarize the sequence of decisions in Figure 1.

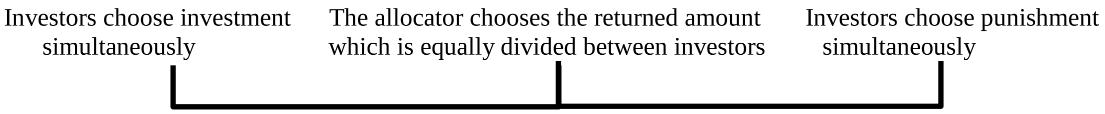

Figure 1. Timeline of decisions

Before proceeding with the treatment conditions, it is worth noticing that we extend upon the investment in Cassar and Rigdon (2011) to allow for the punishment stage. In their game, Cassar and Rigdon (2011) find that the amount received by each investor may be important when allocators decide their return. To overcome this issue, our design restricts the investment decision to be a binary choice in Stage 1. We also make sure that the returned amount is equally split between investors in Stage 2. This is to avoid that investors compare themselves before deciding whether to punish the allocator in Stage 3; in fact, punishment decisions in our setting are independent on the amount received by the other investor (or the amount sent by investors).

Treatments.—

In our **first treatment (UP$_{30,60}$)**, punishment is uncoordinated. If only one of the investors decides to punish, the allocator's payoffs are reduced by λ_1= 0.30. If both investors decide to punish, the allocator's payoffs are reduced by λ_2 = 0.60.[5]

In our **second treatment (CP$_{0,60}$)** punishment is coordinated and the allocator's payoffs are reduced by λ_1= 0 [λ_2 = 0.60] if only one of the investors [both investors] decide to punish. Hence, our second treatment incorporates the idea that *investors need to coordinate their actions* to reduce the allocator's payoffs.

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[4] Note that there is not a proportional relationship between the cost of the punishment and the inflicted damage over allocators. The idea is to avoid that investors' power for punishment decrease when allocators return very few since this is precisely the case where investors may have more motives to punish.

[5] If the investment is successful and the allocator returns nothing, the payoffs would be π_I = 15 ECUs for investors and π_A = 50 ECUs for allocators. If any investor punishes, she pays 5 ECUs and the allocator's payoffs are reduced by 0.3 (50) = 15 ECUs. This implies that the effectiveness of punishment (i.e., the factor which punishment reduces the allocator's payoff) is equal to 15/5 = 3. This factor is frequently used in other experiments that allow for punishment in the investment game; e.g., see Charness, Cobo-Reyes and Jimenez (2008) or Rigdon (2009).

Our **third treatment ($\mathbf{CP_{0,80}}$)** incorporates the two characteristic aspects of coordinated punishment, as the allocator's payoffs are reduced by $\lambda_1 = 0$ [$\lambda_2 = 0.80$] if only one of the investors [both investors] decide to punish.

Payoffs.— Let the dummy 1_i take the value 1 if investor *i* decides to invest an 0 otherwise, while the dummy $1_J = 1$ when both investors decide to invest and 0 otherwise. Similarly, let the dummy 1_p denote whether the investor decides to punish or not. The final payoffs for investors, π_I, are determined as follows:

$$\pi_I = \begin{cases} 20 - 5\,1_i & \text{if } 1_J = 0 \\ 15 + \left(\frac{x}{2}\right) - 5\,(1_p) & \text{if } 1_J = 1 \end{cases} \qquad (1)$$

The final payoff for the allocator, π_A, is determined as follows:

$$\pi_A = \begin{cases} 20 & \text{if } 1_J = 0 \\ (50 - x)(1 - \lambda_n) & \text{if } 1_J = 1 \end{cases} \qquad (2)$$

where $\lambda_n = \{\lambda_0, \lambda_1, \lambda_2\}$ denotes the reduction in the allocator's payoff after the punishment decisions.

We summarize our treatment conditions in Table 1.

Table 1. Summary of treatment conditions

	Reduction in allocators' payoff (λ_n)		
Treatment	**None of the investors punish (λ_0)**	**Only one investor punishes (λ_1)**	**Both investors punish (λ_2)**
$\mathbf{UP_{30,60}}$	0%	30%	60%
$\mathbf{CP_{0,60}}$	0%	0%	60%
$\mathbf{CP_{0,80}}$	0%	0%	80%

Note. Punishment has an individual cost of 5 ECUs and it is only allowed in Stage 3 if both investors decided to invest in Stage 1.

B. *Procedures*

We recruited a total of 225 subjects (75 per treatment) to participate in six different sessions, conducted at the LINEEX (University of Valencia). Subjects were business and economics undergraduate students with no experience in similar experiments. The experiment was conducted using the z-Tree software (Fischbacher, 2007), and no subject participated in more than one session. Subjects were recruited using the electronic recruitment system of the laboratory.

In our experiment, subjects played the team investment game for 15 periods in a partners matching protocol with the subjects' role, investor (Player A) or allocator (Player B), being fixed during the whole session.[6] In each period, investors had to choose whether to invest in a joint project. We employed the strategy method for allocators by asking them the amount of money that they would like to return if the investment turned out to be successful in each period; this decision was binding and disclosed to investors in the case of joint investment. More precisely, a screenshot at the end of each period informed subjects about the decisions of each investor in the group, the amount returned by the allocator (if there were joint investment) and the punishment decisions of investors when punishment was feasible.

At the end of the experiment, subjects were paid a 5€ show-up fee plus their earnings from the experiment. All the amounts referred to ECUs in our experiment, which were transformed into Euros to pay subjects (3 ECUS = 1€). On average, each person received about 16€ for a 60 minutes session. A questionnaire at the end of the session was used to elicit, among others, the subjects' gender, age, cognitive abilities (Frederick, 2005), risk aversion (Gneezy and Potters, 1997) or trusting behavior, as measured by the question in the General Social Survey (Glaeser et al. 2000). We shall use these variables as controls in our econometric analysis. The interested reader can find the translated version of the experimental instructions and the complete questionnaire in Appendix B. This includes a summary of the demographic variables that we collected in our questionnaire (see Table B.1)

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[6] In the experiment, the repeated game was preceded by a practice round for subjects to get familiar with the software. Subjects were re-matched after the practice round to play the repeated game, where we fixed the groups. At the end of the experiment, subjects were paid for their practice round and one randomly selected period of the repeated game. We observe no difference between the decisions in the practice round and the first period of the repeated game in any of the treatment. The interested reader can consult Appendix C for this analysis and further details about the decisions in the practice round.

III. Theoretical predictions with two-sided incomplete information

There is ample evidence that subjects are not purely self-interested but have other-regarding preferences (Fehr and Schmidt 1999, Bolton & Ockenfels 2000, Charness and Rabin 2002). Next, we present a two-sided incomplete information model, where investors can be selfish or reciprocal, and allocators can be profit-maximizers or fair-minded. Then, we derive our theoretical predictions of the stage game. More precisely, we characterize the efficient pooling equilibria in which both the selfish and the reciprocal investor decide to invest in equilibrium in Stage 1.[7] In this section, we also discuss what is the optimal return behavior of allocators in equilibrium and present our testable hypotheses for the repeated game.

A. *The model*

We consider two types of investors selfish (s) and reciprocal, (r) and two types of allocators: profit-maximizers and fair-minded. Types are private information but it is common knowledge that there is a proportion $q_r \in [0,1]$ of reciprocal investors and a proportion $m_f \in [0,1]$ of fair-minded allocators; the remaining $(1 - q_r)$ investors have selfish preferences given by (1), while the remaining $(1 - m_f)$ allocators are profit maximizers and have utility function given by (2).

Reciprocal investors dislike a disadvantageous inequality and thus care about the distribution of the surplus. In particular, they compare their payoff to half of the payoff that the allocator decided to keep from the amount generated by the investment (disregarding allocators' initial endowments). [8]Their utility after receiving the return is as follows:

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[7] The predictions are trivial if players are purely self-interested and preferences are given by equations (1) and (2). In this setting, investors will never incur the cost of punishment in the last stage of the game. This, in turn, implies that allocators will decide to return nothing in the second stage and investors will not invest in the first stage, as a result. These predictions hold in all the three treatments.

[8] We assume that the utility loss from disadvantageous inequality α is 2 in our model, following the estimation by Fehr and Schmidt (1999). For evidence that people are willing to punish to attain more equal outcomes see, among others, Houser and Xiao (2010) or Bone and Raihani (2015).

(3) $U_I^r = \pi_I - 2\max\left\{\frac{30-x}{2} - \frac{x}{2}, 0\right\} = \pi_I - 2\max\{15 - x, 0\}$.

where π_I is given by equation (1). The utility function of reciprocal investors embraces the idea of inequity aversion preferences put forward by Fehr and Schmidt (1999) and it implies that any return that falls below what reciprocal investors consider a "fair return" (x = 15) will generate disutility for them.[9] When reciprocal investors observe an "unfair return" (x < 15), they may punish the allocator. The payoffs of reciprocal investors who decide to punish will be given by:

(4) $U_I^r = \left(10 + \frac{x}{2}\right) - 2\max\left[(1-\lambda_n)\left(15 - \frac{x}{2}\right) - \frac{x}{2}, 0\right]$

where the value of $\lambda_n \in [0,1]$ depends on the number of investors in the team who decided to punish.

In the population, there is also a proportion $m_f \in [0,1]$ of fair-minded allocators. We hereafter assume that their dominant strategy is to choose the fair return x = 15. The rest of allocators are selfish, and their optimal strategy would consist in comparing the expected cost of being punished and the cost of avoiding punishment, as we will discuss below.

B. *Theoretical predictions*

We solve our model by backward induction. We first characterize the punishment behavior of investors in Stage 3. Then, we derive the optimal return of allocators in Stage 2, and the optimal behavior of investors regarding whether or not to invest in Stage 1. Appendix A presents these predictions in more detail. Next, we discuss the conditions for the existence of an efficient (Perfect Bayesian) pooling equilibrium in which both the selfish and the reciprocal investor decide to invest in Stage 1. We also discuss the punishment behavior of investors in the efficient

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[9] Although we derive our predictions under the assumption that reciprocal investors want to receive at least half of the surplus, our results are robust to other specifications. For example, we may consider that it is fair for investors to receive at least their investment (x = 10) or for allocators to divide the surplus in three identical parts including allocators' initial endowment (x = 20). These alternative modelizations have an effect on the thresholds values for which allocators will be willing to punish, but they do not affect our qualitative predictions regarding the effects of the treatments.

equilibrium in Stage 3 and the consequences of this behavior for the optimal return that allocators choose in Stage 2.[10]

If there is joint investment, the allocator forms beliefs about the probability of facing a reciprocal investor. We denote these beliefs as μ := Prob (r | 1_J = 1). In a pooling equilibrium, the allocator's beliefs coincide with the proportion of reciprocal investors in the population; i.e., $q_r = \mu$. Our Proposition 1 characterizes the efficient pooling equilibria, which depends on the proportion of reciprocal investors (q_r) and fair-minded allocators (m_f) in the population. We let $\bar{q}$ and $\bar{m}(q_r)$ denote the minimum proportion of reciprocal investors and fair-minded allocators for the existence of the equilibria.

Proposition 1. (Efficient pooling equilibria)

a) *In the* $\boldsymbol{UP_{30,60}}$ *treatment, there is an efficient pooling equilibrium in which both types of investors choose to invest if (the proportion of reciprocal investors)* $q_r \geq \bar{q} = 0.3464$ *and (the proportion of fair-minded allocators)* $m_f \geq \bar{m}(q_r)$*, where* $\bar{m}(q_r)$ *is increasing in q. In this equilibrium, fair-minded allocators set the fair return,* $x^* = x^F = 15$*, and selfish allocators set the minimum reward such that reciprocal investors do not punish them. This optimal return,* $x^*(q_r) = ((12\ q_r + 4)/(0.3+1.4\ q_r)) < 12.5$*, is decreasing in q.*

b) *In the* $\boldsymbol{CP_{0,60}}$ *treatment [*$\boldsymbol{CP_{0,80}}$ *treatment], there is an efficient pooling equilibrium in which both types of investors choose to invest if* $q_r \geq \bar{q} = 0.6$ *[*$q_r \geq \bar{q} = 0.5$*] and* $m_f \geq \bar{m}(q)$*, where* $\bar{m}(q)$ *is decreasing in q. In these equilibria, fair-minded allocators set the fair return* $x^* = x^F = 15$ *and selfish allocators set the minimum reward such that reciprocal investors do*

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[10] The efficient pooling equilibrium with joint investment is not the only one in the game. As detailed in Appendix A, there exists also an inefficient pooling equilibrium with joint investment where selfish allocators return nothing for low values of q_r and high values of m_f (see Proposition 2 in Appendix A4). These equilibria are inefficient because reciprocal investors optimally punish these unfair returns and there is surplus destruction. For low values of q and intermediate values of m_f there also exists a separating equilibrium in all treatments, where reciprocal investors do not invest while selfish investors choose to invest. In these equilibria, joint investment has a probability $(1 - q_r)^2$ and selfish investors decide to invest because there is a significant fraction of fair-minded allocators who return the fair amount. Reciprocal investors choose not to invest in these equilibria because of the presence of a significant fraction of selfish allocators who pay very low returns. This explains why this equilibrium only exists for an intermediate range of values of m_f. Finally, for every $q_r \in [0,1]$ and $m_f \in [0,1]$ there is always an inefficient (non-investment) equilibrium in which both types of investors decide not to invest.

not punish them. This optimal return, $x^*(q_r) = ((30q_r - 5)/(2q_r)) < 12.5$*, is increasing in* q_r.

Proof. See Appendix A3.

Figure 2 depicts the set of pairs (q_r, m_f) for which there is joint investment in the (Perfect Bayesian) pooling equilibrium for each treatment. To check whether joint investment is more likely when punishment is coordinated, we can derive the expression of the area in which the efficient pooling equilibrium exists:

$$A = (1 - \bar{q}) - \int_{\bar{q}}^{1} \bar{m}(q)\, dq$$

We prove in Appendix A3 that this area is larger in $CP_{0,80}$ than in $CP_{0,60}$, and it is also larger in $CP_{0,60}$ than in $UP_{30,60}$. Thus, we expect the joint investment to be more likely when punishment is coordinated. If the proportion of reciprocal investors (q_r) is sufficiently high (e.g., when $q_r > 0.6$), we can also observe in Figure 2 that the set of efficient pooling equilibria in the $\mathbf{UP_{30,60}}$ treatment is contained in both, the set of the $\mathbf{CP_{0,60}}$ treatment and that of the $\mathbf{CP_{0,80}}$ treatment (when $q_r > 0.5$).

