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Abstract

Does probation pay a double dividend? Society saves the cost of incarceration, and convicts preserve their liberty. But does probation also reduce the risk of recidivism? In a meta-study we show that the field evidence is inconclusive. Moreover it struggles with an identification problem: those put on probation are less likely to recidivate in the first place. We therefore complement the field evidence by a lab experiment that isolates the definitional feature of probation: the first sanction is conditional on being sanctioned again during the probation period. We find that probationers contribute less to a joint project; punishment cost is higher; efficiency is lower; inequity is higher. While experimental subjects are on probation, they increase their contributions to a joint project. However, once the probation period expires, they reduce their contributions. While in the aggregate these two effects almost cancel out, critically those not punished themselves do trust the institution less if punishment does not become effective immediately.

Keywords: probation, recidivism, public goods, punishment, experimental economics

JEL: C91, D03, H41, K14, K42

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“If an adolescent or an adult has committed a crime, and we set him free, the risk of recidivism is smaller than if we send him to prison.”
(von Liszt 1905:339 [our translation]).

1. Introduction

In criminal law practice, the choice between prison and probation is mainly driven by necessity. Prison space is scarce and costly. Despite public pressure to “get tough on crime”, legislators are not willing to give more and more money for building and running prisons. Consequently, in the US the number of probationers is steadily on the rise. In 2006, 4,237,023 inhabitants, or 1.42 % of the population, were on probation.[1] This amounted to 58.75 % of persons under correctional supervision, while only 20.70 % of them were in prison (Glaze and Bonczar 2007:2). But what are the effects of probation on the behavior of convicts? Is probation only effective as long as convicts are under the threat of punishment becoming effective? Do they change their behavior once the probation period expires? These are the questions that we address in this paper, and we will do so by means of a laboratory experiment. To the best of our knowledge, we are the first to use this method for studying the behavioral effects of probation. We thereby contribute to the nascent experimental law and economics literature (characteristic contributions include Croson and Johnston 2000; Gneezy and Rustichini 2000; Dickson and Shepsle 2001; Druckman 2001; Arlen, Spitzer et al. 2002; Loewenstein and Moore 2004; McAdams and Nadler 2005; McKee, Santore et al. 2007). To that end, we build on a firmly established paradigm from experimental economics, namely the public goods game with a punishment option (Fehr and Gächter 2000; Falk and Fischbacher 2002; Fehr and Gächter 2002; Gächter, Renner et al. 2008; Herrmann, Thöni et al. 2008; Milinski and Rockenbach 2008; Nikiforakis and Normann 2008), which had a prelude in psychology (Yamagishi 1986) and in political science (Ostrom, Walker et al. 1992).

From a policy perspective, the primary question ought to be whether probation is effective in curbing future crime. If Franz von Liszt, the founding father of German criminology, had it right, there would be a double dividend. Probation not only saves public money and respects private freedom, it even does a better job in taming recidivism. In section 2 we summarize those 14 studies that compare recidivism in prisoners and in probationers directly. Chi square tests show that there is a significant difference in all but four studies. Yet in different studies this difference points in opposite directions. Thus, so far the literature appears to be inconclusive. On the basis of the existing field evidence we conduct a meta study. Irrespective of methodology, we establish a significant positive overall effect of probation: those who have been put on probation are less likely to recidivate than those incarcerated.

However, for two reasons, one should not put too much trust in this result. First, if one properly takes the heterogeneity of the evidence into account, the overall effect might as well be the other way round. More importantly even, all these studies work with field data. A problem when draw-

1 Source for population data http://www.census.gov/popest/states/tables/NST-EST2006-01.xls.

ing conclusions from field data is that, in criminal law practice, those who are considered less likely to recidivate are more likely to be granted probation. The existing studies from the field adopt different strategies to tackle this identification problem. From a policy perspective, it is troubling news that the most sophisticated, and the most recent, study comes to the opposite conclusion. Exploiting a discontinuity that, arguably, is unrelated with the decision to grant probation, it shows that prisoners, not probationers are less likely to recidivate (Hjalmarsson 2008).

The only way to establish full control over identification is a randomized experiment. Yet for ethical reasons (Wilkins 1957:201), and for the sake of potential victims, it is problematic to do this randomization in the field with real-life convicts.[2] However, it can be done in the "wind tunnel" of a *laboratory* experiment – which is the approach we adopt in this paper. We build upon a literature that has developed in experimental economics. Student subjects are exposed to "punishment" in a repeated public goods game (Fehr and Gächter 2000; Fehr and Gächter 2002). The public goods game is a stylized model of a community in which each person's well-being depends on own and other persons' contributions. Individually, each member is best off if she free-rides on other members' efforts for the common good. However, jointly everyone is best off if all contribute fully.[3] What is usually observed is that cooperation unravels over time if this game is repeated, and that the community ends up in a situation that is much worse than if all had contributed right from the beginning.

This makes it particularly interesting to study the effect of sanctions. In this literature, sanctions are implemented the following way: each player is empowered to spend money with the effect that even more money is destroyed from the recipient's income (Fehr and Gächter 2002). We extend this procedure by introducing "probation": if a player is punished, she will not be sanctioned immediately. The punishee loses money only if she is punished again during the following three periods.

We do not claim that this is a direct, or even a complete test of all the many factors that contribute to recidivism in the field. In the real world much more is at stake for a probationer than just losing money. While she is on probation, a person is under the threat of a prison sentence. She has to respond to her probation officer, she is listed in the crime register, and she is often exposed to minor immediate sanctions. Hopefully, probation supervision helps better align the convict's character with social expectations. In the field, recidivism is defined as a second conviction within the following years, while we retest our participants within the following minutes. In the field, a crime record is kept, which makes it less likely that one is repeatedly on probation. Skeptics compile such lists of contextual factors to raise doubts about generalisability, often also called external validity (a prominent voice is Levitt and List 2007). We agree that one should be cautious when deriving policy recommendations from experimental findings. Before taking action, policy makers would certainly want to check back with research from the field, and with those running the criminal system in practice. We do, however, believe in the complementary

2 The only notable exception is a study by (Levin 1971). The California Youth Authority agreed to randomly place convicted juveniles with a comparable recidivism risk either in prison or on probation.

3 The exact details of the game will be given in the next section.

value of the experimental method. In our case, such complementary evidence is particularly desirable, given the field evidence is inconclusive, wrestles with an identification problem, and suffers from measurement error.

The experimental method gives us full control over factors that are hard, if not impossible, to control in the field (cf. Falk and Heckman 2009). Our design is not plagued by measurement error. In the field, many acts that would qualify as recidivism never catch the attention of the police, and therefore do not trigger new sanctions. We can also rule out that recidivism is driven by pressure from a convict's peer group, like in Falk and Fischbacher (2002). We can rule out that our participants interpret a prespecified fine as a price, and feel justified to impose damage on others (Gneezy and Rustichini 2000). We need not be concerned that being in prison might train inmates to commit offenses that are even worse. Most importantly, we are able to exclusively vary what arguably is the key difference between prison and probation: that sanctions are unconditional if the convict is sent to prison, and conditional on a second sanction if she is granted probation. We do this by testing two variations. In the *Direct* treatment, only immediate sanctions are available. By contrast, in the *Probation* treatment, sanctions are always delayed until the recipient is punished a second time, and they never become effective if this person goes unpunished for the next three periods.

Given this research question, it was paramount to rule out potential contributing factors by design. In the field, criminal law is heavily value laden. Criminal sanctions do not only impinge on convicts' freedom and property. Society also ostensibly disapproves of the convict's conduct. This "moral component" of the sanction always becomes immediately effective, even if the convict is not immediately sent to prison. By contrast, we deliberately avoid the word punishment (and speak of "points subtracted"). Only the punishee learns that she has been sanctioned.

We introduce "probation" as a variation of a tried and tested tool of experimental economics, the public good game. That way, our results can be compared with the findings from a rich literature, and we may be sure that our results are not an artifact of the parameters chosen. This choice has further advantages. In the field, one only knows that someone recidivates if she is actually caught committing the crime, while positive effects on convicts' behavior are usually not observable and thus also not reported. Field data therefore suffers from a severe selection problem. In our setting, improvements and deterioration of contribution behavior are equally observable. In the field, recidivism is a binary variable: either a person is reconvicted, or reincarcerated for that matter, or not. In our experiment, behavior is measured continuously. That way we are not only in a position to see whether punishment matters at all. We can also show by how much behavior moves into the socially desirable direction, depending on whether a person is punished conditionally or unconditionally. The gradual nature of our dependent variable is particularly conducive to relating reactions of culprits separately to being set on probation, to actually having been sanctioned, and to the fact that the probation period has elapsed.

