It's What You Say Not What You Pay:

An Experimental Study of Manager-Employee Relationships
in Overcoming Coordination Failure

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Abstract: We study manager-employee interactions in experiments set in a corporate environment where payoffs depend on employees coordinating at high effort levels; the underlying game being played repeatedly by employees is a weak-link game. In the absence of managerial intervention subjects invariably slip into coordination failure. To overcome a history of coordination failure, managers have two instruments at their disposal, increasing employees' financial incentives to coordinate and communication with employees. Synthesizing methods drawn from psychology and economics, we quantify the impact of specific types of communication on workers' effort levels and manager's profits. This methodology allows us to rigorously compare the efficacy of communication and direct incentives in an environment where both are available. We find that communication is a more effective tool than incentive changes for leading organizations out of performance traps. Examining the content of managers' communication, the most effective communication strategy is quite simple: specifically request a high effort, point out the mutual benefits of high effort, and imply that employees are being paid well.

Keywords: Change, Incentives, Coordination, Communication, Experiments, Organizations

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1. Introduction.

Coordination has long been studied by behavioral game theorists,[1] but typically for cases in which the only players in the game are the agents attempting to coordinate. In many settings, there are important external agents who have a stake in whether coordination is achieved. If a firm earns low profits because of coordination failure among its employees, the management and shareholders suffer as well. If a country fails to develop economically because of coordination failure among industries, the government has a serious problem. It follows that these external agents have strong incentives to take actions that will lead to improved coordination. In this paper we study the role of external agents with a stake in the outcome, specifically managers of underperforming corporations, in moving organizations from coordination failure to successful coordination.

Our work focuses on the effectiveness of two prominent management tools used to influence behavior in such situations, financial incentives and communication. We study experiments set in a corporate environment, called the "turnaround game," where managers' and employees' payoffs depend positively on employees coordinating at high effort levels – the underlying game being played by employees is a weak-link game. In an initial phase where managerial intervention is absent, employees almost invariably slip into coordination failure. An additional subject is then introduced as a manager. This manager, faced with overcoming a history of coordination failure, can take action in two ways. First, he can change employees' incentives to coordinate by shifting some portion of the organization's surplus over to employees. From an economists' point of view this is an obvious tool to use. Second, he can communicate with employees. Studies in organizational behavior suggest that communication is one of the crucial variables that influence change.[2] Indeed, there is good reason to expect that communication will be particularly effective in organizations afflicted by coordination failure as this is primarily a problem of beliefs.

The weak-link game being played by employees is fairly simple and presumably after a few periods most subjects realize that mutual gains would occur if all employees switched to higher effort levels. However, no one employee can affect a positive change unilaterally. To be willing to choose a higher effort level, an employee must believe that others will also choose to increase their effort levels. In the face of a history of coordination failure such beliefs are understandably difficult to achieve. A manager's task therefore fundamentally

[1] See Camerer (2003, ch. 7) for a recent summary of this literature.

[2] For examples see Ford and Ford (1995) and Kotter (1996).

consists of finding a way to affect employees' beliefs about other employees' actions. Because communication can directly address employees' beliefs, it potentially serves as a potent tool for influencing employees' beliefs in a positive way.

Our experimental design systematically varies the avenues of communication available to managers and employees. In our baseline treatment, managers only control financial incentives and no communication is possible. In two other treatments we allow for one-way communication – managers can send messages to employees – and two-way communication – managers can send messages to employees and vice versa. The content of communication between managers and employees was completely unstructured as subjects could send any messages they desired subject only to minor restrictions.

A main feature of our work lies in a systematic analysis of the impact of the content of different types of messages. We recorded all of the messages and quantified the content using a systematic coding scheme, a common methodology in psychology studies that involve verbal protocols as well as in preceding studies from economic experiments that involve communication. We then adapt methods from econometrics to quantify the impact of specific types of communication on workers' effort levels and manager's profits. This combination of methods allows us to rigorously compare the efficacy of communication and direct incentives in an environment where *both* are available, precisely identify the impact of various types of communication on managerial payoffs, and characterize the most effective managerial communication style. Our goal is not to just establish that communication is a valuable tool for managers but to explain *how* communication improves managerial payoffs.[3]

The experimental results emphasize the importance of communication. As the available avenues of communication increase, both employees' effort and managers' profits increase. Communication is a more effective tool for increasing manager profits than financial incentives. The marginal profit from increasing incentives is actually slightly negative, as increased payments to employees more than consume the added revenue from increased effort by employees. In contrast, the most effective types of messages increase profits by over 30% on average

While managers try a wide variety of communication strategies, including complex multi-round plans, the most successful communication strategy is quite simple: Explicitly

[3] An alternative approach for comparing the efficacy of communication and direct incentives is to include a treatment where managers could communicate with their employees but no change in incentives was possible. However, this approach misses the interaction between incentives and communication that is a central feature of our experimental design and dataset. As shall be seen, incentives are a major topic of the communications and discussion of incentives plays a central role in an effective communication strategy.

request that all employees choose a high effort level, emphasize the mutual benefits of coordinating at a high effort level, and assure the employees that they are being paid well (although it is not necessary to actually pay them well). This is the central result of our paper – for managers attempting to overcome a history of coordination failure, it's what you say, not what you pay, that largely determines your success.

Successful communication from employees to managers takes a surprising form – the most valuable thing an employee can do is give advice to the manager on how he should approach the other employees. Because employees benefit from having the manager successfully coordinate their effort levels, they have strong incentives to offer useful advice. Managers in the two-way communication treatment therefore enjoy the benefits of group decision making.

Two secondary results of our analysis are worth noting. First, sessions for these experiments were conducted at two locations, Case Western Reserve in Cleveland and Universitat Autònoma de Barcelona in Barcelona. Barcelona managers perform significantly worse than Cleveland managers. This poor performance occurs primarily in sessions where managers can communicate with employees and reflects underlying differences in the types of messages sent by managers in the two locations. This result does not necessarily reflect cultural differences, as the two populations differ on a number of dimensions, but demonstrates how performance can vary between populations because of systematic differences in managerial communication strategies.

Second, we checked whether, controlling for differences in financial incentives, employees managed by a computer did worse than employees managed by another human absent any possibility of communication. We find no significant differences, giving us added confidence in the results of earlier studies where the manager is computer-controlled.

Beyond the specific results, several broad lessons can be drawn from our work. Our experiments demonstrate that an external agent, such as a manager, can play a central role in helping an organization overcome a history of coordination failure. This role consists primarily of acting as an unambiguous coordinating device. On first inspection our results also seem to indicate that financial incentives, the bedrock of most economic models, are less important for successful management in the turnaround game than elements of communication (or cheap talk) that economic models rarely include. This isn't exactly true – incentives are quite important in our experiments, but primarily in conjunction with communication. Directly raising incentives is a poor managerial strategy, but pointing out to employees that everyone can make more money if all employees work harder is quite

effective. Finally, our results illustrate how the relationship between economics and psychology is a two way street. The psychology and organizational behavior literatures suggested that communication would play a critical role in managerial leadership; econometric tools drawn from economics allow us to more rigorously measure how the impact of specific types of communication compare with those of changing incentives.

Section 2 introduces the turnaround game. Section 3 describes our experimental design and summarizes the primary research questions arising from our design. Section 4 presents our procedures. Section 5 reports the results and Section 6 concludes.

2. The Corporate Turnaround Game

An experimental firm in the corporate turnaround game consists of four employees and a manager. The manager's choice of the bonus leaves the weak-link structure of the game played by the employees unaltered, but changes the levels of the payoffs involved. The structure of the game played is governed by the following pair of basic design choices.

1) The firm's technology has a weak-link structure.

As described by Kremer (1993), for many organizations the individual (or unit) doing the worst job – the "weak link" – determines the overall productivity of an organization. As a prototypical example, imagine a firm producing via an assembly line where the slowest worker determines the speed of the entire line.[4]

Knez and Camerer (1994) argue that the game played within many firms takes on the specific form of the "minimum game" introduced by Van Huyck, Battalio, and Beil (1990) and refer to it by the more evocative term "weak-link game," a terminology that we follow. This is a type of coordination game. Each player simultaneously chooses an effort level. Effort is costly and a player's payoff is an increasing function of the minimum effort chosen by the players in the group. Payoffs are set up so that it is worthwhile for a player to raise his effort level if *and only if* it will increase the minimum effort for the group. Coordinating on any of the available effort levels is a Nash equilibrium for a weak-link game, but results from earlier experiments suggest that play often evolves towards the payoff dominated equilibrium where all players choose the lowest possible effort level.[5]

By studying a production technology with a weak-link structure, we focus on a worst-case scenario. Presumably many organizations face coordination problems in more forgiving

[4] For examples from field settings, see Knez and Simester's (2002) study of Continental Airline's turnaround and Ichniowski, Shaw, and Prennushi's (1997) work on steel production.

[5] See Van Huyck *et al* (1990), Knez and Camerer (1994), and Weber, Camerer and Knez (2004).

settings where positive change is easily achieved. However, if we can understand how to overcome coordination failure in organizations with a weak-link structure, a tough environment, it should be even easier to accomplish in less difficult circumstances.

2) Both managers and employees get limited information feedback.

We assume the firm manager observes the minimum effort selected (which is revealed by the firm's productivity), but cannot observe any individual employee's effort level. Likewise, employees observe their own effort and the minimum effort for the firm, but not the individual efforts of the other three employees. Limiting managers' information implies that they lack the necessary information to tailor bonuses to the effort put forth by individuals and can only offer bonuses based on the minimum effort over all employees.

Limiting the manager's information about employees' choices implies that, consistent with the spirit of most principal-agent models, he has difficulty monitoring them. There is no particular reason to believe that this assumption has either more or less external validity than assuming the manager individually monitors the employees, as both cases occur in reality.

We limit the employees' information feedback for three reasons. First, with full feedback there is asymmetric information between the manager and the employees since the manager can only see the minimum effort. While interesting, the potential sharing of information between employees and managers adds a layer of complexity to the experiments that we prefer to avoid. Second, we want to increase the difficulty of the manager's task in overcoming coordination failure. With limited feedback managers still have a chance to lead their group out of coordination failure, but the results of Brandts and Cooper (2006b) suggest it won't be easy.[6] Presumably lessons learned from such a harsh environment will be valuable in more forgiving settings as well. Finally, limiting the employees' information accentuates the importance of leadership by the manager. When employees can see the choices of other employees, leading by example often takes place.[7] One or more employees make a large increase in their effort levels presumably in the hope of leading laggards to match this effort, thereby overturning a history of coordination failure. This sort of internal leadership works reasonably well with full feedback, but can't work with limited feedback as laggards can't see the effort choices of putative leaders. Limited feedback therefore leaves managers as the primary source of potential leadership within the firm.

[6] On a related point, we use four employees rather than either a smaller or larger number to make it possible but not trivial to overcome a history of coordination failure. The results of Van Huyck et al (1990) suggest that coordination failure may not occur initially with smaller groups while overcoming a history of coordination failure may be nearly impossible with substantially larger groups.

[7] See Brandts and Cooper (2006b).

Turning to the specifics of the turnaround game, a round of the game starts with the manager setting a bonus rate (B) that determines how much additional pay each employee receives per unit increase in the minimum effort. Bonuses are based solely on the minimum effort, consistent with our assumption that the manager cannot observe individual efforts. We restrict the feasible bonus rates to the integers $B \in \{6,7,8,9,10,11,12,13,14,15\}$. Given the other parameter values used in our experiments, this is the set of (integer) bonus rates that neither make the choice of positive effort a dominated strategy nor allow the manager to lose money. The employees observe B and subsequently select effort levels. 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. Employee i's effort, E_i, is restricted to be in ten hour increments: $E_i \in \{0,10, 20, 30, 40\}$. The players' payoffs are then given by the following payoff functions:

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

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

The manager's payoff depends on the minimum effort selected by his employees, consistent with our assumption that the firm's production technology has the weak-link property. As can be seen in Equations 1 and 2, the bonus transfers a portion of the firm's revenues to its employees. Managers are limited to a linear bonus scheme.

To develop the basic theoretical properties of the turnaround game, we first focus on the proper subgame where employees choose effort levels after the manager has chosen a bonus rate. For all available values of the bonus rate the resulting subgame is a weak-link game. Coordinating on any of the five available effort levels is a Nash equilibrium. Given this multiplicity of equilibria in the subgame, subgame perfection has little predictive power for the full game. Any combination of bonus rate and minimum effort can be supported by a subgame perfect equilibrium.

