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

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In analyzing these issues, I have benefitted from discussions with and contributions from many people, including Jon Baker, Rick Brunell, Peter Carstensen, Joe Farrell, Brian O’Dea, Steve Salop, and Chris Sagers. I have received excellent research assistance from Shawn Kilpatrick and Pinshuo Wang and much assistance in earlier development of the data base from Dan Greenfield and Chengyan Gu. I am also grateful for helpful comments from editors of this journal. All views and remaining errors are the sole responsibility of the author.

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Crucial areas of antitrust in the United States have undergone tectonic shifts in the past thirty or forty 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 essay I will provide 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 balance in question concerns errors of omission vs. 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 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. 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,¹ 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,

¹ 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 Communications v. Law Offices of Curtis Trinko*, 540 U.S. 398, 414 (2004). While this was a nonmerger case, this guidance is reflected in other areas of antitrust.

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 paper 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 outright challenged 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.

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 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 has not been the subject of debate or controversy, much less the subject of serious empirical analysis of its validity. Rather, it 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 essay 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 and examines the concentration and share characteristics of the firms and markets in which they operated. 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 essay 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 paper proceeds as follows. The next two parts provide brief histories of and rationales for the structural presumption and the safe harbor, respectively. Parts V and VI analyze the theoretical and empirical bases for reliance on market structure in the analysis of the effects of mergers. Part VII 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 VIII 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. Overall, this evidence suggests the merits of close but careful attention to market structure in the competitive analysis of mergers. The evidence on actual case-bringing that concludes this essay identifies an important 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 the Philadelphia National Bank case (hereafter, PNB),² that term actually 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 a 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 judicial administration.” Further, it opined that intense congressional concern with the rising tide of concentration “warrants dispensing, in some cases,

² United States v. Philadelphia National Bank, 374 U.S. 321 (1963). An excellent review can be found in Peter Carstensen, *The Philadelphia National Bank Presumption: Merger Analysis in an Unpredictable World*, 80 ANTITRUST LAW JOURNAL (2015).

with elaborate proof of market structure, market behavior, or probable anticompetitive effects.” These observations were followed by the key prescriptive 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 competition substantially that it must be enjoined in the absence of evidence clearly showing that the merger is not likely to have such anticompetitive effects.”³

Shortly thereafter, the 1968 Merger Guidelines formalized something akin to a structural presumption. Those guidelines asserted that in a highly concentrated market—defined as one where four-firm concentration exceeded 75 percent—“the Department will ordinarily challenge mergers accounting for” various stated market shares.⁴ 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 Merger Guidelines, in 1992.⁵ Mergers in highly concentrated industries that

³ Richard Posner, who, as Justice Brennan’s law clerk, in fact 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 illegality 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.” C. Scott Hemphill, *Philadelphia National Bank at 50: An Interview with Judge Richard Posner*, 80 ANTITRUST LAW JOURNAL (2015).

⁴ Horizontal Merger Guidelines, 1968

⁵ Horizontal Merger Guidelines, 1992

raised concentration by certain amounts were declared “likely to create or enhance market power or facilitate its exercise. This *presumption* [emphasis added] 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.”⁶

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 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 enforcement, such as the infamous Von’s Grocery case,⁷ 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, Rodino

⁶ Horizontal Merger Guidelines, 2010

⁷ U.S. v. Vons Grocery Co., 384 U.S. 270 (1966)

(1990) decried what he viewed as the growing failure of policy to adhere to legislative intent, urging adherence to what he termed the “structural presumption.”⁸ James (1993) 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.”⁹

Cautionary views found their way into the courts, notably with the 1990 Baker-Hughes opinion and the 2001 Heinz opinion, both by the DC Circuit.¹⁰ These opinions contended 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.¹¹ Some observers went farther

⁸ Peter Rodino, *The Future of Antitrust: Ideology vs. Legislative Intent*, 35 ANTITRUST BULLETIN (1990).

⁹ Charles James, *Overview of the 1992 Horizontal Merger Guidelines*, 61 ANTITRUST LAW JOURNAL (1993).

¹⁰ U.S. v. Baker-Hughes, Inc., 908 F.2d 981 (D.C. Cir. 1990). FTC v. H.J. Heinz Co., 246 F. 3d 708 (D.C. Cir., 2001).

¹¹ Jonathan Baker and Carl Shapiro, *Reinvigorating Horizontal Merger Enforcement*, in HOW THE CHICAGO SCHOOL OVERSHOT THE MARK, 252 (Robert Pitofsky ed., Oxford 2008). Sullivan notes the replacement of the “clear showing” standard of PNB with a “sliding scale” between the strength of the prima facie case and its rebuttal. Sean Sullivan, *What*

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.”¹² Wright and 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.”¹³

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.¹⁴ Where

Structural Presumption, JOURNAL OF CORPORATION LAW, forthcoming.

