Paper Trail: Working Papers and Recent Scholarship

Editor's Note: *Editor Bill Page reviews a paper by Jeremy Bertomeu, John Harry Evans III, Mei Feng, and Ayung Tseng addressing the anticompetitive effect of information sharing among firms, using the automobile industry as test of the generated hypotheses. Editor John Woodbury considers a paper by Gregory J. Werden challenging the outcomes of retrospective studies of mergers and another paper by Justin P. Johnson offering a benign explanation for "loss leaders" when consumers are characterized by bounded rationality.*

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—William H. Page and John R. Woodbury

Recent Papers

Jeremy Bertomeu, John Harry Evans III, Mei Feng & Ayung Tseng,
Tacit Collusion and Voluntary Disclosure: Theory and Evidence from the U.S. Automotive Industry,
http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2613970

This paper studies how rivals can share information to facilitate tacit collusion.[1] The authors offer both theory and empirical evidence in their analysis. The theoretical model is of two firms that compete over many time periods by, first, deciding whether to share information ("the information decision") and then choosing outputs ("the operation decision"). They see first a "public signal," such as a forecast of overall economic activity, then a "private signal" of their own forecast of demand in the market. At that point, knowing that both of those factors will affect the eventual market price, they decide whether to share the private signal with their rival—providing the information to some intermediary like a trade association, on the condition that it will distribute it only if both rivals opt to share. Only after the rivals receive the shared information (or not) do they make their profit-maximizing output decisions for the period.

If the rivals only competed in one time period, no firm would ever share its private information, because its rival would use it to estimate the firm's output choice then opportunistically choose a higher output, thus lowering the firm's expected profit. If the firms compete over many periods, however, the firms would recognize that choosing the higher output would damage their reputation and likely provoke retaliatory price cuts (the "punishment path" or the "Nash equilibrium of the

[1] The authors use "tacit collusion" in the usual economic and legal sense of interdependent noncompetitive pricing. *See, e.g.*, Brooke Group Ltd. v. Brown & Williamson Tobacco Corp., 509 U.S. 209, 227 (1993) (Tacit collusion is "the process, not in itself unlawful, by which firms in a concentrated market might in effect share monopoly power, setting their prices at a profit-maximizing, supracompetitive level by recognizing their shared economic interests and their interdependence with respect to price and output decisions."). The authors use the term "tacit agreement" to describe an instance of tacit collusion involving the information sharing they describe. Although the Supreme Court continues to include tacit agreement (but, confusingly, not tacit collusion) within the scope of Section 1 of the Sherman Act, Bell Atlantic Corp. v. Twombly, 550 U.S. 544, 553 (2007) ("[T]he crucial question is whether the challenged anticompetitive conduct stem[s] from independent decision or from an agreement, tacit or express."), the authors of the paper under review do not use the term in any legal sense. They recognize, however, that the mechanisms of information sharing and their effectiveness in maintaining a noncompetitive price level are relevant to the Sherman Act analysis.

single-period game") in later time periods. Consequently, both firms would have a greater incentive to share their private information and use it to increase joint profits by tacit collusion. The authors recognize that this process could involve cheating, punishment, and then renegotiation, but such a scenario might require direct communications that would attract the interest of antitrust enforcers.

A key implication of the model is that, at lower market demand levels, firms can tacitly coordinate on the profit-maximizing output by sharing the forecast information. At high demand levels, however, the incentive to cheat on a tacit arrangement to set jointly profit-maximizing outputs would be greater because the short-term returns from opportunistically setting a higher output would be greater; consequently, the incentive to cheat would overwhelm the expected cost of future punishments, and the agreement would break down. At higher demand levels, the firms would have to settle tacitly on a somewhat lower, but "incentive-compatible" combined output.

If the firms do not share information, then they can only act on the public macroeconomic forecast and their own private demand forecasts, so they will lack the information necessary to achieve the full profit-maximizing output by tacit collusion. When their estimated demand is low, based on the public signals and their private signals, the firms will always share information and maximize joint profits. But if the demand is higher than some threshold level, they will anticipate that the incentive to cheat will be correspondingly higher, so they will not share information—the expected profit from doing so will actually be lower because of the incentive to increase output opportunistically using the shared information. Thus, they will settle for the profit in the single-period game without information sharing.

