Blind Fines in Coops[1]

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Abstract

Cooperatives are a common business form in the agricultural sector all around the world. They confer economies of scale, risk insurance and market power to agricultural farmers. However, cooperatives also have drawbacks. In this paper we focus on inefficient product quality arising from a free-riding problem. Individual incentives are not aligned with group gains in coops because individual members bear all the cost of offering higher qualities whereas higher qualities benefits are shared among all members. We present a blind mechanism whose quality-enhancing properties are analyzed in a theoretical model. This mechanism, that does not need individual monitoring, achieves significant efficiency gains using experimental methods.

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Abstract

Cooperatives are a common business form in the agricultural sector all around the world. They confer economies of scale, risk insurance and market power to agricultural farmers. However, cooperatives also have drawbacks. In this paper we focus on inefficient product quality arising from a free-riding problem. Individual incentives are not aligned with group gains in coops because individual members bear all the cost of offering higher qualities whereas higher qualities benefits are shared among all members. We present a blind mechanism whose quality-enhancing properties are analyzed in a theoretical model. This mechanism, that does not need individual monitoring, achieves significant efficiency gains using experimental methods.

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The analysis of non-market organizations remained outside the economic scope until the arrival of informational economics in the 70's. Once strategic situations were tackled by game theoretical tools, the analysis of virtually any economic institution became possible. One of the first institutions to be analyzed was *teams*, a basic unit inside firms; Alchian and Demsetz (1972) pointed out the existence of a *free rider* problem whenever actions (i.e. efforts) of the team members were not observable. The basic problem is that in a team, efficiency gains coming from individual actions are shared by all team members whereas the effort costs are born individually.

This moral hazard problem in teams is in fact relevant to one of the most important forms of business in agriculture activities around the world: the *agricultural cooperatives*. In the US, approximately one third of all agricultural products are marketed through cooperatives,[1] whereas this ratio rises to almost half of the production in Western Europe, reaching even higher figures in some southern European economies. [2]

It is hard to argue about the potential benefits of forming a cooperative. The key incentives are a lower variance in output prices, economies of scale and increasing market power; see for example Olson (1965). However, there are also weaknesses linked to the principles that define the cooperative business. Cooperatives are owned and controlled by the people who use it, being all benefits shared by their users.

These principles provide coop members a conflictive behavioral benchmark, in terms of economic incentives. Think for example of an individually costly action improving the cooperative profits. As decisions are decentralized, any rational member will perform the action whenever the incurred cost is smaller than her revenue. As benefits are equally shared by all members, individual profits are smaller than cooperative profits by definition. So, there will be actions that increase collective profits which are not carried out by rational coop members. This is in essence the *free riding* problem which is embedded in the coop nature.

As there are many ways in which individual actions could potentially increase collective profits, the free rider problem is multidimensional and potentially severe. In this paper we focus on a specific dimension of the free riding problem: those actions that affect the product quality of the cooperative. Better qualities are rewarded by consumers with larger prices, therefore generating larger profits to agricultural coops. But, good quality usually comes only at an individual cost.[3]

The free-riding phenomenon associated to product quality has been intensely analyzed in the literature on cooperatives; see for example Phillips (1953). The basic story is nicely captured by the Alaskan (wild) Salmon Industry as stated by Babcock and Weninger (2004) "… suppose two fishermen deliver to a single processor. The fishermen know that part of the investment in quality that increases price will end up in the pocket of the other fisherman. The two fishermen get roughly a half-share of the benefit of quality-control efforts, yet both bear the full cost of those efforts". As a consequence, individual efforts to provide high quality salmon are not exerted, the quality offered is low, the market price is low and finally profits to fishermen are accordingly low. Another good example is provided by Frick (2004) regarding wine production in Germany. In a study based on 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.

An alternative instance in which free-riding in product quality plays a fundamental role is the so called *Protected Designations of Origin* (PDO) labels and *Protected Geographical Identification* (PGI) labels. PDO and PGI labels have been put in practice in both the US and the European Union as a strategy to differentiate products on the basis of excellence and quality standards. Washington Apples or Andalusian Products are two well known examples of PGI and PDO, respectively.

Products under PDO and PGI are usually experience goods whose quality is unknown to the consumer before purchase. Consumers must then rely on the reputation of the product, i.e. on the quality attached to the label. Being quality individually costly, *free riding* in the form of offering low quality products under the umbrella of the PDO or PGI label is prevalent. As a consequence, the quality of these label-protected products naturally declines in the long run. Quagrainie, McCluskey and Loureiro (2003) support this idea in their analysis of the declining quality of Washington Red Delicious apples, based on daily observations from a number of cities from July 1996 to November 1999.

Some alternatives have been proposed to tackle this free-riding problem in cooperatives. Minimum quality standards are mentioned by Winfree and McCluskey

(2005): “the apple industry in Washington should consider establishing minimum standards for what constitutes eating quality… Indeed, the organization formerly known as the *Washington Apple Commission* has proposed to create standards for a new elite apple”. Babcok 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 by the market. A supposedly cheaper alternative, also considered by the *Alaska Quality Seafood Program* is to monitor and estimate the salmon quality delivered by each individual and pay fishermen accordingly.

The key underlying idea of these mechanisms is to deter free-riding by threatening with the exclusion. Low quality providers will not benefit from the effort of high quality providers. Free riders are then excluded from the cooperative, from the protected-label program or from the high-quality program. It is interesting to notice that these solutions are in line with the ideas arising from theoretical analysis of moral hazard in teams. Holmström (1982) was the first paper to propose full exclusion from the team whenever the observed output is not the efficient one as a mechanism to implement the efficient solution. Rasmusen (1987) proposed the *massacre* and the *scapegoat* contracts. In the former, all but one randomly chosen member of the team are penalized to pay a positive amount of money to the survivor, which in addition collects the joint product. In the latter, all but one (randomly chosen) shares the joint product and the penalty paid by the Guinean pig.

In all these mechanisms proper monitoring of individual quality and enforcement of the consequences of misbehavior are needed. But, there are at least two reasons which might advise against its practical implementation: technical unfeasibility and prohibitive implementation costs. In the salmon example, discerning salmon quality on board is a challenging task and the benefits of higher qualities might not always countervail the monitoring costs. [4]

In this paper we propose a mechanism which does not require individual monitoring. The intuition is to estimate the quality of the coop product[5] and to compare it to the highest quality that might be achieved under full observability conditions. This comparison reveals the *success ratio* of the coop. This ratio is a measure of the coop benefits both low and high quality providers are going to make. We then pay each member of the cooperative according to this ratio, with a blind probabilistic punishment. Regardless of their individual quality, all members get her share of the coop benefits with a probability that matches the coop success ratio.

As it will be discussed below, our mechanism has some interesting and balancing features. It is blind in the sense that it does not discriminate between high and low quality providers. But, it does not require any individual information about performance. It exclusively relies on a collective measure of performance (the success ratio). But, this collective measure determining exclusion is endogenously generated by the cooperative members. In our mechanism 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 joint benefit.

In the investigation of the properties of our blind mechanism we perform a two-fold exercise. As a first step, we develop a theoretical model of an agricultural cooperative with an arbitrary number of members in which the incentive problem referred to in this introduction clearly appears. In the model, each member decides on the quality of the product that he delivers to the coop and the coop product quality is an increasing function – more specifically is the average- of the delivered products qualities. The unique Nash equilibrium of the model involves each member offering the lowest quality and as a consequence, the product offered by the cooperative locates at the bottom of the quality space.

Once the free rider problem is established, we introduce in the model our *blind* mechanism. For some regions of the parameters space, we show that a new Nash equilibrium appears whose outcome corresponds to all coop members delivering the highest quality. The game is transformed from a theoretical point of view. Rather than a single inefficient equilibrium in which all coop members provide low quality inputs, we do have now a game with two different equilibria in pure strategies: an efficient but strategically risky one and a safe but inefficient equilibrium solution.

In a second step, we use the experimental methodology to understand the equilibrium selection process.[6] We specifically test for the effectiveness of our mechanism in alleviating the incentive problem in a computerized environment. Our experimental results suggest that in the coop model without the blind mechanism, the average quality delivered by the members of the coop declines over time until settling around 35-40% of the highest possible quality. However once the mechanism is introduced, its quality-enhancing properties are shown as the average coop quality does hold around 70% of the maximum quality in the long run. To control for the psychological effects of the blind punishment mechanism (subjects choose higher qualities just because of 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 the introduction of the blind mechanism is null, in this control case.

The rest of the paper is as follows. Section 2 surveys recent experimental literature on team production and punishment to introduce the novelty of our mechanism and test. Section 3 carries out the theoretical exercise by putting forward a coop model in which the theoretical properties of the blind mechanism are investigated. Sections 4 and 5 evaluate the theoretical predictions through an experimental setting and present and discuss the main experimental results. Finally, section 5 concludes.

Experimental Background

In this paper we use the methodology of experimental economics. Experiments in economics differ from those in other fields in two aspects. First, subjects are paid in cash their earnings in the experiment. This practice, first termed “induced valuation” by Smith (1876), ensures that the incentives assumed in the models are salient for the participants. Because their earnings are real, the decisions they make are consequential for them, reducing or eliminating some of the problems associated with experiments in other fields (experimenter demand, etc.). The second way in which economics experiments differ is that we do not deceive our experimental participants. This practice is designed to ensure that subjects are playing the game we think they are playing, and not attempting to uncover the ‘true purpose’ of the experiment.

