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Conspiracy against the Public – An Experiment on Collusion

Abstract

We study to what extent collusive behavior is affected by the awareness of negative externalities. Theories of outcome-based social preferences suggest that negative externalities make collusion harder to sustain than predicted by standard economic theory, while sociological theories of social ties and intergroup comparisons suggest that bilateral cooperation can be strengthened if there exist outsiders that gain from cooperative break down. We investigate this in a laboratory experiment. Subjects play the infinitely repeated prisoner's dilemma with and without a negative externality. The externality is implemented by letting subjects make a positive contribution to a public good if they choose to defect from cooperation, i.e. cooperation is collusive since the gains are at the expense of the public. We find that this negative externality increases collusive behavior. Subjects cooperate more if it hurts a third party.

JEL-Codes: C910.

Keywords: infinitely repeated prisoner's dilemma game, negative externality, cooperation, collusion, experiment.

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"People of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in a conspiracy against the public, or in some contrivance to raise prices."
(Adam Smith, (1776), pp. 111)

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1. Introduction

There are two main motives for cooperative behavior in repeated game prisoner's dilemma. First, it may be profitable: Cooperation builds reputation and can thus be sustained as an equilibrium even if parties are selfish and fully rational. Second, it may satisfy social preferences. People sometimes reciprocate cooperative behavior even if it has material costs.

However, the effect of social preferences on cooperative behavior is not so straightforward in many real world social dilemmas. The reason is that cooperation sometimes yields negative externalities. Collusion is an important example. Firms can cooperate on price increases or quantity restrictions in order to achieve higher profits, but this comes at the public's expense.

One question is how these negative externalities imposed on the public affect the behavior of colluding parties. Prominent theories of social preferences do not give a clear answer. Theories of outcome-based social preferences (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000) suggest that collusion may be harder to sustain since it reduces social surplus, while theories of intention-based reciprocity (Rabin, 1999, and Dufwenberg and Kirschsteiger, 2004), suggest that negative effects on passive third parties need not matter, since parties care about actions, not outcomes.

However, sociological theories of social ties, in-group and out-group trust and intergroup comparisons suggest that bilateral cooperation may be *strengthened* if there exist outsiders that gain if their cooperation breaks down (see e.g. Turner, 1975; Brewer and Kramer, 1986; Tajfel, 1982 and Putnam, 2007). Anecdotal evidence from court cases against illegal cartels supports this conjecture: the secret "conspiracy" against the consumers creates bonds between the colluding parties that makes the cartel agreement more robust.[1] A recent paper by Malmendier and Schmidt (2017) formalizes a similar idea: If parties cooperate, despite the fact that their cooperation yields negative externalities, then this may increase the weight that the cooperating parties attach to each other's welfare. They find support for their theory in a gift exchange experiment with negative externalities.

In this paper, we investigate – by use of a controlled lab experiment - the effect of negative externalities on cooperative behavior in a repeated prisoner's dilemma (PD). In the baseline treatment (taken from Dal Bo and Frechette 2011), pairs of subjects simultaneously choose between cooperation and defection in an infinitely repeated PD, i.e. a game that each period ends with a probability less than one. In the "public good treatment", subjects play the same repeated PD, with the difference that they contribute to a public good when they defect from the bilateral cooperation with their partner. That is, if they engage in bilateral cooperation they do not contribute to the public good, and hence the gains from cooperation are at the expense of the public. In the experiment, the public good is represented by a student organization that provides services with public good properties to the students at the university.

[1] See e.g Hammond (2005) on the case against the Lysine cartel, where the world's five largest producers of lysine, an animal feed additive, succeeded in doubling the world price of lysine for several years. This cartel overcharged consumers and customers by an estimated US$ 140 million. The cartel was prosecuted and charged after a member was caught on tape saying that "Our competitors are our friends. Our customers are the enemy."

In line with Malmendier and Schmidt (2017), our conjecture is that the negative externality imposed by cooperation, increases the level of cooperation. Indeed, this is what we find: The overall level of cooperation is higher in the public good treatment. Initially, the level of cooperation is lower, but as subjects gain experience and observe that their partners choose to cooperate despite the negative externality, they cooperate significantly more than in the baseline treatment. Furthermore, the collusive relations are stronger in the public good treatment: cooperation is more stable and lasts longer.

These results fit very well with the mechanism proposed by Malmendier and Schmidt: If subjects in the public good treatment think that their partner might care about the public good, they will initially expect less cooperation compared to subjects in the baseline treatment. Hence, when a subject in the public good treatment experiences cooperation, given the lower expectations, s/he will attach more weigh to the welfare of the colluding partner compared to subjects in the baseline treatment. In other words: the cooperative "bonds" between two colluding parties become stronger in the public good treatment, since the expectations to cooperate are initially lower.

Our paper contributes to a small but growing literature investigating prosocial behavior in situations where the behavior comes at the expense of third parties. As noted by Malmendier and Schmidt, such negative externalities have largely been ignored in the theoretical and experimental literature. They investigate the negative externalities of gift giving, e.g. business gifts, where the giver hopes to get favorable treatment from the receiver. They find that the gift triggers an obligation to repay the gift, even if the gift is given with the intention to affect the decision of the recipient at the expense of a third party. In fact, the gift has a stronger effect when it imposes a negative externality on a third party. In a related experiment, Pan and Xiao (2016) study to what extent gift giving triggers the obligation of the receiver to favor the gift giver. They show that the pure gift effect is present even when it leads to a less efficient outcome, i.e. when it comes at the expense of a third party. Currie, Lin, and Meng (2013) study how gift-giving affects third parties in Chinese hospitals. A pair of trained actors visits physicians and plays the role of patients. If the first patient gives a small gift to the physician, s/he receives better service and is less likely to be prescribed unnecessary and costly medication. If the first "patient" introduces the second "patient" as a friend, this patient also receives better service.

