Four Essays on Experimental Economics

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Ph. D. Thesis/Tesi Doctoral

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[1] This is a joint work with Florian Englmaier, Loreto Llorente, Sander Orderstal & Rupert Sausgruber.
[2] Joint with Jordi Brandts.

[3] Joint with Christiane Schwieren & Gianandrea Staffiero.

1 Introduction

This Ph. D. dissertation consists of four independent studies included in chapters 2 to 5. All of them have in common that economic experiments were used as an analytical tool besides the usual economic theory and econometrics or statistics. There are more similarities. Three of them study the issue of cooperation, chapters 3,4 and 5. Among these three there are two Experimental Industrial Organization papers, chapters 3 and 4, in which the abstract cooperation phenomenon becomes collusion between firms.

Chapter 2 has as a title "The Chopstick Auction: A Study of the Exposure Problem in Multi-Unit Auctions". Multi-unit auctions are sometimes plagued by the so-called exposure problem. In this paper, we analyze a simple game called the 'chopstick auction' in which bidders are confronted with the exposure problem. We analyze the chopstick auction with incomplete information both in theory and in a laboratory experiment. In theory, the chopstick auction has an efficient equilibrium and is revenue equivalent with the second-price sealed-bid auction in which the exposure problem is not present. In the experiment, however, we find that the chopstick auction is slightly less efficient but yields far more revenue than the second-price sealed-bid auction. We conclude that auction designers do not have to worry that the exposure problem leads to low revenue and inefficiency.

Chapter 3 is called "Collusion and Fights in an Experiment with Price-Setting Firms and Production in Advance", here we present results from 50-round market experiments in which firms decide repeatedly both on price and quantity of a completely perishable good. Each firm has capacity to serve the whole market. The stage game does not have an equilibrium in pure strategies. We run experiments for markets with two and three identical firms. Firms tend to cooperate to avoid fights and when they fight bankruptcies are rather frequent. On average, pricing behavior is closer to that for pure quantity than for pure price competition and price and efficiency levels are higher for two than for three firms. Consumer surplus increases with the number of firms, but unsold production leads to higher efficiency losses with more firms. Over time prices tend to the highest possible one for markets both with two and three firms.

Chapter 4 is called "Price-Quantity Competition and Edgeworth Cycles". In this study I consider markets in which eight firms compete deciding simultaneously on price and quantity with a given capacity. Every round the same eight firms met each other in the same market. If a firm made high enough losses it went bankrupt and left the market. In this case capacity was distributed among surviving firms following a proportional profit rule. All observations exhibit strong cycles in price and supplied quantity that resemble Edgeworth cycles. Actually firms tended to undercut each others price increasing their production at the same time. Eventually prices reached the marginal cost, then a firm set a very high price and a small quantity and the others followed it and another cycle started.

Finally, chapter 5 has as title "Feeding the Leviathan". Here, using a step-level public good game, we analyze the effects on contributions of having played under a costly sanctioning regime. We find that "educational" effects, in terms of learning a particular way to coordinate towards "good" equilibria, are more relevant than motivational "crowding out" effects, whereby cooperating to avoid sanctions spoils intrinsic incentives. In one of our treatments people vote on whether to remove the sanctioning system, whereas in the other removal is automatic. In the voting treatment participants cooperate as much as in the automatic removal only when the decision to remove the sanctioning device entails a clear "trust" message.

I have to acknowledge the kind help and support received from my advisor Prof. Jordi Brandts. I give special thanks to Pablo Brañas-Garza, Róbert Veszteg, Pedro Rey Biel and Aurora García Gallego for they useful comments. I have to notice the patience of my tolerant coauthors Jordi Brandts, Florian Englmaier, Loreto Llorente, Sander Orderstal, Rupert Sausgruber, Christiane Schwieren and Gianandrea Staffiero. Finally, I thank the IDEA program students and the faculty of the Economics Department at Universitat Autònoma de Barcelona and Institut d'Anàlisi Econòmica for their nice formal and informal comments.

2 The Chopstick Auction: A Study of the Exposure Problem in Multi-Unit Auctions[4]

2.1 Introduction

Multi-unit auctions are sometimes plagued by what is called the 'exposure problem'. We speak of an exposure problem when bidders aim at winning several units but are *exposed* to the risk of buying too few as competition on some of these units turns out to be tougher than expected.[5] Several economists have argued that the exposure problem in auction should be prevented as it leads (1) to an inefficient outcome of the auction and (2) to low revenue. In this paper, we will investigate whether these claims are true, both using a game theoretical model and a laboratory experiment.

Economic theory sketches a mixed picture about both claims. Theoretical papers by Robert Rosenthal and coauthors include situations in which the exposure problem is present. Szentes and Rosenthal (2003a, 2003b) find efficient equilibria in multi-unit auctions of homogeneous objects. However, Krishna and Rosenthal (1996), and Rosenthal and Wang (1996) construct inefficient equilibria in the case of heterogeneous objects. In these papers, the authors analyze multi-unit auctions with two types of bidders, namely 'local bidders' who are interested in only one object, and 'global' bidders who try to acquire several. The global bidders, in competition with the local ones, face the exposure problem when attempting to realize synergies between the objects. In line with this, Bykowsky et al. (1998) give an illustrative example in which the equilibrium outcome is such that either the allocation is inefficient or at least one of the bidders ends up paying more for the purchased items than they are worth to her.

Other theorists have investigated the relationship between efficiency and revenue. Ausubel and Cramton (1998, 1999) stress the importance of efficiency of the auction outcome in terms of revenues for the seller in auctions of perfectly divisible objects. Ausubel and Cramton (1999) show that efficiency of the auction outcome is necessary for revenue

[4]This is a joint work with Florian Englmaier, Loreto Llorente, Sander Orderstal & Rupert Sausgruber.
[5]See also Bykowsky et al. (1998), and Milgrom (2000).

maximization when the auction is followed by a perfect resale market and when the seller cannot commit to not selling some units. However, usually there is a trade-off between efficiency and revenue. In Myerson's (1981) model, the seller maximizes his expected revenue by imposing a reserve price and hence excluding bidders with low values from winning the object. Milgrom (2000) constructs an example in which there is a trade-off between efficiency and revenue in the case of multi-unit auctions: the seller realizes a less efficient outcome when using larger packages but gets a higher revenue.

In practice, it is also not clear whether the exposure problem is a major issue. At least Klemperer (2002) does not include the warning 'avoid the exposure problem' in his list of issues that are of practical importance in the design of (multi-unit) auctions. However, Van Damme (2000) claims that the exposure problem led to low bids and an inefficient outcome in the Dutch DCS-1800 auction. In February 1998, the Dutch government auctioned licenses for second generation mobile telecommunication using an auction with almost the same rules as the FCC auctions in the US.[6] A difference between the Dutch DCS-1800 auction and the American auctions was that in the American auctions, the exposure problem was not seriously present as bidders were allowed to withdraw their bids. Van Damme argues that the FCC auction format would have lead to a higher revenue and a more efficient outcome.

Does the exposure problem indeed lead to inefficient outcomes and low revenues? In order to answer this question, we designed a laboratory experiment in which we confronted subjects with a simple auction game called 'the chopstick auction' (CSA).[7] In this auction, a seller simultaneously sells three chopsticks. There are 2 bidders in the auction, who independently submit a bid, which is the price for one chopstick. Call the second highest bid p. The outcome of CSA is such that the highest bidder gets two chopsticks for a price of $2p$ and the second highest bidder gets one chopstick for p. We compared CSA with the second-price sealed-bid auction (SPSB) in which two chopsticks are sold as one

[6] See McMillan (1994), Cramton (1995, 1998), McAfee and McMillan (1996), and Milgrom (2000) for descriptions and discussions of these auctions.

[7] The credit for the name of this auction game goes to Mary Lucking-Reiley. Thanks to Balasz Szentes and Robert Rosenthal for pointing this out to us.

bundle. The only difference with the 'usual' second-price sealed-bid auction is that the winning bidder has to pay the second highest bid twice, once for each chopstick. We investigated bidding behavior in CSA and in SPSB in the following setting. Bidder i's ($i = 1, 2$) marginal values are zero on the first chopstick, v_i on the second, and zero on the third. The signals v_i are drawn independently from a uniform distribution on the interval $[0, 100]$. As the second highest bidder wins a worthless chopstick for a positive price, bidders face the exposure problem in CSA.

A game closely related to CSA is the dollar auction. In the dollar auction, two bidders play an ascending auction in order to win \$1. The highest bidder wins the dollar, but both bidders pay the price at which the second highest bidder decided to step out. Note that bidders face the exposure problem here: the second highest bidder fails to win a dollar but still has to pay a positive price. In equilibrium, bidders play a mixed strategy in which both independently pick a price level at which they leave the auction. This price level follows an exponential distribution with mean 1. The expected revenue for the seller of the dollar is exactly one dollar. However, when Shubik (1971) and others confronted subjects with this game in the lab, the average revenue was significantly larger than \$1.

There are two main differences between Shubik's experiment and ours. First, subjects in the dollar auction are completely informed about the value of the auctioned object (which is \$1 for both bidders), whereas in our experiment, subjects are only incompletely informed about the value of the other bidder. Moreover, the winner in Shubik's experiment pays the same amount as the loser, whereas in CSA, the winner pays twice the loser's bid.

In this paper, we study CSA and SPSB in a laboratory experiment and confront the outcomes with theoretical predictions. In section 2, we will give theoretical properties of CSA and SPSB in terms of equilibrium behavior and expected revenue.[8] Assuming that both bidders are risk neutral and draw their value v_i from a uniform distribution on $[0, 100]$, we find that CSA has an efficient Bayesian Nash equilibrium. From standard auction theory we learn that SPSB has an efficient equilibrium in dominant strategies in which each bidder submits a bid equal to half her value for each chopstick. The revenue

[8]See Onderstal (2002) for a more detailed theoretical investigation of the chopstick auction.

equivalence theorem (Myerson, 1981) then implies that CSA is revenue equivalent with SPSB. In other words, in this theoretical setting, auctions in which the exposure problem is present perform as well as auctions in which it is not. That makes this setting a useful benchmark to test the two claims we started with.

In section 3, we describe the experimental design and present the results of the experiment. Our first result is the striking difference between the obtained revenue in CSA and SPSB. In line with the outcomes of the dollar auction, revenue tends to be higher when bidders are confronted with the exposure problem than if they are not. Our second finding is that there is a significant but small difference between the efficiency of CSA and of SPSB. Both auctions turn out to be reasonably efficient. Although we observe only little learning during the experiment, these results seem to be robust.

Our third result may seem somewhat surprising: in SPSB, the average revenue was about 20% above the theoretical outcome, assuming that the bidders play a weakly dominant strategy, i.e., bidding half their value. This finding is in contrast to what is found in experiments by Kagel et al. (1987), Kagel and Levin (1993), and Harstad (2000) on the 'standard' second-price sealed-bid auction. In these experiments, the average revenue was only about 10% above the dominant strategy.[9] A possible explanation of this result is that we have complicated the game somewhat: when winning, a bidder has to pay *twice* the second highest bid, once for each chopstick. This is different than what happens in the usual second-price sealed-bid auction, in which the winner pays the second highest bid only once. This framing effect shows that even a slight complication of the environment may make it harder for people to act rationally. Also this result is robust in the sense that we do this observation both in the earlier and the later periods in the experiment.

We conclude that our experiment does not give a convincing reason why the warning 'avoid the exposure problem' should be added to Klemperer's list of practical issues in the design of auctions. CSA is virtually as efficient as SPSB and turns out to yield much more revenue.

[9] See Kagel (1995) for an overview of laboratory experiments on the second-price sealed-bid auction.

2.2 Theory

Consider a situation with 2 bidders, labelled 1 and 2, who wish to eat Chinese food. However, none of the bidders has anything to eat with. Suppose that a seller sells 3 chopsticks in the chopstick auction (CSA) which has the following rules. The price starts at zero and is continuously raised. Bidders have the opportunity to leave the auction at any price they desire. The auction ends when one bidder quits. She wins one chopstick and pays the price at which she leaves. The remaining bidder wins two chopsticks and pays two times the price at which the second highest bidder has quit. If there is a tie, the winner of the auction is determined by tossing a fair coin.

The value $V_i(s)$ bidder i attaches to owning s chopsticks is given by

$$V_i(s) = \begin{cases} v_i & s = 2, 3 \\ 0 & s = 0, 1, \end{cases}$$

where v_i is a private signal for bidder i. In words, a bidder attaches a value of v_i to winning two chopsticks and no value to winning only one chopstick or to winning a third one. We assume that the v_i's are drawn independently from a uniform distribution on the interval $[0, 100]$.

Each bidder is a risk neutral expected utility maximizer. Suppose that the price realized in CSA is equal to p. The utility for bidder i having drawn a value equal to v_i is given by

$$u_i(v_i, s, p) = \begin{cases} v_i - 2p & s = 2 \\ -p & s = 1 \end{cases}.$$

Observe that CSA is strategically equivalent to the following sealed-bid auction. The highest bidder, let's say i, wins an object with value v_i and pays twice the bid of the other bidder. The other bidder receives nothing, but pays his bid once. We use this sealed-bid version in our laboratory experiment.

Proposition 1 gives equilibrium bidding in CSA. By a standard argument, this bid function must be strictly increasing and continuous. Let $U(v,w)$ be the utility for a bidder with signal v who behaves as if she has signal w, whereas the other bidders play according to the equilibrium bid function. A necessary equilibrium condition is that

$$\frac{\partial U(v,w)}{\partial w} = 0$$

at $w = v$. From this condition, a differential equation is derived, from which the equilibrium bid function is uniquely determined.

Proposition 1 *Let $B(v)$, the bid of a bidder with signal v, be given by*

$$B(v) = v + [100 - v][\log(100 - v) - \log 100].$$

Then B is the unique symmetric Bayesian Nash equilibrium of CSA. The outcome of the auction is efficient. The bidder with the lowest possible value obtains zero utility.

Proof. See the appendix. ■

Let us compare the outcomes of CSA with the second-price sealed-bid auction in which two chopsticks are sold as one bundle (SPSB). The only difference with the 'usual' second-price sealed-bid auction is that the winning bidder has to pay the second highest bid twice, once for each chopstick. Proposition 2 gives the equilibrium properties of SPSB.

Proposition 2 *Let $b(v)$, the bid of a bidder with signal v, be given by*

$$b(v) = \frac{1}{2}v.$$

Then b is the unique symmetric Bayesian Nash equilibrium in weakly dominant strategies *of SPSB. The outcome of the auction is efficient. The bidder with the lowest possible value obtains zero utility.*

Proof. Standard. ■

Propositions 1 and 2 show that both auctions are efficient. In other words, a seller who is concerned about efficiency is indifferent between the two auction types.

Moreover, both auctions turn out to be revenue equivalent, and generate the same expected utility for the bidders. This is a direct consequence of the revenue equivalence theorem (Myerson, 1981), using the following two observations. First, both CSA and SPSB are auctions of a single object, namely a set of two chopsticks. Second, according to Propositions 1 and 2, both auctions are efficient and the utility of the bidder with the lowest possible value is equal to zero. The interpretation of this revenue equivalence result is that a risk neutral seller interested in revenue is indifferent between using CSA and SPSB to sell the chopsticks. Proposition 3 summarizes this finding, and gives the expected revenue and the expected utility for the bidders.

Proposition 3 *Suppose that bidders play CSA and SPSB according to the strategies given in Propositions 1 and 2 respectively. Then for both CSA and SPSB, expected revenue equals* $33\frac{1}{3}$ *and ex ante expected utility for a bidder is* $16\frac{2}{3}$*.*

2.3 Laboratory experiment

We present the results of our laboratory experiment in four parts. In the first part, we describe the experimental design. In the second part, we analyze bidding behavior. Part three presents total revenue generated by the auctions. In the final part, we focus on efficiency.

2.3.1 Experimental design

In a computerized laboratory experiment, we studied CSA and SPSB in a setting that is closely related to the theoretical setting.[10] The main differences are the following. First of all, the subjects in the lab were confronted with the sealed-bid version of CSA. Subjects did not see the price rise until one of them indicated to leave the auction. Instead, subjects were asked at which price they would desire to quit. However, the two games are strategically equivalent, so that we did not expect much differences in the outcomes.[11]

Secondly, we approximated the continuous signal and bidding spaces with fine grids. Subjects drew their values from a grid between 0 and 100, with 1 as the smallest step. Our theoretical results have been based on the assumption that bidders draw their signals from the interval [0,100]. Moreover, subjects could choose prices from a finite grid between 0 and 999, with 1 as the smallest step. The theory has been based on the assumption that bidders can choose their bids from a continuous action space. However, both grids are sufficiently fine to approximate the continuous signal and action space.

The experiments were conducted at the faculty of economics and social sciences at Innsbruck University between 15 May and 3 June 2002. We conducted four sessions, each consisting of 24 subjects. We had between-subject treatments. In two sessions, subjects played CSA, and in the other two sessions, subjects were confronted with SPSB. In all sessions, the subjects were separated in groups of four. We ran three practice periods, followed by 40 paid trading periods. Before the start of each period, the subjects were randomly re-matched to an opponent in their group of four, resulting in 12 independent observations per treatment. In each period, all subjects drew a new value for two chopsticks. At the beginning of each session, subjects read the instructions (see the appendix). Questions were answered privately.

Subjects were paid a lump sum transfer (5 euros) for showing up and an additional

[10] The experiment has been programmed and conducted with the software Z-tree (Fischbacher, 1999).

[11] Still, we should be somewhat cautious, as 'framing effects' may occur. For instance, in experiments by Coppinger et al. (1980) and Cox et al. (1982), the first-price sealed-bid auction turned out to generate higher prices than the Dutch auction, despite the fact that both games are strategically equivalent.

reward equivalent to their gains in the auctions.[12] They earned points which were calculated as the difference between the value of the chopsticks they won minus the price they paid. Points were exchanged into cash according to the exchange rate

$$100 \text{ points} = 3 \text{ Euro}.$$

In CSA, the winner of just one chopstick gets a negative score equal to the amount he paid for it. The maximum score in a period is 100 points, i.e., the maximum value (100) minus the minimum payment (0). If subjects played according to the bidding strategies in Propositions 1 and 2, they would have earned 25 euros on average.[13] The experiments lasted about 45 minutes, so that the subjects could earn a salient amount of money.

2.3.2 Results: revenue

What is the average revenue in the auctions? In CSA, revenue equals three times the price: the winner of two chopsticks pays this price twice, the winner of one chopstick once. In SPSB, revenue is equal to twice the price. See figure 1 for the average revenue in each period.

[12] Paying every period as we did induces behavior towards risk neutrality. Paying according to one randomly selected period, instead, may increase subjects' willingness to take risks (Davis and Holt, 1993).

[13] The calculation is as follows. According to Proposition 3, subjects earn on average $16\frac{2}{3}$ points per period. Given than they play 40 periods, and the exchange rate of 100 points = 3 Euro, they expect to earn $16\frac{2}{3} * 40 * \frac{3}{100} = 20$ Euro. Add to this number the 5 Euro lump sum transfer in order to end up with 25 Euro.

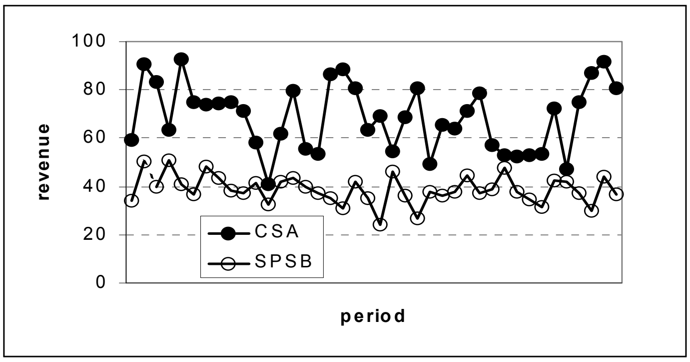

Figure 1: Average revenues in CSA and SPSB.

Result 1 *CSA yields more revenue than predicted by the theory.*

Subjects turned out to pay much more in CSA than predicted by the theory. Given the values in the experiment, the average revenue would have been 33 points per period if bidders had bid according to the bid function in Proposition 1. In reality, the average revenue was 68 points. As a consequence, the average payments to a subject was very low, i.e., about 60 eurocents. It is unclear to us what drives this result. Perhaps bidders are loss averse, and submit high bids in order to avoid losses. However, Onderstal (2002) shows that in CSA, loss averse bidders bid *lower* than risk neutral bidders so that loss aversion does not seem to be a satisfactory explanation. Of course, it could be that the subjects experienced difficulties in understanding the auction. The observation that a substantial number of bids constituted weakly dominated strategies indeed indicates this. We tested for learning by making statistical comparisons separately for the first and last 20 periods in order to account for learning effects. We found some learning in the sense that the difference between the average payment and the theoretical revenue was lower in the last 20 periods than in the first 20. However, convergence was slow as bids remained much higher than the theoretical prediction. We do not know what would have happened if the subjects had played more periods.

Result 2 *SPSB yields more revenue than predicted by the theory.*

For SPSB, the theory predicts that revenue would have been 32 points on average per period. In the experiment, average revenue was equal to about 39 points, 22% more than the theoretical prediction. The subjects earned 19 euros on average, which is clearly less than the 25 euro they could have earned if they had played the weakly dominant strategy. As we have observed in the previous section, although a large fraction of the bidders bids according to the equilibrium strategy, a substantial subset of the bidders bid more than 50% of their value, playing a weakly dominated strategy. A possible explanation of this result is that we have complicated the game somewhat: when winning, a bidder has to pay *twice* the second highest bid, once for each chopstick. This is different than what happens in the 'usual' second-price sealed-bid auction, in which the winner pays the second highest bid only once. Overbidding in SPSB may be driven by the framing. Also this result turns out to be robust in the sense that overbidding is still present in later periods.

Result 3 *CSA and SPSB are not revenue equivalent: CSA yields much more revenue than SPSB.*

Our third result is the striking difference between the obtained revenue in CSA and SPSB. In line with the outcomes of the dollar auction, revenue tends to be higher when bidders are confronted with the exposure problem than if they are not. A Mann-Whitney test reveals that the difference in revenue between CSA and SPSB is significant at a 1% level (p=0.0005). For both CSA and SPSB we observe a trend towards the prediction by the theory. In periods 1-20, the average revenue in CSA is 71 and in SPSB 40. In periods 21-40, we observe 66 for CSA and 38 for SPSB. The difference is still large and significant at a 1% level for the later periods.

2.3.3 Results: efficiency

Efficiency is defined as follows

$$\text{Efficiency} = \frac{\text{value of the winning bidder}}{\max\{v_1, v_2\}}.$$

Figure 2 shows the development of efficiency over the periods in both CSA and SPSB.

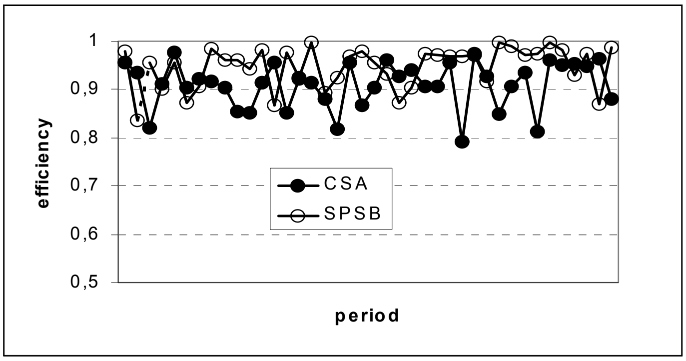

Figure 2: Average efficiency of CSA and SPSB.

Propositions 1 and 2 predict that both auctions are 100% efficient. In a worst case scenario, if the two chopsticks are assigned using a lottery, expected efficiency equals 75%.[14] In CSA, we observed an average efficiency equal to 91%, which means that

Result 4 *CSA is reasonably efficient.*

[14]The calculation for this number is the following. As both bidders are ex ante symmetric, we may assume without loss of generality, that the lottery always assigns two chopsticks to bidder 1. Expected efficiency is then given by

$$\begin{aligned} \text{Expected efficieny} &= \int_0^{100} \left[\int_0^{v_2} \frac{v_1}{v_2} * \frac{1}{100} dv_1 + \int_{v_2}^{100} 1 * \frac{1}{100} dv_1 \right] \frac{1}{100} dv_2 \\ &= \frac{3}{4}. \end{aligned}$$

The first term in the inner integral refers to the case that bidder 2 has a higher value than bidder 1 (so that efficiency equals $\frac{v_1}{v_2}$). In the second term, bidder 1 has the higher value (so that efficiency equals 1).

The efficiency of CSA is much closer to the theoretical prediction of 100% than the outcome of a lottery. The same holds true for SPSB in which efficiency was 95%. This finding is probably explained by the fact that several subjects bid their value instead of half of it. Still,

Result 5 *SPSB is reasonably efficient.*

Using a Mann-Whitney test, we observe that the difference in efficiency between CSA and SPSB is significant only at a 5% level ($p = 0,0209$). Still the difference is not large, so that we conclude that

Result 6 *SPSB is only slightly more efficient than CSA.*

We have checked whether these results change during the course of the experiment. This turns out not to be the case. For the first 20 periods, we observed 91% efficiency in CSA versus 94% in SPSB. In the final 20 periods (periods 21-40) we observe virtually no difference (91% in CSA and 95% in SPSB). In both the early periods and the late periods, the difference between CSA and SPSB is significant at a 5% level.

2.4 Conclusions

In this paper, we have investigated the effect of the exposure problem on bidding behavior in auctions. In contrast to some theoretical papers and concerns raised by the outcome of the Dutch DCS-1800 auction, we conclude that auction designers do not have to worry that the exposure problem leads to low revenue and inefficiency. On the contrary: our experiment has shown that auctions in which the exposure problem is present may yield far more revenue for the seller than auctions in which it is not. Moreover, the difference in efficiency is rather small.

Does this mean that we recommend governments to design auctions in which the exposure problem is present? Probably not: especially if large amounts of money are at stake, bidders are wise enough to hire experts in auction theory whom we expect to convince bidders to correctly take the risks into account associated with the exposure problem. Still, in the case that a government is not sure about the conditions on the demand side, it may safely split up supply in small parts. The bidders could sort out themselves how many units they need to secure sufficient surplus. Depending on the shape of demand, the government may then design an auction in which the exposure problem is present. Our experiment has shown that this need not have a detrimental effect on the outcome.

What has remained somewhat puzzling to us is the observation that subjects in SPSB do not play weakly dominant strategies. We conclude that we have touched a broader topic in experimental economics or even in economics in general: a slight complication of the environment has a significant effect on the outcomes. This may be important for many applications/situations, from the introduction of elaborate pricing schemes to new currencies like the euro.

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2.6 Appendix

2.6.1 Proof of Proposition 1

Let $B(v)$, the bid of a bidder with signal v, be given by

$$B\left(v\right)=v+\left[100-v\right]\left[\log\left(100-v\right)-\log 100\right].$$

Then B is the unique symmetric Bayesian Nash equilibrium of CSA. The outcome of the auction is efficient. The bidder with the lowest possible value obtains zero utility.

Proof. The following observations imply that a symmetric equilibrium bid function must be strictly increasing. First, a higher-value type of a bidder cannot exit before a lower-value type of the same bidder would exit. (Suppose the lower type is indifferent between two different strategies, giving her two different probabilities of being the ultimate winner of two chopsticks. The higher type then strictly prefers the strategy with the higher probability to win. Therefore, she will never quit earlier than the lower type.) Furthermore, there is no range in which the bid function is flat. (Suppose there is the bid function is flat at a price level of p. Then each bidder being in the range of signals that bid p exits the auction with positive probability at p. But if this is the case, then each bidder strictly prefers staying just a bit longer.)

Let $\tilde{B}$ be a symmetric and strictly increasing equilibrium bid function. If the other bidder bids according to $\tilde{B}$, the expected utility of a bidder with signal v who bids as if she has signal w is given by

$$U(v,w)=-(1-\frac{w}{100})\tilde{B}\left(w\right)+\int_{0}^{w}(v-2\tilde{B}\left(x\right))\frac{1}{100}dx.$$

The first (second) term of the RHS refers to the case that the bidder makes the second highest (highest) bid.

The FOC of the equilibrium is

$$\frac{\partial U(v,w)}{\partial w} = -(1-\frac{w}{100})\tilde{B}'(w) - \frac{\tilde{B}(w)}{100} + \frac{v}{100} = 0 \tag{1}$$

at $w = v$. Rearranging terms we find

$$\frac{(100-v)\tilde{B}'(v) + \tilde{B}(v)}{(100-v)^2} = \frac{v}{(100-v)^2},$$

which is equivalent to

$$\frac{\tilde{B}(v)}{100-v} = \int_0^v \frac{x}{(100-x)^2}dx + C,$$

for some C. C must be zero (C must be at least zero, otherwise a bidder with signal 0 submits a negative bid; if C is larger than zero, a bidder with signal 0 submits a strictly positive bid. As $\tilde{B}$ is (by assumption) strictly increasing, this bidder submits the lowest bid with probability one, and has to buy one chopstick for a positive price. Clearly, she is strictly better off by bidding zero.) Also the SOC is fulfilled as $sign(\frac{\partial U(v,w)}{\partial w}) = sign(v-w)$. It is readily checked that B is the unique solution.

