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By

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# Experimental evidence of a sunk-cost paradox: a study of pricing behavior in Bertrand–Edgeworth duopoly

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## Abstract

A well-known implication of microeconomic theory is that sunk costs should have no effect on decision making. We test this hypothesis with a human-subjects experiment. Students recruited from graduate business courses, with an average of over six years of work experience, played the role of firms in a repeated price-setting duopoly game in which both firms had identical capacity constraints and costs, including a sunk cost that varied across experimental sessions over six different values. We find, contrary to the prediction of microeconomic theory, that subjects' pricing decisions show sizable differences across treatments. The effect of the sunk cost is non-monotonic: as it increases from low to medium levels, average prices decrease, but as it increases from medium to high levels, average prices increase. These effects are not apparent initially, but develop quickly and persist throughout the game. Cachon and Camerer's (1996) loss avoidance is consistent with both effects, while cost-based pricing predicts only the latter effect, and is inconsistent with the former.

Keywords: oligopoly, posted prices, capacity constraints, loss avoidance, cost-based pricing.

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# 1 Introduction

One of the most fundamental concepts of microeconomic theory is marginal analysis—the model of decision making involving the comparison of the costs and benefits of small changes in behavior. A primary implication of marginal analysis for firm behavior is that firms should make their choices based only on variable revenues and costs. Sunk costs—costs that are the same, irrespective of firms’ decisions—by definition do not affect either variable revenues or variable costs; therefore, their level is presumed not to affect firm decisions. Standard game-theoretic equilibrium concepts for simultaneous-move games (e.g., Nash equilibrium) have the same implication. Changing the *level* of a player’s payoffs—adding or subtracting a constant from all payoffs—has no effect on differences in expected payoff between strategies (even mixed strategies), and thus has no effect on a player’s best-response correspondence. As a result, for any game with a unique Nash equilibrium, this equilibrium is invariant to any change in payoff levels. Even McKelvey and Palfrey’s (1995) “logit equilibrium”, in which players need not play best responses, but rather play strategies with higher likelihood as their expected payoff increases, is unaffected by changing the payoff levels.<sup>1</sup>

On the other hand, there is some empirical evidence suggesting that changes in payoff levels might affect behavior. Kahneman and Tversky’s (1979) prospect theory includes the notion of “loss aversion”, according to which losses—from whatever the decision maker considers to be the status quo—weigh more heavily than equal-sized gains.<sup>2</sup> (See also Tversky and Kahneman (1991).) Such effects have also been seen in strategic decision making. Cachon and Camerer (1996) found that subject decisions in a coordination-game experiment were sensitive to changes in payoff levels when these changes affected the *signs* of payoffs, such as positive payoffs becoming negative. They speculated that subjects showed “loss avoidance”, which they defined to be a tendency to avoid choices that with certainty yield negative payoffs in favor of alternatives that could yield positive payoffs. There are also several empirical studies in the business literature examining the differential treatment of gains and losses by firms.<sup>3</sup>

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<sup>1</sup>Logit equilibrium is only one of many possible specifications of McKelvey and Palfrey’s “quantal-response equilibrium”, albeit the most widely-used one. Other versions of quantal-response equilibrium are sensitive to payoff-level changes.

<sup>2</sup>It should be noted that Kahneman and Tversky used hypothetical, not monetary, payments, so their results should be viewed as suggestive, not conclusive. Some research has compared behavior under real incentives versus hypothetical incentives; for example, Holt and Laury (2002) found that when payments are small, decision-making behavior is similar under either monetary or hypothetical incentives, but as payments are scaled up, subjects become more risk averse under monetary incentives but not under hypothetical incentives.

<sup>3</sup>For example, Burgstahler and Dichev (1997) find evidence consistent with firm-level loss aversion; specifically, fewer firms report small accounting losses and more firms report small accounting gains relative to the numbers expected under a smooth cross-sectional distribution of earnings. This finding is generally attributed to manipulating reported accounting earnings in an effort to avoid reporting losses. Along these lines, Ball and Shivakumar (2006) find that modeling negative accounting profits—by using an indicator variable consistent with prospect theory—improves the intertemporal association between reported accounting income and realized cash flows. Also, see Hayn (1995) and Freeman and Tse (1992) for additional evidence of prospect theory’s loss-aversion effects at the capital-market level.

The objective of this paper is to examine the effect of varying the level of sunk costs on pricing behavior in a duopoly game with simple rules but a complex equilibrium. Even though both standard microeconomic theory and standard game theory imply that differences in sunk-cost levels should not have an effect, there are reasons to believe that they might, even beyond those reasons listed above. Carmichael and MacLeod (2003) show theoretically that caring about sunk costs (in the sense of conditioning behavior on them) can be rational in bargaining situations where there is a potential holdup problem. Sunk costs can also affect firms' prices if decision makers, rather than setting prices by marginal analysis, use "cost-based pricing"—that is, if their prices are chosen based on average total cost, rather than marginal cost. Under cost-based pricing, higher sunk costs mean higher average total costs, and thus higher prices. While such behavior is normally considered to be at odds with economic theory, cost-based pricing may be a useful heuristic, especially when marginal cost is difficult to determine, as is often the case in real-life situations.<sup>4</sup> There is ample survey evidence that firms actually do use cost-based pricing (see, for example, Govindarajan and Anthony (1983), Drury et al. (1993), and Shim and Sudit (1995)). Finally, there is an extensive literature in psychology and economics dealing with the "sunk-cost fallacy", according to which individuals irrationally—from the standpoint of microeconomic theory—fail to ignore sunk costs in their decision making. In psychology, this literature dates back at least to Aronson and Mills (1959). (For a survey of early results, see Thaler (1987); for a more recent experiment, see Friedman et al. (2007).)

In this paper, we report the results of a human-subjects experiment designed to examine the effect of sunk costs on firms' pricing behavior. Students recruited from graduate business courses, with an average of over six years of business experience, took on the role of firms. Each played a thirty-round price-setting game against one other firm. Within a pair of firms, both had identical, unchanging, publicly-announced cost functions, including sunk costs that took on one of six different values. We found, contrary to standard microeconomic theory, that pricing behavior varied substantially and significantly with the level of the sunk cost. Contrary to the prediction of cost-based pricing, the relationship between sunk-cost level and prices was not monotonic. At low levels, increasing the sunk cost led to a decrease in average prices (the opposite of what cost-based pricing predicts), but for higher levels, increasing the sunk cost led to an increase in average prices (as cost-based pricing predicts). We find no evidence of a sunk-cost effect in the first round, but it develops quickly and is apparent by the fifth round; it then persists throughout the experiment. Examination of individual decisions shows that prices change over time and in response to results from past play;

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<sup>4</sup>It is not even completely clear that cost-based pricing is inconsistent with rational behavior. Al-Najjar, Baliga, and Besanko (2004) consider a theoretical price-setting oligopoly model with differentiated products, and discuss conditions under which cost-based pricing can be found in equilibrium.

this is inconsistent not only with cost-based pricing, but also with other static theories such as Nash equilibrium, competitive equilibrium, and tacit collusion. Edgeworth cycle theory (Edgeworth (1925)) does predict nonstationary prices, but not the sunk-cost effect. Loss avoidance (Cachon and Camerer (1996)) is consistent with the non-monotonic effect of the sunk cost, but does not predict the observed changes over time.

Pricing dynamics in industries with high fixed investment and capacity considerations (e.g., the airline, car rental, electric power generation, and telecommunication industries) are complicated and not well understood. Issues such as demand uncertainty (Göx (2002), Kruse et al. (1994)), the ability to backlog demand and store inventory (Shang and Song (2003)), predatory pricing (Isaac and Smith (1985), Harrison (1988)), hard versus soft capacity constraints (Göx (2002)), number of competitors (Kruse et al. (1994)), and the financial health of competitors (Ciliberto and Schenone (2007)) have all been shown to affect firms' pricing behavior in these settings. Balakrishnan and Sivaramakrishnan (2002) argue that pricing is only one part of a larger problem; therefore, jointly considering the capacity-planning and product-pricing problems may be necessary to better understand these pricing dynamics. With such factors in mind, our study is—in a sense—troublesome because we provide evidence that sunk-cost magnitude represents yet another factor that can affect competitive pricing decisions in these and other industries. That said, we highlight a paradox of decision making: irrelevant data ought to be ignored when making business decisions, and sunk costs are the archetypal example of irrelevant information (Garrison et al. (2006)). However, evidence that sunk costs can have non-monotonic effects on pricing decisions may offer a useful piece to this relatively complicated puzzle.

## 2 The duopoly market

The experiment entailed repeated play of a two-player Bertrand-Edgeworth oligopoly stage game, with subjects taking the role of firms. In each round of the game, a firm is able to produce up to 40 units of a homogeneous good at a constant marginal cost of \$5. Production beyond 40 units is impossible. In addition to the constant marginal cost, each firm incurs a sunk (unavoidable, even when producing zero units) cost, which may be either \$2000, \$3000, \$4000, \$5000, \$6000, or \$7000 per round. In each round of the game, the two firms first simultaneously and independently choose their prices, which were restricted to whole-dollar amounts between \$5 and \$500. Next, each firm produces and sells enough units of the good to meet its quantity demanded (subject to the capacity constraint of 40 units). Consumers are automated; the quantity demanded for each firm is determined from the two firms' prices, along with a random component, as follows. For each multiple of \$5 between \$5 and \$500, there is exactly one potential customer with this reservation price (100 customers total). These

customers queue in one of six equally likely orders, shown in Appendix A. Each customer, in turn, buys one unit of the good if it can buy at a price lower than or equal to its reservation price.<sup>5</sup> If so, the customer buys from the lower-priced seller if that seller has units left to sell (that is, if that seller has not yet sold its 40 units); otherwise, the customer buys from the higher-priced seller if that seller has units left to sell. If both sellers choose the same price, each sells half the quantity demanded at that price, up to their capacities.<sup>6</sup>

In order to keep the rules of the game simple, we forbid entry and exit in this market, and we automate the firms' second-stage production decisions, so that each firm's quantity produced simply equals the quantity demanded from that firm. Thus, the two stages of each round (pricing, then production) collapse to a single stage, consisting only of the two firms' pricing decisions.

Before discussing the theoretical predictions for this game, we note that the firms' capacity constraints play an important role in the strategic environment. These capacity constraints are what distinguish Bertrand-Edgeworth duopoly from standard Bertrand duopoly. In our setup, the two firms' products are perfect substitutes for each other. So, if neither firm had a capacity constraint, the higher-priced firm would never be able to sell any units of output, which would make the incentive to choose a low price extremely strong. Indeed, the only Nash equilibria of the no-capacity-constraints analogue to our game have a very low price—either \$5 or \$6—chosen by both firms, irrespective of any sunk cost.

Now, turning back to our game (with the capacity constraints), we show in Figure 1 some of the economically-relevant features. The left panel shows a standard market diagram, with curves representing demand and supply, as well as joint marginal revenue (MR). Also shown are two benchmark predictions for play in this game. A competitive equilibrium price is one at which the demand and supply curves intersect; this happens over the interval (100,105]. Because we restrict prices to be whole-dollar amounts, competitive equilibrium price choices by firms are \$101, \$102, \$103, \$104, and \$105. The collusive price—at which joint profits are maximized—is the price at which the marginal revenue curve intersects the supply curve; this happens at a price of \$255.<sup>7</sup> In our experiment, no communication between subjects is permitted, so that no formal or informal collusive agreements are possible. However, because subjects in the experiment play repeatedly (with no definite final round; see Section 3) against the same opponent, there also exist subgame perfect equilibria in which there

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<sup>5</sup>Due to our specification of demand and costs, our restriction of prices to those between \$5 and \$500 should not make much difference; prices below \$5 earn a negative profit on every unit sold, and thus are weakly dominated by a price of exactly \$5, while prices above \$500 guarantee no units sold, and thus are weakly dominated by a price of exactly \$500.

<sup>6</sup>Thus, if the quantity demanded was an odd number, each firm will sell a fractional unit of the good. While odd-sounding, this could be interpreted as each firm receiving its expected profit, given customers' randomly choosing between equal-priced sellers.