To understand why overcoming coordination failure can be so difficult in this environment, consider the game induced by a bonus value of B = 6 (see top panel of Table 1). Suppose that the employees have previously all chosen effort level 0. An employee who thinks about raising his effort from 0 to 10 faces a certain payoff reduction of 50 pesetas due to increased effort, while his maximum possible gain is only 10 pesetas beyond the 200 pesetas he gets without risk by choosing 0. For the proposed increase to have a positive expected profit, the employee must believe the probability of the three other employees

simultaneously raising their efforts from 0 to 10 equals at least 5/6. Treating the other three employees as statistically independent, this translates into requiring a 94% chance of increased effort for each of the other three employees.[8] Given these grim incentives, overcoming coordination failure is quite unlikely with a low bonus rate.

(Insert Table 1 Here)

Now imagine that a new manager takes over the firm. Determined to shake the firm out of its underperforming ways, he raises the bonus rate to B = 14, the highest bonus rate at which the firm manager can earn a profit. This yields the payoff table shown in the bottom panel of Table 1. The incentives for employees to increase effort are strengthened by this change. Again suppose we start with all four employees choosing effort level 0. An employee increasing his effort from 0 to 10 still faces a certain loss of 50 pesetas, but the potential gain is now 90 pesetas. The break-even probability that the other employees will simultaneously increase their efforts is now 5/14. Assuming the other three employees are independent, this translates into requiring a 71% chance that each employee raises his effort. These are better odds than we saw with B = 6, but still daunting. Although one can imagine employees now at least attempting to overcome coordination failure, there is clearly room for the manager to play a leadership role beyond increasing the bonus rate.

3. Experimental Design

In this section we present our experimental design, outline specific research questions, and summarize the relevant theory. To understand the experimental design, it is necessary to realize that *our focus is not on comparative static results*. It is quite possible that subjects with no previous experience will generally converge to a more efficient equilibrium in the turnaround game with B = 14 than in the game with B = 6. This, however, isn't our point. Instead, we want to know if and how managers will overcome a history of coordination failure. The experiments are therefore designed so that employees will usually fail to coordinate prior to the possibility of managerial interventions.

At the beginning of a session, subjects were randomly assigned to either the role of an employee or a manager. Subjects then played thirty rounds of the turnaround game in fixed groups ("firms") of five: one manager and four employees. For the first ten rounds of the experiment the manager was strictly an observer; managers could see the same round by round information feedback that they would receive in later rounds, but could neither control the bonus rate nor communicate with employees. Managers were not paid for these rounds,

[8] To derive this probability, solve for p such that $200 = 150*(1-p^3) + 210*p^3$.

although employees and managers were both shown the payoffs that the manager would have earned. The bonus rate was fixed at B = 6 for the first ten rounds, this being the lowest (integer) bonus rate that does not make choosing positive effort levels a dominated strategy. The goal was to get a high percentage of firms coordinated on the inefficient outcome with minimum effort equal to zero. This seemed likely given the results of previous experiments (Brandts and Cooper, 2006a and 2006b).

For the remaining twenty rounds the manager actively managed his firm. The employees were informed when the manager took over control of the firm. In all treatments the manager was then responsible for choosing a bonus rate in each round and received payoffs as shown in Equation 1.

(Insert Table 2 Here)

Table 2 summarizes the treatments in our experimental design. The primary treatment variable in our sessions is what type of communication was possible between a firm's manager and employees. The three communication conditions we studied are:

- No Communication.
- One Way Communication. At the same time that firm managers selected a bonus rate they were given a text box that could be used to type a message to their employees. These messages were shown to the employees, along with the bonus rate, prior to the employees choosing effort levels. The messages were cheap talk as any commitments made in the communication stage were non-binding.
- Two Way Communication. Managers could send messages as above. Employees were given a similar text box to send a message to the manager at the same time as they chose an effort level. These messages were sent only to the manager, not to the other employees. As above, messages were cheap talk.

As a secondary treatment, we also report results from sessions where the firm manager was played by the computer with bonus levels determined exogenously for all rounds. This treatment controls for whether using a subject as the manager as opposed to the computer affects employees' choices independent of financial incentives. This is an important methodological issue if the current study is to be compared with earlier work using a computer as the manager (e.g. Brandts and Cooper, 2006a and 2006b). Employees in these sessions knew that the manager was always the computer rather than another subject. For these sessions the bonus rate equaled 6 for the first ten rounds and 10 for the remaining 20 rounds, similar to the average bonus level chosen in the sessions with human managers.

Our subjects were undergraduate students from Case Western Reserve University in Cleveland and from either Universitat Pompeu Fabra or the Universitat Autònoma in Barcelona.[9] While the use of two populations was not intended as a treatment per se, there are a number of differences in behavior between the two locations that will be described in the results section. Because of a related project using EMBAs as subjects, we needed to run most of the one-way communication sessions in Cleveland. We therefore made no attempt to balance the sessions between the two locations, instead relying on statistical analysis to control for location effects. We ran at least 5 experimental firms for each treatment in each location. We believe this is sufficient to statistically identify any location effects. We ran at least 20 experimental firms for each of the treatments with human managers.

Our experimental design is intended to address five specific research questions; in the results section we will present five corresponding regularities.

QUESTION 1: In the absence of communication and holding the bonus rate fixed, will minimum effort be significantly different with human managers than with computer managers?

In answering question 1, we might conjecture that, absent communication, the level of the bonus is the primary force driving employees' effort levels regardless of whether it is set by man or machine. However if play with human managers taps into emotions that aren't present with computer managers, then effort levels are likely to depend on whether the manager is a fellow subject.[10]

QUESTION 2: Will more avenues of communication lead to higher minimum effort holding financial incentives fixed? Specifically, will average minimum effort be greater with one way communication than with no communication (and human managers)? Will average minimum effort be greater with two way communication than with one way communication?

QUESTION 3: Ceteris paribus, will increased financial incentives (e.g. higher values of B) lead to higher minimum effort?

QUESTION 4: Which communication strategies will be most effective in increasing the minimum effort?

[9] Barcelona sessions with computer managers were run at UPF and all other Barcelona sessions were run at UAB. There is little difference between the student bodies at the two universities.

[10] There exists a large literature on experimental labor markets demonstrating a positive relationship between wages and effort (Fehr and Falk, 2002). This bilateral gift exchange is presumably due to reciprocity, as employees reward a higher wage with greater effort. Note that the environment being studied in this literature is quite different from that studied here, as there is typically only one employee and effort is a strictly dominated strategy. The focus is on issues of trust and trustworthiness rather than coordination as is the case here.

QUESTION 5: Will firm managers' choices of financial incentives be more important in determining their profits than their choice of communication strategies?

Questions 2 – 5 cut to the heart of the matter. With respect to question 2, the relevant theory literature concerns the effect of adding cheap talk to games. Applying subgame perfection, adding cheap talk does not change the set of pure strategy equilibrium outcomes since a babbling equilibrium can be used to support any outcome generated by an equilibrium without cheap talk. In general, the effect of adding cheap talk to a game is to expand the set of Nash equilibria to include all correlated equilibria of the original game (Myerson, 1991). This is of little use when, as in our case, the equilibrium set is already too large. We need an approach that refines the equilibrium set rather than expanding it.

One approach to refining the equilibrium set is the evolutionary approach of Kim and Sobel (1995). For a class of games including weak link games, Kim and Sobel prove that only the efficient outcome is stable for an adaptive process in which players gradually switch to new strategies that are weakly payoff improving. Kim and Sobel's result can be extended to the game with manager communication, but the evolutionary nature of the model limits its applicability to our experiments. Subjects in the laboratory only have a limited amount of time to reach equilibrium, so an argument that relies heavily on a slow process of drift is unlikely to have much predictive value.

An alternative method for refining the equilibrium set draws on arguments related to forward induction. Messages suggesting a certain vector of strategies can be *self-committing*, in the sense that the sender has an incentive to do what he said in his message if he expects it to be believed or, even more strongly, *self-signaling*, in the sense that the sender only wants the message to be believed if he is truthful (Aumann, 1990; Farrell and Rabin, 1996). Unfortunately, these attractive refinements cannot be directly applied to the weak link game. This failure occurs because there are more than two employees. To see this consider a simpler case than the game we study – suppose there is no manager and employees communicate among themselves. Imagine one employee sends a message stating he intends to choose the highest effort level and expects all others to do so as well. It is incentive compatible for him to choose the highest effort level if he expects all others to do so as well. However, to get the other three employees to choose the highest effort level it is not sufficient that they believe the sender's claim that he will choose the highest effort level. They must also believe that the other two employees who received the message will do likewise. Thus, the message is not self-committing. Similar problems defeat attempts to directly apply these concepts to the more complex case of messages between managers and employees since no

one player, even if believed, can unilaterally change the incentives of other players by credibly committing to a strategy.

Although the formal theory gives little guidance about the answer for Question 2, it seems intuitive that the babbling equilibria will not emerge. While the theory assigns no a priori meaning to the various messages, real languages (English, Spanish, or Catalan) are naturally meaningful. If a manager says, "All employees choose 40," it seems unlikely that this will be interpreted identically to the manager saying, "Peas and carrots." While it need not follow that employees will all choose effort level 40 when the former message is sent, it does suggest that the manager's message can naturally be used as a tool for making the efficient equilibrium a focal point. We therefore conjecture that the answer to Question 2 will be positive.

This conjecture is reinforced by existing experimental results. There exist a number of experiments where coordination is improved by the possibility of communication (e.g. Cooper, DeJong, Forsythe, and Ross, 1992; Blume and Ortmann, forthcoming; Capra, Tanaka, Camerer, Munyan, Sovero, Wang, and Noussair, 2005). As such, our expectation was that communication would be helpful. Our interest lies more in how communication helps rather than whether it helps.

Simple economic intuition suggested a positive answer for Question 3 – higher incentives lead to higher effort – but results from our earlier study with computer managers (Brandts and Cooper, 2006b) suggest otherwise. We had no strong prior about the answer to Question 4. A recent study by Charness and Dufwenberg (forthcoming) analyzes the content of communication in a trust game with hidden action. They classify their subjects' messages into three rough categories – promises, empty talk and no message – and find that promises from trustee to truster enhance trust and efficiency. Our content analysis allows us to study the effect of promises, and ex ante we thought these might be influential. However, our environment is quite different from theirs and our detailed content analysis allows for numerous other possibilities so there is little reason to believe their results will directly apply to our case. Finally, an answer to Question 5 above is not easy to anticipate. Superficial economic intuition would suggest that incentives are more important than a good communication strategy, since changing incentives affects the mathematical structure of the game in ways that communication cannot. However, given the effectiveness of communication in other settings and the insensitivity to relative incentives observed previously for the turnaround game, this point is far from obvious prior to seeing the data.

4. Procedures.

All sessions were run on a computerized network. Sessions with subjects as firm managers were run using a web application specifically developed for this purpose while the sessions with computer managers used a z-tree program. There are minor differences in how information is presented between the programs (e.g. the screens are laid out in a slightly different fashion).

Instructions were framed in neutral but naturalistic terms. Subjects received instructions and payoff quiz questions for both roles. Subjects were told that in the role of employees they must allocate 40 hours a week, in ten hour blocks, between Task A and Task B. Task A is equivalent to effort, but this term, due to its possibly strong connotations, was never used with subjects through the instructions or otherwise. Subjects knew the number of rounds in the experiments and knew in advance when the bonus would be set by the computer and when it would be set by another subject acting as manager. Subjects were reminded of this information throughout the course of the experiment.

In the one and two-way communication treatments managers were given – at the same time that they were asked to choose a bonus level - a text box in which they could type a message. Subjects were given no instructions about the content of the messages except that they could not identify themselves or use offensive language. Although we did not monitor messages in real time, our ex post reading of them indicates that subjects universally abided by these restrictions. Subjects were given no time or length limit on entering messages. Indeed, some of the messages were quite long and took a lengthy time to type. Once the manager was finished, whatever message he wrote was sent to all the employees in the firm at the same time they saw his choice for the bonus rate.

In the two-way communication treatment, employees were given a text box in which they could type a message at the same time that they chose a bonus level. Their instructions about messages were identical to those given to managers. Employees' messages were sent to their manager, appearing at the same time the manager saw the minimum effort chosen by their employees in the previous round. Employees could *not* send messages among themselves. The manager could not identify which employee was specifically responsible for a particular message as messages were randomly ordered and displayed without any identifying information.

