

## MANAGERIAL LEADERSHIP, TRUTH-TELLING AND EFFICIENT COORDINATION\*

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We study the manager-agent game, a novel coordination game played between a manager and two agents. Unlike commonly studied coordination games, the manager-agent game stresses asymmetric information (agents know the state of the world, but managers do not) *and* asymmetric payoffs (for all states of the world, agents have opposing preferences over outcomes). *Efficient* coordination requires coordinating agents' actions *and* utilising their private information. We vary how agents' actions are chosen (managerial control versus delegation), the mode of communication (none, structured communication or free-form chat) and the channels of communication (i.e., who can communicate with each other). Achieving coordination per se is not challenging, but, averaging across all states of the world, total surplus only surpasses the safe outcome when managerial control is combined with three-way free-form chat. Unlike weak-link games, advice from managers to agents does not increase total surplus. The combination of managerial control and free-form chat works because, under these conditions, agents rarely lie about their private information. Our results suggest that common findings from the experimental literature on lying are not robust to changes in the mode of communication.

There exists a large experimental literature studying whether managers can use communication to coordinate agents' actions on an efficient outcome. This literature focuses on cases like the weak-link game where agents share common and known objectives with each other and the manager. In settings with aligned interests, efficient symmetric outcomes and symmetric information, it is well established that (i) communication among agents and (ii) advice from the manager to agents both increase the probability of efficient coordination. Allowing managers to directly control agents' actions makes efficient coordination trivial in such settings. The manager's task is far more difficult when the underlying problem is asymmetric. Specifically, we are interested in settings that have the following properties. (i) All parties would be better off if the agents coordinate on a common course of action. (ii) Although all parties gain from coordination, they have differing preferences over which a common course of action should be chosen. (iii) The state of the world varies over time, changing which outcome is efficient in the sense of maximising total surplus. (iv) Agents know the state of the world, but the manager does not. The combination of properties (ii) and (iv) implies that agents have an incentive to lie to their manager. Ideally, a

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manager achieves efficient coordination, inducing her agents to coordinate on a common course of action *and* doing so in a way that uses their private information to reach the efficient outcome. Achieving this is non-trivial, given the agents' opposing objectives and the manager's lack of critical information.

Many problems like this arise within organisations. Managers and their agents must make choices about what inputs to use, what people to hire, what products to produce and what strategies to pursue. Gains from coordination are possible when these decisions are made. For example, costs are lower if employees all use the same equipment or software, there are beneficial synergies if middle managers hire workers with complementary skills and there are economies of scale if a firm's stores all sell the same products. But agents often have differing preferences over the available options that are independent from what is good for the organisation as a whole. Workers like to use equipment and software they are already familiar with, middle managers prefer to hire people for the group they manage (e.g., the people in charge of product design always want more people for the product design group even if the firm needs more help in marketing) and store managers want product types that conform to tastes in their specific location. It is natural that agents who are 'in the field' will be better informed than their manager. Workers who actually use a piece of software are better informed about its merits than their boss. Store managers who interact with customers know more about the latest trends in consumer demand than an upper-level manager sitting in a glass tower. The problem is that these agents may have strong incentives to not truthfully report their information in an attempt to influence their manager into enforcing the agent's preferred option.<sup>1</sup>

Can coordination on an efficient outcome be achieved in the face of these difficulties? If the manager imposes a decision on her agents ('managerial control'), can she get her agents to truthfully reveal their information against their interests? If so, does the manager's ability to elicit truthful information depend on what type of communication is possible? More broadly, what is the mechanism behind getting agents to tell the truth? Is it better to delegate decision-making to agents ('delegation'), giving up control in exchange for eliminating the need to extract information? Can the manager still play a useful leadership role while delegating authority by suggesting a course of action to her agents ('managerial advice')? The purpose of this paper is to address these questions.

We examine these issues using a novel coordination game, the manager-agent game ('MA game'). This is a three-player game with one player in the role of manager and two acting as her agents. The four properties listed above are all present: coordinating on a common action that benefits all agents, agents have divergent preferences over possible outcomes, managers lack the necessary information to simply impose efficient coordination on their agents and agents possess the relevant information, but have little reason to truthfully reveal it. The MA game confronts subjects with a challenging coordination problem that accentuates the contrast between managerial control and delegation.

<sup>1</sup> A real-world example is the adoption of standards for new technologies such as HDTV and wireless communication in the 1990s. Europe used a more centralised process than the United States. This was especially true for wireless communication, where the United States largely delegated the problem of choosing a standard to firms. This made some sense, as firms are better informed about the merits of various technologies than a central regulator, but also created a thorny coordination problem. Both network externalities and IP issues gave firms reasons to prefer differing technologies. Indeed, the US market suffered through an extended period where firms failed to coordinate on a single technology and was widely seen as lagging behind Europe (for HDTV, see Farrell and Shapiro, 1992; for wireless communication, see Gandal *et al.*, 2003). Other related examples from the field include software adoption (Brynjolfsson and Kemerer, 1996) and product line selection (Thomas, 2011).

The experimental design features repeated play of the MA game in fixed groups. We vary how actions are chosen (delegation versus managerial control) and what type of communication is available (none, structured communication with pre-specified messages or free-form chat). The baseline treatment features delegation and no communication; this is expected to yield poor performance since there is no mechanism to address the challenging coordination problem faced by agents. Within each type of communication (structured communication or free-form chat) we consider three possible mechanisms for choosing actions: delegation with pre-play communication between agents, delegation with managerial advice and managerial control with messages from agents to the manager.

Under managerial control, the MA game has a unique pure-strategy equilibrium in which the manager receives no information from the agents. Lacking information, the manager requires agents to coordinate on a 'safe outcome' (safe in the sense that agents' actions correspond to the secure equilibrium under delegation with agents always using their maximin actions). Beyond being safe, the safe outcome also has the attractive properties of being simple (the same actions are used in all states of the world) and equalising agents' payoffs on a round-by-round basis. The safe outcome is *not* efficient in terms of maximising total surplus because efficient coordination requires using the agents' information. The safe outcome therefore serves as a benchmark for what successful coordination can achieve *without using agents' information*. With this in mind, our analysis focuses on outperforming the safe outcome.

Turning to the data, total surplus is always higher with managerial control than with delegation. With no communication or structured communication, delegation invariably leads to coordination failure and lower total surplus than the safe outcome. Managerial control with structured communication solves the coordination problem, improving matters over delegation, but rarely achieves *efficient* coordination due to poor information transmission from agents to managers. As a result, total surplus is indistinguishable from the safe outcome. With free-form chat, delegation with no managerial role also solves the coordination problem. There is no issue of information transmission, but agents opt for the safe outcome more often than efficient coordination. The safe outcome offers an easy way to coordinate and agents take advantage of this. Once again, total surplus is no better than in the safe outcome. Managerial advice helps little, as an increase in efficient coordination is offset by an increase in coordination failure. Only with the combination of managerial control and free-form chat is the total surplus significantly greater than the safe outcome, achieving roughly half of the possible efficiency gains. Managerial control, combined with free-form chat, solves the coordination problem *and* makes good use of agents' information.

Why does the combination of free-form chat and managerial control outperform the safe outcome when the theory predicts that agents should transmit no information to managers? Contrary to both the theory and the data with structured communication and managerial control, lying by agents is almost non-existent when free-form chat and managerial control are combined. This yields unambiguous transmission of information, making efficient coordination possible.

The preceding observation raises an important question: why is there so little lying with free-form chat? To address this question, we add a follow-up treatment that uses structured communication, but includes a number of features that mimic important aspects of free-form chat. Enriching structured communication has no effect on performance and less than half the effect of free-form chat on the frequency of lying. These observations strongly suggest that free-form communication is necessary per se for the sharp decrease in lying observed with the combination of managerial control and free-form chat.

Our work contributes to existing research in multiple ways. There is a large experimental literature about the effects of communication, advice and leadership on efficient coordination. (See Section 1 for a full summary of related research.) This research focuses on settings like the weak-link game where the interests of all individuals are aligned and there is no dispute about the most desirable outcome. Communication from a leader, such as advice from the manager to agents, is effective in these settings, but it is relatively easy for the manager to act as a coordination device when everyone has the same information and aligned interests. Achieving efficiency via managerial control is trivial in a weak-link game, presumably explaining why this has not been studied.<sup>2</sup> Like a weak-link game (and unlike a battle-of-the-sexes game), the MA game has an inefficient symmetric equilibrium that makes it easy to coordinate. The challenge facing a leader is *not* to achieve coordination *per se*, but rather to achieve *efficient* coordination. Compared to weak-link games, efficient coordination is difficult in the MA game. Managers need to do more than just overcome strategic risk. They must either induce turn-taking through managerial advice or impose turn-taking via managerial control. Achieving efficient coordination via managerial control is non-trivial in the MA game due to asymmetric information; the manager has to acquire information that their agents have no incentive to provide. Managerial advice proves insufficient to outperform the safe outcome, unlike weak-link games, but the combination of managerial control and free-form chat performs surprisingly well.

Our work also relates to research, both theoretical and experimental, comparing centralisation and decentralisation. The recent literature sees this trade-off in terms of a comparison between the benefits of coordination (meant in a somewhat different sense than here) and the costs of distorted information that accompany centralisation. These trade-offs play an important role in our work as well, but there are major differences. Unlike the games studied in papers like Alonso *et al.* (2008a) and Rantakari (2008), the MA game with delegation is a true coordination game where the critical issue is selecting among multiple equilibria. We stress the role of active leaders using free-form communication to achieve efficient coordination. The effects of free-form communication are particularly relevant for the literature comparing centralisation and decentralisation. Performance with managerial control and free-form chat surpasses the safe outcome because agents reveal more information than is consistent with their financial incentives. Even though the games are significantly different, the same insight presumably applies to comparisons of centralisation and decentralisation—existing theories may systematically underestimate the benefits of centralisation due to overestimating the willingness of agents using free-form communication to distort reports about their information for strategic purposes.

Our work contributes to the large and growing literature on whether and when individuals are willing to lie. The typical finding is that individuals lie less than is payoff maximising, adjust the frequency of lies in response to changing incentives (including both their own and other's payoffs) and frequently use partial lies (neither telling the truth nor lying to the full extent that would maximise profits). When agents are limited to sending a bare message about the state of the world, our data exhibit all of these standard patterns. It is striking that none of these patterns are present in the treatment with managerial control and free-form chat. Agents almost never lie, the frequency of lying does not respond to incentives and partial lies are rare. The follow-up treatment eliminates many possible reasons for the sharp reduction in lying, such as

<sup>2</sup> There is little work on leadership in asymmetric games. The most relevant is the one-way communication treatment in the study by Cooper *et al.* (1989) of communication in the battle of the sexes, but this is equivalent to communication between agents rather than managerial advice. See Section 1 for a detailed discussion. Managerial control is trivial in the battle of the sexes due to the lack of asymmetric information.

agents fact-checking each other (i.e., calling out when the other agent lies), managers requesting truthful messages and asynchronous communication. The relatively high frequency of lies in the follow-up treatment suggests that some effect of free-form communication per se causes the reduction in lying. We speculate in the conclusion on what this effect might be, but it remains an open question why free-form communication has such a strong effect on lying.

Finally, the unwillingness of agents to lie plays a critical role in generating efficient coordination with managerial control and free-form chat. Previous work has found that pre-play communications in the form of free-form chat is more effective than restricted communication at fostering efficiency and cooperation (Brandts *et al.*, 2019), but our work identifies a new channel by which this occurs.

## 1. Related Literature

There are a number of experiments showing that leaders can increase the likelihood of efficient coordination either by leading by example (e.g., Weber *et al.*, 2004; Cartwright *et al.*, 2013; Sahin *et al.*, 2015) or by sending messages (Weber *et al.*, 2001; Cooper, 2006; Brandts and Cooper, 2007; Brandts *et al.*, 2015; Sahin *et al.*, 2015; Cooper *et al.*, 2020).<sup>3</sup> Unlike our work, these papers study symmetric games, mainly weak-link games. In a weak-link game, there is no dispute over what equilibrium should be chosen. The primary role of a leader involves overcoming strategic uncertainty. Choosing the efficient equilibrium is risky, and leaders help by establishing common beliefs that everyone will choose the efficient outcome. With the exception of Cooper *et al.* (2020), asymmetric information does not play an important role in the existing literature. Asymmetric payoffs and the resulting disputes are at the heart of the problem managers face in our experiment, and asymmetric information exacerbates the difficulty.<sup>4</sup>

Closely related, several experiments study the effect of advice on efficient coordination. This includes papers that study advice from either the experimenter (e.g., Van Huyck *et al.*, 1992; Brandts and MacLeod, 1995; Chaudhuri and Paichayontjivit, 2010) or from another subject who has previously played the game (Chaudhuri *et al.*, 2009). Advice can be effective, particularly if it is common knowledge and the interests of advisors and advisees are aligned. Once again, these papers about advice focus on symmetric games where players have aligned interests. We confront managers with the more challenging problem of resolving the conflicting interests of their agents.

Many papers show that pre-play communication among players (as opposed to an external leader) leads to greater efficiency in social dilemmas (e.g., Dawes *et al.*, 1977; Isaac and Walker, 1988; Ostrom *et al.*, 1992; Charness and Dufwenberg, 2006; Cason and Mui, 2007; Cooper and Kühn, 2014) and symmetric coordination games with Pareto-ranked equilibria (e.g., Cooper *et al.*, 1992; Blume and Ortmann, 2007; Kriss *et al.*, 2016; Blume *et al.*, 2017). Especially relevant for our work, Cooper *et al.* (1989) studied the effect of pre-play communication on coordination in a battle-of-the-sexes game, the best-known example of an asymmetric coordination game. Communication is limited to pre-play announcements of intended play. Without communication,

<sup>3</sup> It has also been shown that leaders can increase contributions in public goods games, either leading by example or by transmitting their superior information about the state of the world. See Cooper and Hamman (2021) for a survey of this literature.

