

# Michigan Law Review

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Volume 84 | Issue 8

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1986

## Workable Antitrust Policy

Frank H. Easterbrook

*United States Court of Appeals for the Seventh Circuit*

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### Recommended Citation

Frank H. Easterbrook, *Workable Antitrust Policy*, 84 MICH. L. REV. 1696 (1986).

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## CORRESPONDENCE

### Workable Antitrust Policy

*Frank H. Easterbrook\**

One of the schools of thought in the economics of antitrust was called “workable competition.” The adherents to this school believed that markets were prone to cartelization and that concentration was death on competition, but that occasionally competition might prove “workable.” These scholars were suspicious of almost every industrial practice they saw. One of the manifestations of their work came to be known as the “structure-conduct-performance paradigm.” The thesis was that you could tell whether competition was feasible from the structure of the market. If the top four firms had fifty percent or so of the sales, we should abandon hope of competition — unless, perhaps, the government should be able to break up the largest firms and restore workable competition. The vision was supported by data showing that the most concentrated industries were also the most profitable — and monopoly profit seemed the only source of the higher returns.

This vision of markets fell under attack by other scholars who were skeptical about the interpretation of the data. Concentration was a fact, and no one doubted that concentrated markets were easier to cartelize (or to organize informally) than atomistic markets. Indeed some of those most skeptical about the implications of concentration also worked out the models showing how concentration could duplicate the effects of collusion even without agreement.<sup>1</sup> But the skeptics doubted the *model* of competition on which the structure-conduct-performance paradigm rested. It is a model of “perfect competition” taken from classical economics, a model in which everyone is perfectly informed and makes hyper-rational decisions on a moment’s notice, a model in which everyone is minuscule compared to the market and so cannot affect anyone else’s acts or the price, a model in which these

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\* Judge, United States Court of Appeals for the Seventh Circuit; Senior Lecturer, University of Chicago. — Ed. I thank Barry E. Adler, Michael E. DeBow, Eleanor M. Fox, Douglas H. Ginsburg, Herbert Hovenkamp, Richard A. Posner, Eric Simonson, and Lawrence A. Sullivan for helpful comments on an earlier draft.

1. R. POSNER, ANTITRUST LAW: AN ECONOMIC PERSPECTIVE 37-77 (1976); G. STIGLER, *A Theory of Oligopoly*, in THE ORGANIZATION OF INDUSTRY 39-63 (1968); cf. Turner, *The Definition of Agreement Under the Sherman Act: Conscious Parallelism and Refusals to Deal*, 75 HARV. L. REV. 655 (1962).

atoms will compete price down to marginal cost. The skeptics thought the model incomplete, because production must be organized to take advantage of the division of labor (and perhaps economies of scale). Perhaps concentrated industries are those in which economies of scale dictate big firms. Then the structure-conduct-performance paradigm is just a longing for a world in which artisans made leather artifacts in tiny shops; it is not a useful way to think about real economies or a suitable source of proposals for antitrust policy.

The skeptics did a little testing. The proposition turned out to be false. Economies of scale could not explain the concentration in many industries.<sup>2</sup> Their skepticism unabated, these doubters asked why some firms grow and others do not. Logically those who make a product for the least cost should be able to achieve the largest sales, so that size is a result of satisfying consumers' wants. Perhaps those that grow are managed more efficiently, so that their size brings a greater share of production under the low-cost techniques. A quick check of the data showed that the "excess" profits in the concentrated industries were earned by the largest firms; smaller firms got only normal returns.<sup>3</sup> This showed that concentrated industries were efficient industries.

But wait. Maybe these industries were monopolistic and the smaller firms were "fringe" firms trying to expand production. They would take the monopoly price as given and expand their output. Higher costs would dissipate their profits; the larger firms would make larger profits; the industry could fit the data without the slightest efficiency.<sup>4</sup> So more tests were in order. Perhaps data could show growth of more efficient firms; this would refute the thesis that concentrated industries comprised indifferent monopolizers and inefficient fringe competitors. Perhaps data could be used to pull apart the effects of industrial concentration from the effects of the size of the largest firm. If profits came from more production in the largest firm — as opposed to more concentration in the industry, holding other things constant — this would support the "efficiency" hypothesis. Perhaps data could show the effects of imports into a market, pinpointing circumstances under which "concentration" was a misleading variable. All of these tests were carried out over a decade. The results show that concentra-

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2. G. STIGLER, *The Economies of Scale*, in THE ORGANIZATION OF INDUSTRY 71-94 (1968). But cf. J. MCGEE, IN DEFENSE OF INDUSTRIAL CONCENTRATION (1971).

3. Demsetz, *Industry Structure, Market Rivalry, and Public Policy*, 16 J.L. & ECON. 1, 6 (1973).

4. Compare G. STIGLER, *The Dominant Firm and the Inverted Umbrella*, in THE ORGANIZATION OF INDUSTRY 108-12 (1968), with Parsons & Ray, *The United States Steel Consolidation: The Creation of Market Control*, 18 J.L. & ECON. 181 (1975).

tion in an industry does not increase profit and that efficient firms grow.<sup>5</sup>

Today it is hard to find an economist who believes the old structure-conduct-performance paradigm.<sup>6</sup> The results of this work have influenced proposals to “break up” concentrated industries and have changed merger policy, which is now much more tolerant of concentration than it used to be.

Many of the skeptics who objected to using the classical, atomistic model of competition as a prescription for antitrust policy are known as the “Chicago School” of antitrust. Their success in undermining the structure-conduct-performance paradigm was the result of decades of grubbing about in the data, including some false starts. Their legacy is thoroughgoing skepticism — doubts about the use of abstract models, about the wisdom of legal rules designed to move the economy closer to a model of atomistic competition, about the ability of firms to cartelize a market no matter how hard they try. These doubts, coupled with data backing up many of their claims, have coincided with a change in the Supreme Court’s antitrust jurisprudence that emphasizes efficiency and consumers’ welfare.<sup>7</sup>

