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RESEARCH ARTICLE

Coordination via correlation: an experimental study

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Abstract We report on an experiment exploring whether and how subjects may learn to use a correlation device to coordinate on a correlated equilibrium of the Battle of the Sexes game which Pareto dominates the mixed-strategy Nash equilibrium of that game. We consider a direct correlation device with messages phrased in terms of players' actions as well as an indirect device with a priori meaningless messages. According to the revelation principle, it does not matter whether the correlation device is direct or indirect so long as it implements a correlated equilibrium. However, we find that subjects had an easier time coordinating on the efficient correlated equilibrium with a direct rather than an indirect device. Nevertheless, subjects were able to learn to use the indirect device to better coordinate their play. We further find that, when paired

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with a fixed partner, subjects utilized history-contingent strategies (e.g., “alternation”) as a coordinating device and were more likely to ignore the correlation device in this setting; the fixed-matching protocol can thus serve as a substitute for a correlation device in achieving an efficient coordination outcome.

Keywords Alternation strategy · Battle of the sexes game · Coordination · Correlated equilibrium · Laboratory experiment · Revelation principle

JEL Classification C72 · C73 · D83

1 Introduction

Consider the simple, two-player Battle of the Sexes game shown in Fig. 1. This game has two pure strategy Nash equilibria: one where both players play X with a payoff to Player 1 (2) of 9 (3) and another where both players play Y with a payoff to Player 1 (2) of 3 (9). There further exists a unique mixed-strategy Nash equilibrium where Player 1 (2) plays X with probability $\frac{3}{4}$ $\left(\frac{1}{4}\right)$, yielding each player an expected payoff of $\frac{9}{4}$. This multiplicity of Nash equilibria qualifies the Battle of the Sexes game as a “coordination game”; it is one of the simplest, classic games used to study issues of coordination and equilibrium selection.

These Nash equilibria are all found under the standard assumption in non-cooperative game theory that each player’s strategies are probabilistically independent of the strategies played by others. Relaxing this assumption by allowing players’ strategies to be correlated by means of an external correlation device results in a larger set of equilibrium possibilities known as correlated equilibria (Aumann 1974, 1987), of which the Nash equilibria are special cases. This enlargement in the set of equilibria brings with it the possibility of Pareto improvements over the Nash equilibria, but as it generates additional equilibria, it also exacerbates the coordination problem. In this paper, we use the Battle of the Sexes game in Fig. 1 to experimentally study whether and how subjects may learn to use a correlation device to coordinate their play in this game. Our focus is on the nature of a correlation device’s messages, investigating how that may affect the role of the correlated messages as coordination devices.

For a strategic-form game, a correlated equilibrium can be presented as a Nash equilibrium of the game extended to include a correlation device (Myerson 1994). The device sends private, correlated messages to players according to a known distribution and, a Nash equilibrium of the extended game exists (correspondingly a correlated equilibrium of the original game exists) if it is a best response for the play-

		Player 2 X	Player 2 Y
Player 1	X	9, 3	0, 0
	Y	0, 0	3, 9

Fig. 1 Battle of the Sexes Game

ers to condition their actions on the messages received. In offering this alternative perspective for Aumann's (1974) notion of correlated equilibrium, Myerson (1994) applied his famous revelation principle (Myerson 1982): Despite the fact that a correlated equilibrium can be implemented using a myriad of different correlation devices, it is without loss of generality to focus on a direct device where the correlated messages are labeled in terms of the players' actions.[1] The revelation principle implies that the nature of the messages is irrelevant so long as all the players use the device in the same manner. Using the Battle of the Sexes game, we investigate the extent to which this theoretical insight is supported by laboratory behavior.

We use a 3×2 experimental design, where the first treatment variable concerns the correlation device. For this variable, we consider three environments (treatments): one with a direct device, one with an indirect device, and a third control environment without any device. For the direct device, the two direct message profiles, (X, X) and (Y, Y), are generated and sent to subjects with equal probabilities. For the indirect device, the two messages are expressed in symbols that have no immediate association with the action labels in the game; the two message profiles, (@, @) and (#, #), are generated and sent to subjects with equal probabilities just as in the direct-device treatments. The probability distributions of the messages of both devices, which are known to the players and constitute a correlated equilibrium of the Battle of the Sexes game, make our devices public devices in that after receiving his own message, a player knows what message his opponent has received. Playing the corresponding correlated equilibrium generates an expected payoff of 6 to each player, providing the best "fair" outcome and representing a Pareto improvement over the mixed-strategy Nash equilibrium of the game.

Our second treatment variable is the protocol by which players are repeatedly matched into pairs to play the Battle of the Sexes game. We consider two matching protocols: random and fixed. Under random matchings, each repetition of the game is akin to a one-shot encounter; under fixed matchings, subjects may have a better opportunity to learn how to use the correlation device or to make use of other, history-contingent strategies.

The theoretical equivalence of the direct and indirect correlation devices rests on the premise that in equilibrium players perfectly understand the realizations of the device and are able to establish a common mapping from these realizations to the action set of the game. Departing from the theoretical ideal, this mapping might not be immediately apparent to subjects, especially for an indirect device. Such a possibility raises a secondary coordination problem with respect to how the correlated messages should be interpreted and used.

We indeed find that subjects had an easier time coordinating on the efficient correlated equilibrium when the device was direct rather than indirect, especially when they were randomly matched to play the game. While the secondary coordination problem deprives us of an empirical equivalence between the direct and indirect devices, we find that subjects were able to learn to develop a common mapping from the symbolic

[1] More precisely, Myerson (1994) provides a mediator interpretation for the direct correlation device in which the direct messages represent the mediator's non-binding recommendations as to the actions the players should take.

messages of the indirect device to the action space of the Battle of the Sexes game. Furthermore, there was more variety in these mappings under the indirect device than under the direct device: When an endogenous mapping could be established, message X (Y) of the direct device was always associated with action X (Y), but for the indirect device we observed different mappings in different groups of subjects.

We also find that, under the shared common history made available by our fixed-matching protocol, subjects often utilized history-contingent strategies ("alternations" or "turn-takings") as a means of coordination, ignoring the correlation device altogether, regardless of whether it was direct or indirect. Such alternation strategies are deterministic (as opposed to the random play resulting from following the correlation devices) and yield the same expected payoff (6) as does playing according to the correlated equilibrium. Thus, our findings suggest that history-contingent strategies made feasible by a fixed-matching protocol may serve as a substitute for a correlation device, direct or indirect, in achieving a more efficient coordination outcome.

1.1 Related literature

Aumann (1974) was the first to relax the standard assumption of non-cooperative game theory that players' strategies are probabilistically independent of one another. He provided the insight that solutions to non-cooperative (as opposed to cooperative) games are characterized by the self-enforcing property of the solution concept and do not require probabilistic independence of strategies. Furthermore, he noted that allowing for correlation in players' strategies should be regarded as rather natural, e.g., players might simply communicate their intentions to one another.[2] Aumann (1974) showed how, if players could condition their strategies on an external random device, the resulting set of self-enforcing "correlated equilibria" could lie outside the convex hull of Nash equilibria and in certain cases Pareto dominate the probabilistically independent Nash equilibria of the same game. The equal probability play of the strategy profiles (X, X) and (Y, Y) in our Battle of the Sexes game is one such example of a Pareto-improving correlated equilibrium (over the mixed-strategy Nash equilibrium of the game).

Our study is inspired by Myerson's (1985, 1994) mediated-play version of correlated equilibria. His formulation, which was further used by Forges (1986) and Mertens (1994) among others, posits that without loss of generality one can assume that the correlation device takes the form of a third-party mediator who operates the device and makes private, non-binding recommendations about players' actions. Our direct correlation device is essentially a mediator.

Prisbrey (1992) was the first to report experimental evidence on subjects coordinating on efficient, alternation strategies in coordination games played under a fixed-matching protocol. He did not, however, allow or study the role of an external correlation device.[3] The first set of experiments involving recommended play in

[2] On the naturalness of correlated strategies, Myerson also has quipped: "If there is intelligent life on other planets, in a majority of them, they would have discovered correlated equilibrium before Nash equilibrium."

[3] Recent papers that have studied turn-takings include Lau and Mui (2008, 2012) and Cason et al. (2013).

non-cooperative games were conducted by Huyck et al. (1992), Brandts and Macleod (1995), and Seely et al. (2005). These studies examined whether subjects could follow non-binding public recommendations about what actions to take. The difference with our study is that their recommendations were always direct (specifying an action for subjects to play) and did not make use of any random device, i.e., the probability distributions of the recommendations were unknown to subjects and not the focus of interest. Instead, their focus was on whether public recommendations (chosen by the experimenter) would by themselves affect equilibrium selection in coordination games. Cooper et al. (1989) also used the Battle of the Sexes game as the experimental game to study pre-play communication, but their focus was on the effect of communication for equilibrium selection and did not introduce any random device for correlated equilibrium play.

Later studies by Cason and Sharma (2007), Duffy and Feltovich (2010), and Bone et al. (2013) all attempted to use commonly known probability distributions to implement different correlated equilibria in 2×2 coordination games. A major finding from these studies is that the experimenter's recommendations are followed but only imperfectly so, and they are followed decidedly less often when the probability distribution of the recommended actions does not comprise a correlated equilibrium or if the recommended correlated equilibrium is payoff-dominated by some other equilibrium of the game. A common feature of these experiments is that the recommendations are always *direct*, phrased in terms of the players' actions. By contrast, we are interested in the case where the external correlation device need not provide direct recommendations about which actions to play so that the mapping from the device's realizations to the players' action choices has to be learned.

Based on the theoretical studies by Crawford and Haller (1990) and Blume (2000), Blume and Gneezy (2000) experimentally investigated learning in repeated coordination games where there was no a priori common-knowledge description with respect to players' roles and actions. Our exploration of how subjects may learn to develop mappings from a priori meaningless symbolic messages to actions can also be considered as an attempt to study learning in the absence of a common-knowledge description of the environment. Our study also addresses the effect of matching protocols on learning; for a recent paper on this topic (see Feltovich and Oda 2014).

Experiments by Blume at al. (1998, 2001) and Weber and Camerer (2003) have also examined the learning of mappings from messages to actions, but these studies were conducted in the context of sender–receiver games with incomplete information not involving any external random device.[4] Selten and Warglien (2007) also used a sender–receiver setup to investigate the effects of costs and benefits of linguistic communication on the emergence of a common code as a coordination device.

Finally, we note that there is a related literature on sunspot equilibria in dynamic macroeconomics models where agents use realizations of non-fundamental sunspot variables as a coordination mechanism for selecting among multiple dynamic equilibrium paths (see, e.g., Azariadis 1981; Cass and Shell 1983). Experimental testings of

[4] There is also a theoretical literature that studies the evolution of meaning in pre-play communication with messages sent by players. See, for example, Kim and Sobel (1995) and a more recent development by Demichelis and Weibull (2008).

the notion of sunspot equilibria (e.g., Marimon et al. 1993; Duffy and Fisher 2005) have revealed that the extent to which subjects coordinate on sunspot equilibria depends on whether it is incentive compatible to do so and whether there is a clear precedent or mapping from realizations of the sunspot variable to actions; if there is no clear precedent or mapping, the realizations of the sunspot variable are ignored. In this paper, we study a simpler environment than that used in the sunspot experimental literature—a game rather than a market—but our question is similar to the sunspot literature question as to whether extrinsic (non-fundamental) uncertainty—the realizations of an external correlation device—can facilitate coordination.

