

THE ATTACK ON ANTITRUST POLICY AND CONSUMER WELFARE:

A RESPONSE TO CRANDALL AND WINSTON

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Introduction

In their paper “The Effect of Antitrust Policy on Consumer Welfare,”¹ Robert Crandall and Clifford Winston advance an argument that others have previously made—namely, that antitrust policy in the United States is, on balance, welfare reducing. While past arguments have typically been ideological in nature or at most anecdotal in their support, Crandall and Winston (hereafter, CW) purport to provide empirical evidence: “This paper synthesizes the available scholarly evidence regarding the effect of antitrust policy on consumer prices and in deterring anti-competitive behavior.” (CW, p. 3) They conclude that deadweight loss is small, the extent of anticompetitive activity is “quite limited,” and it is the market rather than antitrust which is responsible for “spurring competition and curbing anti-competitive abuses.”² (CW, p. 29)

Providing empirical evidence on the question of the effects of antitrust policy is a commendable objective. Along with many other observers, this author has long noted the absence of systematic evidence or even many good case studies that establish the effects. This is in sharp contrast, for example, to regulation and deregulation, whose effects are often and well studied in economics. In the case of regulation, however, the effects are usually both broad and deep, so that empirical work can discern the incremental effect of policy. Much antitrust, in contrast, works at the margins of conduct, often presents difficult counterfactuals, and in any case has effects easily swamped by other forces

¹ “The Effect of Antitrust Policy on Consumer Welfare: Assembling the Empirical Evidence,” Robert Crandall and Clifford Winston, paper presented at the Justice Department Antitrust Division seminar, June 2002, *Journal of Economic Perspectives*, forthcoming.

²In an earlier draft of this paper, based on the same evidence, the authors went considerably farther, endorsing consideration of outright repeal of the antitrust laws.

impinging upon the firm or industry. For these reasons, modeling and data adequate to the task of assessing antitrust policy have been hard to develop.

Crandall and Winston have not developed new modeling techniques or data. Instead, what they offer as “empirical evidence” is a review of selectively chosen examples of the application of antitrust, which they criticize for reasons both relevant and irrelevant to their point, and are ultimately inconclusive anyway, together with statistical work that appears to show the ineffectiveness of antitrust but in reality is based on a flawed model and data incapable of discerning any effect. This comment is intended to highlight these aspects of the CW attack on antitrust policy and consumer welfare.

Four Broad Problems

There are four broad problems with the CW critique—the question posed, the nature of their evidence, their interpretation of their evidence, and what they offer as new empirical work. Here we discuss these four issues, followed in the next section by more specific aspects of their argument.

Q First, CW state the key question of the paper as follows: “Should the United States pursue a vigorous antitrust policy?” (CW, p. 1) Yet without explanation or caveat, they use the entire 100-plus year history of antitrust to form a judgment about the merits of antitrust policy *today* and in the *future* (which is, presumably, what “should” means). That is obviously unjustifiable and misleading. The Standard Oil case of 1911, merger policy in the 1950s, and so forth simply do not tell us anything about legal and enforcement standards and practices *today*. In fact, with the exception of AT&T, all the examples of monopoly cases discussed in CW are at least a half century old. Elsewhere CW concede that antitrust has changed over time (they hesitate before using the term “improved”), but they

nonetheless attempt to skewer antitrust for real and perceived sins a century old.

Q Second, the CW attack on antitrust cases brought against monopolies lumps together all possible objections to these cases, regardless of their relevance and certainly without regard to important distinctions. No distinction is drawn, for example, between errors by the enforcement agency and errors of the judiciary—although they imply quite different things about policy. Nor do CW distinguish between errors in the formal allegations and errors in the remedy sought or implemented. They simply seize on whatever they deem to best undermine the argument for antitrust. Nor is there a distinction between poorly crafted remedies and those that simply worked out badly but unforeseeably. Lumping all these together may make for entertaining reading, but it does not contribute to a discerning analysis of antitrust.

Q Third, the discussion relies upon a startlingly selective use of the evidence. They evaluate several monopoly cases, more collusion cases, and mergers generally while offering scarcely any references to studies showing benefits of antitrust intervention or costs of failing to act. There are brief mentions of Comanor and Scherer on early monopoly cases, Barton and Sherman on the photo film merger, Kwoka on collusion in real estate auctions, but these get short shrift in the discussion, and the many other case studies or reviews of cases showing antitrust in a more favorable light get no shrift at all.

