THE STRUCTURAL PRESUMPTION AND THE SAFE HARBOR IN MERGER REVIEW: FALSE POSITIVES OR UNWARRANTED CONCERNS?

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Crucial areas of antitrust in the United States have undergone tectonic shifts in the past 30 or 40 years. Views of strategic pricing, vertical restraints, monopoly practices, and mergers that dominated policy until the 1960s have been transformed, resulting in approval of firm conduct and market structures that would have been unthinkable some years earlier. While the extremes of past practice have few advocates, some observers have expressed concern that the transformation may have gone too far and tipped the balance in favor of policy that is too permissive. Of course, it is not easy to get the balance right. Individual cases differ, evidence varies in quality, and alternative explanations abound; hence, some policy errors are inevitable. It is, on the other hand, easy to get the balance wrong, and in this article I will provide some evidence that recent policy in the area of merger control has gotten that balance wrong. The specific issue is the role of market structure in merger review, and the balance in question concerns errors of omission versus commission.

The proper role for market structure in merger review usually devolves to the familiar debate over what is called the “structural presumption.” This term is shorthand for the belief that mergers beyond certain concentration and/or share thresholds are, with high probability, likely to be anticompetitive, and hence enforcement by the agencies and courts can rely on those thresholds for predicting anticompetitive outcomes from such mergers. Of course, few doubt

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that structural conditions make some difference to pricing and other market outcomes, and few advocates would go so far as to make the presumption completely irrefutable. Much of the debate has therefore centered on the question of how accurately structural characteristics of a market predict competitive outcomes and hence how much reliance should be placed on concentration and share data.\(^1\) Put differently, this issue is one of the magnitude of errors made by relying on concentration and shares, and more specifically, on the errors made by different thresholds for those data.

The policy errors from relying on market structure can take two forms: errors of commission (Type I errors), in which policy mistakenly acts against a benign merger, and errors of omission (Type II errors), involving the failure to challenge a competitively harmful merger. Not surprisingly, these errors move in opposite directions: policy that acts more aggressively reduces the rate of Type II error, but at the expense of increased Type I error. Permissive policy lowers Type I error rates but suffers from greater Type II errors. As we shall see, in the area of mergers, but also with respect to other antitrust issues, commentators, the courts, and the antitrust agencies increasingly appear concerned with Type I errors,\(^2\) resulting in what some critics believe is an overly permissive policy toward mergers.

While much ink has been spilled in this debate, it has largely proceeded without the benefit of facts—actual measures of the frequency of errors and their consequences. Rather, anecdotal evidence is usually cited—instances of overly aggressive enforcement or of lax enforcement—but specific examples run considerable risks of either being sui generis or, worse yet, selected for strategic reasons. This article advances that debate by providing the first analysis of this issue based on a comprehensive compilation of reliable available evidence. The key evidence consists of the outcomes of numerous actual mergers as established by careful economic studies—mergers that have been investigated and then either cleared, subject to remedies, or challenged outright by either the Federal Trade Commission or the Antitrust Division of the Department of Justice. We shall see that these data support a strong presumption that market concentration past some point correctly predicts, with high probability, that a merger will be anticompetitive.

\(^1\) One of the most recent collections of articles on this topic can be found in Philadelphia National Bank at 50, 80 Antitrust L.J. 189 passim (2015).

\(^2\) The Supreme Court made clear its preference for minimizing Type I errors in one recent case, opining that “Against the slight benefits of antitrust intervention here, we must weigh a realistic assessment of its costs . . . . Mistaken inferences and the resulting false condemnations ‘are especially costly, because they chill the very conduct the antitrust laws are designed to protect . . . .’ The cost of false positives counsels against an undue expansion of § 2 liability.” Verizon Commc’ns v. Law Offices of Curtis V. Trinko, LLP, 540 U.S. 398, 414 (2004) (quoting Matsushita Elec. Indus. Co. v. Zenith Radio Corp., 475 U.S. 574 (1986)). While this was a non-merger case, this guidance is reflected in other areas of antitrust.
The second aspect of merger policy that is examined here is another structure-based presumption, but one that has been much less scrutinized than the first. This concerns the bounds on concentration below which mergers are seen as likely to convey no significant competitive threat. Commonly called a “safe harbor,” this, too, has become part of the standard apparatus of merger control and is in principle little different from the above presumption involving mergers in highly concentrated markets. Both presumptions rest on beliefs that certain levels of concentration provide sufficient certainty about the effects of a merger as to yield a presumptive policy conclusion—in this case, that coordination in the “safe harbor” zone is unlikely and hence that mergers falling in that category can be presumed benign. In contrast to the structural presumption, the safe harbor appears largely to have been accepted as self-evident, thus providing considerable assurance to companies with safe harbor characteristics that their merger will be approved.

This article is the first systematic investigation of the empirical validity of this safe harbor provision in the merger review process. It draws on the same comprehensive set of careful empirical studies of actual merger outcomes. For reasons that will be described, this evidence is not as strong as that with respect to the structural presumption, but it nonetheless raises questions about reliance on the safe harbor in approving mergers.

The distinctive contribution of this article rests on combining information on merger outcomes with data on the market structure conditions in each industry at the time of the merger. This provides the basis for testing the current structural presumption and safe harbor, as well as various alternative market structure-based rules, for their ability to accurately predict merger outcomes. This article proceeds as follows. The next two Parts provide brief histories of and rationales for the structural presumption and the safe harbor, respectively. Parts III and IV analyze the theoretical and empirical bases for reliance on market structure in the analysis of the effects of mergers. Part V is devoted to testing the current structural presumption and safe harbor, as well as alternatives, for their accuracy in distinguishing mergers that prove to be anticompetitive versus those that are in fact benign. These tests essentially measure Type I errors and offer some insights into Type II errors. Having established those criteria, Part VI compares data on actual challenges to mergers by one of the antitrust agencies with the implications of this analysis of the structural presumption and safe harbor.

This analysis demonstrates that there are indeed market structure characteristics strongly associated with mergers that prove to be anticompetitive. These define a structural presumption that can justifiably be relied upon to produce a more effective and efficient merger control policy. It further shows that reliance on a lower bound of concentration below which mergers should be approved may be misplaced, since there are numerous mergers below that bound...
that nonetheless result in competitive harm. Although the number of cases in
the database is not large, the evidence suggests the merits of close but careful
attention to market structure in the competitive analysis of mergers. The evi-
dence on actual case-bringing that concludes this article identifies an impor-
tant gap between agency practice with respect to mergers and the implications
of this research.

I. THE ORIGINS AND STATUS OF
THE STRUCTURAL PRESUMPTION

Although the “structural presumption” originated with the Supreme Court
opinion in Philadelphia National Bank,\(^3\) that term cannot be found there.
Rather, the court made the case for relying on market structure in a series of
observations about both procedure and substance. First, it observed that
whether “the effect of the merger ‘may be substantially to lessen competition’
. . . is not the kind of question which is susceptible of a ready and precise
answer in most cases” due to the predictive nature of the question. It went on
to urge that “in any case in which it is possible . . . to simplify the test of
illegality, the courts ought to do so in the interest of sound and practical judi-
cial administration.”\(^4\) Further, it opined that intense congressional concern
with the rising tide of concentration “warrants dispensing, in certain cases,
with elaborate proof of market structure, market behavior, or probable anticompetitive effects.”\(^5\) These observations were followed by the key prescrip-
tive statement that “a merger which produces a firm controlling an undue
percentage share of the relevant market, and results in a significant increase in
the concentration of firms in that market is so inherently likely to lessen com-
petition substantially that it must be enjoined in the absence of evidence
clearly showing that the merger is not likely to have such anticompetitive
effects.”\(^6\)

Shortly thereafter, the 1968 Merger Guidelines formalized something akin
to a structural presumption. Those guidelines asserted that in a highly concen-
trated market “the Department will ordinarily challenge mergers between

found in Peter C. Carstensen, The Philadelphia National Bank Presumption: Merger Analysis in

\(^4\) PNB, 374 U.S. at 362.

\(^5\) Id. at 363.

\(^6\) Id. Richard Posner, who, as Justice Brennan’s law clerk, wrote the opinion, has noted his
intellectual debt to Derek Bok’s scholarship on the Clayton Act. As described by Posner, Bok
emphasized the need for “simple rules for determining the legality of a challenged merger, or at
least a simple standard of presumptive illegality” rather than “multifactor tests,” which Posner
calls “a real blot on the American judiciary.” Philadelphia National Bank at 50: An Interview
firms accounting for” various stated market shares. This and analogous language for other categories of mergers that would “ordinarily” be challenged seemed to establish at least a weak presumption against such mergers.

The term “presumption” itself made its first appearance in the next version of the Merger Guidelines, in 1992. Mergers in highly concentrated industries that raised concentration by certain amounts were declared “likely to create or enhance market power or facilitate its exercise. This presumption can be overcome by a showing” of certain listed offsetting factors. Even more explicit language appears in the current 2010 Horizontal Merger Guidelines. In highly concentrated markets, a merger that causes a substantial increase in concentration now “will be presumed to be likely to enhance market power.”

Even as the structural presumption seemed to gain greater stated prominence, the numerical standards that trigger it became looser over time: whereas the 1992 Merger Guidelines distinguished low, moderate, and high concentration by Herfindahl-Hirschman Index (HHI) breakpoints of 1000 and 1800, the latest guidelines raised these to 1500 and 2500. They also raised to 200 the breakpoint for the increase in concentration that triggers the presumption, and to 100 the breakpoint for an increase that “potentially raise[s] significant competitive concerns and often warrant[s] scrutiny.” Other mergers are viewed as “unlikely to have adverse competitive effects and ordinarily require no further analysis.”