What remains to be checked it that B is strictly increasing. From (1), B is strictly increasing if and only if $B(v) < v$ for almost all $v \in [0,100]$. This is true, as

$$\begin{aligned} B(v) &= v + [100-v][\log(100-v) - \log 100] \\ &< v \end{aligned}$$

for all $v \in (0,100)$. As B is strictly increasing, CSA is efficient. ■

2.6.2 Proof of Proposition 3

Suppose that bidders play CSA and SPSB according to the strategies given in Propositions 1 and 2 respectively. Then for both CSA and SPSB, expected revenue equals $33\frac{1}{3}$ *and ex ante expected utility for a bidder is* $16\frac{2}{3}$.

Proof. Expected revenue in SPSB can be calculated as follows. As in equilibrium, each bidder submits a bid equal to 50% of his value, the winner is the bidder with the highest value. She pays twice the bid of the lowest bidder. In other words, revenue is equal to the lowest value. Therefore, expected revenue is equal to $33\frac{1}{3}$ as this is the expectation of the lowest from two numbers independently drawn from a uniform distribution on the interval $[0, 100]$. The utility for the winner of SPSB equals the value obtained minus the price paid. The expected value of two chopsticks for the winner is equal to the maximum of two numbers drawn independently from the uniform distribution on the interval $[0, 100]$, i.e. $66\frac{2}{3}$. Given that he pays $33\frac{1}{3}$ in expectation, the winner's expected utility equals $33\frac{1}{3}$. As ex ante both bidders have probability $\frac{1}{2}$ to be the winner, ex ante expected utility for a bidder equals $16\frac{2}{3}$. CSA yields the same expected revenue and the same ex ante expected utility for each bidder as SPSB. This follows immediately from the revenue equivalence theorem (Myerson, 1981), as in equilibrium, both auctions are efficient and yield zero expected utility for the bidder with the lowest possible value. ■

2.6.3 Instructions for the experiment

Original instructions were in German. These are instructions for treatment CSA.

General information for participants

You are taking part in an economics experiment funded by the *Jubilaeumsfonds der Oesterreichischen Nationalbank*. The purpose of the experiment is to analyze decision behavior in markets.

You will receive 5 Euro for showing up. If you carefully read the instructions and follow the rules you can earn a fair amount of money. During the experiment you can earn additional amounts of money. In this experiment you earn points. These points will be converted with a conversion rate of

$$100 \text{ points} = 3 \text{ Euro}.$$

Your final payoff consists of the initial 5 Euro given to you at the beginning of the experiment and the money you earn in the course of the experiment. You will be paid immediately after the experiment in cash.

During the experiment communication is forbidden. If you have questions, please ask us. We will gladly answer your questions individually. It is very important that you follow this rule. Otherwise the results of the experiment will be of no value from a scientific perspective.

Detailed instructions

In this experiment each participant is a buyer. You and one other buyer will participate in an auction in order to obtain units of a good. There are two possible outcomes. Either you obtain one unit, or you obtain two units. If you obtain only one unit, this is of no value for you. If you obtain two units, this will have a positive value for you. You will be informed about your value of obtaining two units. This value is a number between 0 and 100. Your value of obtaining two units of the good is randomly determined such that each number is equally likely to occur. This value is private information, i.e. neither you know the other buyer's value nor does the other buyer know your value.

The experiment consists of 3 practice periods and 40 trading periods. The practice periods will not account for your final earnings. But you should take these periods seriously since you will gain valuable experience for the trading periods that are paid.

In each period you will participate in an auction with a second buyer. In each period you are randomly matched with another buyer. You will never know whom you are matched with and it may be that you are matched with somebody more often than once.

In each period you and every other buyer are assigned new values for obtaining two units of the good. Notice that your value is very likely to be different from other buyers' valuations.

The auction rules

The good is sold according to the following rules:
Each buyer is asked to submit a bid. This bid is the maximum amount the bidder is willing to pay for one unit of the good. The buyer who submitted the higher bid is the winner and obtains two units of the good. The buyer who submits the lower bid obtains only one unit.

For every unit you obtain you have to pay a price. This price equals the lower of the two submitted bids. The price and your value determine what you earn.

If you are the winner, i.e. you have obtained 2 units, this has a positive value for you but you have to pay the price for each of those units, i.e. you earn a number of points equal to your value minus two times the price.

If you have obtained only 1 unit this is of no value for you but you have to pay the price for this unit, i.e. you lose a number of points equal to the price.

Note that you can, dependent on the price and your value, make losses.

Example

The following examples shall help you to become familiar with the auction and the design of the interface. You will first see the Decision Screen and then the Result Screen.

– *Here the instructions contained a screenshot of the Decision Screen.* –

In this case your valuation for obtaining two units of the good is 74. Your bid is 32.

Important:

If you do not submit a bid within the prespecified time the computer will assign you a bid of 0.

When the time is elapsed you will see the Result Screen. *(Notice that the numbers given in the screens serve illustrative purposes only.)*

– *Here the instructions contained a screenshot of the Result Screen of a winner.*
–

>From the screen above you see that you submitted the highest bid. You obtain 2 units, realize a value of 63 and you pay 2 times the price. Check that you earned 33 point.

– *Here the instructions contained a screenshot of the Result Screen of a loser.*
–

Here you see that the other buyer submitted the winning bid and you obtain only 1 unit of the good. You do not realize your value of 68 but you have to pay the price for one unit. Therefore you lose 30 points.

Good Luck!

These are instructions for treatment SPSB.

General information for participants

You are taking part in an economics experiment funded by the *Jubilaeumsfonds der Oesterreichischen Nationalbank*. The purpose of the experiment is to analyze decision behavior in markets.

You will receive 5 Euro for showing up. If you carefully read the instructions and follow the rules you can earn a fair amount of money. During the experiment you can earn additional amounts of money. In this experiment you earn points. These points will be converted with a conversion rate of

$$100 \text{ points} = 3 \text{ Euro} .$$

Your final payoff consists of the initial 5 Euro given to you at the beginning of the experiment and the money you earn in the course of the experiment. You will be paid immediately after the experiment in cash.

During the experiment communication is forbidden. If you have questions, please ask us. We will gladly answer your questions individually. It is very important that you follow this rule. Otherwise the results of the experiment will be of no value from a scientific perspective.

Detailed instructions

In this experiment each participant is a buyer. You and one other buyer will participate in an auction in order to obtain units of a good. There are two possible outcomes. Either you obtain one unit, or you obtain two units. If you obtain only one unit, this is of no value for you. If you obtain two units, this will have a positive value for you. You will be informed about your value of obtaining two units. This value is a number between 0 and 100. Your value of obtaining two units of the good is randomly determined such that each number is equally likely to occur. This value is private information, i.e. neither you know the other buyer's value nor does the other buyer know your value.

The experiment consists of 3 practice periods and 40 trading periods. The practice periods will not account for your final earnings. But you should take these periods seriously since you will gain valuable experience for the trading periods that are paid.

In each period you will participate in an auction with a second buyer. In each period you are randomly matched with another buyer. You will never know whom you are matched with and it may be that you are matched with somebody more often than once.

In each period you and every other buyer are assigned new values for obtaining two units of the good. Notice that your value is very likely to be different from other buyers' valuations.

The auction rules

The good is sold according to the following rules:
Each buyer is asked to submit a bid. This bid is the maximum amount the bidder is willing to pay for one unit of the good. The buyer who submitted the higher bid is the winner and obtains two units of the good. The buyer who submits the lower bid obtains only one unit.

For every unit the winner obtains, she has to pay a price. This price equals the lower of the two submitted bids. The price and your value determine what you earn.

If you are the winner, i.e. you have obtained 2 units, this has a positive value for you but you have to pay the price for each of those units, i.e. you earn a number of points equal to your value minus two times the price. Note that you can, dependent on the price and your value, make losses.

If you have obtained only 1 unit this is of no value for you and you don't have to pay the price for this unit, i.e. your income in this period is equal to 0.

Example

The following examples shall help you to become familiar with the auction and the design of the interface. You will first see the Decision Screen and then the Result Screen.

– *Here the instructions contained a screenshot of the Decision Screen.* –

In this case your valuation for obtaining two units of the good is 96. Your bid is 88.

Important:

If you do not submit a bid within the prespecified time the computer will assign you a bid of 0.

When the time is elapsed you will see the Result Screen. *(Notice that the numbers given in the screens serve illustrative purposes only.)*

– *Here the instructions contained a screenshot of the Result Screen of a winner.* –

>From the screen above you see that you submitted the highest bid. You obtain 2 units, realize a value of 96 and you pay 2 times the price of 40. Check that you earned 16 point.

– *Here the instructions contained a screenshot of the Result Screen of a loser.* –

Here you see that the other buyer submitted the winning bid and you obtain only 1 unit of the good. You do not realize your value of 68 and you don't have to pay the price for this unit. Therefore your income in this period is 0.

Good Luck!

3 Collusion and Fights in an Experiment with Price-Setting Firms and Production in Advance[15]

3.1 Introduction

In the most prominent theoretical models of oligopolistic competition, going back to Cournot (1838), Bertrand (1883) and Edgeworth (1925), firms only make decisions on one variable: price or quantity. These models have proven to be extremely useful for the study of a large variety of issues. However, for a more complete view of imperfect competition one needs to go beyond this, since firms' actual decision environments surely involve quite a number of dimensions. A natural step forward is to study situations in which firms decide on both price and quantity.

Competition in prices and quantities can be modeled in different ways. One of these ways is the "supply function" approach proposed by Grossman (1981) and Hart (1982). Here firms' strategies consist in complete functions of price-quantity pairs. The outcomes of market competition are the equilibria in supply functions; production is to order so that there is neither over nor underproduction.

Kreps & Scheinkman (1983) approached price-quantity competition using a two stage model in which firms decide on capacities first and then compete in prices. They solved the problem of inexistence of equilibrium in the so called Bertrand-Edgeworth model in which due to capacity restrictions there is no equilibrium in pure strategies. In their game firms actually first decide on capacities and then on prices. This two stage structure plus a surplus-maximizing rationing rule yields Cournot outcomes [Kreps & Scheinkman (1983), Vives (1993)].

A simpler more direct way of representing price-quantity competition is to let firms decide on price and quantity combinations where the quantities have to be produced in

[15] Joint with Jordi Brandts.

advance and the demand buys from the cheapest producer(s). This is characteristic of many retail markets. The fact that total production can now be larger than total sales raises the question of what happens with the overproduced quantity. Underproduction also needs to be taken into consideration. The produced good may be completely perishable or not. If the good is of some durability, then unsold production may be carried over from one production period to another one implying a dynamic situation. Theoretically, the first case has been studied by Maskin (1986) and Friedman (1988) and the second by Judd (1990).

Experimentally, price-quantity competition with advance production of a perishable good has been studied by Mestelman and Welland (1988). They investigate the effects of advance production in posted price and double auction markets with the kind of demand and supply functions with multiple steps that are standard in experimental economics. They compare the performance of the posted price institution with and without advance production and find that for the first case price and efficiency levels are both somewhat lower than in the second case. However, even with advance production, after 15 trading rounds both prices and the distribution of the surplus are very close to the ones corresponding to the Walrasian outcome. Mestelman and Welland (1991) study the case with advance production and inventory carryover and find similar results as for a perishable good.

In this paper we study the performance of experimental markets with posted prices and advance production of a perishable good with simple demand and cost schedules. The simulated demand has a "box-shape", i.e. it is willing to buy a constant maximum quantity for any price up to a maximum. Firms have identical constant marginal costs and a capacity limit. As shown below, this simplified structure facilitates the theoretical analysis and the comparison to previous results with other market rules.

We have several aims. First, we want to find out whether price-quantity competition behaves more like pure quantity or more like pure price competition. In our set-up these two types of interaction lead to very different predictions and existing experimental results also exhibit very different behavioral patterns. Bertrand and Cournot are often used as benchmarks in the context of the analysis of oligopoly. Our results provide an experimental benchmark for price-quantity competition.

Second, we want to study the impact of the number of firms on market outcomes, specifically on price and efficiency levels and on the distribution of the surplus between consumers and producers. For that purpose we compare experimental markets with two and three firms.

Third, we also study the evolution of behavior over time to shed light on how outcomes emerge as the result of the interaction process. In our experiments subjects interact with the same market throughout 50 rounds. This reflects the repeated interaction that takes place in actual oligopoly markets. It is also the way in which most, although not all, market experiments are conducted. We study how players adjust to others' behavior over time and bring about the observed data patterns.

Our experiment is meant to be a contribution to a more general view of how imperfect competition over time relates to the equilibria of certain static games. Theoretical studies of dynamic oligopoly like those of Maskin and Tirole (1987, 1988) and Jun and Vives (1999) typically characterize equilibrium behavior in relation to the static Cournot and Bertrand equilibria. Our results will shed - from a different perspective - some additional light on the comparison between dynamic behavior and static predictions.

With our work we also wish to contribute to a more complete view of the impact of the number of firms on market performance in experimental imperfect competition environments. This issue is one of the central themes of the economic analysis of oligopoly and can be seen as transversal with respect to different specific oligopoly models. It has recently been analyzed in a number of experimental studies with different types of imperfect competition. Dufwenberg and Gneezy (2000) address this question for the case of Bertrand price competition among identical firms with constant marginal costs and inelastic demand. Their results are that prices are above marginal cost for the case of two firms but equal to that cost for three and four firms.

Abbink and Brandts (2002a) examine the effects of the number of firms in a price competition environment in which firms operate under decreasing returns to scale and have to serve the whole market; there are multiple equilibria with positive price-cost margins. The most frequently observed market price is invariant to the number of firms. However, average prices do decrease somewhat with the number of firms, due to the declining prevalence of collusion. Abbink and Brandts (2002b) study price competition under constant but uncertain marginal costs. In accordance with the theoretical prediction for this case, market prices decrease significantly with the number of firms but stay above marginal costs.

Numerous studies report experimental results on related issues from quantity competition environments. Huck, Normann and Oechssler (1999, 2001) provide results and a recent survey of work on the effects of market concentration under repeated quantity competition. Their conclusion is that duopolists sometimes manage to collude, but that in markets with more than three firms collusion is difficult. With exactly three firms, Offerman, Potters, and Sonnemans (2002) observe that market outcomes depend on the information environment: Firms collude when they are provided with information on individual quantities, but not individual profits. In many instances, total average output exceeds the Nash prediction and furthermore, these deviations are increasing in the number of firms. The price-cost margins found in experimental repeated quantity competition are, hence, qualitatively consistent with the Cournot prediction for the static game. The study by Brandts, Pezanis-Christou and Schram (2003) includes evidence that shows that, under supply function competition, an increase in the number of firms leads to lower prices.

The study of how adjustment over time takes place under imperfect competition is important, because it may give - as shown for instance in Selten, Mitzkewitz and Uhlich (1997) - insights into the rationale behind subjects' behavior. Our data exhibit a considerable adjustment stage of about fifteen rounds, during which price and efficiency levels increase. During this stage we also observe fights for the market, some of them leading to bankruptcies. With enough experience we observe considerable tacit collusion at the demand's reservation price, somewhat more so for the case of two than for three firms. In this sense, behavior tends more to what one should expect under quantity competition than to what price competition would yield. Behavior settles down at a price-quantity configuration which is not an equilibrium.

All this is somewhat reminiscent of the view proposed by Chamberlin (1962) for the case of markets in which firms face each other repeatedly. He thought that for the case of few sellers behavior follows from the very structure of the industry. In Chamberlin (1962), p. 48, he states: "If each one [seller] seeks its maximum profit rationally and intelligently, he will realize that when there are two or a few sellers his own move has a considerable effect upon his competitors, and that this makes it idle to suppose that they will accept without retaliation the losses he forces upon them. Since the results of a cut by any one is inevitably to decrease its own profits, no one will cut, and, although the sellers are entirely independent, the equilibrium result is the same as though there were a monopolistic agreement between them".In the process of fighting that we observe in our data, firms realize how disadvantageous this behavior is and learn to avoid it.

In Section 2 we discuss our basic set-up choices and present some theoretical considerations for the game we study. In Section 3 we present design details and explain the experimental procedures. Section 4 presents our results. There are three appendixes. Appendix A contains the instructions, Appendix B includes Overall Tables and Appendix C contains graphs for all experimental markets in both treatments.

3.2 Basic Set-up and Theoretical Considerations

In our game, the demand is willing to buy any amount of the good up to a quantity of $q_{\max}$ at a constant maximum price of 100. This kind of 'box' demand schedule has previously been used for the study of double auctions by Holt, Langan and Villamil (1986) and more recently by Dufwenberg and Gneezy (2000) for the study of Bertrand competition. The buyer auction studied in Roth et al. (1991) has very similar features. This simple set-up has several advantages which will become clear below. We conducted experimental sessions with two and three firms, with $q_{\max}$ being 100 in the first case and, to allow for divisibility, 102 in the second case.

Each of the n firms has the capacity of producing integer quantities up to 100 units at a constant marginal cost of 50 with no fixed costs. Each firm can serve the whole demand at marginal cost, just as typically assumed for standard price or quantity competition.

Firms simultaneously and independently decide on production quantities and on prices between 0 and 100. Once the production decisions are made, the quantities are produced instantaneously and the corresponding costs are incurred. Each firm offers all its produced units at the same price. It is as if they attached a label with the price on each unit of output. One can think of this situation as one in which two factories produce a perishable good like, say, yogurt and send it to the supermarket at a predecided price.

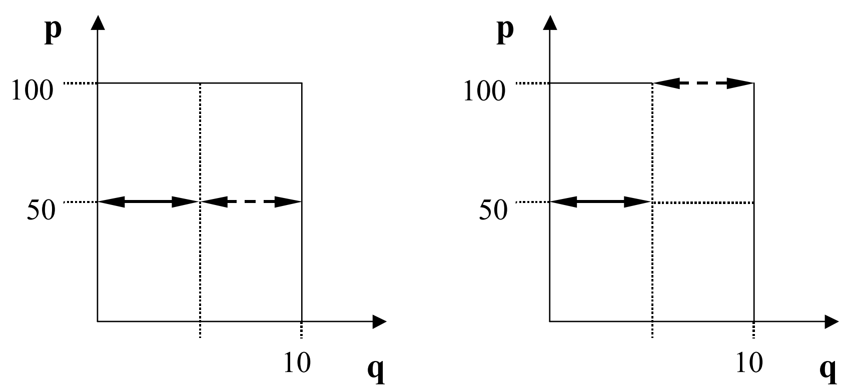
Given the shape of the demand, if total production is less or equal than $q_{\max}$ all units are sold regardless of prices. If total production is higher than $q_{\max}$, then sales will depend on the prices set by the different producers. Taking the case of three firms, then if all three prices are different from each other the demand simply goes from lower to higher prices and keeps purchasing until it reaches $q_{\max} = 100$; due to the type of demand schedule no rationing rule is needed. Some of the units of the highest price firm will remain unsold and are lost, since the good is completely perishable. There are several other possibilities in which two of the firms set the same price, which is different from the one set by the third firm. If two firms set the same price which is lower than the one of the third firm and the sum of the produced quantities of the two firms is smaller than 102, then we are, in essence, in the same situation as when all three prices are different from each other. The two firms with the lowest price both sell their whole production and some of the units of the high price firm will not be sold.

If all three prices are the same, then consumers will buy from the different firms in proportion to the produced quantities: If the three firms have produced quantities q_1, q_2 and q_3 then firm i will have sales of $s_i = \frac{q_i}{q_1+q_2+q_3} * 102$ and the rest of the units of firm i's produced units will remain unsold, $i = 1, 2, 3$.

If two firms set the same price which is lower than the third one and the joint production of the first two firms is higher than 102, then a proportionality rule applies, which is analogous to the one for the case where all three firms set the same price: If the quantities of the two low-price firms are q_1 and q_2, then the sales of firm i will be $s_i = \frac{q_i}{q_1+q_2} * 102$, $i = 1, 2$; the high-price firm sells nothing. If two firms set the same price which is higher than the one of the remaining firm, then the same proportionality rule applies as in the previous case to the quantity that still can be sold after the low-price firms has sold all its production, i.e. if q_1 and q_2 are now the quantities of the two high-price firms and q_3 corresponds to the one low-price firm, then the sales of the two first firms will be $\frac{q_i}{q_1+q_2}(102 - q_3), i = 1, 2$.

Note that in the Box Design PQ Game pure price competition would be predicted to yield prices equal to marginal cost. In fact the study by Dufwenberg and Gneezy (2000) referred to above deals precisely with the case of a box-demand. In contrast pure quantity competition would lead, in the Cournot equilibrium, to the monopoly price equal to the demand's reservation value. We are not aware of any quantity competition experimental study with this kind of demand. However, it is known - see Huck et al. (2004) - that in experimental studies of this type the stage-game Nash equilibrium is a good predictor of behavior.

In contrast, for the price-quantity competition we consider, there exists no equilibrium in pure strategies. We present the reasoning for two firms; it can be easily generalized for any number of firms greater than two.



FIGURES 1 AND 2. JUMPING-UP

Let $[(\bar{p}_1, \bar{q}_1), (\bar{p}_2, \bar{q}_2)]$ be a strategy profile and focus first on the quantity choices. Note first that if $\bar{q}_1 + \bar{q}_2 < q_{\max}$ then the strategy of any of the players is (weakly) dominated by a strategy in which the produced amount equals $q_{\max}$, i.e. at any price between 50 and 100 both firms would benefit from expanding their production until joint production reaches $q_{\max}$. If $\bar{q}_1 + \bar{q}_2 > q_{\max}$ then each of the players has an incentive to reduce production. This is so, because of the unit cost being 50, which is equal to the highest possible profit per unit. Due to the proportionality rules a reduction of production by one unit leads to a reduction in sales of less than one unit; foregone unit profits are hence less than 50 while saved costs are 50. If unit costs were zero firms would have an incentive to always throw their total capacity on the market. Our parameter choices can, hence, be expected to lead to the simple situation in which production ends up being equal to sales.

Now to prices. Observe first that if a firm's price is below 100 and is producing a positive amount then a unilateral increase in price will always be profitable (see the "jumping up" in Figures 1 and 2). This is illustrated by the contrast between Figures 1 and 2, where in Figure 1 we have chosen to represent both firms producing the same quantity at marginal cost. A unilateral increase of firms 2's price leads to the positive profit represented by the shaded area. At $p_1 = p_2 = 100$ there is no possibility of unilateral price increases, but in this case unilateral under-cutting and expansion production to $q_{\max}$ will always be profitable for at least one of the firms, since the demand will only buy from the firm with the lower price. For instance, if $s_1 = 49$ and $s_2 = 51$, then the undercutting to a price of 99 with a simultaneous production expansion to 100 will be profitable for either firm (see Figures 3 and 4).

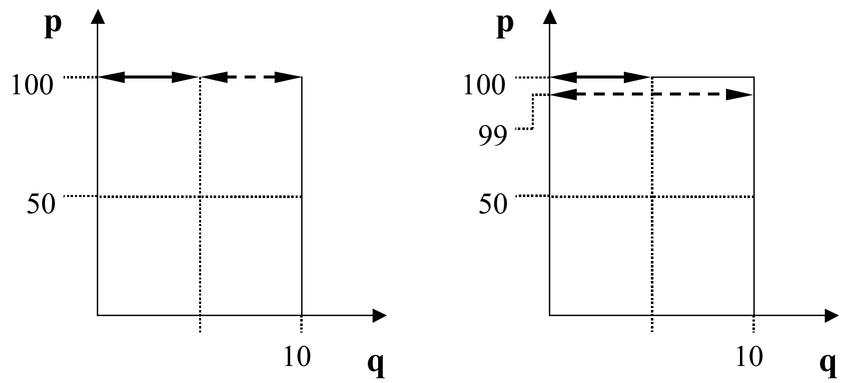
This only stops being true for firms with very high sales, the threshold being at 98 units. If a firm sells 99 or 100 units then undercutting will not be profitable, since in that case the firm is already a virtual monopolist. But, of course, in this case it is the other firm that will have a strong incentive to undercut. There is no equilibrium in pure actions.[16]

Since the game actually played in the experiments is finite, we know that there does

[16] If the market is shared 50-50 at a price of 51, there are no incentives to undercut, and at a price of 52 firms are indifferent between undercutting and expanding production or staying put.

exist an equilibrium in mixed strategies. With respect to that, note that the payoff matrix of the game has $(101 \times 101)^2$ cells, so that mixing would be over the two variables. We will not consider the mixed strategy equilibrium. Indeed, as with applications to many other real contexts, taking it in account would not be very natural in our context. In addition, even a large experiment does not generate enough data to reliably check the use of such a strategy.

Up to this point we have analyzed the one shot game. In our experiments we run 50 periods, our game is finitely repeated. Applying backwards induction we have the same lack of equilibrium in pure strategies. However some experimental results (see Selten, Mitzkewitz and Uhlich (1997)), claim that people actually behave like in an infinitely repeated game when the number of periods is large enough and the end of the game is far away.[17] If we think of our game as an infinitely repeated one, any result can be maintained in time if δ, the discount factor, is high enough. In particular cooperation, to share the market at the monopolistic price could be maintained until a few periods before the last one. In the theoretical approach the threat that maintains collusion is typically considered to be Nash reversion, but in looser, broader terms one may think of the threat being any kind of fight for the market. We will get back to this issue when we discuss the results.



Figures 3 and 4. Under-Cutting

[17] Vives (1999) discusses the possible rationalizations of cooperation in finitely repeated games.

3.3 Experimental design

We obtained data from two experimental treatments. One with two firms (hereafter, 2F) and another one with three firms (hereafter, 3F).[18] In treatment 2F each firm chooses a quantity between 0 and 100 and one price (expressed in ECUs, Experimental Currency Units) also between 0 and 100. There is a constant cost of 50 ECUs per unit produced. Treatment 3F only differs in the number of firms and in that they can choose production levels from 0 to 102.[19]

To accommodate losses we granted subjects an initial capital balance of 20,000 ECUs. If a firm lost more than this starting money it was considered bankrupted and forced to abandon the market. However, to preserve anonymity subjects that went bankrupt were asked to remain in their place until the end of the experimental session. Bankruptcies did actually occur in both our treatments so that monopolies appeared in 2F and doupolies and monopolies appeared in 3F. We will elaborate on this in the experimental results section.

As mentioned above fixed markets of subjects interacted in the same market during 50 rounds to represent the repeated nature of oligopolistic interaction. We conducted 14 markets of the 2F treatment and 9 markets of the 3F treatment. Below we consider each separate market to be one independent observation.

We ran all the experiments in the "LeeX" (Laboratori d'Economia Experimental) at Universitat Pompeu Fabra in Barcelona during the second half of the year 2002. The experiments were programmed using Urs Fischbacher's zTree toolbox. The total earnings of a subject from participating in this experiment were equal to his capital balance plus the sum of all the profits he made during the experiment minus the sum of his losses. We paid to each subject 2 EUR as a show-up fee and their profits at the rate of 2 cents of Euro per 100 ECU earned. Experiments lasted approximately one hour and a half. Average earnings in the experiment were 16.5 EUR.

[18] Appendix A contains instructions for the case of three firms.

[19] Because 102 divided by 3 equals 34, an integer.

3.4 Experimental results

Figure 5 shows for both treatments the evolution of average weighted market prices, defined as the average of prices at which units have been sold weighted by their respective market shares[20]. These averages include prices from all markets, among them those in which firms went bankrupt. Below we will distinguish between behavior in markets with and without bankruptcies. Observe first that for both the cases of two and of three firms average weighted prices are evidently much closer to the quantity competition Cournot equilibrium than to the price competition Bertrand equilibrium. Prices actually appear to tend to the highest possible one. Stage-game equilibrium analysis does not suggest this, but - after the fact - it may appear quite plausible that a reduced number of firms is able to establish high prices in a situation of (albeit, finitely) repeated interaction. Below we elaborate on how these prices come about.

Note first that the prices shown in Figure 5 exhibit upward trends; for n=2 prices stabilize after fifteen periods whereas for n=3 the trend appears to continue for a longer interval. Prices are about 50% higher in final rounds than in early ones. one can see that experiments with fewer rounds would have given an inappropriate impression of behavior for this kind of interaction.

[20] For tehcnical reasons, we had to end one of the sessions of the 2F treatment in round 47. For thsi reason we only show the average wighted price up to that round.

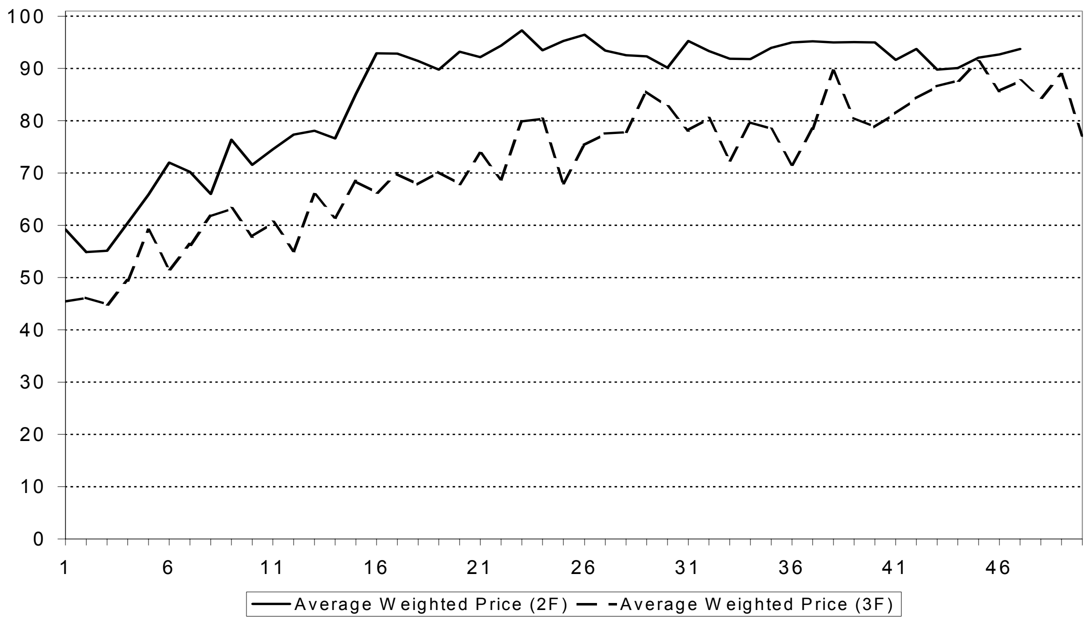


Figure 5. Average Weighted Price series, 2F and 3F

We now move to the comparison of behavior in 2F and 3F. We first describe the data at a descriptive level and then move to the presentation of statistical test results. Observe that prices for n=2 are always above those for n=3; for this last case we observe that prices go down a little in the last 3-4 rounds. This is the so-called end effect that has been observed before (see, for example, Selten and Stoecker (1986)) and is intuitively plausible: Firms behave less cooperatively when the end of the experiment comes near.

