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Price competition and market concentration: an experimental study

Martin Dufwenberg[a], Uri Gneezy[b,*]

[a]Department of Economics, Stockholm University, SE-10691 Stockholm, Sweden
[b]Faculty of Industrial Engineering and Management, Technion, Israel Institute of Technology, Technion City, Haifa 32000, Israel

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Abstract

The classical price competition model (named after Bertrand), prescribes that in equilibrium prices are equal to marginal costs. Moreover, prices do not depend on the number of competitors. Since this outcome is not in line with real-life observations, it is known as the 'Bertrand Paradox.' In experimental price competition markets we find that prices do depend on the number of competitors: the Bertrand solution does not predict well when the number of competitors is two, but (after some opportunities for learning) predicts well when the number of competitors is three or four. A bounded rationality explanation of this is suggested. © 2000 Elsevier Science B.V. All rights reserved.

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1. Introduction

The investigation of oligopolistic markets is central in economics. It is often assumed that firms in such markets compete in prices (see e.g. Tirole, 1994, p.

*Corresponding author.
E-mail address: gneezy@econ.haifa.ac.il (U. Gneezy)

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224). In the classical model of price competition (named after Bertrand, 1883), the equilibrium entails that price is equal to marginal cost whenever at least two firms are in the market. In effect, each firm makes zero profits even in a duopoly situation. Since observations from real markets are not in line with this result, it is referred to as the 'Bertrand Paradox.'

In this paper we report experimental results of markets in which participants compete in prices. In particular, we consider the effect of changing the number of competitors on market outcome. We study the following game, which corresponds to a discrete version of the Bertrand model:

> *Each of N players simultaneously chooses an integer between* 2 *and* 100. *The player who chooses the lowest number gets a dollar amount times the number he bids and the rest of the players get* 0. *Ties are split among all players who submit the corresponding bid.*

N is a control variable in the experiment, which in different treatments take the respective values 2, 3, and 4. The unique Nash equilibrium in each treatment is a bid of 2 by all players, and each player gets a payoff of only $2/N$.[1] The equilibrium payoffs are not zero, as in the standard Bertrand model, but they are almost zero and very low relative to what is otherwise available in the game.

This game has several attractive features that obviate some common critiques of the Bertrand model. Economists have addressed the Bertrand paradox along two different lines. First, it has been argued that certain assumptions that underlie the Bertrand model are not realistic. Edgeworth (1925), Hotelling (1929), Kreps and Scheinkman (1983), and Friedman (1977) respectively point out that the Bertrand paradox goes away if the assumption of constant return to scale is relaxed, if goods are not homogeneous, if capacity constraints are introduced, or if firms compete repeatedly. The firms may furthermore have incomplete information about cost functions or demand (as Bertrand models resemble first-price auctions, Vickrey, 1961 is relevant), and, with reference to Cournot (1960); model, one may also argue that firms compete in quantities rather than prices. The second line of attack is aimed at the game-theoretic foundations of the Bertrand reasoning. The assumption of Nash conjectures has been criticized (this type of objection has pre-Nash roots; see Bowley, 1924), and the use of weakly dominated strategies in equilibrium is problematic if 'admissibility' is viewed as a reasonable decision-theoretic requirement to impose on strategic choices (see, e.g., Luce and Raiffa, 1957 (Chapter 13) for supporting arguments). Canoy (1993) discusses many of these references in more depth.

The game we investigate is designed to give the Bertrand model its best shot at

[1] The reason that we do not include 0 and 1 in the strategy sets is that the equilibrium would then not be unique.

not being rejected by the data. If the Bertrand model would fail to perform well under such circumstances, there would be good cause to reject it. The game can be derived from an economic model of price competition with constant returns, homogeneous goods, no capacity constraints, no repeated interaction, and no incomplete information about demand (which is completely inelastic) or costs. The unique Nash equilibrium is strict, and hence does not involve the use of weakly dominated strategies. A bid of 2 is furthermore the unique rationalizable strategy of the game, so the solution has a strong decision-theoretic foundation and Nash conjectures need not be assumed.

