

## **Commentary: Kenneth Davidson, And the Walls Came Tumbling Down: Maurice Stucke's Article on Antitrust in the 21<sup>st</sup> Century**

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### COMMENTARY

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### AND THE WALLS CAME TUMBLING DOWN

A Commentary on Maurice Stucke's article "Behavioral Economists at the Gate: Antitrust in the Twenty-First Century" 38 *Loyola Univ. Chicago L.J.* 513 (2007)

This is a landmark article. In the forty year since I graduated from law school, it is the most coherent discussion that I've read of what we call antitrust law in the United States. Stucke, an attorney in the Antitrust Division, meticulously takes apart the law and economics house built by Posner, Bork, Stigler, Friedman and others of the Chicago School and proposes a different way of conceptualizing anticompetitive behavior using Behavioral Economics.

His primary focus is on the Merger Guidelines developed by law professor William F. Baxter when he was Assistant Attorney General for Antitrust in the early 1980s. The Guidelines are an especially good lens with which to examine the assumptions of the Chicago School because, as Stucke notes, "[F]or Posner and others, the intellectual journey for merger review has come to an 'end' – an ending that represents 'a modest vindication of the Chicago School's approach'." Or as Posner also put it less modestly, the Guidelines represent "the triumph of the economic approach."

The Guidelines not only incorporate the Chicago assumptions, their methodology is intentionally transparent so that both private attorneys and government antitrust agencies can determine in advance the likelihood that a merger will be challenged. Transparency provides a measure of predictability to antitrust parties; it also sets forth the detailed set of assumptions that determine the outcome of merger review. Thus, any analyst can use this transparency to examine whether there is evidence to support those assumptions. Through the use of well known findings of behavioral economists and the mining of much neglected data concerning civil and criminal cartel cases brought by the Justice

Department, Stucke's look inside the transparent Chicago house reveals that its naked assumptions are without empirical support. He finds to the contrary that Behavioral Economists' portrait of business decision making are more consistent with years of cartel cases.

Stucke's writing is to be admired for its clarity, persuasiveness and erudition. Who would have thought to find trenchant citations in an antitrust law review article to John Stuart Mill, Soren Kirkegaard, Aristotle, John Milton, Plutarch, Seneca, Marx and Engels, and Adam Smith's *The Theory of Moral Sentiments*? Unlike so many in academic articles, he does not avoid the writings of the school he is criticizing. He quotes liberally from the works of Bork, Becker, Stigler, Milton Friedman, David Ricardo, Ronald Coase, Aaron Director, and of course Posner.

Indeed, to set up the subject of this article, Stucke noted that Coase complained when receiving the Nobel Prize for Economics that his economist colleagues engaged in "blackboard economics:" "What is studied is a system which lives in the minds of economists but not on earth . . . . The firm and the market appear by name but they lack any substance." Stucke notes a more wistful expression of the same sentiment by Posner about the lack of empirical research investigating the work of Aaron Director, one of the founders of the Chicago School: "It is a curiosity, and a source of regret, that to this day [1979] very few of Director's ideas have been subject to systematic examination."

As Stucke explains, the absence of empirical work on Chicago School theory is a function of the nature of the theory. Chicago neoclassical economics is built on the "profit-maximization assumption." That assumption allows the economist to predict how consumers and producers will act. Consumers will buy the cheapest good of the best quality and service. Producers will be required to conform their production to these criteria or be driven out of business by their rivals. To the extent that individual consumers or producers deviate from this pattern, they will soon find that Adam Smith's Invisible Hand will have eliminated those products. Thus while it is easy to find exceptions to rational choice, those exceptions are temporary and not representative of market forces that will bring production and consumption into equilibrium. The rational choice assumption is thus "crucial" to escape the intractable morass of facts that will be disclosed by the study of market behavior of any particular sets of transactions. The theory built on an assumption of rational profit maximizing behavior is deductive and thus not easily falsifiable by fact.

The assumptions, when elaborated, posit that consumers' preferences are stable and known to both the consumer and the producer, that consumers can distinguish between better and worse products (making their personal trade-off between price and quality) and choose the better products in accordance with their needs and wants. The producer is equally rational and omniscient, knowing his costs, competitors and consumer demands. The real world mistakes that consumers or producers make create a lot of factual noise making empirical proof difficult but the tendency of the market toward equilibrium is inevitable.

Stucke uses the recent empirical work of behavioral economists to attack the assumption that rational choice, as defined by the Chicago School, is the dominant force in determining market outcomes. His citations to Thaler, Kahneman, and Levitt compile a lot of experimental data showing that individuals' preferences deviate substantially and consistently from the "economic man" posited by the Chicago School.

