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United We Stand: On the Benefits of Coordinated Punishment

Vicente Calabuig
University of Valencia

Natalia Jiménez
Universidad Pablo de Olavide

Gonzalo Olcina
University of Valencia

Ismael Rodriguez-Lara
Chapman University, ismaelrl@uma.es

United we stand:

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Vicente Calabuig [1] • Natalia Jiménez [2] • Gonzalo Olcina [1] • Ismael Rodriguez-Lara [3,4,*]

[1] ERI-CES and Department of Economic Analysis. University of Valencia, Campus dels Tarongers. Facultad de Economia. Avda. dels Tarongers s/n. Valencia 46022 (Spain)

[2] Dpto. Economía, Métodos Cuantitativos de Historia Económica, Edificio 3, 3ª planta, despacho 19. Universidad Pablo de Olavide, Carretera de Utrera, Km 1. Sevilla 41013 (Spain).

[3] Universidad de Granada. Departamento de Teoría e Historia Económica. Campus Universitario de Carturja. Granada 18071 (Spain).

[4] Economic Science Institute (ESI). Argyros School of Business and Economics. Chapman University. One University Drive, Orange, CA 92866.

* Corresponding author: Email: ismaelrl@ugr.es, ismaelrl@gmail.com, Tel: (+34) 958220147

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United we stand:

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Abstract

Coordinated punishment occurs when punishment decisions are complements; i.e., this punishment device requires a specific number of punishers to be effective; otherwise, no damage will be inflicted on the target. 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.

Keywords: Team investment game, coordinated punishment, uncoordinated punishment, free-riding, cooperation.

JEL Classifications: C9, D02, D03, D69, J01

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

1. Introduction

United we stand, divided we fall. The maxim, which seems to stem from one of the Aesop's fables, highlights that members of a group may need to coordinate their actions in order to succeed.[1] This is indeed a pervasive idea; e.g., when members of a group want to inflict punishment on someone outside the group. There are numerous examples of what we call *coordinated punishment*; take, for example, workers who decide to go on strike to negotiate agreements on pay and conditions with their employers or the boycott of consumers who want to express their discontent with unethical practices by corporations. In all these settings, the need to achieve a certain threshold of punishers to inflict any real damage on the target arises naturally. There are also plenty of examples of institutions or organizations that have developed norms based on coordinated punishment to punish deviant or inconvenient behaviour of group members. Historical evidence includes the institution of ostracism in ancient Athens, the punishment of non-compliant-norm members in rural areas in Africa or the expelling of members in medieval guilds. In modern societies, there are also many instances of coordinated punishment in institutions such as partnerships, professional societies or unions, which require complementarities in the punishment decisions of their group members for punishment to have an effect on the target. In many cases, the need to achieve a certain threshold of punishers is indeed extreme in that unanimity is required to inflict any damage on the target.

While there are numerous examples of coordinated punishment in a variety of settings, most current models and experimental work usually consider that punishment is *uncoordinated*. In particular, it is often assumed that punishment decisions are substitutes and punishment is carried out on an individual basis. Arguably, this way of modelling punishment leaves unexplained a plethora of situations in which members of a group cannot inflict any damage by their own. Why does coordinated punishment exist? Is there any benefit of coordinated punishment with respect to uncoordinated punishment?

This paper aims to provide a theoretical explanation and an experimental validation to show the better performance of coordinated punishment compared with the traditional uncoordinated punishment. We focus our analysis on a relevant kind of asymmetric social interactions based on specialization and division of labour: a team investment game. This game can be thought as a hold-up game under incomplete contracts with several investors.

[1] Read "The Four Oxen and the Lion" (Aesop, 1867).

Team investment situations where the proceeds of aggregate investment of the team members (investors) is under the control of another agent (the allocator) are quite ubiquitous in real economies. A prominent example appears in the labour market. In many employment relations, a group of employees is hired by a single employer (the firm). The labour contract in these cases is highly incomplete and usually assigns significant authority to the employer. This asymmetric distribution of decision rights puts the employees in danger of being exploited, leading to inefficiencies if they refuse to cooperate (Gambetta 2000). In team investment situations, investors (employees) face the collective action problem of credibly threatening to punish opportunistic behaviour from the allocator (the firm). In this setting, players differ in their roles and strategies, and may have different opportunities to punish. Only investors can punish. The team investment game is therefore well suited to examine how coordinated and uncoordinated punishment work in an asymmetric situation. A relevant question is when, and under which conditions, will coordinated punishment perform better than uncoordinated punishment in the sense of yielding higher levels of joint investment and boosting the return set by allocators.[2]

One possible reason for the better performance of coordinated punishment, according to some authors (Boyd et al. 2010, Olcina and Calabuig 2015) is that coordinated punishment is associated to *increasing returns to scale*. This feature implies that the individual cost of punishment decreases with the number of punishers and/or the effectiveness of the punishment (i.e., the damage inflicted on the target) increases exponentially with the number of punishers. This, in turn, calls for modelling coordinated and uncoordinated punishment as two different technologies as it is implicitly assumed that individuals using coordinated punishment can inflict the same damage at a lower cost, or more damage at the same cost when they punish *together as a group* than when they do it individually (fighting their own battles). As a result, the benefits of coordinated punishment compared with uncoordinated punishment could be explained by a higher effectiveness of the punishment (or a more beneficial "fine-to-fee" ratio) in the former device.[3] In this paper, we show the benefits of coordinated punishment *even if* the successful coordination does not result in "increasing returns to scale", although this feature enlarges the benefits of coordinated punishment.

We rely on a team investment game with two investors and one allocator (Cassar and Rigdon 2011, Olcina and Calabuig 2015). Investors can be considered to be the workers of a firm or partners in a joint venture who choose whether or not to exert effort in a joint project, while the allocator controls the proceeds of the investment and decides how to share the returns of investment. We assume that investment decisions are complements (e.g., Harrison and Hirshleifer 1989, Van Huyck et al. 1990, Brandts and Cooper 2006, Riedl et al. 2015) and the allocator values the investors' decisions equally. This, in turn, implies that the allocator will return the same amount to them if there is joint investment. Investors decide whether or not

[2] This strategic situation clearly differs from a public good game in which all players have the same strategies and (usually) the same opportunities to punish.

[3] There is evidence that the effectiveness of the punishment influences behavior; e.g., in the public good (Nikiforakis and Normann, 2008) or the investment game (Rigdon, 2009).

to punish the allocator observing the returned amount. Our design is deliberately simple as we would like to compare the performance of coordinated and uncoordinated punishment in a "small numbers" scenario with few investors, when we avoid any interference from other motivations such as envy or inequality aversion among investors. In our paper, we consider three different settings depending on how the decision to punish affects the earnings of the allocator. If punishment is uncoordinated, the allocator's earnings are reduced by 30% (60%) if one (both) of the investors decides (decide) to punish. If punishment is coordinated, investors reduce the allocator's earnings if and only if they both decide to punish; i.e., the allocator's payoffs are not reduced if only one investor punishes, while the allocator's earnings are reduced by 60% if both investors punish. This, in turn, incorporates the idea that punishment requires coordination to be effective. Finally, we consider the case in which investors can reduce the allocator's earnings by 80%, therefore coordinated punishment is more effective than uncoordinated in this setting if both investors punish. This later condition allows us to examine a setting in which coordinated punishment decisions exhibit returns to scale.

Our theoretical model builds on the assumption that individuals display social preferences (Bolton and Ockenfels 2000, Fehr and Schmidt 1999, Charness and Rabin 2002). In particular, we propose a two-sided incomplete-information model with two possible types of investors (selfish and inequality-averse) and two possible types of allocators (fair-minded and profit-maximizer). We assume that selfish investors focus on their own material payoffs and consequently never punish, while inequality-averse investors care about the return of the investment and will be willing to punish the allocator if they do not receive a fair return.[4] 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 an *efficient pooling equilibria* in which both the selfish and the inequality-averse investor decide to invest in equilibrium. We also show that joint investment is more likely to occur in the efficient pooling equilibria when punishment requires coordination and this effect is amplified for increasing returns to scale. The rationale is that requiring coordination prevents the free-riding behavior of inequality-averse investors in the punishment stage. In equilibrium, allocators anticipate that (for certain values of the proportion of inequality-averse investors) investors will be more willing to punish if punishment is coordinated, thus they return a larger amount when punishment is coordinated than when it is uncoordinated. We focus on this efficient static equilibrium in order to characterize the behavior of the players in the finitely repeated game framework, using Folk theorems (Benoit and Krisna 1985, Friedman, 1971, Fudenberg and Maskin, 1986).

[4] Inequality-averse 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 3.

We conduct a series of laboratory experiments to test the predictions of our model.[5] Since our examples on labor conditions and punishment as an institutional feature of some groups can be associated to long-term relationships, our experiment relies on a fixed-matching protocol in which the same subjects interact repeatedly. This feature of the design allows subjects to undertake actions on the benefits of others so as to obtain a direct or indirect benefit in their future interaction (Trivers 1971, Axelrod and Hamilton 1981, Leimar and Hammerstein, 2001), which seems to be likely to occur in the workplace.[6]

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. We also find that increasing returns to scale in coordinated punishment help in fostering the level of joint investment but does not seem to have an effect on the return. This, in turn, shows that the benefits of coordinated punishment may be observed even if returns to scale are absent. One might expect that these findings translate into efficiency gains when punishment is coordinated. Empirically, this does not appear to be the case when we look at the sum total payoffs across treatments, which does not seem to differ across treatment. This occurs because joint punishment is used more frequently when punishment is coordinated, thus there is a significant surplus destruction in this setting. However, we also find that the final payoffs of investors and allocators are below their initial endowments (i.e., there are *loses from trade*) when punishment is uncoordinated. This does not occur when punishment is coordinated.

While we are mainly interested in showing the benefits in the team investment game of the coordinated punishment compared with the uncoordinated punishment, our paper includes a second study where we test how each punishment device performs compared with a setting in which punishment is not possible. One relevant finding in the dyadic version of the investment game (Berg et al. 1995) is that allowing for the possibility of punishment can have a detrimental effect on behavior. In particular, Fehr and Rockenbach (2003), Fehr and List (2004) and Houser et al. (2008) provide evidence that the possibility of punishment leads to lower returns from allocators, while Rigdon (2009) or Calabuig et al. (2016) show that allowing for punishment might not increase the level of investment, except if the the fee-to-fine ratio or the investor's capacity of punishment is sufficiently high. The results of our second study are intended to shed light on whether these results hold in a team investment game. Our main finding is that the detrimental effect of punishment mainly occurs if punishment is uncoordinated, while we do not find any evidence that coordinated punishment is worse than no allowing for punishment if the former device exhibits returns to scale.

[5] Experimental evidence can also be helpful to understand behavior in the workplace (Falk and Fehr 2003, Charness and Kuhn 2010, Herbst and Mas 2015),

[6] In addition, our matching protocol allows us to examine the convergence to the theoretical prediction.

To our knowledge, we are the first to investigate the effect of coordinated punishment in an asymmetric situation like the team investment game.[7] 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 punishers increases. In their setting, however, there is also the possibility of peer punishment, since investors can punish each other after observing the punishment decision of the other team members. Our contribution to this literature is to directly compare the use of coordinated and uncoordinated punishment in an asymmetric situation like the team investment game, which also resembles a hold up team situation. In this 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. In addition, we complement our theoretical predictions with empirical evidence gleaned from a laboratory experiment that attempts to show the benefits of coordinated punishment in a controlled environment. The findings in our experiment dovetail with other studies that rely on the idea that punishment may require coordination; e.g., in the form of voting. Tyran and Felds (2006), Casari and Luini (2009), Ertan et al. (2009), Putterman et al. (2011), Noussair and Tan (2011) or Van Miltenburg et al. (2014), among others, allow subjects to vote over different punishment schemes in a Public Goods Game. Their results suggest that this usually results in efficiency gains because subjects tend to punish below-average contributors and strong cooperators are barely punished. Our findings that subjects tend to free-ride on the punishment decisions of others when punishment is uncoordinated but are more likely to punish together when punishment is coordinated relate also our paper to other studies that discuss the importance of conditional punishment in the public good game (Cinyabuguma et al. 2006, Casari and Luini 2009, Kamei 2014). Key to our discussion is the fundamental difference between the public good game (where players have symmetric roles and identical opportunities to punish) and the team investment game (in which players differ in their roles and opportunities to punish). Further, we do not allow for voting but instead consider that coordinated punishment occurs when players undertake an individual decision that is costly to them (e.g., they decide to go on strike).

The rest of the paper is organized as follows. Section 2 presents our experimental design. We present our theoretical predictions in Section 3 and summarize our findings in Section 4. We compare our results with coordinated and uncoordinated punishment with a treatment without punishment in Section 5. Section 6

[7] As noted above, 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 dyadic version of the game, thus they are not well-suited to compare the effectiveness of uncoordinated and coordinated punishment in a team investment situation.

concludes. The proofs are relegated to the online appendix. This contains additional material such as the experimental instructions or further analysis of our data.

2. The team investment game with punishment

2.1. Experimental Design

Investment game.—

We consider a team investment game with two investors and one allocator, as this is the minimal setting in which coordinated punishment can be studied in an asymmetric situation (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 or not to invest in a joint project. The investment decision is binary decision that can be interpreted as investors putting effort or not in a joint project, or hiring an external agent. The individual cost of the investment equals 5 ECUs. We focus on the case in which decisions are complements (e.g., Harrison and Hirshleifer 1989, Van Huyck et al. 1990, Brandts and Cooper 2006, Riedl et al. 2015); as a result, the game ends if none of the investors (or only one of them) decide to invest.[8] When both investors decide to invest, the game proceeds to stage 2.
- **STAGE 2 (Return).** The joint investment results in a surplus of 30 ECUs. In Stage 2, the allocator chooses the amount of ECUs to be returned to the investors. As the investment of each investor is equally valuable, we impose that any return $x \in [0,30]$ will be equally divided between the two investors, thus each of them receives $x/2$.[9] The allocator adds the amount he keeps (30 - x) to his initial endowment of 20 ECUs.
- **STAGE 3 (Punishment).** Investors are allowed to punish the allocator upon observing the returned amount. The punishment decision has a cost of 5 ECUs for each investor and it can be interpreted as investors deciding whether or not to protest or go on strike. This cost is paid regardless of the punishment inflicted to the allocator, whose payoffs are reduced in a given share $\lambda_n \in [0,1]$, depending on the number of investors that punished, $n \in \{0, 1, 2\}$. 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.

[8] We are interested in analyzing group punishment and the inclusion of some subgames in which we only allow to punish the investor who decides to invest would not add to the analysis.
[9] This feature of our design prevents also that investors compare themselves before deciding whether or not to punish the allocator; i.e., punishment decisions are independent of the amount received by the other investor.

Treatments.— 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)
$UP_{30,60}$	0%	30%	60%
$CP_{0,60}$	0%	0%	60%
$CP_{0,80}$	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. In all the three treatments, investors have to pay the individual cost of punishment if they want to reduce the allocator's payoffs, regardless of whether or not the earnings of the allocator are reduced.

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.

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 one [both] of the investors decides [decide] to punish. Hence, our second treatment incorporates the idea that *investors need to coordinate their actions* to reduce the allocator's payoffs.

Our **third treatment ($CP_{0,80}$)** incorporates the two characteristic aspects of coordinated punishment; i.e., the fact that coordination is needed and the increasing returns to scale. In this treatment, the allocator's payoffs are reduced by λ_1= 0 [λ_2 = 0.80] if one [both] of the investors decides [decide] to punish.

