Remembrance of Things Past: Antitrust, Ideology, and the Development of Industrial Economics*

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Abstract

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Why study the mistakes of the past?

> Reaction in a faculty meeting to the suggestion that the Ph.D. program include a required course on the history of economic thought.[1]

1. Introduction

What it is that economics can, does, or should bring to antitrust is a topic that turns up fairly often in the literature.[2] The question of what antitrust has brought to industrial economics has received less attention, and it is that question to which I devote myself in this essay.

Stigler (1982, p. 6) has noted the income redistribution consequences of antitrust for the economics profession. Such transfers accrue to individual members of the profession, and are not my topic. Closer to what I have in mind is the invaluable role of antitrust as a rich source of raw material for industrial economists (Coase, quoted in Kitch (1983, p. 193)):

> I think you can often learn more about how the economic system works by reading law books and cases in law books than you can be reading economics books because you do get descriptions of actual business practices which are difficult to explain.

But this raw material is not without its price. I will argue that the antitrust policy implications that can be drawn from scientific research by industrial economists have drawn ideological currents into academic debate as moths are drawn to a flame, and that this phenomenon contributes to explaining a puzzle that has continued for a period now going on thirty years. This puzzle is the ability of advocates who favor a minimal role of antitrust enforcement in the economy to portray their views, to the legal community, as generally accepted by economists, when this claim is now and has always been manifestly incorrect.

Concerning the current views of the profession, Bolton *et al.* (2000, p. 2242) write

> A powerful tension has arisen between the foundations of current legal policy and modern economic theory. The courts adhere to a static,

[1] I owe this heading to an anecdote of my late teacher and onetime colleague Walter Adams.

[2] See among others Bok (1960), Sullivan (1977), Baxter (1983), Turner *et al.* (1983) and Rowe *et al.* (1984). Let me note here that anonymous referees have suggested that I include here a discussion of the impacts of contrasting schools of thought in industrial economics on antitrust. That is an interesting topic, and one I take up in Martin (2006).

> non-strategic view of predatory pricing, believing this view to be an economic consensus. This consensus, however, is one most economists no longer accept.

U.S. courts continue to be heavily influenced by what Posner (2001, p. 194, fn. 2) terms "orthodox 'Chicago School'" views toward most strategic behavior, not merely predatory pricing. Bolton *et al.* are correct that most economists do not now accept the orthodox Chicago School analysis. Consideration of the literature suggests that orthodox Chicago School views were *never* accepted by mainstream economists.

Of course, scientific validity is not a matter of majority vote, and as John Bates Clark wrote long ago (1887, p. 45) "Conclusions reached by valid reasoning are always as true as the hypotheses from which they are deduced. If we admit the fact of unlimited competition, we concede in advance many doctrines which current opinion is now disposed to reject." Mainstream economists have never disputed that the policy recommendations of the orthodox Chicago School follow as a matter of logic from the assumption that observed prices and quantities can be treated as good approximations to long-run competitive equilibrium values (see the discussion, below, of Reder (1982)). What has never been accepted by most industrial economists is that it is appropriate to make this "good approximation" assumption.

In Section 2, I discuss the rise of industrial economics as a branch of microeconomics. In Section 3, I review the rise of the first Chicago School of industrial economics, which advocated affirmative government action to obtain and maintain good market performance. In Section 4, I turn to the rise of the Second Chicago School, which argued that no such government action was needed, and that markets could, with few exceptions, be treated as if they were in long-run perfectly competitive equilibrium (the "good approximation" assumption). Section 5 discusses the oligopoly problem, its role in dislodging the structure-conduct-performance paradigm by the Second Chicago School as a source of antitrust advice and in the rise of game theoretic approaches to the analysis of imperfectly competitive markets by economists. Section 6 discusses the evolution of the Second Chicago School in the face of the evident failure of mainstream economists to accept the good approximation assumption. Section 7 concludes with a few remarks on ideology and its impact on industrial economics.

2. Early Development of Industrial Economics[3]

2.1. Origins

Industrial economics is generally said to have emerged as a distinct branch of microeconomics with Edward S. Mason's Harvard seminar of the 1930s (Markham and Papanek, 1970, pp. vii–viii), but the topics that occupy industrial economists have concerned economists since before the emergence of political economy as a distinct branch of the social sciences in 1776.

Policy questions that remain at the heart of industrial organization were the subject of widespread academic and popular debate in the United States between 1880 and 1900, a debate that continued at only a slightly less intense level between 1900 and 1920. Seven of the first 10 presidents of the American Economic Association played active roles in this debate.[4] Marshall's 1919 *Industry and Trade* made international comparisons in industrial organization and drew conclusions for economic development. Like the bourgeois gentleman who spoke prose without realizing it, economists who studied "railway problems" (Ripley, 1907) or "trust and corporation problems" (Burns, 1937, p. 663) studied industrial economics in everything but name.

But they did so using analytical tools that seemed to them to be ill-suited to the task. The mainstream price theory of the early twentieth century consisted of a theory of competitive markets and a theory of monopoly, with a vast wasteland in between.[5] This theory of competitive markets was not the modern model of perfect competition, but its Marshallian predecessor. To classify a market as competitive in this sense required only that it "would be possible for other businesses to produce a commodity with the same technical specifications as the product of any particular firm, and to offer it for sale to that firm's customers." If this condition were met (Andrews, 1951, pp. 141–142), "the possibility of entry of other producers would ensure that long-run price would be equal to the normal average cost of production."

[3] For discussions of the development of the field of industrial economics, see Bain (1949a, pp. 129–133), E. T. Grether (1970), Phillips and Stevenson (1974), David Dale Martin (1976), Hay and Morris (1979), Schmalensee (1982, 1987, 1988), Davies and Lyons (1989), Bonanno and Brandolini (1990), and, from a perspective of legal scholarship, Hovenkamp (1991, Chapter 22).

[4] These are Francis A. Walker, John Bates Clark, Henry C. Adams, Arthur T. Hadley, Richard T. Ely, Edwin R. A. Seligman, and Jeremiah W. Jenks. The works of two others (Tassig and Patten) on tariff policy touched upon topics that would now be classified in industrial economics.

[5] Schumpeter (1934, p. 249); Bain (1944, p. 4); Stigler (1949, p. 12); Andrews (1951, p. 141; 1952, p. 72); Schneider (1967, p. 139).

Economists of the period were aware of the disconnect between the implications of this theory of competitive markets and the industrial world around them (Marshall, 1925 [written in 1890], p. 268):

> it is chiefly from America that a cry has been coming with constantly increasing force for the last fifteen years or more, that in manufactures free competition favours the growth of large firms . . . ; that such firms, if driven into a corner, will bid for custom at any sacrifice; that, rather than not sell their goods at all, they will sell them at [marginal cost]. . . , which is sometimes very little; that, when there is not enough work for all, these manufacturers will turn their bidding recklessly against one another, and will lower prices so far that the weaker of them will be killed out, and all of them injured; so that when trade revives they will be able, even without any combination amongst themselves, to put up prices to a high level; that these intense fluctuations injure both the public and the producers; and the producers, being themselves comparatively few in number, are irresistibly drawn to some of those many kinds of combinations to which, nowadays, the name Trust is commonly . . . applied.

Spurred by this perceived disconnect, some economists developed new theoretical tools, while others turned to empirical approaches.

