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Beware of Broken Windows!
First Impressions in Public-good Experiments*

by

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Abstract

Broken Windows: the metaphor has changed New York and Los Angeles. Yet it is far from undisputed whether the broken windows policy was causal for reducing crime. In a series of lab experiments we show that first impressions are indeed causal for cooperativeness in three different institutional environments: absent targeted sanctions; with decentralised punishment; with decentralised punishment qualified by the risk of counterpunishment. In all environments, the effect of first impressions cannot be explained with, but adds to, participants' initial level of benevolence. Mere impression management is not strong enough to stabilise cooperation though. It must be combined with some risk of sanctions.

JEL: C91, D03, D63, H41, K14, K42

Keywords: Broken Windows, Impression Management, Criminal Policy, Public Good Experiment

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1. Motivation

Times Square, Manhattan, 1990: clearly not the place to be. You would have met all sorts of outcasts and would have exposed yourself to a serious risk of violent crime. Times Square, Manhattan, 2000: indulge in the world's most vibrant city, at its best. Don't be afraid of violence. The crime rate is substantially below the national average.[1] Usually Mayor Rudolph W. Giuliani and New York Police Dept. Commissioner William Bratton are credited with the success (Zimring 2007). In recent years, William Bratton has repeated the New York success in Los Angeles. (Wagers 2008). In both cities, he explicitly relied on the "broken windows" policy (Wilson and Kelling 1982; Skogan 1990; Kelling and Coles 1996), which was inspired by an experiment conducted by Philip Zimbardo in 1969. Zimbardo simultaneously placed two otherwise identical cars in public spaces, one in the Bronx, the other in Palo Alto. Neither car had license plates, and the hood was open. Within 26 hours the first car was totally pillaged and destroyed, while the second stayed pristine for an entire week. Once the experimenters themselves broke a window with a hammer, it went to ruins within hours, even in the sheltered and prosperous Californian town (Zimbardo 1969).

Correlation analysis supports the claim that the broken windows policy, measured by the number of traffic tickets (Wilson and Boland 1978), the number of arrests per police officer for disorderly conduct or driving under influence (Sampson and Cohen 1988) or of misdemeanour arrests (Kelling and Sousa 2001; Corman and Mocan 2005), contributed to the decline in serious crimes, even if one controls for economic conditions and for crime deterrence (Corman and Mocan 2005) (see also Cruz Melendez 2006: for the link to the "Moving to Opportunity" Program). Yet, other studies did not find a significant effect. They used a complex index of perceived social disorder as the independent variable (Sampson and Raudenbush 1999). Information about law-abiding or the number of abandoned buildings did not have a significant influence either on young males' beliefs about the risk of being convicted (Lochner 2007); (see also the mixed results by Taylor 2001). More importantly, it is far from undisputed whether correlation can be interpreted as causation (Harcourt 1998; Harcourt 2001; Sampson, Morenoff et al. 2002; Harcourt 2005; Harcourt and Ludwig 2006). Note, however, that time series evidence from Switzerland shows tougher enforcement of mild crimes to reduce the incidence of severe crimes in later years (Funk and Kugler 2003). Nonetheless, hardly any observer doubts that the success in US cities has had multiple causes. In this paper, we try to mitigate these concerns by supplementing the existing field evidence with behavioural data gathered under *controlled conditions* in laboratory experiments. That way we are also able to isolate the effect of first impressions, and can stay clear from the accompanying danger of justifying the use of stereotypes, like race (cf. Stewart 1998), or from using the broken windows metaphor as a pretext for generating social segregation (Seiler 2008).

The data which we use stems from a series of public-good experiments that were conducted by several authors all over the world; including our own, new contributions to this literature. Our

1 For details, see Uniform Crime Reports, at http://www.fbi.gov/ucr/ucr.htm .

data set features regular public-good games, public-good games with punishment opportunities, and public-good games with punishment and counter-punishment opportunities. On first inspection, this data does not appear very appealing for our purpose. In regular public-good experiments, subjects' contribution rates vary widely (for surveys see Ledyard 1995; Zelmer 2003). While giving participants the costly opportunity to punish each other tends to raise average contributions (Fehr and Gächter 2002), there is still a high degree of variance in the observed contribution rates (Herrmann, Thöni et al. 2008). The variance is also present when those receiving punishment are given a chance to strike back (Denant-Boèment, Masclet et al. 2007; Nikiforakis 2008), although contribution rates now drop on average. Essentially, the large body of experimental data appears almost chaotic; apparently independent of the design, some groups are able to sustain a considerable degree of cooperation throughout the game, while other groups completely fail. This even holds if experiments are run under the same protocol and in one and the same lab.[2]

However, as we demonstrate in this paper, the apparent puzzle dissolves as soon as we control for initial impressions. By the very fact that we can generate order in this dataset, we can show that "broken windows" destroy socially desirable behaviour, even under the context-free, clean conditions of a lab experiment. If others contribute a substantial amount of their endowment in the beginning, the group is very likely to collect a lot of money for the joint project. If initial impressions are bad, the opposite effect can be predicted. Initial impressions also have a significant effect if we control for the respective player's own initial contributions, i.e., for her type. Thus, we do not measure favourable attitudes, but we indeed see the effect of one's environment on one's behaviour. In the lab, participants do not literally break windows, or see them broken. Yet they all know full well that it would be socially desirable for everyone to contribute their entire endowments to the joint project. The less the other group members do so in the first round, the more "windows are broken" in the local environment in which this one player happens to be for the rest of the game.

The remainder of this paper is organised as follows. Section 2 links our work to the related literature. Section 3 describes the dataset and the experimental designs. Section 4 presents the puzzling evidence we observe in the dataset. Section 5 dissolves the puzzle by using different measures for first impressions. Section 6 discusses implications for the behavioural theory of public-goods and for public policy.

2. Related Literature

The closest analogue to our study in the legal literature is a field experiment that randomly exposed 12 of 24 matched violent crime places in Jersey City to intense police scrutiny and intervention. In the places chosen, crime rates dropped substantially, while they did not in the unaf-

2 For details, see the comparison below between our experiments and the Hermann experiments in the Bonn EconLab.

fected places (Braga, Weisburd et al. 1999). In a similar vein, in a series of sociological field experiments, when there were signs of disorder, like graffiti, abandoned shopping carts, or bicycles locked where they were not supposed to be, this induced passers-by also to break these and other rules (Keizer, Lindenberg et al. 2008).

Our dataset differs in that our "intervention" is much more light-handed; it is confined to the first impressions subjects happen to make. Moreover, since we conducted lab experiments, we need not have second thoughts about the influence of explanatory variables beyond our control. A further advantage of our approach stems from the nature of both the dependent and the independent variables. In the field, both are categorical: people either break the law or they obey it; people either see disorder or they do not. In our setting, "disorder" is measured by the distance from socially optimal behaviour, and socially desirable behaviour is measured by the amount bystanders contribute to the joint project. Due to that feature, we are also able to distinguish between the overall level of disorder and the maximum disorder participants experience in the group of which they happen to be a member. Finally, since all our data is from games repeated over 10 periods, we can also analyse the dynamics triggered by favourable or unfavourable first impressions.

Another lab experiment from the legal literature demonstrates that the law can serve as a focal point if participants perceive the situation as a coordination problem (McAdams and Nadler 2008). We, however, go one step further, in that our setting exposes participants to a true dilemma. In game-theoretic parlance, we are studying a prisoner's dilemma, while the previous experiment tested a hawk/dove game. Since in a prisoner's dilemma defection is a dominant strategy, ours is an even stronger test for the power of orientation.

In the economics literature, the closest analogue is an experiment where, in a first stage, participants were screened for their cooperativeness. In the second stage, they played a standard public-good game, knowing that they were interacting with partners that scored like them in the pre-test. In a voluntary contribution mechanism, this unequivocally increased cooperation, even for those scoring low in the pre-test. However with punishment, overall contributions decayed, due to very poor performance of those scoring low in the pre-test (Gächter and Thöni 2007). The effect of sorting is positive throughout if subjects are rematched every round according to their cooperativeness in the previous round (Gunnthorsdotir, Houser et al. 2007). Likewise, if groups have a chance to exclude freeriders, this improves cooperation in a dilemma setting (Cinyabuguma, Page et al. 2005; Croson, Fatas et al. 2008), as does a mechanism that allows members to self-select into groups (Page, Putterman et al. 2005), in particular if freeriders are effectively excluded by a rule that sacrifices a portion of the group income to outsiders (the Red Cross, as it was) (Brekke, Hauge et al. 2009). Our study differs from this literature in that all we use is an element present in any public good game, and in any real life social dilemma: the first impressions participants happen to make.

