Preprints of the
Max Planck Institute for
Research on Collective Goods
Bonn 2010/06

Turning the Lab into Jeremy Bentham's Panopticon
The Effect of Punishment on Offenders and Non-Offenders

Christoph Engel
Bernd Irlenbusch

MAX PLANCK SOCIETY

Preprints of the
Max Planck Institute
for Research on Collective Goods Bonn 2010/06

Turning the Lab into Jeremy Bentham's Panopticon The Effect of Punishment on Offenders and Non-Offenders

Christoph Engel / Bernd Irlenbusch

February 2010

Max Planck Institute for Research on Collective Goods, Kurt-Schumacher-Str. 10, D-53113 Bonn
http://www.coll.mpg.de

Turning the Lab into Jeremy Bentham's Panopticon
The Effect of Punishment on Offenders and Non-Offenders

Christoph Engel[*]
Bernd Irlenbusch[†]

Abstract

The most famous element in Bentham's theory of punishment, the Panopticon Prison, expresses his view of the two purposes of punishment, deterrence and special prevention. We investigate Bentham's intuition in a public goods lab experiment by manipulating how much information on punishment experienced by others is available to would-be offenders. Compared with the tone that Jeremy Bentham set, our results are non-expected: If would-be offenders learn about contributions and punishment of others at the individual level, they contribute much less to the public project. Our results confirm the special prevention effect but show that the deterrence effect is smaller the more information on individual punishment is available.

Keywords: Punishment, Deterrence, Special Prevention, Jeremy Bentham, Experiment, Public Good

JEL: C91, H41, K14, K42

Research assistance by Karsten Lorenz and Lilia Zhurakhovska and helpful comments by Markus Englerth and Andreas Nicklisch are gratefully acknowledged.

* Corresponding author: Christoph Engel, Max Planck Institute for Research on Collective Goods, D 53113 Bonn, Kurt-Schumacher-Straße 10, engel@coll.mpg.de

† London School of Economics and Max Planck Institute for Research on Collective Goods, Bonn

I. Research Question

Not many scientific achievements are cast in stone. Jeremy Bentham's theory of punishment is among the rare exceptions. Inspired by his younger brother Samuel, a naval architect based in Russia in the 1780s, he translated his theory into a blueprint for the design of prisons (Semple 1993). Over time, all over the world some 20 "panopticon" prisons have been built[1]. The basic idea is simple. The circular construction places each and every inmate under permanent control by the supervisor, located at the centre. Every spectator cannot but realize that prisoners have fully lost their autonomy.

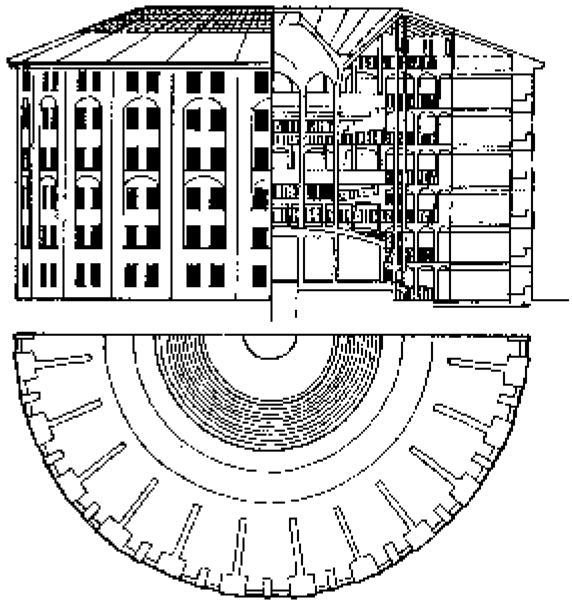


Figure 1
Jeremy Bentham's Design of the Panopticon

The architecture implements the purpose punishment is supposed to serve (Bentham 1830:Book V Chapter III):

> "Hence the prevention of offenses divides itself into two branches: Particular prevention, which applies to the delinquent himself; and general prevention, which is applicable to all the members of the community without exception.
>
> General prevention is effected by the denunciation of punishment, and by its application, which, according to the common expression, serves for an example. The punishment suffered by the offender presents to every one an example of what he himself will have to suffer if he is guilty of the same offense.
>
> General prevention ought to be the chief end of punishment, as it is its real justification. [...] The punishment inflicted on the individual becomes a source of security to all. [...] That punishment, which, considered in itself, appeared base and repugnant to all generous sentiments, is elevated to the first rank of benefits, when it is regarded

[1] University College London, Bentham Project and http://en.wikipedia.org/wiki/Panopticon.

> not as an act of wrath or of vengeance against a guilty or unfortunate individual who has given way to mischievous inclinations, but as an indispensable sacrifice to the common safety" (Bentham 1830:Book I Chapter III).

In this paper we investigate Bentham's intuition of special prevention and general prevention (deterrence) in a lab experiment. A lab experiment has the decisive advantage to give us full control over the institutional setting. We can therefore manipulate how much information on punishment experienced by others is available. Additionally, we can unambiguously see whether and to which degree subjects are well-behaved, and whether or not they change their behavior after having observed or experienced punishment. Our "work-horse" is a standard public goods game, that has already been extensively investigated (Ostrom, Walker et al. 1992; Fehr and Gächter 2000; Fischbacher and Gächter 2009). Four players interact over 10 periods. In each period they can contribute to a public good. The individual return of each player from one unit of contribution, i.e. the marginal capita return, is such that from an individual perspective it is unprofitable to contribute. However, since all players benefit, full contribution would maximize total profit. In a second stage of each round, a fifth player observes the individual contributions and, based on this information, can punish each of the four players who can make contributions. Punishing is costly for the fifth player, i.e., she must invest her own money if she decides to diminish the payoff of one or more of the others. All five players individually have the same return from the public good irrespective of whether or not and how much they contributed.

To investigate Jeremy Bentham's idea, we manipulate feedback. In the *low* treatment, contributors only learn aggregate contributions. In the *medium* treatment, they also learn aggregate punishment. In the *high* treatment, they know individual contributions and individual punishment. As a further test, we invite another group of four contributors for another 10 periods. The group supervisor stays in office. Before the second group starts playing, graphs inform them about their predecessor's performance. The information about the contributions and, if applicable, about punishment in the first 10 periods is the same as was given to contributors during the first phase. Also the degree of feedback is kept constant across phases. For instance if feedback was *low* in phase 1, the group of successors is not informed about punishment either, neither with respect to their predecessors, nor with respect to other players of the current group.

Compared with the tone that Jeremy Bentham set, we have a non-expected result: punishment information is at best immaterial. If bystanders learn both behavior and correctional responses at the individual level, they contribute much less to the public project. However, punishment does not miss its intended effect, neither on those punished themselves, nor on bystanders. Yet the effect is indirect. Bystanders and newly arrived group members are mainly influenced by the observed previous contribution levels in their groups that, of course, have been affected by punishment.

II. Related Literature

Behavior in our work-horse setting exhibits a robust and well-known pattern. In the absence of punishment, players contribute significantly in the beginning, but contributions decay quickly (for summaries see Ledyard 1995; Zelmer 2003). More recent studies show that contributions stabilize if subjects are given a chance to punish each other after having observed individual contributions (for a summary see Herrmann, Thöni et al. 2008). In this literature, punishment is typically decentralized. Few experimental studies use centralized punishment, but they focus on different research questions. For example, they investigate whether unaffected outsiders are willing to spend money for disciplining others (Fehr and Fischbacher 2004), how leaders can motivate members of their team by the threat of punishment (Güth, Levati et al. 2007; Gürerk, Irlenbusch et al. 2009), whether centralized punishment develops endogenously when players can voluntarily join a sanctioning institution (Kosfeld, Okada et al. 2008), how centralized punishment affects behavior in a threshold public goods game (Guillén, Schwieren et al. 2006), and how mild central sanctions interact with social norms (Tyran and Feld 2006; Galbiati and Vertova 2008b; Galbiati and Vertova 2008a) Putterman, Tyran, and Kamei (2010) are able to choose efficient formal sanctions by voting.

Very few experimental studies investigate the impact of information about others' behavior (also called "social history") on own behavior. (Berg, Dickhaut et al. 1995) find that providing a social history increases cooperation in their trust game setting. (Fehr and Rockenbach 2003), however, do not find a change in subjects' behavior in a gift exchange game with punishment. Informing responders about the average offers before they decide whether to accept or reject their specific offer seems to significantly increase offers and offer-specific rejection probabilities (Bohnet and Zeckhauser 2004). In a binary dictator game (Krupka and Weber 2009) find that showing subjects what others actually do produces more pro-social behavior. Interestingly, this is even the case when observed subjects are mostly selfish. They also find support for an informational effect: observing more people behaving pro-socially generally produces more pro-social behavior. We are not aware of any study that investigates the influence of information about others' behavior in a public good setting with punishment.

There is a growing body of experiments in criminology (for summaries see Farrington 2003; Nagin and Pogarsky 2003; Petrosino, Turpin-Petrosino et al. 2003; Farrington and Welsh 2005; Farrington and Welsh 2006; Petrosino, Kiff et al. 2006). Many are quasi experiments in the field (Farrington and Welsh 2006). Apparently though no experiment has tried to assess the effect of punishment on true outsiders to the criminal system (cf. the comprehensive survey by Farrington and Welsh 2006).

In modern parlance, Jeremy Bentham proposes deterrence. The deterrence literature is rich (for summaries see Cameron 1988; Ehrlich 1996; Nagin 1998b; Nagin and Pogarsky 2001; Sherman 2002; Levitt 2004; Matsueda, Kreager et al. 2006). There is widespread consensus that punishment has an effect on non-criminals (Andenaes 1974; Gibbs 1979; Katyal 1997:2444; Frase 2005:69). Specifically the social benefit from punishment is not confined to incapacitation, i.e. to keeping would-be criminals behind prison doors (Blumstein, Cohen et al. 1978; Levitt 1998; Nagin 1998a; Kessler and Levitt 1999). Most empirical researchers find that increasing the certainty and the celerity of punishment reduces crime rates more strongly than greater severity (Cramton 1968; Gibbs 1968; Glass 1968; Schwartz 1968; Tittle 1969; Bean and Cushing 1971; Chiricos and Waldo 1971; Logan 1972; Ehrlich 1973; Erickson and Gibbs 1973; Silberman 1976; Witte 1980; Sherman and Berk 1984; Paternoster 1989; Trumbull 1989; Grogger 1991; Pogarsky 2002; Robinson and Darley 2004; Tonry 2006).

Much empirical effort has been devoted to showing that and how the objective certainty, celerity and severity of sanctions is modulated by perception (Williams and Hawkins 1986; Paternoster 1987; Nagin 1998b; Anderson 2002; Piquero and Pogarsky 2002; Pogarsky, Piquero et al. 2004; Wright, Caspi et al. 2004; Matsueda, Kreager et al. 2006). The difference can be pronounced. For instance, one interview study with prison inmates showed that 76% of active criminals, and 89% of most violent criminals, either perceive no risk of apprehension or are incognizant of the likely punishment (Anderson 2002:295).

One particularly troubling finding has been called the resetting effect. Empirically, those who have been punished in the past not so rarely increase rather than decrease criminal behavior (Piquero and Paternoster 1998; Piquero and Pogarsky 2002; Pogarsky and Piquero 2003). A prominent explanation for this observation is Bayesian updating. Through the one experience with the judicial system, offenders learn that the probability of arrest, conviction and enforcement is smaller than expected, and that they suffer less from the sanction than they thought (Paternoster, Saltzmann et al. 1983; Nagin 1998b:15 f.). If this experience spreads, the collateral damage of criminal justice is even larger. Also criminals cannot be stigmatized twice, which is why the opportunity cost of the second sanction is lower (Harel and Klement 2007). Through stigma, the prospect for earnings from legal occupation have decreased (Schmidt and Witte 1984).

III. Hypotheses

Our first hypothesis translates Jeremy Bentham's proposal into our setting:

H_1: The more punishment is visible, the higher are contributions.

From the more recent literature in criminology, we derive

H_2: The more punishment is severe, and the more it is consistent, the higher are contributions.

Jeremy Bentham's proposal is rooted in his conviction that would-be criminals are also sensitive to foreign experiences with the criminal system. This triggers

H_3: Those not punished in the past contribute at least as much to the public project as those punished. More detailed information about contributions and punishment of predecessors has a favorable effect.

IV. Experimental Design

324 students (149 female) from a variety of majors participated in an experiment conducted at the Econ Lab of Cologne University in December 2008. The experiment was implemented in zTree (Fischbacher 2007). Participants were invited using Orsee (Greiner 2003) and were randomly assigned to treatments.[2]

Participants played ex ante announced 10 rounds of a standard public goods game in anonymous groups of four (called "players of type A" in the instructions) that stayed together during the entire experiment. Per period, each participant received an endowment of 20 talers. Players of type A could decide how many talers to invest in a project. Each taler contributed to the project created a marginal per capita return of 0.4. Before the start of the game, per group one additional subject was randomly assigned as the group supervisor whom we called "player of type B" in the instructions. In each round supervisors were informed about the individual contributions of each type A-player. Supervisors had the same endowment and the same marginal per capita return from the project. However, they could not contribute. Rather they could spend their endowment on individually punishing the type A-players. For type B players, contributions of type A players thus are the equivalent of a levy from which a public official is financed. Following Fehr and Gächter (2002), the punishment technology was linear: one taler invested for punishment destroyed three talers of the punished player.

