



1 Experimental cheap talk games: strategic complementarity 2 and coordination

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4 Accepted: 25 November 2020
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6 Abstract

7 This paper investigates experimentally the effects of communication in distinct
8 games with complete information. We design four games resulting from the inter-
9 action between two incentive elements: strategic complementarity and coordination.
10 These incentive elements allow to analyse the use of cheap talk as an efficiency-
11 enhancing and coordinating device. We implement a restricted communication
12 protocol (one-sided, optional, and closed-form) in repeated settings with fixed
13 partners. Our findings provide robust evidence about how cheap talk interacts with
14 incentives to explain strategic behaviour in a dynamic way. As expected, cheap talk
15 increases efficiency under complementarity incentives, and the coordination level
16 under coordination incentives. As novelty, the use of limited communication in
17 repeated interactions has led to identify specific time-varying message profiles as
18 the most effective messages in the coordination games. While the content of
19 messages is explained by the complementarity incentives, faithfulness to credible
20 messages is determined by the coordination incentives.

21
22 **Keywords** Cheap talk · Incentives · Strategic complementarity · Coordination ·
23 Complete information · Laboratory experiment
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A1 **Electronic supplementary material** The online version of this article ([https://doi.org/10.1007/s11238-](https://doi.org/10.1007/s11238-020-09795-9)
A2 [020-09795-9](https://doi.org/10.1007/s11238-020-09795-9)) contains supplementary material, which is available to authorized users.

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27 **1 Introduction**

28 Since the 1980s, theorists have studied the use of cheap talk in the transmission of
 29 strategic information in games. In this context, cheap talk is defined as non-binding
 30 and costless communication which does not influence the incentive structure of the
 31 games. The literature tackles two kinds of problems: the revelation of private
 32 information in sender–receiver games with private information (see the seminal
 33 paper of Crawford and Sobel 1982) and the revelation of intentions about future
 34 actions in games with complete information (Farrell 1987, 1988; Rabin 1994;
 35 Farrell and Rabin 1996). Our research delves into this second field. Specifically, we
 36 investigate the effectiveness of one-way restricted-form cheap talk in finitely
 37 repeated complete information games.

38 With that aim, we design a laboratory experiment to compare individual
 39 behaviour across games which differ in two incentive dimensions: strategic
 40 complementarity and coordination. Namely, we have defined a general game which
 41 generates a 2×2 set of games according to whether: (1) players' strategies are
 42 complementary or not, and (2) the strategic setting is a coordination game or not.
 43 The general framework is a parameterised version of the standard two-person
 44 Beauty Contest Game (BCG, henceforth). In this game, two players simultaneously
 45 choose a number between 0 and 100, both included. The winner is the person whose
 46 number is closest to $2/3$ of the average of the two chosen numbers, that is, the
 47 minimum. The winner gains a prize, the loser gets zero (see Nagel 1998; Grosskopf
 48 and Nagel 2008; Costa-Gomes and Crawford 2006; Camerer et al. 2004; Rydval
 49 et al. 2009, or Nagel et al. 2017).

50 The parameter configurations are built varying two features of the BCG's payoff
 51 function: (1) whether the winner's prize depends on the chosen numbers or the prize
 52 is a fixed value, and (2) whether, in the case of a tie, both players get the full prize or
 53 the prize is shared equally. These two elements implement the strategic comple-
 54 mentarity dimension and the coordination dimension, respectively.

55 Most previous research has compared the impact of several communication
 56 protocols in a given game. Here, we follow a different approach implementing a
 57 specific cheap talk device in several games. To the best of our knowledge, the
 58 closest work to the present one is Duffy and Feltovich (2002) which also examines
 59 the impact of cheap talk in different games with complete information (prisoner's
 60 dilemma, stag hunt, and chicken games). However, unlike Duffy and Feltovich
 61 (2002)'s games, our games are *parameter-related* to study the marginal effects of
 62 the two incentive dimensions.

63 Although the designed experimental games noticeably differ from each other in
 64 the incentive structures, they share a common decision framework: an abstract and
 65 neutral description of the game rules and a fairly wide strategy space. To investigate
 66 the effectiveness of cheap talk in such a challenging setting, we use an optional one-
 67 sided cheap talk protocol with a fixed message format (numerical) but with a large
 68 message space. Closed-form messages are shown to be less effective than other
 69 forms of less-restricted communication like chats in one-shot games (Crawford
 70 1998). For these reasons, we allow subjects to interact repeatedly during six rounds

71 in fixed groups (see, Blume and Ortmann 2007; Cason et al. 2012, for similar
72 designs.)

73 In addition, allowing repeated communication opportunities lets us examine how
74 incentives shape cheap talk over time, an aspect that remains hardly explored in the
75 experimental literature. Our empirical strategy follows Cooper and Kühn (2016): we
76 study how faithfulness to messages and players' attempts to correct previous actions
77 change dynamically. Furthermore, we can assess how subjects use the communi-
78 cation protocol (identical for all games) under the (different) incentive structures of
79 each game. The big message space directly linked with the choice space will allow
80 us to identify the behavioural determinants of message and decision patterns over
81 time.

82 Theoretically, in games with complete information, the informative content and
83 credibility of messages are determined by how conflictive players' preferences are.
84 According to Farrell and Rabin (1996), a cheap talk message can only be credible if
85 it is part of an equilibrium. Therefore, if cheap talk does not generate any additional
86 equilibrium in the underlying game, behaviour should remain unchanged irrespec-
87 tively of the communication conditions in games with a unique equilibrium (under
88 the rationality hypothesis). The experimental literature has shown that, against these
89 theoretical predictions, communication leads to efficiency gains by promoting
90 cooperation on out-of-equilibrium Pareto superior outcomes (see the surveys by
91 Sally 1995 and Balliet 2010; see also Charness and Dufwenberg 2006; Fonseca and
92 Normann 2012 or Yamamori et al. 2008).

93 On the contrary, the theoretical analysis of cheap talk is non-trivial in
94 coordination games with multiple equilibria. In these games, the primary interest
95 of researchers has been to examine whether communication increases the
96 occurrence of actual equilibrium play (in pure strategies) or efficiency (Costa-
97 Gomes 2002). Experimental evidence shows that cheap talk fosters coordination
98 but, as the theory predicts, its specific role depends on the degree of alignment of
99 players' interests (see the surveys by Crawford, 1998 and Camerer, 2003, chapter 7).
100 For instance, in pure coordination games like the matching game, pre-play
101 communication promotes coordination by generating focal points (Schelling 1960);
102 in games with Pareto-ranked symmetric equilibria like the stag hunt, cheap talk may
103 promote the efficient equilibrium selection (Cooper et al. 1992; Charness 2000;
104 Clark et al. 2001 or Blume and Ortmann 2007). One key feature of our coordination
105 games is that, being all messages equally credible, effectiveness of messages
106 depends on their usefulness to mitigate strategic uncertainty or to increase efficiency
107 in a dynamic way.

108 Our experimental results provide robust evidence that dynamics of behaviour
109 changes noticeably with communication. Not only does cheap talk help subjects to
110 respond more consistently to underlying incentives, but also subjects learn gradually
111 how to use the communication protocol in response to the incentive structures.
112 Cheap talk is significantly effective in enhancing efficiency under complementarity
113 incentives and the coordination level under coordination incentives. Furthermore,
114 cheap talk exerts such predicted effects from the outset of play, being the initial
115 effects reinforced by a selective use of communication along time. Efficiency gains
116 and coordination are mainly reached through sending credible messages, given the



117 properties of every strategic environment. In the coordination environments, we find
 118 that faithfulness depends crucially on the usefulness of messages.

119 The remainder of the paper is organised as follows. Section 2 describes the
 120 experimental design, procedures, and hypotheses. Section 3 reports the main
 121 experimental findings related to the design variables, whereas the effects of
 122 communication in each specific game are discussed in Sect. 4. The conclusion is
 123 drawn in Sect. 5.¹

124 2 Experimental design, procedures, and research hypotheses

125 2.1 Experimental design

126 The experiment consists of four games and eight treatments, with a $2 \times 2 \times 2$
 127 factorial design. The design comprises four symmetric two-person games; each
 128 game is played under two communication conditions: with and without cheap talk
 129 (COMM vs. NON-COMM).

130 The four games are specific parameterisations of a general game, built upon a
 131 standard two-person Beauty Contest Game (BCG): both players have to simulta-
 132 neously choose a number (up to two decimal places) between 0 and 100, both
 133 included. That is, the space of actions has 10,001 alternatives. The winner is the
 134 person whose number is the closest to $2/3$ of the average of the two chosen numbers
 135 (the player with the lowest choice). The winner gains a fixed prize, the loser
 136 receives nothing. In case of a tie, the prize is shared equally. This game constitutes
 137 our baseline, with neither strategic complementarity nor coordination incentives.
 138 The other three games are the result of modifying the BCG's payoff function.

139 Strategic complementarity (COMPL) is implemented by making the winner's prize
 140 proportional to the chosen actions (variable payoffs).² In particular, the prize is set
 141 to be equal to the winner's number. Thus, the payoff associated with the best
 142 response function is increasing in the other player's choice. A change in a player's
 143 choice encourages the opponent to move in the same direction, which leads to a
 144 positive alignment of their preferences. If we measure efficiency as the sum of the
 145 two players' payoffs, complementarity allows for efficiency gains when both
 146 players increase their choices. By contrast, when the prize is fixed as in the standard
 147 BCG, a change in one player's choice does not affect the other player's marginal
 148 payoff and, consequently, no efficiency improvement is possible (non-complementarity,
 149 NON-COMPL).

