

## **Why *Competitive Strategy* (1980) Succeeds - and With Whom<sup>1</sup>**

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### **Introduction**

Academics anxious about their professional identity might be given to checking Google Scholar for reassurance they exist. On this basis Michael Porter does exist, with at least three hits, *Competitive Strategy* (1980a), *Competitive Advantage* (1985) and *Competitive Advantage of Nations* (1990). The citation counts for all three are around 16,000. In comparison, the seminal work by our discipline's sole Nobel winner, Herbert Simon's *Administrative Behavior* (1947), comes in around 10,000 and the 2009 economics' Nobel co-winner Oliver Williamson's *Market and Hierarchies* (1975) and *Economic Institutions of Capitalism* (1985) score around 12,000 and 19,000. It is not easy to know what these numbers mean but they clearly confirm Porter's work is widely recognized within the group of scholars whose citation behavior is being measured - especially those interested in management and strategy.

But Porter has also impacted communities that neither visit Google Scholar nor share our academic anxieties. He served on the President's Commission on Industrial Competitiveness in 1983, as Chair of the Massachusetts Governor's Council on Economic Growth and Technology, and on the Boards of several US corporations. The Massachusetts State Legislature honored him for his work on that state's competitiveness; the State of South Carolina did likewise, as did the US Department of Commerce for his lifetime contributions to national competitiveness. He has advised the governments of ten countries including Russia, Spain, Singapore, Taiwan and the UK and led economic strategy studies for six countries including Canada, India, New Zealand and Thailand. He has two diplomatic awards and 13 honorary doctorates - in addition to his many academic awards. Plus he is a successful entrepreneur, in 1983 helping found Monitor, the global consultancy that presently has around 1,500 employees in 16

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<sup>1</sup> During the research for this chapter the Editors and JC Spender met with Professor Porter at HBS. He was, as ever, generous and gracious but our conversation revealed little that was not already in the public domain from previous interviews such as Argyres & McGahan (2002) or Stonehouse & Snowden (2007) and for that reason we do not quote extensively from our interview. But the discussion was personally productive and highly appreciated in that it triggered thoughts about the methodological issues that form the core of this chapter.

countries. An active philanthropist, Porter founded the Initiative for a Competitive Inner City. He is also a public authority - the New York Times calls him for tips about the stimulus to be passed on to the Obama Administration and Wall Streeters ask him how to deploy philanthropy to repair their tattered image.

Inevitably this record of private, public, academic, and corporate accomplishment has diverse roots, but it is clear that the genesis, publication and impact of *Competitive Strategy* - henceforth CS - shifted a promising career as a young award-winning Harvard economist into a very different gear. In this chapter we puzzle why CS has such impact and with whom - evidently widely embraced by managers, policy makers, consultants, and strategy teachers, while often criticized for shortcomings by strategy theorists. But it is impossible to overlook a curiosity – the disregard of Porter's work in the ongoing debates about the relevance of our field's research. For decades there has been rising concern about the gap between business school research and teaching and its value to the business community (Bennis & O'Toole, 2005; Locke, 1996; Mintzberg, 2004, 2009; Spender, 2007; Starkey, Hatchuel, & Tempest, 2004). Hambrick's 1993 Presidential Address for the Academy of Management asked 'What if the Academy actually mattered?' (Hambrick, 1994) and successive Academy Presidents have asked similar questions. In the breast-beating that has followed (Ghoshal, 2005; Pfeffer, 2007) that Porter's work matters to a powerful constituency of business and national policy-makers has somehow been ignored. Thus one of the lessons to be drawn from Porter's work is that some of our discipline's contributions do indeed matter - greatly (Brandenburger, 2002). The puzzle, of course, is why.

In the following sections we summarize CS's main points - even though widely known – and look at how the book was received by the various parties, reviewers, management educators, theorists and business practitioners. We consider the intellectual context and sources behind the book and probe explanations of its extraordinary impact. We conclude with some further lessons.

## **The Book**

The iconic symbol of CS is the 5-force diagram appearing on page 4 as Figure 1-1. Its comprehensiveness obscures that it is the book's point of departure not its conclusion. Many readers of CS or its derivatives forget this and for them the diagram becomes the book; there is little more to be said. But Porter has something different in mind. His first sentence reads 'The essence of formulating competitive strategy is

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relating a company to its environment' (CS: 1). This seems conventional enough but does not explain why Porter finds it necessary to model the firm's environment in an unconventional way. Porter's 5-force diagram (hereinafter FFD) is this framing, modeling both the firm's industry and the competitors within it. It is a framing of considerable subtlety with several levels, identifiable entities and interactions. In contrast to microeconomic and marketing theory conventions, the FFD proposes the firm's environment comprises of more than competitors. Likewise, 'industry' has little to do with industry as economists use the term - as defined, for instance, by Standard Industrial Classification (SIC) codes. Porter defines an industry as 'the group of firms producing products that are close substitutes for each other' (CS: 5). Porter's intent becomes evident in his next sentence: 'In practice there is often a great deal of controversy over the appropriate definition, centering around how close substitutability needs to be in terms of product, process or geographic market boundaries.' There is more than curious grammar here.

Defining products in terms of their substitutes seem oblique. Yet it provides a novel way of analyzing the firm's environment. Porter's definition refers to a conceptual 'barrier' between products rather than to any straightforward description of the goods and services the firm's production function generates. For instance, we know that inputs of glass, steel, plastic, leather, rubber, copper wire, computer parts, skilled labor, and so forth can produce many different types of automobile. Yet, though the engineering is closer than some would care to admit, a BMW is not like a Chevrolet when it comes to a competitive strategic analysis of the firms producing them. Strategizing based on microeconomics alone might focus on manufacturers' pricing and its impact on the interplay between supply and demand, noting the maximization of revenue at the point of market equilibrium. Porter goes well beyond such neoclassical thinking. The barriers he has in mind are not related to a single product market or industry; he presumes a multiplicity of interacting products and industries. Nor is an industry to be defined by its products; it is defined by 'barriers'. These vary from the impenetrable to the easily penetrated. As an extreme example, the distance between tableware and fashion garments is so great that it constitutes a virtually impenetrable barrier. In contrast, the barrier between automobiles and fashion is often penetrable; a Smart Car or a Maserati may be more of a fashion statement than a means of transport. More obviously, the barrier between motorized mowers and chain saws is highly permeable (Porter, 1983: 189). In CS the barriers of interest are those between industries capable of interfering with each other - 'substitutability' being a term of art that implies some interference with the firm's pursuit of its goals.

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The analysis Porter offers stands on a material difference between 'competition' as economists define it, and what Porter refers to as 'rivalry', the interactive situation managers should consider in their strategic choosing. Porter's analysis is for managers. For economists, competition is a structural matter, whether a firm has competitors or not, competition that can be measured in various ways e.g. the industry's concentration ratio or Herfindahl Index. Much of the power of the FFD is that it reaches beyond an industry's incumbent firms to other entities. Porter argues real business is a condition of 'extended rivalry' in which customers, suppliers, firms offering substitutes and potential entrants are all rivals when they are able to influence or interfere with the focal firm's rent streams (CS:8). This presumes economic power based on some economically significant sources or resources. Economists distinguish various types of rent - Ricardian, Paretian, entrepreneurial, Penrosian, and so on - some arising from Nature's randomly distributed gifts, others from Man's inventions, others resulting from government's actions, yet others on management-created entry barriers - and the relationships between them are complex (Mahoney & Pandian, 1992; Rumelt, 1987: 144; Spender, 1994). Porter glosses these, as well as the way some rents are affected by factors beyond management's control - global warming, technological change, resource depletion, and so on. CS focuses directly on the rent-shaping behaviors influenced by the firm's management without identifying the rents in question for Porter's attention is on rent-shaping rivalry not the rents themselves. While seldom able to influence the presence or absence of competition managers can often alter an industry's degree of rivalry. Even when there is plenty of competition in the sense of many suppliers and a low concentration ratio, there may be little or no rivalry. Competing firms often engage in collusion as a method of decreasing rivalry while even a monopolistic firm can face rivalrous pressure from its customers and suppliers.

Porter's analysis begins by disaggregating the factors that influence rivalry into distinct categories - the 'five forces'. Having introduced these at an abstract level Porter brings them into the context of practical management with a Bain-based discussion of the various kinds of entry barriers (CS:7). For Porter economies of scale are strategically significant not because they can lead to lower production costs but because they can be both warning signals and practical impediments to entry by other firms who have not achieved such economies. Switching costs present a potential entrant with disincentives. To be denied access to distribution channels provides the potential entrant with no viable way of entering, and so on. The stress is on the dynamics of preventing others' strategic action, rather than on determining the industry's structure and its consequences. The analysis in CS becomes explicitly dynamic and akin to multi-period game theory as Porter considers 'expected retaliation' as incumbents respond to an

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entrant's acts or threats to act. All of this leads up to the FFD-based characterization of the firm's situation or the state of rivalry it is experiencing (CS: 17). The FFD implies three levels of interaction - (a) the conventional economist's level, between the industry's incumbent firms, (b) a well-defined extended level, between the firm and its existing trading partners, and those already offering substitute products and services, and (c) a more nebulous extended level, between the firm and the potential industry entrants offering new substitute products and services. CS's primary objective is not to help managers see rivalry's importance - they know that well enough. It is to illuminate this three-level complex and thereby help managers get a handle on what exactly they might be able to manage.