Economics experiments have several advantages in studying team production and cooperatives. An experiment is akin to a formal theoretical model, in that it is an abstraction from the world, simplifying in order to focus on key aspects or elements of a particular situation. Thus, like their mathematical cousins, experiments are not “realistic” simulations of cooperatives, but rather are designed to isolate and examine separately the critical aspects of particular situations. This ability to isolate one or more key factors is one advantage of the experimental approach.

A second advantage is the superior control over the data generating process that the lab affords. The quality of microeconomic data on cooperatives is especially difficult to control in the field, because of the incentives of coop members to misrepresent their information about real production costs and product quality. In the lab we avoid these problems, and also gain the ability to build experimental models that replicate most of the assumptions of formal models, thus testing the theory ‘on its own domain’. As Plott (1986) has pointed out, if a model is true in the field, it should also be true in the lab.

However, experiments have limitations as well. In particular, the question is always raised about whether the behavior of subjects (typically students) in a laboratory setting sufficiently resembles the relevant cooperative situation. We minimize this limitation by not merely examining behavior, but focusing instead primarily on ‘comparative statics’ – i.e., how behavior changes when the conditions change. While cooperative members may be different from the general population, and may play a given game differently, there is no reason to think that their reactions to changes in the game will be different.

Our experimental design incorporates a special, probabilistic, kind of sanctions based on the temporary exclusion of some group members. It is not surprising that our results go in line with previous results on punishment and exclusion. Exclusion has received considerable attention in recent years in the economics literature. But, it can be considered as a fairly common 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; societal defectors are incarcerated or expelled (Hirshleifer and Ramusen 1989); and countries that violate international conventions are boycotted. Some 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% and exclusion of the bottom 10%.

Some recent papers have investigated exclusion in the laboratory. Swope (2002) and Kocher, Sutter and Waldner (2005) implement group exclusion if subjects cooperate below an exogenously predetermined level, chosen by the manager. Exogenous exclusion initially increases cooperation but cannot avoid a decrease with repetition. A related literature examines a variant of exclusion, endogeneous group formation. Cinyabuguma, Page and Putterman (2005) allow participants to expel group members based on a majority vote. Their results show high levels of cooperation among non-expelled members. In other experiments, individuals decide in which group to participate as in for example, Erhard and Keser (1999) and Ahn, Isaac and Salmon (2005). Charness and Yang (2007) consider exclusion of individuals and merger of groups by vote.[7]

Another well known branch of the literature focuses on a different type of horizontal punishment: social sanctions among peers in public good games. Punishment is decentralized in these experiments; that is, it is carried out by individuals which are not governed by a central authority. In this literature punishment reduces earnings of individuals punished, but it is also costly for punishers. Cooperators punish those who violate the pro-social norm of cooperation, or defectors punish those who try to establish such a norm (Fehr and Gächter 2000; Gintis 2000; Masclet et al. 2003; Carpenter and Matthews 2004; Carpenter, Matthews and Ong'ong'a 2004; and Gächter and Herrmann 2005).

A punishment mechanism was first experimentally analyzed in economics[8] by Ostrom, Walker and Gardner (1992) in a common-pool resource setting and by Fehr and Gächter (2000) in a public good experiment. In both cases, subjects can send costly punishment *points* to the other group members to reduce the recipient's income. The results show that the ability of players to monitor and punish each other improves the level of cooperation significantly. Interestingly, the efficiency enhancing result of social punishment critically depends on the existence of complete information. In the original paper by Fehr and Gächter (2000), all subjects get full information about the individual decisions of every other subject in their group. Fatas, Melendez-Jimenez and Solaz (2008a) suggest that this positive effect no longer holds under incomplete information.

Croson, Fatas and Neugebauer (2008) analyze an alternative version of punishment in different team production games. In all games, punishment is based on competitive exclusion. The worst performer is excluded from the benefits of team production, so a competition between group members determines who is (not) going to be punished. Their experimental results show that excludability produces large increases in contribution and, in the right conditions, pareto-efficiency. Note that full information about individual contributions is not needed to implement exclusion. A simple ordinal ranking of individual contributions suffices.

Our blind mechanism differs from the ones previously considered in the literature in a number of respects. First, and contrary to Croson, Fatas and Neugebauer (2008) and Fehr and Gächter (2000), the individual information requirements are null. Within each group, all members share the same probability of being punished, so no individual information is needed to be implemented. This dramatically reduces the implementation costs. The cooperative manager does not need to observe the quality delivered by the coop members, neither using an ordinal or a cardinal measure. These lower informational requirements are most likely the reason why excludability has been observed in the field. In addition, it involves lower informational requirements than does exogenously fixed exclusion. The manager does not need to determine in advance the threshold below which individuals will be excluded (how low is too low?).

Second, the probability of being excluded comes from the own group's performance, and if effective, its potential gains are unbounded. Exclusion based on exogenously determined thresholds has more than one contractual structure. Everyone knows in advance how much they need to cooperate to be included, and many cooperate at exactly that minimum level. Our probabilistic mechanism makes uncertainty to work in favor of increasing levels of cooperation; the only way to fully eliminate the risk of being excluded is to reach the maximum contribution in the group.

Third, random punishment does not depend on the willingness to pay for punishing. It is not based on peer pressure, as in Fehr and Gächter (2000), but it is applied on a vertical way. This makes the success (or failure) of the mechanism independent of the existence of strong punishers. Fourth, a slight modification of the theoretical prediction gets into the analysis by the backdoor of risk. For extreme risk adverse players, fully contributing to the public good is an equilibrium strategy. And last but not least, in the present paper we do not consider permanent firing but rather assume that some contributors are temporarily excluded from the benefits of group production. The incentive scheme of random exclusion is equivalent to within-group competition for benefit sharing. In the context of a firm, our design translates to the exclusion from profit sharing or stock options.

In contrast, there are some potential dark sides in our random exclusion device. Although being common, exclusion has met with much controversy, and in particular, critics contend that excludability can discourage teamwork. Moreover, our mechanism might be perceived as procedurally unfair by the best cooperative performers. It is interesting that some of the cooperation enhancing properties of sanctioning mechanisms cannot be understood out of a deep analysis of its psychological dimensions. The desire to induce higher delivered qualities to coop, and thus ensure a higher individual return in future periods, constitutes a strategic motivation for punishment. However, since subjects also punish when they are certain that they will not interact again with the same person in the future, non-strategic motives, such as emotions, have been suggested as alternative explanations. Masclet et al. (2003) find that both material and non-material sanctions increase contribution levels by a similar amount in the short run, but material sanctions are more effective in the long term to increase contribution levels.

We consider the psychological dimension of punishment in our study. On the one hand, we expect to find hot behavioral responses by top performers when punished. Negative emotions such as anger, may become one non-strategic motive for individuals to stop sacrificing to improve performance. In the opposite direction, Hopfensitz and Reuben (2005) emphasize the importance of social emotions such as shame and guilt in the punished as an essential component for the successful enforcement of cooperation. So, these hot emotions induced by the blind punishment procedure might generate a positive reaction by those deserving to be punished.

The psychological aspects of our mechanism were considered in Fatas, Morales and Ubeda (2008b), where a similar blind mechanism was implemented in an abstract and much simpler public good setting. There, it was found that subjects react to the unfair procedures by polarizing their individual behavior towards full or null contribution to the public good. This polarized behavior is also present in our coop setting. In fact, this could help to explain why the blind mechanism does not promote product quality all the way up, but it stays around 70%.

The theoretical model

In this section we present the basic model of an agricultural cooperative, describing the free-riding problem that lies at its heart. We then introduce the punishment mechanism based on blind fines and analyze the efficiency properties.

The basic game

An agricultural technology is composed of a crop quality function together with a harvesting cost function. Both functions depend on the harvest time *t*. Consider the following canonical technology

$$(1) \qquad q(t;t^*,Q) = \begin{cases} \left(\frac{t}{t^*}\right)Q & if \quad 0 < t < t^* \\ \left(2 - \frac{t}{t^*}\right)Q & if \quad t^* < t < 2t^* \\ 0 & otherwise \end{cases}$$

$$(2) \qquad c(t;t^*,Q) = \begin{cases} c & if \quad 0 < t < t^* \\ \left(2 - \frac{t}{t^*}\right)c & if \quad t^* < t < 2t^* \\ 0 & elsewhere \end{cases}$$

Note that the growing period for this canonical technology spans from 0 to 2t* and that the highest crop quality (Q) is achieved at harvest date t* (hence 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 first flat and once the efficient harvest time is surpassed, it decreases in a linear fashion, vanishing at the end of the growing period. We assume that Q>c so that production is profitable.[9] Figure 1 contains a graphical representation of the basic technology.

INSERT FIGURE 1 AROUND HERE

From the above canonical technology, we next define a family of technologies F(T) indexed by T whose members are all possible translations of the basic technology to the right. Hence, the technology F(T)={q(T+t*), c(T+t*)} has the same features of the canonical technology although its growing period starts at T and ends at T+2t*.