We wish to study the behavior of *experienced* participants, and so must let them play the game several times. Following the classic contribution by Fouraker and Siegel (1963), most other studies of experimental price competition cater for experience by letting a fixed group of participants interact repeatedly.[2] However, a drawback with this approach is that a confounding effect is introduced. Since *the same* firms interact repeatedly. opportunities for cooperation of the kind studied in the theory of repeated games (see Pearce (1992) for a general overview, and Friedman (1977) for the application to oligopoly) may be created. We wish to isolate the effects of experience from repeated game effects, and therefore let participants play the game several times *but not facing the same rivals in each round*.

In three out of the four experimental treatments described in this paper, twelve bidders participated. These treatments differed only in terms of how many bidders were matched in each round (two, three, or four). Markets operated for ten rounds. At the beginning of each round all twelve participants placed their bids. We then randomly matched N bidders together ($N = 2$, 3, or 4), resulting in $12/N$ different matchings per round. The actual matching and the entire bid vector were then posted on a blackboard. Note that it was relatively unlikely that two participants would run into each other in two consecutive rounds. The set-up is intended to reduce the impact of repeated game effects and to retain the one-shot character of the Bertrand game while allowing for learning over time.

In all these treatments, behavior differed greatly from the theoretical outcome in the first round. In the $N = 2$ treatment this was also the case in the last round. However, in the $N = 3$ and $N = 4$ treatments the winning bids converged towards the competitive outcome by the 10th round. Somewhat surprisingly, these results are roughly consistent with those reported by Fouraker and Siegel (1963, Chapter 10) for the case of repeated experimental price competition within a fixed group of participants. This suggests that it is experience that has the most important impact on price competition, rather than the build-up of reputation or mutual cooperation that may be possible when a given set of firms interact repeatedly.

However, there is a possible objection to this. Strictly speaking, our design

[2] For overviews of this literature. see Plott (1982, 1989) and Holt (1995).

creates a repeated game too, one with twelve ordinary players plus nature. Maybe the participants are concerned with building a reputation of not being interested in price wars even if the next-period match is stochastic? Maybe such an effect is relevant with a 'small' pool of randomly matched subjects, but not in a larger pool where others will find the probability of being matched with the reputation-builder to be negligible? So, could it be that the results change if there is random matching in a larger group than one with twelve participants? In order to control for this, we include a fourth treatment in which $N = 2$ but with random matching among 24 instead of twelve participants. It turns out that the results essentially do not change, so our aforementioned finding appears to be robust.

The theoretical literature on Bertrand competition does not offer an explanation of these observations. We suggest one that relies on bounded rationality. The idea is to illustrate the disruptive effect of 'noise' on the viability of the Bertrand outcome when there are sufficiently many firms. If with some 'small' probability any firm in the market may bid differently from what the Bertrand model prescribes, then deviations from the Bertrand outcome can depend on the number of firms.

2. Experimental procedure[3]

We now refer to the four treatments as 2, 3, 4, and 2*. We ran two sessions of each treatment. In these sessions groups of respectively two, three, four, and two students were matched in each round, with random matching among twelve students in treatments 2, 3, and 4, and 24 students in treatment 2*.

The students received an introduction, were told they would be paid 7.50 Dutch guilders[4] for showing up, and were randomly assigned private 'registration numbers' with an additional student becoming a 'Monitor'[5] who checked that we did not cheat. They received instructions (see the Appendix) and ten coupons numbered 1, . . . , 10. Each student was asked to write on the first coupon her registration number and bid for round 1. Bids had to be between 2 and 100 'points,' with 100 points being worth 5 guilders. Each students put her coupon in a box carried by the Monitor. In treatments 2 and 2* the Monitor randomly took two coupons from the box and gave them to the experimenter, who announced the registration number and bid on each coupon. If the bids were different, the low bidder won as many points as her bid and the other bidder won 0 points. If the bids were equal, each bidder won half of the bid. The Monitor wrote this on a

[3] This is a somewhat shortened account relative to the presentation in Dufwenberg and Gneezy (1999)

[4] At the time of the experiment, $1 = 1.7 Dutch guilders.

[5] The Monitor was paid the average of all other subjects participating in that session.