In Porter's view every firm is located in a lumpy rent-field mapped by barriers that separate various domains of economic activity - each offering different rent possibilities. Firms may make strategic moves from one domain to another seeking higher rents, a better 'position' in the strategic landscape, exiting one line of business and entering another. Thus many put Porter's work in a 'positioning school' (Bowman, Singh, & Thomas, 2002; Mintzberg, 1987, 1990a, 1990b). This clouds the analysis actually offered in CS. In the FFD barriers define the rivalrous entities. Thus the management of these entities' rivalry requires an understanding of the barriers defining the strategic situation - and the relevant entities and their relationships. The positioning metaphor suggests the firm's strategic objective is to re-position itself from one domain to another and the boundaries are penetrable to the degree the rents in another domain are accessible to the firm entering that domain. Porter argues that while strategic re-positioning is possible, rivalry can be provoked and managed by many other means. The FFD's implicit question is not 'which domain to enter' in the pursuit of new rents. On the contrary, the emphasis is on protecting the firm's existing rent streams from interference (reduction) by the other entities comprising the firm's extended rivalry network. Porter discusses 'force' in multiple terms - bargaining power and signaled threats as well as actual acts of domain entry (CS: 6). Force and barrier are complementary, force being required to penetrate a barrier. Forces describe the dynamic relationships between the entities; thereby the analysis is tilted towards an active process view rather than to a static structural view.

This turn to force and dynamic process gradually reveals the book's true objective - far from summarized by the one-directional arrows in the FFD diagram. While Porter's aesthetic may be to show the firm under pressure from everyone else, the reality is that quasi-monopolistic firms such as Wal-Mart exert considerable pressure on their suppliers, to the point of forcing them into bankruptcy perhaps, and

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otherwise diminish the power of their customers (Moreton, 2009). Other firms, such as Microsoft, pressure their customers directly. Moreover, having acquired 'business intelligence' and developed expectations that a specific potential entrant was planning a strategic move against it, a firm might negotiate an alliance with the potential entrant, to their mutual benefit, or re-set its prices. Likewise, at the middle level of negotiating with a specific supplier, the firm might 'second source', looking into the market for alternative suppliers or substitutes.

CS not only shows managers how to view their rivalrous environment from the specific strategic vantage point of identifying and protecting their own current rent streams, but also how to pay attention to goings on that might affect their rents in the future. For instance, they should note the number of competitors and their balance or concentration, the presence or absence of switching costs, the lumpiness of productive and distributive capacity, the height and strategic location of the exit barriers, and so on. The point here is the diverse and contingent nature of these rivalry-defining characteristics with the FFD revealed as a sketch of the network of power relations. In short, CS is about the nature and management of the firm's economic power and the resulting rents, and how it might manage its responses to the actions and threats of powerful others. The firm's power is contingent on the specifics of its strategic and economic situation and the actions and strategic intentions of those able to engage in it; multifaceted empirical details that cannot be summarized in overarching concepts, economic, structural, strategic or behavioral. The FFD is a framework for viewing, selecting and attending to these empirical realities. CS's proposition is that a firm's managers must understand these different interactions and types of power if they are to deploy the power available to them effectively.

Aside from obvious questions about how to operationalize these ideas or make a specific strategic decision, there is some question about whether CS actually proposes a theory of strategy. Many presume Porter's analysis eventually identifies a domain of strategic opportunity determined by the industry environment and that the firm can then position itself within this field by selecting one of the three generic strategies (cost leadership, differentiation, and focus) (CS:34). We think this interpretation peripheral rather than central to CS, even though it has given rise to the main academic critiques of CS (e.g. Hendry, 1990). To the extent that strategy is about executives deciding what to pay attention to, given their limited attention, CS is very much about strategy even though a specific strategic prescription is not generated. Likewise if strategy is about making actionable sense of a complex uncertainty-driven situation that in practice cannot be grasped with a rigorous one or two-dimensional model, then CS is

about strategy. Inasmuch as strategy is about formal analysis and decision-making CS pushes the discussion forward from obsessive concern with the market share held by the industry's other firms and towards understanding the industry's attractiveness and growth rate. But this usefulness comes at some cost to clarity - how can a firm's capabilities be measured against those of unknown potential entrants at the third, most nebulous, level of analysis? How can the possibilities opened up by the dynamic non-equilibrium analysis in CS be reconciled with rigorous econometric models? Evidently balancing the use-value of the analysis against its clarity calls for some strategic decision-making in its own right, and there seems no question Porter's academic strategizing has proven successful.

### **Initial Reception**

To understand how CS was received, one should note Porter's earlier writings, in particular his 1976 article in *California Management Review* (CMR) and his 1979 article in *Harvard Business Review* (HBR). The latter, entitled *How Competitive Forces Shape Strategy* (Porter, 1979a) attracted considerable attention. Its telling sub-title is 'Awareness of these forces can help a company stake out a position in its industry that is less vulnerable to attack', the active process view we also find in CS. In the light of the complex and multi-layered nature of the concepts involved, such as the difference between price changes, bargaining power and threats, the clarity and directness of the HBR article's language is striking. It almost completely eschews academic style and terminology and is a fine example of writing for the 'reflective practitioner'. Its sole citation is to Porter's earlier 1976 article on exit barriers in the *California Management Review* (CMR) (Porter, 1976b). The difference between the two articles is also striking. In the CMR piece Porter deploys the list-making style so evident and perhaps overdone in CS. But after 7 pages he moves to report his PIMS-based analysis of 310 firms with chronically low returns, candidates for industry exit whose non-exit indicated the presence of strategically significant exit barriers. Though he does not present the statistical tables typical of our scholarly journals, he reports the statistical significance of the variables chosen and so on. We can see from Porter's earlier works that he was no stranger to statistical economic analysis (Porter, 1973, 1974, 1976a, 1979b). Likewise, his economics articles published jointly with Caves and others are academic (Caves, Gale, & Porter, 1977; Caves, Khalilzadeh-Shirazi, & Porter, 1975; Caves & Porter, 1976; Caves & Porter, 1977, 1978; Caves & Porter, 1980; Caves, Porter, Spence, & Scott, 1980). In contrast, CMR is a practitioner-oriented journal, the West Coast equivalent of HBR, and Porter's 1976 piece is an attempt to write for a non-academic

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business audience that has its mind on the firm's performance. Its citations are also revealing, 10 only and with the exception of Gilmour's DBA thesis on divestment decision-making, all practitioner-oriented.

Clearly Porter learned a great deal about writing for practitioners between the 1976 CMR and 1979 HBR articles. He also placed a version of the HBR article in the *Financial Analysts Journal* in mid-1980 (Porter, 1980b). Both suggested a larger work in the background - one of the two manuscripts mentioned in the bio details of the HBR article. During our interview Porter told us he drafted the first version of CS around 1975, only to put it aside as clumsy and overly dependent on 'stylized' academic economics. Around the same time he initiated and was teaching his class on Industry Competitive Analysis (ICA). It became a hit with the MBA students. For these students he wrote the first of several versions of his *Note on the Structural Analysis of Industries* (Porter, 1975). It eventually evolved into Chapter 1 of CS and, as we have seen, it contains all the fundamental concepts of the book - with one notable exception, Chapter 2's 'generic strategies'. The Note was also the basis for the 1979 HBR article. Leveraging from what he was learning through teaching his ICA course, Porter spent three years re-writing CS, fleshing out the 1975 Note written not to his economic colleagues or even the business practitioners he mentions in CS's Preface - but to his students.

On the whole CS was well received by academic reviewers. Most praised its usefulness to practitioners. The book was brought to the attention of the business world in October 1981 by Kiechel who did a series on management strategies in *Fortune* (Kiechel III, 1981). He devoted three full pages to an upbeat executive summary, accentuating Porter's three generic strategies. Lewis, a consultant with Strategic Planning Associates, reviewed CS in *Strategic Management Journal* (SMJ). He began 'Finally, the power of economic theory has been brought to bear on the practical problems of business strategy ... CS does this with a conceptual framework that managers and students of business can both understand and apply. CS represents a quantum leap in the development of strategic planning ... CS promises to revolutionize the creative process underlying the selection and formulation of strategies' (Lewis, 1981: 93). He concludes 'Although CS is written for the practitioner, the concepts presented are extremely comprehensive and dynamic. They are not simplifications. What makes the complexity quite readable is the clarity with which the techniques are presented and the large number of industry and company examples. The book may well be one of the most important to the discipline of strategic planning' (Lewis, 1981: 95). Lewis pointed to the difficulties of getting managers to 'stop thinking about today and to consider the next few years' and to the difference between analysis and the creative activity of

developing a strategy. CS, he says, makes a real contribution to the second. Another important supportive comment by Shubik, the father of modern game theory, appeared in 1983: 'The book by Porter contains a useful checklist that to some extent puts the older economic studies of oligopoly theory and industrial organization on their heads and reviews their contributions from the viewpoint of the corporate planner' (Shubik, 1983: 170).

More critical was a 1981 three-part review by the Steiner, Hertz and Boyd that appeared in the *Journal of Business Strategy* (JBS). Steiner of UCLA, then corporate strategy's leading academic, reported Porter had introduced 'an important new frame of reference and new techniques that strategic managers will find of great value' and that the attention to competition, largely overlooked in the strategy literature of the time, was done 'superbly' (Steiner, 1981: 84). But finding something to criticize Steiner expressed concern about the enormity, time- and cost-wise, of a full-blown industry analysis. Hertz, likewise generally complimentary, noted talking about competition was not new and that much of Porter's analysis arose out of management consulting practice - even though CS provided a road map to the strategy-choosing process, giving CEOs 'much needed comfort as well as a supportive basis for making hard decisions' (Hertz, 1981: 87). Yet 'it should be apparent that the generalizations inherent in these methods have been well understood and utilized, implicitly and explicitly, by successful industrial leaders for generations'. Finding another worm in the apple Hertz asks 'Where are the nuances of uncertainty, the feeling for risk versus reward, the opportunities for swift maneuver? ... Professor Porter's excellent guidelines cannot replace boldness and initiative in competitive strategy, but they can provide the elements that can hold a corporate enterprise on a stable, if perhaps unexciting, course' (Hertz, 1981: 88). Boyd, a marketing academic, noted the marketing literature is replete with analyses of the relationship between strategy and the specifics of product, price, channels and promotions, and that previous work on product positioning and market segments had been particularly significant. 'What has been missing is a framework for developing an 'optimum portfolio of product-market strategies' that would link market response to expenditures to obtain a 'best yield'' (Boyd, 1981: 85). Then he added: 'The Porter book does not fill this void'. While he regarded several chapters as worth reading, he concluded CS in its entirety could not be recommended to marketing practitioners.

