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By: **David L. Dickinson**, David Masclat, & Marie Claire Villeval

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Norm enforcement in social dilemmas: An experiment with police commissioners[☆]

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A B S T R A C T

Do individuals trained in law enforcement punish or reward differently from typical student-subjects? We analyze norm enforcement behavior of newly appointed police commissioners in both a game with positive externalities (based on a Voluntary Contribution Mechanism) and a similar game with negative externalities. Depending on the treatment, a reward or sanction institution is either exogenously or endogenously implemented. Police commissioners cooperate significantly more in both games and bear a higher burden of the sanction costs compared to non-police subjects. When the norm enforcement institution is endogenous, subjects favor rewards over sanctions, but police subjects are more likely to vote for sanctions. Police subjects also reward and sanction more than the others when the institution results from a majority vote. Our experiment suggests that lab evidence on social dilemma games with positive or negative externalities and enforcement institutions is rather robust.

Keywords:

Norm enforcement

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Voluntary contribution mechanism

Police

Experiment

1. Introduction

When a police officer pulls you over, a reward for not breaking the law is usually the last thing on your mind. Yet, in recent years numerous police forces around the world have experimented with “positive

ticketing”, which involves giving out reward tickets and vouchers for good behavior.¹ These reward programs go against the old paradigm of the corrective policing model (Becker, 1968).

One obvious reason why police almost exclusively use sanctions is that it seems unnatural to reward those who comply with the law—rewards are usually reserved for going above-and-beyond a norm, if

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¹ For example, drivers in Sandy, Utah, were given vouchers for movie tickets for safe driving behaviors in 2013, and drivers in the south of France were given 20 Euro gas tickets for driving below the blood alcohol limit during 2014 New Year’s celebrations. Youth in Decatur, IL might receive a free food voucher for using crosswalks or skateboarding in designated areas, and Toronto Police have articulated hopes that positive ticketing will help facilitate communication and build trust in addition to encouraging good and legal behaviors (see <http://www.positiveticketing.ca/default.aspx>). While often targeted at youth, such programs may reward anyone behaving virtuously or simply not breaking the law. A positive ticketing program pioneer, Ward Clapham (a retired Canadian Mountie), estimates that over 25 countries currently use such programs to at least some extent (see <http://news.msn.com/world/police-hope-positive-tickets-will-reduce-crime>).

they are given at all. There may be additional reasons why police prefer sanctions to rewards for norm enforcement. Firstly, there may exist a pure framing effect: sanctions may resonate more with norm enforcement in “destructive” contexts compared to “constructive” contexts. Norm violations involving active destruction may trigger more negative emotions than norm violations involving passive acts of omission (e.g., failure to contribute to a public good). Secondly, sanctions may be more effective than rewards at norm enforcement when norm violation involves the destruction of wealth. Finally, one may conjecture the existence of a pure police-specific effect. Specifically, police officers are more exposed to destructive contexts and disorderly elements of society. As such, they may have a stronger inclination to use sanctions (Skolnick and Fyfe, 1995).²

Social dilemmas are popular for studying cooperation and social norms because group welfare is at odds with the dominant strategy of selfish free riding behavior. Early laboratory experiments have shown that initial contributions in Voluntary Contribution Mechanism games are substantially above the Nash prediction, but decline steadily as the game is repeated (Isaac et al., 1984; Andreoni, 1988; Ledyard, 1995). Research has also shown that, in otherwise parallel games, there is a reduced willingness to cooperate when externalities are negative rather than positive, because the warm glow is stronger than the cold-prickle (Andreoni, 1995). However, cooperation can be sustained in the long run when punishment is available (Yamagishi, 1986; Fehr and Gächter, 2000; Gächter et al., 2008). Punishment is typically directed at those who violate the norm of cooperation, which is given by the average group contribution level. This finding is robust to various environmental conditions (Maslet et al., 2003; Bochet et al., 2006; Anderson and Putterman, 2006; Carpenter, 2007). Other studies have attempted to investigate the effectiveness of reward mechanisms to enforce the norm of cooperation (Dickinson, 2001; Andreoni et al., 2003; Walker and Halloran, 2004; Sefton et al., 2007; Rand et al., 2009; Sutter et al., 2010; Dugar, 2013). Most of them show that rewards are somewhat less effective than sanctions in enforcing cooperation.

The originality of our paper is threefold. First, we experimentally investigate the effectiveness of punishment and reward institutions not only in a constructive (Giving Game) social dilemma context but also in an equivalent destructive (Taking Game) context. In the Giving Game non-cooperation is failing to contribute to a public account, whereas in the Taking Game, non-cooperation involves active withdrawals from a common account. The two games are linear public good/bad games with the same dominant strategy Nash equilibrium of no cooperation.³ While the effect of sanctions/rewards is well documented in a positive frame, their effectiveness is less well-established when the social dilemma is negatively framed. Using Common Pool Resource (CPR) games, some find that sanctions improve cooperation (Ostrom et al., 1992; Casari and Plott, 2003; van Soest and Vyrastekova, 2006), but others find the opposite result (Janssen et al., 2010; Cason and Gangadharan, forthcoming). In the same vein, the use of rewards to enforce cooperation in a CPR context has received less attention (exceptions include Vyrastekova and Van Soest, 2008; Stoop et al., 2013), even though its relevance to the real world is clear. One novelty of our design is that we can compare the impact of the various norm enforcement mechanisms in constructive versus destructive, but otherwise identical, linear social dilemmas. This allows us to test for social dilemma framing effects on sanctions and rewards.

² The comments of a police officer in the “Pops for Cops” program in Decatur, IL, illustrate this point: “Like many professional law enforcement officers, I brought a certain mentality to the job. I wanted to hunt down criminals – chase bad guys, kick in doors, get the bust. It was hunter vs. hunted... I can't escape the realities of my job – I have to hunt down criminals. But could I also work on the other end of the spectrum? Could I build positive relationships strong enough to keep youth out of trouble?”

³ Our Taking Game is therefore distinct from the Common Pool Resource game that represents a non-linear social dilemma game with an interior equilibrium in the choice space (Ostrom et al., 1992).

The second originality of our paper is that we enroll a representative sample of new French police commissioners to form mixed groups with participants from a standard student subject-pool. Our aim is to analyze whether police commissioners behave differently in terms of institutional choices and norm enforcement. This population is perfectly suited for our study because police commissioners have self-selected into a ‘mission-oriented’ occupation in the destructive context of crime deterrence, and because their training and core function are in law enforcement (Besley and Ghatak, 2005). Additionally, some of these commissioners had completed their training two years prior to our experiment, while others were still in training. This allows us to examine whether some experience with crime and enforcement affects behavior in our games. When comparing commissioners with non-police subjects, our intuition is that commissioners may have a stronger preference for sanctions due to both their occupational selection and explicit training to favor sanctions over rewards (Raganella and White, 2004; Wu et al., 2009).^{4,5} Our artefactual field experiment therefore contributes to discussion regarding the external validity of experiments by comparing career professionals with student-subjects (Dyer et al., 1989; Cadsby and Maynes, 1998; Cooper et al., 1999; Alatas et al., 2009; Carpenter and Seki, 2011). In his survey, Frechette (2015) finds no evidence that conclusions based on standard student-subject pools cannot generalize to professionals, as well as to the literature on how experience affects framing effects (e.g., Gächter et al., 2009; List, 2003).

Finally our third contribution is to vary the way the enforcement institution is implemented (exogenously or endogenously through a majority vote) so that we can test whether, as government agents, police commissioners are more willing to utilize an institution when it results from a democratic choice. We thus contribute to the literature on endogenous institutions in social dilemmas (Güerker et al., 2006; Tyran and Feld, 2006; Kosfeld et al., 2009; Sutter et al., 2010; Putterman et al., 2011; see Vyrastekova and van Soest, 2003 for a common pool context).

