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Blind Fines in Cooperatives

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Abstract *In this paper we focus on inefficient product quality arising from a free-riding problem in agricultural cooperatives. Individual incentives are not aligned with group gains in cooperatives because individual members bear the costs of offering higher qualities, whereas the benefits from these higher qualities are shared among all members. We present a blind mechanism whose quality-enhancing properties are analyzed in a theoretical model. This mechanism, which does not require individual monitoring, consists of individually punishing co-op members by using aggregate co-op performance in such way that the better the co-op quality, the lower the exclusion probability. In a computerized environment, using experimental methods, we specifically test the effectiveness of our mechanism in alleviating the incentive problem. Experimental results show that our blind punishment mechanism achieves significant efficiency gains.*

Key words: Random punishment, free-riding behavior, collective action, agricultural cooperatives.

JEL codes: C92, H41, Q13.

Introduction

Alchian and Demsetz (1972) note the existence of a free-rider problem inside teams whenever the actions of the individual members are not observable. The basic free-rider problem is that in a team, efficiency gains from individual actions are shared by all team members, while the effort costs are born individually.

This moral hazard problem is particularly relevant to one of the most important forms of agricultural business around the world: agricultural cooperatives. In the United States, for example, approximately one-third of all agricultural products are marketed through cooperatives (USDA 2002), and account for almost half of agricultural production in Western Europe (COGECA 2005).

In this paper we focus on a specific dimension of the free-riding problem: the product quality of the cooperative. The relevance of product quality and the goal of offering higher qualities as a survival strategy have been stressed by some authors from both the academic (Cook 1995) and governmental sectors (USDA 2002).

Babcock and Weninger (2004) and Frick (2004) both discuss the relevance of the free-riding phenomenon associated with product quality. In the former, the quality of the Alaskan salmon industry is studied. The authors find that declining salmon prices, due primarily to expansion of farmed salmon production, have reduced revenues for Alaska's wild salmon fisheries by roughly 62 percent over the past 10 years. In the latter, a study based on data from a sample of 300 German wineries from 1996 to 1999, it is found that the average wine quality offered by German cooperatives is significantly lower than that offered by firms with other ownership structures.

Two other instances of the free-riding problem with product quality are the *Protected Designations of Origin* (PDO) and the *Protected Geographical Identification* (PGI) labels. PDO and PGI labels have been utilized in both the United States and the European Union to enforce a strategy that differentiates products based on excellence and quality standards. Washington apples and Andalusian olive oil are two well known examples of PGI and PDO, respectively.

PDO and PGI products are usually experience goods whose quality is unknown to the consumer prior to purchase. Consumers must then rely on the reputation of the product's designation, that is, on the quality associated with the label. Because offering quality individually is costly, free-riding by offering low quality products under the umbrella of the PDO or PGI label can occur. As a consequence, the quality of these label-protected products may decline in the long run (Zago 1999, Loureiro and McCluskey 2000, Quagrainie et al. 2003). In particular, Quagrainie et al. (2003) find support for maintaining and building on good reputation and stopping the declining quality in their analysis of Washington red delicious apples, based on daily observations from a number of cities from July 1996 to November 1999.

There are, however, mechanisms that can counteract this negative tendency. One example is the use of minimum quality standards. Winfree and McCluskey (2005, page 212) state that, "The organization formerly known as the *Washington Apple Commission* has proposed to create standards for a new elite apple." Babcock and Weninger (2004) report that the *Alaska Quality Seafood Program* adopted quality analyses of wild salmon delivered by individual fishermen in order to achieve a quality-certified wild salmon, which hopefully would be high-priced on the market. A purportedly cheaper alternative is to monitor and estimate the salmon quality delivered by each individual and to pay fishermen accordingly.

In the abovementioned mechanisms, it is critical to have proper monitoring of individual quality, as well as enforcement of penalties for misbehavior. There are at least two factors that stand in the way of practical implementation: technical infeasibility and prohibitive implementation costs. In the salmon example, discerning salmon quality on board a ship is a challenging task and the benefits of higher qualities might not fully offset the monitoring costs.[1]

[1] *It is interesting to note that these solutions are in line with the ideas arising from the theoretical analysis of moral hazard in teams. Holmström (1982) conducted the first analysis to propose full exclusion from the team whenever the observed output is not as efficient as a mechanism for implementing the efficient*

In this paper we propose a mechanism that does not require individual monitoring. The key element lies in estimating the quality of the co-op's product and comparing it to the highest quality that might be achieved under conditions of total transparency. The estimation might be obtained either by evaluating the product quality of a sample of members or by using the market price.[2] The comparison would reveal the success ratio of the co-op. We propose paying each member of the cooperative according to this ratio, with a blind probabilistic punishment; that is, regardless of her individual quality, each member receives her share of the co-op's benefits with a probability that matches the co-op's success ratio.[3]

As discussed in greater detail below, our mechanism contains some interesting and balancing features. It is blind in the sense that it does not discriminate between high and low quality providers. Nor does it require any individual information about performance. Rather, it exclusively relies on a collective measure of performance (the success ratio). However, the collective measure that determines exclusion is endogenously generated by the cooperative members. The *individual* probability of being excluded from the collective benefits is endogenous because it depends on the common *cooperative* performance. A better (worse) collective performance is always associated with a lower (higher) probability of being excluded from the collective benefit.

In the description of our blind mechanism, we perform a two-step exercise. In the first step, we develop a theoretical model of an agricultural cooperative with an arbitrary number of members. We do not consider the participation constraint, as we are interested in analyzing the tension between individual actions and collective outcome, that is, the incentive compatibility constraint.[4] In the model, co-op members privately decide on the quality of the product to be delivered to the co-op. The co-op's product quality is an increasing function of the quality of the delivered products. The dominant strategy involves each co-op member offering the lowest quality, and as a consequence, the product offered by the cooperative moves toward the bottom of the quality space.

Once the free-rider problem is established, we introduce the blind mechanism. We show that there are cases in which a new optimal behavior appears with an outcome that corresponds to all co-op members delivering the highest quality. A coordination problem then arises. Rather than a single inefficient optimal behavior in which all co-op members

solution. Rasmusen (1987) proposes the so-called massacre and scapegoat contracts. In the former, all but one (the survivor) randomly chosen member of the team are penalized to pay a positive amount of money to the survivor, who collects the joint product. In the latter, all but one (the guinea pig) randomly chosen member share the joint product and the penalty paid by the guinea pig.

[2] *The market price is, however, sensitive to changes in consumers' tastes, incomes, etc.*

[3] *A question arises as to the use of the excluded profit shares. In this paper we implicitly assume that their use has no direct impact on co-op members' welfare, but is used for different purposes (improving facilities, etc.). This way, exclusion truly means exclusion. See Fatas et al. (2010) for alternative allocation rules.*

[4] *As a referee points it out, the participation constraint seems a critical aspect, especially in a cooperative setting, where there is no vertical authority imposing the mechanism. The analysis of the role of this constraint is not a trivial issue. Some papers address it by having subjects choose among competing compensation schemes (see for example Fehr et al. 2007 and Wu and Roe 2006). Other papers note that the mere possibility of voting on rewarding or punishing institutions has a large and positive effect on contributions (see Sutter et al. 2010). Finally, Noussair and Tan (2009) show that the existence of the voting process may lead to suboptimal institutions.*

provide low quality inputs, we now have two different optimal behaviors (equilibria): an efficient but risky one, and a safe but inefficient outcome.