Finally, in the field, all one can observe is behavior of a former convict. Now, back to Jeremy Bentham, criminal policy has not only been concerned about recidivism. Punishment has also been justified as a tool for deterrence. Others, who might be tempted to break the law themselves, should observe that misbehavior does not pay, and react by resisting the temptation (Bentham 1830). In our setting, we are able to compare the deterrence effect of punishment that becomes immediately effective with punishment on probation.

The bottom line of our findings is that making sanctions conditional does not necessarily pay a double dividend. The main reason is that we observe two effects, pointing into opposite directions. As long as sanctions are pending, our participants increase their cooperation level. This is the beneficial effect. However, once the threat is removed and participants have a clean slate again, a significant fraction of persons decide to reduce their contributions – which is the socially detrimental effect. In our setting, the social detriment is almost as strong as the social benefit. However, the deterrence effect on those who have not been punished themselves is significantly smaller.

The paper proceeds as follows. The next section presents the evidence from the meta-study. Section 3 deals with our experimental setup and design. We introduce our two treatments and discuss possible predictions. Results are presented in section 4. We will discuss the effects of the different institutions on contributions, efficiency, distribution and punishment behavior. Section 5 concludes with a brief discussion of policy implications.

2. Meta-Study

Most of the empirical literature (for a review of reviews see Lipsey and Cullen 2007) investigates recidivism separately for prisoners (Langan and Levin 2002; Kohl, Hoover et al. 2008) and for probationers (the literature is reviewed by Whitehead 1991; Geerken and Hayes 1993; Morgan 1993; Minor, Wells et al. 2003:31f.; Stalans, Yarnold et al. 2004:254). Searching the National Archive of Criminal Justice Data,[4] the Index of Legal Periodicals,[5] and the Social Science Research Network,[6] with no time limit, and checking back with the bibliographies of the papers thus found,[7] we identified a total of 14 studies that directly compare recidivism of probationers and of prisoners (Wilkins 1957; Davis 1964; Babst and Mannering 1965; Beattie and Bridges 1970; Levin 1971; Hopkins 1976; Bartell and Winfree 1977; Menard and Covey 1983; Petersilia and Turner 1986; Glaser and Gordon 1988; Cohen, Eden et al. 1991; Copas and Marshall 1998; Jehle, Heinz et al. 2003; Hjalmarsson 2008). These papers differ widely in terms of coverage, methodology, sophistication and, most disturbingly, the direction and the size of the effect. Table 1 summarizes the precise definitions of the object of study, the period of investigation, the jurisdiction, the sample size, the length of the period of observation, and the results. As a descriptive measure, it uses the fraction of recidivists, in percent, of probationers and prisoners, respectively. This table also presents chi square tests, showing that the difference between re-

4 Keyword „probation“, 67 hits.
5 Keyword “probation”, 318 hits.
6 Keyword “probation”, 119 hits.
7 Plus adding the only pertinent study from our country of origin, (Jehle, Heinz et al. 2003).

cidivism of probationers and prisoners is significant in all but four studies. It finally lists the identification strategy, if any.

Author	Year	Dimension	Identification	Period	Jurisdiction	Sample Size	Length	RecProb	RecPri	χ^2 p-value
Wilkins	1957		quasi experiment	1952	UK	100	3	40	44	.685
Davis	1964			1956-1958	California	9082	4	34	40	<.001
Babst/Mannering	1965	no prior conv		1954-1959	Wisconsin	5274	2	24	32	<.001
Babst/Mannering	1965	one prior conv		1954-1959	Wisconsin	1340	2	41	43	.457
Babst/Mannering	1965	>= two prior conv		1954-1959	Wisconsin	866	2	51	48	.369
Beattie/Bridges	1970			< 1970	California	8534	1	34	51	<.001
Levin	1971	15 months	experiment		California	802	1	27	52	<.001
Levin	1971	24 months	experiment		California	802	2	37	60	<.001
Hopkins	1976		quasi experiment	1961-1964	Connecticut	86	5	32	72	.009
Bartell/Winfree	1977			1971	Albuquerque	79	4	n.a.	n.a.	-
Menard/Covey	1983		matching	1979-1980	Colorado	138	2	25	76	<.001
Petersilia/Turner	1986	rearrest	matching	1980	California	1022	2	63	72	.002
Petersilia/Turner	1986	reconvict	matching	1980	California	1022	2	37	53	<.001
Petersilia/Turner	1986	incarcerated	matching	1980	California	1022	2	30	46	<.001
Glaser/Gordon	1988	all later criminal interventions		1984	Los Angeles	200	2	50	33	<.001
Cohen/Eden	1991		matching	1978-1979	Israel	202	5	44	60	.026
Copas/Marshall	1998	1987 data		1987	UK	6000	2	56	54	.019
Copas/Marshall	1998	1990 data		1990	UK	2000	2	55	53	.370
Jehle/Heinz	2003			1994	Germany	105011	4	44	56	<.001
Hjalmarsson	2008		instrument	1981-1998	Washington State	20542	2	27	19	<.001

Table 1
Meta Study, Descriptives

To synthesize the evidence, Table 2 uses two alternative methodologies; eventually methodology turns out to make little difference. To make findings comparable, we first calculate the odds ratio. We thus calculate

$$odds\ ratio = \frac{\dfrac{\#\ probation\ and\ recidivist}{\#\ probation\ and\ no\ recidivist}}{\dfrac{\#\ prison\ and\ recidivist}{\#\ prison\ and\ no\ recidivist}}$$

If the odds ratio is 1, in the respective study probationers are as likely to recidivate as are prisoners. If the odds ratio is below 1, probationers are less likely to recidivate than prisoners. If the odds ratio is above 1, probationers are more likely to recidivate than prisoners. In this dataset, the odds ratio ranges from a very low value of 0.103 (Menard and Covey 1983) to a very high value of 2.030 (Glaser and Gordon 1988).[8] For probationers, recidivism probability, within the respec-

8 Two studies do not report the data such that the odds ratio can be calculated this way. One study reports regression coefficients for recidivism probabilities plus the grand mean, so that the odds ratio can be safely reconstructed (Menard and Covey 1983). The other (methodological) outlier is more difficult to integrate. This paper uses time series analysis, and reports the hazard rate for the reconviction of prisoners, compared to

tive period of observation, ranges from 25 % (Menard and Covey 1983) to 63 % (Petersilia and Turner 1986: if one uses the most encompassing definition of recidivism, namely rearrest). For prisoners, in the comparable time period the recidivism risk ranges from 19 % (Hjalmarsson 2008) to 76 % (Menard and Covey 1983). Across studies, the number of observations differs widely, ranging from as little as 79 observations (Bartell and Winfree 1977) to as many as 105,011 observations (Jehle, Heinz et al. 2003). In our first meta-analytic approach, using the procedure introduced by Mantel and Haenszel (1958), we weight these results by the number of observations,[9] to get an overall odds ratio of 0.671. We reject the null hypothesis that the odds ratio is 1, i.e. that there is no difference in terms of recidivism between prisoners and probationers, at z = 31.37, p <. 0001. However, heterogeneity is pronounced, chi square = 421.07, p < .0001.[10] 95.7 % of the variance can be attributed to this heterogeneity.[11]

Author + Study	Dimension	odds ratio	N obs weight	risk difference	variance weight
Wilkins		0.848	.09	-0.040	4.04
Davis		0.759	7.16	-0.065	8.88
Babst/Mannering a	no prior conv	0.676	2.82	-0.080	8.73
Babst/Mannering b	one prior conv	0.918	1.05		
Babst/Mannering c	>= two prior conv	1.132	.65		
Beattie/Bridges		0.491	8.99	-0.172	8.88
Levin a	15 months	0.358	.95	-0.240	7.83
Levin b	24 months	0.392	.96		
Hopkins		0.176	.06	-0.407	2.47
Menard/Covey		0.103	.19	-0.515	4.75
Petersilia/Turner a	Rearrest	0.662	.90		
Petersilia/Turner b	Reconvict	0.542	1.11	-0.151	8.04
Petersilia/Turner c	Incarcerated	0.505	1.09		
Glaser/Gordon	all later criminal interventions	2.030	.11	.170	5.65
Cohen/Eden		0.531	.22	-0.157	5.60
Copas/Marshall a	1987 data	1.129	4.59	0.030	8.82
Copas/Marshall b	1990 data	1.084	1.57	0.020	8.48
Jehle/Heinz		0.624	65.49	-0.117	8.99
Hjalmarsson		1.576	1.98	0.080	8.84
Mantel-Haenszel pooled OR		0.672			
DerSimonian Laird pooled RD				-.093	

Table 2
Meta Study: Analysis

probationers. It finds that, after statistical controls, the recidivism risk of a prisoner is about two thirds of the recidivism risk of a probationer (Hjalmarsson 2008). For our purposes, we use the raw data.