Payoffs were converted from 'experimental pesetas' to dollars/euros at a 500:1 ratio. Average payoffs were $24.37 in Cleveland and 16.59€ in Barcelona. Much of this difference come from the need for a larger show-up fee in Cleveland ($10) to attract subjects than in

Barcelona (5€). Given that the average exchange rate during the period experiments were being run was about $1.15/1€, the marginal cost of effort and the marginal benefit of increasing minimum effort were somewhat higher in Barcelona. Sessions for this experiment typically lasted about ninety minutes.

For one session with one-way communication, containing four firms, the messages were accidentally not saved. The software worked correctly for this session, so all the data on subjects' choices that was saved has been included in our dataset, but obviously data from this session is not reflected in any content analysis of managers' messages.

5. Results

Before getting into the data, it is useful to reiterate our terminology. A "firm" refers to a fixed grouping of four employees and a manager. "Firm-level" data consists of a single observation per round per firm, the minimum effort chosen by the four employees of the firm. Throughout this paper, unless otherwise specified, we are using firm-level data. Unless otherwise noted, tests for whether the difference between two means is statistically significant are t-tests with a correction for unequal variances.

A. Treatment Effects: For our treatments to be interesting, it is necessary that a history of coordination failure be established in the first ten rounds. The data clearly meets this pre-condition. The average minimum effort falls from 7.09 in round 1 to a paltry 2.37 in round 10. Given that minimum effort is drawn from the set {0,10,20,30,40}, the averages we observe are quite low. The minimum effort is zero in round 10 for 77 out of a total of 86 firms. When human managers take over in round 11, the need for a turnaround is almost always present.

Table 3 summarizes the average bonuses, minimum efforts, and payoffs for rounds 11 – 30 of the different treatments.[11] Standard errors are given in parentheses. Given that average bonus rates are similar across treatments, Table 3 provides a first impression of the answers to Questions 1 and 2.[12]

(Insert Table 3)

The average minimum effort is higher for the computer manager treatment than the no communication treatment (11.8 vs. 4.1). This suggests the answer to Question 1 is positive,

[11] An observation in Table 3 consists of the mean value for a single firm over rounds 11 – 30.

[12] The difference between average bonus rates is not significant at the 10% level comparing the no communication and one way communication treatments (t = 0.04) or the one and two way communication treatments (t = 1.36). The average bonus rates for the no communication and one way communication treatments are significantly different at the 5% level from the bonus rate of 10 used for the computer manager sessions (t = 2.75 and t = 2.06 respectively).

especially since the null hypothesis that these means are equal can be rejected at the 10% level (t = 1.83). However, the preceding test result is misleading as differences between these two treatments are largely driven by random differences in minimum efforts for round 10.

This point can be seen by comparing Figures 1 and 2. Figure 1 shows the evolution of average minimum effort for the four main treatments, using data from all firms, while Figure 2 only includes experimental firms for which coordination failure occurred in round 10 (e.g. minimum effort equals 0 in round 10). The latter are the most pertinent data, since those few groups that do not fall into coordination failure fail to satisfy the precondition for our study of turnaround. Consistent with Table 3, Figure 1 displays a sizable and persistent difference between the computer manager and no communication treatments. However, consider the data from round 10 shown at the left of Figure 1. By chance, the average minimum effort in round 10 is highest for the computer manager treatment and lowest for the no communication treatment. In Figure 2, where the playing field is leveled by only considering firms that start from a history of coordination failure, the difference between the computer manager and no communication treatments largely vanishes. Regression analysis reported in Table 4 makes this point more formally, showing that the difference between the computer manager and no communication treatments is not statistically significant with controls for the minimum effort in round 10.

(Insert Figures 1 and 2)

REGULARITY 1: Controlling for the average minimum effort in round 10, there is little difference between the average minimum efforts in rounds 11 – 30 for the computer manager and no communication treatments.

Returning to Table 3, a comparison of the three treatments with human managers indicates that we can answer Question 2 affirmatively – additional avenues for communication lead to higher effort levels (holding bonus rates approximately constant).[13] Moreover, greater possibilities for communication lead to higher earnings for both managers and employees.[14]

Comparing Figures 1 and 2, this conclusion holds even if our attention is limited to only those firms that start from a history of coordination failure. Looking at Figure 2, note

[13] The null hypothesis that the mean minimum effort is equal for the no communication and one way communication treatments is rejected at the 1% level (t = 3.14). Equality of the mean minimum efforts for the one and two way communication treatments is rejected at the 10% level (t = 1.97).

[14] Equality of manager's payoffs for the no communication and one way communication treatments is rejected at the 1% level (t = 2.95). We cannot reject the null hypothesis of equal managerial payoffs for the one and two way communication treatments (t = 0.85). Equality of employee's payoffs is rejected at the 5% level between the no communication and one way communication treatments (t = 2.46) as well as between the one and two way communication treatments (t = 2.24).

how much larger the difference is between the one and two way communication treatments than between one way communication and no communication.

REGULARITY 2: Increased avenues of communication lead to higher minimum effort and higher payoffs for all parties.

To determine whether the preceding conclusions are robust to controls for location (Cleveland or Barcelona) and initial conditions, Table 4 presents the results of regressions on firms' minimum efforts in rounds 11 – 30. The dependent variable is a firm's average minimum effort between round 11 and the final round of play. Each firm appears as a single observation in the data set. There are 22 firms that had a minimum effort of 0 in all relevant rounds. To correct for the left censored data that results, we use a tobit specification. The first line of equation 3 gives the unrestricted version of the equation being estimated for the latent variable with the remaining two lines specifying how the latent variable is converted into the observed value. The values of the constant (α) and the coefficients (β_1, β_2, β_3, β_4, β_5, and β_6) are parameters to be estimated. The variable ε is a normally distributed iid error term. This error term captures any natural variation in the experimental population that the exogenous variables cannot account for.[15] A discussion of tobit regressions, including the underlying assumptions can be found in Greene, 2000 (pp. 908 – 911).

$$\begin{aligned} \text{MinEffort}^{*} &= \alpha + \beta_1 D_H + \beta_2 D_C + \beta_3 D_{TW} + \beta_4 D_B + \beta_5 \text{MinEffort}_{10} + \beta_6 D_B D_C + \beta_7 D_{SS} + \varepsilon \\ \text{MinEffort} &= 0 \text{ if MinEffort}^{*} \le 0 \\ \text{MinEffort} &= \text{MinEffort}^{*} \text{ if MinEffort}^{*} > 0 \end{aligned} \qquad \text{(eq. 3)}$$

The base in these regressions is data from the computer manager treatment. All four models include three variables measuring the treatment effects: a dummy for all treatments in which the manager was a subject (D_H), a dummy for the two treatments with communication (D_C), and a dummy for the two-way communication treatment (D_{TW}). The first of these three dummies captures the difference between the computer manager and no communication treatments, the second measures the difference between the no communication and one-way communication treatments, and the final dummy quantifies the difference between the one and two way communication treatments. Inspecting Table 3, a consistent pattern emerges of lower minimum effort levels for data gathered in Barcelona. This could reflect a number underlying differences between the two subject populations, including cultural differences, differences in the selectivity of the universities, and differences in the socioeconomic status of students at the two campuses. To control for location effects, a dummy for data gathered

[15] For example, the risk attitudes of subjects (for which we have no direct measures to use as controls) can play an important role in determining their willingness to attempt a movement to higher effort levels.

in Barcelona (D_B) is included in Models 2 – 4. The location effect appears to be more pronounced in treatments with communication. Models 3 and 4 therefore include an interaction between the dummy for data gathered in Barcelona and the dummy for treatments with communication (D_B * D_C). Firms that start with a minimum effort above zero tend to keep a minimum effort above zero. Without appropriate controls, good luck in drawing firms that have a positive minimum effort in round 10 can be confused with a positive treatment effect. To control for firms' differing initial conditions, Models 2 - 4 include the minimum effort in round 10 ($MinEffort_{10}$) as an exogenous variable.

Five firms in our sample failed to complete the full 30 rounds. These firms came from two sessions. One ended prematurely because a construction worker accidentally cut the power to the building. The other session started late because of network problems, and two of the four firms were not finished at the end of the session's timeslot. The number of rounds completed is endogenous, as different firms within a session go at different speeds, but being in a session where firms couldn't finish all thirty rounds is clearly exogenous. Model 4 therefore includes a dummy for the two short sessions (D_{SS}) as a proxy for the number of rounds completed.

(Insert Table 4)

The results of Model 1 parallel the t-tests reported above as expected given the lack of controls for location and starting conditions.[16] Model 1 serves as a point of comparison for models that include controls. The negative estimate for human managers (D_H) indicates that minimum effort is higher in the computer manager treatment than the no communication treatment. This result is weak, only achieving statistical significance at the 10% level. The estimated difference between minimum effort in the no communication and one way communication treatments (D_C) is both larger and more statistically significant than the estimated difference between the one and two way communication treatments (D_{TW}). The preceding results change in Model 2 when controls are added for location (D_B) and initial conditions ($MinEffort_{10}$). These variables are statistically significant at the 5% and 1% level respectively. With controls for location effects and initial conditions, the parameter estimate for sessions with subject managers (D_H) is halved and is no longer statistically significant. This confirms Regularity 1. The parameter estimate for communication (D_C) is almost halved and is only significant at the 10% level while the parameter estimate for two way communication (D_{TW}) jumps in magnitude and is now statistically significant at the 1% level.

[16] Identical results are obtained if we use OLS rather than a tobit specification.

The results of Model 2 are consistent with our interpretation of Figure 2 – controlling for locations effects and initial conditions, the impact of adding two way communication is greater than the effect of allowing one way communication.

Model 3 examines whether the location effect is present across all treatments or is tied to the availability of communication. The parameter estimate for sessions run in Barcelona (D_B) becomes small with the inclusion of the interaction term between location and contest and is no longer statistically significant. The estimated coefficient for the interaction term ($D_B * D_C$) is large and significant at the 5% level. The results of Model 3 confirm that the location effect comes almost entirely from sessions where communication is available.[17]

Model 4 checks whether the preceding results are driven by firms that were unable to complete the full thirty rounds. The parameter estimate for short sessions (D_{SS}) is tiny and far from statistical significance. This is unsurprising since average minimum effort levels change little over the final ten rounds (see Figures 1 and 2).[18]

The results on Table 4 establish treatment effects for average minimum effort, but Regularity 2 also refers to managerial and employee payoffs. To check the treatment effects for managerial payoffs, we ran a tobit regression identical to Model 2 except the dependent variable is average payoff for the manager over rounds 11 - 30 rather than average minimum effort.[19] The regression results indicate that manager's payoffs are significantly higher when communication is allowed ($t = 1.93$; $p < .10$) as well as with two way communication as compared to one way communication ($t = 2.64$; $p < .05$). Managers' payoffs are significantly lower for firms run in Barcelona ($t = 2.74$; $p < .01$). To estimate treatment effects on employee payoffs we ran an OLS regression identical to Model 2 with employees' average payoff over rounds 11 – 30 as the dependent variable.[20] Allowing communication does not significantly increase employees' payoffs ($t = 1.19$; $p > .10$), but two way communication has a strongly significant effect as compared to one way communication ($t = 3.46$; $p < .01$). Employees' payoffs are significantly lower for firms run in Barcelona ($t = 2.50$; $p < .05$).

To summarize, the regression results confirm Regularities 1 and 2. Beyond this, we confirm that average minimum efforts (as well as average payoffs for all parties) are

[17] The estimated effect of allowing communication (D_C) is larger and more statistically significant in Model 3 than in Model 2. This reflects the larger effect of allowing context in Cleveland sessions.

[18] As an alternative, we ran an IV tobit with the short session dummy instrumenting for the number of rounds completed. The first stage is quite strong ($z = 11.77$, $p < .01$ for the instrument), but the estimate for rounds completed in the second stage is small – 0.16 – and not statistically significant ($z = 0.18$; $p > .10$).

[19] A tobit specification is used because 24 managers had the minimum possible payoff (100 ECUs) in all rounds.

[20] The average payoffs for the four employees in a firm are averaged to make a single observation.

significantly lower for firms run in Barcelona. This effect is almost entirely due to sessions where communication is possible.

B. The Content of Messages: We have now reached the heart of the paper: what kinds of statements are linked to high effort levels and how does the impact of these statements compare with that of financial incentives?

