7-2019

An Experiment on Innovation and Collusion

Andrew Smyth
Marquette University, andrew.smyth@marquette.edu

Economic Inquiry, Vol. 57, No. 3 (July 2019): 1526-1546. DOI.

An Experiment on Innovation and Collusion

Andrew Smyth

Department of Economics, Marquette University, Milwaukee, WI

Abstract

This paper examines the relationship between product innovation and the success of price collusion using novel laboratory experiments. Average market prices in low innovation (LO) experiments are significantly higher than those in high innovation, but otherwise identical experiments. This price difference is attributed to LO experimental subjects' greater common market experience. The data illustrate how collusion can be perceived as the "only way to make it" in LO markets where product innovation is not a viable strategy for increasing profits. They suggest that product homogeneity can be a proximate cause, and product innovation an ultimate cause, of collusion.

Keywords

innovation, collusion, antitrust, experimental economics

ABBREVIATIONS

HI High Innovation
LA Liberal Arts School
LO Low Innovation
LOB Line of Business
R Research School
R&D Research and Development
SHI "Super" High Innovation
SIC Standard Industry Classification
VIFs Variance Inflation Factors

I. Introduction

> [W]e're not competing with a unique article here. Our bags and boxes aren't really any better or worse than those of our competitors...The only way to get a buyer is to sell at a lower price. Thus competitors may think that the only way to make it is to get together and fix prices.
>
> — Folding box executive who participated in a price conspiracy[1]

This paper tests whether price collusion is more successful in markets like the epigraph's folding cardboard box market, than in markets where firms can more easily innovate to escape competition. It reports data from laboratory experiments where subjects repeatedly make "product innovation" and pricing decisions. The experimental treatments differ only in the *ex ante* likelihood of innovation, and so mimic two very different markets: "high innovation" (HI) markets where firms frequently develop differentiated new products and "low innovation" (LO) markets where firms almost always sell homogeneous products.

The empirical price fixing literature finds that collusive markets are often characterized by product homogeneity.[2] Product innovation affects the degree of product homogeneity in a market, so it is natural to ask: how does product innovation affect price collusion? This paper aims to help fill a void in the literature by empirically examining the causal link between product innovation, product differentiation, and price collusion.[3]

In the experiments reported here, "product innovation" is a function of an exogenous parameter that determines the likelihood of innovative success, and of subjects' endogenous decisions about how much to spend on innovation. Innovative success results in perfect product differentiation, whereas innovative failure means perfect product homogeneity. The experimental design varies the aforementioned exogenous innovation parameter across treatments—holding all else constant.

By design there are no predicted price differences between the HI and LO treatments, yet observed prices in the LO treatments are significantly greater than those in the HI treatments. The data show that subjects in the LO treatments are better at maintaining supra-competitive prices than their HI counterparts. Moreover, while this collusive success is affected by the exogenously determined likelihood of innovative success, collusive success does not affect innovation expenditure, so the price results are driven by the exogenous innovation parameter.

The results reported in this paper suggest that a lack of product innovation can be the *ultimate* cause of collusive success, whereas product homogeneity resulting from a lack of product innovation is a *proximate* cause of collusive success. The experimental data illustrate how collusion can come to be perceived as the "only way to make it" in LO markets where product innovation is not a viable way to increase profits. In the next section, I motivate the use of laboratory experiments. In Section III, I outline the experimental design, calculate innovation benchmarks for the experiments, and report the experimental data. Section IV concludes the paper.

II. Why an Experiment?

Before discussing the experimental design, I first motivate my use of laboratory experiments by outlining the shortcomings of using archival data to examine the relationship between innovation and collusion. I created a sample of historical price conspiracies by examining all citations listed under "price fixing" in the indices of Commerce Clearing House *Trade Cases* books for the years 1972–1982. My sample includes all (prosecuted) horizontal price conspiracies that took place in a Standard Industry Classification (SIC) manufacturing industry. I chose this 10-year sample period in order to match the conspiracy sample with data from the Federal Trade Commission's Annual Line of Business (LOB) Report for 1977.[4]

Table A1 in the Appendix lists the final sample, which totals 50 conspiracies. Thirty-seven of the 50 (74%) occurred in industries with below-average research and development (R&D) intensity, as calculated from the LOB data.[5] A robust rank order test concludes that the mean of the distribution of R&D intensities for collusive industries is lower than the corresponding mean for noncollusive industries ($U = 1.86$, $p = 0.032$, one-tailed).

Table gives estimation results for two Probit specifications.[6] The variable *Collusion* is an indicator for a conspiracy having been detected and punished in the SIC industry during a 10-year window around 1977. *Profit* is calculated as the ratio of operating income to sales, *ADInt* is a proxy for product differentiation and is calculated as the ratio of advertising expense to revenue, *Size* proxies barriers to entry and is the natural logarithm of assets, and *C4* is the industry's adjusted four-firm concentration ratio.[7] Finally, *RDInt* is R&D intensity, calculated as the ratio of firm R&D costs to revenue.

Table 1: Probit Estimates

	Dependent Variable: Collusion	
Independent Variable	**(1)**	**(2)**
Constant	−2.854	−3.288
	(1.275)	(1.438)
Profit	−2.965	−1.159
	(2.562)	(2.751)
ADInt	−9.418	−9.659
	(6.738)	(6.601)
Size	0.196	0.234
	(0.094)	(0.104)
C4	−0.013	−0.012
	(0.007)	(0.007)
RDInt		−19.280
		(10.156)
Observations	217	202
Log-likelihood	−84.43	−78.14

1 *Note*: Standard errors in parentheses.

- 2 * Significant at the 10% level.
- 3 ** Significant at the 5% level.
- 4 ***Significant at the 1% level.

Model 1 is similar to a specification in Asch and Seneca's (1976) well-known empirical price-fixing study, and the estimates here are qualitatively the same. Model 2 adds *RDInt* to the specification. Its coefficient estimate is statistically significant and negative in sign. The addition of *RDInt* to the specification causes a statistically significant improvement in log-likelihood ($LR = 12.57$, $p < 0.001$).

The inverse relationship between *Collusion* and *RDInt* in Model 2 is at least consistent with innovation affecting price collusion. However, collinearity is a potential issue here.[8] Another possible problem is that the price conspiracy data suffer to an unknown degree from selection bias. *Collusion* may indicate not only collusion-prone industries, but that subset of collusion-prone industries which are also prosecution-prone. It is certainly possible that successful collusive occurred in additional industries but escaped the detection of antitrust authorities.[9]

Even ignoring possible econometric issues, the significant, negative coefficient estimate on *RDInt* in Model 2 reveals correlation between price collusion and R&D intensity, not necessarily causation. The inverse relationship might stem from firms who are successfully colluding, reducing their innovation intensities. Such behavior has been empirically documented: Erickson (1976) reports that price conspiracies had a detrimental effect on cost innovation in gymnasium seating, rock salt, and structural steel.

With these issues in mind, laboratory experiments were conducted to see if exogenous variation in the likelihood of innovation causes observed variance in the success of price collusion.[10]

III. The Experiments

These experiments were designed to incorporate "product innovation" into laboratory markets so as to permit exogenous variation in the likelihood of innovation across multiple treatments. In this paper, "successful collusion" refers to firms' abilities to maintain supra-competitive prices. If the data reveal differences in market prices across treatments, they support the conjecture that innovation affects the success of price collusion.

The laboratory research most related to these experiments involves product differentiation (see Brown-Kruse, et al., 1993; Brown-Kruse and Schenk, 2000; Collins and Sherstyuk, 2000; Garcia-Gallego and Georgantzis, 2001; Barreda-Tarrazona, et al., 2011). In these papers, differentiation is captured by location choice. Here, innovation success or failure determines the number of firms in a market. Innovation is not rivalrous—one subject's innovation success is independent of another's.[11] If successful, subjects enjoy one period of monopoly power; if unsuccessful, they must compete with other unsuccessful subjects in a Bertrand–Edgeworth market.

In this paper, successful innovation affords an innovator a perfectly appropriable market. When unsuccessful, appropriability is nil; subjects compete in a perfectly homogeneous market whose size varies from one to four firms. This stark design allows for exogenous variation in the *ex ante* likelihood of innovative success. The experiments reflect two types of markets: one in which firms frequently develop short-lived, perfectly differentiated new products and another in which firms rarely develop such "killer" products and so almost always compete to sell a homogeneous product.[12]

A. Experimental Design

In these experiments, undergraduate students with no prior experience in similar experimental markets made innovation and pricing decisions. Prior to the start of the experiment, the subjects were randomly assigned into groups of four, and they remained in their group for 25 subsequent periods. Each period was subdivided into two stages: an Innovation stage and a Market stage. In Innovation stages, subjects made innovation expenditure decisions, and in Market stages they made pricing decisions. Table 2 lists the key experimental parameters.

