private information has yet to show clearly how the ability
to talk changes oligopoly outcomes relative to a scenario
where talk is prohibited. For example, in the Athey and
Bagwell [2001] model, firms are unable to either signal or
split market shares in the case in which they are not
allowed to talk. How talk matters in the absence of these
restrictions is not clear. The Compte [1998] and Kandori
and Matsushima [1998] papers, on the other hand, do not
show what happens in the absence of communication. More
importantly, while communication to reveal private infor-
mand may well be of some importance, communication to
improve coordination seems a much larger part of most
price-fixing conspiracies.

It is in some sense paradoxical that the least controver-
sial area of antitrust is perhaps the one in which the basis of the
policy in economic theory is weakest. Of course, most econ-
omists are not bothered by this, perhaps because they be-
lieve (as I do) that direct communication (and especially
face-to-face communication) often will matter for achieving
cooperation, and that procompetitive benefits of collusion
are both rare and difficult to document. Nonetheless, it
would be good if economists understood better the econom-
ics behind this belief. Moreover, as we will see in section 2.4,
such an understanding could also help guide enforcement
efforts.

Evidence on the Effects of Price Fixing
If formal economic theory is surprisingly silent about the
effects of the Sherman Act’s ban on firms’ communications
and agreements about prices, perhaps existing empirical
work offers strong support for the view that preventing
oligopolists from talking has a substantial effect on the
price they charge? In fact, the existing published literature

offers less evidence for this proposition than one might
expect.

Sproul [1993], for example, examines 25 of the
approximately 400 cases in which individuals 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. He then examines the ratio of
the actual price to the predicted price in the period follow-
ing the indictment. Figure 2.2 shows the average effect he
observes. (In constructing the figure, the underlying series
for the 25 products are aligned so that in each case the in-
dictment occurs in “month 100.”)

If anything, prices seem to rise (relative to the predicted
price) after the indictment. Examining the price changes fol-
lowing 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 panels (a)–(c) in figure 2.3 show.\textsuperscript{15}

Certainly, there is little in Sproul’s study to suggest that a government price-fixing enforcement action leads to any significant reductions in price. One serious concern with Sproul’s study, however, is that his price data often come from the Bureau of Labor Statistics (BLS) price indices that may include many products other than the specific product that is the focus of the indictment. If so, the effects of ending a conspiracy could be lost in the noise from other price movements. Likewise, several of Sproul’s cases are gasoline price-fixing cases for which he uses a citywide average price. Whether these are, in fact, problems is hard to tell from the information in Sproul’s article.

A study that examines the issue at a much more disaggregated level using price data that are at an appropriate level of aggregation is Block, Nold, and Sidak [1981] (henceforth, BNS). BNS examine prices in sixteen local (city-level) bread markets from 1965 to 1976. During this period the DOJ prosecuted a number of bread producers for price fixing. BNS construct what they call a “mark-up” measure for these local bread markets from the fitted values of the regression

\[ p_{it} = IC_{it} + \sum_j \beta_j w_{ijt} + \epsilon_{it}, \]  

(2.1)

where \( p_{it} \) is a citywide BLS retail price index for bread, \( IC_{it} \) is 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 \( j \) (electricity, natural gas, or labor) in market \( i \) in year \( t \). BNS then define the mark-up to be

\begin{figure}
\centering
\includegraphics[width=\textwidth]{figure23}
\caption{Figure 2.3}
\end{figure}

Effects on prices of ending a conspiracy, imposing government penalties, and awarding civil damages (Sproul [1993]).
\[ M_{it} = \frac{p_{it} - \bar{p}_{it}}{\bar{p}_{it}}. \] (2.2)

(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 on measures of antitrust enforcement in the first-difference form

\[ \Delta M_{it} = \alpha_0 \cdot \Delta \text{Budget}_i + \alpha_1 \cdot \text{DOJREG}_{it} + \alpha_2 \cdot \text{DOJREM}_{it} + \epsilon_{it}, \] (2.3)

where \( \Delta \text{Budget}_i \) is the change in the DOJ's Antitrust Division budget in year \( t \), \( \text{DOJREG}_{it} \) takes the value of 1 for city \( i \) in year \( t \) if a different city in the same region had a price-fixing enforcement action against the bread industry in year \( t - 1 \), and \( \text{DOJREM}_{it} \) 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 \( t - 1 \). The first column of numbers in table 2.1 shows the result of this regression (\( t \)-statistics are in parentheses). The regressions whose results are reported in the next two columns include measures of price changes in the food sector (\( \Delta \text{FOODM} \)) and general manufacturing (\( \Delta \text{GENM} \)) to control for unrelated factors affecting bread prices.