<sup>4</sup> In Cooper *et al.* (2020), the leader is better informed than the followers, and the primary problem created by asymmetric information is that the leader has an incentive to make the state of the world appear better than it is, but risks losing her credibility in the long run. In our paper, the problem is that the leader does not know which equilibrium maximises total surplus and the well-informed followers have strong incentives to deceive her.



coordination is difficult due to the lack of a focal equilibrium. With one-way communication, coordination rates are high as the sender can call for her preferred equilibrium and the receiver generally follows. This can be seen as an example of successful leadership in an asymmetric game. There are many differences between our setup and the experiments of Cooper *et al.* (1989), but perhaps the most important is our focus on *efficient* coordination. Coordination is achieved in a number of our treatments, and we have little doubt that one-way communication would promote coordination as well. *Efficient* coordination is an entirely different matter. For Cooper *et al.* (1989), efficient coordination is a non-issue as the two pure-strategy equilibria in the battle-of-the-sexes game are not Pareto ranked.

Cooper *et al.* (1989) found that two-way communication is less effective, although coordination rates improve somewhat with multiple rounds of two-way communication. Our treatment combining structured communication between agents with delegation resembles the Cooper *et al.* (1989) treatment with multiple rounds of two-way communication, but, due to two important differences, we anticipated that pre-play communication would be more effective in the MA game. Unlike the battle-of-the-sexes game, the safe outcome in the MA game provides a simple, safe way of coordinating without asymmetric payoffs. Second, we use partner matching while Cooper *et al.* (1989) used stranger matching. This provides more opportunities for agents to reach an agreement, and also makes it possible to equalise (expected) payoffs while using asymmetric choices. In spite of our optimism, we also observed little effect relative to no communication.

We find large differences between our structured communication treatments and the parallel chat treatments. These findings parallel existing evidence that the pro-efficiency effects of communication are greater with free-form chat than structured communication (e.g., Ben-Ner and Putterman, 2009; Lundquist *et al.*, 2009; Charness and Dufwenberg, 2010; Cooper and Kühn, 2014; Brandts *et al.*, 2016). The mechanism underlying our result, specifically the shift to truth-telling by agents with free-form chat, differs from these previous studies.

An extremely active experimental literature on subjects' willingness to lie has developed over the past fifteen years (e.g., Gneezy, 2005; Erat and Gneezy, 2012; Fischbacher and Föllmi-Heusi, 2013; Gneezy *et al.*, 2018; Abeler *et al.*, 2019). Several striking regularities have emerged: (1) subjects often tell the truth even when lying would pay more,<sup>5</sup> (2) the likelihood of lying is sensitive to incentives and (3) partial lies (failing to either tell the truth or the payoff-maximising lie) are common. In the treatment with structured communication and managerial control, where agents can only send bare messages about the state of the world, all of these regularities are present in agents' messages. However, lying is *not* sensitive to incentives and partial lies are rare in the treatment with managerial control and chat. Messages are observable in both cases and the message space in both cases is sufficient to communicate the full state of the world, yet the nature of truth-telling is quite different. Both theorists and experimenters have made a great deal of progress in understanding why individuals tell partial lies, but we are unaware of any results that explain the differing results with structured communication and chat. The conclusion discusses some possibilities, but a full explanation is beyond the scope of this paper.

Beyond the general literature on lie aversion, our treatment with structured communication and managerial control (SC-MC) is related to work by Lai *et al.* (2015) and Vespa and Wilson (2016). Both papers study information transmission in variations of the multidimensional cheap-talk model with multiple senders proposed by Battaglini (2002). Lai *et al.* (2015) found that

<sup>5</sup> Studies of cheap talk games find a similar bias towards telling the truth (Cai and Wang, 2006; Sánchez-Pagés and Vorsatz, 2007), which could stem from either an aversion to lying or a failure to grasp the strategic benefits of lying. See Blume *et al.* (2023) for contrary evidence.

information transmission is better with two senders than one and is particularly good when the receiver's interests are *in all states* aligned with one of the sender's interests along one of the two dimensions of the state space. Vespa and Wilson (2016) found that receivers perform poorly at extracting information in games where it is relatively difficult (but possible) to infer the state of the world from senders' messages. The MA game with structured messages and managerial control is also a cheap talk game with multiple senders, but differs along a critical dimension from the games studied by Lai *et al.* (2015) and Vespa and Wilson (2016). In keeping with Battaglini (2002), they studied games where the state space is multidimensional and messages fully reveal the state of the world in equilibrium. This contrasts with the MA game where the state space is unidimensional and messages are *not* fully revealing (or even informative) in equilibrium.<sup>6</sup> We intentionally made the informational problems under managerial control as severe as possible. That said, we also observe receivers (managers) struggling to extract information from senders' (agents') messages. If anything, the problem is even more severe as managers make systematic errors even when information extraction is trivial.

Our work has a clear relationship to the extensive literature comparing centralised and decentralised firm management. See Mookherjee (2006) for a survey of the older theoretical literature. Prominent recent examples in the theory literature include Alonso *et al.* (2008a,b; 2015), Hart and Moore (2008), Rantakari (2008), Dessein *et al.* (2010), Hart and Holmstrom (2010). Recent empirical studies using observational data include Thomas (2011) and McElheran (2014), and experiments by Evdokimov and Garfagnini (2019) compare centralisation and decentralisation.

Our work is not intended to test the predictions of any existing theory comparing centralisation and decentralisation, and our focus differs from the recent theoretical literature. Papers like Alonso *et al.* (2008a) and Rantakari (2008) concentrate on the relationship between the parameters of the game and the quality of information flowing between the various agents in equilibrium. The MA game with delegation is a true coordination game with multiple equilibria. Rather than focusing on how the equilibrium changes between organisational structures, we study the effect on the likelihood of efficient coordination with a stress on the roles of active leadership and free-form communication. As noted above, our results have some relevance for the theoretical literature comparing centralisation and decentralisation, but interested readers should see Evdokimov and Garfagnini (2019) for experiments directly testing some of the recent theoretical models. They found support for theoretical predictions from the models of Alonso *et al.* (2008a) and Rantakari (2008).

In our environment, the manager does not decide whether to delegate decision-making rights or keep control. Nevertheless, our work bears some relation to the experimental work on control, power and delegation. The seminal work here is Fehr *et al.* (2013) and Bartling *et al.* (2014). For some more recent work, see Neri and Rommeswinkel (2017), Ferreira *et al.* (2020) and Pikulina and Tergiman (2020).

## 2. The Manager-Agent Game

The MA game confronts subjects with a challenging environment that accentuates the trade-offs between having the manager make decisions for her agents and delegating decisions to the agents. The MA game is played by two agents (A1 and A2) and a manager (M). The basic structure of

<sup>6</sup> See Section 2 below on Battaglini (2002) for a discussion of the unidimensional case.

agent  $i$ 's payoffs is given by

$$\pi_{Ai} = k_1 - k_2 * \text{coordination loss} - k_3 * \text{adaptation loss} - k_4 * \text{state loss}.$$

They face three types of losses. (1) 'Coordination losses' are losses from not choosing the same option as the other agent. In our simple example, it would be difficult to co-author a document if the two engineers used different software packages. Our model assumes that coordination is paramount, so the worst outcome is to have the agents fail to agree on an option. (2) 'Adaptation losses' are losses due to deviations from an agent's most desired outcome (the agent has to 'adapt' to the wants and needs of others). Adaptation losses do *not* depend on the state of the world. To maximise conflict, agents have diametrically opposed tastes in the MA game, with A1's most preferred option being A2's least preferred option (and vice versa). (3) 'State losses' are state dependent, capturing that some options are inherently more or less attractive, depending on the state of the world. If coordination is the foremost concern *and* agents care more about getting their most preferred option than the option that is best for the task at hand, it follows that  $k_2 > k_3 > k_4 > 0$ . Imposing these inequalities makes the game induced by any state of the world into a coordination game where the two agents have diametrically opposed interests.

The manager's payoff is the sum of the agents' payoffs, implying that management seeks to minimise total costs. This represents a setting where the manager is rewarded for how her unit does as a whole, and should not be interpreted as benevolence on the part of the manager. Because the agents have directly opposed interests, adaptation costs play no role in the manager's decisions under managerial control. Anything that makes one agent happier will necessarily make the other agent less happy. The misaligned incentives that play a central role in most principal-agent problems are also present in the MA game since agents care about whether or not coordination occurs at their preferred option, but the manager does not.

### 2.1. Stage Game Payoff Functions

This sub-section formally describes the MA game. There are three players in the game, a manager (M) and two agents (A1 and A2). Let  $\gamma$  denote the state of the world:  $\gamma \in \{1, 2, 3, 4, 5\}$ . As standard nomenclature, we refer to the states of the world by the games they induce (e.g., game 1 for  $\gamma = 1$ ). The value of  $\gamma$  is randomly determined before players take any actions. Draws of  $\gamma$  are independent and identically distributed with each game equally likely. To ease comparisons across treatments, we used the same draw of games for all sessions (although different groups in a session faced different draws). Both agents know the draw of game G, but the manager only knows the ex ante distribution over games. Agents A1 and A2 choose (under delegation) or are assigned (under managerial control) actions  $\alpha_1$  and  $\alpha_2$ :  $\alpha_i \in \{1, 2, 3, 4, 5\}$ . For simplicity, we use the term 'outcome' to refer to the pair of actions  $(\alpha_1, \alpha_2)$  chosen by or assigned to agents A1 and A2.<sup>7</sup> The payoff functions for A1, A2 and M are respectively given by

$$\pi_{A1} = k_1 - k_2|\alpha_1 - \alpha_2| - k_3|\alpha_1 - 5| - k_4|\alpha_1 - \gamma|, \quad (1a)$$

$$\pi_{A2} = k_1 - k_2|\alpha_1 - \alpha_2| - k_3|\alpha_2 - 1| - k_4|\alpha_2 - \gamma|, \quad (1b)$$

$$\pi_M = \pi_{A1} + \pi_{A2}.$$

<sup>7</sup> From a technical point of view, the MA game is a game of imperfect information. The outcome of the game depends on the strategies chosen by players and the state of the world. We abuse terminology to simplify the exposition.



Table 1. *Stage Game Payoffs* ( $k_1 = 54, k_2 = 14, k_3 = 7$  and  $k_4 = 4$ ).

	C1	C2	C3	C4	C5
Game 1					
R1	26, 54, 80	12, 29, 41	-2, 4, 2	-16, -21, -37	-30, -46, -76
R2	15, 40, 55	29, 43, 72	15, 18, 33	1, -7, -6	-13, -32, -45
R3	4, 26, 30	18, 29, 47	32, 32, 64	18, 7, 25	4, -18, -14
R4	-7, 12, 5	7, 15, 22	21, 18, 39	35, 21, 56	21, -4, 17
R5	-18, -2, -20	-4, 1, -3	10, 4, 14	24, 7, 31	38, 10, 48
Game 3					
R1	18, 46, 64	4, 29, 33	-10, 12, 2	-24, -13, -37	-38, -38, -76
R2	15, 32, 47	29, 43, 72	15, 26, 41	1, 1, 2	-13, -24, -37
R3	12, 18, 30	26, 29, 55	40, 40, 80	26, 15, 41	12, -10, 2
R4	1, 4, 5	15, 15, 30	29, 26, 55	43, 29, 72	29, 4, 33
R5	-10, -10, -20	4, 1, 5	18, 12, 30	32, 15, 47	46, 18, 64
Game 5					
R1	10, 38, 48	-4, 21, 17	-18, 4, -14	-32, -13, -45	-46, -30, -76
R2	7, 24, 31	21, 35, 56	7, 18, 25	-7, 1, -6	-21, -16, -37
R3	4, 10, 14	18, 21, 39	32, 32, 64	18, 15, 33	4, -2, 2
R4	1, -4, -3	15, 7, 22	29, 18, 47	43, 29, 72	29, 12, 41
R5	-2, -18, -20	12, -7, 5	26, 4, 30	40, 15, 55	54, 26, 80

Note: Each cell contains the payoffs for A1 ( $\pi_{A1}$ ), A2 ( $\pi_{A2}$ ) and M ( $\pi_M$ ).

For all treatments,  $k_1 = 54, k_2 = 14, k_3 = 7$  and  $k_4 = 4$ . Table 1 displays the payoff tables for  $\gamma = 1, 3$  and 5. Copies of all five payoff tables can be found in [Online Appendix A](#). The three numbers in each cell of Table 1 correspond to the payoffs, denominated in experimental currency units (ECUs), of A1 ( $\pi_{A1}$ ), A2 ( $\pi_{A2}$ ) and M ( $\pi_M$ ). The row and column are the actions chosen by A1 and A2, respectively (or chosen for them by M). The row (R) and column (C) numbers correspond to the actions chosen by the agents (e.g.,  $R3 \equiv \alpha_1 = 3$ ;  $C4 \equiv \alpha_2 = 4$ ).

## 2.2. Equilibrium Under Delegation

With delegation, each agent chooses an action and the manager is a passive bystander. Ignoring the payoff for M, all five games are coordination games with five pure-strategy Nash equilibria where the two agents choose the same action:  $(\alpha_1 = \alpha_2 = 1)$ ,  $(\alpha_1 = \alpha_2 = 2)$ ,  $(\alpha_1 = \alpha_2 = 3)$ ,  $(\alpha_1 = \alpha_2 = 4)$  and  $(\alpha_1 = \alpha_2 = 5)$ . We refer to these outcomes as outcome 1, outcome 2, etc.

In all five games, there is a tension similar to the battle-of-the-sexes game since A1 most prefers outcome 5 as an equilibrium and least prefers outcome 1, the reverse is true for A2 and M prefers the equilibrium that maximises total surplus. This implies that M always wants a different equilibrium than at least one of her agents and wants a different equilibrium than either A1 or A2 in games 2, 3 and 4. Alternative principles for equilibrium selection, such as safety and efficiency, suggest different ways of resolving the tension stemming from agents' differing interests.

Unlike a battle-of-the-sexes game, but similar to a weak-link game, the MA game with delegation offers an equilibrium that is safe, simple and fair, but is *not* efficient in the sense of maximising total surplus. Always coordinating on outcome 3 (the 'safe' outcome) is safe because  $\alpha_i = 3$  is the maximin action for both agents in all five games. Coordinating on the safe outcome in all five games is simple because agents use the same action in all states of the world. It is fair because the safe outcome yields the same payoff to both agents in all five games. Except

in game 3, the safe outcome does *not* maximise total surplus. Despite this, the attractive features of the safe outcome give it drawing power in our data.

