Cooperative and noncooperative R&D in experimental duopoly markets

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Abstract

We analyze the relation between technological spillovers and R&D cooperation in a duopoly experiment based on the well-known model of d'Aspremont and Jacquemin. For scenarios without and with full spillovers, two noncooperative treatments are run, one without and one with non-binding communication possibilities, and one cooperative treatment, with binding contract possibilities. We find that without technological spillovers, binding R&D contracts are needed for R&D decisions to deviate from the subgame perfect Nash R&D level towards the cooperative level. With full spillovers, the possibility of non-binding cheap-talk may suffice to move closer to R&D cooperation.

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1 Introduction

In the last decade an abundance of theoretical papers modelling competition and cooperation in R&D activities with technological spillovers has arisen. Most of these are extensions or modifications of the paper of d'Aspremont and Jacquemin (1988)[2] (henceforth AJ), where duopolists first decide on R&D expenditures and then compete in the product market. In these models, R&D conducted by a firm reduces its unit production cost and may have spillovers, reducing the unit cost of the other. A general finding is that R&D investment and welfare are higher under R&D cooperation than under R&D competition if the spillover is above a certain threshold, and lower otherwise. The results are often interpreted as a rationale for governmental support of research joint ventures in industries with large knowledge spillovers.

A related and important question is whether spillovers increase firms' incentives to cooperate in R&D. A number of empirical studies have addressed this issue, providing mixed results. Cassiman and Veugelers (2002), for example, find that the probability of firms cooperating in R&D is lower when *outgoing* spillovers are high (low appropriability). These results are in conflict with most of the AJ like theoretical models, which predict that—when spillovers are above a critical level—cooperative R&D incentives of firms increase with the level of (incoming ánd outgoing) spillovers. On the other hand, Cassiman and Veugelers (2002) also find that the incentives to cooperate in R&D are higher when *incoming* spillovers are high, which can be viewed as evidence in support of the theoretical prediction. Kaiser (2002) finds that (horizontal) spillovers increase the probability to cooperate in R&D, while Belderbos et al.

[2] Examples are Kamien et al. (1992); Poyago-Theotoky (1995); Leahy and Neary (1997); Petit and Tolwinski (1999); Hinloopen (2000); Amir et al. (2003).

(2004) find no significant influence [3]. Hernán et al. (2003) provide evidence for a positive relationship between outgoing spillovers and incentives to cooperate.

Given the difficulty in measuring spillovers and the differences in data sets underlying econometric estimations, in estimation methods and in ways of defining or computing proxies that should represent technological spillovers [4], it is not surprising that these empirical studies have yielded different results and it is unlikely that a consensus can emerge in the near future.

In this paper we use experimental methods to investigate whether incentives to cooperate in R&D are different for different levels of spillovers. An important advantage of the experimental approach is that the characteristics of spillovers and other assumptions made in the models can be controlled. For two spillover scenarios (no and full spillovers), we ran two non-cooperative treatments (a baseline and a cheap-talk treatment) and one cooperative treatment. In the non-cooperative treatments subjects played a noncooperative R&D game and did not have any contract possibilities. The cheap-talk treatment contained a possibility to send (non-binding) messages containing information on intended R&D investment. We included a cheap-talk treatment since previous oligopoly experiments have shown that allowing for an appropriate form of non-binding communication may increase cooperation rates (see e.g. Holt and Davis, 1990; Cason, 1995; Harstad et al., 1998). In the cooperative treatment, binding contract possibilities were allowed, as in a cooperative R&D game.

We find that in the baseline treatments R&D levels are close to the equilibrium prediction, either with or without technological spillovers. In the coop-

[3] They do find a significant influence of vertical spillovers on vertical cooperation.
[4] Spillovers are difficult to measure empirically because they can arise through different channels, such as through the movement of R&D personnel, networks, meetings, patent applications and reverse engineering (see Veugelers, 1998).

erative game, where binding contracts are allowed, R&D levels are close to the cooperative level when contracts are made, and to the equilibrium prediction otherwise. Subjects learned to use binding contracts and contract at the joint-profit maximizing R&D level during the experiment. In the cheap-talk treatment we find that without spillovers, experimental R&D decisions are close to the subgame perfect Nash prediction and with spillovers to the cooperative R&D level.

There have been few experiments on R&D behavior. A small literature has studied patent races (e.g. Hey and Reynolds, 1991; Sbriglia and Hey, 1994; Zizzo, 2002) and stochastic invention models (Isaac and Reynolds, 1986, 1988, 1992), but none of the existing studies examine the effects of allowing binding contracts or non-binding communication on R&D decisions, nor do they focus on the effect of spillovers on incentives to cooperate in R&D.

The rest of the paper is organized as follows. In section 2, the model on which the experiment is based, is resumed. In section 3, the hypotheses to be tested and the experimental design are gathered from the model. Analyses of the experimental results are presented in section 4 and section 5 concludes the paper.

2 The model

AJ analyze a two-stage complete information game where duopolists simultaneously decide on R&D in the first stage and engage in Cournot competition in the second stage. Consider a symmetric duopoly. The industry is characterized by a linear inverse demand function:

$$P = a - b(Q_i + Q_j) \tag{1}$$

where $a, b > 0$ and Q_i is production quantity of firm i. The unit cost function of firm i is given by $c_i(x_i, x_j) = \alpha - \gamma(x_i + \beta x_j)$ where x_i denotes firm i's R&D and β is a spillover parameter between 0 and 1. The cost of R&D investments are modelled by a quadratic R&D cost function so that profits are given by

$$\pi_i = [P - c(x_i, x_j)]Q_i - \delta \frac{x_i^2}{2}. \tag{2}$$

The game is solved proceeding backwards. In the second stage firms simultaneously choose quantities to maximize profit, taking first-stage R&D investments as given. Maximizing equation (2), for $i = 1, 2$, solving for the Nash-Cournot equilibrium and substituting back into the objective function yields the following first-stage profit function:

$$\pi_i^{\mathrm{e}} = \frac{(a - \alpha + 2\gamma(x_i + \beta x_j) - \gamma(x_j + \beta x_i))^2}{9b} - \delta \frac{x_i^2}{2} \qquad i = 1, 2;\ j \neq i. \tag{3}$$

Suppose further that in the first stage firms play a noncooperative R&D game. If both firms choose to maximize their own profit and expect the same behavior from their competitor, this leads to the following (symmetric) subgame perfect equilibrium R&D level [5] :

$$x^* = \frac{2\gamma(a - \alpha)(2 - \beta)}{9b\delta - 2\gamma^2(1 + \beta)(2 - \beta)} \qquad i = 1, 2. \tag{4}$$

If firms coordinate their R&D activities to maximize the sum of their profits [6] , we obtain the cooperative R&D level [7] :

$$x^{**} = \frac{2\gamma(a - \alpha)(1 + \beta)}{9b\delta - 2\gamma^2(1 + \beta)^2} \qquad i = 1, 2. \tag{5}$$

[5] The second-order condition is $9b\delta > 2\gamma^2(2 - \beta)^2$.
[6] In Kamien et al. (1992) this form of R&D cooperation is called cartelization.
[7] The second-order condition is $9b\delta > 2\gamma^2(1 + \beta)^2$. Salant and Shaffer (1998) argue that only when $b\delta > 2\gamma^2(1 - \beta)^2$ joint-profit maximizing R&D investments are symmetric.