Figure 6 shows the evolution of average total quantities over time for both treatments. Recall that any quantity beyond 100 in 2F and 102 in 3F can not be sold and is a pure loss. The Figure exhibits somewhat higher quantity levels for about 15 rounds; from then on total quantity fluctuates somewhat above 100 (102).[21] In this case visual inspection does not directly suggest a treatment difference.

[21] See the previous footnote for why the data for the 2F treatment extend only to round 47..

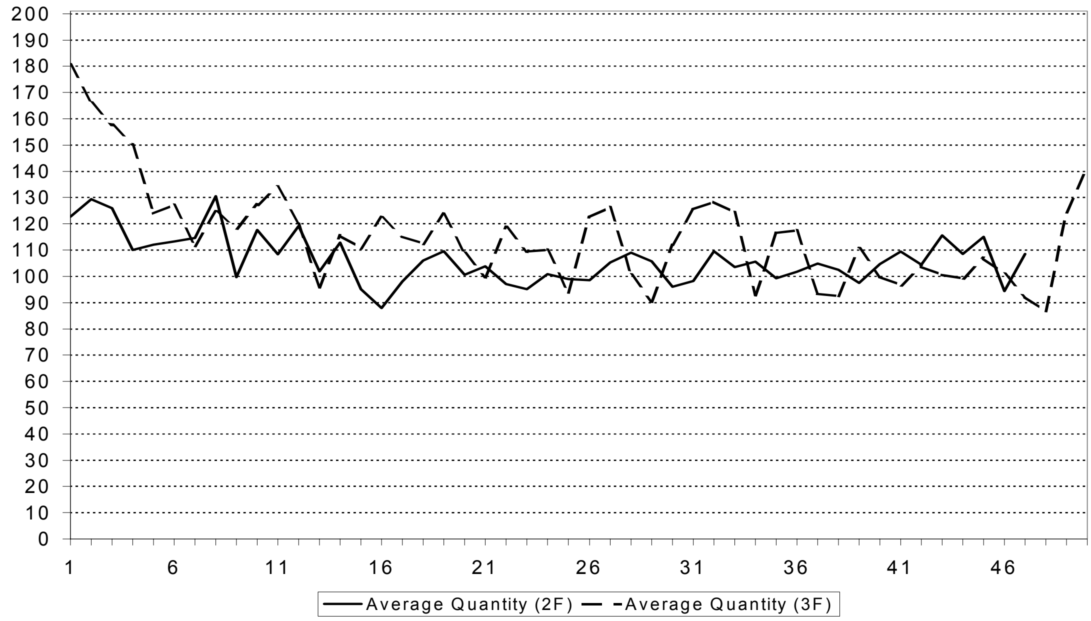


Figure 6. Average Quantity series, 2F and 3F

Figure 7 shows the evolution of average efficiency levels over time. The highest possible surplus is 5000 ECU's, which corresponds to the case where total production is equal to 100%. Efficiency is defined as the sum, in EAU's, of consumer and producer surplus as a fraction of 5000.[22] In our context inefficiency can only be the outcome of too little or too much production, with standard production inefficiency, i.e. less productive firms producing instead of more productive ones, not being possible. The data in Figure 7 suggest that efficiency for n=2 tends to be above that for n=3.

[22]Note that in the case of three firms efficiency can be negative, since high overproduction may make total costs larger than consumer surplus.

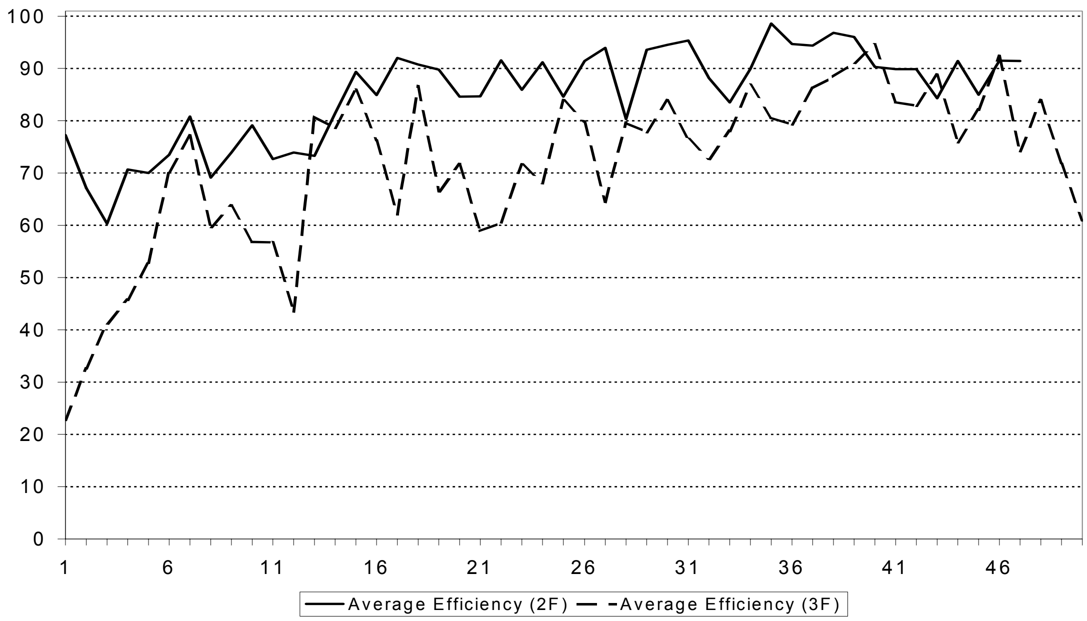


Figure 7. Average Efficiency series, 2F and 3F

In Tables 1 and 2 we show average weighted price (AWP), average quantities and average efficiencies for each market in the two treatments taking in account rounds from 21 to 45. We compute the average using these 25 rounds in order to get rid of ending and starting effects.

We ran three separate permutation tests using the data in Tables 1 and 2 comparing the averages of the variables AWP, Quantity and Efficiency. Average weighted prices and efficiency levels do vary across the two treatments significantly (p=.02 and p=.06) For Quantity we do not find any significant treatment difference.

As mentioned above, some firms did go bankrupt in our experiments. We now look at market behavior distinguishing between markets in which bankruptcies did and did not occur. In Figure 8 we now show three series of average weighted prices for the 2F treatment; we show again the overall prices series, but now also the series for those markets that turned into duopolies and those that turned into monopolies. The Figure illustrates that the Average Weighted Duopoly Price follows the Average Weighted price rather closely. Figure 9 shows the analogous comparison for quantities in the 2F treatment.

Figures 10 and 11 show the price and quantity series pertaining to the 3F treatment. In this case we observe more differences between the different data series.

Market	AWP	Quantity	Efficiency
1	100	100	100
2	87.70	126	74
3	86.72	120.04	79.96
4	100	100	100
5	100	100	100
6	100	100	100
7	100	100	100
8	100	100	100
9	100	100	100
10	74.87	85.16	63.96
11	100	100.80	99.2
12	63.04	123.04	53.36
13	99.29	95.80	95.8
14	100	100	100

Table 1. 2F Data

At this point we know that price-quantity competition leads to high prices. However, it remains to be seen more in detail how these prices emerge. To understand the process behind the regularities we have reported one needs to look at the data from the individual markets. Appendix C presents two graphs for each market In the upper graph we see the evolution of prices for firms involved in the market along time. In the lower graph we see the evolution of quantities.

At first sight one can see that, for both treatments, there is considerable variation across markets. Recall that there are 14 markets in the 2F treatment. Four of these markets resulted in monopolies, and in all these the establishment of the monopoly is preceded by a phase of heavy fighting for the market and subsequent bankruptcy by one of the firms. In 2F markets 7, 8 and 9 the monopoly emerges relatively early on, but in market 12 the fighting continues until almost the end of the session.

In 8 of the 10 markets in which the duopolists persisted until the end, collusion near the monopoly/Cournot equilibrium price was established at some point. The Figures corresponding to 2F markets 1, 4, 5, 6, 9 ,13 and 14 reveal that in 7 of these 8 markets collusion was established rather quickly. Market 2 is, in a sense, an intermediate case. In markets 3 and 10 collusion was never established. In summary, in 2F markets we observe two remarkable patterns, which both lead to high prices. One is collusion after some adaptation or fighting time (markets 1,2,4,5,6,11,13 and 14). The other is the beginning of a monopoly after one firm goes bankrupt as the result of a fight (markets 7,8,9 and 12). With respect to the markets where fighting never stopped, it seems reasonable to speculate that, with more experience, firms would end up behaving according to one of the two patterns identified above.

In nine 3F markets price patterns emerge in a similar way. In some markets some firms leave the market after fighting has led to bankruptcies. Fights lead to duopolies (markets 1, 3 and 6) and then sometimes to monopolies (markets 2 and 4). In some markets firms manage to collude after some time (markets 7,8 and 9). Since there are three firms the time required to stabilize the market is longer and therefore we find more fights until the end of the experiment (markets 1, 5 and 6). That is, the number of rounds required to arrive to the monopoly price appears to be longer in the case of three firms. This could explain the result of lower price and efficiency and higher quantity.

Market	AWP	Quantity	Efficiency
1	69.57	116.32	70.35
2	86.70	109.08	83.80
3	77.24	91.72	63.18
4	65.58	108.92	69.45
5	68.89	95.20	76.31
6	49.70	139.72	49.69
7	98.67	102	100
8	100	103.28	98.75
9	100	102	100

Table 2. 3F Data

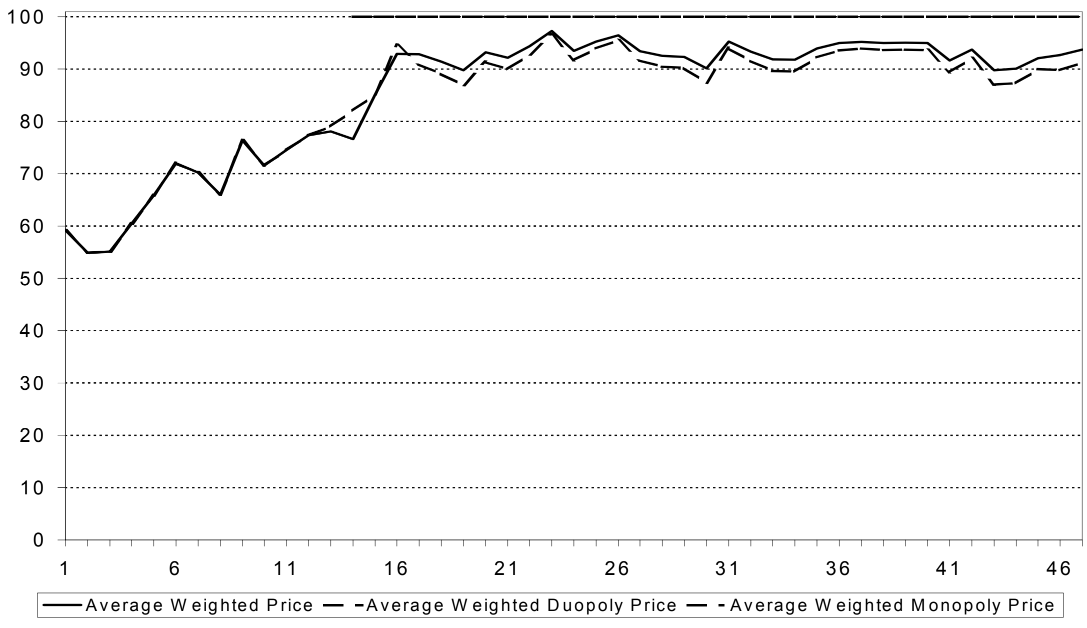


Figure 8. Price Series (2F)

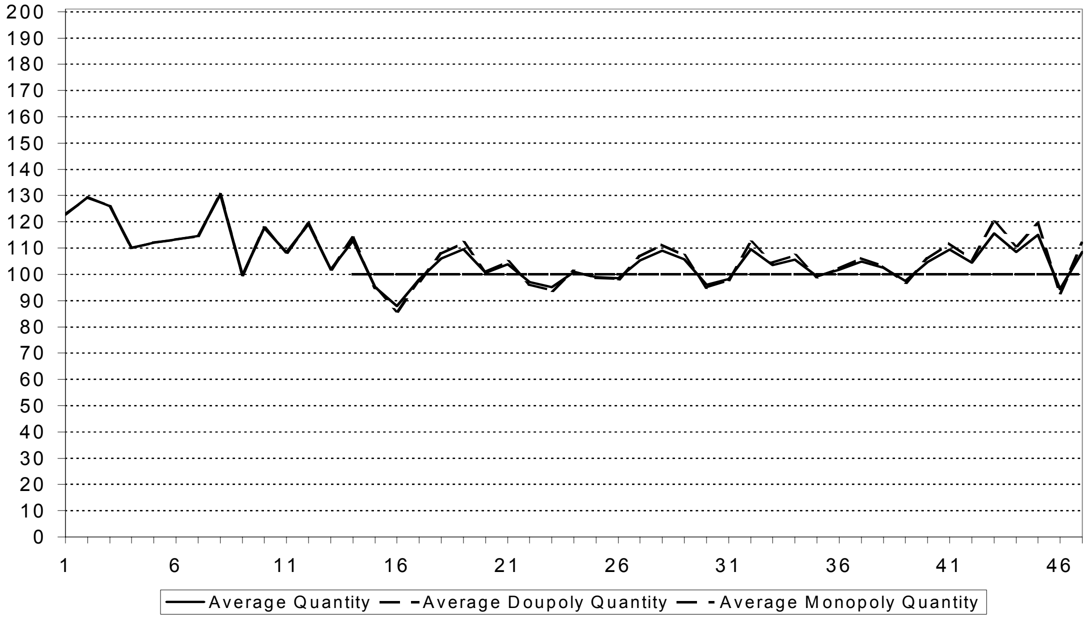


Figure 9. Quantity Series (2F)

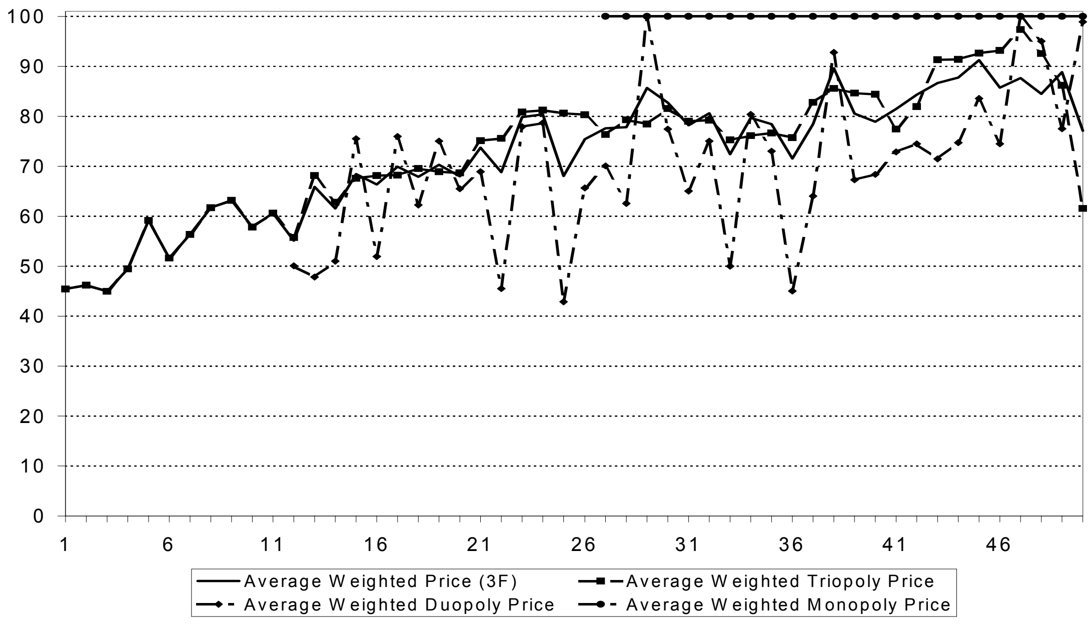


Figure 10. Price Series (3F)

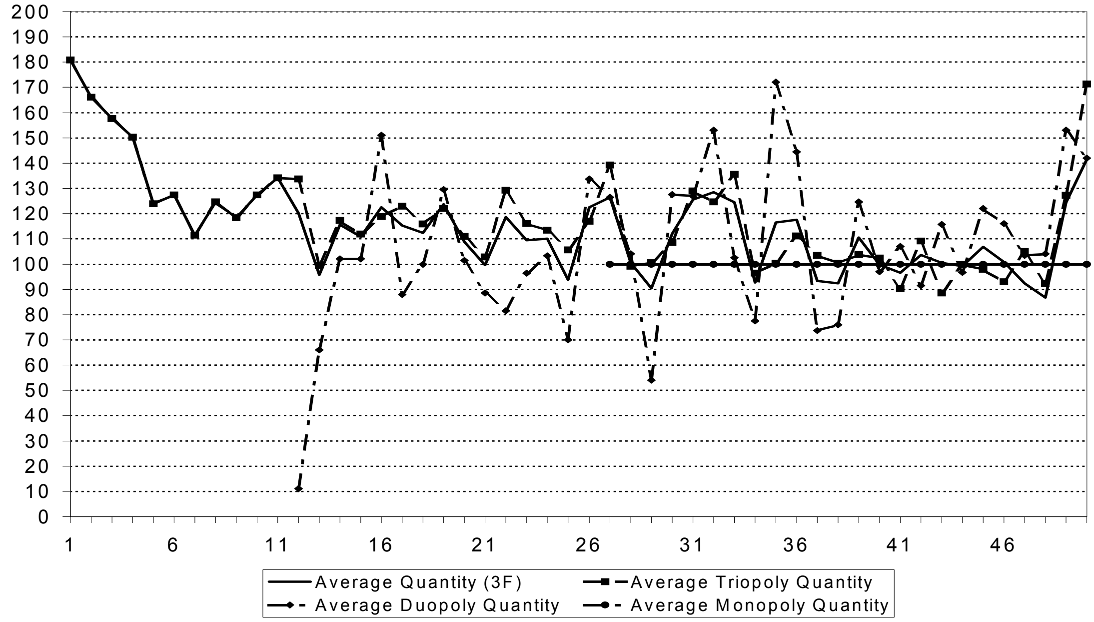


FIGURE 11. QUANTITY SERIES (3F)

3.5 Conclusions

In the standard Bertrand and Cournot models it is assumed that firms leave either price or output for an automatic mechanism to determine. In this paper we present results from experiments in which we relax this rather artificial feature head-on and allow firms to simultaneously choose prices and quantities. However, we do not see this as the "right" environment, but as one more element that will help us understand competition among small numbers of firms.

We find that the kind of price-quantity competition we study leads mostly to behavior like that of standard quantity competition. In our data, in line with the Chamberlin (1962) proposal, no outcome except that of sharing the market at the highest price appears to be stable. In the absence of pure strategy equilibria for the stage game only the highest price has any "focal drawing power".

This results can neither be exclusively attributed to the fact that firms interact repeatedly with each other nor to the type of demand function we use, since we know that pure price competition would - under the very same conditions - lead to much lower prices. Increasing the number of firms does have the effect of favoring consumers. However, this may just be a transitory phenomenon, since pricing tends to the same pattern for both treatments. We speculate that the addition of firms would lead to results in the same line. More firms could lead to more fights and more bankruptcies. This will lead to a longer adaptation phase, but with convergence to the same final price level.

Modifications of the basic market conditions may, of course, lead to different behavior. Our impression is that increasing marginal costs would not substantially alter results. A downward sloping demand function could perhaps have the effect of making collusion more difficult, but with few firms we still expect firms to reach such a stable situation.

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3.7 Appendix A: Instructions

This is an experiment about economic decision-making. The experiment is divided into periods. Each of you will have the role of a firm. In each period firms have to decide which quantity to produce and at which price to sell. To make your decision you should take into account that:

1) You can produce any integer quantity between 0 and 102.

2) You can set any integer price between 0 and 100 EAU (experimental account units).

3) The production of each unit costs you 50 EAU's, whether you sell it or not.

4) You are not the only firm offering products, specifically there will be three firms offering products in each market.

5) At the beginning of the experiment it will be randomly determined which firms will be in which market.

6) In each market the three firms will be the same period after period.

7) Consumers (simulated by computer) will always by 102 units.

8) If the firms set three different prices, consumers will always start buying the cheaper units,

a. if the firm with the lowest price has produced less than 102 units then consumers will buy the rest of the units up to 102 from the firm that has set the second lowest price

b. if the quantity produced by the two firms that have set lower prices is less than 102 then consumers will buy the rest of the units, up to 102, from the third firm.

c. if, in contrast, the firm with the lowest price has produced 102 units then consumers will buy the 102 units from him and the firms with the higher prices will sell 0 units.

9) if the three firms set the same price and the sum of the quantities produced by the three firms is smaller than or equal to 102 then the three firms will sell all produced units.

10) if, in contrast, the three firms set the same price and the sum of the quantities produced is larger than 102 then consumers will buy in proportion to the quantities produced; i.e., if one firms has produced X units, a second firm has produced Y units and the third firm has produced Z units, then the first firm will sell $\frac{X}{X+Y+Z} * 102$ units, the second firm will sell $\frac{Y}{X+Y+Z} * 102$ units and the third firm will sell $\frac{Z}{X+Y+Z} * 102$ and the rest of the units will not be sold.

11) if two firms set the same price, this price is the lowest and the quantity produced by these

two firms is larger than 102, then consumers will buy in proportion to the quantity produced, i.e. if one firm has produced X units and the other firm has produced Y units then the first firm will sell $\frac{X}{X+Y} * 102$ units, the second firm will sell $\frac{Y}{X+Y} * 102$ units and the rest of the units will not be sold.

12) if two firms set the same price and it is not the lowest the same proportional rule than under 11) will be applied subject to the remaining quantity up to 102; i.e. if the third firm produces Z at the lowest price the quantities sold will be $\frac{X}{X+Y} * (102 - Z)$ and $\frac{Y}{X+Y} * (102 - Z)$.

13) if your firms makes losses it will be bankrupt, this means that the program will automatically set a price and a quantity equal to zero for all remaining rounds, the other firms will continue to be able freely set prices and quantities.

14) even if you are the owner of a bankrupted firm you will have to remain in your seat until the end of the experiment to preserve anonymity.

On your computer you will see 5 screens:

1) The input screen where you will have to type your price and quantity in the corresponding spaces.

2) The results screen will inform you about the prices and quantities of the other firms, as well as about your earnings in the period and your accumulated earnings. You can press the button "OK" after you have read it and this screen will disappear in 20 seconds.

3) The history screen will inform you about prices, quantities and earnings of all previous periods.

4) The waiting screen will appear whenever you have to wait till everybody is finished.

5) The total earnings screen will appear at the end of the experiment and will inform you of your total earnings in Euros.

You will not be allowed to communicate with each other during the experiment. If you have any doubt about the instructions you may now ask publicly. If you have a question or doubt during the experiment, raise your hand and we will talk to you personally.

The experiment has 50 periods You start with 20000 EAU's which will always appear added to your total earnings. You will be paid 2 cents of a Euro for each 100 EAU's plus the 3 Euros that you will already have received for your participation.

3.8 Appendix B: Overall Tables

Prices	M1		M2		M3		M4		M5	
	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	70.00	27.39	53.00	9.75	33.20	17.43	81.14	18.06	60.80	21.91
6 to 10	69.90	26.57	56.67	20.95	81.50	8.63	100.00	0.00	100.00	0.00
11 to 15	78.68	25.52	77.00	11.51	73.10	11.45	100.00	0.00	100.00	0.00
16 to 20	100.00	0.00	90.50	9.75	75.16	4.69	100.00	0.00	100.00	0.00
21 to 25	100.00	0.00	82.00	10.95	83.70	4.13	100.00	0.00	100.00	0.00
26 to 30	100.00	0.00	76.00	8.94	84.40	12.60	100.00	0.00	100.00	0.00
31 to 35	100.00	0.00	81.00	12.45	90.00	4.85	100.00	0.00	100.00	0.00
36 to 40	100.00	0.00	100.00	0.00	96.60	2.16	100.00	0.00	100.00	0.00
41 to 45	100.00	0.00	100.00	0.00	63.40	14.42	100.00	0.00	100.00	0.00
46 to 50	100.00	0.00	100.00	0.00	57.50	10.61	100.00	0.00	100.00	0.00

Quantities	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	124.00	41.14	112.00	55.41	160.00	22.36	92.60	17.11	80.40	33.28
6 to 10	160.00	22.36	108.00	38.34	112.00	21.68	100.00	0.00	100.00	0.00
11 to 15	94.20	37.53	100.00	0.00	137.40	25.67	100.00	0.00	100.00	0.00
16 to 20	100.00	0.00	121.00	28.81	122.00	0.71	100.00	0.00	100.00	0.00
21 to 25	100.00	0.00	158.00	42.66	110.20	8.23	100.00	0.00	100.00	0.00
26 to 30	100.00	0.00	122.00	25.88	120.00	27.39	100.00	0.00	100.00	0.00
31 to 35	100.00	0.00	150.00	50.00	100.00	0.00	100.00	0.00	100.00	0.00
36 to 40	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00
41 to 45	100.00	0.00	100.00	0.00	170.00	27.39	100.00	0.00	100.00	0.00
46 to 50	100.00	0.00	100.00	0.00	110.00	14.14	100.00	0.00	100.00	0.00

Prices	M6		M7		M8		M9		M10	
	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	85.96	15.28	55.20	11.08	49.82	9.35	53.02	6.50	53.36	5.60
6 to 10	100.00	0.00	50.08	0.17	58.47	24.30	43.40	6.54	51.00	11.40
11 to 15	100.00	0.00	62.00	21.68	60.00	30.16	59.79	4.35	41.26	12.35
16 to 20	100.00	0.00	100.00	0.00	100.00	0.00	92.00	17.89	67.14	25.33
21 to 25	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	81.00	19.95
26 to 30	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	70.47	24.44
31 to 35	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	75.44	11.44
36 to 40	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	81.95	3.48
41 to 45	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	65.47	19.63
46 to 50	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	47.50	0.71
			m		m		m			

Quantities	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	102.00	2.83	160.00	41.83	154.00	45.06	166.00	13.87	108.00	16.05
6 to 10	100.00	0.00	136.60	60.22	126.00	76.03	166.00	46.69	119.00	5.48
11 to 15	100.00	0.00	100.40	99.50	112.00	57.62	125.80	49.43	143.20	52.17
16 to 20	100.00	0.00	90.00	22.36	100.00	0.00	100.00	0.00	78.40	83.70
21 to 25	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	45.00	45.96
26 to 30	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	82.00	41.02
31 to 35	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	90.80	30.22
36 to 40	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	90.00	28.28
41 to 45	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	118.00	37.01
46 to 50	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00	160.00	56.57
			m		m		m			

2F Overall Tables (1)

Prices	M11		M12		M13		M14	
	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	57.36	4.93	49.76	3.26	62.71	7.08	62.20	15.18
6 to 10	63.60	12.72	34.24	27.95	88.73	2.89	100.00	0.00
11 to 15	97.60	5.37	62.08	11.41	95.32	2.12	90.00	22.36
16 to 20	100.00	0.00	66.28	15.77	97.79	0.24	100.00	0.00
21 to 25	100.00	0.00	78.20	19.81	98.63	0.29	100.00	0.00
26 to 30	100.00	0.00	71.85	25.29	99.16	0.29	100.00	0.00
31 to 35	100.00	0.00	59.76	7.99	99.47	0.00	100.00	0.00
36 to 40	100.00	0.00	53.00	2.39	99.47	0.00	100.00	0.00
41 to 45	100.00	0.00	52.41	4.27	99.69	0.28	100.00	0.00
46 to 50	100.00	0.00	100.00	0.00	100.00	0.00	100.00	0.00

m

Quantities								
	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	114.00	11.40	115.20	34.32	92.20	11.26	100.00	0.00
6 to 10	100.00	0.00	87.00	37.78	97.40	1.34	100.00	0.00
11 to 15	102.00	4.47	84.00	23.82	95.80	1.10	110.00	22.36
16 to 20	100.00	0.00	99.60	39.09	95.00	0.00	100.00	0.00
21 to 25	100.00	0.00	80.00	39.62	95.00	0.00	100.00	0.00
26 to 30	100.00	0.00	121.80	62.14	95.00	0.00	100.00	0.00
31 to 35	100.00	0.00	109.60	66.05	95.00	0.00	100.00	0.00
36 to 40	100.00	0.00	146.20	35.35	95.00	0.00	100.00	0.00
41 to 45	104.00	8.94	157.60	42.09	99.00	2.24	100.00	0.00
46 to 50	100.00	0.00	50.50	70.00	100.00	0.00	100.00	0.00

m

2F Overall Tables (2)