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 price in the next year and ensuing years by 4.6% (of the predicted price \( \bar{p} \)). Certainly this represents a relatively small effect on price.16,17

### Table 2.1

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \Delta \text{BUDGET} )</td>
<td>-0.15*</td>
<td>-0.24</td>
<td>-0.20</td>
</tr>
<tr>
<td>(2.68)</td>
<td>(-4.06)</td>
<td>(-3.65)</td>
<td></td>
</tr>
<tr>
<td>( \text{DOJREG} )</td>
<td>-0.026</td>
<td>-0.025</td>
<td>-0.027</td>
</tr>
<tr>
<td>(2.32)</td>
<td>(-2.09)</td>
<td>(-2.26)</td>
<td></td>
</tr>
<tr>
<td>( \text{DOJREM} )</td>
<td>-0.046</td>
<td>-0.046</td>
<td>-0.044</td>
</tr>
<tr>
<td>(2.32)</td>
<td>(-2.41)</td>
<td>(-2.32)</td>
<td></td>
</tr>
<tr>
<td>( \Delta \text{FOODM} )</td>
<td>+0.058</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2.33)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \Delta \text{GENM} )</td>
<td></td>
<td>-0.010</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(-1.60)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>.013</td>
<td>.014</td>
<td>.017</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>.082</td>
<td>.113</td>
<td>.101</td>
</tr>
<tr>
<td>( F )-statistic</td>
<td>6.04 (3, 204)</td>
<td>6.47 (4, 203)</td>
<td>5.68 (4, 203)</td>
</tr>
</tbody>
</table>

Source: Block, Nold, and Sidak [1981].
Notes: Each regression is based on 208 observations.
* This coefficient is estimated in per million dollars.

Other studies that show small effects of price-fixing enforcement on pricing are Stigler and Kindahl [1970, 92], Feinberg [1980], and Choi and Philippatos [1983].

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 does not matter much because conspiracies simply may be hard to police and maintain without the ability to have binding agreements. Another possibility is that talking does not matter much because firms may be able to collude effectively even without the ability to talk. Still a third possibility is that talking may matter a great deal 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 to 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 discussed above). McCutcheon [1997] suggests another possibility: 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 those 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 of time even after they are no longer talking. If so, those studies simply may have missed the effect by not considering a period long enough after the enforcement event. A second reason is that cartels that talk may be relatively ineffective now because of conspirators' fear of investigation and detection. If so, those studies may not give us a good sense of what prices would be without any form of price-fixing enforcement. Third, firms may take the probability of detection as unchanged even after being caught. If so, their behavior will not change after an indictment, even if the prospect of being caught does affect the extent to which they engage in price fixing.

In addition, a number of studies—including several recent ones—do provide evidence of more substantial elevations in price because of price-fixing conspiracies. Porter and Zona [1999] examine bidding behavior at procurement auctions for school milk in Ohio from 1980 to 1990. The data were collected as part of a case brought by the attorney general of Ohio against thirteen 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 conspiracy on prices, Porter and Zona conduct a regression analysis in which they regress the winning bid on various measures of the contract terms requested by the school district (for example, was a cooler to be provided? straws?), various measures of the costs of the potential bidders (for example, the distance between the school district and the closest second-closest milk plants), and a function of two measures of competition: (i) the inverse of the Herfindahl-Hirschman Index derived from firms' shares of milk processing plants within seventy-five miles of the school district (the number of "equivalent firms") and (ii) the change in the effective Herfindahl-Hirschman Index because of the presence of any defendant firms with plants within seventy-five miles of the school district, denoted as Delta (that is, the amount the index changes when one treats the conspiring firms as a single firm).

Columns (a)-(c) of table 2.2 show for each year the estimated coefficients on variables that include Delta—Delta itself, the square of Delta (labeled Delta²), and an interaction term between Delta and the inverse of the Herfindahl-Hirschman Index. Column (d) shows the average percentage effects on price in each year that are implied by those estimated coefficients for school districts in southwestern Ohio. They 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 (1983–1984, 1989–1990, and 1990–1991), the average is 6.5%.

