Exp Econ (2007) 10: 269–284
DOI 10.1007/s10683-007-9182-0

Leadership and overcoming coordination failure with asymmetric costs

Jordi Brandts · David J. Cooper · Enrique Fatas

Published online: 9 August 2007

Abstract We study how the heterogeneity of agents affects the extent to which changes in financial incentives can pull a group out of a situation of coordination failure. We focus on the connections between cost asymmetries and leadership. Experimental subjects interact in groups of four in a series of weak-link games. The treatment variable is the distribution of high and low effort cost across subjects. We present data for one, two and three low-cost subjects as well as control sessions with symmetric costs. The overall pattern of coordination improvement is common across treatments. Early coordination improvements depend on the distribution of high and low effort costs across subjects, but these differences disappear with time. We find that initial leadership in overcoming coordination failure is not driven by low-cost subjects but by subjects with the most common cost type. This conformity effect may be due to a kind of group identity or to the cognitive simplicity of acting with identical others.

Keywords Experiments · Coordination · Organizational change · Heterogeneous agents · Leadership

JEL Classification C70 · C90 · D63 · D64

J. Brandts (✉)
Institut d'Anàlisi Econòmica, CSIC Campus UAB, 08193 Barcelona, Spain
e-mail: jordi.brandts@uab.es

D.J. Cooper
Department of Economics, Florida State University, Tallahassee, FL 32306-2180, USA
e-mail: djcooper@fsu.edu

E. Fatas
LINEEX, Facultad de Economía, Campus Tarongers, Universidad de Valencia, 46022 Valencia, Spain
e-mail: fatas@uv.es

1 Introduction

In a seminal paper Van Huyck et al. (1990) first studied the weakest-link game and reported the then surprising result of very considerable coordination failure.[1] This game can be seen as a good representation of production settings with complementarities and has been widely used to study issues of under-performance in companies and other organizations.[2] Our study takes these results about coordination failure in the weakest-link game as a starting point for an analysis of the process through which organizations can escape such performance traps. The focus here is on situations in which players have asymmetric effort costs.

The main motivation for our work stems from the potential connections between asymmetries and leadership. When a group is stuck in a coordination trap somebody has to take action to initiate the escape from such a situation. The problem consists fundamentally in changing the beliefs that the people involved have about others' beliefs and actions. Brandts and Cooper (2006) show that, with symmetric costs, successful escapes from coordination traps take place gradually with some people acting as leaders who pull laggards after them. In this process subjects face an additional coordination problem of determining *who* the leaders ought to be. In principle any of those involved could take actions to try to change others' state of mind.

However, if players have different characteristics then these can function as coordination devices for solving the problem of who goes first in raising performance levels. The work we present in this paper starts from the conjecture that asymmetric effort costs may play a role in overcoming coordination failure. The cost differences may point to some of the players as "natural" leaders. Differences in effort costs imply differences in earnings in the different Nash equilibria. Intuition suggests that players with the lowest effort costs should act as leaders in pulling a group out of a performance trap. As a consequence, moves to overcome coordination failure may be more successful in asymmetric than in symmetric costs groups.

We find that improvements in coordination after a change in incentives initially depend on effort cost asymmetries, but not for the reason hypothesized above. Leadership is driven not by low-cost subjects, but rather by subjects with the most common cost type. This conformity effect may be due to a kind of group identity or to the cognitive simplicity of acting with identical others. Over time the effect of asymmetric effort costs largely vanishes.

2 The turnaround game

The "turnaround game" played by subjects in our experiments is a stylized model of a firm that suffers low performance and is trying to improve over time. This firm consists of a number of employees who choose among different effort levels in a

[1] Van Huyck et al. (1990) introduced this class of coordination games as the "minimum game." Hirschleifer (1983) coined the more evocative moniker "weak-link game" which we use.

[2] See Camerer (2003, Chap. 7), for a recent survey as well as Blume and Ortmann (2007), Croson et al. (2005), Fatas et al. (2006), and Weber (2006).

number of consecutive periods. This game confronts employees with the dynamic problem of overcoming a history of coordination failure, rather than with the simple static problem of choosing an effort level once.

The turnaround game embodies three basic design choices. First, the firm's technology has a weak-link structure, with productivity (and profitability) depending in every time period on the minimum effort chosen by an employee. Second, the firm manager only observes the minimum effort selected, but employees can observe all effort levels selected. Third, the firm manager rewards employees with bonuses based on the minimum effort observed and is able to change the bonus rate but cannot otherwise influence the employees' choices. In what follows we present the main features of the turnaround game in more detail.