2.2. Monopolistic Competition

An initial theoretical response was the attempt to refine the cost curve apparatus of Marshall's theories of the firm and of industry supply. Sraffa, commenting on these efforts, drew attention to the importance of the demand side of the market for market performance (1926, p. 543):

> The chief obstacle against which [businessmen] have to contend when they want gradually to increase their production does not lie in the cost of production—which, indeed, generally favours them in that direction—but in the difficulty of selling the larger quantity of goods without reducing the price, or without having to face increased marketing expenses. This necessity of reducing prices in order to sell a larger quantity of one's own product is only an aspect of the usual descending demand curve, with the difference that instead of concerning the whole of a commodity, whatever its origin, it relates only to

> the goods produced by a particular firm; and the marketing expenses necessary for the extension of its market are merely costly efforts . . . to increase the willingness of the market to buy from it—that is, to raise that demand curve artificially.

Two books published in 1933, Edward Chamberlin's *The Theory of Monopolistic Competition* and Joan Robinson's *The Economics of Imperfect Competition*, followed up on Sraffa's theme.[6] Both put forward analytical frameworks that incorporated firm-specific downward sloping demand curves and costly sales efforts as essential aspects of the firm's environment.

Robinson (1969, p. xiii) acknowledges her debt to Sraffa (1926) and the literature of which it was a part. Chamberlin (1961, pp. 517–518) denies any connection between his work and the cost curve controversy. He traces it instead to a much earlier debate on railroad rates, a debate that drew his attention to both oligopoly and product differentiation as features of industrial markets.[7] Railroads were the first wave in the rise of large-scale industry in the United States, and to this extent Chamberlin's work may be more directly connected than that of Robinson with the underlying industrial developments that prompted economists to explore new analytical frameworks.

Samuelson (1967, p. 138) writes of the theory of monopolistic competition as a revolution, leading "economists into a new land," the land of imperfect competition (Samuelson 1967b, p. 108, fn. 5):

> If the real world displays the variety of behavior that the Chamberlin-Robinson models permit . . . then reality will falsify many of

[6] On the relation between the two works, see White (1936) and Fisher (1989, p. 114, fn. 2, emphasis in original):

> It is interesting to note that Joan Robinson, in her *The Economics of Imperfect Competition* (1933) which is often paired with Chamberlin's book, simply failed to understand the oligopoly problem altogether. She assumed (p. 21) that the behavior of each oligopolist can be modelled by creating a demand curve taking the optimal reactions of rivals into account and then having the oligopolist set marginal revenue equal to marginal cost. This totally begs the question of what those optimal reactions are—and the fact that one cannot know the answer to that before creating the theory is the central core of the oligopoly problem. Chamberlin made considerable efforts to differentiate his product from that of Joan Robinson. In the oligopoly dimension, at least, he was right.

[7] The relation between the railroad rate controversy and Chamberlin's work is discussed by Ekelund and Hébert (1990).

> the important qualitative and quantitative predictions of the competitive model. Hence, by the pragmatic test of prediction adequacy, the perfect-competition model fails to be an adequate approximation.

Stigler offers a more measured assessment (1949, p. 24):

> The general contribution of the theory of monopolistic competition ...seems to me indisputable: it has led to reorientation and refinement of our thinking on monopoly. We are now more careful to pay attention to the logical niceties of definitions of industries and commodities. We are now more careful to apply monopoly theory where it is appropriate. The importance of the trade mark and of advertising, and the need for the study of product structure and evolution, have become more generally recognized.

Most economists would probably credit the theory of monopolistic competition with a greater impact than Stigler is willing to concede. If it appears less than revolutionary to contemporary economists, that may well be because they have grown up accustomed to having monopolistic competition as part of their intellectual landscape.[8]

2.3. Structure-Conduct-Performance

The analytical framework that came out of the 1930s was the *structure-conduct-performance* (S-C-P) paradigm. It was formulated in literary rather than mathematical form, and it held center stage in industrial economics for some 40 years.

The economists who erected the S-C-P framework were interested in explaining the way prices were determined in imperfectly competitive markets. This interest was explicitly motivated by contemporary industrial developments (Mason, 1939, p. 63):

> The growth of corporate bureaucracies (with the consequent institutionalization of management decisions), the separation of ownership

[8] For elements of one debate on monopolistic competition, see Archibald (1961, 1963), Stigler (1963), Friedman (1963), and also Chamberlin (1957, Chapter 15). I do not pursue this and other such skermishes, as they proved to be false starts in the debate between Chicago and the rest of the profession. The seed planted by Chamberlin did not fully flower until much later (Dixit and Stiglitz, 1977; Salop, 1979; Wolinsky, 1986).

> from control, and the growing influence of labor organization on policy making are all factors "internal to the firm" which may and do affect its reaction to market situations.

Their view was that the models of competitive markets and of monopoly that economists had to work with were not suited for this purpose (Mason, 1939, p. 61):

> In perfect markets, whether monopolistic or competitive, price is hardly a matter of judgment and where there is no judgment there is no policy. The area of price policy, then, embraces the deliberative action of buyers and sellers able to influence price; that is to say, it covers practically the whole field of industrial prices.

They rejected early formal theoretical models of imperfectly competitive markets as inapplicable in practice (Mason, 1939, p. 62):[9]

> It would no doubt be extremely convenient if economists knew the shape of individual demand and cost curves and could proceed forthwith, by comparisons of price and marginal cost, to conclusions regarding the existing degree of monopoly power. The extent to which the monopoly theorists, however, refrain from an empirical application of their formulae is rather striking. The alternative, if more pedestrian, route follows the direction of ascertainable facts and makes use only of empirically applicable concepts.

They also rejected the then-common institutional approach to industry studies, which they felt was primarily descriptive (Burns, 1937, p. 664, emphasis added):

> studies of particular industries assumed a conventional pattern. ... The technical processes of production were described. The organization of the industry was discussed in terms of the size and location of plants, the scope of ownership control (the size and extent of integration of firms), the organization of marketing, labor conditions, and the history of mergers in the industry. ... The discussion of wages and possibly profits implied an interest in the functioning of the industry, *but the aspect of its functioning most vital to theorists and purchasers, namely its price policy, received scant attention.*

[9] See similarly Bain (1944, p. 5). For later calls for "a return to the data," see Andrews (1951, p. 172, 1952, p. 75) and Coase (1972).

Mason (1939, p. 61) specifically rejects the institutionalist approach.

Economists of the time called for a general analytical framework (Burns, 1937, p. 665):[10]

> The primary necessity ... is some broad framework within which price behavior can be analyzed in various industries. It must explain the relationship between the organization of production and distribution and the behavior of buyers and of prices.

The structure-conduct-performance paradigm developed out of Mason's seminar. It was the organizing framework of research in industrial organization from the 1930s to the 1970s, and was the basis of the two successive leading textbooks in the field.[11]

The earliest research in what became the S-C-P tradition were book-length studies of single industries. Subsequently, Bain (1956) published a comparative study of a small number of industries. He did not employ regression analysis. Other industrial economists soon turned to the econometric analysis of cross-section samples of industry data, first of small numbers of industries, and later of large samples covering essentially all manufacturing.