Our experiment consists of four treatments in both Giving and Taking Game contexts: Baseline, Reward, Sanction, and Vote. The Baseline treatment of the Giving Game is a linear public good game (public bad in the case of the Taking Game) without any enforcement institution. In the Sanction (Reward) treatment, a new stage is added. After being informed of each group member's contribution, subjects can sanction (reward) others at personal cost. Finally, in the Vote treatment, subjects vote in a preliminary stage for their preferred institution (Reward and Sanction), and the majority vote determines the institution that is implemented for all rounds.

Our results show that socially desirable behavior (i.e., contributing in the Giving Game or non-extracting in the Taking Game) is higher in the positive compared to the negative context; the existence of norm enforcement increases socially desired behavior; and police subjects contribute more (extract less) than non-police subjects. We also find that, after controlling for a possible selection bias in the decision to use the institution, the intensity of both sanctions and rewards is higher

⁴ After running our experiment, we became aware of another study of trust and norm enforcement conducted with applicants to the German police by Friebel and Kosfeld (2013). The two studies differ in several respects. First, their study focuses on how individuals self-select into an occupation based on their behavioral characteristics. Instead, we focus on comparing the use and efficacy of norm enforcement institutions in various environments given that subjects are police or non-police. We do not try to determine whether the behavior of police subjects is due to behavioral self-selection into the occupation or whether it results from the training in law enforcement they receive. Second, their subject-pool consists mainly of students in the final year of the high school, who may apply to the police. Instead, most French police commissioners hold a Masters degree. Third, their design is based on a trust game with a third-party and individuals can use both rewards and sanctions in the same periods (in ours it is one or the other), and it does not include endogenous institutions. Our studies are therefore complementary.

⁵ Prendergast (2007) shows that among public employees, if social workers are more likely to be biased in favor of their clients, police officers are more likely biased against their clients, i.e. those who break the law. If behavior in law enforcement transposes to norm enforcement, commissioners may be more inclined to sanction than reward.

in a negative context compared to a positive one. Interestingly, we find that police subjects enforce norms more than non-police subjects and particularly when the institution results from a majority vote. This helps explain the fact that the presence of more police subjects in a group raises its efficiency. Though all subjects prefer rewards, police subjects are relatively more likely to vote for sanctions. Perhaps relatedly, we also find that subjects are more likely to vote for the sanction institution when the vote occurs later in the game, because subjects will have had longer exposure to cooperative decay and norm violation.

2. Experimental design

Our design consists of four treatments: Baseline, Reward, Sanction, and Vote. The Baseline allows us to compare behavior against results established in the literature. The Reward and Sanction treatments add the possibility of assigning costly reward or punishment points, respectively, as a way of enforcing norms of cooperation. Finally, the Vote treatment implements an endogenous enforcement institution by allowing subjects to vote as to whether the reward or sanction institution should be used. These treatments are administered in both positive and negative frames. Therefore, we have a 4×2 mixed design (4 treatments within-subjects, and Taking or Giving Game as between-subjects factors). Police and non-police subjects were matched randomly in groups of five players, using an anonymous partner matching protocol.⁶

2.1. Treatment parameters

2.1.1. Baseline treatments

In the Baseline Giving Game treatment, each of the five homogeneously endowed subjects allocates 20 ECU (*Experimental Currency Units*) to a private or a group account. Payoffs, as a function of the contribution of subject i , x_i , are defined as follows:

$$\pi_i^{GG} = 20 - x_i + 0.3 \sum_{j=1}^5 x_j. \quad (1)$$

In contrast with [Fehr and Gächter \(2000\)](#), we give feedback regarding each other's member contribution after each period for consistency with our other treatments.

In the Baseline Taking Game treatment, each of the five group members may withdraw $w_i \in [0, 20]$ from the group account. The payoff function of subject i is the following:

$$\pi_i^{TG} = w_i + 30 - 0.3 \sum_{j=1}^5 w_j. \quad (2)$$

It can be easily seen from Eqs. (1) and (2) that the two games are equivalent with $w_i = 20 - x_i \forall i \in \{1, \dots, 5\}$.

2.1.2. Sanction treatments

The sanction institution is implemented exogenously. Each period now consists of two stages. The first stage is identical to the decision

⁶ Several reasons motivated our choice of using a partner matching design. The first reason is the necessarily limited number of police-subjects, and a stranger matching requires more participants to get sufficient independent observations. Second, our main variables of interest (framing and police effects) should not be directly affected by the matching protocol. Finally, police officers mention themselves that the relationship with criminals shares several features of a long term-relationship. Indeed, police officers are typically assigned to districts of relatively limited size where they often interact with the same individuals. Furthermore, if criminals are not necessarily confronted with the same commissioners in each instance, repeat interaction is still possible due to the prevalence of recidivism (see for example [Durose et al., 2014](#)).

stage in the Baseline treatment. In the second stage, the subjects have the opportunity to assign costly sanction points, P , to each other group member. The payoff function, shown in the case of the Taking Game, becomes:

$$\pi_i^{TG} = \left(w_i + 30 - 0.3 \sum_{j=1}^5 w_j \right) - \sum_{j \neq i} cP_{ji} - \sum_{i \neq j} kP_{ij} \quad (3)$$

where $\sum_{j \neq i} cP_{ji}$ is the total cost of sanctions assigned by subjects j to subject i , with c indicating the per-unit cost of each point received ($c = 2$ ECUs).⁷ $\sum_{i \neq j} kP_{ij}$ is the total cost to subject i of the sanctions she imposes on all subjects j , with $k = 1$ ECU being the cost to i of each sanction point assigned to any other player. A subject can assign a maximum of 10 sanction points to each other player. The total cost of points received cannot exceed the subject's earnings from the first stage.

2.1.3. Reward treatments

These treatments are similar to the Sanction treatments except that instead of punishing others, each subject can reward them by assigning reward points, R . Each reward point assigned costs $k = 1$ ECU and each reward point received increases one's payoff by $c = 2$ ECUs. The payoff function, again shown in the case of the Taking Game, is:

$$\pi_i^{TG} = \left(w_i + 30 - 0.3 \sum_{j=1}^5 w_j \right) + \sum_{j \neq i} cR_{ji} - \sum_{i \neq j} kR_{ij} \quad (4)$$

where $\sum_{j \neq i} cR_{ji}$ is the total rewards gain assigned by players j to player i and $\sum_{i \neq j} kR_{ij}$ is the total cost to player i of points assigned to others. A maximum of 10 points can be assigned to each other player and the gain of the total points received cannot exceed the subject's earnings from the first stage.

2.1.4. Vote treatments

The sanction or reward institution is implemented endogenously by adding a preliminary stage where subjects vote only once for whether the reward or sanction institution should apply to the subsequent rounds in the treatment. Voting entails no monetary cost, the majority vote institution is implemented, and there is no feedback on individual votes of others.⁸

2.2. Experimental procedures

The experiment was programmed using the z-Tree software ([Fischbacher, 2007](#)). All sessions were conducted at GATE-LAB, Lyon, France. There were nine sessions in total (five Taking Game and four Giving Game sessions). Within a session, the Baseline (B) treatment was always played first, and Reward (R) and Sanction (S) treatments were always adjacent (but counterbalanced in order). The Vote (V) treatment was counterbalanced to be either before or after the Reward and Sanction treatments, which allows us to examine institutional experience effects on reward/sanction preferences. [Table 1](#) displays session details, including the treatment orderings administered.

⁷ In [Fehr and Gächter \(2000\)](#) and in most following studies on sanctions in VCM games, the cost ratio is 1:3. We chose a weaker 1:2 ratio because we wanted to hold this ratio constant for reward and sanction points. We feared that using a higher ratio for reward points would lead subjects to assign points to others not to enforce the cooperation norm, but to create reciprocity and increase payoffs. A lower ratio should, we thought, limit this motivation.

⁸ Our procedure differs from [Sutter et al. \(2010\)](#) where subjects could vote between standard VCM, reward, and punishment by approval voting; voting was costly, voluntary, and repeated until unanimity was achieved.

Table 1
Description of sessions.

