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The Persistence of "Bad" Precedents and the Need for Communication: A Coordination Experiment

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Abstract

Precedents can facilitate successful coordination within groups by reducing strategic uncertainty, but they may lead to coordination failure when two groups with diverging precedents have to interact. This paper describes an experiment to explore how such coordination failure can be mitigated and whether subjects are aware of it. In an initial phase, groups were able to establish a precedent in a repeated weakest-link game, and in a second phase two groups with different precedents are merged into a larger group. As expected, this leads to coordination failures. Unlike most of the previous literature, subjects could endogenously choose to communicate in the merged group for a small fee. The results suggest that communication can mitigate the coordination failure in the merged group and, in most cases, leads to efficient coordination. However, subjects in particular from groups with an efficient precedent in the initial phase are inattentive to the potential coordination failure and choose not to communicate. This can have profound consequences since groups who fail to implement communication are unable to achieve efficient coordination in the second phase. The results may be useful for the understanding of how groups learn to solve coordination problems from past coordination success or failure.

Keywords: coordination, precedent, costly communication, cheap talk

JEL classification numbers: C72, C92, D23, L23

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1 Introduction

Coordination problems are central in many economic and social contexts (see e.g., Arrow, 1974; Cooper, 1999; Lewis, 1969; Schelling, 1960; Skyrms, 2004). They are often a problem of common interest where people prefer to coordinate on the same (equilibrium) outcome, such as using the same technology or meeting at the same place for dinner with friends, but many possible outcomes exist. The difficulty of agreeing on the same outcome arises because individuals are facing strategic uncertainty, i.e., individuals' actions are not mutual knowledge (Van Huyck, Battalio and Beil, 1990). When individuals interact on a repeated basis they may resolve the strategic uncertainty by resorting to their shared experience (precedents) because this can create mutual expectations about what happens next (see e.g., Schelling, 1960; Lewis, 1969).[1]

However, groups (or individuals) are often confronted with a change in the group composition or the environment, examples include migration, interdisciplinary research groups, team formation in sport and organizations, patchwork families or firm mergers. This creates the potential that individuals with different precedents interact, which may lead to a coordination failure because their expectations are not concordant. This paper describes an experiment designed to investigate how such coordination failure can be avoided when two groups with different precedents are combined into a larger group. In particular, the experiment focuses on two questions. First, it explores whether pre-play communication is effective to resolve coordination failures stemming from different precedents in the newly-formed group. Communication of an intended action may influence behavior by creating mutual expectations and thus may provide some reassurance through the same mechanism as a precedent.[2] It is an empirical question how communication interacts with previously established precedents and whether individuals find communication effective for successful coordination, in particular when precedents and communicated intentions do not conform. Put differently, the question is: "Do words speak louder than precedents?" Second, unlike most of the previous literature, the experiment investigates whether subjects endogenously implement communication in the larger group and how this decision is affected by their experience.

[1] In the sense of Lewis (1969) a precedent represents mutual expectations about behavior due to the familiarity of a past solved coordination problem.

[2] There is some game theoretic literature, mostly focusing on inexperienced players, showing that communication through signaling intentions improves coordination in games with complete information and a common language (see Farrell, 1987, 1988; Rabin, 1990, 1994; Farrell and Rabin, 1996). Experimental evidence provides support for the insights from this literature (e.g., Cooper et al., 1992; Charness, 2000; Clark, Kay and Sefton, 2001; Duffy and Feltovich, 2002; Blume and Ortmann, 2007). See also Crawford (2007) and Ellingsen and Östling (2010), who use a level-k framework to address some shortcomings of the theoretical literature. Crawford (1998) provides a survey of the game theoretic literature on cheap talk and the early experimental studies.

Because the divergent precedents in the larger group increase the strategic uncertainty, subjects may naturally choose communication for achieving a good outcome. On the other hand, if one precedent is more salient, subjects may expect to coordinate on this precedent, therefore neglecting the strategic uncertainty and hoping to coordinate tacitly. Thus, given that communication is effective in overcoming coordination failure in a merged group, endogenizing communication allows us to investigate whether subjects are aware of the strategic uncertainty created by the merger and its economic consequences.

The experiment consists of two parts in which the underlying game is a variant of the minimum-effort game (Van Huyck, Battalio and Beil, 1990). In this game the lowest effort of a group member determines the group outcome. Thus, the group is vulnerable to a low effort and can only achieve a good and efficient performance when all group members exert the same high-effort level.[3] But choosing a high effort is costly because there is an uncertainty about other group members' effort. Therefore, precedents and communication may play an important role in reducing strategic uncertainty and avoiding coordination failure.

In the first part of the experiment, subjects play the minimum-effort game in small groups, which aids the development of a precedent. In the second part, two groups with different precedents are combined into one large group. That is, a group with a more efficient precedent is combined with a less-efficient group and both groups learn about the precedent of the other group. Subjects in the larger group then have to coordinate their activities in one of three different treatments: (i) without communication, (ii) with communication and (iii) with an endogenous and costly choice of communication. The communication is simultaneous and allows subjects to indicate their planned action before they make their decision in the underlying game. The first two treatments attempt to demonstrate that diverging precedents lead to coordination failure in the merged group and how it can be resolved. The main focus is on the treatment with an endogenous choice of communication. In this treatment subjects have to reveal their preference for communication through voting. The implementation of communication entails a small fee for each subject. If subjects vote against communication, this can be related to their unawareness of the strategic uncertainty and their underestimation of the possible benefits of communication.

Consistent with previous evidence (e.g., Knez and Camerer, 1994; Weber and Camerer, 2003), groups establish different precedents in the initial part, which causes conflicts when two

[3] Such an incentive structure resembles situations with a high interdependence of tasks which are often present in organizational as well as other economic or social settings, e.g., a social event that can only start when all guests have arrived (see Knez and Camerer, 1994; Milgrom and Roberts, 1992).

groups with a different precedent are combined in the second part. While the initial behavior in the merged group suggests that subjects from original groups with a more inefficient precedent on average adapt their behavior toward the more efficient precedent, the strategic uncertainty prevails and the majority of groups coordinate on a low-efficiency equilibrium. This emphasizes the difficulty of coordination when expectations are not concordant and shows the persistence of inefficient precedents in the merged groups.

The possibility to communicate leads in most cases to a coordination on the most efficient equilibrium, irrespective of subjects' previous experiences in the pre-merger group. This indicates that communication can overcome the persistence of inefficient precedents after the group merger. However, subjects tend to underestimate this persistence. If they have the choice to implement communication for a small cost only about 50% choose to do so. In particular, subjects with a history of more efficient coordination ("good" precedent) in the first part are prone to vote against communication. This suggests that subjects tend to underestimate coordination problems they have not yet experienced themselves. Groups who fail to implement communication, most likely fail to achieve efficient coordination. Unexpectedly, subjects in these groups do not revisit their voting choice. When in some sessions groups are offered the possibility to replicate the second part, subjects mostly stick to their initial vote and only 28% are willing to choose communication.

The findings suggest that the initial coordination success may dilute subjects' recognition of the coordination problem.[4] This may prevent them from reacting to changes in the environment, such as the formation of a new group, and may stop them from adapting their expectations accordingly. On the other hand, initial coordination failure ("bad" precedent) leads subjects to question their experience, which influences their willingness to implement a mechanism to avoid coordination failure. These results may be useful for the understanding of how groups learn from success or failure in order to facilitate a coordination process which is hard to distinguish in a non-controlled setting.

The remainder of the paper is organized as follows. Section 2 introduces the coordination game, reviews the related literature and describes the experimental design in detail. Section 3 presents the results from the first part of the experiment including the voting stage. Section

[4]There is some anecdotal evidence that prior success often causes firms to be inattentive to new problems or information. For example, some commentators reporting on the Airbus A380 crisis, speculated that the problems leading to this crisis originated in part from Airbus' prior success with its A320 model. See for instance, "The Airbus saga: Crossed wires and a multibillion-euro delay", International Herald Tribune, http://www.nytimes.com/2006/12/11/business/worldbusiness/11iht-airbus.3860198.html (accessed October 21, 2009)

4 discusses the coordination outcomes and the impact of communication and, finally, section 5 concludes.