2 Direct and indirect correlation devices

The Battle of the Sexes game is one of the simplest coordination games with two pure strategy Nash equilibria that are perfectly symmetric. The fact that it is unclear which equilibrium players should coordinate on provides a strong role for a random correlation device to act as an external aid for playing a correlated equilibrium, making the game shown in Fig. 1, which we henceforth denote by G, an ideal game for our purpose.[5]

A correlated equilibrium of a strategic-form game, as originally formulated by Aumann (1974), can be presented as a Nash equilibrium of the game extended to include a correlation device (Myerson 1994). To precisely articulate the concept, which forms the core of our experimental inquiry, we formally define a correlation device and the extended game of G as follows:[6]

Definition 1 For the Battle of the Sexes game, G,

(i) a correlation device is a triple, $\Phi = (M_1, M_2, \pi)$, where M_i, $i = 1, 2$, is a finite set of messages for Player i and π is a probability distribution over $M = M_1 \times M_2$; Φ selects a message profile $m = (m_1, m_2) \in M$ according to π and privately sends m_i to Player i;

(ii) the extended game of G, G_Φ, is a game where Φ selects and sends private messages to the players, after which the players play G; a pure strategy of Player i in G_Φ is a map σ_i: $M_i \rightarrow S_i = \{\text{X, Y}\}$ and the corresponding ex ante expected payoff is given by $u_i(\sigma_1, \sigma_2) = \sum_{m \in M} \pi(m) u_i(\sigma_1(m_1), \sigma_2(m_2))$.

A correlation device as defined in Definition 1(i) is generic in the sense that its message space can be anything. One set of our experimental treatments consider a specialized device, which we refer to as a *direct correlation device*, defined as follows:

Definition 2 For the Battle of the Sexes game, G, a direct correlation device, Φ_d, is a correlation device where $M_i = S_i = \{\text{X, Y}\}$, $i = 1, 2$. For such a device, π also denotes the probability distribution over $S = S_1 \times S_2$. Any device that is not direct is an indirect device.

[5] Refer to the subjects' payment procedure in Sect. 3.3, in particular footnote 14, for why we choose the particular payoff numbers in this game.

[6] We are grateful to an anonymous referee who suggested a general variant of these definitions (including Definitions 2 and 3 below), which we adapted to our game.

The use of an indirect correlation device comprises another set of treatments in our study. The particular indirect device adopted in our design is $\Phi_{(@,\#)}$, where $M_1 = M_2 = \{@, \#\}$.

We proceed to define the correlated equilibria:

Definition 3 For the Battle of the Sexes game, G,

(i) a correlated equilibrium is a pair $(\Phi, (\sigma_1, \sigma_2))$, where the strategy profile (σ_1, σ_2) is a Nash equilibrium of the extended game G_Φ;

(ii) a direct correlated equilibrium, denoted by π, is a correlated equilibrium where $M_i = S_i$ and σ_i is an identity map, $i = 1, 2$; the corresponding ex ante expected payoff to Player i is given by $\sum_{s \in S} \pi(s) u_i(s)$; any correlated equilibrium that is not direct is an indirect correlated equilibrium.

Any outcome of a game that can be achieved by an indirect correlated equilibrium can also be achieved by a direct correlated equilibrium. It is therefore without loss of generality to focus on direct correlation device, or what Myerson (1985, 1994) calls a mediator. This is a version of his famous revelation principle (Myerson 1982), one that is applied to correlated equilibria in strategic-form games (Myerson 1994).[7]

In our context, the above means that, so long as the corresponding sets of equilibrium outcomes are the same, there is no difference between using Φ_d or using $\Phi_{(@,\#)}$. The objective of our experimental inquiry is to investigate the extent to which this theoretical insight is supported by laboratory behavior. It is straightforward to verify that π is a direct correlated equilibrium of G if and only if the following incentive constraints are satisfied: $3\pi(\mathrm{X}, \mathrm{X}) \geq \pi(\mathrm{X}, \mathrm{Y})$, $\pi(\mathrm{Y}, \mathrm{Y}) \geq 3\pi(\mathrm{Y}, \mathrm{X})$, $\pi(\mathrm{X}, \mathrm{X}) \geq 3\pi(\mathrm{Y}, \mathrm{X})$, and $3\pi(\mathrm{Y}, \mathrm{Y}) \geq \pi(\mathrm{X}, \mathrm{Y})$. Among the π's that satisfy these constraints, our experimental design adopts $\pi(\mathrm{X}, \mathrm{X}) = \pi(\mathrm{Y}, \mathrm{Y}) = \frac{1}{2}$, a direct correlated equilibrium with the best "fair" outcome, providing each player with an expected payoff of 6. This equilibrium also brings a Pareto improvement over the unique mixed-strategy Nash equilibrium, which provides each player with an expected payoff of only $\frac{9}{4}$.

Two remarks about the limitations of our experimental design are in order. First, we restrict the message space of our indirect correlation device to just two messages, which facilitates a comparison with the direct-device treatments and makes it as easy as possible for subjects to learn a mapping from the symbolic messages to the binary actions. However, a more general test of the revelation principle would allow the indirect device to have a larger message space. Second, in our design, there is never any uncertainty about the message that a player's opponent receives: The probability distribution we adopt for our correlation devices is such that both players always receive the same, randomly drawn messages. This avoids the burden on subjects to apply Bayesian updating (Aumann 1987), again in the interest of making coordination on a correlated equilibrium as simple as possible. The use of such a "public device," however, means that our study should be considered as a first step to understand the empirical properties of indirect correlation devices.

[7] In Myerson's own words: "any equilibrium of any communication game that can be generated from a strategic-form game Γ by adding a system for preplay communication [e.g., a strategic-form game extended with a correlation device] must be equivalent to a correlated equilibrium satisfying the strategic incentive constraints [for the players to follow the mediator's recommendations]. This fact is known as the *revelation principle* for strategic-form games."

Table 1 Experimental treatments

Correlation device	Matching protocol	
	Stranger (random matching)	Partner (fixed matching)
Direct	*Dir-random*	*Dir-fixed*
Indirect	*Ind-random*	*Ind-fixed*
None	*None-random*	*None-fixed*

3 Experimental implementation

3.1 Design and treatments

Our experiment features a 3×2 treatment design (Table 1). The first treatment variable concerns the correlation device provided for the Battle of the Sexes game, whether it is direct (*Dir*), indirect (*Ind*), or, for control purpose, nonexistent (*None*). As discussed above, we use the direct device Φ_d with $\pi(\mathrm{X}, \mathrm{X}) = \pi(\mathrm{Y}, \mathrm{Y}) = \frac{1}{2}$. Correspondingly, the indirect device is $\Phi_{(@,\#)}$ with $\pi(@, @) = \pi(\#, \#) = \frac{1}{2}$.

As noted above, we chose these devices out of simplicity and based on incentive considerations.[8] We wish to provide an environment that is conducive for subjects to condition their decisions on the correlation device so that we can focus on the question of how they make use of those devices. Our devices, which are like a publicly observed coin toss, present a simple and intuitive environment for subjects. The fact that the correlation devices can be used to achieve the best "fair" outcome in a correlated equilibrium should also provide strong incentives for subjects to use the devices. While the correlated equilibria we study are not the most general (e.g., the size of the message space of both devices is small and the same, and there is no private information involved that calls for Bayesian updating), our design serves as a first step to study the problem: If subjects are unable to condition on a correlation device in this simple and highly incentivized setting, it seems unlikely that they would be able to do so in settings with more complicated correlation devices and/or a less strongly incentivized environment.[9]

Our second treatment variable concerns the manner in which subjects are matched to play the stage game. Under our random-matching protocol (*Random*), subjects are randomly matched with a "stranger" in each repetition of the stage game. Under our fixed-matching protocol (*Fixed*), subjects are in fixed pairs, repeatedly playing all rounds of the stage game with the same "partner."

Our inclusion of the matching protocols into the treatment variables is exploratory and based on experimental (rather than theoretical) consideration. The exploration is,

[8] For the indirect device, we use messages @ and # as these are universally common (ASCII), printable, one-character symbols (allowing others to replicate our design) that do not have any immediate association with the action labels X and Y.

[9] In addition, the fair outcome makes our design less susceptible to any other regarding preferences for fairness or altruism that subjects may bring into the laboratory. It also has the theoretical appeal of surviving the axiomatic refinements proposed by Prisbrey (1992).

however, informed by two a priori plausible conjectures. First, in terms of whether the correlation device will be used at all, random matchings may outperform fixed matchings. Fixed matchings and correlation devices may serve as *substitutes* for one another: With a fixed partner, it is possible for subjects to use their shared common history to coordinate without any need to rely on the correlation device. On the other hand, under random matchings, the absence of such a common history may greatly enhance the role played by a correlation device. Second, conditional on the correlation device being used, fixed matchings may outperform random matchings in terms of how well the device is used, especially for the indirect device. It may take substantial learning for subjects to establish a common mapping from the symbolic messages to the actions; having a fixed partner may facilitate and expedite such learning.[10]

3.2 Hypotheses

Our experimental hypotheses concern the relative ease (or difficulty) with which subjects coordinate their actions in the Battle of the Sexes game under the two different correlation devices or the absence of any, interacted with the two different matching protocols. The main yardstick we use to evaluate our hypotheses is the frequency of coordination on a positive payoff outcome [i.e., the play of either (X, X) or (Y, Y)], which is denoted by $F(T)$ for treatment T.

The revelation principle provides a theoretical basis for our main (null) hypothesis that there is no difference in behavior under the direct and indirect correlation devices:

Hypothesis 1 The Revelation Principle: For a given matching protocol, the frequencies of coordination are the same under the direct correlation device as under the indirect device:

(i) $F(Dir\text{-}Random) = F(Ind\text{-}Random)$ and
(ii) $F(Dir\text{-}Fixed) = F(Ind\text{-}Fixed)$.

Against this null hypothesis, we test an alternative hypothesis that the nature of the correlation device matters for successful coordination on the correlated equilibrium. As discussed above, this alternative hypothesis is based on the behavioral conjecture that, relative to the direct device, subjects under the indirect device may require longer interactions to learn and establish a common mapping from the message space to the action space. For expositional convenience, we also include in this alternative hypothesis the comparisons with the control treatments with no devices. We hypothesize that subjects will attempt to use a correlation device (direct or indirect) to coordinate on the correlated equilibrium, achieving higher payoffs relative to the control settings, as our design provides a simple and strongly incentivized environment for subjects to learn to follow the device. In particular, we hypothesize that some common mapping can be learned even under the indirect device. However, this learning is most likely to

[10] Duffy and Ochs (2009) demonstrated the importance of fixed matchings relative to random matchings for coordination on Pareto-superior outcomes in an indefinitely repeated Prisoner's Dilemma game without any correlation device.

occur under the fixed-matching protocol, and thus under the random-matching protocol, we do not expect that the introduction of an indirect device yields any significant improvement in coordination relative to the case of no device. Summarily, we have the following:

Alternative to Hypothesis 1 The direct correlation device is more conducive to coordination than is the indirect device; the existence of a correlation device is conducive to greater coordination than in the case of no device:

(i) $F(Dir\text{-}Random) > F(Ind\text{-}Random) \approx F(None\text{-}Random)$ and
(ii) $F(Dir\text{-}Fixed) > F(Ind\text{-}Fixed) > F(None\text{-}Fixed)$.

Our next hypothesis concerns the effect of the matching protocols, our second treatment variable. This hypothesis is based on the experimental consideration that fixed matchings should outperform random matchings in facilitating learning and expediting convergence to an equilibrium, be it the correlated equilibrium (*Dir* and *Ind*) or the standard Nash equilibrium (*None*):

Hypothesis 2 The fixed-matching protocol is more conducive to coordination than is the random-matching protocol:

(i) $F(Dir\text{-}Fixed) > F(Dir\text{-}Random)$,
(ii) $F(Ind\text{-}Fixed) > F(Ind\text{-}Random)$, and
(iii) $F(None\text{-}Fixed) > F(None\text{-}Random)$.

