Coordination in Games with Strategic Complementarities: An Experiment on Fixed vs. Random Matching

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Abstract

In this paper, we study behavior in a series of two-player supply chain game experiments. Each player simultaneously chooses a capacity before demand is realized, and sales are given by the minimum of realized demand and chosen capacities. We focus on the differences in behavior under fixed pairs and random rematching. Intuition suggests that long-run relations should lead to more profitable outcomes. However, our results go against this intuition. While subjects' capacity choices are better-aligned (i.e., closer together) under fixed pairs, average profits are more variable. Moreover, learning is slower under fixed pairs — so much so that over the last 5 periods, average profits are actually *higher* under random rematching. The underlying cause for this finding appears to be a "first-impressions" bias, present only under fixed matching, in which the greater the misalignment in initial choices, the *lower* are average profits.
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1 Introduction

Firms must make several important decisions when selecting their various supply chain partners, one of which being the duration of the relationship. In this paper, the question of interest is, "which is better: long term or short term relationships?" Note that even within

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the same industry, some players are well-known to engage in long-term relationships, while others focus more on short-run relationships. For example, Toyota is well-known for their focus on the building of supplier *keiretsu*: “close-knit networks of vendors that continuously learn, improve, and prosper along with their parent companies” (Liker and Choi, 2004). In contrast, according to Liker and Choi (2004), Ford has used reverse auctions to select the lowest cost supplier for each contract. Presumably, therefore, at the conclusion of the contract, a supplier would have to win another auction to continue supplying Ford. Similarly, General Motors has historically written contracts which allowed it to switch to a lower cost supplier with little or no advance warning.

Long term relationships can have two distinct mechanisms which may make them preferred to short term relationships. First, the repeated nature of the interaction makes it possible for agents to build and use reputation in order to expand the set of possible outcomes. This may be especially desirable when inefficiencies arise in the equilibrium of a one-shot game. For example, in the voluntary compliance environment first discussed by Cachon and Lariviere (2001), because it is not possible to explicitly contract on capacity, the manufacturer and the supplier instead contract on a wholesale price, which may not coordinate the supply chain. In settings such as this, relational contracts – informal agreements sustained by the value of future interactions – may be beneficial. For example, Taylor and Plambeck (2007b) show the beneficial effects of a relational contract in a model in which a supplier must invest in production capacity before demand is realised and even before the buyer and supplier can contract on a wholesale price. Other examples can be found in Taylor and Plambeck (2007a), Tunca and Zenios (2006) and Gibbons (2005).

The second role for long term relationships is the opportunity for learning and trust-building that they afford. This role typically arises in situations where there are multiple Pareto rankable equilibria such as the “assembler-as-leader” game of Wang and Gerchak (2003) or the two-player “newsvendor” game of Hyndman, Kraiselburd, and Watson (2012). Although firms would like to coordinate on the efficient equilibrium, and many papers assume this to be the case, there is much evidence that coordination failures are quite common (cf. Section 2). Indeed, the operational requirements for coordinating and collaborating in a supply chain are non-trivial so that the introduction of a new product, market, or process, can require a period of adjustment before maximum efficiency and effectiveness is achieved.

In addition to the above benefits, long-term relationships can support the investment in information systems and enabling forms of communication, to further enable learning and signify trust and commitment. For example, in many situations, one or both parties may have access to payoff-relevant private information. If this information could be truthfully communicated, both parties may benefit from it. However, the reality is that the parties may have the incentive to distort their private information to their own advantage, thus rendering communication useless. However, with repeated interactions, players may be motivated to build a reputation for honestly revealing their private information, thus making communication efficiency enhancing (Özer, Zheng, and Chen, 2011).

From an empirical perspective, Toyota's use of long term relationships has been pointed to as a source for its success (Liker and Choi, 2004). As another example, Uzzi (1996) studied data from the International Ladies Garment Workers' Union which cover most of the active firms in the garment industry located in New York. His main question concerned how the mix of long term vs. short term business relationships affected firm survival. He shows that a firm that has only short term contracts (hence, no repeat business) was nearly twice as likely to exit the market as a firm that contracts with only one other firm.

Despite their theoretical benefits and examples pointing empirically to their importance, long term contracts are not without their caveats. As a result of increased dependency from long term contracts, partners can be locked into abusive or profit-losing relationships or be prevented from shifting to more effective relationships. Long term partners can also have higher expectations and be more difficult to satisfy because of the constraints imposed by these contracts (Kalwani and Narayandas, 1995). Beyond this, while long term relationships provide opportunities for building trust between partners, once this trust has been built, firms may be tempted to betray this trust by behaving opportunistically (see, e.g., Neuville (1997) for one striking example from the auto industry).[1] Indeed, it has been noted that, despite their popularity, as many as 7 in 10 joint ventures fail (Coopers and Lybrand, 1986). More recent studies, such as those reported by Bamford, Ernst, and Fubini (2004) and KPMG (2009) put the success rate at closer to 50%, though according to evidence reported in Center for Digital Strategies (2006), the success rate is higher for new product or market joint ventures (60% success rate) than for core business partnerships (40% success rate).

In this paper, we study the behaviour of human subjects engaged in an operations management environment and ask whether, as suggested by much of the theoretical and experimental literature, long term relationships promote efficiency. Specifically, subjects play a two-player version of the newsvendor game studied in Hyndman et al. (2012). Subjects receive a signal about demand and must choose a capacity before demand is realised. Total

[1] Villena, Revilla, and Choi (2011) also argue that building too much social capital in a collaborative buyer-supplier relationship can increases the incidence of opportunistic behaviour by suppliers.

sales are given by the minimum of demand and capacities. When subjects have common information about demand, the capacity choice game has a continuum of Pareto rankable equilibria. For the parameters of the experiment, when subjects have *private* information about demand, there are two equilibria: a complete coordination failure and a monotone equilibrium in which capacities are increasing in one's signal. Both equilibria lead to lower profits than in the efficient equilibrium of the common information game.

We conduct two sets of experiments. The first set of experiments consists of a 2×2 design in which subjects play the capacity choice game under either common or private information, and with either a fixed partner for the duration of the experiment or with a randomly selected partner in each round. Our main concern with these experiments is to test whether fixed pairings lead to higher profits and better alignment of choices. In the second set of experiments, we modify the capacity choice game by adding a communication phase before subjects must choose their capacity. In one treatment, subjects must send a message to their partner at no cost, while in another treatment subjects have the option to send a message to their partner at a small cost. In both cases, subjects are free to lie about their private information. As with the first set of experiments, we run sessions under both fixed pairs and random rematching. Like Özer et al. (2011), we conjecture that, because of reputation concerns, subjects will be more truthful under fixed matching. Moreover, when communication is costly, we conjecture that subjects will be more willing to bear the cost under fixed matching: Since any messages that are sent should be more truthful under fixed pairs, it makes more sense to bear the cost and actually send a message. In contrast, it does not make sense to pay a cost to send a message that will most likely be ignored, as is likely to be the case under random rematching.

We present the following results. First, while average profits over the entire duration of the experiment are generally higher when subjects have a fixed partner, the effect is actually reversed when we consider only the last five periods. We also observe that the standard deviation of average payoffs is (often) significantly higher under fixed pairs. Second, while capacities are better-aligned under fixed matching, the difference is only statistically significant in the private information game, and goes away when we consider only the last five periods. One of the driving factors behind this result appears to be due to the fact that learning is stronger in the random rematching treatments.

Our result that fixed pairings are not efficiency enhancing goes against much of the existing experimental literature, and we seek to understand why. The leading explanation for this apparently contradictory finding is that initial interactions plays a much more important role in determining overall payoffs in the fixed matching treatments. Specifically, we find that, in the fixed matching treatment, the greater the misalignment of choices in the initial rounds, the *lower* the earnings over the rest of the experiment. In contrast, when subjects are randomly rematched each period, the outcome of initial rounds has no impact on overall earnings. Thus, it appears that the literature is correct that fixed pairings create additional opportunities for learning and adjustment. Unfortunately, it seems that players are just as likely to learn that they are a *bad* match for each other as they are to learn that they are a good match. To the best of our knowledge, this apparent "first impressions bias" in fixed matching has not been reported elsewhere.[2]

Introducing pre-play communication leads to higher profits, both when communication is free (i.e., cheap talk) or costly, though again, we do not find that subjects do better under fixed matching. When communication is costly, we find that fixed matching promotes more frequent and more honest communication. However, while these effects go in the predicted direction, the effects are not significant. Another consequence of fixed matching is that when a subject realises that he/she has been lied to, trust breaks down and the subject places less weight on the messages received. By far the bigger effect concerns the differences between costly communication and cheap talk. When communication is costly, subjects are more frequently honest and, when they lie, they distort their private information by a smaller amount than when talk is cheap. Indeed, conditional on receiving a message, subjects actually earn more when communication is costly. Finally, we document that while honesty may not be the best policy, it is certainly counter-productive to lie by "too much".

The rest of the paper proceeds as follows. Section 2 outlines the related experimental literature and places the paper within the Behavioural OM literature. Section 3 presents the game that subjects will play in the experiments and its equilibria, while Section 4 provides details of our experimental design. In Section 5 we discuss the results of our first set of experiments, while Section 6 seeks to understand the cause of the apparent failure of fixed matching to promote efficiency. Section 7 discusses the experiments with pre-play communication. Finally, Section 8 provides some concluding remarks and suggests avenues for future research. Appendix A provides some details on individual-level behaviour.

[2]To be sure, we are not the first to argue that initial outcomes may predict final outcomes. For example, in the repeated "median effort" games of van Huyck, Battalio, and Beil (1991), in all cases, the median action in round 1 van Huyck et al. (1991) was the median chosen in all subsequent rounds. In contrast our result is about the impact that the dispersion of initial choices has on final outcomes.

