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Andreas Nicklisch, Louis Putterman and Christian Thöni

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DFG Research Group 2104 at Helmut Schmidt University Hamburg
http://needs-based-justice.hsu-hh.de

Self-governance in noisy social dilemmas: Experimental evidence on punishment with costly monitoring

Andreas Nicklisch, Louis Putterman, and Christian Thöni*

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Abstract

Recent experimental studies question whether societies can "self-governance" social dilemmas in a decentralized way. One important problem is are dencentralized punishment opportunities under imperfect monitoring of others' behavior. As a consequence "misdirected" punishment increases massively, while cooperation decreases significantly. Our experiment shows that the majority of our experimental participants are willing to pay a modest cost to improve their information. Almost no subjects take up the chance to buy a partial improvement in information at lower price. Rather subjects choose to monitor perfectly or do not improve their monitoring at all. Little punishment takes place with imperfect information. Only those subjects who improve their monitoring punish non-cooperative behavior subsequently, leading to a substantial and significant improvement in terms of efficiency when participants improve in information at their own cost.

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JEL-Classification: C92, D02, H41

1 Introduction

As experimental and behavioral approaches have increased their contributions to economics, there has been both good news and bad news regarding

*Nicklisch: HTW Chur and research group FOR 2104 "Need-based justice and distributive procedures", Switzerland (email: andreas.nicklisch@htwchur.ch); Putterman: Brown University, Providence, Rhode Island (email: louis_putterman@brown.edu); Thöni: University of Lausanne, Switzerland (email: christian.thoeni@unil.ch). Financial support for Nicklisch by the German Research Foundation (Ni 1610/1-1) is gratefully acknowledged.

the ability of human agents to cooperate in the face of social dilemmas. On the one hand, empirical studies have suggested to many that the traditional assumption of universal selfish maximization might yield less accurate predictions than models consistent with conditional cooperation and assuming heterogeneity of type and belief in place of traditional "common knowledge." These views result from the observation of considerable amounts of cooperation in one shot and finitely repeated games, and of relatively sustained cooperation when players are able to sort by type, to communicate, or to sanction each other. On the other hand, important questions have been raised regarding the realism of the environments in which what Ostrom, Walker, and Gardner (1992) called "self-governance" has been demonstrated. One line of questioning to which the present authors have contributed concerns the problem of imperfect and/or costly information. For example, Page, Putterman, and Unel (2005) find that cooperators endogenously sort into groups to sustain cooperation in public goods games, but Kamei and Putterman (2017) find less success in this regard the less perfect is players' information about one another's behaviors. In related examples, Fehr and Gächter (2000) and Gächter, Renner, and Sefton (2008) find that high contributors incur costs to punish free riders and thereby to promote cooperation, when the contributions of each group member is accurately shown to the others at no cost, but Grechenig, Nicklisch, and Thöni (2010) and Ambrus and Greiner (2012) find large increases in "misdirected" punishment which result in less or even negative effects of punishment opportunities on cooperation, when similar interactions occur under imperfect observability.

The present paper addresses the challenge that imperfect and costly information poses for cooperative self-governance by introducing a natural extension to the experimental literature: the incorporation of a costly monitoring option. Put differently, we endogenize the imperfectness of information by letting the agents concerned improve their information's accuracy, should they be willing to incur the requisite cost. The message of Grechenig et al. (2010) and Ambrus and Greiner (2012) might appear to be summarized by the phrase "punishment despite reasonable doubt," which suggests that agents in their social dilemma experiments who were offered the opportunity to engage in peer punishment based on information known to be frequently inaccurate, were not especially reticent about punishing one another although they might be punishing a fellow cooperator when attempting to enforce cooperation. However, a truly cavalier attitude towards punishing in such conditions would imply having little willingness to pay for better information, were it to be available. By offering opportunities to improve information at relatively low as well as higher cost, we investigate how far that characterization should be pushed.[1] We obtain, in the event, the good

[1] Related to our work is the paper by DeAngelo and Gee (2017): they analyze people's willigness to monitor others' contributions at all when monitoring is costly and a precon-

news that many experimental participants are in fact quite willing to pay a modest cost to improve their information, and that little punishment in fact takes place with imperfect information when a monitoring opportunity of modest cost is placed on offer. A particularly impressive aspect of our findings is that although we offer our subjects the chance to buy a partial improvement in information for only half the price of attaining complete accuracy, almost no subjects take up this offer. Subjects who choose to monitor, almost always choose to do so to the highest available degree. And subjects who choose not to buy information, also choose not to punish in the large majority of instances.

Environments in which accuracy of information depends on decisions about monitoring may vary with respect to whether decisions to monitor are observable. In cases in which monitoring itself is public knowledge, if one knew that others are not paying to improve the accuracy with which they observe one's behavior, there are good reasons to reduce one's contributions. First, if one anticipates that others who do not incur the cost to monitor accurately are reluctant to punish, free-riding is associated with a smaller risk of punishment the less monitoring is done. Second, if there is punishment based on noisy monitoring, there is a substantial chance that free-riding will go undetected and unpunished, and a chance that contributors will receive misdirected punishment, so the presence of peer punishment opportunities does less to motivate contributing the less monitoring is done. For this reason, we assume that contributions are positively correlated with others' decision to invest in monitoring, when monitoring is public information.

Finally, if monitoring sends a public indirect warning to those monitored that punishment could be forthcoming should they ignore norms of cooperation, monitoring might make punishment less necessary. Anticipation of that effect might provide additional motivation for incurring the cost to monitor. However, there is also the possibility that if others send out this public signal and incur monitoring costs, one will choose to free-ride on their efforts, turning the monitoring choice into a (third-order) public good. Since there are plausible reasons why publicness of monitoring may spur, but also why it may hinder, both contributions and monitoring investments, the question is best addressed by empirical means. To investigate how (if at all) the observability of the monitoring decision affects both contribution

dition for sanctioning subsequently. That is, the issue of their paper is not whether people want to improve their knowledge on which basis they may or may not execute sanctions, but whether people want to spend money for observing others and may or may not sanction them. Aoyagi, Bhaskar, and Fréchette (2016) have also a somewhat related paper in which they analyze the impact of public versus private knowledge about the noisiness of others' signal on voluntary cooperation in infinitely repeated prisoner's dilemmas. They observe quite different strategies depending on whether information quality is private vs. public but do not study information quality as a costly decision variable.

and monitoring choices, we conduct both "public" and "private" monitoring versions of our high and low monitoring cost treatments in a "two by two" design.

Overall, our results are consistent with monitoring having a "warning" effect: more monitoring is clearly associated with less free riding on contributions, when it is public. However, the anticipation of this effect appears to induce greater monitoring, if at all, only in the low monitoring cost condition. Perhaps a considerable number of subjects in the treatment having both high monitoring cost and publicness of the monitoring decision consider both monitoring and punishment cost before sending a signal, then, deciding that the combined cost is too high, demur from engaging in monitoring altogether. In our paper, we consider the inconsistent impact of monitoring's publicness on the decision to undertake it at some length, but consider our results in that dimension to be more provisional and of less fundamental importance than those about the willingness to incur a cost to monitor at a more general level, as discussed above.

There are already large empirical and theoretical literatures on costly monitoring, but little in the way of laboratory or other controlled experiments on the topic. One prominent area of application has been to the organization of work teams and their management. In a seminal paper, Alchian and Demsetz (1972) (which had over 16,000 citations according to Google scholar, when recently accessed) argued that workers' abilities to reduce effort without correspondingly sharp reductions in compensation, due to the imperfect observability of effort in teams, provides the core explanation of why economies of specialization and team production are not usually realized by worker partnerships. Those economies, the authors argued, are instead harnessed in asymmetric employment relationships where specialist monitors can claim the profit they obtain by carefully observing worker effort, paying proportionate rewards, and earning the profit as an incentive to accurately perform those roles. Note that for Alchian and Demsetz, good monitoring is something workers would view as being in their own interest, not a warning of potential punishment for choosing the wrong effort level. In efficiency wage models like that of Shapiro and Stiglitz (1984) and Bowles and Gintis (1990), in contrast, firms can induce higher or lower effort simply by announcing a higher or lower monitoring level—an anticipatory role of monitoring that, as mentioned above, requires that the occurrence of monitoring be publicly known.

Yet the claim that costly information in the workplace necessitates top down or specialized monitoring has been a controversial one at the empirical level. Labor economists who have investigated the matter, especially Pencavel (2013) and Craig and Pencavel (1995), find that in enterprises of modest size, at least, mutual monitoring is in fact a comparative advantage of profit-sharing enterprises, and their ability to save on the cost of hiring specialist monitors is a recognized cost advantage.[2] Arguably, the knowledge of being observable by fellow workers is one of the factors leading workers to exert more effort in such firms, but we are unaware of any empirical analysis of how (in this context) varying the observability of monitoring affects the extent to which it is undertaken.[3]

Problems of observation and monitoring likewise arise in other contexts. Ostrom's (1990) discussion of attempts to resolve commons problems among fishermen suggests that differences of observability may be key to the success or failure of alternative governance arrangements. The same applies in areas like the policing of restrictions on harvesting trees, where Ostrom found village self-policing could be superior to government supervision in part due to superior observability of one another by community members. Experimental public economics has focused on quality of information, with differences in likelihood of detection being one of the main variables manipulated in studies of tax compliance (Alm, 2012). Manipulation of public perception of the frequency of audits has also been discussed as a policy tool (see, for example, the survey article by Torgler, 2002).