To answer these questions we developed and implemented a systematic scheme for coding message content. The goal was to quantify *any* communication that might be relevant for the play of the game, avoiding prejudgments about which sorts of messages were important and which were not. Our methods paralleled those employed by Cooper and Kagel (2004). We began by randomly selecting ten firms to serve as a test sample, five from the one-way communication treatment and five from the two-way communication treatment. Both co-authors as well as two research assistants independently developed coding schemes for the test sample. In a series of meetings we reconciled these individual efforts into a single coding scheme. Three research assistants then independently coded all messages sent by managers or employees. One bilingual RA (native language English) coded all messages. An American RA coded all Cleveland messages and a Spanish RA coded all Barcelona messages. No effort was made to force agreement among coders – the goal was to have two independent readings of each message so that any coding errors were uncorrelated. At no point in the process of developing or implementing the coding scheme was any RA informed about any hypotheses the co-authors had about the messages. The RAs were repeatedly and explicitly told that their job was to capture what had been said rather than why it was said or what effect it had. Coding was binary – a message was coded as a 1 if was deemed to contain the relevant category of content and zero otherwise. We had no requirement on the number of codings for a message – a coder could check as many or few categories as he or she deemed appropriate. A number of the categories have sub-categories. For example, Category 1 for managers is "Ask for Effort." Under this are three sub-categories, "Polite," "Rude," and "Specific effort level." A coder was free to check as many or few sub-categories as they desired when the corresponding category was checked off.

Our analysis of the codings uses averages across coders unless otherwise noted. We implicitly assume that errors are independent across coders so that the total error is reduced by averaging. Cross-coder correlations for major categories were generally around .6, slightly better than reported by Cooper and Kagel (2004).

(Insert Tables 5 and 6)

Table 5 summarizes the codings of managers' messages and Table 6 provides analogous information for employees' messages. These tables only show codes that appear in at least 7.5% of all available observations. We generally do not include sub-categories (even though several clear the 7.5% threshold) to avoid problems with co-linearity in the regressions. We cannot eliminate the possibility that some of the rarer categories, *if used*, would impact employees' choices, but the data provides insufficient observations of these categories to accurately measure their effect.[21] Our experimental design is not intended to determine the best (or worst) possible messages a manager *could* use, but instead examines what messages work well (or poorly) among those managers *do* use.

For a number of the common categories, the brief descriptions in Tables 5 and 6 don't adequately characterize the nature of the messages. We therefore begin by better describing some key categories along with examples drawn from the messages. Starting with the managers, category 1 codes any request that employees choose a higher effort level. For example, "Please spend more hours on Activity A [effort]. Please." Commonly these messages request a specific effort level (sub-category 1c).

Category 4 codes messages that point out the benefits of choosing higher effort levels for the employees – frequently this involved explicit discussion of the possibility for mutual gains by managers and employees (sub-category 4c). The following quote is typical: "We would all make more money if you, as employees, devoted your time to activity A [effort]."

Categories 5 and 6 are similar but not identical. Messages coded under category 5 involved the manager offering an implicit short term contract. A common form of these implicit contracts was the promise of an increased bonus rate in the next round if the employees delivered a requested minimum effort in the current round (sub-category 5a). As an example, "I'll set the bonus high next time if we all do 40 this time." Category 6 was reserved for longer term plans, often lacking the explicit quid pro quo of the implicit contracts coded under category 5. Many times these plans involved employees choosing a high effort in all rounds while the manager alternated between setting a high bonus rate and a low bonus rate. For example: "I think the best way for everyone to get a lot of money is to all go 40 hours every time and alternate between a 7 and 14 every other time." As in the preceding quote, alternating plans were often presented, either explicitly or implicitly, as a way to even out payoffs between the employees and manager. While this could have been

[21] The analysis reported below has also been done with a cut-off of 5% for inclusion. This more liberal cutoff has no impact on our conclusions as the added categories have little effect on behavior.

accomplished just as well by picking an intermediate bonus rate, there seems to be a preference for alternating, perhaps because it makes the gift exchange clearer.

Category 10 was coded when the manager emphasized the bonus, usually by explicitly stating what the bonus rate was. It is difficult to understand the importance of this category without seeing the messages in context. When a manager specifically refers to the bonus rate it is almost always to make some point other than what the bonus is. For example, consider the following message: "Thanks. I appreciate it. Now I'll raise it [the bonus rate] to 11." The employees have just raised their minimum effort from 20 to 30. The manager is responding by raising the bonus rate from 10 to 11. The implication is clear – the manager is rewarding the employees for their increased effort. As is usually the case for messages coded under category 10, it is implied that a bonus rate of 11 is good pay. Looking at the broader sweep of this particular manager's messages, it is also clear that he is signaling that an increase to a minimum effort of 40 will bring a further increase in the bonus rate. Indeed, he eventually succeeded in getting his employees to coordinate at effort level 40 in exchange for a bonus rate of 12. This is a good example of the implicit references to reciprocity that appears in many messages coded under category 10.

Turning to the employee categories, category 2 is coded when an employee indicatez agreement to a plan, usually one coded under category 6 for the managers. For example, an employee responded "Sounds good to me. [Y]ou earn more, we earn more and everyone's happy" following a (lengthy) proposal by the manager that the bonus rate alternate between 8 and 15 while the all employees choose effort level 40 in all rounds.

Category 4 was coded for messages where an employee offered advice to a manager. Sometimes this advice is clearly self-serving as in the following quote: "[L]ets keep the min bonus at 8.... 6 is the just the LOWEST... you dont want your employees thinking theyre are the lowest, do you?!" In other cases the advice is genuinely intended to help the manager move the group to a higher minimum effort. For example, consider the following message: "Give us more concrete information: ie. If u all do 30, ill put a bonus of 11 the next round, then if u all do 40, till put a bonus of 12 and then if u keep it constant I will continue to raise the bonus." The manager receiving this message had been making vague promises of a higher bonus in exchange for more work (coded under category 5), a tactic that wasn't working as the minimum effort was 0 in the round this message was sent. The manager in the next round somewhat followed his employee's advice, sending, "30 or higher and i will raise it to 10 and so on." This didn't work immediately, as the minimum effort stayed at zero, but in later rounds the manager repeated similar messages and was able to eventually

reach perfect coordination at effort level 40. Sometimes the advice offered to managers is quite simple, like the following plea by an employee faced with an unsuccessful manager who was doing nothing but setting bonus rates: "Send us some messages....."[22]

Before analyzing the effects of the frequently coded categories, it is worth noting some of the categories that *don't* appear frequently. Manager categories 12 – 14 and employee categories 10 – 12 code for explicit references to fairness, trust, and reciprocity.[23] To our surprise, none of these categories are coded with any frequency. This doesn't imply that fairness, trust, and reciprocity don't matter in this setting. As noted previously in our discussion of category 10 (emphasizing the bonus rate) managers frequently refer to them implicitly. It may simply be that notions like trust and reciprocity are so deeply ingrained in our subjects that they don't feel the need to explicitly lay them out.

Several of the categories code for confusion about the instructions or misunderstanding the rules (manager categories 16, 17, and 21; employee categories 13 and 15). All of these categories are rare, giving us some confidence that the subjects understood the experimental instructions.

A first basic question to answer about the frequently coded categories is when (and where) they were used. Table 7 shows how the likelihoods of coding the most frequently used messages relate to a number of variables. Specifically, we report averages over observations where *either* coder checked off the relevant category. The standard errors, reported in parentheses, have been corrected for clustering at the firm level. As a point of comparison, the first two rows of the table provide the same statistics for all observations with communication and for all observations from the two-way communication treatment. We also report the results of statistical tests for whether the frequency of a category was significantly different in Cleveland (US) than in Barcelona (Spain) and, where relevant, significantly different with two-way communication.[24] It should be clear that the likelihood of the various categories isn't random. As a trivial example, positive comments (manager category 3) are generally associated with a high lagged minimum effort while negative

[22] In the round this message was sent, the manager selected a bonus rate of 11 but still received a minimum effort of 0. Subsequently the manager did start sending messages and was able to achieve higher minimum efforts although never reaching perfect coordination at effort level 40.

[23] We did not require that a message specifically include one of these three words to be coded, but instead looked for clear references to these concepts.

[24] Statistical significance is derived from ordered probit regressions over all observations where the coding in question could possibly have been observed. The regressions control for lagged minimum effort, change in lagged minimum effort, minimum effort in Round 10, current bonus (employee codes only), lagged bonus, and late rounds (rounds 21 - 30). Standard errors are corrected for clustering

comments (manager category 2) usually follow a round with low minimum effort. Another notable case is laying out long term plans (manager category 6), which is normally associated with firms that have already achieved a relatively high minimum effort.

(Insert Table 7)

Table 7 shows significant differences in the types of communication used by managers in Cleveland and in Barcelona consistent with our observation that poorer performance in Barcelona was linked to the availability of communication. The most noticeable difference when the dialogues are read is the greater negativity of Barcelonan managers, as captured by the greater frequency of codings for negative responses (manager category 2) and the lower frequency of codings for positive responses (manager category 3). However, as will become clear below, the less frequent coding for emphasizing the bonus rate (manager category 10) in Barcelona may be the most important difference.

Managers generally send more messages in the two-way communication treatment, partially in response to messages from their employees. Of particular importance will be the more frequent usage of asking for effort (manager category 1), particularly requests for a specific effort level (manager subcategory 1c), and emphasizing the bonus rate (manager category 10).

Just because a category of message is used frequently doesn't necessarily mean it accomplishes much. Table 8 examines the relationship between messages and managers' profits. Data is drawn from the 39 firms in the communication treatments with minimum effort of 0 in round 10. Limiting the sample reduces the impact of differing initial conditions as well as focusing attention on the firms of greatest interest. The data is broken down by whether firms achieved earnings above our below the median for this group.[25] We report statistics for all twenty rounds with human managers as well as for just the first five rounds (rounds 11 – 15) when most of the change in employees' choices occurs. For the four resulting cells we report the average bonus rate and the frequency of the most common message categories. We also calculate "all coded comments" which is the sum of the average frequencies over all categories. Sub-categories are not included in this statistic to avoid double counting. "All coded comments" provides a measure for how much a manager communicates. This is a better measure than word counts as it does not give credit for irrelevant communications or for being unusually verbose. Standard errors, corrected for firm level clustering, are shown in parentheses.

[25] The median firm is grouped with those below the median. For categories that could only be coded with two-way communication, we use the median earnings for relevant firms in the two-way communication treatment.

(Insert Table 8)

For rounds 11 – 15 the average bonus rates are almost equal for firms above and below the median earnings. Whatever leads some firms to eventually be more profitable than others, it doesn't appear to be differences in incentives. In contrast, there are fairly obvious differences in what messages are sent. Ignoring the content of messages, managers who earn more than the median earnings send twice as many coded messages as their less successful peers. Greater than median earners are 53% more likely to be coded for asking for effort (category 1), 81% more likely to be coded for discussing the benefits of high effort (category 4), more than three times as likely to be coded for implicit contract (category 5), more than twice as likely to be coded for laying out a plan (category 6), and more than 26 times more likely to be coded for emphasizing the bonus (category 10)![26] Most of these differences persist if we consider the longer sweep of rounds 11 – 30.

While suggestive, the raw statistics shown on Table 8 cannot be used to establish causal relationships between managerial choices (bonuses and messages) and outcomes (minimum effort levels and managerial payoffs). To see why, consider the use of negative responses (category 2). Over rounds 11 – 30, unsuccessful managers are 66% more likely to be coded for this category. Does it follow that negativity by managers leads to low profits? Not really – causality could easily be reversed since managers who are doing badly are more likely to say negative things (see Table 7). In this particular case the true causality can be easily seen by looking at the statistics for rounds 11 – 15 where successful managers are 82% *more* likely to make a negative comment. However it is generally not so simple to pin down causality especially when many categories are coded simultaneously. We therefore turn to more formal statistical analysis to determine the impact of the messages on minimum efforts and, ultimately, manager profits.

Table 9 reports the results of five ordered probit regressions. The purpose of these regressions is to establish the causal relationship between managers' actions and employees' choices. The basic data set is all observations from rounds 11 – 30, although some of the regressions only use a subset of this dataset.[27] The dependent variable is a firm's minimum effort for the round being observed – it is the categorical nature of minimum effort that leads

[26] T-tests indicate varying degrees of statistical significance for these differences. Successful managers are significantly more likely to be coded in Rounds 11 – 15 for category 1 ($t = 3.26$, $p < .01$), category 5 ($t = 3.02$, $p < .01$), and category 10 ($t = 4.16$, $p < .01$). No significant differences exist for category 4 ($t = 1.58$, $p > .10$) or category 6 ($t = 1.21$, $p > .10$).

