ESSAYS ON EXPERIMENTAL COORDINATION GAMES

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ABSTRACT

The research reported in this dissertation explores the coordination problem faced by economic agents in various strategic environments. The first chapter provides an experimental test of a theory of collusion in the presence of price-matching guarantees and thus throws light on the equilibrium selection problem embedded in this market game. The experiment yields important empirical information regarding the competitive nature of these low-price guarantees in the laboratory.

In the second chapter, more general theoretical models are developed that undermine any collusive equilibrium in the presence of price-matching guarantees. Although the theory predicts that the competitive price should emerge in equilibrium in all these models, systematic discrepancy between the theoretical prediction and the observed behavior is found.

In the third chapter, a well-known paradox is tested in the laboratory. Braess paradox (Braess, 1968) consists of showing that, in equilibrium, *adding* a new link that connects two routes running between a common origin and common destination may *raise* the travel cost for each network user. The experiment is designed to study whether the paradox is behaviorally realized in the critical minimal simulated traffic network. Results reject the hypothesis that the paradox is of marginal value and its force, if at all evident, diminishes with experience.

In the last chapter, using controlled laboratory experiments, I study how the problem of coordination failure, as embedded in the 'minimum-effort' coordination game, can be overcome using *structured*, *ex post* feedbacks related to individual

performance among members of a large group. I allow two types of performance feedback mechanisms, namely, negative and positive. I use ‘disapproval’ and ‘approval’ ratings about individual choices by group members as proxies for negative and positive feedback mechanisms, respectively. Results show that where participants are allowed to express only disapproval of others’ choices, play converges towards the most efficient coordination. In contrast, where participants can express only approval of others’ choices, inefficient coordination is obtained.

INTRODUCTION

A central question in economics is how do independent agents coordinate their decisions in a decentralized environment. The question is of great importance since in an array of economic situations miscoordination may result in inefficiency. In such environments, a decision maker's best strategy depends on the strategies adopted by other decision makers. Therefore, understanding how decision makers behave in contexts that exhibit strategic interdependence is the key to achieving efficient coordination.

A coordination game captures the essence of the kind of problem outlined above. Furthermore, a necessary feature of a coordination game is that it involves multiplicity of equilibria that may or may not be Pareto-rankable. In the absence of explicit communication, deductive equilibrium analyses (i.e., Nash equilibrium or any standard refinement technique) fail to determine a unique equilibrium solution in this class of games (see Harsanyi & Selten (1988) on this issue). Hence, the issue of equilibrium selection arises in the context of a coordination game. Given this indeterminacy, the use of laboratory techniques might help to shed light on the equilibrium selection process.

This dissertation explores, experimentally, coordination problems in three diverse strategic environments. Although efficient coordination among economic agents remains the central common theme in all of the environments, the nature of coordination that underlies each problem differs in each case.

The first chapter provides an experimental test of a theory of *tacit* collusion in the presence of price-matching guarantees that are widely used in retail markets. Theoretical literature argues that when all sellers adopt the guarantees, they may non-cooperatively sustain any price between the competitive and the monopoly price in equilibrium. Moreover, these equilibria can be Pareto-ranked in terms of profit and the most efficient coordination (from sellers' viewpoint) is achieved when all sellers choose the same action (i.e., the monopoly price) in equilibrium. However, there is disagreement in this literature as to which of these price equilibria will be actually selected by players. The experiment reported in chapter one throws light on this equilibrium selection problem embedded in this market game. As a whole, the data from the experiment extend great support to the main theoretical prediction that these guarantees yield collusive behavior among sellers. Regarding the multiplicity of equilibria, this study offers considerable evidence of coordination success.

The second chapter investigates a closely related issue. In this chapter, more general theoretical models are developed that undermine any such collusive equilibrium in the presence of price-matching guarantees. In particular, this chapter experimentally investigates whether the collusion-facilitating nature of these guarantees survives the reasonable modification of hassle costs, that is, costs borne by buyers to invoke the guarantees. The theory predicts that the competitive price should emerge in equilibrium in all these models. Results from the experiment indicate a systematic disagreement between the theoretical prediction and the observed behavior in the laboratory.

In the third chapter, a well-known paradox is tested inside the laboratory, which is known as the Braess paradox (Braess, 1968). This paradox shows that, in equilibrium, *adding* a new link that connects two routes running between a common origin and a common destination may *raise* the travel cost for each network user relative to a scenario where no such new link connects the two routes. This chapter tests two network games in the laboratory to yield empirical information regarding the paradox. The first game depicts a network with three routes (after a new link adds the two old routes) and the second game consists of a network with two routes (before a new link is added). The first game involves a strictly dominant strategy equilibrium (where coordination is not an issue) in which each network user adopts the newly available route in equilibrium. The second game involves a large number of Pareto-equivalent pure strategy Nash equilibria, which depend on coordination among agents' route choices. The results from two simulated traffic networks demonstrate that the route choice behavior is in accordance with the theoretical prediction and therefore the paradox is not of marginal value.

The last chapter uses controlled laboratory experiments to study how the problem of coordination failure, as embedded in a pure coordination game, can be overcome by using a *non-deductive* selection principle, namely, *structured*, *ex post* feedback related to individual performance among members of a large group. Two types of performance feedback mechanisms are used - negative and positive. ‘Disapproval’ and ‘approval’ ratings about individual choices by group members are used as proxies for negative and positive feedback mechanisms, respectively. Results show that where participants are allowed to express only disapproval of others’ choices, play converges towards the most efficient coordination. But where participants can express only approval of others’ choices, inefficient coordination is obtained. The results find major support from a strand of psychology literature that hypothesizes (1) that ‘social’ feedbacks of different kinds (including the ones used in this study) may obtain efficient outcomes, and (2) that negative events (e.g., receiving criticism) are stronger than positive events (e.g., receiving praise). Taken together, data from the experiment substantiate both the claims made in this literature.

CHAPTER 1: DO PRICE-MATCHING GUARANTEES FACILITATE TACIT COLLUSION? AN EXPERIMENTAL STUDY

1.1 Introduction

Retail markets are characterized by a plethora of selling strategies designed to increase a seller's market share at the expense of its rivals. One such widely used strategy is a price-matching guarantee (hereafter PMG). In retail markets, a seller adopts this guarantee to ensure buyers that its price for the product is the lowest among all of its competitors; if not so, then the seller will match any lower price offered by its rivals. At a first glance, a guarantee like this may appear competitive in nature, assuring buyers the 'best deal' in the market. However, careful reasoning might suggest something different.

Several studies, starting with Salop (1986), have argued that sellers may use PMGs as a device for facilitating tacit collusion[1]. The basic idea is that by adopting PMG, a seller automatically matches any lower price, and therefore the Bertrand epsilon undercutting argument does not increase that seller's market share; rather it just leads to lower prices and profits for all sellers. Hence, adopting the PMG and maintaining the collusive price is optimal for each seller and any price – between the competitive and the monopoly price – can be supported in equilibrium. However, in the absence of PMGs,

[1] See Doyle (1988), Logan et al (1989), Baye & Kovenock (1994), Chen (1995). Doyle (1988) analyzed the collusive argument for *n* firms. Anticompetitive effects of these low-price guarantees have been shown to be invariant to whether the guarantees and prices are chosen simultaneously or sequentially in Chen (1995) and the effect of PMGs, where firms are differentiated, have been investigated in Logan & Lutter (1989).

collusive pricing strategy is not optimal. Any market price above marginal cost cannot be sustained in equilibrium since each seller has an incentive to undercut its competitors and capture the entire market at a slightly lower price. Thus, PMGs may critically alter the pricing incentives of sellers and help them collude tacitly. This anticompetitive effect of PMGs on prices has also received support from antitrust authorities and legal scholars (e.g., Sargent, 1993; Edlin, 1997).[2]

Existing empirical evidence on the effects of PMGs on prices is far from conclusive. Some of the studies based on data from naturally occurring markets have found evidence in support of the collusive theory (e.g., Hess and Gerstner, 1991) while other studies (e.g., Manez, 1999; Arbatskaya et. al., 1999a) have concluded otherwise[3].

[2] Even though the dominant view in the literature is that PMGs are anticompetitive in nature, this may not be the sole purpose behind the adoption of matching guarantees. Other purposes may include price discrimination (e.g., Belton, 1987; Pig & Hirschleifer, 1987; Corts, 1996; Chen et al 2001; Lin, Y. Joseph 1988), low cost signaling (Moorthy and Winter, 2002) or entry deterrence (Arbatskaya, 2001).

[3] Hess & Gerstner (1991) concluded that average market prices of the products included in their study increased over a two-year period following the PMG adoption relative to the excluded products, however this price increase could be due to any possible cost or demand differences between included and excluded products, which were not controlled for in their study. On the other hand, Arbatskaya et al. (1999a) found that PMGs lead to lower advertised tire prices, but their study did not find a statistically significant effect of PMGs on retail tire prices. Even though, their study employed a set of control variables to account for firm and location specific heterogeneities present in the data, there may still remain some other sources of differences (e.g., cost asymmetries) across firms, which, if accounted for, might make the PMG coefficient significant.

Evidently, studies based on sheer observation of market data generally encounter two serious problems. First, there is a counterfactual problem that one cannot observe what prices would have prevailed if no seller had a PMG. This problem seriously restricts the scope of a direct comparison of behavior of prices with and without the guarantees in the market. Second, there is an informational problem in the absence of reliable cost and demand side information that makes it difficult to estimate the deviation of prices from the competitive price level in the presence of PMGs.

Against this backdrop, this study develops an experimental framework with induced cost and demand conditions inside the laboratory and tests the collusive theory of PMG. The main focus of this study is to investigate the collusive potential of PMGs from an experimental viewpoint. There is, however, a second and a more conceptual rationale for this study. Theories in the literature suggest that there exist multiple equilibria (i.e., a whole set of equilibrium prices between the competitive and the monopoly price) when all the sellers adopt the guarantee. Theoretical prediction in this case fails to pin down the actual behavior of players *a priori*. In contrast, laboratory experiments are often appropriate for investigating problems of multiplicity of equilibria.[4] Therefore, the use of laboratory techniques might be able to shed light on the behavior of equilibrium prices that is strongly indeterminate in nature.

This paper studies two highly stylized market models, derives testable predictions, and lays out the design of the controlled experiment to test the predictions. The

[4] Examples of the use of experiments in contexts with multiple equilibria are Van Huyck et al. (1990), Cooper et al. (1990).

laboratory experiment of these two market models reported below is well suited for yielding relevant empirical information about the theoretical predictions of the collusive theory. If the result from the experiment is inconsistent with the collusive theory, then predictions of the collusive theory are called into question. On the other hand, if the result is in keeping with the theory, then the research can proceed further to examine the predictive power of the collusive theory by subjecting the theory to more complicated market structures.

Fatas & Manez (2001) used experimental methods to study the behavior of prices in the presence of PMGs. Their design resembles a game in which a seller decides whether or not to adopt the PMG at an interval of every five rounds (so the PMG decision is intact for a block of five consecutive rounds before a seller could change the matching decision). However a seller could choose different prices in each period within such a block. Clearly, such a design creates a strategic link between matching decision and market outcomes between periods within a block, which makes it difficult to interpret the results[5]. Also, their model makes particular assumptions regarding own and cross-price elasticities of firms' demand conditions that render their results relevant for only a specific type of market. In contrast, this study tests the collusive theory inside the laboratory as a *one-shot* game, and therefore any direct strategic link (other than the

[5] Although the proposed focus of their study is to test the predictions of the collusive theory of PMG, which is usually presented as a one-shot game in the literature and also in their study, the experimental design employed in their study does not retain the one-shot character of the game. Hence, the design excludes the scope of a direct comparison of their data with the predictions of the collusive theory.

accumulated experience over the rounds) between rounds is clearly absent. Also, the model used in this study does not put similar restrictions on the demand elasticities. Deck and Wilson (2003) examined the impact of the endogenous selection of three pricing strategies and found that the PMGs generate significantly higher prices relative to other pricing algorithms.

Section 2 discusses the models that are studied inside the laboratory. Section 3 discusses the experimental procedure. Section 4 analyzes the experimental data derived under each of the two treatments, and finally section 5 concludes.

1.2 Models

The goal of this section is to develop a benchmark price competition model in which matching guarantees are absent and which will serve as a reference point for an enriched market model in which PMGs are present. The comparison of outcomes from these two different market models would help evaluate the potential of PMGs as a collusion-facilitating device. In particular, a discretized version of the classic one-shot Bertrand model of price competition similar to that introduced by Dufwenberg & Gneezy (2000), is considered for the benchmark model. In the augmented version, this benchmark model is extended to accommodate the possibility of price matching. Although the theoretical studies in the PMG literature consider both symmetric and asymmetric markets, this study restricts the analysis to symmetric markets only. Each model described below corresponds to an experimental treatment in this study. The theoretical analysis and the ensuing experimental design develop around these two models.

1.2.1 The Benchmark Model

Assume that there are three sellers in a market[6]. Each seller in the market simultaneously chooses a price in the set {1, 2, 3,… ,100}. The seller choosing the lowest price receives a total profit equal to the lowest number and the rest of the sellers earn zero profit. Ties are split equally among the sellers choosing the lowest price. This game captures the following assumptions of a simple Bertrand price competition model. Sellers sell homogeneous products[7] using constant-returns to scale production function, marginal cost for each seller is zero, there are no capacity constraints, buyer demand is inelastic up to the reservation value of all the buyers, which is equal to 100 in this case, and there is no cost or demand uncertainty. It is assumed throughout that buyers are perfectly informed about all the prices and policies prevailing in the market and they buy from the seller(s) that offer(s) the lowest price. The prediction of the Benchmark model is briefly summarized below.

[6] Although the theoretical prediction remains unaltered whether there are only two sellers or more than two sellers in the market, the reason for analyzing the case with three sellers in this study is the following. The extant experimental literature (see, Dufwenberg & Gneezy (2000), Fouraker and Siegel (1963)) on price competition have already established the result that in three–seller case, market prices tend to be relatively lower compared to two-seller case. Hence, the choice of a three seller market in this study would subject the collusive theory to a stricter test inside the laboratory.

[7] Given that in practice PMGs are typically offered on identical products only, a homogeneous product model appears to be more appropriate than a differentiated product model.

<u>Hypothesis 1</u>: *The Benchmark model has a unique Nash equilibrium in pure strategies, namely, (1,1,1). That is, in the equilibrium each seller chooses a price equal to 1 and earns a profit level equal to 1/3.*

In the augmented version of the Benchmark model, each seller has an option to adopt the PMG before price is chosen. Thus, in the augmented version, a second stage is appended to the one-stage Benchmark model. From a game theoretic point of view, a one-stage (in which guarantees and prices are chosen at the same time) or a two-stage (in which guarantees and prices are chosen sequentially) decision process yields the same qualitative predictions[8]. However, in retail markets, guarantees are not as easily changed as prices are. Sellers often adopt guarantees for extended periods of time while they frequently adjust their prices. Hence, a two-stage decision process has the only advantage over the former in the sense that it retains this specific aspect of the natural markets[9].

1.2.2 The Price-Matching Guarantee Model

The PMG model has *two* stages. In stage one, each of the three sellers simultaneously decides whether or not to adopt the PMG. A seller choosing a PMG will have to subsequently match the lowest price chosen in the market. The model assumes that sellers do not incur any cost to commit to a matching guarantee. In stage two, after observing the first stage decisions by all sellers, each seller simultaneously chooses a price in the set {1, 2, 3,…, 100}. The total profit in the market in the price-matching model will be equal to the lowest price chosen in the market. This profit will be shared

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[8] See Doyle (1988), Logan et al (1989), Baye & Kovenock (1994) and Chen (1995).

[9] See Logan et al. (1989) for a similar argument.

equally among the sellers that chose that lowest price directly or indirectly by adopting the PMG. The seller(s) who do not choose the lowest price and do not adopt a guarantee will earn zero profit.

The sellers' payoffs depend on their price choices just as in the Benchmark model except that payoffs may be altered by sellers' price-matching decisions. The price-matching model has 8 subgames[10]. Note that once a seller chooses to match its rivals' lower price(s), then that seller's effective selling price *may* differ from its posted price through its offer to match.

The PMG model has multiple subgame perfect Nash equilibria. The most interesting subgame is one in which all three sellers adopt the PMGs. In this case, given any (a)symmetric posted price triplet, price undercutting by a seller leads to lower profits for all and a higher price by a seller does not increase that seller's profit. Hence, this subgame has the following prediction.

<u>Hypothesis 2</u>: *Any symmetric effective selling price triplet between (1, 1, 1) and (100, 100, 100) is sustainable in equilibrium in the subgame where all three sellers adopt the guarantee. All the equilibria except (1, 1, 1) involve successful collusion in this subgame*[11].

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[10] There are 3 subgames in which only one seller adopts the guarantee, 3 subgames in which two sellers adopt the guarantee and 2 subgames in which either no seller or all three sellers adopt the guarantee.

[11] In this subgame, consequent levels of profit from the symmetric effective price triplets between (1, 1, 1) and (100, 100, 100) are strictly Pareto rankable and, therefore, this poses a serious coordination problem for

There are other subgames where incentives for collusion are either almost or completely absent. The three subgames in which two sellers adopt the guarantee, there are three Nash equilibria in pure strategies, namely, (1, 1, 1), (2, 2, 2) and (3, 3, 3), due to discretization of the strategy space. However, the two additional equilibria (i.e., (2, 2, 2) and (3, 3, 3)) are almost similar to (1, 1, 1) in terms economic incentive. All three sellers make insignificant amount of additional profit by deviating to either of the two equilibria. However, these equilibria have one interesting interpretation. These equilibria show that when the number of sellers adopting the guarantee increases from one to two, sellers may tacitly coordinate on one of the two higher price (though little gain is associated with those) equilibria. The equilibria in all the remaining four subgames are identical to that of the Benchmark model discussed in the beginning of the section and therefore involve no collusion. Hence, results from all other subgames can be summarized in the following way.

Hypothesis 3: *In the subgames in which at least one seller does not adopt the guarantee, all equilibria involve marginal cost pricing (i.e. (1, 1, 1))*[12] *and therefore involve no collusion.*

Regarding the guarantee, it must be noted that adopting the guarantee is a weakly dominant strategy for each seller. The intuition for that is the following. If a seller directly chooses the lowest price in the market, then (s)he is indifferent between adopting

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sellers like ones in Van Huyck et al. (1990). Only (100, 100, 100) survives the Pareto- dominance equilibrium selection criterion and involves monopoly pricing.

[12] Except the three cases mentioned above in which there are two additional equilibria.

or not adopting the guarantee. However, if (s)he does not directly choose the lowest price in the market, in that case adopting the guarantee constitutes a weakly dominant strategy for that seller and the seller does not lose the market to the rivals.

Table. 1.1 gives an overview of the two market models and the equilibrium predictions.

1.3 Procedure

The experiment was conducted at the University of Arizona's Economic Science Laboratory. Four sessions were run for each treatment, each session involving 10 rounds of play with 4 groups of 3 participants. Hence, a total of 96 participants took part in the experiment. Participants were mostly undergraduate students at the University of Arizona. Special care was taken to ensure that no one participates in more than one session. Participants were given a $5.00 show-up fee and any additional money they made during a session[13].

[13] It should be noted at this point that given the exchange rate used in the experiment (i.e., 1 experimental point is equal to 10 cents), in the Benchmark treatment a participant would receive very little money if (s)he plays according to the equilibrium prediction in all rounds. On the other hand, in the PMG treatment, a participant, if successfully colludes in every round, would make relatively high amount of money. Clearly, equilibrium payoff creates a large incentive problem for participants in the Benchmark treatment. This incentive problem is mainly due to the typical feature of a Bertrand game in which some actions are more than adequately rewarded while the equilibrium action has little saliency in terms of payoff. From a methodological point of view, this is an undesirable feature that could be mitigated by a higher exchange rate. However, this would further enhance the relative attractiveness of the collusive outcome in the PMG treatment and also would make the experiment very expensive. Given the fact that all the Benchmark sessions took no more than 45 minutes, the above mentioned exchange rate was chosen that on an average sufficiently compensates a participant for his/her time.

In both treatments, in each round participants were *randomly* matched into groups of 3 to form a triopoly market. The random matching protocol[14] was followed to retain the one-shot character of the game and to let the participants gain some experience during the session. At the end of each round, each participant knew his/her own profit, price choices, and the profits made by all other participants in that round. This information feedback mechanism was constant across treatments. Throughout the session, no communication between participants was allowed. This was made clear in the instructions that any form of communication would disqualify a participant from the experiment. Both treatments were conducted using experimental dollar, and at the conclusion of each session participants' earnings in experimental points were converted into U.S. dollars. The two treatments differed from one another in terms of the stages involved in the games played and the associated instructions[15].

At the beginning of the experiment, each participant received an instruction sheet for the treatment (s)he was participating in, an 'Earnings' sheet to record each round's profit, and a set of cards for recording each round decision(s). Instruction sheets and the decision cards had a registration number written at the top. This registration number was used to identify a participant during the experiment. The experiment started after all the

[14] The reason for not using a fixed- matching mechanism is to minimize the repeated game effects. Since the objective of the study is to capture any implicit cooperation among sellers in the presence of PMG, a fixed matching protocol would have introduced a channel of cooperation and thus confounded the results.

[15] Instructions can be found in the appendix A1.

participants finished studying the instruction. All questions were answered before each session began. An assistant helped in conducting the experiment[16].

Each session for a treatment was counted as one observation for hypothesis testing in the next section. This particular choice makes each observation quite expensive, but at the same time each data point bears a high degree of independence. This process still generates enough data points to make hypothesis testing meaningful.

1.4 Results

This section reports the main results on the behavior of prices in the two treatments, the adoption pattern of the guarantees in the PMG treatment and compares the average profits earned by participants in the two treatments. Also, it compares the observed price data with the predictions for the subgame in which all three sellers adopt the guarantee to shed light on the coordination problem faced by sellers in the PMG model.

In what follows, the four sessions of the Benchmark treatment are referred to as B1, B2, B3 and B4 and the four sessions of the PMG treatment as M1, M2, M3, and M4. The behavior of prices in round 1 for all four sessions in each treatment is analyzed first as no element of experience exists in that round. Casual observation of Figure 1.1 reveals that the equilibrium outcomes predicted by the two models are not achieved in the first round in any session. However, prices chosen by participants in all PMG sessions are

[16] Participants in the experiment were required to record all information in each round about other sellers' prices, guarantee choices, and profits. In order to avoid any possible mistake by a participant in entering this information, an assistant also recorded this information.

substantially higher than those of the Benchmark sessions. For example, average winning prices (AWPs) for the first round are 36.75, 24.5, 33.75 and 56.25 respectively for the four PMG sessions, while the same for the four Benchmark sessions are 20.5, 9, 17.5, and 20.75. It can be verified from Figure 1.2 that a similar trend exists for average posted prices (APPs) as well.

To test the hypothesis whether the posted prices in the first round in different sessions (across and within treatment(s)) came from the same distribution, a statistical test is conducted using the non-parametric Mann-Whitney U tests based on ranks. Six possible pairs of sessions for each treatment (hence, a total of 12 pairs) are considered to investigate if the sets of observed posted prices in the first round for a pair came from the same distribution. The null hypothesis that the observations came from the same distribution cannot be rejected for any pair (at a 5% significance level). Table 1.2 reports this. This suggests that pricing behavior in all four sessions in the respective treatments did not differ from one another. To test if the presence of the guarantee in the PMG sessions affected the pricing behavior in the first round compared to the Benchmark sessions, each of the 16 possible cross-session pairs are considered. Test results indicate that the presence of a low-price guarantee influenced the pricing behavior in the first round and posted prices in the PMG sessions are statistically higher than that of the Benchmark sessions. Therefore, it can be concluded that:

Result 1: *In the absence of any learning, the presence of PMGs in the market pushes prices to higher level relative to the Benchmark case.*

The development of prices in the later rounds for the two treatments is analyzed next. AWPs and APPs for each treatment are plotted in figures 1.3 and 1.4. Both averages for all the PMG sessions lie substantially above that of the Benchmark sessions. In particular, APPs for all PMG sessions monotonically approach the monopoly price whereas AWPs fluctuate around the price 90. In contrast, both averages for the Benchmark treatment show a declining trend towards the competitive price. APPs show fairly more fluctuations than AWPs for the Benchmark treatment, but both show a gradual decrease towards the competitive price[17]. The AWPs (APPs) for each treatment summed across four sessions, indicate that participants gained experience over the rounds and gradually approached the equilibrium predictions of the respective models. However, in the Benchmark sessions, both averages did not quite converge to the Bertrand price.

Table 1.3 reports the AWPs and APPs for the four sessions in the two treatments. Both averages in all four PMG sessions are much higher than those of the Benchmark sessions. A non-parametric Mann-Whitney test based on ranks for the AWPs (APPs) observed in the two treatments was performed and the results from the tests suggest that AWPs (APPs) in the PMG treatment are statistically higher than those of the Benchmark treatment[18]. Thus, the above observations lead to the following conclusion:

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[17] In B1 and B4 however APPs show upward trend after the sixth round. A closer observation of the data give an impression that some participants might have been frustrated about very little money they made until that round and that might have induced them to bid higher prices.

[18] Formal Null and Alternative hypotheses of these tests are:

H_o: There is no difference between the AWPs (APPs) observed under the two treatments.

Result 2: *A direct comparison of AWPs and APPs for each treatment provides strong evidence that the prices associated with the PMG treatment are notably higher than those of the Benchmark treatment.*

The above result is consistent with the antitrust claim that PMGs act as a tacit collusive device and market prices in the presence of these guarantees can rise significantly higher than the competitive level.

Recall that all the sellers must adopt the guarantee to support supracompetitive prices in equilibrium. The analysis of the guarantee adoption pattern in round 1 for all four PMG sessions is important to check if the participants recognized the collusive potential of these guarantees in the absence of any learning. Out of a possible maximum of 12 adoptions in the first round, the actual numbers of adoptions are 9, 11, 10, and 10 in four sessions, respectively. Data also indicate that amongst the price-matchers in round 1, 5 in M1, 6 in M2, 6 in M3 and 7 in M4 chose a price higher than or equal to 90 in the first round. Sellers who did not adopt the guarantee in the first round, chose prices lower than 50 in most cases. This shows that a relatively high proportion (almost 83%) of participants across sessions in the very first round realized the possible gain from adopting the policy, and 60% of those chose very high prices. A one-tailed Binomial test was performed and the null hypothesis that the proportion of PMG adoptions is equal to the proportion of non-adoptions in the first round was rejected (at a 1% significance level) in favor of the alternative that more than half of the participants chose the

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H_1: AWPs (APPs) observed under the PMG treatment are stochastically higher than that of the Benchmark treatment. Both null hypotheses were rejected in favor of the alternatives at 1% level of significance.

guarantee in the first round. Thus, in round 1, PMGs appear to be a profitable and dominant choice to participants. To confirm the hypothesis that in the first round participants who adopted the guarantees chose significantly higher prices than those who did not adopt, a simple linear regression model was estimated by regressing posted prices observed in round 1 on a dummy variable that equals '1' for a PMG decision and '0' for a no-PMG decision. The estimation yielded a coefficient for the dummy (equal to +39.25) that is highly significant (at 1% significance level) and which implies that it is, indeed, the case that price-matchers chose significantly higher prices than non-price-matchers in round 1. Thus, it can be concluded that:

Result 3: *The adoption pattern of the guarantee and the prices chosen by the price-matchers in the first round overwhelmingly suggests that potential gain from choosing the matching guarantee was immediately realized by a significant number of participants.*

Note that in a given session there can be a total of 120 possible matching adoptions. Data indicate that there were 113 adoptions in M1, 105 in M2, 116 in M3, and 97 in M4. Evidently, the participants did not always adopt the guarantee. However, on an aggregate level, almost 90% (431 adoptions out of a maximum of 480) of the times PMG was adopted across the four sessions. Disaggregating by sessions, it is observed that the PMG adoption rate varies from a low of 81% in M4 to a high of 97% in M3. Also, as a session progressed, participants preferred to adopt the guarantee. Figure1.5 shows that except for M4, in other three PMG sessions participants tended to favor the policy over time. This overwhelming adoption pattern in favor of the guarantee suggests that the participants realized over time that adopting the guarantee not only safeguards them from rivals' undercutting behavior but also allows them to set higher prices and earn higher profits.

To confirm the close relation between the AWPs and the total number of PMG adoptions over rounds, linear correlation coefficients were calculated for each of the four PMG sessions. The correlation coefficients for the four sessions, each based on 10 data points, are very high (0.86, 0.26, 0.90, and 0.75) except for M2 and all are significant (at a 5% significance level), and this confirms the intuition that an increase in the PMG adoptions in a market leads to higher market prices. A least-square model was estimated, regressing average profit on PMG adoption per round, to statistically verify the positive relation between the average profit earned by participants in a round in a given PMG session and the total number of guarantees adopted in that round. The estimation produces a PMG coefficient, equal to +4.20, (significant at a 5% significance level) implying a positive relation between the two. This conjecture can also be confirmed by looking at Figure 1.7 that represents the behavior of average profit per round and the average number of matching guarantees adopted in each round across sessions. Overall, it can be concluded that:

<u>Result 4</u>*: In accordance with the theory, PMGs sustain higher market prices and lead to higher profits for participants. Guarantee adoption rates significantly increased as a session advanced, however the data on adoption pattern show that participants sometimes chose against adopting the guarantee.*

The average profits (in experimental points) for the two treatments for all four sessions are compared. The average profit per participant was 53.41, 27,67, 29.58, and 34.08 in sessions B1- B4 respectively while the same was 284.06, 299.47, 305.39, and 288.06 in sessions M1-M4. It is clear that the profits earned, on average, in the PMG treatment are much higher than those of the Benchmark treatment for all the sessions. However, the sizeable difference in average profits per participant between the two treatments is not surprising given entirely different dynamics of price competition predicted in two market models.

