Wars of Attrition in Experimental Duopoly Markets*

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I. Introduction

Oligopolists often bear large fixed costs. These fixed costs can change, for example through rising property rents, increased taxes, and renegotiated labor contracts. In this paper, we imagine oligopoly market structures that are identical except for the level of fixed costs. Does this level influence the strategic behavior of firms? Since marginal profits are unaffected by fixed costs, one answer is that there should be no change in behavior. Alternatively, firms may seek more cooperative outputs in order to maintain profits. Enough cooperation can even generate bigger profits. But if fixed costs rise to a level where there are too many firms in the market for any seller to earn an adequate profit, a price war could result. Attrition leaves more profits for the survivors and the higher fixed costs are an increased barrier to later entry. Thus, it also can be argued that greater fixed costs engender less cooperation among rivals because agents are fighting over a smaller profit pie.¹

Through the use of experimental markets, this paper shows that fixed costs do have an influence on the level of cooperation after they have reached a relatively high level in a duopoly market structure. Subjects (as rivals) engage in price wars. Outputs are kept large—larger than the Cournot level—until a rival exits the market. By contrast, such behavior does not take place when fixed costs are relatively low. This difference in behavior at low and high fixed cost levels

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1. An increase in fixed costs will cause industry profits to decline, and there are discussions in the industrial organization literature about how declining industry profits in general affect the behavior of oligopolists. Scherer [26] presents the widely accepted arguments that firms are less cooperative during times of low profitability, and therefore price wars between rivals are a more likely event. Green and Porter [11] show that price wars break out when demand, and consequently profit, is unexpectedly low. Porter [21] later found support for this model with data from a railroad cartel in the 1880s. When profitability is cyclical, Rotemberg and Saloner [24] argue that price wars are more likely during industry booms (rather than busts) in a business cycle. During good times the benefit from cheating on an implicit agreement is greater than when profits are down, and if future punishment comes during periods of low profits the cost of cheating is relatively small. This model depends on periods of booms and busts and creates them with random demand shocks that are uncorrelated over time. By contrast, Haltiwanger and Harrington [12] analyze a model for which demand shocks are positively correlated. They also find that firms have the strongest incentive to cheat on implicit price agreements when profits are high, but specifically when the cycle has passed a boom peak and is beginning to turn down.

is statistically significant. We emphasize that price wars are not observed until fixed costs are relatively high. At low levels, i.e., periods of relatively high profitability, a change in fixed costs seems to make rivals slightly more cooperative, but this effect is not statistically significant.

The experimental markets are in fact a repeated game in which two symmetric quantity choosing agents face each other over numerous market periods. Behavior is measured by the choices agents make on a payoff table. Row values represent all the output choices rival i can make; column values are the possible output choices of rival j . The intersection of the i th row and the j th column determines the profit of seller i . Agents simultaneously make choices from identical tables and know their rival has the same table, so that payoffs are common knowledge. Subjects are not told when the experiment will end, so the games have an unknown endpoint.²

In these laboratory markets it is possible for agents to make sufficiently high output choices that cumulative profits become negative for at least one agent. If a subject's cumulative profits become negative they are excused from the experiment and the surviving rival is allowed to then behave as a monopoly.³ Because of bankruptcy, low profitability alters the dynamic payoff structure to agents. In particular, bigger fixed costs reduce the time necessary to drive a counterpart out of the market, and increase the number of periods for monopoly earnings. But other types of behavior can be observed between subject pairs. It is possible that, rather than causing a "war of attrition," larger fixed costs have no impact on choices, as already suggested, or they may even encourage firms to seek higher joint payoff levels. In the latter case, restricted market outputs raise the profits of both sellers, which counteracts the higher costs. We believe oligopoly behavior in the face of rising fixed costs is an empirical issue; the equilibrium of a game may be left unchanged or altered in a number of ways.

Different fixed costs, and the resulting levels of profitability, do not affect the oneshot Nash solution of the game, i.e., the Cournot equilibrium, but this is not to say that agents either do or will exhibit Cournot behavior under any cost conditions. Indeed, it is widely known that the Nash outcome of the stage game is not a good predictor of behavior in a repeated game. The "Folk Theorem" suggests many outcomes that Pareto-dominate the Cournot solution can be an equilibrium of the supergame [7; 10]. In theory, this is accomplished by players cooperating until cheating on an implicit agreement occurs. Following such a defection, each player moves to a punishment phase, where each receives lower payoffs for a period of time, which may be for the duration of the game.⁴ It is possible in this context that defection triggers a war of attrition. Thus,

2. With a finite horizon, the probability that the game will continue may be interpreted as the discount factor. If the factor is sufficiently large, more cooperative outcomes than Cournot/Nash can occur in an equilibrium. The discount factor may also include the belief there is a positive probability that a rival is irrational, as in Kreps and Wilson [17] or Fudenberg and Maskin [10].

3. Choosing to invoke a price war places agents in a war of attrition, which have multiple asymmetric subgame perfect Nash equilibria. These equilibria call for agents to plan on exiting at different moments, so that ultimately one firm exits and one does not. There also exists a unique symmetric mixed strategy, wherein each firm plans on exiting with a given probability (equal to the rival's chosen probability of exit) in each period. See the surveys by Fudenberg and Tirole [9, Chapter 4]; Rasmusen [22, Chapter 3]; and Tirole [33, 311–14 and 380–84].

