



The long-run effects of communication as a conflict resolution mechanism

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ABSTRACT

The paper studies an experimental conflict in a repeated game and tests the robustness of communication as an *intermediate* conflict resolution instrument. The results show a strong and persistent impact of communication. Most conflict parties refrain from conflict expenditures even after the opportunity for communication has expired. Third party involvement with punishment options does not enhance this effect while contesting one prize rather than multiple prizes reduces it. Conflict resolution is less successful even in the long term if initial conflict intensity is rather high.

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1. Introduction

Most conflict resolution processes involve communication between the opposing sides. Talking to each other is a simple, cheap and omnipresent instrument for reconciliation. However, communication does not end hostility automatically and many settlements collapse after a while. At the same time, we know too little about the factors that reduce the effectiveness of communication, in particular in the long-run. One reason for that deficit is that most peace talks occur unrecorded and in deliberate privacy. Moreover, conflict parties give contradicting reasons in case of failure. Since ‘real’ conflict data also suffer from selection and endogeneity problems (Abbink, 2010), this paper uses an experiment to study critical factors that jeopardize conflict resolution via communication.

The paper tests precisely how three typical problems influence the impact of communication as a settlement device. First, conflict *resolution* processes face temporary constraints. They occur only after initial conflict expenditures. This property distinguishes resolution from conflict *prevention* (Ramsbotham et al., 2011). The distinction matters because the concept of reciprocity suggests that an initial conflict history induces some contestants to retaliate against the opponent instead of seeking an accommodating agreement. Conflict intensity can also have an adverse effect on communication quality, e.g. when the conflict parties refuse even to shake hands.¹ As an additional temporary constraint, contestants talk with each other but eventually go home. For example, at the end of World War I, the Stab-in-the-back myth suggested that the German Army had been betrayed by the civilian politician who had signed the Armistice in 1918. This nationalist myth effectively undermined post-war rapprochement and destabilized the Weimar Republic (Barth, 2003).

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¹ This happened at the first high-level talks between Serbian and Kosovo officials after the end of their armed conflict.

The temporary constraints also relate to the second typical feature of conflict resolution that I study in this paper. The economics textbook approach to conflict resolution relies on credible commitments and/or the enforcement of contracts by third parties (Schelling, 1960; Williamson, 1983). However many accommodating agreements do not satisfy these preconditions.² This paper studies whether third party interventions with targeted sanctions can replace such arbiters and support 'pure' communication.

As a third feature of conflict resolution processes, the paper takes into account that it is particularly difficult to make accommodating agreements between conflict parties when prizes are indivisible.³ Theories of (static) social preferences (e.g. Falk and Fischbacher, 2006; Fehr and Schmidt, 1999; Köszegi and Rabin, 2006; Mago et al., 2016) suggest that conflict parties face a coordination problem. The contestants are ready to give up resources in order to implement an outcome that is both efficient and fair. At the same time, they are reluctant to act cooperatively because they fear exploitation by the opponent. Therefore, they want to match the expenditure of the opponent in symmetric binary conflicts with multiple prizes. Now communication can help the contestants to coordinate on the most efficient equilibrium, i.e., the sharing of the prizes at minimum expenditure. However, once prizes are non-divisible and people care about ex-post outcomes, such coordination is not feasible.

In the experiment two opposing parties are locked into a conflict situation, more specifically a variation of the Tullock contest (Tullock, 1980).⁴ They compete either for one indivisible prize or for multiple prizes (one per round). After some periods a window of opportunity arises in some conflict games. The contestants can communicate with each other in order to settle the conflict. In some cases a third party joins the communication and may even be able to punish misbehaving contestants. However, this window of opportunity closes eventually and the contestants fall back into the initial conflict situation until the end of the experiment.

The results show that contestants typically cannot achieve a cooperative outcome without communication. Conflicts with a high level of pre-communication conflict intensity also see less cooperation. However, in most conflicts communication reduces expenditure to a minimum level where it remains until the very end of the experiment, i.e. well after the end of the any verbal interaction. A detailed analysis of the communication shows that people are ready to cooperate if they can agree on a non-binding, mutually beneficial strategy. Hence, many conflict parties agree on a minimum effort level which maximizes the aggregate income and ensures equality in (expected) incomes. In the case of multiple prizes, some also agree on taking turns in winning which ensures an equitable outcome. If communication is de facto a necessary condition to end a conflict, the results also show that it is often not a sufficient one because of the aforementioned restrictions. Temporary constraints and contract incompleteness curtail the effectiveness of communication. In particular, conflict resolution is more likely to fail if an inseparable prize does not allow for equality of ex-post incomes. At the same time, third party interventions do not improve on this outcome even with a punishment option.

The next section describes how this study contributes to the literature while Section 3 the experiment in greater detail. Section 4 provides procedural details. Section 5 the behavioral predictions, and Section 5 documents the results. Section 6 discusses the implications of the results and concludes.

2. Contribution to the literature

Several studies provide clean evidence that communication restricts competitive and conflict behavior (Brandts et al., 2015; Cason et al., 2012; Harbring, 2006; Leibbrandt and Sääksvuori, 2012).⁵ One mechanism in this context is the positive impact of communication on trust (Wuthisatian et al., 2017). My results qualify this insight because they show some mechanisms that limit the effectiveness of communication in the alignment of interests even in rather abstract environments like an economic lab experiment. More specifically, long-term commitment problems provide one reason. This observation is in line with previous evidence. Eisenkopf and Bächtiger (2013), for example, study the impact of communication on conflict prevention in the context of a common-pool problem. They show that communication can lead to a particularly unfair distribution of common pool resources if one side signals trustworthiness but exploits the common pool unilaterally afterwards.⁶ However, the literature does not provide a systematic investigation into the limits of communication. This paper makes three contributions in this context.

² The first Minsk Protocol from September 2014, an agreement to halt the war in the Donbass region of Ukraine, provides a recent example for such an incomplete and unenforceable agreement. Even a subsequent additional memorandum did not eliminate ceasefire violations and clarify different interpretations of the agreed text. Kimbrough et al. (2015) provide more examples.

³ The fact that the Western Wall and the Al Aqsa Mosque are at the very same location is a major complication in the negotiations about the future status of Jerusalem amid the entire Israel–Palestinian peace process. Since these holy sites of Judaism and Islam are inseparable each party wants to exercise control about them.

⁴ The Tullock contest is arguably the most widely used model for the experimental study of conflicts (Abbink, 2010; Dechenaux et al., 2015; Kimbrough et al., 2017). It resembles key features of theoretical conflict models in political sciences (Favretto, 2009; Kydd, 2006; Sambanis and Shayo, 2013). The contestants can invest resources in order to increase the probability to win a prize. Any increase in investment simultaneously reduces the probability of the opponent to win the prize in the specific round.

⁵ See also the survey in Dechenaux et al. (2015).

⁶ The literature provides some experimental evidence on alternative mechanisms that address credibility problems in conflict resolution, for example side-payment agreements (e.g. Charness et al., 2007; Kimbrough and Sheremeta, 2013) or the commitment to a randomly determined outcome (Kimbrough et al., 2014). Kimbrough et al. (2015) show that even without binding contractual agreements individuals often avoid conflict by committing to the outcome of a conflict resolution mechanism.

First, the experimental literature provides no direct information on how conflict experience influences the impact of communication. Typically, communication in a conflict resolution treatment starts in the first experimental period and the relevant papers compare treatments with and without communication. The seminal paper by Isaac and Walker (1988) on the impact of communication on voluntary contributions provides an exception. They observe that people tend to fail to agree on an efficient outcome if they provided low contributions before the communications started. The results documented in this paper provide comparable evidence. Conflicts with a high initial investment level also see higher intensity during the (attempted) conflict resolution and even after a temporary settlement.

