The Interaction of Legal and Social Norm Enforcement

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Abstract

Although legal sanctions are often non-deterrent, we frequently observe compliance with 'mild laws'. A possible explanation is that the incentives to comply are shaped not only by legal, but also by social sanctions. This paper employs a novel experimental approach to study the link between legal and social norm enforcement. We analyze whether the two institutions are complements or substitutes. Our results show that legal sanctions partially crowd out social norm enforcement. The welfare effect from mild laws is positive, however, as a higher level of compliance is achieved at lower enforcement costs.

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Keywords: social sanctions, legal sanctions, norm enforcement, mild laws, laboratory experiment, VCM, public goods.

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1 Introduction

Societies have come up with laws to regulate many situations in economic and societal life where individual and social interests are in conflict (environmental laws, tax laws, etc.). While these laws are usually backed by legal sanctions, potential offenders often face rather low detection risks and relatively mild sanctions. Legal norm enforcement is then non-deterrent, in the sense that rational individuals with reasonable degrees of risk aversion should be expected to deviate from the law. Nevertheless, we frequently observe widespread compliance with 'mild laws'.[1] One explanation put forward by legal scholars is that the incentive to comply with the law not only derives from formal law enforcement, but also "*from the informal enforcement of social mores by acquaintances, bystanders, trading partners, and others*" (Ellickson 1998, p.540). Traxler and Winter (2009) provide survey evidence which supports this reasoning. They show that a substantial fraction of the population is willing to impose sanctions (disapproval or social exclusion) on individuals who are, e.g., drunk driving, fare dodging or cheating on taxes. The importance of such decentralized, social sanctions for the enforcement of compliance is documented in a rapidly growing body of literature.[2] However, a key question is still left unanswered by this literature: how does formal law enforcement affect the informal enforcement of norms? Are these two enforcement institutions substitutes or complements, i.e., do centralized legal sanctions crowd out or crowd in decentralized social sanctions?

Ideally, these questions would be studied in a natural environment. As the introduction of or the change in legal regulations is typically endogenous, it is quite difficult to identify the causal link between legal and social norm enforcement from field data. Moreover, there are obvious constraints in the measurement of social sanctions. To avoid these empirical limitations, our study builds on a simple and parsimonious laboratory experiment. This allows us to assess our research questions in a controlled environment. In the experiment, subjects in groups of four play a one-shot public-goods game in which they face a voluntary-contribution mechanism. Our baseline treatment (Base) captures a situation in which only social sanctioning is possible while legal sanctions are absent. Players observe each others' contributions to the public good and can assign

[1] See, e.g., Becker (2007) on tax evasion and Cohen (2000) on the enforcement of environmental regulations. A general discussion is provided by Tyran and Feld (2006) and Galbiati and Vertova (2008a).

[2] Seminal experimental studies are Fehr and Gächter (2000), Masclet et al. (2003). For a law and economics treatment of social sanctions, see Posner (2000). For a theoretical contribution, see e.g., Rege (2004).

costly punishment points which reduce the payoff of the punished player (Fehr and Gächter 2002, Herrmann et al. 2008). In addition to these decentralized sanctions, the second treatment (LAW) introduces legal sanctions. The law imposes sanctions on deviations from the socially optimal contribution level. More specifically, players are randomly monitored after their contribution and sanctioning decisions. A player who is detected contributing less than the social optimum is fined by a central punishment authority – independently of any social sanctions incurred. Comparing subjects' sanctioning behavior between the two treatments then allows us to analyze the impact of centralized sanctions on social norm enforcement in a controlled set-up.

The impact of legal on social sanctions can be decomposed into a direct and an indirect effect, where the latter is linked to the actual compliance level. Presumably, compliance, i.e., the level of individuals' contributions, is sensitive to the treatment conditions: one might expect contributions to be higher when subjects face the risk of social versus the risk of social *cum* legal sanctions. As a consequence, differences in punishment choices might be due to the change in contributions rather than the introduction of legal sanctions *per se*. To disentangle direct and indirect treatment effects, we developed a method in the spirit of Selten's (1967) strategy method. In particular, we implement a novel strategy method at the punishment stage of the public-goods game.[3] After their contribution choice, players are confronted with a series of different triples of other group members' contributions. One of these triples covers the others' actual contributions, while the remaining ones are hypothetical situations. Subjects have to make punishment decisions in each situation, knowing that only the decisions for the actual contribution triple will be payoff-relevant (of course, without knowing which one this actually is).

This new method has several advantages. First of all, it assures that contribution situations are virtually constant between treatments. Keeping constant others' contributions, we can measure the direct impact of legal sanctions on individuals' sanctioning behavior. Second, we observe a series of individual punishment decisions for truly exogenous contributions rather than single punishment choices that follow endogenous contribution choices. Exposing players to randomly varied contribution triples, we can also analyze the causal effect of others' contribution levels and composition (e.g., facing a group of one high and two low contributions as compared to three intermediate contributions) on the extent of punishment.

[3]Fischbacher et al. (2001) and Fischbacher and Gächter (2009) use strategy methods at the contribution stage of public good games. For a related application in prisoners' dilemma-experiments, see Falk et al. (2005).

Our experiment points out several results. First of all, comparing the punishment behavior in treatment BASE to previous studies on decentralized punishment reassures that our novel strategy-method approach does not affect or bias subjects' behavior in a systematic, unpredicted way. In line with the existing literature, we observe that a large fraction of the population engages in decentralized norm enforcement. Typically, subjects sanction those individuals who contribute less than themselves, with the sanctioning intensity increasing in the difference in contributions. Moreover, our data reveal that the punishment incurred by an individual crucially depends on the contributions of the other group members. Everything else being equal, individuals assign stricter sanctions to a free-rider the higher the contributions of the unaffected players in the group are.

This pattern of social norm enforcement is prevalent in both treatments. In the LAW treatment, however, decentralized sanctioning is significantly less intense: the presence of legal sanctions partially crowds out the social norm enforcement. The decline in sanctions is particularly pronounced when the difference between the punisher's and punishee's contribution is large. Our data further show that actual contributions are nevertheless higher in the LAW treatment. The combination of legal and social sanctions thus produces more pro-social behavior than the decentralized enforcement on its own. As long as the centralized enforcement system comes at reasonable costs, the increase in cooperation is achieved at lower overall costs of legal and social sanctioning. Compared to an 'archaic world' where only decentralized punishment is possible, the introduction of mild laws might thus give rise to substantial welfare gains.

