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The Contributions of Industrial Organization To Strategic Management[1, 2]

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The traditional Bain/Mason paradigm of industrial organization (IO) offered strategic management a systematic model for assessing competition within an industry, yet the model was seldom used in the business policy (BP) field. IO and BP differed in their frames of reference (public vs. private), units of analysis (industry vs. firm), views of the decision maker and stability of structure, and in other significant respects. Development of IO theory during the 1970s has narrowed the gap between the two fields, to the extent that IO should now be of central concern to policy scholars.

The majority of economists studying industrial organization (or industrial economics) and strategic management researchers have, over the years, mostly viewed each other with suspicion—if they knew each other existed. With few exceptions, industrial organization had little effect on the business policy concept of strategy, and business policy had little effect on industrial organization, despite the increasingly clear evidence that much promise for cross-fertilization existed. Why these ships have passed in the night is an intriguing question. Some of the reasons reflect subtle, deep-rooted suspicions and even the type of training that scholars in both fields traditionally received. But many of the reasons reflect real underlying differences in the purposes, frame of reference, unit of analysis, and research values that each field has traditionally embraced.

It is becoming recognized today that industrial organization can offer much to the analysis of strategic choices by firms within industries, and the contribution is growing rapidly as new research breaks down the differences to which I have alluded. In addition to analytical techniques, industrial organization is bringing a new methodological tradition to bear in research on strategic management, one that holds promise of contributing to the development of the policy field in a way quite distinct from the purely conceptual. And the benefits are clearly flowing in both directions—exposure to business policy concepts is having a decidedly positive influence on industrial organization research and, recently, on microeconomic theory.

In this article, I will examine some existing and potential contributions of industrial organization to strategic management, especially to the formulation of competitive strategy in individual industries. I will take a quasi-historical approach, in order to examine why the traditional industrial organization paradigm, while offering a valuable tool, made relatively few inroads into the policy field. Making the reasons for this explicit will highlight some of the conceptual underpinnings and assumptions of both fields and provide a framework for seeing why new industrial organization research is having an impact. The reasons will also provide an agenda for where future progress must be made if the true promise of industrial organization is to be realized.

Most of my attention will be addressed to strategy for competing in an individual industry or at the so-called strategic business unit level, because this is where industrial organization can have the greatest

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[2] This article represents a revision of a paper presented at the annual meeting of the Academy of Management, Atlanta, August 1979.

impact. When considering the contribution of industrial organization to strategy formulation at the level of the diversified firm as a whole, I will clearly note the shift in frame of reference.

The Promise of the Industrial Organization Paradigm

The traditional industrial organization paradigm has long held tantalizing promise for strategy formulation. This is clear when one examines the Learned, Christensen, Andrews, and Guth (LCAG) framework that has become the foundation of business policy [1969; Andrews, 1971]. LCAG defined strategy as how a firm attempts to compete in its environment, encompassing key choices about goals, products, markets, marketing, manufacturing, and so on. The goals of the firm were broadly conceived to encompass both economic and non-economic considerations, such as social obligations, treatment of employees, and organizational climate. Effective strategy formulation from a normative standpoint, according the LCAG, entailed relating the four key elements shown in Figure 1.

The successful firm had to match its internal competences and values to its external environment, and LCAG offered a series of general but logically compelling consistency tests that could help a firm probe its strategy to see if it truly related these elements. These consistency tests stressed the need for a firm's policies in each functional area to be interrelated as well as the need for the entire group of functional policies to make sense, given the environment. The high-performing (high return on investment) firm in LCAG's framework was one that had found or created a position in its industry where such consistency was present. However, LCAG offered no help in assessing the "contents" of each of the boxes in Figure 1 in a particular situation. This was left to the practitioner.

