A Road to Efficiency through Communication and Commitment*

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Abstract

We experimentally examine the efficacy of a novel pre-play institution in a well-known coordination game—the minimum-effort game—in which coordination failures are robust and persistent phenomena. This new institution allows agents to communicate while incrementally committing to their words, leading to a distinct theoretical prediction: the efficient outcome is uniquely selected in the extended coordination game. We find that commitment-enhanced communication significantly increases subjects' payoffs and achieves higher efficiency levels than various non-binding forms of communication. We further identify the key ingredients of the institution that are central to achieving such gains.

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1 Introduction

Economic situations often require agents to coordinate their actions, and coordination failures leading to underperformance are pervasive in society. Players face strategic trade-offs in coordination environments; in particular, to achieve better outcomes, they must choose an action that they are typically unwilling to take unless other players do the same. Although players in these environments desire the same outcome, strategic uncertainty leads to coordination failures. The literature seeking to identify institutions to overcome coordination failures uses controlled experimental environments to compare interactions under different institutions.

Given that players' interests are aligned in coordination games and that failure is rooted in uncertainty, institutions formalizing communication have been a natural starting point for attempted solutions to coordination failure. However, experimental evidence on the effects of communication is mixed, and communication alone may not be enough to ensure success, even in a controlled experimental setting.[1] Additionally, many of the studied pre-play interactions lack theoretical implications; therefore, even if a given intervention empirically improves coordination in the laboratory, it is unclear how to isolate the features underlying its success.

In this paper, we experimentally examine a novel institution studied theoretically by Calcagno et al. (2014)—asynchronous revision pre-play—which predicts that the Pareto-efficient profile is the unique outcome of an extended coordination game. In addition to the unique outcome prediction, the theory is used to obtain testable predictions of agents' dynamic behavior throughout the pre-play phase. The institution in Calcagno et al. (2014) formalizes the intuition that agents must prepare the actions that they intend to take at a predetermined deadline, and these preparations are public. As the deadline approaches, each player receives opportunities to update her prepared action at asynchronous and stochastically determined times. Once the players reach the deadline, their most recently prepared actions are implemented, and players' payoffs are determined by these actions only.

In the laboratory setting, we embed the mechanism into a minimum-effort game, in which a player's payoff depends on her own effort choice and the minimum effort chosen

[1] Some communication protocols, such as two-way communication or public announcements, have been documented to increase coordination (see, e.g., Cooper et al. (1992), Chaudhuri et al. (2009), Charness (2000), Blume and Ortmann (2007), and Burton and Sefton (2004)). However, other protocols, such as one-way communication or private advice, have led to coordination failures (see Cooper et al. (1992), and Chaudhuri et al. (2009)).

by the members of her group. We focus on this game because it is a prominent example of a coordination game with multiple Pareto-ranked equilibria. Moreover, a vast experimental literature observes coordination failures in this environment. To bring this institution to the laboratory, we extend its theoretical results. We introduce a discrete instead of a continuous-time pre-play phase. Furthermore, we solve the game for the specific payoff structure and parameters used in the experiment, which leads to the prediction: subjects' initial choices should be the efficient effort. In the main treatment, *revision mechanism* (RM), the pre-play phase starts with all group members choosing an initial effort. If an opportunity arises, they can update this effort during a preparation phase of 60 seconds. Revision opportunities are awarded randomly to each group member, and the probability of two group members revising in the same instant is zero. In a single dynamic graph, each player can see real-time information on all group members' posted effort choices, including the history of posted efforts, revision opportunities, and updates. Players can change their effort on the screen at any time (i.e., change their intention). Still, these revisions will not be publicly posted on the graph unless the player is awarded a revision opportunity. Throughout the 60-second preparation phase, each player is expected to receive eight revision opportunities. At the end of the pre-play phase, the players' most recently revised efforts are implemented.

In this paper, we test whether asynchronous revision pre-play can reduce coordination failures and allow the individuals involved to reach higher payoffs. Focusing on the efficiency gains, our first main result is, thus, that the mechanism increases efficiency by 18 percentage points (ppts) over one round of public cheap-talk messages, which, in turn, increases efficiency by 16 ppts over an environment without any interaction. The efficiency gains achieved with the revision mechanism (efficiency is 82%) are significant not only in comparison with the Baseline (48%) but also with one round of public cheap-talk messages (64%).[2] Furthermore, we show that this efficiency gain follows from a combination of subjects choosing higher effort and subjects better coordinating on any effort profile (Result 1).

Going beyond the efficiency gains, we test two exact predictions of the theory. First, we examine whether this novel mechanism entirely eliminates coordination failures, leading to 100% efficiency. Second, we examine whether 100% of subjects' initial choices are

[2] The effects of one round of public cheap-talk messages found in this paper are similar to the effects documented in the literature. In Blume and Ortmann (2007), similar communication treatment leads to 69% efficiency, an improvement over the 34% efficiency of their baseline treatment.

the efficient effort from the beginning. We show that efficiency in the revision mechanism treatment (82%) and the frequency of players choosing the efficient effort from the start of the pre-play (86%) are both high, but significantly different from the prediction of 100% (Result 2). The unique outcome prediction, while stark, is also rigid, not accounting for factors that could influence behavior; hence, some distance between the point predictions and subjects' behavior is to be expected.[3]

To further understand how the revision mechanism affects behavior and, in particular, to determine whether that effect can be attributed to the forces behind the theoretical results, we study six additional insights from the theory. We begin by investigating the robustness of the efficiency gain provided by the revision mechanism. The theory predicts that the same outcome should be observed independent of the initial effort profile and for various payoff parameters. To test these predictions, we consider two treatments called *random-revision mechanism* (R-RM) and *revision mechanism VHBB* (RM-VHBB). R-RM is similar to RM, except that subjects' initial choices are randomly assigned to them. RM-VHBB is identical to RM, except that we use the main payoff parameters from Van Huyck et al. (1990). The results indicate that subjects' performance in the revision mechanism is invariant to having exogenous initial choices and to a different set of payoff parameters (Result 3).

We then examine two assumptions that are key to the theoretical results. The unique outcome prediction relies on revision opportunities being frequent and asynchronous. To test these predictions, we consider two treatments called *infrequent revision mechanism* (I-RM) and *synchronous revision mechanism treatment* (S-RM). I-RM is similar to RM, except that the frequency of revisions is reduced to one-eighth of the RM frequency. S-RM is identical to RM, except when a group receives a revision, every member of the group receives a revision opportunity. We find that I-RM and S-RM lead to significantly lower efficiency levels than RM; hence, we conclude that the frequency and asynchronicity of revisions contribute significantly to achieving 82% efficiency in RM (Result 4).

The fifth insight is that commitment matters. Since a player may not have a chance to revise her prepared action, players should not treat their own preparations or others' prepared actions as cheap talk. To test this, we introduce the *revision cheap talk* (R-CT) treatment. R-CT follows RM's protocol with one exception; when the 60-second pre-play is over, the subjects in R-CT can choose any effort they wish, and they are in no way

[3] We further evaluate exact theoretical predictions of other treatments and papers in the literature, with similar failures, in online Appendix C.

committed to what they stated during the pre-play. While R-CT reaches 67% efficiency, RM reaches 82%. Therefore, the commitment in RM is a significant factor in achieving higher efficiency rates in RM compared to R-CT (Result 5). The sixth insight is that a player's best response depends not only on the effort profile chosen by others but also on the time left before the deadline. If the deadline is close, then a player should revise her effort to match the minimum of the group—maximizing her own payoff. In contrast, if the deadline is far enough in the future, it is optimal for a player to revise her effort in a forward-thinking way—that is, to revise her effort upwards. The results show that early revisions are vastly forward-thinking in R-RM, while late revisions are payoff-improving (Result 6).

Let us take an overall look at these results and highlight some observations. First, removing any of the key elements behind the revision mechanism—commitment, asynchronicity, and frequency of revision opportunities—leads to significantly lower efficiency than in RM. Therefore, all three components are necessary to achieve the levels of coordination observed in RM. Second, removing either of the key elements reduces efficiency to the levels of S-CT, which is significantly higher than the level of the Baseline. We take this evidence to indicate that while each of the key elements of RM individually generates an improvement over Baseline, only the combination of all leads to a significant improvement over S-CT.

The institution experimentally studied in this paper is predicated on the combination of three ingredients: a setting in which players would like to coordinate their actions; a pre-play phase during which players publicly display their prepared actions; and incremental commitment, as preparations cannot be changed instantaneously. The revision mechanism can be interpreted in two distinct ways. First, it can represent an intentionally designed institution for implementing the efficient outcome. In this sense, our results have practical implications for the designer, as we illuminate the assumptions and forces relevant to the success of this complex theoretical mechanism. Second, the mechanism can be understood as a feature of real-world coordination environments. Although not always formal, scenarios in which preparation, communication, and incremental commitment go hand-in-hand form an integral part of our social lives.

2 Literature Review

A large experimental literature, spurred by Van Huyck et al. (1990), has established that coordination failures—ubiquitous in the real world[4]—are also common in experimental settings. The main contribution of our paper is the examination of how commitment-enhanced pre-play communication can help reduce coordination failures and improve subjects' payoffs. In addition, given the mechanism studied in this paper, our results relate to the literature that focuses on the effects of commitment and real-time interaction on coordination.

In coordination environments, players face a very particular trade-off because their preferences are more aligned with those of others than in most other strategic situations. The main hurdle for coordination and efficiency is the presence of strategic uncertainty. Some researchers argue that costless pre-play communication could eliminate this hurdle. Blume and Ortmann (2007) and Deck and Nikiforakis (2012) implement a cheap-talk communication phase before the actual play in a minimum-effort game. The pre-play communication in Blume and Ortmann (2007) is done with one round of simultaneous public messages, whereas the protocol in Deck and Nikiforakis (2012) allows for richer interaction, with the subjects having one minute to choose an effort level and the ability to revise their chosen effort at any time.[5] Cheap-talk communication improves coordination and boosts efficiency to 69% and 71% (from 34% and 44%)[6] in Blume and Ortmann (2007) and Deck and Nikiforakis (2012), respectively. Despite the ability to update the messages at any second in Deck and Nikiforakis (2012), the efficiency levels in these papers are similar, suggesting that multiple rounds of cheap-talk communication do little to improve efficiency over a single round. Moreover, the gains from the baseline are higher in Blume and Ortmann (2007) than in Deck and Nikiforakis (2012). Similarly, in this paper, we find that cheap-talk treatments produce similar efficiency levels, regardless of whether pre-play communication consists of simultaneous one-shot public messages, multi-round rich communication, or a richer message space.

[4] For instance, see Rosenstein-Rodan (1943), Murphy et al. (1989), Matsuyama (1991), Rodrik (1996), and Li (2012).

[5] See, also, Brandts et al. (2015) and Bornstein et al. (2002) for alternative mechanisms to increase coordination, through leadership and intergroup competition.

[6] We use normalized efficiency throughout this paper, as it summarizes the strength of the treatments. Also, normalized efficiency allows us to compare results from frameworks with different payoffs or group sizes. In particular, our paper, Blume and Ortmann (2007), and Deck and Nikiforakis (2012) each use a different payoff specification. Group size in the current paper is the same as in Deck and Nikiforakis (2012) but is different than in Blume and Ortmann (2007). We provide more details of the measure in the results section.

The communication studied in this paper contains no explicit cost of sending messages. However, there is an implicit cost of communication–the inability to revise the intended effort choices instantly. Van Huyck et al. (1993) and Devetag (2005) consider a costly form of pre-play communication (a pre-play auction in each round) and conclude that such an extension enables the players to achieve better coordination on the payoff-dominant profile in coordination games. Kriss et al. (2016) study the effects of costly and voluntary communication with full and partial subsidies on coordination in a minimum-effort game. The authors find that even a small cost of message sending deters subjects from communication and leads to high coordination failures. Fehr (2017) endogenizes the presence of pre-play communication in an environment in which two groups with prior coordination history are merged. The author finds that most subjects are unwilling to pay a small cost of establishing and maintaining pre-play communication.

To our knowledge, our paper is the first to experimentally study the effects of incremental commitment in a coordination game. The impact of incremental commitment on cooperation has been studied in the context of public goods games. Building on insights by Schelling (1960), Dorsey (1992) is the first to introduce revisions and real-time monitoring in a voluntary contribution mechanism. Looking at those results from a different perspective, Duffy et al. (2007) test theoretical predictions about the dynamic voluntary-contribution game in Marx and Matthews (2000) and show that, whereas a dynamic setting increases the rate of contributions compared with a static setting, the results do not seem to be driven by the theoretically identified forces. Fundamental differences exist between the forces that impede coordination on the Pareto-efficient equilibrium in coordination games and the forces that drive the lack of cooperation in the public-goods provision and social dilemmas. In the latter, the trade-off is between efficiency and individual rationality. In contrast, in coordination games, the miscoordination is a result of the multiplicity of equilibria along with a lack of selection criteria, leading to strategic uncertainty. Contrasting our results with those of Duffy et al. (2007) highlights how different the two settings are. In this paper, we show that pre-play revisions significantly improve efficiency in coordination games, and we are also able to highlight the critical assumptions for its success.

Roy (2017) experimentally studies market competitiveness in a Cournot duopoly in which firms can simultaneously revise their targeted quantities before the final production. Building on the revision-games theoretical results in Kamada and Kandori (2017), Roy (2017) tests the prediction that a synchronous revision Cournot duopoly may result in higher collusion than in the case without stochastic interaction. Although the theories that predict more collusion in Roy (2017) and higher coordination in our paper have some overlap, the forces behind them are fundamentally different. First, when a revision phase is introduced to a Cournot duopoly, the set of possible equilibrium outcomes increases, and collusion becomes theoretically sustainable. By contrast, the introduction of a revision phase to coordination games shrinks the set of equilibrium-supported outcomes to the unique Pareto-efficient profile. Inefficiencies in coordination settings and the Cournot duopoly arise from fundamentally different forces, calling for distinct mitigation mechanisms.

3 General Framework

3.1 Component Game

Consider a normal-form game $(I, (E)_{i \in I}, (\pi_i)_{i \in I}))$, where I is a finite set of players, $I = \{1, 2, ..., n\}$; E is a finite set of effort levels available to each player i; and $\pi_i(\mathbf{e})$ is the payoff for player i given the strategy profile $\mathbf{e} \in \mathbf{E}$, where $\mathbf{e} = (e_i)_{i \in I}$ and $\mathbf{E} = \prod_{i \in I} E$. Let $\bar{e}$ ($\underline{e}$) be the highest (lowest) element of E and let $\bar{\mathbf{e}}$ ($\underline{\mathbf{e}}$) be the profile for which all players choose $\bar{e}$ ($\underline{e}$).

The results presented in Calcagno et al. (2014) and the results discussed here hold for a wide class of games with common interest.[7] In our experiments, we focus on a particular payoff structure, the minimum-effort game, with payoffs given by

$$\pi_i(\mathbf{e}) = \gamma + \alpha \cdot \min_{j \in I} e_j - \beta \cdot e_i, \tag{1}$$

where $\alpha > \beta > 0$. A player's payoff decreases with a higher choice of effort and increases with the minimum effort among all the players.

Equilibrium analysis of the minimum-effort game: In the minimum-effort game described above, every profile in which all players choose the same pure strategy is a strict Nash equilibrium. These equilibria can be Pareto ranked by the effort choice: the higher the effort, the more efficient is the equilibrium. In particular, $\pi_i(\bar{\mathbf{e}})$ is the highest equilibrium payoff, whereas $\pi_i(\underline{\mathbf{e}})$ is the lowest equilibrium payoff. Note that the minimum-effort game is a game of common interest, as the strategy profile $\bar{\mathbf{e}}$ strictly Pareto dominates any other profile.

We now introduce a definition capturing the level of payoff similarity in a wide set of

[7] A game is a common-interest game if it has a strictly Pareto-dominant action profile.

common interest games, following Calcagno et al. (2014).[8] We then adapt it to our context, applying it to the minimum-effort game.

Definition 1 *Calcagno et al. (2014)*
A component game with common interest is a K-coordination game if, for any pair of players $i, j \in I$ and strategy profile $\mathbf{e} \in \mathbf{E}$,

$$\frac{\pi_i(\bar{\mathbf{e}}) - \pi_i(\mathbf{e})}{\pi_i(\bar{\mathbf{e}}) - \pi_i(\underline{\mathbf{e}})} \leq K \frac{\pi_j(\bar{\mathbf{e}}) - \pi_j(\mathbf{e})}{\pi_j(\bar{\mathbf{e}}) - \pi_j(\underline{\mathbf{e}})}. \tag{2}$$

A game is a K-coordination game if each player can decrease other players' payoffs by at most K times their own cost of punishment. The constant K captures how similar the players' payoffs are between different action profiles. Applying this to the minimum-effort game, if a player choosing the minimum effort reduces their effort choice by one unit, then their own payoff decreases by $\alpha - \beta$, while other players' payoffs decrease by α. In a general game, the smaller K is, the more similar players' preferences are. In particular, if $K = 1$, the game is a pure coordination game, and players have identical payoffs for any outcome. Any finite game with common interest is a K-coordination game for some finite constant $K \geq 1$. Finally, given the payoff structure of the minimum-effort game, the definition can be further simplified: a component game is a K-coordination game if $\frac{\alpha}{\alpha-\beta} \leq K$.