Session	Sequence	Game	# Police/total # subjects	# Groups, by # police subjects in the group					
				0	1	2	3	4	5
1	BRSV	Taking	10/20	–	1	1	1	1	–
2	BRSV	Giving	9/20	1	–	1	1	1	–
3	BSRV	Taking	10/20	–	–	2	2	–	–
4	BSRV	Taking	10/20	–	1	–	3	–	–
5	BVRS	Taking	10/20	–	–	2	2	–	–
6	BVSR	Taking	10/20	–	–	2	2	–	–
7	BSRV	Giving	11/20	–	–	2	1	1	–
8	BVRS	Giving	9/20	–	–	3	1	–	–
9	BVSR	Giving	8/20	1	1	1	–	–	1
Total			87/180	2	3	14	13	3	1
Percentage			48.33	5.56	8.33	38.89	36.11	8.33	2.78

Note: B: Baseline treatment; R: Reward treatment; S: Sanction treatment; V: Vote treatment.

In a session, subjects played ten periods in the Baseline and seven periods in each of the three other treatments.⁹ It was common knowledge that, while the composition of the group was fixed throughout the game, the group member ID numbers displayed on the feedback screens were reshuffled at each period so that it was impossible to reciprocate the action of a specific group member in the previous period. This was intended to prevent the possibility of individual reputation formation and to avoid counter-punishment (Fehr and Gächter, 2000; Denant-Boemont et al., 2007; Nikiforakis, 2008) or counter-rewarding (Stoop et al., 2011). The final earnings in these tasks were the sum of payoffs from all 31 periods.

Each session involved 20 subjects, 8 to 10 of whom were police commissioners. Participants were not aware of the characteristics of the other participants: students did not know that they were interacting with police subjects. The right part of Table 1 indicates the distribution of the groups based on the number of police subjects in the group. In 27 out of the 36 groups of 5 members (75%) there were either 2 or 3 police subjects. Thus the commissioners and students are not perfectly evenly balanced across groups but the distribution does not differ across frames.

Upon arrival, and after informed consent was obtained, subjects drew a ticket from an opaque bag assigning them to a specific terminal in the laboratory. Experimental instructions were distributed for each part after completion of the previous part (see Appendix). Instructions used neutral wording such as contribution (Giving Game) and withdrawal (Taking Game). Sanctions (rewards) were labeled as “points that decrease (increase) others’ payoffs”. After reading the instructions aloud, we used a questionnaire to verify each subject’s understanding and any questions were answered in private. Before the subjects played the Taking or the Giving game, we elicited their risk attitudes and degree of trust.¹⁰ Sessions lasted 2 h on average. Average earnings were €26.19 (S.D. = 4.39), which were paid in private in a separate room.

2.3. Subject pools

In total, 180 subjects from two different pools participated in this experiment. The regular pool includes 93 subjects who were

recruited via the ORSEE software (Greiner, 2004). Most were undergraduate students from local engineering and business schools; a minority of subjects were older participants, either campus employees or retirees.

The other subject-pool consists of 87 volunteers who had recently passed the very competitive national exam of police commissioners. We have two main types of subjects within this pool. The majority (73 of 87) were still studying in the elite national school of police commissioners (*Ecole Nationale Supérieure de la Police*, ENSP, a French “Grande Ecole”), the only school for police commissioners in France, located near Lyon. This sample is a representative of the population of France’s newly appointed police commissioners since each cohort includes 40 students who spend 2 years at ENSP (meaning that the participation rate for the students at ENSP is above 90%). During their training they spend 14 months as interns in operational services; the rest of the time, they are trained in human resources management, intelligence services, and management of public order. A minority (14 subjects) had finished training at ENSP two years prior and either had a permanent position as police commissioner or was in an internship at the time of our experiment. Specifically, at the end of training new commissioners will have to lead specialist departments or police districts and they are immediately appointed to small jurisdictions or are deputy commissioners in medium-sized town police stations. They are directly involved in police duties (maintaining public order, homeland defense, investigations, road safety, policing of organized crime or money laundering, etc.). They direct the work of policemen and lead critical operations.¹¹ These subjects, who have therefore had more crime exposure than those still in training, participated in our experiment during a post-training return to ENSP. Compared to our non-police subjects, police subjects are older (32.82 ± 7.37 year old, min = 23, max = 48 vs. 24.82 ± 9.22 year old, min = 18, max = 64) and more typically male (79.66% vs. 44.26%). Due to these differences, we systematically control for age and gender in our regression analysis.¹²

⁹ Initially, we planned to run 10 periods in each of the four treatments. Unfortunately, and despite a pilot session with our usual subject-pool, we realized during the first session that some subjects were very slow to make decisions. Therefore, we decided to reduce the number of periods to 7 after the Baseline treatment. We kept the same structure for the remaining sessions. For comparisons in the data analysis, we use periods 1 to 7 for all treatments except for the Baseline.

¹⁰ To elicit risk attitudes we used the Holt and Laury (2002) procedure. We found no significant difference between subject pools (the mean numbers of safe choices out of 10 were 5.83 (1.78) and 5.82 (1.71) for police and non-police subjects, respectively (Mann-Whitney test, two tailed, $p = 0.910$)). Then, participants played a trust game (Berg et al., 1995) in both roles under the strategy method. No significant difference was found either in trusting behaviors (the mean amounts sent by player A to player B out of 10 were 2.69 (SD 1.77) and 2.41 (SD 1.88) for police and non-police subjects, respectively ($p = 0.448$)). Feedback on these two tasks was only given at the end of the session.

¹¹ We want to specifically highlight that these subjects are *not* police officers with some supervisory duties but rather, individuals trained as police commissioners, who oversee or will oversee entire city police forces.

¹² With non-standard subject pools, it is especially important to avoid experimenter demand effects, which may be motivated by social desirability, because they could generate differences of behavior. As remarked by Zizzo (2010), what is crucial is the belief that the subjects hold about the experimenters’ objectives and how they can be related to the real objectives. Our subjects were very likely not able to identify the aim of our study. First, they had to participate in several tasks, making the environment multi-dimensional and our objective not transparent. Second, only the directors of the school were aware of the content of our tests and they committed not to reveal it to the participants. Third, it was made clear to the participants before the experiment that the results were collected for scientific reasons and that no individual data would be communicated to the school at any time. Furthermore, participants committed not to reveal the experimental protocol to other subjects before completion of all the sessions.

3. Theoretical predictions

3.1. Standard predictions

We first derive predictions based on rational and selfish money maximizing agents, in which case the predictions are similar for police and non-police subjects. The equilibrium in a negative frame (Taking Game) is the same as in a positive one (Giving Game). Thus, we will merely describe predictions as a function of the enforcement institution and not the framing of the social dilemma.

In the Baseline treatments, it can easily be seen from Eqs. (1) and (2) that the dominant strategy Nash equilibrium is for all subjects to place all 20 ECU in their private account ($x_i = 0$) contributing nothing or, alternatively, extracting all from the public good ($w_i = 20$) in each round of this finitely repeated game. In equilibrium, each subject earns 20 ECU and total group earnings are 100 ECU. In contrast, the strategy leading to the Pareto optimum is $x_i = 20$ (or, $w_i = 0$), which generates total group earnings of 150 ECU (30 ECU per subject).

In both the Sanction and the Reward treatments, the only Subgame Perfect Equilibrium, whether played once or finitely repeated, is also $x_i = 0$ or $w_i = 20$ for all i and no use of sanctions or rewards because both are costly. In the Vote treatment, self-interested individuals should be indifferent between available institutions. Contribution or withdrawal behavior should not be affected by the choice of enforcement institution since sanctions/rewards are not credible. Thus, with money-maximizing self-interested play, the equilibrium prediction is the same across all enforcement institutions, games, and subject pools. Observed differences must therefore be attributed to behavioral factors resulting from a desire to enforce behavioral norms and/or the psychological impact of a positive vs. negatively framed social dilemma decision environment.

3.2. Behavioral predictions

Our first set of conjectures concerns the norm of cooperation in the Baseline. Introducing other-regarding preferences might take the form of advantageous inequity aversion (Fehr and Schmidt, 1999), social preferences for fairness and efficiency (Charness and Rabin, 2002), or imperfect conditional cooperation (Fischbacher et al., 2001; Fischbacher and Gächter, 2010). Such theories predict that cooperation may result with sufficient numbers of cooperators in a group, though contributions may still decline over time. One may therefore conjecture that people with other-regarding preferences should contribute positive amounts of their endowment in all treatments.

