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THE ECONOMICS OF OPEN AIR MARKETS

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ABSTRACT

Despite their current prevalence and historical significance, little is known about the economics of open air markets. This paper uses open air markets as a natural laboratory to provide initial insights into the underlying operation of such markets. Using data on thousands of individual transactions gathered from May 2005- August 2008, I report several insights. First, the natural pricing and allocation mechanism in open air markets is capable of approaching full efficiency, even in quite austere conditions. Yet, a second result highlights the fragility of this finding: allowance of explicit seller communication frustrates market efficiency in a broad array of situations. Making use of insights gained from a "mole" in the marketplace, a third set of results revolves around economic questions pertaining to collusive arrangements that are otherwise quite difficult to investigate. Overall, I find data patterns that are consistent with certain theoretical predictions, as the evidence suggests that i) cheating rates increase as the coalition is expanded, ii) sellers cheat less when they have collusive arrangements in several spatially differentiated markets, and iii) sellers cheat more when they are experiencing periods of abnormally high profits. These results follow from a combination of insights gained from building a bridge between the lab and the naturally-occurring environment. By doing so, the study showcases that in developing a deeper understanding of economic science, it is desirable to take advantage of the myriad settings in which economic phenomena present themselves.

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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.[1] While it is difficult to provide an economic estimate of the importance of such markets, 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.[2] More broadly, such markets are commonly of great importance, especially in developing countries where the institution is crucial in the allocation of goods and services.[3]

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. For example, whether the bread and butter of economics—supply and demand curve shape and location—provide accurate predictions of price and quantity realizations in such domains is relatively unknown. Laboratory experimental results in Chamberlin (1948) suggest that such predictions perform rather poorly. I take a different approach to this fundamental

[1] 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 17th 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.

[2] Regionally, 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 CA Flea Market is known to attract nearly 60,000 customers per weekend.

[3] 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 15,000-30,0000 people" (Geertz, p. 28)) is employed at the local bazaar.

question by exploring behavior in open air markets in a major US metropolitan area, effectively reformulating the problem of stability of equilibria as a question about the behavior of agents in an actual marketplace. My general line of attack 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 and equilibrating tendencies.

A key result from the field 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 (see also List, 2004).

Yet, such markets are ripe for price manipulation. For instance, in certain cases small numbers of sellers provide homogeneous goods that are jointly purchased from middlemen, certain barriers to entry exist, and seller communication is continual. Open communication channels surely remind the astute reader of the forewarning of 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."

Indeed, via 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" and using insights gained from months of interaction in the market, I am able to build a bridge between the lab and the field, effectively exploring the behavior of experimental subjects across sterile and 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 an excellent overview) with parallel behavior in the field.

I begin by collecting data in a controlled laboratory study with student subjects, and 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. I then execute a series of controlled treatments that slowly moves the environment from a tightly controlled laboratory study to a natural field experiment (i.e., a setting where subjects do not know that they are part of an experiment—see Harrison and List (2004)).

Several insights are obtained across both framed and natural field experiments. First, the data patterns are generally consonant with economic theory. For example, the evidence suggests that i) cheating rates are increasing in coalition size, resulting in total market surplus foregone due to collusive arrangements being significantly smaller as the collusive group grows, ii) sellers cheat less when they have multiple collusive arrangements in spatially differentiated markets; as a consequence, the total market surplus foregone increases as sellers become spatially integrated, and iii) sellers cheat more when they are experiencing periods of abnormally large profits, leading to total market surplus foregone being counter-cyclical.

Second, I observe similar behavior across subject pools in tightly controlled settings. Yet, difficulties arise when mapping the effectiveness of collusive arrangements across domains. 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 often. The level of cheating observed in the natural field experiment is larger than cheating rates observed in any of the tightly controlled treatments. Perhaps surprisingly, however, in *aggregate* the best predictor of field cheating rates is behavior in *sterile* laboratory treatments with neutral language instructions.

I interpret these data as also providing more general 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 related markets. Second, inferential power is enhanced by building a bridge between the lab and the naturally-occurring environment and designing complementary experimental treatments to span this bridge. This is made possible by combining experimental methods with an anthropological research approach (i.e., going to great lengths to familiarize oneself with the marketplace, the agents, social relationships, and the like). Third, even though the point estimates on cooperation rates are highly variable across lab and field environments, the conceptual relations among variables are similar, providing support for the notion that qualitative insights gained in one domain are transferable to others.

II. Market Background and Experimental Design

Open Air Economies

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 exchange 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 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 markets as distinct as the Barabazar in Shillong, India (the largest open air market in India) to the local flea markets in the US, buyers and sellers engage in an interaction that has each attempting to extract as much of the available rents as possible. Explanations for why such markets play a more important role in some parts of the world than others naturally pertain to economic considerations. First, due to information costs, such markets are attractive. For example, by centralizing the marketplace, buyers and sellers can interact more easily. Buyers absorb relatively low search costs, and sellers have low fixed costs and trivial expenditures on advertising. Technological advance in developed countries has served to minimize these costs, perhaps leading such economies to rely less on such markets. A second reason for the prevalence of such markets is that the opportunity to gain from price discrimination on individual sales might be quite high. Yet, this advantage might not be important enough to overcome the prohibitive costs of bargaining in some parts of the world, such as in particular developed economies.

Rich accounts exist concerning some specific bazaar economies and negotiation practices in such markets (see, e.g., Cassady, 1968; Geertz, 1978; Sherry, 1990). Since such markets share features with the open air markets that I visited, I will provide only a brief summary of particulars that I have learned through my field research. This is not meant to be an ethnographic description, but will have that flavor since these insights were gained from years of interactions.

<u>The Physical Market</u>

Temporal assignment of the physical marketplace is typically done by a professional association or local seller who rents a large space, such as a coliseum, stadium, large parking area, or a fairground, and allocates tables to dealers for a nominal fee. Rental rates vary from $20-$200 per slot and are filled on a first-come-first-served basis, yet most markets include the same sellers over time. When the market opens, consumers mill around the marketplace, higgling and bargaining with sellers (dealers), who have their merchandise prominently displayed on their tables. The principal market occurs between dealers and naïve consumers, but dealers do trade among themselves before, during, and after the market. In some cases, consumers bring goods to the market and trade or sell them to dealers, or even might sell a good that they recently purchased in the market. In other cases, consumers are interested in merely obtaining appraisals of their wares. The duration of a typical "market" is a day or a weekend, and a lucrative market may provide any given dealer hundreds of exchange opportunities.

Goods purchased and sold in the large metropolitan area markets that I frequented are highly varied, and include furniture, shoes, purses, tools, signs, books and magazines, perfumes, produce, meats, war memorabilia, antiques, sports collectibles, appliances, jewelry, hardware, tobacco products, clothing, coats, CDs, DVDs, ornaments, spices, oils, grains, artwork, and hand crafted products—from woodwork to knitted blankets to woven baskets. Some of the goods are standardized along the quality and quantity dimensions, whilst others are not. For example, some goods, such as tobacco products, CDs, DVDs, clothing, coats, purses, appliances, perfumes, furniture, and shoes, are branded, pre-packaged, or sealed to provide a level of standardization. These goods are highly substitutable, whereas other goods—woven baskets, knitted blankets, and the like—are heterogeneous and many times do not have close substitutes.

