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What makes Law to change Behavior? An experimental study

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Abstract

The use of mild laws to affect people's behavior is pervasive – from environmental regulation to tort law – but little is known about how the law changes human behavior and social outcomes when it uses non-deterrent monetary incentives. We find that when low monetary incentives are used in tandem with an indication of what one should do (i.e., a norm), then the effect on behavior is positive but transitory. The effect is long lasting when we use low monetary incentives in isolation. This suggests that the indication of what one should do makes salient the conflict between people's normative expectations and what others effectively do. This undermines conditional cooperators' own motivation to contribute to public goods. Finally, we compare the effects of mild laws with how mere messages indicating what is moral behavior affect contributions to the public good. Contrary to the existing experimental evidence, we find that messages fail to improve cooperation. We spotlight the conditions under which this is the case.

1 Introduction

What is it about the law that has the potential to change human behavior? Under the standard model of law and economics (see Posner 1973), a law is supposed to change human conduct by altering the returns that individuals get from different kinds of behavior. This is indeed the idea that has been put forward by some legal scholars (Calabresi 1961) and economists (Becker 1968) who are commonly viewed as the founding fathers of law and economics (Parisi and Rowley

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2005). What is paramount in understanding how law impacts behavior, from the standard law and economics perspective, is, to quote McAdams (2000, p. 1650), that "by imposing liability or punishment on individuals, the state changes the payoffs so that cooperation rather than defection is the dominant strategy." Thus, again in McAdams's words, "the first step in the causal chain by which law affects behavior is that the formal sanctions law imposes raise or lower the costs of behavior."

Critiques of the traditional model of law and economics argue, however, that laws rarely provide sufficient monetary incentives for the most selfish persons to behave cooperatively when this is individually costly but collectively beneficial. One reason is that severe – i.e. optimally deterrent – incentives are not feasible for fairness reasons (see Sunstein et al. 2000; Polinsky and Shavell 2000). This is what Basu (1993) called the "ink on paper" problem. The problem, as Basu put it more recently, is that "law, in essence, is nothing more than some words on paper." (2015, p. 10). Basu contends that there are plenty of examples when, in game theoretic terms, the law does not change the options open to money maximizing individuals. Take, for instance, the law that requires grocery stores, in most European countries, to charge a fee for each plastic bag the store provides. As noted in Convery et al. (2007) and in Homonoff (2013), the fee charged is extremely low compared to the behavioral change sought – i.e. to motivate customers to substitute reusable for plastic bags. In Ireland, the pioneer country in this matter, the law requires grocery stores to charge a fee of 0.15 euro cents, and in the first city in the United States to pass such legislation, Washington, D.C., the fee is as low as 0.05 euro cents. Engel (2014) discusses other examples of non-deterrent incentives imposed by law.

If the law does not change the monetary payoffs individuals earn then how can the law change behavior and social outcomes? To go back to our example, Convery et al. (2007) perceptively show that the requirement to charge fees for plastic bags is one of the most popular laws in Europe and its effect has, in fact, been dramatic – a reduction in use in the order of 90%, and an associated gain in the form of reduced littering and negative landscape effects. Thus, even laws that introduce low price-incentives may result in large behavioral changes.

Such laws that rest on low price-incentives are called mild laws (Tyran and Feld 2006). The fact that even mild laws may change behavior prompted legal theorists, social psychologists and, more recently, law-and-economics scholars to focus on additional factors that may affect how legal rules impact behavior. One such factor is what mild laws signal to individuals (see Feldman 2009). Sunstein (1996) and Cooter (1998) made the first attempts to create a framework of law and economics that recognizes that mild law may have an expressive function: lawmaking may motivate people to engage in cooperative interactions if the law is perceived as a public expression of what one ought to do. In this case, the law is not perceived as a mere low price charged for some kind of neutral behavior. Instead, mild law may connote wrongdoing if one decided to engage in that behavior and pay the monetary price for doing so.

Despite the existing rich literature on the expressive function of law, reviewed in McAdams (2015), one important question remains however unexplored: what is the causal impact of the public expression of what one ought to do on people's decision to engage in collectively beneficial but individually costly behavior? In other words, the question is whether framing a law as what one ought to do reinforces the effect of low price-incentives and increases the power of mild laws to change behavior. To answer this question, we use an experimental approach because experimental techniques provide several important advantages over other methods of empirical investigation. The most important advantage is that in laboratory experiments we have the ability to control the environment within which individuals make decisions. We can control both the level of the price-incentive and the public expression of what one ought to do. This allows us to derive clear predictions which can be tested against observed behavior. To do this, we use the firmly established paradigm of public goods games from experimental economics. We use this type of experimental game, first, because the under-provision of public goods from voluntary actions (Samuelson 1954) is seen as one of the major justifications for government activity and lawmaking (Gaechter 2014). Second, in public goods games, both people's preferences and beliefs about what others do play a key role in how different treatments affect people's contribution decision. The two effects, on preferences and beliefs, are also central to the expressive law and economics theories (Cooter 1998; Bohnet et al. 2001).

Our experiment is the following. Each session consists of thirty periods of a four-person public good game. The Baseline, a standard linear public good game, is always in effect in periods 1-10. Between periods 11 and 20 the same individuals are subject to one of the following treatments: the Sanction, the Incentive, or the Message. The Sanction treatment corresponds to mild laws that contain both a low price-incentive and an indication of what one ought to do. Specifically, in this treatment subjects are informed that 0.3 experimental currency units (Ecus) will be subtracted from every token not allocated to the public account. This subtraction rule preserves, however, the nature of the decision as a social dilemma (one that pits an individual's interest against the interest of the group). The price-incentive is therefore non-deterrent, leaving zero contribution as the dominant strategy. Besides this price-incentive, the punitive nature of the subtraction rule is made clear by emphasizing that Ecus are subtracted whenever the subject keeps tokens on his or her private account, thus suggesting that one should contribute to the public account. To summarize, our Sanction treatment introduces a low price-incentive but it also expresses what one ought to do. To disentangle these two possibly confounding effects, we implement our second treatment – the Incentive treatment. In this treatment, subjects are simply informed that the new return from the private account is reduced from 1 to 0.7 Ecus. The Incentive and the Sanction treatments have exactly the same strategic environment. That is, in both treatments, the return from the private account is 0.7 Ecus. The only difference is the framing. Thus, any difference between the two should be the result of the expression of what one ought to do.