This is not new. From the beginning of the twentieth century, there have been many who thought the assumptions of the neoclassical economics were too narrow and artificial. From Thorstein Veblen's Theory of the Leisure Class, to Schumpeter's Theory of Economic Development, to Keynesian macroeconomics, the static microeconomic analysis of the developing Chicago School looked naive.

As an arrogant undergraduate at the University of Chicago (which may be redundant) in the early 1960s, I was surprised anyone would take seriously a theory of human behavior with such simplistic premises. So far as I recall, there were no economics courses offered in the College of the sort being taught in the graduate economics department, the business school or the law school. We did not read Alfred Marshall, George Stigler, or Milton Friedman despite the fact that the latter two were alive and teaching at Chicago along with Ronald Coase, Aaron Director and Henry Simon. We read Aristotle, Plato, Thucydides, Galileo, Locke, Rousseau, the Federalist papers, Darwin, Mendel, Marx, Freud, Kant, Veblen and Robert Redfield. Redfield, then an icon of the Chicago anthropology school, summarized for some of us in his classic, *The Little Community*, the thesis that a single analytic framework such as kinship systems, religious tenets, political structures, economic relationships, etc. each contained a bias that could be corrected only by integrating multiple analytic modes.

As Redfield might have said, the depiction of Chicago's economic man was not wrong, it was and is incomplete. All theories are incomplete. That is in part the role of theory: to simplify in order to make a complex process understandable. Einsteinian physics can calculate gravitational effects more precisely than Newtonian physics, but for most of us it is enough to know that most objects will fall if not held up. Consequently the question is whether there are better theories of economic interactions than Chicago price theory.

For me, the question has been answered over years of reading and experience working at the FTC. I was especially impressed by the seemingly irrefutable logic of Kenneth Arrow's critique of the price theory that formed Chicago economics. Arrow, an early economic Nobel, observed like many others that the price system did not work to control many undesirable externalities such as pollution. He went on in his 1970 Fels Lecture to point out that the logic of a price system is inherently incomplete because it failed to account for the existence of trust, an indispensable market ingredient. In his words:

Trust is an important lubricant of a social system. It is extremely efficient; it saves a lot of trouble to have a fair degree of reliance on other people's word. Unfortunately this is not a commodity which can be bought very easily. If you have to buy it, you already have doubts about what you have bought. Trust and

similar values . . . . are not commodities for which trade on the open market is technically possible or feasible.

This struck me as a flaw so basic that its absence had to distort Chicago analysis. How it would alter economic analysis was not clear beyond Arrow's point that in some cases government regulation is superior to an unregulated market.

Much of my other reading in history, science, philosophy, strategy, business and economics confirmed many of my initial biases. The Hobbesian conclusions of 19<sup>th</sup> Century Social Darwinists that the life of the jungle is red in tooth and claw, like the theorists of profit maximizing seem to have got it wrong. Nature and especially evolution seem to be more about cooperation than they are about competition. From early cellular development that combined formerly separate entities that added to the RNA cell, the DNA nucleus, and mitochondria to form a more complex entity capable of sexual reproduction, to an Earth that produced sufficient quantities of oxygen to allow animals to exist, to species like insects that survive only as cooperating colonies, to humans who exist only because of the voluntary care by their parents we find example after example of nets of cooperation that are as common, if not more common, than the competitive struggle for life.

Evolution and human history appear to be more the result of accident or chance than anything like survival of the fittest. Their mechanisms are opportunistic ones that favor those more adaptable to circumstances that change around them. Species die from meteors that change the atmosphere, from success that creates overcrowding, from viruses that do not produce antibodies. Other species may then enlarge, mutate or change to take advantage of new opportunities. Human history shows much the same role for fortuity. Populations grow fastest where there are indigenous cultivatable grains. Surplus food allows for the emergence of elites who rule by force and knowledge but who are continually overwhelmed by new populations who have superior force, technology, or new diseases or they decline because they destroy the cooperation or the resources that had made them strong.