150 To introduce coordination incentives (COORD), we allow that, in the case of a tie,
 151 each player gains the full prize rather than halving the prize between winners (non-

1FL01 ¹ In the Online Supplementary Material, we present the benchmark model (Section S.1), an additional
 1FL02 descriptive and econometric analysis (Section S.2), and the experimental instructions (Section S.3).

2FL01 ² Strategic complementarity is related to the *positive* effect of a change in a player's choice on the
 2FL02 marginal payoff of the other player (see Cooper, 1999; Eichberger and Kelsey, 2002 or Potters and
 2FL03 Suetens, 2009). We say that strategies are non-complementary if a player cannot increase her payoff
 2FL04 when the other player changes her action. See Morris and Shin (2002) and Nagel (1998) for alternative
 2FL05 ways to introduce strategic complementarity in a BCG.

152 rival payoffs). In this case, any player's best response includes the other player's
 153 choice, i.e., there are no incentives to undercut. Therefore, any number chosen by
 154 both players in the interval $[0, 100]$ constitutes a Nash Equilibrium. However, when
 155 the prize is equally divided in case of a tie (NON-COORD), undercutting the other
 156 player (almost) doubles the individual payoff. In this case, the equilibrium is unique
 157 and given by $\{0, 0\}$ irrespectively of the complementarity condition.

158 Therefore, with respect to our baseline, while complementarity introduces
 159 efficiency issues (without affecting the set of equilibria), coordination increases the
 160 number of equilibria. The four strategic environments are shown in Table 1.

161 We next summarise the theoretical properties of each game:³

162 • *Non-complementarity and non-coordination game (Non-Compl Non-Coord)* The
 163 winner's prize is fixed at 50 points and, in the case of a tie, each player gets half,
 164 i.e., 25 points. The best response correspondence to the other player's choice X_{-i}
 165 is any number below it: $BR(X_{-i}) = [0, X_{-i})$. The unique equilibrium is the dyad
 166 $\{0, 0\}$.

167 As a constant-sum game, any outcome is equally efficient. Hence, players'
 168 strategies are non-complementary, because the winner's payoff is always 50
 169 independently of the chosen numbers. As mentioned above, this environment
 170 corresponds to the standard two-person BCG.

171 • *Complementarity and non-coordination game (Compl Non-Coord)* The winner's
 172 payoff is equal to her chosen number, and each player receives half the prize
 173 when they play the same action. The best response correspondence to the other
 174 player's choice X_{-i} is given by $BR(X_{-i}) = X_{-i} - \epsilon$, being $\epsilon = 0.01$ in our design.
 175 The equilibrium is the dyad $\{0, 0\}$.⁴

176 This is a game with strategic complementarity, because one player's best
 177 response depends positively on the other player's choice. Moreover, efficiency
 178 gains can be achieved by increasing *both* players' numbers up to the upper
 179 bound of 100. In this regard, the environment can be read as a social dilemma or
 180 a Bertrand competition model with unitary demand.

181 • *Non-complementarity and coordination game (Non-Compl Coord)* In the case of
 182 a tie, each player receives a fixed prize of 50 points. The best response
 183 correspondence to the other player's choice X_{-i} is any number below *or equal* to
 184 it: $BR(X_{-i}) = [0, X_{-i}]$. Then, any outcome $\{X_i, X_{-i}\}$ where $X_i = X_{-i}$ is an
 185 equilibrium, but only $\{0, 0\}$ is a strict equilibrium; the remaining profiles are
 186 weak equilibria where each player is indifferent between keeping and reducing
 187 her choice.

188 It is a coordination game with symmetric payoffs in equilibrium and a risk-
 189 dominant equilibrium, $\{0, 0\}$. Players' strategies are not complementary,
 190 because the winner's payoff is always 50 regardless of both players' choices.

3FL01 ³ In section S.1 of the Online Supplementary Material, we characterise a general model and derive the
 3FL02 properties of each game by setting specific values for two payoff function parameters.

4FL01 ⁴ Indeed, as we let subjects choose up to two decimals, the game has 3 symmetric equilibria: $\{0, 0\}$,
 4FL02 $\{0.01, 0.01\}$ and $\{0.02, 0.02\}$. From the viewpoint of payoff saliency in the lab, the difference between
 4FL03 them is insignificant. Hence, we will assume that the COMPL NON-COORD game has a unique Nash
 4FL04 equilibrium given by $\{0, 0\}$.

Table 1 Incentive conditions across games

		Coordination	
		No	Yes
Complementarity	No	NON-COMPL NON-COORD	NON-COMPL COORD
	Yes	COMPL NON-COORD	COMPL COORD

191 In this sense, the game is similar to a matching game. However, unlike the
 192 matching game, out-of-equilibrium payoffs are not zero for one of the players.
 193 Thus, any equilibrium different from zero entails more strategic uncertainty in
 194 our game than in a standard matching game. Notice that (in our game) all
 195 equilibria are equally efficient, but their efficiency level doubles that of any non-
 196 equilibrium outcome.

197 • *Complementary and coordination game (Compl Coord)* When both players
 198 make the same choice, each one receives a payoff equal to that number. The best
 199 response correspondence to the other player's choice X_{-i} is that number:
 200 $BR(X_{-i}) = X_{-i}$. Hence, any outcome $\{X_i, X_{-i}\}$ where $X_i = X_{-i}$ constitutes an
 201 equilibrium.

202 As one player's best response depends positively on the other player's choice,
 203 strategies are complementary. The equilibria are Pareto-ranked, being
 204 $\{100, 100\}$ the efficient one. This game can be considered as a variant of the
 205 stag-hunt game with a much bigger strategy space.

206 To study the role of pre-play cheap talk across games, we opted for a one-sided
 207 closed-form voluntary recommendation to collude. The message was framed as an
 208 invitation to collude to facilitate a cooperative strategy: "We should choose the
 209 number" (see Section S.3 in the Online Supplementary Material). Based on
 210 previous research, this communication protocol is implemented for several reasons:

- 211 1. For a better understanding of the relationship between cheap talk and incentives,
 212 the simplest protocol is needed. We expect that the impact of one-way
 213 communication will be more homogeneous across games, and noise in the data
 214 will be lowered in comparison with alternative protocols, like a chat or free
 215 conversation.⁵ Besides, the closed-form structure is expected to reinforce the
 216 common language property (Farrell 1993).
- 217 2. As Cooper and Kühn (2016) point out, restricted messages have demonstrated
 218 limited effectiveness in enhancing cooperation, because they have been
 219 typically implemented through just one round of communication, which
 220 prevents subjects from building complex proposals such as punishment threats.⁶

5FL01 ⁵ Some notable examples of the advantages of using a one-way protocol for studying the effect of
 5FL02 communication include Cooper et al. (1989), Cooper et al. (1992), Farrell and Rabin (1996), Charness
 5FL03 (2000), Pogrebna et al. (2011), or Feldhaus and Stauf (2016).

6FL01 ⁶ Cooper and Kühn (2016) find that the effectiveness of one-way communication may be higher in
 6FL02 sequential settings where players have future opportunities to modify previous actions or renegotiate
 6FL03 messages. These authors compare the relative effectiveness of two communication protocols (limited
 6FL04 messages vs. chat) in two-period sequential games. They allow subjects to communicate successively, so

221 Instead, we allow subjects to interact repeatedly with the same partners, so that
 222 they can punish and/or renegotiate after any unilateral deviation without sending
 223 threatening or grim-trigger messages. Our focus is to implement a message
 224 structure particularly suitable for refining the message content along the
 225 successive communication opportunities (particularly with full feedback after
 226 each round).

- 227 3. Given the wide choice and message spaces, the straitjacket nature of the
 228 communication protocol enjoys a great methodological advantage: it allows for
 229 clean tests of theoretical predictions on cheap talk effects and subjects'
 230 reactions to messages (i.e., faithfulness) compared with free-form communi-
 231 cation protocols, particularly with repeated interactions (see, Cooper and Kühn
 232 2014, for example).
- 233 4. The use of a non-compulsory protocol allows us to study how incentives
 234 influence on the propensity to communicate, as well as the content of messages.
 235 The incentives to send messages clearly differ across games. For instance, the
 236 optional protocol offers players the possibility of keeping silent (and avoiding
 237 being caught cheating) in a full information context.⁷

238 Each game was played repeatedly for six rounds with fixed partners. We chose just
 239 six repetitions to minimise the possible subjects' interpretation of the games as
 240 supergames. Use of finitely repeated games to test theoretical predictions based on
 241 static games is far from rare in the experimental literature, even with communi-
 242 cation (Cooper et al. 1992; Balliet 2010; Cason and Mui 2014). As mentioned
 243 above, fixed partnership is needed to investigate the impact of communication
 244 opportunities (immediate and future). Parkhurst et al. (2004) particularly study the
 245 interaction of the matching protocol with communication in a repeated Pareto-
 246 ranked coordination game. Their results support our assumption that, in the non-
 247 communication conditions, coordination failure is higher and efficiency is lower
 248 when subjects are randomly matched, although, with communication, behaviour
 249 does not differ between the matching protocols.