Profits calculated in the symmetric competitive and cooperative outcome are respectively

$$\pi^* = \frac{\delta(a-\alpha)^2(9b\delta - 2\gamma^2(2-\beta)^2)}{(9b\delta - 2\gamma^2(1+\beta)(2-\beta))^2}$$
$$\text{and} \qquad \pi^{**} = \frac{\delta(a-\alpha)^2}{9b\delta - 2\gamma^2(1+\beta)^2}.$$

π^{**} is larger than π^* when $\beta \neq 0.5$, in which case $\pi^* = \pi^{**}$ (see also Kamien et al., 1992; Hinloopen, 2000). It is clear that strategic interactions in the noncooperative one-shot R&D game share some properties of a prisoners' dilemma. Although firms have incentives to cooperate in R&D, for all $\beta \neq 0.5$ individual profit maximization is the best response to any of the strategies of the competitor and thus represents the subgame perfect Nash equilibrium. In the cooperative game, joint profit maximization is the prediction for all $\beta \neq 0.5$, given that firms can credibly commit to the (symmetric) cooperative R&D level.

3 Experimental design and hypotheses

The experiment concentrates on the R&D stage that is nested in the more general two-stage game. The quantity decision was controlled by setting production quantity at its Nash-Cournot equilibrium, as a function of firms' R&D expenditures. This enabled us to focus on the R&D decisions and to avoid testing optimization in both stages (R&D and production) and backward induction.

For two levels of spillovers, we ran two treatments without binding contract possibilities—a baseline treatment and a treatment with non-binding communication possibilities—and one contract treatment. The most widely separated levels of spillovers were used, i.e. full ($\beta = 1$) versus no ($\beta = 0$) R&D spillovers, to sharpen possible contrasts in the results (Friedman and Sunder, 1994). As such, six treatments were run; a baseline, cheap-talk and contract treatment for spillovers levels of 0 and 1. Ten subjects were appointed to each treatment.

The parameters of the demand function, the R&D cost function and the unit cost function were treated as constants. The chosen parameters are $a = 250$, $b = 5$, $\alpha = 100$, $\gamma = 2$ and $\delta = 5$ [8] and correspond to the following R&D equilibria, with x^* and x^{**} representing the subgame perfect Nash prediction and the (symmetric) cooperative outcome respectively:

$$x^* = \begin{cases} 5.7 & : \quad \beta = 0 \\ 2.9 & : \quad \beta = 1 \end{cases}$$

and

$$x^{**} = \begin{cases} 2.8 & : \quad \beta = 0 \\ 6.2 & : \quad \beta = 1. \end{cases}$$

We have chosen to exclude the possibility that asymmetric R&D investments maximize joint profit. Given that for $\beta = 1$ the condition for joint-maximizing investment to be symmetric is always satisfied (see Salant and Shaffer, 1998), also for $\beta = 0$ we restricted the cooperative R&D level to be symmetric. In this way, the experimental environment is comparable between treatments with and without spillovers.

[8] The parameter values satisfy requirements of stability as proposed by Henriques (1990).

The experiment consisted of three computerized experimental sessions [9] with 60 participants in total, recruited from undergraduate economics courses and randomly divided into fixed groups of two (duopolies). None of the subjects had participated before in an experiment. They were not informed about the identity of their competitor, were not allowed to talk to each other and participated each in one treatment.

Subjects were told that they were sellers in a market with two sellers of a non-specified product [10]. They were told that they were all in the same situation and subject to the same conditions as their counterpart. Subjects were asked to make investment decisions in the interval of $[0, 25]$ [11]. As to allow for learning, the static R&D stage was repeated 27 times (including one practice round) and subjects knew this. The investment decision decreased the subjects' unit production cost according to the linear unit cost function and induced an R&D cost, calculated on the basis of the quadratic R&D cost function. The decision influenced profit also through equilibrium production quantities. In the complete spillovers case, subjects were told that their R&D decision also decreased unit production cost of the other producer in his/her market, without the latter bearing any cost. The computer program enabled subjects to simulate their production quantity, selling price, unit production cost, total R&D cost and profit by filling in two investment decisions (their own and the other producer's).

Each decision period took around two minutes, except the first one which

[9] The baseline and cheap-talk treatments covered two sessions and the contract treatments covered one session. The software toolkit *z-Tree* developed by Fischbacher (1999) was used for the contract treatments and a program in *PHP* and *MySQL* for the baseline and cheap-talk treatments.
[10] For the complete instructions we refer to `www.ua.ac.be/sigrid.suetens`.
[11] This restriction of the strategy space was necessary to obtain no-nonsense results for variables such as unit production cost.

took longer to let subjects become acquainted with the instructions and the computer program. For participating and following the instructions carefully, subjects received 2.5 EUR. Additional earnings depended on the sum of the profits in the 26 periods (decisions in the first period did not count). Average earnings were around 10 EUR and each session took less than 80 minutes.

In the contract treatments subjects could propose binding contracts to their competitor and accept contracts proposed by their competitor. As in the AJ model of the cooperative R&D game, only symmetric contracts were allowed. There was no restriction on the number of proposals that could be made. Once subjects had proposed a contract they were committed to their proposal when the proposal was accepted. There was no option to deviate from an accepted contract.

In the cheap-talk treatments, subjects were asked to send one message in each period to their competitors with an indication of the interval—that could be anything between one point and the whole interval of $[0, 25]$—in which their decision would be in the considered period. They were explicitly told that this interval was not binding. This treatment resembles the continuous signaling treatment in Cason (1995), since subjects always had an option not to communicate.