The next issue of JBS had further reviews of CS by Collier and Henderson. Collier's was a lengthy summary of the contents of each chapter with little comment. He found CS a well written book, useful for practitioners and students alike that should be in the library of every practitioner of strategic

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planning (Collier, 1981: 85). The review by Henderson – founder of the Boston Consulting Group (BCG) and author of the BCG matrix – was very different. While he found it a ‘monumental book’ that should be in every executive’s library as a reference he criticized the book for having many weaknesses: ‘In spite of the title this book is not about strategy. It does not attempt to integrate this material into a system of relationships ... The book is an operating manual. It is lacking in breadth and comprehensiveness, as well as the integration and systems analysis needed to qualify as a treatise on strategy’ (Henderson, 1981: 84). He duly listed the book’s concepts and continued: ‘There are many specific examples used as illustrations. These examples are unfortunate. Some of the assumptions about company characteristics are not always relevant or even correct ... The book illustrates the best and the worst of teaching and learning by the case method. It illustrates the richness and diversity of the forces and work. It offers little guidance as to how to manage these relationships in a dynamic system’ (Henderson, 1981: 84). Sounding more like an economist than a consultant Henderson continued: ‘The meaning of industry structure is never defined. Nor is there any explanation of why that is fundamental to the equilibrium between competitors. In fact, the concept of competitive equilibrium is never mentioned. This omission converts the book into a collection of observations, hypotheses, philosophy, and subjective opinion rather than an integrated structure of logic’. He continued in like vein: ‘All of this description is interesting and comprehensive. But it evokes an underlying feeling of frustration. Which of these factors are critical? How do they trade off against each other? What are you supposed to do with all this data? It is like trying to troubleshoot a competitor with nothing except a wiring diagram. How does it all fit together as a system?’ (Henderson, 1981: 85). He wrapped up saying the book was worth reading, especially by new business school graduates for whom ‘it is a golden treasury of insight into the subtlety and complexity of business which they must learn to recognize intuitively’ and that if Porter could integrate all this into a system analysis of competitive equilibrium and how to manage it the sequel would be truly worthy of the seminal title ‘Corporate Strategy’.

MIT's Bowman reviewed CS in *Sloan Management Review* and offered mixed views that reflected some of Henderson's concerns in more academic language. Bowman began by arguing management studies had gone through various stages. In the 1920 and 30s the emphasis was on description. Rigorous economic analysis in the 1950s and 60s displaced this with prescription. Following the behavioral work of Simon and others at the Carnegie Institute of Technology, and the analyses by Mintzberg and Lindblom, studies became more descriptive again. Bowman hoped for a new phase of 'sophisticated prescription' and looked to Porter for an exemplar, but noted CS 'is a book largely without people and

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internal process, almost with the implication that these issues aren't very important. His concern with dynamics is outside the firm, not inside' (Bowman, 1981: 66). After going through the contents and complimenting Porter on his pedagogical style Bowman made his concerns explicit. 'There are however some disturbing elements in this style carried throughout the book, which is perhaps explained by the point that it is the practitioner who is being addressed. The reader, at least this academic reader, is frequently left with a question: On what basis can the author make this statement? Many of the lines come out close 'ex cathedra' style. With rare exceptions, no careful published studies are cited to support such statements, and the industrial organization branch of economics is replete with careful empirical studies' (Bowman, 1981: 67). Noting Porter's demonstrated academic talent and citing some of his economic publications Bowman wrote: 'this reviewer hopes that Professor Porter will now write another book, not unlike this one, but addressed to his academic colleagues and their graduate students ... my preference would have been a much more systemic inclusion of research findings in the chapters where appropriate. This would be more satisfactory for the scholar's epistemological search for *'veritas''*.

CS also received attention in the marketing journals. Fox, a marketing academic, reviewed CS in the *Journal of Marketing*. Noting CS was written for practitioners he argued 'CS differs from comparable treatments in several respects. It is not procedural but substantive. Examinations of cause-effect relations are deeper and more coherent than in the relatively few other available books on strategic substance. And, unlike industrial organization texts, Porter's orientation is managerial ... People who have been waiting for this kind of discussion will read and re-read Porter's book enthusiastically ... A simple framework serves as a platform for narrating competitive strategies under a great variety of conditions ... Three generic strategies, quite familiar to most readers of the 'Journal of Marketing', form the analytic underpinning' (Fox, 1982: 124). He concluded 'This short review does not do justice to Porter's numerous findings and insights ... Inevitably in this comprehensive coverage, knowledgeable practitioners and academicians will recognize some familiar ideas. Other findings or conclusions may seem obvious in retrospect. The book could also be faulted for repetitiousness ... This reviewer found the contents highly stimulating and helpful' (Fox, 1982: 124). Another marketing-based review by Leo, an Assistant General Counsel for Scovill Inc., appeared in the *Industrial Marketing Management*. It opened: 'This well-received book ... written in an easily readable, well organized style ... is designed to provide an analytic framework for understanding industries and competitors ... Porter assumes that modern companies have an explicit process for determining strategy and his text seeks to assist those

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considering entering or exiting markets to anticipate competitive response, predict an industry's future evolution, and to analyze the industry as a whole' (Leo, 1982: 318). Leo continued: 'Much of the advice set forth ... is given in a commonsense style that might be obvious to many. The strength of the book, however, is mainly in its comprehensive style rather than its innovativeness ... the benefit of CS is assuring that each element is closely considered as a single factor interrelated with others in the industry environment' (Leo, 1982: 319).

Surprisingly, there were no reviews of CS in any Academy of Management journal until 1985 when Gartner's appeared in *Academy of Management Review* (AMR) (Gartner, 1985). But Porter's next blockbuster *Competitive Advantage* (CA) had already appeared, as had his *Cases in Competitive Strategy* - in short, the FFD machine was rolling and its success undeniable. Gartner reviewed both CS and CA. He opened noting Porter's ideas came from industrial economics while saying: 'Both books make accessible some of the ideas originating in the IO literature, though Porter does not provide references to ground his ideas in the literature.' (Gartner, 1985: 874). Ignoring Porter's 1981 AMR article - which we shall consider in due course - he quoted from Kiechel's 1981 review and added: 'The appeal and power of the books come from Porter's frameworks, which can hold onto so many different ideas in a coherent manner and show their interrelations ... the frameworks will continue to gain in strength as both practitioners and scholars work with them' (Gartner, 1985: 874). Finding Bowman's worm Gartner went on: 'Porter's books overflow with ideas which cry out to be borrowed and put to further use ... These ideas are not systematically supported, that is, the examples and illustrations are selected to support the ideas Porter provides. A suspicion is that other examples might be found to make the counter-argument and this could lead to some excellent studies'. Gartner continued: 'A disappointment with both of these books is their somewhat myopic or selective view of the strategic management literature. No recognition of other views of strategic typologies was given, though many thoughtful, empirically generated studies have been undertaken ... Given that the books are practitioner oriented one might excuse this as simply a decision to omit lengthy or boring discussion of the academic literature. Yet Porter does discuss the literature generated by the Harvard Business School, and he does this very much to the exclusion of the literature developed at other schools, inclusion of which could have improved these books' (Gartner, 1985: 875).

What is to be made of these reviews? Did they foreshadow CS's success? One common thread is Porter's non-academic writing style and this is clearly important. A second is the recognition that CS was for

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practitioners - as Porter emphasized in CS's revealing Preface. Third is its comprehensiveness - lists of the situational characteristics managers should pay attention to - lists, for some, to a fault, but for others, such as Bowman, still missing the firm itself and its people. Fourth, especially from the marketing folks, a sense of *déjà vu* - it is all familiar, though nicely packaged. Fifth, for strategy theorists a valuable correction to the one-dimensionality of much strategy literature and its obsession with market share. But there was nothing that would portend CS's success. Indeed, for some, there was a lack, frustration and pushback from those asking 'where is the theory?' This is explicit in Bowman. Henderson too is asking 'how does it come together into a prescription?' - a practitioner's way of talking about deterministic theory. For Porter this particular criticism was not new. The term 'theory' is glaringly absent from the CMR and 1979 HBR articles, as it is from CS. Likewise Veendorp's 1978 review of Porter's earlier Wells Prize-winning *Interbrand Choice, Strategy and Market Power* (IC) asked the same question (Porter, 1976a). Veendorp wrote: 'The theoretical part of the book ... is somewhat disappointing. The first five chapters offer essentially little more than a lengthy, rather repetitious, and exclusively verbal argument in support of the convenience-nonconvenience good distinction to be tested subsequently. No serious attempt is made to build a precise and rigorous model of the interaction between the information gathering activities of customers and the information providing activities of manufacturers and retailers' (Veendorp, 1978: 671). Curiously, while in IC's Chapter 8 Porter writes: 'These premises spawned the theory presented in Chapters 2 through 5' (Porter, 1976a: 232) there is actually no mention of theory in these chapters. We do, however, find the term 'framework' - and the difference between the two is crucial to Porter's thought and works.

But the reviews also present us with a significant empirical fact - the lack of a specifically testable theory, expressible as the hypotheses that characterize our scholarly journals - was clearly no impediment to CS's success and may, perhaps, have been a kind of cause. In the next section we look at the commentaries on CS that have appeared since these early reviews. For these later authors CS's success is incontrovertible; our agenda is to see if their analyses throw any further light onto why.