One aspect to be noticed is that the value of λ_n can be interpreted as the *capacity of punishment* of investors, as this denotes how their punishment decisions affect the earnings of the allocators. In our setting, this capacity of punishment λ_n denotes the share that investors can destroy from allocators and it is independent on the amount that allocators return. However, there is a direct relationship between the capacity of punishment of investors and the *fine-to-fee* ratio or the effectiveness of punishment, which can be defined as the factor by which punishment reduces the allocator's payoff (Calabuig et al. 2016). In our setting, 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 in the uncoordinated treatment $UP_{30,60}$, she pays 5 ECUs and the allocator's payoffs are reduced by 0.3 (50) = 15 ECUs. This implies that the fine-to-fee ratio is equal to 15/5 = 3 in the $UP_{30,60}$ treatment.[10] In the $CP_{0,60}$ treatment, the fine-to-fee ratio would be equal to 0 (3) when only one investor punishes (both investors punish), respectively. This occurs because any investor is unwilling to reduce the earnings of the allocator by her own, but when they both punish, they reduce the earnings of the allocator by 0.6 (50) = 30 ECUs (at the cost of 10 ECUs). Finally, the fine-to-fee ratio would be 4 or below in the $CP_{0,80}$ treatment, which implies

[10] This factor is frequently used in other experiments that allow for punishment in the investment game (Charness et al. 2008, Rigdon 2009). This factor is endogenous determined in our setting, as the final reduction in the allocators' earnings depends on the returned amount. To see this, note that if the allocator is punished after returning 5 ECUs to each investor in the $UP_{30,60}$ treatment the reduction in her payoffs would be 0.3 (50 – 10) = 12 ECUs, thus the fine-to-fee ratio would be 12/5 = 2.4. If the allocator returns 10 ECUs to each investor, the fine-to-fee ratio would be 0.3 (50-20)/5 = 1.8. As a result, the fine-to-fee ratio decreases as the return of the allocator increases (Calabuig et al. 2016 for further discussion).

that investors have a higher capacity of punishment in this treatment because of the existence of increasing returns to scale. We decided to keep the capacity of punishment constant instead of fixing the fine-to-fee ratio to avoid that the capacity of punishment of investors decreases when allocators return very few since this is precisely the case where investors may have more motives to punish. Other studies in which there is a variation of the fine-to-fee ratio within the same treatment or across treatments are Fehr & Gächter (2000) or Cassari (2005).

Payoffs.—Let the dummy 1_i take the value 1 if investor i decides to invest and 0 otherwise, while the dummy 1_J stands for the case of joint investment; i.e., this takes the value 1 when both investors decide to invest; otherwise $1_J = 0$. Similarly, let the dummy 1_p denote whether investor i decides to punish or not.

The final payoff of each investor, π_i is determined as follows:

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

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

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

where $\lambda_n = \{\lambda_0, \lambda_1, \lambda_2\}$ denotes the reduction in the allocator's payoff after the punishment decisions (see Table 1).

2.2. Procedures

We recruited a total of 225 subjects (75 per treatment) to participate in six different sessions, which were conducted at the LINEEX (University of Valencia). Subjects were 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 a subject's role, investor (Player A) or allocator (Player B), being fixed during the whole session.[11] In each period, investors had to choose whether or not to invest in a joint project. We employed

[11] 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

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. We decided to employ this method to have more observations for the case in which we could not observe joint investment. In addition, there is evidence that the strategy method does not affect the behavior of allocators in the investment game compared with the direct method (see the meta-analysis in Johnson and Mislin, 2011). Importantly, the decision of allocators 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.[12]

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, including a 5€ show-up fee. A questionnaire at the end of the session was used to elicit, among other characteristics, the subjects' gender, age, cognitive abilities (Frederick, 2005), risk aversion (Gneezy and Potters, 1997) or trusting behavior (Glaeser et al. 2000). We shall use these variables as controls in our econometric analysis. The Appendix B contains the translated version of the experimental instructions, the screenshots of the experiment and the complete questionnaire. This includes a summary of the demographic variables that we collected in our questionnaire (see Table B1)

3. Theoretical predictions with two-sided incomplete information

There is ample evidence that subjects are not purely self-interested but have social preferences (Fehr and Schmidt 1999, Bolton & Ockenfels 2000, Charness and Rabin 2002). In fact, these preferences can explain why investors (allocators) decide to invest (return) in the investment game (see, among others, Berg et al. 1995, Eckel and Wilson 2011, Johnson and Mislin 2011, Alos-Ferrer and Farolfi, 2019). Next, we present a two-sided incomplete information model to derive our theoretical predictions. More precisely, we characterize the efficient pooling equilibria in which both the selfish and the inequality-averse investor decide to invest in equilibrium in Stage 1 of the stage game.[13] In this section, we also discuss the optimal return of allocators and present our testable hypotheses for the repeated game.

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.

[12] As the type of information that subjects receive across rounds may be important to determine behavior (e.g., see Nikiforakis (2010) for evidence in the public good game) we keep this constant across treatments. We also keep constant the information that allocators received in regards to the use of the strategy method; in none of the treatments did the investors know that allocators were making a choice for the case in which there would be joint investment.

[13] 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.

3.1. The model

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

Inequality-averse investors care about the distribution of the surplus. In particular, they focus on the amount generated by their investment decision and compare the payoff they receive, $x/2$, to half of the payoff that the allocator decided to keep $(30 – x)/2$. Their utility after receiving the return is as follows:

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

where π_i is given by equation (1). The utility function of inequality-averse investors is such that any return that falls below what inequality-averse investors consider a "fair" return (x = 15) will generate disutility for them, thus inequality-averse investors may be willing to punish to attain more equal outcomes (Houser and Xiao 2010, Bone and Raihani 2015).[14] When inequality-averse investors observe an "unfair" return (x < 15), they may punish the allocator.[15] The payoffs of inequality-averse investors who decide to punish will be given by:

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

where the value of $\lambda_n \in [0,1]$ depends on the number of investors in the team who decide to punish. In the population, there is also a proportion $m_f \in [0,1]$ of fair-minded allocators. They are motivated by positive reciprocity. We hereafter assume that their dominant strategy is to choose the "fair" return x = 15. The rest of the allocators are selfish, and their optimal strategy would consist of comparing the expected cost of being punished and the cost of avoiding punishment, as we shall discuss below.

[14] Investors can have other motives to punish apart from reducing the inequality; e.g., Casari and Luini (2012) find evidence of *expressive* punishment in the public goods game as some subjects may derive utility from the act of punishing.

[15] Although we derive our predictions under the assumption that inequality-averse 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 settings have an effect on the threshold values for which allocators will be willing to punish, but they do not affect our qualitative predictions regarding the effects of the treatments. Our results are also robust to consider that inequality-averse investors do not care about the distribution of the surplus but look at the total payoffs, which includes the initial endowments. This is mainly because all players receive the same initial endowment in our game, thus a model where players receive different endowments should consider this aspect. In our exposition, we decided to use the distribution of the surplus as reference point (instead of the distribution of total final payoffs), because this approach fits better to the applications we have in mind; e.g., in the labor market.

3.2. Theoretical predictions

Next, we discuss the conditions for the existence of an efficient (Perfect Bayesian) pooling equilibrium in which both the selfish and the inequality-averse investor decide to invest in equilibrium in Stage 1. We also discuss the punishment behavior of investors in the efficient equilibrium in Stage 3 and the consequences of this behavior for the optimal return that allocators choose in Stage 2. [16]

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

Proposition 1. (Efficient pooling equilibria)

a) In the ***UP$_{30,60}$*** *treatment, there is an efficient pooling equilibrium in which both types of investors choose to invest if (the proportion of inequality-averse investors) $q_a \geq \bar{q}$ =0.3464 and (the proportion of fair-minded allocators) $m_f \geq \overline{m}(q_a)$, where $\overline{m}(q_a)$ is increasing in q and $\overline{m}(\bar{q}) = 0.88$. In this equilibrium, fair-minded allocators set the "fair" return, $x^* = x^F$ = 15, and selfish allocators set the minimum return such that inequality-averse investors do not punish them. This optimal return, $x^*_{UP}(q_a)$ is decreasing in q_a.*

b) 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 (the proportion of inequality-averse investors) $q_a \geq \bar{q}$ = 0.6 [$q_a \geq \bar{q}$ = 0.5] and (the proportion of fair-minded allocators) $m_f \geq \overline{m}_2(q_a)$, where $\overline{m}_2(q_a)$ is decreasing in q_a and $\overline{m}_2(\bar{q}) = 0.76$. [$\overline{m}_2(\bar{q}) = 0.80$] . In these equilibria, fair-minded allocators set the "fair" return x^* = x^F = 15 and selfish allocators set the minimum return such that inequality-averse investors do not punish them. This optimal return, $x^*_{CP}(q_a)$ is increasing in q_a.*

Proof. See Appendix A3.

Proposition 1 states that there will be joint investment under uncoordinated punishment even if the proportion of inequality-averse investors (q_a) is low, but only if the proportion of fair-minded allocators (m_f) is very high, and higher than a critical value which is increasing with (q_a). On the other hand, under coordinated punishment, there will be joint investment only if q_a is sufficiently high but the minimal value of m_f that is needed for the (Perfect Bayesian) pooling equilibrium to exists is substantially smaller with coordinated punishment. In addition, this minimal value of m_f decreases in q_a.

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[16] In our game, there are also multiplicity of equilibria, including the case in which none of the investors decide to invest, or the case of in which inequality-averse investors punish in equilibrium. We fully characterize these (inefficient) equilibria in Appendix A.

Figure 1 depicts the set of pairs (q_a, 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 for 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; i.e., for any (random) distribution of types, it is more likely to obtain the pooling equilibrium in $CP_{0,80}$ and $CP_{0,60}$ than in $UP_{30,60}$. Further, if the proportion of inequality-averse investors (q_a) is sufficiently high (e.g., when $q_a > 0.6$), we can observe in Figure 1 that the set of efficient pooling equilibria in the $UP_{30,60}$ treatment is contained in both, the set of the $CP_{0,60}$ treatment and that of the $CP_{0,80}$ treatment.[17]

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

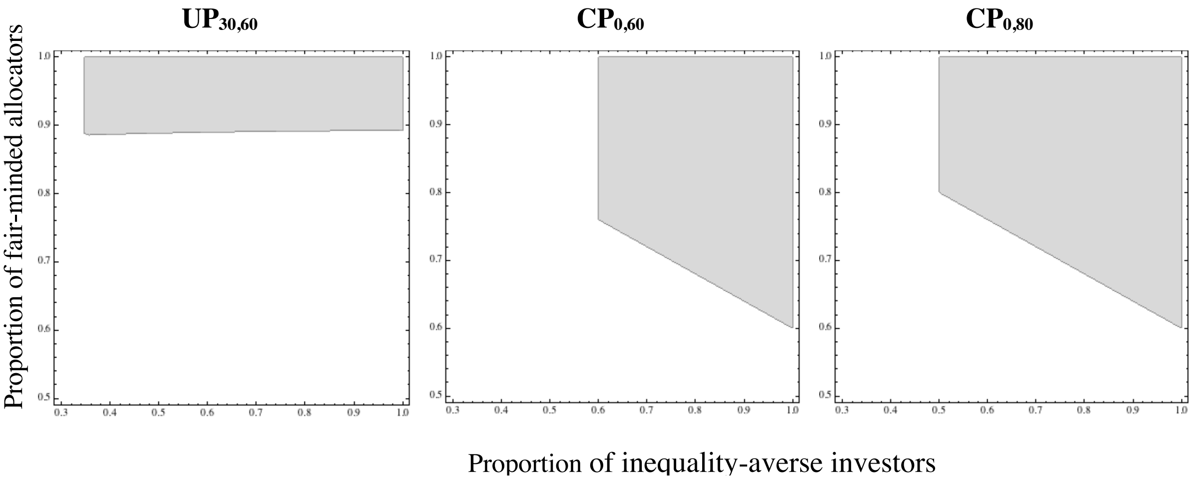


Figure 1. Efficient pooling equilibria in each treatment (grey area)

Coordinated punishment not only facilitates joint investment, 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 inequality-averse investors in equilibrium, which depends not only on the returned amount (x) but also on their beliefs about the proportion of inequality-averse investors in the

[17] Note that the equilibria in the $CP_{0,80}$ treatment is a super-set of $CP_{0,60}$ because we only vary the effectiveness of the punishment (or the fee-to-fine ratio) in these treatments. By affecting this feature of the design (i.e., the value of $\lambda_2 \in [0,1]$), we would enlarge the set of pooling equilibria when punishment is coordinated.

population (μ) (see Lemma 1 in Appendix A1).[18] When punishment is uncoordinated, inequality-averse investors always punish in equilibrium if the return is small, regardless of the proportion of inequality-averse investors in the population. This is because inequality-averse investors can always reduce the inequality by punishing in the uncoordinated treatment. Paradoxically, inequality-averse investors will only punish for intermediate or high values of the return if their belief about the proportion of inequality-averse investors is below a certain threshold. If inequality-averse investors believe that the proportion of inequality-averse 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 inequality-averse 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 of achieving the threshold to reduce the allocator's earnings acts as a coordination device that eliminates the free-riding behavior of investors in the punishment stage. Along these lines, inequality-averse investors always punish in the presence of coordinated punishment when they believe that there are enough inequality-averse investors in the population.[19]

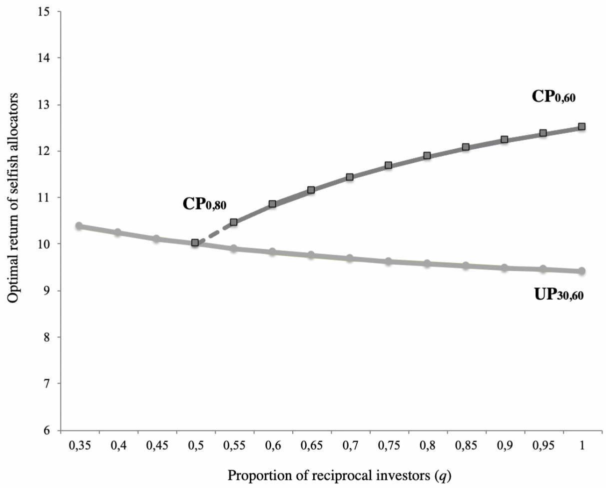


Figure 2. Optimal return of selfish allocators in the efficient pooling equilibria. In these equilibria, fair-minded allocators return the "fair" amount, $x = 15$.

As we show in Figure 2, the punishment behavior of investors in Stage 3 has implications for the reward policy of selfish allocators in Stage 2 (recall that fair-minded allocators always return the "fair" amount $x = 15$ in equilibrium). Selfish allocators compare the expected cost of being punished and the cost of avoiding punishment. Again, their beliefs regarding the proportion of inequality-averse investors in the population play a crucial role to explain behavior. In all treatments, allocators return nothing if they

[18] It is easy to check that no punishment is the dominant action for selfish investors. Thus, only inequality-averse investors may punish in equilibrium in Stage 3. In the main text, we present the intuition for the punishment behavior in each treatment in equilibrium. The proof is presented in Appendix A1.

[19] The threshold for beliefs about the proportion of inequality-averse 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, inequality-averse investors never punish if the return is sufficiently high ($x \geq 12.5$), regardless of the proportion of inequality-averse investors. One may expect that inequality-averse investors will request at least the "fair" return ($x = 15$) not to punish, but recall that punishment is costly for the investors, thus inequality-averse investors account for this cost when choosing whether to punish.

believe that proportion of inequality-averse 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 inequality-averse 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 inequality-averse investors: if punishment is uncoordinated, the increase in μ implies that the free-riding problem is more likely to occur, thus inequality-averse 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 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.

3.3. Repeated game and hypotheses.

In our experiment, subjects play the team investment game for 15 periods. Once we have characterized the efficient pooling equilibria of the stage-game, we follow the approach in Brown et al. (2004) and use some well-known results on finitely-repeated games with incomplete information to derive our testable hypotheses.

