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## The Economics of Open Air Markets\*

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### Abstract

This study explores the economics of open air markets. Despite their current prevalence and historical significance, much is to be learned about the underlying operation of such markets. This paper offers preliminary insights from a series of open air market experiments in a large US metropolitan area. Using data gathered from thousands of individual observations from decentralized naturally occurring market experiments, I find some interesting data patterns. For instance, I find that the natural pricing and allocation mechanism used in open air markets is capable of approaching full efficiency, even in quite austere conditions. Yet, allowance of seller communication frustrates market efficiency in a broad array of situations. Within such collusive arrangements, theoretical predictions are often met, as the evidence suggests that i) cheating rates increase as the coalition is expanded, ii) sellers cheat less when they have multiple collusive arrangements in spatially differentiated markets, and iii) sellers cheat more when they are experiencing periods of abnormally high profits. These results follow from a combination of data gathered via building a bridge between the lab and the naturally-occurring environment. In doing so, the study also has methodological content. In this regard, I report that even though collusive arrangements are robust in tightly controlled experimental markets, in complementary experimental treatments where sellers are unaware that they are taking part in an experiment, considerable cheating occurs. Overall, however, the best *aggregate* predictor of cartel cheating in the field is behavior in *sterile* laboratory treatments with neutral language instructions.

*JEL*: C93 (Field Experiments), D4 (Market Structure and Pricing)

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## I. Introduction

Bilateral bargaining has constituted the foundation of markets for centuries, from peasant economies—such as Athen’s Agora, Rome’s Forum, the medieval fairs and markets in England, and the 1,000 year old market in Morocco—to the substantial bazaars and “flea” markets that litter the landscape of developed and developing countries today.<sup>1</sup> While it is difficult to provide an economic estimate of the importance of such markets, tax data from the state of Florida shows that flea markets account for roughly \$170 million of revenue per month in Florida alone (<http://www.state.fl.us/dor/tables/>), and the San Jose Flea Market attracts nearly 60,000 customers per weekend (see, e.g., Washington Post 2005). The National Flea Market Association reports that the number of flea markets in the US and the recorded gross sales have grown substantially in the past several years, with 2.25 million licensed vendors and more than \$30 billion in sales annually in 2005. This is surely a vast underestimate, however, since a nontrivial portion of the transactions are carried out by non-licensed vendors and via non-taxed sales. More broadly, such markets are commonly of great importance, especially in developing countries where the institution represents an important part of the allocation of goods and services in the formal market.<sup>2</sup>

Surprisingly, despite the importance of such markets in shaping economies of yesteryear and allocating goods and services today, little is known about the basic economic principles of such markets. Whether the bread and butter of economics—supply and demand curve shape and location—provides accurate predictions of price and quantity realizations in such domains is

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<sup>1</sup> The origin of the term “flea market” is difficult to trace. Some argue that it is due to the market in Marche aux puces of Saint-Ouen, Seine-Saint-Denis, in the northern suburbs of Paris. This particular market, which started in the 17<sup>th</sup> century, is said to have earned the name “flea” from the flea-infested old furniture brought for sale as well as the clothing and rags sold at the market.

<sup>2</sup> Examples are ubiquitous. Consider Geertz (1978), who argues that up to 2/3 of the population of the Moroccan town that he studied (Sefrou, “a thriving market center of 15,000-30,000 people” (Geertz, p. 28)) is employed at

relatively unknown. Indeed, results from Chamberlain (1948) predict that they perform rather poorly. This study begins by making a simple step in this direction by exploring behavior in open air markets in a major US metropolitan area. My general approach is to undertake controlled experiments in these markets where factors at the heart of my conjecture are identifiable and arise endogenously. I then impose the remaining experimental controls to learn something about underlying market behavior.

The experimental design is similar to the primitives of Chamberlin (1948), as extended by List (2004). A key result from the initial experiments is the strong tendency for exchange prices to approach the prediction of competitive market equilibrium theory. Even under the most severe tests of neoclassical theory (treatments that predict highly asymmetric rents) the expected price and quantity levels are approximated in many market periods. These results suggest that in mature markets very few of the “typical” assumptions, such as Walrasian tâtonnement or centrally occurring open outcry of bids and offers, are necessary to approximate the predicted equilibrium in the field.

Yet, such markets are ripe for price manipulation. For instance, in many cases small numbers of sellers provide homogeneous goods that are jointly purchased from middlemen, certain barriers to entry exist, and communication is continual for many sellers.<sup>3</sup> Open communication channels surely remind the astute reader of the forewarning of Adam Smith in the *Wealth of Nations*:

"People of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in a conspiracy against the public, or in some contrivance to raise prices."

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the local bazaar.

<sup>3</sup> See the excellent overview of Levenstein and Suslow (2006).

Indeed, through my interaction with an individual seller in this marketplace I learned interesting details of just such conspiracies in these markets. Armed with knowledge from my “mole,” I am able to build a bridge between the lab and the field, effectively exploring the behavior of experimental subjects across quite sterile and quite rich settings. This approach has a dual benefit in that it affords an opportunity to marry the vast experimental literature on collusion in laboratory experiments (see, e.g., Holt, 1995 for a review) with parallel behavior in the field.

To tackle this issue, I begin by collecting data in a controlled laboratory study with student subjects. I proceed to collect data using the exact same protocol with subjects from the open air market (denoted “market” subjects below) who have selected into various roles in the marketplace. Making use of this knowledge, I place them in similar roles in the experiment. I proceed with a series of controlled experiments that slowly moves the environment from a tightly controlled laboratory study to a natural field experiment (i.e., subjects do not know that they are part of an experiment).

Several insights are obtained. First, I find several results consistent with theory. For example, the evidence suggests that i) cheating rates are an increasing function in coalition size, ii) sellers cheat less when they have multiple collusive arrangements in spatially differentiated markets, and iii) sellers cheat more when they are experiencing periods of abnormally large profits. Second, I observe similar behavior across subject pools in tightly controlled settings. Yet, difficulties arise when mapping the importance of collusive arrangements on market prices across these environments. Whereas cooperation rates are found to be quite high in the tightly controlled experiments, I find that when sellers do not know that they are part of an experiment they cheat on the explicit agreements much more readily. The level of cheating is considerably larger than cheating rates observed in any of the tightly controlled treatments. Perhaps

surprisingly, however, in *aggregate* the best predictor of field cheating rates are the student and market subjects' behavior in *sterile* laboratory treatments with neutral language instructions. Nevertheless, matching individual data from these same neutral language treatments to actual field behavior within the same individuals, I find that cheating in one environment is not a good predictor of who cheats in another.

I interpret these data as providing several lessons. First, consistent with the theoretical work beginning with Stigler (1964), the field data suggest that maintaining strict compliance to the collusive agreement is difficult to sustain in a repeated game with secret price cuts and demand uncertainty. In this manner, the data are consistent with the notion that inherent problems associated with maintaining collusive agreements *might* preclude conspiracies from having considerable influence on prices in similar market structures. Second, if one is interested in predicting aggregate field data patterns, then it is not always the case that more similar environments leads to a greater ability to generalize results across environments. Together, these lessons teach us that caution should be taken when making inference about the importance of seller conspiracies on price realizations, as the extensive experimental literature (see Holt's (1995) review and List and Price (2005) more recently) likely provides an upper bound estimate of the effectiveness of such conspiracies. In this spirit, one prominent aspect of the data is that one should take great care when generalizing results across domains. Finally, the paper highlights the substantial inferential gain that one achieves by combining field experimental data with laboratory data.

The remainder of this study proceeds as follows. The next section provides an overview of the open air markets that I frequented and summarizes the experimental design. Section III provides a results discussion. Section IV concludes.

## **II. Market Background and Experimental Design**

### *Market Background*

Bilateral bargaining represents one of the earliest forms of exchange. As far back as 2 million years ago, the appearance of manufactured stone tools in certain parts of the world provides suggestive evidence that our hominid ancestors practiced some form of primitive exchange, almost certainly organized by a simple bilateral mechanism. Bilateral markets continued to develop, extending in the period 30,000-40,000 years ago to portable art, personal ornamentation, and ever-more-intricate weapons and tools (Ofek, 2001). Forms of ‘commodity money’ developed at during this period, further fueling the expansion of human trade; and combined with the development of agriculture around 10,000 years ago, laid the groundwork for human civilization (Diamond, 1992).

Today, bilateral trading in a multi-lateral setting represents the backbone of markets worldwide. From bazaars that are housed along tourist destinations as far removed as the Bahamas and Tokyo, to the Barabazar in Shillong, India (the largest open air market in India), to the local flea markets in the states, buyers and sellers engage in an interaction that has each making an effort to extract as much of the available rent as possible. Rich accounts exist concerning some specific bazaar economies and negotiation practices in such markets (see, e.g., Cassady, 1968, Geertz, 1978, Alexander and Alexander, 1991). Since such markets are similar, I will provide only a brief summary of a myriad of particulars that I have learned through my field research at the open air markets in a large metropolitan area in the US. This is not meant to be an ethnographic description, but will share that flavor.

Layout

Temporal assignment of the physical marketplace is typically done by a professional association or local dealer who rents a large space, such as a large parking area or fairgrounds, and allocates tables to dealers for a nominal fee. When the market opens, consumers mill around the marketplace, higgling and bargaining with dealers, who have their merchandise prominently displayed on their six-foot table. The duration of a typical show is a day or weekend, and a lucrative show may provide any given dealer hundreds of exchange opportunities (buying, selling, and trading of goods).

In this sense, the current experimental design matches real-world settings that economic theory attempts to explain: traders endogenously select into the market and they are likely to have previous experience buying, selling, and trading. This experimental strategy may lead to different results than an experiment with exogenously induced roles (e.g., subjects are randomly placed in the buyer or seller role), but it is my belief that a rigorous examination of behavior in an actual environment that our theory intends to explain is an important step in testing the validity of economic models.

Goods—quality uncertain versus quality certainty

Bargaining (information, etc.)