We assume that every member of a coop of size n is endowed with a member of the technology family F(T) for crop growing. Hence, the coop 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 (potentially) different growing period.[10]

We finally assume that members' profits are given by the average quality of the coop crop minus the harvesting cost. Note that individual efficient harvest is advantageous to the group (as it increases the average quality) but the associated higher costs are fully bear at individual level. In fact this is the essence of the free-riding problem as we next show.[11]

We now aim at characterizing the Nash equilibrium of the game played by the members of the crop cooperative. To this end, we compute the best response function of a member of the cooperative. Without loss of generality we assume that member i is endowed with the canonical technology.[12]

It is easy to see that all harvest times smaller than the efficient date t* are dominated by t*, i.e. they all yield smaller profits because they all entail the same cost c but yield poorer qualities than t*. Hence we can restrict attention to harvest times t equal or larger than the efficient date t*. The profit function becomes

$$(3) \qquad \pi_i(t_i, t_{-i}) = \left(2 - \frac{t_i}{t^*}\right)\left(\frac{Q}{n} - c\right) + \frac{\sum_{j \neq i} q_j(t_j)}{n} \qquad for\ t^* \leq t_i \leq 2t^*$$

Given that the growing period extends at most until *2t**, the best response function of player *i*, which is independent of the quality offered by the remaining members of the cooperative, is driven by the sign of *(Q/n) – c*.

$$4) \qquad BR_i(t_{-i}) = \begin{cases} t^* & if\ \dfrac{Q}{n} > c \\ 2t^* & if\ \dfrac{Q}{n} < c \end{cases}$$

As we can see, whenever the per capita return for producing the highest quality exceeds the highest harvesting cost c, the dominant strategy is to harvest at the efficient date t*. Otherwise, members' dominant strategy is to offer the lowest crop quality. This is the content of Proposition 1.

Proposition 1. Efficient harvest time occurs whenever c < Q/n. Otherwise the equilibrium quality is the lowest.

Proof: It easily comes from the best response function.

Note that the coop organization offers the lowest quality in parameters configurations for which individual farmers would achieve the efficient crop quality.[13] It is precisely this inefficiency that leads us to focus on alternative mechanisms.

The random punishment game

Assume now that with some exogenous probability *p*, a member of the cooperative is not excluded from receiving the cooperative share. Then expected profits for member *i* becomes[14,15]

$$\pi_i(t_i, t_{-i}; p) = \left(2 - \frac{t_i}{t^*}\right)\left(\frac{pQ}{n} - c\right) + p\frac{\sum_{j\neq i} q_j(t_j)}{n} \qquad for\ t^* \leq t_i \leq 2t^* \tag{5}$$

Note that the introduction of an exogenous probability of exclusion worsens the situation (relative to the basic model) as now the Nash equilibrium yields the Pareto efficient outcome only when the *expected* per capita quality exceeds the highest harvesting cost c.

Some advances are obtained by making endogenous the exclusion probability. Of course, the most direct way would be to exclude those members whose quality is lower than the highest quality Q.[16] However, this mechanism would require a perfect 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. These strong informational requirements render it impossible to apply in practice.

In this article, we propose a mechanism applicable when the growing periods of coop members are private information. The key element is to rely on a variable which is observable without cost: the *success ratio* of the cooperative (*R*). It is defined as the ratio of the realized quality of the coop product over the highest possible quality Q. We propose to set the exclusion probability to *1-R*.

$$R = \frac{\sum_j q_j(t_j) \big/ n}{Q} \tag{6}$$

This mechanism links the exclusion probability to the collective performance and therefore it should not be interpreted as unfair at a collective level. However, given that the exclusion probability is not directly linked to the individual performance, the mechanism might be perceived as unfair at the individual level. This duality will play a crucial role in the explanation of the experimental results.

Under the blind mechanism, expected payoffs are

(7) $$\pi_i(t_i,t_{-i})=\frac{\left[\left(2-\frac{t_i}{t^*}\right)Q+\sum_{j\neq i}q_j(t_j)\right]^2}{Qn^2}-\left(2-\frac{t_i}{t^*}\right)c \qquad \textit{for } t^*\leq t_i\leq 2t^*$$

Given that expected payoffs are convex,[17] the best response function picks one of the two corners of the restriction $t^*\leq t_i \leq 2t^*$. Some algebraic exercise shows the following bidirectional implication

(8) $$\pi_i(t^*,t_{-i})\geq \pi_i(2t^*,t_{-i})\Leftrightarrow \sum_{j\neq i}q_j(t_j)\geq\frac{cn^2-Q}{2}$$

Hence, the best response function for player *i* in the coop game with blind fines is the following

(9) $$BR_i(t_{-i})=\begin{cases} t^* & \textit{if } \sum_{j\neq i}q_j(t_j)\geq\frac{cn^2-Q}{2} \\ 2t^* & \textit{otherwise}\end{cases}$$

As opposed to the basic game, the best response function *does* depend on the expected behavior of her coop mates. Next proposition shows the equilibrium structure of the coop game with blind exclusions.

Proposition 2. The Nash equilibrium structure of the game is as follows:

– If $c<\frac{1}{n}\frac{Q}{n}$ then the unique equilibrium is the efficient one.

– If $c\in\left(\frac{1}{n}\frac{Q}{n},\frac{2n-1}{n}\frac{Q}{n}\right)$ then there are two equilibria, the efficient and the inefficient.

– If $c>\frac{2n-1}{n}\frac{Q}{n}$ then the unique Nash equilibrium is the inefficient one.

Proof: It easily comes from the best response function.

We next compare the efficiency properties of the mechanism relative to the basic game and the individual farmer decision problem. Figure 2 graphically illustrates this comparison.

INSERT FIGURE 2 AROUND HERE

As we see, the positive side of the mechanism is that it enlarges the set of parameter constellations for which the efficient quality is provided by the coop. The efficient outcome extends now up to harvest cost $\frac{2n-1}{n}\frac{Q}{n}$, which roughly speaking amounts to 2 times the previous burden. The negative side is that it extends the efficient outcome without eliminating the inefficient equilibrium. This means that the mechanism generates a coordination problem with two Nash equilibriums.

Before proceeding to the next Section in which we explore the merits of the mechanism with blind fines in an experimental setting, some words on risk attitudes must be said. Our theoretical analysis has been done under the assumption that coop members are risk neutral. It is however well known that risk aversion it is an inefficiency source in many aspects of agricultural activities and therefore the measure of risk attitudes in the agricultural sector has been a hot research topic in the last thirty years (see for example Just and Pope 2003). Here, we would like to emphasize that the inefficient provision of product quality in a coop is related by no means to risk attitudes. However, the blind fine mechanism introduces random payoffs and therefore calls for the analysis of the impact of risk attitudes on the equilibrium structure of the blind mechanism. To this respect, it must be said that the presence of risk aversion strengths the efficient equilibrium, as a unilateral deviation from it is more severely penalized in terms of utility because it introduces a positive probability of been excluded from the coop share.

Experimental Design and Procedures

Our experiment consists of two games and two treatments. The two games are the basic game (NP hereafter) and the random punishment game (RP hereafter) introduced in the previous section, with the following parameters values: n=4, Q=500 and t*=250. The two treatments correspond to two different cost values: c=200 (LC treatment) and c=300 (HC treatment).

In both treatments, players always went through the same sequence: first, they played the NP game for twenty periods and then they played the RP game for another twenty periods. Beginning with Fehr and Gächter (2000), the literature strongly suggest that no mayor order effects are present in this game. The cooperation enhancing effect is independent of whether it was played before or after a similar game with no punishment. Fatas, Morales and Ubeda (2008b) and Eckel, Fatas and Wilson (2008) find similar results. So, we opt for making all subjects go through the same sequence of blocks. The main elements of the experimental design are summarized in table 1 below.

INSERT TABLE 1 AROUND HERE

In each period, each player had, in addition to her profits, an additional fixed payment of 200 ECU (experimental currency units, all ECU were privately changed at the end of the experiment for real money). The values of the treatment variable were chosen in light of the equilibrium structure of the NP and RP games as shown in figure 3. As we see, for cost values between 125 and 218.75 the NP game has a unique (and inefficient) Nash equilibrium whereas the RP game displays a second (and efficient) Nash equilibrium. Hence, the treatment design is devised to test for the effectiveness of the blind mechanism in promoting high quality products.

INSERT FIGURE 3 AROUND HERE

Our study reports the results of computerized experiments conducted at LINEEX, the experimental laboratory at the University of Valencia, using z-Tree.[18] Subjects were recruited electronically with the LINEEX web based system. None of them had ever participated in a similar experiment before, even when some of them had already participated in different experiments. By participating in the experiment a participant made an average of US$42, and experiments took around 90 minutes to run.

Complexity in our experiment is relatively high. So, one of our mayor concerns was to guarantee that subjects fully understood the rules of the game, without conditioning their decisions. 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.[19] After the experiment, subjects were debriefed by an on screen questionnaire including some questions about their strategies (just to double check that they did understand the structure of incentives). Given their replies and the procedure, we are confident that both the tasks and the incentives were understood by subjects.