We note that subjects may coordinate under the direct device by choosing X (Y) after receiving the message Y (X), which results in the same coordination outcome as when the messages are directly followed. The strong focal salience of the direct messages suggests, however, that this should rarely occur, if at all. On the other hand, it seems more likely that different mappings will emerge under the indirect device, i.e., subjects may coordinate on choosing either X or Y after receiving either @ or #. These observations give rise to another hypothesis:

Hypothesis 3 Conditional on observations of coordination, different mappings from the message spaces of the correlation device to the action spaces are more prevalent in the *Ind-Random (Ind-Fixed)* treatment than in the *Dir-Random (Dir-Fixed)* treatment.

While our main goal is to compare direct and indirect correlation devices for the achievement of correlated equilibrium, we also wish to account for *any* type of coordinated play that we may observe. Therefore our last hypothesis concerns possible alternative means of coordination. As noted in Sect. 3.1, subjects' shared common history when they play in fixed pairs provides them with an alternative means to coordinate, namely, via alternations of their actions or coordinated turn-takings [e.g., playing the outcome sequence (X, X), (Y, Y), (X, X),. . ., or its variants], which achieve an outcome that is equally efficient as coordinating on the correlated equilibrium.[11]

[11] This type of turn-taking behavior can be readily distinguished from the use of a correlation device in that a pattern of history-dependent alternation will be *deterministic* and independent of the device (while any use of a correlation device will necessarily entail a sequence of coordinated outcomes that is perfectly

By contrast, under random matchings, history-dependent strategies are not available, and the remaining viable alternatives to coordination may include playing the unique mixed-strategy Nash equilibrium or tacitly agreeing to play X or Y by all the subjects in a matching group.[12]

Given that establishing a common mapping with the indirect device requires additional effort in processing the symbolic messages, we conjecture that alternations or other coordination strategies not involving a correlation device will be used more often when the device is indirect rather than direct. It is also natural to expect that, relative to the coordination observed in the control treatments with no devices, the presence of a device will not result in more coordination in which the device is disregarded. Let $F(T|\not{\Phi})$ denote the frequency of coordination not involving the use of the correlation device in treatment T. The above discussion yields the following:

Hypothesis 4 Coordination in the absence of a correlation device is more prevalent than coordination not involving the use of an available device; coordination not involving the use of a correlation device is more prevalent when the device is indirect than when it is direct:

(i) $F(None\text{-}Random) > F(Ind\text{-}Random|\not{\Phi}) > F(Dir\text{-}Random|\not{\Phi})$ and
(ii) $F(None\text{-}Fixed) > F(Ind\text{-}Fixed|\not{\Phi}) > F(Dir\text{-}Fixed|\not{\Phi})$.

3.3 Procedures

Our experiment was conducted in English using z-Tree (Fischbacher 2007) at the Hong Kong University of Science and Technology. A total of 214 subjects who had no prior experience with our experiment were recruited from the undergraduate population of the university. Upon arrival at the laboratory, subjects were instructed to sit at separate computer terminals. Each received a copy of the experimental instructions. To ensure that the information contained in the instructions was induced as public knowledge, the instructions were read aloud, aided by slide illustrations and a comprehension quiz.

For each of the three random-matching treatments, we conducted three sessions. Three matching groups participated in each session. A matching group consisted of six subjects: three as Player 1 (Red Player) and three as Player 2 (Blue Player). As we regard each matching group in the random-matching treatments as an independent observation, we have nine observations for each of these treatments. For each of the three fixed-matching treatments, we conducted only one session. Either nine (*Dir-Fixed* and *None-Fixed*) or eight (*Ind-Fixed*) matching groups participated in a session,

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Footnote 11 continued
correlated with the random messages). See Vanderschraaf and Skyrms (2003) for a further discussion of the distinction between the alternating (turn-taking) equilibrium and a correlated equilibrium. See also Kaplan and Ruffle (2012), who experimented on a two-player repeated entry game with fixed partners, in which they considered the role of shared common history in promoting alternations versus the role of private random payoff fundamentals in promoting cutoff strategies as the coordination mechanisms.

[12] Note that these two alternatives are likely to be more difficult to achieve under random matching as compared to the history-dependent alternations that are possible under fixed matchings. These alternatives are also weakly dominated in terms of efficiency or fairness considerations by conditioning play on the realizations of the correlation device.

where a matching group consisted of two subjects as a fixed pair. Viewing each fixed pair as an independent observation, we thus have eight or nine observations for each of our three fixed-matching treatments. In all sessions, subjects participated in 60 rounds of play under a single treatment condition (i.e., *between-subject* design was used). At the beginning of a session, half of the subjects were randomly labeled as Red Players and the other half labeled as Blue Players. The role designation remained fixed throughout the session.

For the random-matching treatments, in each round one Red Player was randomly paired with one Blue Player within their matching group. Repeating matches were allowed. For the fixed-matching treatments, the matched pairs were randomly determined in the first round and remained the same throughout the session. Subjects were presented with a colored version of the payoff table (Fig. 12, Appendix 2). Red Players were told that they must choose between the rows of the payoff table, while Blue Players were instructed that they must choose between the columns. They were also told that their choices would jointly determine their earnings for the round according to the numbers given in the table.

We implemented the correlation devices as "computer announcements." Subjects in the direct-device treatments (*Dir-Random* and *Dir-Fixed*) were told that at the beginning of each round there was a 50 % chance that the computer program would announce X to both players and another 50 % chance that the program would announce Y to both players. Similarly, in the indirect-device treatments (*Ind-Random* and *Ind-Fixed*), subjects were instructed that at the beginning of each round there was a 50 % chance the computer program would announce @ to both players and another 50 % chance that the program would announce # to both players. The public nature of these announcements was made clear: Subjects were told that by seeing their own announcement they would know the announcement made to the subject with whom they were matched. We were careful to avoid any use of the word "recommendation." Further, we did not suggest in any way that the computer announcement could or should be used as a coordination device; in this way, we depart to some extent from the third-party recommendation design used in prior experimental studies. After seeing the computer announcements, subjects made their row/column choices. Feedback, which includes information on the announcement, the choices of both players, and the earnings of both players for the round, was provided at the end of each round.[13]

We randomly selected two rounds out of the 60 total rounds for each subject's payment. The sum of the payoffs a subject earned in the two selected rounds was converted into Hong Kong dollars at a fixed and known exchange rate of HK$10 per payoff point.[14] In addition to these earnings, subjects also received a show-up fee of

[13] Appendix 2 contains a sample instruction used for *Ind-Random*; the instructions for other treatments are similar.

[14] Thus, given our payoff profile, (9, 3), (3, 9), and the two instances of (0, 0), the maximum amount a subject could earn in a round (excluding the show-up fee) was HK$90 (≈ US$11.54), while the minimum amounts with and without coordination were, respectively, HK$30 (≈ US$3.85) and HK$0. We choose these payoff numbers for our game as they make it easy to calculate the expected payoff from following the correlation device (6). We also view these payoffs as providing reasonable monetary incentives for subjects to attempt to coordinate, as coordination earns them a minimum of US$3.85. The difference $9 - 3 = 6$ (US$7.69) should also create a reasonably strong "conflict" between the two symmetric Nash equilibria so

HK\$30. Subjects' total earnings averaged HK\$119.38 ($\approx$ US\$15.31).[15] The duration of a session was about 1 h.

4 Experimental findings

We report our experimental results as a number of findings that address our hypotheses as set forth in Sect. 3.2. Our first three findings, which are reported in Sect. 4.1, concern the unconditional frequency of coordination on the nonzero payoff outcomes, (X, X) and (Y, Y), which we simply refer to as the frequency of coordination. Focusing on this unconditional frequency allows us to make comparisons across all correlation device environments, including the *None-Random* and *None-Fixed* treatments where no device was available. We then consider in Sect. 4.2 the frequency of coordination conditional on the realizations of the correlation device. Finally, we consider in Sect. 4.3 the frequency of coordination not involving the use of correlation devices.

4.1 Unconditional coordination

Finding 1 *We reject* Hypothesis 1(i) *in favor of the Alternative to* Hypothesis 1(i) *that* $F(Dir\text{-}Random) > F(Ind\text{-}Random) \approx F(None\text{-}Random)$.

Support for Finding 1 can be found in Fig. 2 and Table 2, where we see that, averaging across all independent observations (matching groups), unconditional coordination frequencies were highest in *Dir-Random*, intermediate in *Ind-Random*, and lowest by a small margin in *None-Random*.[16] Using the independent group-level average coordination frequencies over all rounds as reported in Table 2, we reject the null hypothesis [Hypothesis 1(i)] of no difference in coordination frequencies between *Dir-Random* and *Ind-Random*, in favor of the alternative that coordination was greater in *Dir-Random* ($p < 0.01$, Mann-Whitney test). The frequency of coordination was also significantly higher in *Dir-Random* than in *None-Random* ($p < 0.01$, Mann-Whitney test), but there was no significant difference between *Ind-Random* and *None-Random* (two-sided $p = 0.67$, Mann–Whitney test).[17] These same conclusions continue to

Footnote 14 continued
that a role is created for the correlation device. Note further that the mixed-strategy equilibrium earns subjects an expected payoff of HK\$22.5 ($\approx$ US\$2.88), while the expected payoff in the correlated equilibrium is considerably higher, at HK\$60 ($\approx$ US\$7.69).

[15] Under the Hong Kong's currency board system, the Hong Kong dollar is pegged to the US dollar at the rate of HK \$7.8 = US\$1.

[16] Table 2 also reveals that, in *Dir-Random*, the frequencies of the two outcomes, (X, X) and (Y, Y), were often approximately equal. As will become apparent in the next subsection, this was due to subjects closely following the direct correlation device, which generated two messages with equal *ex ante* probabilities. (This close correspondence is also apparent in the top left panel of Fig. 2 for the *Dir-Random* treatment where unconditional "Coordination" frequencies closely correspond with conditional "Coordination on Announcement" frequencies over rounds.) The two coordination outcomes were less equal in *Ind-Random*, with a variety of frequency profiles observed in different matching groups. The average frequency of (Y, Y) across all matching groups was, however, higher than that of (X, X). A similar outcome was obtained in *None-Random*, but in this treatment the average frequency of (X, X) dominated that of (Y, Y); in the absence of any device, subjects might have considered (X, X) to be more salient.

[17] A Kruskal–Wallis test confirms that the three coordination frequencies significantly differed in at least one comparison ($p < 0.01$).

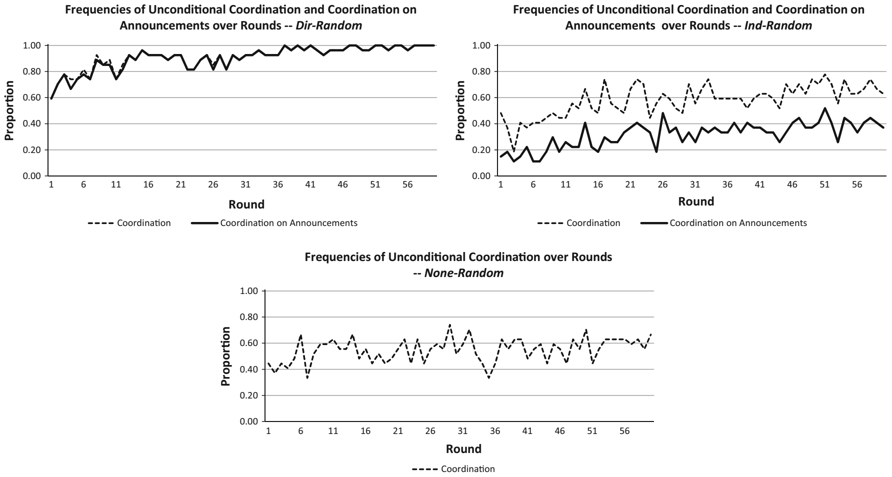

Fig. 2 Aggregate frequencies of unconditional coordination and coordination on announcements over rounds, random-matching treatments. *Note* The *dashed line* represents the round-by-round frequencies of unconditional coordination [frequencies of either (X, X) or (Y, Y)] averaged across all independent observations (matching groups). The *solid line* represents the same for coordination conditional on announcements; details of how conditional coordination was determined are provided in Sect. 4.2

hold if attention is restricted to the coordination frequencies over the last 10 rounds of each session. The differences in coordination frequencies are also reflected in payoff differences. We conclude that, under *random* matchings, the direct correlation device is efficiency enhancing relative to the indirect device or the absence of any such device.