Prices	M1		M2		M3		M4		M5	
	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	39.29	37.52	47.92	19.50	53.49	3.45	31.17	22.95	51.58	2.00
6 to 10	55.86	38.44	54.98	8.97	39.85	17.72	51.80	0.73	49.91	0.22
11 to 15	62.92	14.41	64.46	12.10	47.88	21.56	60.56	7.30	52.31	3.42
16 to 20	80.69	20.86	55.21	7.19	51.18	0.82	54.56	3.63	50.60	1.23
21 to 25	51.00	36.47	53.08	12.04	80.75	17.65	54.32	5.50	79.58	4.36
26 to 30	67.97	28.38	80.40	43.83	95.02	4.95	58.16	6.92	68.04	8.44
31 to 35	79.02	26.57	100.00	0.00	58.35	12.92	54.58	3.76	60.66	11.00
36 to 40	71.52	21.91	100.00	0.00	67.79	14.37	60.85	17.08	69.68	8.77
41 to 45	78.35	24.86	100.00	0.00	84.29	21.69	100.00	0.00	66.50	5.29
46 to 50	78.51	24.58	100.00	0.00	99.80	0.45	100.00	0.00	78.78	14.67
	d		m		d		m		t	

Quantities	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	169.60	110.29	133.60	38.37	159.60	81.92	184.80	47.40	175.00	12.75
6 to 10	100.60	57.06	136.60	62.07	165.40	64.16	117.00	40.47	137.80	23.49
11 to 15	132.60	27.02	135.20	58.46	89.60	57.08	81.40	31.80	104.60	13.79
16 to 20	104.00	68.47	158.00	51.45	132.20	57.56	112.60	32.00	90.20	20.75
21 to 25	110.20	73.69	139.80	65.93	60.40	24.10	115.40	46.40	63.00	30.32
26 to 30	110.60	61.82	103.60	39.66	102.00	1.00	85.20	48.44	111.40	44.28
31 to 35	122.00	44.72	102.00	0.00	130.80	56.48	119.20	56.93	116.20	37.51
36 to 40	105.80	3.83	102.00	0.00	85.20	69.92	122.80	38.11	92.20	10.62
41 to 45	133.00	41.14	102.00	0.00	80.20	44.46	102.00	0.00	93.20	34.04
46 to 50	134.40	40.17	102.00	0.00	113.00	24.67	102.00	0.00	94.60	57.03
	d		m		d		m		t	

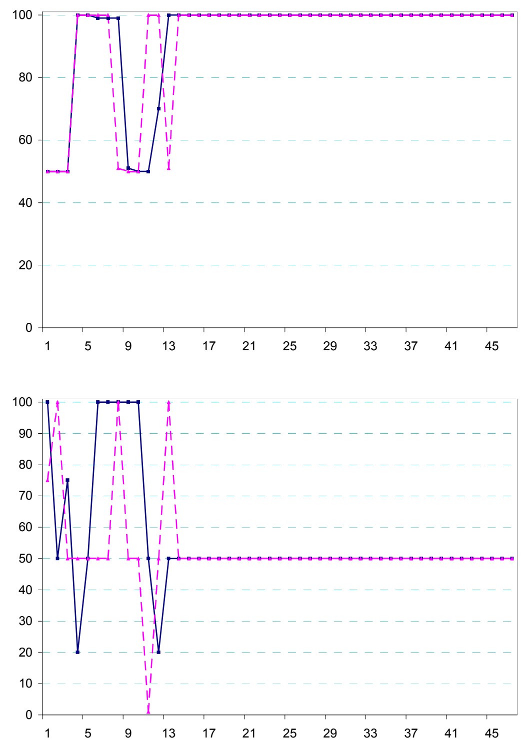
Prices	M6		M7		M8		M9	
	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	
1 to 5	61.70	2.03	51.50	12.32	55.62	8.28	49.02	11.09
6 to 10	72.19	6.44	57.55	6.00	57.87	13.42	83.33	17.95
11 to 15	58.71	4.10	54.59	5.31	69.63	14.67	89.80	22.81
16 to 20	67.04	4.43	56.98	5.75	100.00	0.00	100.00	0.00
21 to 25	55.62	3.56	93.33	9.43	100.00	0.00	100.00	0.00
26 to 30	48.88	1.66	100.00	0.00	100.00	0.00	100.00	0.00
31 to 35	48.18	2.14	100.00	0.00	100.00	0.00	100.00	0.00
36 to 40	48.44	1.77	100.00	0.00	100.00	0.00	100.00	0.00
41 to 45	47.39	19.96	100.00	0.00	100.00	0.00	100.00	0.00
46 to 50	40.00	41.83	91.84	17.69	77.98	23.98	96.00	8.94
	d		t		t		t	

Quantities	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.	Avg.	Est.Dev.
1 to 5	153.20	28.73	105.20	8.76	182.20	43.76	138.40	31.67
6 to 10	139.80	44.80	121.20	27.22	76.00	21.62	102.00	0.00
11 to 15	135.00	37.46	135.80	24.80	96.40	40.46	126.80	34.22
16 to 20	123.00	3.81	127.40	32.21	99.20	5.54	102.00	0.00
21 to 25	161.40	35.00	102.00	0.00	102.80	0.84	102.00	0.00
26 to 30	173.80	17.04	102.00	0.00	102.80	0.84	102.00	0.00
31 to 35	160.00	42.87	102.00	0.00	103.60	0.89	102.00	0.00
36 to 40	109.40	2.61	102.00	0.00	103.20	0.45	102.00	0.00
41 to 45	94.00	78.96	102.00	0.00	104.00	1.00	102.00	0.00
46 to 50	60.00	54.77	122.00	30.20	125.20	43.65	129.20	60.82
	d		t		t		t	

3F Overall Tables

3.9 Appendix C: Graphs

For every market the upper picture represents individual price series and the lower individual quantity series.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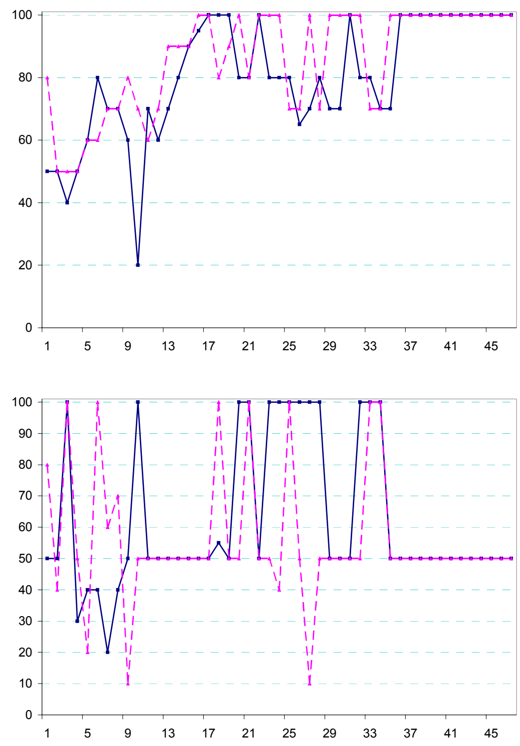
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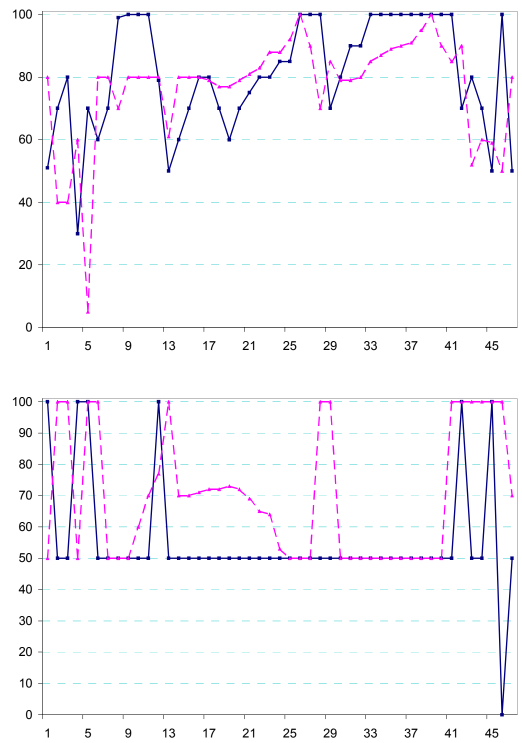
M2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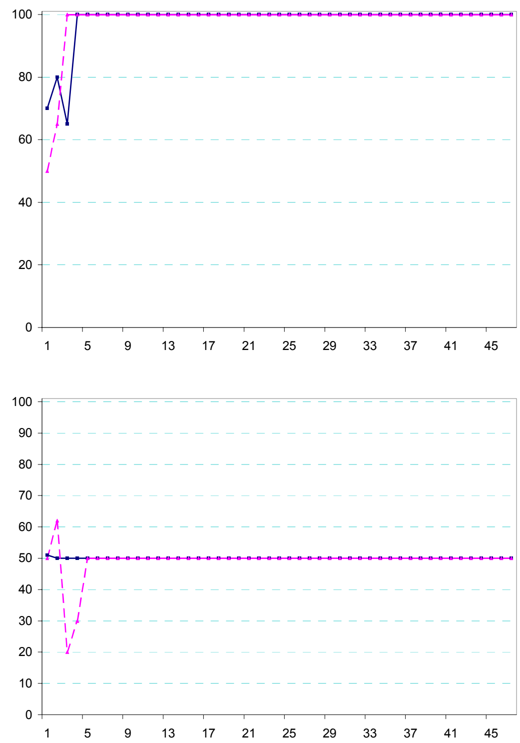
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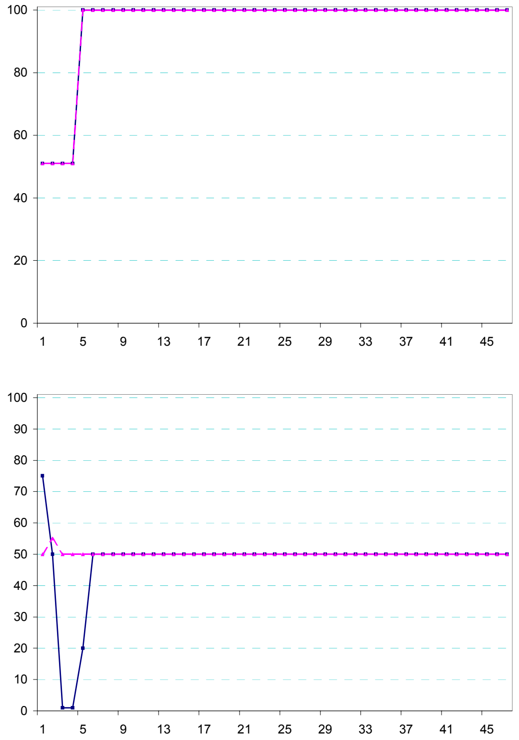
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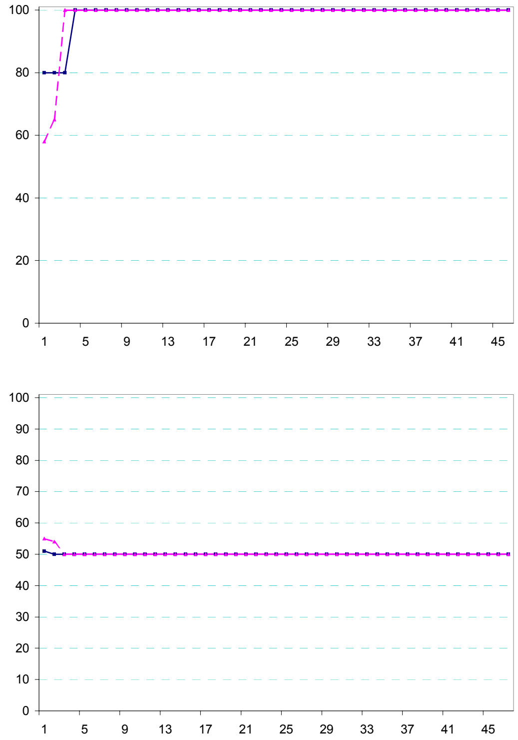
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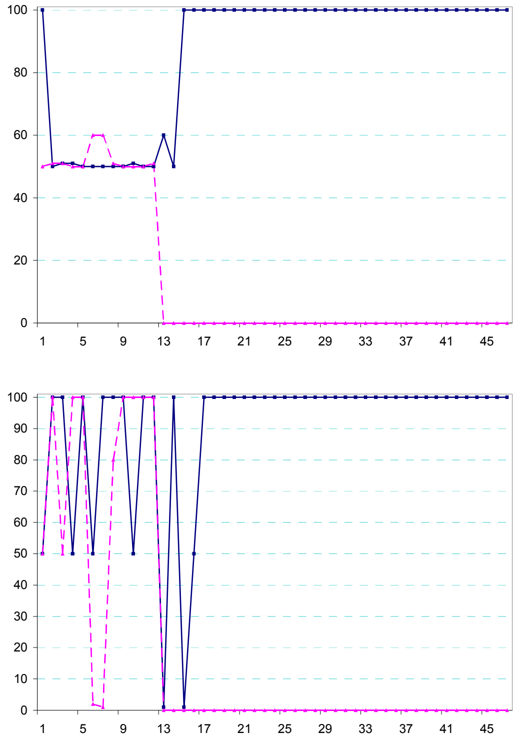
M6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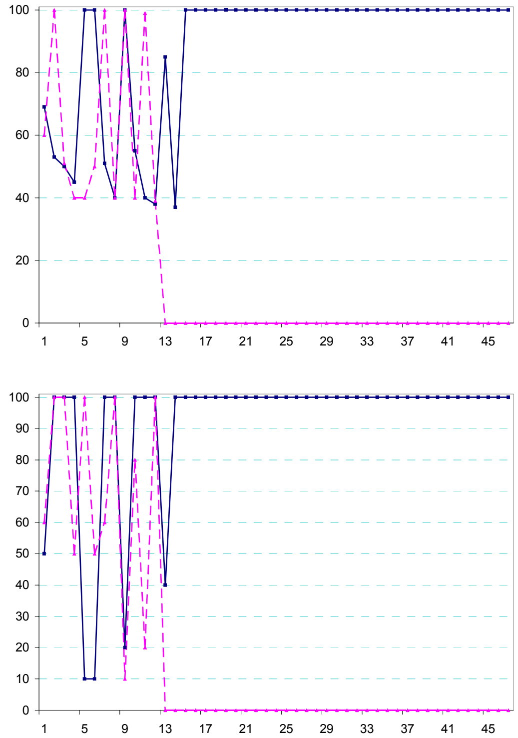
M7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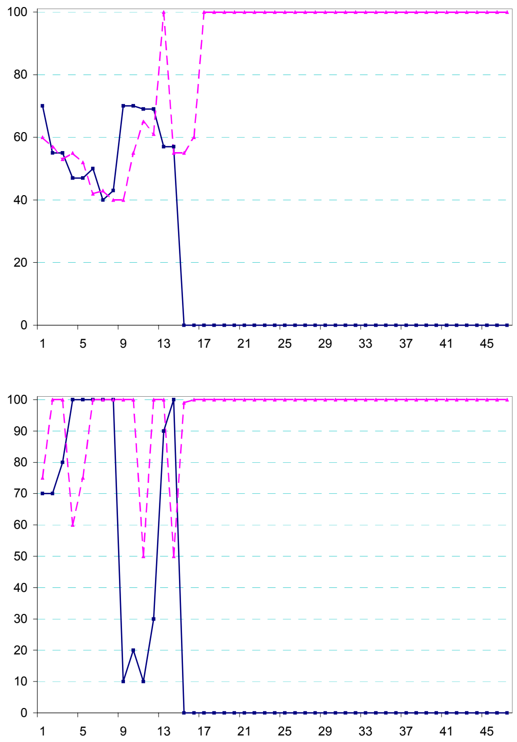
M8



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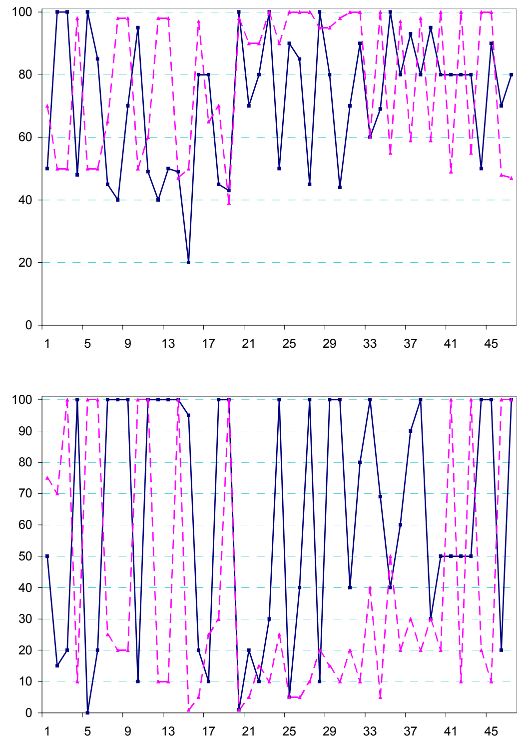

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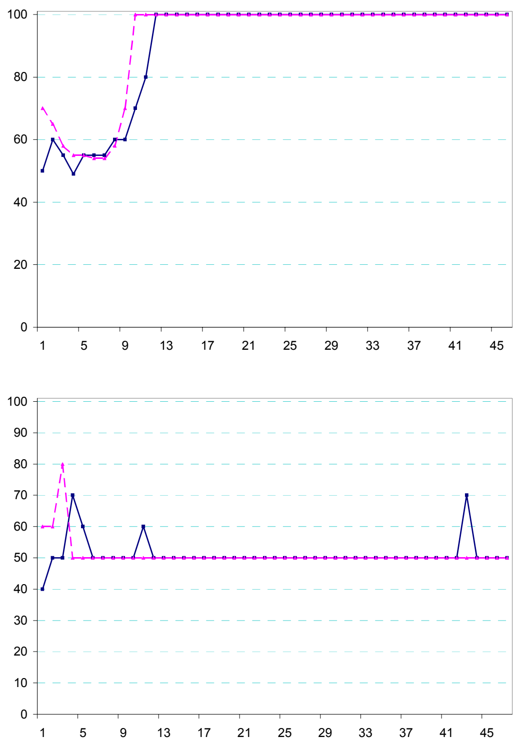

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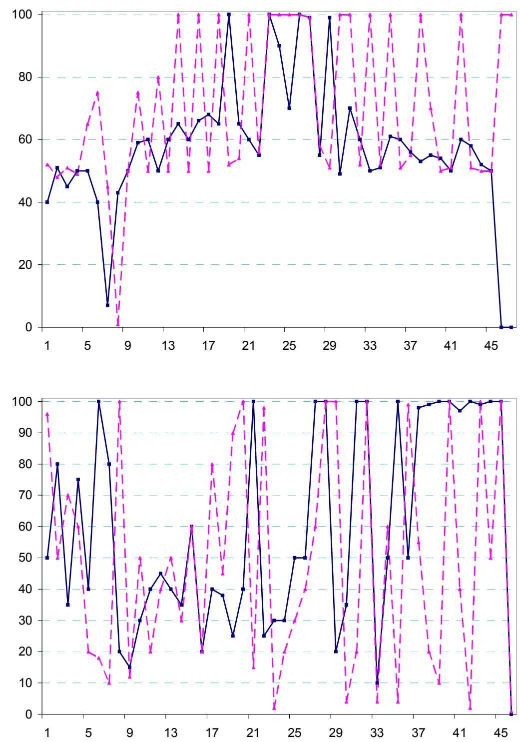

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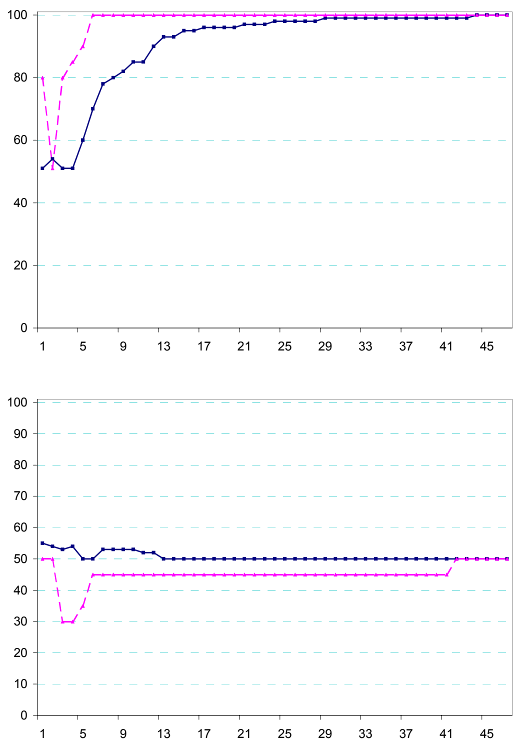

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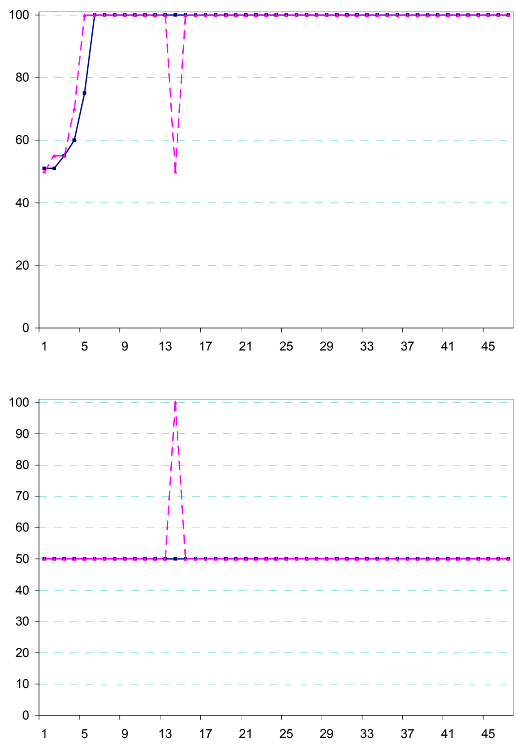

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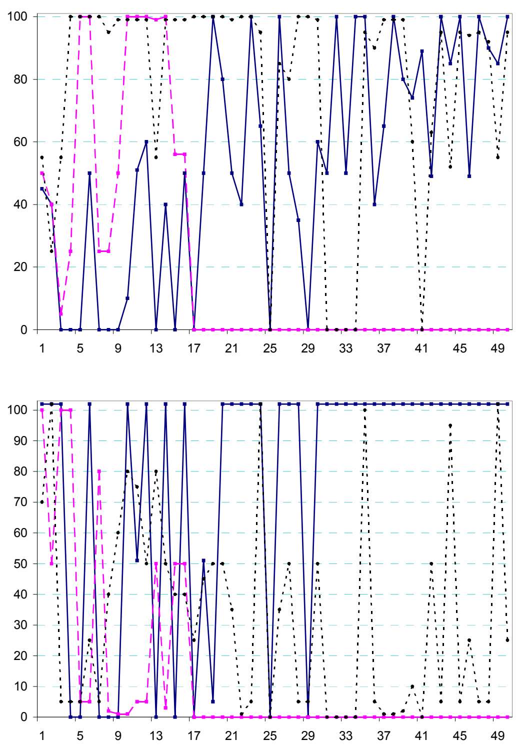

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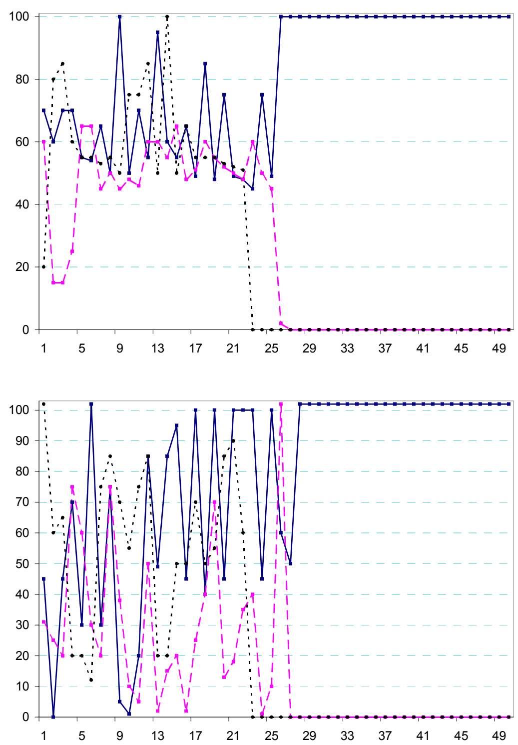

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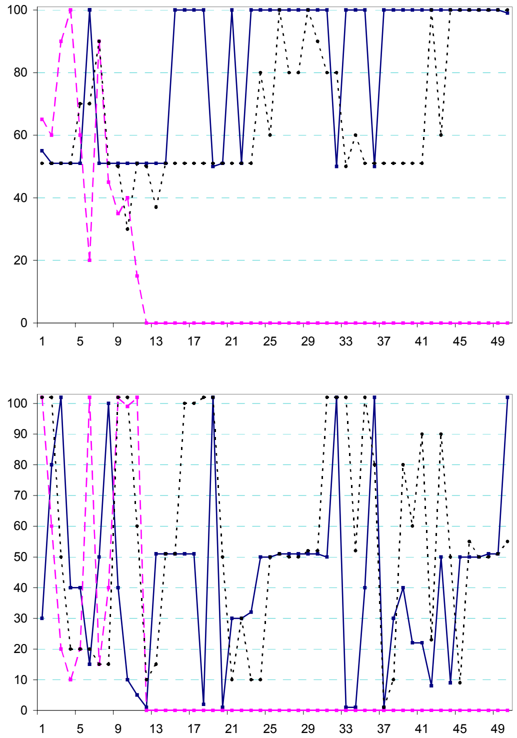
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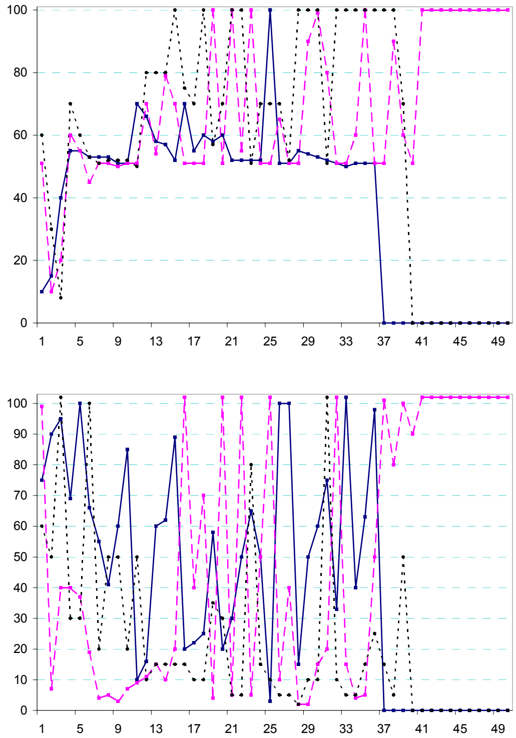
M3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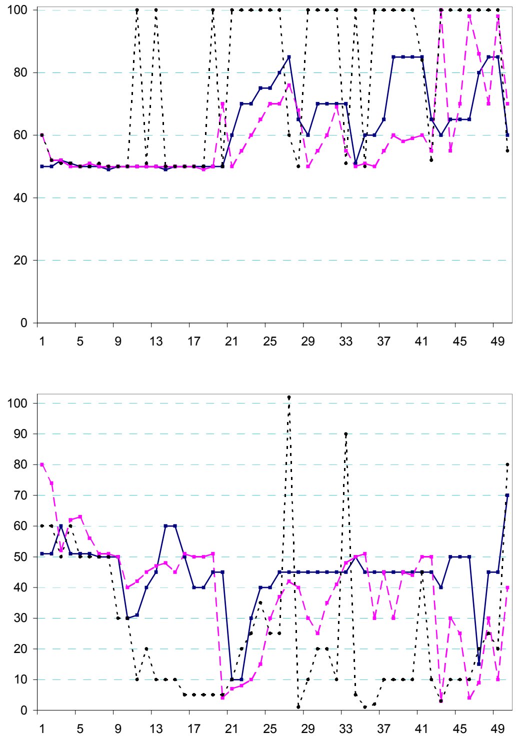
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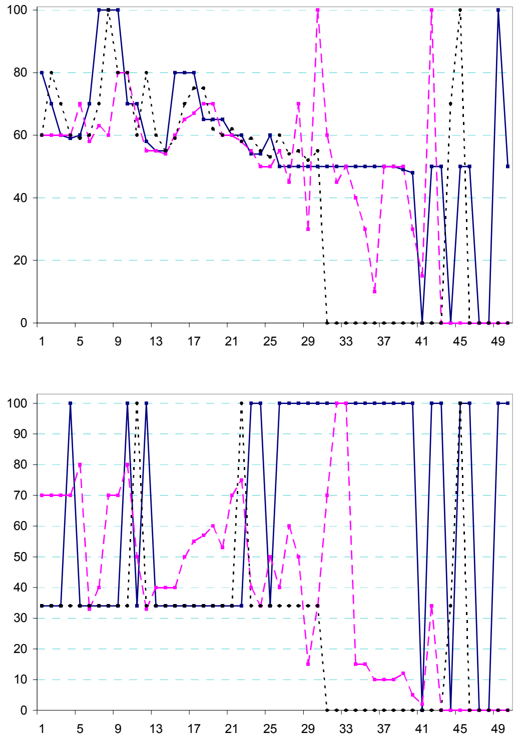
M5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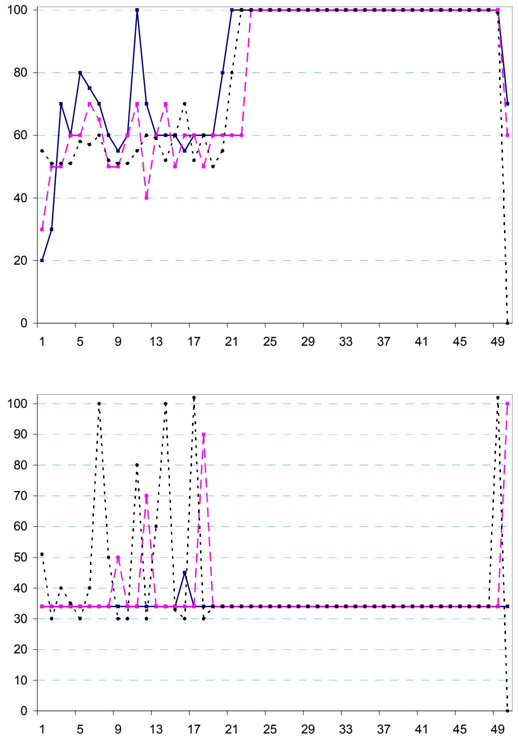
M6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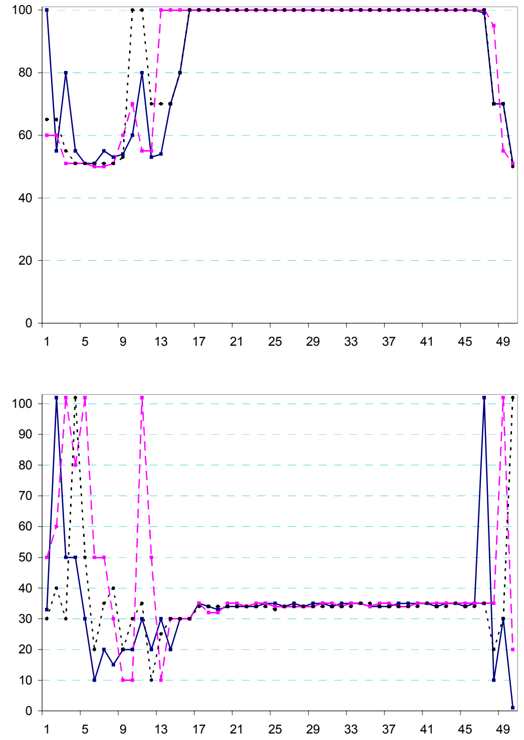
M7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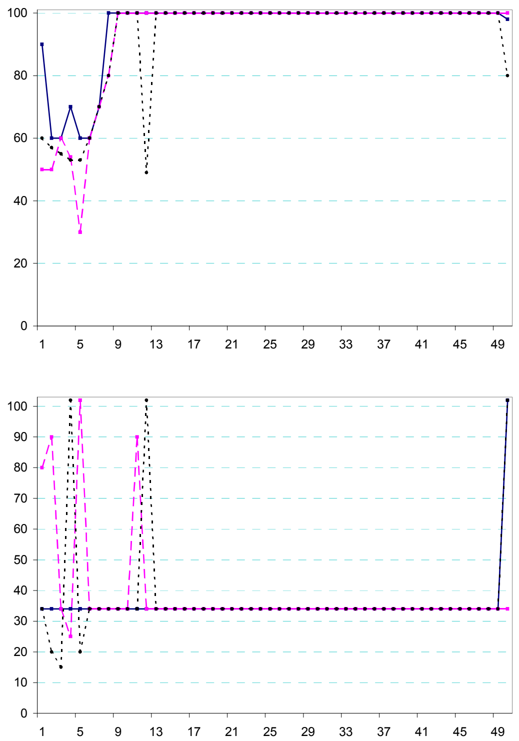
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M9