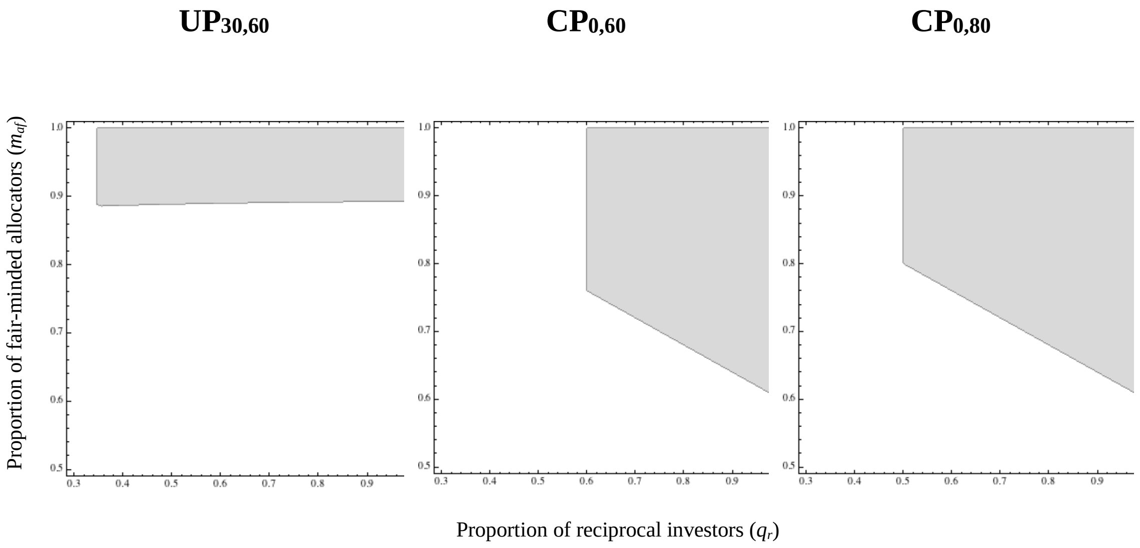

Figure 2. Efficient pooling equilibria in each treatment (grey area)

These results highlight the benefits of coordinated punishment for joint investment. The intuition is for a given q_r, it is needed a higher proportion of fair-minded allocators in the uncoordinated punishment than in the coordinated one to get positive expected returns from investment.

Coordinated punishment does not only facilitate joint investment but it also boosts the amount returned by allocators, compared with the case in which punishment is uncoordinated. This occurs because of the punishment behavior of reciprocal investors in equilibrium, which depends not only on the returned amount (x) but also on their beliefs about the proportion of reciprocal investors in the population (μ) (see Lemma 1 in Appendix A1).[11] When punishment is uncoordinated, reciprocal investors always punish in equilibrium if the return is small, regardless of the proportion of reciprocal investors in the population. This is because reciprocal investors can always reduce the inequality by punishing in the uncoordinated treatment. For intermediate or high values of the return, however, reciprocal investors paradoxically will only punish if their belief about the proportion of reciprocal investors is below a certain threshold. If reciprocal investors believe that the proportion of reciprocal investors is sufficiently high, then they may find it optimal to free-ride on the punishment decision of the other investor, which is likely to be reciprocal as well. This logic cannot be applied to coordinated punishment, where punishment by only one investor yields no losses for the allocator, therefore it is always ineffective in reducing the inequality; in fact, the necessity to achieve the threshold to reduce the allocator's earnings acts as a coordination device that eliminates the free-riding behavior in the punishment stage. Along these lines, reciprocal investors always punish in the presence of coordinated punishment when they believe that there are enough reciprocal investors in the population.[12]

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[11] It is easy to check that no punishment is the dominant action for selfish investors. Thus, only reciprocal investors may punish in equilibrium in Stage 3. In what follows, we present the intuition for the punishment behavior in each treatment in equilibrium. The proof is presented in Appendix A1.

[12] The threshold for beliefs about the proportion of reciprocal investors in the case of uncoordinated punishment $\mu^*(x)$ is decreasing in the allocator's return x, while it is increasing in the allocator's return if punishment is coordinated, and this affects the optimal return (as we shall discuss below). In all treatments, reciprocal investors never punish if the return is sufficiently high ($x \geq 12.5$), regardless of the proportion of reciprocal investors. One may expect that reciprocal investors will request at least the fair return ($x = 15$) not to punish, but recall that punishment is costly for the investors, thus reciprocal investors account for this cost when choosing whether to punish.

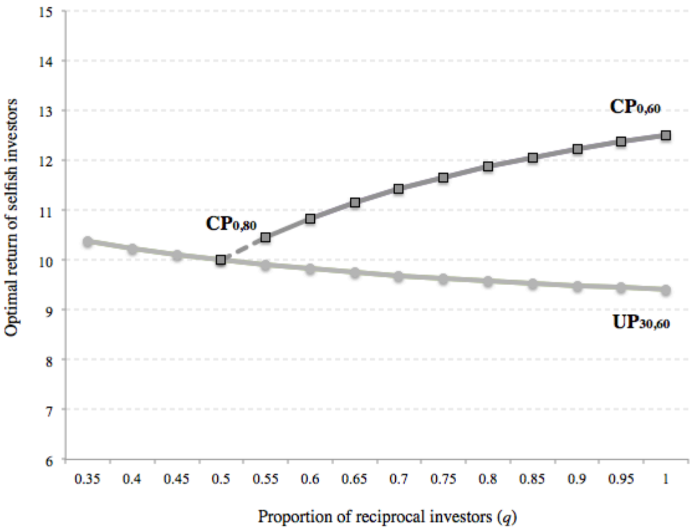

Figure 3. Optimal return of selfish allocators in the efficient pooling equilibria. In these equilibria, fair-minded allocators return the fair amount, $x = 15$.

As we show in Figure 3, the punishment behavior of investors in Stage 3 has implications for the rewarding policy of selfish allocators in Stage 2 (recall that fair-minded allocators always return the fair amount $x = 15$ in equilibrium).[13] Selfish allocators compare the expected cost of being punished and the cost of avoiding punishment. Again, their beliefs regarding the proportion of reciprocal investors in the population play a crucial role to explain behavior. In all treatments, allocators return nothing if they believe that proportion of reciprocal investors is low (i..e, for low values of μ), because the expected cost of being punished is sufficiently low. However, there is a different critical value of μ such that it is optimal for the allocator to return the minimum return that guarantees no punishment from reciprocal investors. We denote this minimum return by $\hat{x}(\mu)$. If punishment is uncoordinated, then $\hat{x}(\mu)$ is decreasing with μ, while $\hat{x}(\mu)$ is increasing in μ when punishment is coordinated (see Figure 3). The difference in behavior between the uncoordinated and coordinated treatment occurs because of the free-riding behavior of reciprocal investors: if punishment is uncoordinated, the increase in μ implies that the free-riding problem is more likely to occur, thus reciprocal investors will be less likely to punish. As a result, selfish allocators decrease the reward that they need to pay to avoid the punishment. If

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[13] Note that return only exists if there was joint investment in the first stage and this depends on having a sufficiently high proportion of reciprocal investors, which is different in each treatment.

punishment is coordinated, however, the increase in μ implies that investors are more likely to punish, therefore selfish allocators need to increase the return to avoid being punished.

C. Repeated game and Testable predictions.

In our experiment, subjects play a repeated game for 15 periods. Once we have characterized the efficient pooling equilibria of the stage-game, we use some well-known results on finitely repeated games with incomplete information to derive our testable predictions.

Using Folk theorems (Benoit and Krisna 1985, Friedman, 1971, Fudenberg and Maskin, 1986) we can posit that for a given finite horizon *T,* there is a set of distributions of the populations of investors and allocators (q_r, m_f) for which there will be joint investment along most of the equilibrium path (except probably for the last periods). Namely, there exist some minimal critical values of both *q* and *m* such that for higher values, joint investment will be observed in the equilibrium path of the repeated game. As already mentioned, the set of pairs of distributions (q_r, m_f) for which joint investment occurs in Perfect Bayesian Equilibrium in the stage game is larger for coordinated treatments than for uncoordinated treatments. Additionally, this set for the $CP_{0,60}$ treatment is contained in the set of the $CP_{0,80}$ treatment; therefore, we conclude that it is more likely to observe joint investment under coordinated punishment than under uncoordinated punishment in the repeated game.

Prediction 1. *Joint investment is more likely when punishment is coordinated.*

A similar argument can be applied for allocators' behavior, using our results in Figure 3. Because investors are more likely to punish in the presence of coordinated punishment when q_r is sufficiently high, allocators have to increase their return when punishment is coordinated if they want to avoid being punished. This yields the following prediction for allocators:

Prediction 2. *Allocators return more when punishment is coordinated.*

It is also well-known that in a repeated game with finite horizon and if there is multiplicity of Perfect Bayesian Equilibrium in the stage game, any succession of Nash equilibria of the stage game will constitute a subgame perfect equilibrium of the repeated game. Our partner-protocol might facilitate the use of different trigger strategies in the repeated game in order to sustain the joint investment. Recall that by employing trigger strategies, each investor will play the efficient pooling equilibrium of the stage game as long as the other investor does so and the allocator's return is sufficiently high, but any defection can trigger a period of punishment, in which investors deviate to a non-cooperative solution; e.g., an inefficient equilibrium of the stage game.

We argue that there are two punishment strategies to be used in our game as a trigger strategy. On the one hand, investors can punish the allocator in a given period by making use of the punishment and then reducing the allocator's payoffs in that round. As an alternative, investors can choose not to invest in future rounds. As punishment is more powerful when it is coordinated (because it is immune to the free-riding problem) we expect that investors will be more likely to use the trigger strategy that punishes in the current period when punishment is coordinated, while investors will be more prone to using the strategy that consists of non-investing in future rounds when punishment is uncoordinated.

Prediction 3. *More punishment will be observed when punishment is coordinated while more non-investing periods will be observed when punishment is uncoordinated.*

IV. Results

Next, we present some descriptive statistics for the level of the joint investment and the allocator's return. We also present an analysis on group heterogeneity as well as an econometric analysis, where we control for the dynamics in the repeated game and the demographic characteristics of individuals. We discuss the punishment behavior and the differences in total payoffs across treatments in Section IV.B. Overall, our experimenatl data lend support for the predictions that coordinated punishment fosters the levels of joint investment and increases the returned amount (Predictions 1 and 2). We also find that investors punish more often when punishment is coordinated, while they refrain from investing when punishment is uncoordinated (Prediction 3). We observe no differences in terms of efficiency across treatments.

A. *Investment decisions and returned amount*

Descriptive statistics.— Table 2 summarizes the average behavior of investors and allocators in each of the three treatment conditions. The reported values pool the observations by groups across the 15 periods. [14]

Table 2. Investment decisions and returned amount

	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$
% Joint investment	16%	25%	42%
Average intended return	8.3	12.1	11.6
% Non-zero return	66%	81%	77%
% Fair return (≥ 15)	27%	44%	41%
N (investors)	50	50	50
N (allocators)	25	25	25

Table 2 shows that coordinated punishment has a positive effect in the level of joint investment and the intended return. Statistically, the average level of joint investment in $CP_{0,60}$ is higher than in $UP_{30,60}$ ($p = 0.017$), but it is smaller in $CP_{0,60}$ than

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[14] We use the strategic method for allocators, thus we report their intended return in each period. We have also computed the return for periods in which there was a joint investment and the figures are very similar to the ones reported in this table. In fact, the test significance is even higher when we focus the analysis on these observations.

in the CP$_{0,80}$ (p = 0.031).[15] The intended amount returned is higher in CP$_{0,60}$ than in UP$_{30,60}$ (p = 0.014) but indistinguishable in CP$_{0,60}$ and CP$_{0,80}$ (p = 0.71). These findings support our theoretical Predictions 1 and 2 and may be summarized as:

Result 1. *Coordinated punishment fosters the level of joint investment.*

Result 2. *Coordinated punishment boosts the reward set by allocators.*

Dynamics of the joint investment and the returned amount.— Figure 4 displays the relative frequency of joint investment in each treatment across periods. First of all, in all the treatments, there seems to exist an end-period effect, which is not surprising given that subjects knew that there were 15 periods. Thus, results in the last period may be taken with caution. In the first period, the frequency in the joint investment is very similar in the coordinated conditions, while in UP the initial frequency of joint investment (40%) is double than in CP$_{0,60}$. Nevertheless, this 40% of group investment in the first period of UP$_{30,60}$ is followed by a dramatic decrease in the second period; thereafter the level of joint investment remains steady around 20%. There is a positive trend for the joint investment in CP$_{0,60}$ in the first 4 periods, in which joint investment is close to the CP$_{0,80}$ treatment. In period 5, however, the joint investment drops close to the level of UP$_{30,60}$. Joint investment in the CP$_{0,80}$ treatment is around 40%-50%, with the percentage being quite stable over time during the first 9 periods. Then, the frequency of joint investment goes from 56% in period 10 to 28% in period 15 (42% in period 14).[16]

Result 3. *The likelihood of joint investment decreases over periods.*

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[15] Unless otherwise noted, we rely on the Wilcoxon rank-sum (Mann-Whitney) test for pairwise comparisons and the p-values refer to one-tailed tests. We use the group averages across all 15 periods as units of observation. This guarantees independence but also ignores a large amount of information, which we account for in the econometric analysis.

[16] Statistically, the Jonckheere-Terpstra test suggests that the frequency of joint investment decreases over periods in UP$_{30,60}$ and CP$_{0,60}$ ($p < 0.001$ and $p < 0.001$, respectively), but the results are not significant in CP$_{0,80}$ ($p = 0.357$, two-tailed test). Results hold if we remove period 15 from the analysis (p = 0.013, $p < 0.001$ and p = 0.500, for UP$_{30,60}$, CP$_{0,60}$ and CP$_{0,80}$, respectively). As we will discuss below, the results for the CP$_{0,80}$ are not robust to an econometric analysis, which suggests that the likelihood of joint investment decreases over periods. (Correlation coefficient for joint investment and period is -12%, -19% and -3% for UP, CP60 and CP80, without period 15: -9%, -17% and 0%).¿Valdría la pena comentar que decrece casi el doble en CP60 que en UP?