Although a 6.5% elevation is not large, two points should be noted. First, Porter and Zona have some direct information on the firms’ costs. While the bid predicted for a non-defendant dairy that is twenty miles from the school district is between 12.5 and 13.0 cents per half-pint carton (depending on the model used), variable costs are roughly 10 cents per carton. The 6.5% price increase therefore represents roughly a 30% increase in the mark-up over costs. Assuming no reduction in quantities purchased (school demand for milk is in fact very inelastic), the percentage increase in profits because of 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 2.2 shows the average percentage increase in price in each year because of the conspiracy when attention is limited to southwestern districts in which one of the defendants was an incumbent in the previous year, as these were likely to be markets in which the defendants jointly had greater market power. The average price increases in each year for those markets are substantially larger, ranging as high as 49% and averaging roughly 24.6% over the eight years in which the cartel was effective.

Another study showing substantial effects of price fixing in procurement auctions is 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 postconspiracy

<table>
<thead>
<tr>
<th>School year</th>
<th>Estimated delta coefficient</th>
<th>Estimated interaction coefficient</th>
<th>Estimated average effect</th>
<th>Estimated effect conditional on incumbency</th>
</tr>
</thead>
<tbody>
<tr>
<td>1980–1981</td>
<td>0.0140</td>
<td>0.0177</td>
<td>0.0109</td>
<td>3.2%</td>
</tr>
<tr>
<td>1981–1982</td>
<td>0.0120</td>
<td>0.0170</td>
<td>0.0090</td>
<td>4.0%</td>
</tr>
<tr>
<td>1982–1983</td>
<td>0.0134</td>
<td>0.0225</td>
<td>0.0116</td>
<td>22.2%</td>
</tr>
<tr>
<td>1983–1984</td>
<td>0.0278</td>
<td>0.0168</td>
<td>0.0116</td>
<td>11.2%</td>
</tr>
<tr>
<td>1984–1985</td>
<td>0.0217</td>
<td>0.0156</td>
<td>0.0116</td>
<td>3.0%</td>
</tr>
<tr>
<td>1985–1986</td>
<td>0.0130</td>
<td>0.0116</td>
<td>0.0116</td>
<td>3.4%</td>
</tr>
<tr>
<td>1986–1987</td>
<td>0.0120</td>
<td>0.0120</td>
<td>0.0116</td>
<td>3.4%</td>
</tr>
<tr>
<td>1987–1988</td>
<td>0.0130</td>
<td>0.0116</td>
<td>0.0116</td>
<td>3.4%</td>
</tr>
<tr>
<td>1988–1989</td>
<td>0.0130</td>
<td>0.0116</td>
<td>0.0116</td>
<td>3.4%</td>
</tr>
<tr>
<td>1989–1990</td>
<td>0.0130</td>
<td>0.0116</td>
<td>0.0116</td>
<td>3.4%</td>
</tr>
<tr>
<td>1990–1991</td>
<td>0.0130</td>
<td>0.0116</td>
<td>0.0116</td>
<td>3.4%</td>
</tr>
</tbody>
</table>

Source: Porter and Zona [1990].
period and project back into the conspiracy period to get “no-conspiracy” predicted prices (also known as “but for” prices) for the earlier conspiracy period. Doing so, they find an estimated price elevation of 27.3% over the entire conspiracy period.

Kwoka [1997] studies a long-lasting conspiracy among bidders in real estate auctions in Washington, DC. Kwoka is able to get an estimate of the cartel overcharge (since Kwoka examines a buyer cartel, this is the amount that bids were reduced) by comparing the price paid by the cartel in the auction to the price at which the item was sold later in a postauction “knock-out auction” among the cartel members. From this comparison, Kwoka estimates an overcharge of roughly 32%.

Howard and Kaserman [1989] examine the effects of a price-fixing conspiracy among firms bidding on city sewer construction contracts. The evidence in the case indicated that at one point the firms became frightened of being discovered (because of federal criminal investigations in the road paving business) and ceased their collusive activity. This allowed Howard and Kaserman to compare bidding on seven rigged and thirty-nine nonrigged jobs. They estimate an overcharge of roughly 40% because of price fixing.