Much more needed here. Theory, predictions, etc.

Market particulars—coordinate, cheating, entry

### *Experimental Design*

Before proceeding to the details of the experimental design, it is worthwhile to provide a roadmap of the treatments executed. This might serve as a useful source to not only the impatient reader, but also provide a “forest-like” reminder to those readers who might get engrossed in the specific details of the “trees” below.

Figure 1 provides an outline of the treatments, making use of the terminology of Harrison and List (2004). The “laboratory” treatments are in the spirit of the extant literature (see, e.g., Isaac and Plott, 1981; Isaac et al., 1984; Davis and Holt, 1998; Cason and Mason, 1999; Feinberg and Snyder, 2002; Davis and Wilson, 2002; Aoyagi and Frechette, 2003; see Holt, 1995, for a review). Within these treatments I vary the i) subject pool—from students to actual market participants at open air markets, ii) anonymity conditions (none to perfect anonymity between subjects), and iii) context of the game. In terms of context, I begin with instructions that use neutral language and proceed to instructions that include wording such as “collusive pricing,” “collusive partner,” etc. As Table 1 highlights, these treatments are meant to span the range of “traditional” laboratory experiments to framed field experiments.

Moving one step closer to the market environment of ultimate interest, I depart from these abstract games and experiment with an institution in which the participants are naturally engaged in this market—bilateral negotiation markets—in a set of framed field treatments. A few features of this particular market make it attractive to examine predictions of competitive theory in multi-lateral decentralized markets. First, I am observing behavior of agents who have endogenously chosen certain roles within the marketplace—such as being a buyer (nondealer) or seller (dealer). Second, the task and the stakes in the experimental treatments are quite similar to the naturally occurring market: when the experimental market opens, consumers mill around the marketplace, higgling and bargaining with dealers, who have the experimental good prominently displayed on their tables. In this sense, I am gathering data in a natural environment while still maintaining the necessary control to execute a clean test of theory.

This set of treatments includes variants that preclude communication among sellers as well as several treatments that permit communication. This approach is used for two reasons: i)

testing whether competitive theory adequately organizes data in multi-lateral decentralized market institutions is interesting in its own right, and ii) no communication treatments serve to provide a baseline of comparison for the communication treatments.

The ultimate treatments—denoted natural field experiment in Table 1—have my confederates approach sellers who do not know that they are taking part in an experiment. In these treatments I use identical demand structures to those used in the framed field treatments. These treatments are possible because I have knowledge of certain explicit agreements (overtly collusive agreements) in this marketplace from a “mole” who has informed me of sufficient details to execute a controlled test in such an environment. I now turn to a more detailed description of the experimental design, which was carried out from May 2005-September 2006.

#### **A. Lab Experiment Particulars**

My overarching goal in designing the laboratory experiments was to stay true to the extant experimental literature and provide a first step in creating a bridge between the lab and field. With these dual goals in mind, I opted to utilize a design similar to Holcomb and Nelson (1997) and Feinberg and Snyder (2002). The experimental instructions are contained in Appendix A.<sup>4</sup>

The employed game is simple and calls for two players in a market to simultaneously choose one of three prices: P1, P2, and P3, where these prices represent the “collusive” price, an “undercutting” price, and a “punishment” price (though in the instructions price choices are labeled P1, P2, and P3). The resulting payoffs from each of the 9 cells are given in Table 1A in

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<sup>4</sup> Some readers might find the spirit of these treatments similar to the Green and Porter (1984) model. In that case, the stage game has two firms simultaneously choosing prices for a homogeneous product. They split demand equally for identical price choices, but the low price firm obtains all of the demand if the prices are unequal. With constant marginal cost and market demand fluctuations that are observable, the collusive price is sustainable for a discount rate less than or equal to 100% (a discount factor of  $\frac{1}{2}$  or greater). For more impatient sellers, the unique equilibrium involves marginal cost pricing. For the case of secret price cuts, or demand shocks that are unobservable, the minimum discount factor that sustains collusion is higher.

Appendix A. Subjects were informed that their partner was determined randomly and that they would remain partners throughout the experiment. Further, the experiment was to be run for 10 periods after which a randomization scheme would determine if the experiment concluded: a six-sided die was tossed and if a 1 or 2 was thrown the game ended, otherwise another round was played. After this round, again a die was thrown and the game ended with a  $1/3$  probability. Thus, the game had a continuation beyond 10 periods with a  $2/3$  probability and each round thereafter followed a similar continuation rule.<sup>5</sup> In many cases, experimenters use an infinite horizon game to permit collusive agreements to be an equilibrium; under the parameters of this game, however, cooperation is an equilibrium.<sup>6</sup>

I conducted one experimental session with undergraduate participants from a major university for each of four treatments. Twenty-four students participated in each session. Subjects participated in one treatment; therefore inference is made with purely between-subject variation. In Treatment LabIS (denoting lab treatment I with students as subjects), after each period subjects are informed only of their payoffs. Of course, given that they are aware of their own choice and the figures in the payoff matrix, they can easily deduce the other's choice since there is no stochastic element of demand in Treatment LabIS. This is similar to Feinberg and Snyder's (2002) baseline treatment.

Treatment Lab IIS (denoting lab treatment II with students) is identical to Treatment LabIS except there are demand shocks. Subjects were again informed about their own payoffs

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<sup>5</sup> As Rasmusen (1989, p. 103) notes, "the reason why games with a constant probability of ending are like infinite games is nicely pointed out by a verse from *Amazing Grace*:

When we've been there ten thousand years,  
Bright shining as the sun,  
We've no less days to sing God's praise  
Than when we'd first begun."

<sup>6</sup> To see this, note that in a 5-period game a strategy of "play collusion for rounds 1-4, then play undercutting for round 5 if no prior defections; if prior defection play punishment for the remainder of the game," has the mutual best response property and therefore represents an equilibrium to the finite game.

after each period but not about rival's actions. In this case, however, a negative demand shock gave each player a zero payoff regardless of their choices. Participants were instructed at the outset of the experiment that shocks would occur in roughly 10% of the periods (and the shock was carried out in period 5). Thus, given that subjects were unaware of whether their payoffs were due to demand shocks or their rival's price cut, they cannot deduce whether or not their rival had deviated. Readers familiar with Stigler (1964) will find similarities of this treatment to his modeling of an oligopolist facing uncertain demand.

The third student treatment, Treatment Lab IIIS (denoting lab treatment III with students), is identical to Treatment LabIIS except students are introduced to their partner directly before the game begins: before the first practice round begins, the monitor asks the pairs to stand up one by one, look at each other, and make a brief introduction by stating their name and year in school. Subsequent to all pairs completing their introduction, play began.

The final treatment, denoted Treatment LabIIISC, uses instructions with context. For instance, rather than labeling the price choices as P1, P2, and P3 in the protocol, I denote them as the "collusive price," "undercutting price," and "punishment price." In addition, the framework is now denoted a "market," sellers are determining their choices within a duopolistic setting, and they are receiving "profits" rather than "payoffs" (see Appendix A).

Complementing these laboratory explorations are identical treatments carried out with sellers at various open air markets ("flea" markets) in a large US metropolitan area. These treatments are labeled LabIM, LabIIM, LabIIIM, and LabIIIMC (denoting lab treatments with market subjects, or "artefactual" field treatments in Table 1). In this particular large area more than 20 flea markets operate on any given weekend. Each participant's experience typically followed four steps: (1) consideration of the invitation to participate in an experiment, (2)

learning the market rules, (3) actual market participation (in their actual sales booth), and (4) conclusion of the experiment. In Step 1, before the market opened, a monitor randomly approached sellers and inquired about their interest in participating in an experiment that would take about 30 minutes during the weekend. Once the prerequisite number of sellers agreed to participate (i.e., a seller had a partner), monitors thoroughly explained the experimental rules in Step 2.

The experimental instructions for the various treatments were similar to Appendix A. Identical to the student treatments, there was no participation fee paid. Different from the student treatments is that the marketers completed the experiment period by period in their actual sales booth during the trading day since there was no “sterile” environment in these markets where I could house the subjects. Thus, after each subject made their decision, both were provided with information from that period. Similar to the student treatments, it is important to note that in the anonymity treatments sellers were never made aware of their partners and they were informed this would be the case. In Step 3 the sellers participated in the experiment, and in Step 4 they were paid their earnings in private after they filled out the survey in Appendix B.

For the parameters employed, it can be shown that a strategy of only one period of reversion is required to sustain collusion (i.e., a tit-for-tat strategy) for both the treatments that have known opponent choices (Lab IS and Lab IM) as well as the treatments that have unobserved choices (Lab IIS, Lab IIIS, Lab IIISC, Lab IIM, Lab IIIM, and Lab IIIMC). This equilibrium holds whether the trigger strategies employed utilize the undercutting (P2) or the punishment (P3) choice.

Table 1 provides a summary of the various laboratory experimental treatments and provides relevant sample sizes. In total, I observe the behavior of 96 students across 4 treatments

and 96 marketers across 4 treatments. Given that each student (marketer) subject made at least 10 choices, I have more than 1900 observations across these treatments.

## **B. Framed Field Experiment Particulars**

To move one step closer to the actual market setting that these agents naturally engage, I design a series of field treatments that explores behavior in face-to-face continuous bilateral bargaining in a multi-lateral market, which is quite similar to the bazaar economies or flea markets scattered around the world today. These treatments vary basic aspects of the market structure: demand and supply shapes, the number of sellers, and the ability to communicate. A first objective is to examine under what conditions the predictions of competitive market equilibrium theory, conditional on the induced preferences, conform to the actual quantities exchanged and the prices at which these exchanges actually take place. In this sense, these treatments suppress important aspects of the common marketplace—information asymmetry among buyers about market possibilities, socio-cultural factors, etc.—to focus on whether the institution in its simplest form can yield efficient outcomes.

