Language and Coordination: An Experimental Study[*]

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Abstract

We report on an experiment exploring whether and how players may learn to use a random device to coordinate on a correlated equilibrium that Pareto dominates the Nash equilibria of a two-player Battle of the Sexes game. By contrast with other studies exploring recommendations and correlated equilibria, the mapping from the random device to the action space of the game is not necessarily known by subjects *a priori*, which serves to highlight the additional coordination problem that is introduced by the use of such a random device. We find that subjects have an easier time coordinating on the efficient correlated equilibrium of the game when there is a common language mapping from the realizations of the random device to the action space of the game. However, we also find that it is possible for subjects to learn to develop a language mapping from realizations of the random device to the action space of the game when that mapping is not common to begin with. We further find that a random device is more useful as a coordination mechanism when subjects are randomly matched. When subjects are in fixed matches, other strategies that ignore the random device and condition instead on the players' joint history, e.g., "alteration" or "turn-taking" can achieve the same efficient outcome that is made possible by introduction of the random device.

Keywords: Correlated Equilibrium; Coordination; Common Knowledge; Language; Learning; Equilibrium Selection; Experimental Economics.

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> And the Lord said, Behold, the people is one, and they have all one language; and this they begin to do; and now nothing will be restrained from them, which they have imagined to do. Go to, let us go down, and there confound their language, that they may not understand one another's speech.
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> *Genesis 11:6-7*

1 Introduction

Nash equilibrium play often results in inefficient outcomes. The one-shot Prisoners' Dilemma game provides a classic example: the unique, mutual defection equilibrium outcome of that game is strictly Pareto-dominated by the mutual cooperative outcome. Whenever there is the potential for such Pareto improving play one may expect that players will seek ways of transforming the game so as to expand the set of equilibria to include those that achieve more efficient outcomes. Non-binding communication is perhaps the least costly way to augment the strategic environment, providing an additional channel for cooperation. Indeed, casual observation suggests that non-binding communication is one of the most prevalent coordination mechanisms found in many social and economic interactions. While pre-play communication does not always yield improvements over equilibrium outcomes without communication – for example, it should have no effect in the one-shot Prisoner's Dilemma game – Aumann's (1974) notion of correlated equilibrium provides an avenue by which non-binding, *mediated communication* can often lift the restrictions imposed by Nash equilibrium (Myerson, 1985) and result in Pareto-improving outcomes. Indeed, correlated equilibrium is an important generalization of the Nash equilibrium concept, and it came about somewhat later in the development of game theory.[1]

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

Figure 1: Battle of the Sexes Game

Consider the simple Battle of the Sexes Game shown in Figure 1. This game has two pure strategy Nash equilibria – one where both players play X and another where both players play Y and there further exists a unique mixed strategy equilibrium where Player 1 plays X

[1] Indeed, Myerson has reportedly quipped that "If there is intelligent life on other planets, in a majority of them, they would have discovered correlated equilibrium before Nash equilibrium."

with probability $\frac{3}{4}$, while Player 2 plays X with probability $\frac{1}{4}$, each receiving an equilibrium payoff of $\frac{9}{4}$.

Suppose, alternatively, that players coordinate through a neutral third party mediator who makes non-binding "recommendations" about the strategies the two players choose. Suppose further that the mediator draws, with equal probability, one of two recommendation profiles, (X, X) and (Y, Y), and this fact is common knowledge among the players. Upon a realization of the draw, the mediator privately recommends to each player to play his part in the drawn strategy profile. Given this recommendation rule and conditional on the other player following the mediator's recommendation, it can be shown that a player's strict best response is to follow the recommendation. The equilibrium in this expanded game with mediated communication is a correlated equilibrium of the original Battle of the Sexes Game. More importantly, the correlated play results in a Pareto improvement relative to the mixed-strategy Nash equilibrium of the game without communication as both players receive a payoff of 6 in the correlated equilibrium as compared with the payoff of $\frac{9}{4}$ in the mixed strategy Nash equilibrium.

Communication requires a common language as a medium. However, language itself has no place in standard game theory because equilibrium solution concepts are silent about the semantics of the message space. In the current context, the mediator's recommendations are generated in accordance with mappings from the space of strategy profiles to a message space. A correlated equilibrium can be implemented using a myriad of different message spaces, not just those with messages that carry literal meanings such as "I recommend that you play X."[2] The players are assumed to perfectly "understand" the mediator's recommendations not simply because they understand the recommendations (messages) themselves, but because in equilibrium they *know* the mappings from recommendations to strategies. While communication as a coordination device can certainly improve strategic outcomes, it may serve better in theory than in practice as the addition of communication can potentially result in an additional coordination problem as to how individuals decode the messages that they receive (Farrell and Rabin, 1996). The frequency of mishaps due to language misunderstandings ranging from daily interactions between individuals of different skills with a common language to international encounters with individuals speaking different languages are legion and allow one to appreciate the magnitude of this secondary coordination problem. Yet, as it stands, theory does not inform us about the magnitude of the frictions that are often created in the encodings and decodings of language.

In this paper, we use experimental methods to gain some insight into the extent of this secondary coordination problem. We use the mediated communication game described above

[2]This is a defining characteristic of cheap-talk communication (Crawford and Sobel, 1982).

as a platform to experimentally investigate the role of language in coordination.[3] Among other things, we explore and assess the significance of the secondary coordination problem revolving around the use of language. We find that subjects have an easier time coordinating on the efficient correlated equilibrium of the game when there is a common language mapping from the realizations of the random device to the action space of the game. However, we also find that it is possible for subjects to learn to develop a language mapping from realizations of the random device to the action space of the game when that mapping is not common to begin with. We further find that a random device is more useful as a coordination mechanism when subjects are randomly matched as opposed to being in fixed matches, and that under fixed matches subjects are more likely to ignore the random device in favor of history contingent, turn-taking strategies.

1.1 Related Literature

Aumann (1974, 1987) was the first to relax the standard assumption of non-cooperative game theory that players' strategies are probabilistically independent of one another. 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. He showed how if players could condition their strategies on a common, external random device that the resulting set of self-enforcing, mutual best response "correlated equilibria" could lie outside the convex hull of Nash equilibria and in certain cases could Pareto dominate the probabilistically independent Nash equilibria of the same non-cooperative game. The equal probability play of the strategy profiles (X,X) and (Y,Y) in the Battle of the Sexes game as described above is one example of such a Pareto improving correlated equilibrium. Aumann's insight was to note 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 the players' strategies. Myerson (1985, 1994) provided the mediated play version of correlated equilibrium wherein without any loss of generality the external random device takes the form of a centralized and trusted third party mediator who operates the random device and makes *private* recommendations to each player regarding their own part of the recommended strategy profile based on the outcome of the random device.

Prisbrey (1992) was the first to report experimental evidence of players coordinating on efficient, alternation strategies in non-cooperative coordination games played under a fixed

[3] As far as correlated equilibria are concerned, mediated communication is one of many correlation devices that players can use. Other non-human correlation devices, most notably sunspots, include commonly observable signals thought to be generated by nature. For these devices, the issues of decodings matter even more because the signals carry no contextual meaning. See, for example, Duffy and Fisher (2005) for an experimental study on sunspots that addresses the semantics of the signals.

matching protocol but he did not allow or study the role of external announcement mechanisms.[4] The first experiments involving recommended play in non-cooperative games were conducted by Van Huyck et al. (1992), Brandts and Macleod (1995) and Seely et al. (2005). These studies examined whether subjects would follow non-binding public recommendations on actions to play (as made by the experimenter) but the recommendations 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, the focus was on whether public recommendations would by themselves affect equilibrium selection in coordination games. 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 various different types of correlated equilibria in 2×2 coordination games. A main finding from these studies is that recommendations are followed but only imperfectly and decidedly less so when the probability distribution of recommended actions does not comprise a correlated equilibrium of the game or if the recommended correlated equilibrium is payoff dominated by some other equilibrium of the game. A common feature of all of these experiments is that the recommendations, as communicated by the experimenter, are always phrased in terms of the available actions in the game that each individual subject should choose, e.g., in the Battle of Sexes game shown above a subject's recommendation might be of the form: "it is recommended that you play action X." By contrast, in this paper we are interested in the case where recommendations need not be phrased in terms of the available action set but where the "language" mapping from realizations of the random device to the available action space might itself have to be learned.[5]

Experiments by Blume at al. (1998, 2001) and Weber and Camerer (2003) also involve the learning of a language but these studies are conducted in the context of asymmetric information, sender–receiver type setups where there is only one-way communication from an informed sender to an uninformed receiver and which do not involve the use of a random device.[6] Selten and Warglien (2007) also use a sender–receiver setup to investigate how costs and benefits of linguistic communication affect the emergence of language as a coordination device when no common language is available. Relatedly, Hong, Lim and Zhao (2013) explore how different languages emerge and evolve through individuals' optimization process in response to different social environments. Duffy and Fisher (2005) show how the semantics

[4]Recent papers that study turn taking include Lau and Mui (2008, 2012) and Cason, Lau and Mui (2013).

[5]Our other treatment variable is the matching protocol. For a recent paper that studies the effect of matching protocols on learning, see Feltovich and Oda (forthcoming).

[6]There is also a theoretical literature that studies the evolution of meaning in pre-play communication with messages sent by players not a random device/mediator. See, for example, Kim and Sobel (1995) and a more recent development by Demichelis and Weibull (2008).

of messages generated by a random device can matter for equilibrium coordination in a market setting, but they do not provide any evidence that a language mapping from the random message space to the action space can be learned by self-interested individuals. By contrast in this paper we provide a setting under which a language mapping from a random device to the action space of a simple game *can* be learned so that the random device can be used to achieve coordination on a more efficient equilibrium.

