Observability and Overcoming Coordination Failure in Organizations

An Experimental Study

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Abstract: Many organizations suffer poor performance because its members fail to coordinate on efficient patterns of behavior. In previous research, we have shown that financial incentives can be used to find a way out of such performance traps. Here we examine the sensitivity of this result to the ability of people to observe others' choices. Our experiments are set in a corporate environment where subjects' payoffs depend on coordinating at high effort levels; the underlying game being played repeatedly by the employees of an experimental firm is a weak-link game. Treatments vary along two dimensions. First, subjects either start with low financial incentives for coordination, which typically leads to coordination failure, and then are switched to higher incentives or start with high incentives, which typically yield effective coordination, and are switched to low incentives. Second, as the key treatment variable, subjects either observe the effort levels chosen by all employees in their experimental firm (full feedback) or observe only the minimum effort (limited feedback). We find three primary results: (1) The use of full feedback improves the ability of organizations to overcome coordination failure, (2) The use of full feedback has no effect on the ability of successful organizations to avoid slipping into coordination failure, and (3) History-dependence is strengthened by the use of full feedback.

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1. Introduction

Coordination failure lies at the root of underperformance by many entities ranging from single firms to entire national economies. As such, both overcoming existing coordination failure and avoiding a slide into coordination failure are critical issues for the survival and success of many organizations. For example, Knez and Simester (2002) study the successful turnaround of Continental Airlines in the mid 1990s. They establish that a central feature of the problem facing Continental executives was the role of interdependencies among autonomous groups of employees in determining the airline's performance. Only through coordinated changes in behavior could the firm's performance be improved. On a larger scale, underdeveloped economies can be seen as the result of coordination problems between different parts of an economy (Rosenstein-Rodan, 1943; Hirschman, 1958). It may be that no one sector of an economy can profitably industrialize alone; instead, coordinated industrialization across many sectors is required for an underdeveloped nation to achieve greater prosperity. The opposite can happen as well, with the unraveling of coordination causing a descent into poor performance both in organizations and entire economies.

In previous research, we have used controlled laboratory experiments to study whether and how financial incentives can be used to overcome coordination failure (Brandts and Cooper, 2004a; henceforth B&C). B&C explored an experimental environment, labeled the "corporate turnaround game," which simulates a corporate environment in which coordination failure has occurred. Specifically, the corporate turnaround game involves repeated play of a game between four "employees" of a "firm". The productivity and profitability of the firm is determined by the minimum effort level chosen by its four employees. This is meant to represent a situation with a very strong form of complementarities. Employees' incentives are determined by a bonus rate set by an exogenous manager. This bonus rate determines the fraction of the firm's profits transferred to the employees and hence governs the benefit to the four employees of coordinating at a high effort level. For any given bonus rate, the game played by the employees for any one round of the corporate turnaround game is what Knez and Camerer (1994) have called a "weak-link game."[1] In the experimental design of B&C, employees initially faced a low bonus rate and,

[1] This class of coordination games was introduced into the experimental literature as the "minimum game" by Van Huyck, Battalio, and Beil (1990). See also Knez and Camerer (2000) and Weber, Camerer, and Knez (2004).

hence, low incentives to coordinate. Under these circumstances the experimental firms typically become coordinated at the least efficient of the possible outcomes.

B&C then studied how changes in the bonus rate can lead to an improvement in firms' situation. An increase in the bonus rate generally led to greater coordination. In other words, financial incentives can in this case be used as an effective tool for reversing a history of coordination failure. Surprisingly, the size of the bonus rate increase had little impact on the likelihood of successfully overcoming coordination failure. Apparently a change in the bonus rate is less important for increasing the benefits of coordination than as a pure coordination device, cuing employees to attempt a coordinated change to higher effort levels. The results indicate that the process of overcoming coordination failure has important dynamic elements and can not be fully understood on the basis of comparative statics.

The preceding observations raise the issue of which factors are conducive to overcoming coordination failure and serve as the starting point for our current work. One important feature of the B&C environment is that each employee receives feedback about the choices of all other employees in his firm. This can be seen as representative of some environments, but not others. In this paper we study the effect of varying the feedback on subjects' ability to achieve and maintain coordination.

In terms of a purely static analysis, the feedback received by subjects is of little import, changing nothing about predicted outcomes. However, the observability of others' choices could play a critical role in whether or not subjects overcome the inertia of a history of coordination failure. The B&C results suggest this possibility. By the nature of a weak-link game, outcomes can only improve if all employees increase their effort levels. Yet, in the B&C data a unanimous response to a bonus rate increase is relatively rare. Because employees receive feedback about all choices, both leaders and laggards can observe the strong effort increases of some of their peers *even if minimum effort has not changed* and are able to react to this information in later periods. This plays an important role in coordinating at higher effort levels as laggards tend to eventually follow the lead of employees who respond to the bonus rate increase with large increases in effort. If only the ultimate outcome can be observed rather than all the choices that led to this outcome, then laggards cannot follow the example of fellow subjects who move to higher effort levels (since their actions can't be observed) and therefore may not increase their effort levels subsequently. Likewise, leaders cannot observe that other subjects have also moved to higher effort levels and may therefore give up (by choosing a lower effort level) before the laggards have time to move to higher effort levels.

Observability may also be an important factor in relation to what can be seen as the opposite problem, maintaining coordination when a decrease in the bonus rate is implemented. A high bonus rate may be necessary for attaining a good level of coordination but can also be financially unsustainable for the organization. Therefore, firms have a strong incentive to lower the bonus payment if this doesn't disrupt existing coordination. B&C show that this approach works if employees have feedback on the choices of all other employees – decreases in the bonus rate have little or no impact on existing coordination. However, the same may not hold if only the minimum effort can be observed. If a decrease in the bonus leads to some employees lowering their effort levels, both leaders and laggards will be unable to observe that some of the other employees in their firm have kept up their effort levels. For laggards this could reinforce their decision to reduce their effort, while for leaders it could lead to a tendency to quickly imitate the initial effort decrease of some of their co-workers.

As the preceding suggest, the experiments below vary the feedback given employees in the corporate turnaround game to determine the effect of feedback on coordination. "Full feedback" shows subjects the choices of all other employees in their firm while "limit feedback" only gives them the minimum effort for their group. We find three regularities in the data: (1) The use of full feedback improves the ability of organizations to overcome a history of coordination failure, (2) The use of full feedback has little effect on the ability of successful organizations to avoid slipping into coordination failure, and (3) History-dependence is strengthened by the use of full feedback. Together, these results suggest that publicizing unsuccessful attempts to overcome coordination failure may be a valuable tactic for managers and/or policy makers seeking an escape from coordination failure.

2. The Corporate Turnaround Game

Our experimental firms consist of four employees who choose among different effort levels. A firm's overall productivity (as well as profitability) is determined by the effort of its employees. To motivate these employees the firm uses a bonus system that transfers some of its profits back to employees. In this stripped down environment, altering the bonus structure is the sole tool firms have available to change employees' incentives in the firm.

Our design hinges on three basic choices. First, the firm's technology has a weak-link structure, with productivity depending on the minimum effort chosen by an employee. Second, the firm manager only observes the minimum effort selected, which determines the output. 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 employees' choices.

As discussed above, in this paper our main focus is on how the information available to employees affects behavior. We therefore conducted sessions under two information feedback conditions. Under "full information," employees are informed after each round of interaction about the effort choice of all four employees. In the other condition, "limited feedback," employees only learn the minimum level of effort exerted in their firm in the previous round. Our aim is to compare behavior under these two information conditions.

Apart from the four subjects in the employee roles, each of our experimental firms also has a fictitious manager. Even though the manager's choices are exogenous, for expositional purposes it is useful to treat the manager as a player in the game. The game starts with a predetermined flat wage that each employee receives regardless of the outcome and a bonus rate (B) that determines how much additional pay each employee receives for each additional 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 i^{th} 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.[2]

The payoffs are given by Equations 1 and 2. All payoffs are denominated in "experimental pesetas." These were converted to monetary payoffs at a rate of 1 dollar or 1 euro equals 500 experimental pesetas:

[2] In the experiments, terms with strong connotations such as "effort" and "loafing" were not used. Employees were told that they spend 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.

$$\text{Firm Manager:} \quad \pi_F = 100 + \left((60 - 4B) \times \min_{i \in \{1,2,3,4\}} \left(E_i \right) \right) \text{(eq. 1)}$$

$$\text{Employee i:} \quad \pi_e^i = 200 - 5E_i + \left(B \times \min_{j \in \{1,2,3,4\}} \left(E_j \right) \right) \text{(eq. 2)}$$

The firm manager's profit depends on the minimum effort contributed by its employees, consistent with our assumption that the firm's production technology has the weak-link property. Bonuses are based on the minimum effort, as implied by the assumption that the manager cannot observe individual efforts. As can be seen from the equations, produced surplus depends only on the minimum effort. The bonus simply transfers a portion of the firm's profits to its employees; the higher the bonus the higher is the employees' share in the surplus.