Much like Smith's (1962) set-up, the market mechanics in these bilateral bargaining markets are not Walrasian. Unlike Smith (1962), however, in these markets subjects set prices as they please, with no guidance from a centralized auctioneer. Thus, this design shifts the task of adaptation from the auctioneer to the agents, permitting trades to occur in a decentralized manner, similar to how trades are consummated in actual free unobstructed markets. In doing so, the market structure reformulates the problem of stability of equilibria as a question about the behavior of actual people as a psychological question—as opposed to a question about an abstract and impersonal market.

The basic market design is similar in spirit to Chamberlin (1948), as extended by List (2004).<sup>7</sup> Each participant's experience typically followed four steps: (1) consideration of the invitation to participate in an experiment, (2) learning the market rules, (3) actual market participation, and (4) conclusion of the experiment and exit interview. In Step 1, before the market opened, a monitor randomly approached sellers in the flea market (located in a large US metropolitan area) and inquired about their interest in participating in an experiment that would take about 45 minutes during the day. Since most sellers are accompanied by at least one helper, it was not difficult to obtain agreements after it was explained that money could be earned during the experiment. To gather the non-seller subject pool, a monitor randomly approached potential subjects in the market and inquired about their level of interest in participating in an experiment that would last about 45 minutes.

Once the prerequisite number of sellers and buyers agreed to participate, monitors thoroughly explained the experimental rules in Step 2. The experimental instructions for the various treatments represent an extension of List (2004), and are standard in the market experimental literature (see Davis and Holt (1993; pp. 47-55)). A few aspects of the experimental design should be highlighted. To ensure transactions at reservation values, a \$0.50 commission for each executed trade was provided for both buyers and sellers.

Second, buyers (nondealers) were informed that the experiment consisted of at least 5 rounds and that they would be consumers. In each round, each buyer would be given a "buyer's card," which contained a number, known only by that buyer, representing the maximum price that he or she would be willing to pay for *one* unit. Dealers were informed that they would be sellers in the market. In each round, each seller would be given a "seller's card," which

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<sup>7</sup> The experimental design discussion in this section closely follows List (2004) and List and Price (2005).

contained numbers, known only to that seller, representing the minimum for which he or she would be willing to sell their units. Importantly, all agents were informed that this information was strictly private and that reservation values would change each round. They were also informed about the number of buyers and sellers in the market and that agents may have different reservation values. Further, similar to the laboratory treatments, a randomization scheme determined if the experiment concluded after round 5: a six-sided die was tossed and if a 1-5 was thrown the game ended, otherwise another round was played. After this round, again a die was thrown and the game ended with a  $5/6$  probability.

Third, the monitor explained how earnings (in excess of the commission fees) were determined: for sellers, the difference between the actual contract price and the minimum reservation value determined producer rents. Likewise, buyers' earnings were determined by the difference between the contract price and the maximum reservation value. Several examples illustrated the irrationality associated with selling (buying) the commodity below (above) induced values.

Fourth, the commodities used in the framed field treatment were various goods (e.g., CDs and DVDs), which were useless (cracked, split, or in pieces taped together), making them valueless outside of the experimental market. Thus, the assignment given to sellers was clear, and an everyday occurrence: sell the good for as much as possible. Likewise, the task confronting buyers was also clear: enter the marketplace and purchase the good for as little as possible. The goods and participating sellers were clearly marked to ensure that buyers had no trouble finding the commodity of interest. Fifth, buyers and sellers engaged in several short practice periods to gain experience.

In Step 3, subjects participated in the market. Each market session consisted of at least 5 market periods that lasted 5 minutes. After each 5-minute period, a monitor privately gathered with buyers and gave them a new buyer's card, while a different monitor privately gave sellers a new seller's card. It should be noted that throughout the no communication sessions, careful attention was given to prohibit discussions between sellers (and buyers) that could induce collusive outcomes. In the communication treatments seller communication was permitted (unbeknownst to buyers). I followed Davis and Holt (1998) in the information allowance. For example, subjects were not allowed to discuss nonpublic information such as unit costs, post-session side payments, or threats of a physical nature. Step 4 concluded the experiment – after subjects completed a survey, they were paid their earnings in private (Appendix B contains the survey).

This procedure was followed in each of a series of treatments, which are summarized in the middle column of Table 1. The first treatment, denoted FramedNCsymm (framed field experiment with no communication, symmetric demand and supply), contains 12 buyers with unit-demand and 4 sellers, who each have 3 units to supply.<sup>8</sup> No communication is allowed among sellers or among buyers, but importantly, after each transaction is recorded, everyone in the market is made aware of the price. This is done via a board and an announcement to the experimental subjects. Table 2 and Figure 1 presents buyer and seller induced values.<sup>9</sup> In Figure 1, each step represents a distinct induced value that was given to buyers (demand curve)

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<sup>8</sup> Consistent with the previous literature, these treatments are production-to-demand: sellers only pay the cost of producing the good upon sale. An interesting exploration is to determine whether similar properties hold in two-stage cases: the seller pays to produce the product and then takes it to the market, with an excess supply being valueless. Pilot treatments along these lines show much more volatility in prices, but similar equilibrating properties. I reserve this discussion for another occasion.

<sup>9</sup> It should be noted that (unbeknownst to buyers and sellers) within each session all agents received at least two reservation values that would place them “in the market” if competitive predictions prevailed. For the collusive sessions, each agent received at least a single reservation value that would place them “in the market” if monopoly predictions prevailed.

and sellers (supply curve). The efficient competitive outcome yields \$37 in economic rents per round, with associated equilibrium price between \$13.00 and 14.00 and a quantity of 7. This represents the extreme point of intersection of buyer and supplier rent areas in Figure 1. Under competitive assumptions, producer surplus ranges from \$15-22, with the remaining rents (\$15-\$22) accruing to buyers.

Treatment FramedNCasymm (denoting framed field experiment with no communication, asymmetric supply) is identical to FramedNCasymm except for an important deviation: the supply schedule is perfectly elastic at \$2, \$7, and \$13.50, depending on treatment. Thus, in one treatment the 4 sellers have a constant marginal cost of \$2 for each of their 3 units, in another \$7, and in a third the 4 sellers have a constant marginal cost of \$13.50 for their 3 units. In these treatments, the efficient perfectly competitive outcome yields \$144, \$84, and \$18.50 in rents per period for the \$2, \$6 and \$13.50 treatment, which occurs where competitive price theory predicts the static price/quantity equilibrium of Price = \$2-\$9 (\$2 treatment), \$7-\$9 (\$7 treatment), and \$13.50, and Quantity = 12, 12, and 7 to be reached. The \$13.50 sessions represent a stringent test of neoclassical theory, as in equilibrium the entire rents are allocated to buyers due to the five excess units.

Treatment FramedCasymm (denoting framed field experiment where seller communication is allowed, asymmetric supply) is identical to Treatment FramedNCasymm, but now communication is allowed between sellers. For instance, explicit seller communication between rounds to induce attempts to fix prices is permitted, as sellers (unbeknownst to buyers) are permitted to discuss strategies between each period. Importantly, a monitor records for each period whether there is an agreed upon collusive price and therefore “cheating” on the agreement can be observed (by the monitors). Within this treatment are again several variants. For

example, I include three constant marginal cost levels: \$2, \$7, and \$13.50. Thus, similar to above, each seller is informed that he cannot sell profitably at a price below the constant marginal cost. Under these treatments, the efficient joint profit-maximizing strategy for sellers yields a price of \$11 or \$12 in the \$2 treatment, \$14 in the \$7 treatment, and \$17 in the \$13.50 treatment. This provides economic rents of \$90, \$49, and \$10.50 in every period for the sellers.<sup>10</sup>

I build on the FramedCasymm \$7 treatment in several iterations, as follows (each builds on the preceding treatment with noted deviations):

1. Treatment Framedinf: I provide sellers with an infinite supply at a \$7 marginal cost.
2. Treatment Framednoprice: I do not announce price realizations after a transaction is executed.
3. Treatment FramedShock: players are told that I have randomly pre-selected one period in which demand will be such that payoffs will be zero (i.e., no sales will be possible), and that no one will be told either before or after which period was the demand shock period;
4. Treatment Framedtable: rather than conduct the experiment in 5-minute periods, I actually place the good on the seller's table and inform them that occasionally a potential buyer will be stopping by to negotiate a purchase of the good. I attempt to space the visits in a fashion similar to visitation rates of the parallel goods in this market. The experiment lasts the entire day/weekend of the market and rather than having a structured period-by-period event, I have each buyer enter the market with ten different induced values which are taken from the demand curve used in the above treatments.

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<sup>10</sup> Of course, monopoly prices and rents are based upon the setting of a single monopoly price. In our environment this is akin to sellers agreeing upon a single take-it-or-leave-it price. Such a strategy does not permit sellers to

For example, buyer 1 is given induced values 19, 14, 17, 13, and 14 for the first 5 “periods” to approach seller group 1 and is given 18, 9, 10, 17, and 11 for the last 5 “periods” to approach seller group 2 (see Table 2, buyer 1 and 2 values).<sup>11</sup> This approach is used to ensure that the exact same induced values are utilized across treatments. And, buyers are informed that they have five minutes to execute a transaction with that induced value. After the five minutes are used, or directly after the buyer executes a transaction, the buyer receives a new induced value and returns to the market. Sellers are free to discuss pricing issues in the presence of monitors, but are prohibited from discussing nonpublic information such as unit costs, post-session side payments, or threats of a physical nature. This treatment is meant to move from a “hot” environment to a “cooler” environment that might more closely represent actual market conditions.<sup>12</sup>

5. Treatment Framedtable2: I include 2 sellers rather than 4 sellers. In some of the pairs I have sellers face the exact same demand curve in FramedTable; in others I have sellers face the same effective individual seller demand as FramedTable (i.e., every other step is

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engage in price discriminating behavior and may not necessarily reflect the optimal selling mechanism.