Second, the literature provides little insight into the impact of communication in the long run once chat-rooms have been closed. Evidence from experiments on communication in a Bertrand oligopoly suggests a limited long-term effect (Fonseca and Normann, 2012). Meanwhile Isaac and Walker (1988) observe that voluntary contributions deteriorate significantly from group optimality after communication has ended.

Last, but not least, third party interventions have received rather little attention in the relevant experimental literature even though they play a prominent role in many studies on interpersonal conflict resolution.⁷ The aforementioned study of Eisenkopf and Bächtiger (2013) also analyses the impact of third party interventions on the prevention of common pool resource exploitation. They find that communication and third party punishment act as complementary inputs which in combination deter almost any unilateral exploitation of the common pool. The key difference between this paper and the study by Eisenkopf and Bächtiger (2013) is the intervention stage of the third party. In Eisenkopf and Bächtiger (2013) third parties can prevent conflicts because they intervene before any other party can act, while in this paper they have to wait for several periods in which a conflict can unfold.

3. The experimental design

The experiment induced conflict via a Tullock contest (Tullock, 1980). Since the paper focuses on the long-term interaction of conflict partners, two players (call them A1 and A2) played this contest with each other for 50 rounds. At the beginning of a round each player A received an endowment of 1000 points (≈ 0.5 €) and could invest these points as conflict expenditure. Any points not invested were added to the player's point balance. As soon as every contestant had made a decision, the computer determined according to the probabilities defined by investment levels which of the two players would win the round. The probability of a player winning the round was equal to the total number of points invested by her, divided by the sum of points invested by both players A. If no player A had invested any points, the winning probability was 50% for each player. After the lottery each player A was informed about whether she had won or lost. Each player A also learned about how many points the other player A had invested in that round. Of course, participants did not know the identity of their opponent(s). The experiment then proceeded to the next round.

Each contest also affected a third party (Player T). This player T was attached to two contests simultaneously and received 1500 points per round. The investments of the players A in both contests reduced this endowment. More specifically, 4 points of investment by any player A reduced T's payoff by 1 point. Hence, player T's payoff was equivalent to the average income of the involved four players A in each round. During the experiment, however, player T only learned about the decisions and their payoff consequences from one contest while she did not learn anything about the other contest until the end of the experiment. This procedure guaranteed a behavioral independence between the two conflicts.

The experiment followed a 2×4 design with two conflict settings and four treatments within each setting. In the first ten rounds (out of 50) the subjects engaged in the conflict and knew about their specific setting. The two settings were identical in everything but the prize allocation procedure.

- In the *Multiple Prizes Setting* a player received a prize of 1000 points each time she won a round. The prize money was added to the winner's point balance at the end of the round.
- In the *One Prize Setting* two players competed for a single prize of 50,000 points. More specifically, a computer program determined randomly at the end of the experiment which of the 50 rounds was relevant for the prize allocation. The winner of that specific round received the 50,000 points. In all other rounds the respective winner received no prize at all. Of course, at the end of a round each player A was informed about whether she had won or lost that specific round.

At the end of the 10th round the subjects also learned about their specific treatment. The four experimental treatments had the same characteristics in both settings. They varied the conflict management mechanisms in rounds 11–25. Three of the four treatments allowed for pre-play communication in each round. During this communication stage the involved participants could send free-form messages to each other. All communication took place via onscreen chat boxes and lasted for 60 s per round. All participants were told not to reveal their identity. The four treatments differed as follows:

- In the *Communication Treatment* only the contesting players A could chat with each other in rounds 11–25. Player T suffered from any investment but was not involved in the interaction. Player T did not receive any information about the decisions during the entire experiment.

⁷ The interest in third party interventions is most obvious in political sciences (e.g. Bercovitch et al., 1991; Carnevale and Choi, 2000; Favretto, 2009; Goltsman et al., 2009; Greig, 2001; Greig, 2005; Kydd, 2003; Kydd, 2006; Kydd, 2010; Ramsbotham et al., 2011; Smith et al., 2002; Svensson, 2007; Wall et al., 2001) but other disciplines like psychology or legal studies have also taken an interest in the topic (Carnevale and Pruitt, 1992; Conlon et al., 2002; Mawritz et al., 2014; Subourne, 2003).

Table 1

The experimental design across the 50 rounds.

Settings	Rds. 1–10		Rounds 11–25	Rds. 26–50
Multiple prizes (1000 points per round) or One Prize (50,000 points in a randomly chosen round)	No communication	After Round 10: Treatment revelation	Control No communication. T cannot observe decisions. Communication Communication between the two contestants. T cannot observe decisions. Mediation Communication between the two contestants and T. T observes decisions. Punishment Communication between the two contestants and T. T observes decisions and can punish.	No communication

- In the *Mediation Treatment* the contesting players A and player T could chat with each other in rounds 11–25. Player T also received information about the decisions of the two players A in this contest in each of the 50 rounds.
- In the *Punishment Treatment* the contesting players A and player T could chat with each other in rounds 11–25. Moreover, player T could arbitrarily take points away from any of the two players A at the end of each of these rounds. The withdrawal of one point implied a cost of 0.2 points to player T. Again, player T received information about the decisions of the two players A in this contest in each of the 50 rounds.
- In the *Control Treatment* the contesting players A could NOT chat with each other in rounds 11–25. They had the possibility to write about their behavior while subjects in the other treatments chatted with each other. As in the Communication Treatment, the relevant player T suffered from any investment but did not receive any information about the decisions during the entire experiment.

Table 1 shows the timeline of the two settings and the treatments. Within a specific setting all subjects received identical instructions about the experiment and its four treatments before the experimental session started (see appendix). They learned about their specific roles (i.e., player A or T) immediately before round 1. All subjects knew that each session in a specific setting included all four treatments. During the first ten rounds player T was a passive observer in some conflicts but players A did not know whether they were observed. Players A learned about the assigned treatments and third party involvement in round 11. A Player T also learned at this time whether she was able to punish a player A in the contest in which she was actively involved. As mentioned above, the players T never learned about the decisions and the specific treatment conditions in the other contest that affected her payoff. They could only infer that the conflict was from either the Control or the Communication Treatment. From round 26 onwards the game resumed as in the first ten rounds. Again, player T was a passive observer in some conflicts (the same as in the first rounds) but players A now knew whether they were observed or not.

3.1. Procedure

I conducted 12 sessions with a total of 273 subjects.⁸ All sessions took place between January and December 2014 at the Lakelab at the University of Konstanz. Subjects were students from the University of Konstanz who were recruited with the software “ORSEE” (Greiner, 2015). The experiments were computerized with the software “z-Tree” (Fischbacher, 2007). Each subject participated in one of the treatments only. They received written instructions and comprehension questions that had to be answered correctly before the experiment could start. An English translation of the instructions is included in Appendix A of this paper. The sessions lasted approximately 120 min. and subjects received on average 34.50 Euro. All subjects received their payment privately at the end of their session.

4. Behavioral predictions

The experimental game yields a unique equilibrium across all settings, treatments and rounds if we assume common knowledge that all players are rational, risk-neutral and purely selfish. In this case, any communication represents cheap talk and players T will not punish. All players just care about their own payoff. The payoff function of a player A_i (with $i \in \{1, 2\}$) in the Multiple Prizes Setting in any round t is as follows:

$$\Pi_{A_i,t} = 1000 + \frac{c_{it}}{c_{it} + c_{jt}} 1000 - c_{it} \quad (1)$$

⁸ I do not include two additional subjects in the analysis. One subject in the Communication Treatment felt sick during the experiment and asked to leave the lab. She and the other subject involved in the specific conflict have been excluded from the sample. I also informed the affected Third Party about the leaving person. However since she did not mention this event in her discussions in her chat room correspondence in the other conflict in the Mediation Treatment I kept these subjects in the sample. All other participants in the session were told that their outcomes were not affected by the leaving person.