Finding 2 *Consistent with* Hypothesis 1(ii)*, we find no significant difference between F(Dir-Fixed) and F(Ind-Fixed).*

Support for Finding 2 comes from Fig. 3 and Table 3. Table 3 reveals that, over either all rounds or just the last 10 rounds, the unconditional coordination frequency was highest in *Ind-Fixed*. The second highest frequency was either *Dir-Fixed* or *None-Fixed*, depending on whether attention is restricted to all rounds or the last 10 rounds.[18] Nevertheless, using the independent group-level average coordination frequencies over all rounds as reported in Table 3, we cannot reject the null hypothesis [Hypothesis 1(ii)] of no difference between the frequencies in *Dir-Fixed* and *Ind-Fixed*. In fact, there were no significant differences in all three pairwise treatment comparisons (two-sided $p \geq 0.63$, Mann–Whitney tests).[19] This absence of significant differences across the fixed-matching treatments was also reflected in an absence of payoff differences;

[18] In all three fixed-matching treatments, the frequencies of the two outcomes, (X, X) and (Y, Y), were, in most cases, approximately equal. Note in particular the difference of *None-Fixed* from *None-Random*, in which the two outcomes (X, X) and (Y, Y) in *None-Fixed* were significantly more even than those in *None-Random*; as an indicator, the frequency of (Y, Y) was significantly lower in *None-Random* than in *None-Fixed* ($p \leq 0.01$ using both all-around data and last 10-round data, Mann–Whitney tests).

[19] Using observations from the last 10 rounds, we also cannot reject there being no differences in the coordination frequencies except for the comparison between *Ind-Fixed* and *None-Fixed*, in which the frequency in the former was higher than in the latter with statistical significance ($p = 0.02$, Mann-Whitney test).

Table 2 Frequencies of (X, X), (Y, Y), and coordination [either (X, X) or (Y, Y)] and average payoffs, random-matching treatments

Session–Group	(X, X)/(Y, Y) All Rnds.	Coord. All Rnds.	Avg. Payoff All Rnds.	(X, X)/(Y, Y) Rnd. 51-60	Coord. Rnd. 51-60	Avg. Payoff Rnd. 51-60
Dir-Random						
1-1	0.389/0.344	0.733	4.40	0.467/0.533	1.000	6.00
1-2	0.456/0.505	0.961	5.77	0.500/0.500	1.000	6.00
1-3	0.394/0.439	0.833	5.00	0.500/0.467	0.967	5.80
2-1	0.528/0.439	0.967	5.80	0.500/0.500	1.000	6.00
2-2	0.461/0.511	0.972	5.83	0.467/0.533	1.000	6.00
2-3	0.417/0.405	0.822	4.93	0.567/0.433	1.000	6.00
3-1	0.467/0.489	0.956	5.73	0.367/0.633	1.000	6.00
3-2	0.533/0.467	1.000	6.00	0.533/0.467	1.000	6.00
3-3	0.511/0.445	0.956	5.73	0.400/0.567	0.967	5.80
Mean	0.462/0.449	0.911	5.47	0.478/0.515	0.993	5.96
Ind-Random						
1-1	0.389/0.233	0.622	3.73	0.400/0.233	0.633	3.80
1-2	0.428/0.289	0.717	4.30	0.367/0.367	0.734	4.40
1-3	0.389/0.178	0.567	3.40	0.467/0.267	0.734	4.40
2-1	0.105/0.367	0.472	2.83	0.033/0.567	0.600	3.60
2-2	0.378/0.383	0.761	4.57	0.533/0.467	1.000	6.00
2-3	0.195/0.172	0.367	2.20	0.200/0.133	0.333	2.00
3-1	0.044/0.528	0.572	3.43	0.000/0.800	0.800	4.80
3-2	0.117/0.416	0.533	3.20	0.200/0.500	0.700	4.20
3-3	0.122/0.484	0.606	3.63	0.067/0.467	0.534	3.20
Mean	0.241/0.339	0.580	3.48	0.252/0.422	0.674	4.04
None-Random						
1-1	0.239/0.155	0.394	2.37	0.300/0.133	0.433	2.60
1-2	0.311/0.183	0.494	2.97	0.267/0.200	0.467	2.80
1-3	0.111/0.389	0.500	3.00	0.133/0.633	0.766	4.60
2-1	0.106/0.272	0.378	2.27	0.033/0.233	0.266	1.60
2-2	0.117/0.372	0.489	2.93	0.067/0.367	0.434	2.60
2-3	0.600/0.011	0.611	3.67	0.700/0.033	0.733	4.40
3-1	0.839/0.000	0.839	5.03	0.633/0.000	0.633	3.80
3-2	0.567/0.027	0.594	3.57	0.767/0.000	0.767	4.60
3-3	0.545/0.083	0.628	3.77	0.867/0.000	0.867	5.20
Mean	0.382/0.166	0.548	3.29	0.418/0.178	0.596	3.58

it can also be seen visually over time, in the analogous patterns of round-by-round coordination frequencies across the three treatments (Fig. 3).

We note that in all fixed-matching treatments, the extent of coordination was surprisingly high, even when the correlation device was absent—the coordination frequency

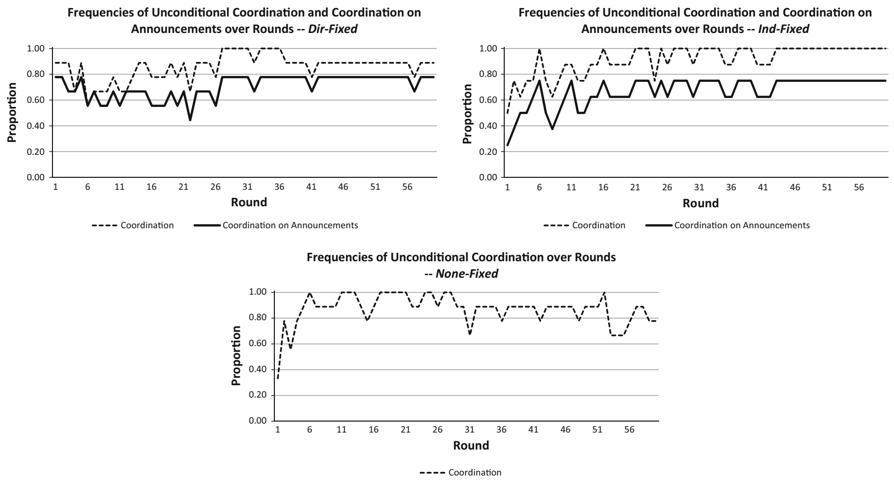

Fig. 3 Aggregate frequencies of unconditional coordination and coordination on announcements over rounds, fixed-matching treatments. *Note* The *dashed line* represents the round-by-round frequencies of unconditional coordination [frequencies of either (X, X) or (Y, Y)] averaged across all independent observations (matching groups). The *solid line* represents the same for coordination on announcements; the details of how the conditional coordination is determined are discussed in Sect. 4.2

averaged 86.9 % in *None-Fixed*. This suggests that there may be little room for correlation devices to yield significant improvements in coordination when the game is played with a fixed partner.

We next compare the impact of the two matching protocols on the three correlation device environments:

Finding 3 *Consistent with* Hypothesis 2*, we find that F(Ind-Fixed) > F(Ind-Random) and F(None-Fixed) > F(None-Random). However, inconsistent with Hypothesis 2, there was no significant difference between F(Dir-Fixed) and F(Dir-Random).*

Support for Finding 3 comes again from the coordination frequencies reported in Tables 2, 3. Using these group-level observations over all rounds or over the last 10 rounds, we can reject the null hypothesis of no difference in coordination frequencies between *Ind-Random* and *Ind-Fixed* ($p < 0.01$, Mann–Whitney tests) and between *None-Random* and *None-Fixed* ($p < 0.05$, Mann–Whitney tests). The null hypothesis, however, cannot be rejected for the comparisons between *Dir-Fixed* and *Dir-Random* (two-sided $p \geq 0.23$, Mann–Whitney tests).

Summarizing our results to this point, we find that under random matchings the presence of a direct correlation device is efficiency enhancing relative to an indirect device or the absence of any device. In the absence of a direct device (either the device is indirect or there is no device at all), we find that the fixed-matching protocol is efficiency enhancing relative to the random-matching protocol. Finally, under fixed matchings, the presence of a device, either direct or indirect, appears to make little difference for efficiency relative to the baseline case of no device.

Table 3 Frequencies of (X, X), (Y, Y), and coordination [either (X, X) or (Y, Y)] and average payoffs, fixed-matching treatments

Group	(X, X)/(Y, Y) All Rnds.	Coord. All Rnds.	Avg. Payoff All Rnds.	(X, X)/(Y, Y) Rnd. 51-60	Coord. Rnd. 51-60	Avg. Payoff Rnd. 51-60
Dir-Fixed						
1	0.417/0.433	0.850	5.10	0.500/0.400	0.900	5.40
2	0.450/0.400	0.850	5.10	0.600/0.400	1.000	6.00
3	0.350/0.333	0.683	4.10	0.500/0.500	1.000	6.00
4	0.817/0.000	0.817	4.90	1.000/0.000	1.000	6.00
5	0.500/0.500	1.000	6.00	0.600/0.400	1.000	6.00
6	0.383/0.617	1.000	6.00	0.400/0.600	1.000	6.00
7	0.467/0.533	1.000	6.00	0.600/0.400	1.000	6.00
8	0.517/0.483	1.000	6.00	0.600/0.400	1.000	6.00
9	0.000/0.517	0.517	3.10	0.000/0.000	0.000	0.00
Mean	0.433/0.424	0.857	5.14	0.533/0.345	0.878	5.27
Ind-Fixed						
1	0.417/0.517	0.933	5.60	0.300/0.700	1.000	6.00
2	0.500/0.483	0.983	5.90	0.500/0.500	1.000	6.00
3	0.467/0.483	0.950	5.70	0.500/0.500	1.000	6.00
4	0.433/0.483	0.967	5.80	0.500/0.500	1.000	6.00
5	0.533/0.467	1.000	6.00	0.200/0.800	1.000	6.00
6	0.500/0.367	0.867	5.20	0.500/0.500	1.000	6.00
7	0.350/0.267	0.617	3.70	0.600/0.400	1.000	6.00
8	0.417/0.583	1.000	6.00	0.500/0.500	1.000	6.00
Mean	0.458/0.456	0.915	5.49	0.450/0.550	1.000	6.00
None-Fixed						
1	0.500/0.500	1.000	6.00	0.500/0.500	1.000	6.00
2	0.283/0.367	0.650	3.90	0.200/0.500	0.700	4.20
3	0.467/0.467	0.934	5.60	0.000/0.900	0.900	5.40
4	0.000/0.467	0.467	2.80	0.000/0.100	0.100	0.60
5	0.450/0.467	0.917	5.50	0.500/0.500	1.000	6.00
6	0.467/0.433	0.900	5.40	0.300/0.200	0.500	3.00
7	0.483/0.483	0.966	5.80	0.500/0.500	1.000	6.00
8	0.483/0.500	0.983	5.90	0.500/0.500	1.000	6.00
9	0.500/0.500	1.000	6.00	0.500/0.500	1.000	6.00
Mean	0.404/0.465	0.869	5.21	0.333/0.467	0.800	4.80

4.2 Coordination conditional on realizations of correlation devices

We next turn to an analysis of whether and how subjects conditioned their play on the realizations of the correlation devices. Tables 4 and 5 report, respectively, for the random- and fixed-matching treatments, how random announcements of (X, X)/(Y, Y) for the direct-device treatments and of (@, @)/(#, #) for the indirect-device treatments impacted on the four possible game outcomes over all rounds.