More broadly, "transparency" is frequently mentioned as a key desideratum of effective governance, and it may well depend not only on rules and practices of officials, but also on the inclination of citizens to expend resources on monitoring their behavior. The free press itself, which is touted as a crucial underpinning of democracy, may be capable of fulfilling that function in a self-sustaining manner only to the extent that citizens show sufficient interest as consumers of its investigations and exposés. Governmental monitoring of citizens' (e.g., of their tax compliance) and citizens' monitoring of governmental non-corruption are both examples of costly monitoring in the public sphere.

The rest of our paper proceeds as follows. In Section 2, we describe the design of our experiment, which uses the well-known experimental paradigm of a public goods game (the voluntary contributions mechanism) played a finitely repeated number of times in groups of fixed composition, extends it to the domain of imperfect information, and introduces new decisions on information acquisition by group members. In Section 3, we discuss theo-

[2]An experiment that finds some support for the Alchian and Demsetz argument is van der Heijden, Potters, and Sefton (2009), while one obtaining more mixed results on the question is Grosse, Putterman, and Rockenbach (2011). A theoretical appraisal of Alchian-Demsetz and related literature is provided by Dow (2017). Kremer (1997) argues that worker-owned enterprises may typically fail due to internal political pressures towards wage compression, a phenomenon indirectly supported by the experimental results of Balafoutas, Kocher, Putterman, and Sutter (2013), and by the empirical findings of Burdín (2016).

[3]For public goods games where subjects have the choice between different punishment institutions the results of Nicklisch, Grechenig, and Thöni (2016) suggest that under limited observability subjects tend to favor centralized structures of enforcement, while with perfect information decentralized punishment prevails.

retical considerations and offer predictions concerning subjects' behaviors. Section 4 presents and analyzes the results of the experiment. Section 5 presents concluding discussion.

2 Design and procedures

Our experimental tool is the standard voluntary contribution mechanism (VCM) with decentralized punishment. We analyze behavior in a standard repeated VCM game with four players and twenty periods. The group composition remains constant over the periods (partner design). At the beginning of each period, each player receives an endowment of 22 ECU (experimental currency units). In a first stage, each player chooses whether or not to buy information. We denote the amount of information purchased as m_i where $m_i \in \{0, 1, 2\}$. Buying information is costly, the marginal cost per unit of information is k ECU.

Players simultaneously choose how many ECU from their endowment to contribute to the public good, g_i, with $g_i \in \{0, 1, 2, \ldots, 20\}$. Notice that we restrict contributions to at most 20 ECU. Each ECU contributed to the public good yields a benefit of 0.4 ECU (the marginal per capita return) to every player in the group.

After the contributions are made, each player receives a signal s_j $(j \neq i)$ about the contributions of each other player in the group, such that

$$s_j = \begin{cases} g_j & \text{with probability } 0.5 + 0.25m_i \\ \tilde{g}_j & \text{with probability } 0.5 - 0.25m_i, \end{cases} \tag{1}$$

where $\tilde{g}_j$ is an independent random draw from $\{0, 1, 2, \ldots, 20\} \setminus \{g_j\}$, all numbers with equal probability. Thus, for the contribution signals of the other three players, there is one independent random draw for each player within each group determining whether players with the same accuracy level of information receive the accurate signal, and if not, another independent draw which determines a random number to display. That is, all players in the group who choose $m_i = 0$ $(m_i = 1)$ see the same accurate or false signal.

Each number (except g_i) is equally likely to appear if the signal does not correspond to the true contribution. For example, player 1 contributes 10 ECU and player 2 chose $m_i = 1$ beforehand. There is a probability of 75 percent that player 2 sees the signal "10 ECU" for player 1's contribution, while with a probability of 25 percent player 2 sees a randomly picked number between 0 and 20, except 10 (for instance "3 ECU"). The labels "player 1", "player 2", etc. are randomly assigned anew to players at the beginning of each period, making the identification of other players across periods impossible.

Then players enter a third stage. Here they can punish the other players. Each punishment point assigned to another player leads to a deduction of three ECUs from the punished player's account, but also reduces the punisher's income by one ECU. Each player can spend up to 10 ECU to punish each other player in the group. Amounts spent on punishment are deducted from the particular player's account. Thus, player i's payoff in a given period is

$$\pi_i = \max(22 - g_i + 0.4 \sum_j g_j - 3 \sum_{j \neq i} p_{j \to i} - \kappa m_i, 0) - \sum_{j \neq i} p_{i \to j}, \qquad (2)$$

with $0 \leq g_i \leq 20$ and $0 \leq m_i \leq 2$. After each period, players learn their own payoff and the points they received (but get no detailed information on who distributed points). Players then proceed to the next period; payoffs accrue over periods. All parameters, the signal technology, and payoff functions are common knowledge.

We apply two treatment dimensions: Along the cost dimension, we vary whether κ is modest (*Low*) or substantial (*High*). For *Low* we choose $\kappa = 0.2$ making the marginal cost for a step of information accuracy one fifth of the marginal cost for punishment, while for *High* we choose $\kappa = 1$ making the marginal cost for a step of information accuracy equal to the marginal cost for punishment.

Along a second treatment dimension, we vary whether individual decisions on m_i are either common knowledge prior to the contribution decision (*Public*) or not (*Private*). In other words, the monitoring decision in *Public* can be seen as a strategic signal indicating to all potential free-riders that one is ready to sacrifice own payoffs in order to punish non-cooperative peers in the group.

We ran a total of twelve sessions with 60 groups (240 subjects) on the 2×2 factorial design. For each of the four treatment combinations (*PublicLow*, *PublicHigh*, *PrivateLow*, and *PrivateHigh*) we have 15 independent observations (i.e., groups). Each subject participated in only one treatment condition. The experiments were conducted at the Wiso-Lab of the University of Hamburg with mostly undergraduate students from various fields. Once all subjects were seated, the written instructions were handed to them before the experimenter read them out loud (see appendix A.2). Subjects were given the opportunity to ask questions (in private). Before the experiment started subjects had to solve a set of control questions. A session lasted for about 90 minutes. Payoffs were converted at an exchange rate of 3 Euro-Cent per ECU. Subjects earned on average 20.20 Euro (standard deviation 3.20 Euro), including a show-up fee of five Euro.

3 Hypotheses

Under the assumptions of traditional economic theory, players determined to maximize their own payoff who assume all others to be of this type and to operate with common knowledge will neither contribute to the public good nor punish (Fehr & Gächter, 2000). However, a large experimental literature demonstrates that the predictions based on such assumptions are routinely violated. Equilibria that include contributing and threats of punishment can exist in models of social preferences (e.g., Fehr and Schmidt, 1999). Our focus is, however, not on the theoretical modeling of the preferences that underlie contributions and punishment, but on derive informal behavioral hypotheses. We assume that—in line with past observations—there is a non-negligible subset of players with inclinations to contribute to the public good if others are believed (and subsequently observed) to do likewise (e.g. Falk, Fehr, and Fischbacher, 2005; Fischbacher and Gächter, 2010), and that when contributed amounts vary among group members, there is a subset of players willing to engage in punishment, despite its monetary cost, if that cost is sufficiently small and if the harm to the punished individual is sufficiently large (e.g., Chaudhuri, 2011).

We further assume that the majority of punishers derive substantially more utility from punishing low than from punishing high contributors. We speculate that for such individuals, the direct utility from punishing a low contributor is more than sufficient to compensate for the monetary cost, whereas the utility from punishing a high contributor is not, and—following the idea of Fehr and Schmidt, 1999— might even be negative. These assumptions imply that there will be demand for accurate information about the contributions, if the cost is sufficiently low.

As for the difference between *Private* and *Public* we hypothesize that more information is demanded in the latter. When paying for information is public knowledge, it may serve as a signal to other group members that one is willing to punish. Potential free riders might respond with higher contributions, allowing to subsequently economize on expenditures for punishment. This leads to

H_{1a} : Demand for information is price sensitive, that is, $m^{Low} > m^{High}$.

H_{1b} : Demand for information is higher when information acquisition is public knowledge, that is, $m^{Private} < m^{Public}$.

Among players with some willingness to pay for punishment, we expect heterogeneity in willingness to pay for information. Such heterogeneity can result both from differences in maximum willingness to pay to punish a free rider and from heterogeneity of utility difference from punishing a co-operator versus a free rider.[4] The lower the cost of information, the more players purchase it, thereby increasing success at punishing low but not high contributors and improving the efficacy of the punishment mechanism.

[4]Idiosyncratic factors, such as differences in pure demand for social information or different degrees of optimism about others' cooperativeness, implying different expectations regarding the utility of the contribution information, can also be at work.

