

WELFARE ASPECTS OF INDUSTRIAL MARKETS

NIJENRODE STUDIES IN ECONOMICS

Volume 2

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Welfare aspects of industrial markets

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A. P. Jacquemin and H. W. de Jong

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INTRODUCTION

The present volume of essays on industrial organization, which are based on conferences held at Nijenrode and Brussels, differs considerably from its predecessor. Even more than in the first volume the essays demonstrate the broad scope of industrial organization analysis. Besides the traditional topics such as economies of scale, monopoly and competition policy, there are essays on methodology, on stagflation, and on the relationship between industrial structure and international trade and trade policies. The latter topics are of growing importance. The issue of restructuring industries and the question of whether free trade or some measure of protection is more appropriate are topics of increasing relevance today (and will no doubt continue to be in future years as well). The problem of persistent inflation which other essays address is also of major concern.

Apart from being broad in scope and venturing into new fields, this volume is also controversial. Its central feature is a debate about welfare aspects. Here, more than in pure analysis, economists tend to entertain different points of view. One of the participants in the Nijenrode conference, Professor John Blair, who died in December 1976 and whom we honour as having been an active promoter of this kind of meeting, wrote to the editors shortly before his death to say that the first volume had succeeded very well in acquainting the reader with the results of empirical investigations, notably on trends and levels of concentration. In contrast, many of the papers given in the 1976 conferences are policy-oriented and controversial. Presumably, one of the goals implicit in their controversial nature is to get nearer to the 'truth' by subjecting each argument to the searching criticisms of those who know their subject and who can detect weakly-based empirical and deductive statements. By so doing, one can discover other aspects of the problem and verify whether the policy conclusions correspond to the evidence and analysis presented. In this perspective, two aspects of the papers warrant particular attention. First, the usual skepticism of specialists in industrial organization towards the validity of general welfare statements is reinforced by the various contributions which highlight the complexities

introduced by information costs, uncertainty, external effects and the dynamics of the structure-conduct-performance relationship. These aspects are shown to be more important than the traditional considerations, such as scale economies. Often the trade-off between competition and efficiency is a multidimensional problem; certainly it is a more complex issue than is implied in the way it is presented in static models. Second, there are great dangers in making simplistic comparisons between the performance of firms or industries in different countries, such as in the U.S. and E.E.C. countries. Distinguishable objectives and constraints, including items such as different managerial attitudes toward risk or the degree of industrial dependance upon international trade, can make behaviour or policies considered inefficient in some countries acceptable in others.

The following comprises some of the welfare controversies which arise in this volume.

In the first section, Roger Sherman, after briefly tracing the roots of industrial organization theory, focuses attention on a very old theme developed, among others, by Plato and (more recently) by Luigi Pirandello, that appearances may not be representative of reality: 'things are not always what they seem.' In industrial organization this has recently come to mean that risk, randomness and imperfect information play an important role in modifying the conclusions of neo-classical theory. He shows the broad scope of such considerations, particularly in a world dominated by large firms; and he draws attention to their implications for our theories. Sherman ends on a relatively optimistic note, suggesting 'a new wave of research', anticipated as being 'based on a theory that is more powerful because it omits less.' However, in his paper, Shepherd maintains that current theory leaves the dominant firm an unsolved puzzle. He emphasizes the analysis of the rate of change of market share (or its absence) of those firms. Exploring the causes and effects of the 'hardening of market structures' (at least in the U.S. economy), Shepherd also puts emphasis on how the firm is owned and controlled and what incentives can be used to alter manager behaviour. This fits in well with the third article in this section, in which Silberston and Nyman raise the question of ownership control versus managerial control of large firms. Consistent with other recent studies, they are relatively sceptical of the proclaimed level and trend of managerial control that prevails in large firms. A persisting question is the degree to which the differences reflect different transatlantic situations. As Silberston and Nyman also note, there is room for further research into the implications of ownership types on the welfare of firms.

