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The Limits of Self-Governance when Cooperators Get Punished: Experimental Evidence from Urban and Rural Russia

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The limits of self-governance when cooperators get punished: Experimental evidence from urban and rural Russia[+]

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Abstract

We report evidence from public goods experiments with and without punishment which we conducted in Russia with 566 urban and rural participants of young and mature age cohorts. Russia is interesting for studying voluntary cooperation because of its long history of collectivism, and a huge urban-rural gap. In contrast to previous experiments we find no cooperation-enhancing effect of punishment. An important reason is that there is punishment of contributors in all four subject pools. Thus, punishment can also undermine the scope for self-governance in the sense of high levels of voluntary cooperation that are sustained by sanctioning free riders only.

JEL: H41; C91; D23; C72
Keywords: social norms, free riding, misdirected punishment, experiments.

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1. Introduction

Since the seminal papers by Yamagishi (1986) and Ostrom, et al. (1992) there is substantial experimental evidence that many people are willing to punish free riders at own costs. Punishment can mitigate the free rider problem and induce high cooperation levels.[1] Thus, a conclusion from these experiments is that pro-social motivations coupled with costly informal sanctions of free riders make "self-governance" in the sense of high levels of voluntary cooperation possible (Ostrom, et al. (1992); Ostrom (2000)).

In this paper we report evidence that informal punishment can limit successful self-governance. We derive this conclusion from public goods experiments with and without punishment that we conducted with four different subject pools in Russia. The participants were 566 urban and rural residents from all walks of life and two distinct age cohorts – young people with an average age of 20, and mature people aged 30 to 76, with an average age of 44. We observed substantial punishment not only of free riders, but also of people who contributed the same or more than the punishing subject.

The presence of punishment influenced voluntary contributions. A between-subjects treatment comparison among the urban mature participants shows that cooperation in the presence of a punishment option was even lower than in its absence. In the other three subject pools contributions were the same in the presence as in the absence of punishment. In experiments where participants played a public goods game with punishment after playing one without punishment, we observed that contributions decayed in all four subject pools – regardless of the presence of a punishment option. Contributions also declined in the reverse sequence.

We conclude from these results that punishment is not necessarily beneficial for cooperation but can also limit successful self-governance. Punishment of cooperators has been largely neglected in previous research on social preferences because it was negligible compared to the punishment of free riders.[2] Our results show that this neglect is not warranted because punishment of cooperators can be very significant in some subject pools. Moreover, our finding also has theoretical relevance, since in some widely applied social preference models preferences that would give rise to the punishment of cooperators are

[1] By now a host of publications analyze under what conditions punishment can increase and stabilize cooperation. See for instance Fehr and Gächter (2000), Fehr and Gächter (2002); Masclet, et al. (2003); Egas and Riedl (2008); Noussair and Tucker (2005); Page, et al. (2005); Bochet, et al. (2006); Gürerk, et al. (2006); Botelho, et al. (2007); Carpenter (2007); Kroll, et al. (2007); Ones and Putterman (2007); Sefton, et al. (2007); Denant-Boemont, et al. (2007); Gächter, et al. (2008); Herrmann, et al. (2008); Nikiforakis (2008); Nikiforakis and Normann (2008); Bochet and Putterman (2009); Carpenter, et al. (2009); Reuben and Riedl (2009); Rand, et al. (2009); Ule, et al. (2009); Nikiforakis (2010); and Sutter, et al. (forthcoming). See Gächter and Herrmann (2009) for a survey.

[2] The studies by Falk, et al. (2005) and Cinyabuguma, et al. (2006) are exceptions. The latter authors refer to the punishment of cooperators as "perverse punishment". Herrmann, et al. (2008) call it "antisocial punishment".

excluded by assumption (see, e.g., Fehr and Schmidt (1999) and the discussion of this fact in Falk, et al. (2005)).[3]

Our results on the importance of punishment of cooperators emerged from a research endeavor that investigates the extent to which the societal background shapes pro-social behavior and its enforcement via altruistic punishment. This endeavor was inspired by theoretical arguments that social preferences are endogenous to the societal environment (Bowles (1998)) and can be studied by laboratory experiments (Camerer and Fehr (2004)). We conducted our experiments in urban and rural Russia and among young and old age cohorts because these subject pools differ starkly from one another on many sociological and economic dimensions. In particular, our 2×2 factorial subject pool design allows us to study systematically two social background characteristics that might be relevant with respect to norms of cooperation and punishment. The two factors we are interested in are age and whether one is living in an anonymous urban or a close-knit rural area.

Age is interesting as there is growing evidence that pro-social behavior is linked to age (e.g., List (2004); Holm and Nystedt (2005); Bellemare and Kröger (2007); Sutter (2007); Sutter and Kocher (2007); Egas and Riedl (2008); Thöni, et al. (2009)). Moreover, our two age cohorts also differ in the extent to which they experienced collectivism. Our mature participants, in particular the older ones, were for a large part of their life exposed to a collectivist ideology and economy.[4] The experience of young urban and rural participants has been shaped less by communist ideology and more by the rocky transition to a market economy and the accompanying sociological changes. Among the changes are widespread perceptions of ubiquitous unfairness in the economic process and a lack of trust in the rule of law.[5]

The second dimension of our investigation is whether norms of cooperation are different between urban and rural people. There are two reasons why this might be so. First, due to several developmental lags, the gap between urban and rural areas is large in Russia (Fitzpatrick (1994); Spulber (2003)). This also holds true for the region of Kursk, where we conducted our experiments. In contrast, the urban-rural gap has largely vanished in western countries (e.g., Hofferth and Iceland (1998)). The rural areas were particularly strongly shaped by collectivism, because economic and social life was dominated by monopolist collective farms. Second, at a theoretical level, differences between groups can emerge easily

[3] Fehr and Schmidt (1999) assume that people dislike advantageous inequality (i.e., $\beta \geq 0$). However, given the parameters in our experiments, our observation that many people punish the cooperators implies that they *increase* rather than decrease the payoff differential. Such behavior is ruled out by assuming $\beta \geq 0$.

[4] In addition to being a collectivist economy (e.g., Spulber (2003); Gregory and Harrison (2005)), Russia was the longest-lived attempt to create a collectivist society where the individual, from the earliest childhood on, was supposed to pursue the interests of the group and to abandon the pursuit of self-interest (e.g., Clawson (1973)).

[5] See Shleifer and Treisman (2005) for a recent account on Russia's development, Brainerd (1998) on the rise of inequality, Fedotkin (2003) and Kluegel and Mason (2004) on fairness perceptions, and Hoff and Stiglitz (2004) on the rule of law.

due to an evolved psychology of "conformist transmission" (e.g., Henrich and Boyd (1998)). Moreover, norm enforcement is easier in close-knit parochial communities than in anonymous large groups with limited monitoring possibilities (see, e.g., Bowles and Gintis (2002) for theoretical arguments and Carpenter (2007) for experimental evidence). Rural residents might be used to different levels of social control and may therefore have internalized different norms of cooperation to those of their urban counterparts.

A further aspect of our four subject pools is that they share the same language, political system and the broader Russian background in general. Our factorial subject pool design allows us therefore to address the extent of differences between social groups that exist *within* a society – in our case between 'young' and 'mature' age cohorts and between urban and rural subject pools. While there are several studies by now that compare students and non-students or look at behavior of villagers (e.g., Barr (2001)), there is, to our knowledge, no study that investigates differences in social preferences between young and mature cohorts and urban and rural residents in a factorial design.[6]

Our most important results – in addition to the surprisingly high rate of punishment of cooperators and its effect on cooperation – are as follows. We find that rural residents and mature participants are more cooperative than urban residents and young people, respectively. While overall punishment behavior is not affected by socio-demographic variables, we find that highly educated people and people who are a member of at least one voluntary civic organization are more likely to punish cooperators.

2. Experimental design and procedures

2.1. Subject pools and locations of experiments

We devised a 2×2 factorial subject pool design, where we vary whether a subject is (i) at least 30 years ('mature'), or younger than 30 years ('young') and (ii) a rural or an urban resident. We document the most important details about our mature subject pools in Table 1; further information and details can be found in Appendix A.

In total we conducted 19 experimental sessions. We ran our experiments with the urban participants in the city of Kursk (roughly 430'000 inhabitants) and the neighboring city of Zheleznogorsk (approx. 100'000 inhabitants). Kursk and Zheleznogorsk are located in the heartland of the former Soviet Union, the so-called 'Central Black Earth Zone' 400 miles

[6] See, for instance, Fehr and List (2004); Carpenter, et al. (2005); Egas and Riedl (2008); Bellemare and Kröger (2007) and Carpenter and Seki (forthcoming) for recent studies that compared students to non-students within a given society. Herrmann and Thöni (2009) study conditional cooperation in four different student subject pools in Russia (and found not difference). List (2004) and Thöni, et al. (2009) study age effects (among other things) in voluntary contributions. Sutter and Kocher (2007) observe trust and trustworthiness across six different age cohorts.

south of Moscow. The 185 urban 'mature' participants (55 percent females) were between 30 and 68 years old, the average was 44.6 years and 25 percent were older than 50 years. In contrast to the rural participants, most of the urban mature participants had spent most of their lives in larger cities.[7] Sixty percent held white-collar jobs and 40 percent were blue-collar workers. Fifty percent held a university degree. The urban mature participants were strangers to one another. A participant knew on average only 3.4 percent of the other participants in a session.

We conducted the experiments with the rural residents in two small villages (Ivanovka and Sedmikhovka) in the area of Kursk. Although the city of Kursk is only 40 to 50 miles away rural life in these villages is very insular for a lack of good roads, cars, and modern communication technologies. The 92 rural mature participants (53 percent females) were between 30 and 70 years old; the average age was 43 years and 17 percent were older than 50 years. Thus, the urban and rural mature participants were similar with respect to age and gender composition (statistical comparisons confirm this as well). Fifty-eight percent were blue-collar workers and 42 percent had a white-collar job. Slightly less than a third of the rural mature participants had a university degree. While the urban participants were strangers to one another, the rural participants knew each other well. The average participant knew 43 percent of the other participants in a given session. The differences in the number of people's acquaintances in the different subject pools fits well with the dense personal networks in rural areas and the anonymity of urban living conditions. Most of the rural residents had actually spent most of their lives in the countryside. The difference in "city size", which measures the "urban-rural" background of participants, is highly significant and substantial (see Table 1).

The 140 urban young participants were mostly undergraduates from various universities and polytechnic institutes in the city of Kursk; twelve percent were non-students who mostly held blue-collar jobs. We recruited the majority of the 149 rural young participants in Ust-Kinel because we could only find 42 young rural volunteers in the area of Kursk.[8] In both young subject pools the average age was roughly 20 years; 95 percent were younger than 22 years. Among the urban (rural) young participants 21 (34) percent were females. On average, an urban (rural) young participant knew 9 (25) percent of the other participants in a session. Further details can be found in Appendix A.