All five games have an equilibrium that maximises total surplus. This is always equivalent to the game number (i.e., outcome 1 in game 1, outcome 2 in game 2, etc.). Efficient coordination, where the agents play the surplus-maximising equilibrium in all states of the world, is procedurally fair (i.e., equalises expected payoffs under the veil of ignorance about the state of the world; Bolton *et al.*, 2005), but yields asymmetric payoffs for all games except game 3. Efficient coordination is also relatively complex because the agents must change their actions as the state of the world changes.

### 2.3. Equilibrium Under Managerial Control

The following discussion is based on structured communication, but extends in a straightforward manner to free-form chat. With managerial control, agents do not choose rows and columns directly. After being informed about the state of the world (i.e., game 1, game 2, etc.) each agent independently sends a message to the manager indicating which state of the world has been selected ( $\mu_i \in \{1, 2, 3, 4, 5\}$ ). After receipt of the two messages ( $\mu_1$  and  $\mu_2$ ), the manager chooses both a row and a column ( $\alpha_1$  and  $\alpha_2$ ). She has no knowledge about which game has been selected beyond the initial distribution over states of the world and whatever information she gleans from the agents' messages.

Conditional on enforcing coordination, Equations (1a) and (1b) imply that the manager does not care about the adaptation losses, but the agents do. Given their opposing interests, the agents have no incentive to be truthful with the manager. If both agents always report the game where the efficient outcome is best for them (game 5 for A1, game 1 for A2), the best the manager can do is to choose the safe outcome ( $\alpha_1 = \alpha_2 = 3$ ).<sup>8</sup> Any benefits from the agents' private information are lost and the manager generally will not choose the efficient outcome.

More formally, we can prove the following theorem that implies that the only pure-strategy perfect Bayesian equilibria (PBEs) are babbling equilibria where the safe outcome ( $\alpha_1 = \alpha_2 = 3$ ) is always chosen. In [Online Appendix B](#) we show that the manager must pick the same row and column ( $\alpha_1 = \alpha_2$ ) in any PBE. Given this, we henceforth refer to the manager as choosing a single action in response to the agents' messages.

**THEOREM 1.** *There does not exist a pure-strategy PBE for the MA game with managerial control where the manager chooses different actions for two different states of the world.*

**PROOF.** See [Online Appendix B](#). □

Finite repetition of the MA game with managerial control does not expand the set of possible pure-strategy equilibria to include informative equilibria. There is a unique pure-strategy equilibrium payoff vector in the stage game. The set of equilibrium payoffs only expands if players can take advantage of differing payoffs across stage game equilibria to prevent deviations. If there were payoff differences between the equilibria, a punishment scheme could be constructed to support equilibria that are not babbling equilibria in the repeated game. But, given that payoffs do not vary across equilibria, this is not possible.

<sup>8</sup> Given that payoffs are linear, this is not transparent. Define a manager's error as the difference between the action she chooses (assuming that  $\alpha_1 = \alpha_2$ ) and her payoff-maximising choice, given the state of the world. Choosing the safe equilibrium limits the size of manager errors. If she chooses the safe equilibrium, her average error is 1.2. If she chooses action 2 or 4, the average error rises to 1.4. Choosing 1 or 5, the average error goes up to 2.0.

The absence of an informative equilibrium does *not* reflect a generic property of cheap talk games with multiple senders; such games generically have an informative equilibrium when the state space is multidimensional (Battaglini, 2002). The MA game intentionally gives the two agents diametrically opposed interests over a unidimensional state space. The resulting lack of an informative equilibrium makes information transmission theoretically impossible with managerial control. This is in keeping with our goal of confronting subjects with a challenging environment that accentuates differences between the manager retaining control or delegating choices to her agents. In the quest for efficient coordination, managerial control exchanges the problem of having multiple equilibria for the problem of needing to get agents to reveal information against their interests.

The theory assumes that messages are cheap talk, with agents incurring no costs, pecuniary or psychological, for sending false messages. If we add a psychological cost for sending false messages, as in Kartik (2009), it is trivial to construct cases where truthful revelation is consistent with an equilibrium. For example, let  $c_L|\mu_i - \gamma|$  be agent  $i$ 's psychological cost of lying. If  $c_L > k_3 - k_4$ , there exists an equilibrium in which both agents truthfully reveal their information.<sup>9</sup>

There does not exist a pure-strategy equilibrium that is informative, but there do exist mixed strategy equilibria that are partially informative. These equilibria are delicate; in [Online Appendix B](#) we show that no such equilibria exist if we require that an equilibrium is robust to the introduction of trembles and manager strategies are monotonic. Empirically, we see no evidence of such equilibria occurring. As such, we regard the partially informative equilibria as theoretical oddities rather than predictive of subject behaviour.

#### 2.4. Discussion of the MA Game

A number of the MA game's features were chosen to accentuate specific aspects of the coordination problems facing managers and agents. (1) Without asymmetric information, the problem facing managers under managerial control is trivial as information transmission is a non-issue. (2) Having a single common shock rather than two independent shocks accentuates the difference between managerial control and delegation. Under delegation, asymmetric information plays no role, but agents' conflicting preferences make coordination difficult. With managerial control, coordination is trivial, but achieving efficiency requires the manager to overcome the asymmetric information between her and her agents.<sup>10</sup> (3) The functional forms in (1a) and (1b) use absolute values of differences, rather than squared differences as used by Alonso *et al.* (2008a) and Rantakari (2008). Because of this choice, there are multiple equilibria in the MA game with delegation rather than a single equilibrium.<sup>11</sup> Multiplicity plays a central role in our paper, as the main problem facing managers is trying to achieve efficient coordination rather than defaulting to the safe outcome. (4) We use five possible actions and five states of the world rather than two

<sup>9</sup> If both agents send the same message, the manager chooses the corresponding outcome. If  $\mu_i = 1$  and  $\mu_j = 2$ , where  $i, j \in \{1, 2\}$  and  $i \neq j$ , the manager chooses outcome 2. If  $\mu_i = 4$  and  $\mu_j = 5$ , where  $i, j \in \{1, 2\}$  and  $i \neq j$ , the manager chooses outcome 4. Otherwise, the manager chooses outcome 3.

<sup>10</sup> Models like Alonso *et al.* (2008a) and Rantakari (2008) use two independent shocks. Using the terminology of our paper, this leads to a comparison of information flows between agents under delegation versus information flows from agents to managers under managerial control. Our paper focuses on the coordination problem with delegation and eliminates any issues due to asymmetric information between agents.

<sup>11</sup> With absolute values, an agent maximises his payoff by picking exactly the same action as the other agent. With squared differences, agents want to shade their choice away from their own preferred outcome and towards the other agent's action rather than matching the other agent's action.

(as in a battle-of-the-sexes game) or three. Going from two to three possible actions adds the safe outcome as an equilibrium that plays a critical role in subjects' choices. Going from three to five actions makes it easier to distinguish whether play is consistent with the efficient or safe outcome, since the two are equivalent less frequently, and easier to detect partial lies. (5) To accentuate their differing preferences, agents are paid based solely on their own payoffs rather than a weighted average over the two agents' payoffs. Implicitly, this eliminates incentive schemes that include revenue or profit-sharing components. (6) Finally, the interaction between the manager and agents under managerial control is modeled as a cheap talk game rather than a problem of mechanism design. Implicitly, we assume that the manager cannot commit to a mechanism for eliciting information.

### 3. Experimental Design and Hypotheses

#### 3.1. *Experimental Design and Procedures*

The initial design included seven treatments, broken into three broad categories by type of communication (no communication, structured communication or chat). The six treatments with communication cross two types of communication (structured or free-form chat) with three different interaction structures between the manager and agents (delegation with communication between agents, delegation with advice from managers and managerial control). Before getting into the details of the experimental design, we pause to discuss the rationale for including both of these dimensions.

Structured communication and chat can be seen as extreme types of communication that serve different roles in our design.<sup>12</sup> Structured communication allows for clean tests of theoretical predictions since the message space matches the theoretical model being tested. More generally, the simplicity of structured communication makes it easier to pin down the mechanism by which communication affects outcomes. Free-form chat is richer and offers a more realistic form of communication. There exists ample evidence that free-form chat has differing effects from structured communication (Brandts *et al.*, 2019), generally being more effective at promoting cooperation. There is no existing work that we know of comparing the effects of structured and free-form communication on either coordination or truth-telling.

Turning to the other dimension of the design, the first interaction structure, delegation with communication between agents, gives the manager no role. The latter two treatments, delegation with advice from managers and managerial control, explore different ways in which a manager might try to achieve efficient coordination, either persuading the agents to coordinate efficiently, essentially acting as a focal point, or imposing actions upon the agents. In the absence of asymmetric information, it is trivial to achieve efficient coordination by imposing actions. But, it is no longer obvious which approach will be most effective with the addition of asymmetric information. Our treatments emphasise different approaches to the manager's problem, letting us see which will be more effective.<sup>13</sup>

<sup>12</sup> Section 5 below presents a new treatment where the mode of communication is intermediate between these two extremes.

<sup>13</sup> The treatments with structured communication separately identify the effects of pre-play communication between agents and managerial advice. Given that neither had much effect in isolation, it seems safe to assume that the combination, paralleling treatment **CH/A-D**, would also have little effect.

The seven initial treatments are as follows.

*No communication with delegation (NC-D).* This was the baseline treatment where subjects played the MA game with delegation, as described in Section 2.2, without any additional communication.

*Structured communication between agents with delegation (SC/S-D).* This treatment was identical to the NC-D treatment, except pre-play communication between agents was added. Prior to the agents' choices of actions, each game began with three rounds of messages. Within each round of messages, the agents simultaneously chose a pair of messages suggesting actions for themselves and the other agent. The message space was limited in structured communication treatments; in treatment SC/S-D, for example, the agents chose messages by clicking on radio buttons labelled with the five available actions and could not send any other messages. Agents observed each other's messages at the end of each round of messages. The purpose of having three rounds of messaging (rather than one) was to make it easier for agents to agree upon a course of action.

*Structured communication with advice and delegation (SC/A-D).* This treatment was identical to the NC-D treatment, except the manager sent a message to the agents prior to each round of play. This message suggested actions for both agents in each of the five possible games. In other words, the manager advised a course of action contingent on the realised state of the world. The full message (a  $5 \times 2$  matrix) was shown to both A1 and A2 prior to their choice of actions. The agents knew that both received identical messages.

*Structured communication and managerial control (SC-MC).* In this treatment, subjects played the game with managerial control as described in Section 2.3. In each round, the two agents viewed the state of the world (i.e., game 1, game 2, etc.) and sent simultaneous messages to the manager reporting the state of the world ( $\mu_i \in \{1, 2, 3, 4, 5\}$ ). There was no requirement that these messages be truthful, a point emphasised in the instructions. After receipt of the two messages, M chose both a row and a column. There was no requirement that the row and column match.

*Chat between agents with delegation (CH/S-D).* This treatment was identical to the NC-D treatment, except pre-play chat between agents was added. The agents had two minutes to engage in free-form discussions via chat before choosing actions. They could discuss whatever they chose. In practice, discussions largely focused on the obvious topic, how to play the game. The manager saw the discussion, but could not participate.

*Chat with advice and delegation (CH/A-D).* This treatment was identical to the CH/S-D treatment, except the manager could participate in the chat prior to her agents choosing actions. As in treatment SC/A-D, the manager had no control over the agents' actions, but could advise them. Free-form chat opened many possibilities beyond simply advising a course of action, such as explaining the rationale for adopting efficient coordination or persuading agents of the mutual benefits of trusting each other.



*Chat and managerial control (CH-MC)*. This treatment was identical to the **SC-MC** treatment, except the structured messages about the state of the world were eliminated and replaced by free-form chat between the agents and their manager. The agents were not specifically instructed to share information about the state of the world, but this was a natural and typical topic of conversation, making the structured messages redundant. Again, there was a two-minute time limit. Unlike the **CH/A-D** treatment, the manager had control over the outcome and the agents had reason to not truthfully reveal the state of the world.

Beyond the seven treatments reported in the main text, we ran an additional four treatments. These were modifications of the **NC-D** and **SC-MC** treatments to examine secondary issues. The main experimental design holds incentives fixed to focus on the effects of changing decision-making rights and the types of available communication. The **HSL-D** and **HSL-MC** treatments examine the effect of changing incentives by increasing state losses ( $k_4 = 6$  versus  $k_4 = 4$ ). This reduces the tension between agents, making the efficient outcome more attractive relative to the safe outcome. Play shifts towards efficient coordination, consistent with the change in incentives, but our qualitative conclusions are unaffected. In particular, lowering state losses does not significantly increase efficiency gains relative to the **SC-MC** treatment and efficiency gains remain significantly lower than in the **CH-MC** treatment. In other words, a strong increase in incentives to play the efficient equilibrium has significantly less impact than allowing free-form communication. The **STR-D** and **STR-MC** treatments used stranger matching rather than partner matching. This made efficient coordination harder, as expected, but the effects are not significant and our qualitative conclusions are unaffected. [Online Appendix D](#) provides further description of these four additional treatments and the results.

We used a between-subject design, so each subject participated in just one of the treatments. There were three sessions per treatment and nine three-person groups per session, giving twenty-seven subjects per session, twenty-seven independent groups per treatment and a total of 567 subjects in 189 independent groups.

Subjects played eighteen rounds in all treatments. They were assigned the role of M, S1 or S2 at the beginning of the session and kept these roles throughout the session. Partner matching was used (i.e., participants were matched with the same two subjects throughout the entire experiment). In treatments with delegation, the participants in the M role were pure observers. We did this to keep the possible influence of other-regarding preferences constant across treatments.

The state of the world (i.e., the game being played) was randomly and independently determined at the beginning of each round. Common seeds were used across treatments, limiting the possibility that treatment effects could be driven by differing draws. At the end of each round, subjects received feedback about the realised game and the chosen actions. In the treatments with managerial control, this made it possible for managers to know if an agent had lied about the game being played.

Sessions were run using z-tree (Fischbacher, 2007). Each session began with instructions (see [Online Appendix C](#)). Participants had printed copies of the payoff tables for all five games. Sessions were run at the LINEEX lab at the University of Valencia, with undergraduate students from the university as participants. The payoffs were denominated in experimental currency units, with 1 ECU = 0.2€. Participants received their cumulative earnings for all rounds. Including a 5€ show-up fee, average pay was 19.90€. Sessions lasted approximately an hour.