3. The dataset

In the public-good games that we consider in this paper, players interact repeatedly for t periods in groups of size n. Each player in the group has the following payoff function π_i:

$$\pi_i = e_i - g_i + \mu \sum_{k=1}^{n} g_k - \alpha(p_{ij}) - \beta(p_{ji}) - \gamma(cp_{ij}) - \delta(cp_{ji})$$

where e_i is the endowment, g_i is this player's contribution to the public good, and μ is the marginal per capita rate of return for contributions to the public good, $k \in n$ is any of the group's members, including i. If punishment is an option, players observe each others' contribution decisions and can then assign costly punishment points, where p_{ij} is the number of punishment points player i gives to any player $j \neq i$, and p_{ji} is the number of punishment points player i receives from either player j. The payoff loss associated with received and given punishment points is a function of punishment points given to all other or received from all other group members, respectively.

If the game also features counter-punishment opportunities, players receive feedback about the punishment decision made by others and can then again assign costly counter-punishment points, where cp_{ij} is the number of counter-punishment points player i gives to player j, and cp_{ji} is the number of counter-punishment points player i receives from player j. The payoff loss associated with received and given counter-punishment points is a function of the sum of counter-punishment points given or received.

The following is partly a reanalysis of data from public good experiments that are already published (Denant-Boèment, Masclet et al. 2007; Herrmann, Thöni et al. 2008; Nikiforakis 2008), and partly of our own, hitherto unpublished data. Table 1 informs about the different design features and parameters in more detail. All games are played in groups of $n = 4$, with an endowment of $e_i = 20$ per player, and a marginal per capita rate of return of $\mu = 0.4$.

The first column indicates whether participants had no technology for targeted sanctions (VCM), or whether they could punish each other without (Pun) or with the risk of counterpunishment (CPun). The second column lists whether subjects stayed together in the same group of four throughout the game (partner design, *P*) or whether they were rematched every round (stranger design, *S*). Column three has identifiers for each experiment, to be used in later tables. The fourth column indicates the origin of the data, where *MPI* denotes our own experimental data, *DEN* is data provided by Denant-Boèment et al. (2007),[3] *NIK* is data taken from Nikiforakis (2008), and *HER* is data published in Herrmann et al. (2008), which consists of 16 structurally identical experiments run in different countries.[4] The fifth column gives the total number of individual decisions in the respective dataset. The sixth column denotes the number of periods that

3 The original dataset of Denant-Boèment et al. (2007) contains 20 periods. To keep datasets comparable, only the first ten periods of each matching group are considered in our analysis.

4 Athens (Number of observations N = 440), Bonn (600), Boston (560), Chengdu (960), Copenhagen (680), Dnipropetrovs'k (440), Istanbul (640), Melbourne (400), Minsk (680), Muscat (520), Nottingham (560), Riyadh (480), Samara (720), Seoul (840), St. Gallen (960), Zurich (920).

were played. More detail on experimental procedure and on the instructions of our own, new data is to be found in the Appendix.

game-type	matching	exp #	dataset	# obs.	T	P techn.	CP techn.	punishment feedback
VCM	P	2	MPI	240	10	-	-	-
VCM	P	12	NIK	960	10	-	-	-
VCM	P	18	MPI	480	12	-	-	-
VCM	S	13	NIK	960	10	-	-	-
Pun	P	6	DEN	480	10	FG	-	-
Pun	P	11	MPI	240	10	FG	-	-
Pun	P	14	NIK	480	10	FG	-	-
Pun	P	16	HER	10400	10	1:3	-	-
Pun	S	15	NIK	480	10	FG	-	-
CPun	P	1	MPI	680	10	FG	FG	own
CPun	P	3	NIK	480	10	FG	FG	own
CPun	P	7	DEN	480	10	FG	FG	all
CPun	P	8	DEN	480	10	FG	FG	others
CPun	P	9	DEN	480	10	FG	FG	own
CPun	P	10	MPI	480	10	FG	SEV	own
CPun	S	4	NIK	480	10	FG	FG	own
CPun	S	5	MPI	640	10	FG	FG	own

Table 1
Data Structure

The seventh and eighth columns denote which punishment or, as the case may be, counter-punishment technologies were used. Here, *1:3* indicates that a linear technology was used where each punishment point assigned costs 1 token and reduces the other's payoff by 3 tokens, i.e. $\alpha(p_{ij}) = \sum_{j \neq i} p_{ij}$ and $\beta(p_{ij}) = 3\sum_{j \neq i} p_{ji}$. *FG* indicates that a technology based on the seminal paper by Fehr and Gächter (2000) was used, where the cost function $\alpha(p_{ij})$ is convex and each assigned (counter-)punishment point reduces the receiver's payoff by 10%.[5] *SEV* indicates that a severe technology was used, where each assigned counter-punishment point costs 1 token and reduces the receiver's net payoff (after the effect of received and the cost of given punishment are subtracted) by 25 %. The last column describes the amount of information that subjects were given on the counter-punishment stage, where *own* indicates that subjects only knew the amount of punishment they had received themselves, *others* indicates that subjects only knew by how much the other members of the group had been punished, and *all* indicates that subjects knew whether and by how much each subject had been punished.

4. The puzzle

As can be seen in Table 2, overall means are representative of what is typically found in the corresponding designs: contributions are higher if the same four players stay together over all ten periods, compared to the stranger protocol where they are randomly re-matched every period. Contributions are lowest in the absence and highest in the presence of punishment opportunities.

5 For more detail, see Nikiforakis (2008) and Denant-Boèmont et al. (2007).

Counter-punishment dampens contribution rates, though they are still substantially higher than without punishment.

	VCM	CPun	Pun	Total
Stranger	5.41	10.29	11.63	8.71
Partner	7.63	13.28	13.57	12.90
Total	6.80	12.32	13.49	12.30

Table 2
Mean Contributions

Yet, if we look at the mean contribution rate for each dataset individually, one already sees the chaotic nature of the data (cp. Figure 1). There is huge variance in the mean contribution rates. Even if we control for the matching protocol, the data still looks unstructured, e.g., for CPun the lowest mean is observed under a stranger matching, while the lowest mean in VCM and in punishment stems from a partner matching. Also if we control for the location of the laboratories, contribution rates remain rather unstructured. In all locations there is huge variance within data from one and the same lab. For example, although our own experiments that were run in Bonn have the highest means in all three game types, in the Hermann data set there is an identical experiment in the same lab where mean contributions are only 14.49, while they are 14.65 in Seoul, 15.01 in Nottingham, 16.15 in Zurich, 16.73 in St. Gallen, 17.75 in Copenhagen and 17.98 in Boston.

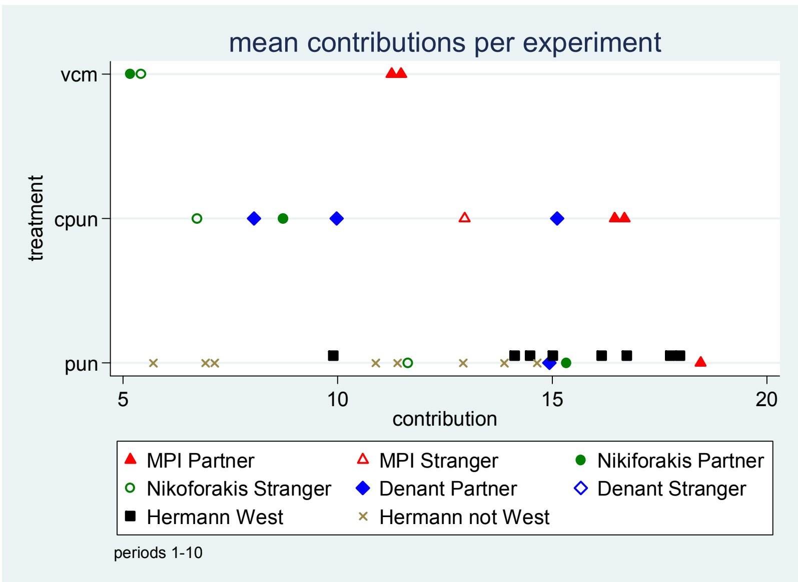

Figure 1
Mean Contributions per Experiment

There is huge variation even within each experimental design, with mean contributions per experiment ranging between [5.16 11.48] in the VCM-designs, [6.72 16.68] in CPun, and [5.70 18.46] in Pun. This variation is also stable across the periods of the respective experiment. Figure 2 illustrates this with the counter-punishment data, displaying mean contributions per treatment and period. In all periods, mean contribution rates differ substantially between experiments. The first column in Table 3 shows that almost all of these differences even reach statistical significance.