Regarding subject pool effects, we predict that police commissioners will be more cooperative than others, given their stronger norm of civic cooperation and because they self-selected into a public good oriented occupation where helping others is key motivation (Raganella and White, 2004). Indeed, civil servants, whose job is to serve public interests, might be more cooperative because people in mission-oriented jobs are usually intrinsically motivated agents (Besley and Ghatak, 2005) who place more value on the output of public organizations (François, 2000).¹³ This prediction is also based on evidence from cross-cultural studies showing that cooperation in social dilemmas is higher in countries with stronger norms of civic cooperation and weak laws (Herrmann et al., 2008).

Based on the previous literature, we also expect to observe framing effects that promote higher cooperation in the Giving Game than in the Taking Game (Andreoni, 1995; Park, 2000; Messer et al., 2007). We summarize our first set of conjectures as C1:

- C1: a) Police subjects cooperate more than non-police subjects.
- b) Cooperation is more likely in the Giving Game than in the Taking Game.

Our second set of conjectures is related to exogenous norm enforcement institutions. Sutter et al. (2010) demonstrate that the use of rewards in equilibrium is predicted by the Charness and Rabin (2002) model of social preferences only if there are enough subjects who value social welfare as strongly as their own payoff. Punishment in equilibrium is predicted by the inequity aversion model only under very restrictive conditions. Based on previous evidence, we conjecture that a fraction of the subjects will sacrifice resources to sanction and/or reward and that rewards will be preferred to sanctions because they are more efficient (Sefton et al., 2007; Rand et al., 2009; Sutter et al., 2010). Because their occupation is mission-oriented (Besley and Ghatak, 2005) and because it requires expertise in law enforcement to fight deviant behavior (Wu et al., 2009), we conjecture that police subjects will be willing to bear a disproportionate burden of enforcement costs. Finally, regarding framing effects, we expect more sanctions in the Taking Game both because we predict less cooperation and because norm violation involves active destruction of the public resource. Such an act of commission may trigger negative emotions more than the norm violation by an act of omission (i.e., failure to contribute in the Giving Game). In the same vein, not withdrawing money from the existing group account may also trigger more positive emotions than non-contributing in the Giving Game.

We summarize our second set of conjectures as C2:

- C2: a) Police subjects enforce norms more than non-police, particularly with sanctions and in the negative frame. b) More sanctions (rewards) are assigned in the Taking Game.

Our last set of conjectures is related to the endogenous norm enforcement institutions. Because police fight crime with threats and sanctions, we conjecture that police commissioners will be more inclined than non-police subjects to vote in favor of sanctions. We also predict that, as government agents, police will use a norm enforcement institution more intensely when it results from a vote. Finally, we predict that endogenously selected institutions will promote more cooperation (Tyran and Feld, 2006; Kosfeld et al., 2009; Sutter et al., 2010). We summarize our third set of conjectures as C3:

- C3: a) Police subjects vote more in favor of the sanction institution than others. b) Subjects, especially police subjects, use enforcement more intensively when the institution results from a democratic vote. c) Endogenous institutions increase cooperation relative to exogenously implemented institutions.

4. Results

4.1. Cooperation

In what follows, 'contribution' refers to the amount *not* extracted from the group account in the Taking Game or the amount placed in the group account in the Giving Game. Figs. 1 and 2 display mean contributions over time per treatment in the Giving and Taking games, respectively.

¹³ Investigations on the motivation for entering the police emphasize the importance of non-monetary dimensions, such as the willingness to help people and social status (Hageman, 1979; Moon and Hwang, 2004; Raganella and White, 2004; Tarnig et al., 2001; Wu et al., 2009). Social contribution is also ranked as the first determinant of job satisfaction by police officers (Carlan, 2007).

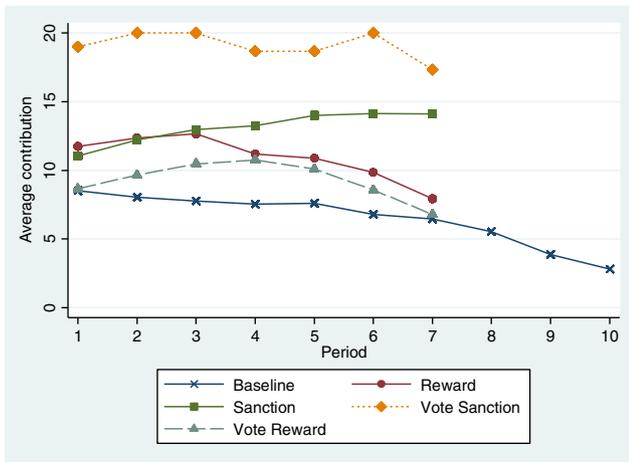


Fig. 1. Evolution of the average contribution over time, by treatment – Giving Game.

Both figures exhibit the standard decay of contributions over time in the Baseline treatments.¹⁴ Focusing first on the exogenous enforcement institutions, we find that mean contributions are significantly higher in Sanction treatments compared to Baseline (Mann–Whitney test, $p < 0.001$ and $p = 0.025$ in the Giving and the Taking Games, respectively).¹⁵ Mean contributions are also higher in the Reward treatments compared to Baseline treatments ($p = 0.029$ and 0.051).¹⁶ Sanctions tend to outperform Rewards, but the difference is not significant ($p = 0.291$ and 0.204). These findings are consistent with previous literature. Figs. 1 and 2 also indicate that when a vote resulted in the sanction institution, cooperation increases dramatically compared to Baseline ($p = 0.007$ and 0.001 , in Giving and Taking Games, respectively). Endogenous sanctions also increase contributions compared to exogenous sanctions in the Giving Game ($p = 0.065$), but not in the Taking Game ($p = 0.221$). Compared to Baseline, the increase in contributions from endogenous rewards is not statistically significant ($p = 0.148$ and 0.484 in the Giving and the Taking Games, respectively), nor is the increase seen with exogenous rewards ($p = 0.511$ and 0.526). This is intriguing, as we could have expected that the positive signal given by a majority voting in favor of rewards would have resulted in a higher willingness to contribute. Regarding framing effects, our data show that contributions are higher in the Giving compared to Taking Game, but the differences are not significant. Baseline mean contributions (ECUs) are 6.49 in the Giving Game and 5.46 in the Taking Game ($p = 0.324$). In Sanction treatments they are 13.10 (Giving Game)

¹⁴ Since we were not able to randomize all the possible orderings of treatments, due to an insufficient number of police-subjects, the Baseline treatment was always played first. This may have transferred learning to the following treatments (Andreoni, 1988). It is therefore important to check that our treatment differences are not driven by a different behavior in the Baseline. We tested that the mean contribution behavior in the Baseline is not different across the various sequences of treatments (BRSV, BSRV, BVRS and BVS), by means of Kruskal–Wallis tests. The mean group contribution in the first ten periods gives one independent observation. No significant difference across sequences of treatments was found ($p = 0.141$ in the positive frame and $p = 0.679$ in the negative frame).

¹⁵ Unless if specified otherwise, all the non-parametric tests reported in the paper are two-tailed Mann–Whitney tests in which the group mean’s behavior (contribution, for example) over the set of periods in one treatment is taken as one independent observation. This implies that when we compare contributions in two treatments, we do not take into account the fact that a treatment may have a different location in a sequence across groups. This is a limitation because it does not take into account sequence order effects. This constraint comes from the limited number of groups in each treatment of each sequence. Therefore, the results of the tests should be taken cautiously.

¹⁶ Our non-parametric tests are qualitatively similar when one restricts our data analysis to the first seven periods in all treatments including the Baseline (see footnote 9). The only exception is that the difference between the Baseline and the Reward treatments in the Taking Game is no longer significant. Our econometric analysis is however unchanged whether or not we restrict the number of periods in the Baseline to the first seven periods.