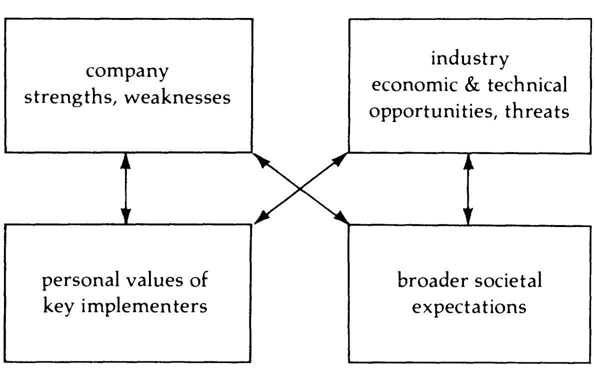

Figure 1
The Four Key Elements of Effective Strategy Formulation

The concept of strategy emerged from the need to help the practitioner (particularly the general manager) transform the daily chaos of events and decisions into an orderly way of sizing up the firm's position in its environment. As a result, the early policy literature on strategy formulation subsequent to LCAG was largely process oriented, translating the basic LCAG paradigm and extensions of it into a sequence of logical (and very general) analytical steps [e.g., Ansoff, 1965]. Recently, quite a bit of research has sought to identify broad categories of factors that might be considered in assessing the contents of each box in Figure 1 and has offered generalized strategic alternatives and some of their pros and cons [Cannon, 1968; Hofer & Schendel, 1978; Uyterhoeven, Ackerman, & Rosenblum, 1977]. Another recent thread of research has been examining the social, political, and organizational processes by which strategic choices are actually made [Bower, 1970; MacMillan, 1978].

Empirical research on the substance of strategy formulation in individual industries (as distinguished from the administrative processes by which firms arrive at strategies) has been rather limited until recently. This is no doubt in part because the broad concerns of policy researchers have encompassed the complex role of the general manager, of which strategy formulation is but a part. Much policy research attention has been given to the more administrative dimensions of the general manager's job, such as relating organizational arrangements to strategy, resource allocation processes, strategic planning systems, and the like.

In the backdrop of the LCAG paradigm and subsequent work, the traditional Bain/Mason industrial organization (IO) paradigm of the 1950s and 1960s held obvious promise at one level. The essence of this paradigm is that a firm's performance in the marketplace depends critically on the characteristics of the industry environment in which it competes. This is expressed in the familiar structure-conduct-performance framework shown in Figure 2.

Industry Structure ⟶ Conduct (Strategy) ⟶ Performance

Figure 2
The Traditional Bain/Mason Industrial Organization Paradigm

Industry structure determined the behavior or conduct of firms, whose joint conduct then determined the collective performance of the firms in the marketplace [Bain, 1968; Mason, 1953]. *Performance* was defined broadly and in the economist's sense of social performance, encompassing dimensions such as allocative efficiency (profitability), technical efficiency (cost minimization), and innovativeness. *Conduct* was the firm's choice of key decision variables such as price, advertising, capacity, and quality. Thus, in policy terms, conduct could be viewed as the economic dimensions of firm strategy. Finally, *industry structure* was defined as the relatively stable economic and technical dimensions of an industry that provided the context in which competition occurred [Bain, 1972]. The primary elements of structure identified as important to performance in the early IO research were barriers to entry [Bain, 1956], the number and size distribution of firms, product differentiation, and the overall elasticity of demand [Bain, 1968]. A final crucial aspect of the Bain/Mason paradigm was the view that because structure determined conduct (strategy), which in turn determined performance, we could ignore conduct and look directly at industry structure in trying to explain performance. Conduct merely reflected the environment.

An important branch of IO research was so-called oligopoly theory, or the study of the outcome of competitive interactions in markets where one firm's actions affect its rivals (for a survey, see Scherer [1970]). Oligopoly theory sought to specify the link between industry structure and firm-to-firm rivalry, providing a rich set of determinants of the difficulty firms face in coordinating their actions in the marketplace (for the classic analysis, see Fellner [1949]). It filled a gaping hole for the analysis of real markets that had been left by economists' traditional exclusive focus on the polar cases of pure competition and pure monopoly. Game theory, born at nearly the same time as the Bain/Mason paradigm itself, introduced a potentially rich framework for examining competitive interaction [Schelling, 1960; Van Neumann & Morganstern, 1953]. Game theory took its place in IO as a part of oligopoly theory.