250 Another important feature of our design is that the incentive structure determines
 251 the credibility of messages. This is consistent with the theoretical literature on cheap
 252 talk. For example, in a slightly modified stag-hunt game, Aumann (1990)
 253 conjectures that messages could be meaningless if the sender has a strict preference
 254 over the other player's choice regardless of her intention (the sender) of playing (or

6FL05
 6FL06 Footnote 6 continued

6FL07 that messages can express intentions about contingent strategies (first-period actions and second-period
 6FL08 actions as a function of the first-period outcome).

7FL01 ⁷ Besides, the optional protocol allows to identify those subjects who resort to secret handshakes to reach
 7FL02 coordination. In games with private information, recent literature incorporates optional communication as
 7FL03 a way of distinguishing between distinct behavioural models when players' preferences are not aligned:
 7FL04 preference for truth-telling, lying aversion, etc. (see, for example, Goeree and Zhang 2014 or Cai and
 7FL05 Wang 2006). An example of optional communication in complete information games can be found in
 7FL06 Andersson and Wengström (2012). In our experiment, the motivations for keeping silent depend on the
 7FL07 specific game; for instance, in the NON-COMPL NON-COORD game, rational players have incentives to avoid
 7FL08 communication, so that non-rational players take more time attempting to capture the strategic features of
 7FL09 the game.

255 not) according to the submitted signal. Farrell and Rabin (1996) propose two
 256 necessary conditions for a message to be credible: self-signalling and self-
 257 committing. A message is self-signalling when the sender prefers the receiver to
 258 respond to it optimally *if and only if* she (the sender) truly intends to play the
 259 signalled action. The second condition, self-committing, is related to the sender's
 260 best response if she thought the receiver would believe the message, that is, when
 261 the sender's signal is part of a Nash equilibrium. When messages satisfy both
 262 conditions, they should be truthful and should be believed by rational players.⁸

263 Following Farrell and Rabin's nomenclature, any message with value zero is
 264 credible in all our games. Messages above zero are credible only in the two
 265 coordination games. Namely, in the coordination games, any message in the interval
 266 [0, 100] is both self-signalling and self-committing.⁹

267 However, Andersson and Wengström (2012) point out that, in dynamic games,
 268 the credibility of pre-play messages may depend on the existence of future
 269 communication opportunities in subsequent stages of the game: for a given message
 270 to be credible, it may be not enough to be part of a Nash equilibrium but, also, it
 271 should be immune to renegotiation opportunities. Applied to our design, with pre-
 272 play messages in each round, the players should consider that the intentions of
 273 action elicited in a particular message may be revised in the subsequent
 274 communication round. Thus, a message, even being credible, should propose
 275 actions that are robust to renegotiation in terms of efficiency or coordination.
 276 Otherwise, it is not obvious to interpret any unilateral deviation from messages as
 277 cheating or, instead, as attempts of a receiver to signal better future outcomes.
 278 Another implication of such an argument is that non-robust messages can evolve
 279 towards outcomes that are preferred by both players or, by contrast, can be used as
 280 a deception instrument. Our design allows to identify the specific goals of cheap talk
 281 by observing time-varying message profiles under the different incentive structures
 282 of our games (Blume and Ortmann 2007). In this regard, we introduce the concept
 283 of usefulness to characterise the robustness of credible messages to renegotiation.

284 Although, in the two coordination games, all messages are equally credible, some
 285 specific message values may be more useful than others to reduce strategic
 286 uncertainty, particularly in the NON-COMPL COORD game where payoffs are fixed
 287 regardless of choices. Likewise, when equilibria are Pareto-ranked as in the COMPL
 288 COORD game, the messages signalling the efficient outcome may be more useful to
 289 enhance profits.

290 Therefore, in our repeated setting, we posit that faithfulness to messages will
 291 depend on their credibility *and* their usefulness. While credibility is determined by
 292 coordination incentives, usefulness is applied to those credible messages which
 293 point towards efficiency-improving choices or uncertainty-reducing choices.

8FL01 ⁸ In the game-theoretic sense, rationality assumes that players' decisions are the result of maximising
 8FL02 their own selfish payoff functions conditional on their beliefs about the other players' optimal behaviour
 8FL03 (see, Bicchieri 2004).

9FL01 ⁹ Yet, in the NON-COMPL COORD game, all equilibria but 0 are "weak", since players are indifferent
 9FL02 between playing the equilibrium and deviating downwards. Hence, the sender has no *strict* preferences for
 9FL03 playing the announced number if she (the sender) thought the receiver would believe her. Here, we can
 9FL04 say that the messages are weakly credible.

294 2.2 Experimental procedures

295 There are two sessions for each treatment, with 20 and 30 subjects each. We have 25
 296 independent couples in each treatment, with subjects participating in only one
 297 (between-subject design). In total, 400 undergraduate students at the University of
 298 Valencia participated in the experiment. All sessions were computerised using the z -
 299 Tree software (Fischbacher 2007) at LINEEX laboratory. Sessions lasted about 30
 300 min. Average earnings were 13.4 euros.

301 Payoffs were accumulated over the 6 rounds and participants were paid
 302 individually and privately at the end of each session, using an exchange rate of 0.06/
 303 0.07 euros per point. We decided to pay for total profits, instead of choosing one
 304 round at random, to avoid zero payments given the discontinuity of the payoff
 305 function.

306 In each session, participants were seated at independent terminals and paired
 307 using an anonymous fixed-matching protocol. Instructions were read aloud by the
 308 experimenter. After each round, subjects received full feedback about both players'
 309 choices, the average of the couple, $2/3$ of the average, the winner(s), and the
 310 individual prize.

311 In the cheap talk treatments, one of the couple members had the chance of
 312 suggesting a number to her partner before playing each round. Participants were
 313 informed that the sender would be randomly selected at the beginning of the
 314 experiment and would maintain that role during the session. Setting a fixed sender
 315 role eliminates the possible interactions between the player role and the round of
 316 play and reduces, thereby, the noise in the data.

317 2.3 Research hypotheses

318 While game theory provides sharp predictions for each static game, we did not
 319 expect to find much equilibrium play in the first round of the experiment (Grosskopf
 320 and Nagel 2008; Dufwenberg and Gneezy 2000; Dufwenberg et al. 2006; Devetag
 321 and Ortmann 2007). Nevertheless, individual behaviour is expected to be
 322 progressively influenced by the properties of each game. Hence, we state the
 323 behavioural hypotheses in terms of the expected directional effects of game
 324 properties. In particular, taking the baseline as reference, we postulate the
 325 hypotheses according to three types of effects: (1) effects of the design variables
 326 on choice behaviour; (2) effects of the incentive variables on the communication
 327 process; and (3) effects of communication on efficiency and coordination. We define
 328 coordination as any pair of choices in which both players select the same number
 329 regardless of the game (i.e., either non-coordination games or coordination games).
 330 Therefore, there will be coordination at any equilibrium of each game, since all
 331 equilibria are symmetric.

332 Regarding the effects of the design variables, we anticipate that:

333 **Hypothesis 1a** When communication is absent, subjects will select higher numbers
 334 with strategic complementarity than without it.



335 **Hypothesis 1b** When communication is absent, subjects will select higher numbers
 336 and reach a higher coordination level with coordination incentives than without
 337 them.

338 **Hypothesis 1c** Cheap talk will help reinforce the effects of strategic complemen-
 339 tarity and coordination.

340 The first two hypotheses refer to the expected effects of incentives without
 341 communication. On one hand, it is well known that strategic complementarity
 342 induces experimental subjects to deviate from a Pareto-inefficient Nash equilibrium,
 343 given that the effect of a change in a player's choice on the marginal payoff of
 344 another player is positive (see, for example, Potters and Suetens 2009). We
 345 therefore conjecture that subjects will choose higher numbers under complemen-
 346 tarity, even when equilibrium is unique, with the aim of increasing payoffs. On the
 347 other hand, in line with previous research (Camerer 2003; Devetag and Ortmann
 348 2007), it is reasonable to assume that the coordination incentives will increase the
 349 coordination rate. Besides, choices are expected to be higher in the coordination
 350 games than in the non-coordination games due to the lack of incentives to undercut
 351 other players' choices.

352 Based on previous research (Crawford 1998; Balliet 2010; Farrell and Rabin
 353 1996; Cooper and Kühn 2016), in the third hypothesis, we assume that our
 354 communication protocol will foster the incentive effects on choices and coordina-
 355 tion level.

356 With respect to the effects of the incentive variables on the communication
 357 process, we expect the following:

358 **Hypothesis 2a** Both strategic complementarity and coordination incentives will
 359 increase the intensity of use of cheap talk.

360 **Hypothesis 2b** Both strategic complementarity and coordination incentives will
 361 positively influence the message values.

362 **Hypothesis 2c** Coordination incentives will increase the faithfulness to messages.
 363 Such a positive effect will be stronger when the coordination incentives are
 364 reinforced by the complementarity incentives.

365 Both complementarity and coordination are expected to increase the communi-
 366 cation rate with an optional protocol. In the baseline game (NON-COMPL NON-
 367 COORD), little or none communication is expected as non-zero messages are not
 368 credible in this constant-sum game. Additionally, full feedback information implies
 369 that any deception will be observed after each round, so that subjects who are
 370 unwilling to deceive or are afraid to be caught cheating will prefer keeping silent in
 371 the environments where messages should be ignored (Gneezy 2005).¹⁰ Instead,
 372 literature shows that communication can be used to achieve higher payoffs and to
 373 coordinate actions. Blume and Ortmann (2007) provide evidence that cheap talk

10FL01 ¹⁰ Not only can a deceitful message have a psychological cost of lying for the sender, but also it can have
 10FL02 the (small) effort of thinking about it. This could reduce the chance of sending deceitful messages when
 10FL03 collusion is not expected.