Based on theoretical work by Crawford and Haller (1990) and Blume (2000), Blume and Gneezy (2000) experimentally investigate learning in repeated coordination games. Similar to our work, they consider an environment without a common language. However, in their work the absence of a common language pertains to players' inability to distinguish between their roles or their actions. In our paper, players know their roles and actions, and a common language refers to the mapping from the random device to the player' actions in the augmented game. Blume and Gneezy (2000) find that certain environmental conditions, e.g., limits on the cognitive demands placed on players and the information available to them, can foster learning of the optimal strategy. Similarly, we find that certain matching protocols, e.g., random matching can foster the learning of a language mapping from a random device to the action space of the game when no such common language exists at the outset of play.

2 Correlated Equilibria and the Role of Language

We use the Battle of the Sexes Game, as shown earlier in Figure 1, as the platform to experimentally investigate the role of language in the achievement of correlated equilibria. We use this game as we view it as the simplest coordination game in which correlated strategies can yield improvements over (mixed) Nash equilibrium play. As noted in the previous section there are two equivalent approaches to defining correlated equilibrium. One approach, as in the original formulation of Aumann (1974), involves the use of an external random device. The other approach involves a mediator making direct, non-binding recommendations (Myerson, 1994). Leveraging on a version of the revelation principle, the mediator approach provides a powerful and parsimonious route to define and characterize equilibria. The random device approach, on the other hand, is better suited to elucidate the language issues that are the focus of this paper. In this section, we first characterize the correlated equilibria of the Battle of the Sexes Game using the mediator approach. We then define a random device and reformulate the Battle of the Sexes game using that random device. Using the reformulated game, we discuss within and outside the game the issues of language, setting the stage for our experimental treatments.

The set of pure strategy profiles of the Battle of the Sexes Game is $C = \{\mathrm{X}, \mathrm{Y}\} \times \{\mathrm{X}, \mathrm{Y}\}$.

Consider a mediator who communicates with each player separately and privately about which strategy in $\{\mathrm{X}, \mathrm{Y}\}$ is recommended. The mediator determines her recommendations according to the commonly known probability distribution $p \in \Delta(C)$. We say that p is a correlated equilibrium if each player prefers to follow the recommendation for him than to not follow it. Suppose Player 1 believes that Player 2 follows the mediator's recommendation. Player 1's conditional expected payoff from following the recommendation X is

$$\frac{p(\mathrm{X},\mathrm{X})}{p(\mathrm{X},\mathrm{X})+p(\mathrm{X},\mathrm{Y})} \times 9 + \frac{p(\mathrm{X},\mathrm{Y})}{p(\mathrm{X},\mathrm{X})+p(\mathrm{X},\mathrm{Y})} \times 0.$$

If Player 1 chooses Y instead, his conditional expected payoff will be

$$\frac{p(\mathrm{X},\mathrm{X})}{p(\mathrm{X},\mathrm{X})+p(\mathrm{X},\mathrm{Y})} \times 0 + \frac{p(\mathrm{X},\mathrm{Y})}{p(\mathrm{X},\mathrm{X})+p(\mathrm{X},\mathrm{Y})} \times 3.$$

Player 1 thus prefers to follow the recommendation if and only if $3p(\mathrm{X},\mathrm{X}) \geq p(\mathrm{X},\mathrm{Y})$. Analyzing other cases in a similar fashion, we obtain the following characterization:

Proposition 1. *Suppose a mediator separately and privately recommends a strategy to each player in the Battle of the Sexes Game according to the commonly known probability distribution $p \in \Delta(C)$, where $C = \{\mathrm{X},\mathrm{Y}\} \times \{\mathrm{X},\mathrm{Y}\}$. Then, p constitutes a correlated equilibrium if and only if it satisfies the following incentive constraints:*

$$3p(\mathrm{X},\mathrm{X}) \geq p(\mathrm{X},\mathrm{Y}), \tag{1}$$

$$p(\mathrm{Y},\mathrm{Y}) \geq 3p(\mathrm{Y},\mathrm{X}), \tag{2}$$

$$p(\mathrm{X},\mathrm{X}) \geq 3p(\mathrm{Y},\mathrm{X}), \tag{3}$$

$$3p(\mathrm{Y},\mathrm{Y}) \geq p(\mathrm{X},\mathrm{Y}). \tag{4}$$

Note that the Nash equilibria of the game are also correlated equilibria: the pure-strategy Nash equilibrium (X,X) is obtained with $p(\mathrm{X},\mathrm{X}) = 1$; (Y,Y) is obtained with $p(\mathrm{Y},\mathrm{Y}) = 1$; and the mixed-strategy equilibrium in which Player 1 plays X with probability $\frac{3}{4}$ and Player 2 plays X with probability $\frac{1}{4}$ is obtained with $p(\mathrm{X},\mathrm{X}) = p(\mathrm{Y},\mathrm{Y}) = \frac{3}{16}$, $p(\mathrm{X},\mathrm{Y}) = \frac{9}{16}$ and $p(\mathrm{Y},\mathrm{X}) = \frac{1}{16}$. Payoffs in the three Nash equilibria are, respectively, $(9,3)$, $(3,9)$ and $(\frac{9}{4},\frac{9}{4})$. As noted earlier, an example of a non-Nash correlated equilibria is $p(\mathrm{X},\mathrm{X}) = p(\mathrm{Y},\mathrm{Y}) = \frac{1}{2}$, which, with payoffs $(6,6)$, Pareto-dominates the mixed-strategy equilibrium. Thus we see how the set of correlated equilibria enlarges the set of possible equilibria and is thus a generalization of the Nash equilibrium concept.

While any distribution over C that satisfies the incentive constraints (1)-(4) can be supported as a correlated equilibrium, a mediator making direct recommendations is but one of the many ways to induce these distributions. There are an infinite number of random devices that can implement the same correlated equilibria. While any such device can be reduced to a mediator under the revelation principle and therefore considerations of individual devices are inconsequential as far as equilibrium characterizations are concerned, the nature of the coordination problem may be more demanding under one random device than under another. It is our contention that in the laboratory, where players may fall short of theoretical ideals, an important property that has decisive influence on the coordinating role of any particular random device is the language of that device.

To further elucidate the issues, we formally introduce a random device into the game. A random device is an information structure identified by a triple, $\Gamma = (\Omega, \{H_i\}, \pi)$, where Ω is a finite state space corresponding to the outcomes of the device, H_i is Player i's information partition of Ω, and π is a probability measure on Ω. We consider a class of devices $\mathcal{G}$ with the following profile of state space and partitions:

1. A state space with four elements, $\Omega = \{\omega_1, \omega_2, \omega_3, \omega_4\}$.
2. Player 1's information partition is $H_1 = \{\{\omega_1, \omega_2\}, \{\omega_3, \omega_4\}\}$ and Player 2's is $H_2 = \{\{\omega_1, \omega_3\}, \{\omega_2, \omega_4\}\}$.

A device within the class $\mathcal{G}$ differs from another device only by the labels of the states and we use ω_i here to represent generic labels. In the Battle of the Sexes Game augmented with one of such commonly known devices, a pure strategy of Player i is $\sigma_i : H_i \to \{\mathrm{X}, \mathrm{Y}\}$, $i = 1, 2$. Given Proposition 1, the following characterization is immediate:

Proposition 2. *Strategy profile* $\sigma_1(\{\omega_1, \omega_2\}) = \sigma_2(\{\omega_1, \omega_3\}) = \mathrm{X}$ *and* $\sigma_1(\{\omega_3, \omega_4\}) = \sigma_2(\{\omega_2, \omega_4\}) = \mathrm{Y}$ *constitutes a correlated equilibrium of the Battle of the Sexes Game with a random device in the class* $\mathcal{G}$ *if and only if*

$$3\pi(\omega_1) \geq \pi(\omega_2), \tag{5}$$
$$\pi(\omega_4) \geq 3\pi(\omega_3), \tag{6}$$
$$\pi(\omega_1) \geq 3\pi(\omega_3), \tag{7}$$
$$3\pi(\omega_4) \geq \pi(\omega_2). \tag{8}$$

For π' *satisfying restrictions similar to* (5)-(8) *but with* ω_1 *interchanged with* ω_4 *and* ω_2 *interchanged with* ω_3*, an alternative strategy profile* $\sigma'_1(\{\omega_1, \omega_2\}) = \sigma'_2(\{\omega_1, \omega_3\}) = \mathrm{Y}$ *and* $\sigma'_1(\{\omega_3, \omega_4\}) = \sigma'_2(\{\omega_2, \omega_4\}) = \mathrm{X}$ *also constitutes a correlated equilibrium of the game.*

Given these strategy profiles, a comparison of the restrictions on the probability distributions in (5)-(8) with the incentive constraints in (1)-(4) makes it clear that the set of all correlated equilibrium outcomes can be implemented with the class of random devices $\mathcal{G}$ (and many others not under scrutiny). While this is nothing but a straightforward consequence of the revelation principle, Proposition 2 (in light of Proposition 1) provides an important theoretical benchmark for our experimental inquiry. It suggests that the set of correlated equilibrium outcomes is invariant to the labels used by a random device; the revelation principle, or more generally the notion of equilibrium, suggests that language does *not* play a role in the theoretical regime.

To articulate on the role of language that may arise in the laboratory, we depart from pure equilibrium considerations. Consider two random devices within the class $\mathcal{G}$, Γ_{Comm} and Γ_{Neo}, with respective labels of the states:

1. $\omega_1 = (\mathrm{X}, \mathrm{X})$, $\omega_2 = (\mathrm{X}, \mathrm{Y})$, $\omega_3 = (\mathrm{Y}, \mathrm{X})$, $\omega_4 = (\mathrm{Y}, \mathrm{Y})$.