<sup>11</sup> Buyer 2 is given induced values 19, 14, 17, 13, and 14 for the first 5 “periods” to approach seller group 2 and is given 18, 9, 10, 17, and 11 for the last 5 “periods” to approach seller group 1. To mimic the lab experiments, I split buyers into groups that approach the seller groups uniformly. Thus, buyers 1-12 approach seller groups 1 and 2; buyers 13-24 approach seller groups 3 and 4 (there are 4 seller groups). In this way, each seller group is approached by the entire demand in one session of the lab experiments (i.e., is approached with each of the induced values in Table 2) and each buyer approaches the “4 seller market” 5 times, consistent with the lab experiments (though in this case buyers approach 2 “4 seller markets” rather than one). In aggregate, therefore, each 4 seller market is approached 60 times (12 buyers frequenting the market 5 times each).

<sup>12</sup> One result from the psychology literature is that there are important behavioral differences between short run (*hot*) and long (*cold*) run decision making. In the hot phase, visceral factors and emotions might prove quite important, whereas in the cold phase immediate reactions are more carefully suppressed. In this sense, the hot/cold settings can lead to much different behaviors (see, e.g., Loewenstein and Schkade, 1999). Loewenstein (2005) reviews some of the empirical evidence on behavioral differences across cold and hot states.<sup>12</sup> The evidence is sparse within the experimental economics literature on this issue, but there is one study that provides a first test of the gift exchange hypothesis in an actual labor market (Gneezy and List (2006)). They find that worker effort in the first few hours on the job is considerably higher in the gift treatment than in the non-gift treatment, but after the initial few hours no difference in outcomes was observed.

taken out of the demand curve in Figure 1).<sup>13</sup> Additionally, I vary partnership arrangements: in some cases I have two sellers who are part of an explicit agreement outside the experiment as partners, in other variants I make two sellers partners who are not part of an explicit agreement outside the experiment.

6. Treatment FramedHighstakes: I multiply all *seller* earnings by 5. Thus, if a seller earns \$40, then I make it \$200 in this treatment.

In summary, the monitor gives each buyer and seller a reservation price(s) for units of the good and allows agents to engage in bilateral haggling and bargaining until they enact a contract(s) or the trading period terminates. After each contract is completed, (i) a monitor posts the exchange price (when applicable) on a public board, and (ii) monitors inform all buyers and sellers of the exchange price (or signal) in case they are removed from the public board.

Table 1 provides a summary of the various framed experimental treatments and provides sample sizes. In total, I observe the behavior of more than 300 unique buyers and 116 unique sellers in these treatments. Since buyers and sellers are observed several times, I again have thousands of observations—both completed and uncompleted transactions—in this set of treatments.

### **C. Natural Field Experiment Particulars**

The goal of the natural field experimental design is to begin with an examination of whether sellers' collusive agreements are robust. In doing so, a natural test is whether the insights gained from the tightly controlled experiments discussed above transfer to the domain of interest, which in this case is the unfettered marketplace.

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<sup>13</sup> The arrangement of buyers and sellers is otherwise identical as Treatment FramedTable: buyers approach the market 10 times and seller groups are approached uniformly (of course, in the cases where demand is cut in half, those seller groups are approached 30 times rather than 60).

My experimental approach in this section is similar to the natural field experiments in List (2004; 2006), and attempts to maintain the integrity of the framed field treatments. Under this approach, my goal is to maintain a level of control that permits clean identification while matching the real-world setting that economic theory attempts to explain. Importantly, in these treatments subjects do not know that they are taking part in an experiment. Thus, the natural field experimental strategy employed in this section might lead to different results compared to insights gained from the framed field treatments, but a rigorous examination of behavior in a naturally occurring environment is an important step in testing the validity of economic theories.

The natural field treatment (hereafter denoted Field), which includes data gathered from the same flea markets (as described above) in the greater metropolitan area in early 2006, has participants following four steps: (1) consideration of the invitation to participate in an experiment, (2) learning the market rules, (3) actual market participation, and (4) conclusion of the experiment and exit interview.

In Step 1, potential subjects approached the monitor's dealer table and inquired about various goods—CDs, DVDs, etc.—displayed on the table. The monitor then asked if the agent was interested in participating in an experiment that would last about 15-25 minutes. If the agent agreed to participate, a monitor thoroughly explained the experimental rules.

The monitor began by explaining how earnings were determined: the difference between the price paid for the commodity and the maximum reservation price determined market earnings. Similar to the treatments above, I used the induced values from Figure 1 (with the necessary adjustments made to values when the marginal cost was less or greater than \$7).<sup>14</sup> The

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<sup>14</sup> For instance, in the cases where the marginal cost was \$5, I shifted the demand function down \$2; thus in this case the intercept was \$17 and the lowest induced value was \$7. Also note that subjects were informed that if they purchased the good above their reservation value that the difference would be taken out of their profits. No subject purchased the good at a price above their reservation value.

commodities that were used in the experiment were similar to the goods that were on the monitor's table (in which the consumer initially expressed an interest in purchasing).<sup>15</sup>

To gather the seller subject pool, I consulted with my mole to determine which sellers had collusive arrangements with one another and with my mole. A typical agreement in this marketplace was a constant percentage mark-up rule. For example, since in the majority of cases the sellers purchased product jointly from the same supplier, they were fully aware of one another's marginal cost for the item. They then applied a specific rule, such as "double the price," or "mark-up by 100%." From discussions with my mole, I earmarked 27 sellers across 8 different local flea markets who were part of an explicit 2, 3, or 4 seller conspiracy (in some cases with my mole). Note that these sellers often had agreements on multiple goods, and agreements with multiple sellers, thus I can examine robustness across classes of goods and number of conspirators in an agreement. Also, note that these same sellers are included as subjects in some of the experiments above. I attempted to space the experiments—in some cases across several months—in a manner that would preclude cross-contamination.

A few noteworthy design issues should be mentioned before proceeding to the results discussion. First, each dealer was approached several times during the natural field treatment. The spacing of visits was such to attenuate any suspicion—one example is that dealer  $i$  was approached by agent  $n$  on Saturday afternoon for good  $j$  and by agent  $m$  on Sunday morning for the same good. The seller was then approached on separate days in a similar fashion as well.

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<sup>15</sup> Note that the subject having an interest in the good provides attractive realism in that dealers naturally face this subject type in the marketplace, but it comes at a potential cost—dubious consumers may use this bargaining session to arrange for purchase of the good from the dealer later rather than purchase it in the experiment. To avoid this potential issue, I ensured the subject that if s/he would like to purchase the good from me after the experiment I would guarantee to sell the good to him at the same price at which s/he purchased the good from the dealer, upon proof of a receipt.

And, the ordering of the visits was random. I observed no ordering effect, so I suppress further discussion of this issue.

Second, unlike audit studies that test for market discrimination (see Riach and Rich, 2003), I am directing the agent to buy the good. In this sense, these are not transactors who obliquely discontinue bargaining if the seller accepts an offer; these are actual transactions and I am obtaining actual sales prices. And, since transactions are typically in cash at flea markets, I provided the necessary funds to purchase the goods when necessary. Third, note that great care was taken to ensure that the data were gathered from interactions that would naturally occur in the marketplace. Subjects were entering the market to buy goods that were very similar to the good that I had them buying. Fourth, I induce a similar demand structure as to what I induced in the various framed field treatments.

Similar to the framed field treatments, each confederate had a 5-minute time limit imposed; akin to the framed field treatments, in very case but a few, interactions were completed well before the 5 minutes were consumed. It should be noted that throughout the experiment the sellers were not aware that an experiment was occurring. This ensured that the process was as natural as possible for sellers. I should stress that my confederate buyers did not know that this was a study on collusion; rather they were informed that it was “an economics experiment,” and that this was to be quite completely private. Step 4 concluded the experiment—after subjects completed a confidential survey they had begun earlier, they were paid their earnings in private.

In total, I observe the behavior of 27 sellers who were each visited by several confederates. I used 82 buying confederates, who approached anywhere from 2-10 sellers. None of the confederates approached the same seller twice. And, similar to the treatments

above, the confederates only purchased 1 unit per seller per visit, and bundling of items was not allowed (i.e., buyers did not purchase another good to bundle with the good of interest).

### **III. Experimental Results**

Similar to the experimental design structure of Section II, I begin with a summary of the overall data patterns and proceed to discuss the empirical support for drawing these conclusions in three separate sub-sections. First, I find similar behavior across subject pools in tightly controlled settings: both students and marketers display a fair amount of cooperation, even in cases where it is difficult to maintain such ties. Second, the context of the experiment matters greatly for the market subjects: when made aware that they are playing a collusion game, they are much more likely to maintain collusive pricing arrangements. This result holds whether or not the subjects are paired with other sellers who they have conspiracy agreements outside of the experiment (though this matters as well). Furthermore, as the treatments slowly move toward the naturally occurring setting, collusive pricing arrangements remain quite robust and even strengthen: under conditions that theory would suggest are quite austere (little knowledge of past price or quantity realizations), few sellers cheat on their collusive agreements.

Difficulties arise, however, when mapping such behavior to outcomes in the treatment that had my confederates approach the same sellers who, unbeknownst to them, were taking part in an experiment. Using the exact same underlying demand structure and experimental protocol, when sellers do not know that they are part of an experiment they cheat on the explicit agreements much more often. Such cheating rates are considerably higher than all of the other observed cheating rates among sellers in the various other treatments. Nevertheless, perhaps surprisingly, in aggregate, the closest predictor of field cheating rates are the student and market subjects' behavior in sterile laboratory treatments with neutral language instructions. Yet,

making use of the individual data from these neutral language treatments by matching marketer behavior in the lab and in the field, I find that cheating in one environment is not a good predictor of who cheats in another.

The “control” treatments also yield some insights. For instance, different from Chamberlain (1948), I find a strong tendency for exchange prices to approach the prediction of competitive market equilibrium theory in this institution. I view this result as important because it suggests that the invisible hand of Adam Smith is much more powerful than many would surmise: Walrasian tâtonnement or centrally occurring open outcry of bids and offers are not necessary for the predicted equilibrium in the field to be observed. This result is consistent with List’s (2004) finding using a much different subject pool (sportscard and pin enthusiasts). I now turn to a more detailed description of the results separated into the three major experimental types.

