

Whinston, Michael D.

**Working Paper**

## Lectures on Antitrust Economics, Chapter 2: Price Fixing

CSIO Working Paper, No. 0040

**Provided in Cooperation with:**

Department of Economics - Center for the Study of Industrial Organization (CSIO), Northwestern University

*Suggested Citation:* Whinston, Michael D. (2003) : Lectures on Antitrust Economics, Chapter 2: Price Fixing, CSIO Working Paper, No. 0040, Northwestern University, Center for the Study of Industrial Organization (CSIO), Evanston, IL

This Version is available at:

<https://hdl.handle.net/10419/23469>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

THE CENTER FOR THE STUDY  
OF INDUSTRIAL ORGANIZATION  
AT NORTHWESTERN UNIVERSITY

Working Paper #0040

Lectures on Antitrust Economics  
Chapter 2: Price Fixing<sup>\*</sup>

By

Michael D. Whinston  
Northwestern University and NBER

---

<sup>\*</sup> Copyright 2003, Michael D. Whinston.

# Lectures on Antitrust Economics

## Chapter 2: Price Fixing

Michael D. Whinston\*

Draft — Comments Welcome

### Contents

<b>1</b>	<b>Introduction</b>	<b>2</b>
<b>2</b>	<b>Price Fixing and the per se Rule</b>	<b>2</b>
<b>3</b>	<b>Effects of the Ban on Price Fixing</b>	<b>7</b>
3.1	The Theory of Price Fixing . . . . .	7
3.2	Evidence on Price Fixing's Effects . . . . .	14
<b>4</b>	<b>Detecting Price Fixing</b>	<b>27</b>
4.1	Structural Evidence . . . . .	28
4.2	Behavioral Evidence . . . . .	33
<b>5</b>	<b>Afterword: Antitrust Policy Toward Tacit Collusion</b>	<b>41</b>

\*Northwestern University and NBER. Copyright 2003, Michael D. Whinston.

## 1 Introduction

In this chapter, we begin our discussion of antitrust economics by considering what many consider its most central element: its ban on “price fixing” — that is, agreements among competitors over the prices they will charge or the outputs they will produce.<sup>1</sup> Indeed, the prohibition on price fixing is one area of antitrust law that even those generally skeptical of governmental competition policy typically regard approvingly. Nevertheless, despite its current uncontroversial status, we shall see that some significant and challenging open questions remain in this area.

## 2 Price Fixing and the *per se* Rule

A short summary of United States law in this area always reads “price-fixing is *per se* illegal”. By that, what is meant is that if a firm engages in “price fixing” — say, by meeting with its competitor at the *Golden Fleece Motel* and agreeing on the prices they will charge — it will be found guilty without any inquiry into the potential anticompetitive effects, or procompetitive benefits, of the agreement. This *per se* rule contrasts with the *Rule of Reason* approach adopted in most other areas of antitrust, in which these benefits and costs are said to be weighed explicitly.<sup>2</sup>

<sup>1</sup>Price fixing therefore includes market division and bid rigging schemes, which can both be viewed as a form of price and/or output agreement. It can also include related types of agreements, such as a limitation on hours of operation (say, for retail stores).

<sup>2</sup>The clearest enunciation of the *per se* rule is probably found in Justice Douglas’ opinion in *Socony-Vacuum Oil* [310 U.S. 150 (1940)], “price-fixing combinations are illegal *per se*; they are not evaluated in terms of their purpose, aim, or effects in the elimination

This seemingly straightforward rule masks, however, a more complex reality. This complexity is both legal and economic in nature. On the economic side lies the fact that nearly every price-fixer has a reason why their particular price-fixing scheme is in fact good for society (or at least it seems so at times). For an example of such human ingenuity, one need not look beyond the very first antitrust case to come before the Supreme Court after the passage of the Sherman Act. In 1897 the Court was faced with the *Trans-Missouri* case [166 U.S. 290 (1897)], in which 18 railroads west of the Mississippi River had formed an association to set railroad rates. In the lower courts the railroads had argued that their agreement was not illegal because their rates were reasonable and, absent the agreement, ruinous competition would ultimately lead to monopoly, and consequently to higher prices.

Can this ruinous competition argument be dismissed as being simply illogical and preposterous? Like many proposed justifications for price-fixing arrangements, the answer is in fact “no.” The railroad industry is one of high fixed costs, and an oligopolistic structure. It is well-understood by now that the number of firms that unfettered competition can support in a market need not be efficient in such cases (see, for example, Mankiw and Whinston [1986]). The *Trans-Missouri Freight Association’s* ruinous competition argument can be viewed as saying exactly this: that unrestricted oligopolistic competition would lead to too few firms (namely, one firm) relative to what is socially efficient. In such cases, it is possible that an inducement to entry in the form of cartelized prices could actually raise social welfare.

To see a simple example, suppose we have an industry with demand of competitive evils.”

function  $x(p) = 1 - X$ , marginal costs of zero, and an entry cost of  $1/16$ . If entry of a second firm would result in Bertrand competition (and hence a price of zero), then only one firm will enter. In this case, price is  $1/2$ , the monopoly profit is  $1/4 - 1/16 = 3/16$ , and consumers enjoy a surplus of  $1/8$ . Hence, aggregate welfare equals  $5/16$ . Suppose instead that duopolists are allowed to talk, and that this allows duopolists to raise price to  $1/4$  (i.e., they cannot sustain the full monopoly price). If so, a duopolist's gross (before entry costs) profit will instead be  $3/32$  and so a second firm will enter. In this case, price will be  $1/4$  instead of  $1/2$  which increases consumer welfare to  $9/32$ , and aggregate profits will be  $3/16 - 2/16 = 1/16$ . Thus, aggregate welfare will rise from  $5/16$  to  $11/32$ . Thus, both consumers and society as a whole would be better off.<sup>3</sup>

If valid arguments for price-fixing conspiracies are possible, why would a sound competition policy not consider these possible benefits? The answer is that while possible, they appear improbable, and a sound policy must also consider the costs of administration. If nearly every firm caught engaging in

<sup>3</sup>Theory does tell us, however, that at least in the case of homogeneous products we should typically expect *too much* entry from the perspective of aggregate welfare in the absence of a conspiracy (for a precise statement of these conditions, see Mankiw and Whinston [1986]). In such cases, allowing price fixing would worsen this problem. Moreover, as long as free entry holds profits to exactly zero, any such reduction in aggregate surplus must imply as well a reduction in consumer surplus. Matters become more complicated, however, once product differentiation is introduced, since consumers may benefit directly from the introduction of additional products.

Fershtman and Pakes [2000] provide a related result in a computational analysis of a dynamic model. There, allowing more effective price collusion leads to greater product variety (i.e., more entry) and greater quality (due to a greater incentive to invest to capture market share), which in their simulations offsets any negative effect on consumer welfare from higher prices. However, in their simulations industry profits are lower with collusion, by an amount that is greater than the gain in consumer surplus, and so aggregate welfare nonetheless falls when more effective collusion is allowed.

price fixing can come up with some theoretical argument that its price fixing is socially beneficial, and if actually measuring the social benefits and costs of a particular price-fixing conspiracy is very difficult (as it certainly is), price-fixing cases will be extended and costly affairs indeed (good for economists and lawyers, but bad for everyone else). Moreover, if our sense is that in most cases we will reject such claims because socially beneficial price-fixing conspiracies are rare, then it makes sense to refuse to listen to and evaluate these claims, despite their theoretical possibility - that is, to have a *per se* rule. As George Stigler [1952] noted early in his career, “economic policy must be contrived with a view to the typical rather than the exceptional, just as all other policies are contrived. That some drivers can safely proceed at eighty miles per hour is no objection to a maximum-speed law.”

This justification of the *per se* rule is really nothing more than an application of optimal statistical decision-making. The importance of administrative costs for the design of optimal antitrust policy has not, I think, been adequately recognized in either the economic or legal literatures. On the economics side, it is common for a journal article that shows that a particular practice may either raise or lower welfare to conclude that this implies that the practice should be accorded a Rule of Reason standard. As the foregoing discussion suggests, such a conclusion makes little sense. On the legal side, there appears to be surprisingly little formal application of the theory of optimal statistical decision-making to the issue of optimal legal rules.<sup>4</sup>

<sup>4</sup>For one exception, see Beckner and Salop [1999]. As one application of optimal statistical decision making, for example, we would expect optimal antitrust policies in countries with less well-developed legal systems (i.e., higher costs of judicial administration) to involve greater use of *per se* rules. As another application, we would expect that as econo-

This underlying economic complexity engenders a legal complexity as well. The legal complexity arises whenever the courts are called upon to decide whether a novel set of facts should in fact be called “price fixing”. Although this categorization process has often seemed to take on a particularly semantic nature (as in, do the words “price fixing” describe this behavior?), the real issue is of course whether the practice seems to be one for which a *per se* approach seems appropriate.<sup>5</sup> But for this, at least some quick look at the underlying economic facts is necessary. In this regard, the *per se* rule is perhaps best thought of as a very fast Rule of Reason analysis, in which the court first takes a quick look to see whether further analysis is appropriate. Although the courts struggled with this issue for a long time, they have increasingly adopted this view. For example, in *Broadcast Music* [441 U.S. 1 (1979)], two cooperative organizations of copyright holders (BMI and ASCAP) each issued blanket licenses that gave a user the right to play any of that cooperative’s music in unlimited amounts. The economic benefit of such a license is that monitoring of use is unnecessary when someone purchases a blanket license. Here the Court noted that “easy labels do not always supply ready answers”: the blanket license involves price fixing in the literal sense [but] “[a]s generally used in the antitrust field, ‘price fixing’ is a short-hand

mists become better at providing evidence of particular price-fixing conspiracies’ effects, optimal policy would shift toward less reliance on a *per se* rule.

