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Review of Schendel and Hofer's Book Entitled Strategic Management: A New View of Business Policy and Planning

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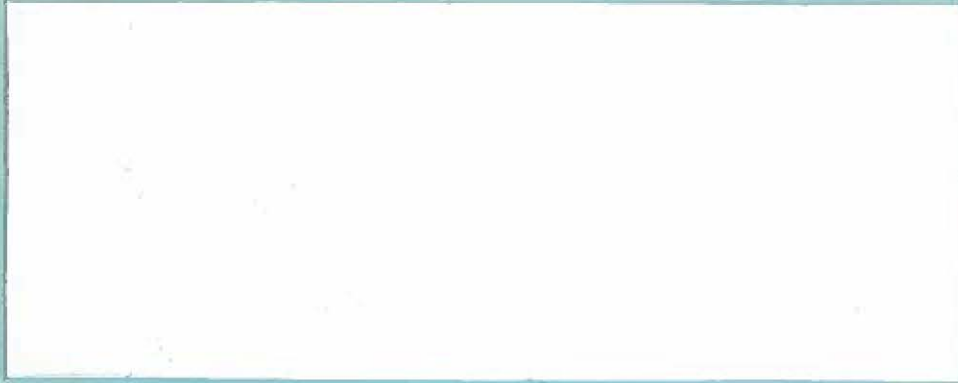


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REVIEW OF SCENDEL AND HOFER'S BOOK ENTITLED
STRATEGIC MANAGEMENT: A NEW VIEW OF BUSINESS
POLICY AND PLANNING.

Roger Evered

January 1980

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- * Comments to Roger Evered, Associate Professor of Organizational Science, Department of Administrative Sciences, Naval Postgraduate School, Monterey, CA 93940. Tel. (408) 646-2646.
- * NOT TO BE QUOTED WITHOUT PERMISSION FROM THE AUTHOR.

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BOOK REVIEW

Strategic Management: A New View of Business Policy and Planning

Dan E. Schendel and Charles W. Hofer. Little, Brown and Company, 1979.
539 pp.

Schendel and Hofer have made an important contribution to the rapidly developing literature on strategic management. Their latest book is a substantive collection of 14 research papers, 25 commentaries (by 19 academics and 6 practitioners), and numerous meta-commentaries (by the two editors). All of these statements grew out of the Conference on "Business Policy and Planning Research" held at the University of Pittsburgh in May 1977. The Conference was attended by 93 persons--82 academics and 11 practitioners--and yielded 39 separate statements for the two editors to work with. The book provides a reasonably representative 'state-of-the-art' of the business policy/strategic management/corporate planning field, circa 1978. (Few references are given post-1977, other than the authors' other recent book (Hofer and Schendel, 1978)).

The central objectives of both the Conference and the book were to define the domain of the emerging field (now labeled "Strategic Management"), to survey existing research, and to suggest fruitful directions for future research. By and large these objectives are well accomplished. The papers, commentaries and meta-commentaries are generally well-written, substantive and thoughtful. Collectively they yield a fairly cohesive sense of the substantive content of 'the field'--at least what the 2 editors and the 44 active contributors take the field to be. Clearly a different set of attendees and contributors would have yielded a different view. The 14 core

research papers were commissioned by the editors, who are themselves aware that their "knowledge of existing research (or lack thereof) influenced these selections" (p. viii). What has been generated, therefore, is a particular, but important, view of 'the field' as construed by a significant subset of those working in the broad field of 'strategic management.' Most readers of the Attendees list will undoubtedly think of some key persons who really should have attended. (For me, the names of Mitroff and Mason came immediately to mind.)