Participants were assigned to one treatment when recruited. When they reached the laboratory, they were randomly allocated to a cubicle choosing a numbered chip (with cubicles' numbers) from a basket. At the beginning of every session they were randomly allocated to a group of four and remained together throughout the whole experiment. As all subjects went through the same sequence of games, every subject participated in 2 blocks of 20 rounds. The experiments entailed 20 initial rounds of play (the No Punishment game, NP, without any punishment mechanism) and then, after a surprise restart, another 20 rounds (the Random Punishment game, RP, with the blind punishment device). The surprise restart technique has been widely used in the experimental literature (see Croson, 2000, for details). In our setting, that meant that 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, 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 stay and participate in another experiment. All subjects decided to stay in the second block of this experiment, but this has not been always the case, see Brandts, Fatas and Lagos (2008).

After each round, subjects received information about their past individual delivered quality and their own earnings. They also got information about their group performance (average quality) and whether or not they had been randomly punished, in the RP block. At the end of the experiment, all subjects were privately paid using sealed envelopes with the number of their cubicle typed in the outside. It was impossible to link names and decisions.[20]

Results and Discussion

We start this section presenting a descriptive analysis of our data. Subsequently, we will carry out a more formal analysis. Figure 4 shows the relative average quality (R) in the first block of 20 rounds, with no punishment, both for treatments LC and HC. The quality of the final product is positive and far from zero even in the last rounds. In the LC treatment, the success ratio starts around 80% of the maximum quality and declines towards 40%, on average. The decreasing trend also happens in the HC treatment; but, not surprisingly, at a lower quality level. Subjects react to the larger opportunity cost of supplying a high quality product. While the LC average quality represents 55.5% of the maximum quality, it is only around 40% in the HC treatment.

INSERT FIGURE 4 AROUND HERE

Figure 5 plots the average quality ratio for both blocks of 20 rounds, including the second one, when individuals faced the random punishment mechanism. After the surprise restart, the picture changes in a remarkable way. 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 around 65% to 20%. The average quality accounts for 40.58% of the maximum level, quite similar to the one observed in the first block. However, in the LC treatment, the average quality ratio does hold around 70% after a slight decrease in the first six rounds. The success ratio is roughly doubled, on average. [21]

INSERT FIGURE 5 AROUND HERE

Running a more formal analysis, we estimate a panel data model, with random effects at the individual level and group clusters. The data set includes repeated observations from the same individuals which can't be treated as statistically independent. For this reason, the standard errors are corrected for clustering at the group level (see Liang and Zeger, 1986). Each group is treated as a separate cluster. Table 2 shows the results of our estimations by blocks of rounds. Dcost is a dummy variable to account for treatment effects: it takes the value of 1 when the treatment is HC, 0 otherwise. Period takes values from 1 to 20 (the number of rounds within each block), while Quality1 is the quality delivered by every subject in the first period, to control for subjects' types. The dependent variable is the individual quality delivered to the coop in every period.

INSERT TABLE 2 AROUND HERE

We find a significant treatment effect in the data, as the negative and significant coefficients of the dummy variable suggests. Subjects choose a significantly lower quality in the HC treatment than in the LC, being this reduction greater in the second block as coefficients in absolute terms indicate (51.24 vs. 126.08). As a common result in the related experimental literature, we obtain a negative and significant coefficient for Period in both blocks. These results are robust to the introduction of subjects' types (Quality1) in the regression. The higher quality subjects provided in the first round, the higher quality they keep providing during the session. This significant effect neither alters nor mediates the treatment effect. [22]

We analyze now the impact of RP on subjects' behavior. One interesting feature of our design is that it allows for contrasting the individual reaction to the random punishment mechanism after experiencing a *cooperation failure* in the first block. Table 3 presents the estimations of our within-subject analysis. Block is a dummy variable that takes the value of 1 when RP is implemented (second block of 20 rounds), 0 otherwise. It is always significant and positive, what means that subjects increase significantly the quality when the environment incorporates the blind punishment. Moreover, in our models, RP increases the quality contributed around 50% (145 over 317 in LC and 180 over 280 in HC).

INSERT TABLE 3 AROUND HERE

Table 3 suggests that RP makes the success ratio to significantly increase in both HC and LC. However, it is interesting to note that it also confirms the patterns pointed out in our descriptive analysis with respect to the different dynamics over time. 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 makes all gains obtained in HC by the introduction of random fines to become dissipated at the end of the second block.

One simple way to account for this effect from a statistical perspective is to check whether the average quality (or equivalently, success ratio) differs over time for each treatment. Tables 4 and 5 show the results of non parametrical tests (Wilcoxon signed-rank tests) applied to our 10 groups in different points in time. We reckon that this analysis provides an interesting complementary result to the one obtained in table 3.

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 (upper index f).[23] Quality is significantly higher in the first round than in the last round in all the comparisons but in LC-RP, where the quality at the end of the 40 rounds is not significantly lower than quality in round 21. The opposite holds for the other three cases. These results support the idea that while no differences are observed in LC when the random punishment device is operating, a significant decline is observed in HC.

INSERT TABLE 4 AROUND HERE

Finally, table 5 provides a comparison between the average qualities provided at the group level at the end of both blocks (with and without random punishment), for every cost condition (high and low). In line with previous results, at the end of the second block of 20 rounds, all gains generated by the random fine mechanism in the HC condition are dissipated through the decline. This is not the case in the LC condition, as the delivered quality is still significantly higher at the end of the second block than at the end of the first one.

INSERT TABLE 5 AROUND HERE

Given that our theoretical analysis establishes an equilibrium selection process, we think it is interesting to have a look at individual decisions. Did subjects coordinate in the LC treatment? Did treatment effects make subjects to play the efficient equilibrium with a higher frequency? To provide an answer to this question, we analyze the distribution of decisions across treatments. Figures 6 to 9 plot the histograms of individual performance across LC and HC treatments by blocks of rounds.

INSERT FIGURES 6 AND 7 AROUND HERE

From figures 6 and 7, both of them referred to LC treatment, it comes easily that individuals choose the equilibrium strategies more frequently in the second block, with RP (about 43%, 10% for the inefficient equilibrium and 33% for the efficient one) than in the first one, with no random punishment (about 15%). Moreover, even though the proportion of times that individuals choose either zero or the maximum quality (500) is similar in Block 1 (around 15% each one), the efficient choice turns considerably into the modal option in Block 2.

INSERT FIGURES 8 AND 9 AROUND HERE

For HC treatment, figures 8 and 9 show that subjects play slightly more frequently the unique Nash equilibrium (zero) with RP than without it (33% vs. 28%). However, choosing the maximum quality is not a modal response when the random punishment mechanism is operating. Almost one third of the times subjects choose the lowest quality in the second block (the treatment mode). We can summarize these histograms in a table. Given that in both cost conditions LC and HC, the quality provided collapses at the end of the first block, we focus on the evolution of equilibrium play in the second block, in which the random punishment mechanism is at play. Table 6 shows that the overall number of times any equilibrium action (0 or 500) is played is roughly the same across treatments (327 vs. 340 in absolute terms, 40.88% vs. 42.25% in percentages). But, the distribution of decisions is remarkably different: while 3 out of four times the highest quality is chosen in LC, this frequency is mirrored in treatment HC, in which subjects choose the minimum quality overwhelmingly.[24]

INSERT TABLE 6 AROUND HERE

Given that subjects strongly react to punishment but in a very different way in each treatment, our last question to be answered is the following: what are the behavioral determinants of the treatment effects? Table 7 provides some valuable information, trying to discriminate about the impact of our punishment within each group. We are interested in understanding whether or not the good coop members react to this kind of unfair, blind punishment. To do so, we estimate again panel data models with the same technique described above, with random effects at the individual level, clustering by groups. To measure the relative impact of RP in different subjects, we control for their performance in the previous round. *Lrp* is 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, 0 otherwise). *Lp* accounts the immediate effect of being punished (it takes the value of 1 when the subject was punished in the previous round, 0 otherwise) and Lrp*Lp is the interaction term.

INSERT TABLE 7 AROUND HERE

In line with some of the results presented above, the decreasing trend is now only significant in the HC treatment, when we control for the subjects' types. The lagged relative performance is significant and positive in both treatments, suggesting the existence of inertia, even when their types are controlled by Quality1. On average, subjects do not seem to react in a hot behavioral way to the immediate experience of being punished. That is, relative to those non-punished in the previous round, punished subjects choose a slightly lower quality, being the difference not significant.[25] However, the interaction coefficient suggests that top performers decrease the quality delivered after being punished. This behavioral reaction is present in both treatments and it is in line with the findings of Fatas, Morales and Ubeda (2008b).

Conclusions

In this article we focus on the free-riding problem in the provision of high quality products within a common organizational form: agricultural coops. Empirical evidence shows that, in some particular sectors, cooperatives offer lower quality product than those firms with a different ownership structure. Related literature, both from theoretical and experimental perspectives, has proposed either exclusion or punishment mechanisms when individual actions are unobservable. In this article, we present a blind mechanism that incorporates both of them for the first time, showing relevant advantages with respect to previous proposals. Our mechanism links the probability of being punished to the group performance in such way that the larger group quality, the lower its members' exclusion probability.