Based on the theoretical predictions in the noncooperative and cooperative R&D game and given that the finitely repeated R&D stage game has a unique subgame perfect Nash equilibrium that corresponds to the prediction in the one-shot R&D game, theory-based conjectures would be that subjects will always choose to maximize individual profit in the baseline and cheap-talk treatments and joint profit in the contract treatments, irrespective of the level of spillovers. The AJ model also predicts that without binding contracts firms underinvest in R&D relative to the joint profit maximization level when $\beta = 1$ and overinvest when $\beta = 0$. On the other hand, the subgame perfect Nash R&D level is higher for 'low' spillovers ($\beta = 0$) than for 'high' spillovers ($\beta = 1$). Given these theoretical predictions we set up the following hypotheses. Note that these hypotheses implicitly contain the expectation that R&D in the cheap-talk treatments does not differ from R&D in the baseline treatment.

Hypothesis 1a R&D in the contract treatment is lower than in the baseline treatment when $\beta = 0$.

Hypothesis 1b R&D in the contract treatment is lower than in the cheap-talk treatment when $\beta = 0$.

Hypothesis 2a R&D in the contract treatment is higher than in the baseline treatment when $\beta = 1$.

Hypothesis 2b R&D in the contract treatment is higher than in the cheap-talk treatment when $\beta = 1$.

Hypothesis 3a R&D in the baseline treatments is higher with $\beta = 0$ than with $\beta = 1$.

Hypothesis 3b R&D in the cheap-talk treatments is higher with $\beta = 0$ than with $\beta = 1$.

Hypothesis 4 R&D in the contract treatments is lower with $\beta = 0$ than with $\beta = 1$.

4 Experimental results

Duopolists are influenced by their competitor's decisions in making their R&D decisions, such that observations on R&D decisions are not independent within the duopolies. Using the aggregate R&D decisions, referred to by $X = x_1 + x_2$, circumvents this problem and creates independent observations per duopoly.

Given that the theoretical R&D benchmarks are symmetric, duopoly benchmarks are simply twice the individual benchmarks (see e.g. table 1 for the theoretical duopoly benchmarks). In what follows we first have a look at descriptive statistics. We further provide results of non-parametric comparisons between and within treatments. In the last two subsections we provide more detailed analyses of contracting and communication behavior in the contract and cheap-talk treatments respectively.

4.1 Descriptive statistics

Table 1 presents averages and standard deviations of average duopoly R&D decisions based on all periods, the first ten and the last ten periods. The presented standard deviations only measure cross-sectional heterogeneity, not variability in time. In the baseline treatment without spillovers, average R&D decisions are clearly very close to the subgame perfect Nash R&D levels. In the baseline treatment with full spillovers, average R&D in the first periods lies between both theoretical benchmarks, while in the ten last periods it is close to the subgame perfect Nash equilibrium.

[**Table 1 about here**]

In the contract treatment without spillovers, average R&D is below R&D in the baseline treatment, and lies between the subgame perfect Nash and cooperative benchmarks. In the contract treatment with full spillovers, average R&D in the last ten periods is higher than in the baseline treatment and also lies between both theoretical benchmarks.

In the cheap-talk treatment without spillovers, average R&D is generally close to the subgame perfect Nash prediction. With spillovers, average R&D is close to the cooperative prediction, especially in the last ten periods.

The table further provides evidence for cross-sectional variance in R&D decisions being lower in the final ten periods than in the first ten periods for all treatments, on the one hand, and generally lowest in the contract treatments, on the other hand. A decrease in cross-sectional variability during the experiment could indicate that decisions converge towards each other. A plausible explanation for variation being lower in the contract treatments is that the joint profit maximizing decisions are better sustainable and easier to make (simply by proposing and committing to a contract). Joint profit maximization in the baseline and cheap-talk treatments is not an equilibrium strategy and thus much harder to establish.

[**Table 2 about here**]

We further look at duopoly-specific data. Tables 2, 3 and 4 contain averages of R&D decisions based on all, the first ten and the last ten periods for each of the five duopolies in the baseline, cheap-talk and contract treatments respectively. In table 3 we distinguish between averages in periods where intervals have actually been communicated by both duopoly's subjects ('messages') and periods without communication ('no messages'), where either no messages or intervals equal to $[0, 25]$ were communicated [12]. Standard deviations now refer to variation across periods. In table 4 separate statistics are provided for periods where R&D contracts have actually been made ('chosen') and periods without contracts ('not chosen').

[**Table 3 about here**]

[12] Alternatively, we could have opted to divide data on the basis of periods where at least one duopoly's subject has sent an interval. Data would look very similar then, but more data would be lacking in the 'no messages' rows.

[Table 4 about here]

Table 2 shows that in the baseline treatment without spillovers, average R&D decisions based on all or the last ten periods of all duopolies except duopoly 2 are very close to the subgame perfect Nash equilibrium prediction. The average R&D decision of duopoly 2 based on the last ten periods is at the cooperative level. In the baseline treatment with spillovers average R&D decisions are also generally closer to the subgame perfect Nash prediction than to the cooperative R&D level but in a less obvious way than without spillovers. In the baseline treatments R&D decisions with spillovers seem to diverge more between duopolies than without spillovers, when ignoring the cooperating duopoly 2.

Table 2 further indicates that decisions in the first ten periods diverge more from the subgame perfect Nash prediction than at the end of the experiment, and are subject to more cross-sectional and temporal variation. This suggests that subjects have learned to play the equilibrium strategy during the experiment.

With respect to the cheap-talk treatments, table 3 shows that without spillovers, R&D decisions are generally close to the subgame perfect Nash prediction, irrespective of whether messages have actually been sent or not. With spillovers, experimental R&D is generally closer to the cooperative level, both in periods with and without messages, except for duopoly 5. The tendency to learn is less clear than in the baseline treatment, but variability in R&D decisions often declines in the final periods.

Finally, table 4 indicates that in the contract treatments averages of R&D decisions based on all periods or the last ten periods are close to cooperative levels when contracts are chosen, and close to subgame perfect Nash levels when contracts are not chosen. This goes for both spillover scenarios. Without spillovers, all duopolies seem to invest even more than equilibrium R&D in periods without contracts at the end of the experiment, while with spillovers they invest less (except duopoly 5). In the first ten periods decisions diverge more from the theoretical predictions, and their standard deviations have also decreased in the last ten periods, suggesting that subjects have learned to contract cooperative R&D levels.

4.2 Non-parametric analyses

In this subsection results of non-parametric tests of differences in experimental R&D decisions between and within treatments are presented. Statistics of contract and cheap-talk treatment effects are in table 5. A first important issue that needs to be addressed is the effect of allowing contracts on R&D decisions. Therefore we compare average R&D decisions of the duopolies in the contract treatments with the decisions in the baseline treatments. Results of Mann-Whitney tests are under the header 'Between contract and baseline' in table 5. The null hypothesis of the non-parametric tests is that R&D decisions in the baseline treatments do not differ from R&D decisions with contract possibilities.