The authors' model implies that the degree of information sharing should depend in substantial part on industry demand and on the extent of the rivals' focus on short-run profit. Using historical data from the American auto industry, they authors conducted regressions to test these hypotheses. The industry was well-suited to the study, at least until the mid-1990s, because it was fairly concentrated among the big three domestic producers—between 60–90%. Moreover, the means of information sharing was similar to those hypothesized by the theory. The automakers observed the level of macroeconomic activity each month, made their own forecasts, and then chose whether to submit their own forecasts to Ward's Automotive Report, which would then publish the information in weekly newsletters to their subscribers, which included both automakers and dealers, only if all three submitted forecasts. The forecasts they submitted could also be relatively more accurate, if a producer invested more in information-gathering, then submitted revised forecasts during a given month to be distributed in a later week's newsletter. The automakers' actual production each month then revealed the accuracy of their predictions.

The authors use frequency, time horizon, and accuracy of production forecasts to measure the degree of information-sharing. Their proxies for the firms' degree of near-term focus are the rate of turnover of the CEO of the producer (a high rate would tend to make the producers more focused on short-term profit) and the Altman z-scores[2] for the firms (a measure of financial distress, which would also tend to focus the firm on the near-term bottom line). Their proxy for automotive demand is the monthly unemployment rate, which is apparently a very good predictor of new-car sales. They also take account of a variety of control variables that could also affect the automakers' incentives to share information.[3]

[2] Edward I. Altman, *Financial Ratios, Discriminant Analysis and the Prediction of Corporate Bankruptcy*, 23 J. Fin. 589 (1968).

[3] These included capacity utilization, inventory level, production volatility, inflation, and the prices of inputs like steel and electric power.

The findings of the study generally confirmed both of the paper's hypotheses of the determinants of information-sharing. The authors found significant correlations between their measures of the degree of information sharing and their measures of demand (the unemployment rate) and short-term focus (CEO turnover and financial distress). They conclude that their findings were "generally consistent with automobile manufacturers communicating with each other more intensely, reflected in more frequent production forecasts and longer forecast horizons, when the expected demand in the industry is lower and when the firms face longer decision horizons." (p. 21.) In a later section, they also show that, when unemployment is high, the producers tend to revise their production forecasts and their actual production over the course of the month to take account of their rivals' forecasts distributed during the same month.

The paper also includes an extensive and illuminating review of both the empirical and theoretical literature on disclosure and information sharing under different competitive conditions.

The authors suggest that "the intuition" of their results "would appear to generalize to other industries in which there is evidence of tacit collusion," (p. 5) citing both other studies and three antitrust cases.[4] They also suggest that their results "have potential regulatory implications," because they "show how information sharing under tacit agreement can potentially facilitate coordination of total industry production to the detriment of consumers," (p. 5.). They cite two information-exchange cases from the 1920s as "document[ing] early antitrust actions against information sharing in trade associations.[5]

I would add that courts view exchanges of predictions of *future* competitive conduct, like the automakers' production forecasts this paper studies, more suspiciously than, for example, exchanges of *present* prices, but the (relatively) public distribution of the forecasts in a trade journal would almost certainly have insulated the automakers from liability.[6]

Gregory J. Werden, **Inconvenient Truths on Merger Retrospective Studies** (Jan. 5, 2015), *available at* http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2545343

In a recent issue of *The Antitrust Source*, Robert Skitol reviewed a book by John Kwoka assessing retrospective merger analyses.[7] Skitol notes that "Kwoka undercuts any confidence that the agencies have effectively protected consumers from the anticompetitive effects of merger activity." (Review p. 1.) Skitol quotes Kwoka as concluding that the net effect of enforcement policy "has been to focus on mergers most directly causing harm, but the diminished attention to mergers involving somewhat lower market shares and concentration appears to have resulted in approval of significantly more mergers that prove to be anticompetitive."[8] As Skitol notes, one of Kwoka's

[4] The citations are incomplete, but they apparently refer to United States v. Container Corp., 393 U.S. 333 (1969); United States v. Apple Inc. 952 F. Supp. 2d 638 (S.D.N.Y. 2013), *aff'd*, Nos. 13–3741–cv, 2015 WL 3953243 (2d Cir. June 30, 2015); United States v. AU Optronics Corp., 09-CR-0110 SI, 2012 WL 2120452 (N.D. Cal. June 11, 2012).