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_a, 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_a and m_f 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_a, 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 predict that it will be more likely to observe joint investment under coordinated punishment than under uncoordinated punishment in the repeated game, if we assume any random distribution of types. This prediction also holds if the proportion of inequality-averse investors is sufficiently high.[20] The hypothesis we want to reject is then as follows:

[20] Brown et al. (2004) follow a similar approach in the context of relational contracts that may not be enforced by a third party. They show that high levels of effort can be sustained in a perfect Bayesian equilibrium if there exists a sufficient number of fair-minded workers who reciprocate generous contracts. To show the benefits of coordinated punishment, we require that the distribution of types is random or the proportion of inequality-averse investors is high enough to facilitate joint investment. If the proportion of inequality-averse investors is low, and the proportion of fair-minded allocators high, then joint investment may be more likely if punishment is uncoordinated (see Figure 1).

Hypothesis 1. *Joint investment does not vary across treatments; i.e., investors are equally likely to invest when punishment is uncoordinated and when it is coordinated.*

A similar argument can be applied for allocators' behavior. Using our results in Figure 3, we expect that allocators will return a higher amount when punishment is coordinated. The hypothesis we want to reject is as follows:

Hypothesis 2. *The returned amount does not vary across treatments; i.e., allocators return the same amount when punishment is uncoordinated and when it is coordinated.*

Our prediction is that coordinated punishment will have an effect in the amount that allocators return because investors are more likely to punish in the presence of coordinated punishment when q_a is sufficiently high. As a result, allocators have incentives to increase their return when punishment is coordinated so as to avoid being punished.

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 reducing the allocator's payoffs. As an alternative, investors can choose not to invest in future periods. The hypothesis that we want to reject is that investors will follow the same strategy across treatments.

Hypothesis 3. *The strategy that investors use to punish unfair returns by allocators does not vary across treatments; i.e., investors will be equally likely to punish or refrain from investing when punishment is coordinated and when it is uncoordinated.*

Since 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 periods when punishment is uncoordinated; i.e., we predict that more punishment will be observed when punishment is coordinated ($CP_{0,60}$ and $CP_{0,80}$) while more periods of non-investing will be observed when punishment is uncoordinated ($UP_{30,60}$).

4. Results

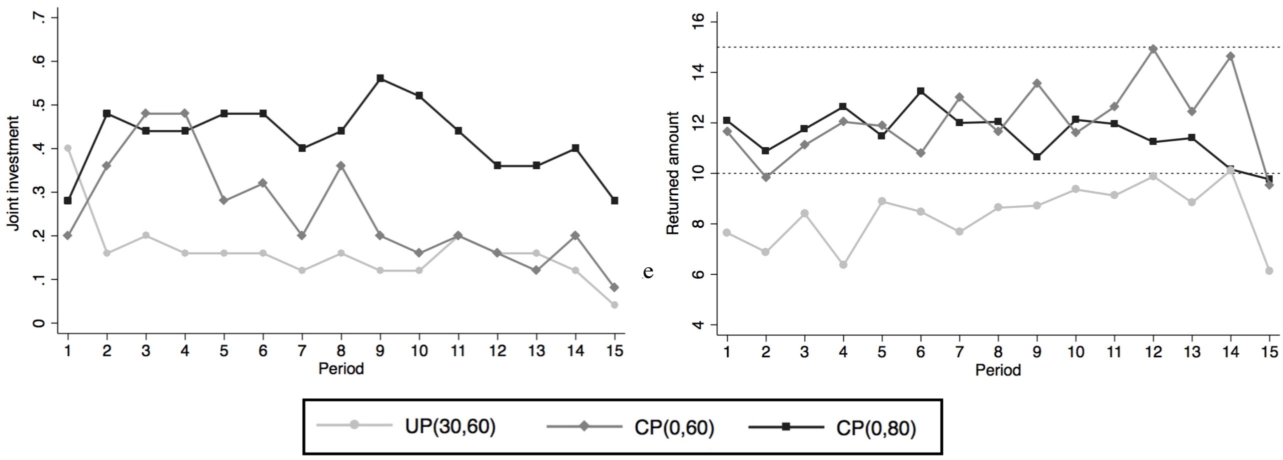
Section 4.1 presents some descriptive statistics for the level of the joint investment and the allocator's return, including our non-parametric analysis. In this section, we also perform an econometric analysis to control for the dynamics in the repeated game and the demographic characteristics of individuals. We discuss the punishment behavior and the differences in total payoffs across treatments in Section 4.2. This includes an overview on how the payoffs of investors and allocators vary across treatments. Overall, our experimental data lend support for the predictions that coordinated punishment fosters the levels of joint investment and increases the returned amount. We also find that investors punish more often when punishment is coordinated, while they refrain from investing when punishment is uncoordinated. We observe no differences in terms of efficiency across treatments.

4.1. Investment decisions and returned amount

Descriptive statistics and dynamic.— Figure 3 displays the relative frequency of joint investment in Panel (a) and the intended return in Panel (b) across periods, for each of the treatment conditions separately. As we use the strategy method for allocators, one may argue that their decision takes place in a "cold" state, as they do not know whether or not their choices will be implemented. To address this issue, Table 2 includes *i*) the effective return of allocators (i.e., their reward in periods of joint investment) and *ii*) the return of allocators in a "hot" state, which is assumed to occur when allocators made a choice after observing that both investors invested in the previous round (i.e., this choice corresponds to the intended return of allocators when they know that their previous choice was implemented). At the bottom of Table 2 we show the proportion of investors who benefited from trade and ended up with more than 20 ECUs after receiving the return from the allocator. This is an important measure of efficiency that relates to the idea in Gambetta (2000) that the investment decision should be repaid so that it is worth engaging in some form of cooperation in the future (see also Alos-Ferrer and Farolfi, 2019). Table 2 presents also the results of our non-parametric analysis where we pool the observations by groups across the 15 periods to guarantee independence.[21] In our analysis, we focus on the effect of coordinated punishment on the likelihood of joint investment and the return of allocators by comparing the behavior of investors and allocators in $UP_{30,60}$ and $CP_{0,60}$ (see the column $CP_{0,60}$). We examine the effects that the capacity of punishment has (if any) by looking at the comparison between $CP_{0,60}$ and $CP_{0,80}$ (see the column $CP_{0,80}$).[22]

[21] 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.
[22] We do not compare $UP_{30,60}$ and $CP_{0,80}$ as two elements change across treatments, namely *i*) the need to coordinate the actions and *ii*) the increasing returns to scale (or the effectiveness of the punishment). The interested reader can consult Calabuig et al. (2019)

(a) Frequency of joint investment (b) Intended return



In the first period, the frequency of joint investment is quite similar in the two coordinated conditions ($CP_{0,60}$ and $CP_{0,80}$) while in $UP_{30,60}$ the initial frequency of joint investment (40%) is double than in $CP_{0,60}$. Nevertheless, this 40% of joint 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 13% (see Table 2). 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 and close to the level of $UP_{30,60}$ in the last 5 periods of the experiment (see Table 2). Joint investment in the $CP_{0,80}$ treatment is around 40%-50%, with the percentage being quite stable over periods (see Table 2).[23] Statistically, the Krusall-Wallis test indicates that the levels of joint investment differ across treatments ($p = 0.002$). When we compare the behavior of investors across treatments we find that the average level of joint investment in $CP_{0,60}$ is higher than in $UP_{30,60}$ ($p = 0.031$), but it is smaller in $CP_{0,60}$ than in the $CP_{0,80}$ ($p = 0.017$). There are also differences in the dynamics across treatments. The proportion of groups that invest in a subsequent period after investing in the current one is 43% in $UP_{30,60}$, 62% in $CP_{0,60}$, and 80% in $CP_{0,80}$. The differences are significant using a test of proportion when we compare $UP_{30,60}$ and $CP_{0,60}$ ($p = 0.028$) and $CP_{0,60}$ and $CP_{0,80}$ ($p = 0.029$), thus the possibility of coordinated punishment facilitates that investing-groups keep investing, and the increasing returns to scale seems to amplify this effect.[24]

There are three results regarding the intended return in Figure 3 (b) that are worth mentioning. First, we observe that the behavior of allocators is quite stable across rounds in all the three treatments (see also Table 2). Second, 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 in Figure 2. Finally, the observed return is

for details on this analysis, but the main findings follow from our analysis below: the level of joint investment and the return is higher in $CP_{0,80}$ than in $UP_{30,60}$.

[23] In all the treatments, we observe an end-period effect, which is not surprising given that subjects knew that the game was repeated for exactly 15 periods. The results in the trend 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).

[24] We provide further evidence that coordinated punishment faciltates joint investment in Appendix D (see Table D1), where we classify groups depending on the number of periods that there is joint investment. Our results suggest that the frequency of high-investing groups (that invested 7 or more periods) is higher when punishment is coordinated ($UP_{0,30}$: 16%, $CP_{0,60}$: 28%, $CP_{0,80}$: 52%).

below the horizontal line of 10 ECUs when punishment is uncoordinated, thus investors do not retrieve on average what they invest in this treatment (see Figure D2 in the Appendix D for the distribution of intended return across treatments).[25] These findings have consequences for the behavior of investors in future rounds, as we shall discuss below. Statistically, the Krusall-Wallis test indicates that the intended return differs across treatments ($p = 0.036$). When we do pairwise comparisons, we find that 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 $CP_{0,80}$ ($p = 0.72$). These findings are robust when we look at the behavior of allocators across treatments using the effective return or the return of allocators in a "hot" state, which is higher in $CP_{0,60}$ than in $UP_{30,60}$ ($p < 0.007$) but indistinguishable in $CP_{0,60}$ and $CP_{0,80}$ ($p > 0.87$).

As a result, we observe that investors are more likely to retrieve their investment if punishment is coordinated; in fact, Table 2 shows that roughly half of the investors (52%) who received a return from their allocator benefited from trade, while this proportion goes up to two thirds (66%) when punishment is coordinated.[26]

Overall, these results lend support to reject the hypotheses that coordinated punishment does not affect behavior in our team investment game. We find that coordinated punishment fosters the level of joint investment and boosts the reward set by allocators. Moreover, our findings suggest that the increasing returns to scale might help in encouraging higher levels of joint investment, while it has barely an effect on the return decision; i.e., it is sufficient that investors need to coordinate their actions to increase the reward set by allocators, and lead investors gain from trade.

Table 2. Investment decisions and returned amount across treatment conditions

	$UP_{30,60}$	$CP_{0,60}$	$CP_{0,80}$
% Joint investment			
Pooled data	16%	25%	42%
Mann-Whitney (*p*-value)		(0.031)	(0.017)
Periods 1-5	21%	36%	42%
Periods 6-10	13%	25%	48%
Periods 11-15	13%	15%	37%

[25] We observe that allocators are heterogeneous in their return with spikes in the data taking place in 0 ECUs (no return), 10 ECUs (investors retrieve their investment), 15 ECUS (fair-minded allocators, according to our theoretical model), and 20 ECUs (allocators divide the joint surplus equally among the three members of the team).

[26] It is also possible to obtain the share of investors that are not harmed from trade by considering those who receive 5 ECUs or more, with a very similar picture (24% in $UP_{30,60}$, 35% in $CP_{0,60}$ and 42% in $CP_{0,80}$).

Intended return			
Pooled data	8.3	12.1	11.6
Mann-Whitney (*p*-value)		(0.014)	(0.72)
Periods 1-5	7.6	11.3	11.8
Periods 6-10	8.8	12.8	10.9
Periods 11-15	8.6	12.1	12.0
% Positive return	66%	81%	77%
Mann-Whitney (*p*-value)		(0.024)	(0.29)
% "Fair" return (≥ 15)	27%	44%	41%
Mann-Whitney (*p*-value)		(0.12)	(0.97)
Effective return			
Pooled data (effective return)	6.2	11.6	11.3
Mann-Whitney (*p*-value)		(0.003)	(0.91)
Pooled data ("hot" state)	5.9	11.9	11.6
Mann-Whitney (*p*-value)		(0.003)	(0.88)
% Investors benefit from trade			
Pooled data	52%	67%	66%
Mann-Whitney (*p*-value)		(0.054)	(0.91)
Number of obs. (investors)	50	50	50
Number of obs. (allocators)	25	25	25

Econometric analysis.— While our previous findings highlight the benefits of coordinated punishment, there are some variables (e.g., the individual characteristics or the history of decisions) that could affect these results. 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.[27]

Our first specification for the likelihood of joint investment rests on the model of Arellano-Bond (1991). We believe that this is appropriate to our setting since we have a potential endogeneity problem (due to the partners matching) and we do not have exogenous variables to use as instruments; in fact, a test of

[27] The reason to focus on the intended return of allocators is twofold. First, this allows us to increase the number of observations. Second, this is the most conservative approach given the results in Table 2.

serial auto-correlation confirms that the endogeneity problem is present in our data, hence using a model that does not account for the endogeneity problem could lead us to overestimate or underestimate the coefficients of the regressors. The main advantage of this procedure is that the lagged endogenous and predetermined variables are used as instruments, taking the panel-data structure of the sample into consideration. In addition, we estimate a logit random-effects model with lagged dependent variables to show that our findings are robust to this specification.[28]

Table 3 summarizes the estimates 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*$_{t-1}$.[29] In one of our regressions, we consider the dummy variable *Joint 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 at most one of the investors punished). We interact this dummy with our treatment conditions to see whether joint investment in previous periods has a different affect depending on whether the punishment is uncoordinated or coordinated. Our specification allows for the possibility that allocators care about unfair outcomes. The variable *Payoff Difference*$_{-1}$ measures the difference between the allocator and the investors' payoffs in the previous period. In all the regressions, we control for the possibility that each treatment exhibits a different dynamics.[30] We account for the individual observed heterogeneity by including a subset of the variables of the questionnaire. The standard errors in the random-effects logit specification are robust and clustered at the group level.

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[28] The Hansen test suggests that the instruments we use in each case are valid for joint investment ($p > 0.191$) and the returned amount ($p > 0.218$). See Roodman (2006, 2009) for a discussion on how to select a valid set of instruments in the Arellano-Bond model. For other experimental papers that use this methodology see Fischbacher and Gächter (2010), Brañas-Garza et al. (2013) or Charness et al. (2017). We also estimate a logit model for the probability of individual investment (see Table D2 in Appendix D) and multinominal probit models for the number of investors who decide to invest (see Table D3 in Appendix D).

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

[30] 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). We take the different dynamics into account in our econometric by allowing the possibility that joint investment follows a quadratic (linear) specification in $UP_{30,60}$ and $CP_{0,60}$ ($CP_{0,80}$), respectively. As the Jonckheere-Terpstra test suggests that the intended return does not change over periods in any of the three treatments ($p > 0.174$), we will only allow for a linear specification in this case by including an independent variable for the period.