To the best of our knowledge, we are the first to present clear-cut evidence on the interaction of legal and social sanctions. While the importance of the interplay between the two sanctioning institutions is generally acknowledged among lawyers and economists, practitioners and theorists likewise (Ellickson 1998, Falk et al. 2005, Meares et al. 2004), up to now both have only been studied in isolation. Decentralized sanctions are discussed in a large body of mainly experimental literature, showing that many subjects are willing to forego personal profits to punish anti-social behavior.[4] Although social sanctions help to overcome the free-rider problem, an increased level of cooperation is usually accompanied by significant efficiency losses caused by the cost of social sanctioning (at least in the short run; see Gächter et al. 2008).

[4]See, e.g., Fehr and Gächter (2000, 2002), Masclet et al. (2003), Gürerk et al. (2006), Carpenter (2007), Nikiforakis and Normann (2008).

A different strand of experimental research considers centralized sanctions in isolation. Cardenas et al. (2000), Tyran and Feld (2006), Galbiati and Vertova (2008a, 2008b), Hörisch and Strassmair (2008), and Kosfeld et al. (2009) explore if, when, and why legal sanctions influence compliance. Similarly, studies of tax evasion in the lab consider auditing and centralized fines (see, e.g., Alm and McKee, 2006, Fortin et al., 2007). In line with economic theory, they typically find that strong (deterrent) centralized sanctions are effective in enforcing compliance – in particular, if they emerge endogenously via, e.g., voting. Galbiati and Vertova demonstrate that even non-deterrent sanctions might foster compliance. Their results suggest that mild laws serve as coordination devices, shaping beliefs about others' behavior and hence the degree of cooperation in the short run. However, Galbiati and Vertova (2008a) also show that mild laws *per se* are not sufficient to establish norm-compliance in the long run. Our study, which is the first to combine the analysis of centralized and decentralized sanctioning, suggests that the combination of social and mild legal sanctions – as proposed by Ellickson (1998) – might be successful in achieving compliance with mild laws.

2 The Experiment

To assess the impact of legal on social sanctions, we consider a situation with scope for social norm enforcement. Players are randomly matched into groups of $n = 4$ and play a public-goods game at **stage I**. Each player $i \in \{1..4\}$ has to decide how many tokens c_i of his initial endowment $E = 20$ to contribute to a joint project (the public good), and how many to keep for himself. Each token allocated to the public good increases the payoff of each player in the group by αc_i, with the marginal per capita return $\alpha = 0.4$. The primary individual payoff π_i^I is then given by $\pi_i^I = E - c_i + \alpha \sum c_j$. As $\partial \pi_i^I / \partial c_i = -1 + \alpha < 0$, each player has the dominant strategy to free-ride on others' contributions. The unique Nash equilibrium therefore is to keep the entire endowment E. However, since $1/n < \alpha$, it is socially optimal if all players contribute their entire endowment to the public good. Players thus face a social dilemma: individuals' and group's interests are in conflict and free-riding can be considered as anti-social behavior. Society may want to regulate the dilemma by establishing a centralized norm enforcement institution ('law', 'police'), that imposes sanctions on free-riders. In addition, sanctions may also be imposed in a decentralized way.

To account for centralized and decentralized sanctioning institutions, we add two additional stages to the game. At **stage II**, players observe each others' contributions and can then decide to impose social sanctions by assigning up to 10 punishment points to each player. Each punishment point d_{ij} costs the punisher i one token, while at the same time it reduces the punished player j's payoff by three tokens (Fehr and Gächter 2002). The individual payoff from stage II is thus given by $\pi_i^{II} = -\beta \sum_{j \neq i} d_{ij} - \gamma \sum_{j \neq i} d_{ji}$ (with $\beta = 1$ and $\gamma = 3$). In this situation, social sanctions constitute a second-order public good. While everyone would be better of if free-riding were credibly sanctioned (such that the social first-best is achieved without any actual punishment), individually each player has a dominant strategy not to invest in sanctioning at stage II. To see why, let us assume that at stage I all players would anticipate to be sanctioned if $c_i < E$, and that they expect a sanctioning pattern to be such that $\partial(\pi_i^I + \pi_i^{II})/\partial c_i < 0$. Rational players would then choose $c_i = E$. However, the corresponding beliefs cannot be part of a Baysian Nash equilibrium, since $\partial \pi_i^{II}/\partial d_{ij} = -\beta < 0$.

Finally, at **stage III**, legal sanctions are implemented as a stochastic payoff. A player is monitored with a fixed probability $p = 1/8$. In this case, he faces a fine which is proportional to the deviation from the contribution level that is prescribed by the law, $L = E = 20$, such that his expected payoff from stage III becomes $\pi_i^{III} = -p\, s\, (L - c_i)$, with $s = 1.2$. The expected payoff from all three stages is given by

$$\Pi_i = \underbrace{E - c_i + \alpha \sum_{j=1}^{4} c_j}_{\pi_i^I} \; - \; \underbrace{\beta \sum_{j \neq i} d_{ij} - \gamma \sum_{j \neq i} d_{ji}}_{\pi_i^{II}} \; - \; \underbrace{p\, s\, (L - c_i)}_{\pi_i^{III}} \tag{1}$$

Our choice of parameters implies that legal sanctions in isolation are 'non-deterrent', in the sense that contributing zero is still the dominant (expected payoff-maximizing) strategy: $\partial(\pi_i^I + \pi_i^{III})/\partial c_i = -1 + \alpha + ps < 0$. Our design therefore provides a scenario in which social sanctions can potentially help to achieve compliance with a mild law.

Note that we implement a sequence where social sanctions are applied before potential legal sanctions take place. This sequence not only appears to be natural for many situations (consider, for instance, the case where a partner learns about the spouse's income tax evasion before the tax authorities do). One might also argue that it is a harder test for observing an interaction between legal and social sanctions. If crowding effects occur in the sequence implemented here,

we conjecture the effect to be even stronger if we used the opposite order. The confirmation of the hypothesis is left for future research.

Treatments The experiment implements two treatment conditions. Treatment BASE consists of stages I and II only. Hence, free-riders only face the threat of social sanctions. In treatment LAW, we additionally include the third stage with legal sanctions. Comparing behavior between these two treatments allows us to observe individuals' sanctioning behavior d_{ij} in the presence and absence of legal sanctions. As noted earlier, individual sanctioning decisions are measured using a strategy method. At stage II of both treatments, each subject i faces 11 contribution triples $\{c_j^t, c_k^t, c_l^t\}$, $t \in [1, 11]$ (with the subindices $j \neq i$, $k \neq i$, $l \neq i$ denoting the other subjects' hypothetical contributions). The order of these triples is randomized for each subject. One of the triples covers the actual contributions of the other players in the matching group. The remaining ten triples are hypothetical combinations of contributions, each being randomly drawn from a pre-defined set of combinations.[5] For each contribution triple, subjects have to decide how many punishment points (if any) they want to allocate; knowing that 10 out of the 11 situations are hypothetical. It is also common knowledge that only the decisions made in the actual contribution-triple are payoff-relevant – without knowing which one is the 'real' triple, of course. In this way, we collect data from 3×11 sanctioning choices for each subject.The procedure assures that the hypothetical contribution situations are exogenous and orthogonal to our treatments. By comparing the punishment decisions between LAW and BASE made for the ten hypothetical cases, we can therefore identify the *ceteris paribus* impact of the legal system on individuals' social sanctioning behavior (i.e., keeping constant the other player's contributions).