The most striking result is from the MPI severe treatment (exp # 10). In this experiment we made counterpunishment extremely powerful. At the cost of just one token, participants could destroy a quarter of the period income of those who had punished them. Nonetheless, contributions are significantly above all other non-MPI counterpunishment experiments.

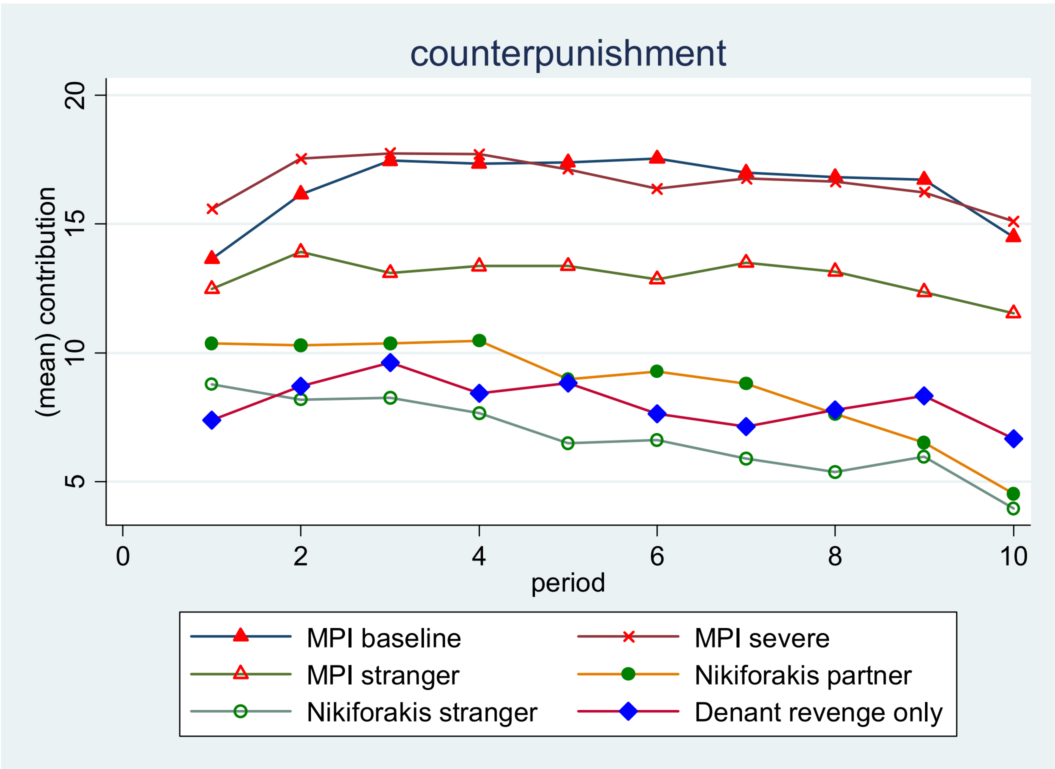

Figure 2

Counterpunishment: Contributions per Treatment and Period

exp #	1	5	10	3	4	9
data-set	MPI	MPI	MPI	NIK	NIK	DB
partner	P	S	P	P	S	P
# of ind. observations	17	6	12	12	4	6
1	-					
5	2.311*	-				
10	-1.063	-1.593	-			
3	3.410***	1.686*	3.061***	-		
4	2.688**	1.919*	2.548**	.728	-	
9	2.871**	1.441	2.718**	.281	-.640	-

Table 3

Pairwise comparisons of mean contributions per matching group between counter-punishment datasets (two-sided ranksum test)

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

5. Broken Windows in the Lab

The apparent chaos dissolves as soon as we control for initial impressions – in particular when a group's average contributions in the first period are used as a proxy for initial impression. Even when any context is deliberately and painstakingly removed, people are strongly impressed by the experiences they make when they enter such an artificial community. If "the windows are broken", i.e., if other participants are selfish and do not contribute to the joint project, they reduce their contributions as well. In Figure 3, we plot the mean contribution in the first period versus the mean contribution in the nine subsequent periods. If a point lies on the y=x line, initial impressions have fully determined subsequent behaviour. We find that the VCM results lie somewhat below this line, Pun results lie somewhat above it, and CPun results can be found on either side of the line. This slight qualification resulting from the different institutional framework notwithstanding, the correlation between first period's impression and subsequent behaviour is clearly visible in all three game-types. Note that we are not only demonstrating a detrimental effect from observing "broken windows". We also show that the *degree* of deviation from the social optimum is tuned to the initially observed degree of norm compliance.

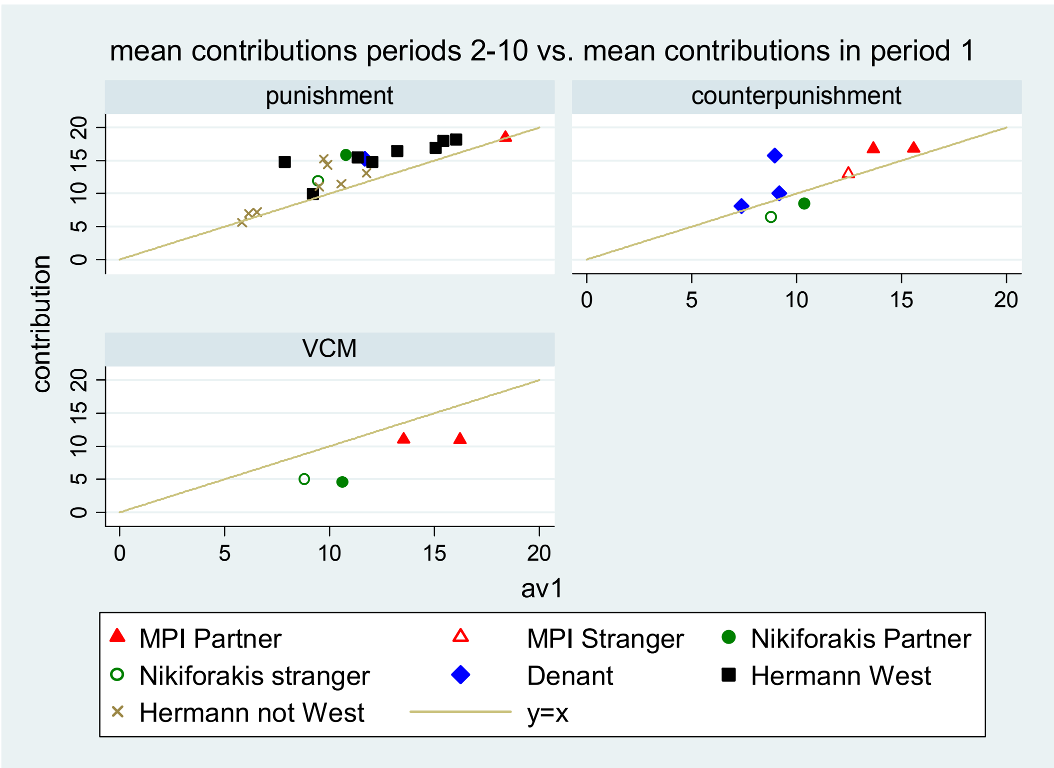

Figure 3

Effect of Average Contributions in the First Period

Mean contributions in all periods, of course, excludes the first period's contribution

Average contributions in the first period inform subjects about the level of cooperativeness in their group. Additionally, they learn how strongly they risk being exploited by looking at the first period's minimum contribution in their group. We thus not only observe whether any "windows are broken" in the community. We also observe, on a gradual scale, how badly the worst member of the group behaves, and how this affects the behaviour of other group members in later rounds. As Figure 4 shows, minimum contributions in the first period and mean contributions over all periods are related as well. The higher the minimum contribution in the first period, the higher the overall contributions in this group. The fact that most points lie above the y=x line reflects that, on average, the remaining participants do not behave as poorly in later periods as the worst behaving member in the first period – but their behaviour is clearly pulled down to this worst observation.