Player A_i thus wins the prize of 1000 points with a certain probability, which depends on her own cost of investment (c_{it}) in that round relative to the investments of both contestants ($c_{it} + c_{jt}$). The equilibrium investment in the last round is therefore 250 points for each contestant. Via backward induction we obtain the same prediction for any of the 50 rounds. The One Prize Setting yields the same stage game equilibrium. The expected prize per round is $\frac{1}{50} \times 50,000 = 1000$.

As mentioned above, the empirical results in the literature suggest that such a model is too parsimonious for a reasonable behavioral prediction. The well-observed positive impact of communication on aggregate outcomes (e.g. Brandts et al., 2015; Cason et al., 2012; Harbring, 2006; Leibbrandt and Sääksvuori, 2012) suggests that players start to cooperate once they can talk to each other. Most people regard nonbinding messages as a credible signal to implement pareto-improving outcomes. Mediation and, in particular, the threat of punishment should lead to yet more cooperation since a lack of cooperation can that induce the Third Party to reduce points from the uncooperative player. The evidence in Eisenkopf and Bächtiger (2013) supports these considerations. More specifically, that paper shows a positive interaction between mediated communication and punishment, as third parties can explain how whether and how they will sanction undesired behavior.

I do not expect that these treatment differences vanish immediately after the end of the communication interval. Players have learned that both sides are ready to implement mutually beneficial outcomes. However, while the incentive effects of the prize remain constant, the impact of the conflict mediation instruments should decline over time as the conflict parties cannot verbally reassure each other anymore that they will stick to the cooperative strategy. These considerations imply the following hypothesis.

Hypothesis 1 (Treatment differences within both settings):

- 1 Once communication is possible, conflict expenditure decreases in both settings.
- 2 Between round 11 and round 25, the expenditure levels are ordered as follows across the treatments:

Control > Communication $\geq$ Mediation $\geq$ Punishment.

- 3 Aggregate expenditure in rounds 26–50 is lower than in the first 10 rounds but higher than in rounds 11–25.

Now the focus turns on the difference between the two settings. The key expectation in this context is that there is less cooperation in the One Prize-Setting. Because of the partner matching protocol, the contestants essentially participate in one large conflict across 50 rounds. The One-Prize-Setting provides for a clear-cut winner in this context. Eisenkopf and Teyssier (2013) for example, show that shared prizes lead to substantially lower investments in tournaments. This reasoning implies lower conflict expenditure with multiple prizes and, crucially, a lower incentive to reignite the conflict after period 26.⁹

Hypothesis 2 (Differences across settings):

- 1 Across treatments and rounds, contest expenditure is higher in the One Prize Setting than in the Multiple Prize Setting.
- 2 Conflict Settlements are more stable in the Multiple Prize Settings.

5. Results

The expenditure level of the contestants over time forms the key variable for the analysis. Fig. 1 shows the development of this expenditure across the different settings, treatments and the 50 rounds. Note that in 6 out of altogether 109 conflicts, the contestants managed to resolve the conflicts on their own in the first 10 rounds. I ignore these cases in the subsequent treatment comparisons. If not mentioned otherwise, treatment comparisons focus on average conflict expenditure in a pair of contestants across a specific time frame. Reported p-values derive from Wilcoxon rank-sum tests (for between-treatment comparisons in a given time frame) or signed-rank tests (for comparisons within treatments across time).

The most prominent observation in Fig. 1 is the steep decline in expenditure in round 11 across all treatments except the Control Treatment. Once communication is possible, the expenditure level decreases well below the Nash equilibrium. Note that the decrease is highly significant ($p < .01$) in all treatments and settings, using Wilcoxon rank-sum tests and signed-rank tests respectively. This holds both for comparisons between the resolution treatments and the respective Control Treatments as well as for intragroup comparisons between rounds 1–10 and rounds 11–25, rank-sum tests also show that the differences between the various resolution treatments are not significant. This holds for comparisons of different treatments within the same setting as well as for comparisons of the same treatment across settings (all p-values > 0.1). Hence, I will pool the different treatments for the subsequent analysis. Across all resolution treatments, the expenditure in rounds 11–25 is slightly higher in the One Prize Setting than in the Multiple Prizes Setting.

In rounds 26–50, contest expenditure increases again in the resolution treatments. Compared to rounds 11–25 the increase is significant in the One Prize Setting but not in the Multiple Prizes Setting, according to the signed-rank test. However, this comparison ignores that some conflicts require more rounds to settle. A comparison of average expenditure at contest level in rounds 26–50 with rounds 21–25 only addresses this deficit. It yields significant differences in both settings (both p-values < 0.1). However, the average expenditure level is still significantly lower in comparison to the first ten rounds (both p-values < 0.1).

Result 1 (Treatment differences within both settings):

⁹ Note that inequity aversion would yield similar predictions in this context (Fehr and Schmidt, 1999; Herrmann and Orzen, 2008)