9 Specifically, following Mantel and Haenszel (1958), the weight is given by $b_i c_i / N_i$, where b_i is the number of convicts put on probation who did not recidivate, and c_i is the number of convicts put in prison who did redivate, while N_i is the total of observations, per study i.

10 Cochrane's Q is computed by summing up the squared deviations of each study's estimate from the overall meta-analytic estimate, weighting each study the same way as for the meta study. P values are obtained by comparing the resulting statistic with a chi square distribution with k-1 degrees of freedom, where k is the number of studies (Higgins, Thompson et al. 2003).

11 To get this number, $I^2 = (100*(Q-k))/Q$ is calculated (Higgins, Thompson et al. 2003).

While odds ratios are an intuitive measure for categorical data, most meta studies prefer to pool risk differences. In our case, this means that we calculate, for each study, the difference between the probabilities of recidivism of probationers versus prisoners. In this second estimation, we only include one finding from those studies that partly or fully reestimate the same evidence more than once. Using the random effects methodology, and weighting studies by their estimated variance[12], as proposed by DerSimonian and Laird (1986), we again establish a significant advantage of probation over prison. If recidivism was equally likely upon both interventions, the risk difference would be 0. We reject this null hypothesis at $z = 3.50$, $p < .0001$. Again, heterogeneity is pronounced, chi square = 493.79, $p < .0001$, which implies that even 97.4 % of the variance is due to heterogeneity. With the random effects methodology, we are also in a position to calculate a confidence interval for a predicted future test.[13] The 95 % confidence interval runs from -.40 to .21. It thus largely exceeds 0, so that we cannot even be sure whether we should expect probation to be more or less effective in curbing recidivism, let alone about the size of the comparative advantage or disadvantage. The forest plot of Figure 1 visualizes the findings from this meta-analytic approach. Note that, for this second way of analyzing the data, we have to assume that the individual studies are random draws from a normal distribution, while the Mantel-Haenszel approach can be interpreted as a fixed effects model, (which is more conservative).

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12 The sampling variance s^2 of each study is calculated according to $w_i = \frac{1}{s_i^2 + \tau^2}$. In this, the variance per individual study i is given by $s_i^2 = r_{Ti}(1 - r_{Ti})/n_{Ti} + r_{Ci}(1 - r_{Ci})/n_{Ci}$, where r is the proportion of convicts recidivating under treatment T (for probation) and control C (for prison) (DerSimonian and Laird 1986). The overall variance within the entire meta study is calculated according to
$\tau^2 = \max\{[Q - (k-1)]/[\sum w_i - (\sum (w_i^2)/\sum w_i)],0\}$

13 This interval is calculated as $mean \pm t_{df} * \sqrt{(s^2 + \tau^2)}$ where t is the t-statistic with $k - 2$ degrees of freedom, s^2 is the squared standard error, and τ^2 is the between-studies variance, for background see (Higgins and Thompson 2001).

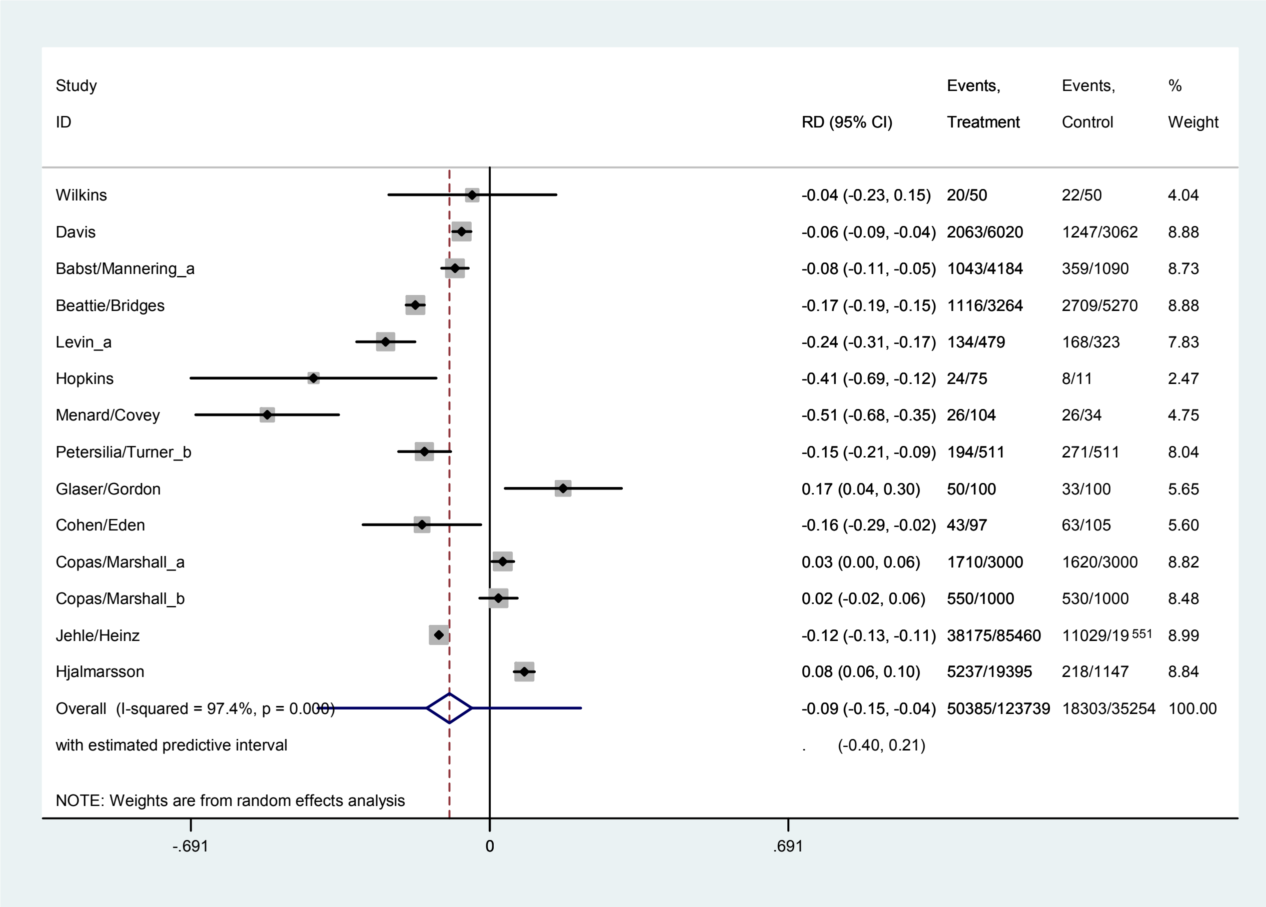

Figure 1
Meta Study, Forest Plot

Given the extreme heterogeneity of field data, and given the vexing identification problem, we believe in the complementary value of the experimental method. As will become clear when presenting the design and the results, the inherent limitation of the experimental method is its major strength. It abstracts from the obviously much richer context, to test the key difference between prison and probation: if a convict is put on probation, effective punishment is conditional on a second sanction. We show that conditional punishment is indeed less effective in realigning behavior with social expectations than unconditional punishment.

3. Experimental Method

SETUP: In February 2009, 96 students were randomly recruited from the *B*onn*E*con*L*ab's pool of about 3,500 subjects (from all kinds of majors) and participated in one of the two different treatments described below. We have 12 independent observations per treatment. After subjects arrived in the lab, they were randomly and anonymously allocated into matching groups. Subjects then received a written copy of the instructions. Additionally, in order to create common information about the instructions, we read them out aloud to our subjects. The instructions were written in neutral language, avoiding words like "punishment", "sanctions", "prison", or "probation" to avoid framing and demand effects.[14] Before the game started, participants had to answer a set of control questions to make sure that everybody had understood the rules of the game. The experiment lasted for approximately 60 minutes. Subjects were paid according to their cumulated period payoffs at a rate of 3 Eurocents per token. Participants earned about €13 on average, including a show-up fee of €4. The experiment was programmed in zTree (Fischbacher 2007) and participants were recruited using ORSEE (Greiner 2003).