Again, with regards to the publicness of information acquisition, there is the possibility that many take a conditionally cooperative view of both monitoring and punishing—that is, they are willing to take on these costly jobs provided that others do so, but not otherwise. If monitoring is more than a little costly, subjects might be reluctant to engage in it, and seeing that others are also doing little monitoring may reinforce the tendency to avoid doing it. Given these conflicting considerations, we cannot make strong a priori predictions about how publicness of monitoring will affect its frequency, but can only predict that when monitoring is public, more monitoring is likely to be associated with higher contributions to the public good. Therefore, we hypothezise

H_{2a} : Lower cost of information and public knowledge about information acquisition lead to better informed punishers which in turn improve the efficacy of punishment in enforcing high contributions, i.e., $g^{Low} > g^{High}$.

H_{2b} : In *Public*, information acquisition serves as a threat. Contributions increase in the amount of information acquired by the other subjects: $g_i = f(m_{-i})$ with $f' > 0$.

Deriving hypotheses about how the use of punishment correlates with monitoring is not straightforward. Our assumption that people pay for information because they have a desire to punish the free-riders and avoid mistakenly punishing others suggests that the treatments *Low* and *Public* result in more punishment. On the other hand, if these conditions produce more effective punishment which appreciably deters free-riding, then we should see the opposite, namely very little actual punishment.

Thus, instead of formulating hypotheses regarding the use of punishment at the treatment level, we state a hypothesis about the relation between information acquisition and the use of punishment at the individual level, which we expect to hold in all treatment conditions.

H_3 : There is a positive correlation between information acquisition and punishment of free-riders.

Finally, we formulate a hypothesis about how the efficiency of group outcomes depends on the treatment conditions. From our hypotheses stated so far it is clear that we expect earnings to be higher in *Low* than in *High*, as well as higher in *Public* than in *Private*. We also expect the effect of the two variations to be additive, such that the most efficient outcomes should be observed in *PublicHigh*, while the least efficient ones should be seen in *PrivateLow*. For the ranking of the two intermediate treatments we do not have a clear prediction.

H_4 : Average earnings for the treatments are:

$$\pi^{PublicLow} > \pi^{PrivateLow} \sim \pi^{PublicHigh} > \pi^{PrivateHigh}$$

4 Results

We provide a brief overview of the results of our four experimental treatments before beginning a more detailed analysis in which our initial focus is on our main interest, subjects' willingness to pay to improve their information. Figure 1 shows the averages of the three main dependent variables across treatment. Spikes indicate clustered standard errors. In addition, Table 1 shows the significance levels of the differences for all bilateral treatment comparisons based on Wilcoxon rank-sum tests.

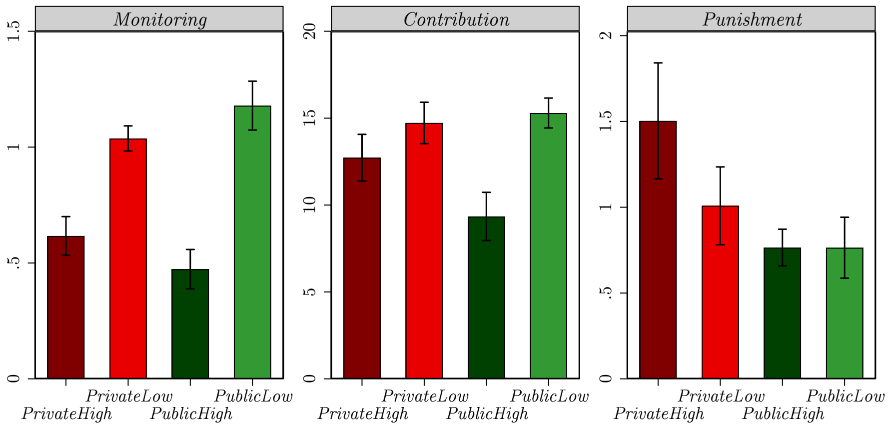

Figure 1: Averages of the main dependent variables over the 20 periods and by treatment. Spikes indicate standard errors, clustered on group.

We note, first, that the average contribution is well above 50% of endowment in all treatments except *PublicHigh*. The stylized fact from past VCM experiments is that contributions begin in the neighborhood of 50% of the endowment, and decline with repetition. That contributions average near 75% of endowment in the two treatments with low cost of information, and 74% of endowment in *PrivateHigh*, thus suggests that the threat and use of punishment increased and sustained contributions despite the quite imperfect information starting point and the costliness of information acquisition. This is likely to reflect amelioration of the information problem by decisions to acquire costly information, to which we turn momentarily.

Second, the cost of information appears to influence information acquisition and contributions in the manner that we predicted: the two treatments with low cost of information acquisition (*PrivateLow* and *PublicLow*) display more information acquisition and higher contributions than their high cost of information counterparts (Wilcoxon rank-sum test with pooled data, *High* vs. *Low*: $p = .000$ for information acquisition and $p = .005$ for contributions). However, whether the decision to acquire information is made public or not does not seem to have a systematic effect. While contribu-

Table 1: Bilateral treatment comparisons.

Variable	Treatment	Average	p-values of bilateral comparison		
			PrivateHigh	*PrivateLow*	*PublicHigh*
Monitoring	*PrivateHigh*	0.62			
	PrivateLow	1.04	.000		
	PublicHigh	0.47	.177	.000	
	PublicLow	1.18	.000	.254	.000
Contribution	*PrivateHigh*	12.72			
	PrivateLow	14.73	.330		
	PublicHigh	9.35	.071	.007	
	PublicLow	15.30	.191	.983	.003
Punishment	*PrivateHigh*	1.50			
	PrivateLow	1.01	.290		
	PublicHigh	0.76	.191	.803	
	PublicLow	0.76	.089	.395	.604

Notes. Average of information acquisition, contributions, and punishment across the 20 periods and p-values of Wilkoxon ranksum tests for bilateral treatment comparisons. All tests based on independent group averages.

tions and information acquisition are slightly higher in *PublicLow* than in *PrivateLow*, the reverse is the case when comparing *PublicHigh* to *Private-High*. Overall, the two treatments in which information acquisition is public are not different from the two in which it is private, with regard to either contributions or information acquisition (*Private* vs. *Public*: $p = .848$ for information acquisition and $p = .255$ for contributions).

Punishment does seem lower in the two public treatments, consistent with the idea that a public monitoring signal plays a warning role; but the story turns out to be different as between the two public treatments, and we reserve it for later discussion. First, we will focus on the information acquisition decisions and show how they function as precursors to those of punishment. We show that the warning effect of monitoring can affect observed punishment by rendering it unnecessary, at least in the *PublicLow* treatment.

4.1 Monitoring

Figure 2 shows the demand for information across the 20 periods. Throughout the game the difference between high and low cost of information remains highly significant. On the other hand, the graphs for the *Private* and *Public* member of each treatment pair are never far apart and often overlap. In the last third of the game it seems like public results in more monitoring, but the differences do not significance. There is a negative trend in monitoring, especially in the two treatment with high cost of information. This might

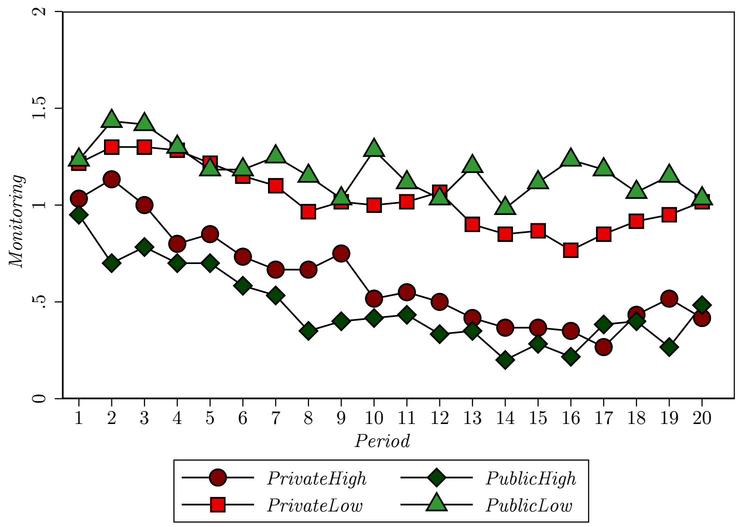

Figure 2: Monitoring over the course of the 20 periods.

indicate that—similarly to what is observed for punishment—monitoring is initially necessary to establish the credibility of punishment, but no longer later in the game. The negative trend is, however, weaker for monitoring than for punishment, especially for low cost of information (we will provide some statistics on this later). This indicates that continued monitoring might be necessary to ensure self governance.

While average monitoring is in the neighborhood of one unit, very few subjects demand one unit of information. In the overwhelming majority of the cases (95 percent) subjects acquire either no information or perfect information. Furthermore, over time information acquisition in *Public* might be conditional on information acquisition of the others in the group. If that is the case we should see groups converging to low levels and groups converging to high levels of information. Figure 3 shows histograms for the sum of information acquisition within a given group and period for the four treatments. Observations with even numbers are generally more frequent than odd numbers, again reflecting the fact that the majority of subjects spends either zero or two units on information acquisition. The range spans from 0 (no one acquires information) to 8 (all four subjects fully informed). Comparing the *Private* to *Public* treatments suggests that extreme outcomes are somewhat more frequent in the latter. This holds in particular for fully uninformed groups in *High* and fully informed groups in *Low*.