4 Price-Quantity Competition and Edgeworth Cycles

4.1 Introduction

Perfect competition theory assumes price-taking firms. They have to be very small in size and have to decide on the output that maximizes their profits given the so-called market price [see Mas-Colell et al. (1995)]. In equilibrium markets clear (supply equals demand) and price equals marginal cost. This means that firms have no chance to behave strategically. It is true that firms do decide on production. However, they do it only under technological and market price restrictions. Firms take prices to be unaffected by their own actions. It turns out that perfect competition is a good explanation for markets in which prices are formed following auction rules[23] like raw material markets or stock markets (in the very short run). However, it is not clear how prices are formed in non-centralized markets. Even if one accepts that in this case there is a high level of competition in a multi-firm market, between for instance, groceries, it is not clear at all whether and how markets clear and whether price equals marginal cost.

Oligopolistic competition theories claim that firms have the ability to decide strategically. Firms competing in an oligopolistic way may affect market results with their actions. Cournot (1838) and Bertrand (1883) started the most important traditions in oligopoly competition theory. In both Cournot and Bertrand models firms can set just one variable. However these two theories differ radically in their assumptions and results. Competition *à la* Bertrand, in price, leads to the same result as perfect competition, price equals marginal cost, while competition *à la* Cournot, in quantity, predicts less production than in perfect competition and therefore higher prices. Many economists think that price is easier to change than production (or supplied quantity), and that therefore price has to be considered as the strategic variable in the short run. However, at the same time, they also think that a result close to the Cournot prediction is more likely to reflect

[23] Indeed, Leon Walras supposed an auction mechanism to find the competitive price.

what happens in the real world. This tension has not yet been resolved. It is possible to go beyond the simple distinction between price and quantity competition. A natural development is to study situations in which firms can decide both on price and quantity. From a theoretical point of view price-quantity oligopoly competition is not easy to study. Indeed, if both variables are set simultaneously there is no equilibrium in pure strategies. Although Dasgupta & Maskin (1986) and Maskin (1986) studied the existence of equilibrium in mixed strategies for such market structures, they did not provide a solution. They just proved the existence of an equilibrium. Hence, this equilibrium cannot be used as a prediction for market behavior. Kreps & Scheinkman (1983) approached price-quantity competition using a model in which firms decide on capacities first and then compete in prices. Actually they solved the problem of inexistence of equilibrium in the so called Bertrand-Edgeworth model in which due to capacity restrictions it is not possible to find an equilibrium in pure strategies. In their two stage game firms actually first decide on capacities and then on prices. This two stage structure plus a surplus-maximizing rationing rule eventually yields Cournot outcomes [Kreps & Scheinkman (1983), Vives (1993)]. However capacity is not the same thing as supplied quantity. Firms can, and they do, supply less quantity than their capacity. This is particularly true for retail markets. There is another well known approach to price-quantity oligopoly competition, this is the Grossman (1981) and Hart (1982) supply function equilibrium model. In such a model firms decide on price-quantity portfolios under a market clearing constraint.

There is, at least, one additional problem. In the real world firms meet each other repeatedly in markets. The theories reported until now are static. It is true, however, that when there exists a unique equilibrium in pure strategies and the game is repeated a finite number of times the equilibrium for the repeated game is the repetition of the stage-game equilibrium. If the game can be considered infinite any level of collusion may be maintained if the discount rate is high enough. Theoretically all this is also true for a mixed strategy equilibrium. Nevertheless to observe a repeated mixed strategy in a real market is very difficult. It is only possible to observe the realization of such a strategy. Tthis has at least one advantage: if a steady result appears it means firms are not using any mixed strategy.

There are other solution proposals for repeated market games. For instance Chamberlin (1962) claimed that a low number of price-setting firms would not undercut each others prices because this would be very bad for all of them in the long run. *"If each one [seller] seeks its maximum profit rationally and intelligently, he will realize that when there are two or a few sellers his own move has a considerable effect upon his competitors, and that this makes it idle to suppose that they will accept without retaliation the losses he forces upon them. Since the results of a cut by any one is inevitably to decrease its own profits, no one will cut, and, although the sellers are entirely independent, the equilibrium result is the same as though there were a monopolistic agreement between them". Chamberlin (1962) p.48.* So, according, to Chamberlin firms would collude repeatedly, probably at the monopolistic price. Edgeworth (1925) thought that in a repeated Bertrand game with capacity restrictions firms would actually undercut each others prices, but that when they reached the marginal cost one of them would increase its price to the monopoly level and then the undercutting would restart. This result is based on the expectation that other sellers maintain their prices from the previous period. This so called Edgeworth price cycles theory takes into account irrational expectations, this is because a firm which knows itself it is not maintaining its price from the previous period should not think the other firms are more naive. Judd (1990) computed a solution for a dynamic model in which firms decide on prices and quantities and overproduction can be stored and sold in the next periods.

Maskin & Tirole (1988) presented a model in which two firms behave more or less like Edgeworth assumed. In this model cycles are part of a dynamic Markov equilibrium. However, firms choose prices not at the same time but alternatively. When prices are above marginal cost firms alternatively undercut each other. When prices have reached the marginal cost there is, in equilibrium, a positive probability that one firm sets the maximum price and the other follows. Then undercutting restarts again until prices converge to marginal cost and then the same process starts again.

Several economic experiments have been run in order to check competition theories.

There is a huge evidence of centralized markets, like double auctions, that work in a perfect competition way [see Holt in Kagel & Roth (1995)].

Dufwenberg and Gneezy (2000) studied the case of Bertrand price competition among identical firms with constant marginal costs and inelastic demand. Their results are that prices are above marginal cost for the case of two firms but equal to that cost for three and four firms. Abbink and Brandts (2002) examined the effects of the number of firms in a price competition environment in which firms operate under decreasing returns to scale and have to serve the whole market; there are multiple equilibria with positive price-cost margins. The most frequently observed market price is invariant to the number of firms.

Numerous studies report experimental results on related issues from quantity competition environments. Huck, Normann and Oechssler (1999, 2001) provide results and a recent survey of work on the effects of market concentration under repeated quantity competition. Their conclusion is that duopolists sometimes manage to collude, but that in markets with more than three firms collusion is difficult. With exactly three firms, Offerman, Potters, and Sonnemans (2002) observe that market outcomes depend on the information environment: Firms collude when they are provided with information on individual quantities, but not individual profits. In many instances, total average output exceeds the Nash prediction and furthermore, these deviations are increasing in the number of firms. The price-cost margins found in experimental repeated quantity competition are, hence, qualitatively consistent with the Cournot prediction for the static game. The study by Brandts, Pezanis-Christou and Schram (2003) includes evidence that shows that an increase in the number of firms leads to lower prices under supply function competition.

Indeed, there exists some experimental literature reporting evidence of Edgeworth price cycles. Cason et al. (2003) run a series of posted-price experiments in which data exhibit a significant cycle. Actually, they found a significant and slow downward trend in posted prices followed by a much faster upward reaction. Then prices started to slowly decrease again. They consider markets in which supply comes from six identical firms. Demand is simulated and depends on a size parameter and a search cost parameter. Consumers have a reservation price based on those parameters. Firms sell only if they set a price under the reservation price. Firms sell more if there are less firms setting lower prices. Note that this is a ranking rule, it does not matter how much lower is the price of the other firms. There is also some evidence of price cycles in experiments using the Bertrand-Edgeworth capacity restricted games [Kruse et al.(1994)]. They computed a theoretical Edgeworth cycle prediction based on the assumption that other firms maintain prices and compared it with actual results. Some of the markets behaved in a way resembling price cycles. However, in the other markets results were far away.

Brandts & Guillén (2003) designed an experiment in which the same two or three firms choose prices and quantities simultaneously for fifty rounds. This is close to the Maskin (1986) theoretical model. The main idea behind the design was to check how adding a new decision variable would matter. Actually most market experiment designs restrict the degrees of freedom to the ones of the model they want to check, that is, subjects can only decide on the variables that the theoretical model include. It is supposed that other variables are not important, so subjects are not allowed to change them. We acted in a different way. We actually allowed the firms to decide in prices and quantities to check how close the results are to what restricted (price or quantity) models predicted. We found that despite the inexistence of equilibrium in pure strategies prices and quantities converged clearly to those in a monopolistic situation.[24] There were two ways in which convergence to the monopolistic outcome took place. The first arose because sometimes firms competed strongly until some of them went bankruptcy. The second convergence path to the monopolistic situation was due to collusion. Actually our result resembled very much the Chamberlin (1962) hypothesis for a repeated oligopoly game: firms would collude to avoid price wars. We obtained collusion and price wars with the result of a bankruptcy. We observed similar behavior and results in experiments with two and three firms which meet repeatedly in the same market. Taking these results as a reference and starting point the question arises: What would happen in the case of much more competition?. This means having exactly the same supply and demand market structure

[24] In our design the monopolistic and Cournot solutions coincided. So the Cournot prediction explained the observed results better than the Bertrand prediction.

and the same total capacity but a number of firms as large as possible. Following a simple economic logic it seems that with many firms a collusive outcome would be much harder to obtain. It is reasonable to think that firms would compete with the result of many bankruptcies. With a low enough number of firms collusion would start.

This paper analyses experimental markets consisting of eight identical firms. This is the biggest possible market size in the lab available at Pompeu Fabra University because it is not possible to have less than two markets per session, having just one would mean a complete lack of anonymity. The number of rounds was intended to be high enough to give time for the markets converge to a steady situation.[25] There were three important features of the experimental design. First, firms decided simultaneously on price and quantity. Second, bankruptcies were allowed. This is unavoidable because firms could make losses since they produced in advance and if they set a price high enough part or all the production would remain unsold and lost. Third, when a bankruptcy happened the remaining firms inherited their capacity proportionally to their accumulated profits[26]. This is so because of the fact that one single firm was not enough to cover the whole demand. Therefore, if the capacity of a bankrupted firm disappeared the total capacity in the market decreased. This made the experiment with eight firms not possible to compare with treatments in Brandts and Guillén (2003). Actually at the beginning of the experiment each firm had a capacity equal to one quarter of the maximum amount that can be bought. That is, each firm had a capacity equal to two times the maximum demand divided by the number of firms.[27]This means the total capacity doubles the maximum demand.

No market among the eight independent observations reached a size small enough to

[25]The original design tried to get one hundred rounds. However, in the first session it became clear that this was not compatible with a reasonable experimental time.

[26]This rule has economic logic and it is still simple.

[27]This rule holds as well for the duopoly treatment in Brandts and Guillén (2003). Observe that in a duopoly this means that each firm has capacity to cover the whole market. This is not true anymore when there are more than two firms. Hence when a firm goes bankrupt its capacity must not be neglected, otherwise the nature of competition is even more strange. I have to point out that the final design of the experiment is quite complex. Nevertheless it is still no more than a stylized way to approach the real world.

produce a collusive result. The lowest number of firms in a market at the end of the experiment was five, in two cases all the eight firms survived. Moreover, most of the bankruptcies took place in the first half of the experiment. However, it happened that every market behaved in a cyclical way. That is, the quantity supplied, or offered, by firms and the average market price showed a cyclical behavior in any market. This result is general and independent of the number of surviving firms. Cycling market results are related to firm individual behavior. If prices were above the marginal cost firms produced a high quantity and tried to set a price slightly lower than the other firms prices. Nevertheless, all firms tried to do it at the same time and this created a dynamic that in several rounds made prices to be equal to the marginal cost. At this point some firm set a very high price and a very low production. The others observe it and imitate. Then, with prices over the marginal cost, another cycle started.

Cycles were observed in all markets until the last rounds. It is true that cycles were usually slower at the beginning of the experiment, so there were possible a learning process. However, it is reasonable to think that cycles could stay. There are several reasons that could make this true. For instance, there were always too many firms to make collusion possible and more bankrupts are not expected since firms are making profits, or at least they are not making losses, when they follow the cycle. Moreover, behave cyclically was the best one firm can do if the others in the same market did so. Hence, once cycles started it seems they were not to stop.

There is empirical evidence of price fluctuations in many markets. However, empirical studies focus on price and do not take into account the supplied quantity. Price wars in gas markets, for instance, have been studied for several US and Canada cities [Castanias & Johnson (1993)]. The main result of this literature is that retail prices tend to move around an almost steady wholesale price. There is also evidence of price fluctuations in airline markets [Ross (1997), Busse (2002)]. However, gas and air travels are not the same good. Gas can be stored and what is not sold today can be sold tomorrow. This may not be the case for air travels. Airline companies are very sensitive to having their capacity completely used. This means that when they compete aggressively in prices they have to do some adjustment on quantities. When a company offers a bargain of low price seats it can actually offer a bigger quantity of cheap tickets, offering other more expensive at the same time. It is also possible to think also in this kind of price-quantity competition in retail markets for perishables or fashion products. Think, for instance, of a shoe shop. Shop managers have to decide each season how many pairs of shoes to buy from the producer. This amount would be their supply. This cannot be bigger than the storage space, that would be the capacity. Finally, the shop tenders attach a label to the shoes and put them in the window. Unsold pairs during the season become out-of-fashion and the shop has to get rid of them at a residual price.

This paper consists of this Introduction, an Experimental Design section, an Experimental Results including an ARIMA Analysis subsection and a Conclusion section. There are two Appendixes, Appendix A contains the experiment's instructions and Appendix B contains pictures showing average prices, average quantity and total supply series for all the eight markets. Pictures included in Appendix B also show price and quantity decisions for both the most and the less successful firm (excluding bankrupts) in each market.

4.2 Experimental design

The design consists of one experimental treatment. There were eight symmetric firms in each market. They met each other repeatedly in the same market (partner treatment). Four sessions were run each consisting of two markets, so that eight independent observations were collected.

Firms had to choose a quantity to produce between 0 and their capacity, which was set to 25 at the beginning of the experiment. At the same time they decided about a price between 0 and 100. There was a constant cost of 50 ECU (Experimental Count Units) per unit produced. After all subjects decided their prices and quantities, they were informed about the prices and quantities set by the other subjects in the same market, about their own profits and about their own accumulated profits.

To accommodate possible losses each subject was granted an initial capital balance of

5000 ECU, that is, four times the maximum loss in one round.[28] If a firm used up the initial capital it was considered bankrupt and forced to abandon the market. Then prices and quantities of that firm were automatically set to zero for the remaining rounds. In order to preserve anonymity subjects that went bankrupt were asked to stay seated at they terminal in the laboratory until the end of the session.

There was an additional rule. To preserve the total capacity in the industry the capacity of firms that went bankrupt was shared among the surviving firms proportionally to the current accumulated profits.

Demand worked in the following way. Computer simulated consumers bought 100 units if production was equal or higher to 100, they bought the whole production otherwise. They started buying the cheapest units. In case firms produced at the same price consumers bought proportionally more from the firms that had produced more (see also the experimental instructions in Appendix A).

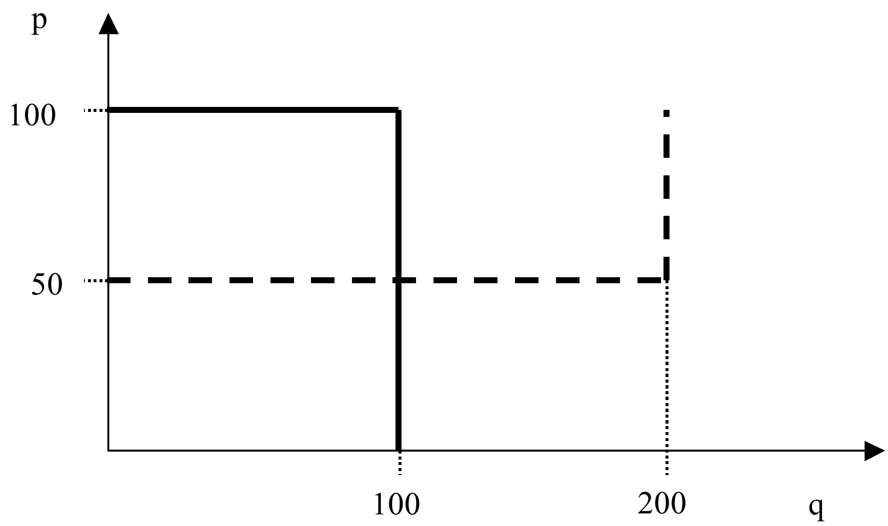

Figure 1. Supply (Dashed Line) and demand (Continuous Line) Functions

[28]This rule is the same as in the two firm (2F) treatment in Brandts & Guillén (2003).

There were several reasons to choose a box design market like the one shown in Figure 1. First, because of a comparability issue the design needed to be as close as possible to the treatments run in Brandts & Guillén (2003). However, the more substantial reason for such a choice was to make the structure clear enough for the subjects. It is well known that experiments based on a Cournot design generate noisy results [see, for instance, Huck, Normann and Oechssler (1999, 2001)]. Bertrand experiments also generate some amount of noise. Since a simultaneous price-quantity experiment is quite complicated the noise produced using a standard downard sloping demand function, in which Cournot and monopoly quantities are not obvious, may make the results very difficult to analyze and understand. That is, a box design contributes making strategic decisions much simpler, subjects may know easily what is to collude and what is not (see, for instance, the chapter about double auction markets in Kagel and Roth [1995] for a detailed discussion on box desin).

Treatment 2F in Brandts & Guillén (2003) is taken as a baseline.[29] In this design there were two firms and both of them can produce from 0 to 100 units. Any other feature is the same besides the number of rounds which was only 50. Notice that a rule stating that the capacity of the bankrupt firm would be inherited by the surviving firm was not necessary in 2F, since firms can cover the whole demand. In 2F firm capacity equals twice the demand divided by the number of firms so capacity equals the demand. In 8F the starting capacity equals twice the demand divided by the number or firms, there are eight firms so the initial capacity was set to 25.

All sessions were run at "LeeX" (Laboratori d'Economia Experimental) at Universitat Pompeu Fabra in Barcelona during the second half of the year 2003. The total earnings of a subject from participating in this experiment were equal to his capital balance plus the sum of all the profits he made during the experiment minus the sum of his losses. Each subject received 5 EUR as a show-up fee and their profits at the rate of 4 cents of Euro per 100 ECU earned. On average subjects earned about 8 EUR (including bankrupts).

[29] "2F" will refer to treatment 2F in Brandts & Guillén (2003). "8F" will refer to the current treatment with 8 firms.

Experiments lasted approximately one and a half hour.

The experiments where computerized. They were programmed using Urs Fischbacher's (2002) zTree toolbox.

4.3 Experimental results

Taking as reference the 2F results and following the simplest economic logic it was expected to find lower prices and higher quantities produced in 8F. In the treatment with just three firms there were already many more fights than in 2F. It is rather obvious that a higher number of firms makes collusion much harder to obtain. Table 1 presents, for 8F, market overall averages and standard deviations of the average price weighted by the quantity sold by each firm (Average Weighted Price, AWP) and supplied quantities (SQ) taking into account all the rounds. These simple statistics are quite uniform across markets. Table 2 presents the comparison of weighted prices and quantities between 2F and 8F. According to a Permutation (non-parametric) Test average weighted price was, as expected, lower in 8F *(p=0.0002)* and average supplied quantity higher *(p=0.0293)*. In addition, a high level of inefficiency was generated in the experimental markets. Efficiency was computed as the percentage of surplus generated related to the maximum possible surplus. Since the demand was constant for prices between 0 and 100, there were a fully efficient outcome if demand equaled supply, no matter the prices. Total Supplied Quantity (TSQ) series are represented as dashed lines in the lower pictures at the Appendix B[30] We can see there how TSQ is most of the time over or under 100, the efficient outcome. Then inefficiency appeared as a consequence of over or underproduction. Inefficiency was higher in 8F than in 2F (see Table 2). This is due to the fact that the frequent collusive or monopolistic outcomes in 2F are fully efficient.

[30] Appendix B includes three figures per market. The upper figure shows AWPs, the middle shows ASQ (Average Supplied Quantity) and the lower shows both AWP and TSQ.

	WP		SQ	
MARKET	AVERAGE	STD. DEV.	AVERAGE	STD. DEV.
M1	66.87	15.60	113.44	28.11
M2	58.36	9.36	106.42	31.05
M3	60.79	10.31	114.75	31.01
M4	61.11	11.18	110.34	33.98
M5	60.14	10.38	111.57	34.07
M6	64.51	8.52	106.68	27.31
M7	65.39	15.15	120.52	30.20
M8	76.84	16.15	130.48	36.98

TABLE 1. WP & SQ STATISTICS

	WP	SQ	EFFICIENCY
2F	85.68	106.57	85.07
8F	64.65	114.28	70.99

TABLE 2. OVERALL AVERAGES

There exists a theoretical solution proposal for the continuous stage game. It implies an mixed strategy equilibrium with a support function. This result was proved by Dasgupta & Maskin (1986) and Maskin (1986). Playing repeatedly the underlying strategies would be a solution for the studied repeated game. However, the experimental results are not compatible with such equilibrium. The repetition of a mixed strategy equilibrium with support function would imply necessarily no serial correlation, because every period outcome should be a realization of the mixed strategy which must be independent of the former ones. AWP and SQ series show quite strong and clear cycles (see Appendix B) typical of autorregresive series. Table 3 reports the first degree autocorrelation coefficients (r_1) for AWP and SQ series for all markets. Note that this coefficients are significant at

5% if they are bigger than $2/\sqrt{T}$, where T is the length of the series.[31] Since $T = 65$, then $2/\sqrt{T} = 0.248$. Therefore, AWP and SQ present serial correlation at 5%. Note also that in any case the coeficients are bigger for AWP than for SQ. This may indicate that autocorrelation is stronger in AWP series (see also the "ARIMA analysis section").

	M1	M2	M3	M4	M5	M6	M7	M8
r_1 (AWP)	0.762	0.500	0.434	0.721	0.617	0.714	0.886	0.746
r_1 (SQ)	0.583	0.311	0.366	0.510	0.455	0.567	0.713	0.545

Table 3. Autocorrelation Coefficients

A fast decrease in the number of firms followed by collusion of the remaining ones was expected. Another possible result may have been a decrease in the number of firms in markets but with no time to arrive to collusion. Production was supposed to be highly concentrated in some firms. This follows the fact that surviving firms inherited capacity from the ones that went bankrupt proportionally to their past accumulated profits. None of the to former conjectures was confirmed by data. Table 4 shows the amount of bankruptcies per market, taking into account whether they happened before or after period 33 of 65. The number of bankrupt firms is quite low and collusion was never reached in any market (see also the Appendix B).

[31] According to the Eviews 3.0 help file.

MARKET	BEFORE 33	AFTER 33
M1	3	0
M2	3	0
M3	0	2
M4	0	2
M5	3	0
M6	0	0
M7	0	2
M8	0	0

TABLE 4. NUMBER OF BANKRUPTCIES

Looking at the series plotted in the Appendix B it is quite clear that results do not converge to any stable point as it was the case in 2F. In particular they do not converge to a collusive result. Actually it is possible to differentiate two stages just looking at price and quantity plots. In every market there is a first stage in which market prices (AWPs) converge to the marginal cost, they actually stay there some time and eventually jump up quite high, see any of the upper pictures in Appendix B. All this takes from 10 to 20 rounds. This process is then repeated until the end of the experiment, however, the following cycles are faster. Cycles usually take from four to eight rounds to come back to the marginal cost. Supplied quantities (either TS or ASQ) also follow a cyclical pattern, although this is a bit less apparent in the graphs, see the ones in the middle of the page. Quantity cycles have more or less the same length as price cycles, but they are different in phase. Overall one can say that when prices increase quantities decrease and *vice versa*. More precisely however, a price maximum does not exactly coincide with a quantity minimum, the price maximum comes often a bit later, see lower pictures. Moreover quantities oscillate more randomly than prices, actually the standard deviations for quantities are always higher as a proportion of the corresponding average for quantities (see Table 1). This makes quantity cycles less clear.

Cycling behavior can be explained as follows. Downward phases of AWPs reflect individual undercutting behavior. Production increases during undercutting phases because firms offer quantities as high as possible. Once prices equal marginal cost the best firms can obtain are zero profits. Nevertheless many of them make losses due to overproduction or even because some firms set prices under marginal cost. Then one of the firms increases the price to the maximum and sets a very low quantity at the same time. Hence the other firms follow this behavior. Immediately quantity and profits start to increase. Very soon undercutting restarts and another cycle begins. The whole process took longer the first time it happened, it is not obvious that one should increase the price and decrease quantity when the market price equals marginal cost. There is, indeed, a learning process and cycles got faster. This sort of individual behavior eventually results in the market aggregate cycling behavior showed in the Appendix B and explained according to ARIMA models in the next section.

In order to give some statistical validity to the former paragraph a series of regressions were run. Regressions explaining individual behavior were done using pooled data techniques. Data for bankrupted firms were omitted from the period when the firm broke to the end. First consider the estimation of the naïve models:

$$p_{it} = \beta_1 p_{it-1} + \varepsilon_{it} \tag{1}$$

$$q_{it} = \beta_1 q_{it-1} + \varepsilon_{it} \tag{2}$$

where i denotes the subject and t the round. $\hat{\beta}_1 = 0.97$ in model (1) and $\hat{\beta}_1 = 0.85$ in (2). These indicates that individual prices and quantities vary little from one period to the next.