<sup>7</sup>Because of the capacity constraints, this collusive price is not the same as the monopoly price of \$305.

is tacit collusion. If tacit collusion is perfectly successful, prices will be at the collusive level shown in

Figure 1: Characteristics of the Game

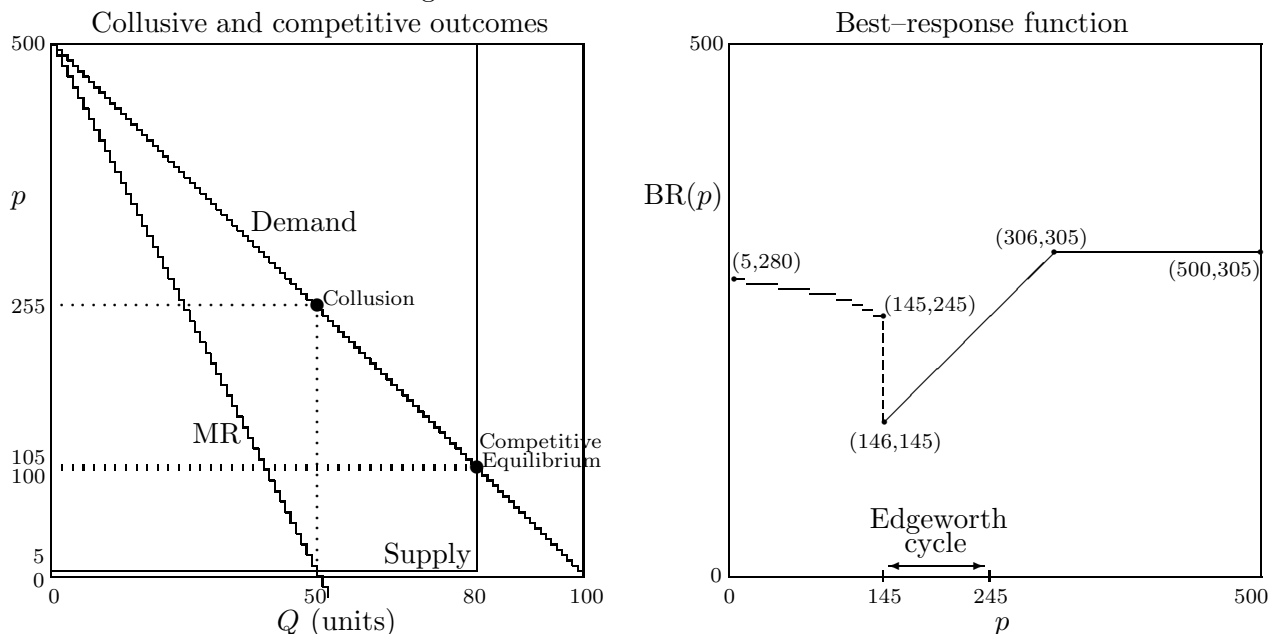


Figure 1, irrespective of the sunk cost.

The right panel of Figure 1 shows the game’s best-response function. In addition to other applications, the best-response function can be used to find this game’s Edgeworth cycle. Edgeworth cycle theory (Edgeworth (1925)) is a dynamic, non-equilibrium theory that assumes, roughly, that a firm will play a myopic best response to its opponent’s previous-round strategy.<sup>8</sup> In our game, as is typical in Bertrand oligopoly with homogeneous products, there is a wide range of prices (from \$146 up to \$306) to which the best response is a price one dollar lower. Also, the best response to \$145 is \$245. So, the Edgeworth cycle lies between \$145 and \$245; the associated prediction is that, within a pair of players, prices will slowly decrease from \$245 to \$145, jump back up to \$245, and continue over and over again in this way.<sup>9</sup>

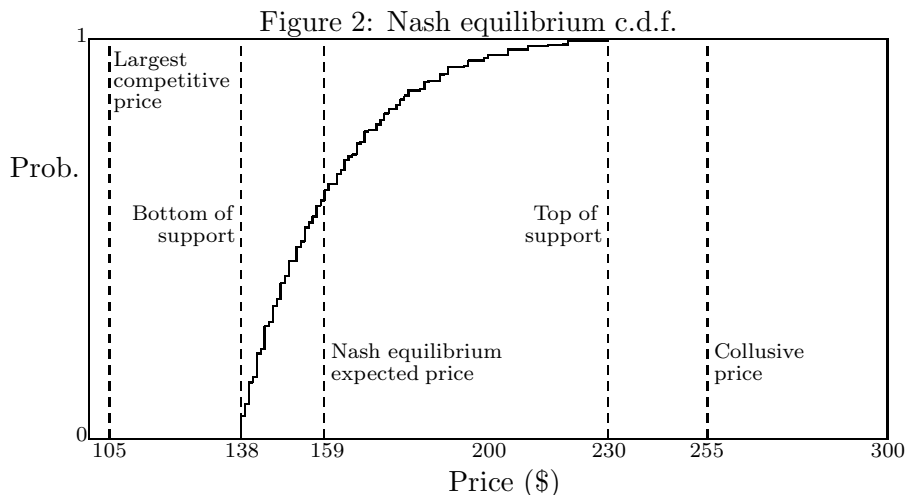
As is typical for Bertrand-Edgeworth duopoly, this game has no pure-strategy Nash equilibrium. It has a symmetric mixed-strategy Nash equilibrium, the support of which is a subset of  $\{138, 139, \dots, 230\}$ ; this equilibrium is unaffected by the level of the sunk cost. The cumulative distribution function for the Nash equilibrium is shown in Figure 2.<sup>10</sup> Also shown is the resulting equilibrium

<sup>8</sup>The Edgeworth cycle tends to arise as a prediction in price-setting oligopolies when capacity constraints are present. However, capacity constraints are not necessary. Maskin and Tirole (1988) found that Edgeworth cycles could emerge as equilibrium behavior in a repeated Bertrand duopoly—without capacity constraints, but with firms making alternating pricing decisions.

<sup>9</sup>Holt and Solis-Soberon (1992) discuss an important feature of the Edgeworth cycle: under certain conditions, its support is the same as that of the Nash equilibrium mixed strategy. They also give an example illustrating that this is not true in general. The game we use has a step-level demand function which violates one of the conditions they mention, and indeed we will see that the Edgeworth cycle does not coincide with the support of the Nash equilibrium mixed strategy (though they overlap substantially, neither is a subset of the other).

<sup>10</sup>We are grateful to Ted Turocy for his work in finding this equilibrium.

expected price of approximately 160, as well as the competitive and collusive prices, for sake of comparison. In the Nash equilibrium, per-round expected gross profits (not including the sunk cost) are approximately \$5294 for each player, so that equilibrium expected per-round net profits are negative when the sunk cost is 6000 or 7000, and positive otherwise. The predictions made by each of the four



theories mentioned above, as well as cost-based pricing (mentioned in the introduction) are shown in Table 1.

Table 1: Predictions

Theory	Mean price(s)	Positive variance?	Changes over time?	Effect of sunk-cost increase	Expected per-round profits (gross of sunk cost)
Competitive equilibrium	{101,...,105}	Yes	No	None	3840–4000
Tacit collusion	255	No	No	None	6250
Nash equilibrium	158.92	Yes	Only due to changing realizations	None	5293.78
Edgeworth cycle	—	Yes	Yes	None	—
Cost-based pricing	—	No	No	Price increase	—

Our design adds to a large body of experimental research on price-setting oligopoly. Much of this research has examined various types of Bertrand oligopoly without capacity constraints (see Holt (1995) for a survey). A typical finding in these experiments is weak support for the equilibrium prediction, combined with frequent attempts to collude. Such attempts are often relatively successful in duopoly experiments, but less so as the number of firms is increased.

When capacity constraints are present, on the other hand, subjects' choices of prices often do not



converge to a single value (or even a stationary frequency distribution), so that all static solution concepts perform poorly.<sup>11</sup> As a result, there have been attempts to use dynamic theories such as the Edgeworth cycle to describe the way prices change over time. A common result from these attempts is that prices exhibit dynamics consistent with the most general qualitative predictions of Edgeworth cycle theory (slow decreases, then a quick rise once prices reach a low point), but attempts to use it to make more specific quantitative predictions do not seem to work. Isaac and Smith (1985) perform an experiment in which they look for predatory pricing in a duopoly where one firm has a cost advantage and “deep pockets” (a higher cash balance to start the experiment).<sup>12</sup> In a few sessions, they find price dynamics consistent with an Edgeworth cycle, but most of their sessions produce behavior consistent with what they call “dominant firm equilibrium”: a simultaneous-move analogue to Stackelberg equilibrium. Millner, Pratt, and Reilly (1990) look for contestable market outcomes in a duopoly experiment with an avoidable fixed cost and everywhere-decreasing average cost. They find that the price dynamics in a few of their sessions are consistent with an Edgeworth cycle, but many more are consistent with a slightly different dynamic theory: that of Coursey et al.’s (1984) “unstable prices hypothesis”. The “unstable prices hypothesis” predicts dynamics similar to the Edgeworth cycle; the only difference is that when prices reach the bottom of the cycle, one firm drops out, after which the other raises price to (or toward) the monopoly level, so that the firm that had exited soon re-enters—whereas in the Edgeworth cycle, neither firm exits. (In our experiment, exit is not possible, so there is no difference between the unstable prices hypothesis and the Edgeworth cycle.) Cason, Friedman, and Wagener (2005) look at an experimental six-firm Bertrand-Edgeworth oligopoly, in which consumers have positive search costs (somewhat weakening the incentive for firms to cut prices). They find that subject behavior is not perfectly described by the Edgeworth cycle, but some features of dynamics are consistent with a weak version of the Edgeworth cycle.

Our design also adds to a small but growing strand of experimental research on the effects of varying the level of payoffs in games. We do not present an extensive literature review here; one can be found in Feltovich, Iwasaki, and Oda (2008). Rather, we limit our focus here to experimental work looking specifically at markets in which sunk costs are present.<sup>13</sup> Kachelmeier (1996) considered double-

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<sup>11</sup>An exception is found in Kruse (1993), who found prices either consistent with or converging toward the Nash equilibrium predictions in two versions of a Bertrand-Edgeworth duopoly experiment.

<sup>12</sup>Like our experiment and the rest of the experiments discussed in this section, they had subjects assume the role of firms, while consumer behavior was automated.

<sup>13</sup>There has also been some research into the effects of *avoidable* costs, which are fixed at all positive levels of output, but can be avoided if the firm produces zero output. In addition to Millner, Pratt, and Reilly (1990), mentioned above, Van Boening and Wilcox (1996) look at an experimental double-auction market in which firms have avoidable costs, and find that the presence of avoidable costs leads to erratic price fluctuations and low efficiencies relative to typical double-auction markets. Also, Cooper et al. (1993) and Van Huyck, Battalio, and Beil (1993) found that allowing subjects to incur avoidable costs could affect which equilibrium was selected in coordination games, consistent with forward-induction arguments.

auction markets with five buyers and five sellers. Sellers faced sunk costs that varied across sellers within a round, and from round to round, in each market. Kachelmeier’s main treatment variable was the format in which end-of-round accounting reports were given to sellers; a “historic cost” treatment emphasized the sunk cost, while a “redemption value” treatment emphasized economic profit (hence ignoring the sunk cost). The results were mixed: both buyers’ bids and sellers’ offers were significantly higher when the sunk cost was emphasized than when it was not, but there was little difference in transaction prices between the treatments. In all treatments, these prices tended to be within the range of market-clearing prices. Waller, Shapiro, and Sevcik (1999) considered posted-offer markets with five sellers (and automated buyers, like in our experiment). Like Kachelmeier (1996), Waller, Shapiro, and Sevcik varied the way sellers’ costs were framed: an “absorption costing” treatment emphasized the sunk cost, while a “variable costing” treatment did not. They found that except in the first round, there was no significant difference in sellers’ offers between the treatments; in both, offers converged reasonably quickly to market-clearing levels.

The results of Kachelmeier and Waller, Shapiro, and Sevcik suggest that it might be very difficult in any experiment to find evidence of an effect from varying sunk costs. However, the strong tendency toward competitive pricing in both experiments may have been at least partly due to the way the experiments were designed. The double auction, used by Kachelmeier, has repeatedly been shown experimentally to lead to prices close to their competitive levels, even with relatively small numbers of buyers and sellers, and under weak assumptions about the amount of information and level of cognitive abilities they have (see Holt (1995) for a survey). The posted-offer market used by Waller, Shapiro, and Sevcik shows less tendency than the double auction to push prices to competitive levels (again, see Holt (1995)), but even so, their specific version of the market—with five sellers in each market, as compared to the two in our design—could be expected to facilitate rapid price convergence.<sup>14</sup> Thus, it is quite possible that in both of these studies, the effects of the particular market institutions they use may have overwhelmed any potential tendency by subjects to choose any prices other than the competitive price. In contrast, our experimental design offers a wide range of rationalizable pricing choices, thus allowing the possibility for sunk costs to have a persistent effect on competitive prices.

