Organizing Industrial Organization: Reflections on the Handbook of Industrial Organization

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Organizing Industrial Organization: Reflections on the Handbook of Industrial Organization

Turning and turning in the widening gyre
The falcon cannot hear the falconer;
Things fall apart; the centre cannot hold;
Mere anarchy is loosed upon the world.
W. B. Yeats, ''The Second Coming''

Bliss was it in that dawn to be alive,
But to be young was very heaven!
William Wordsworth, ''French Revolution''

PARTS 2 AND 3 of the *Handbook of Industrial Organization* are respectively entitled ''Analysis of Market Behavior'' and ''Empirical Methods and Results.''[1] The first section is almost exclusively theoretical, whereas the second, as its title makes clear, is empirically oriented. Both sections deal with the analysis of markets, particularly with oligopolistic ones.[2]

This paper is dedicated to Carl Kaysen on the occasion of his seventieth birthday.

1. All references to the *Handbook* are to Schmalensee and Willig (1989). In general, these references are to parts 2 and 3.

2. A review of the remaining sections of the *Handbook*—''Determinants of Firm and Market Organization'' (part 1), ''International Issues and Comparisons'' (part 4), and ''Government Intervention in the Marketplace'' (part 5)—is in the paper in this volume by Alvin Klevorick.

Reflection on the *Handbook* has two aspects. First, is the *Handbook* a good book—that is, does it succeed in its stated aims? The second suggests a broader and more important set of questions. Reading the *Handbook* provides the opportunity for thinking about the state of the art, about the field of industrial organization. What does it include? What are the organizing principles? In what direction is the field growing? Is that the correct destination?

This latter set of questions is the subject of most of this paper. But the first question also deserves attention, and the two are not unrelated.

The *Handbook* as a Book

In considering the *Handbook* as a book (and in later assessing the state of the art) I necessarily paint with broad strokes. The *Handbook* is immense, and a detailed review of its chapters would be tedious, if not impossible. As a result, there are exceptions to many of my general comments, and especially to my criticisms, and I hope authors of particular chapters will forgive me for not pointing them out.

This said, my first reaction is enthusiastic praise. The *Handbook* is a very good book. Every chapter is well written and a mine of information. Most of them are far more than surveys of the state of the art in a particular area. They are coherent essays that themselves add to the art. But I do not suggest that one should sit down to read the *Handbook* straight through. It is not intended for cover-to-cover reading (except by exhausted reviewers). I can do no better in this regard than to quote advice given in the introduction to a recent collection of political jokes:

> One final word of advice to any prospective reader of this volume: *Do not read it*! If you try to follow the King's instructions to the White Rabbit in *Alice in Wonderland*—"Begin at the beginning, and go on till you come to the end: then stop,"—you will very soon become sated and overcome first with a numbed indifference and then with nausea (as with a box of chocolates—some sweet, some bitter, some hard- and some soft-centered). We advise, rather, judicious sampling.[3]

3. Lukes and Galnoor (1987; p. xiii).

Rather, the work is intended to be exactly what it says it is: a handbook, a reference whose purpose, as stated by its editors, is "to provide reasonably comprehensive and up-to-date surveys of recent developments and the state of knowledge in the major areas of research . . . as of the latter part of the 1980s, written at a level suitable for use by nonspecialist economists and students in advanced graduate courses."[4]

Is the *Handbook* successful in achieving this goal? I think only partially so. In the first place, particularly in the theoretical chapters of part 2, the nonspecialist will often find the going heavy, even though the necessary tools have been provided. (Part 2 begins sensibly with chapter 5, Drew Fudenberg and Jean Tirole's overview of the methods and results of noncooperative game theory.) The authors of chapters in part 2 sometimes succumb to the temptation to deal with their own most recent, sometimes unpublished work (and perhaps that of their students and friends). This is not necessarily a bad thing—after all, the authors were chosen because of their work in their respective subject areas. But the urge to describe all the latest wrinkles occasionally tells the reader more than he or she may want to know, and one comes away from such discussions without a clear sense that the literature has been systematically surveyed.

That is less so in the empirical chapters of part 3, but here a different problem arises. It is difficult to write a survey of a large set of empirical studies. For one thing, the material is typically less easy to organize than is the case with a theoretical theme. For another, empirical studies vary vastly in quality. It is not easy to describe both what is known and the degree of certainty with which we know it. Here, Richard Schmalensee (chapter 16) and Wesley Cohen and Richard Levin (chapter 18) have the daunting task of dealing with cross-industry studies. They do a good job of organizing their respective topics but are less successful in providing a detailed, critical guide to the relevant literature. Because, as we shall see, the studies surveyed are open to considerable theoretical objection, it is of particular importance to single out which studies and which conclusions are solidly based. Although both chapters (especially Schmalensee's) do address the underlying problems, they fail in the perhaps impossible task of carefully separating good, soundly based studies from more questionable ones. Instead the

4. Schmalensee and Willig (1989, vol.1, p. xi).

reader gets the author's own (and doubtless often correct) impressions about what the literature shows.

It is also often (but not always) the case that the authors of the theoretical chapters in part 2 have only a general idea about the results of the empirical work presented in part 3. This, however, reflects a deeper problem in the field itself, and I shall discuss it later.

In this connection, Dennis Carlton's essay (chapter 15) on the theory and facts of market clearing stands in sharp contrast to most of the chapters of part 2.[5] Carlton considers the actual facts on such things as price changes and delivery lags and shows that simple theories cannot explain them. His chapter is a welcome blend of theory and fact. In level and tone, it comes far closer than most of the other chapters of part 2 to meeting the purpose cited by the editors.

The other theoretical chapters of part 2, as indicated, spend little time on systematic examination of empirical results. Either they pay little attention to empirical work, or they resort to casual observation. Only occasionally, as in Janusz Ordover and Garth Saloner's excellent piece on predation, monopolization, and antitrust (chapter 9), does one find a real attempt to apply theory to the detailed facts of particular industries.

This brings me to the subject of work in the field that is not well surveyed in the *Handbook*. For a very long time now, a good deal of effort has been spent on detailed industry studies. Such studies—of varying quality and analytic content, to be sure—can provide the basic information from which theory can generalize. I do not know to what extent such work is still common (although I certainly know that it still goes on). More important, I cannot tell much, if anything, about it from the *Handbook*, and this is a gap in coverage.

I realize, of course, that such work is troublesome to survey in a systematic way. Each industry study tends to be idiosyncratic, with organizing principles linking such studies difficult to find. (As we shall see, I do not believe that this is an accident.) But the *Handbook* fails to make the attempt, although some individual studies are mentioned in passing. It is symptomatic of the *Handbook* (and of the profession) that the closest one comes to a survey of work on particular industries

5. The contrast is sharp enough to be jarring. One wonders why Carlton's chapter appears in part 2 at all.

is Timothy Bresnahan's essay on empirical studies of industries with market power (chapter 17). That chapter, excellent and interesting in itself, is focused on work that uses a particular set of techniques; it does not pretend to survey the wider field.

The second area that is not systematically surveyed is related to the first: public policy. Issues of antitrust policy are discussed in several of the chapters—for example, the essay by Ordover and Saloner, already mentioned; that by Hal Varian on price discrimination (chapter 10); and the one by Michael Katz on vertical contractual relations (chapter 11). But the *Handbook* makes no separate, concerted attempt to tie together economic and legal thinking about public policy on market power and the related issues. Because much of the practical use of industrial organization comes in antitrust cases, which also supply the occasion for substantial work on particular industries, this is an unfortunate omission.

These two omissions, industry studies and antitrust-related matters, are also troublesome because of the opportunity that a systematic survey (were one possible) might allow one to see theory in action. The authors of the theoretical chapters of part 2 obviously believe that theory provides a rich set of tools for application when studying particular industries. Thus, Carl Shapiro states, after an extensive discussion of theories of oligopolistic behavior,

> Let me close with a sort of user's guide to the many oligopoly models I have discussed. By "user," I mean one who is attempting to use these models to better understand a given industry (not someone out to build yet another model). Here is where the "bag of tools" analogy applies. After learning the basic facts about an industry, the analyst with a working understanding of oligopoly theory should be able to use these tools to identify the main strategic aspects present in that industry.[6]

Had the *Handbook* successfully surveyed industry studies and analyses of particular antitrust cases, it might have been instructive to see how those tools have been used or *might* have been used. As we shall see, however, I suspect that such an exploration would have revealed that views such as Shapiro's cited above, are far too sanguine about the usefulness of theory in its present state. There is a serious (and

6. Schmalensee and Willig (1989, vol. 1, p. 409).

unhappy) gap between theory as revealed in the *Handbook* and the actual analysis of real industries and real antitrust cases. The existence of that gap is by no means the exclusive fault of the theorists. Facts analyzed by means of poorly understood theory are just as big a problem as theory misapplied to poorly understood facts. A survey of the use of industrial organization in antitrust actions could have revealed the problems involved on both sides and might have better indicated which theoretical developments seem promising in practice.