These four studies focus on auction settings in which collusion may be relatively easy (government procurement auctions in Porter and Zona [1999], Froeb, Koyak, and Werden [1993], and Howard and Kaserman [1989]; an oral ascending auction in Kwoka [1997]). Some recent highly publicized international cartels have provided evidence of substantial price increases in nonauction settings (Connor [2001a], Griffin [2001]).

One such conspiracy was the highly publicized lysine cartel, which fixed prices from 1992–1995. Lysine is a feed additive that promotes the growth of lean muscle in animals. Figure 2.4 shows the price of lysine from January 1990–December 1995 (drawn using data in White [2001]). Prior to 1991, the lysine industry involved a small number of firms, all foreign. Then, in early 1991, Archer Daniels Midland (ADM) entered the industry with a new production facility. This facility massively increased industry capacity from 390 million pounds to 640 million pounds a year. By mid-1992, with this plant still producing at only 40% of its capacity, lysine prices had fallen dramatically, to the point where

Figure 2.4
ADM no longer was covering its variable costs. The cartel formed in June of 1992 with an agreement to raise prices from their 69 cent level to over 1 dollar. Prices rose, but then in early 1993 adherence to the cartel agreement began to unravel. In October 1993, the cartel reached a new agreement fixing market shares, after which prices stabilized at a high level. Then, in June 1995, the FBI raided ADM headquarters, ending the conspiracy.

Judging exactly how much of the lysine price increase was because of the conspiracy requires disentangling the regular seasonal cycle in lysine prices (which decline in summer and rise each fall, with the notable exception of 1994 during the conspiracy) and also determining what prices would have been without the conspiracy. A plaintiff’s witness in the case estimates the overcharge at 17% (Connor [2001a, 264]; see also Connor [2001b] and White [2001]).

Overall, then, the published evidence on the effect of price-fixing conspiracies is somewhat mixed. Given the fact that significant damage awards in price-fixing cases are a relatively common occurrence, and those are by law based on evidence regarding the overcharge resulting from the conspiracy, it is surprising how limited the published literature is that documents significant effects of price fixing. It would be good to see more of this evidence documented in print (in refereed settings). Also, to the extent that the mixed empirical evidence reflects a real diversity of effects, it would be useful to learn something about the factors associated with greater price increases from price fixing.

2.4 Detecting Price Fixing

In many cases direct evidence that a price-fixing conspiracy exists may not be available (for example, evidence of meet-ings 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 might 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”—that is, no direct evidence that any discussions took place—and may need to decide based on indirect evidence whether to find the defendants guilty. The question in both cases is what kinds of evidence we should 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.

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]. They discuss various structural factors that might be expected to influence the likelihood of the firms in an industry engaging in price fixing, and then they 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 it, including the expected costs of the conspiracy being detected and successfully prosecuted, which might be written as

\[ \pi(\text{talk}) - \pi(\text{do not talk}) - E(\text{costs}). \] (2.4)
We can divide the factors that might be expected to affect this net benefit into three categories:

(i) Factors that affect the potential size of \( \pi(\text{talk}) - \pi(\text{do not talk}) \).

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 result. One set of factors affecting this potential difference relates to the incentives to cheat. These factors make it harder to sustain any given increase in price above the noncooperative 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, 247–248]),
- the degree of observability of firms’ 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, 248]),
- 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 [2002]).

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.\(^{22}\)

(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. 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 theoretically ambiguous effects because a factor that makes coordination easier is likely to make coordination easier both when firms talk and when they do not. 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, and so on, 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 determines how a given factor affects the incentive to engage in price fixing is the extent to which it makes coordination relatively easier when firms talk than when they do not. In essence, Hay and Kelley’s discussion assumes that firms are very unlikely to coordinate successfully without explicit communication, so that only changes in the ease of coordinating with explicit communication matter. However, as we saw in section 2.3, relatively little is currently known about this issue.\(^{23}\)

(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 authorities), 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. In addition, there may be costs of price fixing unrelated to detection and punishment, including costs of meetings, bargaining, and monitoring. These costs are likely to increase, for example, with the number of participants.