Given that in the experiments reported below the manager is exogenous, we now focus on the proper subgame where employees choose effort levels. For the two values of the bonus rate used in the experiments presented below, B = 6 and B = 10, the resulting subgame is a weak-link game between the employees. Coordinating on any of the five available effort levels is a Nash equilibrium.

The left panel of Table 1 below shows payoffs for the game induced by a bonus value of B = 6. Suppose that employees have previously coordinated on the worst possible equilibrium where all choose the lowest available effort level, 0. This circumstance is the natural starting point for all considerations of a possible turnaround. What are the possibilities of a spontaneous joint effort increase starting from a situation of complete coordination breakdown with B = 6? One can see, intuitively, that the incentives to increase effort beyond the minimum are quite weak and the risks are high. An employee who considers raising his effort to 10 knows that this action certainly causes his payoff to shrink by 50 pesetas due to the cost of increased effort. If all the other subjects follow his lead, his net gain is only 10 pesetas beyond the 200 pesetas he gets without risk by choosing 0. For the proposed change in his effort level from 0 to 10 to have a positive expected profit, the employee must believe the probability of all three other subjects raising their efforts from 0 to 10 equals 5/6. Treating the other three subjects as statistically independent, this translates into requiring a 94% chance of increased effort for each of the other three subjects.[3] In other words, our fictitious subject must be almost certain that the other subjects will increase their efforts for such an increase to be worthwhile for him. Given these grim incentives, the most likely outcome as employees become experienced is that all employees choose low effort levels. As noted previously, this is consistent with earlier experimental results.

Table 1
Employee i's Payoff Tables

B = 6								**B = 10**						
		Minimum Effort by Other Employees								Minimum Effort by Other Employees				
		0	10	20	30	40				0	10	20	30	40
Effort by Employee i	0	200	200	200	200	200		Effort by Employee i	0	200	200	200	200	200
	10	150	210	210	210	210			10	150	250	250	250	250
	20	100	160	220	220	220			20	100	200	300	300	300
	30	50	110	170	230	230			30	50	150	250	350	350
	40	0	60	120	180	240			40	0	100	200	300	400

Now imagine that a new manager takes over the firm. Determined to shake the firm out of its underperforming ways, he decides to raise the bonus rate to B = 10. The payoffs for this new bonus level are shown in the right panel of Table 1. The incentive to increase effort is now stronger for the employees. Once again, consider an employee who has experienced the inefficient equilibrium and is thinking of increasing his effort from 0 to 10. While the certain losses remain 50 pesetas, the potential gain is now 50 pesetas. The probability that all three other subjects will increase their efforts required to make this change break even is now only 1/2. Assuming the other three employees are independent, this translates into requiring a 79% chance that each employee increases his effort. While still high, this is better than what we saw with B = 6. With the less daunting odds, one can imagine subjects at least attempting to coordinate at higher effort levels.

Analogously, consider a firm that, with a bonus level of B = 10, has managed to coordinate at the maximum possible effort, 40. If an employee decreases his effort to 30 he loses 50 pesetas which is also the loss if all his co-workers act just like him. If the bonus is kept at B =

[3] To derive this probability, solve for p such that $200 = 150*(1-p^3) + 210*p^3$. Given the linear payoff structure the same trade-off arises for one-step increases of effort starting at a level higher than 0, as well as for two or more step increases when feasible.

10, there is no clear reason to expect a sudden breakdown of coordination; after all any common effort level is a Nash equilibrium. If the bonus in now lowered to B = 6, so that the loss from lowering the effort to a level of 30 is only 10, it seems reasonable to conjecture that coordination will now break down more easily than if the bonus had been maintained at its initial value even though the set of Nash equilibria hasn't changed.

3. Experimental Design and Hypotheses

The preceding examples illustrate that the central questions we ask in our experimental design are *not* about the comparative statics of behavior in the weak-link game. It is quite possible that subjects with no previous experience will generally converge to the inefficient equilibrium in the game with B = 6 and to a more efficient equilibrium in the game with B = 10. However, our main focus is on studying whether people's behavior can be changed once they have interacted frequently under fixed circumstances and, more specifically, whether this ability to change depends on the information feedback they receive. We are interested in a dynamic issue rather than a purely static one.

Our ex ante research hypotheses were based on the simple intuition presented in Section 1. Before seeing our results we conjectured that both the attainment of better coordination and the preservation of good coordination are easier in an environment with full feedback information than one with limited feedback. Specifically, if the bonus rate is increased from B = 6 to B = 10 we expected the observability of other co-workers' attempts at increasing effort would make coordination at higher levels more likely. With respect to the preservation of good coordination if the bonus is decreased from B = 10 to B = 6, our intuition was weaker. However, we leaned towards the conjecture that the effect of full feedback would be symmetric to the effect for the case of a bonus increase. We expected that observing that some co-workers do not decrease their effort, even if minimum effort decreases, would make it more likely that play would return to coordination at high levels.[4]

[4] These hypotheses are presented on a purely intuitive basis rather than relying on a formal model. In B&C we explored the ability of assorted game theoretic models to track changing behavior in the corporate turnaround game with full feedback as the bonus rate changes. Quantile response equilibrium (McKelvey and Palfrey, 1995) fares poorly as might be expected given its static nature. More surprisingly, variants of the EWA learning model (Camerer and Ho, 1999) also fail to track changing behavior in spite of the model's inherently dynamic nature. This failure cannot be attributed to narrow technical issues about how the model is formulated or fit to the data, but instead reflects an inability of the model to replicate how laggards respond to increases in effort by other employees. In the data laggards tend to follow strong increases in effort by others, but in the learning models the laggards

In our experimental sessions subjects played thirty rounds in fixed groups ("firms") of four employees. Treatments varied along two dimensions. The first was whether subjects received *full* feedback about all partners' effort choices in previous rounds or only *limited* feedback about the minimum effort in the group. The other dimension pertained to how the bonus changed across rounds, reflecting our interest both in how coordination failure can be overcome and in how good coordination can be made to last.

Each session consisted of three blocks of ten rounds each. During a session the bonus changed in a predetermined way.[5] The bonus rate and the resulting payoff-matrices were announced at the beginning of each of the three ten-round blocks and were fixed during that time frame. While playing in a round with a particular bonus subjects did not know what the bonus in subsequent blocks would be. Other than the bonus rate, no detail of the experimental environment was varied between rounds. In particular, in the instructions it was explained which type of information feedback would be available in that session and that it would be the same throughout the experimental session.

Table 2 summarizes our 2 x 2 design. The first bonus sequences we use has low bonus rate, B = 6, for the first ten-round block followed by an increase to a higher bonus rate, B = 10, for the remainder of the experiment. Assuming that the low initial bonus rate induces coordination failure, this 6/10/10 bonus sequence allows us to study the effect of the feedback condition on the ability of firms to re-coordinate on a better outcome. The second reverses the order of playing with a high and low bonus rate – subjects first play two blocks with a bonus rate of B = 10 followed by a final block with the low bonus rate of B = 6. If initial play with at a relatively high bonus rate leads to coordination at an effort level above the minimum, the 10/10/6 bonus sequence lets us study the effect of the feedback condition on the ability of firms to maintain good coordination in the face of decreased incentives.[6]

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remain laggards. Rather than being good followers, the laggards wear down the leaders into abandoning any attempt at improved coordination. Given these findings, it seems unlikely that the obvious types of formal models will have good predictive power for the experiments reported here.

[5] In Brandts and Cooper (2004c) we study what bonus rates would be set by subjects in the role of manager and how employees' responses are affected by the use of a subject as the manager.

[6] Note that the bonus rate change takes place earlier in the 6/10/10 treatment than in the 10/10/6 treatment. This allows us a ten round overlap in which we can compare play with a current bonus of B = 10 but a differing history of previous bonuses (B = 6 vs. B = 10). Results from B&C indicate that it matters little whether a bonus rate change takes place after ten or twenty rounds.

Table 2
Our Treatments

Information Feedback	Full	Limited	Full	Limited
Bonus Rate Rounds 1 – 10	6		10	
Bonus Rate Rounds 11 – 20	10		10	
Bonus Rate Rounds 21-30	10		6	

4. Procedures

Sessions were run both at Pompeu Fabra and at Case Western Reserve. In both cases, a computerized lab was used to run the sessions. For the 6/10/10 bonus sequence with full feedback we have data from nine independent groups of four subjects while for each of the other three treatments we have ten groups. Our focus here is not on the cross-country comparison. In our regression analysis we control for the location effect, which turns out to be rather secondary.

Subjects were recruited from the undergraduate populations at Pompeu Fabra and Case Western Reserve using newspaper ads, posters, emails, and classroom announcements. Subjects were only allowed to participate in a single session.[7] 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 full information treatment is given in the appendix.

