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ORIGINAL ARTICLE

Endogenous timing in duopoly: experimental evidence

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Abstract In this paper we experimentally investigate Cournot duopolies with an extended timing game. The timing game has observable delay, that is, firms announce a production period (one out of two periods) and then they produce in the announced sequence. Theory predicts simultaneous production in the first period. With random matching we find that, given the actual experimental behavior in the subgames, subjects play a timing game more akin to a coordination game with two symmetric equilibria rather than the predicted game with a dominant strategy to produce early. As a result, a substantial proportion of subjects choose the second period.

Keywords Commitment · Endogenous timing · Observable delay · Cournot · Stackelberg · Experimental economics

JEL Classification Numbers C72 · C92 · D43

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1 Introduction

There is substantial interest in the theoretical literature on endogenous timing in games. This literature started with Saloner (1987), Hamilton and Slutsky (1990), and Robson (1990) and developed into a rich and active research area in game theory with recent contributions by Henkel (2002), Matsumura (2002), Normann (2002) and van Damme and Hurkens (2004). The basic question these models try to answer is simple but significant. When are firms likely to play either a simultaneous-move game or a sequential-move game? In models with endogenous sequencing, the order of output or price decisions is not exogenously specified. Instead, it is derived from firms' decisions in a timing game.

Several recent experiments have attempted to validate the theory empirically[1] but support for the theory was by and large not found. In these experiments, simultaneous-move Cournot outcomes are modal – in contrast to the theory which predicts Stackelberg equilibria here. Even when sequential moves occur, Stackelberg leaders produce less than predicted while followers produce more.

Why does theory perform rather poorly in experiments? The theory underlying the experiments predicts the emergence of Stackelberg equilibria and typically there exist two Stackelberg equilibria. This causes two problems. First, coordination problems occur in the experimental markets since either firm may emerge as the Stackelberg leader. Neither Stackelberg equilibrium is preferable to the other and subjects find it difficult to coordinate on one.[2] Second, it is difficult to see from a behavioral perspective why players should coordinate on an equilibrium with large payoff differences (as it is the case in a Stackelberg leader–follower outcome). It is well known that subjects in experiments may exhibit an aversion against disadvantageous inequality (e.g., Fehr and Schmidt 1999). Such inequality aversion might render the Stackelberg equilibria unappealing candidates for convergence in an experiment.

Recent theoretical research, in turn, attempts to rationalize the experimental data on endogenous timing by allowing players to be inequality averse. Lau and Leung (2006) analyze the standard Stackelberg duopoly with *exogenous* timing when players are inequality averse. They show that a simplified version of Fehr and Schmidt's (1999) model is consistent with the experimental data of Huck, Müller and Normann's (2001). Santos-Pinto (2006) applies the Fehr and Schmidt (1999) framework to Hamilton and Slutsky's (1990) action

[1] Huck et al. (2002) investigate Hamilton and Slutsky's (1990) action commitment game. Müller's (2005) experiments are on Saloner's (1987) model, extended by Ellingson (1995). Fonseca et al. (2005) analyze endogenous timing with asymmetric cost, as modeled by van Damme and Hurkens (1999). See also Huck et al. (2001) for experiments on exogenously Stackelberg games.

[2] Most of the theoretical literature has ignored the coordination problem firms face in a duopoly with endogenous timing. An exception are van Damme and Hurkens (1999, 2004) who analyze a timing game with cost differences between firms. In their models, a unique Stackelberg equilibrium with the efficient firm as the Stackelberg leader is selected. However, Fonseca et al. (2005) still observe simultaneous play as the modal case in related experiments.

commitment game and Saloner's (1987) timing game. He can rationalize many but not all aspects of the experimental data of the papers cited in footnote 1. In particular, inequality aversion cannot explain delay observed in the experiments on Hamilton and Slutsky's (1990) action commitment game.