2 The Coordination Game and Related Literature

In each session of the experiment subjects played a variant of the minimum-effort game (Van Huyck, Battalio and Beil, 1990). In this game n players (henceforth denoted as workers) have to simultaneously choose an effort level from an ordered set of integers, $e_i \in \{0, 10, 20, 30, 40\}$.[5] The payoff of worker i is a decreasing function of his own effort e_i and an increasing function of the minimum effort of all n workers and is given by (see also Table 1)

$$\pi_i = 200 - 5 * e_i + 8 * \min_{j \in n} (e_j) \tag{1}$$

It follows immediately from (1) that the strategies of the workers are strategic complements because higher efforts of the other workers provide an incentive to choose the same higher effort as well, because it increases the minimum. This implies that any coordinated effort level constitutes a Nash equilibrium and that the different equilibria are Pareto rankable due to a positive spillover, i.e., a higher effort level of all other workers yields a higher payoff for worker i. In particular, all workers choosing $e = 40$ is the Pareto-efficient equilibrium, which can be an appealing selection criterion (e.g., Harsanyi and Selten, 1988). But this is equally true for the lowest effort level (security) because the coordination outcome crucially depends on the minimum effort, i.e., a worker does not want to waste his effort. Hence, the minimum-effort game nicely illustrates the conflict between a shared interest resulting in a mutually beneficial outcome and the individual interest of security due to the strategic uncertainty about others' behavior.

That this strategic uncertainty leads to frequent coordination failure is convincingly demonstrated in the early literature on coordination experiments (Van Huyck, Battalio and Beil, 1990; Cooper et al., 1990). Coordination failure is usually understood as individuals' inability either to coordinate on one of the equilibria (miscoordination) or to achieve the Pareto-efficient equilibrium.[6] Throughout the paper, a coordination failure denotes the inability to coordinate on the Pareto-efficient equilibrium and a coordination failure is larger the less efficient the equilibrium is. The

[5] This particular version was introduced by Brandts and Cooper (2006*a*), who labeled it as corporate turnaround game.

[6] In many cases coordination failure is a sequential process, where groups initially miscoordinate and after some time coordinate on one of the less efficient equilibria.

Table 1: Minimum-Effort Game.

		Minimum effort in group				
		0	**10**	**20**	**30**	**40**
	0	200				
	10	150	230			
Effort by worker i	**20**	100	180	260		
	30	50	130	210	290	
	40	0	80	160	240	320

observations from these seminal papers initiated a vast research agenda mostly focusing on potential factors for efficient coordination (for extensive overviews see Ochs, 1995; Devetag and Ortmann, 2007). Two stylized facts are of particular relevance to this study and are reviewed in brief. Probably the most robust result is the strong negative impact of group size on efficient coordination. For small groups ($n = 2$) it seems to be easy to coordinate on the Pareto-efficient equilibrium (e.g., Van Huyck, Battalio and Beil, 1990). Groups with $n = 3$ constitute a knife-edge case that results in a tri-modal distribution of minimum effort, i.e., coordination on the Pareto-inferior and Pareto-efficient equilibrium as well as an equilibrium in between (e.g., Knez and Camerer, 1994, 2000; Weber, Camerer and Knez, 2004). Larger groups almost certainly lead to the Pareto-inferior equilibrium (e.g., Brandts and Cooper, 2006*a*,*b*; Cachon and Camerer, 1996; Van Huyck, Battalio and Beil, 1990). Thus, these results illustrate that each additional player makes the iterated thinking process about others' beliefs more complicated. This increases the strategic uncertainty because the larger the group, the higher the likelihood that at least one player chooses the secure option.

Other studies have attempted to investigate the necessary factors to facilitate efficient coordination, such as communication.[7] Adding costless pre-game communication does not change the equilibria of the underlying coordination game, but it may serve as an equilibrium selection device (Farrell, 1987, 1988; Rabin, 1990, 1994; Farrell and Rabin, 1996).[8] This is corroborated

[7]Other successful factors are, for instance, group competition (Bornstein, Gneezy and Nagel, 2002), increasing the saliency of the payoff-dominant equilibrium (e.g., Brandts and Cooper, 2006*a*; Goeree and Holt, 2005; Hamman, Rick and Weber, 2007), allowing subjects to choose their interaction partners (Riedl, Rohde and Strobel, 2009) or growing slowly small and efficient groups by sequentially adding new group members or by using entry barriers (Weber, 2006; Salmon and Weber, 2009). Engelmann and Normann (2010) present results from a standard minimum-effort game showing that larger groups can achieve efficient coordination. They explain their results with the share of Danes in a particular group, who seem to initially have more positive expectations and are more patient than non-Danes.

[8]Farrell and Rabin (1996) emphasize that messages need to be truthful to impact behavior and suggest two criteria. They note that, "a message that is both self-signaling and self-committing seems highly credible" (p. 112). A message is self-signaling if the sender only sends it, if it is true and it is self-committing when it creates an incentive for the sender to fulfill it, if it is believed by the receiver. However, Aumann (1990) points out, that cheap talk does not convey any informational content and should be ignored, if the sender has a (weak) preference over the receiver's

by many experiments, which show that costless and structured communication (cheap talk) helps to prevent coordination failure in small groups (e.g., Charness, 2000; Cooper et al., 1992; Clark, Kay and Sefton, 2001; Duffy and Feltovich, 2002, 2006) and in large groups (Blume and Ortmann, 2007).[9] While in these experiments communication was mandatory and free, Blume, Kriss and Weber (2011) look at endogenous and costly communication in a 2-player stag-hunt game (see also Kriss and Weber, 2011, for a multi-player extension). They find that subjects overwhelmingly resort to tacit coordination but often achieve the efficient outcome, indicating that foregoing communication might signal the intention to coordinate efficient as well. In Fehr (2010), I investigate endogenous and costly communication in a multi-player minimum-effort game. In contrast to the previously mentioned two papers on endogenous communication, there is no signaling possible and communication is irreversible. Despite these differences, subjects communicate too little, which can be related to their low level of impatience and cognitive ability, and which leads to coordination failure.

There are two papers close to this study. Knez and Camerer (1994) use a seven-action minimum-effort game to show that shared experience is not transferable to new environments.[10] They merge two small groups into one single larger group and, similar to the result of the control treatment in the present study, observe that the lowest equilibrium is obtained in 80% of the cases. Weber and Camerer (2003) replicated this result with an ingenious experimental design to demonstrate a potential pitfall of mergers due to conflicting corporate cultures. In their experiment subjects had to repeatedly describe subsets of unique pictures to another subject who has to correctly guess the pictures. Both are rewarded for how quickly they completed the task and over the course of the experiment they could develop a unique code – a proxy for a distinct corporate culture – allowing them to complete the task faster. This specific code is however useless when describing the picture to a subject from a different group. As in Knez and Camerer (1994) this leads to a coordination failure, i.e., longer task completion times after the merger. Both papers do not address the question of how coordination failures can be avoided when two groups with

action choice, i.e., when the message is not self-signaling. This is, for example, the case in the minimum-effort game used in this study. Recently, Crawford (2007) and Ellingsen and Östling (2010) used the level-k framework to relax the strong belief assumptions of the cheap talk theory and show that with reasonable assumption level-k thinking is able to explain the existing experimental evidence.

[9] Other forms of communication, for example, vertical free-form communication have a positive effect when subject managers communicate with subject workers in combination with monetary incentives (e.g., Brandts and Cooper, 2007), but not when a randomly chosen subject leader speaks to a large group (e.g., Weber et al., 2001).

[10] A few other papers investigate how subjects can transfer a precedent of efficient play from one game to another, but similar game (e.g., Devetag, 2005; Knez, 1998; Knez and Camerer, 2000; Van Huyck, Battalio and Beil, 1991). In these experiments the same group of subjects interact in both games.

different precedents are combined. They are also not concerned with subjects' awareness of the conflicts in the merged group and the subjects' willingness to implement communication in order to align their expectations.