[27] We have also run the regressions on Table 9 with only the observations from sessions with communication. This slightly affects the statistical significance of some categories, but has no impact on our main conclusions.

to our use of an ordered probit model. The first four lines of equation 4 give the unrestricted version of the equation being estimated for the latent variable with the remaining five lines specifying how the latent variable is converted into the observed minimum effort level. The values of the coefficients ($\beta_1 - \beta_{22}$) and the cutoffs (Cut1, Cut2, Cut3, and Cut4) are parameters to be estimated. The variable ε_t is a normally distributed error term which captures any natural variation in outcomes that the exogenous variables don't account for. Subscripts on variables refer to the round (e.g. Bonus_t is the bonus rate for round t).

$$\begin{aligned}
\text{MinEffort}_t^* &= \beta_1 D_H + \beta_2 D_C + \beta_3 D_{TW} + \beta_4 D_B + \beta_5 \text{MinEffort}_{10} + \beta_6 \text{Bonus}_t \\
&+ \beta_7 \left(\text{Bonus}_t - \text{Bonus}_{t-1}\right) + \beta_8 \text{Bonus}_{11} D_{t \geq 13} + \beta_9 D_{t \geq 21} + \beta_{10} \text{MinEffort}_{t-1} + \beta_{11} \text{MinEffort}_{t-2} \\
&+ \beta_{12} \text{MC1}_t + \beta_{13} \text{MC2}_t + \beta_{14} \text{MC3}_t + \beta_{15} \text{MC4}_t + \beta_{16} \text{MC5}_t + \beta_{17} \text{MC6}_t + \beta_{18} \text{MC10}_t \\
&+ \beta_{19} \text{MC18}_t + \beta_{20} \text{MC19}_t + \beta_{21} \text{EC1a}_{t-1} + \beta_{22} \text{EC2}_{t-1} + \beta_{23} \text{EC4}_{t-1} + \varepsilon_t \\
\text{MinEffort}_t &= 0 \text{ if MinEffort}_t^* \leq \text{Cut1} \qquad \text{(eq. 4)} \\
\text{MinEffort}_t &= 10 \text{ if Cut1} < \text{MinEffort}_t^* \leq \text{Cut2} \\
\text{MinEffort}_t &= 20 \text{ if Cut2} < \text{MinEffort}_t^* \leq \text{Cut3} \\
\text{MinEffort}_t &= 30 \text{ if Cut3} < \text{MinEffort}_t^* \leq \text{Cut4} \\
\text{MinEffort}_t &= 40 \text{ if Cut4} < \text{MinEffort}_t^*
\end{aligned}$$

The most important issue in designing this specification is determining whether correlations between message categories and minimum efforts reflect causal relationships or shared relationships with lagged minimum efforts. The following example illustrates the underlying econometric problem. Once again consider the negative relationship between minimum effort and negative responses by the manager (manager category 2). We could try to establish a causal relationship between negative responses and minimum effort by estimating a linear probability model. (Using a linear probability model rather than an ordered probit simplifies the exposition, but does not affect the underlying logic of the problem.) In reality, suppose the current minimum effort, MinEffort_t, depends *solely* on the lagged minimum effort, MinEffort_{t-1}: $\text{MinEffort}_t = \alpha + \beta * \text{MinEffort}_{t-1} + \varepsilon_t$ where α and β are constants and ε_t is an iid error term. Also suppose that whether the manager sends a negative response in the current round, MC2_t, depends solely on the lagged minimum effort: $\text{MC2}_t = \gamma + \delta * \text{MinEffort}_{t-1} + \nu_t$ where γ and δ are constants and ν_t is an iid error term. Assume $\beta > 0$ and $\delta < 0$. Now imagine that we estimate the following equation: $\text{MinEffort}_t = \zeta + \eta \text{MC2}_t + \theta_t$. This model yields omitted variable bias in the estimated coefficient for MC2_t, $\hat{\eta}$, because the lagged minimum effort has not been included. Even though there is no causal relationship between MC2_t and MinEffort_t, the estimated coefficient will (on average) be

negative: $E(\hat{\eta}) = \beta/\delta < 0$.[28] If we modify the regression equation by including the lagged dependent variable, $MinEffort_t = \zeta + \eta MC2_t + \kappa MinEffort_{t-1} + \theta_t$, the resulting estimates are unbiased: $E(\hat{\zeta}) = \alpha$, $E(\hat{\eta}) = 0$, and $E(\hat{\kappa}) = \beta$. Including the lagged dependent variable makes it possible to (in the limit) correctly identify that the current minimum effort does not depend on $MC2_t$. More generally, if managers' and employees' messages are a function of employees' lagged action, as Table 7 suggests, then the causal relationship between messages and employees' actions can only be identified by including lagged dependent variables.

The regression equation therefore includes both the lagged minimum effort ($MinEffort_{t-1}$) and the twice-lagged minimum effort ($MinEffort_{t-2}$). Twice lagged minimum effort must be included to determine the causal relationship between employee messages and minimum effort. The regression equation includes *lagged* employee codes as independent variables, which are a function of the *twice lagged* minimum effort. This implies that parameter estimates for lagged employee codes will be biased unless the twice lagged minimum effort is included as an independent variable. Using lagged dependent variables does not require us to drop any data since the first data being included is from round 11, not round 1. Parameter estimates for the lagged dependent variables are not reported in Table 9 to save space, but these are always positive and statistically significant at the 1% level. A version of Table 9 including all dependent variables is available from the authors.

As additional controls, the regressions also include a dummy for sessions in which the manager was a subject (D_H), a dummy for session in which communication was possible (D_C), and a dummy for the two-way communication treatment (D_{TW}). To control for location effect, a dummy is included for data gathered in Barcelona (D_B). The minimum effort in round 10 ($MinEffort_{10}$) is included to control for the firm's initial conditions. Looking at Figures 1 and 2, most increases in the minimum effort take place over rounds 11 – 20. The regression equation therefore includes a dummy for rounds 21 – 30 ($D_{t\geq 21}$).

Table 9 does not report the parameter estimates for any of these basic controls to conserve space. The parameter estimates for the treatment variables (D_H, D_C, and D_{TW}), the Barcelona dummy (D_B), and the minimum effort in round 10 ($MinEffort_{10}$) tend to be relatively small and often fail to be statistically significant. This shouldn't be taken as evidence on treatment effects or location effects, but instead reflects the presence of lagged dependent variables in the specification. The estimated coefficient for rounds 21 – 30 ($D_{t\geq 21}$)

[28] To see this, note that $MinEffort = \zeta + \eta MC2_t + \theta_t = \zeta + \eta(\gamma + \delta * MinEffort_{t-1} + \nu_t) + \theta_t = (\zeta + \eta\gamma) + \eta\delta * MinEffort_{t-1} + (\nu_t + \theta_t) = \alpha + \beta * MinEffort_{t-1} + \varepsilon_t$. Therefore, $\beta = \eta\delta$ or $\eta = \beta/\delta$.

is always negative and usually statistically significant. Since the regressions capture *changes* in minimum effort levels, these negative estimates reflect the ossification of employees' choices over the final ten rounds.

The variables of greatest interest in the regressions on Table 9 measure what the managers and employees said in their messages. For manager messages, we include the average coding (e.g. across the two coders) in the current round for all categories above the 7.5% frequency threshold ($MC1_t$ for manager category 1, etc.). To avoid co-linearity, subcategories were not included. These variables were set equal to zero for all observations where coding wasn't possible and were demeaned for all observations where coding was possible.[29] For employee messages ($EC1a_{t-1}$ for employee category 1a, etc.), we include the average lagged employee codes (rather than the current round's average coding) since the current round's employee messages haven't yet had an opportunity to affect the behavior of other subjects. We made an exception to our rule of not using subcategories by including employee category 1a rather than category 1. Given the opposing natures of categories 1a and 1b (positive vs. negative generic responses to the manager), we were concerned that a false negative could occur if these categories were combined with the positive effect of category 1a being cancelled out by the negative effect of category 1b. As it turns out, neither subcategory is statistically significant. The variables for employee codes are demeaned in the same fashion as manager codes.[30]

Three additional variables control for the impact of incentives on minimum effort levels. The current bonus rate ($Bonus_t$) and the change in the bonus rate ($Bonus_t - Bonus_{t-1}$) are included as separate variables. These two variables capture different ways in which incentives might matter. If the bonus rate only affects employee choices by changing the game they play among themselves, then only the current bonus rate should be significant. However, Brandts and Cooper (2006a) find that *changes* in the bonus rate are more important than the *level* of the bonus rate following a change in determining whether a firm overcomes a history of coordination failure. In this earlier paper we hypothesized that changes in the bonus rate serve as a coordinating device, helping employees synchronize increases in effort. The environment in our current experiments differs substantially from the earlier study – the manager is now played by a subject rather than the computer and employees no longer see the

[29] Observations from the four firms in the one-way communication treatment for which messages weren't saved are included in the set of observations for which coding isn't possible. Dropping these observation from the dataset has little impact on the regression results.

[30] Round 11 data are included in the set of observations for which coding isn't possible. For the one regression that didn't include managers' categories we deleted data from Round 11.

effort levels chosen by others – but if the bonus rate remains a useful device for coordinating employees' increases in effort then the parameter estimate for the change in the bonus rate should be positive. Having a subject as the manager adds an additional dimension along which changing the bonus rate could affect employees' choices independent of the level of the bonus rate: reciprocity. If employees respond to kindness by the manager (a higher bonus rate with higher effort level, we again hypothesize that the change in the bonus rate should have a positive parameter estimate. The final independent variable related to incentives is the bonus rate for round 11. If the bonus rate for round 11 is added to Models 2 - 4 on Table 4, the resulting parameter estimates are positive and significant at the 5% level. This suggests that the first bonus rate set by a member has a persistent effect. The bonus rate for round 11 is interacted with a dummy for rounds 13 – 30 and demeaned to avoid any interference with the estimates for "Current Bonus" and "Change in Bonus.

Our use of an ordered probit model implies a specific functional form for the likelihood function. For a discussion of the ordered probit model, including the underlying assumptions, see Greene, 2000 (pp. 875 – 879). In designing a specification, four issues beyond the inclusion of lagged dependents variable merit discussion.

1) We have explored adding additional lagged values of the minimum effort to the model. Adding the three and four times lagged values significantly improves the fit.[31] Beyond this point, additional lagged values of the minimum effort do not significantly improve the fit. Adding additional lagged minimum efforts has little impact on the results, although a few variables which just miss significance at the 10% level (current bonus and manager category 19) in Model 3 on Table 5 reach this standard with the additional terms.

2) Other sources of omitted variable bias may exist beyond the lagged values of minimum effort. The most obvious is subjects' unobservable "types." If messages and effort levels are both driven by unobservable types, omitted variable bias can occur even with the inclusion of lagged dependent variables. This issue is not important in the analysis of managers' messages. Because messages and effort levels are determined by different individuals and because subjects are randomly selected into firms and roles, there is no reason to believe that the personal characteristics of the manager choosing messages are correlated with the personal characteristics of the employees choosing effort levels. Unobservable types might play a more important

[31] The log-likelihood for Model 3 improves from -1231.70 to -1207.95 by including the two additional terms. A log likelihood ratio test rejects the null hypothesis that the two new parameters equal zero ($\chi^2 = 47.50$; $p < .01$).

role in the analysis of employees' messages. For example, Models 2 and 3 indicate that giving the manager advice (employee category 4) leads to higher minimum effort levels. This could reflect employees' unobserved types – employees who are sufficiently motivated to offer advice to their manager may also be more motivated than typical employees to try a higher effort level. Given that managers' actions respond significantly following advice from employees, a causal relationship between giving advice probably exists.[32] More generally, because changing the minimum effort usually requires multiple employees to change their effort levels, omitted variable bias in estimates for employee codes is unlikely to be large.

3) Average codings are noisy measures of the "true" coding. We therefore have a problem with measurement error. The only way to eliminate this problem is to have a very large number of coders, but this is infeasible given the costs of coding. The usual problem of biasing estimated coefficients toward zero is therefore present, leading to an increased likelihood of incorrectly failing to reject the null.

4) Observations taken from the same firm in different rounds cannot be considered statistically independent. We therefore correct the standard errors for clustering at the firm level (see Liang and Zeger, 1986).