An admirable feature of the book is the extent to which genuine dialogue is captured. It goes way beyond the usual conference-proceedings format. The interactive commentaries and meta-commentaries add a rare and exciting vitality to the book. The editors share with us the process of generating the Conference, the Conference schedule, and the post-Conference Delphi process amongst the participants. It seems clear that most of the authors rewrote their own Conference statements after having been exposed to and influenced by the statements of other authors, presenters, attendees. The editors have succeeded in extending the multi-person dialogue beyond the Conference (which, I am informed, was itself highly participative) and their book is a substantial invitation to join in further dialogue. (I view this book review as an acceptance of their invitation to dialogue.) What we have, in fact, is a fine example of Bateson's metalogue; that is, a multi-person conversation about some problematic subject in which the participants discuss both the problem and the structure of the conversation as a whole (Bateson, 1972).

Although both the Conference and the book are remarkably broad-based, the collection of statements cannot claim to be comprehensive, definitive, value-free or ultimate--a point well captured by the editors in their closing

remarks. "Go forth and make this material obsolete..." they urge (p. 530). While most topics were mentioned somewhere there was, inevitably perhaps, a lack of balance in the treatment of topics. Some topics were repetitively presented (how many times did I read about the PIMS project?), while others are perfunctorily mentioned but not dealt with (such as CEO personality influences and Board characteristics). Still other topics, such as the concept of strategy, are dealt with in diverse and often conflicting ways (see later). A few aspects seemed awkwardly treated--such as the authors' attachment to the dichotomy of "analytical/conceptual" versus "social/political." This is surely a misuse of the term "analytical," since the distinction being made is between rational and organic (p. 98 and elsewhere). Otherwise, what meaning would there be to such terms as social analysis, political analysis, value analysis, or organizational analysis?

There were a number of important substantive aspects that should have been more explicitly considered. The area of environmental analysis was not adequately recognized, notwithstanding the two fine pieces by Utterback and H. Klein. Issues of the structure of the environment, environmental scanning and indicator analysis needed more attention. The psychological processes associated with the perception/appreciation of environmental change, is virtually neglected, except perhaps in the person-strategy discussion of Miner. Futures research and forecasting methodologies (scenarios, Delphi, structured modeling, etc.) are only obliquely mentioned--despite the stated importance of Toffler's Future Shock in the Preface. The impact of 'the economy' on the firm, once the focal interest of business policy in the 1950's, seems now to have dropped out. Analysis of the regulatory environment and the business-government interface is likewise missing. There is nothing on

boards-of-directors, social responsibility, and societal change; nothing on issues derived from values, corporate ethics and responsibility to the future. In the section on teaching implications of policy and planning research, there is no mention of business simulation games and venture projects, which, after the case method, are probably the most widely used teaching methods in this field. Most importantly, there was insufficient attention given to the issue of relating policy research to actual praxis in specific concrete real organization settings. I would like to have seen the relation between practice and theory building/testing more broadly attended to, not just in the Spender paper, but throughout. I shall return to this later in this review.

The collection of 14 papers and their associated commentaries make for fascinating and provocative reading, despite their varied quality and depth. Generally, however, the quality is high, and some pieces seemed to me outstanding--such as the papers by Galbraith/Nathanson, Mintzberg, Steiner and Rumelt, and the commentaries by Dill, Child, Spender and Macmillan. Several other pieces would have made equally good examples.

Of the papers, I derived most pleasure from the twin pieces on qualitative and quantitative research by Duncan and Hatten respectively, with their accompanying commentary by Naumes. I found them lucidly written, probing and reasoned. I especially liked their respect for both the conceptual complexity of the issues addressed and the limitations of the methods under discussion. In marked contrast, and rather surprisingly, I least enjoyed the lead paper by Ansoff, which I found to be somewhat simplistic, categorical and uninformative. Ansoff argues that the strategic problem has evolved over the last few decades, but it seems at least as plausible to conclude that it

is Ansoff's conceptualization which has evolved, rather than the problem itself. The paper added little beyond the notion of 'legitimacy strategy.' Did anyone else not see the Emperor's new clothes?

Many of the commentaries, such as those by Channon and Vesper made important contributions and extensions well beyond the papers they addressed. Most of them were insightful and responsive, but a few commentators, such as Miner, limited themselves to a less interactive review of their own research findings, which diluted the crucial sense of dialogue.