2.4. Did the S-C-P Paradigm Treat Market Structure as Endogenous or Exogenous?

Because of the role the issue played in the dislodging of the S-C-P approach by what I will call the Second Chicago School and the subsequent dislodging of the Second Chicago School by game-theoretic analysis, I wish to deal explicitly with the question whether the S-C-P paradigm treated market structure as being determined by economic forces, or as exogenous, determined outside the marketplace?

Some economists have taken the view that the S-C-P approach paid scant attention to the determinants of market structure. McGee (1988, p. 2, emphasis in original) takes the view that causation in the S-C-P paradigm was mostly in one direction: "In the beginning, most economists seem to have believed that the structure-conduct-performance relationship was largely or altogether one way: to a significant degree the *structure* of an industry determines the *conduct* of firms in it; and how firms behave to a significant degree determines how well the industry *performs*." Davies and Lyons (1996, p. 89) write that economists who

[10] See similarly Mason (1939, p. 61) and Andrews (1952, p. 75).

[11] Bain (1959); Scherer (1970, 1980), and Scherer and Ross (1990).

worked in the S-C-P tradition "gave relatively little thought to the fundamental determinants of concentration itself."

This is a difficult position to defend. Heflebower's (1954) "Theory of industrial markets and prices" was as much about factors determining market structure as about factors determining market performance. A widely-known and generally-accepted schematic representation of the S-C-P framework shows clear feedback links from firm conduct to market structure (Scherer, 1970, p. 5). A key theoretical element of the S-C-P analytical framework was the limit price model, associated with Bain (1949b).[12] In its simplest version (Modigliani, 1958; Sylos–Labini, 1957, 1962) the limit price model supposes that an incumbent firm or several incumbent firms can discourage future entry by setting a low current price. While this formulation has been subject to criticism from the game-theoretic perspective (Friedman, 1979; see also Bain, 1949b, pp. 452–453), it may be mentioned here simply to note that it explicitly makes the number of firms on the supply side of the market depend on business conduct, so that supply-side market structure is endogenous.

Passing from the theoretical to the empirical, there is a large literature in the structure-conduct-performance tradition that seeks to explain cross-industry differences in market structure in terms of technological and conduct variables. What may be the earliest of these, Fuchs (1961), studies the impact of multiplant operation on market structure. A prominent paper, Comanor and Wilson (1967), in research that Ekelund and Hébert (1990, p. 28, footnote 3) describe as "continuing the Harvard tradition," argued that advertising was a factor causing seller concentration. Research seeking to explain the causes of market structure is difficult to reconcile with the claim that the S-C-P approach took market structure to be exogenous.

S-C-P economists thought market structure changed slowly (Bain, 1970).[13] But they generally recognized that the nature and rate of change of market structure was affected by economic forces, including underlying demand- and supply-

[12] There are many anticipations of the limit price model, including Marshall (1925/1890, p. 270):

> The leaders in the movement towards forming Trusts seem to be resolved to aim in the future at prices which will be not very tempting to any one who has not the economies which a large combination claims to derive ... from its vast scale of business and its careful organization...

[13] Later empirical evidence suggests they were correct about this.

side conditions and the conduct of firms in the market.

3. The First Chicago School

While it is customary to write of "the Chicago School," at least two can be distinguished (Bronfenbrenner, 1962, pp. 72–73).[14] The First Chicago School may be dated roughly to the 1930s and 1940s, the Second, insofar as industrial economics is concerned, from immediate post-World War II period.[15]

The First and Second Chicago Schools had in common beliefs in the efficacy of the market system of organizing economic activity and that the role of government should be as limited as possible, consistent with making the market system work.[16] Where they differed was in their views on what is the minimum necessary role of the government. Economists associated with the First Chicago School were advocates of a *laissez faire* government policy, which (Lange, 1945-1946, pp. 31–32):[17]

> maintains that the capitalist economy, provided it is not hampered by government planning, spontaneously operates in such a way that it secures the maximum of public welfare.

[14] Reder (1982, p. 1) writes that

> The influence of specialty upon one's perspective of Chicago economics is not trivial. . . . [the] Chicago corner of the economics profession can look quite different to someone in Monetary Theory or International Trade than to a specialist in Labor, Industrial Organization or Law and Economics.

When I discuss the Chicago School or Schools, I refer to what Posner (1979) terms "the Chicago School of antitrust analysis."

[15] The 1946 appointment of Aaron Director to the University of Chicago Law School was critical to the transition from the First to the Second Chicago Schools; see Coase (1998), Peltzman (2005). See also Stigler (2005).

[16] Compare Friedman (1974, p. 11):

> In discussions of economic policy, "Chicago" stands for belief in the efficiency of the free market as a means for organizing resources, for scepticism about government intervention into economic affairs, and for emphasis on the quantity of money as a key factor in producing inflation.

[17] Director (1964, p. 2) writes that "Laissez faire has never been more than a slogan in defense of the proposition that every extension of state activity should be examined under a presumption of error."

They were also convinced that government should set the ground rules for private competition by means of a strong antitrust policy.

The essential views of the First Chicago School on this matter are laid out in Henry C. Simons' 1934 essay *A Positive Program for Laissez Faire*. Simons defended the market mechanism of resource allocation on the grounds that it was more effective than other systems and that it was essential for the preservation of a free society. *A Positive Program* took the view that government had to play an affirmative (positive) role to maintain the functioning of a market economy. This view was a child of its time (Director, 1947, p. vi):

> There may once have been substantial merit in the notion that the free-market system would steadily gain in strength if only it were freed of widespread state interference. By 1934 it became evident that a combination of [this] negative attitude, which permitted the proliferation of monopoly power, and promiscuous political interference, which strengthened such power, threatened "disintegration and collapse" of the economic organization. And only the "wisest measures by the state" could restore and maintain a free-market system.

Simons' positive program envisaged government making a clear distinction between parts of the economy where competition could be an effective resource allocation mechanism and parts of the economy where it could not. Where competition *could* be effective, Simons would have had the government pursue an activist antitrust policy, to ensure that competition *would* be effective. Not only should the government prohibit collusion, and punish it if detected, it should proactively control the size of firms to maintain a market structure consistent with competitive outcomes (1936, pp. 70–71):

> There must be vigorous and vigilant prosecution of conspiracy in restraint of trade...
>
> Sharp separation must be made between operating companies and investment trusts ... Operating companies should be denied the right to own securities of other such companies... Operating companies must be limited in size, under special limitations prescribed for particular industries by the Federal Trade Commission, in accordance with the policy of preserving real competition.

Where the underlying technology dictated that competition could not be an effective resource allocation mechanism,[18] Simons saw the policy choice as being between regulation and public ownership. His view of regulation, based on observation of the way regulation worked in practice, was largely negative (1947, pp. 50–51):

> With the railroads, the abuse of private monopoly power led finally to real control over the prices of services. We have developed in the Interstate Commerce Commission an unusually competent and scrupulous public body. Even here, however, the preposterous system of relative charges (freight classification), and the disastrous rigidity of freight rates during the depression, testify eloquently to the shortcomings of the regulation expedient; the intrenched position of the railway brotherhoods indicates clearly how governments reconcile the interests of small, organized groups and those of the community at large. In the field of local utilities a half-century of effort at regulation yields up a heritage of results, a cursory inspection of which should suffice to dampen anyone's enthusiasm for a system of private monopoly with superimposed government regulation.