Finally, in the subgame where all three sellers adopt the guarantee, any symmetric (effective) price triplet is a reasonable candidate for equilibrium and hence it poses a serious coordination problem for sellers. The selection of a particular equilibrium therefore remains an empirical question. The ensuing analysis would shed light on this equilibrium selection problem.

Out of a total of 160 triopoly markets in all four PMG sessions, in 134 markets all three sellers chose to match the others' prices. Therefore, almost 84% of all the triopoly markets in PMG treatment correspond to this subgame. Disaggregating by each session, 34, 35, 36, and 29 such markets were observed in the four sessions, respectively. It would be interesting to check if the monopoly price emerged as the equilibrium price in all these markets and if the successful coordination resulted. Figure 1.8 displays this information by each session. For example, out of a total of 34 such markets in M1, in 29 markets monopoly price was chosen. Close inspection reveals that while in M4, 89% of those markets settled on monopoly price, in M2, only 64% did so. On an average, in 81% of such markets, monopoly price prevailed as the equilibrium price. In this sense, price-matchers in this subgame mostly succeeded in tacit coordination. Thus, the following picture emerges:

Result 5: *Even though, on average, most participants succeeded in solving the coordination problem, moderate to high levels of coordination failure were observed in this subgame*[19].

To summarize, while the anticompetitive predictions of the model are clearly borne out by the data, in few cases the observed behavior is not an accurate description of the theoretical prediction. For example, the adoption pattern of the guarantee closely approximates the theoretical prediction, but not entirely.

1.5 Discussion

This study started out with two clearly stated objectives. First, the major goal of this study was to examine the collusive potential of PMGs in a controlled setting. Second, a related aim was to provide empirical evidence regarding the equilibrium selection from a set of Pareto-rankable equilibria for the PMG model. As a whole, the data extend great support to the main theoretical prediction that these guarantees yield collusive behavior among sellers. This finding is in line with the theoretical stand taken by Salop (1986), Belton (1987), Doyle (1988), Chen (1995) etc. Regarding the multiplicity of equilibria, this study found considerable (but not full) evidence of coordination success for the particular subgame where all the sellers adopt the guarantee.

[19] A coordination failure in this context refers to those cases where the monopoly price did not emerge as the market outcome.

The results that emerge from this experiment have important policy implications. As mentioned above, PMGs can act as device for tacit coordination among sellers despite their pro-competitive appearance. Thus, it supports the widely held belief that these guarantees can change the pricing incentives, both of the price-matcher and of its rivals, in anticompetitive ways. Also, these guarantees lead to a dramatic reduction in rivals' incentives to cut prices. Hence, this study provides considerable evidence in favor of the argument that PMGs support anticompetitive outcome.

One question that may naturally arise is that to what extent the results from this study can be extended to more complex market conditions? To answer this, it must be noted that the research goal of this study was to delineate the effects of these types of low-price guarantees in a simple setting, which would, in turn, guide the future path of research in this area. To keep the environment simple inside the laboratory, this study started out with a very basic model that naturally lacks many of the complexities of natural markets and concluded that these guarantees are collusion enhancing devices. Therefore, the conclusion of this study cannot be more than a starting point. To meaningfully add to the results from this study, future research should advance mainly in two directions. First, it would be interesting to see if the same qualitative results in terms of prices could be obtained by allowing many possible asymmetries (e.g., cost asymmetry) between sellers. Second, Hviid and Shaffer (1999) argue that the collusive impact of PMGs will be completely undermined if buyers incur costs in terms of hassles to invoke these guarantees. Examining the collusive potential of PMGs by allowing buyer heterogeneity in terms of hassle costs would subject the collusive theory to a stringent test[20].

[20] In a recent study, Dugar and Sorensen (2005) tested the predictions of Hviid and Shaffer (1999) and found that prices return to the competitive level in the presence of positive hassle costs buyers.

CHAPTER 2: HASSLE COSTS, PRICE-MATCHING GUARANTEES AND PRICE COMPETITION: AN EXPERIMENT

2.1 Introduction

An important question in oligopoly theory is whether firms can collude to reap monopoly profits. Under current antitrust laws, overt formal coordination among firms to fix prices is prohibited. However, firms may tacitly collude, recognizing their mutual interdependence, and consequently achieve the monopoly outcome.[21] But without explicit agreements, it is usually difficult for firms mutually to agree upon a price and/or firms find it hard to resist the idea of profitable price undercuts. Nonetheless, tacit collusion among firms is a prominent possibility in the presence of some price facilitating practices widely observed in retail markets, namely price-matching guarantees (hereafter PMGs).

A PMG is a competitor-based low-price guarantee that typically states that a firm will match any lower prices offered by its rivals. Salop (1986) first argued that PMGs could facilitate tacit collusion that will support prices above marginal cost in equilibrium. The basic idea is the following: Since in the presence of PMGs all firms *automatically* match any lower price through their matching policies, Bertrand undercutting does not increase a firm's market share; rather it just leads to lower prices and profits for all firms. Hence, maintaining the collusive price is optimal for each firm, and the market price may

[21]This concept has been known as conscious parallelism, as in Posner (1976). This idea has its roots in Chamberlin (1962)'s discussion of oligopoly firms.

rise well above the marginal cost in equilibrium and may even reach the monopoly price. Thus, PMGs might sustain monopoly prices in the market without any formal agreements among firms.

Since Salop, the anti-competitive effects of PMGs have been supported in a variety of settings. In particular, Doyle (1988) analyzed the collusive argument for *n* firms and showed that collusion exists if all firms adopt the PMGs. This effect has also been shown to be invariant to whether the guarantees and prices are chosen simultaneously or sequentially in Chen (1995), and whether the products are homogeneous or differentiated in Logan and Lutter (1989). Baye and Kovenock (1994) examined a model with three types of price-related advertisements (only prices, PMGs, or price-beating advertisements) and supported the anti-competitive predictions. Edlin and Emch (1999) examined the welfare effects of these guarantees and concluded that PMGs create greater welfare losses in markets with a low ratio of fixed to marginal cost. Even textbooks on industrial organization[22] recognize this adverse effect, and PMGs have received attention from antitrust authorities and legal scholars. See Sargent (1993), and Edlin (1997)[23] for excellent discussions.

Most of these theoretical models implicitly assume that firms automatically match rivals' low price, which implies that invoking these guarantees is almost a 'no hassle'

[22]See Dixit and Nalebuff (1991) and Oster (1994).

[23]Even though the dominant view in the literature is that PMGs are anti-competitive in nature, this may not be the sole purpose behind the adoption of matching guarantees. Other purposes may include price discrimination (e.g., Belton (1987), Png and Hirschleifer (1987), Corts (1996), Chen et al. (2001), and Lin (1988)), low cost signaling (Moorthy and Winter (2005)), and entry deterrence (Arbatskaya (2001)).

task for a buyer.[24] However, Hviid and Shaffer (1999) (HS, henceforth) maintain a different view (HS, p. 490). They argue that even an arbitrarily small level of positive hassle costs, borne by all buyers to invoke these guarantees, render PMGs much less effective than the dominant view in the literature would suggest and therefore firms will no longer find it optimal to set the market price above marginal cost in the presence of PMGs.

The central idea of HS is that, in the presence of strictly positive hassle costs, all the buyers strictly prefer to buy from the firm with the lowest price (whether or not its rivals are committed to a PMG). Each firm then has an incentive to undercut its competitors. This undercutting process will continue until the market price reaches the competitive level. The result holds true for an arbitrarily positive level of hassle costs borne by all buyers.[25] This argument is a reasonable theoretical extension in light of two important observations: (1) The prior theoretical literature establishes the collusive outcome by assuming that buyers invoke these guarantees *costlessly*; and (2) in contrast, buyers have to expend costs in terms of price searches, filling in forms, and transportation in order to meet the restrictions attached to these PMGs. Collectively, these costs are referred to as 'hassle costs' in HS. Thus, a strictly positive level of hassle costs may have a dramatic impact on the competitiveness of a market.

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[24]See Arbatskaya et al. (2004) for the incidence and variety of this type of low price guarantees.

[25]HS establish the result for symmetric and asymmetric markets by dealing with the magnitude of the hassle costs strictly bounded away from zero. However, if the market is symmetric, the magnitude of hassle cost does not matter so long as all buyers face strictly positive hassle costs.

In this paper, we experimentally investigate whether the collusion-facilitating nature of PMGs survives the reasonable modification of hassle costs. To achieve this, we develop four stylized one-shot price competition models that have testable hypotheses. The first model directly captures the implications of HS in which all buyers bear positive hassle costs. The last three models deal with the theoretical analysis of hassle costs in symmetric markets by introducing heterogeneity on the buyer side, previously unexplored in the literature. In particular, we segment buyers into two groups: positive and zero hassle cost buyers. We then alter the fraction of such buyers for each of the three models, derive testable predictions, and test all four models in the laboratory.[26] While theory predicts that the competitive price should emerge in equilibrium in all four models, any significant difference in prices among them would provide evidence against this sharp prediction.

The existing empirical evidence (using data from naturally occurring markets) regarding the impact of PMGs on prices is inconclusive. Some of the studies have found evidence in support of the collusive theory (e.g., Hess and Gerstner (1991)) while other studies (e.g., Manez (1999), Arbatskaya et al. (1999)) have concluded otherwise. To date, there are three studies that have examined the impact of PMGs on prices using experimental techniques. Fatas and Manez (2004) used experimental methods to study the behavior of prices in the presence of PMGs in a differentiated product market and

[26]The positive hassle costs buyers also have an alternative interpretation. These buyers may be viewed as those buyers who are unaware of PMGs or who simply do not believe in PMGs. We are grateful to a referee for suggesting this point to us.

found that the PMGs lead to higher prices. Deck and Wilson (2003) examined the impact of the endogenous selection of three pricing strategies and found that the PMGs generate significantly higher prices relative to other pricing algorithms. The previous chapter also tested the collusive prediction of these guarantees using a homogeneous product model and found a reasonably close agreement between the predictions of the collusive theory and the observed behavior.

While experimental studies have validated the dominant view (i.e., collusion) inside the laboratory, there is no empirical work to date that evaluates the effectiveness of hassle costs as a vehicle of competition. One possible reason for this paucity of empirical work may be the difficulty of quantifying hassle cost.[27] This study designs an experiment that attempts to fill this gap. Despite the wide chasm between the laboratory and natural markets, the experiment in this study is designed to address issues of economic policy that have major antitrust implications.

Section 2 discusses and lays out the theoretical predictions for each of the four stylized market models. Section 3 describes the experimental design. Section 4 describes

[27]There are also other reasons for not using the data from naturally occurring markets to test the implications of PMGs. First, there is a counterfactual problem that one cannot observe what prices would have prevailed if no seller had a PMG. This problem seriously restricts the scope of a direct comparison of behavior of prices with and without the guarantees in the market. Second, there is an informational problem in the absence of reliable cost and demand side information that makes it difficult to estimate the deviation of prices from the competitive price level in the presence of PMGs.

estimation procedures used in the data analysis. Section 5 reports results, and Section 6 concludes.

2.2 Models

The foremost goal of this section is to develop four price competition models that build on the PMG model of the previous chapter. For pedagogical purposes, we refer to the PMG model as HC0 model. The HC0 model used a market context (with no buyer incurring positive hassle costs) to test the collusive impact of the PMGs, and the data obtained therein extend significant support to the theoretical prediction. Thus, the HC0 model and its results will serve as a foundation for the present study. Each of the four variations of the HC0 model in this study (which we shall call augmented models) consists of a different and positive fraction of buyers in the market that incur hassle costs to enact guarantees. The theoretical analyses and the subsequent experimental design will evolve around these four augmented versions of the HC0 model that follows next.

2.2.1 HC0 Model

- There are three identical firms.[28]

[28] Although the theoretical prediction remains unaltered whether there are only two firms or more than two firms in the market, the reason for analyzing the case with three firms in this study is the following: This study takes HC0 model of the previous chapter as the benchmark case in which a 3-firm market context was chosen to give the collusive theory of HC0 the best shot inside the laboratory, and the results from the present chapter could be adequately compared to the results from the previous chapter.

- The HC0 model has *two* stages.[29] In stage one, each of the three firms simultaneously decides whether or not to adopt the PMG (costlessly). A firm choosing a PMG will subsequently have to match the lowest price chosen in the market.

- In stage two, after observing the first stage decisions by all firms, each firm simultaneously chooses a price in the set {1, 2, 3,…, 100}.

- There are 6 *zero* hassle cost buyers in the HC0 model.[30] These buyers always buy from the firm that charges the lowest price directly or indirectly (i.e., by choosing to match the lowest price) in the market. In the event of price ties (through direct or indirect policies), buyers evenly split themselves among the three firms.

- Each buyer *must* buy exactly one unit of the product, and therefore there is a total market demand for 6 units of the product.

- Buyers are perfectly informed about all the prices and policies prevailing in the market.

- The total profit in the market in the HC0 model will be equal to the lowest price chosen times the number of buyers in the market. This profit will be shared equally among the firms that chose the lowest price directly or indirectly by adopting the PMG. The firm(s) that did not choose the lowest price and did not adopt the PMG will earn zero profit.

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[29] This set-up corresponds to an environment where PMGs are chosen less frequently than prices. This is often the case in actual markets. See Logan and Lutter (1989) for a similar argument.

[30] The justification behind the choice of parameters concerning the number of buyers and units and the proportion of positive hassle costs buyers in the market will be discussed later in this section.

The HC0 model captures the assumptions of a simple Bertrand price competition model with an additional feature, the PMG option.[31] The HC0 model has eight subgames.[32] Note that once a firm chooses to match its rivals' lower prices, then that firm's *effective* selling price *may* differ from its posted price through its offer to match. In order to develop an intuition for this, it may be helpful to consider an example:

Example 1: Suppose that in stage one, firms 1, 2, and 3 choose to match and in stage two, the prices are 36, 72, and 91 for the three firms respectively. Since all the firms in this subgame adopt the policy, the *effective* selling price (or the lowest price in the market) is 36, and that is the market price for all three firms. Total profit in the market is (36 X 6) or 216 units, and each firm earns a profit of 72 units. Clearly, firms 2 and 3 sell at an effective selling price that is different from their posted prices.

The HC0 model has multiple subgame perfect Nash equilibria. In particular, the most interesting case is where all three firms adopt the PMGs. *Any symmetric effective selling price vector between (1, 1, 1) and (100, 100, 100) is sustainable in equilibrium in this subgame.* All the equilibria except (1, 1, 1) involve successful collusion in this subgame. There are other subgames where incentives for collusion are either almost or completely absent. In the three subgames in which two firms adopt the guarantee, there

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[31] For a discussion on the discrete version of the HC0 model, see Dufwenberg and Gneezy (2003).

[32] There are three subgames in which only one seller adopts the guarantee, three subgames in which two sellers adopt the guarantee, and two subgames in which either no seller or all three sellers adopt the guarantee.

are three Nash equilibria in pure strategies: (1, 1, 1), (2, 2, 2), and (3, 3, 3).[33] The equilibria in all the remaining four subgames (i.e., three subgames in which only one firm adopts the guarantee and the subgame in which no firm adopts the guarantee) involve no collusion --i.e., (1, 1, 1).

2.2.2 Augmented Models

Next, we discuss four modifications of the HC0 model. Each modification corresponds to one experimental treatment in our study. We shall refer to these four models, as well as to the corresponding experimental treatments, by the number of positive hassle cost buyers in the market. Each of these models has the same structure as the HC0 model except for the alterations on the buyer side.

The buying policy of the positive hassle cost buyers differs from that of the zero hassle cost buyers. Positive hassle cost buyers strictly prefer to buy from the firm with the lowest price chosen *directly,* whether or not its rivals are committed to PMGs. Accordingly, the computation of the profit for each firm changes. A firm that *directly* chooses the strictly lowest price in the market will always serve all the positive hassle cost buyers, and will also serve a share of zero hassle cost buyers. This share will depend on the price-matching decisions of the firm's rivals. When a firm chooses a direct lowest

[33]Consider the triplet (4,4,4). Each firm sells to 2 buyers (1/3 of the market) and earns a profit of \$8. If one of the two price matching firms were to lower its price to 3, it would now split the market only with the other price matching firm, capturing half of the 6 units in the market. Profits would then be \$9 for each seller; thus the firm has an incentive to deviate from the triplet (4,4,4) or any triplet of higher prices, as the incentive to deviate is strictly increasing in price.

price that is tied with other firms, the positive hassle cost buyers will be split among the firms choosing that price. A firm that did not choose the lowest price directly, but adopted the guarantee, only serves a share of zero hassle cost buyers -- again, the share depending on the price-matching decisions of others. Firms that did not adopt the guarantee and did not charge the lowest price in the market receives zero profit. At this point, in order to enhance the feel for the payoffs, it may be helpful to consider another example:

Example 2: Suppose that there are four positive hassle cost buyers and two zero hassle cost buyers. In stage 1, firms 1 and 2 adopt the guarantee, and firm 3 does not. In stage 2, firms 1, 2, and 3 choose prices of 33, 59, and 71, respectively. Note that firm 1 chooses the lowest price *directly* in the market. Therefore, firm 1 will make sales to all four high hassle cost buyers and share the profit from 2 zero hassle cost buyers with firm 2. The payoff for firm 1 will be [(33 X 4) + (33 X 2)/ 2] = 165. The payoff for firm 2 is [(33 X 2)/ 2] = 33, and the payoff for firm 3 is 0 since it did not adopt the guarantee and charged the highest price in the market. Note that this particular strategy of play is not an equilibrium strategy profile. Four augmented models are introduced next.

2.2.3 All Positive Hassle Costs Model (HC6)

Model HC6 works exactly like the HC0 model, except that all buyers incur positive costs to enforce the guarantee. Relative to the HC0 model, model HC6 stands out as an extreme case. In this case, all eight subgames produce the same symmetric price vector, namely (1, 1, 1), in equilibrium. This result stands in stark contrast to the results from the HC0 model where a range of collusive outcomes is plausible. Thus, the theoretical prediction undermines any incentive for collusion. In equilibrium, each firm shares the total market demand equally and earns a profit of 2 units. Note that adopting the guarantee in this market structure is either a redundant strategy (for all firms in the case of price ties or for the firm with the direct lowest price) or an 'ineffective' strategy (for the firm(s) that do not have the direct lowest price).

2.2.4 Two-Thirds Positive Hassle Costs Model (HC4)

In this model two-thirds of the six buyers (i.e., four buyers) have positive hassle costs, while the rest incur zero hassle costs. In this case, again, the symmetric price vector (1, 1, 1) is the unique equilibrium for all eight subgames. In equilibrium, each seller shares the total market demand equally and makes a profit of 2 units, and, again, the competitive market outcome emerges. In this model, unlike model HC6, PMGs may help a firm if it did not choose the lowest price directly in the market. The firm will lose sales from the positive hassle cost buyers, but it will still be able to make sales to zero hassle cost buyers through its offer to match. Thus, in this case, the PMG will act as *insurance* for a firm that directly chooses a higher price than its rivals, thus enacting a PMG is a weakly dominant strategy.

2.2.5 One-Third Positive Hassle Costs Model (HC2)

This model has one-third of the six buyers (i.e., two buyers) incurring positive hassle costs, while the rest incur zero hassle costs. In this case, all the subgames produce the price vector (1, 1, 1) in equilibrium, except in the subgame in which all three firms adopt the guarantee. In that case, there are two Nash equilibria, namely, (1, 1, 1) and (2, 2, 2). Again, the additional equilibrium is the result of the discretization of the strategy space. Firms have the same incentive to adopt the guarantee as in model HC4.

2.2.6 One-Sixth Positive Hassle Costs Model (HC1)

In our final model, one-sixth of the six buyers (i.e., one buyer) incur positive hassle costs, and the rest incur zero hassle costs. Again, all the subgames predict the price vector (1, 1, 1) in equilibrium, with the exception of the subgame where the entire market adopts the guarantee. As in the previous model, (1, 1, 1) and (2, 2, 2) are sustainable. The price triplets (3,3,3) and (4,4,4) also become sustainable price equilibria for this model, and the firms' incentives to adopt the guarantee remain the same.

All the preceding models theoretically establish that PMGs have no power to generate anti-competitive outcomes, which stands in stark contrast to the collusive prediction.[34] The inclusion of the positive hassle cost buyers in the market eliminates the price-facilitating tendency among firms, in contrast to the HC0 model, in which all buyers have zero hassle costs and the monopoly outcome may potentially arise.

At this point, some comments are in order to explain our choices of the fractions of two types of buyers. The percentages of positive hassle costs buyers are 100%, 66.7%, 33.3% and 16.7%, respectively in the order that the four models are presented above. Since the primary goal of this study is to determine the effect of varying the number of positive hassle costs buyers on market prices, we gradually decrease the fraction of positive hassle costs buyers, starting from the market structure laid out in HS. While a

[34]The last two models can support an equilibrium of (2, 2, 2) and (2,2,2), (3,3,3) and (4,4,4), respectively. However this equilibrium has very little economic significance in terms of firms' profits.

finding of no significant difference in market prices among these models would corroborate the precise theoretical prediction, substantial price differences would provide evidence that competition in the market crucially hinges on the share of positive hassle costs buyers. Therefore, the main advantage of our chosen parameterization is that it may produce a large enough effect to be at least in principle observable in data.

Table 2.1 gives an overview and key features of the four stylized models from this study, as well as the HC0 model and the Bertrand model from the previous chapter. The Bertrand model is identical to the HC0 model, except for the absence of any chance to adopt a PMG.

2.3 Experimental Procedure

Experimental sessions were conducted at the University of Arizona's Economic Science Laboratory. Table 2.2 describes the sequence of treatments for each session conducted for the current and this chapter. Our original experimental design consisted of first six sessions, where all participants played in treatments HC6, HC4, and HC2 in each session. One session was run for each of the permutations of order for these three treatments. Later, we conducted three additional sessions for treatment HC1 with a between-participant design.[35] In sessions 1-9, there were six participants randomly matched into two triopoly markets each round, while the buyer side was automated.[36]

[35] I am grateful to an anonymous referee for the suggestion to study the last market model (i.e., HC1).

[36]Although a random matching protocol was adopted in the experiment, given the small number of participants in each session, a repeated game effect is bound to take place.

Participants were University of Arizona undergraduate students and were paid a show-up fee of $5 plus any additional money they earned during a session. None of the sessions took more than one hour to complete, including instructions and payments to the participants. Each participant was randomly assigned a registration number at the beginning of the session. These registration numbers were used to pay participants after the experiment. No form of communication was allowed between participants during a session. All sessions were conducted using experimental points and at the conclusion of each session, participants' earnings in experimental points were converted into U.S. dollars using the experimental exchange rate.[37] The experimental sessions 1-9 were computerized whereas the sessions under the previous chapter were conducted with paper and pencil. Each session started after all the participants finished studying the instructions. All questions were answered before each session began to ensure that everyone understood the instructions[38].

Once all participants had finished the first stage, the computer terminal in front of them displayed how many firms in the market had decided to adopt the PMG. During the second stage, participants were asked to enter prices from the set {1, 2, … , 100}. After all participants finished the two stages in the round, computers presented each participant with summary information about profits, price-matching decisions, and price choices for each of the firms in both groups. Screen shots for the two stages and participant instructions are available upon request from the author.

[37] Again we maintain the same argument as in chapter 1 regarding the exchange rate used in the current chapter.

[38] Instructions can be found in the appendix A2.

2.4 Econometric Identification of Treatment Effects

2.4.1 Data

As mentioned above, the first six sessions were conducted using a within-participant design, with participants playing three different treatments within a given session. However, participants in these sessions were not informed that they would participate in more than one treatment. Each treatment was played for 10 rounds, where the six subjects in the session were randomly assigned to one of the two triopoly markets in each round. Thus, we obtain a total of 20 observations (two market prices in each round, ten rounds of play) for treatments HC6, HC4, and HC2, from each of the first six sessions. Similarly, for treatment HC1, we have a total of 60 observations from sessions 7, 8 and 9.

In addition to the data from the four treatments, we also include data from the last chapter in our analysis. These data are from the treatments corresponding to the HC0 model and a Bertrand price competition model (described above). These two treatments were conducted for four 10 round sessions, each with four randomly matched triopoly markets in each round, producing 160 observations of market price for each treatment. Despite these differences, the similarity between the current and the last chapter in terms of the underlying market models and the experimental design renders it a benchmark case for comparison purposes.

2.4.2 Identification

The primary goal of our econometric analysis is to identify the four treatment effects. Apart from the treatment effects, other factors may determine market prices as well in our design. In particular, experience may influence play by participants. We control for the experience in two different ways. First, we control for the effect of the *order* of a treatment on the dynamics of play. In particular, we account for whether a specific treatment is played 1st (in rounds 1-10), 2nd (in rounds 11-20), or 3rd (in rounds 21-30) in order in the first six sessions. Second, we also account for the amount of experience gained by participants within a given treatment in the first six sessions, which we shall call the *round* effect. Employing appropriate dummy variables controls for these effects.

The inclusion of these additional variables, though, will not affect the point estimates of the treatments effects for HC2, HC4, and HC6. The treatments run with a within participant design, however will produce more efficient estimates. Under a very similar experimental design, Oaxaca and Dickinson (2005) show that the treatment dummy variables are independent of the experience dummy variables. The invariance of the estimated treatment effects to the inclusion of these dummy variables follows from this independence property.

One additional matter that we explore concerns the PMG adoption rate and market price. Preliminary data analysis indicates that the number of positive hassle cost buyers directly affects market prices and also indirectly affects market prices via its impact on PMG adoption rates. Accordingly, we develop a simple econometric model to separately identify these two effects. In our econometric model, the direct effect is the impact of a specific treatment on market price, holding the number of price-matchers in the market constant. In the first stage of the game, a treatment may influence the PMG decisions of sellers, which, in turn, may have an impact on market price in the second stage. This is the indirect effect.[39]

Altogether, we estimate four econometric models to address the above points. Three of these models identify the treatment effects, while the fourth model estimates the effects of different treatments on the number of price matchers in a market. In the first model (see equation 1), we regress market price on a constant term and dummy variables for the treatments. Our second model (see equation 2) adds dummy variables to the first model to control for the experience or learning effects. X represents a vector of covariates, such as experience, and the β vector in equation (2) identifies both the direct and indirect effects. By holding experience constant, model (2) allows us to compare the treatment effects between the within-participants sessions and the between-participants sessions.

(1) $$P_i = \beta * Treatment_i + \varepsilon_i$$
(2) $$P_i = \beta * Treatment_i + \alpha * X_i + u_i$$
(3) $$P_i = \gamma * Treatment_i + \phi * PM_i + \alpha * X_i + \xi_i$$
(4) $$PM_i = \delta * Treatment_i + \eta * X_i + \xi_i$$
(5) $$\beta = \gamma + \phi * \delta$$

The third model (equation 3), controls for the number of sellers adopting the PMG; the ϕ parameter identifies the effect of the number of PMG adopters on market price. The δ vector in equation (4) identifies the effect of each treatment on the number

[39] I am grateful to two anonymous referees for suggesting this analysis.

of price matchers.[40] We estimate the first three equations by OLS and equation (4) with a Negative Binomial regression. Finally, equation (5) shows how the estimates from the above equations allow us to decompose the treatment effect into direct and indirect effects. Again, γ captures the direct effect, while the product of δ and ϕ captures the indirect effect.[41]

Figure 2.1 reveals a clear pattern of average market price across treatments. Treatments Bertrand and HC6, where PMG is either unavailable or completely ineffective, produce the most competitive outcomes. As the number of positive hassle costs buyers in the market is reduced, average market price approaches the monopoly price in a monotonic fashion. Table 2.3 presents further evidence on these observations. In the first round, without any element of experience, average market prices in all treatments do not corroborate the theoretical prediction. Treatment HC0 produces the most collusive outcomes, whereas treatment HC6 produces the most competitive outcomes. The percentage of price-matchers in the market is decreasing with the number of positive hassle costs buyers. These effects are more pronounced for the terminal round as the average market prices under treatments HC0 and HC6 closely approach the corresponding equilibrium predictions. The average market prices for other treatments lie

[40]The last two equations need not be thought of as a system of equations; equation (4) simply expresses the relationship between two exogenous variables in equation (3). Therefore, equations (3) and (4) produce consistent estimates of the delta and gamma vectors when estimated separately.

[41]Due to the non-linear nature of equation (4), equation (5) may not hold true for a finite sample; however, the identity will always be satisfied if equation (4) is estimated by OLS.

far off from the equilibrium predictions. The similar trend is observed while focusing on data for all rounds.

Table 2.4 reports the results from all four econometric models. The treatment HC6 is the reference group for the analysis. For the first three models, the coefficients on all treatment dummy variables can then be interpreted as the effect of the corresponding treatment on market price, relative to the prices observed in treatment HC6. In model 1, we observe that, in accordance with expectation, treatment HC0 produces the highest price, followed by HC1, HC2, and HC4. A series of t-tests, not reported here, confirm that the differences between all coefficients are statistically significant at the 5% significance level.

In model 1, treatments Bertrand and HC6 produce notably lower market prices than the other treatments, and the market price in Bertrand (10.83) is significantly higher than in HC6 (5.29). Recall that the Bertrand sessions were conducted with a between-participants design, while the HC6 sessions were run with a within-participants design.

Therefore, participants in HC6 sessions were, on average, more experienced than participants in Bertrand sessions. Once these learning effects are accounted for in model 2, we find that Bertrand and HC6 produce market prices that are not significantly different from one another. Again, in model 2, as the number of positive hassle costs buyers decreases, market prices increase significantly. Also note that for treatments HC4 and HC2, point estimates remain the same. However, they are marginally more efficient than in model 1.

