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ORGANIZING INDUSTRIAL ORGANIZATION:
REFLECTIONS ON PARTS 2 AND 3 OF THE
Handbook of Industrial Organization

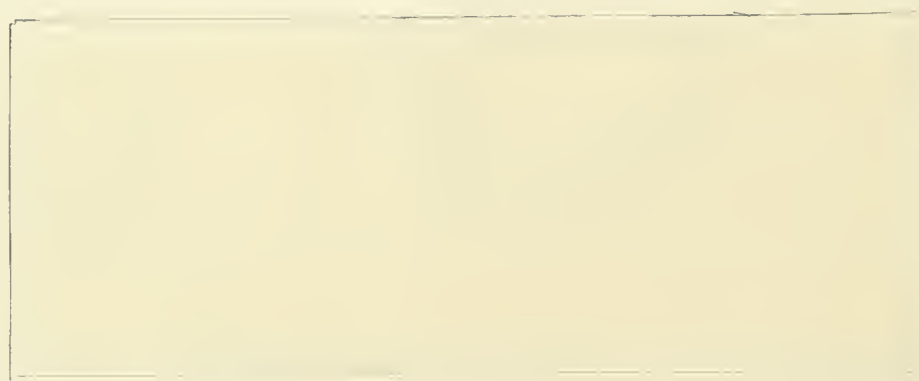
Franklin M. Fisher

Number 566

October 1990

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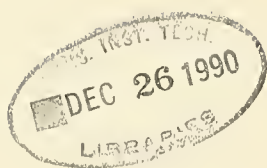
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ORGANIZING INDUSTRIAL ORGANIZATION: REFLECTIONS ON
PARTS 2 AND 3 OF THE Handbook of Industrial Organization

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* Paper prepared for the Brookings Papers on Economic Activity Microeconomics Conference, December, 1990. This paper is dedicated to Carl Kaysen on the occasion of his 70th birthday.

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"

1. Introduction

My task in this paper is to reflect on Parts 2 and 3 of the Handbook of Industrial Organization.¹ These are the sections

1. Richard Scmalensee and Robert D. Willig, eds., Handbook of Industrial Organization, (Amsterdam, New York, Oxford, and Tokyo: North-Holland, 1989). All references are to this unless otherwise stated.

respectively entitled "Analysis of Market Behavior" and "Empirical Methods and Results". The former section is almost exclusively theoretical, while the latter, as its name suggests, is empirically oriented. Both sections deal primarily with the

analysis of markets, particularly of oligopoly. (For convenience, the Table of Contents of the entire Handbook is reproduced below.)²

2. A review of the remaining sections of the Handbook: "Determinants of Firm and Market Organization", "International Issues and Comparisons", and "Government Intervention in the Marketplace" is being written by Alvin Klevorick. For convenience, when I speak about "the Handbook" I shall generally mean Parts 2 and 3.

Such reflection has two aspects. First, there is the matter of the Handbook itself. Is it a good book? Does it succeed in its stated aims? Second, however, is a broader and more important set of questions. Reading the Handbook provides the opportunity for thinking about the state of the art, about industrial organization as a field. Where is it? What are the organizing principles? Where is the field going? Is that the correct destination?

This latter set of questions are the subject of most of this paper. But before I get to them, the former set deserves attention, and, in fact, the two sets are not unrelated.

2. The Handbook as Book

I begin, then, by considering the Handbook as a book. In doing so (and, indeed, in considering the state of the art below), I necessarily paint with a pretty broad brush. The book is immense, and detailed review of the individual chapters would be tedious, if not impossible. As a result, there are plenty of

exceptions to many of my general comments and especially to my criticisms, and I hope particular authors will forgive me for not pointing them out.

Having said this, I add at once that my first reaction is that of enthusiastic praise. This is a very good book. Every chapter is a well-written 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 which themselves add to the art.

On the other hand, I do not suggest sitting down to read the Handbook straight through. It is not intended for that. Indeed, I cannot do better here than quote the advice given by the editors of a rather good collection of political jokes (Lukes and Galnoor 1987, p. xiii):

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.

This is not criticism, however, for the Handbook is not intended for cover-to-cover reading (except by exhausted reviewers). Rather it is intended as a handbook, a reference work whose purpose, as stated by the editors (p. xi) 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 non-specialist economists and students in advanced graduate courses.

Is the Handbook successful in achieving this goal? I think only partly so. In the first place, particularly in the theore-

tical chapters of Part 2, the non-specialist will often find the going pretty heavy, even though the underlying tools have been provided. (Sensibly, Part 2 begins with Fudenberg and Tirole's overview of the methods and results of noncooperative game theory.)