Table 3. Investors' decisions: Likelihood of joint investment using Arellano-Bond and random-effect logit

	$UP_{30,60}$ vs $CP_{0,60}$				$CP_{0,60}$ vs $CP_{0,80}$			
	Arellano-Bond		Random-effect logit		Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Joint investment $_{t-1}$	0.201***	0.061**	-0.301	-0.210	0.180***	0.253***	-0.0325	0.701*
	(0.036)	(0.024)	(0.673)	(0.414)	(0.016)	(0.022)	(0.567)	(0.421)
Joint investment $_{t-2}$	0.065***	0.093***	0.548*	0.601*	0.041***	0.042***	0.779*	0.800*
	(0.020)	(0.014)	(0.322)	(0.329)	(0.011)	(0.013)	(0.428)	(0.445)
Return $_{t-1}$ * Joint investment $_{t-1}$	0.016***	0.020***	0.171***	0.172***	0.010***	0.008***	0.144***	0.124***
	(0.002)	(0.001)	(0.052)	(0.041)	(0.001)	(0.001)	(0.040)	(0.028)
$UP_{30,60}$ * Joint Punishment $_{t-1}$	-0.838***		-1.026					
	(0.203)		(1.190)					
$CP_{0,60}$ * Joint Punishment $_{t-1}$	0.207***		0.414		0.020		0.279	
	(0.059)		(0.618)		(0.104)		(0.568)	
$CP_{0,80}$ * Joint Punishment $_{t-1}$					0.127**		1.147	
					(0.054)		(0.806)	
Payoff Difference $_{t-1}$		0.004		-0.012		-0.006***		-0.059
		(0.002)		(0.053)		(0.001)		(0.036)
$UP_{30,60}$ * Period	0.0003	0.033*	0.171	0.173				
	(0.018)	(0.018)	(0.340)	(0.327)				
$UP_{30,60}$ * Period ^{2}	-0.0004	-0.002**	-0.016	-0.0161				
	(0.001)	(0.001)	(0.019)	(0.0184)				
$CP_{0,60}$ * Period	-0.062***	-0.049***	-0.435**	-0.439**	0.089***	0.084***	-0.461**	-0.460**
	(0.008)	(0.011)	(0.194)	(0.197)	(0.017)	(0.012)	(0.193)	(0.190)
$CP_{0,60}$ * Period^2	0.003***	0.002***	0.014	0.0136	-0.006***	-0.006***	0.015	0.016
	(0.0004)	(0.0004)	(0.010)	(0.0101)	(0.001)	(0.001)	(0.010)	(0.010)
$CP_{0,80}$ * Period					-0.005*	-0.004*	-0.062*	-0.064*
					(0.003)	(0.002)	(0.035)	(0.035)
Coord. Punish ($CP_{0,60}$)	0.275***	0.400***	2.740**	2.880**				
	(0.066)	(0.086)	(1.389)	(1.350)				
Coord. Punish ($CP_{0,80}$)					0.369***	0.357***	1.827**	1.700**
					(0.073)	(0.058)	(0.859)	(0.845)
Constant	-0.131*	-0.298***	-4.638***	-4.493**	0.025	0.121	2.961**	3.455**
	(0.077)	(0.097)	(1.759)	(1.810)	(0.138)	(0.134)	(1.427)	(1.447)
Number of obs.	650	650	650	650	650	650	650	650

Notes. Significance at the *10%, **5%, ***1% level

Our treatment dummies confirm the positive effects of coordinated punishment on the levels of joint investment. Hence, the dummy variable for coordinated punishment $CP_{0,60}$ is always significant when comparing it to $UP_{30,60}$ in the Arellano-Bond ($p < 0.005$) and the random-effect logit ($p < 0.049$) specifications. The difference between $CP_{0,60}$ and $UP_{30,60}$ is also statistically significant at any common significance level in both specifications (Arellano-Bond: $p < 0.001$, random-effect logit: $p < 0.044$).

> **Result 1.** *Coordinated punishment fosters the level of joint investment compared with uncoordinated punishment; i.e., joint investment is more likely if investors need to coordinate their actions. The increasing returns to scale in coordinated punishment amplify its benefits and result key in fostering the levels of joint investment in the long-run* ($UP_{30,60} \leq CP_{0,60} < CP_{0,80}$)

In line with Cassar and Rigdon (2011), we also find evidence of "homegrown trusting preferences" in that investors are more likely to invest if they did so in the previous period. As predicted by our theory, investors care about the amount returned by the allocator; hence the amount received by investors in the previous period has a positive effect on the likelihood of joint investment. The results of the Arellano-Bond model suggest that the effect of both investors punishing in the past is negative (positive) when punishment is uncoordinated (coordinated), thus the use of punishment seems to facilitate the joint investment when punishment is coordinated

Table 4. Allocators' decisions: Intended return using Arellano-Bond and random-effect Tobit specifications

	$UP_{30,60}$ vs $CP_{0,60}$				$CP_{0,60}$ vs $CP_{0,80}$			
	Arellano-Bond		Random-effect logit		Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intended Return $_{t-1}$	0.036**	0.069***	0.135**	0.144**	0.184***	0.166***	0.182***	0.182***
	(0.014)	(0.011)	(0.064)	(0.0638)	(0.022)	(0.022)	(0.063)	(0.062)
Intended Return $_{t-2}$	0.075***	0.060***	0.204***	0.206***	0.075***	0.040*	0.146**	0.138**
	(0.020)	(0.016)	(0.064)	(0.0641)	(0.026)	(0.021)	(0.062)	(0.061)
$UP_{30,60}$ * Joint Investment $_{t-1}$	8.140***	0.651*	4.951**	1.383				
	(1.839)	(0.364)	(2.014)	(1.671)				
$CP_{0,60}$ * Joint Investment $_{t-1}$	1.580***	0.949***	-0.525	-0.622	0.305*	0.031	-0.862	-0.952
	(0.293)	(0.078)	(1.894)	(1.896)	(0.171)	(0.144)	(1.925)	(1.910)
$CP_{0,80}$ * Joint Investment $_{t-1}$					-0.259	-0.299	-0.187	-0.499
					(0.308)	(0.271)	(1.867)	(1.866)
$UP_{30,60}$ * Profit Reduction $_{t-1}$	-26.40***		-12.24**					
	(7.398)		(5.714)					
$CP_{0,60}$ * Profit Reduction $_{t-1}$	-5.884***		-1.504		-2.244***		-1.515	
	(0.482)		(2.946)		(0.436)		(2.996)	
$CP_{0,80}$ * Profit Reduction $_{t-1}$					5.246***		2.422	
					(1.304)		(1.843)	
Payoff Difference $_{t-1}$		0.045***		0.045***		-0.005		0.041
		(0.009)		(0.004)		(0.005)		(0.032)
Period	-0.018	0.045***	-0.035	-0.015	-0.031**	-0.056***	-0.175**	-0.166**
	(0.012)	(0.007)	(0.087)	(0.087)	(0.013)	(0.012)	(0.085)	(0.084)
Coord. Punish ($CP_{0,60}$)	3.932***	2.573***	3.720**	3.568**				
	(0.706)	(0.772)	(1.804)	(1.807)				
Coord. Punish ($CP_{0,80}$)					-1.668	-0.414	-0.203	-0.510
					(1.024)	(0.900)	(1.771)	(1.793)
Constant	4.351***	4.701***	3.505**	3.267**	10.04***	10.16***	8.611***	8.542***
	(1.217)	(0.948)	(1.582)	(1.582)	(3.039)	(3.054)	(1.781)	(1.780)
Number of obs.	650	650	650	650	650	650	650	650

Notes. Significance at the *10%, **5%, ***1% level

We look at the allocator's decision in Table 4. In this case, we consider the intended return of the allocator as our dependent variable, which we estimate using an Arellano-Bond and a random-effect Tobit specification. 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 include a dummy variable *Joint investment* $_{t-1}$ that takes into account whether or not there was a joint investment in the previous round; i.e., this variable takes the value 1 if the intended return was indeed received by the investors so that allocators can be assumed to take place in a "hot" state. 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 the difference in payoffs between the allocator and the average of the two investors within a group in the previous period. The rest of the covariates include dummy variables for the treatments where punishment is coordinated

($CP_{0,60}$ and $CP_{0,80}$), the dynamics of the returned amount in each of the treatments and controls for individual heterogeneity.

Our main result is that coordinated punishment boosts the returned amount. This is explained by the fact that investors need to coordinate their actions, rather than by the increasing returns to scale of coordinated punishment; i.e., the intended return is larger $CP_{0,60}$ than in $UP_{30,60}$ (Arellano-Bond: $p < 0.001$, Random-effect logit: $p < 0.048$) but it is indistinguishable in $CP_{0,60}$ and $CP_{0,80}$ (Arellano-Bond: $p > 0.103$, Random-effect logit: $p > 0.77$).

> **Result 2.** *Coordinated punishment boosts the reward set by allocators. In particular, allocators return more to investors when investors need to coordinate their actions. Increasing returns to scale in coordinated punishment does not affect the return of allocators* ($UP_{30,60} < CP_{0,60} = CP_{0,80}$)

We also observe evidence for "homegrown trustworthiness" in that allocators intend to return more in the current period if they had returned more in the previous periods (Cassar and Rigdon, 2011). The fact that investors received the return set by allocators in the previous round seems to encourage allocators to return more in the current one. Interestingly, we find that the reduction in the allocators' earnings in the previous period can have different effects on the intended return of allocators, depending on the treatment. While allocators may return less after being punished in the $UP_{30,60}$ or the $CP_{0,60}$ treatments, the effect in the $CP_{0,80}$ may be positive. In this regard, our findings relate to other papers that highlight that receiving punishment can have detrimental effects on behavior; e.g., by lowering contributions to the public good game (Casari and Luini, 2009) and seem to indicate that *the size of the stick* might be important to explain the return of allocators (Rigdon 2009, Calabuig et al. 2016). Finally, we find some support for the idea that allocators might be inequality-averse in that they return more if they were ahead in the previous period (see Ciriolo (2007), Smith (2011) or Bejarano et al. (2020) for related evidence that inequality in favor of allocators favor their level of reciprocity).

4.2. Punishment behavior and efficiency

So far, we have shown that coordinated punishment can facilitate investment decisions of investors and foster the return of allocators. It is well-documented in the literature that 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. In what follows we examine how investors punish in each treatment and the effect of their decisions on the final payoffs. In Section 5, we compare the levels of joint investment and the return of allocators in each of our treatment conditions ($UP_{30,60}$, $CP_{0,60}$ and $CP_{0,80}$) with their behavior when investors are not allowed to punish.

Punishment behavior.— Table 5 summarizes the punishment behavior of investors in each treatment when punishment is feasible. The composition of punishment within each group and the (punishment) trigger strategy that investors employ in each treatment are presented at the bottom of the table. As we discussed in Section 3.3, 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.[31] In Figure 4 we report the likelihood of observing joint investment in period t+1, depending on the investors' punishment decisions in period t.

Table 5. Punishment behavior of investors

	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$
% Individual punishment	36%	49%	48%
Test of proportion (p-value)		(0.088)	(0.92)
% Individual punish if "low" return (< 15)	46%	59%	73%
Test of proportion (p-value)		(0.090)	(0.14)
% Individual punish if "high" return (≥ 15)	15%	33%	27%
Test of proportion (p-value)		(0.017)	(0.51)
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%
Number of obs.	50	50	50

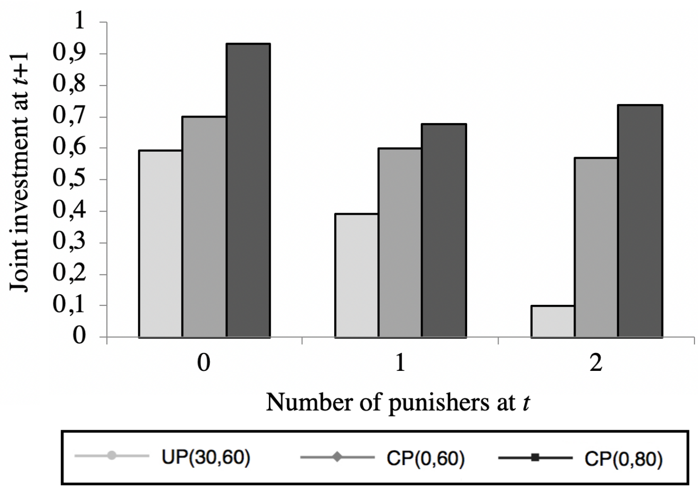

Figure 4. Likelihood of joint investment at t+1 depending on the number of punishers at t

[31] 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 D3).

In Table 5, 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.[32] 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%, 30% and 36% in $UP_{30,60}$, $CP_{0,60}$ and $CP_{0,80}$, respectively). Figure 4 shows that the likelihood of joint investment is always higher when punishment is coordinated (than when it is not), regardless of the number of investors who decided to punish in a previous period. Further, investors seem to follow different strategies in each of the treatments. In particular, investors are more likely to keep investing in the $UP_{30,60}$ treatment if neither punished in the previous period, compared with the case in which at least one of the investors punished; i.e., the number of investors who punished in the previous period seems to affect the likelihood of investing and investors barely invest if they both decided to punish in the previous period. In the $CP_{0,60}$ and the $CP_{0,80}$ treatments, investors keep investing if they did so in the previous period, even if they both decided to punish. We summarize these findings as follows:

> **Result 3.** *Joint punishment (trigger strategy) is used more frequently when punishment is coordinated. In these treatments, majority of investors decide to punish but keep investing in subsequent periods, while investors end up not investing (trigger strategy) when punishment is uncoordinated.*

We attempt to explain the determinants of punishment decisions by means of an econometric analysis in which the individual decision to punish is the dependent variable. Recall that investors can only punish if joint investment took place. Our first specification in Table 6 is a Tobit model where the dependent variable 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.[33] We constraint the analysis to the cases in which punishment was possible in the logit specifications, i.e., we have missing values of the dependent variable if there was no joint investment. Our final specification considers 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 joint 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. Table 6 reports the results for the likelihood of punishment (the selection model that estimates the probability of joint investment is presented in Table D4 in the Appendix D). The set of independent variables include the punishment decision in the last period, $Punish_{t-1}$ and the observed behavior from the other investor in the previous period, which we measure interacting the dummies for each treatment with $Other\ Punish_{t-1}$, which is a dummy variable that

[32] Our theoretical model predicts that investors will use the punishment (depending on the proportion of inequality-averse 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 $UP_{30,60}$, 73% in $CP_{0,60}$ and 83% in $CP_{0,80}$.

[33] Note that we obtain exactly the same results (except the coefficient of the constant) if we use a different categorization for the dependent variable (e.g., if the independent variable took the value 0 when there was no joint investment and the value 1 (2) when only one investor (both investors) punished).

takes the value 1 if the other investor punished in the previous period and 0 if she did not when punishment was feasible. In our theoretical model, investors are expected to punish if the return of the allocator is low, thus we include the explanatory variable *Received Return$_t$* to account for the return of allocators. Our findings are robust to other specifications; e.g., using the payoff difference between the allocator and the investor before punishment (see Calabuig et al. 2019) All of our regressions control for individual heterogeneity.

Table 6. Investors' punishment decisions: Econometric analysis

	$UP_{30,60}$ vs $CP_{0,60}$				$CP_{0,60}$ vs $CP_{0,80}$			
	Tobit	Tobit	Logit	Heckman	Tobit	Tobit	Logit	Heckman
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Punish $_{t-1}$	0.432***	-0.043	-0.290	0.0831	0.488***	-0.018	-0.093	0.148***
	(0.121)	(0.037)	(0.268)	(0.073)	(0.083)	(0.027)	(0.228)	(0.0494)
$UP_{30,60}$ * Other Punish $_{t-1}$	-0.170**	-0.049*	-0.356*	-0.086***				
	(0.074)	(0.026)	(0.182)	(0.028)				
$CP_{0,60}$ * Other Punish $_{t-1}$	0.013	0.008	0.057	0.022	-0.019	0.005	0.022	0.021
	(0.053)	(0.015)	(0.112)	(0.017)	(0.040)	(0.014)	(0.111)	(0.014)
$CP_{0,80}$ * Other Punish $_{t-1}$					0.030	0.029***	0.260***	0.050***
					(0.036)	(0.011)	(0.097)	(0.012)
Received Return $_t$		-0.072***	-0.490***	-0.069***		-0.057***	-0.448***	-0.060***
		(0.008)	(0.083)	(0.011)		(0.005)	(0.055)	(0.005)
Period	-.122***	-0.007	-0.053	0.001	-0.082***	-0.008*	-0.060	-0.005
	(0.017)	(0.007)	(0.049)	(0.010)	(0.011)	(0.004)	(0.040)	(0.005)
Coord. Punish ($CP_{0,60}$)	0.683**	0.229***	1.555**	0.265***				
	(0.341)	(0.083)	(0.606)	(0.095)				
Coord. Punish ($CP_{0,80}$)					0.466*	-0.024	-0.201	-0.058
					(0.262)	(0.066)	(0.556)	(0.060)
Constant	-3.782**	1.104***	5.186**	1.166***	-0.876	1.031***	4.674	0.833***
	(1.484)	(0.327)	(2.482)	(0.347)	(1.218)	(0.331)	(2.916)	(0.280)
Heterogeneity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	1,300	282	282	1,286	1,300	484	484	1,274

Our first specification indicates that investors are more likely to punish if they did so in the past. This finding can be related to the possibility that subjects have expressive preferences for punishment, as it is suggested in Casani and Luini (2012). However, this evidence of "homegrown punishing preferences" is not always robust; e.g., when we control for the return of allocators. This variable is always negative and significant suggesting that investors are less likely to punish when the return of allocators increases. Importantly, 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. The free-riding effect is not observed in the coordinated devices; in fact, our analysis for $CP_{0,80}$ suggests that there is a *solidarity effect* in that investors are more likely to punish if their partner did it in the past. These results provide evidence that investors seem to care about disadvantageous inequality and that the punishment decision of others affect the willingness to punish (see Houser and Xiao (2010) for evidence that inequality can affect the willingness to punish and Kamei (2014) for evidence that punishment decisions can be conditional on others' punishment decisions).[34]

> **Result 4.** *Investors are more likely to punish if punishment is coordinated. The likelihood of punishment depends also on the partner's decision in the previous period. In the $UP_{30,60}$ treatment, investors are less likely to punish if they observe that their partner punished in the previous period. (free-riding behavior). In the $UP_{0,80}$ treatment, investors are less likely to punish if they observe that their partner punished in the previous period (solidarity effect).*

Overall, these differences in punishing behavior help to explain why we fail to identify efficiency gains across treatments when we look at the final payoffs of investor and allocators across treatments. However, our analysis for efficiency in the next section shows that there are some benefits of coordinated punishment when we consider the initial endowments as a reference point in the analysis.