Table 2 shows the results of OLS estimates explaining the demand for information at the level of individual and period. We use robust standard errors with clustering on group. Model (1) explains information acquisition by period, a dummy for the final period, and two dummies for the two treat-

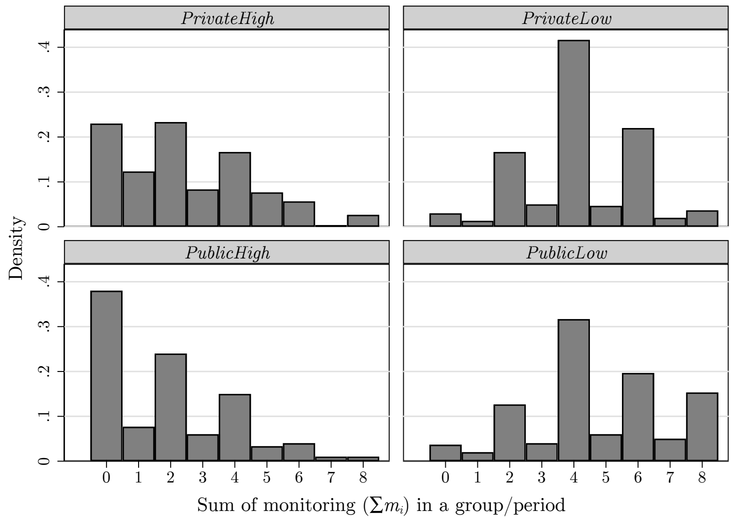

Figure 3: Histogram of the sum of information acquisition in a given group and period.

ment variations. We confirm the overall negative time trend, but observe a significant increase in the final period, presumably indicating that subjects anticipate the danger of end-game effects in contributing. The cost level is highly significant, while *Private* does not seem to matter. In Model (2) we add an interaction of the two treatment variables. The interaction is weakly significantly positive, indicating that high cost discourages information acquisition less when *Private*. In Model (3) we investigate whether contributions from the previous period affect the decision to acquire information. Here we test whether information is a reaction to free riding in the previous period. We add the subject's own contribution (g_i^{t-1}) and the average contribution of the other three group members ($\bar{g}_{-i}^{t-1}$) in the previous period as explanatory variables. Thus averaged, the contributions of the others do not seem to affect the information decision, while there is a strong correlation between a subject's contribution in the previous round and her demand for information, perhaps because those who contribute more in general are also stronger demanders of information. When we replace the others' average contribution by the standard deviation of the four contributions in the previous period, in Model (4), the effect is significant, i.e., subjects acquire more information if the contributions were more heterogenous in the previous period.

Result 1 *Demand for information is price sensitive. For both cost levels, subjects acquire either no information or perfect information. Subjects who contributed in previous periods are more likely to acquire information, and more information is demanded when past contributions were heterogenous.*

Table 2: OLS estimates for monitoring

	Dependent variable: *Monitoring*			
	(1)	(2)	(3)	(4)
Period	-0.028^{***}	-0.028^{***}	-0.028^{***}	-0.025^{***}
	(0.004)	(0.004)	(0.004)	(0.004)
Final period	0.186^{***}	0.186^{***}	0.193^{***}	0.186^{***}
	(0.054)	(0.054)	(0.051)	(0.050)
High	-0.563^{***}	-0.706^{***}	-0.660^{***}	-0.634^{***}
	(0.084)	(0.132)	(0.143)	(0.142)
Private	0.001	−0.142	−0.142	−0.136
	(0.084)	(0.116)	(0.117)	(0.121)
High × *Private*		0.285^{*}	0.249	0.213
		(0.164)	(0.165)	(0.168)
g_i^{t-1}			0.018^{***}	0.018^{***}
			(0.005)	(0.005)
$\bar{g}_{-i}^{t-1}$			−0.007	
			(0.007)	
$\text{sd}(g^{t-1})$				0.022^{**}
				(0.011)
Constant	1.393^{***}	1.464^{***}	1.301^{***}	1.097^{***}
	(0.094)	(0.110)	(0.173)	(0.177)
F-test	28.4	23.1	25.4	24.7
Prob > F	0.000	0.000	0.000	0.000
R^2	0.112	0.117	0.130	0.133
N	4800	4800	4560	4560

Notes: OLS estimates. *High* and *Private* indicate treatment variations; g_i^{t-1} ($\bar{g}_{-i}^{t-1}$) indicates a subject's (the others' average) contribution in the previous period; $\text{sd}(g^{t-1})$ denotes the standard deviation of the contributions in the previous period. Robust standard errors, clustered on group, in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$.

4.2 Contributions

In all four treatments contributions are, after some initial increase, fairly stable over time. *PublicHigh* is consistently lower than *PrivateHigh*, while the two treatments with low monitoring cost show very similar patterns (see Figure A1 in the appendix).

Table 3 shows estimates for the contribution decision, also at the individual and period level. Model (1) shows that high information cost significantly reduces contributions, while time and privateness of information acquisition have no significant effect. The overall time trend is insignificant, but there is a significant end-game effect in contributions. In Model (2) we add lagged explanatory variables from the previous round. We control for a subject's contribution (g_i^{t-1}), the other group members' average contribution ($\bar{g}_{-i}^{t-1}$), and the number of punishment points received from other subjects ($p_{\to i}^{t-1}$).[5] In addition, we interact the variable for received punishment with a

[5]Model (2) shows a significant negative coefficient for period. This should not be

Table 3: OLS estimates for contribution

	Dependent variable: *Contribution*			
	(1) All obs.	(2) All obs.	(3) *Public*	(4) *Public*
Period	0.059	-0.031^{***}	0.096^{*}	-0.004
	(0.037)	(0.009)	(0.050)	(0.013)
Final period	-1.675^{***}	-0.717^{*}	-1.379^{***}	-0.261
	(0.415)	(0.376)	(0.442)	(0.439)
High	-3.977^{***}	-0.476^{***}	-4.289^{**}	-0.103
	(1.210)	(0.172)	(1.561)	(0.366)
Private	1.405	0.031		
	(1.210)	(0.148)		
g_i^{t-1}		0.568^{***}		0.521^{***}
		(0.031)		(0.040)
$\bar{g}_{-i}^{t-1}$		0.361^{***}		0.349^{***}
		(0.032)		(0.050)
$p_{\to i}^{t-1}$		0.236^{***}		0.144
		(0.083)		(0.126)
$p_{\to i}^{t-1} \times (g_i^{t-1} \geq \bar{g}^{t-1})$		-0.220^{*}		-0.138
		(0.118)		(0.174)
$\bar{m}_{-i}$			2.353^{***}	1.258^{***}
			(0.826)	(0.294)
Constant	13.774^{***}	1.434^{***}	11.587^{***}	0.696
	(0.869)	(0.273)	(1.289)	(0.491)
F-test	7.2	782.5	9.6	587.0
Prob $> F$	0.000	0.000	0.000	0.000
R^2	0.096	0.636	0.218	0.648
N	4800	4560	2400	2280

Notes: OLS estimates. *High* and *Private* indicate treatment variations; g_i^{t-1} ($\bar{g}_{-i}^{t-1}$) indicates a subject's (the others' average) contribution in the previous period; $p_{\to i}^{t-1}$ indicates punishment i received by others in the previous period (with $p_{\to i} = \sum_{j \neq i} p_{j \to i}$); $\bar{m}_{-i}$ denotes the average monitoring of the other subjects. Robust standard errors, clustered on group, in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$.

dummy for whether the subject's contribution was higher than the average contribution in the group. Lagged contributions as well as received punishment significantly increase contributions, unless punishment is received in combination with above average contributions, in which case the joint coefficient is insignificant (Wald test: $p = .806$). In Models (3) and (4) we look at the results of the *Public* treatments only to test whether known information acquisition by others affected a subject's contributions. We add the average amount of information acquisition by the other subjects in the group ($\bar{m}_{-i}$). The coefficient is highly significant, indicating that information acquisition works as a signal that improves others' cooperation, when public. Indeed, when we add back, in Model (4), the controls for own and others' last period

interpreted as pure time trend, as we control for lagged contributions. In particular, the coefficient loses significance if we remove $\bar{g}_{-i}^{t-1}$.