In the section on stagflation, von Weizsäcker pleads for an appreciation of the importance of short-term price rigidity. Far from contributing to the destabilizing tendencies that occur in perfectly competitive markets which adapt to

upward and downward fluctuations, rigid prices promote specialisation, investment and other beneficial outcomes. Thus, competition policy should not aim at artificially bringing about price flexibility but should have a long-term structural perspective. The reader might wish to compare this argument with Lenel's in favour of price intervention in the first volume (p. 316–23). Heusz uses a different approach to explain the stagflation dilemma: the combination of external technical progress and oligopolistic (or monopolistic) market structure induce inflationary tendencies without securing full employment. Technical change is differentiated into internal and external varieties, the latter gaining in importance as market limitations become increasingly visible in many branches of industry in the industrialized world.

Šoškic underlines the fact that stagflation problems are not absent from a workers' self-managed economy, but are, if anything, more pronounced. He attributes this to the shift in favour of consumption at the cost of accumulation. Therefore, an incomes policy is necessary but it should be differentiated according to the cyclical situation prevailing. This applies to other policies too. Such policies should also aim at measures to push monopolistic organizations into investing in productive output and at the removal of structural disequilibrium.

In the section on international trade and industrial organization a number of interesting current debates are discussed. Caves and Khalilzadeh-Shirazi note some impacts of international trade on the character of product differentiation in industries exposed to and sheltered from trade, and on the scale of plants and the interdependency of oligopolistic sellers. These findings support the time-honoured position that international trade is an important variable sustaining the competitive structure of an economy. William Adams broadens this contention by comparing large European and North American firms. He finds much less support for the allegation that national differences in corporate performance are more important than is commonly assumed. Though company structures are affected by nationality differences, this is less true for performance variables such as cash flow, profitability or company growth. In view of the current European debate on firm profitability, investment and employment, his findings that firm growth 'depends on neither the availability of internal funds nor the proportion of assets actually financed via debts' is especially interesting (the correlation between growth in capital stock and availability of internal funds is extremely small).

The policy question is approached with different views – and with different conclusions – by Adams and Dirlam, on the one hand, and Singh, on the other. The first two authors stress that import controls – or considerations to institute them – raise far more questions than can be answered, and that a necessary

consequence of quotas is the obstruction of competition. They point out that the latter would have to be stimulated in order to restrict the welfare loss. Both alternatives – dictatorial regulation and government ownership of the industry in question – introduce rigidities and inefficiencies, and present-day governments would do well to ponder the alternatives before adopting import restrictions, measures which are now *en vogue*. Ajit Singh approaches the question from the point of view of Britain's deplorable economic performance, especially during the past ten years. In the U.K. there is less and less employment in manufacturing, though output continues to increase. Such de-industrialization exhibits the increasing failure of the manufacturing sector to pay for the growing imports by means of exports. Thus, the structure of U.K. manufacturing production needs adaptation. Though he leaves open the ways in which this can be achieved (he even suggests that the participation of the U.K. in the international economy might have to be reconsidered), Singh rejects the likelihood of a spontaneous correction by means of market forces. Also, the discussion in this paper on the relative welfare aspects of the manufacturing industry and services merits attention in view of the increase in the latter sector in nearly all western European countries.

The next section continues the debate about the virtues of competition, but from a different angle. The advantages flowing from economies of scale and monopoly come under review. Williamson continues the discussion of economies of scale as a defense in merger cases which he initiated in his article in the *American Economic Review* in 1968. His argument is that from an economic point of view, such economies should be taken into consideration, although he is not inclined to overrate their importance. Revisiting the field, he finds some signs of hope in the context of U.S. policy. For European readers, the subject is also of importance, not so much because the economies' defense has to be advocated, but because of the various considerations which have come to play a role in the trade-off between efficiency and competition (which are not always explicitly taken into account).