No such survey is provided, however, and the *Handbook* may leave the erroneous impression that economists' expertise is not often called on in antitrust cases. Of course this is far from the truth. Economic expertise is called on all the time. But the tools used in such cases are in general not those of the type of theory that dominates the *Handbook*. I shall return to these matters below.

Before leaving my discussion of the *Handbook* as a book and moving on to the broader question of what it reveals about the state of the art, I must mention a minor matter. The proofreading and copyediting of the *Handbook* are a disgrace. Names are misspelled; sentences are often ungrammatical; cross-references to other chapters are incorrect, and, although meaning is seldom totally obscured, one occasionally has to think about what the author must have meant to say.

Three examples will suffice here. Bresnahan refers to a "higher or at least higherfaulting theoretical language." He also states that he will "mention a consistent notation throughout, rather than adopting the notation of individual papers." But the greatest of all such quotes comes from Stiglitz, who says of the Walrasian auctioneer that "no one probably took the tantamount process seriously."[7] I single out these two authors only because the slips are amusing. The level of care here is consistently low, and I suspect that the authors were not given the opportunity to proofread their own papers.

Having made these criticisms, however, I want again to emphasize that my principal reaction is quite favorable. I found every chapter educational (which is not to say that I had no substantive disagreements with the authors). This is a book of which authors and editors should be proud.

7. Schmalensee and Willig (1989, vol.2, pp. 1020; 1015n; vol. 1, p. 773n).

Organizing Principles of Industrial Organization

I turn now to the more difficult, but rather more important, task of considering the state of the art as exemplified in parts 2 and 3 of the *Handbook*. This is not easy to do, for the writing of a systematic essay requires that one find organizing themes. In this regard, the very explosion of material reflected in the *Handbook* is daunting.

After considerable thought, I have decided to proceed in the manner of some authors of the *Handbook*'s chapters. Schmalensee (chapter 16), for example, organizes his summary in terms of a series of "stylized facts." Similarly, Eaton and Lipsey begin their essay on product differentiation with a list of seven "awkward facts that are available to constrain theorizing."[8] Because this review is empirical to the extent that it reports and summarizes the field as seen through the *Handbook*, I shall proceed in similar fashion with a series of "organizing principles."[9]

ORGANIZING PRINCIPLE 1: *Industrial organization has no organizing principles (except for those that are subcases of this one).*

This is no joke. As we shall see, I believe that there are deep reasons for such a lack, and it manifests itself in several different ways. I shall begin with pure theory.

ORGANIZING PRINCIPLE 2: *The principal result of theory is to show that nearly anything can happen.*

The principal mode of theorizing in industrial organization is the creation of interesting examples in which problems are stripped of all but their most essential features. The result is, in effect, a formalized anecdote in which the theorist demonstrates that certain outcomes can actually occur—sometimes contrary to what one might have thought.

8. Schmalensee and Willig (1989, vol.1, p. 725).

9. I trust that I will be forgiven for emulating some of the authors of the *Handbook* in a different way and for referring to my own work a bit too frequently. That, too, as I have mentioned, is characteristic of the field. The views expressed here are consonant with those in Fisher (1989)—an article whose publication certainly contributed to my being asked to write this review.

This sort of theory is what I have elsewhere called "exemplifying theory."[10] It is a powerful method for producing counterexamples to general propositions. Further, it *may* lead to insights about phenomena that can also be found in more general and complex situations. But the result does not appear to be leading to any "generalizing theory" or, indeed, to a theory with much real content in the sense of being suited to empirical verification or rejection.[11] Rather, the method has produced a taxonomy—a laundry list of a vast number of possibilities that rules out little.

This fact has not escaped the attention of some authors of the *Handbook*. Jacquemin and Slade state in their essay on cartels, collusion, and horizontal merger that

> Economic thought concerning collusive practices and mergers has changed profoundly, mainly in the light of game-theoretic analysis. Unfortunately, this change has not led to more general and robust conclusions. On the contrary, it is the source of a more fragmented view. The diversity of models and results, which are very sensitive to the assumption selected, suggests a "case-by-case" approach where insight into the ways in which firms acquire and maintain positions of market power becomes essential. It is nevertheless important to bring to light a *typology* of situations and practices for which recent developments in economic analysis offer sounder theoretical characterizations than in the past.[12]

They later say, "The multiplicity of equilibria is one of the problems associated with the repeated-game approach. Instead of providing us with a theory of oligopoly, it can explain all possible behaviors."[13]

Gilbert states in his essay on mobility barriers and the value of incumbency that the "scope for oligopolistic interactions is so wide that a predictive model of how firms behave may be no easier to construct than a model of the weather based on the formation of water droplets." He refers to a "taxonomy of behavior in response to entry."[14]

This situation is not the fault of the theorists. The theoretical facts are as they have recited them, and the possible outcomes are extremely

10. Fisher (1989).
11. Fisher (1989).
12. Schmalensee and Willig (1989, vol.1, p. 416, emphasis added).
13. Schmalensee and Willig (1989, vol.1, p. 441).
14. Schmalensee and Willig (1989, vol.1, pp. 478, 509).

numerous and assumption-dependent. Further, the Folk theorem for repeated games assures us that, with low enough discount rates, this phenomenon is endemic in any situation of serious interest.[15] One must not blame the messenger for the bad news (although one can be skeptical about how surprising the news really is). Yet one can reasonably question whether theorists are working on a useful research agenda. We now know that no general results will emerge that map the route by which simple facts about market structure become performance outcomes.

ORGANIZING PRINCIPLE 3: *Stripped-down models of theory often fail to provide helpful guides for the analysis of real situations.*

The problem is that real firms operate in a far more complex world than is captured by theory in its present exemplifying state. Real firms do not set only quantity or price. They set a complex variety of strategic variables and frequently offer multiple products in multiple locations. Contrary to the optimistic view expressed in Shapiro's "bag of tools," the analyst working on a particular industry will often not be able to decide what tools apply (if any do).[16]

Quotations from the *Handbook* are illuminating here. Fudenberg and Tirole state that "Firms typically do not only choose a time to enter a market, but also decide on the scale of entry, the type of product to produce, etc. This *detail* can prove unmanageable, which is why industrial organization economists have frequently abstracted it away."[17] Jacquemin and Slade state that

> In all of these models, price wars are equilibrium strategies of supergames; no one ever cheats. This is perhaps [!] a shortcoming of the models from a practical if not from a game-theoretic point of view. Our intuitive feeling is that firms do intentionally cheat on collusive agreements (recall the electrical-equipment conspiracy) and that there are many reasons why price wars occur in addition to demand shocks. Nevertheless,

15. An outcome of a game is called "individually rational" if it gives each player at least as much as the minimum amount the player could secure for himself or herself. The Folk theorem states that if discount rates are low enough, then any outcome in an infinitely repeated game that is individually rational is supportable as a Nash equilibrium. See Fudenberg and Tirole's discussion in chapter 5 of the *Handbook* (pp. 279–81).