4. For more description of the trigger strategy see Friedman [7, 85–103] and Tirole [33, 246]. Shorter punishment periods than the Nash choice for the duration of the game are possible. Friedman discusses viable trigger strategies for repeated games with both an infinite and finite horizon. Most of these models assume the game has an infinite horizon, though. Benoit and Krishna [2] also describe how trigger strategies can support cooperative outcomes in a game with a finite horizon. Rasmusen [22] points out that when the endpoint is uncertain the game is similar to one with an infinite horizon. It can be argued that because the endpoint in our experiments is unknown and carries a subjective probability of ending, our experiments are not stationary games. In other experimental designs we have altered the information given subjects about the end of the game in two ways: (1) subjects are told that after a specified period (e.g., period 35) the experiment has a known probability p of continuing to the next period, and (2) at the outset subjects are informed there

an agent may depart from a cooperative strategy in order to drive the other from the market, or the punishment phase of the strategy may leave only one survivor.

II. War of Attrition Models

Tirole [33] argues that modern analysis on wars of attrition originated in the theoretical biology literature with Maynard Smith [31] and Bishop, Cannings, and Smith [3]. The fundamental idea, beginning with Darwin [5], is that when the survival of a species is threatened, attrition leaves only the most fit, and it is the most fit that hasten the demise of the weak in order to survive. With respect to markets, this idea may have preceded Darwin's treatise. For example, in their classical works on markets, Adam Smith [30] and David Ricardo [23] noted in different contexts that low cost producers will naturally drive out high cost producers as competition fosters market efficiency. Later, Joseph Schumpeter [28] wrote that entrepreneurship would leave even the most secure monopolies vulnerable to obsolescence. In effect, no firm is safe from threatened extinction.

Several papers provide technical models of wars of attrition, starting with Kreps and Wilson [17] and Fudenberg and Tirole [9]. In Tirole [33, 311–15, 380–84] fixed costs play a crucial role in the firm's decision to continue with the status quo or fight to the death over greater control of the market. Modeling the exit decision, Tirole shows that the higher is a firm's fixed cost, the sooner will there be a war of attrition and the more quickly a weaker firm will exit as a casualty. Relatively high fixed costs move duopolists toward war because profits are low and survival is threatened. Also because it takes less time to force a rival's exit, there are greater discounted returns for the survivor provided there is no threat of future entry.

But wars of attrition are inherently risky ventures. The time at which one firm will exit is generally unknown because the financial reserves and resolve of the weaker firm are unknown. Market demand and future costs also may be subject to random shocks. Finally, if we consider a game theoretic setting, play could be finite with a random endpoint reflecting a number of real market risks. Such risks include the uncertain nature of the product life cycle or an unexpected innovation that suddenly reduces market size. Thus net gains from a war of attrition may plausibly be regarded as stochastic, and so risk averse agents will require a large expected premium over current returns before they instigate a war of attrition. Nevertheless as survival is threatened by increasing fixed costs, a war of attrition becomes a more likely event in an industry [33, Chapters 8 and 9].

Wars of attrition bear a resemblance to predatory behavior.⁵ Both concepts involve one firm attacking another firm to induce exit from the market. Both suggest that the period of war (or predation) be carried out in the short run. Otherwise the future gains may not be sufficient to cover the wartime losses. One important difference between predatory behavior and wars of attrition, however, is that predation by definition entails a dominant firm driving smaller firms (or entrants)

is some small fixed probability of ending in each period. For the same payoff tables we have not observed changes in behavior as the instructions about the game regarding the endpoint have changed [18].

5. Predatory behavior occurs when "the firm forgoes short-term profits in order to develop a market position such that the firm can later raise prices and recoup lost profits." This definition is in *Janich Bros. v. American Distilling Company*, 570 F.2d 848 (9th Cir. 1977), *cert. denied*, 439 U.S. 829 (1978). Alternative interpretations of predation can be found in Areeda and Turner [1], Joskow and Klevorick [15], Saloner [25], Scherer [27], and Williamson [34]. Isaac and Smith [14] could not generate predatory pricing behavior for 11 subject pairs. Rather than using payoff tables, their design had subject pairs choose quantities without knowing demand and in most cases without knowing a rival's cost.

from the market in order to protect or build upon market share. A war of attrition is a fight for survival that could take place between any rival firms, be they of the same or different size. Before the war begins no agent has high or above-normal profitability. It is the dismal future that prompts the war.

It has been difficult for the theoretical literature to describe the market conditions that cause wars of attrition. Furthermore, it is not clear that such wars have been observed in naturally occurring markets [21]. Indeed there is noticeable skepticism among economists that wars of attrition or acts of predation should ever be observed among market rivals [32]. Carlton and Perloff [4] review several cases, and conclude the evidence supporting any sort of price war behavior is weak. On the existence of these wars they write [4, 407] that allegations “often reflect the complaint of one rival about another’s fierce (and socially desirable) competition.” The motivation for selling large quantities at low prices generally can be attributed to fiercely competitive behavior; after all, competition is a fight for survival. In uncontrolled market environments there can be many reasons for different levels of competitive behavior, and the level of competitive behavior itself can be difficult to observe and measure from field data.

We believe the effective use of field data to study the market conditions that induce aggressive behavior on the part of rivals is hindered by a variety of factors that cannot be controlled. For just one example, shifts in underlying basic demand or cost conditions can be both deterministic and random, and so present substantial difficulties for constructing good estimators. An alternative to collecting field data is to obtain data from laboratory markets. Our laboratory markets are based on a simple design that captures the essential ingredients of an oligopoly environment; the environment is controlled. In these markets there are no shocks to the system, no vertical or horizontal influences, no escalations in cost or changes in technology, no threat of entry. By using a sufficiently simple framework, the effects resulting from a change in some preselected treatment variable across markets can be isolated. As we explain below, this allows the rigorous testing of hypotheses relating to the behavior of agents as fixed costs are altered.