2 Literature Review

2.1 Related Experimental Economics Literature

There are several papers in experimental economics that explicitly examine the differences in behavior and outcomes between fixed pairs and random rematching. Cooper, DeJong, Forsythe, and Ross (1990, 1992) show that under random rematching in a "stag-hunt" game with two Pareto rankable equilibria, subjects converge to the *inefficient* equilibrium. Clark and Sefton (2001) replicate this result under random rematching but show that under fixed matching it is considerably more likely that subjects will coordinate on the efficient equilibrium. Studying four different stag-hunt games, Schmidt, Shupp, Walker, and Ostrom (2003) also show that fixed pairs generally leads to more efficient outcomes. In the minimum effort (aka "weak-link") game of van Huyck, Battalio, and Beil (1990), they also show that, at least for groups of two, play under random rematching generally converges to the worst equilibrium, while under fixed pairs, most groups converged to the best equilibrium. Thus, as Camerer (2003) says, "the stability and mutual adjustment in fixed pairings are also required" to ensure convergence to the efficient equilibrium.

Repeated interaction also opens the door for so-called *teaching*. That is, even if players are initially stuck in a bad equilibrium, one of the players may attempt to break out of the bad equilibrium by repeatedly choosing his part of the efficient equilibrium strategy profile, with the hope of changing the beliefs of the other player sufficiently so that she now finds it optimal to take the efficient action as well. Such teaching has been shown to facilitate convergence to the efficient outcome by Hyndman, Terracol, and Vaksmann (2009), Brandts, Cooper, and Fatas (2007) and Brandts and Cooper (2006), among others. Thus teaching and learning should lead to higher-ranked equilibria and, therefore, higher payoffs.

In other settings, Duffy and Ochs (2009) show that a cooperative norm emerges in indefinitely repeated prisoners' dilemma games under fixed pairs, but not under random rematching. In a survey of linear public good games experiments, Andreoni and Croson (2008) show that there is no clear picture about the differences in behavior under fixed pairs and random rematching. Specifically, four studies find more cooperation under random matching, five studies find more cooperation under fixed matching and four studies find no difference at all. In an incomplete information step level public goods game, Palfrey and Rosenthal (1994) show that fixed matching leads to higher efficiency, but much less than theory predicts.

2.2 Relationship to Hyndman et al (2012) and Broader Behavioural OM Literature

Our paper is very closely related to Hyndman et al. (2012). In that paper, we studied, both theoretically and experimentally, the role of private information about demand on agents' ability to coordinate. We showed that when agents have common information about demand, then there are a continuum of Pareto rankable equilibria. In contrast, when agents have private information about demand, then there are at most two equilibria. The complete coordination failure always exists, while, if the cost of capacity is below a threshold, there is also an equilibrium in monotone strategies. However, in this equilibrium, profits are lower than in the efficient equilibrium of the common information game. We also showed that two-way (cheap talk) communication may be efficiency enhancing.

The experiments of Hyndman et al. (2012) sought to test the above theoretical results. We showed that private information does not have the predicted negative effect on firm profits and also that pre-play communication does have the potential to increase profits. Despite these results, subjects consistently deviated from the equilibrium predictions and earned significantly less than predicted by the efficient equilibria. Therefore, in this paper, guided by strong evidence in its favour, we seek to test whether allowing agents to interact repeatedly can mitigate these effects and lead to higher efficiency.

Within the broader context of behavioural OM, we are not aware of any papers which explicitly examine the difference in behaviour when agents have private vs. common information. Note also that we are largely unconcerned with the behavioural biases that have been reported in several traditional newsvendor experiments, most notably by Schweitzer and Cachon (2000). It is very likely that any such biases that may be present will be dominated by the effects of strategic interaction between the subjects in our experiments.

Besides Hyndman et al. (2012), the most closely related paper is Özer et al. (2011). They show in an experiment on forecast information sharing between a supplier and a manufacturer, that repeated interactions lead to lower forecast inflation, higher capacity and higher channel efficiency than in a random rematching environment.

3 Theoretical Background

In this section, we outline the basic theoretical environment that we will then take to the experimental laboratory. Suppose that there are two symmetric firms. In order for demand to be met, each firm needs to invest in capacity to meet potential demand. Denote by K_i, $i \in \{1, 2\}$ the capacity of firm i. For each firm, the unit cost of capacity is $c > 0$ and the *net revenue* per unit sold is $p > c$. Finally, assume that all parameters are common knowledge.

The timing of events is as follows. First, each firm receives a signal, θ_i about demand. Second, firms simultaneously choose capacity, K_i. Finally, demand, x, is realized and total sales are $\min\{x, K_s, K_m\}$. Therefore, the profits of firm $i \in \{1, 2\}$ can be written as:

$$\Pi_i(x, K_s, K_m) = p \min\{x, K_s, K_m\} - cK_i.$$

Let $\alpha = (p - c)/p$ denote the critical fractile based on the traditional newsvendor tradeoffs.

It remains to specify the properties of signals, θ_i, and demand, x. First, assume that demand is uniformly distributed on $[a, b]$ with $0 \le a < b < \infty$. Prior to choosing capacities, each firm receives a private signal $\theta_i = x + \epsilon_i$, where $\epsilon_i \sim \mathcal{U}[-\eta, \eta]$ and $\eta > 0$ measures the noisiness of the signals. Following Hyndman et al. (2012), we consider two cases:

1. Common Information (CI): $\epsilon_m = \epsilon_s$; and

2. Private Information (PI): ϵ_s and ϵ_m are *independent* draws from $\mathcal{U}[-\eta, \eta]$.

Note that under private information, given a signal θ_i received by firm i, this firm believes that the true state is uniformly distributed on $[\max\{a, \theta_i - \eta\}, \min\{b, \theta_i + \eta\}]$. However, the probability density function of player i's beliefs about the signal received by player j has support $[\max\{a, \theta_i - \eta\} - \eta, \min\{b, \theta_i + \eta\} + \eta]$, and it is not uniform.

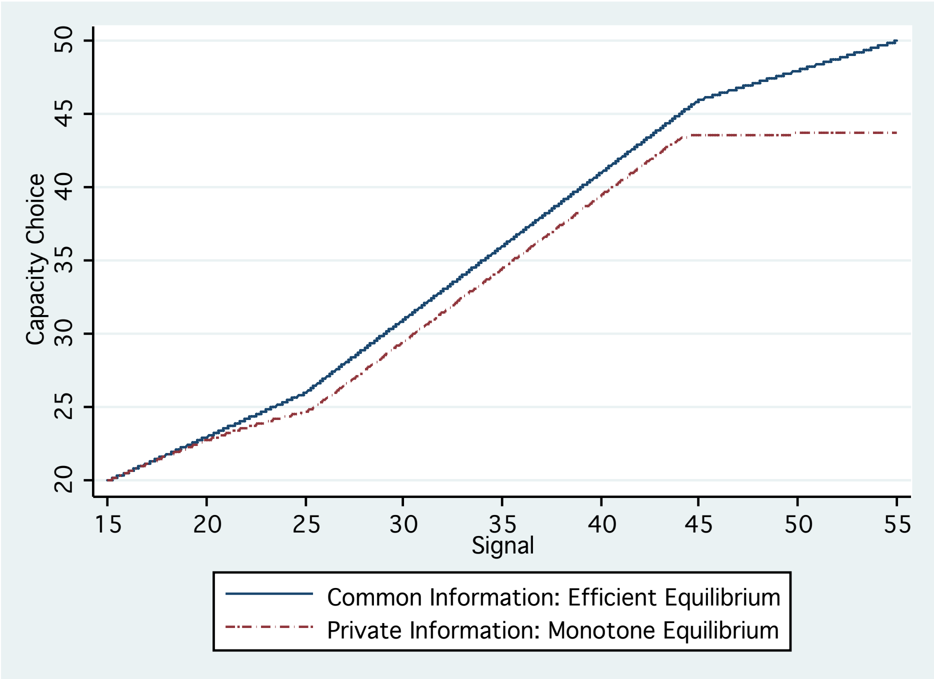
Under common information, it is straightforward to show that there is a continuum of Pareto rankable equilibria, with the Pareto efficient equilibrium corresponding to the optimal solution of the single-person newsvendor model. That is,

$$K^{\text{CI}}(\theta) = \alpha \min\{\theta + \eta, b\} + (1 - \alpha) \max\{\theta - \eta, a\},$$

where θ is the common signal received by both players.

Under private information, it is trivial to see that there will always exist equilibria of the following form: for any $k \in [0, a]$, both firms choose a capacity of k for all possible signal realisations. Such equilibria are typically not interesting. Instead, one looks for so-called *monotone* equilibria in which the capacity choice function is monotonically increasing in the signal received. When the prior distribution on demand has a compact support, as in our experiment, analytically solving for the monotone equilibrium capacity choice function is not possible.[3] However, it is possible to solve for the equilibrium numerically. For the parameters of the experiment, Figure 1 shows the Pareto efficient equilibrium of the common information game and the monotone equilibrium of the private information game. Notice that capacity choices in the efficient equilibrium of the CI game are always at least as high as the monotone equilibrium of the PI game. The difference is most stark at high signal realizations where the capacity choice function is much flatter in the PI game. The reason for this is because, a player who receives a high signal actually has more precise information about demand, but also believes that it is very likely that the other player received a lower signal (and so will choose a lower capacity). Not wanting to risk being under-cut, the player receiving the high signal also chooses a relatively lower capacity.

Figure 1: Equilibrium Capacity Choice Functions



Notice also that in any equilibrium of the common information game, the capacity choices of the two firms are perfectly aligned. In contrast, in the private information game, because players receive independent signals, with capacities being a monotone function of signals, the two firms' capacities will be misaligned with probability 1. Finally, for a wide range of signals, the slope of the equilibrium capacity choice functions are 1. In the experiment, the

[3] Hyndman et al. (2012) analytically characterise the monotone equilibrium under the assumption that the prior on demand is diffuse over $\mathbb{R}$.

extent to which the estimated choice functions have a slope of less than 1 can be used as a measure of the coordination failure.