Our starting point is the development of a theoretical model of an agricultural cooperative in which each member privately decides on the quality of product to deliver to the coop. Given that higher qualities come at the expense of higher costs, the unique Nash equilibrium of the model consists of all members delivering the lowest product quality. This equilibrium is highly inefficient as the efficient outcome implies all members delivering the highest quality.

Once the basic setup is built, we introduce a mechanism based on random exclusions: each member is included in the coop benefits with a probability that matches the relative performance of the coop. We are able to characterise in our model those regions of the parameters space for which the efficient outcome is achieved as a Nash equilibrium. Unfortunately, we find a multiplicity of equilibria as we can not get rid of the inefficient Nash equilibrium.

We finally test for the effectiveness of our mechanism in alleviating the free-rider problem by running an experiment. Our experimental coops start from high quality levels – the initial average quality is around 80%- although they all experience a declining trend over time (20 periods) that reduced the success ratio by half. After this initial episode of cooperation failure, we let subjects continue the experiment (surprise restart) with a unique modification: the presence of the blind mechanism.

Those experimental coops for which the inefficient equilibrium was the unique Nash prediction once the blind mechanism was enforced, went again through a similar cooperation failure episode: the mechanism did not make any difference. It was however different for those coops whose experimental parameters implied the existence of the second efficient equilibrium, as their average quality did held around 70% during the rest of the experiment.

We therefore find a strong positive effect of the blind mechanism on the cooperative performance: On average, the quality was doubled, what implies efficient gains of 100%. The reason for this success is that when the random mechanism is present, one out of three individual qualities delivered to the coop corresponds to the highest quality (it is in fact the modal decision), whereas this ratio lowers to one out of six in the absence of the mechanism (the modal decision corresponds to the inefficient Nash equilibrium).

Our experimental results extend our knowledge about sanctions and exclusions mechanisms in a number of directions. Given that misbehaviour occurs at the individual level, it might seem necessary to also monitor at the individual level in order to refrain individual misbehaviour. This is in fact the line pursued by several economic organisations in practice and also propugnated by scholars and governmental offices. See for example the Alaska Quality Seafood Program and the Washington Apple Commission reported in the introduction.

However, this approach is problematic as it faces potentially severe problems. One of them is of economic relevance as the efficiency gains could be offset by high implementation costs whereas a second one is of a technical nature as individual monitoring would require advanced (and therefore expensive) technology.

In this article, we are able to obtain sizeable efficiency gains without resorting to individual monitoring but using aggregate information. In fact, one of the major advantages of the mechanism is that the information requirements are cheap to fulfill because the mechanism is based on the quality of the coop final product. This information can be obtained either directly from the market price if the coop is acting in a competitive market or by performing adequate quality samples whenever the price is not an unbiased signal of the product quality.

There is however an implicit cost built in the mechanism in terms of efficiency. We are unable to obtain the most efficient quality because the punishment scheme is blind, in the sense that it is not targeted to shirkers but to any member regardless of his delivered quality. Therefore it sometimes happens that the mechanism excludes the wrong members, i.e. high quality deliverers, because of its random nature. And these non-deserved sanctions do discourage cooperation although at a lesser extent. Overall, the net effect on product quality is positive.

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[1] USDA 2002 Report on Agricultural Cooperatives.

[2] For example, wine and olive oil in Spain, see the survey on European Agricultural Cooperatives conducted by the General Confederation of Agricultural Co-operatives in the European Union, COGECA, back in 2005.

[3] In fact, the importance of product quality and the aim at offering higher qualities as a surviving strategy by cooperatives has been stressed by several authors, both from the academic, see for example Cook (1995) as well the governmental sector, and see for example the 2002 USDA report on *Agricultural Cooperatives in the 21st Century*.

[4] See a similar challenge in non-point pollution recently studied in environmental economics both from a theoretical–Segerson (1988) and Xepapadeas (1991) – and experimental point of view –Spraggon (2002)-.

[5] This estimation might be obtained either by evaluating the product quality of a sample of members or by relying on the market, i.e. by using the market price as an indicator of the coop product quality.

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

[7] Unfortunately, the effect on cooperation due to this voluntary group selection is negligible, as free-riders infiltrate groups with high levels of cooperation.

[8] Yamagishi (1988) did first in social psychology, although his work is very rarely recognized as the seminal one.

[9] Our main results would remain unaltered if we had considered a more general harvesting cost function, as for example increasing costs up to the optimal harvest time. Our particular election is motivated by simplicity considerations in both the algebraic and experimental arena.

[10] This construct is crucial because in this way the harvest time is by no means a revealing signal of the delivered quality.

[11] In this paper 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 yet it allows a simpler model.

[12] This assumption is harmless because for given agricultural technology F(T), the crop quality is exclusively determined by the harvest time relative to the growing period.

[13] At the outset there are reasons which strongly support the creation of cooperatives. We abstract from them in this paper because our objective is to focus on the tension between individual actions and collective outcomes.

[14] As in any other mechanism, it is important that money not delivered to coop members be allocated to other activities that do not have a direct impact on members' welfare.

[15] As before, harvest times smaller than the optimal date $t*$ are dominated by $t*$.

[16] An exclusion mechanism based on ordinal individual information is implemented in a team production setting by Croson, Fatas and Neugebauer (2008). In their setting, the individual offering the lowest quality is

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excluded from the team profits. Their results strongly support that exclusion raises contributions to the efficient level.

[17] The second derivative is strictly positive, $\frac{\partial^2 \pi_i(t_i, t_{-i})}{\partial t_i^2} = \frac{2Q}{(nt*)^2} > 0$

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

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

[20] Privacy and anonymity were strictly guaranteed. Subjects were never asked to introduce any personal information in the system. Even when they needed to use their names to get electronically recruited, when they reached the laboratory, they were just asked to show an ID picture to one of the assistants and get in, choosing a numbered chip at random. The distribution of subjects was unknown to the experimentalists, so were their decisions. Moreover, we believe that participants perceived this fact. The full protocol is available from authors upon request.

[21] The average quality provided for every round and every group is plotted in figures 1a to 4a, in the appendix. These figures show that the representation of the average quality for every treatment is very informative, as the behavior is remarkably homogeneous across groups.

[22] The treatment effect is robust to the estimation method. A much more conservative non parametrical test yields identical results. Table 1a in the Appendix shows the results of Mann-Whitney tests using our 10 group level independent observations in each round and treatment. As we can see, the average quality is significantly higher in LC than in HC in round 1, round 20, round 21 and round 40.

[23] Table 4 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 offering significantly higher qualities.

[24] Given the complexity of the game, mutual best response actions were rarely observed. Even when groups reached equilibrium according to our intuition, we find the analysis of limited relevance, given the low frequencies. Table 2a summarises this data in the appendix.

[25] 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 those 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 NP block, subjects in RP block deliver significantly higher qualities.

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FIGURES

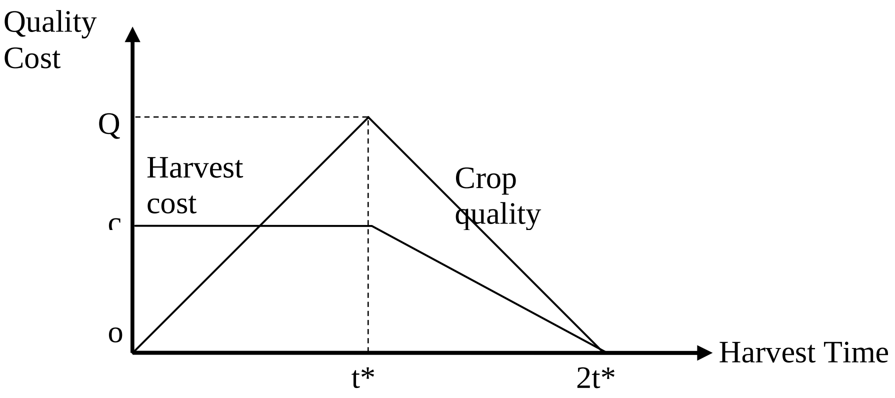


Figure 1. The canonical technology

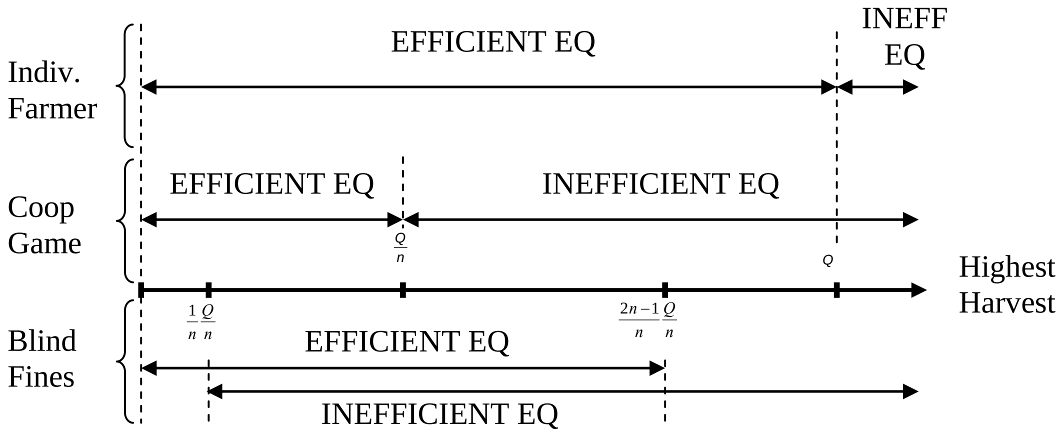


Figure 2. Efficiency properties of the mechanism with blind fines.