Simons therefore reluctantly came down on the side of public ownership of natural monopoly industries (1936, p. 74):

> In my pamphlet, I suggested early transition to government ownership for the railroads, and gradual movement in that direction with the other utilities. Candidly, I feel that our situation with respect to these industries will always be unhappy, at best; and I have no genuine enthusiasm for public ownership. My advocacy of the change is motivated primarily as an attack upon the notion, now common in high places, that our arrangements with respect to the railroads provide a simple and admirable model for the control of other industries generally.

For Simons, the political and economic justifications for his positive program were inextricably intertwined (1936, p. 75):

[18] Here, notice, we come up against the endogeneity of market structure.

> This is the compelling reason for stamping out private monopoly. For every suppression of competition gives rise to an apparent need for regulation; and every venture in regulation creates the necessity of more regulation; and every interference by government on behalf of one group necessitates, in the orderly routine of democratic corruption, additional interference on behalf of others. The outcome ... is: an accumulation of governmental regulation which yields, in many industries, all the afflictions of socialization and none of its possible benefits; an enterprise economy paralyzed by political control; the moral disintegration of representative government in the endless contest of innumerable pressure groups for special political favors; and dictatorship.

4. The Second Chicago School

The Second Chicago School carried further the antipathy of the First toward government involvement in the marketplace, rejecting any antitrust policy beyond a prohibition of collusion and mergers to monopoly or near-monopoly,[19] rejecting government regulation of natural monopoly, and certainly rejecting public enterprise.

Reder writes that a distinguishing characteristic of the Second Chicago School was what he calls the "Tight Prior equilibrium" assumption, the view that the economy could be treated as if it were essentially *Pareto optimal*, that (1982, p. 11): "decision makers so allocate the resources under their control that there is no alternative allocation such that any one decision maker could have his expected utility increased without a reduction occurring in the expected utility of at least one other decision maker."

The view that the economy can be treated as if it is Pareto optimal follows from four assumptions that Reder ascribes to Chicago economists (1982, p. 11, emphasis in original, footnotes omitted):

> (1) *most* individual transactors treat the prices of all goods and services that they buy or sell, as independent of the quantities that they transact;

[19] Posner (1979, p. 928). Posner also writes that opposition to cartels was (p. 932) "[p]artly, perhaps, for tactical reasons (not to seem to reject antitrust policy in its entirety)...".

> (2) the prices at which individuals *currently agree* to transact are market clearing prices that are consistent with optimization by all decision makers;
>
> (3) information bearing on prices and qualities of all things bought and sold, present and future, is acquired in the quantity that markets its marginal cost equal to its price; i.e., information is treated like any other commodity;
>
> (4) neither monopoly nor governmental action (through taxation or otherwise) affects relative prices or quantities sufficiently to prevent either marginal products or compensation of identical resources from being approximately equal in all uses.

As Reder sees it, students of the Chicago School of antitrust policy took the view that (1982, p. 12):

> in applied work, . . . in the absence of sufficient evidence to the contrary, one may treat observed prices and quantities as good approximations to their long-run competitive equilibrium values. Call this the "good approximation assumption."

4.1. Chicago I and Chicago II

The Second Chicago School repudiated Simons' *Positive Program for Laissez Faire,* on the ground that it advocated an interventionist role for government. This shift may reflect a change in the political atmosphere (Friedman, in Kitch (1983, p. 178)):

> You have to recognize what the environment was at the time. By comparison with almost everybody else [Simons] was very free market oriented. I've gone back and reread the *Positive Program* and been astounded at what I read. To think that I thought at the time that it was strongly free market in its orientation!

It may also reflect evidence about the importance of economies of large scale production, or experience with the ability (or inability) of government to intervene effectively in the economy, that accumulated after the appearance of *A Positive Program* (Friedman, 1962/1982, p. 32). In any case, the claim that later advocates of a government role essentially the same as that put forward by Simons were

making recommendations inconsistent with economic theory would seem to be a delicate one for the Second Chicago School to make.[20]

4.2. Methodology

A methodological debate associated with Friedman (1953) surfaces in connection with the later rise of game theory: is a theory to be judged by the realism of its assumptions, or by the adequacy of its predictions?[21]

Blaug (1986, p. 265) has written that "Methodology is like medicine. We tolerate it because it is supposed to be good for us, but we secretly despise it." Like many medicines, methodology is probably also best if taken in small doses, and that is what will be offered here.

Stigler states the Chicago position (1949, p. 23):

> The purpose of the study of economics is to permit us to make predictions about the behaviour of economic phenomena under specified conditions. The sole test of the usefulness of an economic theory is the concordance between its predictions and the observable course of events. Often a theory is criticized or rejected because its assumptions are "unrealistic." Granting for a moment that this charge has meaning, it burdens theory with an additional function, that of description. This is a most unreasonable burden to place upon a theory: the rôle of description is to particularize, while the rôle of theory is to generalize—to disregard an infinite number of differences and capture the important common element in different phenomena.

As suggested by the qualification "granting that the charge of unrealism has meaning," it may first be noted that all theory is unrealistic. It is the essence of theory that it abstracts from reality. While it may be possible to say that the

[20] The assertion that Simons should be thought of as an interventionist rather than an advocate of *laissez faire* has not gone unchallenged. de Long (1990) argues that Simons has a legitimate claim to the label "classical liberal." de Long's view is that what separates the First and the Second Chicago schools is not their positions about where to go, but rather their positions on how to get there (1990, p. 618) "The conflict between Chicago then and Chicago today is about what the necessary foundations for a competitive free market economy are, and not about the desirability of such an economic order."

[21] On Friedman's methodology, see Samuelson (1963), Machlup (1964), Wong (1973), Boland (1979), Frazer and Boland (1983), as well as comments and replies on the latter in the September 1984 issue of the *American Economic Review*.

assumptions underlying one theory are more or less realistic than those underlying another theory, all theory is based upon assumptions that are, to some extent, unrealistic. Otherwise, it would not be theory, it would be a list.

At the same time, it is difficult to see how one could evaluate the validity of theoretical predictions except within the framework of a model that permits examination of what are thought to be the interesting alternative hypotheses (Friedman, 1953, p. 38, emphasis added):[22]

> It would be highly desirable to have a more general theory than Marshall's, one that would cover at the same time both those cases in which differentiation of product of fewness of numbers makes an essential difference and those in which it does not. *Such a theory would enable us to handle problems we now cannot and, in addition, facilitate determination of the range of circumstances under which the simply theory can be regarded as a good enough approximation.*

Koopmans (1957, p. 142) writes of economic theory "as a sequence of conceptual *models* that seek to express in simplified form different aspects of an always more complicated reality." Industrial economists generally work with models that are less simplified than the neoclassical model of perfectly competitive markets.[23]

5. Confronting the Oligopoly Problem

5.1. S-C-P to Chicago

Since the industrial revolution, a central problem for public policy toward business has been that (Second Chicago School assumptions to the contrary notwithstanding), performance in markets supplied by a few large firms seems often to resemble that which results from collusion, without collusion taking place (Mason, 1949, p. 1277):

[22] This passage is quoted by Chamberlin (1957, p. 16). Friedman continues (1953, p. 38) "To perform this function, the more general theory must have content and substance; it must have implications susceptible to empirical contradiction and of substantive interest and importance." As an example of substantive implications that are subject to empirical contradiction and could not be obtained in analysis that begins from the premise that prices and quantities in the U.S. automobile industry can be treated as if they are long-run competitive equilibrium values, one might cite Bresnahan's (1981) estimate of consumer welfare losses due to quality downshifting.