2. $\omega_1 = (@, @)$, $\omega_2 = (@, \#)$, $\omega_3 = (\#, @)$, $\omega_4 = (\#, \#)$.

Device Γ_{Comm}, which we call the *common-language* device, is not too different from a mediator. The labels of the device coincide with the labels of the players' pure strategies. Device Γ_{Neo}, which we call the *neologism* device, uses labels that have no immediate association with the strategy labels. Given that the devices are elements of the class considered in Proposition 2, it is clear that whether a device involves a common language or a neologism is of no equilibrium consequence. However, equilibrium does impose the rather stringent requirement that each player correctly anticipates what his opponent will do. In the correlated equilibrium under study, this means that Player i knows correctly how Player j interprets and acts on the realized outcome of the random device. The notion of equilibrium is silent on the process by which players acquire such knowledge and factors that are not theoretically essential may play a role once we depart from theoretical ideals.

It is reasonable to expect that the common language in Γ_{Comm} provides a focal point that facilitates the formation of correct anticipations and thus coordination among the players. In the absence of any semantics meaningful to the environment, on the other hand, the neologism in Γ_{Neo} not only may not facilitate coordination but may further introduce another coordination problem on top of the original coordination problem. Motivated by this observation, we designed an experiment that allows us to examine the role of language in facilitating coordination in the laboratory.

3 Experimental Design

3.1 Design, Treatments, and Hypotheses

Among the set of correlated equilibrium outcomes, we implement the one in which only (X, X) and (Y, Y) are played, each with probability $\frac{1}{2}$. In the notation of the class of generic devices, $\mathcal{G}$, considered in the previous section, this correlated equilibrium is induced by the probabilities $p(\omega_1) = p(\omega_4) = \frac{1}{2}$. Since the probabilities of ω_2 and ω_3 are degenerate for our purpose the information partitions of the players can be simplified to $H_1 = H_2 = \{\{\omega_1\}, \{\omega_4\}\}$. We choose this particular correlated equilibrium for our experiment mainly because of its simplicity. This outcome is also the best among those that are fair (i.e., players receive the same expected payoffs), and it is a Pareto improvement relative to the mixed-strategy Nash equilibrium of the game.[7] We wish to provide subjects with the strongest incentives to condition their decisions on the random device so that we can focus on the question of whether and how they make use of the random device. If subjects are unable to condition on the random device in this simplest and highly incentivized setting, then it seems unlikely that they would be able to do so in more complicated settings and/or ones with weaker incentives.

Our major treatment variable corresponds to the two random devices, Γ_{Comm} and Γ_{Neo}, with corresponding simplified partitions $H_{1Comm} = H_{2Comm} = \{\{\mathrm{X}\}, \{\mathrm{Y}\}\}$ and $H_{1Neo} = H_{2Neo} = \{\{@\}, \{\#\}\}$. Note the difference between the two is that under the common language random device, recommendations are made in terms of the available stage game strategies; a recommendation of X should be commonly understood to mean playing the stage game strategy X. By contrast, under the neologism random device it is not immediately clear how to use a recommendation of @ or # to select a stage game strategy; if a common mapping from such recommendations to stage game strategies is adopted, then the random device Γ_{Neo} can be just as effective at coordinating activity as the random device Γ_{Comm}.[8] We also include a control treatment in which a Battle of the Sexes Game is played without the use of any random device. Finally, we note that repeated interactions with the same "partner" may facilitate the evolution of common understanding and adoption of language relative to interactions with randomly matched "strangers." Matching protocols thus serve as an important, additional treatment variable.[9] Therefore we adopt a 2×3 design (Table 1). The

[7]Thus, this outcome should be robust to subjects with other-regarding preferences for fairness or altruism. Further this outcome also survives the axiomatic refinements proposed by Prisbrey (1992).

[8]We chose @ and # for Γ_{Neo} as these are universally common (ASCII), printable, 1-character symbols (allowing others to replicate our design) that do not have any meaning with regard to the available stage game strategy choices which are labeled X and Y.

[9]Duffy and Ochs (2009) demonstrate the importance of fixed partner matching relative to random stranger

details of the experimental procedures, including how we implement the random device, will be covered in the next subsection.

Table 1: Experimental Treatments

Matching Protocol	Random Device		
	None	Neologism	Common Language
Partner (Fixed Matching)	*NoneFixed*	*NeoFixed*	*CommFixed*
Stranger (Random Matching)	*NoneRandom*	*NeoRandom*	*CommRandom*

Our experimental hypotheses concern the relative ease (or difficulty) with which subjects coordinate their actions in the Battle of the Sexes game under different language environments, interacted with whether subjects are facing random strangers or fixed partners. We denote the frequency of coordination (i.e., the play of either (X,X) or (Y,Y)) observed in treatment T by $F(T)$. Our first, major hypothesis reads:

Hypothesis 1. *The existence of random devices is conducive to greater coordination than in the case of no random device, and a common language random device is more conducive to coordination than is the neologism random device:*

1. $F(CommRandom) > F(NeoRandom) > F(NoneRandom)$ *and*

2. $F(CommFixed) > F(NeoFixed) > F(NoneFixed)$.

Hypothesis 1 compares across the columns of Table 1, holding the matching protocol constant. Our second hypothesis compares, holding the language environment constant, across the rows of the treatment table:

Hypothesis 2. *Fixed matching is more conducive to coordination than is random matching:*

1. $F(CommFixed) > F(CommRandom)$,

2. $F(NeoFixed) > F(NeoRandom)$ *and*

3. $F(NoneFixed) > F(NoneRandom)$.

Note that the same coordination outcome can be induced by $p(\omega_1) = p(\omega_4) = \frac{1}{2}$ under the two alternative strategies in Proposition 2. This suggests that different uses of language are

matching for coordination on Pareto superior outcomes in a repeated Prisoner's Dilemma game.

consistent with the same coordination outcome. Given the strong focal salience of a common language, we do not expect to see in *CommonFixed* and *CommRandom* that players coordinate by choosing X after observing {Y} and Y after observing {X} though this is certainly another equilibrium possibility. On the other hand, it is much more likely for different uses of language (or mappings) to emerge in the *NeoFixed* and *NeoRandom* treatments, i.e., players may coordinate on choosing either X or Y after observing either @ or #. These observations give rise to our third hypothesis:

Hypothesis 3. *Conditioned on observations of coordination, different uses of language are more prevalent under the neologism device than under the common language device.*

Finally, we note that the fixed partner matching protocol provides an alternative means for coordination that is independent of the random device but that still achieves the efficient outcome, namely perfect alternation of actions or coordinated turn–taking, e.g., the outcome sequence (X, X), (Y, Y), (X, X), ... (other variants of the alternation strategy are also possible). Such turn-taking behavior is distinguished from use of the random device in that a pattern of alternation is *deterministic* while use of the random device necessarily results in a sequence of coordinated outcomes that are perfectly correlated with realizations of the random device.[10] By contrast, under random matchings, history-dependent strategies (such as alternation) are not available and thus conditioning behavior on realizations of the random device provides players with the best opportunity for Pareto-improving play relative to the mixed strategy Nash equilibrium of the game.

Hypothesis 4. *Conditioned on observations of coordination, coordinated play not involving use of the random device is more prevalent under fixed matches than under random matches.*

3.2 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. Appendix B provides the instructions used in the *NeoRandom* treatment session; instructions for the other treatments are similar. To ensure that the information contained in the instructions was induced as

[10]See Vanderschraaf and Skyrms (2003) for a further discussion of the distinction between the alternating (turn-taking) equilibrium and a correlated equilibrium as implemented by a random device and how the alternating equilibrium can be learned under a fixed matching protocol using a modified (Markov) version of fictitious play.

public knowledge, the instructions were read aloud, accompanied by slide illustrations and a comprehension quiz.

Three sessions were conducted for each of the three *random* matching treatments. Three matching groups participated in each of these sessions. 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 of the random matching treatment as an independent observation, we thus have nine observations for each of the three random matching treatments. For each of the three *fixed* matching treatments, one session was conducted with nine (*CommFixed* and *NoneFixed*) or eight (*NeoFixed*) groups of two subjects (fixed pairs). Again, viewing each matching group as an independent observation, we thus have nine or eight observations for each of our three fixed matching treatments.

In all sessions, subjects participated in 60 rounds of play under a single treatment condition (we used a between–subjects design). At the beginning of a session, half of the subjects were randomly labeled as Red Players and the other half were labeled as Blue Players. The role designation remained fixed throughout the session. Using the *NeoRandom* treatment as an illustration, in each round one Red Player was randomly paired with one Blue Player within their matching group. Subjects were presented with a colored version of the payoff table (see Figure 11 in Appendix B). 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 of the payoff table and that their choices would jointly determine their earnings for the round according to the numbers given in the payoff table.

We implemented both random devices as "computer announcements." Subjects in the *Comm* treatments were told that, at the beginning of each round there was a 50% chance the computer program would announce X to both players and there was a 50% chance that the computer program would announce Y to both players. Similarly, in the *Neo* treatments, subjects were instructed that at the beginning of each round there was a 50% chance the computer program would announce @ to both players and there was a 50% chance that the computer program would announce # to both players. We were careful to avoid any use of the word "recommendation" or to suggest in any way that the computer announcement could or should be used as a coordination device by the subjects; in this way we depart to some extent from the third party recommendations design used in prior experimental studies. After seeing the computer announcements, subjects made their row/column choices. Feedback was provided at the end of each round, including information on the announcement, the choices of both players, and the earnings of both players for the round.