In the second step, we use an experimental methodology to understand the equilibrium selection process.[5] In a computerized environment, we specifically test the effectiveness of our mechanism in alleviating the incentive problem. Our experimental results suggest that in the co-op model without the blind mechanism, the average quality delivered by the members of the co-op declines over time, until settling at approximately 35-40% of the highest possible quality. However, once the mechanism is introduced, its quality-enhancing properties are evident; the average co-op quality holds at approximately 70% of the maximum quality in the long run. To control for the psychological effects of the blind punishment mechanism (subjects choose higher qualities solely due to a demand effect), we additionally run a control treatment in which the blind mechanism does not support high quality choices in equilibrium. Our experimental results show that the impact of introducing the blind mechanism is null in this control case.

The remainder of the paper is organized as follows. Section 2 surveys recent experimental literature on team production and punishment to contextualize our mechanism. Section 3 carries out the theoretical exercise by putting forward a co-op model in which the theoretical properties of the blind mechanism are presented. Sections 4 and 5 evaluate the theoretical predictions through an experimental setting, and present and discuss the main experimental results, respectively. Section 5 concludes.

Experimental Background

In this paper we employ the methodology of experimental economics. Experiments in economics differ from those in other fields in two aspects. First, in the economics experiment, subjects are paid their earnings in cash. This practice, termed "induced valuation" by Smith (1982), ensures that the incentives assumed in the models are salient for the participants. Because their earnings are real, the decisions they make are consequential, thus reducing or eliminating some of the problems associated with experiments in other fields (experimenter demand, etc.). The second manner in which economics experiments differ is that our experimental participants are not deceived. This ensures that subjects are playing the game we intend them to play, and not attempting to uncover the "true purpose" of the experiment.

Economics experiments have several advantages for studying economic institutions (and in this instance, cooperatives). First, experiments are not "realistic" simulations of "institutions." Rather, they are designed to isolate and separately examine the critical aspects of particular settings. This ability to isolate one or more key factors is one advantage of the experimental approach. Another issue is the ability to build experimental models that replicate most of the assumptions of formal models, thus testing the theory "in its own domain". As Plott (1986) points out, if a model is true in the field, it should also be true in the lab. Finally, the lab confers complete control over the data-generating process.

However, experiments have limitations as well. In particular, the extrapolation of subjects' (usually students') behavior in a lab setting to real

[5]*Experiments have been widely used to understand equilibrium selection. See* Devetag and Ortmann (2007) *for a recent review.*

economic situations is always challenging. In this paper we do not merely examine behavior, but focus instead on "comparative statics" – that is, how behavior changes when conditions change. Subject pool differences may be a real issue (see List 2003). However, most studies (e.g. Brookshire et al. 2007) show that different subject pools do not behave in fundamentally different ways, and in fact react in a similar fashion to treatment effects. Falk and Fehr (2003) survey this issue in detail.

Our mechanism is based on random exclusions from the group benefit. In the real world, exclusion is widely used as a disciplinary measure against defectors both at work and in daily social life. Shirking workers are usually fired (Shapiro and Stiglitz 1984); uncooperative neighbors are invited less frequently to social events; criminals are incarcerated or expelled (Hirshleifer and Rasmusen 1989); and countries that violate international conventions are boycotted. Endogenous exclusion mechanisms are used in many organizations as an implicit or explicit incentive mechanism. Jack Welch of GE famously fired the bottom 10% of employees each year, thus implementing competition among employees to stay in the top 90% to avoid exclusion from the group.

In recent years exclusion has also been considered in experimental economics. Swope (2002) and Kocher et al. (2005) implement group exclusion if subjects cooperate below an exogenously predetermined level chosen by the manager. Cinyabuguma et al. (2005) allow participants to expel group members based on a majority vote. In Croson et al. (2008), the worst performer in a team production game is excluded from the benefits of team production, so competition among group members determines who is going to be excluded.

Our punishment mechanism differs from the abovementioned methods in a number of respects. First, random exclusions do not depend on the willingness to pay for excluding others. In a different setting, Fehr and Gächter (2000) showed that cooperation in a public goods game can be enhanced if subjects are given the opportunity to punish each other. This positive result crucially relies on the existence of stronger punishers. However, the presence of strong punishers is not needed in our mechanism.

Second, the threshold for exclusion is endogenous. The unique cooperative without the risk of exclusion is the one that offers the highest quality. This is an advantage over exclusions mechanisms based on exogenously fixed thresholds in which members usually stick to the minimum.

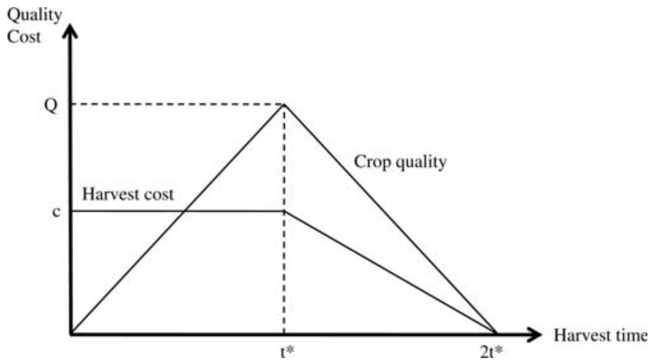
And third, contrary to Croson et al. (2008) and Fehr and Gächter (2000), individual information requirements are null, because the success ratio only requires aggregate data. This is an advantage because it implies smaller implementation costs. An additional advantage is that with some other mechanisms, the manager does not need to know individual performance, but team members must be able to observe one another (this is a requirement for horizontal punishment schemes). In our mechanism, individual team members do not need to be able to observe one another.[6]

Theoretical Model

In this section we present the basic model of an agricultural cooperative. We then introduce the punishment mechanism based on blind fines, and analyze the efficiency properties.

[6] *We thank a referee for pointing this out.*

Figure 1 Basic technology



The co-op game

A technology is composed of a crop quality function $c(\cdot)$ together with a harvesting cost function *t*. Both functions depend on the harvest time *t*. The technology to be considered in this study is depicted in figure 1.