In addition, all government studies are dismissed, though no reason is given. If the issue were a concern with bias, then CW should also have put aside those “studies” of antitrust conducted and paid

for by interested parties, whether done by academics or not.³ Alternatively, CW could—and probably should—have included in what purports to be a comprehensive review studies deriving from any source, with appropriate evaluation of their credibility. That would ensure due attention to the FTC report evaluating merger policy (which included case studies and other evidence) and agency documents which since 1999 have had to provide estimates of the benefits of their actions, including consumer savings from merger and other enforcement actions. Instead, CW appear to have made a prior judgment that all academic-authored studies are credible, regardless of sponsorship, while all government studies are not.

Q Fourth, CW's most novel undertaking is their effort to capture empirically the effects of mergers. They create a data set of 2-digit SIC industries for which they relate price-cost margins to the (lagged) level of antitrust enforcement during the period 1984-1996. Enforcement is measured by the number of court-based outcomes—successful and unsuccessful challenges plus consent decrees. It is ironic to see such reliance placed on this modeling approach from those in a camp that has been vitriolic in its critique of such studies when they disfavor their views. In the present case, one need not debate the details of the large body of studies examining industry structure and conduct, or the adequacy of the CW specification,⁴ for one simple and overriding reason: Two-digit SIC industries are utterly

³ For example, CW cite approvingly work done on behalf of IBM in its defense against the Justice Department suit. There is simply no objective reason why that should be given more weight than corresponding work from the agencies.

⁴ Their specification is defective in numerous ways. It omits concentration itself, for reasons that are not explained. Apart from their variables of interest, in fact, their specification has only three explanatory variables—far fewer than state of the art (whatever one's views of the art), raising the likelihood of omitted variable bias. CW state that a number of other possible explanatory variables

meaningless, as any fair-minded person of any persuasion would agree. Nothing can be learned by looking at margins and antitrust at that level, and the exercise is not saved in any way by claiming that this is the best that can be done or that the parameter estimates should not be compromised. Those are not even close to being correctly defined product or geographic markets. The data are worthless. The results mean nothing.⁵

Many Other Problems

The CW study also suffers from numerous specific problems of fact, analysis, and interpretation. In their order of appearance, these are as follows:

“ The opening statement that economists’ views of antitrust are the result of theory, not empirical work, is incorrect. Bain’s empirical work was an important foundation of the first Merger Guidelines. Empirical work on critical concentration formed the basis for the cutoffs in the 1982 and later Guidelines.

proved to be insignificant (their fn. 30), although this does not help to understand the effect of their inclusion on the policy variables they focus on. Good econometric practice would retain variables required by theory, even if they lack the usual statistical significance, precisely because they may affect other included variables. Moreover, it is unclear whether or how CW address time series and nonstationarity issues, or the varied standards for merger enforcement and court oversight over this 15-year period.

⁵Though CW portray them otherwise, their results actually are largely consistent with the proposition that overall antitrust is welfare enhancing. Successfully blocked mergers would be expected to leave margins unchanged since merger enforcement seeks to prevent margins from rising. CW report a statistically insignificant coefficient for the effect of successfully-blocked mergers. Where the courts have rejected DOJ/FTC challenges, if the courts have been correct in screening out challenges that were incorrectly brought, one would find post-merger margins falling. This is precisely what CW report for the coefficient on “unsuccessfully-challenged” mergers, i.e., those rejected by the court.

“ The recitation of the history of industrial organization economics and antitrust is seriously incomplete in its lack of any reference whatsoever to the “new empirical industrial organization” and also “post-Chicago economics.” These developments over the past decade offer an alternative theoretical and empirical approach that responds to the Chicago critique, but mysteriously they get no mention in this paper that purports to synthesize available evidence.

“ What purports to be a review of monopolization violations is in fact a selective reading of evidence on exactly six cases. Apart from the sheer paucity of cases, all the chosen cases involve government pursuit of structural remedies, thus passing over the far more numerous examples where conduct remedies are sought. Moreover, the cases they discuss date back nearly a century, and only one occurred in the last 50 years. This “evidence,” whatever its other strengths or limitations, has no bearing whatsoever on the question of whether a vigorous current and future antitrust policy is beneficial. As such, there is really no need to take the CW discussion of “monopolization” seriously.

That said, there is something to be learned from the CW discussion. What can be learned is not so much about antitrust, but rather about the methodology and perspective of this study:

- A major part of the CW criticism of the Standard Oil case appears to be that it was brought too late to do much good. That logically implies the need for more timely antitrust—hardly a novel, or even controversial, view, but certainly not one that casts doubt on antitrust itself.

- The American Tobacco case is criticized on the grounds that the breakup remedy was inadequate—three firms were too few to spur meaningful competition, especially with respect to price. This implies the need for tougher remedies in such cases, not that antitrust is of no value.