Table 1
The bids in session 2a ($N=2$)[a]

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10
S1	49	34	24*	22*	16*	15*	100*	100	60	20*
S2	15*	20*	25*	20	19	19	14*	9*	19*	19*
S3	39	39	30	35	40	19	100*	99	99	99
S4	40*	29*	28*	26	18*	16*	13*	80*	40	28*
S5	10*	20*	29	24*	19*	15*	14	100	79	79
S6	40*	30*	26	20*	21	15*	14*	19*	50*	60*
S7	23*	29	31*	24*	28	20*	14*	17*	40*	50
S8	46	32	24*	26	18*	100	20	35	88	66
S9	40	38	25*	25	20*	20	15	40	100	40*
S10	40*	40	35	19*	19*	18	40	39	35*	60*
S11	20	25*	20*	19*	17*	15*	12*	12*	20*	39
S12	40*	35*	30	23*	25	16	14*	18*	39*	35
Average bid	33.5	30.9	27.3	23.6	21.7	24.0	30.8	47.3	55.8	49.6
Average winning bid	29.7	26.5	25.3	22.0	18.1	16.0	35.1	25.8	33.8	37.8

[a] * indicates a winning bid.

Table 2
The bids in session 4b ($N=4$)[a]

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10
S1	34	34	34	34*	34	33	23	21	5*	2*
S2	15*	13	10	10	10*	28	12*	12	10	5
S3	10	10*	15	100	30*	99	25	8*	2*	2*
S4	2*	50	19	100	100	100	9*	9*	3*	2*
S5	2*	14	19*	100	100	100	100	34	10	100
S6	19	21	8*	13	98	74	42	9*	7	4*
S7	40	35	25	100	100	100	100	100	100	100
S8	49	10*	9	100	100	100	28*	24	3*	8
S9	35	10*	20	9	100	30*	97	15	5	2*
S10	100	100	100	10*	100	100	100	25	20	6
S11	48	20	9*	11	75	44*	35	20	16	10
S12	15	8*	10	8*	10*	10*	20	5*	5	5
Average bid	30.8	27.1	23.2	49.6	71.4	68.2	49.3	23.5	15.5	20.5
Average winning bid	6.3	9.5	12.0	17.3	16.7	28.0	16.3	7.8	3.3	2.4

[a] * indicates a winning bid.

blackboard, took out another two coupons, etc., until the box was empty. Then the second round was conducted the same way, etc. After round 10, payoffs were summed up and the students were paid privately.

Treatments 3 and 4 were carried out the same way, except the assistant each time matched three or four students, respectively, instead of two.

3. Results

3.1. The impact of market concentration (Sessions 2, 3, 4)

We refer to the two sessions of treatment 2 as 2a and 2b, etc. To save space, we here report the complete data only from two illustrative sessions: 2a and 4b. See Tables 1 and 2. The complete raw data set from all sessions is given in Dufwenberg and Gneezy (1999) (along with a somewhat more detailed discussion of the results) and can also be obtained from the *IJIO* home page. Average winning bids and average bids for all session are plotted in Figs. 1–6.

We start by discussing the behavior in round 1, because at this stage no elements of learning or experience exist. It is clear that the Bertrand outcome was not achieved in this round in any session. The average bid (average winning bid) was 33.5 (29.7) and 41.8 (23) in sessions 2a and 2b; 26.4 (21.5) and 30.1 (16.5) in sessions 3a and 3b; and 33.1 (24) and 30.8 (6.3) in sessions 4a and 4b. We also perform a statistical test of whether the bids in different sessions come from the same distribution. We consider each of the (15) possible pairs of sessions separately, use the non-parametric Mann–Whitney U test based on ranks, and

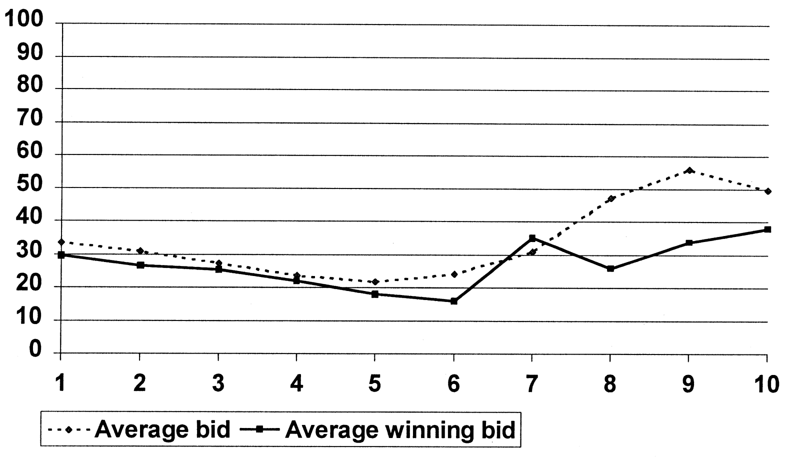

Fig. 1. Session 2a.