[7] The variable "city size" measures the size of the city where the participant had spent most of his or her life. It contains four categories: (1) city size is up to 2'000 inhabitants; (2) between 2'000-10'000 inhabitants; (3) between 10'000-100'000 inhabitants and (4) more than 100'000 inhabitants.

[8] Ust-Kinel is a small village with roughly 5000 inhabitants, 700 miles east of Moscow. The participants all come from similar rural areas and share a similar socioeconomic background as our rural young participants of the Kursk region.

Table 1
Key figures about the mature subject pools (age ≥ 30).

	Urban mature (n=185)	Rural mature (n=92)	Statistical comparisons (p-value; test)
General background data			
Mean age in years	44.6 (8.6)	43.0 (9.7)	0.164 (t-test)
Percent female	55.0	53.3	0.337 (χ^2-test)
Mean of city size category	3.5 (0.8)	1.5 (0.8)	0.000 (χ^2-test)
Share of known participants (in percent)	3.5 (6.4)	43.1 (37.8)	0.000 (t-test)
Professions of participants (in percent)			0.001 (χ^2-test)
White collar	62.7	41.9	
Blue collar	37.3	58.1	
Education (highest level attained, in percent)			0.001 (χ^2-test)
Compulsory education	22.3	27.2	
Secondary school degree	19.0	39.1	
University degree	50.0	32.6	
No data	8.7	1.1	

Note: Numbers in parentheses are standard deviations. Further details can be found in Appendix A.

2.2. The decision situations

In all subject pools the decision situation was a one-shot public good experiment with and without punishment opportunities. Participants were divided into groups of $n = 3$ participants and endowed with 20 tokens. Participants decided simultaneously how many of these tokens to contribute to a public good, called "project". All participants received a marginal per capita return of 0.5 from any contribution to the public good, which was just the sum of all individual contributions c_i to the project. We chose a marginal per capita return of 0.5 to make the calculations for the participants easy. The monetary payoff π_i^1 in the one-stage public goods game without punishment for each subject i in the group was therefore given by

$$\pi_i^1 = 20 - c_i + 0.5\sum_{j=1}^{3} c_j \cdot$$

This payoff function is widely used in public goods experiments. It offers the participants a monetary incentive to free ride completely (i.e., to choose $c_i = 0$), since the marginal per capita return of a contribution to the public good is less than 1. The social marginal return is 1.5, which implies that the social payoff is maximized if everyone contributes his or her whole endowment to the public good.

A second decision stage was added under punishment conditions. Participants were informed about the contribution of the other two members of their group after the simultaneous investment decision of the first stage. Participants then had the possibility to

simultaneously punish their group members by assigning so-called "deduction points". The allocation of a deduction point p_{ij} by player i to player j reduced the first-stage payoff of player i by one token and that of player j by 3 tokens. If player i received p_{ji} deductions points from the other group members and assigned p_{ij} deduction points to member j, the final pecuniary payoff of subject i, π_i, was

$$\pi_i = \max[\pi_i^1 - 3\sum_{j=1}^{3} p_{ji}, 0] - \sum_{j=1}^{3} p_{ij}$$

(see also Fehr and Gächter (2002), who used exactly the same punishment function).[9,10] All participants played two one-shot games: one game without punishment (called N-experiment) and another game with the punishment option (called P-experiment). Participants interacted with the same group members in both games. We ran two sequences. In the N-P sequence participants first played the N-experiment and then the P-experiment. In the P-N sequence the order of experiments was reversed. Thus, our experiment is a within-subject design controlled for sequence effects (Davis and Holt (1993)). Participants learned that the first experiment takes place only once. Participants were also not told that there would be another experiment. Only after participants had finished the first experiment were they informed that there would be another *one-shot* experiment after which the whole experiment would be finished. Group composition stayed the same and participants were aware of this.[11]

2.3. Discussion of the design

Our design has two main purposes. First, we want to measure the participants' initial cooperative attitudes in a situation that is not confounded with strategic considerations

[9] This punishment function permits losses but only due to a subject's own punishment; losses inflicted by the punishment received by others were not possible (in 8.25 percent of the cases subjects had a loss in the P-experiment). Bankruptcy was no issue because (i) participants received a show-up fee which covered self-inflicted losses and (ii) also participated in an experiment without punishment were losses were impossible by design. The instructions did not mention that self-inflicted losses would have to be covered by the show-up fee but it was explained verbally to any subject who asked about it.

[10] The choice of punishment function is important for the impact on cooperation one can expect. The existing evidence (from sequences of one-shot games) shows that for our chosen parameters contributions increase significantly relative to a no-punishment benchmark (see e.g., Fehr and Gächter (2002) and in particular Egas and Riedl (2008) and Nikiforakis and Normann (2008) who provide comparative-static evidence of the impact of punishment effectiveness on cooperation).

[11] The rationale for this design choice is as follows. Keeping the pairing fixed maximizes the number of independent observations we can use for testing whether adding or removing a punishment opportunity has a significant effect on cooperation. Moreover, since we ran the experiments with non-students outside well-functioning university laboratories, we could not plan perfectly on the number of participants per session. Any other than the fixed matching procedure we applied would have created further logistical problems beyond those that already existed in organizing these experiments. Finally, since varying session sizes were quite likely, other matching procedures could have implied that (i) only the whole session would have constituted an independent observation and (ii) would also have implied that session statistics (like the mean) would have been based on different number of observations. Thus, the fixed matching was the obvious design choice and informing the participants about it ensured that we controlled their beliefs about whom they are matched with. Moreover, participants were told orally in public and in the instructions that the new experiment will be a one-shot game, so concerns that participants actually believe they might play a repeated game should not arise.

coming from repeated play. Therefore, we deliberately had the participants play each treatment only once. Take the N-experiment of the N-P sequence. Since the game is one-shot, each player has a dominant strategy to free ride if he or she only cares about own payoff. Thus, under conditions of anonymity, this game measures the *extent of initial non-strategic cooperativeness* that is present among our subject pools. In the P-experiment of the P-N sequence we measure two things, (i) the participants' degree of cooperation in the presence of a punishment option and (ii) the participants' punishment behavior. A selfish and rational subject will not punish, since punishment is costly and the game is one-shot. Yet, we know from numerous experiments that many people are prepared to punish free riding (see footnote 1). Almost all previous experiments involved repeated play, however. Our one-shot experiment is therefore a particularly demanding environment for observing cooperation and punishment.[12]

Second, our design also allows measuring the effect of introducing or removing a punishment opportunity. In the N-P sequence, for instance, we can measure how participants change their contribution from the observed level in the N-experiment, if we add the punishment option. In the P-N sequence we measure the effect of taking away the punishment option.

2.4. Procedures

We recruited the urban residents and villagers through announcements in factories, public places and in addition by approaching people on the streets and in public transport. Word-of-mouth also played a significant role and worked very well, in the sense that people from all walks of life participated in the experiments, in particular in Kursk. We recruited most of the young participants by announcements in universities. In the city of Kursk we conducted the experiments in lecture halls of the Academy of Agricultural Sciences. In Zheleznogorsk and the rural areas we ran the experiments in lecture halls of local schools. On average 31.9 people participated in a session.[13]

Moving beyond university student subject pools creates some challenges for the experimenter (see also Henrich, et al. (2004); Ortmann (2005)). The "experimenter demand effect" (see Zizzo (2010) for a recent discussion) is a potentially important issue which we tried to minimize by several measures. First, we used a neutral frame and conducted all experiments according to a detailed script that contained (i) the exact rules how to conduct the

[12] In the Fehr and Gächter (2000) and Fehr and Gächter (2002) experiments participants played the games with and without punishment repeatedly either with random matching or stable groups in each period (in Fehr and Gächter (2000)) or with a 'perfect stranger' matching (in Fehr and Gächter (2002)) that ensured that the same group members only interacted once. Our design can thus be seen as a one-shot version of these earlier designs. See also the one-shot experiments without punishment of List (2004) and the one-shot experiments with and without punishment of Walker and Halloran (2004), Cubitt, et al. (2008) and Gächter and Herrmann (2009).

[13] The nineteen sessions of our experiments took place between November 2001 and September 2002.

experiment and (ii) a summary of the rules, payoffs, and procedures which we read to the participants. Second, the team of assistants who helped conducting the experiments was always the same in all experiments. They were all Russians who did not know the experimental participants. We trained them well before the experiment. Third, one person, a Russian native and academic teacher at a university in Kursk, conducted all experiments to minimize the experimenter demand effect that may come from the German nationality of the principal investigator, Benedikt Herrmann. Fourth, all assistants and the lead experimenter received extensive training about the experimental procedures, and were supervised during the experiment by Benedikt Herrmann who speaks Russian fluently.

A further important challenge of any one-shot experiment is that participants understand the game. We took several steps to ensure this. First, we wrote the instructions in German and then had them translated into Russian and back into German (by another translator) to control for language-induced differences in meaning. Second, we used a neutral framing to control for possible framing effects that might also be different between our urban and rural subject pools. The instructions were very detailed and explained the calculation of rules and payoffs step by step. We also added several completed numerical examples to show how payoffs are calculated. Third, participants could read the instructions at their own pace. Participants could also ask questions at any time (in private). Fourth, participants had to answer a set of control questions that tested their understanding of payoff calculations. The huge majority of participants had no difficulty at all in understanding the rules of the experiment.[14] Fifth, the lead experimenter summarized the rules of the game and the payoff calculation (by reading a prepared script). Sixth, before the experiment actually started, there was a further possibility to ask questions. We document a sample copy of the instructions in Appendix B.

The experiments were hand-run. Cardboard partitions separated the participants to maximize the between-subject anonymity of decisions. We also took several steps to maximize subject-experimenter anonymity (see Hoffman, et al. (1996); Bohnet and Frey (1999)). First, given the country's past, we never asked for the names of our participants and made clear throughout that the sole purpose of this experiment is scientific. Second, participants had to submit their decision sheets in closed envelopes and received the results of the contribution decisions of their group in closed envelopes. Thus, the experimenter could not observe an individual's decision. Moreover, all the calculation of results and the preparation of information sheets were done in a separate room, by assistants other than those who collected the decision sheets. Finally, the participants received their payoffs

[14] Our research assistants helped those who had problems, according to strictly determined rules how to answer questions. Some participants were unable or unwilling to answer all questions correctly. For the data analysis we apply a conservative approach and discard all observations from these participants. In total, we discard the data from 40 participants. This leaves us with data from 566 participants (185 urban mature; 92 rural mature; 140 urban young; 149 rural young); 330 participated in the N-P sequence and 236 in the P-N sequence.

anonymously and in sealed envelopes. The participants were fully aware of all these 'almost double-blind' procedures.