2.1 Design and Procedure

The experiment consisted of two parts. The purpose of the first part was to let groups establish different precedents, which is an important precondition for the implementation of the different treatments in the second part. In the first part (pre-treatment part) workers played 10 periods of the described minimum-effort game (see Table 1). The group size was $n = 3$ and fixed throughout the pre-treatment part, since, as indicated above, it most likely results in groups with different precedents. At the end of each period workers received information about their payoff and the minimum effort in their group. The pre-treatment part was the same in each session.

After period 10 workers received new instructions and the second part was announced in which group k and group l would form a new group (k, l) of size $n = 6$ (post-treatment part). Each session consisted of four groups of size $n = 3$, which were ranked according to their minimum effort in the last (10th) period of the pre-treatment part.[11] For the post-treatment part, a group of rank $r \in \{1, 2\}$ was combined with a group of rank $r+2$. There are several things to note concerning this procedure. First of all, workers did not know the exact procedure but received information on the history of the new group members. That is, group k was informed about the 10th period minimum effort in group l and vice versa. Second, the ranking depended on the behavior within groups over the first part and the random formation of the groups in the beginning. Workers could not influence the ranking since they were unaware of other groups' behavior, the merger and its exact procedure. Hence, this procedure can be taken as exogenous. Finally, the procedure provides a high enough likelihood of combining two groups with different minimum effort, which is an important prerequisite for the research questions.

The post-treatment part consisted of three different treatments which will be explained in detail below. All treatments have in common the number of periods, the group size and the feedback after a period. This means that workers played another 10 periods of the minimum-effort game in the newly formed groups of $n = 6$ and that they were informed about their own payoff, the minimum effort as well as the distribution of individual efforts within their group. The latter departs from feedback given in the pre-treatment part. This departure aims at providing the most

[11] In case of a tie the rank was determined randomly.

convenient environment to make communication efficient. Observing the individual efforts of other group members could affect the beliefs about others' behavior and potentially tip the subsequent efforts in a particular direction. But, if anything, it would affect behavior in all treatments in the same way.[12]

The treatments differ with respect to the availability of communication (no communication or communication) and how communication is implemented (exogenous or endogenous). The BASE-treatment provided no opportunity to communicate before workers play the minimum-effort game. It serves as a benchmark for the ability of groups to coordinate efficiently without communication. The EX-treatment exogenously implemented a costless and non-binding pre-play communication stage. This means that workers could send a message (cheap talk) to all group members before actual play. The message space corresponded to the action space so that the meaning of the message was as clear as possible. In particular, workers were told that they could use these messages to indicate their intended play to the group (see also Blume and Ortmann, 2007). Before making their decision, workers received an overview of the distribution of messages, i.e., they could observe for each possible message the number of sent messages. Note that the only difference to BASE is the pre-play communication stage.

In the treatments with endogenous choice of communication, workers had to vote on the implementation of communication. Hence, depending on the voting decision they either faced the same environment as in the EX-treatment or the BASE-treatment. In the following the endogenous implementation of communication is denoted as END-treatment (pre-play communication) and the endogenous non-implementation is denoted as END-B treatment (no pre-play communication). The voting procedure was as follows. After learning about the reorganization and the minimum effort in the other group, each worker in a group had to cast a vote. In each group the voting decision of a randomly selected worker was then implemented. In this way the possibility to signal intentions is minimized since the group does not know how many members think that they can coordinate efficiently with or without communication. In addition, we can view the choice of the institution (communication or not) as exogenous. Although the vote represents the preferences of the chosen worker the implementation is a randomization over all voters. The crucial feature of the implementation of communication is the small cost c for the communication stage, which was set to $c = 20$ per period.[13] If workers anticipate the downward spiral of effort and the resulting

[12] In some control sessions not reported here, subjects did not receive the distribution of individual efforts. But the results do not differ to the case when they did learn the distribution and are available upon request.

[13] Note that this amount is smaller than the gain from a coordinated increase in effort by one level, e.g., from 10

coordination failure they should be willing to incur a small cost for communication in order to increase the likelihood of efficient coordination.[14] If there were no cost, workers would just be indifferent between voting for or against communication, which would not allow to discriminate between the workers' anticipation of coordination failure and a pure preference for communication.

In some of the sessions with an endogenous choice of communication (END and END-B) workers were surprised with a restart after the second part.[15] This third part was a replication of the second part, i.e., the group composition did not change, workers had to vote and play the game depending on the voting outcome. The only difference was that workers played only 5 instead of 10 periods. The purpose was to investigate whether groups in END-B corrected the voting decision they had made in the second part.

The experiments were run in the experimental lab at the Technical University Berlin using the software toolkit z-tree (Fischbacher, 2007). Subjects were recruited with ORSEE (Greiner, 2004). The sessions were conducted in 2008 and 2009 and in total 204 students (128 male and 76 female) from various fields of study participated in this experiment. Subjects received the instructions for the relevant part of the experiment, i.e., they were not aware of the second part and were also not aware that behavior in the pre-treatment part affects group composition in the second part. The instructions were framed as a working task to ease subjects understanding but avoided the use of connoted terms like bonuses, cooperation, deadlines, etc. Before the beginning of a part, subjects were required to answer several questions regarding the payoff function and procedural details. The experiment started only after all subjects had answered all questions correctly. After the experiment, subjects had to complete a socio-economic questionnaire and in END and END-B they had to answer questions related to communication. At the end of a session 4 out of 10 periods from each part were randomly and publicly selected to determine the earnings of the subjects.[16] Subjects were paid in private the sum of DM earned in the selected periods plus

to 20 hours.

[14]In other words, workers should vote for communication as long as the cost c is smaller than the expected payoff gain. Contrary, workers who do not anticipate or expect coordination difficulties, need not vote for communication. In this case they expect no change in behavior or even expect that higher effort levels can be reached without communication, i.e. the communication cost c is always larger than the expected payoff gain. The latter case is similar to a forward-induction argument, where no money will be burned and the Pareto-efficient equilibrium will be played. However, such a reasoning requires some sophistication and a strong belief in the rationality of others and if it is true one would observe efficient coordination with and without communication. There is no evidence for this argument.

[15]Subjects were recruited for a longer time span than needed and were told that the experiment had been finished ahead of time so that we would still have time left to repeat the second part. It was made clear that this part was definitely the last one and that afterwards they would receive their earnings from all three parts.

[16]In case there was a third part (END and END-B only) subjects received the payoffs of two randomly selected periods of the third part on top of the earnings from the first two parts.

Table 2: Distribution of Effort and Minimum Effort in Period 1 and Period 10.

	Individual effort		**Minimum effort**	
	period 1	period 10	period 1	period 10
0	6%	30%	16%	35%
10	8%	13%	19%	12%
20	31%	26%	52%	28%
30	17%	10%	9%	6%
40	38%	21%	4%	19%
median	30	20	20	20
mean	27.2	17.9	16.4	16.2

a show up fee of 600 DM. The conversion rate was 200 DM = 1 Euro. A session lasted about one hour and subjects earned on average €12.50.

3 Precedents and Voting on Communication

3.1 Establishing Different Precedents

The goal of the pre-treatment part was to establish a variety of precedents by period 10. In particular groups should establish histories of coordination failure and success, which are a crucial precondition for the second part. For the analysis the data of all sessions is pooled, since the experimental conditions are the same in each session. Table 2 provides an overview of the distribution of effort and minimum effort in period 1 and period 10.

As one can observe, the vast majority of effort choices in the first period are above 10 with a median choice of 30. In period 10 there is a substantially increased share of zero choices and the median effort dropped to 20, resulting in a tri-modal choice pattern. The important result is, however, the minimum effort in period 10 and the distribution of minimum effort is displayed in column 3 and 4 in Table 2. About 19% of the groups managed to coordinate on the Pareto-efficient equilibrium while 35% of the groups ended in the inefficient equilibrium (for similar results see Knez and Camerer, 1994).