374 facilitates coordination on the unique efficient equilibrium in Pareto-ranked
 375 coordination games, and Andersson and Wengström (2007) find that, in Bertrand
 376 supergames with cheap talk, subjects communicate frequently, although they have
 377 some difficulties in keeping collusive agreements. Thus, we expect that senders will
 378 submit messages more frequently in those environments where players' interests are
 379 more aligned, in terms of strategic complementarity or coordination.

380 To hypothesise how subjects will use the communication process, we relate the
 381 message content to the incentive variables. Given the natural linkage between
 382 message values and choices, the Hypothesis 2b posits our expectations about the
 383 message content in line with the previous hypotheses on choices. Hence, it is
 384 expected that the message values will be higher under complementarity incentives
 385 in search of higher profits. Likewise, message values will be higher under
 386 coordination incentives to send messages that are credible and useful.

387 To examine subjects' reactions to messages, we introduce Hypothesis 2c. The
 388 theoretical and experimental literature shows that credibility of messages is key to
 389 understand individual responses (Crawford 1998; Farrell and Rabin 1996). In our
 390 design, the credibility is determined by the coordination incentives. Moreover, in
 391 games with Pareto-ranked equilibria, both players have strict interests in being
 392 faithful to efficiency-enhancing messages (Cooper et al. 1992).

393 Finally, we summarise the expected effects of the *actual* communication on
 394 efficiency and coordination in the following two hypotheses:

395 **Hypothesis 3a** Sending pre-play messages will increase profits under strategic
 396 complementarity.

397 **Hypothesis 3b** The positive effect of pre-play messages on the coordination level
 398 will be higher in the coordination games than in the non-coordination games.

399 The efficiency effect is stated in Hypothesis 3a. The experimental literature
 400 shows wide evidence about how communication helps achieve higher profits (for
 401 example, Cooper et al. 1992; Balliet 2010; Fonseca and Normann 2012). It should
 402 be noted that, in our games without strategic complementarity (fixed payoffs), there
 403 is opportunity to get efficiency gains only when players coordinate under
 404 coordination incentives. However, in the games with strategic complementarity
 405 (variable payoffs), we predict that the use of cheap talk will increase players' profits.

406 With regards to the coordination effect of Hypothesis 3b, experimental research
 407 provides strong support for the positive influence of communication on coordination
 408 in games with multiple equilibria. In our context, given the wide strategy space,
 409 messages might predominantly serve as focal-point signalling or reassurance
 410 devices (Crawford, 1998, and Camerer, 2003).

411 3 Results

412 This section is divided into three parts according to the research hypotheses. The
 413 first part will cope with the main effects (initial and accumulated) of the design
 414 variables on both choice behaviour and coordination. The second part will delve

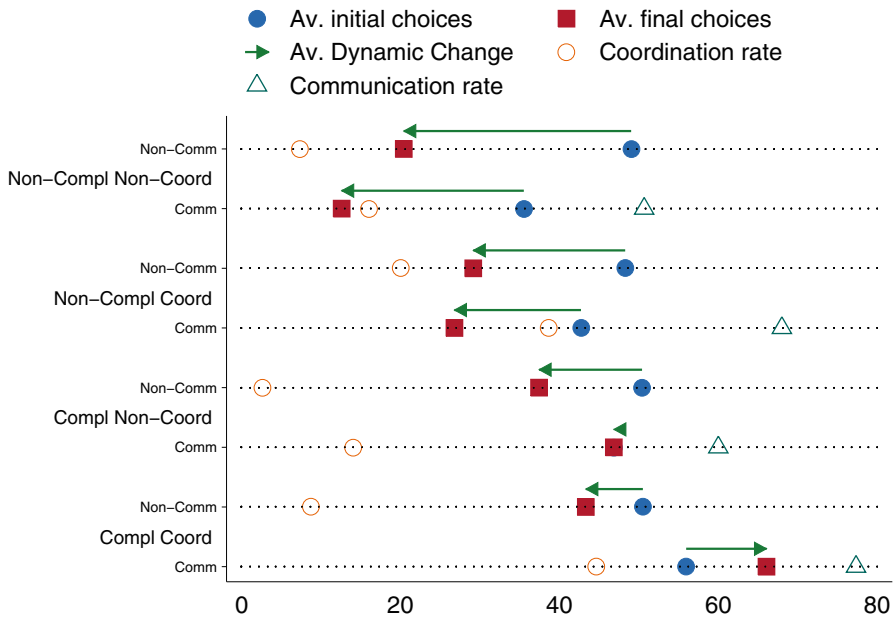


Fig. 1 Basic results by treatment

415 into the actual use of communication, the content and credibility of messages, and
 416 faithfulness. The third part will analyse the effectiveness of communication to
 417 achieve efficiency gains and coordination.

418 **3.1 Design variables and behaviour**

419 Figures 1 and 2 summarise the main experimental results at the design variable
 420 level.¹¹

421 Figure 1 shows some aggregate measures related to choices (first- and last-round
 422 averages with solid symbols and arrows for their temporal variations), coordination,
 423 and communication rates (both with hollow markers) by treatment. The coordina-
 424 tion rate is defined as the proportion of instances in which both players' choices
 425 coincide, while the communication rate represents the ratio of the total number of
 426 emitted messages over the maximum along the six rounds (6 per couple).

427 Figure 2 plots the time paths of the average choices and the coordination rates
 428 across incentive and communication conditions. The figure is divided into four
 429 panels, so that each panel illustrates the marginal effect of one incentive variable
 430 under one communication condition. The two upper graphs vary the complemen-
 431 tarity (left) and coordination (right) incentives when communication is absent, while
 432 the two bottom graphs exhibit such incentive effects when communication is
 433 allowed. For each graph, the red lines (Yes) are used when the studied variable is

11FL01 ¹¹ Further results are presented at the treatment level in section S.2 in the Online Supplementary Material
 11FL02 (see Table S.2 and Figures S.2 and S.3).

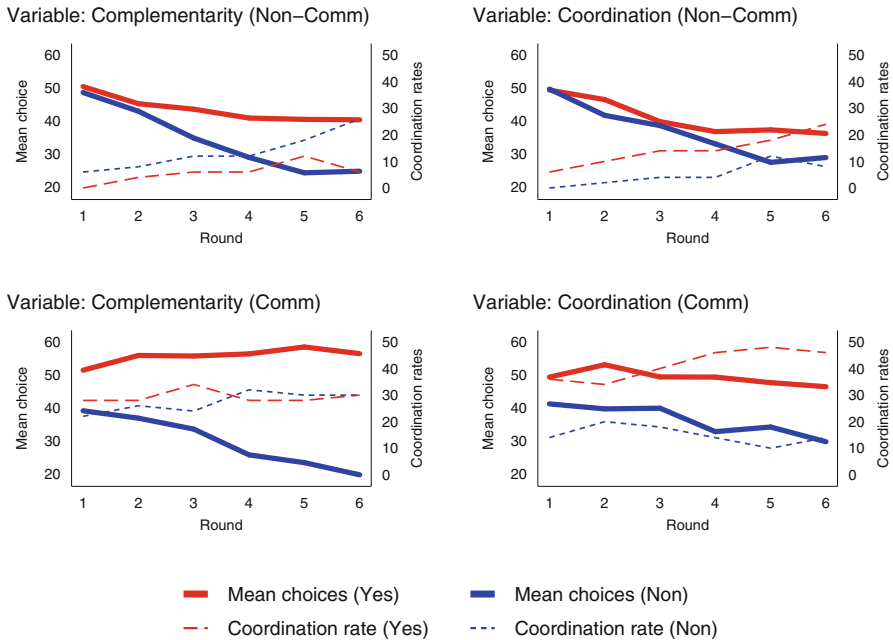


Fig. 2 Dynamics over time (choice and coordination rate) across incentive and communication conditions

434 present, while the blue lines are employed for its absence (Non). For instance, the
 435 thick red line (Yes) on the upper left graph represents the evolution of the average
 436 choices for the COMPL NON-COORD NON-COMM and COMPL COORD NON-COMM
 437 treatments, while the thin blue line (Non) represents the evolution of the
 438 coordination level (on the right scale) for the NON-COMPL NON-COORD NON-COMM
 439 and NON-COMPL COORD NON-COMM treatments.

440 From Figs. 1 and 2, we observe that the first-round choices tend to hover around
 441 the middle of the choice interval in all treatments. Nevertheless, clear directional
 442 effects develop with repetition and communication.¹²

443 Observed behaviour is consistent with the hypotheses we have formulated for the
 444 effects of the incentive structures.

445 *Complementarity* incentives exert a positive impact on choices. While the NON-
 446 COMPL treatments show a remarkably decreasing trend in choices (with and without
 447 communication), the COMPL treatments present either a slight reduction (without
 448 communication) or a noticeable increase in choices (with communication) as Fig. 1
 449 displays. The two left panels of Fig. 2 reinforce these results: the positive effect of
 450 COMPL on choices appears after some repetitions when there is not communication
 451 (top left graph), but, when communication is allowed (bottom left graph), choices
 452 are higher with COMPL than without COMPL along the six rounds. It is worth
 453 emphasising that complementarity does not seem to affect the coordination rate

12FL01 ¹² First-round data seem to indicate that players initially fail to completely comprehend the specific game
 12FL02 they are playing. This can be due to the cumbersome frame used in the instructions.