16. Schmalensee and Willig (1989, vol.1, p. 409).

17. Schmalensee and Willig (1989, vol.1, p. 292, emphasis added).

economists have devised few theories to explain cheating in collusive agreements.[18]

Reinganum states in her essay on the timing of innovation that

> One important goal of future research should be to develop testable models of industry equilibrium behavior. The papers summarized here have used stark models in order to identify the significant characteristics of firms, markets and innovations which are likely to affect incentives to invest and/or adopt [innovations]. But since it is largely restricted to . . . special cases . . ., this work has not yet had a significant impact on the applied literature in industrial organization; its usefulness for policy purposes should also be considered limited. For these purposes, one needs a predictive model which encompasses the full range of firm, industry and innovation characteristics.[19]

Cohen and Levin, writing on empirical studies of innovation and market structure, agree with this, although they are certainly not wholly pessimistic:

> One difficulty with testing the implications of recent game-theoretic models of R&D [research and development] rivalry is that they analyze behavior in highly stylized and counterfactual settings. . . . Moreover, many of the results obtained . . . depend on typically unverifiable assumptions concerning the distribution of information, the identity of the decision variables, and the sequence of moves. Nonetheless, empirical effort on the effect and importance of strategic behavior is warranted. Inspiration might be drawn from Lieberman's (1987) empirical examination of the role of strategic entry deterrence in affecting capacity expansion in a sample of chemical and metals industries. He concluded that strategic considerations were not paramount in most industries, but he identified several specific instances in which strategic considerations *may* have been important.[20]

In something of the same vein, Ordover and Saloner state that

> Theoretical findings and prescriptions are difficult to translate into workable and enforceable standards that in *actual market settings* would, without fail, promote conduct that enhances social welfare and would, without fail, promote conduct that harms welfare. The source of the

18. Schmalensee and Willig (1989, vol.1, p. 447).
19. Schmalensee and Willig (1989, vol.1, p. 905).
20. Schmalensee and Willig (1989, vol.2, p. 1096, emphasis added).

> problem is the strategic setting itself. In the context of strategic interactions, it is difficult to distinguish between those actions, which are intended to harm actual (and potential) rivals[,] that stifle competition, and thereby reduce economic welfare, and those actions which harm present rivals and discourage future entry but which, nevertheless, promote economic welfare. Or, as legal scholars are often fond of saying, actions which are consistent with "competition on the merits."[21]

Stripped-down models can, in fact, be very useful, but, as Eaton and Lipsey observe in their essay on product differentiation "Tractability in deriving incorrect results is no advantage."[22] For "incorrect," read "inapplicable." Industrial organization theory has a long and arduous way to go.

ORGANIZING PRINCIPLE 4: *Some (by no means all) theorists have a casual attitude toward what constitutes verification.*

With a bewildering variety of possible models to choose from, one can reasonably ask what could constitute the verification or falsification of a particular model. Here there is sometimes an underlying attitude that a theory has been "successful" or "applicable" if one can use it to tell a logically consistent story of what *might* have happened—a story consistent with the few facts that the theorist happens to know.

The excerpt from Cohen and Levin (chapter 18) given above is one illustration. Others can be found in the very casual citation of certain antitrust cases by some authors.[23] Thus, to take an example that I know well, *Telex* v. *IBM* is cited by Gilbert as providing an example of contracts and entry prevention. But this case does so only in terms of the plaintiff's allegations. It is cited again for the effects of "locked-in" customers in producing alleged price discrimination.[24] Here the allegation made no economic sense, and the principal so-called "lock-in" part of the case was not the one cited. These points are not hard to find.[25]

21. Schmalensee and Willig (1989, vol.1, p. 538–39, emphasis in original).
22. Schmalensee and Willig (1989, vol.1, p. 759).
23. This is definitely *not* to say that all authors of the *Handbook* are casual in this regard. Ordover and Saloner (chapter 9), for example, have clearly read the literature on the cases they cite.
24. Schmalensee and Willig (1989, vol.1, pp. 502n, 507n).
25. See Fisher, McGowan, and Greenwood (1983, pp. 196–204, 316–17, and 325–28.

To continue with the computer industry, Gilbert writes that

> Despite its theoretical limitations, the Gaskins model of dynamic limit pricing (along with its refinements) is an appealing description of pricing behavior for industries that are characterized by dominant firms. The exogenous specification of the entry flow is not theoretically justified, but it *may* capture an important element of dynamic competition. . . . If it *were* possible to model [certain underlying] these aspects of the entry process, the result *could* be an entry flow rate that appears similar to the . . . Gaskins model. . . . For these reasons, it is not surprising that the Gaskins model has been used *successfully* in empirical models of dominant firm pricing, such as . . . Brock (1975).[26]

The issue, of course, is what constitutes "success." I suggest that a serious knowledge of the complexities of the computer industry does not lead one to believe that this is the best example, however appealing it may seem for its relative simplicity.

Similarly, the notion that merger policy should be made on the assumption that real firms follow Cournot behavior is naive, if not bizarre.[27] That theorists can produce a simplified model with clean results does not mean that the world works in that way. Further, the idea that the cross-section empirical studies surveyed in part 3 of the *Handbook* somehow verify simplistic theory is simply wrong. The difficulties with such studies (perhaps especially with the use of accounting profitability) do not appear to be fully appreciated by the theorists in part 2.[28] I now turn to such empirical work.

ORGANIZING PRINCIPLE 5: *Much empirical work, especially cross-industry empirical work, is not informed by (or, sometimes, about) theory.*

The years of drought in industrial organization theory were years in which the cross-section farmers went on planting. Not surprisingly, the harvest was not bountiful, and the recent flood of theory has not irrigated the crops.

Cross-sectional attempts to verify (or disprove?) the structure-

26. Schmalensee and Willig (1989, vol.1, pp. 514–15, emphasis added).
27. Farrell and Shapiro (1990).
28. See, for example, pp. 437, 449, and 455 of the *Handbook*; and Shapiro (1989, p. 133).

conduct-performance paradigm have never been very soundly based in theory. Not only has theory not provided much quantitatively useful guidance about exactly how structure affects performance, even at the level of what variables should be used, but also the empirical practitioners often had only a rudimentary understanding of what theory did say.

An outstanding, but not the only, example of this came in the area of capital theory, where inability to move beyond the simplest one-period model was striking indeed. To be more specific, attempts to use profitability as the basic measure of performance simply misunderstood both the role and the measurement of profitability in economic theory.

In the first place, it is not true that there are no economic profits earned in competition. Profits are the driving force of the competitive process. Only in long-run equilibrium are profits (adjusted for risk) driven to zero. It is a great mistake—and one that consistently runs throughout economics—to behave as though all that matters is long-run equilibrium. Competition is a dynamic process; real firms operate in real time, and the fact that economists find it difficult to deal with such dynamics does not make the dynamics go away.

Put this aside, however, and suppose that comparison of a firm or industry's profitability to some ''normal'' standard is an appropriate way to test for market power. What profitability measure should be used? To the extent that it is appropriate to speak in terms of profit rates at all (as opposed to present values discounted at some suitable rate of return), economic theory teaches that the risk-adjusted profit rate that is equalized under competition is the internal or economic rate of return—the rate that makes the present value of the stream of returns from investment equal to the direct capital costs.

The profitability rate used in cross-section studies is not of this (admittedly hard to measure) magnitude. Rather, many studies have used the accounting rate of return (profits divided by stockholders' equity or by the value of capital stock). Because capital stock purchased now is done so with an eye to future profits, and because current profits are earned in part because of investments made in the past, it should come as no surprise that such measures do not carry a great deal of information about the economic rate of return. (Indeed, the remarkable fact is that there should exist *any* circumstances under which the two are closely related.) Nevertheless, despite others' having made similar points in the past, this fact did cause a great deal of surprise (not to say outraged protest) when John McGowan and I pointed it out some years ago.[29]

A similar problem infects studies using a different profitability measure, the profits-sales ratio. Even making quite favorable assumptions, it turns out that this quantity does not equal (or possibly even approximate) the Lerner measure of monopoly power (price minus marginal cost, all divided by price) except under *very* special circumstances.[30]

These are not difficult results to derive from the theory of the firm. Yet at least one leading practitioner seems to have been wholly unaware that the economic rate of return was of any importance.[31] Others simply found it difficult to believe that they were measuring the wrong thing.

Schmalensee (chapter 16), who understands the issues involved, attempts to get round them by surveying the literature as providing stylized facts rather than solid results. Those ''stylized facts'' often concern accounting profitability, and industrial organization theory *may* need to explain them. But one must not yield to the temptation to suppose that the explanation is that the magnitudes studied in empirical work are necessarily closely allied to those that are the objects of theory.