3.2 Asynchronous Revision Game

Consider an environment in which players must prepare their actions before they execute them. We follow Calcagno et al. (2014) in modeling this as an asynchronous revision game: there is a pre-play phase, during which a player can revise their prepared action only if a revision opportunity is awarded to them. At the end of the pre-play phase, the most recently prepared action profile is played, and players collect the payoff associated with that action profile. While Calcagno et al. (2014) model an asynchronous revision game with a continuous-time pre-play phase and revisions governed by independent Poisson processes, we extend their results to an environment with discrete time and multinomial revisions.

Formally, we model this as an asynchronous revision game with discrete time, $t \in \{-T, ..., -1, 0\}$. The game proceeds as follows. First, at time $-T$, an initial effort profile is in place. It can be exogenously given to the players, or each player can simultaneously and independently choose an effort level before the pre-play starts. Second, during the

[8] See Takahashi (2005) for the definition and discussion of the concept.

pre-play phase, $t < 0$, each player obtains revision opportunities according to a random process with a symmetric arrival rate. At each instant, a revision opportunity is awarded to the group with probability $p \in (0, 1]$. If a revision opportunity is awarded to the group, then it is allocated to one of the players with equal probability. Third, at the end of the countdown, $t = 0$, the posted effort profile is implemented, and each player receives the payoff as specified in the component game.

This is a sequential game with multiple rounds of asynchronous play and perfect information, as players observe all the past events in the revision game. The natural solution concept is subgame perfect equilibrium. We refer to a subgame perfect equilibrium of a revision game as *revision equilibrium*.

We now present the main theoretical result, which is an extension of the result in Calcagno et al. (2014) to the framework presented above. Proposition 1 formalizes the intuition that a player can, if far from the deadline, revise her effort choice upwards with an eye on leading others to follow her. If all follow and the group reaches the efficient profile, then players do not revise their choices until the deadline; if players do not follow, then she can backpedal her effort choice. Doing so has a low cost, as the deadline is far and, hence, the chance of no further revisions is very small. The proof is presented in online Appendix A.

Proposition 1 *In a discrete-time asynchronous revision game with a symmetric arrival rate of revision opportunities, if the component game is a K-coordination game with the strict Pareto-dominant action profile,* $\bar{\mathbf{e}}$*, and the game satisfies* $(n-2)K < (n-1)$*, then for any* $\varepsilon > 0$*, there exists* $T' > 0$ *such that for all* $T > T'$*, all revision equilibria have* $\mathbf{e}(\mathbf{0}) = \bar{\mathbf{e}}$ *with probability higher than* $1 - \varepsilon$*.*

Let us briefly discuss the argument of the proof (the proof closely follows the steps from Calcagno et al. (2014) and is presented in online Appendix A). The proof can be divided into two parts. The first part shows that the Pareto-dominant equilibrium profile, $\bar{\mathbf{e}}$, is absorbing. The second step of the proof constructs a payoff lower bound for a player who chooses the highest effort well before the deadline. This is done by induction on the number of players choosing the highest effort, $\bar{e}$. To construct a payoff lower bound for player i, one needs to consider the case that another player obtains a revision opportunity before i does. In that case, we rely on the similarity of different players' payoffs, guaranteed by The condition in Definition 1. Finally, the condition stated in the proposition is sufficient to guarantee that the pre-play length needed for the induction step is finite. Formally, the proposition gives us that, when far before the deadline, independent of the current effort choices, by selecting the highest effort, a player is guaranteed a payoff close to the efficient payoff with a probability close to one.

According to Proposition 1, in any revision equilibrium of a long enough revision game, all the players choose the efficient effort in the payoff-relevant moment with probability close to one. This result holds independent of the effort configuration at the beginning of the revision phase. If the time horizon is long enough, then at $t = 0$, all players will be choosing the efficient effort (with probability $1 - \varepsilon$). That is, even if all players start with the minimum or randomly determined effort, or if players choose simultaneously at $-T$, all players will be choosing the efficient-effort at $t = 0$ with probability at least $1 - \varepsilon$. It is essential to highlight that, if the conditions for the proposition fail—for instance, if the pre-play phase is too short—then Proposition 1 does not indicate anything about equilibrium selection. In particular, the efficient effort profile, $\bar{\mathrm{e}}$, would still be one possible effort profile played at the end of pre-play in an equilibrium of the game, but it would not be the unique outcome of a revision equilibrium. A different equilibrium could have all players preparing the lowest effort at the end of pre-play.

In online Appendix B, we go beyond the proposition and, given the parameters used in the experiment, we numerically solve the game by backward induction. Consequently, we gain two additional insights into the revision equilibrium strategy. First, if players can choose their effort before the pre-play phase, then the revision equilibrium prescribes that they all choose the efficient effort from the start. Second, if a player has a revision opportunity far enough from the deadline, then it is optimal to revise to the efficient effort, irrespective of what other players are preparing. Solving the game by backward induction also allows us to go beyond the proposition. We show that, focusing on the minimum-effort game, the condition stated in Proposition 1 is sufficient, but not necessary.

3.3 Pre-play Communication

Theoretical work regarding cheap-talk pre-play communication in coordination games has focused on evaluating the credibility of a message profile. The idea is that pre-play communication will promote Pareto-efficient Nash equilibrium play if players' messages are credible when they communicate their intentions to take a certain action. The literature has proposed several requirements for a message to be considered credible.

The early literature considers one-way communication and analyzes the credibility of a message in isolation. For instance, Farrell (1988) postulates that a message is credible if it is self-committing: if the message is to be believed, a sender's best response to the actions induced by her message, is to follow the intention stated in the message. If we consider the minimum-effort game with one player sending a public message to all other players, then sending an efficient-effort message is self-committing. If all receivers believe the message and choose the efficient effort, the sender's optimal choice is to follow the message and choose the efficient effort. Aumann (1990) challenges the above reasoning, focusing on whether the sender has a strict preference over the other players' strategy choices. The author argues that, when the sender wants the receiver to believe the message, whether or not they intend to act in accordance with it, the message has no credibility. For Aumann (1990), a message leads to effective communication only if it is self-signaling: the sender wants their message to be believed if, and only if, they plan to follow the intention conveyed in the message. Note that, in the minimum-effort game, a player weakly prefers that other players choose the efficient effort level, independent of the player's choice; hence, a message signaling the intent to choose the efficient effort is not self-signaling.

Self-committing and self-signaling are both concepts that relate to individual messages. Although the definitions could be generalized to the case of multilateral communication, message credibility needs to be defined for profiles of messages, not for individual messages. A player can simultaneously be a sender and a receiver of a message, and a player might send a message linked to one equilibrium and receive a message linked to another. In particular, Blume (1998) argues that communication makes an equilibrium profile more attractive for a player only if all players communicate homogeneously, agreeing on the equilibrium in question. In a minimum-effort game with one-shot multilateral communication, if the chosen message profile is homogeneous, then the associated equilibrium profile could be considered more salient. However, no consensus exists on how to interpret heterogeneous message profiles, and the standard prediction is that communication will be ignored.[9]

[9] Empirical evidence on the topic is mixed and is context- and game-dependent. In coordination games in which there is some conflict of interest, He et al. (2019) find support for the 'feigned-ignorance principle'—i.e., players ignore messages unless they reach an agreement in which both players are weakly better off. In a coordination game, Cooper et al. (1992) find that actions following heterogeneous messages significantly differ from actions without communication. Focusing on one round of cheap-talk communication, using the data in Blume and Ortmann (2007), and in our paper, we find that in cases with heterogeneous message profiles, there is a significant correlation between minimum message and minimum effort chosen. Hence, even when subjects face a heterogeneous message profile, they seem to extract information instead of entirely ignoring communication.

3.4 Exact predictions and qualitative insights

Before we describe the experimental design of the paper, we distinguish between *the exact theoretical predictions* and *the qualitative insights* laid down by the forces behind such results. Two predictions arise from the theoretical setup. First, players' payoff should be close enough to the efficient payoff. Second, focusing on players' choices, when enough time remains before the deadline, it is optimal to revise the effort choice to the efficient effort at the first revision opportunity awarded (see online Appendix B). As a consequence, if players can choose their initial effort, all players should choose the efficient effort from the start.

Going beyond these exact predictions, we consider six qualitative insights. First, the theory suggests that one should expect the same outcome, regardless of whether the players choose the initial choices or they are picked for them randomly before the pre-play starts. Second, the theory proposes that the same outcome should be expected for different payoff specifications (within some parametric limits).

Third, as explicitly stated in Proposition 1 and verified numerically in online Appendix B, the uniqueness of the revision equilibrium is conditional on the presence of frequent enough revision opportunities. If revisions are infrequent, then all equilibria of the component game are an outcome of a revision equilibrium of the extended game.

Fourth, revisions being asynchronous is key to the backward induction argument behind the proof of Proposition 1. If a player cannot condition her behavior on other players' effort choices when a revision opportunity arises—for instance, if other players also have a revision opportunity at that time—then the repetition of any static Nash equilibrium is a revision equilibrium of the extended game.

Fifth, commitment matters: even if revisions are frequent, it is key that players do not treat their own preparations, or others' prepared actions, as cheap talk. A revision is not cheap talk, as there is a strictly positive chance of not having any revisions before the deadline. As time passes, this chance increases, and players are further committed to their prepared actions.

Sixth, a player's best response to a revision opportunity (given the effort profile being prepared) depends on the time left before the deadline. For instance, consider that, at a time $-t$, all players are choosing the minimum effort, and player i has a revision opportunity. If t is large enough, the proposition dictates that it is optimal for player i to choose in a forward-thinking way, to revise her effort upward, and to initiate a chain reaction that will end with all players choosing the highest effort. In contrast, if t is small, the probability of further revision opportunities for all players is negligible. Hence, it is optimal for player i to choose the minimum effort.[10]

4 Experimental Design

In this section, we first present three main treatments that establish the effect of the revision mechanism. Then, we describe additional treatments to test the exact prediction and theoretical insights discussed in Section 3.4. The instructions used in our experiment can be found in the online appendix.

The experimental sessions were conducted at the Center for Experimental Social Science (CESS) laboratory at New York University (NYU) and at the Interdisciplinary Experimental Laboratory (IELAB) at Indiana University (IU), using the software z-Tree (Fischbacher (2007)).[11] All participants were NYU or IU students. The experiment lasted about 45 minutes, and subjects earned, on average, \$18, which included the \$8 show-up fee. In each session, written instructions were distributed to the subjects and also read aloud.

In all treatments, participants are randomly divided into groups of six, and they participate in a sequence of ten rounds as a part of that group. In each round, the group plays the minimum-effort game with effort choices from 1 to 7 ($E = \{1, 2, 3, 4, 5, 6, 7\}$). For all but one treatment, the subjects have the same payoff function: $\pi_i(e) = 0.18 - 0.04 \cdot e_i + 0.2 \times \min_{j \in I} e_j$. For treatment revision mechanism VHBB (RM-VHBB), the payoff function is $\pi_i(e) = 0.60 - 0.1 \cdot e_i + 0.2 \times \min_{j \in I} e_j$, as the primary parameters in Van Huyck et al. (1990). The payoffs are described to subjects in matrix form, and the subjects take a comprehension test to ensure that they understand the payoff structure. After ten rounds,

[10] To gain some intuition, consider a player facing a scenario in which every other player is choosing the lowest effort, 1. On the one hand, if there are 59 seconds left before the deadline, the player should revise her choice to 7, if given the opportunity. Figure 5a shows us that when the deadline is far away, revising the effort choice to the highest effort is dominant, irrespective of other players' choices. On the other hand, if there is only 1 second left in the pre-play, then the probability of reaching the optimal strategy is 0; therefore, the player should revise the effort to the minimum effort choice by the group, i.e., 1, if given the opportunity. The best response during the pre-play period depends on both the time left and the prepared profile. In particular, given our parameters, the number of iterations required for 7 to be the dominant effort choice is 13 (see Figure 5a). If there is less time left in the pre-play interaction, choosing 7 is not a dominant choice, and the best response depends on the current profile.

[11] The sessions at NYU were conducted in December 2015, February 2016, and April through July 2018. At NYU, a session included either 12 or 18 subjects. The sessions at IU were conducted in October-November 2020 and March-April 2021. Due to COVID social-distancing norms, each session included only one group with six subjects.

subjects answer a short survey and are paid their final payoff, which is the sum of the payoffs from all ten rounds plus the show-up fee.

Baseline The baseline treatment replicates the standard control treatment in the literature. Subjects play the normal-form one-shot game. After each round of playing a standard simultaneous minimum-effort game, participants receive feedback on the minimum number chosen in their group in that round. This information is the only history available to them in the baseline treatment.

Revision Mechanism (RM) We design a treatment that closely replicates the conditions of our theoretical setup. However, implementing this institution in the laboratory presents several challenges.

One challenge is that the game involves frequent interaction among players, and they need to have all the information at every round. Thus, each player's revision opportunity and posted effort, as well as the history of posted efforts and revisions, should be available to all players at all times. We compile this information in a graph that summarizes all the key points and represents the players' efforts in different colors. Every time a player receives a revision opportunity, a dot appears on that player's action line. The graph summarizes all the key information and makes it easily accessible to the subjects. Figure 1 presents an example of the graph after 30 seconds have passed.[12]

Another challenge is the implementation of revision opportunities. Theory dictates that revisions should happen frequently. If we stop ("freeze") the phase every time a subject receives a revision opportunity, the phase could last a long time. To control how long a phase lasts, we let subjects change a number any time they want; all they need to do is place the cursor over the button on the screen. However, the subject's new selection is updated on the graph only after the subject receives a revision opportunity. A byproduct of this method is that we gather two streams of data: payoff-relevant decisions and what subjects want to do (we use these data streams to test whether our choices for the frequency of revisions and the length of the pre-play interaction restrict players' behavior; see online Appendix E for more details).

In the RM treatment, each round begins with all group members simultaneously choos-

[12] We thank Bigoni et al. (2015) and Friedman and Oprea (2012) for sharing their code with us.

ing a number from 1 to 7.[13] Once all group members make their initial choices, a graph[14] appears, and a one-minute countdown begins. In Figure 1, we present an example of the graph after 30 seconds of the countdown. The time in seconds is on the horizontal axis, and the number chosen by each group member is on the vertical axis. The initially chosen numbers are along the vertical line above the zero-second mark. Each player is represented on the graph by a different color. As the countdown progresses, at any time, any member of the group can change their chosen number by placing the cursor on the desired number on the left side of the screen. When a subject selects a number, the respective button turns green on the subject's own screen (see the number 4 in Figure 1). The subjects do not see their group members' "planned choices." The number posted on the graph updates only when the subject receives a revision opportunity, and the entire group can see this update on the graph.

On average, a subject receives eight revision opportunities in one round. Formally, at every second, the group has an 80% chance of receiving a revision opportunity; if that occurs, then the six group members have an equal probability of $1/6$ of receiving the revision opportunity. Only the numbers posted at the end of the countdown matter for the payoff. The initially chosen efforts and all the revision effort choices are irrelevant for the payoff calculation.

Standard cheap talk (S-CT) The standard cheap-talk treatment offers subjects multilateral one-shot communication, similar to the main communication treatment in Blume and Ortmann (2007). In this treatment, before subjects make their payoff-relevant effort choices, they simultaneously send a public message (a number from 1 to 7). This is followed by 60 seconds during which subjects see all the messages sent by their group members (including their own message).[15] After the subjects see the messages, buttons appear, and they make their payoff-relevant effort choices (all the group messages are visible on the screen when subjects make their payoff-relevant decision). At the end of the round, the subjects see a feedback page with their choice and the minimum number chosen by the group.

[13] Data for RM were collected both at NYU and IU. Data for eight groups were gathered at NYU and for eight groups at IU, resulting in 16 groups for this treatment. We do not find any considerable differences between the two locations, and we, therefore, combine the data for the analysis throughout the paper.

[14] We explained the graph in great detail in the instructions, and all subjects took a comprehension test regarding the graph and payoff table.

[15] To ensure that subjects spend the same amount of time in the lab and have an experience similar to that of the RM treatment, we display the messages sent by subjects on a graph and give the subjects 60 seconds to make the payoff-relevant effort choice.