<sup>5</sup>A particularly amusing example of the extremes to which a purely semantic approach can take the Court can be found in Justice Blackmun’s concurrence in the *Topco* case [405 U.S. 596 (1972)] involving a cooperative of small to medium sized groceries’ development of a private label. Blackmun laments, “Today’s ruling will tend to stultify competition. The *per se* rule, however, now appears to be so firmly established by the Court that, at this late date, I could not oppose it. Relief, if any is to be forthcoming, apparently must be by way of legislation.”



way of describing certain categories of business behavior to which the *per se* rule has been held applicable.”

### **3 Effects of the Ban on Price Fixing**

#### **3.1 The Theory of Price Fixing**

The Sherman Act’s ban on price fixing helps prevent anticompetitive collusive pricing in two ways. The first, although rarely explicitly discussed, is critical: it makes any formal contract among competitors regarding the prices they will charge unenforceable.<sup>6</sup> The second is more commonly acknowledged: the Sherman Act prohibits firms from talking and reaching an “agreement” about prices, outputs, or market division.<sup>7</sup> What is not so recognized is how little formal economic theory tells us about the manner in which this second prohibition prevents anticompetitive pricing and improves welfare.

A first problem, of course, concerns the law’s focus on “agreement,” whose meaning can be difficult to pin down. For example, one can imagine a scenario in which two firms sit down at a table with each declaring, in sequence, “I am morally opposed to price-fixing, but tomorrow I will be setting my price equal to 100.” Should such unilateral speech be treated differently than if they instead each said “I’ll set my price equal to 100 if you do”? And does that differ from the situation in which firm 1 says “Let’s set our prices equal to 100 tomorrow”, and firm 2 replies “I agree”? Perhaps there is a

<sup>6</sup>This is, in fact, somewhat inaccurate, since the courts typically did not enforce naked price-fixing agreements even before passage of the Sherman Act. (This is why, for example, the JEC cartel described in Porter [1983] had to revert to price wars rather than courts to enforce their agreement.)

<sup>7</sup>The Sherman Act also prohibits other activities that may aid firms in colluding such as the exchange of side payments and certain types of information.

difference (certainly the law often believes there is, not only in reference to the Sherman Act, but also in areas such as contract law), but economists have essentially nothing to say about this. With this first problem granted, what does economics have to say about the effects of the act of talking itself?

Modern economic theory tells us that oligopolists who seek to come to an agreement to sustain high prices but who cannot sign binding agreements (note here the effect of the first critical role of the Sherman Act) face two principal problems: an *incentive problem* and a *coordination problem*. The incentive problem can be formally stated as follows: To be credible, any agreement must be a subgame perfect Nash equilibrium. If it was not, then some party to the agreement would find it profitable to cheat. But note that this is exactly the same condition that economic theory uses to identify the set of outcomes that are sustainable without any direct communication, that is, through “*tacit*” collusion. So if the Sherman Act’s prohibition on talking helps prevent high prices, it must be because it worsens the oligopolists’ coordination problem. The coordination problem arises because typically there are *many* possible subgame perfect Nash equilibrium outcomes. One of these is always the purely non-cooperative (i.e., static) outcome: if a firm expects all other firms to be fully non-cooperative, it will be optimal for that firm to be so as well. Frequently, however, a range of more cooperative outcomes is possible, including in some cases the joint monopoly solution.

Notably, however, economic theory has relatively little to say about the process of coordination among equilibria. It is natural to think that talking may help with this coordination, but exactly to what degree, and in what

		Player 2	
		L	R
Player 1	U	8,8	-10,7
	D	7,-10	1,1

Figure 1:

circumstances is less clear.

The most relevant work in economic theory concerning this coordination issue is the literature on “cheap talk” about intended play in games.<sup>8</sup> “Cheap talk” is speech that has no direct payoff consequences, but that may nonetheless be informative because should those that hear it believe it and respond to it in favorable ways, those who speak will have incentives to speak informatively. As this suggests, one possible outcome is that cheap talk is regarded by everyone as meaningless (this is the so-called “babbling equilibrium”). What the literature on cheap talk about intended play has struggled with is the question of the exact circumstances in which we should expect it to be meaningful. Consider, for example, the two player game depicted below in Figure 2.1.

<sup>8</sup>For a good introduction to this literature, see Farrell and Rabin [1996]. The literature on cheap talk originated with Crawford and Sobel [1982].

Here player 1 chooses U or D, while player 2 chooses L or R. There are two Nash equilibria: (U,L) and (D,R). The former is better for both players than the latter. However, for player 1, choosing U is very risky: unless he is very confident that player 2 will play L, player 1 should play D. Similarly, L is very risky for player 2. Suppose that before the game is played player 1 can say either “I will play U”, “I will play D”, or can remain silent. One view is that we should expect the players to successfully coordinate on (U,L) here because “I will play U” is a message that, if believed by player 2, creates an incentive for player 1 to act as he claims. Hence, one could argue, player 2 will believe such a claim. On the other hand, observe that player 1 would like player 2 to play L *regardless* of what he intends to play. This fact leads some game theorists to argue that in the above game player 2 might not believe player 1’s claim that he will play U. Oligopoly settings are similar to this situation since a firm will want always to convince its rival to behave cooperatively (in its price or output choice) regardless of its own actual intentions. The main difference, and complication, is that firms may wish to communicate about their intended dynamic strategies, rather than about simple actions.<sup>9</sup>

There has been some experimental work aimed at examining when cheap talk about intentions matters for play in games. Much of this work has concerned play of static coordination games, although some has considered re-

<sup>9</sup>A further complication in predicting the effect of communication about intended play on repeated oligopoly outcomes is that the ability to coordinate on desirable outcomes may actually undercut a cartel’s ability to maintain high prices by reducing the likelihood of punishments following a deviation (see McCutcheon [1997]). Interestingly, Genesove and Mullin [2001] note that episodes of cheating in the Sugar Institute cartel of 1927-1936 (which engaged in extensive communication) were rarely met with retaliation unless they were gross violations of the cartel’s agreement.

peated oligopoly games. The results appear mixed. In some cases cheap talk matters a lot and leads to significant coordination by the players. In other cases, it appears to make little difference. Also, the type of communication that is most useful varies across games – sometimes 1-sided communication is better than 2-sided; sometimes the reverse. Likewise, it can matter whether communication is unregulated, or tightly structured. Holt [1993] and Crawford [1998] survey this work.<sup>10</sup> Unfortunately, there does not yet appear to be a consensus in the experimental literature about the exact circumstances and manner in which cheap talk about intended play matters.

While relatively little is known about how cheap talk about intentions affects oligopolistic coordination, the economic theory literature has had more to say about a different role for cheap talk: communication about private information. The literature has studied extensively the problem faced by a cartel whose members' costs may differ at any given time. A profit-maximizing cartel wants to allocate sales to the firm whose costs are currently lowest, and may also want to make its current price depend upon this firm's cost level. But because a firm's costs will often be precisely known only to that firm, the cartel must devise some way to uncover this information by creating incentives for firms to reveal it truthfully.

This problem was initially studied in a series of papers using static mechanism design models (Roberts [1985], Cramton and Palfrey [1990], McAfee and McMillan [1992], and Kihlstrom and Vives [1992]). In these papers, the firms each announced their cost "type" and were assigned an output or price. It was simply assumed that firms would abide by these assigned

<sup>10</sup>Kuhn [2001, pp. 16-7] and Neven [2000, pp. 71-6] also discuss some of this work.

prices/outputs. The papers then addressed whether the cartel could achieve its full information outcome, and the form of the optimal mechanism. The papers differed in their assumptions in several respects: whether transfers were allowed, the set of possible cost types, and the nature of any individual rationality constraints imposed.

More recently, Athey and Bagwell [2001] (see also Athey, Bagwell, and Sanchirico [2002]) imbed this type of mechanism design framework in a dynamic model. One key issue in the Athey-Bagwell setting is that firms now need to have incentives not to deviate from their assigned prices/outputs, and this may affect the cartel's optimal pricing/output policies.<sup>11</sup> A second aspect of their model is that even when monetary transfers across firms are prohibited, firms have the ability to use future play as a transfer mechanism by shifting future market shares in response to firms' current efficiency claims. One notable aspect of these models of privately observed costs is that practices that aid the firms in achieving high collusive profits may also improve aggregate welfare by improving productive efficiency.

A second role for communication of private information in oligopolies arises when firms may have different information about how likely it is that some cartel members have previously cheated. Papers by Compte [1998] and Kandori and Matsushima [1998] show how firms can coordinate collective punishments for deviators using public claims about the signals they privately

<sup>11</sup>In the previous mechanism design papers it was clear that for large enough discount factors firms could be prevented from deviating from their assigned prices/outputs in the optimal mechanism. What is different then in the dynamic setting is the characterization of how the cartel adapts when the discount rate is below this level.

observe.<sup>12</sup>

While these contributions significantly increase our understanding of how talk can be used to reveal information in collusive oligopolies, the literature on communication of private information does not yet show clearly how the ability to talk changes oligopoly outcomes relative to a scenario where talk is prohibited. For example, in Athey and Bagwell [2001], firms are artificially limited in how they can split market shares in the case in which they are not allowed to talk. How talk matters absent this restriction is unclear. In Compte [1998] and Kandori and Matsushima [1998], the authors do not show what happens in the absence of communication.