¹² Joshua Wright, *The Guidelines Should Be Revised to Reject the PNB Structural Presumption*, 26 TRUTH ON THE MARKET (2009). Wright also notes that the usual presumption depends on correct definition of the antitrust market and at most has relevance to coordinated effects theory.

¹³ Douglas Ginsburg and Joshua Wright, *Philadelphia National Bank: Bad Economics, Bad Law, Good Riddance*, 80 ANTITRUST LAW JOURNAL, 386 (2015). See also Statement of Commissioner Joshua Wright, Dissenting in Part and Concurring in Part in the Matter of Holcim Ltd. and Lafarge, May 8, 2015

¹⁴ The usually cited case illustrating the application of sophisticated economics (made possible by good data) is *FTC v. Staples*, 970 F. Supp. 1066 (1997). See also Serdar Dalkir and

that is feasible and cost-effective, no enforcer would opt for anything else, and indeed, the original PNB court viewed the structural presumption not so much 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.¹⁵ 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, Baker (2002) 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.”¹⁶ In his review of the impact of Chicago School economics on antitrust, Rubinfeld declared himself “troubled that the concern about false positives (bringing inappropriate cases)

Frederick Warren-Boulton, *Prices, Market Definition, and the Effects of Merger: Staples-Office Depot*, in *THE ANTITRUST REVOLUTION* (John Kwoka and Lawrence White eds., 2014).

¹⁵ 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 Gavil, *Burden of Proof in U.S. Antitrust Law* in *1 ISSUES IN COMPETITION LAW AND POLICY 125* (ABA Section of Antitrust Law 2008). More on this below.

¹⁶ Jonathan Baker, *Mavericks, Mergers, and Exclusion: Proving Coordinated Competitive Effects Under the Antitrust Laws*, *77 NEW YORK UNIVERSITY LAW REVIEW*, 200 (2002).

has tended to trump worries about false negatives (failing to bring appropriate cases).¹⁷ Baker and Shapiro used survey responses to document the decline in merger enforcement and advocated “partially restoring the structural presumption.” And most recently, Carstenson’s review and analysis lead him to argue for a return to the fundamentals of the structural presumption.¹⁸

The debate between these positions has been vigorous and occasionally enlightening. But as Sullivan has rightly noted, “the opposing sides of the debate are simply at an impasse.”¹⁹ As noted previously, this paper 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, we examine the history and basis for the so-called “safe harbor,” the other structure-based presumption, 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 Horizontal Merger Guidelines. At the ABA Antitrust Section meetings that year just prior to the release of those guidelines, Attorney

¹⁷ Daniel Rubinfeld, *On the Foundations of Antitrust Law and Economics* in Pitofsky, op. cit., p. 57.

¹⁸ Carstensen, op. cit.

¹⁹ Sullivan, op. cit., p. 3.

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 1000 in the Herfindahl index.”²⁰

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 postmerger Herfindahl Index is below 1000.”²¹ 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 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

²⁰ W. F. Smith, *Changing Enforcement Policy*, 51 ANTITRUST LAW JOURNAL, 101 (1982).

²¹ William Baxter, *A Justice Department Perspective*, 51 ANTITRUST LAW JOURNAL, 291 (1982). Interestingly, the PNB opinion contemplated a single breakpoint point and effectively two zones, whereas the guidelines create a third, middle zone.

found with a merger answering that description.”²²

Interestingly, officials at the Federal Trade Commission appeared to prefer somewhat greater discretion over mergers in that range. The Director of the Bureau of Competition at the FTC urged a “case-by-case” determination, a statement viewed as preserving a greater possibility of a challenge than literally implied by the term “safe harbor.”²³ 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. In one of the few questions or comments raised at the time, Calkins (1983) declared that “it would be a mistake to guarantee that mergers in this region will not be challenged.”²⁴

The original boundary of the safe harbor—an HHI of 1000—was raised to 1500 in the 2010 Merger Guidelines. Those guidelines stated that “Mergers 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 agency practice—a de facto standard. This policy and

²² Ibid.

²³ Thomas Campbell, *A Federal Trade Commission Perspective*, 51 ANTITRUST LAW JOURNAL, 297 (1982).

²⁴ Stephen Calkins, *The New Merger Guidelines and the Herfindahl-Hirschman Index*, 71 CAL. L. REV., 420 (1983). Kauper generally disputed this concern. Thomas Kauper, *The 1982 Merger Guidelines: Of Collusion, Efficiency, and Failure*, 71 CAL. L. REV. 497 (1983).

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. We 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 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. Moreover, if anticompetitive and benign mergers are divided by a bright line, then the safe harbor and the structural presumption are one and the same: above that line, mergers are presumptively anticompetitive, and below, presumptively benign.

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. A bit of formal modeling in the next subsection sets out the foundation for the simple version of this proposition.

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 Baxter described a third, intermediate range where structural considerations are relevant but not dispositive. Accordingly, the second subsection 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.