DESIGN: The basic structure of our treatments follows the well-established design of a repeated linear public-goods game dating back already to the early 1980s; (see Isaac, Walker et al. 1984; Yamagishi 1986; Ostrom, Walker et al. 1992; Fehr and Gächter 2000; Fehr and Gächter 2002). In our version of this game, four players form a group and (anonymously) interact repeatedly for 10 periods. In each period, each player receives an endowment of 20 tokens. They have to decide independently, but simultaneously, how much of their endowment they want to contribute to a public good. The sum of contributions is multiplied by a factor of 1.6 and then split equally across all four group members. Therefore, if player *i* (*i*=1, …, 4) contributes c_i, her payoff π_i in a given period is determined as follows:

$$\pi_i = 20 - c_i + 0.4\sum_{j=1}^{4} c_j$$

As can be seen immediately, players' individual and joint interests are in conflict here. If all players were to cooperate fully and contribute their entire endowment, each of them would end up with 32 tokens. However, each player has the incentive to "misbehave" by reducing her own contribution: each token not contributed to the public good increases her individual earnings by 0.6 but deprives each of the other three players in the group of a gain of 0.4 tokens.

In each period our game features an additional second stage to allow for enforcement of contributions in the social dilemma. In this second stage, players are informed about each others' contribution decisions and can then decide to "punish" the others by assigning punishment points to them. The exact effects of punishment points vary with our treatments.

TREATMENTS: In our *Direct* treatment, which builds on the design of Fehr and Gächter (2002), each assigned punishment point leads to an immediate sanction of 3 tokens, i.e., it immediately reduces the punishee's payoff by 3 tokens. Therefore, the final payoff function for each period is given by:

$$\pi_i = 20 - c_i + 0.4\sum_{j=1}^{4} c_j - \sum_{i \neq j} p_{ij} - 3\sum_{i \neq j} p_{ji}$$

where p_{ij} denotes the punishment points player *i* gives to the other players *j*, and p_{ji} denotes the punishment points player *i* receives from the other players *j*. This institution is meant to capture the essence of a direct sentence, e.g., prison or a large fine.

14 Instead, we used terms like "to assign points", "direct points", "pending points", "transfer to a project", etc, which have been previously used in comparable studies. An English translation of the German instructions is included in the Appendix. The instructions in German are available from the authors on request.

In our second treatment *Probation*, the effect of each assigned punishment point depends on the current state of the punishee. i) If she did not receive any punishment points during the last three periods, she is put on probation. This means that the corresponding sanctions (the deduction of 3 tokens per punishment points, $-3\sum_{i \neq j} p_{ji}$) are pending. They are only carried out if she again receives punishment within the next three periods; otherwise, the sanctions are not applied and erased (the slate is wiped clean). Nonetheless, the cost of punishment is directly subtracted from the punisher's period income. That way we make sure that differences between treatments are not driven by differences in the cost functions. ii) If the punishee already was on probation, the new punishment points not only trigger the pending sanctions, but the corresponding sanctions of the new punishment points immediately become effective as well. This institution is meant to capture the essence of probation. As is standard in this literature, participants cannot identify each other across periods. Hence if A punishes B, she cannot know whether B is on probation or not. That way we make sure that punishment decisions are indeed independent of each other, and that the second punishment is not driven by the desire to make earlier punishment effective or vice versa, that the decision not to punish in the following period is not driven by the concern that total punishment might be excessive.

PREDICTIONS: In both treatments, punishment can be used to discipline another player.[15] Therefore, it will be interesting to see how those players who receive punishment points behave in the subsequent period. Given previous data from experiments that were run in the baseline design (Yamagishi 1986; Ostrom, Walker et al. 1992; Fehr and Gächter 2002; Nikiforakis and Normann 2008), we should expect that punishment is able to discipline free-riders. Thus, punishees are expected to react by increasing their contribution in the next period.

While this might also hold true in *Probation*, consider that the incentive structure differs between the two treatments. The situations are strategically similar for a player only if she is already on probation; actually then the disincentive to misbehave is even stronger than in the *Direct* treatment since earlier punishment is triggered together with newly received current punishment. If a person has not been on probation, the consequences of punishment differ, because sanctions are immediately triggered in *Direct*, while the person can be sure not to experience immediate punishment in *Probation*. So if we hypothesize that not meting out punishment *per se*, but the actual enforcement, induce players to contribute more, we should expect a difference in behavior under the two treatments. The difference should be particularly strong after a player's probation period has expired in *Probation*. If she now misbehaves, she again does not need to fear losing money immediately.

15 The interested reader should notice that, according to standard economic theory, homo oeconomicus should not be expected to punish at all in this game. To see why, consider that no one should punish in the last period, because it is costly to carry out punishment. Since no punishment should be expected, one should not contribute to the public good. Using backward induction, it turns out that the only subgame-perfect Nash equilibrium is indeed zero contribution and zero punishment by any player in any period. However, given the overwhelming empirical evidence from previous work, it would be naïve to expect no player ever to punish in this game.

4. Results

Table 3 descriptively compares treatments *Direct* and *Probation*; in the Appendix we also provide graphs, both per treatment and for individual groups (Figures 6 and 7). Our main result becomes immediately visible. Conditional sanctions are less effective than unconditional sanctions. Contributions and net profits are lower, punishment is higher, inequality (measured by the difference between the lowest payoff and the highest payoff per group and period) is larger.

	Contribution	Net profit	Inequality	Punishment
Direct	15.615	26.194	5.875	0.794
Probation	13.163	23.891	15.583	1.275

Table 3
Descriptives

4.1 Contributions

Figure 2 shows mean contributions over time in the presence and in the absence of direct sanctions. As can be seen, average contributions in *Probation* are below *Direct* in each period. Consequently, the institution of probation is less effective than direct punishment in aligning behavior with the social optimum, which would be to contribute the entire endowment.

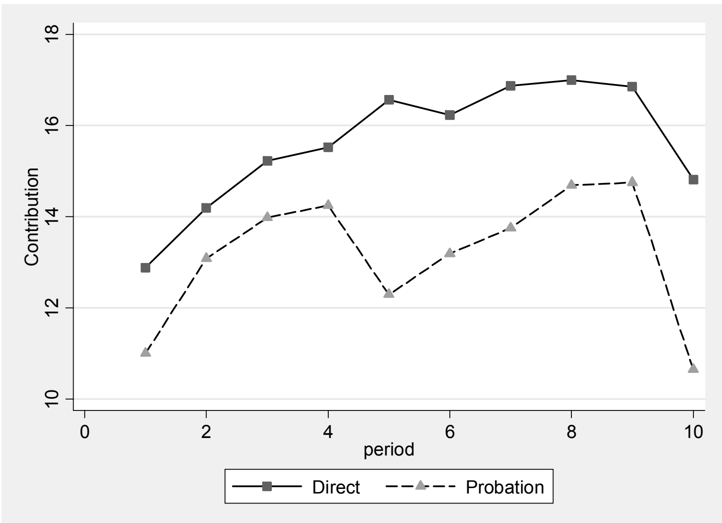

Figure 2
Contributions

Contributions move up over the first four periods in both treatments, but on average they are always higher in *Direct*, with the difference growing over time. In total, the mean degree of cooperation (measured as the fraction of the endowment that is contributed) is 20% higher in *Direct* than in *Probation*. It equals approximately 78% of possible contributions in *Direct* and 66% in

Probation. However, if we compare matching-group means over all ten periods using a non-parametric test, we do not find a significant difference (Mann Whitney, N=24, p=.194, two-sided). This seems to be driven by two features of our data. We observe strong censoring at 20 in both treatments (in *Direct*, in 46 % of all cases, participants contribute their entire endowment of 20, in *Probation*, this happens in 39 %); groups are fairly heterogenous.[16] To control for both features, we run a random effects Tobit model with group fixed effects. Using this procedure, we establish substantially and significantly lower contributions in *Probation*, Table 4.