The growing period spans from *0* to *2t**, and the highest crop quality *(Q)* is achieved at harvest date *t** (henceforth, we refer to *t** as the efficient harvest time). For simplicity, we assume that the crop quality function is single-peaked and symmetric with respect to the efficient harvest time. Regarding the harvesting cost, we assume that it is flat until the efficient harvest time is exceeded; then it decreases in a linear fashion until vanishing at the end of the growing period. We assume that $Q > c$; that is, production is profitable.[7]

We further assume that each member of the co-op is endowed with this technology, although their starting points for the growing period differ. Hence, the co-op is quite homogeneous in the sense that all members have the same crop technology, although it is heterogeneous in the sense that each member is endowed with a different growing period. Finally, we assume that members' profits are given by the average quality of the co-op's crop, minus the harvesting cost. [8]

Note that the cost depends on one's own actions, but revenues depend on the decisions of all co-op members. Hence, the optimal decision by a co-op member is not trivial, and might depend on what she expects others to do. We next prove that regardless of their expectations about the behavior of the remaining members, co-op members have an optimal (dominant) action.

Proposition 1. *Efficient harvesting is dominant whenever $c \leq Q/n$. Otherwise, offering the lowest possible quality is dominant.*

Proof: see the Technical Appendix

This proposition states that efficient harvesting, although profitable in an individual setting (we assume that the individual return from producing the highest quality *Q* is larger than its harvesting cost c) will only be

[7]*Our main results remain unaltered if we consider a more general harvesting cost function (for example if costs are increasing up to the optimal harvest time). Our particular election simplifies both algebraic and experimental processes.*

[8]*We assume a deterministic framework in that there is no uncertainty as to the quality crop beyond the harvest time decision. This assumption is not crucial to the free-riding problem, and allows a simpler model.*

provided when the *per capita* return from producing the highest quality product exceeds its harvesting cost. This is the inefficiency inherent in a cooperative setting.

The co-op game with random exclusions

Assume now that with some probability, a member of the cooperative can be excluded from receiving a share of the cooperative's benefits. Of course, the optimal way to settle this exclusion probability would be to exclude those members whose qualities are different from the efficient Q. However, this mechanism would require complete knowledge of individual qualities, either by performing quality tests in the field or by knowing the particular values of the production functions of each member.[9]

We propose a mechanism that is applicable when the growing periods of co-op members are considered private information. The key element of the mechanism is to rely on a variable that is observable without cost: the *success ratio* of the cooperative (R), defined as the ratio of the realized quality of the co-op product over the highest possible quality Q,

$$R = \frac{\Sigma_j q_j(t_j)}{nQ}. \tag{1}$$

In the following, we set the exclusion probability to $1 - R$.[10] Under the blind mechanism, the expression for expected payoffs is more involved, as now the actions of the co-op members enter twice: (i) they affect the size of the share; and (ii) the probability of obtaining the share. We next show that under the blind mechanism, the strategic interaction inside a co-op does not yield a unique optimal behavior.

Proposition 2. In a cooperative with the blind mechanism,

- If $c \leq \frac{1}{n}\frac{Q}{n}$, then efficient harvesting is dominant.
- If $\frac{1}{n}\frac{Q}{n} \leq c \leq \frac{2n-1}{n}\frac{Q}{n}$, then offering the efficient or the lowest quality is optimal.
- If $c \geq \frac{2n-1}{n}\frac{Q}{n}$, then offering the lowest quality is dominant.

Proof. See the Technical Appendix

Proposition 2 shows that if the harvesting cost is sufficiently small, then co-op members find it optimal (regardless of their expectations about the behavior of the other members) to offer the efficient quality. Also, if the harvesting cost is sufficiently large, then inefficient harvesting is the unique optimal action.[11] However, there exists an intermediate region for which the optimal behavior depends on expectations (technically speaking, it is a Nash equilibrium): a member will harvest at the efficient time, provided he expects others to do the same. This means that the mechanism generates a coordination problem with two Nash equilibria.

[9] *Recall that coop members are endowed with heterogeneous growing periods. It is this heterogeneity that renders it impossible to discern the quality upon observing the harvest time.*

[10] *One can devise alternative mechanisms that, using of the same information, differ either in the size of the penalty or the sensitivity to the success ratio. Our mechanism assumes a linear relationship sets the largest fine as exclusion. Future research should investigate the existence of optimal mechanisms (conditional on the same informational input) and their experimental merits.*

[11] *Note that this threshold is larger (specifically, twice the size) than in the coop game without the blind mechanism.*

Experimental Design and Procedures

Our experiment consists of two games and two treatments. The two games are the co-op game with no punishment (NP), and the random punishment game (RP) considered in the previous section, with the following parameters values: $n = 4$, $Q = 500$, and $t^* = 250$. The two treatments correspond to two cost values: $c = 200$ (LC treatment) and $c = 300$ (HC treatment). In each period, each player has, in addition to her profits, an additional fixed payment of 200 experimental currency units (ECU). All ECU are privately changed to real money at the end of the experiment.

In both treatments, players adhere to the same sequence: first, they play the NP game for twenty periods (a block) and then they play the RP game for another block. Hence, experimental subjects always play the NP game in block 1, and the RP game in block 2. Beginning with Fehr and Gächter (2000), the literature strongly suggests that no major order effects are present in this game. The cooperation-enhancing effect is independent of whether it is played before or after a similar game with no punishment. Fatas et al. (2010) and Eckel et al. (2010) find similar results. The main elements of the experimental design are summarized in table 1 below.

The two last columns in table 1 display the equilibrium outcomes across blocks and treatments according to the theoretical analysis and parameter values. In the HC treatment, zero quality is the dominant strategy in both blocks. This is also true in the NP game for the LC treatment, although the RP game has a second (and efficient) Nash equilibrium. Hence, the experimental design is devised to test for the effectiveness of the random mechanism in promoting the highest quality.

Our study presents the results of computerized experiments conducted at Laboratorio de Investigación en Economía Experimental (LINEEX), the experimental laboratory at the University of Valencia,[12] using z-Tree.[13] Subjects were recruited electronically through the LINEEX Web-based system. None of the subjects had ever participated in a similar experiment before, though some had participated in different LINEEX experiments. By participating in the experiment, each participant made an average of US$42, and experiments took approximately 90 minutes to complete.

The complexity of our experiment is relatively high. Thus, one of our major concerns was to guarantee that subjects fully understood the rules of the game. At the beginning of a session, written instructions were read aloud. A basic quiz with closed answers was run before the experiment began to maximize the understanding of the experiment.[14] After the experiment, subjects were debriefed by an on-screen questionnaire including questions about their strategies (to double-check that they understood the structure of incentives). Given their replies and the procedure, we are confident that both the tasks and the incentives were correctly understood by the subjects.

Participants were assigned to one treatment when recruited. When they entered the lab, they were randomly allocated to a cubicle by choosing a numbered chip (with a cubicle's number) from a basket. At the beginning of every session, each participant was randomly allocated to a group of

[12] *The University of Valencia does not require this protocol to be approved by a human subjects review board.*

[13] *See Fischbacher (2007) for details about the z-Tree toolbox used in the experiments.*

[14] *The translated instructions are included in the appendix. The original instructions, in Spanish, are available from the authors upon request.*

Table 1 Summary of treatments, equilibrium outcomes

			Blocks			Games	
Treatment	Subjects	Groups	1	2	Sessions	NP	RP
LC	40	10	NP	RP	1	0	0 and 500
HC	40	10	NP	RP	1	0	0
2	80	20			2		

four, whose composition remained unchanged throughout the entirety of the experiment. All subjects went through the same sequence of games: two blocks of 20 rounds. The experiments involved a first block and then, after a surprise restart, a second block (where the Random Punishment game, RP, is played). The surprise restart technique has been widely cited in the experimental literature (see Croson 2000 for details). In our setting, the subjects did not know at the beginning of the experiments that they could participate in the second part of the experiment.