For the non-standardized products, there is a level of suspicion that nothing is what it appears to be (i.e., trimmed, adulterated, short-weighted, etc.), but for standardized goods there is more confidence that the goods are as advertised. Combining this fact with the realization that it is difficult to reward honesty and punish deceit, unscrupulousness becomes not a matter of one's compromised ethical code, rather it is a precondition for continuation in the market, or the "rationality of the market" (Akerlof, 1970). Under such conditions, either scrupulous sellers are driven out of the market or scruples are driven out of the sellers. In this way, for the non-standard goods, few consumers are deceived because they understand caveat emptor, whereas for standardized goods many seemingly buy unaware that the good is likely not as advertised.

The Pricing and Allocation Mechanism

Typically prices are not posted in this market, rather they are determined by haggle, where a buyer and seller take active roles in setting the final price. Casual buyers typically engage in several rounds of negotiations before bargaining to sale on an item, but bargaining is much different across standardized and non-standardized goods. For non-standardized goods, buyers focus their search intensively—honing in on one dealer and asking numerous questions about the good. This situates the locus of competition on the buyer-seller pair. Alternatively, for standardized goods, buyers typically search extensively by haggling with numerous dealers and focusing on the price dimension. This approach moves the locus of competition between sellers. Through introspection, most readers likely will find this type of strategy familiar, as they commonly engage in this practice as well (i.e., consider your strategy the last time you purchased a new (standardized) or used (non-standardized) car).

While the form and extent of bargaining varies widely across goods, there are some stylized facts in the marketplace. First, sellers typically make a first offer, then the buyer makes a bid, then the offers and bids roughly alternate. Backward moves in offers and bids are usually shunned, and accepted bids are honored. Yet, lying seems to be expected, as unreliable assertions in the give and take of bargaining seem to be the norm. Second, usually bargaining is more drawn out for expensive goods and usually not protracted for low ticket items such as produce. Third, both sellers and buyers employ tricks at the point of bargaining impasse: sellers might offer another item at the same price, increase the quantity of the good, add a cheap "free" item, or the like. Consumers typically "walk off," perhaps to gather composure or to send a signal that s/he is a tough bargainer. Sellers, of course, do not walk off, but an analogous action is to put a halt to negotiations by striking up a conversation with bystanders or a neighboring seller. Fourth, for goods with a certain amount of haggle, there appears to be statistical discrimination: the well-dressed seem to meet higher initial offers as do Caucasians; especially those who have kids by their side and are shopping for children's goods.

Geertz (1978, p. 29) provides an interesting description of the pricing and allocative efficiency of open air markets that he frequented: "in the bazaar information is poor, scarce, maldistributed, inefficiently communicated, and intensely valued." As Geertz (1978) notes, this makes "clientelization" efficient since the imperfect information makes having a limited number of trading partners potentially more profitable than a series of impersonal market transactions. Under this view, long-term relationships are an efficient means to overcome the lack of information in such economies, especially for goods that are heterogeneous.

In the markets that I frequented, social relationships between buyers and sellers do exist, but they are typically far removed from the institutionalized form of such partnerships that have formed elsewhere. For example, I never noticed anything similar to the *pratik* in Haiti (Mintz, 1967) or the *suki* in the Phillipines (Davis, 1973), where partners are described as linked by credit arrangements and informal supply contracts that increase trust in the relationships. Price dispersion in the markets that I frequented therefore tend to be a manifestation of ignorance, bargaining ability, consumer observables, and partnerships.

Market Sundries

Before proceeding to the experimental design, it is worthwhile to detail a few other market characteristics. First, in this market sellers have continual interaction with one another. One might suspect that such interaction could lead to attempts to fix prices, especially in the standardized good case. There are at least three important factors facing sellers attempting to collude in any market: coordination, monitoring cheating, and entry.

From months of interactions I learned that some of the standardized goods in the markets that I frequented were purchased from the black market, and that a subset of goods is purchased from a common middleman. Thus, coordinating on a collusive price is much easier since sellers are aware of one another's marginal costs. Detecting cheating on agreements is also seemingly straightforward since sellers are in constant contact and consumers are constantly discussing exchange prices. Yet, cheating is difficult to detect because i) buyers tend to shade their purchase prices when asked, and ii) even though sellers purchase from the same middleman, they oftentimes participate in some different markets so strict quantity monitoring is difficult.

To gain insights on i), I ran a survey in the marketplace asking buyers how much they paid for the goods they just purchased. More than 65% stated a value that I later determined to be too low (via either a monitor directly observing the price paid or an interview with the seller). It seems that buyers quote a lower transaction price because they fear being thought to be taken, or "played the fool." I learned of particulars concerning ii) from discussions with sellers and from survey responses. Prohibiting entry is the final task of the colluders, but this seems to be handled via a close watch on who is allowed to set up at the market. Thus, upon taking care of the cheating incentive, it would seem that the conditions are ripe for attempts at price fixing in this market. Coupling this, with the fact that there is certainly a "culture of collusion" present in these markets that perhaps rivals that of the rayon industry (see, e.g., Gallet and Shroeder, 1995), leads to an expectation that a degree of explicit collusive arrangements among sellers of some standardized goods exists in these markets.

As detailed further below, discussions with a market "mole" led me to conclude that 27 sellers across 8 different local flea markets were part of an explicit 2, 3, or 4 seller conspiracy. These sellers often had agreements on multiple goods, and agreements with multiple sellers. I further learned from my "mole" that a strong majority of the collusive arrangements set prices by adding markup to costs. In nearly all of the cases sellers simply applied a specific rule, such as "double the price," or "mark-up by 100%" and remained with this price for long periods of time.

Experimental Design

Prior to discussing the details of the experimental design, I outline 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.

Table 1 provides an outline of the various 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). 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 the context used in the experimental instructions, 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 and artefactual 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. 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, which is summarized in column 2 of Table 1, includes variants that preclude communication among sellers as well as several treatments that permit communication. This approach is used for two primary reasons: i) no communication treatments serve to provide a baseline of comparison for the communication treatments and ii) testing whether competitive theory adequately organizes data in multi-lateral decentralized market institutions is interesting in its own right.

The ultimate treatments—denoted natural field experiment in column 3 of 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. Much can be learned from these treatments because of the knowledge I have of certain explicit agreements (overtly collusive agreements) in this marketplace from a mole who provided enough detailed information to execute a controlled test in the market. I now turn to a more patient description of the experimental design, which was carried out from May 2005-August 2008.

A. Lab Experiment Particulars

My overarching goal in designing the laboratory experiments is to maintain congruence with 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 use a design similar to Holcomb and Nelson (1997) and Feinberg and Snyder (2002). Experimental instructions are in Appendix A.

The employed game is simple and calls for two players in a market to choose simultaneously one of three prices: P1, P2, and P3, where these prices represent the "collusive" price, an "undercutting" price, and a "punishment" price (although 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 Appendix A.[4] 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

[4] 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 ½ 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.

each round thereafter followed a similar continuation rule.[5] In many cases, experimenters use an infinite horizon game such as this one to permit collusive agreements to be an equilibrium; under the parameters of this game, however, cooperation is an equilibrium.[6]

I conducted one experimental session with undergraduate student 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 using purely between-subject variation. In Treatment LabIS (denoting lab treatment I with student subjects), after each period subjects are informed only of their payoffs. Given that they are aware of their own choice and 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 (2002).

