Why *Competitive Strategy* Succeeds – and With Whom[1]

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Introduction

Porter's extraordinary record of private, public, academic, and corporate accomplishment has diverse roots, but it is clear that the genesis, publication and impact of *Competitive Strategy* shifted a promising career as a young award-winning Harvard economist into a very different gear. In this chapter we puzzle why the book has such impact and with whom – evidently widely embraced by managers, policy makers, consultants, and strategy teachers, though often criticized for its shortcomings by strategy theorists. At the same time 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 and O'Toole, 2005; Locke, 1996; Mintzberg, 2004, 2009; Spender, 2007; Starkey, Hatchuel, and 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 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 question, of course, is why Porter's work matters when so much of our work does not.

In the following sections we summarize *Competitive Strategy*'s principal points and look at how the book was received by reviewers, management educators, theorists and business practitioners. We consider the intellectual context and sources behind the book and probe explanations of its huge impact. We conclude with some lessons for the rest of us.

The Book

The iconic symbol of *Competitive Strategy* is the 5-force framework diagram appearing on page 4 as Figure 1-1. Its comprehensiveness obscures that it is the book's point of departure not its conclusion.

[1] The Editors and JC Spender interviewed Professor Porter while researching this chapter. He was generous, as ever. The discussion was especially appreciated in that it triggered thoughts about the methodological issues that form the core of this chapter.

Many readers of *Competitive Strategy* or its derivatives forget this and for them the diagram becomes the book; there is little more to be said. But Porter has something very different in mind. His first sentence reads 'The essence of formulating competitive strategy is relating a company to its environment' (Porter, 1980a: 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 framework (hereinafter FFF) is this new model, configuring both the firm's industry and the competitors within it. It is a model of considerable subtlety with several levels, identifiable entities and interactions. In contrast to microeconomic and marketing theory conventions, the FFF proposes the firm's environment comprises of more than competitors. Likewise, 'industry' has little to do with industry as economists use the term i.e. Standard Industrial Classification (SIC) codes. Porter defines an industry very differently – as 'the group of firms producing products that are close substitutes for each other' (Porter, 1980a: 5). The point of doing this 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 might seem oblique. But it provides a novel way of analyzing the firm's environment. Porter's definition presumes a conceptual 'barrier' between products rather than 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 pays attention to 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 barrier impenetrable even for Martha Stewart. 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 *Competitive Strategy* the barriers of interest are those between industries capable of interfering with each other – 'substitutability' being a term of art that implies interference with the firm's pursuit of its goals.

The analysis Porter offers stands on the 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 – industry concentration ratio or Herfindahl Index. Much of the power of the FFF is that it reaches beyond the industry's incumbent firms to consider other entities. Porter argues competitive strategy is a condition of 'extended rivalry' in which customers, suppliers, firms offering substitutes and potential entrants are all rivals whenever they are able to influence or interfere with the focal firm's rent streams (Porter, 1980a: 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 from management-created entry barriers – and the relationships between these rent types are complex (Mahoney and Pandian, 1992; Rumelt, 1987: 144; Spender, 1994).

Porter glosses over these differences, as well as the way some rents are affected by factors beyond management's control – global warming, technological change, resource depletion, and so on. *Competitive Strategy* focuses directly on the rent-shaping behaviors influenced by a firm's management without identifying the rents in question, for Porter's attention is on rent-shaping rivalry not on the rents themselves. While seldom able to influence the presence or absence of competition managers can often alter an industry's degree of rivalry, particularly by engaging in or eschewing price competition. 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 (Porter, 1980a: 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 that 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 FFF's stress is on the dynamics of preventing others' strategic action rather than on determining the industry's structure and its consequences.

The analysis in *Competitive Strategy* becomes explicitly dynamic and akin to multi-period game theory as Porter considers 'expected retaliation' as incumbents respond to an entrant's acts or threats to act. All of this leads to the FFF-based characterization of the firm's situation or state of rivalry it is experiencing (Porter, 1980a: 17). The FFF implies three levels of interaction – (a) the conventional micro 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. *Competitive Strategy*'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 the higher rents of a 'better position' in the strategic landscape, by exiting one line of business or entering another. Thus many put Porter's work in a 'positioning school' (Bowman, Singh, and Thomas, 2002; Mintzberg, 1987, 1990a, 1990b). This clouds the analysis actually offered in *Competitive Strategy*. In the FFF barriers define the significant rivalrous entities. Managing them 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 an entrant from another domain.