	Contribution
Probation	-5.638*
Period	.746***
Period 10	-6.103***
cons	20.332***
N	960
Wald chi square	423.17
p model	<.001

Table 4
Explaining Contributions
*** p < .001, ** p < .01, * p < .05
The dependent variable is contributions per participant and period. *Probation* is a dummy which equals 1 for treatment *Probation* and 0 for treatment *Direct*. Period captures the time trend by indicating periods 1 to 10. Period 10 takes care of the endgame effect. We estimate a random effects Tobit model, with upper limit 20. Estimates for group dummies not reported.

The kink in period 4 of the curve for the *Probation* treatment in Figure 1 results from the fact that many participants have been punished in the first period, increase their contributions in the following three periods to prevent punishment from becoming effective, but start misbehaving as soon as the punishment period has elapsed. The remaining group members do not seem to appreciate this behavior. Low contributors are punished again, and once more improve their contributions. As Table 5 shows, in treatment *Probation* those who had been punished in period 1 are much more likely to be punished again in period 4 than in treatment *Direct*.

	Direct	*Probation*
not punished in period 4	11	8
punished in period 4	5	17

Table 5
Punishment in Period 4, if Subject had been Punished in Period 1
Fisher's exact, two-sided, N = 41, p = 0.029

Result 1: The Direct institution elicits higher contributions than the Probation institution.

16 Compare also the separate plots of the matching groups in Figure 6 of the Appendix.

4.2 Punishment

The mean number of assigned punishment points is significantly higher in *Probation* than in *Direct* (1.275 compared to .794, Mann Whitney, N=24, p=0.0079, two-sided). As a consequence in the absence of direct sentences, the total cost spent on punishment increases by more than 60%!

In public goods experiments, as in reality, most participants do not punish at random or out of spite. They react to what they perceive as undesirable behavior. In the experimental setting, the most likely motive for punishment is a difference between punisher and punishee contributions. Does the treatment difference survive if we control for this driver of punishment? Figure 3 provides a summary of punishment behavior, conditional on the difference between punishee's and punisher's contributions. In *Direct*, participants react more vigorously to contributions that are much lower than their own.

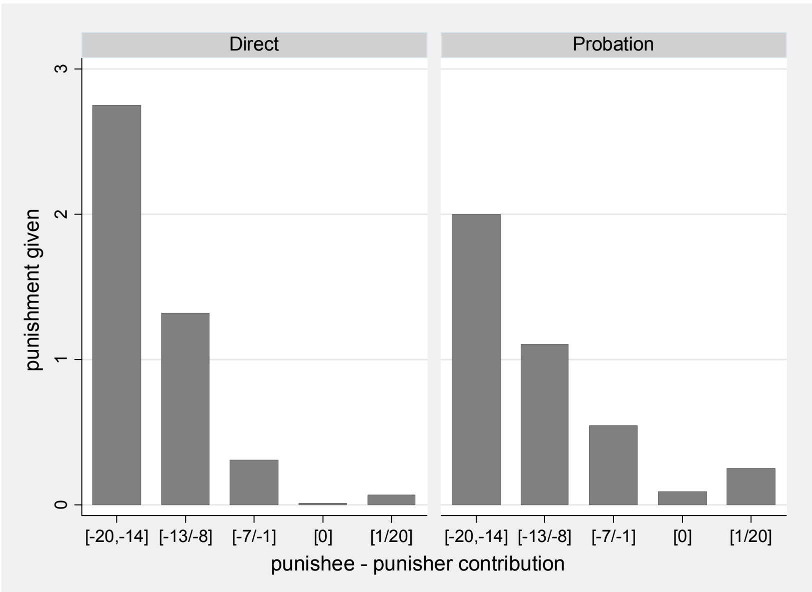

Figure 3
Deviation from Punisher's Contribution and Corresponding Punishment

Each period, each participant has the possibility to punish each of the remaining group members. In *Direct*, participants only use 11.18 % of these punishment options, compared to 21.53 % in *Probation*. This difference is significant (Mann Whitney, N = 24, p = .0055, two-sided). Conditional on punishing at all, the average punishment per unit of contribution difference is lower in *Probation* (Mann Whitney, N = 24, p = .0094, two-sided).[17] The regression reported in Table 6 disentangles the treatment effect from the effect of differences in contributions. Even if we hold the difference between punishers' and punishees' contributions constant, players are significantly more likely to punish in *Probation*. Interestingly, while punishment is more likely in *Probation*, in this treatment the decision to punish is less sensitive to the difference between

17 The test variable is [given punishment /(other's contribution – own contribution)].

punisher and punishee contributions, as demonstrated by the significant interaction effect.[18] Conditional on the decision to punish, the main effect of treatment *Probation* even disappears. Yet there remains a significant interaction effect. Sensitivity to contribution differences is only half as pronounced in *Probation* as in *Direct*: in *Direct*, only the main effect (-.132) matters; in *Probation*, overall sensitivity is given by -.132 [main effect] + .074 [interaction effect] = -.058.

	(1) Decision to punish	(2) Amount of punishment, conditional on being punished
Probation	1.938**	.376
Difference between punisher's and punishee's contribution	.436***	.132***
Probation*difference of contributions	-.167***	-.074***
Cons	-4.966***	1.414***
N	2880	471
Wald chi square	139.20	109.46
p model	<.001	<.001

Table 6

Explaining Punishment

*** p < .001, ** p < .01, * p < .05. In Model 1 the dependent variable is the decision to punish. Each period, each participant takes three such decisions. Probation is a dummy which equals 1 for treatment Probation and 0 for treatment Direct. Probation*difference of contributions is the interaction term between treatment Probation and the difference between punisher and punishee contributions. Since the dependent variable is binary, we estimate a logit model. We capture relatedness of observations by a random effects model, with standard errors clustered for period, nested in subject, nested in group.

In Model 2 the dependent variable is the number of punishment points inflicted on each other individual member of the group in each period, conditional on the punishee being punished by this punisher. We estimate a random effects model, with standard errors clustered for period, nested in subject, nested in group.

Result 2: Both in absolute terms and if one controls for the difference between own and others' contributions, in treatment Probation punishment is more frequent and less sensitive to differences between punisher and punishee contributions.

4.3 Reactions to Punishment

To gain a better understanding why we observe differences in contributions let us have a closer look at the reactions to punishment in the different treatments. In treatment *Direct*, whenever a person receives punishment, she is immediately sanctioned. If we check how an individual changes her cooperation level afterwards, we see that direct sentences seem to work pretty well. Individuals' cooperation increases by 64 percentage points in the subsequent period, while subjects on average decrease their cooperation by about 3 percent if they have not been punished in the previous period.[19]

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18 Note that a negative difference between punishee and punisher contributions indicates that the punishee has contributed less. Hence the negative main effect indicates that the smaller the punishee's contribution, compared to the punisher's contribution, the more the punisher is likely to punish. The positive interaction effect shows that this sensitivity to the difference in contributions is smaller in Probation.

19 The difference in change of contributions is significant at p=.0022 (signrank-test, N = 12, two-sided, comparing means per group in treatment Direct).

In treatment *Probation*, we need to distinguish between four situations: in the first, a participant is neither on probation, nor has she been punished in the previous period, nor has her probation period ended in the last period; then, on average, the person reduces her contributions by almost 14 %. In the second situation, the person is newly put on probation. In this situation, participants increase their contributions by 36 %. In the third situation, she is on probation the second or the third period. Then participants on average reduce their contributions by 3 %. Finally in the fourth situation, the probation period is over, be that because punishment has become effective or because the participant has not been punished a second time during three periods; then in the subsequent period, she *reduces* her contributions on average by 26 %.[20]

It is even more informative to compare the distribution of changes. Figure 4 looks at two situations: a player either has not been punished at all, or punishment has become effective. In *Direct,* this happens whenever the player has received punishment. In *Probation,* this requires that the player is punished a second time while on probation. The differences are striking. In *Probation*, when they are not effectively punished, players are considerably more likely to increase their contributions. However, after punishment has become effective, in *Probation* increases in contributions are rare, while decreases are frequent. The opposite is true in *Direct*.