Now consider the following regressions run using random effects[32]:

[32] This approach has been used, for instance, in Croson et al. (2004). The random variables α_{it} of the model account for idiosincratic behavior and are uncorrelated to the white noise error terms ε_{it}.

$$p_{it} = \beta_0 + \beta_1 p_{it-1} + \beta_2 q_{it-1} + \beta_3 AWP_{t-1} + \beta_4 ASQ_{t-1} + \alpha_i + \varepsilon_{it} \tag{3}$$

$$q_{it} = \beta_0 + \beta_1 p_{it-1} + \beta_2 q_{it-1} + \beta_3 AWP_{t-1} + \beta_4 ASQ_{t-1} + \alpha_i + \varepsilon_{it} \tag{4}$$

where the lagged AWP and ASQ are referred to i's market, α_i are the individual indicator variables. Table 5 summarizes the results.

	Constant	p_{it-1}	q_{it-1}	AWP_{t-1}	ASQ_{t-1}
(3)	13.51**	0.78**	0.053*	0.10**	−0.34**
(4)	0.43	−0.010	0.84**	0.039**	0.032

Table 5. Estimation of (3) and (4)

*, $p < 1\%$

**, $p < 5\%$

^, $p < 10\%$

In general terms regressor are less significant in the estimation for quantities. This is no surprising, it reflects the fact that quantity cycles are less clear and therefore more difficult to estimate. Actually, the only important decision variables for the quantity model seem to be just the former own price and the former AWP. All the regressors are significant in (3), and all the signs are reasonable. Note that the negative sign for ASQ_{t-1} is also reasonable, together with the other regressors it reflects the logic of the undercutting behavior. q_{it-1} seems to be not very important in the adjusting process. The own lagged prices were very important in current period decision formation.

Another way to check individual behavior is to check how successful it is in terms of profits. Table 6 shows the final profits of the surviving firms per market. Table 7 shows the overall number, percentages and percentage excluding broke (-b) of firms classified into: broke, winner (if final earnings were more than the initial balance capital, 5000) or looser (if final earnings were less than 5000). Data contained in these tables indicate that most firms were able to make significant profits.

	M1	M2	M3	M4	M5	M6	M7	M8
AVERAGE	15522	6832	5107	6457	6499	9659	8386	12146
STD. DEV.	6393	2992	3914	5422	3912	4039	3903	5905

TABLE 6. PROFITS AT ROUND 65

	TOTAL	%	%(-BROKE)
BROKE	15	23	–
WINNERS	38	59	78
LOSERS	11	17	22
TOTAL	64	100	100

TABLE 7. FIRM CLASSIFICATION

The next regression explains individual profits:

$$\pi_{it} = \beta_0 + \beta_1 pd_{it} + \beta_2 qd_{it} + \beta_3 pd_{it-1} + \beta_4 qd_{it-1} + \beta_5 (pd_{it} * qd_{it}) + \beta_6 (pd_{it-1} * pd_{it-1}) + \beta_7 c_{it} + \alpha_i + \varepsilon_{it} \quad (5)$$

	Constant	pd_{it}	qd_{it}	pd_{it-1}	qd_{it-1}	$pd_{it} * qd_{it}$	$pd_{it-1} * pd_{it-1}$	c_{it}
(5)	−234.44**	−16.74**	1.78	14.01**	6.45**	−0.17*	−0.14*	9.18**

TABLE 8. TABLE 5. ESTIMATION OF (5)

Where d always means distance to a market. Then, for instance, $pd_{it} = p_{it} - AWP_{it}$ but, $pd_{it-1} = p_{it} - AWP_{it-1}$. The same for quantities. Multiplied variables take into account common effects. c_{it} is the capacity of firm i in time t. Notice that this distance is not in absolute value, then these regressors may take positive or negative values. According to this the signs of pd_{it} and pd_{it-1}coefficients have an interesting interpretation. The first one says that profits are bigger if the price of firm i is lower than the market price in period t, it means setting the lowest price is profitable. However, the positive sign of pd_{it-1} coefficient explains profits are not bigger if firms set prices lower than the market price in the former period. This may have a buffering effect on the cycle, that is, this may make cycles slower. Having a bigger capacity seems to be important to get higher profits, therefore if this capacity is actually used must be reflected in a positive sign for qd_{it-1} coefficient. It is so. Finally the common effect are both significant but not so important.

4.3.1 ARIMA analysis

A Box-Jenkins ARIMA method has been used in order to estimate the generating processes of the observed time series. The main reason to do this is to run Chow Breakpoint Tests for each series in order to identify learning processes. It is also interesting to have a tool to determine whether the series are stable.

The first step in ARIMA estimation is to decide on series stability. According to the correlograms showing ACF (autocorrelation function) and PACF (partial autocorrelation function) plots all series have to be considered time stable. In time series analysis a value in a certain moment is explained using lagged values (autorregresive process or AR) and or moving averages (MA).

Tables 9 and 10 present the parameter estimation for price and quantity processes respectively. When there is more than one estimation for one particular market it means that the identification is not clear. Typical an AR(1) process can be confused with an MA(1).[33] The parameters ρ_i and θ_j are the AR and MA estimated coefficients respectively. In any case all the coefficients are significant according to $t - tests$. Goodness of fit is much better for price series than for quantity series. The last column in tables 9 and 10explains whether there is structural change, identified as learning, according to a Chow Breakpoint Test at period 20.

[33] The first lag is signicant in both ACF and PACF functions and the other are statistically zero.

Series	ρ_1	ρ_2	θ_1	Str. Change (20)
AWP M1	0.762629	–	–	*yes*^
AWP M2	0.503780	–	–	*yes***
AWP M2	–	–	0.600224	*yes***
AWP M3	0.438260	–	–	*no*
AWP M3	–	–	0.508384	*no*
AWP M4	0.976975	−0.327857	–	*no*
AWP M5	0.848934	−0.345190	–	*yes***
AWP M6	1.025140	−0.453006	–	*yes*^
AWP M7	1.363179	−0.515308	–	*yes***
AWP M8	1.053996	−0.365677	–	*no*

Table 9. Average Weighted Price Process Estimations

There is structural change at least in three markets, at most in five markets. This means that the tests detects learning in more or less half of the markets. Although, at a first glance, it seems that there is learning in every market (see Appendix B).

Series	ρ_1	ρ_2	θ_1	θ_2	Str. Change (20)
SQ M1	0.597846	–	–	–	*yes***
SQ M2	0.311090	–	–	–	*no*
SQ M2	–	–	0.306183	–	*no*
SQ M3	0.388258	–	–	–	*yes*^
SQ M3	–	–	0.511597	–	*no*
SQ M4	0.515035	–	–	–	*no*
SQ M5	0.460321	–	–	–	*yes*^
SQ M5	0.543333	–	–	–	*yes***
SQ M6	0.874267	−0.483839	–	–	*yes***
SQ M7	–	–	0.612248	0.789341	*yes***
SQ M8	0.792170	−0.417418	–	–	*no*

Table 10. supplied quantity Process Estimations

4.4 Conclusions

This paper was initiated with the intention of experimentally examining firm behavior in markets when there is a high level of competition. Firms had the possibility of choosing both on prices and quantities. A high level of competition with the result of the bankruptcy of many firms was conjectured. A collusive behavior of the remaining firms in the market was expected. However, nothing of this was observed.

Overall average weighted prices were smaller and offered quantities higher than in former price-quantity two firm experiments took as a reference [see Brandts & Guillén (2003)]. This indicates a higher level of competition. Nevertheless, the number of bankruptcies was never enough in any market to make collusion possible. Also a high level of inefficiency, higher than in 2F, was generated because of overproduction and underproduction.

Any market among the eight independent observations produced a collusive result.

At least five firms survived in any case. In two markets all the firms survived until the end of the experiment. However the most remarkable result was that cycles were observed in all markets from the beginning until the final rounds That is, the quantity supplied, by firms and the average market price showed a cyclical behavior in any market. This result is general and independent of the number of surviving firms. Cycling market results are related to firm individual behavior. If prices were above the marginal cost firms produced a high quantity and tried to set a price slightly lower than the other firms prices. Nevertheless, all firms tried to do it at the same time and this created a dynamic that in several rounds made prices to be equal to the marginal cost. At this point some firm set a very high price and a very low production. The others observe it and imitate. Then, with prices over the marginal cost, another cycle started.

It is reasonable to think that cycles could continue in time. Collusion does not seem an alternative result, since there were always too many firms to make it possible. More bankruptcies are not expected since firms are making profits, or at least they are not making losses, when they follow the cycle. Moreover, to follow the cycle was the best one firm can do if the others in the same market did so. Hence, once cycles started it seems they were not to stop.

Price cycles resemble the ones suggested by Edgeworth (1925). They have been also found in empirical studies about gas [see Castanias et al. (1993)] and airline markets [see Ross (1997) and Busse (2002)].

Price cycles have been found before in experimental studies. Cason et al. (2003) found significant cyclical patterns in posted price experiments. The critical difference with the study this paper presents is cycles are slower and, possibly, price dispersion is higher. This may be so because in Cason et al. (2003) firms do not produce in advance. Hence, they do not face the problem of making losses because they cannot sell (or store) output produced at a certain cost. Firms facing a riskier situation would follow the cycle in a tighter way in order to avoid losses.

This experiment and its results try to be nothing more than a little step forward. I tried to keep the design as simple as possible. It is true that the box design may be unrealistic for many real markets, nevertheless, it have been widely use both in the experimental and theoretical literature (in Hotteling model, for instance). It is maybe more important that this is a design that takes bankruptcies into account, however, it does not consider entrance of new firms. Hence, future research should consider these features.

4.5 References

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4.6 Appendix A: Instructions

This is an experiment about economic decision making. It is completely forbidden any type of communication once you have started to read this instructions sheet. If you have any doubt now or during the experiment you can raise your hand and we shall attend you privately.

The experiment is divided into rounds. Each of you will have the role of a firm. In each period firms have to decide which quantity to produce and at which price to sell. To make your decision you should take into account that:

1) production can be any multiple of 0.1 between 0 and your production capacity. Every firm starts with a capacity equal to 25 units. Capacities may change during the experiment and it will be indicated at the beginning of each round

2) any price multiple of 0.1 can be set between 0 and 100 ECU (Experimental Count Units)

3) to produce one unit costs 50 ECUs, whether you sell it or not

4) the experiment will start with eight firms offering products in each market (group), that is, your firm and other seven

5) at the beginning of the experiment it will be decided randomly which firms will be in which market

6) in each market there will be the same firms round after round, you will be grouped with the same participants during all the rounds

7) computer simulated consumers will always want to buy 100 units

8) consumers will buy according to the following rules:

let q(equal) the quantity produced by your firm plus the firms which have set the a price equals to yours

let q(smaller) the quantity produced by firms which have set a price smaller than yours

there can happen one of the three following cases:

if q(smaller) + q(equal) $<=$ 100 then you will sell your whole production

if q(smaller) $>=$ 100 then you will not sell anything

if q(smaller) + q(equal) $>$ 100 and q(smaller) $<$ 100 then you will sell proportionally to your production following the formula q(sold) = (100 - q(smaller)) x q(produced)/q(equal)

that is, consumers always buy the cheapest units first

9) every firm starts with an initial capital balance of 5000 ECUs

10) a firms can go bankrupt if their total profits (including its initial capital balance and the accumulated round profits) are smaller than zero

11) the owner of a bankrupted firms must stay in their place until the experiment ends

12) the capacity of a bankrupted firm will be shared among the surviving firms (in its market). Each surviving firm will add to their former capacity a share of the bankrupted firm according to the proportion of its accumulated profits on the total accumulated market profits, that is, it will increase its capacity in:

capacity(bankrupted) x profits (firm)/profits(group)

During the experiment you will see five screens:

-The input screen, including the ID number of your firm, your capacity and two fields in which you will type your price and quantity. You may use also the Windows calculator double-clicking the corresponding icon.

-The results screen, presenting prices, produced quantities, sold quantities for every firm in the market, the ID of your firm will be presented in bold. Moreover in the lower part of the screen your round and accumulated profits will be presented in ECU and Euro units.

-The history screen, will present the past prices and quantities for every firm in the market, your sold quantity and your round profits.

-A waiting screen.

-The final results screen, presenting your profits at the end of the experiment.

The experiment will take 65 rounds. You will get 4 Euro cents per 100 ECUs plus 5 EUR as a show-up fee.

4.7 Appendix B

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	Dashed Line:	Highest Profits Individual Quantity
	Dotted Line:	Lowest Profits Individual Quantity
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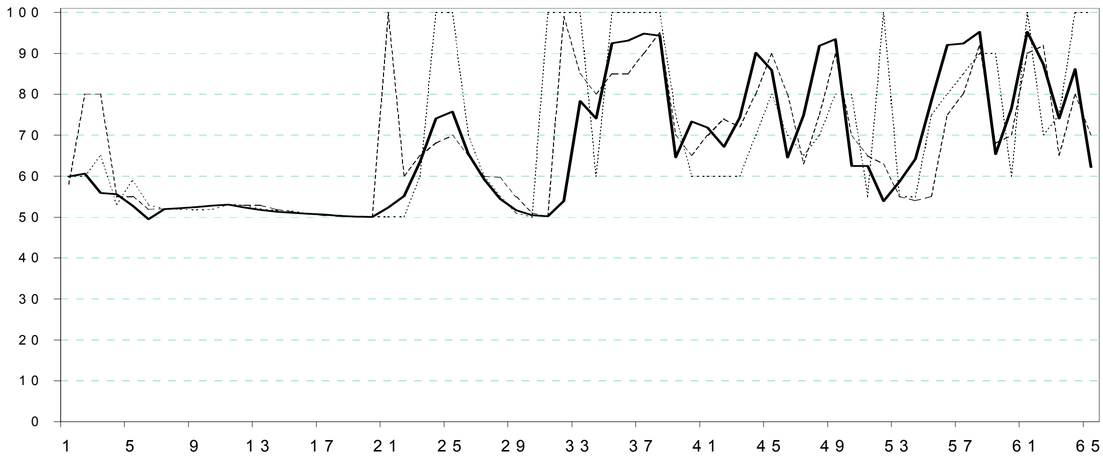
	Dashed Line:	TSQ (Scale on the Right)

Legend for Graphs in Next Pages



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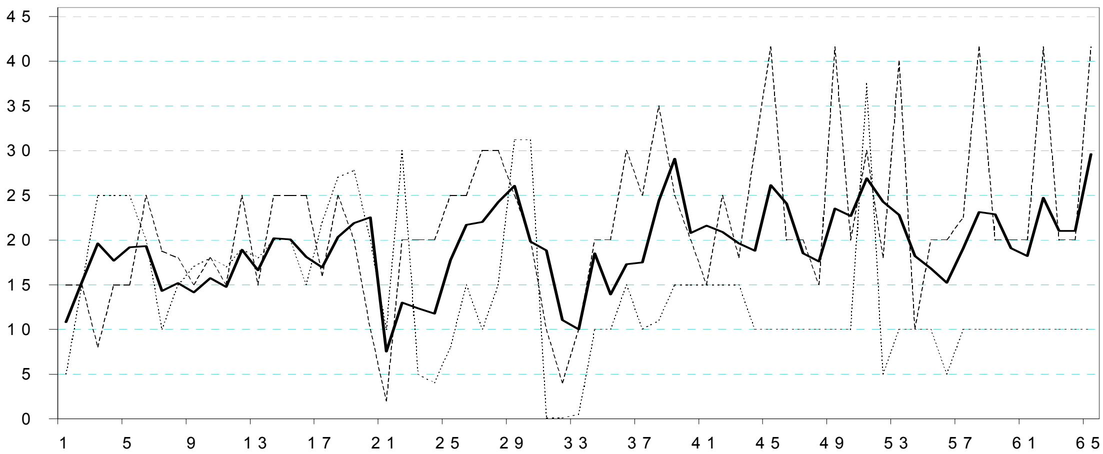

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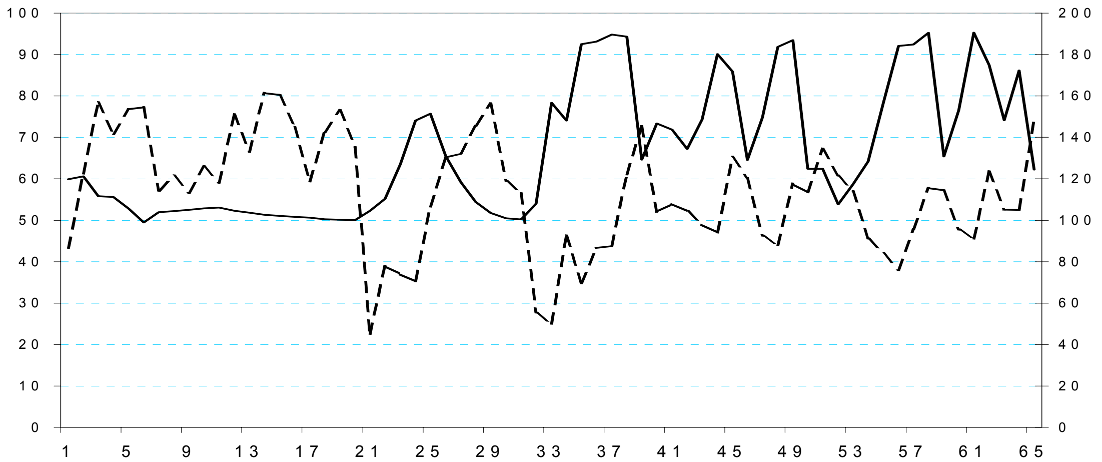

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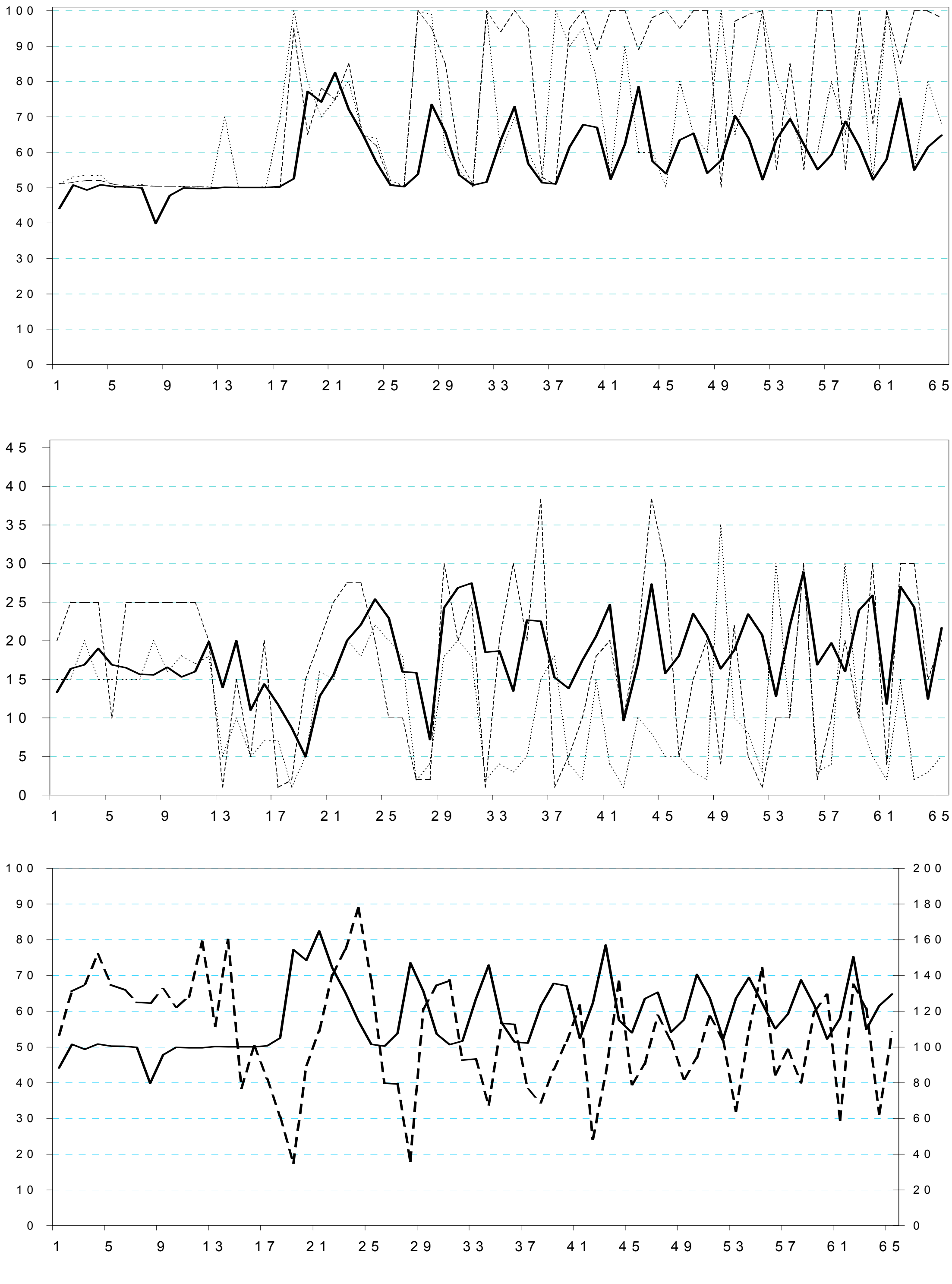
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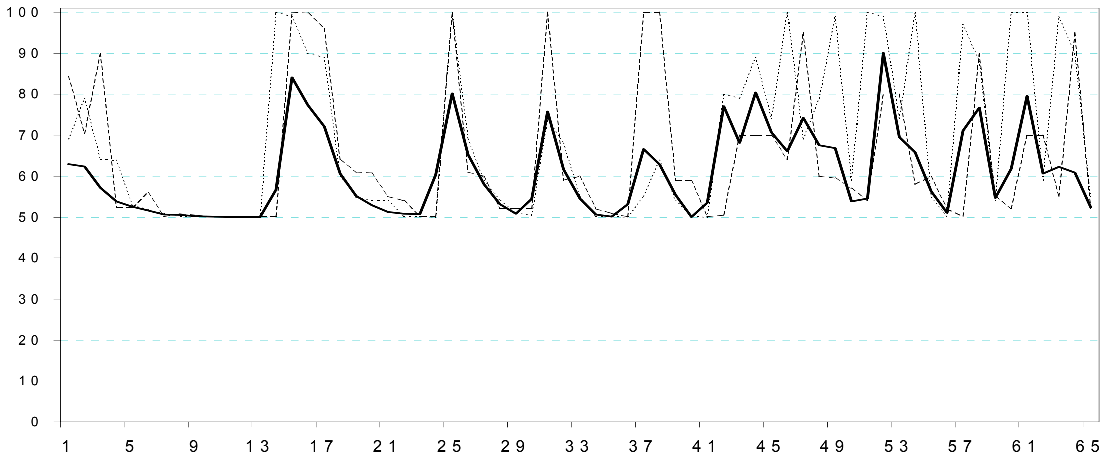
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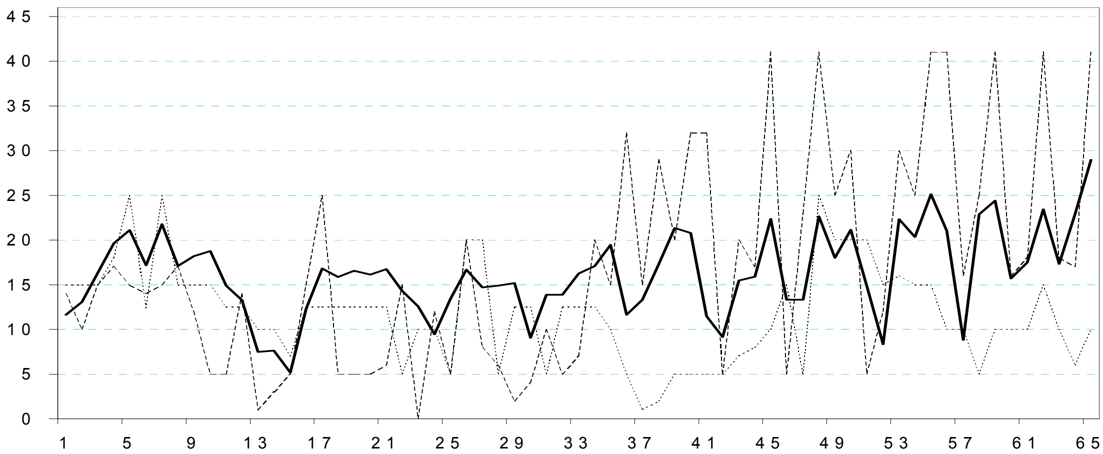
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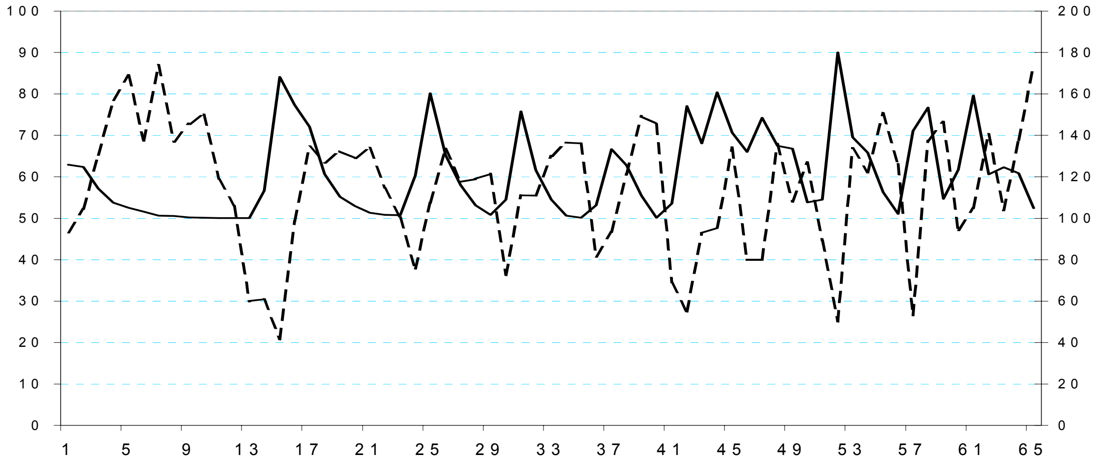
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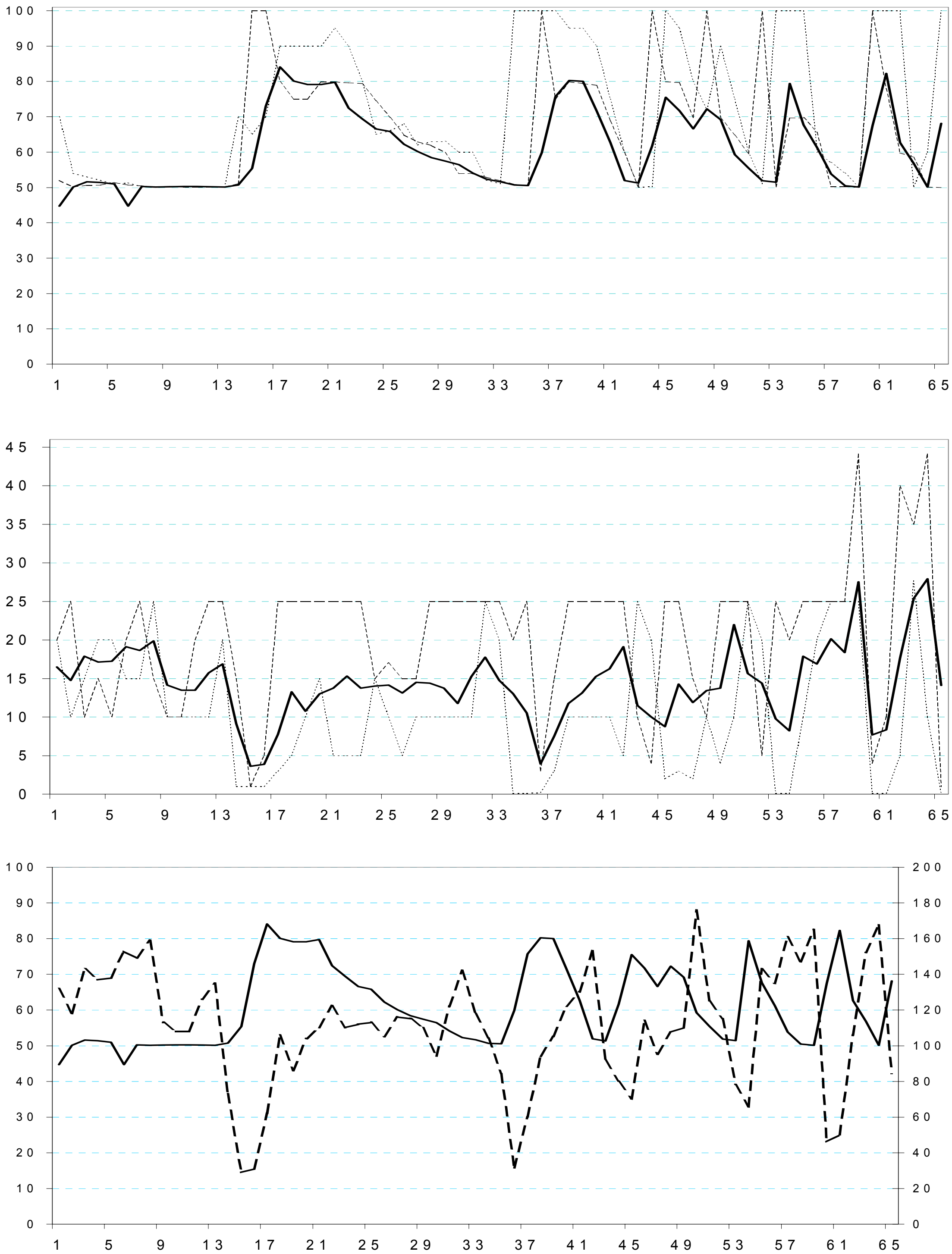
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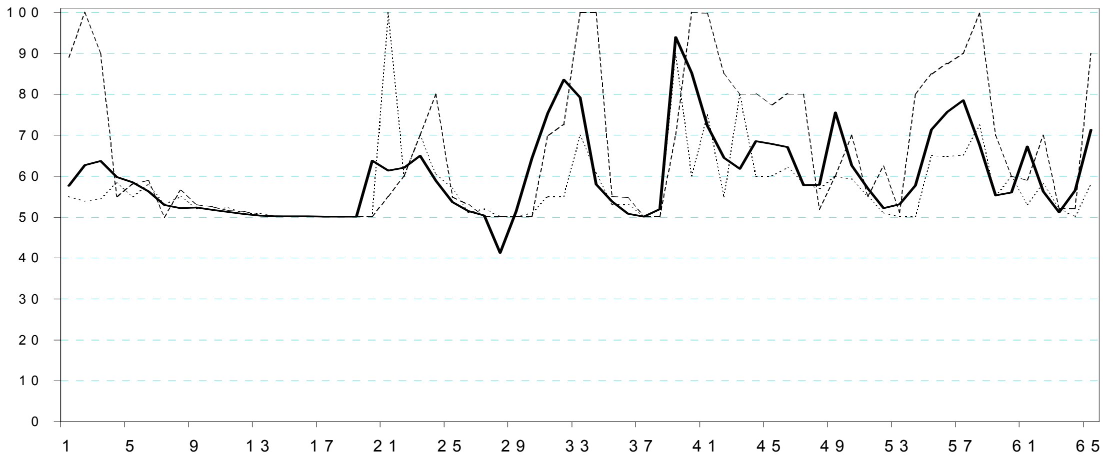
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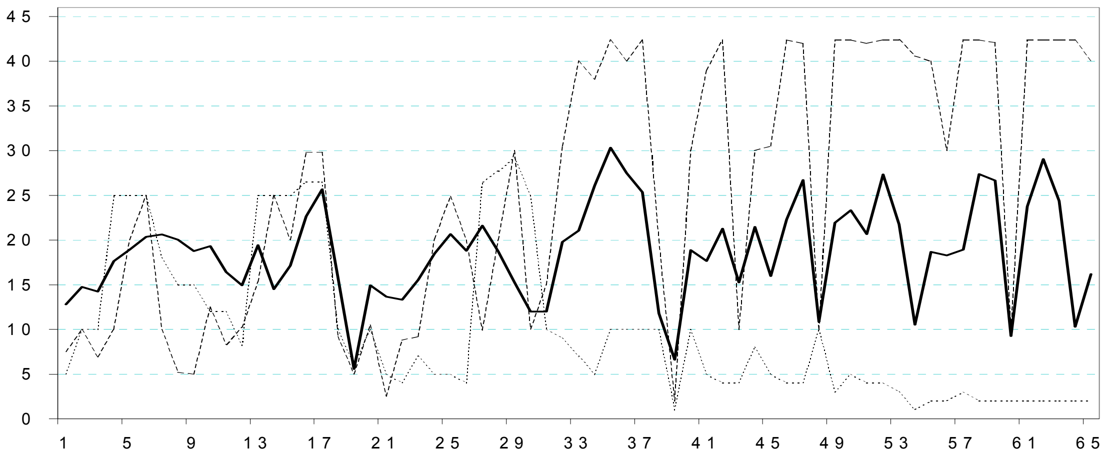
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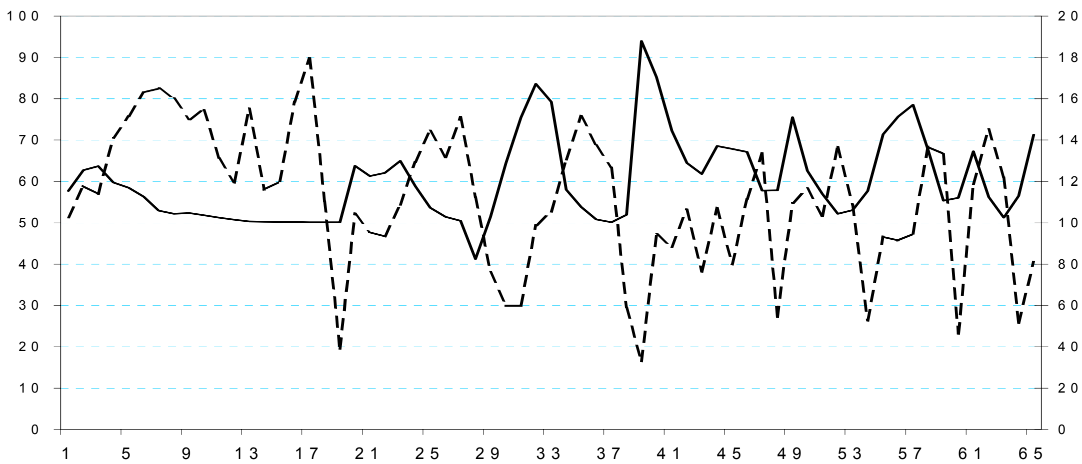
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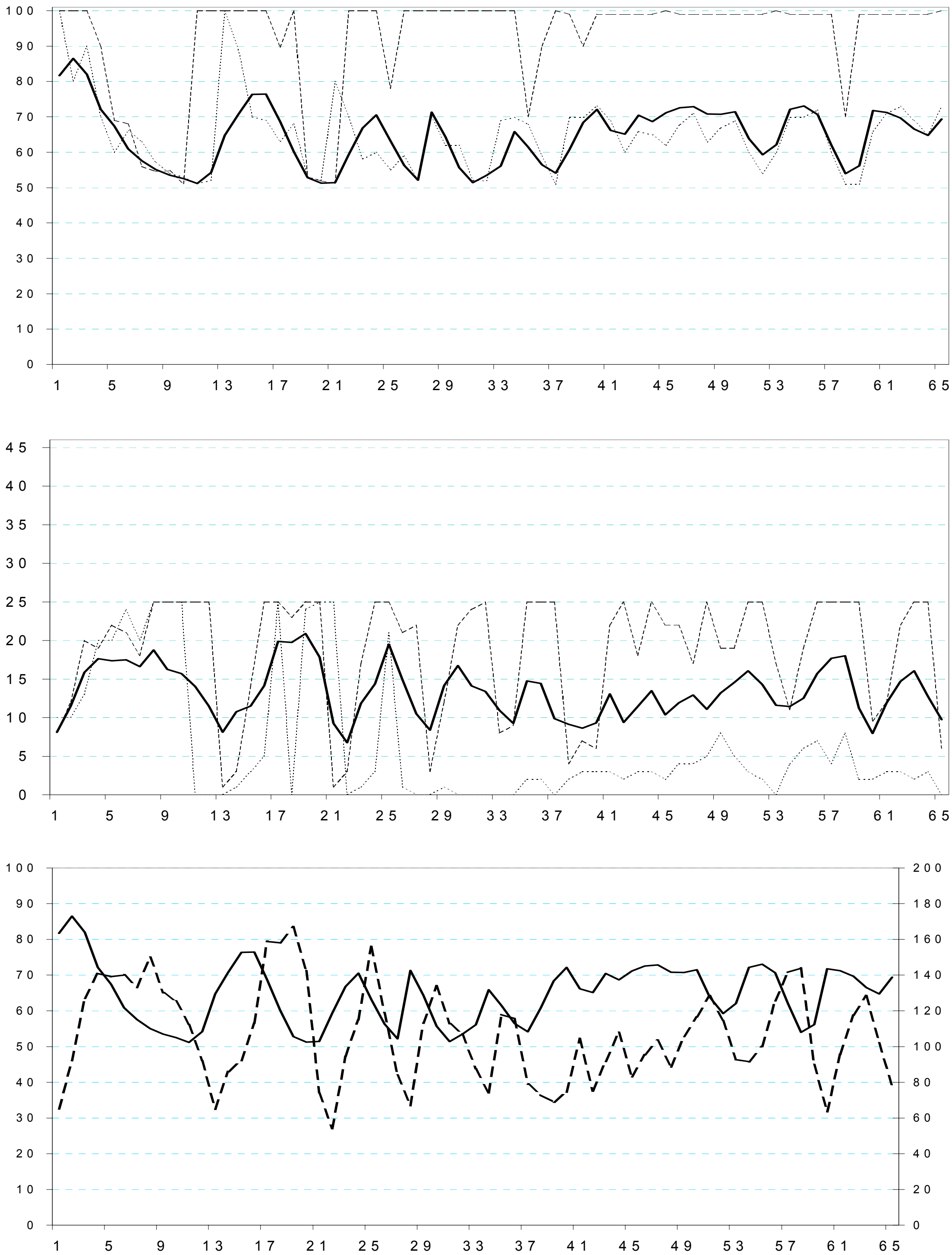