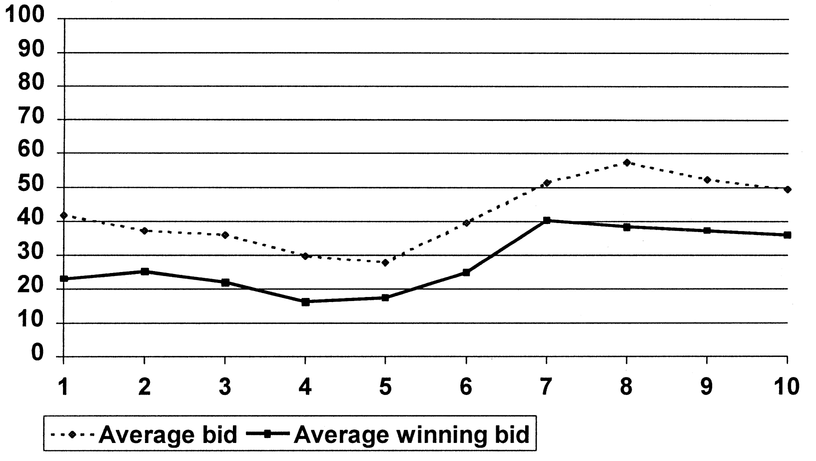

Fig. 2. Session 2b.

cannot for any pair reject (at a 95% significance level) the hypothesis that the observations come from the same distribution.

When comparing the convergence of bids in later rounds, however, we observe great difference between treatments. In sessions 2a and 2b, we see a slow decrease of the average winning bid for a few initial rounds, but then there is an upwards tendency of the average winning bid. It is clear that no convergence to bids of 2 is observed. The bids in the sessions of treatment 2 were much alike in round 10; the

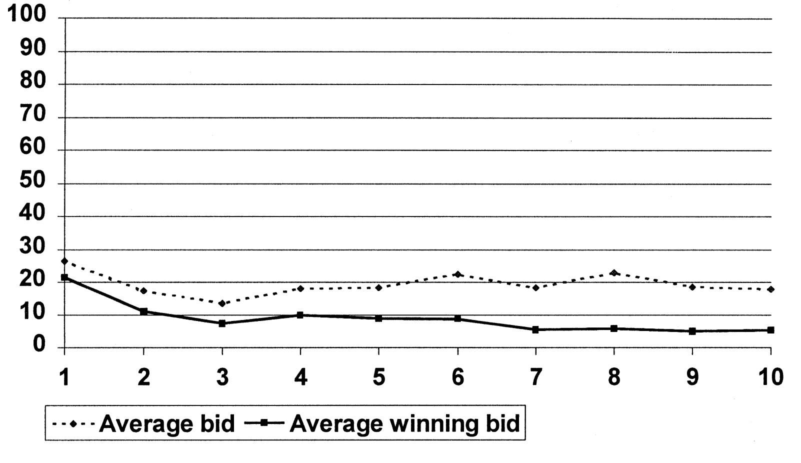

Fig. 3. Session 3a.