Treatment Lab IIS (denoting lab treatment II with students) is identical to Treatment LabIS except there are demand shocks. Subjects are again informed about their own payoffs after each period but not about rival's actions. In this case, however, a negative demand shock yields a zero payoff to each player regardless of their choice. 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

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[5] 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."

[6] To see this, note that in a 5-period game with the payoff structure imposed 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.

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 laboratory treatment, denoted Treatment LabIIISC, is identical to Treatment Lab IIIS but uses instructions with context. For instance, rather than labeling the price choices as P1, P2, and P3, I denote them as the "collusive," "undercutting," and "punishment" prices. 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 with student subjects are identical treatments carried out with actual sellers from various open air markets. These treatments are labeled LabIM, LabIIM, LabIIIM, and LabIIIMC (denoting lab treatments with market subjects, or artefactual field treatments in Table 1), and are carried out in a large US metropolitan area. In this particular area more than 20 flea markets operate on any given weekend.

Each flea market 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 approached sellers in a randomly determined order and inquired about their interest in participating in an experiment that would last approximately 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 these treatments are identical to Appendix A. Similar 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 during the trading day while physically located in their actual sales booth. Thus, after each subject made a decision, both were provided with payoff information for that period. Similar to the student treatments, in the anonymity treatments sellers were never made aware of their partners and they were informed this would be the case. In Step 3, sellers participated in the experiment. In Step 4, they were paid their earnings in private after they filled out the survey in Appendix B.

For the parameters employed, 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 opponent 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 or the punishment choice.

Column 1 in Table 1 provides a summary of the various laboratory 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 gathered more than 1900 observations in these treatments.

B. Framed Field Experiment Particulars

To move one step closer to the actual market setting in which these agents naturally engage, I design a series of field treatments that allow observation of face-to-face continuous bilateral bargaining in a multi-lateral market. Such a constructed market shares similarities with the bazaar economies 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 of this exercise is to examine under what conditions do prices and quantities converge to the intersection of supply and demand. To do so, 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 people as a question within the realms of psychology, as opposed to a question about an abstract auctioneer steering the market adjustment process.

The basic market design is similar in spirit to Chamberlin (1948), as extended by List (2004).[7] 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 market (in a large US metropolitan area) and inquired about their interest in participating in an experiment that would take about 45 minutes. Since most sellers are accompanied by at least one helper, it was not difficult to obtain

[7] The experimental design discussion in this section closely follows List (2004).

agreements after it was explained that money could be earned during the experiment. To gather the buyer subject pool, a monitor randomly approached buyers 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 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. First, to ensure transactions at reservation values, a $0.50 commission for each executed trade was provided for both buyers and sellers.

Second, buyers (dealers) were informed that the experiment consisted of at least 5 rounds and that they would be consumers (sellers) in the experiment. 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 experimental market. In each round, each seller would be given a "seller's card," which 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 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 each. After each 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. 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 the treatments 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.[8] 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 present buyer and seller induced values. In Figure 1, each step represents a distinct induced value that was given to buyers (demand curve) and sellers (supply curve). The efficient outcome yields \$37 in rents per round, with an associated equilibrium price between \$13.00-\$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 one deviation: supply 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 \$13.50. In these treatments, the efficient perfectly competitive outcome yields \$144, \$84, and \$18.50 in rents per period for the \$2, \$7 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

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[8] 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.

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, unbeknownst to buyers. 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. 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 seller rents of \$90, \$49, and \$10.50 in every period.[9]

To move closer to the actual marketplace in increments, I build on the FramedCasymm \$7 treatment, 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.
3. Treatment FramedShock: players are told that I have randomly pre-selected ~20% of the periods 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. In practice, each of the buyers received an induced value of \$6 in period 3 in this treatment.

[9] Of course, monopoly prices and rents are based upon the setting of a single monopoly price. In this case, this is akin to sellers agreeing upon a single take-it-or-leave-it price. Such a strategy does not permit sellers to engage in price discriminating behavior and may not necessarily reflect the optimal selling mechanism.

4. Treatment Framedtable: rather than conduct the experiment in 5-minute periods, I place the good on the seller's table and inform them that occasionally a potential buyer will be stopping by to purchase 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 induced values taken from the demand curve used in the above treatments.

For example, buyer 1 is given induced values 19, 14, 16, 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).[10] 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 any of the four sellers with that induced value. After the five minutes, or directly after the buyer executes a transaction, the buyer receives a new induced value and returns to the market. Similar to the treatments discussed above, sellers are free to discuss pricing 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.[11]

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[10] Buyer 2 is given induced values 19, 14, 16, 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 (with induced values 12 and 16 inserted randomly to complete the demand function). To mimic the lab experiments, I have buyers approach the seller groups uniformly. Thus, buyers 1-12 approach seller groups 1 and 2; buyers 13-24 approach seller groups 3 and 4. 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).

[11] 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., Gneezy and List (2006)).

5. Treatment Framedtable2: 2 sellers rather than 4 sellers are included in each session. In some of the pairs sellers face the exact same demand curve in FramedTable; in others sellers face the same effective individual seller demand as FramedTable (i.e., every other step is taken out of the demand curve in Figure 1).[12] Additionally, partnership arrangements are varied randomly: 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 (I expand on how I know this information below).

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

Column 2 in Table 1 provides a summary of the various 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 treatments is to maintain the integrity of the framed field treatments discussed above, while exploring aspects associated with the naturally-occurring collusive arrangements. In these treatments subjects do not know that they are taking part in an experiment. This exploration is made possible by the information that my mole conveyed through a series of discussions.

More specifically, in 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

[12] 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).

early 2006-2007, 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 goods 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 that 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.

The recruitment of buying agents typically followed four steps: (1) consideration by the buyer 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 buying 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 use the induced buyer values from Figure 1 (with the necessary adjustments made to values when the marginal cost was less or greater than $7).[13] The 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).[14]

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 several other days in a similar fashion as well. 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; on the contrary, these are actual transactions wherein I am obtaining actual sales prices. Since transactions are typically in cash at flea markets, I provided the necessary funds to purchase the goods when the buyer was short of funds. Third, note that great care was taken to ensure that the data were gathered from interactions that would naturally occur in the marketplace. Subjects initially entered 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.

[13] For instance, in the cases where the known seller 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, similar to the framed treatments 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.

[14] The subject having an interest in the good provides realism in that dealers naturally face this subject type in the marketplace, but it comes at a potential cost—dubious consumers may use the bargaining session to arrange for later purchase of the good from the dealer. To avoid this potential issue, I ensured the subject that if s/he would like to purchase the good after the experiment I would sell at the same price at which s/he purchased the good.

Finally, similar to the framed field treatments, each confederate had a 5-minute time limit imposed; in every 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 to purchase the good for me, and that this was to be kept completely private. They were further informed that all earnings would be forfeited if I found they conveyed any information to sellers. Step 4 concluded the experiment—after subjects completed a confidential survey, they were paid their earnings in private.

