Readings

The Case for Informal Conflict Resolution

The Litigation Explosion: What Happened When America Unleashed the Lawsuit
by Walter K. Olson
(Truman Talley Books, 1991), 388 pp.

Reviewed by Robert D. Tollison

Olson offers a readable analysis of the explosion of litigation and lawyering in the United States over roughly the past thirty years. He also offers some proposals for reform. His analysis stresses the following types of factors—attorney advertising, contingency fees, the expansion of the geographic reach of litigation, the relative ease with which litigation can be conjured and brought (we can sue anybody anytime anywhere), the invasive and costly use of discovery, the lack of credible standards for determining damages, the arbitrariness of legislation (we can be sued for A, and not A), and the demise of literalness in contracts. Undoubtedly, I have missed some points, but this list is instructive of what Olson emphasizes. The results of these forces are sleazy, overreaching, and expensive practices. Olson's solutions are the inverses of the causes, including notably more restraints on the practice of law and other calls for what amounts to the legal procedures of an earlier era, in which entry into litigation was substantially harder. Olson's most significant proposal is his call for "strict liability for lawyering," in which "a wrongful litigator" would pay for the harm caused to an opponent. Olson's book is thoughtful and well written, and his proposals deserve serious consideration.

The Problem. Virtually all of the incentives facing actors in the legal system, most particularly, lawyers, existed well before the current litigation explosion. Why do we only now face these problems and issues? Why was the litigation industry "deregulated"?

Olson's Answers. His general answer is that all of this is the result of a self-seeking legal industry, which needs to police itself better. But this answer founders upon the problem—why have these incentives won out now? Olson might also argue that piecemeal changes in legal practices, often well intended, resulted in unintended consequences. This is fine, and I would not fault Olson on this score. It is worth arguing that lawyers are not like eyeglasses and cannot be "deregulated" in the same way. Nonetheless, there is perhaps a level of subtlety here that such an answer misses.

Another Answer. Something in the economy changed so that it became efficient to shift conflict resolution mechanisms from a mostly informal to a formal basis. This approach suggests that the breakdown of the family, the decline in church attendance, and related socioeconomic developments substantially reduced the scope and activities of more informal conflict resolution procedures in the economy. In a word, the quotient of trust in the economy has fallen. The institutional arrangements that Olson decries are the next-best set of institutions to handle the task of conflict resolution. The institutions are more costly by definition, but at the same time are least costly in that they are the best alternatives now available for such work. These institutions may be ostensibly gross. They are impartial, often unfair, without substance, overly costly, distorting of incentives, and so on. But they are still, arguably, the best institutions to do the job in the face of the demise of more informal arrangements. Whereas the clergy would have informally advised couples in the case of a family conflict in the past, divorce courts now hear cases, divide property, and award custody of children. Indeed, litigation may be the vanguard of *more* efficient arrangements in such cases. Who is to say

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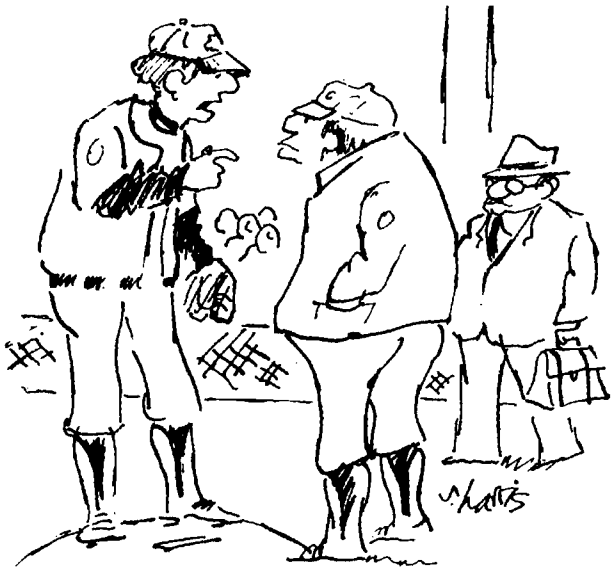

"Talk to my lawyer."

that marital markets are not more efficient when divorce is, on average, easier?

Olson's Solutions. They are all admirable. The call for "strict liability of lawyering" is healthy and worth pursuing. The courage and fortitude of Vice President Quayle at the recent meeting of the American Bar Association in Atlanta was commendable. And no doubt there will be some tort reform to come out of all such efforts. I wholeheartedly endorse and suggest discussion of such issues.