The trend over time is nicely illustrated in Figure 1. The left panel shows the average effort and average minimum effort over the first 10 periods. There is a clear downward trend for effort levels. Comparing the average effort level of groups in period 1 and period 10 yields a significant difference (Wilcoxon signed-rank test, $z = 4.811$, $p < 0.01$). There is also a strong path dependency

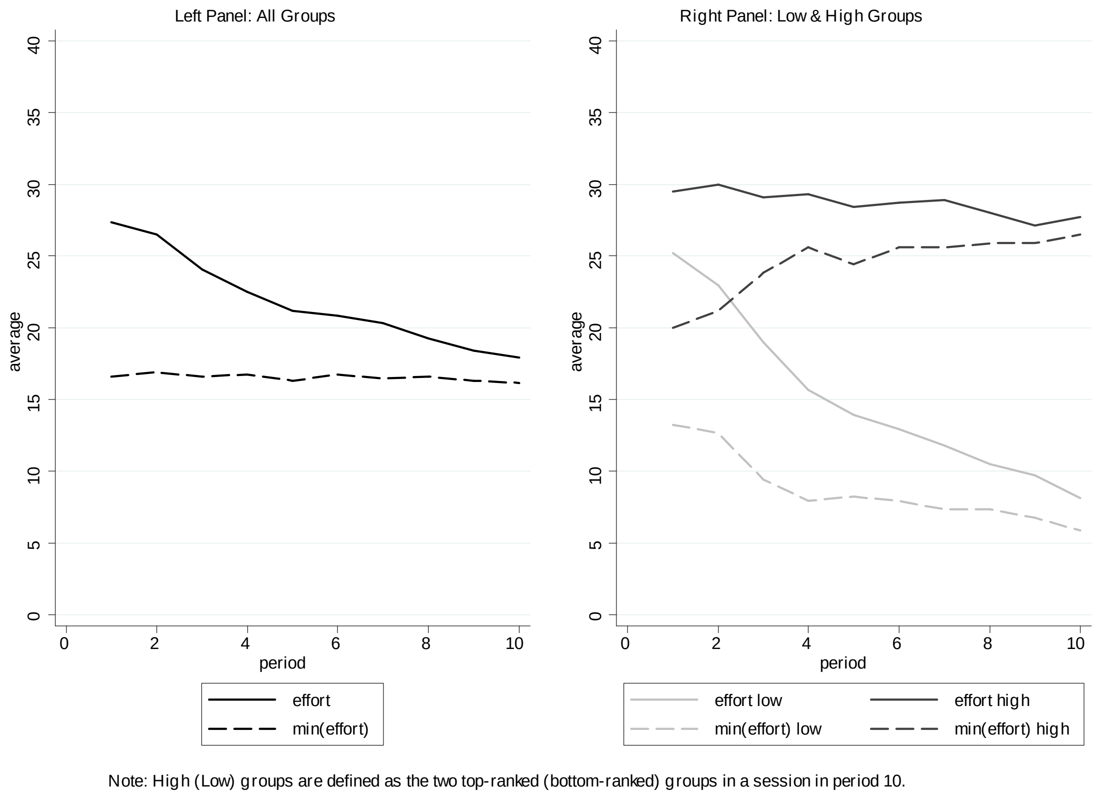

Figure 1: Pre-treatment Part: Average Effort Level and Minimum over Time.

for the minimum effort. For 34 out of the 68 groups the minimum is the same in period 1 and period 10.

Observation 1 *There is a considerable variation in minimum effort by period 10. Some groups establish a history of coordination success, while others experience a history of coordination failure.*

The ranking of groups according to their minimum effort in period 10 opens the possibility to distinguish between high-effort and low-effort groups, which will be useful in the latter analysis. Thus, the two top-ranked groups in a session are defined as high-effort groups and the two bottom-ranked groups as low-effort groups. Accordingly, workers in these groups are called high-effort workers and low-effort workers, respectively. The right panel of Figure 1 depicts the time trend for effort and minimum effort of low-effort and high-effort workers and groups, respectively. The figure provides three interesting observations. First, it shows that high-effort groups do not start with a high minimum from scratch but rather develop a norm of high effort. On the other hand, the minimum effort in low-effort groups decreases further and they never experience an efficient level of effort. They are stuck in a coordination failure from the beginning and this shared negative experience further reinforces the low-effort norm.

Second, the observed discrepancy of effort and minimum effort indicates that some groups are miscoordinated, i.e., they are not able to coordinate efforts on any of the equilibria. Groups are obviously coordinated when all workers choose the same effort level. The coordination rate is in each period lower for low-effort groups, i.e., they are more miscoordinated. In low-effort groups on average 63% of individual decisions are coordinated, while in high-effort groups the average is above 70%. The difference of the average coordination rate over all 10 periods between low- and high-effort groups is statistically significant (Mann-Whitney test, $z = 2.540$, $p < 0.015$). The lower rate of coordination, especially in low-effort groups, points to a sluggish adjustment process toward an equilibrium level as there are always single attempts to raise effort. These attempts are obviously more frequent in low-effort groups, but are doomed to failure because only a coordinated change can increase the minimum effort. Third, Figure 1 shows that the miscoordination declines over time. If groups have established a precedent, we should observe only little disagreement and, hence, a high rate of coordination within groups. Indeed, the individual coordination rate in period 10 is about 88% and does not differ between low- and high-effort workers ($t_{202} = 0.638$, $p > 0.52$). The minimum effort in period 10 is also highly correlated with the average effort over all previous periods (Spearman's $\rho = 0.86$, $p < 0.01$), as well as with effort in period 1 (Spearman's $\rho = 0.46$, $p < 0.01$). Hence, workers seem to agree on a precedent within the group and it remains to be tested how behavior responds to the merger of two groups.

3.2 Voting on Communication

Only in treatment END and END-B did subjects have the possibility to vote on communication. Of the 96 subjects in these two treatments, 49 (51.04%) voted for communication and 47 (48.96%) voted against communication. Low-effort workers are significantly more likely to vote for communication than high-effort workers ($\chi^2_{(1)} = 5.04$, $p = 0.025$). Looking at the relation of effort in period 10 and voting behavior provides a more detailed and convincing picture. The median effort in period 10 was 20 and of those workers with an effort of 20, only 38% voted for communication. The picture is not very different for workers with an effort level above the median. They voted for communication in only 36% of cases. That is in strong contrast to workers with an effort below the median who vote in 69% of cases for communication. Thus, voting is negatively related to effort in period 10 (Spearman's $\rho = -0.35$, $p < 0.01$). This result clearly emphasizes that workers with a previously low-effort level and, hence, those who experienced coordination failures are in favor of communication.

Result 1 *Only every second worker is willing to incur a small cost to implement communication. Larger coordination failures in the first part lead to a higher willingness to vote for communication.*

To provide further support for this result, Table 3 presents the results of OLS regressions (marginal effects from a probit regression yield similar results). The regressions allow to check how voting behavior depends on own experience as well as on the experience of the new group members. The model in column 1 confirms Result 1. The negative coefficient indicates that workers with a higher effort level in the first 10 periods have a lower likelihood of voting for communication. In other words, experiencing a larger coordination failure in the first part is related to a significantly higher likelihood of voting for communication. This result is robust for various controls (see columns 2–4). In particular, workers who expect more of their co-workers to vote for communication are also more likely to vote for communication.[17] For example, pro-communication voters expect that 74% of their co-workers will also vote for communication, whereas no-communication voters only expect 40% of their co-workers to vote for communication. This difference is highly significant at a 1%-significance level ($t_{94} = 5.85$). It is consistent with a (false) consensus effect. This is the tendency of people to expect own behavior also from others (Ross, Greene and House, 1977; Dawes, 1989).

Interestingly, neither the history of the new co-workers nor the initial willingness to provide high effort plays a role. Both coefficients indicate a positive impact but far from a significant level (see column 3 and 4). In particular, the insignificant coefficient for others' history suggests that workers completely neglect this piece of information. High-effort workers seem to be overly optimistic about low-effort workers' willingness to increase their effort levels, which goes hand in hand with their beliefs. They may falsely believe that the other workers also do not see the necessity of communication, which mistakenly leads them to expect high effort from all group members. Evidence from the post-experimental questionnaire support the idea that high-effort workers may fail to take the perspective of others and may assume that coordination is easy to achieve. Asked about their assessment of the difficulty of coordination on a scale of 1 (easy) to 5 (difficult), high-effort workers found it significantly more easy to coordinate on a common effort

[17] On average, workers expect 57% of their co-workers to vote for communication. In general, accuracy is low; 33% (14%) of expectations of communication advocates (opponents) are correct. These shares increase to 69% and 53%, respectively, if one allows for a deviation of one vote.