III. Data From Controlled Duopoly Markets

We examine the actual behavior of decision makers when payoff entries differ by the addition or subtraction of a constant in this section of the paper. The choices made by 116 subjects, or 58 duopoly pairs, are tracked. Subject pairs are presented with payoff tables that have entries differing only by a fixed cost. The payoff tables have been designed to capture the essential features of a duopoly market environment with fixed costs. The payoff entries for player i are derived from the profit function:

$$\pi_i = q_i[(1800 - 15(q_i + q_j))/289] - F, \quad (1)$$

where the term in brackets is inverse demand.⁶ The output choices made by players $i, j = 1, 2$ are q_i and q_j . Fixed costs (F) are symmetric and set at five different levels ranging between 46.647

6. Our use of payoff tables represents incentives to players in terms of the normal form for the stage game. James Friedman draws close parallels between repeated games and duopoly market environments, and as a consequence has used game theory to develop the theory of strategic behavior in duopoly and oligopoly markets. For more reading see Friedman [8, Chapter 9] and Friedman [7]. All choices in the payoff tables were scaled to integers between 1 and 30, but represented output levels of 25 to 54 in the profit function. The function in eq. (1) gives payoffs in cents; to convert these into payoffs in tokens we multiplied by 10.

and 90.830. Earnings, as quoted in the payoff tables for subjects, were measured in a fictitious currency called tokens. Tokens were exchanged at the rate of 1,000 tokens = \$1.00 at the end of the experiments. This allowed earnings during the experiment to be measured to the tenth of a cent.

This profit function can be easily solved to determine the joint profit maximizing output choices, the symmetric Cournot/Nash choices, and the zero profit choice level. A perfectly collusive pair of subjects, acting as a single monopoly, would make choices summing to 12; the symmetric combination is (6,6). The symmetric one-shot Cournot/Nash equilibrium entails the combination (16,16).⁷ While both of these combinations are independent of the level of fixed costs, the zero profit outcome does depend on fixed costs. At the lowest fixed cost level of 47.647, the choice combination (27,27) would yield the symmetric zero payoff. At the highest level of 90.830, the zero payoff would occur at (11,11). Because the Cournot choice levels remain at (16,16), Cournot behavior would give subjects negative earnings when fixed costs were 90.830. A reduced copy of one table is attached at the end of this paper. This table has fixed costs set at 63.426; subjects earn zero profits if they choose (23,23), and a number of choice pairs give one or both of the players a negative payoff.

All of the experimental markets were created by using identical instructions and procedures. Subjects were recruited from beginning economic classes at the University of Wyoming. They reported to one of two reserved classrooms, where the instructions were read aloud as everyone followed along with their own copy. Questions were taken and one practice period was conducted with a sample payoff table different from the one actually used in the experiment. In the practice period a monitor chose the counterpart value while all subjects simultaneously chose their row value from a sample payoff table.

To get the experiment underway subjects were randomly split into two groups of equal size. One group was moved to another room and these people became an anonymous counterpart to those who stayed. During each choice period a subject wrote his or her choice on a record sheet and a colored piece of paper. The colored slips were then exchanged between rooms, and earnings for the period were tabulated from the payoff tables. Subjects were not told the number of periods in the game nor how much time the experiment would take.⁸

Every subject was given a starting cash balance on their record sheet to cover potential losses. This balance was \$3.00 in experiments 1 through 5. In experiment 6 it was \$5.00, because so few players survived in the experiment 5 market; we elaborate on this point below. If a subject's balance went to zero or below, then he or she was asked to leave the experiment with a \$2.00 participation fee, which was paid to every person regardless of their earnings. The instructions made it clear that the remaining player in the game would then select both the row and column value and collect the earnings of the player who was forced to leave in addition to their own. The player who was left in the game was free therefore to behave as a monopolist that controlled two identical production plants. In all payoff tables the remaining player's payoff was maximized at (6,6).⁹

7. Because of rounding, there are two additional (asymmetric) Cournot equilibria: (15,17) and (17,15). Our view is that the symmetric combination is focal.

8. Indeed subjects were really facing the end of a treatment. After the number of periods shown in Table I, the experiment did not end, but subjects were given new tables. This change in payoffs was unannounced and designed to study other behavioral propositions, i.e., the importance of history to current strategies. The initial games last approximately one hour. During this time average earnings were about \$13.00. Subjects were seated for a total of two hours in each experiment.

9. This design feature is a relatively simple way to provide subjects with monopoly opportunities. An alternative would be to include a column for 0 output and to expand the number of rows to include the monopoly output. While

Table I. Fixed Cost Treatments in Experiments 1–6

Experiment	Fixed Costs	Periods	Number of Pairs	Average Choice	Standard Deviation
Exp 1	63.4256	25	10	25.558	13.540
Exp 2	47.6471	25	9	23.906	12.867
Exp 3	70.0692	25	11	23.269	14.305
Exp 4	55.9516	25	11	23.556	12.979
Exp 5	90.8304	14	6	35.571	18.406
Exp 6	90.8304	20	11	35.111	16.509

Allowing a player to be driven out of the market gave subjects an opportunity to effectively enter a price war. In the short-run a subject in these experiments could choose large values incurring zero or negative payoffs for both players. The player who did not match this move or had a lower balance was forced out of the market. This strategy was not seriously employed in the first four experiments. But in experiment 5, when subjects faced payoff tables with high fixed costs and negative Cournot profits, a number of subjects were forced from the market. In this experiment half of the 12 subjects who started the experiment were forced to leave before the end of the treatment; in one of the markets two subjects reached a zero or negative balance in exactly the same period. This experiment was terminated earlier than the other previous four experiments. The balances of those subjects who had not gone bankrupt by period 14 were low. We believe that if this treatment had continued through to period 25 all of the markets would have had one or zero agents left.