Repeated Game Effects. The above discussion has presumed that players interact only once. As we noted in Section 2, because both the common and private information games have multiple (CI: continuum; PI: two) Pareto rankable equilibria, the question is not whether repetition expands the equilibrium set relative to the one-shot game, but whether repetition can help players to reach the efficient equilibrium.

4 Experiment

We investigate the differences between fixed pairs and random rematching through a series of experiments on human subjects. All of the sessions reported in this paper were run at the experimental economics laboratory of a public university in the United States Southwest. Subjects were recruited from undergraduate classes and had no previous experience in our experiments. Subjects received a \$5.00 participation fee in addition to earnings from the experiment which depended on their performance. Subjects earned "experimental currency units" in the experiment, which were converted to USD at the rate of $\$1 = 165$points. In each session, after subjects read the instructions they were also read aloud by an experimental administrator. Each session lasted approximately 75 minutes and each subject participated in only one session. Throughout the experiment, we ensured anonymity and effective isolation of subjects in order to minimize any interpersonal influences that could stimulate uniformity of behavior.[4]

The first set of experiments that we will comment on is a 2×2 design in which subjects played either the common information game or the private information game under fixed pairs or random rematching. In the sequel, we will also study the effects of pre-play communication under both fixed pairs and random rematching, but we delay providing the details of these experiments. Regardless of the treatment, subjects played the capacity choice game for 30 periods. In all treatments, the structure of the game was as follows: The prior distribution of demand was uniform on $[20, 50]$. Subjects received a signal, $\theta_i = x + \epsilon_i$, which was centered on the true demand, x, with additive noise $\epsilon_i \sim \mathcal{U}[-5, 5]$. Under common information, both subjects received the same signal, while under private information subjects

[4]Participants' workstations were isolated by cubicles making it impossible for participants to observe other's screens or to communicate. We also made sure that all remained silent throughout the session.

received independent signals. That is, ϵ_i and ϵ_j were independent draws. Finally, the payoff parameters were:

$$\pi_i(x, K_i, K_j) = 5 \cdot \min\{x, K_i, K_j\} - 2K_i \quad (1)$$

where x denotes demand, K_i denotes the choice of subject i and K_j denotes the choice of his/her match. At the end of each round subjects learned the realised demand and the capacity choice of their partner but not, in the PI games, the signal received by their partner.

Table 1 summarizes the details of our experiment. A sample of the instructions used can be found in the supplemental notes. The experiment was programmed using z-Tree (Fischbacher, 2007). Note that results from the random rematching treatments were reported in Hyndman et al. (2012), while the fixed matching treatments are unique to this paper.

Table 1: SUMMARY OF EXPERIMENTS[†]

Treatment	Num. of Subjects	Matching
CI	24	Fixed
CI	20	Random
PI	18	Fixed
PI	18	Random

[†] We also ran a treatment of CI and PI under fixed matching only with $c = 3$. We will delay commenting on these treatments until Section 6.

5 Results

5.1 Basic Results

We begin by providing some basic descriptive statistics on average earnings and also misalignment. Our main goal is to test whether, consistent with previous experimental results on coordination games with multiple Pareto rankable equilibria, fixed matching enables players to coordinate on more efficient equilibria and thus, earn more. Moreover, regardless of whether subjects play the efficient equilibrium, we would also expect subjects to be better-aligned under fixed matching because learning about the decision rule of a single partner should be easier than learning about the entire population, which may be quite heterogeneous. Unless specifically stated otherwise, all tests reported take the subject average as the unit of independent observation.

5.1.1 Average Earnings

Summary statistics on average earnings per round (in experimental points) from our treatments are presented in Table 2. Although our primary concern is on the differences between fixed and random matching, Table 2 also speaks to the role of information. As can be seen, looking at either the full 30 rounds, or only the last 5, there is virtually no difference between the CI and PI treatments under pairs. In contrast, under random rematching, subjects earn more in the CI treatment. Over both timeframes, the difference is significant at $p < 0.02$.

Turn now to the comparison of fixed pairs vs. random rematching. Averaging across all 30 rounds, we see that there is no difference in the average earnings between fixed pairs and random rematching in the CI treatment, while average earnings are, consistent with our prediction, significantly higher under fixed matching in the PI treatment. However, these averages are somewhat misleading because, as we also see in Table 2, for both treatments, there is significantly greater variation in average earnings under fixed pairs than under random rematching. Thus, under fixed matching some groups fair quite poorly, while others fair quite well.

Table 2: Summary statistics: Average Profits

(a) Average Over All 30 Periods

Treatment	Matching	Payoff	Std. Dev	Min	Max	Mean Test	Variance Test
CI-FM	Fixed	84.0	15.2	51.2	109.4	$t_{42} = 0.21$	$F_{23,19} = 6.11$
CI-RM	Random	84.8	6.2	70.0	95.7	$p = 0.833$	$p < 0.01$
PI-FM	Fixed	83.8	6.9	70.6	95.0	$t_{34} = 2.94$	$F_{17,17} = 2.70$
PI-RM	Random	78.2	4.2	71.0	86.3	$p < 0.01$	$p = 0.048$

(b) Average Over Last 5 Periods

Treatment	Matching	Payoff	Std. Dev	Min	Max	Mean Test	Variance Test
CI-FM	Fixed	84.5	18.2	44.8	121.4	$t_{42} = 1.69$	$F_{23,19} = 2.22$
CI-RM	Random	92.5	12.2	77.4	120.7	$p = 0.099$	$p = 0.082$
PI-FM	Fixed	84.8	9.6	72.6	101.6	$t_{34} = 0.43$	$F_{17,17} = 0.94$
PI-RM	Random	83.4	9.9	68.1	101.2	$p = 0.667$	$p = 0.897$

As noted above, Camerer (2003) argues that fixed matching creates opportunities for "stability and mutual adjustment". Therefore, even if we don't see strong differences in earnings averaged over the entire experiment, then we would still expect higher earnings towards the end of the experiment under fixed matching. In fact, exactly the opposite result occurs. Whereas there was no difference between fixed pairs and random rematching in the

CI treatment, over the last five periods, average profits are now significantly higher under random rematching. Similarly, whereas average profits were significantly higher under fixed pairs in the PI treatment, over the last five periods, the gap has almost entirely vanished and the difference is no longer statistically significant. All of this suggests, contrary to our expectations, that learning is actually faster under random rematching.

5.1.2 What is the extent of misalignment?

Here we quantify the amount of misalignment in each group's choices with a particular focus on whether alignment is better under fixed pairs than under random rematching. Let d_t^j denote the absolute difference between the choices of the subjects in pair j in round t, and let $\bar{d}$ denote the average over all groups and rounds. For each treatment, Table 3 reports these data.

Table 3: THE EXTENT OF MISALIGNMENT ($\bar{d}$)

(a) Average Over All 30 Periods

Treatment	Matching	$\bar{d}$	Std. Dev	Mean Test	Variance Test
CI-FM	Fixed	3.55	2.73	$t_{42} = 1.53$	$F_{23,19} = 1.81$
CI-RM	Random	4.68	2.03	$p = 0.113$	$p = 0.193$
PI-FM	Fixed	5.60	2.33	$t_{34} = 0.59$	$F_{17,17} = 5.16$
PI-RM	Random	5.96	1.03	$p = 0.561$	$p < 0.01$

(b) Average Over Last 5 Periods

Treatment	Matching	$\bar{d}$	Std. Dev	Mean Test	Variance Test
CI-FM	Fixed	2.67	2.56	$t_{42} = 0.88$	$F_{23,19} = 1.07$
CI-RM	Random	3.35	2.49	$p = 0.382$	$p = 0.892$
PI-FM	Fixed	4.40	2.29	$t_{34} = 0.93$	$F_{17,17} = 1.82$
PI-RM	Random	5.02	1.70	$p = 0.359$	$p = 0.229$

Consistent with our expectations about fixed matching, we see that subjects are better-aligned under fixed pairs, than under random rematching for both the CI and PI treatments. However, we only come close to significance in the CI treatment averaging over all 30 periods. As was the case with average profits, we see that the variability of misalignment is consistently higher in the fixed matching treatments, though the difference only appears to be significant for the PI treatment. Note also that over the last five periods, subjects are considerably less misaligned than over the entire experiment. Moreover, unlike the case with average profits, alignment appears to improve (by between 15.8 and 28.4%) over the last five periods in all four of our treatments.

Finally, notice that subjects in the CI treatments are significantly better-aligned than subjects in the PI treatments. This holds, at $p < 0.03$, for both the fixed pairs and random rematching treatments and also over all 30 rounds, or only the last 5. Thus, as predicted by theory, private information is detrimental to subjects' ability to align their actions.

5.2 Learning

In this section we discuss whether subjects are able to learn. Learning can take two, potentially different, forms. First, players can learn to align their actions better with their match. However, they may learn to align by consistently choosing low capacities regardless of the state of demand. Therefore, we also study whether subjects' profits are increasing as the experiment proceeds.

5.2.1 Do subjects learn to align their choices?

Recall that subjects played the game for 30 periods, either with a fixed partner or with random rematching each period. Especially in the fixed matching treatments, it is natural to expect that alignment should improve over time as players learn about the strategy employed by their match. However, even under random rematching, subjects may be able to learn about the pool of subjects with which they interact. In Table 4 we show the results of a series of random-effects regressions where we regress d_t^j on the round and other control variables. If subjects learn to align choices, then we would expect a negative estimated coefficient on the variable `round`. Indeed, this is exactly what we see. The coefficient on `round` is negative and significant at the 1% level in all four treatments. We also see a positive and significant (in 3 treatments) coefficient on `demand`, indicating that subjects find it more difficult to align their actions when demand is high. To some extent, this result is expected because with higher demand, there is simply more scope for subjects' choices to be misaligned. However, it could also be due to heterogeneous risk preferences.