Controversy about the appropriate degree of reliance on market structure has attended this entire history. Critics of merger policy, and especially of structure-based policy, were quick to cite examples of apparently overzealous

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7 U.S. Dep’t of Justice, 1968 Merger Guidelines ¶ 5 (1968) [hereinafter 1968 Merger Guidelines], www.justice.gov/atr/merger/11247.pdf. The opinion in PNB called the legal result of high concentration an “inference,” making it sound as if it could only be rebutted by evidence “clearly showing that the merger is not likely to have such anticompetitive effects.” PNB, 374 U.S. at 363. But, over time, this burden-shifting framework has moved towards the presumption described in the Merger Guidelines and operates more like a traditional legal presumption. See, e.g., United States v. Anthem, Inc., 855 F.3d 345, 349 (D.C. Cir. 2017); United States v. Baker Hughes Inc., 908 F.2d 981, 991 (D.C. Cir. 1990) (citing United States v. Citizens & S. Nat’l Bank, 422 U.S. 86, 120 (1975)).


9 Id. (emphasis added).

10 U.S. Dep’t of Justice & Fed. Trade Comm’n, Horizontal Merger Guidelines ¶ 5.3 (2010) [hereinafter 2010 Merger Guidelines], ftc.gov/os/2010/08/100819hmg.pdf. It may be significant that these guidelines state only that such mergers are “presumed to be likely” rather than simply “presumed.”

11 Id.

12 Id.
enforcement, such as the infamous Von’s Grocery case, and argued strenuously against any such presumption. They emphasized that these cases were not harmless mistakes, but rather, they prevented efficiency-enhancing changes in market structure, harming consumers and competitors alike. As enforcement became more cautious, Congressman Peter Rodino decried what he viewed as the growing failure of policy to adhere to legislative intent, urging adherence to what he termed the “structural presumption.” Assistant Attorney General for Antitrust Charles James responded by citing approvingly the reluctance of the agencies and the courts “to condemn mergers based solely on evidence of high concentration, notwithstanding the structural presumption set forth in the 1963 Philadelphia National Bank case.”

Cautionary views found their way into the courts, notably with the 1990 Baker Hughes opinion and the 2001 Heinz opinion, both by the D.C. Circuit. For example, the court in Baker Hughes explained that while market structure might make a “convenient starting point,” merger analysis should be considerably more wide-ranging, in particular, evaluating efficiencies and entry as offsetting factors.

These criticisms served to further weaken the presumption and to increase the burden of proof faced by the agencies seeking to challenge specific mergers. Some observers went further yet, advocating the full and explicit rejection of the structural presumption. Perhaps most prominently, FTC Commissioner Joshua Wright characterized it as “an explicitly economic test that is no longer justified by modern economics” and dismissed it as a “convenient litigation tool to shift the burden to defendants when courts are not persuaded by a competitive effects story.”

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18 Baker Hughes, 908 F.2d at 984.
20 Joshua Wright, The Guidelines Should Be Revised to Reject the PNB Structural Presumption, TRUTH ON THE MARKET (Oct. 26, 2009), truthonthemarket.com/2009/10/26/the-guidelines-
Ginsburg asserted that they “can find no serious defense of the proposition that a PNB-like presumption reflects the best of modern economic thinking about mergers or that presuming the illegality of transactions above the [sic] any particular threshold is good economic policy for consumers.”\(^{21}\)

To be sure, the alternative approach that Wright would prefer—a case-specific, fact-based demonstration of competitive effects—represents the gold standard of merger analysis.\(^{22}\) Where that is feasible and cost-effective, no enforcer would opt for anything else, and indeed, the original PNB court viewed the structural presumption not as a shortcut to avoid analysis but as an appropriate policy tool precisely when such detailed analysis was impractical but the effects were nonetheless predictable.\(^{23}\) In keeping with this perspective, there always have been supporters of the presumption and critics of the notion that merger enforcement had become excessive. While noting a distinct weakening of the presumption, Jonathan Baker asserted it nonetheless remained “appropriate for courts and enforcers to rely upon, but only when it is not possible to provide a more compelling explanation of the mechanism by which competition would be harmed.”\(^{24}\) In his review of the impact of Chicago School economics on antitrust, Daniel Rubinfeld declared himself “troubled that the concern about false positives (bringing inappropriate cases) has tended to trump worries about false negatives (failing to bring appropriate cases).”\(^{25}\) Jonathan Baker and Carl Shapiro used survey responses to document the decline in merger enforcement and advocated “partially restoring the structural presumption.”\(^{26}\) And most recently, Peter Carstensen’s review and


\(^{23}\) Cost effectiveness plays an important role in determining how to proceed. More information about most mergers can be developed at ever-increasing cost, but if the incremental information scarcely affects the prediction of the merger outcome, such a pursuit would not be cost effective. Andrew I. Gavil, Burden of Proof in U.S. Antitrust Law, in 1 ABA Section of Antitrust Law, Issues in Competition Law and Policy 125, 129 (W. Dale Collins ed., 2008). More on this below.


\(^{26}\) Baker & Shapiro, supra note 19.
analysis lead him to argue for a return to the fundamentals of the structural presumption.27

The debate between these positions has been vigorous and occasionally enlightening. But as Sean Sullivan has rightly noted, “The opposing sides [of the debate] are simply at an impasse.”28 As noted previously, this article seeks to break that impasse by introducing something that has largely been lacking in the debate to this point, namely, actual evidence concerning the relationship of market structure conditions and merger outcomes. Before discussing that evidence, I examine the history and basis for the so-called safe harbor, the other structure-based presumption, the one that grants clearance for mergers falling below certain thresholds.

II. THE ORIGINS AND STATUS OF THE SAFE HARBOR

The policy history of the safe harbor for mergers is far shorter and less controversial than that for the conventional structural presumption. This term first made its appearance in the context of merger enforcement at the time of the 1982 Merger Guidelines. At the ABA Antitrust Section meetings that year just prior to the release of those guidelines, Attorney General William Smith and Assistant Attorney General for Antitrust William Baxter discussed the forthcoming major revision of the guidelines. While the new guidelines would be most famous for their new method for defining an antitrust market and for the introduction of the HHI as a measure of market concentration, Smith also noted that “[a]nother change from the old Guidelines will be our attempt to define relatively ‘safe harbors’ as well as conditions of special danger. For example, we are unlikely to challenge a horizontal merger if the post-merger concentration of the market would be less than 1,000 on the Herfindahl Index.”29

Later that year, after release of the new guidelines, Baxter stated that “we tried to carve out three regions where generalization was or was not possible. We identified what we will view as a ‘safe harbor’ region, namely, where the post-merger Herfindahl Index is below 1,000.”30 The concept of a safe harbor was not, however, entirely without controversy. Taken literally, the term safe harbor would seem to imply a true exemption rather than a rebuttable presumption. Baxter attempted to describe the strength of the presumption by further stating the following: “Heeding the admonition ‘never say never,’ we

27 Carstensen, supra note 3.
28 Sullivan, supra note 19, at 406.
30 William F. Baxter, New Merger Guidelines: A Justice Department Perspective, 51 ANTITRUST L.J. 287, 291 (1982). Interestingly, the PNB opinion contemplated a single breakpoint and effectively two zones, whereas the guidelines create a third, middle zone.
did not quite come out and say 'we will never attack mergers in that region.' But I truly expect it to be an unusual phenomenon where difficulty is found with a merger answering that description.”

Interestingly, officials at the FTC appeared to prefer somewhat greater discretion over mergers in that range. The FTC Director of the Bureau of Competition urged a “case-by-case” determination, a statement viewed as preserving a greater possibility of a challenge than literaly implied by the term “safe harbor.” Despite this equivocation about the exact meaning of the “safe harbor,” its role in fostering an expectation concerning the treatment of mergers at the lower end of the concentration spectrum did not raise much controversy, certainly not the controversy associated with the high-concentration structural presumption.

The original boundary of the safe harbor—an HHI of 1000—was raised to 1500 in the 2010 Merger Guidelines. Those guidelines stated that “[m]ergers resulting in unconcentrated markets are unlikely to have adverse competitive effects and ordinarily require no further analysis,” but offered no empirical justification for this statement or for the increase in the HHI threshold. Many viewed it as simply acknowledging actual agency practice—a de facto standard. This policy and its revision have been met with quiet acceptance.

The following two Parts examine the economic foundation for both the structural presumption and the safe harbor. That foundation has two pillars—theory and empirical evidence. I take these up in order.

III. THEORETICAL FOUNDATIONS OF THE ROLE OF MARKET STRUCTURE

Although the histories of the safe harbor and the structural presumption are quite different, the two presumptions share a common conceptual basis. Both describe rules for efficient antitrust decision-making with respect to mergers in the presence of imperfect foresight, asymmetric information, and the costs of case-by-case investigation. If, as a factual matter, all or almost all mergers in relatively unconcentrated markets, and especially between firms with modest shares, are competitively harmless, then a safe harbor for those mergers

[31] Id.
[33] 2010 Merger Guidelines, supra note 10, ¶ 5.3.
would avoid incurring the costs of a case-specific investigation—which in any event is unlikely to resolve all uncertainty. At the other extreme, if all or almost all mergers in highly concentrated markets, and certainly those involving firms with significant shares, pose near-certain competitive risks, then a structural presumption against such mergers would be equally meritorious.