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 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, an important issue for some subject pools used in the broader design. It can not be ruled out that the use of meaningful context per se has some

[7] The smaller number of groups for the 6/10/10 bonus sequence with full feedback is due to exclusion of data from a group that inadvertently included a subject who had previously participated in the experiment. Inclusion of this observation has no impact on our conclusions.

impact on subjects' choices.[8] However, in this paper we are mainly interested in the treatment effects of varying the information feedback and there is no particular reason to expect any interaction effect between context and information effects.

Subjects played the game in cohorts of four employees. As mentioned previously, these cohorts remained constant during the course of the experiment, a fact that was stressed in the instructions. For experimenters who are used to worrying about repeated game effects, this may seem like a strange design choice. However, the field settings that we are interested in representing involve repeated interactions among the same agents. Moreover, repeated game and learning effects as well as hysteresis are presumably quite natural in these settings and are precisely the issues we want to get at with our design. Therefore, we think that our experiments must incorporate repeated interactions to be a useful tool.[9]

In each round the four employees of a firm simultaneously chose their effort levels for the round. In both information feedback treatments the screen where subjects made this decision displayed the current bonus rate and the formula for the firm's fictitious manager's payoff. The amount was not actually paid out to any subject, but was displayed to maintain parallelism with sessions where another subject played as the firm manager. The subjects were also shown a payoff table, similar to the ones displayed in Table 1, showing their 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. 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.

Separate windows on the feedback screen showed them the results from earlier rounds and informed them about others' efforts in the preceding round. Here is where the difference between the two information feedback conditions came in. As mentioned above, in the full feedback condition subjects were informed about all four employees' effort levels while in the limited feedback condition they were only informed about the minimum effort.

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

[9] 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 for the fixed pairings than for the random pairings.

At the end of the session, each subject was paid in cash for all rounds played plus a show-up fee. Payoff was done on an individual and private basis. Recall that all payoffs are in "experimental pesetas" and that they were converted to dollars at a rate of one dollar equals five hundred experimental pesetas. An identical conversion rate was used between experimental pesetas and euros. This has the effect of yielding slightly higher marginal payoffs to coordination for Spanish sessions. Given that the conversion rate between dollars and euros was very close to 1 to 1 during the time period when sessions were run, we doubt that this had any impact on the results. Subjects in Cleveland received a show-up fee of ten dollars while the show-up fee was five euros in Barcelona. The larger show-up fee in Cleveland was deemed necessary to insure an adequate supply of subjects. There is no reason to believe that the differing show-up fees affected our results. The average total payoff was $20.52 in Cleveland and 14.23 € in Barcelona. Once we account for the differing show-up fees, the average earnings are almost identical across the two locations. These earnings were sufficiently large to generate a good supply of subjects in both locations.

5. Results

Figure 1 compares the evolution over time of average minimum effort at the firm level for the 6/10/10 bonus sequence with full and limited feedback. In the first ten rounds behavior is basically indistinguishable across the two conditions. Observe that when the bonus is increased to B = 10 in round 11 the average minimum effort increases by similar amounts in both conditions. However, under limited feedback it flattens out in round 12, while under full feedback it continues to increase. This is exactly the difference in patterns that is consistent with our hypotheses about the effect of limited feedback on the behavior of leaders and laggards after some employees attempt to push effort levels upwards; below we will see that effort data at the individual level confirm this. Observe also that the effect of pausing play to announce the (unchanged) bonus for the last ten rounds has an additional positive effect under full feedback but not under limited feedback.[10]

Figure 2 shows the same comparison between treatments as Figure 1, but with individual employee data on effort levels rather firm data on minimum effort levels. Here it is more

[10] The downward drops under full feedback in rounds 20, 29 and 30 are the result of the behavior of very few individuals, as will be evident from Figure 2.

apparent that, even with limited feedback, many individuals raise their effort levels in the first round following the bonus rate increase. The difference between limited and full feedback is that effort decays from round 12 onward with limited feedback. In contrast under full feedback the increase in round 11 are then followed by further increases as the leaders maintain their high effort levels and the laggards catch up. A similar, albeit less pronounced, pattern can be seen starting in round 21. One can also see how at the individual level the drops in rounds 20, 29 and 30 are minor.

Figure 3 makes it apparent how the different feedback treatments affect the behavior of leaders and laggards. The left side of this figure shows data from the 6/10/10 bonus sequence and the right side shows data from the 10/10/6 bonus sequence. A subject is classified as a "leader" if his effort in the preceding round was greater than the minimum effort for his firm. If his effort in the preceding round equaled the minimum effort for his firm, a subject is classified as a "laggard." Data is included for all rounds except the first round of each block.[11] The statistic being reported is the average change in effort.

Looking at the data from the 6/10/10 bonus sequence, Figure 3 tells a clear story. Changing the feedback has little effect on laggards – on average their effort increases slightly regardless of treatment. However, varying the feedback has a dramatic effect on leaders. For both treatments leaders tend to sharply decrease their effort, but the decrease is 75% larger on average in the limited feedback treatment. Because leaders quickly abandon their move to higher effort levels, it is unlikely that they will pull laggards up to a higher effort level. A similar pattern, albeit weaker, is seen in the 10/10/6 data.

Intuitively, this makes perfect sense. Overcoming coordination failure requires several things. First, there needs to be a coordinated move by several leaders to higher effort levels. Second, these leaders need to be willing to bear losses for a number of rounds while they wait for the laggards to follow. Finally, the laggards need to move higher effort levels. With limited feedback, the second step breaks down. Suppose an employee moves to a higher effort level but the minimum effort for their firm doesn't change. This employee can't tell whether the lack of change in the minimum effort is caused by a single laggard, in which case they might be inclined

[11] For round 1, there is no preceding round. More generally, in the first round of a block subjects are reacting to the restart and, when relevant, change in bonus rather than to whatever happened in the previous round.

to wait the laggard out, or if all the other employees are laggards. As a result, there are cases where a leader would have been persistent with full feedback but retreats with limited feedback. It is these cases that drive the general inability of firms to overcome coordination failure with limited feedback.

Figure 4 compares average minimum effort at the firm level for the two feedback conditions with the 10/10/6 bonus sequence. Here our focus is on what happens in round 21 and after, when the bonus rate is lowered to B = 6. Our hypothesis was that under limited feedback there would be a larger increase in coordination failure than with full feedback. The data in the figure suggest that this is not the case. There is no large difference between the treatments. If anything, there appears to be somewhat of a downward trend under full feedback, which is absent under limited feedback.[12]

Examining the corresponding employee level data, shown in Figure 5, the forces underlying the *lack* of a strong treatment effect with the 10/10/6 bonus sequence are actually quite similar to those driving the *presence* a strong treatment effect with the 6/10/10 bonus sequence. Consider play in round 1. There is little difference in the individual effort between full and limited feedback. However, in the limited feedback treatment, effort levels steadily decay over the next ten rounds while they hold largely steady with full feedback. This is a less dramatic version of the pattern seen in rounds 11 – 20 with the 6/10/10 bonus sequence – with limited feedback, the laggards pull other employees down to their low effort level. This is consistent with our hypothesis that limited feedback makes coordination difficult because high effort employees cannot tell that others are also choosing high effort, consistent with the data shown on the right hand side of Figure 3. Due to the differing initial effects of the two types of feedback, play across the treatments starts in very different places for the restart in round 11 and the bonus decrease in round 21. In both cases, there is a strong movement towards higher effort levels in the individual level data for the limited feedback treatment. Little movement is seen for the full feedback treatment, presumably because most groups are already doing rather well. As follows round 11 for the limited feedback treatment with the 6/10/10 bonus sequence, these attempts to move to higher effort levels quickly fail. Thus, the treatment effect that manifests

[12] The sawtooth pattern in rounds 21 – 30 of the limited feedback data is due to a single subject who oscillated between an effort of 0 and an effort of 30, causing his group to oscillate between minimum efforts of 0 and 20.

itself here is an initial gap between the two feedback conditions and a failure to close that gap despite repeated attempts. These effects are driven by the inability, with limited feedback, of firms to overcome their laggards.

On a broader level, it is notable across both treatments that a decrease in the bonus rate does not cause a corresponding decrease in effort levels. Once subjects are coordinated, they tend to stay coordinated even if the incentives to coordinate decline. Remarkably, this desire to coordinate is sufficiently strong that the bonus rate decrease triggers a substantial increase in individual effort levels for round 21 of the limited feedback treatment. The resilience of coordination explains why the predicted treatment effect couldn't possibly occur. We had hypothesized that there would be a steeper increase in effort levels for the limited feedback treatment because of an inability to overcome effort levels decreases by some employees. Since no such decreases take place, it is irrelevant what effect limited feedback would have had.