We begin with a standard economic model that of homogeneous product Bertrand competition, in which firms compete using price.²⁵ 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

²⁵ For background, see any standard industrial organization text, e.g., Jeffrey Church and 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 Baker, *Market Concentration in the Antitrust Analysis of Horizontal Mergers*, in *ANTITRUST LAW & ECONOMICS*, (Keith Hylton, ed., 2010)

monopolist would earn. All firms share in the profit equally, earning X/n in that period and in all future periods so long as they adhere to this joint profit maximizing strategy. Discounted to the present at the rate r ,²⁶ this yields a present value of profits as follows:

$$\frac{X/n}{(1+r)} + \frac{X/n}{(1+r)^2} + \frac{X/n}{(1+r)^3} + \dots = \frac{X/n}{r} \quad (1)$$

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 $(1/n)^{\text{th}}$ of total profit X exceeds that full monopoly profit X in one period, that is:

$$\frac{X/n}{r} > X \quad (2a)$$

or simply

$$1/r > n \quad (2b)$$

²⁶ 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.

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^*+1 to n^* would predictably result in cooperative pricing and excess profit, and such a merger would therefore be anticompetitive.

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.

We next examine variations on this basic proposition, variations that are important in operationalizing this insight and understanding its connection to the structural presumption.

B. Firm Asymmetries and the Structural Presumption

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

$$r^* < 1/n \tag{3}$$

Thus, for any given number of firms n , there is a discount rate common to all firms that serves to trigger cooperation: sufficiently patient firms (i.e., those with low values of r) value the longer stream of modest profits more highly, and will rationally choose cooperation in order to maintain that stream. Suppose, however, that not all n firms have that necessary degree of patience, in particular, that firm 1 has a uniquely high discount rate, reflecting a higher value of upfront

money than for other firms.²⁷ With an $r_1 > r^*$, this firm will lower price (“cheat”), forcing the other firms to respond, and the equilibrium may involve no profits to any firm.

The effect of a merger involving firm 1 is straightforward. If such a merger into any firm 2 through n replaces firm 1's uniquely high r_1 with the r^* for that other firms, eliminating this asymmetry will result in cooperation throughout the industry.

A second scenario alters the assumption that all firms are of equal size. Suppose that firms have shares $s_i, i = 1, \dots, n$, such that $s_1 > s_2 > s_3 > \dots > s_n$. Each firm's profit from cooperating is a proportional part of total monopoly profit X , that is, firm 1's profit is $s_1 * X$, while firm 2's is $s_2 * X$, which is less than $s_1 * X$, and so forth down to the smallest firm's stake $s_n * X$. Each firm now compares its own profit stream from cooperation to that from competition, thus:

$$\frac{X/s_i}{r_i} > X \quad (4)$$

Suppose now that this inequality is satisfied for all firms but the smallest, so the n^{th} firm earns more profit from cheating. Much as for the firm with a uniquely high discount factor, the small n^{th} firm cheats and prevents cooperation by the other firms. Again suppose that this n^{th} firm is acquired by or merges with any other firm, and the merged entity thereby achieves a sufficiently large size to have a stake in cooperation that exceeds the profit from cheating. Eliminating that asymmetry therefore results in a shift of the industry to the cooperative equilibrium.

²⁷ This could be due to urgent cash needs if the firm is facing financial difficulties, or has limited access to capital markets.

A third example involves an asymmetry in the initial behavior of one of the n firms in the market. This scenario is best explained using a model in which the competitive response by any one firm to its rivals is measured by a parameter θ , usually called the “conjectural variation.”²⁸ Higher values of θ are associated with more cooperative behavior. Suppose initially that all firms have a value that results in cooperation throughout the market. Let that value be $\theta_j, j = 2, \dots, n$. Alternatively, suppose that firm 1 is more aggressively competitive than its rivals 2 through n , behaving in a manner described by a value of $\theta_1 > \theta_j$. By itself, this firm prevents cooperation in the market, since if the other firms pursued cooperation, firm 1 would deviate and take full advantage of the others’ behavior. Such a firm is commonly known as a maverick firm.

Under these circumstances, a merger that absorbs firm 1 and replaces its competitive behavior measured by a conjectural variation θ_1 with behavior conforming to all others’ value θ_j will generally result in a more cooperative industry equilibrium.²⁹ Its elimination permits uniformly cooperative forces throughout the industry since it eliminates the competitive constraint

²⁸ The conjectural variation is shorthand for more complex behavior that usually characterizes firms’ interactions over time. It is generally used in models where quantity rather than price is the strategic variable, but is used here for expositional convenience. See, for example, John Kwoka, *The Private Profitability of Horizontal Mergers with Non-Cournot and Maverick Behavior*, 7 INTERNATIONAL JOURNAL OF INDUSTRIAL ORGANIZATION, (1989).

²⁹ Kwoka, op. cit. In addition to changed behavior, the increased size of the merged firm also provides an incentive to raise price.

previously imposed by the maverick firm.