An interesting recent paper by Bland and Nikiforakis (2015) studies how third party externalities affect equilibrium selection in one-shot coordination games. They find that subjects are willing to incur a cost to try to avoid imposing large negative externalities on third parties. However, they ignore the negative externalities if the incentives are sufficiently at odds with third party interests. Like us, they demonstrate that third party externalities affect equilibrium selection, but in contrast to us, they do not find that third party externalities lead to stronger coordination on self-interested outcomes. The plausible explanation is that we study repeated interaction, in which cooperative bonds can develop over time. In fact, consistent with Bland and Nikiforakis, we also find that subjects put positive weight on the welfare of third parties in the first round of the repeated game.

Another strand of literature that (implicitly) studies the effect of negative externalities on prosocial behavior is the experimental papers on bribery games, which are similar to repeated gift exchange games. In Abbink, Irlenbusch, and Renner (2002) and Abbink (2004), the gains from corruption are at the expense of the public. The reciprocal relationship between the briber and the bribee is undesirable with regard to social welfare and is subject to punishment when discovered. They find that bribery relationships do develop over time despite negative externalities, but they do not find that the public aspect leads to *more* corruption. In a similar bribery game, Barr and Serra (2009) finds that bribers are *less likely* to offer bribes when the negative externalities imposed on the public are high, and the experiment is framed as corruption.

On a more general level, our paper is related to the experimental literature on collusion. For recent surveys, see Potters and Suetens (2013) and Armstrong and Huck (2014). The public aspect we introduce is usually present only implicitly in this literature. Subjects in the room participate in a market, and prices increase when they collude. However, we are not aware of any papers in this literature where third parties experience an actual welfare loss from the increased collusive cooperation.

Our paper also illuminates the experimental literature on cooperation in infinitely repeated games, see Dal Bó and Fréchette (2018) for a recent survey. A general finding is that higher defection payoff reduces the level of cooperative behavior. Our experimental results indicate that this does not necessarily hold in situations where the defection payoff is somewhat dubious. An uncertain change in defection pay-off – which is due to uncertainty about opponent’s social preferences - might reduce expectation for cooperative behavior, and thereby lead to more robust cooperative relationships for those who choose to cooperate.

The rest of the paper is organized as follows: Section 2 presents the experimental design, while Section 3 presents the behavioral predictions. The results are presented in Section 4, while Section 5 concludes.

2. Experimental design and procedure

The experiment uses a between-subject design and consists of two treatments. The baseline is a standard infinitely repeated prisoner’s dilemma game where the gains from cooperation are not at the expense of the public. The second treatment is based on the same infinitely repeated prisoner’s dilemma as the baseline, but now the gains from cooperation are at the expense of the public, hence it can be referred to as collusion. The public is represented by a student organization, StOr.

The baseline is a replication of a treatment in Dal Bó and Fréchette (2011), who study cooperation in infinitely repeated games. The subjects are divided into pairs, denoted “matches”. Each match consists of an ex-ante unknown number of rounds. Infinity is simulated using what is known as a random continuation rule. The random continuation rule assigns a fixed probability of continuation, and in this experiment it is equal to 3/4. In each match, all the subjects in a room are divided into random pairs. They play the first round and then a lottery decides if there will be another round. There is a 75% chance that this round will be followed by another round. This means that in expectation each match lasts for 4 rounds. When the match ends, the subjects are all randomly re-matched, and the same procedure is repeated until the experiment is over, which is after an hour. The shortest session consists of 31 matches, the longest session consists of 42 matches. Before the subjects leave the room, they also make one more decision (more below) and fill in a questionnaire.

Table 1: The baseline

		Player 2	
		Collude	Defect
Player 1	Collude	40, 40	12, 50
	Defect	50, 12	25, 25

All decisions are paid, as is standard in this literature (see e.g. Sherstyuk, Tarui, and Saijo, 2013). The payoff when both subjects collude is equal to 40 experimental units (ECU) for both subjects. Temptation payoff is 50, sucker's payoff is 12, and if they both defect, they both get 25 each. The exchange rate is 10 ECU for 1 NOK (or ca. 75 ECU per 1 EUR). The public good treatment has the exact same payoffs as the baseline. The only difference is that now the subject contributes 25 to a public good if he or she chooses defect. The treatments are identical in all other regards. When both subjects choose defect, they both contribute 25 to the public good, 50 in total. When one of the subjects chooses defect, and one subject chooses collusion, the defecting subject contributes 25 to the public good. Finally, when both subjects choose to collude, the contribution to the public good is zero. The payoffs are shown in Table 2:

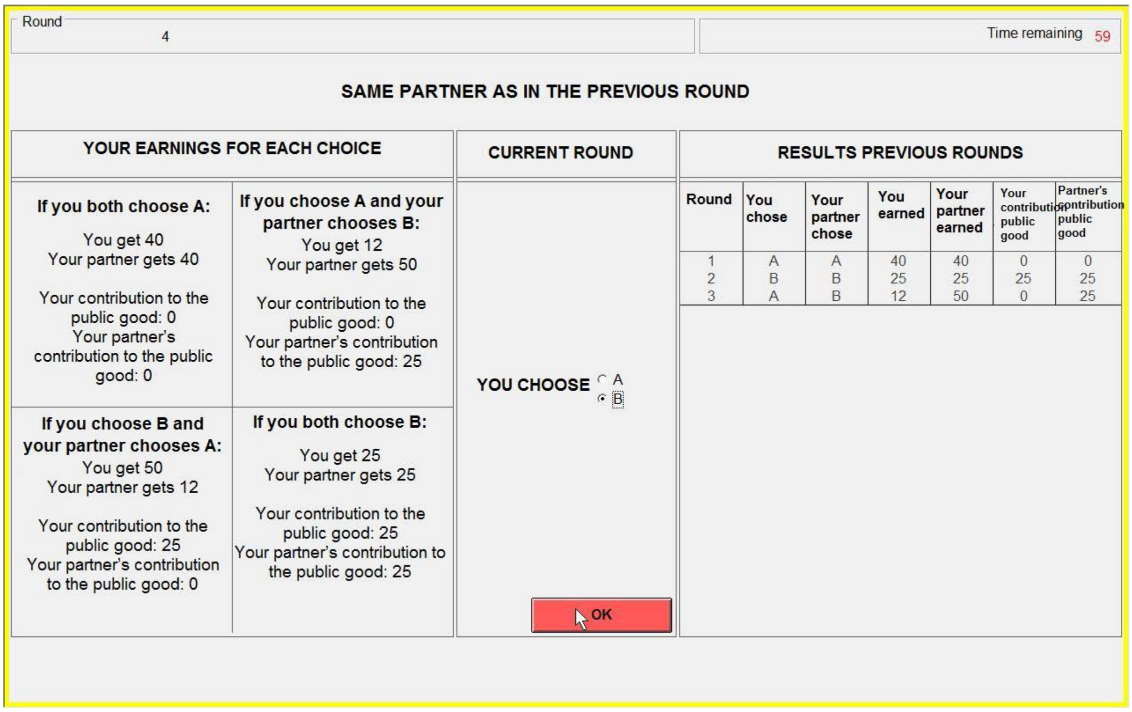
Table 2: The public good treatment

	Collude-Collude	Collude-Defect	Defect-Collude	Defect- Defect
Player 1	40	12	50	25
Player 2	40	50	12	25
Public good	0	25	25	50

The public good in this experiment is provided by the student organization StOr at the home university of the subjects, the University of Stavanger. StOr is a non-party affiliated interest organization, where all students at the UiS are members. The organization is responsible for life on campus, student welfare, student elections, student organizations, international students, exchange programs, legal issues regarding exams, syllabus and so on. In sum, it provides services that have public good properties. The services provided by this organization allow multiple agents to consume most of it at the same time (non-rival), and it is not possible to exclude subjects who did not contribute to the good from consuming it (non-excludable). The contribution to the public good is a fixed amount, and since the organization already exists, there is no provision point that needs to be reached. When the subjects contribute to the public good, it will translate into a very small increase in the provision of the public good. This is meant to capture the fact that when firms refrain from colluding, this increases efficiency in the economy, from which both consumer and firms benefit (although to a smaller extent than the consumers).

The instructions and the design were presented in a neutral language (*A* is collusion, *B* is defection/ not colluding), and the subjects were provided with an overview of all the information within each match, but not between matches. Figure 1 is a translated screen-shot from the public good treatment, see the Appendix for instructions for both treatments.

Figure 1: Screen-shot public good treatment (translated from Norwegian).



A total of 120 students at the University of Stavanger (Norway) participated in the experiment, with 20 students in each session. The subject sample in each treatment is similar to that of recent papers investigating cooperation using infinitely repeated prisoner's dilemma games (see e.g. Duffy and Ochs, 2009; Fréchette and Yuksel, 2013; Fudenberg, Rand, and Dreber, 2012; Kagel and Schley, 2013; Sherstyuk, Tarui, and Saijo, 2013).The subjects earned an average of EUR 55. The subjects were rematched on average 34 times. As the shortest session lasted 31 matches, the analysis is based on the first 31 matches for every session. The average number of rounds per match was 4.1, and the maximum number of rounds was 24. All instructions were given both written and verbally. The experiment was conducted and programmed with the software z-Tree (Fischbacher, 2007).

After the prisoner's dilemma game and before the questionnaire, the subjects were given a final task. Subjects in both treatments were asked if they preferred either *ECU 40 for themselves*, or *ECU 25 for themselves and ECU 25 to StOr* (incentivized). This decision involves the same payoffs as the prisoner's dilemma, but the difference is that it does not involve any interaction with other subjects.

3. Behavioral Predictions

In order to explain favoritism in gift exchange, Malmendier and Schmidt (2017) proposes a model of social preferences with endogenous references groups. We will here briefly present their novel behavioral assumption, leading to our behavioral prediction.

Consider a two player game of perfect information, where player *i* chooses strategy s_i, and *s* denote a pure strategy profile for both players. The utility of player *i* is given by:

$$U^i = m^i(s) + \alpha_i^j(s|\sigma) \cdot m^j(s),$$

where $m^i(s)$ is player *i*'s material payoff, and $\alpha_i^j(s|\sigma)$ is the weight player *i* puts on the payoff of player *j*. This weight depends on the expected strategy profile, σ. The profile σ can consist of mixed strategies, and is expected to be played because of e.g. past experience, social norms, or simply because σ is an equilibrium that the players expect to be played. Formally, Malmendier and Schmidt makes the following assumption:

If player j chooses a strategy s_j *that increases (decreases) player i's payoff that i would have received if j had chosen the expected strategy* σ_i*, then the weight of player j's payoff in player i's utility increases (decreases) compared to the weight if j had chosen* σ_j*. That is:* $m^i(s_j,\sigma_i) \geq m^i(\sigma_j,\sigma_i) \rightarrow \alpha_i^j(s_j,\sigma_i|\sigma) \geq \alpha_i^j(\sigma|\sigma)$ *and equivalently* $m^i(s_j,\sigma_i) \leq m^i(\sigma_j,\sigma_i) \rightarrow \alpha_i^j(s_j,\sigma_i|\sigma) \leq \alpha_i^j(\sigma|\sigma)$.

Consider now the payoff matrix for player *i* (the row player) in the prisoner's dilemma game (where C denotes cooperation and D defection):

	C	**D**
C	R	S
D	T	P

The PD payoff matrix consists of the reward from joint cooperation (R), the temptation payoff (T), which is the payoff to defecting when the other cooperates, the punishment from mutual defection (P), and the sucker's payoff from cooperation when the other defects (S). For these payoffs to define a PD game, it must be that T > R > P > S. Given the utility function above, the players do not actually play the same stage game every period. The reason is that if a player identifies a different strategy than expected, then the payoffs in the game change.