The expressive law and economics theories have often emphasized that, in some cases, publicly expressing what one ought to do may suffice to achieve cooperative interactions. This implies that we may, under some conditions, rely solely on public messages indicating a course of action without the use of any price-incentives. An often cited example is the publicly displayed signs prohibiting smoking in public places (see Sunstein 1996) without any enforcement (i.e. with no price-incentives for non-compliance). Another example is provided by the numerous messages encouraging people to recycle because that is in "our common interest" (Bazart and Romaniuc 2014), with no monetary consequences for not doing so. More generally, as Stout (2011, p. 194) explains, governments often encourage people to act cooperatively – recycle, pay their dues on time, contribute to funding cultural goods such as museums, etc. – "through the simple and remarkably cheap expedient of *telling them that's what they ought to do*" (emphasis original). Stout further explains that this kind of indications from central authorities, such as judges or legislatures, act as a focal point[1].

In addition to the Sanction and the Incentive treatments, we test the causal impact of a mere indication of what one should do. This corresponds to the Message treatment in our experiment. The main difference with the other two treatments is that in the Message treatment subjects are explicitly told what is the group desirable action and there is no price-incentive whatsoever to follow this indication – which makes this indication cheap talk. Similarly to the Sanction and the Incentive treatments, the Message treatment is in effect in periods 11-20. This allows us to compare the effect of the three relative to the Baseline under identical controlled conditions. Additionally, we want to understand how the Sanction and the Incentive interact with the Message treatment. To study this, we added a third segment to each session, which

[1]A focal point is a capacity that human beings, possessing common cultural background or other common experience, have and that enables them to predict what others are likely to do (see Schelling 1960).

corresponds to periods 21-30. Thus, after 10 periods played under the Sanction or the Incentive, subjects are informed that a message will be displayed on their computer screens in periods 21-30[2].

To preview our results, we find that the Sanction treatment increases cooperation compared to Baseline conditions. However, its effect is transitory. Surprisingly, lifting the frame, under the Incentive treatment, maintains higher cooperation levels over time. This implies that the expression of what one ought to do, if anything, reduces the effectiveness of mild laws over time. We explain the difference between the two treatments by the fact that the Sanction makes salient the norm of conditional cooperation (Fischbacher et al. 2001). However, since the price-incentive is low, free-riding remains substantial and conditional cooperators respond by reducing their own contributions. Our findings indicate that conditional cooperators, over time, revise their beliefs about others' behavior, which is consistent with the focus on beliefs about what others do in the expressive theories of law. We also find that contributions in the Message treatment are similar to our Baseline conditions. That is, the message that was publicly displayed in our experiment failed to act as a focal point. The explanation that we propose for this result points to group composition, and specifically the matching protocol in our experiment, as the key motive that prevents subjects from uptading their beliefs under the Message treatment.

The remainder of the paper is organized as follows. In section 2 we review the relevant literature. In section 3 we present the experimental design. Section 4 presents and discusses the results and section 5 concludes.

2 Related literature

This paper relates primarily to the experimental law and economics literature that investigates the expressive function of law. Bohnet and Cooter (2001) propose, to our knowledge, the first experimental investigation of how low price-incentives on which rest mild laws influence preferences and beliefs in games with multiple equilibria. They find that announcing a low payment that subjects need to make for the "wrong" choice in coordination games causes behavior to jump from the Pareto-inferior to the Pareto-superior equilibrium. Their results also suggest that these payments affect behavior more by changing beliefs than by changing preferences. Since many real-world situations are characterized by conflicts of interest rather than coordina-

[2] It is worth noting that only one treatment is used in each segment. For example, after period 20, we inform subjects that conditions are identical to the first segment (Baseline) with the message being displayed on their computer screens.

tion failures, Bohnet and Cooter's paper prompted the question of how mild laws may affect contributions to public goods. Tyran and Feld (2006) propose the first tentative answers to this question. They make the contribution to a public good obligatory and implement a mild law for free-riding behavior. The results from their one-shot public good experiment indicate that mild laws fail to significantly improve cooperation when they are exogenously imposed. However, the same laws have norm-activating effects if they are implemented through a specific voting procedure. The authors then suggest that endogenously chosen laws lead individuals to update their beliefs about others' behavior in an optimistic direction. The idea that mild laws may express the reigning norms in a group and can discipline people by showing what is "appropriate" behavior is also at the core of Galbiati and Vertova's (2014) experiment. The authors find that mild laws work only when used in tandem with non-binding obligations to contribute to the public good.

There are several key points to be noticed about the experiments reported above. The first is that Tyran and Feld's experiment simulate one-shot interactions where subjects are exposed to one treatment during the entire experimental session. While one-shot between-subject analysis is generally favored when one wants to avoid learning effects, in most real-life dilemmas, the same individuals are often exposed to various changes in the strategic environment within which they repeatedly interact with each other (see Charness et al. 2012, who further discuss between-subject versus within-subject design). Our experiment complements their study by imagining groups of individuals who, first, get accustomed to significant degrees of free-riding, and, then, are exposed to a change (even if mild) in the strategic environment within which they interact.

The second key point is that both Tyran and Feld's and Galbiati and Vertova's studies cannot really disentangle two possibly confounding effects: (i) a pure price-incentive effect resulting from the change in the return from the private account introduced by the mild law, and (ii) a framing effect as a consequence of presenting the law to subjects as a subtraction rule that applies *to tokens not contributed to the public account*, thus connoting wrongdoing. This last point is important since the particular framing of the law in laboratory conditions may change subjects' understanding of what is the appropriate behavior. This particular framing, although subtle, may in effect change the way subjects interpret deviations from full cooperation (on framing effects in experiments, see Andreoni 1995). Our experiment is the first, to our knowledge, in which the framing of mild laws (i.e. indicating what one ought to do) and the price-incentive effects are disentangled from each other.

On the one hand, framing laws so as to indicate what one ought to do echos the modern strain of legal scholarship to which we referred above as the expressive theory of law (for a review of this literature, see Feldman 2011). Expressive law theorists believe that legal rules convey an authoritative message about what is appropriate behavior in a particular context (see Sunstein 1996; Cooter 1998). On the other hand, the study of low price-incentives for neutral behavior leads us to a strand of literature which focuses on crowding-out/in effects of incentives. Excellent surveys of this topic can be found in Gneezy et al. (2011), and Bowles and Polania-Reyes (2012). Our paper complements this literature by showing the relatively long term benefits of laws that are presented as mere price-incentives compared to when additionally there is an indication of what one ought to do.