We administered an anonymous questionnaire at the end of the experiments. We asked for (i) socio-demographic information (see Table 1 and Appendix A) and (ii) trust attitudes which we discuss in a separate paper (Gächter, et al. (2004)).

We aimed at paying participants on average roughly the equivalent of two days income, net of show-up fees. Since an experiment lasted on average two hours, the hourly wage was rather high and our participants were fairly excited about the level of stakes. On average our participants (who were paid in Rubles) earned the equivalent of $8.3 including a show-up fee of $3.[15]

3. Cooperation and punishment

3.1. Cooperation in the presence and absence of punishment opportunities in the first experiment

Result 1: *In the first experiment urban young participants were the least cooperative group. The presence of a punishment opportunity did not enhance cooperation. Among urban mature participants, cooperation in the P-experiment was even weakly significantly lower than in the N-experiment.*

Figure 1 contains the main support for Result 1. It depicts, separately for the subject pools, the histograms of contributions in the N- and P-experiments, respectively, when these experiments were played as first experiments (i.e., we look at contributions in the N-experiments of the N-P sequence and the P-experiments of the P-N sequence).

Figure 1 conveys three observations. First, contributions in the N-treatment are differently distributed between subject pools. The urban and rural mature participants contributed on average 10.7 and 10.5 tokens (i.e., 53.7 and 52.5 percent, respectively, of their endowment). The rural young participants contributed 50.6 percent and the urban young participants contributed 37.1 percent of their endowment. A non-parametric Kruskal-Wallis test which compares the distribution of contributions in one joint test reveals that these differences are significant between subject pools ($\chi^2(3)=10.89$, n=330; p=0.010).[16] The significant differences are due to the urban young participants. Thus, the cooperation rate by

[15] We did not treat urban and rural mature participants differently, although there are some cost of living differences between them (see http://www.socpol.ru/print.asp?f=/atlas/portraits/kursk.shtml). The reason is that many rural participants, who lived across various small villages in the area of Kursk had a long way to come to the experiment and therefore had higher opportunity costs of participating.

[16] In all Kruskal-Wallis tests reported in this section we used all individual contributions of participants who understood the control questions as independent observations. Using individual contributions is justified because participants made their contribution decisions simultaneously and therefore independently from one another.

the urban young participants provides a lower bound for the cooperativeness in our subject pools. The contribution rates of the other subject pools are not significantly different from one another (Kruskal-Wallis test, $\chi^2(2)=0.43$; n=267; p=0.806).

Second, we find no significant differences in cooperation rates between subject pools in the P-experiment (Kruskal-Wallis test, $\chi^2(3)=5.01$; n=236; p=0.171). Thus, the presence of a punishment option led to very similar contributions across subject pools. The reason for similar contributions in the P-experiment is that the urban young participants and rural mature participants had higher contributions in the P- than the N-experiment, whereas for the urban mature and rural young participants the opposite was true (see Figure 1). The third finding is that in all subject pools the presence of a punishment option in the P-experiment did not shift contribution rates to a statistically significantly higher level than in the N-experiment. Among the urban mature and the rural young participants contribution rates were on average even *lower* in the presence than in the absence of a sanctioning mechanism; this effect is weakly significant for the urban mature participants.

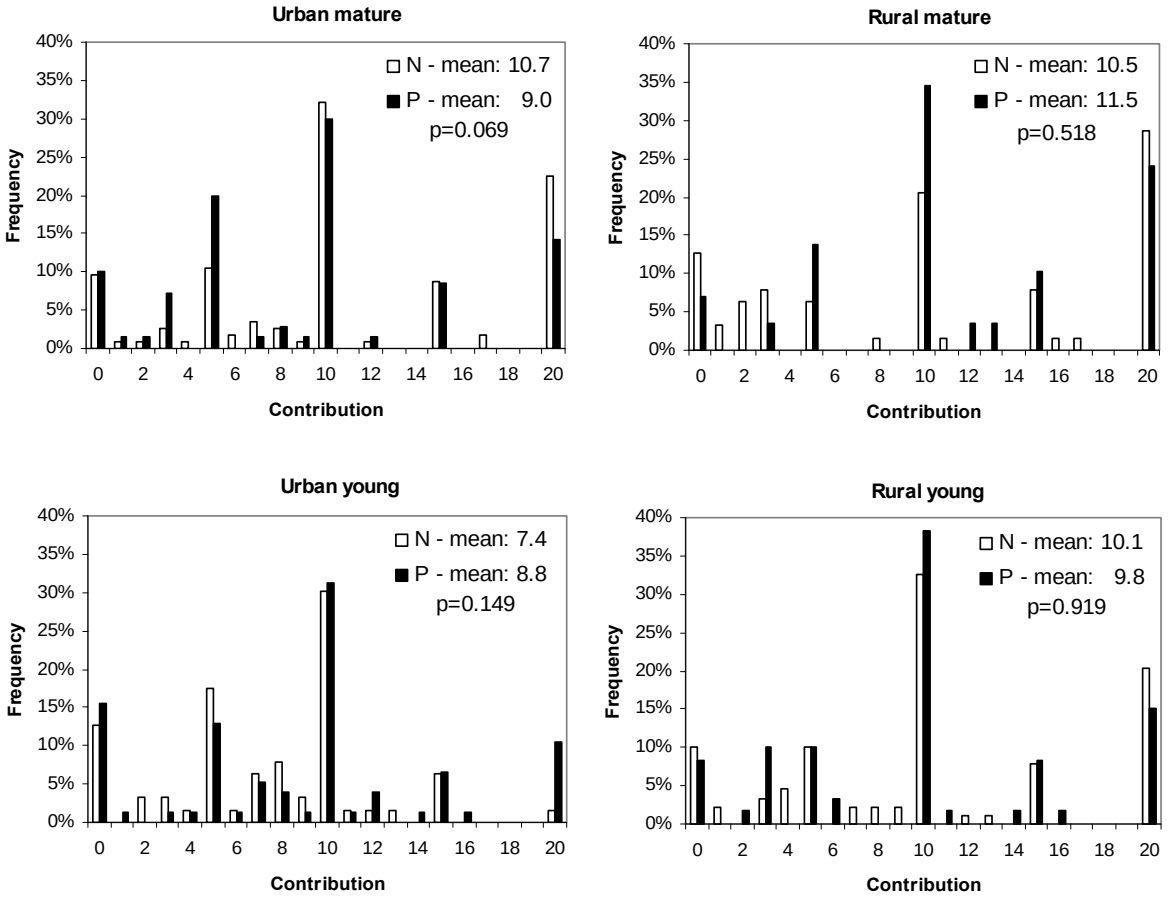

Fig. 1. Contributions in the first experiment in the no-punishment (N) and the punishment experiment (P).

Figure 1 reveals some interesting similarities and differences between subject pools. In the N-experiments the fraction of zero contributions was remarkably similar across subject pools. The most frequent contribution was 10 tokens, except for the rural mature participants where the modal contribution was 20 tokens. By contrast, among the urban young participants virtually nobody contributed 20 tokens. In the P-experiments the modal contribution was 10 tokens in all subject pools. The frequency of full contributions was lower in all subject pools in the P-experiment than in the N-experiments; with the exception of urban young participants. A particularly noteworthy observation is that among urban mature participants the frequency of low contributions was *higher* and the frequency of high contribution was *lower* in the presence of a punishment option than in its absence. Our analysis of punishment behavior below suggests that a likely explanation for this finding has to do with the punishment participants might have anticipated.

3.2. Reactions to changed opportunities to punish

Our design allows us investigate how a change in opportunities to punish influenced cooperation. We added punishment in the second experiment in the N-P sequence and we counterbalanced our within-subject design by removing the punishment opportunity in the second experiment in the P-N sequence. From previous experiments (e.g., Fehr and Gächter (2000); Fehr and Gächter (2002)) we predict that contributions in the N-P sequence increase in the P-experiment relative to the N-experiment and fall in the P-N sequence. Result 2 records the evidence.

Result 2: *In the N-P sequence contributions decayed significantly in the second experiment regardless of the presence of a punishment option. This result, which holds in all subject pools except in the rural mature subject pool, is in contrast to existing evidence. The reason for the decay was that in all subject pools the high contributors in the N-experiment reduced their contributions substantially whereas the low contributors in the N-experiment did not increase their contributions in the P-experiment.*

Figure 2 provides support for Result 2. The presence of a punishment opportunity is symbolized with a black diamond and the absence with a white circle. We also show the 95-percent confidence intervals of the mean contributions.

Contribution rates declined in the P-N sequence in line with the prediction based on previous literature. Among the urban participants this decline is significant at the 0.1-percent

level (two-sided Wilcoxon signed-ranks tests with group averages as observations), whereas among the rural participants the decline is insignificant.[17]

Contribution rates did not increase when punishment was added (i.e., in the N-P-sequence), which is contrary to predictions and reported evidence.[18] Contribution rates even decayed on average. The decay is highly significant in the urban mature and the rural young pools; weakly significant among the urban young participants and insignificant among the rural mature participants (two-sided Wilcoxon signed ranks tests with group averages as independent observations).

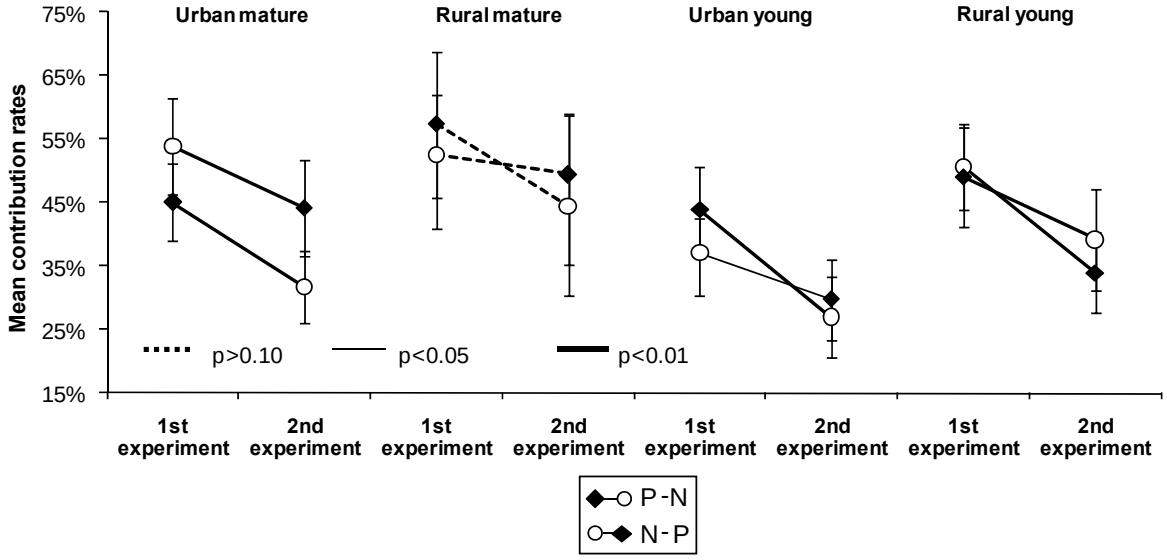

Fig. 2. Mean contributions (and 95-percent confidence intervals) under changing opportunities to punish in the N-P and the P-N experiments.
Note: The thickness of lines indicates the significance level of Wilcoxon-matched pairs tests.