The motivation for this experimental paper is to further explore the reasons for the weak predictive power of standard game theory and the role of inequality aversion and coordination failure by investigating a timing game with a unique and symmetric equilibrium. The basis of the experiments is Hamilton and Slutsky's (1990) extended game with observable delay in a quantity-setting framework. The equilibrium of this extended timing game is in simultaneous moves and has equal quantities as firms have symmetric costs. At first sight, it appears that in these new experiments neither coordination failure nor inequality aversion should hinder the emergence of the predicted equilibrium. If symmetric outcomes fueled by inequality aversion and coordination failure have been previously observed even though they were not predicted, then it appears that the theory should be vindicated if symmetric outcomes are predicted. However, sequential-move Stackelberg leader–follower outcomes can still occur in the experiment – if only by mistake. We argue below that lower profits in the asymmetric timing subgame (possibly due to inequality aversion) can transform the timing game from one with a unique and symmetric equilibrium into a coordination game with two symmetric equilibria. Accordingly, coordination failure and inequality aversion can still play a role and affect the outcomes in the experiments.

A second novelty is that we run experimental sessions both with randomly matched participants as well as with participants in fixed duopoly pairs. Previous experiments have simulated one-shot interaction (random matching) between participants since the endogenous timing models are based on static games. However, repeated interaction is the norm in the field. With fixed matching, the likelihood of collusion is increased and then the timing of duopoly decisions may have an entirely different nature (on which we elaborate in the next section). Further, firms should be better able to resolve coordination failure problems with fixed matching. The reason is that it is more difficult for subjects to form accurate beliefs about their counterparts' actions with random matching than with fixed matching. This provides another motivation for analyzing a treatment with fixed matching as this should, ceteris paribus, lead to a higher frequency of observations consistent with theory.

It turns out that our results do not thoroughly support the theory, as in previous studies. Many timing decisions are out of equilibrium as subjects often delay their output decisions to the second period. For example, in the treatment with random matching, it turns out that Stackelberg leader profits in the sequential-move subgames are indeed lower than in simultaneous-move Cournot subgames. That is, given the behavior in the experiments, the timing game subjects are actually playing is more akin to a coordination game with two symmetric equilibria rather than a game with a dominant strategy to produce early. Furthermore, we will argue below that our results are consistent with recent findings of Tykocinski and Ruffle (2003) who suggest that subjects often have a preference to delay their decisions even when waiting does not provide any direct material gain or additional information. In general, our results suggest that additional forces (next to inequality aversion and coordination problems) that are not captured in the endogenous timing models might influence participants' decisions.

2 Model and predictions

In Hamilton and Slutsky's (1990) extended game with observable delay two firms can produce in one of two possible periods (period 1 or 2). A pure strategy for firm $i = 1,2$ is a choice of a production period $t_i \in \{1,2\}$ and a set of functions $\tau_i : \{(1,1), (1,2), (2,1) \times R^+, (2,2)\} \rightarrow R^+$ which is firm i's quantity choice as a function of production periods, (t_1, t_2), and the output of firm $j \neq i$ when firm i is the Stackelberg follower. Given the decisions to produce in period 1 or 2, firms will not randomise over outputs.

In the experiments we used the following linear inverse demand function

$$p(q_1 + q_2) = \max\{30 - (q_1 + q_2), 0\} \tag{1}$$

where q_i denotes firm i's output. Linear costs of production in both periods were given by

$$C_i(q_i) = 6q_i, \qquad i = 1,2. \tag{2}$$

Profits are denoted by $\Pi_i = p(q_1 + q_2)q_i - 6q_i$.