Our work also relates to studies which focus on how to raise the psychological-cost of selfishness in the context of voluntary provisions of public goods. In his well-known survey on voluntary contribution mechanisms, Ledyard (1995) mentions messages about what is "moral" behavior as one of the instruments to improve cooperation. This force remained, however, unexplored for a decade. Only recently, field studies focused on such normative kind of messages as a way to encourage tax compliance. With data gathered from a field experiment in Switzerland, Torgler (2004) indicates that such messages indicating what one should do to be consistent with "moral behavior" has hardly any effect on taxpayers' compliance behavior. Fellner et al. (2013) come to a similar conclusion in the context of TV license fees. Hallsworth et al. (2014), however, in a natural field experiment, demonstrate that such messages enhance tax compliance.

Field studies, notwithstanding their interest, face important identification hurdles. Dal Bo and Dal Bo (2014) propose the first laboratory public good experiment in which they isolate the effect of messages about what is moral behavior on cooperation. They compare the effects of different types of messages relative to no-message conditions. Their within-subject experiment reveals that the message with a consequentialist character – that it is moral to maximize gains at the group level – is the most effective in encouraging cooperation. Their findings prompt three key questions. The first is whether such messages are more effective than mild laws in improving cooperation. The answer to this question has important implications for the design of new and cost-effective public policies (for a review of cost-effective behavioral policies, see Madrian 2014). The second question is whether it makes a difference in terms of contribution levels if the message is used in an environment in which individuals had been accustomed to mild laws, compared to when the message is introduced in a neutral context (after the Baseline). Finally, the third question is whether moral appeals are always able to create a focal point leading people to adopt the socially desirable action. These are some of the questions we take up in this paper since we use an identical message to Dal Bo and Dal Bo's Utilitarian message. The only difference between our design and theirs is that we do not vary group composition after each round. This allows us to compare our results to theirs and to understand if group composition may constitute a limit to the power of messages to motivate cooperative conduct.

3 Experimental design

3.1 The experimental game

The basic structure of our experimental game follows the well-established design of a repeated linear public good game employing standard parameters. Ledyard (995) and more recently Chaudhuri (2011) provide elaborate descriptions of how public good games are implemented. In our experiment, groups are composed of n = 4 subjects. Each subject is endowed with E_i = 20 tokens at the beginning of each period, which must be allocated to either a public account (g_i) or left on subject's private account (c_i). Each participant i must make a contribution decision g_i ($0 \leq g_i \geq 20$). Contributions are made simultaneously, without any communication, in whole tokens. Each token left on the private account generates a benefit equal to 1 Ecu. In addition to the tokens kept on the private account, each participant receives a fixed benefit, α = 0.4 Ecus, from the total group contribution to the public account, where $0 < \alpha < 1 < n\alpha$. Thus, the individual payoff function (π_i) is the following:

$$\pi_i = 20 - g_i + 0.4 \sum_{j=1}^{4} g_j$$

This setting corresponds to a linear public good game. The value provided to individuals by the public good is a linear function of how much of the public good is provided. From $1 < n\alpha$ it follows that the Utilitarian optimum and the efficient symmetric outcome is for all group members to contribute their entire endowments to the public account. This would, in effect, maximize the gains at the group level. However, at the individual level (assuming pure self-interest), each subject is better off from contributing zero to the public account. Thus, the Nash equilibrium in our public good game is for each group member to contribute zero to the public account. Since the game is symmetric, this is the dominant strategy for each participant in each four-persons group.

Our public good game differs, however, from standard public good games (e.g. Fischbacher et al. 2001) in two main dimensions.

The first dimension we manipulate is the strategic environment within which subjects make decisions. In our Sanction treatment, subjects are informed that 0.3 Ecus will be subtracted from every token not allocated to the public account and which therefore remains on subjects private account. The payoff function in the Sanction treatment is given by

$$\pi_i = E_i - g_i + \alpha \sum_{j=1}^{n} g_j - s_i(E_i - g_i)$$

The intensity, framing, and implementation of the subtraction rule were chosen so as to replicate three specific characteristics of many mild laws. First, the monetary punishment of offenders is typically mild (Engel 2014). Also Ostrom et al. (1992) note that many successful communities had frequently recourse to mild punishments. In order to implement a mild law, we set the subtraction rule so as to ensure that donating zero remains the dominant strategy of money-maximizing individuals, which preserves the nature of the decision as a social dilemma, i.e. one that pits an individual's interest against the interest of the group. To see why this is the case, consider the individual payoff with our subtraction rule, which yields the following individual payoff function: π_i = 0.7*$(E_i - g_i)$ + 0.4*$\sum_{j=1}^{n} g_j$.

While full contribution from every subject in the group yields π_i = 32 Ecus, contributing zero and paying s_i = 0.3 for every token kept on the private account, yields π_i = 38 Ecus for the free-rider. Thus, a money maximizing individual does not contribute to the public account so long as $s_i < 1 - \alpha$.

Second, the punitive nature of mild laws is typically clear in real-world settings. Cooter (1984) perceptively argues that legal punishments are payments "imposed for doing what is *forbidden*" (emphasis added) rather than "the price of doing what is permitted" (p. 1523). To emphasize the punitive nature of our Sanction treatment, we frame the subtraction rule so as to make explicit the fact that Ecus are subtracted when individuals deviate from the action that benefits the group. Specifically, the instructions read that 0.3 Ecus are subtracted from tokens not allocated to the public account. Generally, in public good experiments, it is assumed that members of the group share the understanding that the desirable action of each individual is one that favors the interest of the group, and that deviations from this action are undesirable (e.g. Andreoni and Gee 2012). Our Sanction treatment makes salient this contribution norm by emphasizing that keeping tokens on the private account constitutes deviations from the norm and these deviations have monetary consequences. We avoid using words such as tax, punishment, or sanction in order to control for framing (Andreoni 1995) and minimize experimenter demand effects (Zizzo 2010).

Third, to mimic a centralized punishment, in our experiment, the subtraction rule is applied by the central computer. We thus eliminate any source of uncertainty about its application. Thus, while some recent experiments (e.g. Engel 2014) used a randomly selected subject to act as the punishing authority, we elect to deliver the punishment through the experimenter. We choose this design because we wish the punishing authority to be seen as legitimate, as the legitimacy of enforcement figures has been shown to play an important role in public goods experiments with punishment opportunities (Baldassarri and Grossman 2011). The experimenter is most likely to be seen as a legitimate authority (Milgram 1963; Karakostas and Zizzo 2015) and thus closer to the image people have of judges or other central authorities.