Efficiency.— Table 7 presents the average total payoffs in each group, disaggregated by roles (investors and allocators) and treatments, both before and after the punishment decision of investors.[35] This table presents also a measure of the difference between the average payoff of investors and the payoff of the allocator in each treatment.

Table 7. Average total payoffs before and after the punishment decision of investors

	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$
Average total payoffs (before punishment)	61.85	63.45	67.39
Average total payoffs (after punishment)	59.67	60.20	60.05
Average payoff of investors (before punishment)	19.21	19.47	20.24
Average payoff of investors (after punishment)	18.92	18.85	19.21
Average payoff of allocators (before punishment)	23.42	24.51	26.90
Average payoff of allocators (after punishment)	21.82	22.51	21.62
Difference in payoffs (before punishment)	4.20	5.03	6.65
Difference in payoffs (after punishment)	2.89	3.66	2.40
Number of obs. (investors)	50	50	50
Number of obs. (allocators)	25	25	25

[34] We provide additional regressions in Calabuig et al. (2019) where we do not control for the return of allocators but for the difference between the investor and the allocator's payoffs.

[35] We relegate to Appendix D the payoffs of investors and allocators across periods in each of the treatments (before and after punishment, see Figure D4). Appendix D presents also the histogram with the payoffs of investors and allocators in each treatment (see Figure D5).

We observe that the positive effects of more joint investment when punishment is coordinated materialize into higher total payoffs *before* the punishment decisions; in fact, when we look at the total sum of payoffs in each treatment, the Wilcoxon matched-pairs signed-ranks test indicates that this is above the initial sum of endowments (60 ECUs) in $CP_{0,60}$ ($p = 0.004$) and $CP_{0,80}$ ($p = 0.003$), but not in $UP_{30,60}$ ($p = 0.114$). There are also significant differences in total payoffs across treatments, using the Krusall-Wallis test ($p = 0.04$). As for the payoffs *after* the punishment decisions we find that there is a significant reduction in the total payoffs in all the treatments ($p < 0.01$), and the Krusall-Wallis test suggest that payoffs do not differ across treatments ($p = 0.68$). However, we observe that the total sum of payoffs is below the initial sum of endowments when punishment is uncoordinated ($p = 0.043$). The difference between the total sum of payoffs and the initial sum of endowments is not statistically significant if punishment is coordinated ($CP_{0,60}$: $p = 0.64$ and $CP_{0,80}$: $p = 0.68$). This, in turn, indicates that there are *no gains from trade* when punishment is coordinated in the $CP_{0,60}$ and $CP_{0,80}$ treatments, but the uncoordinated punishment (i.e., the $UP_{30,60}$ treatment) is detrimental for the level of efficiency, as the sum of final payoffs falls below the sum of the initial endowments. We summarize these findings as follows:

> **Result 5.** *The higher levels of joint investment lead to higher total payoffs when punishment is coordinated. If we compare the total payoffs before and after the punishment decisions, we find that there is surplus destruction in all treatments; in fact, there are efficiency losses (total payoffs fall below the initial sum of endowments) if punishment is uncoordinated, while there are no efficiency gains or losses (the sum of final payoffs is not significantly different from the sum of the initial endowments) if punishment is coordinated.*

If we look at the payoffs of investors and allocators across treatments, we observe that allocators are the one that benefit from the joint investment the most, as they receive more than investors in any of the treatments. In addition, the payoffs of allocators are always above their initial endowment (20 ECUs) both before ($p < 0.001$) and after ($p < 0.001$) the punishment decisions of investors, except in the $CP_{0,80}$ treatment ($p = 0.143$), where allocators do not receive on average more than their initial endowment once investors have punished. In this respect, our results suggest that allocators are the ones that are harmed from punishment the most in terms of payoffs differences when we look at their ex-ante and ex-post punishment payoffs, 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%). As for the payoffs of investors, we know that investors are more likely to benefit from trade when punishment is coordinated (see Table 2). However, the payoff of investors are (on average) below their initial endowment after their punishment decisions ($p < 0.013$), thus reducing the payoff difference comes at a high cost for investors in every treatment. The Krusall-Wallis tests suggests that the differences in payoffs (in favor of the allocator) is significant across treatments before the punishment decision of investors ($p = 0.07$) but not after these decisions ($p = 0.16$).

5. Should we allow for punishment at all?

There exists evidence that the possibility of punishment may be detrimental for behavior; e.g., it might have a crowding out effect on the intrinsic motivation of individuals (e.g., see Gneezy and Rey-Biel 2010 for a revision). In the dyadic version of the investment game, Rigdon (2009) finds that allowing for punishment might not help to increase the levels of investment, except if the fee-to-fine ratio or the investor's capacity of punishment is sufficiently high. Similarly, Fehr and Rockenback (2003) find that the possibility of imposing a fine on the allocators increase the desired-payback of investors but they do not invest more if they can impose the fine, compared with the case in which fines are not possible. As for the behavior of allocators, Fehr and Rockenbach (2003), Fehr and List (2004) and Houser et al. (2008) experimentally show that the possibility of punishment may backfire in that returns from allocators is lower if they can be punished. Calabuig et al. (2016) argue that when the investor has a high capacity of punishment (specifically when the investor's endowment is much larger than the one of the allocator), this encorages the investor to invest more because there is credible theat of punishment. However, the *big stick* does not facilitate return from allocators; in fact, the thereat of punishment increases the return of allocators only when the capacity of punishment of investors is sufficiently low so as not to destroy the intrinsic motivation to return.

To address these questions, we decided to conduct a second study at the LINEEX with a total of 45 subjects (i.e., 15 pairs). This treatment follows the same procedures as the previous ones, but we did not allow investors to punish after observing the return of the allocator. Our main findings are that allowing for punishment in the team investment game has negative effects on the levels of joint investment and the return of allocators if punishment is uncoordinated. On the contrary, these negative effects are not observed (and can even be positive when we examine the return of allocators) if punishment is coordinated (see further details of this analysis in online Appendix E).

6. Concluding remarks

Coordinated punishment is a prevalent phenomenon in the society. When lenders reclaim debt from a country that has defaulted on its obligation to repay the debt or when parents want to punish their children, they need to coordinate their actions and "punish together" for the punishment to be successful. In this paper, we look at the effects of coordinated punishment in an asymmetric situation that resembles the labor relationship. While there is mounting evidence on the effects of punishment in various settings, we are not aware of any paper that directly examines the benefits of coordinated punishment in an asymmetric situation such as the team investment game. Our paper is an attempt to fill this gap.

We posit a theoretical model that assumes that investors may need to coordinate their actions and punish together to inflict any damage on the allocator. In addition, we allow for coordinated punishment to exhibit returns to scale, compared with uncoordinated punishment. Our main theoretical result emphasizes that when the proportion of inequality-averse investors is high enough, coordinated punishment is better than uncoordinated punishment as the former device faciliates joint investment from investors and results in higher rewards from allocators. This is evidenced with a greater range of equilibria with joint investment under coordinated punishment, along with higher rewards set from allocators, even if the proportion of fair-minded allocators in the population is relatively low. The rationale for this result is that investors are more demanding (i.e., they are willing to punish the allocator for a wider range of returns) if punishment is coordinated, thus profit-maximiser allocators face a stronger punishment threat when punishment is coordinated. Moreover, the existence of increasing returns to scale in coordinated punishment can facilitate the joint investment of investors.

Our experimental evidence lends support for the idea 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 find significant differences in the levels of efficiency across treatments when we look at the payoffs before punishment. These gains dissapear after the punsihment decision of investors. 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 periods. When punishment is uncoordinated, a substantial proportion of investors refrain from investing after a few periods, while investors keep investing and punishing in group when punishment is coordinated.

In our paper, we also compare the results of our study to a setting in which punishment is not possible. In this respect, our findings seem to indicate that the negative effects of punishment that have been identified in the dyadic version of investment game (Fehr and Rockenbach 2003, Fehr and List 2004, Houser et al. 2008, Calabuig et al. 2016) are also present in our team investment game when punishment is uncoordinated. These negative effects tend to vanish when punishment is coordinated.

Our paper might be viewed as a first attempt to show the benefits of coordinated punishment in an asymmetric situation when players have different roles and opportunities to punish. One importat question to be addressed concerns how (and why) coordinated punishment emerges in a society or in a group. This is a relevant question as some authors have shown that it may be difficult to explain the evolution of cooperation from an evolutionary perspective when punishment is uncoordinated (Boyd and Richerson, 1988; Guala, 2012). We argue that punishment decisions may be considered as a public good. In that regard, coordinated punishment, as opposed to uncoordinated punishment, has the great advantage of eliminating the free-riding behavior of punishers. This idea paves the way to rationalizing the benefits of coordinated punishment in a team hold up relationship.

We acknowledge that our design is simple but our aim was to highlight the benefits of coordinated punishment in an asymmetric setting with few investors. 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. We believe that an interesting extension would be considering a continuum of investors. This would require using other analytical tools but the results of this model could shed light on the succed of mobilizations and protests that may require for punishment to be coordinated; e.g., the social upheavals in Chile or the Arab Spring, among others.[36] There are other aspects of our design that can be extended as well. For example, we deliberately focus on the case in which investment is a binary decision and the returns of the allocator are equally split between the investors. We have in mind a labor setting in which investors (or workers) choose whether or not to exert effort in a common project that requires complementarities and their effort is equally valuable for the allocator (i.e., the firm). A natural extension would be to consider a setting in which investors can choose different investments and the allocator is allowed to reward them differently (Cassar and Rigdon, 2011). It may also be possible to consider a setting in which the creation of the surplus does not require for the joint investment, but it suffices that one investor decides to invest to generate efficiency gains. Finally, we could also consider the possibility of coomunication (Charness and Dufwenberg 2006, Choi and Lee 2014) in that punishment is sometimes "coordinated by means of gossips and other communications" (Boyd et al. 2010, Fehr and Williams 2013).

Regarding the policy implications of our study, it seems reasonable to wonder whether people would opt for an institution with coordinated punishment if they had the opportunity to choose. 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 research. In that regard, it may be worth considering a setting where subjects can endogenously decide the punishment institution they want to implement, if any (Kosfeld et al. 2009; Fehr and Williams, 2013). This may help us in explaining the (natural) emergence of institutions where punishment has to be inflicted by the group, such as trade unions or partnerships. We believe these possible extensions reflect important situations that have not been studied so far, so we hope our research sparks further interest in these areas.

[36] Noticeably, extending the results to these settings may also result considering a different game; e.g., revolutions has been modeled as coordination or stag-hunt games in De Mesquita (2010), Edmond (2013), Kiss et al. (2017) or Barbera and Jackson (2020), among others.

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SUPPLEMENTARY MATERIAL FOR ONLINE PUBLICATION

United we stand:
On the benefits of coordinated punishment

Contents

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

We solve our model by backward induction and consider the punishment stage first. For simplicity, we omit the subscript for the proportion of inequality averse investors (q) and the proportion of fair-minded allocators (m)

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

We denote μ the probability of facing an inequality averse type of investor after observing joint investment, that is, μ := Prob ($\tau^i = a \mid 1_J = 1$) where $\tau^i = a$ stands for the inequality averse type and the indicator function 1_J 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 inequality averse (selfish) type of investor, respectively. Thus, the profile (p, np) indicates that the inequality averse 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 inequality averse 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 **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 **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 **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 $\mathbf{UP_{30,60}}$ treatment. Suppose that the allocator offers $x < 15$. Using the utility function of inequality averse investors (equation (4) in the main text), it follows that *p* is a best response for inequality averse 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 inequality averse 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 inequality averse investors will accept the return without punishing the allocator.

b) Next, we look at the optimal behavior of investors in the $\mathbf{CP_{0,60}}$ treatment. As the previous case, we can check that *p* is a best response for inequality averse 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 inequality averse investors $\forall\mu$.

c) Finally, p is a best response for inequality averse investors to (p,np) in the $\mathbf{CP_{0,80}}$ 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 inequality averse 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 an inequality averse investor, μ := Prob ($\tau^i = a \mid 1_J = 1$).

Lemma 2.

a) *In the* $\mathbf{UP_{30,60}}$ *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* $\mathbf{CP_{0,60}}$ *treatment, the selfish allocator will return:*

$x_s = 0$ *if* $\mu < 0.278$,

$x_s = ((18\mu\text{-}5)/(0.6\mu))$ *if* $\mu \in [0.278,0.306]$,

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

$x_s = ((30\mu\text{-}5)/(2\mu))$ *if* $\mu \geq 0.6$

c) *In the* $\mathbf{CP_{0,80}}$ *treatment, the selfish allocator will return:*

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

$x_s = ((24\mu\text{-}5)/(0.8\mu))$ *if* $\mu \in [0.2083, 0.223]$,

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

$x_s = ((30\mu\text{-}5)/(2\mu))$ *if* $\mu \geq 0.5$.

Proof.

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

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

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 50-30μ. 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 $\mu=\mu^*(x)=((0.3x\text{-}4)/(12\text{-}1.4x))$, 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) $\mathbf{CP_{0,60}}$ 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 inequality averse 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 $\mathbf{CP_{0,80}}$ 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 $\mu \le \mu(x) = 5/(24\text{-}0.8x)$, there will be no punishment even when the allocator returns nothing $x=0$ (if $\mu \le \mu(0)=0.2083$). This is because the proportion of inequality averse investors is so small that nobody is going to punish. However, when the allocator offers $x < 5$ but $\mu > \mu(x) = 5/(24\text{-}0.8x)$, there will be punishment and the expected payoff of the selfish allocator will be $\mu^2((0.2)(50\text{-}x)+2\mu(1\text{-}\mu)(50\text{-}x)+(1\text{-}\mu)^2(50\text{-}x) = (50\text{-}x)(1\text{-}0.8\mu^2)$. In this case, the best option is to offer $x = 0$ and the payoff for the allocator is $50\text{-}40\mu^2$.

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

Comparing the above expressions we obtain that exists a critical value of μ resulting from the cubic equation $32\mu^3\text{-}24\mu+5=0$. 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 $\mu \le \mu(x) = 5/(30\text{-}2x)$, there will be no punishment (see Lemma 1c). However, if the allocator offers $x \in (5, 12.5)$ but $\mu > \mu(x) = 5/(30\text{-}2x)$, there will be punishment with an expected payoff of $(50\text{-}x)(1\text{-}0.8\mu^2)$. And, in this case, the best option is to offer $x = 0$ with an expected payoff for the allocator of $50\text{-}40\mu^2$. As we know, the allocator can offer a reward of $x > 0$ so as to avoid punishment. This reward is $x^* = ((30\mu\text{-}5)/(2\mu))$ and yields a payoff for the allocator equals to $(50\text{-}\ x^*)$.

