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An Experimental Examination of Competitor-Based Price Matching Guarantees

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Abstract

We use experimental methods to demonstrate the anti-competitive potential of price matching guarantees in both symmetric and asymmetric cost duopolies. Our findings establish that when costs are symmetric, price-matching guarantees significantly increase market prices. In markets with cost asymmetries, guaranteed prices remain high relative to prices without the use of guarantees, but the overall ability of price guarantees to act as a collusion facilitating device becomes contingent on the relative cost difference. Lesser use of guarantees, combined with lower average prices and slower convergence to the collusive level, suggest that the mere presence of cost asymmetries may curtail collusive behavior.

JEL Codes*:* L13, L40, D43
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1. Introduction

In many market environments sellers 'guarantee' their price by promising that it is the lowest amongst all their competitors; if not, they will match the lowest price that the customer can find elsewhere in the market. The use of such competitor-based price matching policies is widespread, appearing in industries such as banking, natural gas, and construction as well as retail products such as electronics, kitchen appliances, pharmaceuticals, diamonds, auto-parts, tires, and prescription drugs.[2] Taken at face value, these policies appear pro-consumer. For example, when a funeral home in Chicago promised to match prices from any local competition, the article in *Financial Times* was titled "Price Wars Spreading" (Millman, 1994). In the theoretical literature, however, such policies are regarded as mechanisms to facilitate collusion amongst competing firms either through consumer-enforced information exchange, by rewarding consumers for reporting discounts of the competitors, or through incentive management, by altering the payoff structure so that joint profit maximization is optimal and the prisoner's dilemma is avoided. This paper uses experimental methods to demonstrate the anti-competitive potential of competitor-based price matching guarantees in both symmetric and asymmetric cost markets. We find that, irrespective of the level of asymmetry, experimental duopoly markets with price matching guarantees have significantly higher prices relative to those without such guarantees.

The marketing literature holds the view that consumers regard price matching policies as a heuristic for low prices. Consumers believe that sellers, who either enjoy a cost advantage or want to build their market share, use such guarantees to signal low prices (e.g. Chatterjee and Roy, 1997), Jain and Srivastava, 2000). The refund cost the seller stands to incur if the low-price signal is incorrect acts as a bond, which further enhances the 'false sense of security'. Not just the ordinary consumer, even industry analysts have viewed price matching announcements as intensifying price competition.[3]

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[2] Edlin and Emch (1999) document that the use of price guarantees can be traced back to 1947 (International Salt Co. versus United States 332 US 131). Over time, low-price guarantees have assumed different forms such as most favored customer clauses, meet-or release clauses, price matching and price beating policies (either by a fraction, dollar or percentage difference amount). Even price matching policies may be differentiated based on whether they are retroactive (applicable to seller's own past prices) or competitor-based (applicable to seller's price relative to its competitor). In this study, we concentrate solely on competitor-based price matching policies.

[3] For example, when Esso, a UK petrol retailer, announced a policy to match prices, their competitors said a price war was the likely outcome of the company's actions, and Shell retaliated by immediately cutting their prices (Corzine, 1996).

Price guarantees seem beneficial to the consumer because they ensure purchase at the lowest price in the market. A subtle, yet important, difference is that it does not ensure that the seller offering the guarantee is in fact the lowest priced seller. A price matching seller could raise prices without losing any customer until its competitors also raise their prices, thereby creating collusion without any formal agreement. The ability of price guarantees to eliminate a competitor's incentive to undercut prices was first pointed out by Salop (1986). Since then, the anti-competitive effects of price matching policies have been examined in a variety of settings. Doyle (1988) presents a formal analysis for multiple sellers of a homogeneous good and shows that collusion exists if and only if all sellers adopt price matching strategies. Belton (1987) and Logan & Lutter (1989) extend the result to a differentiated goods duopoly. In the former study, sellers are symmetric while in the latter, an asymmetric cost structure is explored. Both studies consider a two-stage game where sellers first decide whether or not to adopt price guarantee and then choose their price. However, Chen (1995) shows that the collusive outcome is invariant to the timing of price-and-guarantee decisions, whether simultaneous or sequential. Other research supports the anti-competitive results using advertising (Baye and Kovenock, 1994), vertical price fixing (Butz, 1993) and overlapping-generation models (Schnitzer, 1994).[4] Edlin and Emch probably sum up the literature best, by noting that, "adopting a guaranteed-low-price policy is a good substitute for actually having low prices…" (pg. 145, 1999).

The empirical literature aimed at testing the effect of price matching policies is both scarce and inconclusive. Hess and Gerstner (1991) examine weekly supermarket prices before and after the adoption of price matching guarantees and find support for the collusive theory. They show that guarantees not only yield greater conformity in prices, but also result in a statistically significant increase in average price for products included in the guarantee relative to those not covered by guarantees. Citing evidence from the SEC and Department of Justice, Edlin and Emich (1999) note that the computerized matching system on the NASDAQ stock exchange may explain the higher-than-competitive level of bid-ask spreads. On the other hand, Arbatskaya et al. (1999) use data from retail tire markets to show that although an increase in the percentage of sellers adopting low-price guarantees does tend to

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[4] While facilitation of collusion features predominantly as a reason for adoption of price guarantees; other reasons for adoption cited elsewhere in the literature include entry deterrence (Belton, 1987), signaling (Moorthy and Winter, 2005) and price discrimination (Png & Hirschleifer, 1987)

raise the advertised price in the market, there is no significant difference between prices advertised by sellers offering these guarantees relative to those offering no such guarantees. Further, Arbatskaya et al. (2004) document that while price matching guarantees are the most common form of low-price guarantees (accounting for almost nearly two-thirds of all guarantees and are adopted by 73 percent of the sellers), nearly 44 percent of the price matching guarantees appear inconsistent with their use as collusion facilitating device. Differentiating between price matching and price beating policies, Arbatskaya et al. (2005) find that price matching leads to higher advertised prices in the tire retail market, whereas Liu (2006) finds that price beating policies are more consistent with higher prices in electronic goods markets.

This lack of unequivocal conclusion regarding price guarantees is not surprising when we account for the difficulties encountered in the field data. To start, it is difficult to construct counterfactual prices that would have prevailed if stores did not have price guarantees. Furthermore, in the absence of any other feasible alternative, most empirical studies utilize data from different stores that have different types of guarantees at different points in time. Much of the difference in results can therefore be explained by seller, market, product and guarantee heterogeneity. Experimental methods, on the other hand, can provide direct empirical evidence. In the laboratory, basic underlying structural and informational conditions are induced and hence known. We can directly control for unrealistic assumptions such as product homogeneity, complete information about demand and costs and no capacity constraints, as well as various complicating factors that plague field data such as reputation formation and brand proliferation. The focus on predicted effectiveness of price matching guarantees as a collusion facilitating device is therefore unambiguous.

Despite the numerous methodological advantages, to date, few studies have examined competitor-based pricing policies in a laboratory setting. Fatás and Mañez (2004) analyze a differentiated product, price-setting duopoly model where price matching policies may be either imposed as an institution or made available as an option. Irrespective of the implementation mechanism, they find that price matching leads to higher prices when compared to no price matching. Deck and Wilson (2003) report that price matching policies generate significantly higher prices and profits when compared to either undercutting or trigger pricing. In related works, Dugar (2005) and Dugar and Sorensen (2006) test the

collusive prediction of these guarantees in a homogeneous goods market and find support for the anti-competitive theory, but only in the absence of hassle costs. Although these experimental studies concur with the dominant view of price matching as a collusion facilitator, none have examined the impact of these guarantees in the presence of cost asymmetries between sellers. Cost asymmetry influences the relative gains from collusion and therefore can provide a direct test of the robustness of price guarantees as a collusion facilitating device.

This paper uses experimental methods to examine how price matching guarantees affect prices in duopoly markets with both symmetric and asymmetric costs. The symmetric cost treatment replicates the price matching environment of previous studies, with few distinctions. Unlike Fatás and Mañez (2004), we employ a homogenous goods environment, since price matching guarantees are more likely to be instituted for identical products.[5] This homogeneity of goods assumption has important implications for profit calculations, since the competitive equilibrium yields zero profit in our study, whereas competitive profit is positive with differentiated goods. Our model therefore allows an examination of the upper bound of the possible price effects from price matching. It is also a much simpler setting, where determination of quantity relative to price choices of both sellers in the market is clear. In Fatás and Mañez (2004), subjects lacked information about the underlying demand model, making the relationship between subjects' price choices and consequent profit relatively imprecise. Also contrary to the design of Deck and Wilson (2003), where subjects chose from a wide variety of complex rules, we consider only two basic settings. The control treatment of no price matching is compared to markets where price matching is available as an option. This singularity in the type of pricing policy is not only easier for the subjects to understand, but also releases the data from the endogeneity imposed by differing adoption rates for different policies. Finally, in order to obtain parallelism with the field environment, unlike Dugar (2005) and Dugar and Sorensen (2006), we allow the price matching option to be available every four periods, while the pricing decision is made every period.[6] This feature of

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[5] Chen (1995, pg. 684) state that "an identical product model appears to be more appropriate than a differentiated product model for an analysis of MCC (meeting competition clause)".

[6] Theoretical literature has also emphasized the relative permanent nature of guarantee decisions (Logan and Lutter, 1989). Fatás and Mañez (2004) and Fatás et al. (2005) employ a similar design choice for guarantee decisions in their experiments.

our design also allows subjects to learn the relationship between their guarantee decisions and its possible consequences over price and profit.

Despite the above-documented differences, our results when costs are symmetric are similar to those obtained in these studies: Price matching facilitates collusion. It is therefore crucial to note that our motivation to include the symmetric cost treatment is not just replication in a slightly different environment but, more importantly, to provide a benchmark for comparison when costs are asymmetric.

The asymmetric cost treatment is the novel feature of our study. Both theoretical and experimental literature suggests that asymmetry in costs would make tacit collusion significantly more difficult. The reasoning is simple: with asymmetric costs, high cost and low cost sellers experience differential gains from cheating on an implicit agreement. This implies that agents in these asymmetric markets have relatively different incentives to cooperate, making collusion both less viable and less stable. Since most naturally occurring oligopolies are characterized by cost asymmetries, it is important to analyze whether the anti-competitive outcome of price matching policies can be extended to (or is sustainable under) cost asymmetries. This may be particularly relevant from the perspective of policy prescription. Another justification for the treatment is provided by casual observation that despite the much touted collusive result, not all sellers adopt price matching policies. By controlling the demand side and other noisy factors such as buyer search cost and hassle cost, we can explore the reason for lack of universal adoption from the perspective of the seller alone. Accordingly, in our design we consider two different levels of cost asymmetry: one where price matching is optimal for both sellers in the market and prices are collusive, and another where adoption of price matching does not curtail the seller's incentive to undercut and high-marginal cost pricing prevails.

The remainder of this paper is organized as follows. The next section presents the theoretical model while Section III presents our experimental design and procedures. Section IV articulates the theoretical predictions and testable hypotheses. The results are detailed in Section V and Section VI concludes.

2. Theoretical Model

Consider a price-setting duopoly where each seller i supplies a homogeneous product at a constant marginal cost, c_i. Sellers compete by simultaneously choosing a posted price p_i and announcing whether or not they will match their competitor's price. A seller's strategy space is therefore given by $\{p_i, \Phi_i\}$ where:

$$\Phi_i = \begin{cases} NPM = no\ price\text{-}matching\ guarantee \\ PM = price\text{-}matching\ guarantee \end{cases}$$

The price policy, Φ_i, implies that the effective price of seller i is p_i^e, where:

$$p_i^e = \begin{cases} p_i & \text{if } \Phi_i = NPM \\ min\{p_i, p_{-i}\} & \text{if } \Phi_i = PM \end{cases}$$

On the demand side of the market, we assume that all n consumers are fully informed of the prices and policies of both sellers.[7] They purchase at most one unit of the good at the lowest price equal to or below a reservation value r, and incur no cost to redeem the price guarantee. Consumers are assumed to have no preference with respect to either seller, and distribute themselves evenly between the two in the event of identical effective prices. This consumer demand model is known to both sellers, and is given by:

$$q_i = \begin{cases} n & \text{if } p_i^e < p_{-i}^e \\ \dfrac{n}{2} & \text{if } p_i^e = p_{-i}^e \\ 0 & \text{if } p_i^e > r,\ p_{-i}^e < p_i^e \end{cases}$$

The profit of seller i, given cost of production c_i, is simply:

$$\Pi_i = (p_i^e - c_i)q_i$$

Using the Nash solution concept, the equilibrium is defined as a set of prices, price policy and effective prices $\{p_i, \Phi_i, p_i^e\}$ that are consistent, i.e. $p_i^e = \Phi_i(p_i^e, p_i)$ so that neither seller has an incentive to deviate. We restrict our focus to equilibrium in pure strategies.