Since only one subject pair from experiment 5 remained at the end of period 14, we were left with an inadequate data set. For time series analysis we desired more observations. A way to keep subjects in the market longer is to raise the beginning balance. Of course, giving subjects deeper pockets could influence the decision to begin a price war. But if there was an effect, it would most likely be to discourage the aggressive behavior observed in experiment 5, since the bigger balance combined with an unknown endpoint in the game reduces the probability of a successful war. In the interest of obtaining a data set of satisfactory length, we chose to run experiment 6 using the same payoff table as in experiment 5, but giving subjects a \$5.00 starting balance. The data show there were still price wars, but the deeper pockets given to players in experiment 6 led to a smaller proportion of subjects leaving the experiment. In period 19 one of the twenty-two subjects was forced to leave the experiment before its end; this was the only subject bankrupted. Aside from the potential influence on the frequency of price wars, the reader may wonder if these deeper pockets might affect a subject's choice behavior in general. If this were the case, we would expect a notable difference in the mean choices in experiments 5 and 6. As we show in Table I, this was not the case.

Table I summarizes the fixed cost levels used in each experiment, the starting balance, the number of periods subject pairs made choices from the payoff table, the average choice for subject pairs during that experiment, and the standard deviation of those choices.¹⁰ In experiments 1

this could have introduced more flexibility to monopoly payoffs, allowing for returns to scale, it also would significantly increase the size of the payoff table.

10. Calculation of this average is straightforward: We summed all pair choices in the session and divided by the total number of choices. In the experiments without any bankruptcies the number of observations used equals the number of pairs times the number of periods. In the experiments where bankruptcies occurred, we retained only those observations where both subjects were still present. In both applications, the procedure is equivalent to forming the mean choice for each subject pair across all retained observations, and then computing the average of these means across all subject pairs in the experiment.

Figure 1. Average Paired Choices, Treatments 1–4

through 4 subjects faced relatively low levels of fixed costs, while subjects faced high fixed costs in Experiments 5 and 6. The last two columns in Table I show that choice behavior in the first four experiments is similar, but that it differs from behavior in the last two experiments. Average choices are large in experiments 5 and 6, indicating the presence of price wars. In addition, choices in experiments 5 and 6 are quite similar on average, which suggests that the higher balance given to subjects in experiment 6 did not substantially alter behavior. In particular, it does not appear to have created a more cooperative environment.

A more detailed view of the data from the first four experiments is summarized in Figure 1. Average choices for all subject pairs are plotted in each period through period 25. In the figure we use the notation “AV” to refer to average choice, while “FC1” through “FC4” correspond to the fixed cost levels in Table I. The data show that experiments 1 through 4 have much in common. Average market choices in a period begin relatively high, close to the Cournot choice level of 32. In all of the experiments a 95% confidence interval would include the Cournot choice for some of the beginning periods. But the average choice levels steadily move downward over time. They settle somewhere between the Cournot and monopoly (collusive) choice level of 12 units after about period 15, and remain there for the duration of the experiment. For nearly all periods after period 15 a 95% confidence interval would not include the Cournot or collusive levels; the bounds would be somewhere in between these possible equilibria. No discrete shifts in behavior are noticeable across the time series as fixed costs are raised across these first four experiments, and they all have a trend such that over time the average paired choice declines. This pattern of increased cooperation and stability in the market is supportive of the assertion that subjects reach an implicit agreement that is more cooperative than the one-shot Nash choice.

Market behavior in experiments 5 and 6 is considerably different than the first four experiments. Recognizing that the beginning balances are different, the dashed plot (AV56) in Figure 2 is the average choice level for all *subject pairs* in experiments 5 and 6 (the choices of monopoly agents are not included). In both of these experiments the average subject earns losses. Also, a 95% confidence interval would cover the Cournot choice level in all periods. There is no dis-

Figure 2. Average Paired Choices, High vs. Low fixed costs

cernable downward trend in the choice levels. Indeed, average choices increase with time in these experiments. These data suggest that when fixed costs are relatively high, many subjects decide the market is too small for both agents. The contrast in choice behavior between experiments 5 and 6 on the one hand, and the first four experiments on the other hand, are highlighted in Figure 2. The lower broken schedule (AV1234) in the figure is the average choice for all subjects in experiments 1–4. Comparing this with average choices in experiments 5 and 6 (the solid schedule AV56), the data show that subjects pairs begin by choosing about the same outputs, but then after about period 5 behaviors become different as evidenced by the schedules pulling apart.

We want to be careful about attributing motives to participants as we review these data. The claim we make is that subjects in experiments 5 and 6 engage in price wars and suffer losses as a consequence. The average subject could experience losses in some periods as part of learning the game; however, we believe they would be so small relative to the starting balance that bankruptcy would not result. Another explanation for negative returns is that subjects simply become losers through some kind of strategic behavior. Actually, numerous reasons for the negative payoffs in experiments 5 and 6 could be suggested, neither large choices nor negative payoffs were observed in the first four experiments. Indeed, if the average quantity choices made in experiments 1–4 had been made in experiments 5 and 6, there would have been no bankruptcies in these markets.¹¹ We maintain the change in fixed costs caused behavior to be different in these last two experiments. As Figure 2 shows, subjects in experiments 5 and 6 were on average choosing quantities well above the Cournot choice after period 5. These are not reasonable actions for the duration of a repeated game with any strategy. Because of this observed behavior we conclude that in experiments 5 and 6, many subjects decided their long run interests were best served by becoming the only agent in the market.