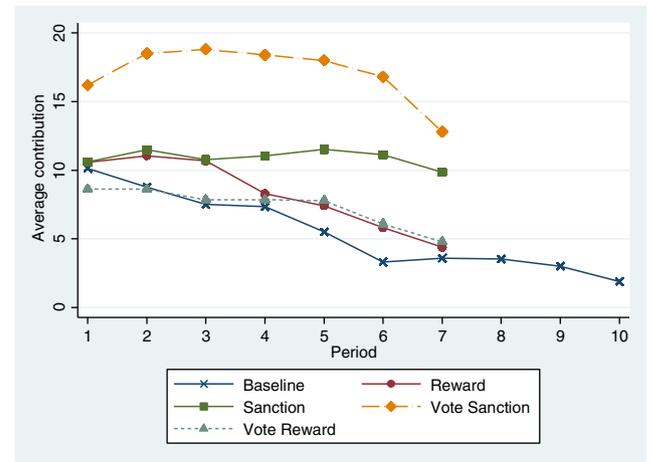
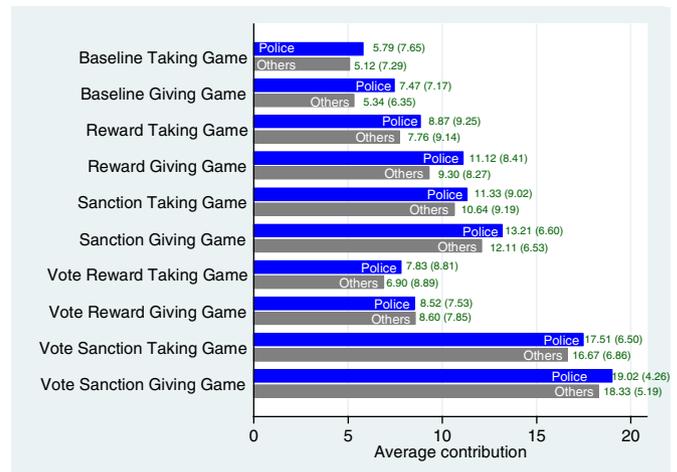


Fig. 2. Evolution of the average contribution over time, by treatment – Taking Game.

compared to 10.91 (Taking Game) ($p = 0.347$), and in Reward treatments they are 10.94 (Giving Game) and 8.32 (Taking Game) ($p = 0.156$).

We turn next to comparisons across subject-pools. Fig. 3 displays average contribution levels for police and non-police subjects, by treatment in the Giving and the Taking Games. This figure suggests that in all treatments police subjects cooperate more on average than non-police. However, we find no statistically significant subject pool effects in any treatment ($p > 0.10$ in all cases). If we pool the Taking game and the Giving game together, then we find that the Vote-Sanction treatment police subjects contribute significantly more than non-police (18.42 vs. 17.03 ECUs, $p = 0.088$). The fact that most non-parametric tests fail to identify significant effects here is not that surprising since our subject-pools differ on characteristics not controlled for in the non-parametric analysis. We therefore appeal to parametric data analysis to further investigate differences across subject pools.

Table 2 reports the marginal effects of two random-effects Tobit regressions analyzing the determinants of the contribution decision. Random effects control for the lack of independence of the contributions of a given subject across decision rounds and Tobit models are justified by the fact that contributions are censored both at 0 and 20 ECU. We pool the data from all treatments and we standardize contributions of



Note: Mean values are indicated at the right of each bar. Standard deviations are indicated in parentheses.

Fig. 3. Average contributions (i.e., amount relative to efficient outcome), in ECU. Note: Mean values are indicated at the right of each bar. Standard deviations are indicated in parentheses.

Table 2
Determinants of contribution levels.

Dependent variable	Random effects Tobit models	
	(1)	(2)
Contribution		
Sanction treatment	1.373***(0.110)	1.399***(0.111)
Reward treatment	0.837***(0.091)	0.853***(0.092)
Vote Sanction treatment	2.991***(0.229)	3.045***(0.230)
Vote Reward treatment	0.681***(0.088)	0.694***(0.089)
Giving Game	0.484** (0.189)	0.469** (0.194)
Order of treatment	-0.150*** (0.030)	-0.153*** (0.030)
Period	-0.121*** (0.011)	-0.123*** (0.012)
Last period	-0.130** (0.062)	-0.131** (0.063)
Police	-	0.432* (0.224)
Police with experience	-	-0.342 (0.375)
Age	-	-0.007 (0.012)
Male	-	0.066 (0.202)
Trust	-	0.056 (0.051)
Political orientation	-	0.023 (0.045)
N	5580	5580
Left-censored obs.	1903	1903
Right censored obs.	1643	1643
Log-likelihood	-10,524.828	-10,521.476

Note: This table reports marginal effects. Standard errors are in parentheses. *** ** * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively for the 2-tailed test. The 5580 observations correspond to the 180 subjects observed in 31 periods of game.

individual i in period t as the dependent variable by defining contributions in the Taking Game as the amount $(20 - w_{it})$ that is *not* withdrawn. Independent variables include a dummy variable for the *Giving Game* and treatment dummies (*Baseline* is the omitted reference treatment). *Order of treatment* captures whether the treatment was played 1st, 2nd, 3rd, or 4th within the session. We also include controls for *Period* (1–7) and a *Last period* dummy to control for end-game effects. Model (2) adds a *Police* subject dummy, a *Police with experience* dummy for commissioners in an active position and variables for age, gender, trust and political orientation.¹⁷

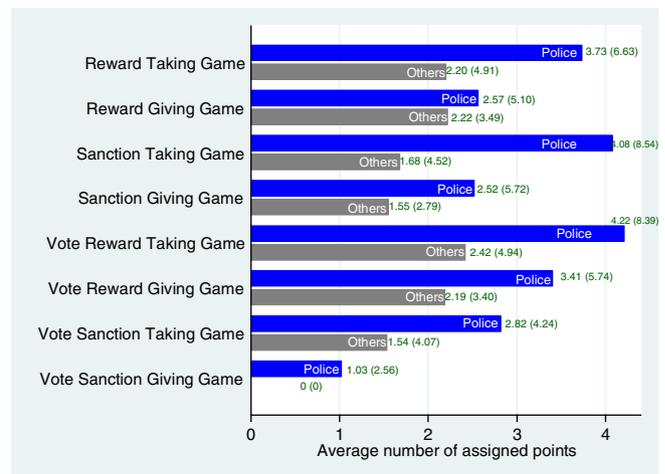
Table 2 shows that, relative to *Baseline*, cooperation is higher when norm enforcement is possible. This is especially true with *Sanction*, as its coefficient is significantly higher than the coefficient on the *Reward* treatment. We also find that endogenous sanctions (*Vote Sanction*) lead to even higher contributions than exogenous sanctions. T-tests for all these pairwise comparisons in the models give $p < 0.001$. In contrast, the coefficient on *Vote Reward* is significantly smaller than the coefficient on *Reward* ($p = 0.008$). This could result from the fact that a majority vote in favor of sanctions is a clearer signal against free-riders than a vote in favor of rewards. These estimated effects are robust across models.¹⁸

Table 2 also shows that the police-subjects contribute significantly more than the non-police subjects.¹⁹ This finding supports conjecture C1a. Experience in police work has no additional effect. Once we control for other variables, contributions are significantly higher in the *Giving Game* than in the *Taking Game*, which supports C1b. Not surprisingly, cooperation declines over time, both across periods within a treatment and across treatments within a sequence. In additional estimations (available upon request), we also tested whether the framing effect

¹⁷ “Trust” corresponds to the amount sent by player A to player B in the trust game. The “political orientation” variable corresponds to the response to the following question in the post-experimental questionnaire: “In politics one usually speaks of right and left. Where do you situate yourself on a scale from 1 on the left to 10 on the right?”

¹⁸ We also ran these regressions after adding interaction terms between the *Vote Reward* and the *Vote Sanction* treatments, respectively, and a binary variable capturing the fact that the subject voted or not against the majority of his group. These terms are not significant in any model, showing no evidence of a crowding-out effect of not belonging to the majority on contributions (regressions available upon request).