Table 2 Design variables effects

Variables	(M1) Choices	(M2) Choices	(M3) Coord	(M4) Coord
First choice	0.418*** (0.0433)	0.418*** (0.0433)		
First period coord.			2.552*** (0.340)	2.614*** (0.353)
Period	- 2.575*** (0.403)	- 6.401*** (0.638)	0.136*** (0.0360)	0.235*** (0.0812)
COMPL	14.77*** (2.131)	0.128 (2.166)	- 0.280 (0.220)	0.135 (0.361)
COORD	7.060*** (2.037)	1.758 (2.180)	0.658*** (0.224)	0.162 (0.366)
COMM	5.715*** (2.076)	- 1.119 (2.171)	0.425* (0.234)	1.191*** (0.391)
Period × COMPL		4.185*** (0.729)		- 0.112 (0.0739)
Period × COORD		1.515** (0.729)		0.132* (0.0747)
Period × COMM		1.953*** (0.729)		- 0.198** (0.0774)
Constant	16.19*** (3.126)	29.58*** (2.794)	- 2.794*** (0.299)	- 3.232*** (0.429)
No obs.	2400	2400	1200	1200
Wald chi2	287.2	321.7	90.73	93.25

Results from panel data analysis

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

454 (thin lines); by contrast, the existence (or not) of communication determines
 455 noticeably the *dynamics* of coordination.

456 Comparison between the two right graphs shows that the **coordination** incentives
 457 influence positively on both choices and coordination rates, being the effects on
 458 coordination more salient. In particular, the coordination rates are higher in the
 459 COORD treatments than in the NON-COORD treatments. This positive effect is achieved
 460 through repetition when there is no communication. Yet, with communication,
 461 coordination rates move hastily upwards in the first round with COORD incentives,
 462 and hold a slightly increasing trend from then (see the bottom right graph in Fig. 2).

463 Statistical tests support our previous statements. Table 2 reports the effects of the
 464 design variables on our two behavioural variables: individual choices (Models M1
 465 and M2) and coordination instances (Models M3 and M4).¹³ As explanatory
 466 variables, we include: (1) initial behaviour, measured by the first-period individual

13FL01 ¹³ Models M1 and M2 are panel data regressions where the dependent variable is the individual choice
 13FL02 ([0, 100]). Models M3 and M4 are probit panel data models at the couple level where the dependent
 13FL03 variable is a dummy that takes value 1 if the two members of a couple coordinate in a given period and 0
 13FL04 otherwise. All the models shown in Table 2, and Tables 4 and 5, are estimated with random effects and
 13FL05 standard errors are corrected for clustering at the couple level (when individual data are utilized) due to
 13FL06 the bias introduced by the fixed-matching procedure (see, Liang and Zeger 1986). To control for
 13FL07 individual unobservable characteristics, we include the first-period value of the dependent variable in
 13FL08 each regression. Therefore, to study the treatment effects and control for individual heterogeneity, we
 13FL09 estimate random-effects panel models, rather than fixed-effects, and include initial decisions as
 13FL10 independent variables.

467 choice (*First Choice* in M1–M2) and a dummy that takes value 1 if the couple
 468 coordinates in the first period and 0 otherwise (*First Period Coord.* in M3 and M4);
 469 (2) *Period*, which takes values from 1 to 6, and captures the effects of the dynamics
 470 over time; (3) three dummy design variables (COMPL, COORD, COMM), and (4) in M2
 471 and M4, the interactions with *Period* ($Period \times COMPL$, $Period \times COORD$, and
 472 $Period \times COMM$) to capture the dynamics of the design variables.¹⁴

473 Models M1 and M2 confirm the significant and positive impact of the three
 474 design variables on choices. The introduction of the interaction terms in M2
 475 supports the dynamic intuition behind Fig. 2: the significant effects of the three
 476 design variables develop along time. In addition, it should be noted that initial
 477 choices are highly significant in both models.

478 Models M3 and M4 confirm that coordination increases significantly with
 479 repetition and coordination incentives. Interestingly, M4 gives a new and intriguing
 480 result: while communication has a positive immediate effect (see the coefficient of
 481 COMM), this effect gets reduced with repetition ($Period \times COMM$). In the following
 482 subsection, we will provide an explanation for this phenomenon. First-period
 483 coordination also determines subsequent coordination in both models.

484 Table 3 provides an alternative statistical analysis for the initial and long-term
 485 effects of the design variables. We regress the first- and last-period decision
 486 variables on the two incentive variables, separately for NON-COMM and COMM
 487 treatments.¹⁵ The results support the behavioural patterns observed in Figs. 1 and 2.

488 Without communication (the top models in Table 3), no incentive variable
 489 significantly affects behaviour in the first period (see M5; M7 cannot be estimated
 490 due to the absence of coordination instances). However, in the last period, we find
 491 significant effects of both incentive variables. As expected, COMPL increases choices
 492 (M5), whereas COORD enhances coordination (M7).

493 When communication is allowed (the bottom models in Table 3), the incentive
 494 variables are relevant at the beginning and the end of the experiment. That is, the
 495 positive impact of COMPL on choices is highly significant in both periods (M9 and
 496 M10), being the last-period coefficient (36.77) three times as high as the first-period
 497 coefficient (12.31).¹⁶ Besides, the existence of coordination incentives, COORD,
 498 increases significantly choices in the last period (M10), and the coordination rates in
 499 both periods (Models M11 and M12).

500 In summary, consistent with our expectations about incentives, the experimental
 501 findings show the existence of long-run effects of the two incentive variables when
 502 there is no communication, and of immediate and long-run communication effects.
 503 The results can be summarised as follows:

14FL01 ¹⁴ For individual choices, we also tested for alternative specifications using random-effects two-limit
 14FL02 Tobit models, with the dependent variable doubly censored at 0 and at 100. The estimates are fairly
 14FL03 similar to those of the standard panel data model. We settle on the standard model, because it allows to
 14FL04 control for couple effects.

15FL01 ¹⁵ We estimate linear regression models for choices and probit regression models for coordination. All of
 15FL02 them use data at the couple level (in the case of choices, the average of the two numbers).

16FL01 ¹⁶ SURE tests for the equality of the coefficients of COMPL give significant differences: for instance, for
 16FL02 $\beta_{COMPL}(M9) = \beta_{COMPL}(M10)$, $\chi^2(1) = 13.75$, $p < 0.001$.



Table 3 Immediate and accumulated effects of the incentive variables across communication conditions

Variables	Non-communication treatments			
	Choices		Coordination	
	(M5) First period	(M6) Last period	(M7) First period	(M8) Last period
COMPL	1.798 (3.852)	15.60*** (4.379)	–	– 1.010*** (0.365)
COORD	– 0.324 (3.852)	7.318* (4.379)	–	0.811** (0.352)
Constant	48.81*** (3.336)	21.12*** (3.792)	–	– 1.097*** (0.288)
Observations	100	100	–	100
F-value/LR chi2	0.11	7.74***		13.72***
Variables	Communication treatments			
	Choices		Coordination	
	(M9) First period	(M10) Last period	(M11) First period	(M12) Last period
COMPL	12.31*** (4.309)	36.77*** (5.106)	0.165 (0.281)	– 0.010 (0.277)
COORD	8.131* (4.309)	16.71*** (5.106)	0.715** (0.286)	0.980*** (0.283)
Constant	35.05*** (3.731)	11.34** (4.422)	– 1.159*** (0.259)	– 1.076*** (0.258)
Observations	100	100	100	100
F-value/LR chi2	5.86***	31.28***	6.98**	12.68***

Results from linear and probit regressions (at the couple level)

Standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

504 **Result 1** Behaviour responds to incentives through repetition when communication
 505 is absent. However, communication leads to immediate reaction of behaviour to the
 506 incentive structures and reinforces the incentive effects along time.

507 **Result 2** Choices significantly increase under complementarity and under coordi-
 508 nation incentives. The coordination level is significantly increased under coordi-
 509 nation incentives.

510 These results support Hypotheses 1a, 1b, and 1c.

511 Therefore, communication seems to accelerate reactions to incentives in terms of
 512 choices and coordination. As we will show in the next subsection, such behavioural
 513 dynamics depend on the actual use of cheap talk and the faithfulness to messages.