### 3.2. Hypotheses

Hypothesis H1 below draws on the theory developed in Section 2 to compare the **NC-D** and **SC-MC** treatments. Efficient coordination, which is an equilibrium in the **NC-D** treatment, uses agents' information to achieve the maximum possible total surplus. In the **SC-MC** treatment, only inefficient pure-strategy babbling equilibria exist. Hypothesis H1 follows. This hypothesis was a straw man. The MA game with delegation resembles a battle-of-the-sexes game, a setting where coordination is known to be difficult in the absence of communication (Cooper *et al.*, 1989). Even though the presence of a safe equilibrium should make coordination easier, we still doubted that agents could coordinate, let alone coordinate efficiently, in the absence of communication.

**HYPOTHESIS H1.** Total surplus will be greater in treatment **NC-D** than in treatment **SC-MC**.

The theory predicts play of a babbling equilibrium in the **SC-MC** treatment, implying that agents' messages will be uninformative. Hypothesis H2 below follows. Once again, there were good reasons to be sceptical. Our design differed from most existing experiments, especially since more than one subject sent messages, but the general finding that individuals are reluctant to lie seemed likely to apply.

**HYPOTHESIS H2.** (a) In the **SC-MC** treatment, agents' messages will contain no useful information about the state of the world.

(b) Total surplus will not exceed the payoff from the safe outcome (the babbling equilibrium).

Turning to the treatments with structured communication and delegation, **SC/S-D** and **SC/A-D**, neither type of pre-play communication changes the theoretical prediction. We nevertheless expected total surplus to increase relative to the **NC-D** treatment in both cases. The different types of communication (between agents versus managerial advice) emphasised different mechanisms by which communication might yield efficient coordination. Communication between agents gave them an opportunity to directly coordinate their choices prior to picking actions. The agents did not face any asymmetric information in the **SC/S-D** treatment, but lacked an obvious mechanism for resolving conflicts due to their divergent interests. Cooper *et al.* (1989) observed modest improvements from adding three rounds of bilateral pre-play structure communications to the battle-of-the-sexes game. Based on this evidence, we expected a modest increase in total surplus between the **NC-D** and **SC/S-D** treatments.<sup>14</sup> With managerial advice, we expected managers to act as a coordination device promoting efficient coordination. Because the power to choose actions resides with the agents, asymmetric information should not be an issue in the **SC/A-D** treatment. It is always in the manager's interest to promote efficient coordination, and unlike the **SC/S-D** treatment, agents in the **SC/A-D** treatment have a single, common source of guidance on how to play. We hoped that the benefits of certain coordination would overcome reluctance to accept a less preferred outcome. Thus, we anticipated that efficient coordination would be more likely with managerial advice than communication between agents. The preceding conjectures are summarised in Hypothesis H3 below. This hypothesis is stated relative to the **NC-D** treatment, as the structured communication treatments with delegation modify the **NC-D** treatment, but combining Hypotheses H1 and H3 yields a prediction that both treatments will also yield higher total surplus than the **SC-MC** treatment.

<sup>14</sup> There are differences in the structure of our experiment and game, described in Section 1, that increased our optimism about the relative performance of the **SC/S-D** treatment.

HYPOTHESIS H3. Total surplus will be greater in treatment **SC/A-D** than in treatment **SC/S-D**, and greater in treatment **SC/S-D** than in treatment **NC-D**.

The final hypothesis covers the chat treatments. Many papers have compared the effects of structured communication versus chat. The general finding is that communication has a greater impact on outcomes with chat rather than structured communication (Brandts *et al.*, 2019). Particularly relevant to our current work, Cooper and Kühn (2014) found that free-form communication outperforms structured communication in a two-period Bertrand game, *largely by improving coordination on an efficient equilibrium*. While the games are different, we expected that the ability to make unlimited asynchronous proposals along with the ability to explain proposals would similarly increase efficient coordination under delegation. We therefore expected the chat treatments to yield higher total surplus than the parallel structured communication treatments. We also expected the comparison of total surplus across chat treatments to match the order across structured communication treatments. Consistent with the theory above, we anticipated that agents would not truthfully communicate the state of the world in the **CH-MC** treatment, leading to play of the safe outcome. We expected that chat would solve the coordination problem in the other two treatments (**CH/S-D** and **CH/A-D**) and would improve efficiency beyond the safe equilibrium. Hypothesis H4 below summarises our conjectures.

HYPOTHESIS H4. (a) Treatment **CH/S-D** will yield higher total surplus than treatment **SC/S-D** and treatment **CH/A-D** will yield higher total surplus than treatment **SC/A-D**. No difference is predicted between treatments **SC-MC** and **CH-MC**.

(b) Comparing chat treatments, total surplus will be highest in treatment **CH/A-D**, followed by treatments **CH/S-D** and **CH-MC**.

## 4. Results

Section 4.1 gives an overview of the main treatment effects, and Section 4.2 examines the process underlying these treatment effects.

### 4.1. Treatment Effects

The discussion of treatment effects is based on the second half of the experiment (rounds 10–18) when play has settled down.<sup>15</sup> Versions of Tables 2 and 3 based on the first half of the experiment can be found in Online Appendix A. Unless otherwise noted, statistical tests comparing treatments are Wilcoxon rank-sum tests and comparisons with total surplus from play of the safe outcome are Wilcoxon matched-pair signed-rank tests. The *p*-values come from exact tests. An observation is the average value of the variable in question for a single group over rounds 10–18. Total surpluses from the safe outcome are adjusted for the random draw of games.

Table 2 summarises outcomes by treatment from rounds 10–18. To maximise total surplus, the choices of the two agents need to be coordinated ( $\alpha_1 = \alpha_2$ ) and these choices have to take advantage of the agents' information ( $\alpha_1 = \alpha_2 = \gamma$ ). Along these lines, the first column of Table 2 reports the percentage of games where the choices were coordinated, and the second shows the frequency of efficient coordination *subject to the agents' choices being coordinated*. The final

<sup>15</sup> The average change in total surplus across rounds 1–9, comparing the first and last three round blocks, is more than ten times larger than the change across rounds 10–18 (6.40 versus 0.60).

Table 2. *Summary of Outcomes, Rounds 10–18.*

Treatment	% Coordinate	% Efficient (subject to coordinate)	Total surplus	Efficiency gain (%)
<b>NC-D</b>	69.5	39.1	61.4	–108.3
<b>SC/S-D</b>	77.8	57.1	65.7	–57.5
<b>SC/A-D</b>	69.5	58.7	64.4	–77.4
<b>SC-MC</b>	99.6	45.9	71.7	6.5
<b>CH/S-D</b>	97.5	48.1	72.2	14.0
<b>CH/A-D</b>	90.9	60.6	71.6	2.8
<b>CH-MC</b>	100.0	61.7	75.2	44.3

Table 3. *Frequency of Types of Coordination if  $\gamma \neq 3$ , Rounds 10–18.*

Treatment	% Safe	% Efficient	% Other
<b>NC-D</b>	48.1	12.6	4.9
<b>SC/S-D</b>	36.1	30.6	8.2
<b>SC/A-D</b>	32.8	30.1	4.4
<b>SC-MC</b>	31.1	36.1	32.2
<b>CH/S-D</b>	41.0	36.1	20.8
<b>CH/A-D</b>	29.5	44.8	15.8
<b>CH-MC</b>	34.4	50.8	14.8

two columns give measures of overall performance: the third column of Table 2 shows average total surplus and the fourth column reports the average ‘efficiency gain’. Total surplus is defined as the sum of the payoffs for A1 and A2, equivalent to the manager’s payoff. Efficiency gain is defined as the difference between a group’s total surplus for the nine-round block and the total surplus it would have achieved by playing the babbling equilibrium ( $\alpha_1 = \alpha_2 = 3$ ) throughout, divided by the difference between the total surplus from efficient coordination and the total surplus from the babbling equilibrium. This transformation of the total surplus makes it easier to see how well groups do relative to the babbling equilibrium. Playing the efficient equilibrium yields an efficiency gain of 100%, while the babbling equilibrium leads to an efficiency gain of 0%. Negative efficiency gains reflect failure to coordinate. For treatments where coordination is high, the efficiency game is a good measure of how well groups make use of the agents’ information

Hypothesis H1 hypothesised that total surplus would be greater in treatment **NC-D** than in treatment **SC-MC**. This was a straw man, relying on the best-case scenario of efficient coordination for treatment **NC-D**, and indeed Hypothesis H1 is strongly rejected as total surplus is significantly greater in treatment **SC-MC** than in treatment **NC-D** ( $p < .001$ ). It is not difficult to see the reason for this difference. Managers understand the importance of coordination, leading to a coordination rate of almost 100% in treatment **SC-MC**. Lacking a coordination device, coordination is significantly lower in treatment **NC-D** ( $p < .001$ ).

**RESULT 1.** *Total surplus is significantly higher in treatment SC-MC than in treatment NC-D. The data are not consistent with Hypothesis H1. Significantly lower coordination rates largely explain the lower total surplus in treatment NC-D.*

While performance is far stronger in treatment **SC-MC** than in treatment **NC-D**, it does not follow that much use is made of the agents’ information. The efficiency gain is only 6.5% for treatment **SC-MC**, indicating that little of the possible gain over the babbling equilibrium is

achieved. The difference between total surplus in treatment **SC-MC** and the babbling equilibrium is not statistically significant ( $p = .444$ ).

To help us better understand why performance varies across treatments, Table 3 summarises the frequency of specific outcomes in games 1, 2, 4 and 5. As defined previously, the safe outcome refers to a mutual choice of 3 ( $\alpha_1 = \alpha_2 = 3$ ) and the efficient outcome indicates coordinated choices matching the state of the world ( $\alpha_1 = \alpha_2 = \gamma$ ). ‘Other’ refers to any other outcome where the agents’ actions are coordinated ( $\alpha_1 = \alpha_2$ ), but at neither the safe nor the efficient outcome. Table 3 does not use data from game 3 because the efficient and safe outcomes coincide in this case (hence, the coordination rates in Table 3 do not add up to the figures shown in the first column of Table 2).

Table 3 makes it clear that coordination failure is *not* the only problem in treatment **NC-D**. When agents coordinate and the safe and efficient outcomes do not coincide ( $\gamma \neq 3$ ), they usually coordinate at the safe outcome (73% subject to coordinating) rather than the efficient outcome (19% subject to coordinating). The safe outcome provides a relatively easy route to coordination in the challenging environment of treatment **NC-D**, and agents take advantage of this even though it means *not* using their information.

Matters are a bit more complex in treatment **SC-MC**. In some ways performance is better than the babbling equilibrium. For  $\gamma \neq 3$ , the efficient outcome is slightly more common than the safe outcome. Unfortunately, average total surplus for the 32% of outcomes in the ‘other’ category of coordination is *lower* than could have been achieved via the babbling equilibrium (62.1 versus 67.7). These outcomes often do *not* involve shading the difference between safety and efficiency. For games 1 and 5, 56% of ‘other’ outcomes use actions that are *farther* away from the efficient outcome than the safe outcome. Managers attempt to use their agents’ information, but often do so poorly.

**RESULT 2.** *In treatment NC-D, agents generally coordinate by playing the safe outcome, implying a failure to use their information. Total surplus in treatment SC-MC is almost identical to the babbling equilibrium prediction, consistent with Hypothesis H2(b), but play is consistent with neither the babbling equilibrium (repeated play of the safe outcome) nor efficient coordination. This implies a failure to use the agents’ information.*

The results in Table 2 provide little support for Hypothesis H3. Total surplus in rounds 10–18 is higher for treatments **SC/S-D** and **SC/A-D** than treatment **NC-D**, but the differences are small and not statistically significant ( $p = .156$  and  $p = .422$ , respectively). Total surplus is slightly lower in treatment **SC/A-D** than in treatment **SC/S-D**, rather than higher as predicted. Neither treatment **SC/S-D** nor **SC/A-D** does as well as treatment **SC-MC**, with both differences significant across rounds 10–18 ( $p = .056$  and  $p = .009$ , respectively).

Treatments **SC/S-D** and **SC/A-D** have little effect on total surplus in rounds 10–18 because neither increases the coordination rate significantly relative to treatment **NC-D** ( $p = .325$  and  $p = .947$ , respectively). To the limited extent that these treatments do better than treatment **NC-D**, it is by making better use of agents’ information. Subject to coordinating in  $\gamma \neq 3$ , the frequency of the efficient outcome rises from 19% in treatment **NC-D** to 41% and 45% in treatments **SC/S-D** and **SC/A-D**, respectively.

**RESULT 3.** *Treatments SC/S-D and SC/A-D do not yield significantly higher total surplus than treatment NC-D, and do significantly worse than treatment SC-MC. The data do not support Hypothesis H3.*



The three treatments with free-form chat all yield significantly higher average total surplus across rounds 10–18 than treatment **NC-D** ( $p < .001$  if all three cases). Hypothesis H4(a) fares well with delegation, but not with managerial control; treatments **CH/S-D** and **CH/A-D** yield significantly higher total surplus than the parallel structured communication treatments ( $p = .021$  and  $p = .005$ , respectively), but total surplus is significantly lower in treatment **SC-MC** than in treatment **CH-MC** ( $p = .003$ ). Treatment **CH-MC** also yields significantly higher total surplus than treatment **CH/S-D** ( $p = .014$ ). Strong performance in the **CH-MC** treatment is not due to chat *or* managerial control, but rather the conjunction of the two.<sup>16</sup>

Oddly, total surplus is not significantly higher in treatment **CH-MC** than in treatment **CH/A-D** ( $p = .162$ ) even though total surplus across rounds 10–18 is *lower* on average in treatment **CH/A-D** than in treatment **CH/S-D**. This reflects the high variance of outcomes in the **CH/A-D** treatment. The SD of total surplus is more than double in treatment **CH/A-D** (10.5) than the other two chat treatments (4.7 and 4.0 for treatments **CH/S-D** and **CH-MC**, respectively). Looking at the nine groups in the chat treatments that achieve a perfect total average surplus of 80, treatment **CH/A-D** ties with treatment **CH-MC** for the most at four apiece. But, if we look at the nine *worst* groups, treatment **CH/A-D** again leads with five while treatment **CH-MC** has none. It would be a mistake to describe performance in treatment **CH/A-D** as either good or bad; a more accurate adjective would be ‘erratic’.