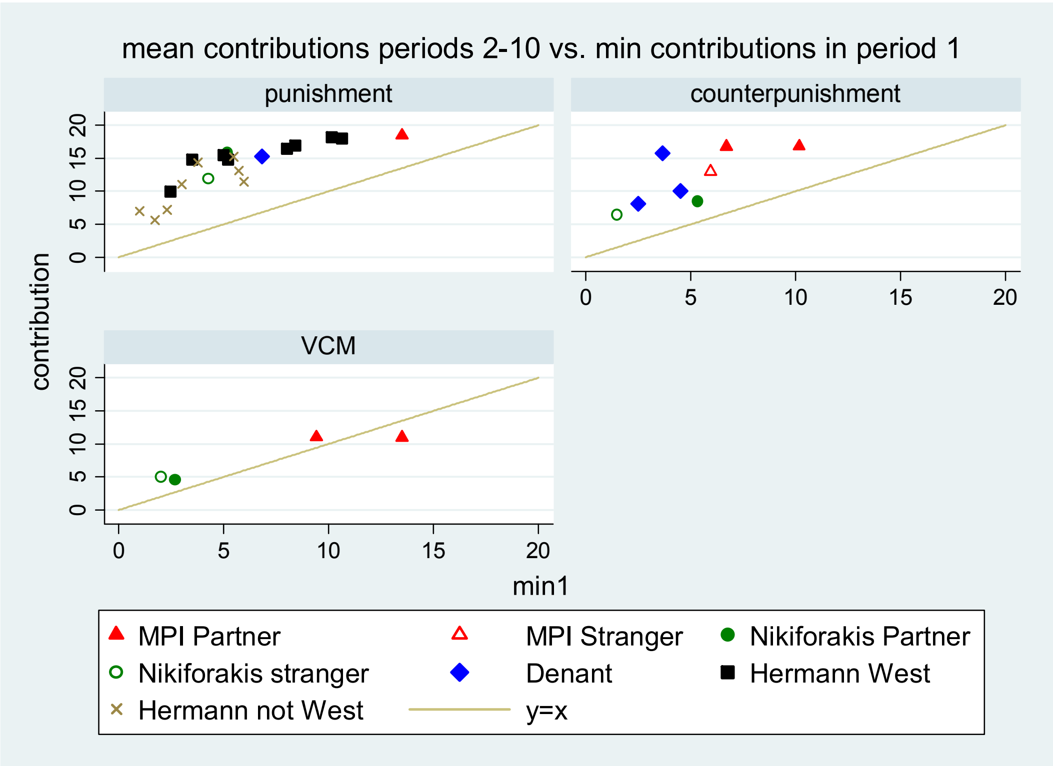

Figure 4
Effect of Minimum Contribution in the First Period

The impressions from the graphs are supported by regression analysis. A first series of regressions reported in Table 4 only uses means per group of four, over periods 2-10 each, as the dependent variable. Regressor *av1* is the average of the contributions of the four group members, in the first period. Regressor *min1* is the smallest contribution of one of these four members, again in the first period. In the reference category, group members are rematched every period. They stay together in the *Partner* design. In the reference category, targeted punishment is not possible. This is different in *Ptreat*. While in *CPtreat*, there is also punishment, it comes at the risk of counterpunishment. In a model which controls for partner vs. stranger design, and for VCM vs.

punishment vs. counterpunishment, plus the average contribution in the first period, we explain 61% of the variance. In the second model, the minimum contribution in the first period is also highly significant. We still explain 46% of the variance. However, if we add both regressors (model 3), the estimate for *min1* is very small and insignificant. The adjusted R^2 is virtually the same as in model 1. As model 4 demonstrates, this is due to the interaction between both parameters for initial impressions. If one adds the interaction term, *min1* is again significantly positive, while the interaction term is small, but negative. This result is best interpreted in an example. Assume a VCM stranger game with av1 = 15, min1 = 10. Then model 4 predicts -4.081 [cons] + 15*.853 [av1] + 10*.390 [min1] – 15*10*.021 [av1*min1] = 9.464 mean contributions. The more both first impressions are favourable, the more their combined effect has to be discounted. But in relative terms, discounting is small. It never reverses either main effect. There is no significant interaction between either *av1* or *min1* and the partner design (model 5).

Yet model 6 shows that first impressions matter more, and differently, with either punishment or counterpunishment. If one controls for these interactions, the main effect of *av1* is no longer significant, while the main effect of *min1* is. Again the prediction is best understood in an example. Assume a punishment stranger game, again with av1 = 15, min1 = 10. Model 6 predicts -.981 [cons] + 15*.396 [av1] + 10*.860 [min1] + 1.421 [ptreat] – 15*10*.021 [av1*min1] + 15*.733 [av1*ptreat] – 10*.546 [min1*ptreat] = 17.365. Compare the regressors for *av1* and *av1*ptreat*, and for *min1* and *min1*ptreat*: While the effect of *av1* becomes even stronger with punishment, the effect of *min1* is reduced (but the overall effect is still positive).

This is intuitive: punishment gives participants a chance to discipline freeriders. They are the more likely to make productive use of this opportunity, the more the overall impression from the group is positive. The respective interaction terms with counterpunishment show the same picture. This indicates that, behaviourally, counterpunishment is mainly punishment. Interestingly, in model 6 the main effects for punishment and counterpunishment are no longer significant. The main effect is fully explained by the interactions with *av1* and *min1*. We learn that "broken windows" not only deteriorate the willingness of bystanders to abide by the law. They also reduce their preparedness to defend the law themselves (punishment) and to do so at the risk of being attacked in reaction (counterpunishment). Not only law obedience suffers. Courage to stand up for the common good wanes as well.

	model 1	model 2	model 3	model 4	model 5	model 6
av1	.851***		.810***	.853***	.835***	.396
min1		.578***	.042	.390**	.547*	.860**
av1*min1				-.021**	-.020**	-.021**
Partner	.873	1.006	.845	.881	1.284	3.238
av1*partner					.017	-.234
min1*partner					-0.162	.035
Ptreat	7.272***	6.660***	7.243***	7.049***	7.011***	1.474
Cptreat	6.139***	5.872***	6.119***	5.845***	5.793***	-.871
av1*ptreat						.733***
min1*ptreat						-.546***
av1*cptreat						.791**
min1*cptreat						-.552*
Cons	-3.872***	2.872***	-3.549***	-4.081***	-4.429*	-.981
N	405	405	405	405	405	405
adj R^2	.618	.469	.617	.624	.622	.628

Table 4

Effect of First Impressions on Mean Contributions per Group

OLS, robust standard errors, period 1 excluded, reference group: VCM stranger

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

First impressions do not only matter for the level, but also for the development of contributions over time. We again first only use one observation per group as the dependent variable. Variable *trend* is the coefficient for regressor period in a fixed effects regression explaining contributions with period (and a constant), separately for each group.[6] A positive trend means that, in this group, contributions increase over time. The positive regressors for *partner*, *ptreat* and *cptreat* corroborate what is generally observed: in the partner design, and with punishment, contributions are more likely to increase over time. Once more, *min1* is insignificant without the interaction term (model 1), but significant if one adds the interaction (model 2). Interestingly, the regressor for *av1* is negative throughout. This finding should be put into perspective. If participants stay together for the entire game, and if they can punish each other, even if all had contributed the maximum of 20 in the first round, the trend remains positive.[7] Moreover if the minimum contribution in the first round is high, the negative coefficient for av1 is neutralised. Initial overall impressions only lose their influence over time if both the worst group member behaved very badly in the first round, and if institutions are not powerful enough to bring her under control. From a policy perspective, the finding for *min1* is most relevant. High *min1* can be equated with a setting where no windows are broken at all, or where infractions are at most very minor. According to our regression, in such a context, not only a high overall degree of socially desirable behaviour can be expected. One can even expect that the willingness to contribute to the common good grows substantially over time, the more so, the better the worst member behaved initially.