Some subject pairs in experiments 5 and 6, however, avoid a price war. Focusing on ex-

11. The average subject could experience losses in some periods. However, these would be so small relative to the starting balance that bankruptcy would not result.

periment 6, we define non-warring (NW) subjects pairs to be those with choices at or below 40 for thirteen of the first fifteen periods of the experiment.¹² We identify seven pairs that fit this definition and their average choices are graphed in Figure 2 as the schedule (AV56NW). These pairs have a choice pattern that closely parallels the pattern of average choices in experiments 1 through 4. It therefore appears from the data that behavior is dichotomous. For some subject pairs a critically low profit threshold was passed, leading to a war of attrition. But for other subject pairs the war was avoided in the interest of implicitly seeking more cooperative outcomes as shown by the downward trend in choice behavior like that in experiments 1 through 4.

IV. Econometric Analysis

While the discussion presented in section II provides some useful benchmarks, there are reasons to doubt that any behavioral model is a perfect description of human agents. Bounded rationality, for example, will limit the ability to calculate optimal strategy choices in a period. Furthermore, even if one player attempts to infer a rival's likely actions, he or she is unlikely to be sure of the rival's rationality. For these reasons, we do not expect to see agents instantly computing, and then selecting outputs corresponding to, subgame-perfect equilibria. Nevertheless, there is good reason to believe that agents will converge to an equilibrium given sufficient time [16; 19]. Over time subjects move toward stable behavior and this has implications for the econometric analysis we discuss in this section.

We use two distinct econometric models to evaluate the impact of fixed costs. In subsection A, we compare market choices across fixed cost treatments and test for differences. Taking the perspective that market behavior is the relevant statistic, we analyze paired choices. For this model, we interpret the data set as a pooled cross-section/time-series sample, where the dependent variable is subject pairs' choice. This analysis reveals significant differences between the four sessions with low fixed costs and the high cost treatment. Further, a comparison across the low fixed cost experiments indicates that behavior does not differ significantly across relatively low fixed cost levels. The second study, presented in subsection B, focuses on the high fixed cost treatment. We use a two-way contingency analysis to determine if there is a significant relation between the frequency of attrition wars and the level of fixed costs. With a high degree of confidence, we conclude that such a relation does exist. Then, using a time series model, we explore the nature of this difference. We find no significant difference in behavior between those pairs in the high fixed cost treatment that did not engage in price wars and subject pairs in the low fixed cost sessions.

Paired Choice as the Dependent Variable

In this section we first estimate subject pair behavior, comparing the four experiments where fixed costs are small enough that wars of attrition apparently do not occur against the sessions with high fixed costs. To this end, we regard our data set as a pooled cross-section time-series

12. This level is well above the Cournot/Nash market choice of 32. So a choice of 40 cannot be mistaken as Nash behavior. Also, symmetric losses at (20,20) are nearly twice what they are at the symmetric Nash choice of (16,16), -155 compared to -78. This difference in earnings also distinguishes a price war from Nash behavior. Finally, using this definition, for those pairs that do not go to war we estimate in section IV that average choice behavior is 25.9242, and for those pairs that enter a price war average choices are 39.3113. So this definition is consistent with the econometric model introduced below.

sample. Each subject's choice in period t is likely to be linked to the rival's choice in $t - 1$ [8]; this yields a dynamic reaction function:

$$q_i(t) = A + Bq_j(t - 1), \quad j \neq i = 1, 2.$$

The structural model we utilize in our analysis is obtained by aggregating the dynamic reaction functions for the two agents, and allowing for noise:

$$Q_k(t) = \gamma_k^0 + \gamma_k^1 Q_k(t - 1) + \epsilon_k(t), \quad (2)$$

where $Q_k(t)$ is pair k 's period t choice and $\epsilon_k(t)$ is a residual capturing variations about the equilibrium. There are a host of reasons to expect serial correlation in this structure. Any attempts at signalling a desire to cooperate hinge on an intertemporal connection [29]. Similarly, any learning implies a connection between current and preceding choices [16]. These concerns suggest serially correlated disturbances, which we model as a first order autoregressive process.¹³ Assuming this to be the case,

$$\epsilon_k(t) = \rho_{k2} \epsilon_k(t - 1) + \mu_k(t), \quad (3)$$

where the residual $\mu_k(t)$ is white noise (i.e., $E\mu_k(t) = 0$, $E[\mu_k(t)\mu_k(s)] = 0$ for $t \neq s$ and $E\mu_k(t)^2 = \sigma_k^2$). Equations (2) and (3) imply the relation:

$$Q_k(t) = \beta_k + \rho_{k1} Q_k(t - 1) + \rho_{k2} Q_k(t - 2) + \mu_k(t). \quad (4)$$

The parameter restrictions $|\rho_{k1}| < 1$, $|\rho_{k2}| < 1$, and $|\rho_{k1} + \rho_{k2}| < 1$ are necessary for choices to converge [6], in which case we may interpret

$$\alpha_k = \beta_k / (1 - \rho_{k1} - \rho_{k2}) \quad (5)$$

as the steady state, or equilibrium, choice for subject pair k . In turn, this indicates that α_k may be viewed as the natural parameter to focus on when asking questions about the equilibrium of the system.

Our approach to estimating the parameters in (4) is to regard the residuals $\mu_k(t)$ as generated from a multi-variate distribution. Estimation then follows standard techniques for analyzing pooled cross-section/time-series data, once the covariance structure is specified. We shall allow for different variances across subject pairs, and assume that no cross-equation covariance exists: $E[\mu_k(t)\mu_h(s)] = 0$, for $k \neq h$.

The first hypothesis we wish to analyze is that there is no difference between behavior in the four sessions with low fixed costs and the two sessions with high fixed cost. Because most of the subject pairs did not survive to the end of session five, and there are only fourteen observations for the one pair that did survive, we limit this analysis to experiments 1 through 4 and experiment 6. With a pooled cross-section/time-series approach we require the same number of observations

13. In a similar experimental setting Mason, Phillips, and Redington [20] argue that market choices are best described by an AR(2) process. This is equivalent to assuming that the disturbances $\epsilon_k(t)$ follow an AR(1) process in our model.