Predictions Let us now discuss different predictions regarding the treatment's impact on social sanctioning. The benchmark for the rational (and risk-neutral) self-interested player is straightforward. As illustrated above, social norm enforcement is a second-order public good and standard theory predicts zero sanctioning in both treatments. Given the overwhelming empirical evidence, however, we know that subjects do make use of their punishment option (see Nikiforakis

[5]The following procedure was used. First, we defined three sets of contributions (low, intermediate and high): $c^L \in \{0, ..., 4\}$, $c^M \in \{5, ..., 15\}$, $c^H \in \{16, ..., 20\}$. Within each of the combinations, $\{c^L, c^L, c^L\}$, $\{c^L, c^L, c^M\}$, $\{c^L, c^L, c^H\}$, $\{c^L, c^M, c^M\}$, $\{c^L, c^M, c^H\}$, $\{c^L, c^H, c^H\}$, $\{c^M, c^M, c^M\}$, $\{c^M, c^M, c^H\}$, $\{c^M, c^H, c^H\}$, $\{c^H, c^H, c^H\}$, we randomly generated 8 different contribution triples by sampling with replacement from the corresponding sets c^L, c^M and c^H (see Appendix I). E.g., a subject might face $\{0, 2, 3\}$ for the combination $\{c^L, c^L, c^L\}$ and $\{1, 2, 10\}$ for $\{c^L, c^L, c^M\}$, etc. Another subject might face $\{1, 3, 3\}$ for the former and $\{0, 2, 14\}$ for the latter. If the set $\{0, 2, 14\}$, by chance, did correspond to the real triple, the subject would not face this situation, but instead another one of the pre-defined contribution triples for $\{c^L, c^L, c^M\}$ would be randomly selected.

and Normann 2008 for a recent survey). Social-preference models, surveyed in Fehr and Schmidt (2006), are able to account for this behavior. These models, in particular the consequentialist theories, predict that the introduction of legal sanctions crowd out social sanctions.

The intuition behind this prediction is best illustrated within the framework of inequality aversion (Fehr and Schmidt 1999), where punishment is motivated by the disadvantageous inequality experienced by the punisher. Legal sanctions clearly reduce the payoff inequality that a compliant contributor i ($c_i = 20$) experiences with respect to a free-rider j ($c_j = 0$). Without legal sanctions the difference in π^I would be $c_i - c_j = 20$ token; with legal sanctions, the expected gap in $(\pi^I + \pi^{III})$ equals $(c_i - c_j)(1 - ps) = 17$ token.[6] By mitigating the payoff inequality that arises from free-riding, the centralized sanctions reduce the extent of decentralized sanctions imposed by an inequality-averse player. Hence, legal sanctions should partially replace social sanctions.

A different strand of literature has emphasized the role of emotions, in particular anger, in motivating individuals' sanctioning behavior (Bosman and van Winden 2002, Hopfensitz and Reuben 2009). Along these lines, the introduction of the legal system could also crowd out social punishment, if the anticipation of legal sanctions reduces the punisher's emotional drive to impose social sanctions on the free-rider. One might argue, however, that the strategy method used in our approach alters the subjects' emotional states and induces more 'cold' decision making at the sanctioning stage. We might therefore observe a lower level of punishment than in the case of unconditional punishment decisions.[7] As we apply the strategy method in all treatments, this should not have an impact on treatment differences.

Finally, one can derive an alternative prediction regarding the treatment effect from legal theories. In particular, expressive theories of law (Cooter 1998, McAdams 2000) suggest that the introduction of a legal system will shape player's perceptions on what is 'good' and 'bad' conduct. In turn, we should observe that players coordinate on imposing more sanctions on those who more clearly deviate from the legal norm, $L = 20$. Legal sanctions should therefore crowd in social sanctions for low contribution levels.

[6] Trautmann (2009) explores the case of inequality aversion under risky payoffs. Following the proof of proposition 5 in Fehr and Schmidt (1999), one can easily show that the punishment pattern supporting an equilibrium $c_i^* = c_j^* \in [1, 20]$ is given by $d_{ij} = (c_i^* - c_j)(1 - ps)/\mu$ (where μ captures the exogenous distribution of preference types and the punishment costs β and γ). Hence, social sanctions are decreasing in ps. Note that this observation applies to any of the multiple equilibria that can emerge.

[7] Note that a number of studies (e.g., Brandts and Charness 2000) did not find significant differences in behavior between strategy and direct response methods in other contexts.

Implementation The computerized experiments (using z-Tree; Fischbacher 2007) were run in March 2009 in Bonn. 96 students from all majors were randomly recruited (using Orsee; Greiner 2004) and participated in one of two treatment sequences. In Sequence 1, subjects first played the BASE treatment and afterwards played the LAW treatment. The other sequence reversed the order, i.e., subjects first played LAW and then BASE (Sequence 2).[8] After subjects arrived in the lab, they were randomly and anonymously allocated into matching groups. Subjects then received a written copy of the instructions for the first treatment. Additionally, in order to create common knowledge about the instructions, we read them out aloud. The instructions and the information on the computer screens were written in a neutral language, omitting words like punishment, sanctions, or law to avoid framing and demand effects. Before the experiment started, subjects had to answer a set of control questions to make sure that they understood the rules of the game.[9] After they finished the first treatment, subjects were told that they would now participate in a second treatment. They did not receive any feedback about their payoffs or the others' actual contribution choices from the first treatment. After playing the alternative treatment, which followed the same procedure as before, subjects received feedback about the outcomes of both treatments. Thereafter, they had to fill out a short questionnaire asking, e.g., for socio-economic data. An entire session lasted approximately 60 minutes. Subjects were paid according to their cumulated period payoffs at a rate of 20 Cent per tokens. Participants earned about 13 Euros on average, including a show-up fee of four Euros.

3 Results

This section presents the results from the experiment. Our main focus is on the impact of legal on social sanctions. Moreover, we analyze cooperation and the overall welfare effect from mild laws. We first present the findings from the between-subject comparison. These results are corroborated by the within-subject comparison, which is presented thereafter.

[8] Sequence 1 captures the introduction of a mild law into a society which is initially governed by decentralized norm enforcement. Sequence 2 starts from a situation with legal and social norm enforcement and then removes the centralized enforcement institution.