¹⁹ Gächter et al. (2004) also report that non-students contribute more than students but their results are from a one-shot VCM environment as opposed to our multi-period environment. In what follows, our additional results regarding norm enforcement and preference for punishment over rewards helps us attribute our subject pool effects to the special attributes of police officers, as opposed to non-students.



Note: Mean values are indicated at the right of each bar. Standard deviations are indicated in parentheses.

Fig. 4. Average enforcement points assigned. Note: Mean values are indicated at the right of each bar. Standard deviations are indicated in parentheses.

was different across subject pools by including an interaction term *Police Subject* Giving Game*. This variable is not significant, indicating that some experience with real world norm enforcement does not impact framing effects on cooperation in our experiments.²⁰ We also fail to find any significant effect of a variable capturing the number of police subjects among the other group members, which is not surprising since this information was unknown to the subjects.

These findings are summarized in Result 1.

Result 1. a) Police subjects contribute significantly more than non-police subjects. b) Contributions are significantly higher in the *Giving Game* than in the *Taking Game*. c) Cooperation is higher when norm enforcement is possible. d) Sanctions are more effective than rewards. e) Endogenous sanctions lead to higher contributions than exogenous sanctions.

4.2. Sanctions and rewards

Fig. 4 shows the average assigned enforcement points per treatment and subject-pool. It shows that subjects are willing to use both rewards and sanctions to enforce norms, even at personal cost. This figure also shows no systematic tendency to use sanctions more intensively relative to rewards, except when the institution is endogenous. In this case, subjects are more willing to reward than punish in the *Giving Game* (Mann–Whitney test, $p = 0.093$), but not in the *Taking Game* ($p = 0.513$). Fig. 4 suggests that police subjects enforce norms more intensively than non-police subjects in all treatments. Finally, it shows that the mean number of both sanction and reward points is lower in the *Giving Game* than in the *Taking Game*.

Table 3 analyzes the determinants of norm enforcement by means of a random-effects GLS model (column 1) and two random-effects Tobit models (columns 2 and 3). The dependent variable is the number of assigned points (both punishment and reward points) by each player i to each other player j within her group. The independent variables in Table 3 are mostly similar to those in Table 2. In model (1), we include controls for the *Giving Game* and for each enforcement institution (*Reward* treatment is the reference category). Controls are included for order of treatment in the sequence, period of the game, a dummy for last period and demographics. Model (2) adds controls for the points received in the previous period and dummies for police subjects. Finally,

²⁰ This contrasts with other studies finding that real world experience reduces framing effects (e.g., List, 2003; Gächter et al., 2009).

Table 3
Determinants of sanction or reward decisions.

Dependent variable: Number of reward or sanction points	Random effects	Random effects	Random effects
	GLS	Tobit	Tobit
	(1)	(2)	(3)
Sanction treatment	-0.072** (0.029)	-0.085*** (0.024)	-0.125*** (0.034)
Vote Sanction treatment	0.004 (0.057)	-0.079 (0.052)	-0.229*** (0.089)
Vote Reward treatment	0.032 (0.032)	-0.020 (0.025)	-0.057 (0.035)
Giving Game	-0.187# (0.115)	0.072 (0.083)	0.069 (0.083)
Order of treatment	-0.147*** (0.015)	-0.105*** (0.012)	-0.106*** (0.012)
Period	-0.106*** (0.007)	-0.066*** (0.007)	-0.066*** (0.007)
Last period	0.077* (0.043)	-0.012 (0.036)	-0.012 (0.036)
Police	-	0.189** (0.096)	-
Police with experience	-	0.045 (0.163)	0.040 (0.161)
Police*Sanction treatment	-	-	0.210** (0.100)
Police*Reward treatment	-	-	0.132 (0.099)
Police*Vote Sanction treatment	-	-	0.364** (0.140)
Police*Vote Reward treatment	-	-	0.205** (0.101)
Points received in previous period	-	0.056*** (0.003)	0.056*** (0.003)
Age	0.011* (0.006)	0.006 (0.005)	0.006 (0.005)
Male	0.168 (0.120)	0.001 (0.087)	-0.002 (0.09)
Trust	0.040 (0.031)	0.021 (0.022)	0.006 (0.005)
Political orientation	-0.047* (0.027)	-0.002 (0.019)	-0.003 (0.019)
Constant	1.222*** (0.255)	-	-
N	15,120	12,960	12,960
Left censored obs.	-	9954	9954
Log-likelihood	-	-11,450.914	-11,447.505

Note: This table reports marginal effects for the Tobit models. Standard errors are in parentheses. GLS: Generalized Least Squares. ***, **, * and # indicate significance at the 0.01, 0.05, 0.10, and 0.11 levels respectively for the 2-tailed test. The 12,960 observations correspond to the 180 subjects' decisions regarding each of their four group members in the 18 periods of game where sanctions or rewards can be assigned (excluding the first period of each of the three treatments due to the introduction of the lagged variable points received in $t-1$).

model (3) includes several interaction variables to check whether police subjects' decisions differ across treatments. Table 3 reports marginal effects.

Model (1) shows that subjects assign significantly fewer punishment points compared to reward points, and the number of points assigned declines over time and over the sequence of treatments, possibly because the use of norm enforcement is less necessary once it has been previously used within a group. It also shows that subjects tend to assign fewer points in the Giving game compared to a negative frame, although this effect fails reaching significance ($p = 0.11$). Model (2) indicates that police subjects engage significantly more resources in norm enforcement than others and model (3) shows that, in general, police subjects distribute more sanctions than others but not more rewards. Police subjects also use both rewards and sanctions significantly more than others when the institution is implemented by vote.

The Tobit regressions shown in Table 3 are based on the strong assumption that independent variables affect the decision to punish (reward) and the intensity of punishment (rewarding) to the same degree. However, this may not be the case. Thus, alternative Heckman two-step estimation procedures are reported in Table 4 to separately analyze the determinants of sanction points assignment (columns (1) to (3)) or reward points assignment (columns (4) to (6)) by a subject to each other group member. Specifically, a selection equation is first estimated by means of a random-effects Probit to explain the binary decision to punish (reward) (columns (1) and (4)). We then explain the intensity of punishment (rewarding), conditional on the decision to punish (reward), corrected for a potential selection bias via the introduction of the inverse of the Mills ratio (IMR) from the

selection equation as an explanatory variable. This second equation is estimated as a random-effects Generalized Least Squares in columns (2) and (5) for sanctions and rewards, respectively. The independent variables in the selection equations are mostly similar to those in Table 3. In addition, we add variables measuring a positive deviation (*Pos Dev Avg*) and the absolute value of a negative deviation of the contribution of the subject being assigned points from the rest of the group average ($|Neg Dev| Avg$). We also include the *Avg Contr Others* variable that measures the effect of others' contributions (excluding the target's contribution but including the subject's contribution) on one's own decision to assign points. The second-step GLS regressions include the same variables as the selection equations except for the time trend variables (period and end game) that allow us to identify the model. In columns (3) and (6) we add interaction terms to check whether police subjects' decisions differ when the institution is chosen by the majority of members in the group. Table 4 reports marginal effects.

The left panel of Table 4 indicates that the probability of assigning sanctions declines over time. Column (1) shows that older participants are more likely to sanction than younger subjects. Subjects contributing more (less) than the group average are less (more) likely to be punished, and subjects are less likely to punish the more others contribute. There is evidence of blind negative reciprocity, which suggests that retaliation is also important in our experiment, as in Herrmann et al. (2008). This effect is stronger in a positive frame as shown by the positive coefficient associated with the interaction variable *Points received in $t-1$ * Giving Game*. Complementary estimates (available upon request) in which we interact *Police* with *Received points in $t-1$* show that being a police subject has no effect on negative reciprocity. Finally, the Giving Game variable captures a positive coefficient, suggesting that subjects are more likely to punish in a giving context compared to a destructive context. However, this effect is counterbalanced by the fact that the intensity of punishment is significantly higher in the Taking Game, which partly supports our conjecture that a negative frame triggers more emotional arousal.²¹ In column (1), the coefficient associated with police subjects with more experience is significant ($p = 0.090$), suggesting that these subjects are more likely to punish free riders than others. Moreover, columns (2) and (3) show that police subjects assign significantly more punishment points than non-police subjects and this effect is stronger when punishment results from a majority vote (model (2)), in particular for more experienced commissioners (model (3)).