It is in some sense paradoxical that the least contested area of antitrust is perhaps the one in which the basis of the policy in economic theory is weakest. Of course, most economists are not bothered by this, because they believe (as I do) that direct communication (and especially face-to-face communication) will often matter for achieving cooperation, and that any procompetitive benefits of collusion are both rare and difficult to document. Nonetheless, since most economists could be wrong, it would be good if we understood better the economics behind this belief. Indeed, as we will see in Section 4 of the chapter, such an understanding could also help better guide enforcement policy.

<sup>12</sup>In this regard it is interesting to note Genesove and Mullin's [2001] description of the extensive communication among members of the Sugar Institute cartel aimed at adjudicating claimed defections and proposing appropriate punishments.

## 3.2 Evidence on Price Fixing's Effects

If formal economic theory is surprisingly silent on the effects of the ban on price fixing, perhaps existing empirical work can offer some support for the view that preventing oligopolists from talking has a substantial affect on the price they charge? Here too, the existing state of the published literature offers surprisingly little evidence for this proposition.

Sproul [1993], for example, examines 25 of the approximately 400 cases in which individuals and/or firms were indicted for price-fixing from 1973 to 1984 (these 25 cases were the ones in which the necessary data were available). For each case, he constructs a “predicted price” based on a regression of the product’s price on related prices for the period prior to the indictment.<sup>13</sup> He then examines the ratio of the actual price to the predicted price in the period following the indictment. Figure 2.2 illustrates the average effect observed. (In the figure, the series for the 25 products are aligned so that in each case the indictment occurs in “month 100”.)

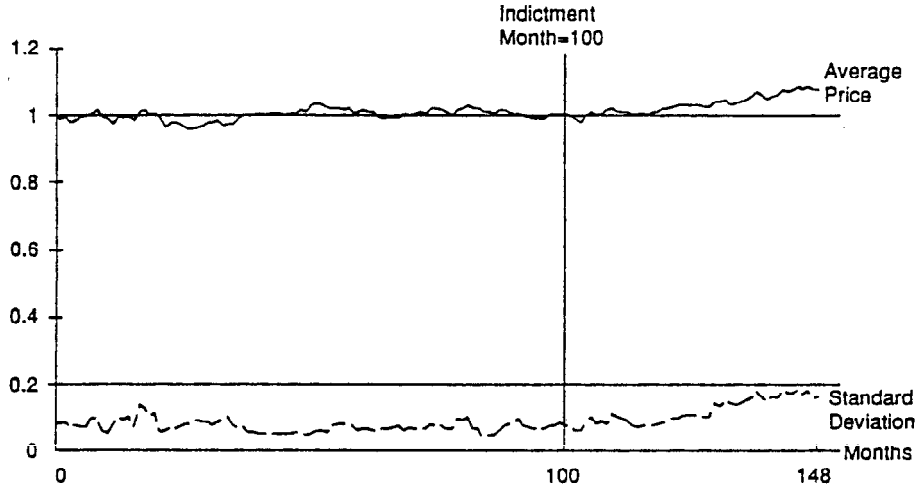
If anything, prices seem to rise (relative to the predicted price) after the indictment. Examining the price changes following other important events — the date the government believed the conspiracy to have ended, the date government penalties were imposed, or the date civil penalties were imposed — does not change this basic conclusion, as shown in Figures 2.3(a)-(c).<sup>14</sup>

<sup>13</sup>Sproul attempts to choose the related prices to have a close correlation to the price of the product in question prior to the indictment but be relatively unaffected by the indictment or other factors affecting the market for which the indictment occurred.

<sup>14</sup>In these figures the series are again aligned so that the event in question occurs in



Figure 2: Effect of Price-Fixing Indictments on Prices (Sproul [1993])



Interestingly, Sproul at the same time finds some effect on prices of more severe penalties. In Figure 2.4, for example, we see that prices rose less (and even initially fell slightly) following indictments after 1976, when the DOJ first prosecuted under the stiffer penalties passed into law in 1974, than for those before 1976. Also, Table 2.1 shows the results of a regression relating prices following the indictment to the severity of punishment, as measured by the ratio of total government-imposed fines to the sales of the indicted firms (FINES), the ratio of the total number of days of jail time served to the same numbered month in each case; Sproul chooses this month based on the average number of months between the indictment and the relevant event (e.g., on average, seven months elapsed between the indictment and the imposition of government penalties, so the series are aligned so that in each case government penalties were imposed in “month 107”). Also indicated in each figure is the number of cases used to construct the index in question; missing information means that this number is less than 25 in each figure.

Figure 3: Effects on Prices of Ending a Conspiracy, Imposing Government Penalties, and Awarding Civil Damages (Sproul [1993])

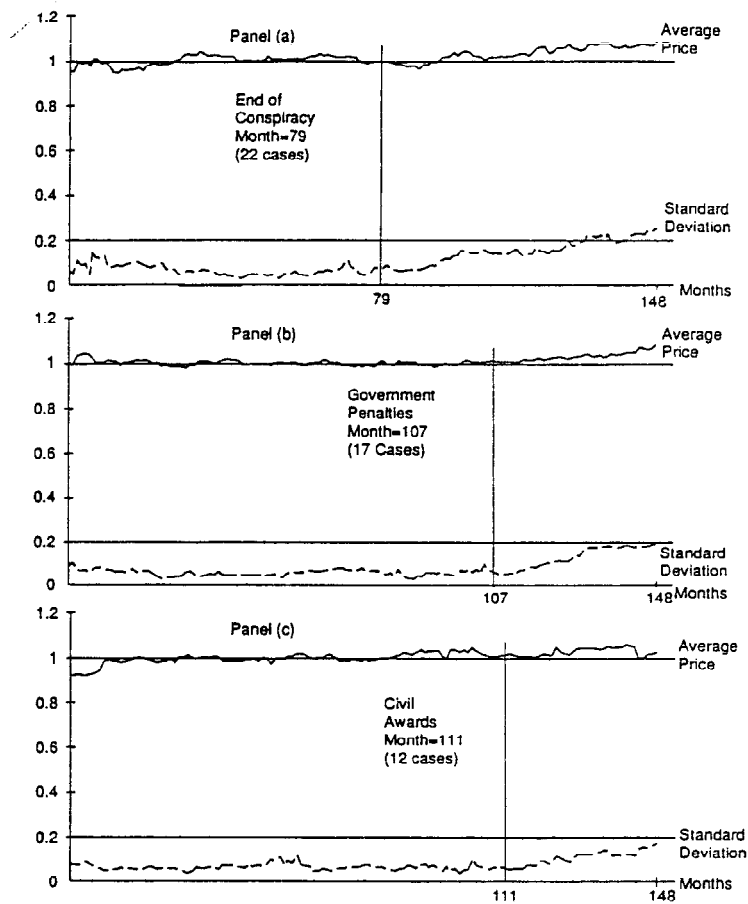
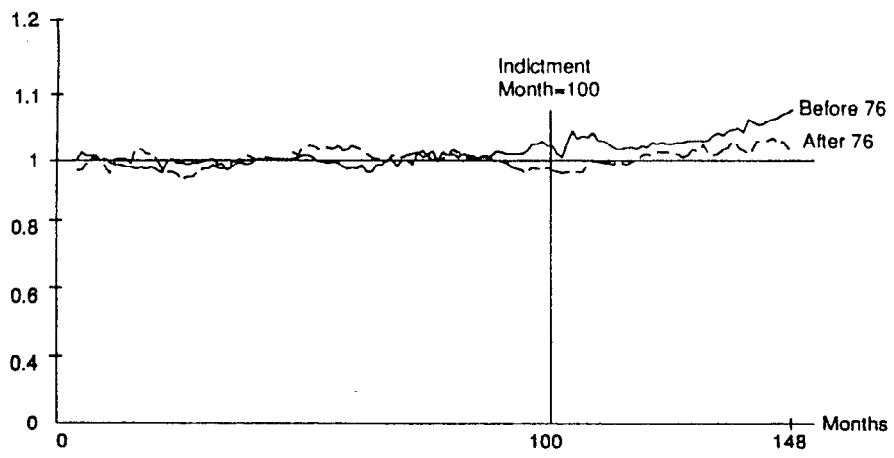


Figure 4: Average Prices for Industries Indicted Before and After 1976 (Sproul [1993])



their sales (JAIL), the ratio of the number of months of probation to their sales (PROB), and the ratio of known civil penalties to their sales (CIVIL).<sup>15</sup> The dependent value in each regression is the unweighted average of the ratio of actual to predicted prices for the months between the indictment and the month in question (i.e., for the 3 months following the indictment in the first column of the table). Larger fines are found to reduce the increase in price (relative to predicted price). Increases in jail time served do as well, but only in the initial months following the indictment. On the other hand, increases in probation, holding fixed fines and jail time, have the opposite effect, increasing prices following the indictment at least in the initial months following the indictment.