From the Sanction treatment, it is clearly difficult to say whether this treatment may change behavior (or fail to do so) because of the low price-incentive that it introduces or because the law is framed as a threat of punishment for deviations from the norm of contribution and thus indicates what one should do. To disentangle the two, we introduce the Incentive treatment. Similarly to the implementation of the Sanction treatment, the Incentive is introduced after ten periods played in the Baseline. The unique difference between the two is that in the Incentive treatment, subjects are simply informed that henceforth each token kept on their private account will yield 0.7 Ecus. Obviously, the two treatments have identical strategic environments. That is, in both treatments the return from each token kept on the private account is 0.7 Ecus. The Sanction indicates that deviations from full contribution are undesirable and will have monetary consequences, while the Incentive treatment does not contain such a framing.

The second dimension along which the conditions in our experiment vary from standard public good games concerns the information available to subjects when they make their contribution decisions. In the Message treatment, we provide subjects with the following message: *"An action is moral if it maximizes the sum of everyone's payoffs. If you were to act accordingly, you would allocate the totality of your tokens to the public account."*[3] This message was first tested by Dal Bo and Dal Bo (2014) in similar conditions in the context of a public good game. They claim that this kind of message creates a focal point through its effect on subjects' beliefs about others'

[3]The original message in French stated: *"Une action est morale si elle maximise les gains de tous les membres du groupe. Agir ainsi revient a placer la totalite de vos jetons sur le compte collectif."*

choices. For our results to be comparable with existing laboratory studies, we chose to use Dal Bo and Dal Bo's Utilitarian message. Their study constitute, indeed, the first experimental test of the effect of various payoff-irrelevant messages on cooperation in laboratory conditions. We introduce the Message treatment after ten periods played in the Baseline, similarly to the Sanction and the Incentive treatments. But, in separate sessions, we also implement the Message after the Sanction or the Incentive treatments had been in effect in the second segment of the game. In this case, the Message is introduced in the third segment, in periods 21-30. This allows us to understand if the Message may work in environments in which individuals had become accustomed to mild laws.

Table 1 provides information about the three treatments. The baseline condition is omitted since it corresponds to standard linear public goods games.

Table 1: Treatments

Treatment	**Framing**
Sanction	An amount of 0.3 Ecus will be subtracted from every token that you choose to keep on your private account, and which is therefore not allocated to the public account.
Incentive	Every token allocated to your private account now yields 0.7 Ecus.
Message	An action is moral if it maximizes the sum of everyone's payoffs. If you were to act accordingly, you would allocate the totality of your tokens to the public account.

The message used by Dal Bo and Dal Bo (2014) raises some concerns which are worth discussing before going further. It may seem controversial to have the experimenter claim that maximizing group's payoffs is the moral action to engage in. The hitherto experimental studies on normative messages – e.g. what is the appropriate behavior in a particular situation – have either allowed subjects to communicate about what they believe to constitute socially appropriate or moral behavior (Andrighetto et al. 2013) or they informed subjects about what their peers thought was socially appropriate to do (Bicchieri and Xiao 2009). We accept to bear the cost of this limitation at the benefit of comparing and testing the interaction between mild laws and a message that proved effective in increasing cooperation in similar conditions to ours. What is more, in order to minimize experimenter demand effects that may result from the experimenter telling subjects what they ought to do, when introducing the message, we make clear that a message will be displayed on each participant's computer screen, and that the message is the same for every participant in the room, but the content of the message does not appear in the Instructions, nor is it read aloud by the experimenter.

3.2 Procedures

The experiment consists of six sessions conducted at the Laboratoire Montpellierain d'Economie Theorique et Appliquee (LAMETA) in Montpellier, France. 20 subjects participated in each session, for a total of 120 participants (60% were females) invited via the ORSEE software (Grenier, 2004). 80% of the subjects were students at one of the universities in Montpellier and 25% of them had an economics background. 88.33% of the subjects had previously participated in a laboratory experiment. We ensured, however, that none had previously participated in a game with similar parameters. Subjects interacted through individual computer terminals. Terminals were separated by lateral partitions to ensure complete anonymity.

The payment was made privately at the end of the session. The exchange rate was 15 Ecus = 1 euro. Subjects earned an average of 21 euros. A session lasted less than 90 minutes, including initial instruction and payment of subjects.

At the outset of each session, subjects were informed that the central server would allocate them randomly to groups of four people. Group assignments remained the same for the entire session. That is, partner matching conditions were in effect. Each session consisted of 30 periods, divided into three segments of 10 periods. The total number of segments in the session was common knowledge[4], as was the fact that at the end of the experiment only one segment out of the three would be chosen at random for payment. This design allows us to limit the effect that cumulative earnings may have on subjects' decisions, which may lead them to care less about the consequences of their decisions in the last periods of the game[5].

[4]Subjects were informed about the total number of segments for the simple reason that we wanted to avoid strong restart effects that are generally due to an unexpected restart of the same game. The restart effect is arguably less pronounced when subjects know in advance that the experiment consists of more than one segment (see Andreoni 1995).

[5]This procedure of not paying for all periods has been used by others in public good games, e.g. Andreoni and Miller (2002) and Goeree et al. (2002). Goeree et al. (2002) argue that paying for all decisions may provide higher incentives, but paying for only some decisions may induce subjects to think more clearly about the payoff consequences of each decision rather than focus on the relative earnings aggregated over all decisions.

In each session, subjects first played 10 periods of a standard public good game, which corresponds to our Baseline. The same subjects then played another 10 periods in one of these treatments: Sanction, Incentive, or Message. When the Sanction or the Incentive treatments were implemented in periods 11-20, then in the last segment of the game, that is in periods 21-30, subjects were informed that the game is identical to periods 1-10 except that a message will be displayed on their computer screens.

We chose this design for two reasons. First, we make subjects play the first 10 periods under Baseline conditions because we want to identify what makes the Sanction, the Incentive, and the Message work in a realistic and belligerent situation in which groups become accustomed to significant degrees of free-riding. This creates a challenging environment for each of our treatments. Second, the addition of a third segment allows us to identify the effect of using the message in an environment in which groups had been accustomed to mild laws. We can thus test whether the two types of instruments conflict with each other.

Table 2 provides detailed information about the described segments.