Treatment **CH-MC** has by far the highest efficiency gain of any treatment (44.3%), and is the only treatment that yields significantly higher total surplus than repeated play of the safe outcome ( $p < .001$ ).<sup>17</sup> Efficient coordination has two components: agents’ choices must be coordinated and must reflect their information. Treatment **CH-MC** does well on both accounts. It is the only treatment where groups achieve 100% coordination in rounds 10–18. Treatment **CH/S-D** does almost as well at achieving coordination, but treatment **CH/A-D** has a lower coordination rate that largely explains its relatively low total surplus.<sup>18</sup> Not only is coordination 100% perfect in treatment **CH-MC**, but play of the efficient equilibrium is significantly increased relative to either treatment **SC-MC** ( $p = .027$ ) or **CH/S-D** ( $p = .064$ ). Treatment **CH-MC** outperforms these two treatments because of superior use of the agents’ information. This is *not* true for treatment **CH/A-D** where the rate of efficient coordination is only slightly lower than in treatment **CH-MC** ( $p = .449$ ).

**RESULT 4.** *The data are only partially consistent with Hypothesis H4(a). All three chat treatments produce significantly higher total surplus than the parallel structured communication treatments—Hypothesis H4(a) predicts identical surplus for treatments SC-MC and CH-MC.*

**RESULT 5.** *Across all seven treatments, the combination of free-form chat with managerial control yields the highest total surplus. This reflects both high levels of coordination and improved usage of the agents’ information in treatment CH-MC. This is not consistent with Hypothesis H4(b), which predicts that treatment CH-MC will have the lowest total surplus across the three chat treatments.*

To summarise, either managerial control or free-form communication improves performance, but only the combination of both beats repeated play of the safe outcome. The only treatment

<sup>16</sup> Neither the **CH/S-D** ( $p = .664$ ) nor **CH/A-D** ( $p = .267$ ) treatment improves performance significantly over the **SC-MC** treatment.

<sup>17</sup> Equivalent test statistics for treatments **CH/S-D** and **CH/A-D** are  $p = .111$  and  $p = .105$ , respectively.

<sup>18</sup> The average coordination rate in treatment **CH/A-D** hides a great deal of heterogeneity: nineteen of twenty-seven groups achieve 100% coordination, but the other eight groups only have an average coordination rate of 69%.

that **CH-MC** fails to significantly outperform is treatment **CH/A-D**, but this reflects the high variance of outcomes in the latter treatment. Given that treatment **CH-MC** offers higher average total surplus *and* significantly lower risk than treatment **CH/A-D**, it is difficult to argue that treatment **CH-MC** is not doing better. The high performance of treatment **CH-MC** reflects both high coordination rates and a relatively strong ability to use the agents' information. The next section digs into why this treatment does well on both dimensions relative to the other treatments, focusing on how well information is transmitted from agents to managers.

#### 4.2. *Process*

This subsection examines the processes underlying the treatment effects described in Section 4.1 with a focus on information transmission in the treatments with managerial control (**SC-MC** and **CH-MC**). In both cases managers achieve almost perfect coordination, but only in treatment **CH-MC** do managers take advantage of their agents' information to outperform the babbling equilibrium. We show that this reflects what information is communicated to managers and how they utilise it.

Unless otherwise noted, data from all rounds are used in this section. The eventual outcomes in late rounds are strongly affected by the process in early rounds, plus we often compare how managers behave between early (rounds 1–9) and late (rounds 10–18) rounds.

##### 4.2.1. *Structured communication*

Contrary to Hypothesis H3, neither treatment combining delegation with structured communication (**SC/S-D** and **SC/A-D**) has a significant impact on total surplus relative to treatment **NC-D**. The fundamental problem in both cases is a failure to significantly improve the coordination rate. In treatment **SC/S-D**, agents only reach an agreement in 63% of the observations, and the coordination rate falls to 40% without an agreement. Turning to treatment **SC/A-D**, managers often fail to give good advice and, even when they do, agents often fail to follow it. Coordination is *not* recommended in 15% of managers' messages, a figure that improves little with experience (12% in late rounds). Managers often take a conservative approach when they do advise coordination, calling for the safe outcome rather than efficient coordination (37% for both if  $\gamma \neq 3$ ). Agents often fail to coordinate even when recommended to do so (65%), a figure which improves little even if the safe outcome is suggested (71%).

Coordination is not the problem in treatment **SC-MC**; rather, the issue is a lack of *efficient* coordination. Two things have to happen in treatment **SC-MC** for a group to take advantage of the agents' information. The agents have to send messages that are informative about the state of the world, and the manager has to correctly interpret the information contained in their messages. The theory presented in Section 2 focuses on the first issue and concludes that information transmission will fail since the agents have no incentive to send informative messages. Built into the theory is an assumption that the messages would be interpreted correctly if informative. In reality, the messages sent by A1 and A2 contain useful information, but managers make frequent errors in using messages. Total surplus is about the same as predicted by the babbling equilibrium (repeated play of the safe outcome) because the advantages from better-than-expected information transmission are wiped out by errors in using this information.

Table 4 displays the messages sent in treatment **SC-MC** as a function of the game. The data from A2 players have been remapped to be from an A1's point of view, allowing us to combine

Table 4. *Messages as a Function of the Game.*

		Game (mapped)				
		1	2	3	4	5
Message (mapped)	1	44	1	3	4	4
	2	2	71	3	2	2
	3	19	27	155	5	8
	4	32	27	21	143	8
	5	80	69	46	41	155

Table 5. *Manager Choices as a Function of Messages.*

		Agent 2 ( $\mu_2$ )				
		1	2	3	4	5
Agent 1 ( $\mu_1$ )	1	1.38	–	–	–	–
	2	1.57	2.25	–	–	–
	3	2.42	2.58	2.98	–	–
	4	2.72	3.00	3.67	3.71	4.00
	5	2.90	3.08	3.52	4.50	4.79

data for the two roles.<sup>19</sup> If messages are uninformative, as the theory predicts, there should be no correlation between messages and the game being played. Instead, there is strong positive correlation ( $\rho = 0.34$ ). Play of a babbling equilibrium implies that agents only tell the truth in 20% of the observations, but the observed likelihood of truth-telling is 58%. Even when it is least beneficial to do so (game 1 for A1 or game 5 for A2), 25% of messages tell the truth. If truth-telling is solely due to a failure to grasp the strategic value of lying, agents should lie more as they learn that lying pays. This is not the case, with 58% truth-telling in both rounds 1–9 and rounds 10–18. Purely self-interested agents should always send a message corresponding to their most preferred outcome. This is the most common type of lie, but 35% of self-serving lies are partial lies (i.e., the message lies strictly between the true game and the agent's preferred outcome).<sup>20</sup>

**RESULT 6.** *The messages sent by agents are informative. The data are not consistent with Hypothesis H2(a). Partial lies are common.*

On aggregate, managers respond to the information in their agents' messages. Table 5 shows the managers' average choices as a function of the messages sent by the two agents. Cells with five or fewer observations are left blank due to the small amount of data, and we delete the small number of observations (7/486) where the manager did not choose the same action for her two agents. When the two messages coincide, the manager follows the messages closely (but not perfectly). When the two messages differ, the manager's choices generally increase in each agent's message (holding the other's message fixed). The response of managers to messages is strong and statistically significant.<sup>21</sup>

<sup>19</sup> Recall that outcome 5 is the most desired outcome for A1 and outcome 1 is the most desired outcome for A2. We remap games for A2:  $G' = 6 - G$ . Messages are remapped in an analogous fashion:  $\mu_2' = 6 - \mu_2$ .

<sup>20</sup> Self-serving lies are shaded away from the actual game towards the agents' preferred outcome. There are a small number of messages (4.1%) that are shaded in the direction of the *other* agent's preferred outcome.

<sup>21</sup> To establish statistical significance, we ran a regression where the dependent variable is the common action chosen by the manager for her two agents, and the independent variables are the two messages. The parameter estimates are 0.361 and 0.392 with SEs, corrected for clustering at the group level, of 0.051 and 0.049.

Given that agents send useful information and managers respond to their agents' messages, why is total surplus no better than in the babbling equilibrium? The problem is that managers often make choices that seem to be clear errors. For example, when the two agents' messages match ( $\mu_1 = \mu_2$ ), they are almost certainly telling the truth (98%). Not surprisingly, it is an empirical best response to assign both agents the action that corresponds to their messages ( $\alpha_1 = \alpha_2 = \mu_1 = \mu_2$ ), but managers fail to do so in 18% of these observations. Managers making this type of error earn an average payoff of 66.6 ECUs, compared with 79.6 ECUs for those who play the best response. Another common error occurs when A1 and A2 send diametrically opposed messages by choosing  $\mu_1 = 5$  and  $\mu_2 = 1$ . Obviously, at least one of the agents is lying. The safe outcome is the empirical best response to diametrically opposed messages, but only 35% of managers follow this course of action. This type of error also reduces average total surplus (66.9 versus 64.7 ECUs). Making matters worse, managers do not learn to avoid these errors. The frequency of the first type of error falls a bit between the first and second halves of the experiment (19% versus 16%), and the frequency of the second error type increases slightly from 59% to 70%.

One possible explanation for managerial errors is repeated game effects. Over time, agents have the opportunity to establish a reputation for truthfulness. Agents potentially might benefit from building reputations for being truthful and then abandoning them at an opportune moment, and choices that we identify as managerial mistakes might actually represent optimal strategic decisions where managers take advantage of what they have learned about agents' truthfulness to increase their payoffs. In line with this, managers' choices are sensitive to the previous truthfulness of the agents, responding more strongly to messages from agents who have a history of being truthful. However, there is little indication that agents strategically manipulate their reputations, and the data strongly suggest that the managerial decisions we have identified as errors do indeed reflect mistakes rather than strategic choices as part of a reputational equilibrium. See [Online Appendix D](#) for a detailed exploration of these issues.

Managers' errors explain why total surplus in treatment **SC-MC** is no better than repeated play of the safe outcome. To see how well managers could do just by avoiding obvious errors, suppose that they adopt the following simple rule: if the agents' messages agree, choose the action that matches their messages; otherwise, play the safe outcome. This rule yields an average total surplus of 73.1, compared to 71.0 for the babbling equilibrium and 70.8 for the average total surplus actually achieved by managers. The efficiency gain from the simple rule is 24.0%, compared to the -1.6% actually achieved, and it yields significantly higher total surplus than either repeated play of the safe outcome ( $p < .001$ ) or the realised total surplus ( $p = .002$ ). Managers could easily outdo the babbling equilibrium, but fail to effectively use the information transmitted by their agents.

**RESULT 7.** *Managers in treatment **SC-MC** respond to agents' messages, but make frequent errors using the information contained in the messages, causing their failure to beat repeated play of the safe outcome.*

There are three specific things to take away from the various structured communication treatments. First, in all three treatments there is room for improvement. Even in treatment **SC-MC**, the one case where coordination is *not* a problem, little advantage is taken of agents' information. Second, managers are error prone. Whether giving poor advice, being excessively conservative or failing to grasp obvious information from their agents' messages, managers

Table 6. *Frequency of Coding Categories.*

Coding category	CH/S-D	CH/A-D	CH-MC
# Messages (manager)	N/A	3.33	4.56
# Messages (agent)	4.45	3.72	5.30
Any suggestion	93.1%	73.3%	90.7%
Suggest safe outcome	54.1%	37.6%	60.6%
Suggest efficient outcome	48.4%	41.0%	57.7%
Agreement to suggestion	78.9%	54.0%	67.9%
Discuss need to coordinate*	6.4%	3.5%	4.0%
Discuss fairness*	31.8%	34.6%	43.9%
Discuss efficiency	39.4%	16.0%	37.7%
Questions about rules of the experiment*	11.7%	8.8%	15.0%
Questions about how to play*	10.9%	6.0%	14.2%
Explanation*	21.7%	39.3%	32.3%
Ask what game is being played (M)	N/A	14.9%	19.4%
Truthfully reveal game	N/A	28.8%	68.4%
Lie about game	N/A	0.0%	3.4%
Contradict (one tells truth, other lies)	N/A	0.0%	2.5%

*Note:* Categories marked with an asterisk are cases where the two coders had a Cohen's kappa of less than 0.5. These categories were recoded by a third coder.

consistently make mistakes that hold down total surplus. Finally, and most importantly, even though there is no incentive to reveal their information, agents frequently do so. The manner in which they do so would not surprise anyone familiar with the literature on lie aversion; some agents tell the truth, but lying is common, including the frequent use of partial lies.

4.2.2. *Free-form chat*

To evaluate the impact of specific message types in the three chat treatments, we developed a systematic scheme for coding message content. The goal was to quantify communication that might be relevant for the play of the game, avoiding prejudgements about which sorts of messages were important. We employed the methods developed by Cooper and Kagel (2005). After reading a random sample of conversations, we developed a coding scheme. Two research assistants then independently coded the content of all chat conversations. No effort was made to force agreement among coders. For several categories (marked with asterisks in Table 6), the initial two coders had a Cohen's kappa of less than 0.5, indicating relatively low agreement. These categories were recoded by a third coder who was given extensive training in an attempt to improve the quality of the coding. The research assistants were not informed about any hypotheses the co-authors had about the messages. They were told that their job was to simply capture what had been said without concern to the possible effects of what had been said. Coding was binary—a message line was coded as a 1 if it was deemed to contain the relevant category of content and 0 otherwise. We had no requirement on the number of codings for a message line—a coder could check as many or few categories as he or she deemed appropriate. A number of the categories also had sub-categories. For example, the coding scheme has a category for suggesting what actions should be chosen and sub-categories for specific suggestions (e.g., suggesting play of the efficient outcome). A coder was free to check whatever sub-categories they deemed appropriate when the corresponding category was checked off. Our analysis of the coding uses averages across coders unless otherwise noted.

Table 6 reports the frequency of the coding categories, broken down by treatment. Some of the categories are not relevant in treatment **CH/S-D** since the manager cannot send messages, and



hence no figures are reported. ‘Contradict’ is not a category per se, but instead is a combination of the preceding two categories that accounts for cases where one agent truthfully reported what game was being played and the other lied. Table E1 in [Online Appendix E](#) provides a fuller description of the categories. The unit of observation is the entire conversation prior to play in a single round rather than a single message line within that conversation or messages from only one individual in the conversation. So, for example, in 93.1% of the pre-play dialogues in treatment **CH/S-D**, at least one agent suggested what actions should be chosen.