Consider the predictions in the static game first. We start with the second stage. In the subgame with $t_1 = 1$ and $t_2 = 1$, firms play the simultaneous-move Cournot equilibrium in period 1 with $q_i = 8$ and resulting in payoffs of $\Pi_i = 64$ $(i = 1,2)$. The same holds in the subgame with $t_1 = 2$ and $t_2 = 2$. In the subgame with $t_1 = 1$ and $t_2 = 2$, firms play the Stackelberg equilibrium with firm 1 choosing $q_1^L = 12$ in period 1 whereas firm 2, the Stackelberg follower, chooses $q_2^F = 6$ in period 2. This implies payoffs of $\Pi_1^L = 72$ and $\Pi_2^F = 36$. Outputs and payoffs for the subgame with $t_1 = 2$ and $t_2 = 1$ are $q_2^L = 12, q_1^F = 6$ and $\Pi_2^L = 72$ and $\Pi_1^F = 36$. Then we go back to the first stage. From $\Pi_i^L = 72 > \Pi_i = 64$ (if $t_j = 2$) and $\Pi_i = 64 > \Pi_i^F = 36$ (if $t_j = 1$), choosing period 1 is a dominant strategy and thus we have $t_1 = t_2 = 1$ in the unique subgame perfect equilibrium.

With repeated interaction in the fixed matching sessions, it is well known that collusion can occur even though the game is only finitely repeated (Selten and Stoecker 1986). It is easy to verify that $q_i = 6$ is the symmetric joint-profit maximizing strategy which results in payoffs of $\Pi_i = 72$ $(i = 1,2)$. Given both firms collude, the timing decisions are immaterial. However, in particular in the early rounds of the experiment, there may be uncertainty about the other players' willingness to collude, and in that case timing decisions may play an important role. For example, producing at $t_i = 2$ may resolve the uncertainty whether the other player colludes, and at $t_i = 2$ non-colluding rivals may also be punished. Producing at $t_i = 1$ provides an opportunity to signal collusive intents. Note that if these incentives for moving first or second materialize, they would be rather different from those in the static endogenous timing models.

When players are inequality averse (e.g., Fehr and Schmidt 1999), several new issues come into play. We refer here to the comprehensive studies of Lau and Leung (2006) and Santos-Pinto (2006) and highlight only a few insights important for our setting. Firstly, in the Stackelberg subgames, inequality aversion will generally cause the follower to produce more than the best reply whenever $q_i^L > 8$, regardless of the matching scheme. If Stackelberg leaders are playing against an inequality averse follower but still choose $q_i^L > 8$, this can reduce the Stackelberg leader profits to be below the profits in the simultaneous-move Cournot games. (Whether this actually occurs depends on the follower's output which, in turn, depends on the degree of inequality aversion). If this is the case, we have a different prediction for the static game. Choosing period 1 is no longer a dominant strategy, and the game is transformed into one with two symmetric timing equilibria ($t_1 = t_2 = 1$ and $t_1 = t_2 = 2$). Second, inequality aversion can facilitate collusion. Inequality averse subjects are less inclined to exploit attempts to cooperate even with random matching,[3] so, this generally gives rise to collusion. Ultimately, this implies that collusion can occur not only with fixed but also with random matching. However, with random matching, successful cooperation might still be difficult as subjects cannot be sure about the type of the player they play against. The timing opportunities may also be used to facilitate collusion even in the one-shot game (in the same spirit as outlined above for the repeated game).

3 Experimental design and procedures

We implemented two treatments: one with random and one with fixed matching among participants. The experimental markets were repeated over 30 rounds in order to allow for learning. A minor difference to the game as formally stated above is that subjects had to choose their quantities from a truncated and discretized strategy space, yielding a standard payoff bi-matrix. Subjects had to choose integer quantities between 3 and 15 (see Appendix B).[4]

In both treatments, subjects got individual feedback about what happened in their market at the end of each round. That is, the computer screen[5] showed the production period, the quantity, and the profit of both duopolists. In sessions with random matching (henceforth Random), subjects were rematched by the computer at the beginning of each round. We conducted five random-matching

[3] As pointed out by an associate editor, collusion can even occur when a perfectly selfish Stackelberg leader meets a strongly inequality averse follower. If the first mover knows the type of the follower, we will observe $q_i^L = 6$ and $q_j^F = 6$.

[4] We used the same payoff matrix as in Huck et al. (2001).

[5] We used the software toolbox "z-Tree" (Fischbacher 1999).