3.3. Punishment behavior

We pool the data from the N-P and the P-N sequences and control in the statistical analyses for the order in which the punishment experiment occurred by adding a dummy variable 'Second experiment'.[19] We record the following result.

[17] The data reported in Figure 2 are from all participants who understood the instructions. For the statistical tests, however, we only looked at groups were all members in a group had answered all questions correctly.
[18] See the references in footnote 1. Cooperation is higher in the presence than the absence of punishment in all these experiments.
[19] Before we pooled the data we also ran Mann-Whitney tests as well as regression analyses to see whether there are differences in punishment patterns of the P-experiment in the P-N sequence and the N-P sequence, respectively. We ran the tests conditional on the deviation intervals reported in Figure 3. The only significant difference between sequences we found occurred for non-negative deviations among the rural mature participants.

Result 3: *We find in all subject pools substantial punishment of people who contributed less than the punishing individual but also of people who contributed more than the punishing individual. The expenditures on punishment of cooperators across subject pools ranged from 35 to 78 percent of the expenditures on punishment of free riders.*

Figure 3 and Table 2 provide the support for this result. Figure 3 shows the data from the perspective of a punishing subject. Figure 3 depicts for each subject pool the average punishment expenditures of a punishing subject as a function of the punished subject's deviation from the punisher's contribution at the first stage of the P-experiment. A punished subject's contribution in the first stage of the P-experiment can deviate from the punisher's contribution by –20 to +20 tokens. For expositional ease we divide this range into five intervals. For instance, if the difference is –15, this means that the punished subject contributed 15 tokens less than the punishing subject and we categorize the corresponding punishment act into the deviation interval [-20,-11]. If the difference is positive, the punished subject contributed more than the punisher. If the difference is zero, the punisher and the punished subject contributed exactly the same amount to the public good at the first stage. The figure also shows the 95-percent confidence bounds. We also indicate the degree of 'misdirected punishment' μ. μ denotes the ratio of mean expenditures on the punishment of non-negative deviations to the mean expenditures on punishment of negative deviations.

Figure 3 reveals a pattern of punishment that is quite different to that observed elsewhere, with the exception of the urban young participants. For instance, the mean pattern of punishment of urban mature participants is u-shaped. Strikingly, the urban mature participants also punished those who contributed the *same* (zero deviation) with more than one punishment point. Even more surprising is the observation that the urban mature participants also punished those who contributed *more* than they did. For instance, on average punishers expended almost two money units to punish those who contributed between 11 and 20 tokens more to the public good than they did. The urban mature participants also punished negative deviations, which is in line with the existing evidence. The more the punished subject's contribution fell *below* the punisher's contribution, the more strongly the deviator got punished. For rural mature participants the pattern was similar, although less pronounced.

We find that among the young participants the rural ones punished across the board. The punishment of urban young participants (which consists mostly of students) came closest to the punishment observed in western student subject pools (see the references in footnote 1). Yet, we find substantial punishment of contributors who contributed more than the punishing subject even among the urban young participants. Across subject pools the ratio of misdirected punishment, μ, varied between 35 percent among urban young participants and

78 percent among urban mature participants.[20] In other words, although less important than punishment of free riders, misdirected punishment was very substantial in our subject pools; across all subject pools the highest contributors in a group were punished in 47 percent of the cases.

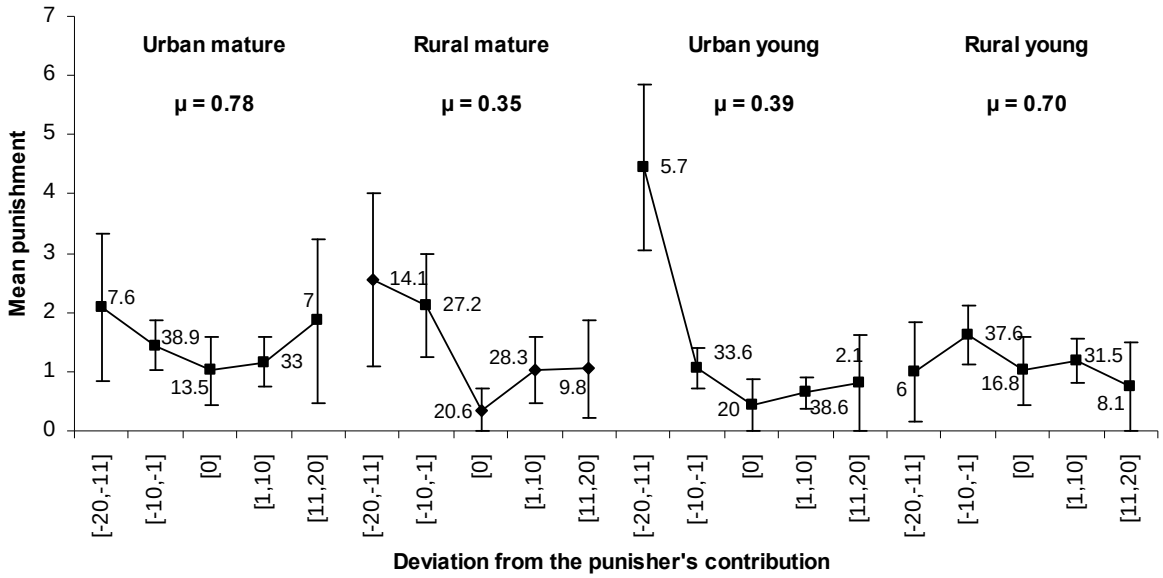

Fig. 3. Mean punishment (and 95-percent confidence intervals) of group members as a function of their deviation from the punisher's contribution.
Notes: μ denotes the ratio of misdirected punishment. Numbers indicate percentage of cases in the respective category of deviations.

An econometric analysis of punishment behavior corroborates the findings of Figure 4. We take a separate look at the four subject pools. Since punishment for negative and positive deviations is different we introduce the interaction variables 'Absolute negative deviation' and 'Positive deviation'. The variable 'Absolute negative deviation' is the absolute value of the actual deviation of the punished subject's contribution from the contribution of the punishing subject in case the punished subject contributed strictly less than the punishing subject, and zero otherwise. We define the variable 'Positive deviation' analogously. We also include the total group contribution (split up in own contribution and the sum of the other group members' contribution) to account for a possible impact of the absolute cooperation level on punishment. The variable 'Second experiment' is a dummy for the P-experiment of the N-P sequence. The estimation method is Tobit, since punishment is censored between 0

[20] Recall that μ = (mean punishment of non-negative deviations)/(mean punishment of negative deviations). Then the detailed results are as follows: $\mu_{\text{Urban mature}} = 1.192/1.537 = 0.78$; $\mu_{\text{Rural mature}} = 0.787/2.263 = 0.35$; $\mu_{\text{Urban young}} = 0.592/1.514 = 0.39$; $\mu_{\text{Rural young}} = 1.079/1.533 = 0.70$. See Section 4 for a discussion and some results from a comparable one-shot experiment conducted in Zurich which underscores that the observed μ's are exceptionally high.

and 10 (ordered Probit estimations yield the same qualitative results). We calculate robust standard errors and cluster on the independent groups. Table 1 records the results.

Punishment was significantly correlated to free riding (i.e., negative deviations from the punisher's contribution) in all subject pools; the rural young participants were the exception. The more a subject's contribution deviated from the contribution of the punishing subject, the more this subject got punished. The coefficient of 'Absolute negative deviation' is similar for urban and rural mature participants ($\chi^2(1) = 0.19$, $p=0.660$). The rural young participants punished free riders less harshly than all other subject pools ($p<0.063$, pair-wise $\chi^2(1)$-tests). The young urban participants punished free riders significantly more harshly than the young rural participants ($\chi^2(1) = 15.50$; $p=0.0001$); they also punished more harshly than the mature urban participants ($\chi^2(1) = 4.69$; $p=0.030$) and exhibited no significantly different punishment behavior of free riders than the rural mature participants ($\chi^2(1) = 2.45$; $p=0.118$). Punishment decreased in the positive deviations, but only significantly so with rural young participants. Holding everything else constant, punishment also decreased in the level of own contributions (except among the rural mature where this effect is not significant).

Table 2
An econometric analysis of punishment behavior

	Urban mature	Rural mature	Urban young	Rural young
Absolute negative deviation	0.360	0.440	0.648	0.124
	(0.111)***	(0.182)**	(0.133)***	(0.127)
Positive deviation	-0.073	-0.091	-0.069	-0.264
	(0.116)	(0.112)	(0.071)	(0.085)***
Own contribution	-0.211	-0.158	-0.235	-0.267
	(0.119)*	(0.139)	(0.116)**	(0.098)***
Sum of contribution of other group members	0.120	0.015	0.159	0.021
	(0.063)*	(0.106)	(0.062)***	(0.060)
Second experiment	-0.517	3.742	0.118	-0.003
	(1.259)	(1.828)**	(0.861)	(0.992)
Constant	-4.095	-6.504	-3.987	0.560
	(1.356)***	(2.835)**	(1.372)***	(1.556)
Observations	370	184	280	298
Wald chi(5)	19.48***	23.51***	19.90***	16.63***

Tobit regressions with robust standard errors clustered on independent groups. Robust standard errors in parentheses;
* p < 10%; ** p < 5%; *** p < 1%.

The presence of a punishment option in the P-experiments led to substantial losses in earnings relative to the earnings in the N-experiments. This holds true in all subject pools. For instance, the average earning of an urban mature participant in the N-experiment was 24.7 money units. In the P-experiment the earnings dropped to 13.7 money units, which implies a relative loss in earnings of 44.5 percent. The average relative loss in earnings of a rural

mature participant was 44.6 percent; urban young participants lost 29.2 percent and for the rural young participants the relative loss amounted to 39.7 percent. These outcomes can be explained by the dual facts that punishment did not increase cooperation and that there was substantial punishment.