This Part begins by reviewing some relevant theory that demonstrates why in the simplest of circumstances there might be a bright line between benign and harmful mergers. The basic logic is straightforward and well known: as concentration rises in an industry, at some point the behavior of the remaining firms reflects a growing sense of interdependence, stronger incentives to cooperate, and ever greater probability that those forces will overcome any instinct to act independently, that is, to compete.35

In reality, of course, there is no truly bright line, but rather a line made blurry by a multiplicity of factors relevant to equilibrium among the firms. It is for this reason that the 1982 Merger Guidelines added a third, intermediate range where structural considerations are relevant but not dispositive.36 Accordingly, the second section below offers three variations on the simple theory that illustrate the need to generalize the simple theory and the practical consequences of generalization. This is followed by sections that discuss the structural implications of unilateral effects modeling and then the decision theoretic basis for the structural presumption.

A. BENCHMARK MODEL

I begin with a standard economic model—that of homogeneous product Bertrand competition, in which firms compete using price.37 With \( n \) identical firms in the market, full cooperation would result in total profit of \( X \) per period, where \( X \) is also the amount that a monopolist would earn. All firms share in the profit equally, earning \( X/n \) in that period and in all future periods so

35 One of the early influences on the theory of collusion and the ability to model concentration with HHIs can be found in George Stigler, A Theory of Oligopoly, 72 J. Pol. Econ. 44, 55 (1964), and in GEORGE STIGLER, THE ORGANIZATION OF INDUSTRY 39 (1968). It should also be noted that this theory and support for market structure derive from concern with collusion rather than unilateral effects from merger.


37 For background, see, e.g., JEFFREY R. CHURCH & ROGER WARE, INDUSTRIAL ORGANIZATION: A STRATEGIC APPROACH (2000). This model describes coordinated effects. For an analogous development of the presumption based on both coordinated and unilateral effects, see Jonathan B. Baker, Market Concentration in the Antitrust Analysis of Horizontal Mergers, in 4 ANTITRUST LAW AND ECONOMICS 234 (Keith Hylton ed., 2010).
long as they adhere to this joint profit maximizing strategy. Discounted to the present at the rate \( r \), this yields a present value of profits as follows:

\[
\frac{X}{n} + \frac{X}{n} + \frac{X}{n} + \frac{X}{n} + ... = \frac{X}{n} \left( \frac{1}{1+r} \right) \left( \frac{1}{1+r^2} \right) \left( \frac{1}{1+r^3} \right) \frac{1}{r}
\]

(1)

Now suppose that instead of adopting this cooperative strategy, one firm lowers price below the level to which all others adhere. Consumers would migrate to the seller offering the cheapest price, so that firm would obtain all sales and profits in that period. The other \((n-1)\) sellers can be expected to react quickly and strongly. Suppose they respond with what is called the “trigger strategy”—producing so much in the next and all future periods that price is driven to unit cost and profit falls to zero for all time. The effect of this strategy is that the cheating firm, after obtaining the full monopoly profit \( X \) for one period, has to settle for zero profit thereafter. Its present discounted value of profits from this strategy is therefore simply \( X \) itself.

Which of these strategies this one firm pursues is determined by comparing the two profit streams. Specifically, it should cooperate so long as the present value of the stream of \((1/n)\)th of total profit \( X \) exceeds that full monopoly profit \( X \) in one period, that is:

\[
\frac{X}{n} > X \quad \text{(2a)}
\]

or simply \( 1/r > n \) \quad \text{(2b)}

Thus, for any given value of \( r \), there is a critical value of \( n \): for larger \( n \), the firm cheats, but below that value it suddenly finds it worthwhile to cooperate. The reason is that, when \( n \) is smaller, the firm’s present value of profit from cooperating is correspondingly larger—\(1/n\)th of the total—whereas for larger \( n \), the earnings from cheating become greater. I shall label this critical value of \( n \) at which cooperation becomes rational \( n^* \).

This simple model is instructive for a number of reasons. It emphasizes the fact that at some critical \( n^* \), firm behavior abruptly changes. Moreover, this happens strictly due to the firm’s self-interest: it is not responding to pressure from other firms, or a greater cooperative spirit, much less is it actually colluding. Its behavior changes simply because it is profitable for the firm to do so, and hence this change is fully rational—and predictable—when that critical value \( n^* \) is reached. Thus, a merger that would reduce the number of firms in the market from \( n^* + l \) to \( n^* \) would predictably result in cooperative pricing and excess profit, and such a merger would therefore be anticompetitive.

\[\text{38 The discount rate is specific to the firm, reflecting its own time-preference, and need not equal the market interest rate. The importance of this will be evident below.}\]
The implications of this demonstration for present purposes are clear: there is a safe harbor for mergers so long as \( n^*+1 \) or more firms remain, and there is also a specific merger that is distinctly anticompetitive. The bright line \( n^* \) divides the safe harbor from the zone where the structural presumption holds. Of course, this finding is simplistic in the extreme. It does not take account of a number of other factors. It suggests that a merger in an already concentrated market (e.g., going from \( n^*−1 \) to \( n^*−2 \)) as well as a merger in an unconcentrated market (i.e., with more than \( n^*+1 \) firms) will necessarily be harmless. And in any event, it does not give a precise answer to the crucial question: what is \( n^* \)? Nonetheless, its key insight—that structural change in the form of a change in firm numbers can by itself precipitate an abrupt shift toward cooperation and joint profit maximization—forms one cornerstone of the structural presumption in merger analysis.

B. FIRM ASYMMETRIES AND THE STRUCTURAL PRESCRIPTION

The above demonstration of the role of firm numbers is now familiar in policy as well as economics. Variations in the assumptions of this model highlight factors other than firm numbers that can give rise to merger-driven changes in pricing behavior. Three noteworthy variations depend on differences or asymmetries among the firms—differences in their discount factors, differences in their sizes, and differences in their initial behavior. Each variation may produce an anticompetitive merger above and beyond those that simply reduce the number of firms in the market from \( n^*+1 \) to \( n^* \). Firms in the supposed safe harbor as well as above the bright-line number that defines the structural presumption can be anticompetitive. Each of these scenarios deserves brief attention.

First, we note that Equation (2b) above solved for the critical number of firms at which cooperation takes root for any discount rate, but that condition can also be used to determine a critical value of the discount rate \( r \), given a fixed number of firms. Rearranging (2b) yields

\[
r^* < \frac{1}{n} \tag{3}
\]

Thus, if all firms are sufficiently patient (low values of \( r \)), then cooperation may emerge. Suppose, however, that one firm has a uniquely high discount rate, reflecting a higher value of upfront money than for other firms.\(^39\) That firm will lower price (“cheat”), forcing the other firms to respond, and the equilibrium may involve no profits to any firm. But a merger of that firm with any other will eliminate this firm’s incentive to lower price and can result in cooperation throughout the industry and competitive harm to consumers.

\(^39\) This could be due to urgent cash needs if the firm is facing financial difficulties or has limited access to capital markets.
A second scenario alters the assumption that all firms are of equal size. But each firm’s profit from cooperating is a proportional part of total monopoly profit \( X \), so that the smallest firm gains the least. That in turn makes it the most likely to break rank, lower price, and thereby prevent cooperation by the other firms. Again, supposing that this firm merges with any other, the merged entity will achieve a large size and may now have a stake in cooperation that results in a shift of the entire industry to the cooperative equilibrium.

A third example involves an asymmetry in the initial behavior of one of the \( n \) firms in the market. Suppose in particular that one firm has a business plan that makes it uniquely aggressive in the market, and as a disruptive influence, prevents cooperation among the remaining firms. Under these circumstances, a merger that absorbs that firm and replaces its aggressive behavior with that conforming to all other firms’ will generally result in a more cooperative industry equilibrium.\(^4\) By eliminating the competitive constraint previously imposed by the maverick firm, such a merger permits cooperation.

These three scenarios are variations on the model of coordinated conduct that was central to policy analysis and judicial views of mergers up through and well past the time of the \( PNB \) decision.

I now turn to a second and more recent competitive concern, namely, unilateral effects.

**C. Unilateral Effects and the Structural Presumption**

Beginning with the 1992 Merger Guidelines, merger analysis has also been concerned with unilateral competitive effects—the advantage to a single firm from acquiring a direct competitor even in the absence of coordination. That advantage takes the form of the gain in profit from merging and controlling prices of two substitute products. The mechanism is familiar: raising price on one of the products would previously have resulted simply in lost sales and profit, but now some fraction of those otherwise lost sales flow to the second product, adding to its profit and thereby strengthening its incentive to raise price.

Formally, this added incentive is a function of two factors. The first is the diversion ratio between the products of the two merging firms, denoted \( D_{12} \) and defined formally as

\[
D_{12} = \frac{\Delta q_1}{\Delta q_2}
\]

\(^4\) John E. Kwoka, Jr., *The Private Profitability of Horizontal Mergers with Non-Cournot and Maverick Behavior*, 7 *Int’l J. Indus. Org.* 403 (1989). In addition to changed behavior, the increased size of the merged firm also provides an incentive to raise price.
This measures the gain in sales of product 1 that results from the decrease in sales of product 2 due to its price increase. The product of $D_{12}$ and the margin on the first good, given by $(p_1 - c_1)$, represents added profit due to the merger. Since the diversion ratio measures changes in quantities, not levels or shares, this formulation would not seem to imply any particular number of firms, market shares, or concentration that are associated with significant competitive effects. Under certain circumstances, however, these can be shown to be related. Specifically, to the extent that all products are “equally close” in product space, sales diverted from product 2 may be assumed to go to all other products in proportion to their market shares. This is literally correct for certain demand formulations and often is taken as a useful first approximation, or perhaps even the best that can be done.\footnote{It is in fact directly implied by the logit form of demand, but that is only of several possible functional forms.} Under this assumption, the diversion ratio can be written as follows:

$$D_{12} = s_1 / (1 - s_2)$$  (5)

This formulation establishes a direct relationship between market shares and diversion.