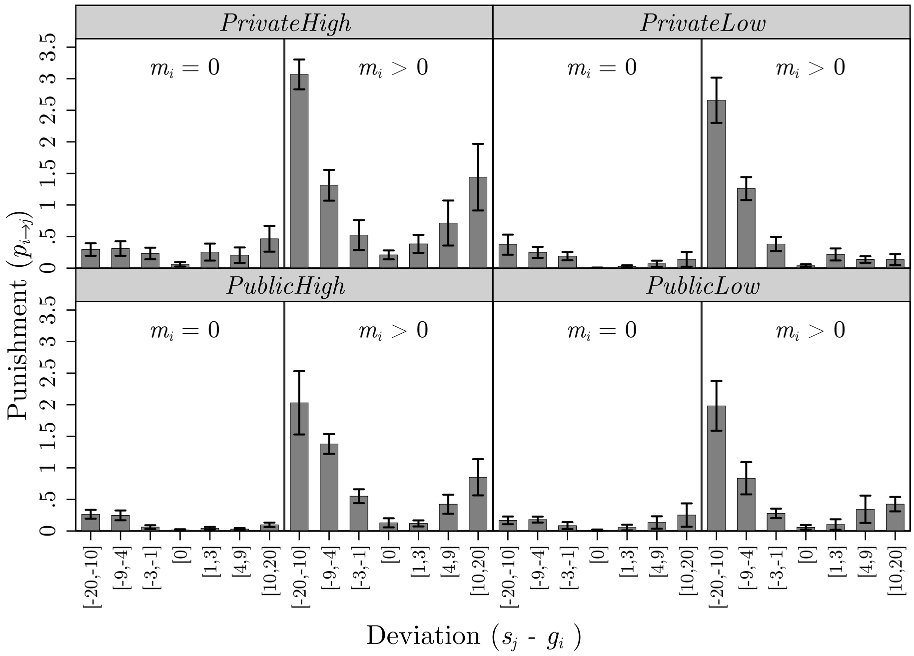

Figure 4: Assigned punishment in relation to the difference between the signal and the punisher's contribution ($s_j - g_i$). Averages over all cases (including zero punishment). Separate panels show the punishments in case the punisher acquired no information ($m_i = 0$), or one or two units ($m_i > 0$).

contributions and for punishment received, the estimated coefficients on the contribution variables remain about as large and significant as in Model (2), but the punishment measures lose significance, suggesting that the warning effect of monitoring delivers in advance much of the news that then comes with punishment.[6]

Result 2 *Low costs for information acquisition spur contributions. In treatments where monitoring is public, others' information acquisition spurs contributions.*

4.3 Punishment

As usual in public goods experiments with punishment we observe the use of punishment to decline over time. This is true for all treatments and the ordering of the treatments shown in Figure 1 remains relatively stable throughout the 20 periods (see Figure A2 in the appendix). This is partly

[6]Estimating Model (4) without the coefficient for monitoring results in very similar parameter estimates for received punishment as Model (2), albeit at lower levels of significance.

Table 4: OLS estimates for assigned punishment ($p_{i \to j}$)

	Dependent variable: Punishment			
	(1) All obs.	(2) All obs.	(3) *High*	(4) *Low*
Period	-0.011^{***}	-0.004	-0.009^{*}	0.000
	(0.003)	(0.003)	(0.004)	(0.003)
Final period	0.035	-0.021	-0.032	-0.003
	(0.038)	(0.033)	(0.051)	(0.050)
High	0.193^{***}	0.127^{*}		
	(0.068)	(0.065)		
Private	0.130^{*}	0.113^{*}	0.170^{*}	0.044
	(0.066)	(0.061)	(0.095)	(0.075)
d_{ij}^{-}	-0.073^{***}	-0.015^{***}	-0.013^{**}	-0.014^{**}
	(0.007)	(0.004)	(0.005)	(0.006)
d_{ij}^{+}	0.023^{***}	0.008^{**}	0.010^{*}	0.008
	(0.005)	(0.004)	(0.005)	(0.007)
m_i	0.266^{***}	0.006	-0.009	-0.009
	(0.028)	(0.019)	(0.042)	(0.010)
$m_i \times d_{ij}^{-}$		-0.089^{***}	-0.100^{***}	-0.083^{***}
		(0.007)	(0.010)	(0.009)
$m_i \times d_{ij}^{+}$		0.014^{***}	0.030^{***}	0.005
		(0.005)	(0.010)	(0.005)
Constant	-0.190^{***}	0.019	0.138^{**}	0.048
	(0.067)	(0.059)	(0.058)	(0.067)
F-test	25.1	39.6	37.6	17.3
Prob > F	0.000	0.000	0.000	0.000
R^2	0.143	0.263	0.258	0.283
N	14400	14400	7200	7200

Notes: OLS estimates. *High*, *Low*, and *Private* indicate treatment variations; d_{ij} indicates the deviation between a signal and a punisher's contribution. It is calculated as $d_{ij} = s_j - g_i$, and $d_{ij}^{+} = \max\{d_{ij}, 0\}$, $d_{ij}^{-} = \min\{d_{ij}, 0\}$; m_i indicates monitoring. Robust standard errors, clustered on group, in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$.

due to some groups reaching full contribution after some initial rounds of punishment. It is also noteworthy that the expenses for punishment drop more sharply than the expenses for monitoring. If we make a simple comparison of the first and second half of the game, then punishment drops from 1.35 to 0.67 (reduction by 50%), while the reduction in monitoring is 0.96 to 0.70 (reduction by 27%).

There is a strong correlation between monitoring and the use of punishment. Subjects who acquired information punish more often than uninformed subjects. Among the subjects who did not acquire information in a given period, 14 percent punish another subject in that period. The corresponding percentage is 44.7 percent among the subjects who acquired information. Figure 4 shows how the strength of punishment varies with the

Table 5: OLS estimates for received punishment ($p_{\rightarrow i}$)

	Dependent variable: Received punishment			
	(1) All obs.	(2) All obs.	(3) *Public*	(4) *Public*
Period	-0.070^{***}	-0.050^{***}	-0.059^{***}	-0.061^{***}
	(0.011)	(0.011)	(0.013)	(0.013)
Final period	0.429^{***}	0.223	0.266^{*}	0.270^{*}
	(0.130)	(0.133)	(0.147)	(0.146)
High	0.001	0.098	0.050	0.055
	(0.202)	(0.255)	(0.227)	(0.230)
Private	0.244	0.299		
	(0.281)	(0.268)		
High × *Private*	0.494	0.541		
	(0.445)	(0.431)		
$\bar{g}_{-i}$		0.107^{***}	0.104^{***}	0.105^{***}
		(0.017)	(0.023)	(0.023)
g_i		-0.163^{***}	-0.133^{***}	-0.143^{***}
		(0.018)	(0.022)	(0.024)
$\bar{m}_{-i}$		0.607^{***}	0.453^{***}	0.466^{***}
		(0.112)	(0.152)	(0.147)
m_i			-0.143^{***}	-0.369^{**}
			(0.051)	(0.178)
$m_i \times g_i$				0.016
				(0.011)
Constant	1.481^{***}	1.414^{***}	1.443^{***}	1.543^{***}
	(0.239)	(0.387)	(0.356)	(0.372)
F-test	10.0	21.6	9.3	7.8
Prob > F	0.000	0.000	0.000	0.000
R^2	0.054	0.235	0.205	0.208
N	4800	4800	2400	2400

Notes: OLS estimates. Dependent variable is the sum of received punishment points, i.e., $p_{\rightarrow i} = \sum_{j \neq i} p_{j \rightarrow i}$; *High* and *Private* indicate treatment variations; $\bar{m}_{-i}$ denotes the average monitoring of the other subjects; g_i ($\bar{g}_{-i}$) indicates a subject's (the others' average) contribution; m_i denotes the subject's monitoring. Robust standard errors, clustered on group, in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$.

difference between the signal and the punisher's contribution, by treatment ($d_{ij} = s_j - g_i$). We distinguish between situations in which the punisher acquired information ($m_i > 0$), and ones in which she did not acquire information ($m_i = 0$). (We pool all non-zero investments in information, because there are only a few observations for $m = 1$.) Punishment is much stronger among informed subjects, and in both panels we observe non-negligible amounts of antisocial punishment.[7] Interestingly, it seems that the treatments with higher monitoring cost seem to provoke stronger punishment of high contributors.

Table 4 shows OLS estimates for the punishment decision. We control for the two treatment dimensions and for the deviation between the signal and a punisher's contribution. We estimate separate slopes for positive (d_{ij}^{+}) and negative deviations (d_{ij}^{-}). In addition, we control for monitoring and time. Punishment drops over time, but there is no additional end-game effect in the final period. We observe significantly higher punishments when information costs are high. The slopes are highly significant both for signals below the punisher's contribution and for ones above the punisher's contribution. In Model (2) we interact the deviation with the information acquisition and find, in accordance to Figure 4, the reaction to the signal is a lot stronger among subjects who acquired information. In Model (3) and (4) we re-estimate the model for the treatments *High* and *Low* separately. We find that for antisocial punishment (the coefficient for δ^{+}) exhibits a significant effect only when information costs are high. Thus, it could be that high costs unlike low costs either create some sort of commitment to punish, or screen out those less determined to punish strongly.

In Table 5 we show the results of OLS estimates for punishment received, at receiver and period level. Each observation combines the punishment a recipient j receives from up to three group members (i, k, l), yielding one third the observation numbers of Table 4. Model (1) uses the dimensions of treatment variation and period as explanatory variables.

Apart from a negative trend over time and the final period term, none of the variables are significant.[8]

In Model (2) we add measures for contribution and the average infor-

[7] Here, we define antisocial punishment as punishment targeted at subjects with a weakly higher contribution than the punisher (Herrmann, Thöni, & Gächter, 2008). However, we substitute signal of potential target s_j for j's actual contribution g_j, and in case of imperfect information ($m_i < 2$) we cannot be certain that the punishment of a high signal member is due to antisocial motives, since we cannot rule out belief that the signal is not accurate.