[23] It is mainly in the analysis of dynamic models of entry that the literature works with an explicit "as if competitive" assumption, and even in that literature the assumption is far from universal.

> high overhead costs, large cyclical variations in the volume of sales, and immobility of resources are combined in a substantial number of industrial markets. Given these conditions, together with a small number of firms, some economists have contended that such phenomena as price uniformity, price leadership and the relative inflexibility of prices ... are frequently compatible with the independent action of firms all recognizing their interdependence.

The problem is, given such an industry, what to do about it (Mason, 1949, p. 1277):[24]

> If the behavior is really the result of agreement, enjoining the agreement may, by securing independence of action, change the market behavior. But if the action of firms is already independent, this remedy is useless.

One answer, of course, may be to do nothing. Simons argued that regulation often begins with the goal of protecting the consumer from the regulated industry and ends up protecting the regulated industry from competition. Government planning may crash on the shoals of information problems and bureaucracy. Breaking up large, efficient firms for the sake of artificially maintaining a larger number of smaller and less-efficient firms imposes unnecessarily high production costs on society. If regulation, government ownership, and imposed limits on firm size are the only policy options, then perhaps to do nothing is best.

But from the 1950s through the 1970s, mainstream economists championed the kind of deconcentration plan that had been put forward by Henry Simons. Stigler (1952, p. 162, p. 164) wrote that few disinterested people would deny the facts that

1. Big businesses often possess and use monopoly power.
2. Big businesses weaken the political support for a private-enterprise system.
3. Big businesses are not appreciably more efficient or enterprising than medium-size businesses.

[24] Posner (1976, 2001), and *In re High Fructose Corn Syrup* 295 F.3d 651 (2001), is willing to infer collusion from evidence of market performance that would result from explicit collusion. This position may reflect the view that collusion can never be entirely tacit. It is a policy view that seems to confront *Theatre Enterprises* (346 U.S. 537 1954).

He continued that (1952, p. 164) "to deal with the problems raised by big business" "The obvious and economical solution ... is to break up the giant companies."[25]

Stigler came to feel that these views were mistaken (1988, pp. 97–108). But the fact that he held them in the early 1950s suggests that they were not the hallmark of atheoretical pseudo-economists who did not understand neoclassical economic theory. It *is* probably fair to say that economists who advanced deconcentration proposals did not accept the position that prices and quantities in most industries, most of the time, could be treated as if they were at their competitive equilibrium values. But that assumption is not part of neoclassical economic theory.

Kaysen and Turner put forward a detailed deconcentration proposal (1959, pp. 113–114):

> The logic of our policy goal ... calls for a widespread application of dissolution remedies, on the ground that an increase in numbers and reduction of concentration is the surest and most durable way of reducing market power.

They would not, however, have broken up existing firms if that had meant the loss of economies of large scale production (1959, p. 114).

A government advisory panel (White House Task Force on Antitrust Policy, 1969) subsequently recommended adoption of a law very much like the one suggested by Kaysen and Turner.[26] A bill proposing such a law, Senate Bill 1167 ("The Hart Bill") was introduced in the United States Senate in 1967.[27]

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[25] Stigler apparently held these views at least through December 1955, when he concluded his contribution to an American Economic Association panel discussion with the statement (Stocking *et al.*, 1956, p. 507) "Those of us who wish to see greater use made of what is often the only real remedy are not reckless innovators; we are simply traditionalists who wish to regain the 1911 level of use of the remedy of dissolution."

[26] The task force recommendation is commonly known as the Neal Report, after its chairman, Phil C. Neal, then Dean of the University of Chicago Law School.

[27] For the text of the Hart bill, see Goldschmid *et al.* (1974, pp. 444–448). Perusal of this volume brings to mind Viner's description of an earlier conference (this is quoted by Reder, 1982, footnote 19, from Patinkin, 1981, p. 266; emphasis in original)

> It was not until after I left Chicago in 1946 that I began to hear rumors about a 'Chicago School' which was engaged in *organized* battle for laissez faire and the 'quantity theory of money' and against 'imperfect competition' theorizing and 'Keynesianism.' I remained sceptical about this until I attended a conference sponsored by University of Chicago professors in 1951. The invited participants

The reaction of the Second Chicago School was to attack the S-C-P paradigm, and the case for the deconcentration bill, on two fronts.

The first was to argue on several grounds that the results of empirical research in the S-C-P tradition were invalid. Some of these grounds were technical in nature, and need not detain us here.[28] The part of this critique that has made the most lasting impression was that S-C-P researchers had misinterpreted the consistent finding that profit rates tended to be higher where markets were supplied by a small number of firms. In the Second Chicago School view, far from demonstrating the greater ease of tacit or overt collusion in more concentrated markets, this result reflected a causal link going in the other direction, from firm performance to market structure. This causal link instead demonstrated the endogeneity of market structure, an endogeneity which, according to the Second Chicago School caricaturization, the S-C-P paradigm had ignored. The alternative causal mechanism put forward is that more efficient firms, which are more profitable because they are more efficient, also tend to grow large. Comparing different industries, industries supplied by a few large firms will have higher profit rates, on average, than industries supplied by many small firms, but this is a sign of differential efficiency, not market power.[29]

The idea of a deconcentration policy is not now on anybody's radar screen. But many of the same issues arise if an antitrust authority is faced with a decision whether or not to permit a merger, or if dissolution is proposed as a remedy when a firm has been found guilty of monopolization or abuse of a dominant position.

Since there is a good case to be made that the S-C-P school did in fact regard market structure as endogenous, there are really two pertinent questions to ask. The first is, taking it as given that market structure in any industry tends to

> were a varied lot of academics, bureaucrats, businessmen, etc., but the program for discussion, the selection of chairmen, and everything about the conference except the unscheduled statements and protests from individual participants were so patently rigidly structured, so loaded, that I got more amusement from the conference than from any other I ever attended. Even the source of the financing of the Conference, as I found out later, was ideologically loaded. There is a published account of the proceedings of the Conference, but it does not include the program, etc., as presented to the participants to direct their discussion. From then on, I was willing to consider the existence of a 'Chicago School'...

[28] See Martin (2002, Chapter 6) for discussion.

[29] This argument is particularly associated with Demsetz (1973, 1974), whose work was primitive by the standards of its time (Rosenbluth, 1976). Subsequent research that controls for efficiency differences finds evidence of market power and efficiency effects on profitability.

converge to an equilibrium configuration, must that configuration necessarily be efficient? Since the Second Chicago School rejected the possibility of single-firm action to obtain or enhance market power (Posner, 1979, p. 928), their answer to this question was yes.[30]

The second question is how quickly market structure approaches its equilibrium configuration. If market structure is endogenous and market structure adjusts very quickly to the most efficient arrangement, a deconcentration bill or a vigilant merger policy is unnecessary. The market will get to the efficient configuration on its own, before government can nudge it along.