3.4. The impact of socio-demographic differences on cooperation and punishment

Our final step is to look at the relevance of the socio-economic variables for cooperation and punishment. We pool the data of all four subject pools and of all N- and P-experiments, respectively, and set up a Tobit regression model that explains the contribution rates to the public good as a function of important socio-economic variables (for details see Table 1 and Appendix A). We include gender; a dummy for mature participants; a dummy whether one is a rural or an urban resident ('rural'); dummies for our non-students, who were either blue collar or white collar workers; two dummies for the highest education achieved ('secondary school' and 'university degree'); a dummy whether one is a 'member in any organization'; a dummy whether one is 'religiously active' or not; the 'number of known other participants' as a proxy for social proximity of participants; a dummy 'city size > 2'000 inhabitants', which is 1 if the subject has spent most of his or her life in a city with more than 2'000 inhabitants, irrespective of a person's current situation as an urban or rural resident[21]; and finally a dummy ('second experiment') if the experiment was second in sequence. We have chosen these variables because their relevance has been suggested by previous literature[22] and/or because they follow directly from our research questions, like whether there is – ceteris paribus – a difference between urban and rural residents, or young and mature age cohorts. Result 4 collects our findings.

Result 4. *In both the N- and the P-experiment we find that – ceteris paribus – rural residents and mature participants contributed more to the public good than urban and young participants. Punishment is largely unrelated to socio-demographic factors. Contributions are significantly lower in the second experiment.*

We document the support for this result in Tables 3 and 4. Our strategy is to estimate first a model where the two main explanatory variables relate to the variables which are our main interest – whether a subject is at least 30 years old (dummy variables 'mature') and whether a subject is a rural or an urban resident (dummy variable 'rural'). We split the mature

[21] This variable is a proxy for the dominant experience of the social background of one's life (see Appendix A).
[22] Many studies have found gender effects in social preferences (see Croson and Gneezy (2009) for a survey). List (2004), Sutter (2007) and Sutter and Kocher (2007) find a relationship between age and social preferences. Putnam (2000) and Glaeser, et al. (2002) argue for the relevance of memberships in civic organizations. Sosis and Ruffle (2003) have evidence for the relevance of religious activity for cooperation.

subjects into two groups – those aged 30 to 40 years and those older than 40 years and thereby measure the importance of age relative to the young subject pools (the omitted category).[23] In a second step we control for other potentially important covariates (the socio-economic variables described in Section 2.1 and Appendix A). We document the estimation results on cooperation in the N- and P-experiments in Table 3. Table 4 will present the results on punishment.

Table 3
The impact of socio-demographic factors on cooperation

	Dependent variable			
	Cooperation rate in N		Cooperation rate in P	
	(1)	(2)	(1)	(2)
Mature age 30 to 40	0.046	0.032	0.083	0.113
	(0.044)	(0.059)	(0.039)**	(0.058)**
Mature age 41+	0.106	0.066	0.148	0.153
	(0.038)***	(0.058)	(0.036)***	(0.056)***
Rural resident	0.103	0.094	0.067	0.074
	(0.034)***	(0.043)**	(0.032)**	(0.043)*
Second experiment	-0.177	-0.171	-0.112	-0.118
	(0.034)***	(0.035)***	(0.031)***	(0.031)***
Female		0.014		-0.020
		(0.036)		(0.033)
White-collar worker		0.062		-0.026
		(0.077)		(0.080)
Blue-collar worker		-0.017		-0.139
		(0.066)		(0.069)**
Secondary school		0.044		0.099
		(0.057)		(0.054)*
University degree		0.033		0.078
		(0.058)		(0.052)
Member in any organization		-0.015		-0.047
		(0.036)		(0.033)
Religiously active		0.037		-0.016
		(0.040)		(0.034)
Share of known other participants		-0.052		-0.047
		(0.079)		(0.076)
City size > 2'000 inhabitants		-0.021		-0.011
		(0.017)		(0.017)
Constant	0.382	0.405	0.378	0.459
	(0.030)***	(0.074)***	(0.031)***	(0.068)***
Observations	566	521	566	521
Wald χ^2	42.6***	57.0***	30.5***	53.3***

Tobit regressions with robust standard errors clustered on independent groups. Robust standard errors in parentheses; * p < 10%; ** p < 5%; *** p < 1%. All variables except 'Share of known other participants' are dummy variables.

We find in the *N-experiments* that both mature participants (in particular those aged 41+) and rural participants contribute more than their younger and urban counterparts,

[23] Those aged 30 to 40 years were in their (early) twenties at the time the Soviet Union ceased to exist, whereas those older than 40 were already adults for a significant period of time when the Soviet Union still existed.

respectively. When we control for covariates we find that none of the covariates is significant at conventional levels (the socio-demographic variables are also jointly not significantly different from zero ($\chi^2(8)=8.30$, $p=0.405$)). Both dummy variables for the mature subjects are no longer significant, while 'rural' remains significant. We find it particularly noteworthy that the education variables are insignificant. This suggests that contributions are not due to confusion if we assume that better educated people are less easily confused. We conclude that the observation that rural participants contribute significantly more than urban residents is a robust finding, whereas the age effect seems to be fragile.

We get slightly different results with respect to contribution rates in the *P-experiment*. First, we find again that mature participants and rural participants contributed significantly more than young and urban participants. Both effects remain (weakly) significant when we control for covariates ((which are jointly weakly significantly different from zero ($\chi^2(8)=14.99$, $p=0.059$)). Among the covariates we find that blue-collar workers contributed significantly less than students (the benchmark) and people whose highest degree is from a secondary school contributed significantly more than students. All other variables are insignificant at conventional levels. Interestingly the level of acquaintance between participants does not influence cooperation behavior neither in the N-experiment nor in the P-experiment.[24] In both the N-experiment and the P-experiment contributions are significantly lower in the second experiment.

We turn next to punishment behavior and record the estimation results in Table 4. In our first models we do not distinguish whether the punished subject had deviated positively or negatively from the punisher's contribution. We also control for the same variables as in Table 3.

We find that no socio-demographic variable, including the two variables of main interest, are significantly related to punishment. In models (3) and (4) we only look at punishment of negative deviations, i.e., situations where the punished subject had contributed *less* than the punishing subject. Again we detect no influence of socio-demographic variables on punishment of free riders. Finally, when we confine our attention to punishment of non-negative deviations (models (5) and (6)), there is no difference between mature and young participants, and urban and rural participants. This also holds if we control for covariates. Here we get three noteworthy results. People who were a member in any voluntary organization punished non-negative deviations from their own contribution weakly significantly more than people with no memberships. People with a university degree punished weakly significantly more than students and white-collar workers weakly significantly less than students. Thus, higher degrees of education did not lower misdirected

[24] We also checked for a possible non-linear relationship between share of known other participants and cooperation behavior by squaring the 'share of known other participants' variable. This did not change the results.

punishment, which suggests that punishment of non-negative deviations was not due to confusion.

Table 4
The impact of socio-demographic factors on punishment

	Dependent variable					
	All punishment		Punishment of negative deviations		Punishment of non-negative deviations	
	(1)	(2)	(3)	(4)	(5)	(6)
Mature age 30 to 40	-0.330	-0.178	-0.014	2.060	-0.630	-1.335
	(0.803)	(1.091)	(1.056)	(1.544)	(1.058)	(1.432)
Mature age 41+	0.357	0.730	0.342	2.451	0.298	0.125
	(0.670)	(1.059)	(0.869)	(1.509)	(0.913)	(1.395)
Rural resident	0.295	0.795	0.290	0.795	0.350	1.083
	(0.597)	(0.790)	(0.791)	(1.025)	(0.753)	(0.995)
Absolute negative deviation	0.417	0.410	0.492	0.452		
	(0.067)***	(0.067)***	(0.113)***	(0.108)***		
Non-negative deviation	-0.135	-0.138			-0.011	-0.024
	(0.050)***	(0.051)***			(0.063)	(0.063)
Own contribution	-0.257	-0.258	-0.443	-0.434	-0.108	-0.115
	(0.059)***	(0.060)***	(0.109)***	(0.108)***	(0.085)	(0.087)
Sum of others' contribution	0.089	0.085	0.142	0.115	0.040	0.050
	(0.034)***	(0.033)**	(0.058)**	(0.056)**	(0.050)	(0.050)
Second experiment	0.373	0.303	-0.316	-0.273	1.045	0.833
	(0.609)	(0.617)	(0.755)	(0.723)	(0.798)	(0.828)
Female		-0.437		-0.296		-0.756
		(0.589)		(0.701)		(0.868)
White-collar worker		-1.608		-2.511		-1.450
		(1.423)		(1.952)		(2.034)
Blue-collar worker		0.324		-2.035		1.602
		(1.058)		(1.694)		(1.382)
Secondary school		0.644		-0.494		1.312
		(0.981)		(1.196)		(1.469)
University degree		1.402		0.097		2.388
		(1.078)		(1.273)		(1.446)*
Member in any organization		0.792		0.319		1.476
		(0.586)		(0.733)		(0.818)*
Religiously active		0.265		-0.365		0.969
		(0.625)		(0.928)		(0.798)
Share of known other participants		-0.510		-0.067		-1.119
		(1.298)		(1.955)		(1.644)
City size > 2'000 inhabitants		0.295		0.321		0.224
		(0.265)		(0.337)		(0.388)
Constant	-3.158	-4.186	-1.498	-1.169	-4.324	-6.599
	(0.889)***	(1.435)***	-1.104	(1.773)	(1.142)***	(2.010)***
Observations	1132	1042	469	433	663	609
Wald χ^2	66.9***	70.7***	22.1***	27.5**	4.9	22.5*

Notes: Tobit regressions with robust standard errors clustered on independent groups. Robust standard errors in parentheses; * p < 10%;
** p < 5%; *** p < 1%. All socio-economic variables except 'Share of known other participants' are dummy variables.

4. Discussion – putting the results into perspective

The results are unexpected and in strong contrast to previous findings (Fehr and Gächter (2000); Fehr and Gächter (2002) and many experiments referenced in footnote 1 and surveyed in Gächter and Herrmann (2009)). The first surprise is that in all subject pools punishment was almost completely ineffective in raising contributions. The second unexpected finding is that there was a lot of punishment across the board. Based on evidence from previous experiments on punishment we expected some punishment in case of free riding but we did not anticipate such a high degree of punishment of people who contributed *the same or more* than the punishing subject. For instance, in the Fehr and Gächter (2002) experiments misdirected punishment amounted to $\mu = 0.23$, which is substantially lower than the μ's we observe in our Russian subject pools.

A plausible response to our findings is that they are maybe an artifact of one-shot games. For instance, since all previous experiments were played repeatedly (see footnote 1), it might be that the drop in cooperation we see is due to the one-shot nature of our game. However, we do not believe that this is the case. Two sets of results from related studies, one from one-shot games and one from repeated games, support our belief.