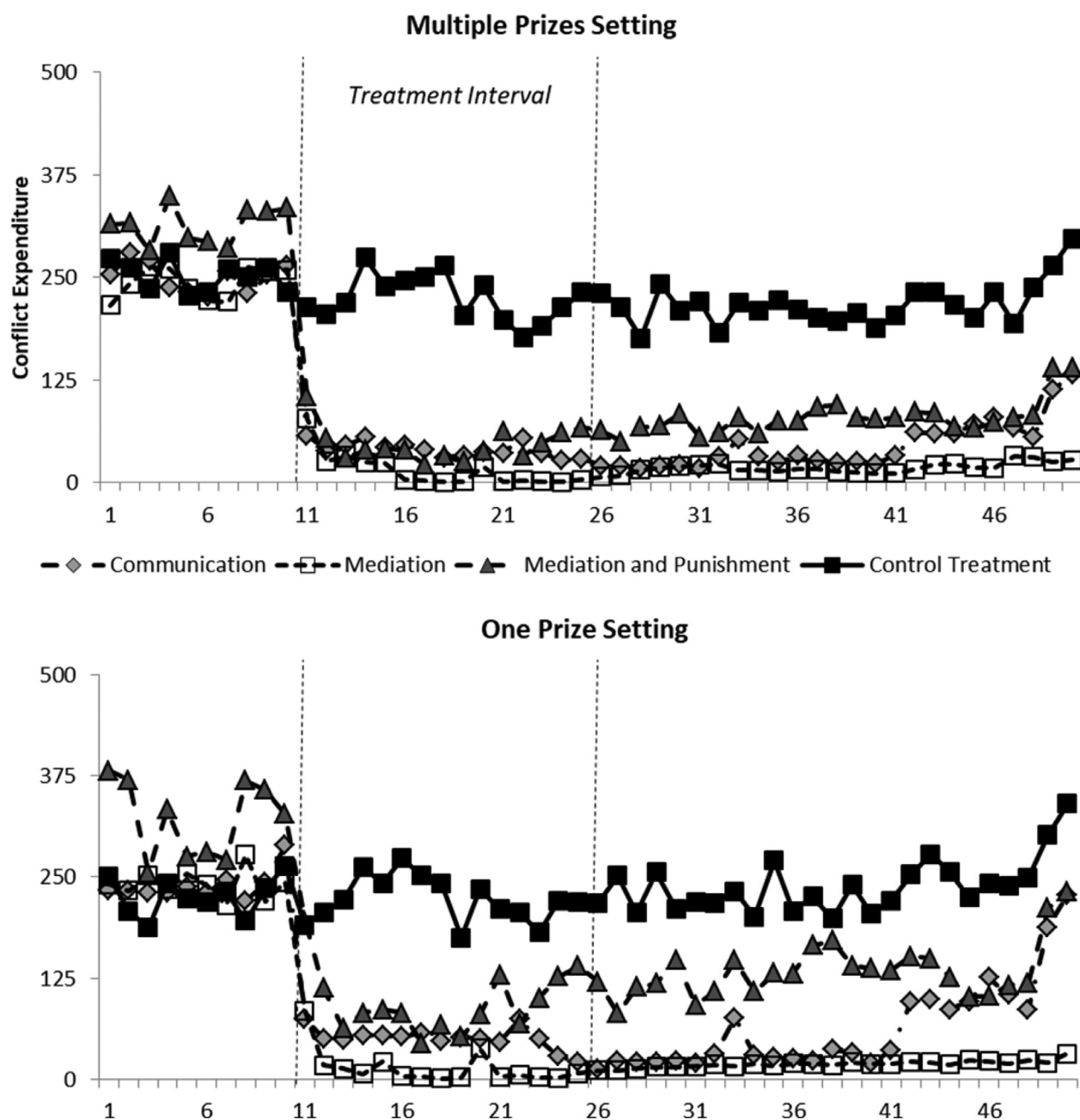


Fig. 1. Average conflict expenditure across settings, treatments and over time.

- 1 Once communication is possible, conflict expenditure decreases in both settings.
- 2 Between round 11 and round 25, the expenditure levels do not differ across the treatments with communication.
- 3 Aggregate expenditure in rounds 26–50 is lower than in the first 10 rounds but higher than in last rounds of the communication period.

The first statement in this result confirms the corresponding prediction in Hypothesis 1 regarding the impact of communication. The second statement of the first hypothesis is rejected. There are no significant differences between the treatments that allow for communication. The data provide qualified support for the third prediction regarding the long term impact of communication.

The estimations in Table 2 provide more detailed information about conflict expenditure of a subject in a round. They show the results from random-effects GLS regressions with standard errors clustered at the level of conflicts, separated for the different settings and the time zones (i.e., pre-treatment, during treatment and post-treatment). The Control Treatment

Table 2
Estimations of contest expenditure over time and across settings.

Multiple prizes			
Dep. Var.: Contest expenditure	Rounds 1–10	Rounds 11–25	Rounds 26–50
Communication	–0.52 (26.70)	–108.95*** (33.67)	–68.85** (33.18)
Mediation	5.15 (26.81)	–119.15*** (33.67)	–72.89** (30.52)
Punishment	26.34 (30.84)	–128.66*** (27.93)	–67.67** (32.13)
Round	–2.32 (2.00)	1.32* (0.67)	.50** (0.25)
Expenditure t–1	.64*** (0.05)	.48*** (0.06)	.59*** (0.10)
Winner t–1	–79.94*** (14.92)	–12.37** (6.02)	–23.02*** (8.46)
Constant	144.76*** (32.42)	100.87*** (37.24)	73.04** (36.44)
N	1008 (112 Conflicts)	1680 (112 conflicts)	2800 (112 conflicts)
R ²	.39	.55	.50
Single prize			
Communication	7.89 (29.65)	–68.80** (29.81)	–26.68 (20.04)
Mediation	394 (25.26)	–80.11*** (23.95)	–40.94** (18.97)
Punishment	18.76 (27.31)	–49.98* (26.85)	–57.27*** (19.91)
Round	3.43* (1.94)	2.59*** (0.48)	1.26*** (0.36)
Expenditure t–1	.74*** (0.06)	.66*** (0.07)	.75*** (0.06)
Winner t–1	–71.71*** (12.97)	–21.45** (8.81)	–19.13*** (6.98)
Constant	74.73*** (26.61)	39.79 (28.84)	24.24 (21.38)
N	954 (106 conflicts)	1590 (106 conflicts)	2650 (106 conflicts)
R ²	.51	.57	.61

*** $p < .01$;

** $p < .05$;

* $p < .1$. All models report results from random-effects GLS regressions with standard errors clustered at the level of conflicts (i.e., the interacting pair of contestants)

Table 3
Estimations of the impact of communication over time and across settings.

Dep. Var.: Contest expenditure	Rounds 1–10	Rounds 1–25	Rounds 11–50	Rounds 1–50
Control Treatment	–11.45 (15.64)	–12.05 (17.67)	77.24 (17.15)	–11.77 (16.92)
Round 11–25		–121.66*** (15.73)		–106.64*** (13.08)
Round 26–50			–7.27* (3.96)	–113.32*** (13.76)
Control Treatment × Round 11–25		90.16*** (16.92)		86.90*** (15.16)
Control Treatment × Round 26–50			–18.06** (7.76)	68.88*** (15.94)
Round	.47 (1.40)	2.14*** (0.49)	1.23*** (0.23)	1.21*** (0.23)
Constant	129.28*** (26.92)	98.21*** (20.24)	–33.44*** (12.49)	84.63*** (16.64)
Control Var.	Yes	Yes	Yes	Yes
N (# conflicts)	1962 (218)	5232 (218)	8720 (218)	10,682 (218)
R ²	.45	.56	.56	.57

*** $p < .01$;

** $p < .05$;

* $p < .1$. All models report results from random-effects GLS regressions with standard errors clustered at the level of conflicts (i.e., the interacting pair of contestants). Observations from treatments with communication are pooled and provide the benchmark. Setting, Expenditure t–1 and Winner t–1 have been included as additional control variables.

provides the benchmark.¹⁰ The regressions include treatment dummies for the other treatments, a trend variable (round) and information about the previous round (the expenditure of the subject and whether she has won that round).

The pre-treatment estimations (rounds 1–10) do not reveal any significant treatment effect in both settings. This is also due to the large heterogeneity in conflict expenditure within each treatment.¹¹ Unsurprisingly, past conflict expenditure correlates highly with current investments. Winners (losers) of the previous conflict decrease (increase) their stakes in the conflict. After round 11, all treatments in both settings differ from the Control Treatment. The relevant coefficients become smaller – and in one case insignificant – after period 26.

5.1. The impact of communication

The preceding results show that the start of communication has a strong and persistent impact in the experiment. Table 3 provides estimations of the size of this impact. The different models show the results from random-effects GLS regressions

¹⁰ Replicating the estimations with another treatment as benchmark yields insignificant treatment coefficients except for the Control Treatment. Hence, we see no significant treatment differences in the parametric estimations between the three treatments with communication.

¹¹ Of course, this heterogeneity decreases sharply once the conflict parties use communication to agree on a cooperative strategy.

Table 4

Categories for coding messages and observed frequency in chat communication across settings and treatments (First round communication in parentheses).