Before discussing the content of messages, the first two lines of Table 6 report the average number of messages sent per round, broken down by role. Managers send significantly more messages in treatment **CH-MC** than in treatment **CH/A-D** ( $p = .016$ ), and agents send significantly more messages in treatment **CH-MC** than in either treatment **CH/S-D** ( $p < .062$ ) or **CH/A-D** ( $p = .005$ ).<sup>22</sup> Recall that total surplus has high variance in treatment **CH/A-D**. Underlying this, managers’ behaviour also has high variance in treatment **CH/A-D**. The three most and the three least talkative managers come from treatment **CH/A-D**, and, more generally, the variance in the frequency of messages sent by managers is higher in treatment **CH/A-D** than in treatment **CH-MC** ( $SD = 3.17$  versus  $SD = 1.99$ ). Our prediction of relatively high performance in treatment **CH/A-D** depended on leadership by managers, but a surprisingly large fraction of managers fails to provide *any* leadership.

Turning to message content, recall that treatment **CH/S-D** significantly improves total surplus relative to treatment **SC/S-D**, the parallel treatment with structured communication. Performance in treatment **SC/S-D** is limited by failures to agree on what actions should be used as well as a tendency to not agree on efficient coordination. Agreements are more frequent in treatment **CH/S-D** than in treatment **SC/S-D** (79% versus 63%). Given that agents almost always coordinate their actions if an agreement is reached (95%), the higher agreement rate translates into improved coordination and, by extension, higher total surplus. Treatment **CH/S-D** does not solve the second problem that plagued treatment **SC/S-D**. In cases where the safe and efficient outcomes do not coincide ( $\gamma \neq 3$ ), only 34% of agreements in treatment **CH/S-D** call for play of the efficient outcome. This differs little from the 32% figure for treatment **SC/S-D**. When agents agree on efficient coordination, they usually follow through (96%), but treatment **CH/S-D** does no better than the safe outcome because such agreements occur too rarely.

**RESULT 8.** *Total surplus is higher in treatment **CH/S-D** than in treatment **SC/S-D** because agreements are much more common in treatment **CH/S-D**. This promotes coordination, but does not improve the likelihood of efficient coordination.*

Total surplus is basically equal in treatments **CH/A-D** and **CH/S-D**, but the factors driving performance differ. Subject to reaching an agreement when  $\gamma \neq 3$ , agreements on efficient coordination are more frequent in treatment **CH/A-D** than in treatment **CH/S-D** (45% versus 34%) and are usually followed (91%). The problem is that agreements of *any* kind are much less frequent in treatment **CH/A-D** than in treatment **CH/S-D** (54% versus 79%),<sup>23</sup> and failing to reach an agreement is associated with lower coordination rates (pooling treatments **CH/A-D** and **CH/S-D**, 71% versus 95%). This does not go away with experience, as the agreement rate

<sup>22</sup> For agents, an observation for the statistical test is the average number of messages sent by the *pair* of agents in a group. The difference between treatments **CH/S-D** and **CH/A-D** is not statistically significant ( $p = .329$ ).

<sup>23</sup> Consistent with the high variance of total surplus, there is more variance in the groups’ ability to reach agreements for treatment **CH/A-D** than treatment **CH/S-D**. Looking at the number of periods (out of eighteen) that a group reaches an agreement, the SD is 4.53 for treatment **CH/A-D** versus 2.87 for treatment **CH/S-D**.

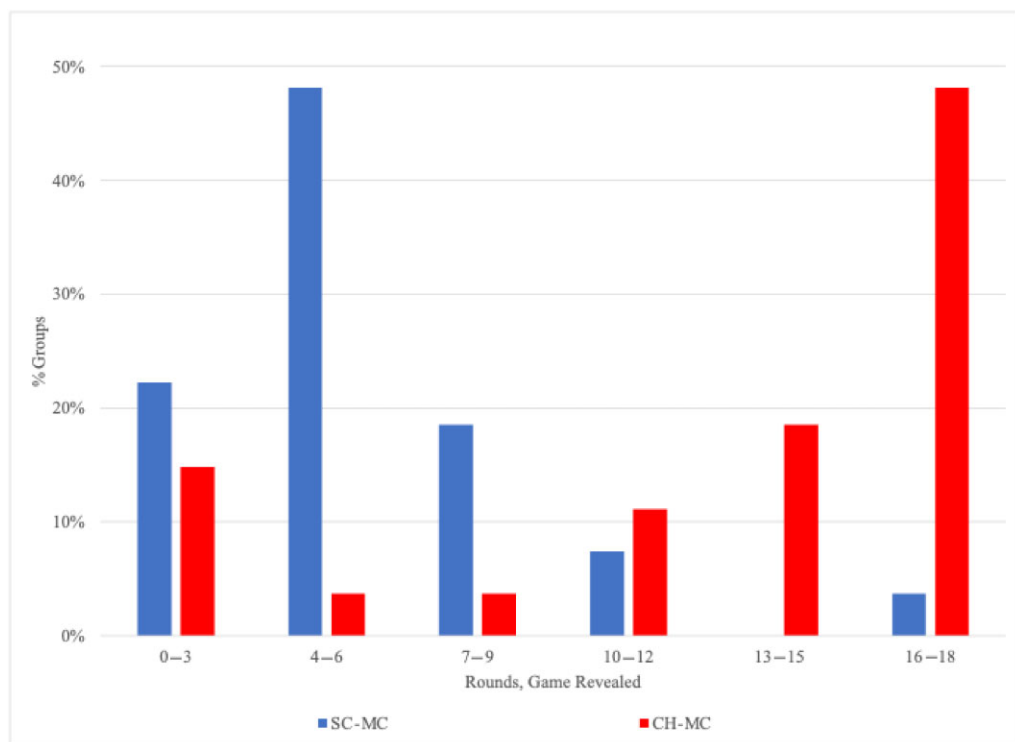


Fig. 1. *Distribution of Rounds in which the Game is Revealed.*

falls slightly from 58% to 50% between the first and second halves of the experiment. The result is an odd combination of lower total surplus and more efficient coordination.

In treatment **CH-MC**, coordination *per se* is trivial; the question is whether the manager can achieve *efficient* coordination, given that she cannot observe the state of the world. A central finding of our work is that managers in treatment **CH-MC** get remarkably good information about what game is being played, making efficient coordination possible. In most cases (68%), at least one agent truthfully reveals the game being played and only rarely (3%) does an agent lie about the game. Rather than falling, these figures improve slightly with experience from 65% and 5% in rounds 1–9 to 72% and 2% in rounds 10–18.

Information transmission is far cleaner in treatment **CH-MC** than in treatment **SC-MC**. This can be seen in Figure 1. Define the game as being ‘revealed’ to the manager in a round if at least one agent tells the truth without contradiction from the other. An observation in Figure 1 is the number of rounds over the entire session where the game was revealed for a group. Figure 1 shows the distribution of how often agents in a group revealed the game. For example, in treatment **SC-MC**, the game was revealed in 0–3 rounds for 22% of the groups. The distribution is shifted to the right for treatment **CH-MC** relative to treatment **SC-MC**. For 89% of groups in treatment **SC-MC**, the game is revealed for *less* than half of the rounds. In treatment **CH-MC**, the game is revealed for *more* than half of the rounds in 78% of the groups.

Managers get some useful information in treatment **SC-MC** (see Section 4.2.1), but it often involves conflicting reports (69%) that are difficult to interpret. On top of this, managers in treat-

Table 7. *Frequency of Truth-Telling and Lying.*

Game (remapped)	SC-MC		CH-MC	
	Truth	Lie	Truth	Lie
1	24.9%	75.1%	46.6%	2.5%
2	36.4%	63.6%	42.8%	3.1%
3	68.0%	32.0%	43.0%	2.2%
4	73.3%	26.7%	50.1%	1.9%
5	87.6%	12.4%	47.1%	0.3%

ment **SC-MC** often make mistakes extracting information from agents' messages. In treatment **CH-MC**, managers receive some report about the game in 69% of observations. For 95% of these cases, they receive a truthful report without contradiction. Almost always, managers in treatment **CH-MC** either have no information, and therefore do not face an information extraction problem, or have unambiguous information that makes information extraction trivial.

The high quality of information transmission in treatment **CH-MC** is enormously important for efficiency. When the safe and efficient outcomes do not coincide ( $\gamma \neq 3$ ), the frequency of efficient coordination rises from 19% when neither agent truthfully reveals the game to 52% if at least one tells the truth.<sup>24</sup> The frequency of efficient coordination changes little when one agent tells the truth and the other lies (53%), albeit based on very few observations. The truth wins in this environment.

RESULT 9. *Better transmission of the agents' information occurs in treatment CH-MC than in treatment SC-MC. This happens because agents frequently tell the truth, almost never lie and rarely confront managers with conflicting reports. Truth-telling is strongly associated with efficient coordination.*

Accurate transmission also takes place in treatment **CH/A-D**. Agents are far less likely to report what game is being played than in treatment **CH-MC**, but always tell the truth when they do so. The lack of lies is less surprising for treatment **CH/A-D** than treatment **CH-MC**: there is little incentive to lie since the manager does not control what actions are chosen. Like treatment **CH-MC**, accurate transmission promotes efficient coordination in treatment **CH/A-D**. The frequency of efficient coordination is 51% when the game is truthfully reported (and  $\gamma \neq 3$ ), compared with 33% otherwise.

The nature of truth-telling strongly differs between treatments **CH-MC** and **SC-MC**. Most agents in treatment **SC-MC** mix between telling the truth and lying: 69% both tell the truth in at least a third of the rounds and lie in at least a third of the rounds. There are only two agents that never lie and none that never tell the truth. Partial lying is common and agents are strategic about telling the truth, doing so more often when it is to their benefit to be believed. This can be seen in Table 7. As in Table 4, the games have been remapped for the A2 role so all observations are from the point of view of A1 (i.e., outcome 1 is the worst outcome and outcome 5 is the best). Agents are most likely to lie when the efficient outcome would be worst for them ( $\gamma = 1$ ), and most likely to be truthful when it would be best for them ( $\gamma = 5$ ).

<sup>24</sup> It may seem surprising that the rate of efficient coordination is not closer to zero when there is not a truthful report and  $\gamma \neq 3$ . In 87% of cases with efficient coordination and no truthful report, there is a suggestion that the efficient equilibrium should be played. These suggestions may serve as an indirect method of revealing the game, making a direct report unnecessary.

These patterns change in treatment **CH-MC**. Mixing between truth-telling and lying is largely non-existent. There are forty-seven subjects in the agent role who send at least one message reporting what game is being played, averaging 9.8 reports over the course of eighteen rounds. Thirty-six of forty-seven reporting agents never lie and another five of forty-seven only lie once. None lie in more than 35% of their reports. Unlike treatment **SC-MC**, there are *no* agents that both tell the truth in at least a third of the rounds and lie in at least a third of the rounds. Subjects mix, but it is almost entirely between telling the truth and not reporting. Subject to lying, partial lies are common (43% of lies), but in absolute terms partial lies are necessarily rare, given the low overall rate of lying. Returning to Table 7, truth-telling is not sensitive to incentives.<sup>25</sup> Telling the truth is about as likely when it is most advantageous for an agent to lie (46.6% in game 1) as when it is most advantageous to tell the truth (47.1% in game 5).

**RESULT 10.** *The frequency of truth-telling, lying and non-reports in treatment CH-MC is not sensitive to what game is being played. Unlike treatment SC-MC, the patterns of truth-telling in treatment CH-MC do not parallel what is typically reported in the literature on lying.*

The different pattern of truth-telling in treatment **CH-MC** suggests that the psychological mechanism underlying truth-telling is altered by the real-time, asynchronous communication available in this treatment. One possible reason for infrequent lying in treatment **CH-MC** is that agents feel guiltier about lying to their manager when they have been directly asked for a report. However, it is surprisingly rare for managers to request reports about what game is being played (19%), and the fraction of lies increases from 2% to 9% when a report is requested. Another possibility is that agents avoid lying because they are concerned about being ‘fact-checked’. In both treatments **SC-MC** and **CH-MC**, the manager knows *ex post* when an agent has lied, but in treatment **CH-MC** it is possible for the other agent to call out a liar in real time. Indeed, in 40% of the observations where an agent lies, the other agent corrects them.<sup>26</sup> It may be more embarrassing to be actively called out as a liar than to merely be revealed as a liar.<sup>27</sup> We explore both of these potential explanations in the follow-up treatment reported in Section 5 below.

Failing to report what game is being played could be considered a ‘soft’ lie. However, agents that do not report the game usually have little reason to do so. If one agent has truthfully revealed the game, there is little need for the other to reiterate this information. In line with this, the other agent has reported truthfully in 44% of the cases where an agent does not make a report. It is also pointless to report what game is being played if the safe outcome will be chosen regardless. Consider cases where the safe and efficient outcomes could be distinguished in the *previous* round ( $\gamma \neq 3$ ). If the safe outcome was played in the previous round, *neither* agent makes a report for the current round in 41% of the observations; this makes sense if agents expect the manager to choose the safe outcome regardless of any new information. By contrast, if agents expect the efficient outcome to be played then they have an incentive to guide the manager’s decision by reporting the current game. Indeed, when the efficient outcome was chosen in a previous round with  $\gamma \neq 3$ , *neither* agent reports for only 16% of the observations. Overall, 78%

<sup>25</sup> Table 7 reports the frequency that an individual agent reports truthfully at some point during the pre-play communication. This differs from the figure reported under ‘Truthfully reveal game’ in Table 6, which shows the frequency that at least one of the two agents reports truthfully at some point during the pre-play communication.

<sup>26</sup> This is different from the figure reported as ‘Contradict’ in Table 6, which measures cases where one agent reported truthfully and the other lied. The 40% figure refers to ‘fact-checking’ where one agent explicitly corrects a false report by the other (e.g., ‘It is game 3’; ‘No, it is really game 2’).

<sup>27</sup> Fact-checking helps explain why the frequency of efficient coordination remains high when one agent tells the truth and the other lies, since fact-checking gives the manager guidance about which agent to believe.