Table 1 Relative frequency of period 1 choices

	Third 1	Third 2	Third 3
Random	57	69	72
Fixed	50	51	53

sessions with ten participants each. The two sessions with fixed matching (henceforth Fixed) had ten participants as well, so there were five fixed duopoly pairs in each session. Treatments were conducted in an identical way, except for the matching scheme.

The experiments were conducted at Royal Holloway College, University of London, in spring and summer 2002. Altogether 70 subjects participated in the experiment. They were students from various departments, many from fields other than economics or business administration.

In the instructions (see Appendix A) subjects were told that they would act as a firm which, together with another firm serves a market for 30 rounds, and that in each round both were to choose when and how much to produce. After having read the instructions, participants could privately ask questions. Before the first round was started subjects were asked to answer two control questions (which were checked) in order to make sure that everybody had full understanding of the payoff table.

The monetary payment was computed by using an exchange rate of 300 "points" for £1 and adding a flat payment of £4.[6] Subjects' average earnings were £13.02 ($19.53 at the time) including the flat payment. The sessions lasted about 60 to 90 min.

4 Experimental results

We report the results of treatments Random and Fixed separately. When discussing the results, we often refer to third 1 (rounds 1–10), third 2 (rounds 11–20), and third 3 (last ten rounds).

4.1 Random matching

Table 1 shows the evolution of the relative frequency of t=1 choices over time. In Random the relative frequency of t=1 decisions increases from 57 to 72% (from third 1 to third 3). This is a clear trend towards equilibrium timing behavior. However, the relative frequency of $t = 1$ choices is still below the equilibrium prediction of 100% towards the end of the experiment. Moreover the increase slows down considerably from third 2 to third 3.

[6] This payment was made since subjects could have made losses in the game.

Table 2 Average individual quantities in the subgames over time

		Third 1		Third 2		Third 3	
		$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$
Random	$t=1$	9.0, 9.0	10.6, 7.8	9.0, 9.0	10.3, 8.9	8.7, 8.7	9.3, 9.0
	$t=2$		8.3, 8.3		9.1, 9.1		8.5, 8.5
Fixed	$t=1$	9.0, 9.0	9.2, 8.4	9.4, 9.4	9.0, 8.0	9.7, 9.7	8.5, 7.7
	$t=2$		9.0, 9.0		9.2, 9.2		7.6, 7.6

Since we have random matching, the relative frequency of timing decisions immediately implies the relative frequencies of the timing outcomes. The equilibrium prediction with both firms choosing $t = 1$, occurs with only 55% (third 3). Simultaneous play in $t = 2$ occurs with 10% and sequential play with the remaining 35% (third 3). Since $t = 1$ choices increase over time, the relative frequency of the subgame where both firms choose $t = 1$ increases whereas the frequency of the other two subgames decreases.

Once firms have made their timing choices, they know in which sequence they choose their outputs. How do firms behave in the subgames? Table 2 shows average individual quantities across time contingent on the timing decisions.[7] In Random , we observe that after a short learning phase (third 1), quantity choices in the $t_1 = t_2 = 1$ and $t_1 = t_2 = 2$ subgames are almost identical and move towards the Cournot prediction.

However, in the asymmetric subgame, attempts to exploit a first mover advantage by choosing a higher than Cournot quantity of 8 is punished by followers. Note, for instance, that the best response to a first mover's quantity of 10 and 9 is 7 and 8 respectively.[8] Moreover, first movers' output is smaller than predicted (12 units). As a consequence, both Stackelberg leaders' and followers' payoffs are smaller than the payoffs in the two simultaneous subgames, as shown in Table 3.[9]

In fact, in the last two thirds the payoffs in the two simultaneous-move subgames are almost the same and higher than in the sequential-move subgame. This has two effects. First, it provides an incentive for the subjects to avoid the sequential-move subgame by choosing the first production period $t = 1$ more often (which also avoids to get into the disadvantageous position of a Stackelberg follower). This might explain why we see a clear increase in $t = 1$ choices during the first two thirds. Second, the fact that over time payoffs in

[7] Since the two players in the game are symmetric there are two, albeit identical subgames where one player moves first and the other delays. However, that same symmetry allows for the aggregation of the data as if it was only one subgame. We then omit the lower left-hand corner cell in all matrices in Tables 2 and 3.