Table 3: Determinants of Voting Behavior.

dependent variable:	Vote			
	(1)	(2)	(3)	(4)
Avg. effort in periods 1 - 10	−0.015*** (0.004)	−0.011*** (0.004)	−0.011*** (0.004)	−0.011*** (0.004)
Expectations about others votes		0.147*** (0.024)	0.149*** (0.025)	0.149*** (0.025)
Period 10 minimum of others			0.003 (0.003)	
High effort in period 1 (d)				0.063 (0.097)
Constant	0.828*** (0.092)	0.333** (0.139)	0.282* (0.150)	0.290* (0.158)
R^2	0.12	0.34	0.34	0.34
N	96	96	96	96

* p<0.10, ** p<0.05, *** p<0.01
OLS regression (marginal effects from a probit regression yields similar results). Robust standard errors in parentheses. The variable "High effort in period 1" is a dummy which equals one if effort in period 1 was above 20 and below 30 in period 10. The variable "Period 10 minimum of others" is the information about the new group members before the voting stage.

level than low-effort workers ($t_{94} = 2.50$, $p < 0.015$).[18]

Further evidence for the optimism of high-effort workers is presented in Figure 2, which depicts the change in effort levels from period 10 to period 11 for high-effort and low-effort workers in treatment BASE and END-B, respectively.[19] There is virtually no difference between these two treatment and in both one can observe that high-effort workers do not lower efforts. In fact, comparing the individual efforts of high-effort workers in period 11 to the history of their new co-workers (low-effort workers) clearly shows that they neglect the information about others' behavior, confirming the regression results. About 55% of high-effort workers do not change their effort from period 10 to 11 and 20% even increase their effort.[20]

It seems that the initial optimism of high-effort workers is to some extent justified. Not surprisingly, low-effort workers significantly increase in both treatments their effort level (Wilcoxon

[18]The question was: What do you think, how difficult is it to coordinate on the same effort level in your group? The scale was from 1 (easy) to 5 (difficult).

[19]The figure only considers the two treatments without communication, because communication influences the expectations and, hence, the chosen effort in period 11.

[20]These observations are also supported by answers of high-effort workers in the post-experimental questionnaire. Most statements read as follows: [..] it is obvious what the best for us would be and I cannot understand why one would choose 0,[..].

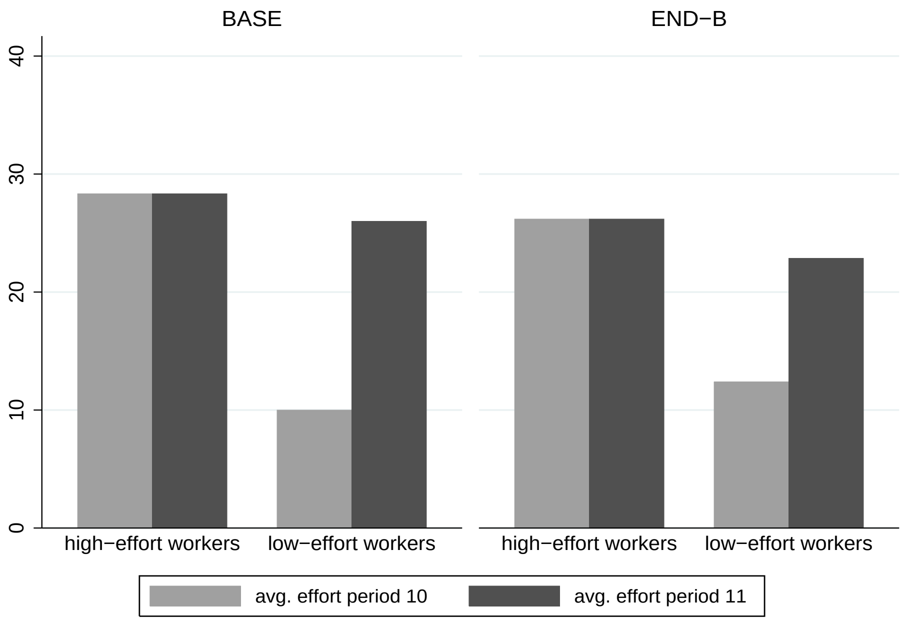

Figure 2: Period 11 Behavior of High-effort and Low-effort Workers in BASE and END-B.

signed-rank test, $z = 4.224$, $p < 0.01$ for BASE and $z = 3.117$, $p < 0.01$ for END-B).[21] When increasing their effort, low-effort workers orient themselves at their new co-workers' history. The average effort of low-effort workers is smaller than the average minimum effort of high-effort groups in period 10, but the difference is not significant statistically (Wilcoxon signed-rank test, $z = 1.076$, $p > 0.28$ (BASE) and $z = 0.949$, $p > 0.34$ (END-B)). The lower average effort level indicates that low-effort workers are still cautious. This can also be seen by the level of effort increase of low-effort workers. About 20% of low-effort workers do not increase their effort at all and about 30% only by one level, e.g., from 10 to 20. There is no difference in the levels of increase between BASE and END-B (Mann-Whitney test, $z = 1.442$, $p > 0.14$), which is evidence that the signaling value of implementing no communication is negligible in the short run.

[21] The increase in effort of low-effort workers in the BASE and END-B treatment indicates that the mere effect of introducing high-effort workers in a group of workers who experienced a history of coordination failure positively affects their expectations and effort levels. This observation is in strong contrast to previous minimum-effort game experiments which started off with 6-person groups. These experiments have shown that in more than 70 percent of cases 6-person groups end up with the most inefficient equilibrium in the first period of play (e.g., Camerer, 2003, provides an overview of results from various minimum-effort games (Chapter 7)). In BASE, however, only 10% of groups ended in the most inefficient equilibrium in period 11.

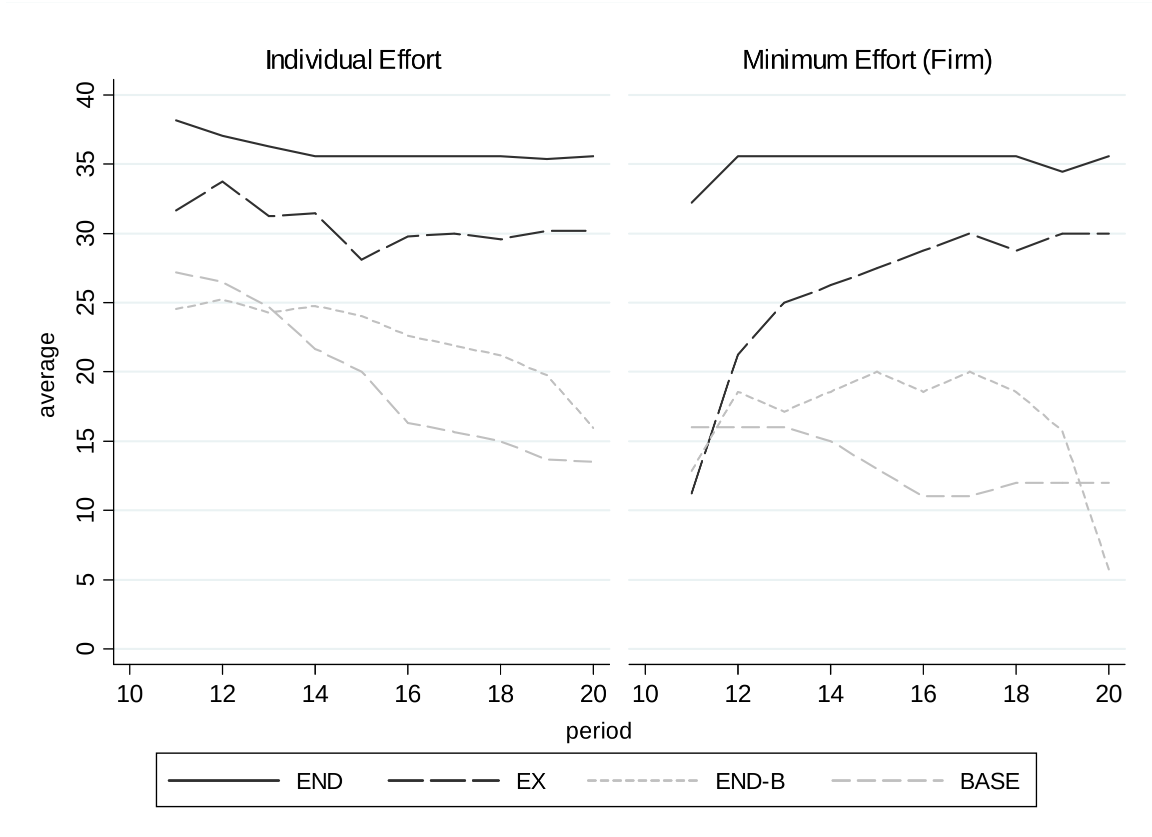

Figure 3: Post-treatment: Evolution of Effort and Minimum Effort.