Cat	Description	Multiple Prizes Setting Prize = 1000 points in each round			One Prize Setting Prize = 50,000 points in one round		
		Direct Comm.	Comm. with Mediator	Mediator with Pun.	Direct Comm.	Comm. with Mediator	Mediator with Pun.
1	One Participant A does not communicate	.014 (0.071)	.164 (0.143)	.093 (0.213)	.030 (0.076)	.05 (0)	.108 (0.077)
2	Mediator does not communicate	–	.071 (0.143)	.243 (0.392)	–	.121 (0.143)	.123 (0.192)
3	The participants A agree on minimal investments	.321 (0.5)	.379 (0.429)	.393 (0.607)	.377 (0.5)	.478 (0.464)	.315 (0.577)
4	The participants A agree on a different behavior	.064 (0)	.064 (0.142)	.007 (0.036)	.015 (0.038)	.15 (0.32)	.185 (0.192)
5	The participants A agree on taking turns in winning	.278 (0.393)	.171 (0.286)	.164 (0.25)	.046 (0.115)	.007 (0)	.069 (0.077)
6	Mediator threatens withdrawal of points	–	–	.043 (0.143)	–	–	.031 (0.077)
7	The participants criticize / insult each other	0 (0)	.057 (0.071)	.057 (0)	.008 (0)	.014 (0)	.038 (0)
8	<i>The participants discuss the rules of the experiment.</i>	.035 (0)	.128 (0.071)	.121 (0.036)	.115 (0.038)	.214 (0.036)	.231 (0.115)
9	<i>One participant A signals a strong desire to win.</i>	0 (0)	0 (0)	0 (0)	.015 (0)	0 (0)	.008 (0.038)
10	<i>The discussion focuses on past behavior</i>	.071 (0)	.128 (0.071)	.236 (0.071)	.069 (0.038)	.071 (0.071)	.115 (0.038)
11	<i>Discussion focuses on future earnings.</i>	.385 (0.5)	.486 (0.679)	.314 (0.536)	.4 (0.346)	.393 (0.464)	.423 (0.538)

Bold Letters denotes categories with Cohen's Kappa reliability above 0.6. *Italics* denote codes with Cohen's Kappa reliability below 0.3.

with standard errors clustered at the level of conflicts. The observations from all six treatments with communication are pooled. They provide the benchmark which I compare with behavior in the Control Treatment without communication. The regressions include treatment dummies for the other treatments, a dummy for the setting, a trend variable (round, reported in the table) and information about the previous round (the expenditure of the subject and whether she has won that round).

The first two models in Table 3 confirm the preceding findings. Model 1 estimates the investments in the first 10 rounds and observes no meaningful differences between the treatments. Model 2 extends the observation until round 25. A dummy variable indicates whether the observation is from the rounds 11–25. This model shows again that communication strongly reduces contest expenditure.

Model 3 provides novel insights. It excludes the first 10 rounds and compares the behavior in the treatment period (rounds 11–25) and the post treatment period (rounds 26–50). Hence it identifies whether the elimination of communication leads to a stronger increase in contest expenditure. The trend variable *Round* is highly significant (as it is in Model 2). Even throughout the treatment period there is a rather strong intertemporal trend against cooperation (see also the preceding Table 2). In such a context, the end of communication actually has a significant *positive* effect on cooperation. Model 4 finally includes the observations from all rounds. It confirms that the post-treatment behavior is significantly different from the pre-treatment action if one takes the intertemporal trend into account.

Now the focus turns on how communication fosters cooperation by studying the content of the chat messages. The procedure of the content analysis resembles the approaches in Cason et al. (2012), Leibbrandt and Sääksvuori (2012) or Eisenkopf (2014) who again built there procedures on many similar previous studies, in particular Brandts and Cooper (2007). First, I randomly selected two sessions from each setting to develop a coding scheme with 11 categories. Each chat room discussion was coded by two individuals independently according to these categories. Coders could propose other categories after reading the first communication protocols but did not do so. Coders were instructed to signal incidents in which participants lifted their anonymity, e.g. by sending their real name, e-mail addresses or phone numbers). They did not report any incident.

Coding focused on all messages in a given round in each conflict (i.e., between the two contestants and the third party, if applicable). If such a unit of observation was deemed to contain the relevant category of content, it was coded as 1 for that category and 0 otherwise. Each unit was coded under as many or few categories as the coders deemed appropriate. The coders also knew the experimental instructions provided to subjects so that they understood the strategic environment the subjects faced. The analysis focuses on those categories in which the coders had a moderate or substantial agreement, as measured by Cohen's Kappa (Cohen, 1960). This measure takes a value of 0 when the agreement is consistent with random chance and 1 when the coders agree perfectly. The categories with bold letters in Table 4 indicate a strong degree of consistency (Cohen's Kappa > 0.6) while those in italics do not even have a moderate degree of consistency (< 0.3). Categories

Table 5

Communication content and conflict expenditure.

Multiple Prize Setting Dep. Var: Contest Expenditure	Cat. 3 (agreement on minimal investments)	Cat. 4 (agreement on other behavior)	Cat. 5 (agreement on taking turns)	Cat. 7 (critique / insult)	All categories
Cat. 3.	–127.81*** (44.98)				–96.44** (35.97)
Cat. 4		227.83*** (28.17)			173.05*** (22.37)
Cat. 5			–101.48*** (35.75)		–80.26*** (25.19)
Cat. 7				19.99 (73.10)	21.52 (68.08)
<i>N</i>			Controls included 168 (38 conflicts)		
<i>R</i> ²	.36	.41	.35	.32	.45
One Prize Setting					
Cat. 3.	–116.60*** (33.89)				–136.47*** (34.27)
Cat. 4		–11.89 (44.46)			–75.71* (44.51)
Cat. 5			–286.36** (119.91)		313.11*** (97.99)
Cat. 7				253.30*** (85.13)	185.45*** (70.29)
<i>N</i>			Controls included 366 (37 conflicts)		
<i>R</i> ²	.47	.45	.46	.46	.50

*** $p < .01$;** $p < .05$;

* $p < .1$. All models report results from random-effects GLS regressions with standard errors clustered at the level of conflicts (i.e., the interacting pair of contestants). The regressions include all pair of contestants in rounds 11–25 that could communicate and did not manage to settle in the preceding round (i.e., aggregate conflict expenditure ≥ 1). Control variables include: Treatment dummies, the period, contest expenditure in the preceding period.

8–11 fall into this group and we ignore them in the subsequent analysis. Furthermore, the first two categories show that we have some cases in which at least one of the involved participants does not communicate.

The descriptive statistics in Table 4 show category 3 (“The participants A agree on minimal investments”) as the most frequently coded category in both settings. However, there are also interesting differences between the settings. Groups with third parties involved are more likely to agree on other (i.e., inefficient) outcomes in the One Prize Setting (Category 4). Meanwhile, in the Multiple Prize Setting, rather many groups also explicitly agree on taking turns, while just a few groups do so in the One Prize Setting.

The question is now how such features of communication relate to the subsequent conflict expenditure. Table 5 provides results from random-effects GLS regressions with standard errors clustered at the level of conflicts, differentiating between the two different settings. It focuses on the behavior of conflict parties that could communicate and did not settle their conflict in the preceding round. The regressions include some control variables (Treatment dummies, the period, contest expenditure in the preceding period). The table includes five different regressions, One regression for each category separately and one regression in which all categories are independent variables.

The key observation is that an agreement on minimal expenditure has a strong negative effect on actual conflict investments in both settings (category 3). The results also show lower expenditure if groups agree on taking turns (category 5). Interestingly, the coefficient in the One Prize Setting is more than twice as large as in the Multiple Prizes Setting but the difference between the coefficients is not significant. This underlying higher variation in the One Prize Setting indicates that some contestants do not stick such an agreement in this setting. Such an increased behavioral risk is in line with the theoretical predictions. It provides an explanation why fewer contestants agree on taking turns in the One Prize Setting. The results also show interesting differences across the settings. Agreements on alternative expenditure levels increase contest investments in the Multiple Prizes Setting (where they are less likely) but not in the One Prize Setting. We see the opposite picture in case the contestants criticize or insult each other. Such behavior increases expenditure in the One Prize Setting but not in the Multiple Prize Setting.