An experimental firm in the turnaround game consists of a fixed grouping of four employees who interact for thirty consecutive rounds, broken into three ten-round blocks. Each block starts with the announcement of a common bonus rate (B) for the ten rounds of the block that determines how much additional pay each employee receives for each unit increase in the minimum effort. All four employees observe B and then simultaneously choose effort levels, where E_i is the effort level chosen by the ith employee. We restrict an employee's effort to be in ten hour increments: $E_i \in \{0, 10, 20, 30, 40\}$. Intuitively, employees spend forty hours per week on the job, and effort measures the number of these hours that they actually work hard rather than loafing.[3] Employees' payoffs are given by (1) below. Effort is costly, with C_i denoting the cost of a unit of effort for the ith employee. The effort cost of each employee is held fixed throughout an experimental session. All payoffs are in "experimental currency units" (ECUs). These were converted to monetary payoffs at a rate of 1 euro equals 500 ECUs:

$$\text{Employee } i\text{:}\quad \pi_e^i = 200 - C_i E_i + \Big(B \times \min_{j \in \{1,2,3,4\}} (E_j)\Big). \tag{1}$$

In all our treatments the average cost of effort equals 7 (e.g. $\frac{1}{4}(C_1 + C_2 + C_3 + C_4) = 7$). The four treatments differ only in the distribution of these costs over employees. In all of the experiments described below employees initially receive a low bonus rate of $B = 8$, which in round 11 is increased to $B = 14$. As a useful example for developing some initial hypotheses about the impact of asymmetric costs, consider one of our treatments in which $C_1 = 1$ and $C_2 = C_3 = C_4 = 9$. With $B = 14$, the payoff tables shown in Table 1 result. The table on the left is for the low cost type and the table on the right applies to the three high cost types. If $C_i < B$ for all $i \in \{1, 2, 3, 4\}$, as in this example with $B = 14$, the resulting game is a weak link game. Standard game theoretic analysis provides little insight into the likely outcome of such games as coordinating on any of the five available effort levels corresponds to a Nash equilibrium. If $C_i > B$ for any $i \in \{1, 2, 3, 4\}$, as occurs in this example when $B = 8$, then the unique Nash equilibrium is for all employees to choose effort level zero.

[3] In the experiments, terms that we judged to have strong connotations such as "effort" and "loafing" were not used. Employees were told that they spent 40 hours per week on the job, and that these hours could either be allocated to Task A or Task B. Task A plays the role of effort and Task B plays the role of loafing.

Table 1 Employee i's payoff tables for $B = 14$

$C_i = 1$

Effort by employee i	Minimum effort by other employees: 0	10	20	30	40
0	200	200	200	200	200
10	190	330	330	330	330
20	180	320	460	460	460
30	170	310	450	590	590
40	160	300	440	580	720

$C_i = 9$

Effort by employee i	Minimum effort by other employees: 0	10	20	30	40
0	200	200	200	200	200
10	110	250	250	250	250
20	20	160	300	300	300
30	−70	70	210	350	350
40	−160	−20	120	260	400

In the initial phase with $B = 8$, choosing an effort level of 0 is a dominant strategy for the three high cost types. It is therefore likely that employees enter the second phase of the experiment, where $B = 14$, coordinated on the worst possible equilibrium of the new game.

Once the bonus is increased to $B = 14$ the new game is a weak-link game. All employees can now profit from raising their effort levels, *but only if all other employees raise their effort levels as well*. For each of the high cost types ($C_2 = C_3 = C_4 = 9$) the incentives to increase their effort beyond the minimum are weak and the risks are high. An employee who considers raising his effort from 0 to 10 knows that this action causes a certain loss of 90 ECUs due to the cost of increasing effort. If the other three employees match this increase, his net gain is only 50 ECUs beyond the 200 ECUs he gets without risk by choosing 0. For the proposed change in his effort level from 0 to 10 to have an immediate positive return, the employee must believe the probability of all three other subjects simultaneously raising their efforts from 0 to 10 exceeds $9/14$. Treating the other three subjects as statistically independent, this translates into requiring at least an 86% chance of increased effort for each of the other three subjects.

The incentives are kinder for the low cost type ($C_1 = 1$), as an increase to effort level 10 only entails a sure loss of 10 ECUs as opposed to a potential gain of 130 ECUs. Assuming the other three subjects are independent, this only requires a 42% chance of increased effort from each of the other three subjects for the low cost type to break even. However, the required increases must come entirely from high cost types who have poor incentives.

3 Experimental design and research questions

The top portion of Table 2 summarizes the experimental design. The treatments differ only in the costs of effort. In all sessions, subjects (employees) played thirty rounds in fixed groups (firms) of four. For the first ten rounds the bonus rate was set at $B = 8$. It was then raised to $B = 14$ for the remaining twenty rounds.

In control sessions all four employees had the same cost of effort: $C_1 = C_2 = C_3 = C_4 = 7$. Consistent with the results of earlier work on the turnaround game

Table 2 Summary of experiments

Treatment name	Symmetric	One low Three high	Two low Two high	Three low One high
Player 1 Cost	7	1	5	5
Player 2 Cost	7	9	5	5
Player 3 Cost	7	9	9	5
Player 4 Cost	7	9	9	13
Number of firms	19	20	21	20
Number of sessions	2 sessions	2 sessions	2 sessions	3 sessions
Ave. min. effort, R10	4.21	0.00	0.00	0.00
Ave. min. effort, BL2	19.11	18.55	12.57	12.7
Ave. min. effort, BL3	19.16	21.55	18.90	17.3
Ave. effort, BL2	24.76	24.6	17.58	16.75
Ave. effort, BL3	23.93	24.15	21.12	19.23
Ave. payoff, BL2	294	289	255	264
Ave. payoff, BL3	301	335	318	310

Notes: All firms include four employees. Block 1 is rounds 1–10, block 2 (BL2) is rounds 11–20, and block 3 (BL3) is rounds 21–30

with symmetric costs (Brandts and Cooper 2006), we expected low average minimum efforts to emerge over the first ten rounds.[4] After the increase in the bonus rate we expected to see a sharp increase in average minimum effort although not to the level of perfect coordination at an effort level of 40.