On the other hand, if market structure is endogenous but market structure adjusts slowly to the most efficient arrangement, one might make a case for a deconcentration bill or a rigorous merger policy, provided policymakers can identify cases in which firms are larger than required for efficient operation. The Second Chicago School rejected this possibility (McGee, 1974, p. 104):[31]

> I see little reason to spend much more time estimating optimum plant or firm sizes except perhaps, in a completely centralized and governmentally controlled economy in which the State tries hard to keep markets from working and consumers from expressing preferences. When properties and markets are at work, and consumers are permitted to choose what and from whom to buy, it is, as far as I am concerned, a trivial matter what the facts of technical economies are, or what economists have to say about them.

This view reflects the Second Chicago School assumption that real world data can be treated as if the real world is Pareto optimal. Thus the Second Chicago School criticized the empirical research that had been used to argue in favor of a

[30] Posner (2001, p. 251) writes that the accusation that the "Chicago School" denies the possibility of single-firm exclusion of efficient competitors reflects a misunderstanding that has endured for at least a quarter-century. If the indicated view of the orthodox Chicago School position is a misunderstanding, it is at least one of respectable vintage.

[31] See also Friedman (1955, p. 237):

> If we ask what size firm has minimum costs, and define "minimum costs" in a sense in which it is in a firm's own interest to achieve it, surely the obvious answer is: firms of existing size.

deconcentration bill.[32]

It also criticized the deconcentration bill on the ground that its theoretical underpinning, the S-C-P approach, was inconsistent with the tight prior equilibrium assumption (Posner, 1979, p. 929):[33]

> Casual observation of business behavior, colorful characterizations (such as the term "barrier to entry"), eclectic forays into sociology and psychology, descriptive statistics, and verification by plausibility took the place of the careful definitions and parsimonious logical structure of economic theory. The result was that industrial organization regularly advanced propositions that contradicted economic theory.

What is meant here by "economic theory" is the neoclassical theory of perfectly competitive markets.

The Hart deconcentration bill never saw the light of day, and for a period of perhaps 10-12 years from the mid-1970s, the Second Chicago School monopolized the giving of antitrust advice to U.S. courts and policymakers.

One can find statements from this period that the Chicago approach had become the mainstream approach among professional economists (Posner, 1979, p. 925):

> I shall argue in this paper that although there was a time when the "Chicago" school stood for a distinctive approach to antitrust policy, especially in regard to economic questions, and when other schools, particularly a "Harvard" school, could be discerned and contrasted with it, the distinctions between these schools have greatly diminished. This has occurred largely as a result of the maturing of economics as a social science, and, as a corollary thereto, the waning of the sort

[32] This criticism should perhaps be viewed in light of Reder's (1982, pp. 12–13, footnote 28) comments on the likely reaction of students of the Chicago approach if Harberger's (1954) estimates of welfare losses in the U.S. economy had suggested significant losses:

> But suppose the losses had been "large" (say, 25 per [cent] of potential GNP), would this have lead to an abandonment of [the Tight Prior Equilibrium]? My conjecture is negative; the measurement would have been attacked, both substantively and methodologically, and research would have proceeded on the assumption that the measurements were incorrect.

[33] See also the remarks of Becker quoted at page 173 in Kitch (1983).

> of industrial organization that provided the intellectual foundations of the Harvard School.

The reference to "the Harvard School" is disingenuous, since the S-C-P approach was subscribed to by the bulk of the profession.

Nelson's reaction to Posner's assertion included (1979, p. 949):

> Posner contrasts the "old" school of industrial organization (Harvard) which he proposes was atheoretic with the "new" school (Chicago) which based itself rigorously on price theory. But the price theory to which Posner refers is the old fashioned price theory of the textbooks of twenty years ago. What Posner does not see is that over the last decade or so a newer price theory is replacing the old. I suggest that the new price theory probably provides better support for the old industrial organization than it does for what Posner calls the new. Indeed, the journals are full of a "new new" industrial organization literature based on the newer price theory, viewing the problem in a way that is more consistent with old Harvard than with new Chicago.

Table 5.1 reports the results of an examination of 117 articles on industrial economics, price theory, and antitrust published in the Papers and Proceedings issue of the *American Economic Review* between 1951 and 1982.[34] I classified these papers in three groups: those consistent with the good approximation theorem, those inconsistent with the good approximation theorem and consistent with the structure-conduct-performance approach, and those inconsistent with the good approximation theorem and also urging the advantages of heterodox alternative approaches.

There is of course an element of judgment in this classification. Few positions are so straightforward as Nutter's (1954, p. 70):

[34] Details of the classification are given in the Appendix. I began the survey with 1951 as the first year of the first postwar decade. I ended the survey in 1982 on the ground that Schmalensee's statement suggests that if ever Chicago had commanded the mainstream of industrial economics, that moment had by then passed (1982, p. 24):

> Recent work follows Harvard in acknowledging the possibility of markets not well described by either perfect competition or pure monopoly, and it follows Chicago in stressing the value of deductive analysis of explicit economic models.

I did not include papers on regulation in the survey, and I did not include discussions.

	1950s	1960s	1970-1982	Total
Chicago	7	9	6	22
Mainstream	24	17	20	61
"Think outside the box"	11	9	14	34

Table 5.1: Classification of 117 articles from the Papers and Proceedings issues of the American Economic Review, 1951-1982.

> We must certainly be struck by the fact that, when we are faced with concrete problems, we place our trust overwhelmingly in the simple and familiar tools of supply and demand, with results that are generally quite satisfactory for most purposes,

as Miller's (1954, p. 15):

> It has become clear that the competitive process is no simple thing and that market structures and behavior defy reduction in either law or economics to a simple dichotomy of monopoly and competition,

or as Simon's (1962, pp. 14–15):

> ...conclusions about welfare in such areas as tax and antitrust policy depend in an important way upon the underlying postulates about the behavior of the individual firm. The picture of the firm that is emerging from the new research is that of a searching, information processing, allocating mechanism. It is doubtful that the propositions that hold under the assumption of static, profit-maximizing firms under conditions of certainty hold for such firms.

Where classification is ambiguous, my intention has been to give the benefit of the doubt to either the first or the third category, as appropriate. The message of Table 5.1, however, seems clear: it has never been the case that the tenets of the Chicago School were the mainstream view of industrial economics.[35]

[35] I have not included Kearl *et al.* (1979) in the sample classified in Table 5.1. They report the results of a survey of a stratified random sample of 600 1974 members of the American Economic Association. Statement 12 in their survey is "Antitrust laws should be used vigorously to reduce monopoly power from its current level." They report that 49 per cent of respondents generally agreed with the statement, 36 per cent agreed with provisions, and 15 per cent generally disagreed. For comparison purposes, the 22 papers I have classified as consistent with the good approximation assumption are 19 per cent of the 117 papers examined.

In retrospect, it seems entirely possible that the views of industrial economists about the Chicago School of antitrust policy were something to which Chicago scholars were in some measure indifferent. In its manifestation that is discussed here,[36] the Chicago School was primarily a school of *antitrust analysis*, and only secondarily a school of thought in industrial economics. Chicago advocates found that (Bork, at p. 8 in Shapiro *et al.*, 1984) "...basic price theory ...is really a quite simple and logical system that can be applied by judges" and that is what appears to have interested them.