If we compare the coefficient on `round` between the fixed pairs and random rematching treatments, we see that the magnitude of the coefficient is larger under random rematching (indicating faster learning). However, only for the CI treatment are we able to say that there is a statistically significant difference in the speed of learning under fixed pairs and random rematching.[5] Thus, while learning occurs in both treatments, it is somewhat counter-intuitive

[5]Formally, we pooled the data from the fixed pairs and random rematching treatments and estimated the

Table 4: Random-effects regressions of d_t^j on round

	CI-FM	CI-RM	PI-FM	PI-RM
`round`	-0.0772***	-0.159***	-0.0953***	-0.136***
	[0.0187]	[0.0217]	[0.0292]	[0.0234]
`demand`	0.0612***	0.0840***	0.0118	0.181***
	[0.0152]	[0.0195]	[0.0442]	[0.0250]
`cons`	2.613***	4.168***	6.669***	1.753**
	[0.717]	[0.857]	[2.012]	[0.851]
`N`	720	600	540	540
R^2	0.0255	0.0898	0.0226	0.144

*** significant at 1%; ** significant at 5%; * significant at 10%
Standard errors in brackets. Clustered at group level in fixed matching treatments and at subject level in random rematching treatments.

to find that learning is *slower* under fixed matching.

5.2.2 Do subjects earn more in later rounds?

The previous subsection shows a tendency towards improved coordination in capacity choices as the experiment proceeds. We now analyze whether subject's earnings increased as the experiment proceeds. To do this, we regress earnings on round number, the (unknown) state of demand and also on the match's choice. The results are reported in Table 5.

Table 5: Random-effects regressions of profits on round

	CI-FM	CI-RM	PI-FM	PI-RM
`round`	0.206***	0.444***	0.378***	0.381***
	[0.0462]	[0.0630]	[0.115]	[0.0579]
`demand`	0.399***	0.533***	1.084***	-0.229**
	[0.129]	[0.140]	[0.343]	[0.107]
`oth. choice`	2.315***	2.110***	1.856***	2.736***
	[0.167]	[0.179]	[0.346]	[0.177]
`cons`	-6.597***	-9.315***	-22.21***	-5.065*
	[2.128]	[1.669]	[6.210]	[2.937]
`N`	720	600	540	540
R^2	0.773	0.772	0.714	0.74

*** significant at 1%; ** significant at 5%; * significant at 10%.
Clustered standard errors (at subject level) in brackets.
† `oth. choice` denotes one's match's choice.

model allowing for the coefficient on `round` to differ for the two matching protocols. We then tested whether the coefficients are identical. For the CI treatment, this hypothesis was rejected at the 1% level.

We focus our attention on the coefficient on `round`; the interpretation of the other coefficients is straightforward and not central to our discussion of learning. As can be seen, the coefficient on `round` is always positive and significant at the 1% level. Thus, across a lll treatments, subjects' earnings are increasing over the course of the experiment. However, just as was the case for alignment, learning appears to be faster in the random rematching treatments; though, again, the difference is only significant in the CI treatment.

6 Discussion: What's Wrong With Fixed Pairs?

6.1 The impact of initial interactions

The most surprising result in this paper is the counter-intuitive result that fixed pairs does not out-perform random rematching, and that learning seems to be faster under random rematching. In particular, as Table 2 showed, average payoffs do not differ much, but the variance of payoffs is much higher under fixed pairs. Moreover, as shown by Tables 4 and 5, learning is stronger under random rematching.

One possible explanation for these findings may be that, under fixed pairs, the outcome of early rounds is a strong predictor of how the interaction between the pair will continue for the rest of the experiment. If the pair has a successful early interaction, then this success may be likely to continue, while if the early interaction is unsuccessful, then this too may be expected to continue. On the other hand, under random rematching, since matches are temporary, initial rounds may not affect future choices as much.

To test this conjecture, we regress average profits over the final $N - k$ periods on the extent of misalignment in the first k periods, where $k \in \{2, 3, 4\}$. We pool the data across all treatments and matching protocols and include treatment dummy variables as well as interactions for the matching protocol with our explanatory variables. The results are on display in Table 6. As can be seen, for the fixed pairs treatment subjects who were initially more misaligned in early periods had significantly lower earnings than those who were better-aligned. In contrast, for the random rematching treatment, the effect of initial misalignment is never significantly different from zero. Note also that higher average initial profits also appear to lead to higher profits overall in the fixed matching treatment but not in the random rematching treatment (though as can be seen, the treatment interaction is not significantly different from zero). Thus, under fixed matching, both initial success and initial failure appear to be reinforced, leading to the higher variability in earnings that we have

Table 6: Initial misalignment and total profits

	2 periods	2 periods	3 periods	4 periods
`Random Matching`	-8.165**	5.358	-7.571*	-5.562
	[3.539]	[10.23]	[4.203]	[4.596]
`ave. initial profit`		0.127**		
		[0.0631]		
`(ave. initial profit)`		-0.157		
× `(Random Matching)`		[0.114]		
`initial misalignment`	-0.538***	-0.424***	-0.472***	-0.338***
	[0.121]	[0.132]	[0.0952]	[0.0762]
`(initial misalignment)` ×	0.528***	0.390*	0.371**	0.228*
`(Random Matching)`	[0.191]	[0.213]	[0.148]	[0.135]
`cons`	58.15***	50.05***	59.54***	59.06***
	[2.898]	[4.948]	[2.937]	[3.005]
`N`	112	112	112	112
Treatment Dummies	yes	yes	yes	yes
R^2	0.742	0.752	0.753	0.744

*** significant at 1%; ** significant at 5%; * significant at 10%.
Standard errors in brackets.

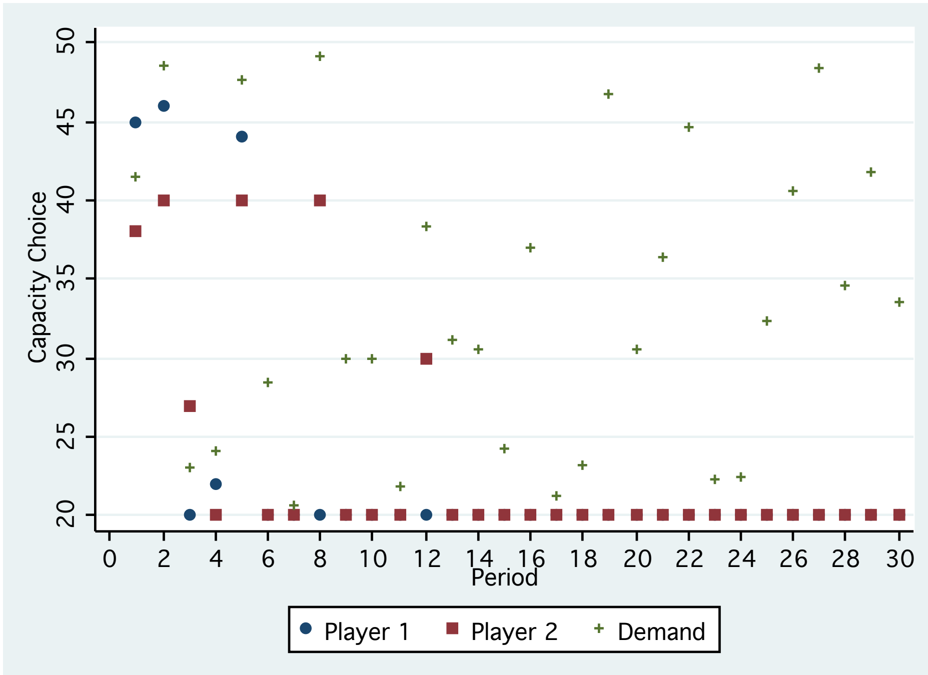
documented.

In Figure 2, we provide an example of the "worst-case" scenario which, though rare, only occurred in our fixed matching treatments. In this figure, we plot the capacity choices of each of the players (the • and ■) as well as the realized demand (+) in each round. As can be seen, after some initial misalignment in capacity choices, both players repeatedly chose the lowest possible capacity until the end of the game. Observe that the complete coordination failure essentially emerged after only 6 rounds (though player 2 appears to have made an unsuccessful effort to break free in rounds 8 and 12).

6.2 The adjustment process

The above results suggest that the outcome of initial interactions matters much more under fixed matching. Those results also speak to the process by which subjects adjust their capacity choice function from round to round. As we have discussed extensively, the common view in the literature is that, over time, fixed matching is more conducive to convergence to equilibrium (or to the efficient equilibrium in case there are several equilibria). Recall from Section 3 that under both common and private information, the slope of the capacity choice function should be 1 over a wide range of signals. In Table 7 we report random effects

Figure 2: Example of complete coordination failure under fixed matching



Tobit regressions of choice as a function of one's estimate, as well as an interaction between the round and estimate. As can be seen, under both fixed pairs and random rematching, the coefficient on `estimate` is less than 1, indicating that subjects are generally suffering from a partial coordination failure by choosing too low a capacity. However, in the random rematching treatments, we see a positive and significant coefficient on the interaction term. Thus, over time, subjects' behaviour in the random rematching treatment comes closer to optimal, while subjects in the fixed pairs treatments remain mired in the partial coordination failure.