5.2. Differences between the settings

Now the focus is on the expenditure differences between the settings. The aforementioned results in Fig. 1 already suggest a meaningful difference in behavior between the settings. Wilcoxon rank-sum tests confirm this observation. Average conflict expenditure in a pair of contestants, does not differ in the in the first 10 rounds ($p = .45$). However, it is significantly higher in the subsequent rounds in the One Prize Setting if people can communicate ($p = .01$ for periods 11–25 and $p < 0.1$ for the last 25 rounds). There are no significant differences in conflict expenditure between the respective Control Treatments throughout the experiment (all p -values > 0.51).

These insights raise the question whether Multiple Prizes lead more often to successful conflict resolution. To do so, I disentangle different resolution patterns for the individual conflicts. Table 6 provides an overview of average and median expenditures as well as successful conflict settlements across settings and treatments, differentiating for the levels before, during and after the specific interventions. I consider a conflict as (temporarily) settled if the average expenditure in the last

Table 6

Average contest expenditure across treatments.

Settings	Treatments		Rounds 1–10	Rounds 11–25	Rounds 26–50
Multiple Prizes Setting	Control	MeanMedianSettlements /N	275.13283.61/14	226.5236.51/14	197.8169.61/14
	Communication	MeanMedianSettlements /N	260.4250.80/14	31.8113/14	29.0110/14
	Mediation	MeanMedianSettlements /N	246.4213.30/14	16.2114/14	18.118 / 14
	Punishment	MeanMedianSettlements /N	305.6289.41/14	2.2114/14	29.3112/14
One Prize Setting	Control	MeanMedianSettlements /N	225.9182.51/13	222.5223.31/13	238.7227.31/13
	Communication	MeanMedianSettlements /N	239.5108.50/13	51.219/13	63.060.24/13
	Mediation	MeanMedianSettlements /N	241.3270.42/14	14.4112/14	19.419/14
	Punishment	MeanMedianSettlements /N	322.2285.51/13	95.919/13	134.826/13

A conflict is (temporarily) settled if average expenditure in rounds 6–10 / 21–25 / 46–50 is smaller or equal to 1. *N* indicates the number of observed conflicts

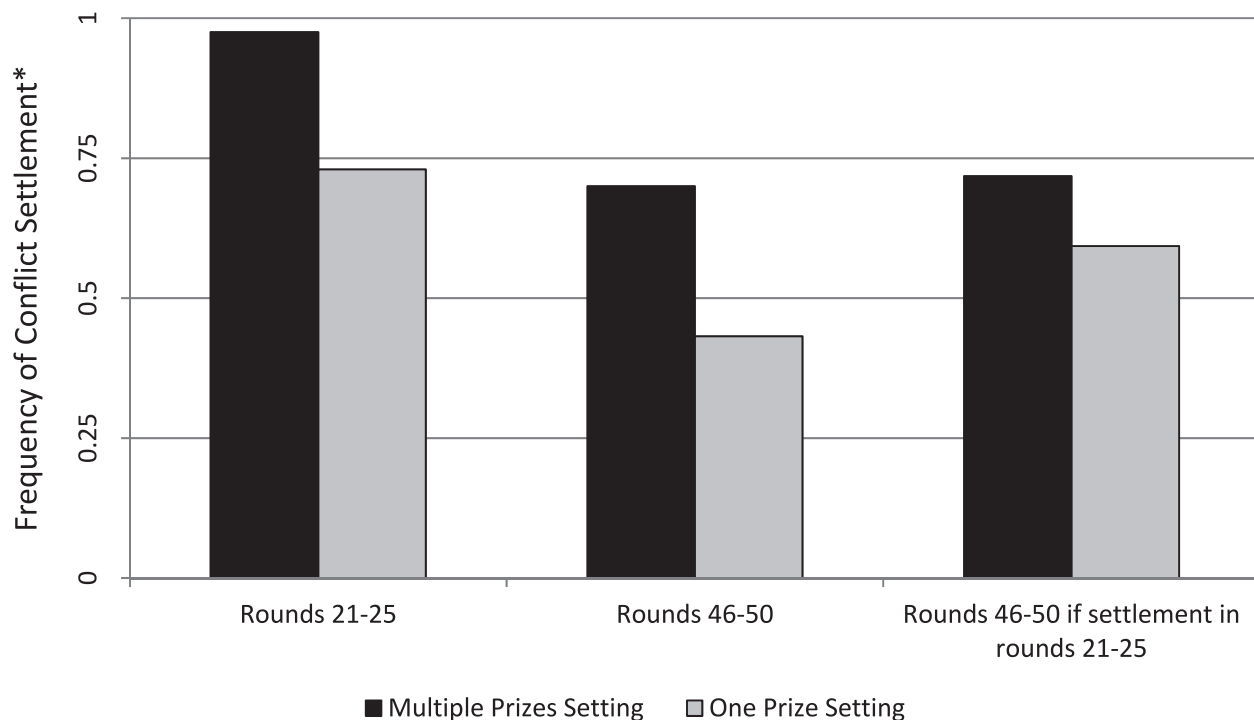


Fig. 2. The share of settled conflicts across the different settings and stages of the experiment (Without observations from the Control Treatment and conflicts with settlements in round 1–10)

* A conflict counts as (temporarily) settled if the group expenditure in rounds 21–25 / 46–50 is smaller or equal to 1.

five rounds of the treatment phase (i.e., rounds 21–25) or the entire experiment (i.e., rounds 46–50) is smaller or equal to one. This definition is rather strict because the focus of the paper is on the long-term stability of conflict resolution. Hence, a stable resolution should withstand one-off deviations, boredom and in particular end-of-the-game effects. Nevertheless, most conflicts see a long-term settlement, i.e., successful conflict resolution. However, the One Prize Setting results in more unresolved conflicts. In these unresolved conflicts the expenditure levels can be very high. This explains why the differences between the different resolution treatments are not significant.

Note that the threshold “ ≤ 1 ” in Table 6 for conflict settlement takes into account that some groups agreed on taking turns such that one contestant provided a positive investment and the other one an investment of zero before changing the roles in the following round. This occurred more frequently in the Multiple Prizes Setting than in the One Prize Setting (15% vs 5.4% for all conflicts in rounds 26–50, see below). A key motive for this difference is that the role-shifting guarantees equal payoffs with multiple prizes but not with a unique prize.

Fig. 2 combines the data on conflict settlements in the three treatments with communication across the two settings.

The first pair of bars in Fig. 2 shows that all but one conflict in the Multiple Prizes Setting (or 97.5%) are settled in rounds 21–25. In the One Prize Setting, 73.6% of the conflicts are at least temporarily settled. This is still an impressive number but a Fisher exact test reveals that the One Prize Setting contains more unsettled conflicts ($p < 0.05$). The second pair of bars shows that, at the end of the experiment, 70% of all conflicts in the Multiple Prizes Setting and 42.3% in the One Prize Setting are settled. Again, this difference in settlement rate is significant according to the Fisher exact test ($p < 0.05$). Note that all

Table 7

Conflict history and (post-)treatment expenditure.

	Model 1	Model 2	Model 3	Model 4
Dep. Var.:	Input rounds 11–25	Input rounds 11–20	Input rounds 26–50	Input rounds 26–50
Contest expenditure	All conflicts with communication	Settlement in rounds 21–25	All conflicts with communication	Settlement in rounds 21–25
Group Input rounds 1–10	.164** (0.078)	.036** (0.018)	.153** (0.066)	.046* (0.027)
One Prize Setting	37.273* (20.920)	–3.611 (4.984)	46.927** (21.398)	11.802 (12.468)
Mediation	–25.362 (22.392)	17.201** (7.278)	–26.651 (18.906)	–4.534 (13.154)
Punishment	–4.385 (30.836)	–3.591 (3.459)	24.823 (29.049)	2.606 (16.607)
Round	–1.688*** (0.590)	–2.760** (1.140)	1.868*** (0.521)	1.408** (0.548)
Constant	–25.260 (43.207)	42.563* (20.637)	–133.830*** (48.184)	–60.891 (27.085)
N	2460 (82 conflicts)	1420 (71 conflicts)	4100 (82 conflicts)	3550 (71 conflicts)
R ²	.085	.048	.118	.027

*** $p < .01$;** $p < .05$;* $p < .1$. All models report results from random-effects GLS regressions with standard errors clustered at the level of conflicts (i.e., the interacting pair of contestants)

conflicts with a settlement in rounds 46–50 also saw a settlement in rounds 21–25. These settlements remained permanent throughout rounds 26–50. Restricting the comparison of final settlements to those conflicts that also had seen a settlement in rounds 21–25 does not show significant differences anymore (72% vs 59%, as the third pair of bars in Fig. 2 shows).