A basic design choice we made was to hold the average cost of effort fixed at 7 across the three treatments and to vary the asymmetry of costs of effort. All three treatments have low and high cost types, but vary the number of each type. In the "one low, three high" treatment, $C_1 = 1$ and $C_2 = C_3 = C_4 = 9$.[5] These specific costs of effort were chosen to make the cost of the low cost type as low as possible while keeping all costs positive and the average cost equal to 7. In the "three low, one high" treatment, $C_1 = C_2 = C_3 = 5$ and $C_4 = 13$. These costs are chosen to make the cost of the high cost type as large as possible while keeping all costs below the bonus rate (so that the game is a weak-link game) and the average cost equal to 7. As a result the low cost employee in the "one low, three high" treatment has a lower cost than the three low cost employees in "three low, on high". For our third treatment, "two low, two high", we chose the following cost levels $C_1 = C_2 = 5$ and $C_3 = C_4 = 9$. There are other possible "two low, two high" cost distributions. However, our choice allows us to make comparisons across treatments of subjects who have the same cost of effort and bonus rate but are grouped with other employees that have a different distribution of costs of effort. Specifically, we can compare the behavior of low cost types in the "two low, two high" treatment with the behavior of low cost types in the

[4]The payoff function in that case was $\pi_e^i = 200 - 5E_i + (B \times \min_{j \in \{1,2,3,4\}}(E_j))$.

[5]Employee ID numbers are used for expositional convenience and were not shown to subjects.

"three low, one high" treatment and also compare the behavior of high cost types in the "two low, two high" treatment with that of high cost types in the "three low, one high" treatment.

We can now formulate the following research questions:

- Is it easier to overcome coordination failure with asymmetric costs? If so, does relative frequency of low versus high cost types matter?
- In the asymmetric cost treatments, do differences in costs help determine which subjects take the role of leaders in reaction to the bonus increase?

A priori, we anticipated improved coordination following the bonus rate increase for all of the treatments. We expected this effect to be particularly effective with asymmetric costs of effort and fewer low cost employees. Our basic intuition was that scarce low cost employees would easily emerge as "natural" leaders.

These predictions were based on the central role of leadership in overcoming coordination failure and the likely effects of asymmetric costs on the incidence of leadership. Previous experiments with the turnaround game found that the process of overcoming coordination failure is typically gradual (Brandts and Cooper 2006). In firms that successfully overcome coordination failure there is usually a group of employees who take a leadership role by increasing their effort levels before others and maintaining it for some time. Eventually laggards are pulled up to the leaders' effort levels. With symmetric costs, there are no externally visible characteristics to mark which employees should take this risky leadership role. Which subjects become the leaders is determined solely on the basis of the subjects' innate qualities – for example, which subjects are particularly willing to bear risk or psychologically more inclined to taking the initiative than others. Asymmetric costs might help solve the additional coordination problem of leader selection by creating a focal point for who the leaders should be.

Low cost types are natural candidates to become leaders, as they face the least risk from leadership and have the largest potential gains. Building on this, intuition suggested that one lonely low cost employee naturally would have a stronger drawing power than two low cost employees, two more than three and finally three more than the identical four subjects in the control condition. In other words, having a relatively rare type that is particularly well suited to leadership might make it especially likely that an employee becomes a leader even if he was not naturally inclined to this role. As such we conjectured that the impact of asymmetric costs would be decreasing in the number of low cost types. Our confidence in this prediction was tempered by other elements of the problem. In particular, Brandts and Cooper (2006) find, for the symmetric case, that multiple leaders are far more effective than a single leader in overcoming a history of coordination failure. Having more low cost types might make it less likely that any one low cost type became a leader, but might make it more likely that the necessary multiple leaders emerge.[6]

[6]The idea here is that of leadership by example: the more people who set the example the more effective it will be.

4 Procedures

Sessions were run at the LINEEX computer lab of the University of Valencia. We aimed to gather data from twenty firms for each treatment, which generally required two sessions. The realized number of firms varies slightly across treatments due to variations in show-up rates.

Subjects were recruited from the undergraduate population of the University of Valencia, mainly among economics, business and law students. Students received an e-mail message announcing sessions. They then had to physically sign themselves in on a bulletin board. Subjects were only allowed to participate in a single session. For the most part, the experimental procedures were quite standard. At the beginning of each session subjects read the instructions directly from their computer screens. Before beginning to play, all subjects were asked to complete a short quiz about the payoffs and the rules of the experiment. The full text for the instructions and quiz for the "one low, three high" treatment is available on the journal website.