Within this general framework, we consider two cases. One, where both sellers have the same cost ($c_i = c$) and another, where the cost of production differs across the two sellers ($c_l <$

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[7] Hviid and Shaffer (1999) find that price matching policies lose their collusive potential when consumers have to incur hassle costs in order to obtain refunds. Assuming the demand side to be perfectly informed allows us to isolate the extent to which the adoption of price matching guarantees is influenced by seller characteristics alone.

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c_h). In case of symmetric cost, without the availability of price matching guarantees, the unique Nash equilibrium is (c,c). The intuition is simply that at any price above marginal cost, undercutting is profitable. By slightly decreasing his price a seller can potentially supply the entire market and thereby attain a substantial increase in profit. The presence of price matching policy, however, eliminates this incentive to undercut. By adopting price matching, a seller is effectively guaranteeing its competitor that any attempt to decrease price will be matched immediately, thereby eliminating any gains from price cut. Doyle (1988) and Corts (1995), amongst others, have shown that when sellers have symmetric costs, the unique Pareto efficient equilibrium is monopoly pricing with all sellers adopting price matching policies ($p_i^e = r$).[8] The reasoning is simple: if both sellers adopt price matching, undercutting the competitor's price is no longer optimal for seller i since lowering the price $(p_i < r)$ only lowers the effective price while yielding no increase in seller i's market share. Similarly, raising the price above the buyer's reservation price $(p_i > r)$ does not change the effective price and therefore profit remains unchanged. Finally, not adopting the price matching policy by seller i at most leads to lower effective price if $p_i \leq p_{-i}$ with no change in profit, and yields zero profit otherwise.

When sellers' costs differ, Logan and Lutter (1989) demonstrate that the ability of price matching guarantees to act as a facilitating device is no longer universally dominant, but instead is contingent on the level of cost asymmetry. For the low cost seller (c_l), the question is whether to collude and split the market with the high cost seller, or to employ its cost advantage and capture the entire market. The answer lies in the magnitude of the cost difference. When the cost difference is small ($c_l < c_{h1}$), it is advantageous for both sellers to collude and price at the reservation value r. That is, there exists an equilibrium in which both sellers adopt price matching and prices rise to the Pareto optimal level. However, with a significantly larger cost difference ($c_l << c_{h2}$), it is no longer profitable for the low cost seller to cooperate with the high cost seller. Instead, the low cost seller earns greater profit by

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[8] The collusive equilibrium is not a unique Nash equilibrium. The decision by one or both firms to refrain from price matching with both firms setting their (effective) prices at the Bertrand level is also an equilibrium. Moreover, when both firms adopt price matching, every price in the interval [c,r] constitutes a symmetric Nash equilibrium. The collusive equilibrium is the unique equilibrium only upon applying some refinement to the Nash equilibrium concept. Belton (1987) argues that monopoly price equilibrium emerges because of sub-game perfection. Moorthy and Winter (2005) employ trembling-hand perfect equilibrium, while Doyle (1988) uses iterative elimination of weakly dominated strategy.

undercutting the high cost seller and capturing all of the consumer demand. This makes the adoption of a price matching guarantee sub-optimal for the high cost seller, causing the Bertrand-Nash equilibrium price to prevail.[9, 10]

3. Experimental Design and Procedures

This experiment uses the standard posted offer market institution to organize the duopoly market. Sellers produce a homogeneous good at a cost which may differ across the two sellers. Information about the two sellers' costs is always common knowledge in the market. Sellers do not face any capacity constraints, and can satisfy entire consumer demand. On the demand side, the market is comprised of ten automated buyers, each with a reservation value of $10. These automated buyers are programmed to purchase from the lowest-priced seller or, in the event of tie, split themselves equally across sellers.

Each period, sellers simultaneously post their prices. In order to make the experimental environment compatible with the field counterpart we allow for a nearly continuous price space {$0,$0.01,…,$98.99,$99} with prices up to 2 decimal points. Depending on the treatment, every 4 periods sellers also decide whether or not to adopt price guarantees. There is no cost associated with the adoption of price guarantee. At the end of each period, sellers receive information regarding their and other seller's price and pricing policies (where relevant) and their own profits. This information is also made available as a history table on subjects' decision screens.

We chose to randomly reassign subjects to different duopolies every guarantee decision period. This 4-period fixed matching enables us to concentrate on price dynamics, keeping both the sellers in the market and their choice of price guarantee constant. It also parallels the field environment where market structure does not change often. On the other hand, using a random re-matching mechanism reduces the repeated game incentives, thereby providing a stronger test for the collusion hypothesis.[11] In the experiment, 16 subjects comprised one

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[9] The low-cost seller is always indifferent between adopting and not adopting the price matching guarantee.

[10] For the parameters of the model, it is easy to show equilibrium is collusive as long as $c_h < \dfrac{r + c_l}{2}$. If the cost of the high-cost seller increases beyond this threshold, competitive Bertrand pricing emerges. Refer to Logan and Lutter (1989) for the formal proof.

[11] Fatás et al. (2005), for example, find that average prices are significantly higher under price beating policies when the matching protocol involves repeated play with the same rival.

experimental cohort. Each cohort was split into two sessions of 8 sellers each, and the random matching took place within these distinct sessions. This enables us to obtain two independent observations from each cohort.[12]

The experimental design features two treatment variables: availability of price matching guarantees and level of cost asymmetry. Each session proceeds through a sequence of 64 trading periods. We always began with the baseline No Price Matching (NPM) treatment, where sellers choose only their prices every period. This treatment provides a benchmark for comparison with the Price Matching (PM) treatment, wherein sellers may incorporate price matching guarantees as an additional option into their profit-maximizing strategies, while still setting prices every period. Three points need to be noted with regard to the PM treatment. First, price matching is an option (as opposed to an institution) and sellers can decide whether or not to use it every four periods. This long-term determination of guarantee usage can be rationalized by the fact that firms are able to change their price more often than they can change their guarantee policy. Second, we employ a within-subjects design to study the impact of price guarantees. Having the same set of subjects making decisions under different treatments, both NPM and PM, directly controls for subject variability. However, it also may result in a hysteresis effect, with subject experience in one treatment influencing their behavior in the other treatment. To account for such sequencing effects, we employ an A-B-A design. Finally, each PM treatment lasted for (at least) 24 periods, while the NPM treatment lasted for only 16 rounds. Longer PM treatments allows for greater learning. Since price guarantees can only be adjusted every four rounds, it is important to provide a significant number of opportunities for subjects to experience this option.

The experimental design for the symmetric cost treatment is summarized in Table 1.A. In this treatment, $c_i = c = 5$. Our main focus is to examine how the availability of price matching as a possible pricing strategy affects prices and competition and, in particular, whether the use of guarantee pricing policies facilitates collusion. The asymmetric cost treatment extends the analysis further to test the robustness of price guarantees as a collusive device when one seller has a cost advantage. Accordingly, in our design we consider two levels of asymmetry. The

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[12] This increases the number of statistically independent observations while keeping the greatest number of outside factors constant. Duffy and Ochs (2006) find that the total number of participants in a session does not affect the value to random matching, making the return to having 16 subjects in one session not significantly better than 8 subjects per session.

low cost (c_l) is equal to 2 in both cases, while the high cost is controlled at two levels: $c_{h1} = 5$ and $c_{h2} = 8$.

The experimental design for the asymmetric cost treatment is summarized in Table 1.B. As discussed later in the results section, our design evolved over the course of study, as we included more sessions to answer questions posed by prior sessions. We initially ran a set of eight "dual cost" sessions where we varied both availability of price guarantees and the level of cost asymmetry. In these sessions, the sequence of treatments was as follows: subjects first participated in a NPM treatment for 16 periods with a particular level of cost asymmetry (i.e. c_l = 2 and c_{h1} = 5). In the next sequence of 24 periods, the option of a price matching guarantee was made available, while keeping the level of cost asymmetry unchanged. The final sequence of 24 periods featured a PM treatment with a different level of asymmetry (c_l = 2 and c_{h2} = 8). Furthermore, in the asymmetric treatment, cost of a seller was switched every four periods i.e. if seller 1 is a high cost seller in periods 1-4, she becomes a low cost seller in periods 5-8, and so on.

This orthogonal, within-session design allowed for an analysis of the effect of both cost asymmetries and price guarantees, while controlling for subject variability. Our primary goal was to determine whether subjects are able to comprehend the relative cost advantage afforded by each asymmetry level and incorporate the resulting difference in incentives into their pricing decisions. Switching costs every 4 periods enabled subjects to learn the incentives as both a high and low cost seller. It also meant equal opportunities to make profit so that the impact of other-regarding preferences like inequity aversion and reciprocity could be avoided. However, unlike the symmetric cost sessions where prices in the PM treatment converged relatively quickly to the collusive level, the variation in prices was much greater in the asymmetric cost sessions. The experimental design component that changed seller costs every four periods, along with the guarantee decision, for a total of six times, now assumed significance. It was conjectured that variation in prices might simply be indicative of lack of comprehension, and would decline if subjects had more time to learn the incentives under each cost structure.

To address these concerns, we ran ten additional "single cost" sessions where the cost asymmetry was fixed at one level throughout the session.[13] That is, sessions featured either small cost asymmetry ($c_l = 2$ and $c_{h1} = 5$) or large cost asymmetry ($c_l = 2$ and $c_{h2} = 8$). As before, the first 16 periods employed the baseline NPM treatment, but now the PM treatment, with constant cost asymmetry, spanned the remaining 48 periods. Another variation was the number of periods sellers experienced a certain cost. In the previous set of "dual cost" sessions, sellers' costs switched every four periods, accompanied by new price guarantee decision. To avoid intermixing guarantee decisions with changing seller costs, we ran four sessions where seller costs in the PM treatment switched every 12 periods and four sessions where the switch took place every 24 periods.[14] Finally, we ran two large cost asymmetry sessions where the price matching decision was made available every period.

All experimental sessions were conducted at the Vernon Smith Experimental Economics Laboratory at Purdue University using the program z-Tree (Fischbacher, 1999). A total of 208 undergraduate students participated in 26 sessions. Upon arrival, subjects were instructed to sit at a computer. The instructions (contained in the Appendix) were read aloud prior to each section of the experiment, and subjects were not allowed to communicate at any time. At the end of the session, they received total profits, privately and in cash, converted from Experimental Dollars ($) to U.S. dollars using a pre-determined conversion rate. The sessions usually lasted about 90 minutes and the average earnings were approximately US$22.00.

4. Theoretical Benchmarks and Testable Hypotheses

In the symmetric cost treatment, sellers make a maximum payoff of $25 per period by colluding to price at the reservation price. However, the incentive to "cheat" and charge slightly less than the competitor is the basis for the classical model of price competition's prediction that, in equilibrium, price equals marginal cost. The availability of price matching serves to decrease or eliminate the incentive to cheat and makes the collusive outcome sustainable. The testable hypotheses for the symmetric cost treatment, following predictions by Corts (1995), may therefore be summarized as follows:

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[13] We thank the associate editor for this design suggestion.
[14] In sessions S19, S20, S23, and S24, a subject was a low-cost seller in the first 24 periods of the PM sequence and high cost seller in the next 24 periods. The duration for cost switch was 12 periods in the remaining "single cost" sessions.

Hypothesis 1: Prices are higher in the PM treatment than in the NPM treatment. Specifically, prices in the PM treatment converge to the collusive level ($10, $10) with sellers making a profit of $25 each, whereas the NPM treatment yields marginal cost pricing ($5, $5).