First, Gächter and Herrmann (2009) conducted three-player one-shot experiments (no repetitions) with the same parameters and designs like the ones used in this paper in two subject pools in Russia (Belgorod and Yekaterinburg) and two subject pools in Switzerland (St. Gallen and Zurich). Like in this paper, participants (n=606 students, comparable to the urban young subject pool used in this paper) played either the N-P sequence or the P-N sequence. Thus, the experiments are directly comparable.[25] Figure 4 reports the results.

Figure 4 shows that the new experiments largely replicated the findings reported above (compare Figure 2, in particular the comparable urban young subject pool, who, in the first experiment also contributed about 35 percent of their endowment). Contributions decayed in both sequences. Interestingly, the decay is significant in the N-P sequence and insignificant in the P-N sequence. In both Swiss subject pools contributions increased significantly in the N-P sequence and dropped significantly in the P-N sequence.

Misdirected punishment was substantially smaller in both Swiss subject pools than in both Russian subject pools. Even if one believes that some punishment in a one-shot game is due to errors, the results suggest that the extent of (misdirected) punishment which we observed in our Russian subject pools cannot be solely erroneous, because (i) we replicate the findings reported above and because (ii) the extent of misdirected punishment is much smaller in both Swiss subject pools and similar to μ=0.23 found by Fehr and Gächter (2002) in experiments conducted with students in Zurich.

[25] The only difference to the present experiments is that the new experiments were computerized. The instructions and procedures were adapted to the computerized version of these experiments but were otherwise very similar to the ones used in this paper.

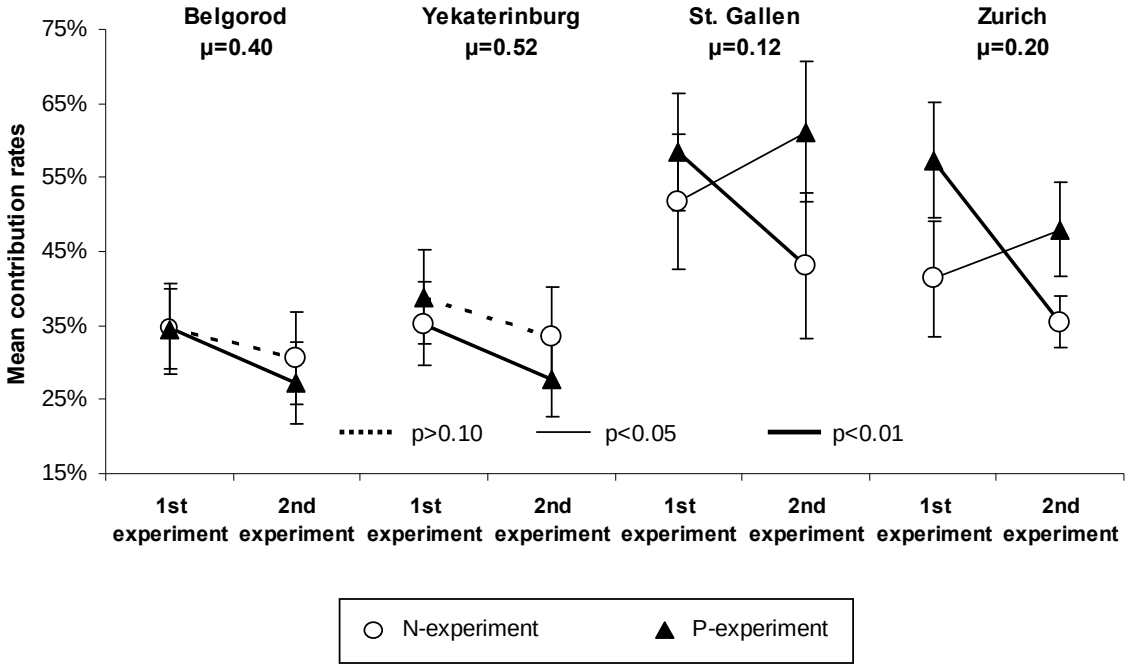

Fig. 4. Mean contribution rates (and 95-percent confidence intervals) in one-shot games in Russia (Belgorod and Yekaterinburg) and Switzerland (St. Gallen and Zurich).
Notes: N=606 students. Data source: Gächter and Herrmann (2009), own calculations. The thickness of lines indicates the significance level of Wilcoxon-matched pairs tests with group average contributions as independent observations.

Second, Herrmann, et al. (2008) report evidence from finitely repeated public goods games. Self-governance might be much easier in repeated games, which also provide an intrinsic incentive to limit misdirected punishment. Figure 5 shows data on cooperation (panel A) and punishment (panel B) of experiments in which the same group members ('partners') interacted for ten periods and were aware of it. Specifically, groups first played ten periods with no punishment and were then informed that they would play another ten periods with punishment. Herrmann et al. conducted these experiments with 80 undergraduates in Samara, a large Russian city.

Figure 5 shows the main results from these experiments. In the absence of punishment (periods 1-10) average contributions follow the usual declining pattern. When punishment is added (periods 11-20), the declining trend is stopped. Yet, on average, contributions only increase moderately (from an average of 10.6 to 12 tokens). The increase is statistically insignificant (two-sided Wilcoxon signed-ranks test, p=0.218; group averages over all periods as independent observations).[26] Thus, our observation from the one-shot games that punishment does not have a cooperation-enhancing effect also holds for the repeated games.

[26] These conclusions also hold if we only compare periods 1 and 11. Subjects contributed 10.8 tokens in period 1 and 11.0 tokens in period 11 (p=0.823).

This finding stands in stark contrast to Fehr and Gächter (2000). They report that in their comparable sequence contributions in the finitely repeated public good game with punishment reached almost the maximum contribution level. The increase from the N-experiment to the P-experiment was significant at any conventional level.

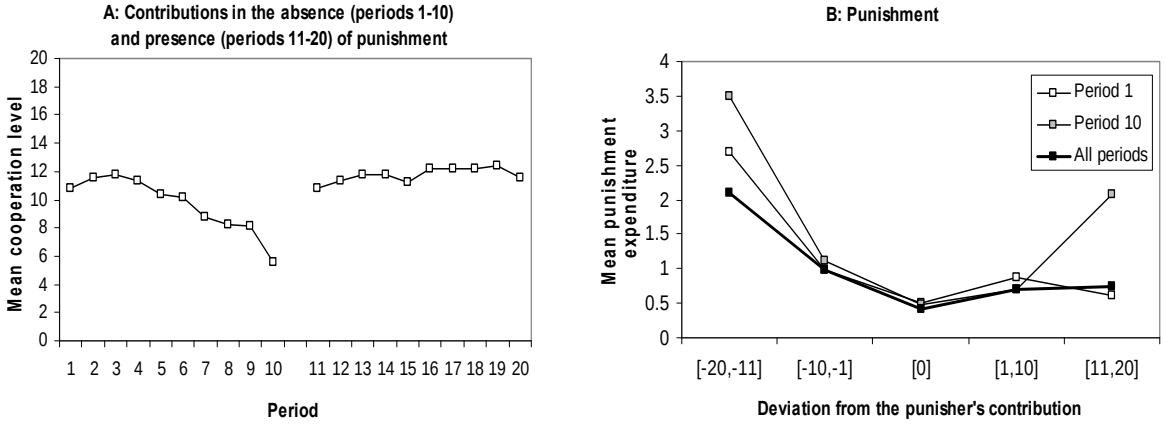

Fig. 5. Cooperation and punishment in repeated games in Samara.
Note: Data source: Herrmann, et al. (2008), own calculations.

Figure 5B looks at punishment as a function of the punished subject's deviation from the punisher's contribution. We distinguish between period 1 and period 10, to check for temporal stability. We also include the average punishment over all periods. We find that punishment is very similar at the beginning and at the end of the experiment. This holds in particular in the range of deviations from -10 to +10, which comprises 88 percent of all observations. Experience with the game does not diminish punishment. To the contrary punishment of large deviations – both negative and positive – is harshest in the final period. As a comparison with Figure 3 reveals, the punishment patterns in the repeated game are also remarkably similar to the pattern exhibited by the young Kursk subjects, who are most comparable to the Samara students with respect to their socio-demographic background. Even quantitatively results are very similar, in particular for positive deviations and deviations between [-10,-1].

In summary, these complementary results support our conclusion that misdirected punishment might limit successful self-governance. Future research should address the sources of misdirected punishment.

5. Summary and concluding remarks

We conducted experiments with 566 adult participants in urban and rural Russia. We employed a 2×2-factorial subject pool design to investigate potential differences in

cooperation and punishment behavior between (i) 'mature' and 'young' participants and between (ii) residents of urban and rural areas, which still differ sharply with respect to living conditions. Our mature participants were socialized during communism, whereas the young participants spent their adolescent years in the turbulent transition period after the breakdown of the Soviet Union. Thus, these subject pools differ starkly from the western undergraduate subject pools used in most experiments on voluntary cooperation. Our experiments therefore provide (i) a 'robustness check' of previous findings in sociologically different subject pools, and (ii) allow us to uncover the potential impact of socio-demographic factors on cooperation and punishment.

We observe in all subject pools substantial levels of voluntary cooperation and punishment of free riders. This finding supports previous conclusions about the importance of altruistic cooperation and punishment, and reciprocity in general (e.g., Fehr and Gächter (1998); Fehr and Fischbacher (2003); Dohmen, et al. (2009); Gächter and Herrmann (2009)). We have the following main new results:

1. *The sociological background matters for voluntary cooperation. In particular, we found higher levels of voluntary cooperation among rural residents than among urban residents; the student–non-student distinction does not matter in our data.* This result highlights that probably the student–non-student distinction is less important than previously suggested (e.g., Carpenter, et al. (2005)) if we take the broader sociological environment, like the urban-rural distinction, into perspective.

2. *We found in all subject pools very high levels of punishment of people who contributed the same or even more than the punishing subject.* Although misdirected punishment has been observed in previous experiments on cooperation and punishment, it was substantially lower than in the present experiment. This explains why hitherto misdirected punishment has been rather neglected.

3. *In no subject pool did punishment lead to a significant increase in cooperation. Instead, the presence of a punishment option resulted in substantial payoff losses.* Thus, misdirected punishment can undermine the positive impact of punishment for cooperation and thereby limit the success of 'self-governance'. Our results show that it does not take 'counter-punishment' (i.e., multiple rounds of retaliatory punishment for having got punished) to limit successful self-governance (Cinyabuguma, et al. (2006); Denant-Boemont, et al. (2007); Nikiforakis (2008)).