Consider now our experiment, and assume that the players care about the public good. In the first period, T and P are then higher in the public good treatment than in the baseline. We will thus expect to see more cooperation in baseline. Assume now that a player *i* observes player *j* playing C in the public good treatment, despite expected play of D. She may then attach more weight α_i^j to player *j*'s welfare, and hence the R/T ratio increases, which can lead to more cooperative play in the public good treatment than in the baseline.

There are of course numerous equilibria in super games like this, and it is straightforward to construct equilibrium strategies where the players after *n* periods cooperate more in the public good game. This is also our behavioral prediction: We will initially see less, but then more cooperation (denoted collusion) when cooperation is at the expense of the public good.

4. Experimental results

Table 3 displays collusion rates for the baseline and the public good treatment. The top left panel describes the collusion rates in the first round of the first match, and the bottom left panel describes collusion rates for all first rounds for all matches. The top right panel describes collusion for all rounds in the first match, and the bottom right panel describes collusion rates for all observations. Statistical significance in Table 3 is assessed by estimating probit regressions (robust standard errors clustered at the individual level) with an indicator variable for the public good treatment (see Table 8).

Table 3: Collusion rates

First match First round			First match All rounds		
Baseline		Public good	Baseline		Public good
0.55	> **	0.35	0.44	>***	0.23
∨		∧ ***	∨		∧ ***
All matches First round			**All matches All rounds**		
Baseline		Public good	Baseline		Public good
0.49	<	0.57	0.37	<*	0.48

Notes: Probit regressions with robust clustered standard errors (see Table 8) in the Appendix. * p < 0.10, ** p < 0.05, *** p < 0.01.

Different matches have different numbers of rounds, and the level of collusion may vary across rounds. Hence, we start by investigating the first rounds. In the baseline, the initial collusion rates are significantly higher compared to the public good treatment: In the first round, 55 percent of the individuals in the baseline choose to collude, compared to 35 percent in the public good treatment. In the first match, 44 percent in the baseline choose to collude, compared to 23 percent in the public good treatment.

However, this pattern changes to the opposite as the subjects gain experience. Looking at the whole first round and pooling all observations, it is clear that the collusion rates are significantly higher in the public good treatment: In the first round of all matches, 49 percent in the baseline choose to collude, versus 57 percent in the public good treatment. When we look at all the rounds in all the matches, 37 percent in the baseline choose to collude, versus 48 percent in the public good treatment (significant at the ten percent level).

Collusion rates in the baseline do not change significantly as the subjects gain more experience (from 0.55 to 0.49, and from 0.44 to 0.37). Meanwhile, collusion rates are increasing with experience in the public good treatment, from 0.35 to 0.57, and from 0.23 to 0.48 (both significant at the 1 percent level). These differences between the first match and all matches suggest, as in Dal Bó and Fréchette (2011), that experience affects how subjects play in repeated matches. In the following section, we look into how collusion evolves as the subjects gain more experience.

Figure 2 graphically illustrates how collusion rates evolve match by match. The graph displays the rate of subjects who choose to collude in the first round of each match in each treatment. The rate of collusion in the first few matches falls from about 0.55 down to 0.30 in the baseline, and then it slowly increases to levels reaching 0.6. In the public good treatment, the level of collusion starts out around 0.35, and remains low until match 8, from which collusion increases steadily and reaches levels above 0.6 for the remaining ten matches. Collusion is higher in the public good treatment compared to the baseline in the matches following the first ten matches. The public good slope displays a clear positive trend well above the baseline. We investigate this closer below.

Finding 1: *When subjects gain experience, collusion is higher in the public good treatment.*

Figure 2: Evolution of collusion (first rounds)

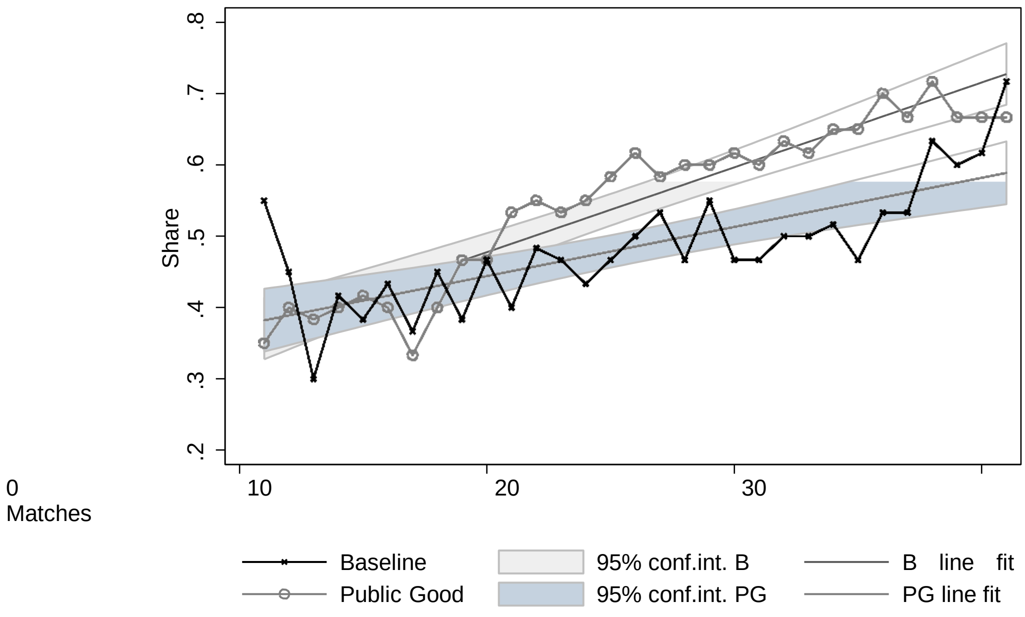


Table 6 in the Appendix provides a summary of the subjects randomly placed into each treatment and tests for balance in predetermined variables (gender, age, field of study) across treatments. Attitudes towards the organization representing the public good and previous membership are also included, as well as political preferences. Considering an F-test for the joint significance, the data are balanced across these characteristics, with the exception of gender. The share of females is slightly higher in the public good treatment (significant at the ten percent level). Pre-determined control variables are therefore included in the regression analysis in Section 5.2 (gender, age, field of study, and previous/ current membership in the organization). On a scale from 1 to 10, the subjects on average rate the importance of the student organization's work at 6.8 (std.err. 0.19), which supports that they value the services of the organization.