Even that, of course, offers no guidance with respect to the relevant magnitudes. Some insight along these lines is contained in a report on merger investigations by the FTC. In discussing what constitutes a “significant competitor,” that report states:\footnote{FED. TRADE COMM’N, HORIZONTAL MERGER INVESTIGATION DATA, FISCAL YEARS 1996–2011 (2013) [hereinafter FTC, MERGER DATA 1996–2011], www.ftc.gov/sites/default/files/documents/reports/horizontal-merger-investigation-data-fiscal-years-1996-2011/130104horizontalmergerreport.pdf.}

When the primary concern was that the transaction would result in the exercise of unilateral market power, significant competitors include those firms identified as “close rivals” (even if they may not be close enough to constrain a price increase), as well as those that might reposition or otherwise affect the likelihood of an anticompetitive price increase.

A footnote to this passage adds, “These firms usually have market shares in excess of 10%, but market shares alone are not determinative of significance.”\footnote{Id. at 3 n.17.} Despite some ambiguity in the meaning of this passage, it seems clear that market shares can be related to diversion.\footnote{See the discussion of the potential relationship between diversion and shares in Carl Shapiro, The 2010 Horizontal Merger Guidelines: From Hedgehog to Fox in Forty Years, 77 ANTITRUST L.J. 49, 61–64 (2010).}
D. Decision Theory

In a significant new perspective, Steven Salop has recently cast the structural presumption in decision theoretic terms. As applied to antitrust rules, two propositions from decision theory are most relevant. First, any administrative rule with some probability results in errors. Second, that error rate may be reduced by some alternative procedure for examining specific cases, but at a cost. Thus, the optimal decision rule balances three factors: the probability of error, the cost of error, and the likely net benefits of further information gathering (i.e., through an investigation or trial). The probability of error depends on the size of the overlap in the distributions of likely benefits and likely costs. With little overlap, a strong presumption makes few errors and would be justified. This is essentially the case with respect to price-fixing agreements. With greater overlap in outcomes, a presumption might not be appropriate—unless, importantly, a fact-finding inquiry in the form of an investigation or trial does not lead to a significantly more accurate prediction in any specific case. Indeed, to the extent that a trial is unlikely to reduce the error probability much if at all, then the presumption might still be warranted.

Salop views the language of the PNB opinion through this lens. He recounts that courts’ statements that the precise effect of a specific merger is not “susceptible of a ready and precise answer in most cases,” that congressional intent should not be subverted by “permitting a too-broad economic investigation,” and hence that, where possible, the courts ought to “simplify the test of illegality” with a presumption. This line of reasoning closely follows decision theory: For mergers where general economic evidence is strong, and especially where case-specific evidence is weak or costly to develop, the judicial system might appropriately rely on a presumption based on shares and concentration. This approach would avoid policy paralysis due to an inability to “prove” the actual effect of a specific merger with characteristics found by the broader economic evidence to be nearly dispositive.

A crucial element in this approach is, of course, the probability of error. As noted at the outset, there are in fact two different errors that any rule or approach might make. Type I errors in merger control are errors of commis-


46 In reality, there are small exceptions to this per se rule. While these are motivated by the desire to lower the error rate, the exceptions may result in a large increase in costs of administration as many firms accused of illegal pricing interactions claim to be the exceptions. This disproportionate increase in administrative costs from small error rates played an important role in the Court’s PNB opinion.

sion—challenging a benign merger—while Type II errors are errors of omission, i.e., permitting anticompetitive consolidation. The key concern with respect to a structural presumption against certain mergers is Type I error, but the existence of Type I error is not itself an argument against a presumption. As Salop notes, if a presumption is correct 80 percent of the time, but a trial has a 25 percent chance of an erroneous decision, there is no reason to prefer the trial. In short, “The evidence should trump the presumption when the evidence is more reliable than the presumption, and the presumption should rule when the evidence is less reliable.” As a result, Salop concludes, a “hypothetical appellate court could . . . deal with the imperfection of evidence by mandating a per se rule that such mergers are impermissible when they fall into a category with such a high presumption of harmfulness, given the knowledge that the trial evidence has a significant potential for error.”

E. CONCLUSIONS FROM ECONOMIC AND DECISION THEORY

All of the previously discussed theories, as well as others, share the essential feature of the basic oligopoly model that market structure is an important determinant of pricing equilibrium in an industry. But variations in these theories demonstrate why there is no “magic number” of firms or market concentration, why mergers both above and below some apparent breakpoint can affect market equilibrium, and why there is an intermediate zone of concentration between the safe harbor and the boundary of the structural presumption. Because of these theoretical ambiguities, many have looked toward empirical evidence for confirmation of the importance of structure and for practical guidance for policy. The next part examines that evidence.

IV. EMPIRICAL FOUNDATION OF THE ROLE OF STRUCTURE

There is a long history of empirical research in economics that relates various aspects of market structure to performance—research that, as noted, has served as a foundation for structure-based merger enforcement. That research history has been reviewed elsewhere, so that only certain especially relevant aspects require attention here. Importantly, however informative that research has been on the role of market structure, most of it is not oriented to the

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48 Type II errors arise to the extent harmful mergers that do not exceed the thresholds of the structural presumption are cleared. Current merger control policy does not go that far; those falling below the share and concentration thresholds remain subject to antitrust review, although as noted, those well below the thresholds may enjoy something like a safe harbor.

49 Salop, supra note 45, at 292.

50 Id. at 282.


specific question of ranges for a safe harbor or for the structural presumption. It is only from the more recent research initiative known as merger retrospectives that reliable quantitative insights can be developed and specific structural criteria for the safe harbor and the structural presumption tested. I offer a brief outline of earlier work and then discuss the merger retrospectives literature in greater detail.

A. Concentration-Based Studies

The role of concentration in determining market performance is a longstanding question of interest to economics and to policy. Early research compiled and analyzed data from numerous industries on structural characteristics, such as market shares and concentration, and performance outcomes measured as price-cost margin or profits. The standard cross-sectional regression model to be estimated could then be represented as follows:

\[ \text{PERF}_i = a + b \times \text{CONC}_i + c \times X_i + e_i \] (6)

In this expression, \( \text{PERF} \) denotes a measure of noncompetitive performance, such as profit or price, \( \text{CONC} \) denotes an index of market concentration, \( X \) is a set of control variables for other factors that differ among observations, and \( e \) is the usual random error term. The results of such estimation typically reported a positive and statistically significant coefficient \( b \) on the concentration measure. Taken at face value, this would seem to imply that higher concentration results in above-competitive price or profit.

In its PNB opinion, the Supreme Court relied on such economic research—both case study and statistics-based—in articulating its presumption concerning the role of market structure in merger analysis. It cited several sources in support of its statement that there was “common ground among most economists” in the belief that “[c]ompetition is likely to be greatest when there are many sellers, none of which has any significant market share.”53 While that was indeed a correct statement of belief at the time, virtually simultaneously this line of empirical research came under escalating attack, the net effect of which was to erode this basis for concern over concentration.54

One of the key issues with the role of market structure in merger analysis was that the commonly used cross-sectional data did not actually measure the effects of mergers, but rather the effect of different levels of concentration across observations. The latter might differ for many reasons in addition to mergers, and their effects might not be identical. In addition, and perhaps relatedly, the statistical models often seemed to have weak predictive power.

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54 See, for example, the history of dissenting views in Ginsburg and Wright, supra note 21, at 382–86 and sources cited therein.
in specific cases. Explanations were not hard to find: potentially important factors other than those included in the regressions, and the fact that the statistical results represented “average” effects, with variation around those averages.

The net effect of this and many other criticisms was to shift attention away from large cross-sectional regressions of profits against concentration, to studies of particular markets where product price could be compared over time and sometimes also across geographic areas (e.g., cities). To a large extent, these single-industry price studies avoided many of the issues noted above, including those associated with profit measures, entry conditions, efficiencies, and other market-specific factors that confounded interpretation of cross-sectional studies. The downside, of course, is that any single industry or market study might only capture what was true about that one industry, and fail to provide adequate support for the general proposition that underlay the structural presumption.55

It is noteworthy, then, that these single-industry price studies showed much the same thing as did the older cross-sectional studies that had been so heavily criticized: greater concentration was still associated with greater market power and harm to consumers, now directly measured as higher price. It did not appear that the earlier statistical findings were simply an artifact of debatable methodology.56 That said, it remained true that the data used for a single-industry price study might or might not actually contain a merger episode, raising some question about the relevance of these findings for mergers and merger policy. For these reasons studies examining the effects of particular mergers began to draw more attention for merger policy.