Table II. Analysis of Subject Pair Behavior—the Impact of High Fixed Costs

Parameter	Estimate	Standard Error
1. All five sessions		
α	25.4307	.2634
ρ_1	.4213	.0108
ρ_2	.1997	.0110
R^2 : .2845		
Durbin-Watson Statistic: 1.9328		
2. Sessions 1–4		
α	24.1087	.2619
ρ_1	.4096	.0123
ρ_2	.1716	.0125
R^2 : .2534		
Durbin-Watson Statistic: 2.0840		
3. Session 6		
α	31.2247	.6587
ρ_1	.3935	.0232
ρ_2	.2436	.0236
R^2 : .2889		
Durbin-Watson Statistic: 1.9218		
Test statistic for Chow test: 4.6870		
1% critical value ($F_{3,878}$): 3.78		

from each subject pair and so consider the first nineteen choice periods from each experiment.¹⁴ The hypothesis of interest is that subject pairs' choices were not significantly different between the first four experiments and the sixth experiment; the alternative is that behavior was different in the sixth experiment. This hypothesis may be tested by a Chow test. To facilitate this test, we estimated equation (4) for the entire sample, and then re-estimated it separately for the two sub-samples. There are 41 subject pairs from experiments 1 through 4 and 11 subject pairs from experiment 6; with 17 observations per pair this yields 697 observations from the first group and 187 observations from the second group, for a total of 884 observations.¹⁵

The results from these three regressions are presented in Table II. Under the null hypothesis of no structural change, the test statistic will have the F distribution with 3 and 884 degrees of freedom; our test statistic exceeds the 1% critical value, and so we reject the null hypothesis of no behavioral change with great confidence.

Having concluded that there are significant differences between the low fixed cost sessions and the high fixed cost session, we now ask if behavior differs with the level of fixed cost across experiments 1 through 4. Within these sessions we want to identify any clear pattern of change in equilibrium choices as a result of changes in fixed costs. We assume that α_k , ρ_{k1} , and ρ_{k2} are the same for all subject pairs in a given experiment, and that any variation across pairs is captured by the respective residual terms. The parameter vectors are thus assumed to be

14. The length of the time series analyzed is dictated by the shortest series. Since one subject in experiment 6 was bankrupted in period 19, we use the first 19 observations in this part of our analysis.

15. Because choices are lagged twice, we base our analysis of eq. (4) on $t = 3, \dots, 19$. This gives us 17 observations for each pair.

$$(\alpha_k, \rho_{k1}, \rho_{k2}) = \begin{cases} (\alpha_1, \rho_{11}, \rho_{12}) & \text{if pair } k \text{ is in experiment 1;} \\ (\alpha_2, \rho_{21}, \rho_{22}) & \text{if pair } k \text{ is in experiment 2;} \\ (\alpha_3, \rho_{31}, \rho_{32}) & \text{if pair } k \text{ is in experiment 3;} \\ (\alpha_4, \rho_{41}, \rho_{42}) & \text{if pair } k \text{ is in experiment 4.} \end{cases}$$

This system may be efficiently estimated by feasible generalized least squares, yielding the efficient estimator vector (b_n, r_{n1}, r_{n2}) for $(\beta_n, \rho_{n1}, \rho_{n2})$, $n = 1, 2, 3, 4$. We may then consistently estimate α_n by

$$a_n = b_n / (1 - r_{n1} - r_{n2}). \quad (6)$$

Under plausible assumptions, a_n may be regarded as the maximum likelihood estimator. The results of this estimation procedure are given in Table III, where we report the estimates and standard errors for a_n , r_{n1} , and r_{n2} for each experimental design.¹⁶

We observe that in each design, the parameter restrictions on the ρ_n s are met, so that the estimates of α_n may properly be regarded as maximum likelihood estimates of the equilibrium values in the respective designs. The hypothesis of interest is that there is no difference between the equilibrium values in the four designs, i.e., $H_0 = \alpha_1 = \alpha_2 = \alpha_3 = \alpha_4$. This is tested by means of an F -test, with resultant test-statistic well below the 5% critical value. We conclude that fixed costs did not influence behavior within the first four experiments.

Analysis of High Fixed Cost Designs

The next hypothesis of interest, and the crux of our analysis, is that wars of attrition were more prevalent when fixed costs became relatively large. Here, we analyze the effect of a treatment on the frequency of wars of attrition, where we contrast between the low-fixed cost designs, experiments 1 through 4, and the high-fixed cost design, experiments 5 and 6. The data indicate price wars are observed in experiments 5 and 6, but not in experiments 1 through 4.

The comparison of these two sets of sessions is most readily done by use of a two-way contingency table. The conditions we use to separate the data are low versus high fixed costs, which determines the column in the contingency table, and whether or not a price war was observed, which determines the row. Results of this analysis are summarized in Table IV. Under the null hypothesis that the row and column treatments are not correlated, the test statistic is distributed as a central chi-squared variate with one degree of freedom. In the application at hand, the test statistic is far larger than the critical value, and so we reject the null hypothesis in favor of the hypothesis that a war of attrition was much more likely when fixed costs were set at the high level.