On the basis of one-tailed tests we conclude that with spillovers, R&D decisions are overall higher in the contract treatment than in the baseline treatment with 4.8% significance. Without spillovers, the conclusion is the opposite with a significance level of 7.5% and R&D decisions are lower in the contract treatment than in the baseline treatment. The same goes for the last ten periods. In the first ten periods, we do not find any significant difference in R&D decisions between baseline and contract treatments. As such, experimental behavior corresponds to the theoretical predictions as summarized in hypotheses

1a and 2a, provided that subjects have had a chance to learn.

Comparisons between R&D in the contract and the cheap-talk treatments are under the header 'Between contract and cheap-talk'. Without spillovers, we should consider one-tailed test results since the expectation is that R&D is lower in the contract treatment than in the cheap-talk treatment. The test results support the theoretical expectation with 4% significance. Thus, hypothesis 1b is not rejected. Given that with spillovers we expect R&D to be higher in the contract treatment than in the cheap-talk treatment, and in the experiment average R&D in the cheap-talk treatment is higher than in the contract treatment, we should rely on two-tailed test results. According to two-tailed tests, there exists no significant difference between R&D in the cheap-talk and contract treatments with spillovers. Thus, with spillovers, incentives to cooperate in R&D do not differ between the cheap-talk and the contract treatment. Therefore, we reject hypothesis 2b.

Similar tests have been performed to compare R&D between the cheap-talk and baseline treatments. Results are under the header 'Between cheap-talk and baseline'. Here we should rely on two-tailed test results given that we do not expect any difference in R&D decisions between treatments. Without spillovers, no significant difference is found between R&D in the baseline and the cheap-talk treatment. With spillovers, evidence exists that in the final ten periods R&D in the cheap-talk treatment is different from (higher than) R&D in the baseline treatment, with 10% significance. The observation that cheap-talk increases R&D cooperation in the final periods only when $\beta = 1$ and not when $\beta = 0$ may indicate that incentives to cooperate in R&D are higher with spillovers than without spillovers [13].

[13] Similar results have been found in the public goods/bads literature, in this context often referred to as a framing effect (see Andreoni, 1995; Willinger and Ziegelmeyer,

In another series of tests, results of which are reported in the same table under the header 'Within contract', R&D decisions in the contract treatments averaged over periods where no contracts were made, are compared with average contracted R&D decisions. The results are based on Wilcoxon signed ranks tests. With full spillovers R&D levels are higher if R&D contracts are committed to with 3.1% significance, irrespective of whether all, the first ten or the last ten periods are used. In the no-spillover treatment R&D in periods with contracts is lower than R&D in periods without contracts, with an overall significance of 3.1%. The significance reduces to 6.3% in the first ten or the last ten periods.

[**Table 5 about here**]

Similarly, we have compared R&D in periods where both subjects in a duopoly sent a message with R&D in periods where maximum one subject sent a message using Wilcoxon signed ranks tests. We do not find any within-significance for neither of the spillover levels.

To summarize, evidence exists for contracted R&D being higher (lower) with (without) spillovers than non-contracted R&D in the contract treatment, and than R&D in the baseline treatment, when subjects have had the chance to learn. Furthermore, allowing non-binding communication did not have any effect on R&D investment without spillovers, but has elicited R&D with spillovers above the level obtained in the baseline treatment towards the R&D level in the contract treatment. Thus, without spillovers, more bindingness than the sending of intervals that contain intended R&D investment is needed to move towards R&D cooperation, away from 'baseline' behavior. With spillovers, the possibility of sending non-binding messages may move

1999; Park, 2000).

decisions close to the joint-profit-maximizing R&D level.

In a second set of non-parametric tests we analyze the spillover treatment effect as to test whether R&D decisions are different for $\beta = 0$ and $\beta = 1$. For this purpose we only performed 'between' tests (i.e. Mann-Whitney tests) of differences in R&D decisions between $\beta = 0$ and $\beta = 1$ in the baseline, cheap-talk and contract treatments. Results of these tests are in table 6. The null hypothesis of these tests is that R&D decisions do not differ significantly between $\beta = 0$ and $\beta = 1$.

[**Table 6 about here**]

With respect to the baseline treatments the test results indicate that R&D decisions are higher when $\beta = 0$ than when $\beta = 1$ with an overall one-tailed significance of 5% [14]. The same goes for the final ten periods. For the first ten periods, no significant difference is found. Thus, without spillovers R&D is higher than with spillovers if no contract possibilities are available and subjects have had the chance to learn. Hypothesis 3a is not rejected.

In the cheap-talk treatments we do not find a statistical difference between $\beta = 0$ and $\beta = 1$ for any of the subsets of periods. Given that the equilibrium R&D prediction for $\beta = 0$ (5.7) is close to the cooperative R&D level for $\beta = 1$ (6.2), this again suggests that incentives to cooperate in R&D are higher with spillovers than without spillovers. As such, hypothesis 3b is rejected.

For the contract treatments, no significant difference is found in the first ten periods. R&D decisions in all and the final ten periods of the contract treatment are significantly higher with full than without spillovers, with one-tailed

[14] Given that theory predicts that in the noncooperative R&D game R&D decisions are higher when $\beta = 0$ than when $\beta = 1$, we use one-tailed test results.

significancies of 7.5% and 0.4% respectively. As such, R&D with spillovers is higher than without spillovers when there are R&D contract possibilities and hypothesis 4 is not rejected, provided that subjects had the opportunity to learn.

4.3 Contracts

The aim of this subsection is to analyze further how contracting behavior evolves during the experiment. In table 7 percentages of periods in which contracts are proposed and chosen, are given for each duopoly. In the first part of the table the percentages refer to the shares of periods in which contract proposals were made by at least one subject in a duopoly. In the second part of the table percentages of shares of contracts that were actually committed to, are given. In the columns of the table a distinction is made between the first ten and the last ten periods, so as to investigate learning behavior of the subjects. The average shares across all duopolies are in the last rows of both parts of the table.

We observe that generally more contracts were proposed and agreed to in the treatment without technological spillovers, but the difference is not significant [15].