Further, again especially in Part 2, the authors sometimes succumb to the temptation to deal with their own latest, sometimes unpublished work (and perhaps that of their students and friends). This is not necessarily a bad thing. 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 wants 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 hard 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 both to describe what is known and also the degree of certainty with which we know it. Here, Schmalensee (Chapter 16) and Cohen and Levin (Chapter 18) have the really hard task of dealing with cross-industry studies. They do a good job of organizing their respective topics, but are less successful at providing a detailed critical guide to the underlying literature. Since, as we shall see, the studies surveyed are open to considerable theoretical objection, it is of particular importance to sort out which studies and which conclusions are

solidly based and which are not. While both chapters (especially Schmalensee's) do deal with the underlying problems involved, they fall down in the perhaps impossible task of carefully separating good, soundly-based studies from more questionable ones. Rather we get the authors' own (and doubtless often correct) impressions as to what the literature shows.

It is also often (but not always) the case that the authors of the theoretical chapters have only a general idea about the results of empirical work. 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 the Facts of How Markets Clear" stands out in sharp contrast from most of the chapters of Part 2.³ Carlton

3. Indeed, the contrast is so sharp as to be jarring. One wonders why Carlton's chapter appears in Part 2 at all.

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 facts. In level and tone, it comes far closer than most of the other chapters of Part 2 to meeting the purpose quoted above.

The other theoretical chapters of Part 2, as indicated, spend little time on systematic examination of empirical facts. They mostly pay little attention to empirical work or else resort to casual observation. Only occasionally, as in Ordover and Saloner's excellent piece on "Predation, Monopolization, and Antitrust" (Chapter 9) does one find a real attempt to apply the

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 in-depth 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.

I realize, of course, that such work is hard to survey in a systematic way. This is because each industry study tends to be idiosyncratic with organizing principles linking them hard 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 is Timothy Bresnahan's essay (Chapter 17) on "Empirical Studies of Industries with Market Power". That chapter, excellent and interesting in itself, is focussed on work that uses a particular set of techniques; it does not pretend to survey the wider area.

The second area that is not really systematically surveyed is related to the first; it is the area of public policy. Anti-trust policy issues are indeed discussed in several of the chapters (the Ordover and Saloner essay already mentioned, Varian on "Price Discrimination" (Chapter 10), and Katz on "Vertical Contractual Relations" (Chapter 11), for example). But the Handbook

makes no separate systematic attempt to tie together economic and legal thinking as to public policy on market power and related issues. Since 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 afford to see theory in action. The authors of the theory chapters obviously believe that theory provides a rich set of tools for application when studying particular industries. Thus, Carl Shapiro states (p. 409) after an extensive discussion of "Theories of Oligopoly Behavior (Chapter 6):

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.

Had the Handbook successfully surveyed industry studies and analysis of particular antitrust cases, it might have been very instructive to see how those tools have been used or how they might have been used. (As we shall see, however, I suspect that such an exploration would in fact have revealed that views such as that just quoted are far too sanguine about the usefulness of theory in its present state.)

Before leaving my discussion of the Handbook as a book and

moving on to the wider arena of what it reveals about the state of the art, I must discuss one minor matter. The proofreading of the Handbook is a disgrace. Names are misspelled, sentences are often ungrammatical, references to other chapter numbers incorrect, and, while 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 (p. 1020) to a "higher or at least higherfaulting theoretical language". He also states (p.1015, n. 5) 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 (p. 773, n. 4) who says of the Walrasian auctioneer that "no one probably took the tantamount process seriously."

And 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.

3. 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 reflected 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 same way that some of the authors of the Handbook chapters do. Schmalensee (Chapter 16), for example, organizes his summary of "Inter-Industry Studies of Structure and Performance" in terms of a series of "Stylized Facts". Eaton and Lipsey (Chapter 12) begin their essay on "Product Differentiation" with a list of seven "awkward facts that are available to constrain theorizing" (p. 725). Since the present paper 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."⁴

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

Organizing_Principle_1:

Industrial Organization has no organizing principles except for those that are subcases of this one.

This is not a joke. As we shall see, I believe that there are deep reasons for such a lack, and it manifests itself in a number of 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 in fact occur -- sometimes contrary to what one might have thought.

This sort of theory is what I have elsewhere called "exemplifying theory" (Fisher 1989). 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" (fisher 1989) or, indeed, to a theory with much real content. Rather it has produced a taxonomy -- a laundry list of a vast number of possibilities which rules out very little.