Table 9 presents several variations on the basic specification to pin down specific features of the data. Model 3 includes all categories for both managers and employees that clear the 7.5% frequency hurdle – this corresponds to the unrestricted model described by Equation 4. It is the central regression in our discussion of message content. Model 1 includes controls only for managers' messages and Model 2 only considers employees' messages, isolating the effects of managers' and employees' messages. Model 4 considers the toughest situation for managers, the subset of the data where the lagged minimum effort is zero. Model 5 explores which messages have a persistent effect by using the forward minimum effort as the dependent variable.

(Insert Table 9)

We begin our discussion of Table 9 by considering the impact of incentives. The current bonus rate ($Bonus_t$) in isolation does not have a significant impact on minimum effort levels, but changes in the bonus rate ($Bonus_t - Bonus_{t-1}$) do have a significant positive effect.

[32] We ran ordered probits using data from the two-way communication treatment where the dependent variables were either the current bonus or current codings for manager categories 1, 4, and 10 (the categories that consistently increase minimum effort). The independent variables are lagged employee codes, lagged bonus rate, lagged minimum effort, and a dummy for rounds 21 – 30. The coefficient for employee category 4 is positive and significant for manager category 1 ($z = 1.80$; $p < .10$) and manager category 10 ($z = 2.54$; $p < .05$).

This suggests that increasing the bonus rate increases minimum effort primarily through its role as a coordination device rather than its direct effect on incentives, consistent with Brandts and Cooper (2006a). This result is also consistent with positive reciprocity between employees and managers. The bonus rate for round 11 ($Bonus_{11}$) has a long term positive effect, albeit of marginal statistical significance, on minimum effort levels. The bonus rate for round 12 does not have a persistent impact in an analogous regression, emphasizing the importance of the first bonus rate set by a manager.

REGULARITY 3: Although the level of the bonus rate has little effect on the minimum effort, changes in the bonus rate lead to increases in the minimum effort. The bonus rate set in round 11 has a persistent impact on minimum efforts.

Turning to the managers' categories, we see that asking for effort ($MC1_t$), discussing the monetary benefits of higher effort ($MC4_t$), and emphasizing the bonus ($MC10_t$) have strong positive effects on minimum effort levels. These effects get even stronger if we replace category 1 with category 1c (requesting a specific effort level) and category 4 with category 4c (stressing mutual benefits). Oddly, laying out a plan ($MC6_t$) has a small but (marginally) statistically significant negative effect on minimum effort. Soliciting feedback from employees ($MC18_t$) has a large negative impact but the estimate is imprecise, leading to modest statistical significance. Neither negative responses ($MC2_t$) nor positive responses ($MC3_t$) have statistically significant effects. By extension, the negativity of Barcelonan managers is probably not responsible for their relatively poor performance. Implicit contracts ($MC5_t$) also fail to be statistically significant. Although managers often use implicit contracts as a strategy for raising minimum effort, they apparently do little good. Among the employee categories, only giving the manager advice ($EC4_t$) achieves statistical significance. Comparing Models 2 and 3, the strength of this effect is greatly lessened when the manager categories are added. Given that employees' advice to managers often includes suggestions about what messages should be sent, this weakening is to be expected.[33]

In an alternative version of Model 3 we added dummies for the first time a category was coded. With one exception, the parameter estimates for these dummies are small and not statistically significant. Most message categories have no greater impact the first time they are used than in later usages. The exception is discussing the monetary benefits of higher effort ($MC4_t$). The parameter estimate for the first time this category is coded is significant at the 5% level while the parameter estimate for its average coding becomes smaller and ceases

[33] If the effect of advice is largely through getting managers to send messages that would not otherwise be sent (see fn.32.), the effect of advice will largely be absorbed by these variables when manager categories are included in the regression.

to be statistically significant. Presumably employees immediately get the point that higher effort is mutually beneficial and don't need to be told a second time.

The usefulness of some categories that have a positive effect is obvious. Employees face a difficult coordination problem, and a manager can help by acting as a coordination device. Requesting a specific effort level (manager category 1c) and pointing out that everybody gains from increased minimum effort (manager category 4c) makes an increase in effort less risky for an employee as he has good reason to expect that others will also increase their effort levels. Thus, manager categories 1 and 4 have a strong impact on minimum effort levels. The positive impact of employee category 4 is also straightforward – given that employees have a stake in the manager's success, they can help by offering good advice.

The impact of other categories is more mysterious. Why should emphasizing the bonus rate (manager category 10) have such a strong positive impact? After all, the bonus rate is already prominently displayed for employees. It helps to recall that messages coded under category 10 often imply that a "good" bonus rate has been chosen or that the bonus is a reward for high effort is common. Thus, category 10 likely prompts higher effort by making employees believe there are large rewards for increasing effort, both in the current round and in future rounds, and by making them feel better about their relationship with the manager.

It also seems strange that laying out a long run plan (category 6) has a negative impact. Understanding this result becomes easier after considering Model 4. This regression has the same variables as Model 3, but only includes observations for which the lagged minimum effort is zero, i.e. cases where the firm is experiencing coordination failure. Category 6 now has a positive and significant parameter estimate! Combining this with our detailed reading of the messages, it becomes clear why category 6 has the effect that it does. When a firm is going badly, long term plans are usually a method of building a good working relationship between the manager and employees. Often the manager suggests a series of increases in the bonus rate in exchange for gradual increases in effort levels. On the other hand, when the firm is already doing well (in terms of high minimum effort), long term plans usually involve the manager trying to get a larger share of the surplus. Not surprisingly, this doesn't go over well with the employees.

The negative effect of manager category 18 came as a surprise, since we thought "soliciting feedback" from employees would be a valuable component of building a positive relationship. Looking at messages coded under category 18, many could be better labeled as

"requests for advice."[34] It isn't surprising that clueless managers fail to generate much immediate improvement from their employees.

Returning to Model 4, we see that it matters not just what messages are sent by a manager but also when they are sent. We've already noted that the sign of the estimate for laying out a plan ($MC6_t$) switches when the sample is restricted to observations with lagged minimum effort equal to zero. In conjunction with this, the parameter estimate for agreeing to the manager's plan ($EC2_{t-1}$) goes from being small and insignificant to being large, positive, and significant at the 1% level. The parameter estimate negative responses ($MC2_t$) is now negative and significant at the 1% level, while discussing the benefits of higher effort ($MC4_t$) and giving advice to the manager ($EC4_{t-1}$) cease to be statistically significant. Asking for higher effort ($MC1_t$) and emphasizing the bonus rate ($MC10_t$) still have positive and statistically significant estimates, reflecting their status as the most reliable paths to higher effort.[35]

Thus far we have only looked at the immediate effect of the various types of message. Model 5 checks for persistence – the dependent variable for an observation in round t is the minimum effort in round t + 1. All the variables are otherwise identical to those in Model 3. The results of Model 5 reinforce some of our previous conclusions. Not only do changes in the bonus rate, asking for effort ($MC1_t$), discussing the benefits of higher effort ($MC4_t$), and emphasizing the bonus rate ($MC10_t$) have an immediate impact on minimum efforts, they also have a persistent impact. These persistent effects likely reflect both direct and indirect effects, as managers' actions help to build an ongoing relationship between the manager and the employees. Interestingly, implicit contracts ($MC5_t$) now have a positive and statistically significant effect. This seems unlikely to be a direct effect, as the implicit contracts coded under category 5 are short-run in nature. Instead this result probably reflects the beginning of a positive relationship between a manager and his employees. No employee category appears to have a persistent effect on minimum effort – the parameter estimate giving the manager advice ($EC4_{t-1}$) is tiny and flips sign from Model 3.

Calculating marginal effects on minimum effort for Models 3 and 5, even some of the variables that have a persistent effect lose much of their impact.[36] For example, the effect of

[34] e.g. "What could I do to make you work more hours on activity A?"

[35] The results must be interpreted with care as an ordered probit is a non-linear model. Even though the parameter estimates for manager categories 1 and 10 are larger in Model 4 than Model 3, the marginal effect on minimum effort is actually smaller for both variables.

[36] Marginal effects are based on discrete changes. For the bonus rate, the calculation is based on the difference between the mean bonus rate minus ½ and the mean plus ½. To capture the full effect of a change in the bonus

a bonus rate increase on the lead minimum effort is 28.4% of its effect on the current minimum effort. In a less extreme case, the impact of asking for effort ($MC1_t$) on the lead effort is only 48.9% of its effect on the current effort level. In contrast, other variables actually have a greater effect on the lead minimum effort. Notably, the marginal effects of discussing the benefits of higher effort ($MC4_t$), implicit contracts ($MC5_t$), and emphasizing the bonus rate ($MC10_t$) on the lead minimum effort are respectively 142.7%, 330.6%, and 128.8% of their marginal effects on the current minimum effort.

REGULARITY 4: The effectiveness of messages depends both on what is said and when it is said. Manager messages that request greater effort (category 1) and emphasize the bonus rate (category 10) most consistently have a positive persistent impact on minimum effort.

The regressions reported in Table 9 measure the impact of incentives and messages on minimum effort, but do not directly address the question of how manager profits are affected. We have therefore calculated marginal effects on manager profits for Models 3 and 4 from Table 9. (See f.n. 36 for details of how marginal effects are calculated.) The results are displayed in Table 10. The top panel shows the results for all data and the bottom panel limits the sample to those observations with lagged minimum effort of zero, the cases where the manager faces an immediate past of coordination failure. To give the reader some sense of the accuracy for these estimated marginal effects, we also recapitulate the statistical significance of the variables as reported in Table 9.

(Insert Table 10)

In either panel of Table 10, increasing the bonus rate has a minimal effect on profits. Indeed, the marginal effect across all observations is negative! The increase in minimum effort caused by a bonus rate hike does not cover the additional bonuses that the manager must pay to employees. Even when the firm faces an immediate past of coordination failure, the best case scenario for changes in the bonus rate being helpful (as the marginal effect cannot be negative), the impact on profits is small, amounting to 5.6% of average profits.[37] In contrast, many of the message categories have large positive impacts. Across all observations, asking for effort ($MC1_t$), discussing the benefits of higher effort ($MC4_t$), and emphasizing the bonus rate ($MC10_t$) increase profits by 33.0%, 18.9%, and 33.5%

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rate, we re-ran Model 3 with "Current Bonus" and "Lagged Bonus" as independent variables rather than "Current Bonus" and "Change in Bonus." For message categories, the calculation is based on the change from a coding of 0 to a coding of 1. All variables other than the one being changed are held fixed at their average levels. Calculations of marginal profits, shown in Table 10, use the average bonus rate for the dataset.

[37] It may appear that a bigger impact on profits can be generated by increasing the size of the bonus rate hike – a one unit increase at the mean is being considered here. This argument is flawed due to the non-linear relationship between bonus rates and profits. Consider the largest possible bonus rate increase from B = 6 to B = 15. Even if lagged minimum effort equals zero, this cannot possibly lead to an increase in manager profits.

respectively. Restricting attention to observations following a minimum effort of 0, asking for effort ($MC1_t$), laying out a plan ($MC6_t$), and emphasizing the bonus rate ($MC10_t$) increase profits by 28.0%, 36.3%, and 24.9% respectively. Even in the best case for incentive payments, the marginal impact of the most effective comments is roughly 5 – 7 times greater than the marginal impact of a bonus rate increase. The bottom line is clear – it's what you say, not what you pay!

REGULARITY 5: The manager's communication strategy has a larger impact on his profits than his choice of a bonus rate.

The marginal effects reported on Table 10 only reflect the current round; e.g. the round in which the bonus rate increase or message takes place. These figures do not capture any long term effects on manager profits. To the extent that some of the messages have a persistent impact, the true effect on profits is larger than the numbers reported here.

6. Conclusions

A novel feature of our experiments is the presence of an external agent, the manager, who has a stake in the coordination game being played among employees. Our goal is to study how such an agent can best help overcome a history of coordination failure. The overarching conclusion from our analysis is that effective communication between managers and employees plays a more important role in escaping coordination failure than good incentives. Allowing communication between managers and employees leads to increased coordination and higher payoffs for managers and employees. In addition, certain well-chosen messages are better tools for increasing managerial profits than manipulating employees' bonus rates. There was no obvious reason to anticipate the latter result. Facing coordination failure, it is in everybody's interest for the firm to improve coordination. We would therefore expect that any coordination device would serve this purpose. In addition, simple economic intuition suggests that financial incentives should have strong drawing power. Although bonus rate increases have an overall positive effect – see also the results on incentive changes by computer managers reported in Brandts and Cooper (2006a) – they don't work as well as sending the right messages.