B. MERGER RETROSPECTIVES

Merger retrospectives are detailed studies of the actual effects of individual mergers on outcomes of interest—mostly, price—controlling for other important influences. In principle, these might involve a range of alternative methodologies for isolating the effects of mergers, although most relied upon a technique commonly known as difference-in-differences (DID). This method can be explained as follows: Supposing that the focus of attention is on the price effect of a particular merger, data on price before and after the merger

55 This literature is typified by the very large number of studies relating concentration to prices charged by airlines, in banking markets, and by supermarkets, among others. Good summaries of the broader literature can be found, inter alia, in Church & Ware, supra note 37; Martin, supra note 52.

56 For this reason, the Ginsburg and Wright conclusion—that there is no support for a structural presumption because the traditional profit-concentration framework is invalid—does not follow. Other methodologies, not subject to their concerns, find similar results. Ginsburg & Wright, supra note 21, at 384.
correctly capture that change only if no other relevant factors also change. In general, of course, that is unlikely to be the case and certainly cannot be assumed. The DID methodology controls for those other factors by also compiling data on the price of an otherwise similar product over the same time period, but—crucially—not affected by the merger. In this context “otherwise similar” means a product that is subject to the same demand, cost, and other forces as the product in question. The difference in the price of the product of the merging firms before and after their merger can be adjusted for the difference in the price of the “otherwise similar” product in order to get the net effect of the merger.  

Assuming the control product is correctly chosen and with attention to certain other considerations, the DID technique can provide good evidence of the effect of a merger on price or other dimensions of performance without the need for elaborate modeling, collection of data on control variables of no special interest, and choice of functional forms. It has become the preferred technique for ex post merger analysis, and over the past 30 years dozens of merger retrospectives have been published.

There are two recent noteworthy compilations of the evidence about the outcomes of mergers from such retrospectives, those by Orley Ashenfelter, Daniel Hosken, and Matthew Weinberg, and by this author. The focus of Ashenfelter et al. is on the influence of Robert Bork’s treatise, The Antitrust Paradox, on merger enforcement, and whether retrospective studies bear out Bork’s argument that enforcement has been too stringent. Their assessment of this argument is based on summaries of some 49 merger retrospectives and similar studies of merger outcomes. They do not attempt to make these various estimates comparable, or to aggregate estimates from multiple studies of

57 If, for example, the pre-merger price of a product is 100 and its post-merger price is 110, DID would examine the price change for an otherwise similar product unaffected by the merger. If this latter price rose by 7%, then the price increase properly attributed to the merger would be the difference, or 3%.

58 For detailed discussion, see John Kwoka, Mergers, Merger Control, and Remedies, A Retrospective Analysis of U.S. Policy 9–38 (2015) [hereinafter Kwoka, Mergers, Merger Control, and Remedies]. Gregory Werden raises several of these issues, ultimately concluding that merger retrospectives are inferior to full-information case studies done by the antitrust agencies. Gregory J. Werden, Inconvenient Truths on Merger Retrospective Studies, 3 J. Antitrust Enforcement 287 (2015). But reliance on the latter encounters budget constraints, less interest in past than present cases, and an aversion to self-criticism—presumably the very reasons that few such case studies have in fact emerged from the agencies.


60 I say “similar” since Ashenfelter et al. include some studies that do not have actual post-merger data but rather project post-merger price and from that seek to infer the effects of mergers. For reasons discussed in Kwoka, this approach may not produce estimates of the same reliability. Kwoka, Mergers, Merger Control, and Remedies, supra note 58. In addition,
the same merger, or to separate out individual merger effects where a study may have reported on multiple mergers. Instead, they rely on the fact that 36 of these 49 studies conclude that the merger or mergers they investigated resulted in higher prices, while 13 did not. Based on this evidence, the authors conclude that while Bork’s argument for shifting enforcement away from some of its earlier practices had merit, at the current enforcement margin, his policy recommendations “may have been too permissive.”

A more recent and comprehensive survey of the merger retrospective literature is my 2015 compilation, *Mergers, Merger Control, and Remedies.* The screening procedures and comparability standards make this the largest database of carefully controlled studies of the effects of mergers, and it is this source I rely upon here. The procedure I used involved initially identifying all published merger retrospectives and then imposing objective criteria for inclusion. It limited attention to those studies that examined horizontal mergers or other horizontal transactions involving U.S. companies and markets, that used a technique such as difference-in-differences meeting modern standards of research design, and that appeared in a refereed journal or respected working paper series. These criteria reduced the number of studies from several hundred to about 60. These covered almost 50 distinct transactions—some studies covered the same transaction—and nearly 120 individual product prices. For present purposes, the database consists of 42 actual mergers, plus four airline code-shares and three petroleum industry joint ventures.

It will be instructive for later purposes to briefly report a few of the most relevant results from my analysis. First, the mean effect from all transactions was found to be a 5.8 percent price increase. Recall, of course, that after controlling for other factors, as DID does, a benign or successfully remedied transaction should show zero net effect. That is clearly not the case. For true mergers, the effect was larger yet—7.2 percent—and of these, more than 80 percent resulted in price increases, with increases averaging more than 10 percent. Non-mergers—joint ventures and code-sharing—seemed to have no net effect in either direction, although their numbers in the database were too small for confident judgments. The implication of these results is that most carefully studied mergers are found to be anticompetitive, often to a substantial degree.

Ashenfelter et al. include retrospective studies of non-U.S. mergers. Whether their economic and institutional setting makes them sufficiently comparable is an open question.

61 Ashenfelter et al., *supra* note 59, at S96.
62 KWOKA, *MERGERS, MERGER CONTROL, AND REMEDIES,* *supra* note 58.
63 A companion set of studies of groups of mergers covered a total of more than 3000 mergers, but due to its more aggregated nature, it is not useful for present purposes.
64 To be clear, this assessment is based on price effects, not quality or other performance outcomes. A subset of these data has been used to model the accuracy of the stated Merger
C. Conclusions from Empirical Research

The findings of individual merger retrospectives and from compilations of retrospectives corroborate the results of traditional empirical work examining the statistical relationship between market concentration and prices. Despite limitations of each approach, both support the proposition that mergers can and often do result in competitive harm. The merger retrospectives database can be used to examine the question of the degree to which reliance on “low” concentration to conclude that a merger poses no competitive problems is correct, and also the question of whether greater reliance on market concentration would correctly identify competitively harmful mergers or, as critics allege, such a presumption would have incorrectly captured many benign mergers. To that issue I now turn.

V. Testing the Safe Harbor and the Structural Presumption

This article has recounted the longstanding interest in a structure-based rule for identifying competitively problematic mergers and competitively benign mergers. It has also reviewed the theoretical considerations and empirical evidence that suggest the rationality and possibility of such rules. What remains is to use the merger retrospective database to test alternative possible structure-based standards. I examine numerous thresholds—both existing and possible alternatives—for their empirical validity. By “empirical validity,” I mean the ability of each threshold to correctly predict whether the mergers in the database ultimately prove to be benign or anticompetitive. Some errors are inevitable, of course. I am more concerned with both the absolute rate of error and also with how that rate varies with alternative concentration-based rules. Once this array of criteria and error rates is constructed, one can decide what thresholds for the structural presumption and for the safe harbor yield sufficiently high rates of correct predictions to warrant reliance for policy purposes.

I first describe in greater detail the sources of these data on merger outcomes, and then proceed with the testing. At the end I will bring some additional evidence into the picture.

A. Sources of Data

As noted previously, merger retrospectives provide information on outcomes of specific mergers. Here I draw on the compilation of such retrospec-

tives described in Kwoka and Gu. This consists of a total of 40 mergers analyzed in one of these retrospectives. For each, the price outcome was recorded according to the protocol previously described. The other major piece of necessary data is market concentration at the time of the merger. Such data are sometimes—but not often—reported in the merger retrospectives studies themselves. In other cases concentration data are drawn from documents filed by the antitrust agencies as part of their investigations or merger challenges. But where there is no challenge and hence no such filing, information about the actual concentration at the time of a merger had to be developed from a variety of public sources.

That process begins by identifying the likely markets in which the operations of the merging firms overlap. For example, in airlines these would be routes or hubs; in journals, fields and specialties; in petroleum retailing, gasoline types and locations; in upstream petroleum operations, primarily locations. After these product and geographic markets are distinguished, data on each are compiled from available public sources and then concentration measures at the time of the merger are constructed. Thus, the DB1B database published by the Department of Transportation is used to capture route and hub concentration for each specific airline merger. In the case of petroleum refining, the Energy Information Administration’s Refinery Capacity Reports provide the necessary data. For gasoline retaining, some concentration data have been published by the Federal Trade Commission, and so forth.

The result of this process is a compilation of various concentration measures associated with each of the 40 markets where mergers were the subjects of retrospective studies of outcomes. These measures are as follows:

- HHI, the now-standard measure of market concentration. HHI is the sum of squared market shares of all firms in the market, and for purposes of merger analysis is calculated for the post-merger market.

- Delta, denoting the change in HHI that would be caused by the merger. As is well known, Delta is given by twice the product of the pre-merger shares of the merging firms.

\[^{65}\text{Id.}\]

\[^{66}\text{All measures of concentration are dependent on reasonably accurate market definitions. For these reasons as well as others previously noted, reliance on concentration and the structural presumption makes more sense when competitive concerns with a merger are with coordinated conduct rather than unilateral effects.}\]

\[^{67}\text{All primary sources are cited in Kwoka & Gu, supra note 64.}\]
B. THE STRUCTURAL PRESUMPTION PUT TO THE TEST

I begin by testing the structural presumption stated in the 2010 Merger Guidelines, namely, that a merger resulting in an HHI of 2500 or more, and increasing HHI by at least 200 points, should be “presumed” to create or enhance market power. The key question here is whether that presumption is an accurate guide to anticompetitive mergers or whether it makes so many Type I errors—capturing benign mergers—as to invalidate it as a predictive devise. I now proceed to test this and other criteria.