Hypothesis 2: Both sellers adopt price matching guarantees when available.

In the asymmetric cost treatment, theoretical predictions differ for the two levels of asymmetry. In the case of small cost asymmetry, $c_l = 2$ and $c_{hl} = 5$, a 'competitive' outcome results when the low cost seller undercuts the high cost seller and supplies the entire market. In this equilibrium, the low cost seller makes a profit of $29.90 (if pricing exactly one cent below the high marginal cost), while the high cost seller earns zero profit. The 'collusive' prediction, on the other hand, is that both sellers charge the reservation value of $10 and split the market. In this case, the low cost seller makes a profit of $40, while the high cost seller makes a profit of $25. Though the collusive outcome provides higher profits for both sellers, making it Pareto superior to the Bertrand-Nash outcome, the incentive for either seller to "cheat" and charge slightly less than their competitor, makes it unlikely that the collusive strategy will be observed in the experiment.

Similar to the case with symmetric costs, the availability of price matching makes the collusive outcome much more likely, since any undercutting would be immediately matched by the rival seller. The profit maximizing prediction for the PM treatment with small cost differences is for both sellers to match prices and charge the reservation value in every round. However, introducing cost asymmetries into a market with price matching may make it harder for sellers to reach the collusive outcome than if costs were symmetric. When costs are symmetric, both sellers gain an equal increment of $25 from colluding, but when costs differ, the payoff of the low cost seller increases by a maximum of $10.01 per round.[15] Thus, while unilateral deviation from the collusive equilibrium is unprofitable for either seller, its attainment is contingent on the low cost seller being fully convinced that the high cost seller will behave in accordance with the equilibrium. Since examples of irrationality abound and the consequences could be severe, it may take longer to establish the viability of collusion.

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[15] The payoff increases by a maximum of $10.01 when both sellers price at r and adopt guarantees. However, consider a case where although both sellers price match, the high cost seller chooses a price less than $8. The profit of the low cost seller will then be less than the Bertrand profit of $29.90.

This conjecture is also in line with findings of Mason et al. (1992) that markets where sellers face different costs are significantly less cooperative.

In the case of large cost asymmetry, $c_l = 2$ and $c_{h2} = 8$, collusion at the reservation price continues to yield the low cost seller a maximum profit of $40 per round. However, it is no longer the profit maximizing option. The low cost seller earns a higher profit of $59.90 per round if she undercuts the cut the cost of the high cost seller to capture the entire demand. Accordingly, the low cost seller is indifferent between adopting and not the price matching guarantee. The high cost seller should not choose to match prices, since this may lead to negative earnings in equilibrium if the low cost seller prices below the high marginal cost.

Note that, unlike before in case of large cost differences, price matching guarantees should cease to act as a collusion facilitating device. Profit maximization yields different solutions for the two sellers, and guarantee adoption alone can no longer coordinate sellers towards a common collusive equilibrium. However, since the collusive equilibrium yields maximum profit to the high cost seller, the high cost seller has an incentive to "teach," or signal to the low cost seller by price matching in early periods. In our experiments, we consider a finite period game, where subjects know the termination period and are randomly re-matched every four periods, but such repeated game behavior cannot be ruled out.[16] A more detailed discussion on these repeated game incentives and their impact on subject behavior appears in the results section.

To summarize, the inclusion of two asymmetric cost treatments, one where employing price guarantees is optimal and another where it is not optimal, enables us to analyze whether subjects understand the incentives entailed by the cost advantage for both pricing and the adoption of price matching guarantees. The small cost asymmetry treatment tests for the robustness of the collusive equilibrium in the presence of cost asymmetries; and the large cost asymmetry treatment tests whether the use of guarantees arises out of mere novelty value or if policy adoption is grounded in sound profit considerations. The testable hypotheses for the asymmetric cost treatment, following predictions by Logan and Lutter (1989), may be summarized as follows

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[16] We thank the associate editor for bringing this to our attention.

Hypothesis 3: In the small cost asymmetry treatment, both sellers price at the collusive level ($10, $10) and adopt price matching guarantees when available. Sellers make a profit of $40 each.

Hypothesis 4A: In the large cost asymmetry treatment, the low cost seller is indifferent between adopting or not adopting price matching guarantees, while the high cost seller will not adopt price matching guarantees.

Hypothesis 4B: In the large cost asymmetry treatment, low cost sellers price at the Bertrand-Nash level, $p = \$7.99$, earning a profit of $59.90 while the high cost seller earns zero profit.

4. Results

For simplicity of exposition, we will divide our analysis along the lines of symmetric and asymmetric cost treatments. In each subsection we begin with a graphical description of the data. Formal tests, both parametric and non-parametric, are then employed to test the model's predictions and results are evaluated as support (or lack thereof) of the testable hypotheses, which are summarized in Table 2. In order to account for initial learning and the hysteresis effect arising from treatment changes, formal tests exclude the first 8 periods of each treatment run. We therefore restrict the analysis to last 8 periods of the NPM run and last 16 (or 40) periods of each PM run.[17]

4.A Symmetric cost treatment

Market transaction prices, averaged across the four duopolies in each symmetric cost session, appear in Table 3. Figures 1 and 2 show the time series of average market prices in all 8 symmetric cost sessions. Broadly speaking, prices in the experiment are consistent with the equilibrium prediction, although in some cases full convergence to the equilibrium price did not occur.

We begin with the NPM treatment. Relevant to this discussion are sequences 1 and 3 (rounds 1-16 and 41-56) from Figure 1 and sequence 2 (rounds 17-40) from Figure 2, where subjects are not allowed to match prices. As can be seen from the figures, prices in all 8

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[17] We are interested in the test of the equilibrium prediction of the model. Therefore, we exclude the initial periods where behavior is more erratic as subjects learn about the market structure and the incentives they face. Exclusion of the first 8 periods of each treatment run was based on the premise that subjects would have experienced the price guarantee decision at least twice. However, results are not significantly different if we exclude the first 12 periods.

sessions tend towards the predicted equilibrium level of $5, but do not converge to the Bertrand price, as predicted. This observation is consistent with previous experimental work by Fouraker and Siegel (1963) and Dufwenberg and Gneezy (2000), who find that in duopoly markets prices remain much higher than the Bertrand prediction. Of course, from a methodological point of view, one might suspect that the attraction of the equilibrium outcome is considerably undermined by the non-saliency of equilibrium payoffs (zero profit). The contest between the desire to make positive profit and the incentive to undercut rival's price is evident from the fact that prices are higher when subjects are re-matched into new duopolies but decline over the four period interaction (p-value = 0.00).[18] In contrast to the predicted behavior of competing away all profits, subjects in the experiment made an average profit of $65.20 by pricing above the Bertrand level.

Sequence 2 (rounds 17-24) in Figure 1 and sequences 1 and 3 (rounds 1-24 and 41-64) in Figure 2 display the times series of average market transaction prices for the PM treatment. Consistent with the equilibrium prediction, market prices in the PM treatment reach the profit-maximizing level of $10 in almost all sessions, sometimes as early as round 4 for Session S2. Only once, in session S5, did subjects fail to achieve the reservation value in their first experience with price matching opportunities, although the prices in the last 8 rounds of that treatment were strictly increasing. Upon gaining access to price matching again in sequence 3, prices in this particular session almost immediately converged to the predicted price. The square markers identify the first round in which 100 percent of subjects chose to offer price matching guarantees. It is clear that in almost all cases, subjects recognize the profit maximizing potential of price guarantees fairly early and eventually all subjects chose to match prices in 11 of the 12 sequences where the option was available.

Result 1: Prices in the PM treatment are significantly higher compared to prices in the NPM treatment. (Support for Hypothesis 1)

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[18] This edgeworth cycle-like pricing reflects a 'restart' phenomenon in cooperative behavior. Duffy and Ochs (2006) examine cooperative behavior in an infinitely repeated Prisoner's Dilemma game with a fixed matching protocol. Similar to our study, they find that the restart phenomenon does not dampen with experience. The aggregate frequency of cooperation increases at the beginning of each new pairing as subjects attempt to encourage a social norm of cooperation. Analogous to the subsequent undercutting observed in our study, they also find a decline in the level of cooperation following the first round.

Support: The difference in the results between the PM and NPM treatment amounts to an increased average market price of $3.78 when price matching is available. Within session comparison of prices in the two treatments using the sign test and sign rank test favor the alternative hypothesis that prices in the PM treatment are significantly greater than prices in the NPM treatment (p-value=0.004 for a one-sided sign test and p-value=0.01 for the signed rank test, sample sizes n=m=8). This result is significant, irrespective of sequence order of treatments, and therefore the level of experience.[19]

The same result holds true for the across session comparisons. Comparing sequence 1 prices in sessions S1-S4 (NPM) with those in sessions S5-S8 (PM) gives us an estimate of the difference in the two treatments using observations which are untainted by experience. The Mann-Whitney test based on statistically independent observations from each of the eight sessions rejects the null hypothesis that prices are the same in the first sequence across the two treatments (p-value = 0.02). Similarly, a comparison of prices in sequence 3, where subjects have experienced both PM and NPM treatments, concurs with the previous result that prices are higher with price matching (p-value = 0.02).

Table 5 provides estimates for the coefficient of variation (CV) of prices across the PM and NPM treatments. As a standardization of the standard deviation, CV allows for a comparison of variability estimates, regardless of the magnitude of prices. The low values of CV especially in the last two sequences of the PM treatment indicate that most prices are close to the mean, which in turn is close to the collusive outcome of $10. Thus, price matching guarantees not only increase the average market price, but also provide stability to the pricing structure.

To further validate these results and control for factors such as time trend, we employed panel data econometric methods that model the dependence of observations and errors arising from repeated measures drawn from the same set of subjects. Correct statistical inferences can be made by including random effect models for errors arising out of subject-level pricing decisions, fixed effect models for session-level and sequence-level analyses and a time trend

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[19] As can be seen in Figure 1, the results from the two NPM sequences are different. Prices in the third sequence are significantly higher than price in the first sequence (one-tailed sign test p-value=0.0625 and sign rank test p-value=0.0679, sample sizes n=m=4). However despite the sequence ordering and consequent learning, in all symmetric cost sessions, prices in the PM treatment remain significantly higher compared to the NPM treatment. The importance of this result is underlined by the results of the asymmetric cost sessions, where prices in different asymmetric cost runs can be explained in large part by sequence ordering alone.

variable (ln(period) or 1/period). The results from these regressions confirm those obtained using nonparametric tests. Furthermore, while neither session dummies nor time trend was significant, we found that prices were increasing in lagged profit and lagged prices (both posted and market transaction prices). This is consistent with the visual observation of a trend of increasing prices. It is also in sharp contrast with Fatas and Mañez's (2004) largely inexplicable result that the market prices fall over time when price guarantees are optional.

Result 2: Both sellers adopt price matching guarantees. (Support for Hypothesis 2)

Support: Aggregating across all symmetric cost sessions, 87.5 percent of the subjects adopt PM in the very first round of availability. The average adoption rate for all periods is 94.3 percent, with almost complete unanimity in the last guarantee decision period (period 20). These results are consistent with other studies like Fatás and Mañez (2004) and Dugar (2005) who report an adoption rate of 70 and 90 percent respectively. Moreover, after having chosen to match prices, less than 2 percent of the subjects deviate back to no-price matching. The dominance of PM over NPM as a policy choice is therefore irrefutable.

What is interesting, however, is not the guarantee adoption rate in isolation, but its accordance with the pricing decision. Subjects do realize the collusive nature of guarantees. The correlation between policy adoption and higher prices is positive (0.7) and significant at the 1-percent level. When subjects chose to match prices, the average posted price is $9.92 and although the frequency of non-adoption of price matching was low, when chosen, the NPM option is accompanied by an average posted price of $7.28. Panel data regressions also confirm the fact that prices posted by PM sellers are significantly higher than prices posted by NPM sellers (p-value = 0.01).

Summarizing the results thus far, we can state that when costs are symmetric, price matching guarantees successfully act as a collusion facilitation device, resulting in prices that are significantly higher than the Bertrand-Nash level.