The field has recently moved on a bit. Focus has shifted from profits to prices as measuring performance.[32] The shift to prices has its own serious measurement problems, but the difficulty in this area is not merely one of measurement.[33] Theory does not provide—perhaps theory *cannot* provide—a clean, detailed model that goes from measurable aspects of structure to performance, whether performance is measured by profits or by prices. The Folk theorem and the wealth of exemplifying theory show that market equilibria depend on a host of underlying, often unobservable factors. Further, equilibria are not all that matter in the constantly changing world in which real firms and real industries operate. In the absence of a suitably informing general theory, I do not

29. Fisher and McGowan (1983); see also Long and Ravenscraft (1984), and Fisher (1984).

30. Fisher (1987a).

31. Fisher, McGowan, and Greenwood (1983, p. 257).

32. Weiss (1989).

33. Comparison of the prices charged by different firms requires that the goods being priced be (or be made to be) comparable. Even in apparently simple cases, this may not be easy because goods carry such attributes as service, promptness, ease of dealing, and general reputation of the firm. That these attributes can make a substantial difference has been forcefully pointed out by Newmark (1989).

believe it useful to go on with empirical studies that crudely apply the relatively rudimentary theory of the past to measures that are not the objects that theory discusses.

Somewhat similar (if less pervasive) problems arise in the empirical literature on innovation and returns to scale. Here Peter Temin and I long ago pointed out that the theory of the firm does not yield an unambiguous prediction about the effects of firm size on R&D in the presence of economies of scale.[34] That result holds both for R&D input and R&D output. Yet the literature keeps on growing.

Cohen and Levin's treatment of this issue in their survey of empirical studies of innovation and market structure (chapter 18) is perhaps indicative of the impatience that empirical workers feel with such demonstrations. They state that

> [Fisher and Temin] demonstrated, *among other things*, that an elasticity of R&D [input] with respect to size in excess of one does not necessarily imply an elasticity of innovative output with respect to size greater than one. Kohn and Scott . . . established the conditions under which the existence of the former relationship does imply the latter.[35]

They then go on to what they consider the "more fundamental" problem stemming from the argument that "Schumpeter did not postulate a continuous effect of firm size on innovation."

The point is that the proposition about the relations between the two elasticities is a relatively minor one. Among the "other things" that Temin and I demonstrated was that the literature was not actually testing (and probably was not able to test) *any* of the propositions that it purported to examine. Apparently that finding didn't stop anybody.

As in the case of the use of profits as a performance measure, more theory is needed. That theory should not concentrate on showing that under some circumstances the standard empirical approaches are correct. Rather, it should illuminate what variables must be measured to restore the possibility of getting an answer. Unlike the use of profit rates to measure performance, I think there may be some hope here.

The picture I have painted of careless disregard for theory by empirical workers is, of course, too general to be totally accurate. In at

34. See Fisher and Temin (1973, 1979); Rodriguez (1979); and Kohn and Scott (1982).
35. Schmalensee and Willig (1989, vol.2, p. 1071, emphasis added).

least one area, moreover, it is certainly not correct. Bresnahan's essay (chapter 17) reports on econometric studies of particular industries that were undertaken to test whether those industries behave competitively and to measure market power. This literature recognizes that ''firms' price-cost margins [cannot be] taken to be observables [because] economic marginal cost . . . cannot be directly or straightforwardly observed.'' At least as important is that

> Individual industries are taken to have important idiosyncrasies. It is likely that institutional detail at the industry level will affect firms' conduct, and even more likely that it will affect the analyst's measurement strategy. Thus, practitioners in this literature are skeptical of using the comparative statics of variations across industries or markets as revealing anything except when the markets are closely related.[36]

This literature stands out from most of the empirical work surveyed in the *Handbook* in that it certainly does use theory. The theory it uses is not closely related to that of the game-theoretic analyses in part 2, however, but harks back to the earlier literature on conjectural variations. It is not much of an exaggeration to say that the theory involved is much the same as was used by Iwata in his early, important paper in this area.[37]

Further, although some progress has been made in the detection of market power, Bresnahan states that

> Only a very little has been learned from the new methods about the relationship between market power and industrial structure. . . . We know essentially nothing about the causes, or even the systematic predictors of market power, but have come a long way in working out how to measure them.[38]

Maybe so, but I am more skeptical than Bresnahan about our ability to measure market power (or even to know what the right measure is). The work in this area seems most successful in determining whether an industry is in competitive equilibrium. As Bresnahan suggests, it is less convincing in its attempts to locate the sources of departure from competition.

36. Schmalensee and Willig (1989, vol.2, p. 1012).
37. Iwata (1974).
38. Schmalensee and Willig (1989, vol.2, pp. 1053, 1055).

But knowing whether or not an industry is in competitive equilibrium is not usually a remarkably interesting thing to know. Most industries most of the time are not characterized by perfect competition, let alone by perfectly competitive equilibrium. The issues of interest typically involve the question of what, if anything, can be done to make an industry more competitive, with the recognition that perfect competition is an unattainable goal.

On this point, as already suggested, the literature surveyed by Bresnahan does not seem helpful. That literature does not appear usually to estimate structural equations; rather, the typical piece sets forth a structural model and derives some quasi-reduced-form implications. Even the conclusions drawn from the estimation of these, I suspect, tend to be heavily dependent on the functional forms used.

Despite such problems, the work surveyed by Bresnahan is miles ahead of much of the field in its use of theory. As I have already emphasized, there *is* no adequate theory on which to base the cross-section empirical work. That has always been true, but it is important to realize that recent developments have not provided the missing foundation. We still have no theory on which to base a *structural* model of the structure-conduct-performance paradigm. That Schmalensee's survey (chapter 16) is reduced to listing stylized facts is a reflection of this lack. The listing of thirty such stylized facts, moreover, makes one wonder whether this literature has turned up much that is really systematic.

In short, there can be no doubt that empirical attempts to verify, test, or estimate the parameters of the relations between structure and performance have not succeeded. Even if the general empirical literature is taken on its own grounds and the kinds of analytic defects pointed out above are ignored, most results can be said to be uncertain and ambiguous. Further, the explosion in theory is having no effect. The empirical literature makes essentially no use of the modern methods or results, which is hardly surprising because theory is not providing propositions that are testable in practice (Organizing Principle 3).

A Research Agenda

The failure of the empirical literature is no accident. In one (not helpful) sense, that literature does indeed confirm a principal result of theory in this area: nearly anything can happen (Organizing Principle 2). There *is* no simple mapping from elementary (let alone imperfect) measures of structure, such as concentration or firm size, to performance. Those models (such as the simplest Cournot models) that suggest there is arrive at that result by stripping the problem of features essential to the understanding of real industries (Organizing Principle 3). Hence the empirical finding that such relationships are ambiguous does indeed verify the prediction of theory (although not in a helpful way).

In short, the structure-conduct-performance paradigm is dead, *if* (and this is a big if) one thinks of it as relating simple structural measures to characteristics of conduct and performance. The theoretical counterpart is that the program of investigating how perfectly rational opponents will behave in overly simplified settings has also failed (or, if you wish, has succeeded too far). Despite outward appearances, the field of industrial organization is not in a happy state, at least as regards the analysis of oligopolistic markets and related subjects.

But this conclusion rests on a somewhat limited view of what the appropriate research agenda for industrial organization really is. The failures just described come as little surprise to those who carefully read Fellner's *Competition among the Few* or have worked extensively on industry studies.[39] The simple-structure-measures–rational-behavior model does not lead to very useful results because the context of particular industries in which firms operate strongly affects the outcome they will or can achieve.

I give the simplest example. In an infinitely repeated game (with low enough discounting), the cooperative (joint-profit-maximizing) outcome is typically a Nash equilibrium independent of the number of firms or of industry concentration. Yet no sensible person supposes that such an outcome is just as likely when there are a thousand firms of equal size as it is when there are two. In this sense, current theory provides neither a guide nor a justification for studies that attempt to measure the effect of concentration or numbers on outcomes.