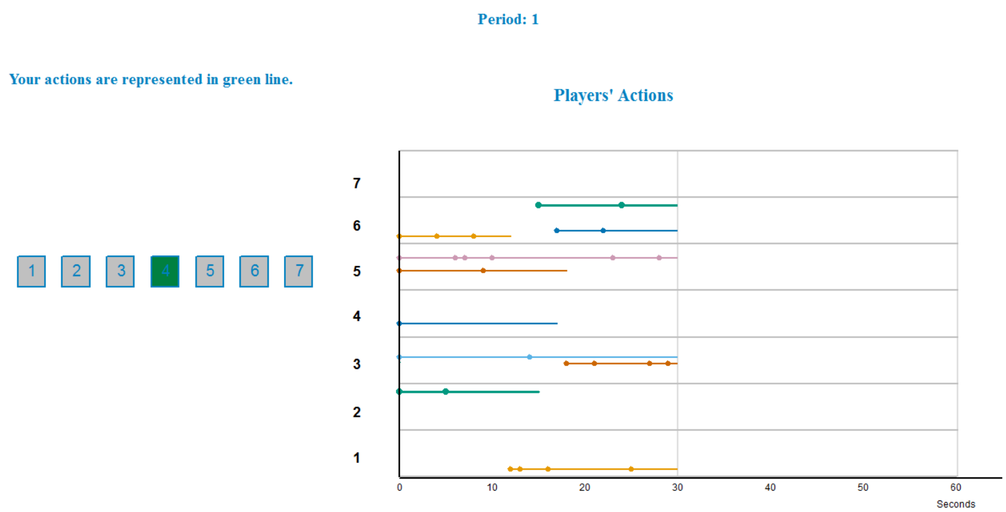


Figure 1: Sample Screen after 30 Seconds in RM

Random Revision Mechanism (R-RM) In the random revision mechanism (R-RM), initial choices are randomly chosen for the subjects using a uniform distribution over all possible efforts, with all groups facing the same initial effort. However, the rest of the round is executed identically to the RM protocol. We know, theoretically, that the outcomes of this treatment should be similar to those of RM; however, we get a much richer best-response behavior due to the initial heterogeneity of effort choices.

Revision mechanism VHBB (RM-VHBB) The revision mechanism VHBB treatment is identical to the RM treatment, except for the payoff parameters. In RM-VHBB, we use the main payoff parameters used in Van Huyck et al. (1990) ($\alpha = 0.2$, $\beta = 0.1$, and $\gamma = 0.6$).

Infrequent revision mechanism (I-RM) In the infrequent revision mechanism, we reduce a group's probability of having a revision opportunity from 0.8 of RM to 0.1. In RM, the chance of having no more revisions 60 seconds before the deadline is approximately 0.01%, while in I-RM, it is 36%. In online Appendix B, we numerically solve the game with 0.1 as the probability of a revision opportunity, and we find that at least 164 seconds per round would be needed for the theoretical results to hold.

Synchronous revision mechanism (S-RM) The synchronous revision mechanism is identical to RM, except that the revisions for all subjects in a group coincide. Recall that in RM, a revision is awarded to a group with an 80% chance every second. When a revision opportunity is awarded to a group, it is given to one of the group members with equal probability. In S-RM, we have essentially combined each group member's one revision occurring asynchronously (six revisions in total) into one simultaneous revision when all six group members can revise simultaneously. The realizations of revision opportunities used in S-RM are taken from the realizations of revisions used in RM for one of the group members. That is, in S-RM, when a group receives a revision opportunity, all group members receive it at once with a 1/6 chance, or no group member receives it with a 5/6 chance. This ensures that the expected number of revisions per subject in S-RM is eight, the same as in RM. The only difference between RM and S-RM is that S-RM revisions are synchronous, while revisions in RM are asynchronous.

Revision cheap talk (R-CT) The revision cheap-talk treatment follows the RM pre-play phase protocol. First, all members of the group simultaneously choose an integer from 1 to 7; then, once everyone makes a choice, the one-minute countdown begins. As in the revision mechanism, all members of the group see the same real-time graph, and the chosen effort is updated only when a revision opportunity is awarded.[16] In contrast to RM, the choice at the end of the countdown is not payoff-relevant in R-CT. Once the 60-second countdown is over, a new screen appears, and subjects choose an integer from 1 to 7 that determines their payoffs.[17]

Richer revision cheap talk (R-R-CT) In the richer revision cheap-talk treatment, subjects can inform others what they intend to play, as well as what they think everyone should play. R-R-CT has a protocol similar to that of R-CT, except that in R-R-CT, subjects observe two sets of buttons and two graphs similar to Figure 1. After 60 seconds, the graphs stay on the screen and buttons appear, which subjects use to make payoff-relevant choices. After every group member has made their choice, subjects observe a feedback page presenting their choice and the minimum of their group.

Table 1 summarizes our experimental design and highlights the differences between our treatments based on the two main dimensions: communication and commitment.

[16] The revision realizations used in R-CT are the same as in RM.

[17] This treatment is conducted in two ways. First, for eight groups, while subjects are making payoff-relevant choices, the graph from the round is not present; we refer to this treatment as R-CT-O. To avoid concerns about memory issues, we re-run this treatment with an additional eight groups; we refer to this treatment as R-CT-M. For these subjects, when payoff-relevant buttons appear, the graph with the history of 60 seconds is present on the same screen. Because we do not find any considerable differences between R-CT-O and R-CT-M treatments, we combine the data, 16 groups, and refer to this as R-CT treatment.

[18] One session of the baseline treatment was voided because one of the subjects publicly announced an intended action and asked others to play the same. We ran an extra session to replace the voided session.

Table 1: Experimental Design

Treatment	Communication	Commitment	# Subjects	# Groups
Baseline[18]	None	NA	48	8
Revision Mechanism (RM)	Revisions	Gradual	96	16
Standard Cheap-Talk (S-CT)	One-shot	None	48	8
Random RM (R-RM)	Revisions	Gradual	48	8
RM VHBB (RM-VHBB)	Revisions	Gradual	48	8
Infrequent RM (I-RM)	Revisions	Abrupt	48	8
Synchronous RM (S-RM)	Revisions	Gradual	48	8
Revision Cheap-Talk (R-CT)	Revisions	None	96	16
Richer R-CT (R-R-CT)	Revisions	None	48	8

5 Results

In this section, we first establish the main result of the paper: introducing a revision mechanism to a minimum-effort game significantly improves efficiency compared with both the one-shot game and the one-shot game preceded by a round of one-shot cheap-talk communication (RM vs. Baseline and RM vs. S-CT). We then test the exact predictions of the theory highlighted in Section 3.2. We proceed by using additional treatments described in Section 4 to shed light on the six insights provided by the theory, discussed in Section 3.4. Further, we take a deeper look into how subjects communicate and how commitment affects their communication.

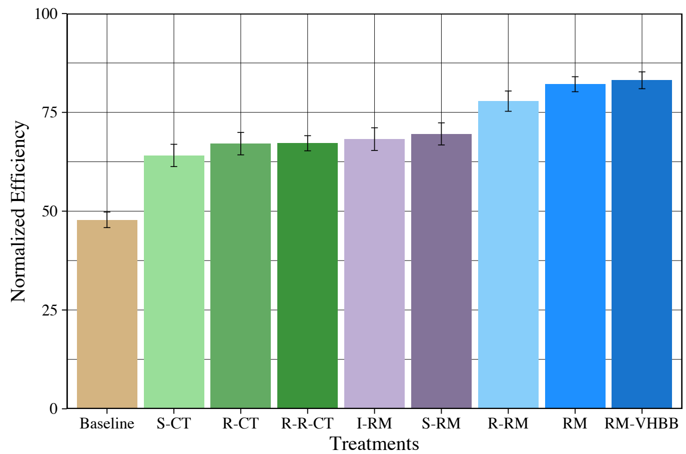
5.1 Overall effect of revision mechanism

We first focus on the overall efficiency of treatments. We follow the literature and calculate normalized efficiency as

$$\textit{Efficiency} = \frac{Actual - Min}{Max - Min}, \tag{3}$$

where $Actual$ is the average amount earned in a treatment, and Min (Max) is the average minimum (maximum) possible amount that a subject can earn. Results are displayed in Figure 2. The RM efficiency is 82.1%, whereas, in Baseline and S-CT treatments, the efficiency is 47.8% and 64.1%, respectively (Mann–Whitney U (MWU) tests lead to p-values of less than 1% for both RM vs. S-CT and RM vs. Baseline). The introduction of the revision mechanism restores more than half (65.7%) of the efficiency loss in the Baseline. While these results are based on all ten rounds combined, the differences among earnings in the Baseline, S-CT, and RM treatments get stronger over the ten rounds. In the first round, the normalized efficiency is 43.1%, 51.1%, and 69.7% in the Baseline, S-CT, and RM, respectively. In contrast, in the last round, the normalized efficiency is 43.8%,

Figure 2: Treatment normalized efficiency



68.0%, and 92.0%, respectively.[19]

Given the payoff function for the minimum-effort game (see equation 1), deviations from the efficient equilibrium reduce payoffs in two ways. First, subjects choose an inefficient minimum effort, and, second, subjects miscoordinate and select different efforts. We now focus on outcome variables to capture the effect of RM over these two forces and compare it with other treatments. To capture whether subjects try to coordinate on the efficient profile, we analyze the minimum effort of the group, as well as the frequency of efficient effort choices. To assess the coordination on any effort profile achieved by the group, we rely on the fraction of fully coordinated groups and a novel measure, *equilibrium deviation*, which captures how far a group is from full coordination. Equilibrium deviation calculates, for each group, the average distance between the effort choices and the myopic best response—the minimum effort chosen in the group. Note that equilibrium deviation calculates how far the choice is from the minimum of the group. This measure does not capture the distance from the subject's current choice and the revision equilibrium.

A comparison of the treatments clearly reveals that S-CT falls short of RM on all four of these measures. In particular, the average minimum effort is lower (4.61 vs. 5.83), as are the average frequency of efficient effort (0.44 vs. 0.78) and the average fraction of fully coordinated groups (0.28 vs. 0.66).[20] In contrast, the average group equilibrium deviation

[19] For most of this section, we focus on aggregate statistics and the overall effects of the treatments. In online Appendix D.5, we provide more details on variables of interest over the ten rounds of play and also within the 60-second pre-play phase.

[20] See Tables 4 and 5 in online Appendix D.2 for details on average payoffs and other relevant variables, as well as the statistical test results between the revision mechanism and other treatments.

is higher in S-CT (0.86 vs. 0.49). We reject the hypothesis of equal distributions for all measures using Mann–Whitney U (MWU) tests, with $p < 0.001$, using the group average in a round as a unit of observation.[21]

We run a regression analysis with payoffs and the four aforementioned measures as endogenous variables. We cluster standard errors at the group level so that one group is treated as one independent observation. The results of the regression analysis are presented in Table 2. The control group is S-CT treatment. Baseline and S-CT perform similarly on payoffs, minimum effort, and frequency of efficient effort. But compared to Baseline, S-CT leads to more fully coordinated groups and lower equilibrium deviation. The regression highlights that RM performs better than the two treatments on an aggregate level and all four considered measures. Note that there are eight groups in the S-CT treatment, and, therefore, some results are only marginally significant, with errors clustered at the group level.

Table 2: Regression Analysis

	Dependent variable:				
	Payoffs	*Minimum Effort*	*Freq Efficient Effort*	*Full Coordination*	*Eqbm Deviation*
Baseline	-0.168	-1.025	-0.173	-0.250^{**}	0.589^{**}
	(0.124)	(0.750)	(0.125)	(0.116)	(0.268)
Revision Mechanism	0.210^{*}	1.213^{*}	0.339^{**}	0.388^{***}	-0.395^{*}
	(0.120)	(0.712)	(0.139)	(0.141)	(0.221)
Quiz	-0.034				-0.089
	(0.027)				(0.094)
Constant	1.017^{***}	4.612^{***}	0.442^{***}	0.275^{**}	0.840
	(0.190)	(0.618)	(0.122)	(0.115)	(0.712)
R^2	0.213	0.200	0.313	0.305	0.094
Observations	1920	320	320	320	1920
Demographics	Yes	NA	NA	NA	Yes

Note: errors clustered at the group level are in parentheses; $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.
Reference category is Standard Cheap Talk treatment;
Payoffs variable is a subject payoff in a round. *Minimum Effort, Freq Efficient Effort, Full Coordination* are group-level measures and subject demographic information is not applicable. *Eqbm Deviation* is a subject-level variable.

Finally, we run two alternative regression analyses of the five outcome variables and display them in Tables 6 and 7 in online Appendix D.3. We compare the treatments that incorporate the revision mechanism with those that introduce cheap talk. We begin by comparing RM with standard cheap talk and revision cheap talk jointly. In the last regression,

[21] Note that there are 16 groups in RM and 8 groups in S-CT, and each group plays the game for ten rounds. Hence, we consider 160 vs. 80 observations for each of the measures.

we compare all of the revision mechanism treatments (RM, R-RM, and RM-VHBB) with the control group of all treatments that incorporate cheap talk (S-CT, R-CT, and R-R-CT). In all regressions, errors are clustered at the group level. All regression analyses indicate the same outcome: revision mechanisms significantly outperform cheap-talk treatments. This result can be observed in players' payoffs and in the four measures we focus on.

Result 1 *The revision mechanism significantly increases efficiency over Baseline and S-CT treatments, by 34 ppts and 18 ppts, respectively. The increased efficiency is achieved through increases in the minimum effort chosen by the group, as well as by the overall coordination on any effort profile.*

5.2 Evaluating exact predictions of the revision mechanism

We proceed by testing the exact predictions listed in Section 3.4. We restate them and test each one separately. The first exact prediction is that subjects' payoffs should be close enough to the efficient payoff. Or, alternatively, efficiency of the revision mechanism should be 100%. Recall that efficiency of RM is 82.1%, which is significantly lower than 100% with $p < 0.01$. The second exact prediction is that subjects' initial effort choice should be the efficient effort. We test whether the fraction of subjects initially choosing 7 is 100%. In the last round, 93.8% of players' initial choices are the efficient effort. On average, 85.7% of the initial choice is the efficient effort, which is significantly lower than 100% with $p < 0.01$.

Result 2 *The revision mechanism achieves 82.1% efficiency, which is significantly lower than 100%. In addition, the frequency of the initial choice of efficient effort, 85.7%, is significantly lower than 100%.*

The evidence is not sufficient to support the exact theoretical predictions of the asynchronous revision mechanism; we see that subjects' behavior is significantly different from the predictions. Given the specificity of the predictions, some distance between subjects' behavior and the point predictions is expected, in some sense. For instance, as an alternative theoretical prediction, pure-strategy Nash equilibrium dictates that, even in the Baseline treatment, all subjects should choose the same effort. In the Baseline, however, the groups fully coordinate on an effort choice (any effort profile, not necessarily efficient) in only two out of 80 cases. Similar low full coordination results are found in the literature;

for example, in Van Huyck et al. (1990), out of 70 cases, there are zero cases with full coordination. In the baseline treatment for the minimum-effort game of Blume and Ortmann (2007), in three out of 32 cases do the groups fully coordinate on an effort level.

5.3 Evaluating the general insights from the theoretical framework

With the goal of understanding how the revision mechanism affects subjects' behavior in the laboratory and, in particular, to determine whether such an effect can be attributed to the forces behind the theoretical results, we now turn our attention to the six general insights presented in Section 3.2. We start with the two insights related to the robustness of the theory.

5.3.1 Robustness to exogenous initial choices and to different payoff parameters

The theory postulates that, if the initial choices for the players are picked at random, then—with a long enough pre-play phase—the outcome should be the same as with the endogenous initial choice, with a probability close to 1. We test this prediction by comparing the RM with the R-RM treatment. We find that the RM and R-RM treatments lead to similar behavior in all dimensions. We find no statistically significant difference in efficiency, with R-RM achieving 77.8% efficiency, compared with 82.1% in RM. Not only is the average payoff similar (10.43 vs. 10.93), but the average minimum effort (5.54 vs. 5.83), the average frequency of efficient effort (0.71 vs. 0.78), the average fraction of fully coordinated groups (0.60 vs. 0.66), and the average equilibrium deviation (0.57 vs. 0.49) are both statistically and economically indistinguishable. We cannot reject MWU tests of equal distributions for any of the five measures, with $p = 0.173$, $p = 0.169$, $p = 0.152$, $p = 0.343$, and $p = 0.319$, respectively. (For further details, see Tables 4 and 5 in online Appendix D.2.)

We now focus on the robustness of the prediction to different payoff specifications: the efficient profile is the unique revision equilibrium for the payoff parameters from the original experiment on the minimum effort game, Van Huyck et al. (1990). Our data indicate that the RM and RM-VHBB treatments lead to similar behavior in most dimensions. We find no statistically significant difference in efficiency, with R-RM achieving 82.2% efficiency, compared with 82.1% for RM. Not only is the efficiency similar, but the fraction of fully coordinated groups (0.61 vs. 0.66) and the average equilibrium deviation (0.40 vs. 0.49) are both statistically and economically indistinguishable. We cannot reject MWU tests of equal distributions for these two measures, with $p = 0.447$ and $p = 0.772$. The minimum effort is marginally lower in RM-VHBB (5.44 vs. 5.83), with $p = 0.061$, and the frequency of efficient effort is significantly lower (0.59 vs. 0.78), with $p < 0.01$.