But Porter also argues that while strategic re-positioning is possible, rivalry can be provoked and managed by many other means. The FFF'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 various other entities comprising the firm's extended rivalry network. Porter discusses 'force' in several ways – bargaining power and signaled threats as well as actual acts of domain entry (Porter, 1980a: 6). Force and barrier are thought complementary, force being required to penetrate a barrier. Forces describe the dynamic relationships between the entities; tilting the analysis towards an active process view and away from a static structural view.

The treatment of force and dynamic process gradually reveals the book's true objective – little to do with the one-directional arrows in the FFF diagram. While Porter's aesthetic may be to show the firm under pressure from everyone else, the relationships are two-way. Quasi-monopolists such as Wal-Mart exert considerable pressure on their suppliers, perhaps forcing them into bankruptcy, and likewise rebut 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 to act against it. Likewise, at the analysis's extended level, when negotiating with a specific supplier, a firm might 'second source', looking into the market for alternative suppliers or substitutes.

Competitive Strategy not only shows managers how to view their rivalrous environment from the vantage point of identifying and protecting their own current rent streams, but also advises them 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 FFF revealed as a sketch of the network of power relations. In short, *Competitive Strategy* 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. This power is contingent on the specifics of the firm's strategic and economic situation and the actions and strategic intentions of those able to engage in it. The multifaceted empirical details here cannot be summarized in overarching concepts, economic, structural, strategic or behavioral. The FFF is a framework for selecting, viewing and attending to these heterogeneous empirical realities. Porter'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 *Competitive Strategy* 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 should then position itself there by selecting one of the three generic strategies (cost leadership, differentiation, and focus) (Porter, 1980a: 34). We think this interpretation peripheral rather than central to the book, even though it has given rise to the principal academic critiques of *Competitive Strategy* (e.g. Hendry, 1990). More to the point, if strategy is about executives deciding what to pay attention to, given their limited attention, *Competitive Strategy* 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-dimensioned model, then *Competitive Strategy* is about strategy. Inasmuch as strategy is about formal analysis and decision-making Porter's book 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 it comes at some cost to clarity – how might a firm's capabilities be measured against those of unknown potential entrants at the third, most nebulous, level of the FFF's analysis? How might the possibilities opened up by its dynamic non-equilibrium analysis be reconciled with rigorous econometric models? Evidently balancing the use-value of the FFF against its clarity calls for some authorial strategic decision-making, and there seems no question Porter's academic strategizing has proven successful.

Initial Reception

To understand how *Competitive Strategy* was received, we have to take Porter's earlier writings into account, in particular his 1976 article in *California Management Review* (CMR) and his 1979 article in *Harvard Business Review* (HBR). In the CMR piece Porter deploys the list-making style so evident and perhaps overdone in *Competitive Strategy*. 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 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, and Porter, 1977; Caves, Khalilzadeh-Shirazi, and Porter, 1975; Caves and Porter, 1976, 1977, 1978, 1980; Caves, Porter, and Spence, 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 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.

Porter further polished his style of addressing practitioners in the HBR article 'How competitive forces shape strategy' (Porter, 1979a), which attracted considerable attention. Its revealing 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 find laid out in *Competitive Strategy*. In the light of the complex and multi-layered nature of the concepts involved and the conceptual differences between price changes, bargaining power, and threats, the clarity and directness of the HBR article's language is striking. It 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 CMR (Porter, 1976b).

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-1980s (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 *Competitive Strategy* around 1975, only to put it aside as clumsy and overly dependent on 'stylized' academic economics. Around the same time he began teaching a 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). This evolved into Chapter 1 of *Competitive Strategy* and, as we have seen, it contains all the fundamental concepts of the book – with one notable exception, Chapter 2's 'generic strategies' that, in our interview, Porter suggested was almost an afterthought. The Note was also the basis for the 1979 HBR article. Leveraging from what he was learning from teaching his ICA course, Porter spent three years re-writing the manuscript, fleshing out the 1975 Note written not to his economic colleagues or even the business practitioners he mentions in *Competitive Strategy*'s Preface – but to his students.