•The key criticism of the Alcoa case appears to be that events other than the decree were largely responsible for the erosion of the company's market position. But since the remedy was postponed due to World War II and then altered to take advantage of government-built wartime plants, antitrust can scarcely be faulted for delays and opportunities resulting from the war.

•The Paramount and related decrees against movie distributors are said to have been ineffective. But all the systematic evidence of the effects of this decree is washed away in a tidal wave of other changes in the business. Statements like "the average real price of a movie ticket rose in the two decades following" cannot be taken seriously as evidence. More useful, but hardly noteworthy, is their recommendation for "modifications in [a] decree...that is not working." (CW, p. 13)

•CW have a harder time dismissing the United Shoe case, since its conduct was widely believed to be exclusionary. They concede that the decree established a secondhand market for machines and reduced United's market share, but they quickly move on to the (largely irrelevant) facts that its revenue simply grew, that its rate of return was "relatively" constant, and that the ratio of machinery expenses to shoe value did not change. This is simply not empirical evidence.

•CW offer grudging admission that the AT&T decree spurred an increase in long-distance competition and produced lower phone rates. Given the awkwardness of this conclusion for their overall position, they devote precisely two sentences to this observation and then quickly move on to other issues regardless of their relevance to the efficacy of antitrust. They discuss the FCC's role in preventing entry by MCI (which is what the antitrust case sought to circumvent), and then simply assert that the market would have done the whole thing better.

In summary CW profess surprise that “policymakers...continue to claim that certain firms are monopolists and should be prosecuted” (p. 17).⁶ Their review actually does not dispute the proposition that certain firms are monopolists, but rather it strives to show that monopolization decrees are ineffective. Even here they come up far short. What they actually show is that six specific monopolization cases, all structural, and dating back as much as 100 years, had quite mixed effects, and for a wide variety of reasons--tardy cases, poorly crafted decrees, unforeseeable events that intruded, etc. A more worthwhile analysis would have examine the root causes of any inadequacy or ineffectiveness, and determined whether blame, if any, was appropriately assigned to the antitrust agencies, the courts, the companies, or the state of nature.

“ The next section of CW addresses collusion proceedings in antitrust. Here they rely uncritically on a single review of cases plus two well-chosen examples as the basis for their conclusion that “DOJ may be primarily prosecuting firms that are engaging in practices that do not raise price, but instead reduce costs or enhance distribution.” (p. 19). Their two lengthy examples--ASCAP/BMI and college financial aid--are both taken from a narrow antitrust exception for “essential restraints,” those agreements required to create a particular product that does not displace others.

These are *not* typical of collusion cases, as anyone familiar with Section 1 cases knows. But CW create the impression that these are the norm for conspiracies and imply that they are somehow unrecognized by the courts. Neither proposition is correct. Moreover, there is another well-known

⁶ In connection with this point, they inexplicably seize on a recent example of a predation case, despite the fact that predation allegations and conduct remedies were not the issues in the monopolization cases they just reviewed and from which they seek to draw inferences.

exceptional case they do not cite. That case—NCAA—was found *against* the defendants anyway.

Thus, CW have selectively focused on exceptional cases, and then selected out one that inconveniently proved something quite different,

Faced with news reports of numerous cartels involving lysine, vitamins, and other products, CW concede in one sentence that such cartels have raised prices, but they quickly dismiss the notion that antitrust has had anything to do with the disruption or demise of the cartels. Here they overlook lots of good evidence about these and other cartels. Indeed, their review of collusion generally fails to note most of the literature examining the effects of cartels on price.

“ Nowhere is the effect of CW’s determination to dismiss antitrust more apparent than in their discussion of merger policy. I have already discussed their attempt to give credence to empirical work that, if others had offered it in support of the proposition that concentration elevates margins, would have encountered withering criticism. Beyond that, they selectively cite one of their own studies in support of the proposition that certain airline mergers lowered fares, failing even to disclose to readers the larger literature analyzing those same mergers that comes to a different conclusion.⁷ The present version of this paper does cite one study of a merger that raised price, but the reader is left with the incorrect impression that that is the exception, whereas “economist’s assessments of mergers generally conclude that they are not anticompetitive.” (p. 24). They also dismiss a study of a challenged railroad merger that showed the benefits of antitrust action first by raising doubts about one firm’s viability

⁷ Among others, see G. Werden, A. Joskow, and R. Johnson, “The Effects of Mergers on Price and Output: Two Case Studies from the Airline Industry,” *Managerial and Decision Economics*, 1991.

(inconveniently for their story line, it survived until another merger partner came along), and then, in a complete *non sequitur*, by noting that other railroad mergers occurred later anyway.