The results for the order effects indicate that, when playing multiple treatments in a session, prices tend to decrease between treatments. The coefficients for the round dummy variables indicate that, within a given treatment, market prices tend to be lower in the first round and thereafter show an upward trend. They do not, however, vary much after that.

While models 1 and 2 reveal that the intensity of collusion varies inversely with the proportion of hassle costs buyers present in a market, models 3 and 4 allow us to break down the treatment effect into direct and indirect effects discussed in the last section. Model 3 shows that the number of price matchers in a market is positively and significantly correlated with the market price. In other words, *ceteris paribus*, as the number of price-matchers in a market increases, the market outcome tends to be more collusive in nature. In the final model, we determine the process that governs the number of price-matchers in a market. Column 4 reveals that as the number of positive hassle costs buyers is reduced gradually across treatments, the PMG adoption rate increases accordingly, and t-tests (not reported here) confirm that each of these increases is statistically significant at the 5% level.

Table 2.5 reports the breakdown of the treatment effect into two components. The first row of Table 5 shows the estimated effect of an additional price-matcher on market price. The next row presents, across the four treatments, the estimated effect of each treatment upon the number of participants adopting the PMG in the first stage. The product of the numbers on the first two rows, reported in the third row, yields the indirect effect. The direct effects are the estimates of the corresponding treatment effects from model 3, in which we hold the number of price-matchers constant. Note that the share of the direct effect is the greatest in the HC0 treatment, and decreases as the number of positive hassle costs buyers increases. This implies that for markets with a moderate number of positive hassle costs buyers, PMG adoption patterns contribute approximately half of the observed price increases.

2.5 Conclusion

Despite the dominant view in the literature that PMGs help firms tacitly collude, HS maintain that the literature implicitly assumes that firms automatically match prices and therefore underestimate the role of hassle costs as an instrument that can restore the competitive outcome. Casual observations suggest that hassle costs typically exist in retail markets, and HS argue that markets with all *positive* hassle cost buyers may render the collusion enhancing nature of PMGs moot. Against this backdrop, we test the precise theoretical prediction of HS along with the predictions from the three stylized market models that permit varying proportions of *positive* and *zero* hassle cost buyers in the market. Additionally, we contrast the results of the current study with that of the previous chapter, which confirms the predicted collusive effects of PMGs in the absence of hassle costs. While theory predicts that each of the four models should produce competitive prices in equilibrium, we find that prices differ across our market models.

First, market prices increase significantly as the number of positive hassle cost buyers is reduced across treatments. The market with all (HC6) positive hassle costs buyers generates the most competitive outcome, thereby confirming the prediction of HS.

These results stand in sharp contrast to the HC0 model in the last chapter, where much higher prices were obtained in the absence of positive hassle cost buyers.

Second, the PMG adoption pattern varies inversely with the number of positive hassle costs buyers. Additionally, a significant percentage (24%) of participants decided to adopt PMG in HC6, in which adopting PMG is redundant or ineffectual. It is possible that participants chose to match prices simply because this was a new option, and they assumed that it must be what they are supposed to do.[42]

Our findings suggest that the presence of buyers who incur positive hassle costs to invoke PMGs acts as a market-disciplining device. Since buyers who are unaware of the existence of PMGs will behave in the same way as the positive hassle buyers, an increased share of such uninformed buyers will have a favorable impact on the competitiveness of a market. As a result, policy makers may need to take note of the potentially anti-competitive effects of the PMG related *advertisements*.

In view of the findings of this and the last chapter, it is natural to ask why firms adopt guarantees that impose costs on buyers, since these costs may have the effect of lowering prices. This suggests that PMGs may be adopted for reasons other than tacit collusion. One reason may be that firms adopt these guarantees and use hassle costs as a device to screen buyers in terms of their willingness to pay the lowest price for the product. This suggests that PMGs may be adopted for price discrimination purposes.

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[42]This possibility of experimenter expectation effect has been well recognized in the experimental economics literature. For a discussion on this, see Rosenthal (1966).

One question that may naturally arise is to what extent the results from this study can be extended to more complex market conditions? To answer this, it must be noted that the research goal of this study was to delineate the effects of hassle costs associated with PMGs in a simple setting, which would, in turn, guide the future path of research in this area. Therefore, the conclusion of this study cannot be more than a starting point. To check the robustness of the results from this study, future research should examine if the same qualitative results in terms of prices could be obtained by allowing some asymmetries (e.g., cost asymmetry) between sellers. This remains the task for future research.

CHAPTER 3: CHOICE OF ROUTES IN CONGESTED TRAFFIC NETWORKS: EXPERIMENTAL TESTS OF THE BRAESS PARADOX

3.1 Introduction

Transportation and communication networks are among the best examples of frequently used physical networks in which vertices (nodes) correspond to locations in space and edges (links) to connections between them. They provide the infrastructure for humans to conduct much of their social and economic activities. It would seem rather natural to believe that increasing the capacity of an existing network or adding one or more new edges to traffic or communication networks would definitely not worsen and most likely improve efficiency. Braess (1968) has shattered this deeply entrenched belief by demonstrating that, paradoxically, adding a link that connects two alternative routes in parallel running between a common origin and common destination may *raise* the total travel cost of *all* the network users. This phenomenon, subsequently labeled the *Braess Paradox* (BP), has stimulated a rapidly growing body of research in transportation science, computer science, and applied probability. Researchers have attempted to classify networks in which the addition of a single link could degrade network performance (Frank, 1981; Steinberg & Zangwill, 1983), discovered new paradoxes (Arnott, De Palma, & Lindsey, 1993; Cohen & Kelly, 1990; Dafermos & Nagurney, 1984; Fisk, 1979; Hagstrom & Abrams, 2001; Pas & Principio, 1997; Smith, 1978; Steinberg & Stone, 1988), proved that detecting the BP is algorithmically hard (Roughgarden, 2001), and quantified the degree of degradation in network performance due to unregulated traffic (Kousoupias & Papadimitriu, 1999; Roughgarden & Tardos, 2002).

Steinberg and Zangwill concluded an early analysis of the BP with the claim that "under reasonable assumptions, Braess' Paradox is not a curious anomaly but in fact might occur quite frequently (1983, p. 317)." However, no direct empirical evidence has been forthcoming to buttress this claim. In a postscript to his exposition of the BP more than 35 years ago, Murchland (1970) remarked that Knödel had noted that major road investments in the center of the city of Stuttgart had failed to yield the benefits expected. This briefly mentioned case is the only example that has subsequently been cited by Kelly (1991), Roughgarden and Tardos (2002), and others. The New York Times also hinted at the counterintuitive consequences of road closures in an article with a provocative title that appeared on Christmas Day, 1990: "What if they closed 42nd street and nobody noticed?"

This sketchy and mostly anecdotal evidence is clearly insufficient to counter arguments about the importance and relevance of the BP to real transportation and communication problems. A claim can be made that the BP is no more than a theoretical curiosity, that it is too simple to model real-life traffic and communication networks, and that examples closer to the complexity of real life would prevent those kinds of paradoxes from being realized. Another argument is directed not so much against the realism of the model but against the assumed behavior of the network users. In formulating the BP, network users are viewed as independent "selfish" agents participating in a noncooperative game, where each agent wishes to choose a path from a common origin to a common destination that minimizes her travel cost. The paradox is based on an equilibrium analysis of a weighted traffic network both before and after one or more edges are added to it. But real network users, it may be claimed, may quickly learn (possibly by repeated interaction) to avoid traversing the new edges—and thereby depart from equilibrium play—in an attempt to escape the adverse effects of the BP.

As empirical evidence about the occurrence of the BP is very difficult to come by, the approach that we pursue in the present study is to simulate simple traffic networks that are susceptible to the BP in the laboratory, have subjects choose routes in these simulated traffic networks before and after one or more edges are added to them for payoff contingent on performance, and find out whether systematic and replicable patterns of behavior emerge. If they do, it is of significant interest to determine whether they support the argument that the BP is of marginal value and its force, if at all evident, diminishes with experience or, alternatively, that they support the equilibrium analysis that gives rise to the BP.

Rooted in the methodology of experimental economics, this approach is not common in transportation research. We are familiar with only four experimental studies of the effects of traffic congestion. The first is an experiment by Schneider and Weimann (2004) that was designed to test a simple model of bottleneck congestion on a single route in a rush-hour situation. The model was originally proposed by Arnott, De Palma, and Lindsey (1990, 1993). The second is an experimental study by Gabuthy, Neveu, and Boement (2004), who generalized the analysis of Arnott et al. (1990) to traffic networks with a single origin and single destination connected by two routes. The experimental evidence is mixed; Schneider and Weimann report evidence in support of equilibrium departure time in their first but not second experiment, whereas Gabuthy et al. report no support. The third study is due to Selten et al. (2004), who conducted laboratory experiments of a day-by-day route choice game with two parallel roads but no crossroad. Unlike the two studies by Gabuthy et al. and Schneider et al., the experiment by Selten et al. is not concerned with endogenous departure time but with route choice. They report aggregate road choices that are accounted for quite well by the Nash equilibrium predictions and large fluctuations around the mean choice frequencies that do not seem to diminish with experience. In a fourth study, Helbing (2004) repeated these experiments with more iterations, and further tested additional experimental conditions in an attempt to better understand the reasons for the fairly large fluctuations around the mean choice frequencies. None of these studies is concerned with the counterintuitive implication of the BP that is the focus of the present study.

Section 2 introduces terminology and illustrates the BP in a network with the simplest possible form (called the *Minimal Critical Network* by Penchina, 1997). Section 3 reports the results of an experiment designed to examine route choice in the iterated Minimal Critical Network game. Section 4 concludes.

3.2 The Braess Paradox

3.2.1 Notation and Terminology

We consider road networks with a common origin O and common destination D that are modeled as a directed graph G=(V, E) with vertex (node) set V, edge (link) set E, and a set $K \subseteq V \times V$ of origin-destination (OD) pairs. We consider a finite, commonly known, and relatively small number of users, n, in contrast to the more common case discussed in the transportation literature that assumes infinitely many users. The traffic in the road network is described by the number f_{ij} of cars (users) moving along the edge (i,j) from vertex i to vertex j. The cost for each user of traversing from i to j along the link (i,j) when the flow on this link is f_{ij} is denoted by $c_{ij}(f_{ij})$. Travel costs are typically measured by time spent in travel or by gasoline consumed. It is assumed that the cost of traveling on edge (i,j) at a given level of traffic is the same for all the users traversing this edge. Total travel cost is the sum of the edge costs over all edges in the travel route (also called *path*) from O to D.

Each of the n users is assumed to independently seek a path that minimizes her total travel cost. In equilibrium, the n users are distributed over one or more routes so that a unilateral change of path by any one user does not decrease the total travel cost for that user, given that all other n-1 users do not change their routes. In both experiments, we consider road networks with linear costs, where for each edge $(i,j)\in E$, $c_{ij}=a_{ij}f_{ij}+b_{ij}$ for some a_{ij}, $b_{ij}\geq 0$. The *fixed* component b_{ij} can be interpreted as the minimum time to traverse edge (i,j) with no traffic, whereas the *variable* component a_{ij} corresponds to the effect of congestion. We chose affine cost functions because they are most easily explained to the subjects in network experiments. Moreover, the BP was originally stated in terms of affine cost functions (Braess, 1968; Murchland, 1970), and almost all subsequent papers have focused on this case.

The original game (hereafter called the basic game) presented by Braess included a simple network with four vertices, four edges, and an anti-symmetric (Penchina, 1997) arrangement of the edges (Fig. 3.1a). One path consisted of an edge with a high fixed cost and low congestion cost starting at the origin followed by an edge with no fixed cost and high congestion cost. On the second path, the edges had identical costs to those of the first, but were arranged in a reverse order. In the augmented game (Fig. 3.1b), these edges were connected by a transversal edge (crossroad, bridge) of low fixed cost and low congestion cost connecting the end of the edge with no fixed cost in one path to the beginning of the edge with no fixed cost in the other path (edge (*A*,*B*) in Fig. 3.1b).

3.2.2 Minimal Critical Network

The essence of the BP comes from six qualitative properties (Penchina, 1997). The first three are necessary and sufficient for the BP to occur.

The network *G* must have both fixed and variable user costs.

The two paths in the basic network must have an opposite order of appearance of the edges dominated by fixed vs. variable costs.

The fixed cost on the bridge in the augmented network must be smaller than the difference in fixed costs between the edges dominated by fixed costs and those dominated by variable costs.

Three additional properties were proposed by Penchina to simplify the analysis:

Zero user cost on the bridge.

The two congested edges have identical linear variable user cost functions and zero fixed cost.

The two uncongested edges have identical fixed user cost functions and zero variable cost.

Networks satisfying these properties are called *Minimal Critical Networks* (Penchina, 1997). The one we study experimentally is Section 3 (Figs. 3.1a and 3.1b) has the cost functions: c_{OA}=10f_{OA}, c_{BD}=10f_{BD}, c_{AD}=c_{OB}=210, and c_{AB}=0. This cost structure satisfies all six properties above.

To illustrate the BP, assume the cost structure in Fig. 3.1 with *n*=18. Consider first the basic network in Fig. 3.1a (Game A). There are $\frac{18!}{9!9!} = 48{,}620$ pure-strategy equilibria with 9 users traversing route (*O*-*A*-*D*) and 9 others traversing route (*O*-*B*-*D*). The cost for each user is 210+10×9=300. There is an additional symmetric mixed-strategy equilibrium with an associated travel cost of 305 where each route is chosen with equal probability. The augmented network in Fig. 3.1b (Game B) has a unique pure-strategy equilibrium where all users traverse the route (*O*-*A*-*B*-*D*) for total travel cost of 360. The counterintuitive feature of this example is that the improvement of the network by adding a new cost-free link causes every user to be worse off by 20 percent of the original travel cost. Commenting on this counterintuitive effect, Cohen writes, “Adam Smith’s Invisible hand leads everyone astray (1988, p. 583).”

Even a stronger effect in this network is obtained with *n*=20. In equilibrium, 10 users traverse route (*O*-*A*-*D*) and 10 others route (*O*-*B*-*D*) for a total individual travel cost of 310. When the cost-free edge (*A*,*B*) is added (Fig. 3.1b), in equilibrium all 20 users traverse the route (*O*-*A*-*B*-*D*) for total travel cost of 400, an increase of approximately 29 percent. Assuming a linear cost structure and continuum of users, so that each user controls a negligible fraction of the overall traffic, Roughgarden and Tardos proved that the degradation of the network performance by the lack of central authority—dubbed the "price of anarchy" by Papadimitriu (2001)—cannot exceed 4/3. As explained below, a major feature of our experimental design considerably enhances the adverse effect of the BP by subtracting the individual travel cost from a fixed endowment that assumes the *same value* in both Games 1A and 1B.

3.2.3 A Two-route Symmetric Network with One Additional Edge

The experiment has two major purposes. The first is to test for the occurrence of the BP in the Minimal Critical Network. The coordination problem faced by the n=18 players in Game A is far from trivial as, under pure-strategy equilibrium play, they have to coordinate on one of many equilibria that are not Pareto rankable. Because equilibrium play is only likely to be reached with considerable experience, Games A and B were each iterated 40 times.

The counterintuitive feature of the BP is that the *addition* of a cost-free edge causes every user to be *worse off*. An alternative way of viewing this paradoxical result is to start with the augmented network in Game B, *delete* the cost-free edge (*A,B*), and note that, in equilibrium, all users *benefit* from the degradation of the network. Phenomenologically, network users may perceive these two alternative formulations ("framings") of the BP—one in terms of loss and the other in terms of gain--quite differently. The second purpose of the experiment is to compare these two alternative framings of the BP.

3.3 Experimental Method

3.3.1 Subjects

The subjects were 108 undergraduate and graduate students at the University of Arizona, who volunteered to participate in a computer-controlled experiment for payoff contingent on performance. Males and females participated in almost equal numbers. The subjects were divided into 6 groups (sessions) of 18 members each. Three groups participated in Condition ADD and three others in Condition DELETE (see below). A session lasted about 90 minutes. Excluding a $5 show-up bonus, the mean payoff across the six sessions was $20.63.

3.3.2 Procedure

All six sessions were conducted at a large computerized laboratory with forty terminals located in separate cubicles. Upon arrival at the laboratory, each subject was asked to draw a marked chip from a bag that determined her seating. Subjects were then handed written instructions that they read at their own pace. Questions about the procedure were answered individually by the experimenter[43].

Each session was divided into two parts. Specific instructions were handed to the subjects at the beginning of each part. The instructions for Part I displayed the traffic network in Game A and explained the procedure for choosing one of the two routes in this game. The instructions for Part II displayed the traffic network in Game B and explained the procedure for choosing one of the three routes in this game. Condition ADD was structured as follows. The subjects were first handed the instructions for Part I, and then played Game A for forty identical trials without being forewarned about Part II. After completing Part I, the same subjects were handed a new set of instructions for Part II, and then asked to play Game B for forty additional trials (a total of 80 trials for the

[43] Instructions can be found in the appendix A3.

session). Condition DELETE was the same with the exception that the order of presentation of Parts I and II was reversed.

The instructions for Part I graphically displayed the traffic network in Game A, explained the cost functions, and illustrated the computation of the travel cost for links with either variable or fixed costs. At the beginning of each trial, each subject was given an endowment of 400 travel units (points). The payoff for the trial was computed separately for each subject by subtracting her travel cost from her endowment. To choose one of the two routes—(*O*-*A*-*D*) or (*O*-*B*-*D*)—the subject had to click on the two links of this route and then press a "confirm" button. Clicking with the mouse on a link changed the link's color on the screen to indicate the subject's choice. The subject was then asked to verify her choice of route by clicking on a "Yes" button. After all the group members independently registered and subsequently verified their route choices, a new screen was displayed with the following information:

--The route chosen by the subject.

--The number of subjects choosing each of the routes.

--The subject's payoff for the trial.

The instructions for Part II displayed the network in Game B, explained the cost functions, and illustrated the computation of the travel cost for edges with either variable or fixed cost. Because Game B, unlike Game A, allows for negative externalities, these were explained in detail.

After Part II was completed, the subjects were paid their earnings in four randomly chosen trials from the forty trials in Part I and four additional trials randomly drawn from the forty trials completed in Part II. The eight payoff trials were drawn publicly by the experimenter at the end of the session. Points were accumulated across the eight payoff trials and then converted to money at the exchange rate of 25 points=$1.00. Subjects were paid their earnings individually and dismissed from the laboratory.

3.3.3 Main Features of the Design

Four major features of the design warrant brief discussion. First, no communication between subjects was possible. In accordance with the assumptions underlying the BP, the value of *n* was commonly known. Second, the experiment was conducted under full information; at the end of each trial the subjects were informed of the distribution of network users across all possible routes. This feature was introduced to facilitate learning over iterations of the stage game. Third, we opted for a within- rather than between-subject design so that the *same* subjects would experience the effect of adding (Condition ADD) or deleting (Condition DELETE) the cost-free edge (*A*,*B*). This design feature was introduced to compare the effects of the two alternative framings of the BP. Fourth, and perhaps most importantly, the same endowment of 400 travel units was given in both Games A and B. Under pure-strategy equilibrium play by all group members, this would have resulted in payoffs of 100 and 40 travel units per trial in Games A and B, respectively. One might expect—we certainly did—differences in behavior between the two experimental conditions. If the subjects in Condition ADD were to reach equilibrium in Game A, they would be expected to resist being drawn into choosing route (*O*-*A*-*B*-*D*) in Game B and thereby watch their payoff plummeting to 40 percent of their earlier earnings. In contrast, having no prior experience with Game A, subjects in Condition DELETE would be expected to converge quickly to route (*O*-*A*-*B*-*D*) in Game B and thereby increase their earnings by a factor of 2.5.

3.4 Results

There is a consensus that the group, rather than the individual subject, is the appropriate unit of analysis in interactive decision making games that are iterated in time. When the groups are as large as in our experiment, conducting 15-20 sessions for each condition is not feasible. We approach this problem in the following analyses in three different ways. First, several of the findings are so obvious that no statistical tests are required. Second, whenever possible we consider the session as the unit of analysis and use non-parametric tests to compare the two games to each other. Third, in several analyses we invoke the assumption that subjects are independent. In justification of this assumption, we notice that, clearly, if the group includes two members who interact with each other repeatedly, as in the Prisoner's Dilemma, their responses cannot be considered as independent. But if the group is very large, say with 1,000 members or more, the effect of any member on the group is negligible and, for all practical purposes may be ignored. In such cases (e.g., financial markets), the group is considered as a population and its members as independent. Whatever effect a player's decision may have on subsequent decisions of other group members, this effect diminishes rapidly in the value of n. We argue that when the group consists of 18 members and reputation effects are prohibited, the assumption of independence is not entirely unreasonable. Nevertheless, we invoke it as a first-order approximation only, and base our conclusions on the results of the statistical test together with close inspection of the aggregate statistics.

This section proceeds as follows. First, we examine session effects within each condition. Not finding significant session differences, we combine the data across sessions. Next, we test the implications of the BP on the aggregate level and compare the two conditions to each other. We complete this section by discussing sequential dependencies and individual differences.

3.4.1 Session Effects

The purpose of this section is to test for differences among the three sessions in each condition. If significant differences are found, then individual sessions have to be presented and analyzed separately. If not, then the data of the three sessions can be amalgamated. For this purpose, the following five statistics were computed for each of the six sessions. In Game A, we first counted the number of times (out of 40) that each subject chose route (*O*-*A*-*D*) (the frequency of route (*O*-*B*-*D*) is obtained by subtraction). We then computed the mean and standard deviation of route (*O*-*A*-*D*) choices across the members of each group (column 2 of Table 3.1). Since the network in Game B includes three rather than two routes, we computed the mean route choices for two of the three routes, namely, routes (*O*-*A*-*D*) and (*O*-*B*-*D*) (columns 4 and 5 of Table 3.1). Once again, the mean choice frequency for route (*O*-*A*-*B*-*D*) is obtained by subtraction from 40. Define a *switch* for some player i, if i chooses two different routes on trials t and t+1, t=1, 2, … , 39. Clearly, a subject in Game A can only switch from one route to another, whereas in Game B she can switch to any of the two other routes. Columns 3 and 6 of

Table 3.1 present the means and standard deviations of the number of switches per subject (out of 39) for Games A and B, respectively. Using a series of one-way ANOVAs, we tested the null hypothesis of equality of means across the three sessions for each of these five statistics. None of the five tests rejected the null hypothesis in Conditions ADD and DELETE.[44]

In additional analyses designed to compare the three sessions to one another in terms of the dynamics of play, we computed similar statistics for each round (rather than subject). For example, we counted the number of subjects who chose route (*O*-*A*-*D*) in each round. We then computed the mean of these frequencies for each block of 10 consecutive rounds, and compared the three sessions to one another in terms of the trend, if any, across the four blocks. Once again, no significant difference between the three sessions in either of the two conditions was detected. The mean number of subjects (out of 18) who chose route (*O*-*A*-*D*) in Game 1B in Condition ADD decreased from 2.8 in block 1 to 0.4 in block 4 in session 1, from 4.1 to 0.7 in session 2, and from 3.2 to 0.6 in session 3. The corresponding means in Condition DELETE were (2.6, 0.4), (2.6, 1.1), and (3.0, 1.1). The results for route (*O*-*B*-*D*) in Game B were practically the same. The mean number of switches per block in Game B Condition ADD decreased from 2.9 in block 1 to 0.4 in block 4 of session 1, from 3.4 to 0.4 in session 2, and from 3.2 to 0.5 in session 3. The corresponding means in Condition DELETE were (3.7, 0.5), (3.0, 0.3), and (2.3,

[44] Condition ADD: mean (O-A-D) choice in Game A: $F(2,51)=0.20$, $p=0.82$; mean (*O*-*A*-*D*) choice in Game B: $F(2,51)=1.02$, $p=0.37$; mean (O-B-D) choice in Game B: $F(2,51)=0.24$, $p=0.79$; mean number of switches in Game A: $F(2,51)=0.55$, $p=0.59$; mean number of switches in Game B: F(2,51)=0.20, p=0.82. Condition DELETE: mean (*O*-*A*-*D*) choice in Game A: $F(2,51)=0.00$, $p=1.00$; mean (*O*-*A*-*D*) choice in Game B: $F(2,51)=0.16$, $p=0.85$; mean (*O*-*B*-*D*) choice in Game B: $F(2,51)=0.51$, $p=0.60$; mean number of switches in Game A: $F(2,51)=1.29$, $p=0.29$; mean number of switches in Game B: $F(2,51)=0.53$, $p=0.59$. None of these test results is significant at the 0.05 level.

0.9). In contrast, block effects in mean choice of route (*O-A-D*) and in number of switches were minimal in Game 1A. Having failed to reject the null hypothesis of no significant differences between sessions for each of the statistics reported above, the raw data were combined across sessions.

3.4.2 Aggregate Route Choices

We proceed to test the implications of the BP. For each trial separately, we counted the number of subjects who chose route (*O-A-D*) in Game A. These frequencies were then averaged across subjects, trials, and sessions. Similar means were computed for route (*O-B-D*) in Game A and for each of the three routes in Game B. Table 3.2 presents the means and standard deviations for each condition separately. The bottom row presents the equilibrium predictions. The standard deviations (2.12) in the bottom row refer to the symmetric mixed-strategy equilibrium where each player chooses routes (*O-A-D*) and (*O-B-D*) in Game 1A with equal probability.

Three observations about Table 3.2 are in order. First, for each of the two conditions, it is evident (columns 2 and 3) that the two routes in Game A were chosen equally likely. No statistical tests are needed to support the equilibrium prediction. Second, Table 2 shows that routes (*O-A-D*) and (*O-B-D*) in Game B were jointly chosen, on average, on 18 percent of the trials. This observation is qualified below when we examine the mean route choices by trial. The third observation concerns the standard deviations reported for Game A. These seem to be inordinately close to the theoretical standard deviations under mixed-strategy equilibrium play. In fact, the symmetric mixed-strategy equilibrium accounts for the aggregate route choices in Game A almost perfectly. Once again, no statistical tests are necessary.

3.4.3 Dynamics

Turning next to the dynamics of play, Figs. 3.2 and 3.3 exhibit the mean number of subjects choosing each of the two routes in Game A. To more clearly exhibit the main trend across trials, we present the running means in steps of 5 (i.e., trials 1-5, 2-6, … , 36-40). Consistent with Table 3.2, we observe no difference in the choice of the two routes. Each of the two means hovers over 9 but is seldom equal to 9. There is no convergence to pure-strategy equilibrium. A trial-by-trial analysis of the route choices in Game A resulted in a finding that we had not anticipated. Due to the difficulty in coordination on any of the multiple pure-strategy equilibrium outcomes, we expected that once subjects reach equilibrium—with 9 subjects choosing each route—there would occur no further deviations in *all* subsequent trials. After all, unless she randomizes her route choices, why should a subject deviate from equilibrium behavior and thereby increase her travel cost from 300 to 310 (and, correspondingly, decrease her earnings for the trial by ten percent)? The results did not support our expectation. In fact, across all six sessions, we find only five runs of two consecutive (9,9) splits, and two runs of three consecutive (9,9) splits. Informal post-experimental interrogation of some of the subjects seems to indicate the following reason for such deviations. If a (9,9) split is reached on trial t, a forward looking subject might wish to deviate from route j to route k on trial t+1, incurring a ten percent decrease in earnings, because of her expectation that two or more subjects would deviate from the more heavily chosen route k to route j on trial t+2. If this happens, she may recuperate her loss by choosing route *k* also on trial *t*+2. As we show below, this forward-looking "sophisticated" strategy did not pay off.

Recall that Condition ADD presented Game A in Part I, whereas Condition DELETE presented Game A in Part II. However, neither Tables 1 and 2 nor the more detailed Figs. 3.2a and 3.2b indicate a significant difference between these two conditions. These observations are supported by the following statistical analysis. For each subject in each condition, we counted the number of times she chose route (*O*-*A*-*D*) in each of the 10-round four blocks in Game A. These frequencies were then subjected to a two-way mixed ANOVA with a between-group factor distinguishing between the two conditions and a within-group block factor. Neither of the two main effects due to condition ($F(1,106)=0.01$, $p=1.00$) and block ($F(3,318)=0.02$, $p=1.00$), nor the condition by block interaction effect ($F(3,318)=0.07$, $p=0.94$) were significant.

Turning next to Game B, the most critical finding of Experiment 1 is exhibited in Figs. 3.4 and 3.5 that display the running mean number of subjects choosing each of the three routes in Game B. Figure 3.3.a shows the results for Condition ADD and Fig. 3.3b for Condition DELETE. In both conditions, almost 2/3 of the subjects already chose the dominant route (*O*-*A*-*B*-*D*) during the first five trials. With experience in traversing the traffic network in Game 1B, the frequencies of choice of the two dominated strategies converged to zero, and the frequency of choice of route (*O*-*A*-*B*-*D*) converged to 18. Figure 3 shows that all 40 trials were required to reach convergence. Results not reported here show that a few subjects struggled to avoid choosing the "bridge" in Game B. But with no communication possible, their signals were not heeded by the other subjects as the attraction of the Pareto deficient equilibrium strategy proved too strong to resist.

Similarly to Game A, we compared the route choices in Game B between the two conditions. To do so, we counted for each subject the number of times she chose route (*O-A-B-D*) in each block of ten trials. These frequencies were then subjected to the same two-way mixed ANOVA as in Game A. In agreement with Figs. 3.3a and 3.3b, the ANOVA yielded a significant block effect ($F(3,318)=81.96$, $p<0.001$). With experience, route (*O-A-B-D*) was chosen more frequently (see Fig. 3.3). However, neither the effect of condition ($F(1,106)=0.01$, $p=0.93$) nor the block by condition interaction effect ($F(3,318)=1.40$, $p=0.24$) were significant.