Hjalmarsson introduces dynamic efficiency aspects. He restricts these to the reductions in production costs due to an optimal path of capacity expansion by firms producing homogeneous goods in multiplant structures. He rightly says that this makes economies of scale an endogeneous concept imparting a dynamic impulse to industry development. This is a worthwhile first attempt, but one which has to be rounded out by other considerations. The dynamics of demand and price remain of obvious importance in a market economy.

Two papers follow on economies of scale from a comparative international perspective in industries (iron and steel and brewing) where economies of scale are said to be important. These empirical studies, by Carlsson and Cockerill,

emphasize that economies of scale do play a role, but that they are only one factor (and sometimes not the most important one) in an overall assesment of the industry's performance.

The book concludes with a section on the various anti-trust and industrial policies. Apart from evaluations of U.K. and French competition policies which stand as eminent long-period surveys, these essays deal with the roles of the capital market in steering efficient industrial development and of public utility pricing. Like the empirical studies, the policy surveys have limited application: that is, to the countries and time periods to which they are relevant. Nevertheless, it remains instructive to compare experiences among countries and to benefit from their strengths and weaknesses by taking them into account when pursuing policies in other nations.

To sum up, the collection of papers presented here shows a number of dichotomies: some papers show a neo-classical approach; others diverge rather strongly therefrom. Some papers are theoretical in nature; others are mainly empirical. Some papers aim at policy conclusions or have an implicit policy message; others abstain from such recommendations. The reader can explore for himself the various combinations. By doing so, he will see how these papers broaden the traditional analytical framework used by industrial organization economists and how they provide alternative explanations for the differences among countries in their policies with respect to market structure and firm conduct and performance.

A. P. Jacquemin

H. W. de Jong

PART ONE

THEORETICAL AND STATISTICAL QUESTIONS IN INDUSTRIAL ORGANIZATION

I. THEORY COMES TO INDUSTRIAL ORGANIZATION

Roger Sherman

1. Introduction

Industrial organization knowledge is largely empirical. Some of the empirical findings tend to confirm implications from neoclassical economic theory, but the theory rarely has offered exact hypotheses. Predicted effects of explanatory industry variables could be specified in only a crude way from the theory, perhaps that A , B , and C are apt to affect D , and they were usually estimated by ordinary least squares whether that method was appropriate or not. Some of the operational measures for important explanatory variables, such as 'industrial concentration,' also were rationalized in only an informal way, and dependent variables such as industry performance were represented even less adequately. Despite these methodological weaknesses, the hunt for relationships can be defended because available economic theory was unable to predict events any more precisely, and a great many results of interest nevertheless have been found. But empirical evidence without theory cannot yield a compelling understanding of what really is going on. For clear and convincing explanations we need sound and tested theory.

Maybe no unified theory can serve the vast empirical and policy orientation of industrial organization knowledge.¹ Our institution-free, deterministic neo-classical theory repeatedly has been found inadequate to deal with specific industrial organization questions.² Yet the advantages of theory, for keeping track of past discoveries and for generating fruitful expectations about new ones, seems as undeniable here as it is in other areas of research. So this paper discusses the now visible prospect of elaborating our severely deterministic neoclassical model to accomodate risk, dynamic effects and imperfect information from the

1. Joel Dirlam kindly called my attention to the similarity between the title of my paper and the title of Robert Hichen's short story, 'How Love Came to Professor Guildea' (in Dorothy Sayers, ed., *The Omnibus of Crime*, New York: Prayson and Clarke, 1929). After he had lived most of his life without it, love ruined Professor Guildea. To a field that has developed without depending heavily on theory its arrival conceivably might bring similarly drastic consequences.

2. See, e.g., criticisms by Shubik (1970).

real world, rather like adjusting for effects of friction in physics so that pure theories could be tested. Through such elaboration we may understand more of what we see while the unity of neoclassical analysis might also be preserved, even across a broad subject like industrial organization which is concerned generally with how markets work and particularly with how the way they are organized will affect how they work.