[8] The control for final period checks for an end-game decline, which might reveal that punishment is mainly strategically motivated (Falk et al., 2005). The significant increase in punishment in the final period is likely to be caused by subjects free riding towards the end of the game. Model (2) shows that the coefficient for the final period loses significance once we control for contributions. However, the results still favor the view that punishment is mainly non-strategic, as we do not observe a decline in punishment in the final period.

mation acquisition of the other subjects in the group. All coefficients are highly significant and have the expected sign: received punishment is higher the more information the others in the group acquired and the higher their contributions, and received punishment is strongly decreasing in the subject's contribution. In Model (3) we restrict the sample to the *Public* treatments, and we add the subjects' information acquisition to the model to test whether information acquisition might be seen by the group as a punishment threat and motivate retaliatory punishment acts. The results suggest that this is not the case. It is in fact the opposite: the negative coefficient suggests, remarkably, that the group on balance punishes a player for not acquiring information. Finally, in Model (4) we test for interaction effects between the recipient's contribution and the information acquisition, but the coefficient is close to zero and insignificant.

Result 3 *Punishment is predominantly assigned by informed subjects, while the likelihood to receive punishment increases with others' information acquisition. High costs for information increase the likelihood of antisocial punishment.*

4.4 Earnings

Finally we compare the overall efficiency of the treatments in Figure 5. The vertical axis shows the percentage of the potential efficiency gains from cooperation that subjects realize in the four treatments.[9] In accordance to H_4 we find *PublicLow* to be the most efficient treatment condition and *PrivateHigh* the least efficient. The intermediate treatments suggest that the lion's share of the variation in efficiency gains is attributable to the variation in the cost of information, while the publicness of the information acquisition decision seems of minor importance. If we pool the data to compare the *High* to low *Low* treatments we find a highly significant difference (p = .001), while pooling to compare *Public* to *Private* yields an insignificant difference (p = .790).

Is paying for information efficient? Within groups, it is likely that those who acquire a lot of information end up with relatively low monetary payoffs, both because the cost of information and because, as we have shown earlier, punishment is predominantly meted out by informed subjects. On the group level, it might be that groups having more well informed members achieve more efficient outcomes than those with less well informed ones. In a simple OLS regression, we relate efficiency to the average level of information

[9]That is, what proportion of the potential gains from moving from the predicted selfish Nash equilibrium under standard assumptions, where average earnings are 22 (and information acquisition, contribution and punishment are all zero) to the social optimum, where average earnings are 34 (with full contribution, zero information acquisition and no punishment). Note that values below zero are possible.

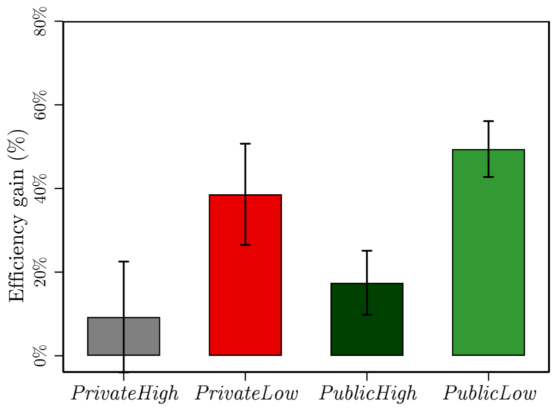

Figure 5: Average efficiency gain in the four treatments, all periods. Efficiency gain is calculated as the percentage of distance between Nash equilibrium earnings under standard assumptions (22) and maximum possible average earnings (34).

acquisition in a group. Over all treatments we find a weakly significant positive effect ($\beta = 0.223$, $p = .079$) which indicates that a fully informed group ($\bar{m} = 2$) on average realizes roughly 45 percentage points higher efficiency than an fully uninformed group ($\bar{m} = 0$). Estimating such regressions for each treatment individually, we find that none of the coefficients reaches significance. Moreover, the coefficients are positive only in the two treatments with public information, whereas for the two treatments with private information they are negative. If we pool the two *Public* treatments, we obtain a coefficient ($\beta = 0.287$, $p = .010$), whereas pooling the *Private* treatments yields an insignificant coefficient estimate ($\beta = 0.082$, $p = .771$).

Result 4 *The cost of information significantly affects the efficacy of the outcome, while the public vs. private variation does not. Acquiring information is efficient from the group's perspective in the* Public *treatments, while we do not find systematic differences between well informed groups and less well informed groups in the* Private *ones.*

5 Conclusion

Recent experimental studies have demonstrated a number of important challenges for the "self-governance" ability of societies. One such challenge is that there may well be imperfect information regarding others' behaviors, and subjects' willingness to impose sanctions despite grounds for doubt cast may not auger well for societal self-organization (Grechenig et al., 2010 and

Ambrus and Greiner, 2012). In the absence of affordable remedies, noise sharply increases "misdirected" punishment and eliminates or even reverses the effects on cooperation of peer punishment opportunities that, with perfect information, often successfully promote it.

The good news of our study is that the majority of our experimental participants are willing to pay a modest cost to improve their information, while little punishment takes place with imperfect information when perfect information is affordably available. Moreover, although we offer the chance to buy a partial improvement in information at lower price, almost no subjects take up this offer. Rather subjects choose to monitor fully or do not improve their monitoring at all. Those who choose not to buy information, also choose not to punish in the large majority of instances. In other words, not to buy information serves as some kind of commitment device not to execute punishment. In turn, we observe significantly more antisocial punishment when information costs are high: expensive knowledge acquisition may serve as a commitment device, also for antisocial punishment. While the public improvement of monitoring offers the opportunity to signal strategically one's willingness to punish non-cooperators, participants do not use this opportunity significantly more often than when improvements are private.

Overall, there is a substantial and significant improvement in terms of efficiency when participants improve information at their own cost. Our subjects do not punish despite reasonable doubts, but try to resolve those doubts. However, the demand for information is price-sensitive. This implies, it is in the best interest of a group's members to make relevant information about the behaviors of each be available to the others at moderate costs. However, the costs of information acquisition or verification are to some degree exogenously given, and there are in some cases economies of scale in observation or advantages of having an entity with the power to insist on information disclosure. The relative ease of decentralized versus centralized monitoring may therefore be a major determinant of which social dilemmas are resolved via decentralized and which via centralized mechanisms.

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A Online appendix

A.1 Additional analysis

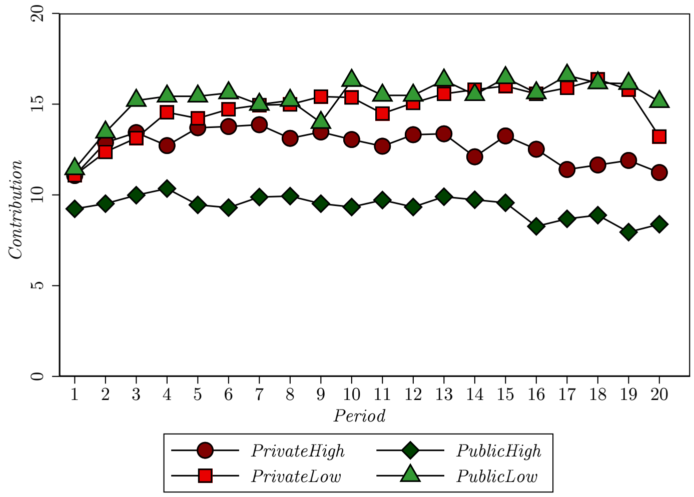


Figure A1: Monitoring over the course of the 20 periods.

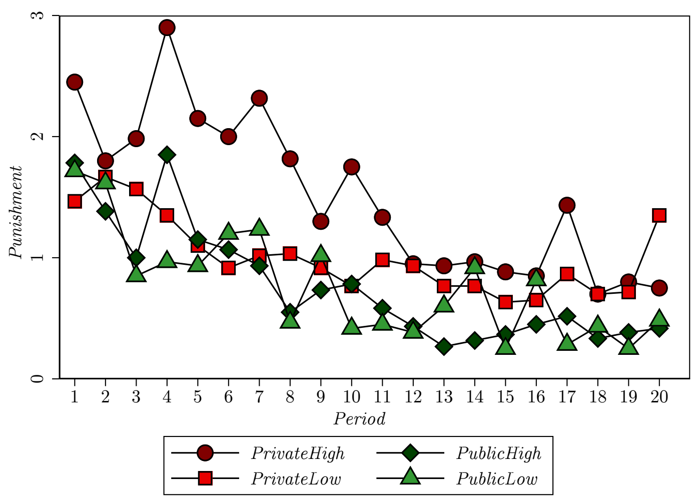


Figure A2: Punishment over the course of the 20 periods.