Our experiment consisted of two phases. The first group of four type A players in phase 1 was followed by a second phase with a fresh group of four subjects. Also the second phase consisted of ex ante announced 10 rounds. Only the supervisor stayed the same in both phases. Before starting to play themselves, the second group received graphs informing them about the performance and/or received punishment in their respective group of predecessors.

Our three treatments differed in the degree of feedback for type A-players. An overview of the differences in feedback is provided in Table 1. In each phase feedback is provided about previous rounds, and in phase 2 also about play of the respective group in phase 1. In treatments *low* and *medium*, the supervisor cannot identify players of type A across periods. By contrast, in treatment *high*, players of type A are identified by numbers that stay constant over all rounds.

2 The translation of the instructions for one of our treatments are provided in the appendix. Original instructions were in German. All instructions can be obtained on request from the authors.

Treatment	feedback about previous rounds	feedback about all rounds in previous phase (in phase 2 only)
low	• average contributions • own received punishment	• average contributions
medium (in addition to information provided in *low*)	• average received punishment	• average received punishment
high (in addition to information provided in *medium*)	• individual contributions • individual earnings • individual received punishment	• individual contributions • individual earnings • individual received punishment

Table 1

Feedback Provided to Type A-Players in Different Treatments

Of course, in a number of respects, our design differs from the reality of criminal policy. Stakes are much lower, both for society and for the "offender". But we keep the basic dilemma structure that also underlies most criminal offenses: the criminal is best off if she ignores the harm she inflicts on other members of society. Also there is no criminal code. Norms are implicit in our setting. They result from the sanctioning policy of the supervisor. Yet in criminal policy, a related effect is not uncommon: the criminal authorities use the degrees of freedom they dispose of to flexibly react to crime, despite the fact that, at face value, criminal offenses are precisely defined in the respective penal code. Finally, in our experiment, sanctioning is not accompanied by words that express social disapproval or moral indignation. In the experiment, all value judgement is through correctional action. The effect of the negative incentive is less visibly backed up by appealing to the offender's identity as a member of this one society.

Yet in recompense for the slight reduction of external validity, our design gives us control over elements that have plagued criminal policy and empirical research alike. Our supervisor benefits directly from the contributions of group members to the project. They cannot personally identify group members so that repeated game effects after the experiment are not an issue. Effects of retribution can be excluded (cf. Wood 2002). Incapacitation is impossible in our setting (cf. Kessler and Levitt 1999). Intervention cannot shift criminal activity to another location (cf. Hakim, Spiegel et al. 1984). Victims cannot respond by moving to a different town or quarter (cf. Anderson 1990). Players are perfectly symmetric, so that bystanders have no reason to expect that they will be treated any differently if they behave the same way (cf. Robinson and Darley 2003:973). Since each round only lasts minutes, arguably discounting should be negligible (cf. Levitt 1998:353).

Given our completely neutral framing and decontextualisation and the fact that there cannot be competing tasks, we need not be too much concerned about impulsivity (cf. Shepherd 2004) or about crime as symbol (cf. Matsueda, Kreager et al. 2006:103). The risk of sanction misperception (Nagin 1998b:19) is minimized. Subjects are perfectly informed about punishment inflicted on themselves and, depending on our treatments, on others. Arguably, habituation does not matter, given that players only play 10 rounds, and that the entire experiment lasts little longer than an hour (cf. Hawkins 1969:560). Again due to anonymity, the fact that would-be offenders are members of a peer group with criminal propensity cannot explain behavior (cf. Kahan 1997a:2486). Punishment cannot serve as a "badge of honor" (cf. Wilson and Herrnstein 1985:304). Moral credibility (cf. Kahan 1997a:2481) and the mirror concept of moral condemnation (cf. Kahan 1997b:383) cannot matter either. We deliberately do not speak of "punishment", but only of a "reduction of income". The supervisor is neutrally labeled as a "player of type B". Thereby we reduce the expressive function of punishment (cf. Kahan 1997a:2483) to a minimum. Since we guarantee anonymity, formal sanctions cannot be supplemented or complemented by informal sanctions in treatments *low* and *medium* (cf. Cameron 1988:302). In treatment *high*, free-riders are labeled (cf. Lemert 1951; Becker 1963). But other group members do not have an opportunity for a targeted reaction.

Our treatments discriminate between several explanations for the effect of punishment. The treatments are directly targeted to sanction risk perception (cf. Nagin 1998b:1, criticizing the absence of empirical evidence on this). In treatment *low*, subjects do not get feedback about sanctions directed to other group members. Consequently participants not punished themselves in the respective period (whom, as a shorthand, we call loyals) can at best be indirectly influenced by the sanctioning policy of their respective supervisor, through its effect on contributions. This is different in treatment *medium*. However, loyals still do not learn the precise response of the supervisor to a specific deviation from the (implicit) group norm. This information is only available in treatment *high*.

We can precisely disentangle personal and vicarious experiences (cf. Stafford and Warr 1993). In treatment *low*, vicarious experiences are excluded by design. In treatments *medium* and *high*, in the first round of phase 2, all experiences are vicarious. In all other rounds of both phases, culprits combine personal with vicarious experiences, whereas loyals make differently precise vicarious experiences only. This makes it also possible to test for the resetting effect (cf. Pogarsky and Piquero 2003).

In two complementary ways, we can analyze the effect of ambiguity and uncertainty (cf. Nagin 1998b:11). In treatment *low*, loyals are completely uncertain about the sanctioning policy of their supervisor. In treatments *medium* and *high*, in phase 2 players can adjust their behavior to the sanctioning policy from phase 1. Also in treatments *medium* and *high*, we can test how the consistency of the respective supervisor (cf. Katyal 1997:2451) plays itself out in contribution rates. If there is stigmatization (Harel and Klement 2007), this should become visible in comparing treatments *medium* and *high*. For only in treatment *high*, culprits are individualized with a reputation over rounds.

A final advantage of our design results from the character of our dependent variable. In the field, and in criminological research on recidivism for that matter, it usually is binary: either a person abides by the law, or she breaks it. In our experiment, contributions are (quasi) continuous, on a scale from 0 to 20. We therefore cannot only investigate whether those who have been punished, or those who observe how others are punished, respect the law in the future. We can also measure *how much* they contribute to the public project. Along with this, we get a meaningful dynamic measure. We can investigate, by how much a participant *changes* her contribution level after punishment, or upon observing punishment in action.

Participants received a show up fee of 2.50 €. Theoretically, subjects could make real losses. Since the lab has built a reputation that subjects do not put their own money at risk, we gave them an extra 50 talers at the beginning of the experiment, explicitly motivated to cater for potential losses. Earnings were individually and anonymously paid out to all participants at an exchange rate of 0.04 € per taler. On average, total earnings of contributors were 22.23 € (sd 2.64, range [12.07, 33.3]). Total earnings of supervisors, who played 20 periods each, were on average 45.60 € (standard deviation 5.51, range [34.36, 56.71]).

V. Results

1. Anticipation

In the first period of the first phase, active players have no own or vicarious experiences with the respective punishment institution. But they are fully informed about institutional design. Is this information sufficient to induce different behavior in different treatments? Is a potential effect of the institution anticipated? Although descriptive figures point into the direction of the treatment effects[3], if we only consider data from the first period we do not find a significant effect of the treatments, neither if we test non-parametrically[4] nor if we test parametrically.[5]

2. Phase 1

a. *Full Transparency Reduces Contributions*

Contributions and deductions through punishment were as in Figure 2 and in Table 2. In the Appendix, we also provide graphs for individual groups. Our main result is visible immediately: full transparency hurts, while partial transparency is immaterial. In treatment *high*, absolute contributions are lower than in the remaining two treatments. The difference in absolute contributions results from less favorable contribution dynamics. While contributions rise quickly in treatments *low* and *medium*, they remain almost stable in treatment *high*. Visual inspection suggests that

3 Mean contributions are *low* 14.521, *medium* 14.563, *high* 13.208.

4 Mann Whitney, *low* vs. *medium*, N = 96, p = .932; *low* vs. *high*, p = .2955; *medium* vs. *high*, p = .2514. All tests are two-sided. Note that in the first period individual contribution decisions are still fully independent of each other.

5 In the first period, 61 out of 144 (active) participants contribute their entire endowment of 20 taler. 4 keep the whole endowment for themselves. This data structure makes a Tobit model appropriate. In this model, treatment *high* is the reference category. Regressors for treatments *low* (p = .229) and *medium* (p = .184) are independently and jointly insignificant (Wald test, F(2, 142) = 1.09, p = .3376).

informing participants about average punishment, i.e. the difference between treatments *low* and *medium*, is close to irrelevant.

Phase	first		second	
treatment	contr	recpun	contr	recpun
Low	17.45	1.7	18.07	1.65
Medium	16.99	2.68	18.18	1.72
high	14.02	2.84	15	1.85

Table 2
Means of Contributions and Received Punishment

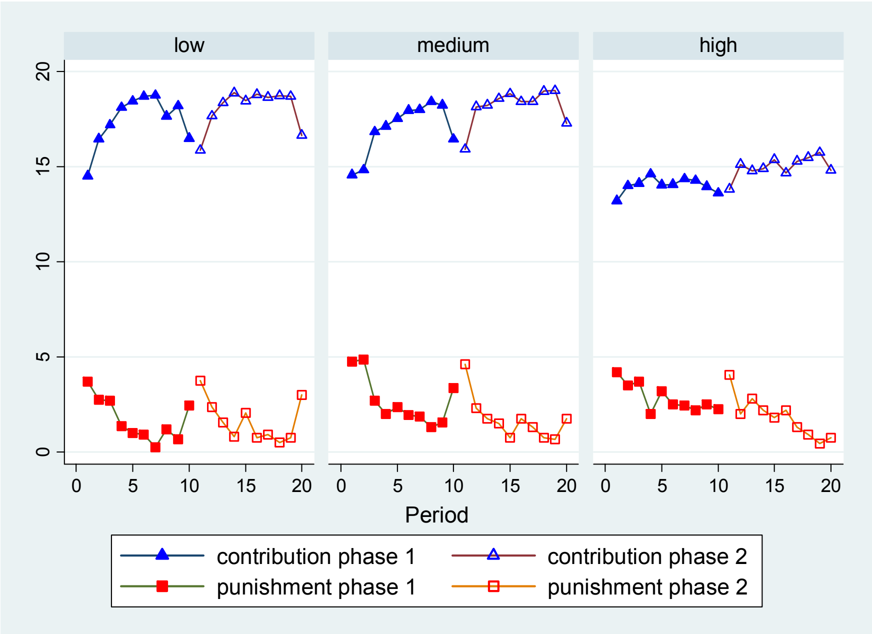


Figure 2
Summary Results

For this and all later non-parametric tests, we consider means over the first ten rounds. We report p-values from two-sided tests. If we compare treatments, we use a Mann-Whitney test. In this test, the difference between *low* and *high* is indeed weakly significant (N = 24, p = .0647), while the remaining comparisons are insignificant. In the Appendix, we explain our strategy for parametric estimation. Using this strategy, in the regression of Table 3 we establish significantly higher contributions in the *low* and *medium* treatments, compared to treatment *high*, which serves as the reference category; the endgame effect is captured by the regressor for the final period.[6] Full feedback (*high*) thus reduces contributions. We squarely refute **H$_1$**: contributions are *lower*, the more punishment information is available.

[6] On the mirror model that ignores censoring and clustering, the Hausman test is insignificant, which indicates that we are justified to prefer the more efficient random effects over the fixed effects model.

low	8.185**
medium	6.749*
final period	-1.283
cons	16.847***
N	1440
left censored	52
right censored	784
p model	<.001

Table 3

Parametric Test of Treatment Effects

depvar: contribution

random effects Tobit, bootstrapped at the group level, 500 reps

*** p < .001, ** p < .01, * p < .05

Making average punishment explicit (*medium*) does not significantly improve contributions, as shown by a Wald test of the null hypothesis that coefficients for *low* and *medium* are the same (p = .6096).

b. Impression Management is Key

As the models of Table 4 show, contributions in the next periods are chiefly explained by contributions in the respective previous periods. In both models, apart from the regressor for the end-game effect, this is the only regressor that is significant at conventional levels. Actually it is highly significant.[7] Those who have not been punished in the previous period contribute more, but this effect is only weakly significant, model 2. This is not just a sign of stickiness or conservatism. As has been shown, in motivational terms groups tend to be heterogeneous. There is a small fraction of true altruists, and a somewhat larger fraction of profit maximizing egoists. Yet most group members are conditional cooperators. They are happy to contribute (much) to the public project, as long as they perceive other group members to contribute (much) as well (Fischbacher, Gächter et al. 2001; Fischbacher and Gächter 2009). Conditional cooperators need not be deterred. But they care about the risk of being "the sucker". Hence they care about a signal of expected cooperativeness in their group. As the regressions of Table 4 demonstrate, average contributions in the previous round serve as this signal.