The early Mason approach of identifying many structural factors that were important in influencing conduct and performance soon gave way to the Bain-initiated focus on a few key aspects of structure. Bain also pioneered an empirical tradition of statistical studies relating aspects of industry structure to conduct and performance (usually profitability). Literally hundreds of studies of this form, drawn from large samples of industries all over the world, have formed the backbone of IO literature.

The Bain/Mason paradigm of IO (enriched with oligopoly theory) is a useful contribution to strategy formulation in an industry, though it has been a little-used one. It offers a systematic model for assessing the nature of competition in an industry—one aspect of the four-part LCAG framework: industry opportunity and threats. Identifying the "structure" of the industry in IO terms casts the spotlight on the crucial aspects of the firm's industry environment, and illuminates such critical concepts as barriers to entry and demand elasticity. The model also allows an analysis of the performance a firm could hope to achieve in its industry. It reinforces the important point that not all industries are equal in terms of their potential profitability. Thus the model can help firms predict a level of performance that can reasonably be expected. Unlike the ad hoc approach to industry analysis embodied in most policy literature, Bain/Mason is potentially a systematic and relatively rigorous one backed by empirical tests. Bain/Mason does *not* help the strategist with the other three key elements identified by LCAG, but it clearly helps with one.

The Limitations of The Bain/Mason Paradigm

Faced with this clear promise, why didn't strategy teachers and practitioners stumble over each other to embrace the new IO paradigm? A number of the reasons relate less to the substance of the paradigm than to the scholarly traditions in the IO and BP fields. Many policy scholars were not aware of developments going on in IO, a different, selfcontained discipline with its own jargon. Some policy scholars also seemed to be innately suspicious of economists' work, possibly because of their exposure to microeconomics at the introductory level, where it is laced with assumptions and normative premises that practitioners cannot live with. And there was little cross-fertilization between people; either one was an economist, or one was a business teacher or practitioner.

However, these reasons may have been only reflections of more fundamental, substantive reasons why well-informed policy practitioners *should have been* skeptical of IO. Some important ones are outlined below:

There were translation problems owing to different frames of reference. Policy practitioners were interested in improving a firm's performance from a private viewpoint, which meant increasing return on investment (ROI). IO researchers were motivated to improve performance from a social viewpoint—which could mean *reducing* ROI to the purely competitive level. IO was historically oriented toward informing public policy, and the literature was written from this frame of reference.

As I have argued, the IO explanation of industry competition and performance could clearly be applied to *either* purpose—private or social. For example, public policymakers could use their knowledge of the sources of entry barriers to lower them, whereas business strategists could use theirs to raise barriers, within the rules of the game set by antitrust policy. However, this fundamental difference in the frame of reference meant that IO theory had to be translated before it appeared compatible with the private perspective and thereby recognizably useful to policy practitioners. This translation was not made in the literature.

Policy teachers and practitioners also had a different definition of their task. Policy aimed at understanding the multiple functions and multiple objectives of the general manager, only some of which were purely economic. IO theory focused much more narrowly on the economic bases of competition.

IO differs in its unit of analysis and related assumptions. Policy practitioners have been vitally interested in the problems of the individual company, and have viewed each firm as a unique entity with unique strengths and problems. Terms such as "distinctive competence" are hallmarks of the policy field in defining the bases on which firm strategies should be set. Simple observation clearly revealed that firms differed a great deal in performance even though they competed in the same industry.