Table A1: OLS estimates for assigned punishment ($p_{i \to j}$)

	Dependent variable: Punishment			
	(1) All obs.	(2) All obs.	(3) *High*	(4) *Low*
Period	-0.011^{***}	-0.011^{***}	-0.004	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Final period	0.035	0.039	-0.021	-0.017
	(0.038)	(0.039)	(0.033)	(0.036)
High	0.193^{***}	0.205^{***}	0.127^{*}	0.053
	(0.068)	(0.062)	(0.065)	(0.053)
Private	0.130^{*}	0.133^{*}	0.113^{*}	0.107^{*}
	(0.066)	(0.067)	(0.061)	(0.061)
d_{ij}^{-}	-0.073^{***}	-0.078^{***}	-0.015^{***}	-0.015^{**}
	(0.007)	(0.011)	(0.004)	(0.006)
d_{ij}^{+}	0.023^{***}	0.016^{***}	0.008^{**}	0.007
	(0.005)	(0.005)	(0.004)	(0.007)
m_i	0.266^{***}	0.269^{***}	0.006	-0.006
	(0.028)	(0.029)	(0.019)	(0.018)
$d_{ij}^{-} \times$ *High*		0.011		0.001
		(0.015)		(0.007)
$d_{ij}^{+} \times$ *High*		0.010		0.003
		(0.010)		(0.009)
$m_i \times d_{ij}^{-}$			-0.089^{***}	-0.082^{***}
			(0.007)	(0.009)
$m_i \times d_{ij}^{+}$			0.014^{***}	0.005
			(0.005)	(0.005)
$m_i \times d_{ij}^{-} \times$ *High*				-0.019
				(0.012)
$m_i \times d_{ij}^{+} \times$ *High*				0.026^{**}
				(0.012)
Constant	-0.190^{***}	-0.195^{***}	0.019	0.064
	(0.067)	(0.068)	(0.059)	(0.055)
F-test	25.1	20.3	39.6	37.6
Prob $> F$	0.000	0.000	0.000	0.000
R^2	0.143	0.144	0.263	0.268
N	14400	14400	14400	14400

Notes: OLS estimates. *High*, *Low*, and *Private* indicate treatment variations; d_{ij} indicates the deviation between a signal and a punisher's contribution. It is calculated as $d_{ij} = s_j - g_i$, and $d_{ij}^{+} = \max\{d_{ij}, 0\}$, $d_{ij}^{-} = \min\{d_{ij}, 0\}$; m_i indicates monitoring. Robust standard errors, clustered on group, in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$.

A.2 Instructions

Instructions[1]

General explanations for participants

You are taking part in an economic science experiment. If you read the following explanations carefully, you can earn a rather significant sum of money, depending on the decisions you make. It is therefore very important that you pay attention to the following points.

The instructions you have received from us are intended solely for your information. *During the experiment, you will not be allowed to communicate with anyone*. Should you have any questions, please direct them directly to us. Not abiding by this rule will lead to exclusion from the experiment and from any payments.

In this experiment, we calculate in Taler, rather than in Euro. Your entire income will therefore initially be calculated in Taler. The total sum of Taler will later be calculated in Euro as follows:

10 Taler = 25 Euro cents

The euro you will have accrued plus 5.00 Euro for your participation will be paid to you privately *in cash* at the end of the experiment.

The experiment is divided into separate periods. It consists of a total of 20 periods. Participants are randomly assigned to groups of four. You will thus be in a group that has three other members, apart from you. During these 20 periods, the composition of your group will remain unaltered. *That is, you will be in the same group for 20 periods.* Please note that the identification number assigned to you and the other members of the group changes randomly in each period. Therefore, given group members cannot be identified from one period to the next.

The following pages outline the exact procedure of the experiment.

[1]English translation of the German original for High. Numbers change in Low accordingly. Treatment differences between Private and Public are indicated by {...} and […].

Information on the exact procedure of the experiment

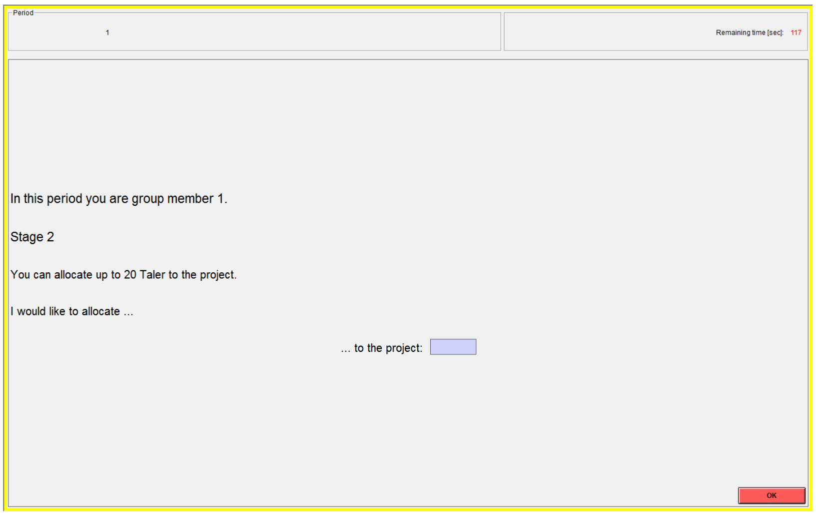
Each of the twenty periods contains three stages. In the first stage, you decide whether to invest in information about others' behaviors, information that will be made available to you in the third stage. [Prior to the second stage, you will be informed about what others in your group decided, with respect to information acquisition.] In the second stage, you will make a decision on how much to allocate to a group project. In the third stage, you will make decisions regarding possibly reducing others' incomes by assigning reduction points to them. When making your third stage decisions, you are given information about other group members' allocations from the second stage. That information may or may not be accurate. The accuracy of the information increases the more you invest in the first stage.

Stage 1

In each period, each participant is allotted *22 Taler,* which we shall henceforth refer to as his *endowment.* From this endowment, each participant can invest 0, 1, or 2 Taler for information acquisition. Taler spent on information acquisition are deduced from participant's endowment. [Prior to Stage 2, each participant is informed about the investments for information acquisition of the other group members.] {Group members are not informed about one another's investments for information acquisition.}

Stage 2

In Stage 2, each individual has to make a decision with regard to using parts of his or her endowment. You have to decide how many of 20 Taler of your endowment you wish to allocate to a *project* and how many you wish to keep for yourself. You will see the following input screen of Stage 2:



The input screen of stage 2

In the left upper corner of the screen you will find the *period number*. In the right upper corner you will find the *remaining time* for your decision *in seconds*.

You may allocate at most 20 Taler to the project. You make a decision on your project allocation by typing any whole number between 0 and 20 into the appropriate field on your screen. Together your decision in stage one and stage two determine how many Taler from your endowment you keep for yourself, i.e., *22 – Your investment for information acquisition – Your allocation to the project*. Once you have done this, your decision for this period is irreversible.

Your income at the end of stage two consists of two parts, namely:

(1) the Taler you have kept for yourself

(2) the“ *income gained from the project”*. Your income from the project is calculated as follows: *Income from the project = .4 * sum of all contributions to the project*

Your *income in Stage 2* of each period equals:

22 (endowment)
– Your investment for information acquisition
– Your allocation to the project
+.4(sum of allocations to the project)*

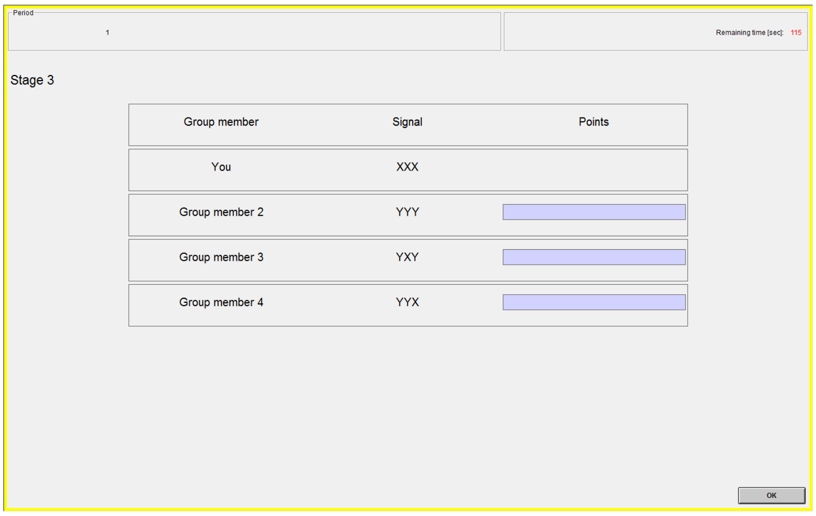
The allocations to the project are summed over the four group members, including yourself, and the total income in Stage 2, in Taler, is calculated using the same formula for each member of the group. If, for example, the sum of the allocations from all group members adds up to 60 Taler, you and all other members each receive a project income of .4 x 60 = 24 Taler. If the group members have allocated a total of 9 Taler to the project, you and all other members each receive an income of .4 x 9 = 3.6 Taler from the project.

For each Taler you keep for yourself, you earn an income of 1 Taler. If, on the other hand, you allocate one Taler from your endowment to your group's project, the sum of the allocations to the project increases by one Taler and your income from the project increases by .4 x 1 = .4 Taler. However, the income of each individual group member also increases by .4 Taler, so that the group's total income increases by .4 x 4 = 1.6 Taler. The other group members thereby also profit from your allocation to the project. In turn, you profit from other members' allocations to the project. For each Taler allocated to the project by another group member, you earn .4 x 1 = .4 Taler.