[7] For both models, if one conducts the Hausman test on the mirror model that ignores censoring and clustering, it is significant. The problem can, however, be remedied by a Hausman Taylor model, see Appendix for methodology. In this model, we treat regressor "final" as exogenous.

	model 1	model 2
not punished in previous period		.915$^+$
Average contributions in previous round	1.502***	1.475***
Low	5.224	5.365
Lavcontr*low	-.140	-.153
Medium	1.540	1.798
Lavcontr*medium	.041	.029
final period	-3.155***	-3.241***
Cons	-4.272*	-4.515*
N	1296	1296
p model	<.001	<.001

Table 4
Effect of Observed Cooperativeness
depvar: contribution
Lavcontr: average contribution in previous period
random effects Tobit, bootstrapped at the group level, 500 reps
*** p < .001, ** p < .01, * p < .05, $^+$ p < .1

c. Starting a Virtuous Cycle

Supervisors in our experiment seem to try to establish a norm of high cooperation right from the beginning. Inspecting Figure 2, one realizes that contributions in treatments *low* and *medium* on the one hand and treatment *high* on the other hand differ most in the beginning. In all treatments, there is quite some punishment in early rounds. The amount of punishment in period 1 does not significantly differ between treatments, neither in a nonparametric (Mann Whitney, N = 24, *low* vs. *high*, p = .8165; *low* vs. *medium*, p = .6215; *medium* vs. *high*, p = .8391) nor in a parametric test.[8] However, the substantial intervention works well in *low* and *medium*, while it is only mildly effective in *high*. Actually, in *medium*, active players do not immediately adjust to the implicit group norm. It takes one more period until contributions jump up. Yet if one compares the difference between period 1 and period 3 contributions, at the group level one establishes a significant difference between *low* and *high* (Mann Whitney, N = 24, p = .0119) and a weakly significant difference between *medium* vs. *high* (p = .0937).

d. The Indirect Effect of Punishment

Why does punishment seem to work better if individual contributions and punishment are less visible? Let us first show that punishment works. If it works, it induces punishees to increase their contributions. We therefore work with first differences of contributions as the dependent variable. In the regression of Table 5, the dummy for not having been punished in the previous

8 If one regresses average punishment per group on treatment dummies, the dummies are insignificant, *low* p = .723, *medium* p = .737.

period is highly significant (z = -5.69) and strongly negative.[9] This implies that those who have been punished are much more likely to increase their contributions than those who have not.

However, both in *low* and in *medium*, the treatment effect and its interaction with not having been punished in the previous period more or less cancel out. Since the negative main effect of not having been punished is bigger than the positive constant, in all three treatments the model predicts that those not punished slightly reduce their contributions (-.439 in *low*, -.625 in *medium*, -.530 in *high*). Wald tests show that, for those not punished in the previous period, there is no difference between treatments (*low* vs. *medium*, p = .5790; *low* vs. *high*, p = .8031; *medium* vs. *high*, p = .8110).[10] Consequently, in a dynamic perspective, the difference between treatments does not originate in the reaction of those who have not been punished.

The critical difference is the reaction of those who have received punishment. In all treatments they increase their contributions. However, the increase is the bigger the less punishment is transparent (4.184 in *low*, 2.982 in *medium*, 1.740 in *high*); the treatment effect adds to the constant. The more punishment is transparent, *the less* it works on those to whom it has been addressed.

not punished in previous period	-2.271***
Low	2.244**
low*Linnoc	-2.353*
Medium	1.242*
medium*Linnoc	-1.337*
final period	-1.217**
Cons	1.740***
N	1296
p model	<.001
R^2 within	.1959
R^2 between	.1164
R^2 overall	.1562

Table 5
Explaining Changes in Contributions
Random effects, robust standard errors, clustered for groups
Linnoc: not punished in previous period
*** p < .001, ** p < .01, * p < .05

To appreciate this finding, it is informative to separately consider those who have and who have not been punished in the previous period, which we do in Figure 3 for the critical first three periods. In all treatments, those who have been punished are more likely to increase their contribu-

9 Data on first differences is not censored, so that there is no need for Tobit. The Hausman test on the mirror model that ignores clustering turns out significant. But the problem can be remedied by a Hausman Taylor model, again treating regressor "final" as exogenous..

10 To illustrate methodology: for the first comparison, we test the nul hypothesis (linnoc +) medium + medium*linnoc = (linnoc +) low + low*linnoc.

tions than those who have not (Mann Whitney, N = 68, p = .0029).[11] Most of those not punished do not change their contribution level at all. If they had made substantial contributions in the first round, this is good news. If, however, first-round contributions were low, it is bad policy for the supervisor not to intervene. Most importantly, it is very rare in *low* for a punished participant to reduce her contributions in response. This is more frequent in *medium*, and much more frequent in *high*.[12] In criminological parlance: the higher punishment transparency, the more pronounced the resetting effect.

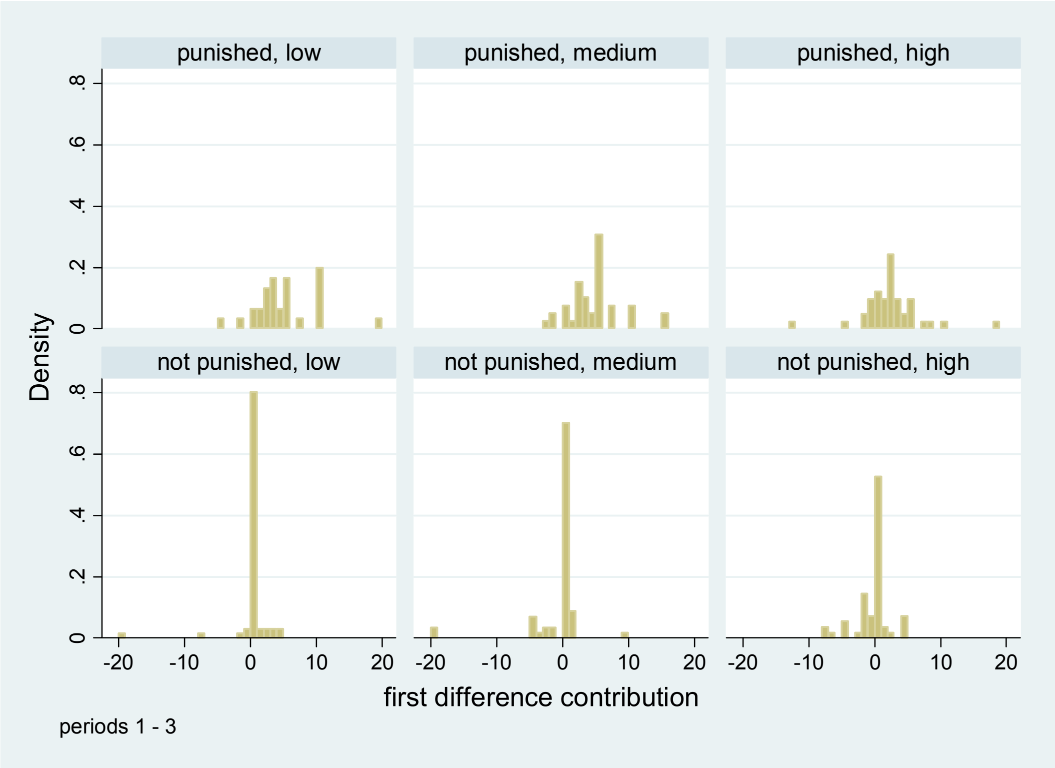


Figure 3

Initial Dynamics

depvar: contr[2] – contr[1], contr[3] – contr[2]

indepvar: dummy that is 1 if the participant has been punished in period 1 (for the first change) and in period 2 (for the second change)

e. *Why Does Transparency Make Punishment Less Effective?*

We therefore must look out for reasons why higher transparency might make punishment less effective. A first hint is provided by the regression in Table 6.[13] The more a participant has been punished in the previous period the less she contributes in the following period. Since we have

11 Two datapoints per group, one is the average of those who have not been punished, the other is the average of those who have been punished. Punishment is considered for periods 1 and 2, the dependent variable for periods 2 and 3. N < 72 results from the fact that in 3 groups nobody was punished, and in 1 group all were punished.

12 The difference between *low* and *high* is significant, Mann Whitney, N = 22, p = .0269; the difference between *medium* and *high* is weakly significant, N = 23, p = .0647.

13 Once more, on the mirror model that ignores censoring and clustering, the Hausman test turns out significant, but the problem can be remedied by a Hausman Taylor model.

already shown that punishment has a positive effect on changes of contributions (Table 5), we can conclude that the reaction to punishment is imperfect. Punishment induces participants to adjust their behavior. But they do not immediately realign with the implicit group norm.

In treatment *high*, this is patent. All participants know how much all other individual participants have contributed in each period, and by how much they have been punished in response. In treatment *medium*, participants only have a noisy signal of the sensitivity to punishment. In treatment *low*, they only see two things: how they have been punished themselves, and the effect of total punishment on contributions. Interestingly, since we have shown that contribution dynamics do not differ across treatments for those who have not been punished (Table 5), the differences in information across treatments are not relevant for these participants. Primarily, they seem to orient themselves to the contribution level in the previous period (Table 4). By contrast, those who have been punished are sensitive to this information (Table 5).

received punishment points in previous period	-.849*
Low	8.419**
Lrecpun*low	.561
medium	7.143*
Lrecpun*medium	.018
final period	-2.097*
Cons	17.941***
N	1296
left censored	48
right censored	723
p model	<.001

Table 6
Effect of Received Punishment on Absolute Contributions
depvar: contribution
Lrecpun: received punishment points in previous period
random effects Tobit, bootstrapped at the group level, 500 reps
*** $p < .001$, ** $p < .01$, * $p < .05$, $^{+}$ $p < .1$

The critical question is this: on which pieces of information do participants condition their behavior – in particular those who have been punished in the previous period? Since treatments mainly differ with respect to contribution dynamics, in the following we focus on this dependent variable.

In all treatments, two pieces of information are available. The participant recalls how severely and how consistently she has been punished herself. We measure severity the following way: for each participant and period, we calculate the ratio of received punishment relative to the tokens not invested:

$$indpunrat = \frac{recpun}{20 - contr}$$

Our first independent variable is the average of this measure for all past periods, per individual. The second independent variable measures the consistency of punishment as the standard deviation of the severity variable, again for all past periods and separately per individual. We also interact these two variables with received punishment in the previous period. The main effect for past severity is insignificant, but there is a highly significant interaction with received punishment. The more punishment has been severe in the past, the less punishment now is effective (models 1 and 3).[14] There is also a weakly significant effect of past punishment consistency, in the expected direction: the more punishment has been inconsistent in the past, the less participants increase their contributions now (model 2). Interestingly, if one adds past severity and its interaction with punishment in the previous period, the main effect of past inconsistency is no longer significant. We then, however, establish a significant interaction with received punishment in the previous period (model 3). Surprisingly, the higher punishment now, the more participants increase their contributions if punishment has been (fairly) inconsistent in the past. This finding is, however, in line with research aiming at establishing a potentially beneficial effect of ambiguity in (criminal) law (Baker, Harel et al. 2004; Horovitz and Segal 2006). With these qualifications, we thus support $\mathbf{H_2}$.

	model 1	model 2	model 3
received punishment points in previous period	1.636***	.807***	1.646***
past severity of individual Punishment	-1.260		-1.322
Lrecpun*Lsevindpun	-1.460***		-1.822***
past inconsistency of individual punishment		-1.481^{+}	-.922
Lrecpun*Linconindpun		-.319	.907**
Low	$.653^{+}$	.495	.535
Medium	.426	.446	.544
Final	-1.342***	-1.348***	-1.328***
Cons	-.701**	-.447	-.624*
N	1296	1150	1150
R^2 within	.3062	.2645	.3332
R^2 between	.0731	.0318	.0406
R^2 overall	.2221	.1584	.2361
p model	<.001	<.001	<.001

Table 7

Explaining Changes in Contributions with Individual Punishment Experiences

depvar: changes in contributions

Lrecpun: received punishment in previous period

Lsevindpun: past severity of individual punishment

Linconindpun: past inconsistency of individual punishment

Random effects, robust standard errors, clustered for groups

*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

[14] On all mirror models that ignore clustering, the Hausman test turns out significant, but the problem can be remedied by a Hausman Taylor model. In models 2 and 3, the N is smaller since calculating the standard deviation is impossible for a single observation, per participant.