Convergence to equilibrium implies that deviations from equilibrium decline over time. To formally test this implication, we define deviation from equilibrium as the mean absolute difference between the expected and observed frequency of subjects choosing each route computed across all routes. Then, for each session separately in both conditions ADD and DELETE, we computed a rank-order correlation (Kendall's tau) between the round number and the deviation score. In complete agreement with Fig. 3.2, three of the six correlations in Game A were negative, three positive, and none exceeded 0.28: -0.19, 0.27, 0.08, 0.16, -0.28, and -0.15. for sessions 1, 2, 3, 4, 5, and 6, respectively. In contrast, all the six correlations for Game B were negative, relatively high, and statistically significant: -0.62, -0.70, -0.61, -0.66, -0.63, and –0.51. These negative correlations are in complete agreement with the aggregate results depicted in Fig. 3 that display convergence to equilibrium.

3.4.4 Aggregate Payoffs

Denote by (f_j,f_k) the number of subjects choosing routes j and k in Game A, respectively. The mean payoff in Game A cannot exceed 100, and it decreases in the absolute difference $\Delta=|f_j-f_k|$. Thus, if $\Delta=0$ (a (9,9) split), then the mean payoff is 100. Mean payoffs for the (10,8), (11,7), and (12,6) splits are 98.8, 95.56, and 90, respectively. The expected payoff under mixed-strategy play is 95 and the associated standard deviation is 7.01. The mean payoff computed across all 54 subjects in sessions 1-3 of Condition ADD is 94.92. The corresponding mean for Condition DELETE is 95.21. Once again, we observe that the mixed-strategy equilibrium accounts for the mean Game A payoffs in both conditions extremely well.

There is no mixed-strategy equilibrium for Game B. If all 18 players choose route (*O*-*A*-*B*-*D*), then each would earn 40 payoff units. Figures 3.6 and 3.7 exhibit the running mean payoff by game for Conditions ADD and DELETE, respectively. The mean payoffs for Game 1B start around 80 in trial 1 in each condition and slowly decrease to about 40. Combining results across the two conditions and using the session as the unit of analysis, we compared the mean payoffs between Games A and B for each half of the session. The mean payoff for the first half (rounds 1-20) was significantly higher that the mean payoff for Game B (z=2.2, p<0.05) by the Wilcoxon signed rank test. The corresponding result for the second half (rounds 21-40) assumed exactly the same value. Combing the different comparisons of conditions ADD and DELETE, our analyses suggest that the two alternative framings of the BP have the same effect on route choice and payoffs in both Games A and B. Consequently, the six sessions are combined across the two conditions in subsequent analyses.

3.4.5 Switches

Selten et al. conducted an experiment on traffic networks quite similar to Game 1A. They studied a network with two parallel roads—a main road *M* and a side road *S*—connecting a common origin to a common destination. The cost functions were linear: c_M=6+2f_M and c_S=12+2f_S, and the group size was n=18. These cost functions result in multiple equilibria in which 12 players choose road *M* and 6 choose road *S*. Subjects chose their routes independently of one another in a stage game that was iterated 200 times. Similar to the results of Game A reported above, Selten et al. reported strong support for equilibrium play on the aggregate level. The mean number of subjects choosing route *S* in two different experimental conditions (that differed from each other in the outcome information at the end of each trial) were 5.98 and 6.06. Despite increasing the number of trials five-fold, they found no convergence to pure-strategy equilibrium. Rather, they observed considerable fluctuations around the means, not unlike the ones displayed in Figs. 3.2a and 3.2b. The standard deviations of the number of subjects choosing route *S* on any given trial assumed values between 1.53 and 1.94. Helbing (2004) reported similar results.

Neither Selten et al. nor Helbing invoked the mixed-strategy equilibrium solution to explain these substantial fluctuations. Rather, they attributed them to the multiplicity of equilibria. This may not fully explain the deviations from equilibrium once it has been reached. We proposed above a "sophisticated" strategy where a few forward-looking players deviate from equilibrium *deliberately* in the hope of exploiting this deviation in subsequent trials. Another possible reason may be grounded in the demand characteristics of the game. Some subjects may simply not believe that they are expected to stick to the same route once equilibrium is reached. Randomization of routes may account for the behavior of yet another fraction of the subjects. All of these reasons interact with the opportunity cost of a single deviation from a (9,9) split to (10,8) split that, as we reported above, reduces the individual earnings by only 10 percent.

Figure 3.8 exhibits the running mean number of switches for Games A and B. The means for each trial are computed across all the 108 subjects. As subjects converge to choosing route (*O*-*A*-*B*-*D*) in Game B, the number of switches converges to zero. Consistent with Figs. 3.3a and 3.3b, Fig. 3.5 shows that learning in Game B is slow; on average, the mean frequency of switches decreases by 1 every 5 trials. Figure 5 further shows no downtrend in the mean number of switches in Game A. Our results are consistent with those reported by Selten et al. (2004, Fig. 3). Although there is a negative trend over the 200 trials in their experiment, their figure suggests no discernible trend in the first 40 trials. The mean number of switches in Game A is about 6 (Fig. 3.5), whereas the mixed-strategy equilibrium predicts a mean of 9. This is the first piece of evidence that rejects the symmetric mixed-strategy equilibrium. On the whole, subjects do switch their routes in Game A but not as frequently as predicted.

Is it beneficial to switch? To answer this question, we conducted two analyses. First, across all six sessions for each game separately, we correlated the individual frequency of switches (min=0, max=39) and the individual payoff across all the 108 subjects. Both correlations were negative and significant: -0.43 for Game A and –0.83 for Game B (p<0.05 in both cases). To assess the magnitude of decrease in earnings associated with switching, we computed for each subject the mean payoff across all the trials that were *preceded* by a switch and the mean payoff across all trials *not preceded* by a switch. This computation yielded two means for each subject, pay_{switch} and $pay_{non\text{-}switch}$ (pay_s and pay_{ns} for short). Subjects who switched less than four times were excluded from this analysis. Our results show that $pay_{ns}>pay_s$ for 58 of the 94 subjects in Game A and 76 of 87 subjects in Game B. Both results are significant (p<0.05) by a sign test. On average, the subjects' mean earnings decreased by 2.86 in Game A and 11.72 in Game B as a result of switching.

The first analysis does not consider the possibility that a payoff following a switch may, indeed, be lower than the preceding payoff but nevertheless higher than it would have been without the switch. Our second analysis checked this possibility. For each subject across all the rounds in a given game, we computed the mean difference between her payoffs following a switch and the payoffs she would have received without switching (assuming that the remaining players would play as they did). The mean differences for sessions 1, 2, 3, 4, 5, and 6 in Game A were –5.45, -1.25, -7.86, -1.84, 0.26, and 0.05. The corresponding results for Game B were –0.57, -1.98, -3.59, -3.14, 3.27, and –10.75. Taken together, both analyses show that, on average, switching was not beneficial.

3.4.6 Individual Differences

Moving from aggregate to individual analyses, our results show that the symmetric mixed-strategy equilibrium does not account for the individual route choices. Figure 3.9 displays the frequency distribution of the number of subjects choosing route (*O*-*A*-*D*) in Game A. Except of the two classes with a single frequency of 0 and 40, all the other frequencies are grouped in classes of 3 (1-3, 4-6, …, 37-39). The mean and variance of this grouped frequency distribution are 20.06 and 91.1, respectively. The expected number and variance under mixed-strategy equilibrium play are 20 and 10, respectively. The observed frequency distribution differs significantly from the theoretical distribution in its variance (a 9:1 ratio) but not in its mean. The hypothesis that *all* subjects follow the mixed-strategy equilibrium with probability 0.5 of choosing route (*O*-*A*-*D*) is flatly rejected. Figure 3.6 shows that 6 subjects chose route (*O*-*A*-*D*) no more than 3 times and 11 other subjects chose it at least 37 times. The corresponding theoretical frequencies are essentially zero.

Figure 3.9 suggests a mixture of subject types, with a few subjects choosing the same route on almost all 40 trials and most others mixing their route choices although not necessarily with equal probabilities. For all the subjects who chose each of the two routes in Game A at least once (101 subjects), we conducted a run test to test the considerably weaker null hypothesis that each sequence of 40 route choices is generated by a Bernoulli process with fixed probability p ($0<p<1$) that may differ from one subject to another. This hypothesis was rejected for only 30 of the 101 subjects. The null hypothesis of random play in Game A cannot be rejected for about 70 percent of the subjects. The majority of the subjects seem to be mixing their choice of routes but not in the proportions dictated by the symmetric mixed-strategy equilibrium.

3.5 Discussion

There are three major findings. First, the symmetric mixed-strategy equilibrium accounts for the choice data in all six sessions of Game A extremely well. However, there is no support for it on the individual level. Rather, most subjects do mix their route choices but not in the proportions implied by equilibrium play. Second, in support of the BP, when a cost-free edge is added to the basic game, players' route choices converge to the Pareto deficient and dominant equilibrium strategy that results in a 60 percent decrease in their earnings. Third, route choice and rate of learning are immune to alternative framings of the BP in terms of adding a cost-free edge to the basic game or deleting it from the augmented game. These findings are restricted to the Minimal Critical Network—the simplest traffic network in which the BP may be realized; they may not generalize to other networks with a different architecture.

CHAPTER 4: BAD IS STRONGER THAN GOOD - THE ASYMMETRICAL EFFECTS OF POSITIVE AND NEGATIVE PERFORMANCE FEEDBACKS ON LARGE-GROUP COORDINATION PROBLEM IN THE LABORATORY

4.1 Introduction

Efficient coordination is an important ingredient for success in large organizations (March & Simon, 1958; Arrow, 1974; Cooper, 1999). Ability of independent agents to coordinate their actions efficiently is an important problem faced by firms. As a result, economists have devoted considerable attention, particularly in laboratory experiments to understand precisely how one can obtain efficient coordination among large groups of independent agents. (Van Huyck, Battalio & Beil, 1990; Weber, 2006; Bornstein et al., 2002; Brandts et al., 2005; Camerer & Knez, 1997). This chapter investigates this coordination problem in controlled laboratory experiments. I borrow insights from psychology and organizational theory literatures and demonstrate that the ability of large groups to coordinate successfully can be critically affected by the *quality* of feedback about individual performance.

Substantial experimental evidence, mostly from a game known as the minimum-effort coordination game (hereafter called the MECG), illustrates that large groups never coordinate efficiently (e.g., Van Huyck et al., 1990). Large groups of 10-16 individuals, who cannot communicate with one another, almost never coordinate successfully, and repetition alone does not solve the problem.

However, this is inconsistent with the real world observations where firms that are much larger than those in laboratory experiments manage to coordinate successfully. Therefore, it is natural to ask that if ‘large’ laboratory groups fail to coordinate in this type of game, how do large firms manage to often do so? A number of experimental studies have demonstrated, using the MECG, how to improve coordination in large groups that are prone to slipping into the traps of miscoordination. For example, Brandts and Cooper (2005 & in press) conclude that communication with employees (by a manager) is a more effective tool than providing financial incentives to employees for leading organizations out of low performance traps. On the other hand, Weber (2006) finds that financial incentives of “all-or-none” type – regardless of whether they are substantial or nominal in monetary value, and whether they are targeted to a specific outcome or untargeted – are effective at improving large group coordination. Blume and Ortmann (henceforth, BO) (2005) allowed costless, structured, and non-binding pre-play messages among agents to investigate the impact of cheap talk on coordination. Their main finding is that costless communication can facilitate coordination on the most efficient equilibrium. Battalio et al. (2001) and Goeree et al. (2005) also show that the level of coordination in closely related games is responsive to the magnitude of monetary incentives.

In this paper, I adopt a novel approach to address the coordination problem faced by large groups in the laboratory. I explore how the problem of coordination failure can be overcome using *structured* feedbacks related to individual performance among members (‘employees’) of a large group (‘firm’). I vary the performance feedback variable along a dimension – namely, the quality of feedback. I examine two types of feedbacks - negative and positive. These forms of 'horizontal' feedbacks (i.e., feedback among employees in an organization) are often present in real world firms (Cyert, 1992; Greve, 2003). Furthermore, they are more *natural* form of communication, easily implementable since they are *non-pecuniary* in nature, cost *efficient* relative to financial instruments, and more importantly they arise *endogenously* in a group. Therefore, understanding the distinctions between devices already investigated in this literature (e.g., financial incentives, cheap talk, and 'vertical' communication (between a manager and employees)) and the tools used in this paper (performance feedbacks of two types) for facilitating large group coordination is a productive research program.

To organize thinking, consider a firm producing via assembly line where the slowest worker determines the speed or the productivity of the entire line. If all the workers exert minimal effort, the firm slips into a regime of low level of productivity, but the line could become more productive if all workers tried harder. However, workers who try harder unilaterally waste their efforts if there is a single worker who does not try hard enough. This particular effort choice by our hypothetical worker imposes considerable penalty on other 'productive' workers. In particular, the hypothetical worker is better off than the rest since her cost of effort (assuming that cost function is monotonic in effort choice) is smaller relative to others while the benefit is even across all the workers. More importantly, the entire production line is stuck at a low level of productivity due to a single worker. Therefore, the slowest worker's effort is most likely to evoke displeasure in other workers. This collective displeasure, if communicated to the worker, might create "peer pressure", which in turn *may* force this worker to put in higher efforts in future. Hence, the firm may attain efficient coordination. So, there may be instances where a type of negative feedback (i.e., peer pressure) may prove beneficial for efficient group outcome (Kandel and Lazear, 1992). This paper explores this avenue. It examines the impact of two types of feedbacks on group coordination process by controlling their structures.

The central research question of this paper is: can performance feedbacks of different kinds promulgate efficient coordination in large groups? The answer to this question is of real importance, since coordination failure plays a vital role in a number of economic settings and horizontal performance feedbacks are seen to be critical for organizational survival and superior performance in organizational theories. There is a burgeoning literature in organizational theory, called *performance feedback theory*, that underscores the importance of performance feedbacks at the employee level for efficient functioning of large organizations (see Dickson et al., 1996 and references therein; Greve, 2003). One insight emerging from this literature is that the lack of a proper feedback mechanism among employees may be at the root of many organizational problems. Existence of an effective feedback system may prove useful in overcoming coordination breakdown. Extending this strain of research in settings where low effort from any one individual may destroy incentives for higher efforts by others and where group output is sensitive to the lowest individual effort therefore may prove valuable. The policy question in this context is how to create the best performance feedback mechanism, which may lead an organization out of low performance traps.

To investigate this, I conduct an experiment, which is designed to capture an environment in which coordination failure may occur. I then systematically study the effects of changing the quality of performance feedbacks on group coordination. I introduce the MECG in the next section and then discuss about the two types of feedbacks used in this paper.

4.2 The MECG

I employ the MECG first studied experimentally by Van Huyck et al. (1990). Table 4.1 presents the payoff matrix of the MECG. This game is a generalization of the two-person stag hunt game that has multiple Pareto-ranked equilibria. The stage game is iterated a finite number of times by a group of n players, each of whom simultaneously chooses an effort level in the set $\{1, 2, \ldots , 7\}$. The payoff of a player depends on that player's own effort choice as well as the *minimum* effort choice in the group. The payoff parameters are chosen so that all n players benefit from choosing a high minimum, but there is a considerable penalty for choosing an effort level higher than the group's minimum. The stage game has seven strict Nash equilibria, and at each of this equilibrium every player chooses the same effort level. The equilibrium in which every player chooses effort level 7 yields the highest payoff to each player. However, without any pre-game communication, strategic uncertainty embedded in the payoff function may induce each player to choose effort level 1 that yields the lowest payoff. Following Harsanyi and Selten (1988), choice of 1 by each player leads to the secure equilibrium and choice of 7 by each player leads to the payoff-dominant equilibrium.

The MECG corresponds to a situation in which minimum effort (in the group) condition determines the individual outputs, but individual effort choice decides the individual cost of effort. Given the payoff structure of the game, players' effort choices must trade-off security (achieved at the least efficient equilibrium) with efficiency (achieved at the highest efficient equilibrium). This game captures key features of the kinds of coordination problems faced by real world firms (Camerer & Knez, 1994; Weber, 2000; Brandts & Cooper, in press) [45]. Hence, this game is a natural choice for our purpose.

The experiment involves repeated play of the MECG among the members of a large group. Each round involves two decision stages[46]. In the first stage, each group member chooses her effort level. These choices immediately determine the payoffs for each member of the group. Each group member then observes the efforts chosen by everyone else in the group in a manner that protects the anonymity of each member's choice[47]. Decision *choice*s available to each group member in the second stage correspond to experimental treatments in the paper.

This chapter induces two treatment effects to explore the efficacy of two different structured performance feedback mechanisms. In one treatment, in the second stage each group member can express her levels of *disapproval* by (numerically) rating each of the other first stage effort choices. In another treatment, in the second stage each group

[45] Researchers have claimed that the airline industry is an example where coordination problem is commonplace and strategic environment is similar to the one modeled in the MECG, see Knez and Simester (2001), Gittell (2001).

[46] The original MECG involves one decision stage. In this paper, I augment the original game.

[47] I elaborate on the experimental design in section 5.

member can express her levels of *approval* by (numerically) rating each of the other first stage effort choices. Thus, this paper uses 'disapproval' and 'approval' ratings by group members (or 'employees' of a 'firm') as proxies for negative and positive performance feedbacks, respectively. These ratings have two common features: (1) it is costless for a member to assign ratings to other effort choices, and (2) these ratings have no direct payoff consequences for the recipients. Instead of reducing or increasing the payoffs of group members, as is the case for financial incentives, each member is given the opportunity to communicate her 'emotion' feedback via ratings of disapproval and approval. Moreover, these feedbacks are *ex post* in nature in the sense that they have no impact on the current round play. Hence, the coordination problem of the original MECG remains. However, in a repeated game context, these feedbacks might serve as a form of pre-play communication for future rounds. Therefore, enhanced coordination may be expected.

There are good reasons to believe that the opportunity to express approval and disapproval of others' choices in itself may improve coordination in a group setting. The opportunities for group members to express positive and negative reactions about others' actions are commonplace in the real world. In many team settings, individuals observe actions of others and react to them in ways that may impose no pecuniary costs on both parties, but strategic use of feedbacks may influence future behavior of group members.

For example, Kandel and Lazear (1992) explore, theoretically, conditions under which peer pressure – a form of negative performance feedback - interacts with team incentives and can produce the most efficient outcome. In this paper, I adopt this line of research by evaluating the impacts of two performance related feedback mechanisms on team performance. A recent experimental paper by Masclet et al. (2003) also investigates the impact of 'disapproval ratings' on contribution levels of players in a standard VCM game. In their experiment, the first stage corresponds to a standard VCM game. In the second stage, individual decisions are anonymously revealed to each group member who has an opportunity to costlessly assign disapproval points that are payoff-neutral. Expressions of disapproval thus perform as a means of communication among players in their design. The main result of their paper indicates that non-pecuniary disapproval ratings raise contributions by as much as monetary sanctions[48].

The instruments employed in this chapter will potentially allow us to draw inferences regarding prescription for the best kind of performance feedback mechanism that might be used in real firms to induce coordination on efficient actions. Therefore, our case corresponds to the one in which the efficient functioning of large organization relies on the use of two endogenous performance feedback mechanisms.

Section 3 discusses the literature extant in psychology and organizational theory that inspired this experiment. Section 4 discusses research related to the MECG, section 5

[48] Although the current study employs framework of Masclet et al. (2003), the underlying strategic consideration in a usual VCM game is strikingly different from a MECG. In the MECG, there is no incentive problem for a player to choose a higher effort level, but in the VCM game a player has strong incentive to free ride on others. Instead, in a pure coordination game, strategic uncertainty plays a key role that makes the coordination among players a difficult task.

elaborates on the experimental design, section 6 reports results and the last section concludes.

4.3 Psychology, organizational theory, and performance feedbacks

In the following section, I draw on insights mainly from psychology and organizational theory literatures to argue that the two types of feedback mechanisms used in this paper are fitting choices for our purpose.

There is a rapidly increasing literature in organizational behavior that highlights the importance of feedback related to individual performances for achieving efficiently coordinated large organizations (see Dickson et al., 1996). Greve (2003) offers an intriguing analysis of how firms evolve efficiently in response to feedback. Based on ideas from organizational theory, social psychology, and research from many industries, he argues that high-performing organizations quickly learn from their team-based feedbacks, but low-performing organizations only slowly benefit from those feedbacks. One of the central observations in this literature is that the difference in performance between organizations may eventually reduce to the existence and quality of effective feedback mechanisms. The emphasis in the literature is, however, more on horizontal than vertical mode of communication since the former is less 'directive' in nature and hence is less susceptible to employee retaliation effects (see Kiesler et al., 1992 and references therein). As a result, I choose horizontal feedback mechanism over vertical.

Furthermore, psychology literature presents two important findings that provide significant support for the research question investigated in this paper. First, there exists a vast literature in social psychology that emphasizes the use of 'social' feedbacks as tools or strategies to obtain socially desirable outcomes (See, for example, Ellickson, 2001; McAdams, 1997; Mahoney & Sanchirico, 2000 for excellent discussions). According to this strand of literature, these feedbacks may often assume the form of, for example, social approval and disapproval and might engender socially efficient outcomes. The reasoning is that individuals, in an extreme case, may internalize or self-enforce such informal social rewards and sanctions by inculcating feelings of pride or guilt in themselves and therefore may respond in a manner that is consistent with socially desirable outcome (Posner& Rasmusen, 1999; Blau, 1960). Thus, efficacy of such tools of social control in promoting socially efficient outcome is widely recognized in these streams of research[49]. This particular literature, therefore, indicates that communications of non-monetary approval and disapproval ratings may very well capture the essence of positive and negative feedback, respectively. Moreover, they may steer large groups out of inefficient performance traps.

Second, there is a hypothesis in the psychological literature claiming that negative events (e.g., receiving criticism) are stronger than positive events (e.g., receiving praise). This hypothesis appears to be consistently supported across a broad range of psychological phenomena (Baumeister et al., 2001) Diverse studies in psychology provide evidence that, other things being equal, negative events are more salient, potent, dominant, and generally efficacious than positive events (Taylor, 1991; Baumeister et al.,

[49] In standard economic theory, phenomena such as peer pressure (Kandel and Lazear, 1992), and the avoidance of social disapproval (Akerlof, 1980; Lindbeck et al., 1999) have also been integrated into theoretical models to produce socially desirable outcomes.

2001; Ito et al., 1998). The disparate instances of greater sensitivity to negative events represent the operation of what has been termed as 'negativity bias.' The basic argument is that negative events seem to command more attention (Rozin & Royzman, 2001, p. 301), have powerful signaling value (Henderlong et al., 2002), and are generally weighted more heavily than positive events (Taylor, 1991, p.70)[50].

Taylor (1991)'s *mobilization -minimization hypothesis* also buttresses this claim that people attempt to minimize or avoid 'bad' events such as criticism coming from others and distance themselves from them more than strive to maximize the 'good' events such as praise conferred upon by others. In other words, people are more concerned about avoiding (minimizing) 'bad' feedback than about maximizing 'good' feedback. This major finding naturally begs this question of whether this general finding in psychological literature carries over to an economic setting. Specifically, translated into our context, this finding may indicate a greater motivational power of disapproval ratings than approval ratings for facilitating coordination.

I draw on these important insights from these two strands of literature and design an experiment that focuses on two questions. (1) Can positive and/or negative feedbacks solve the large group coordination problem as embedded in the MECG? (2) Are asymmetrical effects of positive and negative feedbacks on coordination outcome observed? The <u>first</u> question stresses the close connection between the quality of information feedback and the group coordination problem. The <u>second</u> question delves

[50] Some evidence suggests that responses in the brain are stronger to bad than good things. See Baumeister et al., (2001) for a survey of research in this area.

into the issue of asymmetrical effects of two information feedback mechanisms on the group coordination process.

Although a pattern of asymmetrical effects has gradually emerged in the psychology literature, it has not yet been explicitly tested using experiments based on monetary incentives, as it is the case with laboratory experiments in economics. This paper affords such a perspective. The above discussion leads to the following two hypotheses that are central to this paper:

Hypothesis 1: The opportunity for players to either express approval or disapproval of others' decisions may result in higher levels of coordination outcome in the MECG relative to no such expressions.

Hypothesis 2: The opportunity for players to disapprove of others' decisions may result in higher levels of coordination outcome in the MECG relative to the opportunity for players to approve of others' decisions.

4.4 The present experiment & related literature

This section has one major goal, namely, to relate the experiment to a growing body of laboratory experiments aimed at understanding how to obtain efficient coordination in the MECG.

Both BO (2005) and this paper allow players to communicate among themselves in the framework of MECG, therefore an in depth discussion of their paper is warranted. BO allowed costless, structured, and non-binding pre-play messages among participants to experimentally investigate the impact of cheap talk on coordination outcome in the minimum and median effort games. Their main finding is that costless messages can facilitate coordination on the Pareto-dominant equilibrium in finitely repeated minimum and median effort games. So, adding an explicit ‘communication’ stage prior to playing the MCEG can overcome strategic uncertainty and solve the equilibrium selection problem.

Although both this paper and BO investigate the impact of communication on coordination using the same game, there are two features that distinguish this paper from their work. First, in BO a *pre-play* communication stage precedes the original MECG. In contrast in this chapter, revealed opinions about others’ actions are *ex post* in nature, which is distinct from pre-play messages. As a result, usual cheap talk arguments of Farrell (1987) for efficient coordination do not apply to our setting[51]. Second, pre-play cheap talk has informative value of signaling players’ intentions about current round action choices, which has no *direct* reward or punishment aspect attached to it. Compared to this, expression of approval or disapproval is a straightforward form of informal reward or punishment that may not have any obvious signaling value for players. However, in a repeated game context these ratings can function as a warning of lower future effort choices if the recipient does not increase her own effort choice. Thus, I contend that while both papers share a common research theme, i.e., the impact of communication among players on coordination, the specific communication device utilized in each paper differs.

There are other papers that also study the effectiveness of communication in allowing groups to coordinate efficiently (Charness, 2000; Chaudhuri et al., 2001). A host

[51] See references in BO (2005) for more discussion on this issue.

of other papers also explore conditions that may engender efficient level of coordination[52]. For example, Bornstein et al. introduces incentives in the form of inter-group competition in the context of the MECG to obtain efficient coordination, Berninghaus et al. (1998) varies the horizon of the repeated game to understand the impact of time on dynamics of coordination process. The present paper contributes to this body of literature by experimentally examining the effects of performance feedback mechanisms on large group coordination. While most of the above studies introduce financial incentives, disincentives, cheap–talk, vertical form of communication, time horizon, and inter-generational advice in the MECG to produce the Pareto-dominant outcome, I introduce payoff neutral, emotion based horizontal communication about performance among 'employees' that may arise endogenously in a large 'firm' to study the same problem. Understanding the distinctions among these instruments is important for practical purposes, and this is the first paper that makes an attempt in this direction.

4.5 Experimental design

The experiment consists of 18 sessions that were conducted at the Economic Science Laboratory at the University of Arizona. Four sessions were conducted in which participants did not have the opportunity to express either approval or disapproval of others' choices. This constitutes the benchmark (B) treatment in our study. I decided to conduct B sessions for two reasons. (1) In order to facilitate the comparison of

[52] See Ochs (1995) and Camerer (2003) for excellent surveys of experimental studies, and Cooper (1999) for outstanding discussions of theoretical studies in this area.

coordination levels with and without feedback mechanisms, and (2) to replicate the baseline finding of the previous literature so as to detect if there is any 'subject pool' effect that may contaminate the results. Fourteen more sessions were conducted; seven for the approval treatment and seven for the disapproval treatment in which each participant could either express his/her levels of approval or disapproval of others' actions. I shall refer to these treatments as A and D, respectively[53]. Each session consists of 10 participants and 10 rounds. The participants were University of Arizona undergraduates with non-economics majors who were inexperienced in this particular type of experiment. No participant played in more than one session of the experiment. On average, a session under treatment B lasted 30 minutes, whereas a session under treatment A or D lasted 45 minutes, including initial instruction and payment of participants. The experiment was conducted using Z –tree software, Fischbacher (1999).

In all treatments, the payoff-dominant equilibrium (7, 7, . . . , 7) paid $1.30 to each participant whereas the secure equilibrium, (1, 1, . . . , 1) paid $0.70. Participants were given this information in the form of a payoff table (Table 4.1). A questionnaire was distributed to participants before the start of the experiment to ensure that they understand the payoff matrix. A record sheet was provided to each participant to keep track of the earnings[54].

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[53] The difference in the number of B sessions was planned. Previous experiments have extensively documented the robustness of the results obtained in the baseline treatment in Van Huyck et al. (1990). Therefore, I decided to run less number of sessions for B.

[54] Instructions can be found in the appendix A4.