Yet such an attempt is not thereby rendered senseless. We think that the two cases just described differ, not because the Nash equilibria are fundamentally different in the two cases but because the two-firm industry will somehow find it easier to achieve the cooperative outcome

39. Fellner (1949).

than will the thousand-firm one. Further, we can all give at least verbal reasons why that is true. If numbers and concentration were all that mattered to such ability, then empirical studies attempting to relate performance (properly measured) to numbers and concentration would be successful despite the Folk theorem.[40]

The difficulty, of course, is that numbers and concentration are not all that matter. A great many other things are likely also to be important. As Carlton states in his essay on how markets clear,

> Much of industrial organization seems fixated on answering how the behavior of markets differs as industry concentration changes. Although this is certainly an interesting question, industry concentration is only one of many ways in which markets can differ. Market liquidity, hetertogeneity of product, variability in demand and supply, the ability to hold inventories, and the ability to plan are also interesting characteristics, and differences in these characteristics lead to different market behavior. Yet the effect of these other characteristics has received much less attention from industrial organization economists than the effect of differences in industry concentration.[41]

Further, once one leaves the question of market clearing, the list of interesting characteristics gets longer still. But empirical studies pay little attention to this, and theory has managed mostly to verify that the list is long.

I believe that the proper research agenda for industrial organization is the study of how the context of particular industries or market situations determines *which* equilibrium will be reached and what happens on the way. In particular, we need to study how context affects the ability to achieve the joint-profit-maximizing outcome. This is not what most of current theory is doing. Further, as I have elsewhere explained in detail, I do not believe that the theoretical tools now so popular are particularly well suited for that task.[42]

In the absence of strong guidance from theory, we need to know what happens *in fact*. This surely requires the *detailed* study of particular industries. The cross-section literature is too simplistic to be of much

40. Further, merger policy that relies on such measures would be entirely sensible. On this point, see Fisher (1987b).

41. Schmalensee and Willig (1989, vol.1, p. 911).

42. Fisher (1989).

assistance here, and the somewhat casual attitude of some theorists toward empirical verification (Organizing Principle 4) is of no help at all. (The econometric literature surveyed by Bresnahan in chapter 17 of the *Handbook* is at least potentially useful in this regard, but it too suffers from a lack of richly articulated structural variables adequate to describe the underlying context.)

It is always dangerous, of course, to jump into empirical description without any guidance from theory, but it would be wrong to suppose that we do not have any such guidance. We do know in general (but only generally) what can matter. The problem is that we have known that for more than forty years. What we need to know now is what aspects of the contextual setting matter in practice.

This may be where experimental methods come in. Plott, in his review of the applications of experimental methods in industrial organization (chapter 19), lists several cogent reasons for the use of such methods.[43] He does not explicitly mention the possibility that, by carefully controlling the context in which marketlike games are played, one can gain insight into what aspects of context are likely really to matter in nonexperimental situations. But that possibility comes across from his survey.

Relating to Antitrust

Plott ends his survey by contemplating that experimental data might be used in court in antitrust cases.[44] At least for the present, that seems to me to be utter fantasy, but it is instructive to consider the extent to which *any* modern developments—especially game-theoretic developments—illuminate the issues in antitrust cases. As I have already suggested, the answer is "not much," and this is a depressing comment on the state of the field.

Do not misunderstand me. Industrial organization analysis has much to contribute to the analysis of antitrust cases and policy. Indeed, it is an indispensable element. The question I am asking is whether the recent developments described in the *Handbook* have added to this usefulness.

43. Schmalensee and Willig (1989, vol.2, pp. 1165–69).
44. Schmalensee and Willig (1989, vol.2, pp. 1170–71).

To fix ideas, consider an antitrust case involving an oligopoly—in particular, a case in which defendant firms are charged with anticompetitive behavior and collusion. In such a case (as in most areas of antitrust), analysis typically begins with a consideration of market definition—the question of what are the products and services that must be considered.

I have elsewhere pointed out that the question of the definition of the relevant market is not a truly well-posed one, and that the answer should serve only as a classificatory framework for analysis.[45] It is neither surprising nor unfortunate that most recent developments (and all modern theory) ignore this issue.[46]

The second aspect of the case is likely to involve the measurement of market share or concentration. As I have already observed, the empirical literature relating such measures to performance (and hence to competition or the lack of it) is not reliable. The theoretical work is nonexistent. Of course, this reflects the fact that there *are* no simple relations between structure and performance, but, again, what can be said now could have been said long ago.

An attempt may also be made to use profits as an indicator of market power. As discussed above, this has no analytic foundation. Current theoretical developments are (mercifully) silent here.

The last element of market structure that will typically be examined concerns barriers to entry. Here current analysis has more to say. But even here, what can be said about ''natural'' barriers to entry could be said years ago. Current theoretical developments do illuminate the analysis of ''artificial'' barriers, but this is a special case of their illumination of conduct issues, which I consider below.

These structural aspects of the antitrust case all lead to the question of whether departures from competition are possible or, perhaps, likely. They do not address the question of whether such departures have happened. The literature surveyed by Bresnahan (chapter 17) clearly provides a way of investigating precisely that point (although, as mentioned, it makes no use of current game-theoretic methods).

45. Fisher (1979, pp. 12–17); and Fisher, McGowan, and Greenwood (1983, pp. 31–33, 43–44).

46. As with other generalizations, this one is too broad. Jacquemin and Slade (chapter 7, pp. 454–55) survey some suggestions in this area. None of them relies on any development not available years ago.

Even if one can decide that perfectly competitive equilibrium does not characterize the defendants' behavior, one has not gotten very far. One must now examine conduct and decide whether conduct was anticompetitive.

Here, current theory is likely to have more to say. Particular conduct by the defendants can be analyzed in an attempt to decide whether it contributed to an anticompetitive result and whether it appears to have required agreement. Unfortunately, current theory will typically not provide a definitive answer. Instead, the concentration of theorists on providing examples of what *might* happen will come into play. Using a stripped-down, simplified model, an economist may testify for the plaintiff that certain forms of behavior *could* be anticompetitive. Another economist may very well testify for the defendants, explaining the neutral or pro-competitive aspects of the questioned behavior.

Does this mean that it will be impossible to choose between such explanations? Not at all, but doing so is likely to require examination of the detailed facts of the industry and firms involved—detailed examination of the context of the case. Modern theory, by merely showing that a variety of things *can* happen, is likely to stimulate plaintiffs' imagination. It can certainly be suggestive; it will almost never be definitive (Organizing Principle 3).

Further, focus now specifically on an allegation of collusion. In the case of collusion, one has to decide whether departures from competition can be explained only by collusion or whether the observed results could have occurred through oligopolistic rationality without agreement among the defendants.[47] Here, current theory is of no help whatever. Because the principal result of theory at present is (roughly) that anything can happen when rational players oppose each other (Organizing Principle 2), the only guidance that one is likely to get from this literature is that the outcome *could* be an equilibrium in a noncooperative game. That is no help at all.

Such a result is no help because it comes from asking the wrong question. The issue is not whether the outcome could be an equilibrium in a stylized noncooperative game. The issue, rather, is whether it is plausible to believe that the defendants could have achieved that particular equilibrium without explicit communication or some form of

47. Of course, this supposes that there is no direct evidence of collusion.

agreement. This has to do with the rich context in which the defendants operate. Such factors as the difficulty of detecting cheating or the number of variables that must be coordinated come crucially into play. But, although industrial organization economists can have a good deal to say about such matters, current developments have added little, if anything.

At least one principal aim of industrial organization should be to inform public policy toward, and court decisions about, competition or the lack thereof. In this respect progress—at least as revealed by the *Handbook*—has not been remarkably rapid (nor has it been absent). The field is marked by increasing technical sophistication, but that is not the only or the best way of measuring progress. As I have described in this section and the preceding one, the field appears to me to have lost sight of the basic question of how the context within which oligopolists operate determines which equilibrium will be reached and what happens on the way—in particular, the question of whether or to what extent oligopolists will achieve the joint-profit-maximizing solution without collusion.[48]

Concluding Remarks

Despite my favorable remarks on the *Handbook* itself, this essay no doubt conveys a somewhat negative tone. The reactions of some readers, however, suggest that I should be quite explicit about what my message actually is.

In the first place, I am not ''antitheory.'' Indeed, I take the view that theory can and should play an indispensable part in informing empirical analysis. Some of my criticism of empirical work in industrial organization stems precisely from the failure of that work to have a sound theoretical foundation.

Second, I am not even ''anti-exemplifying-theory.'' The stripped-down models often used can and do provide insights into what can

48. See also Fisher (1989).

happen. Moreover, they provide counterexamples to easy generalizations. Some of my own work has been of this nature.[49]

But appreciating such contributions does not imply satisfaction with the state of the art as regards either theory or empirical work. Theory and empirical work need to illuminate each other. To often, poorly understood scraps of empirical material are used by theorists to provide a suggestive context. Too often, poorly understood theory (and usually not very current theory) is used by empirical workers to reach a conclusion that is not soundly based.