Beyond the interactions between feedback and changes in the bonus rate, a general point that emerges from our data is that the richer information available in the full feedback treatment converts a repeated coordination game into a more dynamic environment. Specifically, it leads to more history-dependence. The upper panel of Figure 6 shows that, for the case of limited feedback, behavior in the periods with B = 10 is remarkably similar across the 6/10/10 and the 10/10/6 treatments. The lower panel of Figure 6 shows the corresponding data for the case of full feedback. In this case a bonus of B = 10 leads to higher average minimum effort in the 6/10/10 than in the 10/10/6 treatments. In other words, with full feedback when a bonus of B = 10 is part of a new beginning it leads to better coordination than when it is just part of the status quo.[13]

The two panels of Figure 7 compare the periods with B = 6 across the two treatments for limited and full feedback respectively. The data shown differ in whether B = 6 is being used before or after 20 periods with B = 10. One can see that behavior is again more history-dependent under full feedback as there is a much larger difference in the average minimum effort level across the two treatments.

[13] We can reach similar conclusions by comparing behavior in the ten period overlap of rounds 11 – 20. In these rounds, both bonus sequences have B = 10 but differ in the bonus rate for the preceding block of ten rounds. Previous play with B = 6 leads to better coordination that previous play with B = 10.

The differences in history dependence are driven by differences in how the two treatments impact employees' ability to coordinate. In the full feedback treatment, strong efforts to coordinate are generally rewarded. It therefore matters whether the previous history of employees fosters especially concerted efforts to coordinate, as with the 6/10/10 bonus treatment. With limited feedback, attempts to coordinate tend to fail. It therefore doesn't matter that some individuals make a strong effort to coordinate following a history with B = 6.

To put our observations on the data on a firmer footing, we now turn to formal statistical analysis. In particular, we aim to verify that our preceding conclusions about the treatment effects are robust to controls for the location of the sessions as well as firm effects. Tables 3 and 4 show results from ordered probit regressions where the dependent variable is minimum effort for a firm. In all cases we use two ways of controlling for firm effects: clustering and random effects. These alternative approaches give us upper and lower bounds on the statistical significance of the estimated treatment effects.

The regressions in Table 3 estimate the treatment effects of the feedback. The dependent variables include a dummy for the location of the session, dummies for the time period, and interactions between the time dummies and a treatment dummy. Note the overlapping structure of the time dummies. This implies that we are estimating differences. For example, the parameter estimated for "Round $\geq$ 11" captures the difference in behavior between rounds 11 – 15 and rounds 6 – 10. Likewise, the interaction terms between the treatment dummy and the time dummies are capturing differences in differences. Thus, for the 6/10/10 treatment the parameter estimate for "Round $\geq$ 11 * Full Feedback" captures the treatment effect on responses to the change in bonus rate for round 11.

Starting with the results from the 6/10/10 treatment, shown in the left panel, we can see that full feedback does not have a significant impact on play in the first ten rounds. However, it does impact the response to increasing the bonus rate. With full feedback, the immediate response to the bonus rate increase is significantly larger. Moreover, there is a persistent effect as full feedback leads to significantly larger increases for rounds 16 – 20 and rounds 21 – 25 as well. The large treatment effect observed in Figure 1 is statistically confirmed.[14]

The right hand side of Table 3 shows the same regressions for the 10/10/6 treatment. Consistent with what we observed on Figure 3, the initial effect of full feedback is positive and statistically significant. After this, we generally see negative signs on the treatment effects to the extent that significant effects are observed, reflecting uncontrolled regression to the mean.[15] It is worth noting that the parameter estimate for "Round ³ 21 * Full Feedback" is not statistically significant. The response to a bonus rate cut does not differ significantly across treatments, although this largely indicates a general lack of response.

The regressions in Table 4 focus on history dependence. We ran separate regressions for limited and full feedback to identify whether history dependence is significant. The dependent variables include a dummy for the location of the session, dummies for the current bonus rate interacted with how long the bonus rate had been in use, and interactions between the preceding dummies and a dummy for whether a different bonus rate had been used previously. Unlike the regressions in Table 3, we are estimating levels rather than differences in Table 4. For example, the interaction terms between the dummy for the first five rounds of B = 10 and the dummy for previous use of B = 6, "6/10/10 * B = 10 * First 5 Rounds" captures the difference between the first five rounds of play with B = 10 in the 6/10/10 and 10/10/6 bonus sequences. The bases of comparison are the first five rounds with B = 6 and B = 10 respectively.

Looking at the parameter estimates, the more conservative clustering approach yields results that are consistent with the impression given by the data in Figures 6 and 7: there are no statistically significant history-dependence effects with limited feedback, but numerous history-dependence effects are significant with full feedback. With the more powerful random effects specification, we generally find some statistical significance for the history-dependence effects in the limited feedback data, but the parameter estimates are smaller and the statistical significance

[14] It is worth noting that the Barcelona dummy has a negative sign and is statistically significant with stronger controls for firm effects (e.g. the random effects specification). All relevant jokes about the Barcelona dummy can be forwarded to Professor Brandts.

[15] In particular, the fairly strong negative sign on the interaction between full feedback and the dummy for period 11 is driven by groups stuck at a minimum effort of zero moving to higher effort levels with the restart. Almost all of these groups are in the limited feedback treatment due to the initial treatment effect.

lower than what we observe with full feedback. The regressions support our conclusion that history dependence is stronger with full feedback.

6. Final remarks

The ability to observe others' actions is crucial in the process of overcoming coordination failure. By extension, it also may play an important role in the turning around of under-performing companies and other organizations. To re-coordinate at a better outcome, one needs both strong leaders and good followers. However, to be a strong leader it turns out to be very important to be able to observe the actions of other leaders. Our results strongly suggest that in the process of getting out of low efficiency traps, managers of organizations should disseminate information even about unsuccessful attempts to improve things to the rest of the organization. This allows leaders to know that they are not alone.

In the symmetric situation, involving a decrease in the incentives for coordination, things are different. The impact of observability on effort levels is initially large, but does not affect the reaction to a bonus rate cut. It appears that subjects in successful firms are influenced by the general insight that a 'signal' calling for worse coordination should not be followed and hence do not respond to a decrease in the bonus rate. Looking more carefully at the data, the main effect of the treatments is that limited feedback snuffs out what otherwise would be strong movement to higher effort levels. The same forces that drive the negative impact of limited feedback on attempts to overcome coordination failure are at work here as well. This again suggests that a policy of broad dissemination of attempts to increase effort should be followed.

Two concluding notes are in order. First, an unexpected feature of the data was that full feedback led to greater history dependence. In our earlier work, B&C, we have suggested that "shock therapy" is likely to be an effective strategy for firms attempting to overcome coordination failure – a short term increase in incentives can yield a long term increase in effort levels without necessitating permanently high payments to workers. To the extent that full feedback is not possible, this suggests that shock therapy of this sort is less likely to be effective.

Second, because we are looking at an explicitly dynamic setting, our results have implications for other dynamic settings where the stage game is not a coordination game. For example, consider indefinitely repeated competition among oligopolists. Even though the stage game typically resembles a prisoner's dilemma, the resulting supergame yields multiple Pareto ranked equilibria. This suggests that the issues of coordination raised by our experiments capture an important element of the strategic problem facing players.

The issue then is whether the availability of firm-specific data on actions and profits will lead to more or less competition. Standard wisdom from game theory would suggest that greater observability should help collusion by making it more difficult for firms to cheat on a collusive agreement, although the experimental evidence is at this point somewhat mixed.[16] Our results lend additional weight to the standard wisdom, since greater observability should help firms solve their coordination problem.

[16] See Vega-Redondo (1997) for an alternative theoretical viewpoint. In the experimental literature, Huck et al. (2000) present evidence that information about rivals' choices leads to significantly more competition when competition is in quantities, while the effect is not significant when competition is in prices. For homogeneous good quantity competition, Offerman, Potters, and Sonnemans (2002) observe that firms collude when they are provided with information on individual quantities, but not with information on individual profits. Bosch-Domènech and Vriend (2003) look for, but fail to find support for imitative behavior.

APPENDIX
INSTRUCTIONS FOR THE 6/10/10 TREATMENT WITH FULL FEEDBACK

General information: The purpose of this experiment is to study how people make decisions in a particular situation. From now on and till the end of the experiment any communication with other participants is not permitted. If you have a question, please raise your hand and one of us will come to your desk to answer it.

You will receive 500 pesetas for showing up on time for the experiment. In addition, you will make money during the experiment. Upon completion of the experiment the amount that you make will be paid to you in cash. Payments are confidential; no other participant will be told the amount you make.

Parts, Rounds and Groups: This experiment will have several *parts*. In Part 1 there will be 10 *rounds.* After these ten rounds have finished, we will give you instructions for the next part of the experiment. In each round you will be in a *group* with 3 other participants. The participants you are grouped with will be the same in all rounds.

Description of the Decision Task(*s*) in Part 1 of the Experiment: You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. In each week, each of the employees in each firm spends 40 hours at the firm. You have to choose how to allocate your time between two activities, Activity A and Activity B. Specifically, you will be asked to choose how much time to devote to Activity A. The available choices are 0 hours, 10 hours, 20 hours, 30 hours and 40 hours. Your remaining hours will be put towards Activity B. For example, if you devote 30 hours to Activity A, this means that 10 hours will be put towards Activity B.