Table 4 presents the results from linear probability models with collusion as the dependent variable.[2] Only indicator variables are used, rather than multivalued variables, in order to preserve many of the finite sample properties that simple comparisons of means have (see Athey and Imbens, 2016). The dependent variable is equal to 1 if a subject colludes, 0 otherwise. All regressions include dummy variables for rounds, matches, age (deciles), gender, field of study, and membership in the organization, unless otherwise stated.

[2] We employ linear probability models from here and onwards in order to adjust for covariates.

Table 4: Rate of collusion

	(1) Collude	(2) Collude	(3) Collude	(4) Collude	(5) Collude	(6) Collude	(7) Collude	(8) Collude
PG	0.14***	0.10	0.02	0.00	0.16***	0.14*	0.21***	0.16**
	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.08)	(0.06)	(0.08)
Observations	15,160	3,720	4,540	1,200	5,700	1,200	4,920	1,320
R-squared	0.09	0.08	0.04	0.03	0.08	0.08	0.13	0.10
Round	all	1	all	1	all	1	all	1
Match	all	all	1-10	1-10	11-20	11-20	21-31	21-31
Match FE	yes	yes	yes	yes	yes	yes	yes	yes
Round FE	yes	no	yes	no	yes	no	yes	no
Individual controls	yes	yes	yes	yes	yes	yes	yes	yes
Cluster	ind	ind	ind	ind	ind	ind	ind	ind
Mean ind.var.	0.52	0.50	0.59	0.50	0.53	0.50	0.44	0.50
SD ind.var.	0.50	0.50	0.49	0.50	0.50	0.50	0.50	0.50
Mean dep.var	0.42	0.52	0.31	0.41	0.42	0.53	0.50	0.61
SD dep.var.	0.49	0.50	0.46	0.49	0.49	0.50	0.50	0.49

Note: Robust clustered standard errors on the individual level in parentheses, * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The dependent variable is equal to 1 if a subject colluded/ cooperated in a round, 0 otherwise. Individual controls include field of study, membership in PG organization, gender, and age. Sample consists of matches 1-31.

The overall collusion rate is significantly higher in the public good treatment, and the size of the effect is equal to about a third of the average collusion rate in the experiment (0.14/0.42). In (2) only first rounds are included in the sample and we see that the collusion rates are not significantly different when we only compare the start of each match. In (3) - (8) the sample is divided into three equal parts: the ten first matches, the ten matches in the middle, and the last 11 matches. In (3) we see that we find no significant differences in collusion rates in the baseline and the public good treatment when we look at the early matches (all rounds). The same is true when we only look at first rounds (4). In matches 11-20 in columns (4) and (5), we see that collusion rates are significantly larger in the public good treatment. This result holds when we only include first rounds, but the result is now only marginally statistically significant at the ten percent level. In matches 21-31 in columns (7) and (8), collusion rates are significantly higher in the public good treatment, and the result is also statistically significant at the five percent level when only including first rounds.

Hence, the results in Table 4 imply that when we take individual characteristics, trends, repeated interactions, and the length of matches into consideration, the results confirm the second finding that collusion is higher in the public good treatment when subjects gain experience.

Are the collusive relations/ social relations also stronger in the public good treatment? In the following we present a measure meant to capture the strength of the collusive relation: For each match, we calculate the share of rounds where two subjects collude/ cooperate continuously. If a pair colludes in every round of the match, they are assigned the maximal value of 1. If a pair does not collude in any round of the match, they are assigned the minimum value 0. A pair which colludes continuously in 3 out of 4 rounds is assigned the value 3/4=0.75.[3] Figure 3 displays the results, and we see that the collusive relations are not stronger initially, but they do grow stronger as subjects gain experience. The collusive relations in the public good treatment are on average at least 10 percent longer for the last 20 matches.

[3] If the game only lasts for one round, and they colluded, this is defined as a collusive relation and given the value 1. If a match lasts for 7 rounds, and a pair colludes in rounds 1-3 and in rounds 5-6, the strength of the collusive relation is equal to ((3+2)/7)=0.86 in this match.

Figure 3: Continuous collusion.