Result 3 *Subjects' performance in the revision mechanism is invariant to having exogenous initial choices and to alternative payoff parameters that do not satisfy the condition of Proposition 1.*

5.3.2 Frequency and asynchronicity of revision opportunities

The frequency and the asynchronicity of the revision opportunities are key for the backward-induction argument used to select a unique revision equilibrium outcome in Proposition 1. We test the importance of these two elements by comparing RM to I-RM and to S-RM.

We observe that I-RM leads to significantly lower efficiency, 69.6%, compared with 82.1% in RM. Moreover, subjects' performance in I-RM falls short of the performance in RM on every one of the other five measures we consider. In particular, the average payoff is lower (9.47 vs. 10.93), and so are the average minimum effort (4.99 vs. 5.83), the average frequency of efficient effort (0.49 vs. 0.78), and the average fraction of fully coordinated groups (0.39 vs. 0.66). In contrast, the average equilibrium deviation is higher in I-RM (0.78 vs. 0.49). We reject the hypothesis of equal distributions for each of the five measures using MWU tests, with $p < 0.001$.

We find that subjects' behavior in S-RM leads to significantly lower efficiency than in RM, 67.1%, compared with 82.1%. Furthermore, subjects' performance in S-RM is worse than the performance in RM for every one of the other five measures we consider. In particular, the average payoff is lower (9.47 vs. 10.93), and so are the average minimum effort (4.99 vs. 5.83), the average frequency of efficient effort (0.49 vs. 0.78), and the average fraction of fully coordinated groups (0.39 vs. 0.66). However, the average equilibrium deviation is higher in S-RM compared with RM (0.69 vs. 0.49). We reject the hypothesis of equal distributions for payoffs, minimum effort, and frequency of efficient effort measures using the MWU test, with $p < 0.001$, and for fully coordinated groups and equilibrium deviation measure using the MWU test, with $p = 0.003$ and $p = 0.006$, respectively.

Result 4 *The frequency and asynchronicity of revisions are significant contributing components to achieving 82% efficiency in RM. When the frequency of revisions is reduced from 8 to 1 or when revisions are synchronous instead of asynchronous, the efficiency is reduced by 13 and 15 percentage points, respectively.*

5.3.3 The importance of commitment

A key factor in the proof of Proposition 1, is that players cannot and, therefore, do not treat their own preparations—or others' prepared actions—as cheap talk. Comparing the performance of the R-CT treatment with that of RM empirically highlights the importance of commitment.

We observe that R-CT leads to significantly lower efficiency, 67.2%, compared to RM at 82.1%. Accordingly, subjects' performance in R-CT is worse than the performance in RM on every one of the other five measures we consider. We find that R-CT leads to lower payoffs (9.19 vs. 10.93), as well as to lower minimum effort (4.86 vs. 5.83), lower frequency of efficient effort (0.55 vs. 0.78), and lower fraction of fully coordinated groups (0.32 vs. 0.66). Furthermore, R-CT leads to a significantly higher equilibrium deviation compared with RM (0.97 vs. 0.49). In keeping with this, we reject the hypothesis of equal distributions for all five measures using MWU tests, with $p < 0.001$.

Result 5 *The commitment in the revision mechanism is a significant contributing component to achieving 82% efficiency in RM. Removing commitment from the mechanism reduces efficiency by 15 percentage points.*

5.3.4 The best response dynamics

This section discusses the dynamic behavior implied by the proof of Proposition 1 and the numerical exercise in online Appendix B. A player's best response depends not only on the effort profile of other agents, but also on the time left before the deadline.

We look at subjects' dynamic behavior in R-RM and classify all the revisions taken into three categories—forward-thinking, myopic down, and others—detailed below.[22] We display in Figure 3 the types of moves taken as a function of the amount of time that has passed in the pre-play phase.

(i) **Forward Thinking:** moves that would decrease the player's payoff if they occur in the last instant; however, these moves will increase the payoff if other players follow. This category combines two types of moves: (1) the subject's current choice is above the

[22] The R-RM treatment creates variance in the initial choice, which is lacking in RM since the initial effort was determined at random. We explore this variance in initial choices to analyze the dynamic best-response behavior of our subjects. The distribution of initial efforts over rounds: 7% were 1; 13%, 2; 25%, 3; 13%, 4; 18%, 5; 13%, 6; and 10%, 7. This pattern is different from the initial choices in RM. In RM, the distribution of the initial choices was 1% were 1; 1%, 2; 1%, 3; 1%, 4; 4%, 5; 6%, 6; and 86%, 7.

group's minimum, but they still increase their chosen effort; and (2) the subject's choice is the group's minimum, and they increase their effort above the second minimum effort.

(ii) **Myopic Down:** moves decreasing the player's effort that would increase their payoff if the moves occur in the last instant. This category contains the moves that get the subject's effort closer to the group minimum.[23]

(iii) **Other:** moves not included in the above categories. This includes moves increasing the subject's effort choice, which would increase their payoff, and moves decreasing the subject's effort choice, which would decrease their payoff (e.g., because the subject is already at the group minimum or moves below the current minimum).

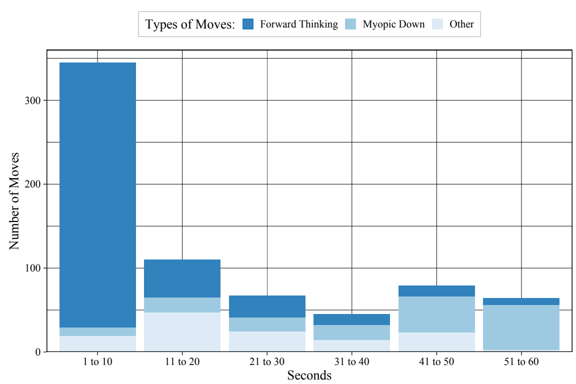


Figure 3: Classification of moves over time

In the first ten seconds of a round, more than 300 revision opportunities are used for forward-thinking moves, representing around 92% of all the revision opportunities taken. In contrast, in the last ten seconds, 84% of the revision opportunities taken are used for myopic down moves. Note that the subjects have about the same number of revision opportunities available in the first ten and in the last ten seconds of the pre-play interaction. Despite this, in the first ten seconds of the round, subjects use a similar number of revision opportunities as in the last 50 seconds combined.

Also, note that subjects do not use all early opportunities to revise the prepared effort to the highest effort immediately, as predicted by the theory. We observe that 78.2% of revisions taken in the first ten seconds are used to revise the effort choice to 7, which is sizable but significantly lower than 100%. Given the evidence, the subjects' behavior is

[23] Notice that if there is not enough time left for others to adjust, moving down to the minimum of the group is a best-response behavior.

largely aligned with the general insight of the theory, even if significantly different from the point prediction.

Result 6 *In the revision mechanism, early revisions are forward-thinking, while late revisions are myopically payoff-improving. In the first ten seconds,* 91.6% *of moves are forward-thinking, and* 2.9% *of moves are myopic-down. In contrast, in the last ten seconds,* 12.5% *of moves are forward-thinking, and* 85.0% *of moves are myopic-down.*

5.4 Communication and Commitment

We now shift from the theoretical insights to focus on how subjects communicate in different treatments. We examine three aspects: how commitment affects communication, the differences between communication and actions in the cheap talk treatments; and the impacts of a richer message space.

Effects of commitment on communication Commitment affects communication in two interconnected ways. First, commitment makes communication more credible, with subjects acting differently in the presence of commitment after a particular profile is revealed. Second, commitment changes the optimal communication in that subjects communicate differently in the presence of commitment. Below, we try to parse these two distinct but interconnected forces.

We first compare the credibility of messages in RM and in R-CT. To do so, we follow the theoretical discussion in Blume (1998), and we examine whether the whole group converging on a particular message profile makes that effort profile (an equilibrium in the component game) more salient, thus leading to higher coordination on that profile. We look at all groups that converged to stating their intention to choose the same effort level (not necessarily the efficient effort profile). This happens with a frequency of 75.6% and 56.2% in RM and R-CT, respectively. Out of all the times that a group converges to a common effort profile during the 60-second pre-play, that profile is implemented at the payoff-relevant moment in 87.6% of these rounds in RM, compared with 51.1% in R-CT.

We now focus on how commitment affects what is communicated. In Figure 4a, we display the average equilibrium deviation of the prepared—and, hence, publicly posted on the graph—efforts over the 60-second interval.[24] The graph shows a decline in the equilibrium deviation over the pre-play in RM, as players coordinate more. We also document a

[24] In Figure 4, we use the last five of the total ten rounds. In online online Appendix D, we present the graph for all ten rounds (Figure 11) and additional treatment, R-R-CT.

difference between RM and R-CT, especially in the latter part of the pre-play phase. Facing a group member who is choosing a smaller effort close to the deadline, a subject reduces their chosen effort (thus reducing the equilibrium deviation) in RM, but not in R-CT. In the absence of commitment, lowering the choice of effort is unnecessary because revising it in the final instant is possible.

Result 7 *Convergence to a common message leads to more credibility of the communication in the presence of commitment. When a group converges to a common effort profile during the 60-second pre-play, that profile is implemented at the payoff-relevant moment 87.6% and 51.1% of the time in RM and R-CT, respectively.*

Differences between communication and action We now turn to the differences between the profile communicated in the last instant of the pre-play phase and the payoff-relevant effort choices in cheap-talk treatment. As depicted in Figure 4b, both the average effort and the fraction of fully coordinated groups of the payoff-relevant profile are significantly lower than those of the 60th second profile.

Comparing the payoff-relevant efforts in R-CT and the 60th second message in R-CT, we have that the average payoff is lower (9.19 vs. 10.18), as are the average minimum effort (4.9 vs. 5.5), the average frequency of efficient effort (0.56 vs. 0.77), and the average fraction of fully coordinated groups (0.32 vs. 0.48). However, the average equilibrium deviation is similar (0.97 vs. 0.92). We reject the hypothesis of equal distributions for payoffs, minimum effort, and frequency of efficient effort measures using the MWU test, with $p < 0.001$, and for fully coordinated groups using the MWU test, with $p = 0.002$. We cannot reject the equal distribution hypothesis of equilibrium deviation, as the MWU leads to $p = 0.103$. The difference between the messages and actions leads to a 10.7% loss in payoffs.

Result 8 *Substantial differences exist between the preparations at the last second and the effort implemented, in the absence of commitment in R-CT.*

Richness of communication The results above point to the lack of credibility of the messages as a possible culprit behind the differences in subjects' behavior between the RM and R-CT. It might be that players do not believe in others' communication since they cannot distinguish positive communication about intentions ("I intend to play...") from normative communication about the group's effort profile ("We should all coordinate on..."). We now

(a) Eqbm. deviation of revised efforts (b) Average effort and full coordination

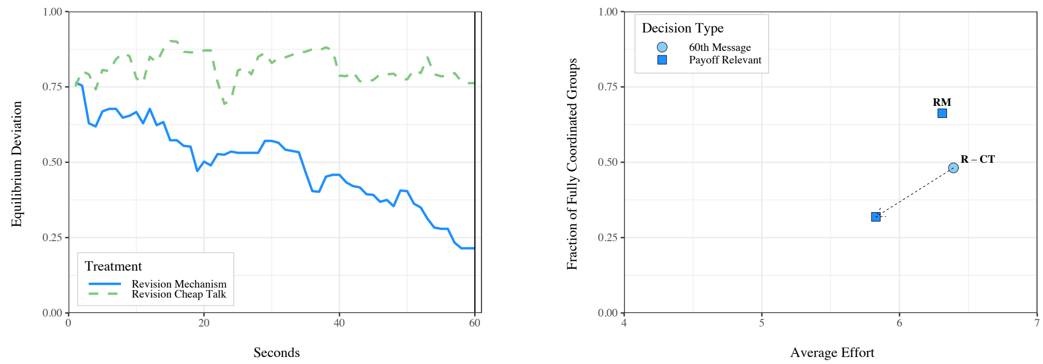


Figure 4: Equilibrium Deviation over Time

consider whether enriching the message space to include a second message—"I think we should all choose"—improves coordination.

We find that R-CT and R-R-CT treatments lead to similar behavior in all dimensions. Not only is the average payoff similar (9.20 vs. 9.32), but the average minimum effort (4.86 vs. 4.94), the average frequency of efficient effort (0.56 vs. 0.59), the average fraction of fully coordinated groups (0.32 vs. 0.36), and the average equilibrium deviation (0.97 vs. 0.96) are statistically indistinguishable. We cannot reject MWU tests of equal distributions for any of the five measures, with $p = 0.685$, $p = 0.672$, $p = 0.385$, $p = 0.499$, and $p = 0.687$, respectively. Accordingly, R-R-CT performs significantly worse on all five measures compared with RM.

Result 9 *Enriching the cheap-talk messages to include explicitly labeled normative messages about what the group's choice should be does not significantly improve subjects' payoffs. Richer R-CT treatment leads to 68.2% efficiency, which is statistically indistinguishable from the efficiency of 67.2% in R-CT.*

6 Concluding discussion

Coordination environments are prevalent in real-world situations, and coordination failures leading to inefficiencies are widespread. In this paper, we provide experimental evidence that commitment-enhanced communication can significantly reduce coordination failures in a particular coordination game—the minimum-effort game. A helpful way to summarize our results and to compare the treatments is to look at the efficiency:

$$\text{RM} > \text{S-CT} \approx \text{R-CT} \approx \text{I-RM} \approx \text{S-RM} > \text{Baseline}. \tag{4}$$

RM delivers significantly higher efficiency than S-CT treatment, and this boost is achieved through increases in the minimum effort and overall coordination on any effort profile. While the revision mechanism undoubtedly provides gains in efficiency, these gains are based on the presence of all key ingredients at the same time: commitment (RM vs. R-CT), asynchronicity (RM vs. S-RM), and frequency of revisions (RM vs. I-RM).

Let us take a closer look at the S-CT $>$ Baseline part of expression (4). The theory we examine in this paper is not equipped to explain this result. Even if we consider alternative theories, as discussed in Section 3.3, introducing cheap-talk communication does not necessarily reduce the set of equilibria in a minimum-effort game. Previous literature has already documented the beneficial effects of communication even without theoretical support. In a Stag-Hunt game—in which a message indicating intent to cooperate is self-committing but not self-signaling—Charness (2000) finds that one-sided communication increases the coordination on the efficient outcome. Communication has beneficial effects even beyond coordination games. It leads to more egalitarian allocations and increases efficiency in a divide-a-dollar bargaining game with a unanimity voting rule (see Agranov and Tergiman (2019)). Venturing into social-dilemma games, communication leads to higher rates of contributions in a public goods game (see Isaac and Walker (1988), Ostrom et al. (1992), Oprea et al. (2014), and Palfrey et al. (2017)). This growing empirical evidence could further inspire theoretical work to account for the effects of cheap-talk communication in various environments.

Continuing our discussion of expression 4, let us explore a rationale for R-CT, S-RM, and I-RM faring significantly better than Baseline. The gains of the R-CT treatment over Baseline are unsurprising. One round of restricted message exchange in S-CT delivers a significant improvement over the one-shot game without any interactions; therefore, it is expected that the multiple exchanges of messages allowed in R-CT should do at least as well. More surprisingly, the richness of communication in R-CT does not improve upon S-CT treatment. Our examination of communication credibility suggests that, if a message is not seen as credible, then it does not matter whether it is stated once (S-CT) or multiple times (R-CT). The efficiency gains from S-RM and I-RM over Baseline might be expected, as both contain many features similar to cheap talk. For instance, in I-RM, although revisions are infrequent, the initially submitted choices can still be thought of as messages—even if it is not cheap talk.[25]

[25] Additionally, in S-CT, the subjects receive feedback only on the group minimum, while in I-RM, all the payoff-relevant choices are observable. Berninghaus and Ehrhart (2001) show that providing information

Finally, the additional efficiency gains from RM over S-RM suggest an alternative interpretation of our results. The success of RM may stem from transforming a simultaneous move game into a sequential game, without creating asymmetries by pinning down an order of play. Asynchronicity has long been recognized as an important element in coordination games (see Dutta (2012) and Ambrus and Ishii (2015) for theoretical work on the relevance of asynchronicity in coordination environments and Schotter et al. (1994), Weber et al. (2004), and Li (2007) for experimental work). Although the experimental evidence suggests a significant improvement in making the game sequential, a substantial amount of coordination failure remains. Our results highlight that, beyond asynchronicity, there are other key driving forces behind coordination (i.e., commitment and frequency of interactions). Nevertheless, a study with sequential moves—focusing on the horizon and the order of play—would shed further light on the importance of the random, repeated, and sequential nature of moves in RM in achieving higher efficiency. We leave this for future work.

about the distribution of group members' efforts enhances coordination.

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Appendices

A Proofs

Similar to the proof in Calcagno et al. (2014), ours relies on a backward-induction argument. In the first step of the proof, we show that the Pareto-dominant equilibrium profile $\bar{\text{e}}$ is absorbing in the revision game. If all players choose the maximal effort, then, for any following subgame, the unique subgame perfect equilibrium is for all players to play $\bar{\text{e}}$. More formally, we show that, at a certain period, it is true that if all other players are preparing $\bar{e}_{-i}$, player i will prepare $\bar{e}_i$ if they have a revision opportunity. In the period immediately before, if all other players are preparing $\bar{e}_{-i}$, player i will prepare $\bar{e}_i$ if they have a revision opportunity.