514 3.2 The use of communication: messages and faithfulness

515 Figure 3 illustrates, just for the COMM treatments, the dynamics of the communi-
 516 cation rates (with blue hollow circles), the mean value of messages (with red solid

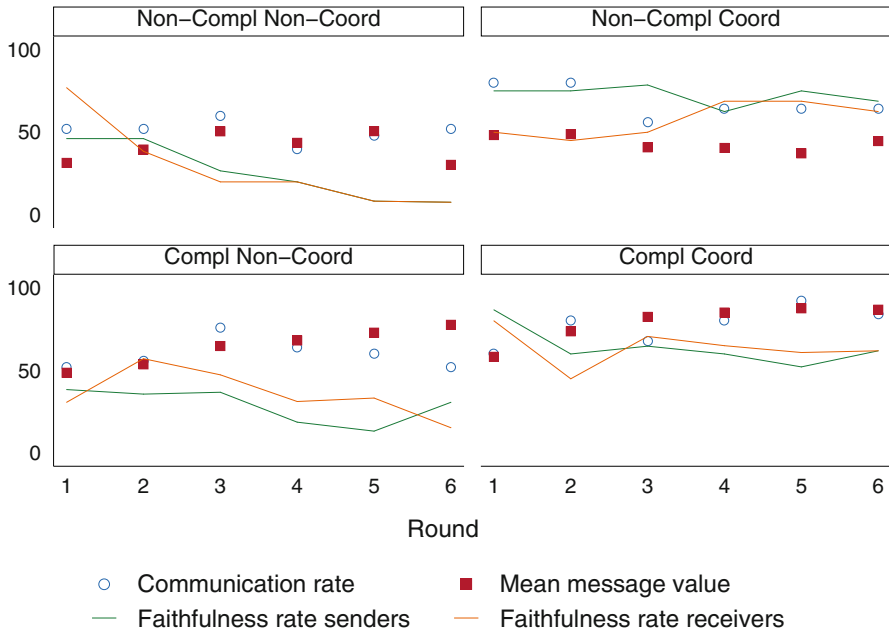


Fig. 3 Communication rates, choices, messages, and faithfulness rates in the communication treatments

517 squares), and the faithfulness rate of senders (green lines) and receivers (orange
 518 lines). In both cases, the faithfulness rates are defined as the proportion of subjects
 519 who choose the same number as the content of the message (when it is emitted).¹⁷

520 Three findings stand out. First, the communication rates are relatively high in all
 521 games (64%, on average).¹⁸ As expected, NON-COMPL NON-COORD has the lowest
 522 communication rate (50.7%), while COMPL COORD has the highest one (77.3%). This
 523 is the only game with a clearly positive trend (an increase of 21 %).

524 Second, the message content is primarily driven by the complementarity
 525 incentives. The message values are noticeably higher in the two COMPL environ-
 526 ments along the six rounds. On average, the message value rises from 53.8 in the
 527 first round to 83.3 in the last one in the COMPL treatments (an increase of 55 %),
 528 while in the NON-COMPL treatments, average messages do not change much along
 529 time (41.7 in the first round and 38.2 in the last one).

530 Third, the faithfulness to messages is chiefly determined by the coordination
 531 incentives (thereby, by the credibility of messages). The faithfulness rates are
 532 noticeably higher in the two COORD environments along the six rounds (71.4% in the
 533 first round and 63.5% in the last, a 11% reduction). By contrast, faithfulness shows a
 534 remarkably decreasing trend in the two NON-COORD environments (from 48 to

17FL01 ¹⁷ In Sect. S.2 of the Online Supplementary Material, we provide additional analysis in Table S.3 and
 17FL02 Figures S.5–S.9.

18FL01 ¹⁸ Recall that the sender of each couple is randomly selected at the beginning of the experiment and
 18FL02 maintains that role along the six periods. Thus, we have 100 senders in total, 25 by treatment. Out of 100
 18FL03 senders, 12 subjects never send a message, and 35 subjects send messages in all periods.

535 15.4%, a 68% reduction). By player role, faithfulness is fairly similar between
 536 senders and receivers. For the whole experiment, the faithfulness rate is 53.3% for
 537 senders and 48.4% for receivers.¹⁹

538 Table 4 reports the determinants of the communication use (M13 and M14), the
 539 message content (M15 and M16), and faithfulness (M17 and M18).²⁰

540 The econometric results confirm the previous picture. On one hand, the
 541 likelihood of sending a message increases significantly in the COMPL treatments,
 542 particularly when the interaction with *Period* is considered (models M13 and M14).
 543 On the second hand, complementarity also influences positively on the message
 544 content, being the dynamic effects particularly significant and increasing (models
 545 M15 and M16). Putting together these two points, subjects increasingly use the
 546 communication device to signal possibilities of higher efficiency gains, as we will
 547 see in the next subsection.

548 Besides, the likelihood of being faithful depends significantly and positively on
 549 COORD incentives and their dynamic interactions (models M17 and M18). Even
 550 though COMPL has no significant direct effects (neither static nor dynamic), we
 551 obtain its indirect effects through message values. Notice that *Sender* is no
 552 significant and *Period* is significantly negative.²¹ We summarise the findings as
 553 follows:

554 **Result 3** The emission of messages increases with the existence of complemen-
 555 tarity incentives. Moreover, complementarity leads to significantly higher messages.

556 **Result 4** Faithfulness is significantly higher with coordination incentives. Both
 557 player roles (sender vs. receiver) exhibit a similar faithful behaviour.

558 These two results partially support Hypotheses 2a, 2b, and 2c.

559 3.3 Effects of actual communication: efficiency and coordination

560 Figure 4 displays, for each game and round, the average profits and coordination
 561 levels when pre-play messages are sent, as the differences with respect to the
 562 comparable levels reached in the NON-COMM treatments.

19FL01 ¹⁹ A two-sided test for equality of proportions gives a result of $z = 0.505$, $p = 0.610$. By treatment, the
 19FL02 only case where there are significant differences between roles is NON-COMPL COORD COMM: 72.5% for
 19FL03 senders vs. 56.9% for receivers, $z = 2.34$, $p = 0.019$.

20FL01 ²⁰ Models M13 and M14 present probit panel models to estimate the probability of submitting a message,
 20FL02 models M15 and M16 give the panel data regressions for the message values submitted by the senders,
 20FL03 and models M17 and M18 estimate the probability of being faithful (sender or receiver) by choosing the
 20FL04 same number as the message value. The new explanatory variables are the following: (1) the initial values
 20FL05 of the corresponding dependent variables (*First Period Message*, *First Period Faith.* and *First Period*
 20FL06 *Mess. Value*); (2) the message content (*Mess. Value*) and its interaction terms with the design variables
 20FL07 ($Value \times COMPL$ and $Value \times COORD$); and (3) the player role (*Sender*), a dummy that takes value 1 for the
 20FL08 sender and 0 for the receiver.

21FL01 ²¹ The decreasing trend in faithfulness can be explained by simply pondering that the repetition of the
 21FL02 game increases the chance of facing unfaithful behaviour of a player's partner, which seems to be a
 21FL03 dealbreaker for subjects. That is, a player is less likely to behave faithfully as the accumulated experience
 21FL04 of unfaithful responses given by her partner increases along time. See Table S.4 in the Online
 21FL05 Supplementary Material where the coefficient of *Past Partner Unfaithfulness* is negative.

Table 4 Determinants of the communication use, the message value, and faithful behaviour in the COMM treatments

Variables	(M13) Message Emission	(M14) Message Emission	(M15) Message Value	(M16) Message Value	(M17) Faithfulness	(M18) Faithfulness
First period message	1.941*** (0.316)	1.977*** (0.328)				
First period faith.					2.326*** (0.361)	2.394*** (0.362)
First period mess. value			0.810*** (0.09)	0.794*** (0.090)		
Mess. value					- 0.011** (0.004)	- 0.016** (0.007)
Value × COMPL						0.019** (0.009)
Value × COORD						- 0.007 (0.008)
Sender					- 0.056 (0.182)	- 0.042 (0.182)
Period	0.007 (0.044)	- 0.143* (0.076)	2.845*** (0.852)	1.095 (1.435)	- 0.189*** (0.067)	- 0.432*** (0.108)
COMPL	0.562** (0.273)	- 0.192 (0.270)	14.02*** (4.422)	- 0.843 (4.000)	0.490 (0.347)	- 0.822 (0.564)
COORD	0.446* (0.268)	0.213 (0.265)	0.802 (4.596)	3.263 (4.269)	1.059*** (0.294)	0.368 (0.529)
Period × COMPL		0.221** (0.089)		4.567*** (1.583)		0.068 (0.124)
Period × COORD		0.0710 (0.086)		- 0.747 (1.608)		0.308** (0.128)
Constant	- 1.089*** (0.283)	- 0.587** (0.289)	2.866 (5.057)	9.018** (4.381)	- 1.065*** (0.328)	- 0.123 (0.444)
Observations	600	600	299	299	598	598
Wald chi2	43.62	43.62	133.3	175.5	52.77	68.12

Results from panel data analyses

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

563 Taking NON-COMPL NON-COORD as the baseline (where the messages are mainly
 564 unhelpful),²² we can observe that introducing complementarity (left bottom graph)
 565 does not change much the effects of messages. Profits are nearly identical to the
 566 NON-COMM case, and the initial increase in coordination also disappears by the end
 567 of the experiment.

22FL01 ²² Notice that, in our baseline game, the winner's payoffs are fixed (50 points, 25 points in the case of a
 22FL02 tie). Hence, in NON-COMPL NON-COORD, the average profits must be exactly 25 points regardless of the
 22FL03 communication condition.

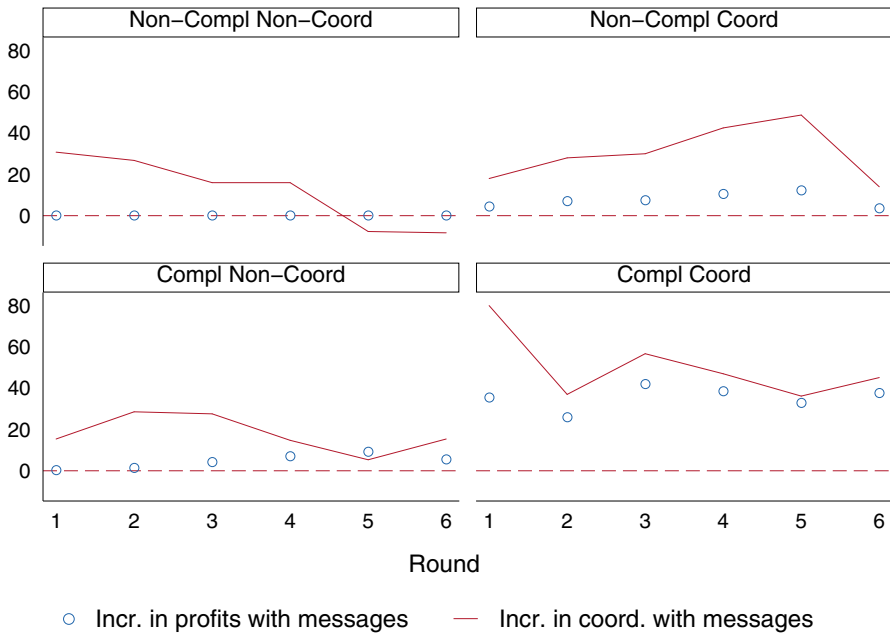


Fig. 4 Differences in profits and coordination between COMM with message and NON-COMM by game

568 Instead, with coordination incentives (top right graph), both profits and
 569 coordination steadily increase along time, but at the last round. This end-of-game
 570 effect is the consequence of that subjects end up coordinating in the NON-COMM
 571 treatment, which reduces the differences with respect to the situation with messages.