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5. Some early skeptical work is reported in *INDUSTRIAL CONCENTRATION: THE NEW LEARNING* (H. Goldschmid, H. Mann & J. Weston eds. 1974). For the principal recent empirical work, see Eckbo, *Mergers and the Market Concentration Doctrine: Evidence from the Capital Market*, 58 J. BUS. 325 (1985); Gisser, *Price Leadership and Dynamic Aspects of Oligopoly in U.S. Manufacturing*, 92 J. POL. ECON. 1035 (1984); Hirschey, *Market Structure and Market Value*, 58 J. BUS. 89 (1985); Leitzinger & Tamor, *Foreign Competition in Antitrust Law*, 26 J.L. & ECON. 87 (1983); Peltzman, *The Gains and Losses from Industrial Concentration*, 20 J.L. & ECON. 229 (1977); Ravenscraft, *Structure-Profit Relationships at the Line of Business and Industry Level*, 65 REV. ECON. & STATISTICS 22 (1983); Schmalensee, *Do Markets Differ Much?*, 75 AM. ECON. REV. 341 (1985); Smirlock, Gilligan & Marshall, *Tobin’s q and the Structure-Performance Relationship*, 74 AM. ECON. REV. 1051 (1984). The literature through 1982 is collected in Pautler, *A Review of the Economic Basis for Broad-Based Horizontal-Merger Policy*, 28 ANTITRUST BULL. 571 (1983). There is a related question whether any data based on profits are reliable, see Benton, *The Validity of Profits-Structure Studies with Particular Reference to the FTC’s Line of Business Data*, 75 AM. ECON. REV. 37 (1985), but that is another debate. There is also a possibility that concentration leads to monopoly prices but that unions capture all the rents, leaving no excess profits. See Salinger, *Tobin’s q, Unionization, and the Concentration-Profits Relationship*, 15 RAND J. ECON. 159 (1984). The implications of this possibility are obscure. Firms that don’t make monopoly profits are deterred automatically, and unions largely stand outside antitrust. Again, this is another debate.

6. Even Leonard Weiss, one of the strong proponents of the approach, has slowly retreated. Compare Weiss, *The Concentration-Profits Relationship and Antitrust*, in *INDUSTRIAL CONCENTRATION: THE NEW LEARNING*, *supra* note 5, at 184-233, with Weiss, *The Structure-Conduct-Performance Paradigm and Antitrust*, 127 U. PA. L. REV. 1104 (1979), and L. Weiss, *Concentration and Price — Not Concentration and Profits* (Oct. 1986) (unpublished manuscript).

7. You can find the data in *The Journal of Law and Economics* and many other journals with interests in industrial organization. The following cases in the Supreme Court have used an approach to antitrust strongly influenced by the consumers’ welfare or efficiency approach: *FTC v. Indiana Fedn. of Dentists*, 106 S. Ct. 2009, 2018-20 (1986); *Matsushita Elec. Indus. Co. v. Zenith Radio Corp.*, 106 S. Ct. 1348, 1354-60 (1986); *Aspen Skiing Co. v. Aspen Highlands Skiing Corp.*, 105 S. Ct. 2847, 2857-59 (1985); *Northwest Wholesale Stationers, Inc. v. Pacific Stationery & Printing Co.*, 105 S. Ct. 2613, 2619-21 (1985); *NCAA v. Board of Regents*, 468 U.S. 85,

Some of the most important work of the Chicago School has been based on the rejection of simple models of competition. For example, George Stigler invented the economics of information, asking how market transactions are affected if we assume that information is costly to acquire and use.<sup>8</sup> Lester Telser used the high costs of information and the difficulty retailers encounter in charging for some of their most distinctive services (such as demonstrating, explaining, and certifying the products they carry) to show that it may be necessary to control by contract their freedom to set prices and choose their merchandizing techniques.<sup>9</sup>

100-15 (1984); *Copperweld Corp. v. Independence Tube Corp.*, 467 U.S. 752, 767-71, 775-77 (1984); *Jefferson Parish Hosp. Dist. No. 2 v. Hyde*, 466 U.S. 2, 11-18 (1984) (majority); *Jefferson Parish*, 466 U.S. at 32-47 (O'Connor, J., concurring); *Monsanto Co. v. Spray-Rite Serv. Corp.*, 465 U.S. 752, 762-64 (1984); *Reiter v. Sonotone Corp.*, 442 U.S. 330, 343 (1979); *Broadcast Music, Inc. v. Columbia Broadcasting Sys.*, 441 U.S. 1, 9-10, 19-21 (1979); *Continental T.V., Inc. v. GTE Sylvania Inc.*, 433 U.S. 36, 50-59 & n.21 (1977); *Illinois Brick Co. v. Illinois*, 431 U.S. 720, 731-44 (1977); *United States Steel Corp. v. Fortner Enterprises*, 429 U.S. 610, 620-22 (1977); *Brunswick Corp. v. Pueblo Bowl-O-Mat, Inc.*, 429 U.S. 477, 488-89 (1977). None can be said to "adopt" the Chicago School — not only because no multi-member court can adopt one approach to a complex problem but also because the Chicago School is not a "school" so much as it is a method of asking questions. If you examine the opinions written by people often associated with an efficiency-oriented approach to antitrust, you will see differences in emphasis but agreement on the sort of questions that should be asked and the sort of data that are important to the answers. See, e.g., *Rothery Storage & Van Co. v. Atlas Van Lines*, 792 F.2d 210 (D.C. Cir. 1986) (Bork, J.); *Ball Memorial Hosp., Inc. v. Mutual Hosp. Ins., Inc.*, 784 F.2d 1325 (7th Cir. 1986) (Easterbrook, J.); *General Leaseways v. National Truck Leasing Assn.*, 744 F.2d 588 (7th Cir. 1984) (Posner, J.); *United States v. Waste Management, Inc.*, 743 F.2d 976 (2d Cir. 1984) (Winter, J.); *Barry Wright Corp. v. ITT Grinnell Corp.*, 724 F.2d 227 (1st Cir. 1983) (Breyer, J.). This approach in the appellate courts is not limited to former academics. It is hard to distinguish these opinions from other thoughtful work of otherwise divergent judges. See, e.g., *Westmann Commn. Co. v. Hobart Intl., Inc.*, 796 F.2d 1216 (10th Cir. 1986) (McKay, J.); *Local Beauty Supply, Inc. v. LaMaur Inc.*, 787 F.2d 1197 (7th Cir. 1986) (Cummings, C.J.); *Business Electronics Corp. v. Sharp Electronics Corp.*, 780 F.2d 1212 (5th Cir. 1986) (Charles Clark, C.J.); *Business Electronics*, 780 F.2d at 1221-22 (Edith Jones, J., concurring); *MCI Communications Corp. v. American Tel. & Tel. Co.*, 708 F.2d 1081 (7th Cir.) (Cudahy, J.), *cert. denied*, 464 U.S. 891 (1983); *Foremost Pro Color, Inc. v. Eastman Kodak Co.*, 703 F.2d 534 (9th Cir. 1983) (Wallace, J.), *cert. denied*, 465 U.S. 1038 (1984); *Berkey Photo, Inc. v. Eastman Kodak Co.*, 603 F.2d 263 (2d Cir. 1979) (Kaufman, J.), *cert. denied*, 444 U.S. 1093 (1980).