The experimental designs most closely related to ours are those of Kruse et al. (1994) and Offerman and Potters (2006). Kruse et al. consider a four-firm Bertrand-Edgeworth oligopoly. Firms’ costs

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<sup>14</sup>Many researchers have found a qualitative difference in competitive behavior between markets with a very small number of sellers (two or three) and markets with only a few more sellers. In the empirical industrial organization literature, Bresnahan and Reiss (1991) look at a large number of geographically-separated markets in several industries, and find that once three sellers are present in a market, prices are essentially competitive. In the experimental IO literature, Isaac and Reynolds (2002) find that four-firm posted-offer markets produce prices close to competitive levels, but when only two firms are present, prices are significantly higher. (See also Dufwenberg and Gneezy (2000), mentioned above.) In the theoretical IO literature, Selten (1973) demonstrates that firms are substantially more likely to behave competitively in markets with five or more firms than in markets with fewer firms.

are similar in nature to those we use, and demand is roughly hyperbolic. They do not vary the sunk–cost level, but instead vary firms’ capacities and the amount of information firms have about demand and other firms’ costs. They compare the experimental data to the predictions of competitive equilibrium, tacit collusion, mixed–strategy Nash equilibrium, and the Edgeworth cycle. Neither competitive equilibrium nor tacit collusion characterize the data, though they find some support for a weak form of tacit collusion in which prices are between the competitive equilibrium price and collusive price. Nash equilibrium fares badly in its point predictions, though it correctly predicts the direction in which prices change when firms’ capacities change. They find more support for Edgeworth cycle theory, but like Cason, Friedman, and Wagener (2005), support becomes weaker as they strengthen the demands they make on the theory. (Firms’ prices tended to move in the direction of the best response to opponents’ previous round prices, but they didn’t actually choose best responses.)

Offerman and Potters (2006) conducted experimental monopoly and Bertrand duopoly (with differentiated products) markets in which the right to enter the market is auctioned off beforehand. As controls, they had another duopoly treatment in which subjects didn’t bid to enter the market, but rather were required to pay a fixed entry fee, as well as a third treatment (for both monopoly and duopoly), in which subjects didn’t have to pay at all to enter. Offerman and Potters found that prices in the duopoly games were higher when subjects had a positive fixed cost—whether it was from paying the entry fee (and thus a true sunk cost) or from winning the auction (so that it was an avoidable cost)—than when there was no fixed cost. Consistent with cost–based pricing, they also found that average prices tended to rise as the fixed cost increased. However, they found no effect of sunk costs on pricing in the monopoly games, so they concluded that subjects weren’t simply using cost–based pricing techniques; rather, more subjects were willing to attempt tacit collusion in duopoly when sunk costs were present.

### **3 Experimental procedures**

The experimental sessions were conducted at the University of Houston in 1999 and 2000 using pen and paper. (The monitor used a computer to make calculations, and to print out the feedback given to subjects at the end of each round.) Subjects were mainly University of Houston MBA students, along with a few business–college Ph.D. students, recruited from graduate business classes. No economics graduate students took part in the experiment. Subjects had an average of 6.3 years of work experience. No subject participated in more than one experimental session. Four subjects took part in each session, so that there were always two duopoly games taking place. This allowed us to maintain a measure of subject anonymity, as subjects would not be able to identify which of the other three subjects in

the session was their opponent. All subjects in a session had the same sunk-cost level, which did not change from round to round; we therefore will refer to our treatments as SC2000, SC3000, SC4000, SC5000, SC6000, and SC7000. Each subject played the duopoly game 30 times against the same opponent, though the number of rounds was not told to the subjects.<sup>15</sup>

At the beginning of an experimental session, each subject was seated and given written instructions. A sample copy of the instructions can be found in Appendix A; note that these instructions provide plenty of context to subjects, rather than describing the game in abstract terms. Instructions were also given orally in an effort to make the rules of the experiment common knowledge. After the instructions were read and any questions answered, no communication was allowed between subjects.

Subjects started the experiment with a lab-money balance that depended on the treatment, as described in Note 16 below. At the beginning of a round (which was called a “day” in the experiment), subjects were asked to write their choice of price for that round on a slip of paper. As already mentioned, prices were restricted to whole-dollar amounts between \$5 and \$500. After all four subjects’ slips were collected by the monitor, one of the players was chosen to roll a die to determine the order of customers from the six shown in Appendix A. Quantities and profits were then calculated, and each subject received a slip of paper with the following information: own price, opponent price, own quantity and profit for the round, and total profit up to that round. The next round would then begin.

After the last round of play, subjects were paid in cash. A subject’s payment was determined by her final lab-money balance, which was equal to her initial balance plus the (possibly negative) sum of her profits in all rounds. This final balance was converted into US dollars at an exchange rate that had been announced at the beginning of the session.<sup>16</sup> Sessions lasted between 75 and 90 minutes.

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<sup>15</sup>Because subjects were not told how many rounds there would be, there is the possibility of supergame effects in our experiment, particularly due to the repeated play against the same opponent. For example, as long as the probability of continuing the game for an additional round is high enough, the collusive outcome can be sustained as a subgame perfect equilibrium. However, subjects were told publicly that the session would last no more than 90 minutes, so they could not have thought the game was infinitely repeated, and it is likely that they had some idea of when the last round would be—or at least an upper bound on when it would be. This should be enough to eliminate supergame effects, though they may still be possible if subjects’ upper bounds are not common knowledge, depending on the nature of their beliefs about each other. Even if subjects did treat the game as infinite (or act as though they do), it should be noted that the size of the incentive for any particular type of supergame play is unaffected by the level of sunk costs.

<sup>16</sup> We did not pay subjects a showup fee per se, but in order to roughly equalize the expected earnings of subjects (for university human subjects approval), we varied across treatments the initial lab-money balances and the exchange rate between lab money and real money. In order to avoid confounding the effects of changes in the sunk-cost level with those of changes in the initial balance and/or exchange rate, we also varied these within some treatments. Subjects in the 2000 cell received either a zero initial balance and an exchange rate of 5000 experimental dollars=1 US dollar, or an initial balance worth \$10 in real money and a rate of 10000 experimental dollars=1 US dollar. Subjects in the 3000 and 4000 cells received a zero initial balance and an exchange rate of 5000 experimental dollars=1 US dollar. Subjects in the 5000 cell received an initial balance worth \$15 in real money and a rate of 5000 experimental dollars=1 US dollar. Subjects in the 6000 cell received an initial balance worth either \$20, \$25, or \$30, and a rate of 5000 experimental dollars=1 US dollar. Subjects in the 7000 cell received an initial balance worth \$25 and a rate of 5000 experimental dollars=1 US dollar. In Section 4.2, we show some evidence that variation in the initial balance does not seem to affect behavior. We also note that our design allows for subjects to earn negative amounts, even including the initial balance. In the event, all subjects had positive earnings, though there remains the possibility that limited liability (our lack of ability to collect money owed by subjects earning negative amounts) may have affected behavior in some way.

Subjects earned an average of roughly \$19.00 for participating in each session, with a high amount of variance: earnings for individual subjects ranged from about \$5 to about \$35.

## 4 Experimental results

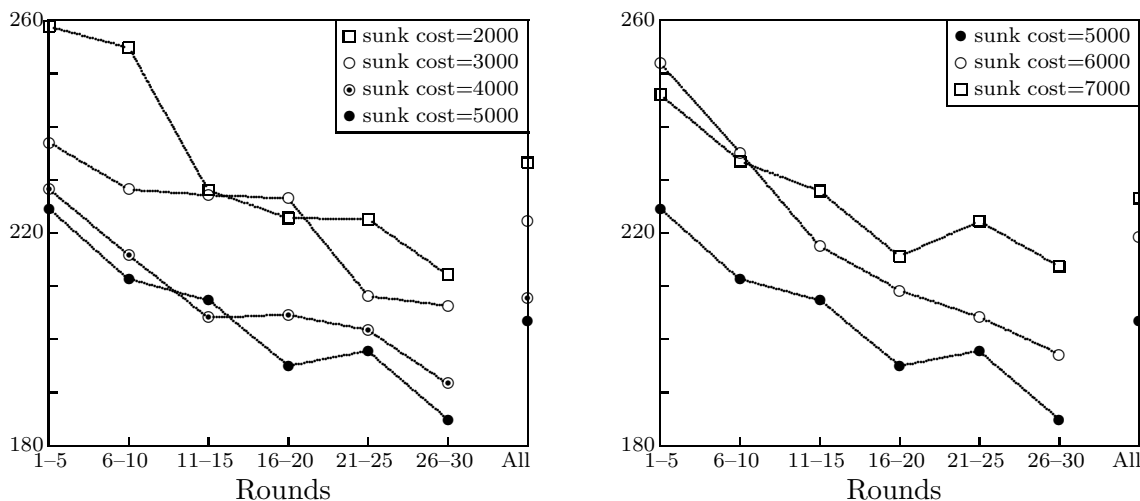
Six sessions were conducted with each sunk-cost level, for a total of 36 sessions with 144 subjects.<sup>17</sup>

We look first at treatment-wide aggregates in Section 4.1, then at more disaggregated behavior in Sections 4.2 and 4.3.

### 4.1 Aggregate behavior

Some of the main qualitative features of subject behavior in this experiment can be seen in Figure 3. This figure shows, for each treatment, the mean choice of price over each 5-round block, aggregated over all subjects in that treatment. Several features of the data are immediately apparent from this

Figure 3: Average choice of price (5-round averages and all-round averages)



figure, all of which cast doubt on the ability of any of the static models—tacit collusion, competitive equilibrium, Nash equilibrium, and cost-based pricing—to characterize subject behavior in this experiment. First, we see that in each treatment, mean prices tend to decline over time, from between 220 and 260 in the first five rounds, to between 180 and 220 in the last five, depending on the sunk-cost level. (Static models, of course, predict stationary behavior.) The decrease in price choices over time can be detected easily by nonparametric statistical tests. For each treatment, average price choices are significantly higher in the first five rounds than in the last five (two-tailed Wilcoxon signed-ranks test, individual-pair data,  $p < .01$  for the SC2000, SC5000, and SC6000 treatments,  $p < .05$  for the other treatments), and prices decrease significantly over the six five-round blocks (two-tailed Page

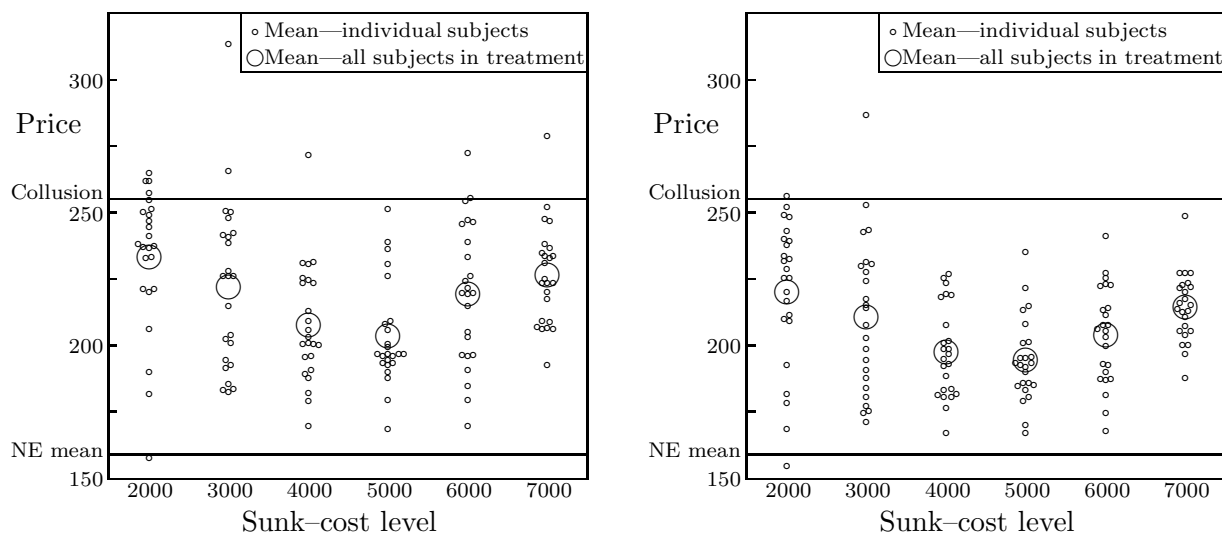
<sup>17</sup>The raw data are available from the corresponding author upon request.

test for ordered alternatives, individual-pair data,  $p < .02$  for the SC3000 treatment,  $p < .002$  for each of the other treatments).<sup>18</sup>

Second, the point predictions of these three models also fare badly as descriptions of aggregate behavior in the experiment. Thirty-round mean price choices vary across treatments from just under 200 to just under 230—less than the collusive price level of 255, but well above the mixed-strategy Nash equilibrium price level of just under 160, let alone the competitive price level of at most 105. (However, the decreases in average price over time mentioned above could be considered weak evidence in favor of Nash equilibrium or competitive equilibrium as an asymptotic prediction.) Differences between average price choices according to Nash equilibrium and those from the observed data are significantly different at the 1% level (Wilcoxon signed-ranks test, individual-pair data) for the experiment as a whole (72 pairs), as well as for each treatment (12 pairs each). Differences between observed price choices and those implied by either tacit collusion or competitive equilibrium are also significant at the 0.1% level for each treatment and the entire experiment.