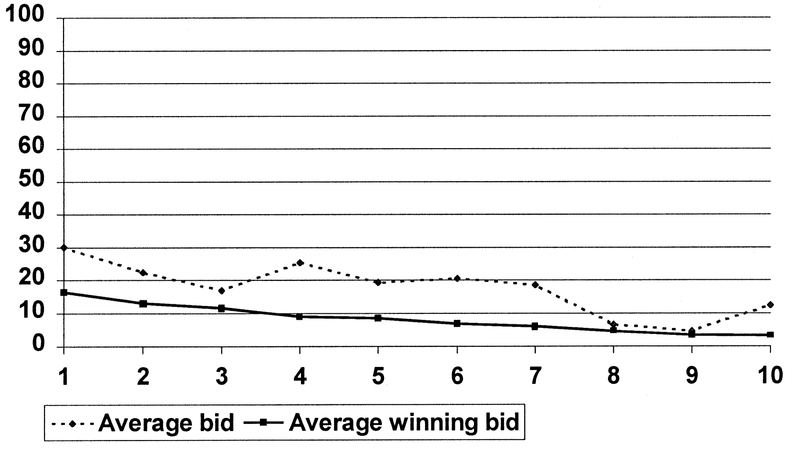

Fig. 4. Session 3b.

average bids were 49.6 and 49.3 in sessions 2a and 2b respectively, and the average winning bids were 37.8 and 36.[6,7]

In sessions 3a and 3b we see a steady decrease in the average bids as well as the average winning bids from round 1 to round 10. Like in the case of treatment 2, the bids in both sessions were much alike in round 10; the average bids were 17.9 and 12.3 in sessions 3a and 3b respectively, and the average winning bids were 5.3 and 3.2.

In session 4a we again see monotonic decreases in the average bid as well as the average winning bid from round 1 to round 10. It is striking that already in round 8 the average winning bid was 2, where it stayed till the end of the session. Session 4b is, however, quite different from Session 4a in the first rounds. When observing Fig. 6 we see a hump in the average bid. In fact, we see from Table 2 that the average bid in round 5 was 71.4 (which is the highest average bid in a single round in the entire experiment), with 6 out of the 12 participants bidding 100![8] A similar

[6] Unlike the case of first round behavior, it is not appropriate to use the Mann–Whitney test, because the assumption that all observations are independent is not justified.

[7] We note an additional interesting observation: One participant in session 2b used a constant bid of 2 throughout the experiment (as seen in the data set given on the *IJIO* home page). Of course, this bid was 'strange' given the fact that the average bid in round 10 was almost 50 (and the next lowest bid was 38). This behavior was not enough to move the other bids to the neighborhood of 2.

[8] It appears as if participant S10, who chose 100 also in the first three rounds, was attempting to 'signal' a willingness to cooperate with the others. We note that related observations have been made in experimental oligopoly studies with repeated interaction among a fixed group of firms. See Fouraker and Siegel (1963, pp. 185–88), Hoggatt et al. (1976), and Friedman and Hoggatt (1980). See Plott (1982, pp. 1513–17) for a discussion. In future research we plan to investigate the role of price signals within a random matching set-up, by considering treatments where information about losing bids is not given.

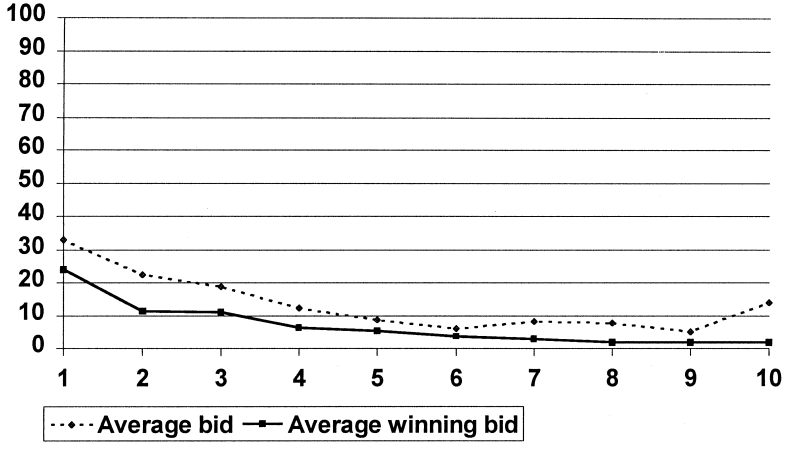

Fig. 5. Session 4a.

trend was observed in the average winning bid; it rose from 6.3 in round 1 to 28 in round 6. However, from that round on it seems as if participants ‘gave up’, and the average winning bid decreased steadily to 2.4 in round 10. with 8 out of the 12 participants bidding between 2 and 6. Although the outcome in the intermediate markets was very different between sessions 4a and 4b, the results of round 10 show almost total convergence of the average winning bid in both sessions to the equilibrium. The average bids were 13.9 and 20.5 in sessions 4a and 4b respectively, and the average winning bids were 2 and 2.4 in the respective sessions.