In total, I observed 455 individual negotiations. These negotiations are observed from the behavior of 27 sellers who were each visited by several of my 82 buying confederates. Each buying confederate approached anywhere from 2-10 sellers, with the average slightly greater than five. 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 Section II, I discuss the empirical results in three separate sub-sections. I begin with a summary of the lab and artefactual field treatments, proceed to the framed field treatments, and conclude with a discussion of data from the natural field experiment.

A. Lab Experimental Results

Table 3 provides a snap shot of the 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. General data patterns are consistent with previous efforts in this literature, in that the degree of collusive play is quite high.

In the statistical 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 collusive *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. Such data might be 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 that i) collusive rates are higher in treatment Lab IS than treatment Lab IIS, and 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 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 5th 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 some 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.[15] This result fits in nicely with a broader literature on how dimensions of anonymity affect behavior. For example, List et al. (2004) found that the degree of anonymity between subjects influenced their propensity to contribute to a public good in a one-shot decision: subjects cooperated to a greater extent when they know the identity of their partner, and vice versa (see Levitt and List, 2007, for further discussion). 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 IIIM who have had previous interaction not only collude more often than other groups in Lab IIIM, but also collude more often than subjects in Lab IIM.[16]

Further, I find that collusive rates in LabIIIMC are greater than collusive rates in LabIIIM, 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 IIIM treatments and a majority of players did not place a collusive context on the game when asked about its nature.

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[15] 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 comparative static changes in the first.

[16] Since I do not know which partners in Lab IIM have had previous interaction, I cannot determine whether this result is due to treatment or selection.

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} ~N[0,1], where C_{it} equals unity if agent *i* made a collusive choice in period *t*, and equals zero otherwise; X_i includes the treatment effect dichotomous variables and the observables from the survey.[17] 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 treatments. 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 a trading price of $14.00 (std. dev.=1.71).

Results from the symmetrical demand and supply structure provide evidence that the intersection of supply and demand does an adequate job of predicting data trajectories. 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.[18]

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, by period 5 the average

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[17] I estimate Butler and Moffitt's (1982) random effects probit model via maximum likelihood, where the likelihood function for can be written as $L = \Pi_i L_i = \int_{-\infty}^{\infty} (2\pi)^{-1/2}\, \Pi_t \exp(-e_{it})\phi(g_{it}q_{it})$, where $g_{ij} = 2C_{ij} - 1$; and $q_{ij} = \beta`X_{ij} + [\mathrm{corr}(e_{ij}, e_{is})/(1 - \mathrm{corr}(e_{ij}, e_{is}))]^{1/2} e_i$.

[18] I define efficiency as the portion of total surplus ($37 in the symmetric case) captured. Thus, a 95% rate of efficiency represents profits of roughly $35 in that period.

price decreases to \$13.59, and the allocation of rents is almost entirely tilted toward buyers. This tendency toward neoclassical expectations yields 11 of 14 last period trades at the equilibrium price of \$13.50. 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 the basic predictions of supply and demand intersection. This result is at odds with Chamberlin'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 (Levenstein and Suslow, 2006). Treatments CAsymm\$2, CAsymm\$7, and CAsymm\$13.50 provide some insight in this regard. In the field, if the costs associated with maintaining collusive arrangements are prohibitive, then such agreements will be rendered ineffective. These treatments likely give collusive arrangements a good chance to succeed by i) providing a simple platform for enacting collusive arrangements, ii) immediate price posting, iii) not allowing entry, and iv) 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-$9 for the $2 treatment; $7-$9 for the $7 treatment; and $13.50 for the $13.50 treatment). Overall, the data point to the fact that communication between sellers can inhibit efficiency and serves to 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 summary contained 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 ($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 more," or "we have to lower the price or we will sell nothing" that lead to the data patterns in Table 4. Nonetheless, the data patterns are interesting in that they appear to approach the joint profit-maximizing strategy, especially in the $7 and $13.50 treatments.

The next set of treatments—Framedinf, FramedNoprice, and FramedShock—draw the framed field treatments closer to the naturally-occurring environment. Yet, as Table 4 makes clear, none of these three treatments has a considerable influence on outcomes. While there is some treatment influence 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 negotiated seller offer 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 initial and a final offer prices—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. For example, summary data in row 1, which includes pooled results for the various framed treatments (data from the CAsymm$7, FramedInf, FramedNoprice, and FramedShock), reveals the significant effects of collusion, as only 47 percent of visits result in a purchase. This figure is considerably less than the percent transacted in Treatment NCAsymm$7, which reached 100 percent in period 5 and was nearly 90 percent in the final three periods (see Table 3). There is some cheating observed in these framed field treatments, however, as nearly 10 percent of executed transactions were lower than the agreed upon collusive arrangement. Of those cheating transactions, the price was 6.4 percent below the agreed upon collusive price.

Overall, summary figures in Table 5 show that the various treatments have important influences on seller behavior. Again, to explore whether the treatment effects are statistically significant, I begin with a conservative approach wherein each collusive group provides one observation, which yields some useful insights. First, cheating rates are significantly higher in the FramedTable treatment compared to the framed (pooled) treatments at the $p < .05$ level using a Mann-Whitney rank-sum test of treatment differences. This result implies that simply moving from the five minute per period setting where 12 buyers and 4 sellers attempt to transact in every period to the more natural setting causes sellers to cheat on their agreements much more often. This change in treatment clearly makes the market more fluid, as now roughly 64 percent of visits result in a transaction, and the intensity of cheating increases too: for those transactions that violated the explicit collusive agreement, price was nearly 13 percent below what was agreed upon versus 6.4% in the framed (pooled) treatments. Both of these differences are statistically significant at the $p < .05$ level using a Mann-Whitney rank-sum test.

Second, when the number of sellers in the collusive arrangement decreases from four to two (FramedTable versus Framed2Sellers), cheating rates halve (declining from 33% to 16%). The reluctance to cheat among two seller arrangements is also found in the average negotiated seller quotes, whereas 18.4 percent of the FramedTable price quotes are below the agreed upon price, only 7.8 percent of the Framed2Sellers quotes are below the agreement. Both of these differences are significant at conventional levels. Finally, conditional on coalition size, when the stakes are increased cheating rates rise: rates triple from 16 percent to 48 percent, and the average cheating deviation is 16 percent below the arranged price. This leads to a much lower average transacted price of $12.42. These differences are each significantly different across Framed2Sellers and FramedHighStakes. Interestingly, the FramedHighStakes price remains significantly above the prices realized in the latter periods of Treatment NCAsymm$7, but are below the average transacted prices for the other framed field experimental treatments.

As a robustness check, I estimate an empirical model that makes the dependent variable either i) whether the transaction is cheating on a collusive agreement, or the i) degree of cheating. Specifically, for i) I estimate $Ch_i = \beta`X_i + e_i$, where Ch_{it} equals unity if the *ith* transaction cheats on the collusive agreement, 0 otherwise; X_i includes the treatment effect dichotomous variables, the buyer and seller observables from the survey, and I account for the data dependencies among buyers and sellers by exploring models that include fixed effects (when possible) and random effects. For ii), I estimate $\%Ch_i = \beta`X_i + e_i$, where $\%Ch_i$ equals the percentage price deviation from the agreed upon price in the *ith* transaction; other particulars are consistent with estimation of i). Because empirical results are broadly consistent with the statistical findings discussed above, I make these results available upon request.