Table 2: Experimental Parameters

Parameter	Value
Endowment	$4.00
Attempts	[0, 20]
Cost per attempt	$0.10
Prob(Innovation|1 Attempt)	5%, 15%, or 25%

Price	[$8.25, $20.00]
Unit production costs	
$q \leq 3$	$8.15
$q = 4$	$8.25
$q > 4$	∞
Market stage length	
Periods 1–5	60 seconds
Periods 6–25	40 seconds

5 *Note*: The $ sign denotes experimental dollars.

At the beginning of the experiment, subjects were endowed $4.00 (where the $ sign denotes experimental dollars). In each Innovation stage, every subject was given the option of purchasing a innovation attempts. Each attempt cost $0.10. Subjects could purchase up to 20 attempts each period. Innovation was a Bernoulli process; innovation attempts resulted in innovation success according to the function $\theta(a) = 1 - (1 - \rho)^a$. The probability that any one attempt was successful, ρ, was 5%, 15%, or 25% as discussed below. Attempts were purchased prior to the realization of the innovation outcomes, so all a attempts were paid for, regardless of whether they were necessary to achieve innovation success ex post.

If a subject was successful, they developed a "New product" that they could sell as a monopolist for one (the current) period. In other words, if a subject was successful in an Innovation stage, they posted a price in their own New product market during the subsequent Market stage. Subjects who attempted no innovation, or who were unsuccessful in their attempts, competed in a Bertrand–Edgeworth market with other unsuccessful sellers from their group to sell a homogeneous "Standard product." As a function of the subjects' endogenous innovation expenditures and the stochastic innovation process, this Standard product market contained either 1, 2, 3, or 4 sellers.[13] If three of the four sellers in a group were successful, the lone unsuccessful subject in the Standard product market had their price automatically set to the lowest allowable price of $8.25. This ensured that no unsuccessful innovator enjoyed monopoly power. Figure 1 shows how market type and the number of firms in the market were determined.[14]

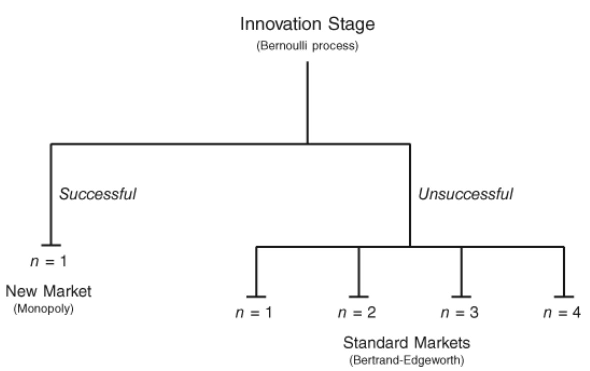


Figure 1: Determination of Market Type and Size

The Market stage was timed. During the first five periods of the experiment, subjects had 60 seconds to submit a price. For the final 20 periods, they had 40 seconds.[15] They were permitted to change their price as many times as they wished before time expired. While they could adjust their price, they could not see other subjects' prices prior to the end of the stage. A red timer counted down the remaining market time in a prominent location on each subject's computer screen.

For the entire experiment, the first 3 units a subject might sell cost $8.15 to produce. The 4th unit they might sell cost $8.25. Sellers were capacity-constrained at 4 units. Units were "made to order," so production costs were only borne for units actually sold. Market demand and one seller's marginal costs are depicted in Figure 2.

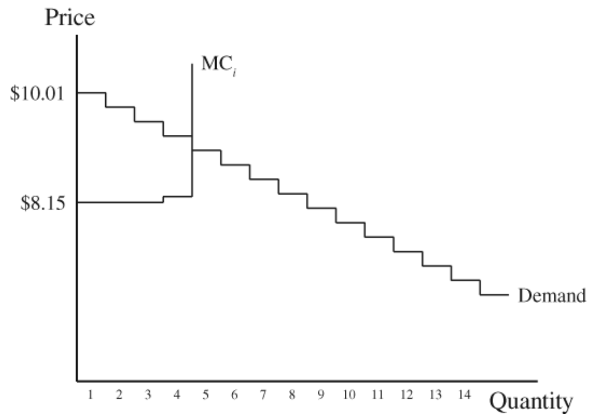


Figure 2: Experimental Market Demand

The demand sides of the markets were automated. Each computerized buyer demanded a single unit at a unique reservation price. The queue was not random; buyers "queued up" in descending order of their reservation price ($10.01, $9.76, $9.51,...). In New markets, the monopolist seller sold up to 4 units, depending on how many buyers had reservation prices above their posted price. In Standard markets, the seller posting the lowest price had the opportunity to make sales first. Buyers bought from a seller, conditional on that seller's price being less than their reservation price. If there was residual demand after the low-price seller made sales, the seller with the next lowest price could make sales. Thus, it was possible (and most often the case) that units of the homogeneous product sold for different prices in the same Standard market. When two or more sellers posted the same price, market demand was split evenly when possible. The experimental software randomly awarded the extra unit(s) in cases where demand could not be evenly split.

Because I am interested in differences in collusion across treatments and not collusion per se, the Market stage was constructed to lessen the coordination burden of collusion. It had the following features: (1) subjects could adjust their price as many times as they wished before market time expired, (2) their prices were publicly posted, (3) subjects were identified by numbers (i.e., Seller 1, ..., Seller 4) that were fixed throughout all 25 periods, (4) subjects could send unrestricted chat messages during Standard Market stages, and (5) subjects received feedback at the end of each period on the quantities sold by all members of their group. These features facilitated collusion in other experimental studies.[16] They were present in *all* treatments.

There were two main treatments: a LO treatment where the chance of innovation success per attempt was $\rho = 5\%$, and a HI treatment where $\rho = 15\%$. A third, "super" high innovation (SHI) treatment with $\rho = 25\%$ is discussed below. Aside from the different ρ's, the treatments were exactly identical. Prior to the start of the experiment, subjects read instructions and had to successfully complete a short quiz on their content before proceeding. Although the rationing rules for the two market types were explained to the subjects in detail, they were not told the specific reservation prices of the automated buyers. Please see Appendix S1 (Supporting Information) for the instructions.

B. Innovation Benchmarks

In this section, I report innovation benchmarks for each treatment.[17] Because innovation decisions were independent across periods, I construct the benchmarks for a single, representative period. To derive innovation benchmarks, I first determine Market stage profits and then use these values to calculate the benchmarks. I assume risk-neutral firms who innovate symmetrically. In other words, I assume that four firms independently select a innovation attempts each period.

The Market stage prices, quantities, and profits used to calculate innovation benchmarks are shown in Table 3. Recall from Section III.A that price in the $n = 1$ Standard market is set to $8.25, which implies 4.00 units sold. A unique pure strategy equilibrium of \$8.25 exists for the three- and four-seller Standard markets but there is no pure strategy price equilibrium for the two-seller market.[18] In the three-seller Standard market, firms sell 2.67 units in expectation (8 units divided by three sellers), and in the four-seller Standard market each firm sells 2.00 units. For the two-seller case, I calculate the mean of the distribution of prices in the symmetric mixed-strategy equilibrium to be \$8.59, and I assume a quantity of 3.00 units.[19] Finally, in the $n = 1$ New market, profit-maximization implies 4.00 units sold at a price of \$9.26. Importantly, the prices in Table 3 are the same across the LO, HI, and SHI treatments. In addition to calculating benchmarks using the profits in Table 3, I calculate a second set of benchmarks using actual profit data from the experiments (this is described below).

Table 3: Market Values

Market Type	Price	Quantity	Profit
Standard $n = 1$	8.25	4.00	0.30
Standard $n = 2$	8.59	3.00	1.32
Standard $n = 3$	8.25	2.67	0.27
Standard $n = 4$	8.25	2.00	0.20
New ($n = 1$)	9.26	4.00	4.34

Every period, there are 16 (2^n) possible innovation outcomes in the four firm market. Firm i successfully innovates in eight of the outcomes and is unsuccessful and ends up in a Standard market in the other half of the outcomes. For the three firms that are not Firm i, let $\phi_n(a) = [\theta(a)]^{3-n}[1 - \theta(a)]^n$ be the probability that $n \leq 3$ of these firms fail to successfully innovate when all firms independently make a innovation attempts.