[5] *Id.* They cite *Maple Flooring Mfrs. Ass'n v. United States*, 268 U.S. 563, 584 (1925) and *Am. Column & Lumber Co. v. United States*, 257 U.S. 377 (1921).

[6] *See* William H. Page, *Signaling and Agreement in Antitrust Law* (June 19, 2015), *available at* http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2620786. An earlier version of this paper is forthcoming in Concurrences J. (2015).

[7] Robert Skitol, *Book Review, A Harsh Report Card on the Merger Enforcement Process*, Antitrust Source (Feb. 2015), http://www.americanbar.org/content/dam/aba/publishing/antitrust_source/feb15_full_source.authcheckdam.pdf.

[8] *Id.* at 2 (citing John Kwoka, Mergers, Merger Control, and Remedies: A Retrospective Analysis of U.S. Policy 155 (2015) [hereinafter Kwoka]).

conclusions is that based on retrospectives, the "agencies should develop 'a better understanding of the a priori characteristics of incorrectly cleared mergers, and hence the sources of policy errors.'"[9]

In a recent paper entitled "Inconvenient Truths on Merger Retrospective Studies," Gregory J. Werden, Senior Economic Counsel in the Justice Department's Antitrust Division, effectively tells us "not so fast." As Werden notes (p. 2), most of the retrospective merger studies rely on the "difference-in-difference" (DiD) methodology. These are studies that focus on whether a consummated merger has resulted in market power as evidenced by post-merger price increases. More specifically, the DiD methodology compares the changes in prices for products/services offered by the merged firm (the "treatment" group) as compared to changes in the prices of a control group of products/services. If the merger led to market-power driven higher prices, then the change in price for the products/services affected by the merger will exceed those in the control group.

Werden makes two overarching criticisms of the DiD merger retrospective approach. First, he explains why the DiD retrospectives are a weak foundation for any evaluation of the price effects of mergers. Second, he argues that, in any event, the retrospectives cannot inform sensible changes in merger enforcement policy. But he also suggests the possibility that these analyses can be more informative if conducted in the context of a case study (although even here, Werden seems skeptical).

"Inconvenient Truths" in the Use of DiD Analyses

As most researchers are well aware, the single most significant choice in conducting a DiD analysis is identifying a suitable control group. If the choice of the control group is incorrect, the results of the DiD analysis are worthless. That is, comparing the changes in the prices of the merged firm to those in a flawed control group would be a meaningless exercise. If the price changes for the merged firm were greater than those in the incorrectly-defined control group, one could not reasonably conclude that the merger resulted in higher prices.

For example, in a broad product market, using the prices of products that are similar to but not produced by the merging parties as the control is not likely a reasonable choice. If the merger affects prices of the merging firm's output, it's likely to affect the prices of other "like" products produced by other firms. Werden seems to suggest that if markets are "local," products sold in locations not affected by the merger could serve as a more suitable control, although the different locales must still face identical demand and cost conditions. (note 14.) While frequently DiD analyses will include supply and demand variables as a way to account for differences in the treatment and control groups, it still must be true that "the unaffected prices must be influenced by exactly the same supply and demand forces that influence the affected prices" (p. 4)—something difficult to verify.

"As with most econometric work" (p.6), the researcher is confronted with the need to choose the data, the time period, the price measures and the statistical methodology for the investigation. Werden notes that "[f]ew results are so robust that no choice matters, and different researchers are apt to make choices different enough to produce substantially different estimates of merger effects." (p. 6.) Werden provides a number of examples that illustrate this sensitivity. In one review of the Northwest/Delta merger, the researchers used a variety of control groups and found that the

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[9] *Id.* at 3 (citing KWOKA, *supra* note 8, at 159).

post-merger price increase ranged from 0 to 6 percent. Using an algorithm that identifies the best match between the control and treatment groups, the estimated fare increase was only 1 percent.[10]

The merger retrospective that focuses on price increases may be misleading if there are significant post-merger quality improvements that are available to consumers at a higher price. The retrospective will then conflate anticompetitive price increases with price increases resulting from improved quality. "While quality changes might be accounted for, that is difficult and unlikely to be attempted." (p. 7.)