Conversely, the IO theory of the 1950s and 1960s took the *industry* as the unit of analysis. Mason's early work showed a strong interest in firm conduct but this was largely lost when Bain's influence came to be felt. That it was lost made sense from the traditional IO frame of reference, since it is collective industry performance and not one firm's performance that determines the quality of resource allocation in the economy and hence social performance. Furthermore, IO theory implicitly assumed that all firms in an industry are identical in an economic sense, except for differences in their size; the other differences are random noise. As a result, there was little room for stable differences in the performance of firms in the same industry. Therefore, while IO is useful for determining the likely average profitability of an industry, in its traditional form it clearly is not very useful for sorting out the different performances of different companies.

IO and BP have different views of the decision maker. IO, by and large, viewed the firm as a single decision-making unit making choices based on economic objectives. Some IO literature recognized that firms were really collections of individuals [Cyert & March, 1963], and there was some discussion of objectives other than profit maximization, but these isolated efforts were hardly integrated into mainstream theory. Policy practitioners, on the other hand, placed great stress on how the personality of the leader, political processes within the firm, and a broad range of possible firm objectives have a major impact on a firm's actual behavior in the market place (a good illustration is Bower [1970]). Also, BP has always stressed the difficulty leaders have in correctly perceiving environmental change, and the long-standing assumptions that often create strategic myopia. The human dimension was central in BP; no humans were visible in IO.

IO views the firm as a free-standing entity. IO has implicitly viewed the firm as a free-standing entity competing in a single business, and the IO literature on diversification is largely distinct from the literature on competitive outcomes in oligopolistic markets. Yet policy practitioners have long recognized that the individual business unit is often but one part of a diversified firm's "portfolio" of businesses, and that the needs of the corporation as a whole often strongly affect the objectives of the unit as well as the resources made available to it. All this can strongly influence market outcomes. Furthermore, manufacturing, marketing, distribution, and research costs are often shared among related business units in a firm even though they are in distinct industries from the viewpoint of conventional approaches to industry definition. For example, a firm might manufacture motors it then uses to manufacture such disparate products as hair dryers and cooling fans. Its costs in motor manufacturing, a key part of its end-product costs, are thus in part determined by the total sales of these essentially unrelated products. To handle such shared costs, many firms must formulate strategy both at the individual business unit level and for the entire group of related business units.

Related to the failure to see the business unit as part of a centrally managed corporate portfolio is the failure of much IO research to recognize the simultaneous determination of firm behavior in such areas as advertising, research, and vertical integration. The concept of strategy stresses the need to interrelate these individual functional policies. IO research has tended to look at each function in a piecemeal fashion.

IO had a static perspective. The Bain/Mason paradigm of the 1950s and 1960s was a static, cross-sectional one that sought to explain the industry performance that resulted from a given industry structure. Structure was definitionally stable. Although the static model is a useful one as far as it goes, policy practitioners are used to having to cope with changes in structure. Concentration rises and falls, as do entry barriers and the other measures of structure identified in the IO paradigm. It is these structural changes that seem to raise the most fundamental strategic problems for firms in competition. A key question from the policy viewpoint, unanswered in Bain/Mason, was what made structure what it was, and what did one do about changes in structure from a strategic standpoint?

Determinism was an element of IO theory. Traditional IO theory took industry structure as exogenously given, and held that the firm's strategy and performance were fully determined by this structure. Thus the firm was stuck with the structure of its industry and had no latitude to alter the state of affairs. Policy practitioners, on the other hand, have long observed that firms can fundamentally *change* the structure of their industries through their actions. The policy field has a long tradition of emphasizing the insight, creativity, and even vision that some firms have exhibited in finding unique ways to change the rules of the game in their industries.

However, firms cannot always change industry structure, and thus understanding industry structure in the traditional IO sense is crucial. Furthermore, one must know what the key elements of structure are before one knows what to change, so that the traditional IO model is an important place for firms to start in formulating strategies that change the rules of competition. Nevertheless, the determinism in the traditional IO paradigm was a limitation to weigh against these benefits.