Hay and Kelley present a summary of successful criminal price-fixing cases brought by the DOJ from 1963 to 1972. Altogether they find sixty-five such cases (a summary of these cases can be found in the appendix of the Hay and Kelley paper). The conspiracies were detected in a variety of ways. Of the forty-nine cases for which Hay and Kelley know how the conspiracy was detected, twelve were uncovered as a result of a grand jury investigation in another case; ten were because of a complaint by a competitor (a somewhat puzzling fact, perhaps indicating that the firms were engaged in exclusionary behavior as well); seven were because of a customer complaint; six were because of a complaint by a local, state, or federal agency; and three were because of a complaint by current or former employees. (The remaining cases were detected in various ways, with each method of detection accounting for one or two cases.)

One of Hay and Kelley’s most striking conclusions is that these cases were weighted heavily toward highly concentrated markets. Table 2.3 summarizes the distributions of the number of conspirators for the sixty-two cases in which this information was available and the four-firm market concentration ratio for the fifty cases in which this information was available. Of the fifty latter cases, twenty-one involved a market with a four-firm concentration ratio over seventy-five (42%), and thirty-eight of the fifty involved a market with a four-firm concentration ratio over fifty (76%). In comparison, 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 eighty, and 17.6% with concentration over sixty. This finding 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. Table 2.3 also reveals that almost all conspiracies involving a large number of firms involved a formal trade association. What is less clear from these statistics is
whether there is a reduced likelihood of price fixing at the very highest levels of concentration (for example, once there are only two or three firms).

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 other markets were as well, lending support to the view that there are structural factors that affect the likelihood of collusion. Relative to our discussion in section 2.3 concerning interpretations of findings of small effects from price-fixing enforcement actions, it is noteworthy that Hay and Kelley also observe that an industry that was prosecuted successfully once often was prosecuted successfully again later.

Finally, thinking about the incentives for firms to engage in price fixing has some potentially interesting implications for interpreting empirical results on price fixing’s effects. Specifically, as penalties for price fixing become more severe, the level of effectiveness at which firms find price fixing to be worthwhile should increase. Since U.S. price-fixing penalties have increased markedly over time, especially after the passage of the U.S. Sentencing Guidelines, we might expect more recent price-fixing conspiracies to have greater price effects. The evidence we reviewed in section 2.3 seems to show some of this pattern.

**Behavioral Evidence**

One might also hope to draw inferences about the likelihood of price fixing from evidence of firms’ behavior. Can observation of firms’ behavior 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 earlier: formal economic theory tells us that any outcome that is possible with talking is also possible without it. If we are to draw an inference then, it must be because we think that certain types of behaviors are nonetheless more likely when firms are able to explicitly coordinate. But, as we have seen, formal economic theory currently offers little help on this point.

Intuition suggests that we might in some cases be inclined to draw an inference of price fixing. Suppose, for example, that we observe complicated parallel behavior: each of ten firms charge 19.174 per unit on Friday and all simultaneously (that is, without first observing other firms doing so) change to 20.343 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 profit loss from being the only firm to charge the higher price is severe, making a unilateral price increase quite risky for a firm, as in the game depicted in figure 2.1. It is certainly possible that such behavior could result without any communication. But it appears unlikely, even if this can unfortunately be said at present mostly at an intuitive, rather than at a formal theoretical level.

Economists' efforts at providing evidence of conspiracy instead typically focus on identifying whether firms have been exhibiting "cooperative" behavior. In the simplest form of this work, an economist charged with convincing a judge or jury that a conspiracy has taken place (or an economist at the DOJ or FTC looking for evidence of a conspiracy) would look at whether prices were high relative to costs compared to other similar markets or time periods. More generally, an economist might look for any differences in behavior across markets, time periods, or firms (that is, suspected conspirators versus nonconspirators).

Two interesting attempts to look for cooperative behavior in the context of procurement auctions appear in a pair of 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 one might feel confident in using them when one suspects collusion may be taking place.)