Werden notes that the magnitude of the price effects may depend on the time period chosen. While merger retrospectives typically focus on a one- or two- year period after the merger, he notes that "a few merger retrospectives have found long-term merger effects that differed materially from short-term effects." (p. 7.) But then he goes on to argue that one really cannot assess the long-term effects of the merger: "It is not credible that a change in pricing five years after a merger is from the merger." (p. 8.)[11]

In addition, he observes that many of the retrospectives generate puzzling results, at best. For example, a retrospective of the Maytag-Whirlpool merger found significant price increases for dryers, but not for washers, even though the market shares for each product were about the same. "[I]f the study were taken at face value, an unknown force must have caused the merger to have dramatically different effects in nearly identical markets." (p. 14.)

Werden concludes that "[a]n inconvenient truth is that econometric merger retrospectives cannot come close to definitively determining actual merger effects." (p. 9.)

Inability of Merger Retrospectives to Inform Merger Enforcement Policy

Even if the DiD merger retrospectives were not characterized by the infirmities he describes, Werden concludes that "inconvenient truths preclude nearly every way of using econometric merger retrospectives to recalibrate merger enforcement." (p. 9.) For example, Werden notes that using the retrospectives to adjust the HHI thresholds in the 2010 Merger Guidelines would not be particularly helpful "for the simple reason that market shares are determinative of neither agency assessments nor actual merger effects." (p. 9.) And the retrospectives, while addressing price effects, do not address the agency "failures" in identifying the relevant antitrust market.

Werden also argues that any "meta" analyses of the retrospectives—relating the estimated price effects from the retrospectives to various factors—is a futile and potentially misleading research path: "Merger assessment is so heavily fact dependent that every case—or at least every close case—is unique, so the available data cannot trace out a general rule." (p. 10.)

More generally, Werden takes retrospective advocates to task because they "have not explained what guidance they anticipate or how retrospectives provide it. Presumably, they intend something simple; for example, if merger retrospectives often estimate adverse merger effects, the guidance would be to tighten enforcement. But such guidance is too vague to be of much help." (p. 12.)

[10] Werden, p. 7 (citing Aditi Mehta & Nathan Miller, *Choosing the Appropriate Control Group in Merger Evaluations*, *in* THE PROS AND CONS OF MERGER CONTROL 189 (Swedish Competition Auth. 2012)), *available at* http://www.microeconomix.eu/ressources/pros-and-cons-merger-control-0. Werden does not opine on whether the use of this algorithm substantially mitigates the danger of picking the wrong control group.

[11] It is unclear, then, why Werden gives any weight to the long-term differences.

Increasing the Usefulness of Retrospective Analyses

While Werden apparently does not consider the retrospectives by themselves particularly useful, he does suggest how their antitrust utility can be increased. In addition to assessing price effects, retrospective studies "could ask whether postmerger competition conformed to the agency's prior predictions, and if not, what specific aspects of the agency's assessment were inaccurate. Identifying a source of systematic error in agency merger assessment could lead to a significant recalibration of enforcement . . . ." (p. 15.) In other words, a retrospective that looks more like a case study could prove useful. It would require that the researcher evaluate the reasoning behind the agency's decisions, identifying the apparent source of the erroneous prediction as "some combination of faulty facts, erroneous economics, bad breaks, and random variation." (p. 15.) Retrospectives that found a pattern of faulty facts or erroneous economics could result in significant modifications to the merger review process. While this approach requires detailed information on the merger (or other) assessment, Werden seems to agree with Dennis Carlton that the "record-keeping" of the agencies must become far more detailed and careful for the case-study approach to be feasible. (note 57.)

Closing Observations

I certainly agree that retrospectives combined with detailed information on the agencies decisions to (e.g.) clear a merger or not could prove very informative, although the data requirements will be substantial.