20 Signrank tests over mean changes in contributions per group (N = 12), comparing these four situations, have the following two-sided p-values:

	newly on probation	extended probation	effectively punished
no sanction	.0022	.0029	.8753
newly on probation	-	.0029	.0150
extended probation	-	-	.2721

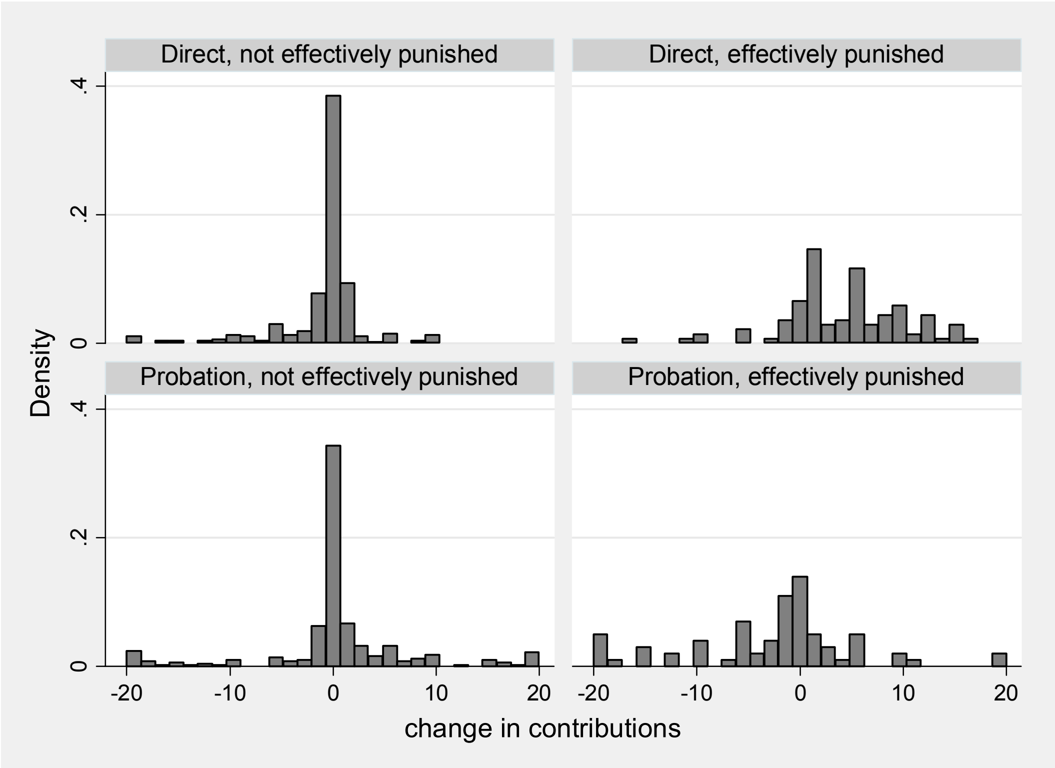

Figure 4
Reaction to Sanctions across Treatments

Figure 5 distinguishes the four situations present in *Probation*. If they receive no sanction, more than 60 % of participants contribute in the following period as much as they contributed in the previous period. If they change their contribution level, they rather tend to decrease contributions. If they have newly been put on probation, players strongly increase their contributions. Almost no player decreases contributions in this period. Extended probation requires that a player had already been on probation for at least one period. Almost 70 % of players who had already been on probation in the previous period do not change their contributions in the subsequent probation periods. They thus stabilize their contributions at the high level induced by the introduction of probation. Some participants increase their contribution levels in later probation periods, yet more subjects seem to adjust their contribution level downwards. However, players strongly *decrease* contributions after punishment has become effective, i.e. after they again have a clean slate.

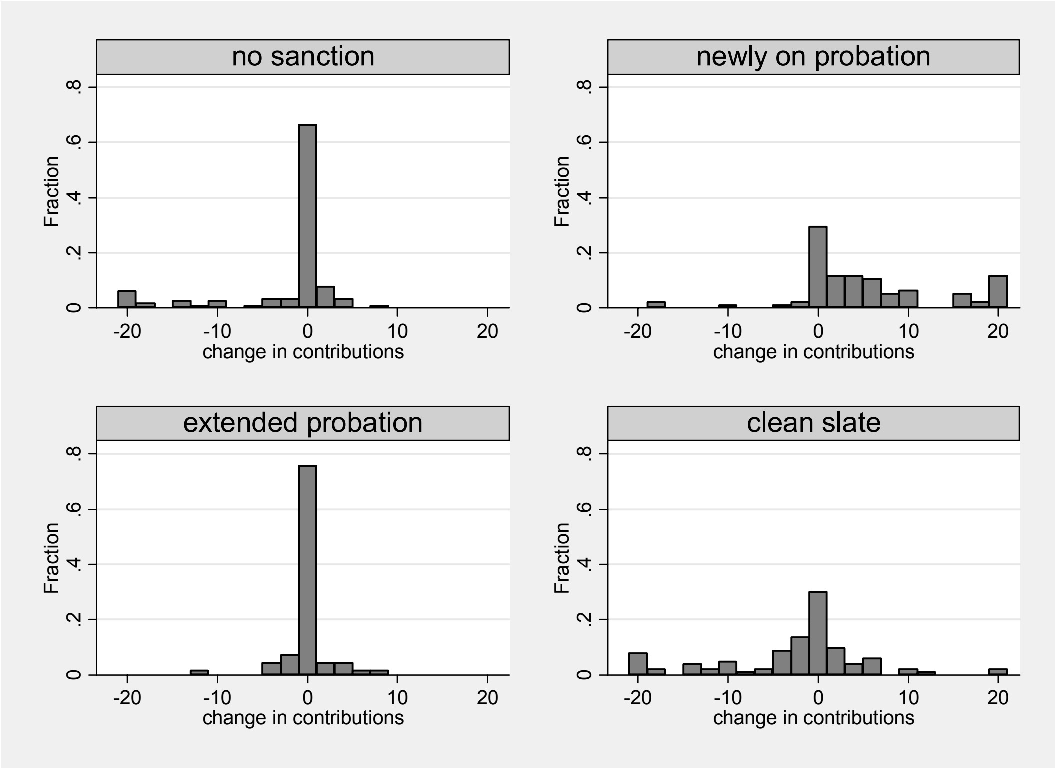

Figure 5
Reaction to Sanctions in *Probation*

Result 3: Probation seems to have a beneficial and a detrimental effect on punishees' behavior. They raise contributions while on probation, in particular immediately after the beginning of the probation period, but they start misbehaving again once the probation period is over. Moreover, those who are not on probation and have not been punished in the previous period decrease their contributions, whereas no such reduction is discernible in Direct.

4.4 Efficiency and Equality

From a normative perspective, two further measures are of interest. Which institution is better for total (monetary) welfare? And which institution leads to a more equitable distribution of income? The former we measure by aggregate net profit; note that net profit takes the cost of punishment, i.e. the cost of the institution, into account; while it abstracts from the costs of installing and keeping up the institution. Equity we measure by the difference between minimum and maximum profit per group and period. In *Direct*, net profit is on average 26.194, while it is only 23.891 in *Probation*. This difference is weakly significant (Mann Whitney $N = 24$, $p = .0735$, two-sided). In *Direct*, the mean difference between the highest and the lowest payoff is only 5.875, while in *Probation*, it is 15.583. This difference is strongly significant (Mann Whitney $N = 24$, $p = .0003$, two-sided).

Result 4: Direct outperforms Probation in terms of efficiency and of equality.

5. Conclusions

Our results can be read in different ways. Those interested in the comparative effectiveness of conditional versus unconditional sanctions get a clear message: if sanctions are only conditional, there is less norm compliance; we observe this result, although there is more punishment; the total transaction cost are higher, resulting from the effort needed to mete out punishment plus the damage incurred by the targets of punishment; the effects are less just, as measured by spreads of earnings. We thus have isolated the effects of a key feature of probation on behavior: while a person is on probation, punishment is conditional on being sanctioned for recidivism. Our results suggest that probation does not necessarily pay a double dividend. At least in the clean setting of the lab, Franz von Liszt did not have it right. While most of the field studies yield the opposite result, our lab study provides additional support to the most recent, and the most sophisticated field study, which shows a clear disadvantage of probation over prison, in terms of recidivism (Hjalmarsson 2008). Taken together, the detrimental effect of misbehaving after probation has ended and the beneficial effect of being cautious while on probation more or less add up to the same effect as without probation. However, on average, contributions are lower in the probation treatment. The lower overall performance of *Probation* results from the fact that those currently not punished are not effectively deterred. It seems that they put less trust in the institution. Conditional cooperators are less willing to run the risk of being exploited. Interestingly, if we had taken a similar approach to what is usually done when field data is at hand, namely to only look at recidivism as a binary variable, we might have ended up with a much less nuanced conclusion. In that case, we would have simply noted that, in *Probation*, participants are on average punished 4.35 times, while they are only punished 2.31 times in *Direct*. In our view, this further underlines how the laboratory wind-tunnel can open up new perspectives and provide new insights (not only) in this specific context.