We randomly selected two rounds out of the 60 total rounds that were played for each subject's payment. The sum of the payoffs a subject earned in the two randomly selected rounds was converted into Hong Kong Dollars at a fixed and known exchange rate of HK$10 per point. A show-up fee of HK$30 was also provided. Subjects on average earned HK$119.38.[11] A session on average lasted for about one hour.

4 Experimental Findings

We report our experimental results as a number of different findings that address our hypotheses as set forth in the previous section. Our first two results concern the unconditional frequency of coordination on the non-zero payoff outcomes, (X,X) and (Y,Y), which we simply refer to as the frequency of coordination. We consider the frequency of coordination unconditional on any realizations of the random device so that we can make comparisons across all three treatments, including the *NoneRandom* and *NoneFixed* treatments where no random device was available. Later, we will consider frequencies of coordination conditional on realizations of the random device.

4.1 Unconditional Coordination

Finding 1. *Consistent with Hypothesis 1, we find that* $F(CommRandom) > F(NeoRandom) > F(NoneRandom)$.

Support for Finding 1 is found in Table 2, where we see that, averaging across all independent observations (matching groups) coordination frequencies over all rounds or over the last 10 rounds are highest in treatment *CommRandom* and lowest in treatment *NoneRandom*, and are intermediate in *NeoRandom*. Using the nine independent group-level average coordination frequencies, a nonparametric Mann-Whitney test reveals that we can reject the null hypothesis of no difference in coordination frequencies between *CommRandom* and *NeoRandom* or between *CommRandom* and *NoneRandom* in favor of the alternative that coordination was greater in *CommRandom* ($p < .01$ for both tests). On the other hand, applying the same test we cannot reject the null hypothesis of no difference in coordination frequencies between the *NeoRandom* and *NoneRandom* treatments ($p = .63$).[12] These same conclusions continue to hold if attention is restricted to the coordination frequencies over the final 10 rounds of each session. These differences in coordination frequencies are also reflected in payoff differences. Thus it appears that, under random matching, a random

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

[12] A Kruskal-Wallis test also confirms that the frequencies significantly differ in at least one comparison ($p < .01$).

device where the language mapping from realizations to strategies is commonly understood is efficiency enhancing relative to the absence of such a device.

Finding 2. *Inconsistent with Hypothesis 1, we find no consistent differences in F(CommFixed), F(NeoFixed) or F(NoneFixed).*

Support for Finding 2 comes from Table 3 where we see that, over all rounds or over just the last 10 rounds, the frequency of coordination is greatest in the *NeoFixed* treatment and is second best in either *NoneFixed* or *CommFixed* depending on whether attention is restricted to all rounds or the final 10 rounds. Using the independent group-level averages for each treatment, Mann-Whitney tests reveal that we cannot reject the null hypothesis of no difference in all pairwise comparisons of coordination frequencies across the three fixed matching treatments over all rounds or over the last 10 rounds ($p > .10$ in all six tests). The absence of differences in coordination frequencies across the fixed matching treatments is again reflected in an absence of payoff differences across those three treatments. We observe that in all fixed matching treatments, coordination is surprisingly high even without the random device – it averages 86.9 percent in the *NoneFixed* treatment – suggesting that there may be little room for random devices to yield significant improvements in coordinating play of the game under a fixed matching (partner) protocol.

We next compare the impact of the two matching protocols on the three communication treatment conditions.

Finding 3. *Consistent with Hypothesis 2, we find that $F(NeoFixed) > F(NeoRandom)$ and $F(NoneFixed) > F(NoneRandom)$. However, inconsistent with Hypothesis 2, there is no significant difference between F(CommFixed) and F(CommRandom).*

Support for Finding 3 comes again from the coordination frequencies reported in Tables 2-3. Using these group-level observations over all rounds or the last 10 rounds, a Mann-Whitney test reveals that we can reject the null of no difference in coordination frequencies between the *NeoFixed* and *NeoRandom* treatments ($p < .01$) and between the *NoneFixed* and *NoneRandom* treatments ($p < .05$) but not between the *CommFixed* and *CommRandom* treatments ($p = .96$ using data from all rounds, $p = .80$ for data from the last 10 rounds).

Summarizing our results to this point, we find that under a random matching protocol the presence of a common-language random device is efficiency enhancing relative to a neologism random device or to the absence of any such device. In the absence of a common-language random device we find that fixed matchings are efficiency enhancing relative to random matchings under both the neologism or no random device treatments. Finally, under a fixed matching protocol, the presence or absence of a common-language or neologism random device seems to make little difference for efficiency relative to the baseline case of no device.

CommRandom Session-Group	F(Coord) All Rnds	Avg Payoff All Rnds	F(Coord) Last 10 Rnds	Avg Payoff Last 10 Rnds
1-1	0.733	4.40	1.000	6.00
1-2	0.961	5.77	1.000	6.00
1-3	0.833	5.00	0.967	5.80
2-1	0.967	5.80	1.000	6.00
2-2	0.972	5.83	1.000	6.00
2-3	0.822	4.93	1.000	6.00
3-1	0.956	5.73	1.000	6.00
3-2	1.000	6.00	1.000	6.00
3-3	0.956	5.73	0.967	5.80
Mean	0.911	5.47	0.993	5.96
NeoRandom Session-Group	F(Coord) All Rnds	Avg Payoff All Rnds	F(Coord) Last 10 Rnds	Avg Payoff Last 10 Rnds
1-1	0.622	3.73	0.633	3.80
1-2	0.717	4.30	0.733	4.40
1-3	0.567	3.40	0.733	4.40
2-1	0.472	2.83	0.600	3.60
2-2	0.761	4.57	1.000	6.00
2-3	0.367	2.20	0.333	2.00
3-1	0.572	3.43	0.800	4.80
3-2	0.533	3.20	0.700	4.20
3-3	0.606	3.63	0.533	3.20
Mean	0.580	3.48	0.674	4.04
NoneRandom Session-Group	F(Coord) All Rnds	Avg Payoff All Rnds	F(Coord) Last 10 Rnds	Avg Payoff Last 10 Rnds
1-1	0.394	2.37	0.433	2.60
1-2	0.494	2.97	0.467	2.80
1-3	0.500	3.00	0.767	4.60
2-1	0.378	2.27	0.267	1.60
2-2	0.489	2.93	0.433	2.60
2-3	0.611	3.67	0.733	4.40
3-1	0.839	5.03	0.633	3.80
3-2	0.594	3.57	0.767	4.60
3-3	0.628	3.77	0.867	5.20
Mean	0.548	3.29	0.596	3.58

Table 2: Frequencies of Coordination on (X,X) or (Y,Y) and Payoffs, All Rounds and Last 10 Rounds of All Three Random (Stranger) Matching Treatments

CommFixed Group	F(Coord) All Rnds	Avg Payoff All Rnds	F(Coord) Last 10 Rnds	Avg Payoff Last 10 Rnds
1	0.850	5.10	0.900	5.40
2	0.850	5.10	1.000	6.00
3	0.683	4.10	1.000	6.00
4	0.817	4.90	1.000	6.00
5	1.000	6.00	1.000	6.00
6	1.000	6.00	1.000	6.00
7	1.000	6.00	1.000	6.00
8	1.000	6.00	1.000	6.00
9	0.517	3.10	0.000	0.00
Mean	0.857	5.14	0.878	5.27
NeoFixed Group	F(Coord) All Rnds	Avg Payoff All Rnds	F(Coord) Last 10 Rnds	Avg Payoff Last 10 Rnds
1	0.933	5.60	1.000	6.00
2	0.983	5.90	1.000	6.00
3	0.950	5.70	1.000	6.00
4	0.967	5.80	1.000	6.00
5	1.000	6.00	1.000	6.00
6	0.867	5.20	1.000	6.00
7	0.617	3.70	1.000	6.00
8	1.000	6.00	1.000	6.00
Mean	0.915	5.49	1.000	6.00
NoneFixed Group	F(Coord) All Rnds	Avg Payoff All Rnds	F(Coord) Last 10 Rnds	Avg Payoff Last 10 Rnds
1	1.000	6.00	1.000	6.00
2	0.650	3.90	0.700	4.20
3	0.933	5.60	0.900	5.40
4	0.467	2.80	0.100	0.60
5	0.917	5.50	1.000	6.00
6	0.900	5.40	0.500	3.00
7	0.967	5.80	1.000	6.00
8	0.983	5.90	1.000	6.00
9	1.000	6.00	1.000	6.00
Mean	0.869	5.21	0.800	4.80

Table 3: Frequencies of Coordination on (X,X) or (Y,Y) and Payoffs, All Rounds and Last 10 Rounds of All Three Fixed (Partner) Matching Treatments

4.2 Coordination Conditioned on Realizations of Random Device

We next turn to an analysis of whether and how subjects conditioned their play on realizations of the random device in our common language and neologism treatments. Tables 4-5 report on how random announcements of either (X,X) or (Y,Y) in the *Comm* treatments or random announcement of (@,@) or (#,#) in the *Neo* treatments impacted on the four possible game outcomes over all rounds.