[9] Two subjects, one in each treatment, had substantial problems understanding the game. They repeatedly failed to answer the control questions. As we did not want to embarrass them in front of the other students, we let them take part in the experiment but excluded their data from the analysis. This can be further motivated by the fact that the two assigned 635% (444%) more punishment points than the median (average) of the remaining subjects. The results presented in the following section are robust to excluding the two groups of the two 'drop-out' subjects.

3.1 Between-subject comparison

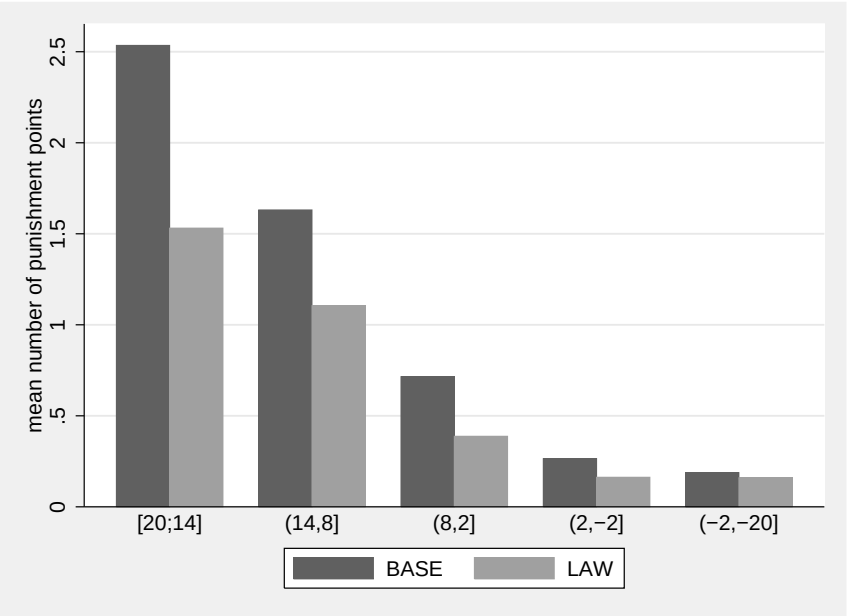
Legal and social sanctions First, we consider the direct impact of legal on social sanctions. Below we will show that subjects' actual contributions are endogenous to our treatment conditions. To identify the direct treatment effect, we therefore focus on subjects' punishment decisions for the 10 hypothetical contribution triples.[10] As these triples are – in expectation terms – the same between treatments, we can assess the direct impact from the treatment manipulation, i.e., holding constant the contributions that the punishers face.

In line with the existing literature discussed above, we observe a substantial amount of social sanctions. Figure 1 displays the sanctioning pattern by comparing the mean amount of punishment (on the vertical axis) for different gaps $c_i - c_j$ in public-good contributions between punisher i and punishee j (horizontal axis). The figure reveals that sanctions are literally social in both treatments, in the sense that they are mainly targeted at free-riders: subjects impose sanctions on those players who contributed less than they themselves did, with the sanctioning intensity increasing in the inequality in contributions. In contrast, anti-social punishment (see Herrmann et al. 2008), i.e., sanctions targeted at contributors where $c_i - c_j < 0$, is rarely observed in either treatment (which is why Figure 1 groups those observations in the bar for $(-2, -20]$). As is clearly visible from Figure 1, social sanctioning is less intense in Law than in Base. While the treatment effect is small for minor 'offenses', the decline in sanctions is particularly pronounced when the difference between c_i and c_j is large, i.e., when the degree of 'non-compliance' – and thus the expected legal sanction – is high. This provides some first, indicative evidence that legal sanctions tend to crowd out social sanctions.

Averaging over all players and all hypothetical situations, the number of assigned punishment points d_{ij} is 18% lower in Law than in Base, dropping from .84 down to .69 points. However, a non-parametric test reveals no significant difference (rank-sum test, comparing the mean number of punishment points assigned over all hypothetical situations per subject, $p = 0.528$, two-sided). The reason is likely to be that those observations with small differences $c_i - c_j$, where there is hardly any punishment in either treatment, are substantially over-represented. Put differently, averaging over all contribution situations neglects that punishment choices and the treatment effect seems to depend, among other things, on the gap in i and j's contributions. In order to

[10] More precisely, this subsection focuses on the punishment decision for the 10 hypothetical contribution triples of the first part of the experiment. Including the real contribution triple does not change any of the following results.

Figure 1: Mean Punishment Patterns



control for c_i and c_j, we conduct a regression analysis. Making use of the panel structure of our data (recall that for each subject we observe 3×10 sanctioning decisions for exogenous triples) and accounting for $0 \leq d_{ij} \leq 10$, we estimate a random-effects Tobit model. Since each subject faced different contribution triples in a random order, we also control for the sequence of triples (t). Furthermore, we also control for the (hypothetical) contributions of the other, unaffected, group members ($c_{k,l} = (c_k + c_l)/2$).

Estimation results are presented in Table 1. The specification from Column (1) replicates the non-parametric test result. The treatment on its own does not reach statistical significance (even with a control for potential sequence effects). Test statistics indicate a very poor performance of this first estimation: the null hypothesis that all regressors are equal to zero cannot be rejected ($p = 0.284$), suggesting that the specification is missing crucial explanatory variables. The picture changes substantially once we account for the gap in the punisher's and the punishee's (hypothetical) contributions, $c_i - c_j$. Column (2) shows a highly significant effect of this gap. The positive coefficient indicates that subjects impose more punishment points the larger the difference between the own and the other's contribution.[11] At the same time, the estimated treatment ef-

[11] We also considered different specifications, e.g., using c_i and c_j as separate regressors, or using j's deviation from the group's minimum contribution or from the group's mean contribution as benchmarks. The estimation of these models delivers results that are very similar to those reported here. In particular, $c_i - c_j$ seems to be quantitatively the most important predictor of the punishment decisions.