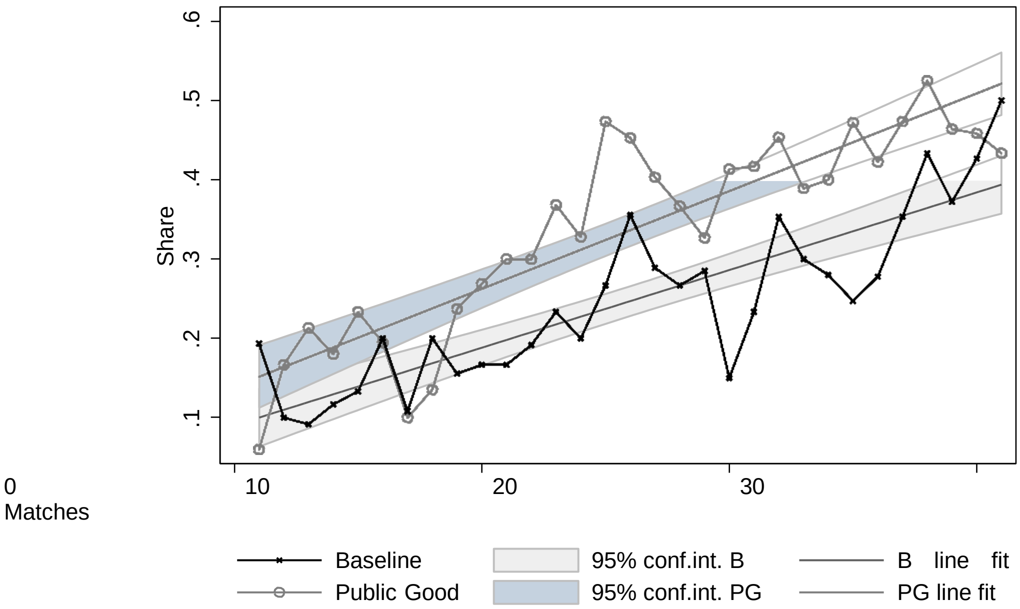


Table 5 presents the results from linear probability models with the share of continuous collusion within each match as the dependent variable. We use the same specifications as in Table 4. Regression (1) confirms the overall picture from Figure 3: Collusive relations are significantly stronger in the public good treatment. After taking individual characteristics, trends, repeated interactions, and the length of matches into consideration, we see that the size of the effect is equal to a third of the mean (0.11/0.29). In (2) we only look at the ten first matches, and we see that the coefficient is small and insignificant, but positive. From match 10 to 20 the effect is large and significant at the one percent level, with the same results for the last ten matches. Collusive relations are becoming stronger in both treatments, but when collusion/ cooperation is at the expense of a third party, the effect is significantly stronger.

Finding 2: *Collusive relations are stronger in the public good treatment - collusion is more stable and lasts longer in the public good treatment.*

Table 5: Continued collusion/ cooperation

	(1) ContC	(2) ContC	(3) ContC	(4) ContC
PG treatment	0.11*** (0.04)	0.04 (0.03)	0.16*** (0.05)	0.13** (0.05)
Observations	3,720	1,200	1,200	1,320
R-squared	0.09	0.03	0.07	0.08
Match	all	1-10	11-20	21-31
Match FE	yes	yes	yes	yes
Individual controls	yes	yes	yes	yes
Mean ind.var.	0.50	0.50	0.50	0.50
SD ind.var.	0.50	0.50	0.50	0.50
Mean dep.var	0.29	0.16	0.31	0.39
SD dep.var.	0.44	0.35	0.45	0.48

Note: Robust clustered standard errors on the individual level in parentheses, * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The dependent variable is the share of rounds where the subjects cooperated continuously within a match. Individual controls include field of study, membership in PG organization, gender, and age. Sample consists of matches 1-31.

5. Conclusion

This paper studies to what extent collusive behavior is affected by the awareness of negative externalities. Theories of outcome-based social preferences suggest that negative externalities make collusion harder to sustain than predicted by standard economic theory, since collusion reduces social surplus. On the other hand, sociological theories of social ties and intergroup comparison suggest that bilateral cooperation can be strengthened if there exist outsiders that gain from cooperative break down.

Our experimental results give support for the latter: Cooperation is strengthened when it comes at a third party's expense. Initially, the level of cooperation is lower, but as subjects gain experience, there is more cooperation when cooperation is at the expense of the public, and the collusive relationships become more stable. Malmendier and Schmidt's (2017) model of social preferences with endogenous reference groups provides a potential explanation for our experimental results: Negative externalities may lower expectations for collusion. If they still collude, despite negative externalities, then this may increase the weight that the colluding parties attach to each other's welfare and create stronger ties.

Our finding is consistent with the experimental findings of Malmendier and Schmidt, and a few other gift exchange papers with third party externalities. In contrast to this literature, we study a *repeated simulations move game* – i.e. the prisoner's dilemma - with third party negative externalities. This resembles well the strategic situation facing two colluding firms that tacitly decide to keep prices above marginal costs, and thereby capture surplus from consumers.

We believe we will see a growing literature examining the effect of negative externalities on prosocial behavior and equilibrium selection, both in social dilemmas and coordination games with strategic uncertainty. The importance of third-party externalities and the inconclusive predictions from the prevailing theories of social preferences suggest that investigating these questions is important. As Bland and Nikiforakis (2015) notes, the fact that third-party externalities were found to have an effect in their coordination games "suggests that it is interesting to explore in future research how they affect tacit coordination in different classes of coordination games, e.g., when decision-makers' incentives are not aligned". Our paper contributes to this research agenda.

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5 Appendix

Randomization

Table 6: Orthogonality

	(1) Base	(2) PG	(3) (1) vs. (2)	(4) p-value from joint orthogonality test of treatment arms
Age	23.97	24.07	-0.10	0.89
	(0.44)	(0.56)	(0.71)	
HF	0.05	0.10	-0.05	0.30
	(0.03)	(0.04)	(0.05)	
SV	0.28	0.28	-0.00	1.00
	(0.06)	(0.06)	(0.08)	
TN	0.63	0.62	0.02	0.85
	(0.06)	(0.06)	(0.09)	
Shares females	0.50	0.67	-0.17	0.06
	(0.07)	(0.06)	(0.09)	
Attitude PG	6.62	6.93	-0.32	0.40
	(0.27)	(0.26)	(0.38)	
Member PG	0.10	0.07	0.03	0.51
	(0.04)	(0.03)	(0.05)	
Left	0.23	0.27	-0.03	0.68
	(0.06)	(0.06)	(0.08)	
Center	0.10	0.12	-0.02	0.77
	(0.04)	(0.04)	(0.06)	
Right	0.45	0.38	0.07	0.46
	(0.06)	(0.06)	(0.09)	
N	60	60	120	

Standard errors in parentheses. Pair-wise t-tests in square brackets (control versus public good treatment). Field of study: arts and education (*HF*), the faculty of social sciences (*SV*), the faculty of science and technology (*TN*). Political preferences: Right, Center, Left. Attitude PG: *How important is the work done by StOr, in your opinion.* Member PG: “Have you previously been/ are you currently a member of StOr?”.