How the results on fines and jail time should be interpreted is a little unclear. If most of the variation in penalties is coming from changes in the law, or in the ease of proving a conspiracy in different industries, then these effects might arise from the elimination of price fixing after the indictment. (In terms of the model at the end of Chapter 1, the cartel may have learned that the probability of detection is higher than it thought.) But it is also possible that the causation runs the other way: when price drops just prior to or just after an indictment, this fact may be used as evidence of overcharges, and make conviction and large penalties more likely. Regardless of interpretation, however, there is certainly little in Sproul's study to suggest that a government price-fixing enforcement action leads to any significant

<sup>15</sup>Civil penalties are damages paid in privately brought cases. Often these cases are settled and the awards kept secret as a condition of settlement. Information on civil penalties was available for only 11 of the 25 cases; Sproul assumed they were zero for the other 14 cases.

reductions in price.

Table 2.1: Effects of Penalties on Average Price 3,6,12,24,and 48 Months After Indictment (Sproul [1993])

Dependent Variable: Average Price Following Indictment					
	Months Elapsed Since Indictment				
	3	6	12	24	48
CONST	1.003 (61.99)	1.015 (51.11)	1.022 (46.99)	1.026 (52.86)	1.054 (39.57)
FINES	-4.726 (-2.286)	-5.128 (-2.023)	-5.407 (-1.947)	-5.907 (-2.383)	-7.961 (-2.340)
JAIL	-.046 (-2.034)	-.044 (-1.567)	-.035 (-1.158)	-.015 (-.567)	-.006 (-.161)
PROB	.085 (2.032)	.072 (1.404)	.072 (1.247)	.070 (1.392)	.080 (1.158)
CIVIL	-.040 (-.408)	-.041 (-.343)	-.038 (-.288)	-.043 (-.368)	-.095 (-.590)
$R^2$	.130	.053	.012	.067	.071

Note — t-statistics are in parentheses.

A study that examines the issue at a much more disaggregated level is Block, Nold, and Sidak [1981] (henceforth, BNS). BNS examine prices in 16 local bread markets from 1965 to 1976. They construct what they call a “mark-up” measure for these local bread markets from the fitted values of the regression

$$p_{it} = IC_{it} + \sum_j \beta_j w_{ijt} + \varepsilon_{it}, \quad (1)$$

where  $IC_{it}$  the cost of ingredients in market  $i$  in year  $t$  (derived using a standard recipe for bread), and  $w_{ijt}$  is the cost of non-ingredient input  $j$  in market  $i$  in year  $t$ . BNS then define the “mark-up” to be

$$M_{it} = \frac{p_{it} - \hat{p}_{it}}{\hat{p}_{it}}. \quad (2)$$

(It should be noted that this variable is better thought of as the deviation from the sample average cost-adjusted price of bread than as a mark-up. For example, if all markets set the same mark-up over costs in every period, this measure would be identically zero.) BNS then regress this mark-up measure against measures of antitrust enforcement in the first-difference form

$$\Delta M_{it} = \alpha_0 \cdot \Delta Budget_t + \alpha_1 \cdot DOJREG_{it} + \alpha_2 \cdot DOJREM_{it} + u_{it}, \quad (3)$$

where  $\Delta Budget_t$  is the change in the DOJ's Antitrust Division budget in year  $t$ ,  $DOJREG_{it}$  takes the value of 1 for city  $i$  in year  $t$  if a city in the same region had a price-fixing enforcement action against the bread industry in year  $t - 1$ , and  $DOJREM_{it}$  takes the value of 1 for city  $i$  in year  $t$  if there was a price-fixing enforcement action against the bread industry in city  $i$  in year  $t - 1$ . Column 2 of Table 2.2 gives the result of this regression.<sup>16</sup>

<sup>16</sup>The regression in the first column omits  $\Delta DOJREM_{it}$ , while the third and fourth columns include measures of price changes in the food sector ( $\Delta FOODM$ ) and general manufacturing ( $\Delta GENM$ ).

Table 2.2: Estimated Effects of Changes in DOJ Enforcement on Changes in Markups in the Bread Industry (Block, Novak, and Sidak [1981])

Independent Variables				
$\Delta$ BUDGET	-.015*	-.015	-.024	-.020
	(-2.74) <sup>†</sup>	(-2.68)	(-4.06)	(-3.65)
DOJREG	-.025	-.026	-.025	-.027
	(-2.05)	(-2.21)	(-2.09)	(-2.26)
DOJREM		-.046	-.046	-.044
		(-2.32)	(-2.41)	(-2.32)
$\Delta$ FOODM			+.058	
			(2.33)	
$\Delta$ GENM				-.010
				(-1.60)
Constant	.011	.013	.014	.017
$R^2$	.055	.082	.113	.101
$F$ -statistic	5.93 (2, 205)	6.04 (3, 204)	6.47 (4, 203)	5.68 (4, 203)

Note—Each regression is based on 208 observations.

\*This coefficient is estimated in per million dollars.

<sup>†</sup>The value of the estimated coefficient divided by its estimated standard error.

Increases in the Antitrust Division budget, price-fixing enforcement actions in neighboring cities, and price-fixing enforcement actions in a given city all are found to lower prices. But the effects on price appear small. An enforcement action in a given city is found to lower the “mark-up” in the next year (and ensuing years) by 4.6%; e.g., reduce a mark-up of 25% to 20.4%. Certainly this represents a relatively small effect on price: by comparison, we shall see in Chapter 3 that the DOJ and FTC generally do not even concern themselves when price increases from horizontal mergers are predicted to be less than 5%.

How can we interpret these results that show little or no reductions in

price following a price-fixing enforcement action? One possibility is that talking doesn't matter very much because conspiracies simply may be hard to police and maintain absent an ability to have binding agreements. Another possibility is that talking doesn't matter much because firms may be able to collude very effectively even absent the ability to talk. Still a third possibility is that talking may matter a lot for increasing prices, but firms may simply ignore the risks of being caught, even after having been caught once. In any of these three cases, there may not be much of a gain from the ban on talking.

It is also possible that talking has some procompetitive price-reducing effects that fully or partially offset any tendency toward higher prices. Sproul [1993], for example, argues that many price-fixing conspiracies may be engaged in socially beneficial activities that reduce costs and, hence, prices (perhaps by allocating output more efficiently across firms, as we discussed in Section 2.1). Another possibility is McCutcheon [1997]'s suggestion that the Sherman Act's ban on talking may make collusion *easier* because it makes renegotiation of planned punishments more difficult. Certainly these last two possible explanations would be consistent with the view that the Sherman Act's ban on talking was doing more harm than good.

Yet there are several reasons why these studies could be missing some of the price-reducing effects of the ban on talking. The first is an issue with measurement: it may be that firms who have been engaged in price-fixing are able to maintain high prices for a period even after they are no longer talking.<sup>17</sup> If so, these studies may have simply missed the effect by not

<sup>17</sup>Daughety and Forsythe [1987], for example, show an effect of communication on cooperative behavior in experimental repeated Cournot games even after communication has



considering a period long enough after the conviction. A second is that cartels that talk may currently be relatively ineffective because of conspirators' fear of investigation and detection. If so, these studies may not give us a good sense of what prices would be absent any form of price-fixing enforcement. Third, if firms were ignoring the risk of detection this may be because, at least during the sample period of these studies, the penalties for price-fixing were much smaller than they are today. (In this regard, it would be interesting to see if the same patterns would be found more recently, after the very large increase in price-fixing penalties.)

Moreover, while these may be small percentage increases in price, they may represent substantially larger percentage increases in firms' profit or consumer surplus. For example, if demand is highly inelastic, the percentage reduction in firms' profit would be roughly proportional to the percentage reduction in the price-cost margin (Lerner Index), which is typically substantially small than the price. For consumer surplus, suppose that demand takes the constant elasticity form  $x(p) = p^{-\varepsilon}$ . Then if price increases from  $p$  to  $\alpha p$  where  $\alpha > 1$ , the percentage reduction in consumer surplus is given by

$$\frac{CS(\alpha p) - CS(p)}{CS(p)} = \alpha^{1-\varepsilon} - 1.$$

Using a Taylor approximation, for small changes ( $\alpha \approx 1$ ) this is approximately equal to  $(1 - \varepsilon)(1 - \alpha)$ . For example, when  $\varepsilon = 2$ , a 4.6% increase in price leads to approximately a 4.6% decrease in consumer surplus. However, if  $\varepsilon = 4$ , it would lead instead to a 13.8% reduction in consumer surplus.

Two more recent studies of bid-rigging do provide estimates of more sub-

been stopped.

stantial elevations in price due to price-fixing conspiracies. Porter and Zona [1999] examine bidding behavior at procurement auctions for school milk in Ohio from 1980 to 1990. The data was collected as part of a case brought by the attorney general of Ohio against 13 Ohio dairies as a result of the 1993 confessions of two dairies operating in the southwestern part of the state (who testified that they had rigged bids with other firms in the area). As a measure of the effect of the conspiracies on prices, Porter and Zona conduct a regression analysis in which the winning bid is regressed on various measures of the contract terms requested by the school district (e.g., was a cooler to be provided? straws?), various measures of the costs of the potential bidders (e.g., the distance between the school district and the closest and second-closest milk plants), and two measures of competition: (i) the inverse of the Herfindahl Index derived from firms' shares of milk processing plants within 75 miles of the school district and (ii) the *change* in the *effective* Herfindahl due to the presence of any defendant firms with plants within 75 miles of the school district, denoted as *Delta*. The estimated effects of *Delta* and the square of *Delta* (labeled  $\Delta^2$ ) in each year are shown in columns (a)-(c) of Table 2.3.