Third, Figure 3 suggests that the sunk-cost level has a substantial effect on price choices, and that this effect is U-shaped; as the sunk cost increases from 2000 to 5000, price choices decrease (left panel), while from 5000 to 7000, price choices increase (right panel). This effect is even more apparent in Figure 4. The left panel of this figure shows the 30-round treatment-wide mean price choices for each treatment, as well as each individual subject's 30-round mean price choice. The right panel of this

Figure 4: Average choices of price (left) and average transaction prices (right)—all rounds



<sup>18</sup>Because there was no strategic interaction and no communication across pairs of subjects, even in the same experimental session, it seems reasonable to consider data from subject pairs to be independent of data from other subject pairs. Price choices from an individual subjects, on the other hand, are not independent of those from the other subject in the pair. The subject pair is thus the smallest statistically independent unit. See Siegel and Castellan (1988) for thorough descriptions of the nonparametric statistical tests used in this paper. Critical values for the robust rank-order test statistic used later in this section are from Feltovich (2005).

figure does the same for the mean transaction prices for each treatment and each individual subject. The mean transaction price is a weighted average of price choices, where the weighting for each price is proportional to the quantity sold at that price (since lower prices tend to sell more units, this measure tends to weigh lower prices more heavily compared to mean price choices). The collusive and Nash equilibrium prices are also shown in both panels, for comparison. (The competitive prices would be well below the bottom of the figure.) Note that only one subject averages below the Nash equilibrium expected price, though a few others are close. Also, only a few subjects consistently choose prices at or above the collusive level (left panel), and those who do sell very few units (right panel). No subject chooses prices that average anywhere close to the competitive prices. (In fact, fewer than 1% of price choices overall are at or below the highest competitive equilibrium price of 105, and only about 1.6% are at or below 130.)

Nonparametric statistical tests verify that sunk-cost levels do affect mean prices. The null hypothesis that sunk cost has no effect on average price choices can be easily rejected when session-level data are used (Kruskal-Wallis one-way analysis of variance,  $p < .01$ ), and the rejection is even stronger when data from individual pairs are used ( $p < .001$ ). Of course, however, these tests show only that prices are not the same in all treatments, not that the U-shaped relationship suggested by Figure 4 is the correct alternative.

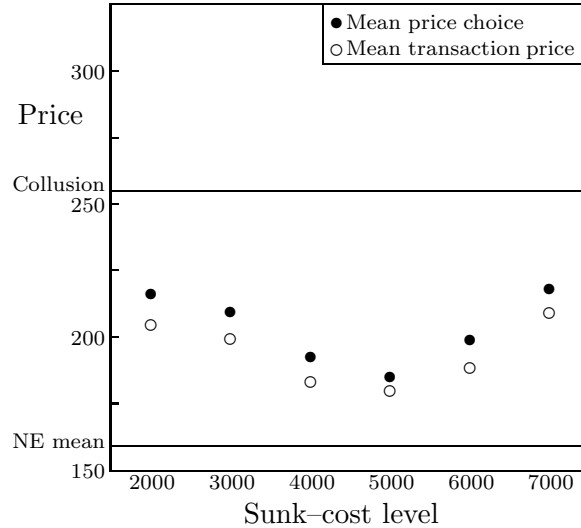
Figure 3 shows that in all treatments, there is a substantial amount of variation in prices over time. One might wonder if the U-shaped relationship between sunk costs and prices may actually be due to differences in early-round behavior that go away as subjects become more experienced in this strategic environment. To address this possibility, we report in Figure 5 average price choices and transaction prices for only the last five rounds of each treatment. (Using the last ten rounds instead of the last five rounds gives results that are broadly similar.) As the figure shows, the U-shaped relationship between sunk costs and prices continues to hold even when we look only at late rounds of play, and we can reject the null hypothesis that sunk cost has no effect on average price choices in these rounds (Kruskal-Wallis one-way analysis of variance, individual-pair data,  $p < .001$ ).

One might also wonder if the effect of sunk costs on prices is due entirely to introspection at the beginning of the session. As Figure 3 shows, differences in prices across treatments are already present by round 5, suggesting that they might even be present in round 1. Table 2 shows that this is not the case. Except for the SC2000 treatment, mean first-round price choices are essentially the same

Table 2: Mean round-1 price choices

SC2000	SC3000	SC4000	SC5000	SC6000	SC7000
258.67	238.29	242.08	238.25	249.88	243.50

Figure 5: Late-round average price choices and average transaction prices (rounds 26–30)



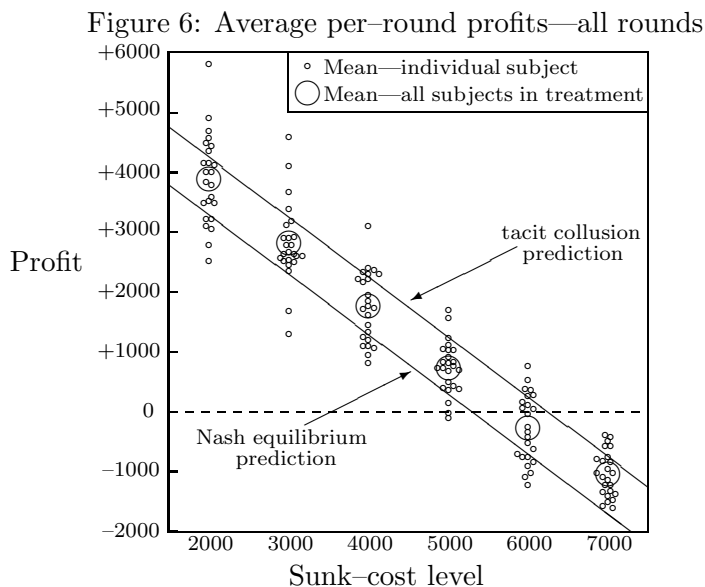
across the board. A Kruskal–Wallis one–way analysis of variance fails to reject the null hypothesis that sunk cost has no effect on first–round price choices in these rounds (individual data,  $p > .10$ ). Even pairwise tests between the one apparent outlier (SC2000) and the other treatments find no significant differences (two–tailed robust rank–order tests, individual data,  $p > .20$  for all pairwise comparisons).<sup>19</sup> On the other hand, a Kruskal–Wallis test on round–5 prices does find a significant difference (individual–pair data,  $p < .01$ ), suggesting that even though the sunk cost has no apparent effect on initial choices, its effect develops quickly.

Because subjects’ choices systematically deviate from the Nash equilibrium prediction for this game, it is worth considering whether these deviations cause them to suffer lower payoffs than they would have earned had they all been playing equilibrium strategies. Figure 6 suggests that, by and large, this is not the case; indeed, the opposite seems to be true. This figure shows the mean per–round profit for every subject over the 30 rounds of the experimental session, as well as the average over all subjects in each treatment. Also shown are the Nash equilibrium and tacit–collusion levels of profit for each treatment. Depending on the sunk–cost level, between 67% and 100% of individual subjects earn profits greater than the Nash equilibrium expected profit, and between 71% and 92% earn profits less than they would under tacit collusion, with between 42% and 79% strictly in between these levels. If we look at treatment–wide mean profits (aggregated over all subjects under a given sunk–cost level), we see that these are always strictly between the Nash equilibrium and tacit–collusion levels.

<sup>19</sup>In round 1, individual choices are independent of each other, even within a matched pair; thus our tests use individual–rather than pair–level data. Using pair–level data, there are still no significant differences at the 10% level. We also note here that caution is needed when taking failure to reject a null hypothesis to imply rejection of the alternative hypothesis, so we stop short of concluding that there is no systematic difference in first–round prices, though the very high  $p$ –values suggest that such a conclusion might be warranted here. Nevertheless, in our later parametric statistical analysis (Section 4.2), we continue to allow for the possibility that later behavior is associated with first–round choices.



Indeed, even if we aggregate only to subject–opponent pairs, we find that all but six pairs collectively earns more than the equilibrium profit, and all but four pairs earn less than the tacit–collusion profit. Nonparametric tests on pair–level data confirm that profits are significantly more than in equilibrium and significantly less than tacit–collusion levels (Wilcoxon signed–ranks test,  $p < .01$  for each sunk–cost level and for the entire experiment). These results suggest the possibility that deviations from Nash equilibrium are not random mistakes, but rather attempts to tacitly collude, though these attempts are only partially successful.



In order to make meaningful statistical comparisons of profits across treatments, we look at gross profits—before subtracting the sunk cost. Not surprisingly, average gross profits are highest when average prices are highest—when sunk cost is either 2000 or 7000. We can reject the null hypothesis of equal profits across treatments (Kruskal–Wallis one–way analysis of variance, individual–pair data,  $p < .001$ ). Pairwise comparisons show that profits are higher in the SC2000 treatment than in the SC5000 and SC6000 treatments (two–tailed robust rank–order test, individual–pair,  $p < 0.10$ ), and higher in the SC7000 treatment than in the SC3000, SC4000, SC5000, and SC6000 treatments ( $p < 0.01$ ,  $p < 0.05$ ,  $p < 0.001$ , and  $p < 0.001$  respectively). We find no significant differences in any other pairwise comparisons of profits.

## 4.2 Sunk–cost effect—parametric statistical analysis

In an effort to look more rigorously at the effect of the sunk–cost level on prices, we next examine the results of regressions using individual subjects’ current–round price choices as the dependent variable. We include the sunk–cost level on the right–hand side in one of two ways: first, as five indicator variables taking on the value of one when the sunk–cost level is 2000, 3000, 4000, 6000, and

7000 respectively (we leave out 5000 to avoid perfect collinearity with the constant term), and zero otherwise; second, as two continuous variables, one for the sunk cost itself and one for the square of the sunk cost. We also include variables for the initial lab–money balance (per round, so as to be comparable with the sunk cost) and its square, and one for the round number. Finally, as Figure 3 suggested that initial price choices might at least partly explain the variation in later price choices, we included the subject’s first–round price choice on the right–hand side of some of our regressions.

Since price choices were restricted to be no lower than 5 and no higher than 500, we use a Tobit model.<sup>20</sup> However, since there were so few choices near these endpoints—there were no price choices below 50, and only one above 450—OLS regressions we ran gave nearly the same results. Also, in order to address possible heterogeneity across subjects, we included individual–subject random effects in the model. The results are shown in Table 3. We report coefficients and standard errors for each right–hand–side variable, and log likelihoods for each regression.

Table 3: Current price choices—Tobit regressions with random effects (standard errors in parentheses)

Dependent variable: $p_t$	Model 1 (rounds 1–30)	Model 2 (rounds 1–30)	Model 3 (rounds 2–30)	Model 4 (rounds 2–30)
constant	224.310*** (10.948)	312.054*** (17.084)	203.882*** (13.099)	292.424*** (18.693)
SC2000 treatment	33.356*** (9.071)	—	33.270*** (8.964)	—
SC3000 treatment	21.742* (11.952)	—	25.414** (11.830)	—
SC4000 treatment	7.238 (11.952)	—	10.008 (11.823)	—
SC6000 treatment	27.365* (14.618)	—	24.608* (14.471)	—
SC7000 treatment	33.964** (13.934)	—	32.164** (13.784)	—
sunk cost/1000	—	−33.797*** (8.632)	—	−32.826*** (8.548)
(sunk cost/1000) <sup>2</sup>	—	3.567*** (1.053)	—	3.448*** (1.042)
initial balance/1000	5.876 (8.655)	−3.160 (4.742)	7.078 (8.552)	−2.909 (4.686)
(initial balance/1000) <sup>2</sup>	−1.867 (2.221)	0.838 (1.256)	−1.863 (2.194)	0.835 (1.241)
round	−1.538*** (0.077)	−1.538*** (0.077)	−1.508*** (0.077)	−1.508*** (0.079)
$p_1$	—	—	0.069*** (0.028)	0.069** (0.028)
−ln(L)	22624.004	22625.551	21753.269	21754.879
Bayesian info. crit.	45340.09	45318.07	43606.58	43584.79
Akaike info. crit.	45270.01	45267.10	43530.54	43527.76

\* (\*\*,\*\*\*): Coefficient significantly different from zero at the 10% (5%, 1%) level.

