the case was heard in April 1922. A few months later the Supreme Court decided in baseball's favor and the antitrust exemption was born.

Bradbury then distorts the record further by asserting "At the heart of the argument that MLB acts like a monopolist is the existence of the antitrust exemption" (p. 205). He cites no sources for this claim, because there are none. Each team sport league is a monopolist because it is the sole producer of its product and has no close substitutes. The NFL has no blanket exemption and it is a monopoly; likewise the NBA. Bradbury then writes referring to the NFL, NBA, NHL, and MLB that "each of these enjoys some antitrust exemptions for collective bargaining with labor unions. . . ." Here, of course, it is not an exemption granted to the leagues, but a general statutory exemption granted to all labor unions by the Clayton Act of 1914. Bradbury continues "There is no strong evidence that the antitrust exemption provides any monopoly privileges to MLB other than protecting it from expensive lawsuits" (p. 208). While the value of baseball's exemption today is not what it used to be, there is still a good case to be made that MLB's minor leagues and perhaps its amateur draft could not exist in their present form were it not for the exemption.

Bradbury's last essay argues that the market for top-level professional baseball in the United States is contestable. If this were true, then the earlier question about whether or not MLB is a monopoly might be moot. Here Bradbury makes two points. First, if there is an aspect of the industry that is not a natural monopoly and, hence, constitutes an artificial barrier to entry, it is the subsidies from local governments that teams receive for the construction of their stadiums. But, he avers, this is not really an issue because "the public does not seem averse to subsidizing major sports teams from leagues other than the dominant existing league" (p. 220). It is clear that Bradbury has never been involved in starting a new or nondominant league. His notion that politicians are not averse to providing subsidies to teams from these upstart leagues is just plain wrong. Second, Bradbury goes on to argue that MLB's market is contestable. He does this by discussing the emergence of the American Association in 1882 and the American League in 1901. He further adduces what he erroneously calls the "Central League" (real name: the Continental League) forcing baseball to expand the number of its teams in 1961. Leaving details aside, the difficulty with Bradbury's claim is that the industry's economic structure today is very different from what it was 57 or 120 years ago.

Bradbury, then, whiffs in his effort to expand his analysis beyond the narrow confines of the baseball diamond. After a promising beginning, *The Baseball Economist* fails to expose the real game.

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Lectures on Antitrust Economics. By Michael D. Whinston. Cairoli Lecture Series. Cambridge: MIT Press, 2006. Pp. xii, 249. $30.00. ISBN 978–0–262–23256–2. *JEL 2007–0599*

Whinston's elegant volume, derived from lectures given at Torcuato University in Argentina, drills into three important topics in competition policy: collusion, mergers, and exclusive contracts. Its coverage of both theoretical and empirical work on these topics is thorough and up to date. At the end, the reader is left hoping for a successor volume on other important topics, notably predatory pricing, tying, bundling, vertical mergers, and vertical price fixing.

The volume is laudably free of the narcissism that infects many books derived from invited lectures. Not only is the work of many other theorists given full weight, but the author reports extensively on empirical work, notwithstanding his firm placement in the tribe of theory.

Whinston apologizes for his focus on economics and his limited treatment of antitrust law, but I find this a strength. For one thing, the law is gradually shifting toward the principle that an antitrust case is a demonstration that a specific intervention in a market improves social welfare. To prevail in a challenge to a merger, for example, the government needs to demonstrate that customers would be better off without the merger than with it. Modern courts are losing their single-minded devotion to the formulaic approach of defining a relevant market, measuring market power within that market, and only then considering the effects of conduct challenged as harmful to competition. In place of that rigid formula, modern courts would like to know by how much the conduct has raised prices or diminished product quality. Whinston provides sophisticated guidance to economists involved in this process. The audience for the book is the well-trained

specialist in modern economic theory—you won't get far in this book unless you can handle most of the end-of-chapter problems in Mas-Colell, Whinston, and Green's *Microeconomic Theory.* Lawyers may want to hire a member of that fraternity to guide them through the material in Whinston's volume.

The central message of the book is that modern competition economics is way, way more complicated and ambiguous than you thought. Even the most alert student of the literature in this field will find a number of surprising "ah, but no" propositions here. Surely it is a good idea to prevent horizontal rivals from talking to each other. *Ah, but no.* The leading theory of the successful cartel posits that the cartel will punish cheaters by reverting to low-profit competitive prices. If cartel members are in touch with each other, they can renegotiate after cheating occurs, to avoid inefficient mutual profit losses. In fact, they cannot resist the temptation to renegotiate, as they lack any way to commit to carrying out the threatened punishment. But cheaters, knowing that the punishment is an empty threat, cheat away and the cartel fails. Barbara McCutcheon is responsible for this point.