For each round of the experiment you will receive a flat wage and a bonus that depends on the *minimum* number of hours spent on Activity A by a member of your group. For all rounds of this experiment, the flat wage equals 200 pesetas. The bonus rate, B, may vary between rounds. The bonus rate is selected by the firm manager. In this experiment, the firm manager is being played by the computer. We will always let you know the bonus rate before you choose how many hours to devote to Activity A.

Payoffs: The payoff that an employee receives in a round depends on the number of hours he chooses to spend on Activity A, the number of hours chosen by the others in his firm to spend on Activity A, and the bonus rate B selected by the firm manager. The payoff for the i^{th} employee of the firm, B_i, is given given by the formula below where H_i is the number of hours spent by the i^{th} employee of the firm on Activity A and $\min(H_A)$ is the *smallest* number of hours an employee of the firm spends on Activity A. You do not need to memorize this formula – the computer program will give you payoff tables at any point where you need to make a decision.

$$\text{ð}_i = 200 - (5 * H_i) + (B * \min(H_A))$$

The firm manager's payoff depends on the number of hours spent on Activity A by the employees of the firm and the bonus rate B. The firm manager's payoffs, π_F, is given by the following formula. Again, $\min(H_A)$ is the *smallest* number of hours an employee of the firm spends on Activity A. (Recall that the firm manager is actually being played by the computer. Nobody is actually receiving these payoffs.) Do not worry about memorizing this formula, as the program displays the firm's payoff function any time you make a decision and the computer automatically calculates the firm manager's payoffs for you as a part of the feedback you receive after each round.

$$\pi_F = 100 + (60 - 4B) * \min(H_A)$$

Playing a Round: For each round of the experiment, the computer will display a screen like the one shown below. The payoffs shown in the payoff table will be adjusted for the changing values of B. For the example below, we set B = 8. Notice that this is displayed above the payoff table.

Each employee will choose a number of hours to spend on Activity A using the buttons on the right hand side of the screen. You may change your choices as often as you like, but once you click on "Enter" your choice is final. Note that when you make your decision you will *not* know what the other employees in your firm are doing in the round.

At no point in time will we identify the other employees in your firm. In other words, the actions you take in this experiment will remain confidential.

[Insert Screen Dump Here]

<u>Information that you will receive</u>: After each round you will be informed about the number of hours you have spent on Activity A, the lowest number chosen by all of the employees in your firm, the firm's payoff, your payoff for the latest round, and your accumulated payoffs through the current round. You will also be shown the decisions by you and the decisions of all the other employees of your group from the current and previous rounds.

<u>Payment</u>: At the end of the experiment you will be paid, in cash, the sum of the payoffs that you will have earned in the rounds of the experiment. As noted previously, you will be paid privately and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

Payoff Quiz

Before we begin the experiment, please answer the following questions. For all of these questions, assume that B = 8. This gives employees the payoff table shown below. We will go through the answers to a sample problem before you do the rest of the quiz. Please raise your hand if you are having trouble answering one of the questions.

Bonus Rate = 8

Firm Payoff = 100 + 28* $\min(H_A)$

		Minimum Hours Spent on Activity A by Other Employees				
		0	10	20	30	40
My Hours on Activity A	0	200	200	200	200	200
	10	150	230	230	230	230
	20	100	180	260	260	260
	30	50	130	210	290	290
	40	0	80	160	240	320

Sample Question: Suppose you choose to spend 10 hours on Activity A. The other employees choose to spend 30, 20, and 40 hours on Activity A.

The minimum number of hours an employee of the firm spends on Activity A is ______

Your payoff is ________ pesetas.

1) Suppose you choose to spend 20 hours on Activity A. The other employees choose to spend 30, 0, and 10 hours on Activity A.

The smallest number of hours an employee of the firm spends on Activity A is ______

Your payoff is ________ pesetas.

2) Suppose you choose to spend 0 hours on Activity A. The other employees choose to spend 20, 30, and 10 hours on Activity A.

The smallest number of hours an employee of the firm spends on Activity A is ______

Your payoff is ________ pesetas.

3) I am grouped with the same three individuals for all thirty rounds of the experiment (True/False)? ________

4) My actions and payoffs will be confidential (True/False)? ________

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Figure 1
Effect of Feedback, 6/10/10 Bonus Sequence
Firm Level Data