Rather than using abstract terminology we employ a corporate context. For example, the four players are explicitly referred to as "employees" and are told that they are working for a "firm." We avoided the use of terms with what we considered strong connotations. For example, instead of asking subjects to choose a level of "effort" they are asked to allocate time between "Activity A" and "Activity B," with Activity A playing the role of effort. We used a corporate context to make the instructions easier to understand for participants. While the use of meaningful context may have an impact on subjects' choices, any such effect would only affect our results if it interacted with changes in the costs of effort.[7]

The instructions stressed that firm membership remained fixed throughout the experiment. For experimenters who are used to worrying about repeated game effects, the use of fixed groups may seem like a strange design choice. However, the field settings that we are interested in simulating involve repeated interactions among the same agents. Repeated game effects and history dependence are presumably quite natural in these settings. Moreover, the possibility of history dependence is necessary for the development of leadership by example in the turnaround game. Repeated interactions are therefore the appropriate case to study.[8]

In all rounds subjects knew their own cost of effort, the distribution of other employees' costs of effort (but not the cost of any particular employee), and the current bonus rate. At the beginning of each ten period block of the experiment the bonus rate for the upcoming ten periods was announced on a special screen. Subjects knew that there would be a total of 30 rounds and that the bonus rate might change in future rounds. They did not know what future bonus rates would be. Other than the bonus rate, no detail of the experimental environment varied between blocks. In particular, subjects knew that the costs of effort would not be changing.

[7] Cooper and Kagel (2003) find that the use of meaningful context can speed up the development of strategic play.

[8] Most existing studies of the weak-link game use fixed matching as we do. For one notable exception see Van Huyck et al. (1990)—their Treatment C compares play for fixed and random pairings in two player weak-link games. For the final round of play, effort levels are significantly higher with fixed pairings than with random pairings.

In each round the four employees of a firm simultaneously chose their effort levels for the round. The screen where subjects made this decision displayed the current bonus rate as well as a payoff table similar to the ones displayed in Table 1. This showed the employee's payoff as a function of their own effort level and the minimum effort level chosen by the other employees. This payoff table was automatically adjusted to reflect the current bonus rate. In treatments where costs of effort were asymmetric, paper copies of other employees' payoff tables were handed out as well. At the end of each round employees were told their effort level, their payoff for the round, and their running total payoff for the experiment.[9] Separate windows on the feedback screen showed employees all four employees' effort levels for the round just completed as well as a summary of outcomes from earlier rounds. The four effort levels were sorted from lowest to highest. Subjects were not given any information about which employee had chosen which effort level, and therefore could neither track a particular employee across rounds nor observe any relationship between costs of effort and effort levels chosen.[10]

5 Results

A necessary condition for the experimental treatments to be of interest is that coordination failure occurs prior to the increase in the bonus rate. This occurs uniformly with asymmetric costs of effort, as the minimum effort equals 0—the choice in the unique Nash equilibrium—in period 10 for all 61 firms in these treatments. Moreover, all four employees choose zero in period 10 for 44 of these 61 firms. In the control treatment with symmetric costs, the minimum effort equals 0 in period 10 for 16 of the 19 firms and all four employees choose zero in 9 of the 19 groups. Weaker convergence in the control treatment with symmetric costs is unsurprising as it is not the unique Nash equilibrium for all employees to choose zero. None the less, even in the control treatment firms almost always face a history of coordination failure when the bonus rate is increased.

The bottom half of Table 2 shows descriptive statistics on how outcomes respond to the change in bonus rates for periods 11–30. Detailed pictures of how average minimum effort and average effort evolve over time are given by Figs. 1 and 2.

Looking at Fig. 1, we see that average minimum effort jumps in all four treatments following the bonus rate increase and then continues to rise slowly for a few more

[9] In the instructions employees were also given information on the payoffs of a fictitious manager for the firm. However, they knew at every point that there was no human manager involved in the experiment and that the bonus was changed by the computer. While not important for the current study, this allowed us to maintain parallelism with sessions where a fifth subject played as the firm manager and chose the bonus level in every round (see Brandts and Cooper 2007). The instructions can be found on the journal website.

[10] At the end of the session, each subject was paid in cash for all rounds played plus a show-up fee of five euros. Payoff was done on an individual and private basis. Recall that all payoffs are in "ECUs" which were converted to euros at a rate of one euro equals five hundred ECUs. The average total payoff was 20.29 € and the average session lasted about 90 minutes. These earnings were sufficiently large to generate a good supply of subjects.