For seven subject pairs in experiment 6, fixed costs were not sufficiently high as to trigger what we define as a price war (see footnote 12). For these pairs, we would expect behavior to parallel that of subjects in the first four sessions. To test this conjecture, we estimated the econometric model in equations (2)–(5) for these seven pairs. Results are reported in Table V. The hypothesis of interest is that the estimate of steady state pair choice for this cohort, α_{nw} , is equal to the estimated steady state for subject pairs in the first four sessions, which is reported in Table III as α . Our estimates confirm the maintained hypothesis, that $\alpha_{nw} = \alpha$, as the resultant

16. As we noted in footnote 15, the number of observations is dictated by the shortest time series. Here, all subjects made 25 choices. Since our model has two lags, the estimation is based on 23 observations from each of 41 subject pairs.

Table III. Parameter Estimates under First Four Treatments

Parameter	Estimate	Standard Error
Treatment 1 ($F = 63.4256$)		
α_1	21.9156	1.7540
ρ_{11}	.3245	.0608
ρ_{21}	.3689	.0597
R^2 : .4418		
Durbin-Watson statistic: 2.1160		
Treatment 2 ($F = 47.6471$)		
α_2	24.4036	1.5814
ρ_{12}	.5095	.0685
ρ_{22}	.1327	.0680
R^2 : .3861		
Durbin-Watson statistic: 1.9872		
Treatment 3 ($F = 70.0692$)		
α_3	20.9095	1.7433
ρ_{13}	.5270	.0596
ρ_{23}	.2153	.0582
R^2 : .5430		
Durbin-Watson statistic: 2.1337		
Treatment 4 ($F = 55.9516$)		
α_4	23.4143	1.3489
ρ_{14}	.3938	.0598
ρ_{24}	.2058	.0590
R^2 : .3793		
Durbin-Watson statistic: 2.0024		
All Treatments Pooled		
α	23.1776	.8354
ρ_1	.4255	.0302
ρ_2	.2601	.0304
R^2 : .4421		
Durbin-Watson statistic: 2.0439		

Test statistic on $H_0 : \alpha_1 = \alpha_2 = \alpha_3 = \alpha_4 : 1.1718$; 5% critical value = 2.01

t -statistic is statistically insignificant at conventional levels. We also estimate the model for the four remaining subject pairs, for whom price wars were observed. The steady state estimate for this group is 39.3113, which is significantly different from the steady state for non-warring pairs in experiments 1 through 4 and experiment 6.

V. Conclusion

Our experiments show that high fixed costs can significantly alter behavior by inducing agents to enter a war of attrition with a rival. The differences in behavior between subject pairs in the first four experiments (with lower fixed costs) and the last two (with high fixed costs) is remarkable. At high levels of fixed costs duopoly markets exhibited strong tendencies toward price wars. The

Table IV. Contingency Analysis of Effects of Fixed Costs

	Fixed Cost		Row Sum
	Below Cournot Profits	Above Cournot Profits	
price war			
yes	0	10	10
no	41	7	48
column sum	41	17	58

Test statistic = 29.4122; 5% critical point = 6.99

Table V. Analysis of Warring and Non-Warring Pairs in Experiment 6

Parameter	Estimate	Standard Error
α_{nw}	25.9242	1.9599
ρ_{1nw}	.3710	.0903
ρ_{2nw}	.1828	.0918
R^2 : .2600		
Durbin-Watson Statistic: 2.0518		
α_w	39.3113	1.5580
ρ_{1w}	.0044	.1231
ρ_{2w}	.0850	.1288
R^2 : .1074		
Durbin-Watson Statistic: 1.9415		

Test statistic on $H_0(\alpha_{nw} = \alpha)$: 1.2896;
 Test statistic on $H_0(\alpha_{nw} = \alpha_w)$: 5.3271;
 Test statistic on $H_0(\alpha_w = \alpha)$: 9.1262;
 5% critical value = 1.96.

promise of monopoly returns is realistic in such an environment. In a market environment where the Cournot returns are negative, players see ample opportunity to eliminate their counterpart by choosing large values and causing both to receive negative payments for awhile. Agents judge that sufficient time exists to later recoup. When fixed costs are relatively low this opportunity is not apparent to subjects.

While we are unable to reject the hypothesis that subjects behaved the same in the first four experiments, there is a pattern of increased cooperation as fixed costs rise. Table III shows that the estimated equilibrium output falls monotonically as we consider experiments with progressively larger fixed costs. Although the differences in these choice levels are not statistically significant, this pattern is consistent with the Hay and Kelley [13] hypothesis that higher fixed costs make rivals more collusive. The caveat is that this pattern of cooperative behavior is observed in markets for which profitability is relatively high.

The aggressive behavior observed in experiment 5 was not dispelled by giving players deeper pockets. The endowment in experiment 6 was increased by 66% from that of experiment 5. This effectively relaxed the liquidity constraint and increased the amount of time it would take a firm to drive a rival from the market. Yet without knowing when the experiment would end, subjects continued to believe it was generally in their best interest to drive their rival out of the market. Hence, the behavior observed in these experiments is fairly robust. However, it must be recognized that the conditions under which these price wars are generated are stylized. There are two