No deception was involved, however, as monetary rewards from the first block were independent of the second block earnings. At the end of the first block, subjects were allowed to leave with the money they made in the first block, or they could stay and participate in another experiment. All subjects decided to stay for the second block, though this is not always the case (Brandts et al. 2008).

After each round, subjects received information about their past individual delivered quality and their own earnings. They also received information about the success ratio of their group and, in block 2, whether or not they had been punished. Punishment was independent and identically distributed across subjects, meaning that in each period, each participant faced her own realization of the punishment probability.[15] This implies that for a given success ratio, it could be the case that none, one, or even all subjects in a group were punished. The profit shares of punished subjects were not distributed, but were kept by the experimenter. At the end of the experiment, all subjects were privately paid using sealed envelopes with the number of their cubicle typed on the outside. It was impossible to link names and decisions.[16]

Results and Discussion

In this section we analyze the product quality of the co-op, the equilibrium selection process, and conclude with an econometric analysis of the behavioral consequences of punishment at the individual level.

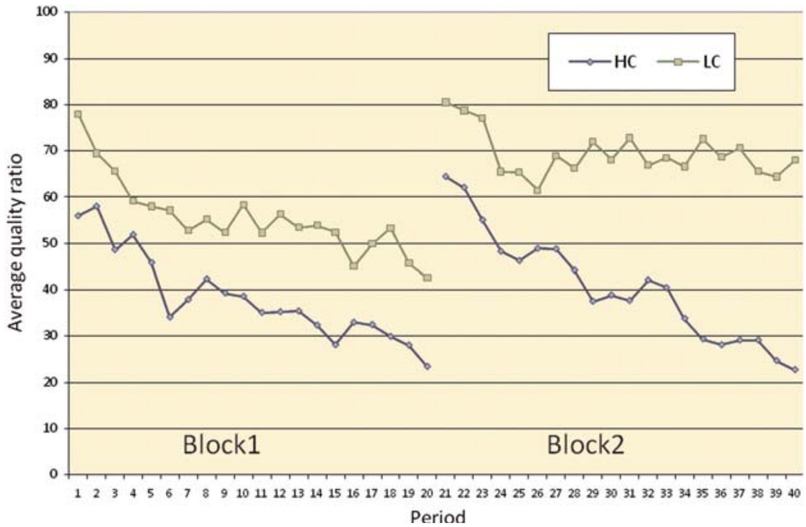
Product quality of the co-op

The main finding of the paper is evident in figure 2, which displays the evolution of the average product quality of the co-op by treatment.

[15] *The random numbers were generated by the z-tree software.*

[16] *Privacy and anonymity are strictly guaranteed. Subjects are never asked to introduce any personal information in the system. Although the electronic recruitment process requires their names, upon reaching the lab, subjects are only asked to show an ID picture to one of the assistants to get in. The distribution of subjects is unknown to the experimentalists, as are their decisions. Moreover, we believe that participants perceived this fact. The full protocol is available from the authors upon request.*

Figure 2 Co-op quality in both blocks by treatment



In the first block, the quality of the final product is positive and far from zero, even in the last rounds of both treatments. In the LC treatment, the success ratio begins at approximately 80% and declines towards 40%. This decreasing trend also occurs in the HC treatment. However, not surprisingly it occurs at a lower quality level, because subjects react to the larger opportunity cost of supplying a high quality product. While the average LC quality is 55.5%, it is only approximately 40% in the HC treatment.

In the second block (rounds 21-40), subjects faced the random punishment mechanism. Here we see a drastic change. Even though both treatments show a strong positive restart effect, they differ noticeably in their dynamics over time. Immediately after the restart, the success ratio declines sharply in the HC treatment from approximately 65% to 20%. The average quality accounts for 40.58% of the maximum level, which is quite similar to that observed in the first block. However, in the LC treatment, the average quality ratio does hold at approximately 70% after a slight decrease in rounds 21 to 26. The success ratio is roughly doubled, on average.[17]

We next analyze the impact of the random punishment mechanism on subjects' behavior. This is an easy task because in both treatments, subjects experience a *cooperation failure* in the first block. One simple way to account for this effect from a statistical perspective is to determine whether the average quality (or equivalently, the success ratio) differs over time for each treatment. Table 2 presents the results of our analysis. Specifically, it shows the results of non-parametrical tests (Wilcoxon signed-rank tests) applied to our 10 groups at different points of time.

We first compare the first and last rounds in each block for every treatment. Concretely, we have four different comparisons: LC-NP (upper index a), HC-NP (upper index d), LC-RP (upper index c) and HC-RP

[17]*These effects are confirmed either by estimating a panel data model, with random effects at the individual level and group clusters, or by performing non-parametrical tests. See Appendix for details (tables 1A and 2A).*

Table 2 Quality in rounds 1, 20, 21 and 40. Within subjects analysis at the group level. Wilcoxon non-parametrical tests

		Round 01		Round 20		Round 21		Round 40
Average quality	LC	389.75[a]	>	213.15[ab]	<	402.6[bc]	=	340.1[c]
	HC	279.6 [d]	>	117.1[de]	<	322.2[ef]	>	113.65[f]

Wilcoxon signed rank-sum non-parametrical test at the group level, within groups, n = 10.
a, b, d, e, f – difference is significant at the 1% level.
c – difference is not significant at any reasonable level (p value = 0.1688).

(upper index f).[18] Quality is significantly higher in the first round than in the last round in all comparisons except LC-RP, where the quality at the end of the 40 rounds is not significantly lower than the quality in round 21. The opposite holds for the other three cases. These results endorse the conclusions from figure 2, that while no differences are observed in LC when the random punishment device is operating, a significant decline is observed in HC.[19]

Finally, we present a comparison between the average qualities provided at the group level at the end of both blocks (with and without random punishment) in each treatment. Similar to previous results, by the end of the second block of 20 rounds, all gains generated by the random mechanism in the HC condition are dissipated (the difference between 117.1 and 113.65 is not significant at any level, $p = 0.5403$). This is not the case in the LC condition, as the delivered quality is still significantly higher by the end of the second block (340.1) than by the end of the first one (213.15). The difference is significant at the 10% level ($p = 0.0745$).

Equilibrium selection

Given that the theoretical analysis uncovers an equilibrium selection process, it is interesting to study decisions at the individual level. Some relevant questions arise: Do subjects coordinate themselves in the LC treatment? Do the treatment effects compel subjects to play the efficient equilibrium with a higher frequency? To provide answers, we analyze the distribution of decisions across treatments.