This fact has not escaped the attention of a number of the authors of the Handbook. Thus Jacquemin and Slade state in their essay on "Cartels, Collusion and Horizontal Merger" (Chapter 7, p. 416, emphasis added):

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.

They later say (p. 441):

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.

Gilbert states in his essay on "Mobility Barriers and the Value of Incumbency" (Chapter 8, p. 478):

[T]he 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 (p. 509) to "a taxonomy of behavior in response to entry."

In one very important sense, of course, this situation is not the fault of theorists. The theoretical facts are as they have recited them, and the possible outcomes are extremely 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. One must not blame the messenger for the bad news (although one can be skeptical as to just how surprising the news really is).

On the other hand, one can reasonably question whether theorists are working on a very useful research agenda. We now know that no general results will emerge that map simple facts about market structure into performance outcomes. Moreover, while theory is often illuminating:

Organizing_Principle_3:

The stripped-down models of theory often fail to provide very 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 quantity or else set price. They set a complex variety of strategic variables. Contrary to the optimistic view expressed in Shapiro's "bag of tools" quote given above, the analyst working on a particular industry will often not be able to decide what tools apply (or if any do).

Quotations from the Handbook are illuminating here. Fudenberg and Tirole state (p. 292, emphasis added):

[F]irms 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

Jacquemin and Slade state (p. 447):

In all of these models, price wars are equilibrium strategies of supergames; no one ever cheats. This is perhaps [!] a shortcoming of these 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, economists have devised few theories to explain cheating in collusive agreements.

Reinganum states in her essay on "The Timing of Innovation: Research, Development, and Diffusion" (Chapter 14, p. 905):

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.

Cohen and Levin, writing on "Empirical Studies of Innovation and Market Structure" (Chapter 18) agree with this, although they are certainly not wholly pessimistic (p. 1096, emphasis added):

One difficulty with testing recent game-theoretic models of R&D 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 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.

In something of the same vein, Ordover and Saloner state (p.538, emphasis in original):

[T]heoretical 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 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".

Stripped-down models can, in fact, be very useful, but, as Eaton and Lipsey observe in their essay on "Product Differentiation" (Chapter 12, p. 759), "[T]ractability in deriving incorrect results is no advantage." For "incorrect" read "inapplicable". Industrial organization theory has a long way to go.

The journey is not made easier by:

Organizing_Principle_4:

Some (by no means all) theorists have a casual attitude towards 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 very few facts that the theorist happens to know.

The quote from Cohen and Levin given above is one illustration. Others can be found in the very casual citation of certain antitrust cases by some authors.⁵ Thus, to take an example that

5. This is definitely not to say that all authors of the Handbook are casual in this regard. Ordover and Saloner, for example, have read the literature on the cases they cite.

I know well, Telex_v._IBM is cited as providing an example of contracts and entry prevention (p. 502 n.). In fact the case only does so in terms of the plaintiff's allegations. It is cited again (p. 507 n.) for the effects of "locked-in" customers in producing alleged price discrimination. 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. See Fisher, McGowan and Greenwood 1983, pp. 196-204, 316-7, 325-8.)

To continue with the computer industry, Gilbert writes (pp. 514-5, emphasis added):

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] aspects of the entry processs (sic), 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 (1972).

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 a terrific 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. (Farrell and Shapiro 1990). The fact 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 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 theorists. (See, for example, pp. 437, 449, and 455, and Shapiro 1989, p. 133.)

I now turn to a consideration of 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 of 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-conduct-performance paradigm have never been very soundly based in theory. Not only has theory not provided much quantitatively useful guidance as to exactly how structure affects performance, even at the level of what variables should be used, but the empirical practitioners have 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. More specifically, attempts to use profitability as the basic measure of performance simply misunderstood both the role and even 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 major mistake -- and one that runs consistently 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 hard to deal with such

dynamics does not make them go away.

Put this aside, however, and suppose that comparison of a firm or industry's profitability to some "normal" standard is in fact 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 -- that 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 this (admittedly hard to measure) magnitude. Rather, many studies used the accounting rate of return (profits divided by stockholders' equity or by the value of capital stock). Since capital stock purchased now is done so with an eye to future profits while current profits are earned in part because of investments made in the past, it should come as no surprise that such measures do not in fact 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 the fact that others had made similar points in the past, this fact did cause quite a lot of surprise (not to say outraged protest) when John McGowan and I pointed it out some years ago. (Fisher and McGowan 1983; see also, e.g., Long and Ravenscraft 1984 and Fisher 1984).