6.3 Is it something with the environment?

There is one main between our setting and that of much of the other experimental literature on coordination games: most of the literature has focused on games of complete information, while in our setting, we have the presence of random demand, which may also determine the minimum. Moreover, in our experiments, because subjects receive signals about demand (i.e., demand forecasts), the environment is unstable because beliefs about demand change each period. There is little one can do about demand uncertainty, which is fundamental to newsvendor and supply-chain settings. Although we view changing beliefs about demand as

Table 7: Adjustments to the capacity choice function

	CI-FM	CI-RM	PI-FM	PI-RM
`estimate`	0.782***	0.793***	0.725***	0.782***
	[0.0263]	[0.0227]	[0.0278]	[0.0236]
`estimate`$\times$`round`$\times 10^{-2}$	0.0104	0.140**	-0.0599	0.290***
	[0.0669]	[0.0565]	[0.0794]	[0.0593]
`cons`	3.981***	3.283***	8.482***	2.213**
	[1.164]	[0.971]	[1.104]	[1.031]
`N`	720	600	540	540
`LL`	-2059	-1643	-1631	-1460

*** significant at 1%; ** significant at 5%; * significant at 10%.
Standard errors in brackets.

natural, some may view it as an additional impediment to learning.

In order to test whether an unstable demand environment contributes to our negative results about fixed matching, we conducted another experiment in which, in each of the 30 periods, demand was a draw from the discrete uniform distribution with support $[1, 100]$. The payoff and cost parameters were the same as in the original experiments, which means that the Pareto efficient equilibrium capacity choice was 60. New subjects were recruited for these experiments and subjects participated under either fixed pairs or random rematching.

Table 8: Fixed pairs vs random rematching in a stable demand environment

(a) Average Over All 30 Periods

Matching	Choice	$\bar{d}$	Profit
Fixed	43.96	11.32	67.13
Random	40.82	15.99	56.15
$t-$test (t_{44})	1.26 (.21)	2.88 (.006)	1.97 (.055)
s.d. test ($F_{23,21}$)	1.79 (.18)	6.89 ($\ll$.01)	5.91 ($\ll$.01)

(b) Average Over Last 5 Periods

Matching	Choice	$\bar{d}$	Profit
Fixed	44.1	10.56	61.8
Random	38.69	13.30	70.25
$t-$test (t_{44})	1.70 (.096)	1.05 (.30)	0.77 (.45)
s.d. test ($F_{23,21}$)	2.55 (.035)	6.91 ($\ll$.01)	2.65 (.028)

$\bar{d}$ the average misalignment of capacity choices between subjects in a pair.
$p-$values underneath the test statistics.

Table 8 summarizes the main results of this new experiment. First observe that subjects' choices are substantially and significantly below the Pareto efficient equilibrium level of 60. Second, observe the same pattern emerges here as in our main results. Averaged over all 30 rounds, profits are slightly higher under fixed matching, but over the last 5 periods, the inequality is reversed. Similarly, over the entire experiment, subjects are better-aligned under fixed matching, but the effect disappears over the last 5 periods. Both of these

Table 9: Choices and Profits Over Time

(a) Profits

	FM	RM
`round`	0.393**	0.874***
	[0.174]	[0.168]
`oth. choice`	1.544***	1.795***
	[0.210]	[0.185]
`demand`	1.595***	1.299***
	[0.150]	[0.061]
`cons`	-93.748***	-99.328***
	[10.527]	[9.828]
`N`	720	660
R^2	0.554	0.546

(b) Capacity Choices

	FM	RM
`round`	-0.031	-0.258***
	[0.084]	[0.072]
`lag oth. choice`	0.254***	0.088**
	[0.055]	[0.038]
`lag demand`	0.035***	0.001
	[0.012]	[0.018]
`cons`	31.125***	41.027***
	[2.848]	[2.827]
`N`	696	638
R^2	0.198	0.0347

*** significant at 1%; ** significant at 5%; * significant at 10%.
Clustered standard errors (at subject level) in brackets.
† `oth. choice` denotes one's match's choice.

suggest, as was the case above, that learning is actually faster under random rematching, a fact confirmed when one looks at Table 9(a), which shows that even after controlling for the demand realization and choice of one's partner, profits increase substantially more from round to round. Third, even stronger than our earlier results, the variance of both average profits and misalignment is substantially and significantly higher under fixed pairs than under random rematching, and this holds true both overall and over the last 5 periods.

While the previous discussion appear to suggest few differences from our previous results, Table 9(b) suggests that there may be some hope for fixed pairs in a stable demand environment. In particular, although the average capacity choice is well below the Pareto efficient equilibrium level of 60, it shows no signs of deteriorating in the fixed pairs condition. In contrast, under random rematching, subjects' capacity choices are declining over time. Moreover, as one might expect, there appears to be stronger positive reinforcement between one's current choice and the choice of one's partner in the previous period. Thus, there is some hope that a virtuous cycle could take hold. To be sure, in the random rematching treatment, this same positive feedback exists, but the effect is weaker and the overall fit of the model is worse.

7 The Role of Pre-Play Communication

In many real-world situations one, or both parties, have access to payoff relevant private information. For example, it is very likely that different firms operating as part of a supply chain each has access to their own unique sources of information which inform them about market conditions. If this information could be truthfully shared to all concerned parties, it would likely lead to a situation in which they could collectively earn higher profits. Unfortunately, such truth-telling mechanisms may be difficult to find, effectively making any pre-play communication cheap talk. This was the case in the experiments of Özer et al. (2011) where one firm, a manufacturer, has private information about demand that it would like to transmit to a supplier who must build capacity before demand is known. In such a setting, it is natural to expect that fixed matching can be efficiency-enhancing since the manufacturer may wish to build a reputation for transmitting accurate and reliable information. Indeed, this is precisely what Özer et al. (2011) find in their paper.

Consider the private information setting from Section 3. In this case, *both* firms have private information about the state of demand. To be concrete, suppose that $\theta_1 < \theta_2$. In this case, firm 1 believes that demand is uniform over $[\theta_1 - \eta, \theta_1 + \eta]$, while firm 2 believes that demand is uniform over $[\theta_2 - \eta, \theta_2 + \eta]$. If the two firms could truthfully share their private information, then both firms would have the belief that demand is uniform over $[\theta_2 - \eta, \theta_1 + \eta]$. That is, both firms have *better* information and both firms now have *common* information, both of which have the potential to lead to higher profits.

One of the interesting results of Hyndman et al. (2012) is that there exists an equilibrium in which players truthfully reveal their private information to each other and subsequently coordinate on the Pareto efficient equilibrium of the post-communication subgame. The basic intuition for this result is that if firms play the efficient equilibrium, then they actually have no incentive to lie, since lying about one's message can never strictly improve one's profits, but may lead to strictly lower profits. On the other hand, if firms do not expect to coordinate on the efficient equilibrium, then it is conjectured that truth-telling can never be sustained as an equilibrium since firms will have the incentive to inflate their message.

7.1 The Experiment

In order to look at the potentially efficiency-enhancing role of communication, which, because of the incentive to build a reputation for honesty, may be further enhanced by fixed matching, we conducted two additional treatments, under both fixed pairs and random rematching. In one treatment, which mimics the theoretical environment of Hyndman et al. (2012), subjects first received their estimates of demand and then were asked to send a message of the form: "My estimate is: x", where x was restricted to the interval $[15, 55]$, but did not have to match one's own signal; *i.e.,* subjects could lie about their true signal. In the second treatment, subjects also first saw their estimate of demand and could, *if they chose*, send a message of the same form. However, in this case, if they chose to send a message, 2 experimental points were deducted. In both treatments, after the communication phase, subjects were taken to a new screen where they could make their decision. On this screen, subjects saw their signal and also, if their partner decided to send a message, they saw their partner's message. Note that the random rematching treatments with cheap talk were reported in Hyndman et al. (2012); all other treatments are new to this paper.

Note that when messages are costly, our expectation was that subjects would be more willing to bear the cost under fixed matching because they are able to build up a relationship and reputation with their partner for the length of the experiment. In contrast, in a one-off relationship, such incentives are not present. Moreover, when messages are costly, we expect them to be more truthful since one could avoid the cost by simply not sending a message, rather than sending a "meaningless" message.

7.2 Results

In Table 10, we first report summary statistics on average profits and average level of misalignment, both over the course of the entire experiment and over the last 5 periods. Before comparing the fixed pairs and random rematching treatment, note that adding pre-play communication leads to higher profits and better alignment. Indeed, relative the the CI treatments average profits are between 7 and 11.7% higher, while relative to the PI treatments average profits are between 9.2 and 19% higher when communication is possible. Allowing communication has a similarly dramatic effect on the extent of misalignment.

Note also that just as was the case in our CI and PI treatments, subjects do not earn significantly more under fixed matching overall and, in the case of costly communication, over the last 5 periods, actually earn significantly less. Unlike our previous treatments, under cheap talk communication, subjects are no-better aligned under fixed matching.

Table 10: The Effects of Pre-Play Communication under Fixed and Random Matching

(a) Cheap Talk Communication

	All 30 Periods		Last 5 Periods	
	profits	misalignment	profits	misalignment
Fixed Matching	93.77	3.30	94.34	2.93
Random Matching	90.77	3.27	92.03	2.28
$t-$test (t_{42})	1.68 (.101)	0.06 (.954)	0.71 (.482)	1.46 (.152)
s.d. test ($F_{21,21}$)	1.05 (.911)	7.72 ($\ll$.01)	1.20 (.685)	5.43 ($\ll$.01)

(b) Costly Communication

	All 30 Periods		Last 5 Periods	
	profits	misalignment	profits	misalignment
Fixed Matching	91.49	2.47	90.06	2.38
Random Matching	93.03	3.85	97.91	3.22
$t-$test (t_{36})	0.80 (.429)	4.30 ($\ll$.01)	1.80 (.080)	1.76 (.087)
s.d. test ($F_{23,13}$)	1.27 (.665)	0.96 (.893)	1.37 (.568)	1.54 (.426)

$p-$values underneath the test statistics.