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. We 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

$$D_{12} = \Delta q_1 / \Delta q_2 \tag{5}$$

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 is associated with significant competitive effects.

Under certain circumstances, however, these can be shown 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—specifically, logit demand—and often is taken as a useful first approximation, or perhaps even the best that can be done.³⁰ Under this assumption, the diversion ratio can be written as follows:

$$D_{12} = s_1 / (1 - s_2) \quad (6)$$

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 as follows:³¹

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.” Despite some ambiguity in the meaning of this passage, it seems clear that market shares can be related to diversion.

D. Decision Theory

³⁰ It is in fact directly implied by the logit form of demand, but that is only of several possible functional forms.

³¹ Source: Federal Trade Commission, Horizontal Merger Investigations data, Fiscal Years 1996-2011 (Jan. 2013)

In a significant new perspective, Salop has recently cast the structural presumption in decision theoretic terms.³² 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.³³ 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

³² Steven Salop, *The Evolution and Vitality of Merger Presumptions: A Decision Theoretic Approach*, 80 ANTITRUST LAW JOURNAL (2015). See also Jonathan Baker, *Taking the Error out of ‘Error Cost’ Analysis: What’s Wrong with Antitrust’s Right*, 80 ANTITRUST LAW JOURNAL (2015).

³³ 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.

probability much if at all, then the presumption might still be warranted.

Salop views the language of the Philadelphia National Bank opinion through this lens. He recounts that court's 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 commission—challenging a benign merger or practice--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.³⁴ As Salop notes, if a presumption

³⁴ Type II errors arise to the extent that 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.

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 more reliable.”³⁵ 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.”³⁶

D. Conclusions from Economic and Decision Theory

All of the previously-discussed theories, as well as others,³⁷ 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.

³⁵ Salop, op. cit., p.

³⁶ Salop, op. cit., pp. 289.

³⁷ See Massimo Motta, *COMPETITION POLICY: THEORY AND PRACTICE* (Cambridge 2004).

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,³⁸ 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 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:

$$PERF_i = a + b*CONC_i + c*X_i + e_i \quad (7)$$

³⁸ For example, see F.M. Scherer, *INDUSTRIAL MARKET STRUCTURE AND ECONOMIC PERFORMANCE* (McNally 1990); and Steve Martin, *ADVANCED INDUSTRIAL ECONOMICS*, (Wiley 2001).

In this expression *PERF* denotes a measure of non-competitive performance such as profit or price, *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. Based on that, competition policy came to view mergers as competitively harmful largely due to the resulting increase in concentration.

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.”³⁹ 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. The specific reasons for casting doubt on this research and its conclusions are worth noting.

First, accounting data on profits were shown to be unreliable guides to true economic profit, and the necessary corrections were complicated. As a consequence, research attention shifted to price-cost margins, and ultimately to price, as measures of performance.

Second, the relationship between concentration and profit was often estimated as linear,

³⁹ Philadelphia National Bank, p. 1741.

implying that *any* change in concentration mattered. While this seemed to justify stringent merger policy, studies that allowed for nonlinearities often found ranges of concentration without any effects, or at least with unchanged effects.

Third, factors other than concentration were found to be important, and worse yet, many of those additional factors—such as entry conditions and the degree of product substitution—were less readily measured and integrated into the empirical framework.

Fourth, issues of endogeneity and causation seemed to offer alternative explanations for the same statistical findings, but with quite different implications. For example, higher concentration and higher profits might both be the result of greater efficiencies, not market power, residing with larger firms.

Beyond these issues were two that went to the heart of the use of structure-performance estimation to motivate the structural presumption. The first of these was that 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 in specific cases. Mergers were found to have results that differed substantially from what the empirical relationship between rising concentration and performance suggested would happen. 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 these 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.⁴⁰

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.⁴¹ 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

⁴⁰ 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 Jeffrey Church and Roger Ware, *INDUSTRIAL ORGANIZATION: A STRATEGIC APPROACH* (Irwin 2000), and Steve Martin, *ADVANCED INDUSTRIAL ECONOMICS* (Blackwell 2001).

⁴¹ For this reason, the Wright-Ginsburg 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 and Wright, *op. cit.*, p. 384.

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. Some retrospectives have relied on structural modeling or reduced form estimation. Structural modeling seeks to estimate fundamental demand and supply or cost conditions in a market, relying on all the basic causal factors, and then determines how these relationships change due to a merger. Reduced form estimation focuses strictly on the price that is determined by these factors, and estimates how that price is changed by the merger. It requires enumeration of the underlying demand and supply/cost factors, but not their functional forms.

Both of these techniques require considerable amounts of data (even on factors that are not important to the issue under investigation), choice of functional forms, and resolution of econometric issues. Those burdens are substantially alleviated by use of a third technique, commonly known as difference-in-differences, which has become by far the most common approach. 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 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 and cost 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.⁴²

Assuming the control product is correctly chosen and with attention to certain other considerations,⁴³ the DID technique can provide good evidence of the effect of a merger on price

⁴² If, for example, the premerger price of a product is 100 and its postmerger 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 percent, then the price increase properly attributed to the merger would be the difference, or 3 percent.