In treatments *medium* and *high*, participants also learn how intensely the supervisor has exercised her punishment power. Do they use this information? As model 1 in Table 8 shows, there is no significant main effect of the information about total punishment in the previous period. However the interaction effect is significant.[15] Since we know that, overall, those unpunished do not change their contribution level, we can conclude that this information works as one would expect: those who have been punished react more sensitively if the supervisor punishes more intensely. This can be interpreted as a (small) severity effect, in line with $\mathbf{H_2}$.

	model 1	model 2
not punished in previous period	-2.188***	-2.609***
total punishment in previous period	.072	
past sensitivity of punishment level to contribution level		.103
Linnoc*Ltotpun	-.171*	
Linnoc*Lpunrat		-.343
Medium	.296	.304
Final period	-1.086*	-1.086*
cons	1.672***	2.021***
N	864	864
p model	<.001	<.001
R^2 within	.1764	.1669
R^2 between	.1039	.0942
R^2 overall	.1351	.1287

Table 8

Effect of Additional Information Available in *medium* and *high*

depvar: changes in contributions

Random effects, robust standard errors, clustered for groups

Linnoc: not punished in previous period; Ltotpun: total punishment in previous period;

Lpunrat: past sensitivity of punishment level to contribution level

*** p < .001, ** p < .01, * p < .05

Since participants both know the development of contributions and of punishment, they are also in a position to estimate their supervisor's sensitivity to the level of contributions. To measure the severity of punishment for contributions below the possible maximum contribution we define the variable *lavpunrat* which is calculated the following way: in a first step, for each group and period,

$$punrat = \frac{total\ punishment}{20 - average\ contribution}$$

is calculated. This is averaged over all past periods. The result is lagged by one period. As model 2 of Table 8 shows, there is no significant reaction to this information (p = .589).

[15] Again the Hausman test turns out significant, but the problem can be remedied by a Hausman Taylor model.

In treatment *high*, participants see individual contributions and how the supervisor reacted to them. How does this additional information influence their behavior, in particular the behavior of those who have been punished in the previous period? Participants are indeed sensitive to information about the lowest contribution in the previous round (Table 9, model 1).[16] More interestingly even, those who have been punished do not react the same way as those who have not. For those who have not been punished, we must add up the negative main effect and the positive interaction effect. Since the balance is positive, such participants react favorably. The higher the minimum contribution in the previous period, the more they increase their contributions in the subsequent period. The opposite is true for those who have been punished. They show a perverse reaction. The higher the minimum contribution in the previous period, the less they increase their contribution now, despite the fact that they have been punished.

In treatment *high*, participants also see how far the minimum and maximum contributions have been apart in the previous period. Model 2 of Table 9 demonstrates that they also condition their behavior on this information. Again those who have been punished use this information in a diametrically opposed way to how it is used by those not punished. If the distance was big, this induces those who have been punished to adjust contributions upwards, whereas those who have not been punished adjust contributions downwards. This makes sense. It describes a movement of convergence towards the group mean.

In treatment *high*, participants also have more information about the relationship between contributions and punishment. Regressor punishment severity is the coefficient of an OLS model that regresses received punishment on contribution. Regressor punishment inconsistency is the standard error of this coefficient. Both the coefficient and the standard error are calculated for all previous periods. For instance in period 4, they cover information from periods 1-3. We do not find a statistically significant effect of information on punishment severity, model 3. The effect of information on punishment inconsistency again differs between those who have been punished and those who have not, model 4. The significant interaction effect is best interpreted from the perspective of those who have been punished. For them, the sign is not swapped. Consequently, the less punishment was consistent in the past, the less they increase their contributions now. Interestingly, if we interact severity and consistency, both consistency and the interaction are significant, model 5. Severity has a *negative* effect if it is coupled with high inconsistency. Conversely, severity only has a beneficial effect if the punisher is predictable.

16 If we estimate the models of Table 9 as random effects models, the Hausman test turns out significant. Since all models only work with time variant regressors, there is no need for a Hausman Taylor model. We just use the fixed effects model.

	model 1	model 2	model 3	model 4	model 5
not punished in previous period	-4.850**	-1.295	-2.485	-4.060***	-3.795**
lowest contribution in previous round	-.134⁺				
Linnoc*Lmincontr	.151*				
distance between minimum and maximum contribution in previous round		.196**			
Linnoc*Ldistcontr		-.273***			
past severity of punishment			-3.793		9.955
Linnoc*sevpun			4.110		
past inconsistency of punishment				-4.008	-49.079*
Linnoc*inconstpun				12.450	
sevpun*inconstpun					-162.030**
final period	-.002	-.184	.078	.106	-.020
cons	3.523***	.638	1.428	2.703***	5.508*
N	432	432	428	428	428
p model	<.001	<.001	.0007	<.001	.0015
R^2 within	.1218	.1404	.1173	.1126	.1377
R^2 between	.1073	.1311	.1046	.1049	.0030
R^2 overall	.0876	.1047	.0838	.0825	.0422

Table 9

Effect of Information Only Available in *high* on Contribution Dynamics

depvar: changes in contributions

Linnoc: not punished in the previous period; Lmincontr: smallest contribution in previous round;

Ldistcontr: difference between minimum and maximum contribution in previous round;

sevpun: coefficient of contribution in regression explaining punishment

inconstpun: standard error of this coefficient

fixed effects, robust standard errors, clustered for groups

*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

f. *Testing the Hypotheses*

What do we learn from Phase 1 results for our hypotheses? In our experiment, letting everyone see the criminal system in action does not help, but hurts. We refute **H_1**. In line with **H_2**, participants are sensitive to the severity and consistency of punishment they have received themselves, Table 7. Information about average sensitivity of punishment to the average level of contributions is immaterial though, but those who have been punished in the previous period are more influenced by information about the total amount of punishment than those not punished, Table 8. Information about punishment consistency is only available in treatment *high*. In this treatment, contributions are lowest. This seems to speak against the relevance of overall consistency for deterrence. Evidence from treatment *high* points into the same direction. If consistency information is provided, only those who have been punished are sensitive to it, Table 9. The solution is provided by model 5 in Table 9. Consistency and severity interact. While severity does not have a beneficial main effect, it aggravates the negative effect of inconsistency. This qualifies **H_2.**

We partly support $\mathbf{H_3}$. Those who have not been punished do indeed contribute more, Table 6. However, information about total punishment and about the average sensitivity of punishment to behavior is immaterial for this, Table 8, as is information about the relationship between behavior and punishment at the individual level, Table 5. For those not punished, the criminal system exclusively matters indirectly, through its effect on the general willingness to contribute to the common cause, Table 4.

3. Foreign Experiences

In period 11, i.e. in the first period of phase 2, participants contribute significantly more in treatment *low* versus *high* (Mann Whitney, N = 24, p = .0371) and in treatment *medium* versus *high* (Mann Whitney, N = 24, p = .0485), while the difference between *low* and *medium* is insignificant. Comparing contributions, per group, between periods 1 and 11, there is a significant difference in treatment *low* (Wilcoxon, N = 24, p = .0205), but not in the remaining treatments. Hence the disadvantage of full transparency carries over to the next generation of players. Knowing everything about how strangers have been treated by the supervisor is similarly detrimental as knowing the same about one's own community.

In period 11, new active players can only rely on foreign experiences to guide their behavior. They have not yet made their own experiences. As the regressions in Table 10 show, vicarious learning does indeed take place (Stafford and Warr 1993) (for the psychological background, see Bandura 1977). By far the most important factor, both in terms of significance and effect size, is the one that also had the highest explanatory power during phase 1: average contributions. But the development of contributions over time also has a significantly positive effect. This independent variable is the coefficient of a Tobit model that regresses contributions on period, controlling for the endgame effect by an additional coefficient for period 10, separately for each group.

Model 2 checks the effect of those additional pieces of information about the first phase that are only available in treatments *medium* and *high*. Actually, the additional information is of little relevance. The only weakly significant effect concerns severity of punishment. It has a strongly negative effect in treatment *high*, while it is beneficial in treatment *medium*. This independent variable is the coefficient of a linear regression explaining received punishment in the first phase with contributions, separately for each group. None of the additional pieces that are only available in *high* has explanatory power.

	model 1 all treatments	model 2 treatments *medium* and *high*	model 3 treatment *high*
low	-.035		
medium	.724	-3.653	
average contributions in phase 1	.643***	1.100***	1.028**
medium*avcontrphase1		-.389	
development of contributions over time in phase 1	.131**	.441	.439
medium*trendcontrphase1		-.160	
average punishment in phase 1		1.809	1.837
medium*punphase1		3.734	
severity of punishment in phase 1		-16.780$^+$	15.521
medium*punsev1		20.572$^+$	
inconsistency of punishment in phase 1			-15.950
cons	6.349**	3.313$^+$	4.030
N	144	92	48
left censored	2	2	1
right censored	61	39	15
p model	<.001	<.001	.0018
pseudo R^2	.0327	.0730	.0576

Table 10

Explaining Contributions in Period 11

Tobit, robust standard errors, clustered for groups

avcontrphase1: average contributions in phase 1

trendcontrphase1: development of contributions over time in phase 1

punphase1: average punishment in phase 1; punsev1: severity of punishment in phase 1

*** $p < .001$, ** $p < .01$, * $p < .05$, $^+$ $p < .1$

The most important piece of news concerns $\mathbf{H_3}$. Players are not only contributing more if they know that their own group has contributed more. It suffices if they learn that a group managed by the same supervisor has made high contributions, Figure 4. Vicarious experiences matter the same way as own experiences. Comparing treatments *medium* and *high*, we again refute $\mathbf{H_1}$. The effect comes through the fact that punishment information (both regarding the level of punishment and its severity) is counterproductive in treatment *high*, while it is productive in treatment *medium*, which also partially supports $\mathbf{H_2}$.

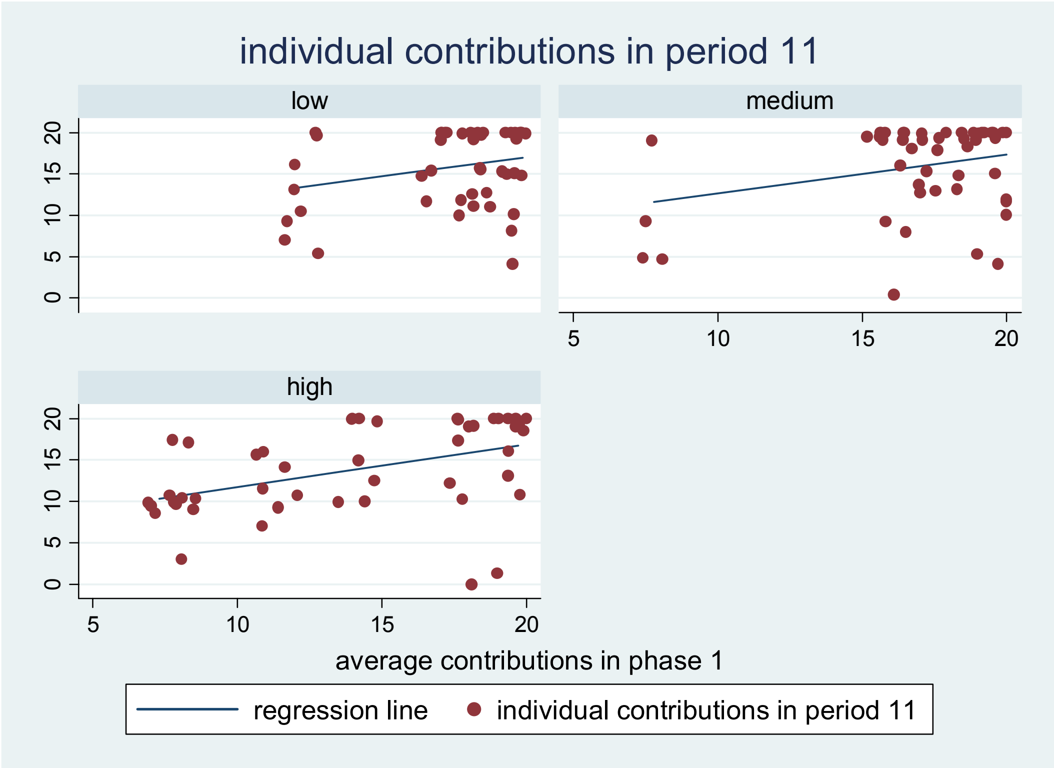


Figure 4
Relation of Contributions in Period 11 to Contributions of Predecessor Group

4. Own and Foreign Experiences Interacting

From period 12 on, own and foreign experiences interact.[17] The two models of Table 11 contrast a static with a dynamic perspective. In both perspectives, the fundamental picture is the same as without foreign experiences. Absolute contributions are chiefly explained by average contributions in the previous round ($z = 4.482$). Note that the coefficient is even bigger than 1. In the following period, participants on average contribute even more than they had on average contributed in the previous period. In a dynamic perspective, again received punishment is the strongest predictor ($z = 13.21$). If they have been punished, participants increase their contributions – the more so, the more punishment they have received.

The level of contributions in the group of predecessors is insignificant in the model explaining levels, and it is only marginally significant in the model explaining first differences. Since this independent variable was crucial for explaining contributions in the initial period of the second group (Table 10), we conclude that vicarious experiences are replaced by own experiences as soon as they are available. In a dynamic perspective, the trend of contributions in the predecessor group keeps explanatory power. Yet through the interaction effects, it becomes clear that this only matters for treatment *high*. This makes sense. From the second period on, participants have their own experiences regarding the level of contributions, but they lack experiences about con-

[17] Again, for all models in, the Hausman test is significant, but the problem can be remedied by a Hausman Taylor model.

tribution dynamics. In treatment *high* they use the information about the predecessor group as a proxy.