In treatment B, each round consists of only one-stage in which participants simultaneously choose an integer between 1 and 7. After each round, the entire distribution of effort choices is revealed to participants along with their individual payoff. In treatments A and D, each round consists of two-stages in which the first stage follows exactly the same rules as in treatment B. At the beginning of the second stage, each participant could assign zero to six (only whole numbers) approval or disapproval points to each of the other 9 first stage choices, depending on the treatment. There was no option to 'abstain'. Points awarded to a participant had no effect on her first stage payoff and were costless to assign. The points represent the levels of approval or disapproval of a participant about another participant's action in the first stage. A rating of 6 points was meant to be assigned for the highest level of approval or disapproval and 0 for the lowest level of approval or disapproval. Note that each participant could assign at most 54 points and at least 0 point to other 9 participants[55]. At the end of the second stage, each participant observes an aggregate 'score' of approval or disapproval points received by

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[55] In order to avoid any reputation effects, the computer displayed first stage action choices and the aggregate approval or disapproval score received by each first stage choice at the end of the second stage in a random and anonymous manner in each round. Thus, no participant knew who chose what number in each round, which prevents any reputation effect to influence the pattern of further play. Although participants' ID numbers remained constant across rounds, the computer did not show these ID numbers. Therefore, a participant was not also able to maintain a history of other participants' decisions.

each first stage action[56]. Given the anonymous protocol used in the design. I induce an impersonal form of feedback in this experiment. However, one could argue, in principle, that in real world most of such feedback is carried out on a personal level. Notwithstanding, I adopted the impersonal feedback mechanisms because I am primarily interested in the question of if participants could use the ratings (based on the pattern of play and not on the basis of the history of participants) to coordinate on higher efficient levels. Therefore, I avoid any reputation effects that may influence the point assignment behavior of participants.

The approval and disapproval points and their meanings were described to the participants in the following language:

"In the second stage, you have the opportunity to register your approval/disapproval of each of the other 9 participant's first stage decisions by distributing points. You can distribute points to any other participant if you approve/disapprove of his or her first stage decision. You can distribute points from 0 to 6 to a participant, that is, 0, 1, 2, 3, 4, 5 and 6. Distributing 6 points to a participant's

[56] It makes a difference whether participants are given information in the form of an aggregate statistic or a distribution of approval or disapproval points. While the latter communicates much better what risk participants are up against, the former one hides that additional information.

choice in the first stage shows the most approval/disapproval and distributing 0 points shows the least approval/disapproval.[57]"

The equilibrium predictions for the three games corresponding to the three treatments remain the same in either the one-shot or the finitely repeated version of the game. For the games corresponding to treatments A and D, any profile of point assignment is compatible with a subgame-perfect equilibrium notion.

4.6 Results

This section proceeds as follows. First, I describe the overall difference in choices between the three treatments. Since there are no significant session differences for a particular treatment, I combine the data across sessions for a given treatment. Next, I explore the dynamics of play for each of the three treatments. I complete this section by discussing the assignment of approval and disapproval points and their effects on choices.

The purpose of this subsection is to test for differences among the three treatments. Figures 4.1 and 4.2 illustrate the time paths of average minimum and average individual choices for the three treatments, averaged over the number of sessions that constitute each treatment. Figure 4.1 indicates that in the first round, before any element of experience exists; average minimum choices are fairly similar in treatments B and A, and the same for treatment D that starts at a slightly higher level. The difference between the former and the latter is one action level (i.e., 2 and 3). After the first round, however,

[57] The bold letters in the instructions create a strong framing effect. This emphasis was a deliberate choice to ensure that participants are aware of the fact that assigning either types of points could be used as a possible sanctioning or rewarding system.

the data for the three treatments show a clear separation and this trend continues for the entire duration of the experiment. With a minor exception, average minimum choices for treatment D shows a monotonic upward trend while the means for treatments B and A converge to the secure action by the eighth round. Although the average minimum choices in treatment A dominate the average minimum choices in treatment B by one action level between the second and seventh round, this domination completely weakens from the eighth round onwards. Overall, Figure 4.1 implies that treatments D and B generate the highest and the lowest level of coordination, respectively. On the other hand, treatment A initially performs a little better than treatment B, but towards the conclusion of the experiment it coincides with treatment B in terms of efficiency.

Time series data for average individual choices also follow a similar pattern for three treatments (Fig. 4.2). Without any learning effects, the mean for treatment D begins one action level higher than the means of other two treatments. With experience, the means for treatments B and A decrease over trials, and this declining trend is stronger for means in B than in A. Overall, in the last few rounds, average individual choices for these two treatments plummet sharply towards the lowest effort level, thus replicating the major finding of Van Huyck et al. (1990). Averages for treatment D oscillate between effort choices 6 and 7 for the entire experiment.

To test the null hypothesis of whether the distribution of effort choices in any two different treatments came from the same distribution pooled Kolmogorov-Smirnov (large) two-sample one-tailed tests were conducted. Three possible pairs of treatments are considered (i.e., B=D, B=A and A=D). For each pair, three tests were performed: for the first round, for all ten rounds, and for the last round of the experiment[58]. None of the three tests for the first round rejected the null hypotheses. This suggests that the distribution of effort choices in all three treatments did not differ from one another without any learning effects. However, test results indicate that the presence of negative feedback in treatment D elicited stochastically higher distribution of effort choices than in treatments B and A for the entire experiment. This observation is valid for the data from the last round as well. Treatment A also yielded stochastically higher distributions of effort choices compared to treatment B when all rounds are considered, but in the last round they induced (statistically) the same level of efficiency.

Two observations about the treatment effects are in order. First, with experience, a form of negative feedback succeeds in inducing higher level of efficiency in large groups compared to a form of positive feedback or no such feedback mechanism. Second, a positive feedback technique induces higher Nash efficient choices in large groups than without any form of feedback (taking into account the data from the entire experiment). So, any of the two available feedback mechanisms is more effective at leading large groups out of the most inefficient equilibrium than no such feedback. Taken together, these findings substantiate the claims made in the psychology literature that (1) negative

[58] Condition B=D: first round choices: $X^2(40,70) = 4.09$, fail to reject at 5%; Condition B=A: first round choices: $X^2(40,70) = 3.71$, fail to reject at 5%; Condition A=D: first round choices: $X^2(70,70) = 4.37$, fail to reject at 5%; Condition B=D: ten rounds choices: $X^2(40,70) = 29.08$, $p<0.001$; Condition B=A: ten rounds choices: $X^2(40,70) = 6.93$, $p<0.05$; Condition A=D: ten rounds choices: $X^2(70,70) = 11.87$, $p<0.01$; Condition B=D: last round choices: $X^2(40,70) = 20.32$, $p<0.001$; Condition B=A: last round choices: $X^2(40,70) = 4.02$, fail to reject at 5%; Condition A=D: last round choices: $X^2(70,70) = 14.43$, $p<0.001$.

and positive events may produce efficient outcomes and (2) there may exist asymmetrical effects of negative and positive events. Data from the experiment confirm these in the framework of MECG. Moreover, in agreement with the performance feedback theory, no feedback is worse than any form of feedback (that are investigated in this paper) in our context. In agreement with treatment effects, average earnings are the highest in treatment D and lowest in treatment B. The average earning for a typical B and A sessions are $10.30 and $11.67 while the same for a typical D session is $15.50, including a $5 show-up fee.

In this subsection I attempt to provide insight into our experimental results. It is shown that the highest level of coordination success obtained in treatment D can be attributed to a simple strategy adaptation rule (SAR) employed at the individual level.

To articulate SAR, I employ a so-called ‘learning direction theory’ which is a qualitative theory in relatively elementary form and has proved to be valuable in explaining different experimental results[59]. To make this ‘learning rule’ more precise, I formulate the SAR. This strategy adaptation rule can be regarded as a particular instance of the learning direction theory.

(SAR): When a player is the minimum player in her group in a given round, she strictly raises her action in the next round. When a player has chosen an action higher than the group minimum in a given round, she increases or at least does not reduce her action in the next round.

[59] For a more thorough presentation of this learning approach, we refer the reader to the studies by Selten and Stoecker (1986) or Selten and Buchta (1994).

Translated into strategic learning in our repeated coordination game, a minimum player may strictly increase her current round action relative to previous round action more when facing disapproval ratings (approval ratings) from others than approval ratings (no such ratings). On the other hand, a non-minimum player may be less willing to choose an action in the current round that is lower than her previous round action in the presence of disapproval ratings (approval ratings) than approval ratings (without any such ratings). In accordance with this, one would expect that a higher proportion of minimum (non-minimum) players under treatment D would choose strictly higher (equal or higher) actions in the current round relative to the previous round than that of treatments A and B. Similarly, one would expect in the data that a higher proportion of minimum (non-minimum) players under treatment A would choose strictly higher (equal or higher) actions in the current round relative to the previous round than that of treatment B.

If the adaptation rules as described above find support in the data, then one may infer that in the presence of negative (positive) ratings, participants' decisions move *more* into the direction of efficiency than in the presence of positive ratings (no such ratings). It should be noted that learning directions theory only predicts the *direction* of a change in the choice of a participant, but not the *magnitude* of that change.

Table 4.2 reports SARs for the three treatments. The adaptations are divided into two categories. The first category consists of the responses of minimum players and the second category consists of the responses of non-minimum players. Within a category, I distinguish three possible cases. Either a player selects an action in the current round that is below her previous round action, she does not change her action in the current round relative to the previous round, or she selects an action in the current round that is above her previous round action. In case all players choosing the same action, each player is designated as the minimum player. In order to make the results of the three groups comparable, I use relative frequencies.

A number of observations can be made from Table 2.

The percentage of minimum players is the highest in B and the lowest in D (5.07% and 4.29%) and the percentage of non-minimum players is the highest in D and the lowest in B (5.71% and 4.93%). This is in accordance with the initial expectation. Groups that coordinated better are likely to include lower proportion of minimum and higher proportion of non-minimum players relative to those groups that did not coordinate that well.

Next, I check if the frequencies of choices by minimum players strictly raising their actions in the next round differ between any two given experimental treatments. There are three pairs of comparison groups: (D & B), (A & B), and (D & A). All of these differences are significant at 1% level[60]. These results mean that the proportion of minimum players strictly raising their actions in the next round is significantly higher for treatments D, A, and D relative to treatments B, B, and A, respectively. The same holds for non-minimum players except for the comparison group (A & B)[61].

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[60] Ns = 6, Nus = 12, Mann-Whitney U statistic, z = 3.76

[61] Ns = 6, Nus = 12, Mann-Whitney U statistic, z = 3.32

Similarly, I examine whether the frequencies of choices by minimum players strictly lowering their actions in the next round differ between any two given experimental treatments. Again, test results indicate that the proportion of minimum players strictly reducing their actions in the next round is significantly higher for treatments B, B, and A relative to treatments D, A, and D, respectively. [62]. The same holds for all three groups in case of non-minimum players[63].

While 73.11 % of non-minimum players' choices correspond to an equal or higher next round action in treatment D, the corresponding numbers for treatments B and A are 45.21 % and 51.37%. These numbers may provide additional support to the findings (i) and (ii).

Eighty-eight percent of minimum players' choices correspond to an equal action in the next round in treatment B, the corresponding numbers for treatments and A are 48.52% and 65.91%, respectively. It must be noted that the group minima (in most rounds) in treatments B and A were overwhelmingly 1 as opposed to much higher group minima in treatment D. This left minimum players in former groups with no scope of choosing a lower action. Also, only 2.57% (28.09%) of minimum players' choices relate to a higher action in the next round in treatment B (A). Taken together, this suggests that minimum players' actions in these treatments displayed considerable 'stickiness' at the lowest action level.

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[62] Ns = 6, Nus = 12, Mann-Whitney U statistic, z = 3.76

[63] Ns = 6, Nus = 12, Mann-Whitney U statistic, z = 2.29 and Ns = 6, Nus = 12, Mann-Whitney U statistic, z = 2.91

The main conclusion is that the behavior of minimum and non-minimum players between three experimental treatments differs crucially with respect to adapting actions from one round to the next. Minimum players in relatively successful treatments were more willing to increase their future actions than in unsuccessful treatments. In addition, non-minimum players in relatively successful treatments were more willing to stay at current action level or increase future actions than in unsuccessful treatments. In general, these results provide the first piece of evidence that may help explain why negative (positive) feedbacks have performed better as statements for efficient coordination as compared to positive feedbacks (no feedbacks).

The above analyses provide an indication of how negative and positive ratings as communication devices may have altered players' behavior over time. In this subsection, I employ econometric models to comprehend better the correlation between the availability of each of these two ratings and the emergence of groups that exhibited different degrees of coordination. In particular, I examine the nature of the assignment of approval and disapproval ratings and their corresponding effects on choices.

The first issue concerns the relationship between the deviation of a player's choice from the group minimum and the total magnitude of approval or disapproval points received by that player in a particular round. Specifically, I explore whether player *i* received less approval or more disapproval points, the smaller the difference between player *i*'s choice and the minimum choice in round *t*. To investigate this issue, I regress the total number of approval or disapproval points received by a player on the difference between that player's choice and the group minimum in a specific round. Additionally, to find out if a minimum player received more or less approval or disapproval points relative to a non-minimum player, I employ a dummy variable that assumes a value 1 if *i* is a minimum player in round *t* and 0 otherwise.

Results reported in Table 4.3 indicate that, conditional on the feedback mechanism, a player's receipt of points is highly contingent on her performance in the group. There exists a (an) one-to-one (inverse) relationship between the total number of approval (disapproval) points received by a player and the difference between her choice and the group minimum in a given round. In other words, the more a player disrupts the group coordination process; the more severe (in case of treatment D) or less favorable (in case of treatment A) is the feedback from other group members. Minimum players receive more adverse feedbacks in both treatments than non-minimum players, as indicated by the coefficients of the dummy variable. Moreover, the magnitude of the coefficient for approval (i.e., 3.17) is higher than the same for disapproval (i.e., 1.45), implying that the intensity of positive feedback is stronger than negative feedback even though the former instrument engendered less efficient coordination.

This finding may implicitly suggest that the assignment of ratings at the individual level may be consistent with the 'pecking order' of the group, in terms of performance in a given round. For example, one would expect that a player who chooses 6 would assign less approval (more disapproval) points to a player who chooses 3 than to a player who chooses 5 in a particular round. Thus, players assign ratings in keeping with their relative ranks in the group. I investigate this issue next.

To determine how a participant expresses opinions to others' choices relative to her own choice in a given round, I estimate two OLS models for each treatment. In the first model, I regress the points assigned by player *i* to player *j* in round *t* on the difference in choices of players *i* and j in round *t* ($DiffAC_{ij,\,t}$ = player *i*'s choice - player *j*'s choice). The variable $DiffAC_{ij,\,t}$ always assumes a strictly positive value in the first model ($DiffAC_{ij,\,t} > 0$), indicating that player *i* is a non-minimum player. In the second model, I regress the same dependent variable on $DiffAC_{ij,\,t}$ that assumes a weakly negative value ($DiffAC_{ij,\,t} \leq 0$), indicating that player *i* could either be a minimum player or a non-minimum player. The second model also contains an interaction variable, $DiffAc_{ij,\,t}$ *X MinDummy* where *MinDummy* assumes a value of 1 if player *i* is the minimum player in the group in a particular round and 0 otherwise. This interaction variable would help distinguish the behavior of minimum players from non-minimum players.

Several observations are in order about the two models. (1) The action of a player who *uniquely* chooses the highest effort in the group in a particular round always appears in model 1; however, the actions of *non-unique* non-minimum players who choose the same highest effort in the group in a particular round appear in both models. (2) Actions of non-minimum players who *do not* choose the highest effort in the group in a round also appear in both models. (3) Finally, actions of players who *uniquely* or *non-uniquely* choose the lowest effort in the group in a particular round always appear in model 2.

First, I focus on Table 4.4 that concerns treatment D. The estimated coefficient in the first model indicates that player *i* expresses more adverse opinion to player *j*'s choice, the higher is the choice of *i*th player relative to *j*th player. However, estimates from the second model imply a point assignment behavior, which may appear counter-intuitive. In particular, player *i* assigns *more* disapproval points to player *j* who chooses a higher effort compared to player *i*'s effort, than to another player say, *j**, who chooses a higher effort than player *i*'s effort but a lower effort compared to player *j's* effort. As indicated by the coefficient of the interaction term, a minimum player expresses more adverse opinion the higher is the difference between her choice and the choice of a non-minimum player.

Next, I examine on Table 4.5 that focuses on treatment A. The estimated coefficient in the first model indicates that player *i* expresses less favorable opinion to player *j*'s action the higher is the difference between the actions of *i*th and *j*th player. Similar behavior was also observed in model 1 for disapproval ratings. Estimates from the second model reveal exactly the same behavioral pattern as treatment D. In particular, player *i* assigns less approval points to player *j* who chooses a higher effort compared to player *i*'s effort, than to another player say, *j**, who chooses a higher effort than player *i*'s effort but a lower effort compared to player *j's* effort. Again, a minimum player expresses less favorable opinion the higher is the difference between her choice and the choice of a non-minimum player.

Taken together, four observations are in order. (1) A minimum player, on average, assigns positive or negative ratings in a manner that is inconsistent with the 'pecking order' of the group; the intensity of her rating increases in the absolute difference between her choice and the choice of a non-minimum player. (2) A non-minimum player, who uniquely chooses the highest action in the group, on average, assigns both types of ratings in keeping with the relative ranks (in terms of performance) of players in the group. (3) A non-minimum player, who non-uniquely chooses the highest action in the group, assigns ratings like a unique non-minimum player, (4) a non-minimum player, who chooses an action that is not the highest action in the group, assigns ratings the intensity of which increases in the absolute difference between her choice and the choice (which could be higher or lower relative to her choice) of the other player.

These results indicate that participants dislike asymmetrical effort choices by others (i.e., effort choices by others that are higher or lower than their own choices). For example, a close inspection of point assignment behavior by minimum and non-minimum players (who choose actions that are not the highest action in the group) reveals that this observation is true. This atypical assignment behavior may have been driven by two entirely different motivations. Assigning harsh ratings to the choices that lie far above one's own choice may have been motivated by the fear of receiving a heavy dose of unfavorable feedback from those high performers, and, therefore, such a participant (e.g., a minimum or a non-minimum player who does not choose the highest effort in the group) retaliates with high measure of negative feedbacks. On the other hand, assigning harsh ratings to the choices that lie far below one's own choice (e.g., a unique, a non-unique non-minimum or a non-minimum player who does not choose the highest effort in the group) may have been triggered-off by the clear intention of informally punishing those low performers.

Finally, to determine the effect of approval and disapproval ratings on the *magnitude* of action choices, I investigate whether player i chooses a higher action in round *(t+1)* relative to round *t* in response to the number of approval or disapproval points she received in round *t*[64]. The regression model reported in Table 4.6 contains the estimates that complement the findings from the action adaptation analyses. Estimates suggest (1) that the difference in actions between two consecutive rounds for player *i*, on average, is increasing (decreasing) in the total number of disapproval (approval) points received in the current round, and (2) if player *i* is a minimum player in the current round in treatment D, then she increases her next round action more than if she is in treatment A. The first finding coupled with the results from the action adaptation analyses provide a better understanding of how the highest level of coordination was obtained in treatment D, but not in other treatments. To sum up, the analyses reported above yield important empirical information about the factors that influenced participants' decisions in a large group coordination problem. In the following section, I make an effort to relate the major findings of this experiment to a number of hypotheses in order to find out which one is the best candidate for explaining them.

4.7 Conclusion

In this study, using controlled laboratory experiments, I examine how the problem of coordination failure as embedded in the MECG may be overcome by using *structured*, *ex post* feedbacks related to individual performance among members of a large group. I use 'disapproval' and 'approval' ratings by group members about individual choices as vehicles for negative and positive performance feedback, respectively. These ratings are

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[64] We also estimated fixed effect models for all the econometric estimates reported here. However the results do not change qualitatively between the fixed effect and pooled OLS models. Therefore, we decided to report the results from the pooled OLS models.

non-pecuniary in nature, therefore cost efficient relative to financial instruments, and they arise endogenously in a group. The modified MECG, therefore, has a psychological aspect in it and the design preserves the richness of the message space, while maintaining the control of an anonymous interaction.

The central observation is that the highest level of coordination emerges in the presence of negative feedback. In the presence of disapproval ratings, players gradually increase their actions and in the end approach the payoff-dominant action. This result invites an analogy with the theoretical work of Kandel & Lazear (1992). In case of approval ratings, however, players' actions unravel towards the less efficient actions over time. Hence, in our paper, the negative mode of feedback constitutes the most effective coordination device for this special class of coordination game[65]. These results corroborate the widespread claims in psychology literature that (1) both positive and negative feedbacks (forms of negative and positive events, respectively) may have the potential to generate efficient behavior and (2) negative feedback is more potent than positive one. Moreover, any of the two types of feedback in our experiment produces better coordination outcome than no feedback. Therefore, it supports, by providing experimental evidence, the claim made in organizational theory that the existence of a feedback mechanism in a large group (organization) may enhance coordination.

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[65] This result stands in stark contrast to Weber et al. (2006) that experimentally demonstrates, in the context of MECG, that positive financial incentives are more effective at moving large groups away from inefficient equilibria than negative incentives.

The use of payoff irrelevant ratings and the increased levels of coordination is inconsistent with a subgame-perfect equilibrium notion in which players maximize their monetary payoffs in a self-interested manner. However, one can borrow insights from psychology literature to reconcile this apparent inconsistency between the assumption of ‘economic man’ and the higher levels of coordination obtained in the presence of these two ratings. For instance, Posner and Rasmusen (1999) observe that normative incentives or disincentives can often alter human behavior and produce the efficient outcome. Disapproval and approval ratings are examples of such normative disincentive and incentive schemes, respectively (in their discussion). The argument is that someone might lose utility directly from believing that others disapprove (do not approve) of her action and gain utility from believing that others approve (do not disapprove) of her action, regardless of whether others’ actions have material implications for her. Although, this observation may characterize the success of disapproval ratings in triumphing over the strategic uncertainty in the MECG, yet the failure of approval ratings to bring about the comparable level of efficiency casts considerable doubt on the explanatory power of this specific premise. This is especially so since this literature does not distinguish between the two in terms of their efficiency inducing effects. When the approval rating treatment is compared with no rating treatment, only then approval ratings appear to impact coordination, though to a limited extent.

Among other possible candidates that may help explain this asymmetrical result, communication and focal point hypotheses (as in Schelling (1960)) attain certain prominence. However, data from our experiment do not lend major support to these hypotheses as well. One may hypothesize that these ratings may have *communicated* participants' expectations concerning the future outcome of the game and thus have guided them towards the efficient levels of coordination. Another hypothesis could be that the mere exchange of these ratings may have created a focal point, which may have facilitated players' decisions to coordinate on more efficient equilibria. If communicative and/or focality of these ratings are at the core of the coordination success, then the *labeling* of these ratings should not mean anything. Then, approval and disapproval ratings should have produced, in principle, the same level of efficiency. However, the data do not mesh well with these hypotheses[66]. Hence, the *label*s of the ratings have some bearing, at least on group coordination. This, naturally begs, the question of why there exists an asymmetrical effect of negative and positive feedbacks on coordination.

This result, however, receives major support from a strand of literature in psychology that depicts the greater general potency of negative events and the positive-negative asymmetry effects. The principle extant in this literature that negative events (e.g., receiving criticism) are stronger than positive events (e.g., receiving praise) appears to be consistently supported across a broad range of psychological phenomena (Baumeister et al., 2001). The basic argument is that negative events, like negative feedbacks seem to mandate more attention, have powerful signaling value, and are generally weighted more heavily than positive events, like positive feedbacks. Taylor

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[66] However, a psychologist would argue that a negative form of feedback is inherently a more effective communication instrument than a positive form of feedback, as documented in this literature. The argument is that it commands more attention, may have higher signaling value.

(1991)'s *mobilization -minimization hypothesis* also supports this claim that people attempt to minimize events such as criticism (i.e., negative feedback) coming from others and distance themselves from them more than strive to maximize the events such as praise (i.e., positive feedback) conferred upon by others.

Our experiment demonstrates a case in point. Analysis suggests that (1) participants assign the two ratings in an asymmetric fashion (see Tables 4 and 5), and (2) the impact of negative ratings on future effort choices is much stronger than the positive ratings (see Table 4.6). These two particular findings generate two strikingly different levels of coordination. The asymmetrical effect of negative versus positive events can also be found at the core of prospect theory, as described in the prospect function and labeled as *loss aversion* (Kahneman & Tversky, 1979; Kahneman & Tversky, 1991). The basic tenet of loss aversion theory is very similar in nature to Taylor (1991)'s mobilization -minimization hypothesis.

Although these two theories to a certain extent share commonality, yet the theory of loss aversion typically requires an objective metric of value, almost always money, against which to measure subjective value whereas the mobilization-minimization hypothesis is often evaluated on a good-bad evaluation scale that may not be readily translated into a pecuniary scale (e.g., Anderson, 1966; Feldman, 1966). Therefore there is very little data on negativity dominance in the loss aversion literature (Rozin & Royzman, 2001) [67]. Unlike the loss aversion theory, in this paper I use a non-pecuniary

[67] The endowment effect allows one route around money, because it involves loss or gain of the same identity, and hence one can presume the objective value is equal in either case.

‘good-bad’ scale to study the dominance of negativity bias in the context of the MECG. The data generated in this regard indicate that individuals may have viewed these two types of feedbacks in an asymmetric fashion. The evidence may also suggest that criticism rather than praise relative to a reference point may exert a stronger influence on coordination behavior. Our paper is the first attempt in this direction that offers some data on the ‘aversion’ to negative feedback compared to positive feedback.

I do not advocate here any specific argument in favor of another for the success of negative feedback in yielding higher orders of coordination outcome. I believe that more than one phenomenon may be involved and I doubt that there is one theory to account for this result. Nonetheless, this experiment provides some evidence that these apparently immaterial expressions of emotions may have significant impact on players’ behavior. Thus, the observations made here entail a suggestion for future theoretical research. Although it is unclear whether there can ever be a consensus on how to model emotions analytically, this experiment raises an important issue for discussion: the potential role of different types of emotion feedbacks and their impact on the economic outcome.

DISCUSSION

The main objective of this dissertation is to investigate the behavior of economic agents in a variety of situations where coordination among agents' actions plays a crucial role. In order to accomplish this, I study, experimentally, three non-cooperative games in which miscoordination may result in inefficiency.

The first context is a market game in which sellers could tacitly coordinate their price choices by adopting price-matching guarantees. Thus, these guarantees act as a coordination device among sellers who cannot communicate with each other in an explicit manner. Against this backdrop, the first chapter develops an experimental framework with induced cost and demand conditions in the laboratory and tests the collusive theory of price-matching guarantees. The main focus is to investigate the collusive potential of the guarantees from an experimental viewpoint. There is, however, a second and a more conceptual rationale. Theories in the literature suggest that there exists multiple equilibria (i.e., a whole set of equilibrium prices between the competitive and the monopoly price) when all the sellers adopt the guarantee. Theoretical prediction in this case fails to pin down the actual behavior of players *a priori*. In contrast, laboratory experiments are often appropriate for investigating problems of multiplicity of equilibria. Therefore, the use of laboratory techniques might be able to shed light on the behavior of equilibrium prices that is strongly indeterminate in nature. The results that emerge from this experiment extend great support to the main theoretical prediction that these guarantees yield anti-competitive behavior among sellers. Additionally, results exhibit considerable evidence of coordination success.

One question that may naturally arise is that to what extent the results from this experiment can be extended to more complex real market conditions? Although, it must be noted that the research aim of this chapter was to demarcate the effects of price-matching guarantees in a simple setting, which would, in turn, guide the future path of research in this area. Therefore, this chapter started out with a very basic model that naturally lacks many of the complexities of real market. As a result, the conclusion of this study cannot be more than a starting point. To importantly add to the results from this study, future research should advance mainly in two directions. First, it would be interesting to see if the same qualitative results in terms of prices could be obtained by allowing many possible asymmetries (e.g., cost asymmetry) between sellers. Second, Hviid and Shaffer (1999) argue that the collusive impact of PMGs will be completely undermined if buyers incur costs in terms of hassles to invoke these guarantees. Examining the collusive potential of PMGs by allowing buyer heterogeneity in terms of hassle costs would subject the collusive theory to a stringent test. This last point has been investigated in a closely related study that is the matter of the second chapter.

Most of these theoretical models in this literature implicitly assume that firms automatically match rivals' low price, which implies that invoking these guarantees is almost a 'no hassle' task for a buyer. However, HS (p.490) maintain a different view. They argue that even an arbitrarily small level of positive hassle costs, borne by all buyers to invoke these guarantees, render PMGs much less effective than the dominant view in the literature would suggest and therefore firms will no longer find it optimal to set the market price above marginal cost in the presence of PMGs.

The second chapter experimentally investigates whether these guarantees can nevertheless sustain collusion among sellers in the presence of hassle costs. To achieve this, I develop four stylized one-shot price competition models that have testable hypotheses. The first model directly captures the implications of HS in which all buyers bear positive hassle costs. The last three models deal with the theoretical analysis of hassle costs in symmetric markets by introducing heterogeneity on the buyer side, previously unexplored in the literature. In particular, I segment buyers into two groups: positive and zero hassle cost buyers. I, then, alter the fraction of such buyers for each of the three models, derive testable predictions, and test all four models in the laboratory. While theory predicts that the competitive price should emerge in equilibrium in all four models, any significant difference in prices among them would provide evidence against this sharp prediction.

The main observation is that there exist significant price differences across four market models. Specifically, market prices increase significantly as the number of positive hassle cost buyers is reduced across treatments. Moreover, the PMG adoption pattern varies inversely with the number of positive hassle costs buyers. Thus, the presence of buyers who incur positive hassle costs to invoke these guarantees acts as a market-disciplining device. In view of these results, one may ask why firms adopt guarantees that impose costs on buyers, since these costs may have the effect of lowering prices. This suggests that these guarantees may be adopted for reasons other than tacit collusion. One reason may be that firms adopt these guarantees and use hassle costs as a device to screen buyers in terms of their willingness to pay the lowest price for the product. This suggests that the guarantees may be adopted for price discrimination purposes. There are a number of directions for future research. First, future research should examine if the same qualitative results could be obtained by allowing cost asymmetries among sellers. Second, to the author's knowledge there is no previous research that tests the price-discrimination theory of price-matching guarantees.