Comparing these expressions we obtain that exists a critical value of μ resulting from another cubic equation $16\mu^3\text{-}6\mu+1 = 0$. 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 $50\text{-}12.5=37.5$.■

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 inequality averse investors choose to invest in Stage 1. We denote $\bar{q}$ and $\bar{m}(q)$ the minimum proportion of inequality averse investors and fair-minded allocators for the existence of the equilibria. In the equilibria, $q = \mu =$ Prob ($\tau^i = a \mid 1_I = 1$).

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* $q \geq \bar{q} = 0.3464$ *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 inequality averse investors do not punish them. This optimal return* $x^*(q) = ((12q+4)/(0.3+1.4q)) < 12.5$ *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* $q \geq \bar{q} = 0.6$ *[*$q \geq \bar{q} = 0.5$*] 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 inequality averse investors do not punish them. This optimal return* $x^*(q) = ((30q-5)/(2q)) < 12.5$ *is increasing in q.*

Proof.

a) $\mathbf{UP_{30,60}}$ treatment. For the inequality averse 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 $\forall m$.

b) $\mathbf{CP_{0,60}}$ treatment. In this case, it is worth investing for an inequality averse 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 inequality averse 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 $\forall m$.

c) $\mathbf{CP_{\{0,80\}}}$ **treatment**. As above, the inequality averse 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 inequality averse 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 $\forall m$.■

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 inequality averse investors do not (see Lemma 6). Finally, there is an inefficient equilibrium without investment in each of the treatments (see Lemma 7).

<u>**Inefficient Pooling equilibrium with punishment**</u>

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 $x = 15$. Inequality averse investors punish the reward of selfish allocators.*

Proof.

The inequality averse 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 $x = 15$. Inequality averse punish the reward of selfish allocators.*

Proof.

The inequality averse 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. Inequality averse investors punish the reward of selfish allocators.

Proof.

The inequality averse 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.■

<u>Separating Equilibrium without punishment</u>

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 inequality averse 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 inequality averse 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 inequality averse 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 inequality averse investors in the population. In particular, q has to be smaller than 1/3 for the existence of this equilibrium.■

<u>The Non-Cooperative Equilibrium: equilibrium without investment</u>

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 paragraph 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 paragraph 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.

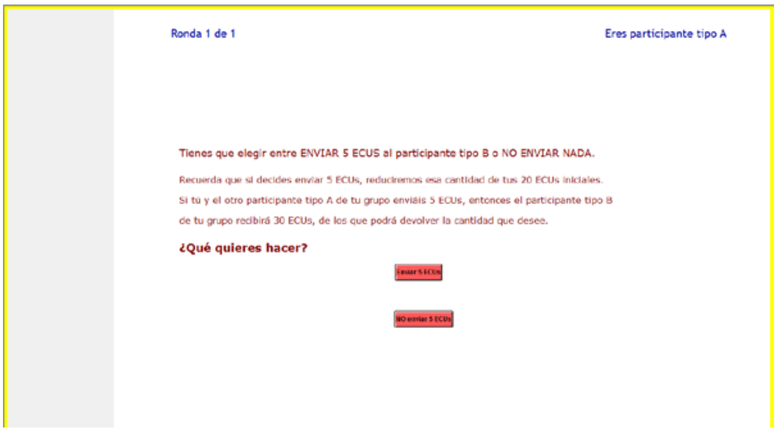
At the end of each round, we will inform you of the decisions of the members of your group. At the end of the experiment, we will pay your decisions for one round, chosen randomly. As in the previous case, we will convert your earnings in ECUs to Euros, using the rate of 3 ECUs = 1 €. You will receive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

Appendix B2. Screenshots for the experiment

In this section, we show the original screenshots of the experiment. The translation appears in the text below each of the figures.

DECISION TO INVEST AND RETURN

Decision to invest (investors)

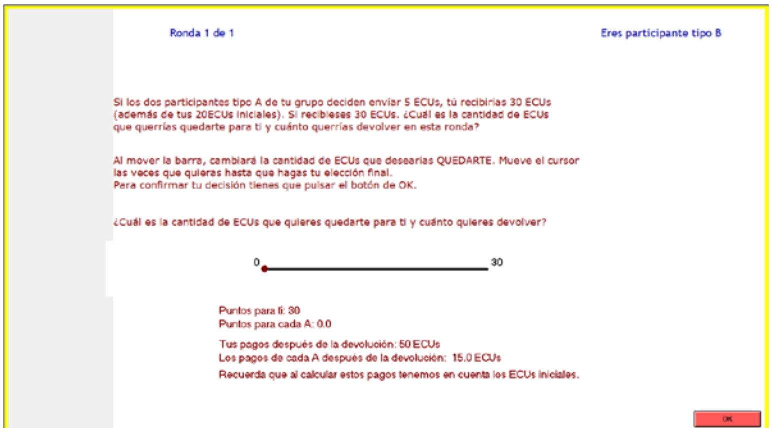


You have to choose between SENDING 5 ECUS to the type-B participant or SENDING NOTHING.

Remember that if you decide to send 5 ECUS, we will deduct this amount from your initial 20 ECUs. If you and the other participant A from your group decide to send 5 ECUs, then the participant B from your group will receive 30 ECUs, and he could return the amount that he/she wishes.

What do you want to do?

Decision to return (allocators)



If both type-A participants in your group decide to send 5 ECUS, you will receive 30 ECUs (apart from your initial 20 ECUs). If you received 30 ECUs, What is the amount you would like to keep for yourself and the amount you would like to return in this round?

When you move the bar, the amount of ECUs you would like to KEEP will change. Move the cursor as many times as you want until you reach a final decision. To confirm your decision you have to click the bottom “OK”.

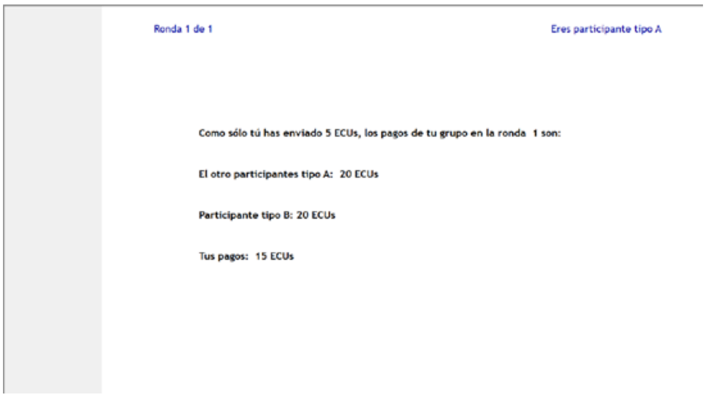
Slider here

What is the quantity in ECUs you want to keep and to return?

Points for you:
Points for each type-A participant:
Your payoff after the return:
A’s individual payoff after the return:
Remember that we have computed your payoff including your initial ECUs.

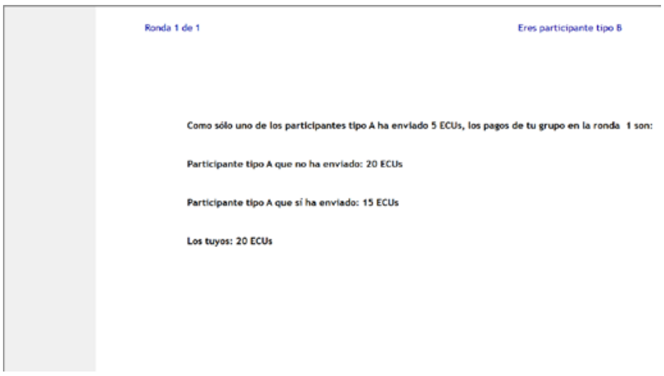
FEEDBACK WHEN THERE IS NO JOINT INVESTMENT

Feedback for investors (if only one invests)



As you are the only one who decided to send 5 ECUs, your group’s payoff in round 1 are:
The other type-A participant: 20 ECUs
Type-B participant: 20 ECUs
Your payoff: 15 ECUs

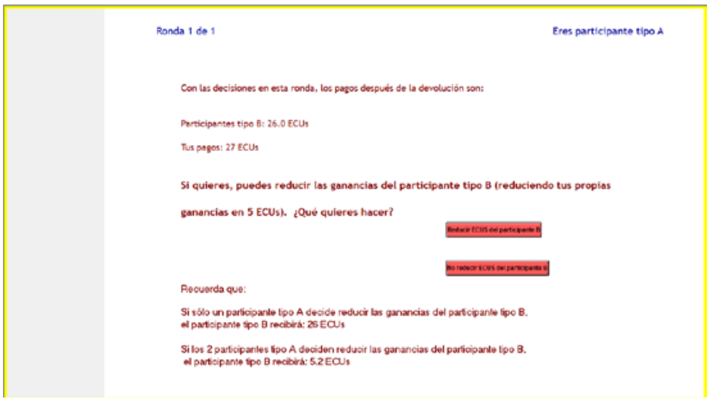
Feedback for the allocator (if only one invests)



As only one type-A participant decided to send 5 ECUs, your group’s payoff in round 1 are:
Type-A participant who did not send anything: 20 ECUs ;
Type-A participant A who sent: 15 ECUs;
Your payoff: 20 ECUs

DECISION TO PUNISH (COORDINATED PUNISHMENT, $CP_{0,80}$)

Decision to punish (investors)



After the decisions in this round, the payoffs after the return are:
Type-B participant: 26.0 ECUs
Your payoffs: 27 ECUs
If you want, you can reduce the type-B participant's earnings (reducing your own earnings by 5 ECUs).

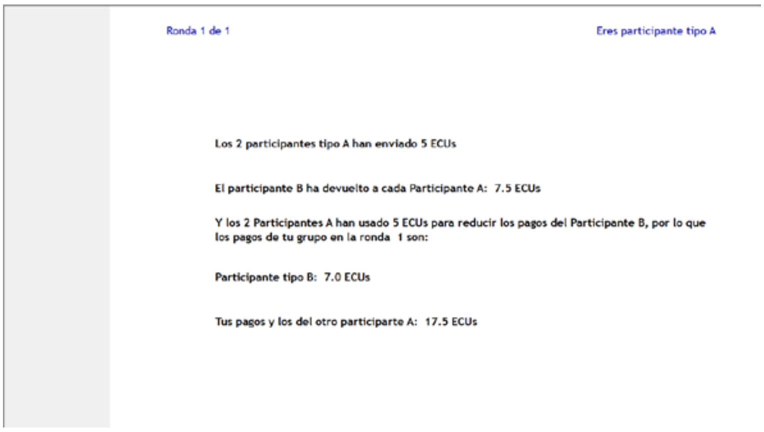
What do you want to do?

( ) Reduce ECUs of Participant B
( ) Not to reduce ECUs of Participant B

Recall that:
If only one type-A participant decides to reduce Participant B's earnings, then Participant B will earn: 26 ECUs
If both type-A participants decide to reduce Participant B's earnings, then Participant B will earn: 5.2 ECUs

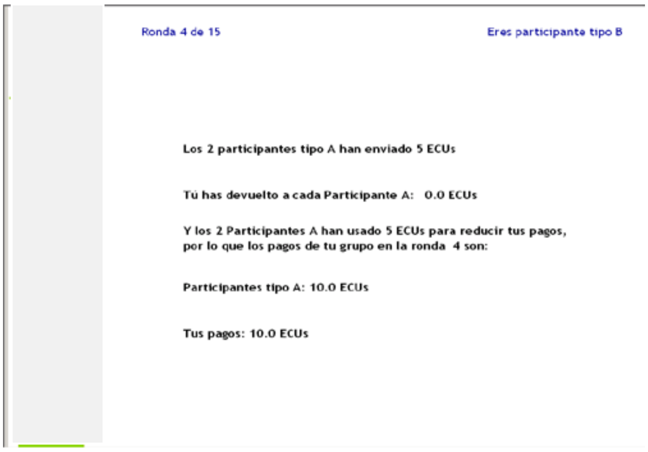
FEEDBACK WHEN THERE IS PUNISHMENT (COORDINATED $CP_{0,80}$)

Feedback for investors (after punishing)



Both type-A participants sent 5 ECUs.
Type-B participant returned to each type-A participant: 7.5 ECUs
Both type-A participants used 5 ECUs to reduce Participant B's earnings, so your group's payoffs in round 1 are:
Type-B participant: 7 ECUs
Your payoffs and those of the other type-A participant: 17.5 ECUs

Feedback for the allocator (after being punished)



Both type-A participants sent 5 ECUs.
You returned to each type-A participant: 0.0 ECUs
Both type-A participants used 5 ECUs to reduce your earnings, so your group's payoffs in round 4 are:
Type-A participants: 10.0 ECUs
Your payoffs: 10.0 ECUs

Appendix B3. 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 (investors)	50	50	50
N (allocators)	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 practice round 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

	Practice round			**Repeated game: Period 1**			**Repeated game: Periods 1-15**		
	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$
% 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 ($p > 0.317$).[1] 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.[2]

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).

[1] 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.

[2] 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 ($UP_{30,60}$ $p = 0.351$; $CP_{0,60}$ $p = 0.976$; $CP_{0,80}$, $p = 0.058$)

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

Joint investment and group heterogeneity. A data analysis at the group level gives additional insight into the dynamics of the joint investment. 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 D1 presents the distribution of groups in each treatment.[3] This includes the dynamics of groups across treatments (i.e., the frequency of groups that decided (not) to invest after (not) investing in the previous period) and the likelihood of observing no joint investment.

Table D1. Joint investment: Group heterogeneity and dynamic across treatments

	$\mathbf{UP_{30,60}}$	$\mathbf{CP_{0,60}}$	$\mathbf{CP_{0,80}}$
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%
% 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%
% No joint investment	56%	42%	36%

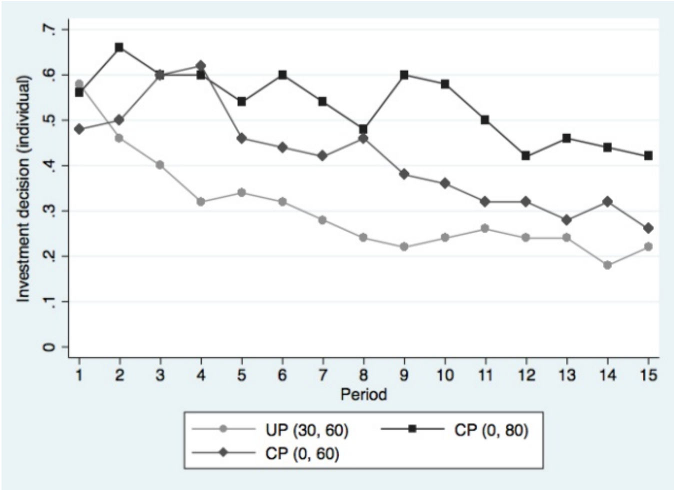
Note. Figures in the classification of groups are rounded to add up to 100%.

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). There is also evidence that joint investment in a previous round facilitates joint investment in the current one when punishment is coordinated.

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 3b in the main text) ($p < 0.014$ 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 3a). In fact, the vast majority of the low-investing groups (80%) received less than half of the fair amount in the first period.

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

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 ($p < 0.001$). The correlation coefficient for joint investment and period is -0.12, -0.19 and -0.03 for $UP_{0,30}$, $CP_{0,60}$ and $CP_{0,80}$ (if we omit period 15, the correlation coefficients are -0.09, -0.17 and 0.00, respectively).