In the Porter and Zona [1999] study of school milk procurement auctions discussed in section 2.3, they look at two features of a firm's behavior: its decision of whether to bid and its decision of how much to bid conditional on submitting a bid. The explanatory variables include the procurement specifications as well as 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) based on the results of their estimations. (The three curves in each figure correspond to the results for their "base" model and two models that include fixed effects for different bidders and for different bidders and different school districts, respectively.) The likelihood of bidding declines sharply with the distance to the school district, while bid levels increase with this distance. In contrast, Porter and Zona show that suspected members of the cartel display radically different behavior. For example, their bids instead often decrease
with 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, New York from April 1979 through March 1985. In contrast to the school milk study, here no characteristics of the job are available in their data. Hence, comparing bid levels across jobs is 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 might be quite different than those for all other cartel bids (the former should be based on costs, the latter may not be), while the determinants of bids for all competitive firms should be the same. They examine this idea 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 $b(X_i\beta + \epsilon_i)$. With just two firms, for example, we can write the probability that firm $i$ bids less than firm $j$ as

$$\Pr(b_i < b_j) = \Pr(\epsilon_i - \epsilon_j \leq (X_i - X_j)\beta).$$

As Porter and Zona observe, if $r_i$ is the identity of the $n^{th}$ highest bidder from among a set of $N$ firms, then we can write

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

Now, if firms are behaving noncooperatively, we should get the same estimates of $\beta$ from either trying to explain the identity of the low bidder from among a group of $N$
Table 2.5
Rank-based estimates of bid determinants for suspected cartel firms in highway paving jobs

<table>
<thead>
<tr>
<th></th>
<th>All ranks</th>
<th>Low ranks</th>
<th>Higher ranks</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observations</td>
<td>85</td>
<td>50</td>
<td>35</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>-73.97</td>
<td>-44.58</td>
<td>-24.92</td>
</tr>
<tr>
<td>UTIL</td>
<td>.0429</td>
<td>.2107</td>
<td>.2310</td>
</tr>
<tr>
<td>UTILSQ</td>
<td>-.0112</td>
<td>-.1128</td>
<td>-.4300</td>
</tr>
<tr>
<td>NOBACK</td>
<td>4306</td>
<td>1.101</td>
<td>-2.537</td>
</tr>
<tr>
<td>CAP</td>
<td>CAPSQ</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
|              |          | .8473     | 1.904        | 3.861
|              | (.9)     | (1.2)     | (1.4)        |

Source: Porter and Zona [1993].
Note: Absolute values of t-statistics are displayed in parentheses.

firms using the probability model \( Pr(r_1|\beta) \), or explaining the ordering of the other \( N-1 \) firms using the probability model \( Pr(r_2, \ldots, r_N|r_1, \beta) \). Porter and Zona estimate these two models for competitive firms and suspected cartel firms separately. Table 2.4 shows the results for competitive firms. Column (1) reports parameter estimates based on explaining the ranks of all bids according to the model \( Pr(r_1, \ldots, r_N|\beta) \), column (2) is based on explaining the identity of the lowest bidder using the model \( Pr(r_1|\beta) \), and column (3) is based on explaining the ranking of all but the lowest bid using the model \( Pr(r_2, \ldots, r_N|r_1, \beta) \). The parameter estimates are very similar in columns (2) and (3) (compare the coefficients on the capacity utilization variables CAP and CAPSQ, which are the only statistically significant variables); one cannot reject the hypothesis that the parameters are the same. In contrast, we see in table 2.5 that the estimates for explaining the lowest bidder from among the cartel firms are very different from those explaining the ranks of the other cartel bids. Here one can reject the estimates being the same at a 94% confidence level.

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 take forms that would not be detectable by this test. For example, firms could collude simply by agreeing to 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, for the reasons we have discussed previously, the methods in
these papers cannot eliminate the possibility that the behavior in question arises without any explicit communication having occurred.

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.31

2.5 Antitrust Policy Toward Tacit Collusion

The discussion at the end of section 2.4 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 to do otherwise. Can we tell them “Do not tacitly collude”? Or “Do not make your pricing decisions with regard to what your rivals do”? Are they not just acting rationally when they make these decisions? And would it be fair to send managers to jail for failing to follow such vague 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, concluded that the elimination of tacit collusion was best left out of section 1 enforcement [Turner [1962]]. 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 takes issue with the underlying premise that the “rationality” of oligopolistic pricing precludes antitrust limits on oligopolists’ pricing practices. After all, does the threat of traffic tickets not alter the behavior of “rational” drivers of automobiles? Posner proposes then that the DOJ and FTC be able to seek monetary penalties if 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 (because of losses of economies of scale or otherwise) often may make restructuring proceedings difficult and costly affairs.