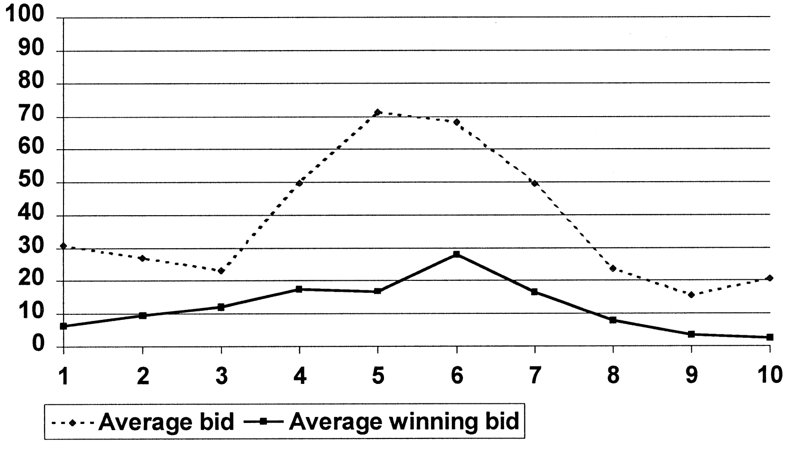

Fig. 6. Session 4b.

It should be stressed that while there seems to be convergence towards the equilibrium for winning bids in treatments 3 and 4, the tendency of convergence over-all is less strong. In many cases certain losing bids were well above the equilibrium level. Another related observation is that while the average winning bids in treatments 3 and 4 were at its lowest point in round 10, the average bid actually went up a bit in round 10 in three out of four sessions. It is not clear how this end game effect can be explained. One speculation is that participants were frustrated as they realized that due to the low level of bidding they were not making much money in the experiment, and so decided to gamble a bit in the last round.

To summarize, the market outcomes in round 1 are similar across sessions. It is also the case that in all sessions the outcomes converge, and relatively little fluctuation is observed at the end of the experiment. However, while the round 10 outcomes in the two sessions of treatment 2 are far from equilibrium, the round 10 winning bids are relatively close to the equilibrium.

3.2. Duopoly with random matching in a larger group (Session 2)*

The raw data of the sessions 2a* and 2b* is given in Dufwenberg and Gneezy (1999) and can also be obtained from the *IJIO* home page. The average winning bids and the average bids are plotted in Figs. 7 and 8.

We first discuss behavior in round 1 in which, like in the other treatments, it is clear that participants did not play the equilibrium. The hypothesis that the bids in sessions 2a* and 2b* come from the same distribution is not rejected. Comparing,

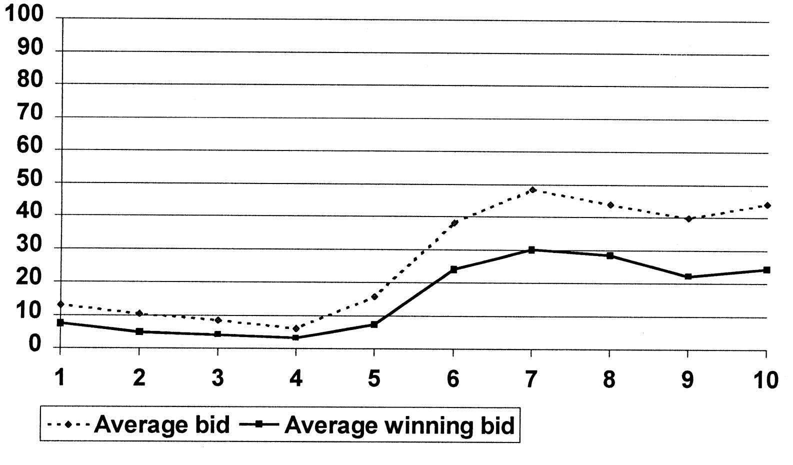

Fig. 7. Session 2a*.