8. G. STIGLER, *The Economics of Information*, in *THE ORGANIZATION OF INDUSTRY* 171-90 (1968). For two applications to antitrust, see Easterbrook, *Maximum Price Fixing*, 48 U. CHI. L. REV. 886 (1981); Posner, *Information and Antitrust: Reflections on the Gypsum and Engineers Decisions*, 67 GEO. L.J. 1187 (1979).

9. Telser, *Why Should Manufacturers Want Fair Trade?*, 3 J.L. & ECON. 86 (1960). For related work and extensions, see R. BORK, *THE ANTITRUST PARADOX* 280-309, 429-40 (1978); Easterbrook, *Vertical Arrangements and the Rule of Reason*, 53 ANTITRUST L.J. 135 (1984); Goldberg, *The Free Rider Problem, Imperfect Pricing, and the Economics of Retailing Services*, 79 NW. U. L. REV. 736 (1984); Klein & Saft, *The Law and Economics of Franchise Tying Contracts*, 28 J.L. & ECON. 345 (1985); Liebel, *Intrabrand "Cartels" Under GTE Sylvania*, 30 UCLA L. REV. 1 (1982); Marvel & McCafferty, *Resale Price Maintenance and Quality Certification*, 15 RAND J. ECON. 346 (1984); Marvel, *Exclusive Dealing*, 25 J.L. & ECON. 1 (1982); Mathewson & Winter, *An Economic Theory of Vertical Restraints*, 15 RAND J. ECON. 27 (1984); Mathewson & Winter, *The Economics of Franchise Contracts*, 28 J.L. & ECON. 503 (1985); Posner, *The Next Step in the Antitrust Treatment of Restricted Distribution: Per Se Legality*, 48 U. CHI. L. REV. 6 (1981). It has turned out to be very difficult to test these theories, largely because of difficulties in measuring output. The empirical work to date is composed largely of case or industry stud-

It is curious that the most common brand of criticism today accuses the Chicago School of not learning its own lessons about the dangers of models untested by data. One example recently appeared in this Review, Herbert Hovenkamp's *Antitrust Policy After Chicago*.<sup>10</sup> There are other, similar, critiques. Two of the more thoughtful, to which I will also make some reference, are scheduled to be published shortly.<sup>11</sup> These critics usually doubt that Congress meant courts to use "efficiency" as the single goal of antitrust law. If "efficiency" is the goal, the argument runs, the Chicago School encounters two problems, each fatal. First, economic analysis does not give single "right" answers to problems of industrial organization, so that the courts cannot answer the problems set before them. Second, the Chicago School is too simple. It peddles static, neoclassical analysis, and real markets cannot be analyzed by static analysis. It is necessary to use more complex models that take account of strategic conduct.

These are odd and inconsistent critiques. If it is too hard for courts to determine what is "efficient" using a simple model, how are courts going to decide cases based on complex, strategic models? But inconsistency is a sidelight. I think these critics misunderstand what the Chicago School is about. It would be more informative to rename the approach the Workable Antitrust Policy School, with apologies to the scholars who developed the "workable competition" school. The Chicago School is based on skepticism, not on a belief that hard questions have ready answers. Here are the fundamental points that characterize the Workable Antitrust Policy School:<sup>12</sup>

(1) No antitrust policy should be based on a belief that atomistic competition is better than some blend of cooperation and competition. The right blend varies from market to market.

(2) No antitrust policy should be based on a belief that courts and other institutions of government can identify the "best" structure of a market. The history of regulation demonstrates that intervention

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ies. See, e.g., T. OVERSTREET, *RESALE PRICE MAINTENANCE: ECONOMIC THEORIES AND EMPIRICAL EVIDENCE* (1983); *IMPACT EVALUATIONS OF FEDERAL TRADE COMMISSION VERTICAL RESTRAINTS CASES* (K. Lafferty, R. Lande & J. Kirkwood eds. 1984); Marvel & McCafferty, *The Political Economy of Resale Price Maintenance*, 94 J. POL. ECON. 1074, 1088-94 (1986); Springer & Frech, *Deterring Fraud: The Role of Resale Price Maintenance*, 59 J. BUS. 433 (1986).

10. Hovenkamp, *Antitrust Policy After Chicago*, 84 MICH. L. REV. 213 (1985).

11. Kaplow, *Antitrust, Law & Economics, and the Courts*, 50 LAW & CONTEMP. PROBS. (forthcoming); Fox, *The Normative Implications of Law and Economics in Judicial Decision-Making: Antitrust as a Case Study*, 61 N.Y.U. L. REV. (forthcoming).

12. This is not the same list you can find in Gerhart, *The Supreme Court and Antitrust Analysis: The (Near) Triumph of the Chicago School*, 1982 SUP. CT. REV. 319, or Posner, *The Chicago School of Antitrust Analysis*, 127 U. PA. L. REV. 925 (1979), but different lists serve different purposes. Mine is to reveal the forces that lead to the results that appear in other surveys.

in pursuit of such goals will be unsuccessful or the playground of special interest groups.

(3) Competition is harder than you think. The desire to make a buck leads people to undermine monopolistic practices.

(4) Practices that look monopolistic (because they involve cooperation) may be beneficial. Cooperation is essential in complex economic endeavors. How much is too much is a thorny problem. Questions about practices precede answers about their effects, and if unanswered questions lead to condemnation beneficial things will be damned with the monopolies.

(5) No antitrust policy may safely disregard the survival of complex practices. We may not know what these practices do, but survival in the face of other practices and products indicates that they serve some function. Long-lived practices and structures should be displaced only if there is very sound evidence that they are damaging.