The promise of great advances in industrial organization analysis is there, but that promise has not yet been realized. In its present state, theory does not provide much opportunity for verification or rejection, and often it is not phrased in terms of observable variables. Theorists need to study real industries in depth, and attention needs to shift away from the analysis of pure strategic interaction and toward the effects of context on behavior. Only then will theory be able to provide a model rich enough to serve as the underpinning for cross-industry studies.

Thus, even if one agrees with my comments on the state of the art, one need not be depressed about industrial organization at this juncture. The field has been undergoing a revolution. Even though that revolution has not produced results as exciting or relevant as some of the revolutionaries would have us believe, the revolution is not yet over.

The two poets quoted in the epigraphs to this paper give different accounts of what it is like to live in revolutionary times. The poem by Yeats describes the anarchy consequent on the destruction of an old order; that by Wordsworth describes the opportunity that such times create, especially for the young.

If attention can now be turned to the sort of agenda I have outlined—to the theory and empirical study of the effects of context on outcomes, to the analysis of models rich enough to capture the facts of real industrial situations—then the promise implied in the quote from Wordsworth can be achieved.

49. See, for example, Fisher (1985). It is not really correct, however, to suppose (as did Joseph Farrell at the conference) that my work on accounting rates of return (Fisher and McGowan, 1983) is exemplifying theory. That work proves some underlying theorems showing that the generalization in question (accounting rate of return equals economic rate of return) is true only under extremely restrictive circumstances. The examples serve to show that the proposition can easily be very far from the truth.

But that promise has not yet been achieved. Those who believe that it has (and who are inclined to dismiss my remarks as just those of an old geezer in training) might do well to reflect on the fact that the full title of Wordsworth's poem is "French Revolution, as It Appeared to Enthusiasts at Its Commencement." As I said earlier, industrial organization has a long way to go.[50]

50. Note: The general discussion following the paper by Alvin Klevorick contains the discussion on Franklin Fisher's paper as well.

Comments and Discussion

Comment by Timothy Bresnahan: I have three literary quotations. Only one is a poem, and I translated it myself.

> Now I've carefully studied dreary
> Law, Philosophy and Theory,
> I've read so much my eyes are sore.
> I know just what I knew before.[51]

This is Faust's opening speech in Goethe's *Faust*. I selected it, of course, because of the last two lines.

Professor Faust has studied the high thinking of his time—philosophy, theology, and all that—and doesn't know what to teach his students. Professor Fisher, ditto. He doesn't know what he has learned from all this theory. He knows just what he knew before.

In particular, almost forty years ago we knew two things about oligopoly. In general, anything can happen. What will happen in a given industry depends on its institutions in a detailed and subtle way. After all this theory, we still know this.

Professor Faust is talking himself into selling his soul to the devil. A devil is coming to Professor Franklin Fisher, as well. He is an inductive devil. The deductive paradigm of positive economics, particularly. We need to replace it with detailed industry case studies, which have information about the industries that does not necessarily appear in standard theories. Perhaps at the end we can let theory back in the door to explain the differences between the industries.

I am going to concentrate on Fisher's economic-science side.

51. The eighteenth-century German word *Theologie,* which has no modern equivalent, has been rendered as *theory*.

There is another quotation, 150 lines later in *Faust*: ''Two souls dwell, alas, in dieser Brust.'' The one soul in me wants to agree with much of Fisher's advice to economists, particularly the inductive part. I don't think that interesting industry case studies at book length were left out of the *Handbook*; I think we banished them from the academy.

There were once books about an industry or about the same business practice in several industries by authors who knew price theory—that is all they needed; they didn't need to know any game theory. When they didn't understand something, they said, ''Yes, I don't understand this'' and went on. This kind of book has largely disappeared. We see it from journalists. We see it from engineers. We see it from economists who disguise it as policy analysis—but we don't see it as part of economic science any more, except in regulatory economics. Fisher is absolutely right. More of that kind of work, alongside systematic statistical evidence, would be very valuable.

On the other hand, my other soul doesn't want to go down to earth; it wants to strive upward toward heaven. So I disagree with most of the line of argument that brought Fisher to his inductive conclusion.

There are not a lot of analytical mistakes economists can make, and in his assessment of game theory I will accuse Fisher of the biggest and most common one, which is the diamond/water mistake. I think the *inframarginal* contributions of game-theory-industrial organization were incredibly valuable.

Around 1975 the Chicago consensus had, largely, won. There wasn't any market power in the economy because cartels always broke down and because barriers to entry, predation and such, could not be equilibrium phenomena.

We now know this argument cannot be established by theory. One important example is the modern theory of incumbency advantage and entry barriers. It is now clear that an industry incumbent can create permanent competitive advantage against entrants *if* there is a long-lived asset whose creation shifts competitive advantage. There were two separate, useful contributions here. First, we developed precisely stated theories in which the conditions favoring or limiting, as well as the possibility of, entry barriers were illuminated. Second, we developed a counterexample to the too-hasty Chicago consensus.

The counterexample mindset quickly became a problem. First, theory was cast as counterexample to perfect competition. Then we developed a series of counterexamples to it. Each year's counterexample becomes next year's received theory. Counterexamples emerged, and so on and so on. As a result, now we know that the order of play matters enormously, and we know that the information structure matters enormously. And, probably, knowing those extra things, we don't know very much, I suspect—but what does one want from theory?

One can't have analysis in a tight way without getting the caveats and the counterexamples. That is one of the advantages of having analysis done in a tight way.

There is a difference between "anything can happen" and interesting analytical statements of why particular things happen. And it is false that, out of this vast welter of counterexamples, no interesting general principles have emerged.

We used to get counterexamples by the dozens. One example. Scholar X assumed Cournot behavior ex post entry and found investment in strategic entry barriers. Scholar Y assumed Bertrand and got the reverse. That produced papers at the rate of one a month for a while. And, then, Bulow, Geanakoplos, and Klemperer cleaned that up. In a broad class of cases, the distinction between strategic substitutes and strategic complements determine the result.

There are thirty other examples like that. There has been a trend toward determining what matters analytically, as well as toward the proliferation of counterexamples in this literature. And that is useful and helpful.

It is a distraction to object to the stylized form in which theory comes. It certainly almost never comes in a form suitable for testing. I agree with Fisher there.

For example, to stay with barriers to entry, theory papers were largely written as if an asset that is long lived is a big plant. The big plant or the low-marginal-cost plant keeps the entrant out of the industry. That has a sort of antiquarian flavor, which feels like the early twentieth century when mass production was the issue. It doesn't feel like the late twentieth century when, according to some papers, though not the main-line ones, the big marketing department, the brand name, the big R&D lab provided long-term competitive advantage.

Indeed the applied work used to think that barriers to entry and economies of scale were competing, substitutable theories of why industries have few firms in them.

One advantage of having a theoretical doctrine of barriers to entry is that from it has emerged the perhaps obvious point that those theories are actually complements. The entry-barring incumbent will find its task easier the steeper the average cost curve. And, one couldn't read, say, Peter Reiss's and my paper from this forum two years ago or John Sutton's book, both empirical studies of entry, without having discovered that useful insight.

That didn't come out of any particular paper in the literature on barriers to entry, but if the formal literature hadn't existed, I doubt we would have ever figured it out.

Theory is being evaluated like diamonds here. It is more like water to me.

My second literary quotation hits closer to home. This concerns my second main theme, which is that the issues before us have absolutely nothing to do with game theory or even with quantitative theory. In saying this, I can't tell whether I am agreeing or disagreeing with Fisher. But if I am agreeing, it needs underlining, and I am disagreeing, it needs saying, "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."[52]

This sentence is from George Stigler's review in 1949 of Chamberlin's book on monopolistic competition, which is graphical at its most mathematical. Stigler, rightly, accused Chamberlin of engaging in exemplifying theory and objected to it in almost exactly Fisher's language. This quotation could have been from today's talk, I think.

Why do I bring up this ongoing debate over the role of abstraction versus knowing in industry economics? Well, mostly because I want to accuse Fisher of being a running-dog lackey of the theorists. He is far too kind to them.