### **Impact**

Getting a handle on CS's impact - beyond citations - is not as simple as it might seem. Hearsay has it that the FFD is taught in most undergraduate and MBA strategy courses around the world (Shih, 2007). No

question it is wonderfully teachable and easily remembered - but so are SWOT, the BCG matrix and break-even analysis. How do FFD and other strategy tools actually impact strategic management or our research and publication practices? The bibliographic data presented by Furrer, Thomas and Goussevskaja (2008) are interesting. They researched the occurrence of keywords in four leading management research journals, AMJ, AMR, ASQ and SMJ, between 1980 and 2005. Bearing in mind Steiner's comment that CS brought 'competition' into the foreground of strategic analysis we can hypothesize an association between the use of 'competition' and CS's publication. Furrer et al.'s findings are that 'competition' occurred in 6.1% of papers between 1980 and 1985, but rose to 16.2% in 1986-1990, to be sustained at a similar rate in the 1991-1995, 1996-2000 and 2001-2005 intervals. 'Planning', plummeted from 26.7% in the 1980-1985 period to 3.2% in the 2001-2005 period. Other terms go up and down; there is a steady erosion of 'methodology' and 'mission' and a big increase in 'alliances' and 'capabilities'. 'Fit' almost disappears and 'leadership' barely makes an appearance.

Bibliographic research reveals key-word usage but does little to explicate a term's meaning. Although objective measures are hard to establish, we can grasp part of CS's impact from the way various writers who trace the evolution of the strategy field have - or have not - written about it. No question, Porter is recognized as a major influence in retrospective analyses of the strategy field (Caves, 1984; Gavetti & Levinthal, 2004; Hoskisson, Hitt, Wan, & Yiu, 1999; McKiernan, 1997, 1996). Yet, the limited mention of CS in earlier studies of this type reminds us of how slowly ideas percolate through our literature. Bower, one of Porter's colleagues at HBS, published a review of the business policy field in 1982. CS rates only four sentences; Bower arguing 'the elaborated language of structural analysis does not solve the strategist's problem, though it makes its specification more precise' (Bower, 1982: 633). From across the river at MIT Hax's review of the strategic management field notes CS without comment as one of three books dealing with understanding the firm's environment (Hax, 1982). Hamermesh, another HBS colleague, reprinted Porter's 1979 HBR article in an edited volume on strategy with a couple of sentences on the FFD as 'a powerful framework' (Hamermesh, 1983). Similarly, Horovitz's review of corporate strategic analysis had but one sentence on CS: 'the most recent and successful book on strategic management is a book on improving the manager's intellectual ability to analyze a sector in order to compete effectively' (1984: 30). Lamb's introduction to a compendium on competitive strategic management, which included a chapter by Porter seemingly reconsidering and extending the FFD, left CS unmentioned (Lamb, 1984). Dutton, Fahey & Narayanan's (1983) SMJ paper on 'strategic issue diagnosis' made a single mention of CS as recognizing the problem of defining market boundaries, but

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without providing guidelines for decision makers to use (Dutton, et al., 1983). In their 1983 paper on the microeconomic foundations of corporate strategy Naylor & Thomas reviewed CS's content and argued it was an 'initial step towards identifying the relevant characteristics of markets and indicating how they may be used by the firm' (1983: 131). Ginsberg & Venkatraman treated CS as just another analysis of how the firm's environment determines its strategic options, paralleled by Snow & Hambrick's 1980 analysis (Ginsberg & Venkatraman, 1985). Eliashberg & Chatterjee's extensive analysis of analytic models of competition in the *Journal of Marketing Research* made but a single mention of CS as dealing with rivals' responses (Eliashberg & Chatterjee, 1985). Enz's AMR paper on the textbooks being used in business policy and strategy courses did not mention CS at all (Enz, 1986). Likewise Bresnahan & Schmalensee's paper on the renaissance of industrial economics had no mention of CS (Bresnahan & Schmalensee, 1987). In short, these early reviews of the strategy field revealed no signs of why CS might eventually become a major influence on strategy.

Meanwhile Kiechel's 1981 *Fortune* article indicated CS was already being widely read by managers; plus there were stories that every recruit to the rapidly expanding consulting and accounting firms would be handed a copy and told to read it as a way of getting to know what they were supposed to be doing. The book's rising sales would support this view. Its uptake seemed immediate; managers found no difficulty with the concepts or the conclusions they led to - nor have subsequent generations of undergraduate and graduate students. As mentioned, Porter's disciplined communication skills and authorial talents, carefully honed in the years CS was gestating, as well as his two-pronged business and academic publishing strategy, were important contributing factors.

Gradually things changed. But the focus also shifted and views of CS began to emphasize its different audiences. In the classroom the FFD took on a life of its own as a highly teachable strategy tool, to be set alongside SWOT, BDG matrix, scenario analysis, Seven-S and so on. Practicing managers did not flock to CS because they were interested in the academic niceties of strategic group definition or the marketing theory behind generic strategies - they liked the FFD. Likewise strategy teachers learned the value of presenting their students with the FFD, maybe as one tool in a strategic toolbox, together with cases on which to try it out. Here the FFD was immediately comprehensible with its implicit balance between industry level opportunities and threats and firm-level resources and processes. Most teachers would gloss over the generic strategies unless they were especially interested or proficient in marketing theory. Most commentators saw the FFD as CS's strongest thrust and comments such as Jørgensen's

(2008) are typical. He argued pre-Porter the strategists' tools were limited to SWOT, the BCG and GE matrices and the Chandler strategy-structure approach. He proceeded by reciting the book's contents as self-evident advances. But such claims to comprehensiveness do not tell us much about the book's appeal or the substantial sustained rent-stream it has engendered for author and publisher alike. More hagiographic was Barney's: 'There is little doubt that Michael Porter has been the most influential scholar in the field of strategic management over the last 25 years' (Barney, 2002: 53). Barney wrote: 'before CS there was little consensus about the objective of strategic management practice or research ... by helping create a consensus about the dependent variable ... Porter helped focus what had previously been a highly fragmented discussion'. In similar vein, HBS's Brandenburger wrote: 'Published in 1980, Michael Porter's CS continues to exert enormous influence on the field of business strategy ... The FFD is surely the most widely known and widely used idea from CS. What is the reason for the power of this model? In my view it is that it gives a clear image of the essential activity of business' (Brandenburger, 2002: 58). While there is no systematic analysis of its evident appeal to practitioners, these commentaries accept CS greatly impacted strategy-making practice.

In contrast an increasing number of strategic, economic, and legal academics took exception to CS's assumptions, casting doubt on the rigor or clarity claimed. While some thought the analysis novel and insightful others felt Porter had borrowed too liberally from Scherer's earlier theorizing (Scherer, 1980). Speed echoes some of the earlier academic reviewers' misgivings: 'The principal criticism of Porter's work is methodological. Many of the points he makes do not appear to have any justification ... O'Shaughnessy (1984) criticizes the choice of forces for two reasons. Firstly, their choice seems arbitrary. There is little to suggest this is necessarily an exclusive or exhaustive list. O'Shaughnessy's second point is that Porter gives no indication of how to operationalize any analysis based on these forces. There is no indication of how to assess the relative power of the forces, nor how to determine what counteractions can be taken' (Speed, 1989: 9). There was also the matter of a missing chapter that might address the political, regulatory and legal constraints on monopolistic activity and corporate form, a 'sixth force' perhaps (Fried & Oviatt, 1989).

Rumelt, Schendel and Teece's examination of the relationship between strategic management and economics provided a more substantial commentary. Reflecting the thinking in an earlier Rumelt & Wensley (1981) paper they argued: 'The most influential contribution of the (1980s) from economics was undoubtedly (CS). In a remarkably short time, Porter's applications of mobility barriers, industry

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analysis, and generic strategies became broadly accepted and used in teaching, consultation, and many research projects' (Rumelt, Schendel, & Teece, 1991: 8). Considering the FFD along with the resource-based view (RBV) and its relatives, they continued: 'The single most significant impact of economics in strategic management has been to radically alter explanations of success. Where the traditional frameworks had success follow leadership, clarity of purpose, and a general notion of 'fit' between the enterprise and its environment, the new framework focuses on the impediments to the elimination of abnormal returns. Depending on the framework employed, success is now seen as sustained by mobility barriers, entry barriers, market preemption, asset specificity, learning, ambiguity, tacit knowledge, non-imitable resources and skills, the sharing of core competences, and commitment' (Rumelt, et al., 1991: 13). They noted a shift in business school culture from behaviorism towards economics impelled largely but not entirely by the success of Porter's approach. A substantial part of their paper was devoted to a historical analysis of this shift, leading to the hypothesis that CS's success may have been less to do with its content than to changes in the business and academic environment. Khurana's recent analysis of this same shift, especially the widespread acceptance of shareholder value over social benefit as the principal aim of management, supported this (Khurana, 2007).

Just as academic reviews of CS were indicating criticism Porter made an interesting move, reworking a 1979 Academy of Management Meeting paper into an AMR paper *Contributions of Industrial Organization to Strategic Management* (Porter, 1981). Had CS been written towards academics rather than practicing strategists and MBAs going beyond the ICA course, it would surely have included this paper's points in a theory and methodology chapter. It might also have included many of the academic arguments evident in Porter's economic papers. Naturally his position was not that the FFD lacked theory; on the contrary, this paper indicated the relevant IO-based theorizing intentionally omitted from CS. It also pointed towards the problems strategy scholars might have with absorbing IO-based theory, implying the critics who complained about the absence of theory were likely blinded by their own methodological presuppositions and unable to see what was truly present.