Table 2: Experimental design

Subjects	**Groups**	**Matching**	**Segment 1**	**Segment 2**	**Segment 3**
40	10	Partner	Baseline	Sanction	Message
40	10	Partner	Baseline	Incentive	Message
40	10	Partner	Baseline	Message	Sanction

4 Results

The presentation of the results is divided into two parts. First, we answer our primary question: how the three treatments – Sanction, Incentive, and Message – affect contribution levels relative to the Baseline? Second, we discuss the interaction between the message and the two types of mild laws – in the form of Sanctions and Incentives.

4.1 Which of the sanction, incentive, and message best improve cooperation?

Figure 1 illustrates the evolution over time of average contribution levels. It gives the contribution level in each period, averaged over the ten groups that make up each treatment. We can see that in the first 10 periods, before the Sanction or the Incentive could be applied, average contribution rates are nearly identical in the four sessions which make up the two Baseline conditions. On average, contribution levels seems to be higher in the Baseline that preceded the Message treatment. However, the difference between the three conditions is not significant.

Figure 1: Average contributions by period and by treatment

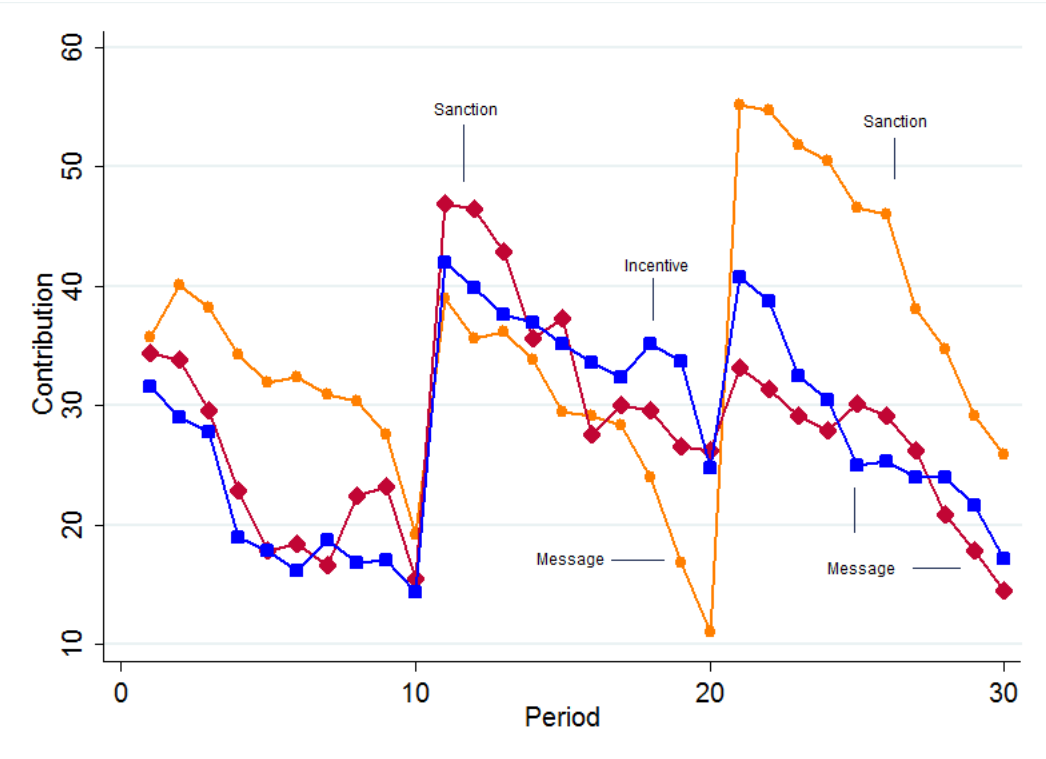

Table 3 also provides detailed information about the change in mean contributions between the three treatments and the corresponding Baseline conditions.

Panel A shows the difference in mean contributions between each treatment and the Baseline over the entire segment of 10 periods. That is, it illustrates the change from periods 1-10 to 11-20. Panel B restricts the analysis to the difference between the last 5 periods in each treatment versus the last 5 periods in the Baseline – i.e. periods 6-10 versus 16-20.

Table 3: Changes in average contributions by treatment

Treatment	Change in contributions	Z	P-value
Panel A: change over the 10 periods			
Sanction	2.86	2.497	0.01
Incentive	3.58	2.295	0.02
Message	-0.93	-1.376	0.16
Panel B: change over the last 5 periods			
Sanction	2.19	1.682	0.09
Incentive	3.82	2.293	0.02
Message	-1.56	-1.244	0.21

Note: p-values rely on non-parametric matched-pairs tests. The null hypothesis is that contributions before and after the treatment stem from the same distribution. We treat average contributions of each 4-person group over the entire segment of 10 periods as a single independent observation.

Panel A shows that when the Sanction is implemented in periods 11-20, average contributions increase compared to periods 1-10, from 5.86 to 8.72 tokens. A Wilcoxon signed-rank test indicates that the difference is significant at $p < 0.05$. However, it should be noted, from Panel B, that the effect of the Sanction is transitory. When we compare the last 5 periods played in the Baseline to the last 5 periods under the Sanction, we find that the difference between the two is only weakly significant under a two-tailed test (p-value of 0.092). This is due to the clear decay in contributions over time under the Sanction treatment, which can be seen from Figure 1.

Figure 1 also indicates that lifting the frame, under the Incentive treatment, increases contribution levels by comparable amounts to the Sanction, relative to Baseline conditions. In effect, average contributions increase from 5.20 to 8.78 tokens. Table 3 shows that according to the Wilcoxon signed-rank test, the difference between the Incentive and the Baseline is significant ($p < 0.05$). Interestingly, while we find that the Sanction causes a transitory increase in average contributions, the Incentive maintains higher average contributions also over the last 5 periods. A Wilcoxon signed-rank test of the difference between the Baseline and the Incentive in periods 6-10 and 16-20 respectively shows that it is significant at the 5% level ($p < 0.05$). This tells us that while both the Sanction and the Incentive increase contribution levels relative to the Baseline, only the Incentive is able to maintain higher average contributions over time[6].