IO was too limited. IO theory identified a relatively few, critical aspects of structure, such as the distribution of firm sizes (particularly concentration) and entry barriers. Policy practitioners, on the other hand, could easily think of examples of other, unmentioned variables that were crucial to strategy in individual industries. This difference was partly a result of BP and IO having different academic traditions. Economists are prone to set forth general categories without fully articulating the subcategories. For example, the concept of entry barriers encompasses a myriad of specific factors, many of which are not elucidated in the usual IO accounts. Policy practitioners are more interested in the "long list" than in the generality of various items. But the problem was more than just stylistic: many relevant structural variables were undiscovered in the IO theory of the 1950s and 1960s.

IO and BP had different loss functions. IO researchers were interested in uncovering structure/performance relationships that generally held true, even if they did not hold in every industry and even if they explained only some of the variation in performance. The desire to advance public policy made such generalization palatable. Uncovering a few statistically robust relationships that could improve competition policy was more important than the risk that a given relationship might not hold or might be unimportant in a particular situation.

Policy practitioners, on the other hand, have always been vitally concerned with each firm's unique situation. It was not acceptable, from their viewpoint, for a particular firm to be the exception and for the hypothesized relationship therefore not to apply. If anything, the focus in policy has been on what makes a particular firm exceptional or unique and thus what might provide the basis for a unique strategy. In statistical terms, then, the loss functions or weights attached to making different kinds of errors in developing theory have been fundamentally different for policy practitioners and IO researchers.

Oligopoly theories were abstract and needed to be translated. A general theory of oligopoly eluded (and still eludes) IO researchers, and established models of oligopoly were built on grossly unrealistic assumptions such as mechanical reaction functions, identical cost and demand functions among competitors, and the like. Game theory, which promised to overcome the deficiencies of using marginal analysis in an oligolopy setting, was articulated using examples not directly taken from industry competition: nuclear war, labor negotiations, or tactical pricing decisions. Game theory therefore required translation to be readily applied to real markets, and suffered from its own set of uncomfortable assumptions, such as the heroic amounts of information utilized by all parties, simplistic strategy options, and one-time games. Testing of oligopoly and game theory concepts was undertaken almost entirely in abstract experimental situations and not actual industries (for a survey of some of the experimental evidence, see Scherer [1970]).

These reasons and others rightly made policy practitioners uncomfortable about embracing IO, but my own view is that even the IO research of the 1950s and 1960s could be highly useful in strategy formulation in industries, provided the translation of the setting and frame of reference is made, and provided it is recognized and accepted that IO is *not* an answer to the broader concern of BP about the general management function. IO offers at least a start toward a systematic understanding of the industry environment, which can always be supplemented with particularistic analysis. IO encompasses some extraordinarily powerful concepts, and game theory offers a framework that can be applied to firm-to-firm warfare. IO does not go all the way, for the reasons discussed, but it goes some of the way.

The New Promise of Industrial Organization

The limitations of IO for strategy formulation that I have discussed are inherent in the classic IO paradigm as personified by Bain/Mason. It is probably accurate to call this a creature of the 1950s and 1960s. Yet IO as a field has continued to develop, aided by increasing exposure to the policy field. In fact, some of the reservations of policy practitioners turned up, not surprisingly, in self-criticisms of the IO field by its own practitioners.

During the 1970s, particularly the last five years or so, IO has been enriched by addressing, at least in a partial way, many of the limitations I have described. Too often IO is criticized as if Bain/Mason were the current state of theory or as if IO consisted solely of Galbraith. As a result of new developments, IO has moved from being a useful tool to consider in strategy formulation to being a field that should take a central place among the conceptual frameworks used in the policy field. Let us review the progress of IO against the limitations of Bain/Mason identified earlier. I must hasten to add that this does *not* purport to be a comprehensive survey, but rather an attempt to provide an overview along with examples of specific research. As one who has been working in the intersection of these two fields, I will undoubtably err in drawing my examples too heavily from work going on at Harvard.