4 Coordination in Large Groups

4.1 Persistent Precedents and the Need of Communication

This section sheds light on the necessity of communication and analyzes the coordination outcomes in the four treatments. Figure 3 shows the evolution of average individual effort and minimum effort from period 11 to 20 for all four treatments. The analysis focuses first on treatment BASE and EX. Looking at the left panel of Figure 3 reveals that in the first period after the merger of groups the average effort level is higher in EX than in BASE ($t_{106} = 2.0$, $p < 0.048$). Also, in EX there is a transient increase in effort in the beginning and the average effort level stabilizes after period 15. In BASE, however, effort levels quickly decline over time.

In contrast to effort in period 11 the observed average minimum effort – shown in the right panel of Figure 3 – is not significantly different in period 11 for EX and BASE (Mann-Whitney test, $z = 0.922$, $p > 0.35$). Nevertheless, the figure provides a clear picture of the evolution of the minimum effort in BASE and EX. Despite the fact that the minimum effort in the first two periods is amazingly high in BASE, we can see a substantial decrease in the long run. In contrast, communication has the expected positive impact with a sharp increase in minimum effort in EX in the early periods.

As the average level suggests, not all groups manage to achieve coordination on the efficient equilibrium. Looking at the communication patterns reveals possible reasons for unsuccessful groups. First of all, about 31% of the messages in period 11 in EX were not truthful and most of these untruthful messages (80%) were self-signaling, i.e., the message was higher than the chosen action. Second, there was no unique message profile in period 11, which indicates the presence of a second coordination problem at least in the beginning. Third, in successful (efficiently coordinated) groups the fraction of truthful messages rose from about 69% in period 11 to 87% in period 12, while the fraction dropped from 66% to 50% for unsuccessful groups. Evidently, unsuccessful groups were never able to coordinate their messages, that is, the message profile in these groups was never unique and, thus, the meaning of their messages degenerated over time. In almost all successful groups message profiles simultaneously became unique and truthful. Another interesting fact to note is that high-effort workers tended to send higher messages than low-effort workers in period 11 (Mann-Whitney test, $z = 1.935$, $p = 0.053$). This is in line with the earlier observation that high-effort workers do not decrease effort in period 11. These observations are summarized in the following result.

Result 2 *Inefficient precedents are persistent in merged groups without communication, whereas most groups can escape from coordination failure when communication is available.*

Figure 3 also shows the evolution of average effort and minimum effort in END and END-B. In END-B we observe a similar pattern as in BASE for both effort and minimum effort. There is a downward trend of effort levels, albeit effort remains at a higher level than in BASE in the last 5 periods. This difference between BASE and END-B is not statistically significant (Mann-Whitney test, $z = 0.837$, $p > 0.40$). Minimum effort in END-B is higher than in BASE but eventually drops in the last periods. Again, the difference is not significant statistically (Mann-Whitney test, $z = 0.516$, $p > 0.60$). Individual effort in END is remarkably high from the beginning and in all periods higher than in EX. In END the minimum effort is in contrast to EX even from the first period high and remains at this level. A Mann-Whitney test of averages over 10 periods yields a significant difference between END and EX ($z = 2.472$, $p < 0.02$).

Result 3 *The endogenous implementation of communication leads to a sustainable increase in effort and minimum effort. Groups without communication largely fail to achieve efficient coordination.*

For statistical support of Result 2 and Result 3 the following regression model for individual and group-level data is estimated:

$$\text{y}_{it} = \beta\text{Treat*Period11-15}_i + \delta\text{Treat*Period16-20}_i + \gamma\text{History}_i + \alpha_i + \varepsilon_{it} \quad (2)$$

The dependent variable y_{it} is either the individual effort or the minimum effort in a group in period 11 to 20. The variables "Treat*Period11-15" and "Treat*Period16-20" are the interaction variables for the treatment dummies and a dummy for periods 11–15 and, respectively, for periods 16–20. Periods 16–20 in BASE serve as a benchmark and the interaction terms measure the treatment effects over time without imposing a linear trend. Additionally, the variable "History" controls for the different histories of the merged groups and is the average (minimum) effort in the two original groups in period 10. To account for possible correlations within groups the regression clusters the error term on the group level. Table 4 presents the results from random-effect GLS regressions and the Appendix A provides several robustness checks in support of the GLS results.

The results provide a clear picture and support the previous observations. Communication clearly leads to higher effort levels (see column 1) irrespective of its implementation – exogenous or endogenous. Figure 3 and the regression coefficients for "Treat*Period16-20" show that effort levels are lower in EX than in END, but the difference is not significant.[22] Apart from that, the regression also documents the downward trend of effort in BASE and END-B. The coefficient for BASE*Period 11-15, i.e., the difference to BASE*Period16-20 is positive and significant. The coefficient for END-B*Period 11-15 is positive and weakly significant, whereas the coefficient for END-B*Period 16-20 is not significant, indicating that there is no difference to BASE*Period16-20 in these periods of the second part. The regression also does not reveal a significant difference between BASE and END-B in periods 11 to 15, indicating that the non-implementation of communication does not provide a signal.[23]

[22]The difference can be tested by recoding the interaction dummy for END*Period 16-20 by taking the absolute difference of EX*Period 16-20 and END*Period 16-20 and rerunning the regression. The resulting coefficient for END*Period 16-20 gives an estimate of the difference between END and EX for periods 16 to 20, which turns out to be insignificant ($z = 0.64$, $p = 0.52$).

[23]Again, this can be assessed by recoding the relevant interaction dummy for END-B*Period 11-15 as described in Footnote 22 and rerunning the regression. The resulting coefficient is insignificant ($z = 0.12$, $p = 0.9$).

Table 4: Regressions on Effort and Minimum Effort after the Reorganization.

dependent variable:	**Individual effort**		**Minimum effort (group)**	
	(coefficient)	*(std. error)*	*(coefficient)*	*(std. error)*
BASE * Period 11-15	0.917***	0.247	0.360	0.225
EX * Period 11-15	1.852***	0.522	1.259*	0.705
END * Period 11-15	2.279***	0.530	2.460***	0.603
END-B * Period 11-15	0.966*	0.539	0.667	0.614
EX * Period 16-20	1.722***	0.657	1.984***	0.711
END * Period 16-20	2.179***	0.590	2.504***	0.604
END-B * Period 16-20	0.538	0.678	0.496	0.667
Effort Period 10	0.063***	0.023		
Min. Effort Period 10			0.054**	0.025
Constant	0.276	0.561	0.197	0.575
p	0.00		0.00	
R^2	0.31		0.34	
N	2040		340	

* p<0.10, ** p<0.05, *** p<0.01
GLS regressions with random effects. Robust standard errors are reported for clustering on group level. The variable "(Min.) Effort Period 10" is a variable for the average (minimum) effort in the reorganized group in period 10. For robustness checks see Appendix.