Among the eight cases where Firm i is unsuccessful, there are three outcomes where two firms besides Firm i are unsuccessful ($3\phi_2$) and three outcomes where one other firm besides Firm i is unsuccessful ($3\phi_1$). There is also one outcome where all three firms besides Firm i are unsuccessful (ϕ_3) and one outcome where Firm i is the only unsuccessful firm (ϕ_0). Putting this together, Firm i 's expected profit in the event that all four firms innovate symmetrically is:

$$\Pi ia = \theta a\pi N + 1 - \theta a\phi 3a\pi 3 + 3\phi 2a\pi 2 + 3\phi 1a\pi 1 + \phi 0a\pi 0 - ca \quad (1)$$

where π_N is the New market profit and π_n is the profit in the Standard market with n firms. The coefficient c is the cost per innovation attempt, which was \$0.10 in the experiments.

The innovation benchmarks that I report for each treatment are the $a \in [0, 20]$ that maximize $\Pi_{\mathrm{i}}(a)$. Equivalently, they are the number of attempts (a^*) for which the expected marginal return from innovation equals the marginal cost of innovation. Figure 3 plots the expected marginal return from innovation for each treatment. The vertical axis is denominated in experimental dollars (\$)—the currency used in the experiments. The expected return varies across treatments because the probability of success per attempt parameter (ρ) varies across treatments. The innovation success function in LO is less concave than the related functions in SHI and HI, so the expected marginal return curve for LO in Figure 3 is flatter than the marginal return curves for SHI and HI.

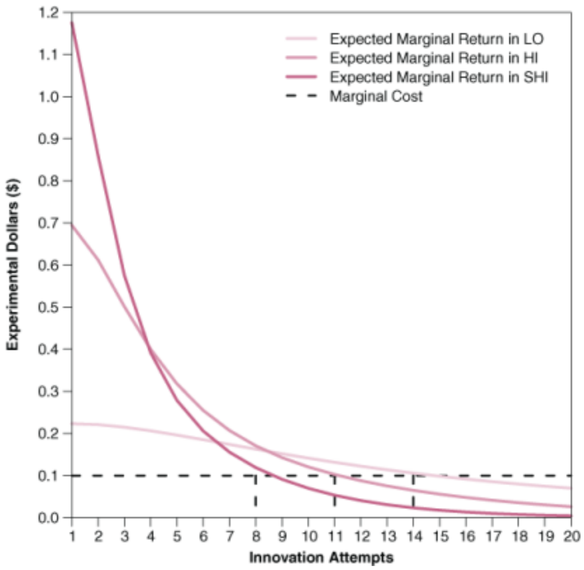


Figure 3: Expected Marginal Return and Cost of Innovation

Table 4 lists the innovation benchmarks and shows the likelihood that a firm ends up in the New market if they choose the benchmark number of attempts, that is, the probability $\theta(a^*)$. Note that the SHI benchmark is eight attempts because, as Figure 3 shows, the expected marginal return to nine attempts is less than the marginal cost of nine attempts and non-integer attempts (e.g., 8.4) were not permitted in the experiment.

Table 4: Innovation Benchmarks

Treatment	Number of Attempts (Calculated with Theoretical Profit)	New Market Likelihood	Number of Attempts (Calculated with Actual Profit)	New Market Likelihood
LO-R	14	0.51	9	0.37
HI-R	11	0.83	8	0.73
LO-LA	14	0.51	9	0.37
HI-LA	11	0.83	7	0.68
SHI-LA	8	0.90	8	0.90

6 *Note*: The actual profit benchmarks were generated using the observed average profits from each treatment (see Table).

Because prices actually observed in the experiments may differ substantially from the prices in Table 3, I also calculate innovation benchmarks using the average prices in each treatment. In other words, I use the prices in Table (see below) for $(\pi_N, \pi_1, \pi_2, \pi_3, \pi_4)$. Table 4 suggests that LO subjects may attempt more innovation than HI or SHI subjects.[20] When actual profits are used to generate innovation benchmarks, the benchmarks suggest similar amounts of innovation attempted in each treatment.

There are two results from this section to reiterate in summary: (1) *For any market type, observed prices should be the same across treatments,* and (2) LO *subjects should attempt more innovation than* HI *or* SHI *subjects, but are likely to spend more time during the experiment in Standard markets than are* HI *or* SHI *subjects.*[21]

C. Results

The experiments were conducted at two universities: a large, public research school (R) and a small, private liberal arts school (LA).[22] Subjects were recruited with ORSEE at the research school (Greiner, 2015) and by proprietary recruitment software at the liberal arts school. In both locations, the experiment was executed in z-Tree (Fischbacher, 2007). All treatments lasted approximately 1.5 hours, including roughly 15 minutes of computerized instructions. There were a total of 240 subjects, 48 in each treatment. Subjects had no previous experience in similar markets and no subject participated more than once.[23] I now report the experimental results.

Did Innovation Vary across Treatments?

I first focus on the Innovation stage data from the LO and HI treatments and ask: did attempted innovation vary across treatments, and if so, did subjects get differential experience in certain market types across treatments?

I begin by reporting the distribution of innovation attempts. Figures 4A and 4D are kernel density estimates of the average number of innovation attempts per subject (the average is across all 25 periods), by treatment. Individual frequency distributions are also presented in the Appendix for all 240 subjects. In Figure 4, there appears to be a treatment difference across LO and HI in the R data, but not in the LA data.

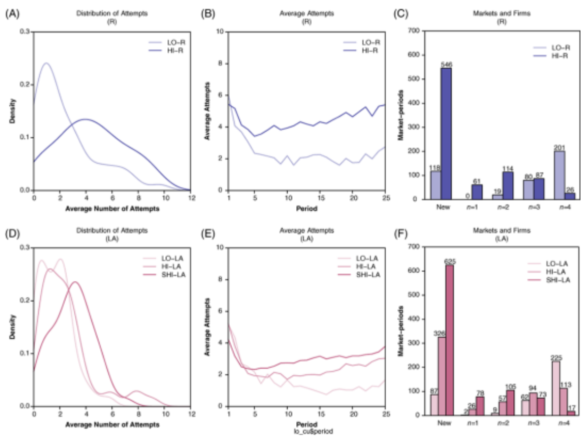


Figure 4: Innovation Results, by Treatment

Figures 4B and 4E show the average number of innovation attempts per market across time. Clearly, on average, subjects in both treatments under-invested in innovation relative to the benchmarks from Section B.[24] Figure 3 suggests a possible explanation for this result: for a small number of attempts, the expected marginal return from an attempt is greater in HI than in LO. Subjects may have keyed on this fact instead of on equating the marginal return and marginal cost of innovation.

Despite the benchmarks suggesting more innovation attempts in LO than HI, HI subjects attempted more innovation than LO subjects in both populations. The attempts graphs in Figure 4 and the average attempts per period figures in Table 5 indicate that the level of innovation attempted was not robust to changes in the subject population. For each treatment, the liberal arts school subjects attempted less innovation than the research school subjects. However, there was a robust treatment effect: in both populations, subjects attempted more innovation in HI than LO.

Table 5: Summary Statistics

	LO-R	HI-R	LO-LA	HI-LA	SHI-LA
Subjects	48	48	48	48	48
Markets	12	12	12	12	12
Success per attempt (ρ)	5%	15%	5%	15%	25%
Mean attempts per period	2.42	4.43	1.67	2.45	2.99
Time in New market	10%	46%	7%	27%	52%
Time in Standard market	90%	54%	93%	73%	48%
Modal # firms in market	4	1	4	1	1
Mean period earnings ($)	0.66	1.62	0.71	1.19	1.93

Mean total earnings ($)	16.94	40.58	18.04	29.91	48.26

7 *Note*: The $ symbol denotes experimental dollars.

Because innovation success was an increasing function of the number of innovation attempts, and because more innovation was attempted in HI, LO and HI subjects had differential experience in certain market types. Figures 4C and 4F show the distribution of market-periods across the number of firms in the market (denoted by *n*).[25] In both figures, "New" refers to the New market, and $n = 1$ refers to the $n = 1$ Standard market.

The number of market-periods of experience increased monotonically with the number of firms in the market in both LO treatments (ignoring the $n = 1$ Standard market type). By contrast, in the HI-R treatment, the number of market-periods decreased monotonically with the number of firms in the market (again, ignoring $n = 1$ Standard markets). Table 5 shows that the modal number of firms in the market was $n = 4$ in LO, but was the New ($n = 1$) market in the HI treatment. Subjects were in Standard markets 90% and 93% of the time in LO-R and LO-LA, respectively, but were in a Standard market just 54% of the time in HI-R.