Punishment for antitrust violations often takes the form of monetary damages. This is the exclusive sanction from civil antitrust proceedings and is increasingly the way that the Justice Department formulates monetary penalties in price-fixing cases (though, oddly, not in other government antitrust cases). By setting out quantitative models and econometric methods for measuring the effects of conduct that harms competition, the volume provides extensive help to those who measure damages. In early pages, though, it has a wonderful *ah, but no* insight: If the victims of price elevation know that they will recover damages for the amount a cartel raises prices, their willingness to pay rises by the amount of the damages. This enables the wrongdoers to set even higher prices. Whinston gives a full analysis of the resulting equilibrium, considering the multiplier relating damages to price elevation (usually three) and the probability that the misconduct will be detected and punished. He does not go on to the next step, which is to alter damages principles to pay most of the damages to the government rather than the victims, though this point has been made by others in the related context of punitive damages. The puzzling reluctance of the government to take serious money away from serious violators, apart from price-fixing cases, diminishes the payoff to government involvement in antitrust enforcement.

The first of the three topics in the book—and the one with the most surprise value—is price fixing. Whinston writes that this chapter ". . . covers what is undoubtedly the most settled area of antitrust. Here I try to unsettle the discourse a bit, suggesting that economists know less about price fixing than they think" (p. 3). He starts with the familiar proposition that our leading framework for thinking about collusion cannot distinguish tacit from explicit collusion. The framework of Nash equilibrium describes an equilibrium but often says nothing about how the participants got to the equilibrium. Antitrust law condemns overt agreements among rivals to cut output and raise price but is less clear about tacit collusion. Within the modern economic view of antitrust—that economists should demonstrate that a particular feasible intervention in a market serves the interests of the public—a prohibition of tacit collusion may fail the test of feasibility. How are we to formulate instructions to firms to avoid tacit collusion? Lawyers—and lamentably many antitrust economists—say that firms should be limited to "competing on the merits," but as Whinston argues convincingly, we don't know how to write the manual of permissible conduct to implement this proposition.

One of the clearest signs of the advanced nature of the book is that Whinston presumes knowledge of modern dynamic oligopoly theory in his discussion of price fixing. Before you pick up this book, be sure you have mastered the basic idea of that theory: High prices are an equilibrium because all sellers know that, should one defect and take away more than their share of the market by setting a lower price, the others will respond by setting low prices in subsequent periods. The notion of a trigger strategy, at the core of modern theory, is not mentioned anywhere in the chapter or the book, because prospective readers know it by heart. This is Economics 257, not 202, and especially not 101.

The theory of "cheap talk" tries to deal with the central question of how communication among rivals might help them achieve the benefits of tacit collusion. Whinston's verdict on cheap talk is skeptical. He does not believe that theories of this type have delivered much so far, though he

observes that economists, including himself, generally believe that communication facilitates tacit collusion. Theories to confirm this common-sense belief have eluded economists to date.

Would-be cartel members face substantial obstacles to gathering the information they need to run an effective cartel. Whinston reviews a body of research that treats this as a revelation problem of the type first considered by James Mirrlees in the context of the problem the government faces in trying to determine an individual's ability to pay for tax purposes. Recent work by Susan Athey and Kyle Bagwell elucidates solutions for revealing cost. Whinston notes a paradox—this information may serve the public interest because it may enable a cartel to allocate output to the efficient producer.

Eliciting information about the information of greatest importance in operating a cartel—adherence to the cartel's agreement on prices or quantities—is a particular challenge to the success of a cartel. A good fraction of the evidence on the incidence and effects of cartels arises in government procurement auctions; Whinston discusses the literature on this point late in the chapter. The sunshine philosophy of government unfortunately aids collusion by solving some or all of the cartel's problem of obtaining reliable information about the actual conduct of putative cartel members. In private business-to-business procurement, buyers go to elaborate lengths to keep the terms of the agreement with the winning bidder secret from the losers. A widespread practice is the off-invoice discount. Only a handful of top executives in the buying and selling companies know the true terms of the transaction. A rival who is able to gain access to an invoice to check adherence to an implicit or explicit agreement about prices will get the impression that the cartel is working, when in fact buyers are paying less than the cartel price. In many industries, the only reliable information available about rivals is their productive capacity. Whinston does not consider cartel theory under this information limitation, but it would be a useful addition to the modern theory of collusion.