On the whole *Competitive Strategy* 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 the book 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 … *Competitive Strategy* promises to revolutionize the creative process underlying the selection and formulation of strategies' (Lewis, 1981: 93). 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 Steiner, Hertz and Boyd that appeared in *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 he expressed concern about the enormity, time- and cost-wise, of a full-blown industry analysis. Hertz, likewise generally complimentary, noted that talking about competition was not new and that much of Porter's analysis arose out of management consulting practice – even though *Competitive Strategy* 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). 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 *Competitive Strategy* in its entirety could not be recommended to marketing practitioners.

The next issue of JBS had further reviews by Collier and Henderson. Collier's was a lengthy summary of the contents of each chapter with little comment. He found *Competitive Strategy* a well written book, useful for practitioners and students alike that should be in the library of every practitioner of strategic 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 also criticized the book for lacking the integration and systems analysis needed to qualify as a treatise on strategy: '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 substance of the book is a catalog of things to be considered in doing an analysis of the competitive situation … But it evokes an underlying feeling of frustration. Which of these factors are critical? How do they trade off against each other?' (Henderson, 1981: 84–85).

MIT's Bowman reviewed the book in *Sloan Management Review* and offered mixed views that reflected some of Henderson's concerns in more academic language. After going through the contents and complimenting Porter on his pedagogical style Bowman was explicitly critical. '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?' (Bowman, 1981: 67). Noting Porter's evident academic talent and citing some of his economics 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"' (Bowman, 1981: 67–68).

Competitive Strategy also received attention in the marketing journals. Fox, a marketing academic, reviewed it in the *Journal of Marketing*. 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 *Industrial Marketing Management*. Leo concluded: '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 *Competitive Strategy* 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 *Competitive Strategy* in any Academy of Management journal until Gartner's appeared in *Academy of Management Review* (AMR) (Gartner, 1985). But Porter's next blockbuster *Competitive Advantage* (Porter, 1985) had already appeared, as had his *Cases in Competitive Strategy* (Porter, 1983) – in short, the FFF machine was rolling and its success undeniable. Gartner reviewed both *Competitive Strategy* and *Competitive Advantage*. He opened noting Porter's ideas came from industrial economics while saying: 'Both books make accessible some of the ideas originating in the Industrial Organization (IO) literature, though Porter does not provide references to ground his ideas in the literature.' (Gartner, 1985: 874). Seeming unaware of Porter's 1981 article on IO's impact on strategy theory in AMR, 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' (Gartner, 1985: 875).

What is to be made of these reviews? Did they foreshadow *Competitive Strategy*'s success? A common compliment is to Porter's non-academic writing style and this was clearly important. A second is the recognition that *Competitive Strategy* was for practitioners – as Porter emphasized in its revealing Preface. Third is the book's comprehensiveness – lists of the contextual 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* – old stuff, 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 the book'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 Porter's CMR and 1979 HBR articles, as it is from *Competitive Strategy*. 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' (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 the book's success and may, perhaps, have been a kind of cause.

In the next section we look at the commentaries on *Competitive Strategy* that have appeared since these early reviews. For these later authors *Competitive Strategy*'s success is not to be doubted; our agenda is to see if they throw any additional light onto why.