Table 8. *Probit Regressions: Effects of Chat on Outcomes.*

Treatment	CH/S-D		CH/A-D		CH-MC
	Coordination	Efficient coordination	Coordination	Efficient coordination	Efficient coordination
Agreement	0.108*** (0.021)	0.217*** (0.061)	0.114*** (0.025)	0.184** (0.082)	0.045 (0.079)
Discuss need to coordinate	−0.036 (0.048)	0.203 (0.173)	0.032 (0.068)	0.079 (0.231)	−0.089 (0.190)
Discuss fairness	0.013 (0.024)	−0.265*** (0.080)	−0.004 (0.024)	−0.087 (0.088)	−0.101 (0.092)
Discuss efficiency	0.011 (0.026)	0.253*** (0.066)	0.050 (0.038)	0.262*** (0.098)	0.111 (0.088)
Questions about rules	−0.055* (0.030)	−0.061 (0.139)	0.218*** (0.057)	0.112 (0.114)	−0.165 (0.105)
Questions about play	0.033 (0.032)	−0.109 (0.149)	−0.060 (0.053)	0.161 (0.167)	−0.117 (0.152)
Explanation	−0.041 (0.031)	0.056 (0.119)	−0.077** (0.031)	−0.166* (0.086)	0.035 (0.099)
Ask what game			−0.055** (0.023)	−0.121 (0.083)	0.121 (0.084)
Truthfully reveal game			0.023 (0.028)	0.205* (0.118)	0.295*** (0.086)
Lie about game					−0.032 (0.205)

*Note:* All models include 459 observations. Marginal effects are reported. SEs (in parentheses) are corrected for clustering at the group level. All regressions include controls for the game being played, a dummy for late rounds and lagged outcomes. Coefficients for these variables are not reported to save space. \*\*\*, \*\*, \* Significance at the 1%, 5% and 10% levels using two-tailed tests.

of non-reports occur in cases where either the other agent has told the truth or the safe outcome is used. Non-reports largely do not appear to be a form of deception.

4.2.3. *The effect of chat content*

None of the preceding establishes a causal relationship between the content of pre-play communication and outcomes. Establishing causality is tricky because outcomes and the content of communication may both depend on lagged outcomes. Table 8 shows the results of probit regressions that control for lagged outcomes. Separate regressions are shown for each of the three treatments with chat. The dependent variable is either a dummy for coordination ( $\alpha_1 = \alpha_2$ ) or efficient coordination ( $\alpha_1 = \alpha_2 = G$ ). First-round data are dropped to allow the use of lagged variables. There is no regression for coordination in treatment **CH-MC** because there was 100% coordination following round 1.

As independent variables, all regressions include dummies for lagged outcomes (coordination failure, safe coordination and efficient coordination with other coordination as the omitted category), game dummies and a dummy for late rounds (rounds 10–18). These are not reported to save space in the table. All regressions include the average coding for the categories reported in Table 6 with the following exceptions. The categories for ‘Lie about game’ and ‘Contradict’ are highly collinear, so we only include the former (we felt that this was the more interesting of the two). There were no cases of lies in treatment **CD/A-D**, so this variable is dropped. Including suggestions about what actions (e.g., suggest efficient outcome) to play makes the regressions circular (subjects do what they say they should do), so these categories are omitted. We report marginal effects. SEs are corrected for clustering at the group level.



We have stressed the importance of agreements for achieving coordination in treatments **CH/S-D** and **CH/A-D**, and the regressions provide additional evidence of this. In both treatments, there is a strong positive relationship between reaching an agreement and either coordination or efficient coordination. Agreements play little role in treatment **CH-MC**. The manager is a dictator in this treatment and does not need the agents to agree on a course of action. The regressions also support our observation that efficient coordination is likelier in both treatments **CH/A-D** and **CH-MC** when at least one agent reports truthfully, with the effect being stronger in the latter case. Lies, on the other hand, have little effect.<sup>28</sup> It is interesting to note that discussing efficiency has a strong positive effect in treatments **CH/S-D** and **CH/A-D**, but not in treatment **CH-MC**. Once again, this illustrates the importance of control. Managers can impose efficient coordination in treatment **CH-MC** without needing buy-in from their agents. Discussion of fairness plays an important role in treatment **CH/S-D**, making the safe equilibrium more common, but plays surprisingly little role in the other two treatments. Perhaps it is difficult to argue persuasively that the safe outcome is fair when it harms the manager (and the manager has a voice).

Modifying the regressions in Table 8 lets us make an important point about the effectiveness of managers in treatment **CH/A-D**. Studies of leadership in experimental economics typically focus on the *average* effect of having a leader, concluding that leadership does not matter if the average effect is zero. This ignores heterogeneity. The average effect of giving the manager an active role in treatment **CH/A-D** is basically zero, but no group gets the average manager. Each group gets a specific individual who may be a better or worse leader than average. Outcomes in treatment **CH/A-D** are highly variable, suggesting that some managers are better than others. Indeed, the data indicate that how the manager communicates in treatment **CH/A-D** affects outcomes, and good managers are better communicators than their less successful peers.

To reach this conclusion, we first run a probit regression analogous to those reported in Table 8. This regression analyses the effect of communication *by managers* in treatment **CH/A-D** on the likelihood of efficient coordination. The independent variables of interest are the average coding of messages sent by the manager. The regressions control for a number of factors: lagged outcomes, the game being played and a dummy for late rounds. We find that efficient coordination is less (more) likely if the manager suggests the safe (efficient) equilibrium. These effects are large (est. =  $-0.408$  for suggesting the safe equilibrium, and est. =  $0.370$  for suggesting the efficient equilibrium), and both effects are easily significant at the 1% level ( $p = .004$  and  $p < .001$ , respectively). This is not a case where the result is circular, as the manager does not directly control the outcome in treatment **CH/A-D**; the only thing a manager can do in this treatment is give advice.

The preceding results establish that the manager's messages affect outcomes even in the absence of direct control. This suggests that successful managers send better messages than other managers, which is indeed the case. To see this, divide managers into thirds by the average payoffs they achieve and label the top third as 'good' managers. To create an index of suggestion quality, a manager gets a score of +1 if they suggest the efficient equilibrium only,  $-1$  if they suggest the safe equilibrium only and 0 if they suggest either both or neither. We then regress the suggestion quality on whether the group had a good manager, with controls for the game being played and a late period dummy. The effect of having a good manager is large (est. =  $0.179$ ), and statistically significant ( $p = .049$ ). To summarise, good managers are more likely to make good suggestions, and groups that receive good suggestions from their manager are more likely to

<sup>28</sup> This remains true if lies are restricted to cases where the false report was not contradicted in any way. The parameter estimate for uncontradicted lies is  $-0.072$  with an SE of  $0.398$ .

achieve efficient coordination. Two points can be taken from this. (1) The varying group outcomes in treatment **CH/A-D** are not a matter of pure chance, but instead reflect differing performance by their managers. (2) The *average* effect of letting a manager give advice in treatment **CH/A-D** is small, but this is *not* because advice given by managers does not affect outcomes. Rather, on aggregate, the positive effect of good managers is offset by the negative effect of bad managers.

## 5. Extended Communication

Performance in treatment **CH-MC** is unambiguously better than in treatment **SC-MC**. Underlying this, information transmission from agents to managers is unambiguously better in treatment **CH-MC**, driven by a striking reduction in lying by agents. This raises an obvious question: what feature(s) of treatment **CH-MC** leads to the low frequency of lies?

The free-form communication process in treatment **CH-MC** allows for a number of possibilities that are not available in treatment **SC-MC**. (1) Agents can be fact-checked in real time. (2) Managers can request reports about the game as well as requesting that agents tell the truth. (3) Agents can tell managers that they reported truthfully. (4) Because communication is asynchronous, an agent can view the other agent's report before making their own and alter their report after seeing the other agent's report. (5) Agents have the option of not making a report about what game is being played. (6) The free-form nature of chat per se might affect behaviour. Subjects must generate message content endogenously, can use any message rather than the limited set available with structured communication and can frame messages in ways that subtly change their meaning from what is expressed by the pre-specified messages.

There is ample evidence from other papers that the sixth item listed above, free-form communication per se, changes behaviour, generally leading to more prosocial outcomes.<sup>29</sup> However, a priori, the first five differences between treatments **CH-MC** and **SC-MC** could also lead to less lying and can be implemented within structured communication. Treatment **CH-MC** does not provide conclusive evidence that any of these differences caused reduced lying. We therefore developed a follow-up treatment to explore whether any of the five differences other than free-form communication reduce lying. Like treatment **SC-MC**, subjects only had a limited number of messages available. Unlike treatment **SC-MC**, structured communication in the follow-up treatment captures features (1) to (5) listed above: fact-checking of lies was possible, managers could ask for (truthful) reports about the game being played, agents could assert that they told the truth, reports were asynchronous and reports were not mandatory. We included all five of these differences, regardless of whether the **CH-MC** data suggested much impact, to give structured communication the best possible chance to reduce lying. If there is little impact with all of these differences present, it strongly suggests that free-form communication per se is a necessary ingredient for reduced lying and improved information transmission in treatment **CH-MC**.

Since the ability of subjects to communicate is extended relative to the **SC-MC** treatment, we refer to the new treatment as the *extended communication and managerial control* (**EC-MC**) treatment. Communication worked as follows. After the two agents observed which game was being played, there was a 45 second period in which all three players could communicate. This was done by pressing buttons containing prespecified messages. These messages could be sent as many times as a player desired. Once a message was sent, it was displayed in a message window on the screens of all three players along with the identity of the sender. The manager could send

<sup>29</sup> See Brandts *et al.* (2019) for a summary of the evidence.

Table 9. *Comparison of MC Treatments, Rounds 10–18.*

	SC-MC	EC-MC	CH-MC
Total surplus	71.7	71.3	75.2
Efficiency gain	6.5%	–4.9%	44.3%
% Coordinate	99.6%	96.7%	100.0%
% Efficient subject to coordinate	45.9%	55.7%	61.7%
Game revealed (group)	30.9%	49.8%	77.4%
Truth-telling (individual)	58.4%	63.6%	48.3%
Lie (individual)	41.6%	28.6%	1.0%

two possible messages: ‘What game are we playing?’ and ‘Please tell me the truth.’ The agents could send four possible messages: ‘It is game [agent entered a game number]’, ‘I am telling the truth’, ‘The other agent is telling the truth’ and ‘The other agent is lying’.

The **EC-MC** treatment allows for the first five aforementioned mechanisms. If one or more of these mechanism(s) leads to reduced lying and, by extension, improved information transmission, we should expect to see similar results in treatment **EC-MC** as in treatment **CH-MC**. However, if free-form communication itself is the critical ingredient, little difference should be observed between treatments **SC-MC** and **EC-MC**.

Three sessions of treatment **EC-MC** were conducted at LINEEX with the same subject pool and basic procedures as the main treatments. Each session contained nine groups, giving twenty-seven independent observations, like the other treatments.

Table 9 reports the main results for the **EC-MC** treatment, and compares them with results from the other two treatments with managerial control. Total surplus and efficiency gains are slightly lower for treatment **EC-MC** than treatment **SC-MC**, rather than higher as expected, although the difference in total surplus is not statistically significant ( $p = .833$ ). Like treatment **SC-MC**, total surplus in treatment **EC-MC** is not significantly better than the babbling equilibrium ( $p = .356$ ) and is significantly lower than in treatment **CH-MC** ( $p = .028$ ). In terms of total surplus or efficiency, there is no evidence that the extra communication options afforded by treatment **EC-MC** affects outcomes relative to treatment **SC-MC**.

Messages are used frequently in treatment **EC-MC**. Subjects almost always send at least one message in a round (95.2% in rounds 10–18), and send an average of 5.9 messages per round in rounds 10–18.<sup>30</sup> Looking at the third line from the bottom of Table 9, treatment **EC-MC** increases the probability that the game is revealed (at least one agent tells the truth without contradiction from the other) relative to treatment **SC-MC**, albeit less than half as much as treatment **CH-MC** (18.9% versus 46.5%). The increase over treatment **SC-MC** is significant ( $p = .023$ ), although the probability of revealing the game is still significantly lower than in treatment **CH-MC** ( $p = .002$ ). These differences in information transmission reflect decreased lying rather than increased truth-telling. The likelihood that an agent reveals the truth is roughly the same for rounds 10–18 across the three treatments, but the likelihood of lying decreases as richer communication becomes possible (41.6% for treatment **SC-MC**; 28.6% for treatment **EC-MC**; 1.0% for treatment **CH-MC**). When the true game is *not* revealed in treatments **SC-MC** and **EC-MC**, it is usually because one agent tells the truth, but is contradicted by the other agent telling a lie (79.8% for treatment **SC-MC**; 83.6% for treatment **EC-MC**). This rarely happens in

<sup>30</sup> Agents sent more than twice as many messages per round as managers (7.4 versus 3.1) in rounds 10–18.

treatment **CH-MC** (9.1%), because agents who do not tell the truth typically do not report what game is being played rather than lying.<sup>31</sup>

Given that information transmission is better in treatment **EC-MC** than treatment **SC-MC**, why is the total surplus no higher? There is, as one would expect, a strong positive relationship in rounds 10–18 between total surplus in treatment **EC-MC** and whether the game is revealed by the agents or not (74.1 versus 68.5). However, this relationship is stronger in treatment **SC-MC** (78.4 versus 68.7). To understand why, define a mistake by the manager as either a failure to coordinate or coordinating at an action that does not lie at or between efficient coordination and the safe equilibrium.<sup>32</sup> Subject to the game being revealed, managers make a mistake in 14.9% of observations for treatment **EC-MC** versus 1.3% for treatment **SC-MC**. Thus, the benefits of better information are largely balanced out by more frequent managerial mistakes.<sup>33</sup>

We previously outlined six differences between treatments **CH-MC** and **SC-MC**, with five of these also being operational in treatment **EC-MC**. Are any of these differences responsible for reduced lying in treatment **EC-MC** relative to treatment **SC-MC**? The first difference is the possibility of fact-checking. Fact-checking refers to cases where an agent who lies about what game is being played is accused of lying by the other agent. As in treatment **CH-MC**, this is common in treatment **EC-MC**. In rounds 10–18, if an agent lies in their initial report, they are fact-checked 79.4% of the time. The problem is that being fact-checked has no immediate effect on truth-telling; no agent who is fact-checked responds by changing their report to the truth. Instead, it is very common for a fact-checked agent to double down by making additional reports that are also lies (90.7%).<sup>34</sup> Fact-checking does have a small delayed effect. If an individual lies and gets fact-checked, they are somewhat less likely to lie in the *next* game than if they are not called on their lie (37.1% versus 45.2%). However, based on a probit controlling for the game being played and period, this effect is not statistically significant ( $p = .150$ ). We previously speculated that fear of fact-checking led to reduced lying in treatment **CH-MC**, but the **EC-MC** data provide little evidence in favour of this mechanism.