I can do this most conveniently by citing the relationship of oligopoly theory to oligopoly empirics. We know what Fisher said about empirical studies of oligopoly; let me tell you what the authors of the studies I reviewed say about their papers. Half of them say they were providing systematic statistical tests of game theory models. A quarter of them,

52. Stigler (1949, p. 23).

including this speaker, say they were providing the first systematic statistical tests of game theory.

Why does this discrepancy occur? Because of a continuing, useful debate over what constitutes the effective use of a theory in empirical work.

Fisher goes along with the theorists in finding that those papers don't use the structure of the theory.

I differ. The main reason that this debate goes on comes from something that is fundamental, I think, in most modern oligopoly theory. The theory has a long-run dynamic, somewhat fancy individually rational part. In this, threats of a general outbreak of competition enforce cooperative outcomes in price, most, if not all, of the time.

We are vastly better at measuring what the threats enforce—the prices above marginal cost—than we are at measuring the threats themselves. Why? Well, this is fundamental in the way we can hope to use the theory of games in applied work.

If one follows the theory in a literal-minded way, if that is what the "structural" means, one tries to measure the threats. Then one ends up regressing unobservables on other unobservables with particular reference to the fact that the crucial relationships occur off the equilibrium path, that is to say, they don't happen. That is hopeless as a guide to applied work.

On the other hand, the implications of the theory may well be useful and testable.

Let me take another simple example. I will take two empirical scholars—the young George Stigler and the young Rob Porter—on the kinked-demand curve of oligopoly. The young George Stigler found that the theory was vacuous—he was a bit of a theorist in those days, too—and he found that the implications were all wrong.

The kinked-demand curve theorists of his day had written their theories in an informal way, which made the key implications completely inpenetrable; and the series of events by which the theorist George Stigler in 1964 resurrected the kinked-demand-curve—followed by more and more formal treatments—made it much easier for the young Rob Porter to test the critical implications.

Theory is occasionally useful, sometimes in a fairly direct way. That, it seems to me, is a terrific accolade for what is basically an exploratory activity. Much of the exploration should lead to dry holes.

We have, in industrial economics, roughly sixty years in which the set of questions hasn't changed. The one Fisher emphasizes is an important one. Where is market power in the economy?

There are other important ones: Where is the private rate of return to R&D close to the social rate? Is it true that, in some kinds of industries, perhaps capital-intensive ones, prices are particularly far from marginal cost in the cyclical trough? Is it true, as they would have said fifty years ago, that there are too few firms in mass production industries? We would now add mass-marketing industries and knowledge-intensive industries to that list, given the way the economy has changed.

I don't see how to attack these questions inductively on an industry-by-industry basis and deductively without the "bag of tools." I just don't see how to go forward.

I have my last quotation. This one is not from a book; this one is from the side-view mirror in a 1986 Ford. "Objects in mirror are closer than they appear."

You know what mirror I mean: the theories. And you know what objects I mean: the industries.

I hope I have been neither too unkind to the mirror by asserting that it is curved, nor to Fisher by saying that I can see in it.

Comment by Joseph Farrell: No doubt it was inevitable in discussing the *Handbook* that literature citations should outweigh direct economic analysis. I did not expect the literature to be poetry, but I will respond as best I can.

My job, to comment on Franklin Fisher's review of the surveys in parts 2 and 3 of the *Handbook*, puts me a very long way from actually studying industrial organization itself, so I too will wax methodological. I will also limit my remarks to the theoretical side (part 2 of the *Handbook*).

Is the World Simple Enough for Generalizing Theory?

The *Handbook's* delayed appearance introduces my first literary quotation, which deals indirectly with some of the issues Fisher associates with some exemplifying theory: "'Things are very difficult, things are very difficult, things are very difficult,' Dixon gabbled into the phone."

In Kingsley Amis's *Lucky Jim*, the villainous L. S. Caton has been hinting to Dixon, through a cloud of evasions, that his article, supposedly accepted for publication, will be delayed indefinitely. Complexities, some described in tedious detail and others left darkly vague, are used as a bar to action and to serious explanation. Dixon, who like most of Amis's heroes is an impatient man, is roused to sarcastic wrath. Does Fisher feel some similar impatience with the "anything can happen" tendencies in modern theoretical industrial organization (IO)? Certainly some people do, and certainly some, like Caton, abuse the complexity for their own purposes, especially in antitrust and in trade policy. This is a real problem, but even if courts cannot tell the difference between good-faith and rigged models, perhaps we can. In any case, I will limit my remarks to good-faith modeling, which seems to be the focus of Fisher's remarks and still creates the impression that "anything can happen" in theoretical IO.

But anything can happen is much too flippant a summary of our subject, and the complexity, uncomfortable though it is, is real and is not to be evaded. As Albert Einstein supposedly said, "Everything should be made as simple as possible, but not simpler." How simple is that? Those who bemoan the complexity and inconclusiveness of theoretical IO often yearn for what they see as the simplicity and generality of other fields. Talking to some colleagues yields two alleged paragons of such simple generality: general-equilibrium theory and Newtonian physics. I'd like to take a closer look at those two subjects, and I will argue that they are not so simple and general.

General-equilibrium theory rests, as we know, on some very special and unrealistic assumptions. What do we get in return for swallowing them? Further, and more unrealistic, assumptions ensure the existence of an equilibrium, which is nice, since the subject is inarticulate if there is no equilibrium. This equilibrium is unique only in the presence of still more assumptions. Finally, the main result about general equilibrium, the welfare theorem, holds only in the presence of complete markets, the most outlandish assumption of all. More typically, the general-equilibrium approach cannot tell us that there is an equilibrium at all; if there is one, there may well be many; and we know essentially nothing about either descriptive or normative properties of the equilibria. Despite these defects of the model, economists have found the general-equilibrium approach extremely helpful in understanding economic questions: but it has taken quite a long time to get from Walras to modern computable general equilibrium. Perhaps we shouldn't be too impatient with IO theory?

We often look outside economics to Newtonian physics as an example of generality and simplicity. Alexander Pope wrote, "Nature, and Nature's ways, lay hid in night; God said, 'Let Newton be,' and all was light." But "all was light" only under the streetlight. It is easy enough to write down the equations of Newtonian physics and to solve certain special problems, but these problems are not typical. Consider analyzing the motion of a small number of bodies under gravitational attraction. With just one body, nothing happens. With two, one gets a simple, rather boring story in which they accelerate toward one another. With three bodies or more, in general, you have an insoluble problem, typically including chaos. Prediction is often impossible, even with vast computing power and accurate measurements of initial conditions. In other words, anything can happen, and while the model is deterministic, qualitative features of the outcome may well depend on details of the initial conditions that are in practice unobservable—in short, quite reminiscent of Fisher's pessimistic view of game-theoretical IO.

Moreover, Newtonian physics is not quite correct. Pope's couplet has a sequel, perhaps by Hilaire Belloc, "It could not last: the Devil, howling 'Ho! Let Einstein be!' restored the status quo." I do not want to overstate the case: of course, Newtonian physics was revolutionary and is practically useful to an extent that IO theory is not and presumably never will be. My point is that simplicity and generality are elusive and often illusory. The first test of a body of theory is whether it helps one to understand problems that one was already interested in; I believe that modern IO theory passes that test.

It is true that, on one level, modern IO teaches one that anything can happen. But that is seldom all one learns from a paper. More often, one gets some insight—within a special model, admittedly, but often one is able to peer out of the model a bit—into when this or that can be expected. It's not ideal, but it is helpful.

More important, though, if good-faith models say that simple predictions are not forthcoming, I think it is counterproductive to bemoan the fact. That's the way it is, and we'd better like it. Fisher doesn't disagree with the substance of this view; for instance, he writes, "The theoretical facts are as [theorists] have recited them, and the possible outcomes are extremely numerous and assumption-dependent.'' But he feels that this statement is ''bad news.'' Maybe it is, but that doesn't seem to me the main point. I think it's a cause for celebration when we learn the facts, even if they stymie our urge for general statements. As Josh Billings put it, ''It is better to know nothing than to know what ain't so.''