As predicted by the innovation benchmarks, LO-R subjects ended up in Standard markets more frequently than HI-R subjects. Interestingly, while HI-LA subjects attempted more innovation than LO-LA subjects, they did not attempt nearly as much innovation as HI-R subjects. As a result, HI-LA subjects spent 73% of their time in a Standard market. Because relatively little innovation was attempted in HI-LA, an additional SHI treatment was conducted with subjects from the liberal arts school population. The chance of innovation success per attempt was ρ = 25% for this treatment. This value of ρ was chosen with the hope of replicating a distribution for the number of firms in the market that is closer to HI-R than HI-LA.

To see the effect of increasing ρ to 25% in the LA subject population, compare HI-R in Figure C to HI-LA and SHI-LA in Figure 4F. This comparison shows that the distribution of the number of firms in the market in SHI-LA was much closer to that in HI-R than it was to the distribution of the number of firms in the market in HI-LA. Having established that LO-R (LO-LA) subjects spent more time in Standard markets and less time in New markets than HI-R (SHI-LA) subjects, I now report Market stage data, beginning with an analysis of chat messages in Standard markets.

How Did Subjects Communicate?

Table 6 lists the total number of chat messages, the total number of chat messages that contained a number (e.g., a price), and the 15 most frequently used words, each by treatment.[26] The total number of messages is also reported per Standard market-period to account for the greater Standard market experience of LO subjects. The data suggest that subjects in LO treatments communicated more frequently than their higher innovation treatment counterparts. Across all Standard markets, LO-R subjects communicated nearly three times as often as HI-R subjects. They sent an average of 2.6 chat messages per market-period, compared to 0.9 messages per market-period in HI-R. In the liberal arts school sessions, LO-LA subjects sent over one and a half times as many messages per market-period as SHI-LA subjects (3.3 to 2.0), and exactly one and a half times as many messages per market-period as HI-LA subjects (3.3 to 2.2).

Table 6: Chat Analysis

	LO-R	**HI-R**	**LO-LA**	**HI-LA**	**SHI-LA**
Total messages (mean per market-period)	783 (2.6)	201 (0.9)	967 (3.3)	591 (2.2)	390 (2.0)
Total messages containing a number (percent of total)	316 (40%)	84 (42%)	417 (43%)	246 (42%)	122 (31%)
Most frequently used words (times used)	all (92)	all (34)	all (110)	all (87)	all (58)
	you (63)	put (25)	you (90)	price (73)	price (55)

	seller (62)	price (24)	time (61)	you (66)	you (47)
	price (49)	lets (23)	try (53)	money (48)	money (32)
	one (48)	same (22)	price (52)	make (48)	have (29)
	put (45)	profit (18)	money (51)	put (37)	high (29)
	time (42)	money (15)	make (48)	try (36)	make (29)
	everyone (39)	make (13)	more (48)	have (36)	everyone (28)
	try (36)	you (13)	everyone (48)	time (35)	will (26)
	money (36)	time (12)	seller (43)	same (34)	more (26)
	then (33)	more (11)	lets (41)	lets (33)	sell (26)
	profit (29)	then (10)	one (41)	high (33)	time (25)
	each (29)	should (10)	then (39)	seller (33)	same (23)
	get (29)	one (9)	will (34)	more (31)	each (23)
	more (29)	will (8)	round (33)	everyone (30)	seller (23)

8 *Note*: Only words with more than two letters are listed and the words *the*, *and*, *this*, *that*, *for*, *what*, and *lol* are excluded.

Several recent papers explore issues related to antitrust enforcement using experiments, but because of the complexity of the subjects' decision task in this paper, these experiments had no "antitrust enforcement."[27] Adding enforcement to this design ran the risk of overwhelming subjects, and as noted in Section III.A, this paper focuses on collusion across treatments, not on the existence of collusion per se. Because subjects faced no threat of punishment for explicitly communicating about prices, messages from early periods included:

> Period 2 of a LO-R market: " do you guys want to each sell at the same price? "
> Period 3 of a SHI-LA market: " lets all do above 8.25 "
> Period 4 of a HI-LA market: " Why don't we both sell at high prices? "
> Period 3 of a LO-LA market: " lets try something like 915? "
> Period 5 of a HI-R market: " dont do 8.25 then none of us profit silly "
> Period 7 of a LO-R market: " can we all agree on $9? "
> Period 8 of a HI-R market: " how about we all put the same price "
> Period 8 of a SHI-LA market: " we will all make more go high not low "

As these examples suggest, price discussions often involve numbers. Table 6 reveals that in four of the five treatments, messages included numbers 40%–43% of the time. In SHI-LA, only 31% of messages contained a number.

The most frequently used words in each treatment are listed in Table 6, and the number of times each word was used is in parenthesis. Note that the words are essentially identical across treatments. High usage of words like *all*, *everyone*, and *lets*, as well as *price*, *money*, and *profit* indicate that as in previous collusion experiments with communication, subjects used the chat interface to further price manipulation. But were subjects equally successful at price fixing across treatments? To answer this question, I turn to this paper's main empirical results that compare prices across the treatments.

Did Prices Vary across Treatments?

In this paper, "collusive success" refers to firms' abilities to maintain supra-competitive prices, so in this section I report price data from the experiments as averages and distributions. Table 7 contains average market prices. For market *m* in period *t*, let the share-weighted market price be:

$$\bar{p}_t^m = \sum_{i=1}^{n_{mt}} s_t^i \cdot p_t^i, (2)$$

Table7: Average Market Prices

Number of Firms	Theory	LO-R	HI-R	LO-LA	HI-LA	SHI-LA	Mean
$n = 1$ (New)	9.26	9.34	9.30	9.15	9.28	9.24	9.26
		(0.04)	(0.01)	(0.06)	(0.02)	(0.01)	(0.03)
$n = 2$	8.59	8.80	8.68	8.71	8.79	8.73	8.74
		(0.14)	(0.04)	(0.13)	(0.08)	(0.05)	(0.09)
$n = 3$	8.25	8.58	8.58	8.85	8.66	8.57	8.65
		(0.05)	(0.04)	(0.07)	(0.05)	(0.05)	(0.05)
$n = 4$	8.25	8.62	8.44	8.72	8.63	8.36	8.55
		(0.03)	(0.08)	(0.03)	(0.04)	(0.05)	(0.05)

9 *Notes*: Theoretical prices are explained in Section III.B. Average market price is Equation (2) averaged over all markets (in a treatment) and time. All prices in experimental dollars. Standard errors in parenthesis.

where n_{mt} denotes the number of sellers in market *m* in period t, and s_t^i and p_t^i are Firm i 's market share and price, respectively. The average market price is $\bar{p}_t^m$ averaged over all similar markets and over all periods.

Table 7 shows that average market prices decreased in the number of firms in the market. Note that the average New market price across all treatments was exactly the theoretical profit-maximizing price. For each Standard market type, average market prices were all above the theoretical prices. Pooling and averaging the price statistics from Table for the two LO treatments and comparing the result to the pooled average for the three HI/SHI treatments, there is no large price difference for the $n = 1$ New markets (the LO average is \$0.01 greater). However, the LO price averages are \$0.05, \$0.09, and \$0.10 greater than the HI/SHI price averages in the $n = 2$, $n = 3$, and $n = 4$ Standard markets, respectively. In other words, average prices were higher in LO markets relative to HI/SHI markets.

Figure 5 shows the distribution of average market prices across treatments. It contains empirical cumulative distribution functions for the New market and the $n > 1$ Standard markets. The horizontal axis in the figures is $\bar{p}_t^m$. Some treatment differences are apparent in Figure 5. In Figures 5A and 5C, the distribution of prices from LO-LA is different from the distributions of prices in the other treatments. In Figure 5D, the distributions of prices from HI-R and SHI-LA are different from the distributions of prices in the other treatments.