Impact

Getting a handle on *Competitive Strategy*'s impact – beyond citations – is not as simple as it might seem. Hearsay has it that the FFF 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 FFF and other strategy tools actually impact strategic management or our research and publication practices? The bibliographic data presented by Furrer, Thomas, and Goussevskaia (2008) are interesting. They researched the occurrence of keywords in four leading management research journals, *Academy of Management Journal*, *Academy of Management Review*, *Administrative Science Quarterly,* and the *Strategic Management Journal* between 1980 and 2005. Bearing in mind Steiner's (1981) comment that *Competitive Strategy* brought 'competition' into the foreground of strategic analysis we can hypothesize an association between the greater use of 'competition' and *Competitive Strategy*'s publication. Furrer et al.'s findings are that 'competition' occurred in 6.1% of papers between 1980 and 1985, but rose sharply 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 slippery, we can grasp part of *Competitive Strategy*'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 and Levinthal, 2004; Hoskisson, Hitt, Wan, and Yiu, 1999; McKiernan, 1996, 1997). Yet, the limited mention of his book in earlier studies reminds us of how slowly ideas percolate through our literature. Bower, one of Porter's colleagues at Harvard Business School (HBS), published a review of the business policy field in 1982. *Competitive Strategy* rates only four sentences with 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). More generally, the early reviews of the strategy field revealed no signs of why *Competitive Strategy* might eventually become a major influence on strategy (Bresnahan and Schmalensee, 1987; Dutton, Fahey, and Narayanan, 1983; Eliashberg and Chatterjee, 1985; Enz, 1986; Ginsberg and Venkatraman, 1985; Hamermesh, 1983; Horovitz, 1984; Lamb, 1984).

Kiechel's 1981 *Fortune* article indicated the book 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 rising sales would support this view. The book's 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 already, Porter's disciplined communication skills and authorial talents, carefully honed in the years *Competitive Strategy* was gestating, as well as his two-pronged business and academic publishing strategy, were important contributing factors.

Gradually things changed, but only after the focus shifted and views of *Competitive Strategy* began to emphasize its different audiences. In the classroom the FFF 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 *Competitive Strategy* because they were interested in the academic niceties of strategic group definition or the marketing theory behind generic strategies – they liked the FFF. Likewise strategy teachers learned the value of presenting their students with the FFF, maybe as one tool in a strategic toolbox, together with cases on which to try it out. Here the FFF was immediately comprehensible with its implicit balance between industry level opportunities and threats and firm-level resources and processes.

In contrast, an increasing number of strategic, economic, and legal academics took exception to *Competitive Strategy*'s assumptions, casting doubt on the rigor or clarity claimed for the FFF. While some thought the analysis novel and insightful others felt Porter had borrowed too liberally from Scherer's earlier theorizing (Scherer, 1970). Others echo some of the earlier academic reviewers' misgivings, i.e. the principal criticism being methodological, with many of the points Porter makes lacking any justification; making the choice of forces seemingly arbitrary; resulting in an inability to operationalize any analysis based on these forces (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 and Oviatt, 1989).

Rumelt, Schendel, and Teece's (1991) examination of the relationship between strategic management and economics provided a substantial commentary. Reflecting the thinking developed in an earlier paper (Rumelt and Wensley, 1981), the authors argued: 'The most influential contribution of the (1980s) from economics was undoubtedly (*Competitive Strategy*). In a remarkably short time, Porter's applications of mobility barriers, industry analysis, and generic strategies became broadly accepted and used in teaching, consultation, and many research projects' (Rumelt, Schendel, and Teece, 1991: 8). Considering the FFF 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 … 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 also noted a shift in business school culture from behaviorism towards economics impelled largely but not entirely by the success of Porter's approach. The historical analysis of this shift comprised a substantial part of their paper, leading to the hypothesis that *Competitive Strategy*'s success may have been less to do with its content than to changes in the business and academic environment. Khurana's analysis of the 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 *Competitive Strategy* began to criticize the lack of theory, 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 *Competitive Strategy* been written towards academics rather than to practicing strategists and his

ICA students, 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 then evident in Porter's economic papers. Thus the 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. *Competitive Strategy*'s core assumption was obviously not neoclassical in the sense of looking for an equilibrating microeconomic model. Rather it presumed 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.

While managers and students revel in the FFF, academic researchers and theorists found *Competitive Strategy* less supported the more closely its foundational assumptions were scrutinized. But, interestingly, they fastened on arguments about the generic strategies, so pushing closer analysis of the FFF and the strategy making of Part III of *Competitive Strategy* to one side. The root question is about the nature of the goods in the *Competitive Strategy* package. If we focus on the FFF, *Competitive Strategy*'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 the book was published. But if *Competitive Strategy*, or more precisely Chapter 1 and Part III, the bits that managers take to immediately, did not offer a viable theory of corporate strategy in imperfect markets – those in which rents are presumed available – what did it offer? And why was what it offered so stunningly popular?