By actual count, there are 21 mergers in the database that satisfy this 2500/200 category. Of these, 18 mergers were found to have resulted in post-merger price increases, while three were reported to have resulted in decreases. Thus, a structural presumption based on the 2500/200 criteria produces a correct price prediction 85.7 percent of the time—that is, in six out seven cases.69 Put differently, this criterion has a Type I error rate of less than 15 percent. This would seem to qualify the Merger Guidelines presumptive standard as a useful, informative tool for purposes of analyzing mergers for their competitive effects.70

Importantly, I note that this percentage is outside the statistical bounds of randomness. A means proportion test establishes that for this number of observations, 85.7 percent is statistically different from a 50/50 random split at the .999 level, leaving little doubt about its informational value. Of course, 85.7 percent is not 100 percent, but it should be recalled that this is a presumption, not a conclusive determination by the antitrust agency.71 As such, it

• Number of significant competitors, where a significant competitor is “a firm whose independence could affect the ability of the merged firms to achieve an anticompetitive outcome.”68

68 FTC, MERGER DATA 1996–2011, supra note 42. By convention, this count of significant competitors includes the merged firm. No share or other criterion is given for identifying a “significant competitor.” The 2010 Merger Guidelines specifically provide for use of the number of significant competitors as an alternative measure of market concentration. 2010 Merger Guidelines, supra note 10, at 18.

69 It should be noted that mergers can have anticompetitive effects above and beyond price increases, but for reasons I have discussed in earlier work, there are many fewer retrospectives investigating these non-price effects. Kwoka, Mergers, Merger Control, and Remedies, supra note 58.

70 Salop, for example, describes a rule that correctly identifies anticompetitive mergers 80% of the time as “a very strong anticompetitive economic presumption.” Salop, supra note 45, at 289.

71 Indeed, of the three mergers that lie above the 2500/200 criterion but do not result in price increases, two were cleared after investigation by the reviewing agency. This suggests that the structural screen, supplemented by agency scrutiny, provides a sequential process resulting in a yet higher rate of correct prediction—20 out of 21, or 95.2%. This view of the merger process and outcome is discussed more fully in Kwoka and Gu. Kwoka & Gu, supra note 64. A critical comparison would be between the error rate from the structural presumption versus that from the
is a criterion that establishes a high likelihood, and therefore appropriately shifts the burden to the respondents to rebut. This interpretation is consistent with Salop’s decision theoretic interpretation of a structural presumption, here buttressed by data indicating the actual rates of correct vs. incorrect prediction.

There are certain features of these data and results that should be borne in mind in drawing inferences. For one thing, these mergers are not a random sample of all mergers with these characteristics that come before the agency. These are mergers that have been carefully studied, often because they are at the enforcement margin and hence of special interest. To that extent, of course, they are arguably very much the set of experiences that should be analyzed, yet for present purposes their incomplete coverage should be borne in mind. Second, the mergers in the database have all in fact been evaluated by the agencies and in some instances already subject to remedies. Accordingly, they should exhibit smaller (if any) price increases, since neither cleared mergers nor those subject to effective remedies should result in net price increases. Third, the number of observations is not especially large, and so increasing the sample size is a high priority for purposes of reliance on these conclusions. And finally, observations in the database come disproportionately from certain industries. This does not, however, bias the summary measures: the mean effect across industry averages is 7.1 percent, whereas across observations it is 7.2 percent.72

An interesting further question concerns the outcomes of alternative criteria for the structural presumption. This question, too, can be answered with this database. For example, the more stringent criterion of 3000/200—that is, a post-merger HHI of 3000 with a change of 200—excludes a total of four mergers, three of which resulted in price increases. But the excluded cases slightly increase the success rate of the higher threshold, to 88.2 percent. As shown in Table 1, a yet higher threshold—3500/200—yields a higher success rate of 92.9. At 4000/200, this drops slightly to 90 percent—still high, by any measure. As would be expected, success rates generally rise and Type I error rates fall as the standard for the presumption increases.

72 Moreover, it is unclear why the outcome of all mergers in one specific industry should have similar effects, since they often occur at quite different times. One might expect at least as much similarity in outcomes among mergers occurring at the same time regardless of industry, perhaps due to agency or administration effects, but that does not appear to be the case either.
TABLE 1: PERCENT OF MERGER OUTCOMES CORRECTLY DETERMINED BY HMG CRITERIA

<table>
<thead>
<tr>
<th>HHI Threshold</th>
<th>Number of Mergers Exceeding Threshold</th>
<th>Number Anticompetitive</th>
<th>Number Procompetitive</th>
<th>Percent Correct</th>
</tr>
</thead>
<tbody>
<tr>
<td>4000</td>
<td>10</td>
<td>9</td>
<td>1</td>
<td>90</td>
</tr>
<tr>
<td>3500</td>
<td>14</td>
<td>13</td>
<td>1</td>
<td>92.9</td>
</tr>
<tr>
<td>3000</td>
<td>17</td>
<td>15</td>
<td>2</td>
<td>88.2</td>
</tr>
<tr>
<td>2500</td>
<td>21</td>
<td>18</td>
<td>3</td>
<td>85.7</td>
</tr>
<tr>
<td>2000</td>
<td>24</td>
<td>21</td>
<td>3</td>
<td>87.5</td>
</tr>
</tbody>
</table>

This series of tests supports the proposition that the structural presumption in fact correctly identifies mergers with a very high likelihood of post-merger price increases. The results in Table 1 underscore that conclusion for a 2500/200 criterion and for any more stringent criterion. A further question is whether that criterion for the structural presumption might be too stringent. I therefore test the criterion 2000/200, and find that 21 of the 24 mergers satisfying this lower threshold in fact prove to be anticompetitive. The success rate for this threshold is 87.5, little different—in fact slightly greater—than that for 2000/200 due to the incremental mergers captured by the lower bar. This certainly suggests that the current 2500/200 may be, if anything, excessively high for an equally high degree of correct identification of competitively problematic mergers.

To cast further light on the properties of the structural presumption, I test the predictive value of one of its components by itself, namely, the level of HHI by itself. Omitting Delta disregards the shares of the merging parties and would essentially entail a presumption against any mergers simply in that region of HHI. To be clear, no such rule has ever been part of the Merger Guidelines, so this is not offered as an alternative but rather as an indication of the sensitivity of the results to the criterion involving the merging parties’ shares. The results of this analysis are shown in Table 2. As one might expect, this looser criterion results in the inclusion of some additional benign mergers, so that its success rate is generally incrementally lower (and Type I error rate higher) than when Delta is included. This appears to be especially the case for mergers in the moderate-to-high region, i.e., those with HHIs up to 3000. The data makes clear that the Delta factor is important in correctly identifying anticompetitive mergers.
TABLE 2: 
PERCENT OF MERGER OUTCOMES CORRECTLY DETERMINED BY HHI

<table>
<thead>
<tr>
<th>HHI Threshold</th>
<th>Number of Mergers Exceeding Threshold</th>
<th>Number Anticompetitive</th>
<th>Number Procompetitive</th>
<th>Percent Correct</th>
</tr>
</thead>
<tbody>
<tr>
<td>4000</td>
<td>10</td>
<td>9</td>
<td>1</td>
<td>90</td>
</tr>
<tr>
<td>3500</td>
<td>14</td>
<td>13</td>
<td>1</td>
<td>92.9</td>
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<tr>
<td>3000</td>
<td>17</td>
<td>15</td>
<td>2</td>
<td>88.2</td>
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<tr>
<td>2500</td>
<td>23</td>
<td>19</td>
<td>4</td>
<td>82.6</td>
</tr>
<tr>
<td>2000</td>
<td>27</td>
<td>23</td>
<td>4</td>
<td>85.2</td>
</tr>
</tbody>
</table>

Additional tests of the structural presumption can be performed on an alternative measure of merger-related concentration, namely, the number of remaining significant competitors. Using the same set of data on mergers, each merger can now be categorized according to the number of remaining competitors of significant size in its market or markets. This number ranges from two upwards in this database. Table 3 displays this array, with the corresponding number and percent of mergers in each category that proved to be correctly identified (i.e., anticompetitive) by that criterion.73 As is evident, there is rather clear break in this sequence in the range of five to six remaining competitors: Indeed, in this database there are no benign mergers with five or fewer remaining competitors, although obviously one should not conclude that such can never actually be the case.74 For mergers with six remaining competitors, four of the five additional mergers are anticompetitive. This 80 percent accuracy on the incremental mergers lowers the overall rate of correct prediction using the criterion of six or fewer remaining competitors to 94.7 percent (18 out of 19 mergers)—still very high. Table 3 also shows that the error rate for predictions rises steadily for mergers resulting in seven or more competitors, as of course, it logically should.

73 These data are reported in a slightly different form than with respect to HHI. Rather than cumulating all mergers up to some particular threshold, the data here are for the specific mergers that fall into each category (e.g., precisely four remaining competitors, rather than up to four remaining competitors). This alternative provides a sharper focus for these data, but of course these are the same underlying observations.