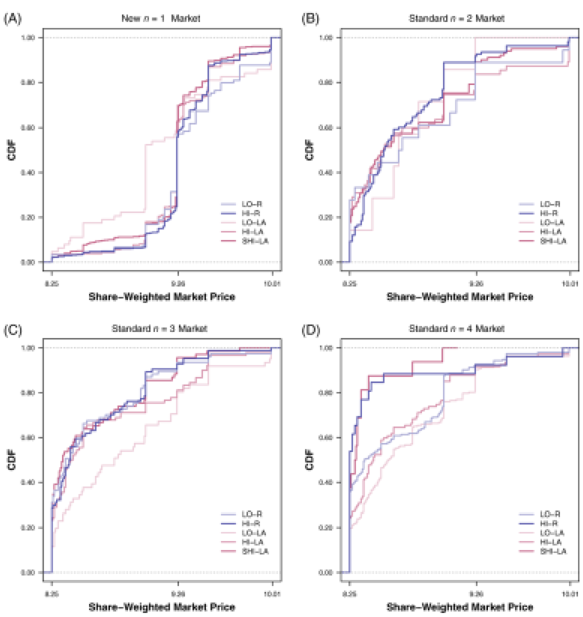


Figure 5: Empirical Cumulative Distribution Functions, by Market Type and Size

I conduct robust rank order tests to determine if the price differences suggested by Table 7 and Figure 5 are statistically significant.[28] The tests were conducted on market average prices over all periods because observations are not independent across periods. I pool the LO data and the HI/SHI data across subject populations, so the null hypothesis for each test is that the mean of the distribution of average market prices for the LO treatments equals the corresponding mean for the HI/SHI treatments.

Table 8 indicates that there is no significant difference in price across the LO and HI/SHI data for the $n = 1$ New market and for the $n = 2$ Standard market. The null hypothesis can be rejected at $\alpha = 0.10$ for the $n = 3$ Standard markets and it can also be rejected at $\alpha = 0.05$ for the $n = 4$ Standard markets.[29] The comparisons in this section all indicate that average prices were higher in LO Standard markets relative to HI and SHI Standard markets. What explains this result?

Table 8: Robust Rank Order Test Results for Price

Number of Firms	U	p Value
$n = 1$ (New)	1.182	0.237
$n = 2$	−0.357	0.721
$n = 3$	−1.693	0.090
$n = 4$	−2.566	0.010

10 *Note*: The null hypothesis for each test is that the mean of the distribution of average market prices for the LO treatments equals the corresponding mean for the HI/SHI treatments.

Does Experience Explain the Price Variance?

In light of the price data, note again the disparity in the number of $n = 4$ market-periods across the low and higher innovation treatments in Figure 4. Did LO subjects' greater experience in the $n = 4$ markets affect prices?

Figure 6 graphs market price on market experience for the Standard markets with the most firms ($n = 4$). Specifically, it shows the average of $\bar{p}_t^m$ over *t* on the number of $n = 4$ market-periods for market *m*. The line in the figure was generated by the ordinary least squares regression:

$$\frac{1}{T}\sum_{t=1}^{T} \bar{p}_t^m = \underset{(0.056)}{8.382} + \underset{(0.004)}{0.016} \cdot \text{Experience}^m, \quad (3)$$

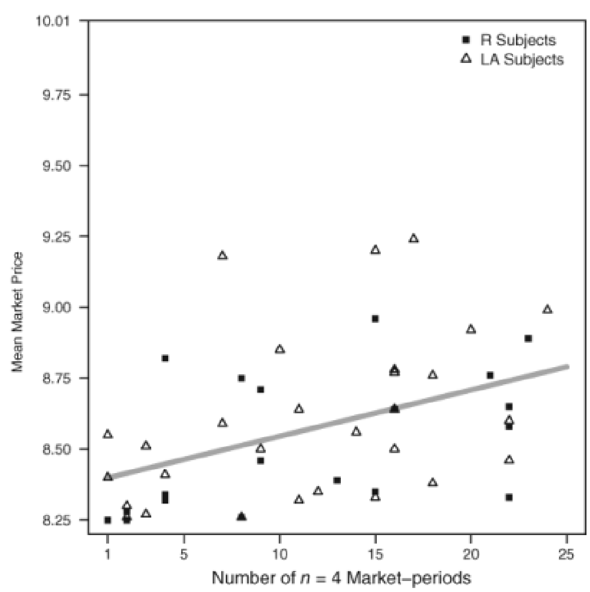


Figure 6: Market Price on $n = 4$ Market Experience

where Experience^m is the total number of periods that market m was in a $n = 4$ market.[30] Across all treatments, market experience had a significant, positive effect on market price in $n = 4$ markets.

While it is conceivable that experience was endogenous to price in the above regression, this is unlikely in principle. Even if all firms post the joint monopoly price of $9.26 in an $n = 4$ market, they only receive one-quarter of the profit they would receive in a New market.[31] So it seems unlikely that firms would reduce their innovation expenditure (which affects experience) because of the market price. Still, I now examine individual innovation decisions to see if past collusive success affected future innovation decisions.

Did Collusive Success Affect Innovation?

The preceding results suggest that the exogenously determined likelihood of innovative success affected market outcomes. It is also possible that market outcomes, in turn, endogenously affected innovation decisions. For example, subjects who successfully coordinated to raise market price may have subsequently curtailed their innovative activity.

To investigate the relationship between collusive success and subjects' innovation expenditures, a distributed lag model was estimated for each subject:

$\text{Innovation}_{i,t} = \beta_{i,0} + \sum_{k=1}^{5} \beta_{i,k} \cdot \text{Profit}_{i,t-k} + \epsilon_{i,t}$, (4)

where $\text{Innovation}_{i,t}$ is subject i's innovation expenditure and $\text{Profit}_{i,t-k}k$ is market profit (gross of innovation expenditures) in period $t - k$. The coefficient estimate $\hat{\beta}_{i,1}$ is the impulse propensity in innovation expenditure from changes in market profit during period $t - 1$. If a subject successfully coordinates with other subjects to raise the Standard market price, and then reduces his or her innovation expenditure in order to profit maximize, $\hat{\beta}_{i,1} < 0$. In other words, if innovation expenditure is endogenous to collusive success, the impulse propensity is negative.

Figure 7 shows $\hat{\beta}_{i,1}$ for each subject, organized by treatment, when specification (4) was estimated separately for all 240 subjects with standard errors adjusted for heteroscedasticity. Because of the five lags, each estimating sample had 20 observations. Estimates that are significantly different from zero at the 5% level (two-tailed *t* test) are filled-in. Table 9 shows the percentage of $\beta i_{,1}$ estimates that are both negative and statistically significant when specification (4) is estimated with between one and five lagged profit values.

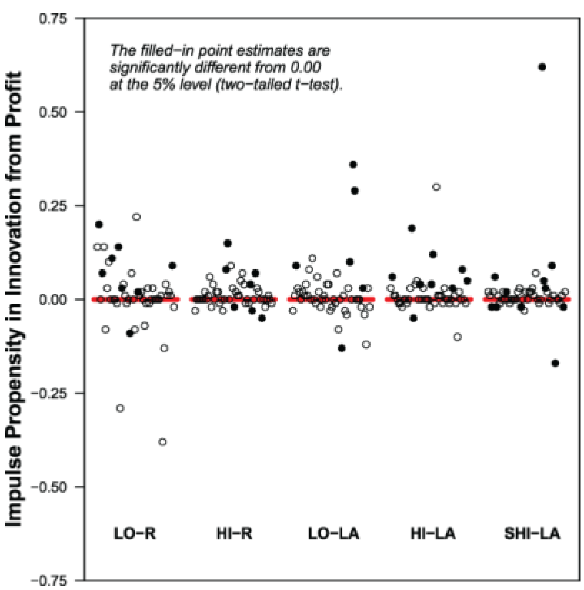


Figure 7: Estimates of $\beta_{i,1}$ in Model 4

Table 9: Summary of Impulse Propensity Estimation

				$\widehat{\boldsymbol{\beta}}_{i,1} < 0$ **Significant at**		
Number of Lags	**Number of Regressions**	**Observations Per Regression**	$\widehat{\boldsymbol{\beta}}_{i,1} < 0$	$\boldsymbol{\alpha = 0.01}$	$\boldsymbol{\alpha = 0.05}$	$\boldsymbol{\alpha = 0.10}$
1	240	24	34.6%	0.4%	2.9%	6.3%
2	240	23	34.2%	1.3%	4.6%	7.1%
3	240	22	36.7%	1.3%	2.9%	7.1%
4	240	21	34.2%	0.8%	3.8%	5.0%
5	240	20	31.7%	1.7%	4.6%	5.0%

Note that regardless of the number of lags included in (4), fewer than 5% of the estimated impulse propensities are negative and significant when $\alpha = 0.05$. To the extent that serial correlation is present in the data, even the significant estimates in Figure 7 and Table 9 may be chimeric, as serial correlation lowers standard errors. Finally, the economic magnitude of the estimates is trivial. For the five-lag specification, they suggest that, on average, a $1.00 increase in market profit resulted in a $0.02 increase in innovation expenditure.