VALUE SELECTED BY THE OTHER PARTICIPANT

Y O U R V A L U E F O R X	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
	274	261	248	235	222	209	196	183	170	157	144	131	118	105	92	79	66	53	40	28	15	2	-11	-24	-37	-50	-63	-76	-89	-102
	297	283	270	256	243	229	216	202	189	175	162	148	135	121	108	94	81	67	54	40	27	13	0	-13	-27	-40	-54	-67	-81	-94
	319	305	291	277	263	249	235	221	207	193	179	165	151	135	122	108	94	80	66	52	38	24	10	-4	-18	-32	-46	-60	-74	-88
	339	325	310	296	281	267	252	238	223	209	194	180	165	151	136	121	107	92	78	63	49	34	20	-5	-9	-24	-38	-53	-67	-82
	359	344	329	314	299	284	269	254	239	224	209	194	179	163	148	133	118	103	88	73	58	43	28	13	-2	-17	-32	-47	-62	-77
	378	362	347	331	316	300	284	269	253	238	222	207	191	175	160	144	129	113	98	82	66	51	35	20	4	-11	-27	-43	-58	-74
	396	379	363	347	331	315	299	283	267	251	235	219	202	186	170	154	138	122	106	90	74	58	42	25	9	-7	-23	-39	-55	-71
	412	396	379	362	346	329	312	296	279	263	246	229	213	196	180	163	146	130	113	97	80	63	47	30	13	-3	-20	-36	-53	-70
	428	411	393	376	359	342	325	308	291	274	256	239	222	205	188	171	154	137	119	102	85	68	51	34	17	-1	-18	-35	-52	-69
	442	425	407	389	372	354	336	319	301	283	266	248	230	213	195	178	160	142	125	107	89	72	54	36	19	1	-17	-34	-52	-70
	456	438	419	401	383	365	347	329	310	292	274	256	238	220	201	183	165	147	129	111	92	74	56	38	20	2	-17	-35	-53	-71
	468	449	431	412	393	375	356	337	319	300	281	263	244	225	207	188	169	151	132	113	94	76	57	38	20	1	-18	-36	-55	-74
	480	460	441	422	403	384	364	345	326	307	288	268	249	230	211	192	172	153	134	115	96	76	57	38	19	-1	-20	-39	-58	-77
	490	470	451	431	411	391	371	352	332	312	293	273	253	234	214	194	174	155	135	115	96	76	56	36	17	-3	-23	-43	-62	-82
	499	479	459	439	418	398	378	358	337	317	297	277	256	236	216	196	175	155	135	115	94	74	54	34	13	-7	-27	-47	-67	-88
	508	487	466	445	425	404	383	362	342	321	300	279	258	238	217	196	175	155	134	113	92	72	51	30	9	-11	-32	-53	-74	-94
	515	494	472	451	430	408	387	366	345	323	302	281	260	238	217	196	174	153	132	111	89	68	47	25	4	-17	-38	-60	-81	-102
	521	499	478	456	434	412	390	369	347	325	303	281	260	238	216	194	172	151	129	107	85	63	42	20	-2	-24	-46	-67	-89	-111
	526	504	482	459	437	415	392	370	348	325	303	281	258	236	214	192	169	147	125	102	80	58	35	13	-9	-32	-54	-76	-99	-121
	530	508	485	462	439	416	393	371	348	325	302	279	256	234	211	188	165	142	119	97	74	51	28	5	-18	-40	-63	-86	-109	-132
	534	510	487	463	440	417	393	370	347	323	300	277	253	230	207	183	160	137	113	90	66	43	20	-4	-27	-50	-74	-97	-120	-144
	536	512	488	464	440	416	392	369	345	321	297	273	249	225	201	178	154	130	106	82	58	34	10	-13	-37	-61	-85	-109	-133	-157
	537	512	488	463	439	415	390	366	342	317	293	268	244	220	195	171	146	122	98	73	49	24	0	-24	-49	-73	-98	-122	-146	-171
	537	512	487	462	437	412	387	362	337	312	288	263	238	213	188	163	138	113	88	63	38	13	-11	-36	-61	-86	-111	-136	-161	-186
	536	510	485	459	434	408	383	358	332	307	281	256	230	205	180	154	129	103	78	52	27	2	-24	-49	-75	-100	-126	-151	-176	-202
	534	508	482	456	430	404	378	352	326	300	274	248	222	196	170	144	118	92	66	40	15	-11	-37	-63	-89	-115	-141	-167	-193	-219
	530	504	478	451	425	398	372	345	319	292	266	239	213	186	160	133	107	80	54	28	1	-25	-52	-78	-105	-131	-158	-184	-211	-237
	526	499	472	445	418	391	364	337	310	283	256	229	202	175	148	121	94	67	40	13	-13	-40	-67	-94	-121	-148	-175	-202	-229	-256
	521	494	466	439	411	384	356	329	301	274	246	219	191	163	136	108	81	53	26	-2	-29	-57	-84	-112	-139	-167	-194	-222	-249	-277
	515	487	459	431	403	375	347	319	291	263	235	207	179	151	122	94	66	38	10	-18	-46	-74	-102	-130	-158	-186	-214	-242	-270	-298

Payment Table

identical firms with complete information, and they are fully aware that there is no threat of entry even if one of them should exit. Strategies may change by further altering the liquidity constraint, changing the relative market shares, allowing the threat of entry in the market, or altering the information given to players. These are only several of many factors that can influence the decision to undertake a war of attrition in the face of declining profits. We suspect as the market resembles less a tightly held oligopoly that wars of attrition would become less frequent. In loose oligopolies, the impact of one firm choosing large outputs upon rivals' profits is diluted, so that it becomes more costly to drive a rival out. At the same time, the potential gain of one rival exiting the market must be shared by all remaining firms, so that the benefits from dispatching a rival are diminished.

The implications of these results are that relatively high fixed costs can lead to the eventual domination of the market by one firm, and at such high levels fixed costs form a barrier to future entry. At relatively low levels variations in fixed costs have no significant impact on the strategic behavior of duopolists. Their increase, for example through increasing taxes, government regulation, or shifts in technology, does not make rivals substantially more or less cooperative. In our first four experimental market structures, profits at the Cournot level range between a high of 354 tokens (when fixed costs are 47.6471) to a low of 130 tokens (when fixed costs are 70.0692). Despite this 63% decrease in profitability, there is no significant difference in strategic behavior. Nevertheless, if high fixed costs correspond to very low industry profits and firms have no expectation of higher profits in the future, we conclude that competitive behavior of the extreme sort exhibited in our laboratory markets can result.

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