(6) No question should be answered without adequate data. The best data and answers come from a study of the practice. The next-best answers come from extrapolations and interpolations from existing data.

(7) Until we know what a durable business practice does, no one should prohibit the use of that practice. The costs of erroneous prohibitions (and the losses as people trim the vigor of their competition to avoid such prohibitions) are apt to be greater than the losses involved in waiting for better data and analysis before acting.

(8) Until we know the costs of alternative forms of regulation, we should be patient. It is never right to compare the visible costs of reality against a presumed cost-free substitute. Every program has costs, and government failures may be more troubling than market failures because no competitive pressures automatically undermine government failures.

This is a profoundly skeptical program — skeptical of simple models, skeptical of simple analysis, skeptical of the ability of courts to make things better even with the best data. Skepticism is why the Workable Antitrust Policy School seems to favor little other than prosecuting plain vanilla cartels and mergers to monopoly. Its adherents are reasonably sure that these two things are harmful to consumers (though there are scattered doubters); these incurable skeptics doubt that other intervention is worth the costs. There can be an “Antitrust Policy After Chicago” only when these doubts have been overcome, only when data establish the benefits of committing to judges regulatory decisions on the basis of complex strategic models. The Workable Antitrust Policy School is the beneficiary of thirty years of

hard work and hard data, much of it in the *Journal of Law and Economics*. It would be a shame to set off in pursuit of some new model of competition before the data show the likelihood of net benefits.

Antitrust is regulation. Regulation ought not rest on hope that judges will solve complex problems. There is not much more to say, but that won't stop me from pointing out the problems in three mainstays of the new criticism: the argument that the antitrust laws are not based on efficiency, the assertion that the Chicago School is confined to simplistic models, and the hope that judges armed with models of strategic interactions can do better.

## I

Back in 1890 Senator Sherman and his colleagues protested the Sugar Trust and other malefactors and told the judiciary to do something about it. They weren't sure just what. Their statute does not contain a program; it is instead a blank check. The Workable Antitrust Policy School thinks judges should use their authority to separate efficient from inefficient business practices. This sets up the critique by Professors Hovenkamp, Kaplow, Fox, and other thoughtful people that it is silly to attribute to Congress in the late nineteenth century a precognition of the neoclassical analysis of imperfect competition.<sup>13</sup> Economists in 1890 thought that cartels were inevitable, maybe even desirable, and dismissed the Sherman Act as political puffery.<sup>14</sup> Not until the 1930s did the economic profession claim to have a partial equilibrium model of monopoly and oligopoly.<sup>15</sup>

So what? The choice is not between believing that Senator Sherman anticipated Joan Robinson and believing that the antitrust laws should be used to redistribute wealth from the titans of industry to "small dealers and worthy men."<sup>16</sup> Members of Congress did not see themselves choosing between "efficiency" and some other goal. The

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13. *E.g.*, Kaplow, *supra* note 11: "The position of some Chicago School advocates in this area represents an instance in which their arguments are so incredible that it is hard to take them seriously."

14. A careful student of the history of economics would have searched long and hard, on the unseasonably cool day of July 2 of 1890, the day the Sherman Act was signed by President Harrison, for any economist who had ever recommended the policy of actively combating collusion or monopolization in the economy at large.

G. STIGLER, *The Economists and the Problem of Monopoly*, in *THE ECONOMIST AS PREACHER* 38, 41 (1982).

15. Joan Robinson's *THE ECONOMICS OF IMPERFECT COMPETITION* (1933) and Edward Chamberlin's *THE THEORY OF MONOPOLISTIC COMPETITION* (1933) put the theory together, but both have substantial intellectual debts to Frank Knight's *RISK, UNCERTAINTY AND PROFIT* (1921) and many other works. See D. DEWEY, *THE THEORY OF IMPERFECT COMPETITION* 5-23 (1969); G. STIGLER, *ESSAYS IN THE HISTORY OF ECONOMICS* 239-44 (1965).

16. *United States v. Trans-Missouri Freight Assn.*, 166 U.S. 290, 323 (1897).



choice they saw was between leaving consumers at the mercy of trusts and authorizing the judges to protect consumers. However you slice the legislative history, the dominant theme is the protection of consumers from overcharges.<sup>17</sup> This turns out to be the same program as one based on "efficiency." There are differences at the margins, such as what if anything to do about price discrimination that rakes in money for large firms but may increase output, but the differences are not very important. In the long run consumers gain the most from a policy that emphasizes allocative and productive efficiency. The few references in the legislative history to "small dealers" are a sideshow. Courts can use economics to protect consumers; they cannot achieve any other goal except at some cost to consumers, and they are not authorized to decide how much should be surrendered for whose benefit. The Handicapper General in Kurt Vonnegut's novels ensured that no one could use his natural abilities to get an advantage over rivals. The Handicapper General levels down. If antitrust serves the role of Handicapper General, stamping out practices by which some firms make products cheaper or satisfy too many consumers (thereby attracting too much of the business), consumers lose. That is the one outcome the ambiguous legislative history places off limits.

Judges ought not read a statute that speaks of competition, monopoly, and other economic terms, written against a legislative history that evinces concern for low prices and consumers' welfare, as if Congress winked and really meant to pursue a bunch of inconsistent goals.<sup>18</sup> Courts exercising a common law power should do their best to have a sensible, consistent program. That means a single goal, for a program that calls on almost a thousand federal judges to maximize multiple, competing goals will yield incoherent results. Given *A* and *not-A*, you can "prove" anything you like. No sane program grants such power to judges and juries. It is not a power to enforce "law" at all, and a multi-goal policy hides from Congress what is afoot. The clearer the courts' focus, the easier Congress' time in knowing when (and how) to step in and make things to its liking.

Goals based on something other than efficiency (or its close proxy

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17. See W. LETWIN, LAW AND ECONOMIC POLICY IN AMERICA: THE EVOLUTION OF THE SHERMAN ANTITRUST ACT 53-99 (1965); Bork, *Legislative Intent and the Policy of the Sherman Act*, 9 J.L. & ECON. 7 (1966); Clark, *Antitrust Comes Full Circle: The Return to the Cartelization Standard*, 38 VAND. L. REV. 1125, 1140-46 (1985); Lande, *Wealth Transfers as the Original and Primary Concern of Antitrust: The Efficiency Interpretation Challenged*, 34 HASTINGS L.J. 65, 93-96 (1982); cf. Baxter, *Separation of Powers, Prosecutorial Discretion, and the "Common Law" Nature of Antitrust Law*, 60 TEXAS L. REV. 661 (1982).