The third chapter tests a well-known paradox, namely, the Braess paradox. The paradox argues that increasing the capacity of a network in the form of adding a new route to a given network (which is presumed to be helpful for the network users) may in fact increase the cost of each network user under certain parametric restrictions relative to the case when this new route was not available. The experiment tests this theoretical result in the simplest possible symmetric network, called, the critical minimum network.

While the results from the experiment provide major support to the theoretical result as envisaged in the Braess paradox, it is natural to wonder whether the paradox is realized in more complex networks. Therefore, it will be interesting to check the robustness of the paradox in richer asymmetric architecture. There are other possible extensions that could be carried out in future. In this chapter, I restrict the route cost functions to be linear in nature. One could test the power of the paradox by using non-linear cost functions as well. Also, this chapter makes no attempt to analyze the learning process that may have guided the route choice behavior of participants in the experiment. For example, one could imagine that in a situation where players' decisions are inter-dependent in nature, each player may have a subjective belief about how the other players will behave. The crucial issue is formulating beliefs about other players' future route choices. The process of generating these beliefs crucially depends upon the 'cognitive' capacity of an individual player. A *k-level* thinking model may capture this strategic thinking of a player based on her 'smartness'. For example, in our context, a player with level-0 intelligence may choose a route randomly. A level-1 player, who believes that all other players are level-0 types, will choose a best response to uniform play. A level-2 type, who believes that all other players are level-0 and level-l types, will choose a best response to this subjective belief etc. Thus, a level- (n + 1) type is smarter than a level-n type in being able to think about the behavior of level-n types. However, no player can anticipate what equally smart or smarter players will do. This particular learning process may account well for the route choice behavior of players at an individual level in our context.

In the last chapter, I explore, experimentally, how the problem of coordination failure, as embedded in the MECG, can be overcome using *structured* feedbacks related to individual performance among members ('employees') of a large group ('firm'). I vary the performance feedback variable along a dimension – namely, the quality of feedback. I examine two types of feedbacks - negative and positive. The experiment focuses on two questions: (1) can positive and/or negative feedback solve the large group coordination problem as embedded in the MECG? (2) Are asymmetrical effects of positive and negative feedbacks on coordination outcome observed? The central observation is that the highest level of coordination emerges in the presence of negative feedback. In case of approval ratings, however, players' actions unravel towards the less efficient actions over time. These results receive major support from a strand of literature in psychology that depicts the greater general potency of negative events and the positive-negative asymmetry effects.

APPENDIX A: Figures

List of Figures

FIGURE 1.1: Comparison of average winning prices by session and treatment

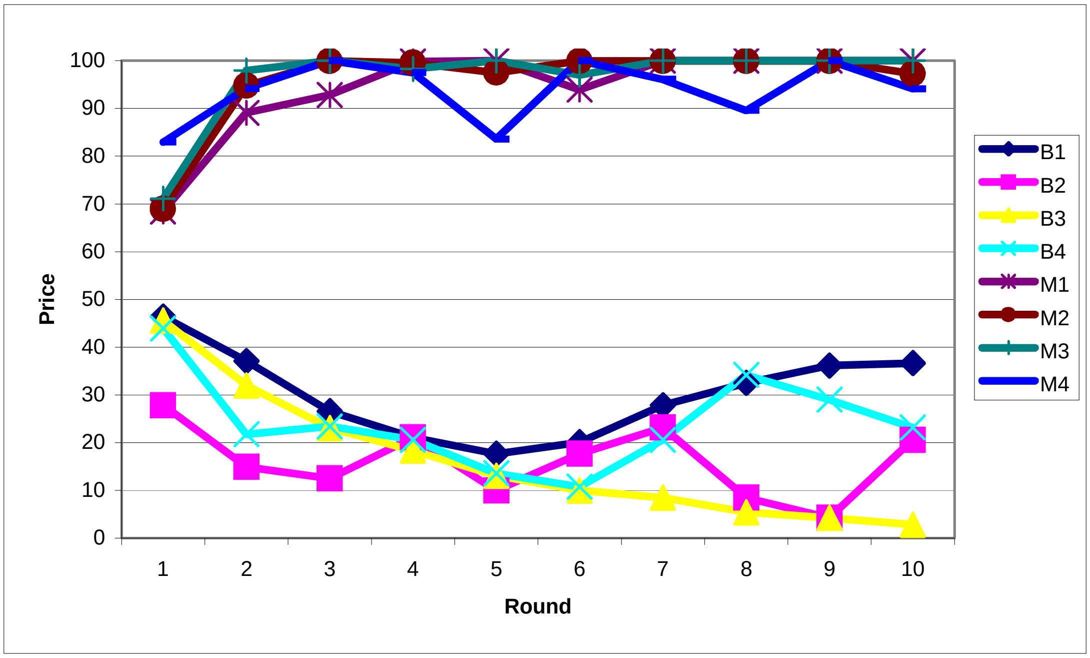


FIGURE 1.2: Comparison of average posted prices by session and treatment

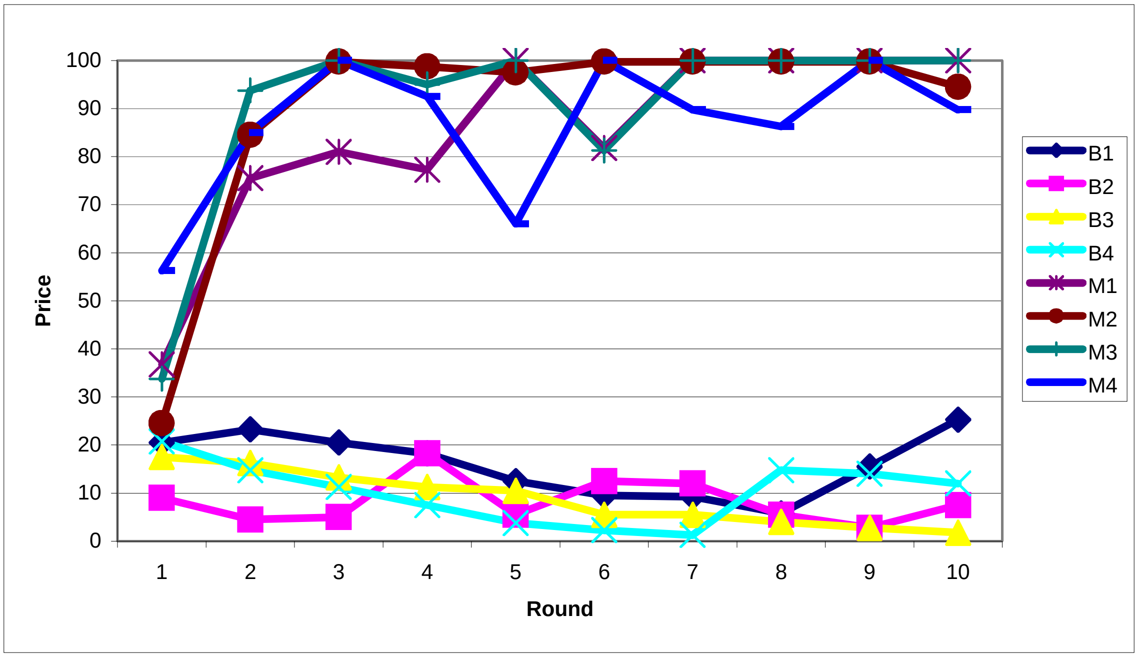


FIGURE 1.3: Average winning prices summed across sessions by treatment

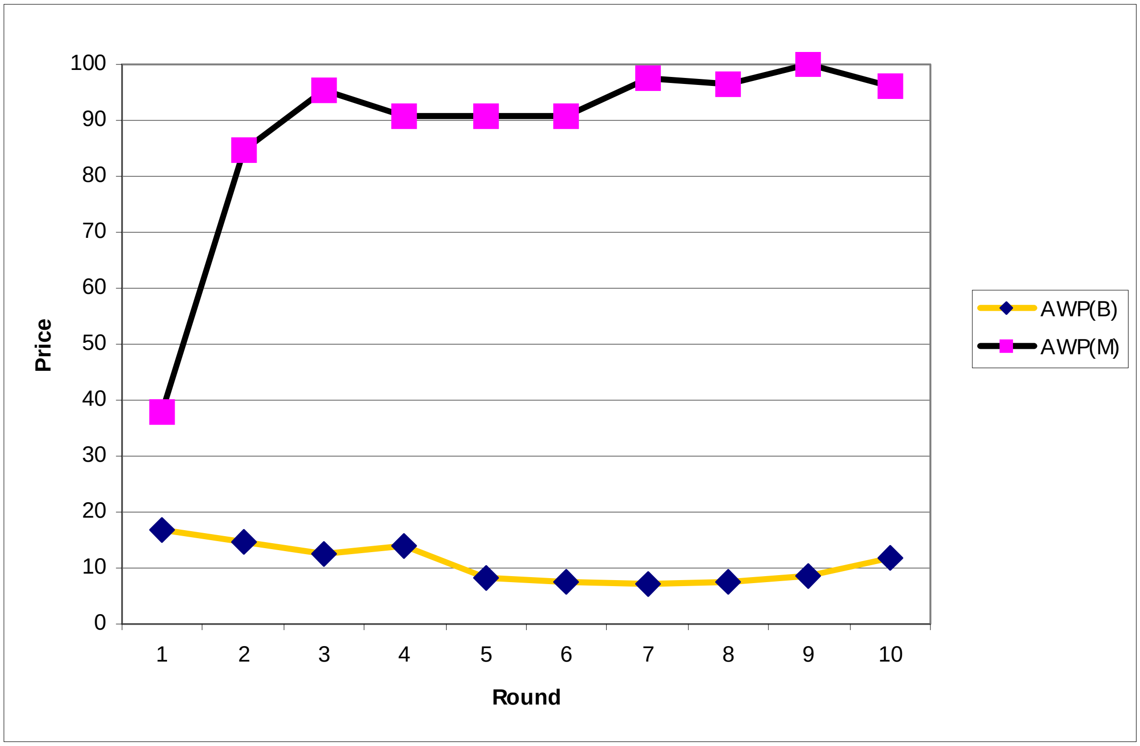


FIGURE 1.4: Average posted prices summed across sessions by treatment

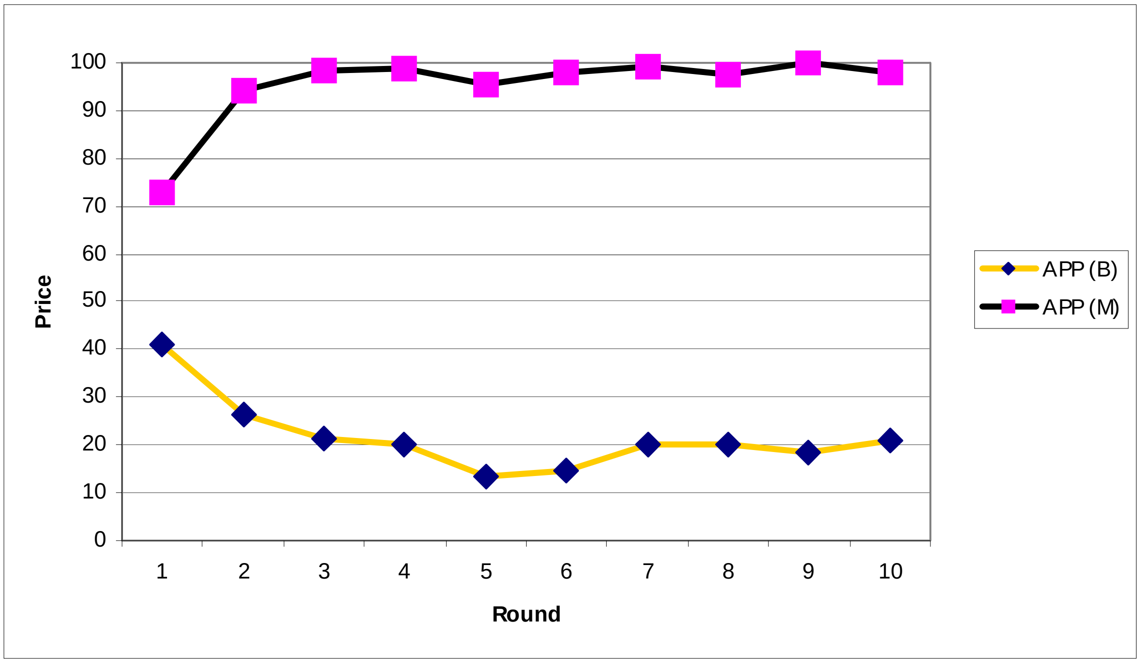


FIGURE 1.5: Price-matching adoption patterns by PMG session

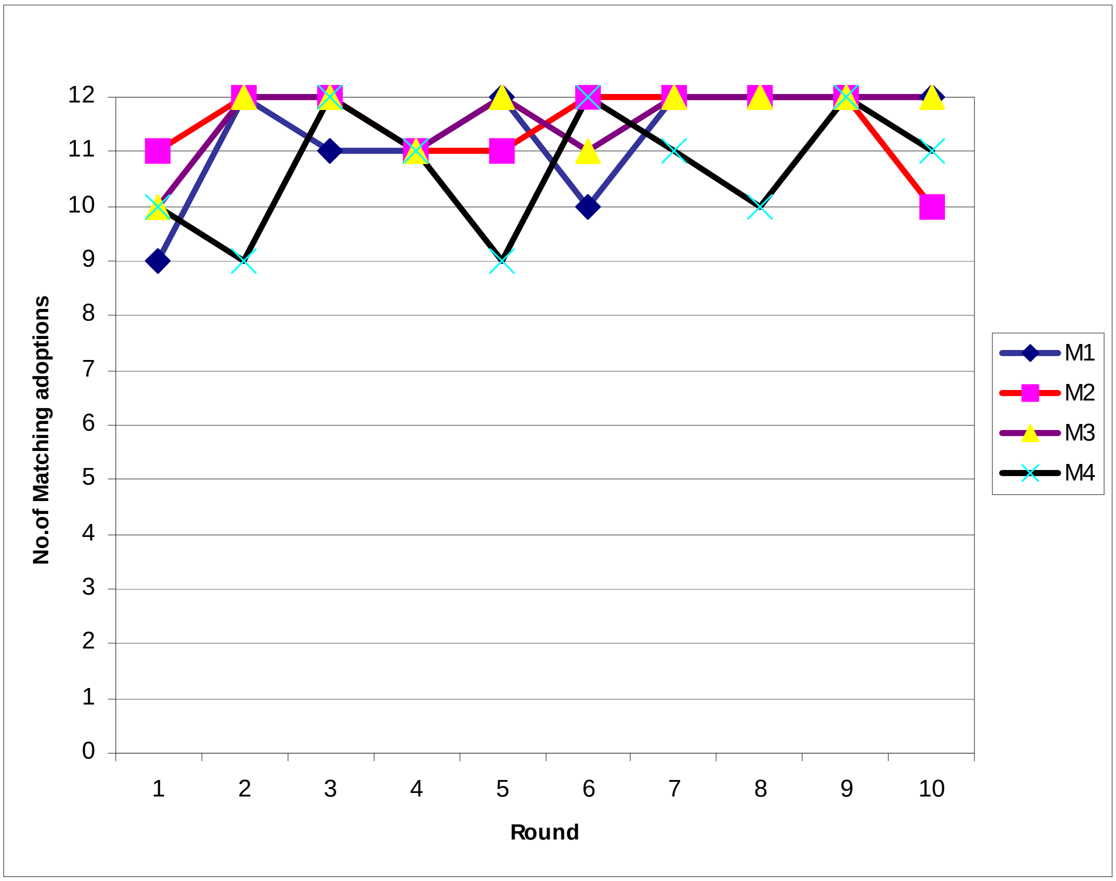


FIGURE 1.6: Average price-matching adoptions in PMG treatment

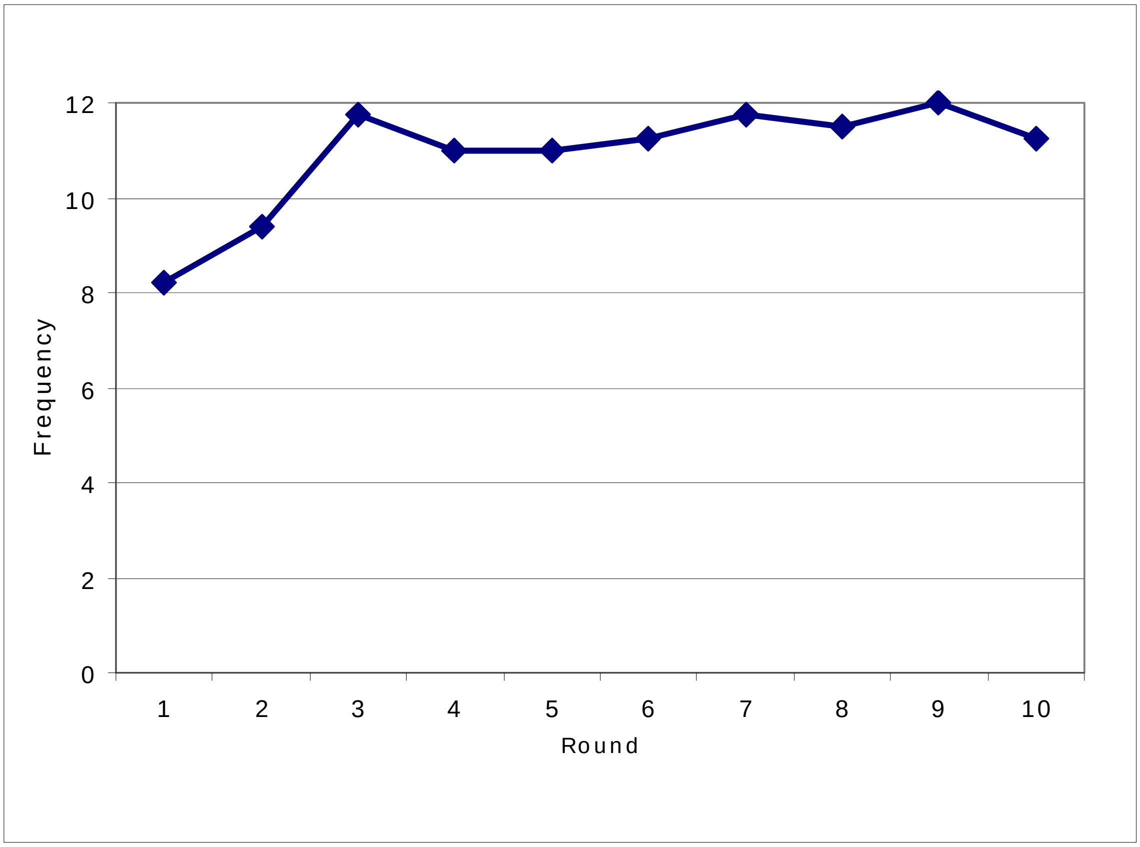


FIGURE 1.7: Average profit and average adoption rates in PMG treatment

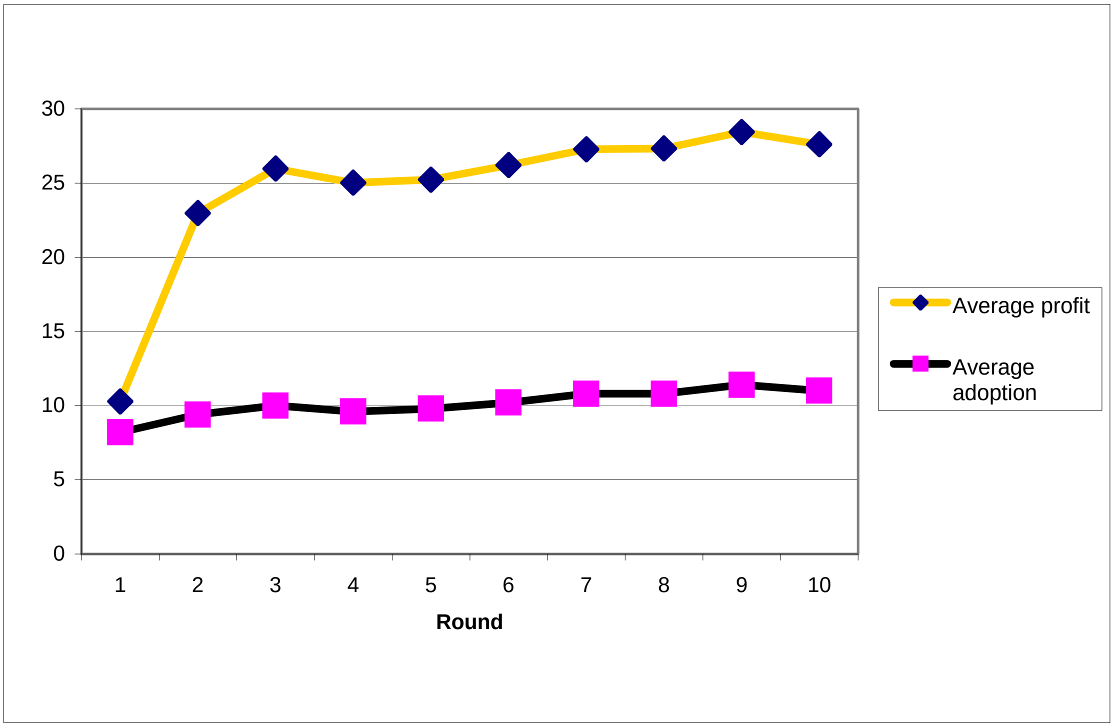


FIGURE 1.8: The % of markets under the subgame {PMG, PMG, PMG} & the % of such markets for which market winning price is 100

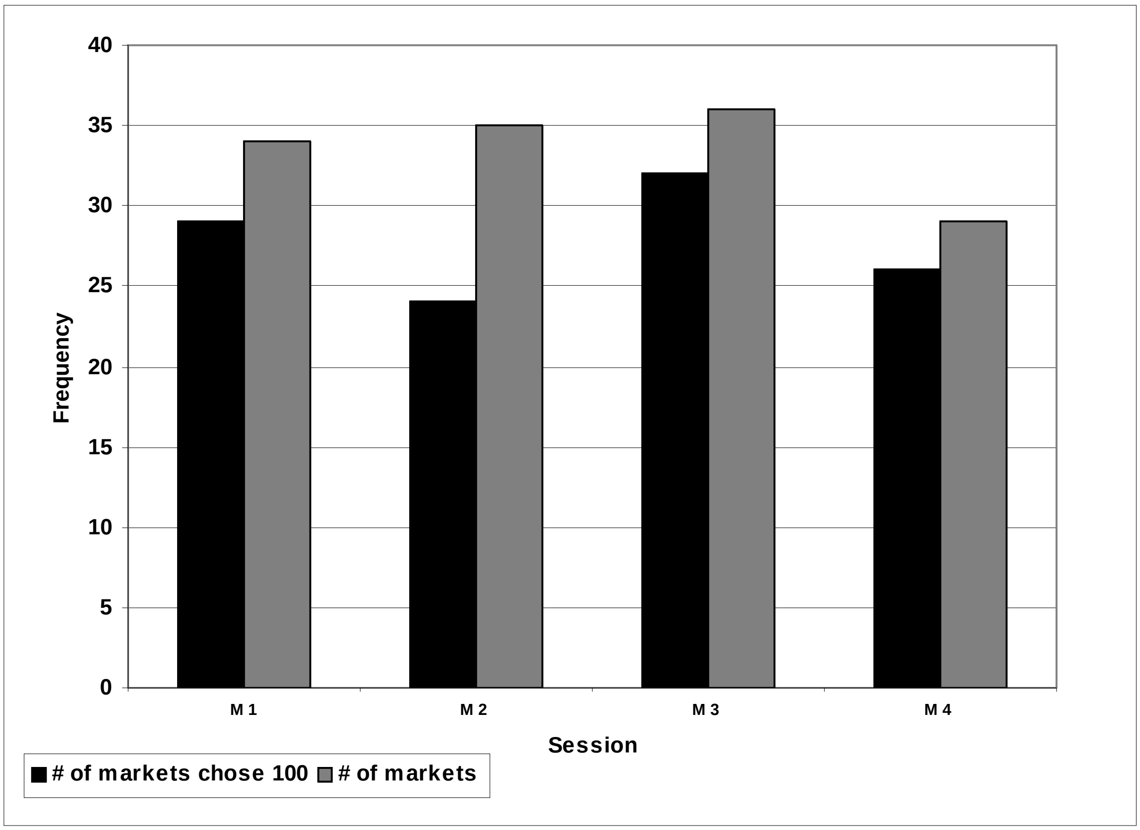


FIGURE 2.1: Average winning prices by treatment

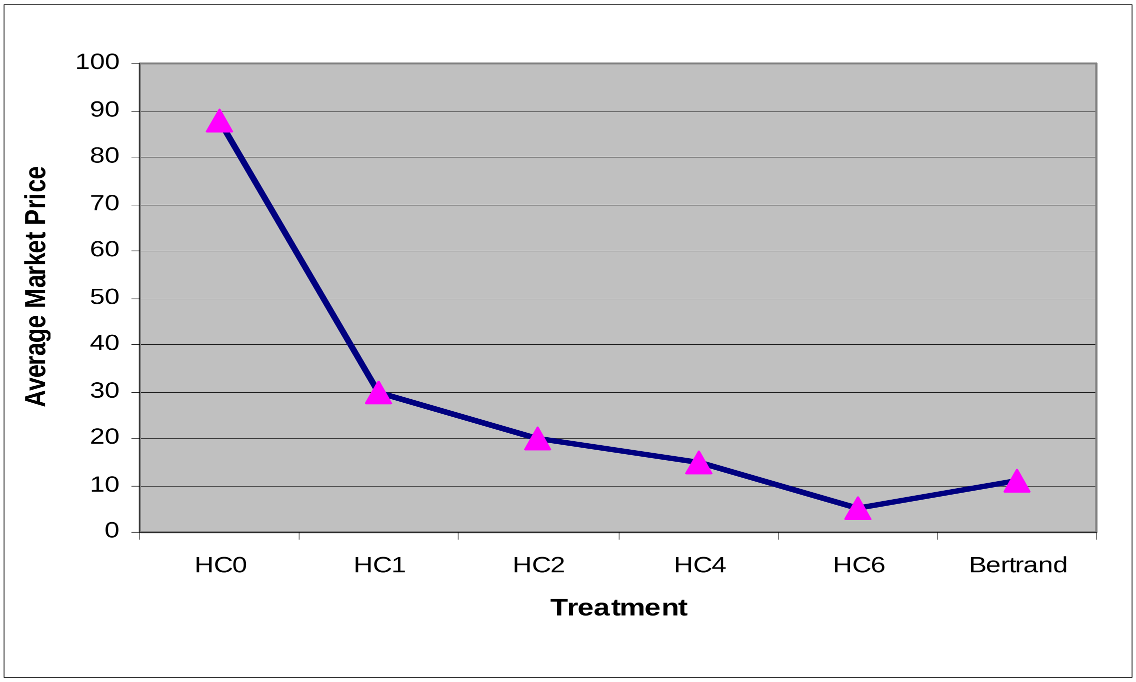
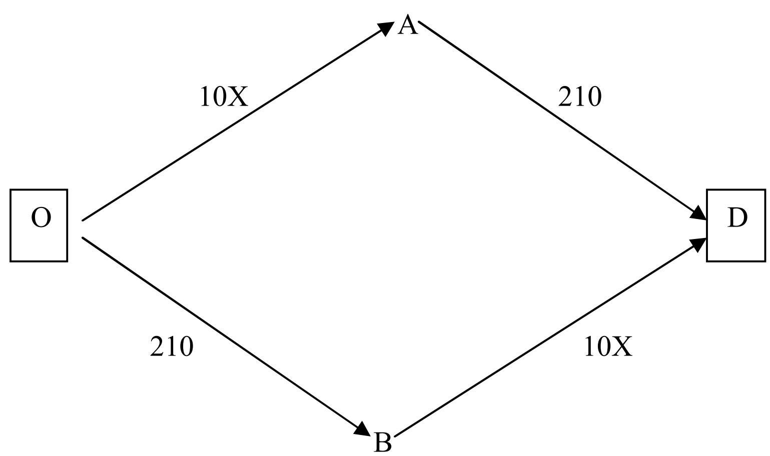