Editorial meta-commentaries are provided at the beginning of each of the ten sections, which serve to summarize, critique and elaborate on the material presented in the section. These editorial critiques and summaries were well done. Though somewhat repetitive, they were most useful in helping the reader digest and synthesize the material to follow. The editors also articulate their own world view of strategic management, and that is useful too. Less useful though is the editors' advocacy of a "new paradigm for strategic management" which, for me at least, detracted from the book. Part of the problem with this 'new paradigm' is that the editors apply the term to their particular core schema (p. 11-18). There are two serious problems with this; 1. The conceptual schema presented is transparently deficient, and 2. Kuhn (whom the editors use as a basis for their arguments (p. 1, 2), repeatedly asserts that by the term 'paradigm' he does not mean the particular substantive concepts and theories of a field, but rather its exemplars. The book represents a new paradigm in the Kuhnian sense, even without the editors' conceptual schema. On the other hand the editors' new schema is in no sense a new paradigm for the field. The recent use, overuse and misuse of the term 'paradigm' in the administrative science literature (Mintzberg, 1978; Morgan,

1978) suggests that further elaboration of this criticism would be appropriate and timely.

First, I will criticize the particular 'paradigm' proposed by the editors, and will subsequently examine the actual paradigm of the book.

Schendel and Hofer attempt to organize the hodge-podge of 39 statements on strategic management with a particular schema, which they label "the strategic management process" (p. 15), and repeat in each section of the book.

We are told in the preface that:

It is only in the light of this (guiding) paradigm that the full measure of each paper and our editorial comments can be evaluated and appreciated. The strategic management paradigm...represents a new way of viewing the policy and planning field that links the developing substance of the field...Thus, the paradigm deserves study because of its significance to management education and practice and not just because it integrates the papers in this book (p. ix).

To this reviewer, the paradigm offered does neither.

It is neither an organizing schema for the book nor a viable conceptual schema for the field. Let us look more closely at their conceptualization of strategic management.

The authors inform us (p. 2) that "the new paradigm we propose for the policy field is that of 'Strategic Management,' and it rests squarely on the concept of strategy." Now Schendel and Hofer have examined in depth and in detail diverse definitions of 'strategy' (Hofer and Schendel, 1978), and as a result "have developed a composite definition of strategy" built around four components: 1) scope; 2) resource deployments and distinctive competencies; 3) competitive advantage, and 4) synergy; and four organizational levels; 1) enterprise (following Ansoff); 2) corporate; 3) business; 4) functional.(p. 11).

When one compares Schendel and Hofer's schema of the strategic management process (p. 15) with their definition of strategy a serious problem

arises; they just don't fit! In fact, they seem almost unrelated. The schema offered derives from the editors' bold assertion that "there are six major tasks that comprise the strategic management process: 1) goal formation; 2) environmental analysis; 3) strategy formulation; 4) strategy evaluation; 5) strategy implementation; 6) strategic control" (p. 14). No other explanation is offered for this decisive categorization. Why six? Why these six? Why are they related in the way shown (p. 15)?

At the very least, I expected to see a box labeled 'resource analysis,' 'resource audit,' 'resource assessment,' or some such; this would have been consistent with the editors' definition of strategy on p. 25 of their 1978 book. Where do we fit the questions, "What kind of business are we/should we be in?"? And, What happens to corporate identity and overall mission articulation? Where does capability assessment fit into their schema? or purpose definition? or situational analysis? or board-of-directors' characteristics? or the regulatory environment? What to do with the notions of rivalry, timing and environmental enactment? Is goal-formulation really as independent of environmental factors as the schema indicates? Does it really make sense to distinguish 'evaluation' from either 'formulation' or 'implementation'? Is there no 'evaluation' after implementation? etc., etc. Unfortunately, little is presented to help resolve these core questions.