C. Natural Field Experimental Results

To begin the discussion of the natural field experimental data, I present the summary statistics of the pooled data—455 individual negotiations—ignoring data dependencies and coalition particulars. Table 6 extends Table 5 and provides summary results by the percent of offers that cheated—both negotiated and transacted offers—the price deviation from the agreed upon collusive price, the percent of induced values (visits) that resulted in transactions, and the percentage of surplus captured in the market. For comparison purposes, Table 6 includes data across both the framed and natural field experiments. I also provide Figure 2, which summarizes the proportion of transactions that had executed prices below the agreed upon collusive price (i.e., the proportion of "cheating" transactions).

An interesting data pattern emerges: for the natural field treatment, many more of the transaction prices are in violation of the collusive agreements. For instance, in 46 percent of the cases, my confederate negotiated a price lower than the collusive price agreement. Further, in 69 percent of the actual transactions, the price was below the collusive price. Using a conservative testing approach wherein each collusive group provides one observation, I find that these percentages are both significantly larger than each of the framed field treatments using a Mann-Whitney rank-sum test of treatment differences. The average price deviation is also significantly larger using a Mann-Whitney rank-sum test (except for the comparison with the FramedHighStakes treatment). Likewise, the percentage of available transactions executed and the surplus captured in the market are also significantly larger in the natural field treatment than the framed field treatments (again, only marginally so versus the FramedHighStakes treatment).

Data from the natural field experiment are sufficiently rich to allow further empirical investigation. For instance, the natural data contains groups from 2-4 per collusive arrangement, and some of the collusive groups are composed of blood relatives (or husband/wife) whereas others are arrangements amongst non-relatives. Moreover, some seller groups contain all women, some are all men, and some are a mix of men and women. Further, some of the sellers have multiple agreements across markets whereas others only have arrangements in one market. Finally, I approached sellers across days that were quite busy as well as slower days.

To account for these differences, I estimate an empirical model in the spirit of the models discussed above: i) $Ch_i = \beta`X_i + e_i$, where Ch_{it} equals unity if the *ith* transaction cheats on the collusive agreement, and equals zero otherwise; X_i includes buyer and seller observables from the survey, coalition size, a dichotomous variable that equals one if the sellers have more than one collusive relationship, 0 otherwise; variables that describe the composition of the group (all women, all men, mix; relatives); controls for the busy market day, where market day equals one on busy days (measured as those days that are in at least the 67th percentile in visitations to my dealer table), 0 otherwise. I account for data dependencies among buyers and sellers by including buyer and seller fixed effects; I also consider models that cluster standard errors at the buyer and seller level and results do not change markedly.

Empirical results are presented in Table 7. Several results emerge. First, consider the relevant outcome relationships with coalition size. Columns 1 and 2 in Table 7 reveal that there is more cheating in the 4-person arrangements than in the 2-person arrangements (3 person arrangements show no statistically significant differences). In terms of magnitudes, sellers in 2-person arrangements are roughly 20% less likely to cheat than sellers in 4-person arrangements. Columns 3 and 4 provide complementary evidence on the nature of price deviations, suggesting that sellers in 2-person arrangements deviate less from the agreed upon price than sellers in 4-person arrangements. If one considers data at the group level, columns 5 and 6 show that the best offer from 2-person seller groups deviates significantly less from pricing agreements than the best offer from 4-person seller groups.

Second, sellers with collusive arrangements across markets cheat much less often than sellers who only have one collusive arrangement. This result can be found in columns 1 and 2, where the data suggest that multiple collusive arrangements has an effect of reducing the propensity to cheat by roughly 20%. Interestingly, this effect is comparable to the effect of moving from a 4-person to a 2-person collusive groups. Columns 3-6 show that price deviations among sellers who have multiple arrangements are also less severe. Like coalition size, having agreements across markets leads to less cheating on both the intensive and extensive margins.

Third, sellers cheat more on high volume, busy days. The estimates indicate that if the transaction occurs on a busy market days, sellers are approximately 25% more likely to cheat on their arrangements. On the intensive margin, there is mixed evidence. In the individual data sellers cheat more intensively on busy days, but the group data yield evidence that is not consonant with this insight. Finally, empirical results on the social variables yield some interesting insights. For example, having a life relationship with a fellow colluder is associated with sellers cheating less often, and less intensively. As for gender effects, men are more likely to cheat on pricing arrangements than women, and all-female seller groups are the least likely to be associated with cheating.

Before moving to the discussion section, an important exploration is to ensure that my information on collusive arrangements from my mole was indeed accurate. In Appendix C I present evidence that is consonant with the hypothesis that all of the arrangements that I learned about were accurate.

IV. Discussion

While many social scientists have experienced open air markets, and have likely been fascinated by the underlying market dynamics and array of goods and services haggled over, few have reflected deeply on the underpinnings of such markets. In this study, I depart from a traditional empirical investigation by using the tools of experimental economics in open air markets. The paper showcases that in building a deeper understanding of economic science, it is desirable to take advantage of the myriad of settings in which economic phenomena present themselves—from the lab to the field. In this spirit, this study presents results from an examination of behavior in several experimental treatments that bridge the lab and the field. In this epilogue, I summarize the results in three subsections: theory testing, speaking to policymakers, and methodological contribution.[19]

A. Theory Testing

On many dimensions the data are consistent with one theory of another. First, the bread and butter of economics—supply and demand curves—provides reasonably accurate predictions of price and quantity realizations and their direction. 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 in open air markets can yield efficient outcomes.

Third, when digging deeper into the findings revolving around the seller communication treatments, several comparative static insights obtained match theory. For instance, evidence from the natural field treatment summarized in Table 7 suggests that successful price fixing conspiracies are more likely with small numbers of firms. This finding is in line with insights gained from early structure-conduct-performance theorists (e.g., Bain, 1951) who argued that an increase in concentration facilitates collusion.[20] The intuition is that as the number of firms increase the 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, my 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

[19] 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.

[20] This led to an extensive literature on the topic. An important line of inquiry was Demsetz (1973, 1974), who critiqued this notion extensively and Schamalensee (1987), Sutton (1998) and Symeonidis (2002) who empirically tested various models.

trustworthy sellers are those who are better able to coordinate smaller collusive groups. In this case, the framed field treatments provide important complementary evidence. Given that I randomly place sellers into various sized groups, I am able to observe how exogenous changes in group size affect successful price fixing. Comparing data across FramedTable and Framed2Sellers (see Table 6 and Figure 2) provides supportive evidence that agents 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 certain theories is the observation in the natural field treatment that sellers cheat less when they have arrangements across multiple markets with another seller (see Table 7). Emergence of such mutual forbearance is consonant with Corwin Edwards' intuition, who first raised the issue in 1955 when noting that (see Sherer, 1980, p. 340):

> When one large conglomerate enterprise competes with another, the two are likely to encounter each other in a considerable number of markets. The multiplicity of their contacts may blunt the edge of their competition. A prospect of advantage from vigorous competition in one market may be weighted against the danger of retaliatory forays by the competitor in other markets.