[8] Note that followers can cheaply punish leaders for over-producing. Consider the case where a leader sets an output of 10, as is the case on average for the early part of the experiment. A follower by selecting an output level 1 unit higher than the best reply foregoes 1 unit of profit but this costs the Stackelberg leader 10 units of profit.

[9] Significant at the 5% level using a Wilcoxon signed-ranks test, where each observation corresponds to the average profits across players from a session.

Table 3 Average individual profits in the subgames over time

		Third 1		Third 2		Third 3	
		$t = 1$	$t = 2$	$t = 2$	$t = 2$	$t = 1$	$t = 2$
Random	$t = 1$	49.8, 49.8	53.9, 40.2	49.8, 49.8	43.7, 37.2	56.2, 56.2	48.7, 46.3
	$t = 2$		58.0, 58.0		50.1, 50.1		55.9, 55.9
Fixed	$t = 1$	47.3, 47.3	52.6, 49.3	43.6, 43.6	55.9, 54.2	41.0, 41.0	64.5, 58.4
	$t = 2$	49.7, 49.7		43.2, 43.2		61.9, 61.9	

the two simultaneous-move subgames become similar (and higher than in the sequential-move subgame) turns the timing game into a coordination game with two strict symmetric equilibria. This provides one reason why the convergence to $t = 1$ choices is not complete.

Nevertheless, overall we note that subjects choosing period 1 earn on average higher payoffs over time than subjects choosing period 2.[10] The profit figures are 51.6 and 47.3 (third 1), 48.9 and 41.6 (third 2), and 54.4 and 49.9 (third 3) for $t = 1$ and $t = 2$ choices, respectively.

Note also that, over time, Stackelberg leaders become less competitive and Stackelberg followers appear to move towards matching the Stackelberg leader's quantity such that payoff differences become less extreme. This means that the incentive to avoid the sequential-move game gets weaker which is another reason why we see a slowdown in the convergence to $t = 1$ choices during the last two thirds.

It is instructive to compare these results to those reported in Huck et al. (2002) (henceforth HMN). Their experimental design is identical to ours but the one major difference is the timing game. HMN used Hamilton and Slutsky's (1990) extended game with action commitment. In this game, a firm can move first only by committing to an output. When doing so, the firm does not know what its competitor is doing. By waiting until the second period, a firm can observe the other firm's first period action. Theory predicts the emergence of Stackelberg equilibria. More precisely, there exist two Stackelberg equilibria and one first period Cournot equilibrium, but only the two Stackelberg equilibria are in undominated strategies.

The surprising insight from the comparison of our data to those of HMN is that results differ only marginally – even though predictions based on subgame perfectness oppose each other. In HMN, the relative frequency of $t = 1$ decisions is 56, 65 and 62% across thirds. These numbers are very close to ours in the first two thirds and only somewhat smaller towards the end of the experiment. Note that in our experiment firms have a strict incentive to choose $t = 1$ (they can only lose by choosing $t = 2$) while, in the extended game with action commitment, firms have a weak incentive to delay (as they can play a best reply to whatever the rival firm did in $t = 1$). Nevertheless, aggregate $t = 1$ choices are rather similar in both studies.

[10] This is, however, not significantly different at any conventional level of significance (two-tailed Wilcoxon signed ranks test).

The similarity of market outcomes in both experiments is also illustrated by a look at the frequency of Cournot outcomes (that is, both firms choosing quantity 8, regardless of the timing decisions). In RANDOM we find 16.0% and in HMN 14.4% Cournot outcomes. This is in contrast to the prediction that we should observe Cournot outcomes only in RANDOM but not in HMN.