Charles Lindblom made a valiant attempt in his 1977 book, *Politics and Markets*, to generalize how human systems operate. He proposed that societies are regulated by three forces: the use of power, voluntary exchanges in the market, and ethical beliefs. Civilizations generally endure through some combination of these forces. He argued so persuasively that some combinations of these forces were stronger than others that the policy offices of the FTC in the late 1970s invited him to advise the Commission on what mixture would best suit the United States. How much regulation? How much market exchange? How much faith? He responded that he thought we were the experts. He may have been right: his example of a well balanced economy was Tito's Yugoslavia which then seemed to him to have the best of market capitalism and best of social planning. Oh well, the policy offices' search for the holy grail of effective governance proceeded from other political scientists, to the nation's leading business historians, to business strategists, to experts in oil and finance. They had much to say and much of it was

criticism, although they generally liked FTC deregulation efforts. But they had little advice on how the FTC could do its antitrust job better.

The search, the debate ended with the election of Ronald Reagan. Like Isaiah Berlin's hedgehog, the new administration knew only one thing, but they knew it well. Given a choice, a consumer would pay less if he was offered the same product at two prices. Price is all that mattered. Everything could be deduced from that principle.

So what have Stucke's behavioral economists done to alter the price theory paradigm? They have shown in study after study that the price does not always matter. Stucke cites the works of Amos Tversky, Daniel Kahneman, and Richard Thaler who build on the work of Herbert Simon and James March to create a portrait of economic man based on repeated studies of behavior rather than axioms derived from the single assumption that, absent all else, a person would prefer to pay less. This literature has been around since the mid 20<sup>th</sup> Century when Simon demonstrated that businesses had "satisficing" objectives rather than profit maximizing objectives. The implication is that firms will for a variety of reasons not pursue profit maximizing strategies; rather they seek to satisfy a host of objectives including worker satisfaction, community support, and executive indulgences in addition to profit or shareholder value. Indeed business school texts routinely describe the goals of businesses as needing to respond to these and other constituencies.

Fifty years has been long enough to persuade the economics profession that empirical studies can generate predictive patterns of human behavior that are consistent with findings of other social sciences. Indeed, Kahneman, a psychologist, received a joint Economics Nobel prize for his work with Tversky, an economist.

Stucke describes three contributions of the behavioral economists that challenge the Chicago model of economic man. First, they have demonstrated that the preferences of individuals are not fixed. Preferences are dependent on circumstance and the way an issue is framed for individuals. For example, people are more fearful of the risk of loss, than the prospect of gain even if the odds of benefit are the same. Second, people will surrender choice as a method of protecting themselves from bad habits. Example: payroll deductions for retirement are effective because people are less likely to spend money they never see. Third, people have a sense of fairness that promotes charitable acts and rejection of profitable deals that seem unfair. Experiments indicate these are common predispositions rather than universal or immutable reactions. Experience with an untrustworthy person is likely to engender distrust of that person.

Understanding these predispositions is made easier by considering the results of experiments. For example, in the "ultimate game" two people who do not know each other have an opportunity to obtain a share of \$100. The rules are explained to the two individuals. Person 1 will be allowed to decide on how the \$100 will be divided between Person 1 and Person 2. Person 2 will then accept the proposed division or reject it. If accepted, the amounts are distributed according to the proposal; but if rejected, neither Person 1 nor Person 2 gets anything. Profit maximization axioms would predict that

Person 2 would accept a division that awards him as little as \$1 (or 1 cent) and the remainder is given to Person 1 because Person 2 is better off getting something than nothing. Experiments have repeatedly shown that Person 2 generally rejects such offers if made and Person 1 generally does not propose such lopsided divisions. The bulk of proposed divisions range between Person 1 keeping \$75 to \$50 with an acceptance rate by Person 2 somewhat higher as the division is more close to equal.

Is this finding significant for antitrust analysis? I believe that it responds to the puzzle that Arrow raised about trust. Markets work because people start with assumptions of fair behavior and trust and these predispositions may prevail over “rational profit maximizing” responses. Stucke provides a more subtle and substantive antitrust implication. The notion of honor among thieves is consistent with durable illegal secret price fixing cartel, but profit maximization axioms would predict such cartels would fall apart because cartel members would cheat to increase their profits. To be sure, a price fixing cartel may involve some cheating, nevertheless behavioral theories provide persuasive explanations for the durability of a cartel. The first behavioral principle described above indicates that preferences or predispositions are not stable, but are a function of circumstance, that is, how an issue is framed. If one group, a mafia, a cartel, or a nation at war defines its members as “us” and the rest of the world as “them,” then cheating “them” or killing them may not violate the honesty or fairness preference of the members. In contrast, cheating a member would violate the trust predisposition. Stucke illustrates the analysis with a quote from the tape recordings of the ADM lysine cartel. One cartel member told his largest competitor at a cartel meeting: “Our competitors are our friends. Our customers are the enemy.”