If only two players are present, this would constitute the whole argument because one player would always be in the position to induce the other player to prepare the maximum effort. However, $n > 2$ complicates the matter, as the unraveling argument is not trivial. We show that the condition $(n-2)K < (n-1)$ is sufficient to have a lower bound on players' payoff of choosing the efficient action converging to the efficient payoff. Thus, at the end of the countdown, all players prepare the maximal effort with probability $1 - \varepsilon$.

Proof. Let $\underline{\text{v}}_i^t(k)$ be the infimum of player i's payoff at t in subgame perfect equilibrium strategies and histories such that there are at least k players who prepare the action $\bar{\text{e}}$, and no player receives a revision opportunity at t. By mathematical induction with respect to $k = n, n-1, ..., 0$, we show that $\lim_{t \to -\infty} \underline{\text{v}}_i^t(k) = \pi_i(\bar{\text{e}}), \forall i$. Step 1 below shows the proof for $k = n$.

Step 1 Consider the final period. All $-i$ players are preparing the profile $\bar{e}_{-i}$ and i has a revision opportunity. It is optimal for player i to prepare $\bar{e}_i$, as this leads to a higher payoff than any other possible effort choice ($\bar{\text{e}}$ is the Pareto-dominant profile and is an equilibrium).

Now, for the inductive step, consider any period τ after t. If all $-i$ players are preparing the profile $\bar{e}_{-i}$ and i has a revision opportunity, it is optimal for player i to prepare $\bar{e}_i$. Consider also that, at the period immediately before t, $t-1$, all $-i$ players are preparing the profile $\bar{e}_{-i}$ and i has a revision opportunity. If player i prepares $\bar{e}$, then they are guaranteed payoff $\pi_i(\bar{\text{e}})$. If player i prepares any other action e, their expected payoff can be bounded by $(1-(1-\frac{p}{n})^{-t})\pi_i(\bar{\text{e}}) + (1-\frac{p}{n})^{-t}(\pi_i(\bar{\text{e}}) - \alpha) < \pi_i(\bar{\text{e}})\ \forall t$, where the bound is obtained by considering that (i) $\pi_i(\bar{\text{e}}) - \alpha$ is the second-best payoff for player i, (ii) with probability

$(1-\frac{p}{n})^{-t}$ player i gets no revision opportunity before the deadline, and (iii) all other players continue to exert maximal effort. This concludes step 1.

Step 2 (inductive argument) Suppose that $\lim_{t\to-\infty} \underline{\mathrm{v}}_i^t(k+1) = \pi_i(\bar{\mathbf{e}}),\ \forall i$, with $k+1 \leq n$,; we will show that $\lim_{t\to-\infty} \underline{\mathrm{v}}_i^t(k) = \pi_i(\bar{\mathbf{e}}),\ \forall i$.

Consider an arbitrary $\varepsilon > 0$. Since $\lim_{t\to-\infty} \underline{\mathrm{v}}_i^t(k+1) = \pi_i(\bar{\mathbf{e}}),\ \forall i$, a finite T_0 must exist such that $\forall t \leq T_0$, $\underline{\mathrm{v}}_i^t(k+1) \geq \pi_i(\bar{\mathbf{e}}) - \varepsilon\ \forall i$. Consider that k players prepare $\bar{\mathbf{e}}$ at a time t before the said T_0, that is, $t = T_0 + \tau_1$ with $\tau_1 \leq 0$. Then, if player j who is not preparing $\bar{\mathbf{e}}$ at time t can move first by T_0, they yield at least $\pi_j(\bar{\mathbf{e}}) - \varepsilon$ by preparing $\bar{e}_j$. This outcome implies that each player i will at least yield $\pi_i(\bar{\mathbf{e}}) - K\varepsilon$. Therefore, we can define a lower bound for a player's utility if k players are preparing $\bar{\mathbf{e}}$ at time t:

$$\underline{\mathrm{v}}_i^t(k) \geq \tfrac{1}{n}(1-(1-p)^{\tau_1})(\pi_i(\bar{\mathbf{e}}) - K\varepsilon) + (1 - \tfrac{1}{n}(1-(1-p)^{\tau_1}))\underline{\pi_i(e)}\ \forall i,$$

where $\frac{1}{n}(1-(1-p)^{\tau_1})$ is the probability that there is a revision and a particular player j who is not preparing $\bar{e}$ at that time is the first to get a revision before time T_0. We also assume that if such a move does not occur, the worst possible payoff will happen.

If τ_1 is a sufficiently long time interval, then there exists finite T_1 such that for all $\tau_2 \leq 0$, if the period is far removed from the deadline; $t = T_0 + T_1 + \tau_2 \leq T_0 + T_1$, then $\underline{\mathrm{v}}_i^t(k) \geq \frac{1}{n}\overline{\pi_i(e)} + (1-\frac{1}{n})\underline{\pi_i(e)} - K\varepsilon\ \forall i$. Introducing a bit of notation, we can define $\alpha_1 = \frac{1}{n}$, and $\underline{\mathrm{v}}_i^t(k) \geq \alpha_1\pi_i(\bar{\mathbf{e}}) + (1-\alpha_1)\underline{\pi_i(e)} - K\varepsilon\ \forall i$

For $t = T_0 + T_1 + \tau_2$ we express the lower bound on i's payoff $\underline{\mathrm{v}}_i^t(k)$ in different cases:

1. If j moves first by $T_0 + T_1$, then a lower bound on player $i's$ payoff depends on whether or not j is preparing $\bar{e}_j$.
   - If j is not preparing $\bar{e}_j$ at time t, then a lower bound on player $i's$ payoff is $\pi_i(\bar{\mathbf{e}}) - K\varepsilon$ as before.
   - If j is preparing $\bar{e}_j$ at time t they will move first by $T_0 + T_1$; then, a lower bound on player $j's$ payoff is given by $\frac{1}{n}\pi_j(\bar{\mathbf{e}}) + (1-\frac{1}{n})\underline{\pi_j(e)} - K\varepsilon$ by the same reasoning as before. Using the formula on Definition 1 to obtain a lower bound on player's i payoff, we have: $\frac{\pi_i(\bar{\mathbf{e}})-\pi_i(\mathbf{e})}{\pi_i(\bar{\mathbf{e}})-\pi_i(\underline{\mathbf{e}})} \leq K\frac{\pi_j(\bar{\mathbf{e}})-(\frac{1}{n}\pi_j(\bar{\mathbf{e}})+(1-\frac{1}{n})\underline{\pi_j(e)}-K\varepsilon)}{\pi_j(\bar{\mathbf{e}})-\pi_j(\underline{\mathbf{e}})}$. Hence, for player i, a lower bound on their payoff is given by $(1-K(1-\frac{1}{n}))\pi_i(\bar{\mathbf{e}}) + K(1-\frac{1}{n})\underline{\pi_i(e)} - K^3\varepsilon$.

2. If i will move first by $T_0 + T_1$, then a lower bound depends on whether or not they are preparing $\bar{e}_i$.

- If they are one of the k preparing $\bar{e}$, then a lower bound is given by $\frac{1}{n}\pi_i(\bar{\mathbf{e}}) + (1-\frac{1}{n})\underline{\pi_i(e)} - K\varepsilon$.
- If they are not one of the k preparing $\bar{e}$, by doing a revision, they can guarantee, by the inductive hypothesis, at least $\pi_i(\bar{\mathbf{e}}) - \varepsilon$.

In total, player i's payoff satisfies:

$$
\begin{aligned}
\underline{\mathrm{v}}_i^t(k) \geq\ & \tfrac{1}{n}(1-(1-p)^{\tau_2})(\pi_i(\bar{\mathbf{e}}) - K\varepsilon) \\
& + (\tfrac{1}{n}(1-(1-p)^{\tau_2}) + (1-p)^{\tau_2})(\tfrac{1}{n}\pi_i(\bar{\mathbf{e}}) + (1-\tfrac{1}{n})\underline{\pi_i(e)} - K\varepsilon) \\
& + (1-\tfrac{2}{n})((1-(1-p)^{\tau_2}))(1-K(1-\tfrac{1}{n}))\pi_i(\bar{\mathbf{e}}) + K(1-\tfrac{1}{n})\underline{\pi_i(e)} - K^3\varepsilon),\ \forall i
\end{aligned}
$$

Taking a sufficiently long τ_2, there exists a finite T_2 such that at $t = T_0 + T_1 + T_2 + \tau_3$ with $\tau_3 \leq 0$, we have that

$$
\begin{aligned}
\underline{\mathrm{v}}_i^t(k) \geq\ & (\tfrac{1}{n} + \tfrac{1}{n^2} + (1-\tfrac{2}{n})(1-K(1-\tfrac{1}{n})))\pi_i(\bar{\mathbf{e}}) \\
& + (1 - \tfrac{1}{n} + \tfrac{1}{n^2} + (1-\tfrac{2}{n})(1-K(1-\tfrac{1}{n})))\underline{\pi_i(e)} - K^3\varepsilon,\ \forall i
\end{aligned}
$$

defining $\alpha_2 = \frac{1}{n} + \frac{1}{n^2} + (1-\frac{2}{n})(1-K(1-\frac{1}{n}))$, we have the second step $\underline{\mathrm{v}}_i^t(k) \geq \alpha_2\pi_i(\bar{\mathbf{e}}) + (1-\alpha_2)\underline{\pi_i(e)} - K^3\varepsilon\ \forall i$.

Recursively, for each $M = 1, 2, ...$, there exists $T_0, T_1, ...$ such that $t \leq T_0 + T_1 + ... + T_M$,

$$\underline{\mathrm{v}}_i^t(k) \geq \alpha_M\pi_i(\bar{\mathbf{e}}) + (1-\alpha_M)\underline{\pi_i(e)} - K^{2M-1}\varepsilon\ \forall i.$$

with $\alpha_M = \frac{1}{n} + \frac{1}{n}\alpha_{M-1} + (1-\frac{2}{n})(1-K(1-\alpha_{M-1}))$. We can express the coefficient as a linear expansion, $\alpha_M = A + B\alpha_{M-1}$ with $A = \frac{1}{n}(n-1-K(n-2))$ and $B = \frac{1}{n}(1+K(n-2))$.

The condition in Proposition 1 guarantees that, in the α_M coefficient above, both A and B are strictly between zero and one. Furthermore, note that $A + B = 1$. This is sufficient to show that α_M is monotonically increasing and converges to 1. Taking a large enough T yields the result. ■

B Numerical solution of the discrete time revision game

In this section, we explain the backward-induction procedure to solve the game for the specific parameters given in our experiment. We consider a particular payoff specification following our experimental setup: a triple of linear coefficients $\gamma,\ \alpha,$ and β; a given number of players n; a given set of actions E; a pre-play length T; and a given revision probability, p. We solve the game for the expected payoff of every player at any time $-t \in \{-T, -(T-1), ..., -1, 0\}$, for every strategy profile. We also obtain the transition probability from any strategy profile to any other strategy profile between any two periods.

At the deadline ($t = 0$): We construct an n-tuple vector with dimension $|E|^n$, called V_0. Each line of V_0 has the payoff of each player if a particular strategy profile, e, is played.

Before the deadline, $t \in [-T, 0)$: Consider a given vector V_{t+1}. An n-tuple of V_{t+1} has the expected payoff of all players at time $t+1$ if that particular strategy profile is prepared (given that all players maximize their payoff if they have a revision in the future). We proceed backwards inductively, given the vector V_{t+1}, we construct V_t in the following way. First, for any strategy profile, we can compute how a player would revise their effort choice. That is, if player i had a revision opportunity, which effort would they choose, given that their expected payoffs of different action profiles are given by the vector V_{t+1}. This gives us the auxiliary matrix V_t^{rev}, a $|E|^n \times n$ matrix of n-tuples. Each element of the matrix gives us the payoff of all players if the strategy line were in place, and the player column had a revision opportunity at time t. The vector V_t is obtained by $(1-p) \times V_{t+1} + \frac{p}{n} \times V_t^{rev} \times \mathbf{1}_{n\times 1}$, where the first term is obtained when no one has a revision (and, thus, the strategy profile is unchanged), and the second term is the expected value of the payoff for each player given that someone has received a revision opportunity. We can iterate this process until V_{-T}.

Using the numerical solution to verify the proposition: For a particular set of parameters, $\gamma,\ \alpha,\ \beta,\ n,\ |E|,\ T,\ p$, we say that Proposition 1 holds if, for a finite $\bar{T}(\gamma,\ \alpha,\ \beta,\ n,\ |E|,\ p) < T$, when a player has a revision opportunity, they choose $\bar{e}_i$ independent of the effort profile in place. That is, for all $t > \bar{T}$, playing $\bar{e}_i$ dominates any other effort choice, and all elements of the matrix V_t^{rev} are equal to $\pi_i(\bar{\mathbf{e}})$. For the first part of Proposition 1, note that for any given $\varepsilon > 0$, if $T' \geq \bar{T} + \tau$, then the profile $\bar{\mathbf{e}}$ is played with probability larger than $1-\varepsilon$. The integer τ is defined as the minimum interval of time such that the probability that all players have at least one revision opportunity in that interval is larger than $1-\varepsilon$; that is, τ is the smaller integer that solves $\left(1-(1-\frac{p}{n})^{\tau}\right)^n \leq 1-\varepsilon$. We can see that the time interval needed, τ, increases with the number of players and decreases with the probability of a revision being awarded. For the second part of Proposition 1, it is sufficient that $T' \geq \bar{T}$. Note that the condition specified on the propositions is sufficient, but not necessary for the particular payoff parameters used in this paper.

Going beyond the proposition: As a byproduct of the construction of V_t from V_{t+1}, we also obtain a transition matrix, M_t, with dimensions $|E|^n \times |E|^n$, that specifies for any strategy profile today the probability that each profile will be chosen in the next period. For any given set of parameters, given a distribution of effort profiles at time $-T$, $e(-T)$, we can calculate the final distribution of efforts, at time 0, for any length of the pre-play phase, $e(-T) \times \prod_{s=-T}^{0} M_s$.

The two plots in Figure 5 highlight how the probability of a revision, p, changes the expected results of the game. We focus on two key dimensions: (i) the number of periods needed for $\bar{e}$ to be the dominant effort choice independent of the profile in place, $\bar{T}$; and (ii) the probability of the profile $\bar{\mathbf{e}}$ being chosen at the end of the countdown, given that $T = 60$ and the game was started with a profile chosen at random.

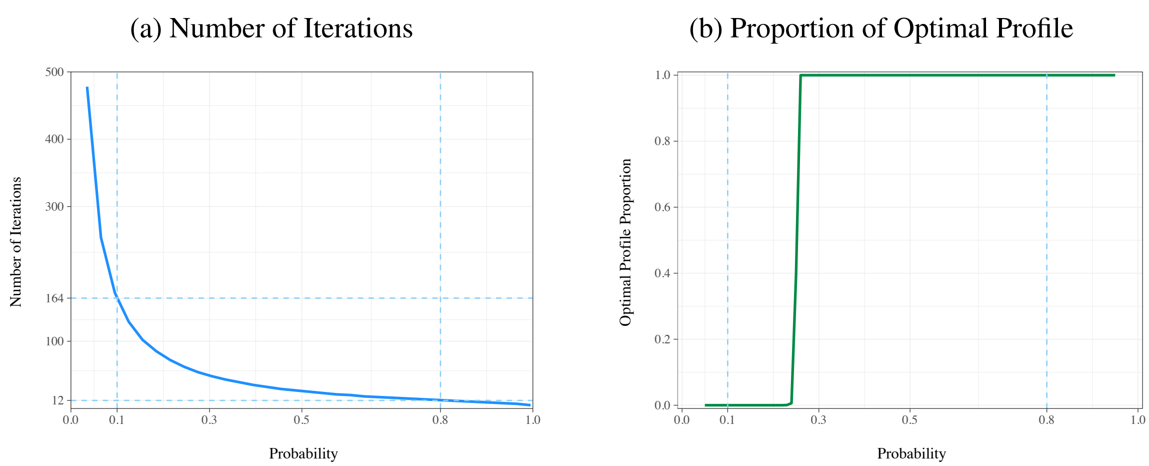


Figure 5: Numerical Solution Outputs

Finally, we re-do the numerical analysis above for payoff parameters in treatment RM-VHBB. We find that with revision opportunities arriving with 80% chance, 14 interactions are sufficient for the uniqueness results to hold. Since the pre-play phase lasts 60 seconds, the theory predicts a unique revision outcome in the revision game and, hence, RM-VHBB results should be similar to those in RM.

C Evaluating the exact predictions for all treatments

In our paper, we highlight the exact theoretical predictions given the environment implemented in the RM treatment. In Section 5.2, we test these exact predictions. We now discuss exact predictions for other treatments. Given the multiplicity of equilibria displayed in many of the treatments (i.e. Baseline, S-CT, R-CT, R-R-CT, I-RM, and S-RM), we concentrate on predictions that hold for all pure strategy equilibria. For that, we focus on variables other than the effort profile chosen; instead, we concentrate on full coordination and on the actual revision process. Below, we describe exact theoretical predictions for all treatments and provide empirical tests for each one.

