

SURVEYS IN ECONOMICS

ECONOMICS OF INDUSTRIAL ORGANISATION

STEPHEN DAVIES
BRUCE LYONS

with
HUW DIXON
PAUL GEROSKI

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HUW DIXON
PAUL GEROSKI

Economics of industrial organisation

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Editors' preface

The aim of this series is to survey the primary literature on selected economic topics at a level designed for intermediate and advanced undergraduate students. Few textbooks contain an adequate perspective on the development of their subject, and still fewer portray the focus of current research; but it has become progressively more difficult to supplement textbooks by selecting journal articles which provide a satisfactory, comprehensive, coherent and self-contained treatment of a topic, at a length and level of technique within the capacity of a student. The widening gap between the pedagogic and the professional literature stems partly from the increasing volume of the latter, the consequent pressure to abbreviate manuscripts, and the dissemination of research in a growing number of more specialised journals. It also reflects the increasing technical sophistication of the subject in all spheres of application, and particularly the integration of theoretical and empirical analysis which characterises much recent research, in marked contrast to the teaching of economics and econometrics in separate compartments of most undergraduate courses.

The surveys have been written for those who are studying or have completed intermediate courses in economic theory and quantitative methods. They aim to guide the reader through the professional literature, paying particular attention to the introduction of empirical work and to synthesising relevant contributions from different areas of specialisation. The major issues are explained and attention is directed towards the most useful sources for further study. By providing a comprehensive overview of its subject, each survey enables users to pursue particular aspects of the topic in greater depth through the medium of primary sources, within a frame of reference which enables the significance of those individual contributions to be assessed in their broader context.

The subjects of the surveys have been selected for their economic importance and for the extent and inaccessibility of the literature in journals and monographs devoted to them. Each is complete and self-contained, and can be read without reference to the surveys of related topics which appear in the same volume. The volumes themselves are not intended to replace textbooks by providing comprehensive coverage of their area, but to supplement textbooks by conveying the current flavour of the state of the art.

The series as a whole has been designed for second- and third-year undergraduate students at universities and polytechnics, but individual surveys will

also appeal to postgraduate students and to practising economists in government, nationalised industries and the private sector who wish to update their knowledge of the subject. Economics has developed rapidly in the last two decades, and even active members of the profession have experienced difficulty in keeping pace with progress outside their own field of specialisation. It is hoped that the series will prove useful to this wider readership of continuing students as well as those beginning their education in economics.

Robert Millward
Michael T. Sumner
George Zis

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Series editors: Robert Millward, Michael T. Sumner and George Zis

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Introduction¹

Stephen Davies and Bruce Lyons

As a prelude to the detailed and specialised surveys which are to follow, we intend in this introductory survey to establish an overall perspective: how is industrial organisation to be defined? What are its roots in economic theory? What are the prevailing methodologies of analysis? What are the implications for public policy? To answer these questions, we offer a short history of thought in industrial organisation, which is presented in the following two sections. The first traces what might be called the mainstream over the last thirty years; this comprises the neo-classical (broadly defined) approach which has dominated industrial organisation, at least as measured by its share of practitioners in the subject. But our second section acknowledges that there are other ways of looking at the subject, and we describe three: Austrian, Schumpeterian and Marxist. Since these schools of thought appear only infrequently in the following surveys, this part of our Introduction is, in some ways, the most important. It may be particularly useful to reread this section following the surveys. The third section introduces the five main surveys of the book.