74 The reasons include the modest number of such cases in the merger retrospectives database and the observation of mergers in that range that, from public accounts, would seem benign.
TABLE 3: PERCENT OF MERGER OUTCOMES CORRECTLY DETERMINED BY EACH ADDITIONAL REMAINING SIGNIFICANT COMPETITOR

<table>
<thead>
<tr>
<th>Incremental Significant Competitor</th>
<th>Count of Mergers</th>
<th>Number Anticompetitive</th>
<th>Number Procompetitive</th>
<th>Percent Correct</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1</td>
<td>1</td>
<td>0</td>
<td>100</td>
</tr>
<tr>
<td>2</td>
<td>2</td>
<td>2</td>
<td>0</td>
<td>100</td>
</tr>
<tr>
<td>3</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>4</td>
<td>4</td>
<td>0</td>
<td>100</td>
</tr>
<tr>
<td>5</td>
<td>7</td>
<td>7</td>
<td>0</td>
<td>100</td>
</tr>
<tr>
<td>6</td>
<td>5</td>
<td>4</td>
<td>1</td>
<td>80</td>
</tr>
<tr>
<td>7</td>
<td>4</td>
<td>2</td>
<td>2</td>
<td>50</td>
</tr>
<tr>
<td>8</td>
<td>3</td>
<td>1</td>
<td>2</td>
<td>33.3</td>
</tr>
</tbody>
</table>

Since this database of studied mergers includes an overall high percentage of anticompetitive outcomes, a question might arise as to whether the proportion of such outcomes for mergers above the threshold of six significant competitors is in fact different from the proportion for mergers with seven or more remaining competitors. As noted above, the percentage of correct predictions for mergers with six or fewer competitors is 94.7. For mergers resulting in seven or more significant competitors, that percentage drops to 65.0. A two-sample means proportion test finds that, for these numbers of observations, the proposition that these percentages are statistically identical can be rejected at the .978 level. This makes clear that significantly different outcomes occur above and below that numerical threshold.

There are noteworthy aspects to these findings with respect to significant competitors. For one, these data have much the same implication as the HHI-based criteria. An industry with six or five significant competitors logically must have HHIs in excess of 1667 to 2000, and in these data such cases have actual HHIs that are somewhat larger. The Type I error rates are roughly similarly low: 12 to 14 percent for these HHI values, and no more than 5 percent based on competitor numbers. The implication seems clear: Both a structural presumption based on 2500/200 and one based on five or six remaining significant competitors correctly identify mergers posing a high risk of anticompetitive outcomes.

These results directly address the concern over Type I errors that have eroded confidence in and use of the structural presumption. In these data, despite their limited number, that error appears to be no greater than 15
percent, and in some formulations, less than 5 percent. While views may differ on what represents an acceptable error rate, these values would not seem to support a skeptical view of the presumption. Rather, they would seem to corroborate the informational value of market structure and support the usefulness of a presumption for policy purposes. Finally, I note that the use of a structural standard based on significant competitors generally has a lower Type I error rate than the HHI-based standard in these data. That advantage, as well as the virtue of a presumption in general, is of course dependent on the precision of the concept of a significant competitor and the measurement of concentration, both in turn based on reasonable definitions of the market.

C. The Safe Harbor Put to the Test

As has been discussed, the other structure-based presumption is the safe harbor for mergers arising in relatively unconcentrated industries. The current Merger Guidelines set out an HHI of 1500 for the value of post-merger HHI below which mergers “are unlikely to have anticompetitive effects and ordinarily require no further analysis.” As noted, this represents an increase from the previous bound of an HHI of 1000. I begin the analysis by examining both of these bounds.

Preliminarily, I note that the mergers in this safe harbor that have been studied in the retrospectives literature represent a very small fraction of all mergers in that range that have occurred. This fact is of greater concern to my conclusions than in the context of the structural presumption, since with respect to the safe harbor the selection bias inhibits the ability to draw conclusions. The reason is that the carefully studied mergers in markets with low HHI values are very likely to be those raising competitive concerns. Consequently, it would be incorrect to take the proportion of accurate predictions to the total number of studied mergers in that range and conclude anything about the empirical validity of that boundary.

That said, it may still be possible to draw some cautious inferences. For example, to the extent that a significant number of mergers satisfying the safe harbor conditions nonetheless prove to result in price increases, one can conclude that those conditions should not in fact be relied upon as entirely sufficient to dispose of policy concerns. A true safe harbor should result in few if any qualifying mergers resulting in price increases, but again, without knowing the total number of such mergers to which those should be compared, I am unable to characterize the frequency of such outcomes.

With those caveats, the outcomes of studied mergers that qualify for the “safe harbor” in the 2010 Merger Guidelines are nonetheless of some interest. In the present data set there are nine mergers where the post-merger HHI is less than 1500. Of those nine, seven mergers resulted in price increases and
only two showed the absence of increases that might be expected of mergers with HHIs below 1500. It should be emphasized that it is not correct to conclude that 78 percent of all mergers in this HHI range are anticompetitive. Rather, it is the fact that researchers had little difficulty in finding anticompetitive mergers in the supposed safe harbor. This observation is corroborated by data in Table 3 on the number of significant competitors. Those suggest that adverse effects in the form of higher prices arise in several cases with medium to relatively large numbers of such competitors.

Overall, these results suggest that if the so-called safe harbor is to be viewed as a presumption, it should be viewed only as a weak and rebuttable presumption that a merger is unlikely to result in a price increase. There is no empirical basis in these data for treating it as a stronger presumption, although in practice that may not in any event be the case.

VI. MERGERS, MARKET STRUCTURE, AND POLICY

The above analysis of the merger retrospectives database has shown that the vast majority of mergers resulting in six or fewer significant competitors or, roughly equivalently, those in markets exceeding a 2500/200 HHI standard, have anticompetitive consequences. In addition, even mergers in markets where HHI falls below 1500 are shown, with some frequency, to result in price increases. Taken at face value, the first finding suggests a low Type I error rate from the structural presumption, while the second implies a potentially higher Type II error rate from the present safe harbor. These results in turn would imply the following policy prescriptions: mergers resulting in six or fewer remaining significant competitors should be presumed anticompetitive, with a very high burden of proof to the contrary. Mergers in markets with HHI below 1500 are likely benign, but the frequency of anticompetitive cases justifies close scrutiny of these.

It is therefore interesting to compare the implications of these findings for merger policy with actual enforcement practice. This comparison is made possible by the data on merger-related activity periodically reported by the Federal Trade Commission. These data consist of the number of second requests issued and the number of “enforcement actions” taken, arrayed according to HHI, its change, and the number of remaining significant competitors. The term “enforcement actions” encompasses formal challenges to mergers but is

75 Reliance on yet lower bounds, such as 1200 or even 1000, does not improve matters, in part because the number of observations at those levels becomes quite limited.

76 FTC, MERGER DATA 1996–2011, supra note 42. For corroborative data focused on unilateral effects models, see Malcolm B. Coate, Benchmarking the Upward Pricing Pressure Model with Federal Trade Commission Data, 7 J. COMPETITION L. & ECON. 825 (2011).
also said to include instances in which the parties abandon their proposed merger in the face of an announced or likely challenge.

The first data release covered the years 1996–2003, and then was updated to cover the periods from 1996 up through 2005, 2007, and 2011. In order to capture the intensity and focus of agency activity, I report the percent of investigations in each structural category that result in enforcement actions. Table 4 reproduces data for the entire 1996–2011 period. They show that the percent of Second Requests that resulted in enforcement action averages about 77–78 percent for this entire period, rising with post-merger HHI, with the change in HHI, and with fewer remaining significant competitors. These results are fully expected. What is of greater interest for present purposes emerges from disaggregating these data into four time periods: 1996–2003, 2004–2005, 2006–2007, and 2008–2011.\(^77\) I do this for HHI brackets and separately for each number of significant competitors.

**TABLE 4:**

**RECENT MERGER INVESTIGATIONS RESULTING IN ENFORCEMENT ACTIONS FTC 1996–2011**

<table>
<thead>
<tr>
<th>By HHI (%)</th>
<th>By Delta-HHI (%)</th>
<th>By Number of Significant Competitors (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0–1,799</td>
<td>40.0</td>
<td>2 to 1</td>
</tr>
<tr>
<td>1,800–1,999</td>
<td>59.6</td>
<td>3 to 2</td>
</tr>
<tr>
<td>2,000–2,399</td>
<td>58.1</td>
<td>4 to 3</td>
</tr>
<tr>
<td>2,400–2,999</td>
<td>71.7</td>
<td>5 to 4</td>
</tr>
<tr>
<td>3,000–3,999</td>
<td>71.5</td>
<td>6 to 5</td>
</tr>
<tr>
<td>4,000–4,999</td>
<td>87.4</td>
<td>7 to 6</td>
</tr>
<tr>
<td>5,000–6,999</td>
<td>89.5</td>
<td>8 to 7</td>
</tr>
<tr>
<td>7,000+</td>
<td>99.0</td>
<td>9 to 8</td>
</tr>
<tr>
<td>Total</td>
<td>77.6%</td>
<td>Total</td>
</tr>
</tbody>
</table>

Source: FTC, Merger Data 1996–2011, supra note 42.