In practice, there is probably even less disagreement here than might appear. For instance, it is commonsense that high accounting profits tend on the whole to reflect high economic profits. But in some ways accounting profits misstate economic profits, and when Fisher, with McGowan, recognized, analyzed, and published these insights, I doubt that he did so with a very heavy heart.[53] Indeed, I remember a lunchtime conversation in which he described some colleagues' incorrect thinking on this matter. As he related one colleague's reaction, Professor X said, ''Yes, Frank, I understand that accounting profits don't measure economic profits in theory. But surely they *sort of* measure economic profits in practice?'' Frank's response: ''No! Not only don't accounting profits measure economic profits—they *really* don't measure economic profits!'' I think this is the right attitude to exemplifying theory, even though economists acting in bad faith might come up with ''anything'' in a superficially acceptable model.

Of course, sometimes an exemplifying model does show only a theoretical possibility, one that is not important in practice. Giffen goods are probably an example. One would like to shrug off these examples as being, perhaps, interesting and worth keeping in mind, but generally not much more. Some economists take this approach with almost the whole literature on market failures, which seems to me to be going too far. But how far should one go? I think one has little choice when empirical testing is unavailable (as it typically is for the most interesting conclusions of economic models, namely, welfare statements), but to ponder the model, try to test it for robustness to the modest extensions of which we are capable, and above all to remember that we could easily be wrong. Except for the recommended humility, these steps seem to me to be roughly what we do in practice.

For example, consider the question, Can truthful product preannouncements reduce welfare? Garth Saloner and I showed, in a special

53. Fisher and McGowan (1983); and Fisher (1987a).

model of course, that when network externalities are important such a thing can happen.[54] Fisher, while complimenting us on "exemplifying theory as its best," nevertheless says he "still believe[s] [the claim that this could not happen] to be usually true."[55] He may well be right, but perhaps it is more fruitful to ask how one could know. Since statements about what reduces welfare are not directly testable empirically, I think the best one could do would be to build a tractable model including parameters that reflect some, inevitably not all, of the ways markets differ, and in which both the proposed norm and the proposed exception can happen (anything can happen). We try to find out, inside the model, which parameter combinations make which statements true, and then, if the important parameters are observable, try to find out empirically what parameter combinations obtain in various markets. Isn't this what theoretical IO, as currently practiced, tries to do?

So I find myself much less unhappy than Fisher about the value of modern theoretical IO. I think most of it is done in good faith, and when one discovers complexities, one should take them seriously.

Categories of Theoretical Industrial Organization

Fisher divides theory into exemplifying and generalizing and is dissatisfied with exemplifying theory, while I am suspicious of generalizing theory. In this section I will propose another division, one that fits better with my views as just suggested.

I think the division between exemplifying and generalizing is often, but not always, unhelpful. We may want a category for theory that consciously sets out to be exemplifying—theory that shows that claim X, which we all know can be true and generally suspect is usually true, isn't always true. But when theorists set out to study a general question in what seems to be an appropriate model, I don't want to make too much of the difference between such a study that concludes "so-and-so is always true" and a study that concludes "it depends." Surely the distinction is in the facts, not in the style or type of theory.

So, in setting up categories for theory, I would instead distinguish

54. Farrell and Saloner (1986).
55. Fisher (1989, p. 118).

first between custom-written or specialist theory and generalist theory. As we know, a specialist knows everything about nothing, and a generalist knows nothing about everything. Correspondingly, specialist theory tries to describe rather accurately a particular market, institution, or practice, and may be of no interest outside that narrow context; generalist theory tries to cast a little light on a wide range of markets but may not illuminate any of them enough to read by.

Specialist theory is a necessary step, often in part unconscious, in studying a market or practice in detail. It has been said that there can be no observation without theory, and this is true in that there must be some idea guiding the selection of facts to notice. At the most basic level, theory is a way of organizing data and deciding what is important and what is not. For example, traditional Cournot oligopoly models encouraged, and were encouraged by, a belief that concentration among existing firms is important for economic performance; contestability models encourage the belief that entry conditions matter most. The theory in this sense—the choice of what to notice—may well, as this example suggests, come from elsewhere, but it will probably be transformed before long into something much more specific. After the analyst completes a case study, she or he will have developed a more mature specialist theory of that case: a theory that will enable her or him to make welfare judgments or positive predictions about what will or would happen in the industry. Perhaps that theory will in turn suggest how to view other cases or industries, but perhaps not: the true practitioner of specialist theory hardly cares.

In contrast, generalist theory is much more easily undertaken in an armchair. A theorist might notice, for instance, that advertisers believe that advertising increases sales and profits and might wonder whether advertising levels are likely to be excessive. He could spend many happy hours modeling this question without even asking whether the advertising is direct mail or broadcast television or newspaper advertising. This observation is not a criticism: some details do not matter, and pointing that out is one of the purposes of theory. If the theorist is smart or lucky, the result will cast some light on each of many industries but will not be the main point about any of them—nor does the true practitioner of generalist theory care.

I think that several of Fisher's complaints about the state of theory might be reformulated as a wish that more specialist, and less generalist, theory should be done, published, and summarized in the *Handbook*. I agree. Certainly I no longer bother to read articles that baldly begin, "Consider a market in which firms first set capacities, then advertising levels, then outputs." Precisely because the world of theoretical IO is so rich, it is important to choose one's questions wisely, and a study of a market can never be so irrelevant as a study of an ill-chosen generalist question. (Probably it is generalist theorists who have "a casual attitude towards what constitutes verification.")

Within the generalist category, I would make a further division into "comfort theory," "discomfort theory," and "exploring the unknown." By comfort theory I mean simple, putatively robust, usually intuitive models that formalize what we think are probably true statements about markets in general. For instance, Cournot oligopoly theory says comfortingly that the more oligopolists there are, the closer the price is to marginal cost. Similarly, the theory of repeated games says, as we already thought, that collusion among oligopolists is typically easier the fewer of them there are.

I will digress on the theory of repeated games, since I disagree with Fisher's remarks on the subject. First, the Folk theorem tells us only that every strictly individually rational outcome is consistent with the incentives for individual optimization for large enough discount factors. That does not mean that every such outcome is equally reasonable, even so far as game theory is concerned. Game theory does try to "refine" this set of possibilities. For example, Eric Maskin's and my theory of "weakly renegotiation-proof equilibrium," extended in a natural way to the symmetric many-player case, says that in an infinitely repeated symmetric Cournot market with linear demand and constant unit costs, ten or more firms *cannot* sustain perfect collusion, no matter how little discounting there is, but eight or fewer firms can, *as far as this analysis is concerned*.[56] Although this is surely not the answer to oligopoly, it seems to be the kind of result that Fisher is complaining of the lack of. Second, the focus on analysis "for sufficiently little discounting" is technically convenient but likely to be misleading. (In almost any repeated-game formulation of oligopoly, a small number of firms can successfully collude for a wider variety of discount rates than can a large number.

56. Farrell and Maskin (1989); see also Bernheim and Ray (1989).

Discomfort theory sets out to disprove intuitive or received ideas. This theory is perhaps the closest equivalent in my schema to Fisher's "exemplifying theory." As just suggested, I think such theory is valuable because of the standing temptation to generalize too far. At its best, such theory keeps us honest; at its worst, it is pedantry.

Exploring the unknown is generalist theory that sets out neither to confirm nor to disprove accepted wisdom but to address with a fairly open mind a relatively new question, or a new form of a question, whose answer is widely acknowledged to be unknown. As my choice of the name might suggest, of the various kinds of generalist theory, I like this one the best. And unlike comfort theory, which tends to become generalizing theory if not disciplined by discomfort theory, and discomfort theory, which may seem nihilistic, exploring the unknown seems to me to offer the best chance of enlarging our understanding.

But enough of these categories!

What Is to Be Done?

What guide, if any, does this give IO theorists looking for advice on how to do research? As the reader will have gathered, I admire specialist theory and exploring-the-unknown theory more than comfort or discomfort theory. Fisher seems pessimistic about the social returns of doing IO theory at all until a way is found of looking at things that allows one to get back to generalizing theory.

Maybe indeed we just haven't yet found the right way of viewing things, the way that will make everything clear. Might some young economist be sitting under an apple tree right now, about to realize that it's all very simple if one looks at it the right way? Yes, there probably is; often there are. So far, they have been wrong, and I suspect they will go on being wrong. The world really is complicated, and the search for great generality and simplicity is doomed to failure—or at best to only partial success.

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