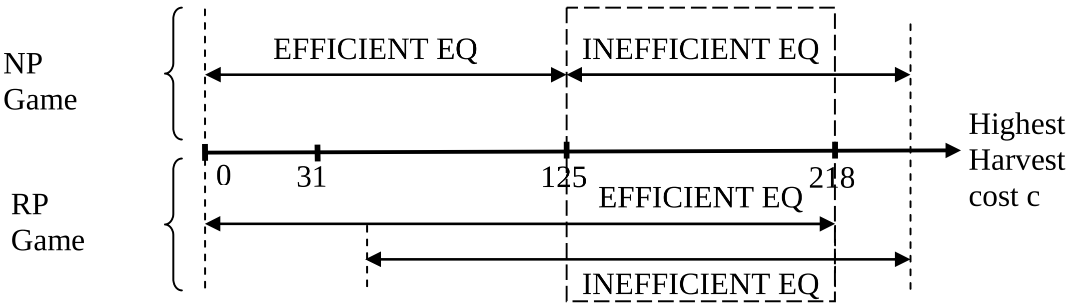


Figure 3. Equilibrium structure of the games NP and RP.

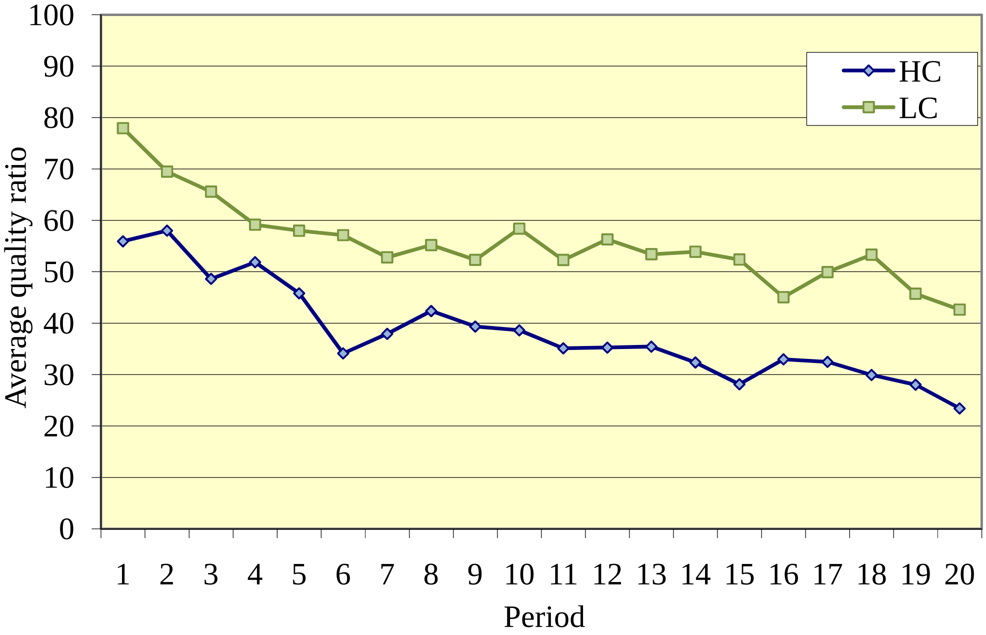


Figure 4. Quality in the first block

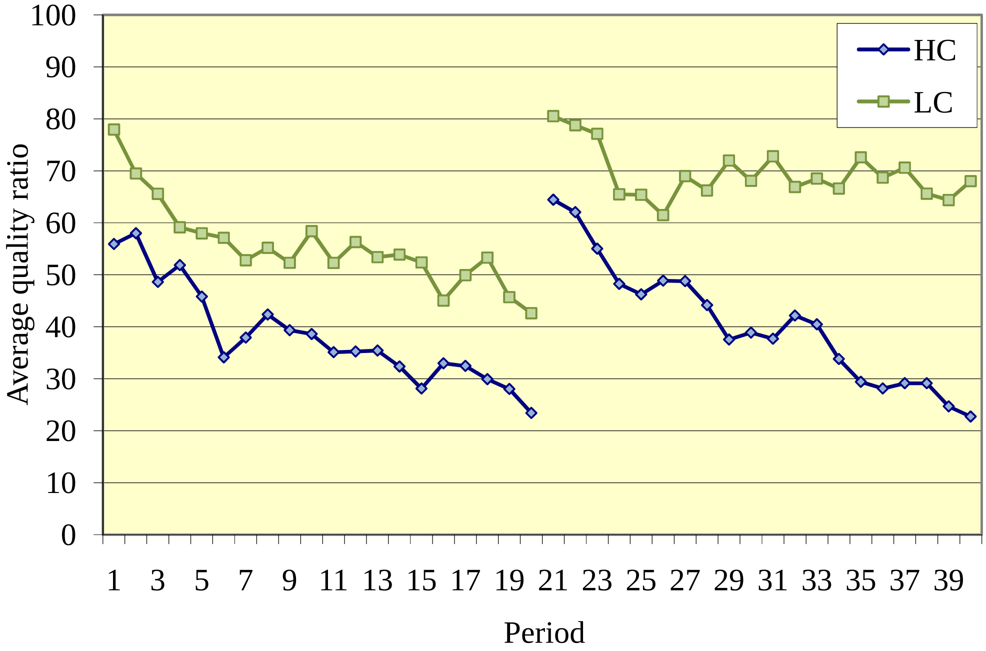


Figure 5. Quality in both blocks

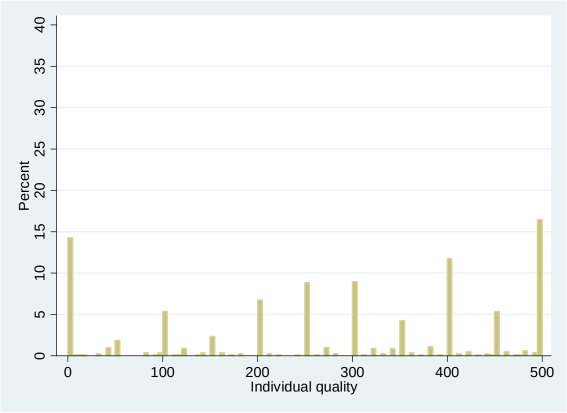


Figure 6. Histogram LC - block 1 - NP

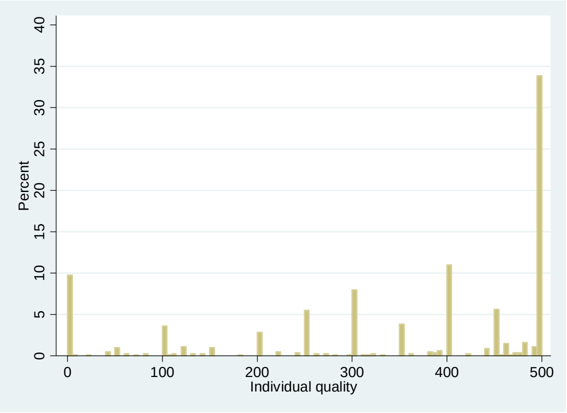


Figure 7. Histogram LC - block 2 - RP

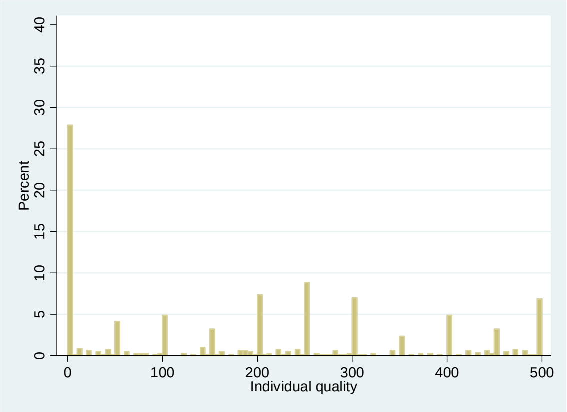


Figure 8. Histogram HC – block 1 - NP

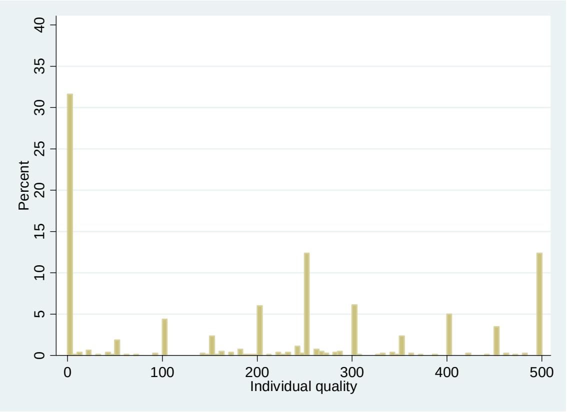


Figure 9. Histogram HC – block 2 - RP

Table1. Summary of Treatments

Treatment	Subjects	Groups	Blocks		Sessions
			1	2	
LC	40	10	NP	RP	1
HC	40	10	NP	RP	1
2	80	20			2

Table 2. Individual Quality. Treatment Effects by Blocks

	Rounds	
	1-20	21-40
Constant	222.7357	455.6926
	(28.6640)***	(52.0294)***
Dcost	-51.2476	-126.0861
	(24.2898)**	(37.8471)***
Period	-6.5852	-5.6697
	(1.1971)***	(1.6359)***
Quality1	0.3185	0.1653
	(0.0627)***	(0.0709)**
Nº Obs.	1600	1600
R-sq:		
Within	0.0820	0.0689
Between	0.3151	0.2894
Overall	0.1821	0.1935
Prob>chi2	0.0000	0.0000

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

Table3. Individual Quality within Subject 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	0.0000	0.0000
Between	0.0000	0.0000
Overall	0.0559	0.0790
Prob>chi2	0.0000	0.0000

Table 4. 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 (pvalue=0.1688)

Table 5. Quality in Rounds 20 and 40

Within Subjects Analysis at the Group Level. Wilcoxon signed-rank test.