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 D2, 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 D2. 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)			
Heterogeneity	No	Yes	Yes	Yes	Yes
Observations	1,950	1,950	650	650	650

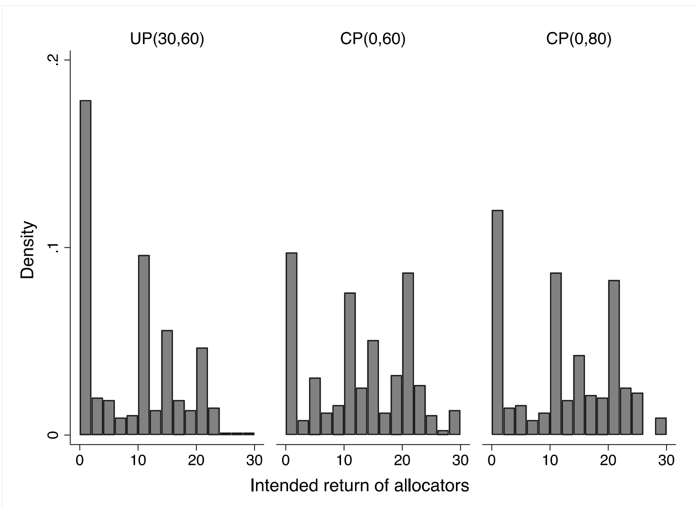
Table D3. Multinomial probit model for the number of investors who decide to invest

	$UP_{30,60}$ vs $CP_{0,60}$				$CP_{0,60}$ vs $CP_{0,80}$			
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Number of investors $_{t-1}$	1.241***	1.294***	1.421***	1.447***	1.104***	1.506***	0.923***	1.406***
	(0.193)	(0.217)	(0.195)	(0.220)	(0.219)	(0.238)	(0.204)	(0.216)
Number of investors $_{t-2}$	0.990***	1.027***	0.997***	1.065***	0.779***	0.936***	0.785***	0.943***
	(0.149)	(0.170)	(0.151)	(0.171)	(0.154)	(0.171)	(0.154)	(0.177)
Return $_{t-1}$ * Joint investment $_{t-1}$	-0.060**	0.038	-0.009	0.093***	-0.024	0.048**	-0.009	0.067***
	(0.028)	(0.027)	(0.035)	(0.035)	(0.023)	(0.021)	(0.024)	(0.023)
$UP_{30,60}$ * Joint Punishment $_{t-1}$	-0.206	-1.453						
	(0.661)	(0.885)						
$CP_{0,60}$ * Joint Punishment $_{t-1}$	0.135	0.221			0.082	-0.054		
	(0.691)	(0.656)			(0.712)	(0.687)		
$CP_{0,80}$ * Joint Punishment $_{t-1}$					-1.223**	-0.754		
					(0.486)	(0.460)		
Payoff Difference $_{t-1}$			-0.150***	-0.161***			-0.029	-0.067*
			(0.052)	(0.051)			(0.041)	(0.040)
$UP_{30,60}$ * Period	-0.128	0.150	-0.108	0.167				
	(0.184)	(0.232)	(0.186)	(0.233)				
$UP_{30,60}$ * Period ^{2}	0.008	-0.010	0.007	-0.010				
	(0.010)	(0.013)	(0.010)	(0.013)				
$CP_{0,60}$ * Period	-0.101	-0.342*	-0.098	-0.351*	-0.142	-0.383*	-0.135	-0.389*
	(0.175)	(0.200)	(0.172)	(0.194)	(0.174)	(0.213)	(0.166)	(0.201)
$CP_{0,60}$ * Period^2	0.007	0.014	0.006	0.014	0.008	0.015	0.007	0.015
	(0.010)	(0.011)	(0.009)	(0.011)	(0.009)	(0.012)	(0.009)	(0.011)
$CP_{0,80}$ * Period					-0.037	-0.032	-0.044	-0.037
					(0.039)	(0.039)	(0.041)	(0.042)
Coord. Punish ($CP_{0,60}$)	0.042	2.139*	0.187	2.448**				
	(1.049)	(1.225)	(1.038)	(1.199)				
Coord. Punish ($CP_{0,80}$)					-0.545	-1.515*	-0.669	-1.677*
					(0.821)	(0.913)	(0.786)	(0.862)
Constant	-2.907***	-4.992***	-1.532	-3.547***	-1.534	0.057	-1.201	0.891
	(0.933)	(1.067)	(1.058)	(1.191)	(1.016)	(1.408)	(1.072)	(1.440)
Observations	650	650	650	650	650	650	650	650

Our analysis in Table D3 differs from previous analysis in that we consider the number of investors who decide to invest (0, 1, 2) as the dependent variable.[4] We then report the results of a multinominal probit model which is appropriate to our setting given the nature of this variable; note that this cannot be said to be an ordinal variable because there is surplus creation only when both investors decide to invest and having one investor who decides to invest will lead to inefficient investment because this investor has to pay for the cost of investment (5 ECUs) but no surplus will be created. As a result, "no investment at all" is "better" than "one investor" in terms of efficiency. Our estimates in Table D3 consider the baseline model is the case of no investors. The model for only one investor (1) should then be interpreted as the likelihood of inefficient investment, while the model for two investors (2) predicts the frequency of joint investment. We consider two different regressions depending on whether we consider the punishment decisions in the previous period or the difference in payoffs in the previous round as explanatory variable. In all the regressions we control for individual heterogeneity (age, gender, risk aversion, attitudinal trust, etc…)

Our results indicate that the number of investors who invested in the previous periods influence the number of investors who decide invest in the current one, in line with our description of the data. When we look at the results of (1) we find that there are no significant differences in the level of inefficient investment across treatments. In line with our results in the paper, the results of (2) indicate that joint investment is more likely in $CP_{0,60}$ than in $UP_{30,60}$, and it is also more likely in $CP_{0,80}$ than in $CP_{0,60}$, thereby suggesting the benefits of coordinated punishment on the levels of joint investment.

Figure D2. Intended return of allocators in each treatment



[4] We are thankful to one of the referees for suggesting this analysis.

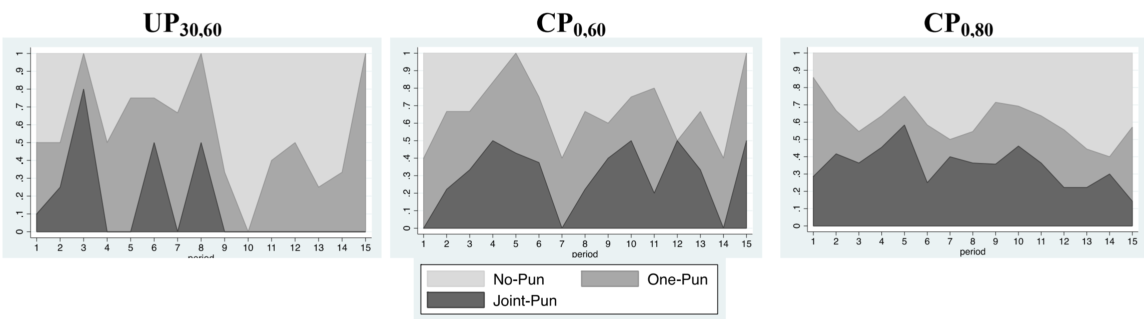
Figure D2 presents the histogram for the intended return of allocators in each treatment. The "spikes" in the data suggest that some of the allocators are purely selfishly as they decided to return nothing to investors, regardless of the possibility of being punished. In line with our description results in the paper, the null return is more likely to occur when punishment is uncoordinated.

If allocators return a positive amount, we observe that they are heterogenous in their fairness views, as some allocators return 20 ECUs (i.e., each investor receives 10 ECUs). This, in turn, implies that some of the allocators tend to divide the surplus (30 ECUs) equally. There is also a group of allocators who return 10 ECUs, what implies that investors retrieve their invested amount (but do not gain from trade). Finally, there are choices that correspond to allocators who are fair-minded (according to our definition) as they return 15 ECUs.

Importantly, our theory relies on the assumption that a proportion of allocators will be fair-minded returning x = 15, but we do not expect to observe this behavior from all allocators; in fact, our theory predicts that there will be (selfish) allocators who return a positive amount (in between 10 and 12.5 ECUs) so as to avoid being punished (see Figure 2 in the paper). At any event, the fact that we observe peaks in the data in 10 ECUs, 15 ECUs, and 20 ECUs make us confident that having x = 15 as a reference point for the "fair" return is a good proxy for the "average" fair return.

In this regard, one may wonder what is the "observed" average return across treatments for high-investing groups as defined in Table D1. Our data suggest that 34% of high-investing groups in $CP_{0,80}$ converge to a returned amount between 14 and 15 (as in our main assumption of the model) while 66% converge to a return of 20-24 ECUs. This is quite similar in CP0,60 where there was a 33% of groups converging to 13 and 66% to 20-30. Nevertheless, the convergence to the returned amount in the uncoordinated condition was quite different: 67% to 15 and 33% to 16-17.

Figure D3. Punishment decisions across rounds (conditional on punishment being feasible).

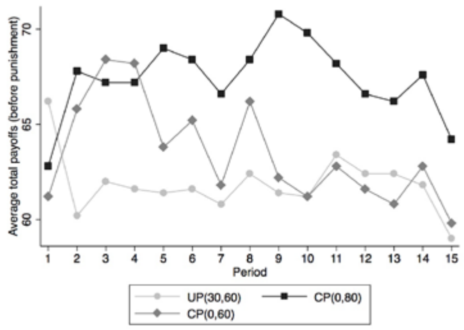


We observe that joint punishment is not used after round 9 in the case of uncoordinated punishment (**$UP_{30,60}$**). This is in sharp contrast with the observed behavior when punishment is coordinated, where joint punishment remains steady around 30%, especially in $CP_{0,80}$.

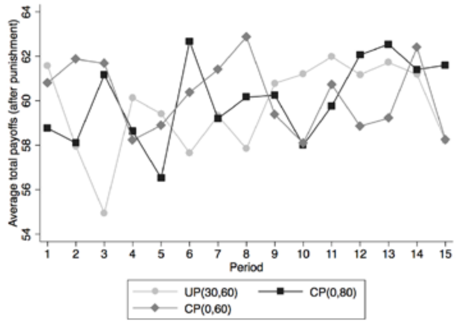
Total payoffs before (Panel a) and after (Panel b) the punishment decision of investors across rounds are presented in Figure D4. We observe that the payoffs in Panel a) mimic the behavior of investors for the case of joint investment (Figure 4 in the main text). When investors punish, there is a decrease 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.

Figure D4. Average total payoffs across rounds

(a) Total payoffs before the punishment

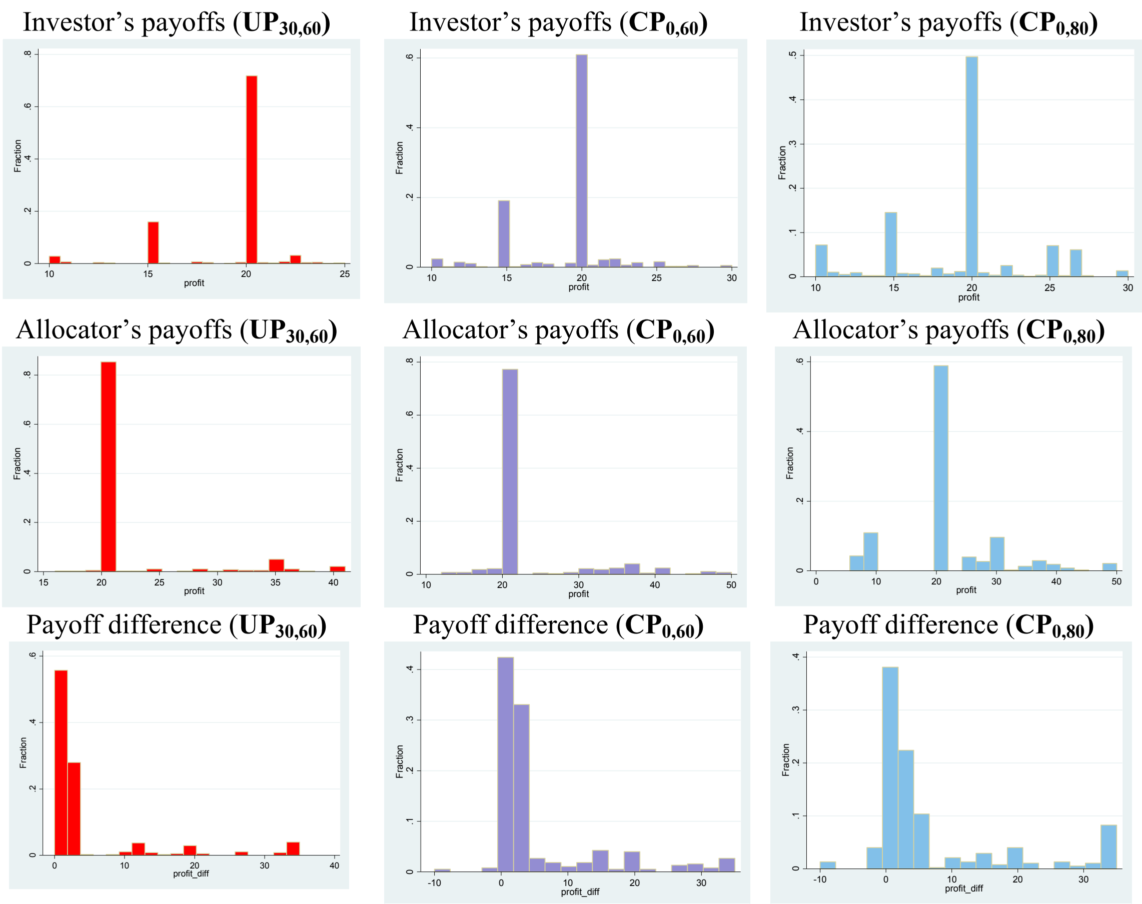


(b) Total payoffs after the punishment



We present the histogram with the payoffs of investors and allocators in each of the treatments in Figure D5. This includes the difference between the investor and the allocator's payoffs in each treatment.

Figure D5. Payoff of investor and allocators in each treatment



The peaks in 20 ECUs correspond to the initial endowments of the investor and the allocator; i.e., in theory, these peaks can account for *i*) no investment decisions, *ii*) for the case in which there is joint investment and the allocator returns investors their investment, or *iii*) there is joint investment and the allocator returns all the surplus to the investors (what never occurs in our data).

We rely on the Heckman's sample-selection model to explain the determinants of punishment decisions. Recall that this is a two-step method in which we first need to estimate the probability that punishment is possible; i.e., the selection model in Table D4 estimates the probability of joint investment using the procedures in Heckman (1979).

Table D4. Heckman selection model for punishment decisions

	$UP_{30,60}$ vs $CP_{0,60}$	$CP_{0,60}$ vs $CP_{0,80}$
	Heckman selection	Heckman selection
Coord. Punish ($CP_{0,60}$)	1.113***	
	(0.316)	
Coord. Punish ($CP_{0,80}$)		-0.156
		(0.247)
Return $_{t-1}$ * Joint investment $_{t-1}$	0.340***	0.291***
	(0.019)	(0.014)
$UP_{30,60}$ * Period	0.023	
	(0.025)	
$CP_{0,60}$ * Period	-0.084***	-0.074***
	(0.0234)	(0.021)
$CP_{0,80}$ * Period		0.004
		(0.017)
Constant	-3.527***	-0.504
	(0.649)	(0.615)
Heterogeneity	Yes	Yes
Observations	1,286	1,274

Notes. Significance at the *10%, **5%, ***1% level

Our results are in line with our findings for the probability of joint investment with the exception of $CP_{0,80}$ which is not significant when we compare the probability that investors can punish in $CP_{0,60}$ and $CP_{0,80}$. This may occur because our sample-selection model does not control for some variables that are important in explaining the probability of joint investment; i.e., the possibility of punishment such as the probability of joint investment in the previous rounds. Our model does not converge when we include these independent variables in the analysis. At any event, the results in Table D4 support our findings that the possibility of joint investment (i.e., of punishment) is more likely in $CP_{0,60}$ compared with $UP_{30,60}$. The return of allocators in the previous rounds is a key driver to explain the possibility of joint investment (and punishment) in the current round. There is also evidence that there are different dynamics across treatments.

Appendix E. Results of Study II (No punishment treatment).