[6]A Mann-Whitney ranksum test for the difference between the Sanction and the Incentive treatments indicates that

Now, the question is whether the the Message may also improve cooperation. Contrary to Dal Bo and Dal Bo's (2014) findings, we observe no significant change in average contributions between the Message and the Baseline. In fact, in periods 11-20, under the Message, contributions tend, if anything, to decrease compared to the Baseline: from 8.01 tokens under the latter to 7.08 under the Message. A Wilcoxon signed-rank test indicates, however, that the difference is not significant under a two-tailed test (p-value = 0.16). Since the matching protocol is the only major difference between Dal Bo and Dal Bo's (2014) design and ours[7], we conclude that the Utilitarian message used by Dal Bo and Dal Bo (2014) may create a focal point in groups that constantly change. However, it fails to do so when the group composition is fixed, as is the case in our experiment. This result does not imply that groups with a fixed composition are always insensitive to the message designed by Dal Bo and Dal Bo. Our experiment rather points to a particular condition under which this may be the case: the history of the play prior to the implementation of the Message may prevent subjects from updating their beliefs in an optimistic direction when they expect to face the same group mates (Crawford et al. 2008, is another study showing the limited power of focal points). This provides additional support for empirical and experimental works that stress the importance of expectations and of the history of the play that one shares with his or her group-mates (see, for example, Bicchieri and Xiao 2009).

To get further insights into how the Sanction, the Incentive, and the Message affect behaviors at the individual level, we employ a parametric data analysis. Our dependent variable is doubly censored since subjects may contribute a minimum of nothing and a maximum of the endowment to the public account. Thus, a two-limit Tobit model is required to estimate subjects' responsiveness to experimental variables (Bardsley and Moffatt 2000 provide a detailed discussion of random effects two-limit Tobit models in the context of public good games). Table 4 reports two random effects Tobit regressions analyzing the determinants of the contribution decision. It is revealing, first, to examine the pooled distribution of contributions. A histogram of this variable is shown in Figure 2. The histogram clearly reveals censoring at the lower limit

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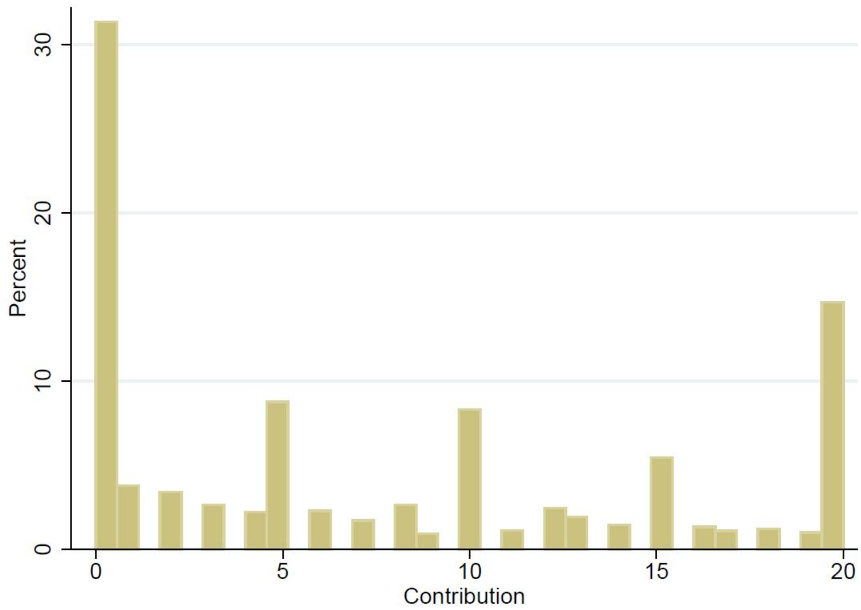
it is non-significant.

[7] It is worth noting that Dal Bo and Dal Bo (2014) study the effect of different messages and subjects are informed that the computer will randomly select one message from a set of five possible messages. This was done in order to avoid that high contributors interpret the message as a signal that contributions are lower than expected by the experimenter, infer that they themselves are contributing too much and respond by contributing less. However, using a hurdle-model, we found no evidence that the message has different effects on free-riders and on contributors respectively. The results from this model are available upon request from the author.

0 and at the upper limit of 20.

Figure 2: Histogram of contributions



In the two regressions presented below (Table 4), data from Baseline before the Sanction or the Incentive could be applied have been pooled together. In models (1) and (2), independent variables include a treatment dummy, and a control for time effects due to the repetition of the game, labeled Period. Model (2) adds two interaction terms between each treatment and gender (taking the value of one if the subject is a male and zero if the subject is female), and subjects' discipline of study (taking the value of one if the subject has an economics background and zero otherwise).

Tables 4 indicates that contribution levels are higher under the Sanction and the Incentive treatments than under the Baseline – even more so under Incentive than under Sanction relative to Baseline conditions. Also, we find that under the Incentive treatment, male subjects contribute significantly more than females. This is in line with some previous experimental studies who found, for example, that males show a stronger positive response to price-incentives than females (e.g. Bazart and Pickhardt 2011). Finally, it appears that subjects with an economics background are significantly less sensitive to price-incentives than "non-economists". This means that price-incentives fail to increase cooperation – a conclusion put forward by Gneezy and Rustichini (2000) – but this conclusion applies only in a small set of cases: when the targeted

Table 4: Determinants of contribution levels

	(1)	(2)
SANCTION TREATMENT	4.711***	3.708**
	(1.186)	(1.752)
SANCTION*PERIOD	−0.998***	−0.996***
	(0.204)	(0.165)
SANCTION*GENDER	-	1.371
	-	(2.464)
SANCTION*DISCIPLINE	-	1.607
	-	(3.825)
INCENTIVE TREATMENT	6.151***	4.440**
	(1.491)	(2.249)
INCENTIVE*PERIOD	−0.833***	−0.828***
	(0.177)	(0.164)
INCENTIVE*GENDER	-	8.183**
	-	(3.708)
INCENTIVE*DISCIPLINE	-	−7.125**
	-	(3.393)
MESSAGE TREATMENT	−1.232	−0.542
	(0.674)	(0.835)
MESSAGE*PERIOD	−0.840***	−0.842***
	(0.130)	(0.102)
MESSAGE*GENDER	-	−1.378
	-	(1.022)
MESSAGE*DISCIPLINE	-	0.344
	-	(0.851)
Constant	7.991***	8.426***
	(1.304)	(1.323)
No. of observations	2400	
Log likelihood	−2676.82	−2647.84

Note: Standard errors, in parentheses, are bootstrapped. ** Indicates significance at 5% level. *** Indicates significance at 1% level.

population are ”economists”.

Tables 4 also confirms the results from the non-parametric tests, namely that the Message is the only treatment that fails to increase average contributions relative to the Baseline.