The main contribution of this paper is to demonstrate that punishment might not only be used to discipline free riders as previous evidence might have suggested. Recent research has started to address the question how prevalent misdirected punishment is across different

cultures (Fehr, et al. (2008); Gächter, et al. (forthcoming)). Future research should address the issue why people punish cooperators, and how such "antisocial punishment" (Herrmann, et al. (2008)) can be explained by (evolutionary) theories of social preferences.

Appendix A: Socio-economic background of our subject pools

In total, we conducted 19 experimental sessions with 606 participants divided into 202 independent groups. The experiments with urban participants took place in the city of Kursk and in the neighboring city of Zheleznogorsk. The experiments with rural subjects took place in the villages of Ivanovka and Sedmikhovka, with participants from eight different villages of the Kursk region (Ivanovka, Kazanka, Kosorzha, Nizhniy Daymon, Sedmikhovka, Nikolaevka, and Matveevka, all around 40 miles north of Kursk), and in Ust-Kinel, 400 miles east of Moscow. Table A1 summarizes how participants were distributed across treatments and location.

TABLE A1: DISTRIBUTION OF SESSIONS AND PARTICIPANTS ACROSS TREATMENT SEQUENCES AND LOCATIONS

Sequence		Urban	Rural
N-P	# sessions	7	4
	# participants	189	162
P-N	# sessions	5	3
	# participants	159	96

Forty participants were not able to solve the control questions of the experiment, so we cannot be sure whether they understood the decision situation properly. We drop them from the data set, which leaves us with valid data from 566 urban and rural participants.

An anonymous post-experimental questionnaire provides us with the socio-demographic details of our subject pools. In addition to obvious socio-demographic variables (age, gender, education and profession), we asked them about the size of the city were they had spent most of their lives, to get a proxy for the formative background of our participants. This variable contains four categories: (1) city size is up to 2'000 inhabitants; (2) between 2'000-10'000 inhabitants; (3) between 10'000-100'000 inhabitants and (4) more than 100'000 inhabitants. This city size variable gives us some information about the background of the participants that is not captured by the place where we ran the experiments. Put differently, this variable gives us some validation for our assumption that the classification of participants as urban or rural is substantially correct. A further proxy for a rural vs. urban background is the actually observed social distance between our participants. To get information on this, we asked them, at the end of the experiment, how many other participants (out of roughly 30 in each session) they knew.

A further piece of background information relates to the social activities of our participants. We have two indicators. One measure is whether a subject is religiously active or not (*Religious activity,* a dummy variable). The second indicator of social activity, the *Membership* variable (a dummy variable), records whether the subject is a member of any civic voluntary associations (political, interest groups, sports, culture, nonprofits, others).

A. *Mature urban and rural participants* (*age* $\geq$ 30)

Table 1 in the main text and Table A2 contain the results for the urban and rural mature participants. Urban and rural mature participants were about equally old (44.6 and 43.0 years, respectively). The share of females between the pools was very balanced and with 55.0 and 53.3 percent the same in both pools. There are no statistically significant differences between urban and rural subject pools with respect to age and gender composition.

The rural mature participants had indeed spent most of their lives in small places: The mean city size category for them is 1.46. By contrast, the urban mature participants reported an average city size category of 3.4, which means that most of them had spent most of their lives in a rather large city. A χ^2-test confirms that our city size variable is significantly differently distributed between the two subject pools. Thus, our categorization of subject pools as urban and rural has substantive content. The urban mature participants knew on average only 3.4 percent of the other participants. By contrast, the villagers knew each other well. On average one participant knew 43.1 percent of the others participants. This difference, which is significant at any conventional level, reflects the different levels of social distance in the urban and rural areas, respectively.

The rural mature participants were significantly less religiously active than the urban mature participants. Both subject pools revealed similarly low levels of societal engagement. This low rate of membership is consistent with observations from other studies that Russians generally have a very low engagement in any civic voluntary association (e.g., Rose (2000));. For instance, based on interviews conducted in 1998, Rose (2000),

reports that 80 to 90 percent of the Russians do not belong to any voluntary association. In our subject pools this was true for 71 percent.

Table A2 also contains detailed information about the jobs our participants held. The urban and rural subject pools differed significantly with respect to professional composition. We distinguish between white-collar and blue-collar workers. Our definition of white-collars comprises all jobs that consist of non-manual work (civil servants, clerks, executives, entrepreneurs); blue-collar workers do manual work (workers, farmers, homemakers). Roughly 63 of the urban mature participants were white-collar workers; in the rural areas their fraction was 46 percent. A fair number of our participants were blue-collar workers. Workers from the villages were predominantly engaged in agricultural production (for example as tractor drivers). In the cities 43.5 percent of the participants were clerks, while in the villages we only had 27.1 percent clerks. In the rural context these participants were mainly teachers at the local schools or local hospital employees. Farmers occurred only among the villagers and accounted for 2.2 percent of the participants. 5.4 percent of the urban and 0 percent of the rural participants considered themselves as entrepreneurs. The share of executives and civil servants was low in both pools.

Finally, we turn to the educational background of our participants, which is also significantly different between subject pools. The urban participants were more highly educated on average than the rural participants. Fewer urban than rural mature participants had only compulsory education. Among the urban mature participants almost half of our participants had a completed university degree. Among the rural mature participants this was true for a third of our participants.

TABLE A2: KEY FIGURES ABOUT THE MATURE SUBJECT POOLS (AGE ≥ 30)

	Urban mature (n=185)	Rural mature (n=92)	Statistical comparisons (p-value; test)
Indicators of social activity			
Religiously active (in percent)	83.2 (37.4)	56.5 (49.8)	0.000 (χ^2-test)
At least one membership (in percent)	30.3 (46.1)	26.1 (44.2)	0.406 (χ^2-test)
Professions of participants (in percent)			0.001 (χ^2-test)
White collar	62.7	45.7	
Clerks	43.5	27.1	
Executives	9.8	9.8	
Entrepreneurs	5.4	0.0	
Civil servants	4.3	8.7	
Blue collar	37.3	58.1	
Workers	26.1	33.7	
Farmers	0.0	2.2	
Homemakers	10.9	18.5	

Note: Numbers in parentheses are standard deviations.

B. *Young urban and rural subject pools*

The 289 young participants were on average 20 years old; the rural young participants were slightly older than their urban counterparts. Between 21 and 32 percent were females. The urban students mostly came from medium-sized and big cities, whereas the rural students were mostly from small and medium-sized cities. This difference is highly significant. The rural students had a significantly higher rate of acquaintance with other participants than the urban students (9.9 percent vs. 24.9 percent). Urban and rural students are about equally religiously active. Yet, when it comes to memberships in civic voluntary organizations, we find a strong difference between subject pools. Among the urban participants, 63 percent reported a membership in at least one civic voluntary organization, whereas only 46 percent the rural participants were members in any civic organization. The majority of our young participants were students, but significantly fewer among the rural participants. Table A3 we summarize key background figures of our young subject pools.

TABLE A3: KEY FIGURES ABOUT THE YOUNG SUBJECT POOLS (AGE < 30)

	Urban young (n=140)	Rural young (n=149)	Statistical comparisons (p-value; test)
General background data			
Mean age in years	20.5 (2.2)	21.0 (2.5)	0.053 (t-test)
Percent Female	21.4	32.4	0.016 (χ^2-test)
Mean of city size category	2.8 (1.1)	1.9 (1.0)	0.000 (χ^2-test)
Share of known participants (in percent)	9.9 (9.8)	24.9 (24.2)	0.000 (t-test)
Indicators of social activity			
Religiously active (in percent)	84.3 (36.5)	81.9 (38.6)	0.517 (χ^2-test)
At least one membership (in percent)	62.9 (48.5)	45.6 (50.0)	0.001 (χ^2-test)
Professions of participants (in percent)			
Students (in percent of participants)	89.3 (31.0)	74.5 (43.7)	0.001 (χ^2-test)
White collar (in percent of participants)	3.6 (18.6)	6.0 (23.9)	0.334 (χ^2-test)
Blue collar (in percent of participants)	7.1 (25.8)	19.5 (39.7)	0.003 (χ^2-test)

Note: Numbers in parentheses are standard deviations.

Appendix B: Instructions

The following instructions were originally written in Russian. We document the translated instructions from the N-P sequence.

General Explanations for Participants

You are now taking part in an economics experiment, financed by several research foundations. If you read the following instructions carefully, then you will – depending on your decisions – earn a considerable amount of money. It is therefore very important that you read these instructions carefully.

The instructions are solely for your private information. During **the experiment it is strictly prohibited to communicate with other participants in any way.** If you have any questions, **please ask us.** A violation of this rule will lead to the exclusion from the experiment and the cancellation of all payments.

During the experiment we will not speak of Rubles but of Guilders. So your whole earnings will be calculated in Guilders first. At the end of the experiment, the total amount of Guilders you have earned will be converted to Rubles at the following rate:

1 Guilder = 2.5 Ruble.

At the end of experiment you will receive the total sum of Guilders you earned plus 100 Rubles in cash as a show-up fee.

At the beginning of the experiment all participants will be divided into groups of three members. Apart from you, your group will have two further members. **Only the experimenter knows the composition of groups. Neither before, nor after the experiment, will you learn who was in your group.**

There is only one task in this experiment. You will have to decide **whether to contribute Guilders to a group project, or whether you keep the Guilders for yourself.** On the following pages we will describe the sequence of events in the experiment in detail. At the end of this introduction you will find exercises that should be helpful to familiarise with the decision situation.

The Decision:

At the beginning of the first stage, every participant receives an "endowment" of **20 Guilders**. You will have to decide how many of these 20 Guilders you contribute to the **project**, and how many you keep for yourself. The two others participants will have to make the same decision. They can as well contribute Guilders to the project or keep them for themselves. Thus, you and the other participants can choose any amount between 0 and 20 Guilders. Every Guilder you do not contribute to the project, will be yours automatically and will be paid to you at the end of experiment at the rate indicated above.
The following will happen with Guilders you contributed to the project: the experimenter will top up the sum of contributed Guilders by 50% and this sum will be **divided equally among the three group members**.

For example, if you contribute one Guilder to the project, the experimenter will add half a Guilder. The sum, namely 1.5 Guilders, will be distributed among all three group members of the group in equal parts, so each group member receives 0.5 Guilders. Therefore, for every Guilder you put into the project you will earn 0.5 Guilder from the project. At the same time the earnings of each other group member will increase by 0.5 Guilders as well, since they receive the same income from the project as you do. Thus, with your contribution of one Guilder to the project, the income of the group rises by 1.5 Guilders. In turn it holds that a contribution of one Guilder to the project by another group member will raise your earnings by 0.5 Guilders.

After all three group members have decided on their contributions to the project, the earnings of every participant are determined.