Treat/Obs	Outcomes Conditional on Announcements								Endog. Meaning of Announcements		F(Coord.) on Announcements	
CommRandom	(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	L 10 Rnds
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
	Outcomes Conditional on Announcements								Endog. Meaning of Announcements		F(Coord) on Announcements	
NeoRandom	(@,@)				(#,#)							
Session-Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(@, @)	(#, #)	All Rnds	L 10 Rnds
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
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

Table 4: Coordination and Meaning of Announcements, Random Matching Treatments

Treat/Obs	Outcomes Conditional on Announcements								Endog. Meaning of Announcements		F(Coord.) on Announcements	
CommFixed	(X,X)				(Y,Y)							
Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(Y, Y)	All Rnds	L 10 Rnds
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
	Outcomes Conditional on Announcements								Endog. Meaning of Announcements		F(Coord) on Announcements	
NeoFixed	(@,@)				(#,#)							
Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(X, X)	(X, Y)	(Y, X)	(Y, Y)	(@, @)	(#, #)	All Rnds	L 10 Rnds
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
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

Table 5: Coordination and Meaning of Announcements, Fixed Matching Treatments

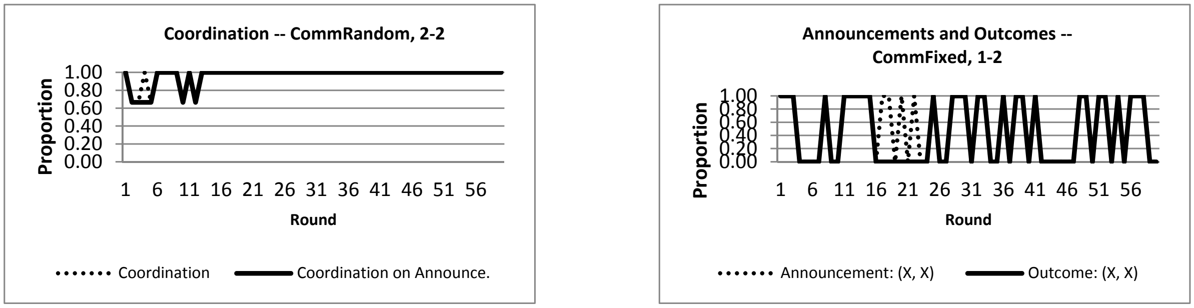

Figure 2: Coordination on Announcements with a Common Language: Two Representative Sessions from the *CommRandom* (left) and *CommFixed* (right) treatments.

These tables also indicate whether or not an endogenously determined meaning of the announcements (Endog. Meaning of Announcements) was achieved by subjects based on the most frequent coordination outcomes given realized announcements in the last 10 rounds of play.[13] If a mapping could be endogenously determined, that mapping is reported, e.g., (X,X) was the most frequently played strategy profile in response to announcement, (@,@). Otherwise, if no mapping from announcements to stage game actions could be ascertained, then the endogenous meaning of messages is reported as "N/A" (not applicable).

Finally, using the endogenously determined meanings of announcements, Tables 4-5 also report on the frequency of coordination on announcements over all rounds (All Rnds) or over the last 10 rounds (L 10 Rnds). For example, in *NeoRandom* observation 1-1, the announcement (@,@) was most frequently associated with the outcome (X,X), while the announcement (#,#) was most frequently associated with the outcome (Y,Y). The coordination frequencies report the percentage of the time that (@,@) resulted in outcome (X,X) and the percentage of the time that (#,#) resulted in play of (Y,Y). Cases where endogenous meanings could not be established are marked as N/A, in which case the frequency of coordination on announcements is reported as zero.

Tables 4-5 reveal that under the *Comm* treatments, the meaning of announcements, when it can be classified, is always "literal" in the sense that announcements of (X,X) most frequently result in play of strategy X by both players while announcements of (Y,Y) most frequently result in play of strategy Y by both players. This literal mapping holds true in both the fixed and random matching versions of the common language treatment. The left panel of Figure 2 shows a representative observation from the *CommRandom* treatment while the right panel of Figure 2 shows a representative observation from the *CommFixed*

[13]The mapping of messages to stage game strategies was determined by us, based on the associations between messages and stage game strategies played over last 10 rounds of play. We note that there is no difference in these endogenously determined meanings if we instead considered the frequencies of outcomes given realized announcements over last 30 rounds.

treatment. Recall that in the *Random* treatments, each session (observation) involved random matching of the 6 subjects into 3 pairs each period so that the period coordination frequencies can be 1/3, 2/3 or 1. By contrast, in the *Fixed* treatment, each observation consists of a single pair of subjects interacting together for all 60 rounds. Thus the left panel of Figure 2 shows that all randomly formed pairs of subjects learn to coordinate quickly on announcements (which may be different for different pairs and are thus not shown). In the right panel of Figure 2 there is a single fixed match pair of subjects and it is seen that the correlation between the announced play of (X,X) and the outcome (X,X) is very high (though imperfect).

The two exceptions to the observation that players in the *Comm* treatments use the natural language encoding of the announcements to coordinate their actions occur under the *CommFixed* treatment (observations 2-1 and 3-3) where the two players coordinated for a sustained period of time on one of the two pure strategies, (X,X) for the pair in observation 2-1 and (Y,Y) for the pair in observation 3-3, ignoring announcements. By contrast, under the *CommRandom* treatment, we can always associate announcements of (X,X) or (Y,Y) with frequent play of X or of Y by both players, as the random matching friction does not allow players to develop reputations for the play of any single strategy.

In the *Neo* treatments, the endogenous meaning of announcements, when they can be classified, is more varied than in the *Comm* treatments. As Tables 4-5 reveal, the message (@,@) is most commonly classified to mean play of (X,X) while the message (#,#) is most commonly classified to mean play of (Y,Y). Specifically, this mapping is observed in 3 out of the 5 classifiable observations for the *NeoRandom* treatment and in 5 out of the 6 classifiable observations for the *NeoFixed* treatment. One might view this mapping as a potentially more focal one in that X comes before Y and the @ symbol sits to the left of # on the computer keyboards used in this experiment (and Chinese students read modern Chinese from left to right). However, in both of these treatments, there exist observations for which the opposite language mapping is detected and classified according to our criterion, i.e., (@,@) →(Y,Y), and (#,#) →(X,X). Specifically, we find this opposite mapping in observations 1-2 and 3-2 of the *NeoRandom* treatment and in observation 2-3 of the *NeoFixed* treatment. By contrast, in the common language treatments, we *never* observe mappings from announcement to stage game strategies of (X,X) →(Y,Y) or (Y,Y) →(X,X), though such mappings would comprise perfectly valid languages as well. We summarize these findings as follows.

Finding 4. *Consistent with Hypothesis 3 there is greater variety in the language mapping between announcements and strategies in the Neo Treatment as compared with Comm treatment.*

A further observation regarding the use of language in the *Comm* versus *Neo* treatments

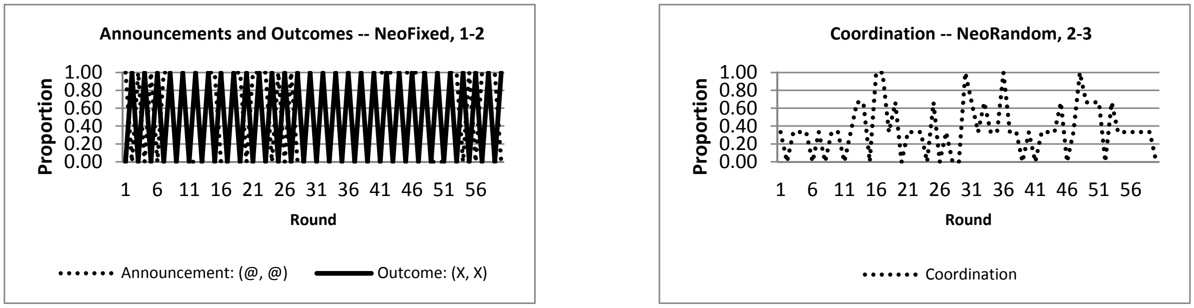

Figure 3: Two Sessions with Non-Classifiable Mappings between Random Device Realizations and Outcomes: *NeoFixed* Session 1-2 and *NeoRandom* Session 2-3

is that there are more non-classifiable mappings in the *Neo* treatments under random matchings. Indeed Table 4 reveals that in the *NeoRandom* treatment, in 4 out of 9 observations the language mapping from the random device realizations to outcomes is not classifiable. By contrast, in the *CommRandom* treatment all 9 observations have classifiable, natural language mappings from random device realizations to outcomes. Under fixed matchings, as Table 5 reveals, we find the same number, 2, of un-classifiable mappings from random device realizations to outcomes in the *CommFixed* and *CommRandom* treatments.

The latter finding might be viewed as somewhat puzzling as fixed matches might seem to be more conducive to the development of a language mapping from random realizations to outcomes. However, as we posited in Hypothesis 4 coordinated play that does *not* involve the use of the random device is more likely under fixed matches than under random matches because fixed matches allow for the development of history-contingent strategies. Indeed, we do find evidence that fixed matches facilitate non-language-mediated mechanisms of coordination as compared with random matches. To see this more precisely, we first compare behavior in our *NeoFixed* and *NeoRandom* treatments and we then compare behavior in our *NoneFixed* and *NoneRandom* treatments.

4.3 Alternative Means of Coordination

Figure 3 shows results from one non-classifiable observation of the *NeoFixed* treatment (session 1-2) and from one non-classifiable observation of the *NeoRandom* treatment (session 2-3). Similar figures for *all* independent (group-level) observations of all treatments are provided in Appendix A for the interested reader. In the *NeoFixed* observation (left panel of Figure 3), the pair of subjects in the fixed match completely ignored realizations of the random device and took advantage of their fixed match to employ alternation (or turn-taking) between the two pure strategy Nash equilibrium as their coordinating mechanism. As they did not make use of the randomly determined announcements, either (@,@) or (#,#), there was no way of classifying the language mapping from these realizations to the stage game strategy space. Indeed the figure reveals that recommendations of (@,@) are frequently ignored. Note that alteration achieves the same expected payoff as following recommendations but is less arbitrary than following the stochastic recommendation process; the actual realization of the random device might (ex-post) benefit one player over the other over the 60 rounds of play in our experiment. By contrast, in the observation shown for the *NeoRandom* treatment (the right panel of Figure 3), the frequency of coordination on either (X,X) or (Y,Y) is also clearly different from 100 percent but is highly variable over time. Recall that in the random matching observations there were *three* pairs of subjects per observation (6 subjects total) so the coordination frequencies in the random treatments can be either 1/3, 2/3 or 1. If all six subjects in this session had coordinated on a common language mapping from realizations of the random device to outcomes then the coordination frequency would have eventually converged to 1.0 and remained there. Instead, as the figure suggests, the pairs in this particular session of the *NeoRandom* treatment appear to have been playing according to the unique mixed strategy Nash equilibrium of the game. Indeed, in this session, the Red (row) players played action X 76.7 percent of the time while the Blue (column) players played action X 25.6 percent of the time; in the mixed strategy Nash equilibrium, row plays X with probability .75 while column plays X with probability .25. This coordination of play on the mixed strategy equilibrium is the explanation for why there was no way to classify the mapping from realizations of the random device to outcomes in this particular *NeoRandom* observation. As in the first case, the *matching protocol* – random matching in this case – seems to have been the dominant factor in determining how subjects selected an equilibrium to play – in this case the unique mixed strategy equilibrium.