Whinston reviews empirical work on the benefits of breaking cartels and other interventions against price-fixing. The general tone of this commentary is that the measured benefits seem fairly small, though definitely detectable. Effects in nonauction markets are almost entirely in single digits. The biggest effect—40 percent—is in a government sewer-construction auction. In a notorious nonauction setting, the market for the animal-feed ingredient, lysine, Whinston's figure 2.4 challenges the reader to find effects associated with the formation and elimination of the cartel. The plaintiff's expert in the civil litigation found an elevation of 18 percent, but the figure suggests that the estimate may have been the subject of vigorous dispute.

The chapter on mergers is generally skeptical of the current practice of estimating their effects using static oligopoly models. In static models, a merger that does not lower marginal cost must necessarily raise prices. Oliver Williamson introduced the proposition—highly influential in merger policy today—that one must look for efficiencies of mergers that lower marginal cost to identify the ones that are good for customers. *Ah, but no,* teaches Whinston. A merger raises the payoff from cheating on a cartel, especially among the sellers not involved in the merger. Thus the merger may preclude an effective cartel equilibrium. In a dynamic setting with trigger strategies, the effect of a merger on prices is ambiguous when it has no effect on marginal cost. Whinston does not come back to this point in his extensive later review of agency procedures for evaluating mergers. The prevailing view among enforcers is that increased concentration makes collusion more likely, so they add a factor for the "coordinated effects" to the "unilateral effects" measured by a one-shot oligopoly model, usually Bertrand. Dynamic oligopoly models have not entered merger-enforcement practice yet. The main reason is the great diversity of equilibria in dynamic models.

The merger chapter spends far more effort than is merited on the formulaic process laid out in the *Merger Guidelines* of the FTC and the Justice Department. As a practical matter, sponsors of a merger gain more traction at the agencies from a direct demonstration of a favorable or neutral effect on prices than they do by defining a relevant market and measuring the change in concentration in that market, following the recipe in the *Guidelines.* This is visible in Whinston's discussion, where the analysis needed to apply the market-definition principles overlaps substantially with the analysis needed to measure the unilateral effects of a merger. Soon, the *Guidelines* will read, "The FTC and Justice

Department review proposed mergers by estimating the effects of the merger on the prices and other characteristics of all products affected by the merger." When this advance occurs, Whinston can take part of the credit.

After slogging through the twenty-two pages devoted to the *Guidelines,* I encountered section 3.5, "Breaking the Market-Definition Mold," with great relief and satisfaction. Here Whinston turns to the methods that economists use in practice to evaluate mergers and the findings that are more likely to influence the agencies in modern merger disputes. The two of most interest are merger simulation and event studies in the stock market.

The obvious defect of the market-definition approach is that it takes a binary in-or-out, weed-or-flower, stand on what we consider parameters, the cross-elasticities of demand between other products and a product affected by a merger. A merger simulation model includes all the products with sufficiently large cross-elasticities (positive or negative) to have significant roles in the calculations. Are SUVs in the same market as compact cars? That is a conundrum for market definition, but a merger simulation would probably include a small positive cross-price elasticity, capturing the small but discernable substitutability of the two kinds of vehicles.

A limited amount of evidence based on comparison of merger-simulation predictions of price changes with actual postmerger prices changes is sobering, as Whinston demonstrates in table 3.1 for airline mergers. The correlation is rather lower than the sponsors of merger simulation could wish for.

Although Whinston ultimately comes down in favor of merger simulation as the best practical alternative, he reminds the reader that the assumptions of the models used in practice are fairly strong. Because they are not dynamic, they cannot deal with collusion supported by trigger strategies and thus miss any changes in collusion that result from a merger. The agencies supplement the findings of merger simulation models with more informal consideration of the coordination effects that those models omit.

Event studies play a role in the quantitative analysis of mergers. These studies measure the changes in stock prices of merging companies, direct rivals not involved in the merger, and customers, that occur when the surprise of an intended merger becomes known to traders. The change in the combined value of the merging companies measures the joint effect of reduced competition and efficiencies, so it has no direct role in merger evaluation. The perplexing number of merger announcements that result in a decline in the combined value, such as HP–Compaq, raises interesting issues, not mentioned by Whinston. The two danger signals in the stock market are increases in the stock prices of rivals—presumably signaling their benefit from reduced competition—and decreases in the stock prices of the customers who will be paying higher prices.

Whinston notes that the statistical power of event studies may be limited because the stock market is noisy, so random variations in stock prices from unknown sources may confuse detection of merger effects. A finding of no significant effect on a stock price has no strong meaning—the effect may be buried in the noise. But a finding of a significant effect is just that—one that is unlikely to be the result of random noise. Many controversial mergers have had effects on stock prices with p values below 0.01.