		Round 20		Round 40
Average quality	LC	213.15^{a}	<	340.1^{a}
	HC	117.1^{b}	=	113.65^{b}

Wilcoxon signed rank-sum non parametrical test at the group level, within groups, n=10
a – difference is significant at the 10% level (pvalue=0.0745)
b – difference is not significant at any level (pvalue=0.5403)

Table 6. Relative Frequencies of Equilibrium Play

	Equilibrium play[a]		Zero quality		Maximum quality		N
	Freq	*(%)*	Freq	*%*	Freq	*%*	
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 – 500 is not an equilibrium action in HC-RP. ‘Equilibrium play’ denotes here any action associated to equilibrium in HC or LC. So, data includes 0 and 500 in both treatments, for the sake of comparability.

Table 7. Behavioral Determinants

	RP game	
	LC Rounds 21-40	HC Rounds 21-40
Constant	350.8567 (46.6946)***	376.3273 (70.2803)***
Period	-1.4839 (1.4773)	-8.0893 (2.0982)***
Lrp	49.8335 (21.3186)**	95.3805 (14.2390)***
Lp	-0.7316 (19.7631)	-19.4160 (18.9679)
Lrp*Lp	-39.8880 (18.4951)**	-51.8227 (16.2127)***
Quality1	0.0542 (0.0987)	0.1923 (0.0578)***
Nº Obs.	800	800
R-sq:		
Within	0.0155	0.1882
Between	0.4412	0.4770
Overall	0.1206	0.2783
Prob>chi2	0.0000	0.0000

Appendix (not to be published)

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 it 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 banned. If you do not follow this rule, you will be excluded from the experiment.

1. The experiment consists of 20 independent rounds. In each round you are 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 of 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:
   [see graph 1]
3. Your payoff from the experiment will depend on your revenues (the price paid by Alpha) and your costs (which depends on your technology and your decisions). Moreover, both of them (revenues and costs) will depend on the production time that you choose to take, as we explain below.
4. As you work for other clients distinct to Alpha, you already have a set of orders that prevent you start the production immediately. So, you have to wait until a minimum date, which we will inform you 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 in 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. The following graph 2 plots the cost function depending on the production time:
   [see graph 2]
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. The following graph 3 shows the relationship between the individual component's quality and the production time. As you can see, it is a symmetric single pinked 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 up to get to be **0** again at 500 days from the minimum day. Since every supplier uses the same technology, the graph is common to all of them:
   [see graph 3]
8. Notice that your costs and revenues are directly related to your production time decision. Next graph 4 shows this relationship more clearly:
   [see graph 4]

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

10. 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. Next **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:
    [see graph 6]
11. As we pointed 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. So, the variable price paid by Alpha is exactly the average quality of the components and equal for all suppliers of the same group regardless their individual quality. Remember that Alpha also pays a fixed payment of 200 ECU.
12. According to what described above, your benefit depends on the decisions made by all members of your group. Following table calculates the **benefits** associated to each decisions profile:

			Delivery time and average quality of the other three members				
	Delivery		*Min day*	*Min + 125*	*Min + 250*	*Min + 375*	*Min + 500*
		Qualtiy	0	250	500	250	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 too, the average quality will be 250. So, 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)
Bº = (200 + 250) - 200 = **250**

b. If you deliver at Min+375 and the others 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** as your cost is 100:

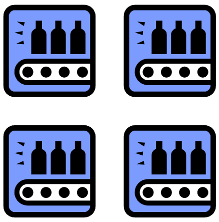
Benefit = Revenues - Cost
(Fixed + Variable)
Bº = (200 + 437.5) - 100 = **537.5**

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 get zero benefits (**0**).

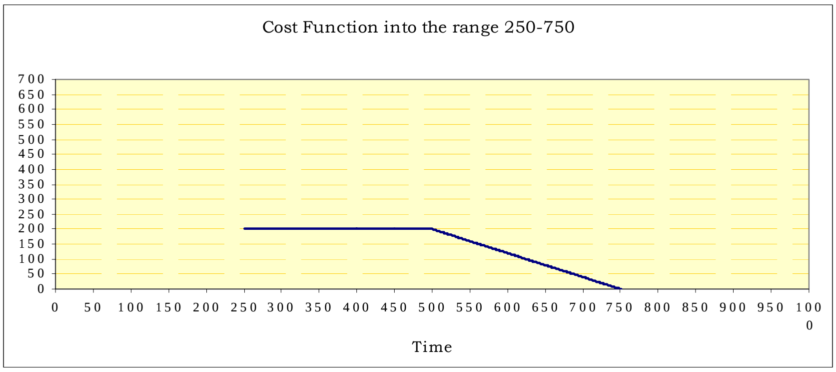
Benefit = Revenues - Cost
(Fixed + Variable)
Bº = (200 + 0) - 200 = **0**

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

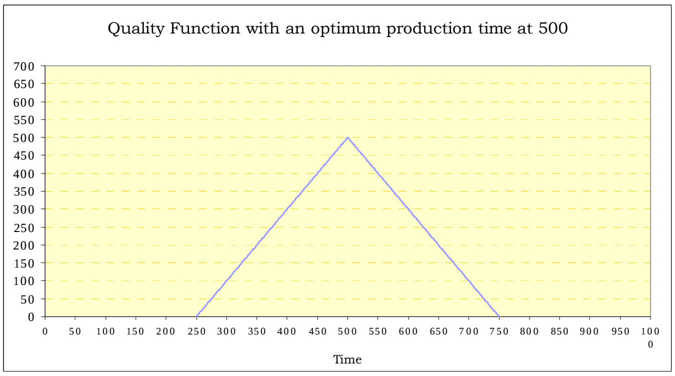
Graph 1



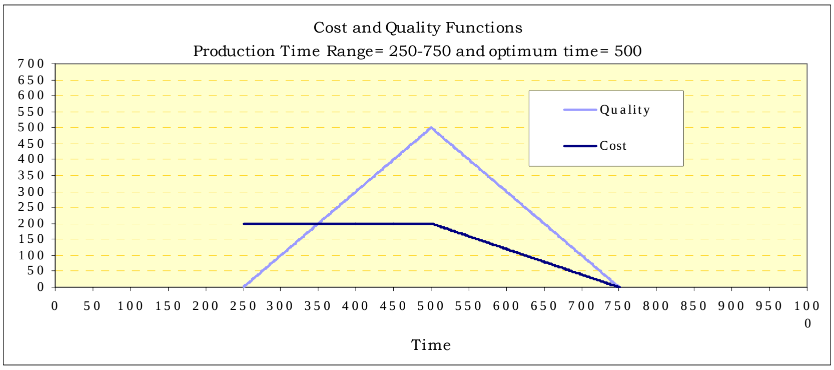
Graph 2



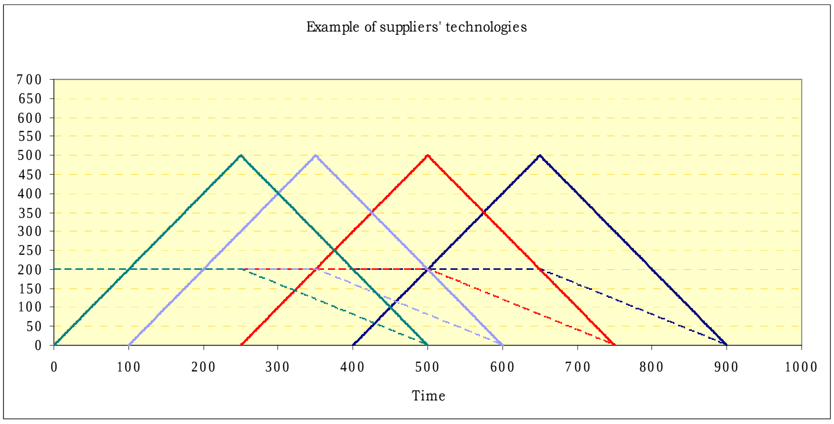
Graph 3



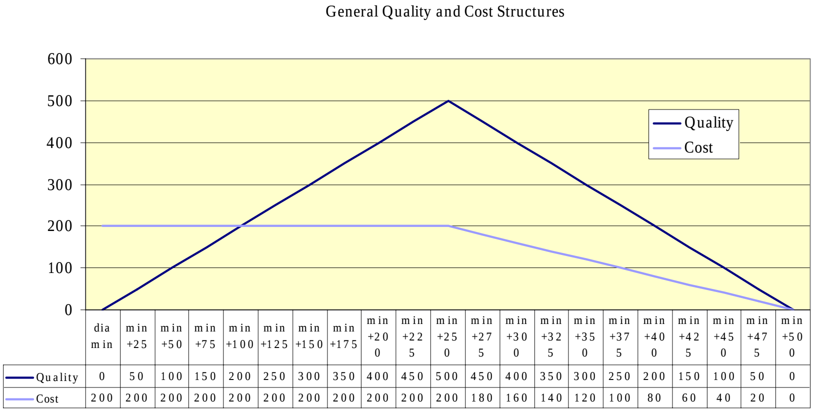
Graph 4



Graph 5



Graph 6



	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

Instructions (LC treatment, RP game)