Porter's 1981 paper helped highlight the differences between the business and academic communities, not merely on the basis of their divergent interests and intellectual modalities, but also in terms of what was going on in their respective environments. CS's core assumption is obviously not neoclassical in the sense of looking for an equilibrating microeconomic model. Rather it presumes the dynamism, heterogeneity and uniqueness of each firm's situation - and academics and business practitioners have

different responses to this. Academics look for generalizations across multiple situations while practitioners look to the impact on their specific situation. Additionally the managerial community in both public and private sectors can be sub-divided into risk-bearing (line) managers versus in-house analysts and hired consultants who offer only advice. The line managers value the FFD's flexibility and wide applicability while advice-givers value the FFD's systematic and model-friendly structure. Along similar lines the academic community can be divided into strategy teachers and researchers, the former valuing the FFD's simplicity and generality for experienced and inexperienced students alike, all the while retaining the gloss of IO and microeconomic analysis's rigor. Academic researchers and theorists, on the other hand, find CS less supported the more closely its foundational assumptions are scrutinized. But, crucially, their focus fastened on arguments about the generic strategies pushing closer analysis of the FFD and the strategy making of Part III of CS to one side.

The root question is about the nature of the goods in the CS package. If we focus on the FFD, clearly CS's principal contribution, the academic complaints about a lack of theory were sustained rather than rebutted. As Bowman anticipated, subsequent theorizing and research has not provided support that was not already available when CS was published. But if CS, or more precisely Chapter 1 and Part III, the bits that managers take to immediately, does not offer a viable theory of corporate strategy in imperfect markets - those in which rents are presumed available - what does it offer? And, of course, why was what it offered so stunningly popular?

### **Success**

In our attempt to explain CS's success we shall look at the background against which the FFD was formed and the theoretical sources on which Porter drew. We reveals how the terms Porter used gathered their meaning, making it possible for us to get behind and problematize them. This takes us into the history of the industrial organization (IO) movement and its associated methodological issues and disputes. The idea is not to develop an academic history of IO, an economic historian's activity. It is to probe CS's appeal to practitioners via an examination of IO's relationship to contemporary business and to show it has little to do with the narrow logic of *homo economicus* or positivist notions of theoretical rigor.

Porter's 1981 AMR paper, the first part of the methodology theory chapter thought missing from CS, sets off from the Learned, Christensen, Andrews and Guth (LCAG) framework that underpinned HBS casework in business policy and strategy at the time, a framework that goes back at least to McNair et al. (1949) and Smith (1951). This framework was HBS's imprimatur when Porter arrived there in 1969 to begin his MBA. It is often presented as the four-box diagram that appears as Figure 1 in Porter's 1981 and Figure I-2 in CS. It says the strategist's task is to reconcile and synthesize four 'strategic elements': (a) company strengths and weaknesses, (b) industry-level economic and technical opportunities and threats, (c) the personal views of key implementers, and (d) broader societal expectations. Like the FFD this diagram deals with complex matters in a disarmingly presentable way. Porter wrote: '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' (Porter, 1981: 610). But he went on to criticize: '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. That was left to the practitioner' (Porter, 1981: 610). Close acquaintance with the LCAG framework, first as an MBA student and then later using it himself in the classroom led to its lack of theoretical substance becoming Porter's intellectual target. As he noted 'I wanted to influence thinking on business strategy by establishing a more analytical approach' (Stonehouse & Snowdon, 2007: 262). Achieving this meant re-balancing the attention given to the LCAG elements.

To understand Porter's thought in relation to the LCAG framework we must delve into the theoretical background of the IO movement – particularly into the differences between Bain and Mason. Bain was one of Mason's PhD students and, though they are often mentioned together, it is important to appreciate their theorizing was towards different policy objectives. Mason pursued a general theory of government control of big business and, following Joan Robinson and Chamberlin, ways of characterizing imperfect markets. Bain was interested in distinguishing industries and their differing degrees of monopoly. Discerning an industry's entry barriers remains crucial to effecting antitrust legislation. Porter summarized: 'The essence of the (Bain/Mason) paradigm is that the firm's performance in the marketplace depends critically on the characteristics of the industry environment in which it competes ... Industry structure determined the behavior or conduct of firms whose joint conduct then determined the collective performance of the firms in the marketplace' (Porter, 1981: 610-611). After mentioning the parallel development of oligopoly and game theory Porter went on to note

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an important change Bain made. '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' (Porter, 1981: 611). The resulting IO paradigm: '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 ... 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' (Porter, 1981: 611). He then went into reasons why strategy theorists had to-date not made much use of the paradigm - an important matter. But he left the relationship between the LCAG and Bain/Mason paradigms up in the air, even though his preference for the comprehensiveness of the LCAG model was obvious. Consequently it is not clear whether he considered the LCAG model to be part of or even compatible with the IO tradition.

Ghemawat's paper on the history of strategy teaching at HBS filled in some of the background to the LCAG paradigm and to the development of today's box of strategy consulting tools; but he paid no attention to the theoretical issues or to the paradigm's sources (Ghemawat, 2002). He noted 'business policy' teaching goes back to HBS's earliest days - in 1912 it was a required second-year course. He recited the policy teaching developments in the 1940s and 1950s that preceded the LCAG period (Ghemawat, 2002: 40-41). He noted the influence of a 'Harvard School' of economists, presuming Mason was a member of the Harvard Economics department. While this was true for a while before WWII (Markham & Papanek, 1970: viii), Mason was actually a political scientist and development economist, and from 1947 to 1958 was Dean of what is now the Kennedy School of Government. Likewise, though Bain spent a year at Harvard (1951-1952), his entire academic career was spent at UC Berkeley. More relevant is that after WWII an informal group of American IO researchers emerged that followed Mason's intellectual lead and that group included Bain and Tennant and the Harvard-based Markham, McKie and Caves (De Jong & Shepherd, 2007: 209). Harvard's economics department is not on the same campus as HBS so both Porter and Ghemawat tell of Porter's re-discovering IO economics 'across the river' through his involvement in the Business Economics PhD program (Stonehouse & Snowdon, 2007: 261). Its problematic was competition, rivalry and price flexibility. As Porter said: 'Suddenly I realized the professors at both (HBS) and the Department of Economics were talking about the same issues but they were coming from completely different perspectives ... While one intellectual tradition focused on case

studies, treating each study separately, the other focuses on theory and statistical analysis. The idea of bridging these two fields led to my doctoral dissertation, which led to (CS). Looking back I can now see how my being in the right place at the right time, with inspiring mentors, allowed me to make the connections that led to my later work' (Stonehouse & Snowdon, 2007: 261).

While Mason was clearly a major influence on three Harvard schools, HBS, Economics and Government, the source of the LCAG paradigm remained unclear. Did it come from Mason or from elsewhere? Was it in the IO tradition at all? The question is not mere academic nit picking for the answer is central to understanding CS's genesis and impact - this chapter's problematic. If Porter turned Bain 'upside down' it may matter that Bain's emphasis on 'industry' was different from Mason's focus on the firm (Phillips & Stevenson, 1974). But is the FFD either 'upside down'? The implication of Porter's story was that Bain stood back and saw industry structure as determining performance while the his own FFD was closer to the firm, focused on its strategic options (generic strategies perhaps) within a domain framed and constrained by industry conditions. While this may over-stress the determinism of Bain's analysis, it actually returns us closer to Mason's position and the trans-industry objective of his theorizing. Mason's focus was big business - or rather corporate power - generally. Unlike Porter's firm-level strategy-making, Mason's work was an attempt to theorize business's socio-economic power and, where considered politically appropriate, effective means of curbing it; an agenda that reflected a stream of progressive theorizing going back a century or more to, for instance, Veblen's *Theory of the Business Enterprise* (1904) (Ganley, 2004; Veblen, 1965; Witt, 2009).

Mason's position was that a firm has power to the extent that its strategic choices are not fully determined by its circumstances, neither by the market's structure nor technology nor anything else. As a student of Robinson and Chamberlin he argued from the market's imperfections rather than its perfections. The firm's monopolistic power pushes against the determining forces of market structure. Put another way, Mason presumed *ex definitio* that every firm has a degree of freedom or agency that ultimately derives from the rents it has created or captured. The antitrust issue is that such agency (especially with regard to price-setting) is sometimes directed against the public and its interest. Thus: 'in addition to market power, the large corporation exercises a considerable degree of control over non-market activities of various sorts. What all this seems to add up to is the existence of important centers of power in the hands of men whose authority is real but whose responsibilities are vague' (Mason,

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1959: 4). In Mason's economics power derives from agency alone. Today we might frame this as an occasion of principal-agent theory, with the public interest as principal and big business as agent.