6 Since, in this model, the only regressor is time-dependent, a random effects model would not be more efficient; which is why we directly go for the consistent model with individual fixed effects. All models are $y_{it} = \alpha_i + trend * period + \varepsilon_{it}$

7 .372 [partner] +.422 [ptreat] – 20*.031 [av1] = .174.

	model 1	model 2
av1	-.035**	-.031**
min1	.005	.041*
av1*min1		-.002
partner	.368***	.372***
ptreat	.442***	.422***
cptreat	.230*	.202*
cons	-.186	-.242
N	405	405
adj R^2	.153	.158

Table 5

Explaining Trend of Contributions over Time per Group

OLS, robust standard errors, period 1 excluded

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

Level and slope means per group do not exploit the full richness of the dataset. More importantly even, at the level of groups we cannot distinguish between a person's own willingness to contribute in the first round and the contributions of the remaining group members. We cannot rule out that the effects we observe in periods 2-10 just reflect differences in group composition. Our results would say something about ex ante heterogeneity, not about the causal effect of initial experiences on later behaviour. However at the individual level, we can disentangle the effect of one's type from the effect of initial experiences. To that end, we apply a random effects model that uses all contributions of all subjects in all periods,[8] Table 6. Model 1 shows that the positive effect of average contributions at the group level does not just reflect the exogenously given type of players. While this type is captured by the own contributions of the respective player in the first round (i.e., by variable *con1*), there is an independent effect of the average contributions of the remaining three players in the first round (i.e., of variable *avf1*). Model 2 shows that the same holds true for the minimum contribution of another player in the first round. Variable *minf1* has a significant independent positive effect for a player's contributions in later periods if one controls for her own contributions in the first period.

As with group data on all four players, if one simultaneously adds the average and the minimum contribution of one of the remaining players as a regressor, minimum contributions are no longer significant, model 3. They are again weakly significant if one adds the interaction of average and minimum contributions to the model, model 4. The interaction term itself is significant and negative. As with group data, the combined positive effect of high average and high minimum contributions in the first round is somewhat corrected downwards.

8 On all models, the Hausman test is insignificant, so that we are justified in using the more efficient random effects model. Qualitatively, results look very similar if we run a random effects Tobit model; see Appendix. In particular, the picture on trend variables (period, con1*period, avf1*period, minf1*period, con1*period*ptreat, avf1*period*ptreat, minf1*period*ptreat, con1*period*cptreat, avf1*period*cptreat, minf1*period*cptreat) remains the same. We may thus be sure that these trend variables do not reflect bottom or ceiling effects.

Model 5 conveys an interesting message: while the beneficial effect of a player's type decays over time, this is not the case with the positive effect of initial impressions. Model 6 looks at differential effects for treatments. The beneficial effect of high average contributions, by the remaining players in the first round, is strongest with counterpunishment, and slightly less strong with punishment. To appreciate the size of the effect, consider the following example: the experiment allows for punishment and counterpunishment in the stranger design; the player under consideration has contributed 5 units in the first round; on average the remaining players have contributed 10 units; the minimum contribution was 1 unit. For contributions in the fifth round, the model predicts a contribution of 7.637 units. If the otherwise identical parameters are from a game with punishment only, the model predicts contributions of 9.641. The larger main effect for punishment is ultimately more important than the smaller interaction with average contributions in the first round. It even neutralises the negative interaction with minimum contributions in the first round.

Model 7 adds the three-way interactions of initial conditions with treatment and period. Comparing with the two-way interaction between the respective initial condition and period, one learns that the beneficial effect of type decays less quickly with punishment or counterpunishment, but that the combined effect of the two-way and the three-way interactions is still negative. Consequently, even when there is punishment or counterpunishment, the beneficial effect of type is not stable. This is different with the effect of average contributions of the remaining players in the first round, when there is punishment. The combined effect of the two-way and the three-way interactions is (slightly) positive (-.047 + .051 = .004). This qualifies the finding at the group level regarding the negative effect of regressor *av1* on variable *trend*. The negative effect at the group level results from the dwindling effect of the player's own type (which enters the calculation of *av1*), not from initial impressions.

	model 1	model 2	model 3	model 4	model 5	model 6	model 7
con1	.409***	.457***	.409***	.411***	.624***	.561***	.718***
avf1	.423***		.437***	.476***	.499***	.221+	.483***
minf1		.266***	-.013	.160+	.152+	.293**	.235*
avf1*minf1				-.011*	-.011*	-.008	-.008
period	-.049	-.049	-.049	-.049	.379***	.379***	.411***
con1*period					-.035***	-.035***	-.062***
avf1*period					-.004	-.004	-.047***
minf1*period					.001	.001	.011
partner	.923	1.364*	.911	1.037+	1.037+	1.374*	1.374*
ptreat	6.924***	6.962***	6.920***	6.908***	6.908***	4.331***	4.331***
con1*ptreat						.065	-.139*
avf1*ptreat						.312*	.009
minf1*ptreat						-.234*	-.168+
cptreat	5.510***	5.879***	5.495***	5.485***	5.485***	1.873	1.873
con1*cptreat						.093+	-.036
avf1*cptreat						.368*	.249
minf1*cptreat						-.213	-.289*
con1*period*ptreat							.034***
con1*period*cptreat							.022**
avf1*period*ptreat							.051***
avf1*period*cptreat							.020
minf1*period*ptreat							-.011
minf1*period*cptreat							.013
cons	-3.008***	-1.111*	-3.052***	-3.680***	-6.246***	-4.049***	-4.244***
N	16092	16092	16092	16092	16092	16092	16092
p model	<.001	<.001	<.001	<.001	<.001	<.001	<.001
R^2 within	.001	.001	.001	.001	.029	.029	.077
R^2 between	.579	.547	.579	.581	.581	.586	.586
R^2 overall	.426	.403	.426	.428	.435	.439	.451

Table 6

Explaining Individual Contributions with First Impressions

Random Effects, robust standard errors, clustered for groups (405 clusters), period 1 excluded

Hausman test insignificant on all models

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

Of course, all of the previous analysis can be redone at the level of game types, Table 7. Interestingly, in the stranger design, first impressions do only have explanatory power in the treatment with counterpunishment. This is intuitive. In the stranger design, impressions from the first period are a much weaker signal than in the partner design. The respective player only learns something about the large group of participants within which players are rematched every round. Arguably, counterpunishment introduces so much uncertainty, though, that even this weak signal from the first period becomes valuable. Moreover, while the beneficial effect of a player’s type decays over time in all treatments, the beneficial effect of first impressions only decays if there is no punishment and if players stay in matched groups for the entire game. Put differently, if there is punishment, even if it is more risky due to counterpunishment, initial impressions are more stable than the autonomous benevolence of a player. If society is able to quickly repair broken windows, this matters more than the good-naturedness of many.

VCM/P/CP	VCM	VCM	P	P	CP	CP
P/S	P	S	P	S	P	S
con1	.675***	.642***	.586***	.624***	.625***	.730***
avf1	.537**	.280	.576***	.472	.716***	.780***
minf1	.054	.164	.092	-.016	.256	.257
avf1*minf1	.002	-.018	$-.012^{+}$	-.015	-.020	-.038**
period	.569**	-.180	.514***	.920***	.519*	-.031
con1*period	-.060***	-.057***	-.029***	-.034***	-.045***	-.028**
avf1*period	-.077***	.022	-.008	-.020	-.021	-.012
minf1*period	.043*	-.026*	.007	-.026	.012	$.027^{+}$
cons	-3.441	1.311	.539	.788	-1.018	-2.798***
N	1440	864	10224	432	2164	1008
N cluster	40	8	284	4	59	10
R^2 within	.2380	.2032	.0325	.0807	.0574	.0655
R^2 between	.5634	.3999	.5242	.3635	.4410	.6176
R^2 overall	.4270	.2878	.3912	.2353	.3353	.4609

Table 7

Separate Regressions for Types of Games

Random Effects, depvar contribution, period 1 excluded, robust standard errors, clustered per matching group

Hausman test insignificant on all models

6. Conclusions

Our own results, together with a reanalysis of data from 30 experiments conducted all over the world, suggest that contribution rates in public-good experiments are highly sensitive to first impressions. Subjects seem to be most attentive to the level of the contributions of others in the first period. If there is a punishment option, the positive effect of high initial average contributions is even stronger.

Our findings are of potential interest to a broad number of experiments exploring cooperation, in particular those who share a public-good-like structure. While the whole enterprise has started off from theoretical models, with actors having standard preferences and therefore contributing nothing, experiments have established that a typical subject pool is heterogeneous. There are some unswerving altruists, and a substantial number of hard-nosed egoists, but typically the majority of subjects seem to be conditional cooperators. These persons are happy to resist the temptation to exploit others, provided the perceived risk of being exploited themselves is sufficiently low (Fischbacher, Gächter et al. 2001; Fischbacher and Gächter 2008). Experiences from the first period are the only signal subjects receive about cooperativeness in the specific group they happen to be assigned to. Consequently, gauging their subsequent behaviour to this initial signal is the best they can do.