We further observe that for both spillover levels more contracts were made in the last periods of the experiment than in the first periods. This difference

[15] Mann-Whitney tests on the statistical significance of the difference in the amount of chosen contracts between both levels of spillovers, yield two-tailed p-values of 0.413, 0.611 and 0.111 when based on all periods, the first ten and the last ten periods respectively. Two-tailed p-values should be used here to test for differences, since theory predicts that in the contract treatment contracts are always chosen, irrespective of the level of spillovers. With respect to contract proposals, p-values are at least 0.444.

is statistically significant with a p-value of 0.002 [16], which provides evidence for learning behavior of subjects. Subjects became acquainted with the contract possibility after some time and thus learned to commit to contracts. This conclusion also coincides with the observation that subjects' R&D decisions finally ended up to be close to the theoretical benchmarks. More specifically, especially in the last periods, contracted R&D decisions are close to the theoretical cooperative R&D level, while R&D decisions in periods where no contracts were made, are close to the subgame perfect Nash R&D level.

With respect to contract proposals, a similar test on the difference between the amount of contract proposals at the beginning and at the end of the experiment gives no significance, i.e. a p-value of 0.125. Thus, it is unlikely that the increase in contracts made, can only be explained by an increase in the amount of contract proposals during the experiment.

[**Table 7 about here**]

It is found that during the experiment more and more contracts were made, but this does not imply that in the end subjects always committed to a contract. It remains to be examined what the reasons are that in some periods subjects did not commit to a contract. A first reason could be that no contract proposals were made in these periods. But, as table 7 shows, the percentages of periods in which contracts have been proposed is high. This implies that a significant amount of contract proposals was not accepted in the whole, the beginning and the end of the experiment. Further, it is unlikely that contract proposals were used as a cooperative signal to mislead the competitor, because once a proposal was made by a subject in a duopoly, the counterpart could always accept the contract so that the proposer was committed to it.

[16] This results from a two-tailed Wilcoxon signed ranks test.

[Table 8 about here]

Another explanation could be that the contract proposals contained R&D decisions which were different from the cooperative level and thus not interesting for subjects to commit to. In table 8 the averages and standard deviations of proposed R&D decisions divided according to whether the contract was accepted or not, are presented for all duopolies. Note that averages of *individual* proposed R&D levels are taken and should thus be compared with the cooperative R&D level of one firm in duopoly. For nine of the ten duopolies in the contract treatments we observe that proposed R&D decisions that were accepted in a contract are closer to the cooperative R&D level (i.e. 2.9 for $\beta = 0$ and 6.2 for $\beta = 1$) than proposed R&D decisions that were not accepted when considering all periods in the experiment. Also, standard deviations of R&D proposals that were not accepted are larger than standard deviations of accepted R&D proposals. Thus, the fact that in some periods proposed R&D was too different from the cooperative R&D level, could have been a reason for subjects not to commit to a contract. In the last ten periods, for most duopolies the difference between average accepted and not accepted R&D proposals vanishes. For the first ten periods, we observe that proposed R&D levels that were either accepted or not, are in general not that close to the cooperative R&D level compared to the general averages [17]. Based on these observations, we conclude that in general subjects learn to propose the cooperative R&D level in a contract, and also learn to commit to it. In the beginning subjects make more mistakes when proposing and choosing 'good'

[17] These observations are confirmed in two-tailed Wilcoxon signed ranks tests. The null hypothesis is that no difference exists in the absolute value of the deviation of R&D decisions from the cooperative level, between periods without and with accepted contract proposals. When taking all periods into account, the p-value is 0.002, for averages of the first ten and last ten periods, the p-values are respectively 0.695 and 0.125.

R&D decisions.

Finally, note that even contract proposals that contained the cooperative R&D level, were sometimes not accepted, especially in the last periods of the experiment. Probably, subjects occasionally tried to deviate from the cooperative R&D level contracted in previous periods in the expectation that their counterparts kept choosing the cooperative R&D level, even when no contract was made. But the fact that decisions end up close to the subgame perfect Nash level in periods where no contracts were made, shows that such strategies were generally unsuccessful.

To summarize, there seems to be no significant difference in the amount of proposed and signed contracts—which can be interpreted as measures for incentives to cooperate in R&D—between the contract treatments with and without technological spillovers. Further, contracts did not arise in all periods for all duopolies. First, contract proposals did not always contain an R&D level close enough to the cooperative level. This is particularly noticeable in early periods, less so in later periods, and suggests that subjects learned which proposals yielded better results with experience. Secondly, even when contract proposals were close to the cooperative R&D level, they were not always committed to. In those cases, subjects deviated from the R&D level in contracts of previous periods, and played a noncooperative strategy.

4.4 Communication

In this subsection we take a closer look at what exactly has been communicated by the subjects to investigate why cheap-talk was successful in moving towards R&D cooperation with full spillovers and unsuccessful without spillovers. In table 9 data are given on the communicated intervals and their contents. The data refer to amounts of observed intervals in the whole experiment [18]. Although participants were asked to send one message in each period, some sent more messages [19]. If a subject did not sent an interval in a certain period, we assumed that the communicated interval was $[0, 25]$. In the analyses that follow we only take up information on the last sent message that the counterpart was able to read when more than one message was sent in a period by a subject.

For small intervals it is less likely that they contained either the subgame perfect Nash or the cooperative R&D level, while one of the two bounds could still be very close to either of the R&D levels. To take into account the communicative extent of this type of messages, we enlarged intervals where the difference between the upper bound and the lower bound was strictly smaller than 2. Moreover, for these intervals, lower (upper) bounds were recalculated by subtracting (adding) 0.5 [20].

If a subject did not sent an interval in a certain period, we assumed that the communicated interval was $[0, 25]$. We interpret the sending of interval $[0, 25]$ as unwillingness to communicate in that period. Therefore, to investigate the communicative contents of the messages, we only consider periods where at

[18] Given the large similarity in conclusions based on separate analyses for the first ten and the last ten periods, we do not provide more details for these subsets of periods. Moreover, we do not find clear evidence for learning behavior based on the analysis of the sent intervals.

[19] One message has been sent in 71% out of 520 periods of play in the cheap-talk treatments (20 subjects times 26 periods), no message in 21% and more than one message in 8% of the cases.

[20] A total of 141 intervals out of 520 were re-scaled. Note that the original size (upper minus lower bound) of only 2 out of these 141 intervals was strictly larger than 1 and smaller than 2. This implies that only those two were re-scaled to a size slightly larger than 2. As such, the 'new' size of 139 intervals was still smaller than or equal to 1.

least one subject in a duopoly has sent a communicative interval not equal to $[0, 25]$. In the table the data in the row 'interval $= [0, 25]$' refer to the number of periods where none of the pair's subjects has sent an interval different from $= [0, 25]$, whereas 'interval $\neq [0, 25]$' refers to the number of periods where at least one pair's subject has communicated an interval different from $[0, 25]$. For $\beta = 0$ and $\beta = 1$, in most periods an interval different from $= [0, 25]$ has been communicated by at least one subject in a duopoly. The number of periods where at least one pair's subject has actually sent an interval, is very similar for both spillover levels.