For the dynamic model, in treatment *low* information about the level of contributions in the predecessor group has a more positive effect than in the other treatments. Yet it remains that received punishment is less effective here than in the remaining two treatments.

	model 1 depvar absolute contributions	model 2 depvar first differences
low	-10.558	-2.331*
medium	4.322	.128
average contribution per group in previous period	1.031***	
lavcontr*low	.063	
lavgrp*medium	-.258	
received punishment in previous period	-.115	1.096***
lrecpun*low	-.153	-.311*
lrecpun*medium	-.750*	-.054
average contributions in phase 1	.108	-.076^{+}
low*avcontrphase1	.723	.178*
medium*avcontrphase1	.186	.018
development of contributions over time in phase 1	2.596	.310**
low*trendcontrphase1	-2.443	-.308**
medium* trendcontrphase1	-2.085	-.282**
final period	-3.838***	-1.472***
cons	.409	.385
N	1296	1296
left censored	24	
right censored	814	
R^2 within		.3252
R^2 between		.1601
R^2 overall		.2581
p model	<.001	<.001

Table 11

Explaining Contributions and Contribution Changes in Periods 12-20

model 1: random effects Tobit, bootstrapped at the group level, 500 reps

model 2: random effects, robust standard errors, clustered for groups

lavcontr: average contribution per group in previous period

(this variable and the interaction terms are omitted in model 2 since they would be endogenous)

lrecpun: received punishment in previous period; avcontrphase1: average contributions in phase 1

trendcontrphase1: development of contributions over time in phase 1

*** $p < .001$, ** $p < .01$, * $p < .05$, $^{+}$ $p < .1$

VI. Conclusions

Experiments are not meant to portray the richness of the criminal system. They deliberately strip off context, in order to establish full control over the one feature that purportedly makes criminal sanctions effective. Jeremy Bentham had the conjecture that punishment must be made transparent if it is to guide those who have not been punished themselves.

Given our results, we cannot but conclude: as a policy maker, Jeremy Bentham did not seem to get it right. At least in our experiment, if contributions and punishments are transparent, the willingness to contribute to the common cause decays. Even more disturbingly: actual sanctions become less effective, the better would-be low contributors can observe how (mildly) the criminal system reacts to offenses. The results suggest that instead of building panopticon prisons in the town centre, government should conceal prisons from public scrutiny. Punishment serves society best if it remains a tool one does not see in action when applied to others. What really matters is information about normabiding behavior of other members of society. To use a metaphor that features prominently in criminal policy: government should spend money on repairing broken windows, not on showcasing correctional action.

Our experimental result is not only at variance with Jeremy Bentham's advice. It also qualifies a strand of the criminological literature. We do find the expected effects of punishment severity and consistency if we consider the sanctions this one individual has received in the past. In the field, the effect might, however, be blurred by the fact that the second offense will often be qualitatively different from the first. Yet normally, if one speaks of consistency and severity as policy variables, one rather thinks of them at the level of society. At any rate, greater transparency only has an effect on the latter. In our setting overall punishment severity occasionally has a minor effect, as has punishment consistency. But both effects are at most marginally significant. What looks like the prime decision variable of criminal policy turns out to be at best secondary, while the main interest should be in the effects of punishment on reducing the amount and severity of crime in the first place.

Nonetheless, this is a paper in the spirit of Jeremy Bentham. While he might have got it wrong as a policy maker, he got it totally right as an analyst. The main task of criminal policy is and ought to be that would-be criminals are induced to exhibit socially desirable behavior. Only the route to the end is a different one. The main tool ought to be impression management, not deterrence.

References

ANDENAES, JOHANNES (1974). Punishment and Deterrence. Ann Arbor,, University of Michigan Press.

ANDERSON, DAVID A. (2002). "The Deterrence Hypothesis and Picking Pockets at the Pickpocket's Hanging." American Law & Economics Review **4**: 295-313.

ANDERSON, ELIJAH (1990). *Streetwise. Race, Class, and Change in an Urban Community*. Chicago, University of Chicago Press.

BAKER, TOM, ALON HAREL and TAMAR KUGLER (2004). "The Virtues of Uncertainty in Law. An Experimental Approach." *Iowa Law Review* **89**: 443-494.

BALTAGI, BADI H., GEORGES BRESSON and ALAIN PIROTTE (2003). "Fixed Effects, Random Effects or Hausman–Taylor? A Pretest Estimator." *Economics Letters* **79**: 361-369.

BANDURA, ALBERT (1977). *Social Learning Theory*. Englewood Cliffs, Prenctice Hall.

BEAN, FRANK D. and ROBERT G. CUSHING (1971). "Criminal Homicide, Punishment, and Deterrence. Methodological and Substantive Reconsiderations." *Social Science Quarterly* **52**: 277-289.

BECKER, HOWARD SAUL (1963). *Outsiders. Studies in the Sociology of Deviance*. London,, Free Press of Glencoe.

BENTHAM, JEREMY (1830). *The Rationale of Punishment*. London,, R. Heward.

BERG, JOYCE, JOHN DICKHAUT and KEVIN MCCABE (1995). "Trust, Reciprocity, and Social History." *Games and Economic Behavior* **10**: 122-142.

BLUMSTEIN, ALFRED, JACQUELINE COHEN and DANIEL NAGIN (1978). *Deterrence and Incapacitation. Estimating the Effects of Criminal Sanctions on Crime Rates*. Washington, National Academy of Sciences.

BOHNET, IRIS and RICHARD ZECKHAUSER (2004). "Social Comparisons in Ultimatum Bargaining." *Scandinavian Journal of Economics* **106**: 495-510.

CAMERON, SAMUEL (1988). "The Economics of Crime Deterrence. A Survey of Theory and Evidence." *Kyklos* **41**: 301-323.

CHIRICOS, THEODORE G. and GORDON P. WALDO (1971). "Punishment and Crime. An Examination of Some Empirical Evidence." *Social Problems* **18**: 200-217.

CRAMTON, ROGER C. (1968). "Driver Behavior and Legal Sanctions. A Study of Deterrence." *Michigan Law Review* **67**: 421-454.

EHRLICH, ISAAC (1973). "Participation in Illegitimate Activities. A Theoretical and Empirical Investigation." *Journal of Political Economy* **81**: 521-565.

EHRLICH, ISAAC (1996). "Crime, Punishment, and the Market for Offenses." *Journal of Economic Perspectives* **10**: 43-67.

ERICKSON, MAYNARD L. and JACK P. GIBBS (1973). "The Deterrence Question. Some Alternative Methods of Analysis." *Social Science Quarterly* **53**: 534-551.

FARRINGTON, DAVID P. (2003). "A Short History of Randomized Experiments in Criminology. A Meager Feast." *Evaluation Review* **27**: 218-227.

FARRINGTON, DAVID P. and BRANDON C. WELSH (2005). "Randomized Experiments in Criminology. What Have We Learned in the Last Two Decades?" *Journal of Experimental Criminology* **1**: 9-38.

FARRINGTON, DAVID P. and BRANDON C. WELSH (2006). "A Half Century of Randomized Experiments on Crime and Justice." *Crime and Justice* **34**: 55-132.

FEHR, ERNST and URS FISCHBACHER (2004). "Third-Party Punishment and Social Norms." *Evolution and Human Behavior* **25**: 63-87.

FEHR, ERNST and SIMON GÄCHTER (2000). "Cooperation and Punishment in Public Goods Experiments." *American Economic Review* **90**: 980-994.

FEHR, ERNST and SIMON GÄCHTER (2002). "Altruistic Punishment in Humans." *Nature* **415**: 137-140.

FEHR, ERNST and BETTINA ROCKENBACH (2003). "Detrimental Effects of Sanctions on Human Altruism." *Nature* **422**: 137-140.

FISCHBACHER, URS (2007). "z-Tree. Zurich Toolbox for Ready-made Economic Experiments." *Experimental Economics* **10**: 171-178.

FISCHBACHER, URS and SIMON GÄCHTER (2009). "Heterogeneous Social Preferences and the Dynamics of Free Riding in Public Good Experiments." *American Economic Review*: ***.

FISCHBACHER, URS, SIMON GÄCHTER and ERNST FEHR (2001). "Are People Conditionally Cooperative? Evidence from a Public Goods Experiment." *Economics Letters* **71**: 397-404.

FRASE, RICHARD S. (2005). "Punishment Purposes." *Stanford Law Review* **58**: 67-84.

GALBIATI, ROBERTO and PIETRO VERTOVA (2008a). How Laws Affect Behaviour http://ssrn.com/abstract=1295948.

GALBIATI, ROBERTO and PIETRO VERTOVA (2008b). "Law and Behaviours in Social Dilemmas. Testing the Effect of Obligations on Commitment." *Games and Economic Behavior* *******: ***.

GIBBS, JACK P. (1968). "Crime, Punishment, and Deterrence." *Social Science Quarterly* **48**: 515-530.

GIBBS, JACK P. (1979). "Assessing the Deterrence Doctrine." *American Behavioral Scientist* **22**: 653-677.

GLASS, GENE V. (1968). "Analysis of Data on the Connecticut Speeding Crackdown as a Time-Series Quasi-Experiment." *Law and Society Review* **3**: 55-76.

GREINER, BEN (2003). An Online Recruiting System for Economic Experiments. *Forschung und wissenschaftliches Rechnen*. Kurt Kremer und Volker Macho. Göttingen**:** 79-93.

GROGGER, JEFFREY (1991). "Certainty vs. Severity of Punishment." *Economic Inquiry* **29**: 297-309.

GUILLÉN, PABLO, CHRISTIANE SCHWIEREN and GIANANDREA STAFFIERO (2006). "Why Feed the Leviathan?" *Public Choice* **130**: 115-128.

GÜRERK, ÖZGÜR, BERND IRLENBUSCH and BETTINA ROCKENBACH (2009). "Motivating Teammates. The Leader's Choice of Positive and Negative Incentives." *Journal of Economic Psychology* **30**: 591-607.

GÜTH, WERNER, VITTORIA M. LEVATI, MATTHIAS SUTTER and ELINE VAN DER HEIJDEN (2007). "Leading by Example With and Without Exclusion Power in Voluntary Contribution Experiments." *Journal of Public Economics* **91**: 1023-1042.

HAKIM, SIMON, URIEL SPIEGEL and J. WEINBLATT (1984). "Substitution, Size Effects, and the Composition of Property Crime." *Social Science Quarterly* **65**: 719-734.

HAREL, ALON and ALON KLEMENT (2007). "The Economics of Stigma: Why More Detection of Crime May Result in Less Stigmatization." *Journal of Legal Studies* **36**: 355-377.

HAUSMAN, JERRY A. and WILLIAM D. TAYLOR (1981). "Panel Data and Unobservable Individual Effects." *Econometrica* **49**: 1377-1398.

HAWKINS, GORDON (1969). "Punishment and Deterrence. The Educative, Moralizing, and Habituative Effects." *Wisconsin Law Review*: 550-565.

HERRMANN, BENEDIKT, CHRISTIAN THÖNI and SIMON GÄCHTER (2008). "Antisocial Punishment Across Societies." *Science* **319**: 1362-1367.

HOROVITZ, ANAT and UZI SEGAL (2006). "The Ambiguous Nature of Ambiguity and Crime Control." *NYU Journal of Law & Liberty* **2**: 541-556.

KAHAN, DAN (1997a). "Between Economics and Sociology. The New Path of Deterrence." *Michigan Law Review* **95**: 2477-2497.

KAHAN, DAN (1997b). "Social Influence, Social Meaning, and Deterrence." *Virginia Law Review* **83**: 349-395.

KATYAL, NEAL (1997). "Deterrence's Difficulty." *Michigan Law Review* **95**: 2385-2476.

KESSLER, DANIEL and STEVEN D. LEVITT (1999). "Using Sentence Enhancements to Distinguish Between Deterrence and Incapacitation." *Journal of Law and Economics* **42**: 343-363.

KOSFELD, MICHAEL, AKIRA OKADA and ARNO RIEDL (2008). Institution Formation in Public Goods Games http://ssrn.com/abstract=930011.

KRUPKA, ERIN and ROBERTO A. WEBER (2009). "The Focusing and Informational Effects of Norms on Pro-Social Behavior." *Journal of Economic Psychology* **30**: 307-320.

LEDYARD, JOHN O. (1995). Public Goods. A Survey of Experimental Research. *The Handbook of Experimental Economics*. J.H. Kagel und A.E. Roth. Princeton, NJ, Princeton University Press**:** 111-194.

LEMERT, EDWIN MCCARTHY (1951). *Social Pathology. A Systematic Approach to the Theory of Sociopathic Behavior*. New York,, McGraw-Hill.

LEVITT, STEVEN D. (1998). "Why Do Increased Arrest Rates Appear to Reduce Crime. Deterrence, Incapacitation, or Measurement Error?" *Economic Inquiry* **36**: 353-372.

LEVITT, STEVEN D. (2004). "Understanding Why Crime Fell in the 1990s: Four Factors that Explain the Decline and Six that Do Not." *Journal of Economic Perspectives* **18**: 163-190.