18. See 1 P. AREEDA & D. TURNER, ANTITRUST LAW ¶¶ 103-13 (1978); R. BORK, *supra* note 9, at 69-89; Areeda, *Introduction to Antitrust Economics*, 52 ANTITRUST L.J. 523, 535-36 (1983).

consumers' welfare) really call on judges to redistribute income. How much consumers should contribute to small grocers is a political choice. Judges have no metric, and we ought not attribute to Congress a decision to grant judges a political power that lacks any semblance of "legal" criteria. The Workable Antitrust Policy School is aware that some people may get more utility out of money than others.<sup>19</sup> But nothing follows from this observation. We cannot readily assume that monopoly profits land in the pockets of cats who are already fat. Profits of big firms end up in federal coffers through taxes, and in the hands of the meek, whose pension money is invested in stock. Some are captured by unionized workers. Monopoly profits therefore may end up in the same sort of pockets from which they departed. Perhaps Congress uses differences in the marginal utility of money to decide whether to redistribute income, but unless redistribution is a goal of antitrust, judges ought not to worry. The observation that money is worth different amounts at the margin to different people could as easily direct income toward the "utility monster" (the person who gets fabulous pleasure from oodles of extra money or from gruesome deeds) as toward consumers or small businesses. Many things that are relevant in principle — the theory of the second-best is the most important — are ignored every day in antitrust. A cartel *could* be best for everyone given other distortions in the economy, but judges ignore such intractable arguments. Cartels are hurtful. The difference between "always" and "almost always" is unimportant in antitrust whether it cuts against or for a practice. Rules that do well on average are the best courts can produce and apply. Judges know that the pursuit of the perfect is the enemy of the good. We take Occam's Razor and slice off ideas that cannot contribute to useful legal rules.

Interpersonal utility comparisons join the theory of the second-best on the scrap pile of useless truths, not only because no one knows who gets how much utility from how much money but also because judges aren't very good at moving money around. A policy designed to hobble large businesses in order to help small ones (or just to prevent the flow of money toward the large ones) may not redistribute money at all. New entrants will undercut the policy and dissipate profits, or the removal of efficient firms will drive up prices so that consumers still pay "too much" for their bread. The growth in the size of markets — reductions in the cost of transportation and communication, coupled with floating exchange rates, make more markets nationwide or worldwide — means that outlawing a practice used by one firm may not

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19. Hovenkamp, *supra* note 10, at 235-48. See R. BORK, *supra* note 9, at 110-15 (discussing this and other points more useful in principle than in practice).

make life easy for another "small" business. It may simply shift the comparative advantage to a business in California or Korea. The insistent process of competition undermines the ability of judges to transfer wealth by changing legal rules.<sup>20</sup>

So although Professor Kaplow, for example, says that judges haven't been convinced by the Workable Antitrust Policy School so much as they have decided to indulge their ideologies using economic jargon,<sup>21</sup> he may have identified a different phenomenon. Twenty years ago, when the antitrust decisions of the Supreme Court ran to populism, there were more small markets than there are today. Courts may be able to protect "small dealers and worthy men" in small markets. But today's cases involve the influx of Japanese television sets, for example, and the "small dealers" in protected markets turn out to be rapacious cartelizers.<sup>22</sup> Justices can learn as well as the rest of us, and when they see the small dealers conspire — and find out that protective policies don't work — they are apt to revise their thoughts no matter what their political views. Many of today's decisions in the Workable Antitrust Policy School are unanimous. It is not that conservatives outvote liberals; it is that everyone has learned from experience — both personal experience and the kind of second-hand experience that expert witnesses, scholarly essayists, and law clerks carry to the judicial process.<sup>23</sup> The ability of judges to learn from other people's experience is one of the glories of a common law system. The Sherman Act set up a common law system in antitrust. The statute and its legislative history authorize the ongoing transition to an efficiency-oriented approach.

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20. See Epstein, *The Social Consequences of Common Law Rules*, 95 HARV. L. REV. 1717 (1982); Schwartz, *Justice and the Law of Contracts: A Case for the Traditional Approach*, 9 HARV. J.L. & PUB. POLY. 107 (1986); see also Easterbrook, *Is There a Ratchet in Antitrust Law?*, 60 TEXAS L. REV. 705, 714-17 (1982).

21. Kaplow, *supra* note 11.

22. See *Matsushita Elec. Indus. Co. v. Zenith Radio Corp.*, 106 S. Ct. 1348 (1986) (TV sets from Japan); *FTC v. Indiana Fedn. of Dentists*, 106 S. Ct. 2009 (1986) (agreement in restraint of trade among sole practitioners of dentistry). Many of the most blatant cartels are among small firms in the construction industry, who rig bids for fun and profit.

23. Of the cases cited in note 7 *supra*, *Brunswick* (1977), *Fortner II* (1977), *Broadcast Music* (1979), *Reiter* (1979), *Monsanto* (1984), *Hyde* (1984), *NCAA* (1984), *Pacific Stationery* (1985), *Aspen* (1985), and *FTC v. Dentists* (1986), are unanimous or close to it. (In *Hyde* the Justices split 5-4 about whether to adopt the Chicago approach whole hog or only part way.) See also *National Socy. of Professional Engrs. v. United States*, 435 U.S. 679 (1978); *Catalano, Inc. v. Target Sales, Inc.*, 446 U.S. 643 (1980). Professor Kaplow seems to think that the Chicago School pins its all on a single quotation in *Reiter* from Judge Bork's book. Kaplow, *supra* note 11. Hardly. These cases show a general willingness to use economics thoughtfully in antitrust cases — not a revolution, certainly, but a change in emphasis often punctuated by explicit reliance on scholarly work in the Chicago School tradition. *E.g.*, *Matsushita*, 106 S. Ct. at 1357-59; *Aspen Skiing Co. v. Aspen Highlands Skiing Corp.*, 105 S. Ct. 2847, 2857-61 (1985); *Continental T.V., Inc. v. GTE Sylvania Inc.*, 433 U.S. 36, 48 n.13, 54-57 (1977).