The tables also indicate whether or not an endogenously determined mapping from announcements to actions (Endog. Mapping of Announcements) was achieved by subjects. This mapping was determined based on the most frequent coordination outcome given realized announcements in the last 10 rounds of play.[20] If a mapping could be determined, it is reported [e.g., in *Ind-Random*, Observation 1-1 (Session 1, Group 1), (X, X) was the most frequently played coordination outcome in response to announcements (@, @)]. Otherwise, if no such mapping could be established, the endogenous mapping is reported as not available ("N/A").

Using the endogenously determined mappings of announcements, Tables 4 and 5 also report on the frequencies of coordination on announcements over all rounds (All Rnds.) and over the last 10 rounds (Rnds. 51-60). For example, in *Ind-Random*, Observation 1-1, the announcements (@, @) were most frequently associated with the coordination outcome (X, X) and (#, #) were most frequently associated with (Y, Y); accordingly, the conditional coordination frequency was calculated as the sum of the percentage of the time that (@, @) resulted in (X, X) and of the time that (#, #) resulted in (Y, Y), weighted by the respective frequencies of (@, @) and (#, #). In the cases where endogenous mappings could not be established, these conditional frequencies are reported as zero. Figures 2 and 3 also present the aggregate average frequencies of coordination on announcements over rounds based on our endogenously determined mappings.

Tables 4 and 5 reveal that, under the direct correlation device, the endogenous mapping of announcements, when it could be established, was always "literal" in the sense that announcements of (X, X) [(Y, Y)] most frequently resulted in play of X [Y] by both players. This literal mapping holds true in both the fixed- and random-matching versions of the direct-device treatments.

The two exceptions to the observation that direct announcements resulted in a literal mapping into actions occurred in *Dir-Fixed*, Observations 4 and 9. In both of those observations, the subject pairs coordinated for a sustained period of time on one of the two pure strategy equilibria—(X, X) for Observation 4 and (Y, Y) for Observation 9—entirely ignoring the announcements (Fig. 9 in Appendix 1; Table 5). By contrast, in *Dir-Random*, we can always associate announcements of (X, X) or (Y, Y) with frequent play of X or Y by both players. The friction created by random matchings, coupled with the presence of a direct device, prevented subjects from developing a reputation for playing any single action.

In the indirect-device treatments, the established mappings between announcements and actions were more varied. As Tables 4 and 5 reveal, an announcement of (@, @) was most commonly associated with play of (X, X), while an announcement of (#, #) was most commonly associated with play of (Y, Y).[21] One might view this mapping

[20] The mappings were ascertained by us. They show next-to-zero variation if we instead restrict attention to the last 30, 20, 5, or 3 rounds.

[21] Specifically, this mapping occurred in 3 out of the 5 observations of the *Ind-Random* treatment where a mapping could be established and in 5 out of the 6 observations of the *Ind-Fixed* treatment where a mapping could be established.

Table 4 Frequencies of conditional outcomes and mappings of announcements, random-matching treatments

Treat./Obs.	Outcomes conditional on announcements								Endog. Mapping of announcements		Coord. on announcements	
Dir-Random	(X, X)				(Y, Y)							
Session–Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(Y, Y)	All Rnds.	Rnds. 51-60
1-1	0.802	0.151	0.047	0.000	0.011	0.319	0.011	0.660	(X, X)	(Y, Y)	0.728	1.000
1-2	0.953	0.047	0.000	0.000	0.000	0.032	0.000	0.968	(X, X)	(Y, Y)	0.961	1.000
1-3	0.787	0.191	0.022	0.000	0.011	0.088	0.033	0.868	(X, X)	(Y, Y)	0.828	0.967
2-1	0.990	0.010	0.000	0.000	0.000	0.060	0.000	0.940	(X, X)	(Y, Y)	0.967	1.000
2-2	0.933	0.022	0.034	0.011	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	0.967	1.000
2-3	0.888	0.113	0.000	0.000	0.040	0.160	0.070	0.730	(X, X)	(Y, Y)	0.800	1.000
3-1	0.944	0.045	0.011	0.000	0.000	0.033	0.000	0.967	(X, X)	(Y, Y)	0.956	1.000
3-2	1.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	1.000	1.000
3-3	0.958	0.031	0.010	0.000	0.000	0.036	0.012	0.952	(X, X)	(Y, Y)	0.956	0.967
Mean	0.917	0.068	0.014	0.001	0.007	0.081	0.014	0.898			0.907	0.993
Ind-Random	Outcomes conditional on announcements								Endog. mapping of announcements		Coord. on announcements	
	(@, @)				(#, #)							
Session–Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(@, @)	(#, #)	All Rnds.	Rnds. 51-60
1-1	0.685	0.304	0.011	0.000	0.080	0.341	0.102	0.477	(X, X)	(Y, Y)	0.583	0.633
1-2	0.054	0.272	0.120	0.554	0.818	0.170	0.000	0.011	(Y, Y)	(X, X)	0.683	0.733
1-3	0.810	0.167	0.012	0.012	0.021	0.635	0.021	0.323	(X, X)	(Y, Y)	0.550	0.733
2-1	0.082	0.412	0.059	0.447	0.126	0.474	0.105	0.295	N/A	N/A	0.000	0.000
2-2	0.688	0.250	0.052	0.010	0.024	0.143	0.024	0.810	(X, X)	(Y, Y)	0.744	1.000

Table 4 continued

Ind-Random	Outcomes Conditional on Announcements								Endog. Mapping of announcements		Coord. on announcements	
	(@, @)				(#, #)							
Session–Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(@, @)	(#, #)	All Rnds.	Rnds. 51-60
2-3	0.283	0.522	0.076	0.120	0.102	0.625	0.045	0.227	N/A	N/A	0.000	0.000
3-1	0.067	0.393	0.112	0.427	0.022	0.308	0.044	0.626	N/A	N/A	0.000	0.000
3-2	0.096	0.372	0.117	0.415	0.140	0.372	0.070	0.419	(Y, Y)	(X, X)	0.283	0.500
3-3	0.202	0.404	0.112	0.281	0.044	0.231	0.044	0.681	N/A	N/A	0.000	0.000
Mean	0.330	0.344	0.075	0.252	0.153	0.367	0.051	0.430			0.316	0.400

"Endog. Mapping of Announcements" is determined based on the most frequent coordination outcome given the realized announcements in the last 10 rounds of play. If a mapping cannot be established, it is reported as "N/A." "Coord. on Announcements" reports the sum of the percentages of the time that the two announcements resulted in the respective coordination outcomes consistent with the established endogenous mapping, weighted by the respective frequencies of the two announcements. In the cases where endogenous mappings cannot be established, the frequencies of coordination on announcements are reported as zero. The frequency of "Outcomes Conditional on Announcements" is based on all-around data

Table 5 Frequencies of conditional outcomes and mappings of announcements, fixed-matching treatments

Treat./Obs.	Outcomes Conditional on Announcements								Endog. Mapping of		Coord. on	
Dir-Fixed	(X, X)				(Y, Y)				Announcements		Announcements	
Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(Y, Y)	All Rnds.	Rnds. 51-60
1	0.893	0.107	0.000	0.000	0.000	0.125	0.063	0.813	(X, X)	(Y, Y)	0.850	0.900
2	0.871	0.129	0.000	0.000	0.000	0.172	0.000	0.828	(X, X)	(Y, Y)	0.850	1.000
3	0.656	0.344	0.000	0.000	0.000	0.286	0.000	0.714	(X, X)	(Y, Y)	0.683	1.000
4	0.897	0.103	0.000	0.000	0.742	0.258	0.000	0.000	N/A	N/A	0.000	0.000
5	1.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	1.000	1.000
6	1.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	1.000	1.000
7	1.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	1.000	1.000
8	1.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	1.000	1.000
9	0.000	0.686	0.000	0.314	0.000	0.200	0.000	0.800	N/A	N/A	0.000	0.000
Mean	0.813	0.152	0.000	0.035	0.082	0.116	0.007	0.795			0.709	0.767
Ind-Fixed	Outcomes Conditional on Announcements								Endog. Mapping of		Coord. on	
	(@, @)				(#, #)				Announcements		Announcements	
Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(@, @)	(#, #)	All Rnds.	Rnds. 51-60
1	0.962	0.000	0.038	0.000	0.000	0.088	0.000	0.912	(X, X)	(Y, Y)	0.933	1.000
2	0.486	0.000	0.000	0.514	0.520	0.040	0.000	0.440	N/A	N/A	0.000	0.000
3	0.407	0.037	0.000	0.556	0.515	0.061	0.000	0.424	N/A	N/A	0.000	0.000
4	0.967	0.033	0.000	0.000	0.000	0.033	0.000	0.967	(X, X)	(Y, Y)	0.967	1.000

Table 5 continued

Ind-Fixed	Outcomes Conditional on Announcements								Endog. Mapping of Announcements		Coord. on Announcements	
	(@, @)				(#, #)							
Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(@, @)	(#, #)	All Rnds.	Rnds. 51-60
5	1.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	1.000	1.000
6	0.000	0.148	0.037	0.815	0.909	0.000	0.091	0.000	(Y, Y)	(X, X)	0.867	1.000
7	0.724	0.103	0.138	0.034	0.000	0.516	0.000	0.484	(X, X)	(Y, Y)	0.600	1.000
8	1.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	(X, X)	(Y, Y)	1.000	1.000
Mean	0.693	0.040	0.027	0.240	0.243	0.092	0.011	0.653			0.671	0.750

Note: “Endog. Mapping of Announcements” is determined based on the most frequent coordination outcome given the realized announcements in the last 10 rounds of play. If a mapping cannot be established, it is reported as “N/A.” “Coord. on Announcements” reports the sum of the percentages of the time that the two announcements resulted in the respective coordination outcomes consistent with the established endogenous mapping, weighted by the respective frequencies of the two announcements. In the cases where endogenous mappings could not be established, the frequencies of coordination on announcements are reported as zero. The frequency of “Outcomes Conditional on Announcements” is based on all-around data as the potentially more focal one since X comes before Y alphabetically and the @ symbol sits to the left of the # symbol on computer keyboards. However, in both of these treatments, there also existed cases for which the opposite mapping, (@, @) → (Y, Y) and (#, #) → (X, X), was established.[22] Specifically, we find this opposite mapping in Observations 1-2 and 3-2 from the *Ind-Random* treatment and in Observation 6 from the *Ind-Fixed* treatment. By contrast, under the strong focal salience of the direct announcements, we *never* observed announcement-to-action mappings of (X, X) → (Y, Y) or of (Y, Y) → (X, X), even though playing according to such a mapping would result in an equally efficient outcome. We summarize these findings as follows:[23]

Finding 4 *Consistent with* Hypothesis 3*, there was a greater variety in the mappings from announcements to actions in the indirect-device treatments than in the direct-device treatments.*

We further observe that mappings from messages to actions were more difficult to establish when the random-matching protocol was interacted with the indirect device. Table 4 reveals that, in 4 out of the 9 observations from *Ind-Random*, a mapping could not be established; by contrast, all 9 observations of the *Dir-Random* treatment had mappings that were established as literal. Despite the theoretical equivalence of direct and indirect correlation devices under the revelation principle, this finding suggests that a corresponding empirical equivalence does not exist and direct devices outperform indirect devices in facilitating the play of a correlated equilibrium, especially when learning is limited by random matchings.