Table 1: Random-effects Tobit regression: Marginal effects on social sanctions

Dependent variable: d_{ij}				
	(1)	(2)	(3)	(4)
Law	-0.097	-0.279*	-0.284*	-0.275*
	(0.158)	(0.148)	(0.149)	(0.151)
$c_i - c_j$		0.064***	0.067***	0.067***
		(0.004)	(0.004)	(0.004)
$(c_i - c_j) \times LAW$				-0.001
				(0.004)
$c_{k,l}$			0.012***	0.012***
			(0.002)	(0.002)
t	-0.008	-0.003	-0.003	-0.003
	(0.005)	(0.004)	(0.004)	(0.004)
Log-likelihood	-3057	-2443	-2428	-2428
Observations	2820	2820	2820	2820

Notes: The table displays the marginal effects evaluated at the mean (conditional on being uncensored) from random effects Tobit regressions. The dependent variable is the number of punishment points assigned from i to j, d_{ij}. The sample covers all punishment choices for hypothetical triples in the initial treatment. The treatment dummy Law equals unity in treatment Law and zero otherwise. Variable $c_i - c_j$ measures the gap between punisher's and punishee's contributions. Variable $c_{k,l} = (c_k + c_l)/2$ measures the average (hypothetical) contribution of the two other, unaffected, group members. t depicts the sequence of triples. Estimation output for the constant is omitted and standard errors are in parentheses. Level of significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

fect becomes significant at a 10%-level. Moreover, the log-likelihood of the specification increases dramatically. In Column (3) we additionally account for the mean contribution of the other two group members. Again, we obtain a significantly negative estimate for the treatment effect that is close to the 5%-level. Hence, including legal sanctions reduces the extent of social sanctions. The estimates suggest that individuals on average allocate 0.28 punishment points less when a mild law is in place. To compare the effect size, note that free-riding by one additional token (a *ceteris paribus* decline in c_j of one token) increases the sanctioning that player i imposes on j by only 0.06 points. This indicates that the decline in social sanctioning in the Law treatment is non-negligible. Furthermore, note that there is a significantly positive effect from $c_{k,l}$. Keeping contributions of players i and j fixed, an increase in average contributions of the unaffected group members thus results in stronger sanctions for player j. Quantitatively, however, the contribution level of $c_{k,l}$ is substantially less important for the punishment choice of i than the direct comparison between c_i and c_j.

Specification (4) explores whether there is an interaction between the treatment dummy and the extent of free-riding, $(c_i - c_j) \times LAW$. The estimation clearly neglects this conjecture. This means that the decentralized punishment of free-riding *on the margin*, i.e., the amount of sanctions that player i imposes on j if the latter reduces c_j by one unit, remains constant between treatments. While marginal social sanctions remain constant, adding the legal sanctions in treatment LAW, increases the expected marginal punishment by $p\,s$ (see equation 1). The marginal overall sanctions – social *cum* legal – are thus higher in LAW than in BASE. This suggest that, despite a partial crowding out of decentralized punishment by centralized sanctions, there are stronger incentives for cooperation under the mild law.[12]

Summarizing the first set of results on the direct impact of legal on social sanctions, we have found a negative treatment effect from *LAW*. The presence of the legal system reduces individuals' sanctioning behavior. Legal norm enforcement replaces rather than reinforces social norm enforcement. This finding is in line with predictions stemming from social-preference models, but it conflicts with expressive theories of law. It is important to note, however, that in the simple game situation at hand, it is quite salient what is socially desirable and what is undesirable. One might conjecture to find support for expressive theories of law in more complicated environments. Regarding the composition of the other group members' contributions, we find that (i) j is punished less the more he contributes and (ii) is punished more, the more i contributes; and (iii) j is punished more in a group of high contributors than in a group of low contributors. The last observation implies that contributions trigger a negative externality on other group members' payoffs at the punishment stage of the game, as they *ceteris paribus* increase the magnitude of sanctions incurred for a given contribution level.

Contributions The next interesting step is to see how subjects' contribution decisions are affected by the introduction of non-deterrent legal sanctions. Figure 2 shows a box-plot of individuals' contribution to the public good. In treatment BASE, where only social sanctions are possible, the mean contribution level is 11.8 (median: 12). In treatment LAW, where legal and social sanctions are effective, the mean is 15.1 (median: 17). The increase in contributions of 28%

[12] All these results (as well as those presented in Table 3 below) are robust when we control for self-reported risk attitudes and other individual characteristics (e.g., age, gender, etc.) – none of which turn out to be significant. The same holds true for the treatment effect on contributions reported below. The data suggest that risk-aversion neither has a direct impact on contributions nor is there any interaction with the treatment: highly risk-averse and close to risk-neutral subjects respond similarly to the LAW treatment.

is highly significant (rank-sum test, $p = 0.0098$, two-sided). This treatment effect extends the results from Galbiati and Vertova (2008a, 2008b). They show that a mild law on its own increases cooperation significantly. Our findings demonstrate that the effect is robust when the mild law is paired with decentralized punishment – even when the formal sanctioning institution crowds out the informal one.

Figure 2: Contributions

The observation that contributions increase in Law is important. It reveals that the combination of centralized and decentralized sanctioning institutions is successful in enforcing cooperation – despite the crowding out of social sanctions. Since our study was designed to analyze the impact of legal on social sanctions, we cannot exactly identify the reasons for the strong increase of subjects' contribution in Law. In contrast to Galbiati and Vertova, who highlighted the coordination function of mild laws, our results point to steeper incentives from punishment. Recall that the marginal social *cum* legal sanction that is imposed for free-riding is higher in treatment Law than in Base. In fact, the point estimates from Table 1 suggest that the expected payoff impact from free-riding is positive in treatment Base, but slightly negative in Law. Based on our parameter choices and the marginal effects from Table 1 (i.e., evaluated at the mean and conditional on being uncensored), we get $\partial(\pi_i^I + \pi_i^{II})/\partial c_i = -1 + \alpha + (n-1)\gamma 0.06 = -0.06$ and $\partial(\pi_i^I + \pi_i^{II} + \pi_i^{III})/\partial c_i = -1 + \alpha + (n-1)\gamma 0.06 + ps = 0.09$. Hence, contributing zero would still be

the expected payoff-maximizing strategy in BASE (although with a very small payoff advantage), but not in LAW. This underlines that a society which manages to establish a mild law together with decentralized norm enforcement can arrive at a successful institutional setup. While Galbiati and Vertova (2008a) have found that mild legal sanctions *per se* fail to maintain high contributions in a repeated game, our results suggest that the combination of legal *cum* social sanctions should be more effective. It is due to future research to test this conjecture.

Welfare effect So far we have shown that the LAW treatment partially crowds out social sanctions and that it increases the actual level of cooperation at the contribution-stage. To assess the overall welfare-impact of the treatment, we have to consider the realized legal sanctions as well as the decentralized sanctioning behavior for the actual contribution triple. In line with the above results, the data show a substantial crowding out effect in the punishment decisions for the actual contribution triples as well. The mean number of punishment points assigned declines from 0.71 in BASE to 0.30 in LAW. This drop of 57% is significant at the 5%-level (rank-sum test, $p = 0.031$, two-sided). The pronounced difference is driven by two effects. On the one hand, the direct crowding out effect that we demonstrated above leads to a reduction in social punishment – even when we keep the others' contributions constant. On the other hand, there is now an indirect effect as contribution levels are higher in LAW than in BASE. Since higher contributions lead to lower punishment, we observe a further decline in social sanctioning. This also implies that the actual welfare gain from introducing legal sanctions is larger than suggested by the analysis of the direct effect.