Mason's focus was on the constraints to a firm's agency. Both his 1939 papers are critical of theory-light antitrust legislation that presupposes the law should be singled out as the main constraint to the firm's agency and therefore the government's main policy instrument (Mason, 1939a, 1939b). His position was there are many constraints, social, technological, historical, and so on, including the firm's internal circumstances, and that legislation needs to be constructed within an empirically grounded framework that recognized this multidimensionality. What he called the 'Hamiltonian' position was the fruitless attempt to establish every constraint definitively - which would have to include 'what Messrs. Ford and Firestone dreamed the night before their big tire deal' (Mason, 1939b: 61). It followed that each analysis would be unique. Noting the story of Dr Moriarty and Sherlock Holmes's bluff and double-bluff about which train they would catch, and the impossibility of determining a game-theoretic outcome given such indeterminacy, the analysis would also be eternally open-ended because of the ongoing stream of action and reaction between the various parties (Mason, 1939b: 64). Thus Mason's framework was one of dynamic disequilibrium, to be grounded and constrained empirically rather than theoretically. His first-step proposal was for an abbreviated empirically established list of constraints that could provide a frame of reference of some generality. As a political theorist with considerable experience of government and public policy, Mason was far from being an abstract ivory-tower equilibrium-seeking theorist. His objective was not ideologically rationalist, in the libertarian antigovernment tradition. His theorizing was fully committed to competition and its benefits. IO's task was to find a socially effective balance between the freedoms granted to the private sector and the public interest: 'the search is for that degree of market power which is necessary to an efficient conduct of business but beyond which there is an inevitable divergence between the particular and the general interest' (Mason, 1959: 7)

In his 1981 AMR paper the first diagram Porter drew of the SCP paradigm has one-directional fully determining arrows (Porter, 1981: 611). Striking a critical pose he went on: '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 on market structure' (Porter, 1981: 615-616). This overlooked the agentic core of Mason's thinking and overemphasized the determinism in Bain. Porter's corrective was to introduce feedback. In Porter's 1981 Figure 3, structure determines performance while performance equally determines structure, the system embracing both industry and firm levels (Porter,

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1981: 616). There are some problems with this. For instance it presumes a notion of industry, as distinct from either a firm or a collectivity of firms, that can be theoretically distinguished from the notion of a firm, a distinction that is clearly problematic given Porter's substitution-based definition of industry. But, more importantly, the implicit determinism, unilateral or bilateral, overemphasized or not, denies the focal firm's agency along with that of the other entities implied in the FFD. At the same time such a denial seemed the very opposite of Porter's intuition as well as of the political and legal shift mentioned earlier; for if HBS stands for anything it is for the freedoms of the private sector and the economic and political viability of a business's agency. In contrast, Berkeley's radicalism probably stands for more constraint, as in Bain's work. Mason sought a middle path or 'third way' that acknowledged both the fundamental notion of inter-firm competition and its economically progressive impact, and the complementary need to constrain business freedom to ensure its social acceptability. Porter's unease on these matters is clear in CS when he spoke of the 'judgmental placement' of an industry's boundaries (CS: 186) and, in a significant revision of the FFD, used two-way arrows in Figure 8-4 (CS: 187).

If Porter inadvertently underplayed the agentic core of Mason/Bain IO, a consequence of Caves's teaching perhaps, or through failing to restrain the deterministic systems thinking familiar to him as an engineering undergraduate or equally from neoclassical economic theory, what about the LCAG paradigm's treatment of agency? Agency is a complex idea with its own vast literature (Emirbayer & Mische, 1998). But it can be expressed as our everyday belief in our ability to make a difference in the world, disrupting the determinism of causal models. Agency's focus, though, is not on us as rational actors, taking in data and arriving at optimal or even satisficing actions. Rather it is on our ability to act on the basis of our judgment when the appropriate facts, objectives and reasoning capacity are not to hand. Agency presupposes a collision between what we now dub the condition of uncertainty or bounded rationality (Simon, 1997) and our determination to act anyway, come what may. There can be no doubt the LCAG framework sets up a discussion of the constraints to management's agency. The puzzle is to see how this is treated.

While the LCAG paradigm is often illustrated with the four-box diagram it was not originally presented that way. Its authors explicitly added an agentic dimension of executive judgment to the normal notion of fact- or evidence-based decision-making when they wrote: 'The ability to identify the four components of strategy ... is nothing compared to the art of reconciling their implications in a final choice of purpose' (Learned, Christensen, Andrews, & Guth, 1965: 21). This suggested a distinction

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between the 'four forces' being determinative and the executive-determined agentic act of their reconciliation, though they did clarify how this was to work. But in the pages before this statement LCAG laid out the constraints to the executive's strategic choices in very different language. They spoke in terms of what the firm (a) might do, in terms of environmental opportunity, or (b) can do, in terms of ability and power (Learned, et al., 1965: 20). They went on to (c) what the executives want to do, and finally (d) what the firm should do in terms of the ethical constraints on its actions. This fourfold 'might do, can do, want to do and should do' characterization of the executive's agentic task is far from accidental and had an important precedent in the work of John Commons. In his *Legal Foundations of Capitalism* Commons wrote: '... a working rule lays down four verbs for the guidance and restraint of individuals in their transactions. It tells us what the individuals must or must not do (compulsion or duty), what they may do without interference from other individuals (permission or liberty), what they can do with the aid of collective power (capacity or right), and what they cannot expect the collective power to do in their behalf (incapacity or exposure)' (Commons, 1924: 6). There seems little doubt the origins of LCAG's four boxes are the diagrams in Commons's Chapter IV (Commons, 1924: 65).

Commons, along with Veblen and Mitchell, was one of the founders of the 'institutional' or 'evolutionary' economic paradigm that eventually merged into IO. IO began as dissension from extreme laissez-faire and led to the founding of the American Economic Association in 1885. Among the movement's leading lights were Seligman, Adams, James (who played a major role in founding the Wharton School), and Ely. Because economics PhD's were not obtainable in the US prior to the founding of Johns Hopkins as the first of the US German-style research universities in 1876, all had trained in Germany and in the German Historicist tradition of economic history. When Ely returned from Heidelberg in 1880, where he had been a student of Knies, he joined the faculty at Johns Hopkins - where Commons was studying - and where he also influenced Veblen. Ely got into a methodological fight with Newcomb, a Johns Hopkins astronomer and mathematician, lost, and departed to Wisconsin, taking Commons with him. There the institutionalist economic agenda was competition, rivalry and performance - and public control of big business or the Trusts. Ely's 1903 *Studies in the Evolution of Industrial Society* helped establish the terminology and concepts of competitive analysis, monopoly and rivalry now associated with IO (Ely, 1971).

The German Historicist data-driven tradition led directly to today's case studies, already legitimate at Harvard before the foundation of HBS in 1904. As Phillips & Stevenson noted: 'There was an increasing

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tendency during this period (1885-1910) to do detailed studies of particular cases. The Bureau of Corporations carried out many such studies ... William Z. Ripley (Professor of Political Economy) at Harvard adapted his courses and publications to the Harvard Law School's inductive case study approach' (McNair & Hersum, 1954; Phillips & Stevenson, 1974: 331). Furthermore, 'Thorstein Veblen and John R. Commons, among others, forged an 'institutional' school of economics through their inductive analysis of evolving industrial patterns and economic processes' (Phillips & Stevenson, 1974: 331). The school made a major impact on Young (at Harvard from 1920), Knight and Chamberlin (at Harvard 1937- 1967) (Sawyer, 2004). Bain's work was included in the Chamberlin edited volume *Monopoly and Competition and their Regulation* that presented papers from a conference attended also by Knight and Machlup, Penrose's supervisor (Chamberlin, 1954: xiii). HBS's first Dean, Gay, like the other IO founders mentioned, had spent many years in Germany and was Schmoller's PhD student. He was hired on his graduation despite grave doubts about the value of his research at the lowest faculty level possible - instructor (Heaton, 1968: 53). One of his first tasks after opening the school was to collect students to conduct the case studies on which HBS's teaching would rely. Aside from serving HBS until Donham's arrival as Dean in 1919, Gay went to Washington to set up the Central Bureau of Planning and Statistics in 1917 (Cuff, 1989). Far from initiating the case-study approach, Donham, who arguably fashioned the school, campus and reputation of today, expanded HBS's case-writing resources and activity.

These historical comments are to suggest the IO tradition ran much further back than Mason, who studied Chamberlin's and Robinson's work at Oxford, at least as far as the German Historicists Schmoller and Knies. Their impact on economics in America was profound - and not uncontested. Ely's fight with Newcomb was a North-American replay of the epochal fight the German Historicists, especially Schmoller, had with marginalism and Menger known as the *Methodenstreit* (Vaughn, 1994). The battle-lines were methodological and lay between those who thought of theorizing in the deductive sense, from first principles or axioms to testable hypotheses, versus those who thought of theorizing inductively, the discovery of generalizations or patterns in accumulated data. Porter's work is clearly in the second tradition. The fact that CS was grounded in: 'detailed studies of hundreds of industries with all varieties of structures and at widely differing states of maturity' (CS: xvi) and that Porter managed a team of researchers who he credited fully for working with him for over five years (CS: x) is often overlooked. He noted: 'I didn't come to the conclusion that there were five forces until I had looked at hundreds of industries' (Argyres & McGahan, 2002: 46). CS was, in this sense, a product of both

collaborative work in a data-intensive tradition but also of a carefully managed team applying techniques that ran back to HBS's founding and its German Historicist inheritance. All this leads to a sense of IO being in HBS's genes. It also leads to a better understanding of the continuing methodological tension between HBS's inductive theorizing and the deductive approach of the economics profession generally, ruled as it is by neoclassical notions of equilibrium or what Winter calls the Friedman Conjecture (Winter, 2005: 517). The historicists were on the losing side in the *Methodenstreit*, marginalism and mathematics won hands-down (Vaughn, 1994: 31) and economic historians became mere historians which, in America, normally means theoretically irrelevant. In a famously sharp comment - which Williamson reprised later - Coase dismissed the 'old institutional economists' saying: 'without a theory they had nothing to pass on except a mass of descriptive material waiting for a theory - or a fire' (Posner, 1996: 419n).