4.B Asymmetric cost treatment

As detailed in Section 3, we began our investigation of the impact of seller cost asymmetry on tacit collusion with the "dual cost" sessions. Each session began with 16 rounds of baseline NPM treatment followed by 48 rounds of PM treatment, where level of

cost asymmetry was switched half way through the PM treatment. Thus, in sessions S9 to S12 subjects faced small cost asymmetry (c_l = 2 and c_{h1} = 5) in the first 16 NPM rounds and 24 PM rounds; and the last 24 PM rounds featured large cost asymmetry (c_l = 2 and c_{h2} = 8). This order was reversed in sessions S13 to S16 to account for sequencing order and level of experience.

Figures 3 and 4 present the time series of average market transaction prices in each round for all eight asymmetric "dual cost" sessions. With small cost asymmetry, the prediction for the PM treatment is collusive pricing at $10. As seen in the second sequence of Figure 3 (rounds 17-40) and the third sequence in Figure 4 (rounds 41-64), this outcome is obtained and sustained in only 3 of the 8 sessions (S9, S13 and S15). Average market price in the remaining sessions trends upwards but fails to reach the reservation price by the last round. Average market prices in the PM treatment with large cost asymmetry can be seen in the third sequence of Figure 3 (rounds 41-64) and the second sequence of Figure 4 (rounds 17-40). On the whole, what stands out in these figures is the fact that the average price increases over time, and continues to increase even after a shock to the level of cost asymmetry. The type of shock, either small or large cost asymmetry, does not seem to make a difference.

Both greater variation in market price and its monotonic increase alludes to the fact that perhaps learning is happening and remains unfinished. To address these concerns we ran additional "single cost" sessions where the level of cost asymmetry was kept constant across the entire session. Figure 5 presents the time series of average market prices for all four asymmetric "single cost" sessions (S17-S20) where the level of cost asymmetry is small. The corresponding presentation for large cost asymmetry sessions (S21-S24) is contained in Figure 6. In these figures, we pool sessions with different spans of fixed seller costs (12 and 24 periods) since this difference was found to be insignificant.

Before proceeding to the formal analysis, we briefly note that the first 16 periods in each of these figures (Figures 3-6) display prices in the NPM sequence, and overall, for both levels of cost asymmetry, the model's Bertrand-Nash price prediction seems to receive adequate support. In fact, introducing cost asymmetry causes price to reach the Bertrand level faster than in the symmetric cost treatment. Of greater interest, however, is the impact of price guarantees on prices when seller costs are asymmetric. Therefore, we begin our analysis by documenting the impact of guarantees on the emergence of collusion.

Result 3: Irrespective of level of cost asymmetry, prices in the PM treatment are higher than prices in the NPM treatment.

Support: Comparing prices in the NPM treatment run with the succeeding PM treatment run provides evidence for this result. Table 4.A and 4.B report the summary statistics for "dual cost" and "single cost" asymmetric sessions. Since subjects made pricing decisions in both NPM and PM treatment, we construct statistically independent pairwise differences for the nonparametric sign test and sign rank test. In constructing these pairwise differences, we aggregate across all sessions which featured a particular level of cost asymmetry. Since the level of asymmetry was varied in the "dual cost" sessions, we restrict our analysis to comparison of the average prices in the first NPM sequence (periods 8 to 16) to the prices in the second PM sequence (periods 24 to 48). For example, in session S9, the average market transaction price is \$5.45 in the NPM treatment and it is equal to \$8.75 in sequence 2 of the PM treatment. The difference of \$3.30 is one of the eight statistically independent pairwise differences for sessions with small cost asymmetry. The difference statistics for other small cost asymmetry sessions are also positive, so both the sign test and sign rank test reject the null hypothesis that positive and negative differences are equally likely (p-value=0.01). Laboratory data therefore provides support to the theoretical prediction that in markets with small cost asymmetry, price matching eliminates incentive to undercut leading to higher prices. However, theory also predicts that the adoption of price matching guarantees should not increase prices in markets with large cost asymmetries. This is refuted in the lab. In all but one session with large cost asymmetry, PM prices are higher than the NPM prices. Since 9 out of 10 differences are positive, we can reject the null hypothesis of no difference using the within session nonparametric tests (p-value= 0.01 for one-tailed sign test and sign rank test).

In addition to comparing the prices in the PM treatment with those in the NPM treatment, we can also compare the PM prices under different guarantee combinations. Figure 7 and 8 present asymmetry treatment-specific average market price when either, both or none of the sellers adopt price-matching guarantees. Regardless of the level of asymmetry, it is clear that markets where both sellers adopt price guarantees have significantly higher prices than those without such guarantees.

Next we address the magnitude of the increase in prices. In the symmetric cost treatment we report that prices rise up to the collusive level, but the differential gains from cooperation in the asymmetric cost treatment can affect both the attainment and sustainability of the precise collusive outcome. Therefore, to explore how the ability of price guarantees to act as a facilitation device for collusion is affected by the presence of cost asymmetry, we compare the level of collusion in the symmetric and asymmetric cost treatments.

Result 4: Cost asymmetry makes collusion more difficult.

Support: As detailed in the previous section, equilibrium predictions for the PM and NPM treatment are invariant to the inclusion of small cost asymmetry. However, despite similar predictions, on average, PM prices in the small cost asymmetry treatment ($c_l = 2$ and $c_{h1} = 5$) are lower than PM prices with symmetric costs ($c = 5$). Note that sequence 2 of S1-S4 (symmetric treatment) is directly comparable to sequence 2 of S9-S13 ("dual cost" asymmetric treatment), where the only difference arises out the asymmetric structure of seller cost in the latter four sessions. Both treatments had 16 rounds of baseline NPM treatment before subjects were allowed the option to match prices, yet in the symmetric cost treatment the collusive price prevails in the 8th round of PM treatment, at the very latest. In contrast, it takes 20 rounds of price matching for session S9 in Figure 3 to reach the reservation value and few of the remaining sessions do. On average, we can state that prices in markets with cost asymmetries do increase during the course of the session, albeit at a much slower rate than when costs are symmetric. The Mann-Whitney rank sum test based on independent observations from the symmetric and "dual cost" asymmetric sessions (S1-S16), supports this visual impression. PM prices with cost asymmetry are significantly lower than PM prices with symmetric costs (p-value=0.001, n=m=8).

Comparison of the NPM prices can be made along similar lines. Comparing sequence 1 prices in Figures 1 and 3, it is easy to see that introducing cost asymmetry into the market causes prices to reach the Bertrand level faster than in the symmetric cost treatment. These low prices may be attributed to the fact that the low cost firm is making strictly positive profits even by pricing at high-marginal cost. The highest price in the NPM sequence of

asymmetric cost sessions is less than the lowest price in the symmetric cost sessions, and so the Mann-Whitney test rejects the null hypothesis of equal prices (p-value=0.02, n=m=4).[20]

The finding that, in the absence of price matching, markets with (small) cost asymmetry reach the Bertrand-Nash prediction faster than the symmetric cost markets, yet take longer to achieve the collusive outcome with price matching, suggests that markets where sellers face different unit costs may be less cooperative. This is in line with the result of Mason et al. (1992), who found that payoff symmetry is a powerful facilitator of collusive behavior. They report that duopoly firms with symmetric costs make decisions that fall between the Nash and collusive equilibria, while markets with cost asymmetry tend closer to the Nash prediction.

Table 6 provides a measure of the variation in observed prices compared to the mean. Unlike the symmetric cost treatment, price matching guarantees are unable to reduce the variance in prices.[21] Although, in some cases, the variance declines over time, no clear pattern of subject learning across different cost asymmetry treatments can be discerned. In fact, from Figures 3 and 4 it is easy to see that while PM prices with small cost asymmetry are much higher than the Bertrand level, they are not very different from the transaction prices attained in the large cost asymmetry treatment. This calls for further investigation.

Result 5: PM prices in the small cost asymmetry treatment ($c_l = 2$ and $c_{h1} = 5$) are not different from PM prices in the large cost asymmetry treatment ($c_l = 2$ and $c_{h2} = 8$).

Support: In the "dual cost" sessions, each group of subjects made decisions in both small and large cost asymmetry treatments, with treatment order varied. Conservative within session nonparametric tests that use the statistically independent pairwise difference in each of the eight sessions fail to reject the null hypothesis that there is no difference in prices across the two cost asymmetry treatments (p-value=0.73 for two-tailed sign test and p-value=0.78 for the sign rank test). Consistent with the visual observation, what seems to make a difference is the order of the sequence. The average market price in the small and large cost asymmetry is $8.55 and $8.46, while the average price in sequence 2 and 3 is $7.94 and $9.12. Accordingly,

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[20] These results hold even when we pool observation across all small cost asymmetry sessions (both "dual" and "single" cost sessions). That is, regardless of how often seller cost is switched, NPM prices with cost asymmetry are lower than NPM prices with symmetric cost (p-value=0.03, n=4, m=8); and PM prices with cost asymmetry are lower than PM prices with symmetric cost (p-value=0.0002, n=8, m=12).

[21] The Mann-Whitney rank sum test based on independent observations from the symmetric and "dual cost" asymmetric sessions (S1-S16), rejects the null hypothesis of equal variance (p-value=0.001, n=m=8).

within session tests favor the alternative hypothesis that prices are lower in sequence 2 than in sequence 3 (p-value=0.04 for the one-sided sign test and p-value= 0.02 for the rank test).[22]

Results are strikingly similar in the "single cost" sessions where the level of cost asymmetry is kept constant through out the sessions. Analogous to the results for the "dual cost" sessions, in the "single cost" sessions we find that prices in sequence 2 (rounds 17-48) are significantly lower than prices in sequence 3 (rounds 41-64) (p-value = 0.07 for two-tailed sign test and p-value=0.04 for the sign rank test). To account for price differences arising out of sequence effect, we conduct the across session asymmetry comparisons separately for sequences 2 and 3. We find that Mann-Whitney test fails to reject the null hypothesis that in sequence 2, prices in the small cost asymmetry sessions S17-S20 are equal to prices in the large cost asymmetry sessions S21-S24 (p-value=0.25, n=m=4). Similar conclusions can be drawn if we compare sequence 3 prices across the two levels of cost asymmetry.

Hence, regardless of whether comparisons are made within session or across sessions and despite the starkly different equilibrium predictions, we find that prices in PM treatment are invariant to the level of cost asymmetry. This conclusion is important since it highlights the fact that guarantees eliminate the disparity between the pricing structures of two different levels of cost asymmetry. This conclusion, however, is based on absolute price levels, and it would be erroneous to infer that the impact of price matching guarantees is the same for both cost structures. Next, we compare whether PM prices under different levels of cost asymmetry converge closer to collusive outcome or remain at the Bertrand-Nash levels.

Result 6: PM prices in the small cost asymmetry treatment ($c_l = 2$ and $c_{h1} = 5$) are closer to the collusive outcome than to the Bertrand-Nash outcome. PM prices in the large cost asymmetry treatment ($c_l = 2$ and $c_{h2} = 8$) lie further away from the collusive outcome than from the Bertrand-Nash outcome. (Qualitative support for Hypotheses 3 and 4B.)

Support: In order to examine whether the behavior in the PM treatment with small cost asymmetry is better described by the collusive or the Bertrand-Nash outcome, we calculate the average deviation in market transaction prices from the two benchmarks. For example, in sequence 2 of session S9, the average deviation of the observed market price from the

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[22] Similar conclusions are reached using the across session comparisons. For both small and large cost asymmetry treatments, prices were lower in sequence 2 when compared to sequence 3 (p-value=0.08 for the Mann-Whitney rank sum test).

Bertrand-Nash equilibrium of $4.99 is 3.75 and the average deviation from the collusive outcome of $10 is 1.25. This level of difference is similar across most sessions with small cost asymmetry.[23] Pooling across all "single and dual" small cost asymmetry sessions, we find the average deviation from the Bertrand equilibrium is 3.67 times the average deviation from the collusive equilibrium. This suggests that the collusive outcome describes subject behavior better than the Bertrand outcome. More formally, nonparametric tests on the pair-matched observations of these average deviations provide evidence in favor of larger deviations of observed prices from the Bertrand level rather than from the collusive price level. In 14 out of the possible 16 negative differences for deviation, summary statistic lead to a sign rank test value of -3.3, which is significant at 1-percent level. We can therefore conclude that although cost asymmetry makes collusion more difficult (Result 4), the average market prices in the PM treatment with small cost asymmetry converge closer to the collusive outcome than to the Bertrand outcome.