Translation Although they are still relatively few, extensions of the IO paradigm to the perspective of strategy formulation are now in the literature. An early effort was my "Note on the Structural Analysis of Industries," originally written in 1974. A modified version appears as Chapter 1 in my 1980 book, a larger study of the IO/strategy link. In addition, mentions of IO concepts have appeared in other recent books and papers on strategy analysis [Hofer & Schendel, 1978; Kasper, 1979; Thorelli, 1977]. There is a course at Harvard that applies contemporary IO concepts to problems of developing competitive strategy, and similar courses have started or are starting elsewhere.

Unit of analysis In the past decade, work in IO has shifted the unit of analysis to *both* the firm and the industry. Empirical researchers began to examine the performance of firms as well as industries [Demsetz, 1973; Gale, 1972; Shepherd, 1972], though lacking clear theoretical models of firm performance. The beginnings of a model have emerged in the concept of *strategic groups*, a term coined by Hunt [1972]. I have made efforts at generalizing this concept [Porter, 1973, 1976a, 1979a, 1980], and Newman [1973, 1978] has artfully examined an important subcase. The concept of strategic groups is that firms within industries can be clustered according to their strategies, and that their reactions to disturbances and the pattern of rivalry will be determined by the configuration of groups.

On the heels of the notion of strategic groups came the generalization of entry barriers to the concept of "mobility barriers" [Caves & Porter, 1977; Porter, 1973]. The argument is that the difficulty of entry into an industry depends on the strategic position the firm seeks to adopt (or on its strategic group). Mobility barriers are deterrents to a shift in strategic position of firms within an industry, deterrents that give some firms stable advantages over others. Thus, mobility barriers provide an explanation of differences in performance by firms in the same industry, and provide a conceptual basis for positioning a firm within its industry.

Mobility barriers, the configuration of strategic groups in the industry, industry-wide structural traits, and aspects of a firm's position within its strategic group have been combined into a theory of the strategic position of firms in their industry and their resulting profitability [Porter, 1978, 1979b]. I have recently tested the theory's implications for the profitability of differently situated firms [Porter, 1979b], and the theory's premises have been supported in work by Hatten [1974], Hatten and Schendel [1976], and Stonebreaker [1976].

The strategic group/mobility barrier extension of IO has additional benefits for strategic analysis. One is that it constitutes the beginning of a systematic way to determine what a firm's strengths and weaknesses are. This is one of the things that much of the literature has left practitioners to handle unaided. Thus the extension promises to increase the scope of IO's contribution to strategic analysis, by enabling it to contribute to the analysis of the upper left box in the LCAG quadrangle (Figure 1). In addition, the strategic group/mobility barrier concept is a starting point for the dynamic modeling of industry evolution, in which firms with different strategies and different objectives make investments in improving their strategic position.

Free-standing entity IO research is beginning to explore the interrelation between business units and their corporate siblings in modeling industry outcomes. The interrelation is embodied in the theory of strategic groups, and Newman [1973] has explored one aspect of the relationship in some detail. Spence and I have built the relationship into a model of capacity expansion in oligopoly [Porter & Spence, 1978; see also Porter, 1980, Chap. 3]. Recent work on so-called economies of scope has explored some implications of shared costs. Removing the assumption of a free-standing entity is a high-priority area for continued IO research.

Static tradition Increasingly, IO research is beginning to encompass dynamic models of industry evolution, some framed from the point of view of the strategic decision facing the individual firm. Numerous studies have investigated the determinants of changes in industry concentration [e.g., Mueller & Hamm, 1974; Orr, 1974] and some have investigated the determinants of entry. The work of the Boston Consulting Group has stimulated a number of rigorous models of learning curve phenomena [e.g., Spence, 1981]. A number of models have explored additional aspects of firm investment and innovation in a dynamic context. Michael Spence and I, for example, have modeled the dynamic capacity expansion problem facing the firm in a growing oligopoly using data drawn from a comprehensive case study of the corn-milling industry. The model exposes the critical role of uncertainty early in an industry's life for the subsequent structural evolution, among other variables. I have also explored the dynamic forces underlying industry change [Porter, 1980; Porter & Spence, 1978]. These and other efforts [e.g., Flaherty, 1976; Kamien & Schwartz, 1972; Spence, 1979] at dynamic modeling are far from fully satisfactory, but they are beginning to yield some important implications for how firms should compete in evolving industries.