LOGAN, CHARLES H. (1972). "General Deterrent Effects of Imprisonment." *Social Forces* **51**: 64-73.

MATSUEDA, ROSS L., DEREK A. KREAGER and DAVID HUIZINGA (2006). "Deterring Delinquents. A Rational Choice Model of Theft and Violence." *American Sociological Review* **71**: 95-122.

NAGIN, DANIEL (1998a). Deterrence and Incapacitation. *The Handbook of Crime and Punishment*. Michael Tonry. New York, Oxford University Press**:** 345-368.

NAGIN, DANIEL S. (1998b). "Criminal Deterrence Research at the Outset of the Twenty-First Century." *Crime and Justice* **23**: 1-42.

NAGIN, DANIEL S. and GREG POGARSKY (2001). "Integrating Celerity, Impulsivity, and Extralegal Sanction Threats into a Model of General Deterrence. Theory and Evidence." *Criminology* **39**: 865-891.

NAGIN, DANIEL S. and GREG POGARSKY (2003). "An Experimental Investigation of Deterrence. Cheating, Self-Serving Bias, and Impulsivity." *Criminology* **41**: 167-193.

OSTROM, ELINOR, JAMES M. WALKER and ROY GARDNER (1992). "Covenants with and without Sword. Self-Governance is Possible." *American Political Science Review* **40**: 309-317.

PATERNOSTER, RAY (1987). "The Deterrent Effect of the Perceived Certainty and Severity of Punishment. A Review of Evidence and Issues." *Justice Quarterly* **4**: 173-217.

PATERNOSTER, RAY (1989). "Decisions to Participate in and Desist from Four Types of Common Delinquency. Deterrence and the Rational Choice Perspective." *Law and Society Review* **23**: 7-40.

PATERNOSTER, RAY, LINDA SALTZMANN, THEODORE G. CHIRICOS and GORDON P. WALDO (1983). "Perceived Risk and Social Control. Do Sanctions Really Deter?" *Law and Society Review* **17**: 457-479.

PETROSINO, ANTHONY, PAUL KIFF and JULIA LAVENBERG (2006). "Randomized Field Experiments Published in the British Journal of Criminology, 1960-2004." *Journal of Experimental Criminology* **2**: 99-111.

PETROSINO, ANTHONY, CAROLYN TURPIN-PETROSINO and JOHN BUEHLER (2003). "Scared Straight and Other Juvenile Awareness Programs for Preventing Juvenile Delinquency. A Systematic Review of the Randomized Experimental Evidence." *Annals of the American Academy of Political and Social Science* **589**(41-62).

PIQUERO, ALEX R. and RAY PATERNOSTER (1998). "An Application of Stafford and Warr's Reconceptualization of Deterrence to Drinking and Driving." *Journal of Research in Crime and Delinquency* **35**: 3-39.

PIQUERO, ALEX R. and GREG POGARSKY (2002). "Beyond Stafford and Warr's Reconceptualization of Deterrence: Personal and Vicarious Experiences, Impulsivity, and Offending Behavior." *Journal of Research in Crime and Delinquency* **39**: 153-186.

POGARSKY, GREG (2002). "Identifying 'Deterrable' Offenders. Implications for Research on Deterrence." *Justice Quarterly* **19**: 431-452.

POGARSKY, GREG and ALEX R. PIQUERO (2003). "Can Punishment Encourage Offending? Investigating the 'Resetting' Effect." *Journal of Research in Crime and Delinquency* **40**: 95-120.

POGARSKY, GREG, ALEX R. PIQUERO and RAY PATERNOSTER (2004). "Modeling Change in Perceptions about Sanction Threats: The Neglected Linkage in Deterrence Theory." *Journal of Quantitative Criminology* **20**: 343-369.

PUTTERMAN, LOUIS G., JEAN-ROBERT TYRAN, and KENJU KAMEI (2010) Public Goods and Voting on Formal Sanction Schemes: An Experiment. Univ. of Copenhagen Dept. of Economics Discussion Paper No. 10-02. Available at SSRN: http://ssrn.com/abstract=1535201

ROBINSON, PAUL H. and JOHN M. DARLEY (2003). "The Role of Deterrence in the Formulation of Criminal Rules. At Its Worst When Doings Its Best." *Georgetown Law Journal* **91**: 949-1002.

ROBINSON, PAUL H. and JOHN M. DARLEY (2004). "Does Criminal Law Deter? A Behavioural Science Investigation." *Oxford Journal of Legal Studies* **24**: 173-205.

SCHMIDT, PETER and ANN D. WITTE (1984). *An Economic Analysis of Crime and Justice. Theory, Methods, and Applications*. Orlando, Fla., Academic Press.

SCHWARTZ, BARRY (1968). "The Effect in Philadelphia of Pennsylvania's Increased Penalties for Rape and Attempted Rape." *Journal of Criminal Law, Criminology and Police Science* **59**: 509-515.

SEMPLE, JANET (1993). *Bentham's Prison. A Study of the Panopticon Penitentiary*. Oxford, Clarendon.

SHEPHERD, JOANNA M. (2004). "Murders of Passion, Execution Delays, and the Deterrence of Capital Punishment." *Journal of Legal Studies* **33**: 283-321.

SHERMAN, LAWRENCE W. (2002). *Evidence-Based Crime Prevention*. London ; New York, Routledge.

SHERMAN, LAWRENCE W. and RICHARD A. BERK (1984). "The Specific Deterrent Effects of Arrest for Domestic Assault." *American Sociological Review* **49**: 261-272.

SILBERMAN, MATTHEW (1976). "Toward a Theory of Criminal Deterrence." *American Sociological Review* **41**: 442-461.

STAFFORD, MARK C. and MARK WARR (1993). "A Reconceptualization of General and Specific Deterrence." *Journal of Research in Crime and Delinquency* **30**: 123-135.

TITTLE, CHARLES R. (1969). "Crime Rates and Legal Sanctions." *Social Problems* **16**: 409-423.

TONRY, MICHAEL (2006). "Purposes and Functions of Sentencing." *Crime and Justice* **34**: 1-46.

TRUMBULL, WILLIAM N. (1989). "Estimations of the Economic Model of Crime Using Aggregate and Individual Level Data." *Southern Economic Journal* **56**: 423-439.

TYRAN, JEAN-ROBERT and LARS P. FELD (2006). "Achieving Compliance when Legal Sanctions are Non-Deterrent." *Scandinavian Journal of Economics* **108**: 135-156.

WILLIAMS, KIRK R. and RICHARD HAWKINS (1986). "Perceptual Research on General Deterrence. A Critical Review." *Law and Society Review* **20**: 545-572.

WILSON, JAMES Q. and RICHARD J. HERRNSTEIN (1985). *Crime and Human Nature*. New York, Simon and Schuster.

WITTE, ANN DRYDEN (1980). "Estimating the Economic Model of Crime with Individual Data." *Quarterly Journal of Economics* **94**: 57-84.

WOOD, DAVID (2002). "Retribution, Crime Reduction and the Justification of Punishment." *Oxford Journal of Legal Studies* **22**: 301-321.

WRIGHT, BRADLEY R.E., AVISHALOM CASPI, TERRIE E. MOFFITT and RAY PATERNOSTER (2004). "Does the Perceived Risk of Punishment Deter Criminally Prone Individuals? Rational Choice, Self-Control, and Crime." *Journal of Research in Crime and Delinquency* **41**: 180-213.

ZELMER, JENNIFER (2003). "Linear Public Goods. A Meta-Analysis." *Experimental Economics* **6**: 299-310.

Appendix A

Individual Groups

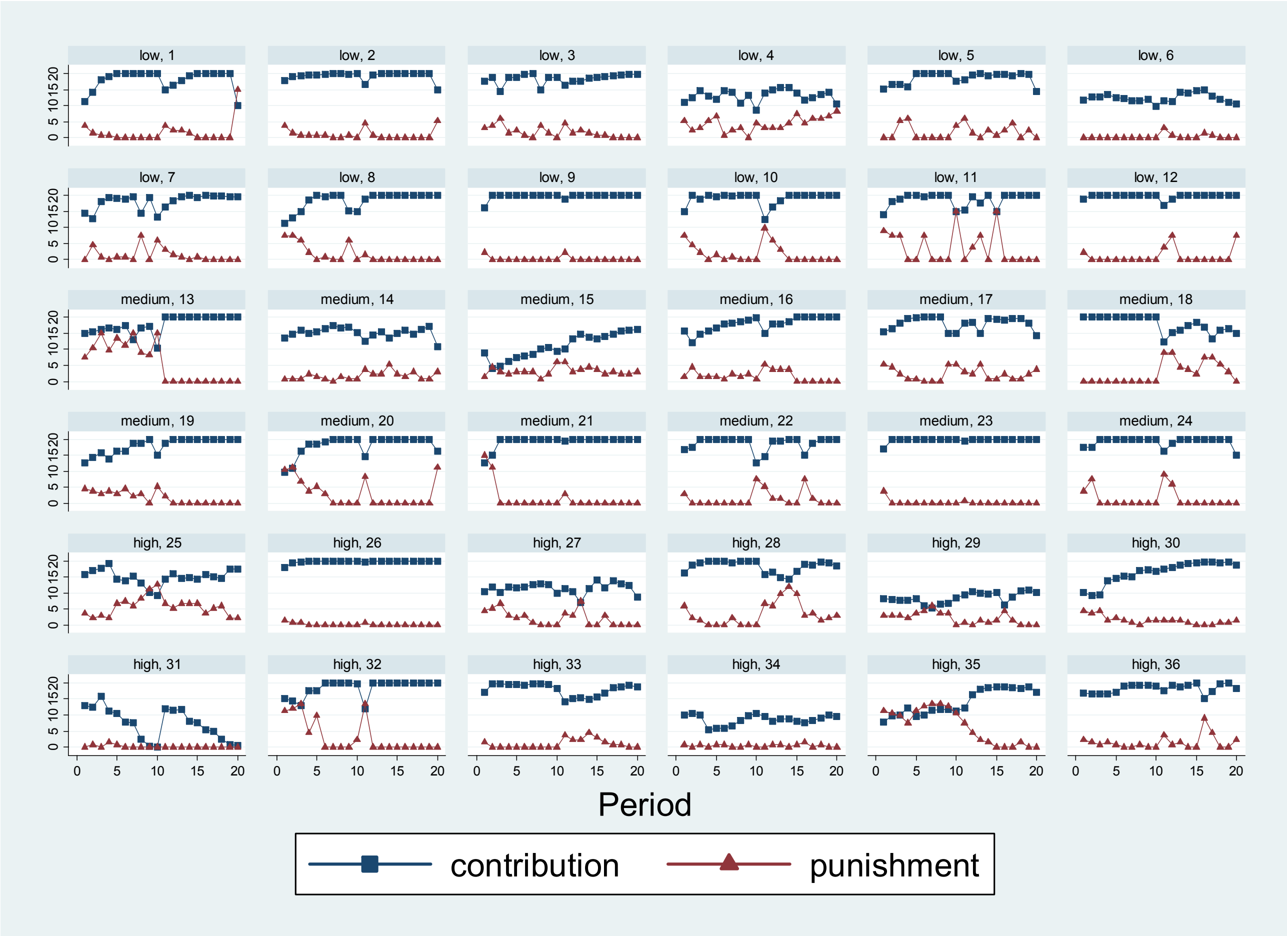


Figure 5
Contributions and Punishment per Group

Box 1
Strategy for Parametric Estimation

We have data from periods, nested in subjects, nested in groups. In many periods, many subjects have contributed their entire endowment of 20. In some periods, some subjects have kept their entire endowment for themselves. Consequently, the contribution data is heavily right censored and slightly left censored. There are standard procedures to separately deal with both features of the data. The nested character is captured by a random or fixed effects model with standard errors clustered at the group level. Censoring is taken into account in a (random effects) Tobit model. Unfortunately there is no standard procedure for clustered *and* censored data. We solve the problem by bootstrapping the ordinary random effects model, with random draws clustered at the group level. Note that, if one were to ignore the fact that observations are not independent for members of one and the same group, i.e. if one did not cluster standard errors, coefficients would still be unbiased. Only the standard errors might be too small. Bootstrapping precisely targets this problem. It generates standard errors that take the clustering into account. In these estimations, we always work with 500 independent draws.

A second statistical problem results from the fact that there is no generally acknowledged fixed effects estimator for censored data. Therefore we cannot perform the Hausman test on the random effects Tobit model. To nonetheless make sure that we are justified to use the more efficient random effects model, we rerun a mirror model that ignores censoring. This of course yields a standard random effects model. We rerun the model as a fixed effects model, and compare the coefficients by way of a Hausman test. In some models, the Hausman test turns out significant. We then are able to remedy the problem by a Hausman Taylor model. This saves the time invariant regressors, in particular the treatment effects, which would cancel out in the fixed effects model via mean differencing. The Hausman Taylor model instruments the time variant regressors by the deviation from their mean, as in the fixed effects model, and it instruments the time invariant regressors by the mean of the exogenous time variant regressors (Hausman and Taylor 1981). A further Hausman test demonstrates that, in all instances where we need this procedure, it has been successful (Baltagi, Bresson et al. 2003).