In case of large cost asymmetry, theory predicts that price matching guarantees are not effectively used in the Nash equilibrium and thus, are not required to support the equilibrium price. Yet, in the laboratory, we document that price guarantees led to prices greater than the NE price, even with large cost asymmetry (Result 3). The question then remains whether the increase in PM prices is sufficiently large to rise up to the collusive level of $10, or if the increase is closer to the Bertrand outcome of $7.99. As with small cost asymmetry, we calculate the average deviation of the observed market price from the collusive and Bertrand outcomes, but unlike before, the result is less stark. In only one-quarter of the sessions with large cost asymmetry (5 out of 20), the deviation from the collusive prediction is less than that from the Bertrand prediction.

Pooling across all sessions, however, the average deviation of the observed price from the collusive level (1.47) is greater than from the Bertrand level (0.97). Nonparametric tests on the relevant pairwise differences also reject the null hypothesis that there is no difference in the average deviation of observed prices from the collusive and Bertrand outcome (p-value = 0.04 for sign test and sign rank test). We can therefore conclude that in case of large cost

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[23] The average deviation from collusive equilibrium is less than from Bertrand equilibrium in only two (out of 16) sessions: S10 and S12.

asymmetry, although PM prices are higher compared to the NPM treatment, the observed prices remain closer to the Bertrand-Nash level than to the collusive level.

Summarizing the results, we can state that, regardless of the level of cost asymmetry, price matching guarantees help sustain a market price above the high-cost seller's marginal cost (Result 3). In case of small cost asymmetry, it takes significantly longer to see an increase in the average market price (Result 4), but on an average, observed prices do converge closer to the collusive rather than to the Bertrand-Nash level (Result 6). With large cost asymmetry, the adoption of price guarantees increases average market prices, but the observed prices remain closer to the Bertrand-Nash level (Result 6). In the experiment, the use of price guarantees implies smaller than predicted differences in prices across the two different levels of cost asymmetry (Result 5). The only questions remaining concern the factors that influence the use of such guarantees.

4.C Adoption of Price Matching Guarantees

Table 7 reports the frequency of price guarantee adoption in the later periods of the asymmetric cost sessions. Unlike the symmetric cost treatment, the price guarantee decision is not straightforward when costs are asymmetric. Theory predicts that the low cost seller ($c_l = 2$) will be indifferent between whether or not to adopt price matching guarantees. In practice, however, these guarantees may serve as an insurance policy against irrational undercutting by the high cost seller and therefore allows the low cost seller to attempt higher prices. Accordingly, in the experiment we find that 89.25 percent of the low cost sellers employ price matching guarantees. Furthermore, subjects who adopt price matching guarantees post prices that are significantly higher than subjects who chose not to adopt such guarantees (p-value<0.001).

In the case of small cost asymmetry ($c_{h1} = 5$), the highest profit yielding equilibrium is for both sellers to price at the collusive level. However, for the low cost seller, the difference between the collusive and the Bertrand profit is small. Furthermore, the level of Bertrand profit is known, whereas collusive pricing requires the low cost seller to make assumptions regarding the rationality of the high cost seller, and therefore is less certain. Price guarantees provide the high cost seller a means to signal her cooperation or rationality to the low cost seller. By adopting price matching guarantees, the high cost seller essentially delegates the

pricing decision to the low cost seller. This delegation to a single seller has the benefit that price will be set closer to the collusive level. A majority of the subjects in the experiment recognized the advantage of this delegation. Overall, in case of small cost asymmetry, 79.9 percent of the high cost sellers chose to adopt price matching guarantees. Within session comparisons show that adoption rates of the high cost sellers are not significantly different from those of the low cost sellers (p-value = 0.2 for the one tailed sign test and p-value =0.08 for the sign rank test).

Of course, it is possible that subjects in the experiment chose to match prices simply because it was a new option and assumed that they were supposed to use it. The large cost asymmetry treatment helps us distinguish the novelty value and experimenter expectation effect from profit considerations. In the case of large cost asymmetry ($c_{h2} = 8$), it is not optimal for the high cost seller to delegate its pricing decision to a seller whose costs and optimal price are very different from its own. By not adopting price guarantees, a high cost seller can keep control of its pricing and ensure itself non-negative profit. In the experiment, we find that less than half the high cost sellers refrain from price matching (46.13 percent). While these adoption rates are clearly much lower than that of the low cost sellers (p-value = 0.02 for the sign test and 0.01 for the sign rank test) or that of the high cost sellers in case of small cost asymmetry (p-value= 0.02 for Mann-Whitney test), they are much higher than the equilibrium prediction. Therefore, the repeated adoption of price guarantees cannot be explained by novelty value, or the subjects' response to assumed experimenter expectations alone.

Roth and Murnigham (1978) find that responsiveness of cooperation is positively related to the probability of continuation in an infinitely repeated prisoner dilemma stage game. Note that in theory, our game involves a one-shot static interaction and the high cost seller can do no better than remain content with zero profit. In practice, however, subjects play the game repeatedly and the dynamics are much more complex. The high cost seller remains matched with the same seller for four periods and therefore has an incentive to teach the other seller to split the market. This is possible only by adopting a price matching guarantee. For instance, consider a situation where in period 1, the low cost seller adopts the equilibrium strategy of marginal cost undercutting and prices at $7.99. The high cost seller, on the other hand, chooses to adopt the price matching guarantee and prices at the collusive level of $10. Since

the high cost seller chose to match prices, the effective price becomes $7.99 and the 10 buyers are equally split between the two sellers. The high cost seller makes an aggregate profit of negative $0.05 in period 1. The profit for the low cost seller is positive ($29.95), but strictly lower than either the collusive profit ($40) or the Bertrand profit ($79.9). Since the price guarantee decision is fixed for periods 2, 3 and 4, and the low cost seller cannot engage in undercutting, she is better off by pricing at the collusive level. Therefore, by incurring a one-period loss of $0.05, the high cost seller can potentially make a profit of $10 in each of the remaining 3 interactions.

Of course, likewise, the low cost seller has the incentive not to give in to strategic play by the high cost seller. In period 1, the threat by the high cost seller to adopt price matching is not credible, but becomes real in the subsequent periods, when the price matching guarantee decision is sunk. By engaging in undercutting, regardless of the guarantee decision of the high cost seller, a low cost seller can teach a high cost seller to remain passive over the length of the session. This credible threat of future retaliation can modify the high cost seller's behavior and restore the Bertrand-Nash outcome.

It is therefore important to examine both sides of play: whether or not the high cost seller employs repeated game strategies and if so, to what extent it affects the opportunistic behavior of the low cost seller.

Our design does not include randomly terminated interactions, so one-to-one comparison to repeated game behavior observed in infinitely repeated games is not feasible. However, some suggestive answer to the first question can be obtained by reversing the argument and investigating whether the observed play in our finite horizon game is similar to the one-shot game considered in the literature. To this end, we compare sessions where the guarantee decision is fixed for the length of the interaction (four periods) to sessions where the guarantee decision is made every period (S25 and S26). In the latter sessions, the high cost seller can no longer force the collusive equilibrium through repeated play, so the best strategy is not to adopt price guarantee. The data show that the adoption rate for the high cost seller decreases to 36.7 percent, compared to an all-sessions average of 50.1 percent, and is still significantly greater than zero. This indicates that repeated play as a strategic move alone cannot explain the disproportionately high adoption rates by the high cost seller.

Further support in this regard is provided in a recent study by Dal Bó (2005). Dal Bó compares the rate of cooperation in a fixed finite horizon game (where subjects remain matched for four periods) to an infinitely repeated game (where the probability of continuation is ¾). He finds that although the expected number of rounds is the same in both games, rates of cooperation in the first round of the finite horizon game is significantly lower. Furthermore, this cooperation declines so the level of cooperation in the final round of the finite horizon game is similar to the level of cooperation in a one-shot game. He concludes that cooperation does not depend on the length of interaction, but on the "shadow of the future". Since the length of interaction is fixed and known in our experiment, following this experimental evidence, we conjecture that repeated game strategies are not likely to have come into play.

The remaining question is the impact of these high adoption rates on the low cost seller's incentive for cooperation. Indirect evidence was documented earlier in Result 6. We report that PM prices in large cost asymmetry sessions are closer to the Bertrand-Nash equilibrium rather than the collusive outcome, which indicates that strategic play by the high cost seller does not invoke cooperation from the low cost seller.[24] More direct evidence can be obtained by comparing the average price posted by the low cost seller in the non-guarantee decision rounds (periods 2, 3 and 4) to the average price in the decision round (period 1). It is conjectured that if the strategic play by the high cost seller is effective, then the average price in the non-decision rounds should be higher. However, both the sign test and sign rank test favor the null hypothesis that pairwise differences in the average price of decision and non-decision round are not significant. Therefore, we find that repeated interaction does not limit subjects' static opportunistic behavior, either in terms of pricing or in terms of guarantee adoption.

Finally, since the phenomenon of repeated adoption of price guarantees by the high cost sellers cannot entirely be explained by either the novelty value or the repeated game incentives, we can potentially invert the causality argument. In particular, we conjecture that perhaps the supra-competitive prices observed in the PM treatment (Results 3) resulted in

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[24]A similar result is obtained by Brown-Kruse et al. (1994) in an oligopoly market experiment. They find that a finite number of repetitions raises prices above the competitive levels, but that prices still remain far below the monopoly outcome. Palfrey and Rosenthal (1994) find that the rate of contribution in a public goods game does increase with repetition but the increase is small (from 29 to 40 percent).

widespread adoption. In the NPM sequence of the large cost asymmetry sessions, the high cost sellers are usually undercut by the low cost sellers and therefore made positive (negative) profit a mere 3.4 (15.9) percent of the time. This contrasts with the PM sequence, where the proportion of high cost sellers making negative profit remains the same (15.5 percent), but 46.8 percent of the sellers made positive profit.

To analyze other factors that may influence guarantee choice behavior across the two cost asymmetry treatments, we employ parametric modeling techniques. Table 8 presents the estimates of random effect probit regressions. The dependent variable is one when the seller adopts a price guarantee. Consistent with the theoretical prediction we find that the use of price guarantees is significantly lower in case of large cost asymmetry (row 1). Results in the second row indicate that low cost sellers adopt price matching guarantees more often than the high cost sellers, irrespective of the level of high cost. Furthermore, the usage of guarantees was found to be increasing in lagged own guarantee usage and lagged other seller's guarantee usage (rows 3 and 4). This suggests that a form of own-and-observational learning takes place over the course of the session. The fact that sequence-level fixed effects are positive and significant is consistent with the previous observation that prices are higher in sequence 3. Lastly, we find that the lagged pricing decisions and consequent lagged profit levels seem to have no impact on a seller's decision of whether or not to adopt price matching guarantees.

Segregating along the lines of large and small cost asymmetry sessions, we find that, as expected, the impact of seller cost on guarantee adoption rate is more pronounced in the large cost asymmetry sessions. High cost sellers are much less likely to adopt price guarantees when the cost asymmetry is large than when the cost asymmetry is small. Similar results obtained in the "dual and single" cost asymmetry sessions indicate that the length of interaction does not have a significant impact on sellers' guarantee decisions.

5. Conclusion

In this experimental study we examine the impact of price matching guarantees on seller's pricing behavior in a homogeneous goods, posted offer duopoly market. Theory suggests that price matching guarantees act as a facilitation device for collusion by eliminating seller's incentive to engage in competitive undercutting. Our findings establish that when costs are symmetric, allowing sellers to institute price guarantees causes markets to attain and sustain

the collusive outcome. When costs are asymmetric, however, differential gains for cooperation affect the collusive ability of price matching guarantees. While prices with guarantees remain higher than prices without the use of such guarantees, the attainment of collusive levels is not absolute. The lesser use of price guarantees combined with lower average prices and slower convergence to the reservation value alludes to the fact that by its mere presence, the cost asymmetry might curtail collusive behavior.