572 Finally, when both complementarity and coordination are present (bottom right
 573 graph), the resulting effects of messages on profits and coordination are the greatest
 574 and the most stable along time. In this game, communication seems to confer
 575 players an initial advantage over just repetition which holds along time. This is
 576 consistent with the reassuring role of cheap talk shown by literature.

577 Therefore, our data reveal the decisive impact of pre-play communication on
 578 efficiency and coordination in the COORD environments, particularly in the COMPL
 579 COORD game where there exist incentives to achieve efficiency gains and such
 580 efficiency gains are part of an equilibrium.

581 Table 5 confirms and expands our previous findings.²³

23FL01 ²³ We estimate three pairs of panel data models: (i) for profits at the couple level (models M19 and M20);
 23FL02 (ii) for likelihood of coordinating on any outcome (models M21 and M22); and (iii) for likelihood of
 23FL03 coordinating on equilibrium (models M23 and M24). *Equil. Coord* is a dummy variable that takes value 1
 23FL04 when subjects coordinate at an equilibrium strategy [any number in the COORD games and (0,0) in the
 23FL05 NON-COORD games]. As explanatory variables, we include: (i) the corresponding first-period values of the
 23FL06 dependent variables; (ii) a dummy for the use of pre-play communication, *Message Use*; (iii) two
 23FL07 dummies for the extreme values of messages 0 and 100, *Mess0* and *Mess100*, and (iv) the number of past
 23FL08 instances of unfaithfulness at the couple level, *Past Unfaithfulness*. In the second model of each pair, we
 23FL09 incorporate the interaction terms of the communication variables with COMPL and COORD.

Table 5 Communication determinants of profits and coordination

Variables	(M19) Profits	(M20) Profits	(M21) Coord	(M22) Coord	(M23) Equil. Coord	(M24) Equil. Coord
First period profit	0.722*** (0.062)	0.558*** (0.079)				
First period coord			2.154*** (0.330)	2.046*** (0.329)		
First period Eq_Coord					4.024*** (0.665)	3.058*** (0.621)
Period	0.867*** (0.296)	0.676** (0.276)	0.202*** (0.042)	0.187*** (0.042)	0.335*** (0.058)	0.304*** (0.058)
Message use	6.155*** (2.013)	1.298 (1.772)	1.265*** (0.254)	0.684** (0.341)	1.655*** (0.402)	- 0.558 (0.857)
Mess100	13.32*** (3.781)	- 13.44** (5.940)	- 0.0002 (0.273)	- 1.297 (1.039)	- 0.609 (0.407)	- 7.097 (481.5)
Mess0	1.895 (1.539)	4.551*** (1.415)	0.456* (0.260)	0.559 (0.366)	0.962** (0.382)	1.402** (0.560)
Past unfaithfulness	- 2.423*** (0.742)	- 2.368*** (0.769)	- 0.444*** (0.126)	- 0.453*** (0.129)	- 0.573*** (0.183)	- 0.655*** (0.202)
Message use × COMPL		- 0.962 (2.861)		- 0.102 (0.332)		- 0.096 (0.589)
Message use × COORD		9.349*** (3.445)		0.991*** (0.343)		3.048*** (0.898)
Mess100 × COMPL		25.69*** (5.800)		1.795* (0.967)		2.097 (1.412)
Mess100 × COORD		12.79** (6.512)		- 0.440 (0.601)		4.584 (481.5)
Mess0 × COMPL		- 8.619*** (1.744)		- 1.120** (0.466)		- 1.794** (0.705)
Mess0 × COORD		1.769 (1.766)		0.340 (0.424)		0.137 (0.616)
Constant	3.571** (1.391)	8.116*** (1.723)	- 2.887*** (0.263)	- 2.738*** (0.258)	- 4.607*** (0.514)	- 4.159*** (0.482)
No obs	1200	1200	1200	1200	1200	1200
Wald chi2	361.7	552.4	110.2	118.4	68.84	76.86

Results from panel data analyses

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

582 All estimates have three common features: First, like in the previous models,
 583 initial conditions are relevant to explain subsequent behaviour as the significant
 584 coefficients of the first-period variables indicate. Second, on average, there exists a
 585 significantly positive effect of *Period* on profits and coordination. Third, the prior
 586 experience of unfaithful behaviour reduces significantly profits and coordination
 587 (see the coefficient of *Past Unfaithfulness* in all estimations).

588 From models M19 and M20, we attain that both the use of messages and their
 589 content influence significantly on profits. The significant coefficient of *Message Use*
 590 in M19 confirms the positive effect of sending pre-play messages on profits shown
 591 in Figure S.10 in the Online Supplementary Material. Model M20 adds the insight
 592 that such an effect is primarily significant in the COORD environments ($Mess\ Use \times$
 593 $COORD$, $p < 0.001$). Regarding the message content, we also find: (i) sending
 594 messages with value 100 increases profits, on average (see *Mess100* in M19), being
 595 such impact significantly high with COMPL incentives ($Mess100 \times COMPL$ in M20);
 596 (ii) sending messages with value 0 increases profits under NON-COMPL (see the
 597 coefficient of *Mess0* in Model M20) and reduces profits under COMPL (the sum of the
 598 coefficients *Mess0* and $Mess0 \times COMPL$ in M20 is negative). Recall that, with NON-
 599 COMPL, a message “0” signals either the unique Nash equilibrium in NON-COMPL
 600 NON-COORD or the risk-dominant equilibrium in NON-COMPL COORD. Therefore,
 601 messages “0” are credible and highly effective in terms of profits in the two NON-
 602 COMPL environments.

603 **Result 5** The use of pre-play communication leads to significant efficiency gains.
 604 The emission of messages is highly effective under coordination incentives.
 605 Additionally, the effect of particular messages is dependent on complementarity
 606 incentives: a message “100” significantly increases profits with complementarity; a
 607 message “0” is highly effective with no complementarity.

608 The last four models in Table 5 deal with the determinants of coordination.
 609 Reasonably, sending pre-play messages significantly increases coordination, mainly
 610 under coordination incentives (see $Mess\ Use \times COORD$ in M22 and M24). The
 611 message content is particularly relevant: First, sending messages with value 0
 612 increases significantly coordination. The positive impact of messages “0” is greater
 613 and more significant in the case of coordination on equilibrium (compare the
 614 coefficients of *Mess0* in M21 and M23) and when there is no complementarity (see
 615 the coefficients of *Mess0* and $Mess0 \times COMPL$ in M24). Indeed, 86.6% of messages
 616 “0” are sent in the NON-COMPL COORD game, with a 88.5% faithfulness rate. Second,
 617 sending messages “100” has no significant impact on coordination. This result may
 618 be due to the fact that a message “100” is (strongly) credible only in one of the four
 619 games: COMPL COORD. Indeed, 56% of all messages “100” are submitted in this
 620 game, with a subsequent 81.5% faithfulness rate.

621 **Result 6** The use of pre-play communication significantly increases coordination.
 622 Its effectiveness is higher with coordination incentives. Additionally, the effect of
 623 particular messages is dependent on complementarity incentives: sending messages
 624 “0” significantly promotes coordination with no complementarity.

625 These two results support Hypothesis 3a and 3b.

626 In sum, the experimental evidence from this study confirms the expected
 627 interplay between cheap talk and incentives. Cheap talk exerts *immediate* positive
 628 effects on coordination and choices (see Table 3 and Result 1). Such initial effects
 629 are reinforced by a selective use of communication *along time* (see Table 4 and
 630 Result 3). As a consequence, cheap talk enhances efficiency under complementarity
 631 incentives and coordination under coordination incentives (Table 5 and Results 5

632 and 6). Faithfulness to messages holds along time when messages are credible and
 633 useful. In the next Section, we will discuss in more detail this last result.

634 4 Discussion

635 In this discussion, we elaborate the previous results at the game level (see
 636 Section S.2 in the Online Supplementary Material).

637 The NON-COMPL NON-COORD game: To the best of our knowledge, there are no
 638 previous BCG experiments where subjects communicate with each other before
 639 making their decisions individually. There are some works where players can speak
 640 freely with the other team members to make joint decisions (Kocher and Sutter
 641 2005) or receive the (incentivised) advice of a previous winner of the game (Sbriglia
 642 2008). In both cases, subjects learn faster the rules of the game.