How are your earnings from your decision calculated?

The earnings of each group member will be calculated in the same way. The earnings consist of two parts:

(1) Guilders one keeps for oneself (**"Income from Guilders retained"**)
(2) **"Earnings from the Project"**.

The earnings from the project amount to

0.5 x (total sum of contributions to project)

Your total earnings will therefore be calculated according to the following formula:

Your total earnings =

Earnings from Guilders retained + Earnings from the project =
(20 – Your contribution to the project) + 0.5 x (Sum of all contributions to the project).

If you contribute nothing to the project, your "earnings from Guilders retained" is 20. If you contribute 10 Guilders to the project, for instance, your "earnings from Guilders retained" is 10. The total amount of contributions to the project increases and therefore your "earnings from the project" increase as well.

To illustrate the calculation of earnings we list some examples here:

- If all three group members **contribute 0** Guilders to the project, all three will receive "earnings from Guilders retained" of 20. Nobody receives therefore anything from the project, since nobody contributed to the project. The total earnings of each group member are therefore 20 Guilders.

Calculation of total earnings for each group member: (20 – 0) + 0.5(0) = 20*

- If all three group members contribute 20 Guilders to the project, a total amount of 60 Guilders will be contributed towards the project. The "earnings from Guilders retained" are zero for everyone; but each member receives earnings from the project of 0.5*60 = 30 Guilders.

Calculation of total earnings for each group member: (20 – 20) + 0.5(60) = 30*

If you contribute 20 Guilders to the project, the second member contributes 10 Guilders, and the third member contributes 0 Guilders, the earnings will be as follows: As you and the second group member have invested in total 30 Guilders, you both will receive an income from the project of 15 Guilders. As you have invested all your 20 Guilders into the project, you earn in total 15 Guilders at the end of the experiment. The second group member also receives 15 Guilders from the project. As he has invested 10 Guilders into the project, 10 Guilders remained at his disposal. Thus in total he receives 10 + 15 = 25 Guilders. The third group member who has contributed nothing, also receives 15 Guilders from the project and additionally the 20 Guilders of the "Earning from Guilders retained", therefore in total 20 + 0.5 x (30) = 35 Guilders.

Calculation of your total earnings: (20 – 20) + 0.5(30) = 15*
Calculation of second group member's earnings: (20 – 10) + 0.5(30) = 25*
Calculation of third group member's earnings: (20 – 0) + 0.5(30) = 35*

- Suppose the other two members contribute 20 Guilders to the project, you contribute nothing. In this case the earnings are calculated as follows:

Calculation of total earnings for the 2nd and 3rd member (contribution of 20):
(20 – 20) + 0.5(40) = 20.*
Calculation of total earnings for you (contribution 0):
(20 – 0) + 0.5(40) = 40*

For your decision, you will receive the following decision sheet (**shown here only as an example)**:

Decision sheet:

Please fill in the box the amount of Guilders you contribute to the project:

(Maximum 20 Guilders)

On a decision sheet like the one above, you will fill into the box how many Guilders you would like to contribute to the project.

After you have made your decision, please put your decision sheet in the provided envelope, seal the envelope and hand it over to a research assistant. Your decision will only be seen by the leader of the experiment. He will then calculate your earnings. You will then get an information sheet, in a sealed envelope, which will tell you how much the other participants in your group invested into the project and what your earnings and those of the other group members are.

Control questions:
Please answer all the questions and write down your calculation. The questions that ask for earnings refer to the amount of Guilders, not the earnings in Rubles. The examples are hypothetical and serve to help you understanding the payoff calculations.

1. Suppose each group member has 20 Guilders at his or her disposal. Nobody (including you) contributes anything to the project.

What are your total earnings from the experiment? ……
What are the others' total earnings from the experiment? ……

2. Suppose each member has 20 Guilders. You contribute 20 Guilders to the project. The two other group members contribute 20 Guilders to the project as well.

What are your total earnings from the experiment? ……

What are the others' total earnings from the experiment? ……

3. Each member is endowed with 20 Guilders. Suppose you contribute 3 Guilders. The second member contributes 10 Guilders and the third member contributes 17 Guilders.

Your total earnings from the experiment? ……
The total earnings of the second member? ……
The total earnings of the third member? ……

4. Each member is endowed with 20 Guilders. Suppose you and the second member contribute 20 Guilders to the project; the third member contributes 0 Guilders.

Your total earnings from the experiment? ……
The total earnings of the second member? ……
The total earnings of the third member? ……

Do you have any questions?

General Explanations for the Participants

We will now repeat the experiment with some changes. As in the previous experiment, you are endowed with 20 Guilders. However, this time you must make **two** decisions. The **first decision** is identical to the decision you made in the experiment we have just conducted. In the first decision you must again make a decision about how many of the 20 Guilders from your endowment you want to contribute to a project (and therefore also how many you will keep for yourself). The earnings at the first stage will be calculated in the same way as they were calculated in the previous experiment. For each Guilder you choose to keep, you will earn one Guilder. For each Guilder you contribute to the project you, and all other group members, will earn 0.5 Guilders. Each Guilder another group member contributes to the project raises your earnings by 0.5 Guilders.

The group composition is the same as in the first experiment. You will not find out who is in your group. This experiment will also be conducted only *once*.

What is different in the new experiment?

New is a **second stage**, consisting directly in the distribution of information sheets about the contribution and the incomes of the other participants.

The 2nd stage:

At the second stage you will learn how much the other group members have contributed to the project. At this stage, through assigning **deduction points**, you can **reduce** the earnings of **each** of the other group members. You can also **leave** the earnings of the other group members **unchanged**. The other group members can also reduce **your** earnings if they so wish. The exact procedure will be described below in greater detail. We will first describe the income consequences that will follow from the assignment of deduction points.

How are your earnings at the second stage calculated?

If you assign deduction points to another group member **the earnings of this group member will be reduced by three times the amount of assigned deduction points.** This means **that if you assign one deduction point to another group member, the leader of the experiment will reduce his earnings by 3 Guilders.**

If you assign 2 deduction points to a group member, his income will be reduced by 6 Guilders. If you assign 9 deduction points his income will be reduced by 27 Guilders, etc.. If you decide to assign 0 deduction points to a particular group member his earnings will not be changed.

You can assign a maximum amount of 10 deduction points to each other member.

If you **assign deduction points, you will also have costs. For each assigned deduction point costs are one Guilder.** For example, if you assign 5 deduction points, you will have costs of 5 Guilders, if you assign 10 deduction points, you will have costs of 10 Guilders, etc.

If you assign no deduction points, you will, of course, have no costs from assigning deduction points.

Your total earnings from both stages will be calculated according to the following formula:

Total earnings from the 2nd stage =

= (earnings from the 1st stage)

***minus* 3 * (the amount of deduction points received from other group members)**

***minus* (the amount of deduction points you assigned to other group members)**

In case the reduction in earnings resulting from the received deduction points exceeds the earnings from the 1st stage, the earnings after the 2nd stage will be *zero minus* the costs of the deduction points *you* assigned to other group members.

Your total earnings at the end of the second stage have thus three components: (1) your earnings from the first stage. (2) The tripled amount of deduction points received from other participants. (3) The costs that you have incurred through assigning deduction points.

Please notice the following special case: If the tripled amount of deduction points received exceeds the income from the first stage, the deduction points of the affected member will be only be deducted by the amount of the earnings from the first stage. This means that the earnings minus the tripled amount of deduction points from the other members will be set to zero. Independent of this, one has to bear the full costs of deduction points one assigns to other members. Please note that you always avoid costs through your own decisions.

How do you make your decisions at the 2nd stage?

As in the first experiment, all participants will first determine their contributions to the project. These decision sheets will be collected. Next, you will receive the decision sheet for the 2nd stage. On the decision sheet for the 2nd stage you will be informed about how many Guilders the other participants have contributed to the project and what your earnings and those of the other group members in Guilders are. Now, in an additional row, you must decide, whether and if so, how many deduction points you will assign to the other members of your group.

Below you will see an example of the decision sheet, which you will receive with the relevant information for the second decision.

Decision sheet for the second stage:

	You	**Second Member**	**Third Member**
Contributions to the project	Your contribution	Contribution of the second group member	Contribution of the third group member
Earnings from the first stage	Your earnings	Earnings of the second group member	Earnings of the third group member
Your deduction points:			

Please decide whether, and if so, how many deduction points you would like to assign to the others. If you would not like to assign any deduction points, please enter a zero. You can assign a maximum of 10 deduction points to each group member. You must, in any case, make a decision and fill in the boxes.

The second decision sheet is designed in the following way:

● In the **first row** you will see the "Contribution to the project" that the three group members made at the first stage. Your contribution at the first stage is listed in the first column.

● In the **second row** ("Earnings from the first stage") you will see, which earnings each group member received from their decision in the first stage. Under the heading "You", you will see *your* earnings, in the second and third columns you will see the earnings of the other group members.

● In the **third row** ("Your deduction points") you have to make your decisions for the second stage: You now have to decide how many deduction points you would like to assign to **each of the other** group members. Enter in the respective box a number between 0 and 10. You have to make an entry into each box. If you would not like to change the income of a certain group member, enter a 0. **You can assign a maximum of 10 deduction points to each of the other group members.**

After you have entered your decisions on the decision sheet regarding the assigning of deduction points at the second stage, put your decision sheet in the envelope and hand it over to an assistant. The leader of the experiment will calculate your earnings and the earnings of the other group members. At the end of the experiment, the assistants will hand out your payoff in an envelope. Only the leader of the experiment will know your decisions.

The experiment will be finished after you have made your decisions about assigning deduction points and the experimenter has collected the envelopes. There will be no further experiment. You have then to answer some questions and then you will receive your payment.

Do you have any questions?

Control questions

All questions must be answered. Please show all your calculations. If you have questions, please ask the experimenters! **The examples are hypothetical and serve only to help you understand the payoff calculations.**

1. You want to assign 6 deduction points to the first member and 8 deduction points to the second group member.
Which costs will you incur? ……
By how much will the earnings of the first group member be reduced? ……
By how much will the earnings of the second group member be reduced? ……

2. You want to assign 10 deduction points to the first group member. You want assign no deduction points to the second group member.
Which costs will you incur? ……
By how much will the earnings of the first group member be reduced? ……
By how much will the earnings of the second group member be reduced? ……

3. You do not assign any deduction points.
Which costs will you incur? ……
By how much will the earnings of the first group member be reduced? ……
By how much will the earnings of the second group member be reduced? ……

4. Suppose the second member of the group earned 10 Guilders in the 1st stage. From you he receives 5 deduction points; and from the third member he receives 6 deduction points.
By how much will the earnings of the second group member be reduced? ……

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