Session statistics

The average number of rounds per match is 4.1, and the maximum number of rounds is 24 (Dal Bó and Fréchette (2011): average 4.4 and maximum 24):

Table 7: Session characteristics

	Session	Subjects	Games	Average no. of rounds	Average Payoff
DBF	1	12	34	3.9	31.4
	2	14	47	3.2	29.2
	3	12	23	5.4	27.6
Baseline	2	20	32	4.6	82.8
	3	20	32	3.6	56.3
	4	20	31	4.1	64.2
Public good	1	20	43	4.3	101.6
	5	20	33	3.5	57.7
	6	20	32	4.6	86.0

Note: The payoffs for Dal Bó and Fréchette (2011) are in 2011 US dollars, while the payoffs in this experiment are in 2013 US dollars, exchange rate May 24th: 5.92 NOK per US dollar. Subjects are on average 24 years old, 58.3 percent of the sample are female, 63 percent are from the faculty of science and technology, 28 percent are from the faculty of social sciences, and 8 percent are from the faculty of arts and education.

Table 8: The share of collusion: Probit model with marginal effects

VARIABLES	(1) C	(2) C	(3) C	(4) C	(5) C	(6) C	(7) C	(8) C
PG treatment	-0.20**	-0.21***	0.06	0.10*				
	(0.09)	(0.07)	(0.07)	(0.05)				
First round&match vs. first match					0.07	-0.21***		
					(0.06)	(0.06)		
First match vs. all							0.07	-0.24***
							(0.06)	(0.04)
Observations	120	540	3,720	15,160	1,860	1,860	7,300	7,860
Match	first	first	all	all	all	all	all	all
Round	first	all	first	all	first	first	first	first
Subset					baseline	PG	baseline	PG

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a binary variable equal to 1 if a subject colludes/ cooperates, 0 otherwise. This Table presents the marginal effects results used in Table 3 from probit regressions with robust clustered standard errors on the individual level. The sample is limited to matches 1-31 as the shortest session ends after match 31.

Instructions Public Good Treatment

Welcome!

- In this experiment you will be asked to make some decisions.
- You will get the opportunity to earn money which will be paid to you in cash and anonymously when the experiment is over.
- In this experiment we use what we denote experimental currency units, ECU. By the end of the experiment your total earnings will be converted into Norwegian Kroners according to the following rate: 10 ECU = 1 NOK.
- Your earnings depend partly on your decisions, partly on others' decisions, and partly on chance.
- We will now go through the instructions in detail. You will be given sufficient time to read the instructions. The experiment will last for about one hour.
- If you have any questions regarding the instructions, please raise your hand and we will come to you.
- The experiment will be conducted using computers, and talking or communicating with others during the experiment is not allowed.
- Please turn your cellular phone off and put it away.

Instructions

1. All participants will be randomly matched into pairs several times during this experiment, and each time you and your partner will be asked to make some decisions. Each matched pair plays a sequence of rounds.
2. The number of rounds in each match will vary from match to match. The number of rounds in each match is determined by a lottery. After one round has been played, there is a 75 percent chance that there will be another round. Another way of saying this is that there will be another round in 3 out of 4 times. This means that when you have finished playing the first round, there is a 75 percent chance that there will be a second round. In other words - when round 2 is finished, the probability of a third round is still equal to 75 percent.
3. When the outcome of the lottery is that there will not be another round, all participants are randomly re-matched again. Your earnings from the previous match will be set aside on your personal account. The number of rounds which you and your new partner will meet will be decided by the same lottery as described in 2.
4. In the table below, you can see what you earn, what your partner earns, and what is contributed to the public good for all the four possible choice sets:

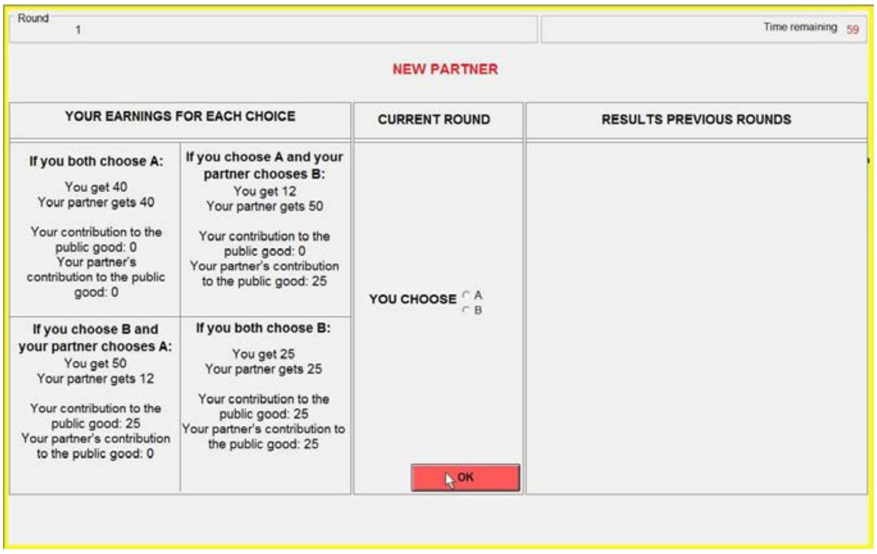
If you both choose A: You get 40 Your partner gets 40 Your contribution to the public good: 0 Your partner's contribution to the public good: 0	**If you choose A and your partner chooses B:** You get 12 Your partner gets 50 Your contribution to the public good: 0 Your partner's contribution to the public good: 25
If you choose B and your partner chooses A: You get 50 Your partner gets 12 Your contribution to the public good: 25 Your partner's contribution to the public good: 0	**If you both choose B:** You get 25 Your partner gets 25 Your contribution to the public good: 25 Your partner's contribution to the public good: 25

In each round you and your partner choose between choice **A** and choice **B**. You and your partner choose simultaneously, and you will get to know your partner's decision after you have made a decision. Your earnings depend on your decision, but they also depend on what your partner's decision is.