There are special problems with their treatment of 'goal formulation.' Throughout the book the editors make it very clear that the formulation of goals and objectives is not a component of the strategic management process as they conceive it. For Schendel and Hofer, strategy is taken to be "the fundamental means an organization uses to achieve its objectives" (Hofer and Schendel, 1978, p. 23) in contrast to Chandler, Andrews and the other Harvard

writers on strategy. "While (our) definition excludes goals and objectives, it recognizes that the achievement of objectives is the aim of strategy..." (p. 11). Why then is goal formulation one of the six components included in their strategic management process 'paradigm'? And why do the editors state (p. 14) that "the 'first' task in the strategic management process is the formulation of a set of goals for the organization"; of (p. 12) that "Corporate strategy addresses the question, 'What business(es) should we be in?'"?

Worse yet! We read in the summary section on the strategy concept the following: "Our strategy definition excludes goal; the processes by which goals are formulated, and the processes by which strategy itself is formulated" (p. 516) (Italics mine). Why then is strategy formulation included in their schema? This is a pervasive difficulty with their schema.

Even if we decide to use their schema to aid in reading the articles, we quickly discover that the book doesn't fit the schema. There is no section for either 'strategic control,' or 'environmental analysis,' the latter being tucked into the 'strategy formulation' section. On the other hand, section 6 deals with 'strategic management and organizational types' even though the schema doesn't.

The concept of strategy remains as fuzzy as ever. In fact, despite the authors' best intentions it is even fuzzier as a consequence of this book. Their concept of strategy lacks the clarity, grounding, balance and conceptual consistency of say the Andrews' book (Andrews, 1971). Schendel and Hofer offer such a variety of conflicting statements and definitions of the strategy concept that the reader is forced to choose. Their definition I most preferred was the following: "Strategy is the match between an organization;s resources

and skills and the environmental opportunities and risks it faces and the purposes it wishes to accomplish" (Hofer and Schendel, 1978, p. 11). I like this particular definition partly because it includes purpose in the strategy notion, and partly because it defines strategy as a fit, rather than a cause-effect plan.

Strategy is variously described throughout the rest of the book--as indeed it is throughout the broader literature. As I read through the book, I jotted down the following diverse descriptors of the concept of strategy: 'comprehensive,' 'key-idea,' 'high-level,' 'long-term,' 'important,' 'non-routine,' 'ill-structured,' 'entrepreneurial,' 'general management,' 'organization direction-setting,' 'domain-defining,' 'overall mission-setting,' 'clarification of corporate purpose,' 'value-setting,' 'configuration of internal and external relationships,' 'organization renewal and growth,' 'mechanism for organizational integration,' 'responding to environmental changes,' 'guidelines for major resource allocation,' 'total enterprise management,' 'the relation between the whole organization and its total environment,' etc. (In an earlier paper, I once defined strategy as 'the art of dealing with the unfamiliar' (Evered, 1975)). Strategy is probably all of the above and more. But, how to capture its essence in a simple conceptual schema?

In truth, we still have no viable conceptual schema or framework that makes sense of what we presently and collectively know about Business Policy/Strategic Management. The paper by Ansoff is perhaps the best single evidence of this. It follows then that there is no apparent framework underlying the papers and commentaries of this book, even though we may all agree that the papers somehow belong together. There are, of course, many ways to slice a new and complex field. While the schema that Schendel and Hofer offer is

simple to grasp, it is clearly deficient, as I've tried to indicate. We need more conceptual clarification in the strategic management field. Embarking on empirical research in all directions will not compensate for the conceptual confusion that resides at the core of the strategic management field.