Instructions for Treatment High Second Phase

(instructions for phase 1 and for phase 2, treatments low and medium are available upon request)

General Instructions for Participants

You are about to take part in an economics experiment. If you read the following instructions carefully, you will be able to earn a substantial sum of money, depending on the decisions you make. It is therefore very important that you read these instructions carefully.

The instructions you have received are exclusively for your private information. **There shall be absolutely no communication during the experiment.** If you have questions, please ask us. Disobeying this rule will lead to exclusion from the experiment and any payments.

The experiment consists of several parts. We will begin by explaining the first part. You will receive separate instructions for the other parts.

You will definitely receive € 2.50 for participating in the experiment. During the experiment, the currency in operation is not euro, but taler. Your entire income is hence first calculated in taler. The total number of taler you will have accumulated in the course of the experiment will then be transferred into euro at the following rate:

1 taler = 3 euro cent.

At the end of the experiment, you will receive a **cash** payment, in euro, of whatever number of taler you have earned.

Participants are divided into groups of five. In other words, there are 4 further participants in your group.

All five participants in your group are taking part in this experiment for the first time. There are two roles: four participants, who have confirmed their presence at this experiment for two hours, are assigned Role A. Another person, who has confirmed his presence at this experiment for 4 hours, is assigned Role B.

The experiment is divided into individual periods, of which there are a total of 10. During these 10 periods, the constellation of your group of five remains unchanged. **You are therefore in the same group with the same people for 10 periods. During these 10 periods, the role you have been assigned also remains unchanged.**

At the beginning of the experiment, each participant is given a lump-sum payment of 50 taler. This occurs only once. You may cover possible losses with these 50 taler.

The following pages give you an outline of the exact proceedings of the experiment.

Information on the Exact Proceedings of the Experiment

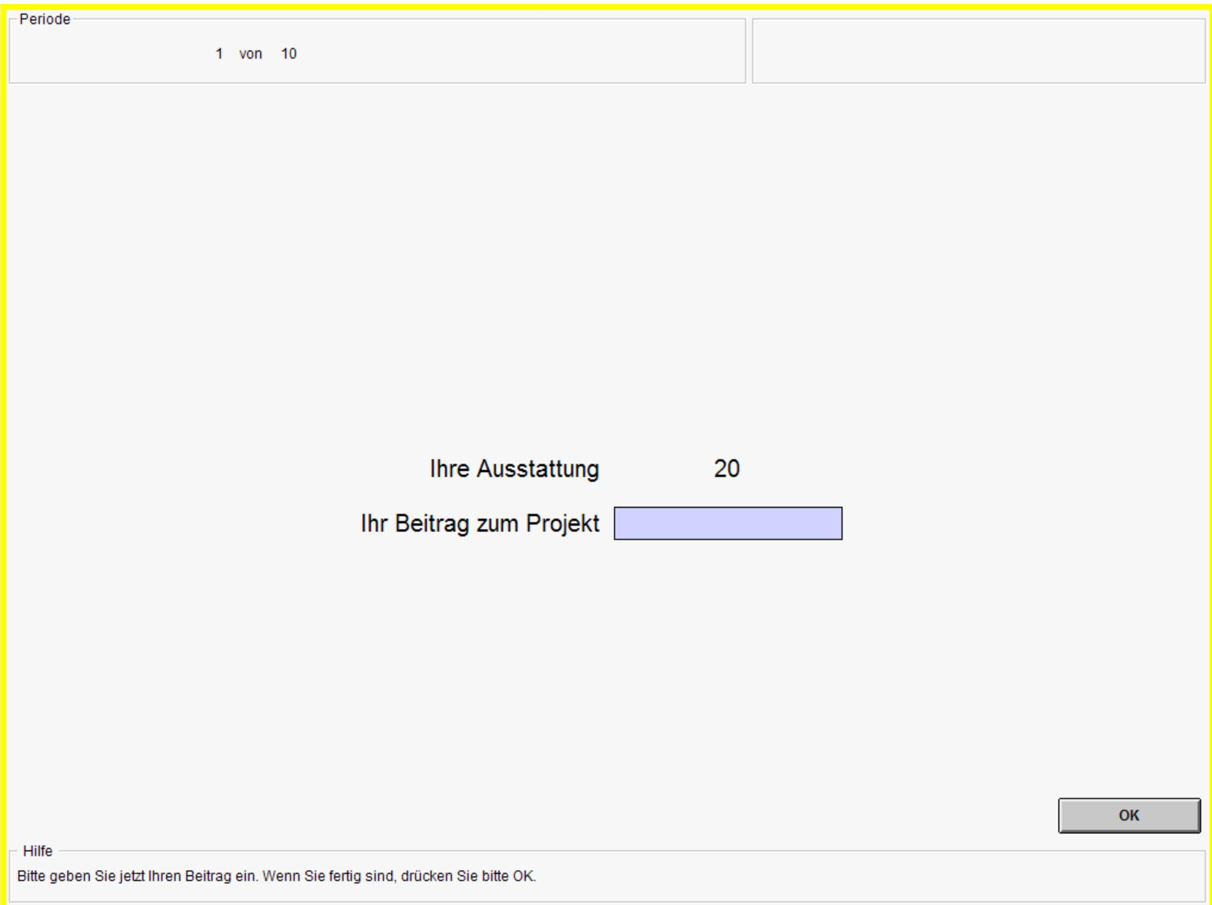
Each of the 10 periods consists of two steps. In Step 1, the participants who have been assigned Role A decide on contributions to a project. In Step 2, the participant who has Role B can reduce the income of the other (Role A) participants. At the beginning of each period, each participant receives **20 points**, referred to henceforth as **endowment**.

Step 1:

In Step 1, **only the four Role A participants** in a group make a decision (should you have been assigned Role B, please read this part of the instructions anyway, in order to find out how a Role A participant can reach a decision). Your task is to reach a decision on how to use your endowment. As a Role A participant, you have to decide how many of the 20 points you wish to pay into a project, and how many you wish to keep for yourself. The consequences of this decision are explained in more detail below.

At the beginning of each period, the following input screen appears:

The input screen:



On the top left corner of your screen, the **period number** is displayed.

As already mentioned, your **endowment in each period is 20 points**. As a Role A participant, you have to make a decision on your project contribution by typing in a sum between 0 and 20 in the appropriate field. You may click on this window by using the mouse. As soon as you have determined the sum you wish to contribute, you have also decided on how many points you keep for yourself: **20 minus your contribution**. Once you have keyed in your amount, press or click **O.K.**, using the mouse or the Enter-key. As soon as you have done this, you can no longer make any changes to your decision.

Your income from the contribution phase consists of two parts:

(1) the points that you have kept for yourself (**"income from endowment kept")**

(2) the **"income from the project"**. The income from the project is calculated as follows:

Your income from the project = 0.4 *times* the total sum of contributions to the project

Your **income from the project, in taler,** for one period is therefore

(20 minus your contribution to the project) + 0.4* (total sum of contributions to the project).

The income of all other group members is calculated according to the same formula, i.e., each group member receives the same income from the project. If, for example, the sum of contributions from all group members is 60 points, then you and all other group members will receive a points income from the project of 0.4*60 = 24 taler. If the group members have contributed a total of 9 points to the project, you and all other group members will receive 0.4*9 = 3.6 taler as your income from the project.

For each point that you keep for yourself, you earn an income of 1 taler. If instead you contribute one point from your endowment to your group project, then the sum of contributions to the project increases by 1 point, and your income from the project increases by 0.4*1 = 0.4 taler. However, this also means that the income of all other group members increases by 0.4 taler, so that the total income of the group increases by 0.4*5 = 2 taler. Through your contributions to the project, the other group members also increase their earnings. On the other hand, you also earn something from the contributions of the other group members to the project. For each point that another group member contributes to the project, you earn 0.4*1 = 0.4 taler.

Please be aware that the Role B participant in a group cannot contribute to the project. This participant receives the same income from the project as each Role A participant.

Step 2:

In Step 2, **only the Role B participant** in each group decides (should you have been assigned Role A, please read this part of the instructions anyway, in order to find out how a Role B participant can reach a decision). As a Role B participant, you can **reduce or leave unchanged** the income of **each** of the other participants in Step 2, namely by assigning **"points"**. This becomes clear once you take a look at the input screen for Step 2:

The input screen for Step 2

Here you can see how much the individual Role A group members have contributed to the project in this period. Please bear in mind that in each period the order in which the members of your group are displayed remains the same. Group members can be identified from period to period.

Now it is up to you to decide for **each** Role A group member in this period whether you wish to allocate points and how many points you wish to distribute. Whatever you decide, you are obliged to enter a figure. If you do not wish to change the income of one particular group member, please enter 0. If you enter a number higher than 0, you reduce this group member's income.

You can move within the input fields under the heading “points” by using the tabulator key (→/) or the mouse.

If you allocate points, this costs you taler; the amount depends on the number of points you allocate. Points are **whole numbers between 0 and 20.** The more points you allocate to a member of your group, the higher your costs are. The following formula gives you the correlation between points allocated and the costs of this allocation in taler:

VII. Cost of points allocated = Number of points allocated.

Each allocated “point” therefore costs you 1 taler. For instance, if you allocate 2 points to a member, this costs you 2 taler; if, in addition, you allocate 9 points to another group member, this costs you 9 taler; if you allocate 0 points to the two other group members, there is no cost. You have therefore allocated a total of 11 points and your **total cost** is, hence, 11 taler (2+9+0+0). If you press **Kostenberechnung** (**Calculate cost**), the total cost is shown to you. Unless you have already clicked **Continue**, you may still change your decision.

If you choose 0 points for a particular group member, you do not change this group member’s income. If, however, you allocate **one** point to a member (i.e., if you choose 1), you **reduce** this member’s income by **3 taler**. If you allocate **2** points to a group member (i.e., if you choose 2), you reduce this member’s income by **6 taler,** etc. **For each point that you allocate to another group member, this member’s income is reduced by 3 taler.**

Please be aware that the Role A participants in a group cannot allocate points. The participant who has been assigned Role B can therefore not receive points in Step 2.

The total taler income of a Role A participant after both steps is hence calculated according to the following formula:

Taler income at the end of Step 2 for Role A = Period income for Role A =

= Income from Step 1
– 3*(sum of *points* received)

The total taler income of a Role B participant is hence calculated according to the following formula after both steps:

Taler income at the end of Step 2 for Role B = Period income for Role B =

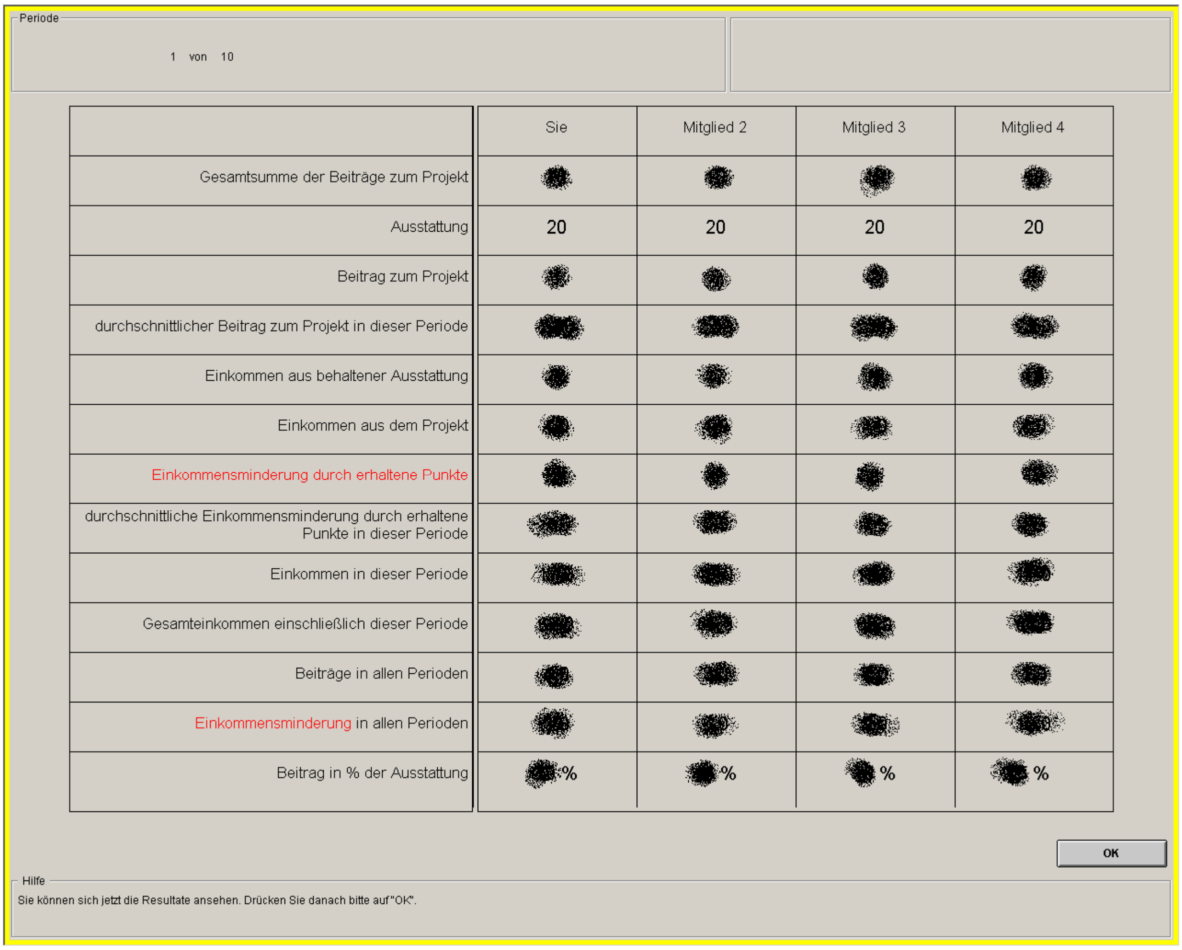
= Income from Step 1
– Cost of *points* allocated by you

Please bear in mind that the taler income can also be negative for Role A participants at the end of Step 2. This could be the case whenever the income reduction from points received is higher than the income from Step 1.