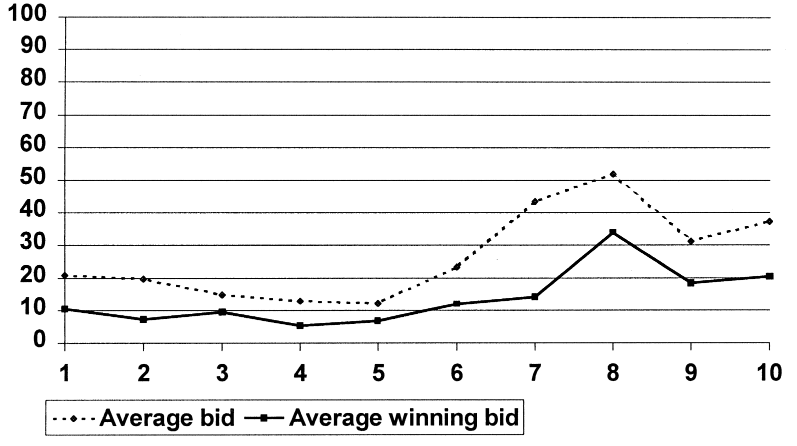

Fig. 8. Session 2b*.

however, the round 1 bids in treatment 2* with the corresponding bids in the other treatments we see significantly (at a 95% significant level) lower bids. The average bid (winning bid) was 20.8 (10.4) and 13.1 (7.4) in session 2a* and 2b* respectively.

We now consider the convergence of bids in later rounds, focusing on the comparison between treatment 2 and treatment 2*. Like in treatment 2, we again observe a decline in bids at the first stages' almost to equilibrium. But then the bids start to increase, much like in session 2. The average bid (winning bid) in round 10 was 37.5 (20.5) in treatment 2a*, and 44.1 (24.3) in treatment 2b*. These values are somewhat lower than the corresponding one in session 2, but they are still far away from the equilibrium level.

Summing up, although the results in treatments 2 and 2* are quantitatively different, they are qualitatively similar. In particular, no convergence to equilibrium is observed. The question why the size of the group influences the results at all, and what would happen if more rounds of play were allowed, is. however, left for future research.

3.3. A comparison of total payoffs across treatments

Finally, we compare the profits of participants in the different treatments. The average profit per participant was 138, 43. 48, and 74 in treatments 2, 3, 4, and 2* respectively. It is interesting to note the difference in average profits between treatments 2 and 2*. It appears that the main cause of this is the different bids at the *initial* rounds of the experiment.

4. Discussion

In this paper we study how the number of competing firms influences the fierceness of competition in a Bertrand oligopoly game. The theoretical prediction is clear; all firms should submit the lowest possible bid irrespective of how many firms are matched. However, when we tested this model experimentally, we found that at the initial stage, competitors set prices higher than in the Nash equilibrium. In subsequent rounds the winning bids (but not all bids) typically converged rather rapidly towards the theoretical prediction when groups of three or four competitors were matched.[9] However, when only two competitors were matched prices remained much higher than the theoretical prediction.

It is striking that these results accord well with those reported by Fouraker and Siegel (1963), who let fixed sets of two or three participants interact repeatedly. Our design differs crucially from theirs in that we have random matching of opponents between rounds, in order to isolate the effects of experience from the opportunities of cooperation that may occur in a repeated game. Nevertheless, in our case, as in Fouraker and Siegel's, duopolists exhibit more cooperative behavior than do triopolists.

Our findings suggest that learning is important, since behavior was not constant across time in all treatments. However, it is puzzling that the participants seem to come close to learning to play the equilibrium only when the number of competitors is sufficiently large. Our primary goal with this paper is not to solve this puzzle, but to document relevant experimental evidence. We conclude, however, by suggesting a reason why one might expect that the number of firms will have important bearing on the viability of the Bertrand equilibrium. We do not aim to provide a quantitatively exact model that fits the experimental data, but rather to hint at a phenomenon which may be qualitatively informative. Providing a quantitatively more accurate model may be a feasible research task, but it is beyond the scope of the present paper.