Given that the quality collapses by the end of the first block in both treatments, we focus on the evolution of equilibrium play in the second block, where the random punishment mechanism is in place. Table 3 shows that the overall number of times any equilibrium quality (either the lowest quality or the highest one) is chosen is roughly the same across treatments (327 vs. 340 in absolute terms, or 40.88% vs. 42.25% in percentages). However, the distribution is remarkably different; while the efficient equilibrium is chosen three out of four times in LC, this frequency is mirrored in the HC treatment, where subjects overwhelmingly choose the inefficient equilibrium. Hence, under the random punishment mechanism, the efficient outcome turns into an equilibrium outcome that actually attracts subjects' behavior in the LC treatment.

[18]*Table 2 also incorporates the comparison of the immediate response to the random punishment mechanism from round 20 to round 21 for every treatment (upper index b for LC treatment and upper index e for HC treatment). In both cases, groups react by offering significantly higher qualities.*
[19]*Table 3A in the Appendix provides a complementary analysis using the panel data technique.*

Table 3 Relative frequencies of equilibrium play

	Equilibrium play[a]		Quality				
			Lowest		*Highest*		
Treatment	**Freq**	*(%)*	**Freq**	%	**Freq**	%	**Number of observations**
LC-RP	327	*(40.88)*	78	*(9.75)*	249	*(31.13)*	800
HC-RP	340	*(42.25)*	242	*(30.25)*	98	*(12.25)*	800

a – Even though the highest quality is not an equilibrium action in HC-RP, "Equilibrium play" denotes any action associated with equilibrium in HC or LC. Thus, data include the lowest and the highest qualities in both treatments, for the sake of comparability.

It is interesting to note, however, that the efficient outcome is played more frequently in block two that in block one in the HC treatment. We think of this as an attempt by some players to avoid exclusion in the second block. Recall that an increase of one's own quality reduces the exclusion probability. This attempt can of course be linked to players' risk aversion, as the introduction of the blind mechanism adds a new source of risk; in addition to the strategic risk, players also face the risk of being excluded. In fact, if all players offered efficient quality, the exclusion probability would be zero. Unfortunately for them, this was not an equilibrium choice for the high-cost treatment. [20]

Behavioral determinants

Our previous analysis shows that subjects react to the presence of the punishment mechanism, but it is important to clarify which subjects react. We must understand whether good co-op members (those providing higher qualities) react to the unfair nature of the blind punishment. To this end, in table 4 we present the estimates of panel data models with random effects at the individual level, clustered by groups.

To measure the relative impact of the random punishment mechanism on different subjects, we control for their performance in the previous round. Thus, let Lrp be a dummy variable that explicitly considers the lagged relative performance (it takes the value of 1 when the lagged individual quality was above the average quality in the group, and 0 otherwise). Lp accounts for the immediate effect of being punished (it takes the value of 1 when the subject was punished in the previous round, and 0 otherwise), and Lrp*Lp is the interaction term. Quality 1 is the quality delivered by every subject in the first period.

Similar to some of the results presented above, the decreasing trend is now only significant in the HC treatment. The lagged relative performance is significant and positive in both treatments, suggesting the existence of inertia. Being punished in the previous round does not generate a noticeable overall effect, as suggested by small Lp coefficients not significantly

[20] *Given the complexity of the game, mutual best response actions are rarely observed. Even when groups reached equilibrium according to our intuition, we find the analysis of limited relevance, given the low frequencies. Table 4A summarizes these data in the Appendix.*

Table 4 Behavioral determinants

	RP game	
Dependent variable: Individual quality	**LCRounds 21-40**	**HCRounds 21-40**
Constant	327.9028***	354.1575***
	(47.1153)	(72.1611)
Period	−0.8088	−7.6136***
	(1.4978)	(2.1095)
Lrp(Lagged relative performance)	51.5037***	116.0405***
	(22.2517)	(12.8243)
Lp(Lagged punishment)	5.2722	−4.9993
	(21.426)	(17.1297)
Lrp*Lp	−41.3629**	−70.6455***
	(20.4475)	(17.4096)
Quality1	0.0488	0.1661***
	(0.0974)	(0.0577)
Nº Obs.	760	760
R-sq:		
Within	0.0103	0.1778
Between	0.4391	0.4849
Overall	0.1239	0.2764
Prob > chi2	<.0001	<.0001

**p < 0.10. **p < 0.05. ***p < 0.01*

different from zero.[21] However, top performers react negatively, diminishing the delivered quality when punished, as the significant interaction coefficient suggests.[22] This behavioral reaction is present in both treatments and is in line with results in Fatas et al. (2010).

Conclusions

Product quality is a major concern for cooperatives. Empirical evidence shows that, in some sectors, cooperatives may offer lower quality products than firms with a different ownership structure. To counteract this negative tendency, some mechanisms have been imposed (for example, minimum quality standards).

The main problem with current mechanisms is that they generally require individual monitoring, which may sometimes impose larger costs than the intended benefits. In this paper we offer an alternative that does not require information at the individual level. We propose individually punishing co-op members by using aggregate information; that is, we link the probability of being punished to the co-op performance in such way

[21] *Note that this does not contradict the existence of treatment effects. In this estimation, decisions in the NP block are not considered. The* Lp *dummy tries to capture the differences in behavior, if any, between subjects punished in the previous round, and subjects who were not punished (but could have been punished, as all of them play the RP rounds). From our previous analysis, we already know that relative to the first block, subjects in the second block deliver significantly higher qualities. Punishment statistics are available in the Appendix (Table 5A).*

[22] *The post-estimation test results from* Lrp Lrp*Lp 0 *show that punishment fully offset the quality boost received from lagged high-performers in the LC treatment ($p = 0.455$) but not in the HC treatment ($p < 0.01$).*

that the better the co-op quality, the lower the punishment probability. Note that this class is unfair in the sense that it can punish co-op members that provide the greatest quality.

This paper must be viewed as a first step in the investigation of the merits of this particular class of "unfair" mechanisms. There are many considerations regarding how much to punish and how sensitive the punishment should be to individual actions. In this analysis, we have chosen the most unfair member of this class – full exclusion from the co-op's benefits – and tested it in a lab. Our experimental results are astonishing, as they show quality gains of 75%.

Of course there are several steps to be taken before advocating the use of these blind mechanisms in real cooperatives. The merits of this class relative to other mechanisms must be investigated. Also, it seems uncertain that co-ops would impose on themselves this kind of unfair institution (though this problem could be ameliorated by fine-tuning the probability and the strength of the punishment). However, our findings are clear: the perverse effects of blind punishment do not discourage good co-op members from providing high quality. Indeed, this is the way in which large quality gains are obtained, and are in addition to the savings in individual monitoring costs, which have not been explicitly taken into account in this study.

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Appendix

This appendix contains the technical content of Sections 3 and 5.