Edwards' intuition is appealing as the scope of punishment deviations certainly does expand as the extent of multi-market contact is enhanced. Yet, Bernheim and Whinston (1990) show such reasoning can be faulty because a seller, realizing that punishment will accrue in every market, will simply decide to cheat in every market, potentially merely raising proportionally the costs and benefits of an optimal deviation. Several characteristics of the market, however, serve to severe Bernheim and Whinston's (1990) irrelevance result. For the purposes herein, these include cases where sellers attach different weights to future outcomes across markets and sellers are heterogeneous, characteristics that are likely prominent in this marketplace.

While there had been previous empirical efforts exploring this issue, the Bernheim and Whinston (1990) study stimulated much activity in this area (see Korn and Baum (1999) for a survey). Empirical results on the relationship between multi-market contact and competition have generally been mixed, this stands to reason since the endogeneity of such arrangements in naturally-occurring data makes it difficult to pin down this effect empirically.

Of course, this issue plagues inference made from the natural field experimental data as well. Again, however, insights gained from the framed field treatments can help since I formed partnerships randomly. 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, conditional on the total number of collusive arrangements. Complementing this evidence is the finding that the number of outside agreements is only marginally correlated with cheating rates in the framed field treatments, suggesting that selection is not a major issue plaguing the natural field experimental data.

A third comparative static insight gained from the naturally occurring data is that sellers cheat more on higher volume days (see Table 7). 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.[21] 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 (see Table 6 and Figure 2) presents some, albeit imperfect, evidence that is consonant with the results from the natural field experimental data.[22]

In sum, each of these three examples suggests that critical evaluation of the natural field experimental data reveals that sharp tests of the theory 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 because of the artificial imposition. Combined, however, the various treatments highlight the gain in inferential power of data observed across several domains.[23]

B. Speaking to Policymakers

For more than 100 years, economists and historians serving as expert witnesses, 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 influence world petroleum prices in the early 1970s, several leading economists

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[21] Haltiwanger and Harrington (1991) present a model whereby the demand shocks are serially correlated—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.

[22] Further, the laboratory data provide results consistent with Green and Porter (1984), in that in many cases "price wars" occurred after one player chose to undercut prices or after the 5th period demand shock. In the field data, it is difficult to parse equilibrium play in the Green and Porter (1984) model from cheating in the Stigler sense.

[23] If I examine the proportion of unique sellers that cheat in these arrangements consonant insights are obtained across the relevant treatments.

predicted its imminent demise. Adelman (1972, p. 71) wrote that "Every cartel has in time been destroyed by one and then some members chiseling and cheating…"

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 sample lasted between 3.7 and 10 years (see p. 51, Table 1).[24] The effect 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 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, the 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, however, in those cases where collusion was stable (the lab and framed field treatments), much surplus was lost. Sellers were able to exact large price increases through explicit collusive arrangements and such conspiracies frustrated market efficiency considerably.

[24] 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) argue that "the most frequent causes of cartel failure are entry and bargaining problems."

These contrasting insights from nearly identical experimental situations highlight a general lesson for policymakers: transference of results across situations (or time, or different industries) is dangerous. Theory teaches us that the determinants of cartel success are rich, 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 collusion is having an important 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. I conclude in noting that empirical work measuring comparative statics, such as those in this study, can provide a hint about where to look for fire, but cannot determine by themselves whether there is an actual fire worth extinguishing.

C. Methodological Content

Beyond permitting sharper inference, an advantage of sampling various environments 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: determine 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.

As a guide, I use the framework put forth by Levitt and List (2007), which highlighted that selection rules into the actual experiment might be important. In this regard, comparisons of interest include observing behavior of (i) identical individuals in the lab and the field, (ii) agents drawn from the same population engaged 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 engaged in lab and field experiments.

In the data, I find that individuals drawn from different populations show considerable signs of an ability to maintain collusive ties, even in austere situations. Yet, there are some behavioral disparities. For instance, students are influenced much more by changes in anonymity whereas marketers are influenced to a greater extent by context. For the case of agents drawn from the marketing population and placed in lab and field experimental roles, I find only marginal evidence that selection is important. For example, when comparing cheating rates in the natural field experiment across those who agreed to participate in the lab experiments and those who refused, I find little evidence of significant differences. Though I should highlight that this comparison is made with a sample size of 17 sellers who agreed to participate in a controlled lab or framed field treatment, and 5 sellers who turned down the request (5 sellers were never asked) but participated (unknowingly) in the natural field treatment.

Finally, examining the behavior of the 17 individual sellers who were in experiments across the lab and the field provides insights into generalizability of results across domains. Levitt and List (2007) 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. Using data from the 17 sellers (11 of whom were in a lab treatment, 3 of whom were in a lab and framed field treatment, and 3 of whom were in a framed field treatment), I find little correlation between cheating rates across the lab treatments with no context and the other environments. However, in a simple regression model, the best predictor of whether they will cheat in the natural field experiment is their measured cheating rate in the framed field treatments and the lab treatment with context.

This result extends the literature in psychology that argues there is only a weak correlation between behaviors in different settings (even across lab settings; see, e.g., Mischel, 1968; Ross and Nisbett, 1991, for reviews). For instance, Hartshorne and May (1928) discovered that people who cheat in one situation are not the people who cheat in another. For my purposes, that does not mean that there is something necessarily wrong with one of the context free lab treatments. Rather, it likely means that subjects saw one situation as relevant to honesty and one as irrelevant.

More broadly, the study highlights that given the nature of the economic science, there is much to be gained from designing experimental treatments that span the bridge between the lab and the naturally-occurring environment. The laboratory provides the sterile environment where the restricted model from physics can be the ideal. Alternatively, experimenting in a natural setting, where the looser model often employed in the biological sciences prevails, provides a useful parallel that strongly complements laboratory results. Where the laboratory can provide crisp inference and solidify insights gained from field data, field experiments can prevent the laboratory from over-developing ideas and concepts that have little parallel in the field. Likewise, if the relationships observed in the lab manifest themselves in the field, one can be re-assured that the lab has not advanced to the point of developing artificial situations that are too far removed from the field. Two-way interactions across lab and field methodologies and between theory and practice permit a much deeper and broader understanding of economics.

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Appendix A Experimental Instructions—Lab

[..] 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 10th 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

Period	Decision	Payoff
Practice 1		
Practice 2		
1		
2		
3		
4		
5		
6		
7		
8		
9		
10		
11		
12		
13		
14		

Appendix B. Confidential Survey

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 you come to this site and/or go to other sites (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:

1st 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?______________________

2nd 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?______________________

3rd dealer:

Etc.

Appendix C. Credibility of Information from the Mole

Proper inference from the natural field experiment relies on credible information from the Mole. While proving that the information is credible is akin to trying to prove that you do not have a sister, I proceed in three directions to address questions on the veracity of the Mole's claims. These identification strategies rely on parallel data gathered across markets, goods, and sellers.