FIGURE 3.1: Basic and augmented networks for Games A and B in Experiment 1

Network A



Network B

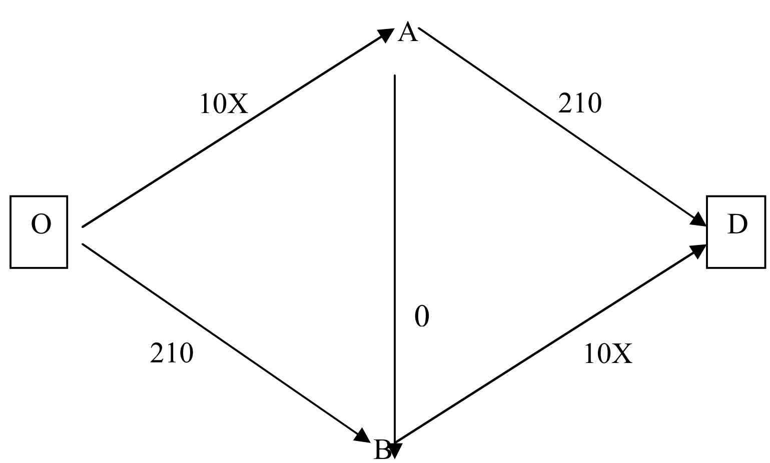


FIGURE 3.2: Mean number of subjects choosing each route in Game 1A Condition ADD

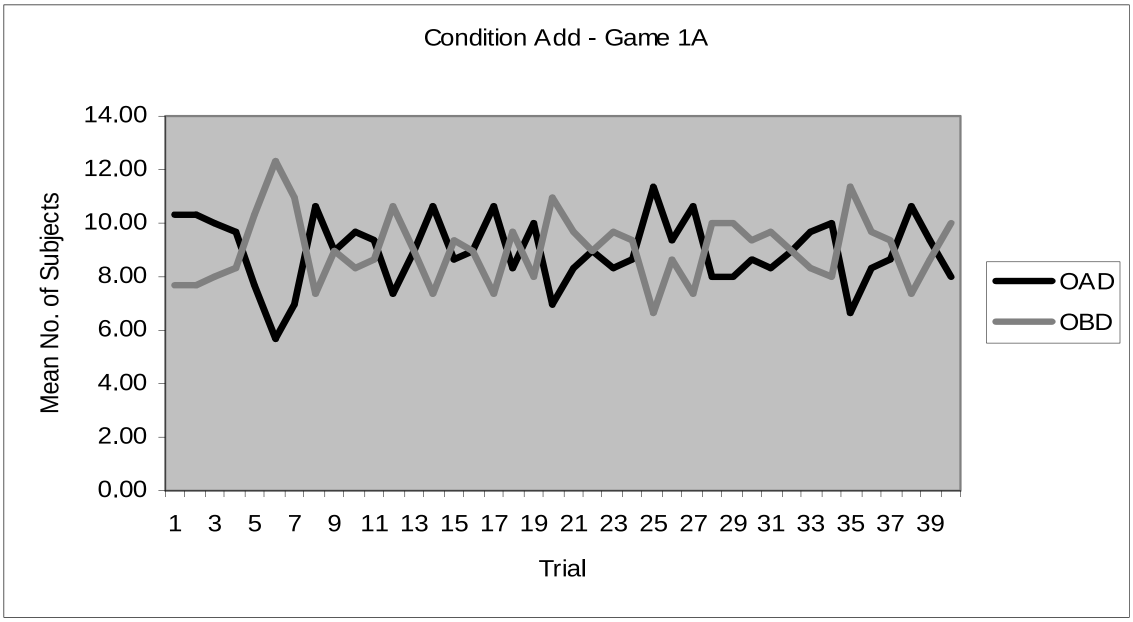


FIGURE 3.3: Mean number of subjects choosing each route in Game 1A

Condition DELETE

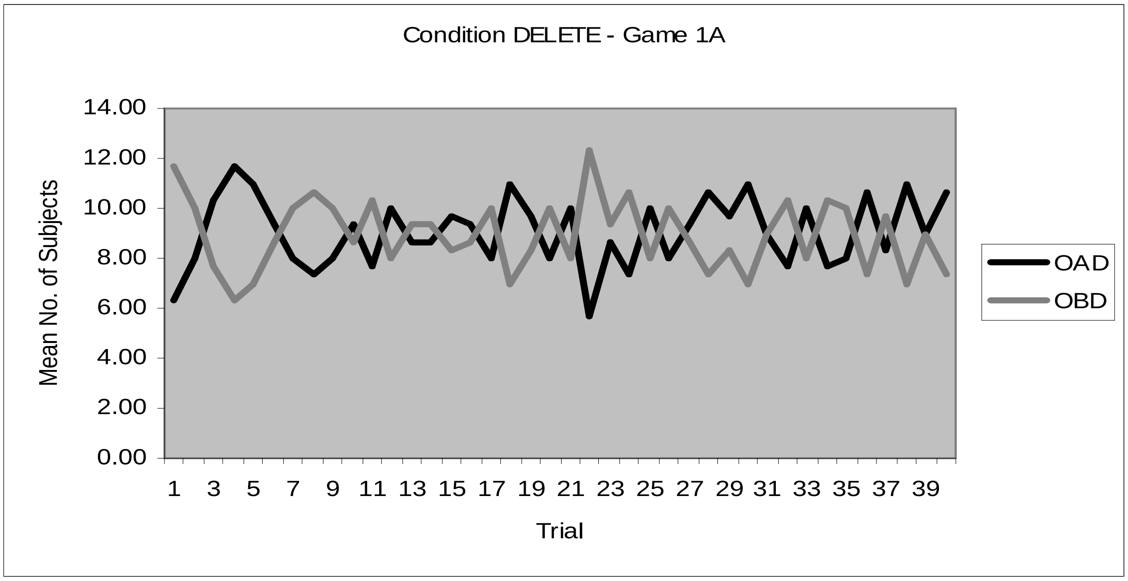


FIGURE 3.4: Mean number of subjects choosing each route in Game 1B Condition ADD

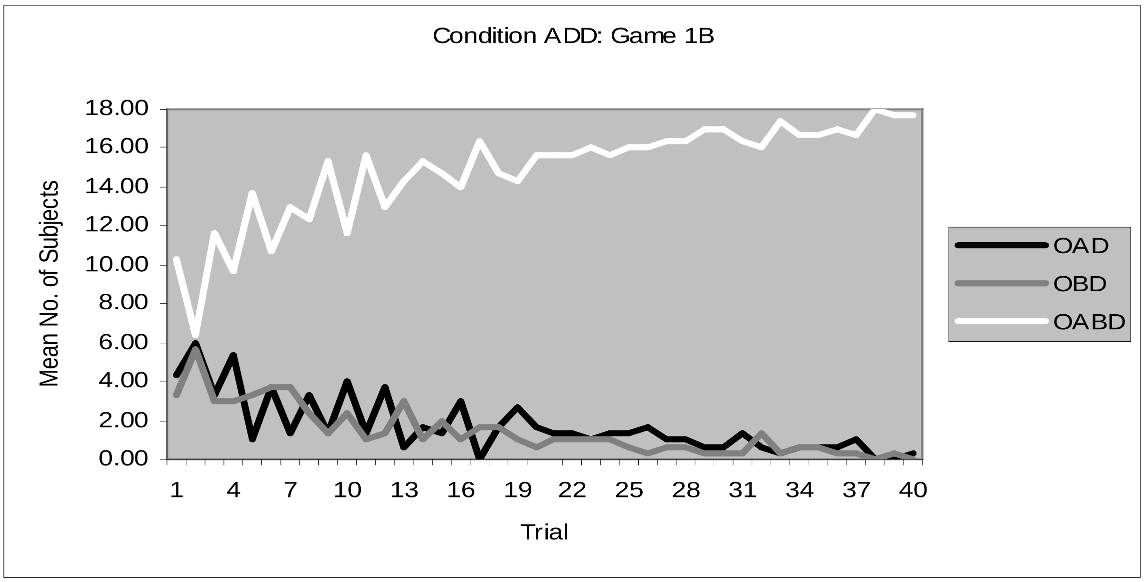


FIGURE 3.5: Mean number of subjects choosing each route in Game 1B Condition DELETE

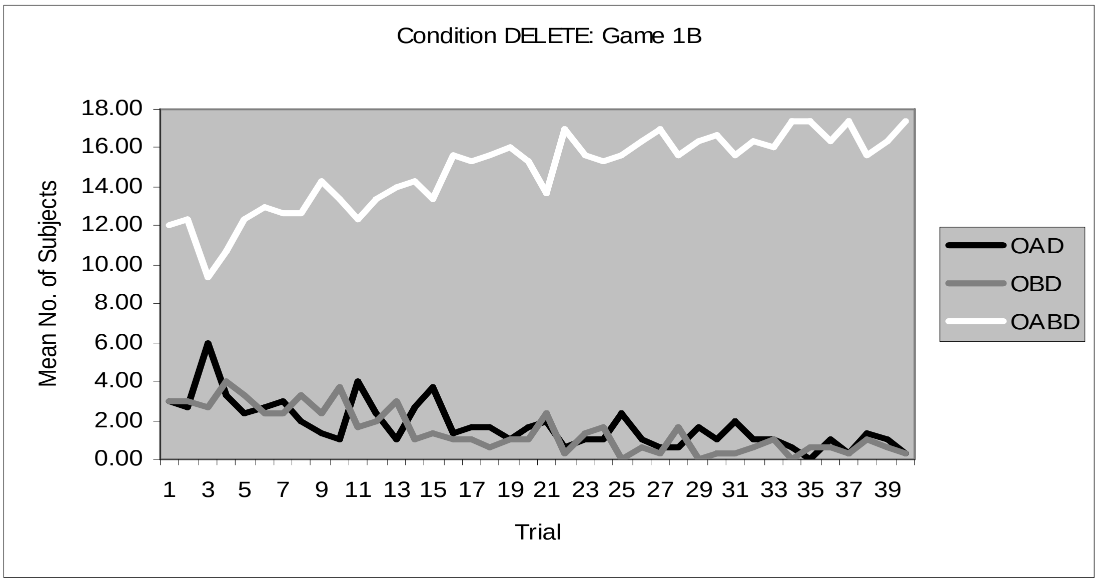


FIGURE 3.6: Mean payoff by game and trial in Experiment 1: Condition ADD

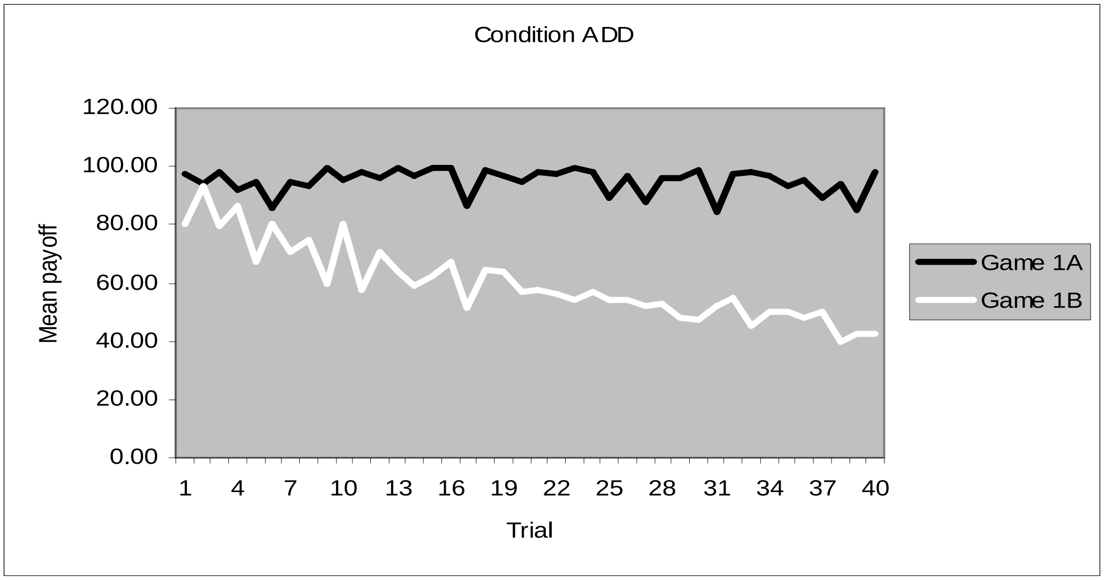


FIGURE 3.7: Mean payoff by game and trial in Experiment 1: Condition DELETE

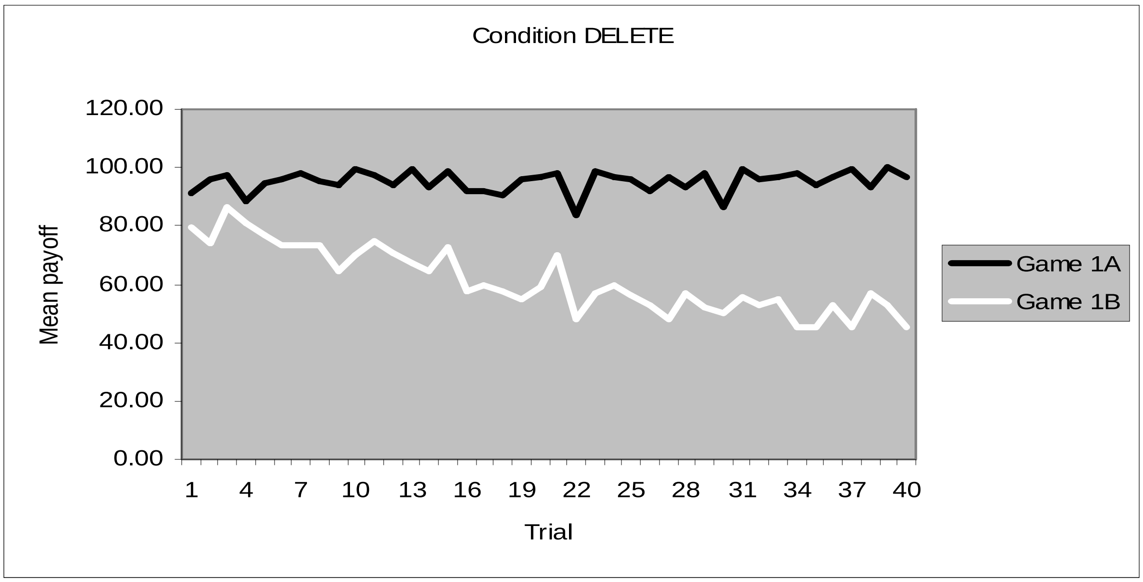


FIGURE 3.8: Running mean number of switches by game in Experiment 1

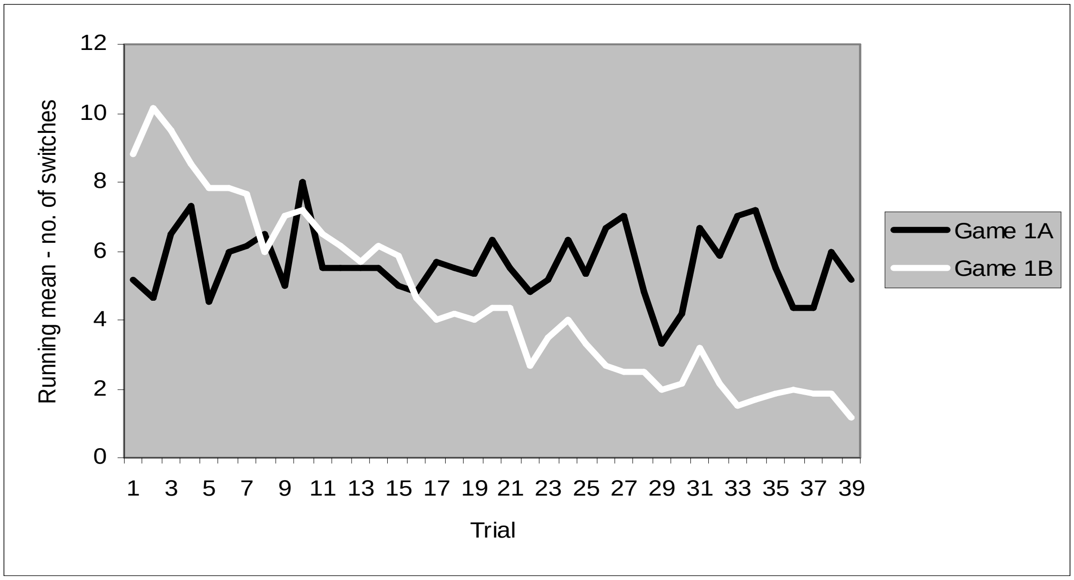


FIGURE 3.9: Frequency distribution of number of subjects in Game 1A choosing route (*O*-*A*-*D*)

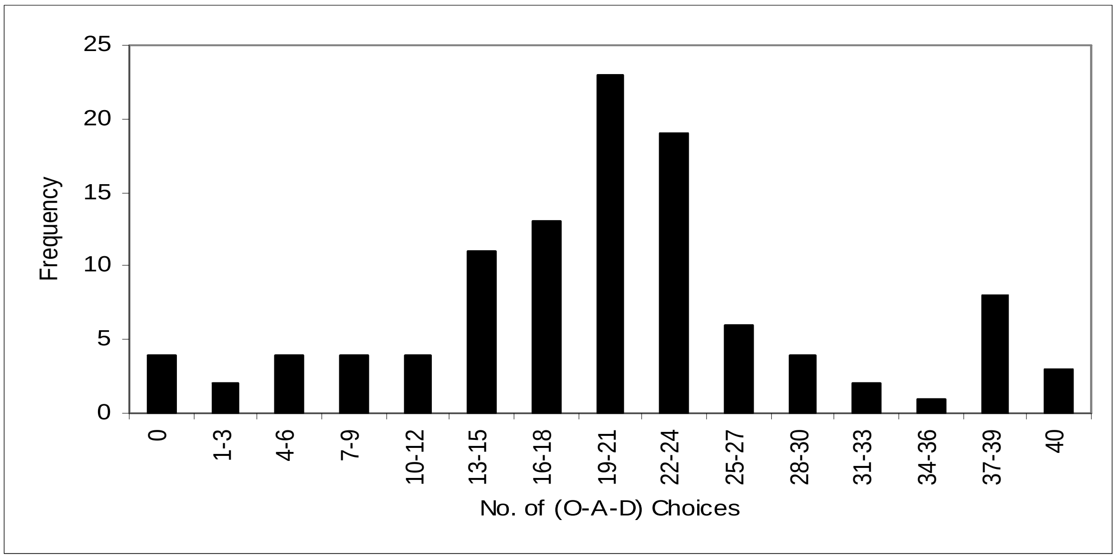


Figure 4.1: Time series of average minimum choices in treatments B, D and A

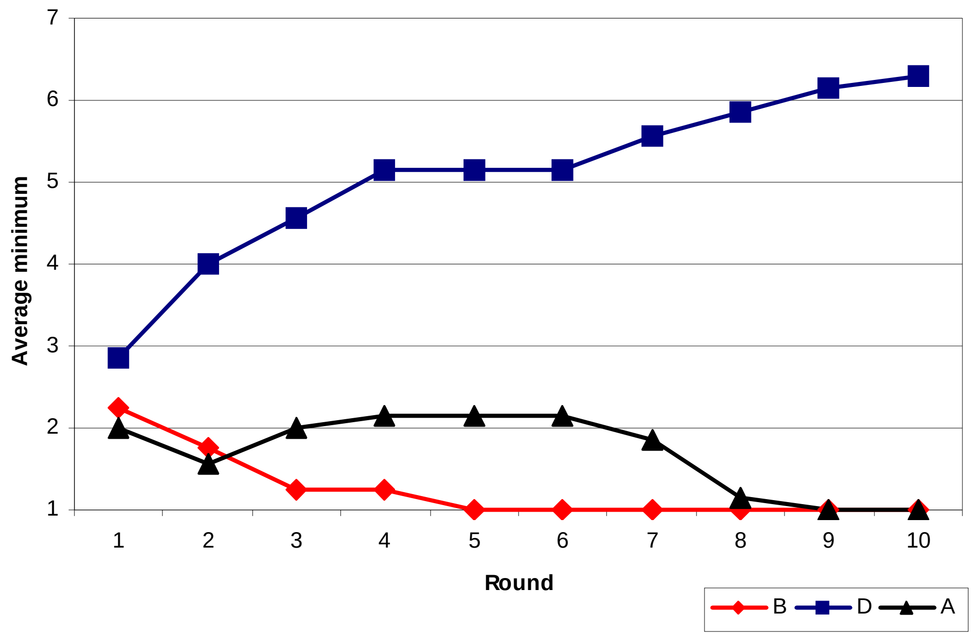
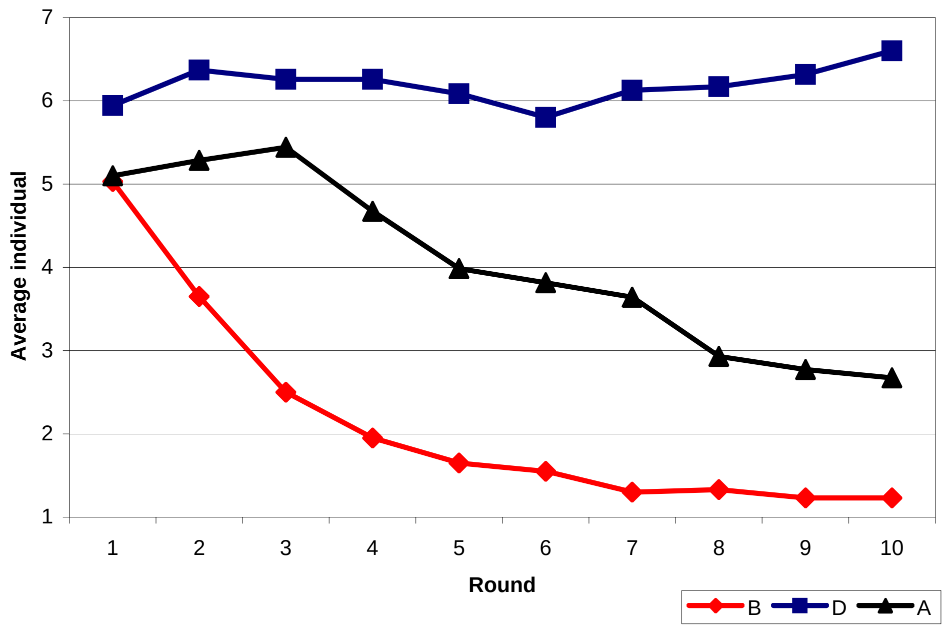


Figure 4.2: Time series of average individual choices in treatments B, D and A



APPENDIX B: Tables

List of Tables

TABLE 1.1: The two market models

Game	Key Features	Equilibria
Benchmark	One stage; simple Bertrand type game	(1, 1, 1)
Price-Matching	Two stages; collusion is possible	
	a) Guarantee adoption by only one seller	(1, 1, 1)
	b) Guarantee adoption by two sellers	(1, 1, 1), (2, 2, 2) or (3, 3, 3)
	c) Guarantee adoption by all sellers	Any symmetric price triplet between (1, 1, 1) and (100, 100, 100) is possible. This is based on effective prices.

TABLE 1.2: A pairwise comparison of posted prices in the first round across sessions using Mann-Whitney U-test based on ranks[a]

	B1	B2	B3	B4	M1	M2	M3	M4
B1	-	2.071*	-0.086*	0.346*	-1.703**	-2.009**	-1.703**	-3.406**
B2		-	-1.587*	-1.616*	-2.713**	-2.713**	-3.031**	-3.781**
B3			-	0.433*	-1.818**	-1.645**	-2.165**	-2.8**
B4				-	-1.789**	-1.934**	-2.078**	-3.723**
M1					-	-0.202*	-0.173*	-1.125*
M2						-	-1.068*	-0.779*
M3							-	-1.564*
M4								-

a. The null hypothesis is that all posted prices come from the same distribution. The numbers in the cells are the z-statistics.

**indicates that the null hypothesis is rejected in favor of the alternative that prices from the PMG sessions are stochastically higher than the Benchmark sessions (at 5% level of significance).

*indicates that the null hypothesis cannot be rejected in favor of the alternative that they are different (at 5% level of significance).

TABLE 1.3: Average winning and posted prices by session and treatment

Session	PMG Treatment (M)		Benchmark Treatment (B)	
	AWP	*APP*	*AWP*	*APP*
Session 1	85.23	93.66	16.03	30.21
Session 2	89.85	95.76	8.25	16.05
Session 3	90.55	96.43	8.83	16.31
Session 4	86.55	93.79	10.23	24.09

TABLE 2.1: Equilibrium predictions of all market games

Model	Key Features			Equilibrium Market Prediction
	Stages	Positive HC Buyers	Zero HC Buyers	
HC0	2	0	6	(1,1,1)…(100,100,100)
HC1	2	1	5	(1,1,1) OR (2,2,2)
HC2	2	2	4	(1,1,1) OR (2,2,2)
HC4	2	4	2	(1,1,1)
HC6	2	6	0	(1,1,1)
Bertrand	1	-		(1,1,1)

TABLE 2.2: Sequence of the treatments by session

	Round		
Session	1-10	11-20	21-30
Session 1	HC6	HC4	HC2
Session 2	HC2	HC4	HC6
Session 3	HC4	HC2	HC6
Session 4	HC6	HC2	HC4
Session 5	HC2	HC6	HC4
Session 6	HC4	HC6	HC2
Session 7	HC1	-	-
Session 8	HC1	-	-
Session 9	HC1	-	-
Dugar Session 1	HC0	-	-
Dugar Session 2	HC0	-	-
Dugar Session 3	HC0	-	-
Dugar Session 4	HC0	-	-
Dugar Session 5	Bertrand	-	-
Dugar Session 6	Bertrand	-	-
Dugar Session 7	Bertrand	-	-
Dugar Session 8	Bertrand	-	-

TABLE 2.3: Descriptive statistics by treatment

	Bertrand (Dugar 2005)	HC6	HC4	HC2	HC1	HC0 (Dugar 2005)
All Rounds						
Mean Posted Price	21.66	16.54	31.24	38.11	48.87	94.91
Mean Market Price	10.83	5.29	14.83	19.94	29.93	88.00
Price Matchers	-	24%	55%	67%	78%	94%
First Round						
Mean Posted Price	40.94	35.7	49.75	44.25	56.5	72.83
Mean Market Price	16.94	12.67	23.75	20.67	26.17	37.81
Price Matchers	-	31%	53%	50%	83%	88%
Last Round						
Mean Posted Price	20.81	10.61	23.31	39.28	46.44	97.83
Mean Market Price	11.63	1.58	8.67	22.25	29.82	96.06
Price Matchers	-	17%	61%	67%	83%	94%

TABLE 2.4: Regression results by model

	Dependent Variable			
	Market Price			Number of Price Matchers
	Model 1	Model 2	Model 3	Model 4
Constant Term	5.29 (3.23)	2.54 (0.89)	-0.59 (-0.21)	-0.48 (-3.05)
Treatment Bertrand	5.54 (2.55)	1.21 (0.48)	4.91 (1.94)	-
Treatment HC4	9.54 (4.11)	9.54 (4.17)	3.84 (1.59)	0.82 (6.35)
Treatment HC2	14.65 (6.32)	14.65 (6.40)	6.73 (2.62)	1.01 (8.11)
Treatment HC1	24.64 (8.68)	20.31 (6.55)	9.51 (2.73)	1.26 (8.32)
Treatment HC0	82.71 (38.13)	78.38 (31.13)	64.48 (19.48)	1.46 (10.83)
Number of Price Matchers	-	-	6.22 (6.26)	-
Order 2	-	-5.62 (-2.45)	-5.67 (-2.54)	0.01 (0.06)
Order 3	-	-7.37 (-3.22)	-9.75 (-4.31)	0.25 (2.40)
Round 2	-	8.77 (3.01)	8.77 (3.09)	0.00 (0.00)
Round 3	-	10.27 (3.52)	9.60 (3.37)	0.07 (0.54)
Round 4	-	7.23 (2.48)	6.73 (2.37)	0.06 (0.41)
Round 5	-	5.35 (1.83)	5.18 (1.82)	0.02 (0.14)
Round 6	-	5.76 (1.97)	4.83 (1.70)	0.10 (0.74)
Round 7	-	7.64 (2.62)	6.63 (2.33)	0.11 (0.80)
Round 8	-	8.61 (2.95)	8.44 (2.97)	0.02 (0.14)
Round 9	-	9.39 (3.22)	7.80 (2.73)	0.17 (1.25)
Round 10	-	7.76 (2.66)	7.17 (2.52)	0.06 (0.48)
Adjusted R square	.748	.754	.767	-
Number of observations	740	740	740	580

* Numbers in the parentheses are the t-statistics

TABLE 2.5: Direct and indirect treatment effects

Treatment	HC4	HC2	HC1	HC0
Effect of PMG Adopters on Market Price	6.22	6.22	6.22	6.22
Treatment Effect on the Number of PMG Adopters	0.82	1.01	1.26	1.46
Indirect Effect	5.10	6.28	7.84	9.08
Direct Effect	3.84	6.73	9.51	64.48
Imputed Total Effect	8.94	13.01	17.34	73.56
% Direct Effect	43%	52%	55%	88%
% Indirect Effect	57%	48%	45%	12%

TABLE 3.1: Mean values of summary statistics by game (1A and 1B), session (1-3), and condition (ADD vs. DELETE) in Experiment 1

	Game 1A		Game 1B		
Statistic	Route (*O-A-D*) Mean No. of choices	Mean No. of switches by subject	Route (*O-A-D*) Mean No. of choices	Route (*O-B-D*) Mean No. of choices	Mean No. of switches by subject
Session 1 Cond. ADD	20.06 (8.02)	12.94 (5.14)	3.22 (2.64)	2.89 (2.45)	9.94 (7.41)
Session 2 Cond. ADD	21.83 (8.87)	11.56 (5.28)	4.56 (3.76)	3.39 (1.79)	10.78 (6.3)
Session 3 Cond. ADD	20.28 (10.75)	10.89 (7.28)	3.61 (1.94)	3.28 (2.56)	9.44 (5.23)
Session 1 Cond. DELETE	20.22 (11.36)	11.33 (7.22)	3.39 (2.97)	3.67 (3.63)	9.39 (6.61)
Session 2 Cond. DELETE	20.06 (11.10)	11.72 (8.37)	4.06 (5.58)	2.72 (1.96)	8.78 (6.48)
Session 3 Cond. DELETE	20.11 (7.68)	14.78 (5.20)	4.11 (3.77)	3.28 (2.61)	11.28 (9.29)

(standard deviations in parentheses)

TABLE 3.2: Mean number of subjects (out of 18) choosing each route by game (1A and 1B) and condition (ADD vs. DELETE) in Experiment 1

Condition	Game 1A Two Routes		Game 1B Three Routes		
	(*O-A-D*)	(*O-B-D*)	(*O-A-D*)	(*O-B-D*)	(*O-A-B-D*)
ADD	9.04 (2.15)	8.96 (2.15)	1.70 (1.86)	1.49 (1.68)	14.82 (2.79)
DELETE	9.08 (2.08)	8.92 (2.08)	1.75 (1.51)	1.45 (1.49)	14.82 (2.27)
Mixed-strategy equilibrium	9 (2.12)	9 (2.12)	0	0	18

(standard deviations in parentheses)

Table 4.1: The MECG payoff table

		7	6	5	4	3	2	1
YOUR CHOICE	7	$1.30	$1.10	$0.90	$0.70	$0.50	$0.30	$0.10
	6	---	$1.20	$1.00	$0.80	$0.60	$0.40	$0.20
	5	---	---	$1.10	$0.90	$0.70	$0.50	$0.30
	4	---	---	---	$1.00	$0.80	$0.60	$0.40
	3	---	---	---	---	$0.90	$0.70	$0.50
	2	---	---	---	---	---	$0.80	$0.60
	1	---	---	---	---	---	---	$0.70

Table 4.2: Action adaptations by groups under treatments B, A and D

	Treatment D	Treatment B	Treatment A
Number of groups	7	4	7
Minimum players	4.29	5.07	4.97
% of lower actions	3.82	8.63	6.14
% of equal actions	48.52	88.80	65.91
% of higher actions	47.66	2.57	28.09
Non-minimum players	5.71	4.93	5.03
% of lower actions	26.89	54.79	48.63
% of equal actions	51.33	37.12	41.91
% of higher actions	21.78	8.09	9.46

Table 4.3: Relationship between the magnitude of approval or disapproval points received by a player and the deviation of that player's action from the group minimum

Dependent variable: Points received by player *i* in round *t*		
	Disapproval	Approval
Constant	17.17*** (25.67)	21.28*** (25.21)
Difference between player *i*'s action and the minimum action in round *t*	-1.45*** (-5.38)	3.17*** (13.7)
$Dummy_{it}$ that assumes a value 1 if *i* is a minimum player in round *t* and 0 otherwise	1.44*** (5.36)	-3.32*** (13.69)

Notes: t-statistics are in parentheses.
**** Significant at the 1% level.*

Table 4.4: Relationship between the difference in actions between players *i* and *j* and the assignment of disapproval points by player *i* to player *j* in round *t*

Dependent variable: Points assigned by player *i* to player *j* in round *t*		
	Model 1	Model 2
	$DiffAC_{ij,t} > 0$	$DiffAC_{ij,t} \leq 0$
Constant	3.55*** (37.33)	4.43***
Difference in action values of players *i* and *j* in round *t*	0.39*** (9.58)	-0.16*** (-2.75)
Interaction variable with the minimum player dummy	-	-0.14** (-2.14)

Notes: t-statistics are in parentheses.
**** Significant at the 1% level.*
*** significant at the 5% level.*

Table 4.5: Relationship between the difference in actions between players *i* and *j* and the assignment of approval points by player *i* to player *j* in round *t*

Dependent variable: Points assigned by player *i* to player *j* in round *t*		
	Model 1	Model 2
	$DiffAC_{ij,t} > 0$	$DiffAC_{ij,t} \leq 0$
Constant	2.37*** (21.94)	1.83*** (11.56)
Difference in action values of players *i* and *j* in round *t*	- 0.19*** (5.08)	0.09*** (-4.02)
Interaction variable with the minimum player dummy	-	0.11** (-5.91)

Notes: t-statistics are in parentheses.
**** Significant at the 1% level.*
*** significant at the 5% level.*

Table 4.6: Relationship between the change in actions between two consecutive rounds [*t* and (*t*+1)] for a player and the magnitude of points received by that player in round *t*

Dependent variable: Change in actions by player *i* between rounds (*t*+1) & *t*		
	Disapproval	Approval
Constant	0.65*** (-5.01)	0.39*** (3.98)
Total points received by player *i* in round *t*	0.13** (1.99)	-0.04*** (-8.32)
$Dummy_{it}$ that assumes a value 1 if *i* is a minimum player in round *t* and 0 otherwise	0.13*** (4.6)	0.03*** (4.8)

Notes: t-statistics are in parentheses.
**** Significant at the 1% level.*
*** Significant at the 10% level.*

APPENDIX C: Instructions

Instructions for chapter 1

Welcome to this experiment. Your registration number is written at the top of this sheet. This number will be used to identify you during the experiment.