However, I believe Werden's dismissal of retrospective studies themselves goes too far. Werden notes that "*as with most econometric work*, merger retrospectives present myriad choices: data sources, price measures, time periods, and statistical methods. Few results are so robust that no choice matters, and different researchers are apt to make choices different enough to produce substantially different estimates of merger effects." (p. 6) (emphasis added) Thus, in broad strokes, the issues raised by Werden with respect to retrospective analyses are the same as those raised for any econometric work, whether estimating price fixing damages or estimating a demand curve. This is not to counsel despair when using econometrics in antitrust (or any other field). Instead, we economists (and lawyers using economists) need to be reminded of the potential fragility of conclusions reached on the basis of econometrics and the need to test the robustness of the conclusions regardless of the specific analysis.[12]

Moreover, one can make lemonade out of the study "lemons." Multiple DiD retrospectives on the same merger that yield different (or puzzling) results regarding the post-merger price changes is not in and of itself a bad thing. It provides researchers a platform to understand the sources of those differences and evaluating which choice combination is likely to yield a more "true" result. Indeed, this "dialectic" is part of the research process itself.

Certainly, Werden's reminder is a cautionary note when deciding how much weight should be ascribed to the conclusions of retrospectives and his paper provides a useful guide to evaluate those studies.

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[12] In my personal experience and in observing colleagues in the course of a merger matter or other litigation matter, the kiss of econometric death is to ignore robustness issues. That is, will relatively small changes in price measures (or other variables of interest), time periods, and statistical methods result in different econometric results? (It is unusual for any given statistical question to have multiple data sets available to the researcher.)

Justin P. Johnson, Unplanned Purchases and Retail Competition (May 5, 2014), http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2319929

While not as current as many of the papers typically considered in *The Paper Trail*, a paper by Justin P. Johnson, Associate Professor at the Samuel Curtis Johnson Graduate School of Management, Cornell University—"Unplanned Purchases and Retail Competition"—has apparently been making the rounds at the agencies and elsewhere. There are three interesting "hooks" in this paper: it assesses the practice of "loss leader" pricing; it accounts for bounded rationality of consumers (thus placing the paper broadly in the behavioral economics literature); and it finds that bounded rationality drives virtually all of the paper's key results.

"Loss-leader" pricing is the practice by multi-product firms of selling "certain products beneath cost, with the hope of attracting additional customers who will also buy other, higher-margin items." (p. 1.) Johnson notes that this practice has been banned in a number of countries and in about half of all U.S. states, explaining that "a serious concern of antitrust authorities is that larger firms often price below cost on the core product lines of smaller rivals." (pp. 1–2.) The conclusion of the paper is that such practices need not be predatory in the presence of bounded rationality, and, indeed a ban on such practices could harm consumers.

More specifically, consumers are assumed to make "unplanned" purchases when they visit retail outlets, purchases that ex ante they believed they would make with a small probability: "It must be [for example] that consumers are (correctly) confident that they need some particular product, such as milk or bread (which will be priced below cost), but underestimate their tendency to purchase other goods." (p. 2.) In Johnson's model, a key parameter is what he calls the "accuracy ratio" that reflects the consumer's biased belief on likely purchases. This is what the consumer ex ante believes his purchase probability of any good x is compared to the unbiased (actual) probability of purchase. If the ratio equals 1, the consumer has an unbiased and accurate assessment of the likelihood of the purchase of good x. A ratio less than one indicates that the consumer is underestimating the likelihood that she will purchase x. With respect to grocery stores, "staples" (like bread and milk) will have a high accuracy ratio compared to the accuracy ratio of unplanned purchases.[13]

In the absence of consumer bias (and when all rival firms carry the same array of products), Johnson shows that loss-leading does not occur. Since a purpose of loss-leaders (in Johnson's model) is to encourage unplanned purchases on the higher-margin items, there is no payoff to the firms when there are no unplanned purchases: "Neither differing shapes of demand nor differing marginal costs by themselves lead to loss leading. Likewise, the fact that demand is stochastic and that there is rivalry among firms is not enough. Rather, if loss leading is to exist . . . it must be that consumers make unplanned purchases." (p. 8.)

Further, in choosing which products are to be loss leaders, all that matters is the set of accuracy ratios. "A price cut is more effective at generating in-store traffic when customers expect that they will buy [the product]," noting that among grocers (for example) loss leaders tend to be staples, products that consumers purchase regularly. (p. 9.)