Actually, the value of this signal is even greater. Not only does each and every conditional cooperator receive this signal herself. She may also be perfectly sure that the other group members simultaneously receive an identical signal. This second property of the signal matters to the extent that other group members are likely to be conditional cooperators, too. The setting then permits the formation of coordinated second-order beliefs. Actually, for the participants in public-good experiments, this is a testable proposition. In the next period, they directly receive a new signal, which they can use to check the reliability of their prediction, and to act accordingly.

Note two differences between the partner and the stranger protocol. Since, in the stranger design, subjects are rematched every period, the signal from the first period is less valuable. It only is a random draw from a larger population. Since, in a different group of four, other subjects have made different initial experiences, subjects also have no chance to coordinate second-order beliefs. Both differences provide a micro-level explanation for the fact that overall contributions are characteristically lower with the stranger protocol.

Our main message, however, is not addressed to fellow experimentalists, but to policy makers. Since, in the lab, we are able to isolate effects, we can prove that it is indeed good policy to repair broken windows as soon as possible, both literally and metaphorically speaking. The closest real-world analogue to our setting is a person who is new to a neighbourhood. If this person perceives a neat environment, she expects to be treated well if she behaves well herself. Note that we do not even need normativity to make this prediction. If, in addition, this person is generally willing to abide by the normative expectations prevalent in this community, of course the effect is even stronger. Neither do we need true altruists. All we need is a sufficient proportion of conditional cooperators plus, crucially, the right signals for those who newly enter the community.

The message to policymakers is straightforward. Money spent on impression management is likely to be money well spent. We can even be more specific. While good first impressions raise overall contributions in the voluntary contribution mechanism, and while they flatten the characteristic negative trend of contributions over time, they are not strong enough to reverse the trend. As many others have shown, both in the lab (Selten, Mitzkewitz et al. 1997) and in the field (Ostrom 1990), for cooperation to be sustainable, vigilance and enforcement are inevitable. However, sanctions alone are also not sufficient. The Hermann et al. experiments are particularly impressive on this. If overall performance was poor in a location, this was typically not due to a lack of (costly) punishment (Herrmann, Thöni et al. 2008). Our data suggests that the combination of favourable initial impressions and the existence of a sanctioning mechanism is essential. Being determined to prosecute culprits is thus not enough. In a consequentialist perspective, it is at least as important to manage impressions. Beware of broken windows!

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Appendix

Appendix 1: Tobit models on individual data

	model 1	model 2	model 3	model 4	model 5	model 6	model 7
con1	.792***	.885***	.794***	.792***	1.205***	.987***	1.135***
avf1	.878***		.792***	.722***	.756***	.015	.433*
minf1		.603***	.086	-.192^{+}	-.169	.050	.006
avf1*minf1				.019**	.020**	.027***	.026***
period	-.031	-.030	-.031	-.031	.756***	.758***	.636***
con1*period					-.069***	-.069***	-.094***
avf1*period					-.006	-.006	-.073***
minf1*period					-.006	-.006	.007
partner	1.841**	2.748***	1.915***	1.710**	1.695**	2.680***	2.672***
ptreat	12.214***	12.373***	12.247***	12.304***	12.275***	2.886^{+}	3.380*
con1*ptreat						.247**	-.077
avf1*ptreat						.824***	.296
minf1*ptreat						-.376**	-.370*
cptreat	9.401***	10.279***	9.504***	9.527***	9.522***	-.383	-.177
con1*cptreat						.275**	.238*
avf1*cptreat						.921***	.627*
minf1*cptreat						-.485**	-.624**
con1*period*ptreat							.052***
con1*period*cptreat							.008
avf1*period*ptreat							.084***
avf1*period*cptreat							.047*
minf1*period*ptreat							-.006
minf1*period*cptreat							.018
cons	-14.947***	-11.319***	-14.691***	-13.706***	-18.368***	-10.755***	-10.321***
N	16092	16092	16092	16092	16092	16092	16092
left censored	2052	2052	2052	2052	2052	2052	2052
right censored	5792	5792	5792	5792	5792	5792	5792
p model	<.001	<.001	<.001	<.001	<.001	<.001	<.001

Table 8

Explaining Individual Contributions with First Impressions

Random Effects Tobit, period 1 excluded

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

Appendix 2: Experimental Procedure and Instructions for New Data

As can be seen from Table 1, six of the 17 data sets stem from experiments conducted at the Max Planck Institute for Research von Collective Goods in Bonn. Subjects were randomly recruited from the BonnEconLab's pool of about 3,500 subjects, mainly students (from all kind of majors), and participated in one of the treatments as indicated in the table below. None of them had previous experience in public good games, with the exception of participants of experiments #2 and #11, which were conducted with subjects that had before participated in experiment #10 (severe counter-punishment technology) as a first part of the respective session.

After subjects arrived in the lab, they were randomly and anonymously assigned to matching groups. Subjects then received a written copy of the instructions. Additionally, in order to create common information about the instructions, we read them out aloud to our subjects. The instructions were written in a neutral language, avoiding words like punishment, sanctions, counter-punishment etc. Instead, we used terms like "to assign points", "direct points", "transfer to a project", etc, which have been previously used in comparable studies. The instructions used were those of Fehr and Gächter (2000) unless otherwise indicated in the table below. For those experiments that made use of a modified version of these instructions, an English translation of the German instructions is included in Appendix 3. The instructions in German are available from the authors upon request. Before the game started, participants had to answer a set of control questions to make sure that everybody had understood the rules of the game. The experiment lasted for approximately 60 minutes. Subjects were paid according to their cumulated period payoffs. The experiments were programmed in zTree (Fischbacher 2007) and participants were recruited using ORSEE (Greiner 2003).

game-type	matching	exp #	date	# sessions	# subjects	T	instructions
VCM	P	2	22 April 2008	1	24	10	Fehr + Gächter (2000)
VCM	P	18	7 April 2008	2	40	12	Fehr + Gächter (2000) but over 12 periods
Pun	P	11	22 April 2008	1	24	10	Fehr + Gächter (2000)
CPun	P	1	24 January 2008 and 14 February 2008	5	68	10	Nikiforakis (2008)
CPun	P	10	22 April 2008	2	48	10	Severe Punishment Technology, instructions see Appendix 2
CPun	S	5	4 March 2008	3	64	10	Nikiforakis (2008)

Table 9
MPI Experiments

Appendix 3: Instructions for the Severe Counterpunishment Treatment (Experiment # 10)

General explanations for participants

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

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

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

1 Taler = 4 Euro cent

In addition to the 4 euro for showing up, each participant will receive from us one instalment of **25 taler**, with which you will be able to counterbalance potential losses. **However, you will always be in a position to exclude <u>with certainty</u> the possibility of losses, with your own decisions!** The taler you will have accumulated and the 4 euro will be paid to you **in cash** at the end of the experiment.

The experiment consists of two parts. To begin with, the first part will be explained. Explanations concerning the second part will be given later.

The experiment is divided into separate periods. It consists of a total of 10 periods. Participants are randomly assigned into groups of four. Each group, thus, has three further members, apart from you. During these 10 periods, the constellation of your group of four will remain unaltered. **For 10 periods you will therefore be in the same group.** Please note that the identification number assigned to you and the other members of the group changes randomly in each period. Group members can therefore not be identified as the periods progress.

In each period, the experiment consists of **3 steps**. In Step 1, you have to decide how many taler you wish to contribute to a project. In Step 2, you are told how much all other players contributed to the project and can decide, by giving points, on whether and by how much the other group members' income from Step 1 should be increased or reduced. In Step 3, those players whose income was reduced in Step 2 can, in turn, reduce the income of the same players who did this to them.