You will play 10 rounds of a market game. You will be randomly matched with two other persons in each round. However in each round you will not know who are the other two persons in your group and you will not know this subsequently.

During the experiment, an assistant will observe the experiment and help from time to time in conducting the experiment. Please do not talk to others while the experiment is in progress.

In this experiment you will be rewarded in experimental dollars. How many experimental dollars you earn in each round depends on the decisions made by you and the other two people in your group. At the end of each round, please write down how many experimental dollars you won in that round on the sheet, labeled 'Earnings' provided to you. At the end of the experiment, all the experimental dollars you have earned will be added up and converted to U.S. dollars and will be paid to you in private. The exchange rate between experimental dollars and U.S. dollars is:

1 experimental dollar (E$) = 0.05 U.S. dollar = 5 cents.

The market game that will be played in each round is introduced next.

In this market game you will compete in prices with the other two people you are matched with. Three of you will form a ‘market’. Each of you will at the same time choose a price that will be a whole number between 1 and 100,that is, you may choose 1,2,3,…99,100. However, before you choose a price, each of you must, at the same time, choose whether or not to adopt a ‘price-matching policy’. Each person’s decision in your group about the price-matching policy will be announced before price choices are made.

The person or the persons who adopt a price-matching policy will have to subsequently match the lowest price chosen in the market.

The total profit in the market will be equal to the lowest price chosen in the market. This profit will be shared equally among the persons who chose that lowest price directly or by adopting the price-matching policy.

The person or the persons who did not choose the lowest price, and who did not adopt a price-matching policy, will earn zero profit.

The procedure that will be followed in each round is explained next.

You have been provided with 10 cards numbered F1, F2, F3,…F10. You should use these cards to record your price-matching decisions in the respective rounds. For example, once you have decided whether or not to adopt a price-matching decision in round 3, you should write down your registration number and the price matching decision for round 3 on card, F3. Write ‘M’ if you have adopted a price-matching policy and ‘NM’ if you have not adopted a price-matching policy. After all the people finish writing their price-matching decisions on their cards, the assistant will collect all the cards and put them in a box. The assistant will then randomly take three cards out of the box. The three people with the registration numbers written on the three cards will form a group. The assistant will then write on a whiteboard the three registration numbers and their price-matching decisions. Then the assistant will take out another three cards randomly and follow the same method as above. This procedure will be repeated for all the cards in the box.

Once the assistant has finished writing all the price-matching decisions and the registration numbers on the whiteboard, you can decide about your price choice. You have been provided with 10 cards numbered S1, S2, S3,…, S10. You should use these cards to record your price choices in the respective rounds. For example, once you have decided about your price choice in round 2, you should write down your registration number and the price choice (i.e., a whole number between 1 and 100) on card, S2. After all the people finish writing their price choices on their cards, the assistant will collect all the cards. Then the assistant will write on the whiteboard the corresponding price choice for each of the registration numbers. Next he will write how many experimental dollars were won by each of the registration numbers in that round. Please write down the experimental dollars you earned in that round on the sheet labeled 'Earnings'.

This will end that round. The same procedure will be repeated for all the rounds.

Please raise your hand if you have any questions. The assistant will be happy to answer those questions. Thank you for your participation!

Instructions for chapter 2

Welcome to this experiment. Your registration number is written at the top of this sheet. This number will be used to identify you during the experiment.

You will play 10 rounds of a market game. You will be randomly matched with two other persons in each round. However in each round you will not know who the other two persons in your group are, nor will you know this subsequently.

Please do not talk to others while the experiment is in progress.

In this experiment you will be rewarded in experimental dollars. How many experimental dollars you earn in each round depends on the decisions made by you and the other two sellers in your group. At the end of each round, the computer screen will show how many experimental dollars you won in that round. At the end of the experiment, all the experimental dollars you have earned will be added up and converted to U.S. dollars and will be paid to you in private. The exchange rate between experimental dollars and U.S. dollars is:

1 experimental dollar (E$) = 1 cent.

The market game that will be played in each round is introduced next.

In this market game you will compete in prices with the other two sellers you are matched with. Three of you will form a 'market'. Each of you will choose a price that will be a whole number between 1 and 100 at the same time, that is, you may choose 1,2,3,…99,100. However, before you choose a price, each of you must, at the same time, choose whether or not to adopt a 'price-matching policy'. The price matching decisions of others in your group will be displayed on your computer screen before each of you chooses your price.

The person or the persons who adopt a price-matching policy will have to subsequently match the lowest price chosen in the market. In other words, as a seller you can choose the lowest price in the market either directly by choosing the lowest price or you can choose that lowest price indirectly by adopting the price matching policy.

The information about the total number of buyers participating in each round is discussed next.

There are 6 computer buyers. Each of the computer buyers will buy 1 unit of the fictitious product. Computer buyers must buy in each period.

There are two types of buyers in this market game. They are called 'type A' buyers and 'type B' buyers. There will be 'type A' buyers and 'type B' buyers. Therefore, the total number of units that will be sold in a market in each round will be 6.

The purchasing decision of a 'type A' buyer is as follows: 'type A' buyer always buys the product from the seller who directly chooses the lowest price in the market. In the case when multiple sellers choose the same lowest price directly in a market, each of these sellers will sell the total number of units demanded by the 'type A', divided by the number of sellers who chose the lowest price directly.

The purchasing decision of a 'type B' buyer is as follows. A 'type B' buyer buys the product from the seller who directly or indirectly chooses the lowest price in a market. In the case where multiple sellers choose the same lowest price directly or indirectly, each of these sellers will sell the total number of units demanded by the 'type

B' buyers, divided by the number of sellers who chose that lowest price directly or indirectly.

The total profit in the market will be computed as described below.

Total Market Profit = The lowest price chosen in the market × total number of units demanded by the 'type A' and the 'type B' buyers.

This total profit will be shared among the sellers according to the following rule.

Individual seller profit is the sum of two parts. The first part is the following amount that will be added to a seller's profit if that seller chooses the lowest price directly:

$$\frac{\text{The lowest price chosen in the market} \times \text{ total number of units demanded by the 'type A' buyers}}{\text{Number of sellers who chose the lowest price directly in the market}}$$

The second part is the following amount that will be added to a seller's profit if that seller chooses the lowest price directly or indirectly:

$$\frac{\text{The lowest price chosen in the market} \times \text{ total number of units demanded by the 'type B' buyers}}{\text{Number of sellers who chose the lowest price directly or indirectly in the market}}$$

The procedure that will be followed in each round is explained next.

You have to make two decisions in two stages.

In the first stage, you have to decide whether or not you want to adopt the price-matching policy. The computer screen in front of you will display the two options: Match Lowest Price and Do Not Match Lowest price. If you decide to adopt the policy, click on "Match Lowest Price" and if you decide not to adopt the policy, then click on "Do Not

Match Lowest Price”. After all the sellers in your group finish deciding about their price-matching decisions, you will move onto the next stage.

In the next stage, the computer screen will inform you about how many sellers in your group adopted the policy. The screen will also display a box asking you to “Please Enter the Price at Which You Wish to Sell to Consumers”. Type the price you wish to charge into the box, and click on the “Confirm Price” button.

After all the sellers in your group finish deciding about their prices, the computer will inform you about the profit that you earned in the round you have just completed, as well as your cumulative profit earned in all previous rounds. These amounts are in experimental dollars. This screen will also report the group of each seller, the direct prices each seller chose, as well as their price matching decision. Under the header of “Price Matcher?”, a ‘0’ will be present for those sellers who chose not to price match, and ‘1’ for those who chose to price match.

This will end the round. The same procedure will be repeated for all the rounds.

Please raise your hand if you have any questions. The assistant will be happy to answer those questions. Thank you for your participation!

Instructions for chapter 3

Game A

Welcome to an experiment on route selection in traffic networks. During this experiment you'll be asked to make many decisions about route selection in a traffic network game. Your payoff will depend on the decisions you make as well as the decisions made by the other participants. A research foundation has contributed the funds to support this research.

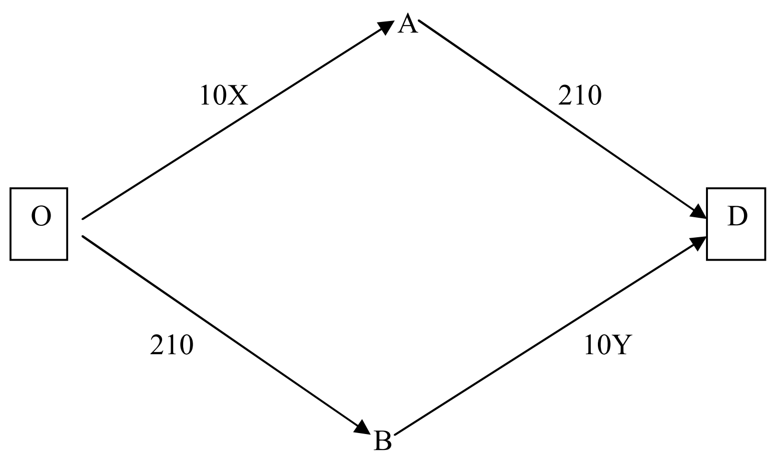
Please read these instructions now. In case you have any questions, please raise your hand and the experimenter will come to answer them.

Please note that hereafter communication between the participants is strictly prohibited. If they communicate with one another by any shape or form, the experiment will be canceled.

There are 18 participants in this experiment, including yourself, who will be asked to serve as drivers and choose a route to travel in two traffic network games that are described below. The two games will differ from one another. Below we present the instructions for Game A. The ones for Game B will be presented later after you complete Game A. You will play Game A for 40 identical rounds.

Description of Game A

Consider the very simple traffic network exhibited in a diagram form on the next page. Each driver is required to choose one of two routes in order to travel from the starting point, denoted by O, to the final destination, denoted by D. There are two alternative routes and they denoted in the diagram by either [O-A-D] or [O-B-D].



Travel is always costly in terms of the time needed to complete a segment of the road, tolls, fuel etc. The travel costs are written near each segment of the route you choose. For example, if you choose route [O-A-D], you will be charged a total cost of 10X+210 where X indicates the <u>number</u> of participants who choose segment [O-A] to travel from O to D plus a fixed cost of 210 for traveling on segment [B-D]. Similarly, if you choose route [O-B-D], you will be charged a total travel cost of 210+10Y, where Y indicates the <u>number</u> of participants who choose the segment [B-D] to drive from O to D. Please note that the cost charged for segments [O-A] and [B-D] depends on the number of drivers choosing them. In contrast, the cost charged for traveling on segments [A-D] and [O-B] is fixed at 210 and does not depend on the number of drivers choosing them. All the drivers make their route choices independently of one another and leave point S at the same time.

Example. If you happen to be the only driver who chooses route [O-A-D], and all other 17 drivers choose route O-B-D, then your travel cost from point O to point D is equal to $(10\times1)+210=220$. If, on another round, you and 2 more drivers choose route [O-B-D] and 15 other drivers choose route [O-A-D], then your travel cost for that round will be $210+(10\times3)=240$.

At the beginning of *each round*, you will receive an endowment of **400** points. Your payoff for each round will be determined by subtracting your travel cost from your endowment. To continue the previous example, if your travel cost for the round is 210, your payoff will be 400-210=190 points. If it is 230, then your payoff for that round will be 400-230=170 points.

At the end of each round, you will be informed of the number of drivers who chose each route and your payoff for that round. All 40 rounds in Game 1 have exactly the same structure.

Procedure

At the beginning of each round, the computer terminal in front of you will exhibit the above diagram with two routes, namely, [O-A-D] and [O-B-D]. Then, you will be asked to choose which of the two routes to travel. To choose a route, simply click on that route. For example, if you choose route [O-A-D], then click once on segment [O-A] and once on segment [A-D]. Clicking on those two segments will change their color to indicate your choice of route. If you decide to change your route, you can click on the two segments of the alternative route. Once you have chosen your route and pressed the "Confirm" button, the computer will ask you to verify your choice. You do so by clicking on the "OK" button.

After choosing a route, you will be presented with a wait message until all 18 drivers have made their decisions. Once each driver chooses one of the two routes, the computer will present you with a screen that includes the following information:

The route you have chosen

The number of drivers who chose route [O-A-D].

The number of drivers who chose route [O-B-D].

Your payoff for that round.

Once you have completed playing 40 rounds of Game A, the computer will inform you that Game A is over and Game B is about to start. Then, a new set of instructions for Game B will be distributed to you.

Payments

At the end of the session you will be paid for 4 rounds randomly selected from the 40 rounds in Game A and 4 more rounds randomly selected from the 40 rounds in Game B. The choice of the payment rounds will be made publicly by drawing 4 cards from a pack of 40 cards numbered from 1 through 40. Once these 8 rounds are chosen, they will be recorded by the experimenter, the payment of each subject will be computed according to his/her earnings on these rounds, and the computer will inform you of your total points and the resulting earnings for the session.

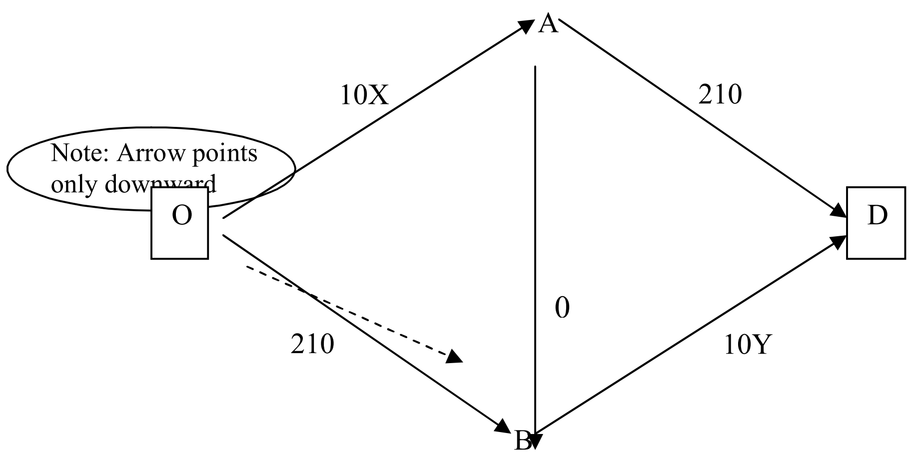
Points will be converted into money at the rate 25 points=$1.00.

All the decisions will only be made by clicking the “mouse”. Therefore, please do not touch the keyboard.

Please place the instructions on the table in front of you to indicate that you have completed reading them. Game 1 will begin within shortly.

Game B

Game B is very similar to Game A with the exception that we have added another segment from point A to point B with zero travel cost. As a result, in choosing a travel route in this new traffic network, now you have to choose one of three routes, namely route [O-A-D], route [O-B-D], or route [O-A-B-D]. Similarly to Game 1, you have to choose a single travel route. The traffic network for Game 2 is displayed below.



Travel costs are computed exactly as in Game A. If you choose route [O-A-D], you will be charged a total travel cost of (10X + 210), where X indicates the number of drivers who chose the segment [O-A] to travel from O to D via either route [O-A-D] **or** route [O-A-B-D]. Similarly, if you choose route [O-B-D], you will be charged a total travel cost of (210 + 10Y) where Y indicates the number of drivers who chose the segment [B-D] to travel from O to D via either route [O-B-D] **or** route [O-A-B-D]. If you choose route O-A-B-D, you have to spend a total travel cost of (10X + 0 + 10Y) where X indicates the number of drivers who chose the segment [O-A] to drive from O to

D, and Y indicates the number of drivers who chose the segment [B-D] to drive from O to D.

Note that, unlike Game A, drivers who choose route [O-A-D] and route [O-A-B-D] share the segment [O-A]. Similarly, drivers who choose routes [O-B-D] and [O-A-B-D] share the segment [B-D].

Example. Supposing that you choose route [O-A-B-D], 3 other drivers choose route [O-A-D], and 14 additional drivers choose route [O-B-D]. Then, your total travel cost for that period is equal to $(10 \times 4) + 0 + (10 \times 15) = 190$. Note that in this example, 4 drivers (including you) traveled on the segment [O-A] and 15 drivers (again, including you) traveled the segment [B-D] to go from O to D. Each of the 3 drivers choosing route [O-A-D] will be charged a travel cost of $(10 \times 4) + (210)] = 250$, and each of the 14 drivers choosing the route [O-B-D] will be charged a travel cost of $(210) + (10 \times 15) = 360$.

Similarly to Game A, at the beginning of each round you will receive an endowment of 400 points. Your payoff for each round will be determined by subtracting your total travel cost from your endowment for that round. The information you receive at the end of each round is the same as in Game A. In particular, at the end of each round the computer will display:

The route you have chosen.

The number of drivers who chose route [O-A-D].

The number of drivers who chose route [O-B-D].

The number of drivers who chose route [O-A-B-D].

Your payoff for that round.

Payoffs will be determined exactly as in Game A (4 payment rounds randomly drawn out of 40).

Thank you for your participation in this experiment.

Instructions for chapter 4

Benchmark Game

Welcome to this experiment. You are going to take part in an experimental study of decision-making. The funding for this study has been provided by a research foundation. The instructions are simple and by following them carefully, you may earn a considerable amount of money. Your total earnings will be paid in cash at the end of this experiment.

There are 10 of you who are participating in this experiment. In each period you will be making decision with 9 other participants in this experiment. The amount of money earned by each of you will depend on the decisions made by you and the other participants.

The experiment consists of 10 periods. Each period consists of one stage. The decision that you will make in this stage of each period is explained below.

In this stage, each of you will, at the same time, choose a number that will be a whole number between 1 and 7, that is, you may choose 1, 2, 3, 4, 5, 6 or 7. The computer terminals in front of each of you will display those numbers. Choose your number by simply clicking on that number. After all of you have made your choices in this stage, the computer terminal will display your choice, your payoff and the choices by other participants. The following equation will determine your payoff after this stage decisions:

YOUR PAYOFF = 0.20 X (Minimum of all the number choices) – 0.10 X (Your number choice) + 0.60

To facilitate the calculation of your payoff, a “Payoff Table” (same for all of you) has been provided to each of you that uses the above equation. The first column in this table is “Your Number Choice” and the first row is the “Minimum Number Choice” of all the choices (including your number). For example, suppose in this stage you chose ‘Y’ (locate number ‘Y’ in the first column) and the minimum of all the numbers chosen is ‘Z’ (locate number ‘Z’ in the first row), then you will receive ‘V’ (the number at the intersection of the two numbers, ‘Y’ in the first column and ‘Z’ in the first row) in this stage. This is your payoff after this stage. All the amounts in the payoff table are in US dollars.

A “Record Sheet” is provided to each of you (attached to the instructions). Your identification number is written at the top-right hand corner of this sheet. Now, please look at the columns of your record sheet. Going from left to right, you will see columns for the “period,” “your number choice,” “minimum number choice,” and “your payoff.” You will write down the period number, your choice, the minimum choice and your earnings in this period. We will also keep a record of each of your choices and the payoffs for each period.

After all of you make and record your decisions for period one, the experiment will go to the second period. This same process is repeated for a total number of ten times.

During the experiment, you are not permitted to speak or communicate in any shape or form with the other participants. Talking to others will disqualify you from participating in this experiment. If you have a question while the experiment is going on, please raise your hand and one of us will come to answer it. At this time, do you have any questions about the instructions or procedures? If you have a question, please raise your hand.

Thank you for your participation.

Approval Treatment

Welcome to this experiment. You are going to take part in an experimental study of decision-making. The funding for this study has been provided by a research foundation. The instructions are simple and by following them carefully, you may earn a considerable amount of money. Your total earnings will be paid in cash at the end of this experiment.

There are 10 of you who are participating in this experiment. In each period you will be making decision with 9 other participants in this experiment. The amount of money earned by each of you will depend on the decisions made by you and the other participants.

The experiment consists of 10 periods. Each period consists of two stages. The decision that you will make in the first stage of each period is explained below.

In the first stage, each of you will, at the same time, choose a number that will be a whole number between 1 and 7, that is, you may choose 1, 2, 3, 4, 5, 6 or 7. The computer terminals in front of each of you will display those numbers. Choose your number by simply clicking on that number. After all of you have made your choices in the first stage, the computer terminal will display your choice, your payoff and the choices by other participants. The following equation will determine your payoff after first stage decisions:

YOUR PAYOFF = 0.20 X (Minimum of all the number choices) – 0.10 X (Your number choice) + 0.60

To facilitate the calculation of your payoff, a “Payoff Table” (same for all of you) has been provided to each of you that uses the above equation. The first column in this table is “Your Number Choice” and the first row is the “Minimum Number Choice” of all the first stage choices (including your number). For example, suppose in the first stage you chose ‘Y’ (locate number ‘Y’ in the first column) and the minimum of all the numbers chosen is ‘Z’ (locate number ‘Z’ in the first row), then you will receive ‘V’ (the number at the intersection of the two numbers, ‘Y’ in the first column and ‘Z’ in the first row) in the first stage. This is your payoff after first stage. All the amounts in the payoff table are in US dollars.

In the second stage, you have the opportunity to register your approval of each of the other 9 participant’s first stage decisions by distributing points. <u>You can distribute points from 0 to 6 to a participant’s first stage choice, that is, 0, 1, 2, 3, 4, 5 and 6. Distributing 6 points to a participant’s choice shows the most approval and distributing 0 points shows the least approval.</u>

However, distributing these approval points is costless for you and receiving these approval points will not affect your first stage payoff in anyway.

The computer will show the first stage choices of other participants. Next to each of these choices (excluding your choice), a box will appear under the heading “Please Enter Your Feedback” in which you can type in approval points (from 0 to 6) for each of the first stage choices. Remember that you must type in a number for each of these choices. After you distribute points to all others, click the “continue” button. The next feedback screen will display the total approval points each of the number choices received in the second stage.

A “Record Sheet” is provided to each of you (attached to the instructions). Your identification number is written at the top-right hand corner of this sheet. Now, please look at the columns of your record sheet. Going from left to right, you will see columns for the “period,” “your number choice,” “minimum number choice,” “your payoff” and “Total number of approval points you received.” You will write down the period number, your choice, the minimum choice, your earnings and the total number of points you received in this period. We will also keep a record of each of your choices and the payoffs for each period.

After all of you make and record your decisions for period one, the experiment will go to the second period. This same process is repeated for a total number of ten times.

During the experiment, you are not permitted to speak or communicate in any shape or form with the other participants. Talking to others will disqualify you from participating in this experiment. If you have a question while the experiment is going on, please raise your hand and one of us will come to answer it. At this time, do you have any questions about the instructions or procedures? If you have a question, please raise your hand.

Thank you for your participation.

Disapproval Treatment

Welcome to this experiment. You are going to take part in an experimental study of decision-making. The funding for this study has been provided by a research foundation. The instructions are simple and by following them carefully, you may earn a considerable amount of money. Your total earnings will be paid in cash at the end of this experiment.

There are 10 of you who are participating in this experiment. In each period you will be making decision with 9 other participants in this experiment. The amount of money earned by each of you will depend on the decisions made by you and the other participants.

The experiment consists of 10 periods. Each period consists of two stages. The decision that you will make in the first stage of each period is explained below.

In the first stage, each of you will, at the same time, choose a number that will be a whole number between 1 and 7, that is, you may choose 1, 2, 3, 4, 5, 6 or 7. The computer terminals in front of each of you will display those numbers. Choose your number by simply clicking on that number. After all of you have made your choices in the first stage, the computer terminal will display your choice, your payoff and the choices by other participants. The following equation will determine your payoff after first stage decisions:

YOUR PAYOFF = 0.20 X (Minimum of all the number choices) – 0.10 X (Your number choice) + 0.60

To facilitate the calculation of your payoff, a “Payoff Table” (same for all of you) has been provided to each of you that uses the above equation. The first column in this table is “Your Number Choice” and the first row is the “Minimum Number Choice” of all the first stage choices (including your number). For example, suppose in the first stage you chose ‘Y’ (locate number ‘Y’ in the first column) and the minimum of all the numbers chosen is ‘Z’ (locate number ‘Z’ in the first row), then you will receive ‘V’ (the number at the intersection of the two numbers, ‘Y’ in the first column and ‘Z’ in the first row) in the first stage. This is your payoff after first stage. All the amounts in the payoff table are in US dollars.

In the second stage, you have the opportunity to register your disapproval of each of the other 9 participant’s first stage decisions by distributing points. You can distribute points from 0 to 6 to a participant’s first stage choice, that is, 0, 1, 2, 3, 4, 5 and 6. Distributing 6 points to a participant’s choice shows the most disapproval and distributing 0 points shows the least disapproval.

However, distributing these disapproval points is costless for you and receiving these disapproval points will not affect your first stage payoff in anyway.

The computer will show the first stage choices of other participants. Next to each of these choices (excluding your choice), a box will appear under the heading “Please Enter Your Feedback” in which you can type in disapproval points (from 0 to 6) for each of the first stage choices. Remember that you must type in a number for each of these choices. After you distribute points to all others, click the “continue” button. The next feedback screen will display the total disapproval points each of the number choices received in the second stage.

A “Record Sheet” is provided to each of you (attached to the instructions). Your identification number is written at the top-right hand corner of this sheet. Now, please look at the columns of your record sheet. Going from left to right, you will see columns for the “period,” “your number choice,” “minimum number choice,” “your payoff” and “Total number of disapproval points you received.” You will write down the period number, your choice, the minimum choice, your earnings and the total number of points you received in this period. We will also keep a record of each of your choices and the payoffs for each period.

After all of you make and record your decisions for period one, the experiment will go to the second period. This same process is repeated for a total number of ten times.

During the experiment, you are not permitted to speak or communicate in any shape or form with the other participants. Talking to others will disqualify you from participating in this experiment. If you have a question while the experiment is going on, please raise your hand and one of us will come to answer it. At this time, do you have any questions about the instructions or procedures? If you have a question, please raise your hand.

Thank you for your participation.

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