The proposition that most mergers are not anticompetitive is, of course, neither novel nor controversial. Indeed, this author has stressed to CW the fact that the total of DOJ and FTC challenges to mergers represent a minuscule fraction of all mergers proposed in any given year.⁸ The antitrust agencies are highly selective—and budget-constrained—in their case-bringing, despite the impression of those agencies that some critics labor to create.

“ In response to comments on an earlier draft, CW now directly address the issue of deterrence. After all, if deterrence is effective, it is quite possible that the benefits from vigorous antitrust is not fully captured by looking only at the firms or mergers in question, since many other anticompetitive outcomes may have been prevented. Most of their discussion under this heading, however, has little or nothing to do with how the existence of antitrust, or its vigorous enforcement vs. lax enforcement, affects the behavior of non-parties to DOJ and FTC actions. Instead, CW recount how market power may erode itself (though some might think that the fact that it took most of a century for that to happen to USSteel might leave room for antitrust), how a large fraction of mergers fail by themselves (hardly a telling point against policy that challenge so few mergers), and how “markets

⁸ The agencies together investigate a modest fraction of “large” mergers in any year, and no smaller mergers, and challenge only a small fraction of those that are reported to them. For example, in the year 2000 there were nearly 5000 reported (that is, large) mergers, somewhat more than 400 investigations, and no more than 50 cases were filed. For data on case-bringing by the DOJ and FTC, see the website of the American Antitrust Institute. That site also has budget data for DOJ and the FTC back to 1970 and 1915, respectively, collected from public sources. CW profess to be unable to find such data for more than the last twenty years (p. 4).

prevent firms from successfully colluding.”

In support of the latter CW offer two lines of reasoning. First, they note that some efforts to cooperate fail—hardly in dispute, but logically irrelevant to the proposition that some succeed. Second, they note that two particular high-margin industries, chosen without explanation, are not known to be collusive—an assertion whose relevance is baffling at best. Their final flourish is to raise doubts about a study that finds deterrence to be important (CW’s rebuttal is that some firms must not have been deterred, since cases are still brought—displaying the same lack of logical relevance), and then to endorse one person’s unsubstantiated musing that deterrence may be excessive, in which case prices may be higher than otherwise. Based on this, CW “question whether [antitrust laws] significantly improve on the market’s ability to deter anticompetitive behavior.” (CW, p. 28) As mentioned, one learns not so much about antitrust as about CW’s perspective, methodology, and objectivity from the reasoning that got them to this conclusion.

Their Conclusion—and Mine

CW close their paper with a section entitled “Where Do We Stand?” Their stance is predictable, but is not at all convincing. They begin by citing Harberger’s estimate that deadweight loss in the US economy is 0.1 percent of GNP.⁹ They do cite other, higher estimates, but their point is that the extent of anticompetitive activity is overall “quite limited.” That is not really in dispute, but in some sense it is also irrelevant. Deadweight loss in the US economy may well be small on average, but it is not the average that matters for policy purposes, but rather its distribution. Losses are greater in the

⁹ More accurately, it was 0.1 percent of manufacturing revenues, based on 1930s data, and using a methodology universally understood to underestimate actual deadweight loss.

very small minority of industries where competition does not reign, which of course are precisely the industries that deserve antitrust attention. Studies reporting deadweight loss by industry always show it to be concentrated in a small number of familiar industries.¹⁰ In addition, there is more to competitive harm than just deadweight loss, as every industrial organization textbook makes clear.

CW make a couple other points in their conclusion. They offer their view that “it is likely that firms try to exploit the antitrust process,” a statement that (whatever one may think of it) is altogether unsupported by any data, studies, or references in their paper. Similar assertions appear throughout this article, which purports to be synthesizing evidence but in fact is a vehicle for displaying pre-conceived ideas. Furthermore, they assert that changes that have occurred in antitrust “have not been guided by empirical assessments,” a proposition that bespeaks unfamiliarity with the intellectual history and evolution of antitrust policy.

CW claim that their study synthesizes available empirical work on the efficacy of antitrust policy, but it does nothing of the sort. Rather, they offer a series of carefully selected references, some failing-grade empirical work, and bald assertions nowhere supported in their study. They ultimately fail to cast any useful light on the question that they set forth at the outset.

¹⁰See for example Cowling and Mueller, “The Social Costs of Monopoly Power,” *Economic Journal*, 1978.. CW acknowledge Cowling and Mueller’s larger estimate, but incorrectly state that it is due to their inclusion of advertising expenditures. Even without advertising, Cowling and Mueller find deadweight loss to be from 10 to 40 times as great as Harberger.