We should acknowledge at the outset an important omission in this Introduction, and indeed the volume as a whole. As explained presently, we interpret industrial organisation as the study of the organisation of industries rather than the firms therein. In terms of the producer side of micro-economics, this places the emphasis on oligopoly theory and its applications rather than the theory of the firm. This delineation is not totally arbitrary – different traditions can be discerned in the literature – but, to be honest, our main motive for so limiting this volume is the simple and obvious one of space constraints. However, an unfortunate consequence is that we largely ignore the important contributions over the last two decades of O. E. Williamson. Building on the earlier insights of Coase (1937), Williamson's transactions costs approach has revolutionised parts of the subject, in particular the explanation of why firms exist in terms of their efficiency relative to the market; the motives for vertical integration; and explanations of the evolution of internal organisational form (the U and M forms).²

1. Mainstream industrial organisation theory

We identify the dawn of the modern economics of industrial organisation (I.O.) with the work of Joe Bain in the 1950s. This is not to deny the importance of earlier

writers. Bain's ideas owe much to the writings of Mason (1949) and Clark (1940), and we argue below that contemporary theoretical analysis flows more directly from the contributions of Chamberlin, Hotelling and the nineteenth-century economists, Cournot and Bertrand. Nor is it true that his approach to the subject has attracted universal approval even among neo-classicists (an appellation to be defined in due course); even from the early days, Bain and others belonging to what is sometimes loosely referred to as the Harvard School, have been the subject of persistent criticism from other notable economists, especially those such as Stigler, Director, Friedman and Demsetz emanating from Chicago. Moreover, we shall not argue that Bain is one of the theoretical giants of the subject; apart from highlighting the role of entry barriers and the notion of limit pricing, his impact on the analytical armoury of I.O. theory is modest. Rather, his historical role was to popularise a framework or paradigm within which to approach the subject. This structure-conduct-performance (S-C-P) paradigm provided a definition of the subject matter of I.O. which has prevailed with only minor modification up to the present time. Perhaps more important, it established an analytical and empirical methodology which was to dominate the subject for at least twenty years. This entailed theoretical analysis of one or more of the causal links in the S-C-P trilogy which was typically subjected to empirical testing against large scale inter-industry data, increasingly with the use of econometric techniques. There is a loose analogy which may be made with the impact of large-scale econometric models on the development of macro-economics over roughly the same period.

The preface to Bain's (1959) textbook delineates quite clearly his own interests:

I am concerned with the environmental setting within which enterprises operate and in how they behave in these settings as producers, sellers and buyers. By contrast, I do not take an internal approach, more appropriate to the field of management science, such as would inquire how enterprises do and should behave in ordering their internal operations and would attempt to instruct them accordingly.

Being concerned in the main with the market behaviour of enterprises, I have given major emphasis to the relative incidence of competitive and monopolistic tendencies in various industries or markets. Correspondingly, my primary unit for analysis is the industry or competing group of firms, rather than the individual firm or the economywide aggregate of enterprises. (ibid.: vii-viii)

This is a fairly exclusive vision of the subject which abstracts from what is usually termed 'managerial economics' and also 'the theory of the firm'. Nevertheless it is a definition which survives today under the title of 'industrial organisation' and one which we follow in this volume.³

It is fashionable among modern scholars in this field to suggest that most of the early work in I.O. was devoid of formal theoretical content, preferring instead a more *ad hoc*, even cavalier, approach to theory. A reading of Bain's work and that of his immediate followers tends to confirm that this particular fashion is not without foundations. To be fair, Bain never claimed to be anything other than an avowed empiricist.⁴ He suggests the theoretical foundations of the subject reside in

'modern Price Theory',⁵ by which we take him to mean the work of Clark and Mason coupled with more traditional price theory. In particular he identifies two 'levels of assumptions', the first concerning the demand and cost setting and the motivation of firms, and the second the influence of market structure. The world is one of downward-sloping demand curves, L-shaped cost curves and profit-maximising firms, and against this backcloth 'market structure may logically be expected (and is observed) to influence the conduct of firms in maximising profits, the interaction of the conduct of competing firms in the same market, and the end performance emerging from the industry' (ibid.: 27). This quote captures the essence of the paradigm. Bain identifies three elements of structure from received theory:

1. The *concentration of sellers*. This is associated closely in his view with the scope for collusive restraint.
2. The degree of *product differentiation*. Differentiated products entail downward-sloping demand curves at the firm level and thus at least 'a little bit of monopoly power' in even large number industries. Equally important, differentiation is typically associated with advertising, and this means an additional policy variable (or element of conduct) in the oligopoly arena.
3. The *condition of entry*, by which is meant 'the degree to which established firms can persistently elevate their prices above minimal average or competitive costs before making it attractive for new firms to enter' (ibid.: 33). This is expected to influence conduct and performance by placing limits on the price obtaining, either because incumbent firms deliberately limit price or because the supply of new entrants depresses price if incumbents do not.