Mean Payoff	BASE	LAW	Δ
Public good, π^I	27.10	29.08	+7%
Social sanctions, π^{II}	-8.51	-3.60	−57%
Legal sanctions, π^{III}		-0.77	
Total payoff, Π	18.59	24.65	+33%

Table 2: Payoff comparison between treatments

A summary of the payoff comparison is provided in Table 2. Taking (i) the increase in cooperation, (ii) the decline in the costs for (giving and receiving) decentralized punishment, as well as (iii) the costs for (receiving) centralized sanctions together, the mean payoff in treatment LAW increases by 33% (as compared to BASE). This comparison does not account for the costs of the legal system. As long as the mild law can be implemented at reasonable costs, however,

there is scope for potentially large welfare gains. If, for instance, the social costs for reducing an individual's payoffs by 1 token were the same if it were via legal or via social sanctions (namely $\beta/\gamma = 1/3$), the treatment would still achieve a net-welfare gain of 32%.

3.2 Within-subject comparison

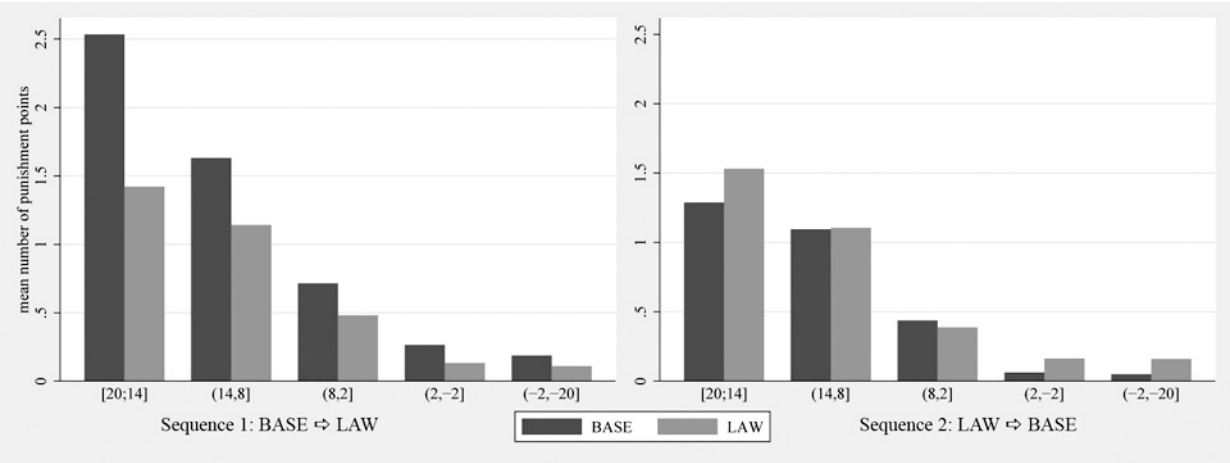
Introducing a mild law It seems natural to think of a situation where legal sanctions are introduced into an environment that features only social sanctions. This exogenous change from an 'archaic' into a 'modern' society is captured in the first sequence of our experiment.[13] In this sequence, subjects first faced treatment BASE and subsequently played treatment LAW (of course, without receiving any feedback in-between). This allows for a within-subject analysis of how the sanctioning behavior reacts to the introduction of a legal system.

As above, we first compare the social sanctions for the hypothetical contribution situations in the two treatments. The left panel of Figure 3 displays the sanctioning pattern in the hypothetical contribution situations for those subjects who first play treatment BASE and treatment LAW afterwards. This figure closely resembles what we have observed above (see Figure 1). The mean amount of punishment for different gaps $c_i - c_j$ in public-good contributions strongly declines when the centralized sanctions are introduced. Decentralized punishment drops from .84 down to .70, the difference now being highly significant in the non-parametric test (sign-rank test, comparing each subject's mean amount of punishment in BASE to the mean amount in LAW, $p = 0.0090$, two-sided). Hence, there is again a significant crowding out of social sanctions by legal sanctions in the within-subject comparison, suggesting that the results from the between-comparison are robust.

The same holds true regarding the analysis of contributions. After the introduction of legal sanctions, the mean contribution increases from 11.8 to 15.3 (sign-rank test, $p \leq 0.001$, two-sided). Thus, the crowding out of social sanctions is again accompanied by an increase in cooperation. Not surprisingly, we also observe a substantial decline in the decentralized punishment imposed on the actual contribution choices (from 2.12 to 1.21; $p \leq 0.001$). Consequently, the introduction of a mild law yields a 28% increase in the average payoff, which coincides qualitatively and quantitatively with the effects displayed in Table 2.

[13] On the endogenous formation of institutions, see Tyran and Feld (2006) and Kosfeld et al. (2009).

Figure 3: Mean Punishment Patterns – Within-Subject Comparison



Removing a legal system Contrary to the introduction of mild laws, one might also ask how removing an initially existing centralized institution affects decentralized norm enforcement. Are social sanctioning levels going to remain at modest levels or will they increase again? To address this question we had subjects play the reversed order, i.e., those who first faced treatment Law subsequently played treatment Base. The punishment behavior for this second sequence is displayed in the right panel of Figure 3.

In contrast to our previous findings, we observe only minor changes in social sanctions in this treatment order. Subjects who started in treatment Law assign 0.69 points on average, and subsequently 0.53 in treatment Base. The decline is significant at a 5%-level (sign-rank test, $p = 0.0112$, two-sided). To assess this effect, note first that the average punishment in the Law treatment is virtually identical to the one for the alternative treatment order (0.70 and 0.69 for Sequence 1 and 2, respectively). However, moving from Law to Base, decentralized punishment declines even further. Hence, in Sequence 2, social sanctions do not fill the gap which is left after removing the mild law. This suggests that the crowding out effect (observed in the other sequence discussed above) spills over from the first treatment where a legal system is in place into the second treatment Base. Once subjects start from a situation where norm enforcement is partially delegated to a centralized institution, they might be less willing to take over this role again when it is abolished. Gächter et al. (2009) report a similar finding in the context of gift-exchange games: exposing workers to explicit performance incentives significantly reduces voluntary cooperation in subsequent trust-contracts – even though explicit incentives have been

abolished. A comparable effect is also reported in Gneezy and Rustichini's (2000) famous kindergarden experiment. After introducing a monetary fine for late-coming parents, the number of late-coming parents increased significantly. When the fine was removed again, no reduction occurred. Our results neatly complement these findings, showing that – for subjects who experience as a benchmark the case with a legal system – the crowding out of decentralized norm enforcement persists, even when the centralized norm-enforcement institution is removed. Removing the mild law thus resulted in a drop in the overall (legal cum social) level of norm enforcement. The evidence then raises the question to which extent the decline in norm enforcement is also reflected in cooperation levels.