Table 2.3: The Effect of Price-Fixing on the Price Paid for School Milk (Porter and Zona [1999])

School Year	Estimated Delta Coefficient	Estimated Delta <sup>2</sup> Coefficient	Estimated Interaction Coefficient	Estimated Average Effect	Estimated Effect Conditional on Incumbency
	(a)	(b)	(c)	(d)	(e)
1980-1981	-.00140	-.00150	.00163	3.0%	3.2%
1981-1982	.01304	.01167	.00103	11.3%	40.2%
1982-1983	.02731	.00225	.00098	8.6%	23.2%
1983-1984	.02995	-.00970	.00156	4.5%	1.1%
1984-1985	.02147	.00106	.00199	6.7%	19.7%
1985-1986	.02684	-.00230	.00122	5.4%	11.5%
1986-1987	.02425	.00173	.00130	6.5%	20.5%
1987-1988	.00368	.02901	.00060	3.3%	49.0%
1988-1989	-.02270	.03636	.00229	2.9%	29.4%
1989-1990	-.04940	.01340	.00410	-1.6%	3.4%
1990-1991	-.02010	-.01260	.00634	-0.3%	-8.3%

Note: The table reports the estimated coefficients on the collusion indices described in the text. We have estimated the effect of collusion based on the mean values of the variables used in the regression. The results reported in column (d) are the expected markups for all districts over competitive prices, in percent.

Column (d) gives the average effect on price of the *Delta* variables for school districts in southwestern Ohio.<sup>18</sup> These average to a 4.6% price elevation over the ten-year time period. Weighting instead by the different number of auctions in the different years, and excluding three years in which the cartel was said to have broken down (1983-4 and after 1989), the average is 6.5%. Although this 6.5% figure is not that large, two points should be noted. First, the bid predicted for a nondefendant dairy that is 20 miles from the school district is between 12.5 and 13.0 cents per half-pint carton (depending on the model used), while variable costs are roughly 10 cents per

<sup>18</sup>Three of the 13 defendants were located in southwestern Ohio, the rest were from eastern Ohio.

carton. Thus, this 6.5% represents roughly a 30% increase in the mark-up over costs. Thus, assuming no reduction in quantities purchased (school demand for milk is in fact very inelastic), the percentage increase in profits due to collusion is substantial even if the price elevation is not. Second, this 6.5% is an average over districts in which defendants did and did not have market power. Column (e) of Table 2.3 depicts the average increase in price in each year due to the conspiracy when one limits attention to southwestern districts in which one of the defendants was an incumbent in the previous year, which were likely to be markets in which the defendants jointly had greater market power. The price increases for these markets are substantially larger, ranging as high as 49% of price, which would represent around a 175% increase in the mark-up (they average roughly 24.6% over the eight years in which the cartel was effective).

The second recent study is by Froeb, Koyak, and Werden [1993]. They examine the effect of a proven conspiracy among bidders in U.S. Department of Defense procurement auctions for frozen perch (a type of fish). They fit a reduced form pricing model from the post-conspiracy period and project back into the conspiracy period to get “no-conspiracy” predicted prices for the earlier conspiracy period. Doing so, they find an estimated price elevation of 27.3% over the entire conspiracy period.

Given the fact that significant damage awards in price-fixing cases are a relatively common occurrence, and these are by law based on evidence regarding the overcharge due to the conspiracy, it is surprising how thin the published literature is documenting significant effects of price fixing.<sup>19</sup>

<sup>19</sup>Taylor [2002] provides some evidence using aggregate output data that the National

Certainly, there is anecdotal evidence to suggest these effects can be large (Griffin [2001]). It would be good to see more of this evidence documented in print.

## 4 Detecting Price Fixing

In many cases there may not be any direct evidence available that a price-fixing conspiracy exists (e.g., evidence of meetings at which prices to be charged were agreed to), but we may want to draw indirect inferences from other evidence. There are two principal reasons why we may wish to do so. First, an enforcement agency may be interested in using various indicia to guide their enforcement efforts; with these in hand, certain industries might be targeted for more in-depth investigation in a search for direct evidence of a price-fixing conspiracy. Second, a court (or jury) in a price-fixing case may be faced with a fact pattern in which there is no “smoking gun” — i.e., no direct evidence that any discussions took place — and may need to

Industrial Recovery Act of 1933, which organized cartels in various industries and provided for a time some element of governmental enforcement, had the effect of reducing output.

Baldwin, Marshall, and Richard [1997] examine bidding behavior in U.S. Forest Timber Sales. In their case, they do not have any direct evidence that collusion was occurring. Rather, they fit structural models of non-cooperative and cooperative bidding behavior, test which fits the data better (they find that the cooperative model does), and measure the difference in prices that would be expected to obtain were the firms to follow the non-cooperative model instead of the cooperative one. They find that the Forest Service earns roughly 7.9% less revenue under the collusive mode of behavior. It should be noted, however, that the non-cooperative behavior that Baldwin, Marshall, and Richard document may have been the result of tacit coordination rather than price fixing.

Some older studies of the effect of price-fixing enforcement on pricing are Stigler and Kindahl [1970, p. 92], Asch and Seneca [1976], and Block and Feinstein [1980]. Feinberg [1980] and Choi and Philippatos [1983] examine the effect of collusion and indictments on profitability. These studies find no evidence of substantial reductions in prices or profits due to price-fixing enforcement.

decide based on indirect evidence whether to find the defendants guilty. The question, then, is what kinds of evidence should we interpret as increasing the likelihood that a price-fixing conspiracy is taking (or has taken) place? The economics and legal literatures have focused on two types of evidence, *structural* evidence and *behavioral* evidence.

## 4.1 Structural Evidence

Structural evidence focuses on characteristics of the industry and its product(s). The most well-known paper on this issue is Hay and Kelley [1974], who discuss various structural factors that might be expected to influence the likelihood of the firms in an industry engaging in price fixing, and who then document the characteristics of the industries in which the DOJ has found price fixing to have occurred.

At a very rudimentary level, we can expect the likelihood of price fixing to be increasing in the net benefit of engaging in price fixing, including the expected costs of the conspiracy being detected and successfully prosecuted, which might be written as

$$\pi(\text{talk}) - \pi(\text{don't talk}) - E(\text{costs of being caught}). \quad (4)$$

The factors that might be expected to affect this net benefit can usefully be divided into three categories:

- (i) **Factors that affect the potential size of  $\pi(\text{talk}) - \pi(\text{don't talk})$ .**

Here we capture the difference between the most profitable outcome possible for the firms (the best possible subgame perfect Nash equilibrium) and the worst. Put simply, if this difference is small, say because there is very

little ability to sustain positive profits in an industry, then there is little reason to attempt to fix prices given the potential criminal penalties that could be imposed. One set of factors affecting this potential difference relates to incentives to cheat. These are factors that make it harder to sustain any given increase in price above the non-cooperative level. Industry characteristics that affect this would include the level of concentration in the industry (greater concentration makes sustaining a given supracompetitive price easier; see Tirole [1988, pp. 247-8]); the degree of observability of firms' offered prices (lesser observability, including more noisy signals of price cuts, make sustaining a given supracompetitive price harder; see Stigler [1964] and Green and Porter [1984]), the lumpiness of demand (lumpy demand makes sustaining a collusive scheme more difficult; see Tirole [1988, p. 248]), and the levels of capacity in the industry (both the level of aggregate capacity and its distribution can matter, although the effect is not necessarily monotonic; see Brock and Scheinkman [1985] and Compte, Jenny, and Rey [forth.]). Another set of factors that affect this potential difference relates to the extent to which a given price increase raises profits. These include, for example, market size (doubling market demand at each price doubles the potential gains from price fixing if costs exhibit constant returns to scale) and the elasticity of demand.<sup>20</sup>

(ii) **Factors that affect the amount of the potential gain that is actually realized by talking.**

Many of the factors discussed by Hay and Kelley fall into this category.

<sup>20</sup>Note that the elasticity of demand could also affect the incentives to cheat on any given agreed-upon price (in a simple repeated Bertrand pricing game, however, it doesn't).

In Hay and Kelley's discussion, they focus on how a given factor affects the ease of coordination with explicit collusion. The difficulty, however, is that most of the factors that one might think of here have ambiguous effects, because a factor that makes coordination easier is likely to make coordination easier *both* when the firms talk and when they don't. For example, when there are more firms in an industry, coordination is likely to be harder *both* with talking and without; when the products are more homogeneous in the sense that there are fewer of them, their characteristics are unchanging, etc., coordination is likely to be easier *both* when talking and when not; and when the firms are more symmetric, coordination is likely to be easier, *both* with talking and without. What matters for the incentive to engage in price fixing is the extent to which each of the factors makes coordination relatively easier when firms talk than when they do not. In essence, Hay and Kelley's discussion assumes that (tacit) collusion is unlikely absent explicit communication. As we discussed in Section 2.1, however, there is relatively little known about this.