643 Without communication, we obtain a slightly slower convergence towards the
 644 equilibrium than those observed in standard BCG experiments, a low coordination
 645 rate (7.3%) and low equilibrium play (3.3%). This may be due to the bigger strategy
 646 space we use (see Nagel 1998). With communication, the unexpected over-
 647 communication (a 50.7% rate) has two effects: a small increase in equilibrium play
 648 (up to 5.3%) and a noticeable increase in the coordination rate (up to 16%).
 649 Faithfulness is relatively low (27.6%) and markedly decreasing (from 62% in the
 650 first period to 7% in the last period). Message values do not influence on faithful
 651 behaviour (Table S.5). Besides, after reaching coordination, subjects are more likely
 652 to maintain their choices with communication than without communication, above
 653 all, when zero choices are excluded (Tables S.6 and S.7). With communication,
 654 subjects hold the coordinated choices in about 50% of the times, although they
 655 would be better-off if they decreased them.

656 The faithfulness to non-credible messages above zero gives us a hint about the
 657 intrinsic (behavioural) value that subjects assign to being faithful, irrespectively of
 658 the usefulness of the message. Additionally, our findings reveal a methodological
 659 caveat: the introduction of an (a priori) irrelevant feature, like restricted pre-play
 660 communication, in a constant-sum game may contribute to further deviations from
 661 the equilibrium.

662 The NON-COMPL COORD game: We are unaware of previous research on this game.
 663 Yet, the design presents some incentive features which are similar to those of
 664 matching games (equilibria are not Pareto-ranked) and others of weak-link games
 665 (out-of-equilibrium payoffs are asymmetric, the strategic uncertainty level is high).
 666 Evidence shows that communication helps to create focal points in matching games
 667 (Van Huyck et al. 1992), and promotes the frequency of efficient equilibrium play
 668 in weak-link games (Blume and Ortmann 2007).

669 Lacking communication, the coordination rate is lower than expected (20%).
 670 When subjects coordinate their choices, they stick to them so strongly that they keep
 671 the chosen numbers 85.7% of times (Table S.6). As a result, it is not surprising that
 672 the subsequent coordination rate is much higher than the overall rate in the
 673 treatment (61.2% vs. 21.6%, see Table S.8). When communication is allowed,
 674 communication is frequently used (68% of instances) and the coordination rate rises



675 noticeably (38.7%). The faithfulness rate is quite high but asymmetric between
 676 player roles (72% senders and 57% receivers). Unsurprisingly, faithful behaviour
 677 decreases in message values in the sense that subjects behave more faithfully with
 678 messages closer to the risk-dominant equilibrium, zero.

679 Behaviour observed in this game highlights two features. First, without
 680 communication, subjects are able to reach coordination through trial-and-error
 681 process. Second, and more interesting, despite the fact that non-zero messages are
 682 weakly credible, the faithfulness rate is the highest of all treatments. This is
 683 particularly true for those messages indicating less riskiness as the risk-dominance
 684 principle predicts.

685 The COMPL NON-COORD game: Related studies show that the ability of players to
 686 achieve high-payoff outcomes is sensitive to the nature of signals (see the survey in
 687 Balliet 2010). For example, Duffy and Feltovich (2002) report that the observation
 688 of others' actions is more effective than cheap talk in a prisoners' dilemma.
 689 Likewise, in a public goods game, Bochet and Putterman (2009) show that promises
 690 are more effective than announced contributions, and some punishment is needed to
 691 increase significantly contributions. Andersson and Wengström (2007) also find that
 692 collusion is not sustainable when communication is costless in an infinitely repeated
 693 Bertrand duopoly.

694 Our findings are consistent with previous research. Without communication, the
 695 coordination level is very low (four instances in total) and choices decrease along
 696 time although at a slower pace compared to NON-COORD NON-COORD NON-COORD.
 697 Profits reach the smallest level of all treatments (14 points, on average). Even
 698 though communication is frequently used (60%), its beneficial effects on
 699 coordination (a 14% rate, the lowest of all communication treatments) and profits
 700 (18 points, on average) are negligible. As expected, the overall faithfulness rate is
 701 relatively small (28.9% for senders and 36.7% for receivers), and significantly lower
 702 than in NON-COORD COORD COMM (see Figure S.12). It is worth noting that, after a
 703 coordination round, choice dispersion increases considerably in the subsequent
 704 round (Table S.6) and, thereby, further opportunities to coordinate fade away
 705 (Table S.8).

706 Despite the limited effectiveness of cheap talk in efficiency and coordination,
 707 participants respond adequately to the incentive structure. That is, subjects seem to
 708 understand the advantages of undercutting their partners' choices (in the short run)
 709 and of signalling higher profits (in the long run). As a consequence, the effects of
 710 communication on faithfulness are as insignificant as in NON-COORD NON-COORD.

711 The COMPL COORD game: In the experimental literature, distinct versions of this
 712 game have been extensively studied (see Van Huyck et al, 1990 and Cooper et al,
 713 1990 for some seminal works). Without communication, coordination failure on the
 714 efficient equilibrium is a frequent result (Devetag and Ortmann 2007). However, as
 715 emphasised by Crawford (1998), communication plays an important reassurance
 716 role in stag-hunt games, because it reduces strategic uncertainty about each other's
 717 decisions. For instance, Cooper et al. (1992), Charness (2000), and Duffy and
 718 Feltovich (2002) find a higher frequency of coordination on the payoff-dominant
 719 equilibrium with (one-way) communication compared with the non-communication
 720 baseline.

721 The experimental data show the beneficial effects of cheap talk in our highly
 722 uncertain environment. When communication is not allowed, behaviour shows a
 723 very low coordination level (8.7%) and low average profits (22 points). As in the
 724 other coordination game, NON-COMPL COORD, subjects primarily maintain their
 725 choices after reaching coordination (80% of the instances; 66.% excluding choices
 726 of 100), which leads to a higher coordination rate (44.7%). On the contrary, those
 727 attempts to get higher profits moving from a coordination outcome to another with
 728 higher choices result to be mainly unsuccessful (immediate coordination rate of 3.3
 729 %). With communication, the picture is completely the opposite: a high
 730 coordination rate (44.7%) and prominent profits (44 points). Faithfulness is quite
 731 notable and symmetric between player roles (62.9%). Moreover, faithfulness
 732 depends significantly on message values. For instance, the faithfulness rate is 81.6%
 733 for message “100”, while it declines to 47.4% for the second most used message,
 734 50. It is worth mentioning that, after reaching coordination, the most frequent action
 735 is to increase choices (71.4% if 100 choices are excluded, Table S.7), which results
 736 in a relatively high successful coordination (35.2%, Table S.8).

737 Thus, adding further complexity to a standard stag-hunt game has not impeded an
 738 effective use of communication.

739 5 Concluding remarks

740 In this paper, we have developed a systematic approach to compare the
 741 effectiveness of pre-play cheap talk in distinct games of complete information.
 742 With that aim, we have proposed and tested experimentally a general game which
 743 encompasses four different incentive structures, derived from the interactions of
 744 strategic complementarity and coordination incentives. As communication protocol,
 745 a voluntary one-way numerical recommendation for choice has been implemented.
 746 Each game is repeated for six rounds with fixed partners.

747 Despite the complexity of strategic environments and the abstract presentation,
 748 subjects behave gradually according to the incentive structures. This process
 749 accelerates noticeably when communication is allowed. Messages clearly help to
 750 promote efficiency under complementarity incentives, and coordination level under
 751 coordination incentives. Credibility of messages as a general theoretical property is
 752 a fundamental prerequisite for communication to exert its beneficial effects. Based
 753 on our incentives design, credibility is determined by the coordination incentives
 754 and, thereby, faithfulness to messages is higher in the coordination games.
 755 Furthermore, subjects are more faithful to those messages which are consistent with
 756 the equilibrium selection criterion of each game: risk dominance or Pareto
 757 dominance (usefulness of messages).

758 The use of a restricted communication protocol in repeated settings has led to
 759 novel insights which may serve as the basis for future research:

760 First, one-way communication is often found in the literature as weaker than
 761 other protocols. Limited communication cannot match the richness of free-
 762 communication data, but the data control and the ability to identify dynamically

763 occurring behaviour patterns make it a valuable tool for understanding the
764 relationship between communication and incentives.

765 Second, the large message space has allowed to identify time-varying profiles of
766 messages that evolve according to the theoretical properties of the games. In the
767 coordination games, cheap talk use is progressively mediated by the equilibria
768 selection criteria. That is, the content of messages and faithfulness respond
769 gradually to the attraction basins created by the dominance criteria. A small strategy
770 space would have blurred such positive evolution. Our research suggests that a
771 further understanding of the conditions which determine the dynamics of messages
772 and actions in games of complete information is needed.

773 **Acknowledgements** The authors gratefully acknowledge the support of the editor and the valuable reports
774 of two anonymous referees which have considerably improved the final version of the paper. We also
775 thank Peter Moffatt, Subhasish Modak Chowdhury, Anders Poulsen, Enrique Fatás, and Miguel Angel
776 Meléndez-Jiménez for helpful comments and suggestions. This research benefited from financial support
777 supplied by the Spanish Ministry of Science (grants RTI2018-097620-B-I00 and MEC-ECO2014-
778 52345P) and the Junta de Andalucía (Grant SEJ-08065).

779 **Author Contributions** Both authors have contributed equally to this paper.

780 **Data Availability Statement** The dataset and the raw zTree files are available from the authors upon
781 request.
782

783 Compliance with ethical standards

784 **Conflict of interest** The authors declare no competing interest.

785 **Code availability** The Stata code used to generate the results is available from the authors upon request.
786

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