5.2. Chicago to Game Theory

While the Second Chicago School was disputing policy primacy with the S-C-P paradigm, there were developments in another part of the forest (Shubik, 1980, p. 21):

> There is a history of mathematical models of oligopolistic competition dating from Cournot (1838) to the theory of games. There is also a literature generated by institutional economists, lawyers, and administrators interested in formulating and implementing public policy. It has been the tendency of these groups to work almost as though the other did not exist.

The founders of the S-C-P approach had rejected contemporary economic theory as inadequate for the analysis of imperfectly competitive markets. So, perhaps, it was.[37] But game theory, which extends neoclassical price theory to environments of incomplete and imperfect information, provides a natural framework for the analysis of strategic behavior. Mainstream industrial economists, having faced criticism for the failure to use formal models, were not slow to turn to game theory as an alternative approach. Game theory directs attention toward the realism of a model's assumptions (Fudenberg and Tirole, 1987, p. 176, emphasis added):

> Game theory has had a deep impact on the theory of industrial organization ... The reason it has been embraced by a majority of researchers in the field is that it imposes some discipline on theoretical thinking. It forces economists to specify the strategic variables, their timing,

[36] See footnote 14.

[37] See Nelson (1979, p. 952) for a similar view.

> and the information structure faced by firms. As is often the case in economics, the researcher learns as much from constructing the model ... as from solving it because *in constructing the model one is led to examine its realism.* (Is the timing of entry plausible? Which variables are costly to change in the short run? Can firms observe their rivals' prices, capacities, or technologies in the industry under consideration? Etc.)

This may be contrasted with the Friedman (1953) view that the realism of a model's assumptions is immaterial, what matters is the accuracy of its predictions.

Like Pandora, who loosed the ills of the world and found they could not be closed up again, the Second Chicago School invoked formal theory in its contest with the S-C-P approach, and found it could not close it up again. Faced with the fact that game theoretic models reproduce, as often as not, the conclusions of the S-C-P paradigm, the reaction of the Second Chicago School was to reject the use of game-theoretic models (Baxter, 1983, p. 320):

> What concerns me is that the economists have rather lapped the bar and the courts. Quite frankly, I do not want them back in the courts talking about new and not well-understood justifications for intervention, some of which sounds [sic] like the half-baked oligopoly theories of twenty years ago (although they are not).

6. Chicago Transformed

Having lost the high ground of theoretical rigor, Chicago turned to empirical research. Coase (1972, p. 62) criticized mainstream industrial organization for its focus on price, an element of market performance: "Industrial organization has become the study of the pricing and output policies of firms, especially in oligopolistic situations...." As we have seen, this focus was intentional on the part of those who developed the S-C-P paradigm.

Coase buttressed his critique of mainstream industrial economics with the plea that attention to market performance was misplaced (1972, p. 60):[38]

> We all know what is meant by industrial organization. It describes the way in which activities undertaken within the economy are divided

[38] In the same place, Coase calls for the study of the division of activity between profit and nonprofit firms and between the private sector and government.

> between firms. As we know, some firms embrace many different activities; while for others, the field is narrowly circumscribed. Some firms are vertically integrated; others are not. This is the organization of industry or—as it used to be called—the structure of industry. What one would expect to learn from a study of industrial organization would be how industry is organized now, and how this differs from what it used to be in earlier periods; what forces were operative in bringing about this organization, and how these forces have been changing over time; what the effects would be of proposals to change, through legal action of various kinds, the forms of industrial organization.

He argued that it was empirical rather than theoretical research that was most likely to generate progress (1972, pp. 70–71):

> it is unlikely that we shall see significant advances in our theory of the organization of industry until we know more about what it is that we must explain. An inspired theoretician might do as well without empirical work, but my own feeling is that the inspiration is most likely to come through the stimulus provided by the patterns, puzzles, and anomalies revealed by systematic data-gathering.

Further, he argued for a specific rather than a general approach (1972, p. 73):

> In my view, what is wanted in industrial organization is a direct approach to the problem. This would concentrate on what activities firms undertake, and would endeavor to discover the characteristics of the groupings of activities within firms. Which activities tend to be associated, and which do not? The answer may well differ for different kinds of firm; for example, for firms of different size, or for those with a different corporate structure, or for firms in different industries.

The position taken by Coase in 1972 has much in common with the position taken by Mason and his students in the 1930s: the theory we have is not satisfactory, let us turn to empirical analysis as a way of laying the foundation for an adequate theory. But the kind of industry study Coase calls for sounds very much like the primarily descriptive industry studies rejected by the developers of the S-C-P approach.[39]

[39] Posner (1979, p. 931), is quite critical of S-C-P industry studies, which in his view had the

7. Final Thoughts

Industrial economics is a contentious field, as are many other branches of economics. Lange (1945–1946, pp. 22–23), discusses four reasons economists may differ in the policy recommendations they draw from economic analysis.

First, Lange writes, economists may differ about social objectives. Two economists who agree about the impact of a tariff on the domestic market will differ in their recommendations for the appropriate tariff level if one economist recommends a policy to maximize consumer welfare and another recommends a policy to protect domestic firms from foreign competition.

Second, economists may disagree about facts. One economist may think that it is possible for economists to evaluate the minimum size firm needed in (say) electric power generation to obtain the lowest possible average cost, and that this size is small relative to the size of the market. Another economist may think that it is not possible for economists to measure the minimum lowest-average cost firm size, or that if such measurement is possible, that the indicated size is large relative to the size of the market. Economists with such alternative views would make different recommendations about merger policy in the electric power industry.

Third, some economists may fail to apply scientific procedures correctly. The remedy for this is straightforward, and will manifest itself if the usual process of give-and-take in academic journals runs its course.

Finally, economists' policy recommendations may differ for reasons of ideology.[40] Wiles (1983, pp. 61–62) defines an ideology as

> a general and coherent *Weltanschauung*, felt passionately and defended unscrupulously. It contains *sacred propositions* of a factual sort. In the face of contrary evidence, the words in these propositions will be redefined, or the philosophical status of the propositions will even be changed, in order not to abandon the original concatenation of words. A special methodology and vocabulary will also grow up, the use of which confines the devotees to problems and approaches that cannot threaten the sacred propositions.

characteristic that "The powerful simplifications of economic theory—rationality, profit maximization, the downward-sloping demand curve—were discarded, or at least downplayed, in favor of microscopic examination of the idiosyncrasies of particular markets."

[40] For a call for greater attention to the impact of ideology on policy, see North (1983).

Discussing the impact of ideology on economics, Lange (1945–1946, p. 23) writes:

> The really important influences, however, are those which are subconscious. The economist subject to them is unaware of their existence; the influences operate through processes of rationalisation of subconscious motivations. The result is the production of *ideologies*, i.e. systems of beliefs which are held not on grounds of their conformity to scientific procedure but as rationalisations of subconscious, non-logical motives. ...[Ideologies] convince only those who share the same subconscious motivations and undergo the same processes of rationalisation.

Ideology permeates economics. It influences the research topics individual economists find interesting. It influences the reactions of journal editors and referees to research results that are subject to the winnowing process of scientific screening (Schumpeter, 1949, p. 349):

> The majority of economists ... are ready enough to admit [ideology's] presence though, like Marx, they find it only in others and never in themselves; but they do not admit that it is an inescapable curse and vitiates economics to its core.