Whinston concentrates on a deeper problem: an event study measures the impact of *all* of the information in a merger announcement, not just the effects that concern evaluation of the competitive effects of the merger. He observes that traders may infer that rivals will benefit from the same alteration in the economic environment that caused a pair of firms to merge. Their stock-price increases are not pure signals of diminished competition from the merger.

Students of merger enforcement can learn a lot from the limited number of court trials of merger challenges that have occurred (most of the time the merging companies call it off if an agency announces a court challenge). Whinston discusses the trial of the proposed merger of Staples and Office Depot only in the context of econometric studies supporting rival market definitions—office superstores against all office-supply retailing. The parties also introduced evidence about the effects of increased competition when a new superstore opened in a particular market. Notwithstanding any ideas about in-or-out market definition, a convincing showing that competition reduced prices in superstores would support the proposition that the merger would raise prices in those markets where Staples and

Office Depot competed before the merger. The court disallowed the merger.

A more recent merger trial, on the Justice Department's challenge to the merger of Oracle and PeopleSoft, has important lessons as well, but is not included in Whinston's discussion. The products at issue were software packages sold to large businesses. A customer buys a set of packages in what amounts to an informal auction, first qualifying several potential suppliers and then soliciting repeated bids until the bidding stops, as in an English auction. The Justice Department engaged a leading auction economics specialist to re-run the actual auctions under the assumption that PeopleSoft and Oracle coordinated their bids rather than acting as rivals. He found important price elevation in those instances where the two companies bid against each other, especially where there was little involvement of other potential suppliers. The court found the government's case unconvincing, not because it departed from the relevant market formula but because it did not go far enough in restating the environment under the hypothetical merger. The court approved the merger, despite the court's other finding that Oracle had failed to demonstrate any efficiencies from the merger.

Both merger trials demonstrate that the agencies and the courts actually put a good deal of weight on analyses that tackle the central issue: What will a merger do to prices?

Whinston's last chapter, on exclusive contracts, has rather a different character because this topic is where he has made most of his many contributions to competition analysis. The *ah, but no* propositions come in layers. The Chicago School (now seen as oversimplifiers even at Chicago and certainly at Northwestern, twenty-seven miles north) analyzed exclusivity as a paid-for element of a purchase. Under the assumption of no externality, the purchaser and seller will bargain to the socially optimal combination of price and exclusivity. Where exclusivity is observed, there must be some efficiency payoff that enlarges the pie enough so that the seller comes out ahead, even after paying the customer for exclusivity.

Ah, but no_1 say Aghion and Bolton in an important 1987 paper that is the starting point for Whinston's analysis. By signing a contract that requires buyers to compensate the seller for lost profit if a buyer decides to buy from an entrant, the buyers and incumbent seller can take away any prospective profit from an entrant. Microsoft used such a contract with computer makers prior to the 1995 consent decree with the Justice Department. Inefficiently little entry will occur. Chicago is wrong because there is no intrinsic joint benefit to exclusivity. *Ah, but* no_2 says Whinston, because this analysis makes the unpleasant assumption that the buyer and seller commit to the action in advance, despite the mutually profitable opportunity to renegotiate once entry has occurred. The renegotiation—as so often!—vitiates the power of the threat.

Ah, but no_3 follows right on: Whinston's work with Ilya Segal adds increasing returns, with the implication of negative externalities across buyers. In this setting, the seller can purchase exclusivity and its attendant barrier to entry at little or no cost. As Whinston states, "The protection of competition is, in a sense, a public good" (p. 143). Under some conditions, exclusivity may not cost the seller anything.

The reader may not find Whinston's ensuing discussion of the intricacies of modern exclusive dealing theory as fascinating as he does, and may want to sample selectively from the many variants he discusses. His focus is entirely on exclusive contracts and he does not go the additional step to study vertical integration by merger.

The book ends with an interesting discussion of the limited empirical research on the consequences of exclusive contracts. Event studies of legal changes suggest that customers are harmed by changes that permit more exclusivity. In beer distribution, where exclusivity is required in some states and banned in others, exclusivity results in slightly higher prices and substantially higher quantity, suggesting that exclusivity causes some reduction in competition by raising barriers to entry and a lot more sales effort by exclusive distributors.

Anyone who has passed Economics 202 and has a practical or theoretical interest in modern competition issues will benefit enormously by spending time with Whinston's excellent book.

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Government Failure versus Market Failure: Microeconomics Policy Research and Government Performance. By Clifford Winston. Washington, D.C.: Brookings Institution Press, Washington, D.C.: American