Figure 4. Relative frequency of joint investment across periods

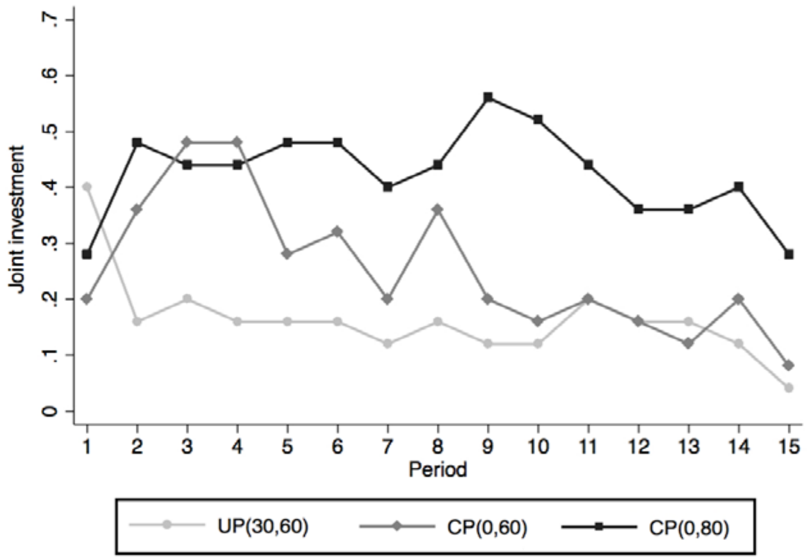

Figure 5 presents the intended returned amount in the three treatments, which does not change over periods according to the Jonckheere-Terpstra test in any of the three treatments ($p > 0.174$).

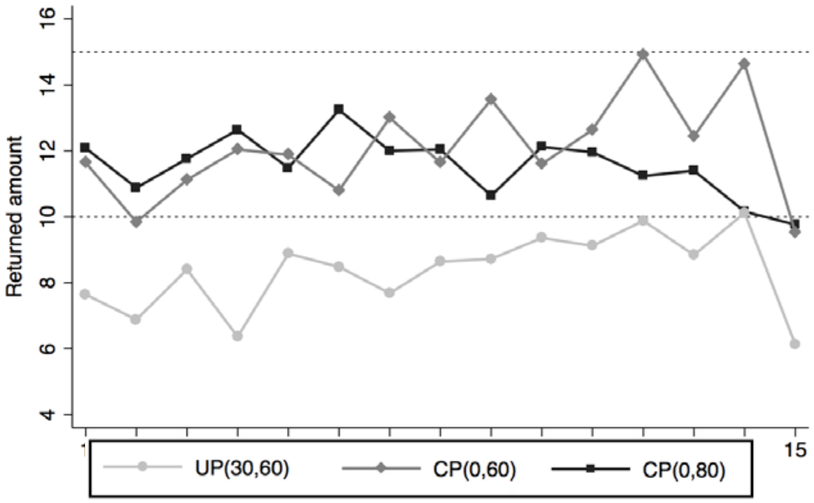

Figure 5. Intended return across periods

Result 4. *In all treatments, the returned amount is kept constant over periods.*

There are two results in Figure 5 that are worth mentioning. First, we observe that if punishment is coordinated the average returned amount is between the fair return (15 ECUs) and the return that allows investors to retrieve their investment (10 ECUs). This behavior is remarkably close to our theoretical prediction (see Figure 3). Second, the observed return is below the horizontal line of 10 ECUs when punishment is uncoordinated, thus investors do not retrieve on average what they invest in this setting. These findings have consequences for the punishment behavior of investors, as we shall discuss below.

Group heterogeneity.— A data analysis at the group level gives additional insight into the dynamics of the joint investment and the returned amount. We define *never-investing groups* as those groups that never invested in any of the 15 periods. For those groups that succeeded (in joint investment) at least once, we distinguish between the *low-investing groups* (that invested less than 7 periods) and the *high-investing groups* (that invested 7 or more periods). Table 3 presents the distribution of groups in each treatment.[17]

Table 3. Joint investment and group heterogeneity across treatments

	UP$_{30,60}$	**CP$_{0,60}$**	**CP$_{0,80}$**
Never-investing groups (no joint investment in any period)	32%	32%	28%
Low-investing groups (joint investment in less than 7 periods)	52%	40%	20%
High-Investing groups (joint investment in 7 periods or more)	16%	28%	52%

Note. Figures are rounded to add up to 100%.

Using a test of proportions, we find that the percentage of *never-investing groups* is alike across treatments ($p > 0.76$) but there are more *high-investing groups* in CP$_{0,80}$ than in CP$_{0,60}$ and UP$_{0,60}$ ($p = 0.004$ and $p = 0.042$, respectively). This confirms that coordinated punishment is beneficial for the dynamics of the investment at the group level.[18]

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[17] There are three groups that invested in exactly 7 periods. All our results are robust if we consider them as low-investing groups.

[18] We present further evidence of the different dynamics across treatments in Appendix D (see Table D2). Groups that do not invest in a particular period tend not to invest in the subsequent period, regardless of the treatment; e.g., the likelihood that there will be no joint investment in period *t* for a group that did not invest in period *t* -1 is always

The decision of the allocator in the first period may be important in explaining this dynamic. If investors retrieve what they invest, this may encourage them to keep investing in subsequent periods, while those who receive a low return may prefer not to invest. In the first period, allocators return more to investors when punishment is coordinated (see Figure 5) ($p < 0.014$ when comparing $UP_{30,60}$ and $CP_{0,80}$ or $CP_{0,60}$). This can explain the decrease in the joint investment that occurs in the $UP_{30,60}$ treatment in the second period (see Figure 4). In fact, the vast majority of the low-investing groups (80%) received less than half of the fair amount in the first period.

Econometric analysis.— While our previous findings suggest a positive effect of the coordinated punishment in the repeated game, there are some variables at stake that can affect these results (e.g., the individual characteristics or the history of decisions). In order to isolate their effect (and to confirm the robustness of our previous findings), we present an econometric analysis where we study the determinants of the joint investment and the intended return.

Our specifications for the likelihood of joint investment and the returned amount rest on the model of Arellano-Bond (1991). This is appropriate to our setting since we have a potential endogeneity problem (due to the partner matching) and we do not have exogenous variables to use as instruments. The advantage of this procedure is that the lagged of endogenous and predetermined variables are used as instruments, and it accounts for a panel data structure of the sample.[19]

Table 4 summarizes the results for the joint investment. The set of independent variables includes the groups' investment decisions in the last two periods, *Joint Investment*$_{t-1}$ and *Joint Investment*$_{t-2}$ and the amount that investors received in the previous period, conditional of the joint investment being positive, *Return*$_{t-1}$***Joint Investment*$_{t-1}$. [20] In one of our regressions, we consider the dummy variable *Joint*

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above 0.84. When a group invests in a particular period, however, we find differences in behavior across treatments. In particular, the likelihood that there will be joint investment in period t for a group that invested in period t - 1 is 0.43 in $UP_{30,60}$, 0.62 in $CP_{0,60}$, and 0.80 in $CP_{0,80}$; thus, the possibility of coordinated punishment leads investing-groups to keep investing.

[19] There are other experimental papers that use this methodology; e.g., Ashley, Ball and Eckel (2003), Fischbacher and Gächter (2010) or Brañas, Buchelia, Espinosa and García-Muñoz (2013).

[20] These are dummy variables that take the value 1 when both investors decided to invest in the previous periods. We include two lags of the dependent variable as explanatory variables since the Arellano-Bond test for AR(2) is significant and the analogous one for AR(1) is not significant. Note that the null hypothesis for AR(1) is that the

Punishment$_{t-1}$ that takes the value of 1 when both investors had the possibility of punishing and decided to do it in the previous period (this variable takes the value 0 when punishment was feasible and only one or none of the investors punished). In line with our theoretical model, we allow for the possibility that allocators care about inequality (Fehr and Schmidt 1999, Bolton and Ockenfels 2000). Our variable *Payoff Inequality$_{t-1}$* measures the difference between the allocator and the investors' payoffs in the previous period. In all the regressions, we control for the *Period* and account for individual observed heterogeneity by including a subset of the variables of the questionnaire.[21] The standard errors reported in Table 4 (in parentheses) are clustered by groups. We use the variance correction by Windmeijer (2005) to prevent inconsistency in small samples. The Hansen test (line above *Observations* in Table 4) checks the validity of the instruments.[22]

Table 4. Investors' decisions: Likelihood of joint investment using Arellano-Bond (1991)

	$CP_{0,80}$ $UP_{30,60}$	$CP_{0,80}$ $UP_{30,60}$	$CP_{0,60}$ $UP_{30,60}$	$CP_{0,60}$ $UP_{30,60}$	$CP_{0,80}$ $CP_{0,60}$	$CP_{0,80}$ $CP_{0,60}$
	(1)	(2)	(3)	(4)	(5)	(6)
Joint Investment $_{t-1}$	0.045	0.141	0.135	0.121	0.158	0.236
	(0.010)	(0.011)	(0.038)	(0.054)	(0.015)	(0.018)
Joint Investment $_{t-2}$	0.018	0.015	0.073	0.060	0.051	0.049
	(0.004)	(0.005)	(0.026)	(0.027)	(0.011)	(0.009)
Return$_{t-1}$ * Joint Investment $_{t-1}$	0.015	0.013	0.020	0.020	0.010	0.007
	(0.0004)	(0.0003)	(0.003)	(0.004)	(0.0008)	(0.0007)
Joint Punishment $_{t-1}$	0.122		-0.071		0.106	
	(0.003)		(0.043)		(0.018)	
Payoff Inequality $_{t-1}$		-0.006		-0.0006		-0.003
		(0.0004)		(0.002)		(0.0007)
Coord. Punish ($CP_{0,80}$)	0.136	0.147			0.106	0.085
	(0.035)	(0.035)			(0.032)	(0.031)
Coord. Punish ($CP_{0,60}$)			0.017	0.027		
			(0.022)	(0.027)		
Period	-0.006	-0.007	-0.011	-0.013	-0.011	-0.015
	(0.001)	(0.0006)	(0.002)	(0.003)	(0.003)	(0.002)
Constant	-0.010	-0.079	-0.057	-0.032	0.295	0.342
	(0.121)	(0.112)	(0.052)	(0.074)	(0.121)	(0.114)
Heterogeneity	Yes	Yes	Yes	Yes	Yes	Yes
Instruments	41	41	32	26	41	41

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dependent variable follows an autocorrelation process exclusively of order 1. When we include a third lag of the dependent variable is always insignificant.

[21] In particular, we included age, the number of correct answers in the Cognitive Reflection test, an index of risk aversion and a dummy that captures trust.

[22] For references about how to select a valid set of instruments in the Arellano-Bond model, see for instance Roodman (2006) and (2009).

Hansen Test	p = 0.459	p =0.439	p = 0.320	p =0.954	p = 0.915	p =0.954
Arellano-Bond Test AR(2)	p < 0.001	p < 0.001	p < 0.001	p < 0.001	p < 0.001	p < 0.001
Arellano-Bond Test AR(1)	p = 0.459	p =0.439	p = 0.320	p =0.954	p = 0.915	p =0.954
Observations	650	650	650	650	650	650

Note:. Robust standard errors in parentheses clustered by groups.

Our treatment dummies confirm the positive effects of coordinated punishment on the levels of joint investment, with the results being driven by the higher efficacy of the coordinated punishment rather than because this needs to be coordinated. In that vein, we find that the dummy variable for coordinated punishment $CP_{0,80}$ is always significant when comparing it to $UP_{30,60}$ or $CP_{0,60}$ ($p < 0.01$), but there is no difference between $CP_{0,60}$ and $UP_{30,60}$. In line with Casar and Rigon (2011), we also find evidence of "homegrown trusting preferences" in that investors are more likely to invest if they did it in the previous period. Unsurprisingly, the amount received by investors in the previous period has also a positive effect on the likelihood of joint investment. The effect of both investors punishing in the past is positive as well (except in the uncoordinated treatment). Consequently, the use of punishment seems to facilitate the investment decision. As for inequality, we find that investors are less likely to invest, the higher the difference between the allocator and the investor's payoffs in the previous period is, especially if punishment is coordinated. Finally, our analysis points out a negative effect of the period, with joint investment decreasing over periods in all treatments.[23] We summarize these findings as follows:

Result 5. *We observe that (ceteris paribus) the likelihood of joint investment depends:*

i) Positively on the previous investment decisions, the amount returned by the allocator in the previous period, and the decision to jointly punish in the previous period,

ii) Negatively on the difference between the allocator and the investors' payoffs and the period in which the decision is made.

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[23] If we conduct a regression for each separate treatment, the period variable is always significant and negative. This is confirmed when estimating a logit model for the probability of individual investment (see Table D1 in Appendix D).

Next, we look at the allocator's decision. In this case, we consider the intended return of the allocator as our dependent variable. In line with our previous analysis, we include the allocator's decision in the last two periods, *Intended Return*$_{t-1}$ and *Intended Return*$_{t-2}$ as explanatory variables. Because these variables merely refer to the allocator's intention to return, we also consider the real return in the previous period, *Real Return*$_{t-1}$, which coincides with the intended return if the allocator's decision was implemented (i.e., if there was joint investment in the previous period). In our setting, investors have the opportunity to punish the allocator upon observing the returned amount. The explanatory variable *Profit Reduction*$_{t-1}$ stands for the reduction in the allocator's payoff (amount of ECUs deducted) in the previous period. We control for inequality by considering the difference in payoffs between the allocator and the average of the two investors within a group in the previous period. The rest of covariates include the *Period* in which the decision is made, the dummy variables for the treatments where punishment is coordinated ($CP_{0,60}$ and $CP_{0,80}$), and controls for individual heterogeneity. Table 5 summarizes the results. We use the variance correction by Windmeijer (2005) and the robust standard errors (in parentheses) are clustered by groups.