It is important to note that the laboratory environment instituted in our experiment lacks the complexity of a naturally occurring field environment. We abstract from structural features such as product heterogeneity, unknown seller cost distribution, consumer heterogeneity in information costs, consumer's perception of such guarantees and their subsequent search behavior. The next step is to explore if incorporating these facets will cause the relationship between higher prices and usage of price guarantees to break down. Similarly, it would be interesting to allow simultaneous institution of both price matching and price beating guarantees, and analyze their impact on seller pricing decisions as well as study their relative rates of adoption.

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Table 1: Experimental Design

Table 1.A: Experimental Design for Symmetric Cost Sessions

$c = 5$			
No. of sessions	Sequence 1	Sequence 2	Sequence 3
4	NPM*	PM*	NPM
4	PM	NPM	PM

Table 1.B: Experimental Design for Asymmetric Cost Sessions

$c_l = 2$; $c_{h1} = 5$ and $c_{h2} = 8$					
Dual cost sessions					
No. of sessions	NPM*	PM*	PM	Cost switch**	PM option***
4	$c_{h1} = 5$	$c_{h1} = 5$	$c_{h2} = 8$	4	4
4	$c_{h2} = 8$	$c_{h2} = 8$	$c_{h1} = 5$	4	4
Single Cost sessions					
No. of sessions	NPM	PM	PM	Cost switch**	PM option***
2	$c_{h1} = 5$	$c_{h1} = 5$	$c_{h1} = 5$	24	4
2	$c_{h1} = 5$	$c_{h1} = 5$	$c_{h1} = 5$	12	4
2	$c_{h2} = 8$	$c_{h2} = 8$	$c_{h2} = 8$	24	4
2	$c_{h2} = 8$	$c_{h2} = 8$	$c_{h2} = 8$	12	4
2	$c_{h2} = 8$	$c_{h2} = 8$	$c_{h2} = 8$	12	1

Notes: *NPM (No Price Matching) treatment lasts for 16 rounds while the PM (Price Matching) treatment lasts for 24 rounds.
** The number of periods after the cost was switched between the two sellers in the PM treatment. The costs were switched after 8 periods in the NPM (12) and not switched at all in the NPM (24).
*** The number of periods after which PM option was made available.

Table 2: Equilibrium Benchmarks

		Cost	Price Guarantee	Market Price	Profit Low cost	Profit High cost
Symmetric Costs	**NPM**	$c = 5$	-	$5.00	$0	$0
	PM	$c = 5$	PM	$10.00	$25	$25
Asymmetric Costs	**NPM**	$c_l = 2$ $c_{h1} = 5$	-	~$4.99	$29.90	$0
		$c_l = 2$ $c_{h2} = 8$	-	~$7.99	$59.90	$0
	PM	$c_l = 2$ $c_{h1} = 5$	PM	$10.00	$40	$25
		$c_l = 2$ $c_{h2} = 8$	No PM	~$7.99	$40	$10

Table 3: Average Market Price in Symmetric Cost Sessions*

Session ID#	Cost	Sequence 1 (Periods 1-16)	Sequence 2 (Periods 17-48)	Sequence 3 (Periods 49-64)
S1	5	$5.49	$9.98	$6.17
S2	5	$5.64	$10.00	$7.72
S3	5	$5.78	$10.00	$7.06
S4	5	$5.86	$10.00	$6.01
S5	5	$8.30	$5.80	$9.87
S6	5	$9.78	$5.82	$10.00
S7	5	$9.53	$6.35	$9.91
S8	5	$9.47	$6.07	$10.00

* Shaded regions in the table indicate periods when price matching guarantee option was available.

Table 4: Average Market Price in Asymmetric Cost Sessions

Table 4.A: Average Market Price in Asymmetric "Dual Cost" Sessions**

Session ID#	Cost Sequence*	NPM (Periods 1-16)	PM (Periods 17-48)	PM (Periods 48-64)
S9	5-5-8	$5.45	$8.75	$9.55
S10	5-5-8	$4.82	$6.54	$8.64
S11	5-5-8	$5.23	$8.67	$9.04
S12	5-5-8	$4.41	$7.26	$8.48
S13	8-8-5	$7.27	$7.23	$9.81
S14	8-8-5	$7.79	$8.13	$8.39
S15	8-8-5	$7.97	$8.82	$9.62
S16	8-8-5	$7.23	$7.84	$9.40

* Cost sequence X-X-Y indicates that the cost of the high cost seller is X in the first two sequences (16 periods of the NPM treatment followed by 24 periods of PM treatment). In the last sequence, PM treatment features high cost equal to Y.

Table 4.B: Average Market Price in Asymmetric "Single Cost" Sessions**

Session ID#	Cost Sequence*	NPM (Periods 1-16)	PM (Periods 17-48)	PM (Periods 48-64)
S17	5-12	$5.65	$9.14	$9.91
S18	5-12	$5.05	$9.26	$8.99
S19	5-24	$5.07	$8.1	$9.92
S20	5-24	$5.64	$9.54	$9.63
S21	8-12	$8.03	$8.65	$9.46
S22	8-12	$7.35	$8.44	$8.69
S23	8-24	$7.84	$8.15	$9.8
S24	8-24	$7.35	$8.51	$9.11

* Cost sequence B-C indicates that the cost of high cost seller is B and the seller costs are switched after C periods.

** Shaded regions in the table indicate periods with large cost asymmetry ($c_l = 2$ and $c_{h2} = 8$).

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Table 5: Coefficient of Variation in Symmetric Cost Sessions*

Session ID#	Cost	Sequence 1 (Periods 1-16)	Sequence 2 (Periods 17-48)	Sequence 3 (Periods 49-64)
S1	5	8.71	1.91	20.56
S2	5	8.91	0	19.70
S3	5	7.78	0	23.01
S4	5	9.21	0.2	12.44
S5	5	19.34	8.81	4.85
S6	5	6.33	11.30	0
S7	5	11.82	16.82	3.29
S8	5	11.45	22	0

* Shaded regions in the table indicate periods when price matching guarantee option was available.

Table 6: Coefficient of Variation in Asymmetric Cost Sessions

Table 6.A: Average Market Price in Asymmetric "Dual Cost" Sessions**

Session ID#	Cost Sequence*	NPM (Periods 1-16)	PM (Periods 17-48)	PM (Periods 48-64)
S9	5-5-8	22.67	19.79	15.66
S10	5-5-8	7.70	37.67	16.38
S11	5-5-8	8.14	20.62	13.32
S12	5-5-8	27.87	34.51	12.17
S13	8-8-5	17.03	29.16	8.32
S14	8-8-5	4.76	8.48	24.66
S15	8-8-5	1.07	12.76	12.35
S16	8-8-5	11.45	14.28	11.39

* Cost sequence X-X-Y indicates that the cost of the high cost seller is X in the first two sequences (16 periods of the NPM treatment followed by 24 periods of PM treatment). In the last sequence, PM treatment features high cost equal to Y.

Table 6.B: Coefficient of Variation in Asymmetric "Single Cost" Sessions**

Session ID#	Cost Sequence*	NPM (Periods 1-16)	PM (Periods 17-48)	PM (Periods 48-64)
S17	5-12	17.06	16.93	4.79
S18	5-12	19.86	18.50	25.01
S19	5-24	5.14	28.12	6.27
S20	5-24	17.03	10.79	10.84
S21	8-12	3.48	9.70	8.16
S22	8-12	23.38	11.03	10.06
S23	8-24	7.31	19.13	9.27
S24	8-24	21.75	9.99	12.56

* Cost sequence B-C indicates that the cost of high cost seller is B and the seller costs are switched after C periods.
** Shaded regions in the table indicate periods with large cost asymmetry ($c_l = 2$ and $c_{h2} = 8$).

Table 7: Frequency of adoption of price matching guarantees

Small Asymmetry $c_l = 2$ $c_{h1} = 5$	Low cost seller	High cost seller	Percentage of time when both sellers adopted PM
Dual cost sessions	83.59	71.83	63.28
Single Cost sessions	93.90	86.52	82.02
All sessions	89.25	79.91	73.57
Large Asymmetry $c_l = 2$ $c_{h2} = 8$	Low cost seller	High cost seller	Percentage of time when both sellers adopted PM
Dual cost sessions	74.22	51.56	42.19
Single Cost sessions	78.02	55.73	40.66
All sessions	76.33	53.87	41.34

Table 8: Random Effects Probit Model of seller i's decision to use price guarantee in the case of cost asymmetry.[a]

Dependent variable: Price Guarantee Dummy =1 if seller used price guarantee (= 0 otherwise)

Variable	All "single and dual" cost sessions			Large cost asymmetry	Small cost asymmetry	Dual cost sessions	Single cost sessions
Asycostdum = 1 if c = 2/8 = 0 if c = 2/5	-0.76** (0.13)	-0.61** (0.12)	-0.62** (0.12)			-0.65** (0.16)	-0.67** (0.18)
High_cost = 1 if c = 5 or 8 = 0 if c = 2	-0.89** (0.1)	-0.90** (0.1)	-0.89** (0.01)	-1.02** (0.12)	-0.58** (0.16)	-0.41** (0.17)	-0.92** (0.13)
Lagg = 1 if seller i used price guarantee last period = 0 otherwise		0.81** (0.11)	0.78** (0.12)	0.95** (0.14)	0.68** (0.21)	-0.81** (0.21)	1.66** (0.15)
Lagotherg = 1 if seller j used price guarantee last period = 0 otherwise		0.34** (0.1)	0.31** (0.1)	0.32** (0.12)	0.47** (0.19)	0.48** (0.17)	0.42** (0.14)
Average Price of seller i in the last 4 periods [c]			0.05 (0.03)				
Sequence = 1 if periods 49-64 = 0 if periods 17-48	0.54** (0.11)	0.36** (0.11)	0.34** (0.11)	0.24* (0.14)	0.48** (0.2)	0.87** (0.17)	0.17 (0.21)
1/(period)	-2.4 (1.93)	-1.63 (1.88)	-1.41 (1.87)	-0.76 (2.38)	-4.12 (3.07)	-4.91 (3.06)	-1.12 (3.93)
Intercept	1.63** (0.21)	0.75** (0.23)	0.34 (0.33)	0.09 (0.25)	0.7* (0.37)	1.52** (0.37)	0.27 (0.41)
Observations	1312	1312	1312	736	576	512	800
Log Likelihood	-596.07	-568.97	-567.31	-375.25	-196.72	-239.82	-277.16

Standard errors are in parentheses. ** denotes 1% level of significance and * denotes 10% level of significance.

[a] Probit model using session and sequence level fixed effects and subject level random effects.

[b] Including an interaction term that accounts for lagged seller cost on past guarantees usage makes no significant difference to the results.

[c] Marginal impact of average price of the last 3 periods (when the firm makes only pricing decisions) or average profit of the last 3 or last 4 periods on the usage of price guarantees is never significant.