Arguments that are made for ideological reasons may nonetheless be correct. The conclusions reached on the basis of those arguments may be correct, in a scientific sense. As Schumpeter also wrote, with the bluntness that a great mind can permit itself (1949, p. 349):

> ...*ideologies are not simply lies*; they are truthful statements about what a man thinks he sees.

Such arguments should not, therefore, be dismissed simply on the ground that they are ideological. They should be considered on the merits.

I began this essay by asking "What has antitrust brought to industrial economics?" "Ideology" is a central part of any answer to that question.

It is interesting to speculate how the field of industrial economics might have developed if deconcentration proposals of the kind put forward by Simons, Stigler, Kaysen and Turner, and others had never seemed to approach a tangible possibility

of adoption. I have suggested above (Section 5.1) that "the Chicago School was primarily a school of *antitrust analysis*, and only secondarily a school of thought in industrial economics." One alternative reality is that without the target provided by deconcentration proposals, no full-blown attack (footnote 32) on the structure-conduct-performance approach would have taken place. Initiatives like Phillips (1960, 1961), Williamson (1965), and Richardson (1972), which in varying ways pointed out that firms operate not only in product markets but also within a dense network of interfirm contracts, and that this network has efficiency as well as market power implications, might very well have had a greater direct following than has been the case.[41] The theoretical tools to formally model imperfectly competitive markets that did not exist in the 1930s had been developed by the early 1970s, and game theory would in any case have supplanted the structure-conduct-performance paradigm.[42] The antitrust policy positions supported by mainstream industrial economics would be very much what they are today. What would be absent would be the misinterpretation of mainstream economic views embodied in some U.S. antitrust precedents.

The Second Chicago School did change the landscape of professional industrial economics. Mainstream industrial economists now look for efficiency as well as market power explanations of real-world observations. Mainstream industrial economists now expect theoretical research to be carried out using formal models that are consistent with mainstream microeconomic theory, and are likely to look askance at theory formulated in the discursive style that characterized the S-C-P paradigm.[43] But mainstream industrial economic theory is not neoclassical price theory, nor is it the theory of perfectly competitive markets. Mainstream industrial economists today reject the Second Chicago School "good approximation" assumption that prices and quantities in real-world markets can, most of the time, be treated as if they are competitive equilibrium values.

[41] In this alternative state of the world, transaction cost economics would certainly have emphasized the efficiency aspects of interfirm relations. It may well be that the transaction cost economics message that enduring institutional arrangements may have efficiency implications has been garbled by the hard-core Chicago message that enduring institutional arrangements can have *only* efficiency implications.

[42] This is at least suggested by the size of the "think outside the box" category in Table 5.1.

[43] If usage of discursive theory now appears in the literature, it is likely to be employed in an institutionalist/transaction cost analysis that is in many ways the successor of the Second Chicago School.

8. Appendix

This appendix lists by group the classification of papers reported in Table 5.1. All papers are from the *American Economic Review*.
(1) Implicitly or explicitly consistent with the view that the good approximation assumption is appropriate for industry analysis, or with policy positions implied by the good approximation assumption.

Ratchford, B. U. "An economist's view," 41(2), May 1951, pp. 299-306.

Taylor, Overton H. "The future of economic liberalism," 42(2), May 1952, pp. 1-15.

Knight, Frank H. "Institutionalism and empiricism in economics," 42(2), May 1952, pp. 45-55.

Reder, Melvin W. "Rehabilitation of partial equilibrium theory," 42(2), May 1952, pp. 182-197.

Brozen, Yale "Determinants of the direction of technological change," 43(2), May 1953, pp. 288-302.

Nutter, G. Warren "Competition: direct and devious," 44(2), May 1954, pp. 69-76.

Levi, Edward H. "The monopoly problem as viewed by a lawyer," 47(2), May, 1957, pp. 293-302.

Telser, Lester G. "How much does it pay whom to advertise?" 51(2), May 1961, pp. 194-205.

Holton, Richard H. "Scale, specialization, and costs in Retailing," 51(2), May 1961, pp. 206-212.

Adelman, M. A. "The antimerger act, 1950-60," 51(2), May 1961, pp. 236-244.

Dewey, Donald "Mergers and cartels: some reservations about policy," 51(2), May 1961, pp. 255-262.

Heady, Earl O. "Efficiency of the farm firm," 54(3), May 1964, pp. 97-106.

Massel, Mark S. "Anniversaries of the Patent and Sherman Acts: competitive policies and limited monopolies," 56(1/2), March, 1966, pp. 284-290.

Blank, David M. "The quest for quantity and diversity in television programming," 56(1/2), March 1966, pp. 448-456.

Telser, Lester G. "Supply and demand for advertising messages," 56(1/2), March 1966, pp. 457-466.

Bork, Robert H. "The goals of antitrust policy," 57(2), May 1967, pp. 242-253.

Telser, L. G. "Searching for the lowest price," 63(2), May 1973, pp. 40-49.

Williamson, Oliver E. "Markets and hierarchies: some elementary considerations," 63(2), May 1973, pp. 316-325.

Carlton, Dennis W. "Uncertainty, production lags, and pricing," 67(1), February 1977, pp. 244-249.

Klein, Benjamin "Transaction cost determinants of 'unfair' contractual arrangements," 70(2), May 1980, pp. 356-362.

Bailey, Elizabeth E. "Contestability and the design of regulatory and antitrust policy," 71(2), May 1981, pp. 178-183.

Stigler, George J. "The economists and the problem of monopoly," 72(2), May 1982, pp. 1-11.

(2) Inconsistent with Chicago School positions

Rostow, W. W. "A historian's perspective on modern economic theory," 42(2), May 1952, pp. 16-29.

Kaysen, Carl "Dynamic aspects of oligopoly price theory," 42(2), May 1952, pp. 198-210.

Galbraith, John Kenneth "Countervailing power," 44(2), May 1954, pp. 1-6.

Stigler, George J. "The economist plays with blocs," 44(2), May 1954, pp. 7-14.

Miller, John Perry "Competition and countervailing power: their roles in the American economy," 44(2), May 1954, pp. 15-25.

Harberger, Arnold C. "Monopoly and resource allocation," 44(2), May 1954, pp. 77-87.

Heflebower, Richard B. "Toward a theory of industrial markets and prices," 44(2), May 1954, pp. 121-139.

Clark, J. M. "Competition: static models and dynamic aspects," 45(2), May 1955, pp. 450-462.

Weintraub, Sidney "Revised doctrines of competition," 45(2), May 1955, pp. 463-479.

Whitney, Simon N. "Vertical disintegration in the motion picture industry," 45(2), May 1955, pp. 491-498.

McKie, James W. "The decline of monopoly in the metal container industry," 45(2), May 1955, pp. 499-508.

Marengo, Louis "The basing point decisions and the steel industry," 45(2), May 1955, pp. 509-522.

Penrose, Edith "Limits to the growth and size of firms," 45(2), May 1955, pp. 531-543.

Blair, John "Economic concentration and depression price rigidity," 45(2), May 1955, pp. 566-582.

Mason, Edward S. "Market power and business conduct: some comments," 46(2), May 1956, pp. 471-481.

Brewster, Kingman Jr. "Enforceable competition: unruly reason or reasonable rules?," 46(2), May 1956, pp. 482-489.

Adelman, M. A. "The 'product' and 'price' in distribution," 47(2), May 1957, pp. 266-273.

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