⁴³ For detailed discussion, see John Kwoka, *MERGERS, MARKET POWER, AND REMEDIES* ch. 2, (MIT Press 2015). Werden raises several of these issues, ultimately concluding that merger retrospectives are inferior to full-information case studies done by the antitrust agencies. Gregory Werden, *Inconvenient Truths on Merger Retrospective Studies*, *JOURNAL OF ANTITRUST ENFORCEMENT* (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. One is also reminded of Stigler’s caution about the reliability of case studies: after one student of the industry has “render[ed] a verdict. It is crucial...that no second [researcher] be allowed to study the industry.” George Stocking, Alfred Kahn, Clare Griffin, and George Stigler, *Report on Antitrust Policy*, *AMERICAN ECONOMIC REVIEW*, 505 (May 1956). Thanks to F.M. Scherer

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 thirty 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 Ashenfelter et al, and by Kwoka.

The focus of Ashenfelter, Hosken, and Weinberg (2014)⁴⁴ 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.⁴⁵ They do not attempt to make these various estimates comparable, or to aggregate estimates from multiple studies of the same merger, or to separate out individual merger effects

for this reference.

⁴⁴ Orley Ashenfelter, Daniel Hosken, and Matthew Weinberg, *Did Robert Bork Understate the Competitive Impact of Mergers?* 57 JOURNAL OF LAW AND ECONOMICS (2014).

⁴⁵ I say "similar" since Ashenfelter et al include some studies that do not have actual postmerger data, but rather project postmerger price and from that seek to infer the effects of mergers. For reasons discussed in Kwoka (2015), this approach may not produce estimates of the same reliability. In addition, they include retrospective studies of non-US mergers. Whether their economic and institutional setting makes them sufficiently comparable is an open question.

where a study may have reported on multiple mergers. Instead, they rely on the fact that 36 of these *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.”⁴⁶

A more recent and comprehensive survey of the merger retrospective literature is the compilation by Kwoka (2013).⁴⁷ The screening procedures and comparability standards make this the largest data base of carefully controlled studies of the effects of mergers, and it is this source that is relied upon here. The procedure used there 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 difference-in-differences or some other technique 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 sixty. These covered fifty distinct transactions—some studies covered the same transaction--and nearly 120 individual product prices.⁴⁸

The next methodological step required extracting comparable price information regarding

⁴⁶ Ashenfelter, et al, p. 596.

⁴⁷ Kwoka (2015).

⁴⁸ 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.

each merger or other qualifying transaction. Where a single study reported multiple estimates, major non-duplicative estimates were averaged. Where multiple studies examined the same merger, the single estimated effects from each study were averaged into a summary statistic. The resulting data base consists of 49 estimates: 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 Kwoka's 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 ten percent. Non-mergers—joint ventures and code-sharing—seemed to have no net effect in either direction, although their numbers in the data base 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.⁴⁹

⁴⁹ A subset of these data has been used to examine the accuracy of the stated Merger Guidelines in predicting anticompetitive effects. Despite the limited nature of that inquiry, it is interesting to note that it found a large fraction of anticompetitive mergers would have been correctly predicted by these criteria. John Kwoka and Chengyan Gu, *Predicting Merger Outcomes: The Accuracy of Stock Market Event Studies, Market Structure Characteristics, and Agency Decisions*, 58 JOURNAL OF LAW AND ECONOMICS, (2015).

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 data base 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 we now turn.

V. TESTING THE SAFE HARBOR AND THE STRUCTURAL PRESUMPTION

This essay 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 data base to test alternative possible structure-based standards. We examine numerous thresholds--both existing and possible alternatives--for their empirical validity. By “empirical validity,” we mean the ability of each threshold to correctly predict whether the mergers in the data base ultimately prove to be benign or anticompetitive. Some errors are inevitable, of course. We are 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.

We first describe in greater detail the sources of these data on merger outcomes, and then proceed with the testing. At the end we 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 we draw on the compilation of such retrospectives described in Kwoka and Gu (2015). 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 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 data base 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 refining, some concentration data have been published by the Federal Trade Commission; and so forth.⁵⁰

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 postmerger 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 premerger shares of the merging firms.

- 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."⁵¹

⁵⁰ All primary sources cited in Kwoka and Gu.

⁵¹ Source: Federal Trade Commission, Horizontal Merger Investigations data, Fiscal Years 1996-2011 (Jan. 2013). 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. United States Department of Justice and Federal Trade Commission. 2010. "Horizontal Merger Guidelines", p. 18.

B. The Structural Presumption Put to the Test

We begin by testing the structural presumption stated in the 2010 Horizontal 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 device. We now proceed to test this and other criteria.