7.2.1 Costly Communication vs. Cheap Talk

In Table 11 we summarize some other variables of interest such as the number of rounds subjects sent messages, the fraction of messages which were honest, the absolute difference between one's message and one's estimate and, finally, average profits when a message was received. First consider the costly communication treatment. Consistent with the conjecture that subjects in a fixed match would have greater incentives to communicate and to build up a reputation for honesty, we do see that subjects in the fixed matching treatment send messages more frequently (13.6 vs. 8.9 rounds), are more frequently honest (0.676 vs 0.455) and distort their messages less (0.87 vs. 1.55). However, while these are the directionally correct comparative static, in no case is the difference statistically significant (in all cases $p > 0.1$). Furthermore, under both fixed pairs and random rematching, profits were significantly higher when a message was received than when a message was not received ($p < 0.03$). It is also of interest to note that subjects were more likely to send a message the higher was their estimate, which makes sense because at higher estimates, the scope for coordination failures is greater, making communication potentially more valuable. Finally, while there does not appear to be any relationship between the estimate received and the probability of lying, it

Table 11: Costly Communication vs. Cheap Talk

Messages	Matching	# Rounds Message Sent	Frac. Honest Messages	$\|mess - est\|^{\sharp}$	Profits When Message Rec'd	Profits When Message Not Rec'd
Costly	Fixed	13.6	0.676	1.89	94.3	84.2
Costly	Random	8.9	0.455	2.29	98.9	90.2
Free†	Fixed	30	0.136	3.69	93.8	—
Free†	Random	30	0.279	3.71	90.8	—

♯ We condition on the fact that $message \neq estimate$.
† Subjects were forced to send a message in the cheap talk treatments.

does appear that subjects distort their estimates by a greater amount for extremely low and extremely high estimates and less so for intermediate messages.

Consider next the cheap talk treatments. Comparing the fixed pairs and random rematching treatments, we actually have the opposite comparative static, with subjects lying *more frequently* (though the difference is not significant). Conditional on lying, there is virtually no difference in the amount by which subjects lie.

Finally, it is of interest to compare costly communication and cheap talk directly. Here the differences are incredibly stark. Pooling across matching protocols, we see that subjects truthfully report their estimate significantly more often under costly communication than under cheap talk. Moreover, even when subjects lie about their estimate, they do so by a smaller amount under costly communication. In both cases, the differences are significant at $p < 0.01$. Thus, it seems that when it is costly to communicate, subjects are more honest than when talk is cheap. Moreover, it turns out that this (weakly) pays off since the average payoffs of subjects, conditional on receiving a message, in the costly communication treatment are higher than in the cheap talk treatment ($p = 0.067$).

7.2.2 How Messages Affect Choices

We can also gain insight on how both the matching protocol and the treatment influence how subjects incorporate messages into their final choice. Observe that if subjects believe that the messages they receive are truthful, then they should give equal weight to the message they receive and their own estimate. To the extent that subjects believe messages to be untruthful, we would expect subjects to give more weight to their own estimate and to discount the message received. In Table 13, we report the results of random effects Tobit regressions of capacity choice on one's estimate and the message received. The results are striking. First,

Table 12: Random Effects Tobit of Capacity Choice on Estimate & Message Received

	Costly Comm.		Cheap Talk	
	FM	RM	FM	RM
`estimate`	0.426***	0.549***	0.652***	0.739***
	[0.023]	[0.059]	[0.025]	[0.023]
`oth.message`	0.532***	0.395***	0.296***	0.193***
	[0.024]	[0.059]	[0.025]	[0.022]
`cons`	0.369	1.027	1.079	0.756
	[0.595]	[1.337]	[0.727]	[0.597]
`N`	326	124	660	660
`LL`	-681.2	-314.6	-1717	-1656

*** significant at 1%; ** significant at 5%; * significant at 10%.
Standard errors in brackets.

comparing fixed pairs with random rematching, subjects place significantly more weight on the message received under fixed matching. Second, comparing costly communication with cheap talk, we also see that subjects place significantly more weight on the message received when communication is costly.[6] Thus, messages that are costly to send and come from a fixed pairing are viewed by subjects as the most credible, while messages that are free to send and come from a one-time pairing are viewed as the least credible.

Finally, note that while subjects never learned the signals received by their partner, there were still two ways that subjects could tell that their partner lied: if $|\theta_i - M_j| > 10$ or $|x - M_j| > 5$, where M_j is the message sent by player j. That is, if the message received was more than 10 points away from one's own estimate or if the message received was more than 5 points away from demand. In the former case, one learns *before* making her capacity choice that her partner is lied, while in the latter case, one only learns *after* making her capacity choice and demand is realised. In both cases, it is natural to expect subjects to react to this information when making their capacity choice and evaluating how much weight to give to the message received. In Table **??** we look at how capacity choices are influenced by *ex ante* and *ex post* discovered lies. We pool the data across costly communication and cheap talk treatments.

As can be seen, in both the fixed pairs and random rematching treatments, when an *ex ante* lie has been discovered, subjects give significantly less weight to the message received and significantly more weight to their own estimate when making their capacity decision. In contrast, when a subject learns that their match lied to them in the previous period (`lag`

[6]To test for significance, we pooled the data and included treatment or matching interaction terms. In both cases, the treatment interactions were significant $p < 0.01$.

Table 13: Random Effects Tobit of Capacity Choice When Lies Are Discovered

	Fixed	Random
`estimate`	0.517***	0.568***
	[0.025]	[0.033]
`(ex ante lie)`×`estimate`	0.190***	0.250***
	[0.041]	[0.045]
`(lag lie)`×`estimate`	0.089**	0.043
	[0.041]	[0.043]
`oth. message`	0.443***	0.370***
	[0.025]	[0.032]
`(ex ante lie)`×`oth. message`	-0.182***	-0.261***
	[0.039]	[0.041]
`(lag lie)`×`oth. message`	-0.104**	-0.044
	[0.040]	[0.041]
`cons`	0.745	0.658
	[0.496]	[0.565]
`N`	874	676
`LL`	-2115	-1666

*** significant at 1%; ** significant at 5%; * significant at 10%.
Standard errors in brackets.
`ex ante lie` takes value 1 if |**`estimate`** − **`oth. message`**| > 10 and value 0 otherwise.
`lag lie` takes value 1 if, *in the previous period*, either |**`estimate`** − **`oth. message`**| > 10 or |**`demand`** − **`oth message`**| > 5 and value 0 otherwise.

`lie`), players in the random rematching treatments do not make any significant adjustments to their choice rule, which makes sense because of random rematching. On the other hand, in the fixed matching treatment, when a subject was knowingly lied to in the previous period, subjects give less weight to the message received and place more weight on their own estimate in the current period. Thus, once a lie has been discovered, trust breaks down and communication becomes less effective.

7.2.3 Is Honesty The Best Policy?

Finally, in Table 14 we look at average payoffs conditioning on the type of message received (M_j): an honest message, a message that was a "small lie" (i.e., $|M_j - \theta_i| \leq 10$) and a message that was a "big lie" (i.e., $|M_j - \theta_i| > 10$). This latter distinction is important because upon receiving a message such that $|M_j - \theta_i| > 10$, then the subject *knows for sure* that she has been lied to by her match. On the other hand, upon receiving a message such that $|M_j - \theta_i| \leq 10$, the subject cannot tell for sure whether she has been lied to but may be able to do so at the end of the period, after demand has been realised. There is a consistent pattern across treatments and matching protocols: First, it is generally better to send an honest message than to send no message at all. Second, small lies may be profitable (though the difference is never significant in any individual treatment, and only weakly significant when pooling the data), but big lies are decidedly unprofitable. Thus, while honesty may not be the best policy, it is generally best to not stray too far from the truth.

Table 14: The Consequences Of Lying: Average Profits Given The Message Received

	No Message		$\theta_j = M_j$		$\theta_j \neq M_j$ & $\lvert M_j - \theta_i \rvert \leq 10$		$\theta_j \neq M_j$ & $\lvert M_j - \theta_i \rvert > 10$
Costly Comm; FM	84.2	$\underset{0.04}{<}$	93.2	$\underset{0.73}{=}$	95.2	$\underset{0.02}{>}$	68.4
Costly Comm; RM	90.2	$\underset{0.28}{=}$	94.4	$\underset{0.11}{=}$	103.8	$>^{\dagger}$	91.0
Cheap Talk; FM	—	—	88.8	$\underset{0.26}{=}$	94.3	$\underset{0.01}{>}$	83.5
Cheap Talk; RM	—	—	87.4	$\underset{0.30}{=}$	92.1	$\underset{\ll 0.01}{>}$	73.0
Pooled	86.5	$\underset{0.11}{=}$	91.4	$\underset{0.096}{<}$	95.7	$\underset{\ll 0.01}{>}$	77.6

† Only one observation; therefore, formal test not possible.

8 Conclusions

In this paper, we set-out to test the conventional wisdom that long run relationships in environments with strategic complementarities should lead to higher average profits and better alignment of decisions than in one-shot games. To do this, we conducted experiments based on the two-player "newsvendor" games studied in Hyndman et al. (2012) under both fixed pairs and random rematching.

Consistent with the conventional wisdom, we do find that capacity choices are (often significantly) better-aligned under fixed pairs than under random rematching. However, when it comes to the main variable of interest, namely profits, then fixed matching fails to deliver. Specifically, while average profits were sometimes higher under fixed matching over the entire experiment, when we look only at the last five periods, random rematching generally out-performs fixed pairs. In addition, we also found that the standard deviation of average profits was (often significantly and substantially) higher under fixed pairs than under random rematching. Thus, while fixed matching produced some high earning subjects, it also lead to some subjects who performed very poorly — so much so that, sometimes, the *complete coordination failure* actually emerged, with both subjects choosing the lowest possible capacity for the final several periods.

The main explanation for this result, which goes against a body of existing results from experimental economics, is that initial conditions are more important under fixed pairs, because they are more indicative of future play. Indeed, this is precisely what we found: under fixed pairs, those pairs that managed to be well-aligned in early rounds had higher earnings than those subjects who were substantially misaligned. In the random rematching treatment, initial conditions appeared to have no influence on overall earnings.