The results above hold when all firms are offering the same set of products. Johnson then considers a strategy of loss leaders when there are smaller firms that carry only a subset of the products carried by the larger firms. A possible anticompetitive concern in this case might be that the

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13 Because the accuracy ratio will tend to be highest for goods or services that the consumers "need," the unplanned purchases can be regarded as "discretionary" purchases.

larger firms will price below cost on those goods that are offered by the smaller firms so as to force their exit or otherwise impair their ability to serve consumers.[14]

The small firm carries as many staple goods as possible, and these are priced lower than non-staple goods. While the small firm could instead carry more non-staples sold at a higher margin, these non-staples are less effective in attracting customers because the consumer underestimates her purchases of these non-staples when choosing what outlet to patronize. While, in principle, the small firm could charge a higher price for the staples, the lower price of staples "increases the number of customers who shop [at the smaller firm]." (p. 16.) This is because consumer utility from shopping at the smaller firm is higher with the lower staples price than with the higher price.

Note that the consumer chooses which store—large or small—to patronize based on a comparison of ex ante expected utility. With consumers underestimating the "true" likelihood that they will purchase non-staples, consumers underestimate the "true" utility of shopping at the larger store. Even though the large firm may be pricing staples below cost, smaller firms can still survive charging a higher price for the staples (to at least break even) because such outlets may be more convenient (lower transport costs) for consumers than the larger firm.

Further, Johnson shows that the below-cost price being charged by the larger store is not a result of the subset of staples carried by the smaller store: "[W]hich goods are priced below cost by [the larger store] is driven entirely by the nature of consumer bias, not as such by the product line of its [smaller rival]. Similarly, [the smaller store] willingly chooses to carry goods that it knows the [larger store] is pricing below cost, where its decision is also driven by the extent of consumer bias across the different products." (p. 17.)

Johnson then considers the welfare implications of a ban on loss leaders, which I only sketch out here. At the outset, Johnson shows that in the absence of consumer bias, more consumers would patronize the larger store. This is because the utility of shopping at the larger store increases because the consumer no longer underestimates the purchase likelihood of non-staples: In the presence of bias, consumers "underestimate . . . the value of goods in [the larger store's] portfolio that [the smaller store] does not carry." (p. 18.) Consequently, the utility from shopping at the larger store increases in the absence of bias. In this sense, larger stores suffer from under-patronage in the presence of consumer bias.

Thus, in the presence of consumer bias, a ban on loss leaders would harm consumers: "[C]onsumers underestimate their needs and tend to have unsatisfied demand [in patronizing the smaller store]—some would be better off if they instead shopped at the larger firm, even though it is less convenient. A ban on loss leading, by constraining the larger firm's ability to attract consumers, leaves more needs ultimately unsatisfied and so reduces welfare." (p. 19.)

Closing Observations

I really enjoyed reading this paper. First, it does incorporate one component of behavioral economics, bounded rationality. Second, as applied to loss leaders, the model is both simple and elegant. Third, and related, the conclusions are powerful: loss-leading by larger stores serves the interests of consumers with biased purchasing probabilities because it encourages them to visit the larger stores and so satisfy demands whose ex ante significance falls below their ex post sig-

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[14] Having said that, Johnson is quick to point out that price predation cannot be the explanation of the loss-leader practice since loss leaders are a persistent phenomenon, ruling out any recoupment. (p. 14.)

nificance. There is no anticompetitive motive by the larger firm in adopting a loss leader strategy. Indeed, in his model, consumers are better off by visiting the larger stores. It is difficult to predict the extent to which behavioral economics will influence antitrust policy, but papers like this certainly advance that cause.

To be sure, there are some assumptions that might give one pause. For example, in this model, consumers do not learn and so they do not revise their probability that they will purchase non-staples—the accuracy ratio does not increase. Having said that, it may be that bounded rationality limits the efficacy of updating.

In addition, the model assumes that consumers only shop at one store. But Johnson notes that "conclusions about the social effects of loss leading depend on whether the small firms are boutiques providing niche or high-end products that offer a large quality advantage, or rather small firms are more convenient for some customers but carry products that tend to be no higher quality than those of the large firm." (p. 20.)[15]

Although the paper can be technical, much of the very technical material is in an appendix and Johnson spends time providing very good intuitive explanations for his results driven by the modeling. Thus, the paper should be understandable to antitrust practitioners of every stripe.

—JRW

[15] Johnson cites other research suggesting that where "banning loss leading helps consumers" is where the small firm is a boutique. (p. 19.)