Result 2 (Differences across settings).

- (1) Across treatments and rounds, contest expenditure is higher in the One Prize Setting than in the Multiple Prize Setting during and after communication.
- (2) Conflict Settlements are more stable in the Multiple Prize Settings.
- (3) The equalization of *expected* payoffs is sufficient for permanent conflict resolution in about half of all cases. The opportunity to achieve approximately equal *actual* payoffs enhance the effectiveness of communication.

The second result only partially confirms the second hypothesis. Conflicts with an indivisible prize are not *per se* more intense, but they are more difficult to resolve in a longer perspective because some people perceive equal chances as insufficient to stick to an agreement.

5.3. The impact of conflict intensity on resolution

Now I study how the first ten rounds in the first experiment influence the subsequent outcomes of conflict resolution. The analysis focuses on whether within-treatment between-conflict heterogeneity in expenditure persists once the conflict parties can communicate with each other. A positive result would suggest that communication is less likely to settle more intense conflict. Table 7 shows the outcome of four random-effect GLS estimations in which conflict intensity in the first rounds (Group Input rounds 1–10) is a predictor for subsequent investments. All models focus on the three treatments with communication possibilities, using the Communication Treatment as benchmark. They control for the different treatments and settings. The results are robust to changes in these control variables, e.g. interaction terms between settings and treatments.

In Model 1 the dependent variable is the conflict expenditure in the treatment phase (rounds 11–25). It shows that the initial conflict history has a significant impact on the subsequent conflict resolution outcome. More intense conflicts are more difficult to resolve. Model 2 shows that this also holds for those conflicts which eventually achieve a (temporary) resolution. Of course, the coefficient is much smaller than in the first model because the conflict parties eventually get to the most efficient expenditure levels. While the coefficients for the conflict history appear rather small they are surprisingly persistent as Models 3 and 4. In Model 3, the dependent variable is the conflict expenditure in the post-treatment phase (rounds 26–50). The impact of conflict history is essentially the same as in model 1. Model 4 uses the same dependent variable as Model 3 but is restricted to those conflicts that achieve a temporary resolution in rounds 21–25 (like Model 2). The weakly significant impact in this very conservative estimation suggests that using communication as a conflict resolution device is more difficult in more intense conflicts.

Result 3: Initial conflict intensity is a significant negative predictor for the subsequent reduction in conflict expenditure.

6. Conclusion

Many studies have shown how communication between people reduces their conflict behavior. This paper extends the literature by studying three potential limitations of communication as a conflict resolution device. In general the results show a strong impact of intermediate communication, as most conflict parties reduce their expenditure. In many cases the effect is also quite persistent once communication between conflict parties is not possible anymore. The evidence in this paper also suggests how communication fosters cooperation in this context. People can signal their intentions to implement specific cooperative actions and outcomes. The repeated interaction then reveals the honesty of the intentions.

As a first limitation, it was unclear whether any coordination effect persists once communication between the conflict parties is not possible anymore. While I observe a substantial long-term effect of communication in many conditions, the indivisibility of the prize reduces the stability of agreements between the contestants. Second, it was not obvious whether preceding conflict experience limits its effectiveness, and to which extent. [Isaac and Walker \(1988\)](#) observe that people fail to agree on an efficient outcome if they provided low contributions before the communications started. At the qualitative level, the results confirm this effect also for a conflict setting but the size of the impact is rather small. Third, the literature provided little evidence whether bipartisan communication would benefit from other additional conflict resolution tools such as third party intervention or punishment. Here the results do not identify significant added value for these additional measures.

The experiment in this paper relied on the Tullock Contest to model the conflict. The observations of this paper imply that this type of contest captures the essence of purely economic conflicts. However, many conflicts do not just rely on economic factors but also on perceived grievances. Some models of reciprocal behavior take considerations of kindness or unkindness into account ([Falk and Fischbacher, 2006](#); [Levine, 1998](#)). [Hopfensitz and Reuben \(2009\)](#) provide nice evidence on how unfair distributions can trigger negative emotions and induce vicious cycles of punishment. My result on the impact of conflict history on resolution success suggests that such attitudes also apply in a repeated Tullock contest. However, in most cases experimental participants have no serious negative attitudes towards their opponents in such a contest. Therefore, experimental researchers intending to study additional limits of communication as a conflict resolution instrument might take recruit their experimental participants from hostile groups.

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Appendix A. Experimental instructions (for both settings, differences are indicated)

Welcome to this economic experiment.

Your decisions and the decisions of the other participants will affect your payoff. Hence, it is important that you read these instructions carefully. Please contact us before the experiment starts if you have any question.

Please do not talk with the other participants during the experiment.

Otherwise we might exclude you from the experiment and any subsequent payment.

During the experiments we always talk about points that determine your income. At the end of the experiment we convert all points into Euros, using the following exchange rate.

2000 points = Euro

You get your payments at the end of the experiment in cash. Now we explain you the experiment in detail.

Experimental setup

In this experiment we distinguish between two participants A (A1 and A2) and one participant B. We describe the role of participant B below. We focus at first on participants A. The experiment consists of 50 rounds. In the beginning two randomly chosen participants A are matched with each other. They interact with each other throughout all 50 rounds.

Multiple Prizes Setting	One Prize Setting
In each round exactly one of the participants A can win 1000 points. The success probability depends on the level of the respective investments in that round.	At the end of the experiment (i.e., after round 50), exactly one of the two participants A can win an additional 50,000 points. The success probability depends on the level of the respective investments in one round which will be chosen by a random device at the end of the experiment.

At the beginning of each round, each participant A gets 1000 points. Each participant A can investment as many of these points as she wants (i.e., between 0 and 1000 points). All points not invested in a round will accrue to the personal account of that participant A. The relationship between the two investments determines the success probability. If both participants A invest the same amount, their success probabilities are 50% each. This also holds if both participants A invest 0 points. If you invest more than the other participant, your success probability is also higher. Of course, this still implies that the participant with the higher investment may not win the prize. The exact formula for the success probability (of participant A1) is:

$$\text{Success Probability} = \frac{\text{Investment Participant A1}}{\text{Investment Participant A1} + \text{Investment Participant A2}}$$

Considering this probability, the computer uses a random mechanism to assign the 1000 points to one of the participants. At the end of each round both participants receive information about both investments and who win the prize in the round. (Setting 2: At the end of each round both participants receive information about both investments and who would win the prize if the round is chosen at the end of the experiment.)

The investments of participants A also have an impact on the payoff for participant B. More specifically increasing investments decrease the payoff of participant B. Participant B gets 1500 points per round. Every invested point by one participant A reduces B's payoff by 0.25 points. Note that the investment decisions of four participants A (two pairs) influence the payoff of player B. However player B observes only the decisions from one pair.

Examples

The numbers for the following examples have been arbitrarily chosen.