The Problem. The point is that we cannot swim in the same river twice. Olson yearns for past standards and practices that delimited legal activities at all levels. Those structures were appropriate to their times. We have now substituted litigation for other procedures for resolving conflicts. That is not a free lunch, for sure. But does one really want to live in a complex world of contracts, torts, responsibilities, and interdependencies, in which it is harder to reach a courtroom with a lawyer? Until the legislature stops passing stupid laws, I, for one, would resist such a change. Another way of saying all this is to suggest that it is probably wiser to work to restore the institutions that mitigated social conflict in the first place than simply to suggest reforms in the legal profession. That is admittedly a harder problem, but it is no more difficult than convincing people to work and save more to remedy other ailments afflicting the economy. Nor is that meant to suggest that we cannot do some of both. But to examine only the legal profession as the source of such problems is to shoot the messenger for bringing the bad news.

The Root Problems. Return to the idea of stupid laws for a moment. The litigation explosion has been driven by the passage of laws and regulations and by the appointment of activist judges. Olson is correct to suggest that the ABA generally has supported such changes. But lawyers are rarely, if ever, going to be the marginal or swing interest group of voters for the passage of such laws. Those laws pass because they represent efficient wealth transfers from the point of view of the legislature, efficient in the sense of contributing to the reelection of the legislature. If there is a problem to be solved here in the spirit of Olson's analysis, I would respectfully suggest that the problem resides most particularly in the structure of legislative institutions and incentives. Better to have low-priced lawyers and less stupid laws than vice versa.

Judges are yet another facet of the same root problem. They are appointed by the legislature to do the legislature's bidding. The selection of judges thus also reflects the texture of interest-group politics. Given the possibility of mistakes and the life-tenure arrangements of the federal judiciary, matters here are harder to turn around. But can the problem be any better encapsulated than in Harris Wofford's campaign advertising in the Pennsylvania Senate race, in which he said (I paraphrase): if the Supreme Court guarantees every criminal a lawyer, why cannot the government guarantee every citizen a doctor? Thomas Robert Malthus and Adam Smith were right: the means of subsistence grow at an arithmetic rate; the means of our undoing grow at a geometric rate. There is much ruin in our country.

Assessing Policy Analysis

Industrial Organization, Economics, and the Law
by Franklin Fisher
(MIT Press, 1991), 490 pp.

Reviewed by Stanley M. Besen

This set of collected papers focuses primarily on the industrial organization facet of Franklin Fisher's

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work. It thus provides a useful compendium of the somewhat distinctive, and vigorously defended—Fisher takes no prisoners—approach that the author has employed in analyzing antitrust and regulatory issues in both his academic writings and his extensive activities as a private consultant. It is fairly clear that much of Fisher's characteristic style in attacking these issues is an outgrowth of his substantial role as a consultant, especially that of the chief economic consultant to the defendant in *U.S. v. IBM*, but it also derives from work done for Ford Motors, Matsushita, and Northwest Airlines, all of which are identified in this book.

The hallmark of Fisher's approach is his insistence that, given the state of economic theory and the inevitable limitations of conventional measures of monopoly power, only a detailed analysis of actual market conditions can possibly be used to measure the extent to which market power can be exercised by one or a number of firms. In his words, "the detection of monopoly power requires detailed investigation of the facts of particular industries."

Fisher argues strongly that these detailed investigations should concentrate on "such issues as barriers to entry and the ability of competitors to expand." He contends that "the analysis of entry conditions is the analysis of a central phenomenon which places or does not place constraints on the behavior of an alleged monopolist" but that "the analysis of entry is . . . the single most misunderstood topic in the analysis of competition and monopoly."

Finally, throughout the book, there are examples of Fisher's aversion to the use of simple quantitative measures when examining questions of industrial organization. In Fisher's words, "[t]here is a great temptation for the antitrust authorities (and perhaps the courts) to focus on quantitative standards as a substitute for real analysis." Fisher would substitute detailed analyses of the functioning of particular markets for summary measures to determine the extent to which incumbent firms, or potential merger partners, are, or would be, constrained in raising prices, that is, whether there is market power. Even where summary statistics are given a role by Fisher, as in his discussion of the use of the Herfindahl-Hirschman index in judging the likely effects of proposed mergers, he argues for limiting that role to identifying combinations that justify further investigation, rather than providing a rigid benchmark to be used in deciding which mergers should be opposed. Indeed, one of the truly remarkable aspects of Fisher's approach is that here we have one of the profession's leading econometricians arguing *against* using quantitative analyses in favor of more qualitative ones to reach policy judgments. Indeed, there is not a single quantitative analysis among the papers in the section of the book on antitrust policy.

The papers in this volume provide examples of Fisher's debunking the use of simple market power measures ("On the Misuse of Accounting Rates of Return to Infer Monopoly Profits" (with John McGowan) and "On the Misuse of the Profits-Sales Ratio to Infer Monopoly Power"), of his using the detailed industry analysis that he advocates ("Pan American to United: *The Pacific Division Transfer Case*"), of his criticizing the detailed analyses of others ("Pan American to United" and "Horizontal Mergers: Triage and Treatment"), and of his disapproval of some of modern theory as applied to industrial organization policy ("Games Economists Play").