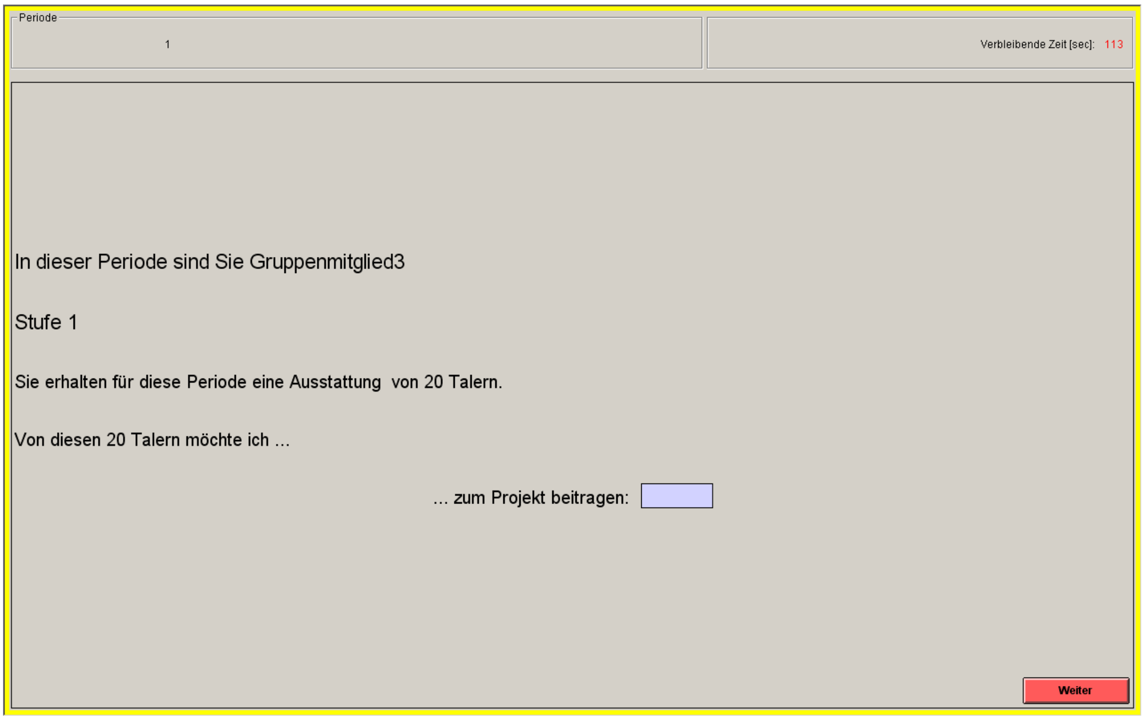
The following pages outline the exact procedure of the experiment.

Information on the exact procedure of the experiment

Step 1

At the beginning of each period, each participant is allotted **20 taler**, which we shall henceforth refer to as his **endowment**. The player's job is now to make a decision with regard to using his endowment. You have to decide how many of the 20 taler you wish to pay into a **project** and how many you wish to keep for yourself. The consequences of your decision are explained in greater detail below.

Your **endowment** is, thus, **20 taler in each period**. You make a decision on your project contribution by typing any one whole number between 0 and 20 into the appropriate field on your screen. This field can be accessed using the mouse. As soon as you have determined your contribution, you have also decided on how many taler to keep for yourself, i.e., **20 – your contribution**. Once you have typed in your contribution, please click on **Continue**, again using the mouse. Once you have done this, your decision for this period is irreversible.



Once all members of the group have made their decisions, you will be told how high the total sum of contributions from **all** group members (including your own) to the project is. In addition, you are informed about your own contribution and the number of taler kept by you; you are also told how many taler you have earned in total during Step 1.

Your income therefore consists of two parts, namely:

(1) the taler you have kept for yourself (**"income from taler retained")** and

(2) the **"income gained from the project"**. Your income from the project is .4 times the total sum of all contributions to the project.

Your total income from Step 1 is therefore calculated as follows:

Total taler income at the end of Step 1

= income from taler retained + income from the project

Income from the project = 0,4 × Total sum of all contributions to the project

The total income at the end of Step 1, in taler, is calculated according to the same formula for each member of the group.

If, for example, the sum of the contributions from all group members adds up to 60 taler, you and all other members each receive a project income of .4x 60 = 24 taler. If the group members have contributed a total of 9 taler to the project, you and all other members each receive an income of .4x9 = 3.6 taler from the project.

For each taler you keep for yourself, you earn an income of 1 taler. If, on the other hand, you contribute one taler from your endowment to your group's project instead, the sum of the contributions to the project increases by one taler and your income from the project increases by .4x1 = .4 taler. However, the income of each individual group member also increases by .4 taler, so that the group's total income increases by .4x4 = 1.6 taler. The other group members thereby also profit from your contributions from the project. In turn, you profit from other members' contributions to the project. For each taler contributed to the project by another group member, you earn .4x1 = .4 taler.

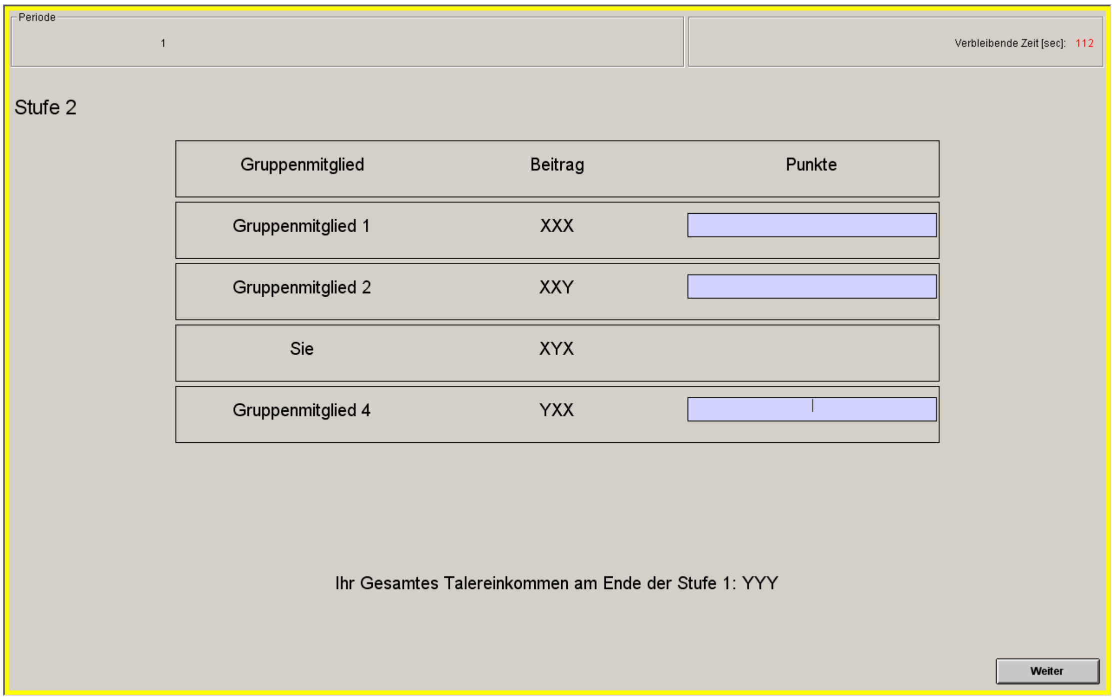
When you have finished, please click on **Continue**, using the mouse. Step 1 is now over and Step 2 about to begin.

Step 2

In Step 2, you will be told how many other group members have contributed to the project. In addition, you can **decrease, or leave as it is**, the income of **each** individual group member by giving points. All other group members are allowed to decrease **their** income, too, if they so wish.

In order to do this, you will be shown on your screen how many taler each individual group member has contributed to the project; in other words, you are told the identification number, for the current period, of each group member, as well as their contributions.

You now have to decide for **every** group member (excluding yourself) how many points you wish to give them. It is compulsory to enter a figure at this stage. If you do not wish to alter a certain group member's income, please insert 0. You can operate within the fields underneath the line **"Points"** by using the tab key (→|) or the mouse.



When distributing points, you incur costs in taler which depend on the number of points you distribute to the individual players. **Distributed points are numbers between 0 and 10.** The more points you give an individual player, the higher your costs are. The total costs in taler are calculated as the sum of the costs of all points distributed to all other group members. The following table shows the connection between the points distributed to an individual group member and the costs of such distribution in taler:

Table 1: Costs of the distribution of points to *one* other group member in Step 2

Points given to a group member	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Cost of these points in taler	0	1	2	4	6	9	12	16	20	25	30

Your total cost of the points distribution is the sum of all costs to all three other group members. For example, if you have allocated 2 points to one member, your cost is 2 taler; if, in addition, you give 9 points to another group member, your cost is 25 taler; if you give the final group member 0 points, you have no costs. The **total cost** to you is therefore 27 taler (2+25+0). As long as you have not yet clicked on **Continue**, you may still change your decision.