There is a different way of reading our evidence. For the reasons listed in the introduction, probation may be preferable to prison. Society may consider the collateral damage from putting a convict into jail or prison to be far greater than the benefit from incapacitation, resocialization or deterrence. Or putting even more people in detention may simply be unaffordable for society (these are factors from which we needed to abstract in the lab experiment to isolate the effect of conditional sanctions). It is then of interest how strongly behavior is likely to react even if sanctions are only conditional. To such policymakers, we have a number of rather comforting messages. Even if punishment is only conditional, contributions nonetheless increase over time. Punishment remains an effective technology for realigning behavior with social expectations. Punishment does not become pointless. Probation has an independent beneficial effect: while a person is on probation, she increases her contributions to the public good, to preempt conditional punishment becoming effective. The effect is strongest directly after a person has been put on probation. Descriptively, the beneficial effect only decays very slowly as long as the person remains on probation. If one properly controls for the remaining determinants of behavior, the effect even remains positive in later probation periods. Subjects continue to improve their behavior slightly. In absolute terms, the beneficial effect of probation is quite strong. Policymakers might exploit the effect when they determine the duration of the probation period.

Are our findings relevant for criminal policy and for the interpretation of criminal law? This of course depends on the trust one is willing to put in the scientific method. An experiment can only shed light on those features of the real life phenomenon between which its treatments discriminate. Yet who would want to deny that this is the characteristic difference between prison and probation: if a convict is granted probation, incarceration is conditional on a second sanction. This basic difference may well be modulated by the concrete conditions of either prison or probation, by the activities of the criminal system that decides upon both, by individual and social perception, and by a host of other contributing factors. But we are convinced that these moderating factors are not likely to mute the fundamental difference we have established.

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Appendix

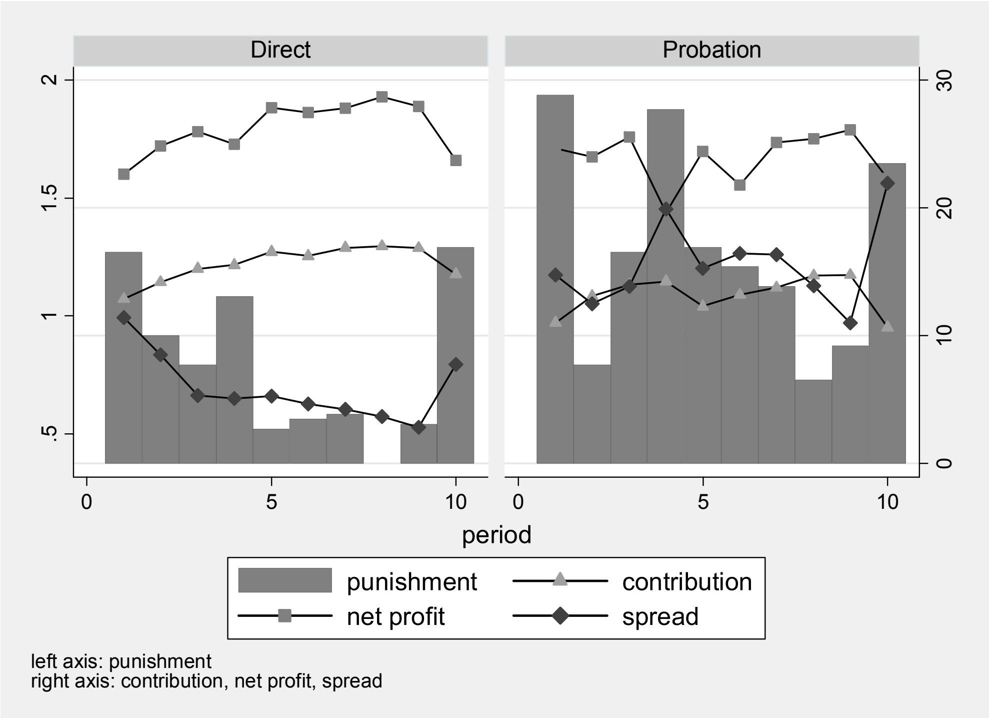

Figure 6
Descriptives per Treatment

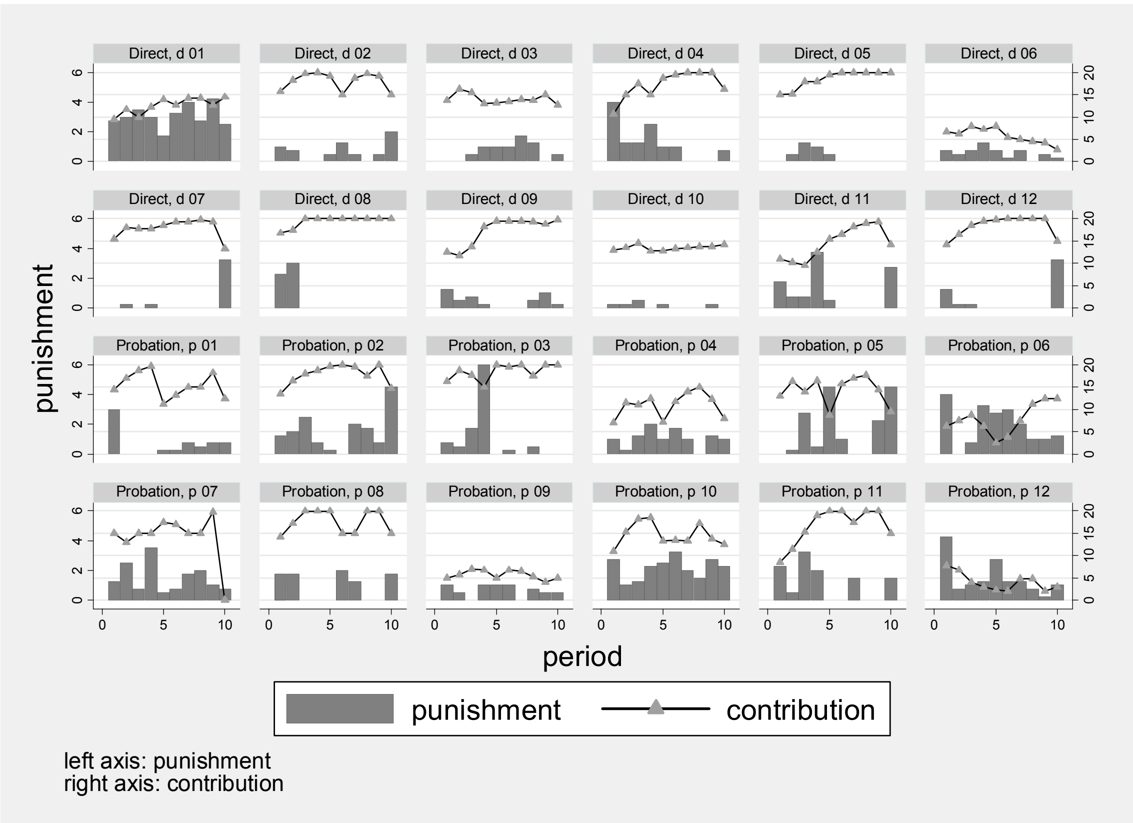

Figure 7
Individual Matching Groups

Instructions

Treatment *Direct*

General Instructions to the participants

You are now participating in an economic experiment. If you read the following explanations carefully, you'll be able to earn a considerable amount of money – depending on your decisions and those of the other participants. Therefore it is important to actually read the instructions very carefully.

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

For showing up today you will be paid 4 Euro. In addition, each participant receives a one-off lump sum payment of 50 Taler which increases or decreases according to the payoffs you receive during the present experiment. The experimental payoffs will be calculated in Taler. The total amount of Taler that you have accumulated during the experiment will be converted into Euro at the end of the experiment at an exchange rate of

1 Taler = 3 Eurocent.

At the end of the experiment your entire earnings from the experiment in Taler (converted into Euro) plus the show-up fee of 4 Euro will be paid to you in **cash**.