1. This second experiment consists also 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 respect to the previous experiment is the introduction of an additional mechanism affecting the variable payment. Concretely, whereas the fixed payoff is maintained the same, 200 ECU, the variable payoff is determined as follows:

$$R = \frac{\text{AverageQuality}}{\text{MaximumQualtiy}} \times 100 = \frac{\text{AverageQuality}}{500} \times 100$$

   where R is a quality rate calculated by Alpha.
3. This rate determines the probability each group member has to be 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 to understand the rule. In case R is 70, all members of group have a 70% chance of earning the variable payoff and a 30% chance of getting 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 variable payoff. Following table records this interdependence more clearly:

			Delivery time and average quality of the other three suppliers				
	Delivery		Min day	Min + 125	Min + 250	Min + 375	Min + 500
		Quality	0	250	500	250	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 to understand the rule:

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

Benefit	=	Revenues				-	Cost		
		(Fixed	+	Variable)					
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				-	Cost		
		(Fixed	+	Variable)					
B°	=	**87.5%**	(200	+	**437.5**)	-	100	=	**537.5**
	=	**12.5%**	(200	+	**0**)	-	100	=	**100**

15. After each round you will get information about the average quality in your group, your payoffs (including your production cost) and the R value.
16. 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€.

Additional Figures and Tables

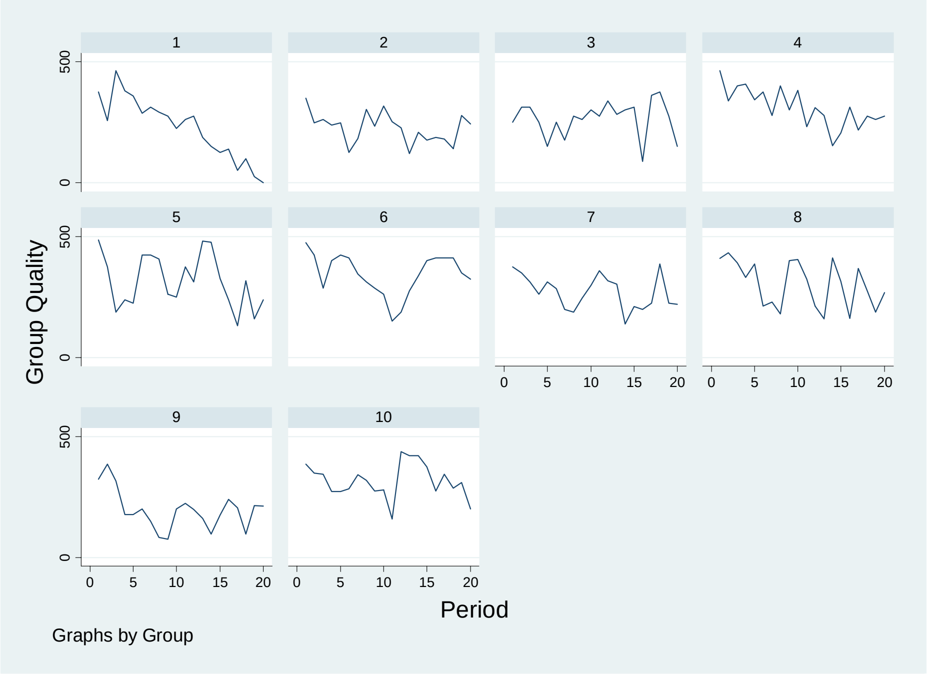


Figure 1a. Average quality by group, treatment LC, block 1 - NP

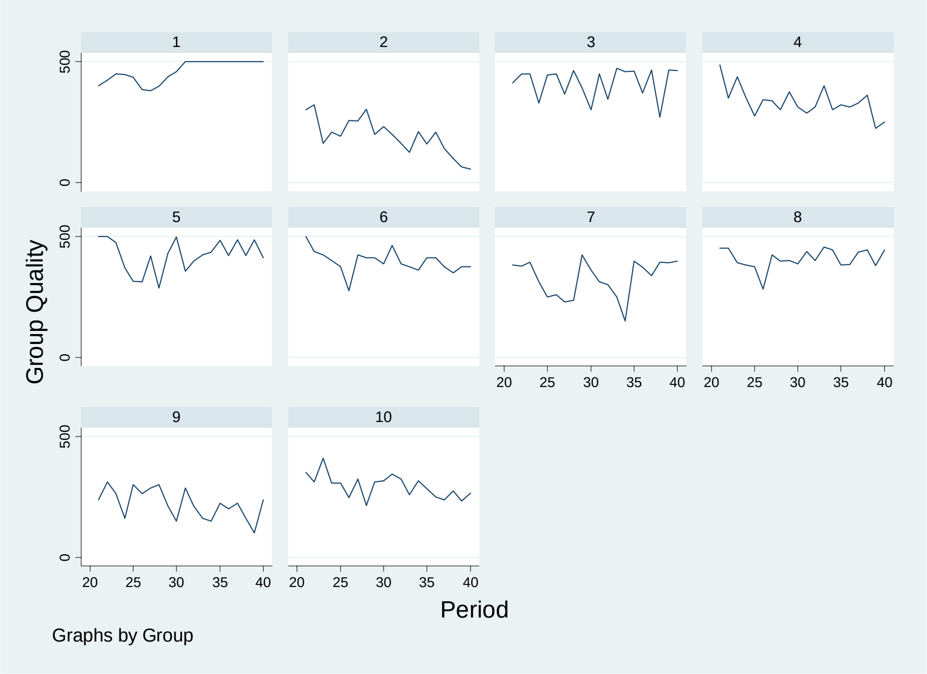


Figure 2a. Average quality by group, treatment LC, block2 - RP

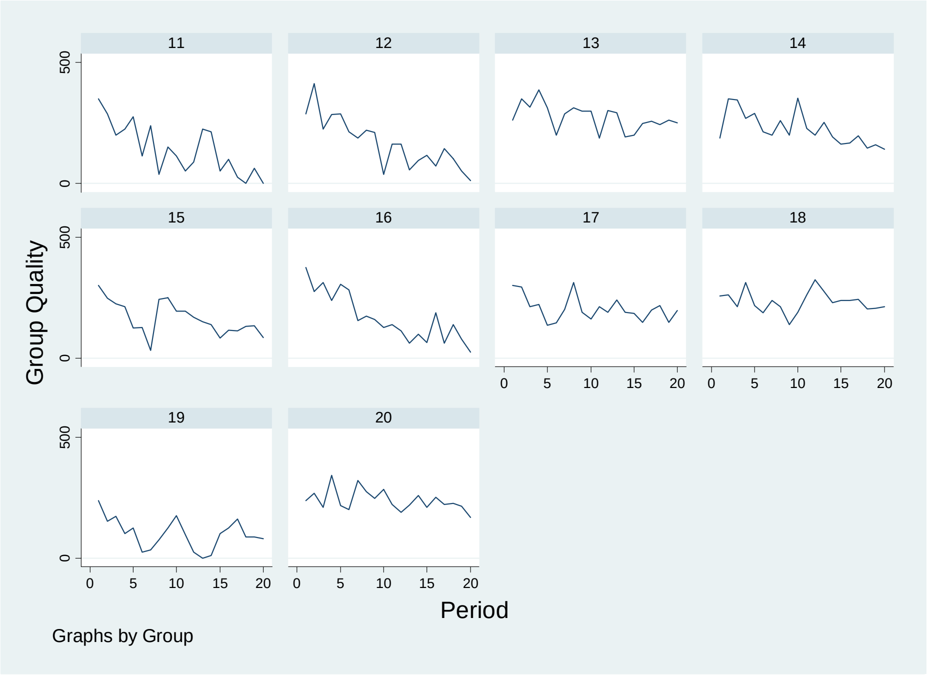


Figure 3a. Average quality by group, treatment HC, block 1 - NP

Figure 4a. Average quality by group, treatment HC, block 2 - RP

Table 1a. Quality in Rounds 1, 20, 21 and 40. Between Subjects Analysis. Mann Whitney Non Parametrical Tests

		Round 01	Round 20	Round 21	Round 40
Average quality	LC	389.75[a]	213.15[b]	402.6[c]	340.1[d]
		>	>	>	>
	HC	279.6[a]	117.1[b]	322.2[c]	113.65[d]

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)

Table 2a. 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