## II

The theme of modern critics of the Chicago School is that it is simplistic. Its practitioners apply a neoclassical model that assumes rational actors and an inevitable drive toward production at marginal cost.<sup>24</sup> The assumptions of the neoclassical model are unrealistic, and so it seems we must conclude that the results are unreliable.<sup>25</sup>

What's wrong with models that contain "unrealistic" assumptions? The purpose of any model is to strip away complications, to make intractable problems manageable, to make things simple enough that we can see how particular variations matter. Without simplification we do not know what to look for. Any approach to antitrust must simplify; modeling is essential; the best model is the simplest one *that can cope with the data*.

Newton's model of gravitation assumes a perfect vacuum. There aren't any perfect vacuums in this universe, but the model is still pretty useful — and it is useful even though Einstein showed it to be wrong. Newtonian dynamics, flawed as they are, give very good approximations for practical use by people sending Voyager 2 to Neptune or baseballs to home plate. Judges need useful models, not "complete" ones, and the test of a model's utility is its ability to generate good predictions. The neoclassical model does very well. It handles small-scale changes and large-scale changes alike. The deregulation of prices on the stock exchange, in the air transportation business, and in the natural gas industry all are tractable using fairly simple models, and neoclassical models have done better than any alternative in predicting and understanding the effects of these

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24. See, e.g., Hovenkamp, *supra* note 10, at 226-29, 256-60; Fox, *supra* note 11; Spivack, *The Chicago School Approach to Single Firm Exercises of Monopoly Power: A Response*, 52 ANTI-TRUST L.J. 651, 669-72 (1983). There are many more in the same vein.

25. A minor theme of the critics is that Chicago just stands up for whatever assists Big Business. See Hovenkamp, *supra* note 10, at 233 ("[I]t is easy to identify the beneficiaries of Chicago School antitrust policy — probably big business, certainly vertically integrated firms, perhaps some consumers."). Now there is a good deal of truth in Secretary Wilson's crack that "What's good for General Motors is good for America" — a country cannot be prosperous if its industries fail. Wealth flows from efficient productive activity, and in a capitalist economy productive activity among consenting adults is apt to make everyone better off. The Chicago School distinguishes social wealth from profits, however. Regulation often stifles competition (there were no new trunk airlines for forty years before 1978); the Chicago School supports less regulation and freer entry to the detriment of the vested interests that hold existing operating rights. Empirical work in the *Journal of Law and Economics* has established that much regulation is pro-producer and anti-consumer; the Chicago School wants to deregulate. Many large firms seek restrictions on imports, which erode sales and profits; the Chicago School stands for free trade and opposes tariffs, quotas, and other exclusionary devices. Many managers of large corporations want to be secure in their jobs; the Chicago School generally supports free competition in the market for corporate control, even though this terrifies managers. This set of views is not a likely combination for a pro-business (as opposed to pro-consumer) approach.

changes.<sup>26</sup> So, too, neoclassical models have done well in understanding problems of industrial organization, as the demolition of the structure-conduct-performance paradigm shows.

When data tell the tale, a scholar protesting that one model is "too simple" should produce a model that fits the data better. I am confident that a better model will be found. The history of science shows that one will be. But we must make do at any time with the best available model. Creative scholars serve society well by designing and testing new models; judges serve society well by treating models with skepticism until they explain data better.

I find it odd, however, to hear that the Workable Antitrust Policy School is the embodiment of any one model. The thoughts associated with Chicago came from *doubts* about the model of atomistic competition. Through much of this century antitrust policy has come to grief because it was under the sway of that model. Judges were apt to condemn every practice that did not look like hearty yeomen competing from moment to moment. The prescription of this model, the dissolution of much economic organization, also would have dissolved efficient forms of cooperation. The task of antitrust policy is to find the right balance of competition and cooperation.<sup>27</sup> No one I know thinks that this task can be done with models uninformed by data, whether the models are simple or complex.

Scholars at Chicago and elsewhere used the neoclassical model to show what is wrong with a yearning for perfect, atomistic competition and to show why no one should be concerned about practices of firms that lack market power. There is broad agreement on these conclusions; they are produced by almost any other plausible economic model. It hardly follows that the scholars are captives of their own efforts to simplify.

What Professor Hovenkamp and others mean by "static" models is models that disregard the effects of time and strategic behavior, models in which the results at one instant are the same as the results at any other. So if at any instant a firm is not making a profit by use of a tactic, it will never make a profit in the model. The appellation is a technical one in the sense that it describes a particular form of equations. It is also sometimes meant to be pejorative, carrying the implication that the simplification is an *oversimplification*. The charge of

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26. See, e.g., Haas-Wilson, *The Effect of Commercial Practice Restrictions: The Case of Optometry*, 29 J.L. & ECON. 165 (1986); Jarrell, *Change at the Exchange: The Causes and Effects of Deregulation*, 27 J.L. & ECON. 273 (1984); Moore, *U.S. Airline Deregulation: Its Effects on Passengers, Capital, and Labor*, 29 J.L. & ECON. 1 (1986).

27. See Easterbrook, *The Limits of Antitrust*, 63 TEXAS L. REV. 1 (1984).

oversimplification requires proof, and proof means the production of a more complex model that explains the data better. It is easy to produce more complex models. There is no outcome, however obvious or absurd, that cannot be demonstrated by the use of some exceptionally complex model. Do these models explain anything better? Who knows?

Many models used in the Workable Antitrust Policy School are static, but many are not. The economics of information is dynamic. It requires multiple periods of shopping and experimenting until an equilibrium is reached. The economics of resale price maintenance developed by Lester Telser and others is dynamic. People at Chicago (physically or mentally) have written about the dynamics of predatory practices, boycotts, and other multi-period devices.<sup>28</sup> People at Chicago contributed mightily to the economics of oligopoly, which is about posturing, signalling, and other dynamic tricks. The Chicago School's standard critique of regulation is that regulators are too simple-minded in assuming that there is not a response to regulation. Rent control calls forth "key money" and other effects. People who think that oligopoly and monopoly are the same thing underestimate dynamic forces in markets that lead to competition. The list of "dynamic" points associated with Chicago is quite long.

It is simply flabbergasting to be told that the Chicago School is committed to one model for all economic phenomena, no matter how inapt the model may be. Dynamic phenomena such as exclusionary conduct should be assessed by dynamic models. This is very hard to do; developments in game theory show that trivial variations in assumptions can produce stupendous differences in results, if the models predict any results.<sup>29</sup> But there's no fun and little scholarly reward in showing the obvious, so dynamic models are being developed everywhere.