Another experiment illustrates more directly the impact of framing an issue. Day care centers typically have problems getting parents to pick up their children on time. The day care personnel explain that they too have family responsibilities and having to wait for late parents puts a real burden on the care givers. In the study, one of two day care centers instituted a fine system for parents who were late picking up their children. To their surprise that day care center found more parents came late than before. Apparently some parents reframed the lateness as an economic rather than a moral issue and decided that it made more sense to work a little longer and pay a little more. The framing issue seems to have persistent effects. When the second day care center abandoned the fine system, the number of parents who picked up their children late decreased, but it did not return to the lower level that existed at that center before the fines were instituted or the level of the day care center that never institute fines.

If those examples are more representative of human economic behavior than price maximization, the implications for antitrust could be profound. For example, I remember debates during the 1970s about treating doctors as business actors and not as professionals with ethical obligations. I questioned then whether the emphasis on a business orientation might have negative effects on health care. Certainly there have been benefits. Doctors have been forced by antitrust actions to allow less costly nurse midwives and nurse anesthetists to practice. But is it possible that there have also been less desirable changes, such as doctors practicing without agreements with other doctors

to cover their patients when they are not available or whether doctors provide as much free care to persons who have no money. If it is possible then, like the imposition of day care fines, we should expect that pushing doctors to reframe their medical practice more like a business may also engender unexpected consequences that we do not like.

Stucke uses the Merger Guidelines as a way of testing the validity of the price maximization theories in predicting behavioral outcomes. The Guidelines present an explicit set of price maximization axioms and the history of Justice Department cartel cases constitutes a natural test of the predictive value of those axioms about human behavior. Stucke finds that the cartel evidence belies the assumptions on which the Guidelines are based. The Guidelines define market power as the ability to maintain a significant price increase. The evidence of many antitrust cases shows that those who obtained market power did not increase prices; rather they eliminated discounting, reduced advertising, stopped research programs, etc. Accordingly, customers who state they do not fear price rises may underestimate their peril from a merger. The assumption that anticompetitive effects are likely only in highly concentrated markets is refuted by numerous Justice cartel cases: “where a trade association facilitated collusion, 33.6 firms was the mean of firms involved . . . ; in cases involving price fixing cartels [without a trade association] 8.3 firms was the mean . . . .” This contrasts with merger challenges that overwhelmingly involve markets with four or fewer firms. The ability of large powerful buyers to detect and defeat cartels was not evident in the ADM lysine cartel whose customers included Tysons Food and Con Agra or in the graphite electrode cartel that raised the price for every steel maker in the world. The same contrast exists between the assumption that anticompetitive effects are likely only in markets with high entry barriers and the large number of cartel cases against industries with low barriers.

The notion that high entry barriers are a prerequisite for successful collusion is laughable after examining Stucke’s list of successful cartel convictions against sellers of turtles, low price carpets, chain link fences, plumbing supplies, steel pails and many more. Moreover, Stucke quotes Assistant Attorney General Klein and Deputy Assistant Attorney General Kolasky as stating that a review of Justice Department cases shows that cartels are much more durable than they would have expected or that the Chicago School assumes. Several were in existence for over ten years, one for 17 years. In addition, the cases reveal that a number of cartels regenerate even after being prosecuted.

Stucke’s case argument against the Chicago School, using the Justice Department’s own cases as evidence and Behavioral Economics as an explanation, is a tour de force. It is a demolition of the structure of price theory that is derived from assumptions that persons, businesses, and market outcomes can be predicted from the self-interest profit maximizing model. It does not mean that people, in general, prefer to pay more rather than less for goods. It does mean that, for antitrust, price should be only part of the story.

For a work that is so brilliant in conception and execution, I find underwhelming Stucke’s recommendations for creating an Antitrust for the Twenty-First Century. He has used an analysis of the merger guidelines as a test of the deductive value of Chicago model. Having found it wanting, we should presumably apply the cartel evidence across

the board in antitrust. It is obviously relevant to analyzing the types of industries that are likely to form cartels, but it is equally relevant to predicting what actions are likely to be effective exclusionary behavior by individual firms and by concerted action, and of course it should be a basis for rethinking merger enforcement standards.

Stucke, in contrast, recommends more study of mergers; rather than a reformulation of antitrust based on what he has demonstrated. I have done a little empirical research in the merger area and I think he overestimates the usefulness of the data he believes should be gathered. He also underestimates the burdens on companies and the agency staffs to generate and make sense of the data. As a consequence, I think it would not be a politically viable approach.