Table 5. Allocators' decisions: Intended return using Arellano-Bond (1991)

	$CP_{0,80}$ $UP_{30,60}$	$CP_{0,80}$ $UP_{30,60}$	$CP_{0,60}$ $UP_{30,60}$	$CP_{0,60}$ $UP_{30,60}$	$CP_{0,80}$ $CP_{0,60}$	$CP_{0,80}$ $CP_{0,60}$
	(1)	(2)	(3)	(4)	(5)	(6)
Intended Return $_{t-1}$	0.086	0.166	0.066	0.057	0.111	0.133
	(0.047)	(0.038)	(0.027)	(0.032)	(0.018)	(0.015)
Intended Return $_{t-2}$	0.154	0.157	0.091	0.100	0.056	0.051
	(0.017)	(0.018)	(0.023)	(0.020)	(0.015)	(0.010)
Real Return$_{t-1}$	0.362	0.222	0.163	0.155	0.186	0.126
	(0.065)	(0.050)	(0.047)	(0.064)	(0.027)	(0.021)
Profit Reduction $_{t-1}$	-1.964		-0.075		-0.437	
	(0.418)		(0.456)		(0.195)	
Payoff Inequality $_{t-1}$		0.021		0.014		0.024
		(0.011)		(0.014)		(0.006)
Coord. Punish ($CP_{0,80}$)	1.480	1.568			-0.600	-0.575
	(0.767)	(0.739)			(0.651)	(0.683)
Coord. Punish ($CP_{0,60}$)			2.665	2.261		
			(0.844)	(1.082)		
Period	-0.097	-0.053	0.041	0.041	-0.041	-0.038
	(0.041)	(0.042)	(0.027)	(0.034)	(0.018)	(0.021)
Constant	13.010	12.020	6.043	4.036	12.470	10.860
	(1.800)	(1.926)	(2.433)	(2.801)	(2.902)	(2.825)
Heterogeneity	Yes	Yes	Yes	Yes	Yes	Yes

Instruments	39	39	39	36	45	45
Hansen Test	p = 0.663	p = 0.532	p = 0.297	p =0.349	p = 0.323	p = 0.627
Arellano-Bond Test AR(2)	p = 0.314	p =0.426	p = 0.457	p =0.375	p = 0.722	p =0.793
Arellano-Bond Test AR(1)	p < 0.001	p < 0.001	p < 0.001	p < 0.001	p < 0.001	p < 0.001
Observations	650	650	650	650	650	650

Notes. Robust standard errors in parentheses clustered by groups.

We observe that coordinated punishment boosts the returned amount. The result is explained by the fact that investors need to coordinate their actions, rather than by the increasing returns to scale of coordinated punishment. There is also evidence for "homegrown trustworthiness" in that allocators intend to return more in the current period if they had returned more in the previous periods (Casar and Rigon, 2011). Interestingly, allocators also intend to return more when their decision for the previous period was implemented. As for the effect of punishment, we observe that the reduction in the allocators' earnings in the previous period has a negative effect in the intended returned amount of the current period; therefore, allocators do not like to be punished.[24] Finally, our findings seem to be consistent with the idea that allocators are inequality averse in that they return more if they were above in the previous period.

Result 6. *We observe that (ceteris paribus) the amount that allocators return depends:*

i) *Positively on the amount that allocators returned in the previous period (specially if this was received by investors), and the difference between the allocator's and the investors' payoffs in the previous period,*

ii) *Negatively on the reduction in the allocator's payoffs in the previous period.*

B. Punishment behavior and efficiency

So far, we have shown that coordinated punishment facilitates investment decisions and foster reciprocal behavior. It is well-documented in the literature that

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[24] This is somehow in line with Casari and Luini (2009), where it is found that receiving punishment lowers contributions to the public good.

punishment decisions can undermine the positive effects of allowing for sanctions; see, e.g., Fehr and Gächter (2000), Cinyabuguma et al. (2006), Chaudhuri (2011) for related evidence in public good games and Calabuig et al. (2016) for evidence in the investment game. We investigate in this section how investors punish in each treatment and the effect of these decisions on the final payoffs.

Punishment behavior.— Table 6 summarizes the punishment behavior in each treatment, when punishment is feasible. The composition of punishment within each group and the (punishment) trigger strategy that investors use are also presented in this table. As we discussed in Section III.C, investors who received the return after their investment decisions may use different strategies, depending on whether they decide to punish in the current period and/or decide not to invest in the subsequent one.

Table 6. Punishment behavior of investors

	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$
% Individual punishment	36%	49%	48%
% Individual punish if low return ($<$ 15)	46%	59%	73%
% Individual punish if high return ($\geq$ 15)	15%	33%	27%
None of the investors in the group punish	44%	31%	39%
Only one investor in the group punishes	39%	38%	25%
Both investors in the group punish	17%	30%	36%

We observe that investors are more willing to punish when punishment is coordinated, and punishment is more likely when the return is below the fair amount.[25] When we look at the punishment behavior of the group, we find a tendency to rely on the joint punishment when punishment is coordinated (17%, 36% and 30% in $UP_{30,60}$, $CP_{0,80}$ and $CP_{0,60}$, respectively). [26]

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[25] Our model predicts that investors will use the punishment (depending on the proportion of reciprocal investors in the population) when the return is below 12.5 ECUs. We find that the likelihood of individual punishment for returns below 12.5 ECUs is 51% in $UP_{30,60}$, 83% in $CP_{0,80}$, and 73% in $CP_{0,60}$.

[26] In Appendix D, we show the punishment behavior across periods and find that the joint punishment is never used in the uncoordinated treatment after period 9 (see Figure D2)

In Figure 7 we report the likelihood of observing joint investment in period *t*+1, depending on the investors' punishment decisions in period *t*.

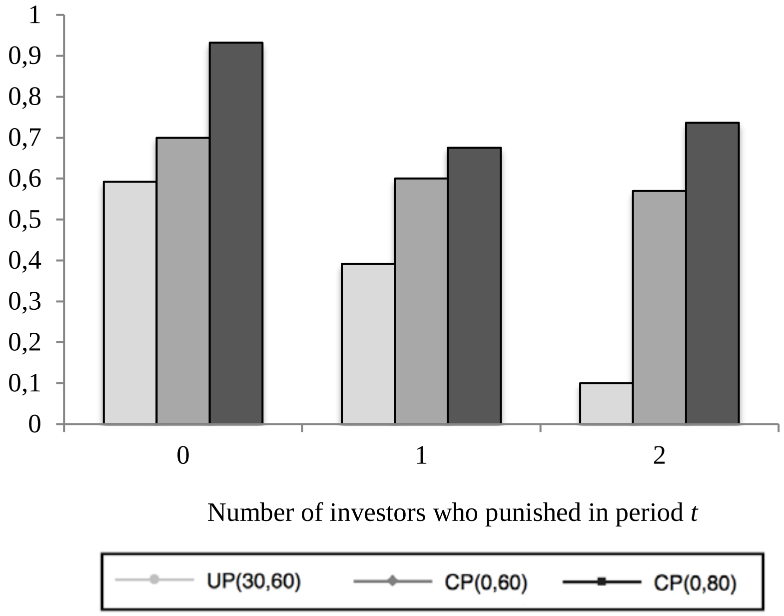


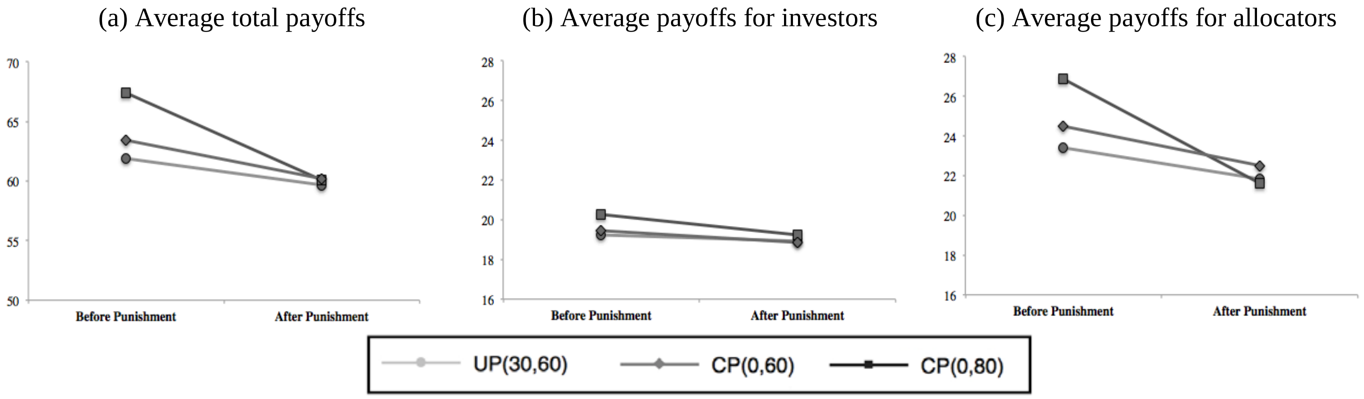
Figure 7. Likelihood of joint investment at t +1 depending on the number of punishers at period t

Figure 7 highlights the fact that investors are more likely to invest when punishment is coordinated. Further, it suggests that investors follow different strategies in each of the treatments. In particular, investors are more likely to keep investing the $UP_{30,60}$ treatment if none of them punished in the previous round, compared with the case in which one of the investors or both of them punished. In the $CP_{0,60}$ and the $CP_{0,80}$ treatments, investors keep investing if they did it in the previous period, even if they both decided to punish. Next result supports our (theoretical) Prediction 3:

Result 7. *Joint punishment (trigger strategy) is used more frequently when punishment is coordinated. In these treatments, majoriy of investors decide to punish but keep investing in subsequent periods, while investors end up not investing (trigger strategy) when punishment is uncoordinated.*

Overall, these differences in punishing behavior help to explain why we fail to identify efficiency gains.

Efficiency.— Figure 8 presents the average total payoffs in each group, disaggregated by treatments and roles, both before and after the punishment decision of investors. We look first at the total payoffs within each group in Figure 8(a). Not surprisingly, the positive effects of the joint investment when punishment is coordinated translate into higher average total payoffs *before* the punishment decisions; in fact, the differences in the total payoffs across treatments are significant using the Krusall-Wallis test ($p = 0.04$). However, we find that average total payoffs do not vary across treatments when we look at the payoffs *after* the punishment decisions ($p = 0.68$).[27] Figures 8(b) and 8(c) suggest that the major losses in efficiency are driven by the decrease in the allocators' payoff, especially in the $CP_{0,80}$ treatment (allocators' payoffs in this treatment go from 26.9 ECUs before punishment to 21.6 ECUs after punishment, thus their payoffs are reduced by roughly 20%). Finally, we observe in Figures 8(b) and 8(c) that allocators tend to receive more than investors in any of the treatments; in fact, they are the ones that benefit from the joint investment in the three treatments as their average payoffs are always above their initial endowments (20 ECUs), even after the punishment decisions of investors.


(a) Average total payoffs

(b) Average payoffs for investors

(c) Average payoffs for allocators

Figure 8. Total payoffs before and after the punishment decisions, disaggregated by treatment and roles.

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[27] Average total payoffs before (after) the punishment decisions are 61.85 (59.67) ECUs in $UP_{30,60}$, 63.45 (60.05) ECUs in $CP_{0,60}$, and 67.39 (60.20) ECUs in $CP_{0,80}$. The exact figures are presented in Appendix D (Table D3). This includes information about the payoffs across rounds (before and after punishment) (see Figure D3).

In Table 7, we attempt to explain the determinants of punishment decisions using a Tobit specification. Recall that investors can only punish if a joint investment took place.[28] Our dependent variable thus takes the value of -1 when investors did not have the possibility to punish. The values of 1 [0] are used when investors decided [not] to punish, respectively.[29] The set of independent variables include the punishment decision in the last two periods, $Punish_{t-1}$ and $Punish_{t-2}$ and the difference in payoffs between the allocator and the investor when investors have to choose whether to punish, $Payoff\ Inequality_{t-1}$. For each of the three treatments, we investigate how investors incorporate the other investor's decision in the previous period by interacting the dummies for each treatment with $Other\ Punish_{t-1}$, which is a dummy variable that takes the value 1 if the other investor punished in the previous period and 0 if she did not when punishment was feasible. All our regressions control for individual heterogeneity.

Table 7. Tobit regression on investors' punishment decisions

	$CP_{0,80}$ $UP_{30,60}$	$CP_{0,80}$ $UP_{30,60}$	$CP_{0,60}$ $UP_{30,60}$	$CP_{0,60}$ $UP_{30,60}$	$CP_{0,80}$ $CP_{0,60}$	$CP_{0,80}$ $CP_{0,60}$
	(1)	(2)	(3)	(4)	(5)	(6)
Punish $_{t-1}$	0.407	0.222	0.546	0.231	0.521	0.260
	(0.101)	(0.046)	(0.134)	(0.056)	(0.090)	(0.052)
Punish $_{t-2}$	0.135	0.028	0.250	0.050	0.235	0.220
	(0.080)	(0.045)	(0.110)	(0.055)	(0.071)	(0.043)
$CP_{0,80}$ Other Punish $_{t-1}$	0.046				0.015	
	(0.040)				(0.037)	
$CP_{0,60}$ Other Punish $_{t-1}$			-0.011		-0.026	
			(0.056)		(0.041)	
$UP_{30,60}$ Other Punish $_{t-1}$	-0.104		-0.131			
	(0.060)		(0.077)			
$CP_{0,80}$ Payoff Inequality $_{t-1}$		0.065				0.067
		(0.003)				(0.003)
$CP_{0,60}$ Payoff Inequality $_{t-1}$				0.100		0.096
				(0.005)		(0.005)
$UP3_{0,60}$ Payoff inequality $_{t-1}$		0.112		0.115		
		(0.006)		(0.007)		
Coord. Punish ($CP_{0,80}$)	0.971	1.313			0.434	1.649
	(0.299)	(0.195)			(0.227)	(0.264)
Coord. Punish ($CP_{0,60}$)			0.550	0.682		

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[28] One alternative specification would be considering the Heckman's sample selection model (1979). This is a two-step method in which a probit model on the probability of the dependent variable being observed is first estimated (in our setting, the probability of full investment), and then, a regression of maximum likelihood with the subsample is considered, including the Heckman's lambda (obtained in the first step) as an additional regression. If we include dummy variables for each period, our results for the Heckman model are consistent with the ones reported in Table 7.