Fisher's role in the IBM case, as well as his harsh criticisms of widely used measures of monopoly power, may have led some to conclude that he believes that monopoly does not exist or, at least, that its existence cannot be shown and, consequently, that the role of antitrust should be severely limited. But there are numerous instances in the papers in this volume where Fisher takes positions that would be comforting to those who would urge a larger role for antitrust enforcement. For example, deviating from the Department of Justice's *Merger Guidelines*, Fisher would use the more stringent competitive price rather than the current price as a benchmark in determining whether a proposed merger would raise prices, for he argues that it is important to retain the possibility that collusion will break down. He would treat sunk costs and economies of scale as entry barriers. He would take the fact that entry takes time into account in deciding whether to permit a merger, although he expresses some skepticism about whether the time period specified in the DOJ *Merger Guidelines* is correct. And he believes that the "raising rivals' costs" strategy is plausible.

It is especially notable that Fisher would place a heavy, but not unreasonable, burden on merger partners to demonstrate the efficiencies that they claim justify their union: "The burden of proof as to cost savings or other offsetting efficiencies . . . should rest squarely on the proponents of a merger, and here I would require a very high standard. Such claims are easily made and . . . often too easily believed." In short, the Fisher we see here is a lot closer to the mainstream than one might expect

from casual observation, although there is still a distinctive Fisher position.

Where the first part of this volume focuses on antitrust issues, the second is concerned with the effects of regulation of the television industry. These papers, which grow out of Fisher's long association with both CBS and the National Association of Broadcasters, contain quantitative analyses of the effect of cable television on local broadcasters and a more qualitative analysis of FCC regulation of the broadcast networks. (A third section on quantitative methods and the law is not discussed in detail here but contains "Multiple Regression in Legal Proceedings"—an especially useful guide to noneconomists on the use of this technique.)

Among the papers in the section on the television industry is an important early article by Fisher (and others) ("Community Antenna Television Systems and Local Television Station Audience") that established the framework for the policy debate over regulation of cable television during the fifteen-year period between the time it was written and the deregulation of distant signal carriage by the FCC in 1990. Even without allowing for the fact that twenty-five years have passed since the study was first submitted to the FCC, this is still a remarkable piece of applied econometric analysis.

Using an innovative approach to surmount significant data limitations, Fisher and his colleagues were able to produce plausible estimates of the effect of cable television on the economic well-being of local broadcasters. Most significant was their ability to produce separate estimates of the effects on a broadcast station's audience of an increase in the *number* of stations with which it competes on cable, on the one hand, and of the *duplication* of its programming by these additional competitors, on the other—both of which turned out to be significant focal points in the subsequent policy debates.

Fisher and his colleagues then used the results on audience diversion, combined with estimates of the relationship between audience and revenue and with information about station costs, to argue that distant signal carriage by cable television would substantially reduce broadcast station profitability and, in the case of some smaller stations, might affect their very viability.

Despite my admiration for the quality of the analysis in this paper, I have some reservations about it nonetheless. These reservations are not based on the fact that some of Fisher's findings have not held up to subsequent research. (In particular, the effect of cable retransmission of broadcast signals on local station audiences seems to have been smaller than Fisher and his colleagues estimated, and UHF stations seem, on balance, to have been helped by cable.) After all, no forecasts are perfect. My concern is primarily the use to which Fisher's results were put.

Broadcasters argued that audience diversion caused by cable would, by reducing their profitability, threaten their ability to continue to offer unremunerative local and public affairs programming. But, of course, the impact could be sizeable only if large numbers of cable subscribers preferred to watch programs on distant signals and were willing to pay to do so. The net effect of the "harm" that Fisher and his colleagues measured might well be to increase consumer welfare. Moreover, even if cable systems attracted large audiences, it is unclear how much of an impact this would have on the amount of public service programming that broadcasters offer. Thus, the policy implications of Fisher's results are, at least, ambiguous and do not clearly support the need for stringent restrictions on cable.

Not surprisingly, given the nature of the game that was being played, cable interests produced results that showed a much smaller impact—that is, they showed that their proposed services were not much desired by viewers. The FCC eventually embraced the latter view when it deregulated distant

signal carriage in 1980. Of course, the effect of cable on broadcasting is now substantial, both because distant signals continue to attract viewers—the effect measured by Fisher—and because other cable program services have developed, although the impact has not been so great as the most dire of the broadcast industry predictions.

Although Fisher never explicitly argued that the effect of cable on economic welfare would be negative—indeed he takes pains to note that "[t]here is no necessary evil in such change"—his results were used by those who did. Thus, despite the high quality of Fisher's empirical analysis, I believe that this effort can be identified as good work in a bad cause, or, at least, it stands for the proposition that even good work can be misused.