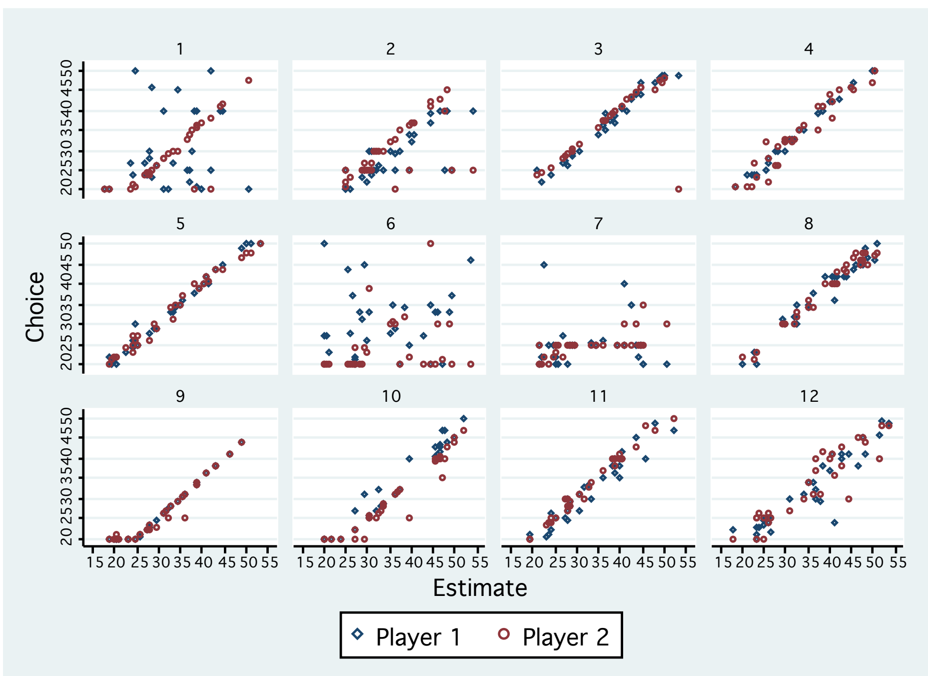
We also showed that communication enhances profits under both fixed pairs and random rematching, and regardless of whether communication was costly or cheap talk. With costly communication, under fixed pairs, the frequency of messages was slightly, but not significantly, higher than under random rematching; moreover, those messages that were sent were more likely to be honest than those sent under random rematching. The main differences in our experiments with communication were not between fixed pairs and random rematching, but rather between costly communication and cheap talk. With costly communication, subjects were more frequently honest and, when they did lie about their signal, they did so by a smaller amount than under cheap talk. Indeed, conditional on receiving a message in the costly communication treatments, subjects actually earned more than did those in the cheap talk treatments. Finally, our results suggest that while small lies may modestly improve profits, big lies lead to significantly lower profits.

Taken as a whole, our results bring into question the desirability of long-term relationships. This follows because there appears to be a strong first-impressions bias, which, if things do not go well from the beginning, can sour the future relationship. In such cases, either managers should strive to avoid over-emphasizing early mistakes, or design strategies that allow an early exit from relationships that got off to a rocky start. We are actively pursuing this in ongoing experimental work. One additional implication for our findings is the importance of screening candidates for long term relationship. If, as our results suggest, long term relationships are not as simple to execute as previously thought, then partners who understand this should be preferred to those that do not.

A A Look At Individual Behaviour

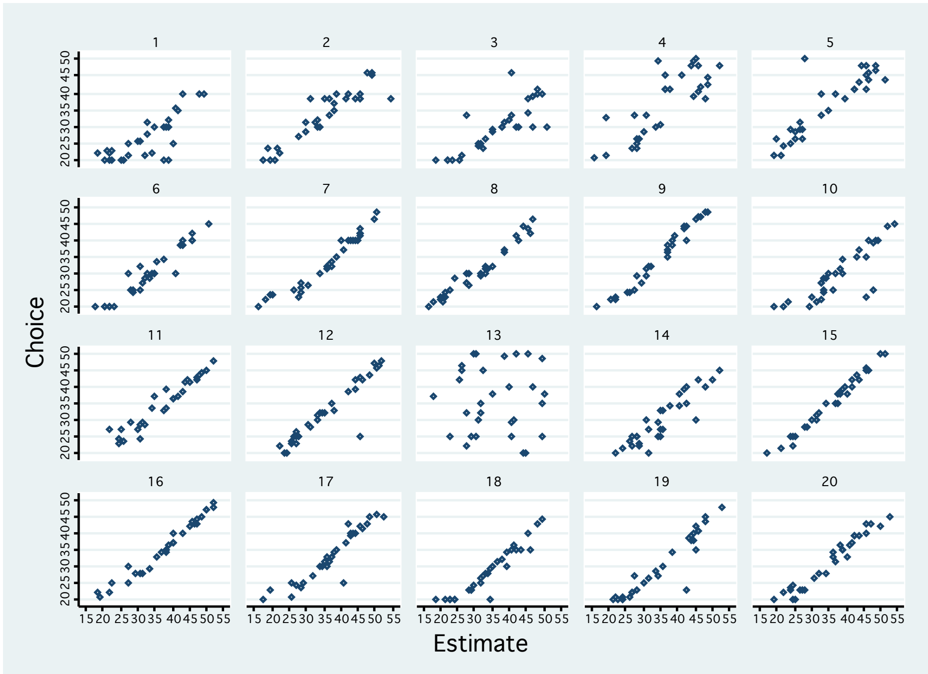
In this section, we would like to briefly take a look at the individual behaviour of subjects in our CI and PI treatments. Such an exercise can also help us to gain insights into the differences between fixed pairs and random rematching. Consider first Figure 3, which shows a scatter plot of choice against the estimate for the fixed matching treatment of the CI game. Each of the sub-figures represents the choices of the 12 fixed pairings. As can be seen, there is a great deal of heterogeneity in the choice rules across the 12 groups. In particular, groups 3, 4, 5, 8, 9 and 11 appear to be using very similar choice rules, while in groups 1, 6 and 7 at least one of the subjects in each group has very erratic behaviour, dooming the groups to substantially lower earnings.

Figure 3: Common Information Game: Choice Rules (Fixed Matching)



Turn next to Figure 4, which shows scatter plots of choice against estimate for the random rematching treatment of the CI game for each of the subjects. As can be seen, nearly all of the subjects have reasonably well-defined choice functions, with only subject 13 behaving in an especially erratic manner. Moreover, since this is a random rematching treatment, the influence subject 13's erratic behaviour is spread out amongst all of the subjects in the session, rather than concentrated on a single subject as in the fixed matching treatment.

Figure 4: Common Information Game: Choice Rules (Random Matching)



Turning now to the PI games, we see that a fairly similar pattern emerges, with the members of some groups in the fixed matching treatment having employing fairly similar choice rules (groups 4, 5 and 7) and others appearing to be much more erratic (groups 2, 3 and 9). Note that the fit is not as tight in the PI games, which is not surprising, since subjects received private signals making some misalignment inevitable, which may lead to difficulty converging to a similar choice rule.

Again, in the random rematching treatment, few subjects have truly erratic behaviour (subject 10 and, to a lesser extent, subjects 6 and 14), with the rest having fairly well-defined choice rules. Moreover, just as with the CI games, in the random rematching treatment of the PI game, the negative influence of these three subjects' behaviour is spread across all subjects in the same session.

Finally, we can quantify the intuition that the random rematching treatments appear to induce more "stable" behaviour. Specifically, for each subject we run the following regression:

$$\texttt{choice}_{it} = \alpha + \beta_1\theta_{it} + \beta_2(\theta_{it} - 25) \cdot [\theta_{it} < 25] + \beta_3(\theta_{it} - 45) \cdot [\theta_{it} > 45] + \nu_{it},$$

where $[A]$ is an indicator variable which takes value 1 if A is true and θ_{it} is the estimate received by subject i in round t. From this regression, we then take the R^2 as a measure of

Figure 5: Private Information Game: Choice Rules (Fixed Matching)