Once all participants have made their decision, a screen informs you of your period income and total income thus far.

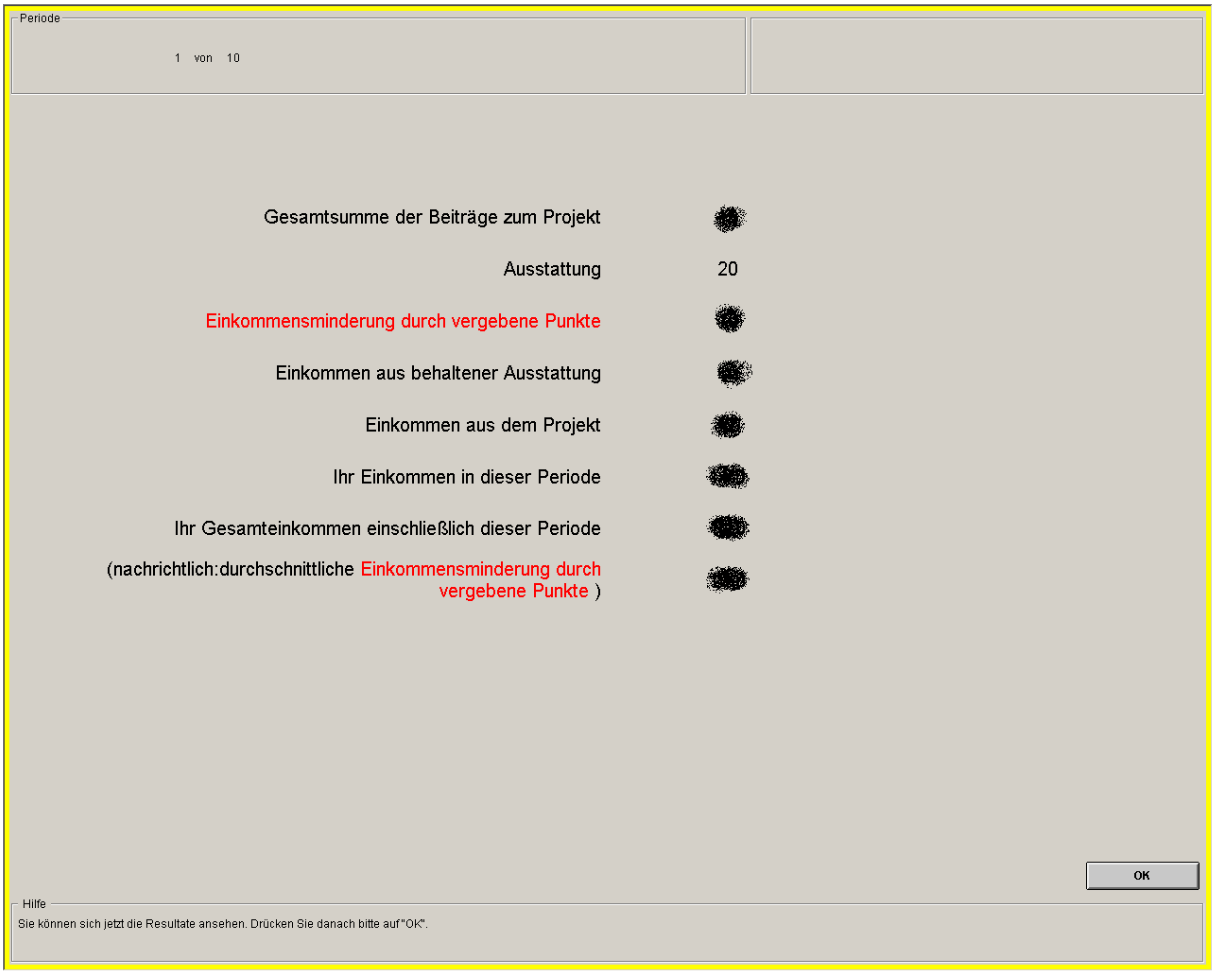
The income screen at the end of Step 2:

If you have been allocated Role A, your screen looks as follows:



	Sie	Mitglied 2	Mitglied 3	Mitglied 4
Gesamtsumme der Beiträge zum Projekt	[redacted]	[redacted]	[redacted]	[redacted]
Ausstattung	20	20	20	20
Beitrag zum Projekt	[redacted]	[redacted]	[redacted]	[redacted]
durchschnittlicher Beitrag zum Projekt in dieser Periode	[redacted]	[redacted]	[redacted]	[redacted]
Einkommen aus behaltener Ausstattung	[redacted]	[redacted]	[redacted]	[redacted]
Einkommen aus dem Projekt	[redacted]	[redacted]	[redacted]	[redacted]
Einkommensminderung durch erhaltene Punkte	[redacted]	[redacted]	[redacted]	[redacted]
durchschnittliche Einkommensminderung durch erhaltene Punkte in dieser Periode	[redacted]	[redacted]	[redacted]	[redacted]
Einkommen in dieser Periode	[redacted]	[redacted]	[redacted]	[redacted]
Gesamteinkommen einschließlich dieser Periode	[redacted]	[redacted]	[redacted]	[redacted]
Beiträge in allen Perioden	[redacted]	[redacted]	[redacted]	[redacted]
Einkommensminderung in allen Perioden	[redacted]	[redacted]	[redacted]	[redacted]
Beitrag in % der Ausstattung	[redacted]%	[redacted]%	[redacted] %	[redacted] %

If you have been allocated Role B, your screen looks as follows:



Your total income at the end of the experiment is the sum of the period incomes according to the following formula:

Total income (in taler) from the experiment =
= 50 + Sum of all period incomes, if the total is not negative. Otherwise, you receive 0 taler
In addition, you are given the sum of 2.50 euro for showing up.

As mentioned above, the member of your group who has been assigned Role B will take part in the same experiment on a further occasion. At the beginning of this future experiment, the new Role A participants, who will then form a group together with your participant B, will receive a chart for their information. This chart depicts the average contributions and the individual contributions as well as the points received by the four individual Role A participants from your current group over 10 periods. The four Role A participants from the future experiment will be different participants to those in this experiment. Only the Role B participant is the same person. The participants in the new group will be told that the chart depicts the behavior of the former group with the same Role B participant.}

Do you have any further questions? If you do, please raise your hand from your booth – one of the experiment supervisors will be with you shortly.

Preprints 2010

2010/05: Gropp R., Hakenes H., Schnabel I., Competition, Risk-Shifting, and Public Bail-out Policies

2010/04: Slemrod J., Traxler C., Optimal observability in a linear income tax

2010/03: Baumann, Florian; Friehe, Tim; Grechenig, Kristoffel: Switching Consumers and Product Liability: On the Optimality of Incomplete Strict Liability

2010/02: Bierbrauer F., Hellwig M., Public-Good Provision in a Large Economy

2010/01: Bierbrauer F., Incomplete contracts and excludable public goods

Preprints 2009

2009/39: Petersen N., The Reception of International Law by Constitutional Courts through the Prism of Legitimacy

2009/38: Engel C., Hennig-Schmidt H., Irlenbusch B., Kube S., On Probation. An Experimental Analysis

2009/37: Engel C., Das schwindende Vertrauen in die Marktwirtschaft und die Folgen für das Recht

2009/36: Jansen J., Share to Scare: Technology Sharing in the Absence of Intellectual Property Rights

2009/34: Lehmann, S., The German elections in the 1870s: why Germany turned from liberalism to protectionism

forthcoming in: Journal of Economic History, In Press.

2009/33: Hakenes H., Schnabel I., Credit Risk Transfer and Bank Competition

2009/32: Jansen J., Beyond the Need to Boast: Cost Concealment Incentives and Exit in Cournot Duopoly

2009/31: Fellner G., Sausgruber R., Traxler C., Testing Enforcement Strategies in the Field: Legal Threat, Moral Appeal and Social Information

2009/30: Lüdemann J., Rechtsetzung und Interdisziplinarität in der Verwaltungsrechtswissenschaft

forthcoming in: Öffentliches Recht und Wissenschaftstheorie, Tübingen, Mohr Siebeck, pp. 125-150, In Press.

2009/29: Engel C., Rockenbach B., We Are Not Alone: The Impact of Externalities on Public Good Provision

2009/28: Gizatulina A., Hellwig M., Informational Smallness and the Scope for Limiting Information Rents

2009/27: Hahmeier M., Prices versus Quantities in Electricity Generation

2009/26: Burhop C., The Transfer of Patents in Imperial Germany

2009/25: Burhop C., Lübbers T., The Historical Market for Technology Licenses: Chemicals, Pharmaceuticals, and Electrical Engineering in Imperial Germany

2009/24: Engel C., Competition as a Socially Desirable Dilemma Theory vs. Experimental Evidence

2009/23: Morell A., Glöckner A., Towfigh E., Sticky Rebates: Rollback Rebates Induce Non-Rational Loyalty in Consumers – Experimental Evidence

2009/22: Traxler C., Majority Voting and the Welfare Implications of Tax Avoidance

2009/21: Beckenkamp M., Engel C., Glöckner A., Irlenbusch B., Hennig-Schmidt H., Kube S., Kurschilgen M., Morell A., Nicklisch A., Normann H., Towfigh E., Beware of Broken Windows! First Impressions in Public-good Experiments

2009/20: Nikiforakis N., Normann H., Wallace B., Asymmetric Enforcement of Cooperation in a Social Dilemma

forthcoming in: Southern Economic Review, In Press.

2009/19: Magen S., Rechtliche und ökonomische Rationalität im Emissionshandelsrecht

2009/18: Broadberry S.N., Burhop C., Real Wages and Labour Productivity in Britain and Germany, 1871-1938: A Unified Approach to the International Comparison of Living Standards

2009/17: Glöckner A., Hodges S.D., Parallel Constraint Satisfaction in Memory-Based Decisions

2009/16: Petersen N., Review Essay: How Rational is International Law?

forthcoming in: European Journal of International Law, vol. 20, In Press.

2009/15: Bierbrauer F., On the legitimacy of coercion for the financing of public goods

2009/14: Feri F., Irlenbusch B., Sutter M., Efficiency Gains from Team-Based Coordination – Large-Scale Experimental Evidence

2009/13: Jansen J., On Competition and the Strategic Management of Intellectual Property in Oligopoly

2009/12: Hellwig M., Utilitarian Mechanism Design for an Excludable Public Good

published in: Economic Theory, vol. 2009, no. July 14, Berlin/Heidelberg, Springer, 2009.

2009/11: Weinschenk P., Persistence of Monopoly and Research Specialization

2009/10: Horstmann N., Ahlgrimm A., Glöckner A., How Distinct are Intuition and Deliberation? An Eye-Tracking Analysis of Instruction-Induced Decision Modes

2009/09: Lübbers T., Is Cartelisation Profitable? A Case Study of the Rhenish Westphalian Coal Syndicate, 1893-1913

2009/08: Glöckner A., Irlenbusch B., Kube S., Nicklisch A., Normann H., Leading with(out) Sacrifice?
A Public-Goods Experiment with a Super-Additive Player

forthcoming in: Economic Inquiry, In Press.

2009/07: von Weizsäcker C., Asymmetrie der Märkte und Wettbewerbsfreiheit

2009/06: Jansen J., Strategic Information Disclosure and Competition for an Imperfectly Protected Innovation

forthcoming in: Journal of Industrial Economics, In Press.

2009/05: Petersen N., Abkehr von der internationalen Gemeinschaft? – Die aktuelle Rechtsprechung des US Supreme Court zur innerstaatlichen Wirkung von völkerrechtlichen Verträgen –

forthcoming in: Völkerrecht im innerstaatlichen Bereich, Vienna, facultas.wuv, In Press.

2009/04: Rincke J., Traxler C., Deterrence Through Word of Mouth

2009/03: Traxler C., Winter J., Survey Evidence on Conditional Norm Enforcement

2009/02: Herbig B., Glöckner A., Experts and Decision Making: First Steps Towards a Unifying Theory of Decision Making in Novices, Intermediates and Experts

2009/01: Beckenkamp M., Environmental dilemmas revisited: structural consequences from the angle of institutional ergonomics, issue 2009/01