(iii) **Factors that affect the expected costs of price fixing.**

The first factor that affects the expected costs of collusion is simple: the severity of punishments. Unfortunately, this will not typically vary across industries (at least within a country). However, a number of factors can be expected to affect the likelihood of detection and are likely to vary across industries. Here we can include the number of necessary participants (more participants is generally thought to make it more likely that some participant will either inform the authorities or tell someone else who will inform the au-



thorities), the sophistication of buyers (if they know the costs of production, they are more likely to know when price levels or increases are not justified, and may then perform their own private investigation), the importance of the product to buyers (greater importance increases buyers' incentive to monitor and investigate privately), and factors that increase the required number of meetings such as the number of products or product characteristics over which agreement must be reached.<sup>21</sup>

Hay and Kelley present a summary of successful criminal price-fixing cases brought by the DOJ during the years 1963 to 1972.<sup>22</sup> All together they find 65 such cases (a summary of these cases can be found in the Appendix of the Hay and Kelly paper). The conspiracies were detected in a variety of ways. Of the 49 cases for which Hay and Kelley know how the conspiracy was detected, 12 were uncovered as a result of a Grand Jury investigation in another case, 10 were due to a complaint by a competitor (a somewhat puzzling fact, perhaps indicating that the firms were engaged in exclusionary behavior as well), 7 were due to a customer complaint, 6 were due to a complaint by a local, state, or federal agency, 3 were due a complaint by current or former employees (the remaining cases were detected in various ways, with each method of detection accounting for 1 or 2 cases).

One of Hay and Kelley's most striking conclusions is that these cases were weighted heavily toward highly concentrated markets. Table 2.4 summarizes

<sup>21</sup>Added to this list is, of course, anything else that the antitrust authorities use as signals to launch investigations.

<sup>22</sup>These are cases won at trial or settled with a plea of *nolo contendere*. Hay and Kelley restrict attention to price-fixing agreements among competitors (e.g., resale price maintenance cases brought under Section 1 of the Sherman Act are excluded).

the distributions of the number of conspirators (for the 62 cases in which this information was available) and the four-firm market concentration ratio (for the 50 cases in which this information was available).<sup>23</sup> Of the 50 latter cases, 21 involved a market with a four-firm concentration ratio over 75 (42%), and 38 of the 50 involved a market with a four-firm concentration ratio over 50 (76%). By way of contrast, Scherer and Ross [1990] report that the population distribution of concentration among four-digit manufacturing industries in 1982 had only 5.1% of the industries with concentration over 80, and 17.6% with concentration over 60.<sup>24</sup> This finding regarding concentration must be considered with some care. Since we are observing a sample of successfully prosecuted conspiracies, the selection process that determines which conspiracies are detected matters here. However, since it seems more likely that conspiracies involving many firms will be detected (and Hay and Kelley report that conspiracies involving many firms did not last long before being detected), these concentration numbers may actually be downward biased relative to the true population distribution of concentration for markets with conspiracies.<sup>25</sup> Table 2.4 also reveals that nearly all conspiracies

<sup>23</sup>In some cases in which four-firm concentration ratio is unavailable, Hay and Kelly calculate market concentration by dividing 100 by the total number of firms in the market and multiplying by 4. Thus, these concentration figures represent a lower bound on the four-firm concentration ratio, assuming of course that Hay and Kelley (and the DOJ Fact Memoranda that they rely on) have not defined the market too narrowly.

<sup>24</sup>Note, however, that four-digit industries may differ markedly from the “markets” identified by the Justice Department (and, therefore, by Hay and Kelley) in these antitrust investigations. As one example, the four-digit Ready-Mix Concrete Industry (#3273) had a 1982 four-firm concentration ratio of 6%, but it is a highly localized industry with significantly higher concentration in local markets.

<sup>25</sup>The most likely effect working in the other direction is the antitrust agencies’ possible tendency to look for price fixing in concentrated markets. Nonetheless, the diversity of ways in which these conspiracies were detected suggests that this probably does not undercut the conclusion that concentrated markets are more likely to engage in price

involving large number of firms involved a formal trade association.

Table 2.4: The Distribution of Conspirators, Market Concentration, and Trade Association Involvement in Successful DOJ Price-Fixing Cases 1963-72.(Hay and Kelley [1974])

Number of Conspirators	2	3	4	5	6	7	8	9	10	11-15	16-20	21-25	>25	Total
Number of Cases	1	7	8	4	10	4	3	5	7	5	2	–	6	62
Trade Association Involvement	–	–	1	–	4	1	–	1	3	1	1	–	6	18
	Concentration Ratios													
Concentration (Percentage)	0-25			25-50			51-75			76-100			Total	
Number of Cases	3			9			17			21			50	

In other dimensions, Hay and Kelley find that nearly all of the cases involve products that are homogeneous across firms and that a majority of the cases involve a conspiracy that was organized in response to price-wars or a “lack of discipline” in the market. In addition, it was often the case that when members of an industry in one local market were found to be colluding, the members in others were as well, lending support to the view that there are structural factors that affect the likelihood of collusion. Finally, relative to our previous discussion of the effects of a price-fixing enforcement actions, it is noteworthy that Hay and Kelly observe that an industry that was successfully prosecuted once, was often successfully prosecuted again later.

## 4.2 Behavioral Evidence

The second type of evidence to which one might look to draw inferences about the likelihood of price fixing is evidence of the firms’ behavior. In fixing.

particular, one may ask whether observation of firms' behavior can be used to infer the existence of a price-fixing conspiracy. What if all firms charge the same price? How about the same very high price? What if they all follow the prices announced by firm A, the largest firm in the industry? What if they, in other ways, seem to behave "cooperatively"? The difficulty we run into with all of these ideas is the same difficulty we ran into before: On the one hand, formal economic theory tells us that any outcome that is possible with talking is also possible without it.<sup>26</sup> If we are to draw an inference then, it must be because we think certain types of behaviors are more likely when firms are able to explicitly coordinate. But, as we have seen, formal economic theory is currently of limited help on this point.

Intuition suggests that we might in some cases be inclined to draw an inference of collusion. Suppose for example that we observe parallel behavior that is very complicated: say, every firm charging 19.174 per unit on Friday, and then all simultaneously (i.e., without first observing other firms doing so) changing to 20.34 on Monday morning. Suppose further that there has been no sudden change in demand, and no change in the price of any significant input. Finally, suppose that the penalty for being the *only* firm to charge the higher price is very severe, making a unilateral price increase very risky for a firm, as in the game depicted in Figure 2.1. It is certainly possible that such behavior could result from purely independent non-cooperative actions. But

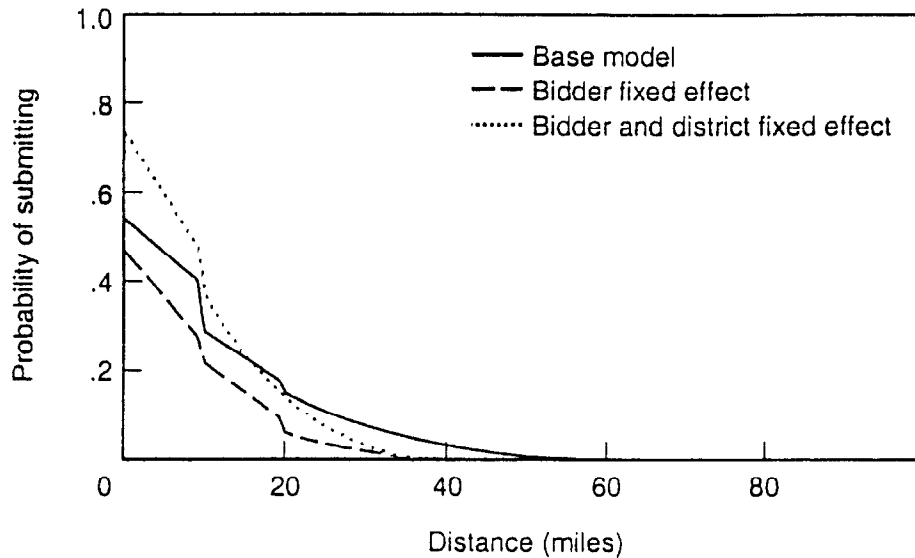
<sup>26</sup>One possible exception to this conclusion arises in settings in which firms possess independent private information. In this case, we might infer that communication is taking place if we see a firm's behavior varying with private information that should only be known to other firms. One difficulty in making such an inference, however, arises from the possibility that firms may obtain some imperfect signals of each others' information even without communication.

it appears unlikely, even if this can unfortunately be said at present mostly at an intuitive level.

Economists' efforts at providing evidence of conspiracy have typically focused instead on identifying whether firms have been exhibiting "cooperative" behavior. Thus, the economist charged with convincing a judge or jury that a conspiracy has taken place would typically look at whether prices were high relative to costs, particularly as compared to other similar markets. He or she would then look for evidence of a change in this pricing relationship as evidence of the start or end of a conspiracy.