The development of IO took American economists to a fork on Method Road. Most appreciated they could start from axioms, such as *homo economicus*, and look for testable hypotheses - like the majority of their discipline - or work with voluminous data and seek out generalizations that would withstand empirical challenge - a program lately reinvigorated as 'behavioral economics'. But with his dismissal of IO Coase missed the 'third way' Porter sought in CS as he attempted to bridge economic theorizing and managerial practice, leveraging off the LCAG framing of the executive task as something only agency can address. How agency crept into the economists' methodological picture is a longer story, buried in the development of American pragmatism that led to what we now call constructivism in its various varieties (Delanty, 1997). But it is clear that Commons - along with Dewey and James - absorbed agency and judgment into his thinking (Commons, 1924: 342)

Thus Porter appealed to the IO theorists' long-buried intuition that aside from the methodological options considered in the *Methodenstreit* - deductive versus inductive deterministic theorizing - there was a third 'agentic' position that, by definition, was central for management theorists. This position captured the silent party to the economists' discussion - the judging economic actor her- or him-self. Theory, whether generated deductively or inductively remains in the academic domain. Economic action lies in the practitioner's domain. It is the academics' conceit that theory alone is sufficient to frame and explain the actions of others. Bounded rationality denies this, ironically admitting the theoretical significance of agency. It presupposes managers have options and market power that are never wholly determined. Those adopting the agentic viewpoint consider strategizing only occurs under conditions of

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uncertainty. Their focus is on entrepreneurial action rather than deductive 'rigorous' analysis or inductively generated heuristics. They presume the entrepreneurial task is to allocate resources in circumstances that presume uncertainty, agentic actors and contextual or situational constraints. As Mason noted many of these are external to the firm, such as market structure, though there are also internal constraints (Mason, 1939b).

Once executive or entrepreneurial agency is admitted into the analysis there are at least two ways of talking about it, and they are complementary. One is to focus on the executive's or firm's options and treat agency as the un-analyzable judgment or constraint the executive projects into the situation, bringing it to closure, positioning judgment as human creativity only revealed through time - agent X chose option Y for reasons that cannot be theorized, merely typified. This is the impulse behind 'traitist' theories of entrepreneurial talent. It gets into the thorny question of 'theorizing' history that, once agency is admitted, can no longer be simply about prediction (Collingwood, 1994). Alternatively – and more productively – one might talk of the subjectivity of the agent's perceptions and expectations about what should be considered when preparing for agentic action. Rather than presume s/he has some curious entrepreneurial power that can be applied in all uncertain situations, the focus is on the way an uncertain situation is characterized by those engaged in it as they experience its specific constraints. Instead of hypothesizing an entrepreneurial trait the emphasis is on subjective judgment born of unique experience. Mason implied the second as he wrote: 'An alternative (to the neoclassical view) would be to conceive of a market as an area of geographic and product space bounded, in Joan Robinson's phrase, by a gap in the chain of substitutes. Within such an area, however, assuming that it could be defined, the position of individual sellers and buyers may be very different with respect to the influences affecting business policy. These and other considerations suggest that at least in the industrial area, the market and market structure must be defined with reference to the position of a single buyer or seller. The structure of a seller's market, then, includes all those considerations he takes into account in determining his business policies and practices. His market includes all buyers and sellers, of whatever product, whose action he considers to influence his volume of sales' (Mason, 1939b: 69). Mason implied the industry for every firm is unique, derived from experience and subjective perception rather than objectively determined or measured fact.

On the face of it theorizing must now be abandoned in the face of the arbitrariness of individual perception. Anything goes and there is no possibility of theory. But this conclusion only follows as we

confuse two different projects. The first more neoclassical or modernist project is to fully predict an economic effect by correctly establishing every determinant (cause) - and how it is to be measured. As Hamilton explains at length, IO's project does not share this methodological position (Hamilton, 1991). Rather IO is directed towards the agent it takes as axiomatic. While the modernist position writes agency out of the picture, denying its relevance to a fully modeled world, IO presumes the project is about providing the executive/firm agent with some kind of analytic support. Even as it grew out of the *Methodenstreit* between deductive and inductive methods, IO absorbed the agentic core of pragmatism and deployed its different epistemology to surface a third way that denied the possibility of any method ever fully theorizing economic activity. This move towards thinking of human (economic) action as more than mere rational choice led Hayek to remark every significant recent change in economics involved greater attention to the subjectivity of actors' choices (Hayek, 1945). But as the analyst abandons the modernist dream of a theory that is universal and applies to every context, the project is made coherent by bringing in the notion of practice-based experience of the particular context. The resulting analysis becomes 'situated' in terms of a time- and space-specific body of experience. Boundaries and entry barriers become axiomatic.

At this point it is useful to turn to Porter's 1991 SMJ paper and see it as the second half of the theory chapter missing from CS. As noted earlier, Porter was remarkably cautious about using the term 'theory', normally preferring the term 'framework'. His 1991 paper attempted to balance the deductive, inductive and agentic modes of analysis - the 'trading off' academic critics (and Henderson) sensed missing from CS. Few within strategy theorizing have tackled this issue, so this paper is one of the few substantial and illuminating discussions of the methodological issues confronting the group of strategic management researchers who, focusing on managerial action rather than on modeling a determining economy, work the distinction between the deductive, inductive and agentic methodologies. The paper contrasts sharply with the bulk of our discipline's methodology texts that at worst simply ignore the deductive-inductive echoes of the *Methodenstreit* and propose a dogmatic faith in statistical analysis - not even Schmolter believed data could ever be unproblematic - or at best paper over the contrast between deductive and inductive methods with palliatives like 'triangulation' or 'commitment' (e.g. Van de Ven, 2007). Instead of pushing the agent out with the language of causality Porter wrote the strategist into the foreground of the analysis: 'Given the goal of informing practice, the style of research in the strategy field, including my own, has involved a very different approach ... Instead of creating models, the approach was to build frameworks. A framework such as (FFD) encompasses many variables and seeks

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to capture much of the complexity of actual competition. Frameworks identify the relevant variables and the questions that the user must answer in order to develop conclusions tailored to a particular industry and company ... In frameworks, the equilibrium concept is imprecise' (Porter, 1991: 98). This is because 'frameworks' make no measurable presuppositions about the agent - rational, entrepreneurial, aware, or otherwise. On the contrary, they simply provide the grist to the particular agent's judgment mill. Frameworks allow the managerial reader to sense the call for their own agency and active strategic contribution.

All this suggests the FFD is more of a reconstruction of the LCAG paradigm rather than an upending of Bain's alleged SCP determinism, a conclusion that goes far deeper than focusing IO's analytic tools onto just one of that paradigm's four boxes. The underlying four-verb structure of the LCAG paradigm is obviously not identical to the FFD and some content realignment follows from the differing languages brought to the analysis. But the space created for the manager's agentic input is similar. While Porter's reconstruction of the LCAG framework clearly introduced new and potentially more rigorous language, stiffening the analysis, it also reframed the implicit model of the firm. Instead of the seemingly single firm context of the LCAG paradigm the FFD put the firm into a multi-firm competitive industry context. As Steiner noted in his review, CS put competition front and center at a time when it seemed underemphasized, even ignored (Steiner, 1981: 84). It follows that CS made a sharply defined place for a fifth force - rivalry - that seemed absent, under-emphasized or lost in the LCAG analysis. At the same time the FFD re-balanced the emphasis between LCAG's four boxes.

Once agency, or agentic judgment-driven choice under uncertainty, or what Porter calls 'managerial choice' (Porter, 1991: 105), is recognized as central to the FFD's value, as it is to the LCAG framework's, there are immediate echoes of Barnard's notion of the executive art of resolving the incommensurability of the organization sub-systems (Barnard, 1968: 287). But hanging the analysis on agency creates a new universe of methodological problems. Given the strategist's synthesis is uniquely situated in time and space the new question is whether any of the elements to be reconciled can be established objectively with measures that might make a more general analysis definitive. In his 1991 SMJ paper Porter cited Andrews on the uniqueness of every strategic situation - the firm's uniqueness being manifest in the performance differences within the industry. Accepting such uniqueness set up the debate about whether the industry or the firm 'matter more' as determinants of performance (Nelson, 1991; Rumelt, 1991; Schmalensee, 1985). In spite of his statements about the determining effects of industry

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structure, Porter said: 'the core notion of strategy is about being unique, about being different in some way to gain competitive advantage' (Stonehouse & Snowden, 2007: 267). But if a strategy is unique what can be said about its genesis that might be general, or more precisely, if rigorous theorizing is to be brought to the discussion of agency, what is its place, if any? Porter touched on this in his 1991 paper: 'The theory embedded in frameworks is contained in the choice of included variables, the way variables are organized, the interactions among the variables, and the way in which alternative patterns of variables and company choices affect outcomes' (Porter, 1991: 98). Theory's function is to refine the variables the agent uses, not to write agency out of the uniqueness of the solution. The role of theory is to embrace a notion of 'objectivity' that helps counter the agent's subjectivity. It is an appeal to others' views to help identify and illuminate the boundaries to the agent's ability to change the situation. Porter's intuition here was a restatement of von Clausewitz's notion of strategy as active agency and his view that theory's role lay in presenting the choices that must be made during the construction of strategy (von Ghyczy, von Oetinger, & Bassford, 2001: 74).

This raises fundamental questions about how strategy is to be theorized. With the distinctions between three analytic methodologies - deductive, inductive, and agentic - to hand it becomes clear the meaning of strategy is unavoidably contingent on the methodology within which its problematic gets framed. In the deductive method strategy is about optimization, the rigorous analysis-based discovery of the one best path to the declared objective. This is the notion behind strategic planning, now more or less discredited as relevant to strategic theorizing. In the inductive frame strategy seems more about heuristics, the selection of previously discovered action options, satisficing perhaps, based on exploration, experience, imitation and so on. Thus March (1991) suggests strategy is about balancing the costs of exploration and exploitation. An experience-based framework allows for 'emergent strategies', especially obvious in Mintzberg & Waters (1985). In an agentic frame the sense of 'outside-in' determination that diminishes the role of the actor, subordinating her/him to the facts of the situation, is reversed into 'inside-out' through an awareness of how managerial choice is more than merely choosing from the options the world presents. It is, in addition, a matter of changing or reconstructing that world to suit one's own ends. It follows the world implied differs from that implied by the other two epistemologies. It is bounded and limited, given the agent's change-making resources are likewise limited. At the same time the experienced limitations to the agent's power define the reality of the firm's situation - as in 'reality check'. The generality of the deductive approach derives from the presupposition of 'rational man'. In the same way, the generality of an agentic view on strategizing

derives *ex assumptio* from the implicit model of the strategist as a human being able to support rational decision-making with acts of judgment (e.g. Frantz, 2005). Implicitly adopting this general agentic model of the individual, Porter claims the FFD as a systematic framework for managing the processes of attention, discovery, decision and action the firm's unique world presents to the corporate strategist.