The experiment is divided into different periods. In total, the experiment consists of **10 periods**. Participants are divided into groups of four. You will therefore be in a group with 3 other participants. The composition of the groups will **stay the same** for all ten periods.

Each participant has an identification number (1, 2, 3 or 4) in each period. The identification number for each group member randomly changes in each round. Group members cannot be identified across periods.

Detailed Information on the Experiment

Each of the 10 periods consists of **two stages.**

<u>The first stage:</u>

At the beginning of each period each participant receives **20 Taler**. We call this his or her endowment. Your task is to decide how to use your endowment. You have to decide how many of the 20 tokens you want to contribute to a **project** and how many of them to **keep for yourself**. The consequences of your decision are explained in detail below.

You decide on the following input screen that appears at the beginning of each period.

The input screen

Period 1 of 10

In this period you are group member 1
For this period you receive an endowment of 20 Taler
Out of these 20 Taler, I want to ….
….contribute to the project

[Treatment **Probation**: The number of pending points is: XX]

Continue

The period number appears in the top left corner of the screen. Underneath you find the identification number that randomly changes in each round.

Below the identification number you find your **endowment (20 tokens in each period)**. You have to decide how many tokens you want to contribute to the project by entering an integer between 0 and 20 into the input field.

This field can be reached by clicking it with the mouse. By deciding on how many points to contribute to the project, you also decide how many points you keep for yourself, namely (**20 – your contribution**) Taler. After having entered your contribution you must click the "Continue" button (using your mouse). Having done this, you cannot revise your decision any more for this period.

[Treatment **Probation**:
Below the input field you see the number of "pending points". We will explain the notion of "pending points" later on.]

After all members of your group have made their decision, the following income screen will show you the total amount of tokens contributed to the project by **all** four group members (including your contribution). This screen also shows how many Taler you have earned at the first stage.

As you see your **income** consists of two parts:

(1) the Taler you have kept for yourself ("Income from retained Taler"),

(2) the "income from the project".

The income screen at the end of Stage 1

Period 1 of 10	
Stage 1	
Your contribution	XX
Sum of all contributions	XX
You kept for yourself	XX
Taler	
Income from the project	XX
Your total income in Taler at the end of Stage 1	XX
	Continue

This income from the project is calculated as follows:

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

Your **income in Taler** of a period is therefore:

(20 – your contribution to the project) + 0.4*(total contributions to the project)

The income of all other group member from the project is calculated in the same way, i.e., each group member receives the same income from the project. If, for example, the sum of the contributions of all group members is 60 Taler, then you and all other group members receive an income from the project of: 0.4*60 = 24 Taler. If the total contribution to the project is 9 tokens, then you and all other member of the group receive an income of 0.4*9 = 3.6 Taler from the project etc….

For each Taler that you keep for yourself, you earn an income of 1 Taler. If you instead contribute 1 Taler of your endowment to the project of your group, then the sum of contributions to the project rises by 1 Taler and your income from the project rises by 0.4*1=0.4 Taler. However, the income of the other group members also rises by 0.4 Taler each. Thus, the total income of the group rises by 1.6 Taler. Your contribution to the project therefore also raises the income of the other group members. On the other hand, you as well earn an income for each Taler contributed by the other members to the project. For each Taler contributed by any member, you earn 0.4*1=0.4 Taler.

The second stage:

At the second stage you can **reduce or leave equal** the income of **each** of the other group members by distributing **points**. The other group members can also reduce **your** income if they wish to. This is apparent from the input screen at the second stage:

The input screen at the second stage

Period 1 of 10

Stage 2

Group member	Contribution	Points
You	XX	
Group member 1	XX	
Group member 2	XX	
Group member 3	XX	

Your total income at the end of stage 1: XX

Continue

Here you see how much each group member contributed to the project. **Your contribution** is displayed in the "**You**" line, while the contributions of the other group members in each period are displayed in a new random order.

You must now decide for each group member (except for yourself) how many points to give to this member. You have to enter a number. If you do not wish to change the income of a specific group member, you must enter 0. If you choose a number larger than 0, you reduce the income of the respective group member. Within the column "points" you can move from one input field to another by pressing the Tab-key ((→/)) or by using the mouse.

If you distribute points, you incur costs in Taler which depend on the amount of points you distribute. Points **are integers between 0 and 10**. The more points you give to a group member the higher your costs are. The following formula shows the relation between points and the costs for distributing points in Taler.

Costs of distributed points = Sum of distributed points (in Taler).

Each point given costs you, therefore, one Taler. If you distribute, for instance, 2 points to a group member, you incur costs of 2 Taler. If, in addition, you distribute 9 points to another group member, you incur costs of 9 Taler. If you distribute 0 points to the last group member, you incur no costs. You distributed 11 points in total and your **total costs** are thus 11 (2+9+0). As long as you have not pressed the **Continue-button**, you can revise your decision.

If you choose 0 points for a particular group member, you do not change his or her income. If you give a member 1 point, however, (by choosing 1), you **reduce** his or her income by **3 Taler**. If you give a member 2 points (by choosing 2), you reduce his or her income by **6 Taler** etc. **Each point you give to a group member reduces this member's income by 3 Taler.**

By how much a group member's income is reduced in total depends on the sum of received points. If somebody, for instance, receives a total of **3 points** (from all other group members), his or her income will be reduced by **9 Taler**. If somebody receives a total of **4 points**, his or her income from the first stage will be reduced by **12 Taler** etc.

[From here to the calculation of total income Instructions for Treatment *Probation* differ; see below]

Your total income from both stages is calculated according to the following formula.

Income in Taler at the end of Stage 2 = period income
= income from Stage 1
– 3 times (the points you *received* in the current period)
– costs of your *distributed* points

After all participants have made their decision, your period income and your received **points will be displayed on the following screen:**

The income screen at the end of Stage 2

Period 1 of 10	
Stage 2	
Your income from Stage 1	XX
Your costs for distributing points	XX
Points received	XX
Taler deduction due to points received	XX
Your total income in Taler from this period	XX
	Continue

Your total income at the end of the experiment is calculated by summing over all period incomes according to the following formula.

Total income (in Taler) =

= 50 + Sum over period incomes if **it is not negative**
[otherwise you receive 0 Taler]

In addition you receive the show-up fee of 4 Euro.

Do you have any further questions?

[Instructions for Treatment *Probation*]

Points you or other participants distribute to a member that did not receive any points before do not become effective immediately but are **"pending"** for a maximum of 3 periods starting in the period after points have been distributed. The points are becoming effective as soon as a member within the next three periods is given another one or more points. In the latter case, the new **as well as** the pending points **immediately become effective** in the current period, i.e., the member's income is **reduced by three times the sum of the new as well as the pending points**. Points having become effective this way are erased afterwards. If a member does not receive any further points in the 3 periods during which his or her points are pending, the pending points lapse and are erased.

If you distribute points to a member that has no pending points in the current period, in analogy to what was said above, these points become pending for a maximum of 3 periods starting in the period after you distributed the points etc.

As group members cannot be identified across periods and as you do not know in general whether a member has pending points in the current period or not, you do not know in general whether the points you give to a member will make pending points effective. Moreover, you do not know in general whether the points you distribute to a member are pending or will immediately become effective. However, you know at all times how many pending points other group members have distributed to you and for how many periods these points are still pending.

Even if the points you distributed to a member are pending and do not become effective immediately – i.e., his or her income is not (immediately) reduced by 3 Taler per point given – you have to bear the costs of the points as early as the period in which you have distributed the points.

Your total income from both stages is calculated according to the following formula.

Income in Taler at the end of Stage 2 = period income

If in the current period no points have been given to you
= income from Stage 1
– cost of your *distributed* points

If in the beginning of the current period you had no *pending* points
= income from Stage 1
– cost of your *distributed* points

If in the beginning of the current period you had *pending* points and further points have been given to you in this period
= income from Stage 1
– 3 times *pending* points
– 3 times (the points you *received* in the current period)
– costs of your *distributed* points

After all participants have made their decisions, your period income and your received points will be displayed on the following screen:

The income screen at the end of Stage 2

Period 1 of 10

Your total income from Stage 1: XX

Group member	Points you received from group member
You	
Group member 1	XX
Group member 2	XX
Group member 3	XX

Taler deduction due to points having become effective in this period: XX

Your income in this period: XX

Continue

[The calculation of total income is the same in all two Treatments.]

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