On the other hand, when coupled with the direct correlation device, the different matching protocols have no impact on announcement mappings. As Table 5 reveals, we obtain the same number of observations, 2, in each of the *Dir-Random* (Observations 4 and 9) and the *Dir-Fixed* (Observations 2 and 3) treatments in which a mapping from announcements to actions could *not* be established. This finding might be viewed as somewhat puzzling, as it is natural to expect that repeated interactions with a fixed partner should be more conducive to the development of a common mapping. However, as we posit in Hypothesis 4, coordinated play that does not involve the use of a correlation device should be more likely under fixed matchings than under random matchings, because playing with the same partner allows for the development of history-contingent strategies. Indeed, in the next section, we provide evidence that

[22] The experimental instructions present the symbolic announcements in the order of @ and then #. Such an order and the alphabetical order of the action labels X and Y may have provided subjects with an a priori mapping from symbols to actions. This might be another important factor contributed to the frequently observed associations of @ with X and # with Y and represents a limitation of our design in providing a clean environment for the mappings to emerge. We note, however, that since @ sits to the left of # on computer keyboards, a potential a priori mapping may not be entirely avoided even when the symbol order is randomized in our instructions. (Subjects in our experiment clicked on the screens without using a keyboard, but a keyboard was always visible when they made decisions.) While in hindsight there is room for improvement in our design, observations of the opposite mapping suggest that presenting the symbols to subjects in a particular order did not inevitably induce a single, focal mapping.

[23] Given the potential existence of an *a priori* mapping using the symbolic announcements as discussed in the above footnote, Hypothesis 3 can be viewed as testing whether the salience of the direct announcements is more conducive to a universal common mapping than the salience of the order (either in the instructions or on the keyboards) of the symbolic announcements.

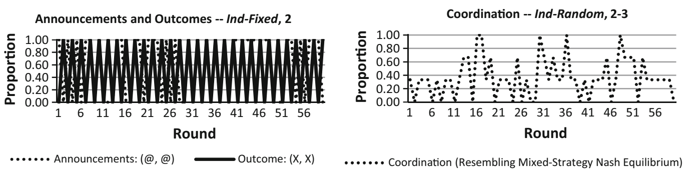

Fig. 4 Non-establishable mapping from announcements to actions: two observations from *Ind-Fixed* and *Ind-Random*

fixed matchings facilitated more efficient device-independent coordination than did random matchings.

4.3 Alternative means of coordination

Figure 4 shows two observations where mappings from announcements to actions could not be established: one from *Ind-Fixed* (Observation 2) and one from *Ind-Random* (Observation 2-3). In the *Ind-Fixed* observation (the left panel), the fixed pair of subjects completely disregarded the correlation device and took advantage of their shared common history to employ alternation between the two pure strategy Nash equilibria as their coordination mechanism. As they did not make use of the random announcements, no mapping could be established. In fact, the figure suggests that the announcements of (@, @) were frequently ignored. Note that while the alternation achieves the same expected payoff, it is less arbitrary than having payoffs generated stochastically according to the random announcements; the actual realizations of the correlation device might (*ex post*) benefit one player relative to the other over the finite, 60 rounds of play.

By contrast, in the observation from the *Ind-Random* treatment (the right panel of Fig. 4), the frequency of coordination on either (X, X) or (Y, Y) was not only clearly different from 100 % but also highly variable over rounds. Recall that for the random-matching treatments there were 3 pairs of subjects per observation so that the coordination frequency could be either 0, 1/3, 2/3, or 1. If all 6 subjects had coordinated based on a common announcement mapping, the coordination frequency would have eventually converged to 1 and remained there. Instead, the figure suggests that the pairs in this observation might be playing according to the unique mixed-strategy Nash equilibrium of the game. This impression is further supported by the frequencies of individual action choices: For this observation, X was played by the Red Players (row players) 76.7 % of the time and by the Blue Players (column players) 25.6 % of the time; recall that in the mixed-strategy equilibrium, row (column) plays X with probability 0.75 (0.25). The coordination on the mixed-strategy Nash equilibrium explains why no announcement mapping could be established for this observation. As in the case of the *Ind-Fixed* observation, the matching protocol—random matching in this case—seems to have been a dominant factor in determining how subjects

Table 6 Frequencies of outcomes, *None-Fixed* and *None-Random* treatments

Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)
1	0.500	0.000	0.000	0.500	1-1	0.239	0.550	0.056	0.156
2	0.283	0.250	0.100	0.367	1-2	0.311	0.406	0.100	0.183
3	0.467	0.033	0.033	0.467	1-3	0.111	0.433	0.067	0.389
4	0.000	0.517	0.017	0.467	2-1	0.106	0.550	0.072	0.272
5	0.450	0.050	0.033	0.467	2-2	0.117	0.422	0.089	0.372
6	0.467	0.067	0.033	0.433	2-3	0.600	0.378	0.011	0.011
7	0.483	0.033	0.000	0.483	3-1	0.839	0.150	0.011	0.000
8	0.483	0.000	0.017	0.500	3-2	0.567	0.394	0.011	0.028
9	0.500	0.000	0.000	0.500	3-3	0.544	0.267	0.106	0.083
Mean	0.404	0.106	0.026	0.465	Mean	0.382	0.394	0.058	0.166
Alternation	0.500	0.000	0.000	0.500	Mixed-Strategy N.E.	0.1875	0.5625	0.0625	0.1875

The frequency of the outcomes is based on all-around data

coordinated when the announcements were not followed—in this case by playing the mixed-strategy Nash equilibrium.[24]

We further examine subjects' behavior in the two treatments with no correlation device. Table 6 reports the all-around frequencies of the four game outcomes in each matching group of the *None-Fixed* and *None-Random* treatments. The device-independent behavior in the two selected observations from *Ind-Fixed* and *Ind-Random* shown in Fig. 4 is also representative of behavior in the absence of a correlation device. For the *None-Fixed* treatment, in 7 out of the 9 observations we find strong evidence for alternation (or turn-taking) strategies as evidenced by the near 50 % frequencies on outcomes (X, X) and (Y, Y) in these observations. Two observations in particular, 1 and 9, indicate perfect coordination on a deterministic turn-taking strategy. While almost all such observations involved a perfect 2-cycle pattern of alternation [i.e., (X, X), (Y, Y), (X, X),. . .] as in Observation 1 (the left panel of Fig. 5), there were two instances of a more prolonged interval of turn-taking. In Observation 3 (the right panel of Fig. 5), each subject in the fixed match obtained his/her preferred outcome, (X, X) or (Y, Y), for approximately 30 consecutive rounds, or about 1/2 of the session for each. Shorter intervals of turn-taking occurred in Observation 2 (Fig. 11, Appendix 1). These findings suggest that history-contingent alternation strategies made possible by fixed matchings may serve as an alternative coordination mechanism in the absence of a correlation device.

By contrast, in *None-Random* the frequencies of the four outcomes appeared to correspond most closely to the unique mixed-strategy equilibrium. In this mixed-strategy equilibrium, the outcomes (X, X) and (Y, Y) should each arise on average 18.75 % of the time, the outcome (X, Y) should arise most often, on average 56.25 % of

[24] In two other observations from *Ind-Random* in which no mapping from announcements to actions could be established, Observations 2-1 and 3-2, most coordination was achieved by subjects choosing (Y, Y). See Fig. 7 in Appendix 1.

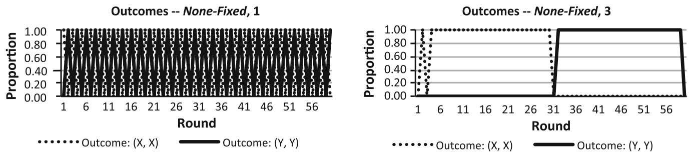

Fig. 5 Two different patterns of coordination on alternation, *None-Fixed* treatment

the time, and the outcome (Y, X) should arise least often, on average 6.25 % of the time. As Table 6 makes clear, 5 out of the 9 observations of this treatment were consistent with the play of this mixed-strategy equilibrium in the sense that the frequency of (X, Y) was the greatest, the frequency of (Y, X) was the lowest, and the frequencies of (X, X) and (Y, Y) were intermediate. The four exceptions were Observations 2-3, 3-1, 3-2, and 3-3, where a substantial but discontinuous amount of coordination occurred on the outcome (X, X).

We proceed to evaluate Hypothesis 4. For each matching group in the four treatments with a correlation device, we measure the frequency of coordination not involving the use of devices (device-independent coordination) by subtracting the frequency of coordination on announcements (as reported in Tables 4, 5) from the frequency of (unconditional) coordination (as reported in Tables 2, 3). These differences are reported in Table 7. For ease of comparison, we also reproduce in Table 7 the frequencies of coordination from *None-Random* and *None-Fixed*.[25]

Using the group-level, all-around average frequencies of device-independent coordination, we test Hypothesis 4(i), which concerns the random-matching treatments. Consistent with the second inequality of this hypothesis, we reject the null of no difference between *Dir-Random* and *Ind-Random* in favor of the alternative that device-independent coordination was more prevalent in *Ind-Random* ($p < 0.01$, Mann-Whitney test). Consistent with the first inequality of the hypothesis, we also find that the frequency of device-independent coordination in *Ind-Random* was significantly lower than the frequency of coordination in *None-Random* ($p < 0.01$, Mann–Whitney test). These same conclusions continue to hold for the last 10-round data.[26]

For Hypothesis 4(ii), which concerns the fixed-matching treatments, device-independent coordination was also more prevalent under the indirect device. However, using the group-level, all-around averages, we do not find a significantly higher frequency of device-independent coordination in *Ind-Fixed* than in *Dir-Fixed* ($p = 0.24$, Mann–Whitney test), thus leading us to reject the second inequality in the hypothesis. The first inequality is nevertheless confirmed: The frequency of coordination in *None-Fixed* was significantly higher than the frequency of device-independent coordination

25 The frequencies of coordination reproduced for *None-Random* and *None-Fixed* can also be considered as a result of applying the same formula of subtracting the frequency of coordination on announcements from the frequency of coordination; in the absence of a correlation device, the former is considered to be zero.

26 A Kruskal–Wallis test also confirms that the frequencies significantly differed in at least one comparison ($p < 0.01$ for both the all-around and last 10-round data).

Table 7 Frequencies of device-independent coordination

Treat./Obs. *Dir-Random*	Freq. of Device-Independent Coord.		Treat./Obs. *Dir-Fixed*	Freq. of Device-Independent Coord.	
Session–Group	All Rnds.	Rnd. 51-60	Group	All Rnds.	Rnd. 51-60
1-1	0.006	0.000	1	0.000	0.000
1-2	0.000	0.000	2	0.000	0.000
1-3	0.006	0.000	3	0.000	0.000
2-1	0.000	0.000	4	0.817	1.000
2-2	0.006	0.000	5	0.000	0.000
2-3	0.022	0.000	6	0.000	0.000
3-1	0.000	0.000	7	0.000	0.000
3-2	0.000	0.000	8	0.000	0.000
3-3	0.000	0.000	9	0.517	0.000
Mean	0.004	0.000	Mean	0.148	0.111
Ind-Random	Freq. of Device-Independent Coord.		*Ind-Fixed*	Freq. of Device-Independent Coord.	
Session–Group	All Rnds.	Rnd. 51-60	Group	All Rnds.	Rnd. 51-60
1-1	0.039	0.000	1	0.000	0.000
1-2	0.033	0.000	2	0.983	1.000
1-3	0.017	0.000	3	0.950	1.000
2-1	0.472	0.600	4	0.000	0.000
2-2	0.017	0.000	5	0.000	0.000
2-3	0.367	0.333	6	0.000	0.000
3-1	0.572	0.800	7	0.017	0.000
3-2	0.250	0.200	8	0.000	0.000
3-3	0.606	0.533			
Mean	0.264	0.274	Mean	0.244	0.250

Table 7 continued

None-Random	Freq. of Coord.		*None-Fixed*	Freq. of Coord.	
Session–Group	All Rnds.	Rnd. 51-60	Group	All Rnds.	Rnd. 51-60
1-1	0.394	0.433	1	1.000	1.000
1-2	0.494	0.467	2	0.650	0.700
1-3	0.500	0.767	3	0.933	0.900
2-1	0.378	0.267	4	0.467	0.100
2-2	0.489	0.433	5	0.917	1.000
2-3	0.611	0.733	6	0.900	0.500
3-1	0.839	0.633	7	0.967	1.000
3-2	0.594	0.767	8	0.983	1.000
3-3	0.628	0.867	9	1.000	1.000
Mean	0.548	0.596	Mean	0.869	0.800

Note: "Freq. of Device-Independent Coord." refers to the frequency of coordination not involving the use of correlation devices, which is calculated as the difference (frequency of coordination − frequency of coordination on announcements). For the *None-Random* and *None-Fixed* treatments with no correlation device, the frequencies of coordination are instead reported (reproduced from Tables 2, 3) in *Ind-Fixed* ($p < 0.01$, Mann–Whitney test). Using only the last 10 rounds of data does not change these conclusions.[27]

We summarize the above findings as follows:

Finding 5 *Consistent with* Hypothesis 4*, we find that device-independent coordination was more prevalent under the indirect device, especially when the random-matching protocol was used. When subjects did not use or have access to a correlation device under the fixed-matching protocol, they coordinated by adopting efficient alternation strategies. Indeed, fixed matchings could serve as substitutes for a correlation device. By contrast, when subjects did not have access to a correlation device under the random-matching protocol, play appeared to be more consistent with the less efficient, unique mixed-strategy equilibrium of the game.*

5 Concluding remarks

The possibility that correlated strategies allow players to achieve more efficient outcomes than the Nash equilibrium outcomes of coordination games has been known for some time. Myerson's (1982, 1994) revelation principle suggests that the message space of the correlation device used to implement the correlated strategies does not matter; the message space can contain direct messages phrased in terms of the players' actions or indirect messages that have no such associations.