### **A. Lab Experimental Results**

Table 3 provides a snap shot of the laboratory results across the student and market subject pools. The table provides the raw proportions of choices for each of the three price categories across subject pool and treatment type. Table 3 also includes average profits per period and a measure of efficiency (measured as total group profits divided by total profits available (\$2)).

A first interesting data pattern is that there is a fair amount of cooperative behavior in these treatments. The percentage of collusive choices ranges as high as 61.4% in treatment LabIIIMC, and even in cases with unobserved demand shocks and complete anonymity between sellers the collusive strategy is chosen in more than 45% of the cases. The general data patterns

are consistent with previous efforts in this literature, in that the degree of collusive play is quite high.

These data patterns are confirmed when one considers various statistical tests. In the tests, I begin with a conservative approach by first calculating the pair-wise mean collusive choices across the periods and then exploring patterns of these means. Thus, rather than each individual providing 10 data points, under this approach each pair provides one observation. Several insights are obtained. First, using a test of proportions, I find that the proportion of collusive play in each treatment is significantly greater than zero. Further, I cannot reject that at least half of the observed choices are collusive. In the literature, such data would be likely interpreted as supporting aggressive antitrust policy since the often embraced comforting notion that cartels are fragile coalitions cannot be broadly asserted from these data.

Second, results from a Mann-Whitney rank-sum test of treatment differences suggests i) that collusive rates are higher in treatment Lab IS than treatment Lab IIS, and that ii) Lab IIIS collusive rates are higher than Lab IIS rates at conventional significance levels. The first comparative static result highlights the increased difficulty associated with colluding when demand shocks are present and unobservable. Examining a level deeper, the data are consistent with subjects using trigger strategies, in that in many cases “price wars” occurred after one player chose to undercut prices or after the 5<sup>th</sup> period demand shock. Players reacted similarly to these events and this lead to less collusive play in the demand shock treatments. Yet, I do observe a stronger tendency for players in treatments Lab IIS and Lab IIIS to undercut price even before period 5, perhaps believing that they can hide behind the veil of the secret demand shock.

The second result provides an indication of the importance of anonymity between subjects.<sup>16</sup> Consistent with List et al. (2004), who found that the degree of anonymity between subjects influenced their propensity to contribute to a public good in a one-shot decision, these

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<sup>16</sup> Anonymity can usefully be broken down to three configurations: relative to other experimental subjects, relative to the experimenter, and anonymity that rules out pecuniary gains through reputation formation. I consider only

data suggest that subjects cooperate to a greater extent when they know the identity of their partner, and vice versa.<sup>17</sup>

Alternatively, an anonymity effect is not found in the marketer's data set, in aggregate. Yet, if one considers the data more carefully, a pattern consistent with the importance of anonymity prevails. Using the insights gained from the survey in Appendix B, I examine whether the subset of marketers who have had previous interaction is influenced by the anonymity condition. I find that those partners in Lab IIM who have had previous interaction not only collude more often than other groups in Lab IIM, but also collude more often than subjects in Lab IIM.<sup>18</sup>

Further, I find that collusive rates in LabIIMC are greater than collusive rates in LabIIM, suggesting that context matters a great deal in the marketer experiment. Interestingly, the context serves to heighten cooperation rates to levels that exceed even the most cooperative treatments in the student sample. During the post-session survey, I informally discussed the experiment with subjects in the LabIM, LabIIM, and Lab IIM treatments and a majority did not place a collusive context on the game when asked about its nature.

To complement the above analysis, I estimate empirically an equation of individual decisions that explicitly controls for the panel nature of the data. Specifically I estimate  $C_{it} = \beta'X_i + e_{it}$ ,  $e_{it} \sim N[0,1]$ , where  $C_{it}$  equals unity if agent  $i$  made a collusive choice in period  $t$ , and

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comparative static changes in the first.

<sup>17</sup> This result fits in nicely with a broader literature on how dimensions of anonymity affect giving in laboratory games. As Levitt and List (2005) note, this rich literature includes many studies (see, e.g., Allen, 1965; Cason and Mui, 1997; Bohnet and Frey, 1999a, 1999b; Frolich et al., 2001; Burnham, 2003; Haley and Fessler, 2005). For instance, Haley and Fessler (2005) find that giving in a dictator game significantly increases when a pair of eyes is shown on the computer screen with which the dictator makes his allocation. This simple manipulation -- meant to signal that the subjects' actions were being observed -- increased the proportion of nonzero givers from 55% in the control treatment to 88% in the eyespot treatment. Likewise, Allen (1965) reports that increases in privacy reduce conformity. Research on hand washing behavior even suggests that individuals are more likely to conform with the social norm of washing when they are being observed (Harris and Munger, 1989).

<sup>18</sup> Of course, since I do not know which partners in Lab IIM have had previous interaction, I cannot determine

equals zero otherwise;  $X_i$  includes the treatment effect dichotomous variables and the observables from the survey.<sup>19</sup> Empirical results are broadly consistent with the findings discussed above so I make these results available upon request.

## B. Framed Field Experimental Results

Table 4 provides summary results for the series of framed field experiments. Entries in Table 4 are at the period level and include average price and its standard deviation and quantity traded. Table 4 can be read as follows: in period 1 of the NCSymm treatment, on average in the two sessions, five goods were purchased at an average trading price of \$14.00 (std. dev.=1.71).

Results from the symmetrical demand and supply structure provide evidence that competitive price theory does an adequate job of organizing the data. For example, 4 out of 5 market periods had average trading prices within the predicted \$13-\$14 competitive range—and several executed trades (41 of 62) were within the competitive market range over the five periods. In addition, the ultimate two periods had the theoretically correct quantity level of units traded. Finally, efficiency levels were quite high, reaching 95% in the latter two periods.<sup>20</sup> I provide Figure 3 as a representative session, highlighting the market dynamics.

Data from the NCAsymm sessions also provide some evidence of convergence. For example, as average price in period 1 of the \$13.50 treatment is \$15.12, yet by period 5 the average price decreases to \$13.59, and the allocation of rents is almost entirely tilted toward

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whether this result is due to treatment or selection.

<sup>19</sup> I estimate Butler and Moffitt's (1982) random effects probit model via maximum likelihood, where the likelihood function for can be written as  $L = \prod_i L_i = \int_{-\infty}^{\infty} (2\pi)^{-1/2} \prod_i \exp(-e_{it}) \phi(g_{it} q_{it})$ , where  $g_{ij} = 2C_{ij} - 1$ ; and  $q_{ij} = \beta' X_{ij} + [\text{corr}(e_{ij}, e_{is}) / (1 - \text{corr}(e_{ij}, e_{is}))]^{1/2} e_i$ . Estimation of this particular model is quite complex, but is amenable to Hermite integration. To estimate the model, we use an eight-point quadrature and use the Berndt et al. (1974) estimator to compute the covariance matrix.

<sup>20</sup> I define efficiency as the portion of total surplus (\$37 in the symmetric case) captured. Thus, a 95% rate of efficiency represents profits of at least \$35 in that period.

buyers. This tendency toward neoclassical expectations yields 11 of 14 last period trades at the equilibrium price of \$13.50. This insight is shown in Figure 4, which plots sequential transaction prices for one of the NCAsymm sessions. Figure 4 shows that prices begin a shade above the equilibrium price, but in the terminal periods many of the transactions are at the expected price of \$13.50. While the other session is clearly not as well-behaved, a similar data trajectory is observed. Data from the \$2 and \$7 treatments also show similar signs of convergence, though the \$7 data is not as strong, and by the fifth period is only bordering on neoclassical expectations. This is likely a result that in equilibrium there are no excess sellers. Nevertheless, quantity predictions are met in each of the treatments.

As a whole, these data indicate that centralized authority of prices, either via the Walrasian tâtonnement or double-auction mechanism, is not a necessary condition for market outcomes to approach neoclassical expectations. This result is at odds with Chamberlain's (1948) seminal results, which suggest that such institutions yield prices that are too low and quantities that are too high. I believe that having incentivized agents who are experienced in their roles and tasks, and who are able to gain experience with the rules and dictates of the market are likely reasons for the disparate results. Importantly, these results show that if left unfettered this institution has the ability to allocate goods and services efficiently.

Economists generally agree that explicit communication amongst sellers will lead to attempts at price fixing, yet little consensus exists as to the ultimate impact of such attempts on market prices. Treatments CAsymm\$2, CAsymm\$7, and CAsymm\$13.50 provide some insight in this regard. In the field, if the costs associated with enacting, monitoring, and maintaining collusive arrangements are prohibitive, then such agreements will be rendered ineffective. These treatments likely give collusive arrangements a good chance to succeed by providing a simple

platform for enacting collusive arrangements, by immediate price posting, and permitting communication after each period.

The data summary in Table 4 provides evidence that opening up communication channels among sellers considerably influences pricing outcomes. While the pricing policies are in many periods different from the efficient joint profit-maximizing strategy for sellers (price of \$11 or \$12 in the \$2 treatment, \$14 in the \$7 treatment, and \$17 in the \$13.50 treatment), there is a tendency toward these price levels. Further, there is a tendency for prices to be above the competitive predictions in these settings (\$2-\$8 for the \$2 treatment; \$7-\$8 for the \$7 treatment; and \$13.50 for the \$13.50 treatment). Overall, the data point to some evidence that communication between sellers can inhibit efficiency and shift rents from buyers to sellers.