Stage 3

In Stage 3, you can decrease each individual group member's income by giving points, or leave it as it is. All other group members are allowed to decrease your income, too, if they so wish. You may assign points in the input screen of Stage 3 which shows, along with the period number and the remaining time, for each group member an indication or "signal" about their allocation to the project. Your allocation will be shown in the row "You".

Please notice that the signal for each of the three other group members only has a 50% probability of equaling his or her actual allocation to the project, unless you paid for information in Stage 1. This means that the signaled allocation to the project for each of the other group members is *accurate* (equals their actual allocation) in 5 out of 10 cases, on average. There is a 50% probability that you will see the *inaccurate signal*, which is a random number which does not correspond to the particular group member's allocation. In this case, there is an equal probability that any integer between 0 and 20 other than the actual allocation will appear.



The input screen of Stage 3

If you invested one Taler for information acquisition in Stage 1, there is a 75% probability that you receive the accurate signal. If you invested two Taler for information acquisition in Stage 1, there is a 100% probability that you receive the accurate signal. That is, in this case you will see the actual allocations of each of the others for sure.

Note that whether a given group member's allocation is signaled accurately is determined by one random draw for all group members who see the signal. That is, either all group members who did not pay for information see the accurate signal (50% chance) or they all see the inaccurate signal (50% chance). Likewise, either all group members who paid 1 Taler for information see the accurate signal (75% chance) or all see the inaccurate signal (25% chance). If the group members who did not pay for information see the accurate signal, then those who paid 1 Taler also see the accurate signal (but not necessarily vice versa). Those who paid 2 Taler always see the accurate signal. If group members see inaccurate signals, they see all the same inaccurate signal for a given group member's allocation.

Finally, the draw determining the signal for one group member's allocation is independent of the draws determining the signals for each other group member. This means there is a 50% chance that the signal for member 2 is accurate and a 50% chance that it is not accurate, for those not investing in information, and likewise there is a 50% chance of accuracy of signal in the case of member 3, and so on. Whether the random draw leads to an accurate or an inaccurate signal for member 2 has no impact on the random draw for member 3 or that for member 4.

Once you view the information on the Stage 3 screen, you have to decide how many points you wish to assign to each group member. You must enter a number at this stage. If you do not wish to alter a certain group member's income, please enter 0. You may still change your decision as long as you have not yet clicked on *OK*.

When distributing points, you incur costs in Taler: each distributed point costs you 1 Taler. The more points you assign, the higher your costs are:

Your cost for assigned points = the sum of points you assign (in Taler)

For example, if you have assigned 2 points to one group member, your cost is 2 Taler; if, in addition, you assign 7 points to another group member, your cost for that is 7 Taler; if you give the final group member 0 points, you have no cost for that member. The *total cost* to you is therefore 9 Taler (2+7+0) which are deducted from the income you had accrued as of the end of Stage 2.

If you choose 0 points for a certain group member, you do not alter this group member's income. If you allocate 1 point (choosing 1) to a group member, you *decrease* this particular group member's Taler income by *3 Taler*. If you allocate 2 points to a group member (choosing 2), you decrease his income by 6 Taler etc. *Each point allocated by you to a particular group member reduces the group member's Taler income by 3 Taler.*

The *overall* reduction in a group member's income from Stages 1 and 2 depends on the total number of points received. If, for instance, one member receives *a total of 3 points* from all other members, the income as of the end of Stage 2 is reduced by *9 Taler*. If a member receives a total of *4 points*, the income is reduced by *12 Taler.* A person who receives points will be informed about the income reduction at the end of each period, without detailed information on the group member (or members) who distributed the point or points.

For your total income at the end of Stage 3, it follows that:

Total Taler income at the end of Stage 3 =

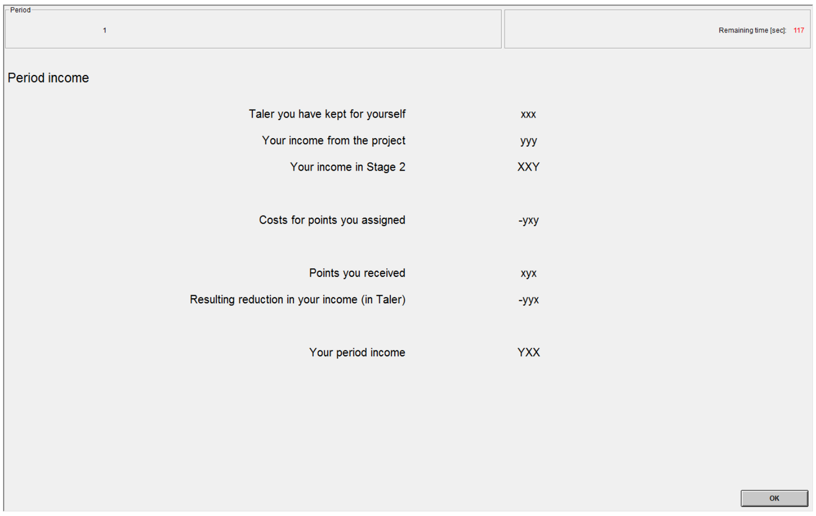
Income after Stage 2

– 3(sum of [effective] points others assign to you)*

– cost of [effective] points you assign to others

Please notice received points cannot decrease your income by more than the income after Stage 2. That is, if the expression [Income after Step 2 – 3*(sum of received points)] yields a negative number, we will reset it to zero. However, your total Taler income at the end of

Stage 3 can be negative if the costs for points that you assign exceed the income after Stage 2 minus the reduction of income due to received points. In other words, there is a limit on the cost others' reductions can impose on your earnings for a period insofar as these alone cannot drive your earnings to below zero, but you always incur the full cost of imposing reductions on others, even if they cause your period income to become negative.



The income screen at the end of Stage 2

Once all members of the group have made their decisions, you will be informed about your period income in the income screen at the end of Stage 3. Here, you see how many Taler you kept for yourself, your income from the project, and the resulting income in Stage 2. In addition, you are informed about the costs for points you assigned, the number of points you received, as well as the resulting reduction in income. Finally, you will see your period income. By pressing the OK button you will proceed to the next period where you receive a new endowment and face all three stages again. There are in total 20 periods and the group composition remains the same.

Your total income at the end of the experiment equals the sum of all period incomes:

Total income (in Taler) = Total sum of period incomes

(If the sum of period incomes is negative, your income is 0 Taler.)

Do you have any further questions?

DFG Research Group 2104

– Latest Contributions

2017:

Paetzel, Fabian, Lorenz, Jan and Tepe, Markus: Transparency diminishes framing-effects in voting on redistribution: Some experimental evidence. Working Paper Nr. 2017-06.
http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-06.pdf

Schwaninger, Manuel, Neuhofer, Sabine and Kittel, Bernhard: Offers Beyond the Negotiating Dyad: Including the Excluded jn a Network Exchange Experiment. Working Paper Nr. 2017-05.
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Kittel, Bernhard, Neuhofer, Sabine and Schwaninger, Manuel: Need-based Justice in Social Exchange Networks. Working Paper Nr. 2017-04. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-04.pdf

Diederich, Adele and Wyszynski, Marc: Need, framing, and time constraints in risky decision making. Working Paper Nr. 2017-03. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-03.pdf

Kittel, Bernhard, Kanitsar, Georg and Traub, Stefan: Knowledge, Power, and Self-interest. Working Paper Nr. 2017-02. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-02.pdf

Traub, Stefan and Krügel, Jan Philipp: Risk Taking and the Welfare State: Some Experimental Evidence. Working Paper Nr. 2017-01. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-01.pdf

2016:

Guo, Lisa, Trueblood, Jennifer S. and Diederich, Adele: Thinking Fast Increases Framing Effects in Risky Decision-making. Working Paper Nr. 2016-04. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-04.pdf

Paetzel, Fabian and Sausgruber, Rupert: Entitlements and loyalty in groups: An experimental study. Working Paper Nr. 2016-03. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-03.pdf

Nicklisch, Andreas, Grechenig, Kristoffel and Thöni, Christian: Information-sensitive Leviathans. Working Paper Nr. 2016-02. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-02.pdf

Greiff, Matthias and Paetzel, Fabian: Less sensitive reputation spurs cooperation: An experiment on noisy reputation systems. Working Paper Nr. 2016-01.
http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-01.pdf

2015:

Schramme, Thomas: The metric and the threshold problem for theories of health justice: A comment on Venkatapuram. Working Paper Nr. 2015-05. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2015-05.pdf

Nicklisch, Andreas, Grechenig, Kristoffel and Thöni, Christian: Information-sensitive Leviathans – the emergence of centralized punishment. Working Paper Nr. 2015-04.
http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2015-04.pdf

Schramme, Thomas: Setting limits to public health efforts and the healthisation of society. Working Paper Nr. 2015-03. http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2015-03.pdf

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http://needs-based-justice.hsu-hh.de