[29] Note that the values chosen for the dependent variable is independent for the results obtained in a tobit model. That is, we would have obtained exactly the same results (except the coefficient of the constant) in Table 7 if the dependent variable took the value 0 when there has been no joint investment, the value 1 when just one investor punished and the value 2 when both investors punished.

			(0.297)	(0.176)		
Period	-0.054	-0.027	-0.129	-0.015	-0.082	
	(0.014)	(0.00817)	(0.020)	(0.010)	(0.013)	
Constant	-1.421	-2.393	-2.980	-2.364	-0.445	-2.053
	(1.070)	(0.190)	(1.291)	(0.649)	(1.048)	(0.972)
Heterogeneity	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,300	1,300	1,300	1,300	1,300	1,300

Our data lend support for the idea of "homegrown punishing preferences" in the sense that investors are more likely to punish if they did it in the past. There is also evidence for free-riding behavior when punishment is uncoordinated ($UP_{30,60}$ *Other Punish* $_{t-1}$) in that investors are less likely to punish if their partner did punish in the previous period. This effect is not significant in the coordinated devices. This is a key variable to our analysis, as it is one of the main predictions on punishment that we derived from our theoretical model. As expected, we observe that the difference between the allocator and the investors' payoffs increases the likelihood of punishment, so that investors seem to care about disadvantageous inequality.[30] This effect is significant in all the treatments. Finally, we obtain the same results than with the test analysis regarding punishment behavior comparisons between treatments.

Result 8. *We observe that (ceteris paribus) the likelihood of punishment depends:*

i) *Positively on own punishing decisions in the last two periods and the difference between the allocator and the investors' payoffs (i.e., payoff inequality).*

ii) *On the partner's decision in the previous period only for the uncoordinated condition. In particular, investors are less likely to punish if they observe that their partner punished in the previous round.*

VI. Concluding remarks

Societies often rely on the use of coordinated punishment. While there is mounting evidence on the positive effects of punishment, we are not aware of any

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[30] For related evidence, see Houser and Xiao (2010).

paper that directly investigates the benefits of coordinated punishment in an asymmetric situation such as the team investment game. We study the efficacy of coordinated versus uncoordinated punishment in a team investment game with two investors and one allocator. Our theoretical model paves the way to rationalizing the benefits of coordinated punishment in the principal-agent relationship. We prove that coordinated punishment can facilitate joint investment and boost the returned amount by showing that this punishment device can prevent the free-riding of investors in the punishment stage.

Our experimental results lend support for these predictions. We find that joint investment is more likely when punishment is coordinated. We also find that allocators return more to investors when punishment is coordinated. Finally, we do not find differences in efficiency across treatments. This occurs because investors tend to *punish together* when punishment is coordinated, while investors free-ride on other investors' punishment decisions when punishment is uncoordinated; in fact, we observe that uncoordinated and coordinated punishment exhibits different dynamics across rounds. When punishment is uncoordinated, a substantial proportion of investors refrain from investing, while investors keep investing and punishing when punishment is coordinated.

In the models of coordinated punishment by Boyd et al. (2010) and Olcina and Calabuig (2015), there are two features that we do not consider in our experimental design. The first one is peer punishment among investors. This may have two possible consequences that may decrease efficiency. The threat of being punished may reduce investors' free-riding behavior in allocators' punishment (especially under the uncoordinated condition) which may lead to more punishment. In addition, if peer punishment is frequently used, profit will be destroyed. The second feature is that the cost of punisment is shared by investors in the coordinated devices. This may have two oposite effects on efficiency. On the one hand, we will expect higher levels of punishment because it becomes cheaper to punish. On the other hand, as the cost is reduced, profits wil raise when coordinated punishment is used. In any case, these remain as open questions for further research.

Although our paper enriches our understading on the use of coordinated punishment in the society, we believe that there are other aspects of coordinated punishment that may be worth considering in future research. For example, punishment is sometimes “coordinated by means of gossips and other communications” (Boyd et al. 2010, Fehr and Williams, 2013), therefore it may be beneficial to look at effect of communication in our setting. Given our findings in the current paper, and the benefits of communication (Charness and Dufwenberg 2006, Choi and Lee 2014), we expect that allowing for coordination will not only facilitate joint investment and boost the return but it may improve efficiency as well.

Workers become members of trade unions so as to negotiate agreements on pay and conditions with their employers, but are they really aware of the positive effects of being united? We believe that testing this question is another avenue for future. In that regard, it may be worth considering a setting where subjects can endogenously decide the punishment institution they want to implement (Fehr and Williams, 2013).

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FOR ONLINE PUBLICATION

United we stand:
On the benefits of coordinated punishment

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Appendix

Appendix A. Theoretical model. Optimal behavior in each stage (Backward induction)
  Appendix A1. Optimal behavior of investors in the punishment stage (Stage 3)
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Appendix B. Experimental Instructions and questionnaire.
  Appendix B1. Experimental instructions
  Appendix B2. Questionnaire and summary of demographics variables
Appendix C. Decisions in the practice round
Appendix D. Further results and statistical analysis

Appendix A. Theoretical model. Optimal behavior in each stage.

We solve our model by backward induction and consider the punishment stage first.

Appendix A1. Optimal behavior of investors in the punishment stage (Stage 3)

We denote μ the probability of facing a reciprocal type of investor after observing joint investment, that is, $\mu := \text{Prob}\ (\tau^i = r \mid 1_I = 1)$ where $\tau^i = r$ stands for the reciprocal type and the indicator function 1_I for joint investment.

A symmetric Bayesian Nash Equilibrium (BNE) in the punishment stage will be defined as a pair of actions (y^r, y^s), where the first (second) element stands for the action of the reciprocal (selfish) type of investor, respectively. Thus, the profile (*p*, *np*) indicates that the reciprocal investor decides to punish, while the selfish investor decides not to punish.

First, it is easy to check that no punishment (*np*) is a dominant action for selfish investors, therefore (*p*, *p*) can never be a BNE in the punishment stage. Notice also that the unique BNE for $x \geq 15$ will be (*np*, *np*) as reciprocal investor will never punish if they receive (at least) the fair return from the allocator.

The next lemma shows when (*p*, *np*) is the BNE of the punishment stage for $x < 15$ in the different treatments.

Lemma 1: *Assume that* $x < 15$,

a) *In the* $\mathbf{UP}_{30,60}$ *treatment, (p, np) is the BNE of the punishment stage for any* $x \leq 9.41$ *and* $\forall\mu$, *for any* $x \in (9.41, 12.35)$ *when* $\mu \leq \mu(x) = ((0.3x-4)/(12-1.4x))$, *and for any* $x \in [12.35, 12.5)$ *when* $\mu \leq \mu(x) = ((25-2x)/(30-2x))$. *Otherwise the BNE will be (np,np).*

b) *In the* $\mathbf{CP}_{0,60}$ *treatment, (p, np) is the BNE of the punishment stage for any* $x \leq 8.57$ *when* $\mu \geq \mu(x) = (5/(18-0.6x))$ *and for any* $x \in (8.57, 12.5)$ *when* $\mu \geq \mu(x) = (5/(30-2x))$. *Otherwise the BNE will be (np,np).*

c) *In the* $\mathbf{CP}_{0,80}$ *treatment, (p, np) is the BNE of the punishment stage for any* $x \leq 5$ *when* $\mu \geq \mu(x) = (5/(24-0.8x))$ *and for any* $x \in (5, 12.5)$ *when* $\mu \geq \mu(x) = (5/(30-2x))$. *Otherwise the BNE will be (np,np).*

Proof.

a) Let us start with the **UP$_{30,60}$** treatment. Suppose that the allocator offers $x < 15$. Using the utility function of reciprocal investors (equation (4) in the main text), it follows that *p* is a best response for reciprocal investors to *(p,np)* if:

$\mu(10+x/2-2[(0.4)\cdot(15-x/2)-x/2] + (1-\mu)[10+x/2-2((0.7)\cdot(15-x/2)-x/2) \geq$

$\mu(15+x/2-2[(0.7)\cdot(15-x/2)-x/2] +(1-\mu)[15+x/2-2(15-x).$

Solving this inequality, we obtain all the results in Lemma 1a. Notice that if the allocator offers $x \geq 12.5$, *np* is the best response to (p, np) for reciprocal investors $\forall\mu$. The reason is that p is best response to (p, np) when $\mu< \mu(x)=((25-2x)/(30-2x))$, but this is zero when $x=12.5$. Therefore, if the allocator offers $x \geq 12.5$ the reciprocal investors will accept the return without punishing the allocator.

b) Next, we look at the optimal behavior of investors in the **CP$_{0,60}$** treatment. As the previous case, we can check that *p* is a best response for reciprocal investors to *(p,np)* if:

$\mu(10+x/2-2[(0.4)\cdot(15-x/2)-x/2] +(1-\mu)[10+x/2-2((15-x/2)-x/2) \geq$

$\mu(15+x/2-2[(15-x/2)-x/2] +(1-\mu)[15+x/2-2(15-x).$

Again, results in Lemma 1b follow from solving the above inequality. Notice again that if the allocator offers $x \geq 12.5$, not punishing np is the best response to (p,np) for reciprocal investors $\forall\mu$.

c) Finally, p is a best response for reciprocal investors to (p,np) in the **CP$_{0,80}$** treatment when $x < 15$ if:

$\mu(10+x/2-2[(0.2)\cdot(15-x/2)-x/2] +(1-\mu)[10+x/2-2((15-x/2)-x/2) \geq$

$\mu(15+x/2-2[(15-x/2)-x/2] +(1-\mu)[15+x/2-2(15-x).$

In this treatment, np is the best response to (p,np) for reciprocal investors when the allocator offers $x \geq 12.5$, $\forall\mu$. ■

Appendix A2. Optimal behavior of allocators regarding the return (Stage 2)

Fair-minded allocators will always choose to return the fair amount ($x = 15$) in Stage 2. Lemma 2 presents the optimal reward strategy of selfish allocators, which depends on their beliefs that they are facing a reciprocal investor, $\mu := \text{Prob}\ (\tau^i = r \mid 1_I = 1)$.

Lemma 2.

a) *In the* ***UP***$_{30,60}$ *treatment the selfish allocator will return:*

$x_S = 0$ if $\mu < 0.3464$ *and*

$x_S = \hat{x} = ((12\mu+4)/(0.3+0.4\mu))$ if $\mu \geq 0.3464$.

b) *In the* ***CP***$_{0,60}$ *treatment, the selfish allocator will return:*

$x_S = 0$ if $\mu < 0.278$,

$x_S = ((18\mu-5)/(0.6\mu))$ if $\mu \in [0.278, 0.306]$,

$x_S = 0$, if $\mu \in [0.306, 0.60]$ *and*

$x_S = ((30\mu-5)/(2\mu))$ if $\mu \geq 0.6$

c) *In the* ***CP***$_{0,80}$ *treatment, the selfish allocator will return:*

$x_S = 0$ if $\mu < \mu^t(0) = 0.2083$,

$x_S = ((24\mu-5)/(0.8\mu))$ if $\mu \in [0.2083, 0.223]$,

$x_S = 0$ if $\mu \in [0.223, 0.5)$ *and*

$x_S = ((30\mu-5)/(2\mu))$ if $\mu \geq 0.5$.

Proof.

a) **UP**$_{30,60}$ treatment. The expected payoff of the selfish allocator in a (p, np) BNE is:

$\mu^2((0.4)(50-x)+2\mu(1-\mu)(0.7)(50-x)+(1-\mu)^2(50-x) = (50-x)(1-0.6\mu)$.

According to Lemma 1a, the allocator will be punished if she offers $x < 9.41$, while she will not be punished if the return $x > 12.5$. Consider first the case of $x < 9.41$. The allocator will be punished regardless of the return, thus her best strategy would be to return nothing to the investors ($x_S = 0$). In this case, her final payoff will be $50-30\mu$. On the other hand, the allocator will not be punished if $x > 12.5$, thus her optimal reward will be choosing $x_S = 12.5$. The allocator's payoff in this case will be $50-12.5 = 37.5$. For any return $x_S \in (0, 12.5)$ the allocator has to choose between returning nothing (and then being punished) or returning a positive amount $x_S > 0$ so as to avoid being punished. Given her beliefs μ, the allocator needs to return an amount $\hat{x}(\mu)$ to avoid the punishment. This amount solves $\mu=\mu^{*}(x)=((0.3x-4)/(12-1.4x))$, and it equals to $\hat{x}(\mu) = ((12\mu+4)/(0.3+1.4\mu))$. The payoff for the selfish allocator would be $50 - ((12\mu+4)/(0.3+1.4\mu))$. When the selfish allocator compares the expected payoff of offering $x = 0$ with the expected payoff of offering $\hat{x}(\mu) = ((12\mu+4)/(0.3+1.4\mu))$, we find that $\hat{x}(\mu)$ is optimal whenever $\mu \geq 0.3464$.

b) **$CP_{0,60}$** treatment.

According to Lemma 1b, there are three situations in which there is no punishment.

i) The allocator offers $x < 8.57$ and $\mu \leq \mu\ (x) = 5/(18-0.6x)$

ii) The allocator offers $x \in [8.57,12.5)$ and $\mu \leq \mu(x) = 5/(30-2x)$.

iii) The allocator offers $x \geq 12.5$

For each possible setting above, we investigate whether or not the allocator is willing to avoid the punishment.

i) Consider first that the allocator offers $x < 8.57$ and $\mu \leq \mu(x) = 5/(18-0.6x)$. In this case, even when $x = 0$ if $\mu \leq \mu\ (0) = 0.278$, the proportion of reciprocal investors is so small that nobody is going to punish. If the allocator offers $x < 8.57$ and $\mu > \mu\ (x)$, there would be punishment and the expected payoff of the selfish allocator would be $\mu^2((0.4)(50-x)+2\mu(1-\mu)(50-x)+(1-\mu)^2(50-x) = (50-x)(1-0.6\mu^2)$. And, in this case, the best option is to offer $x = 0$ and receive a payoff of $50-30\mu^2$.

Another option for the allocator is to offer a reward $x > 0$ that depends on μ, to avoid the punishment of investors. The optimal return in this case would solve $\mu(x) = (5/(18-0.6x)$, thus $x^{*}= ((18\mu-5)/(0.6\mu))$. In this case, the payoff for the allocator would be $(50 - x^{*})$.