Another telling statistic is the ratio of market shares. We calculate the number $s := \max\{q_1, q_2\}/\min\{q_1, q_2\}$ for each individual market and for each round. The average s for the markets in HMN is 1.27 (standard deviation 0.36) and 1.33 (standard deviation 0.48) in RANDOM. Thus, the ratio of market shares in the current study (in which symmetric Cournot outcomes are predicted) is higher than in the previous experiment where asymmetric Stackelberg outcomes are predicted.

4.2 Fixed matching

Let us now consider treatment FIXED. Table 1 above also shows the evolution of the relative frequency of t=1 choices in FIXED. In contrast to RANDOM, period one choices stay roughly constant at a level of 50%. The frequency of timing outcomes is not immediate from Table 1 as they depend on individual duopoly pairs. We find that the frequency of the predicted $t_1 = t_2 = 1$ subgame increases from 17 to 32% (from third 1 to third 3). Surprisingly, the frequency of the t_1=t_2=2 subgame increases, too, from 17 to 26%. As in treatment RANDOM, the frequency of the sequential subgame decreases from 66 to 42% but it is modal in all thirds.

Table 2 reports average quantities. With the exception of the $t = 1$ Cournot subgame, outputs are generally smaller compared to RANDOM, indicating a tendency to collude. We note that output produced in the first period simultaneous subgame is always slightly higher than the Cournot quantity of 8. Whilst the Cournot output in $t = 1$ appears to be larger in FIXED,[11] we observe that average outputs in the sequential subgame is smaller in the FIXED treatment. In fact, both Stackelberg leaders and followers in treatment FIXED are less competitive than those in treatment RANDOM[12] (although, on average, Stackelberg leaders and followers in FIXED do not collude perfectly at the joint-profit maximum). This implies that in treatment FIXED there is less of an incentive to avoid the sequential subgame by choosing $t = 1$.[13]

[11] This difference is not significant (one-tailed Mann–Whitney U test).

[12] This is significant at the 1% level regarding the Stackelberg followers, but not regarding the Stackelberg leaders (one-tailed Mann–Whitney U test).

[13] A look at Table 3 suggests that actual behavior in the subgames turns the timing game into one with two asymmetric equilibria. However, we have fixed matching here and indeed we find strong cohort effects. It appears that timing choices are often used to coordinate on a collusive outcome. Therefore, the payoff differences between subgames should not be interpreted as an indication that some subgame should be played more often. Depending on the group, different timing choices may lead to profits rather different from those in Table 3.

As expected from the lower quantities, profits are usually higher in Fixed. More precisely, average profits after choosing period 1 and period 2, respectively, are 50.8 and 49.4 (third 1), 50.1 and 49.3 (third 2), and 50.3 and 60.3 (third 3), respectively. Hence, timing decisions do not seem to affect profits very much in the first two thirds but towards the end of the experiment subjects seem to coordinate more effectively in the $t_1 = t_2 = 2$ subgame. As mentioned above, one reason why output choices become more collusive when both subjects in treatment Fixed choose to produce in period $t = 2$, is that this choice might signal the intention not to try to exploit the other subject as a Stackelberg leader. This might then gain the trust of the other subject and allows the two subjects to collude. The fact that the frequency of both simultaneous-move subgames rises over time can by and large be explained by observing that some pairs tend to coordinate on $t = 1$ whereas others tend to coordinate on $t = 2$. Recall that production costs are the same in both periods.

5 Discussion

Hamilton and Slutsky's (1990) extended game with observable delay has a unique subgame perfect equilibrium in which both players choose to produce in the first period, implying symmetric Cournot quantities. In this paper we report on an experimental test of this prediction. We run the game both with a random and a fixed matching scheme. With random-matching, we find that subjects choose the predicted production period more frequently over time but choices do not converge to the predicted level as nearly one third of all subjects still chooses to delay toward the end of the experiment. With a fixed-matching scheme we find that the subgame perfect equilibrium has no predictive power with regard to timing choices as throughout the experiment only half of the timing observations are period one choices. The differences in timing choices in the two treatments can to some extent be explained by the deviations from the prediction observed in the sequential-move subgame. In the treatment with random matching, more competitive behavior in the Stackelberg subgame provides an incentive to avoid it by choosing to produce early. This is not the case in the treatment with fixed matching as here the behavior in the sequential-move subgame is less competitive.