Figure E1 presents the dynamic of joint investment and the intended return of allocators in NP treatment using a dashed line; the rest of the treatments in solid lines are presented for the sake of completeness. We observe that the levels of joint investment in the No Punishment treatment (NP hereafter) are around 30-50% (on average, the likelihood of joint investment is 39%). The dynamic of the joint investment in NP resembles the one we observed for the $CP_{0,80}$ treatment, including the end-period effect. As for the intended return of allocators, this is also quite stable across rounds except for the end-period effect, which seems to be more pronounced here than in our previous treatments with punishment; i.e., it seems like no having the threat of punishment encourages allocators to keep most of the surplus in the last period.

(a) Frequency of joint investment (b) Intended return

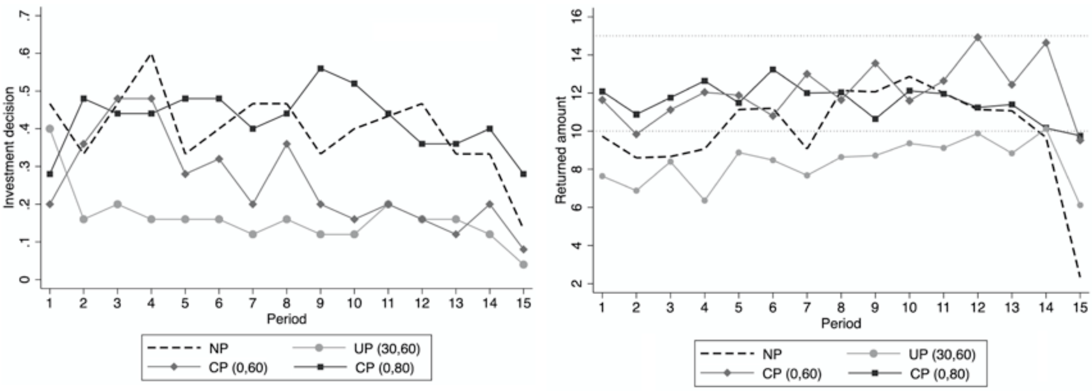


Figure E1. Relative frequency of joint investment and intended return across periods

When we compare the joint investment in NP with the one we observed in previous treatments, we observe that this is significantly higher in NP than in $UP_{30,60}$ (39% vs 16%, p = 0.003). There are also differences in the likelihood of joint investment in NP and $CP_{0,60}$ (39% vs 25%, p = 0.063), mainly because of the decreasing trend of the levels of joint investment in the $CP_{0,60}$ in the last part of the experiment. However, there are no differences in the joint investment between the likelihood of joint investment in NP and $CP_{0,80}$ (39% vs 42%, p = 0.97). The differences in the levels of investment across treatments can also be noticed in the different dynamics across treatments. Recall that after investing, the proportion of groups that kept investing in the subsequent period was 43% in $UP_{30,60}$, 62% in $CP_{0,60}$, and 80% in $CP_{0,80}$. In the NP treatment this proportion is 69%, thus there are significant differences when we compare NP and $UP_{30,60}$ (p = 0.009) but differences are not significant for any of the treatments with coordinated punishment $CP_{0,60}$ (p = 0.48) and $CP_{0,80}$ (p =

0.23).[5] In order to see the effects of punishment on joint investment, we have conducted an econometric analysis analogous to the one conducted in the previous section, having the NP treatment as the baseline and including dummies for each of the treatments in separate regressions (see Appendix E). Our findings indicate that there are no differences between NP and $CP_{0,80}$ but $CP_{0,60}$ and $UP_{30,60}$ are harmful for joint investment compared to NP (see Tables E1-E3).
As for the behavior of allocators, we observe that their intended return in the NP treatment is on average 9.90 ECUs. This lies in between the intended return of allocators in the $UP_{30,60}$ treatment (8.3 ECUs) and the intended return of allocators in $CP_{0,60}$ (12.1 ECUs) and $CP_{0,80}$ treatments (11.6 ECUs). While we focus on the intended return in the analysis, the same pattern is observed when we look at the *effective* return of allocators (6.2 ECUs in $UP_{30,60}$, 8.98 ECUs in NP, 11.6 ECUs in $CP_{0,60}$, and 11.3 ECUs in $CP_{0,80}$) or their return in a "hot" state (5.9 ECUs in $UP_{30,60}$, 11.3 ECUs in NP, 11.9 ECUs in $CP_{0,60}$, and 11.6 ECUs in $CP_{0,80}$). A non-parametric analysis indicates that the differences in the return of allocators are always significant when look at the effective or the hot return of allocators in NP and $UP_{30,60}$ ($p < 0.07$), but there are no differences when we compare the returns in NP and any of the coordinated treatments ($p > 0.31$).[6] In addition, our econometric analysis in the last section of this appendix suggests that the retuned amount is lower when punishment is uncoordinated compared with the NP treatment, while the coordinated punishment has a positive (but not always significant) effect on the return of allocators; in fact, the Arellano-Bond specifications provide evidence that coordinated punishment can facilitate returns from allocators in $CP_{0,60}$ and $CP_{0,80}$, compared with NP treatment (see Tables E4-E6).

Overall, we interpret these findings as evidence that allowing for punishment has negative effects on the levels of joint investment and the return of allocators when punishment is uncoordinated. The negative effects are mitigated when we allow for the possibility of coordinated punishment; in fact, there is suggestive evidence that allowing for coordinated punishment may be beneficial for the return of allocators.

In what follows, we present the results of our econometric analysis to compare the investors' decisions in the baseline treatment without punishment (NP) with their behavior in the treatments where punishment is possible. The dependent variable in all the specifications refer the likelihood of joint investment.

Table E1 reports the results for the $UP_{30,60}$ treatment. Our findings suggest that investors are less

[5] There are also differences in the likelihood of observing non-investing groups (i.e., groups in which none of the investors decide to invest). This occurs less fequenly in the NP treatment (35.7%) than in the $UP_{30,60}$ treatment (55.7%), but the likelihood of non-investing groups in NP is very close to the likelihood in the $CP_{0,60}$ (42.4%) or the $CP_{0,80}$ treatment (35.7%), thereby suggesting that allowing for punishment may be detrimental for joint investment but only when this is uncoordinated.
[6] However, we find that allocators are more likely to return any positive amount or the fair amount if punishment is coordinated, compared with the case of no punishment; e.g., the likelihood of observing a positive (fair) return in NP is 71% (32%), respectively. This behavior is more frequent than in $UP_{30,60}$ (Positive: 66%, Fair: 27%) but less frequent than in the $CP_{0,60}$ (Positive: 81%, Fair: 41%) or $UP_{30,60}$ (Positive: 77%, Fair: 41%) treatments.

likely to invest in $UP_{30,60}$ compared with NP. The same holds when comparing the $CP_{0,60}$ treatment with NP (see Table E2). Finally, there are significant no differences in the behavior of investors in the $CP_{0,80}$ and NP treatments (see Table E3).

Table E1. Investors' decisions: Likelihood of joint investment using Arellano-Bond and random-effect logit specifications: Baseline (NP) vs Uncoordinated Punishment ($UP_{30,60}$)

	NP vs $UP_{30,60}$			
	Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)
Joint investment $_{t-1}$	0.102	-0.116	-0.481	-0.105
	(0.084)	(0.107)	(0.734)	(0.718)
Joint investment $_{t-2}$	0.065	0.009	0.182	0.217
	(0.057)	(0.057)	(0.512)	(0.488)
Return $_{t-1}$ * Joint investment $_{t-1}$	0.026***	0.033***	0.227***	0.219***
	(0.007)	(0.007)	(0.055)	(0.047)
$UP_{30,60}$ * Joint Punishment $_{t-1}$	-0.239*		-0.846	-0.016
	(0.133)		(1.077)	(0.023)
Payoff Difference $_{t-1}$		0.003		
		(0.003)		
Period	0.003	-0.022	0.104	0.109
	(0.021)	(0.023)	(0.245)	(0.238)
Period^2	-0.001	0.001	-0.015	-0.015
	(0.001)	(0.001)	(0.014)	(0.014)
Uncoord. Punish ($UP_{30,60}$)	-0.087*	-0.098**	-0.662*	-0.691*
	(0.044)	(0.044)	(0.371)	(0.362)
Constant	-0.012	0.064	-3.475***	-3.401***
	(0.123)	(0.125)	(0.959)	(0.876)
Number of obs.	505	505	505	505

Notes. Significance at the *10%, **5%, ***1% level

Table E2. Investors' decisions: Likelihood of joint investment using Arellano-Bond and random-effect logit specifications: Baseline (NP) vs Coordinated Punishment ($CP_{0,60}$)

	NP vs $CP_{0,60}$			
	Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)
Joint investment $_{t-1}$	0.120	0.127	-0.208	0.607
	(0.085)	(0.098)	(0.499)	(0.502)
Joint investment $_{t-2}$	0.075	0.072	0.733*	0.730*
	(0.055)	(0.054)	(0.443)	(0.437)
Return $_{t-1}$ * Joint investment $_{t-1}$	0.015**	0.018***	0.174***	0.147***
	(0.006)	(0.006)	(0.035)	(0.031)
$CP_{0,60}$ * Joint Punishment $_{t-1}$	-0.023		0.514	
	(0.098)		(0.530)	
Payoff Difference $_{t-1}$		-0.001		-0.021
		(0.002)		(0.015)
Period	-0.038*	-0.071***	-0.237	-0.230
	(0.023)	(0.026)	(0.162)	(0.161)
Period^2	0.001	0.003**	0.004	0.003
	(0.001)	(0.002)	(0.009)	(0.009)
Coord. Punish ($CP_{0,60}$)	-0.070*	-0.073*	-0.581*	-0.551*
	(0.038)	(0.038)	(0.331)	(0.317)
Constant	0.462**	0.501***	0.635	0.566
	(0.180)	(0.190)	(1.454)	(1.447)
Number of obs.	505	505	505	505

Notes. Significance at the *10%, **5%, ***1% level

Table E3. Investors' decisions: Likelihood of joint investment using Arellano-Bond and random-effect logit specifications: Baseline (NP) vs Coordinated Punishment ($CP_{0,80}$)

	NP vs $CP_{0,80}$			
	Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)
Joint investment $_{t-1}$	0.097	0.221***	0.140	1.218*
	(0.081)	(0.075)	(0.539)	(0.636)
Joint investment $_{t-2}$	0.002	-0.032	0.621	0.618
	(0.055)	(0.058)	(0.746)	(0.685)
Return $_{t-1}$ * Joint investment $_{t-1}$	0.010**	0.009*	0.148***	0.115***
	(0.005)	(0.005)	(0.034)	(0.022)
$CP_{0,80}$ * Joint Punishment $_{t-1}$	0.204**		1.073	
	(0.088)		(0.787)	
Payoff Difference $_{t-1}$		-0.005***		-0.030**
		(0.002)		(0.014)
Period	0.027	0.003	0.154	0.142
	(0.022)	(0.025)	(0.203)	(0.194)
Period^2	-0.002*	-0.001	-0.014	-0.014
	(0.001)	(0.001)	(0.011)	(0.010)
Coord. Punish ($CP_{0,80}$)	-0.044	-0.040	-0.286	-0.266
	(0.037)	(0.035)	(0.413)	(0.379)
Constant	0.444***	0.475***	0.158	0.107
	(0.160)	(0.166)	(1.648)	(1.526)
Number of obs.	505	505	505	505

Notes. Significance at the *10%, **5%, ***1% level

We replicate the analysis for the behavior of allocators so as to compare their intended return when punishment is possible and when it is not. Table E4 reports the results for the comparison between NP and $UP_{30,60}$. The estimates for the comparison between NP and the coordinated punishment devices $CP_{0,60}$ and $CP_{0,80}$ is presented in Tables E5 and E6, respectively.

Our econometric analysis provides evidence that allocators return less in $UP_{30,60}$ compared with NP (see Table E4). As for the behavior of allocators when punishment is coordinated, the Arellano-Bond specification suggests that allocators return more when punishment is coordinated compared with the case in which punishment is not possible, while the random-effect logit specification suggests that there are no significant differences in the behavior of allocators in the NP treatment and the treatments with coordinated punishment $CP_{0,60}$ and $CP_{0,80}$ (see Tables E5 and E6).

Table E4. Allocators' decisions: Intended return of allocators using Arellano-Bond and random-effect logit specifications: Baseline (NP) vs Uncoordinated Punishment ($UP_{30,60}$)

	NP vs $UP_{30,60}$			
	Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)
Intended Return $_{t-1}$	0.163*	0.202***	0.292***	0.323***
	(0.087)	(0.077)	(0.079)	(0.079)
Intended Return $_{t-2}$	0.282***	0.278***		
	(0.086)	(0.083)		
Joint Investment $_{t-1}$	3.076	12.314	3.143***	0.336
	(3.165)	(10.029)	(1.145)	(2.003)
Profit Reduction $_{t-1}$	-24.631		-8.176**	
	(15.573)		(3.406)	
Payoff Difference $_{t-1}$		0.831		0.091
		(0.516)		(0.071)
Period	-0.371**	-0.341*	-0.054	-0.038
	(0.188)	(0.177)	(0.091)	(0.091)
Uncoord. Punish ($UP_{30,60}$)	-8.904***	-7.850***	-2.358***	-2.434**
	(2.971)	(2.773)	(0.524)	(1.128)
Constant	0.000	0.000	1.667	1.782
	(0.000)	(0.000)	(4.675)	(4.808)
Number of obs.	325	325	545	545

Notes. Significance at the *10%, **5%, ***1% level

Table E5. Allocators' decisions: Intended return of allocators using Arellano-Bond and random-effect logit specifications: Baseline (NP) vs Coordinated Punishment ($CP_{0,60}$)

	NP vs $CP_{0,60}$			
	Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)
Intended Return $_{t-1}$	0.016	0.073	0.013	0.014
	(0.065)	(0.064)	(0.067)	(0.066)
Joint Investment $_{t-1}$	4.491*	1.402	2.520**	2.446*
	(2.596)	(3.033)	(0.994)	(1.331)
Profit reduction $_{t-1}$	-12.860*		-0.258	
	(6.960)		(2.998)	
Payoff Difference $_{t-1}$		0.074		0.002
		(0.156)		(0.046)
Period	-0.006	-0.013	0.059	0.059
	(0.115)	(0.112)	(0.083)	(0.083)
Coord. Punish ($CP_{0,60}$)	2.816***	1.753***	2.026	2.004
	(1.035)	(0.618)	(2.873)	(2.865)
Constant	0.782	1.987	-5.915	-5.893
	(2.403)	(2.131)	(8.867)	(8.867)
Number of obs.	545	545	545	545

Notes. Significance at the *10%, **5%, ***1% level

Table E6. Allocators' decisions: Intended return of allocators using Arellano-Bond and random-effect logit specifications: Baseline (NP) vs Coordinated Punishment ($CP_{0,80}$)

	NP vs $CP_{0,80}$			
	Arellano-Bond		Random-effect logit	
	(1)	(2)	(3)	(4)
Intended Return $_{t-1}$	0.633***	0.224***	0.283***	0.307***
	(0.154)	(0.077)	(0.073)	(0.072)
Intended Return $_{t-2}$	0.085	0.202**		
	(0.117)	(0.089)		
Joint Investment $_{t-1}$	9.831**	2.752	1.827	0.248
	(5.006)	(2.571)	(1.439)	(1.210)
Profit Reduction $_{t-1}$	31.663***		2.585	
	(10.611)		(1.743)	
Payoff Difference $_{t-1}$		-0.165		0.082***
		(0.211)		(0.031)
Period	0.081	-0.314*	-0.255***	-0.240***
	(0.232)	(0.167)	(0.087)	(0.086)
Coord. Punish ($CP_{0,80}$)	18.411***	1.031	1.692	2.302
	(6.290)	(1.205)	(2.243)	(2.291)
Constant	0.000	12.481***	5.941	4.592
	(0.000)	(3.234)	(7.255)	(7.404)
Number of obs.	325	475	545	545

Notes. Significance at the *10%, **5%, ***1% level