The second difference is the possibility that managers can request truthful reports. This difference explains much of the improved transmission of information in treatment **EC-MC** relative to treatment **SC-MC**. Consider whether the manager either asks for a report or asks agents to tell the truth *prior to any agent reporting what game is being played*.<sup>35</sup> Both types of messages are common in rounds 10–18; managers ask for a report in 80.2% of rounds and ask for the truth in 32.9% of rounds. Asking for a report has no impact on the likelihood that the game being played is revealed (50.0% versus 49.7%), but asking for the truth does have a substantial effect (44.2% versus 61.3%). The latter effect could be biased downwards if requests for truth-telling are more likely when agents have lied in past periods. To control for this possibility, we run a probit regression where the dependent variable is whether the game was revealed truthfully. The independent variables are dummy variables for whether the manager had requested a report,

<sup>31</sup> For 89.1% of cases where the game is *not* revealed in treatment **CH-MC** for rounds 10–18, neither agent reported what game was being played.

<sup>32</sup> For example, in game 1, either not coordinating or coordinating at 4 or 5 are considered mistakes by the manager.

<sup>33</sup> Treatment **CH-MC** lies somewhere between with a mistake rate of 9.0% when the game is revealed. If managers *never* made mistakes when the game was revealed, instead playing the safe equilibrium, average total surplus across rounds 10–18 would have been 71.7 in treatment **SC-MC**, 73.0 in treatment **EC-MC** and 76.0 for treatment **CH-MC**. Without mistakes, treatment **EC-MC** would do a bit better than treatment **SC-MC**, but not as well as treatment **CH-MC**.

<sup>34</sup> It was generally very rare for agents to change an initial report. For rounds 10–18, this only occurred for 0.9% of observations where an initial report was made.

<sup>35</sup> As documented below, agents rarely change their reports after an initial report. We therefore focus on what is said before reports are made, as requests after reports are made can have little effect.

requested the truth, and the lagged dependent variable. The regression also controls for the round and game being played. SEs are clustered at the group level. The estimated marginal effect of requesting the truth is large (est. = 0.188) and significant ( $p = .044$ ).

Two things are worth noting at this point. First, the theory of guilt aversion (Charness and Dufwenberg, 2006) suggests a mechanism by which requests for truthful reports will reduce lying. Under guilt aversion, an agent's willingness to lie is sensitive to their (second-order) beliefs about whether the manager believes they will tell the truth. If requesting the truth implies that agents believe the manager believes the truth will be told, agents will feel guiltier about lying. If agents are guilt averse, requesting the truth will reduce lying, as is observed in treatment **EC-MC**. Second, the preceding cannot be the mechanism behind reduced lying in treatment **CH-MC** because there are virtually no requests for truthful reporting. Our coding scheme for treatment **CH-MC** did not even include a category for requesting the truth because it is so rare. To the extent that treatment **EC-MC** reduces lying, it appears to do so via a different mechanism than treatment **CH-MC**.

A third difference between treatments **EC-MC** and **SC-MC** is that agents can tell managers that they have reported truthfully. This is a common message type: 61.8% of agents who send a report in rounds 10–18 also send a claim that they are being truthful. The data give no evidence that claiming to be truthful is associated with a reduced likelihood of lying. Instead, agents who claim to have told the truth in rounds 10–18 are slightly *more* likely to lie (27.5% versus 33.2%), subject to making a report about what game is being played.

The fourth difference between treatments **EC-MC** and **SC-MC** is that the asynchronous nature of reporting makes it possible for an agent to view the other agent's report before reporting, and possibly alter their report. There appears to be an order effect in the data: limiting the data to agents who make a report, agents who report first are more likely to initially tell the truth than laggards (75.7% versus 61.7%) in rounds 10–18.<sup>36</sup> However, there is a strong relationship between what game is being played and when agents report: an agent is almost twice as likely to report first when it is their most preferred game (66.7%) versus their least preferred game (37.9%). In other words, they are more likely to report quickly in situations where they want to tell the truth. Running a probit that controls for the game being played and period, the effect of reporting first on truth-telling is not statistically significant ( $p = .395$ ). Another possibility is that laggards respond to whether the first report is true. This appears weakly true in the raw data, but in an odd fashion: laggards are more likely to initially lie if the first agent to report was truthful rather than a lie (33.3% versus 27.8%), but also more likely to not make any report (19.8% versus 5.6%). This again is biased by the strong relationship between truth-telling and what game is being played. The results look more sensible in a probit where the dependent variable is whether a laggard lies and controls are included for the game being played and the period. A truthful first report significantly reduces the probability the laggard initially lies—the estimated marginal effect is  $-0.244$  ( $p = .047$ ). Digging further, we ran a multinomial logit with lying as the base category. A truthful first report makes laggards significantly more likely to not report ( $p = .028$ ) and weakly (and not significantly) more likely to initially tell the truth ( $p = .120$ ). In other words, laggards lie less when the first report was true, but this consists more of not reporting rather than telling the truth.

<sup>36</sup> There are thirteen cases where the two agents were recorded as first reporting simultaneously (time is reported in second increments). In these cases, we include *both* agents as reporting first. If we limit the data to cases where one agent is unambiguously first, the percentage of truthful first reports increases slightly to 77.3%.



A final difference between treatments **EC-MC** and **SC-MC** is that agents are required to make a report on what game was being played in treatment **SC-MC**, but not in treatment **EC-MC**. Agents usually still report what game is being played (92% in rounds 10–18). It is not random when non-reports occur. Almost always when one agent reports and the other does not, the agent that reports tells the truth (97%). In 63% of these cases, the agent who does not report sends a message saying that the other agent has told the truth. In other words, many subjects who do not explicitly report tell the truth implicitly. Non-reporting does not seem to matter much, except to the extent that Table 9 slightly under-reports the extent of truth-telling by individuals.<sup>37</sup>

To recapitulate, we identified six differences between treatments **CH-MC** and **SC-MC**, any of which might explain why lying is less frequent and information transmission is better in treatment **CH-MC**. Treatment **EC-MC** allows for five of these differences, but generates less than half of the effect of treatment **CH-MC**, relative to treatment **SC-MC**, on the likelihood that an agent lies or that the game is revealed. One factor that explains decreased lying in treatment **EC-MC**, requests for truthful reporting, cannot explain decreased lying in treatment **CH-MC**. Asynchronous communication explains some of the effect and could explain some of the decreased lying in treatment **CH-MC**, but not enough to account for the near absence of lies in treatment **CH-MC** (even when the first report is truthful, the rate of lying by laggards is well above the near absence of lies observed in treatment **CH-MC**). These observations strongly suggest that the sixth difference plays a central role in the dramatic effect of treatment **CH-MC** on truth-telling: free-form communication appears to be a necessary condition for the sharp decrease in lies observed in treatment **CH-MC**.<sup>38</sup> In the conclusion we discuss why this might be the case.

**RESULT 11.** *The game is significantly more likely to be revealed in treatment **EC-MC** than in treatment **SC-MC**, but significantly less likely than in treatment **CH-MC**. The positive effect of treatment **EC-MC** stems at least in part from requests by managers for truthful reports and asynchronous reporting. Given that less than half of the effects of treatment **CH-MC** on information transmission and lying can be accounted for by the five factors incorporated into treatment **EC-MC**, we conclude that free-form chat per se is a necessary ingredient for the dramatic decrease of lying in treatment **CH-MC** relative to either treatment **SC-MC** or **EC-MC**.*

## 6. Concluding Remarks

This paper studies coordination in a demanding experimental environment, the MA game. It combines four properties that characterise many organisational settings: coordinating on a common course of action benefits everyone, agents have divergent preferences over possible outcomes, managers lack the necessary information to simply impose efficient coordination on their agents and agents have the necessary information, but also have little reason to truthfully reveal it. Unlike the frequently studied weak-link game, the MA game stresses asymmetries: the manager does not know what game is being played, and the agents' interests are misaligned. Achieving coordination in the MA game is not difficult, but achieving efficient coordination that uses agents' information is a challenge. It is well established that either communication among players or external leadership (like managerial advice) increases efficient coordination in symmetric

<sup>37</sup> The same caveat applies in treatment **CH-MC**.

<sup>38</sup> This evidence is related to results reported by Charness and Dufwenberg (2006; 2010). They found that promises made within free-form communication affect behaviour differently from pre-formulated promises.



coordination games. Managerial control has not been previously studied as its likely effect in symmetric coordination games, unlike the MA game, is obvious. Our primary goal is to study the roles of communication and managerial control in achieving efficient coordination in the difficult environment of the MA game.

Achieving efficient coordination in the MA game requires two things: (1) the choices of the agents have to be coordinated and (2) the agents' information must be incorporated into the choice of action. Either free-form communication (chat) or managerial control are sufficient in isolation to solve the coordination problem, but a combination of chat *and* managerial control is necessary to use the agents' information sufficiently well to outperform the babbling equilibrium, gaining almost half of the possible gains over the babbling equilibrium. Free-form chat can seem like a magic bullet in experimental economics, solving all problems with coordination and/or cooperation. In the MA game, neither rich communication nor managerial advice is sufficient. Even though managers lack critical information, managerial control plays a valuable role in enabling groups to make effective use of agents' information.

The key feature that allows the combination of chat and managerial control to function so well is that information transmission is remarkably good. The MA game with managerial control gives agents strong incentives to lie, and with structured communication, agents often do so. Managers receive only limited information and struggle to use it effectively. When free-form communication is used, agents generally reveal what game is being played and rarely lie. Managers take advantage of their resulting good information to frequently impose efficient coordination.

The patterns of communication about the state of the world (i.e., what game is being played) in treatment **CH-MC** are quite different from either what is observed in treatment **SC-MC** or what is typically observed in experiments on truth-telling. That raises the question of why lying is so infrequent in treatment **CH-MC**. Our initial conjecture was that a fear of being fact-checked (being called out on a lie in real time by the other agent) drives the low rate of lying. Indeed, fact-checking is common when lies are told in treatment **CH-MC**. However, the results of the follow-up **EC-MC** treatment argue against this explanation. Fact-checking is once again common in treatment **EC-MC**, but there is little evidence that this leads to reduced lying. Apparently, the possibility of being called out on a lie, either in real-time or with a delay, is not sufficiently embarrassing to deter lying. Instead, the critical ingredient that leads to reduced lying appears to be free-form communication per se.<sup>39</sup>

Our experiments cannot tell us why free-form communication per se decreases lying. One possibility is strictly mechanical: treatment **EC-MC** omits some specific type of message that is necessary to prevent lying. If so, it is not obvious what such a message might be. The coding exercise reported in Section 4.2.2 was intended to capture all content relevant to play of the game. The obvious candidates that might increase truth-telling are available in treatment **EC-MC**. It is possible that there is some other type of message in the **CH-MC** chat, possibly not coded, that decreases lying. A second possibility is that free-form communication decreases lying via a mechanism that only functions if it emerges endogenously. Once again, it is not obvious what such a mechanism might be. Both fact-checking and asynchronous timing are endogenous in treatment **EC-MC**, and managers must endogenously choose whether or not to ask for a report or request the truth. That leaves us with two possibilities that we view as more likely. Language

<sup>39</sup> The use of free-form communication need not always lead to the almost complete elimination of lies we observe. For example, Lundquist *et al.* (2009) observed fewer deceptive lies with free-form messages, but a substantial fraction of lies still occurs (40%). This may reflect the differing structure of communication, as messages in Lundquist *et al.* (2009) were restricted to a single one-way message as opposed to free-form bilateral (or trilateral) communication.

can be subtle and nuanced, with a gap between what is literally said and what is meant. For example, in the American South, the phrase ‘bless your heart’ often implies the opposite of what is being said.<sup>40</sup> Subtleties and nuances that induce greater truth-telling with free-form chat may be missing even with the relatively flexible structured communication of treatment **EC-MC**.<sup>41</sup> Another possibility is that free-form communication changes preferences by reducing social distance between the managers and their agents. The older psychology literature on free-form communication and cooperation finds that free-form communication increases cooperation by promoting group identity (e.g., Orbell *et al.*, 1988). Similar effects could account for reduced lying in treatment **CH-MC**, with greater group identity increasing concerns about maintaining a positive social image by being truthful.<sup>42</sup> All of the preceding is obviously speculative. We establish that free-form chat is necessary to reduce lying in treatment **CH-MC**, but exactly how that works is an open question that will require additional research to answer.

Free-form communication improves efficiency, but it is still necessary that players come up with the right thing to say. This point is made strongly by the **CH/A-D** treatment. Letting the manager give advice to the agents has a minimal effect *on average*, but this disguises a great deal of heterogeneity. Managers can successfully induce more efficient coordination, but this only works when they actually think to suggest efficient coordination to their agents.

We study an intentionally simple game designed to capture a set of features that are present in many organisations. A natural goal for follow-up work is abandoning some of that simplicity in exchange for greater verisimilitude. One possible approach is using subjects with real-world managerial experience as subjects in the manager role. Existing evidence suggests that using managers would *not* affect our results (for coordination games with leaders, see Cooper, 2006; for games in general, see Fréchette, 2015), but it would still be interesting to see how real-world managers approach the MA game. Another possibility is looking at decision-making by groups. Many decisions within organisations are made by groups, and there is an extensive literature suggesting that groups and individuals do not make identical decisions either for games generally or coordination games specifically (Feri *et al.*, 2010).

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Additional Supporting Information may be found in the online version of this article:

### Online Appendix Replication Package

## References

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<sup>40</sup> If said in a certain way, ‘He means well, bless his heart’ is a gentle way of saying he is an idiot.

<sup>41</sup> Along similar lines, Charness and Dufwenberg (2010) found that ‘bare’ promises do not have the same effect as promises expressed in the context of a free-form message. In both cases it is possible to promise trust-worthy behaviour, but something about the richer language available with free-form messages makes subjects more likely to follow through on their promises.

<sup>42</sup> Cohn *et al.* (2022) reported that truthfulness is significantly higher when individuals interact with a human rather than a machine. They posited that an increased sense of ‘closeness’ may be responsible for the result, increasing concerns about being judged.

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