The first approach is to observe negotiating patterns of the exact same dealers selling identical goods in other markets (markets geographically separated from the markets where the collusive arrangements exist). In this case, I observe 8 of the 27 dealers selling identical goods in other markets. Using the exact same buying approach as in the natural field experiment, I had buying agents systematically approach these sellers. I find in these cases that quotes typically start 10%-25% lower than received quotes from these sellers in collusive markets, and transaction prices are generally 10%+ lower in these markets. These variations are much larger than any of the cross-market price variations observed in the natural field experiment, suggesting that there is an effect of collusion. Moreover, I am not merely capturing cross-market variation with this first robustness test.

The second piece of evidence relates to keeping the seller pool and market constant, but changing the type of good sold. In these cases, I observe the exact same sellers engaging in transactions in one of my 8 markets of interest—i.e., a marketplace where they are colluding over other goods. A shortcoming is that I do not know the marginal costs for these goods, but I can learn something from the price variances because the goods in this case are substitutes for the goods in the main experimental examination. Again, using the exact same buying approach as in the natural field experiment, I find that the price variances—both negotiated and transacted—are much higher than the variances observed in the market over the colluded goods.

The third approach entails observing sellers who are selling in these markets, but who are not part of the collusive ring. I observe 7 dealers selling goods that are very close substitutes to the goods that the colluders are selling.[25] These sellers might be considered the "competitive fringe," and they sell some similar goods in 2 of the 8 markets where I observe collusion. Again, I am not aware of these sellers' marginal costs, but I can observe their bargaining tendencies. Using a similar buying approach as above, I find that in these cases the competitive fringe sellers provide quotes that are in the range of 10%-20% lower than received quotes from colluding sellers, and transaction prices are roughly 10%+ lower from these sellers.

While none of these approaches is air tight in isolation, the concurrence of evidence across the three very different approaches induced me to go forward with the experimental design trusting the Mole's claims.

[25] For the purposes herein, there are some interesting behavioral tendencies among the colluders in these markets that contain fringe sellers, but I will reserve that discussion for a different occasion. These tendencies do not frustrate inference made in this paper, however.

Table 1 Experimental Design

Laboratory Treatments	Framed Field Treatments	Natural Field Treatment
LabIS; n=24; Lab treatment with student subjects; standard game that provides individual cooperation rates **LabIIS**; n=24; Lab treatment with student subjects; adds demand shocks to Lab IS **LabIIIS**; n=24; Lab treatment with student subjects; takes away between subject anonymity from Lab IIS (subjects know the identity of their partner) **LabIIISC**; n=24; adds contextual language to LabIIIS instructions	**FramedNCSymm**; 12 buyers, 4 sellers; 2 sessions; bilateral trading market, no communication allowed between sellers and symmetric demand/supply curves **FramedNCAsymm**; 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. **FramedCAsymm**; 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 The following build on FramedCAsymm MC $7 sequentially with noted changes:	**Field**; 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

LabIM; n=24; identical to LabIS, but with subjects drawn from the open air market **LabIIM**; n=24; identical to LabIIS, but with subjects drawn from the open air market **LabIIIM**; n=24; identical to LabIIIS, but with subjects drawn from the open air market **LabIIIMC**; n=24; adds contextual language to LabIIIM instructions	**Framedinf**; 12 buyers, 4 sellers; 1 session; infinite supply at marginal cost of $7 **FramedNoprice**; 12 buyers, 4 sellers; 1 session; identical to FramedCAsymm except with no price revelation after a deal is consummated **FramedShock;** 12 buyers, 4 sellers; 1 session; identical to FramedNoprice with a demand shock **FramedTable**; 24 buyers, 16 sellers (sellers in groups of 4); good placed on table and buyers approach regularly throughout market day/weekend **Framed2sellers**; 54 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 **FramedHighStakes**; 18 buyers, 12 sellers (sellers in groups of 2), profits multiplied by 5 for sellers; sellers face the same per seller demand as Framed2sellers	

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

	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7
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	P1 Collusive	P2 Undercutting	P3 Punishment	Average Profit (per period)	Eff.
		Price Choice			
Students					
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%
Marketers					
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 profits_i/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

Treatment	Market Period (1)	(2)	(3)	(4)	(5)
NCSymm					
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)
NCAsymm$2					
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)
NCAsymm$7					
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)
NCAsymm$13.50					
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)
CAsymm$2					
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)
CAsymm$7					
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)
CAsymm$13.50					
Average price	19.00	18.00	17.00	16.50	16.44
Std. deviation	(0.0)	(0.0)	(1.41)	(1.41)	(0.50)
Quantity	(Q=0.5)	(Q=2)	(Q=2.5)	(Q=4)	(Q=4)

Table 4 continued

Treatment	**(1)**	**(2)**	**Market Period** **(3)**	**(4)**	**(5)**
FramedInf					
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)
FramedNoprice					
Average price	14.70	14.83	14.00	14.00	13.86
Std. deviation	(0.45)	(1.33)	(0.63)	(0.63)	(0.69)
Quantity	(Q=5)	(Q=6)	(Q=7)	(Q=6)	(Q=7)
FramedShock					
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

Treatment	Seller Negotiated Offer	Percent Cheated	Percent Transacted	Transacted Price	Percent Cheated	Average Cheat
Framed *(lab markets pooled)*	---	---	47%	$14.51 (0.90)	9.6%	6.4%
FramedTable	$13.96 (0.97)	18.4%	64%	$13.51 (1.08)	33%	12.8%
Framed2Sellers	$14.36 (2.14)	7.8%	58%	$13.99 (1.35)	16%	7%
FramedHighStakes	$13.48 (2.30)	29%	69%	$12.42 (1.47)	48%	16%

Note: Figures in the 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 2-seller demand treatments. "Seller Negotiated 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 (in the immediate column to the left) that were lower than the agreed upon collusive arrangement. "Percent Transacted" is the percentage of visits to the market that resulted in a transaction (each unique induced value represents one visit to the market). "Transacted Price" is the average price of the good across every actual sale in that treatment. Of those 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. Hence, the numbers can be read as follows for the FramedTable treatment: the average negotiated seller offer was $13.96, 18.4% of the final individual offers were cheating, 64% of the induced values resulted in a transaction, the average transacted price was $13.51, 33% of transactions were below the agreed upon collusive price, and the average transaction among those observations that were cheating was 12.8% below the price agreement. The various treatments are defined in Table 1.

Table 6 Framed and Natural Field Experimental Data Summary

Treatment	Percent that Cheated		Price Deviation From Agreement	Percent	Surplus
	Negotiated	Transacted	Transactions	Transacted	Captured
Framed (pooled)	---	9.6%	6.4%	47%	61%
FramedTable	18.4%	33%	12.8%	64%	77%
Framed2Sellers	7.8%	16%	7%	58%	72%
FramedHighStakes	29%	48%	16%	69%	84%
Natural Field	46%	69%	19%	81%	90%

Note: Figures in the 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. “Percent that Cheated” is the percentage of price quotes that were lower than the agreed upon collusive arrangement. “Price Deviation from Agreement” is the percentage price deviation from the agreed upon collusive price averaged across all observations in each session. “Percent Transacted” is the percentage of visits to the market that resulted in a transaction (each induced value represents one visit to the market). “Surplus Captured” is the percentage of available rents captured in the market. The various treatments are defined in Table 1.