Result 1*. Both the Sanction and the Incentive treatments lead to higher average contribution levels relative to Baseline conditions. Contribution levels are not higher under the Message than under Baseline* *conditions, suggesting that the power to act as a focal point is limited by the fixed group composition and the history of the play.*

As we noted above, the Sanction and the Incentive do not impact behaviors in the same manner. While the Sanction treatment appears to have only a transitory positive effect on contribution decisions, the Incentive treatment maintains higher contribution levels over time (see Table 3, Panel B). One interpretation for this difference is that the Sanction imposes a low price-incentive for the decision not to contribute to the public account but it also indicates what is the group desirable action. As the price-incentive is low, many subjects still deviate from the group desirable behavior. The indication of what is desirable from the group perspective creates, however, additional expectations among conditional cooperators that one ”should” cooperate. Therefore, the sudden decrease in contributions may be interpreted as a punishment that conditional cooperators inflict to those who do not contribute but who ”should”. This is not the case under the Incentive treatment which was presented as a reduction in the return from the private account – i.e. as a reduction in return from a neutral behavior.

To test for this explanation, we need to consider behavior at the individual level by taking into account the impact of group contributions at period t on any group member's decision at period $t+1$. We expect individual contribution decisions to be linked to the previous group contributions in the Sanction treatment, but not in the Incentive one. Table 5 reports results from a random effects Tobit regression in which the explanatory variable is the individual change in the contribution decision from period t to period $t+1$ as a function of the treatment and of the group contribution in t, controlling for time effects – ceteris paribus, contributions decrease with the repetition of the game. In other words, this tells us how a subject i reacts to the information he or she receives after each period about the aggregate level of contributions in his or her group. Recall that in our experiment, this is the only information subjects received at the end of each period.

Table 5 indicates that the information about the aggregate level of contributions impacts contribution decisions in the Sanction treatment, but this is not the case in the Incentive treatment. This supports our hypothesis that the particular framing that one ”should” cooperate renders the information about past aggregate contributions important in the Sanction treatment. Mere price-incentives, on the other hand, do not make salient such expectations. Engel (2014) builds a model showing how neutral mild laws may lead to large behavioral changes simply because individuals are inequity-averse. In our experiment, we observe that such neutral mild laws –

Table 5: The impact of past aggregate group behavior on individual contribution decisions

	Contributions in $t+1$ under Sanction	**Contributions in $t+1$ under Incentive**
TREATMENT	1.208	0.481
	(0.841)	(0.794)
GROUP CONTRIBUTION IN t	0.252^{***}	0.061
	(0.092)	(0.131)
PERIOD	-0.634^{***}	-0.674^{***}
	(0.148)	(0.131)
Constant	0.227	0.338
	(0.400)	(0.280)
No. of observations	1600	
Log likelihood	-1431.24	-1317.38

Note: Standard errors, in parentheses, are bootstrapped. *** Indicates significance at 1% level.

the Incentive treatment – are effective even over time.

Result 2. *The Incentive treatment, which corresponds to neutral mild laws, have a long lasting positive effect on contribution decisions because it does not indicate what one should do and thus avoids the conflict with what others effectively do.*

4.2 The interaction of the message with the mild laws

There are two questions we want to answer here. First, we want to understand the effect of the Message treatment when it is implemented in a neutral environment in periods 11-20 to its impact when used in an environment in which subjects had been accustomed to mild laws in the form of sanctions or incentives (and the message is in effect in periods 21-30). Second, we want to understand whether mild laws are more effective when employed in a ”neutral” environment – i.e. after the Baseline – than when preceded by the message indicating what is the ”moral behavior”. A real concern in answering these questions is that our design cannot disentangle two possible confounding effects: (i) a pure treatment effect and (ii) a time effect. The reason is that we are not comparing treatments in the same periods here. As we know from the existing literature, average contributions significantly decline over time (see Keser and van Winden 2000). Consequently, we might reasonably expect lower contribution levels in our third segment – periods 21-30 – compared to the second segment – periods 11-20 – ceteris paribus. In

Table 6: Changes in average contributions: periods 21-30 vs 11-20

Treatment	Change in contributions	Z	P-value
Sanction 21-30 vs 11-20	2.09	1.134	0.25
Message 21-30 (after Sanction) vs 11-20	-0.5	-0.718	0.47
Message 21-30 (after Incentive) vs 11-20	-0.02	-0.302	0.76

Note: p-values rely on non-parametric ranksum tests. Under the null hypothesis of no difference between conditions, the sum of the ranks should be equal across conditions. We treat average contributions of each 4-person group over the entire segment of 10 periods as a single independent observation.

other words, the results should be interpreted with caution because the effects of each treatment in periods 21-30 are, obviously, underestimated compared to their impact in periods 11-20.

To answer the first question, we compare average contributions under the Message in periods 11-20 to average contributions under the same treatment but when implemented in periods 21-30, either after the Incentive or after the Sanction treatments. When the Message is introduced at the end of the Sanction treatment, subjects contribute on average 6.5 tokens to the public account over periods 21-30. This is not statistically different from the 7 tokens contributed on average in periods 11-20 under the same treatment following the Baseline. We also find no significant difference when we compare average contributions over periods 21-30 when the Message is preceded by the Incentive (6.98 tokens) to average contributions under the Message in periods 11-20.

To answer the second question, we compare average contributions under the Sanction in periods 11-20 to average contributions under the same treatment but when it is in effect in periods 21-30, after the Message treatment. From Table 5, it appears that, despite time effects, individual contributions are slightly higher, on average, when the sanction follows the Message (10.81 tokens) than when it is implemented after the Baseline (8.72 tokens). However, the difference is not significant. Finally, it should be noted that the Sanction treatment displays the same trend over time in both cases, when used in periods 11-20 and when it is implemented in periods 21-30 – that is, there is a decay in average contributions over time.