Two interesting attempts to look for cooperative behavior in the context of procurement auctions appear in a pair of recent papers, Porter and Zona [1993] and [1999]. Both papers attempt to identify cooperative behavior in a subset of firms known to have colluded by looking for differences in behavior from a control group comprised of the other firms in the market. (The idea is that if the tests work in these cases, then we might feel confident in using them when we suspect collusion may be taking place.) In the Porter and Zona [1999] study of school milk procurement auctions (discussed in the previous section), they compare two features of firms' behavior: their decision of whether to bid, and their decision of how much to bid given that they bid using a probit and an OLS regression (with a sample selection correction) respectively. The variables include various characteristics of the procurement specifications as well variables capturing the firm's cost position absolutely and relative to other firms. Figures 2.5 and 2.6 depict how these two decisions depend on distance for "competitive" firms (those not accused of price fixing)

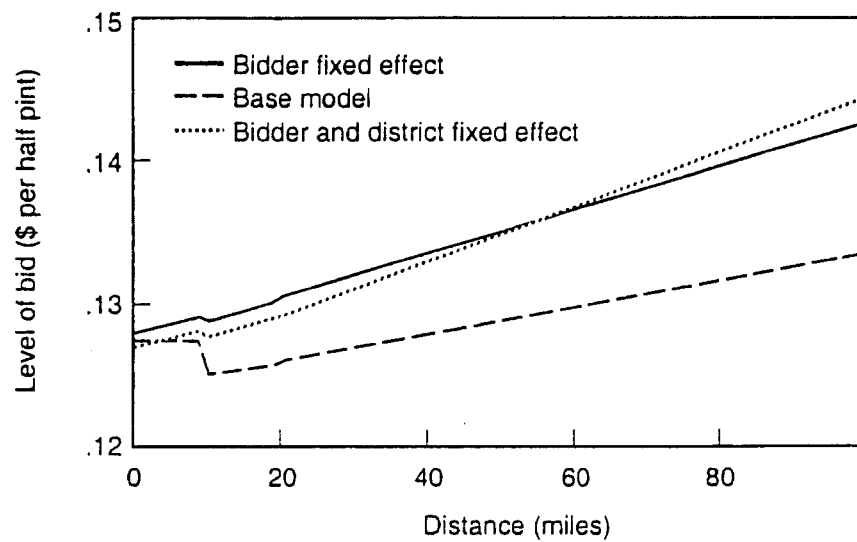
Figure 5: Predicted Probability of Submitting a School Milk Bid by Distance from District for Competitive Firms (Porter and Zona [1993])



based on the results of these estimations: the likelihood of bidding declines sharply with the distance to the school district, and bids increase with this distance. In contrast, Porter and Zona show that the suspected members of the cartel displayed radically different behavior. For example, their bidding behavior is instead often *decreasing* in distance since they bid competitively when they bid in auctions that are far away and not covered by the cartel agreement.

Porter and Zona [1993] studies procurement auctions for highway paving jobs on Long Island, NY from April 1979 through March 1985. In contrast to the school milk study, here no characteristics of the job were available in

Figure 6: Predicted Probability of Submitting a School Milk Bid by Distance from District for Cartel Firms (Porter and Zona [1993])



their data. Hence, comparing bid levels across jobs was not feasible. Instead, Porter and Zona make use of a clever insight: if the suspected firms are engaged in a price-fixing scheme whereby they designate one bidder as the serious bidder and the rest as “phantom” bidders, then the determinants of the lowest cartel bid should be quite different than for all other cartel bids (the former should be based on costs, the latter may well not be), while the determinants of bids for all competitive firms should be the same. They examine this by focusing on the ranking of bids within a job. Specifically, let  $X_i$  denote observable factors affecting the costs of firm  $i$  doing the project (such as the number of jobs the firm currently is handling) and assume as do Porter and Zona that we can write a firm’s bid function as an increasing function  $b(X_i\beta + \varepsilon_i)$ . Then we can write the probability that firm  $i$  bids less than firm  $j$  as

$$\Pr(b_i < b_j) = \Pr(\varepsilon_j - \varepsilon_i \leq (X_i - X_j)\beta).$$

If the firms are behaving according to this non-cooperative model, the same model should explain the ordering of any two firms’ bids regardless of whether these two firms were the two highest or lowest bidders. More generally, if  $r_n$  is the identity of the  $n^{\text{th}}$  highest bidder from among a set of  $N$  firms, then

$$\Pr(r_1, \dots, r_N | \beta) = \Pr(r_1 | \beta) \cdot \Pr(r_2, \dots, r_N | r_1, \beta).$$

Now, we should get the same estimates of  $\beta$  from either trying to explain the identity of the highest bidder from among the  $N$  firms using the probability model  $\Pr(r_1 | \beta)$ , or explaining the ordering of the other  $N - 1$  firms using the probability model  $\Pr(r_2, \dots, r_N | r_1, \beta)$ . Porter and Zona estimate these two



models for competitive firms and suspected cartel firms separately. Table 2.5 depicts the results for competitive firms: here the parameter estimates are very similar for the two models. In contrast, we see in Table 2.6 that while the estimates derived for explaining the lowest bidder from among the cartel firms are very similar to those for the competitive firms, the estimates derived from explaining the ranks of the other cartel bids are very different.

Table 2.5: Rank-Based Estimates of Bid Determinants for Competitive Firms In Highway Paving Jobs (Porter and Zona [1993])

	All Ranks	Low Ranks	Higher Ranks
	(1)	(2)	(3)
Observations	244	75	169
Log likelihood	-291.4	-89.85	-199.4
UTIL	-.0070 (.1)	.0161 (.1)	-.0552 (.3)
UTILSQ	.0986 (.8)	.0534 (.3)	.1596 (1.0)
NOBACK	-.0283 (1.0)	.0089 (.2)	-.0454 (1.3)
CAP	-1.888 (3.8)	-1.641 (2.4)	-2.100 (3.0)
CAPSQ	6.869 (3.9)	6.517 (2.6)	7.020 (2.9)
ISLAND	-.0182 (.3)	-.0759 (.9)	.1016 (.9)

Note—Absolute values of t-statistics are displayed in parentheses. The coefficients of CAP and CAPSQ are scaled up by  $10^4$  and  $10^8$ , respectively.

Table 2.6: Rank-Based Estimates of Bid Determinants for Suspected Cartel Firms in Highway Paving Jobs (Porter and Zona [1993])

	All Ranks (1)	Low Ranks (2)	Higher Ranks (3)
Observations	85	50	35
Log likelihood	-73.97	-44.58	-24.92
UTIL	.0429 (.3)	.2107 (1.0)	.2310 (.6)
UTILSQ	-.0112 (.1)	-.1128 (.6)	-.4300 (.9)
CAP	.4306 (.9)	1.101 (1.3)	-2.537 (1.6)
CAPSQ	-.8473 (.9)	-1.904 (1.2)	3.861 (1.4)

Note—Absolute values of t-statistics are displayed in parentheses. The coefficients of CAP and CAPSQ are scaled up by  $10^4$  and  $10^8$ , respectively.

This is a very nice exercise, but a few caveats are worth mentioning. First, Porter and Zona impose fairly strong functional form restrictions in their estimation. Second, cooperation could well take forms that would not be detectable by this test. For example, firms could collude by agreeing to simply behave as if their costs were inflated by some fixed percentage. By doing so they would be indistinguishable from firms that are behaving noncooperatively. Third, and most importantly, it should be stressed that for the reasons we have discussed previously neither of these papers can eliminate the possibility that the behavior in question arises from purely tacit behavior.

The courts have struggled with this inference issue, and their decisions often appear rather confused both in terms of their goal and how they try to achieve it. Sometimes a court has said that they are trying to infer an express agreement, but has used criteria that do not make any sense, such as mere

evidence that behavior is interdependent. At other, less frequent, times the courts have seemed to say that the occurrence of an express agreement is not even necessary for finding a violation if behavior is sufficiently cooperative — some form of “conscious parallelism” would do.<sup>27</sup>

## **5 Afterword: Antitrust Policy Toward Tacit Collusion**

The discussion at the end of the last section raises a significant question: why should we require an express agreement to find firms guilty of a violation of the Sherman Act? That is, can we not apply the Sherman Act’s prohibition on conspiracies in restraint of trade to include tacit “conspiracies” — that is, tacit collusion? Leaving aside issues of the original intent of the statute, what should we think of such a policy?

It is sometimes argued that a good reason for limiting application of the Sherman Act to express agreements is that it is hard to describe what it is we would be telling oligopolists — who, after all, are just acting rationally when they tacitly collude — to do otherwise. Can we tell them “Don’t tacitly collude”? Or “Don’t make your pricing decisions with regard to what your rivals do”? And would it be fair to send managers to jail for failing to follow such prescriptions? It is also sometimes argued that to apply the Sherman Act to tacit collusion would involve the Courts in an ongoing process akin to price regulation of industries.

Donald Turner, who provided the most forceful articulation of these arguments (Turner [1962]), concluded that the elimination of tacit collusion was

<sup>27</sup>For a useful discussion of these cases, see Areeda and Kaplow [1997], pp. 264-86.

best left out of Section 1 enforcement. Instead, Turner argued for a policy of restructuring highly concentrated markets through divestiture (under either Section 2 of the Sherman Act or new legislation) to address the underlying structural causes of tacitly collusive behavior [Turner (1969)]. This view was also adopted by the well-known *Neal Report* [1968].

A different approach has been championed by Posner [1976, 2001]. Posner starts by taking issue with the underlying premise that the “rationality” of oligopolistic pricing precludes antitrust limits on oligopolists’ pricing practices. After all, doesn’t the threat of traffic tickets alter the behavior of “rational” drivers of automobiles? Posner proposes then that the DOJ and FTC be able to seek monetary penalties/damages in the event that they prove that an industry was engaged in tacit collusion.