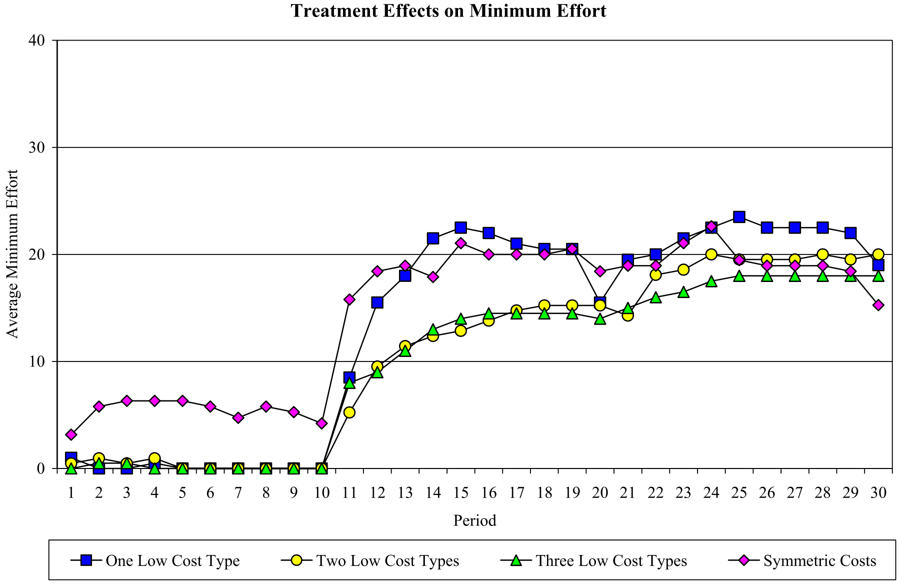

Fig. 1 Treatment effects on minimum effort

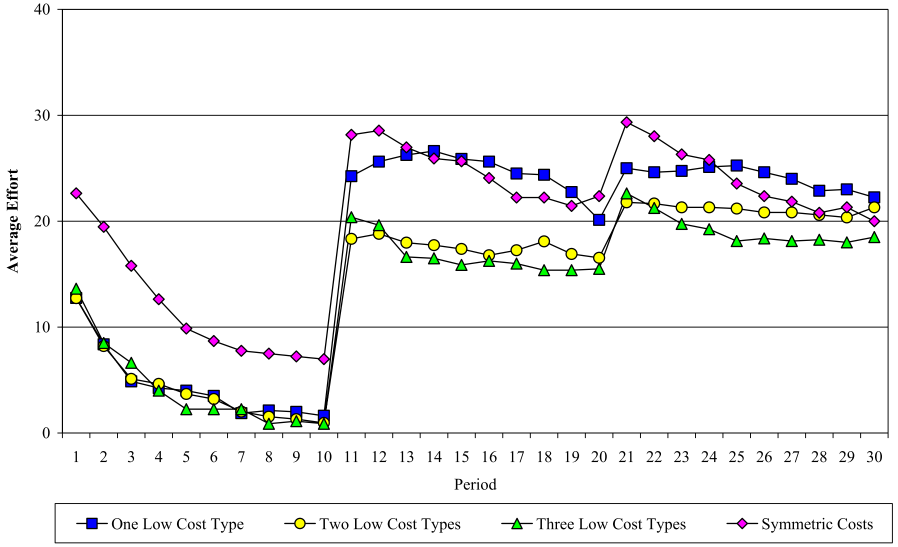

Fig. 2 Treatment effects on effort

periods before stabilizing. The treatments vary in the strength of the performance improvement for periods 11–20, but not for periods 21–30.[11]

[11]For the symmetric costs treatment average effort in round 10 is above zero. We have verified that subsequent behavior in this treatment is not different for the subset of cases where minimum effort is zero in round 10.

In periods 11–20, average minimum effort is higher in the control and “one low, three high” sessions than in the “two low, two high” and “three low, one high” treatments, albeit by a small amount. Consistent with these difference in average minimum efforts, average payoffs are also higher, but only slightly, for period 11–20 in the control and “one low, three high” sessions. There is a mild restart at period 21 caused by the pause to announce that the bonus rate would continue to be $B = 14$. This restart was sufficient to largely eliminate the gap between treatments. Intuitively, a restart is more likely to matter for groups stuck at low effort levels. Since there are more of these groups in the “two low, two high” and “three low, one high” treatments, the restart has a larger effect for these treatments. Using the Wilcoxon rank-sum test, we have made all pair-wise comparisons of minimum effort for the different treatments. For all comparisons of overall (rounds 11–30) and late (21–30) minimum effort we find no difference at the 10% level. For early minimum effort (rounds 11–20) we find a significant difference between the control and the “two low, two high” case ($p = 0.0758$) and between the “one low, three high” and the “two low, two high” ($p = 0.095$).

The employee level data displayed in Fig. 2 shows a similar pattern, but more accentuated than what Fig. 1 shows at the firm level. Following sharp increases for average effort across all treatments in period 11, a persistent gap opens between the average efforts in the in the control and “one low, three high” sessions and average efforts in the “two low, two high” and “three low, one high” treatments. The restart at period 21 is stronger in the employee level data than in the firm level data. Following the restart, average efforts are roughly the same across all four treatments. The Wilcoxon rank-sum comparison tests show again no difference for rounds 21–30. For rounds 11–20 we find significant differences comparing both the control ($p = 0.0901$ and $p = 0.0522$) and “one low, three high” ($p = 0.0256$ and $p = 0.0254$) with both “two low, two high” and the “three low, one high” cases.[12]

While the presence of asymmetric costs of effort causes differing responses to an increase in the bonus rate, the data provides no support for our initial conjectures that minimum efforts would be higher with asymmetric costs of effort and that this effect would be greatest for the “one low, three high” treatment. The answer to our first research question is the following.

Regularity 1: Increasing the bonus rate causes firms in the control and “one low, three high” sessions to do modestly better than firms in the “two low, two high” and “three low, one high” treatments. This advantage disappears with the restart in period 21. The difference between treatments is more pronounced in employee level data than in firm level data.