The Success's Roots

Our explanation of *Competitive Strategy*'s success requires a look at the background against which the FFF was formed and the theoretical sources on which Porter drew. We reveal how the terms Porter used gathered their meaning, making it possible for us to get behind and problematize them. We get into the history of the industrial organization (IO) movement and its associated methodological issues and disputes. The idea here is not to develop an academic history of IO, an economic historian's activity. It is to probe *Competitive Strategy*'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 *Competitive Strategy*, sets off from the Learned, Christensen, Andrews, and Guth (1965) (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 paper and as Figure 1-2 in *Competitive Strategy*. 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 FFF 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 and Snowdon, 2007: 262).

The IO was instrumental in achieving this. As Porter wrote: '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). 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, though his preference for the comprehensiveness of the LCAG model was obvious. Importantly, it is not clear whether he considered the LCAG model to be part of or even compatible with the IO tradition.

Ghemawat's 2002 review 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 the paradigm's sources (Ghemawat, 2002). He noted 'business policy' 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 and 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. After WWII an informal group of American IO researchers emerged that followed Mason's intellectual lead including Bain, Tennant, and the Harvard-based Markham, McKie, and Caves (de Jong and Shepherd, 2007: 209).

Harvard's economics department is not on the same campus as HBS so both Porter and Ghemawat told of re-discovering IO economics 'across the river' in the Business Economics PhD program (Stonehouse and Snowdon, 2007: 261). They talked 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 (*Competitive Strategy*). 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 and Snowdon, 2007: 261).

While Mason was 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 *Competitive Strategy*'s genesis and impact – our chapter's problematic.

Connecting with the Past

Probing the relationship between the IO and the LCAG framework takes us into the theoretical background of the IO movement – especially the differences between Bain and Mason (1939a; 1939b; 1959). 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. Bain was interested in distinguishing industries and their differing degrees of monopoly; discerning an industry's entry barriers remains crucial to effecting antitrust legislation. Mason pursued a general theory of government control of big business and, following Joan Robinson and Chamberlin (1954), ways of characterizing imperfect markets. Mason's focus was big business – corporate power – generally. His work was an attempt to theorize business's socio-economic power and, where politically appropriate, the effective means of curbing it; an agenda that reflected a stream of progressive theorizing going back a century or more to Veblen's *Theory of the Business Enterprise* (1904) (Ganley, 2004; Witt, 2009). Mason argued a firm has power to the extent that its strategic choices are not fully determined by its circumstances, not by market structure, technology nor anything else. In particular Mason's focus was on the constraints to a firm's agency or strategic choices. Bain's emphasis on 'industry' was different from Mason's focus on the firm (Phillips and Stevenson, 1974).

While Bain stood back and saw industry structure as determining performance, Porter's FFF was closer to the firm – Mason's focus – emphasizing its strategic options within a domain framed and constrained by industry conditions. In his 1981 AMR paper the first diagram Porter drew of the Structure-Conduct-Performance (SCP) paradigm has one-directional fully determining arrows (Porter, 1981: 611). 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). Porter's corrective was to introduce feedback. In Figure 3 of the 1981 AMR paper, structure determines performance while performance equally determines structure, the system embracing both industry and firm levels (Porter, 1981: 616). Yet, the diagrams inadvertently display a hint of determinism. 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 FFF.

The denial seemed opposed to Porter's intuition. 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, evident in Bain's work. Mason sought a middle path or 'third way' acknowledging both inter-firm competition and its economically progressive impact, and the complementary need to constrain business freedom to ensure its social acceptability. Porter's unease was clear in *Competitive Strategy* when he spoke of the 'judgmental placement' of an industry's boundaries (Porter, 1980a: 186) and, in a significant revision of the FFF, used two-way arrows in Figure 8-4 (Porter, 1980a: 187). If Porter inadvertently underplayed the agentic core of Mason's thinking in some diagrams, 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? 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 noted the agentic dimension of executive judgment: '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, and Guth, 1965: 21). Their distinction was between the 'four forces' being determinative versus merely framing the agentic act of their reconciliation. In the pages before this statement LCAG laid out the act's constraints in a 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 had an important precedent in the work of John Commons.