What characterizes Chicago is skepticism, not adherence to a set model. Rule out judges' doing anything useful about redistributing

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28. See, e.g., Easterbrook, *Predatory Strategies and Counterstrategies*, 48 U. CHI. L. REV. 263 (1981); R. BORK, *supra* note 9, at 347-64 (predation through use of the government). Other work deals with opportunistic behavior and strategic responses to it. E.g., Joskow, *Vertical Integration and Long-term Contracts: The Case of Coal-burning Electric Generating Plants*, 1 J.L. ECON. & ORG. 33 (1985); Klein, Crawford & Alchian, *Vertical Integration, Appropriable Rents, and the Competitive Contracting Process*, 21 J.L. & ECON. 297 (1978); Williamson, *Credible Commitments: Using Hostages to Support Exchange*, 73 AM. ECON. REV. 519 (1983).

29. See M. SHUBIK & R. LEVITAN, *MARKET STRUCTURE AND BEHAVIOR* (1980); L. TELSER, *COMPETITION, COLLUSION, AND GAME THEORY* (1972). Changes in the assumptions and rules of interactions also have large effects in experimental economics and experimental game theory. See R. AXELROD, *THE EVOLUTION OF COOPERATION* (1984); Plott, *Industrial Organization Theory and Experimental Economics*, 20 J. ECON. LITERATURE 1485 (1982).

income, and you have an economic approach. Rule out confidence that judges are good at doing better than the results of markets — flawed though markets may be — and you have a skeptical economic approach. The remaining element that makes the Chicago School distinctive is adherence to the assumption of rational behavior. This, too, is common among economic modelers of all stripes, and well it should be. Economics is the study of rational behavior under constraint; dynamic as well as static approaches share this assumption. And it is an essential assumption for antitrust, because if you assume people are not rational (on average) then you may as well give up trying to influence their behavior. Drop the assumption of rationality and you have no reason to think that even perfectly competitive markets will be good for consumers. Drop the assumption of rationality and you have no reason to think that the threat of sanctions deters, or that the legal system can achieve anything useful. The triad of data showing that markets do pretty well even with a few large firms (this is the meaning of refuting the structure-conduct-performance paradigm), the assumption of private rationality, and the skepticism that we can do better through regulation has proven a powerful combination.

### III

The development of complex models is one thing, proof of their utility is another. This is where Professor Hovenkamp's last point comes in. He and other scholars believe that models of strategic conduct are the wave of the future and should inform antitrust policy.<sup>30</sup> Of course useful information gleaned from dynamic approaches should inform antitrust policy. But there is a tradeoff between complexity and accuracy in implementation. One of Professor Hovenkamp's points is that static, neoclassical analysis often does not say what the right organization of a market may be — a point with which skeptics of all sorts enthusiastically agree.<sup>31</sup> If simple models do not give determinate results, what are we to make of complex models? Has the history of antitrust and regulation been so satisfying that we are now convinced that judges can manipulate large data bases and mammoth models? The lesson drawn more often from "simple" enterprises, such as rate regulation and the FTC's efforts to compute

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30. Hovenkamp, *supra* note 10, at 260-83; see also Fox, *supra* note 11; Kaplow, *Extension of Monopoly Power Through Leverage*, 85 COLUM. L. REV. 515 (1985).

31. Even me. Much of my work has been about the difficulty of drawing conclusions with the best of information. *E.g.*, Easterbrook, *supra* note 27; Easterbrook, *On Identifying Exclusionary Conduct*, 61 NOTRE DAME L. REV. 972 (1986) [hereinafter cited as Easterbrook, *Exclusionary Conduct*].

“costs” for purposes of cost-justification defenses, is that efforts to take everything into account leave chaos and error in their wake.<sup>32</sup> And attempts to take account of “nature in all its fullness” predispose regulators to intellectual corruption. It becomes too easy to seize on a particular factor to “justify” an answer come to by other means.

Strategic modeling is without doubt important work. It is also very old work and very indeterminate work. “Raising rivals’ costs” is a form of strategic modeling that overlaps old models of “barriers to entry” — the idea in either event is that incumbent firms can raise their own prices when their rivals’ marginal costs go up.<sup>33</sup> Raising rivals’ costs is a form of exclusionary conduct. It must be taken seriously; exclusion and cooperation are the principal ways to make monopoly profits. But raising rivals’ costs also could be a way to control wasteful investments. Enterprising scholars have demonstrated manifold ways in which “exclusion” or cartel-like devices could be highly beneficial for strategic reasons.<sup>34</sup>

Every thoughtful scholar in antitrust works on these problems. They are very deep, however, because it is almost impossible to distinguish exclusion from hard competition. The *Aspen* case, which Professor Hovenkamp discusses, shows the difficulties.<sup>35</sup> The operator of slopes on three mountains near Aspen stopped cooperating with the operator of slopes on a fourth. Professor Hovenkamp takes this as an illustration of one firm raising its rival’s cost by withdrawing an efficient form of cooperation that was more valuable to the rival than to the aggressor. This is a logical possibility. But perhaps the operator of one slope was taking a free ride (slide?). The operator of the three slopes developed Aspen as a resort and advertised heavily to attract skiers to its mountains. The cooperative method in question — a joint lift ticket good at any of the four mountains — enabled the fourth

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32. See S. BREYER, *REGULATION AND ITS REFORM* (1982).

33. Compare Salop, Scheffman & Schwartz, *A Bidding Analysis of Special Interest Regulation: Raising Rivals’ Costs in a Rent-Seeking Society*, in *THE POLITICAL ECONOMY OF REGULATION: PRIVATE INTERESTS IN THE REGULATORY PROCESS* 102-27 (1984), and Krattenmaker & Salop, *Competition and Cooperation in the Market for Exclusionary Rights*, 76 *AM. ECON. REV. (Papers & Proceedings)* 109 (1986), with Demsetz, *Barriers to Entry*, 72 *AM. ECON. REV.* 47 (1982), and G. STIGLER, *Barriers to Entry, Economies of Scale, and Firm Size*, in *THE ORGANIZATION OF INDUSTRY* 67-70 (1968). See also *Ball Memorial Hosp., Inc. v. Mutual Hosp. Ins., Inc.*, 784 F.2d 1325, 1339 (7th Cir. 1986); T. Brennan, *Understanding “Raising Rivals’ Costs”* (Dept. of Justice Working Paper Sept. 1986).