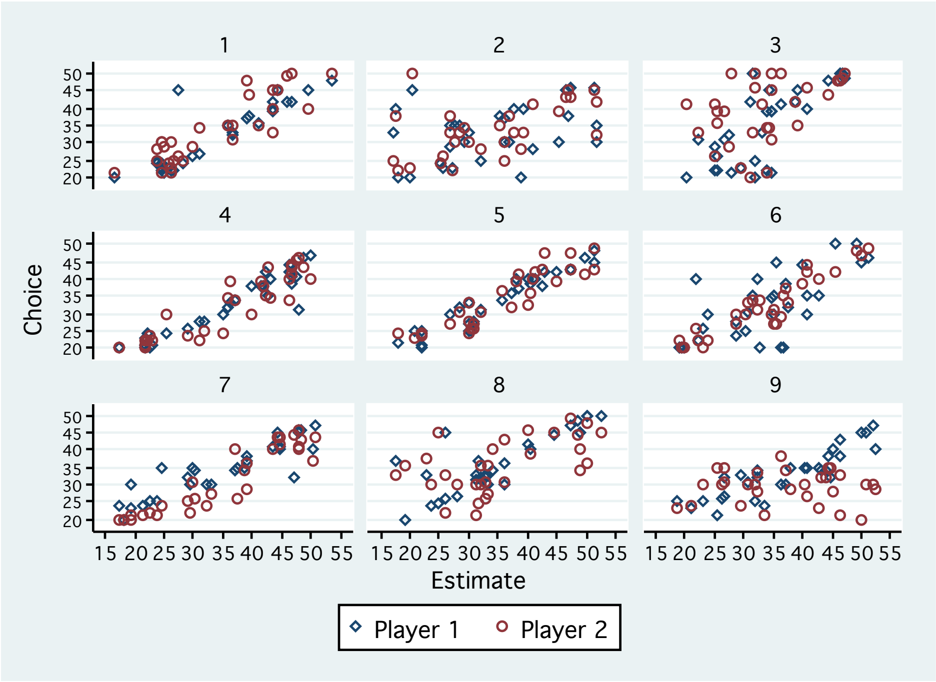
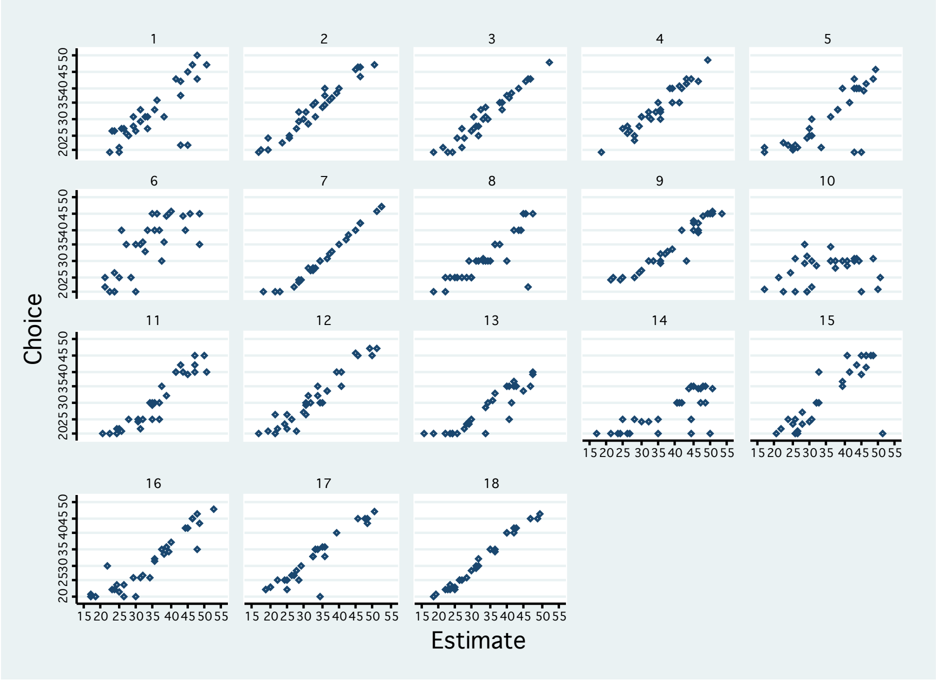


Figure 6: Private Information Game: Choice Rules (Random Matching)



fit. Comparing the fixed pairs and random rematching treatments, we see that the average R^2 is 0.723 under fixed rematching and 0.816 under random rematching. The $p-$value of a two-sided test is 0.113, just missing out on significance. However, we are able to reject the null hypothesis that the standard deviation is equal (0.305 under fixed pairs vs. 0.208 under random rematching) at $p = 0.0196$. Thus, as the figures indicate, the variability of subjects' choice rules is significantly higher under fixed pairs.

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General Instructions

This is an experiment on the economics of decision-making. Your earnings will depend partly on your decisions, partly on the decisions of others and partly on chance. By following the instructions and making careful decisions you will earn varying amounts of money, which will be paid at the end of the experiment. Details of how you will make decisions and earn money are explained below.

In this experiment, you will participate in 30 **independent** decision problems (rounds). In all rounds, you will be **randomly** matched with another participant. In what follows, we will refer to the person with whom you are matched as your *match*. After each round, you will be **randomly** matched with another participant for the next decision problem, and so on. At no point in the experiment will you know the identity of your matches.

Decision Problem

In each round you will be asked to choose a number between 20 and 50. Your match will face the same choice problem. If we denote your choice by K and your match's choice by M, then your decisions will result in the following earnings (the explanation of Q will be given later):

$$\texttt{earnings} = 5 \cdot \min\{K, M, Q\} - 2 \cdot K$$

That is, when determining your profits, we first look for the **smallest** number of K (your decision), M (your match's decision) and Q. Whatever that smallest number is, we will multiply it by 5. From that number, we will deduct 2 times the number you chose.

For example, if you chose $K = 30$, your match chose $M = 25$ and $Q = 27$, then the smallest number is $M = 25$, which means that you will earn $5 \cdot 25 - 2 \cdot 30 = 125 - 60 = 65$ points. On you other hand, your match would earn $5 \cdot 25 - 2 \cdot 25 = 125 - 50 = 75$ points. The software contains a calculator feature to help you calculate your earnings under any hypothetical scenario of your choosing.

What is Q?

Q is a number (up-to two decimals) between 20 and 50 randomly determined by the computer. That means any number between 20 and 50 is equally likely to be picked by the computer.

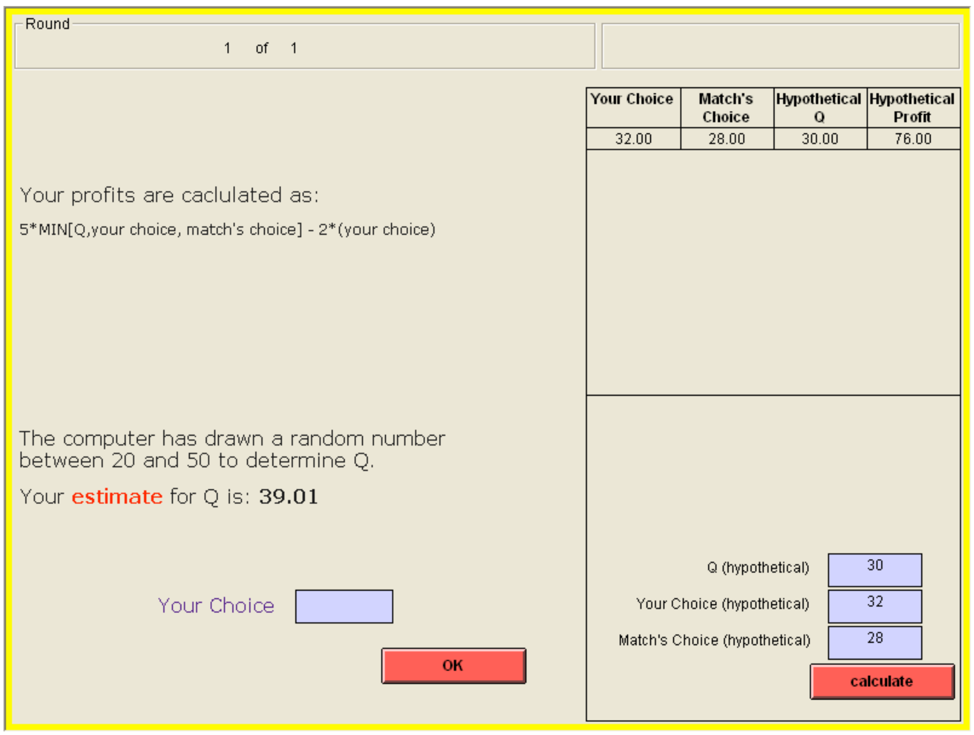
The computer picks Q before each round and the numbers are independent across rounds. That is, the Q chosen by the computer in a round has nothing to do with the Q picked in any other round.

Before you make a decision you will **not** be told what Q is but instead you will receive an estimate of Q, which we will denote by E. Let's be more precise. After the computer randomly determines Q, it also picks a random number (up-to two decimals) between $Q - 5$ and $Q + 5$. This is your estimate E. Any number between $Q - 5$ and $Q + 5$ is equally likely to be picked by the computer. Although E does not tell you what Q exactly is, it gives an estimate of it. For example if you receive an estimate $E = 37.07$, then you know that Q is **not less than** $37.07 - 5 = 32.07$ and it is **not more than** $37.07 + 5 = 42.07$.

Note that although Q will be the **same** for both you and your match, your **estimates** can be different. That is, for the same Q, the computer also randomly picks another estimate exactly in the same manner for your match. Your estimate and your match's estimate are chosen independently. Therefore, it is very likely that they will be different numbers; however, both estimates will be between $Q - 5$ and $Q + 5$. Note also that while Q will be between 20 and 50, your estimate of Q can be between 15 and 55.

The Computer Screen

In each round, you will see the following computer screen:



On the left side of the screen is where you will see your estimate and make your final decision. In this example, you see that your estimate of Q is 39.01. On the right hand side of the screen, you can enter *hypothetical* values for your decision, your match's decision and the value of Q in order to see what your earnings would be under each hypothetical scenario. You can compute your hypothetical earnings under as many different scenarios as you choose.

Your Decision

After you are given your estimate, E, you are ready to make a decision. Your decision is simply to choose a number between 20 and 50. The payoffs for this decision are as described above. Note that you and your match make your decisions *at the same time*; that is, you must choose your number without knowing the choice made by your match.

Payoffs

Your potential earnings in each round depend on your choice, on your match's choice, and on Q. After both you and your match have made your choices, you will see the following screen. You see your estimate of Q, the true value of Q, and your profit. You also see the decision made by both you and your match. In this example, you see that while your estimate of Q was 39.01, its true value was 43.86.

At the end of the 30 rounds, we will add all your earnings in order to determine your total points. This total will be converted to a dollar amount according to the rule:

$$\$1 = 165 \text{ points.}$$

This amount will then be added to the $5.00 participation fee to give your final payment. Payments will be made in private, in cash, after the completion of the experiment.

Rules

Please do not talk with anyone during the experiment. We ask everyone to remain silent until the end of the last decision problem.

Your participation in the experiment and any information about your earnings will be kept strictly confidential. Your receipt of payment and the consent form are the only places on which your name will appear. This information will be kept confidential in the manner described in the consent form.

If you have any questions please ask them now. If not, we will proceed to the experiment.