These results have several noteworthy features. First, we note that because neither Model 1 nor Model 2 is nested in the other, we cannot use a straightforward likelihood–ratio test to compare them; the same is true for Models 3 and 4. Also, Models 3 and 4 use a different sample from that of Models 1 and 2 (rounds 2–30 in the former versus all rounds in the latter), so these also cannot be

<sup>20</sup>We note that our experiment also restricted prices to take on only whole–dollar amounts, so price is not really a continuous variable. However, 496 prices were possible, so continuity is probably a reasonable approximation.

compared with likelihood–ratio tests. However, we can use “information criteria” that, like likelihood–ratio tests, reward goodness–of–fit but punish free parameters. The Bayesian (also known as Schwarz (1978)) Information Criterion and the Akaike (1974) Information Criterion are the most commonly used; their formulas are  $BIC = -2 \cdot \ln(L) + \ln(N) \cdot k$  and  $AIC = -2 \cdot \ln(L) + 2k$ , respectively, where  $\ln(L)$  is the maximized log likelihood,  $N$  is the sample size, and  $k$  is the number of free parameters of the model.<sup>21</sup> The values for these criteria are shown in the table for all four models. Both criteria favor the models with the sunk cost and its square over the models with indicator variables for the treatments, and Model 4 performs best of all four models (though it turns out that all four models by and large yield similar implications).

We look next at the effect of the sunk cost. In Models 1 and 3, the sunk–cost–level indicator variables are used. Taken as a group, they are jointly significant ( $\chi^2 = 23.52$ ,  $df = 5$ ,  $p < 0.001$  in Model 1,  $\chi^2 = 23.23$ ,  $df = 5$ ,  $p < 0.001$  in Model 3). In Models 2 and 4, the sunk–cost level and its square are used. In both models, the coefficient of the sunk–cost level itself is significant and negative, while the coefficient of its square is significant and positive. The two variables’ coefficients are jointly significantly different from zero ( $\chi^2 = 19.96$ ,  $df = 2$ ,  $p < 0.001$  in Model 2,  $\chi^2 = 19.54$ ,  $df = 2$ ,  $p < 0.001$  in Model 4), and their signs imply that price and sunk cost have the U–shaped relationship seen in Figure 4. Using the values from Model 4—the best–fitting model as found above—the coefficients for the sunk cost and its square imply a point estimate for the price–minimizing sunk–cost level of \$4761 and a 95% confidence interval of (4092,5429). (Using Model 2 instead of Model 4 lowers the point estimate to \$4738 and narrows the confidence interval to (4092,5383).)

Estimates of the size of the effect of the sunk–cost level based on Models 2 and 4 are shown in Table 4; these estimates are given relative to a sunk cost of 5000 (for which prices are lowest on average). These numbers suggest that the effect of the sunk cost on price choices is not only

Table 4: Point estimates and 95% confidence intervals for effect of sunk–cost level on price choices (vs. sunk cost=5000)

Sunk–cost level	(lower endpoint, <b>point estimate</b> , upper endpoint)	
	Model 2	Model 4
2000	(+14.26, + <b>26.48</b> , +38.72)	(+13.97, + <b>26.08</b> , +38.18)
3000	(+3.54, + <b>10.52</b> , +17.50)	(+3.59, + <b>10.49</b> , +17.39)
4000	(–2.34, + <b>1.70</b> , +5.73)	(–2.19, + <b>1.80</b> , +5.78)
5000	(0.00, <b>0.00</b> , 0.00)	(0.00, <b>0.00</b> , 0.00)
6000	(–1.63, + <b>5.44</b> , +12.51)	(–1.89, + <b>5.10</b> , +12.09)
7000	(+0.16, + <b>18.01</b> , +35.86)	(–0.66, + <b>17.09</b> , +34.74)

statistically significant, but also economically relevant; for example, a sunk cost of 2000 leads to an

<sup>21</sup>The BIC is equivalent to Klein and Brown (1984)’s “minimum–prior–information” posterior–odds criterion.

increase in price of over 10% relative to a sunk cost of 5000, and a sunk cost of 7000 leads to an increase nearly as large.

Finally, we look at the remaining variables in our regressions. For both models, the coefficient for the round number is negative and significant, consistent with the declines over time observed in Figure 3. Also, the coefficient for the first-round price (in Models 3 and 4) is positive and significant, implying a positive correlation between decisions made in the first round and decisions made in later rounds.<sup>22</sup> However, even when differences in first-round decisions are taken into account, the sunk-cost level continues to have an effect. Lastly, the coefficients for the initial lab-money balance and its square are not significantly different from zero in either model, nor are they jointly significant ( $\chi^2$  ranges from 0.45 to 0.83 across the four models,  $df = 2$ ,  $p > 0.66$ ). On the face of it, this may seem strange; the initial balance plays the same role in subjects' payoffs as the sunk-cost level, in that both affect the level of subjects' payoffs, but not the marginal costs or benefits they face. We speculate that subjects frame the initial balance and the sunk-cost level differently, possibly due to the different way in which these features are presented to the subjects. The initial balance is told to the subjects before play begins, but no mention of it is made during the 30 rounds of the session. In contrast, the sunk-cost level is presented to the subjects over and over, in their feedback at the end of each round, part of which is the subject's profit, which of course depends on the sunk cost. This difference in presentation between the initial balance and the sunk-cost level may lead to a difference in salience between them, causing subjects to ignore the size of the initial balance during the experiment, while taking the sunk-cost level into account in their choosing of prices.<sup>23</sup>

### 4.3 Individual behavior

The previous sections give plenty of evidence that the static models—competitive equilibrium, tacit collusion, and Nash equilibrium—fail to adequately characterize the experimental data. We have seen mixed evidence for Edgeworth cycle theory: on one hand, average price choices decrease over time through the interval of Edgeworth cycle prices (as Edgeworth cycle theory predicts); on the other hand, prices also vary as the sunk-cost level changes (as it does not). In this section, we look further at the extent to which Edgeworth cycle theory describes the experimental data. In order to do so, we concentrate on individual decision making: specifically, how price choices depend on results from

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<sup>22</sup>We are agnostic as to the explanation for this correlation. One possibility is that the initial choices of the subject or the opponent have some effect on later choices, through inertia or through positive feedback. A second possibility is that subjects vary in some unobservable characteristic, not picked up by the random effects, that affects the level of their price choices, such as “cooperativeness” or “aggressiveness”; and such variation affects first-round prices and later-round prices in the same way. Still other explanations may exist.

<sup>23</sup>There is evidence that in corporate settings, profit presentation does affect behavior in seemingly irrational ways. For example, Graham, Harvey, and Rajgopal (2005) survey 400 senior executives and report that 78% would sacrifice long-term economic value in order to smooth reported accounting profits over time.

recent rounds.

Some suggestive evidence is found by simply looking at the way subjects adjust their prices in response to their opponents' previous-round prices. It is worth pointing out the similarity between Edgeworth cycle theory and Selten and Stoecker's (1986) "direction learning theory". When a game has a one-dimensional strategy space, direction learning theory predicts that when a player changes strategy from one round to the next, the change will be in the direction of the best response. Many games' strategy sets do have at least a partial linear ordering.<sup>24</sup> Even in games for which determining the best response to an opponent's previous-round strategy is impossible—because of imperfect observability of opponent strategies—or prohibitively difficult, for computational reasons, it may still be comparatively easy to know the *direction* of this best response, relative to the player's own previous-round strategy. In many cases, the direction of the best response depends on whether the outcome of the game from the player's standpoint could be characterized as a "success" or a "failure".<sup>25</sup>

Table 5 takes a first look at how subjects' price choices depend on aspects of the previous round's result. This table consists of two transition matrices. The range of allowable prices is partitioned into eight intervals: the second through fifth intervals make up the Edgeworth cycle, and these, along with the sixth and seventh, are the prices that are best responses to some opponent prices. (Notice that these seven intervals are of approximately equal size.) We then classify each pair of previous-round price and current-round price according to which intervals the two prices lie in. The top matrix in Table 5 consists of those pairs where the subject's previous-round price was *lower* than the opponent's previous-round price; in the bottom matrix, the subject's previous-round price was *higher* than the opponent's previous-round price.

In Table 5, there are apparent differences between entries in the top sub-table and corresponding entries in the bottom sub-table, so it does seem beneficial to break down price choices according to whether the subject's price was greater or less than the opponent's price. The data in this table are consistent with direction learning theory and Edgeworth cycle theory. Almost regardless of what price a subject chose in a particular round, if it was less than the opponent's price (top sub-table), that subject's price in the following round was likely to be either in the same interval or the next—

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<sup>24</sup>Besides Selten and Stoecker, who used it to describe behavior in a repeated Prisoners' Dilemma (where pure strategies were ordered according to the first round they chose to defect), direction learning theory has been used by Selten and Buchta (1999), Kagel and Levin (1999), and Güth et al. (2003) for auctions, by Selten, Abbink, and Cox (2002) for lemons-market problems, by Nagel and Tang (1998) for the centipede game, and by Grosskopf (2003, 2004) for the ultimatum game and a variant of the ultimatum game. We also note here that the results in this section are consistent with other models of behavior, such as imitation of successful strategies; see, for instance, Offerman, Potters, and Sonnemans (2002) and Apesteguia, Huck, and Oechssler (2007).

<sup>25</sup>As an example, in Selten and Stoecker's (1986) repeated Prisoners' Dilemma, "success" was when a player defected first, and "failure" when her opponent defected first. After a success, it is likely (though not certain) that a higher payoff would have been earned by waiting longer to defect, while after a failure, a better payoff may have been earned by defecting sooner. So, direction learning theory predicts that after success, the player will defect later, while after failure, she will defect earlier.

Table 5: Distribution of current price conditional on previous-round prices, rounds 2–30 (in %)

Previous-round price	Current-round price (when own previous-round price was <b>lower</b> than opponent's)								
	N	5–144	145–170	171–195	196–220	221–245	246–270	271–305	306–500
5–144	103	<b>21.4</b>	<b>25.2</b>	9.7	19.4	1.0	8.7	9.7	4.9
145–170	314	7.3	<b>46.2</b>	<b>21.3</b>	11.2	2.9	5.1	3.8	2.2
171–195	531	0.6	7.5	<b>53.9</b>	<b>24.5</b>	5.1	4.7	2.6	1.1
196–220	538	0.4	2.2	11.7	<b>48.9</b>	<b>16.5</b>	11.0	6.3	3.0
221–245	223	0.4	0.4	3.1	<b>19.3</b>	<b>51.6</b>	14.4	9.9	0.9
246–270	156	1.3	0.0	2.6	9.0	9.6	<b>43.7</b>	<b>24.4</b>	9.6
271–305	59	1.7	0.0	5.1	6.8	1.7	<b>20.3</b>	<b>44.1</b>	<b>20.3</b>
306–500	7	0.0	0.0	0.0	0.0	0.0	0.0	<b>57.1</b>	<b>42.9</b>
Previous-round price	Current-round price (when own previous-round price was <b>higher</b> than opponent's)								
	N	5–144	145–170	171–195	196–220	221–245	246–270	271–305	306–500
5–144	4	<b>75.0</b>	0.0	0.0	0.0	<b>25.0</b>	0.0	0.0	0.0
145–170	94	10.6	<b>52.2</b>	<b>14.9</b>	7.4	2.1	9.57	3.2	0.0
171–195	272	1.5	<b>26.8</b>	<b>52.9</b>	10.3	2.2	2.2	1.8	2.2
196–220	407	2.5	10.6	<b>28.3</b>	<b>42.8</b>	4.2	6.6	3.9	1.2
221–245	299	2.0	2.0	11.4	<b>33.1</b>	<b>35.1</b>	9.7	4.7	2.0
246–270	362	1.4	4.7	10.2	<b>25.1</b>	20.7	<b>27.1</b>	7.2	3.6
271–305	302	3.0	5.6	7.0	17.5	12.3	<b>25.8</b>	<b>23.2</b>	5.6
306–500	191	0.5	3.1	5.2	15.2	8.4	18.3	<b>22.0</b>	<b>27.2</b>

Note: Boldface entries correspond to the two most frequent intervals in each row.

higher interval. On the other hand, if the subject's price turned out to be more than the opponent's price (bottom sub-table), that subject's price in the following round was typically either in the same interval or the next-lower interval. These two intervals make up between two-fifths and four-fifths of all following-round choices, depending on the current-round price and whether it was higher or lower than the opponent price.

We next use parametric statistics in order to look more closely at the factors that influence subjects' round-to-round price changes. First, we run OLS regressions with individual-subject random effects on the entire data set, with the price change ( $p_t - p_{t-1}$ ) as our dependent variable. As before, we use the round number, the sunk cost, and the square of the sunk cost as right-hand-side variables.<sup>26</sup> We also include several aspects of subjects' previous results: the subject's own previous-round price ( $p_{t-1}$ ), her lagged price change ( $p_{t-1} - p_{t-2}$ ), indicators for her having chosen a lower price than her opponent in the previous round ("Lower") or the same price as her opponent ("Same"), and a variable equal to the difference between that and the best response to her opponent's previous-round price ( $p_{t-1} - BR(p_{t-1}^o)$ ). The coefficient of this last variable will capture adjustment of the subject toward a myopic best response, though of course the actual best response may be different if the opponent is expected to change his price from one round to the next.