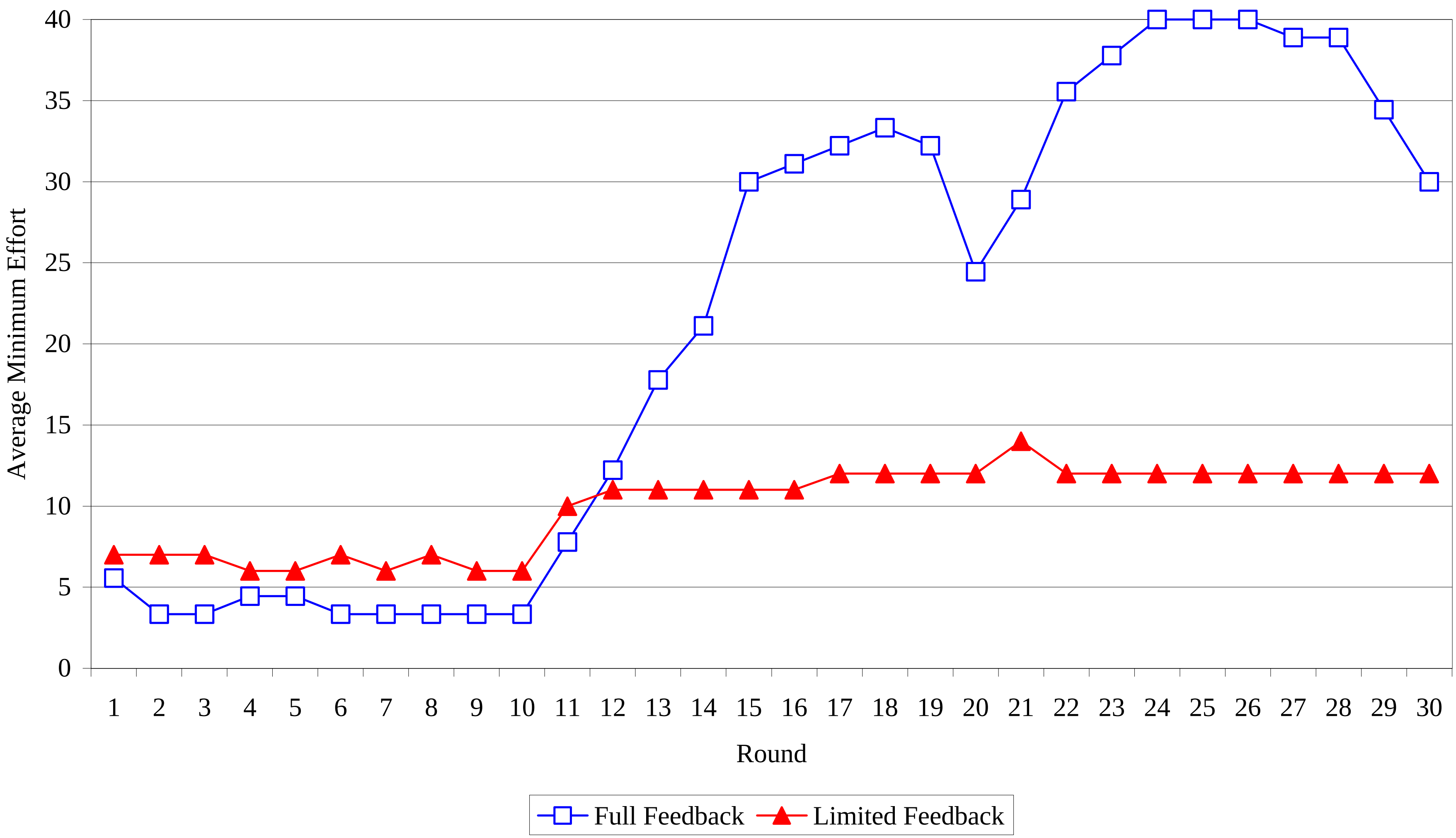

Figure 2
Effect of Feedback, 6/10/10 Bonus Sequence
Employee Level Data

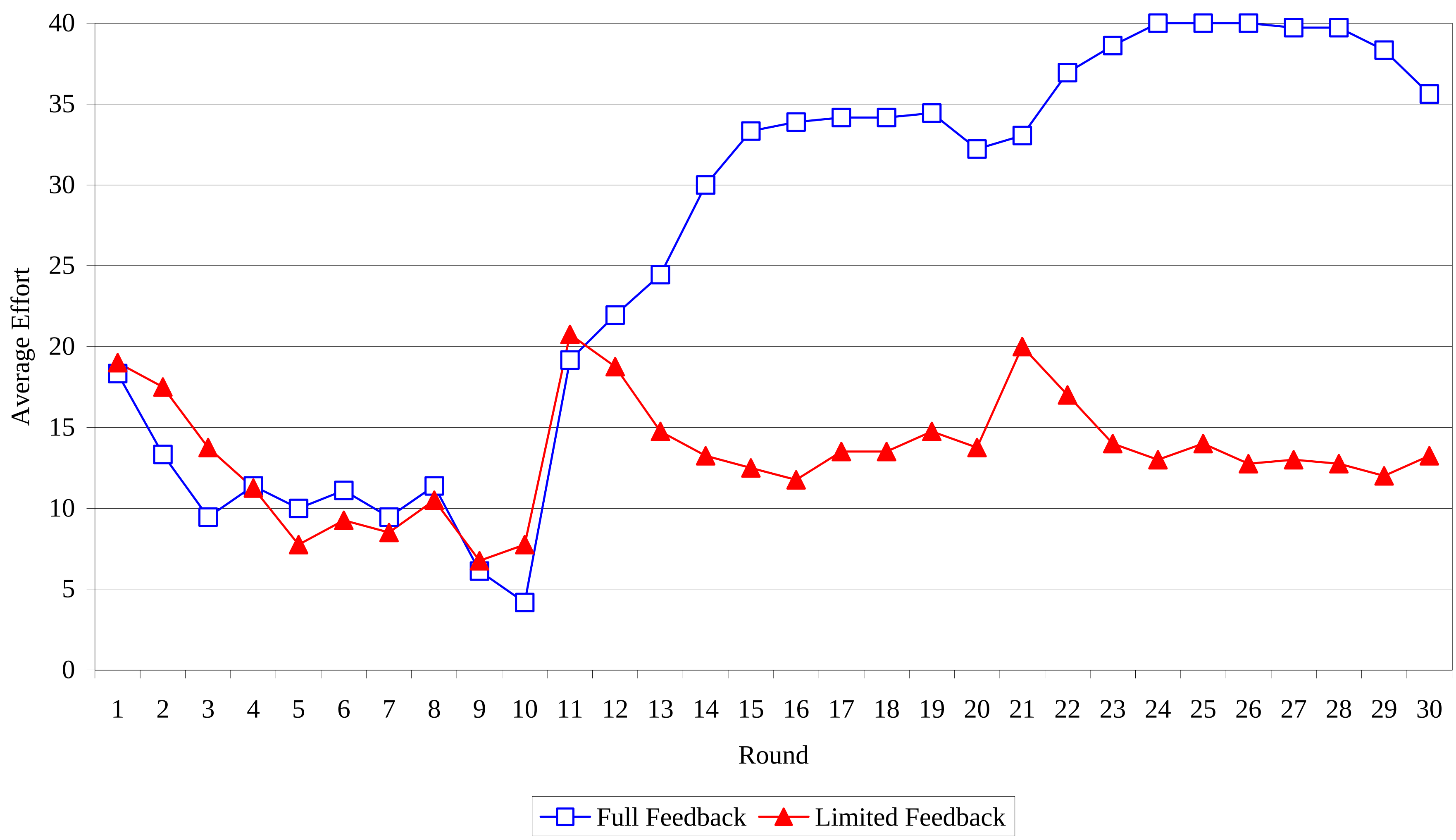

Figure 3
Effect of Treatment on Changes in Effort

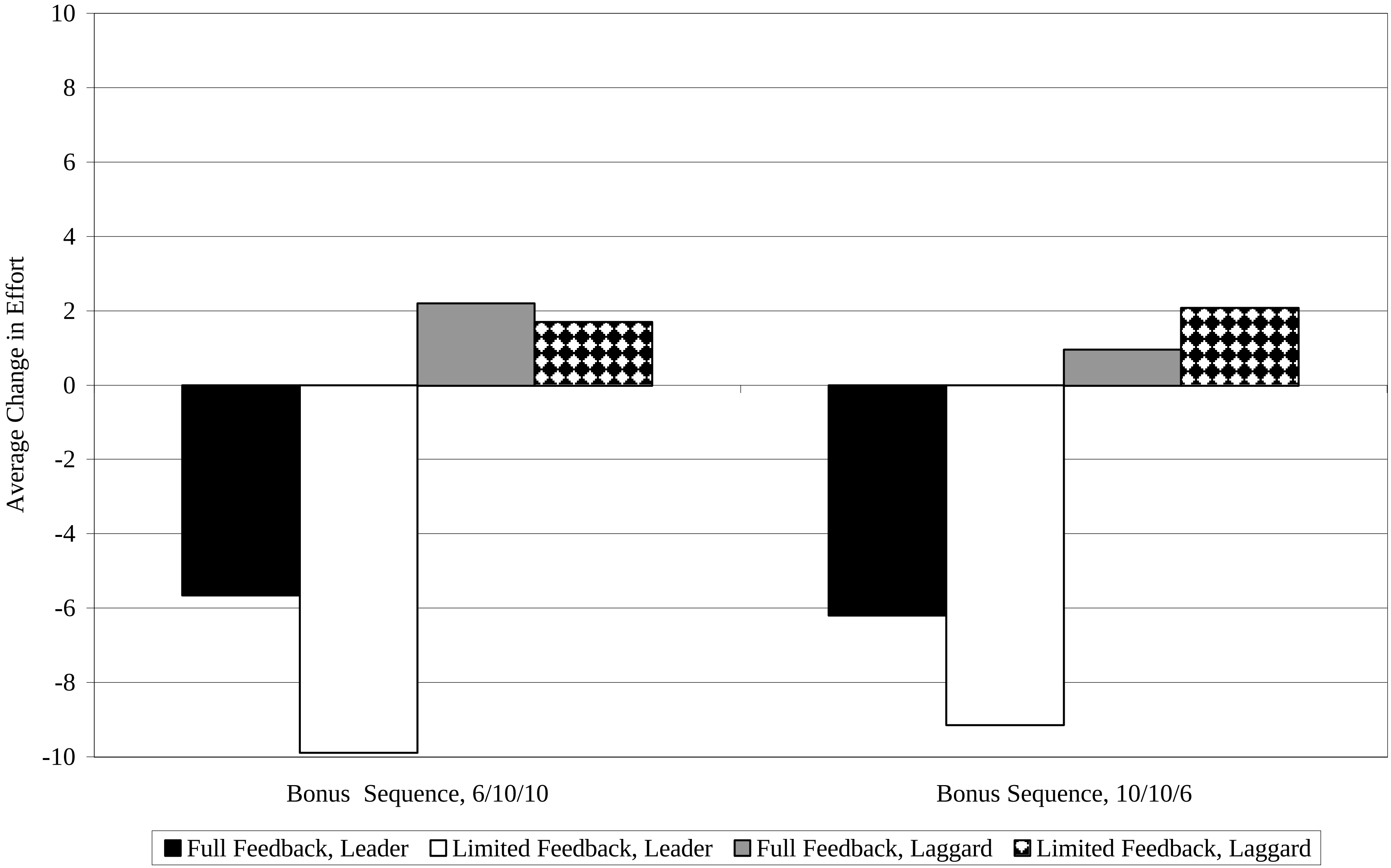

Figure 4
Effect of Feedback, 10/10/6 Bonus Sequence
Firm Level Data

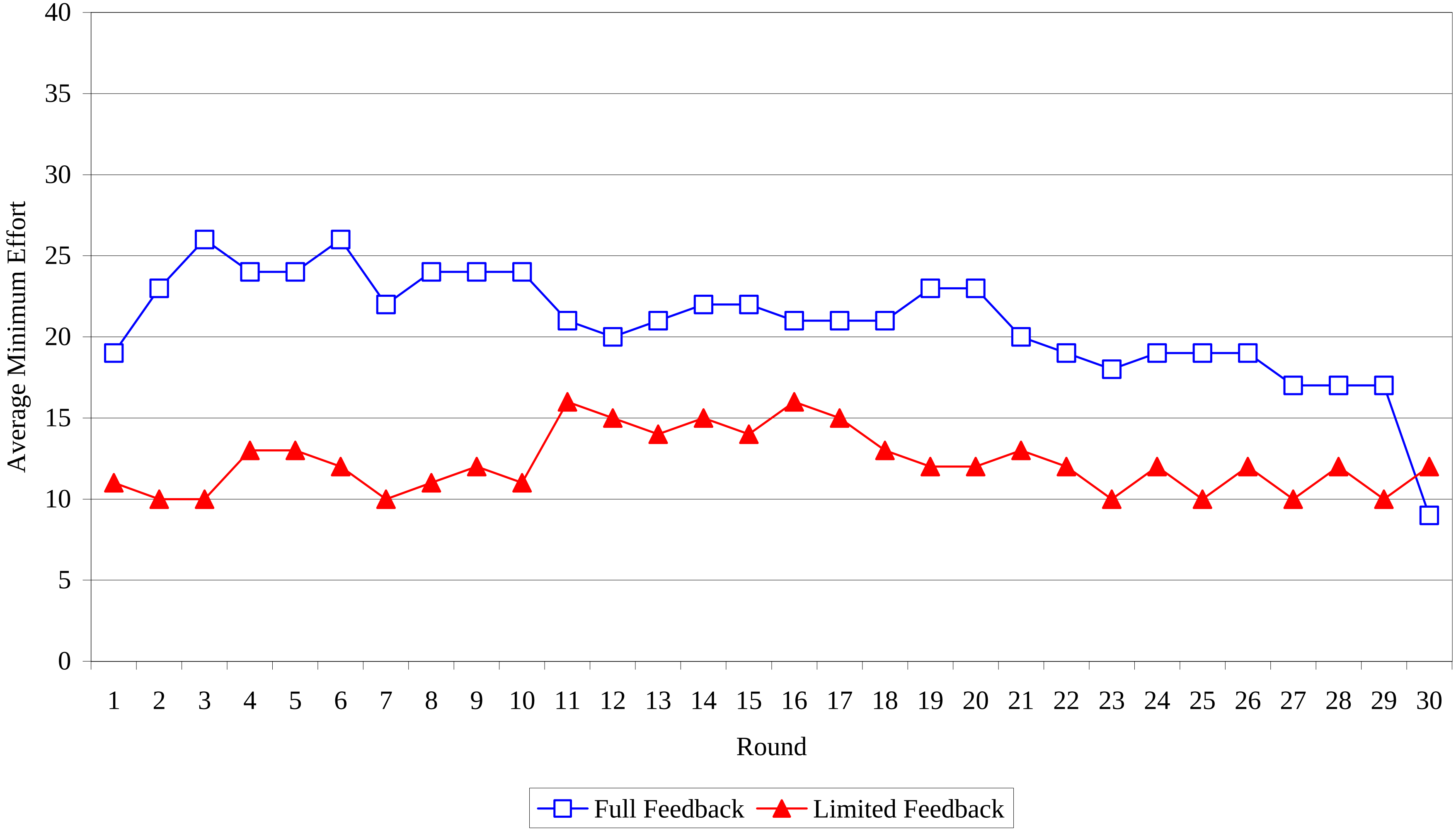

Figure 5
Effect of Feedback, 10/10/6 Bonus Sequence
Employee Level Data

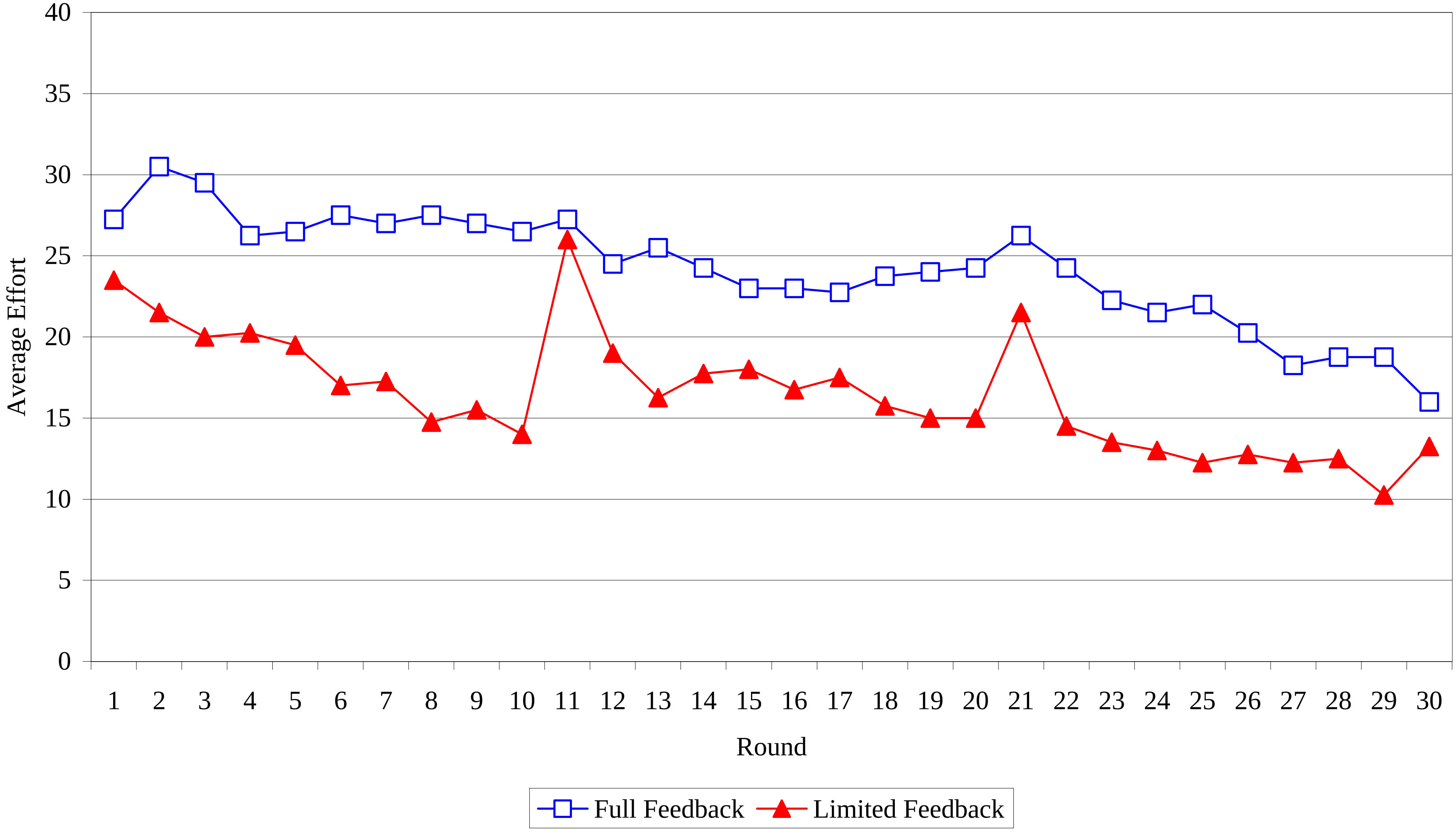

Figure 6
Effect of History Dependence on Minimum Effort, B = 10

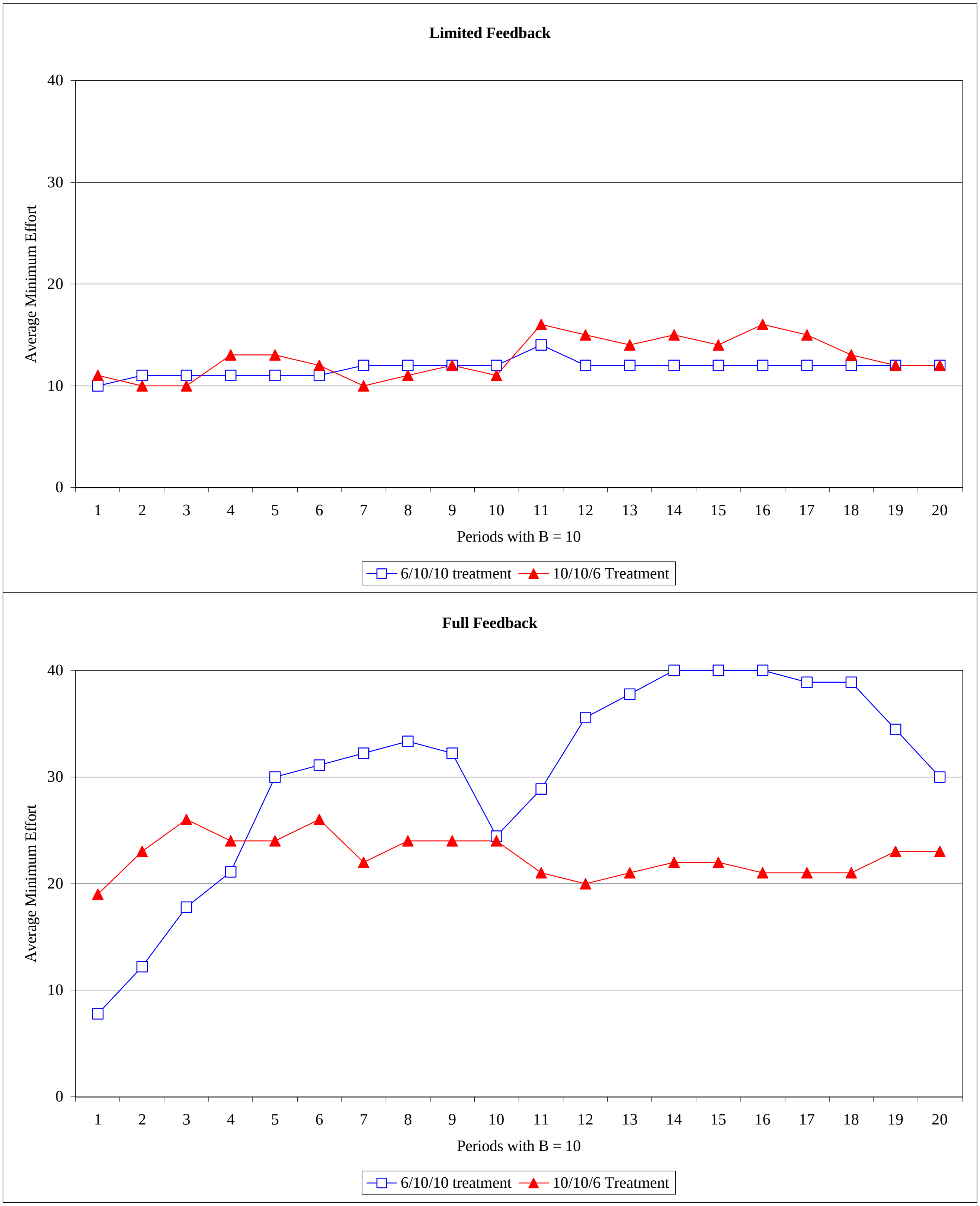

Figure 7
Effect of History Dependence on Minimum Effort, B = 6

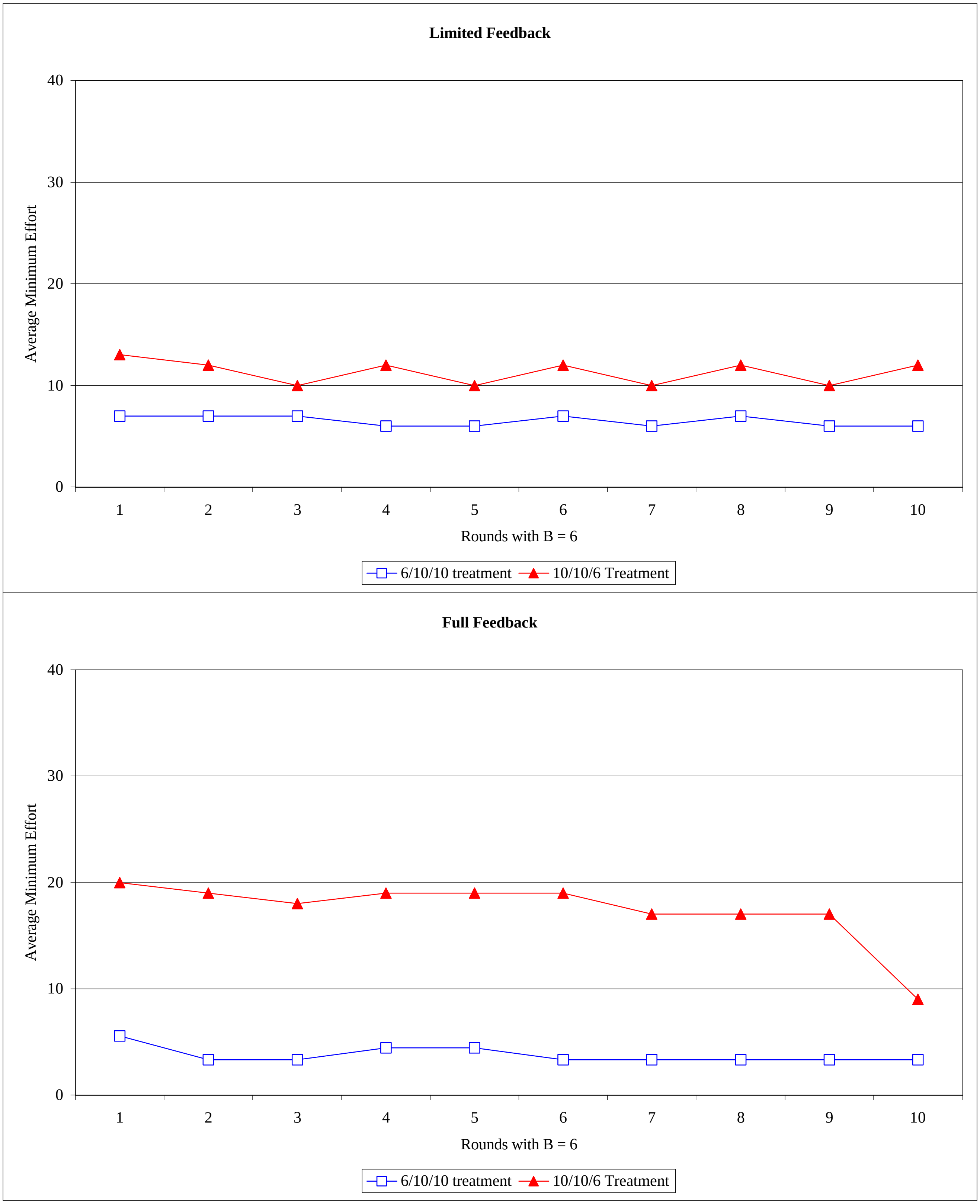

Table 3
Ordered Probit Regressions, Treatment Effect of Changing Feedback

Treatment	6/10/10		10/10/6	
Controls for Individual Effects	Clustering	Random Effects	Clustering	Random Effects
Round ³ 6	-.045 (.148)	-.047 (.171)	-.115 (.230)	-.065 (.128)
Round ³ 11	.591* (.339)	.565*** (.157)	.397* (.235)	.355*** (.130)
Round ³ 16	.109 (.101)	.114 (.132)	-.112 (.112)	-.111 (.125)
Round ³ 21	-.040 (.160)	.003 (.129)	-.232 (.251)	-.211* (.127)