We next ask whether the evidence in support of Hypothesis 4 extends to our two *None* treatments where there was no external random device for subjects to use for coordination purposes. Table 6 reports the frequencies of (unconditional) outcomes in the 9 *NoneFixed* and 9 *NoneRandom* observations.

In 7 of the 9 *NoneFixed* observations we find strong evidence for alternation (or turn-taking) strategies as evidenced by the near 50 percent frequencies on outcomes (X,X) and (Y,Y) in these observations. Two observations in particular, 1-1, and 3-3 indicate perfect coordination on a turn-taking outcome. While almost all such observations involved a 2-cycle pattern of alternation, i.e. (X,X), (Y,Y), (X,X),..., as in observation 1-1 shown in the left panel of Figure 4, there were two instances of a more-prolonged interval of turn-taking (observations 1-3 and 2-1) where each player in the fixed match obtained their preferred outcome (X,X) or (Y,Y) for approximately 1/2 of the 60 total periods or about 30 rounds each. The right panel of Figure 4 shows the outcome of observation 1-3. (For other observations

Treat/Obs: ***NoneFixed*** Group	Outcomes			
	(X, X)	(X, Y)	(Y, X)	(Y, Y)
1	0.500	0.000	0.000	0.500
2	0.283	0.250	0.100	0.367
3	0.467	0.033	0.033	0.467
4	0.000	0.517	0.017	0.467
5	0.450	0.050	0.033	0.467
6	0.467	0.067	0.033	0.433
7	0.483	0.033	0.000	0.483
8	0.483	0.000	0.017	0.500
9	0.500	0.000	0.000	0.500
Mean	0.404	0.106	0.026	0.465
Alternation	0.500	0.000	0.000	0.500
NoneRandom	Outcomes			
Group	(X, X)	(X, Y)	(Y, X)	(Y, Y)
1	0.239	0.550	0.056	0.156
2	0.311	0.406	0.100	0.183
3	0.111	0.433	0.067	0.389
4	0.106	0.550	0.072	0.272
5	0.117	0.422	0.089	0.372
6	0.600	0.378	0.011	0.011
7	0.839	0.150	0.011	0.000
8	0.567	0.394	0.011	0.028
9	0.544	0.267	0.106	0.083
Mean	0.381	0.394	0.058	0.166
Mixed N.E.	0.1875	0.5625	0.0625	0.1875

Table 6: Unconditional Outcome Frequencies, NoneFixed and *NoneRandom* Treatments

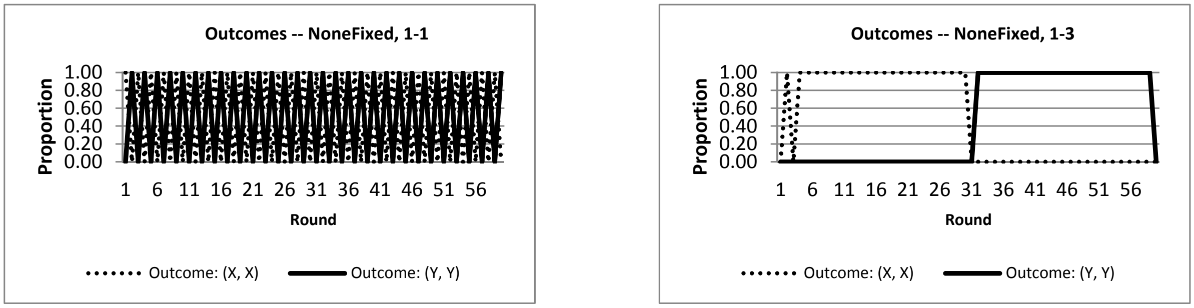

Figure 4: Two Different Instances of Coordination on Alternation Outcomes, *NoneFixed* Treatment

see Appendix A).

By contrast and consistent with Hypothesis 4, in the *NoneRandom* treatment the frequencies of the four outcomes appear to correspond more closely to the unique mixed strategy Nash equilibrium of the game. In this mixed strategy Nash equilibrium, the outcomes (X,X) and (Y,Y) should each arise an average of 18.75 percent of the time, outcome (X,Y) should arise most often, on average 56.25 percent of the time while outcome (Y,X) should arise least often, on average 6.25 percent of the time. As Table 6 makes clear, 5 out of 9 of the observations are consistent with play of the unique mixed strategy equilibrium in the sense that the frequency of outcome (X,Y) is the greatest, the frequency of outcome (Y,X) is the lowest and the frequencies of outcomes (X,X) and (Y,Y) are intermediate. The four exceptions are for observations 2-3, 3-1, 3-2 and 3-3 where a substantial but discontinuous amount of coordination occurred on the outcome (X,X) – see the time series for these observations as reported in Appendix A.

We summarize the findings presented above as follows.

Finding 5. *Consistent with Hypothesis 4, we find that when players do not use or do not have access to a random device, they are more likely to coordinate on equally efficient alternation strategies under a fixed matching protocol. By contrast, under a random matching protocol, play appears to be more consistent with the less efficient, unique mixed strategy Nash equilibrium of the game.*

5 Concluding Remarks

The possibility that players use random devices allowing for correlated strategies so as to achieve outcomes that are more efficient than the Nash equilibria of coordination games has been known for some time. What is less clearly understood is that the introduction of a random device, by itself, may not necessarily make coordination easier; indeed it can result in a secondary coordination problem where individuals must resolve how realizations of the random device map into the strategies they should play in the game. This paper has helped shed some light on this secondary coordination problem, clarifying the important role played by having a common language mapping from realizations of the random device to the strategy space of the game. We have further shown that, absent such a common language as in our *Neo* treatments, a language mapping from realizations of the random device to strategies *can* be learned. However, in efficiency terms we find that it is better to have a common language mapping from the outset, especially under a random matching protocol. Finally we have found that the benefits of a random device accrue mainly in a setting of random matches (one-shot encounters) rather than under fixed matches where other strategies that condition on history, e.g., "alteration" or "turn-taking" can achieve the same efficient outcome that is made possible by introduction of the random device.

We caution that we have only taken a first step in understanding the language-coordination problems presented by having an external random device. For instance, we have only considered the case where there are *two* announcements, the same number as the available stage game strategies; this restriction may have greatly aided coordination relative to environments where the set of announcements was larger (or smaller) than the strategy space of the stage game. Further, we have focused our attention on a correlated equilibrium that is most efficient in the class of fair outcomes (where players receive the same expected payoffs) as we wanted to make the learning of a language mapping from the random device to the strategy space as simple as possible. Nevertheless, this choice may also have facilitated coordination relative to other correlated equilibria that are possible in the game we study. We leave such extensions and robustness checks to future research.