In the second part of the table, the periods where communication occurred by at least one pair's subject are further subdivided on the basis of whether the communicated intervals were actually informative and contained the subgame perfect Nash or cooperative prediction. We distinguish between periods where the communicated interval(s) did not contain the subgame perfect Nash nor the cooperative prediction ($x^*, x^{**} \notin$ interval), periods where one or both communicated interval(s) only contained x^* or where one contained x^* and the other nothing or both theoretical benchmarks (only $x^* \in$ interval), periods where one or both communicated interval(s) only contained x^{**} or where one contained x^{**} and the other nothing or both theoretical benchmarks (only $x^{**} \in$ interval), and the rest of the periods where the interval(s) contained both predictions ($x^*, x^{**} \in$ interval) [21] . Categories where $x^*, x^{**} \notin$ interval or where $x^*, x^{**} \in$ interval can be viewed as less informative.

[Table 9 about here]

[21] This "residual" category refers to periods where at least one of the communicated intervals contained both predictions, or where one subject's communicated interval contained the subgame perfect Nash prediction and the other's the cooperative prediction.

For both spillover levels, the share of intervals not containing any theoretical predictions is quite high for some duopolies (e.g. duopoly 2 for $\beta = 0$ and duopoly 3 for $\beta = 1$), suggesting that communication often was simple 'babbling' for these duopolies. For $\beta = 1$ the communicated intervals in this first category were usually above the theoretical cooperative R&D level. Furthermore, the intervals sometimes contained both theoretical predictions, or were mixed (see '$x^*, x^{**} \in$ interval').

Concentrating on informative intervals in the treatment with $\beta = 0$, most intervals that were sent only contained the subgame perfect Nash R&D prediction for all duopolies, while few only contained the cooperative level. With $\beta = 1$ we observe the opposite. Here, the number of intervals that only contained the cooperative level is larger than the number of intervals that only contained the subgame perfect Nash prediction for all duopolies. This suggests that either the ability to find the cooperative R&D level or the willingness to cooperate is higher with spillovers compared to without spillovers, which coincides with the results in the previous subsection. These findings are supported by nonparametric test results [22] .

To summarize, part of the communicated intervals in the cheap-talk treatment did not contain any informative message. Focusing on the intervals that contained relevant information, the sent R&D intervals usually contained the cooperative level in the treatment with spillovers. Without spillovers, the intervals often contained the equilibrium R&D prediction. This suggests that the more cooperative R&D behavior in the cheap-talk treatment with $\beta = 1$

[22] Mann-Whitney tests of differences of the reported data in the table for 'only $x^* \in$ interval' *between* $\beta = 0$ and $\beta = 1$, and for 'only $x^{**} \in$ interval' *between* $\beta = 0$ and $\beta = 1$, yield one-tailed p-values of 0.004 and 0.056 respectively. Wilcoxon-signed ranks tests of differences between 'only $x^* \in$ interval' and 'only $x^{**} \in$ interval', *within* $\beta = 0$ and $\beta = 1$, yield one-tailed p-values of 0.031 and 0.063 respectively.

can partly be explained by the communication of ‘cooperative intervals’.

5 Conclusion and remarks

We have examined the incentives to cooperate in R&D in experimental duopoly markets for two levels of technological spillovers, assuming Cournot competition in the output market. For each of the spillover levels, $\beta = 0$ and $\beta = 1$, the experiment included a baseline treatment without any possibilities of interaction, a cheap-talk treatment with non-binding communication possibilities and a contract treatment with binding contract possibilities.

We find that theoretical predictions perform quite well in the baseline and contract treatments if subjects have had the chance to learn. Conservative statistical tests support that—in general but especially towards the end of the experiment—R&D in the contract treatment is lower than in the baseline treatment for $\beta = 0$, while for $\beta = 1$ it is higher. Furthermore, within the contract treatment, contracted R&D is lower (higher) than R&D in periods without contracts for $\beta = 0$ ($\beta = 1$). We also find that R&D in the baseline treatment without spillovers is significantly higher than in the baseline treatment with spillovers, and lower in the contract treatments, especially when subjects have had the chance to learn.

Contracts were not always committed to, though. Especially in the beginning of the experiment many contract proposals contained R&D levels that deviated from the cooperative level, and proposals often were not accepted. This can partly be explained by the inexperience of the subjects in the first periods of the experiment. Towards the end of the experiment, subjects were able to propose the cooperative R&D level. But still, although proposals were close to the cooperative R&D level in the last periods, they were not always accepted. R&D decisions close to subgame perfect Nash equilibrium levels prevailed in those cases.

Comparing R&D investment in the cheap-talk treatments with the baseline or contract treatments yields different conclusions for $\beta = 0$ and $\beta = 1$. For $\beta = 0$, R&D investment in the cheap-talk treatment does not significantly differ from R&D in the baseline treatment, while for $\beta = 1$ it does not significantly differ from R&D in the contract treatment. More intervals that contained the subgame perfect Nash prediction were sent in the treatment without spillovers, while with spillovers the intervals often contained the cooperative level.

Thus, without technological spillovers, subjects need more 'bindingness' than the sharing of intentions about R&D decisions to deviate from the subgame perfect Nash R&D level towards the cooperative level. For R&D investments subject to high technological spillovers, the sending of messages containing intended R&D investment suffices for R&D investment to be elicited towards the cooperative level. On the other hand, commitment to binding R&D contracts guarantees—for both scenarios of spillovers—that R&D levels are cooperative.

Although we believe that in order to draw policy conclusions more experimental and other research on the topic is needed, we provide two comments for policy purposes. First, the findings in the lab may be interpreted as supportive of policy conclusions that are based on the theoretical literature on cooperative and noncooperative R&D. Although allowing firms to commit to a binding R&D agreement does not always guarantee that firms actually commit to a contract, it certainly helps to induce cooperative R&D levels, which might be useful in industries with large technological spillovers, where higher levels of welfare can be reached under R&D cooperation. The experimental results also make clear that it is important for government to be able to identify whether technological spillovers in industries are low or high, since even in industries characterized by low spillovers, cooperative R&D levels are mostly committed to, if possible.

Second, if it is assumed that in a noncooperative R&D environment firms' representatives or R&D managers may communicate or meet, without being allowed to engage in binding agreements (as e.g. in the product market), only in industries with high technological spillovers R&D investment moves towards the cooperative level. Government stimulation of R&D cooperation between firms may then become unnecessary.