An interesting point of departure is to consider the inter-workings of the collusive arrangements. As noted earlier, I monitored the discussions between sellers and noted their bantering about optimal strategies. Invariably, pricing based on unit cost dominated the conversation. A first inclination for many of the sellers was to simply “double the price” from the marginal cost (even though sellers were prohibited from discussing costs, it was clear that they could intuit that others had identical cost structures from the discussions). This propensity can be seen in the data in Table 4, but is even more perspicuous in the practice rounds, where sellers’ commitment to the “double rule” in some cases left them selling their goods immediately (\$2 treatment) or not selling anything at all (\$13.50 treatment). This is evident in the early periods of the experiment as well, but seller learning led to comments such as “I think they will pay a lot more,” or “we have to lower the price or we will not sell anything” that lead to the data patterns in Table 4. The data patterns are interesting in that they appear to approach the efficient joint profit-maximizing strategy. This is more apparent in the \$7 and \$13.50 treatments.

The next set of treatments— FramedInf, FramedNoPrice, and FramedShock—draw the market experiments closer to the naturally-occurring environment, but as Table 4 makes clear, none of these three treatments has a considerable influence on outcomes. While there is some influence from each of the treatments in the direction that theory would predict—downward pressure on prices—I continue to find that the collusive arrangements are stable and that buyers pay much more for wares than they would under a comparable setting with no seller communication.

The final set of treatments moves the experimental environment to perhaps an even more natural setting whereby dealers place the experimental good on their dealer table and occasionally are visited by my buying agents. Because these data do not have the natural chronological ordering as the other framed field treatments, I summarize these results in Table 5. Table 5 presents the data by the average negotiated and final transaction price as well as the proportion of each that is considered cheating on the collusive agreement that the sellers have in place. The former data are gathered via each buyer filling in the appropriate portion of the survey in Appendix B upon his return from each “visit” to the market. Each buyer filled in what the approached dealers offered—both initially and a final offer price—before receiving a new induced value for the next round of visits. I also fill in these figures for the treatments discussed above, but since the various framed field bilateral bargaining markets move quickly within any given period, the negotiated prices with each seller are not available (indeed, buyers do not even recall many of the offers that were made in the bargaining process).

Table 5 provides some interesting insights.

### **C. Natural Field Experimental Results**

## **IV. Discussion**

In this study, I depart from a traditional empirical investigation of neoclassical competitive theory by using the tools of experimental economics in an actual marketplace. Examining behavior in several treatments that bridge the lab and the field provides some new insights. In this epilogue I summarize the results in three subsections: speaking to theorists, speaking to policymakers, and methodological contribution.<sup>21</sup>

### **A. Speaking to Theorists**

On many dimensions the data are consistent with extant theory. First, demand and supply shapes provide accurate direction of price and quantity realizations. Second, when communication is allowed between sellers, prices are higher than in cases when no communication is allowed. Both of these results are fundamental and provide insights into whether, and under what circumstances, the natural allocation mechanism can yield efficient outcomes.

Third, when communication is permitted, several comparative static insights obtained match theory. For instance, evidence from the natural field treatment suggests that successful price fixing conspiracies are more likely with small numbers of firms.<sup>22</sup> This finding is in line with early structure-conduct-performance theorists (e.g., Bain, 1951) who argued that an increase in concentration facilitates collusion.<sup>23</sup> The intuition is that as the number of firms increase the

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<sup>21</sup> Some readers might find these distinctions familiar. Alvin Roth used the first two in his 1985 lecture to the Fifth World Congress of the Econometric Society.

<sup>22</sup> This result is consonant with Hay and Kelley (1974), who note that most Department of Justice cases involve 10 or fewer firms.

<sup>23</sup> This lead to an extensive literature on the topic. An important line of inquiry was Demsetz (1973, 1974), who critiqued this notion extensively and Schmalensee (1987), Sutton (1998) and Symeonidis (2002) who empirically

value of each firm's share of collusive profits declines—i.e., there are more firms claiming stake to the monopoly profits after entry.

As stated, this result represents an easy target for criticism since it is unknown whether this data pattern in the natural field experiment is due to treatment or selection (perhaps more trustworthy sellers are those who are better able to coordinate smaller collusive groups). In this case, the framed field treatments provide supporting evidence. Given that I randomly determine group sizes in the framed field treatments and randomly place sellers in these groups, I am able to observe how exogenous changes in group size affects successful price fixing. Comparing data across *FramedTable* and *Framed2Sellers* provides supportive evidence that firms in larger collusive groups are more likely to cheat on their collusive arrangements. A corollary to this result is that the total market surplus foregone due to collusive arrangements is significantly larger when the collusive groups become smaller.

Another comparative static that matches theory is the observation in the natural field treatment that sellers cheat less when they have multiple collusive arrangements with another seller. This was manifested in situations where sellers had more than one agreement in a distinct geographic market (in one flea market) and when sellers had agreements that spanned more than one geographic market (across different flea markets). Benoit and Krishna (1985) provide a theoretical basis for this result. Intuitively, extensive interaction makes the cost of cheating higher and might deter defections on the margin because they are no longer profit maximizing. Given the endogeneity of such arrangements in naturally-occurring data, it is difficult to pin down this effect empirically, and this issue plagues inference made from the natural field experimental data as well.

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tested various models.

Again, however, data from the framed field treatments can help since I formed partnerships randomly. Therefore, I can compare cheating rates across sellers. For example, in some cases sellers in a framed field treatment were paired with a seller with whom they had an outside collusive arrangement. In other framed field treatment cases, some sellers who had outside collusive arrangements were paired with a seller whom they did not have outside collusive arrangements. Considering data across all of the framed field treatments, I find that sellers are less likely to cheat in the framed field experiments when paired with a seller with whom they have an outside arrangement; indeed, when using the framed field data to estimate a probit model I find that the probability of cheating is a decreasing function in the number of outside arrangements the seller has with their experimental partner.<sup>24</sup>

A third comparative static insight gained from the naturally occurring data is that sellers cheat more on higher volume days. If one considers this particular type of market period as part of a business cycle fluctuation, and if such demand shocks are independently and identically distributed, then this counter-intuitive result is consistent with Rotemberg and Saloner's (1986) model.<sup>25</sup> Nevertheless, this result might equally suggest that sellers believe it less likely to get caught cheating on busy days and therefore engage in more cheating during busy market periods. Comparing data from *Framed2Sellers* and *FramedHighStakes* presents some, albeit imperfect, evidence that is consonant with the results from the natural field experimental data.

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<sup>24</sup> The model also provides evidence consistent with the notion that selection is important, as those sellers who have extensive outside arrangements cheat less in the framed field treatments than those with no or fewer outside arrangements. This result is also consistent with the idea that sellers do not cheat in the framed field treatments because they do not want other sellers to believe that they are untrustworthy (and further perhaps these sellers will inform other sellers of this fact, inducing fragility of collusive agreements).

<sup>25</sup> Haltiwanger and Harrington (1991) present a model whereby the demand shocks are serially correlated—for example, unusually high demand today induces an increased probability of unusually high demand tomorrow. In this model, price wars are more likely to occur in periods of downturns, not upturns (as in the Rotemberg and Saloner (1986) model) since the collusive profits increase (decrease) with a positive (negative) demand shock.

In sum, each of these three examples suggests that critical evaluation of the natural field experimental data reveals that sharp tests of the theoretical predictions are not possible with those data alone. Further, some might argue that the framed field experimental data used in isolation might not present a compelling case. Combined, however, the data highlight the gain in inferential power of data observed across several domains.

## **B. Speaking to Policymakers**

For more than 100 years, economists and historians serving as expert witnesses and research scholars, commissioners, and jurists have labored to assess the “effectiveness” of cartels (Conner, 2005). Throughout the years, those critical of aggressive antitrust policy have embraced the Stiglerian notion that cartels are fragile coalitions, fraught with cheating that will eventually produce outcomes that more closely mirror competitive expectations. For example, when the OPEC cartel began to flex its muscle and influence world petroleum prices in the early 1970s, several leading economists predicted its imminent demise. Adelman (1972) wrote that “Every cartel has in time been destroyed by one and then some members chiseling and cheating...”(p.71).

An excellent recent overview of cartel activities is provided by Levenstein and Suslow (2006). They conclude that even though many cartels collapse within a year, the average cartel in their cross sectional samples lasted between 3.7 and 10 years (see p. 51, Table 1).<sup>26</sup> The effects that cartels have on prices is less well understood. Conner's (2005) survey identified hundreds of published social-science studies of private, hard-core cartels that contained 674 observations of long-run overcharges. The primary finding is that the median cartel overcharge for all types of cartels over all time periods is 25%: 18% for domestic cartels, 32% for

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<sup>26</sup> This bimodality of cartel survivorship has been the subject of intense research and our understanding of its causes is just beginning to crystallize. While cheating is certainly an important factor, Levenstein and Suslow (2006, p. 45)

international cartels, and 28% for all successful cartels (the overcharge rate is calculated by comparing cartel prices to a competitive benchmark).

In these regards the experimental data presented herein provide some interesting parallels. First, consistent with Stigler (1964), the natural field experimental data suggest that maintaining strict compliance to the collusive agreement is difficult to sustain in a repeated game with secret price cuts and demand uncertainty. In this manner, those data are consonant with the notion that inherent problems associated with maintaining collusive agreements *might* preclude conspiracies from having considerable influence on prices in similar market structures. Second, in those cases where collusion was stable (the lab and framed field treatments), however, much surplus was lost. Sellers were able to exact large price increases through explicit collusive arrangements and such conspiracies frustrated market efficiency considerably.

These contrasting insights from nearly identical experimental situations highlight a general lesson that policymakers might be interested: transference of results across situations (or time, or different industries). Theory teaches us that the determinants of cartel success are varied, and the empirical data herein highlight that sellers are quite sensitive to these factors, even ones that many might consider ancillary. In this spirit, one prominent aspect of the data is that one should take great care when generalizing results. Theory and comparative static empirical insights can inform us of general principles, such as when and where to expect collusion, and when to suspect that it is having a great influence on pricing and allocation decisions. But, statements on the actual existence, or efficacy, of collusive arrangements are quite difficult to make without actually investigating the industry itself. Using an analogy, comparative static results such as those in this study can provide a hint about where to look for

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argue that “the most frequent causes of cartel failure are entry and bargaining problems.”

fire, but cannot determine whether there is an actual fire worth extinguishing merely from smoke detection.