Using the Battle of the Sexes game as the platform, this experimental study has helped shed some light on this theoretical property from an empirical vantage point, clarifying the important role played by direct correlation devices in facilitating play of correlated equilibria in the laboratory. Our findings indicate that, while a mapping from an indirect device's messages to actions *can* be learned, in efficiency terms a direct device outperforms an indirect device, especially when subjects are randomly matched to play the game. Overall, the benefits of a correlation device accrue mainly under the random-matching protocol; when subjects play in fixed pairs, their shared history allows for the development of history-contingent strategies (e.g., alternation) that can achieve the same efficient outcomes made possible by the introduction of a correlation device. In this sense, the fixed-matching protocol can serve as a substitute for a correlation device in the game we study.

We caution that we have only taken a first step in understanding the empirical implications of the revelation principle as applied to correlated equilibria in strategic-form games. For instance, our experimental design has only considered the case where the correlation devices have two messages, the same number as the available actions; extending the available messages relative to the actions may create additional obstacles for subjects to play the correlated equilibrium. Furthermore, we have focused our attention on a correlated equilibrium that is most efficient among those that produce fair outcomes and which involves fully public signals (no private information). While these restrictions have served our purpose of providing strong incentives for subjects to condition their decisions on the correlation device so that we can focus on the issue of

[27] A Kruskal–Wallis test also reveals that the frequencies significantly differed in at least one comparison ($p < 0.01$ for both the all-around and last 10-round data).

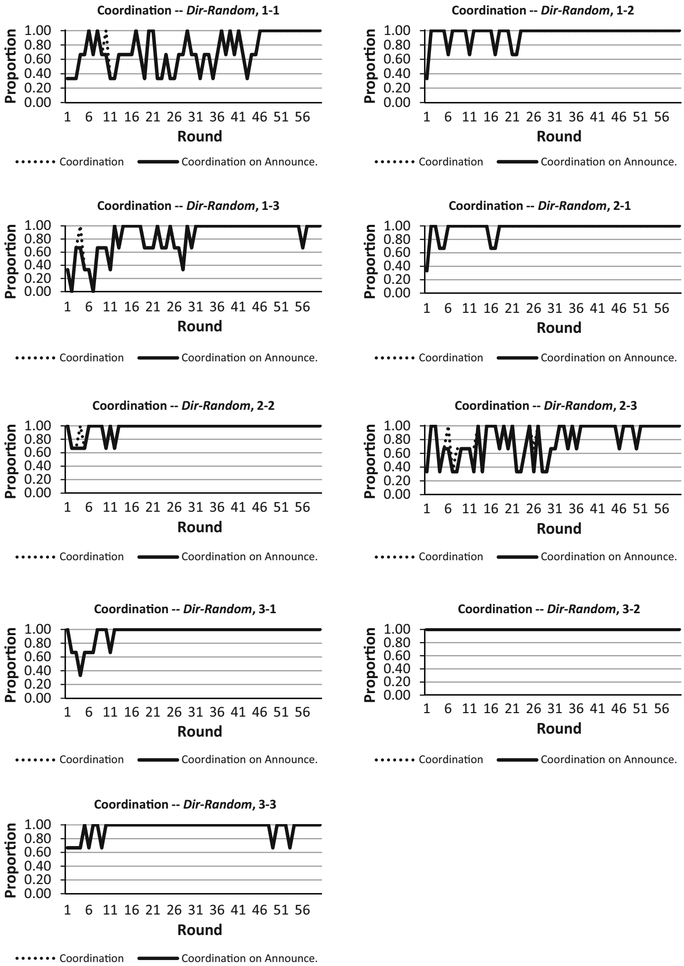

Fig. 6 Group-Level Data, *Dir-Random*. *Note* "Coordination" refers to the frequencies of coordination on either (X, X) or (Y, Y). "Coordination on Announce." refers to the frequencies of coordination on either (X, X) or (Y, Y) conditioned on the endogenously determined mappings from announcements to actions as reported in Table 4

direct versus indirect devices, these choices may also have exaggerated the frequency of coordination on correlated equilibria relative to other, more general settings. We leave such extensions and robustness checks to future research.

Appendix 1—Group-level data, all rounds

See Figs. 6, 7, 8, 9, 10 and 11.

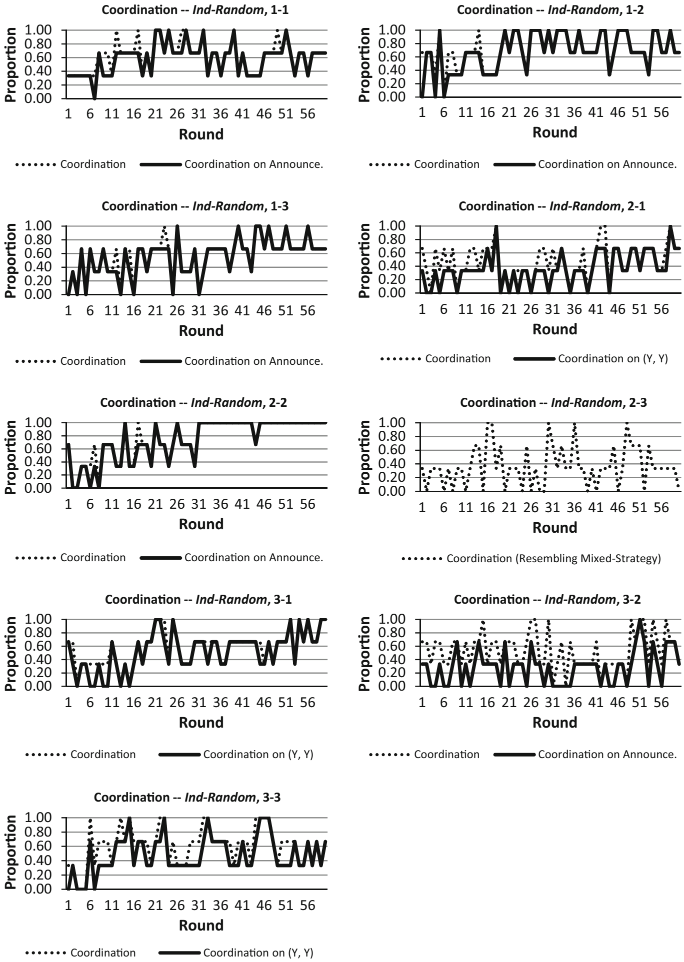

Fig. 7 Group-Level Data, *Ind-Random*. *Note* "Coordination" refers to the frequencies of coordination on either (X, X) or (Y, Y). "Coordination on Announce." refers to the frequencies of coordination on either (X, X) or (Y, Y) conditioned on the endogenously determined mappings from announcements to actions as reported in Table 4. For Observation 2-3, only the unconditional coordination frequencies are plotted, because no endogenous mapping from announcements to actions could be established. For Observations 2-1, 3-1, and 3-3, instead of the frequencies of coordination on announcements, the frequencies of coordination on (Y, Y) are plotted, because there was also no establishable mapping in these observations and (Y, Y) was the most frequent coordination outcome

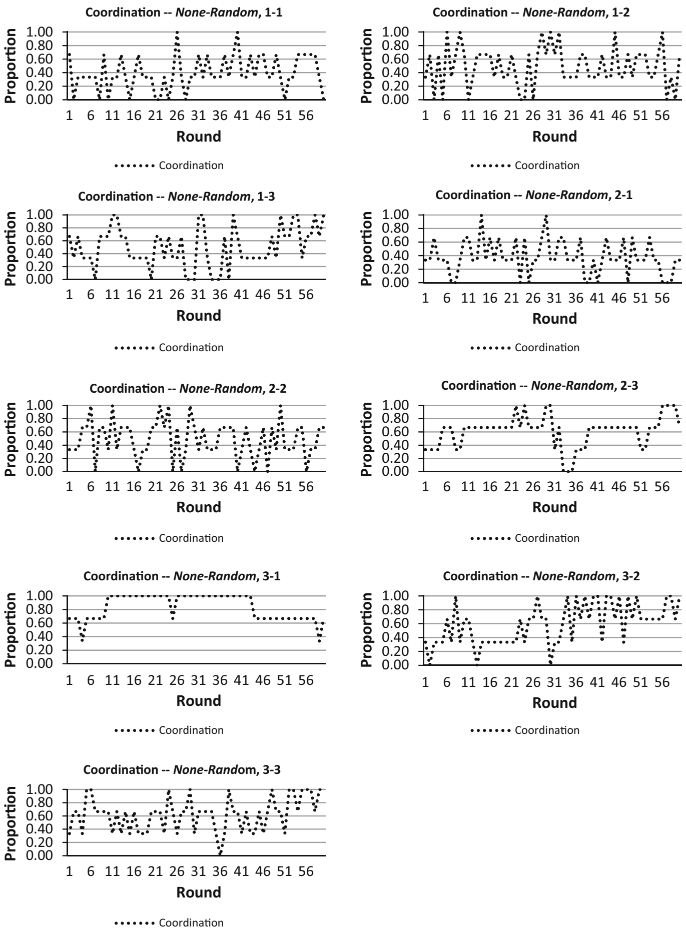

Fig. 8 Group-Level Data, *None-Random*. *Note* "Coordination" refers to the frequencies of coordination on either (X, X) or (Y, Y)