Looking back a quarter of a century later, his explanations of how these elements of structure determine the behaviour or conduct of firms do appear *ad hoc* and imprecise. To be fair, however, he was well aware that the theory of product differentiation was underdeveloped and he himself, along with Sylos-Labini (1956), was responsible for some advances in the theory of limit pricing. But on the central issue of concentration and collusion, we are left with vague assertions that the likelihood of the latter increases, the higher is the former.

Whatever his theoretical limitations, Bain can certainly claim to have been highly influential with respect to empirical methodology:

I have abandoned, in the main, the common approach of studying all these things together in a series of separate industry studies. Although the industry-study approach has a demonstrated entertainment value, it is seriously deficient in that it encourages a casuistic process of 'generalising from a single instance'; correspondingly is prone to engender confusion between accidental associations and fundamental tendencies toward association; nearly always deals with too many parameters and variables for effective analytical handling; and provides no straight or passable road toward scientific generalisations. (ibid.: viii)

This signalled a major break in the prevailing methodology of the subject – case studies were out, to be replaced by large-scale comparative studies of inter-industry differences. To those economists schooled in the last twenty years, Bain's comments will sound familiarly like a call for the econometric methodology. His own work largely pre-dated the econometric revolution and if we suggest that it now appears a little crude technically this is hardly a criticism. Neither do we wish to underplay his painstaking efforts in data collection in a period when official Census data was far less comprehensive than today. The 1960s represent a period of rapid diffusion of structure–conduct–performance and the econometric approach, especially in the US. The research output of this period is brilliantly surveyed in Scherer's (1970) textbook. This summarises the developments made in the subject with an awe-inspiring knowledge and command of the literature, skilfully intertwining and interpreting other case study and cross-section evidence. The strength of this text does not lie in any great originality; but as a summary of the work of a generation of economists working in a specific field, it is an invaluable bible. Figure 1.1, reproduced from Scherer, illustrates two important features of the post-Bain literature. Firstly, the various elements of S–C–P had been noticeably widened. Especially significant, the determinants of two elements of conduct, advertising and research and development, had generated substantial bodies of literature. Secondly, upward pointing arrows indicate a realisation of reverse causalities: not only does structure affect conduct, but also conduct, say advertising, may influence structure. The system had become simultaneous, reinforcing our analogy with the macro model.

This, then, was the heyday of S–C–P, but with a notable change in emphasis from Bain's original conception. Scherer expresses it as follows:

Bain stresses the formulation of direct empirical links between market structure and economic performance, deemphasising intermediate conduct. Only one chapter out of fifteen in his text is devoted explicitly to the analysis of conduct. In this volume, by contrast, much of the analysis in Chapters 5 through 16 attacks the question of structure–performance associations by focusing intensely on the business conduct which spans those phenomena. If the difference in approaches had to be characterised by means of labels, it could be said that Bain is predominantly a structuralist, while the author of the present volume is a behaviorist.

(Scherer: 1970)

This comment on Bain is undoubtedly justified; moreover Scherer's greater attention to conduct reflects a general trend within the literature of his time. It is noticeable, however, that most of the exciting developments during the 1960s were of an essentially empirical nature. There remained a distinctive shortage of what might be called custom-made theory; the analytical backbone of Scherer's survey resided in simple models of price leadership, some developments of limit price theory, and informal (but perceptive) discussions of the conditions facilitating or militating against collusion. While Scherer does include an intuitive discussion of game theory, this is relatively primitive in the light of what was to come.