A similar problem infects studies using a different profita-

bility measure -- the profits-sales ratio. Even making quite favorable assumptions, it turns out that this quantity does not in fact equal (or possibly even approximate) the Lerner measure of monopoly power (price minus marginal cost all divided by price) except under very special circumstances. (Fisher, 1987a).

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. (See Fisher, McGowan and Greenwood, 1983, p. 257.) Others simply found it hard to believe that they were measuring the wrong thing.

Some progress has been made. Recently focus has shifted from profits to prices as a performance measure (Weiss, ed. 1989) -- something that has its own serious problems.⁶ Schmalensee,

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

who understands the issues involved gets 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 which are the objects of

theory.

A somewhat similar (if less pervasive) problem arises 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 as to the effects of firm size on research and development (R&D) in the presence of economies of scale. (See Fisher and Temin 1973, 1979, Rodriguez 1979, and Kohn and Scott 1982.) 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 empirical workers feel with such demonstrations. They state (p. 1071, emphasis added):

[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.

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 as to 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 in fact testing (and probably was not able to test) any of the propositions which it purported to examine. Apparently, that didn't stop anybody.

This picture of careless disregard for theory by empirical workers is, of course, too general to be totally accurate. Moreover, in one area, at least, it is certainly not correct.

Bresnahan's essay (Chapter 17) surveys "Empirical Studies of Industries with Market Power." It reports on econometric studies of particular industries undertaken to test whether those industries behave competitively and to measure market power. This literature recognizes (p. 1012) that "[f]irms' price-cost margins [cannot be] taken to be observables [since] economic marginal cost ... cannot be directly or straightforwardly observed." At least as important:

Individual industries are taken to have important idiosyncracies. 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.

This literature stands out from most of the empirical work surveyed in the ~~Handbook~~ in that it certainly does use theory. On the other hand, the theory it uses is not closely related to the game-theoretic analyses of Part 2. Further, while some progress has been made in the detection of market power, Bresnahan states (pp. 1053, 1055):

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.

Maybe so. I am more skeptical than Bresnahan about our measurement success, but 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 taking the general empirical literature on its own grounds and ignoring the kinds of analytic defects pointed out above, most results can only 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, since theory is not providing propositions that are testable in practice (Organizing Principle 3).

4.1.A. Research Agenda

The failure of the empirical literature is not an accident, however. Indeed, in one (not very helpful sense) that literature does indeed confirm the results of theory. The principal result of theory in this area is that 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 into 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 very 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 conduct and performance character-

ristics. The theoretical counterpart to this is that the program of investigating how perfectly rational opponents will behave in overly simplified settings has failed as well (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 oligopoly markets and related subjects.

This conclusion, however, 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 (Fellner 1949) or have worked extensively on industry studies. The simple-structure-measures-cum-rational-behavior model does not lead to very useful results because the context of particular industries in which firms operates strongly affects which 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 equally-sized firms as it is when there are two. In this sense, modern 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 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.⁷

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

The difficulty, of course, is that numbers and concentration are not all that matter. A great many other things are likely to matter as well. As Carlton states in his essay on how markets clear (Chapter 15, p. 911):

[M]uch 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, heterogeneity 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.

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 affects the ability to achieve the

joint-profit-maximizing outcome. I do not believe that this is what most of modern theory is doing. Further, as I have elsewhere explained in detail (Fisher 1989), I do not believe that the theoretical tools currently so popular are particularly well suited for that task.

Lacking strong guidance from theory, we need to know what in fact happens. This surely requires the detailed study of particular industries. The cross-section literature is too simplistic to be of much assistance here, and the somewhat casual attitude of some theorists towards empirical verification (Organizing Principle 4) is of no help at all. (The econometric literature surveyed by Bresnahan 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 generally (but only generally) know what can matter. The problem is that we have known that for more than forty years. What we need to know is what aspects of the contextual setting matter in practice.

This may be where experimental methods come in. Plott, in his essay, "An Updated Review of Industrial Organization: Applications of Experimental Methods" (Chapter 19), lists a number of cogent reasons for the use of such methods (pp. 1165-9). He does not explicitly mention the possibility that by carefully controlling the context in which market-like games are played, one can

gain insight into what aspects of context are likely really to matter in non-experimental situations. But that possibility comes across from his survey.