In addition, we define six new variables, meant to capture the effect on subjects' price changes of losses incurred in the previous round. We consider the possibility that subjects' price changes depend at least partly on whether they earned positive or negative profits in the previous round; thus, we

<sup>26</sup>Using sunk-cost indicator variables instead of the sunk-cost level and its square results in no substantial changes in the coefficients and significance of other variables.

include the indicator variable “Loss”, which takes a value of one if the subject earned a negative profit in that round (inclusive of the sunk cost), as well as the product of this indicator with the subject’s own previous-round price. To guard against the possibility that any significant coefficients for these variables are due to a spurious correlation, we create two similar variables: “Loss+”, which takes a value of one if the subject earned a profit less than 1000 in the previous round, and “Loss–”, with a value of one if the subject earned a profit less than –1000 in the previous round. Finally, we include the products of each of these variables with the subject’s previous-round price. If subjects do treat gains and losses differently, the two “Loss” variables will have a significant effect, but the “Loss+” and “Loss–” variables will not.

Results of regressions using four combinations of these variables are shown in Table 6. Again, we report coefficients and standard errors for each variable, as well as log likelihoods for each model. Note

Table 6: Price changes—OLS with random effects, rounds 3–30 (standard errors in parentheses)

Dependent variable: $p_t - p_{t-1}$	Model 5	Model 6	Model 7	Model 8
constant	134.480*** (13.341)	132.917*** (12.941)	101.035*** (8.021)	98.479*** (7.342)
sunk cost/1000	-16.064*** (4.707)	-16.583*** (4.661)	—	—
(sunk cost/1000) <sup>2</sup>	1.810*** (0.518)	1.837*** (0.511)	—	—
round	-0.726*** (0.086)	-0.726*** (0.086)	-0.713*** (0.087)	-0.714*** (0.087)
$p_{t-1}$	-0.427*** (0.032)	-0.420*** (0.029)	-0.416*** (0.032)	-0.410*** (0.029)
$p_{t-1} - p_{t-2}$	-0.062*** (0.015)	-0.062*** (0.015)	-0.062*** (0.015)	-0.062*** (0.015)
$p_{t-1} - BR(p_{t-1}^o)$	-0.175*** (0.022)	-0.174*** (0.022)	-0.180*** (0.022)	-0.181*** (0.022)
“Lower” ( $p_{t-1} < p_{t-1}^o$ )	7.729*** (2.766)	8.235*** (2.713)	7.913*** (2.745)	8.157*** (2.796)
“Same” ( $p_{t-1} = p_{t-1}^o$ )	3.361 (2.426)	4.542** (2.233)	3.811 (2.341)	4.620*** (2.199)
“Loss” ( $\Pi_{t-1} < 0$ )	21.223* (12.177)	24.827*** (6.954)	20.454* (12.173)	25.782* (6.906)
Loss · $p_{t-1}$	-0.105* (0.056)	-0.125*** (0.028)	-0.103* (0.056)	-0.128*** (0.028)
“Loss+” ( $\Pi_{t-1} < +1000$ )	-3.424 (9.077)	—	-2.660 (9.034)	—
Loss+ · $p_{t-1}$	-0.0004 (0.043)	—	-0.003 (0.043)	—
“Loss–” ( $\Pi_{t-1} < -1000$ )	5.343 (11.180)	—	8.966 (11.132)	—
Loss– · $p_{t-1}$	-0.017 (0.047)	—	-0.028 (0.046)	—
–ln(L)	20686.912	20688.633	20692.960	20694.986

\* (\*\*, \*\*\*): Coefficient significantly different from zero at the 10% (5%, 1%) level.

that Model 5 nests the other three models, while Model 8 is nested in the other three. Likelihood-ratio tests choose Model 5 over either Model 7 ( $\chi^2 = 12.28$ ,  $d.f. = 2$ ,  $p \approx 0.002$ ) or Model 8 ( $\chi^2 = 16.30$ ,  $d.f. = 6$ ,  $p \approx 0.012$ ), but not over Model 6 ( $\chi^2 = 3.44$ ,  $d.f. = 4$ ,  $p \approx 0.487$ ). Similarly, Model 6 is

preferred over Model 8 ( $\chi^2 = 12.94$ ,  $d.f. = 2$ ,  $p \approx 0.002$ ), but Model 7 is not ( $\chi^2 = 4.05$ ,  $d.f. = 4$ ,  $p \approx 0.399$ ). Neither Model 6 nor Model 7 nests the other, but Model 6 beats Model 7 according to either the Bayesian Information Criterion (41485.19 versus 41510.45) or the Akaike (1974) Information Criterion (41403.27 versus 41415.92). We conclude that Model 6 outperforms the other three models; however, as with the previous set of regressions, we report the results of all of them because they show the robustness of our results.

As the table shows, the sunk-cost level has an effect not only on the levels of price choices (as was seen in Table 3), but also on round-to-round changes in these prices. Indeed, this effect is also U-shaped (with minimum at \$4435 according to Model 6 and \$4514 according to Model 8, and 95% confidence intervals [3997,4874] and [4105,4924] respectively); that is, prices decrease most sharply when the sunk-cost level is in the middle, with slower changes for low and high values. Some support for the Edgeworth cycle hypothesis comes from the negative and significant coefficient for  $p_{t-1} - BR(p_{t-1}^o)$  (the difference between the subject's own price and the best response to her opponent's price). We also see that price changes are affected negatively by both the level of the previous-round price ( $p_{t-1}$ ) and last round's price change from the round before ( $p_{t-1} - p_{t-2}$ ), and that beyond all of these effects, simply having had a price lower than (and in Models 6 and 8, the same as), the opponent's price has a significant effect on the price change.

In all four models, the “Loss” variable and its interaction with the previous-round price are significant, both individually and jointly, suggesting that subjects do indeed update their price choices differently based on whether they earned profits higher or lower than zero in the previous round. As evidence that this relationship is not spurious, we also see that the “Loss+” and “Loss-” variables and their interactions with the previous-round price are insignificant, both individually and jointly, in both Model 5 and Model 7 (the two in which they appear), so that they do not appear to update prices based on whether profit was higher or lower than +1000 or -1000. In fact, the expressions  $\beta_{\text{Loss}+} + \beta_{\text{Loss}+ \cdot p_{t-1}} \cdot p_{t-1}$  and  $\beta_{\text{Loss}+} + \beta_{\text{Loss}+ \cdot p_{t-1}} \cdot p_{t-1}$  (where  $\beta_Y$  is the coefficient of the variable  $Y$ )—which represent the overall effect of the “Loss+” and “Loss-” variables on subjects' price changes—are not significantly different from zero at the 5% level in either of these models for any price in [5,500].

On the other hand, the expression  $\beta_{\text{Loss}} + \beta_{\text{Loss} \cdot p_{t-1}} \cdot p_{t-1}$  sometimes is significant. In particular, according to Model 6—the best performing of these models as mentioned above—this expression is significant and positive for  $p_{t-1} \leq 147$  and significant and negative for  $p_{t-1} \geq 231$ , implying that (all else equal) incurring a loss in the previous round makes a subject more likely to raise her current-round price if her previous-round price was relatively low, and more likely to lower her current price if the previous price was relatively high. A similar result holds according to Model 8;  $\beta_{\text{Loss}} + \beta_{\text{Loss} \cdot p_{t-1}} \cdot p_{t-1}$



it is significant and positive for  $p_{t-1} \leq 154$  and significant and negative for  $p_{t-1} \geq 231$ . According to Models 5 and 7, on the other hand, this expression is insignificant for all but relatively high, seldom chosen prices (at least 360 according to Model 5 and 338 according to Model 7).

#### 4.4 Loss avoidance: a parsimonious explanation?

Consistent with many other oligopoly experiments (see Section 2), price choices in our experiment are well above the equilibrium prediction. However, the extent to which this is so varies with the sunk-cost level; the non-monotonic relationship we saw between the sunk-cost level and prices is the main puzzle in our results. As mentioned before, this relationship is inconsistent with any of the static theories discussed in Section 2, and the only dynamic theory we considered (Edgeworth cycle theory) is silent as to whether sunk costs should have any effect on prices.

One possible explanation is that subjects are attempting to tacitly collude, and that the level of the sunk cost affects how successful these attempts are. Another possibility is that subjects initially choose prices more or less randomly, but slowly learn over time to choose prices more in line with equilibrium, and the speed of this learning is somehow associated with the sunk-cost level. We do not attempt to distinguish between these two possibilities here, but rather note that either is consistent with prices declining over time from their relatively high starting levels, and in either case, there is still the question of why the sunk-cost level has an effect.

As a small step toward a possible understanding of how a relationship between sunk-cost levels and prices could arise, we begin by noting that the individual-level results in the previous section implied that the subjects treated losses and gains differently in how they updated their behavior from round to round. It therefore seems reasonable that part of the answer could come from *loss avoidance*. Loss avoidance was proposed by Cachon and Camerer (1996) as a way of explaining subjects' differential treatment of gains and losses in coordination games; Rydval and Ortmann (2004) and Feltovich, Iwasaki, and Oda (2008) have found evidence of loss avoidance in Stag Hunt experiments.<sup>27</sup> We follow Feltovich, Iwasaki, and Oda in distinguishing between two types of loss avoidance. A decision maker exhibiting *certain-loss avoidance* will avoid choices leading to certain negative payoffs in favor of alternative choices that might lead to gains. Someone exhibiting *possible-loss avoidance* will avoid choices leading to possible losses in favor of other choices yielding certain gains.

When the number of available opponent strategies in a game is small (as in Feltovich, Iwasaki,

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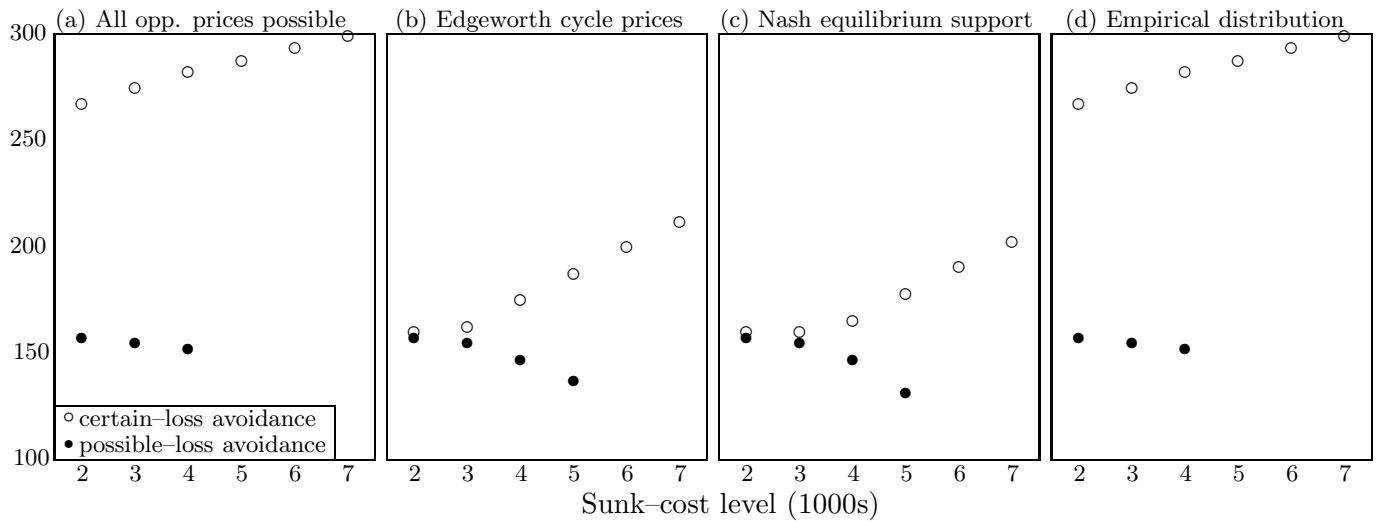
<sup>27</sup>To be precise, Cachon and Camerer considered loss avoidance to be a model of players' beliefs about other players: they did not propose that players avoided losses themselves, but rather that they believed their opponents did so. Of course, to the extent that players' choices are influenced by their beliefs about opponents' choices, loss avoidance would still have an effect on the players' own choices. We also note here that while loss avoidance is similar in spirit to Kahneman and Tversky's (1979) loss aversion, they are not identical; in particular, they sometimes make opposite predictions. See Feltovich, Iwasaki, and Oda for a discussion of the distinction between these two concepts.

and Oda’s and Rydval and Ortmann’s games, where it is two, or as in Cachon and Camerer’s games, where it is seven), the question of which outcomes are “possible” and which are not is relatively straightforward to answer, as it is probably reasonable to view any opponent strategy as being possible. In that case, we could say that a player’s strategy leads to a possible loss (resp., gain) if there is any opponent strategy for which the player’s payoff is negative (resp., positive) in the resulting outcome, and a strategy leads to a certain loss (resp., gain) if for every opponent strategy, the player’s payoff is negative (resp., positive). On the other hand, when the number of available strategies is large—as in our game—it might be more realistic to expect that players view only some of the available opponent strategies as being “possible” strategies. As a result, we generalize Feltovich, Iwasaki, and Oda’s definitions of these phenomena slightly. Specifically, we consider the notion of certain– or possible–loss avoidance with respect to a subset  $A$  of the strategy space;  $A$  will be the set of actions the player believes her opponent will choose with positive probability. (Of course,  $A$  can be the entire strategy set, in which case we revert to the original definitions.) An individual exhibiting possible–loss avoidance with respect to  $A$  will avoid an action that, for some opponent action in  $A$ , will lead to a loss, in favor of another action leading only to gains, as long as the opponent chooses an action in  $A$ . An individual exhibiting certain–loss avoidance with respect to  $A$  will avoid an action that, for all opponent actions in  $A$ , will lead to a loss, in favor of another action that for some opponent action in  $A$ , will lead to a gain.