The profile where all firms bid 2 is the unique equilibrium of the Bertrand game we consider. A firm which unilaterally deviates from the equilibrium reduces its profit. However, in reality it seems highly unlikely that each firm is fully convinced that every other firm will behave in accordance with the equilibrium. Examples abound of irrational activity in economically important situations. Moreover, the consequences of irrationality may be large, even if the probability that individual decision makers are irrational is very small. Two relevant examples

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[9] The predictability of the Bertrand model in these cases is all the more striking in that subjects ended up making so little money. While in the experiment some strategy profiles were amply rewarded, *in equilibrium* the payoffs were not very salient. Though this is a typical feature of a Bertrand game, it is from a methodology of experimental economics point of view an undesirable feature, which one might have suspected would undermine the attraction of the equilibrium outcome.

are Kreps et al. (1982) model of strategic interaction in the finitely-repeated prisoners' dilemma when rationality is not common knowledge, and 'noise trading' in financial markets (see De Long et al., 1990). We now illustrate how a little irrationality can upset the viability of the Bertrand equilibrium if a high enough number of firms interact.

Suppose that in the context of our experimental game the firms believe that with a small probability $\varepsilon > 0$ any given other firm is an irrational 'noise bidder' who always simply submits a bid of 100. It is easy to verify that for a range of rather small values of ε there cannot be an equilibrium where all firms that are not noise bidders bid 2, as long as not too many firms are matched. Let N denote the number of firms matched. Consider the decision problem faced by a non-noise bidding firm that believes with probability one that all other non-noise bidding firms will bid 2. It is clear that the firm should not submit a bid from the set $\{3, \ldots, 98, 100\}$, since each bid in this set does strictly worse than a bid of 99. Let p_x be the probability that x firms out of the $N-1$ other ones bid 2. (Note that $p_0 = \varepsilon^{N-1}$ and that ε^{N-1} is decreasing in N). One now sees that the firm should bid 99 if $\Sigma_{x\in\{0,\ldots,N-1\}} 2p_x/(x+1) < 99p_0$, and that the firm should bid 2 if the inequality is reversed. Given the assumptions, if N is large enough 2 is the optimal bid irrespective of the value of ε. However, for a range of rather small values of ε, a bid of 99 is optimal if N is not too large. As an example, note that with $\varepsilon = 0.05$ and $N \geqslant 3$ a bid of 2 is optimal, but with $\varepsilon = 0.05$ and $N = 2$ a bid of 99 is optimal.[10]

To assume that all noise bidders bid 100 is clearly not realistic, but the main point of the argument goes through for a variety of other assumptions about the nature of noise bidding (e.g. that it is uniformly distributed between 2 and 100). The important insight from the example, which is supported by the experimental findings, is that the viability of the Bertrand outcome depends crucially on the number of firms being matched.

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[10] The purpose of the example is to show that a bid of 2 by all (non-noise bidding) firms is not an equilibrium with $\varepsilon = 0.05$ and $N = 2$. We do not suggest that a bid of 99 by all (non-noise bidding) firms is an equilibrium. Clearly it is not.

Appendix A. Instructions for treatment 2

In the following game, which will be played for 10 rounds, we use 'points' to reward you. At the end of the experiment we will pay you 5 cents for each point you won (100 points equals 5 Dutch guilders). In each round your reward will depend on your choice, as well as the choice made by one other person in this room. However, in each round you will not know the identity of this person and you will not learn this subsequently.

At the beginning of round 1, you are asked to choose a number between 2 and 100, and then to write your choice on card number 1 (please note that the 10 cards you have are numbered 1,2, . . . , 10). Write also your registration number on this card. Then we will collect all the cards of round 1 from the students in the room and put them in a box.

The monitor will then randomly take two cards out of the box. The numbers on the two cards will be compared. If one student chose a lower number than the other student, then the student that chose the lowest number will win points equal to the number he/she chose. The other student will get no points for this round. If the two cards have the same number, then each student gets points equal to half the number chosen. The monitor will then announce (on a blackboard) the registration number of each student in the pair that was matched, and indicate which of these students chose the lower number and what his/her number was.

Then the monitor will take out of the box another two cards without looking, compare them, reward the students, and make an announcement, all as described above. This procedure will be repeated for all the cards in the box. That will end round 1, and then round 2 will begin. The same procedure will be used for all 10 rounds.

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