Round ³ 26	-.048 (.038)	-.052 (.143)	-.027 (.029)	-.023 (.128)
Full Feedback	-.273 (.549)	-.216 (.160)	.844*** (.314)	.976*** (.131)
Round ³ 6 * Full Feedback	-.302 (.401)	-.269 (.246)	.174 (.269)	.219 (.190)
Round ³ 11 * Full Feedback	1.012* (.596)	.710*** (.229)	-.631** (.307)	-.674*** (.195)
Round ³ 16 * Full Feedback	.974*** (.315)	.690*** (.186)	.173 (.148)	.194 (.190)
Round ³ 21 * Full Feedback	.641** (.249)	.431** (.195)	.006 (.288)	-.072 (.187)
Round ³ 26 * Full Feedback	.101 (.355)	.119 (.214)	-.275** (.110)	-.357* (.189)
Barcelona	-.425 (.410)	-.709*** (.065)	-.661 (.448)	-.861*** (.063)
Log-likelihood	-614.93	-431.21	-805.7	-485.19
# Observations	569	569	600	600

*** Significant at 1% level
** Significant at 5% level
* Significant at 10% level

Note: Parameter estimates for the random effects specifications are normalized so the standard deviation of the error term equals one.

Table 4
Ordered Probit Regressions, History Dependence

Treatment	Limited Feedback		Full Feedback	
Controls for Individual Effects	Clustering	Random Effects	Clustering	Random Effects
B = 6 Second 5 Rounds	-.068 (.153)	-.077 (.134)	-.420 (.408)	-.358* (.203)