Under this definition, the set of price choices consistent with certain– or possible–loss avoidance in a particular game will usually depend on which set of opponent actions  $A$  is considered to be possible. Figure 7 shows, for each sunk–cost level, the mean of all price choices that are consistent with certain– or possible–loss avoidance for four reasonable choices of  $A$ : (a) the entire set of prices, (b) the set of prices in the Edgeworth cycle  $\{145, 146, \dots, 245\}$ , (c) the set of prices in the support of the symmetric Nash equilibrium (see Figure 2), and (d) the set of prices actually observed in the experiment at least once. (Note that (a) corresponds to the definitions of certain– and possible–loss avoidance used by Feltovich, Iwasaki, and Oda.)

Figure 7 illustrates two important distinctions between certain– and possible–loss avoidance. First, for a given sunk–cost level, the set of prices consistent with certain–loss avoidance is usually (though not always) larger than the set consistent with possible–loss avoidance. In particular, for the sunk–cost levels used in the experiment, it is always possible to avoid certain losses, but for the higher sunk–cost levels we used, there are no prices that provide safety from any possible loss. Second, certain– and possible–loss avoidance have diverging implications for pricing decisions in this game: the mean of price choices consistent with certain–loss avoidance increases as the sunk–cost level increases, while

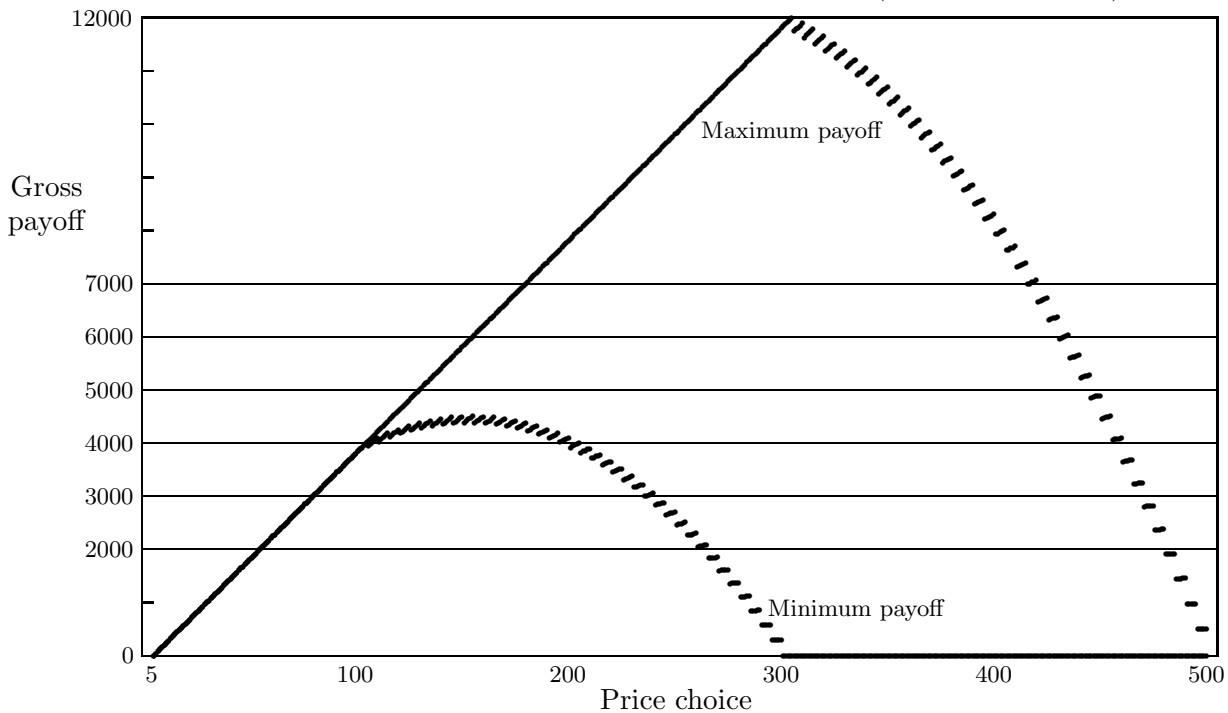
Figure 7: Mean of price choices consistent with loss avoidance, for given beliefs about opponent choices



the mean price choice consistent with possible-loss avoidance (when such prices exist) decreases as the sunk-cost level increases.

To see graphically why certain-loss avoidance implies a rise in prices as the sunk cost increases, while possible-loss avoidance implies a fall, consider Figure 8, which shows, for each price that can be chosen, the lowest and highest per-round payoffs (gross of the sunk cost) that can possibly be obtained in the game, for some opponent choice and realization of the queue of customers. We can

Figure 8: Per-round payoffs possible in the game (gross of sunk cost)



see that very low prices yield one payoff with certainty, while higher prices yield a range of possible payoffs. (Intuitively, low prices result in the firm selling its entire capacity at that price, irrespective

of the rival firm's price; higher prices might yield a high profit if the rival's price was higher, but could yield a low or even zero gross profit if the rival firm undercut it.)

A player's net payoff (including the sunk cost) is positive if the gross payoff is larger than the sunk cost. So, if a player believes that the opponent might choose any of the allowable prices (as in panel (a) of Figure 7), then possible-loss avoidance requires the player to choose a price such that the gross payoff from that price is always more than the sunk cost; that is, the (price, sunk cost) pair must lie below the lower envelope of the graph of (price, payoff) pairs in Figure 8. Prices higher than 300 are ruled out by possible-loss avoidance for any positive sunk cost, as is a price of 5. As the sunk cost increases, the interval of prices consistent with possible-loss avoidance shrinks at both ends. For sunk costs between 2000 and 4000, this interval shrinks more quickly at the right endpoint than at the left endpoint (that is, the lower envelope of the graph of price-profit pairs is steeper on the left than on the right), so the mean price consistent with possible-loss avoidance falls as the sunk cost rises through these levels.

When the sunk cost increases beyond 4000, a player can no longer avoid possible losses, but she can still avoid certain losses by choosing a price such that the gross payoff can be more than the sunk cost (in Figure 8, the (price, sunk cost) pair must be below the upper envelope of the graph of (price, payoff) pairs). As the sunk cost increases, the interval consistent with certain-loss avoidance shrinks at both ends, but does so more quickly at the left endpoint than at the right endpoint. So, the mean price consistent with certain-loss avoidance rises as the sunk cost rises.

Consequently, we can conjecture an explanation for the pattern of prices observed in the experiment: subjects avoid prices leading to possible losses when they are able to do so, and when they cannot, they avoid prices resulting in certain losses. This behavior would result in average prices decreasing as the sunk cost increases through lower values, but increasing as the sunk cost increases through higher values, consistent with what was seen in the data. (See, in particular, the middle two panels of Figure 7, which are similar in some ways to Figures 4 and 5.) We acknowledge that this explanation is not completely satisfactory, both because loss avoidance was not one of our sources of predictions *ex ante*, and because of our ad hoc (though, we think, reasonable) way of combining the two types of loss avoidance. We therefore stop short of claiming that loss avoidance is what is causing the patterns we have seen. However, we do find the similarity between Figure 7 and the experimental results suggestive.

## 5 Conclusion

Standard microeconomic theory predicts that the level of sunk costs should have no effect on firms' pricing behavior. We run an experiment to test this prediction. In our experiment, subjects play a repeated Bertrand–Edgeworth duopoly game, in which sunk costs take on one of six possible values. Neither Nash equilibrium nor any of the alternative models (tacit collusion, competitive equilibrium, Edgeworth cycles, cost–based pricing) adequately characterizes observed pricing behavior in the experiment. Contrary to all of these theories, we find that sunk–cost levels have a substantial (and statistically significant) effect on prices. This effect is non–monotonic: as sunk costs increase up to a point, average prices decrease, but thereafter, average prices increase. We also find that price choices are nonstationary: the aggregate data show a decline in average prices over time in all treatments, and examination of individual data shows that subjects adjust their prices in response to their recent results.

Our research highlights a paradox of decision making applicable to a fairly wide range of industry settings: “irrelevant data” such as sunk costs should be ignored when making decisions (Garrison et al. 2006), but if sunk costs significantly affect the way competitors choose their prices, then these costs are not irrelevant. We speculate that loss avoidance might be at least a partial cause of the particular effect we find; however, further research would be required to test this conjecture. More generally, we hope that our results will encourage other researchers to consider the effects of not only marginal costs and benefits, but the overall levels of costs and benefits, when studying the decision–making behavior of economic agents.

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# Appendix A: Experiment Instructions

## Introduction

The purpose of this business game is to investigate how people use accounting information to set product prices. I hope that you find participating in this research both fun and interesting. In addition, to make the game more realistic, the amount of money you will earn for participating will depend upon the decisions you make.

## Ground rules

1. My commitment to you:

NO DECEPTION. This research will be conducted exactly as described. I promise to relay all information about the experiment accurately and completely.

2. Your commitment to me:

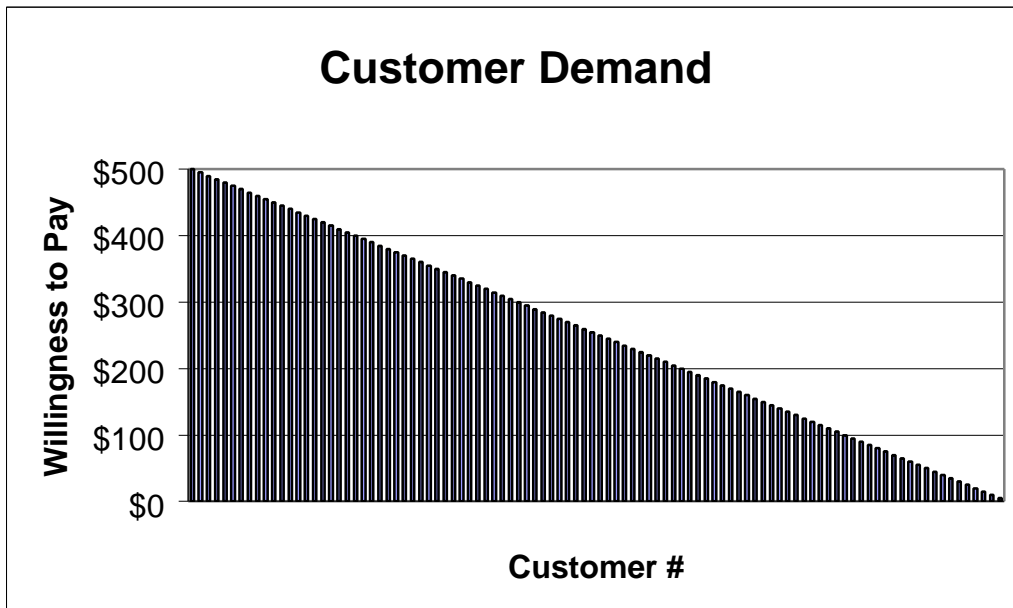
DO NOT DISCLOSE PRIVATE INFORMATION. This is serious research, and I am genuinely interested in the decisions you make. It is important to avoid communicating private information (discussed later) with other participants during the game. If you need clarification or explanation, please raise your hand and I will answer your questions individually.

## Basic Setting

This experiment investigates buying and selling a hypothetical product called a "widget." Widgets are perishable goods that last only one day. In this experiment, buying and selling widgets occurs over a sequence of "days."