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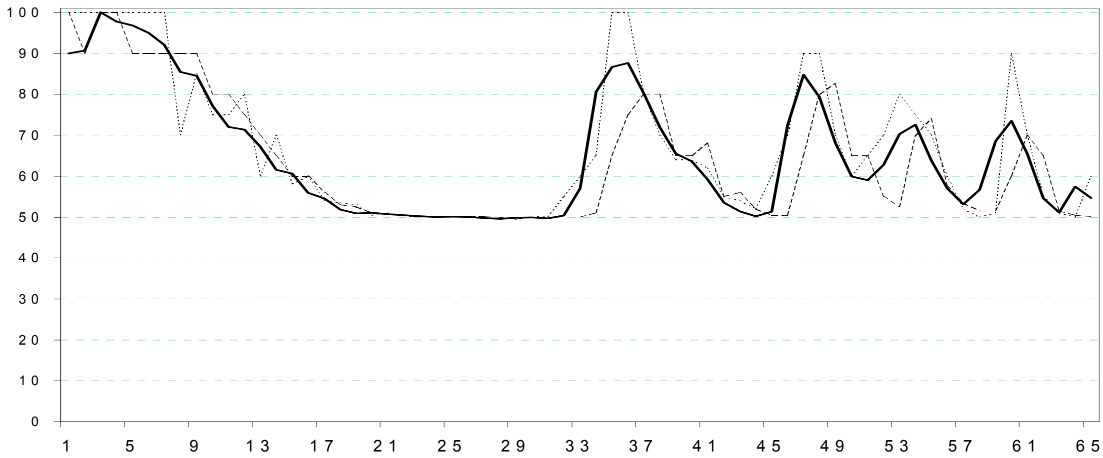
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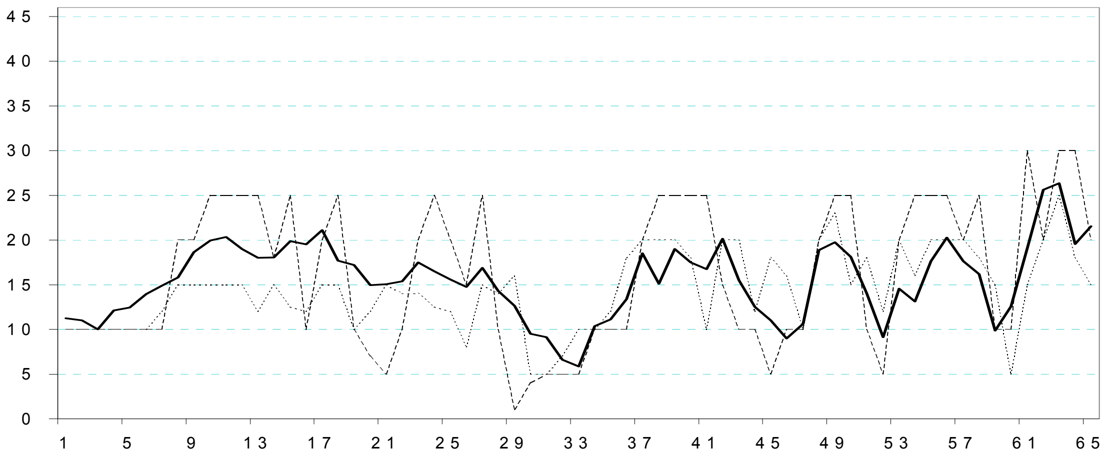
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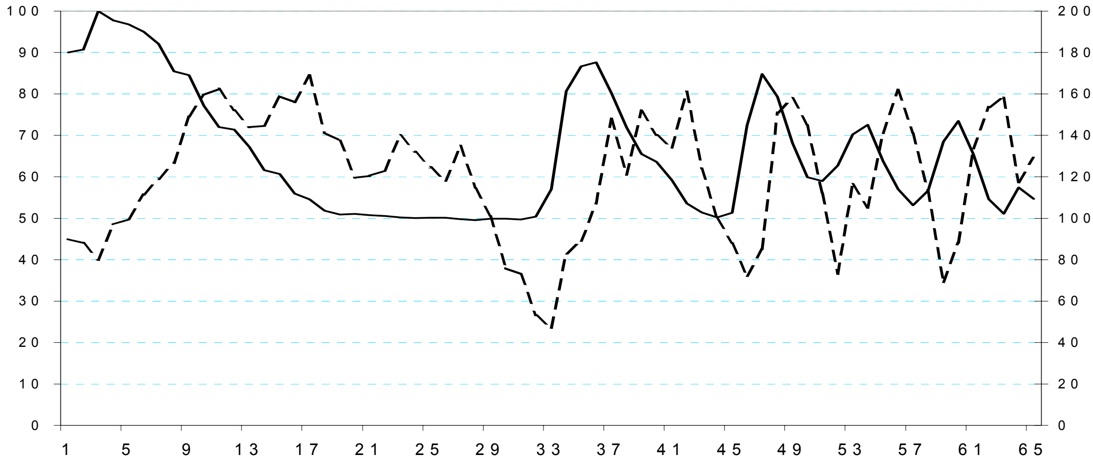
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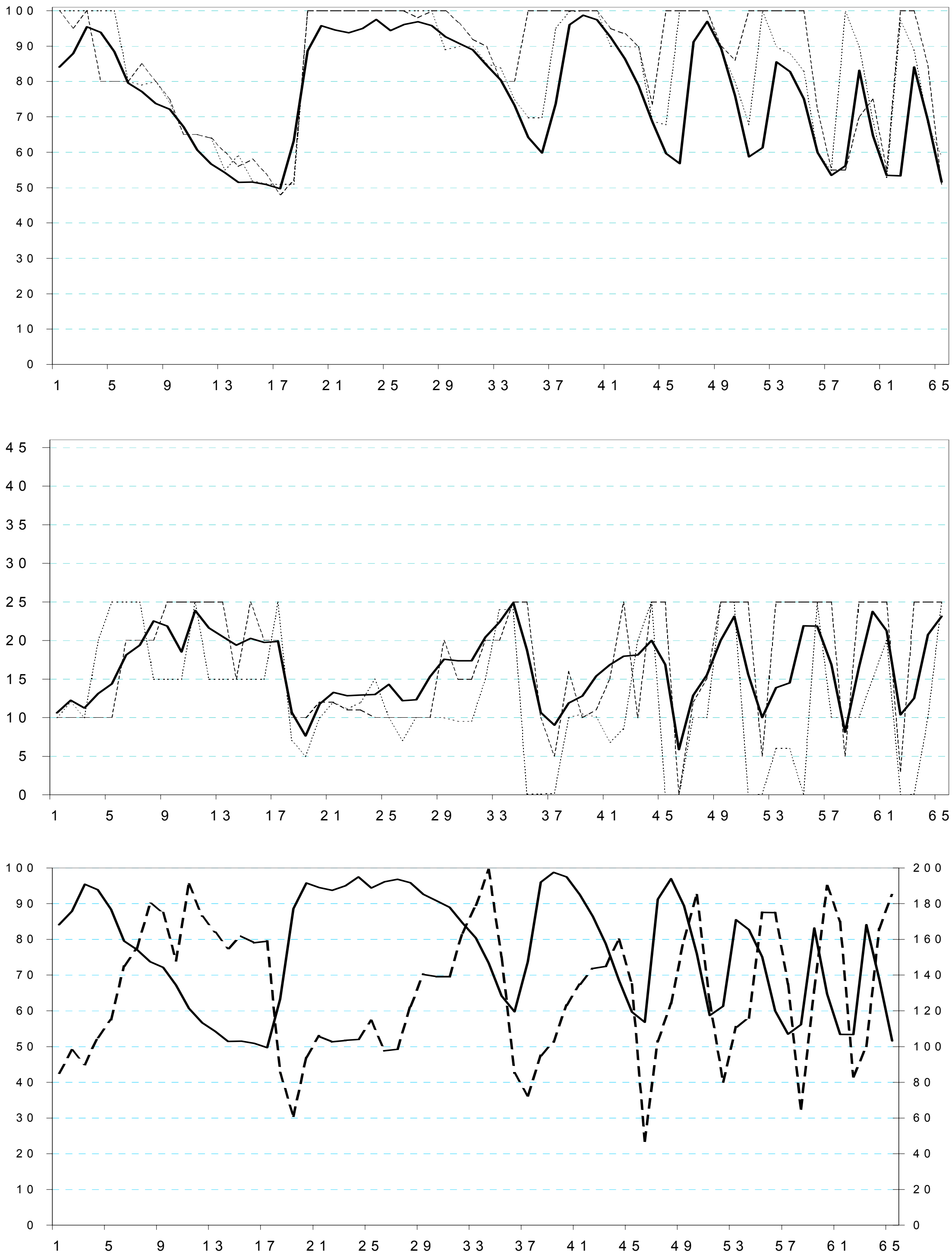

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5 Feeding the Leviathan[34]

5.1 Introduction

A large part of the literature on cooperation in public good and other social dilemma games discusses how to enhance the level of cooperation [see e.g., Ostrom (1998)]. The possibility to punish defectors has been discussed as one mechanism to enhance cooperation [e.g., Fehr and Gaechter (2000), Fehr et al. (2002) or Ostrom et al. (1992)], but is has also been discussed whether this might crowd out voluntary cooperation (Frey and Jegen, (Forthcoming); see also Fehr and Gaechter (2000) in a slightly different context]. The use of bonuses instead of punishment has been analyzed as an alternative with varying results. In the experiment of Rapoport and Au (2001), a treatment using a penalty is more effective in reducing requests in a commons game than a treatment using a bonus, while McCusker and Carnevale (1995) show in their commons game experiments that cooperation is higher in a reward condition than with punishment or without any sanctions. In Fehr and Gaechter´s experiments the reduction of voluntary cooperation is stronger in a "punishment" treatment than in a "bonus"-treatment.

Punishment mechanisms are used in many real-world settings to enhance contributions in social-dilemma-type situations. However, usually this does not imply punishment done directly by the other parties in the social-dilemma situation, as in the experiments mentioned, but rather by a (legitimate) institution[35]. Examples are the police and the juridical system, institutionalized punishment mechanisms for breaking rules within social groups, schools, universities etc., but also more "extreme" situations like UN-peacekeeping missions in countries where a war has been ended. Such institutions are usually established because a social mechanism helping to reach cooperation without punishment is lacking. An important social mechanism in this respect is trust [see, e.g., Smith (2002)]. The idea behind many institutionalized mechanisms for the establishment of cooperation is to

[34] Joint with Christiane Schwieren & Gianandrea Staffiero.

[35] Some experiments now look also at third-party punishment, but with a different focus than our experiment (see below for an example, and for a short overview, Fehr et al. (2002)).

finally reach a situation were mutual trust ensures cooperation and the institution is no longer necessary. However, there are different opinions in the literature about the effects of such mechanisms, which the present paper contrasts: Lewicki and Bunker (1996) suggest that interpersonal trust can develop in situations where trusting behavior is ensured by an efficient punishment mechanism. In their view, such a mechanism can be a first stage in the development of trust. They call this kind of trust "institutional" or "deterrence-based". In this stage, members of a society or group trust only in (the efficiency of) the punishment institution which enforces cooperative behavior. Lewicki and Bunker (1996) assume that from deterrence-based or institutional trust, "knowledge-based" trust can develop, as over time the experience that others cooperate becomes internalized [see also Kramer (1999)]. Shapiro et al. (1992), on the contrary, argue that institutional trust will break down when the punishment mechanism does not work efficiently or disappears. This view is supported by results of experiments of Fehr et al. (2002). In their experiment, after ten rounds with a punishment-option, cooperation broke down immediately when punishment was no longer possible.

This type of evidence is typically explained with the idea of "motivational crowding-out" (see, e.g., Frey (1998) and Frey and Jegen (Forthcoming),), i.e. the notion that punishment mechanisms tend to reduce intrinsic incentives for voluntary cooperation, as establishing and using a punishment mechanism can be seen as a distrusting act by other members of the group or society. Using a version of the gift exchange game [see Fehr et al. (1993), Fehr et al. (2002)] find that first movers achieve much higher cooperation by second movers when they choose not to set-up available bonus or punishment mechanisms than when a such devices are announced and used. However, a legitimate "external" punishment mechanism might not have these negative effects, as research on the effect of fair procedures indicates [see, e.g., Tyler and Kramer (1996)]. Therefore, such a mechanism might be able to solve the problem of how to implement a punishment threat such that it does not lead to crowding out of voluntary cooperation but rather to trust-building over time. This is the logic which appears to be behind the action of peace-keeping troops. Ostman et al. (1999), using external "punishment" institutions in a commons game, found that legitimate external punishment institutions using adequate sanctions do lead to more cooperative behavior. If the sanction was seen as inadequate, it disrupted group solidarity and led to lower levels of cooperation. This is additional evidence that punishment being perceived as a "fair procedure" is important.

One fair procedure recently receiving attention in experimental economics is "voice" or "voting" [see e.g., Aquino et al. (1992), Kroll et al. (2002)]. Voting is perceived as fair because those who are affected by a decision have their voice heard before the decision is made [see, e.g., Tyler and Kramer, (1996)]. A factor determining whether disappearance of the punishment institution leads to a breakdown of cooperation or not could be whether the group decided to dismiss the punishment institution (by a vote) or whether it was just taken away by an external agent. Furthermore, voting is communication, i.e., group members voting for or against the punishment institution send a message to the other members of the group. Communication has been shown in some experiments to enhance cooperation [see, e.g., Isaac and Walker, (1988)]. It is however possible that group members vote against the punishment institution for different reasons. First, casting a vote to dismiss the punishment institution can be an expression of trust. Members of the society or group express their trust that the other members will continue to cooperate even without the punishment mechanism. However, another possibility is that those who are prone to free-ride (individualists or competitors, in terms of social value orientations (see e.g., Kurzban and Houser (2001), Van Vught and Gramzow (Forthcoming)] vote for dismissal of the punishment institution to get rid of its costs and to be able to free-ride on other's effort without being punished. This should especially be the case when cooperation was not reached even with the punishment institution. In such a situation, also people who are cooperatively oriented but do not believe in the cooperation of their peers will vote against the institution. In that case, the vote does not constitute an expression of trust, but rather an expression of distrust in the possibility of cooperation: cooperators suffered from the free-riders *and* had to pay for the - inefficient - punishment institution. Beckenkamp and Guembel (2000) find that the influence of social value orientations on behavior in social dilemma games decreases when the number of experienced defections by others increases. In their experiments, people of all value orientations get more and more angry when cooperation does not work out, and are less prone to cooperate. A moderate sanctioning system proved to be efficient in reducing anger of those who were confronted with experiences of defection.

In the design presented, we build on and extent the work described so far in that we use an "external" punishment device and vary the degree of participation in the procedure of removal of this punishment device. We focus on two main points: whether an external punishment device can operate in a way such that it does not lead to a crowding out of voluntary cooperation, but rather to trust-building and continuous cooperation after removal of the punishment mechanism, and whether the latter effect is reinforced by making the removal an explicit choice by the small "societies" represented by the groups of subjects participating in our experiment. In other words, we investigate the convenience of creating and supporting a "Leviathan", argued by English philosopher Hobbes (1651) to be a necessity for maintaining social order, when a society aims at developing a genuine ability to achieve given goals, with the knowledge that the Leviathan cannot be fed forever and is going to leave citizens alone one day.

In the next section, the general design is described. Then, predictions and hypotheses are developed formally. The fourth section describes the design in more detail. The fifth section shows the results, and the last section discusses them and concludes. Finally, Appendix A includes the three different treatments instructions and Appendix B summarizes overall behavior using one table per treatment.

5.2 General experimental design

A step-level public goods game is played in stable, randomly determined groups of five players, who interact anonymously for fourteen rounds. The step-level, or threshold-based structure allows us to get a more clear definition of sufficient level of contribution, and a related simple way to design the punishment mechanism, which is easily understood by the participants. Second, cooperating in a step-level public good game when others defect is detrimental, as all money invested is lost. Each player in every round has an endowment worth 10 units of experimental currency, and has to decide over his contribution (via the choice of an integer number between 0 and 10) to a step-level public good reached at a total contribution of 31. If 31 is reached each player equally receives 12 points. If 31 is not reached, no public good is created so that any positive contributions are lost and each player only receives what he has kept.

In the basic treatment, player i's payoff in any given round is defined as:

$$\pi_i = \begin{cases} 10 - g_i + 12 & if \sum_{j=1}^{5} g_j \geq 31 \\ 10 - g_i & otherwise \end{cases}$$

The common reward given to all members if the threshold is reached is 12, no matter how the threshold was reached. For instance, we could have a combination of contributions reaching exactly 31 or going beyond, up to 60; contribution levels across members could differ substantially or be equal.

As in typical public good games, higher contributors get relatively lower final payoffs, and that holds whether the threshold is reached or not.

In the two experimental treatments, we introduce an external punishment mechanism, that we name "police" here (but not in the instructions for participants), which punishes players who choose a contribution strictly lower than 6.

As with real-world institutions, the police mechanism in the experiment has a cost, which is set to be 3 points. This makes it desirable to get rid of the police mechanism. The punishment is a fixed amount of 6 points. We get:

$$\pi_i^p = \begin{cases} 10 - g_i + 12 - 3 & if \quad \sum_{j=1}^{5} g_j \geq 31 \quad and \quad g_i \geq 6 \\ 10 - g_i + 12 - 3 - 6 & if \quad \sum_{j=1}^{5} g_j \geq 31 \quad and \quad g_i < 6 \\ 10 - g_i - 3 & if \quad \sum_{j=1}^{5} g_j < 31 \quad and \quad g_i \geq 6 \\ 10 - g_i - 3 - 6 & if \quad \sum_{j=1}^{5} g_j < 31 \quad and \quad g_i < 6 \end{cases}$$

Notice that having all five members in a group choosing the minimal level of cooperation to avoid the fine, six, is not sufficient to reach the threshold of 31. Achieving it requires an additional, small contribution by at least one member. This feature avoids to make the first part of the experiment, in AR and VR, a sheer automatic repetition of the act of pressing button 6. In this way, also in presence of the police players have to learn to reach the common goal.

The experiment consists of three different treatments. In the baseline treatment (BL), the public goods game is just played for 14 rounds without the "police". In the "automatic removal" (AR) treatment, groups play with the "police" for seven rounds, and then seven more rounds without it. In the "voted removal" (VR) treatment, majority voting occurs after seven rounds with the police. Groups determine whether they continue with the same rules or whether the "police" is removed. Subjects know all this from the beginning of the experiment on.

5.2.1 Predictions and comparisons

"Standard" equilibrium analysis is based on the assumption that each player is rational and maximizes his expected payoff, and knows that these characteristics are shared by all other members of his group. In such a case, if we consider a given round in isolation we find that in absence of the police all combinations of contributions summing up to 31 are Nash equilibria, but the same holds for all players choosing zero. The introduction of the police removes those equilibria where one or more players choose contributions between 1 and 5. If, furthermore, we delete weakly dominated strategies, we are left only with equilibria where 31 is reached; more precisely, all players choose 6 but one, who picks 7. A more detailed analysis follows.

In each round with the rules of the basic treatment, i.e. in absence of the police, $\sum_{j=1}^{5} g_j = 31$ is a sufficient condition for any strategy profile $(g_1, g_2, g_3, g_4, g_5)$ to be a Nash equilibrium. In fact, none among the members has an incentive to deviate: neither decreasing his contribution, since the individual loss of 12 can only partially be compensated by saving on g (in particular, if $g_i = 10$ and $\sum_{j \neq i}^{4} g_j = 21$ then if i deviates and chooses 0 his payoff changes from 12 to 10) nor increasing it, thereby "wasting" additional resources. The other Nash equilibrium features $g = 0$ for all players. Notice that this equilibrium, in which every player gets 10, is the only egalitarian one and is Pareto dominated by all previous ones (again, if $g_i = 10$ and $\sum_{j \neq i}^{4} g_j = 21$, then i gets 12, i.e. more than 10, and all others get at least as much). On the other hand, the five equilibria with the lowest inequality and $\sum_{j \neq i}^{5} g_j = 31$ have all players choosing 6 except for one, picking 7.

Notice that it is impossible to Pareto-rank the different Nash equilibria in which 31 is achieved, while they are all Pareto superior with respect to the "all 0's" equilibrium. Of course all profiles out of equilibrium (where some contributions are "wasted") are at least weakly dominated by one or more profiles corresponding to a Nash equilibrium.