Both Bain and Scherer were anxious to relate their subject to policy issues

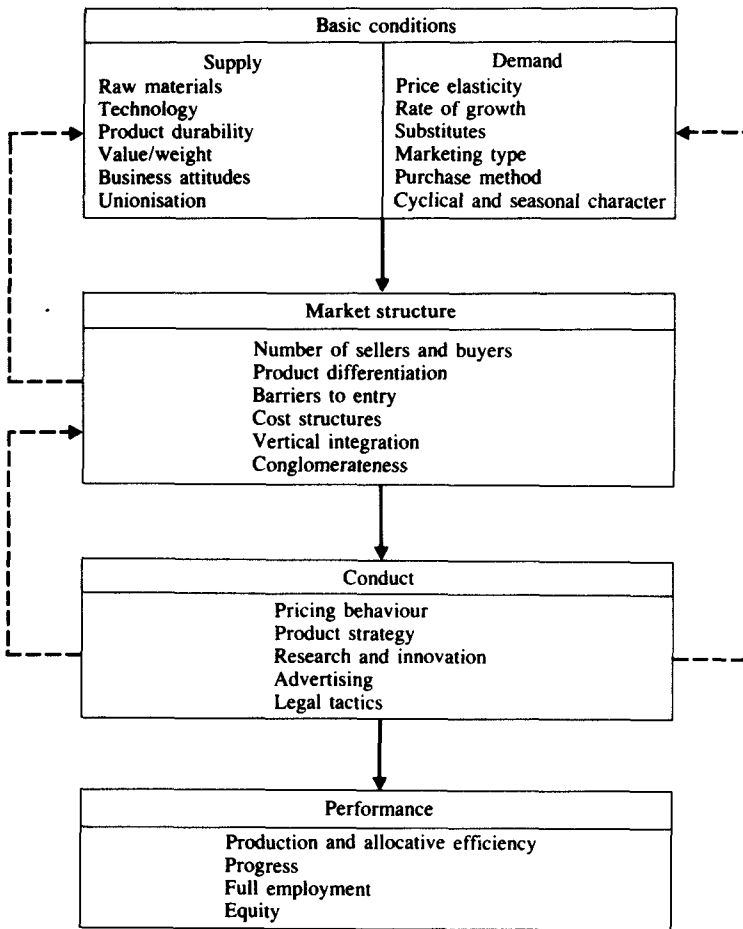


FIG. 1.1

Source: Scherer 1970, p. 5

and given the underlying assumption of the paradigm, that collusion and concentration are closely connected, it carries the undeniable presumption against monopoly power and big business. Nevertheless, Scherer in particular was anxious to signal an open mind:

Readers seeking a precise, certain guide to public policy are bound to be disappointed by this survey, for we have found none. The competitive norm does seem to serve as a good first approximation, but it is difficult to state a priori how much competition is needed to achieve desirable economic performance, nor can we formulate hard and fast rules for identifying cases in which a departure from competition is desirable.

(ibid.: 38)

In the introduction to this section we briefly mentioned dissenting voices

from Chicago. Indeed, one can identify a Chicago School in this subject, as in others. At the heart of many of their criticisms is an unmistakable call for I.O. to return to what might be termed the basics of neo-classical price theory. For example, throughout the 1960s a debate unfolded concerning the value, or otherwise, of Chamberlin's model of monopolistic competition (associated with the Harvard School). The gist of the Chicago critique was that this model offered little if nothing in predictive ability beyond the traditional model of perfect competition. In such circumstances, they argued, it is better to adopt the simple theory rather than the one with apparently more realistic assumptions.⁶ However, there was also an obvious dislike of empirics without theory. For example, Stigler, in his famous explanation of why I.O. had evolved as a subject separate from price theory, suggests that 'much of its literature has been so nontheoretical, or even antitheoretical, that few economic theorists were attracted to it' (1968: 1). Another characteristic of the Chicago School is its more benign view of market outcomes, even if they involve monopoly and concentration; Demsetz, for example, argues that concentration is more often than not the result of efficiency.⁷ But a sweeping identification of Chicago with the defence of Big Business is probably unjustified: for instance, Stigler's (1968: Ch.5) elegant formalisation of the reasons for expecting collusion to be facilitated by concentration has become an influential part of the anti-trust case.