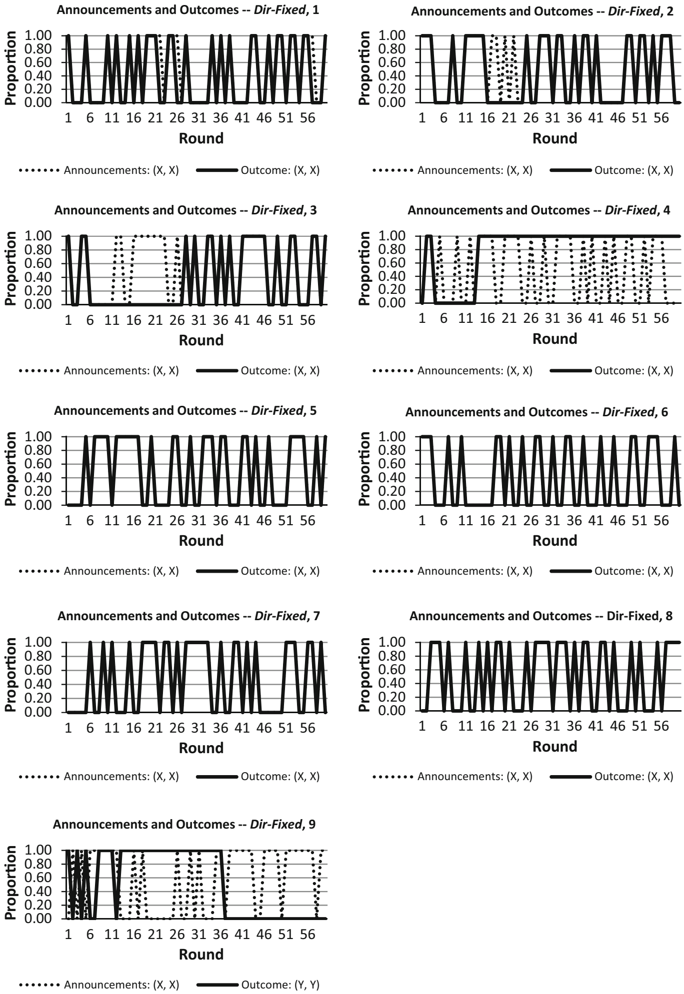

Fig. 9 Group-Level Data, *Dir-Fixed*. *Note* "Announcements: (X, X)" refers to the frequencies of the announcements (X, X). "Outcome: (X, X)" refers to the frequencies of the outcome (X, X). For a fixed pair, these frequencies were either 0 or 1. For Observation 9, the frequencies of (Y, Y) are instead plotted, because no endogenous mapping from announcements to actions could be established and (Y, Y) was the most frequent coordination outcome

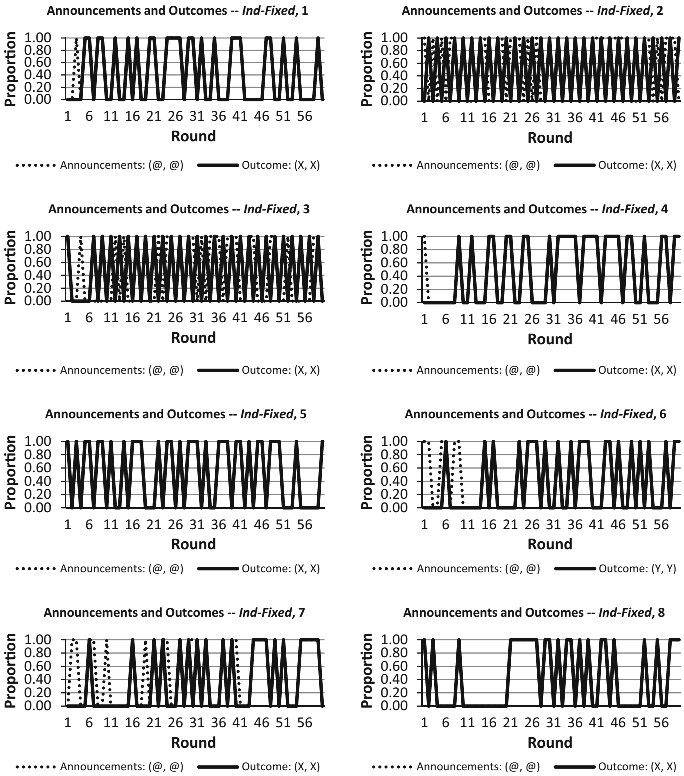

Fig. 10 Group-Level Data, *Ind-Fixed*. *Note* "Announcements: (@, @)" refers to the frequencies of the announcements (@, @). "Outcome: (X, X)" refers to the frequencies of the outcome (X, X). For a fixed pair, these frequencies were either 0 or 1. For Observation 6, the frequencies of (Y, Y) are instead plotted, because the endogenous mapping was established as being from (@, @) to (Y, Y) [and from (#, #) to (X, X)]

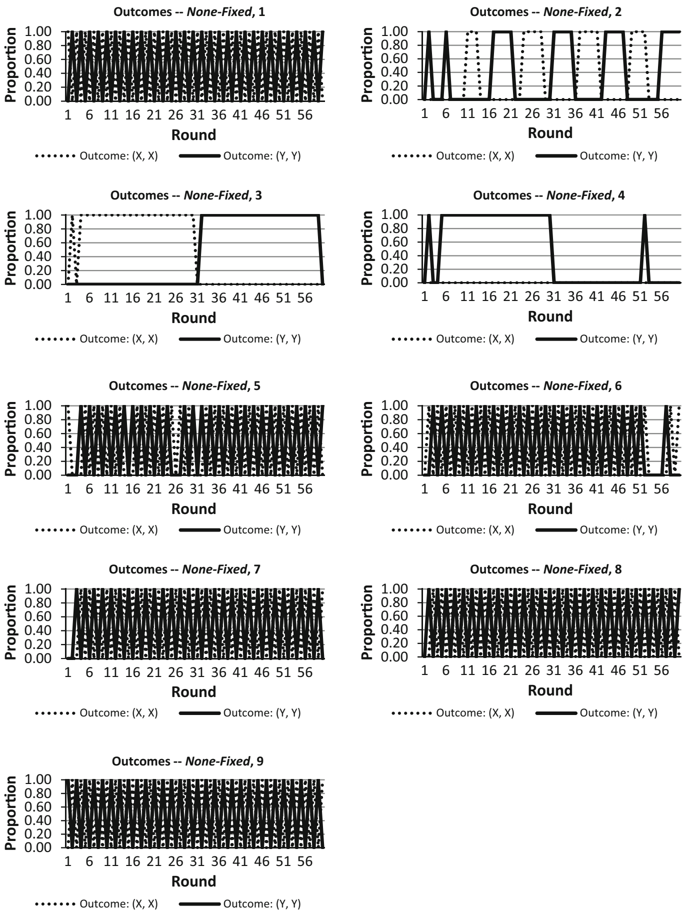

Fig. 11 Group-Level Data, *None-Fixed*. *Note* "Outcome: (X, X)" refers to the frequencies of the outcome (X, X). "Outcome: (Y, Y)" refers to the frequencies of the outcome (Y, Y). For a fixed pair, these frequencies were either 0 or 1

Appendix 2—Experimental Instructions (*Ind-Random*)

Instructions

Welcome to this experiment in the economics of decision-making. Please read these instructions carefully as the cash payment you earn at the end of today's session may depend on how well you understand these instructions. If you have a question at any time, please feel free to ask the experimenter. There is no talking for the duration of this 2-hour session. Please turn off your cell phone and any other electronic devices.

Fig. 12 Your point earnings

Your Role and Decision Group

There are 18 participants in today's session. You will participate in 60 rounds of decision-making using the networked computer workstations of the laboratory. Prior to the first round, one-half of the participants (9) will be randomly assigned the role of *Red Player* and the other half (9) the role of *Blue Player*. Your role as a Red or Blue Player will remain fixed for all 60 rounds.

In each and every round, 1 Red Player and 1 Blue Player will be randomly and anonymously paired to form a group, with a total of 9 groups. Regarding how players are matched, 9 groups are equally divided into 3 classes so that there are 3 groups in each class with 6 participants, 3 Red Players, and 3 Blue Players; in each and every round, you will be randomly matched with a participant in the other role in your class. Thus in a round you will have an equal, 1 in 3 chance of being paired with a participant in the other role in your class. You will not be told the identity of the participant you are matched with, nor will that participant be told your identity—even after the end of the experiment.

Your Decision in Each Round

In each round, you and the participant you are matched with each has to decide which one of two possible actions to choose. The actions are labeled X and Y. The action choices that you and your matched participant make jointly determine the earning (in points) that each of you receive for the round. The following table shows how your point earning is determined, which will be the same for each round (Fig. 12).

In each round, one of the four cells in the above table will be relevant to your point earning in the round ("the relevant cell"). If you are a Red Player, your choice of X or Y will determine which *row* of the table the relevant cell belongs to, and your matched Blue Player's choice of X or Y will determine which *column* the relevant cell belongs to. If you are a Blue Player, the situation is reversed: Your choice of X or Y will determine which *column* of the table the relevant cell belongs to, and your matched Red Player's choice of X or Y will determine which *row* the relevant cell belongs to. In either case, the actions chosen by you and the participant you are matched with determine the relevant cell. The first number in the relevant cell represents Red Player's point earning for the round and the second number represents Blue Player's point earning for the round. The numbers will be displayed in the corresponding colors (Red or Blue) on your computer screen.

Computer Announcements

Before you choose an action in each round, both you and the participant you are matched with will receive an "announcement" by the computer program. These announcement are selected randomly in each round according to the following rules:

- There is a 50 % chance that the computer program will announce @ to the Red Player and @ to the Blue Player.
- There is a 50 % chance that the computer program will announce # to the Red Player and # to the Blue Player.

Thus when you see the announcement for your role (Red or Blue), you will also know the announcement that was made to the other player role (Blue or Red) with whom you are matched. It is up to you whether or not to take the announcements into account when you make your action choices.

Summary of the Experiment

1. At the beginning of each round, you will be randomly matched with another participant in the other role in your class to form a group of two.
2. The computer selects the announcement according to the possibilities specified above. Your screen will display the selected announcement as "The announcement is: ——"
3. Below the announcement, you will be prompted to enter your choice of action, clicking either X or Y.
4. The round is over after you and the participant you are matched with have entered their choices. The computer will provide a summary for the round: the announcement made to you and the participant you are matched with, your choice of action, your matched participant's choice of action, your earning in points, and your matched participant's earning in points.

In all but the final (60th) round, the above steps will be repeated once the round is over.

Your Cash Payment

The experimenter randomly selects 2 rounds out of 60 to calculate your cash payment. (So it is in your best interest to take each round seriously.) The sum of the points you earned in the 2 selected rounds will be converted into cash at an exchange rate of HK$10 per point. Your total cash payment at the end of the experiment will be this cash amount plus a HK$30 show-up fee.

Administration

Your decisions as well as your monetary payment will be kept confidential. Remember that you have to make your decisions entirely on your own; please do not discuss your decisions with any other participants.

Upon finishing the experiment, you will receive your cash payment. You will be asked to sign your name to acknowledge your receipt of the payment. You are then free to leave.

If you have any question, please raise your hand now. We will answer your question individually. If there is no question, we will proceed to the quiz.

Quiz

To ensure your understanding of the instructions, we ask that you complete a short quiz before we move on to play the 60 rounds of the experiment. This quiz is only intended to check your understanding of the written instructions; it will not affect your earnings. If there are mistakes, we will go through the relevant part of the instructions again to make sure that all participants understand the answers to the quiz questions.

1. True or False: I will remain a Red or Blue Player in all 60 rounds of decision-making. Circle one: True / False
2. True or False: I will be matched with the same player in the other role in all 60 rounds. Circle one: True / False
3. True or False: If my computer announcement is @, then the other player's announcement is #. Circle one: True / False
4. What is the chance that you get an announcement of @? __________ What is the chance that you get an announcement of #? ____________
5. True or False: I can see the other player's choice of X or Y before making my own choice of X or Y. Circle one: True / False
6. Suppose you are the Red Player. If you choose X and the Blue Player chooses Y, what is your (the Red Player's) point earning? _________ What is the Blue Player's point earning? _________
7. Suppose you are the Blue Player. If you choose X and the Red Player chooses Y, what is your (the Blue Player's) point earning? _________ What is the Red Player's point earning? _________
8. Suppose that you and the other player both choose X. What is the point earning that each of you earns? _________. Suppose instead that you and the other player both choose Y. What is the point earning that each of you earns? _________
9. True or False: At the end of the experiment, I will be paid my earnings in points from two randomly chosen rounds at the rate of 1 point = HK$10. Circle one: True / False.

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