All of you will be selling widgets. In order to produce widgets, you will rent a widget machine. All widget machines can produce 40 widgets per day. Because widgets are short-lived, you will produce and sell widgets on the same day (i.e., there is no "inventory".)

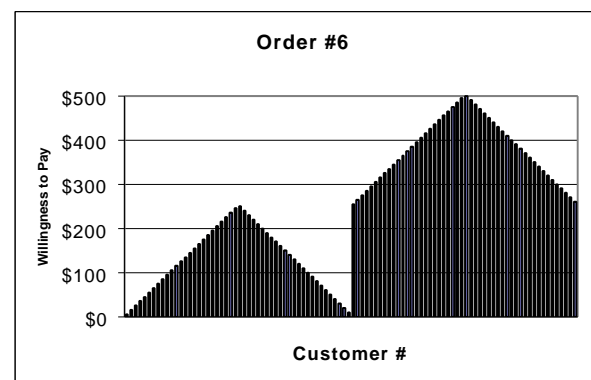
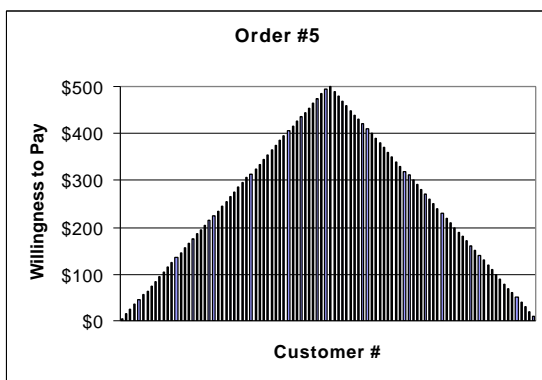
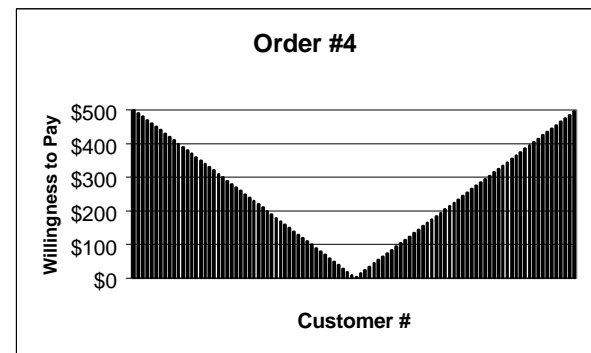
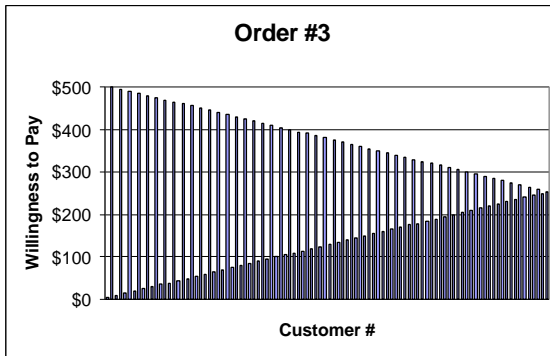
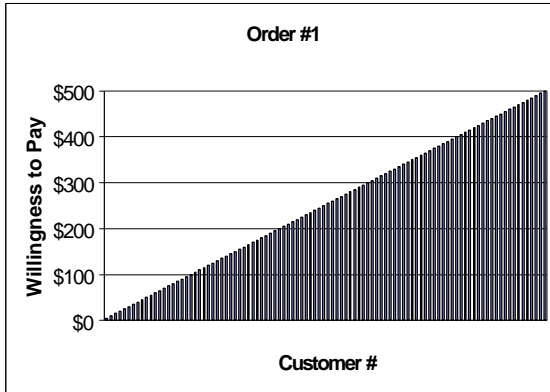
There are one hundred people who would each like to buy one widget. Each potential customer will buy one widget if the price of widgets is low enough. Potential widget customers are willing to pay different amounts for widgets. Graphically, customers are willing to pay the following amounts for one widget each day:



Sadly for you, the widget market is competitive. Another widget seller (your rival) also wants to sell widgets to the one hundred potential customers shown above.

## Buying and Selling Widgets

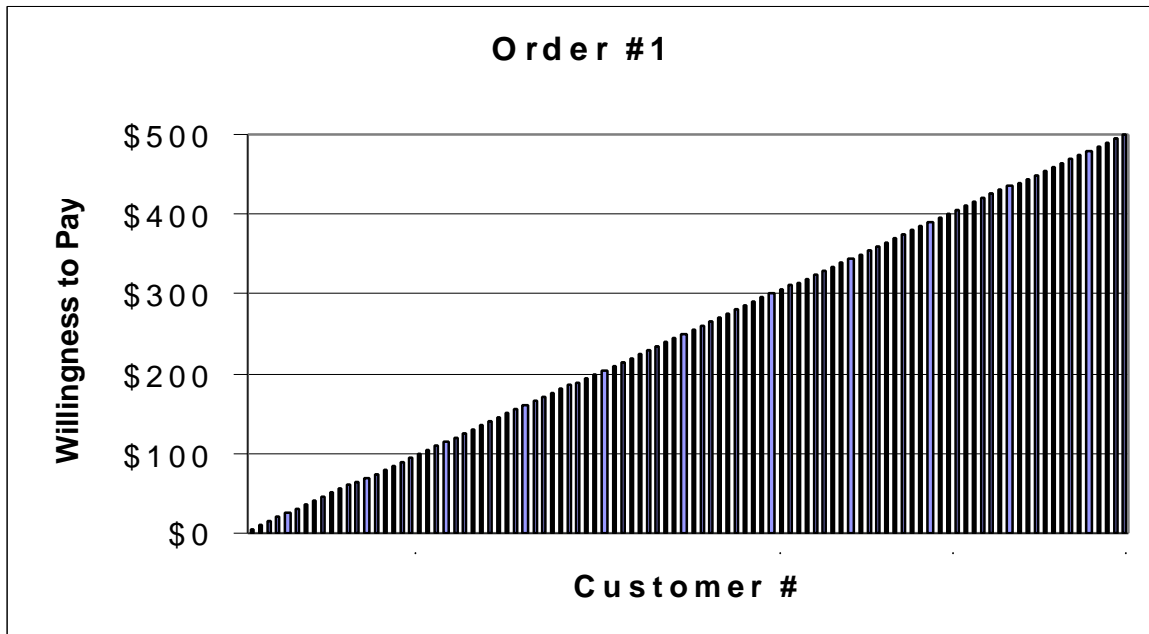
The rules of buying and selling widgets follow. Each day, you and your rival will establish widget prices. Next, the 100 simulated buyers "line-up" to buy widgets. There are six different orders in which buyers can arrive. Each day, buyer order is determined by the roll of a die. Graphically, the six random orders are:



**As illustrated in the following example, buyer order is important.**

Imagine sellers A and B set widget prices at \$100 and \$400, respectively. Next, imagine a "one" is rolled indicating Demand Order #1. As a rule, buyers first purchase widgets from the low-priced seller. The first buyer "in line" has the first opportunity to purchase a widget from the low-priced seller.

Based on their order in line, buyers first deal with Seller A because she is the low-priced seller. The first "willing" buyer is buyer #20 (solid red line, below). As seen below, each subsequent buyer is willing to purchase a widget from Seller A. Seller A continues to sell widgets until her widget machine reaches its 40-unit capacity (dotted red line, below). Buyer #60 can not purchase from Seller A; however, Buyer #60 is unwilling to purchase a widget from Seller B because Seller B's price is too high. No willing buyer exists until buyer #80 (solid blue line, below). As seen below, each subsequent buyer is willing to purchase a widget from Seller B.



Customers # 20 through # 59  
purchase from Seller A.

Customers # 80  
through #100  
purchase from  
Seller B.

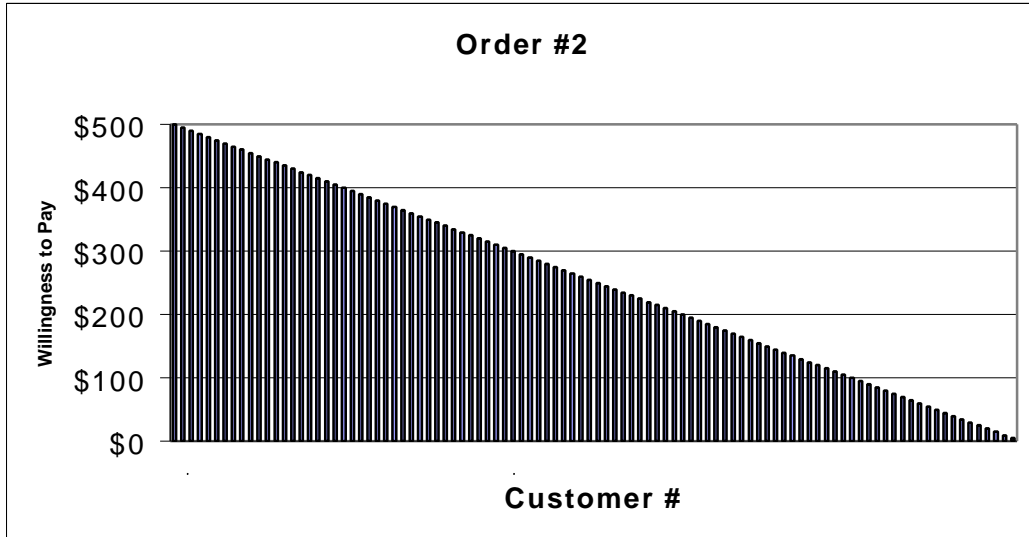
### Example Accounting Reports

If widget machines had a fixed rent cost of \$ 3,000 per day and if variable material costs were \$5 for each widget produced and sold, then the following accounting reports would describe each seller's profit.

	<u>Seller A</u>	<u>Seller B</u>
Unit Sales Price	\$ 100	\$ 400
Units Sold	40	21
Total Revenue	\$ 4,000	\$ 8,400
Unit Variable Cost	\$ 5	\$ 5
Units Sold	40	21
<i>less</i> Total Variable Costs	\$ 200	\$ 105
Contribution Margin	\$ 3,800	\$ 8,295
<i>less</i> Total Fixed Costs	\$ 3,000	\$ 3,000
Profit	\$ 800	\$ 5,295

Your fixed costs may not be \$3,000 per day. Your actual fixed cost is private information and will be provided shortly.

Regarding the preceding example, note that Seller B would have sold zero widgets if a "two" were rolled.



Customers #1 through #40 purchase from Seller A.

No remaining customers are willing to pay Seller B's price of \$400 per widget.

## **Step-By-Step Procedural Review**

Each experimental day, the following sequence occurs.

1. Sellers submit selling prices for their widgets.
2. A die roll determines buyer order.
3. The computer automatically determines sales. Remember, customers (buyers) purchase from the low-priced seller first. *If selling prices are equal, customers are divided evenly between sellers.*
4. You receive a record of your performance and your competitor's price.
5. Repeat Step #1.

## **Money**

The experimental money that you earn during the forthcoming "price-setting days" will be converted to U. S. dollars. You will be given your conversion rate momentarily.

## **General Questions**

If you have any general questions, please ask them at this time. After all general questions are answered, I will distribute your private information sheet then we will begin a series of price-setting days.



**At this point, please ask all questions individually  
(i.e. don't ask questions out loud)**

After each day of our game, you will receive a feedback report like the one below. The following report indicates your identity. Your identity and your competitor will be the same throughout this game (Player A vs. B; Player 1 vs. 2).

Player 1		Day #	0 Performance Report	
Total Revenue:	\$		Your Sales Price:	\$
			Units Sold:	
Total Variable Costs:	\$		Unit Variable Costs:	\$
Contribution Margin:	\$			
less Total Fixed Costs:	\$			
Profit:	\$			
			Your cumulative Profit is:	\$
			Your Opponent's sales price was:	\$
MY NEXT SALES PRICE IS:	\$			

The preceding feedback report is blank because nothing has happened yet. After the first day, information on this report will be completed.

Admittedly, the first day of play may seem confusing. The only information that you have to make a pricing decision is the information discussed in the preceding directions and the following:

**Your Fixed Rent Cost is \$2,000 per day.**

**Your Opponent's (i.e. Player 2's) Fixed Rent Cost is \$2,000 per day.**

**The experimental money that your earn will be converted to U.S. dollars at the rate of 5,000 experimental dollars = U.S. \$1.**

Although information may seem limited at this point, please respond to the following question before we begin: What is the **lowest** price you would feel comfortable charging for one widget?

\$ \_\_\_\_\_

Note: Your response to this question does **NOT** restrict you in any way during the upcoming price-setting days. At this point, I am only interested in the **lowest** price you would feel comfortable charging based upon what you know right now.