In the *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). So there seems little doubt the origins of LCAG's four boxes are the diagrams in Chapter IV of Commons's book (Commons, 1924: 65). Commons, along with Veblen and Mitchell, was one of the founders of the 'institutional' or 'evolutionary' economic paradigm that 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 its 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, 1903).

The German Historicist data-driven tradition led directly to today's case studies, already legitimate at Harvard before the foundation of HBS in 1904, and HBS's first Dean, Edwin Gay, like the other IO founders mentioned, spent many years in Germany. 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 back 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 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 to testable hypotheses, versus those who thought of theorizing inductively, the discovery of generalizations or patterns in accumulated data. Porter's work was clearly in the second tradition. That *Competitive Strategy* was grounded in: 'detailed studies of hundreds of industries with all varieties of structures and at widely differing states of maturity' (Porter, 1980a: xvi) and that Porter managed a team of researchers that he credited fully for working with him for over five years (Porter, 1980a: 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 and McGahan, 2002: 46).

Competitive Strategy was, in this sense, a product of collaborative work in a data-intensive tradition but also of a research team applying techniques that ran back to HBS's founding and its German Historicist inheritance. In this sense IO is in HBS's genes, a view that leads to a better understanding of the 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). Economic historians became mere historians, which, in America, normally means theoretically irrelevant. In a famous comment – that 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 they could work with voluminous data and seek out generalizations that would withstand empirical challenge – like 'behavioral economics'. With his dismissal of IO Coase missed the 'third way' Porter sought in *Competitive Strategy* as he attempted to bridge economic theorizing and managerial practice, leveraging off the LCAG framing of the executive task as something only agency can inform. How agency crept into the economists' methodological picture is a longer story, buried in the development of American pragmatism (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' intuition that aside from the methodological options considered in the *Methodenstreit* – deductive versus inductive determinism – there was a third 'agentic' position, by definition central for management theorists. It captured the party silenced in the economists' discussion – the judging economic actor her/himself. Theory, whether deductive or inductive, 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 significance of agency. Agency presupposes managers have options and market power that are never wholly determined.

Extending the Theory

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 *Competitive Strategy*. Porter was remarkably parsimonious about 'theory', normally preferring 'framework'. His 1991 paper balanced the deductive, inductive, and agentic modes of analysis – the 'trading off' academic critics (and Henderson) sensed missing from *Competitive Strategy*. Few strategy theorists have tackled this issue, so Porter's paper is one of the few substantial discussions of the methodological issues confronting strategic management researchers who, focusing on strategic action rather than on modeling a determining economy, work the distinction between the deductive, inductive, and agentic methodologies. His paper contrasts with the bulk of our discipline's methodology texts that either ignore the echoes of the *Methodenstreit* and propose a dogmatic faith in statistical analysis – not even Schmoller believed data could be unproblematic – or paper over the gap between deductive and inductive methods with palliatives like 'triangulation' or 'commitment' (e.g., Van de Ven, 2007).

Instead of using the language of causality to push the agent out of the analysis, Porter wrote the strategist into its foreground: '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 (FFF) encompasses many variables and seeks 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). 'Frameworks' make no measurable presuppositions about the agent – rational, entrepreneurial, aware, or otherwise. They provide the grist to the agent's judgment mill and allow the managerial reader to sense the call for their own agency and active strategic contribution.

All this suggests the FFF 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 not identical to the FFF 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 introduced a potentially more rigorous language, stiffening the analysis, it reframed the implicit model of the firm. Instead of the single firm context of the LCAG paradigm, the FFF put the firm into a multi-firm competitive industry context. As Steiner noted in his review, Porter placed competition front and center at a time when it was underemphasized, even ignored (Steiner, 1981: 84). It follows that *Competitive Strategy* made a place for a fifth force – rivalry – that seemed under-emphasized or lost in the LCAG analysis. At the same time the FFF re-balanced the emphasis between LCAG's four boxes.