The finding that timing choices in the main treatment with random matching do not converge to the predicted level might be explained by several observations. First, we noted that given subjects' behavior in the subgames, after some experience the timing game more resembles a coordination game with two symmetric equilibria. Thus, as both players choosing either period one or period two become equilibrium choices, it is apprehensible that convergence to equilibrium slows down and remains incomplete. Furthermore, over time subjects' behavior in the asymmetric subgame becomes less competitive which reduces the incentive to avoid it and, thus, slows down the convergence towards the prediction choices further.

A second observation is that there might be preferences that cause subjects to delay their decisions. This is supported by answers given in the post-experimental questionnaire. Some subjects state there that waiting until period two would allow them to react to the other player's first period output (if applicable).[14] This propensity to postpone decisions seems to also exist in the realm of individual decision making. Tykocinski and Ruffle (2003) documented such preferences in their study about "reasonable reasons for waiting". They show that subjects often prefer to delay their decisions even when waiting does not provide any additional information. Our results indicate that subjects sometimes prefer to wait even when doing so puts them at a strategic disadvantage. When choosing period two, our subjects can find out which action the rival firm has chosen, provided this rival chose the first period. Even though they become the Stackelberg follower in this case, they prefer to wait, perhaps to resolve the strategic uncertainty about the other player's action. Once subjects are more familiar with the experimental environment, this preference to wait is getting weaker in the random-matching treatment. Nevertheless many subjects still delay towards the end of the experiment.

With fixed matching, these considerations may be less relevant since subjects face less ambiguity regarding choices of their opponent. As argued above, timing choices may not reflect the incentives suggested by non-cooperative game theory. Instead, timing choices may turn out to be an instrument to support collusion. While we observe only little collusion in our experiments, our results suggest that timing decisions do not affect profits by very much with fixed matching (except towards the end of the experiment).

We found that our results with random matching are similar in many respects to those in Huck et al. (2002) although Stackelberg equilibria are predicted for those experiments. Generally, previous work[15] found that endogenous timing models predicting asymmetric outcomes are of limited behavioral relevance due to coordination failure and inequality aversion. The results in this study show that there are forces sufficiently strong to prevent play from converging to a unique equilibrium of an endogenous timing model even if the equilibrium is symmetric.

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[14] Examples include a subject in the third session with random matching who states: "I started playing in period 2, thinking it would be better because I could chose the best payoff according to the other decision." Another example is a subject in the first fixed-matching session who states "I chose period 2 because I could potentially choose my quantities based on the other firm's decision if they chose period 1."

[15] See Huck et al. (2002), Müller (2005), Fonseca et al. (2005).

Appendix A: Instructions

Welcome to our experiment! Please read these instructions carefully! Do not talk to your neighbors and keep quiet during the entire experiment. If you have any questions, please give us a sign. We will answer your question privately.

In our experiment you can earn different amounts of money, depending on your behavior and that of other participants matched with you. All participants read identical instructions.

You have the role of a firm which produces the same product as a second firm in the market. First you have to decide, at which time you want to produce. Afterwards, you decide on the quantity you want to produce.

Regarding the time when to produce, you can choose either the first or the second production period. As the other firm has the same choice, there are four possibilities. Both first, both second, you first and the other firm second, and you second and the other firm first. In all cases, you will be informed about the timing decision of the other firm before choosing your quantity.

The quantity decisions are made in the sequence resulting from the timing decisions. If both firms choose first or both choose second, quantity decisions are made simultaneously. In those cases, you and the other firm have to make the quantity decisions not knowing what the other one chooses. If you choose first and the other firm second, then the other firm will learn your quantity decision before making its own decision. Likewise, if you choose second and the other firm first, then you will learn the other firm's output decision before making your own decision.