By actual count, there are 21 mergers in the data base that satisfy this 2500/200 category. Of these, 18 mergers were found to have resulted in postmerger 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.⁵² Put differently, this criterion has a Type I error rate of less than fifteen 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.⁵³

Importantly, we 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

⁵² It should be noted that mergers can have anticompetitive effects above and beyond price increases, but for reasons discussed in Kwoka (2015) there are many fewer retrospectives investigating these nonprice effects.

⁵³ Salop, for example, describes a rule that correctly identifies anticompetitive mergers 80 percent of the time as “a very strong presumption.” Salop, p. 289.

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.⁵⁴ As such, it 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 for policy purposes. Second, the mergers in the data base have all in fact been evaluated by the agencies and in some instances already subject to remedies. Accordingly, they should in fact exhibit smaller (if any) price increases, since neither cleared mergers nor those subject to effective remedies should

⁵⁴ 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 percent. This view of the merger process and outcome is discussed more fully in Kwoka and Gu, *op. cit.* Here we focus on the structural presumption itself.

result in net price increases. Third, the number of observations is not especially large, and there are substantial differences in the outcomes from particular mergers. That said, the increases average 11.2 percent; fully 61 percent of the mergers have increases greater than five percent, and 44 percent are greater than ten percent. These magnitudes and frequencies convey significant policy concerns. And finally, observations in the data base come disproportionately from a 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.⁵⁵

An interesting further question concerns the outcomes of alternative criteria for the structural presumption. This question, too, can be answered with this data base. For example, the more stringent criterion of 3000/200—that is, a postmerger 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 for as the standard for the presumption increases.

INSERT TABLE 1 ABOUT HERE

This series of tests clearly supports the proposition that the structural presumption in fact

⁵⁵ 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.

correctly identifies mergers with a very high likelihood of postmerger 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. We 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, we 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 in that region. Such a rule have never been part of the Merger Guidelines, so this is less a practical alternative than 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.

INSERT TABLE 2 ABOUT HERE

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.⁵⁶ As is evident, there is rather clear break in this sequence in the range of five to six remaining competitors: Indeed, in this data base 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.⁵⁷ 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.

INSERT TABLE 3 ABOUT HERE

⁵⁶ 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.

⁵⁷ The reasons include the modest number of such cases in the merger retrospectives data base, and the observation of mergers in that range that, from public accounts, would seem benign.

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 must have HHIs in excess of 1667 to 2000, and in these data such cases have actual HHIs between 2600 and 3100. The Type I error rates are roughly similarly low: 12 to 14 percent for these HHI values, and no more than five 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 reality, that error appears to be no greater than fifteen percent, and in some formulations, less than five 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, we 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 virtue of a presumption based on significant competitors may be accompanied by some imprecision as to exactly what constitutes such a firm,

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 postmerger 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. We begin our analysis by examining both of these bounds.

Preliminarily, we 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 our conclusions than in the context of the structural presumption, since with respect to the safe harbor the selection bias inhibits our 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, we 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, we are unable to characterize the frequency of such outcomes.

With those caveats, the outcomes of studied mergers that qualify for the “safe harbor” in the Merger Guidelines are nonetheless of some interest. In the present data set there are nine mergers where the postmerger 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 (7/9th) 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.⁵⁸ 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 the so-called “safe harbor” boundary should be viewed as a fully rebuttable presumption that a merger may be unlikely to result in a price increase. There is no empirical basis in these data for treating it as a stronger—much less as an irrebuttable—presumption.

⁵⁸ 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 small.

VI. MERGERS, CONCENTRATION, AND POLICY

The above analysis of the merger retrospectives data base has shown that the vast majority of mergers resulting in five 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 five 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.⁵⁹ These data consist of the number of second requests issued and the number of “enforcement actions” taken, arrayed

⁵⁹ Federal Trade Commission, HORIZONTAL MERGERS INVESTIGATION DATA, FISCAL YEARS 1996-2011. For corroborative data focused on unilateral effects models, see Malcolm

Coate, *Benchmarking the Upward Pricing Pressure Model with Federal Trade Commission Data*, JOURNAL OF COMPETITION LAW & ECONOMICS (2011).

according to HHI, its change, and the number of remaining significant competitors. The term “enforcement actions” encompasses formal challenges to mergers but is 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, we 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 postmerger 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 2008-2011.⁶⁰ We do this for HHI brackets and separately for each number of significant competitors.

INSERT TABLE 4 ABOUT HERE

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,

⁶⁰ This time-disaggregation is achieved simply by differencing the FTC data, for example, subtracting 1996-2003 from 1996-2007, to get 2004-2007. FTC, HORIZONTAL MERGERS INVESTIGATION DATA, FISCAL YEARS 1996-2003, 1996-2005, 1996-2007, as well as 1996-2011.

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.

INSERT TABLE 5 ABOUT HERE

INSERT FIGURE 1 ABOUT HERE

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 top HHI brackets. 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.