Theoretical model

In mathematical terms, the crop quality function $q(\cdot)$ and the harvesting cost function $c(\cdot)$ are defined as follows:

$$q(t; t^*, Q) = \begin{cases} (\frac{t}{t^*})Q & if \quad 0 \le t \le t^* \\ (2 - \frac{t}{t^*})Q & if \quad t^* \le t \le 2t^* \\ 0 & otherwise \end{cases} \tag{2}$$

$$c(t; t^*, c) = \begin{cases} c & if \quad 0 \le t \le t^* \\ (2 - \frac{t}{t^*})Q & if \quad t^* \le t \le 2t^* \\ 0 & otherwise \end{cases} \tag{3}$$

Proof of Proposition 1: First, note that harvesting before t^* are strictly dominated by t^* because they all entail the same cost, c but yield poorer qualities than t^*. Hence, we can safely restrict our attention to harvest times belonging to the interval $[t^*, 2t^*]$. The profit function then becomes:

$$\pi_i(t_i, t_{-i}) = \left(2 - \frac{t_i}{t^*}\right)\left(\frac{Q}{n} - c\right) + \frac{\Sigma_{j \neq i} q_j(t_j)}{n} \quad \text{for } t^* \le t_i \le 2t^*. \tag{4}$$

Note that player i's profits are a linear function of harvest time t_i and that the slope, $\frac{(c - \frac{Q}{n})}{t^*}$, is independent of the quality offered by co-op members other than i. Finally, note that the slope is positive (negative) whenever c is larger (smaller) than Q/n. QED.

Proof of Proposition 2: Under the blind mechanism the profit function becomes:

$$\pi_i(t_i, t_{-i}) = \frac{\left[(2 - \frac{t_i}{t^*})Q + \Sigma_{j \neq qj}(t_j)\right]^2}{Qn^2} - \left(2 - \frac{t_i}{t^*}\right)c \quad \text{for} \quad t^* \le t_i \le 2t^* \tag{5}$$

Given that expected payoffs are convex, the best response function picks one of the two corners of the restriction $t^* \le T_i \le 2t^*$. Therefore, the investigation of player i's optimal behavior comes from the comparison of the profits associated with each of them. It follows that:

$$\pi_i(t^*, t_{-i}) \geq \pi_i(2t^*, t_{-i}) \Leftrightarrow \Sigma_j \neq iq_j(t_j) \geq \frac{cn^2 - Q}{2}. \quad (6)$$

Hence, player i's best response is:

$$BR_i(t_{-i}) = \begin{cases} t^* \text{if} \Sigma_{j \neq i} q_j(t_j) \geq \frac{cn^2 - Q}{2}. \\ 2t^* \text{otherwise} \end{cases} \quad (7)$$

We then investigate the properties of the threshold $\frac{cn^2-Q}{2}$. Note that it is negative for values of c smaller than $\frac{Q}{n^2}$, implying that t^* is a dominant strategy for these values of c. Note that $(n-1)Q$ is an upper bound for $\Sigma_{j \neq i} q_j(t_j)$. It is trivial to show that the threshold $\frac{cn^2-Q}{2}$ is larger than this upper bound for values of c larger than $\frac{2n-1}{n}\frac{Q}{n}$. Hence, for these values, $2t^*$ is a dominant strategy. For intermediate values of c, dominant strategies no longer exist, and both t^* and $2t^*$ are Nash equilibria of the game. QED.

Results and Discussion

Table 1A. Individual quality, treatment effects by blocks

	Rounds	
Dependent variable: Individual quality	**1–20**	**21–40**
Constant	346.8874***	520.1312***
	(17.3701)	(47.0819)
Dcost	−86.335***	−144.2975***
	(22.1322)	(38.1224)
Period	−6.5852***	−5.6697***
	(1.1967)	(1.6354)
N° Obs.	1600	1600
R-sq:		
Within	0.0820	0.0689
Between	0.1407	0.2585
Overall	0.1072	0.1760
Prob > chi2	<.0001	<.0001

$^*p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$

This table shows the estimates of a panel data model, with random effects at the individual level and group clusters (see Liang and Zeger (1986)).

Definitions: Dcost is a dummy variable to account for treatment effects. It takes the value of 1 when the treatment is HC, and 0 otherwise. Period is coded from 1 to 20 (the number of rounds within each block).

Implications: A significant treatment effect is found, as the negative and significant coefficient of Dcost shows. Subjects chose lower qualities in the HC treatment than in the LC, as this difference is greater in the second block than the absolute values of the estimated coefficients indicate (86.33 vs. 144.26). We also obtain a negative and significant coefficient for Period in both blocks. The treatment effect is robust to the estimation method.

Table 2A. Quality in rounds 1, 20, 21 and 40. Between subjects analysis. Mann-Whitney non-parametrical tests

	Treatment	Round 01	Round 20	Round 21	Round 40
Average quality	Low cost	389.75[a]	213.15[b]	402.6[c]	340.1[d]
		>	>	>	>
	High cost	279.6[a]	117.1[b]	322.2 [c]	113.65[d]

This table shows the results of non-parametrical Mann-Whitney tests using our 10 group level independent observations in each round and treatment.
Implications: a, d – difference is significant at the 1% level (Mann Whitney-Wilcoxon rank sum test at the group level, between groups). b, c – difference is significant at the 5% level (Mann Whitney-Wilcoxon rank sum test at the group level, between groups). Hence, the average quality is significantly higher in LC than in HC in round 1, round 20, round 21 and round 40.

Table 3A. Individual quality. Within subjects analysis

	LC	HC
Constant	317.7439***	280.0837***
	(14.6911)	(18.5836)
Period	−3.8096***	−8.4453***
	(0.8633)	(1.7499)
Block	145.6532***	180.4046***
	(28.4031)	(54.0556)
Nº Obs.	1600	1600
R-sq:		
Within	<.0001	<.0001
Between	<.0001	<.0001
Overall	0.0559	0.0790
Prob > chi2	<.0001	<.0001

This table shows the estimates of a panel data model, with random effects at the individual level and group clusters.
Definitions: Block is a dummy variable that takes the value of 0 in the first block and 1 in the second block. Period is coded from 1 to 20 (the number of rounds within each block).
Implications: The coefficient of the variable Block is always significant and positive, which implies that subjects significantly increase their quality when the environment incorporates the blind punishment. This increase amounts to approximately 50% (145 over 317 in LC and 180 over 280 in HC). This also confirms the dynamical patterns observed in our descriptive analysis. Delivered quality decreases over time in both treatments, as the significant and negative coefficients of Period suggest, but the slope is considerably sharper in HC than in LC. This sharper negative trend offsets all gains obtained in HC by the end of the second block.

Table 4A. Best response actions

	LC–NP	LC–RP	HC–NP	HC–RP
0-0-0-0	1	0	3	9
500-500-500-500	1	12	0	2
N	200	200	200	200

Table 5A. Summary statistics on punishment

Treatment	Lagged punishment (Lp)	Lagged relative performance (Lrp)	Lrp*Lp
LC	29.25	52.87	15.25
HC	55.75	43.62	21.75

Instructions (LC treatment, NP game)

The aim of the experiment is to study how individuals make decisions in some environments. Instructions are easy and you can make a non-negligible amount of money if follow them carefully. Money will be privately paid at the end of the experiment. If you have any questions, please raise your hand first before you ask. Any communication between you and the other participants is prohibited. If you do not follow this rule, you will be excused from the experiment.