Our policy conclusions are based on the assumption that firms compete in the output stage, irrespective of whether they engage in R&D cooperation with other firms. Whether the formation of research joint ventures affects firms' decisions in the output stage, is left for further research.

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Tables

Table 1
Average R&D decisions per treatment

treatment	benchmarks		average R&D decisions (st. dev.)		
	X^*	X^{**}	periods 1-26	periods 1-10	periods 17-26
$\beta = 0$					
baseline	11.5	5.5	10.7 (2.94)	11.1 (3.51)	10.5 (2.79)
cheap-talk	11.5	5.5	12.3 (1.16)	13.2 (2.23)	11.5 (0.75)
contract	11.5	5.5	8.0 (1.72)	9.3 (1.67)	6.8 (0.74)
$\beta = 1$					
baseline	5.7	12.4	7.2 (1.95)	9.4 (2.84)	5.9 (2.59)
cheap-talk	5.7	12.4	14.2 (6.24)	15.3 (6.42)	12.1 (5.55)
contract	5.7	12.4	9.5 (1.08)	8.8 (3.03)	9.9 (1.22)

Table 2
Baseline treatments

treatment	benchmarks		average duopoly R&D decisions (st. dev.)				
	X^*	X^{**}	1	2	3	4	5
$\beta = 0$							
periods 1-26	11.5	5.5	10.8 (4.3)	5.6 (0.8)	12.7 (6.3)	12.2 (2.5)	12.2 (1.5)
periods 1-10	11.5	5.5	9.1 (6.0)	5.9 (1.2)	14.7 (9.9)	12.5 (2.5)	13.1 (1.7)
periods 17-26	11.5	5.5	11.8 (2.6)	5.5 (0.0)	11.7 (0.5)	11.7 (0.4)	11.6 (1.3)
$\beta = 1$							
periods 1-26	5.7	12.4	7.4 (3.0)	6.1 (1.7)	4.5 (3.3)	9.3 (5.8)	8.7 (5.5)
periods 1-10	5.7	12.4	9.0 (4.1)	6.7 (2.3)	7.1 (3.8)	11.0 (7.5)	13.5 (6.4)
periods 17-26	5.7	12.4	6.1 (1.4)	5.7 (1.3)	2.3 (1.7)	9.6 (5.4)	5.6 (0.1)

Table 3
Cheap-talk treatments

	benchmarks		average duopoly R&D decisions (st. dev.)				
	X^*	X^{**}	1	2	3	4	5
$\beta = 0$							
no messages							
periods 1-26	11.5	5.5	12.8 (3.7)	6.0 (-)	13.0 (3.3)	-	12.4 (6.2)
periods 1-10	11.5	5.5	16.1 (4.3)	6.0 (-)	12.7 (3.7)	-	19.5 (14.8)
periods 17-26	11.5	5.5	10.5 (0.9)	-	12.0 (2.3)	-	11.9 (4.4)
messages							
periods 1-26	11.5	5.5	12.0 (2.8)	10.9 (1.7)	14.3 (-)	11.6 (2.5)	15.2 (5.8)
periods 1-10	11.5	5.5	12.4 (3.3)	10.6 (2.2)	-	11.9 (3.4)	14.1 (3.1)
periods 17-26	11.5	5.5	10.8 (0.6)	10.9 (1.5)	14.3 (-)	11.6 (1.3)	15.7 (-)
$\beta = 1$							
no messages							
periods 1-26	5.7	12.4	19.0 (12.4)	12.6 (0.8)	25.0 (-)	13.1 (5.6)	4.1 (3.2)
periods 1-10	5.7	12.4	32.0 (-)	12.0 (-)	25.0 (-)	15.0 (7.9)	6.5 (3.5)
periods 17-26	5.7	12.4	8.5 (2.1)	13.2 (-)	-	11.2 (1.8)	2.4 (1.9)
messages							
periods 1-26	5.7	12.4	21.2 (7.7)	13.3 (1.2)	17.9 (5.8)	14.4 (5.5)	9.0 (4.2)
periods 1-10	5.7	12.4	24.1 (6.3)	13.3 (2.0)	14.9 (5.2)	15.3 (3.9)	12.0 (-)
periods 17-26	5.7	12.4	16.4 (8.9)	13.2 (0.0)	17.4 (2.1)	12.7 (6.4)	6.0 (-)

Table 4
Contract treatments

	benchmarks		average duopoly R&D decisions (st. dev.)				
	X^*	X^{**}	1	2	3	4	5
$\beta = 0$							
not chosen							
periods 1-26	11.5	5.5	13.9 (1.6)	13.6 (3.6)	11.7 (2.4)	11.3 (5.5)	10.8 (2.0)
periods 1-10	11.5	5.5	12.0 (-)	13.9 (4.6)	9.8 (1.5)	9.8 (6.7)	10.3 (2.2)
periods 17-26	11.5	5.5	-	13.3 (1.1)	14.9 (0.2)	14.5 (-)	12.0 (-)
chosen							
periods 1-26	11.5	5.5	5.6 (0.8)	5.9 (0.1)	6.0 (0.6)	5.9 (1.3)	6.0 (0.1)
periods 1-10	11.5	5.5	5.4 (1.3)	-	6.5 (1.0)	6.5 (2.2)	6.0 (0.0)
periods 17-26	11.5	5.5	5.8 (0.1)	5.9 (0.1)	5.8 (0.0)	5.7 (0.1)	5.9 (0.2)
$\beta = 1$							
not chosen							
periods 1-26	5.7	12.4	4.6 (2.6)	8.5 (4.7)	5.7 (1.2)	5.4 (6.0)	4.8 (1.4)
periods 1-10	5.7	12.4	5.6 (3.1)	10.7 (5.5)	5.8 (1.5)	7.8 (7.8)	4.0 (0.0)
periods 17-26	5.7	12.4	3.7 (1.6)	5.4 (0.8)	5.2 (1.1)	2.8 (2.5)	6.4 (-)
chosen							
periods 1-26	5.7	12.4	12.4 (0.5)	13.0 (3.0)	11.4 (2.4)	12.6 (1.1)	11.9 (0.6)
periods 1-10	5.7	12.4	12.5 (1.0)	20.0 (-)	9.2 (4.0)	13.5 (2.2)	11.5 (0.9)
periods 17-26	5.7	12.4	12.4 (0.1)	12.3 (0.1)	12.1 (0.6)	12.4 (0.2)	12.2 (0.0)