INSERT TABLE 6 ABOUT HERE

INSERT FIGURE 2 ABOUT HERE

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 five significant competitors (and most with six significant competitors) in fact ultimately were anticompetitive, whereas agency practice data show equally clearly that enforcement actions by the FTC against those mergers have in recent years simply ceased.⁶¹ Similarly, mergers in markets with HHIs in the range 2500-3000 were shown often to be anticompetitive, present data show that these mergers have recently been approved by the FTC with considerably greater frequency. This evidence certainly 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—precisely the time frame where the data show greater

⁶¹ This of course implies zero Type I errors, but at the cost of maximizing Type II errors, those of omission.

permissiveness by the FTC--the Wall Street Journal reported that “the federal government has nearly stepped out of the antitrust enforcement business, leaving companies to mate as they wish.”⁶² 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 (not the FTC) approved the merger of Delta with Northwest, briefly making the merged company the largest US carrier. That decision was seen as a 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--was forced to open an investigation of capacity coordination

⁶² Dennis Berman, *Handicapping Deal Hype and Hubris*, WALL STREET JOURNAL, January 16, 2007, at C1.

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.⁶³

VII. CONCLUSIONS

This essay 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*

⁶³ The Justice Department’s own Complaint, initially filed in opposition to the proposed merger of US Airways and American, recounts these very concerns as reasons for *not* approving that merger. Yet DOJ subsequently settled the Complaint, after securing concessions from the merging parties that it claimed were more than adequate. Many disagreed at the time, and the Department’s own subsequent enforcement actions would seem to be evidence against their assertion. The previously cited work by Rodino contains the following eerily prescient comment concerning this industry twenty-five years ago: “The airline mergers of the past few years also illustrate the substantial risk of ignoring the presumption that high market shares will be anticompetitive.” Rodino, 1990, p. 296.

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 essay 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. It indicates that market structure is a valid predictor of postmerger harm. A large, even 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 Horizontal Merger Guidelines, and a presumption considerably stronger than the de facto enforcement standard that now exists.

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, this compilation goes beyond isolated examples and anecdotes, and well beyond pure theory and firmly held convictions. The evidence relied upon here is broad and systematic, and it provides clear support for the use of structure-based presumptions in pursuit of effective and efficient merger control policy.

Table 1
Predictive Power of HMG Criteria: HHI and $\Delta > 200$

HHI	Number of mergers exceeding threshold	Number anticompetitive	Number procompetitive	Percent Correct
4000	10	9	1	90
3500	14	13	1	92.9
3000	17	15	2	88.2
2500	21	18	3	85.7
2000	24	21	3	87.5

Table 2
Predictive Power of Simple HHI Criterion

HHI	Number of Mergers Exceeding Threshold	Number anticompetitive	Number procompetitive	Percent Correct
4000	10	9	1	90
3500	14	13	1	92.9
3000	17	15	2	88.2
2500	23	19	4	82.6
2000	27	23	4	85.2

Table 3
Predictive Power of Number of Significant Competitors by Each Added Competitor

Incremental Significant Competitor	Count of Mergers	Number Anticompetitive	Number procompetitive	Percent Correct
1	1	1	0	100
2	2	2	0	100
3	0			
4	4	4	0	100
5	7	7	0	100
6	5	4	1	80
7	4	2	2	50
8	3	1	2	33.3

Table 4
Recent merger investigations resulting in enforcement actions
FTC 1996-2011

By HHI (%)		By Delta-HHI (%)		By Number of Significant Competitors (%)	
0-1,799	40.0	0-99	13.8	2 to 1	98.0
1,800-1,999	59.6	100-199	44.7	3 to 2	89.2
2,000-2,399	58.1	200-299	54.2	4 to 3	77.3
2,400-2,999	71.7	300-499	66.9	5 to 4	64.1
3,000-3,999	71.5	500-799	72.5	6 to 5	35.2
4,000-4,999	87.4	800-1199	78.6	7 to 6	12.0
5,000-6,999	89.5	1200-2499	88.0	8 to 7	24.0
7,000+	99.0	2500+	97.7	9 to 8	0%
Total	77.6%	Total	77.6%	Total	78.6%
Source: Federal Trade Commission, Horizontal Merger Investigations data, Fiscal Years 1996-2011 (Jan. 2013) <i>available at</i> https://www.ftc.gov/sites/default/files/documents/reports/horizontal-merger-investigation-data-fiscal-years-1996-2011/130104horizontalmergerreport.pdf					

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

HHI	1996-2003 Percent enforced	2004-2005 Percent enforced	2006-2007 Percent enforced	2008-2011 Percent enforced
0-1,799	52.9	4.3	0.0	0.0
1,800-1,999	70.8	0.0	0.0	0.0
2,000-2,399	67.7	40.0	10.0	22.2
2,400-2,999	79.3	41.7	54.5	0.0
3,000-3,999	77.4	23.1	59.5	80.6
4,000-4,999	90.9	50.0	81.3	100.0
5,000-6,999	85.2	91.3	87.5	96.1
7,000+	97.1	100.0	100.0	100.0
Total	77.8	71.4	70.8	88.5