Each of these proposals avoids the problem of continuing price regulation of the industry, and neither involves jail sentences as a possible penalty, but each also has its problems. Regarding Turner’s (and the *Neal Report*’s) proposal, Posner [2001], for example, devotes an entire chapter to arguing that, historically, structural divestiture under the antitrust laws (in response to mergers or monopolization) has been slow, costly, and of minimal benefit. Moreover, the need to consider any possible efficiency losses (due to losses of economies of scale, or otherwise) may often make restructuring proceedings difficult and costly affairs. Posner’s proposal, on the other hand, seems to require that firms be shown to be adopting dynamic strategies (i.e., strategies that condition a firm’s prices on past pricing in the market) to be guilty of “tacit collusion” (high prices from static Cournot competition seem not to

qualify). Such a determination seems likely to be fraught with difficulty.

One can also think of some other possible approaches: Perhaps the trigger for structural intervention or monetary penalties should be evidence of high price-cost margins, rather than either high concentration (which need not always lead to high margins) or “tacitly collusive” behavior. Moreover, if structural remedies are to be imposed, perhaps significant weight should be given to the ease with which such divestitures could be carried out. These ideas too are not without their problems. First, economists are able to determine margins only imperfectly in many cases. False positives are a real danger here. Nonetheless, as our empirical techniques improve (and they have recently been improving very rapidly), this should become less of a problem. Second, there is an important issue of *ex ante* incentives that our discussion has so far ignored. Firms will naturally avoid placing themselves in positions that trigger antitrust intervention, whether monetary damages or restructuring, and this may lead them to shy away from cost reductions or product improvements that might improve their margins.

Still, it is interesting to compare such a policy or restructuring to the existing policy that proscribes anticompetitive horizontal mergers (which I discuss in the next chapter). In contrast to most applications of merger policy, this policy would be retrospective rather than prospective, and so it would be able to use *actual* evidence on pricing behavior to judge the degree to which a given market structure leads to anticompetitive pricing. Moreover, while one important argument for a prospective merger policy (as envisioned by the Hart-Scott-Rodino pre-merger notification process in the

United States) is that it is easier to keep firms apart than to unscramble them after the fact, the policy outlined above would take explicit account of the costs of such dissolutions.

These issues are clearly difficult ones. The last extended public discussion of them occurred some 30 years ago. They were largely forgotten after the 1970s, in the general move away from confidence in activist governmental intervention in the economy. Very recently they have begun to once again receive some attention.<sup>28</sup>

## References

- Areeda, P. and L. Kaplow [1997], *Antitrust Analysis: Problems, Text, Cases, Fifth Edition*, New York: Aspen Law and Business.
- Athey, S. and K. Bagwell [2001], "Optimal Collusion with Private Information," *RAND Journal of Economics* (32), 428-65.
- Athey, S., K. Bagwell, and C. Sanchirico [2002], "Collusion and Price Rigidity," mimeo.
- Baldwin, L.H., R.C. Marshall, and J.-F. Richard [1997], "Bidder Collusion at Forest Service Timber Sales," *Journal of Political Economy* (105), 657-99.
- Beckner, C.F. III and S.C. Salop, [1999], "Decision Theory and Antitrust Rules," *Antitrust Law Journal* (67), 41-???
- Baldwin, L.H., R.C. Marshall, and J.-F. Richard [1997], "Bidder Collusion at Forest Service Timber Sales," *Journal of Political Economy* (105), 657-99.
- Block, K.B., F.C. Nold, and J.G. Sidak [1981], "The Deterrent Effect of Antitrust Enforcement," *Journal of Political Economy* (89), 429-45.

<sup>28</sup>See, for example, Posner's recent [2002] opinion in the high fructose corn syrup case (in his job as circuit court judge in the 7th Circuit Court of Appeals). See also Carlton, Gertner, and Rosenfield [1997].

Brock, W. and J. Scheinkman [1985], "Price-Setting Supergames with Capacity Constraints," *Review of Economic Studies* (52), 371-82.

Carlton, D.W., R.H. Gertner, and A.. Rosenfield [1997], "Communication Among Competitors: Game Theory and Antitrust," *George Mason Law Review* (5), 423-40.

Compte, O. [1998], "Communication in Repeated Games with Imperfect Private Monitoring," *Econometrica* (66), 597-626.

Compte, O., F. Jenny, and P. Rey [forth.], "Capacity Constraints, Mergers, and Collusion", *European Economic Review*.

Cramton, P. and T. Palfrey [1990], "Cartel Enforcement with Uncertainty about Costs," *International Economic Review* (31), 17-47.

Crawford, V. [1998], "A Survey of Experiments on Communication via Cheap Talk," *Journal of Economic Theory* (??), 286-98.

Crawford, V. and J. Sobel [1982], "Strategic Information Transmission," *Econometrica* (50), 1431-51.

Daughety, A.F. and R. Forsythe [1987], "Industry-wide Regulation and the Formation of Reputations: A Laboratory Analysis," in E. Bailey, ed., *Public Regulation: New Perspectives on Institutions and Policies*, Cambridge, MA: MIT Press, 347-98

Farrell, J. and M. Rabin [1996], "Cheap Talk," *Journal of Economic Perspectives* (10), 103-118.

Fershtman, C. and A. Pakes [2000], "A Dynamic Game with Collusion and Price Wars," *RAND Journal of Economics* (31), 207-36.

Froeb, L.M., R.A. Koyak, and G.J. Werden [1993], "What is the Effect of Bid-rigging on Prices?," *Economics Letters* (42), 419-23.

Genesove, D. and W.P. Mullin [2001], "Rules, Communication, and Collusion: Narrative Evidence from the Sugar Institute Case," NBER Working Paper 8145.

Green, E.J. and R.H. Porter [1984], "Noncooperative Collusion Under Imperfect Price Information," *Econometrica* (52), 87-100.

Griffen, J.M. [2001], “An Inside View of a Cartel at Work: Common Characteristics of International Cartels,” in *Fighting Cartels — Why and How?*, Stockholm: Konkurrensverket, Swedish Competition Authority.

Hay, G.A. and D. Kelley [1974], “An Empirical Survey of Price-Fixing Conspiracies,” *Journal of Law and Economics* (??), 13-38.

Holt, C.[1993], “Industrial Organization: A Survey of Laboratory Research,” in J. Kagel and A. Roth, *Handbook of Experimental Economics*, Princeton, NJ: Princeton University Press.

Kandori, M. and H. Matsushima [1998], “Private Observation, Communication, and Collusion,” *Econometrica* (66), 627-52.

Kihlstrom, R. and X. Vives [1992], “Collusion by Asymmetrically Informed Firms,” *Journal of Economics and Management Strategy* (1), 371-96.

Kuhn, K-U. [2001], “Fighting Collusion by Regulating Communication Between Firms,” *Economic Policy: A European Forum* (32), 167-97.

Mankiw, N.G. and M.D. Whinston [1986], “Free Entry and Social Inefficiency,” *RAND Journal of Economics* (17), 48-58.

McAfee, P. and J. McMillan [1992], “Bidding Rings,” *American Economic Review* (82), 579-99.

McCutcheon, B. [1997], “Do Meetings in Smoke-Filled Rooms Facilitate Collusion?,” *Journal of Political Economy* (105), 330-50.

*Neal Report* (White House Task Force on Antitrust Policy) [1968], reprinted in *Small Business and the Robinson-Patman Act, Hearings before the Special Subcommittee on Small Business and the Robinson-Patman Act of the White House Select Committee on Small Business* [1969], 91st Cong., 1st Session, vol. 1, 291-???

Neven, D. [2001], “‘Collusion’ under Article 81 and the Merger Regulation,” in *Fighting Cartels — Why and How?*, Stockholm: Konkurrensverket, Swedish Competition Authority, 56-77.



Porter, R.H. [1983], "A Study of Cartel Stability: The Joint executive Committee 1880-1886," *Bell Journal of Economics* (14), 301-14.

Porter, R.H. and J.D. Zona [1999], "Ohio School Milk Markets: An Analysis of Bidding," *RAND Journal of Economics* (30), 263-88.

Porter, R.H. and J.D. Zona [1993], "Detection of Bid Rigging in Procurement Auctions," *Journal of Political Economy* (101), 518-38.

Posner, R.A. [1975], "The Social Costs of Monopoly and Regulation," *Journal of Political Economy* (83), 807-27.

Roberts, K. [1985], "Cartel Behaviour and Adverse Selection," *Journal of Industrial Economics* (33), 401-13.

Scherer, F.M. and D. Ross [1990], *Industrial Market Structure and Economic Performance: Third Edition*, Boston: Houghton Mifflin Company.

Sproul, M.F. [1993], "Antitrust and Prices," *Journal of Political Economy* (101), pp. 741-54.

Stigler, G.J. [1952], "The Case Against Big Business," *Fortune Magazine*, May.

Stigler, G.J. [1964], "A Theory of Oligopoly," *Journal of Political Economy* (72), 44-61.

Taylor, J.E. [2002], "The Output Effects of Government Sponsored Cartels During the New Deal," *Journal of Industrial Economics* (50), 1-10.

Tirole, J. [1988], *The Theory of Industrial Organization*, Cambridge, MA: MIT Press.

Turner, D.F. [1969], "The Scope of Antitrust and Other Economic regulatory Policies," *Harvard Law Review* (82), 1207-31.

Turner, D.F. [1962], "The Definition of Agreement Under the Sherman Act: Conscious Parallelism and Refusals to Deal," *Harvard Law Review* (75), 655-???