SYMMETRIC COST SESSIONS (NPM - PM - NPM)

Market Transaction Price

Averaged across 4 duopolies per session
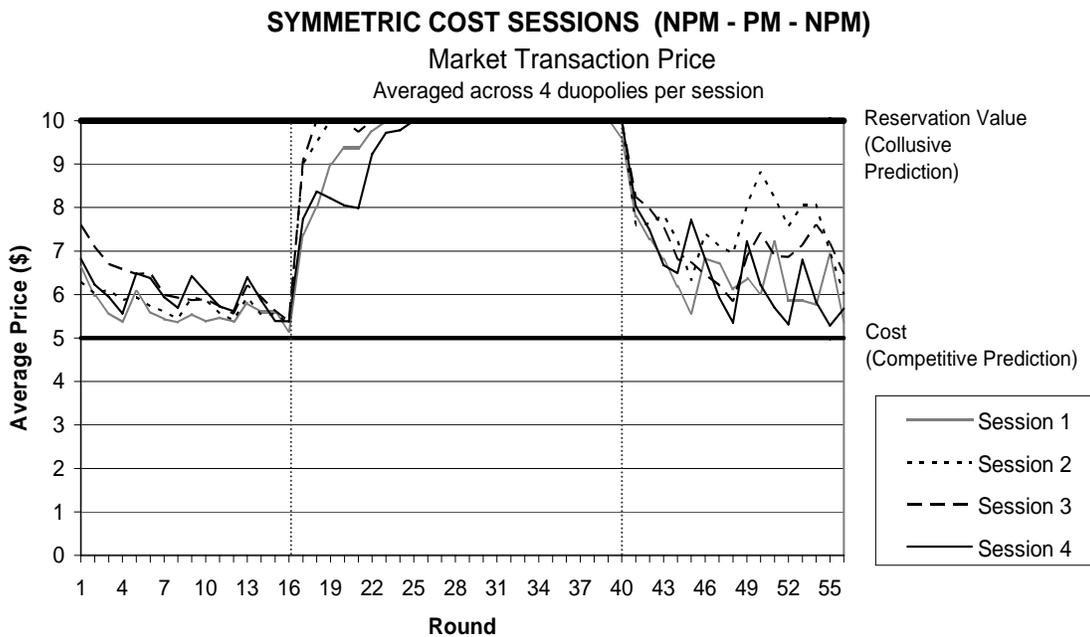
Figure 1: Average Price in Symmetric cost sessions (NPM-PM-NPM)

SYMMETRIC COST SESSIONS (PM - NPM - PM)

Market Transaction Price

Averaged across 4 duopolies per session
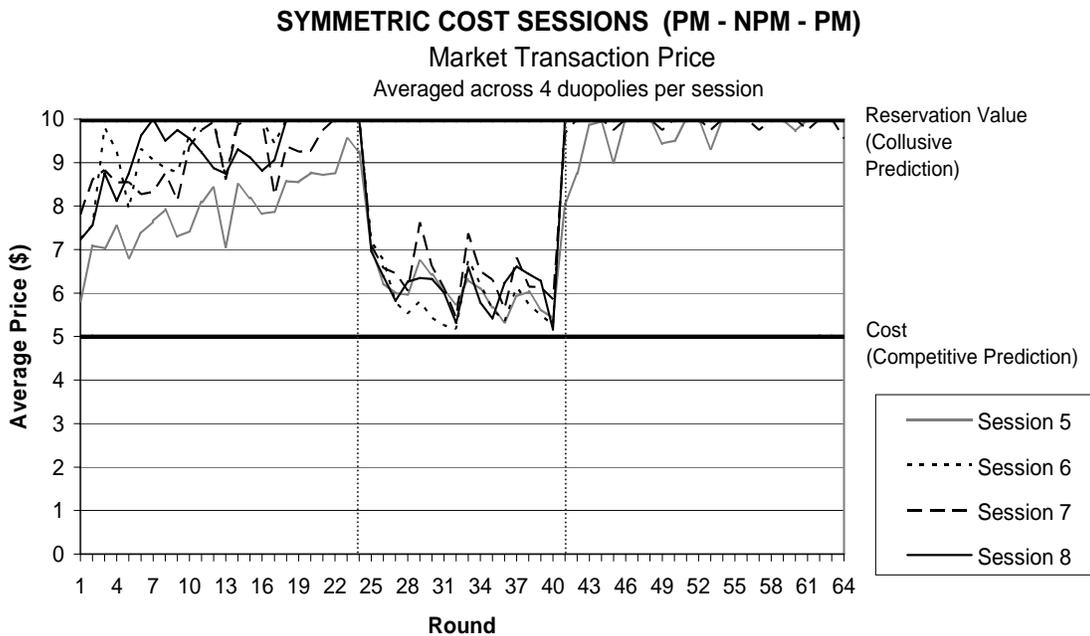
Figure 2: Average Price in Symmetric cost sessions (PM-NPM-PM)
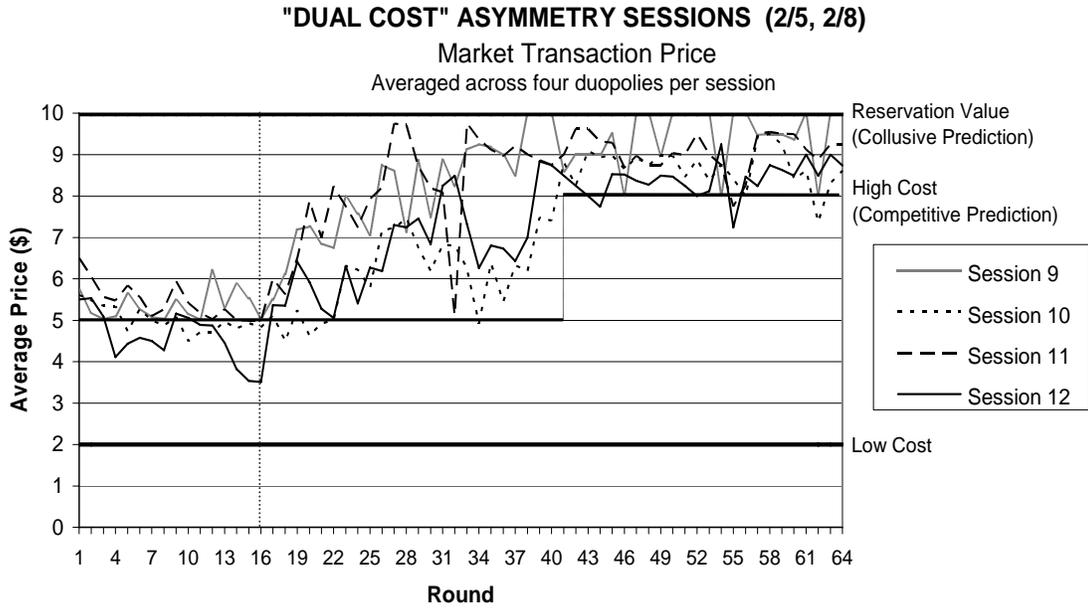
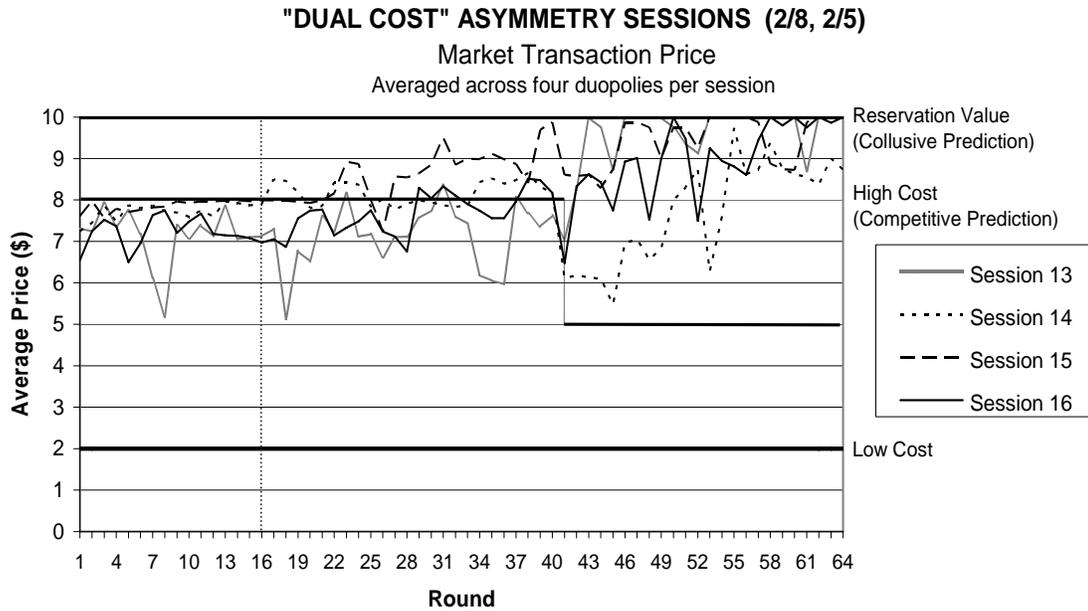Figure 3: Average Price in "Dual Cost" Asymmetric sessions (2/5-2/8)

Figure 4: Average Price in "Dual Cost" Asymmetric sessions (2/8-2/5)

"SINGLE COST" SMALL ASYMMETRY SESSIONS (2/5)

Market Transaction Price
Averaged across four duopolies per session
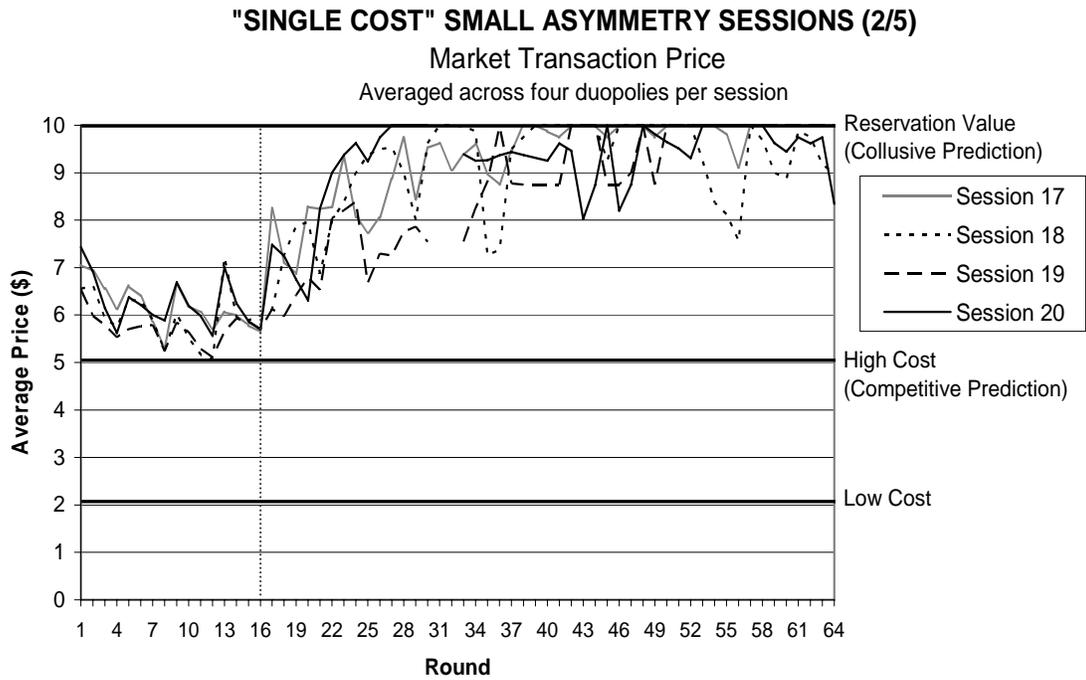
Figure 5: Average Price in "Single Cost" Asymmetric sessions (2/5)

"SINGLE COST" LARGE ASYMMETRY SESSIONS (2/8)

Market Transaction Price
Averaged across four duopolies per session
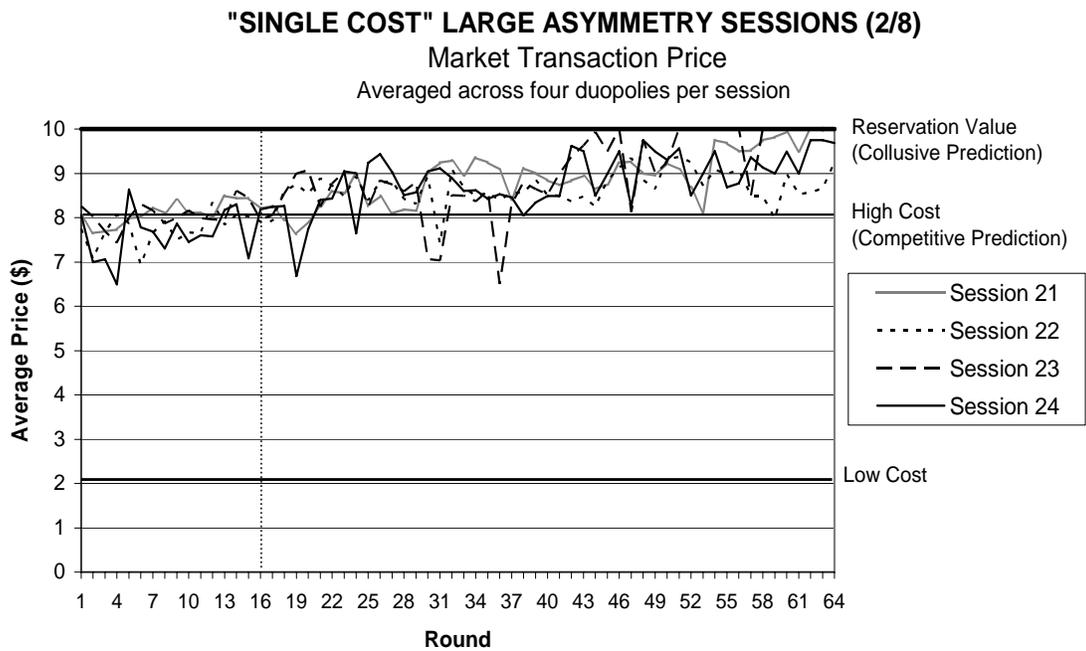
Figure 6: Average Price in "Single Cost" Asymmetric sessions (2/8)