Academic research in the early and middle 1970s continued very much in the tradition of the work summarised by Scherer, though there was a growing unease at what some would call catholic and others call *ad hoc* theorising behind the new empirical work. This gave rise to an approach which is perhaps most appropriately described as 'empirically driven theory'. By this we mean research with an ultimate empirical objective, but based on explicit theoretical model-building designed to establish a formal relationship between the variables concerned; the theoretical model is then used to guide the specification of estimating equations. The most obvious example of this approach concerns the relationship between price-cost margins and concentration which is described in section 1 of Chapter 3. This movement towards empirically motivated theory was joined in the late 1970s by a growing number of mathematically trained economists who were interested in the theoretical problems of I.O., but not necessarily in the specification of econometric work. The latter movement has become known, somewhat controversially, as the 'New I.O.' Such was the confidence generated, that by 1984 Waterson was able to write an I.O. textbook entitled *The Economic Theory of Industry* which included only one 'afterthought' chapter on empirical work.⁸ Such is the speed with which this literature is expanding, that Waterson is already out of date!

We have suggested that the term 'New I.O.' is controversial. This is largely because those who have worked in the subject for many years sometimes find little new in the analytical results, and they probably find the claims for originality hard to take. This may be understandable, but it is our view that the rigour of the new I.O. compared with the catholicism of earlier years is sufficiently different in emphasis to warrant some sort of distinctive label, even if some would wish for an alternative adjective.

Rather than Clark, Mason and Bain, the roots of the new I.O. lie in the work of Cournot (1838), Hotelling (1929), Chamberlin (1933), von Stackelberg (1938), and Schelling (1960). Characteristic features are that the mode of analysis is mathematical and often couched in game-theoretic terms, and the treatment of economic welfare is usually explicit. Furthermore, and probably most importantly, the lines of causality outlined in Fig. 1.1 are explicitly replaced by a methodology which specifies the conduct of firms in terms of an *equilibrium concept*. An example of an equilibrium concept is that used by Cournot: the output rates chosen by firms constitute an equilibrium if, given the outputs chosen by rivals, no firm can improve its own profits by altering its output. Alternatively, if price is the decision variable, then price should replace output in the last sentence; this is often called a Bertrand equilibrium. More generally, output or price may be replaced by any other decision variable and the same basic idea of each firm optimising given the strategy of rivals is known as a Nash equilibrium.⁹ Other more cooperative equilibrium concepts do exist, but the non-cooperative Nash equilibrium is central to most of the new I.O. to date. This choice may, regrettably, have as much to do with ease of modelling as with any fundamental belief in non-cooperative behaviour. Nevertheless, Nash equilibria have provided some particularly fruitful results. A typical methodology in the new I.O. is to specify the initial conditions and equilibrium concept as exogenous, then investigate: (i) the social optimum in a planned economy (where policy makers have access to the same information as industrialists); this is then used as a yardstick to compare with (ii) the market equilibrium (price, output, investment, or whatever is endogenous) when firm numbers are fixed; and (iii) the long-run equilibrium structure and performance when firm numbers are variable. The basic idea is illustrated in Fig. 1.2. Compared with Scherer's Fig. 1.1, both the initial conditions and mode of conduct are exogenous and there is no feedback. Market structure is also treated as more centrally endogenous. Four examples, each of which is developed more fully in later chapters, provide a more concrete introduction to the new I.O.