The right panel of Table 4 reveals that the probability to reward also declines over time while it increases if the institution results from a majority vote. Subjects are more likely to reward above-average contributors, and more likely to reward in general when others contribute more (model (4)). There is also evidence of blind positive reciprocity and complementary regressions (not reported here) show no impact of being a police subject on blind reciprocity. Being more trustful significantly increases the probability to assign reward points.

The Heckit estimates indicate that, after controlling for the selection bias (the IMR coefficient is significant), the intensity of rewards is significantly lower in the Giving Game compared to the Taking Game, which is consistent with our conjecture.²² Surprisingly, the intensity

²¹ The fact that the Giving Game variable captures a positive and significant coefficient in the selection model contrasts with the findings on the intensity of punishment. On the one hand people may be more willing to contribute to the second order public good (if one considers the punishment itself as a public good) in the Giving Game, which may motivate them to punish more (binary decision). On the other hand, emotions triggered by deviations may be stronger in the Taking Game, which may translate into a higher intensity of punishment in this game.

²² The IMR is significant, indicating that it is important to respect the sequential structure of the rewarding decision and analyze separately the binary decision to reward and the intensity of rewards. The IMR was not significant in the sanction estimates, indicating that simple GLS models would have provided rather similar findings.

Table 4
Determinants of sanction and reward decisions.

Dependent variable:	Binary decision to punish RE Probit	Intensity of punishment RE GLS	Intensity of punishment RE GLS	Binary decision to reward RE Probit	Intensity of reward RE GLS	Intensity of reward RE GLS
	(1)	(2)	(3)	(4)	(5)	(6)
Vote sanction/ Reward treatment	−0.016 (0.018)	−0.823 (0.940)	−0.691 (0.945)	0.015* (0.008)	−0.511*** (0.099)	−0.510*** (0.099)
Giving Game	0.061** (0.030)	−1.011** (0.473)	−0.991** (0.475)	−0.014 (0.038)	−0.707** (0.338)	−0.710** (0.339)
Order of treatment	0.013 (0.009)	−0.151 (0.205)	−0.362 (0.227)	−0.030*** (0.006)	−0.236*** (0.051)	−0.236*** (0.051)
Period	−0.016*** (0.003)	−	−	−0.019*** (0.003)	−	−
Last period	0.008 (0.013)	−	−	−0.038*** (0.015)	−	−
Received points in <i>t</i> -1	0.003** (0.001)	−0.004 (0.026)	−0.009 (0.026)	0.008*** (0.002)	0.038*** (0.012)	0.038*** (0.012)
Received points in <i>t</i> -1 *Giving Game	0.005** (0.002)	0.013 (0.039)	0.019 (0.039)	0.004* (0.002)	0.031* (0.017)	0.032* (0.018)
Pos Dev Avg	−0.005*** (0.001)	−0.018 (0.028)	−0.015 (0.028)	0.015*** (0.001)	0.001 (0.016)	0.000 (0.017)
Neg Dev Avg	0.021*** (0.002)	0.081* (0.047)	0.085* (0.047)	−0.029*** (0.002)	0.044 (0.030)	0.043 (0.030)
Avg Contr Others	−0.006*** (0.001)	−0.055* (0.029)	−0.056* (0.029)	0.018*** (0.001)	−0.055** (0.022)	−0.056** (0.022)
Police	−0.004 (0.029)	0.771* (0.460)	0.902* (0.466)	0.047 (0.041)	−0.189 (0.376)	−0.208 (0.377)
Police with Experience	0.080* (0.047)	−0.898 (0.759)	−1.282 (0.783)	−0.023 (0.071)	−0.034 (0.666)	0.077 (0.675)
Police*Vote	−	2.372** (0.971)	1.810* (1.009)	−	0.640*** (0.137)	0.679*** (0.140)
Police with experience *Vote	−	−	1.501** (0.697)	−	−	−0.366 (0.305)
Age	0.004** (0.001)	−0.005 (0.024)	−0.008 (0.024)	0.003 (0.002)	−0.006 (0.019)	−0.006 (0.019)
Male	0.006 (0.026)	0.020 (0.438)	0.053 (0.441)	−0.040 (0.039)	0.462 (0.342)	0.471 (0.344)
Trust	0.006 (0.007)	−0.045 (0.105)	−0.039 (0.106)	0.017* (0.009)	0.011 (0.092)	0.011 (0.092)
Political orientation	<−0.001 (0.006)	−0.081 (0.092)	−0.087 (0.092)	<−0.001 (0.008)	−0.078 (0.074)	−0.080 (0.051)
Inverse Mills' ratio	−	−0.261 (0.416)	−0.292 (0.416)	−	−0.577*** (0.207)	−0.575*** (0.207)
Constant	−	4.551*** (1.508)	5.103*** (1.530)	−	4.528*** (0.956)	4.531*** (0.958)
N	5280	916	916	7680	2090	2090
Log-likelihood	−1522.210	−	−	−2491.376	−	−
R2	−	0.100	0.102	−	0.072	0.072

Note: This table reports marginal effects for the Probit models. RE: Random effects. GLS: Generalized Least Squares. Standard errors are in parentheses. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively for two-tailed tests. The 5280 observations correspond to the 180 subjects' decisions regarding each of their four group members observed in periods of game with a sanction institution except the first period due to the introduction of the lagged variable Received points in *t*-1. The instrument variables are the trend variables *period* and *last period*.

of rewards is lower in the vote treatment. Finally, like for sanctions we observe that police subjects are more likely to assign reward points when the institution results from a majority vote (models (5) and (6)).

To sum up, four results are of particular interest. First, the subjects enforce norms more with reward than sanction institutions (Table 3). Second, police subjects enforce norms significantly more than non-police subjects (Tables 3 and 4). Overall, this supports our conjecture C2a. We attribute these police subject results to the fact that police work is one where law enforcement is almost entirely by use of sanctions, and both training and experience with crime and law breaking in their occupation may explain their higher intensity of punishment. Third, police subjects tend to use both norm enforcement institutions more when they result from a majority vote (Table 4). This police-specific “democracy effect” might result from the civic values in the occupation of police officers. Fourth, after controlling for the potential selection bias in the binary decision to use rewards or sanctions, the intensity of both punishment and rewarding is significantly higher in the Taking Game context (Table 4). This supports our conjecture

C2b. Our main findings regarding norm enforcement are summarized in Result 2.

Result 2. a) Police subjects enforce norms significantly more than non-police, especially with sanctions. b) Police enforce norms significantly more when the enforcement institution results from a majority vote. c) Conditional on the decision to enforce norms by assigning points, the intensity of both sanctions and rewards is higher in the Taking Game.

An analysis of efficiency indicates that sanctions increase cooperation the most, rewards increase overall efficiency, and efficiency is systematically higher in the Giving Game than in the Taking Game (see descriptive statistics on first-stage and final payoffs by treatment in Appendix Table A1 and random-effects GLS estimates in Appendix Table A2). However, it should be kept in mind that the limited number of periods does not allow the long-term efficiency effect of sanctions to fully develop (see Gächter et al., 2008). Appendix Table A2 also shows that, due to their higher cooperativeness and norm enforcement, police subjects do not earn more than the other subjects, but groups populated with more police subjects have significantly higher earnings.

Table 5
Preferences for norm enforcement institution.