At the group level (column 3) only the coefficients for EX and END are significant, indicating the strong impact of communication and the resulting higher minimum effort in these groups.[24] However, as is apparent from Figure 3, exogenous communication does not lead to an immediate increase in efficiency. The results suggest that communication helps to gradually induce the shared expectations of high effort among the workers, pointing to the persistence of the precedents established in the pre-treatment part. This is supported by the low coordination rate of 21% in period 11, which only gradually increases to 66% in period 14. This is in strong contrast to endogenous communication where the coordination rate is already 72% in period 11 and groups immediately achieve the efficient outcome. In both treatments we observe on average the same messages ($t_{100} = 1.216$, $p > 0.22$), but obviously they map differently into effort. In both treatments the initial effort levels are well above 30, nevertheless effort levels are significantly higher in END than in EX ($t_{82} = 3.25$, $p < 0.01$). It is also interesting to note that messages in period 11 are more truthful in END (78%) than in EX (69%) and that the average difference between

[24] Again, recoding of the interaction dummy for END*Period 16-20 as in Footnote 22 to compare END to END-B yields a significant coefficient for END*Period 16-20 ($z = 3.08$, $p < 0.01$).

messages and effort is higher in EX than in END ($t_{100} = 2.15$, $p < 0.035$), i.e., they tend to be more self-signaling in EX. It seems that workers have more confidence in the messages when the decision about communication is at their discretion, that is, messages are then more truthful and the coordination problem associated with communication is mitigated.[25] In sum, the presented regression results largely support Result 2 and 3.

To implement communication in END workers have to incur a small cost. Does this investment pay off? The answer is an unqualified yes. The average net payoff over periods 11 to 20 of workers in END is 281 DM and is substantially higher than workers' net payoffs in EX (254 DM) and, of course, in END-B (220 DM) and BASE (210 DM). Despite the cost of communication workers in END earn on average significantly more than workers in other treatments.[26]

4.2 The Attribution of Coordination Success

Although communication pays off, one could ask whether communication is necessary at all. It could readily be that for groups with a good history in the pre-treatment part communication is redundant. Take, for instance, a high-effort group with a 10th period minimum of 40 and a low-effort group with a 10th period minimum of 20. The merged groups may adopt the "good" precedent, because low-effort workers may adapt their behavior in period 11 toward the observed 10th period minimum of the high-effort group. This conjecture can be tested by considering merged groups in BASE and END-B involving high-effort groups with a history of at least 30.[27] The difference between the average minimum in period 10 and the average minimum over periods 11 to 20 is not significant (Wilcoxon signed-rank test, $z = 0.533$, $p > 0.59$). Hence, there is no conclusive evidence for this conjecture.

In some sessions in END and END-B workers had the chance to reconsider their initial

[25] A worker who is willing to implement communication also signals his intention to increase his effort by at least one level in order to outweigh the additional cost arising from communication. Such a forward induction argument would amplify the signaling of intended play through the messages. However, workers in END only observe the voting decision of one randomly chosen co-worker making it impossible to deduce other group members' intentions. In general, the experimental evidence for forward induction is not conclusive. Forward induction could interact with other selection principles such as loss avoidance (Cachon and Camerer, 1996) reducing its impact or it could cause no effect at all (e.g., Cooper et al., 1993). Others find supportive evidence for forward induction (e.g., Brandts and Holt, 1992; Van Huyck, Battalio and Beil, 1993).

[26] Comparing individual average payoffs of workers in END with workers in the other treatments yield the following p-values, BASE ($t_{112} = 7.59$, $p < 0.01$), END-B ($t_{112} = 6.29$, $p < 0.01$) and EX ($t_{112} = 2.46$, $p = 0.015$). On the group level average payoffs in END are also significantly larger than in BASE (Mann-Whitney test, $z = 2.156$, $p < 0.05$) and in END-B (Mann-Whitney test, $z = 1.793$, $p < 0.075$).

[27] Restricting the test to only those groups from BASE and END-B results in 9 independent observations. Note that the corresponding 10th period minimum effort of the low-effort group is always lower than that of the high-effort group. Subsequently, the average minimum effort in period 10 of a merged group ranges from 20 to 30.

voting decision, i.e., they were confronted with a repetition of the second part. In total, only 12 out of 60 workers (20%) voted for communication in period 21. Certainly, for workers in successful groups there is no need to pay the cost for communication as they already agreed on the Pareto-efficient outcome. Nevertheless, in those groups 17% of workers were willing to incur the cost of communication. Surprisingly, the fraction is not much higher in unsuccessful groups, where only 28% were willing to implement communication. Subsequently, none of the unsuccessful groups managed to implement communication and they could not escape the coordination failure.

Although communication helps to achieve efficient coordination it is not clear that workers in END attribute the success of coordination to communication. The post-experimental questionnaire sheds some light on this issue and asked workers (i) to what factors they attribute their coordination success (failure), (ii) to rate the (voting) decision of the randomly determined worker, and (iii) to assess the importance of communication.[28] When workers in successful groups in the second part, i.e., groups with an average minimum effort larger than 30 over 10 periods, are asked about the reason for their success, they overwhelmingly (58%) attribute it to reasons other than sending messages. It is somewhat striking that high-effort workers, those who experienced no coordination failure in the pre-treatment part, are more likely not to attribute a possible coordination success to the possibility of sending messages ($\chi^2_{(1)} = 7.42$, $p < 0.01$).[29] Thus, high-effort workers experienced an attribution error. They mistakenly attributed good outcomes to the task when in fact the coordination success occurred because of communication which made the task easy. The same mistake can be seen when workers have to judge the implementation of the communication decision on a scale of 1 (negative) to 5 (positive). Remember, the implementation was a random process over votes. Nevertheless, high-effort workers in successful groups did not agree on the implementation decision with an average rating of 2.8. In contrast, the average rating of low-effort workers was 4.2. This difference is statistically significant ($t_{52} = 3.5$, $p < 0.01$). Hence, high-effort workers blame the decision maker for her decision more often than low-effort workers. When workers have to rate the importance of messages for coordination success, again on a scale of 1 (negative) to 5 (positive), we observe the same picture. For high-effort workers messages were less important (average rating of 2.8) for coordination success than for low-effort workers (average rating of 3.96),

[28]The questions were the following. Blame: How do you judge the decision of the team leader? Was his/her decision right or not? (Scale: 1 (not right) – 5 (right)) Importance: What do you think, was the possibility to send messages to your co-workers important? (Scale: 1 (not important) – 5 (important)) Attribution: What do you think, why did (not) your group agree on an effort level of 40? (task was easy, enough trust, willingness to risk, possibility of messages)

[29]Note that the four possible answers to the question on attribution are collapsed into only two categories - possibilities of messages and other reasons.

which is statistically significant ($t_{52} = 2.8$, $p < 0.01$).

In sum, the questionnaire provides further support for the result that high-effort workers in particular do not anticipate the frictions from the merger, especially when communication was possible. The reason may be that they had never experienced a coordination failure and they did not attribute their coordination success in the second part to communication. Of course, this reinforces their beliefs that communication is not important. Though, if we look at the results of the two treatments without communication (BASE and END-B) we see that such reinforced beliefs are wrong. Only communication helps to mitigate the frictions and coordinate efficiently.

5 Discussion and Conclusion

This experiment presented evidence for the difficulty of coordination when initially small groups have to coordinate their activities in a larger and newly-formed group. This is mainly because "bad" precedents are persistent such that they transfer from a small pre-merger group to the larger group. One possible solution to weaken this persistence is to use communication as a means to adapt the expectations of all workers in the merged group. Indeed, communication leads to higher effort and a more efficient outcome. The most important question is, however, whether workers realize the potential persistence of precedents and endogenously choose communication in order to prevent coordination failures. The experimental results, though, clearly show that only every second worker is willing to incur a small cost in order to implement communication. In particular, the results show that this willingness is higher when workers experienced a larger coordination failure before the merger. Groups which fail to implement communication eventually find themselves in an inefficient equilibrium, whereas groups with communication achieve the most efficient outcome. Thereby, workers who neglect the difficulty of coordination forgo a significant amount of money if communication is not introduced.