We now move to the study of leadership. We start by looking at the likelihood of employees becoming “strong leaders,” defined as employees who respond to an increase in the bonus rate by raising their effort at least two levels (e.g. 0 hours to 20 hours). Brandts and Cooper (2006) find that the presence of strong leaders plays a

[12] We have checked for these treatment effects using t-tests (corrected for unequal variances) and ordered probit regressions and found very similar results.

central role in determining whether a firm overcomes a history of coordination failure. This also holds for the data reported here. The average minimum effort in period 20 is a monotonically increasing function of the number of strong leaders in period 11.[13] The number of strong leaders does a good job of predicting whether a firm will eventually overcome coordination failure and it also outperforms other measures of the firm's performance in period 11. To establish this point, we ran an ordered probit regressing firms' minimum efforts in period 20 on the number of strong leaders in round 11, the minimum effort in round 11, and the number of employees who increased their effort in round 11. The number of strong leaders has a statistically significant effect at the 5% level, while the minimum effort is only significant at the 10% level and the number of employees who increased their effort fails to be statistically significant.[14] The marginal effect on minimum effort in period 20 of adding one strong leader to a firm in period 11 is 37% greater than the marginal effect of increasing the minimum effort by one level in period 11.[15]

Now that we know that strong leaders are important we ask how asymmetric costs affect who becomes a strong leader. The mechanism through which we anticipated seeing a treatment effect was that low cost types would be obvious candidates for strong leadership. In other words, the asymmetric costs of effort would create a focal point for which employees should be strong leaders. Looking at employee level data, low cost types are actually somewhat *less* likely than high cost types to become strong leaders in period 11 (50.2% vs. 59.8%). This result seems counter-intuitive as high cost types have less to gain and more to lose by becoming strong leaders. Indeed, as can be seen in Table 3, the factor driving whether an employee becomes a strong leader isn't the cost of effort but rather cost cohort size, *defined as how many other employees have the same cost of effort as oneself*. Table 3 is based on the choices of all 320 employees in period 11. The columns give the cost of effort while the rows

Table 3 Proportion of strong leaders in period 11

Cohort size	Cost of effort					
	1	5	7	9	13	All
0	0.50	–	–	–	0.40	0.45
1	–	***0.43***	–	***0.57***	–	0.50
2	–	***0.55***	–	***0.68***	–	0.62
3	–	–	0.63	–	–	0.63
All	0.50	0.50	0.63	0.64	0.40	–

Note: Cost cohort size is defined as the number of other employees in the firm with the same cost of effort as oneself

[13]For 0, 1, 2, 3, and 4 strong leaders in period 11, the average minimum efforts in period 20 are 4.0, 8.0, 10.9, 24.1, and 27.0 respectively.

[14]The parameter estimates and standard errors (in parentheses) for the number of strong leaders, minimum effort in period 11, and number of employees increasing their effort in period 11 are 0.385 (0.167), 0.281 (0.147), and 0.157 (0.185) respectively. Note that minimum effort in period 11 has been scaled down by a factor of 10 to facilitate the comparison of marginal effects.

[15]To improve coordination, subjects may try to use their decisions to signal intended future play. See Blume and Ortmann (2007) and references therein for the role of communication in weak-link games.

give the cost cohort size.[16] The number reported in each cell in the proportion of employees who were strong leaders in round 11. Increasing the cost of effort does not monotonically affect the likelihood of employees becoming strong leaders, as can be seen from the last row. In contrast, the likelihood of becoming a strong leader is monotonically increasing in the cost cohort size (see the rightmost column). It is particularly telling to compare the two cases where employees had identical costs of effort but different cost cohort sizes.[17] These cells have been highlighted. In both cases, strong leadership is roughly 20% more likely when the cost cohort size is greater.[18]

If the only factor affecting whether a firm overcomes coordination failure was the number of strong leaders, we would expect to see the best performance in the controls and the worst performance in the "two low, two high" treatment since these treatments have the largest and smallest cost cohort sizes respectively. The "one low, three high" and "three low, one high" treatments should perform about the same and should be intermediate cases. Inspection of the data refutes these predictions—the controls and the "one low, three high" treatment yield almost indistinguishable results as do the "two low, two high" and "three low, one high" treatments. In other words, the two treatments which should be intermediate cases aren't.

The explanation for this lies in a second subtle effect of the treatments on employees' choices. Unlike period 11, in periods 12–20 it is more important what an employee's cost of effort is than how many other employees share this cost of effort.

This effect can be seen in the two panels of Table 4. Both panels report data based on employees' choices in periods 12–20. The upper panel is based on the choices of employees who were followers in the preceding period—their effort was weakly less than the minimum effort of the other three employees in their firm. The lower panel is drawn from the choices of employees who were leaders in the preceding period—these employees choose an effort level strictly greater than the minimum effort of the other three employees in their firm. The categories for rows and columns for each panel are identical to those described above for Table 3. The number in each cell reports the average change in effort.

An identical pattern emerges in both panels of Table 4. Looking at the last row, lower costs of effort are consistently associated with more positive changes in effort (more positive for followers, less negative for leaders). Lower costs of effort make followers more responsive to leaders' efforts and make leaders more persistent in trying to pull followers up to their level. The rightmost columns of the two panels reveal no similar effect for changes in the cost cohort size.[19]

[16] For example, data from a low cost type in the "one low, three high" treatment appears in the first column of the first row while data from a high cost type in this treatment is in the fourth column of the third row.