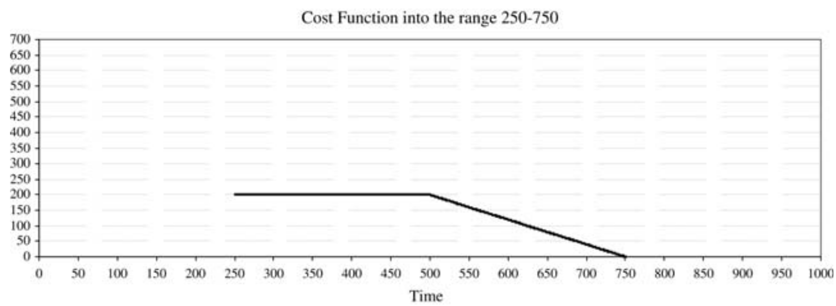
(1) The experiment consists of 20 independent rounds. In each round you are a member of a group of 4 participants who will be called **suppliers**. The composition of each group is randomly determined at the beginning of the experiment and does not change along the experiment. You will never know the identities of the other members in the group.

(2) All suppliers in the same group have to produce and deliver a component to a common client, named **Alpha**. Alpha buys these components from the suppliers to assemble them in his own production process, as the following graph shows:

(3) Your payoff from the experiment will depend on your revenues (the price paid by Alpha) and your costs (which depend on your technology and your decisions). Moreover, both revenues and costs will depend on the production time that you choose to take, as we explain below.

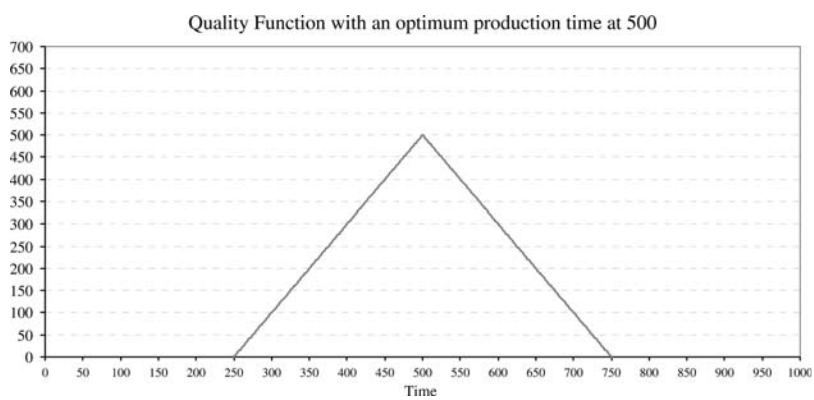
(4) As you work for clients other than Alpha as well, you already have a set of orders that prevent you from starting production immediately. So you have to wait until a minimum date, which we will inform you of at the beginning of the experiment. Each supplier will have a different minimum day, which is private information. In each round, you must only decide when to produce and deliver your component, taking into account that you will be allowed 500 days from your minimum day for delivery.

(5) Your production costs (in Experimental Currency Units, ECU) depend on how fast you deliver your component to Alpha:
   (a) If you produce it quickly (in the first 250 days after your minimum day) you will have to contract additional workers, and then will incur a fixed cost of 200 ECU.
   (b) If you decide to produce in a longer period of time (in the second 250 days from your minimum day), then you will not need to contract anybody. Your production cost will begin at 200 ECU and will proportionally diminish up to zero at the upper limit of 500 days due to the reduction in the extra-hours cost.
   (c) Graph 2 plots the cost function depending on the production time:

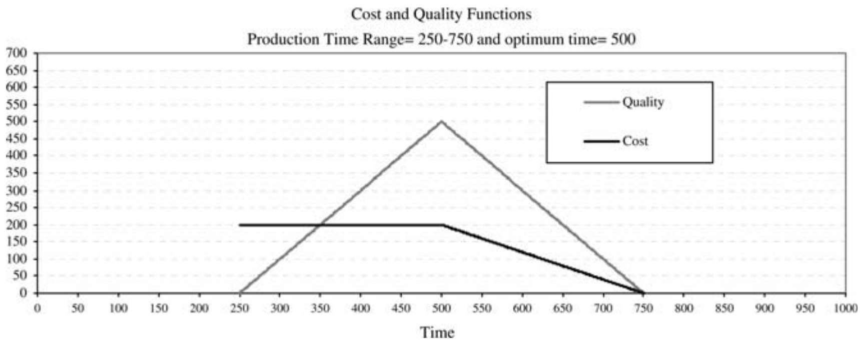


(6) The price paid by Alpha (in ECU) has two elements: one fixed (200 ECU) and the other variable, depending on the components' average quality delivered by you and the other three members of your group. Therefore, the four group members will receive the same price from Alpha.

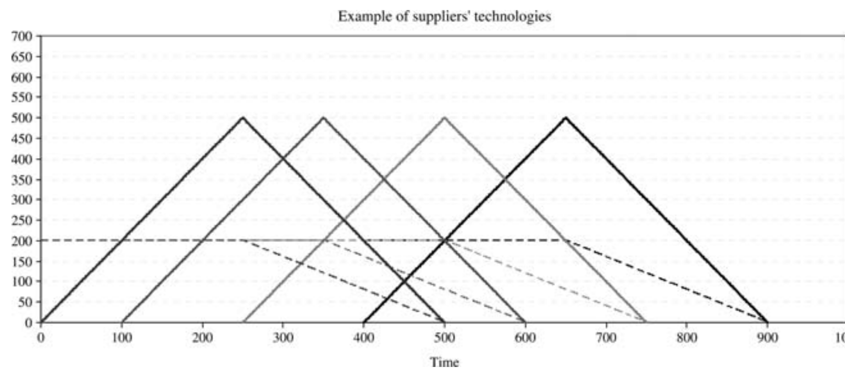
(7) Graph 3 shows the relationship between the individual component's quality and production time. As you can see, it is a symmetric, single-peaked function: the quality is **0** at the minimum day, increases during the first 250 days and, after reaching a maximum of **500**, decreases in the second 250 days to reach **0** again at 500 days from the minimum day. Since every supplier uses the same technology, the graph is common to all of them:



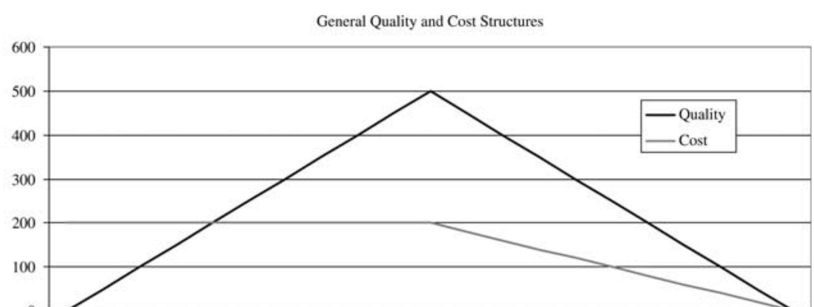
(8) Notice that your costs and revenues are directly related to your production time decision. Graph 4 shows this relationship more clearly:



(9) Since the production minimum day is individually assigned, every supplier will have different starting points for their costs and quality functions. Graph 5 represents an example of **four possible production intervals for a particular group**:



(10) In each round **you must decide** the production time of your component. Your payoff will be the difference between the price paid by Alpha and your production cost. **Graph 6** adds to the generic **cost and quality functions**, a table with some associated values for the production time (in intervals of 25) into the allowed range. However, notice that you can choose any time into your range:



	dia min	min +25	min +50	min +75	min +100	min +125	min +150	min +175	min +200	min +225	min +250	min +275	min +300	min +325	min +350	min +375	min +400	min +425	min +450	min +475	min +500
Quality	0	50	100	150	200	250	300	350	400	450	500	450	400	350	300	250	200	150	100	50	0
Cost	200	200	200	200	200	200	200	200	200	200	200	180	160	140	120	100	80	60	40	20	0

As we pointed out above, your **variable payment** from Alpha depends on the average quality of the group components. Alpha does not verify the individual quality of each component but only the average. Thus, the variable price paid by Alpha is exactly the average quality of the components and equal for all suppliers of the same group regardless of their individual quality. Remember that Alpha also pays a fixed payment of 200 ECU.

As described above, your benefit depends on the decisions made by all members of your group. The following table calculates the **benefits** associated with each decisions profile:

			Delivery time and average quality of the other three members				
	Delivery	**Quality**	***Min day*** **0**	***Min + 125*** **250**	***Min + 250*** **500**	***Min + 375*** **250**	***Min + 500*** **0**
Your delivery time and quality	*Min day*	0	0	187.5	375	187.5	**0**
	Min + 125	250	62.5	**250**	437.5	250	62.5
	Min + 250	500	125	312.5	500	312.5	125
	Min + 375	250	162.5	350	**537.5**	350	162.5
	Min + 500	0	200	387.5	575	387.5	200

Some examples will help to understand the rule.

(a) If you deliver at Min + 125 and the others do as well, the average quality will be 250. Thus, Alpha will pay 450 (average quality + fixed payoff) to every supplier. Since your costs are 200, your benefit will be **250**:

Benefit	=	Revenues		−	Cost	
		(Fixed +	Variable)			
B○	=	(200 +	250)	−	200	= **250**

(b) If you deliver at Min + 375 and the others deliver at Min + 250, the average quality will be 437.5 (that is, (250 + 500 + 500 + 500)/4). In this case, the price paid by Alpha will be 637.5 and your benefits will be **537.5** because your cost is 100:

Benefit	=	Revenues		−	Cost	
		(Fixed +	Variable)			
B○	=	(200 +	437.5)	−	100	= **537.5**

(c) If you deliver at the minimum day and the other group suppliers deliver at min + 500, the average quality will be 0 and Alpha will only pay the fixed revenue of 200. Since your cost is 200, you will gain zero benefits (**0**).

Benefit	=	Revenues		−	Cost	
		(Fixed +	Variable)			
B○	=	(200 +	0)	−	200	= **0**

11. After each round you will receive information about the average quality in your group and your payoffs (including your production cost).

12. At the end of the experiment, the sum of your individual payoffs over the 20 rounds will be privately paid to you at the exchange rate of 500 ECU = 1€.

Instructions (LC treatment, RP game)

(1) This second experiment also consists of 20 independent rounds. As in the previous session, you are a supplier of Alpha. You are allowed 500 days from your minimum day (the same as in the earlier session) for delivery. The other three suppliers of your group are also the same as before.

(2) Your benefit will also be the difference between the price paid by Alpha and your production cost. The main difference with respect to the previous experiment is the introduction of an additional mechanism affecting the variable payment. Concretely, whereas the fixed payoff is maintained at 200 ECU, the variable payoff is determined as follows:

$$R = \frac{\text{Average Quality}}{\text{Maximum Quality}} \times 100 = \frac{\text{Average Quality}}{500} \times 100,$$

where R is a quality rate calculated by Alpha.

(3) This rate determines the probability that each group member has to be a beneficiary of the variable payoff. That is, every subject gets the average quality with probability R, and 0 with probability (1-R). An example will help you understand the rule. In the case that R is 70, all members of the group have a 70% chance of earning the variable payoff and a 30% chance of receiving nothing.

(4) Notice that now the decisions of all suppliers belonging to the same group affect the variable payment in two ways: i) to determine the average quality; and ii) to calculate the probability of getting such a variable payoff. The following table records this interdependence more clearly:

			Delivery time and average quality of the other three suppliers				
	Delivery	**Quality**	***Min day*** **0**	***Min + 125*** **250**	***Min + 250*** **500**	***Min + 375*** **250**	***Min + 500*** **0**
Your delivery time and quality	Min day	0	0 (100%)	187.5 **(37.5%)** 0 (62.5%)	375 **(75%)** 0 (25%)	187.5 **(37.5%)** 0 (62.5%)	0 (100%)
	Min + 125	250	62.5 **(12.5%)** 0 (87.5%)	**250 (50%)** 0 (50%)	437.5 **(87.5%)** 0 (12.5%)	250 **(50%)** 0 (50%)	62.5 **(12.5%)** 0 (87.5%)
	Min + 250	500	125 **(25%)** 0 (75%)	312.5 **(62.5%)** 0 (37.5%)	500 **(100%)**	312.5 **(62.5%)** 0 (37.5%)	125 **(25%)** 0 (75%)
	Min + 375	250	162.5 **(12.5%)** 100 (87.5%)	350 **(50%)** 100 (50%)	537.5 (87.5%) 100 (12.5%)	350 **(50%)** 100 (50%)	162.5 **(12.5%)** 100 (87.5%)
	Min + 500	0	200 (100%)	387.5 **(37.5%)** 200 (62.5%)	575 **(75%)** 200 (25%)	387.5 **(37.5%)** 200 (62.5%)	200 (100%)

Some examples will help you understand the rule:

(a) If you deliver at Min + 125 and the others do too, the average quality will be 250 and **R = 50%**. Your possible benefits will be:

Benefit	=	Revenues (Fixed + Variable)	–	Cost		
B○	=	50% (200 + 250)	–	200	=	**250**
		50% (200 + 0)	–	200	=	**0**

(b) If you deliver at Min + 375 and the others at Min + 250, the average quality will be 437.5 and R = 87.5%. In this case, your possible benefits will be:

Benefit	=	Revenues (Fixed + Variable)	–	Cost		
$B^{\bigcirc}$	=	87.5% (200 + 437.5)	–	100	=	**537.5**
		12.5% (200 + 0)	–	100	=	**100**

13. After each round you will receive information about the average quality in your group, your payoffs (including your production cost) and the R value.
14. At the end of the experiment, the sum of your individual payoffs over the 20 rounds will be privately paid to you at the exchange rate of 500 ECU = 1€.