5. Concluding Remarks

Despite my favorable remarks on the Handbook itself, this essay will no doubt convey a somewhat negative tone. Yet, despite my comments on the state of the field, this need not be a time to be depressed about industrial organization. The field has been undergoing a revolution. Even though that revolution has not produced results nearly as exciting or relevant as they seem to some of the revolutionaries, the revolution is not yet over.

The two poems quoted as epigraphs to this paper give different descriptions of what it is like to live in revolutionary times. The poem by Yeats can be taken as describing 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 outlined in the Wordsworth quote can be achieved.

But that promise has not been achieved as yet. 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 the Wordsworth poem is

"French Revolution, as It Appeared to Enthusiasts at Its Commencement." As I said earlier, industrial organization has a long way to go.

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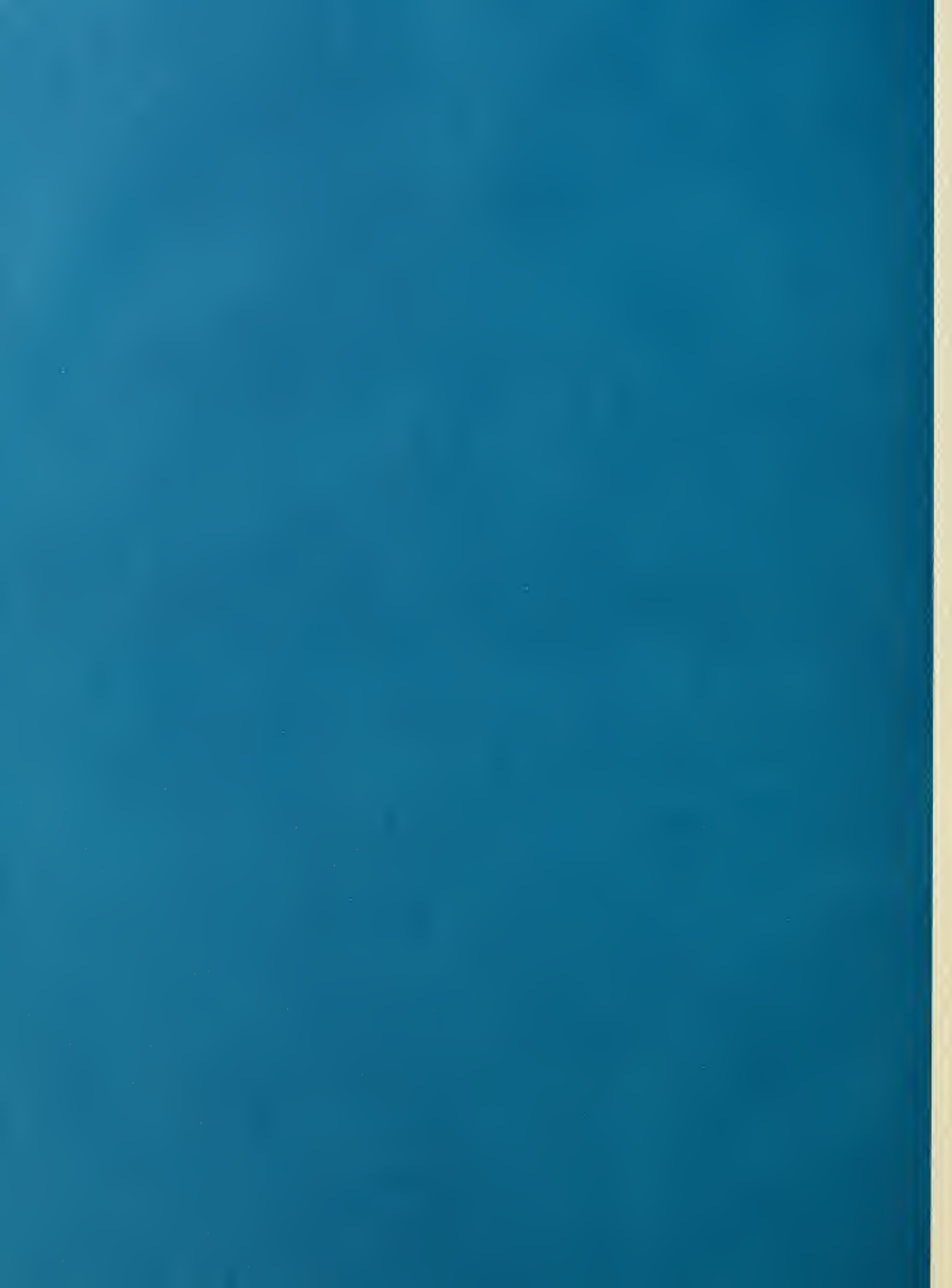
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