In short, CS was never advanced as a deterministic theory of corporate strategy, nor as a check-list of industry or economy-wide decision-making heuristics (Spender, 1989). Rather it is precisely what Porter told us it is, a general framework to help strategists focus on what they need to take into account as the constraints and options that define their firm's field of choice and arrive at the 'managerial choices' their particular situation calls forth. If this is correct, the nature and subtlety of CS becomes clear and our chapter's question about its appeal is more or less answered. The FFD's very lack of prescriptive theory, in the conventional sense of a theory grounded in some determining characteristics of the situation, frames a place for the executive's situated agency to generate closure - and does so in a way that virtually all executives, utterly familiar with the economy's call to their agency, can identify with. They have no need to grasp CS's intricacies to feel its tug. At the same time the book's IO discourse is crisp enough to appeal to those, such as strategy faculty and MBA students who, knowing no such agentic demands, see the FFD as a general model for practical analysis.

### **Concluding Comments**

Behind Porter's successful exploitation of the methodological ambiguities buried within the IO tradition lie lessons for management students, faculty, researchers and administrators alike. How was he able to 'get away with' a style of research that seems almost outlawed in today's Chicago-driven positivistic hegemony? Part of the answer is that his PhD was already done, awarded a prestigious prize, and was in straight economics anyway. When it came to the ICA, the CMR and HBR papers and to CS he took a huge gamble, but in a benign environment, presuming IO is indeed in HBS's genes. He was surrounded by a group of people who, despite their economics training, understood and explained management as leadership and agency - in the Barnard and LCAG framework. It had little to do with rational analysis and rigorous decision-making. Luck and timing played their part. There was also the impossible-to-examine mixture of discipline, personal qualities and good fortune that enabled Porter to attract the institutional support of Christensen and MacArthur, the HBS Dean. In our interview, as in several others, Porter fully

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recognized his break in being freed from his teaching load in the business policy group and left to set up his ICA course. This not only let him concentrate on the development of CS but also excused him from having to work closely with colleagues whose views of strategy and strategy making may have been increasingly divergent from his own. Porter also told us that Christensen, whose view of the professional risks Porter was taking would have been thrown into high relief by his own methodological struggles with other faculty, 'kept pounding on him to find the generalities, to be holistic, to avoid settling for a partial but rigorous view like the economists did'.

There was also the tricky matter of Porter's promotion from Assistant to Associate in 1976. His status as a Wells Prize winning young economist was secure, yet his interests were clearly moving in other directions as he persisted with trying to bridge economics and management practice. Along with his economic papers, many co-authored with established authorities such as Spence who went on to collect a Nobel, he 'threw a version of his *Note on the Structural Analysis of Industries* into his promotion package' (Porter, 1975). The committee noticed but treated it lightly as 'a noble experiment that failed'. The lesson-point being that senior faculty and administrators always hold their institution's future in their hands, bearing a considerable responsibility for how junior members develop as well as for molding the school's defining academic mores and practices, what is allowed and institutionalized, what is contrary and denied. In Porter's case his re-shaping of the agentic and IO principles on which the school was founded paid off handsomely in ways recognized and appreciated far beyond the walls of HBS. But for most young researchers today the balance between professional risk and identifiable return mostly leads them to be highly risk-averse, a posture all too frequently endorsed by their equally risk-averse supervisors and mentors. Under these circumstances, where are the next breakthroughs to come from, if not from able students encouraged to attempt something really challenging?

There are also lessons to be learned about today's methodological prescriptions. It is a commonplace that practice-oriented consultants developed most of the strategy-toolbox's contents. Earlier we suggested that, despite their varying emphases, most of these tools reflect the venerable distinctions underpinning the LCAG boxes, grounded in the Wisconsin IO tradition or maybe even further back. This tradition emphasizes entrepreneurship, agency and situatedness over time- and space-denying neoclassical rigor and mathematical precision. If we look at what has happened in the business strategy field since it became a discipline at the time of HBS's founding there are two signal contributors - Chandler and Porter. Both adopted inductive approaches; Chandler as a professional historian of

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business, Porter as a dedicated case-writer, case-teacher and case-analyzer. It is unlikely that either would be able to get tenure for or even publish this material today. Gay's attempt to establish business history as a distinct discipline survives at Harvard but is almost invisible within the Academy of Management. Yet we look in vain for significant discipline- or practice-shaping products from the deductivist or inductivist schools. The widely differing impacts of Porter's academic articles and his books underlines the disparity between the audiences CS addresses - different products to meet different needs. Today's academic orthodoxies also make the gulf between writing to an academic audience and to a practitioner audience wider than ever with, one must feel, loss to both. Thus the possibility of anyone repeating CS's success and bridging between the different methodologies seems increasingly remote. As we have shown, CS's relevance to the broad and influential policy-making audience in either public or private sectors lies in its attention to agency - suggesting the oft-noted irrelevance at the core of the rigor-and-relevance debate springs from its denial.

The subsequent evolution of Porter's work is also revealing. Our chapter's focus is on CS because there can be no doubt that it was the intellectual foundation of all of Porter's work. Up to the writing of his 1991 SMJ paper - in spite of the other work he was publishing - Porter seemed keen to stiffen up the theoretical dimensions of CS. He clearly had incentives and an opportunity to create the book CS's first academic reviewers (along with Henderson) looked for. We have argued that his two methodology papers successfully clarified CS's objectives and methodology, sketching the missing methodological chapter that would have made CS into the corporate strategy discipline's most significant work to-date. Some might argue CA is this book, though we would disagree. But rather than push towards such theoretical high ground and retreat from the challenge of dealing with uncertainty and agency, Porter saw an alternative development path - to recover Mason's agenda and move from business policy towards regional and national economic policy. Crucially, this enabled Porter to leverage his embrace of agency. Porter's service on President Reagan's Commission on Industrial Competitiveness helped open up new horizons and opportunities wherein the notion of agency was even more central. As Porter shows in the 1991 SMJ article, the FFD translates neatly into the 'diamond', a diagram of familiar complexity that made a place for public policy-level agency. Now the 'turning upside down' is really interesting. Given the changing times, many governments moved from policies intended to restrain the managerial choices that led to monopoly - Mason's agenda - and towards activist policies intended to boost industrial innovation and competitiveness, policies to shape the entrepreneurial agency of those affected but not wholly directed by the policies in question.

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Finally it is curious to see how strategy theorists have shifted their attention away from often ignored richness of Porter's work towards that of Barney and others who focus on the firm's 'resources and capabilities'. Porter's 1991 assessment of what has become our leading conversation, moving the field's researchers and students away from the FFD, is kind (Barney, 2002: 56) but a tad bewildered (Porter, 1991: 107). One might note in passing the RBV has had virtually no impact on business practice and there is obviously nothing yet that could be thought of as a candidate for the strategist's toolbox. This is not the place for a full analysis of the RBV which we have examined it elsewhere (Kraaijenbrink, Spender, & Groen, forthcoming 2010). Yet, the RBV's dominant position in the academic strategy writings warrants some comment and, perhaps, concern.

The RBV is sometimes offered an inside-out complement to the outside-in approach of the FFD. Given this characterization is a misunderstanding, at least when the FFD is seen as an agentic framework that works for managers rather than the deductive or inductive model it seems to offer academics, the relationship between the FFD and the RBV is far closer than Porter concedes. As we argue above, the FFD does not intend to describe an industry in any objective way. As Mason pointed out, the analysis is of a unique firm, and the need is to consider this firm's subjective view of its 'industry', the actors who might impact its rent-stream now and in the future. Thus both the FFD and the RBV have an inside-out framing. In this sense they are the same, the RBV being no more than a reconstruction of the FFD, much as the FFD is a reconstruction of the LCAG boxes. Their crucial difference is that the RBV offers no 'theory of the firm' beyond a 'bundle of resources'. As Grant has pointed out, it offers no theory of 'resource integration' (Grant, 2003). The FFD, in contrast, implies a Barnardian agentic synthesis, even though that theory has remained curiously under-specified by Porter and those who regard the FFD favorably. While the FFD focuses on the managing and reconciling the power relations in the strategic network of extended rivalry, paying little attention to the 'economic resources' that generate that power, the RBV focuses narrowly on the resources paying no attention to the power relations that actually shape the firm's rents or how they might be managed. Ironically, given Porter's lack of interest in Penrose and the RBV's claim to further her theorizing, the difference between the FFD and the RBV spins around the distinction between 'resources' and 'services' - with Porter clearly siding with Penrose (Porter, 1991: 108)

Thus the firm in CS is a dynamic reconciliation, as LCAG suggest, of the various three-level pricing, gaming, bargaining, market and signaling processes identified in the FFD. To go beyond competition and

fully embrace lobbying and negotiation, so rounding out his attention to the three bases of social order - power, game theory and institutionalization (Etzioni, 1961) - would oblige Porter to spell out the reconciliation process. The result would obviously be a major extension, given CS's principal shortcomings are an overemphasis on economic force and an under-attention to how that is acquired - inevitable legacies of IO - as well as less-than-appropriate attention to collaboration. Duly emended, such a broader reconciliation has the potential to be a terrific description of the executive task in a democratic capitalist socio-economy, an analysis that has the potential to best even Barnard's epic work.

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