34. See Bittlingmayer, *Decreasing Average Cost and Competition: A New Look at the Addyston Pipe Case*, 25 *J.L. & ECON.* 201 (1982); Demsetz, *supra* note 33; Telsler, *Cooperation, Competition, and Efficiency*, 28 *J.L. & ECON.* 271 (1985); von Weizsäcker, *A Welfare Analysis of Barriers to Entry*, 11 *BELL J. ECON.* 399 (1980).

35. Hovenkamp, *supra* note 10, at 280-83 (discussing *Aspen Skiing Co. v. Aspen Highlands Skiing Corp.*, 105 S. Ct. 2847 (1985)).



mountain to divert customers once they got there, no matter who was responsible for attracting them. Even in the days of free access to any mountain, the fourth mountain attracted on average only sixteen percent of the skiers who used joint tickets, showing that these skiers thought it an inferior slope. The larger firm's initial demand was that the smaller accept 12.5 percent of the receipts instead of sixteen percent. This may have been no more than a way of requiring the smaller firm to pay for access to the pool of customers that were produced by its rival's hard work and expensive advertising, improvements, and so on.

Which explanation best handles the data? We do not know, because the litigants did not collect important information.<sup>36</sup> It would be no mean feat to find out, because both explanations predict that after the new practice has been established prices will rise in the market and the market share and profits of the smaller firm will fall. It turns out that the *only* data that will separate the exclusion explanation from the competition explanation is output in the market as a whole, and output is exquisitely hard to measure. You must weight number of skiers by the quality of the product and then control for other variables (such as poor snow, an increase in air fares, or a recession) that might affect skiing in Aspen and everywhere else. You may need to take into account how prices at hotels change — if the practice is exclusionary, perhaps hotels will bear the brunt of the loss as they reduce room rates to stay full. Only the most sophisticated consideration of the data could separate the efficiency from the exclusionary explanation in *Aspen*.

This highlights the problem of a more complex antitrust. Overreaching the limits of adjudication will increase the rate of error.<sup>37</sup> We want to hold to a minimum the sum of the costs of harmful activity wrongly condoned and useful activity wrongly condemned (or discouraged). It is fallacious to say that because Rule *X* is better in principle than Rule *Y*, courts should use Rule *X*. That's the Nirvana Fallacy — it assumes away the costs of administration and error. Rule *Y* (here a fairly simple paradigm based on neoclassical models) may be simpler and may confine errors to the unusual cases, cases in which competition ultimately will erode the monopoly anyway. Rule *X* (here a complex review of strategic interactions) may yield error in

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36. See Easterbrook, *Exclusionary Conduct*, *supra* note 31 (discussing *Aspen* and the difficulty of separating exclusionary from aggressively competitive conduct).

37. See Baxter, *Reflections Upon Professor Williamson's Comments*, 27 ST. LOUIS U. L.J. 315, 320 (1983); Schmalensee, *Antitrust and the New Industrial Economics*, 72 AM. ECON. REV. (Papers & Proceedings) 24, 27 (1982).

what should be the core of protected conduct — hard competition that gets mistaken for “exclusion.” I should much rather see unusual, fancy practices wrongly excused than take the chance that courts will condemn or discourage aggressive competition.<sup>38</sup>

Ignorance is the central problem in antitrust, in litigation, in life. We have trouble telling what business practices do. (When a manufacturer says that it has adopted some restricted resale arrangement “to protect my dealers’ margins,” does that mean “to create monopoly profits” or “to induce the supply of services consumers value”?) We have trouble applying models to the facts of cases. Explanations and data may not be developed until the case is over. Until we learn how to deal with ignorance, we cannot make constructive use of fancy strategic models. And although Professor Hovenkamp tells us that a court “cannot defer judgment until all the evidence is in,”<sup>39</sup> it can and should wait for enough evidence to be confident. Until data permit a sound judgment that a certain type of practice is harmful, the courts should say that the plaintiffs have not carried their burden. In antitrust, as in other litigation, ties go to the defendant. The proposition that a court must leave the world as it is, unless there is a very good reason to compel a change, is not exactly novel.<sup>40</sup>

Skepticism should rule the day. The Workable Antitrust Policy School has a skeptical program — rather several.<sup>41</sup> The Supreme Court has decided, for good or ill, to put these programs to the test. The *Matsushita* case makes some simple economic inquiries the centerpiece of litigation.<sup>42</sup> Even these simple inquiries often yield ambiguous answers, and then courts must decide where the burden of ignorance falls. The principle that the proponent of intervention in markets must both show the injury from what is, and demonstrate that what would come next is better, means that ignorance often leads to

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38. I had rather expected Professor Hovenkamp to be on my side in this respect. Maybe half of him is, because his article is littered with recognition that even a “simple” Chicago-style proposal — such as the use of output measures to distinguish beneficial from detrimental conduct — exceeds the abilities of the judicial process, if not the economics profession. See, e.g., Hovenkamp, *supra* note 10, at 225-26, 241, 244, 258 n.213. If simple tests are too complex, what are we to make of complex models? Does radical uncertainty counsel intervention by courts?

39. Hovenkamp, *Rhetoric and Skepticism in Antitrust Argument*, 84 MICH. L. REV. 1721, 1724 (1986).

40. “[T]he prevailing view is that [the] cumbrous and expensive machinery [of the courts] ought not to be set in motion unless some clear benefit is to be derived from disturbing the *status quo*. State interference is an evil, where it cannot be shown to be a good.” O.W. HOLMES, THE COMMON LAW 96 (1881).

41. See R. BORK, *supra* note 9; Easterbrook, *supra* note 27; R. POSNER, *supra* note 1. Areeda and Turner today count as skeptical economists, and their treatise lays out a complex set of proposals. See P. AREEDA & D. TURNER, *supra* note 18.

42. *Matsushita Elec. Indus. Co. v. Zenith Radio Corp.*, 106 S. Ct. 1348, 1354-60 (1986).

inaction. But this is preferable to condemning complex practices on the basis of indeterminate models and of hopes, however sincere, that the replacements for these complex practices would be better. Anti-trust has traveled that road before.