The most effective managerial strategy is rather simple and, ex post, natural. Managers should request a specific effort level and emphasize the mutual benefits of high effort. The goal is to act as a good coordination device. It is useful to point out how well employees are being paid, although it is not important to actually pay employees especially well. The most effective employee messages give advice to the manager, providing the firm with benefits of more than one person thinking about his problems.

It may surprise some economists that effective communication is more important than the choice of bonus rates. Our interpretation of this result centers on how cognition enters into the achievement of coordination. Some coordination devices may be naturally more salient than others. In our context, the use of bonus rate increases as a coordinating device relies on an indirect channel of communication. Employees must correctly infer that the intended "message" of a bonus rate increase is that effort levels should be increased and must also believe that other employees infer the same message. It isn't obvious that these inferences will occur, especially since a bonus rate increase can serve other purposes such as changing the distribution of surplus. Through the use of communication managers can directly point to the need for coordination. The exercise of leadership works better when leaders use cues which followers can grasp more easily.

Although superficially the results of our experiments indicate that incentives matter little, there actually exists a subtle interaction between incentives and communication. While changing the bonus rate accomplishes little for a manager, many of the most effective messages appeal to the financial interests of employees. For example, pointing out the mutual benefits of coordination is effective because employees care about coordinating *and thereby earning higher payoffs*. The mechanism by which the successful managers use incentives is by focusing employees on the existing financial benefits of choosing different actions rather than changing the benefits from achieving coordination. Incentives do matter in our experiments, just not on the margin that economists are used to thinking about.

The specific managerial strategy that works best here will not necessarily work in all environments, but it is clear that one role of a good manager is to act as a good coordinating device. By indicating clearly what is expected of employees[38] and pointing out the benefits of coordinating, a good manager makes it easier for employees to overcome their strategic uncertainty and successfully coordinate. More generally, managers cannot rely solely on increased financial incentives to generate improvement. In some settings, good communication is far more important. This point is driven home by comparing the Cleveland and Barcelona firms, as superior performance in Cleveland is largely driven by differences in the communication strategies used by managers in the two locations.

Several other papers have studied coordination games where some form of communication is available. The experimental studies most closely related to ours are Blume

[38] The value of clarity can also be seen in the results of Capra et al (2005), who find that "specific proposals" are a particular effective form of communication for increasing coordination.

and Ortmann (forthcoming) and Capra, Tanaka, Camerer, Munyan, Sovero, Wang, and Noussair (2005).[39]

Blume and Ortmann study the effects of costless pre-play communication between the (symmetric) players in weak link and median games with seven Pareto-ranked equilibria. Each of the games was played in 8 action stages, with a message stage preceding each action stage. In a message stage, every participant entered the choice "he or she wishes to send" and these messages were then sent to the other players. The results show that costless messages of the kind they study can strongly facilitate participants' coordination on the Pareto-dominant equilibrium, even for more than two players.

Capra, Tanaka, Camerer, Munyan, Sovero, Wang, and Noussair (2005) study coordination failure in a growth model drawn from the development literature. They find that allowing communication leads (on average) to improved coordination.

Our work differs from these contributions in many ways, three of which deserve mention. First, our work focuses on change. Our experimental design generates a history of coordination failure which a new manager and the employees then need to overcome. Second, our game includes an agent, the manager, who does not actually play the coordination game but has a direct stake in its outcome. In Blume and Ortmann and Capra *et al*, all communication is between the agents playing the coordination game. We do not allow any communication between employees so as to focus on the interaction between managers and employees. Finally and most importantly, instead of concentrating solely on communication, we examine both incentives and communication with a focus on which tool is most effective for overcoming coordination failure. The rich communication strategies we observe are largely due to the interaction between incentives and communication as well as the inherent tension between managers and employees. Our ability to quantify the impact of specific types of comments is important because we wish to compare the effect of communication with direct incentives. We view these other experiments as a complement to our own, driving home the point that communication can play an important role in overcoming coordination failure, but their work is not intended to address the interactions between managers and employees that most interest us.

[39] Chaudhuri, Schotter, and Sopher (2004) also study experiments where an external agent has an interest in coordination. A participant in a previous minimum game can offer advice to a "successor" in the current minimum game. The ancestor receives a payoff identical to the successor's earnings in the current minimum game as well as their earnings from their previous play. They find that advice increases coordination if it is common knowledge and of sufficient quality. See also Cooper, DeJong, Forsythe, and Ross (1989 and 1992) and Weber, Camerer, Rottenstreich, and Knez (2001). For the effect of cheap talk in experiments in general see Crawford (1998).

As a final comment, our results are generated from a specific environment where coordination plays a central role. We don't argue that changing financial incentives can never be an effective managerial tool or that incentive design is always less important than communication. There exist ample examples of environments in which incentives play a central role. An important topic for future research is determining which settings, such as those that involve coordination, are particularly amenable to the use of communication and which are more sensitive to the choice of incentives.

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Table 1

Employee i's Payoff Table, B = 6

		Minimum Effort by Other Employees				
		0	**10**	**20**	**30**	**40**
Effort By Employee i	**0**	200	200	200	200	200
	10	150	210	210	210	210
	20	100	160	220	220	220
	30	50	110	170	230	230
	40	0	60	120	180	240

Employee i's Payoff Table, B = 14

		Minimum Effort by Other Employees				
		0	**10**	**20**	**30**	**40**
Effort By Employee i	**0**	200	200	200	200	200
	10	150	290	290	290	290
	20	100	240	380	380	380
	30	50	190	330	470	470
	40	0	140	280	420	560

Table 2
Features of Treatments

Treatment Name	Computer Manager	No Communication	One-way Communication	Two-way Communication
Manager Type Rounds 1 - 10	Computer	Computer	Computer	Computer
Manager Type Rounds 11 - 30	Computer	Human	Human	Human
Communication	None	None	Managers to employees	Managers to employees and vice versa
Bonus Rate Rounds 1 – 10	6	6	6	6
Bonus Rate Rounds 11 – 30	10	Set by Manager in Each Round	Set by Manager in Each Round	Set by Manager in Each Round
Cleveland Firms	5	10	20	6
Barcelona Firms	5	18	7	15

Table 3
Summary of Results, Averages Over Rounds 11 – 30
(Standard Errors in Parentheses)

		Location		
Treatment	Statistic	Cleveland	Spain	Pooled
Computer Manager	# Firms	5	5	10
	Bonus	10 (---)	10 (---)	10 (---)
	Minimum Effort	12.1 (7.5)	11.4 (4.3)	11.8 (4.1)
	Manager Profit	342 (150)	328 (85)	335 (91)
	Employee Payoff	249 (40)	242 (25)	245 (22)
No Communication	# Firms	10	18	28
	Bonus	9.0 (0.6)	9.4 (0.3)	9.3 (0.3)
	Minimum Effort	5.1 (2.1)	3.5 (1.1)	4.1 (1.0)
	Manager Profit	214 (44)	161 (20)	183 (21)
	Employee Payoff	198 (10.9)	189 (9.4)	192 (7.1)
One Way Communication	# Firms	20	7	27
	Bonus	9.3 (0.4)	9.1 (0.9)	9.3 (0.4)
	Minimum Effort	15.4 (3.1)	4.9 (	12.7
	Manager Profit	431 (78)	216 (70)	376 (63)
	Employee Payoff	246 (18)	190 (10)	231 (11)
Two Way Communication	# Firms	6	15	21
	Bonus	10.1 (0.4)	9.8 (0.4)	9.9 (0.3)
	Minimum Effort	28.0 (3.4)	16.9 (3.3)	20.1 (2.8)
	Manager Profit	613 (85)	379 (60)	446 (54)
	Employee Payoff	326 (26)	266 (22)	283 (18)

Table 4
Tobit Regressions, Firm Averages Over Rounds 11 – 30
Dependent Variable: Average Minimum Effort
86 Observations, 22 Left Censored

Variable	Model 1	Model 2	Model 3	Model 4
Constant	9.37** (4.42)	9.27** (4.26)	6.03 (4.42)	5.94 (4.45)
Human managers (D_H)	-9.99* (5.21)	-4.91 (4.63)	-5.39 (4.49)	-5.27 (4.53)
Communication (D_C)	11.92*** (3.84)	6.15* (3.54)	11.12*** (4.18)	11.15*** (4.18)
Two Way Communication (D_{TW})	8.21** (3.96)	13.69*** (3.72)	16.32*** (3.84)	16.67*** (4.29)
Barcelona (D_B)		-7.35** (2.84)	-1.06 (4.02)	-0.92 (4.08)
Minimum Effort Round 10 (MinEffort10)		0.66*** (0.17)	0.68*** (0.16)	0.69*** (0.17)
Barcelona * Communication ($D_B * D_C$)			-11.74** (5.53)	-12.20** (6.04)
Short Session (D_{SS})				-0.99 (5.33)
Log Likelihood	-275.60	-265.26	-263.06	-263.05

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

Table 5
Summary of Manager Codings

Category	Description	Frequency of Coding		
		One Way Communication	Two Way Communication	All Communication
1	Ask for Effort (code appropriate sub-categories as well)	0.271	0.443	0.352
1A	Polite	0.067	0.044	0.056
1B	Rude	0.014	0.006	0.010
1C	Specific effort level	0.141	0.357	0.243
2	Negative response (code appropriate sub-categories as well)	0.080	0.115	0.097
2A	Encouraging	0.014	0.023	0.018
2B	Hostile	0.022	0.018	0.020
2C	"Singling" our an employee	0.010	0.046	0.027
3	Positive response (praise, thanks, appreciation, etc)	0.124	0.134	0.129
4	Discuss monetary benefits of high effort (code appropriate sub-categories as well)	0.113	0.111	0.112
4A	Benefits for manager	0.018	0.001	0.010
4B	Benefits for employees	0.040	0.023	0.032
4C	Mutual Benefits	0.059	0.086	0.072
5	Implicit Contracts (code appropriate sub-categories as well)	0.058	0.110	0.082
5A	More effort today leads to higher bonus tomorrow	0.038	0.076	0.056
5B	Lower effort today leads to lower bonus tomorrow	0.005	0.008	0.007
5C	High bonus today, request higher effort in response	0.017	0.022	0.019
6	Laying out a plan (code appropriate sub-categories as well)	0.033	0.172	0.099
6A	Alternating plan	0.011	0.084	0.045
6B	Ratcheting up effort	0.003	0.033	0.017
7	Surprising employees (code appropriate sub-categories as well)	0.008	0.018	0.013
7A	Choosing higher bonus than specified by plan	0.005	0.015	0.010
7B	Choosing lower bonus than specified by plan	0.000	0.002	0.001
8	Encouragement (should not specifically refer to effort)	0.063	0.087	0.074
9	Use of humor	0.012	0.040	0.025
10	Emphasizing the bonus (includes explicitly stating what the bonus will be)	0.075	0.269	0.167
11	Comments about time (code appropriate sub-categories as well)	0.009	0.033	0.020
11A	Need to hurry to get finished	0.001	0.027	0.013
11B	Will be able to leave sooner if cooperate	0.002	0.004	0.003
12	Explicit reference to fairness	0.004	0.007	0.006
13	Explicit references to trust	0.009	0.010	0.009
14	Explicit references to reciprocity	0.004	0.004	0.004
15	Attempts by managers to appear sympathetic	0.012	0.034	0.022
16	Expressing confusion about the rules	0.000	0.008	0.004
17	Clarifying the rules	0.007	0.011	0.009
18	Soliciting feedback from employees (2-way)	0.007	0.077	0.040
19	Giving feedback to employees (2-way, involves responding to messages from employees)	0.000	0.094	0.045
20	Establishing common knowledge (2-way, passing on a message from one employee to other	0.000	0.031	0.015
21	Misunderstanding rules	0.000	0.002	0.001

Table 6
Summary of Employee Codings

Category	Description	Frequency of Coding per Employee
1	Generic response to manager's comments (code appropriate sub-categories as well)	0.207
1A	Positive	0.119
1B	Negative (code appropriate sub-categories as well)	0.053
1C	Asking for clarification	0.008
2	Agreeing to manager's plan	0.105
3	Disagreeing with manager's plan	0.025
4	Giving manager advice	0.087
5	Discuss monetary benefits of high effort (code appropriate sub-categories as well)	0.017
5A	Benefits for manager	0.002
5B	Benefits for employees	0.000
5C	Mutual Benefits	0.014
6	Implicit contracts (code appropriate sub-categories as well)	0.008
6A	Higher bonus today ? more effort tomorrow	0.007
6B	More effort today, request higher bonus tomorrow	0.001
7	Requesting a higher bonus	0.053
8	Attempting to start a dialogue/soliciting feedback from the manager	0.031
9	Negotiating with the manager	0.025
10	Explicit reference to fairness	0.008
11	Explicit references to trust	0.003
12	Explicit references to reciprocity	0.003
13	Rules (code appropriate sub-categories as well)	0.012
13A	Expressing confusion about the rules	0.005
13B	Requesting clarification of the rules	0.003
13C	Clarifying the rules	0.005
14	Comments about time (code appropriate sub-categories as well)	0.017
14A	Need to hurry to get finished	0.015
14B	Will be able to leave sooner if cooperate	0.001
15	Misunderstanding rules	0.005

Table 7
When Are Frequently Coded Comments Likely to Be Made?