Note that the profit in each round depends only on the chosen quantities, not on the choice of production periods. In the attached payoff table, you can see the resulting profits of both firms for all possible choices of quantity. The table reads as follows: At the head of a row the quantity of your firm is indicated, at the head of a column the quantity of the other firm is stated. In the cell at which row and column intersect, your profit is noted in the lower left and the other firm's profit is stated in the upper right. All profits are expressed in a fictional currency, which we call "Points".

The experiment lasts 30 rounds. After each round, you will be informed about the quantity choice of the other firm, your profit and the other firm's profit.

You do not know with which participant you serve the market. You will be randomly matched with a participant each round. This random move is done by the computer.

Anonymity is kept among participants and instructors, as your decisions will only be identified with a code number. You will discreetly receive your payment at the end of the experiment.

Concerning the payment note the following. At the end of the experiment, your earnings in Points determine your payment in pounds sterling. For every 300 Points you will receive 1£. In addition to this payment, you will receive the show-up fee of 4£ independently of your earnings during the 30 rounds.

Appendix B: Payoff table

Quant.	3	4	5	6	7	8	9	10	11	12	13	14	15
3	54 / 54	51 / 68	48 / 80	45 / 90	42 / 98	39 / 104	36 / 108	33 / 109	30 / 110	27 / 108	24 / 104	21 / 98	18 / 90
4	68 / 51	64 / 64	60 / 75	56 / 84	52 / 91	48 / 96	44 / 99	40 / 100	36 / 99	32 / 96	28 / 91	24 / 84	19 / 75
5	80 / 48	75 / 60	70 / 70	65 / 78	60 / 84	55 / 88	50 / 89	45 / 90	40 / 88	35 / 84	29 / 78	25 / 70	20 / 60
6	90 / 45	84 / 56	78 / 65	72 / 72	66 / 77	60 / 80	54 / 81	48 / 80	41 / 77	36 / 72	30 / 65	24 / 56	18 / 45
7	98 / 42	91 / 52	84 / 60	77 / 66	70 / 70	63 / 72	55 / 71	49 / 70	42 / 66	35 / 60	28 / 52	21 / 42	14 / 30
8	104 / 39	96 / 48	88 / 55	80 / 60	72 / 63	64 / 64	56 / 63	48 / 60	40 / 55	32 / 48	24 / 39	16 / 28	8 / 15
9	108 / 36	99 / 44	89 / 50	81 / 54	71 / 55	63 / 56	54 / 54	45 / 50	36 / 44	27 / 36	18 / 26	9 / 14	0 / 0
10	109 / 33	100 / 40	90 / 45	80 / 48	70 / 49	60 / 48	50 / 45	40 / 40	30 / 33	20 / 24	10 / 13	0 / 0	−10 / −15
11	110 / 30	99 / 36	88 / 40	77 / 41	66 / 42	55 / 40	44 / 36	33 / 30	22 / 22	11 / 12	0 / 0	−11 / −14	−22 / −30
12	108 / 27	96 / 32	84 / 35	72 / 36	60 / 35	48 / 32	36 / 27	24 / 20	12 / 11	0 / 0	−12 / −13	−24 / −28	−36 / −45
13	104 / 24	91 / 28	78 / 29	65 / 30	52 / 28	39 / 24	26 / 18	13 / 10	0 / 0	−13 / −12	−26 / −26	−39 / −42	−52 / −60
14	98 / 21	84 / 24	70 / 25	56 / 24	42 / 21	28 / 16	14 / 9	0 / 0	−14 / −11	−28 / −24	−42 / −39	−56 / −56	−70 / −75
15	90 / 18	75 / 19	60 / 20	45 / 18	30 / 14	15 / 8	0 / 0	−15 / −10	−30 / −22	−45 / −36	−60 / −52	−75 / −70	−90 / −90

The head of the row represents one firm's quantity and the head of the column represents the quantity of the other firm. Inside the box at which row and column intersect, one firm's profit matching this combination of quantities stands up to the left and the other firm's profit stands down to the right. Fourteen entries were manipulated in order to get unique best replies (see Huck et al. 2001)

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