The selfish allocator has to compare the consequences of offering $x = 0$, and suffering the punishment (with an expected payoff of $50-30\mu^2$) or, alternatively, offering $x^{*} = ((18\mu-5)/(0.6\mu))$ and not being punished. Comparing these expressions we obtain that exists a critical value of μ resulting from the cubic equation $18\mu^3-18\mu+5 = 0$. As the discriminant is negative, there are at most two positive unequal roots, $\mu^{*} = 0.306$ and $\mu^{*} =0.81$. Thus, the best reward policy of the selfish allocators is to offer nothing ($x_s = 0$) if $\mu \in [0.306, 0.81]$. Note that this happens when $x^* = ((18\mu-5)/(0.6\mu)) < 8.57$, that is, when $\mu < 0.39$.

ii) Assume that the allocator offers $x_s \in [8.57, 12.5]$. We can solve for the optimal return $x^* > 0$ such that investors do not punish the allocator. This return solves $\mu(x) = (5/(30-2x))$, and it equals to $x^* = ((30\mu-5)/(2\mu))$.

If $\mu \leq \mu(x) = 5/(30-2x)$, there will be no punishment (see Lemma 1b), and the payoff for the allocator will be $(50 - x^*)$. If $\mu > \mu(x) = 5/(30-2x)$, (and the allocator offers $x > 12.5$) there will be punishment with an expected payoff of $(50-x)(1-0.6\mu^2)$. In this case, the best option is to offer $x = 0$ with an expected payoff for the allocator of $50-30\mu^2$.

There exists a critical value of μ resulting from another cubic equation $12\mu^3-6\mu+1=0$. In this case, there are two positive unequal roots, which are $\mu_1(x) = 0.178$ and $\mu_2(x) = 0.6$. Thus, if $\mu \in [0.178, 0.6]$ the best reward policy of the selfish allocator is to set $x = 0$.

iii) Finally, if the allocator offers $x = 12.5$, there is no punishment and her payoff would be $50-12.5=37.5$.

c) In the **CP**$_{0,80}$ treatment, there are three situations in which there is no punishment (see Lemma 1c). We look at each setting separately:

i) Suppose that the allocator offers $x < 5$. If $\mu \leq \mu(x) = 5/(24-0.8x)$, there will be no punishment even when the allocator returns nothing $x=0$ (if $\mu \leq \mu(0)=0.2083$). This is because the proportion of reciprocal investors is so small that nobody is going to punish. However, when the allocator offers $x < 5$ but $\mu > \mu(x) = 5/(24-0.8x)$, there will be punishment and the expected payoff of the selfish allocator will be $\mu^2((0.2)(50-x)+2\mu(1-\mu)(50-x)+(1-\mu)^2(50-x) = (50-x)(1-0.8\mu^2)$. In this case, the best option is to offer $x = 0$ and the payoff for the allocator is $50-40\mu^2$.

Another option is to offer a reward $x^* > 0$, that depends on μ, to avoid punishment by investors. This offer is such that $x^* = ((24\mu-5)/(0.8\mu))$. In this case the payoff for the allocator would be $(50 - x^*)$.

Comparing the above expressions we obtain that exists a critical value of μ resulting from the cubic equation $32\mu^3-24\mu+5=0$. As the discriminant is negative, there are at most two positive unequal roots, which are $\mu_1 = 0.223$ and $\mu_2 = 0.732$.

Thus if $\mu \in [0.223, 0.732]$ the best reward policy of the selfish allocator is to set $x = 0$.

ii) Suppose now that the allocator offers $x \in (5, 12.5)$. If $\mu \leq \mu(x) = 5/(30-2x)$, there will be no punishment (see Lemma 1c). However, if the allocator offers $x \in (5, 12.5)$ but $\mu > \mu(x) = 5/(30-2x)$, there will be punishment with an expected payoff of $(50-x)(1-0.8\mu^2)$. And, in this case, the best option is to offer $x = 0$ with an expected payoff for the allocator of $50-40\mu^2$. As we know, the allocator can offer a reward of $x > 0$ so as to avoid punishment. This reward is $x^* = ((30\mu-5)/(2\mu))$ and yields a payoff for the allocator equals to $(50- x^*)$.

Comparing these expressions we obtain that exists a critical value of μ resulting from another cubic equation $16\mu^3-6\mu+1 = 0$. In this case, there are two positive unequal roots, which are $\mu_1= 0.183$ and $\mu_2 = 0.5$. Thus, if $\mu \in [0.183, 0.5]$ the best reward policy of the selfish allocator is to set $x =0$.

iii) Finally, if the allocator offers $x = 12.5$, there is no punishment and her payoff would be $50-12.5=37.5$.■

Appendix A3. Proof of Proposition 1: Pooling equilibria (Stage 1)

The next proposition corresponds to Proposition 1 in the main text. This characterizes the efficient pooling equilibria in which both selfish and reciprocal investors choose to invest in Stage 1. We denote $\bar{q}$ and $\bar{m}(q)$ the minimum proportion of reciprocal investors and fair-minded allocators for the existence of the equilibria. In the equilibria, $q = \mu = \text{Prob}\ (\tau^i = r \mid 1_I = 1)$.

Proposition 1. (Efficient pooling equilibria)

c) *In the* ***UP**$_{30,60}$* *treatment, there is an efficient pooling equilibrium in which both types of investors choose to invest if* $q \geq \bar{q} = 0.3464$ *and* $m \geq \bar{m}(q)$, *where* $\bar{m}(q)$ *is increasing in q. In this equilibrium, fair-minded allocators set the fair return* $x^* = x^F = 15$ *and selfish allocators set the minimum reward such that reciprocal investors do not punish them. This optimal return* $x^*(q) = ((12q+4)/(0.3+1.4q)) < 12.5$ *is decreasing in q.*

d) *In the* ***CP**$_{0,60}$* *treatment* ***[CP**$_{0,80}$* *treatment], there is an efficient pooling equilibrium in which both types of investors choose to invest if* $q \geq \bar{q} = 0.6$ *[*$q \geq \bar{q} = 0.5$*] and* $m \geq \bar{m}(q)$, *where* $\bar{m}(q)$ *is decreasing in q. In these equilibria,* *fair-minded allocators set the fair return $x^* = x^F = 15$ and selfish allocators set the minimum reward such that reciprocal investors do not punish them. This optimal return $x^*(q) = ((30q-5)/(2q)) < 12.5$ is increasing in q.*

Proof.

a) **UP$_{30,60}$** treatment. For the reciprocal investors is worth choosing to invest if the utility of investing is larger than the utility of non-investing, that is,
$15 + (1-m)x/2 + m(7.5) - 2\{(1-m)((15-x/2)-x/2)\} \geq 20$.
Substituting x by $\hat{x} = ((12q+4)/(0.3+1.4q))$ because $q \geq 0.3464$, we obtain $m \geq \bar{m}(q) = ((70/3 - ((12q+4)/(0.3+1.4q)))/(25-((12q+4)/(0.3+1.4q)) = (20.67q + 3) / (23q + 3.5)$. It follows that the selfish investor will also invest since $(15 + (1-m)((12q+4)/(0.3+1.4q)) + 7.5m) \geq 20$ for $q \geq 0.3464$ and ∀m.

b) **CP$_{0,60}$** treatment. In this case, it is worth investing for a reciprocal investor if:
$15 + (1-m)\, x/2 + m(7.5) - 2\{(1-m)((15- x/2)- x/2)\} \geq 20$.
Given that $x = (30q-5)/(2q)$ (see Lemma 2), the reciprocal type of investors chooses to invest when $m \geq \bar{m}(q) = 1-0.4q$. The selfish investor will also prefer investing since $15 + (1-m)((30q-5)/(2q)) + 7.5m \geq 20$ for $q \geq 0.6$ and ∀m.

c) **CP$_{\{0,80\}}$ treatment**. As above, the reciprocal investor chooses to invest if:
$15+(1-m)x/2+m(7.5)-2\{(1-m)((15-x/2)-x/2)\} \geq 20$.
That is, when $x \geq (30q-5)/2q$ (see Lemma 2) and $m \geq m_2(q) = 1-0.4q$, the reciprocal type of investors chooses to invest. The selfish investor will also prefer investing since $15 + (1-m)((30q-5)/(2q)+7.5m \geq 20$ for $q \geq 0.5$ and ∀m.■

Given our results in Proposition 1, we can obtain the area in which the efficient pooling equilibrium exists.:

$$A = (1-\bar{q}) - \int_{\bar{q}}^{1} \bar{m}(q)\, dq$$

Next, we obtain this area for each of the treatments to conclude that joint investment will be more likely when punishment is coordinated.

$UP_{30,60}$ $A = (1-\bar{q}) - \int_{\bar{q}}^{1} \bar{m}(q)\,dq =$

$A = (1-0.3464) - \int_{0.3464}^{1} \frac{20.67q+3}{23q+3.5}\,dq$

$A = 0.6536 - 0.582091 \simeq 0.07$

$CP_{0,60}$ $A = (1-\bar{q}) - \int_{\bar{q}}^{1} \bar{m}(q)\,dq =$

$A = (1-0.6) - \int_{0.6}^{1} 1-0.4q\,dq$

$A = 0.4 - 0.272 \simeq 0.128$

$CP_{0,80}$ $A = (1-\bar{q}) - \int_{\bar{q}}^{1} \bar{m}(q)\,dq =$

$A = (1-0.5) - \int_{0.5}^{1} 1-0.4q\,dq$

$A = 0.5 - 0.35 \simeq 0.15$

Appendix A4. Other Perfect Bayesian equilibria (Stage 1)

The efficient pooling equilibria in which both types of investors decide to invest in Stage 1 and do not punish in Stage 3 is not unique. In each of treatment, there is also an inefficient pooling equilibrium in which investors do invest in Stage 1 but then punish in Stage 3 (see **Lemma 3-5 below).** There is also a separating equilibrium that is common to the three treatments. In this equilibrium, selfish investors do invest, while reciprocal investors do not (see Lemma 6). Finally, there is an inefficient equilibrium without investment in each of the treatments (see Lemma 7).

Inefficient Pooling equilibrium with punishment

Lemma 3. *In the* $UP_{30,60}$ *treatment there is an inefficient pooling equilibrium in which both types of investors choose to invest but then punish if* $q < 0.3464$ *and* $m \geq m' = ((31-9q)/(33.5-9q))$. *Selfish allocators set* $x = 0$ *and fair-minded allocators set* $x = 15$. *Reciprocal investors punish the reward of selfish allocators.*

Proof.

The reciprocal investors will choose to invest if

$15 - 5(1-m) + (7.5)m - (1-m)2[15\cdot(0.4)q + 15\cdot(0.7)(1-q)] \geq 20$.

This behavior is optimal due to the low (high) proportion of selfish (fair-minded) allocators.

The selfish allocator offers a zero reward and the selfish allocator invests because of the presence of a high proportion of fair-minded allocators.■

Lemma 4. *In the* $CP_{0,60}$ *treatment there is an inefficient pooling equilibrium in which both types of investors choose to invest but there is punishment if* $q \in [0.306, 0.60]$ *and* $m \geq m' = ((40-18q)/(42.5-18q.))$. *Selfish allocators set* $x = 0$ *and fair-minded allocators set* $x = 15$. *Reciprocal investors punish the reward of selfish allocators.*

Proof.

The reciprocal investors will choose to invest if

$15 - 5(1-m) + (7.5)m - 2(1-m)[15\cdot(0.4)q + 15(1-q)] \geq 20$.

This inequality holds when $m \geq ((40-18q)/(42.5-18q))$.

On the other hand, the selfish investor decides to invest when $(15+7.5m) \geq 20$, that is, when $m \geq 1/(1,75)$, that holds when the previous condition also holds.

As in the previous treatment, this behavior is optimal due to the low (high) proportion of selfish (fair-minded) allocators.■

Lemma 5. *In the* $CP_{0,80}$ *treatment there is an inefficient pooling equilibrium in which both types of investors choose to invest but there is punishment for* $q \in (0.223, 0.5)$ *and* $m \geq m' = ((40-24q)/(42.5-24q))$. *Selfish allocators set* $x = 0$ *and fair-minded allocators set* $x = 15$. *Reciprocal investors punish the reward of selfish allocators.*

Proof.

The reciprocal investors will choose to invest if

$15-5(1-m) +(7,5)m-2(1-m)[15\cdot(0.2)q+15(1-q)] \geq 20$.

And this inequality holds when $m \geq ((40-24q)/(42.5-24q))$.

On the other hand, the selfish investor decides to invest when $(15+7.5m) \geq 20$, that is, when $m \geq 1/1.75$. This expression holds when the previous condition also holds.

As in the previous treatment, this equilibrium behavior is optimal due to the low (high) proportion of selfish (fair-minded) allocators.■

Separating Equilibrium without punishment

Lemma 6. *For* $q < 1/3$ *and* $((35-30q)/(37.5(1-q))) \geq m \geq (5/(7.5(1-q)))$, *there is a separating equilibrium in which the reciprocal investor does not invest and the selfish investor chooses to invest. Selfish allocators set* $x = 0$ *and fair-minded allocators set* $x = 15$. *There is no punishment in equilibrium.*

Proof.

In this equilibrium, the reciprocal investors do not invest while the selfish investors do. Therefore $\mu = 0$. The selfish allocators set $x = 0$ and the fair-minded allocators as always offer $x = 15$. For the existence of this equilibrium we need an intermediate number of fair-minded allocators: not too many for the reciprocal investors not to invest and not too few for the selfish investors to invest. Furthermore, the critical proportion of fair-minded allocators also depends on the proportion of reciprocal investors in the population. In particular, q has to be smaller than 1/3 for the existence of this equilibrium.■

The Non-Cooperative Equilibrium: equilibrium without investment

Lemma 7. *For every q and m, there is an Inefficient Pooling Equilibrium in which both types of investors choose not to invest.*

Proof.

The proof is straightforward and is left to the reader. Simply note that any investor will not invest if she believes that the other investor will not invest.

Appendix B. Experimental Instructions and questionnaire

Appendix B1. Experimental instructions (originally in Spanish)

The purpose of this experiment is to study how individuals make decisions in certain contexts. The instructions are simple and if you follow them carefully you will receive a certain amount of cash at the end of the experiment. Your earnings will be received confidentially, so no one in this experiment will know the payment received by the rest of the participants. At any time, you may ask any doubt you may have by raising your hand. Apart from these questions, any type of communication between you and the rest of participants is forbidden and may imply the exclusion from the experiment.