As a robustness check on the impulse propensity results, a second regression was estimated by pooled ordinary least squares. The specification is:

$$\text{Innovation}_{i,t} = \psi + \delta_0 \text{Period}_t + \sum_{k=1}^{L} \delta_k \cdot \text{Profit}_{i,t-k} + \sum_{i=1}^{240} \theta_i \cdot \text{Subject}_i + \epsilon_t,$$

where Period_t is a linear time trend, the number of lags is $L \in \{1, 2, 3, 4, 5\}$, and Subject_i is individual subject i's fixed effect. Standard errors were clustered at the market level.

Table 10 shows the results of estimating specification (5). Regardless of the number of lags that are included in estimation (one to five), the coefficient estimates on lagged profit are always highly significant and *positive*. Moreover, the magnitude of each estimate is very small. Table 10 thus suggests—in line with the summary of individual regression results in Table 9—that an increase in market profit did not reduce innovation expenditure.

Table 10: Regression Results

Independent Variable	Dependent Variable: Innovation Expenditure				
Constant	0.259	0.173	0.162	0.134	0.132
	(0.017)	(0.017)	(0.018)	(0.020)	(0.021)
Period	−0.001	0.001	0.001	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Profit_{t-1}	0.021	0.019	0.018	0.018	0.016
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Profit_{t-2}		0.014	0.013	0.011	0.010
		(0.003)	(0.003)	(0.003)	(0.003)
Profit_{t-3}			0.011	0.009	0.008
			(0.003)	(0.002)	(0.002)
Profit_{t-4}				0.009	0.008
				(0.002)	(0.002)
Profit_{t-5}					0.008
					(0.002)

Fixed effects	Subject	Subject	Subject	Subject	Subject
R^2	0.59	0.64	0.67	0.70	0.71
Observations	5,760	5,520	5,280	5,040	4,800

11 *Notes*: Coefficient estimates on individual subject fixed effects are not reported. Standard errors (clustered at the market level) in parentheses.
12 *Significant at the 10% level.
13 **Significant at the 5% level.
14 *** Significant at the 1% level.

IV. DISCUSSION AND CONCLUSION

The experimental results in this paper can be summarized as follows: the exogenously greater likelihood of innovation in HI and SHI induced more innovation expenditure in those markets relative to the LO markets. This difference translated into more $n = 4$ market experience for LO subjects relative to HI-R and SHI-LA subjects. Market experience then affected the success of price collusion in the manner suggested by Chamberlin (1962):

> If [the firm] is in business permanently, the temporary gains of a price cut are of negligible importance...On the other hand, if [the firm] is in the market only temporarily, bent on disposing of a certain amount of product, the ultimate consequences do not enter into [its] calculations.

In the experiments, it was as though HI-R and SHI-LA subjects inhabited a world of "killer" products. These subjects were in Standard markets far less frequently than their LO counterparts, they rarely ended up in $n = 4$ markets, and often enjoyed monopoly-like profit in New markets. The data suggest that when they were in Standard markets, the long-term benefits of abstaining from price sniping did not resonate with HI-R and SHI-LA subjects.

The situation was different in LO markets. One LO subject lamented, "the innovative stage is a visual representation of [hopes] and dreams being crushed," and another bemoaned, "I wonder what the new market is even like." The LO treatment was like a market devoid of killer products. Meager profits and the prospect of similar future earnings impressed upon LO subjects the necessity of cooperation. Because innovation was infrequent in LO, it was not as disruptive to coordination as in HI-R or SHI-LA.

Importantly, the data provide scant evidence that collusive success affected innovation decisions. Rather, they indicate that successful Market stage collusion did not feed back and greatly affect Innovation stage expenditure. The observed difference in innovation across treatments stemmed from the exogenous difference in the likelihood of innovation and not from any endogenous changes in subject innovation expenditure because of market outcomes.

If the likelihood of product innovation affects price collusion, this helps explain why price collusion appears endemic in many markets. Firms that cannot escape competition through product innovation may turn to conspiracy as an alternative avenue to supra-competitive profit. Because these firms cannot innovate their way to higher profit, they return time and again to price manipulation. Instead of merry trade meetings turning to conspiracy, in LO markets the scene may be better set by Shakespeare than Smith: "O mischief, thou art swift to enter in the thoughts of desperate men!"

APPENDIX: ADDITIONAL TABLES AND FIGURES

Table 11: Collusion Sample

Citation	SIC Code	Industry	R&D Intensity
61,368	3273	Ready-mix concrete	n/a
62,519	3273	Ready-mix concrete	n/a
63,658	3273	Ready-mix concrete	n/a
63,659	3273	Ready-mix concrete	n/a
75,060	3271	Concrete blocks	0.000
63,424	3272	Precast concrete products	0.000
63,091	2026	Dairy products	0.001
63,198	2026	Dairy products	0.001
63,370	2026	Dairy products	0.001
64,503	2026	Dairy products	0.001
64,555	2026	Fluid milk	0.001
63,180	2011	Meat packing	0.001
62,235	2062	Refined sugar	0.001
74,657	3442	Garage doors	0.002
75,197	2051	Bread	0.002
61,664	2051	Bread products	0.002
62,215	2051	Bakery products	0.002
62,217	2051	Bakery products	0.002
65,724	2051	Pastries	0.002
63,586	2951	Asphalt and concrete sales	0.002
62,916	3353	Aluminum roll jacketing	0.003
62,702	3449	Reinforcing steel bars	0.003
64,823	2076	Coconut oil	0.003
74,929	2077	Rendering	0.003
63,090	3449	Reinforcing steel bars	0.003
63,475	3356	Titanium mill products	0.004
62,992	2657	Folding cartons	0.005
61,739	2499	Toilet seats	0.005
64,222	3,452	Standard screws	0.006
63,000	3496	Swine confinement systems	0.006
63,181	2673	Consumer bags	0.007
75,245	2096	Snack foods	0.007
63,643	2041	Blended foods	0.007
63,227	2048	Livestock feed	0.008
62,517	3494	Furnace pipe and fittings	0.010
63,092	3643	Wiring devices	0.013
74,945	2298	Nylon twine	0.013
60,615	2672	Paper labels	0.015
63,205	2672	Pressure sensitive tape	0.015
60,785	3965	Zipper sliders	0.016
63,609	3639	Water heaters	0.016
60,846	3089	Drainage or plastic pipe fittings	0.017
63,215	3613	Fuse products	0.018

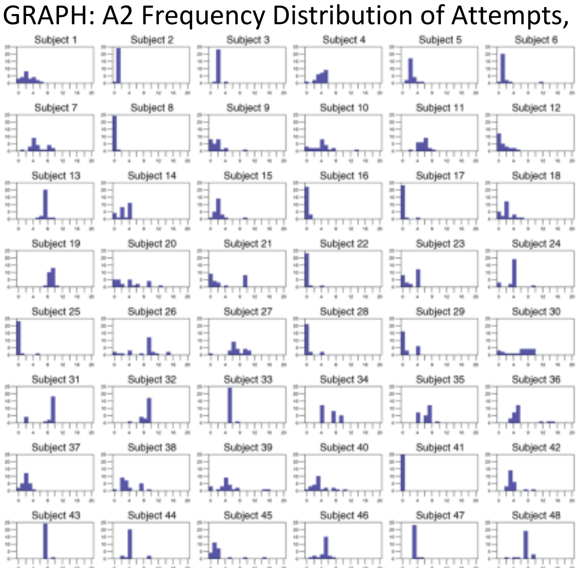
61,447	2865	Dyes	0.020
63,844	2869	Dimethyl sulfoxide	0.020
63,784	3541	Metal-working machinery	0.024
65,742	3952	Art materials	0.024
62,901	2821	Persulfate	0.025
63,610	2821	Coatings resins	0.025
63,622	3824	Gas meters	0.043

15 *Notes*: Horizontal price collusion in manufacturing industries, 1972–1982. Citations from Commerce Clearing House *Trade Cases* books. R&D intensity calculated from LOB data.