Appendix A – Group Level Data, All Rounds

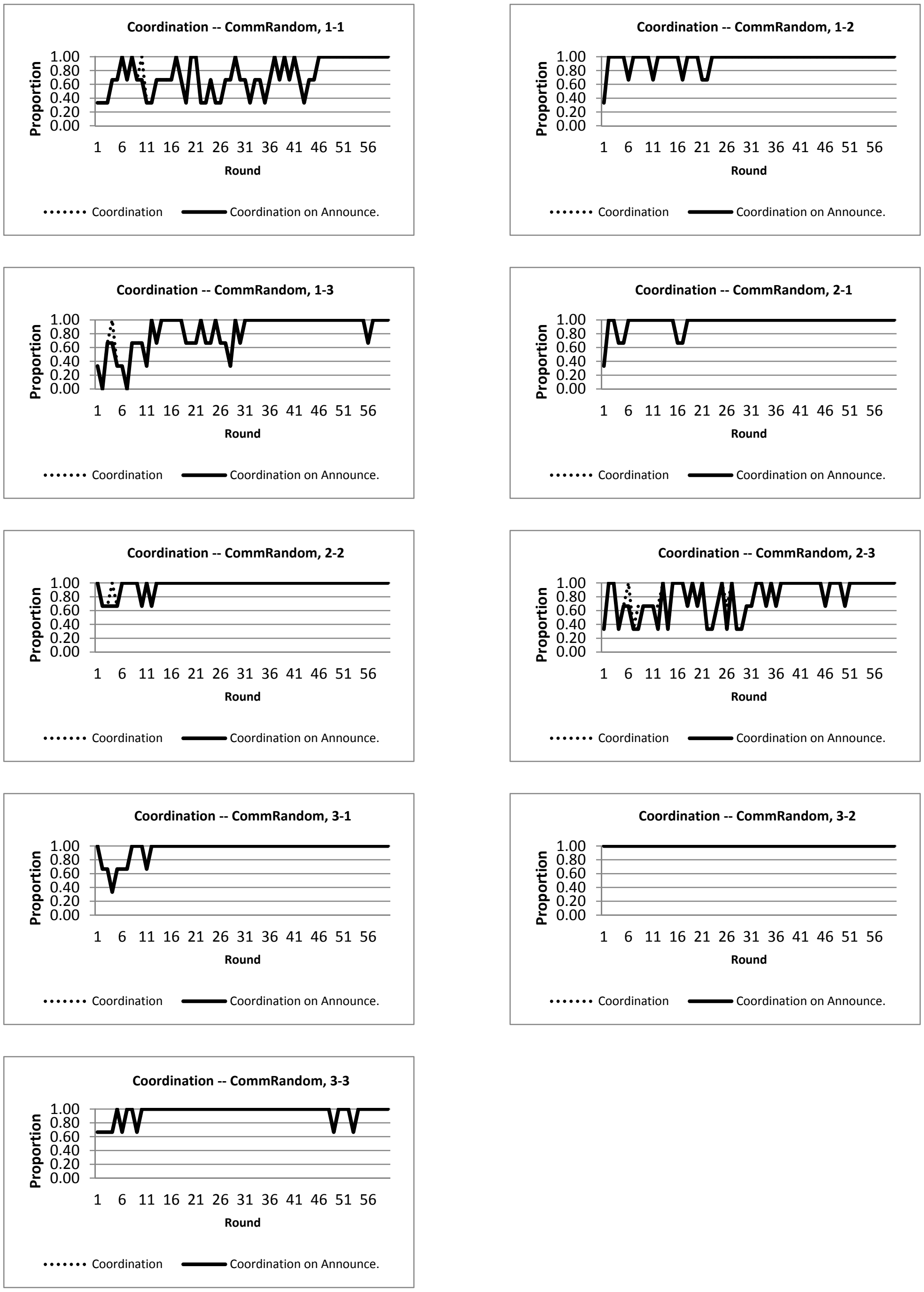

Figure 5: Group Level Data, *CommRandom*

Notes: "Coordination" refers to coordination on (X,X) or (Y,Y). "Coordination on Announcements" means that subjects used the endogenously determined mapping from announcements to stage game strategies (as reported in Table 4) to coordinate on either (X,X) or (Y,Y).

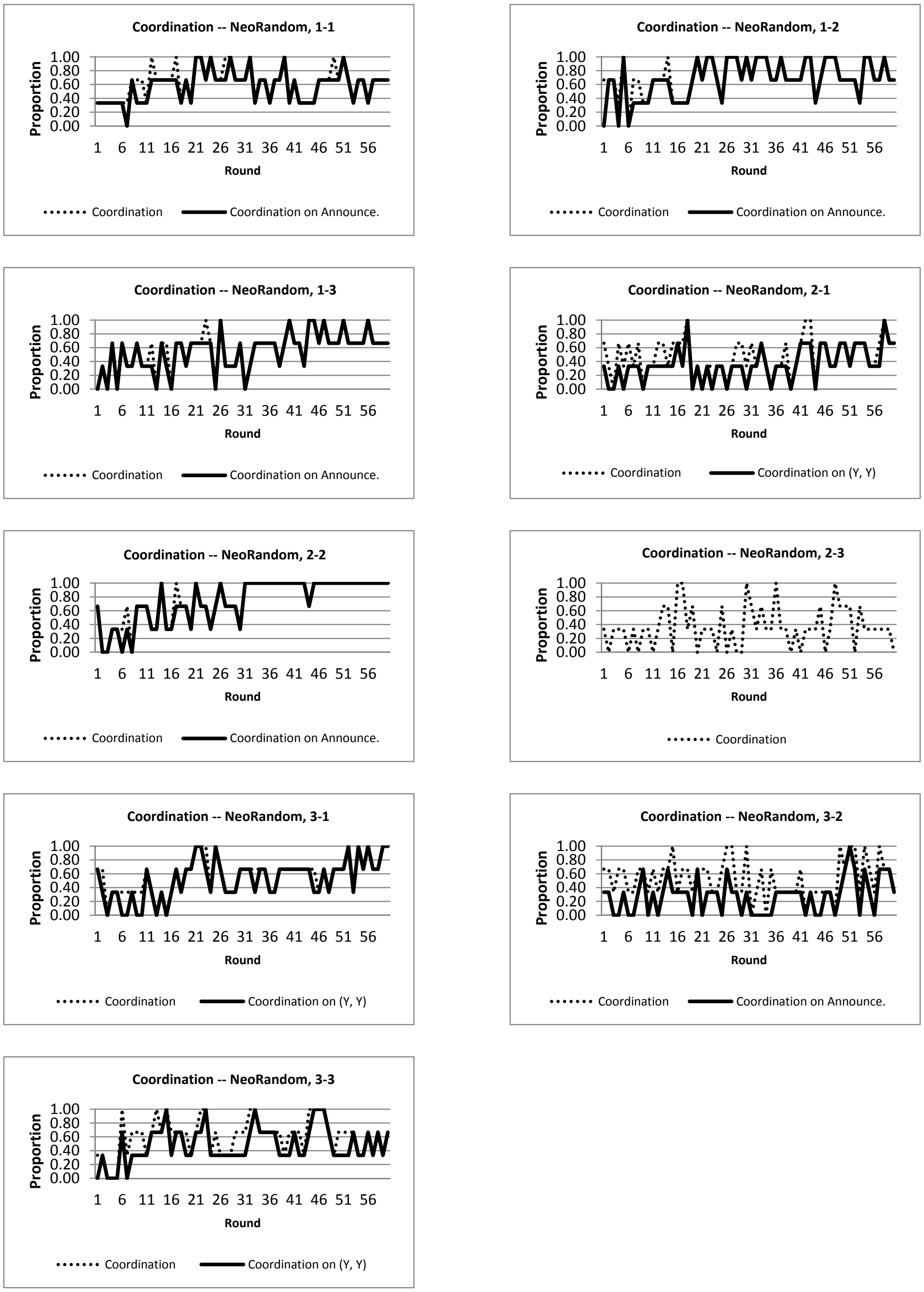

Figure 6: Group Level Data, *NeoRandom*

Notes: "Coordination" refers to coordination on (X,X) or (Y,Y). "Coordination on Announcements" refers to use of the endogenously determined mapping from announcements to stage game strategies (as reported in Table 4) to coordinate on either (X,X) or (Y,Y). For Group 2-3, we plotted the coordination frequency only as there was no endogenously determined mapping from announcements to stage game strategies. For Groups 2-1, 3-1 and 3-3 where there was also no endogenous mapping from announcements to stage game strategies, we instead plot the frequency of coordination on the (Y,Y) outcome, which was the most common coordination outcome in those sessions.

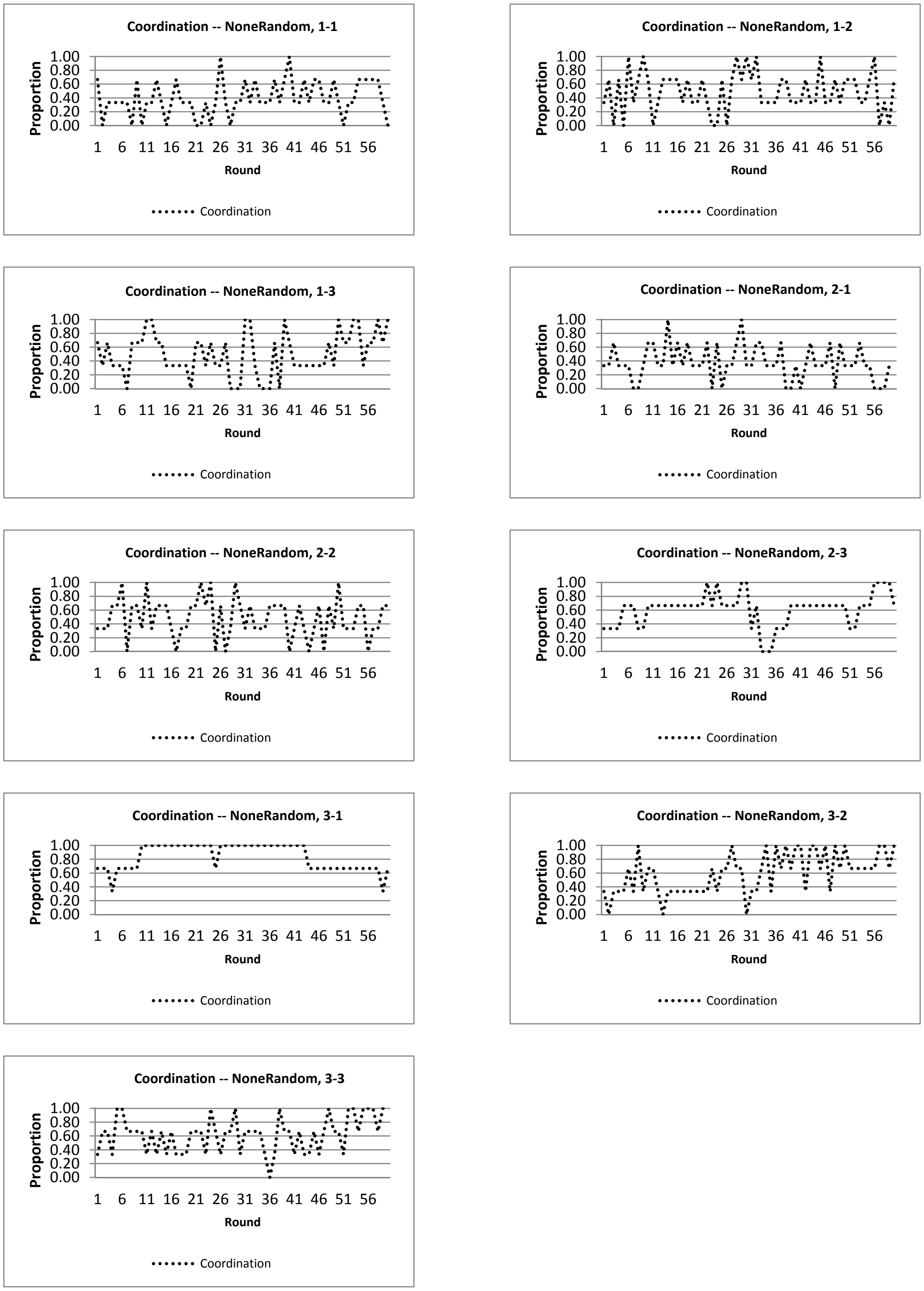

Figure 7: Group Level Data, *NoneRandom*

Note: "Coordination" refers to coordination on (X,X) or (Y,Y).