If you choose 0 points for a certain group member, you do not alter this group member's income. With each **point** allocated to a group member, you **decrease** this particular group member's taler income from Step 1 by **10 per cent**. Thus, if you allocate **2 points** to a group member, for instance, thereby choosing 2, you decrease his income by **20 per cent.** The points allocated by you therefore determine how significantly one group member's taler income from Step 1 is reduced.

Whether, or by how much, a group member's income from Step 1 is reduced **overall** depends on the total number of points received. If, for instance, one member receives **a total of 3 points** from all other members, the income in Step 1 is reduced by **30 per cent**. If a member receives a total of **4 points**, the income in Step 1 is reduced by **40 per cent**. If a member receives exactly 10 points or more, the income in Step 1 is reduced by 100 per cent. The income in Step 1, in this case, would be reduced to Zero for this member. Your total income from the first two steps, in taler, is thus calculated as follows:

Total taler income at the end of Step 2:

= (Total taler income after Step 1) × (10 – points received)/10
– cost of points distributed by you
if points received < 10

= – cost of points distributed by you
if points received ≥ 10

Step 3

In the third and final step, you are told how many points each individual group member has given you. **If group members have given you points in Step 2, you can now reduce the income of these group members by allocating what is known as "*counter-points*".** Only those group members who received points in Step 2 are allowed to allocate counter-points. And these counter-points can only be distributed to group members who gave them points in Step 2.

A counter-point reduces the income that remained in the possession of the member in question at the end of Step 2 **by 25 %.** Should a member receive exactly 4 or more counter-points, the income from Step 2 is reduced by 100%. If you yourself receive 4 or more counter-points from group members to whom you gave points in the previous step, your own income from Step 2 is therefore also reduced by 100%.

The costs of counter-points are calculated just as in Step 2. Note, however, that if you give one group member counter-points in addition to having given him points, then the costs are calculated according to the sum of all the points this group member has received from you in Steps 2 and 3.

The costs of the counter-points can be seen in Table 2. **Example:** If you give Player 1 a total of 2 points in Step 2, your cost in Step 2 is 2 taler. If you give Player 1 a total of 3 further points in Step 3, a further 7 taler are added to your cost.

Table 2: Costs of the distribution of Counter-points to *one* other group member in Step 3

Points you have already given to the group member in Step 2	Counter-points given to the group member in Step 3 Gruppenmitglied in Stufe 3				
	0	**1**	**2**	**3**	**4**
0	0	1	2	4	6
1	0	1	3	5	8
2	0	2	4	7	10
3	0	2	5	8	12
4	0	3	6	10	14
5	0	3	7	11	16
6	0	4	8	13	18
7	0	4	9	14	
8	0	5	10		
9	0	5			
10	0				

On your screen, you can see how many points each individual group member has given to you in Step 2. Now you must decide, for **each** of these group members, how many counter-points you wish to give this member. It is compulsory to enter a figure at this stage. If you do not wish to alter a certain group member's income, please insert 0.

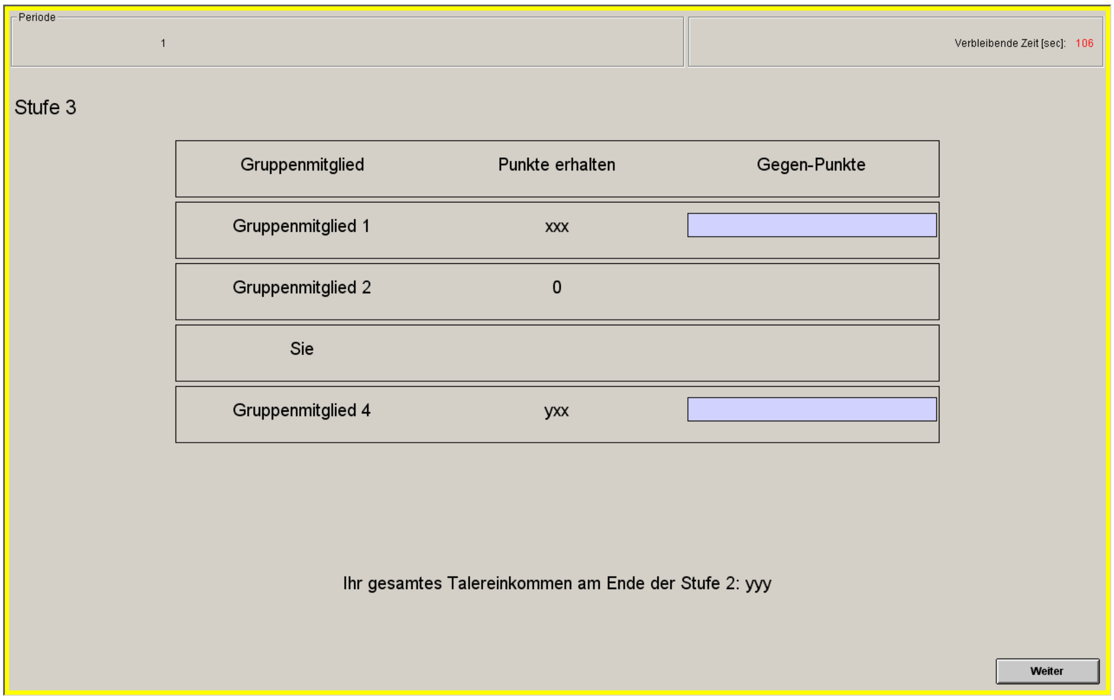
Your total income from all three steps, in taler, is thus calculated as follows:

Total taler income at the end of Step 3 = Period Income

= (Total taler income after Step 2) × (4 – counter-points received)/4
– cost of counter-points distributed by you

if the sum of the counter-points received is < 4
= – cost of counter-points distributed by you

if



the sum of the counter-points received is ≥ 4

You will also find this information on the final screen of each period.

The Payoff

Your total income, in taler, is calculated from the sum of your taler income in each period, in addition to the flat payment of 25 taler given to you at the beginning. As mentioned above, you receive 4 euro cent for each taler. You are also paid 4 Euro for showing up.

Do you have any further questions?

Test Questionnaire

Please answer all questions. Please write down the complete calculation at all times! If you have any questions, please let us know!

1. Each group member has 20 taler at his disposal. Nobody (including you) contributes to the project in Step 1.
   How high is
   Your income after Step 1? ....
   The income of all other group members after Step 1? ....

2. Each group member has 20 taler at his disposal. You contribute 20 taler to the project in Step 1. All other group members also contribute 20 to the project. How high is
   Your income after Step 1? ....
   The income of all other group members after Step 1? ....

3. Each group member has 20 taler at his disposal. The other 3 group members contribute a total of 30 taler to the project in Step 1.

   a) How high is your income after Step 1, if you contribute Zero taler to the project, in addition to the 30 taler?
   Your income after Step 1? ....

   b) How high is your income after Step 1, if you contribute 15 taler to the project, in addition to the 30 taler?
   Your income after Step 1? ....

4. Each group member has 20 taler at his disposal. Your contribution to the project is 8 taler.

   a) How high is your income after Step 1, if the other group members contribute a total of 7 taler to the project, in addition to your 8 taler?
   Your income after Step 1: ....

   b) How high is your income after Step 1, if the other group members contribute a total of 22 taler to the project, in addition to your 8 taler?
   Your income after Step 1: ....

5. In Step 2, you distribute the following points to your three other group members: 9, 5, 0. How high is the total cost of your distributed points?

6. How high is the total cost, if you give all other group members 0 points?

7. By how many per cent is your income reduced after Step 1, if you have received the sum of Zero points from all other group members?

8. By how many per cent is your income reduced after Step 1, if you have received the sum of 4 points from all other group members?

9. By how many per cent is your income reduced after Step 1, if you have received the sum of 15 points from all other group members in Step 2?

10. At the end of Step 2, you distribute the following points to the other three members of your group: 2, 2, 0. In Step 3, you distribute the following counter-points to the other three group members: 1, 1, 1. How high is the total cost for the counter-points distributed by you?

11. By how many per cent is your income reduced after Step 2, if you have received the sum of 3 counter-points from the other group members in Step 3?

Part 2

The second part of the experiment also consists of 10 periods.

In the course of these 10 periods, you interact with the same three other group members familiar to you from Part 1 of the experiment. The constellation of your group of four is therefore unchanged.

The separate periods differ from the first part of the experiment in only one aspect: Each period merely consists of the first two steps. In this part of the experiment, therefore, you only have the possibility of distributing points. No provision is made for counter-points.

All taler income from periods played in Part 2 are added to your total income from Part 1, calculated in euro and paid to you at the end of the experiment.