GRAPH: A1 Frequency Distribution of Attempts, by LO-R Subject

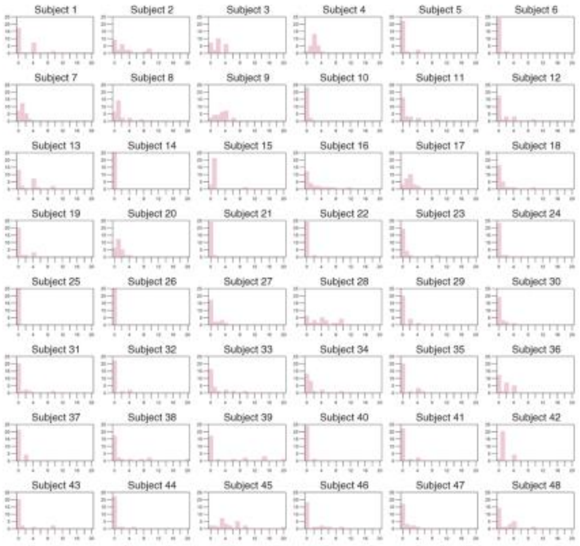
.

GRAPH: A2 Frequency Distribution of Attempts, by HI-R Subject

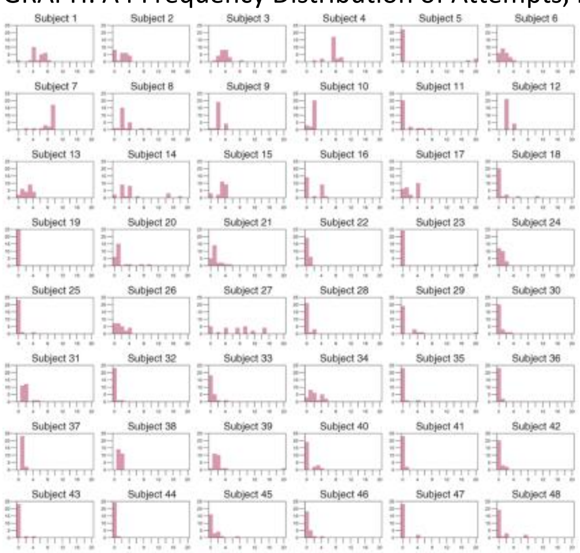


.

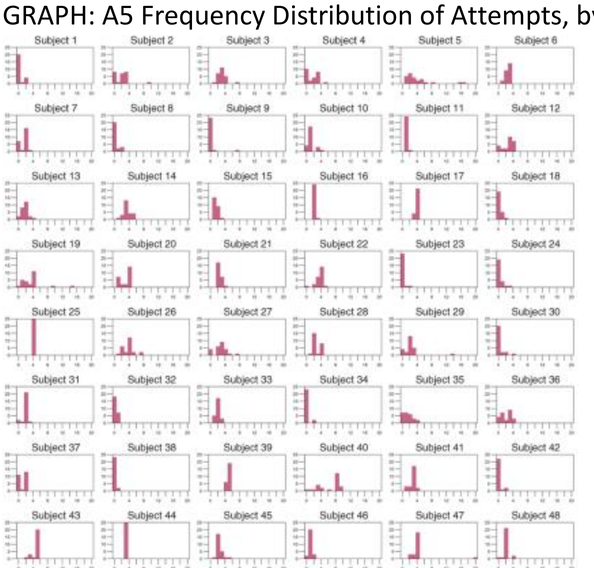
GRAPH: A3 Frequency Distribution of Attempts, by LO-LA Subject



GRAPH: A4 Frequency Distribution of Attempts, by HI-LA Subject



GRAPH: A5 Frequency Distribution of Attempts, by SHI-LA Subject



GRAPH: Appendix S1. Full instructions as they appeared to subjects in the LO [HI] treatment at the research school

Footnotes

1 Quoted in Sonnenfeld and Lawrence ([32]).

2 See Hay and Kelley ([20]), Asch and Seneca ([2]), Fraas and Greer ([17]), Scherer and Ross ([30]), Dick ([13]), Symeonidis ([33]), and Levenstein and Suslow ([25]).

3 The full links between product innovation, product differentiation, and price collusion have also received little attention in the theoretical price fixing literature. For example, in a general model examining "product differentiation-collusion sustainability," Colombo ([11]) treats product differentiation as exogenous.

4 On the use of LOB data, see Scherer et al. ([29]) and Ravenscraft and Wagner ([27]).

5 This assumes that R&D intensity in the ready-mix concrete industry is below average—a safe assumption. Of the 220 industries in the LOB data for which R&D intensity can be calculated, 140 (64%) have below average R&D intensity.

6 Note that these are Probit coefficient estimates and not marginal effects. Because the LOB report cautions: "Special care is necessary when the specialization ratio or the coverage ratio is relatively low," the estimating sample for both specifications is restricted to only include industries with coverage and specialization ratios above the respective ratio's sample mean minus two standard errors.

7 These were obtained for 1977 from Weiss and Pascoe ([34]).

8 Correlations among the regressors and variance inflation factors (VIFs) are all low, but the condition number is high (39.2).

9 Also, SIC industries are not antitrust markets; they are generally much broader in scope than antitrust markets (Werden [35]). An example specific to this sample is a price conspiracy involving three gas meter manufacturers. The relevant SIC industry includes not only gas meters, but also odometers, parking meters, pedometers, production counters, speedometers, tachometers, taxi meters, and many other products.

10 Unfortunately, firm-level data have their own issues. In particular, R&D expenditure data are generally only available for public firms. Moreover, such data are rarely available at the line of business level (where antitrust violations occur). For example, DuPont participated in an automotive refinishing paint price conspiracy in the early 1990s. While aggregate R&D data are easily obtained for DuPont, disaggregated R&D data are not readily available for DuPont's automotive paint LOB.

11 This is not a design where firms cooperate on R&D, and perhaps subsequently engage in price collusion. See Potters and Suetens ([26]) for a survey of experimental work in this domain.

12 This design is geared towards examining the relationship between innovation and conduct, as opposed to innovation and (market) structure. For experiments on the latter, see Darai, Sacco, and Schmutzler ([12]), Sacco and Schmutzler ([28]), and Aghion et al. ([1]).

13 It may be helpful to picture this experimental environment in the following way: four similar firms can engage in product innovation over many years. Developing a new product gives a particular firm temporary market power, but new innovations can be quickly copied by the other firms. Firms that do not develop a new product must compete on price with any other non-innovating firms. As reported in Section C, the data indicate that noninnovators can successfully collude when the duration of market power is just one period. If the duration of market power were more than one period, collusion among unsuccessful subjects might be even more successful.

14 An alternative experimental design where each subject's market type is imposed exogenously would have certain advantages over the design employed, namely, no possibility of market competition affecting

innovation. However, I examine the possibility that market outcomes affect innovation in "Did Collusive Success Affect Innovation?" section and find no evidence that they do.

15 This design element was induced to potentially speed up the experiment, but in practice the time limit was never binding for the vast majority of subjects.

16 For example, Holt and Davis ([21]) report that price announcements increase prices in posted-price markets (at least temporarily), Huck, Müller, and Normann ([22]) show that fixed matching increases collusion, and Fonseca and Normann ([16]) demonstrate that communication increases collusion in Bertrand oligopolies.

17 These are not equilibria levels of innovation.

18 In the two-seller case, either firm would prefer to charge a monopoly price (above 8.25) relative to a residual inverse demand curve, so 8.25 is not an equilibrium price.

19 Either duopolist can be assured $1.22 from selling 2 units at $8.76. This is the upper bound on the price support for the equilibrium mixing distribution. It follows from $4\bar{p}-38.15-8.25=1.22$ that the lower bound is $8.48. The equilibrium cumulative distribution for price is $F(p) = (33.92 - 4p)/(16.40 - 2p)$. The median price of $8.57 is calculated by setting $F(p) = 0.5$. To determine the mean price, $F(p)$ is calculated for all incremental prices of $0.001 on [8.480, 8.760]. The probability of any one incremental price being chosen is estimated numerically. Finally, the mean price of $8.59 is calculated by summing all incremental prices multiplied by their associated probabilities.

20 The result that innovation expenditure is inversely related to the probability of innovation success follows from the fact that market profits and the marginal cost of innovation are assumed to be identical across treatments. This is crucial for focusing on the effect of the likelihood of innovation success on collusion, but if market profit and the probability of success are not orthogonal, then it may be true that more innovation should be attempted in markets with a higher likelihood of innovation success relative to markets with a lower likelihood of innovation success.

21 Because the experiments had known, finite time horizons, a Folk Theorem result with a supra-competitive price equilibrium in the Market stage is not strictly applicable. But experiments have shown that subjects can be cooperative in finite-horizon games (see, e.g., Huck, Normann, and Oechssler [23]). If supra-competitive pricing is observed in the Standard markets it will be precisely because subjects are "cooperative."