Determinism The Bain view that strategic choices do not have an important influence on industry structure is nearly dead. It is now recognized that there are feedback effects of firm conduct (strategy) on market structure, as depicted in Figure 3. For example, firm innovations can enhance or diminish entry and mobility barriers. Some authors have gone a step further to propose and test models in which past performance affects the strategic options available to firms—hence the dotted line in Figure 3 [e.g., Comanor & Wilson, 1974, Chap. 6]. Recognition of both feedback loops has led to the increasing adoption of simultaneous equation models to test IO propositions [Caves, Porter, & Spence, 1980; Comanor & Wilson, 1974, Chap. 6]. Some recent articles have demonstrated how firms can affect or even deter entry into their industries by carefully choosing their strategies [Porter, 1980; Salop, 1979; Schmalensee, 1978; Spence, 1979].

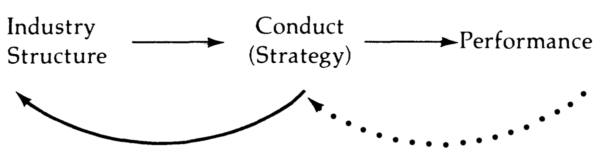

Figure 3
An Updated Version of the Industrial Organization Paradigm

The view is developing that there are some fundamental structural parameters of an industry dictated by the basic product characteristics and technology, but that within those parameters industry evolution can take many paths, depending on such factors as the luck of the draw in terms of the identity of industry rivals and uncertain events, as well as on the strategic choices firms actually make that follow from their unique objective functions. By articulating these factors, IO offers rich insights into strategy formulation.

Completeness IO researchers have identified an increasingly rich set of elements of industry structure that are important to competitive interaction. Richard Caves and I have articulated the concept of exit barriers, such as specialized assets and fixed costs of exit [Caves & Porter, 1977; Porter, 1976b, 1980]. Harrigan [1979] has applied the exit barrier and other IO concepts to the study of competitive interaction in eight declining industries. The study of industry structure has moved beyond looking at the conditions of supply to examine vertical bargaining relations with suppliers and buyers. Lustgarten [1975] and others have examined buyer power in producer goods industries and I have explored manufacturer/retailer bargaining in consumer goods industries and other aspects of the buyer's effect on strategy [Porter, 1974a, 1980]. Labor is being shown to be a competitor for firm profits (see Caves, Porter, & Spence [1980] for a survey).

International trade and competition is being built into IO models of industry competition, and this work is showing how obsolete the view is that all industries are domestic [Caves, Porter, & Spence, 1980; Pugel, 1978]. Linkages between the capital markets and industry competition are receiving attention, reflecting the view that capital market conditions can affect the ability of differently situated firms to compete and that financial strategy can be a competitive weapon [Fruhan, 1979; Hurdle, 1974]. Topics such as vertical integration and franchising have been explored in detail in the IO literature [Caves & Murphy, 1976]. Although there are further frontiers to be explored, IO has already achieved a richness that makes it of great utility in a broad array of strategy formulation problems.

Loss function IO researchers are stretching for still richer models that recognize interfirm and interindustry differences. In part because of contact with the policy literature, they are no longer satisfied with broad conclusions, even for public policy purposes—although the constraints of empirical data may force them to live with such conclusions.