The aim will be to express first in admittedly over-simple terms the nature of theory that has been applied in industrial organization, claiming it to be mainly deterministic neoclassical micro-theory with minor bows to oligopolistic interdependence. Then I shall illustrate, in the important case of the size distribution of firms, how crucially important our presumptions about randomness and uncertainty can be to our conclusions. Having brought out the importance of uncertainty I then make a brisk review of recent developments in risk and imperfect information that bear on industrial organization questions, in an effort to show that these efforts offer some promise of accumulating to yield a unified theoretical basis for industrial organization work. Because risk and imperfect information, and other new topics, serve so well to fill in the inadequacies of deterministic microtheory we can anticipate a great deal of theoretical work which promises to give industrial organization economists a genuine basis for testing and accumulating reliable knowledge.

2. The role theory has played in industrial organization

One might say industrial organization is well founded in theory. After all, as a special field of interest in economics it could claim theorist Augustin Cournot (1838) as its founder. He was the first to bring out how price is affected by the structure of a market, with structure varying all the way from monopoly at one extreme to competition at the other. Or we could date the field from Adam Smith (1776), who articulated so well the role of market competition in turning private, profit-seeking motives of individuals to the service of society. Alfred Marshall (1920) later codified the neoclassical theory, and provided many suggestions about the role of risk and other market imperfections. And the many oligopoly theories, plus monopolistic and imperfect competition theories of this century, provided a basis – good or bad – for industrial organization work.

It is really in this century, particularly in the last forty years or so, that industrial organization has become a focus of attention and a special field of economic and legal research. Indeed, the development of industrial organization as a field may be owed partly to the advent of modern times, with its seeming compulsion for specialization and categorization, rather than to any growing

coherence in the subject.³ I think many would agree that development of the field largely followed Edward Mason's call (1939) for empirical work describing crucial characteristics of an industry, to capture them and their effects in a quantitative way. Although prompted by work in oligopoly and the theory of the firm, early empirical efforts, starting for instance with Gardiner Means (1932), Adolph Berle and Gardiner Means (1932, 1968), and Joe Bain (1956), were attempting to describe the world and only partly to test crude hypotheses derived from theory. Yet their results and others like them would be claimed today by many to constitute much of the substance of industrial organization knowledge.

So although to a degree industrial organization can claim respectable theoretical origins, the subject matter does not rest firmly on a broad theory. All empirical efforts have not used theory; empirical work frequently was undertaken to pursue in measurable terms relationships thought to be important because of previous observations, or due to some perceived social problem or great public policy concern. In the USA, where I think it is fair to say this empirical work in industrial organization started, two forces can be seen posing problems and issues. One is a traditional academic one, where implications from the body of economic theory, largely deterministic neoclassical theory, were to be tested. The other is the world of affairs where public disputes simply had to be settled, disputes such as whether the Alcoa aluminum company was a monopoly in violation of an antitrust law, whether the marketing of milk was to be regulated, or more recently whether government certification of safety is to be required and if so whether to stop with food and drugs, rotary lawnmowers, glass shower doors, toilet seats, or whatever.

Rather than review the empirical findings, which have been summarized recently by several different authors,⁴ I shall note briefly the theory underlying the early empirical work. Where theory was used it often relied on the assumption of static certainty, although imperfect information was acknowledged. The implications emphasized and, in turn, the variables chosen for observation, were drawn from neoclassical theory. Going back to Alfred Marshall (1920) there has been a faith that broad averages, typified in the analysis of 'representative' firms and consumers, would be captured sufficiently well by a deterministic theory that such a theory could serve practical ends. Based on this theory a crucial role could be seen for the condition of entry, and that role was emphasized when Bain (1956) saw imperfect information working to handicap new entrants and sought to measure effects by considering the sum of money a new entrant would have to

3. The words, 'industrial organization,' may have come into use in the late 1940's partly because John Blair created that official job title for economists employed by the US Government.

4. See Blair (1972), Philips (1971), Scherer (1970), Sherman (1974), Vernon (1972), or Weiss (1971).