In rounds including the "police" mechanism, for all players the strategy "6" weakly dominates "0": the value of the fine, 6, compensates the benefit obtained by playing 0 in terms of saving on the contribution. It may be that the contribution 6 is decisive for reaching 31. All positive choices between 1 and 5, are strictly dominated by 6 and, therefore, cannot be part of Nash equilibria. The profile $(0, 0, 0, 0, 0)$ constitutes a Nash equilibrium, since every strategy involved is at least a weak best response to the other players' choices. Also, profiles where two or more players pick 0 and the others 6 are Nash equilibria Other ones consist of one player picking 0 and the others choosing contributions such that the sum is 31. However, if we accept the Nash equilibrium refinement which excludes weakly dominated strategies, we are left with the remaining five equilibria, where one player chooses 7 and the others 6.

Summing up, two main effects are provoked by the police. The first is a beneficial selection of Nash equilibria: if we remove weakly dominated strategies, the goal is always reached in equilibrium. The second has a distributive nature: the equilibria so selected are the most egalitarian ones among those where the threshold is reached.

The same equilibria could be "focal" in absence of the police, as their more egalitarian nature with respect to the other ones may increase the chances of 6 or 7 contribution levels by inequity averse players'. On the other hand, purely selfish players will tend to choose a contribution with the objective of maximizing their expected value. Therefore, they contribute a certain level only if they are convinced that such level is critical for reaching 31. A risk neutral "cooperator" or "altruistic" player, on the other hand, will tend to increase his contribution levels for any given set of beliefs such that the possibility of reaching the goal 31 is not negligible. That follows from the fact that these players take into account the positive externality of increasing the probability of others' payoffs to increase, besides his own. Risk aversion may have ambiguous effects: very high contributions could increases the security to reach the goal, but zero contribution ensures at least 10 as personal payoff in any round.

As we can see, individual preferences but especially expectations about others' choices are particularly critical in absence of a sanctioning mechanism, while in its presence a more clear clue on how to play is given. But how do agents form expectations in a repeated game? A simple hypothesis is that they take choices in the previous round as a basis, as naively assuming that the others repeat the same choice. Therefore, it could be assumed that reaching 31 in a given round, in particular in the first one, may have a positive effect on the chances that it will be reached in the future, as players could tend to stick to their previous choices. On the other hand, when 31 is not reached, contributions may drop radically especially if the gap from the threshold has been substantial.

One of the main questions of this study is which effect is caused in the second stage of the AR and PR treatments by the previous presence of the police. This will be discussed in detail in the next section.

5.2.2 Hypotheses

In light of what we have said so far, it is possible that the way people coordinate in presence of the police, i.e. contributing 6 or 7, could be replicated once it is removed. That is based on the naive assumption of choice repetition by other members. In fact, this assumption could be self-fulfilling: if all players in round 8 of the AR treatment keep the previous choice - as long as 31 was reached in the previous rounds with the police - they will find their expectations realized. While having played with a sanctioning device can help coordination, it is also shown in the "motivational crowding-out" line of research that such a device can have detrimental effects on motivation. As previously argued, the idea is that when the police is there, subjects cooperate *only* because it is there, and lose any other motivation such as desire of cooperating, altruism etc. In the AR treatment, this may cause contributions to drop immediately after the removal of the police. A countervailing effect could be observed in the VR treatment: if groups have obtained 31 with the police and then decide to remove it, this could be seen as a signal of trust. This could spur cooperative motivations and make these groups sustain sufficient contribution levels in the rounds following the vote for police removal. The situation is probably different if 31 has not been reached with the police.

To summarize our hypotheses about behavior in rounds 8-14[36].

Educational effect Playing with a sanctioning mechanism which provides incentive to contribute at least 6 "teaches" players a particular way to achieve the common goal.

If this hypothesis is correct, we should observe (i) higher contribution levels in round 8 in the AR treatment (which is the first round without police) both as compared to round 1 and as compared to round 8 in the BL treatment and (ii) higher contribution levels overall in rounds 8-14 in AR both as compared to rounds 8-14 and as compared to rounds 1-7 in BL[37]. Recall that the removal occurs after seven rounds in which the police operates. As a consequence, round 8 is the first in which players act with the knowledge (reminded on their screen) that the rules have changed, and the sanctioning mechanism is not there anymore.

Crowding-out effect Playing with a sanctioning mechanism can harm intrinsic motivation to cooperate. This can have two effects: each agent has lower cooperative tendencies than he normally would, and expects others to contribute only to prevent sanctions. The comparisons described in the "educational effect" hypothesis should therefore have an opposite sign, if the crowding-out effect holds: i) lower contribution levels in round 8 in the AR treatment both as compared to round 1 and as compared to round 8 in the BL

[36] Behavior in rounds 1-7 is determined by the existence of the police mechanism in the two experimental conditions. We expect that in most cases the threshold of 31 should be reached with the police mechanism.

[37] We compare both with rounds 1 (to 7) and rounds 8 (to 14) in the baseline treatment, because the latter ones are equivalent with respect to the number of rounds players have already played in baseline and in the experimental treatment, while the former ones are equivalent with respect to the number of rounds without the police players have already played.

treatment and (ii) lower contribution levels overall in rounds 8-14 in AR both as compared to rounds 8-14 and as compared to rounds 1-7 in BL

Voted removal effect As we focus on the comparisons of contribution behavior in absence of the "police" in different settings, we are interested in what happens when groups vote to remove the police, rather than in the possible cases when the police is kept.

If the police is "voted away", after reaching 31 in the rounds with the police, this can be interpreted as group members trusting each other in their ability to reach the threshold without the need of coercion. This "trust message" together with self-determination (as the decision is taken by those who are affected) can foster the intrinsic motivation to cooperate. If this is correct, we should observe that groups who have chosen to remove the police in the voting treatment achieve higher contribution levels than in AR - when comparing rounds 8 in VR and AR and when comparing rounds 8-14 in both treatments. Of course, contribution levels should then also be higher in VR than in BL in the respective comparisons.

From our analyses we can also assume that voted removal will have the opposite effect when the threshold was not reached with the police, i.e., contributions then should be equal or lower than in AR, because the vote now is not a "trust-message".

5.3 Sample and Procedure

The sample consisted of 120 students of Universitat Pompeu Fabra in Barcelona, Spain, who participated voluntarily for performance-based payment. They came from different faculties, mostly from Economics, Business and Law.

In each session, 3 stable groups of 5 students played the public goods game together. For the BL treatment, 2 sessions were run. For the other treatments, VR and AR, 3 sessions each were run. The experiment was computer-based, using the experimental software z-Tree [Fischbacher (1999)].

When students arrived at the lab, they were randomly assigned to a computer and received the instructions to read. In the instructions, the sanctioning device was not called "police". Police and non-police situations were described as Rule Set A and Rule Set B.

BL consisted of 14 rounds of the step-level public good game. AR consisted of 7 rounds of the game with the police mechanism followed by another 7 rounds of the game without police. Participants were warned after period 7 that the rules of the game changed for the rounds to come. VR consisted of 7 rounds of the game with police followed by a voting phase. In the voting phase, participants had to indicate whether they wanted to continue with Rule Set A or change to Rule Set B. Participants were then informed about the result of the vote, i.e., whether the valid set of rules in the next 7 rounds was A or B[38]. In both treatments, participants know already at the beginning that there would be some change of rules during the game and how this change would be achieved.

After the experiment, each player was privately paid according to the sum of payoffs across all rounds (3 EUR cent per "payoff unit" were paid) plus a show-up fee of 2 EUR. Average earnings in the experiment were about 6,20 EUR.

5.4 Results

Our experimental design allow us to study the effect of having recently played under a sanctioning mechanism. As we can see in Table 1 the percentage of times the threshold is reached is indeed much higher in treatments AR and VR than in BL[39]. This is true, as could be expected, when the sanctioning mechanism is working (rounds 1-7), but also when it is not, in rounds 8 to 14 (in the case of VR every group voted in favor of getting rid of the sanctioning mechanism). Notice that the police is not working perfectly, that is, the percentage of times the step-level is reached is lower than 100% in periods 1-7 for both AR and VR. Figure 1 depicts the number of groups reaching the threshold, 31, during rounds 8 to 14. We can see that this number does not decrease substantially. At least five groups reach 31 for treatment AR in every considered round, and this number is the

[38] Instructions can be found in the Appendix 1.
[39] The entire results can be found in Appendix 2.

same both in period 8 and 14. In the case of VR the number of groups contributing 31 or more is always lower than in AR. Nevertheless, three groups reach the threshold almost every round, and also in the last one. Figure 2 shows the average contribution per period considering the three different treatments. BL's average is never over the threshold. AR and VR are always over the threshold before round 7. After this round a decline in the average contribution is observed in both treatments. However, as Figure 1 indicates, this decline does not affect every group involved, but rather those who fail to stay above the threshold in the absence of the sanctioning mechanism.

	1-7	8-14
BL	7%	0%
AR	89%	65%
VR	87%	48%
AR AND VR	88%	56%

TABLE 1. PERCENTAGE OF TIMES THE THRESHOLD IS REACHED

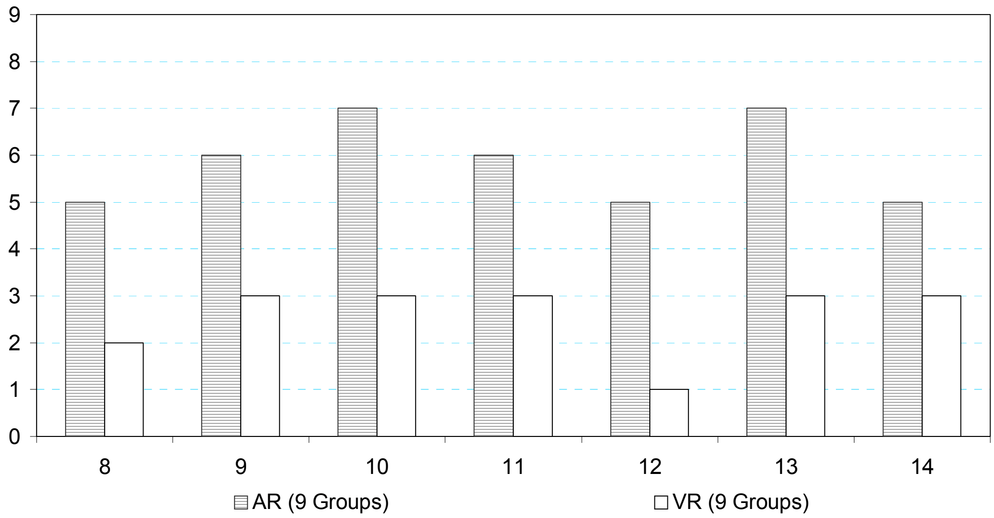

FIGURE 1. NUMBER OF GROUPS REACHING 31

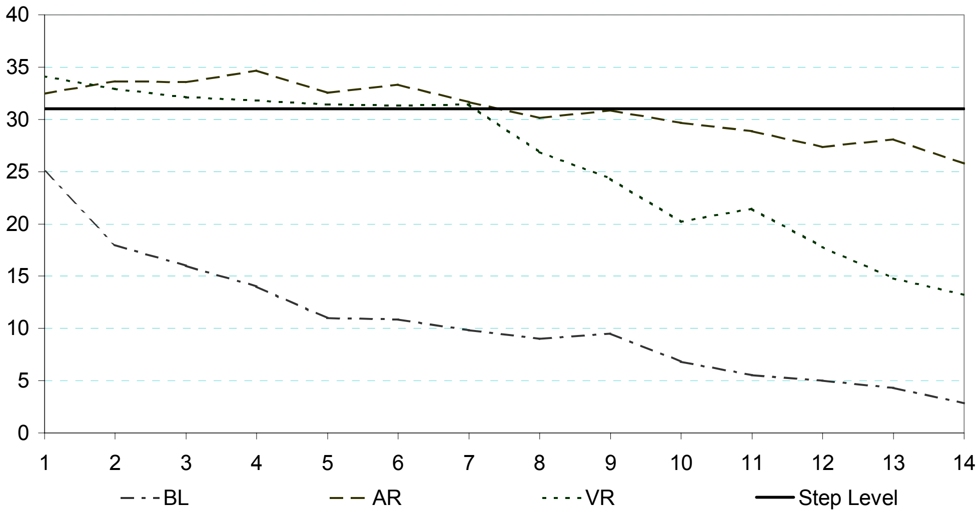

Figure 2. Treatment Average per Period

The facts described up to this point seem to support the hypothesis of an educational effect. The surprising finding is that voting seems not to have a positive effect on contributions but we will take a closer look at this below.

We report statistics using transformed data looking only at whether the threshold is reached or not. For this purpose we generate a variable setting joint contributions of 31 and above equal to one and joint contributions below 31 equal to zero. We transform the data in this way, because using the actual contributions may increase the noise-component. In fact, group contributions very high above 31 or below but close to 31 do affect averages, but in a step-level public good the success or failure in reaching the threshold is the relevant point.

Table 2 summarizes the results of the statistical tests done. We compare the average success rates of the reported treatments using data coming from the round or rounds in brackets.

Comparison	Z	Significancy
BL (1-7) - VR (1-7)	-3.16	0.002
BL (1-7) - AR (1-7)	-3.31	0.001
BL (8-14) - VR (8-14)	-2.11	0.035
BL (8-14) - AR (8-14)	-3.00	0.003
BL (1-7) - AR (8-14)	-2.76	0.006
BL (1-7) - VR (8-14)	-1.46	0.144
AR (1-7) - VR (1-7)	-0.708	0.479
AR (8) - VR (8)	-0.458	0.647
AR (8-14) - VR (8-14)	-2.07	0.039
AR (14) - VR (14)	-0.922	0.357

Table 2. Statistical Results Summary

There is no significant difference between AR and VR in rounds 1 to 7 (see row 7 in Table 2). However, there is a difference in favor of AR in rounds 8 to 14 (see row 9 in Table 2), but this difference does not yet exist in round 8 (see row 8 in Table 2). AR and VR also differ significantly in round 14. Thus, it seems as if voting creates rather less trust in our experiment. Even with the same rate of success at round 8, AR results develop significantly better than VR results in the following rounds.

However, it is possible to relate success or failure in the second stage (rounds 8 to 14) to success and failure in the first stage (round 1 to 7). We define a variable indicating whether the threshold has always been reached ("tar") in rounds 1-7 with the sanctioning mechanism. It takes value 1 in case the total contribution had always been higher or equal than 31 in rounds 1 to 7. We consider data coming from both AR and VR, so "tar" equals 1 eight times and 0 ten times. The correlation between "tar" and the average success in rounds 8 to 14 is highly significant (p = .001, r = .70). If we consider a model with constant it results $avg(8-14) = 0.243 + 0.507 * tar$. Both the constant and the slope are significant. This regression can be understood as "failure to reach the threshold with the police" increases the chance to fail in rounds 8 to 14 by 50.7%. We also look at whether already a single failure in rounds 1 to 7 with sanctioning mechanism affects trust generation. We define the variable "jof" (just once failed) which takes value one in case there had been just one failure in rounds 1 to 7 and zero in case there had been no failure during rounds 1 to 7. We consider data coming from AR and VR, but note that now the domain is restricted by the fact that we are not taking in account groups with more than one failure in rounds 1 to 7. Therefore "jof" equals one six times and zero eight times. The correlation between "jof" and the average success in 8 to 14 is r = .744 and it is highly significant (p = .002). If we consider a model with constant it results $avg(8-14) = 0.750 - 0.536 * jof$. Both coefficients are significant. This regression can be understood as just one failure with the police increasing the chance to fail in rounds 8 to 14 by 53.6%.

Comparing now the two treatments AR and VR, we find that when no failure occurs in rounds 1 to 7, the number of times the threshold is reached does not differ between AR and VR in rounds 8 to 14. When failures occurred in rounds 1 to 7 however, success rates differ significantly between VR and AR in rounds 8 to 14. This indicates that there is a difference in the interpretation (and probably intention) of a vote against the "police" between situations were failure occurred in previous rounds and situations were no failure occurred. Both in AR and VR success rates are lower after removal of the punishment institution when in previous rounds failure occurred. However, in VR this effect is much stronger than in AR, indicating that voting in a situation after failure is seen as a "distrust" message (see Table 3 for statistical results).

Comparison	Z	Significancy
AR_{NF} - VR_{NF} (8-14)	-0.62	0.539
AR_{F} - VR_{F} (8-14)	-1.82	0.069

Table 3. Statistical Results Summary

5.5 Conclusions

We conducted an experiment to test the effects of playing a step-level public good game under a sanctioning system which enforces cooperation on behavior in subsequent rounds, once this system is removed. The removal of the "police" was either automatic or chosen by the groups through a simple majoritarian vote.

The sanctioning system here referred to as the "police" helps coordinating to achieve the threshold, which results in a common reward for all group members, but it leaves room for deviations in the sense that low levels of cooperation are punished but not impeded altogether. Moreover, having all members choosing the minimal level of cooperation allowed is not sufficient to obtain the common reward: a small additional effort by at least one player is needed. In the equilibrium analysis we find that, once weakly dominated strategies are removed, all Nash equilibria for a single round are "good", in the sense that the goal 31 is achieved, while in absence of the police the "all zero" equilibrium is added.

The literature discusses two effects which could arise from a history of playing with a sanctioning device: a positive "educational" effect of learning intuitive ways to coordinate on 31, and to learn to trust in each other's cooperation [e.g., Lewicki and Bunker, (1996)]. The other possible effect is a negative "crowding out" of intrinsic motivation to cooperate *only* due to the presence of the police [e.g., Frey and Jegen, (2001)].

This latter effect is not found in our game. The educational or trust-building effect prevails so that groups which played with the police in earlier rounds are much more successful in keeping contribution levels high enough to reach the common goal for most rounds following removal. This result contradicts what is found in other experiments with standard public good games. In contrast to those experiments, in our design the "police" suggests a particular way to reach a common goal; in continuous public goods games setting a threshold for punishment can undermine the drive to cooperate more than that level, as was found by Fehr and Rockenback (2001), and Frey and Oberholzer-Gee (1997) among others. In our design, going beyond 31 is only wasteful, and therefore learning a particular cooperation level can have beneficial effects.

In a third treatment each group decided by majority vote whether to keep the police or not, to see whether such a procedure has an effect on subsequent behavior in the sense of a trust-message being sent out [see, e.g., Tyler and Kramer (1996)]. We found that all groups decided to remove the punishment institution and cooperation levels after removal were higher than in the baseline treatment, but on average lower than in the automatic removal treatment. This result at first glance contradicts the intuition that removing the police by vote should have a positive effect on contribution levels, arising from the fact that the vote against the police expresses mutual trust between group members. However, there is another possibility why a team could vote against keeping the police, namely, that the police is seen as inefficient but costly. Once we differentiated groups according to whether in rounds 1-7 failure to reach the threshold occurred despite of the presence of the police, we find that groups where such failure never happened do succeed in reaching the threshold after the removal, no matter whether the "police" was voted away or just disappeared. In groups where failures occurred in the first seven rounds with police, contributions went quickly down after removal, especially when the "police" had been voted away. Our interpretation is that in such cases voting to remove the police has no "trustful" meaning, but can be seen as a desire to save on the cost for keeping the police when even in its presence the attainment of the threshold is not guaranteed. The voting treatment thus indicates that if the removal of the police results from a conscious, well deliberated decision, contributions are only kept up when the police-mechanism worked and people really developed trust in each other's contributions. If the police is not completely efficient and therefore trust cannot be learned, a voting against the police is quite the contrary of an expression of trust.

If voting against the police was a clear expression of trust, i.e. in situations where the threshold was always reached with the police, cooperation still did not exceed the levels found in the automatic removal treatment, but this probably is a ceiling effect, as in a step-level public good like ours, 6 resp. 7 is the "logical" contribution, and enhancing it to 8 or 9 would not make sense. Other contexts e.g. continuous public goods games, could well exhibit positive "trust effects" in such a situation.

Our results indicate that it is possible to learn mutual trust by starting out with an external punishment mechanism which is later removed. Secondly, it is important that this punishment mechanism works efficiently, otherwise no trust is learned. If the mechanism disappears automatically, people stick to the "habits" developed in subsequent rounds much longer, than when they think of all the implications of removal of the police. Having thought about it, only those who saw an efficient police continued cooperating.

Future research should more systematically check for the effects of failure in the rounds with a punishment device on later behavior, as this seems to play a larger role especially in the voting-treatment, but could be analyzed here only based on a relatively small number of observations. Another interesting topic for further research would be to use a continuous public good game, to see whether a trust signal, i.e., voting for removal of the police after no failure, could lead to even higher levels of cooperation than pure "habit-formation" as observed with automatic removal does.

Overall, it appears that there is clear need for further clarification of the effects of the various kinds of sanctions implementable in different social and economic contexts. Properly designed experiments are, in our view, a crucial means of analyzing these effects as they allow for controlling the many aspects that normally interact in determining social outcomes.

5.6 References

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5.7 Appendix A

Instructions (Baseline)

This is an economic decision making experiment. It is completely forbidden any kind of communication between you and the other participants once you have started to read these instruction sheet and until the end of the experiment.

Each of you will join a five people group. A computer will decide randomly which is the group you are going to join. This group will not change during the whole experiment.

The experiment will have 14 rounds. At the beginning of each round you will have 10 Experimental Count Units (ECUs). You must decide how many units to put in a fund and how many to keep. In order to make your decision your must take into account that:

1) the amount you are going to put in the fund (Contribution = C) must be an integer number between 0 and 10

2) if the total amount (TC) in the fund for your group of five people is bigger or equal to 31 then each individual will get 12 units, independently of their contribution. Therefore the payoff will be:

$10 - C + 12$

3) if the total amount in the fund for your group of five people is less than 31 then the payoff will be:

$10 - C$

During the experiment you can see three types of screens:

i) the contributions screen where you may write your contribution in the corresponding field

ii) the results screen where you will see your contribution, the total contribution (TC) and your payoff

iii) the waiting screen

Your will receive 2 EUR just for showing up, plus 3 cent of EUR per UCE. If you have any doubt you may raise your hand now or during the experiment, we will attend you particularly.

Instructions (Automatic removal)

This is an economic decision making experiment. It is completely forbidden any kind of communication between you and the other participants once you have started to read these instruction sheet and until the end of the experiment.

Each of you will join a five people group. A computer will decide randomly which is the group you are going to join. This group will not change during the whole experiment.

The experiment will have 14 rounds. At the beginning of each round you will have 10 Experimental Count Units (ECUs). You must decide how many units to put in a fund and how many to keep. In order to make your decision your must take into account that:

1) the amount you are going to put in the fund (Contribution = C) must be an integer number between 0 and 10

2) during the first 7 rounds the experiment will work according to the following rules:

a) if the total amount (Total Contribution = TC) in the fund for your group of five people is bigger or equal to 31 then each individual will get 12 units, independently of their contribution

b) if your contribution to the fund is less than 6 your payoff will be reduced in 6 units

c) in any case all individual payoffs will be decreased by 3 units in their final payoff per round

d) therefore the payoff will be:

$10 - C - 3$

$+12 \; if \; TC \geqslant 31$

$-6 \;\; if \; C < 6$

3) during the final 7 rounds the experiment will work according to the following rules:

a) if the total amount (Total Contribution = TC) in the fund for your group of five people is bigger or equal to 31 then each individual will get 12 units, independently of their contribution. Therefore your payoff will be:

$10 - C - 12$

b) if the total amount in the fund for your group of five people is less than 31 then the payoff will be:

$10 - C$

During the experiment you can see three types of screens:

i) the contributions screen where you may write your contribution in the corresponding field

ii) the results screen where you will see your contribution, the total contribution (TC), a message telling whether you got the 6 unit decrease because of your low contribution and your payoff

iii) the waiting screen

Your will receive 2 EUR just for showing up, plus 3 cent of EUR per UCE. If you have any doubt you may raise your hand now or during the experiment, we will attend you particularly.

Instructions (police removal)

This is an economic decision making experiment. It is completely forbidden any kind of communication between you and the other participants once you have started to read these instruction sheet and until the end of the experiment.

Each of you will join a five people group. A computer will decide randomly which is the group you are going to join. This group will not change during the whole experiment.

The experiment will have 14 rounds. At the beginning of each round you will have 10 Experimental Count Units (ECUs). You must decide how many units to put in a fund and how many to keep. In order to make your decision your must take into account that:

1) the amount you are going to put in the fund (Contribution = C) must be an integer number between 0 and 10

2) there exists a set of rules called "set of rules A". We are going to describe it:

a) if the total amount (Total Contribution = TC) in the fund for your group of five people is bigger or equal to 31 then each individual will get 12 units, independently of their contribution

b) if your contribution to the fund is less than 6 your payoff will be reduced in 6 units

c) in any case all individual payoffs will be decreased by 3 units in their final payoff per round

d) therefore the payoff will be:

$10 - C - 3$

$+12 \; if \; TC \geqslant 31$

$-6 \;\; if \; C < 6$

3) There exists a set of rules called "set of rules B". We are going to describe it:

a) if the total amount (TC) in the fund for your group of five people is bigger or equal to 31 then each individual will get 12 units, independently of their contribution. Therefore your payoff will be:

$10 - C + 12$

b) if the total amount in the fund for your group of five people is less than 31 then your payoff will be:

$10 - C$

4) during the first 7 rounds the "set of rules A" will work

5) at the end of the seventh round the members of each group will decide by majority whether to use the "set of rules A" or the "set of rules B" during the next 7 rounds

During the experiment you can see five types of screens:

i) the contributions screen where you may write your contribution in the corresponding field

ii) the results screen where you will see your contribution, the total contribution (TC), a message telling whether you got the 6 unit decrease because of your low contribution and your payoff

iii) the voting screen (at the end of the seventh round) in order to choose among A or B

iv) the voting results screen

v) the waiting screen

Your will receive 2 EUR just for showing up, plus 3 cent of EUR per UCE. If you have any doubt you may raise your hand now or during the experiment, we will attend you particularly.

5.8 Appendix B

	G1	G2	G3	G4	G5	G6	Average
1	27	24	19	25	29	26	25.0
2	9	10	24	30	14	21	18.0
3	0	11	16	34	12	23	16.0
4	0	6	10	35	11	22	14.0
5	0	1	8	31	7	19	11.0
6	1	0	13	26	8	17	10.8
7	2	1	11	22	11	12	9.8
8	1	0	5	15	22	11	9.0
9	10	0	4	15	22	6	9.5
10	6	0	1	10	16	8	6.8
11	0	0	1	18	7	7	5.5
12	0	0	1	13	5	11	5.0
13	0	0	1	9	13	3	4.4
14	0	0	2	10	3	2	2.8
Avg. 1-7	5.6	7.6	14.4	29.0	13.1	20.0	
Avg. 8-14	2.4	0.0	2.1	12.8	12.6	6.9	
Average	4.0	3.8	8.3	20.9	12.9	13.4	

BL Treatment Data Summary

	G1	G2	G3	G4	G5	G6	G7	G8	G9	Average
1	32	36	35	33	28	26	34	34	34	32.4
2	30	37	35	34	31	29	34	37	36	33.7
3	33	37	35	32	32	33	31	35	34	33.6
4	31	32	35	34	36	41	34	35	34	34.7
5	33	33	34	32	28	34	31	33	35	32.6
6	32	33	36	32	32	36	33	33	33	33.3
7	31	31	30	33	33	28	32	34	33	31.7
8	23	33	33	26	24	29	33	34	36	30.1
9	24	32	34	31	32	30	32	30	33	30.9
10	10	32	37	32	31	29	33	31	32	25.7
11	4	30	36	31	31	30	31	33	34	28.9
12	2	27	32	32	30	31	32	29	31	27.3
13	0	29	32	32	32	31	33	32	32	28.1
14	0	18	26	31	32	32	26	33	34	25.8
Avg.1-7	31.7	34.1	34.3	32.9	31.4	32.4	32.7	34.4	34.1	
Avg.8-14	9.0	28.7	32.9	30.7	30.3	30.3	31.4	31.7	33.1	
Average	20.4	31.4	33.6	31.8	31.4	31.4	31.4	33.1	33.6	

AR Treatment Data Summary

	G1	G2	G3	G4	G5	G6	G7	G8	G9	Average
1	33	33	39	34	32	34	33	32	37	34.1
2	27	34	36	32	32	31	31	34	39	32.9
3	35	33	32	33	32	33	26	33	32	32.1
4	25	34	28	34	32	32	32	31	38	31.8
5	27	32	30	32	32	31	33	33	33	31.4
6	28	31	34	33	31	33	31	30	31	31.3
7	30	27	35	31	33	33	32	32	30	31.4
8	15	26	24	33	30	32	26	29	27	24.9
9	9	14	16	28	32	29	32	26	33	24.3
10	3	5	5	33	33	24	31	22	26	20.2
11	0	6	20	33	32	38	29	26	19	21.4
12	10	3	1	30	32	29	29	17	9	17.8
13	0	3	0	31	32	35	18	7	7	14.8
14	0	1	0	33	33	35	11	0	6	13.2
Avg. 1-7	29.3	32.0	33.4	32.7	32.0	32.4	31.1	32.1	34.3	
Avg. 1-14	5.3	8.3	8.0	31.5	32.0	31.7	25.1	18.1	18.1	
Average	17.3	20.1	20.7	32.1	32.0	32.0	28.1	25.1	26.2	

VR Treatment Data Summary