As you saw in the table above, you have the opportunity to contribute to a public good in this experiment. The public good is the student organization ***StOr*** here at the University of Stavanger, which works to promote the students' interests (see introduction below). When you choose **B**, you contribute to the public good, and by "public good" we always mean ***StOr***. When you choose **A**, you do not contribute to the public good. The same goes for your partner - when your partner chooses **B**, he/ she contributes to the public good. When your partner chooses **A**, he/ she does not contribute to the public good.

Summary

The number of rounds in each match is decided by a lottery. After each round there is a 75 percent chance that there will be another round. When there is no new round, all participants are re-matched. Below you can see what the screen will look like when you are making a decision. The left part of the screen shows what you earn for each choice set. In the middle you see where you make your decision. In the right part of the screen you will see the results from the previous rounds with the current partner. Take your time - feel free to take 30 seconds to make your decision and remember to press the OK-button when you have made your decision.



The lotteries are all drawn by the computer, and they are always randomly drawn.

Please follow the messages which appear on the screen. In the end you will be asked to fill out a short questionnaire, and you will be informed about your total earnings converted into NOK. On the pc cabinet you can see a white sticker with the logo of the university, and a number, for instance D10136. Please write down this number and your total income on the receipt when the experiment is over. When we tell you that the experiment is over, you can leave the room with the receipt in hand. Please bring this to the EAL building, office H-161, to collect your earnings.

Instructions Baseline

Welcome!

- In this experiment you will be asked to make some decisions.
- You will get the opportunity to earn money which will be paid to you in cash and anonymously when the experiment is over.
- In this experiment we use what we denote experimental currency units, ECU. By the end of the experiment your total earnings will be converted into Norwegian Kroners according to the following rate: 10 ECU = 1 NOK.

- Your earnings depend partly on your decisions, partly on others' decisions, and partly on chance.
- We will now go through the instructions in detail. You will be given sufficient time to read the instructions. The experiment will last for about one hour.
- If you have any questions regarding the instructions, please raise your hand and we will come to you.
- The experiment will be conducted using computers, and talking or communicating with others during the experiment is not allowed.
- Please turn your cellular phone off and put it away.

Instructions

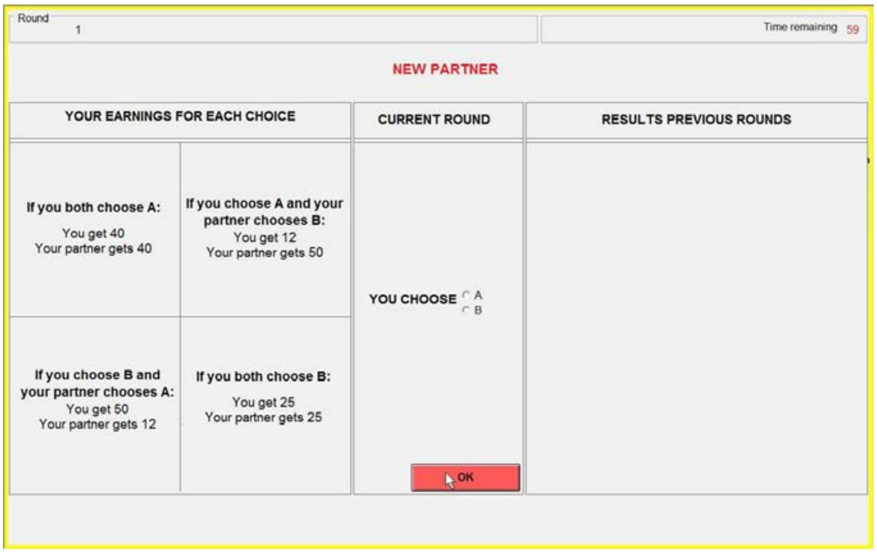
1. All participants will be randomly matched into pairs several times during this experiment, and each time you and your partner will be asked to make some decisions. Each matched pair plays a sequence of rounds.
2. The number of rounds in each match will vary from match to match. The number of rounds in each match is determined by a lottery. After one round has been played, there is a 75 percent chance that there will be another round. Another way of saying this is that there will be another round in 3 out of 4 times. This means that when you have finished playing the first round, there is a 75 percent chance that there will be a second round. In other words, when round 2 is finished, the probability of a third round is still equal to 75 percent.
3. When the outcome of the lottery is that there will not be another round, all participants are randomly re-matched again. Your earnings from the previous match will be set aside on your personal account. The number of rounds that you and your new partner will meet will be decided by the same lottery as described in 2.
4. In the table below, you can see what you earn and what your partner earns for all four possible choice sets:

If you both choose A: You get 40 Your partner gets 40	**If you choose A and your partner chooses B:** You get 12 Your partner gets 50
If you choose B and your partner chooses A: You get 50 Your partner gets 12	**If you both choose B:** You get 25 Your partner gets 25

In each round you and your partner choose between choice **A** and choice **B**. You and your partner choose simultaneously, and you will get to know your partner's decision after you have made a decision. Your earnings depend on your decision, but they also depend on what your partner's decision is.

Summary

The number of rounds in each match is decided by a lottery. After each round there is a 75 percent chance that there will be another round. When there is no new round, all participants are re-matched. Below you can see what the screen will look like when you are making a decision. The left part of the screen shows what you earn for each choice set. In the middle you see where you make your decision. In the right part of the screen you will see the results from the previous rounds with the current partner. Take your time - feel free to take 30 seconds to make your decision and remember to press the OK-button when you have made your decision.



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