In line with intuition, we indeed observe a decline in cooperation. The average contribution significantly drops after removing the legal sanctioning mechanism, from 15.1 in LAW to 13.1 in BASE (sign-rank test, $p = 0.0178$, two-sided). The resulting decline in the payoff from stage I is compensated by a smaller amount of social sanctions in BASE, such that removing the legal sanctions has practically no effect on the total payoff (the relative change in average payoff is 0.003%). While the between- as well as the within-subject comparison for Sequence 1 documents that centralized institutions significantly increase payoffs, this last finding suggests that removing them might not necessarily be harmful to overall welfare. It is up to future research to test the effects from establishing and then abolishing legal sanctions in a dynamic setup.

Full sample analysis We conclude the section by applying the parametric analysis of the decentralized punishment choice from above (see Table 1) to our full sample, i.e., including the data from the first as well as the second treatment. The regression analysis confirms and extends our previous findings. First, the LAW treatment results in a crowding out of social sanctions. As we pool all our data, the effect is now highly significant. At the same time, however, the direct effect is quantitatively less strong than the one found before. This is mainly driven by the fact that the between-treatment differences for the treatments played second are less pronounced. Second, subject i's punishment of j is stronger, the larger the difference in their contributions is, and the higher the other player's contributions are. Third, there is no interaction effect between the treatment and the contribution difference $c_i - c_j$ (Column 4). Hence, the crowding out is in levels rather than on the margin.

Table 3: Random-effects Tobit regression: Marginal effects on social sanctions

Dependent variable: d_{ij}				
	(1)	(2)	(3)	(4)
Law	0.024	-0.083***	-0.086***	-0.085***
	(0.021)	(0.016)	(0.016)	(0.016)
$c_i - c_j$		0.058***	0.060***	0.059***
		(0.003)	(0.003)	(0.006)
$(c_i - c_j) \times LAW$				0.001
				(0.005)
$c_{k,l}$			0.010***	0.010***
			(0.002)	(0.002)
t	-0.008**	-0.005*	-0.005*	-0.005*
	(0.004)	(0.003)	(0.003)	(0.003)
Log-likelihood	-5633	-4446	-4422	-4422
Observations	5640	5640	5640	5640

Notes: The table displays the marginal effects evaluated at the mean (conditional on being uncensored) from random effects Tobit regressions. The dependent variable is the number of punishment points assigned from i to j, d_{ij}. The sample covers all punishment choices for hypothetical triples in the initial and the subsequent treatment. All other regressors are as described in Table 1. Standard errors are in parentheses. Level of significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4 Concluding discussion

This paper studied the link between centralized and decentralized punishment in a public-goods game. We developed a novel experimental approach to elicit subjects' punishment behavior via the strategy method. This allowed us to identify direct and indirect effects from legal on social sanctions. Centralized and decentralized norm enforcement turned out to be substitutes in our experiment: social sanctions were partially crowded-out by legal sanctions. While individuals imposed less punishment on average, the marginal social *cum* legal sanctions faced by a free-rider were higher in the presence of a 'mild law'. In line with this observation, we found that the legal sanctions triggered a substantial increase in public-good contributions – despite the crowding out of decentralized punishment. Thus, a higher level of cooperation was obtained at lower costs of social sanctioning. Consequently, the implicit delegation of norm enforcement to a formal, centralized institution allowed for a significant increase in overall welfare. The combination of mild laws and social sanctions therefore appears to be a successful institutional arrangement to enforce compliance.

From the perspective of expressive theories of law, our results appear surprising at first sight. One might have conjectured that the introduction of a legal system crowds in social sanctions, as the law shapes player's perceptions about what is socially desirable and undesirable. In the context of our public-goods game, however, it seems that there was little ambiguity about what to consider 'good' and 'bad' conduct. Already in the absence of legal sanctions, subjects' punishment behavior was clearly targeted at free-riders and we observed hardly any 'anti-social punishment'. Hence, there was little scope for the law to serve as a coordination device for punishment. In future research, it would be interesting to see if one finds support for expressive theories of law in more complex environments, where the distinction between pro- and anti-social behavior is less salient.

While our study establishes the first experimental evidence on the interplay of legal and social norm enforcement, there are several avenues for future research. We observed that the exogenous shift from an 'archaic' society (where only decentralized norm enforcement is possible) towards a 'modern' society (with a formalized enforcement institution) had a clear impact on players' punishment and contribution choices. In the future, we would like to assess the importance of the different channels that can shape cooperation, i.e., the immediate effect of legal sanctions as well as the laws effect on subjects' expectations regarding the other players' contributions and sanctions. A promising first step in this agenda would rely on an experimental variation of the 'content' of the law and its formal enforcement (fines, sanction risk, etc.). A further important task is to shed light on the long run effects from the combination of formal and informal norm enforcement. Will cooperation sustain when legal and social sanctions are available? What if players are given the possibility to shape these institutions endogenously? Research along these lines promises to provide valuable insights into the functioning of modern societies and will help to increase our understanding of effective enforcement institution.

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Appendix I: Contribution Triplets

In the following, we list the contribution triples that were used within each combination of c^L, c^M and c^H. Before the experiment, these 10 x 8 triples were randomly generated by sampling with replacement from the corresponding sets c^L, c^M, c^H. Each player then faced a randomly selected triple within each combination 1 – 10. If the selected triple correspond, by chance, to the real triple, the subject would not face this situation, but instead another one of the pre-defined contribution triples for the corresponding combination.

(1) (c^L, c^L, c^L): (0,0,0), (0,2,3), (1,1,3), (1,2,2), (1,2,3), (1,2,4), (1,3,3), (1,3,4)

(2) (c^L, c^L, c^M): (0,1,5), (0,2,8), (0,2,14), (1,2,10), (1,2,12), (1,3,14), (2,2,6), (2,3,12)

(3) (c^L, c^L, c^H): (0,3,18), (1,2,20), (1,3,19), (1,4,20), (2,2,18), (2,2,19), (3,3,18), (4,4,17)

(4) (c^L, c^M, c^M): (0,9,11), (0,5,12), (0,13,14), (1,10,15), (2,6,8), (2,9,11), (2,10,15), (3,13,14)

(5) (c^L, c^M, c^H): (0,6,19), (0,14,17), (2,6,17), (2,8,20), (2,11,19), (3,7,18), (4,8,17), (4,10,20)

(6) (c^L, c^H, c^H): (0,18,19), (1,19,19), (2,18,19), (2,18,20), (2,19,19), (3,18,20), (3,19,19), (4,19,20)

(7) (c^M, c^M, c^M): (5,7,12), (5,14,16), (6,6,9), (6,10,10), (7,8,9), (7,10,13), (7,14,16), (8,9,11)

(8) (c^M, c^M, c^H): (5,5,17), (5,8,18), (6,11,20), (8,15,17), (9,12,18), (9,15,18), (11,15,19), (12,15,19)

(9) (c^M, c^H, c^H): (5,18,20), (7,18,19), (9,18,20), (11,17,17), (12,17,18), (12,18,18), (14,17,20), (15,17,19)

(10) (c^H, c^H, c^H): (17,17,19), (17,18,19), (17,18,20), (17,19,19), (17,19,20), (18,18,19), (18,18,20), (20,20,20)

Appendix II: Instructions

Reported below.