Stucke would gather data about a universe of mergers, after a merger has been blocked or permitted that includes all firms that received a Second Request. I have written in an earlier Commentary why I believe this is a skewed cohort to study, but Stucke's article provides stronger reasons for doubting this is a sound design. First, the cases in which Second Requests are issued generally involve markets with no more than four competitors and more often three or two. Based on the cartel cases, the burden of his article is that collusion is likely with many more firms but his suggested research plan would not look at any transactions where mergers have been allowed without a Second Request. To quote Stucke:

As Posner concedes, "it is impossible to specify a threshold [HHI] figure above which collusion becomes an attractive proposition," or below which collusion is unlikely. . . . [E]ach industry may have its own critical threshold HHI whereby collusion is significantly likely. These critical thresholds are only discoverable inductively through systematic testing.

So it appears to me that Second Request firms will provide no data about the low barrier, multifirm industries that he has shown are prone to collusion. It would be better to go back to the mine of cartel data and look at concentration levels, entry barriers, etc in the collusion cases. Moreover, even if the Second Request cohort were a sensible group to start with on the grounds that the agencies know something about the industries, any firm conclusions about those industries would require post-merger (or nonmerger) pricing information, research expenditures, promotional expenditures, innovation from the parties who filed for the mergers and all other participants in the industry including new entrants and firms that have left the industry. Without comprehensive information, it would be difficult to discern the competitive impact of the merger. Rather than examine only firms that have filed Second Requests, it might be more sensible to create a cohort of firms that have filed premerger notification forms in industries with eight firms, based on the cartel cases that indicate industries with 8 firms (or 33 where a trade association existed) will collude.

Obtaining this data from firms, especially firms that have never been investigated, much less been found to have proposed an unlawful merger, is likely to be expensive for the companies and resisted by them. Experience with the Line of Business program in the

1970s was that Congress became unwilling to place the burden on companies to file with the FTC cost and profit data on each of their products for the purpose of research. Finally assuming relevant industry HHI thresholds are different, the process of determining those thresholds is likely to be resource intensive and very slow.

If I were to design this kind of research, I would start with a few industries and use the FTC's authority under Section 6(b) of the FTC Act to survey a few industries to see if the research were feasible and fruitful. But, based on this article, I am extremely dubious that the HHI approach to mergers deserves to survive.

The article contains an eloquent footnote quoting former Assistant Attorney General Charles James praising the Merger Guidelines. The footnote says in part:

[I]t is difficult to fathom the world of merger policy before them. Did we really define markets almost entirely on circumstantial indications . . . . Did we actually make enforcement decisions based upon little more than four- and eight-firm concentration ratios without regard to the actual shares held by the firms? . . . Was there really a time in which merger-related efficiencies were viewed with great skepticism . . . ?

The answer is yes, for those who did not attend merger screening meetings in the 1970s. I would suggest the analysis by the late 1970s was more sophisticated than the worst cases brought in the 1960s, but yes circumstances of an industry were considered and now it appears from the collusion cases that an 8 firm concentration ratio is not necessarily silly.

What then of predictability? The current Merger Guidelines are predictable because they assume mergers are good or at least neutral absent proof by the government of certain specified competitive risks. Those Guidelines were based on the assumption that mergers are good for the economy. Until the Chicago School models prevailed, that assumption did not enjoy universal agreement. Having spent too many years of my life working on this issue, I would say there is now a greater consensus among those who have conducted serious research that most mergers do not enhance efficiency. As a result, I think a predictable set of merger guidelines might be formulated that banned mergers except in specified circumstances that would have to be proven by the parties seeking to merge.

This is not the place to argue about how many or how broad such exceptions should be. It is rather a suggestion that Behavioral Economics and other evidence indicate that we need a new approach to antitrust, not a refinement. This is, I think, most true of antitrust predation law that, trapped by its Chicago assumptions, ignores the strategic literature that suggests that it will be a rare firm that will enter a market that has a known predator or predators.

For those who believe the world might end if antitrust were to be substantially altered, I would point to the fact that American annual productivity increases were highest from 1945 to 1973. After the 1973 oil shocks and the ascendancy of Chicago based antitrust

productivity increases came to almost a standstill until the 1990s. Am I suggesting that old antitrust was responsible for the good times and Chicago antitrust has been responsible for slowing productivity? No, I am suggesting that earlier antitrust enforcement policy was not inconsistent with strong growth in productivity and the American economy. Accordingly, we should not be afraid to borrow some aspects of earlier antitrust policies in designing new policies that take into account the new, new learning of Behavioral Economics.