This experiment has two phases. Next, we will explain to you Phase 1. Once we finish this phase, you will receive new set of instructions regarding Phase 2.

Phase 1 (Practice round)

In this phase, you will receive an initial endowment of 20 ECUs (Experimental Currency Units) and you will be randomly matched with two other people in this room to form a group of 3 people. In each group, there will be two participants in the role of type A and one participant in the role of type B. The computer will randomly choose whether you are a type A or a type B participant in your group.

If you are a type A participant, you can choose whether or not to send 5 ECUs to the type B participant in your group.

- If none of the type A participants in your group sends 5 ECUs, each participant will keep his/her initial endowment of 20 ECUs and Phase 1 will end.
- If only one type A participant in your group sends 5 ECUs, we will deduct this amount from his/her initial endowment and Phase 1 will end. The type A participant that sent the 5 ECUs will receive 15 ECUs (20 initial ECUs – 5 ECUs sent). The rest of participants will receive their initial endowment of 20 ECUs.
- If both type A participants in your group send 5 ECUs, we will deduct this amount from their initial endowment and will multiply by 3 the total amount

send (10 ECUS) before giving it to the type B participant. Thus, if both type A participants send 5 ECUs, the type B participant will receive 30 ECUs.

If you are a type B participant you have to choose the amount “X” of the 30 ECUs that have been generated to return to type A participants and the amount you want to keep. The amount sent back by the type B participant will not be multiplied. The amount sent by the type B participant (if positive) will be equally split between the type A participants of the group. The amount that the type B participant keeps will be added up to his/her initial endowment of 20 ECUs.

After making these choices (**and only if both type A participants decided to send 5 ECUs to the type B participan**t), type A participants have the possibility of reducing the ECUs of the type B participant, after observing what has been returned to them. For each A who decides to reduce the ECUs of B’s, their own ECUs will be reduced by 5.

[Next, we have a different paragrah for each of the treatments]

$UP_{30,60}$. If only one type A participant decides to reduce the earnings of the type B participant (by reducing theirs in 5 ECUs), the type B participant will receive 70% of his/her earnings. If the two type A participants decide to reduce the earning of the type B participant (by reducing theirs in 5 ECUs), the type B participant will receive 40% of his/her earnings.

$CP_{0,60}$. If only one type A participant decides to reduce the earnings of the type B participant (by reducing theirs in 5 ECUs), type B’s earnings will not be affected (that is, type B will receive 100% of their earnings). If the two type A participants decide to reduce the earning of the type B participant (by reducing theirs in 5 ECUs), the type B participant will receive 40% of his/her earnings.

$CP_{0,80}$. If only one type A participant decides to reduce the earnings of the type B participant (by reducing theirs in 5 ECUs), type B’s earnings will not be affected (that is, type B will receive 100% of their earnings). If the two type A participants decide to reduce the earning of the type B participant (by reducing theirs in 5 ECUs), the type B participant will receive 20% of his/her earnings.

[*In what follows, we focus on the* $CP_{0,80}$ *treatment. The rest of treatments do simply change the figures regarding the effect of the punishment on the earnings of the type B participant*].

In sum, the payments in this phase will be determined as follows:

- If none of the type A participants send the 5 ECUs:
  Payment of A = Payment of B = 20 ECUs (initial endowment)

- If only one type A participant sends 5 ECUs and the other does not:
  Payment of A sending = 20 ECUs (initial) - 5 ECUs (sent) = 15 ECUs
  Payment of A NOT sending = Payment of B = 20 ECU (initial)

- If the 2 participants type A send 5 ECUs and the participant type B returns X ECUS of the 30 generated ones:
  - If no A decides to reduce ECUs to B:
    Payment of A = 20 ECUs (initial) - 5 ECUs (sent) + X/2 (received)
    = 15 + X/2 ECUs
    Payment of B = 20 ECUs (initial) + 30 ECUs (received) - X (returned)
    = 50 - X ECUs

  - If only one type A participant decides to reduce ECUs to B and the other does not:
    Payment of A reducing = 15 ECUs + X/2 ECUs - 5ECUs (reduced)
    = 10 + X / 2 ECUs
    Payment of A not reducing = 15 ECUs + X / 2 ECUs
    Payment of B = 50 - X ECUs

  - If the 2 type A participants decide to reduce ECUs to B:
    Payment of A = 15 ECUs + X / 2 ECUs - 5ECUs (reduced) = 10 + X / 2 ECUs
    Payment of B = 20% of (50 - X) ECUs

To pay your choices, we will convert your earnings from ECUs to Euros using the rate 3 ECUs = 1 Euro. You will receive your earnings anonymously at the end of the experiment

Phase 2 (Repeated game)

In this second phase, you will be paired with two other people in this room to form a group of three. As in the previous phase, each group will consist of 2 type A participants and 1 type B participant. Your type will be the same as in the previous phase. This means that if you were a type A participant in the first phase, you will continue to be a type A participant in this phase, and if you were a type B participant, you will still be a type B participant. However, your group will be different from the one in first phase. In particular, there will not be any participant in your group that already interacted with you in the first phase.

This phase has a total of 15 rounds. At the beginning of each round, each participant in your group will receive an initial amount of 20 ECUs (experimental monetary units). If you are a type A participant you can choose in each round between sending 5 ECUs or sending anything nothing to the type B participant in your group.

- If no A participant chooses to send 5 ECUs, each participant in the group will keep their initial amount of 20 ECUs in that round.
- If only one A participant decides to send 5 ECUs, we will deduct that amount from their initial ECUs. The A participant who has decided to send the 5 ECUs will receive 15 ECUs in that round (20 initial ECUs - 5 ECUs sent). The rest of the participants will receive their initial amount of 20 ECUs in that round.
- If the two A participants decide to send 5 ECUs, we will deduct that amount from their initial ECUs and triple the total amount sent by both A participants (10 ECUs) before giving it to B. Thus, if the two A's decide to send 5 ECUs, B will receive the amount of 30 ECUs in that round.

If you are a type B participant, you must choose in each round the amount "X" of the 30 ECUs that could be generated, you want to return the type A participants in your group and how much you want to keep for yourself. The amount returned by type B participants will NOT triple. The amount that B's decide to return to A's (if positive) will be divided equally between the 2 type A participants of their group. The amount that B's decide to keep, will be added to its initial 20 ECUs.

After the previous task and only if the 2 type A participants have decided to send their 5 ECUs in that round, they have the possibility of reducing the earnings of the type B participant in that round, after observing what has been returned to them. For each type A participant who decides to reduce the ECUs of the type B participant, their own ECUs will be reduced by 5.

[*Once again, we have a different paragrah for each of the treatments. Below, we present the translated instructions for the* $CP_{0,80}$ *treatment.*]

If only one A decides to reduce the earnings of B (by reducing theirs in 5 ECUs), B's earnings will not be affected in that round (that is, B will receive 100% of their earnings). If the two A's decide to reduce the profits of B (by reducing theirs in 5 ECUs), B will receive 20% of their earnings in that round.

In sum, the payments of each round would be:

- If no A sends anything:
  Payment of A = Payment of B = 20 ECUs (initial endowment)

- If one A sends 5 and the other does not:
  Payment of A sending = 20 ECUs (initial) - 5 ECUs (sent) = 15 ECUs
  Payment of A NOT sending = Payment of B = 20 ECU (initial)

- If the 2 type A participants send 5 ECUs and the type B participant returns X ECUS:
    - If no A decides to reduce ECUs to B:
      Payment of A = 20 ECUs (initial) - 5 ECUs (sent) + X / 2 (received)
      = 15 + X / 2 ECUs
      Payment of B = 20 ECUs (initial) + 30 ECUs (received) - X (returned) =
      = 50 - X ECUs

    - If only one type A decides to reduce ECUs to B and the other does not:
      Payment of A reducing ECUs = 15 ECUs + X / 2 ECUs - 5ECUs (reduced) =
      = 10 + X / 2 ECUs
      Payment of A not reducing ECUs = 15 ECUs + X / 2 ECUs
      Payment of B = 50 - X ECUs

    - If the two type A participants decide to reduce ECUs to B:

Payment of A = 15 ECUs + X / 2 ECUs - 5ECUs (reduced) = 10 + X / 2 ECUs

Payment of B = 20% of (50 - X) ECUs

It is important that you bear in mind that during the 15 rounds your group will always be the same. This means that you will be paired with the same people during the 15 rounds. However, remember that the two people with whom you will be paired in this phase will be different from those who were with you in the first phase.

At the end of each round, we will inform you of the decisions of the members of your group. At the end of the experiment, we will pay your decisions for one round, chosen randomly. As in the previous case, we will convert your earnings in ECUs to Euros, using the rate of 3 ECUs = 1 €. You will receive your earnings anonymously at the end of the experiment.

Appendix B2. Debriefing questionnaire and summary of demographics variables

Q1: What is your age?. . . years (**Age**)

Q2: What is your gender? (0 = Men, 1 = Female) (**Gender**)

Q3: What is the level of your current studies? (Level = 1 Graduate; 2 Master; 3 Doctorate; 4 I'm not studying at the moment)

Q4: Please choose the field that best fit to your studies (Major = 01 Economics, 02 Law, 03 Business, 04 Engineer, 05 Other, 06 Tourism; 07 Accounting)

Q5: How many years have you been studying at the university? (Years: "1" to "6 or more")

Q6: (**Risk aversion**) This is the investment decision in Gneezy and Potters (1997). Each subject hypothetically received 10 Euros and has to choose how much of it, x, (s)he wanted to invest in a risky option and how much (s)he wished to keep. The amount invested yielded a dividend equal to 2.5x with 1/2 probability, being lost otherwise. The money not invested in the risky option (10 − x) was kept by the subject. In this situation, the expected value of investing is positive and increasing in the amount invested; therefore a risk-neutral (or risk-loving) participant should invest the 10 Euros, whereas a risk-averse participant will invest less. The value of Risk Aversion is measured in our experiment by the amount invested x.

Q7: A bat and a ball cost $1.10. The bat costs $1.00 more than the ball. How much does the ball cost? _ cents (Answer: 5) (**CRT**$_1$)

Q8: If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? minute (Answer: 5) (**CRT**$_2$)

Q9: In a lake, there is a patch of lilypads. Everyday, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake how long would it take for the patch to cover half of the lake? day (Answer: 47) (**CRT**$_3$)

Q10: How many of the last 3 questions you think you have answered correctly? (**Guess**)

Q11. How do you feel in this moment with your life? 1-7-scaled answer from 1 (very satisfied) to 7 (Not at all satisfied) (**Satisfaction**)

Q12: Taking everything into consideration, would you call yourself...

(01 not very happy, 02 quite happy, 03 very happy)

Q13: Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? (Trust = 1. Most people can be trusted, 0 Need to be very careful) (**Trust**)

Q14: Consider the following situation: Two secretaries with the same age do exactly the same work. However, one of them earns 20 euros per week more than the other. The one that is paid more is more efficient and faster, while working. Do you believe it is fair that one earns more than the other? (**Inequality** = 0 No, 1 Yes)

Table B1. Summary of demographics

	Mean	Std. dev.	Min	Max
Age	28.7	6.53	17	63
Gender (=1 for female)	0.50	0.50	0	1
Risk aversion	5.12	2.25	0	10
CRT_1	7.45	10.86	0	110
CRT_2	124.8	666.9	0	10000
CRT_3	30.25	15.4	1	96
CRT	0.28	0.34	0	1
Guess (number correct answers)	2.29	0.76	0	3
Satisfaction	2.44	1.32	1	7
Trust	0.20	0.40	0	1
Inequality	0.83	0.37	0	1
N (individuals)	225			

Note. CRT refers to the proportion of correct answers in the CRT test.

Appendix C. Decisions in the practice round

This appendix reports the decisions in the practice round (Phase 1 of our experiment) and compare the behavior of investors and allocators in the practice round and the first period of the repeated game (Phase 2) that we analyze in the paper.

Table C1. Average decisions in the practice round.

	UP$_{30,60}$	**CP$_{0,60}$**	**CP$_{0,80}$**
% Individual investment	52%	48%	56%
% Joint investment	28%	16%	28%
Average amount returned	6.9	11.0	9.2
% Positive return	64%	84%	80%
Frequency of individual punishment	50%	25%	28.6%
Frequency of joint punishment	28.5%	0%	0%
Investors’ payoffs (after punishment)	17.3	18.6	18.6
Allocators’ payoffs (after punishment)	23.5	22.5	24.7
Total payoffs (after punishment)	58.1	59.6	62.0
N (workers)	50	50	50
N (firms)	25	25	25

“Joint Investment" refers to the frequency of groups in which both investors decided to invest (i.e., total investment = 10 ECUs). "Amount returned" includes all observations elicited with the strategic method. "Joint punish" refers to the likelihood that both investors in the group decided to reduce the earnings of the allocator, considering only the observations in which punishment was feasible.

Our results for the one-shot interaction of Phase 1 indicate that the punishment scheme does not affect the investor’s behavior. When doing pairwise comparisons, we find that both individual and group investments are indistinguishable across treatments ($p > 0.31$). We also find that the return is higher in CP$_{0,60}$ than in UP$_{0,30}$ ($p = 0.027$), but we cannot reject the null hypothesis that the return is the same in any other two treatments ($p > 0.21$). If we do pairwise comparisons, the null hypothesis that a positive return is equally likely across treatments cannot be rejected at any common significance level ($p > 0.11$). We, therefore, conclude that behavior in the three treatments is very similar in the practice round, except for the returned amount.

We observe a very similar pattern in the first period of the repeated game (see Table C2). As in the practice round, there are no differences in the investing behavior of investors across treatments when we look at the likelihood of individual investment or the joint investment ($p > 0.318$). The results in the first period of the repeated game suggest also that allocator return more when punishment is coordinated ($p < 0.027$ when comparing the UP$_{30,60}$ with the CP$_{0,60}$ or CP$_{0,80}$ treatments; $p = 0.745$ when comparing CP$_{0,60}$ and CP$_{0,80}$).