Even though I reject the usefulness of the 'new paradigm' in the sense of a conceptual schema, I fully believe that this book truly represents a new paradigm in the Kuhnian sense of shared examples, and as the constellation of group commitments (Kuhn, 1970, 174-191). "A paradigm is what the members of a scientific community share" (Kuhn, 1970, p. 176). An important virtue of Schendel and Hofer's book is that much pertinent information is provided which reflects the exemplars that this particular community actually shared--ways of viewing the world, language, referents, etc. Additionally, information is provided on the process by which the Conference and the book were generated, the sponsors, the institutional support, how the papers were commissioned, presented, moderated, discussed, and how the subsequent Delphi process was run. This is important stuff. It provides real data on a community of scholars in the process of generating their own field. It provides us with the clues we need regarding what constitutes the core exemplars within the new field--individuals, topics, research studies, norms of professional behavior, journals, etc.

Before we can conclude that a new paradigm has been generated (and I believe it has) we would need to map out this one and show that it differs from what went before.

Schendel and Hofer say very little about the preceding Business Policy paradigm that Strategic Management has allegedly emerged from. They talk of

'strategic management' as the first paradigm in the field without acknowledging that the business policy field has had a distinctive Harvard-Case paradigm, probably since the first policy course was taught at Harvard in 1911. It was at least durable!

The Harvard-Case paradigm is still much in evidence in the Schendel and Hofer book. The source Conference was attended by 82 academics of whom 29 (or 35%) have either trained or instructed at the Harvard Business School, which is to say their teaching and research orientation is toward the case method. And in terms of institutional affiliations, Harvard and Pittsburgh were the two schools most represented at the Conference (and two of the four Pittsburgh attendees were Harvard trained). The case paradigm is also revealed in the references. By far the most frequently cited Journal is the Harvard Business Review, (15% of all journal references.) And, of the 68 Doctoral dissertations cited in the references, 75% are from the Harvard Business School. It would seem that exemplars for the 'new' strategic management field still come from Harvard rather than from say, Chicago, Yale, Princeton or Purdue. Undoubtedly though, the situation is changing.

Examination of the references of a field provide a rich source of paradigm exemplars. The Schendel and Hofer book is valuable in that extensive reference lists are provided. Nine separate reference lists are provided-- at the ends of the first 8 sections and one in the Introduction. (Regrettably, no references were given at the end of the Practitioner's Views, section 9.) Since each section purports to be a different topical area within the field one would suppose that a work cited on several lists might suggest that the work is centrally significant to the field. Inspection of the 9 lists reveals that two works appear on 8 of the 9 lists: 1) Chandler's Structure and

Structure (1962), and 2) Ansoff's Corporate Strategy (1965). These are followed by the Boston Consulting Group's Perspectives on Experience (1968), and Rumelt's Strategy, Structure and Economic Performance (1974) (on 6 of the 9 lists); and then by Andrews' The Concept of Corporate Strategy (1971), Allison's Essence of Decision (1971), and Hofer's article "Toward a Contingency Theory of Management" (AMJ, 1975) (each on 5 of the 9 lists). The data suggest that according to the particular community that generated this book, these are among the core exemplars.

There are a total of 1255 separate references listed on the lists. These are made up approximately as follows:

Book or report references	665
Journal article references	447
Case references	35
Dissertation references	68
Proceedings and presentation references	40
TOTAL	1255

This is a unique distribution that characterizes the strategic management field, circa 1978. Probably it will change dramatically over the next decade in favor of journal articles.

Which journals are cited by the community? What are our journal exemplars?

The 447 referenced journal articles in the Schendel and Hofer reference lists are cited in 95 different journals. The most frequently referenced journals are shown below:

<u>JOURNAL</u>	<u>NUMBER OF CITATIONS</u>
<u>Harvard Business Review</u>	69
<u>Administrative Science Quarterly</u>	48
<u>Academy of Management Journal</u>	25
<u>Long Range Planning</u>	23
<u>Journal of Business Policy</u> (now defunct)	14
<u>Business Horizons</u>	13
<u>Futures</u>	10
<u>Technological Forecasting & Social Change</u>	10
<u>California Management Review</u>	9

<u>Public Administrative Review</u>	9
<u>Journal of Management Studies</u>	8
<u>Journal of Economics and Business</u>	8
<u>Management Science</u>	8
<u>Academy of Management Review</u>	8

It would be interesting to compare the intrinsic paradigm of the book (= exemplars and community features) with: a) other related fields (clearly vastly different from the current O.B. field), and b) previous conferences in the policy field. (Analysis of say the Business Policy Teaching and Research Conference held at Harvard in 1970 would probably show a significant shift away from the Harvard-Case approach.) The Schendel and Hofer book provides a useful means for recording and tracking the emergence of a new paradigm, in the Kuhnian sense.