22 All the LA data were collected after the collection of all of the R data. Subject behavior in the experiments need not be identical across the two schools. What is important is that any treatment differences—if they exist—are robust across the two subject populations.

23 Per the laboratory rules at the two schools, subjects received US$10.00 at the research school and US$7.00 at the liberal arts school for arriving at the computer lab on time. To equalize the average total payments across subject populations, the exchange rate between dollars and experimental currency was US$0.30 for $1.00 for the research school sessions and US$0.50 for $1.00 for the subjects at the liberal arts school.

24 Under-investment is also observed in similar experimental environments in Isaac and Reynolds ([24]) and Smyth ([31]).

25 A market-period is the observation of a particular market type in a particular period. The number of market-periods in any given period ranged from 1 (zero subjects successfully innovated) to 4 (all subjects successfully innovated). Thus, the number of market-periods is not identical to the number markets × periods. During one of the sessions, an error was detected in the software code. This glitch affected two market-periods in the LO-LA treatment. These market-periods are dropped from the analysis.

26 Full chat transcripts are available from the author.

27 See Bigoni et al. ([5]) and the references therein, and Block and Gerety ([6]).

28 Robust rank order tests are used because in three of the four cases, a null hypothesis of equal variance is rejected.

29 A robust rank order test indicates that the mean of the distribution of average market prices for LO-R is greater than the corresponding mean for HI-R (U= 2.13, p= 0.033, two-tailed). The same is true for the equivalent LO-LA and SHI-LA comparison (U= 4.76, p< 0.001, two-tailed).

30 Standard errors in parenthesis. The coefficient estimate on experience is still statistically, significantly different from zero (p< .001) when an indicator variable for subject population is added to specification (3). The coefficient estimate for this indicator is not significantly different from zero (p= 0.297).

31 This calculus does ignore the costs savings from foregoing all innovation attempts.

* I am grateful to the Michael J. Piette Fellowship and to the Economic Science Institute for funding. I thank Mark Isaac, Gary Fournier, Cortney Rodet, Bart Wilson, and seminar participants at Florida State, Chapman, Marquette, Massachusetts Amherst, and the London Experimental Workshop for helpful comments. I also thank two anonymous referees and Anthony Kwasnica for comments that have improved the paper. Naturally, any errors are my own.

REFERENCES

Aghion, P., S. Bechtold, L. Cassar, and H. Herz. " The Causal Effects of Competition on Innovation: Experimental Evidence." Journal of Law, Economics, and Organization, 34 (2), 2018, 162 – 95.

Asch, P., and J. Seneca. " Characteristics of Collusive Firms." Journal of Industrial Economics, 23 (3), 1975, 223 – 37.

Asch, P., and J. Seneca. " Is Collusion Profitable? " Review of Economics and Statistics, 68, 1976, 1 – 12.

Barreda-Tarrazona, I., A. García-Gallego, N. Georgantzís, J. Andaluz-Funcia, and A. Gil-Sanz. " An Experiment on Spatial Competition with Endogenous Pricing." International Journal of Industrial Organization, 29 (1), 2011, 74 – 83.

Bigoni, M., S.-O. Fridolfsson, C. Le Coq, and G. Spagnolo. " Trust, Leniency, and Deterrence." Journal of Law, Economics, and Organization, 31 (4), 2015, 663 – 89.

Block, M., and V. Gerety. "Deterring Collusion: Some Experimental Evidence on the Relative Effectiveness of Changes in Detection and Sanction Levels." Unpublished Manuscript, 1987.

Brown-Kruse, J., M. Cronshaw, and D. Schenk. " Theory and Experiments on Spatial Competition." **Economic Inquiry**, 31 (1), 1993, 139 – 65.

Brown-Kruse, J., and D. Schenk. " Location, Cooperation and Communication: An Experimental Examination." International Journal of Industrial Organization, 18 (1), 2000, 59 – 80.

Chamberlin, E. The Theory of Monopolistic Competition. Cambridge, MA : Harvard University Press, 1962.

Collins, R., and K. Sherstyuk. " Spatial Competition with Three Firms: An Experimental Study." **Economic Inquiry**, 38 (1), 2000, 73 – 94.

Colombo, S. " Product Differentiation and Collusion Sustainability When Collusion Is Costly." Marketing Science, 32 (4), 2013, 669 – 74.

Darai, D., D. Sacco, and A. Schmutzler. " Competition and Innovation: An Experimental Investigation." Experimental Economics, 13 (4), 2010, 439 – 60.

Dick, A. " Identifying Contracts, Combinations and Conspiracies in Restraint of Trade." Managerial and Decision Economics, 17 (2), 1996, 203 – 16.

Erickson, B. " Price Fixing Conspiracies: Their Long-Term Impact." Journal of Industrial Economics, 24 (3), 1976, 189 – 202.

Fischbacher, U. " z-Tree: Zurich Toolbox for Ready-Made Economic Experiments." Experimental Economics, 10 (2), 2007, 171 – 78.

Fonseca, M., and H.-T. Normann. " Explicit vs. Tacit Collusion—The Impact of Communication in Oligopoly Experiments." European Economic Review, 56 (8), 2012, 1759 – 72.
Fraas, A., and D. Greer. " Market Structure and Price Collusion: An Empirical Analysis." Journal of Industrial Economics, 1977, 21 – 44.
García-Gallego, A., and N. Georgantzís. " Multiproduct Activity in an Experimental Differentiated Oligopoly." International Journal of Industrial Organization, 19 (3), 2001, 493 – 518.
Greiner, B. " Subject Pool Recruitment Procedures: Organizing Experiments with ORSEE." Journal of the Economic Science Association, 1 (1), 2015, 114 – 25.
Hay, G., and D. Kelley. " An Empirical Survey of Price Fixing Conspiracies." Journal of Law & Economics, 17 (1), 1974, 13 – 38.
Holt, C., and D. Davis. " The Effects of Non-binding Price Announcements on Posted-Offer Markets." Economics Letters, 34 (4), 1990, 307 – 10.
Huck, S., W. Müller, and H.-T. Normann. " Stackelberg Beats Cournot—On Collusion and Efficiency in Experimental Markets." The Economic Journal, 111 (474), 2001, 749 – 65.
Huck, S., H.-T. Normann, and J. Oechssler. " Two Are Few and Four Are Many: Number Effects in Experimental Oligopolies." Journal of Economic Behavior & Organization, 53 (4), 2004, 435 – 46.
Isaac, M., and S. Reynolds. " Schumpeterian Competition in Experimental Markets." Journal of Economic Behavior & Organization, 17 (1), 1992, 59 – 100.
Levenstein, M., and V. Suslow. " What Determines Cartel Success? " Journal of Economic Literature, 44 (1), 2006, 43 – 95.
Potters, J., and S. Suetens. " Oligopoly Experiments in the Current Millennium." Journal of Economic Surveys, 27 (3), 2013, 439 – 60.
Ravenscraft, D., and C. Wagner. " The Role of the FTC's Line of Business Data in Testing and Expanding the Theory of the Firm." Journal of Law & Economics, 34 (2, Part 2), 1991, 703 – 39.
Sacco, D., and A. Schmutzler. " Is There a U-Shaped Relation between Competition and Investment? " International Journal of Industrial Organization, 29 (1), 2011, 65 – 73.
Scherer, F., W. Long, S. Martin, D. Mueller, G. Pascoe, D. Ravenschaft, J. Scott, and L. Weiss. " The Validity of Studies with Line of Business Data: Comment." American Economic Review, 1987, 205 – 17.
Scherer, F., and D. Ross. Industrial Market Structure and Market Performance. 3rd ed. Boston : Houghton Mifflin, 1990.
Smyth, A. " Competition, Cost Innovation, and X-Inefficiency in Experimental Markets." Review of Industrial Organization, 48 (3), 2016, 307 – 31.
Sonnenfeld, J., and P. Lawrence. " Why Do Companies Succumb to Price Fixing? " Harvard Business Review, 56 (4), 1978, 145 – 57.
Symeonidis, G. " In Which Industries Is Collusion More Likely? Evidence from the UK." Journal of Industrial Economics, 5 (1), 2003, 45 – 74.
Weiss, L., and G. Pascoe. Adjusted Concentration Ratios in Manufacturing, 1972 and 1977. Washington, DC : U.S. Federal Trade Commission, 1986.
Werden, G. " The Divergence of SIC Industries from Antitrust Markets: Some Evidence from Price Fixing Cases." Economics Letters, 28 (2), 1988, 193 – 97.