Table C2. Comparing decisions in the practice round and the repeated game

					Repeated game				
	Practice round			**Period 1**			**Periods 1-15**		
	UP$_{30,60}$	**CP$_{0,60}$**	**CP$_{0,80}$**	**UP$_{30,60}$**	**CP$_{0,60}$**	**CP$_{0,80}$**	**UP$_{30,60}$**	**CP$_{0,60}$**	**CP$_{0,80}$**
% Individual investment	52%	48%	56%	58%	48%	56%	30%	41%	53%
% Joint investment	28%	16%	28%	40%	20%	28%	16%	25%	42%
Average amount returned	6.9	11.0	9.2	7.6	11.6	12.1	8.3	12.1	11.6
% Positive return	64%	84%	80%	76%	92%	88%	66%	81%	77%
% Low return (≤ 10)	80%	56%	60%	76%	52%	44%	47%	50%	50%
% High return (≥ 20)	4%	8%	20%	4%	16%	28%	33%	45%	45%
N (investors)	50	50	50	50	50	50	50	50	50
N (allocators)	25	25	25	25	25	25	25	25	25

Joint investment refers to the frequency of both investors deciding to invest within a group and the *amount returned* includes all observations elicited with the strategic method. Because allocators always allocate the same amount (30 ECUs) the comparison across treatments is neat.

If we compare the behavior of investors and allocators in the practice round and the first round of the repeated game we find no differences for any given treatment when we look at the likelihood of individual investment ($p > 0.512$) or the joint investment ($p > 0.317$).[31] Thus, any difference in behavior of investors in the repeated game should be attributed to the different dynamics, rather than to the experience in the practice round.[32]

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[31] We use a Wilcoxon matched-pairs signed-ranks test in this case as choices in the repeated game are not independent of choices in the practice round.

[32] Allocators' behavior in the practice round is not statistically different from their behavior in the first round of the repeated game, except in the CP$_{0,80}$ treatment (UP$_{30,60}$ $p = 0.351$; CP$_{0,60}$ $p = 0.976$; CP$_{0,80}$, $p = 0.058$)

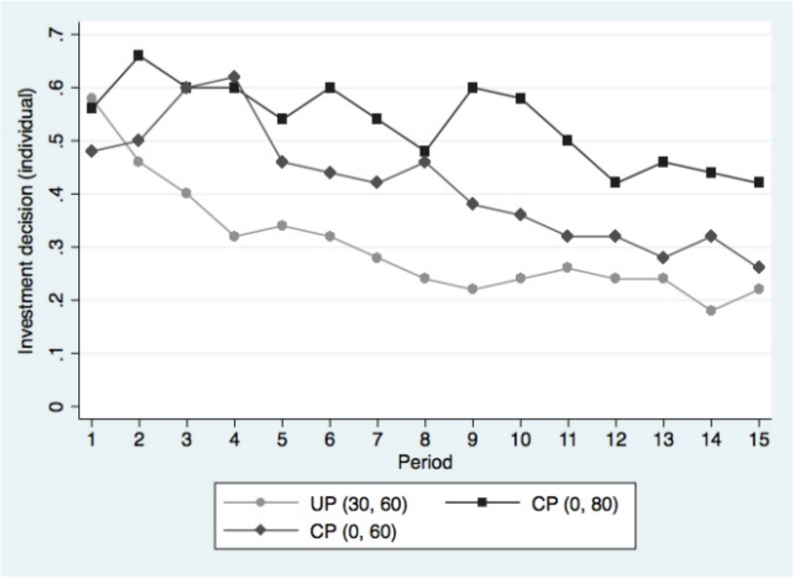
We further analyze the choices of investors in the practice round and the first round of the repeated game by means of an econometric analysis. Table C3 presents the results of a logit specification on the likelihood of investing. We focus on individual investment as the variable for the joint investment is meaningless in the practice round (it all depends on the matching).

Table C3. Likelihood of investing in the practice round and the first round of the repeated game

	Practice round		Repeated (first round)	
	(1)	(2)	(1)	(2)
Constant	0.080	-1.046	0.323	0.515
	(0.284)	(1.482)	(0.324)	(1.501)
Coord. Punish ($CP_{0,80}$)	0.161	-0.007	-0.0816	-0.388
	(0.403)	(0.442)	(0.420)	(0.522)
Coord. Punish ($CP_{0,60}$)	-0.160	-0.266	-0.403	-0.503
	(0.402)	(0.425)	(0.420)	(0.500)
Women		0.240		-0.953
		(0.369)		(0.379)
Age		-0.025		-0.005
		(0.033)		(0.022)
CRT		0.820		1.556
		(0.510)		(0.577)
Risk Aversion		0.175		0.216
		(0.085)		(0.087)
Trust		0.271		-0.006
		(0.432)		(0.446)
Satisfaction		0.098		0.024
		(0.159)		(0.182)
Happiness		-0.030		-0.415
		(0.400)		(0.410)
Inequality		0.390		-0.133
		(0.414)		(0.438)
Observations	150	150	150	150

Appendix D. Further results and statistical analysis

Figure D1. Frequency of individual investment across rounds.



In line with our previous discussion, we observe that the likelihood of individual investment varies significantly across treatments (Krusall Wallis, p = 0.003). When doing pairwise comparisons, the Wilcoxon rank-sum (Mann-Whitney) test suggests that investment is significantly higher in the $CP_{0,80}$ treatment, where punishment is coordinated ($UP_{30,60}$ vs $CP_{0,60}$: p = 0.07; $UP_{30,60}$ vs $CP_{0,80}$: $p < 0.001$; $CP_{0,60}$ vs $CP_{0,80}$: p = 0.08). There exists also a tendency to decrease the individual investment across rounds. This is a significant in any of the treatments when we test for the trend (p < 0.001).

Next, we look at the behavior of investors in the repeated game by means of an econometric analysis. Our logit model is presented in Table D1, where we study the likelihood of individual investment across periods. Our previous findings hold in that investors are more likely to invest when punishment is coordinated in the $CP_{0,80}$ treatment. As already suggested by the Arellano-Bond model in the main text, there is also evidence for *homegrown trusting preferences* and some sort of reciprocity in the investment decision; i.e., investors tend to invest if they did it in the previous periods ($p < 0.01$) or if they observe that the other investor did invest in the previous period ($p < 0.01$). Our estimates for the period are negative suggesting that there is a tendency not to invest across periods. Finally, the results when controlling for individual characteristics seem to suggest that risk aversion plays a role in the investment decision of investors. In particular, risk averse investors are less likely to invest in Stage 1, especially if punishment is coordinated.

Table D1. Likelihood of individual investment in the repeated game: Logit model

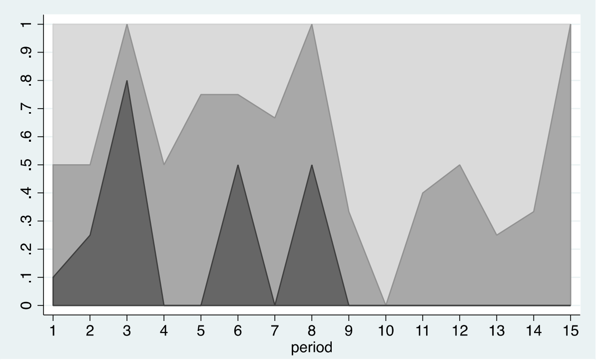
	Pooled data	Pooled data	$UP_{30,60}$	$CP_{0,60}$	$CP_{0,80}$
Constant	-2.322	-2.114	-1.369	-2.548	-1.165
	(0.219)	(0.615)	(1.260)	(1.021)	(1.067)
Investment $_{t-1}$	1.628	1.582	1.169	1.716	1.578
	(0.169)	(0.168)	(0.285)	(0.267)	(0.293)
Investment $_{t-2}$	1.20*	1.145	0.986	0.964	1.273
	(0.192)	(0.192)	(0.316)	(0.289)	(0.386)
Other Investment $_{t-1}$	0.264	0.271	0.231	0.273	0.294
	(0.032)	(0.033)	(0.057)	(0.0494)	(0.0568)
Period	-0.032	-0.035	-0.027	-0.070	-0.034
	(0.013)	(0.013)	(0.032)	(0.020)	(0.018)
Coord. Punish ($CP_{0,80}$)	0.443	0.446			
	(0.186)	(0.208)			
Coord. Punish ($CP_{0,60}$)	0.257	0.239			
	(0.161)	(0.166)			
Women		0.148	0.104	0.048	0.392
		(0.149)	(0.237)	(0.281)	(0.329)
Age		-0.010	0.016	-0.011	-0.101
		(0.010)	(0.008)	(0.020)	(0.037)
CRT		0.185	-0.252	0.140	0.821
		(0.188)	(0.305)	(0.403)	(0.430)
Risk Aversion		0.080	0.0983	0.169	0.093
		(0.0331)	(0.089)	(0.062)	(0.062)
Trust		0.315	0.927	0.415	-0.305
		(0.177)	(0.363)	(0.274)	(0.386)
Satisfaction		0.026	-0.054	0.0808	0.230
		(0.067)	(0.157)	(0.149)	(0.133)
Happiness		-0.286	-0.703	-0.187	0.067
		(0.144)	(0.333)	(0.246)	(0.253)
Inequality		0.0553	-0.085	0.342	-0.206
		(0.156)	(0.269)	(0.289)	(0.292)
Observations	1,950	1,950	650	650	650

Table D2. Dynamics of joint investment across rounds

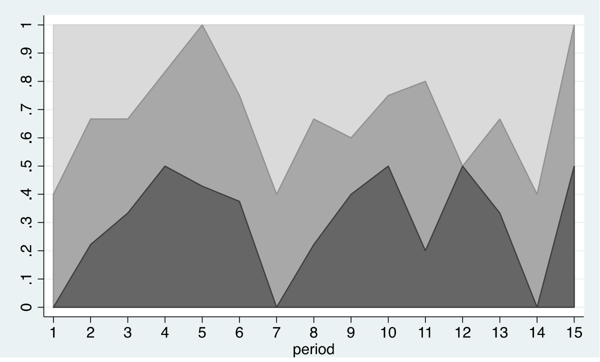
	$UP_{30,60}$	$CP_{0,60}$	$CP_{0,80}$
% Joint investment in *t* provided the group invested in *t*-1	43%	62%	80%
% No joint investment in *t* provided the group did not invest in *t*-1	91%	87%	84%

Figure D2. Punishment decisions across rounds (conditional on punishment being feasible).

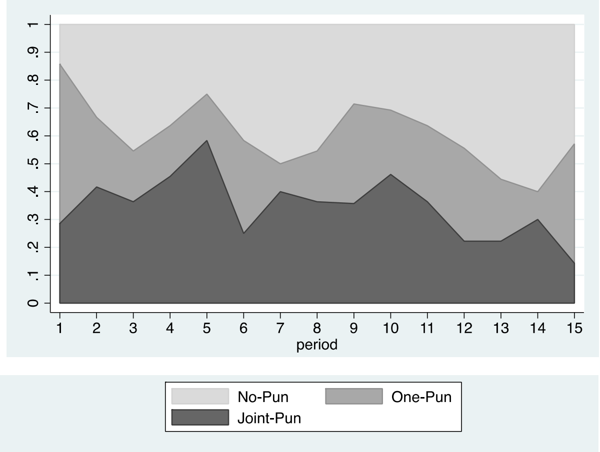
a) Uncoordinated punishment: $\mathbf{UP}_{30,60}$



b) Coordinated punishment; $\mathbf{CP}_{0,60}$



c) Coordinated punishment: $\mathbf{CP}_{0,80}$

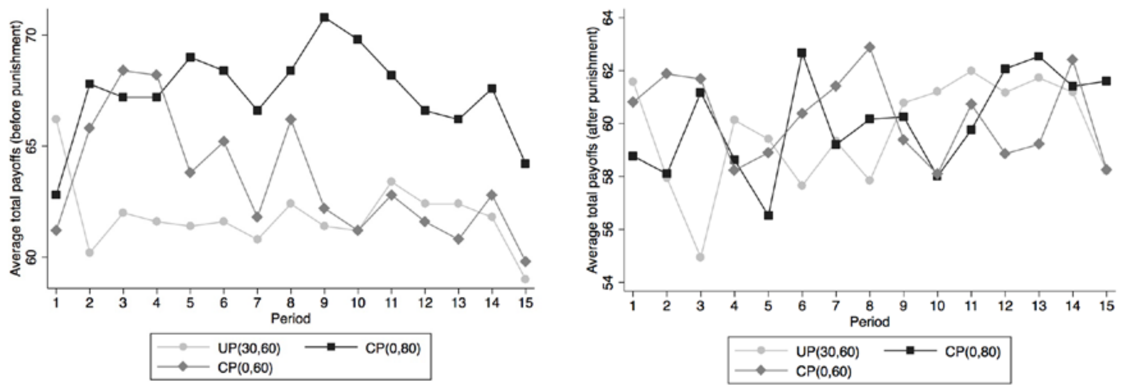


Panel *a*) shows that joint punishment is not used after round 9 in the case of uncoordinated punishment. This is in sharp contrast with the observed behavior when punishment is coordinated, where joint punishment remains steady around 30%, especially in $\mathrm{CP}_{0,80}$; see Panels b) and c).

Table D3. Average total payoffs before and after the punishment decision of investors

	$UP_{30,60}$	**$CP_{0,60}$**	**$CP_{0,80}$**
Average total payoffs (before punishment)	61.85	63.45	67.39
Average total payoffs (after punishment)	59.67	60.05	60.20
Effect of punishment on payoffs (%)	- 0.03	- 0.05	- 0.11
Average payoff of investor (before punishment)	19.21	19.47	20.24
Average payoff of investor (after punishment)	18.92	18.85	19.21
Effect of punishment on payoffs (%)	- 0.01	- 0.03	- 0.05
Average payoff of allocator (before punishment)	23.42	24.51	26.90
Average payoff of allocator (after punishment)	21.82	22.51	21.62
Effect of punishment on payoffs (%)	- 0.07	- 0.08	- 0.20

Figure D3. Average total payoffs across rounds



(a) Total payoffs before the punishment

(b) Total payoffs after the punishment

Total payoffs before the punishment decision of investors in Panel a) mimic the behavior of investors for the case of joint investment (Figure 4 in the main text). When investors punish, there is a decrease in the total payoffs and we observe no differences in total payoffs across treatments. Interestingly, panel b) seems to suggest that total payoffs increase across periods in any of the treatments, what may indicate that punishment may have beneficial results for longer-term interactions.