1. *Product differentiation* as a concept has run through the literature since before Bain's time, yet its exact meaning has often been left implicit and very vague. Explicit modelling has revealed a sharp distinction between vertical and horizontal differentiation. Vertical differences between products are those which we normally

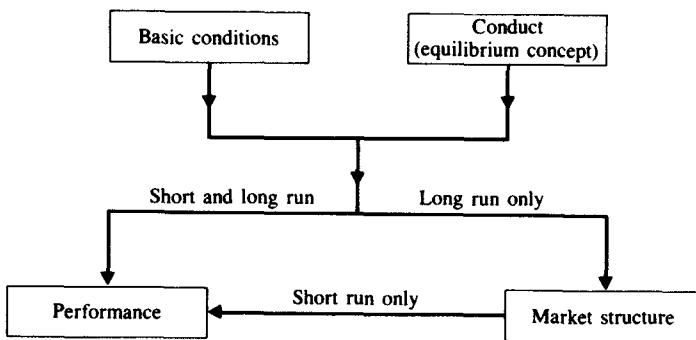


FIG. 1.2

associate with quality. Thus, most consumers will agree that, *ceteris paribus*, a computer with more memory is better than one with less, and will be willing to pay more for the former. Other, horizontal, product differences are not uniquely ranked by all consumers. For instance, farmers with differing types of soil will prefer different types of fertiliser; alternatively, young consumers may value the variety of sweets available at the local shop while being unable to claim that any one is best for their tastes. It turns out that these two types of horizontal differentiation, as well as pure vertical differentiation, can be used to specify different sets of initial conditions and, using the same Bertrand–Nash equilibrium concept, generate very different market equilibria and implications for social welfare. For instance, Shaked and Sutton (1987) show that markets in which firms can differentiate their products vertically by incurring increased fixed costs are likely to exhibit a greater concentration of sellers than are those in which horizontal differentiation is prevalent. Furthermore, the long-standing idea that monopolistic competition leads to too many products being produced at too high a cost is shown to rest on very shaky foundations (Spence 1976; Dixit and Stiglitz 1977; Lancaster 1979; Salop 1979).

2. Cournot's duopoly model and the associated analytical tool of *reaction functions* can be applied to investigate entry barriers simply by labelling one duopolist as the potential entrant and the other as the incumbent firm. This approach is followed by Dixit (1979, 1980), who is able to elucidate a number of issues within a unified framework. For instance, under what conditions will the incumbent prefer to passively accept entry rather than adjust this behaviour to keep the entrant out? Does product differentiation leave potential entrants at a disadvantage or does it aid entry by opening up market niches? And under what conditions can the incumbent credibly threaten retaliation against an entrant? It is the last of these questions that has gained most attention in the literature and two important concepts have emerged as crucial to an understanding of strategic behaviour by firms. First, the advantage obtainable by incumbents depends on their ability to make commitments to the market, and these depend crucially on *sunk costs*, that is costs which cannot be recovered once they have been spent (for instance, advertising but not office space which can easily be re-sold for alternative uses). Second, a great deal of important information on the competitiveness of the market is summarised in the slope of the reaction curves, and this provides a unifying framework for investigating many apparently diverse oligopoly problems (Bulow, Geanakoplos and Klemperer 1985; Fudenberg and Tirole 1985; Dixit 1986; see also Lyons 1987 for a simple introduction).

3. Recent work on *contestable markets* has sought to provide an alternative benchmark market structure to the unrealistic notion of perfect competition. If there are no sunk costs, if there exists at least one potential entrant who could produce exactly the same product lines as the incumbent, and if the equilibrium concept is such that the entrant can undercut the incumbent for long enough to be able to sell her desired output, then we have what is known as a perfectly contestable market (Baumol 1982). Even if there are substantial economies of scale and/or economies of scope, provided there are no sunk costs, there can be no exploitation of monopoly power, as a price in excess of average cost