Category	Average Lagged Minimum	Current Bonus	Spain	Significance US vs. Spain	Two Way Communication	Significance One vs. Two Way
All Observations	14.91 (1.90)	9.52 (0.24)	0.50 (0.08)	---	0.47 (0.08)	---
All Two Way Observations	18.64 (2.56)	9.91 (0.32)	0.73 (0.10)	---	---	---
Manager Category 1	15.55 (1.90)	9.45 (0.29)	0.54 (0.09)		0.59 (0.09)	*
Manager Category 1c	19.25 (2.09)	9.64 (0.33)	0.61 (0.10)		0.69 (0.09)	**
Manager Category 2	6.92 (1.57)	9.33 (0.44)	0.71 (0.09)		0.58 (0.11)	
Manager Category 3	24.50 (2.51)	10.51 (0.44)	0.38 (0.10)	**	0.55 (0.11)	
Manager Category 4	11.76 (1.72)	9.56 (0.31)	0.61 (0.09)		0.50 (0.10)	
Manager Category 5	14.90 (2.00)	9.83 (0.29)	0.55 (0.11)		0.67 (0.10)	***
Manager Category 6	26.67 (2.05)	10.59 (0.30)	0.56 (0.12)		0.84 (0.08)	***
Manager Category 10	22.52 (1.81)	10.26 (0.26)	0.43 (0.11)	**	0.72 (0.08)	***
Manager Category 18	23.47 (2.89)	10.63 (0.42)	0.55 (0.15)		---	---
Manager Category 19	21.87 (3.98)	9.93 (0.45)	0.79 (0.10)	**	---	---
Employee Category 1	19.51 (2.35)	9.83 (0.33)	0.76 (0.09)	**	---	---
Employee Category 1a	20.65 (2.09)	10.38 (0.35)	0.80 (0.08)	***	---	---
Employee Category 2	20.60 (1.70)	10.13 (0.41)	0.70 (0.12)		---	---
Employee Category 4	16.83 (2.06)	9.58 (0.40)	0.72 (0.12)		---	---

*** Significant Effect on Likelihood of Coding, 1% Level
** Significant Effect on Likelihood of Coding, 5% Level
* Significant Effect on Likelihood of Coding, 10% Level

Table 8
Determinants of Managerial Success
Minimum Effort in Round 10 = 0

Variable	At or Below Median Earnings		Above Median Earnings	
	Rounds 11 - 15	Rounds 11 - 30	Rounds 11 - 15	Rounds 11 - 30
Bonus	8.72 (0.32)	9.24 (0.42)	8.92 (0.39)	10.16 (0.26)
All Categories	.940 (.155)	.911 (.139)	1.879 (.154)	1.836 (.149)
Category 1	.305 (.052)	.280 (.054)	.532 (.047)	.465 (.056)
Category 1c	.085 (.032)	.124 (.035)	.342 (.058)	.385 (.060)
Category 2	.055 (.022)	.126 (.027)	.100 (.031)	.076 (.014)
Category 3	.085 (.029)	.095 (.035)	.121 (.036)	.166 (.028)
Category 4	.125 (.032)	.089 (.028)	.226 (.056)	.138 (.024)
Category 5	.070 (.030)	.060 (.021)	.216 (.038)	.106 (.019)
Category 6	.035 (.021)	.034 (.020)	.079 (.030)	.171 (.031)
Category 10	.010 (.010)	.048 (.012)	.263 (.061)	.289 (.060)
Category 18	.040 (.022)	.028 (.012)	.100 (.033)	.124 (.026)
Category 19	.110 (.041)	.085 (.027)	.070 (.026)	.101 (.023)

Table 9
Ordered Probit Regressions

Variable	Model 1	Model 2	Model 3	Model 4	Model 5
Data Included	All Data Round 11 - 30	All Data Round 12 - 30	All Data Round 11 - 30	Lagged Min. Effort = 0	All Data Round 11 - 29
Number of Observations	1684	1598	1684	854	1598
Dependent Variable	Minimum Effort	Minimum Effort	Minimum Effort	Minimum Effort	Lead Min. Effort
Current Bonus	.037 (.025)	.042* (.024)	.035 (.024)	-.048 (.032)	-.011 (.026)
Change in Bonus	.109*** (.030)	.093*** (.028)	.109*** (.029)	.253*** (.034)	.046*** (.022)
Bonus, Round 11 (Rounds 13 – 30)	.073* (.039)	.051 (.037)	.076* (.039)	.026 (.082)	.051 (.040)
Manager Category 1	.623*** (.147)		.614*** (.144)	.809** (.320)	.253* (.138)
Manager Category 2	-.153 (.240)		-.114 (.227)	-1.194*** (.370)	-.080 (.243)
Manager Category 3	.146 (.142)		.163 (.142)	-.723 (.480)	.019 (.204)
Manager Category 4	.379** (.172)		.348** (.162)	.131 (.255)	.409** (.162)
Manager Category 5	.145 (.169)		.139 (.183)	.098 (.267)	.372** (.168)
Manager Category 6	-.266 (.174)		-.308* (.167)	.819* (.492)	-.435** (.197)
Manager Category 10	.629*** (.194)		.611*** (.200)	.643** (.295)	.645*** (.194)
Manager Category 18	-.983* (.581)		-1.033* (.590)	-.530 (.590)	-.070 (.492)
Manager Category 19	-.609 (.476)		-.699 (.474)	-1.232 (1.196)	-.398 (.417)
Lagged Employee Category 1a		.488 (.761)	.891 (.736)	-1.192 (2.168)	.726 (.894)
Lagged Employee Category 2		.098 (.626)	-.332 (.677)	4.847*** (1.702)	.173 (.702)
Lagged Employee Category 4		1.397*** (.510)	.980* (.520)	-.071 (.995)	-.130 (.430)
Log-likelihood	-1235.91	-1202.50	-1231.70	-328.72	1362.07

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

Note: Beyond estimates reported in this table, the regressions included controls for lagged minimum effort, twice lagged minimum effort, human, communication, two-way Communication, minimum effort in round 10, Barcelona, and a dummy for rounds 21 – 30. A table reporting the full results is available from the authors on request.

Table 10
Marginal Impact on Manager Profits

All Data
1684 observations, Average current profit = 314.63

Category	Description	Significant Effect on Min. Effort	Marginal Effect on Profit
N/A	Bonus	***	-21.53
1	Ask for Effort	***	103.68
2	Negative response		-18.69
3	Positive response (praise, thanks, appreciation, etc)		27.36
4	Discuss monetary benefits of high effort	**	59.44
5	Implicit Contracts		23.34
6	Laying out a plan	*	-49.45
10	Emphasizing the bonus (includes explicitly stating the bonus)	***	105.50
18	Soliciting feedback from employees (2-way only)	*	-146.70
19	Giving feedback to employees (2-way only)		-106.13

Lagged Minimum Effort = 0
854 observations, Average current profit = 140.14

Category	Description	Significant Effect on Min. Effort	Marginal Effect on Profit
N/A	Bonus	***	7.87
1	Ask for Effort	**	39.22
2	Negative response	***	-25.07
3	Positive response (praise, thanks, appreciation, etc)		-18.25
4	Discuss monetary benefits of high effort		5.48
5	Implicit Contracts		4.06
6	Laying out a plan	*	50.88
10	Emphasizing the bonus (includes explicitly stating the bonus)	**	34.92
18	Soliciting feedback from employees (2-way only)		-14.85
19	Giving feedback to employees (2-way only)		-18.21

*** Significant Effect on Minimum Effort, 1% Level
** Significant Effect on Minimum Effort, 5% Level
* Significant Effect on Minimum Effort, 10% Level

Figure 1
Comparison of Minimum Effort Across Treatments
All Firms

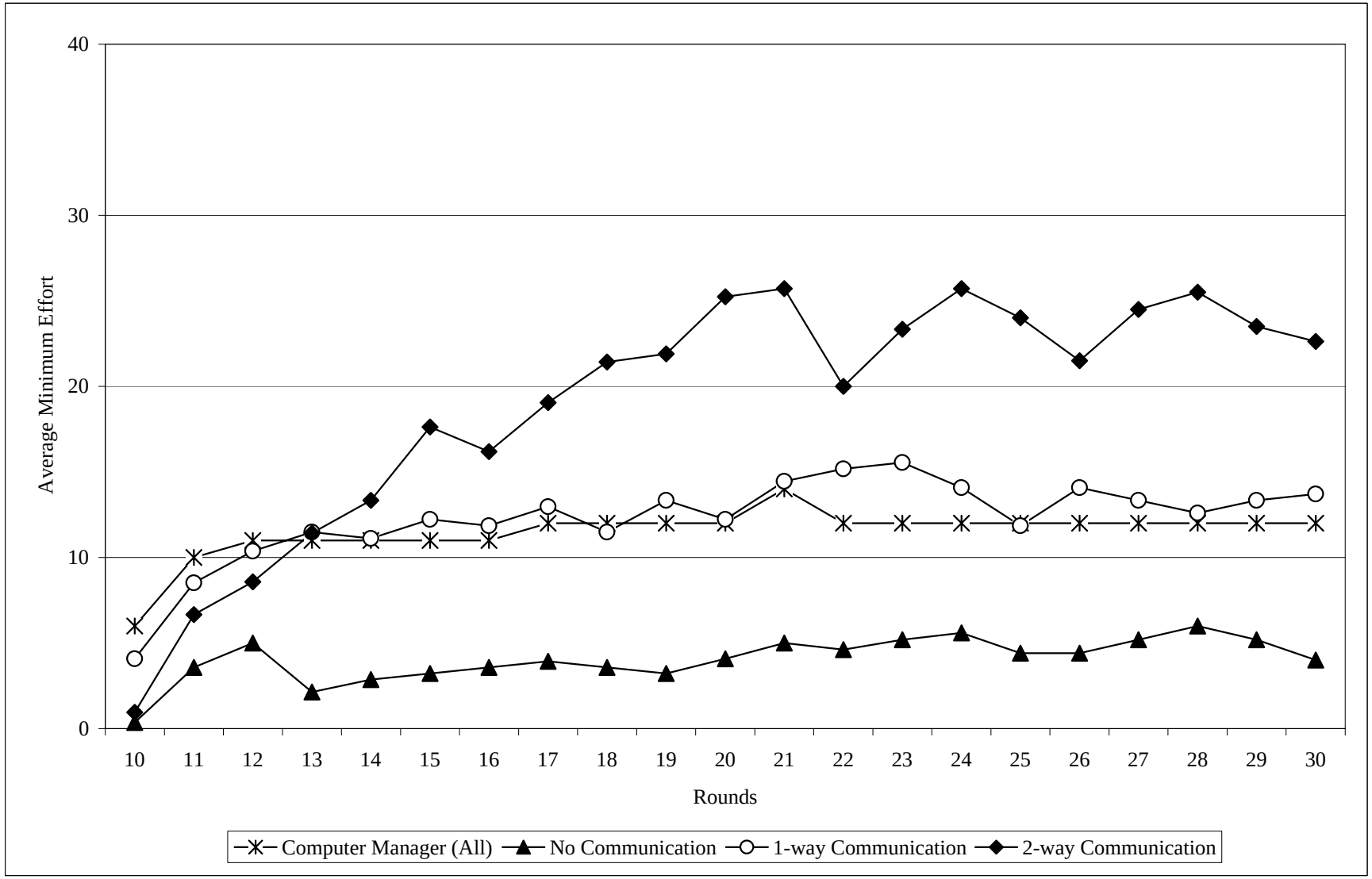

Figure 2
Comparison of Minimum Effort Across Treatments
Firms with Coordination Failure (Minimum Effort = 0 in Round 10)