Coordination In all treatments, the theoretical prediction is that all groups should be fully coordinated at the payoff relevant choice.[26] In Table 3, we present the frequency of fully coordinated groups as well as the equilibrium deviation for the payoff relevant choices of each treatment.

Furthermore, for treatments with uncertainty regarding future revisions, we can go beyond predicting full coordination at the payoff relevant moment. The theoretical prediction is that full coordination should occur from the initial choices. This implies that, for I-RM, S-RM, RM, and RM-VHBB all groups should be fully coordinated from the initial choice

Revising Effort Choices The randomness and asynchronicity of the revision opportunity allow us to obtain additional predictions regarding the revision of effort choices. In the I-RM, RM, and RM-VHBB no player should ever revise their effort along the equilibrium path of any revision equilibrium. In any revision equilibrium of these games, players choose an initial effort configuration that is fully coordinated (in RM and RM-VHBB we can further pin down a unique profile, the efficient effort profile) and never revise their strategy. Please note that, since revisions are randomly and asynchronously awarded, they are not a feature of the equilibrium path of a revision equilibrium. Furthermore, for S-RM, revisions might be a part of the equilibrium path in a revision equilibrium. However, in any pure strategy revision equilibrium, at each revision opportunity either all players jointly revise their effort choices to a profile in which all choose the same effort or no player revise

[26] For R-RM, the prediction is that, with probability high enough the group should be fully coordinated. The exception of R-RM follows from the randomness introduced by exogenous initial choices and random revision opportunities. For instance, it is a possibility that the initial effort profile will not display full coordination and that no-player has any revision opportunity throughout the 60 seconds.

[27] For Deck and Nikiforakis (2012), we have the data from the Neighbourhood Treatment (NT), and the calculations in Table 3 are for that treatment only.

Table 3: Exact predictions and empirical evidence

	Empirical Evidence					Prediction		
Treatment	$FC(PR)$	$ED(PR)$	$FC(I)$	$ED(I)$	NRF	FC	ED	NRF
Baseline	2.5	1.49	NA	NA	NA	100	0	NA
S-CT	27.5	0.86	NA	NA	NA	100	0	NA
R-CT	31.9	0.97	NA	NA	NA	100	0	NA
R-R-CT	36.2	0.96	NA	NA	NA	100	0	NA
I-RM	38.8	0.78	38.8	0.97	46.3	100	0	100
S-RM	46.2	0.69	30.0	1.29	25.0	100	0	100
R-RM	60.0	0.58	NA	NA	NA	100	0	NA
RM	66.2	0.49	50.6	1.04	42.5	100	0	100
RM-VHBB	61.3	0.40	45.0	1.08	42.5	100	0	100
Van Huyck et al. (1990) (B)	0	1.46	NA	NA	NA	100	0	NA
Blume and Ortmann (2007) (B)	12.5	1.74	NA	NA	NA	100	0	NA
Blume and Ortmann (2007) (C)	26.6	1.17	NA	NA	NA	100	0	NA
Deck and Nikiforakis (2012) (NT)[27]	37.0	0.87	NA	NA	NA	100	0	NA
Weber (2006) (B)	1.7	1.5	NA	NA	NA	100	0	NA

Note: FC and ED stand for the percentage of fully coordinated groups and the average equilibrium deviation. NRF presents the frequency of no revision cases. That is, out of all groups and all rounds, in what fraction of cases did groups not revise (every member of a group kept their initial choice till the end of the phase). Finally, PR and I indicate whether the choice is payoff relevant or initial choice. B next to other papers stands for their baseline treatment results. C in Blume and Ortmann (2007) is for the communication treatment in the minimum effort game.

their effort choice and they remain all choosing the same effort level. Hence, one theoretical prediction is that a revision should only be used by a player to change her effort choice if all other players also use that same revision opportunity to change their effort choices.

In Table 3, we provide a test of these predictions. For the treatments I-RM, RM, and RM-VHBB we display the frequency of rounds in which a group does not utilize any revision. For the treatment S-RM, we display the frequency of rounds in which a group either does not utilize any revision or, if a subject utilizes that revision to change her effort choice, then all subject utilize the same revision opportunity.

Taken together, these tests highlight the lack of support for the exact predictions obtained from the theory, in all treatments. In the paper, when evaluating the predictions for RM, we state that the lack of support for exact theoretical predictions is, in some sense, expected given the specificity of such predictions and the simplicity of the equilibrium concepts used. To further substantiate this claim, we also evaluate the empirical support for exact theoretical predictions given the environment implemented in the lab in other papers in the literature.

[28] We would like to thank the authors of the following papers for making their data available for the calculations in the current paper: Weber (2006), Blume and Ortmann (2007), and Deck and Nikiforakis (2012).

Evaluating the exact predictions in the literature[28] We now proceed to testing exact theoretical predictions using data for similar papers in the literature (see Table 3). The introduction of incremental commitment implying the selection of a unique outcome as the subgame perfect equilibrium outcome of the extended game is a novel point of our work, hence we must go beyond theoretical predictions about the effort profile. However, there are alternative dimensions that are clearly pinned down by the theory, even on papers that implement cheap talk communication. Following the same intuition as above, we focus on the prediction that all groups should be fully coordinated in the payoff relevant choice. We note that the prediction that, in all Nash equilibria of the normal form game, a group's effort profile should be fully coordinated holds for simultaneous play minimum effort games. Furthermore, that prediction cannot be altered by pre-play cheap talk communication.

D Additional Tables, Figures and Discussions

In this section we provide more details on overall performance of all treatments, details on non-parametric tests, additional regression analyses, as well as some analyses of the behavior over 60-seconds and 10 rounds.

D.1 Overall performance of all treatments

Figure 6 presents two graphs highlighting the overall performance of all treatment on four dimensions: minimum effort, equilibrium deviation, average effort, and frequency of coordination on median action.

(a) Minimum effort and eqbm deviation (b) Average effort and coordination

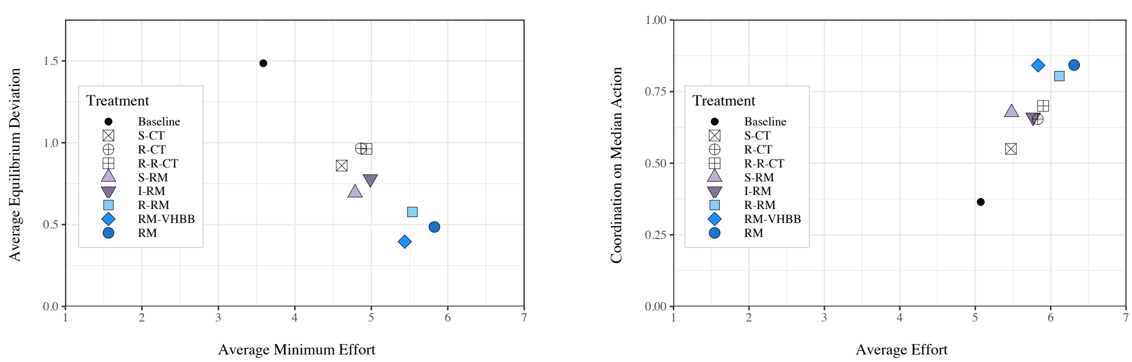


Figure 6: Overall performance of all treatments

D.2 Non-parametric test results

Table 4 includes average values of the main five measures used throughout the paper. Table 5 presents the test results of non-parametric MWU test p-values between RM and other treatments in the paper. We use group average in a round as a unit of observation for all the tests in Table 5.

Figures 7, 8, 9, and 10, include empirical CDFs (ECDFs) of each comparison listed in Table 5. The figures include ECDFs, as well as, the p-value of a non-parametric test, Kolmogorov-Smirnov test, that compares the cumulative distributions of two comparison samples.

Table 4: Average values for focal variables

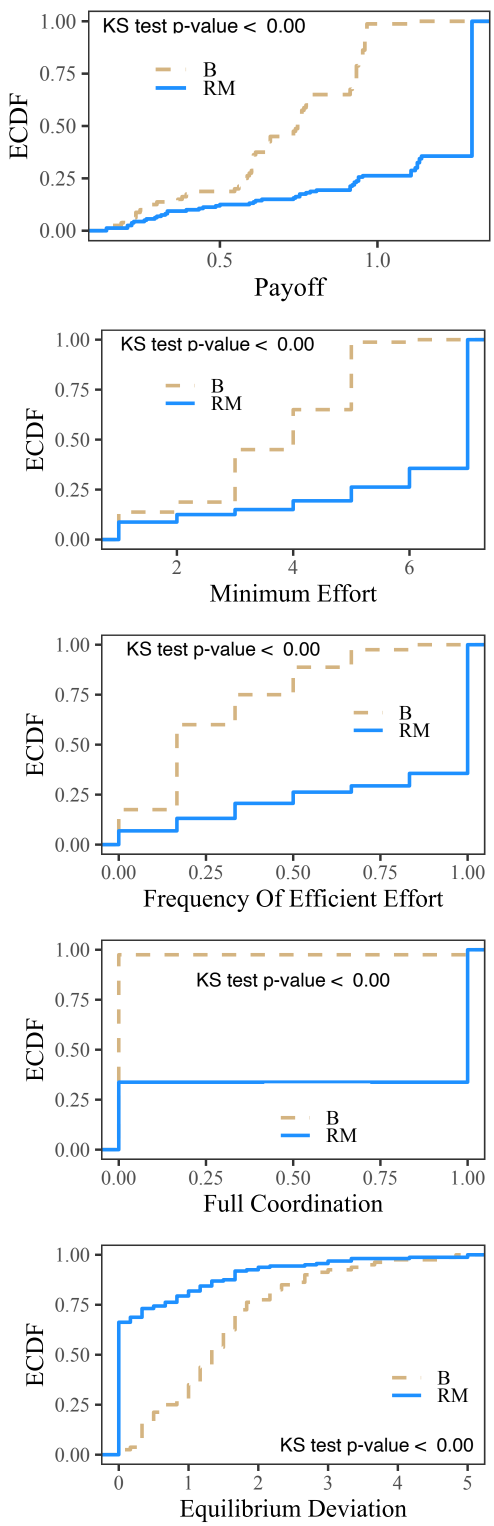
	Payoff	Minimum Effort	Freq 7s	Fully Coord	Eqbm Dev
Baseline	6.946	3.587	0.269	0.025	1.485
S-CT	8.836	4.612	0.442	0.275	0.860
R-CT	9.194	4.862	0.555	0.319	0.966
R-R-CT	9.315	4.938	0.598	0.362	0.963
I-RM	9.468	4.987	0.569	0.388	0.779
S-RM	9.183	4.787	0.490	0.463	0.694
R-RM	10.429	5.537	0.713	0.600	0.577
RM	10.926	5.825	0.780	0.662	0.485
RM-VHBB	11.042	5.438	0.590	0.613	0.396

Table 5: MWU test p-values (group average as a unit of observation)

RM vs.	Payoff	Minimum Effort	Freq 7s	Fully Coord	Eqbm Dev	# of obs.
S-CT	0.000	0.000	0.000	0.000	0.000	160, 80
R-CT	0.000	0.000	0.000	0.000	0.000	160, 160
R-R-CT	0.000	0.000	0.000	0.000	0.000	160, 80
I-RM	0.000	0.000	0.000	0.000	0.000	160, 80
S-RM	0.000	0.000	0.000	0.003	0.006	160, 80
R-RM	0.173	0.169	0.151	0.343	0.319	160, 80
RM-VHBB	0.260	0.061	0.003	0.447	0.772	160, 80