[17] The relevant comparisons are between low cost types in the "two low, two high" and "three low, one high" treatments and between high cost types in the "three low, one high" and "two low, two high" treatments.

[18] To clarify, the calculation is (% big cohort / % small cohort) -1, not % big cohort—% small cohort. A probit regression with robust standard errors finds that the effect of cost cohort size is statistically significant at the 1% level.

[19] There is a weak relationship for leaders and none for the followers.

Table 4 Average change of effort in periods 12–20

Followers (lagged effort ≤ lagged minimum effort of other employees)

Cohort size	Cost of effort 1	5	7	9	13	All
0	2.66	–	–	–	1.22	1.83
1	–	3.39	–	1.95	–	2.60
2	–	2.13	–	2.27	–	2.20
3	–	–	1.91	–	–	1.91
All	2.66	2.62	1.91	2.13	1.22	–

Leaders (lagged effort > lagged minimum effort of other employees)

Cohort size	Cost of effort 1	5	7	9	13	All
0	−4.37	–	–	–	−6.56	−5.05
1	–	−4.74	–	−6.69	–	−5.54
2	–	−6.70	–	−5.94	–	−6.12
3	–	–	−6.13	–	–	−6.12
All	−4.37	−5.59	−6.13	−6.21	−6.56	–

Note: Cost cohort size is defined as the number of other employees in the firm with the same cost of effort as oneself

The regressions reported in Table 5 are designed to put the preceding observations on a firmer statistical footing. The goal is to identify the role of costs of effort and cost cohort size (again defined as number of other employees with the same cost of effort as oneself) in *changes* in an individual employee's effort. Data is taken from periods 11–20, the periods in which the treatment effects are evident.[20] The dependent variable for both regressions is the employee's effort in the current round. An ordered probit specification is used and standard errors are corrected for clustering at the firm level. As dependent variables the regressions include the lagged effort, the twice lagged effort, and lagged minimum other's effort (e.g. the minimum effort of the firm's other three employees in the previous period). The inclusion of these variables is partially driven by our focus on changes, but is also intended to clarify causality. In particular, we are trying to show that cost cohort size matters in period 11 but not in later periods. If lagged dependent variables are not included in the regression, a false positive for cost cohort size can (and does) occur for periods 12–20. To see why this occurs, suppose that lagged effort plays an important role in determining current effort. If a larger cost cohort size causes a bigger increase in effort for period 11, it must also lead to higher effort in periods 12–20. This is not a direct effect of the larger cost cohort size on *changes* in effort in periods 12–20, but instead an indirect effect on the *level* of effort in periods 12–20 coming from the direct effect

[20] The regressions reported on Table 5 have also been run using all data from periods 11–30. This has little impact on the results.

Table 5 Ordered probits, costs vs. compatriots (dependent variable: employee effort)

Variable	Model 1	Model 2
Lagged effort	0.064***	0.058***
	(0.004)	(0.005)
Twice lagged effort	−0.004	−0.004
	(0.003)	(0.003)
Lagged minimum Other's effort	0.074***	0.080***
	(0.006)	(0.007)
Period 11	1.495***	1.470***
	(0.432)	(0.432)
Cost of effort* Period 11	0.017	0.016
	(0.032)	(0.032)
Cost cohort size* Period 11	0.239**	0.242**
	(0.114)	(0.114)
Cost of effort* Periods 12–20	−0.026***	
	(0.007)	
Cost of effort* Periods 12–20* leader		−0.009
		(0.009)
Cost of effort* Periods 12–20* follower		−0.034***
		(0.008)
Cost cohort size* Periods 12–20	−0.007	−0.011
	(0.031)	(0.031)
Log likelihood	−3164.40	−3160.79

Note: Both regressions include 3200 observations from 320 employees. Standard errors are corrected for clustering at the firm level. ***, **, and * denote statistical significance at the $p < 0.01$, $p < 0.05$, and $p < 0.10$ levels respectively. Cost cohort size is defined as the number of other employees in the firm with the same cost of effort as oneself

in period 11. The independent variables of interest are the employee's cost of effort and cost cohort size. These are interacted with a dummy for period 11 and a dummy for periods 12–20; the interaction terms allow us to separate the effects of cost of effort and cost cohort size in period 11 from their effects in later periods. A dummy for period 11 is also included to capture the large effect of changing the bonus rate on effort. In Model 2, the cost of effort for periods 12–20 is interacted with dummies for whether the employee is a leader (lagged effort > lagged minimum effort of other employees) or a follower (lagged effort ≤ lagged minimum effort of other employees). The inclusion of these terms allows us to explore whether the cost of effort matters more in periods 12–20 for followers or for leaders.

Model 1 establishes how period 11 differs from the following periods. Not surprisingly, both the lagged effort and the lagged minimum effort of other employees have large positive (and statistically significant) effects on the current effort level. The twice lagged effort does not significantly impact the current effort level. The dummy for period 11 has a large, positive, and statistically significant parameter estimate. This reflects the immediate jump in effort following the bonus rate increase.