### Table 5: Enforcement Actions as Percent of Investigations Based on HHI FTC Data by Time Intervals

<table>
<thead>
<tr>
<th></th>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>0–1,799</td>
<td>52.9</td>
<td>4.3</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>1,800–1,999</td>
<td>70.8</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>2,000–2,399</td>
<td>67.7</td>
<td>40.0</td>
<td>10.0</td>
<td>22.2</td>
</tr>
<tr>
<td>2,400–2,999</td>
<td>79.3</td>
<td>41.7</td>
<td>54.5</td>
<td>0.0</td>
</tr>
<tr>
<td>3,000–3,999</td>
<td>77.4</td>
<td>23.1</td>
<td>59.5</td>
<td>80.6</td>
</tr>
<tr>
<td>4,000–4,999</td>
<td>90.9</td>
<td>50.0</td>
<td>81.3</td>
<td>100.0</td>
</tr>
<tr>
<td>5,000–6,999</td>
<td>85.2</td>
<td>91.3</td>
<td>87.5</td>
<td>96.1</td>
</tr>
<tr>
<td>7,000+</td>
<td>97.1</td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
</tr>
<tr>
<td>Total</td>
<td>77.8</td>
<td>71.4</td>
<td>70.8</td>
<td>88.5</td>
</tr>
</tbody>
</table>

Source: FTC, Merger Data 1996–2011, supra note 42.

Table 5 and Figure 1 present these data by HHI bracket. While there is obviously some variation, beginning as early as early as 2004–2005 and then accelerating dramatically after 2007, merger enforcement activity by the FTC noticeably shifted. For mergers in markets with HHIs in excess of 3000, the percent of Second Requests leading to challenges in fact increased. The overall average enforcement rate for all investigations in markets with HHIs at or above this level rose from 83.1 percent in 1996–2003 to 94.6 percent by 2008–2011.

In sharp contrast, however, the frequency of actions against mergers in all markets with HHIs less than 3000 began declining with the 2004–2005 period and by 2008–2011 had dropped to literally zero for three of the four HHI brackets up to HHI = 3000. The sole exception was for mergers with HHIs between 2000 and 2399, for which a small fraction—about 22 percent—of investigated mergers continued to result in enforcement actions. In the other three brackets of HHI up to 3000, there were no enforcement actions whatsoever against mergers in the 2008–2011 period (the last for which data are available). The average rate of enforcement for all investigated mergers having HHIs less than 3000 was 11.8 percent in the last period, a precipitous decline from 67.4 percent in 1996–2003.
Merger enforcement activity with respect to the number of significant competitors reveals the same change in agency enforcement practices, perhaps even more dramatically. Table 6 and the graph in Figure 2 document the fact that for most mergers that would result in four or fewer significant competitors, agency actions in fact increased in frequency, much as for mergers with high HHIs. But for all mergers resulting in more than four significant competitors, enforcement rates fell from the earliest period up through 2004–2005, and then dropped precipitously. By 2008–2011 there were literally no enforcement actions taken against any mergers that resulted in five or more significant competitors. This evidence suggests a de facto safe harbor for all such mergers.
TABLE 6:
ENFORCEMENT ACTIONS AS PERCENT OF INVESTIGATIONS
BASED ON NUMBER OF REMAINING SIGNIFICANT COMPETITORS
FTC DATA BY INTERVALS

<table>
<thead>
<tr>
<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>96.2</td>
<td>100.0</td>
<td>100.0</td>
<td>98.4</td>
</tr>
<tr>
<td>2</td>
<td>84.8</td>
<td>89.3</td>
<td>94.7</td>
<td>95.7</td>
</tr>
<tr>
<td>3</td>
<td>76.1</td>
<td>50.0</td>
<td>86.7</td>
<td>91.9</td>
</tr>
<tr>
<td>4</td>
<td>61.5</td>
<td>57.1</td>
<td>69.2</td>
<td>72.7</td>
</tr>
<tr>
<td>5</td>
<td>40.6</td>
<td>28.6</td>
<td>44.4</td>
<td>0.0</td>
</tr>
<tr>
<td>6</td>
<td>20.0</td>
<td>16.7</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>7</td>
<td>50.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>8</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>Total</td>
<td>77.0</td>
<td>78.7</td>
<td>70.8</td>
<td>89.0</td>
</tr>
</tbody>
</table>

Source:  FTC, MERGER DATA 1996–2011, supra note 42.

FIGURE 2:
PERCENT OF AGENCY INVESTIGATIONS THAT WERE ENFORCED
BY NUMBER OF REMAINING SIGNIFICANT COMPETITORS
OVER TIME
Without examining each such proposed merger recorded by the FTC—which involves nonpublic information—it is of course not possible to single out specific mergers that were correctly or erroneously approved, or the reasons for any error. Yet, this evidence concerning that agency’s actions dovetails with the previously analyzed evidence from merger retrospectives. The latter showed that studied mergers leaving only five or six significant competitors ultimately were in fact anticompetitive, whereas agency practice data show equally clearly that enforcement actions by the FTC against those mergers have in recent years simply ceased.  

Similarly, mergers in markets with HHI s in the range 2500–3000 were shown often to be anticompetitive, but present data show that these mergers have recently been approved by the FTC with considerably greater frequency. This evidence strongly suggests that policy has drawn the enforcement line in the wrong place.

There is substantial other evidence corroborating this conclusion, although not the specifics. The previously cited study by Baker and Shapiro documented both the easing of merger policy and the widespread—and accurate—perception that that had taken place. Underscoring the latter point, in 2007—the precisely the time frame where the data show greater permissiveness by the FTC—the Wall Street Journal reported that “[t]he federal government has nearly stepped out of the antitrust enforcement business, leaving companies to mate as they wish.” And while no single merger or industry experience proves the point, many observers have identified recent treatment of mergers in certain industries as signaling a shift from past practice and as likely to be harmful to consumer interests. A widely cited example is the airline industry, a brief review of which is instructive.

As of 2007 seven major carriers served the country—American, Continental, Delta, Northwest, Southwest, United, and US Airways. In 2008, the Justice Department approved the merger of Delta with Northwest, briefly making the merged company the largest U.S. carrier. That decision was seen as a

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78 This of course implies zero Type I errors, but at the cost of maximizing Type II errors, those of omission.

79 One might be concerned that changes in the agencies’ approach to market definition over time would affect this conclusion. In reality, the agencies have tended to make market definitions narrower in litigated cases, which suggests that this analysis understates the risk of unchallenged mergers. For a discussion of the refinement of market definition, see, e.g., FTC v. Advocate Health Care Network, 841 F.3d, 464, 469–73 (7th Cir. 2016) (discussing the demise of the Elzinga-Hogarty test and narrower market definitions in hospital cases).

80 Baker & Shapiro, supra note 19.


policy shift from DOJ’s previous position regarding airline mergers, which was illustrated by its successful opposition to the effort by United and US Airways to merge in 2001. Not surprisingly, the result was to encourage further consolidation in the industry. United and Continental proposed to merge and were allowed to do so in 2010. Southwest proceeded to acquire AirTran in 2011, and American and US Airways merged in 2012. In a period of five years the industry went from seven to four major carriers, a sharp contrast to the steadfast antitrust opposition to major airline mergers prior to that time. The results, to many, were predictable, as stories and studies soon began documenting the effects on prices, capacity, and profits. Tellingly, by 2015 the same Justice Department that had approved all four mergers—some with modest concessions—found itself opening an investigation of capacity coordination among these same carriers, and then another investigation regarding strategic entry deterrence by United, which had gained share at Newark airport from its merger.

This single case does not by itself prove any larger point, and it would certainly be an exaggeration to suggest that the outcome of consolidation in airlines was the direct result of crossing the previously identified threshold of five or six significant competitors. Yet consolidation in this range of firm numbers and market concentration is now seen to raise the likelihood of precisely those effects. Coordination, strategic behavior, and consumer harm should not have been an entirely surprising result of permitting those four mergers.

VII. CONCLUSION

This article has examined two important structure-based presumptions in merger enforcement policy—one favoring mergers in relatively unconcentrated industries (the "safe harbor"), and the other disfavoring mergers above


some concentration threshold (“the structural presumption”). We have seen how these two presumptions have evolved in terms of their numerical range and in terms of their strengths, changes that have been justified by anecdotal evidence and principled arguments. This article has sought to shift the debate on these longstanding presumptions by bringing some of the first systematic data to bear on their factual premises.

The evidence is, simply put, quite strong. True, the number of data points is modest, but they do indicate that market structure is a valid predictor of post-merger harm. A large—in some cases overwhelmingly large—fraction of mergers that lie above identifiable thresholds indeed prove to be anticompetitive. This prediction is stronger when a simple HHI measure is supplemented by a condition on the change in HHI, and stronger yet when couched in terms of the number of significant competitors. All results support a threshold similar to that currently embodied in the 2010 Merger Guidelines, and a presumption considerably stronger than the de facto enforcement standard that now exists. There appears to be a high probability of reduced competition and consumer harm from mergers above these thresholds, justifying a very substantial burden on merging parties to refute the implications of this economic evidence, especially in cases involving allegations of coordinated effects.

Evidence with respect to the safe harbor, on the other hand, casts at least some doubt on the validity of that screening tool. Whereas the safe harbor presumption has long been accepted as quite strong, the evidence shows a not insubstantial number of studied mergers in that range that have proved to be anticompetitive. This suggests that this structure-based presumption should be treated as considerably weaker guidance for merger policy than the term “safe harbor” might imply.

While more evidence would certainly be better—and indeed is needed—this compilation goes beyond isolated examples and anecdotes and well beyond pure theory and simple advocacy. The evidence relied upon here is broader and clearer than what has previously been available, and it provides strong support for the use of structure-based presumptions in pursuit of effective and efficient merger control policy.