Figure 7: ECDFs for relevant measures

(a) RM vs Baseline



(b) RM vs S-CT

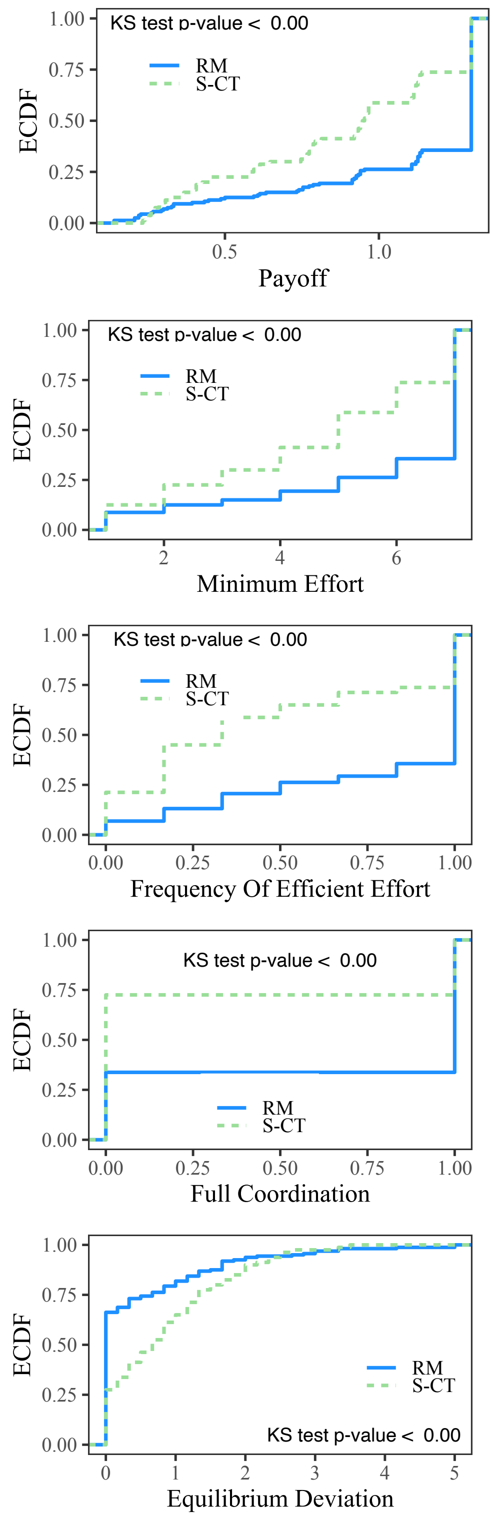


Figure 8: ECDFs for relevant measures

(a) RM vs I-RM

(b) RM vs S-RM

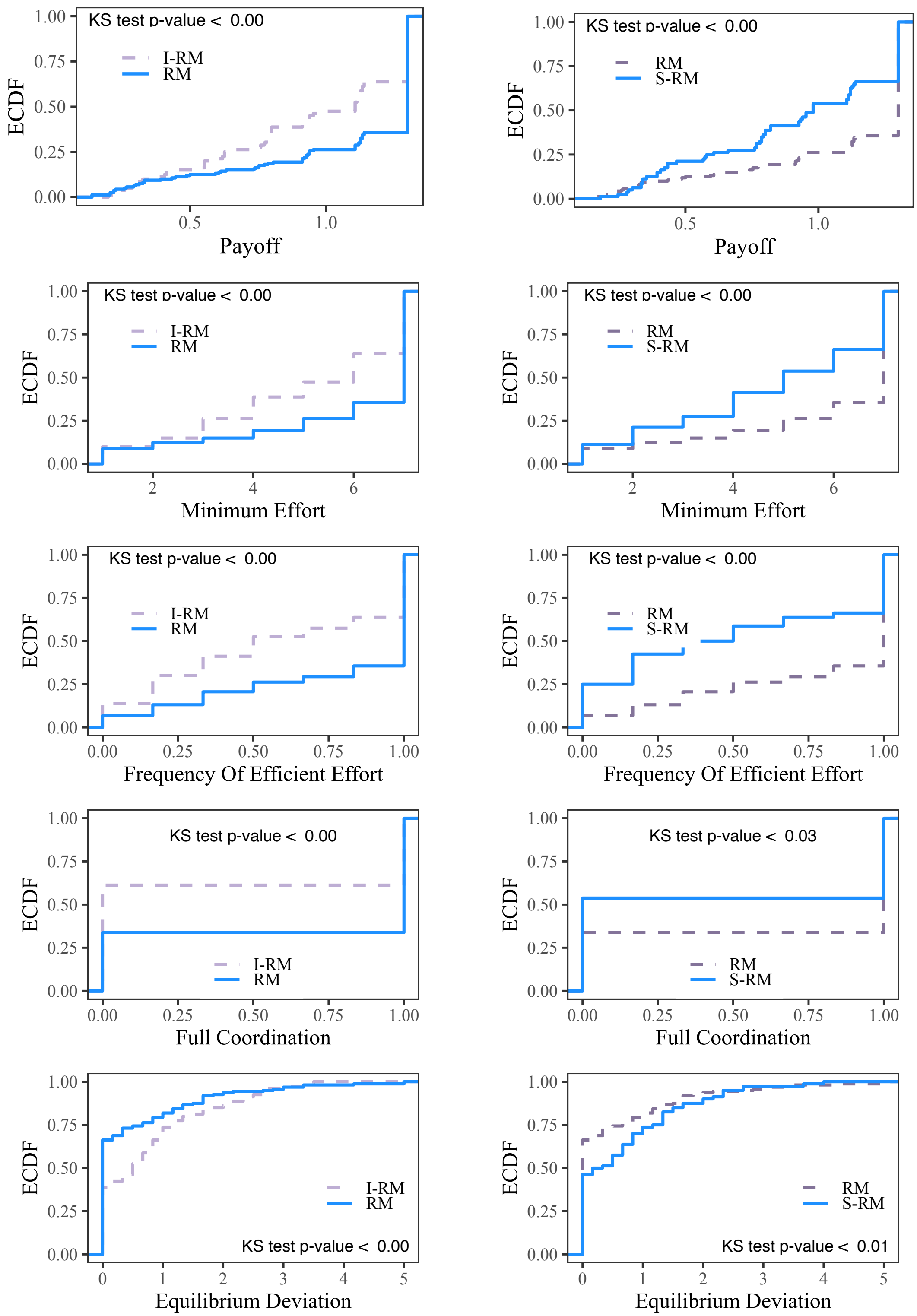


Figure 9: ECDFs for relevant measures

(a) RM vs R-RM

(b) RM vs RM-VHBB

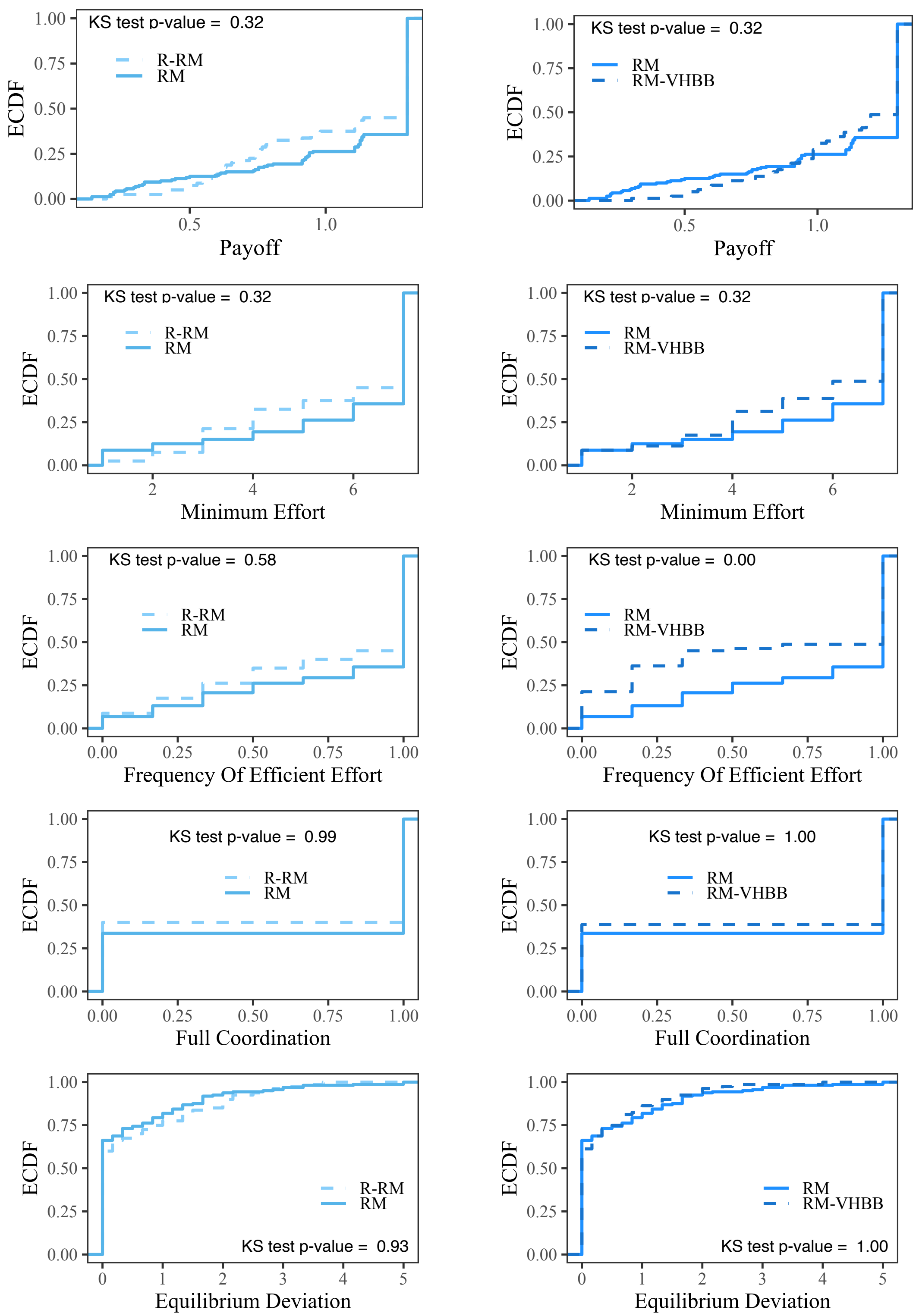
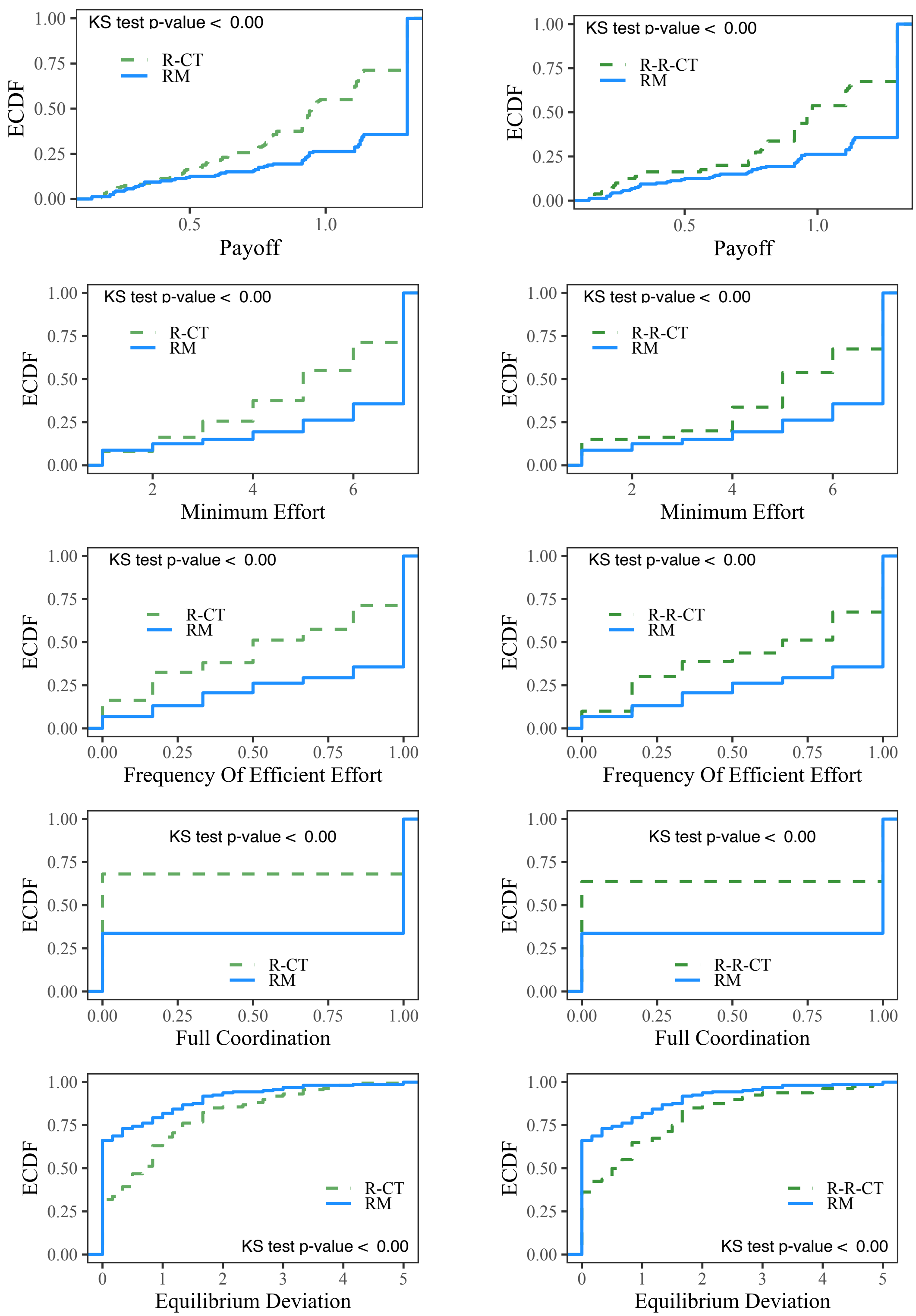


Figure 10: ECDFs for relevant measures

(a) RM vs R-CT

(b) RM vs R-R-CT



D.3 Additional regression analyses

In this section we include additional regression analyses. First we present regressions with combined cheap-talk treatments and then combined revision mechanism treatments.

Table 6: Regression Analysis (S-CT and R-CT as reference)

	Dependent variable:				
	Payoffs	*Minimum Effort*	*Freq Efficient Effort*	*Full Coordination*	*Eqbm Deviation*
Baseline	-0.198^{**}	-1.190^{**}	-0.249^{***}	-0.279^{***}	0.536^{**}
	(0.088)	(0.524)	(0.073)	(0.068)	(0.221)
Revision Mechanism	0.184^{**}	1.050^{**}	0.263^{***}	0.358^{***}	-0.447^{***}
	(0.080)	(0.468)	(0.095)	(0.104)	(0.160)
Constant	0.777^{***}	4.780^{***}	0.517^{***}	0.304^{***}	0.549
	(0.168)	(0.309)	(0.066)	(0.066)	(0.441)
Quiz	0.002				0.010
	(0.022)				(0.040)
R^2	0.141	0.138	0.204	0.215	0.0586
Observations	2880	480	480	480	2880
Demographics	Yes	NA	NA	NA	Yes

Note: *Standard errors clustered at the group level are in parentheses;* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.
Reference category is S-CT and R-CT combined;
Payoffs variable is a subject payoff in a round. *Minimum Effort, Freq Efficient Effort, Full Coordination* are a group level measures and subject demographic information is not applicable. *Eqbm Deviation* is a subject level variable.

Table 7: Regression Analysis (RMs vs CTs)

	Dependent variable:				
	Payoffs	*Minimum Effort*	*Freq Efficient Effort*	*Full Coordination*	*Eqbm Deviation*
Baseline	-0.209^{**}	-1.230^{**}	-0.269^{***}	-0.294^{***}	0.529^{**}
	(0.085)	(0.502)	(0.061)	(0.055)	(0.219)
RMs	0.169^{***}	0.837^{**}	0.178^{**}	0.316^{***}	-0.457^{***}
	(0.059)	(0.343)	(0.073)	(0.072)	(0.130)
Constant	0.837^{***}	4.820^{***}	0.538^{***}	0.319^{***}	0.504
	(0.161)	(0.272)	(0.053)	(0.053)	(0.401)
Quiz	-0.0002				0.021
	(0.022)				(0.039)
R^2	0.122	0.102	0.126	0.173	0.0552
Observations	4320	720	720	720	4320
Demographics	Yes	NA	NA	NA	Yes

Note: *Standard errors clustered at the group level are in parentheses;* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.
Reference category is CTs, combining S-CT, R-CT, and R-R-CT; RMs combine RM, R-RM, and RM-VHBB;
Payoffs variable is a subject payoff in a round. *Minimum Effort, Freq Efficient Effort, Full Coordination* are a group level measures and subject demographic information is not applicable. *Eqbm Deviation* is a subject level variable.

Tables 8 and 9 present regression analyses for all treatments and round variable.

Table 8: Regression Analysis (all treatments)

	DEPENDENT VARIABLE:				
	Payoffs	*Minimum Effort*	*Freq Efficient Effort*	*Full Coordination*	*Eqbm Deviation*
Baseline	−0.177	−1.020	−0.173	-0.250^{**}	0.602^{**}
	(0.127)	(0.744)	(0.124)	(0.115)	(0.269)
Revision Mechanism	0.211^{*}	1.210^{*}	0.339^{**}	0.388^{***}	-0.383^{*}
	(0.121)	(0.706)	(0.138)	(0.139)	(0.224)
Revision Mechanism VHBB	0.221^{**}	0.825	0.148	0.338^{***}	-0.486^{**}
	(0.109)	(0.661)	(0.154)	(0.130)	(0.205)
Random Revision Mechanism	0.159	0.925	0.271^{*}	0.325^{**}	−0.289
	(0.122)	(0.713)	(0.153)	(0.143)	(0.232)
Infrequent Revision Mechanism	0.067	0.375	0.127	0.113	−0.076
	(0.150)	(0.874)	(0.167)	(0.162)	(0.294)
Synchronous Revision Mechanism	0.037	0.175	0.048	0.188	−0.189
	(0.127)	(0.743)	(0.144)	(0.154)	(0.254)
Revision Cheap Talk	0.039	0.250	0.114	0.044	0.100
	(0.120)	(0.702)	(0.143)	(0.139)	(0.232)
Richer Revision Cheap Talk	0.047	0.325	0.156	0.088	0.084
	(0.148)	(0.842)	(0.140)	(0.138)	(0.342)
Constant	0.779^{***}	4.610^{***}	0.442^{***}	0.275^{**}	0.420
	(0.158)	(0.613)	(0.121)	(0.114)	(0.391)
Quiz	0.002				0.026
	(0.019)				(0.037)
R^2	0.104	0.0881	0.129	0.145	0.048
Observations	5280	880	880	880	5280
Demographics	Yes	NA	NA	NA	Yes

Note: *Standard errors clustered at the group level are in parentheses;* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.
Reference category is standard cheap talk S-CT;
Payoffs variable is a subject payoff in a round. *Minimum Effort, Freq Efficient Effort, Full Coordination* are a group level measures and subject demographic information is not applicable. *Eqbm Deviation* is subject level variable.

D.4 Equilibrium deviation and message to action dynamics

Figure 11a presents equilibrium deviation over 60 seconds (for 10 rounds) for 3 treatments. As we described in the main text, the main difference between RM and cheap-talk treatments is in the last 5-10 seconds. Figure 11b shows the movement from 60th second message in R-CT and R-R-CT to the payoff relevant efforts. It is worth noting how similar the results are for R-CT and R-R-CT.

Table 9: Regression analysis (all treatments and round variable)

	Dependent variable:				
	Payoffs	*Minimum Effort*	*Freq Efficient Effort*	*Full Coordination*	*Eqbm Deviation*
Baseline	−0.177	−1.020	−0.173	-0.250^{**}	0.602^{**}
	(0.127)	(0.744)	(0.124)	(0.115)	(0.269)
Revision Mechanism	0.211^{*}	1.210^{*}	0.339^{**}	0.388^{***}	-0.383^{*}
	(0.121)	(0.707)	(0.138)	(0.140)	(0.224)
Revision Mechanism VHBB	0.221^{**}	0.825	0.148	0.338^{***}	-0.486^{**}
	(0.109)	(0.661)	(0.154)	(0.130)	(0.205)
Random Revision Mechanism	0.159	0.925	0.271^{*}	0.325^{**}	−0.289
	(0.122)	(0.713)	(0.153)	(0.143)	(0.232)
Infrequent Revision Mechanism	0.067	0.375	0.127	0.113	−0.076
	(0.150)	(0.874)	(0.167)	(0.162)	(0.295)
Synchronous Revision Mechanism	0.037	0.175	0.048	0.188	−0.189
	(0.127)	(0.743)	(0.144)	(0.154)	(0.254)
Revision Cheap Talk	0.039	0.250	0.114	0.044	0.100
	(0.120)	(0.703)	(0.143)	(0.139)	(0.232)
Richer Revision Cheap Talk	0.047	0.325	0.156	0.088	0.084
	(0.148)	(0.843)	(0.140)	(0.138)	(0.342)
Round	0.031^{***}	0.178^{***}	0.023^{***}	0.047^{***}	-0.097^{***}
	(0.004)	(0.024)	(0.005)	(0.006)	(0.012)
Constant	0.608^{***}	3.630^{***}	0.317^{***}	0.018	0.952^{**}
	(0.159)	(0.623)	(0.120)	(0.114)	(0.392)
Quiz	0.002				0.026
	(0.019)				(0.037)
R^2	0.166	0.15	0.156	0.219	0.0866
Observations	5280	880	880	880	5280
Demographics	Yes	NA	NA	NA	Yes

Note: *Standard errors clustered at the group level are in parentheses;* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.
Reference category is standard cheap talk S-CT;
Payoffs variable is a subject payoff in a round. *Minimum Effort, Freq Efficient Effort, Full Coordination* are a group level measures and subject demographic information is not applicable. *Eqbm Deviation* is a subject level variable.