Game	Vote for institution	Individual votes			Group Majority vote	Satisfaction rate	
		All	Police	Non-police		Police	Non-police
Taking Game	Sanction	N = 100 28 (28%)	N = 50 16 (32%)	N = 50 12 (24%)	N = 20 5 (25%)	62.5%	50%
	Reward	72 (72%)	34 (68%)	38 (76%)	15 (75%)	94.1%	81.6%
Giving Game	Sanction	N = 80 22 (27.5%)	N = 37 13 (35.1%)	N = 43 9 (20.9%)	N = 16 3 (18.7%)	61.5%	22.2%
	Reward	58 (72.5%)	24 (64.9%)	34 (79.1%)	13 (81.3%)	87.5%	94.1%

4.3. Voting on norm enforcement institutions

Table 5 displays the distribution of individual votes and the group majority vote for the reward and sanction institutions for each pool of subjects in the Giving and Taking Games. It also indicates a proxy for satisfaction in each game for each pool of subjects, i.e. the percentage of subjects whose vote corresponds to the majority vote. Table 5 shows that the proportion of subjects preferring the reward over sanction institution is significantly higher in both game frames (binomial tests, $p < 0.01$ for both), and there is no difference between reward institution preference across the two games (χ^2 test, $p = 0.941$). As a result, 81.3% of the 16 groups in the Giving Game and 75% of the 20 groups in the Taking Game implement the reward institution. Although the majority of police subjects also voted for reward, they were more inclined to vote for sanction than non-police (Taking Game: 32% vs. 24%. Giving Game: 35.1% vs. 20.9%). The difference is, however, not significant (χ^2 tests, $p = 0.156$ and $p = 0.373$ for the Giving and Taking Games, respectively).

Table 6 reports the marginal effects of Probit regressions analyzing the individual determinants of a vote for the sanction institution against the reward institution. In model (1), we include controls for the Giving Game, the late occurrence of the Voting treatment in the sequence to

check whether experience with each institution matters in the voting decision, a dummy for police and demographic controls. We also include a dummy variable indicating whether the subject has contributed nothing in the first period of the Baseline treatment, i.e. before receiving any feedback on others' behavior in this experiment. This variable aims at testing whether individuals who immediately free-ride in this experiment are less likely to vote for sanctions than others. Models (2) to (5) report separate estimates for subjects with and without experience with the two enforcement institutions. Models (2) and (3) add the BRSV dummy variable to check whether having experienced the sanction institution instead of the reward institution just before voting favors or not the vote for the sanction institution.

Table 6 indicates that having experienced both institutions before voting increases the preference for sanctions, as shown by the significant positive coefficient of the *Vote Treatment Last* variable in model (1). Sanctions become more appealing after observing the decay of cooperation and the typically higher marginal impact of sanctions relative to rewards. This is consistent with Güreker et al. (2006). In contrast, we find no effect of having experienced either the reward or the sanction institution immediately before the vote. Supporting conjecture C3a, model (1) indicates that police subjects have a 11.7% higher probability to vote in favor of the sanction institution than non-police and

Table 6
Determinants of the vote for the sanction institution.

Dependent variable: Vote for sanction	All sequences	Experience of institution BRSV and BSRV seq.	Experience of institution BRSV and BSRV seq.	No experience of institution BVSR and BVRS seq.	No experience of institution BVSR and BVRS seq.
	(1)	(2)	(3)	(4)	(5)
BRSV	-	-0.128 (0.106)	-0.110 (0.109)	-	-
Giving Game	0.008 (0.075)	0.099 (0.106)	0.116 (0.108)	-0.055 (0.098)	-0.019 (0.086)
Vote Treatment Last	0.107*** (0.039)	-	-	-	-
Null contribution in period 1 of the Baseline	-0.075 (0.092)	-0.152 (0.101)	-0.160 (0.103)	0.075 (0.115)	0.101 (0.111)
Police	0.117* (0.069)	0.199** (0.087)	0.171# (0.105)	-0.085 (0.127)	-0.137 (0.130)
Police with experience	-	-	0.116 (0.147)	-	0.438*** (0.163)
Age	-0.002 (0.004)	-0.008 (0.006)	-0.008 (0.006)	0.009 (0.006)	0.011 (0.006)
Male	-0.016 (0.069)	-0.057 (0.110)	-0.055 (0.109)	0.064 (0.081)	0.044 (0.081)
Trust	-0.005 (0.019)	0.003 (0.029)	0.006 (0.029)	-0.004 (0.018)	-0.007 (0.018)
Political orientation	0.005 (0.015)	-0.015 (0.022)	-0.015 (0.021)	0.033* (0.020)	0.048*** (0.018)
N	180	100	100	80	80
Log-likelihood	-99.418	-60.827	-60.566	-31.780	-29.953
Pseudo-R ²	0.065	0.077	0.081	0.105	0.156

Note: This table reports marginal effects of Random Effects Logit models. Standard errors clustered at the group level are in parentheses. ***, **, * and # indicate significance at the 0.01, 0.05, 0.10, and 0.11 levels respectively, for two-tailed tests. The BRSV dummy variable corresponds to situations in which participants vote just after experiencing the sanction institution. The omitted variable corresponds to the BSRV sequence where participants vote just after experiencing the reward institution.

models (2) and (5) confirm that police subjects exhibit a higher preference for sanctions. Police subjects with a longer experience in the police are much more likely than the other subjects to vote in favor of the sanction institution before experimenting the institutions in the game (model (5)). A possible interpretation is that being exposed to deviant behavior and norm enforcement either during the game or in real life (as it is the case for police officers) reinforces confidence in sanctions. Interestingly, a comparison of estimates (1) and (5) suggests that real experience and experience in the lab are substitutes. Finally, people who free ride at the very beginning of this experiment are no less likely than others to vote for the sanction institution, and a more conservative political opinion increases the probability of voting for sanctions when one has not yet experienced this institution (model (5)). This leads to our final result:

Result 3. *a) More experience with the social dilemma game and norm enforcement increases the preference for sanctions. b) Police subjects are more likely to vote for the sanction institution compared to other subjects.*

5. Discussion

Our study examined norm enforcement by carrots or sticks in two distinct frames of a social dilemma. A field-experiment element was the use of both French police commissioners and standard student subjects. We have three main sets of findings.

First, a negatively-framed “destructive” context (Taking Game) leads to a lower level of cooperation and to the assignment of more sanctions and rewards when subjects decide to use these institutions. One interpretation stems from the fact that negative (positive) norm violation in the Taking Game could generate more negative (positive) emotions, which naturally resonates with the use of sanctions (rewards).

Second, we examined subject-pool effects and showed that individuals in a mission-oriented occupation responsible for norm enforcement, like our new police commissioners, tend to be both more cooperative in social dilemmas and willing to bear more of the costs of the norm enforcement. This is especially true when the enforcement institution uses sanctions. This translates into higher earnings not for police subjects themselves but for groups populated with more police subjects.

Third, regarding the implementation of norm enforcement institutions, we show that a large majority of individuals generally prefer rewards over sanctions. This result extends that of Sutter et al. (2010) to a wider set of conditions. This preference may be due to a willingness to avoid the loss of efficiency associated with sanctions and/or the perspective of mutual benefits through positive reciprocity within groups. Interestingly, police subjects are more likely to enforce norms with either institution when implemented by majority vote. This effect suggests that police subjects are particularly sensitive to the democratic implementation of the enforcement institution. Finally, we find that an endogenous sanction institution leads to higher contributions than an exogenously imposed equivalent institution. We also observe that police subjects prefer sanctions relatively more than non-police, and a longer exposure to free riding in the experimental game leads subjects to favor sanctions over rewards for norm enforcement. Our finding that both types of experience (experimental or in real life through exposure to crime in police work) lead to a stronger preference for sanctions is important because it lends some support to the external validity of evidence generated by laboratory games.

We acknowledge, however, a number of limitations to our study. It would be desirable to recruit more experienced police commissioners. Indeed, this would allow us to better identify whether the police-subject effect is due to intrinsic motivation and self-selection or to the role of experience in law enforcement. It would also allow us to understand whether the higher preference of more experienced police subjects for the sanction institution is due to more direct exposure to

crime in the field or to immersion in police culture. Moreover, the low number of periods in our experiment penalizes sanction efficiency because the benefits of sanctions take time to develop (see Gächter et al., 2008), while the benefits of rewards are more immediate. Finally, it would be also interesting to test how our findings would be affected if groups were rematched after each interaction. Despite these limitations, we believe that it is important to expose norm enforcement mechanisms to a large variety of environmental and institutional conditions so as to evaluate their robustness and derive policy implications on institutional design.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <http://dx.doi.org/10.1016/j.jpubeco.2015.03.012>.

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