Result 3*. The Message is ineffective at improving cooperation regardless of whether it is introduced in a neutral context or in an environment in which subjects had been accustomed to mild laws. Conversely, the Sanction treatment is effective both when employed in a neutral environment and when introduced* *after the Message. However, average contributions always decay over time under the Sanction treatment regardless of the segment in which it is implemented.*

5 Conclusion

Expectations are at the core of the expressive theories of law which argue that mild laws can change human behavior by changing people's beliefs about how others will behave in the future. As our experiment demonstrated, when mild laws are framed so as to indicate what is the group desirable action, cooperation rates are indeed affected by this particular framing. However, the effect is rather negative over time. The mere observation that others continue to follow their self-interest motivates conditional cooperators to reduce their own contributions. In other words, we found that cooperation is undermined when mild laws make people focus on the conflict between normative expectations ("what is believed to be desirable behavior at the group level") and the actual behavior of others ("what people observe that others do"). This is in line with Bicchieri and Xiao (2009) who show experimentally that managing empirical expectations (i.e. what others do) is crucial for designing institutions that encourage cooperative interactions. We complemented this literature by suggesting one way to overcome the conflict between normative and empirical expectations: lawmakers should be careful to present mild laws as mere reductions in the return from a neutral behavior. In effect, our analysis at the individual level showed that this kind of intervention is more likely to maintain higher cooperation rates over time. Although this seems to corroborate the traditional model of law and economics that considers a fine as a price, this conclusion would be incorrect. In effect, from the traditional law and economics perspective, the framing of the law should not matter at all. What is more, mild laws in general should not affect behavior because they do not modify the relative attractiveness of cooperation compared to defection. Our results suggest that, at least under our experimental conditions, the traditional framework does not provide accurate predictions about how mild laws impact human behavior.

The role of expectations is also at the core of our second finding that the message indicating what is moral behavior fails to increase cooperation. As we have shown, the principal limit to the well-functioning of such a message is the fact that when people interact with the same individuals over time, they expect the payoff-irrelevant message to have no impact on free-riding. This feature of human interactions – stable groups – prevents, in effect, people from updating their beliefs in an optimistic direction when they know, for instance, that they will face over time the same neighbors, or office colleagues.

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APPENDIX: INSTRUCTIONS (ORIGINALLY WRITTEN IN FRENCH)

GENERAL RULES

The experiment in which you are going to participate is part of a study on decision-making. Please read the instructions carefully. These instructions are meant to help you understand the experiment. Once all of the participants have read the instructions, the experimenter will then reread the instructions aloud.

Your gains will depend on your decisions as well as the decisions of other participants. All of your responses will be anonymous and will be gathered via a software program. You will indicate your choices on the computer in front of which you are seated, and this computer will calculate the gains you have realized in the course of the experiment. The sum total of money gained during the experiment will be paid to you in cash at the end of the experiment. From this moment on, we ask you to refrain from speaking. If you have a question, please raise your hand and an experimenter will help you in private.

The experiment is composed of three parts. Each part is made up of several periods. The instructions for Part 2 will be distributed to you once Part 1 has been finished, and the instructions for Part 3 will be distributed once Part 2 has been finished. One of the three parts will be drawn at random for payment. Your gain for the experiment will be equal to the gain you obtained during this Part. In the three parts, the gains are expressed in ecus. The conversion rate of emus into euros is 15 ecus = 1 euro.

At the beginning of Part 1, the central computer will create groups of four at random. The composition of the groups will remain unchanged for the duration of the experiment. You will not be able to identify the other members of your group and they will not be able to identify you.

PART 1

This part is composed of ten periods. At the beginning of each period, you as well as the other three members of your group will have an amount of 20 tokens that you must allocate between two accounts: your individual account and a collective account belonging to all of the members of your group (you included). Specifically, you must decide on the number of tokens that you put in the collective account. The remaining tokens are automatically placed in your individual account. You are free to put any whole number of tokens between 0 and 20 into the account.

Individual Account

Each token placed in your individual account earns you 1 ecu. And so, if for example, you put 6 tokens in your individual account, this will earn you 6 ecus. Your individual account only earns ecus for you alone.

The collective account

The collective account is shared by all members of your group. Each token placed in the collective account earns 0.4 ecus to each member of the group. Thus, if for example, you put 6 tokens in the collective account, this will earn 2.4 ecus for each member of the group (you included), amounting to a gain of 9.6 ecus for the entire group.

Gain

Your gain for the period is equal to the sum of the gains of your individual account and the collective account.

Example 1

You put 16 tokens in the collective account and thus 4 tokens in your individual account (put there automatically). Let us suppose that the three other members of your group put a total of 48 tokens in the collective account. In total, the collective account now comprises $48 + 16 = 64$ tokens. Your gain is then equal to 4 ecus (individual account) $+64$ * 0.4 (collective account) = 29.6 ecus.

Example 2

You put 3 tokens in the collective account and thus 17 tokens in your individual account. Let us suppose that the three other members of your group put a total of 40 tokens in the collective account. In total, the collective account now comprises $40 + 3 = 43$ tokens. Your gain is then equal to 17 ecus (individual account) $+43$ x 0.4 (collective account) = 34.2 ecus.

Final Details

All of the members of your group (you included) will make their decisions simultaneously. When all of the members of the group have made their decision, a summary screen will appear.

The screen will remind you of the number of tokens that you have placed in each of the two accounts and inform you of the number of tokens placed in the collective account by your group, and of your gain in ecus for the period.

At any time you can access the history of previous periods by clicking on the history button. The history will appear for each past period, the number of tokens that you put in each of the two accounts, the total number of tokens put in the collective account by your group, your gain in ecus for the period, and your cumulative gain since the first period of that Part.

PART 2

This part is also composed of 10 periods. The composition of the groups is unchanged. Your group is therefore composed of the same members as in Part 1. As in the preceding part, at the beginning of each period, you will have an amount of 20 tokens that you must allocate between your individual account and the collective account.

Additional instructions

From now on, the tokens that you do not put in the collective account, and that remain therefore in your individual account, are subject to a 30% deduction. Thus, 0.3 ecus are deducted for every token that remains in your individual account. This deduction is applied in each period. If for example, you put 12 tokens in your individual account, your gain from this account is $12 - 12$ x 0.3 (deduction) = 8.4 ecus.

The functioning of the collective account is the same as in Part 1 of the experiment: each token placed in the collective account therefore earns 0.4 ecus for each member of the group (you included).

Your gain for the period is still equal to the sum of the gains of your individual account and the collective account.

Example

You put 6 tokens in the collective account and therefore 14 tokens in your individual account. Let us suppose that the three other members of your group put a total of 26 tokens in the collective account. In total, the collective account now comprises $26 + 6$ = 32 tokens. Your gain is thus equal to 14 (individual account) $-$ 14 x 0.3 (deduction) $+$ 32 x 0.4 (collective account) = 22.6 ecus.

PART 3

This part is also composed of 10 periods. The composition of the groups is unchanged. In the same way, at the beginning of each period, you have 20 tokens that you must allocate between your individual account and the collective account. The functioning of these two accounts is the same as in Part 1. Thus, each token placed in your individual account earns you 1 ecu.

Additional instructions

At the beginning of each period, every participant in this room will have a message displayed on his or her computer screen. The message is the same for everybody.

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