THESE ARE THE TRANSLATIONS OF THE GERMAN INSTRUCTIONS FOR TREATMENT LAW. INSTRUCTIONS IN TREATMENT BASE WERE IDENTICAL EXCEPT FOR THE PARTS CONCERNING THE THIRD STAGE.

General explanations to the participants

You are now participating in an economic experiment. Today's experiment consists of more than one part, the parts being independent from one another. If you read the following explanations carefully, you'll be able to earn a considerable amount of money – depending on your decisions and those of the other participants. Thus, it is important to read these instructions very carefully.

The instructions you received are for your private information only**. It is absolutely prohibited to communicate with the other participants during the experiment.** Should you have any questions, please ask us. If you violate this rule, you will be dismissed from the experiment and forfeit all payments

The experimental payoffs will be calculated in Taler. The total amount of Taler that you have accumulated during the experiment will be converted into Euro and paid to you in cash at the end of the experiment. You will be informed about the exchange rate from Taler to Euro at the beginning of each part of the experiment.

The experiment is divided into periods. In each period, participants are divided into groups of four. You will therefore be in a group with 3 other participants. In each period, the composition of the groups will change. Therefore, participants cannot be identified across periods.

Each period consists of **3 stages**. On stage 1, you have to decide how many Taler to contribute to a project. On stage 2, you learn about the others' contributions and then have to decide, if, resp. by how much, you want to reduce the others' income from stage 1. On stage 3, a random device decides if a participant's income is reduced further.

Detailed information about a period

The first stage:

At the beginning of each period each participant receives **20 Taler**. We call this his or her endowment. Your task is to decide how to use your endowment. You have to decide how many of the 20 tokens you want to contribute to a **project** and how many of them to put on your **private account**.

Income from your private account:
For each Taler you put on your private account, you earn one Taler. For example, if you put 20 Taler on your private account (thus contributing zero Taler to the project), you would earn 20 Taler from your private account. If you, e.g., would contribute 12 Taler to the project (thus putting 8 Taler on your private account), your income from the private account would be 8 Taler. Nobody but you receives Taler from your private account.

Income from the project:
For each Taler contributed to the project, you (and each other participant in your group) earn 0.4 Taler. Each participant's income from the project is thus given by:

Income from the project = Sum of contributions to the project x 0.4

Example: If the sum of contributions would equal 20 Taler (e.g., if you and the other three participants in your group would contribute 5 Taler each), you and everyone else in your group would receive an income of 20 x 0.4 = 8 Taler from the project. If the sum of contributions would equal 10 Taler, you and everyone else in your group would earn 10 x 0.4 = 4 Taler from the project.

Income at the end of stage 1:

Your income at the end of stage 1 consists of your income from your private account and the income from the project:

Income from the private account (= 20 – your contribution)
+ Income from the project (= 0.4 × sum of contributions to the project)
Income at the end of stage 1

Let us illustrate how to calculate your income at the end of stage 1 using an example:
You and the other participants in your group contribute 15 Taler each. The sum of contributions thus equals 15+15+15+15=60 Taler. Your income at the end of stage 1 would then be given by:
5 Taler from your private account + 0.4 x 60 Taler from the project = 5 + 24 = 29 Taler

If you would have contributed, e.g., 0 Taler instead, the sum of contributions would equal 15+15+15+0=45 Taler. Thus, your income at the end of stage 1 would then be given by:
20 Taler from your private account + 0.4 x 45 Taler from the project = 20 + 18 = 38 Taler

The others' incomes at the end of stage 1 are calculated accordingly.

The second stage:

At the beginning of the second stage, you are informed about the contribution decisions of the other participants in your group. You can then **reduce or leave constant** the income from stage 1 of **each** participant in your group. The other participants can also reduce **your** income if they wish to.

In order to reduce the income of a participant, you have to assign **points** to him. Each point assigned reduces the other's income by 3 Taler. If you assign 1 point, you reduce his or her income by 3 Taler. If you assign 2 points, you reduce his or her income by 6 Taler etc. If you do not want to change the other's income, you assign 0 points.

The more points you assign to a participant, the higher is his or her income reduction. At the same time, however, each point that you assign reduces your own income as well. Each point assigned reduces your own income by 1 Taler. For instance, if you assign 2 points, you reduce your own income by 2 Taler. If you assign 4 points, you reduce your own income by 4 Taler; if you assign 0 points, you do not incur any costs.

You have to decide for each participant in your group how many points you want to assign to him or her. You can assign up to 10 points per participant.

By how much a participant's income is reduced **in total** depends on the sum of received points. If somebody, for instance, receives 1, 0 and 2 point from the others, his or her income will be reduced by (1+0+2) x 3 = 9 Taler. At the same time, the others have to bear the cost for assigning points of 1, 0, resp. 2 Taler.

The third stage:

On stage 3, a random device decides if a participant's income is **reduced further**. Each participant is selected with a probability of 12.5%. In each group, one participant is selected at most. (A probability of 12.5% can be illustrated as follows: You throw an 8-sided dice. Your income will be further reduced if the dice shows a 1.)

If you are the one who is randomly selected, your income will be reduced by 1.2 times the amount you have put on your private account on stage 1. The same holds true for the other participants.

For instance, if you contributed 5 Taler on stage 1, thus putting 15 Taler on your private account, your income might probably be reduced by
1.2 x 15 Taler = 18 Taler

For instance, if you contributed 19 Taler on stage 1, thus putting 1 Taler on your private account, your income might probably be reduced by
1.2 x 1 Taler = 1.20 Taler

If you contributed 20 Taler on stage 1, your income will not be reduced at all on stage 3.

Consider that the events on stage 3 do not depend on the decisions made on stage 2. That is, the probability that one's income is further reduced is not affected by the number of points assigned or received on stage 2. Furthermore, consider that at the time you are assigning points on stage 2, you do not know whether the income of a participant will be reduced further. Yet, you know the probability with which this is going to happen (namely, 12.5%).

Your Period Payoff:

Your payoff in each period is thus given by:

Income from stage 1
− *Reductions for points received on stage 2*
− *Costs for points assigned to others on stage 2*
− *reductions on stage 3 if you are randomly selected*
Period Payoff

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