In short, *Competitive Strategy* 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). It is precisely what Porter told us it is, a framework to help strategists note what they should take into account as constraints to their options and so arrive at the 'managerial choices' their situation calls forth. If this reading is correct, the nature and subtlety of *Competitive Strategy* becomes clear and our chapter's question about its appeal is more or less answered. The FFF's very lack of prescriptive theory, in the conventional sense of presenting 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, familiar with the economy's call to their agency, can identify with. They have no need to grasp *Competitive Strategy*'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 FFF 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? When it came to the ICA, the CMR and HBR papers and to *Competitive Strategy* he took a huge gamble, but in a benign environment, presuming IO is indeed in HBS's genes. He was in a group that understood management as leadership and agency – in the Barnard (1968) and LCAG framework – that had little to do with rational analysis and decision-making. Luck and timing played their part. There was also the mixture of discipline and personal qualities that enabled Porter to attract the support of Christensen and MacArthur, the HBS Dean. Porter's re-shaping of the agentic and IO principles on which HBS was founded paid off handsomely in ways appreciated far beyond its walls. But for most young researchers today the balance between professional risk and identifiable return inclines them to be risk-averse, a posture frequently endorsed by their 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 challenging?

There are also lessons to be learned about today's methodological prescriptions. It is a commonplace that practice-oriented consultants developed most of our strategy-toolbox's contents. Despite their varying emphases, most of these tools reflect the venerable distinctions underpinning the LCAG boxes, grounded in the Wisconsin IO tradition that emphasizes entrepreneurship, agency, and situatedness over a time- and space-denying neoclassical rigor and mathematical precision. If we look at what has happened to the strategy field since it became a discipline with HBS's founding there are two outstanding contributors – Chandler and Porter. Both adopted inductive approaches; Chandler as a professional historian of business, Porter as a dedicated case-writer, case-teacher, and case-analyzer. It is unlikely either would be able to get tenure for or even publish this material today. Gay's effort to establish business history as a distinctive discipline survived at Harvard, but remains 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 differing impact of Porter's academic articles and his books underlines the disparity between the audiences *Competitive Strategy* addresses – different products to meet different needs. Today's orthodoxies make the gulf between writing to an academic audience versus to a practitioner audience wider than ever – with loss to both. Thus the possibility of anyone repeating *Competitive Strategy*'s success and bridging the different methodologies seems increasingly remote. *Competitive Strategy*'s relevance to the broad and influential policy-making audience in either public or private sectors lies in its attention to agency – suggesting that 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 likewise revealing. Our chapter's focus is on *Competitive Strategy* because it was the intellectual foundation to all of Porter's work. Up to his 1991 SMJ paper – in spite of the other work he was publishing – Porter seemed keen to stiffen up its theoretical dimensions. He had the incentive and the opportunity to create the book *Competitive Strategy*'s first academic reviewers (along with Henderson) looked for. We have argued his two methodology papers successfully clarified the book's objectives and methodology, sketching the missing methodological chapter that would have made it into the strategy discipline's most significant work to-date. Some might argue *Competitive Advantage* 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, as discussed in later chapters of this volume.

Finally it is curious to see how strategy theorists have shifted their attention away from oft-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 FFF, 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 has produced nothing 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 elsewhere (Kraaijenbrink, Spender, and Groen, 2010). Consequently, the RBV's dominance warrants some comment and, perhaps, concern.

The firm in *Competitive Strategy* is a dynamic reconciliation, as LCAG suggest, of the various three-level pricing, gaming, bargaining, market and signaling processes identified in the FFF. To go beyond competition and embrace lobbying and negotiation, rounding out his attention to the three bases of social order – power, game theory and institutionalization (Etzioni, 1961) – would oblige Porter to spell out this reconciliation process. The result would be a significant extension, given *Competitive Strategy*'s overemphasis of economic force and under-emphasis on how it is acquired –

inevitable legacies of IO – as well as less-than-necessary attention to collaboration. Duly emended, the reconciliation has the potential to be a terrific description of the executive task in a democratic capitalist socio-economy, an analysis with the potential to best even Barnard's (1968) classic work.

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