B = 10	.652 (.483)	.749*** (.116)	1.797*** (.425)	1.787*** (.168)
B = 10 Second 5 Rounds	-.100 (.225)	-.036 (.105)	.061 (.145)	.129 (.148)
B = 10 Third 5 Rounds	.271 (.197)	.276*** (.105)	-.157 (.301)	-.129 (.153)
B = 10 Fourth 5 Rounds	.157 (.245)	.160 (.103)	-.108 (.279)	-.069 (.150)
10/10/6 * B = 6 First 5 Rounds	.589 (.554)	.723*** (.120)	1.472*** (.499)	1.482*** (.168)
10/10/6 * B = 6 Second 5 Rounds	.632 (.624)	.781*** (.124)	1.601*** (.704)	1.501*** (.200)
6/10/10 * B = 10 First 5 Rounds	-.110 (.401)	-.274** (.109)	-.499* (.296)	-.752*** (.153)
6/10/10 * B = 10 Second 5 Rounds	.101 (.475)	-.119 (.112)	.581 (.464)	.013 (.148)
6/10/10 * B = 10 Third 5 Rounds	-.264 (.475)	-.345*** (.119)	1.405*** (.459)	.723*** (.212)
6/10/10 * B = 10 Fourth 5 Rounds	-.198 (.506)	-.284** (.119)	1.382** (.561)	.707*** (.205)
Barcelona	-.176 (.430)	-.022 (.052)	-.965** (.446)	-.641*** (.089)
Log-likelihood	-741.40	-380.36	-646.28	-486.06
# Observations	600	600	569	569

*** Significant at 1% level
** Significant at 5% level
* Significant at 10% level

Note: Parameter estimates for the random effects specifications are normalized so the standard deviation of the error term equals one.