Most readers of the book will readily accept that a significant shift has occurred in the policy/strategy field over the past decade. It seems probable that the field is moving quite rapidly from a Harvard-business policy-case approach to teaching and research toward a Schendel/Hofer-strategic management-'scientific' approach. The editors seem to urge us, sometimes with passion, to adopt the scientific method. "There is a need for empiricism that goes beyond mere subjective description and interpretation" (p. 530). Maybe so, but there is also a need for empiricism that is phenomenologically grounded and that goes beyond mere scientific irrelevancy. Given the choice between carefully written cases of unique situations, and technically competent, but ungrounded, trivia that seems to contaminate some of our social science journals, I for one will choose the former.

Clearly there is a need in the field to move beyond the case method as the principle research mode--and the Schendel and Hofer book represents perhaps the most significant attempt to date. Schendel and Hofer articulate in

some detail their prescription for the particular normative science they have in mind for the field. I am left with a real concern that the field may be sliding in the direction of positivism, positively the greatest 'calf-path' of them all, to use the authors' rich metaphor. Positivism would be even more disastrous for policy/strategy than for other areas of the social sciences. The suspicion arises in reading the book that from a research point of view the editors wish to replace the no-good case approach with all-good empirical science. The ideal research paradigm is presumed known by the editors, and important inquiry options are closed out too early. The 'ideal' research paradigm is articulated with very little regard to the inherent epistemological assumptions of what is proposed in relation to the focal phenomena of the field. The initial sentence of the Spender commentary illustrates the point: "The title of this section of the text (Theory building and theory testing in strategic management) already reveals significant assumptions..."

Whatever criticisms there may be of the case approach to research, at least the findings were grounded in the actuality of managers of real organizations. There is a marked tendency in this book to ignore issues of grounding, relevancy and experiential validity in favor of 'doing Science.' Section 8 is devoted to explaining the teaching implications of policy and planning research--that is, how research informs teaching. (The Hegarty article actually turns out to be a discussion of which case to use--once again revealing the Harvard-case paradigm.) None of the book, however, is devoted to how practice informs research. The viewpoints of the corporate planning practitioners, particularly those presented in Section 9, are extremely pertinent and valuable. They are a must reading for all researchers in the strategic management field. The editors are to be commended for providing them.

Despite my criticism of certain aspects of the book, the bottom line for me is that I feel very positive about this book. I think it contains some excellent articles and overall is a major addition to the strategic management literature. It surveys the existing research and points to areas where research is needed. I expect it to be widely read, and I predict it will soon become the most frequently referenced publication in the field. The extensive reference lists, and the fine catalog of some 'research needs and issues' presented in the final section (pp. 515-530) make the book an invaluable research source. A comprehensive subject index would have further helped.

But, I think the real value of the book will be as a stimulant and catalyst. I read many things in it that I hadn't thought of before, and I was stimulated by some of the questions posed. I also read things that aroused me and that I take issue with. The book provides the necessary dialectic from which to strike a new field--including a new journal. In the Preface, the authors state that the book "should be judged primarily by the influence it has over future thought and practice in the Strategic Management area." In that case, I believe the book to be a huge success. But, I believe its professional fruitfulness will come from its value as a stimulant and a catalyst rather than from the truth-value of its concepts.

The new Strategic Management Journal, soon to be published (edited by Schendel) is an exciting extension of the Conference and the book. It offers further opportunity for dialogue, research and development of the strategic management field. Schendel and Hofer have made a remarkable contribution.

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