Anecdotal evidence on merger failure (e.g., DaimlerChrysler, AOL-TimeWarner) or the failure to integrate specialized production steps into a final product are suggestive that organizations underestimate the difficulty of coordination. Heath and Staudenmayer (2000) coined the term "coordination neglect" to account for this tendency. For example, many technology firms establish very innovative and independent research labs, but often fail to turn their projects into profitable products because of mis-coordination between the researchers and other departments (see also Christensen, 1997). Another example is the Airbus A380 crisis, which was marked by delays in the development and production process.[30] An important reason for the crisis was the use of different construction softwares, which impeded the communication between the different assembly plants. The Airbus management responded to the problem with an increase of manpower in the final assembly stage, which presumably further amplified the coordination problem. The results of the BASE-treatment might be interpreted as evidence that adding workers to an existing group can complicate matters instead of solving it. The failure to use a standardized software eventually lead to problems with the wiring and accumulated considerable losses.[31]

The results suggests that experiencing and recognizing failure may be key to avoiding these kinds of coordination failure. While mostly high-effort workers fail to vote for communication because they disregard the previous experience of their new co-workers and do not anticipate the coordination problem in the new group, the result on voting (Result 1) is intriguing as it provides evidence for learning from failure. It suggests that initial failure is needed in order to anticipate possible coordination problems. Indeed, the bad coordination experience in the first part results in low payoffs and, thus, makes it more likely that workers reconsider their past experience. In contrast, success seems to reinforce the pre-merger behavior of high-effort workers. They seem to transfer their view of the task onto low-effort workers and may mistakenly believe that coordination is easy to achieve.

[30] See for instance, "The Airbus saga: Crossed wires and a multibillion-euro delay", International Herald Tribune, http://www.nytimes.com/2006/12/11/business/worldbusiness/11iht-airbus.3860198.html (accessed October 21, 2009)

[31] According to EADS, the parent company of Airbus, the losses accumulated to almost €5 billion, and the A380 is expected to break even at 420 sold aircrafts as opposed to the initially proposed 270, see "The A380 programme", http://www.eads.com/xml/content/OF00000000400004/0/74/41485740.pdf (accessed January 31, 2010)

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APPENDIX - Not for Publication

A Robustness Checks

This section provides several robustness checks to support the regression results in Table 4. A natural candidate for the estimation would be an ordered probit model, since effort and minimum effort are discrete variables which can be ordered on a categorical scale. The results from the probit specification, however, are only reported as additional evidence to the GLS regressions in the main text, which are more powerful and allow more flexibility. The individual-level data includes repeated observations of the same worker, such that decisions are possibly correlated within and across subjects in a group. Following the approach of Brandts and Cooper (2006*a*) to account for a possible trade-off between minimizing type 1 errors and type 2 errors, Table 5 reports the results from a conservative clustering approach on the group level as well as random-effects specifications for the 3-level model when estimating individual data. The latter specification utilizes the gllamm program developed by Rabe-Hesketh, Skrondal and Pickles (2004). The results are reported in Table 5 and are in support of Result 2 and Result 3.

The second part of the experiment (period 11 - 20) constitutes an exogenous implementation of a particular treatment. This applies for all treatments in the experiment including END and END-B since the implementation of communication in these two treatments is a random draw over votes. This fact can be exploited to use differences-in-differences method (DD) to estimate treatment effects. This approach measures the treatment effect by comparing treatment groups to control groups and contemporaneously controlling for the behavior before the receipt of the treatment (pre-treatment part). The differences-in-differences estimator measures then the treatment effect in addition to the differences, which possibly existed before the exogenous implementation of the treatment. However, this requires the implicit assumption that the trend for the dependent variable is the same in treatment and control groups. This requirement is fulfilled in the data and thus one can estimate the following model:

$$\begin{aligned} \text{y}_{it} = \beta_0 + \beta_1\text{Low} + \beta_2\text{EX} + \beta_3\text{END} + \beta_4\text{END-B} + \beta_5\text{Part2} + \\ \beta_6\text{Part2} * \text{EX} + \beta_7\text{Part2} * \text{END} + \beta_8\text{Part2} * \text{END-B} + \alpha_i + \varepsilon_{it} \end{aligned}$$

where y_{it} is either individual effort or minimum effort, "Low" is a dummy variable indicating low-effort groups in the periods before the reorganization and "Part2" is a dummy variable for the

Table 5: Ordered Probit Regressions on Effort and Minimum Effort.

dependent variable:	**Individual effort**		**Minimum effort (group)**	
	(clustering)	*(nested random-effects)*	*(clustering)*	*(random effects)*
BASE * Period 11-15	0.585***	0.887***	0.378	0.603**
	(0.195)	(0.093)	(0.237)	(0.248)
EX * Period 11-15	1.496***	1.111***	0.984**	0.508
	(0.444)	(0.155)	(0.502)	(0.319)
END * Period 11-15	2.307***	2.706***	2.203***	4.010***
	(0.760)	(0.216)	(0.683)	(0.498)
END-B * Period 11-15	0.700**	0.539***	0.490	0.219
	(0.356)	(0.115)	(0.436)	(0.306)
EX * Period 16-20	1.487***	0.657***	1.613***	3.084***
	(0.572)	(0.157)	(0.606)	(0.525)
END * Period 16-20	2.159***	0.590***	2.300***	4.404***
	(0.381)	(0.215)	(0.737)	(0.558)
END-B * Period 16-20	0.381	0.678	0.366	−0.041
	(0.503)	(0.124)	(0.511)	(0.313)
Effort Period 10	0.064***	0.104***		
	(0.021)	(0.005)		
Minimum effort Period 10			0.049**	0.044***
			(0.021)	(0.012)
LL	-2227.82	-1498.04	-391.62	-243.14
pseudo R^2	0.16		0.16	
N	2040		340	

* p<0.10, ** p<0.05, *** p<0.01
Ordered probit regression with clustering and random effects. Robust standard errors are reported for clustering on group level and are in parentheses. The variable "(Min.) Effort Period 10" is a variable for the average (minimum) effort in the reorganized group in period 10. The nested random-effects model was estimated using gllamm.

periods after the reorganization. The model also includes dummy variables for the treatments EX, END and END-B as well as interaction terms of those treatment variables with the Part2 dummy. Note that the specification uses a panel structure with an individual random effect α_i. Again, on the individual level possible correlations among decisions of workers in the same group are taken into account by clustering on the group level. The coefficients of interest are the coefficients of the interactions terms β_6, β_7 and β_8. The results from the differences-in-differences regression are reported in Table 6 and support the results from the GLS regressions in Table 4 and ordered probit regressions in Table 5.

Table 6: GLS Regressions (DD) on Effort and Minimum Effort.

dependent variable:	**Individual effort**		**Minimum effort (group)**	
	(1)	(2)	(3)	(4)
EX	−0.251	−0.251	−0.316	−0.313
	(0.239)	(0.239)	(0.429)	(0.350)
END	−0.419	−0.419	−0.467	−0.464
	(0.311)	(0.311)	(0.409)	(0.343)
END-B	0.034	0.034	0.038	0.038
	(0.305)	(0.305)	(0.396)	(0.339)
Part 2	−0.405	−0.405	−0.523	−0.521
	(0.328)	(0.328)	(0.341)	(0.342)
Part 2 * EX	1.370**	1.370**	1.570**	1.569**
	(0.562)	(0.562)	(0.619)	(0.619)
Part 2 * END	2.079***	2.079***	2.603***	2.603***
	(0.550)	(0.550)	(0.543)	(0.543)
Part 2 * END-B	0.267	0.267	0.212	0.216
	(0.492)	(0.492)	(0.463)	(0.464)
Low-effort Group		−0.739***		−0.760***
		(0.099)		(0.231)
Constant	2.347***	2.716***	1.851***	2.229***
	(0.131)	(0.141)	(0.249)	(0.241)
p	0.00	0.00	0.00	0.00
R^2	0.13	0.19	0.20	0.26
N	4080		1360	

* p<0.10, ** p<0.05, *** p<0.01
GLS regressions with random effects. Robust standard errors are reported for clustering on group level and are in parentheses.

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