For period 11, the effect of the cost of effort does not approach statistical significance while the cost cohort size has a positive effect that is statistically significant at the 5% level. For periods 12–20, the cost of effort has a negative effect that is statistically significant at the 1% level while the cost cohort size no longer has a statistically significant effect. Consistent with our previous observations, the cost cohort size only plays an important role in period 11 when employees are responding to the change in the bonus rate. Cost of effort has little impact on this initial reaction but plays an important role subsequently.

Model 2 clarifies the role of costs of effort in periods 12–20. The interaction term between costs of effort for periods 12–20 and whether the employee is a leader is tiny and nowhere close to statistical significance. The other interaction term, between costs of effort in periods 12–20 and whether the employee is a follower is four times as large and significant at the 1% level. The cost of effort in periods 12–20 acts primarily by making followers less responsive to the actions of leaders.[21]

Regularity 2: Larger cost cohort size makes employees more responsive to the bonus rate increase in period 11 but does not affect their behavior in later periods. Higher costs of effort do not affect employees' response to the bonus rate increase in period 11 but make followers less likely to increase their effort in later periods.

The differing effects of cost cohort size and costs of effort provide an explanation for why behavior differs between the "one low, three high" and "three low, one high" treatments. In some sense the "one low, three high" treatment is a best case scenario. The three high cost types have a relatively large cost cohort so it is likely that there will be multiple strong leaders. While the low cost types are relatively unlikely to be strong leaders, they should be responsive followers due to their low costs. Thus, all the ingredients for a successful turnaround are in place. In contrast, the "three low, one high" treatment lacks a critical element. While the large cost cohort of low cost types should generate a good supply of strong leaders, the high cost types are unlikely to be either strong leaders or responsive followers. High cost types in the "three low, one high" treatment are prime candidates for scuttling a turnaround.

6 Conclusions

The results are more complex than our initial intuition suggested. Asymmetric costs do not improve firms' abilities to improve performance. Underlying the weak treatment effects are interactions between the effort cost structure and individuals' choices. As expected, the process of performance improvement is led by a subset of employees who take the initiative and pull others out of the trap. These leaders are not the low-cost employees, but those whose costs are more frequent in the group. By itself, this cost cohort size effect implies that symmetric costs should be the best case for escaping from the trap. Somewhat mitigating the cost cohort size effect is a second effect that higher cost employees tend to be less responsive as followers. When

[21] This differential effect is robust to a variety of different specifications. The discrepancy between Table 4, where no such effect is obvious, and Table 5 is due to using effort rather than change of effort as the dependent variable. There is regression to the mean in the data which Table 4 isn't accounting for.

these effects work in opposite directions, as in the “three low, one high” treatment, the lack of responsive followers can overcome the enhanced efforts of leaders. More generally, participants may be trying to use their choices to send signals, but it is very hard to understand this process.

The cost cohort size effect we observe is interesting, but we can only speculate about its cause. One possibility is that a large cost cohort eases the common knowledge problem at the heart of coordination games. An employee should only raise his effort if he believes that others will raise their effort as well. He should only expect these increases if he believes that others believe he will raise his effort, etc. Believing that others will make the same increases as oneself may be easier if others are more like oneself. Similarity may make subjects feel they have some insight into the likely reasoning of other players. A second possibility is that cost cohort size effects are due to group identity. If an employee feels more akin to his fellow employees, he may be more willing to take a leadership role. Having the same cost as most of the other employees could lead to this sense of kinship.

Acknowledgements Financial support by the Spanish *Ministerio de Educación y Ciencia* and the *Barcelona Economics* Program of CREA is gratefully acknowledged. Remaining errors are ours.

References

Blume, A., & Ortmann, A. (2007). The effects of costless pre-play communication: Experimental evidence from games with Pareto-ranked equilibria. *Journal of Economic Theory*, in press.

Brandts, J., & Cooper, D. (2006). A change would do you good. An experimental study on how to overcome coordination failure in organizations. *American Economic Review*, *96*, 669–693.

Brandts, J., & Cooper, D. (2007). It’s what you say no what you pay. An experimental study of manager–employee relationships in overcoming coordination failure. *Journal of the European Economic Association*, in press.

Camerer, C. (2003). *Behavioral game theory: Experiments in strategic interaction*. Princeton: Princeton University Press.

Cooper, D. J., & Kagel, J. H. (2003). Lessons learned: Generalizing learning across games. *American Economic Review*, *93*, 202–207.

Croson, R. T. A., Fatas, E., & Neugebauer, T. (2005). *Excludability and contribution: A laboratory study on team production*. Mimeo.

Fatas, E., Neugebauer, T., & Perote, J. (2006). Within team competition in the minimum game. *Pacific Economic Review*, *11*(2), 247–266.

Hirschleifer, J. (1983). From weakest-link to best-shot: The voluntary provision of public goods. *Public Choice*, *41*(3), 371–386.

Van Huyck, J., Battalio, R., & Beil, R. (1990). Tacit coordination games, strategic uncertainty, and coordination failure. *American Economic Review*, *80*, 234–248.

Weber, R. (2006). Managing growth to achieve efficient coordination in large groups. *American Economic Review*, *96*(1), 114–126.