Oligopoly theory Some strides have been made in applying oligopoly and game theory to real market conditions, although difficulties remain with this aspect of IO theory. Fruhan [1972] applied game theoretic concepts to competition in the domestic airline industry and Sultan [1974] to the electrical equipment industry. Aharoni [1966] applied game theory to foreign direct investment decisions. Schelling's [1960] rich qualitative theory of games has stressed concepts that can be applied to market settings, such as commitment, credibility, and focal points [Porter, 1980]. Knickerbocker [1973] has applied oligopoly theory and other IO concepts to the establishment of foreign subsidiaries by multinational firms. The ideas in all this literature could substantially enhance the subtlety with which firms can make and respond to competitive moves.

These developments have pushed IO theory squarely toward the heart of the policy field. The IO framework for analyzing industry structure and evolution, firm position within industries, and competitive interaction and strategic moves is increasingly rich and motivated by values close to those driving the policy practitioner. Many frontiers remain, to be sure, but IO has come of age for contributing substantively to strategy analysis.

My discussion has focused exclusively on strategy formulation at the business unit level, but recently IO research has begun to offer some intriguing possibilities for the study of diversification strategy and strategy implementation. IO-style models of the relation between diversification strategy, industry structure, and firm performance are beginning to appear [Caves, Porter, & Spence, 1980, Chap. 12; Grinyer, Yasai-Ardekani, & Al-Bazzaz, 1980; Rumelt, 1974]. Williamson [1975] and others, reflecting a long tradition dating back to Herbert Simon [1961], are exploring the factors that influence the scope of the firm, or the trade-off between using market and administrative transactions. Williamson identifies contractual failures that prevent certain types of market transactions from taking place. This sort of analysis can be fruitfully applied to questions of vertical integration, joint ventures, and organizational design. Another thread of research views organizational structure as a cost/benefit calculation, weighing coordination costs against the benefits of autonomy [Caves, 1980].

The Methodological Promise

I have been arguing the promise of IO for strategy analysis (and vice versa) in substantive terms. It seems important to consider its possible methodological contribution as well. IO research has developed a strong empirical tradition built around the statistical analysis of populations of firms and industries. Research on strategy is now using such methods to supplement the in-depth case studies that have been the bread and butter of policy research; the PIMS Program is a particularly ambitious example [Buzzell, Gale, & Sultan, 1976]. Recently a hybrid research design has emerged, using a series of mini-case studies to test richer hypotheses than can be feasibly tested in big samples [Harrigan, 1979; Newman, 1978].

The biggest block to further methodological diversity in strategy research is the availability of data. The PIMS Program of the Strategic Planning Institute has made some strides through the collection of an extensive data base that is unique in revealing some aspects of strategic choice. A new effort is underway at Harvard—the Program for Industry and Company Analysis, which is assembling an industry-centered data base designed explicitly for strategy research. The Federal Trade Commission's business-reporting efforts should also yield major benefits in terms of data availability. Recognition by policy practitioners of the potential of cross-sectional and time series research methods will, one hopes, further stimulate data collection of a richness and firm-specificity responsive to policy concerns, aided by the increasing disclosure requirements being placed on public firms.

Concluding Remarks

Most of the areas I have identified as limitations of IO theory still remain as frontiers for IO research and thus they provide a research agenda for IO. Another intriguing frontier for IO research is the development of a model of the competitive interaction among multibusiness firms with business units in partly overlapping markets. Such overlapping complicates coordination among firms, but offers possibilities for threats, deterrence, and side payments that go beyond those possible when competition is on a market-by-market basis. In view of the prevalence of large, diversified firms in many markets, this avenue of research seems to hold great interest.

Despite the long agenda, I am confident that the research frontiers of IO will be pushed back because of the shared research motivations in IO and BP, and the fact that both economists and business scholars are participating.

Frontiers aside, it should be clear that there is gold to mine in applying IO concepts to strategy formulation, just as there has been much gold mined (with much more still in the seam) by employing a policy perspective in IO research. If this article stimulates more cross-fertilization, then it will have served what I believe is a useful purpose. I am confident that, with or without this article, more cross-fertilization is going to occur.

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