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"SMALL COST" ASYMMETRY SESSIONS (2/5)

Average Market Transaction Price

Averaged across duopolies for different price guarantee combinations
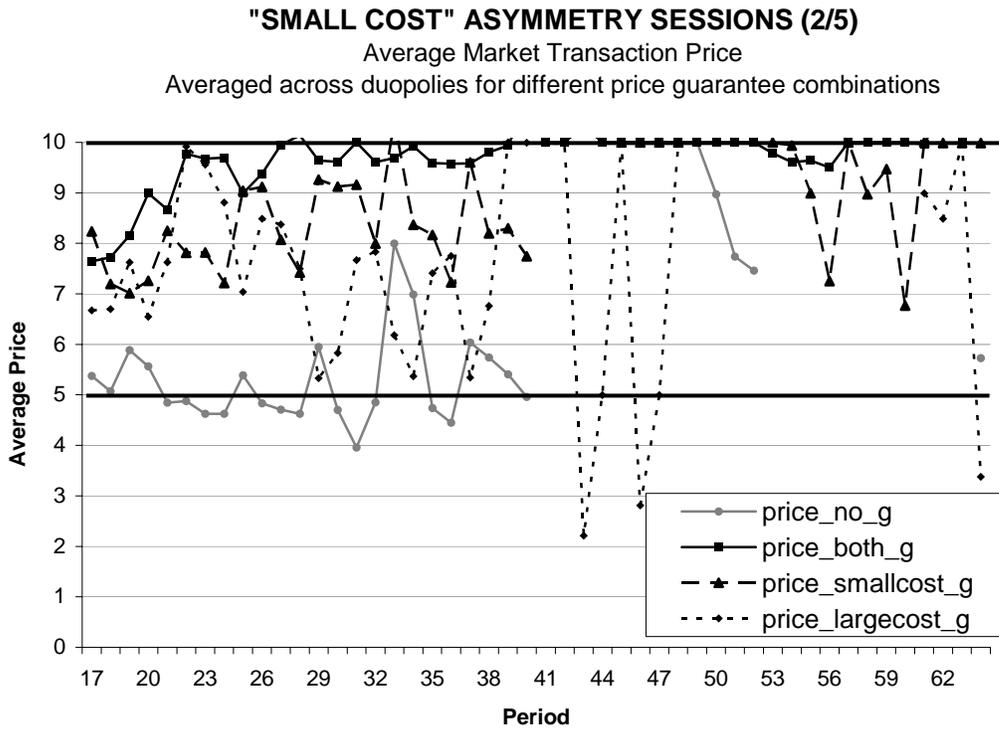
Figure 7: Average Price for different guarantee combinations in "Small Cost"
Asymmetry Sessions (2/5)

"LARGE COST" ASYMMETRY SESSIONS (2/8)

Averge Market Transaction Price

Averaged across duopolies for different price guarantee combinations
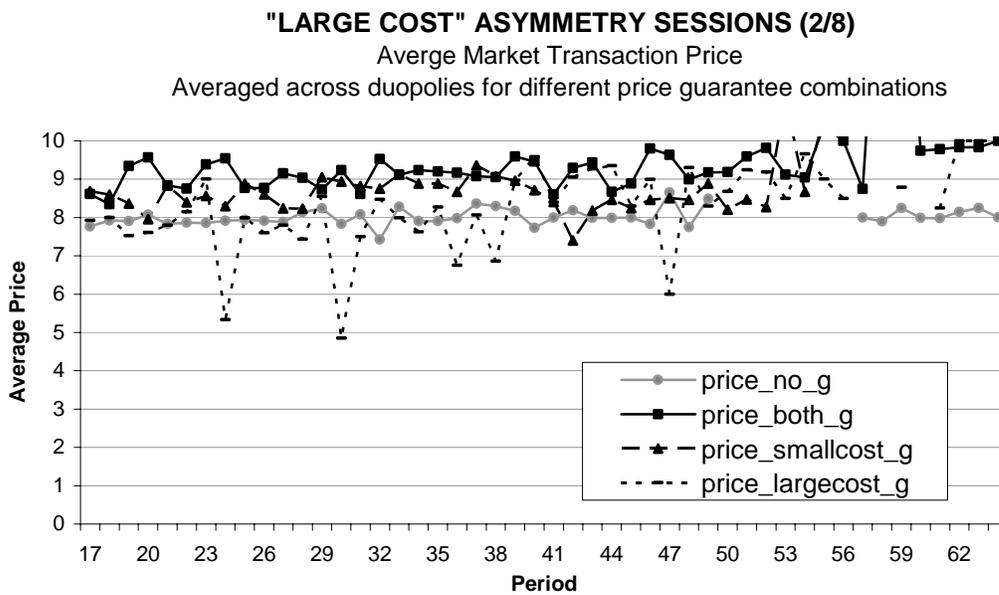
Figure 8: Average Price for different guarantee combinations in "Large Cost"
Asymmetry Sessions (2/8)

Appendix

Sample Instructions (Symmetric Cost sessions)

This is an experiment in the economics of market decision-making. Various research agencies have provided funds for the conduct of this research. The instructions are simple and if you follow them carefully and make good decisions you may earn a considerable amount of money that will be paid to you in cash at the end of the experiment. It is in your best interest to fully understand the instructions, so please feel free to ask any questions at any time. It is important that you do not talk or discuss your information with other participants in the room until the session is over.

All transactions in today's experiment will be in experimental dollars. These experimental dollars will be converted to real US dollars at the end of the experiment at the rate of 55 experimental dollars = $1. Notice that the more experimental dollars you earn, the more US dollars you earn. What you earn depends partly on your decisions and partly on the decisions of other participants in this experiment. Also, before you make any decisions in this experiment, you will be given a starting balance of 55 experimental dollars, equal to $1.00 US. Any earnings you make in this experiment will be added to your starting balance.

In this experiment we are going to conduct markets in which you will be a participant in a sequence of trading periods. The experiment consists of 3 sequences, where each section will be comprised of 16 or 24 trading periods. The instructions for each section will be given at the start of that particular section.

In every period you will be a seller of a fictitious good X. The participants in today's experiment will be randomly re-matched every four periods into 8 markets with 2 sellers in each market. Thus, you will be matched with the same seller for 4 periods in a row, then the specific seller in your market will change randomly every four periods, for example in period 1, period 5, period 9, and so on.

The buyers in each market in today's experiment are simulated by computerized "robots." There are 10 robot buyers. The maximum price that each buyer is willing to pay for a single unit is 10 experimental dollars, and this will be displayed on everyone's decision screen, as shown in   Picture 1 on the next page. Each robot buyer will purchase

one unit of good X each period, as long as the unit price is below the maximum price. The maximum price will remain the same throughout the experiment. Buyers know the price of both the sellers before making their purchase decision, and they will purchase from the seller offering the lowest price. If both sellers set the same price then the 10 buyers are equally split between the two sellers. For example:

- If Seller 1 sets a price of 6 dollars and Seller 2 sets a price of 8 dollars, then Seller 1 sells 10 units while Seller 2 sells 0 units.
- If both Seller 1 and Seller 2 set a price of 6 dollars, then each seller will sell 5 units.

Trading Instructions for Section 1

In this section of the experiment, you will choose your price for good X in each period. Each unit of the good costs you 5 experimental dollars to produce. You will pay this cost only if you sell the good. For example, suppose you sell 10 units. Then your cost is 50 experimental dollars, while if you sell zero units your cost is zero experimental dollars. Also notice that if your cost is 5 dollars and you set any price below 5 dollars, then your earnings will decrease for each unit sold.

You may enter any price greater than zero, but remember that the buyers are not willing to pay a price greater than 10. Thus 3.46, 6.99, and 8 dollars are all acceptable prices. After entering your price, please click the "continue" button. An example of the decision screen is shown on the next page in Picture 1. The past period decisions of both sellers and your past profits are displayed in the lower half of the screen. Notice the history table in Picture 1, that P1 shows your price, while P2 displays the price of the other seller in the market. Similarly, Q1 is the quantity of units you sold, while Q2 is the quantity of units the other seller sold.

At the end of each period, your profit is computed and displayed on the output screen as shown in Picture 2. Your profit is calculated as follows:

Profit = (price x number of units sold)–

(cost of producing the good (5) x number of units sold)

Once the outcome screen is displayed you should record all of the trading information, your price (P1), quantity sold (Q1), the other seller's price (P2) and quantity (Q2) in your Personal Record sheet. Also, record your profit from this period and the

total profit from all previous periods. Then click on the button on the lower right of your screen to begin the next trading period. Recall that you will be randomly re-matched with a different seller every 4 periods.

Are there any questions before we begin?

Picture 1 Decision Screen

Picture 2 Outcome Screen

<u>Trading Instructions for Section 2</u>

This section is similar to Section 1. As in the previous section, you have to decide what price to charge for a unit of good X. All unit costs and consumer rules are the same as before, but in this section you will have an additional decision to make besides your price. Every 4 periods, beginning in Period 1, you will be asked whether or not you would like to use the price guarantee option. There is no cost to using this guarantee option.

- If you decide <u>not</u> to use the price guarantee option, then your price is exactly what you choose it to be. That is, your *effective price* is the same as the price you enter as your decision.
- If you decide to use the price guarantee option, then two possible events may occur.

  1. If the other seller's price is less than yours, your price will automatically match the other seller's price. For example, if your price is 8 dollars and the other seller's price is 4 dollars, then your *effective price* becomes 4 dollars as well.

2. If the other seller's price is higher than yours, then your price stays the same. For example, if your price is 8 dollars and the other seller's price is 9 dollars, then your *effective price* remains at 8 dollars.

Notice that whether or not you decide to match prices in Period 1, this decision is set for the following three periods. Thus if you decide to use the price guarantee option in Period 1, then you are also deciding to match prices in Periods 2, 3, and 4. In Period 5 you will again be asked if you would like to use the price guarantee option, and this will also be your decision for Periods 6, 7, and 8. You can make your decision of whether or not you would like to use the price guarantee option by clicking on the "Yes" or "No" button on your decision screen. Notice that a record of your guarantee decision appears under G1, while the other seller's decision is G2. An example of the decision screen is shown in Picture 3 on the next page.

At the end of the period, your profit is computed and displayed on the output screen as shown in Picture 4. Your profit is calculated as follows:

Profit = (effective price x number of units sold) –

(cost of producing the good (5) x number of units sold)

An example of the outcome screen is shown on the next page. It is similar to the outcome screen in section 1, except that this outcome screen also provides information about whether you and/or the other seller used the price guarantee option.

Picture 3 Decision Screen

Picture 4 Outcome Screen

Once the outcome screen is displayed you should again record all of the trading information, and whether you and/or the other seller offered a price guarantee by circling YES or NO in your Personal Record sheet. Recall that you will be randomly re-matched with a different seller every 4 periods.

Are there any questions before we begin?

Trading Instructions for Section 3

This section is the same as in Section 1, meaning that you <u>do not</u> have a price guarantee option. You will choose a price for good X each period. Each unit of the good still costs you 5 experimental dollars to produce. Recall that you will be randomly re-matched with a different seller every 4 periods.

Are there any questions before we begin?