Table 5
Contract and cheap-talk treatment effects

	Periods 1-26		Periods 1-10		Periods 17-26	
	$\beta = 0$	$\beta = 1$	$\beta = 0$	$\beta = 1$	$\beta = 0$	$\beta = 1$
Between contract and baseline						
exact sig. (2-tailed)	0.151	0.095	0.421	0.841	0.151	0.056
exact sig. (1-tailed)	0.075^a	0.048^b	0.210^a	0.421^b	0.075^a	0.028^b
N	10	10	10	10	10	10
Between contract and cheap-talk						
exact sig. (2-tailed)	0.008	0.151	0.056	0.151	0.008	0.151
exact sig. (1-tailed)	0.004^e	0.075^e	0.028^e	0.075^e	0.004^e	0.075^e
N	10	10	10	10	10	10
Between cheap-talk and baseline						
exact sig. (2-tailed)	0.421	0.151	0.421	0.103	1.000	0.095
exact sig. (1-tailed)	0.210^b	0.075^b	0.210^b	0.052^b	0.500^b	0.048^b
N	10	10	10	10	10	10
Within contract						
exact sig. (2-tailed)	0.063	0.063	0.125	0.063	0.125	0.063
exact sig. (1-tailed)	0.031^c	0.031^d	0.063^c	0.031^d	0.063^c	0.031^d
N	5	5	4	5	4	5
Within cheap-talk						
exact sig. (2-tailed)	0.250	0.625	0.750	0.813	0.250	0.250
exact sig. (1-tailed)	0.125^d	0.313^d	0.375^c	0.406^c	0.125^d	0.125^d
N	4	5	3	5	3	4

[a] $H_1 : X_{baseline} > X_{contract/cheap-talk}$;

[b] $H_1 : X_{contract/cheap-talk} > X_{baseline}$;

[c] $H_1 : X_{notchosen/nomessages} > X_{chosen/messages}$;

[d] $H_1 : X_{chosen/messages} > X_{notchosen/nomessages}$;

[e] $H_1 : X_{cheap-talk} > X_{contract}$.

Table 6
Spillover treatment effects

Periods	1-26	1-10	17-26
Baseline treatments			
exact sig. (2-tailed)	0.095	0.548	0.095
exact sig. (1-tailed)	0.048[a]	0.274[a]	0.048[a]
N	10	10	10
Cheap-talk treatments			
exact sig. (2-tailed)	0.222	0.421	0.222
exact sig. (1-tailed)	0.111[b]	0.210[b]	0.111[b]
N	10	10	10
Contract treatments			
exact sig. (2-tailed)	0.151	0.548	0.008
exact sig. (1-tailed)	0.075[b]	0.274[b]	0.004[b]
N	10	10	10

[a] $H_1 : X_{\beta=0} > X_{\beta=1}$.

[b] $H_1 : X_{\beta=1} > X_{\beta=0}$.

Table 7
Percentages of contracts

duopoly		$\beta = 0$			$\beta = 1$	
	all	1-10	17-26	all	1-10	17-26
Contract proposals						
1	100%	100%	100%	85%	80%	80%
2	73%	30%	100%	81%	80%	80%
3	92%	90%	100%	100%	100%	100%
4	96%	90%	100%	89%	90%	100%
5	96%	100%	100%	100%	100%	100%
average %	92%	82%	100%	91%	90%	92%
Contracts actually committed to						
1	89%	90%	100%	50%	40%	60%
2	39%	0%	80%	32%	10%	50%
3	54%	40%	80%	58%	40%	80%
4	89%	80%	90%	46%	30%	70%
5	69%	40%	90%	89%	80%	90%
average %	69%	40%	88%	55%	50%	70%

Table 8
Average R&D contract proposals and standard deviations

duopoly	$\beta = 0$		$\beta = 1$	
	not accepted	accepted	not accepted	accepted
Periods 1-26				
1	2.99 (0.76)	2.80 (0.40)	8.41 (6.20)	6.22 (0.26)
2	3.93 (3.31)	2.95 (0.05)	5.62 (1.47)	6.49 (1.48)
3	3.44 (0.89)	2.99 (0.29)	5.62 (1.39)	5.70 (1.18)
4	2.49 (1.75)	2.97 (0.66)	6.90 (4.11)	6.32 (0.54)
5	3.17 (0.65)	2.98 (0.06)	5.82 (0.46)	5.97 (0.31)
all	3.20 (2.72)	2.94 (0.75)	6.47 (1.47)	6.14 (0.29)
Periods 1-10				
1	3.00 (0.00)	2.70 (0.65)	6.09 (2.13)	6.28 (0.51)
2	5.83 (5.97)	-	4.79 (2.27)	10.00 (0.00)
3	3.86 (1.00)	3.23 (0.52)	5.08 (1.83)	4.58 (2.00)
4	2.26 (2.28)	3.25 (1.10)	6.59 (1.38)	6.73 (1.10)
5	3.29 (0.76)	3.00 (0.00)	5.67 (0.58)	5.75 (0.46)
all	3.65 (2.00)	3.04 (0.57)	5.64 (1.64)	6.67 (0.81)
Periods 17-26				
1	3.30 (1.28)	2.88 (0.04)	8.79 (7.27)	6.22 (0.04)
2	2.95 (0.07)	2.94 (0.05)	6.16 (0.07)	6.49 (0.05)
3	2.90 (0.00)	2.90 (0.00)	6.07 (0.27)	5.70 (0.29)
4	2.90 (0.00)	2.83 (0.07)	8.94 (6.18)	6.32 (0.09)
5	2.90 (0.17)	2.96 (0.09)	6.10 (0.00)	5.97 (0.00)
all	2.99 (0.30)	2.90 (0.05)	7.21 (2.76)	6.13 (0.09)

Table 9
Communicated intervals

	$\beta = 0$						$\beta = 1$					
duopoly	1	2	3	4	5	all	1	2	3	4	5	all
interval $= [0, 25]$	4	0	16	0	5	25	0	0	0	1	19	20
interval $\neq [0, 25]$	22	26	10	26	21	105	26	26	26	25	7	110
total	26	26	26	26	26	130	26	26	26	26	26	130
$x^*, x^{**} \notin$ interval	3	13	2	2	11	31	11	1	22	11	1	46
only $x^* \in$ interval	14	7	6	19	6	52	0	0	0	0	4	4
only $x^{**} \in$ interval	3	5	2	0	3	13	9	23	4	8	2	46
$x^*, x^{**} \in$ interval	2	1	0	5	1	9	6	2	0	6	0	14
total	22	26	10	26	21	105	26	26	26	25	7	110