(a) Eqbm. deviation of revised efforts

(b) Average effort and full coordination

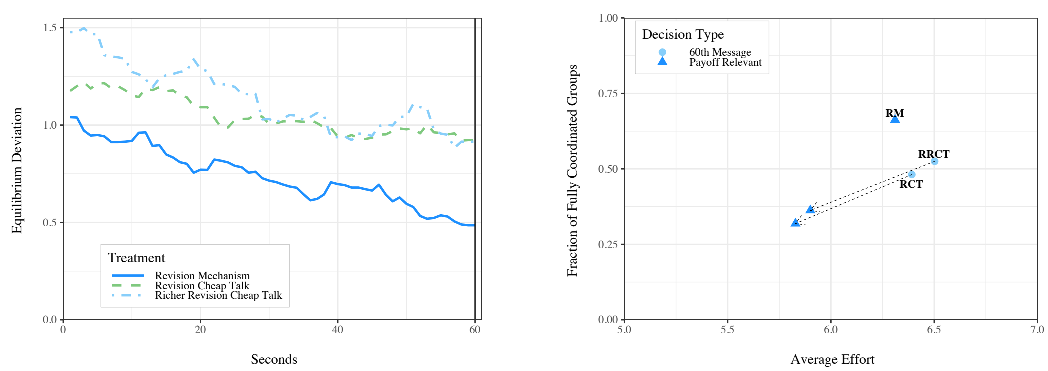


Figure 11: Equilibrium Deviation over Time

D.5 Dynamics over 60 seconds and 10 rounds

In this section we include some graphs highlighting subjects' behavior over 60 second pre-play phase (averaged over all 10 rounds). Then, we present the behavior over 10 rounds separately.

(a) Average effort

(b) Minimum Effort

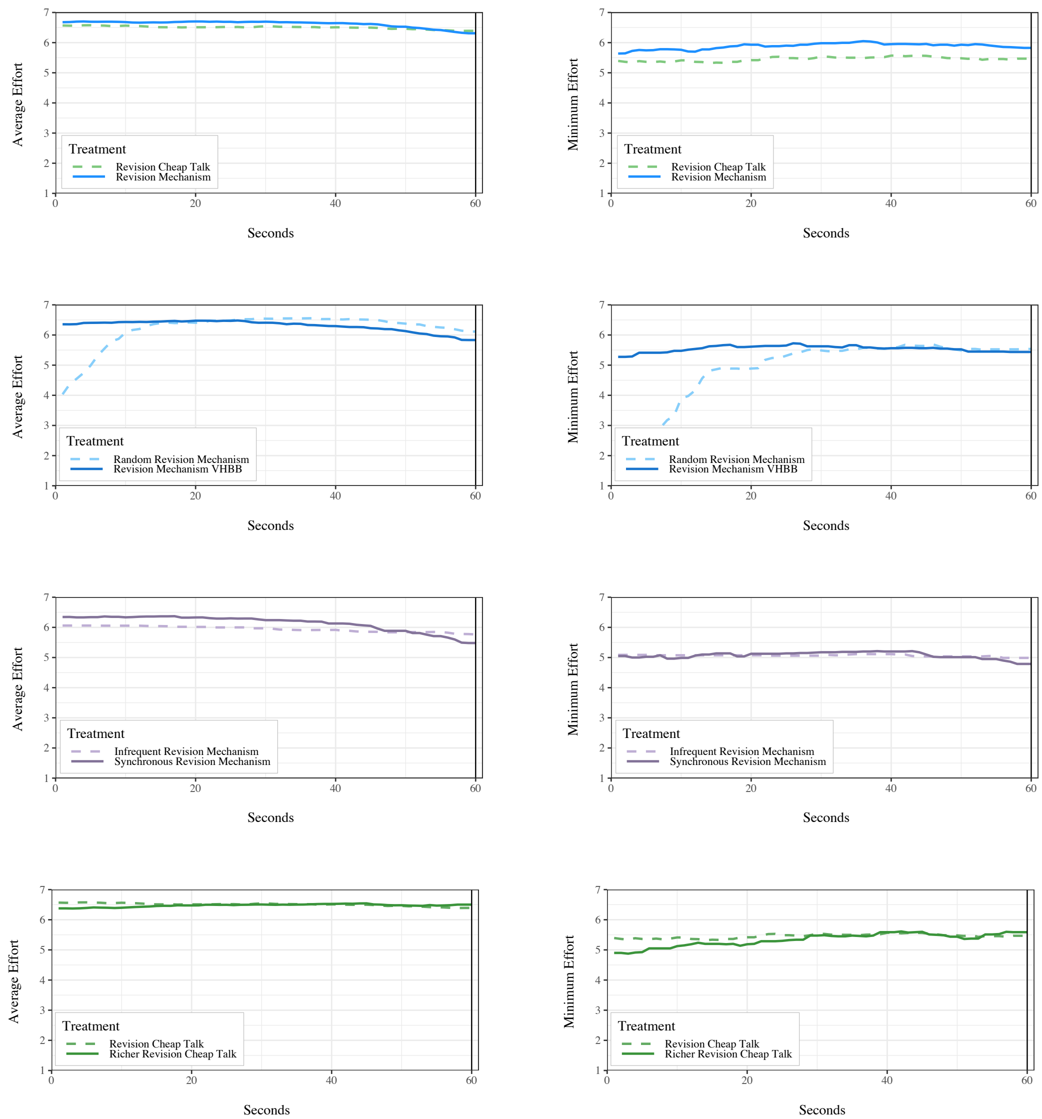
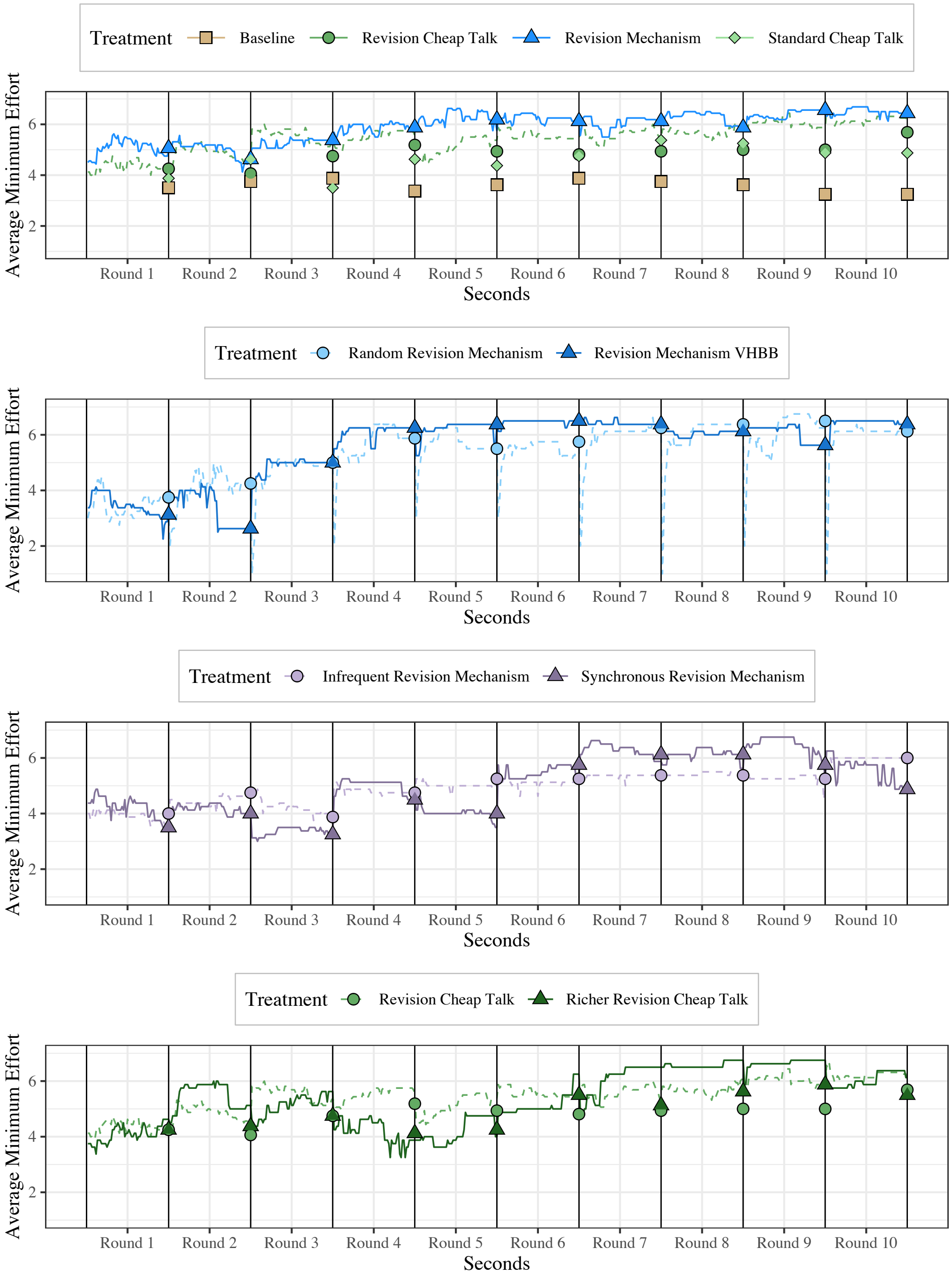


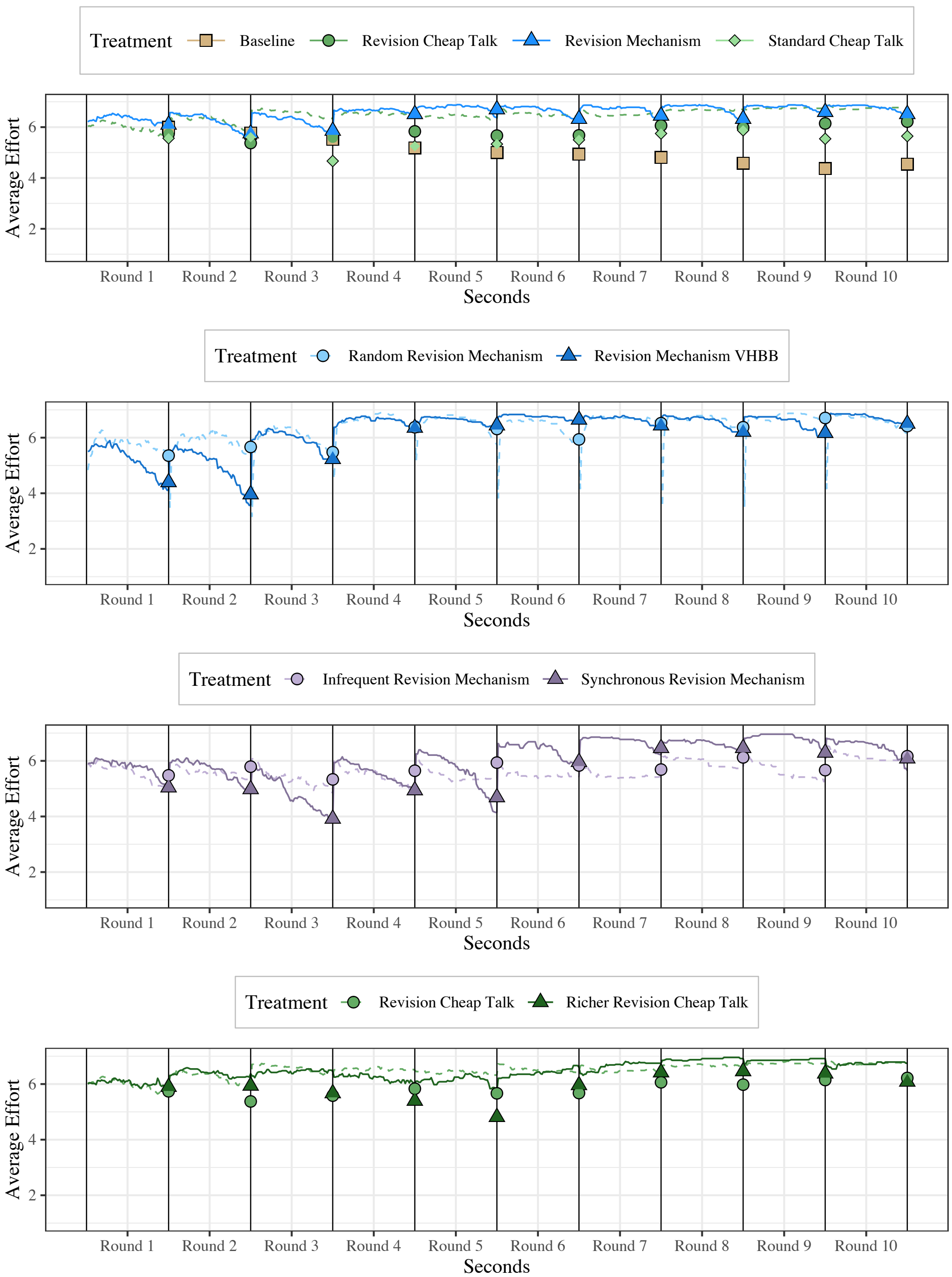
Figure 12: Average and minimum effort over 60 seconds

Figure 13: Average minimum effort over 60 seconds and 10 rounds



For **B, S-CT, R-CT, and R-R-CT,** the 60th second points on the graph represent the payoff relevant choices

Figure 14: Average effort over 60 seconds and 10 rounds



For **B, S-CT, R-CT, and R-R-CT,** the 60th second points on the graph represent the payoff relevant choices

D.6 I-RM vs RM—revised efforts, posted efforts, and initial choices

(a) Average effort (b) Minimum Effort

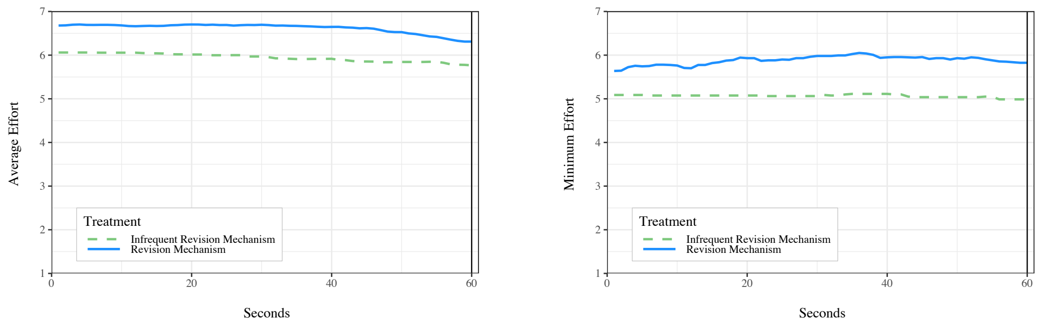


Figure 15: Average effort over 60 seconds

(a) Average effort (b) Minimum Effort

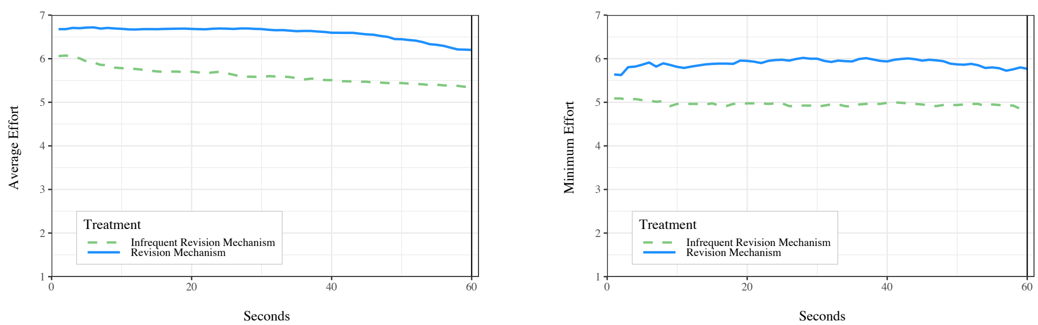


Figure 16: Average and minimum effort over 60 seconds (instant choice data)

Treatment/Effort	1	2	3	4	5	6	7
RM	1.3	0.7	0.4	1.6	4.4	5.9	85.7
I-RM	3.1	1.9	2.9	9.0	7.3	12.7	63.1

Table 10: Initial choice frequency distribution

E Revised effort vs posted effort

In our paper, actions can be changed only when a revision opportunity is awarded. Thus, in any instant, two different data points exist per player: (i) the effort the player is currently committed to, which all the other players are observing, and (ii) the effort currently selected by the player. Only after a revision opportunity is awarded can the selected effort choice become the effort to which the player is committed.

Let us evaluate the robustness of our experimental design by examining whether our choices of the time-interval length and the revision probability had an impact on the choices. We therefore compare the last-instant intended efforts with the efforts played out. If the time interval were too short or the revisions too infrequent, the players' intended actions would differ from the posted actions, even in the last instant, and subjects would have been constrained in their choice process. However, we cannot reject the hypothesis of equal distributions of actions ($p > 0.1$), which indicates the choice of interval length and revision frequency did not bind players' behaviors, thus aligning our experimental design with the conditions of the main proposition.