Multiple Prizes Setting	One Prize Setting
<i>Example 1:</i> Participant A1 invested 10 points and participant A2 50 points. Therefore, participant B gets a reduction of 15 points from this pair $((50 + 10) \cdot .25)$. The probability of participant A1 to get the 1000 points in that round is $1/6$. $\text{Probability} = \frac{10}{10+50} = \frac{1}{6}$ In consequence, the probability for player A2 is $5/6$. If A1 gets the prize her income in this round is $1000 + 1000 - 10 = 1990$ Participant A2 would get 950 points in this case $(1000 - 50)$. If A1 does not get the prize her income in this round is $1000 - 10 = 990$ Participant A2 would get 1950 points in this case $(1000 + 1000 - 50)$. get 990 points and A2 950 points. <i>Example 2:</i> Participant A1 invested 600 points and participant A2 400 points. Therefore, participant B gets a reduction of 250 points from this pair $((600 + 400) \cdot .25)$. The probability of participant A1 to get the 1000 points in that round is $6/10$ or 60%.	<i>Example 1:</i> Participant A1 invested 10 points and participant A2 50 points. Therefore, participant B gets a reduction of 15 points from this pair $((50 + 10) \cdot .25)$. The probability of participant A1 of getting 50,000 points is $1/6$, if this round is chosen at the end of the experiment. $\text{Probability} = \frac{10}{10+50} = \frac{1}{6}$ In consequence, the probability for player A2 is $5/6$. If A1 gets the prize her income in this round is $50,000 + 1000 - 10 = 50,990$ Participant A2 would get 950 points in this case $(1000 - 50)$. If A1 does not get the prize in this chosen round her income in this round is $1000 - 10 = 990$ Participant A2 would get 50,950 points in this case $(50,000 + 1000 - 50)$. If this round is not chosen, participant A1 would <i>Example 2:</i> Participant A1 invested 600 points and participant A2 400 points. Therefore, participant B gets a reduction of 250 points from this pair $((600 + 400) \cdot .25)$. The probability of participant A1 of getting 50,000 points is $6/10$ or 60% if this round is chosen at the end of the experiment.

(continued on next page)

Multiple Prizes Setting	One Prize Setting
$Probability = \frac{600}{600+400} = \frac{6}{10}$ In consequence, the probability for player A2 is 4/10 or 40%. If A1 gets the prize her income in this round is $1000 + 1000 - 600 = 1400$ Participant A2 would get 600 points in this case (1000 – 400). If A1 does not get the prize her income in this round is $1000 - 600 = 400$ Participant A2 would get 1600 points in this case (1000 + 1000 – 400).	$Probability = \frac{600}{600+400} = \frac{6}{10}$ In consequence, the probability for player A2 is 4/10 or 40%. If A1 gets the prize her income in this round is $50,000 + 1000 - 600 = 50,400$ Participant A2 would get 600 points in this case (1000 – 400). If A1 does not get the prize her income in this round is $1000 - 600 = 400$ Participant A2 would get 50,600 points in this case (50,000 + 1000 – 400). If this round is not chosen, participant A1 would get 400 points and A2 600 points.

Variations

Using a random mechanism the computer decides in the beginning of the experiment under which experimental variation the participants make their decisions. There are four different variations of the experiment which we describe in detail now. **These variations concern rounds 11–25 only.** You will learn in round 11 which variation is relevant for you. During the first 10 rounds and the last 25 rounds, the experiment proceeds as mentioned above. As participant B you cannot make any decisions in these rounds.

Variation 1: Both participants A, but not participant B, can chat with each other for 60 seconds in each round before they make their decisions. This holds for the rounds 11–25. At the end of each round both participants A, but not participant B, receive information about the relevant decisions and the prize allocation.

Variation 2: Both participants A and participant B can chat with each other for 60 seconds in each round before they make their decisions. This holds for the rounds 11–25. At the end of each round both participants A and participant B receive information about the relevant decisions and the prize allocation.

Variation 3: Both participants A and participant B can chat with each other for 60 seconds in each round before they make their decisions. After learning about the decisions and the prize allocation, participant B can reduce points from each participant A. Note that the payoff of a participant A in a round cannot be negative. Each reduced point costs participant B 0.2 points. Hence a withdrawal of 10 points for any participant A costs B 2 points. This holds for the rounds 11–25. At the end of each round both participants A and participant B receive information about the relevant decisions and the prize allocation.

If you want to send a chat message, please type the text and press the enter key.

To protect your anonymity it is important that you do not reveal any information about your personal characteristics or your seat number in these chat messages.

Variation 4: Neither participants A nor participant B can chat with each other for 60 seconds in each round before they make their decisions. This holds for the rounds 11–25. At the end of each round both participants A, but not participant B, receive information about the relevant decisions and the prize allocation.

An example on the reduction of points in variation 3.

The numbers for the following examples have been arbitrarily chosen.

Multiple Prizes Setting	One Prize Setting
Participant A1 invested 200 points and participant A2 300 points. Therefore, participant B gets a reduction of 125 points from this pair $((200 + 300) \cdot .25)$. Participant A1 has received the prize, her income in that round is $1000 + 1000 - 200 = 1800$ Participant A2 has received 700 points in that round $(1000 - 300)$. Participant B learned about the payments and the decisions and reduces 200 points from participant A1 and 250 points from participant A2. The final payoffs for participant A1 are therefore 1600 points $(1000 + 1000 - 200 - 200 = 1600)$ and 450 points for participant A2 $(1000 - 300 - 250)$. For participant B his income of 1500 points will be reduced by 125 reduced by 125 points and an additional 90 points $(0.2 \cdot (200 + 250))$. On top of it, further reductions apply from any expenditure of two other participants A whom participant B cannot observe.	Participant A1 invested 200 points and participant A2 300 points. Therefore, participant B gets a reduction of 125 points from this pair $((200 + 300) \cdot .25)$. For participant A1 the income in that round is $1000 - 200 = 800$ Participant A2 has received 700 points in that round $(1000 - 300)$. If the round is chosen at the end of the experiment, participant A1 will get the 50,000 points. However, the participants do not know whether the round will be chosen. Participant B learned about the payments and the decisions and reduces 200 points from participant A1 and 250 points from participant A2. The final payoffs for participant A1 are therefore 1600 points $(1000 - 200 - 200 = 600)$ and 450 points for participant A2 $(1000 - 300 - 250)$. For participant B his income of 1500 points will be reduced by 125 reduced by 125 points and an additional 90 points $(0.2 \cdot (200 + 250))$. On top of it, further reductions apply from any expenditure of two other participants A whom participant B cannot observe.

Procedure

One round proceeds as follows

Multiple Prizes Setting	One Prize Setting
1. Each participants A gets 1000 points, each participant B 1500 points. 2. Rounds 11–25: Communication between the participants A (Variation 1) or between participants A and B (Variations 2 and 3). 3. Investment decisions of participants A: The invested points will be reduced from the 1000 points. 4. The computer allocates the prize to one participant A. 5. Rounds 11–25: Participant B can reduce points from the participants A (Variation 3) 6. Full information about relevant decisions and prize allocations.	1. Each participants A gets 1000 points, each participant B 1500 points. 2. Rounds 11–25: Communication between the participants A (Variation 1) or between participants A and B (Variations 2 and 3). 3. Investment decisions of participants A: The invested points will be reduced from the 1000 points. 4. The computer decides which player A wins the round. 5. Rounds 11–25: Participant B can reduce points from the participants A (Variation 3) 6. Full information about relevant decisions and prize allocations. 7. Round 50: The computer decides which round is relevant for the payment of the prize.

At the end of the experiment we will ask you to fill in a short questionnaire. We will add up all your earned points, convert them in Euro and pay of you in cash.

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