Table 7. Regression Results for Natural Field Experiment

	Individual Level				Group Level	
Variable	**Cheat**	**Cheat**	**%Price Dev.**	**%Price Dev.**	**%Price Dev.**	**%Price Dev.**
Collusive Pair	-0.22	-0.21	-0.024	-0.06	-0.07	-0.07
	(0.07)	(0.09)	(0.015)	(0.023)	(0.03)	(0.05)
Collusive 3-Some	-0.13	-0.15	-0.23	-0.05	-0.11	-0.09
	(0.06)	(0.09)	(0.014)	(0.023)	(0.04)	(0.07)
Multiple Agreements	-0.18	-0.22	-0.06	-0.05	-0.13	-0.16
	(0.07)	(0.08)	(0.014)	(0.03)	(0.04)	(0.07)
High Volume Day	0.25	0.27	0.04	0.04	0.02	-0.003
	(0.05)	(0.08)	(0.012)	(0.02)	(0.04)	(0.04)
Life Relationship	-0.26	-0.24	-0.04	-0.02	-0.05	-0.008
	(0.06)	(0.08)	(0.014)	(0.02)	(0.03)	(0.05)
Male	0.18	0.09	0.03	-0.02	0.002	0.07
	(0.09)	(0.10)	(0.021)	(0.04)	(0.05)	(0.08)
All Female	0.22	0.13	0.03	-0.03	-0.001	-0.08
	(0.14)	(0.20)	(0.03)	(0.09)	(0.05)	(0.09)
Constant	0.48	0.55	0.09	0.13	0.27	0.24
	(0.08)	(0.10))	(0.02)	(0.04)	(0.04)	(0.06)
Seller Effects	No	Yes	No	Yes	No	Yes
Buyer Effects	No	Yes	No	Yes	No	No
R^2	0.13	0.13	0.09	0.05	0.18	0.15
N	455	455	455	455	164	164

Notes:

1. Dependent variable is whether the individual seller cheated in columns 1 and 2 (cheat = 1 if yes); in columns 3 and 4 the percentage price deviation from the agreed upon price is the dependent variable. Columns 5 and 6 contain the data aggregated at the group level, and explore the deviation of the transacted price from the agreed upon collusive price in percentage terms. *Collusive Pair* = 1 (*Collusive 3-Some*) if the collusive group was composed of two (three) sellers, 0 otherwise, and therefore the four person collusive group represents the baseline. *Multiple Agreements* is the indicator variable that equals 1 if the seller had pricing agreements across spatial markets with another seller in the group, 0 otherwise. *High Volume Day* is the indicator variable for market volume and equals one if the experiment was executed on a day that was in at least the 67^{th} percentile of all business days I observed. *Life Relationship* is the indicator variable that equals 1 if the seller was married or was a relative to another seller in the group, 0 otherwise. *Male* = 1 if seller was male, 0 otherwise. *All Female* = 1 if seller group was all female, 0 otherwise
2. Standard errors are in parentheses beneath coefficient estimates.

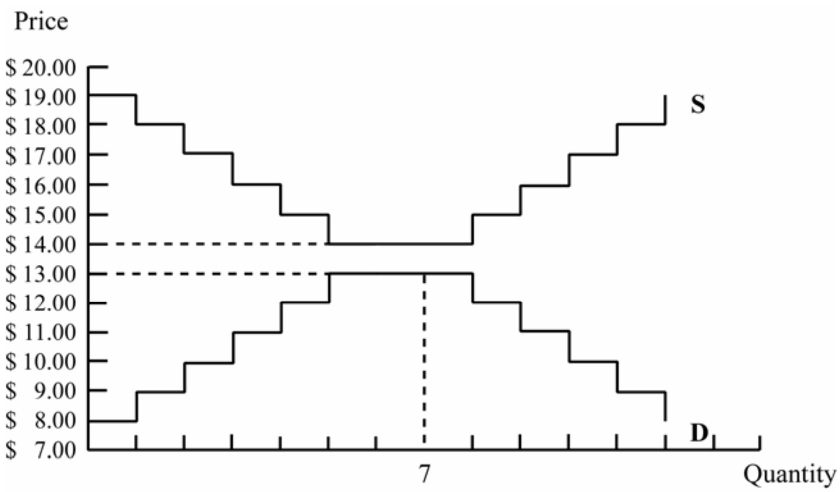

Figure 1. Supply and Demand Structure

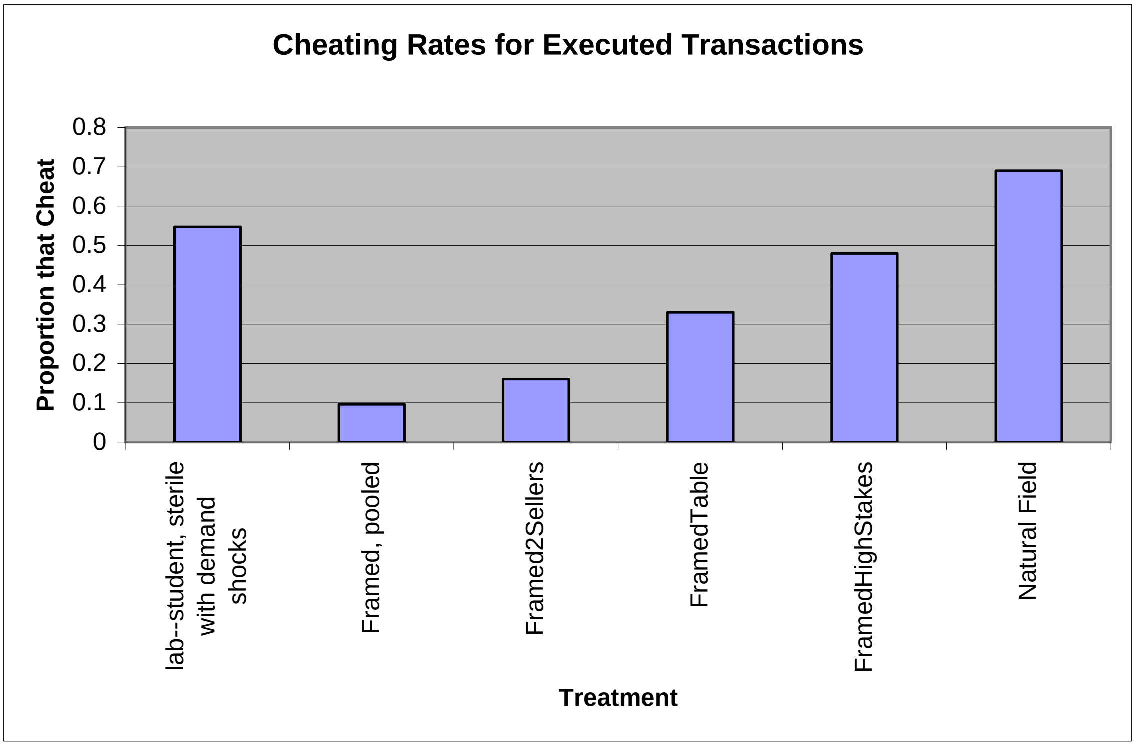

Figure 2. Cheating rates for executed transactions across the various treatments. Figures provide the proportion of actual transactions that were at prices below the agreed upon collusive price.