Source: Federal Trade Commission, Horizontal Merger Investigations data, Fiscal Years 1996-2011 (Jan. 2013) *available at*
<https://www.ftc.gov/sites/default/files/documents/reports/horizontal-merger-investigation-data-fiscal-years-1996-2011/130104horizontalmergerreport.pdf>

Table 6
Enforcement Actions as Percent of Investigations based on
Number of Remaining Significant Competitors

FTC Data by Intervals

Number of significant competitors	1996-2003 Percent enforced	2004-2005 Percent enforced	2006-2007 Percent enforced	2008-2011 Percent enforced
1	96.2	100.0	100.0	98.4
2	84.8	89.3	94.7	95.7
3	76.1	50.0	86.7	91.9
4	61.5	57.1	69.2	72.7
5	40.6	28.6	44.4	0.0
6	20.0	16.7	0.0	0.0
7	50.0	0.0	0.0	0.0
8	0.0	0.0	0.0	0.0
Total	77.0	78.7	70.8	89.0
Source: Federal Trade Commission, Horizontal Merger Investigations data, Fiscal Years 1996-2011 (Jan. 2013) <i>available at</i> https://www.ftc.gov/sites/default/files/documents/reports/horizontal-merger-investigation-data-fiscal-years-1996-2011/130104horizontalmergerreport.pdf				

Figure 1

Percent of agency investigations that were enforced by HHI ranges over time

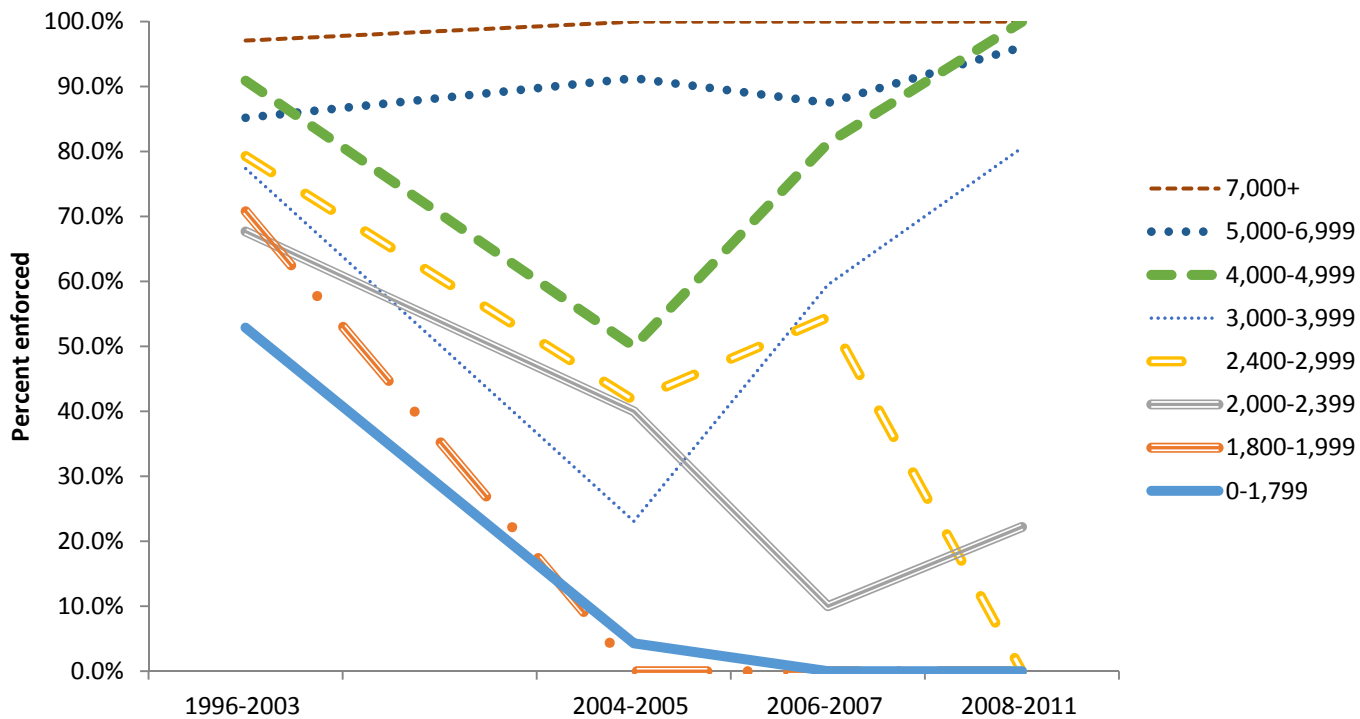


Figure 2

Percent of agency investigations that were enforced by number of remaining significant competitors over time