Figure 8: Group Level Data, *CommFixed*

Notes: "Announcement: (X,X)" and "Outcome: (X,X)" refer to those instances where the announcement or the outcome of play was (X,X). For Group 3-3, where there was no endogenous mapping from announcements to stage game strategies, we instead plotted the frequency of coordination on the (Y,Y) outcome, which was the most common coordination outcome in that session.

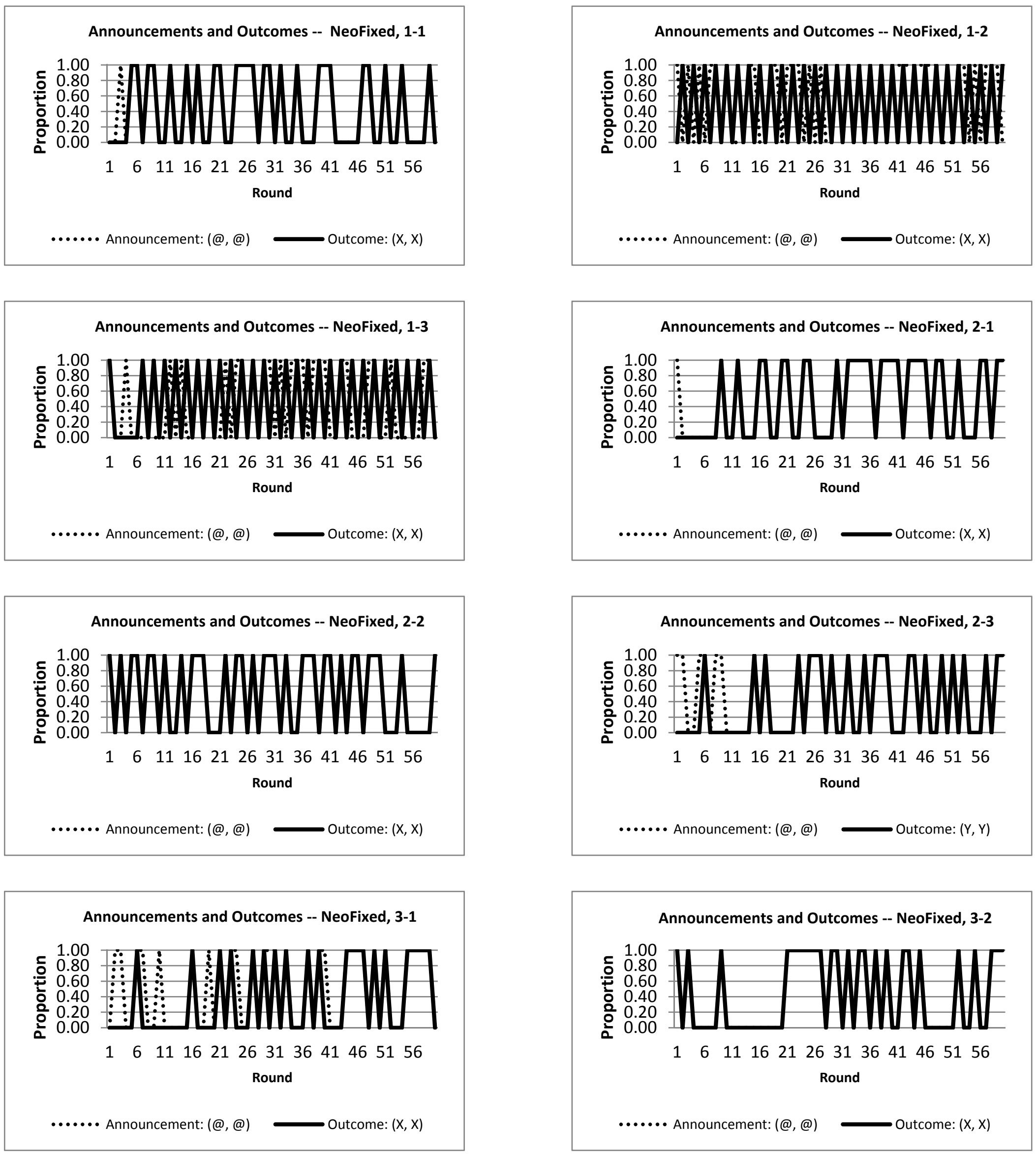

Figure 9: Group Level Data, *NeoFixed*

Notes: "Announcement: (@,@)" and "Outcome: (X,X)" refer to those instances where the announcement or the outcome of play was (X,X). For Group 2-3 where the endogenously determined mapping was from a announcement of (@,@) to (Y,Y), we instead report on "Outcome (Y,Y)," referring to those instances where the outcome of play was (Y,Y).

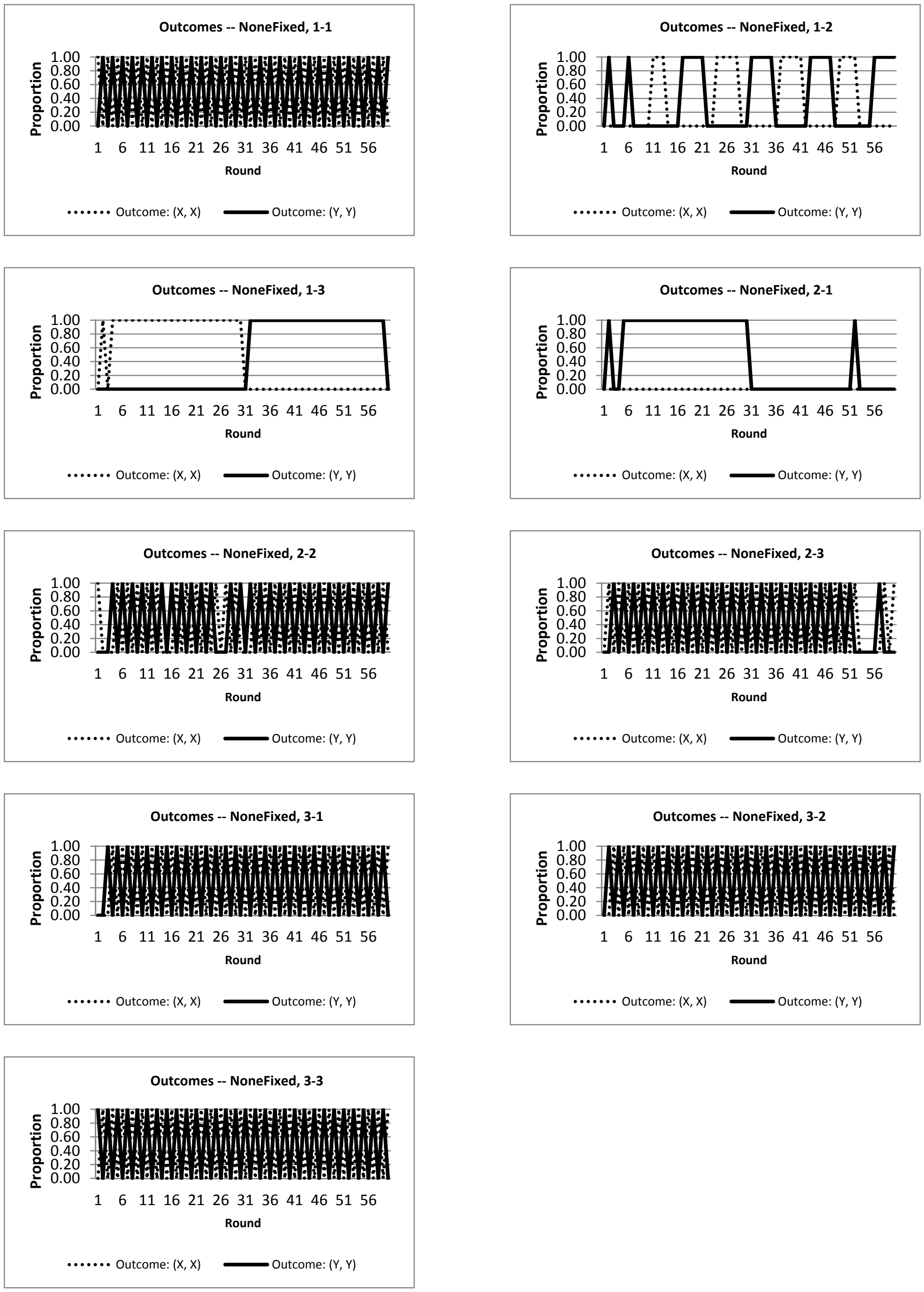

Figure 10: Group Level Data, *NoneFixed*

Note: "Outcome: (X,X)" and "Outcome: (Y,Y)" refer to coordination on (X,X) or (Y,Y).

Appendix B – Experimental Instructions (*NeoRandom*)

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.

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, the 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.

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

Figure 11: Your Point Earnings

of the table the relevant cell belongs to, and your matched Red Player's choice of X or Y will determine which <u>row</u> 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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