### **C. Methodological Content**

Beyond permitting sharper inference, an advantage of building a bridge between the lab and the field is that one can explore the generalizability of behaviors across situations, or domains. For years psychologists have questioned cross-situational consistency of behavior (see, e.g., Mischel, 1968; Ross and Nisbett, 1991), and the results have not been entirely promising. Under the approach in this study, I can pursue something different: whether important factors of the experimental environment, and associated experimental procedures, systematically influence behavior, and how we can use insights on such factors to provide more accurate predictions. In this regard, comparisons of interest include observing behavior of (i) identical individuals across the lab and the field, (ii) agents drawn from the same population in lab and field experiments, where the lab selection rules might be different from the way in which markets select individuals, and (iii) individuals drawn from different populations.

As a guide, I use the framework put forth by Levitt and List (2005). For example, selection rules into the actual experiment might be important. Further, they argue that being part of an experiment in and of itself might induce certain types of behaviors. In the current case, conditional on making collusive arrangements, taking part in an experiment might induce sellers to more readily maintain their collusive promises. More broadly, the conditions set forth in an experimental situation might induce behavioral tendencies that are simply not observed in the field.

I think it is fair to say that the stylized fact among psychologists is that there are weak correlations between behaviors in different settings (even across lab settings). For instance,

psychologists (Hartshorne and May, 1928) discovered long ago that people who cheat in one situation aren't the people who cheat in another. That doesn't mean there is something wrong with one of these situations. Rather, it means either that (a) there isn't a general cross-situational trait called "honesty," or (b) the subjects saw one situation as relevant to honesty (cheating on a spouse) and one as irrelevant (cheating on taxes).

More to come.....

## Appendix A Experimental Instructions—Lab

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[..] denote changes made for context treatments.

This is an experiment in economic decision making. If you follow the instructions carefully and make good decisions you can earn a considerable amount of money. You will be paid in private and in cash at the end of the experiment.

It is important that you do not talk, or in any way try to communicate, with other people during the session. If you have a question, raise your hand and a monitor will come over to where you are sitting [help you] and answer your question in private.

The experiment will consist of at least 10 rounds. At the beginning of the experiment you will be randomly matched with another person in this room [another seller in the market]. You will be paired with this person the entire experiment. You will not know who [which seller] in this room [in the market] is in your group and they will not know with whom they are paired.

In each round, you will have the opportunity to earn money. At the end of the session, we will sum your earnings from each period and you will be paid in cash this amount privately. Let us get started with the actual decisions that are to be made.

Please see the payoff table. The choices across the columns of the table: P1, P2, and P3 [collusive price, undercutting price, punishment price] represent decisions of the other person [the other seller in this market]. Similar choices down the left side represent your own decisions. A given choice of the other person identifies a column in the table and your choice identifies a row. The cell where the column and the row intersect reveals the payoff [profit] that you will receive. This might seem confusing now, but some examples should help.

To make sure that you understand let us proceed through a few examples. Suppose that you both choose P1 [collusive price]. In this case you will each receive \$1 for the period. Alternatively, let us say that you choose P2 [undercutting price] while the other [seller] chooses P1 [collusive price]: you earn \$1.25 in this period and the other person [seller] receives nothing.

Let us try a few other choices. If you both choose P2 [undercutting price] then you will each receive 50 cents. Finally, if either of you choose P3 [punishment price], then both players receive nothing, regardless of the other choice.

[In addition, we have randomly pre-selected some period(s) in which your payoff will be zero regardless of what decisions you and the other person make (you will not be told either before or after which periods these are); these will occur in roughly 10% of the periods.] .....[addition for demand shock treatments]

Please find the decision sheet in your packet. Your task is to record your choice of P1, P2, or P3 [collusive price, undercutting price, or punishment price] for period 1 under the column headed “your decision”. We will then collect your sheet as well as the other person’s [seller’s], determine your profit, and then return the decision sheets. You will then make a choice for the next period. Starting after the 10<sup>th</sup> period, we will roll a standard die after each period, and if a “one” or a “two” shows up we will terminate the experiment and pay your earnings in private.

Let us now begin with 2 practice rounds. Note that there are no other people [no buyers] in these experiments, just you and the other person [seller] will determine your payoff [profit].

**Table 1A. Profit Cells**

---

		Other Person's Choices		
		P1	P2	P3
Your Choices	P1	\$1, \$1	\$0, \$1.25	\$0, \$0
	P2	\$1.25, \$0	\$0.50, \$0.50	\$0, \$0
	P3	\$0, \$0	\$0, \$0	\$0, \$0

**Cell entries represent: your payoff, other's payoff**

**Table 2A. Decision sheet**

---

<u>Period</u>	<u>Decision</u>	<u>Payoff</u>
Practice 1		
Practice 2		
1		
2		
3		
4		
5		
6		
7		
8		
9		
10		
11		
12		
13		
14		
15		

**Appendix B. Confidential Survey**

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These questions will be used for statistical purposes only. THIS INFORMATION WILL BE KEPT STRICTLY CONFIDENTIAL AND WILL BE DESTROYED UPON COMPLETION OF THE STUDY.

- 1. How long have you been active in the flea market? \_\_\_\_\_yrs
- 2. Are you a flea market dealer? \_\_\_\_\_
- 2a. How many years have you been attending markets at this site (as a dealer, if a dealer)? \_\_\_\_\_
- 2b. In a typical month, how often do come to this site (as a dealer, if a dealer)? \_\_\_\_\_
- 3. Gender: 1) Male 2) Female
- 4. Age \_\_\_\_\_
- 5. Date of Birth \_\_\_\_\_
- 6. Race: White Black Other
- 7. What is the highest grade of education that you have completed? (Circle one)
  - 1) Eighth grade 3) 2-Year College 5) 4-Year College
  - 2) High School 4) Other Post-High School 6) Graduate School Education

Addition for lab treatments:

Have you ever had interaction with your partner before? \_\_\_\_\_

Addition for buyers:

1<sup>st</sup> dealer:

- 8. What was the initial offer made by the dealer? \_\_\_\_\_
- 9. What was the final offer made by the dealer? \_\_\_\_\_
- 10. Have you ever had interaction with that dealer? \_\_\_\_\_

2<sup>nd</sup> dealer:

- 8a. What was the initial offer made by the dealer? \_\_\_\_\_
- 9a. What was the final offer made by the dealer? \_\_\_\_\_
- 10a. Have you ever had interaction with that dealer? \_\_\_\_\_

3<sup>rd</sup> dealer:

Etc.

**Table 1 Experimental Design**

<b>Laboratory Treatments</b>	<b>Framed Field Treatments</b>	<b>Natural Field Treatment</b>
<p><b>LabIS</b>; n=24; Lab treatment with student subjects; standard game that provides individual cooperation rates</p> <p><b>LabIIS</b>; n=24; Lab treatment with student subjects; adds demand shocks to Lab IS</p> <p><b>LabIIIS</b>; n=24; Lab treatment with student subjects; takes away between subject anonymity from Lab IIS (subjects know the identity of their partner)</p> <p><b>LabIIISC</b>; n=24; adds contextual language to LabIIIS instructions</p>	<p><b>FramedNCSymm</b>; 12 buyers, 4 sellers; 2 sessions; bilateral trading market, no communication allowed between sellers and symmetric demand/supply curves</p> <p><b>FramedNCAsymm</b>; 12 buyers, 4 sellers total; 4 sessions (\$13.50 has 2 sessions, the others have 1); identical to FramedNCSymm except supply is perfectly elastic at \$2, \$7, and \$13.50.</p> <p><b>FramedCAsymm</b>; 12 buyers, 4 sellers; 6 sessions (2 sessions each); identical to FramedNCAsymm except seller communication allowed; constant marginal cost of \$2, \$7, and \$13.50</p> <p>The following build on FramedCAsymm MC \$7 sequentially with noted changes:</p>	<p><b>Field</b>; 82 buyers, 27 sellers; bilateral trading market; sellers in groups of 2, 3, and 4. Composition of seller conspiracies is endogenous, whereas in Framed Field Treatments I composed them. Purchase of several goods; demand curve is identical to demand curve induced in Framed Field treatments</p>

<p><b>LabIM</b>; n=24; identical to LabIS, but with subjects drawn from the open air market</p>	<p><b>Framedinf</b>; 12 buyers, 4 sellers; 1 session; infinite supply at marginal cost of \$7</p>	
<p><b>LabIIM</b>; n=24; identical to LabIIS, but with subjects drawn from the open air market</p>	<p><b>FramedNoprice</b>; 12 buyers, 4 sellers; 1 session; identical to FramedCAsymm except with no price revelation after a deal is consummated</p>	
<p><b>LabIIIM</b>; n=24; identical to LabIIIS, but with subjects drawn from the open air market</p>	<p><b>FramedShock</b>; 12 buyers, 4 sellers; 1 session; identical to FramedNoprice with a demand shock</p>	
<p><b>LabIIIMC</b>; n=24; adds contextual language to LabIIIM instructions</p>	<p><b>FramedTable</b>; 24 buyers, 16 sellers (sellers in groups of 4); good placed on table and buyers approach regularly throughout market day/weekend</p>	
	<p><b>Framed2sellers</b>; 84 buyers, 28 sellers (sellers in groups of 2); 4 pairs facing the same demand curve as FramedTable; 10 pairs facing the same per seller demand as FramedTable</p>	
	<p><b>FramedHighStakes</b>; 18 buyers, 12 sellers (sellers in groups of 2), profits multiplied by 5 for sellers; sellers face the same per seller demand as Framed2sellers</p>	

Notes: Each entry represents a unique treatment in which I gathered data. For example, “LabIS” in row 1, column 1, denotes that one treatment had 24 student sellers participate in a lab experiment. No student or market subject participated in more than one lab treatment; some selling agents participated in more than one treatment (in most cases, unbeknownst to them).

**Table 2 Buyer and Seller Reservation Values (in dollars) by Market Period**

	<u>Period 1</u>	<u>Period 2</u>	<u>Period 3</u>	<u>Period 4</u>	<u>Period 5</u>	<u>Period 6</u>	<u>Period 7</u>
Buyer 1	19	14	17	13	14	19	14
Buyer 2	18	9	10	17	11	18	9
Buyer 3	17	10	11	16	13	17	10
Buyer 4	16	11	12	15	9	16	11
Buyer 5	13	12	16	14	18	13	12
Buyer 6	14	13	14	19	15	14	13
Buyer 7	15	16	14	12	19	15	16
Buyer 8	12	14	15	11	16	12	14
Buyer 9	11	15	13	10	17	11	15
Buyer 10	10	17	18	9	14	10	17
Buyer 11	9	18	19	14	10	9	18
Buyer 12	14	19	9	18	12	14	19
Seller 1a	8	8	8	8	8	8	8
Seller 1b	14	14	14	14	14	14	14
Seller 1c	18	18	18	18	18	18	18
Seller 2a	9	9	9	9	9	9	9
Seller 2b	13	13	13	13	13	13	13
Seller 2c	17	17	17	17	17	17	17
Seller 3a	10	10	10	10	10	10	10
Seller 3b	13	13	13	13	13	13	13
Seller 3c	16	16	16	16	16	16	16
Seller 4a	11	11	11	11	11	11	11
Seller 4b	12	12	12	12	12	12	12
Seller 4c	15	15	15	15	15	15	15

**Table 3 Laboratory Experimental Results**

Treatment	<u>Price Choice</u>			Average Profit (per period)	Eff.
	P1 Collusive	P2 Undercutting	P3 Punishment		
<u>Students</u>					
LabIS	60.3%	35.6%	4.2%	\$1.44 (0.38)	72.4%
LabIIS	45.3%	51.1%	3.9%	\$1.31 (0.42)	65.6%
LabIIIS	59.8%	35.6%	4.5%	\$1.44 (0.40)	71.7%
LabIIISC	55.3%	38.3%	6.4%	\$1.32 (0.44)	66.0%
<u>Marketers</u>					
LabIM	46.5%	45.8%	7.6%	\$1.23 (0.49)	61.6%
LabIIM	47.9%	40.4%	11.7%	\$1.10 (0.52)	55.0%
LabIIIM	51.9%	41.0%	7.1%	\$1.31 (0.56)	65.7%
LabIIIMC	61.4%	32.2%	6.8%	\$1.39 (0.41)	69.7%

Note: Figures in table represent averages across the various price choices and average profit (with standard deviations in parentheses). For example, in the “LabIS” treatment, 60.3% of subject choices were P1, or the collusive price. And, the average period payoff was \$1.44. I also include the overall average efficiency for each treatment (denoted eff.), where efficiency is measured as  $\text{profits}_i / \text{available profits}$ , which is profits secured by pair  $i$  (averaged over all periods and pairs) in the session divided by profits available (\$2 in all treatments). The various treatments are defined in Table 1.

**Table 4 Experimental Results—Framed Field Treatments**

<b>Treatment</b>	<b>Market Period</b>				
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>
<i>NCSymm</i>					
Average price	14.00	14.60	14.00	13.97	13.48
Std. deviation	(1.71)	(1.50)	(0.96)	(0.92)	(0.92)
Quantity	(Q=5)	(Q=5.5)	(Q=5.5)	(Q=7.5)	(Q=7.5)
<i>NCA<sub>symm</sub>\$2</i>					
Average price	6.78	5.86	5.45	6.55	5.79
Std. deviation	(2.86)	(2.36)	(1.41)	(2.55)	(1.69)
Quantity	(Q=9)	(Q=11)	(Q=12)	(Q=11)	(Q=12)
<i>NCA<sub>symm</sub>\$7</i>					
Average price	13.50	12.83	11.50	10.00	9.75
Std. deviation	(1.29)	(1.32)	(1.51)	(0.53)	(1.29)
Quantity	(Q=4)	(Q=6)	(Q=8)	(Q=11)	(Q=12)
<i>NCA<sub>symm</sub>\$13.50</i>					
Average price	15.12	15.10	14.67	13.80	13.59
Std. deviation	(1.89)	(2.34)	(1.42)	(0.25)	(0.19)
Quantity	(Q=4)	(Q=3.5)	(Q=6)	(Q=7)	(Q=7)
<i>CA<sub>symm</sub>\$2</i>					
Average price	5.84	5.57	6.25	7.00	7.67
Std. deviation	(2.19)	(1.53)	(0.85)	(0.72)	(1.99)
Quantity	(Q=9.5)	(Q=11.5)	(Q=12)	(Q=12)	(Q=12)
<i>CA<sub>symm</sub>\$7</i>					
Average price	16.00	15.75	15.10	14.63	14.15
Std. deviation	(1.10)	(1.39)	(0.57)	(0.43)	(0.32)
Quantity	(Q=3)	(Q=4)	(Q=5)	(Q=6)	(Q=6.5)
<i>CA<sub>symm</sub>\$13.50</i>					
Average price	19.00	18.00	17.00	16.40	16.44
Std. deviation	(0.0)	(0.0)	(1.41)	(1.26)	(0.50)
Quantity	(Q=0.5)	(Q=2)	(Q=2.5)	(Q=5)	(Q=4)

Table 4 continued

<b>Treatment</b>	<b>Market Period</b>				
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>
<i>FramedInf</i>					
Average price	14.00	14.00	14.00	14.00	14.00
Std. deviation	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Quantity	(Q=5)	(Q=7)	(Q=7)	(Q=6)	(Q=7)
<i>FramedNoprice</i>					
Average price	14.70	14.50	14.00	14.00	13.86
Std. deviation	(0.45)	(1.00)	(0.63)	(0.63)	(0.69)
Quantity	(Q=5)	(Q=6)	(Q=7)	(Q=6)	(Q=7)
<i>FramedShock</i>					
Average price	15.75	15.00	0.00	14.50	13.71
Std. deviation	(0.50)	(0.82)	(shock)	(0.54)	(0.76)
Quantity	(Q=4)	(Q=7)	(...)	(Q=6)	(Q=7)

Note: Figures in table represent averages across the sessions in each treatment. Summary statistics are provided for price, its standard deviation, quantity traded, and efficiency for each period. For example, in the “*NCSymm*” sessions, period 1 had an average trading price of \$14 with a standard deviation of \$1.71. On average, 5 goods were purchased. Three of the fifteen sessions proceeded to period 6 (but no further); these data are not significantly different than their respective period 5 sessions so I suppress their presentation, but make these data available upon request.

**Table 5 Framed Field Experimental Summary of Cheating Rates**

<b>Treatment</b>	<b>Seller Offer</b>	<b>Percent Cheated</b>	<b>Percent Transacted</b>	<b>Transacted Price</b>	<b>Percent Cheated</b>	<b>Average Cheat</b>
<i>Framed</i> (lab markets pooled)	---	---	47%	\$14.51 (0.90)	9.6%	6.4%
<i>FramedTable</i>	\$13.96 (0.97)	18.4%	64%	\$13.51 (1.08)	33%	12.8%
<i>Framed2Sellers</i>	\$14.36 (2.14)	7.8%	58%	\$13.49 (1.35)	16%	11%
<i>FramedHighStakes</i>	\$12.75	24%	71%	\$11.80	48%	17%

Note: Figures in table represent averages across the various treatments. *Framed* (lab market pooled) pools data from the CAsymm\$7, FramedInf, FramedNoprice, and FramedShock treatments. *Framed2Sellers* pools data from both demand treatments. “Seller offer” is the average negotiated final price quote across every dealer approached in that treatment (including the dealers who executed a trade). “Percent Cheated” is the percentage of those price quotes that were lower than the agreed upon collusive arrangement. “% Transacted” is the percentage of visits to the market that resulted in a transaction (each induced value represents one visit to the market). “Transacted Price” is the average price of the good across every actual sale in that treatment. “Percent that Cheated” is the percentage of those transaction prices that were lower than the agreed upon collusive arrangement. Of those that prices that are considered cheating, “Average Cheat” is the percentage price deviation from the agreed upon collusive price averaged across all observations in each session. For example, in the Framed “pooled” treatments, 9.6% of transactions were below the agreed upon price floor, and the average transaction among those observations that were cheating was 6.4% below the price agreement. The various treatments are defined in Table 1.

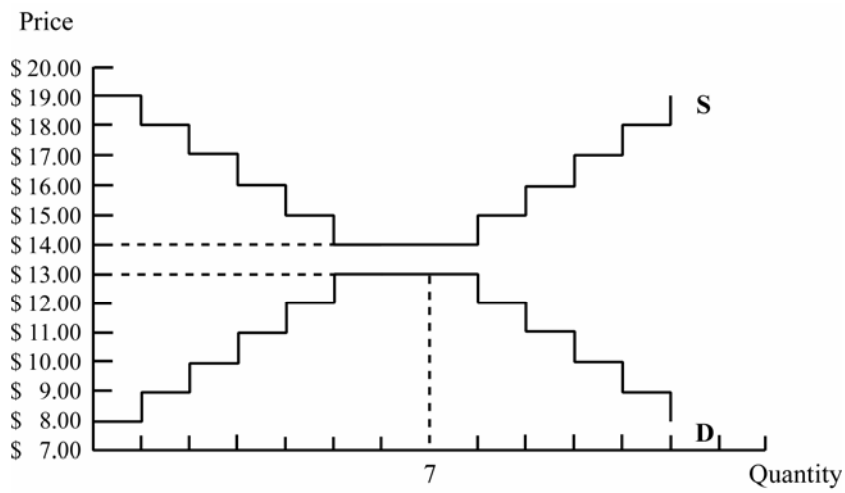


Figure 1. Supply and Demand Structure